# Reporting Peers' Wrongdoing: Experimental Evidence on the Effect of Financial Incentives on Morally Controversial Behavior*

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## Abstract

I show that moral concerns can reverse the effect of financial incentives. I analyze a morally ambiguous behavior: reporting peers' wrongdoing. Agents' peers often know more about their behavior than principals do. However, denouncing a peer to an authority is morally controversial, as it might prevent future misconduct but also harm the peer. Authorities often encourage denunciations through financial rewards; yet these incentives can backfire if peers perceive being paid for harming others as morally unacceptable. I run a field experiment with 2,040 employees of the Afghan Ministry of Education, who are asked to confidentially report on their colleagues' attendance. I use a two-by-two design, randomizing whether or not reporting absence carries a monetary incentive as well as the perceived consequentiality of the reports. In the consequential treatment arm, where employees are given examples of the penalties that might be imposed on absentees, 15% of participants choose to denounce their peers when reports are not incentivized. Remarkably, in this consequential group, rewards backfire: Only 10% of employees report when denunciations are incentivized. In the non-consequential group, where participants are guaranteed that their reports will not be forwarded to the government, only 6% of employees denounce absence without rewards. However, when moral concerns of harming others are limited through the guarantee of non-consequentiality, rewards do not backfire: The incentivized reporting rate is 12%. My results suggest that employees report because they share the government's goal of reducing absence but are morally averse to being paid for harming their peers.

**Keywords:** Absence, Financial Incentives, Morality, Peer Reporting, Whistleblowing

# 1    Introduction

When unable to obtain information about agents directly, principals sometimes solicit knowledge from the agents' peers. In some cases, agents benefit from their peers sharing information about them, since such positive word of mouth can allow them easier access to government benefits, loans, and jobs.[1] In other cases, however, individuals share information intended to damage their peers.[2] For example, governments elicit denunciations from citizens through crime-reporting and whistleblowing programs, and internal channels for reporting coworkers' misconduct are common across many private and public organizations.

Denouncing a peer's wrongdoing to an authority is a morally controversial decision. On the one hand, reporting can lead to the peer's punishment, and harming others violates a core principle of morality. On the other, the punitive action might prevent further harm to victims of the misconduct: In some situations, actively harming someone (the peer) instead of passively letting someone else get harmed (the victim of the misconduct) can be justified on ethical grounds.[3]

Policymakers often encourage potential denouncers to report through financial incentives. In the United States, for example, whistleblower rewards programs are currently run by the Internal Revenue Service, the Securities and Exchange Commission, and the Commodity Futures Trading Commission.[4] The Crime Stoppers program, active in many countries, offers cash rewards to callers who report criminal activities to their local tip hotlines. These policies are based on the standard economic prediction that adding a monetary payoff to reporting will increase its supply. Moreover, reporting can be risky for the reporter, and monetary rewards can help compensate the denouncer for the potential cost of reporting (including any retaliation they might be subject to).

However, monetary incentives can backfire, for example, if individuals find it morally unacceptable to be paid for an action that harms their peers (Bénabou and Tirole, 2003, 2011).[5]

In this paper, I explore the viability of peer reporting as a policy tool for addressing public employees' absence by examining public servants' willingness to inform on their colleagues (Banerjee

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[1]In developing countries, for example, governments often let local communities select the beneficiaries of targeted assistance programs (Alatas, Banerjee, Hanna, Olken and Tobias, 2018; Stoeffler, Mills and del Ninno, 2016; Basurto, Dupas and Robinson, 2019), financial institutions screen potential borrowers through their peers (Bryan, Karlan and Zinman, 2015; Maitra, Mitra, Mookherjee, Motta and Visaria, 2017; Hussam, Rigol and Roth, 2018), and employers rely on job referrals for hiring high-skilled workers (Beaman and Magruder, 2012).

[2]Throughout history, authorities have relied heavily on citizens denouncing their neighbors. Bergemann (2017, 2019) offers a sociological account of the history of denunciations in oppressive regimes like the Spanish Inquisition, Romanov Russia, and Nazi Germany. As an extreme example in the modern age, counterinsurgents often set up hotlines to collect intelligence reports from local civilians on combatants' identities and their activities (Shaver and Shapiro, 2016; Wright, Condra, Shapiro and Shaver, 2017).

[3]Social psychologists describe this "whistleblower dilemma" as a trade-off between loyalty toward the peer and fairness toward the victim (Waytz, Dungan and Young, 2013).

[4]This type of program originated in 1863 with the False Claims Act (Givati, 2018; Nyieröd and Spagnolo, 2019). Since their inception in 2006 (IRS) and 2010 (SEC and CFTC), these whistleblower rewards programs have awarded, respectively, a total of $811 million (IRS, 2018), $326 million (SEC, 2018), and $87 million (CFTC, 2018). Incentives for internal organizational whistleblowing in the private sector, however, are relatively rare (Miceli and Near, 1992; Miceli, Near and Dworkin, 2008, 2009).

[5]Gneezy, Meier and Rey-Biel (2011), Bowles and Polanía-Reyes (2012), Kamenica (2012), and Besley and Ghatak (2018) provide recent reviews of different strands of the existing literature on the efficacy of financial incentives, especially around moral decision-making.

and Duflo, 2006; Finan, Olken and Pande, 2017). I run a field experiment to identify what motivates employees to report on their colleagues' attendance. I test whether monetary rewards encourage denunciations and explore how moral considerations affect the efficacy of financial incentives in morally ambiguous situations.

In particular, the experiment is designed to test whether individuals avoid transactions in which money is offered to them in exchange for information potentially harmful to their peers, even though they would be willing to engage in otherwise similar—but less morally controversial—monetary transactions that are harmless for their peers.[6]

The study takes place in Afghanistan, and its subjects are civil servants working for the Ministry of Education (MoE) in schools distributed across three different provinces (Kandahar, Nangarhar, and Parwan). Teachers' absence rates are an important and widespread problem in this context, as in many other developing countries (Chaudhury, Hammer, Kremer, Muralidharan and Rogers, 2006): Unannounced audit visits conducted in a random sample of schools across the region a few months ahead of the experiment found about 18% of employees absent from work (Blumenstock, Callen, Faikina, Fiorin and Ghani, 2019). Anecdotal evidence and focus-group interviews I organized with teachers indicate that while there are legitimate reasons for employees to miss work (such as health or security issues), employees are sometimes absent for illegitimate reasons, such as working a second job. This misconduct often goes unpunished because of the personal connections absentees have with the school administrators and the lack of effective oversight from the central government.

This is an ideal context in which to study morally controversial behavior. For Afghan school employees, the decision of whether or not to report on colleagues is marked by a tense conflict between two competing moral concerns. On one side, schools are collaborative environments in which employees interact daily with their coworkers, and these repeated interactions heighten the antisocial aspect of harming colleagues. This is especially true in the regions of Afghanistan I study, where many schools are located within small, tight-knit communities and social ties are strong. On the other side, the prosocial aspect of reducing absence is salient among educators who have their pupils' future at heart, especially in a setting where education is woefully deficient and returns to schooling are large. Repeated absences can be viewed as extremely harmful to students already suffering the effects of conflict and poor educational infrastructure, and thus strike an important chord with educators. The strong tension between the antisocial and prosocial elements makes informing on colleagues especially morally contentious in this context.

In collaboration with the MoE, I set up an experiment in which 2,040 employees from 151 schools are asked to complete a confidential phone survey on the attendance of their colleagues. I cross-randomized participants into four different main conditions along two dimensions. The first is

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[6]This idea is related to the concept of repugnance against certain transactions limiting the existence of some markets, such as those for organ transplants (Roth, 2007; Elías, Lacetera and Macis, 2019). A key difference, however, is that I do not consider exchanges in which both buyers and sellers want to engage, but that third parties think should be prohibited. Instead, I study a situation in which it is directly the party who could receive the monetary payoff who might want to refrain from the exchange when the transaction is monetary.

the financial payoff of reporting: some employees are offered a monetary reward for each colleague they report absent ('monetary reward' condition), while others are not offered any incentive ('no reward' condition). This treatment allows me to evaluate whether financial incentives increase or decrease the willingness to report.

The second dimension is the perceived consequentiality of the reports. All participants are told that their responses will be used for an academic study on absenteeism. Importantly, however, in one group ('no punishment' condition), employees are given the assurance that their responses will only be used for this purpose, and not shared with the MoE, removing any risk of penalty for reported colleagues. In the other group ('possible punishment' condition), participants are not given this guarantee: Respondents are told that their reports might be forwarded to the MoE and given examples of the possible penalties that absentees might incur.

This second dimension is a fundamental feature of the experiment, which I designed to identify the moral reasoning at play behind denouncers' decisions. It allows me to compare the effect of monetary incentives on the relatively morally neutral choice of providing inconsequential reports to its effect on the morally controversial decision to provide denunciations that have the potential to harm peers. The efficacy of material incentives could depend on factors orthogonal to the expected use of the reports, such as the size of the rewards and the information they deliver, or a deontological refusal to inform on peers in exchange for money. Alternatively, moral considerations due to the consequentiality of the reports could be important, creating a differential response to incentives in the 'no punishment' and 'possible punishment' conditions. Importantly, an aversion to receiving money in exchange for harming others would lead to incentives backfiring in the second group but not in the first one.

I also build measures of absence based on data I collected from unannounced audit visits to the schools, cross-checks of reports from multiple experimental participants about the same colleague, and administrative attendance records that I obtained from the MoE. I use these to investigate the accuracy of the denunciations. Finally, I complement the experimental results with attitudinal in-person surveys conducted in the schools a few months after the experiment. I ask the employees to express their opinions on whether they considered reporting absence the right thing to do, both when reporting is incentivized and when it is not. The responses help narrow the discussion of the experimental results toward the relevant channels.

I find that without rewards, 15.2% of the participants in the 'possible punishment' condition denounce at least one case of absence at their school in the week preceding the experiment. In half of the schools, there is at least one employee who reports. These findings are important for policymakers considering the viability of peer reporting as a tool to address absence.

My second and main finding is that in the 'possible punishment' condition, incentives backfire: The share of employees reporting cases of recent absence declines by 4.8 percentage points (or 32% of the unincentivized reporting rate). Survey evidence from the attitudinal interviews indicates that 58.3% of the respondents support unpaid reporting, while 41.7% think it is the wrong thing to do. This split in stated preferences shows that informing is indeed ethically contentious, even without

rewards. Importantly, the share of respondents having reservations about incentivized reporting is substantially higher, at 69.0%. This flip in the majoritarian opinion is mostly due to respondents finding it immoral to report on others for a reward. This survey evidence is consistent with the idea that incentives backfire because of participants' moral concerns about providing paid consequential reports.

Third, I find that monetary rewards instead encourage inconsequential reporting. In the 'no punishment' condition, while 6.4% of participants denounce cases of recent absence without rewards, the share increases by 5.7 percentage points (or 89% of the unincentivized reporting rate) when reporting is incentivized: When moral concerns of harming others are limited by the guarantee of non-consequentiality, rewards do not backfire.

Fourth, I note how—without rewards—reporting is higher when the reports are expected to be followed up on by the government: 15.2% of participants report in the 'possible punishment' condition, while 6.4% report in the 'no punishment' condition. This indicates that employees do not report absence simply because they feel compelled to tell the truth (Gneezy, Rockenbach and Serra-Garcia, 2013; Gneezy, Kajackaite and Sobel, 2018; Abeler, Nosenzo and Raymond, 2019) or contribute to the academic study; rather, they are motivated by the consequences their reports might have. The results of the attitudinal survey align with this interpretation: Many of the employees who believe that reporting is morally justified explain that it is because they feel a personal responsibility to fight against absenteeism and corruption.

The fifth finding is that participants are more likely to inform on colleagues who are absent according to alternative sources. For example, employees who are absent during unannounced audit visits are three times more likely to be reported than those who are not. Moreover, contrary to the concerns that rewards might spur malicious reports (Givati, 2016), the accuracy of the denunciations is not affected by the treatment conditions. This also speaks against the idea of false denunciations being directed toward colleagues against whom participants have personal grudges.

Finally, I discuss how the experimental results validate the idea that behavior is affected by the moral concerns around reports' consequentiality, and is not due to other channels. First, I rule out that incentives backfire because they are too small (Gneezy and Rustichini, 2000) or too large (Ariely, Gneezy, Loewenstein and Mazar, 2009): Equally sized rewards are effective in encouraging denunciations when they are guaranteed to be unharmful. Second, for the same reason, I exclude that the detrimental effect of rewards is due to deontological moral-reasoning: Respondents do not follow an imperative to never share negative details about their colleagues in exchange for money independent of context (Bénabou, Falk and Tirole, 2018, 2019). Third, the experimental results allow me to discuss explanations based on updates in beliefs about contextual attributes (Bénabou and Tirole, 2003; Deserranno, 2019). Monetary rewards likely do not signal that the government is committed to contrasting absence: If this were the case, rewards would have to be effective, since respondents display a demand for their reports to be acted upon (without rewards, they report more in the 'possible punishment' than in the 'no punishment' condition). Alternatively, respondents could interpret the offer of rewards as evidence that reporting is otherwise low and needs to be

artificially boosted. I do not find evidence for this explanation, as rewards would have to deliver signals of opposite sign in the two consequentiality conditions in order to fit the pattern of results. I also explicitly test whether incentives deliver information about the riskiness of turning in colleagues by asking respondents whether they think they will face problems for reporting. I can exclude this mechanism, since perceptions of risk prove to be similar across treatment conditions. Moreover, the effects of the treatments on reporting are similar among respondents who fear retaliation and those who do not. Importantly, however, 75.3% of the participants believe that they might face problems if they report a case of absence. Moreover, beliefs are good predictors of behavior: Among those who expect to face repercussions for reporting, the probability of reporting is about 8.3 percentage points lower on average than among those who do not fear any form of retaliation. This points to the need for policymakers to invest in reliable whistleblower-protection tools.[7]

Overall, my findings suggest that some public servants align with the government's objective to battle widespread absence and are willing to act by denouncing their peers' wrongdoing. The detrimental effect of monetary rewards on reporting is consistent with people being averse to violating a moral norm against receiving money for harming others.[8],[9]

This paper contributes to several strands of the economic literature. First, it is closely related to the behavioral literature on moral decision-making and the effect of monetary incentives (Gneezy and Rustichini, 2000; Bénabou and Tirole, 2006; Mellström and Johannesson, 2008; Ariely, Bracha and Meier, 2009; Lacetera, Macis and Slonim, 2012; Ashraf, Bandiera and Jack, 2014; Deserranno, 2019). This literature is primarily focused on prosocial behaviors in which the decisionmaker faces a trade-off between a personal cost and a positive externality to others (as in the cases, for example, of charitable giving and blood donation). Other papers study antisocial behaviors where the trade-off is between a personal benefit and a negative externality (Falk and Szech, 2013, 2017; Falk, 2017).[10] Rather than considering a behavior undoubtedly identifiable either as prosocial or antisocial, I

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[7]Participants are guaranteed about the fact that their identity will remain confidential. However, formal anonymity might offer little actual protection from retaliation. For example, if only a few employees observe the attendance of a colleague, the one reported absent can easily narrow down who the denouncer might be (Chassang and Padró i Miquel, 2018).

[8]Incentives might also crowd out employees' intrinsic motivation to report as a civic duty (Frey and Oberholzer-Gee, 1997; Frey and Jegen, 2001). This specific variation of the mechanism linking morality to consequentiality is also consistent with the experimental results, but the aversion-to-rewards hypothesis receives more support from the survey evidence, where participants explicitly voice their moral disapproval of the rewards. Moreover, I find that the attitudes of employees toward denouncing in exchange for money result in heterogeneous treatment effects: Incentives backfire only in those schools where the majority of employees oppose incentivized reporting, but not where this opposition is minoritarian. While this pattern does not rule out crowding out of intrinsic motivation, it is more consistent with the existence of a moral norm against being paid for harming others.

[9]Individuals might also have reputational concerns over this repugnance and posture as morally opposed to rewards even if they really are not, as in Bénabou and Tirole (2006), where individuals want to signal to themselves and others that they are prosocial. In this paper, incentives do not dilute the signal of prosociality with an over-justification effect. Instead, harming others for money is in itself the immoral action that gives disutility to some individuals and which some other individuals avoid taking for reputational motives. This is related to Cohn, Maréchal, Tannenbaum and Zünd (2019), where individuals return lost wallets not purely because of altruism, but also because of an aversion to viewing themselves as thieves.

[10]As an exception in this literature, Bursztyn, Fiorin, Gottlieb and Kanz (2019) study instead a situation in which individuals become fully conscious of the moral dimension of their actions only when attention is drawn to it through moral appeals.

provide novel insights to this literature by studying a morally controversial decision. Moreover, I introduce a previously undiscussed mechanism through which moral concerns can reverse the effect of financial incentives: the moral aversion to being paid for harming others.

Second, the paper adds to the political economy and development literature on absence in the public sector in developing countries (Banerjee and Duflo, 2006; Chaudhury et al., 2006; Muralidharan, Das, Holla and Mohpal, 2017; Duflo, Hanna and Ryan, 2012) and the emerging literature on using mobile technologies to monitor service delivery (Bhatti, Kusek and Verheijen, 2014; Callen, Gulzar, Hasanain, Khan and Rezaeek, 2018; Cilliers, Kasirye, Leaver, Serneels and Zeitlin, 2018; Bossuroy, Delavallade and Pons, 2019). As in Muralidharan, Niehaus, Sukhtankar and Weaver (2018), I take advantage of the widespread adoption of mobile phones to trial a monitoring system based on outbound calls. However, the system I test does not rely on reports issued directly by the beneficiaries of the services, nor does it use a community-based approach (Björkman and Svensson, 2009). Instead, I study peer monitoring and provide the first results on the willingness of public servants to report absence among their colleagues.

Third, the article contributes to the expanding literature on whistleblowing (Apesteguia, Dufwenberg and Selten, 2007; Bigoni, Fridolfsson, Le Coq and Spagnolo, 2012, 2015; Reuben and Stephenson, 2013; Buccirossi, Immordino and Spagnolo, 2017; Buckenmaier, Dimant and Mittone, 2018). The existing evidence on the effect of financial rewards on the information provided by whistleblowers is either correlational (Dyck, Morse and Zingales, 2010) or measured experimentally in lab settings (Butler, Serra and Spagnolo, 2019; Wallmeier, 2019) and through vignettes (Farrar, Hausserman and Rennie, 2019), and it offers mixed results. I complement this literature by providing the first field-experimental evidence on the willingness of individuals to denounce their peers' wrongdoing and of the effect of monetary incentives on reporting. My secondary results on the perceived risk of retaliation are of importance to the branch of the literature focused on whistleblower protection (Mechtenberg, Muehlheusser and Roider, 2017; Chassang and Padró i Miquel, 2018).

The remainder of this paper proceeds as follows. Section 2 provides an overview of the empirical setting. Section 3 describes the experimental design. Section 4 presents the main experimental results. Section 5 presents additional results and provides a discussion of mechanisms. Section 6 concludes with policy considerations and directions for future research.

## 2 Empirical Setting

The study took place in three provinces of Afghanistan: Kandahar, located on the southern border with Pakistan; Nangarhar, on the eastern border; and Parwan, north of the capital Kabul. In May 2018, before the experiment began, one unannounced audit visit was conducted through a local survey company in each of about 400 randomly chosen government educational facilities in these three provinces. Part of Afghan territory is under the control or influence of insurgents (Taliban and ISIS); nevertheless, a certain number of government schools remain intermittently operative in these regions. While the school audits took place in some of the insurgent-controlled territories, the

locations considered too dangerous to visit by the survey company were not audited. The sample of schools selected for the audits was otherwise intended to be representative of the schools in the three provinces. The majority of these facilities are either primary schools (23%), secondary schools (13%), or high schools (55%), but the list also includes some Islamic schools (6%) and professional schools (3%). About half of the schools are gender segregated: 31% of the schools are for male students only, 22% are female-only schools, 41% are mixed-gender schools with a majority of male students, and 5% are mixed-gender with a majority of female students.

The audit tracked the identity of employees present during the unannouced visit and referenced it against a complete list of employees from the school's payroll records. Estimates indicate that 28% of the employees were not on the premises at the time of the audit visit. The absence rate is 18% for the average school, in line with estimates from other developing countries (Chaudhury et al., 2006). There is, however, large heterogeneity between schools: During this unannounced visit, the absence rate is 10% for the median school and 50% at the ninetieth percentile, while at 43% of the schools all employees were present.[11]

At the school level, as part of standard procedures, attendance is recorded in a logbook that each employee needs to sign twice daily when entering and exiting the school. Each month, a school administrator is tasked with compiling the information from the attendance logbook into a monthly attendance summary detailing the number of days each employee was present. Employees who are absent from work need to provide some documentation to justify their absence in order to be entitled to their daily wage (for example, a doctor's note).[12] Once signed by the principal, the attendance summary is sent to the provincial payroll office and, along with other documentation, forms the basis for the calculation of each employee's monthly salary.

In October 2018, a few weeks after the experiment was completed, I arranged for a small focus group discussion about absence with twenty teachers from Kabul. They attributed employees' absences not only to legitimate reasons—including sickness, urgent family needs, or even the presence of security threats—but also to unsanctioned reasons such as moonlighting or laziness. The respondents also mentioned that absenteeism often goes unpunished because of personal connections between the employees and the school principals and administrators.

It is important to note that the documents provided by school administrations to the payroll offices might not reflect actual attendance. While the central government in Kabul has some weak incentives to keep schools' budgets below certain thresholds, the lack of effective monitoring gives schools an opportunity to inflate their wage bill by under-reporting both authorized and unauthorized absence. High-quality attendance data—collected through unannounced visits, for example—is generally unavailable to the government. The MoE periodically sends monitors

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[11]Only single-shift schools are included in these estimates. Across all schools, including schools with multiple time shifts, about 45% of employees were absent during the unannounced visit. However, this higher degree of absence is at least partially due to employees working during a shift different from the one in which the audit took place. For details, see Blumenstock et al. (2019).

[12]Even if their absence is authorized and daily wage paid, however, employees on any type of sanctioned leave are not paid their daily food allowance. Additionally, absence for twenty consecutive days without reasonable grounds is cause for termination.

to audit its facilities (with the primary purpose of countering ghost employees), but according to teachers, formal inspections are generally announced ahead of time and coordinated with the schools' principals. Moreover, schools that are too insecure or remote from main urban centers are rarely, if ever, subject to administrative oversight.

Beyond budget considerations, there are at least two other reasons why employee absences, even recorded ones, are problematic, according to focus-group participants. First, when teachers are absent, the learning process of their students suffers directly due to reduced instruction time. Second, since students cannot be left unattended, the teachers who are present at school need to divert part of their attention toward monitoring the students of absent colleagues, with negative spillover effects for their own pupils. These could all be reasons for those employees who identify with the mission of the MoE of providing education to the Afghan youth to be invested in contrasting absence and possibly in taking action: for example, by reporting cases of absence to the central government.

## 3  Experimental Design

School employees have more direct visibility on the attendance behavior of their colleagues than does the central government. This information could potentially be elicited by the government in designing personnel policy instruments—for example, setting up a hotline to receive absence denunciations or calling the denouncers directly—, and used as a basis for further investigations.[13] I conduct a field experiment to test whether employees are indeed willing to report their colleagues' absences when asked over the phone. The experimental treatments are designed to investigate what influences the willingness to denounce, with a focus on the effect of monetary incentives.

### 3.1  Sample Population and Implementation

The field experiment is run with the employees of 151 schools. These schools were among those reviewed by the May 2018 audit and were selected according to two criteria. First, at least six employees had to be present during the audit visit: I did this in order to have enough potential participants to randomize into the different experimental conditions when stratifying at the school level. Second, at least seven employees had to be absent during the visit. I did this in order to eliminate schools with low levels of observed absence, where there would likely be too few cases for the participants to report. Thus, the initial experimental sample of potential participants is composed of the 3,242 employees working at these 151 schools (according to payroll records) who were present during the May 2018 unannounced audit visit.

Between July and September 2018, a group of ten surveyors hired by a survey company based

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[13]It is important to note, however, that even when available, governments are sometimes reluctant to use high-quality data on attendance to impose penalties (Dhaliwal and Hanna, 2017). As a consequence, employees could be unwilling to report wrongdoing because they anticipate that the reports may end up not being used by the government anyway. However, the simple presence of such a monitoring system could be enough to improve service delivery (Muralidharan et al., 2018).

in Kabul made phone calls to these employees and asked them to participate in a quick survey. The surveyors were instructed to complete all phone calls to employees in a school before proceeding with the next school. I did this to limit the possibility of past participants talking about the experiment to future participants. Of the 3,242 employees in the initial sample, 2,061 (63%) were reached and said they were willing to participate; 35 (1%) refused to participate; 64 (2%) said they were busy; and 866 (27%) did not answer the phone. For the remaining 216 (7%), the phone number available in the records was wrong.

The surveyors introduced themselves as members of a research team collaborating with the MoE.[14] Employees willing to participate were first asked to speak about any problems they would like to see solved at their school. This question was intended to break the ice and set the stage for an honest and productive conversation with the respondent about their school.[15] The surveyors then told the participants that the survey would focus on absence. They explained that they would next read ten names. Each list of names was composed of three employees who were present during the unannounced visit and seven employees who were not. I did this to have some variation in the observed attendance behavior of the colleagues, in order to be able to assess the truthfulness of the reports. The respondents were not made aware of any of the criteria for the composition of the list. The names in the list were in random order. It was explained that, for each name, the respondent would be asked to confirm that they know the person and that the person is an employee at their school. For each confirmed employee, the respondent would then be asked whether the employee was present at school every day during the week preceding the study, and whether the employee had often been absent in the past. All respondents were then guaranteed that their identity would remain confidential. Once all this was explained, the surveyor asked once again if the respondent was willing to proceed with the survey. The final sample for the experiment is composed of the 2,040 individuals who were willing to continue and provided an assessment for each of the ten colleagues (99% of the 2,061 employees who initially gave their consent to participate in the survey).[16]

### 3.1.1 Sample Characteristics

The first column of Table 1 reports summary statistics for the final experimental sample of participants. About one-fifth of the employees participating in the experiment are women. The average respondent is 40 years old and has completed 14 years of schooling (that is, has some college education). Around three-fourths of subjects self-identified as Pashtun, one-fifth as Tajik, and the remaining 4% as part of another ethnic group. Their average monthly salary is about 8,000 AFN (US $100), making their annual income more than twice as large as the GDP per capita of the

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[14]The text of the scripts used for the phone calls are reported in Appendix B.

[15]The majority of respondents (96%) mentioned at least one of the following: problems related with the infrastructure of the school (mentioned by 75%), the availability of books (75%), salaries (31%), security (11%), the availability of water and electricity (7%), the number and quality of teachers (7%), or absenteeism (2%); 1% mentioned different types of problems, while the remaining 3% mentioned no problem at all.

[16]Of the 21 individuals excluded from the final sample, 11 explicitly refused to proceed with the section on absence, while 10 initially agreed to proceed but interrupted the call before the section was completed. Results remain virtually unchanged if the latter observations are included in the analysis.

country in 2018 (US \$563). Afghanistan uses an eight-level pay structure for civil servants, and all but the top rank are represented among the study subjects: About half of the participants (including the median participant) are at the fifth rank of the pay scale, while one third have a higher and one fifth a lower rank. As for position, 71% of the subjects are teachers, 6% are headteachers, 3% are principals, 7% have other administrative positions, and the remaining 13% hold other positions, mainly menial jobs. In addition to the May 2018 audit visit preceding the experiment, up to two other visits took place at each school after the experiment (in November 2018 and April 2019); the average respondent was present at 72% of the visits (though by design, all were present at least for the first).

### 3.1.2 Random Assignment and Balance Checks

At this stage, respondents were randomized along two dimensions in a two-by-two design for a total of four main treatment groups. Each group was given a different description of the upcoming task depending on their treatment status. First, some employees were offered monetary rewards for their reports, while others were not. Second, some employees were guaranteed that their reports would not be forwarded to the MoE, while others were led to believe that their reports might be forwarded to authorities, and thus could have some consequence for their peers.[17] The randomization was stratified at the school level. Columns 2 to 5 of Table 1 report summary statistics for each of the treatment groups, and column 6 presents tests of random assignment to the treatment conditions. As expected, I find that the sample is well balanced across all characteristics.[18]

Just after reminding participants that their reports were confidential, surveyors asked participants whether they thought that they nevertheless might face problems for reporting a case of absence. I did this to be able to explore whether the perceived riskiness of reporting could be affected by treatment status, and whether this affects the willingness to report. Finally, the surveyor proceeded with reading the first name on the list and asking whether the person was known to the participant, whether they were a current employee of the school, and the respondent's knowledge of their attendance behavior. The survey proceeded in a similar way for the remaining nine names. The responses to these questions constitute the main outcome of interest of the paper.

At the end of the survey, all employees were asked closing questions about their ethnicity and those of their colleagues and thanked for their participation. The subjects were also given more information about the study (such as the identity of the principal investigator and the purpose of the research) and were all guaranteed that the reports they provided would not be forwarded to the MoE or any other agency.[19]

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[17]The experimental treatments are described in detail in Section 3.2 below.

[18]Appendix Table A.1 reports summary statistics and presents tests of random assignment for the initial experimental sample. The table also includes a test for differential non-response: On average, 63% of the employees from the initial sample end up being part of the final sample, with no significant differences between treatment conditions.

[19]See Section 3.2.3 below for a discussion about the ethical considerations of the design.

## 3.2 Experimental Treatments

As explained above, the participants were cross-randomized into four different treatment conditions along two dimensions: financial incentives and expected consequentiality of the reports.

### 3.2.1 Financial Incentives

The first dimension concerns the financial incentives offered for denouncing absence. Some respondents were offered a reward for each employee they reported absent, while others were not. I designed these treatments to test whether financial rewards are an effective tool for encouraging reporting or if they backfire, especially in the presence of moral concerns.

**Monetary Reward.** Respondents in the *monetary reward* condition were read the following before they made their reporting decisions:

> *"As a reward for your help, we will make a top-up of 100 AFG to your phone for each absentee that you report. For example, if you say that seven persons were always present and that three other employees were absent some time last week or in the past, then we will send a top-up of 300 AFG to your mobile phone."*

The reward for reporting a single colleague corresponded to the salary an employee could earn in about 2 hours of work (about 1.30 USD). The reward was paid as a mobile phone top-up for ease of delivery.[20]

**No Reward.** Employees in the *no reward* condition, in contrast, were offered no financial reward nor any other type of incentive for denouncing their colleagues.

### 3.2.2 Expected Use of the Reports

The second dimension varies the expected use of the reports. All participants were told that their reports would be used for an academic study on absenteeism. This gives all participants the same basic rationale for reporting cases of absence. I then randomized whether the participants were given the guarantee that their report would not be forwarded to the MoE before they made their reporting decision, or whether they were told that the reports might be forwarded, making their denunciations potentially consequential. These treatments were intended to create variation in the moral content of the reporting decisions, with consequential reporting being relatively more morally charged than inconsequential reporting.

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[20]To avoid creating any paper trail linking participants to their denunciations, I did not send any specific text message to the receivers of the top-up. The participants who qualified for the reward would have at most received a text message notification from their phone company about a recent top-up to their mobile phone, identical to those they regularly receive when making a top-up. Nevertheless, in Section 5.2.1, I test whether monetary rewards changed the perceived risk of reporting (see, in particular, Footnote 33).

I designed these treatment conditions to achieve several purposes. First, they speak to the motivation to report colleagues. Do participants denounce absence out of a desire to be honest and truthful (Abeler, Nosenzo and Raymond, 2019), or because they wish to support an academic study, even if their reporting is inconsequential? Or are they motivated by the improvements the government could make using their reports?

Second, they allow testing for several mechanisms through which incentives might backfire. Do monetary rewards backfire because the size of the reward is too little (Gneezy and Rustichini, 2000) or too large (Ariely et al., 2009), or because the incentives deliver information about the cost of the action (Bénabou and Tirole, 2003; Deserranno, 2019)? Is there a deontological refusal to denounce peers in exchange for money, even if the denunciations are inconsequential (Bénabou, Falk and Tirole, 2018, 2019)? Or do moral considerations related to the reports' consequentiality determine the effect of the incentives? While explanations related to the latter would be important only in the *possible punishment* condition, all other mechanisms would affect reporting decisions in the *no punishment* condition as well. A cross-comparison of the effect of financial incentives across the two conditions would then allow for testing, and possibly ruling out, many of the aforementioned mechanisms.

**No Punishment.** In the *no punishment* condition, respondents were told that the only purpose of the reports was academic study. They were guaranteed that the names of the reported colleagues would not be forwarded to the MoE, so that no punishment of their peers should be expected:

> *"Your answers will be used only for an academic study on absenteeism. The names of people you report will not be forwarded to the Ministry of Education. So your answers will not be used to impose penalties on any of your colleagues."*

**Possible Punishment.** In the *possible punishment* condition, respondents were told that the reports might also be forwarded to the MoE and could thus result in punishments for the reported employees:[21]

> *"Your answers will be used for an academic study on absenteeism, but the names of people you report might also be forwarded to the Ministry of Education. So your answers might be used to impose penalties on your colleagues."*

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[21]I designed two variations of the *possible punishment* treatment. In the first variation (which I refer to as the *possible mild punishment* variation), respondents were told that an example of a possible punishment could be a small salary reduction for the reported employee. In the second variation (the *possible severe punishment* variation), the respondent was informed that a payroll steering committee had been instituted in Kabul to decide on the future of employees suspected of absenteeism. In this variation, the example of possible punishment for the reported employee given to the respondent is the termination of their contract. As explained in Footnote 12, salary reductions and contract termination are indeed actual consequences that frequently absent employees face at the two opposite extremes of the severity spectrum. In the paper, I pool these two versions, since their results are overall similar. Disaggregated results are displayed in Appendix Table A.5 and discussed in Footnote 25.

### 3.2.3 Ethical Considerations

It is worth discussing a few ethical considerations of the research design. In accordance with IRB guidelines, I established that not harming any employee (participant or colleague) as a result of the study was a top priority. This implied not forwarding the reports to the MoE, as doing so would have likely resulted in the government taking punitive action against the reported employees. This outcome would have been especially problematic since I could not guarantee the veracity of the information provided; after all, even employees who were actually present could have been subject to malicious and untruthful absence denunciations. Guaranteeing the protection of the denouncers against retaliation in such a situation could also have proven problematic.

I took these implementational constraints into account in designing the treatments. In order to create variation in the expected use of the reports, in the *no punishment* condition I heavily emphasized that the reports would be used only for an academic study on absenteeism, not sent to the MoE, and so would not have resulted in any consequence for the reported employees. In the *possible punishment* condition, I used a more restrained language: The scripts still mentioned the academic study as the first reason for eliciting the reports and only mentioned forwarding of reports and imposition of penalties as possibilities. Moreover, at the end of the phone call, all employees were told that their reports would not be forwarded to the MoE. This avoided any negative update of the participants' beliefs about the effectiveness of the government, which could have resulted from observing the lack of any government action upon the (supposedly) reported cases of absence. My concern here was not that this update could have influenced the results of the experiment: Beliefs could only have changed after the reporting decisions were already made. Rather, I did not want the study to cause any damage to the participants' ex-post perceptions of the government's effectiveness, which would have been an undesirable and unnecessary outcome.

Deceiving the respondents by saying that the reports would definitely be sent to the MoE and result in punishments would likely have resulted in larger treatment effects. However, I preferred to avoid any unintended consequences, such as diminished trust on researchers, even though participants are unlikely to be the subjects of other behavioral experiments in the future.

## 3.3 Data

The dataset I use in my analysis combines the results from the experiment with administrative data from MoE payroll records, attendance data from unannounced audit visits conducted in the schools, and data from a short survey given to employees afterwards about attitudes toward reporting absences.

### 3.3.1 Administrative Payroll Data

I first obtained digitized administrative data from the MoE provincial payroll departments of Kandahar, Nangarhar, and Parwan. The payroll records contain information about all employees paid by the MoE, including their gender, date of birth, educational attainment, position rank, and job

title. They also include, on a monthly basis, information about the school in which the employees work (and some characteristics of those schools) and their salaries, including a detailed breakdown of the various salary components. I used this data in three ways. First, I used it in combination with the data from the first unannounced visit to determine the lists of both participants and their colleagues. Second, I used to inform the description of the sample characteristics and the tests of balance presented in Table 1 and construct participant-level covariates. Third, the food allowance component of the salary helped me construct a measure of absence, which I describe below.

### 3.3.2 Data from Unannounced Audit Visits

A survey company conducted an unannounced audit visit in May 2018 in each of the 151 schools in the sample. Enumerators were tasked with listing all the employees who were present at the school at the time of the visit. The same exercise was repeated in follow-up visits in November 2018 and April 2019. Due to worsening of security situations, two schools could not be audited in the second visit, and four schools could not be visited in the third visit (one of these was one of the two not visited for the second audit). I matched this data with the payroll records data to identify which employees were present or absent during the audit visits. I used this matched dataset in two ways. First, I used the data from the initial visit to determine the lists of participants and colleagues. Second, I used it to build a measure of absence based on the audits for all the denounceable colleagues, which I detail below. For the participants of the experiment, I build a similar measure of presence.

### 3.3.3 Survey Data on Attitudes toward Reporting

During visits to the schools that took place after the experiment was conducted, the employees were asked whether or not they considered reporting on colleagues' attendance as the right thing to do, whether in the presence of monetary rewards or not. The survey respondents were also asked to describe the motivation and reasoning behind their opinions. I use this data for descriptive purposes, as well as to construct a dummy variable equal to one for the experimental participants whose majority of colleagues think that denouncing absence for rewards is wrong and zero otherwise. I use this variable in a heterogeneity analysis intended to explore whether reporting behavior (especially when incentivized) is correlated with the opinions prevalent at the school, indicating a possible role for social norms (or shared personal moral norms) in the backfiring of monetary rewards.

### 3.3.4 Main Outcomes of Interest

The study's main outcomes of interest are related to participants' responses to a question about ten of their colleagues' attendance in the very recent past:

> *"Can you confirm that [name of colleague] was working at your school every day last week?"*

The first outcome of interest, defined at the participant level, is a dummy variable $Y_i$ equal to one if respondent $i$ reported at least one colleague absent in the week preceding the experiment and zero otherwise. The second, defined at the participant×colleague level, is a dummy variable $Y_{ij}$ equal to one if respondent $i$ reported a specific colleague $j$ absent in the week preceding the experiment and zero otherwise.

I also define two additional outcomes at the participant×colleague level. One is a dummy variable equal to one if respondent $i$ reported colleague $j$ as always present in the week preceding the study and zero otherwise. The other is a dummy variable equal to one for the residual case in which the respondent $i$ reported something else regarding the colleague $j$. This primarily includes cases in which the respondent reported either not knowing the person or not knowing whether the colleague was absent in the recent past or not.

### 3.3.5  Absence Index Measures

I use data from unannounced visits, administrative records, and respondents' reports to build three measures of absence for each of the ten denounceable colleagues, through which I investigate the accuracy of the reports. In this section, I provide details on the construction of these three measures.

**Unannounced Visits.** The first measure of absence is based on the three unannounced visits conducted in the schools. For each denounceable colleague, I calculate the number of times in which the employee was not present during an audit visit and divide it by the total number of visits conducted in the school. Of the 1,645 denounceable colleagues, 16% were present in all the visits that took place in their school, 25% were absent once, 27% were absent twice, and 32% were absent during all three visits.

**Administrative Records.** Employees at the MoE are entitled to a daily food allowance of 30 AFG for each day of work, up to five days a week (although the working week for all employees is six days). However, as explained in Footnote 12, employees are not entitled to the allowance if they are absent from work, even if the absence is justified. Thus the total amount for each employee varies month to month, depending not only on their attendance, but also on the monthly schedule of the school. The payroll records contain information about the total allowance paid to each employee per month. Using the payroll information, I calculate the number of days a food allowance was paid to each employee in a given month, then define a dummy variable equal to one for denounceable employees who were absent in the month of the experiment more than their modal colleague working at the same school and zero otherwise. This serves as a second measure of absence. I use the food allowance of the modal employee to partial out the variation in allowance amount due to school-level schedule variation. According to this measure, 29% of the denounceable colleagues were absent more than other employees at their school in the month of the experiment.[22]

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[22]This is not a perfect measure of attendance as recorded in the monthly attendance logbooks kept at the schools, since attendance is measured only relative to the modal employee and does not take Thursday absences into account (employees are not entitled to an allowance on Thursday, although it is a working day). Moreover, any measure of

**Other Respondents' Reports.** The third index of absence is built around the fact that for 93% of denounceable employees (1,533 out of 1,645), multiple colleagues reported their attendance. This is because the participants belong to the same set of schools, and are generally asked to report to the same set of colleagues.[23] I use multiple reports on the same employee to build an absence index at the respondent×colleague level that is equal to the number of other respondents who reported the colleague absent in the recent past divided by the number of other respondents who were asked to report on the colleague. The variable is missing if only one report is available for a specific denounceable colleague. In total, the measure is available for 20,288 respondent×colleague observations. It is equal to zero in 69% of the cases, its mean is 2%, and its $75^{\text{th}}$ and $90^{\text{th}}$ percentile are 3% and 7%, respectively.

## 3.4 Empirical Strategy

Since treatment status was randomly assigned, my identification strategy is straightforward. To estimate differences in reporting rates across treatment conditions and, in general, for outcomes defined at the participant level, I use a regression of the following form:

$$Y_i = \beta_0 + \beta_1 Reward_i + \beta_2 Punishment_i + \beta_3 Reward_i \times Punishment_i + \epsilon_i \tag{1}$$

Similarly, for outcomes defined at the participant×colleague level, I use a regression of the form:

$$Y_{ij} = \beta_0 + \beta_1 Reward_i + \beta_2 Punishment_i + \beta_3 Reward_i \times Punishment_i + \epsilon_{ij} \tag{2}$$

Treatment status was randomized at the participant level and stratified by school. When examining outcomes defined at the participant level, as in equation (1), I calculate robust standard errors. For outcomes defined at the participant×colleague level, as in equation (2), I cluster standard errors at the participant level in order to take into account possible correlation between the ten observations available for each participant.

For the main analysis, in addition to results drawn from sampling-based inference, I report the results of randomization-based inference, where I calculate Fisher exact p-values for the sharp null

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absence based on administrative records might underestimate actual absence if school administrators underreport it. In order to validate this measure, I built a similar dummy variable based on the food allowance paid in the month in which the first unannounced audit visit took place. I find that 18.04% of the employees who were present at school on the day of the visit received an allowance lower than that of the modal colleague in the same month, and that employees who were not present at school at the time of the audit are 13.22 percentage points (s.e = 2.23) more likely to receive a smaller allowance than the modal colleague. This indicates that the payroll-based measure is informative about actual absence.

[23]The average number of reports per employee is twelve, the median is ten, and the maximum is fifty-five. The number of reports available for each of the 1,645 employees depends on two factors. The most important is the number of experimental participants from the same school, since larger schools have a larger number of experimental participants surveyed about the same set of colleagues. The second factor depends on the fact that, while generally all participants are asked to report on the same set of ten colleagues, a participant is never asked to report on his own absence. If a participant belongs to the main list of ten reportable colleagues for his school, instead of being asked about his own absence, he is asked to report on an eleventh alternative reportable colleague. As a result, fewer reports are available for these "eleventh colleagues."

hypotheses of no effect. As sample statistics, I use the difference in means by treatment status (or the difference-in-differences for inference on the interaction term $\beta_3$). Given the large sample size, calculation of the sample statistic for all possible realizations of the treatment assignment mechanism is computationally infeasible. For this reason, p-values are based on permutation tests with 10,000 iterations, using random sampling with replacement from the same subset of the universe of possible treatment assignments used for the original randomization. That is, treatment status is assigned at the participant level, as opposed to the participant×colleague level, and the randomization is stratified at the school level.

### 3.4.1 Heterogeneity

To explore patterns of heterogeneity in the results, I use regressions in which the treatment-condition dummies are interacted with a variable for the heterogeneity factor of interest. For example, when outcomes and covariates are defined at the participant level, I use a specification of the following form:

$$
\begin{aligned}
Y_i = {} & \beta_0 + \beta_1 Reward_i + \beta_2 Punishment_i + \beta_3 Reward_i \times Punishment_i + \\
& + (\beta_4 + \beta_5 Reward_i + \beta_6 Punishment_i + \beta_7 Reward_i \times Punishment_i) \times X_i + \epsilon_i.
\end{aligned}
\tag{3}
$$

## 4 Results

### 4.1 Treatment Effects: Reporting Recent Absence

In this section, I examine differences in reporting across treatment conditions.

#### 4.1.1 Effect of Monetary Rewards

**Extensive Margin.** I start by considering the effect of monetary rewards on the share of respondents who reported at least one colleague absent in the week preceding the experiment. Figure 1 presents the unconditional reporting rates of recent absence by treatment condition. Column (1) of Table 2 also reports estimates of the differences in reporting rates across treatment conditions from the regression specification displayed in equation (1), along with standard errors and p-values for tests of equality.[24]

I begin by presenting the reporting rates for participants in both the *no reward* and *monetary reward* treatments in the *no punishment* condition (corresponding to the two left columns of Figure 1 and the first two rows of Panel A in Table 2). In the *no punishment, no reward* group, the share of participants reporting at least one colleague absent in the week preceding the experiment is 6.38%. In the *no punishment, monetary reward* condition, the share reporting jumps to 12.09%. This

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[24]In the Appendix Tables A.2, A.3, and A.4, I report the corresponding estimates from regressions including, respectively, all the respondent covariates listed in Table 1, school fixed effects, and both covariates and fixed effects. The estimates remain very similar across all specifications, consistent with successful treatment randomization and experiment implementation.

implies that when the participants are told that their reports will not result in any penalty for their colleagues, monetary rewards increase the share of respondents reporting by 5.72 percentage points (s.e. = 2.21, as shown in the third row of Panel A in Table 2). The difference in reporting rates using sampling-based inference is significant at the 1-percent level. Using randomization-based inference, I also reject the null hypothesis that the monetary rewards had no effect on reporting (Fisher exact p-value = 0.01).

Next, I examine the reporting rates for participants in both the *no reward* and *monetary reward* treatments in the *possible punishment* condition. I present these in the two right columns of Figure 1 and Panel B of Table 2. In the *possible punishment, no reward* condition, the share of participants reporting at least one case of absence is 15.21%. In the *possible punishment, monetary reward* group, the share is 10.46%. That is, when the participants expect their denunciations could result in some negative consequence for reported colleagues, monetary rewards decrease the share of reporting respondents by 4.76 percentage points (s.e. = 1.81). The difference in reporting rates is significant at the 1-percent level using both sampling-based and randomization-based inference.

Taken together, these results indicate that monetary rewards have opposite effects depending on the expected consequences of reporting: incentives are effective when the reports are inconsequential, but backfire when the reports might lead to adverse outcomes for the reported colleagues. The difference in differences between the effect of monetary rewards when reports are inconsequential and the effect of rewards when reports are (expected to be) possibly consequential is 10.48 percentage points (s.e. = 2.86, as shown in the last row of Table 2, Panel C). This coefficient of the interaction term is significant at the 1-percent level using both sampling-based and randomization-based inference.

**Intensive Margin.** Next, I examine the intensive margin effects of monetary rewards on reporting by looking at outcomes defined at the respondent×colleague level. I calculate differences in reporting across treatment groups using the regression specification displayed in equation (2).

In column (2) of Table 2, the outcome is a dummy variable $Y_{ij}$ equal to one if respondent $i$ reported colleague $j$ absent in the week preceding the experiment and zero otherwise. As shown in Panel A, in the *no punishment* group, the probability of reporting a specific colleague increases from 1.33% in the *no reward* condition to 2.18% in the *monetary reward* condition. The difference of 0.85 percentage points (s.e. = 0.53) is only marginally significant. In contrast, as shown in Panel B, in the *possible punishment* condition, the probability of reporting is higher without rewards (2.66%) than with monetary rewards (1.44%). The difference in reporting of 1.22 percentage points (s.e. = 0.36) is significant at the 1-percent level using both sampling-based and randomization-based inference, as is the difference-in-differences coefficient of 2.07 percentage points (s.e. = 0.64).

Finally, in the last two columns of Table 2, I consider the varying ways participants could respond to the question about their colleagues' recent attendance when they did not report them absent. In column (3), the outcome variable is a dummy equal to one if the respondent reported the colleague as always present in the week preceding the experiment. This is the answer given in

the vast majority of cases. As expected, the sign of the treatment effects on reporting presence is always the opposite than for reporting absence. In the *no punishment* group, monetary rewards did not have a significant effect on the likelihood of reporting a colleague present: the probability is 79.91% in the absence of rewards and 79.17% in the presence of monetary rewards. On the contrary, in the *possible punishment* condition, respondents were 4.07 percentage points (s.e. = 1.34) more likely to report a specific colleague present in the *monetary reward* condition than in the *no reward* condition. The difference between the probability of 81.78% in the presence of rewards and of 77.71% in the absence of reward is significant at the 1-percent level according to either type of inference. The difference-in-differences coefficient of 4.81 percentage points (s.e. 2.33) is here associated with a p-value of 0.04 and a Fisher p-value of 0.02.

In column (4), the outcome variable is a dummy equal to one for the residual cases in which the respondent reported something else about their colleague, like not knowing any person associated with that name or not knowing about the recent attendance behavior of the colleague. While not explicitly indicating absence, these answers could still be potentially damaging for the colleagues. Consistently, the treatment effects on this outcome parallel in some respect those for explicitly reporting absence. In the *possible punishment* condition, the probability of reporting neither presence nor absence is 19.63% in the absence of monetary rewards, but this decreases to 16.77% in the *monetary reward* group. The difference of 2.86 percentage points (s.e. = 1.30) is associated with a p-value of 0.03 and a Fisher p-value of 0.02. The difference-in-differences coefficient of 2.75 percentage points (s.e. = 2.27) is, in this case, not statistically significant. The same is true for the difference between the two reward conditions in the *no punishment* group: 18.75% of responses are in the residual category in the *no reward* group, compared to 18.64% in the *monetary reward* group.

### 4.1.2 Effect of Possible Punishment

In this section, I discuss the effects of the expected use of the absence reports. The first row of Panel C in Table 2 presents differences in reporting between the *possible punishment* and *no punishment* conditions in the absence of monetary rewards. The possibility of punishment for reported colleagues increases the share of respondents reporting at least one colleague absent by 8.84 percentage points (s.e. = 1.91) and the probability of reporting a specific colleague by 1.33 percentage points (s.e. = 0.46), as shown respectively in columns (1) and (2). P-values and Fisher p-values are either equal or lower than 0.01 for both outcomes. This indicates that respondents may be motivated to report absence by the expectation that their reports will be sent to the MoE, possibly resulting in some penalty for the reported colleagues.[25]

When reporting is incentivized with monetary rewards, respondents are, if anything, less likely

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[25]See Appendix Table A.5 for results disaggregated by the two versions of the *possible punishment* treatment. These results also suggest a positive relationship between the likelihood or severity of the punishment and the willingness to report absence. While the difference between the two versions is never significant, at the intensive margin, the probability of reporting a specific colleague is 0.58 percentage points (s.e. = 0.63) higher in the *possible severe punishment* condition than in the *possible mild punishment* condition.

to denounce absence when their reports are potentially consequential than when they are inconsequential. The share of participants reporting at least one colleague and the probability of reporting a specific colleague are, respectively, 1.64 percentage points (s.e. = 2.13) and 0.74 percentage points (s.e. = 0.45) lower in the *possible punishment* condition than in the *no punishment* one. Only the second estimate, however, is statistically significant at the 10-percent level.

## 4.2 Accuracy of the Reports

Participants' reports on their colleagues are ultimately unverifiable, in the sense that complete and definitively reliable information about the actual absence of employees in the week preceding the experiment (for example, from a biometric attendance system) is unavailable.[26] Nevertheless, there exist other sources of information against which the reports can be verified: unannounced audit visits, administrative payroll records, and reports by other respondents. In this section, I investigate the accuracy of the reports by testing whether participants are more likely to denounce colleagues who are also absent according to these other sources. I also test whether accuracy varies by treatment condition—and in particular, whether monetary incentives make the reports less accurate.[27]

I use data from unannounced visits, administrative records, and respondents' reports to build three measures of absence for each of the denounceable colleagues.[28] First, up to three unannounced audit visits were conducted in the schools both before and after the experiment took place; I use these to build an absence index equal to the proportion of times a colleague was absent during these visits. Second, administrative payroll records contain information about the number of days for which a food allowance was paid to each employee in a given month. Since an allowance is not paid for employees who are marked absent on a given day, I use this information to build a dummy variable equal to one for reportable employees who were absent more than their modal colleague in the month of the experiment. Third, multiple participants (working at the same school) are asked about the absence of the same colleagues, so that their reports can be compared with one another; I use multiple reports on the same employee to build an absence index at the respondent×colleague level equal to the proportion of other respondents who reported the colleague absent.

In Table 3, I present the results of regressions that test whether there is heterogeneity in reporting rates by these three measures of absence and whether treatment effects are heterogeneous. I begin with documenting, in the first row of the table, how reporting rates in the *no punishment, no reward* condition correlate with these three measures. In columns (1) and (4), I show that employees who were absent for all the unannounced visits conducted in their school were 1.40 percentage points (s.e. = 0.57) more likely to be reported absent and 12.75 percentage points

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[26]The absence of reliable information is the reason that motivates authorities to elicit information from peers in the first place. The decision on whether or not to report wrongdoing in cases in which the authority already has information about wrongdoing is fundamentally different, less common, and beyond the scope of this project.

[27]While the possibility that monetary rewards might discourage reporting has been relatively overlooked, both the academic literature (Givati, 2016) and the policy discourse has focused on the possibility that financial incentive might encourage false reports.

[28]See section 3.3.5 for details on the construction of these measures.

(s.e. = 2.03) less likely to be reported present than employees who were always present during the audit visits. This indicates that absence during unannounced visits is a strong predictor of being reported: Participants are significantly more likely to report absent during the week preceding the experiment those colleagues who are often absent from school (even in other weeks). Moreover, the coefficient on non-reporting presence is an order of magnitude larger than the one on reporting absence, which suggests that not reporting someone present, even when not explicitly reporting them absent, provides a signal of absence that is worthy of attention. In column (2), I show that employees who are absent according to the administrative-records measure are only 0.44 percentage points (s.e. = 0.48) more likely to be reported absent than those who are not. This suggests that participants report absent even many colleagues who are not marked absent by the school administration. The negative effect of 5.23 percentage points (s.e. = 1.87) on being reported present is instead an order of magnitude larger and significant at the 1-percent level, as displayed in column (5). Finally, in columns (3) and (6), I show that a one percentage-point increase in the proportion of other respondents who report a colleague absent increases reporting rates of absence by 0.43 percentage points (s.e. = 0.19) and decreases reporting rates of presence by 0.48 percentage points (s.e. = 0.25). This indicates a significant degree of agreement between the reports of different participants. This is not necessarily evidence of the reports being accurate; it could be the case that multiple participants dislike the same colleague and report them absent independently of their actual attendance behavior. However, the fact that reports are also correlated with other measures of absence speaks against this possibility.

Next, in the second to fourth rows of Table 3, I examine whether the quality of the reports is affected by the payment of monetary rewards or the expected use of the reports. I do not find evidence in favor of either of these hypotheses. Indeed, none of the interactions of the three measures of absence with the treatment dummies (or their interaction) are statistically significant. This suggests that although financial rewards could in principle incentivize participants to report absent even colleagues who were actually present (especially because the rewards are not conditional on the reports' accuracy), they do not lead to any significant increase in the number of false reports. Similarly, while in the *possible punishment* condition participants could misreport a colleague with whom they have a private dispute in order to cause harm, the estimates do not provide any evidence of this type of malicious accusation, even when the reports are expected to be consequential. Finally, even the combination of monetary incentives and consequential reports is not associated with changes in the quality of the reports.

## 5 Discussion of Mechanisms

In this section, I discuss the possible reasons motivating participants to report their colleagues and to react negatively to monetary rewards. I begin by describing the self-reported attitudes of MoE employees toward denouncing their colleagues' absence, both in the presence and absence of monetary rewards. Next, I use the combination of experimental results and survey responses to

discuss the relative importance of three factors through which moral concerns can reverse the effect of incentives: aversion to being paid for harming others, crowding out of intrinsic motivation, and image concerns. Finally, I rule out other alternative mechanisms—unrelated to morality—through which incentives can backfire.

## 5.1  Attitudes toward Reporting

**Unincentivized Reporting.**  In the additional in-person surveys conducted after the experiment, employees were asked their opinion on whether or not reporting their colleagues was the right thing to do.[29] Among those who answered this question (18.2% refused to do so), 58.3% believed that reporting absence was the right thing to do, while 41.7% thought it was not. The high number of employees who believe that reporting is wrong or refuse to answer the question indicates that denouncing absence is, indeed, perceived as a morally controversial behavior.

The employees were also asked to explain their opinion. The most cited reasons for reporting had to do with the employees being invested in their students, their school, and their country (42.7%); wanting to counter absenteeism and corruption (23.9%); or feeling that reporting was a matter of personal responsibility (20.0%). The majority of employees who thought reporting was wrong answered that reporting was not their responsibility or that they did not have the authority to do so (57.7%); many refused to motivate their answer (19.8%); some were tolerant of absence or sympathetic toward their colleagues (8.8%); and others said that absenteeism was not a major problem at their school (6.3%).

Taken together, these survey responses suggest that the employees internalize the MoE's objective of fighting absence. These self-reported attitudes are consistent with the patterns of experimental results described above: first, reporting is higher when it is expected to be consequential; and second, reports are accurate overall.

**Incentivized Reporting.**  Next, the respondents were asked their opinion on incentivized denunciations. Even more employees refused to answer this question (25.7%), and the majoritarian opinion flipped from reporting to not reporting. Among the 69.0% of employees who thought reporting when offered a reward was not right, 37.3% believed that it was immoral to do so, 18.3% said that they would only report if the reward was not there, and 15.4% said that reporting was not their responsibility or that they did not have the authority to do so. Again, many employees did not provide an answer (11.3%). The motivations of the 31.0% of employees who still believed that reporting was right are similar to those reported above: being invested in students, school, and country (43.9%); feeling a sense of personal responsibility (16.6%); and wanting to counteract absenteeism (12.0%). Importantly, even among these employees, some specified that they would report even in the absence of a reward, and that being motivated solely by the reward would be immoral (5.9%).

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[29]The survey was administered to all employees who were present at the time of the visit, independently of participation in the experiment. The text of the questions asked during this additional survey is reported in Appendix B.VII.

Considering these self-reported attitudes, a moral aversion toward reporting in exchange for financial incentives seems to be at the root of the backfiring of rewards. The monetary transaction shifts the framing of the situation from purely moral to financial, making it unjustifiable on ethical grounds. Once again, this is consistent with the patterns of experimental results: Monetary rewards backfire only when the reports are consequential, making the decision morally charged, and not when the decision is morally neutral.

## 5.2   Why do Incentives Backfire?

The existing literature identifies two fundamental ways in which incentives can backfire in domains involving morality. First, incentives can crowd out individuals' intrinsic motivation to behave prosocially (Frey and Jegen, 2001). Second, individuals can gain less reputational benefits for behaving prosocially when they are paid for doing so (Bénabou and Tirole, 2006). Given the prosocial component of denouncing wrongdoing, these two explanations can be relevant in this context. However, the antisocial element of reporting peers introduces a third reason why incentives can backfire: an aversion to being paid for harming others. The main experimental results linking morality and consequentiality do not rule out any of these explanations. However, the survey responses help narrow the interpretation of the experimental results toward the most significant channels. I next provide a discussion of their relative importance.

**Moral Aversion to Incentivized Reporting.**   The survey evidence is consistent with the idea that individuals find it morally unacceptable to be paid for an action that harms their peers and that this is a primary reason why incentives backfire in this context. Indeed, in the in-person surveys conducted after the experiment, a large portion of employees explicitly voice their moral disapproval of the incentives. Moreover, the relatively high non-response rate to the question about rewards suggests that some individuals might perceive incentivized reporting as a "taboo tradeoff" that should not even be contemplated (Bénabou and Tirole, 2011).

To explore this further, I examine whether reporting behavior in the experiment is related to the local attitudes of employees toward denouncing in exchange for money. For each participant of the experiment, I create a dummy indicating whether a majority of their school colleagues consider reporting the wrong thing to do in the presence of rewards. In columns (1) and (2) of Table 4, I split the sample based on whether a minority or majority of colleagues oppose incentivized reporting. Column (3) displays estimates of the differences between the two subsamples.[30] I find that local opinions do not matter for inconsequential reporting (Panel A), but they are strongly predictive of the reporting behavior in the *possible punishment* condition (Panel B). Monetary incentives do not backfire when negative opinions about incentivized reporting are minoritarian: Reporting rates are 15.17% and 15.95% respectively in the *no reward* and *monetary reward* conditions. When the majority of employees express negative attitudes toward the idea of receiving a reward for reporting, the reporting rate remains similar in the *no reward* condition (16.30%). However, the reporting

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[30]Appendix Figure A.1 displays the same results in graphic format.

rate with monetary rewards is 7.43%: This corresponds to a significant drop of 8.88 percentage points (s.e. = 2.39) with respect to the *no reward* condition. The difference-in-differences of the effect of monetary rewards between the two subsamples is also significant (9.66 percentage points, s.e. = 2.15), as is the difference in the reporting rates in the *monetary reward* condition (9.52 percentage points, s.e. = 2.79). In other words, incentives backfire only in those schools where the majority of employees oppose incentivized reporting, but not where this opposition is minoritarian.

Thus, a primary interpretation of the detrimental effect of monetary rewards on reporting is that there exists a moral norm against harming peers in exchange for money—even if the action helps someone else—and that at least some agents experience a utility cost from violating such a norm. Indeed, I speculate that the heterogeneity results suggest that there might even be a social norm sustaining such behavior, or that personal moral norms are shared locally.

While the aversion-to-rewards mechanism seems to have a primary role in the backfiring of incentives, I do not exclude that other morality-based explanations can compound its effect.

**Crowding out of Intrinsic Motivation.**  First, incentives might crowd out employees' intrinsic motivation to report absence. Leaving apart for a moment the antisocial aspect of denouncing peers, the decision to report wrongdoing can be considered as a purely prosocial action because it helps the victims of the misbehavior. The experimental results can then be interpreted through the lenses of motivation crowding theory in the following way. In the *no reward, possible punishment* condition, participants report because they believe that working to reduce absence is their civic duty, and they get intrinsic utility for doing so. In the *monetary reward, possible punishment* condition, however, they get less civic-duty utility from reporting absence, because the compensation crowds out their intrinsic motivation: Rewards backfire because the relative price effect is dominated by the motivational crowding out. Finally, in the *no punishment* group, incentives do not backfire, because there is no intrinsic motivation to crowd out: The sense of civic duty pertains only to consequential reporting.

Even though the responses to the attitudinal survey are more supportive of the aversion-to-rewards mechanism, I do not rule out that incentives might also crowd out participants' motivation to report as a civic duty. Nevertheless, I consider the antisocial aspect of harming peers a fundamental determinant of the reporting behavior, and of the efficacy of rewards for morally controversial decisions.

**Reputational Concerns.**  Leaving apart once again the antisocial aspect of reporting, one can also interpret the experimental findings through the lenses of the Bénabou and Tirole (2006) model of prosocial behavior. Absent concerns for harming others, reporting is a clearly moral action because it involves a personal cost for the denouncer (the risk of being subject to retaliation) and it benefits others (the pupils) by reducing absence. By reporting absence, individuals might then signal, either to themselves or to others, that they are prosocial. Following this logic, without rewards, reporting is higher in the *possible punishment* than in the *no punishment* condition because

there is no reputation to gain in providing inconsequential reports. Relatedly, in the *no punishment* condition, incentives do not backfire because there is no reputation to lose. However, in the *possible punishment* group, rewards backfire because they dilute the signal of prosociality by providing an over-justification effect for acting morally.

This is certainly a possibility; however, two points are worthy of consideration. First, given the confidentiality of the reports in this context, social-image concerns are probably dominated by self-image motives (Falk, 2017). Second, the idea that reporting peers is undoubtedly prosocial rather than morally controversial is not supported by the survey evidence. This suggests that the relevance of this type of reputational considerations might be quite limited in this setting.

Nevertheless, I note that the antisocial aspect of denunciations and the aversion to rewards introduce another type of reputational concerns: Instead of prosociality, individuals might want to signal that they are averse to accepting money for harming their peers. Some individuals in the population might experience disutility from being compensated for an action that harms others. Other individuals, who do not have these moral preferences, would gain image utility if they were perceived as having them. In this case, the role of monetary incentives is not the same as in Bénabou and Tirole (2006): Rather than diluting the morality signal, incentives here define the action upon which morality is judged. In this sense, this specific version of the reputational-concerns mechanism does not compete with the aversion-to-rewards one, but is instead built upon it.

### 5.2.1 Ruling out Alternative Explanations

There are a few channels aside from moral motivations that could explain the negative effect of financial incentives. In this section, I discuss how one can rule out these possible mechanisms that are unrelated to moral concerns around reports' consequentiality.

**Size of the Incentives.** The design of the experiment and its results allow me to rule out backfiring explanations related to the size of the incentives. In different contexts, it has been shown that incentives can backfire when they are too small or large. I can rule out this channel since incentives of the same size do not backfire in the *no punishment* condition. This does not imply, however, that there does not exist a reward amount that would result in a positive net effect on reporting.[31]

**Deontological Aversion to Rewards.** Using the same arguments, it is also possible to rule out an explanation based on a deontological aversion to accepting money for reporting, independently of the consequentiality of the denunciations. If this were the case, I would also observe a negative

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[31]My interpretation is that in the *no punishment* condition, I identify the price effect of incentives, which is the effect in the absence of moral concerns. In the *possible punishment* condition, the detrimental effect of incentives due to moral concerns dominates and completely crowds out this price effect. However, there could exist a reward amount for which the price effect would be large enough to be only partially crowded out by moral concerns, resulting in an overall positive effect. Investigating how the size of incentives affects morally controversial decision-making could be an interesting avenue for future research.

effect of monetary rewards in the *no punishment* condition, rather than only in the *possible punishment* one.

Next, I examine whether incentives backfire because they signal some information about important attributes of the context.

**Rewards as Signals for the Riskiness of Reporting.**   Another important determinant of the decision to report is likely the amount of risk of possible retaliation the could-be denouncer perceives. In what follows, I investigate how this perception influences reporting decisions. In doing so, I also test whether monetary rewards have any effect on the perceived riskiness of denouncing, thus providing a possible mechanism for their backfiring.

Right before being asked for feedback on the first colleague on the list, participants were asked whether they thought they might face problems if they reported a case of absenteeism. I used their responses to define a dummy variable equal to one for respondents who reported believing that they might face problems. On average, 75.34% of the participants responded affirmatively, even though they were assured by the surveyor that their identity would remain confidential and that they should not expect any issues no matter their response.[32]

In column (1) of Table 5, I present estimates of the share of respondents who expressed a belief that reporting was risky, by treatment condition. I also report estimates of the differences in beliefs across treatment conditions from the regression specification displayed in equation (1), along with standard errors and p-values for tests of equality. I begin with examining, in Panel A, differences in reporting by reward condition for the *no punishment* group. The share fearing retaliation is 73.33% in the absence of reward and 78.17% when reporting is incentivized. The difference of 4.84 percentage points (s.e. = 3.28) is not statistically significant. In other words, in the *no punishment* condition, respondents were more likely to denounce when paid for their reports, even though the financial incentives also made them marginally more concerned about the risk of reporting.

Next, in Panel B, I present the same estimates for the *possible punishment* condition. In this case, there is only a 1.40 percentage-point difference (s.e. = 2.35) between the 74.45% share of respondents afraid of retaliation in the *no reward* group and the 75.85% share in the *monetary reward* group. The difference is not only statistically insignificant, but also small relative to the 4.76 percentage-point decrease in the share of respondents who report recent absences.

Even though column (1) documents that the average perceptions are fairly similar across groups, in the remaining columns of Table 5, I investigate how the treatment effects vary depending on the perceived riskiness of reporting. To do so, I split the respondents into subsamples based on their

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[32]This prevalent fear of retaliation is not necessarily an indication of participants not believing the confidentiality guarantee. Participants may have thought, for example, that formal confidentiality would offer little actual anonymity and protection from retaliation if only a few employees were capable of making a denunciation. When only a small set of people has knowledge about a peer's misbehavior, it is easier for the misbehaving peer to identify the denouncer and retaliate against them (Chassang and Padró i Miquel, 2018). Following this argument, employees in small schools should be more concerned about the riskiness of reporting than employees in larger schools, where plausible deniability is higher. However, I do not find support for this hypothesis in the data, possibly because even in large schools, employees might still only have daily interactions with a small group of colleagues.

perceptions of risk. In column (2), I restrict the sample to respondents who reported believing that denouncing absence was not risky, while in column (3), I study respondents who perceived reporting as risky. For each column, I replicate the analysis presented in the first column of Table 2 on the relevant subsample, and I display unconditional reporting rates of absence along with estimates of differences in reporting by treatment status. In the subsample of respondents who are not concerned about retaliation, monetary rewards increase reporting by 10.49 percentage points (s.e. = 5.64) when the reports are inconsequential, but decrease reporting by 4.89 percentage points (s.e. = 4.32) when reports are expected to have possible consequences. This is the same pattern of results as for the whole sample. While the latter estimate is not statistically significant, the p-value for the difference-in-differences estimate of 15.38 percentage points (s.e. = 7.10) is 0.03. Similarly, even among the respondents who believe denouncing is risky, financial incentives increase inconsequential reporting by 4.67 percentage points (s.e. = 2.30) but decrease potentially consequential reporting by 4.55 percentage points (s.e. = 1.93). In this case, both the two first differences and the difference-in-differences estimate of 9.22 percentage points (s.e. = 3.00) are statistically significant at the 5-percent level. Column (4) reports estimates of the differences in effects between the two subsamples and documents that the effects are not significantly heterogeneous.

This is inconsistent with the perceived risk of denouncing being the main driver of the results presented in the previous section: Incentives do not backfire because they signal information about the riskiness of the action.[33] It is important to note, however, that perceptions about the likelihood of facing retaliation costs are strongly associated with lower reporting. As shown in column (4) of Table 5, within each treatment condition, the share of respondents reporting any wrongdoing is always lower among those who think reporting is risky than among those who believe it is not, with differences in reporting ranging between a minimum of 4.64 percentage points (s.e. = 3.40) for the *no punishment, no reward* condition to a maximum of 10.46 percentage points (s.e. = 5.03) for the *no punishment, monetary reward* condition. This has implications for the need to design policies aimed at guaranteeing strong protection for denouncers.

**Rewards as Signals for the Expected Use of the Reports.** A second element that the rewards might signal is that the government is particularly committed to investigating absence. However, this explanation is inconsistent with the pattern of experimental results, since the experimental variation in terms of consequentiality unequivocally indicates that there is more denouncing when reports are expected to be followed up on by the government, and the rewards decrease consequential reporting instead.

**Rewards as Signals for the Prevailing Behavior.** Finally, the presence of rewards designed to encourage reporting might signal that reporting is otherwise low. Individuals might reduce their

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[33]These results also rule out the possibility of participants believing that the rewards introduce an element of risk, for example, by increasing the visibility of denunciations through the text message that is sent to deliver the mobile phone top-up. As explained in Footnote 20, the top-up notification sent to deliver the reward is identical to those regularly sent by mobile network operators, and thus do not constitute an observable paper trail linking participants to their denunciations.

reporting rates because they surmise that other individuals are not reporting, or that there exists a social norm against denouncing (independent of whether or not reporting is incentivized). Once again, this explanation is ruled out by the fact that incentives backfire only when the reports are consequential, but not when they are inconsequential.

# 6    Conclusions

In this paper, I provide field-experimental evidence of the detrimental effect of monetary rewards on the willingness of individuals to report their peers' wrongdoing to an authority who can impose a punishment on the misbehaving peers.

The interplay of financial and moral considerations in economic decisions is context-dependent. Therefore, studying how financial incentives operate in other settings (including those in which rewards are designed to incentivize truth-telling rather than overall reporting) is an important avenue for future research. Another interesting area of research is whether different types of incentives, such as appeals to morality (Dal Bó and Dal Bó, 2014), might work better for encouraging behaviors in domains where decisions are morally controversial.[34]

The paper also documents how the fear of possible retaliation reduces the willingness of employees to report any wrongdoing. It is therefore important for authorities eliciting information from peers to offer adequate protection to whistleblowers, and for researchers to test the efficacy of alternative mechanisms designed to offer such protection (Chassang and Padró i Miquel, 2018).

Finally, from a policy perspective, a government interested in routinely using peer reports as a personnel policy tool (either as a primary source of information or as a backup instrument in case other monitoring technologies fail) should take into consideration both possible general equilibrium effects and potential drawback. First, it is possible for employees to learn to collude; for example, they may decide not to report a case of absence in exchange for a bribe from the absent colleague. The design of reporting schemes should then incorporate mechanisms to make this type of collusion harder (Ortner and Chassang, 2018).[35] Second, while possibly promoting a culture of integrity and accountability, the introduction of a monitoring system could also create an environment of distrust between colleagues, which could be detrimental to the overall functioning of the schools.[36] While a full cost-benefit analysis of this type of reform is beyond the scope of this paper, investigating the possible negative impact of encouraging reporting, especially in collaborative environments like schools, should be a priority for interested policymakers.

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[34]Private appeals, rather than social-image incentives, might be more suitable in settings where protecting the identity of agents and keeping their behavior confidential is important.

[35]The mechanism proposed by Chassang and Padró i Miquel (2018) to garble the information provided by whistleblowers to protect them from retaliation could also make collusion between could-be whistleblowers and corrupt employees harder.

[36]This concern could actually lower the willingness of employees to report their peers' wrongdoing (Muehlheusser and Roider, 2008). Indeed, in the attitudinal survey, a small group of respondents said that they would prefer not reporting any colleague, because reporting would create distrust between coworkers.

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# Figures and Tables

Figure 1: Reporting Rates by Treatment Condition
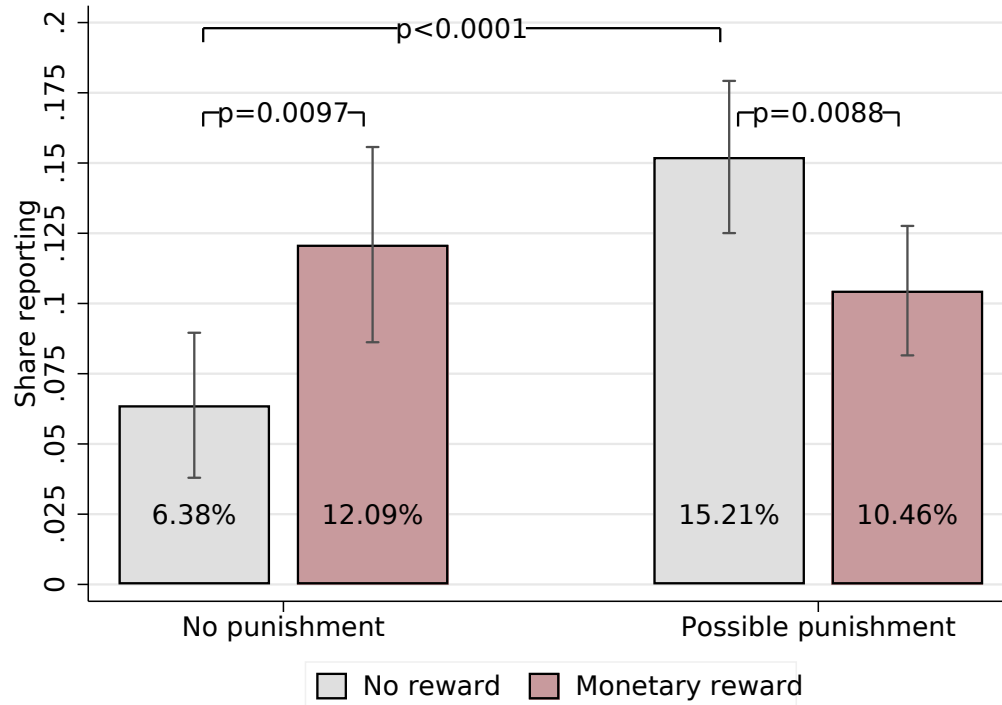


*Notes*: This figure presents the means and 95 percent confidence intervals of the unconditional reporting rates of recent absence by treatment condition. The two bars on the left display the share of respondents who reported at least one colleague absent in the week preceding the experiment for individuals in the *no reward* and *monetary reward* treatments in the *no punishment* condition (respectively N=345 and N=339). The two right bars display the same information for respondents in the *possible punishment* condition (respectively N=677 and N=679). The confidence intervals are based on robust standard errors. Top horizontal bars show p-values for t-tests of equality of means between different experimental conditions.

Table 1: Summary Statistics and Covariates Balance

| | Full sample | Treatment | | | | p-value |
| | | No Punishment | | Possible Punishment | | |
| | | No reward | Monetary reward | No reward | Monetary reward | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Female | 0.2000 | 0.2116 | 0.2242 | 0.1920 | 0.1900 | 0.5461 |
| | (0.4001) | [0.0220] | [0.0227] | [0.0152] | [0.0151] | |
| Age | 40.5750 | 40.3913 | 40.4130 | 41.2245 | 40.1016 | 0.4765 |
| | (13.6450) | [0.7466] | [0.7429] | [0.5172] | [0.5260] | |
| Years of education | 13.7088 | 13.9217 | 13.7581 | 13.7179 | 13.5670 | 0.6697 |
| | (4.4472) | [0.2274] | [0.2436] | [0.1679] | [0.1772] | |
| *Ethnic group* | | | | | | |
| Pashtun | 0.7627 | 0.7333 | 0.7817 | 0.7755 | 0.7555 | 0.3842 |
| | (0.4255) | [0.0238] | [0.0225] | [0.0161] | [0.0165] | |
| Tajik | 0.2005 | 0.2232 | 0.1799 | 0.1876 | 0.2121 | 0.3533 |
| | (0.4005) | [0.0224] | [0.0209] | [0.0150] | [0.0157] | |
| Other | 0.0382 | 0.0464 | 0.0413 | 0.0369 | 0.0339 | 0.7986 |
| | (0.1918) | [0.0113] | [0.0108] | [0.0073] | [0.0069] | |
| Salary (AFN) | 7870.0990 | 7856.1855 | 7932.1386 | 7871.2614 | 7845.0353 | 0.9438 |
| | (2050.5946) | [106.6005] | [119.9630] | [76.1511] | [79.6468] | |
| *Rank* | | | | | | |
| Rank > 5 | 0.3167 | 0.3188 | 0.3363 | 0.2954 | 0.3270 | 0.4975 |
| | (0.4653) | [0.0251] | [0.0257] | [0.0176] | [0.0180] | |
| Rank = 5 | 0.4779 | 0.4986 | 0.4484 | 0.4786 | 0.4816 | 0.6111 |
| | (0.4996) | [0.0269] | [0.0270] | [0.0192] | [0.0192] | |
| Rank < 5 | 0.2054 | 0.1826 | 0.2153 | 0.2260 | 0.1915 | 0.2762 |
| | (0.4041) | [0.0208] | [0.0223] | [0.0161] | [0.0151] | |
| *Position* | | | | | | |
| Principal | 0.0328 | 0.0290 | 0.0472 | 0.0295 | 0.0309 | 0.5690 |
| | (0.1783) | [0.0090] | [0.0115] | [0.0065] | [0.0067] | |
| Admin staff | 0.0686 | 0.0667 | 0.0560 | 0.0650 | 0.0795 | 0.5221 |
| | (0.2529) | [0.0134] | [0.0125] | [0.0095] | [0.0104] | |
| Head teacher | 0.0627 | 0.0870 | 0.0678 | 0.0635 | 0.0471 | 0.1062 |
| | (0.2426) | [0.0152] | [0.0137] | [0.0094] | [0.0081] | |
| Teacher | 0.7118 | 0.7072 | 0.7109 | 0.7149 | 0.7113 | 0.9953 |
| | (0.4531) | [0.0245] | [0.0246] | [0.0174] | [0.0174] | |
| Other staff | 0.1240 | 0.1101 | 0.1180 | 0.1270 | 0.1311 | 0.7669 |
| | (0.3297) | [0.0169] | [0.0175] | [0.0128] | [0.0130] | |
| Times present | 0.7238 | 0.7425 | 0.7188 | 0.7292 | 0.7113 | 0.2851 |
| | (0.2608) | [0.0138] | [0.0143] | [0.0100] | [0.0100] | |
| Observations | 2040 | 345 | 339 | 677 | 679 | |

*Notes:* This table reports summary statistics for the sample of 2,040 respondents and presents tests of random assignment to the treatment conditions. The unit of observation is a respondent. Column (1) reports the mean level of each variable, with standard deviations in parentheses, for the full sample. Columns (2) to (5) report the mean level of each variable, with robust standard errors in brackets, for each experimental condition. Column (6) reports, for each variable, the p-value of a joint F-test that means are the same in all the experimental conditions. The *possible punishment, no reward* condition pools together the *possible mild punishment, no reward* and *possible severe punishment, no reward* conditions. Similarly, the *possible punishment, monetary reward* condition pools together the *possible mild punishment, monetary reward* and *possible severe punishment, monetary reward* conditions.

Table 2: Treatment Effects: Reporting Recent Absence

| | Dummy: respondent reported ≥ 1 absent colleague | Dummy: respondent reported specific colleague as absent | Dummy: respondent reported specific colleague as present | Dummy: respondent reported specific colleague as other |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | *Panel A. No Punishment* | | | |
| No reward | 0.0638 | 0.0133 | 0.7991 | 0.1875 |
| | [0.0132] | [0.0034] | [0.0132] | [0.0129] |
| | | | | |
| Monetary reward | 0.1209 | 0.0218 | 0.7917 | 0.1864 |
| | [0.0177] | [0.0041] | [0.0138] | [0.0135] |
| | | | | |
| Difference | 0.0572*** | 0.0085 | -0.0074 | -0.0011 |
| (monetary reward - no reward) | [0.0221] | [0.0053] | [0.0190] | [0.0186] |
| | (0.0099) | (0.1099) | (0.6982) | (0.9527) |
| | {0.0117} | {0.0977} | {0.6961} | {0.9624} |
| | *Panel B. Possible Punishment* | | | |
| No reward | 0.1521 | 0.0266 | 0.7771 | 0.1963 |
| | [0.0138] | [0.0031] | [0.0099] | [0.0095] |
| | | | | |
| Monetary reward | 0.1046 | 0.0144 | 0.8178 | 0.1677 |
| | [0.0118] | [0.0018] | [0.0089] | [0.0088] |
| | | | | |
| Difference | -0.0476*** | -0.0122*** | 0.0407*** | -0.0286** |
| (monetary reward - no reward) | [0.0181] | [0.0036] | [0.0134] | [0.0130] |
| | (0.0088) | (0.0008) | (0.0024) | (0.0277) |
| | {0.0089} | {0.0004} | {0.0007} | {0.0175} |
| | *Panel C. Differences (possible punishment - no punishment)* | | | |
| No reward | 0.0884*** | 0.0133*** | -0.0220 | 0.0088 |
| | [0.0191] | [0.0046] | [0.0165] | [0.0160] |
| | (<0.0001) | (0.0039) | (0.1820) | (0.5840) |
| | {0.0001} | {0.0125} | {0.2321} | {0.5962} |
| | | | | |
| Monetary reward | -0.0164 | -0.0074* | 0.0261 | -0.0187 |
| | [0.0213] | [0.0045] | [0.0164] | [0.0161] |
| | (0.4415) | (0.0993) | (0.1123) | (0.2453) |
| | {0.4421} | {0.0572} | {0.0821} | {0.2131} |
| | | | | |
| Difference | -0.1048*** | -0.0207*** | 0.0481** | -0.0275 |
| (monetary reward - no reward) | [0.0286] | [0.0064] | [0.0233] | [0.0227] |
| | (0.0003) | (0.0013) | (0.0387) | (0.2263) |
| | {0.0005} | {0.0014} | {0.0218} | {0.1705} |
| Observations | 2,040 | 20,400 | 20,400 | 20,400 |
| $R^2$ | 0.0092 | 0.0017 | 0.0017 | 0.0009 |

*Notes:* This table presents unconditional reporting rates of recent absence and differences in reporting rates across treatment conditions. The unit of observation is a respondent in column (1) and a respondent×colleague in columns (2) to (4) (each respondent was asked about the absence of ten colleagues). In column (1), the outcome variable is a dummy equal to one if the respondent reported at least one colleague absent in the week preceding the experiment and zero otherwise. In column (2), the outcome variable is a dummy equal to one if the respondent reported a specific colleague absent in the week preceding the experiment and zero otherwise. In column (3), the outcome variable is a dummy equal to one if the respondent reported the colleague present. In column (4), the outcome variable is a dummy equal to one for the residual case in which the respondent reported something else regarding the colleague ("I don't know (about the presence/absence of the colleague)," "I don't know the person," or "other"). Panel A presents unconditional reporting rates in the *no punishment* condition for both the *no reward* and *monetary reward* conditions, and differences in reporting rates depending on the reward condition. Panel B presents the same information for the *possible punishment* conditions. Panel C presents differences for each cell between the *no punishment* and *possible punishment* conditions. The differences in reporting rates are calculated from OLS regressions of the dependent variable on a dummy for the *monetary reward* condition, a dummy for the *possible punishment* condition, and the interaction of the two. Robust standard errors for column (1) and standard errors clustered at the respondent level for columns (2) to (4) are presented in brackets. Sampling-based p-values from standard errors are presented in parentheses. Randomization-based Fisher exact p-values for the sharp null hypothesis of no effect from permutation tests with 10,000 repetitions are presented in curly parentheses. For differences in reporting rates, we denote (using the sampling-based p-values): * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Heterogeneous Treatment Effects: Accuracy of Reports

| | Dependent Variable | | | | | |
|---|---|---|---|---|---|---|
| | Dummy: respondent reported specific colleague as absent | | | Dummy: respondent reported specific colleague as present | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Absence Index | 0.0140** | 0.0044 | 0.4348** | -0.1275*** | -0.0523*** | -0.4795* |
| | [0.0057] | [0.0048] | [0.1930] | [0.0203] | [0.0187] | [0.2522] |
| Absence Index | | | | | | |
| × Monetary reward | 0.0047 | 0.0083 | 0.1130 | -0.0105 | -0.0119 | 0.1332 |
| | [0.0083] | [0.0093] | [0.2793] | [0.0309] | [0.0263] | [0.3292] |
| × Possible punishment | -0.0057 | 0.0045 | -0.1090 | -0.0316 | -0.0176 | 0.2763 |
| | [0.0079] | [0.0068] | [0.2119] | [0.0260] | [0.0231] | [0.2893] |
| × Monetary reward | -0.0054 | -0.0075 | -0.2027 | 0.0089 | 0.0065 | -0.0510 |
| × Possible punishment | [0.0108] | [0.0111] | [0.3011] | [0.0378] | [0.0323] | [0.3781] |
| | | | | | | |
| Monetary reward | 0.0056 | 0.0060 | 0.0055 | -0.0006 | -0.0043 | -0.0080 |
| | [0.0048] | [0.0049] | [0.0049] | [0.0262] | [0.0208] | [0.0201] |
| Possible punishment | 0.0168*** | 0.0118*** | 0.0148*** | -0.0020 | -0.0166 | -0.0260 |
| | [0.0051] | [0.0046] | [0.0041] | [0.0222] | [0.0181] | [0.0174] |
| Monetary reward | -0.0173** | -0.0184*** | -0.0158*** | 0.0430 | 0.0465* | 0.0467* |
| × Possible punishment | [0.0071] | [0.0061] | [0.0059] | [0.0314] | [0.0252] | [0.0245] |
| | | | | | | |
| Constant | 0.0047* | 0.0119*** | 0.0054* | 0.8779*** | 0.8159*** | 0.8071*** |
| | [0.0027] | [0.0033] | [0.0030] | [0.0180] | [0.0145] | [0.0140] |
| | | | | | | |
| Absence Index Measure | Unannounced Visits | Administrative Records | Other Respondents | Unannounced Visits | Administrative Records | Other Respondents |
| Observations | 20,400 | 20,400 | 20,288 | 20,400 | 20,400 | 20,288 |
| $R^2$ | 0.0025 | 0.0027 | 0.0169 | 0.0184 | 0.0079 | 0.0027 |

*Notes:* This table presents heterogeneity in reporting rates and heterogeneous treatment effects by measures of absence of the reportable colleagues. The unit of observation is respondent×colleague (each respondent was asked about the absence of ten colleagues). In columns (1) to (3), the outcome variable is a dummy equal to one if the respondent reported a specific colleague absent in the week preceding the experiment and zero otherwise. In columns (4) to (6), the outcome variable is a dummy equal to one if the respondent reported the colleague present. In columns (1) and (4), the absence index is a variable equal to the proportion of times the reportable colleague was absent during the (up to) three unannounced audit visits conducted in the schools. In columns (2) and (5), the absence index is a dummy equal to one if the colleague was absent at least once during the month preceding the experiment according to the administrative records. In columns (3) and (6), the absence index is a variable equal to the proportion of other respondents who reported the colleague absent in the week preceding the experiment. Each column reports the results of an OLS regression of the dependent variable on the absence index, treatment-condition dummies (a dummy for the *monetary reward* condition, a dummy for the *possible punishment* condition, and the interaction of the two) and the interaction of the absence index with the treatment-condition dummies. Standard errors clustered at the respondent level are presented in brackets. We denote: * significant at 10%; ** significant at 5%; *** significant at 1%.

## Table 4: Heterogeneous Treatment Effects: Attitudes toward Reporting for Rewards

| | Dependent Variable | | |
| --- | --- | --- | --- |
| | Dummy: respondent reported $\geq 1$ absent colleague | | |
| | Colleagues Consider Wrong to Report for Rewards | | Difference |
| | Minority | Majority | (majority - minority) |
| | (1) | (2) | (3) |
| | Panel A. No Punishment | | |
| No reward | 0.0410 | 0.0756 | 0.0346 |
| | [0.0180] | [0.0202] | [0.0271] |
| | | | (0.2024) |
| Monetary reward | 0.1111 | 0.1264 | 0.0153 |
| | [0.0291] | [0.0252] | [0.0386] |
| | | | (0.6916) |
| Difference | 0.0701** | 0.0509 | -0.0193 |
| (monetary reward - no reward) | [0.0343] | [0.0324] | [0.0343] |
| | (0.0420) | (0.1169) | (0.6828) |
| | Panel B. Possible Punishment | | |
| No reward | 0.1517 | 0.1630 | 0.0114 |
| | [0.0248] | [0.0193] | [0.0314] |
| | | | (0.7168) |
| Monetary reward | 0.1595 | 0.0743 | -0.0852*** |
| | [0.0241] | [0.0140] | [0.0279] |
| | | | (0.0023) |
| Difference | 0.0078 | -0.0888*** | -0.0966** |
| (monetary reward - no reward) | [0.0345] | [0.0239] | [0.0345] |
| | (0.8209) | (0.0002) | (0.0215) |
| | Panel C. Differences (possible punishment - no punishment) | | |
| No reward | 0.1107*** | 0.0875*** | -0.0232 |
| | [0.0306] | [0.0279] | [0.0306] |
| | (0.0003) | (0.0018) | (0.5755) |
| Monetary reward | 0.0484 | -0.0522* | -0.1005** |
| | [0.0378] | [0.0289] | [0.0378] |
| | (0.2017) | (0.0716) | (0.0349) |
| Difference | -0.0623 | -0.1396*** | -0.0773 |
| (monetary reward - no reward) | [0.0487] | [0.0402] | [0.0631] |
| | (0.2008) | (0.0005) | (0.2206) |
| Observations | 682 | 1,064 | 1,746 |
| $R^2$ | 0.0172 | 0.0160 | 0.0169 |

*Notes:* This table presents heterogeneity in reporting rates and heterogeneous treatment effects on reporting by whether a majority of the colleagues in the school believe that reporting absence for rewards is wrong. The unit of observation is a respondent. The outcome variable is a dummy equal to one if the respondent reported at least one colleague absent in the week preceding the experiment and zero otherwise. Column (1) restricts the sample to respondents for whom only a minority of colleagues believe that reporting for rewards is wrong. Column (2) restricts the sample to respondents whose majority of colleagues perceived reporting for rewards as wrong. Columns (3) report results for the sample of 1,746 respondents for which the opinion of colleagues is available. In columns (1) and (2), Panel A presents the unconditional mean of the outcome variable in the *no punishment* condition for both the *no reward* and *monetary reward* conditions, and differences in means depending on the reward condition; Panel B presents the same information for the *possible punishment* conditions; and Panel C presents differences for each cell between the *no punishment* and *possible punishment* conditions. Column (3) presents differences for each cell between column (1) and column (2) depending on the opinion of the colleagues. In columns (1) and (2), the differences are calculated from OLS regressions of the dependent variable on treatment-condition dummies (a dummy for the *monetary reward* condition, a dummy for the *possible punishment* condition, and the interaction of the two). In column (3), the differences are calculated from OLS regressions of the dependent variable on a dummy for the opinion of the majority of the colleagues on reporting for rewards, treatment-condition dummies, and the interaction of the opinion dummy with the treatment-condition dummies. Robust standard errors are presented in brackets. Sampling-based p-values from standard errors are presented in parentheses. For differences, we denote: * significant at 10%; ** significant at 5%; *** significant at 1%.

## Table 5: Heterogeneous Treatment Effects: Perceived Riskiness of Reporting

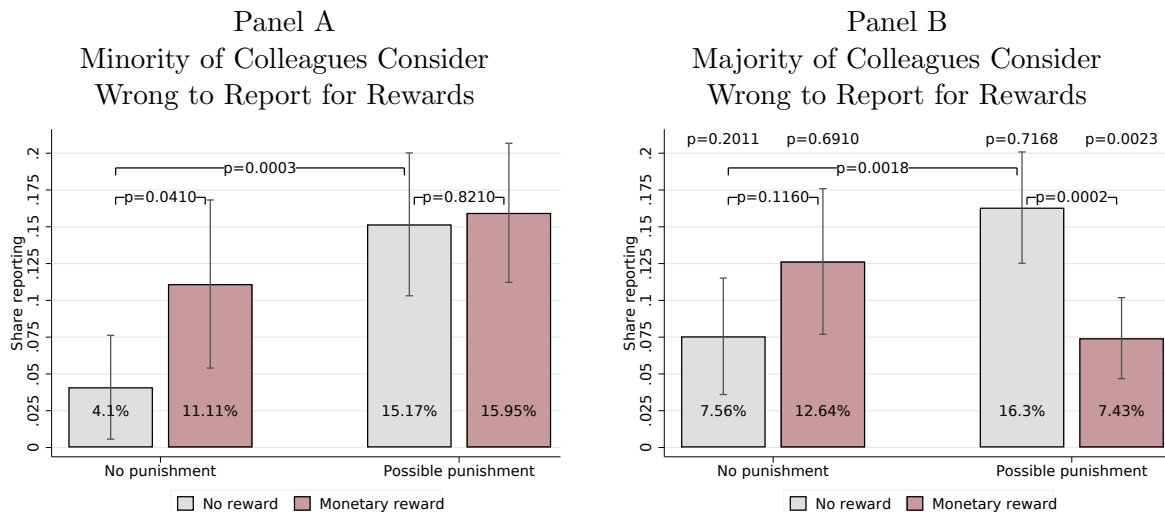| | Dummy: respondent believed reporting was risky | Dummy: respondent reported $\geq 1$ absent colleague | | |
| | | Perceived Riskiness | | Difference |
| | | Not risky | Risky | (risky - not risky) |
| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | | **Panel A. No Punishment** | | |
| No reward | 0.7333 | 0.0978 | 0.0514 | -0.0464 |
| | [0.0238] | [0.0311] | [0.0139] | [0.0340] |
| | | | | (0.1733) |
| Monetary reward | 0.7817 | 0.2027 | 0.0981 | -0.1046** |
| | [0.0225] | [0.0469] | [0.0183] | [0.0503] |
| | | | | (0.0384) |
| Difference | 0.0484 | 0.1049* | 0.0467** | -0.0581 |
| (monetary reward - no reward) | [0.0328] | [0.0564] | [0.0230] | [0.0562] |
| | (0.1402) | (0.0648) | (0.0426) | (0.3389) |
| | | | | |
| | | **Panel B. Possible Punishment** | | |
| No reward | 0.7445 | 0.2197 | 0.1290 | -0.0907*** |
| | [0.0168] | [0.0316] | [0.0149] | [0.0349] |
| | | | | (0.0095) |
| Monetary reward | 0.7585 | 0.1707 | 0.0835 | -0.0872*** |
| | [0.0164] | [0.0295] | [0.0122] | [0.0319] |
| | | | | (0.0063) |
| Difference | 0.0140 | -0.0489 | -0.0455** | 0.0034 |
| (monetary reward - no reward) | [0.0235] | [0.0432] | [0.0193] | [0.0431] |
| | (0.5510) | (0.2581) | (0.0186) | (0.9418) |
| | | | | |
| | **Panel C. Differences (possible punishment - no punishment)** | | | |
| No reward | 0.0111 | 0.1218*** | 0.0776*** | -0.0442 |
| | [0.0291] | [0.0443] | [0.0204] | [0.0442] |
| | (0.7027) | (0.0064) | (0.0002) | (0.3642) |
| | | | | |
| Monetary reward | -0.0232 | -0.0320 | -0.0146 | 0.0174 |
| | [0.0278] | [0.0554] | [0.0220] | [0.0553] |
| | (0.4039) | (0.5647) | (0.5065) | (0.7707) |
| | | | | |
| Difference | -0.0344 | -0.1538** | -0.0922*** | 0.0616 |
| (monetary reward - no reward) | [0.0403] | [0.0710] | [0.0300] | [0.0769] |
| | (0.3939) | (0.0307) | (0.0022) | (0.4234) |
| | | | | |
| Observations | 2,040 | 503 | 1,537 | 2,040 |
| $R^2$ | 0.0013 | 0.0128 | 0.0085 | 0.0225 |

*Notes:* This table presents beliefs about the riskiness of reporting and differences in beliefs across treatment conditions, along with heterogeneity in reporting rates and heterogeneous treatment effects on reporting by the participants' beliefs. The unit of observation is a respondent. In column (1), the outcome variable is a dummy equal to one if the respondent reported believing that he might face problems for reporting a case of absence and zero otherwise. In columns (2) to (4), the outcome variable is a dummy equal to one if the respondent reported at least one colleague absent in the week preceding the experiment and zero otherwise. Columns (1) and (4) report results for the sample of 2,040 respondents. Column (2) restricts the sample to respondents who reported believing that reporting was not risky. Column (3) restricts the sample to respondents who perceived reporting as risky. In columns (1) to (3), Panel A presents the unconditional mean of the outcome variable in the *no punishment* condition for both the *no reward* and *monetary reward* conditions, and differences in means depending on the reward condition; Panel B presents the same information for the *possible punishment* conditions; and Panel C presents differences for each cell between the *no punishment* and *possible punishment* conditions. Column (4) presents differences for each cell between column (2) and column (3) depending on the perceived riskiness of reporting. In columns (1) to (3), the differences are calculated from OLS regressions of the dependent variable on treatment-condition dummies (a dummy for the monetary reward condition, a dummy for the possible punishment condition, and the interaction of the two). In column (4), the differences are calculated from OLS regressions of the dependent variable on a dummy for the perceived riskiness of reporting, treatment-condition dummies, and the interaction of the beliefs dummy with the treatment-condition dummies. Robust standard errors are presented in brackets. Sampling-based p-values from standard errors are presented in parentheses. For differences, we denote: * significant at 10%; ** significant at 5%; *** significant at 1%.

# A Appendix Figures and Tables

Figure A.1: Heterogeneous Treatment Effects: Attitudes toward Reporting for Rewards



Panel A
Minority of Colleagues Consider
Wrong to Report for Rewards

Panel B
Majority of Colleagues Consider
Wrong to Report for Rewards

*Notes*: Panel A presents the means and 95 percent confidence intervals of the unconditional reporting rates of recent absence by treatment condition for the subsample of participants for whom only a minority of colleagues at the school consider reporting the wrong thing to do in the presence of rewards. The two bars on the left display the share of respondents who reported at least one colleague absent in the week preceding the experiment for individuals in the *no reward* and *monetary reward* treatments in the *no punishment* condition (respectively N=122 and N=117). The two right bars display the same information for respondents in the *possible punishment* condition (respectively N=211 and N=234). Panel B presents the same information for participants whose majority of colleagues at the school consider reporting the wrong thing to do in the presence of rewards. The sample sizes are respectively N=172, N=174, N=368, and N=350. The confidence intervals are based on robust standard errors. Top horizontal bars show p-values for t-tests of equality of means between different experimental conditions. The values on top of the bars in Panel B show p-values for t-tests of equality of means between the two subsamples for any given experimental condition.

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## Table A.1: Summary Statistics and Covariates Balance

| | | Treatment | | | | |
|---|---|---|---|---|---|---|
| | Full sample | No Punishment | | Possible Punishment | | p-value |
| | | No reward | Monetary reward | No reward | Monetary reward | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Female | 0.2372 | 0.2463 | 0.2514 | 0.2298 | 0.2330 | 0.7382 |
| | (0.4254) | [0.0186] | [0.0187] | [0.0128] | [0.0129] | |
| Age | 40.0364 | 39.9833 | 39.9168 | 40.7096 | 39.4408 | 0.1883 |
| | (13.6363) | [0.6011] | [0.5790] | [0.4099] | [0.4169] | |
| Years of education | 13.5293 | 13.6741 | 13.6285 | 13.4605 | 13.4762 | 0.7604 |
| | (4.6619) | [0.1955] | [0.1969] | [0.1425] | [0.1442] | |
| *Ethnic group* | | | | | | |
| Pashtun | 0.7590 | 0.7312 | 0.7771 | 0.7721 | 0.7511 | 0.4105 |
| | (0.4278) | [0.0239] | [0.0226] | [0.0161] | [0.0166] | |
| Tajik | 0.1995 | 0.2225 | 0.1789 | 0.1868 | 0.2108 | 0.3502 |
| | (0.3997) | [0.0224] | [0.0208] | [0.0150] | [0.0156] | |
| Other | 0.0429 | 0.0491 | 0.0469 | 0.0412 | 0.0395 | 0.8822 |
| | (0.2027) | [0.0116] | [0.0115] | [0.0076] | [0.0075] | |
| Salary (AFN) | 7710.9025 | 7671.6185 | 7763.8614 | 7681.3980 | 7733.8882 | 0.8070 |
| | (1978.3499) | [83.5473] | [88.0749] | [58.0011] | [61.8800] | |
| *Rank* | | | | | | |
| Rank > 5 | 0.3526 | 0.3537 | 0.3586 | 0.3364 | 0.3653 | 0.5492 |
| | (0.4778) | [0.0206] | [0.0206] | [0.0143] | [0.0147] | |
| Rank = 5 | 0.4676 | 0.4852 | 0.4492 | 0.4724 | 0.4632 | 0.6605 |
| | (0.4990) | [0.0215] | [0.0214] | [0.0151] | [0.0152] | |
| Rank < 5 | 0.1798 | 0.1611 | 0.1922 | 0.1912 | 0.1715 | 0.3403 |
| | (0.3841) | [0.0158] | [0.0170] | [0.0119] | [0.0115] | |
| *Position* | | | | | | |
| Principal | 0.0262 | 0.0204 | 0.0314 | 0.0221 | 0.0308 | 0.4041 |
| | (0.1598) | [0.0061] | [0.0075] | [0.0045] | [0.0053] | |
| Admin staff | 0.0629 | 0.0611 | 0.0555 | 0.0662 | 0.0643 | 0.8437 |
| | (0.2429) | [0.0103] | [0.0098] | [0.0075] | [0.0075] | |
| Head teacher | 0.0592 | 0.0667 | 0.0739 | 0.0597 | 0.0475 | 0.1509 |
| | (0.2361) | [0.0107] | [0.0113] | [0.0072] | [0.0065] | |
| Teacher | 0.7156 | 0.7278 | 0.7116 | 0.7086 | 0.7185 | 0.8624 |
| | (0.4512) | [0.0192] | [0.0195] | [0.0138] | [0.0137] | |
| Other staff | 0.1360 | 0.1241 | 0.1275 | 0.1434 | 0.1389 | 0.6581 |
| | (0.3429) | [0.0142] | [0.0144] | [0.0106] | [0.0106] | |
| Times present | 0.7170 | 0.7188 | 0.7039 | 0.7261 | 0.7136 | 0.4213 |
| | (0.2649) | [0.0113] | [0.0114] | [0.0080] | [0.0081] | |
| Participant | 0.6292 | 0.6389 | 0.6266 | 0.6222 | 0.6328 | 0.9148 |
| | (0.4831) | [0.0207] | [0.0208] | [0.0147] | [0.0147] | |
| Observations | 3242 | 540 | 541 | 1088 | 1073 | |

*Notes:* This table reports summary statistics for the sample of 3,242 subjects and presents tests of random assignment to the treatment conditions. The unit of observation is a respondent. Column (1) reports the mean level of each variable, with standard deviations in parentheses, for the full sample. Columns (2) to (5) report the mean level of each variable, with robust standard errors in brackets, for each experimental condition. Column (6) reports, for each variable, the p-value of a joint F-test that means are the same in all the experimental conditions. The *possible punishment, no reward* condition pools together the *possible mild punishment, no reward* and *possible severe punishment, no reward* conditions. Similarly, the *possible punishment, monetary reward* condition pools together the *possible mild punishment, monetary reward* and *possible severe punishment, monetary reward* conditions.

## Table A.2: Treatment Effects: Reporting Recent Absence (Covariates)

| | Dummy: respondent reported ≥ 1 absent colleague | Dummy: respondent reported specific colleague as absent | Dummy: respondent reported specific colleague as present | Dummy: respondent reported specific colleague as other |
|---|---|---|---|---|
| | **Dependent Variable** | | | |
| | (1) | (2) | (3) | (4) |
| | *Panel A. No Punishment* | | | |
| Difference | 0.0550** | 0.0080 | -0.0003 | -0.0076 |
| (monetary reward - no reward) | [0.0222] | [0.0053] | [0.0185] | [0.0180] |
| | (0.0134) | (0.1330) | (0.9858) | (0.6717) |
| | | | | |
| | *Panel B. Possible Punishment* | | | |
| Difference | -0.0464** | -0.0118*** | 0.0393*** | -0.0275** |
| (monetary reward - no reward) | [0.0183] | [0.0037] | [0.0130] | [0.0126] |
| | (0.0115) | (0.0013) | (0.0026) | (0.0291) |
| | | | | |
| | *Panel C. Differences (possible punishment - no punishment)* | | | |
| No reward | 0.0852*** | 0.0125*** | -0.0185 | 0.0060 |
| | [0.0193] | [0.0046] | [0.0158] | [0.0153] |
| | (<0.0001) | (0.0065) | (0.2422) | (0.6937) |
| | | | | |
| Monetary reward | -0.0162 | -0.0073* | 0.0211 | -0.0138 |
| | [0.0212] | [0.0044] | [0.0162] | [0.0158] |
| | (0.4434) | (0.0977) | (0.1928) | (0.3824) |
| | | | | |
| Difference | -0.1014*** | -0.0197*** | 0.0396* | -0.0199 |
| (monetary reward - no reward) | [0.0287] | [0.0064] | [0.0227] | [0.0220] |
| | (0.0004) | (0.0020) | (0.0805) | (0.3671) |
| | | | | |
| Controls | Yes | Yes | Yes | Yes |
| Observations | 2,040 | 20,400 | 20,400 | 20,400 |
| $R^2$ | 0.0176 | 0.0039 | 0.0234 | 0.0246 |

*Notes:* This table replicates the differences in reporting rates in Table 2, controlling for respondent covariates (gender, age, education, ethnicity, salary, rank, position, and attendance). The unconditional means presented in Table 2 are suppressed to reflect the fact that differences here are conditional on covariates. The unit of observation is a respondent in column (1) and a respondent×colleague in columns (2) to (4) (each respondent was asked about the absence of ten colleagues). In column (1), the outcome variable is a dummy equal to one if the respondent reported at least one colleague absent in the week preceding the experiment and zero otherwise. In column (2), the outcome variable is a dummy equal to one if the respondent reported a specific colleague absent in the week preceding the experiment and zero otherwise. In column (3), the outcome variable is a dummy equal to one if the respondent reported the colleague present. In column (4), the outcome variable is a dummy equal to one for the residual case in which the respondent reported something else regarding the colleague ("I don't know (about the presence/absence of the colleague)," "I don't know the person," or "other"). Panel A presents differences in reporting rates in the *no punishment* condition depending on the reward condition. Panel B presents the same information for the *possible punishment* conditions. Panel C presents differences for each cell between the *no punishment* and *possible punishment* conditions. The differences in reporting rates are calculated from OLS regressions of the dependent variable on a dummy for the *monetary reward* condition, a dummy for the *possible punishment* condition, and the interaction of the two. Robust standard errors for column (1) and standard errors clustered at the respondent level for columns (2) to (4) are presented in brackets. Sampling-based p-values from standard errors are presented in parentheses. For differences in reporting rates, we denote (using the sampling-based p-values): * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.3: Treatment Effects: Reporting Recent Absence (School FE)

| | Dependent Variable | | | |
|---|---|---|---|---|
| | Dummy: respondent reported $\geq 1$ absent colleague | Dummy: respondent reported specific colleague as absent | Dummy: respondent reported specific colleague as present | Dummy: respondent reported specific colleague as other |
| | (1) | (2) | (3) | (4) |
| | Panel A. No Punishment | | | |
| Difference | 0.0526** | 0.0066 | -0.0054 | -0.0012 |
| (monetary reward - no reward) | [0.0220] | [0.0050] | [0.0174] | [0.0169] |
| | (0.0169) | (0.1874) | (0.7563) | (0.9448) |
| | Panel B. Possible Punishment | | | |
| Difference | -0.0445** | -0.0116*** | 0.0401*** | -0.0286** |
| (monetary reward - no reward) | [0.0182] | [0.0035] | [0.0122] | [0.0118] |
| | (0.0148) | (0.0009) | (0.0011) | (0.0158) |
| | Panel C. Differences (possible punishment - no punishment) | | | |
| No reward | 0.0803*** | 0.0114*** | -0.0141 | 0.0026 |
| | [0.0190] | [0.0042] | [0.0152] | [0.0148] |
| | (<0.0001) | (0.0071) | (0.3537) | (0.8579) |
| Monetary reward | -0.0168 | -0.0067 | 0.0314** | -0.0248* |
| | [0.0212] | [0.0043] | [0.0149] | [0.0144] |
| | (0.4282) | (0.1210) | (0.0346) | (0.0860) |
| Difference | -0.0971*** | -0.0181*** | 0.0455** | -0.0274 |
| (monetary reward - no reward) | [0.0286] | [0.0060] | [0.0213] | [0.0207] |
| | (0.0007) | (0.0025) | (0.0326) | (0.1850) |
| School Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 2,040 | 20,400 | 20,400 | 20,400 |
| $R^2$ | 0.1037 | 0.0304 | 0.0763 | 0.0791 |

*Notes:* This table replicates the differences in reporting rates in Table 2, controlling for school fixed effects. The unconditional means presented in Table 2 are suppressed to reflect the fact that differences here are conditional on fixed effects. The unit of observation is a respondent in column (1) and a respondent×colleague in columns (2) to (4) (each respondent was asked about the absence of ten colleagues). In column (1), the outcome variable is a dummy equal to one if the respondent reported at least one colleague absent in the week preceding the experiment and zero otherwise. In column (2), the outcome variable is a dummy equal to one if the respondent reported a specific colleague absent in the week preceding the experiment and zero otherwise. In column (3), the outcome variable is a dummy equal to one if the respondent reported the colleague present. In column (4), the outcome variable is a dummy equal to one for the residual case in which the respondent reported something else regarding the colleague ("I don't know (about the presence/absence of the colleague)," "I don't know the person," or "other"). Panel A presents differences in reporting rates in the *no punishment* condition depending on the reward condition. Panel B presents the same information for the *possible punishment* conditions. Panel C presents differences for each cell between the *no punishment* and *possible punishment* conditions. The differences in reporting rates are calculated from OLS regressions of the dependent variable on a dummy for the *monetary reward* condition, a dummy for the *possible punishment* condition, and the interaction of the two. Robust standard errors for column (1) and standard errors clustered at the respondent level for columns (2) to (4) are presented in brackets. Sampling-based p-values from standard errors are presented in parentheses. For differences in reporting rates, we denote (using the sampling-based p-values): * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.4: Treatment Effects: Reporting Recent Absence (Covariates and School FE)

| | Dependent Variable | | | |
|---|---|---|---|---|
| | Dummy: respondent reported ≥ 1 absent colleague | Dummy: respondent reported specific colleague as absent | Dummy: respondent reported specific colleague as present | Dummy: respondent reported specific colleague as other |
| | (1) | (2) | (3) | (4) |
| | Panel A. No Punishment | | | |
| Difference | 0.0517** | 0.0064 | -0.0027 | -0.0037 |
| (monetary reward - no reward) | [0.0222] | [0.0050] | [0.0171] | [0.0166] |
| | (0.0200) | (0.1986) | (0.8723) | (0.8244) |
| | | | | |
| | Panel B. Possible Punishment | | | |
| Difference | -0.0439** | -0.0115*** | 0.0391*** | -0.0276** |
| (monetary reward - no reward) | [0.0184] | [0.0035] | [0.0122] | [0.0117] |
| | (0.0170) | (0.0012) | (0.0014) | (0.0189) |
| | | | | |
| | Panel C. Differences (possible punishment - no punishment) | | | |
| No reward | 0.0786*** | 0.0112*** | -0.0117 | 0.0005 |
| | [0.0192] | [0.0043] | [0.0149] | [0.0145] |
| | (<0.0001) | (0.0090) | (0.4326) | (0.9734) |
| | | | | |
| Monetary reward | -0.0169 | -0.0067 | 0.0301** | -0.0234 |
| | [0.0212] | [0.0043] | [0.0148] | [0.0143] |
| | (0.4241) | (0.1169) | (0.0418) | (0.1014) |
| | | | | |
| Difference | -0.0956*** | -0.0179*** | 0.0418** | -0.0239 |
| (monetary reward - no reward) | [0.0287] | [0.0060] | [0.0211] | [0.0204] |
| | (0.0009) | (0.0029) | (0.0472) | (0.2407) |
| | | | | |
| Controls | Yes | Yes | Yes | Yes |
| School Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 2,040 | 20,400 | 20,400 | 20,400 |
| $R^2$ | 0.1108 | 0.0317 | 0.0836 | 0.0880 |

*Notes:* This table replicates the differences in reporting rates in Table 2, controlling for respondent covariates (gender, age, education, ethnicity, salary, rank, position, and attendance) and school fixed effects. The unconditional means presented in Table 2 are suppressed to reflect the fact that differences here are conditional on covariates and fixed effects. The unit of observation is a respondent in column (1) and a respondent×colleague in columns (2) to (4) (each respondent was asked about the absence of ten colleagues). In column (1), the outcome variable is a dummy equal to one if the respondent reported at least one colleague absent in the week preceding the experiment and zero otherwise. In column (2), the outcome variable is a dummy equal to one if the respondent reported a specific colleague absent in the week preceding the experiment and zero otherwise. In column (3), the outcome variable is a dummy equal to one if the respondent reported the colleague present. In column (4), the outcome variable is a dummy equal to one for the residual case in which the respondent reported something else regarding the colleague ("I don't know (about the presence/absence of the colleague)," "I don't know the person," or "other"). Panel A presents differences in reporting rates in the *no punishment* condition depending on the reward condition. Panel B presents the same information for the *possible punishment* conditions. Panel C presents differences for each cell between the *no punishment* and *possible punishment* conditions. The differences in reporting rates are calculated from OLS regressions of the dependent variable on a dummy for the *monetary reward* condition, a dummy for the *possible punishment* condition, and the interaction of the two. Robust standard errors for column (1) and standard errors clustered at the respondent level for columns (2) to (4) are presented in brackets. Sampling-based p-values from standard errors are presented in parentheses. For differences in reporting rates, we denote (using the sampling-based p-values): * significant at 10%; ** significant at 5%; *** significant at 1%.

## Table A.5: Treatment Effects: Reporting Recent Absence (Disaggregated)

| | Dummy: respondent reported ≥ 1 absent colleague (1) | Dummy: respondent reported specific colleague as absent (2) | Dummy: respondent reported specific colleague as present (3) | Dummy: respondent reported specific colleague as other (4) |
|---|---|---|---|---|
| | | Dependent Variable | | |
| **Panel A. No Punishment** | | | | |
| No reward | 0.0638 | 0.0133 | 0.7991 | 0.1875 |
| | [0.0132] | [0.0034] | [0.0132] | [0.0129] |
| Monetary reward | 0.1209 | 0.0218 | 0.7917 | 0.1864 |
| | [0.0177] | [0.0041] | [0.0138] | [0.0135] |
| Difference | 0.0572*** | 0.0085 | -0.0074 | -0.0011 |
| (monetary reward - no reward) | [0.0221] | [0.0053] | [0.0190] | [0.0186] |
| | (0.0099) | (0.1099) | (0.6982) | (0.9527) |
| **Panel B1. Possible Mild Punishment** | | | | |
| No reward | 0.1509 | 0.0237 | 0.7746 | 0.2018 |
| | [0.0195] | [0.0040] | [0.0143] | [0.0138] |
| Monetary reward | 0.1098 | 0.0142 | 0.8220 | 0.1639 |
| | [0.0168] | [0.0024] | [0.0123] | [0.0121] |
| Difference | -0.0411 | -0.0095** | 0.0474** | -0.0379** |
| (monetary reward - no reward) | [0.0258] | [0.0046] | [0.0189] | [0.0184] |
| | (0.1114) | (0.0410) | (0.0123) | (0.0393) |
| **Panel B2. Possible Severe Punishment** | | | | |
| No reward | 0.1534 | 0.0295 | 0.7796 | 0.1909 |
| | [0.0196] | [0.0048] | [0.0138] | [0.0132] |
| Monetary reward | 0.0991 | 0.0147 | 0.8135 | 0.1718 |
| | [0.0164] | [0.0027] | [0.0130] | [0.0127] |
| Difference | -0.0543** | -0.0148*** | 0.0339* | -0.0191 |
| (monetary reward - no reward) | [0.0256] | [0.0055] | [0.0189] | [0.0183] |
| | (0.0340) | (0.0075) | (0.0741) | (0.2977) |
| **Panel C1. Differences (possible mild punishment - no punishment)** | | | | |
| No reward | 0.0871*** | 0.0103** | -0.0246 | 0.0142 |
| | [0.0235] | [0.0052] | [0.0194] | [0.0188] |
| | (0.0002) | (0.0477) | (0.2068) | (0.4502) |
| Monetary reward | -0.0111 | -0.0077 | 0.0302 | -0.0226 |
| | [0.0245] | [0.0048] | [0.0185] | [0.0181] |
| | (0.6495) | (0.1072) | (0.1023) | (0.2139) |
| Difference | -0.0982*** | -0.0180** | 0.0548** | -0.0368 |
| (monetary reward - no reward) | [0.0339] | [0.0070] | [0.0268] | [0.0261] |
| | (0.0038) | (0.0107) | (0.0411) | (0.1594) |
| **Panel C2. Differences (possible severe punishment - no punishment)** | | | | |
| No reward | 0.0896*** | 0.0162*** | -0.0195 | 0.0033 |
| | [0.0236] | [0.0059] | [0.0191] | [0.0184] |
| | (0.0002) | (0.0061) | (0.3071) | (0.8572) |
| Monetary reward | -0.0218 | -0.0071 | 0.0218 | -0.0147 |
| | [0.0242] | [0.0049] | [0.0189] | [0.0185] |
| | (0.3661) | (0.1478) | (0.2502) | (0.4285) |
| Difference | -0.1115*** | -0.0233*** | 0.0413 | -0.0180 |
| (monetary reward - no reward) | [0.0338] | [0.0077] | [0.0268] | [0.0261] |
| | (0.0010) | (0.0024) | (0.1245) | (0.4912) |
| **Panel C3. Differences (possible severe punishment - possible mild punishment)** | | | | |
| No reward | 0.0025 | 0.0058 | 0.0051 | -0.0109 |
| | [0.0276] | [0.0063] | [0.0199] | [0.0191] |
| | (0.9278) | (0.3515) | (0.7979) | (0.5672) |
| Monetary reward | -0.0107 | 0.0006 | -0.0085 | 0.0079 |
| | [0.0235] | [0.0036] | [0.0179] | [0.0176] |
| | (0.6482) | (0.8777) | (0.6369) | (0.6530) |
| Difference | -0.0132 | -0.0053 | -0.0135 | 0.0188 |
| (monetary reward - no reward) | [0.0363] | [0.0072] | [0.0267] | [0.0259] |
| | (0.7154) | (0.4642) | (0.6125) | (0.4678) |
| Observations | 2,040 | 20,400 | 20,400 | 20,400 |
| $R^2$ | 0.0093 | 0.0019 | 0.0018 | 0.0010 |

*Notes:* This table presents unconditional reporting rates of recent absence and differences in reporting rates across treatment conditions. The results are disaggregated by the two versions of the *possible punishment* condition. The unit of observation is a respondent in column (1) and a respondent×colleague in columns (2) to (4) (each respondent was asked about the absence of ten colleagues). In column (1), the outcome variable is a dummy equal to one if the respondent reported at least one colleague absent in the week preceding the experiment and zero otherwise. In column (2), the outcome variable is a dummy equal to one if the respondent reported a specific colleague absent in the week preceding the experiment and zero otherwise. In column (3), the outcome variable is a dummy equal to one if the respondent reported the colleague present. In column (4), the outcome variable is a dummy equal to one for the residual case in which the respondent reported something else regarding the colleague ("I don't know (about the presence/absence of the colleague)," "I don't know the person," or "other"). Panel A presents unconditional reporting rates in the *no punishment* condition for both the *no reward* and *monetary reward* conditions, and differences in reporting rates depending on the reward condition. Panel B1 presents the same information for the *possible mild punishment* conditions. Panel B2 presents the same information for the *possible severe punishment* conditions. Panel C1 presents differences for each cell between the *no punishment* and *possible mild punishment* conditions. Panel C2 presents the same information for the *no punishment* and *possible severe punishment* conditions. Panel C3 presents the same information for the *possible mild punishment* and *possible severe punishment* conditions. The differences in reporting rates are calculated from OLS regressions of the dependent variable on a dummy for the *monetary reward* condition, a dummy for the *possible mild punishment* condition and its interaction with the *monetary reward* dummy, a dummy for the *possible severe punishment* condition and its interaction with the *monetary reward* dummy. Robust standard errors for column (1) and standard errors clustered at the respondent level for columns (2) to (4) are presented in brackets. Sampling-based p-values from standard errors are presented in parentheses. For differences in reporting rates, we denote (using the sampling-based p-values): * significant at 10%; ** significant at 5%; *** significant at 1%.

# B  Survey Scripts

## B.I  No Rewards and No Punishment

Hello, my name is [name of surveyor], and I am calling you from Kabul on behalf of the Mobile Salary Payments research team collaborating with the Ministry of Education, the Ministry of Finance, and the Ministry of Communication and Information Technology. This research is funded by the International Growth Centre at the London School of Economics and Political Science. Am I speaking with [name of Ministry of Education employee]?

Do you have a few minutes to talk for a quick survey?

I would like to ask you some questions related to your experience as a Ministry of Education employee. Participation is voluntary. The alternative to participation is not to participate. You can decide not to participate in this survey or to interrupt the survey at any point without any adverse consequence for you. The survey would last about 15 minutes. If you decide to participate, I will ask you about problems at your school, and especially about absenteeism among your colleagues. There is no compensation for participating in this study.

Do you have any questions?

Would you like to participate in the survey?

First, I would like to ask you: what do you think are the most important problems at your school that you would like to be solved?

Next, I would like to ask you some questions about absenteeism among your colleagues. I will read you ten names. For each name, if that person is currently an employee at your school, I want to know whether you saw that person working at your school every day last week and whether in the past that person was generally working every day or was often absent. Your identity will remain confidential. No one will know whether you reported someone as an absentee.

So, are you willing to proceed?

Your answers will be used only for an academic study on absenteeism. The names of people you report will not be forwarded to the Ministry of Education. So your answers will not be used to impose penalties on any of your colleagues.

Once again, remember that your reports are anonymous, and your name will never be shared with anyone. So you should not face any problem with reporting absenteeism to us. If you report

a case of absenteeism to us, do you think that you might face problems (for example, with the employee you reported punishing you)?

In any case, you should take into account the risks associated with reporting absenteeism. These include potential retaliation from the colleagues you report as absentees, and possible sanctions imposed by the Ministry of Education on the absentees you report. Are you willing to proceed?

The first name is [name of the first colleague]. Do you know this person?

Is [name of colleague] currently an employee at your school?

Can you confirm that [name of colleague] was working at your school every day last week?

And in the past, was [name of colleague] generally working every day, or was he/she often absent?

The next name is [name of the second colleague]. Do you know this person?

[...]

Which ethnic group do you belong to?

Which ethnic group does [name of the first colleague] belong to?
[...]

Thanks for your cooperation. At this moment, I would like to explain to you a bit more about this survey. Stefano Fiorin, a Postdoctoral Scholar at the University of California San Diego in the United States, is conducting a study to find out how to motivate the Ministry of Education employees to report on the absenteeism of their colleagues. You have been asked to participate because you are a Ministry of Education employee in one of the 151 schools selected for this study. There will be approximately 2,000 participants in this study. Please note that because this was simply a research study to help find out what works best for encouraging the Ministry of Education employees to report absenteeism, we will not actually be reporting any absent employees to the Ministry of Education based on what we learn in this study, and we will never provide the information you gave to the Ministry of Education. If you had any worries or concerns about whether you should report a co-worker, you should understand that we will not be reporting anyone and that what you said will be kept confidential and used only for research purposes. What you said will not be shared with the International Growth Centre either. The risk of a breach of confidentiality is minimal: Your responses will be recorded on the online survey platform Qualtrics, which uses industry best standards to guarantee the security of the data collected. The data will be available

for download on Qualtrics only to the PI. Since this is an investigational study, there may be some unknown risks that are currently unforeseeable. You will be informed of any significant new findings. Do you have any questions regarding this study?

If you have additional questions or need to report research-related problems, you may contact [name of PI] at [phone number of PI] or through [name of field research manager] at [phone number of field research manager]. You may also call the Human Research Protections Program at +1(858) 246-4777 to inquire about your rights as a research subject or to report research related problems. Have a nice day.

## B.II  Monetary Rewards and No Punishment

Hello, my name is [name of surveyor], and I am calling you from Kabul on behalf of the Mobile Salary Payments research team collaborating with the Ministry of Education, the Ministry of Finance, and the Ministry of Communication and Information Technology. This research is funded by the International Growth Centre at the London School of Economics and Political Science. Am I speaking with [name of Ministry of Education employee]?

Do you have a few minutes to talk for a quick survey?

I would like to ask you some questions related to your experience as a Ministry of Education employee. Participation is voluntary. The alternative to participation is not to participate. You can decide not to participate in this survey or to interrupt the survey at any point without any adverse consequence for you. The survey would last about 15 minutes. If you decide to participate, I will ask you about problems at your school, and especially about absenteeism among your colleagues. There is no compensation for participating in this study, but there may be compensation based on the information you provide.

Do you have any questions?

Would you like to participate in the survey?

First, I would like to ask you: what do you think are the most important problems at your school that you would like to be solved?

Next, I would like to ask you some questions about absenteeism among your colleagues. I will read you ten names. For each name, if that person is currently an employee at your school, I want to know whether you saw that person working at your school every day last week and whether in the past that person was generally working every day or was often absent. Your identity will remain confidential. No one will know whether you reported someone as an absentee.

So, are you willing to proceed?

As a reward for your help, we will make a top-up of 100 AFG to your phone for each absentee that you report. For example, if you say that seven persons were always present and that three other employees were absent some time last week or in the past, then we will send a top-up of 300 AFG to your mobile phone.

Your answers will be used only for an academic study on absenteeism. The names of people you report will not be forwarded to the Ministry of Education. So your answers will not be used

to impose penalties on any of your colleagues.

Once again, remember that your reports are anonymous, and your name will never be shared with anyone. So you should not face any problem with reporting absenteeism to us. If you report a case of absenteeism to us, do you think that you might face problems (for example, with the employee you reported punishing you)?

In any case, you should take into account the risks associated with reporting absenteeism. These include potential retaliation from the colleagues you report as absentees, and possible sanctions imposed by the Ministry of Education on the absentees you report. Are you willing to proceed?

The first name is [name of the first colleague]. Do you know this person?

Is [name of colleague] currently an employee at your school?

Can you confirm that [name of colleague] was working at your school every day last week?

And in the past, was [name of colleague] generally working every day, or was he/she often absent?

The next name is [name of the second colleague]. Do you know this person?

[...]

Which ethnic group do you belong to?

Which ethnic group does [name of the first colleague] belong to?
[...]

Thanks for your cooperation. At this moment, I would like to explain to you a bit more about this survey. Stefano Fiorin, a Postdoctoral Scholar at the University of California San Diego in the United States, is conducting a study to find out how to motivate the Ministry of Education employees to report on the absenteeism of their colleagues. You have been asked to participate because you are a Ministry of Education employee in one of the 151 schools selected for this study. There will be approximately 2,000 participants in this study. Please note that because this was simply a research study to help find out what works best for encouraging the Ministry of Education employees to report absenteeism, we will not actually be reporting any absent employees to the Ministry of Education based on what we learn in this study, and we will never provide the information you gave to the Ministry of Education. If you had any worries or concerns about whether you should report a co-worker, you should understand that we will not be reporting anyone and that

what you said will be kept confidential and used only for research purposes. What you said will not be shared with the International Growth Centre either. The risk of a breach of confidentiality is minimal: Your responses will be recorded on the online survey platform Qualtrics, which uses industry best standards to guarantee the security of the data collected. The data will be available for download on Qualtrics only to the PI. Since this is an investigational study, there may be some unknown risks that are currently unforeseeable. You will be informed of any significant new findings. Do you have any questions regarding this study?

If you have additional questions or need to report research-related problems, you may contact [name of PI] at [phone number of PI] or through [name of field research manager] at [phone number of field research manager]. You may also call the Human Research Protections Program at +1(858) 246-4777 to inquire about your rights as a research subject or to report research related problems. Have a nice day.

## B.III   No Rewards and Possible Mild Punishment

Hello, my name is [name of surveyor], and I am calling you from Kabul on behalf of the Mobile Salary Payments research team collaborating with the Ministry of Education, the Ministry of Finance, and the Ministry of Communication and Information Technology. This research is funded by the International Growth Centre at the London School of Economics and Political Science. Am I speaking with [name of Ministry of Education employee]?

Do you have a few minutes to talk for a quick survey?

I would like to ask you some questions related to your experience as a Ministry of Education employee. Participation is voluntary. The alternative to participation is not to participate. You can decide not to participate in this survey or to interrupt the survey at any point without any adverse consequence for you. The survey would last about 15 minutes. If you decide to participate, I will ask you about problems at your school, and especially about absenteeism among your colleagues. There is no compensation for participating in this study.

Do you have any questions?

Would you like to participate in the survey?

First, I would like to ask you: what do you think are the most important problems at your school that you would like to be solved?

Next, I would like to ask you some questions about absenteeism among your colleagues. I will read you ten names. For each name, if that person is currently an employee at your school, I want to know whether you saw that person working at your school every day last week and whether in the past that person was generally working every day or was often absent. Your identity will remain confidential. No one will know whether you reported someone as an absentee.

So, are you willing to proceed?

Your answers will be used for an academic study on absenteeism, but the names of people you report might also be forwarded to the Ministry of Education. So your answers might be used to impose penalties on your colleagues. These typically include minor consequences, like a one-time 20 AFG reduction of their salary.

Once again, remember that your reports are anonymous, and your name will never be shared with anyone. So you should not face any problem with reporting absenteeism to us. If you report a case of absenteeism to us, do you think that you might face problems (for example, with the

employee you reported punishing you)?

In any case, you should take into account the risks associated with reporting absenteeism. These include potential retaliation from the colleagues you report as absentees, and possible sanctions imposed by the Ministry of Education on the absentees you report. Are you willing to proceed?

The first name is [name of the first colleague]. Do you know this person?

Is [name of colleague] currently an employee at your school?

Can you confirm that [name of colleague] was working at your school every day last week?

And in the past, was [name of colleague] generally working every day, or was he/she often absent?

The next name is [name of the second colleague]. Do you know this person?

[...]

Which ethnic group do you belong to?

Which ethnic group does [name of the first colleague] belong to?
[...]

Thanks for your cooperation. At this moment, I would like to explain to you a bit more about this survey. Stefano Fiorin, a Postdoctoral Scholar at the University of California San Diego in the United States, is conducting a study to find out how to motivate the Ministry of Education employees to report on the absenteeism of their colleagues. You have been asked to participate because you are a Ministry of Education employee in one of the 151 schools selected for this study. There will be approximately 2,000 participants in this study. Please note that because this was simply a research study to help find out what works best for encouraging the Ministry of Education employees to report absenteeism, we will not actually be reporting any absent employees to the Ministry of Education based on what we learn in this study, and we will never provide the information you gave to the Ministry of Education. If you had any worries or concerns about whether you should report a co-worker, you should understand that we will not be reporting anyone and that what you said will be kept confidential and used only for research purposes. What you said will not be shared with the International Growth Centre either. The risk of a breach of confidentiality is minimal: Your responses will be recorded on the online survey platform Qualtrics, which uses industry best standards to guarantee the security of the data collected. The data will be available for download on Qualtrics only to the PI. Since this is an investigational study, there may be some

unknown risks that are currently unforeseeable. You will be informed of any significant new findings. Do you have any questions regarding this study?

If you have additional questions or need to report research-related problems, you may contact [name of PI] at [phone number of PI] or through [name of field research manager] at [phone number of field research manager]. You may also call the Human Research Protections Program at +1(858) 246-4777 to inquire about your rights as a research subject or to report research related problems. Have a nice day.

### B.IV  Monetary and Possible Mild Punishment

Hello, my name is [name of surveyor], and I am calling you from Kabul on behalf of the Mobile Salary Payments research team collaborating with the Ministry of Education, the Ministry of Finance, and the Ministry of Communication and Information Technology. This research is funded by the International Growth Centre at the London School of Economics and Political Science. Am I speaking with [name of Ministry of Education employee]?

Do you have a few minutes to talk for a quick survey?

I would like to ask you some questions related to your experience as a Ministry of Education employee. Participation is voluntary. The alternative to participation is not to participate. You can decide not to participate in this survey or to interrupt the survey at any point without any adverse consequence for you. The survey would last about 15 minutes. If you decide to participate, I will ask you about problems at your school, and especially about absenteeism among your colleagues. There is no compensation for participating in this study, but there may be compensation based on the information you provide.

Do you have any questions?

Would you like to participate in the survey?

First, I would like to ask you: what do you think are the most important problems at your school that you would like to be solved?

Next, I would like to ask you some questions about absenteeism among your colleagues. I will read you ten names. For each name, if that person is currently an employee at your school, I want to know whether you saw that person working at your school every day last week and whether in the past that person was generally working every day or was often absent. Your identity will remain confidential. No one will know whether you reported someone as an absentee.

So, are you willing to proceed?

As a reward for your help, we will make a top-up of 100 AFG to your phone for each absentee that you report. For example, if you say that seven persons were always present and that three other employees were absent some time last week or in the past, then we will send a top-up of 300 AFG to your mobile phone.

Your answers will be used for an academic study on absenteeism, but the names of people you report might also be forwarded to the Ministry of Education. So your answers might be used to

impose penalties on your colleagues. These typically include minor consequences, like a one-time 20 AFG reduction of their salary.

Once again, remember that your reports are anonymous, and your name will never be shared with anyone. So you should not face any problem with reporting absenteeism to us. If you report a case of absenteeism to us, do you think that you might face problems (for example, with the employee you reported punishing you)?

In any case, you should take into account the risks associated with reporting absenteeism. These include potential retaliation from the colleagues you report as absentees, and possible sanctions imposed by the Ministry of Education on the absentees you report. Are you willing to proceed?

The first name is [name of the first colleague]. Do you know this person?

Is [name of colleague] currently an employee at your school?

Can you confirm that [name of colleague] was working at your school every day last week?

And in the past, was [name of colleague] generally working every day, or was he/she often absent?

The next name is [name of the second colleague]. Do you know this person?

[...]

Which ethnic group do you belong to?

Which ethnic group does [name of the first colleague] belong to?
[...]

Thanks for your cooperation. At this moment, I would like to explain to you a bit more about this survey. Stefano Fiorin, a Postdoctoral Scholar at the University of California San Diego in the United States, is conducting a study to find out how to motivate the Ministry of Education employees to report on the absenteeism of their colleagues. You have been asked to participate because you are a Ministry of Education employee in one of the 151 schools selected for this study. There will be approximately 2,000 participants in this study. Please note that because this was simply a research study to help find out what works best for encouraging the Ministry of Education employees to report absenteeism, we will not actually be reporting any absent employees to the Ministry of Education based on what we learn in this study, and we will never provide the information you gave to the Ministry of Education. If you had any worries or concerns about whether you

should report a co-worker, you should understand that we will not be reporting anyone and that what you said will be kept confidential and used only for research purposes. What you said will not be shared with the International Growth Centre either. The risk of a breach of confidentiality is minimal: Your responses will be recorded on the online survey platform Qualtrics, which uses industry best standards to guarantee the security of the data collected. The data will be available for download on Qualtrics only to the PI. Since this is an investigational study, there may be some unknown risks that are currently unforeseeable. You will be informed of any significant new findings. Do you have any questions regarding this study?

If you have additional questions or need to report research-related problems, you may contact [name of PI] at [phone number of PI] or through [name of field research manager] at [phone number of field research manager]. You may also call the Human Research Protections Program at +1(858) 246-4777 to inquire about your rights as a research subject or to report research related problems. Have a nice day.

## B.V   No Rewards and Possible Severe Punishment

Hello, my name is [name of surveyor], and I am calling you from Kabul on behalf of the Mobile Salary Payments research team collaborating with the Ministry of Education, the Ministry of Finance, and the Ministry of Communication and Information Technology. This research is funded by the International Growth Centre at the London School of Economics and Political Science. Am I speaking with [name of Ministry of Education employee]?

Do you have a few minutes to talk for a quick survey?

I would like to ask you some questions related to your experience as a Ministry of Education employee. Participation is voluntary. The alternative to participation is not to participate. You can decide not to participate in this survey or to interrupt the survey at any point without any adverse consequence for you. The survey would last about 15 minutes. If you decide to participate, I will ask you about problems at your school, and especially about absenteeism among your colleagues. There is no compensation for participating in this study.

Do you have any questions?

Would you like to participate in the survey?

First, I would like to ask you: what do you think are the most important problems at your school that you would like to be solved?

Next, I would like to ask you some questions about absenteeism among your colleagues. I will read you ten names. For each name, if that person is currently an employee at your school, I want to know whether you saw that person working at your school every day last week and whether in the past that person was generally working every day or was often absent. Your identity will remain confidential. No one will know whether you reported someone as an absentee.

So, are you willing to proceed?

Your answers will be used for an academic study on absenteeism, but the names of people you report might also be forwarded to the Ministry of Education. The Ministry of Education recently instituted a payroll steering committee in Kabul. The committee decides on the future of employees who are suspected of absenteeism. So your answers might be used to impose penalties on your colleagues. These might include major consequences, like the termination of their contract.

Once again, remember that your reports are anonymous, and your name will never be shared with anyone. So you should not face any problem with reporting absenteeism to us. If you report

a case of absenteeism to us, do you think that you might face problems (for example, with the employee you reported punishing you)?

In any case, you should take into account the risks associated with reporting absenteeism. These include potential retaliation from the colleagues you report as absentees, and possible sanctions imposed by the Ministry of Education on the absentees you report. Are you willing to proceed?

The first name is [name of the first colleague]. Do you know this person?

Is [name of colleague] currently an employee at your school?

Can you confirm that [name of colleague] was working at your school every day last week?

And in the past, was [name of colleague] generally working every day, or was he/she often absent?

The next name is [name of the second colleague]. Do you know this person?

[...]

Which ethnic group do you belong to?

Which ethnic group does [name of the first colleague] belong to?
[...]

Thanks for your cooperation. At this moment, I would like to explain to you a bit more about this survey. Stefano Fiorin, a Postdoctoral Scholar at the University of California San Diego in the United States, is conducting a study to find out how to motivate the Ministry of Education employees to report on the absenteeism of their colleagues. You have been asked to participate because you are a Ministry of Education employee in one of the 151 schools selected for this study. There will be approximately 2,000 participants in this study. Please note that because this was simply a research study to help find out what works best for encouraging the Ministry of Education employees to report absenteeism, we will not actually be reporting any absent employees to the Ministry of Education based on what we learn in this study, and we will never provide the information you gave to the Ministry of Education. If you had any worries or concerns about whether you should report a co-worker, you should understand that we will not be reporting anyone and that what you said will be kept confidential and used only for research purposes. What you said will not be shared with the International Growth Centre either. The risk of a breach of confidentiality is minimal: Your responses will be recorded on the online survey platform Qualtrics, which uses industry best standards to guarantee the security of the data collected. The data will be available

for download on Qualtrics only to the PI. Since this is an investigational study, there may be some unknown risks that are currently unforeseeable. You will be informed of any significant new findings. Do you have any questions regarding this study?

If you have additional questions or need to report research-related problems, you may contact [name of PI] at [phone number of PI] or through [name of field research manager] at [phone number of field research manager]. You may also call the Human Research Protections Program at +1(858) 246-4777 to inquire about your rights as a research subject or to report research related problems. Have a nice day.

## B.VI   Monetary Rewards and Possible Severe Punishment

Hello, my name is [name of surveyor], and I am calling you from Kabul on behalf of the Mobile Salary Payments research team collaborating with the Ministry of Education, the Ministry of Finance, and the Ministry of Communication and Information Technology. This research is funded by the International Growth Centre at the London School of Economics and Political Science. Am I speaking with [name of Ministry of Education employee]?

Do you have a few minutes to talk for a quick survey?

I would like to ask you some questions related to your experience as a Ministry of Education employee. Participation is voluntary. The alternative to participation is not to participate. You can decide not to participate in this survey or to interrupt the survey at any point without any adverse consequence for you. The survey would last about 15 minutes. If you decide to participate, I will ask you about problems at your school, and especially about absenteeism among your colleagues. There is no compensation for participating in this study, but there may be compensation based on the information you provide.

Do you have any questions?

Would you like to participate in the survey?

First, I would like to ask you: what do you think are the most important problems at your school that you would like to be solved?

Next, I would like to ask you some questions about absenteeism among your colleagues. I will read you ten names. For each name, if that person is currently an employee at your school, I want to know whether you saw that person working at your school every day last week and whether in the past that person was generally working every day or was often absent. Your identity will remain confidential. No one will know whether you reported someone as an absentee.

So, are you willing to proceed?

As a reward for your help, we will make a top-up of 100 AFG to your phone for each absentee that you report. For example, if you say that seven persons were always present and that three other employees were absent some time last week or in the past, then we will send a top-up of 300 AFG to your mobile phone.

In any case, you should take into account the risks associated with reporting absenteeism. These include potential retaliation from the colleagues you report as absentees, and possible sanc-

tions imposed by the Ministry of Education on the absentees you report. Are you willing to proceed?

Your answers will be used for an academic study on absenteeism, but the names of people you report might also be forwarded to the Ministry of Education. The Ministry of Education recently instituted a payroll steering committee in Kabul. The committee decides on the future of employees who are suspected of absenteeism. So your answers might be used to impose penalties on your colleagues. These might include major consequences, like the termination of their contract.

Once again, remember that your reports are anonymous, and your name will never be shared with anyone. So you should not face any problem with reporting absenteeism to us. If you report a case of absenteeism to us, do you think that you might face problems (for example, with the employee you reported punishing you)?

The first name is [name of the first colleague]. Do you know this person?

Is [name of colleague] currently an employee at your school?

Can you confirm that [name of colleague] was working at your school every day last week?

And in the past, was [name of colleague] generally working every day, or was he/she often absent?

The next name is [name of the second colleague]. Do you know this person?

[...]

Which ethnic group do you belong to?

Which ethnic group does [name of the first colleague] belong to?
[...]

Thanks for your cooperation. At this moment, I would like to explain to you a bit more about this survey. Stefano Fiorin, a Postdoctoral Scholar at the University of California San Diego in the United States, is conducting a study to find out how to motivate the Ministry of Education employees to report on the absenteeism of their colleagues. You have been asked to participate because you are a Ministry of Education employee in one of the 151 schools selected for this study. There will be approximately 2,000 participants in this study. Please note that because this was simply a research study to help find out what works best for encouraging the Ministry of Education employees to report absenteeism, we will not actually be reporting any absent employees to the Ministry of Education based on what we learn in this study, and we will never provide the informa-

tion you gave to the Ministry of Education. If you had any worries or concerns about whether you should report a co-worker, you should understand that we will not be reporting anyone and that what you said will be kept confidential and used only for research purposes. What you said will not be shared with the International Growth Centre either. The risk of a breach of confidentiality is minimal: Your responses will be recorded on the online survey platform Qualtrics, which uses industry best standards to guarantee the security of the data collected. The data will be available for download on Qualtrics only to the PI. Since this is an investigational study, there may be some unknown risks that are currently unforeseeable. You will be informed of any significant new findings. Do you have any questions regarding this study?

If you have additional questions or need to report research-related problems, you may contact [name of PI] at [phone number of PI] or through [name of field research manager] at [phone number of field research manager]. You may also call the Human Research Protections Program at +1(858) 246-4777 to inquire about your rights as a research subject or to report research related problems. Have a nice day.

## B.VII    Additional Survey on Attitudes towards Reporting

[...]

Next, I would like to ask you some questions about absenteeism.

Imagine a teacher is asked by someone from the Ministry of Education in Kabul to confidentially report the colleagues who are sometimes absent from school, in order to punish absenteeism. If the teacher knows that someone is sometimes absent, what is the right thing for the teacher to do?

Why?

Now imagine that the teacher is also offered by the Ministry of Education 100 AFG for each colleague that he/she reports as absent. In this case, what is the right thing for the teacher to do?

Why?

[...]