Grain Inflation: Identifying Agent Discretion in Response to a Conditional School Nutrition Program[1]

December 2009

Gauri Kartini Shastry
(Wellesley College)

Leigh L. Linden
(Columbia University)

Abstract:

Many incentive programs rely on local agents with significant discretion to allocate benefits. We estimate the degree of discretion exercised by teachers within a conditional transfer program designed to encourage student attendance. The program allocates grain to students every month their attendance exceeds 80%, creating an incentive for teachers to inflate attendance to award certain students the grain. We find that teachers do manipulate students' records, changing the incentives to attend school faced by these children. These changes also vary for different students. Teachers inflate more for girls, better students, and students from lower castes, but less for Muslim students. JEL Codes: H,I,O.

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[1] Correspondence: gshastry@wellesley.edu. This project could not have been completed without the assistance of many people. We thank Marc Shotland, Mukesh Prajapati, Rajashree Kabare, and Nandit Bhatt for their assistance in collecting the data used in this study. Pratima Bandekar provided particularly valuable assistance both by helping with the collection of the attendance data but also by conducting many of the informational interviews needed to better understand the context of the program. We are also indebted to Abhijit Banerjee, Shawn Cole, David Cutler, Esther Duflo, Caroline Hoxby, Michael Kremer, Sendhil Mullainathan and seminar participants at the University of Virginia, Virginia Commonwealth University and Wellesley College for their assistance and invaluable suggestions. We are particularly indebted to Esther Duflo and Abhijit Banerjee for providing us with the data used in this paper. All errors are, of course, our own.

I. Introduction

The efficient provision of social assistance and transfers to the poor hinges on the ability of governments to target those in need of aid. One common proposal to improve such targeting and reduce the cost of gathering the necessary information is to decentralize the task of allocating such transfers among potential beneficiaries to local agents. These local agents are likely to have more and better information about potential beneficiaries, allowing them to more effectively target those who need the service. For example, these agents may be able to determine which beneficiaries genuinely suffered adverse shocks or which beneficiaries will respond least to moral hazard. However, agents can also be motivated by private incentives that are orthogonal to those of policy makers. For example, agents motivated by personal prejudices could discriminate against particular types of students or extract rents from vulnerable beneficiaries. In this paper, we study how local agents (teachers) use their discretion to allocate the benefits of school nutrition program in Mumbai, India. We find that these teachers exercise significant discretion, strategically manipulating official records to award benefits to children who failed to meet the programs' formal requirements.

There is a substantial literature looking at the relationship between decentralization and poverty alleviation.[2] Various arguments regarding decentralization include differences in cost efficiency, ability to target benefits, accountability and resource capacity. In this paper, we focus on whether and how local agents use the discretion a decentralized system affords them.[3]

The program we study, a school nutrition program, is intended to improve the performance of primary education in India. Increasing school participation in poor countries requires an increase in the number of primary schools, but also policies to ensure children attend regularly. Policy makers and researchers have tried to encourage investment in education by reducing the cost of attendance or increasing the benefits through direct incentives.[4] While many studies have evaluated these policies, relatively little research has focused on how they work.[5]

[2] See Bird and Rodriguez (1999) and Klugman (1997) for two reviews of this literature.
[3] Alderman (2002) and Coudouel et al (1998) study specific poverty programs in Albania and Uzbekistan, respectively, to demonstrate that social assistance is better targeted when local agents are responsible for the allocation. On the other hand, Das (2004) demonstrates that subsidies to local governments distributed with clear guidelines and legislated rules reach the targeted schools, while discretionary subsidies do not.
[4] For studies on reducing the cost, see, among others, Kremer, Moulin, and Namunyu (2003), Miguel and Kremer (2004) and Bobonis, Miguel, and Sharma (2002). On the benefits side, see, among others, Vermeersch and Kremer (2004), Kremer, Miguel and Thornton (2005), Schultz (2001), Gertler (2004).
[5] Recent exceptions include Barrera-Osorio, Bertrand, Linden, and Perez (2007), Fernald, Gertler, and Neufield (2006), and Paxson and Schady (2007) among others.

Since many of these programs rely on teachers and administrators to enforce the rules of the incentive schemes, the manner in which they use their discretion should be taken into account when designing these schemes.

Unfortunately, the degree to which agents manipulate incentive schemes is difficult to identify empirically because typically the only information on student behavior available is that provided by the local agents. One option is to look for unusual spatial and inter-temporal correlations in reported student behavior. Jacob and Levitt (2002) use this method to demonstrate that about 4-5 percent of US teachers cheat on standardized tests each year. Our approach, instead, is to collect independent data on student behavior and compare it with official records.[6] We exploit a unique dataset that contains overlapping observations of student behavior. One measure, recorded by teachers, determines the allocation of the benefits of a conditional nutrition program while the other, collected by external monitors, has no bearing on student or teacher well being. This strategy allows for a more precise and direct analysis of teacher behavior. In particular, we are able to identify misreporting when the prevailing incentives elicit similar patterns from teacher misreports and true student behavior. Relying only on reported information, we would not be able to distinguish between a student responding to the nutrition program and a teacher revising the official records in order to allocate the benefit to the student. With the data we have, however, we are unable to conclusively determine whether the response on the part of teachers is welfare-improving or -reducing.

Specifically, we evaluate teacher behavior within a conditional grain distribution program in Mumbai, India. The program distributes 3 kilograms of uncooked rice to the families of students with attendance rates of at least 80 percent in a given month. Similar to most conditional transfer programs in education, eligibility for the program is determined solely by the teachers' attendance records, allowing teaches to manipulate who does and does not receive the benefit by falsifying those records. Since this falsification changes the attendance rates required of the students, it alters the incentives of the overall program.

We have access to two years of official teacher-taken attendance records and the records of an independent monitor hired to visit the classroom once a week, allowing us to directly compare the records. Our results suggest that teachers do, in fact, use their discretion to inflate

[6] Martinelli and Parker (2009) use a similar strategy to study a program that allows individuals to report the information themselves. They find widespread underreporting in order to qualify despite stigma motives for overreporting.

certain attendance records. As a result, students who do not appear to earn the grain based on the formal rules of the program often receive it anyway. At least 40% of the students (6079 out of 15519) in our sample received the grain at least once when they did not appear to deserve it.

It is important to note that teachers also face other incentives to inflate attendance, which may explain the discrepancies in monthly attendance rates. To ensure that these are not driving our results, we exploit variation in the benefit of misrepresenting attendance that could only result from the grain program's monthly threshold. We first describe a dynamic model to identify the benefit to inflating a particular child's attendance on a given day.

We provide four pieces of evidence to suggest that teachers respond to the incentives to misrepresent attendance created by the grain program. First, we use aggregated data to distinguish the patterns in the data from random measurement error and graphically demonstrate that teachers consistently move children from around 80% to above 80% attendance. Second, we use monthly attendance rates to show that children often receive the grain when their attendance patterns suggest they did not earn it. Third, we use daily attendance comparisons between the teachers' and monitors' records to show that teachers respond to variation in the incentives to misrepresent attendance within each month. For example, they are more likely to misrepresent attendance for children who are still in the running for the program and misrepresent attendance more towards the end of the month when they know which children they can assist.

Finally, we find suggestive evidence that teachers inflate attendance more for certain students, indicating that this behavior creates different incentives for individual students. Among children not eligible for the grain, Muslim children are 7% less likely to receive it, boys are 3.5% less likely to receive it and children with one standard deviation lower test scores are 2.5% less likely to receive it. We also find some evidence that children of higher castes are 6-9% less likely to receive the grain when they have not earned it. [7]

It is important to note that the ultimate effects of such behavior are ambiguous. First, assuming the benefits reach the children, this behavior may improve their nutritional status, but it reduces the incentives for these children to attend school in the future. Teacher behavior undermines the program's primary goal to increase attendance. Second, allowing agent

[7] It is important to note that differential treatment by student characteristics alone is not evidence of discrimination. For example, at least one teacher noted that Muslim students in her school tended to be better off financially than other students. However, in contexts where the potential for discrimination is high, there is a significant concern that teachers may allow their personal prejudices to influence how they change the incentives faced by individual groups of students.

discretion may improve the targeting of the program since girls and children of lower castes have traditionally been discriminated against in the Indian school system. Similarly, teachers may reward effort by excusing absences for students with higher test scores. However, such discretion comes at a price in an area with ethnic or religious tension. We demonstrate that this behavior exists and that it must be taken into account in the design of the program.

The remainder of the paper is as follows. In section II, we describe the details of the grain distribution program. Next, in section III, we describe the unique dataset that allows us to view both actual attendance and teacher-taken attendance. In section IV, we discuss a dynamic model of the incentives teachers face on a given day. Section V describes the empirical specifications we use to demonstrate that teachers respond to the grain incentives. In section VI, we describe the empirical results using aggregate and monthly data and in section VII, we present empirical evidence using daily attendance records. Finally, in section VIII we extend the empirical results to examine how teachers respond to student characteristics. We conclude in section IX.

II. The Grain Distribution Program

The conditional distribution of grain is part of the National Programme of Nutritional Support to Primary Education (hereafter NSPE) created in 1995. The NSPE mandated the provision of cooked food to primary school children all over India. Initially, states had significant discretion in how the program was implemented, and due to a lack of sufficient funds, the program was often implemented as the distribution of bags of uncooked rice to parents whose children achieved attendance rates greater than or equal to 80 percent. This program operated in Mumbai in this conditional form during the period of our analysis, but starting in 2005, the national government required all municipalities to provide cooked midday meals.

The Mumbai grain program targeted the poorest households by distributing grain through public municipal schools, where these families sent their children. All children in first through fifth grade were eligible and there was no explicit poverty target. The program was administered monthly: a child with 80% attendance in a month earned one bag of grain that contained 3 kilograms of uncooked rice (Mumbai Interviews 2005).

Attendance records were individually compiled by teachers at the beginning of each day. Each teacher was provided with a single sheet of paper that listed each registered child with space to record the child's attendance on each day of the month. Any consideration of a student's attendance would most likely be a split second decision made at the beginning of the day.

Every month, principals gathered attendance records from the teachers, calculated the number of children who had at least 80% attendance that month and gave the government the attendance records with a request for the correct number of bags of grain. Every three months, an administrator would examine these attendance records. If the forms were properly completely, the administrator would give the principal the bags of grain. Teachers then asked parents to come pick up the bags. Parents would pick up one, two or three bags of rice depending on the number of months their child had at least 80% attendance. In L Ward, the district of Mumbai where our data was collected, the system seemed to run smoothly. According to the interviews, principals almost always got the number of bags they requested (Mumbai Interviews 2005).

Auditors were responsible for periodically visiting the schools and inspecting attendance records, but this did not seem to have happened often. When auditors did come, the principal would be responsible for any discrepancies, but this would be limited to reconciling the bags of grain with the attendance records. The auditors had no way of verifying the attendance of the actual children except by observing suspicious peculiarities in the recorded attendance patterns (such as a significant number of children with exactly 80 percent attendance). With such limited monitoring, teachers could manipulate attendance with significant discretion.[8]

Interviews with teachers suggest that this did, in fact, occur (Mumbai Interviews 2005). Teachers were reluctant to admit manipulating records themselves (though one did), but generally suggested that those who did were responding to the extreme poverty in which their students lived. While dishonest, the action was characterized as a compassionate response to a difficult situation and there were no reports of teachers demanding kick backs. This is supported by the fact that teachers earn much more than the parents and the quality of the grain distributed was much lower than that teachers would normally buy.[9]

[8] Recall that teachers may inflate students' attendance records for reasons other than the grain. To distinguish between general misreporting and grain-specific misreporting, we will test for patterns we would expect only as a result of misreporting for the grain and not as a result of general misreporting.

[9] Comparing the two attendance records also provides support against corruption. If teachers intend to extract rents from parents in exchange for awarding their children the grain, we would expect them to concentrate this behavior among families that are amenable to such negotiations. Our data suggest, instead, that almost 70% of the children who receive the grain without appearing to deserve it in at least one month do not fall into this category in any other month. Only 7% of those who receive the grain without earning it do so more than twice. Similarly, having received the grain in a previous month makes a child 60% more likely to receive the grain in February (the last month of our data), but having received it without earning it in a previous month makes a child 3% less likely to receive the grain.

III. Data and Summary Statistics

The uniqueness of our data allows us to compare two overlapping records of student attendance. The data were collected during an evaluation of a remedial education program in Mumbai run by the Pratham Mumbai Educational Trust (Banerjee, Cole, Duflo, and Linden, 2007)[10]. As part of the study, we acquired all attendance records taken by teachers in the third and fourth grades in L Ward during the 2001-02 and 2002-03 academic years; these data were used to allocate the grain. However, due to widespread concern that teachers significantly over-estimated the attendance of students, we also hired a team of independent surveyors who visited each class in our sample once a week on a randomly chosen day and time to record attendance.

The attendance data from the teacher's official rosters (hereafter the 'roster' attendance data) include the name of each child who ever attended the class during either the 2001-02 or 2002-03 academic years and the student's recorded attendance for every day that the school was open. The Pratham-collected data (hereafter the 'monitored' attendance data) includes the same student identification information along with the dates the class was observed and the students who were present during the visits. In addition, the dataset contains student-specific data such as age, gender and scores on math and language tests administered at the beginning of the school year. We also know the language of instruction for each school.

The dataset does not contain the religion of each child, but there is a close correspondence between name and religion in India. Classifying the student's name by religion reveals that almost all the students in schools taught in Urdu are Muslim, while there are Hindu, Jain, Christian, Muslim and Sikh students in non-Urdu schools. We also classified student names by caste between Brahmin, Kshatriya, Vaishnav and Shudra in descending order of status, but are only able to classify 22% of students.

The sample contains 77 schools with an average of two classes per school in the first year (for grade 3) and four classes per school in the second year (two classes for grades three and four each), resulting in data from 436 teachers on approximately 15,000 children. Attendance monitors recorded data from September to February each academic year. This includes almost the entire school year, but excludes August when student enrollment is extremely low as students begin to come back to school and March when students are taking end of the year exams.

[10] The remedial education program was shown to have no affect on attendance, and even if it had, it would not have caused the monthly attendance patterns observed in this paper.

Table 1 presents summary statistics. Panel A summarizes student characteristics such as age, religion and caste. Panel B describes the monthly attendance rates. We have data on approximately 75,000 student-months. The average roster attendance rate was 90.5%, while the average monitored attendance rate was only 85.9%. Note that we do not have accurate measures of monthly attendance rates taken by the monitors since monitors only visit a classroom 3 to 5 times a month depending on how many weeks school was in session. Around 87% of children attended more than 80% of the month according to the roster data, but we estimate that only 72% of children reached that attendance rate from the monitored data. Of all student-months, 20% of those with recorded attendance greater than 80% had monitored attendance below 80%; in other words 20% of student-months awarded the grain did not appear to have earned it.[11]

Panel C presents summary information on the variables in the model and empirical specifications described in section IV. This data is measured at the student-school day level but only for days on which the student was absent when teachers have the choice of whether to overstate his attendance. Overall, teachers misrepresent attendance 43% of the time on average, but the tendency varies by teacher; one teacher in our sample marked 125 student-days incorrectly present (out of an observed 128 absences) while another never exaggerated her students' attendance (out of an observed 57 absences).

The large number of cases in which a teacher erroneously records an absent child as being present provides the first piece of evidence pointing towards systematic misrepresentation. Compared to the large number of cases in which students are recorded by the teacher as present while the monitor records the child as absent (43% of monitored absences), there are only a small number of cases in which the opposite occurs (2% of monitored presences). The bias is clearly towards exaggeration and not understatement. This is not evidence of teachers manipulating records in response to the grain program specifically, but it does illustrate the feasibility of doing so. Also for this reason, we will use 'misrepresent attendance' to refer only to marking a child present on a day he is absent unless otherwise noted.

Finally, the method of data collection raises a possible concern. First, because the attendance monitors are directly visiting the teachers' classes, there is the possibility that the measurement strategy may have caused teachers to be more cautious than they would have

[11] In section VI, we estimate how much of this discrepancy could be due to sampling error from the small number of monitored days.

otherwise been (a type of Hawthorne effect). The very fact, however, that teachers still mis-record a large fraction of absences (43%) belies this concern. We test for this by comparing teacher-taken attendance records on days the monitor visited to teacher-taken attendance records on days the monitor did not visit. The difference in attendance is miniscule: a student is 0.035% more likely to be recorded as present on a monitored day, with a p-value of 0.19. In addition, the data in the first year was collected at the end of the academic year which made it impossible for teachers to know in advance that the attendance data could be used to scrutinize their records (we find similar effects using data from each year).[12]

A related concern is the timing of the monitor's visit. Monitors could have arrived after a child had left school early, or similarly could have arrived before a tardy student. In either case, we will erroneously record the teacher as having misreported attendance. This effect would only add noise to the regressions since it should not vary with the incentives of the program, but even so, we do not believe this is a significant concern. First, students only attend school for 4 hours a day, making it unlikely that a child would only come for a fraction of the data or that they would be missed by the monitor if they attended. Second, attendance monitors varied the time they arrived to take attendance and usually adjusted these times in response to the school schedule. Note that this source of noise might vary with the incentives of the grain program if students respond to the grain by coming to school only to leave early. In this case, it is important to realize that teachers are still using their discretion to not mark the child as having left early. The goal of the program is make children attend the entire day of school, not just arrive in time to be marked present and then leave. One could argue that allowing children to leave early is less of a concern than marking them as present incorrectly. However, for this to drive the discrepancies in our data, it must happen quite frequently (teachers misrepresent attendance 43% of absences). At this frequency, even allowing children to leave early undermines the program.

IV. Theoretical Framework

The primary goal of the model is to allow us to distinguish between grain-based incentives to misrepresent attendance and other explanations for the discrepancies. The disproportionate number of cases in which teachers report a student erroneously present rather

[12] Even if the teachers did believe that the monitors' data would be used to scrutinize their records, this would simply add a uniform cost to mis-recording absences. This cost should not covary with the incentives arising from the grain distribution's monthly threshold.

than absent argues against random measurement error, but teachers do face general incentives to inflate student attendance. Teachers, for example, may be unwilling to report low attendance rates to the principal and school administrators or teachers may inflate attendance to assist children in meeting their annual attendance requirement. Note that these other incentives are rather weak in general. First, teachers are rarely held accountable for their own attendance; it would be surprising if they were held responsible for their students' (Kremer et al., 2005).[13] Second, teachers report significant flexibility in helping children meet matriculation requirements. If a child attended too infrequently to be promoted, there was enough subjectivity in other parts of the promotion decision to compensate.

To differentiate between these incentives and the grain program, we exploit the fact that only the grain program creates incentives to misrepresent attendance that vary across the month and that depend on a child's previous attendance that month. In the next section, we show that the grain program generates a specific set of testable hypotheses that relate a teachers' decision to mis-record an absence on a given day to incentives that change over the course of a month. The uniqueness of this monthly pattern allows us to causally attribute it to the grain program rather than the other incentives to inflate attendance. In what follows, we develop a model of teachers' incentives within the program and then lay out a series of empirical specifications that allow us to test for the relationships suggested by the model.

A. A Dynamic Model of Teacher Behavior

The value of misrepresentation from the grain program, derives solely through securing the grain for a child by making the child's records appear as if the child has 80 percent attendance in a given month. This could be a warm glow from having given the child the grain or a bribe from the parents, or it could also be some peace of mind since parents of children who do not receive the grain often complain. We assume a constant cost to the teacher of mis-representing attendance. The cost could be the psychic cost of lying, a probability of getting caught by principals or administrators, or a reputational cost: if students stopped attending school

[13] We will include teacher-day fixed effects in most of our specifications to take out any variation that might be specific to how many children are presently in the classroom – this should account for any human measurement error that might be related to a larger number of students present or any embarrassment that might be related to fewer students present on a given day.

because they knew their teacher would ensure that they received the grain anyway, this may reflect negatively on the teacher.

Consider the problem faced by a teacher trying to decide whether or not to mis-record an absent child as present on a given day. The value of inflating a child's attendance depends nonlinearly on the number of days left in the month, the student's roster attendance this month, the teacher's expectation of the child's future attendance this month and interactions of variables derived from these three. Early in the month, children whose attendance patterns will put them below 80 percent may make up their deficit, rendering an early misreport possibly wasted. Closer to the end of the month a teachers' mis-representation is more likely to be decisive. How close a teacher can wait until the end of the month depends on the child's attendance rate. For children who have not attended at all in a given month, teachers must start mis-recording attendance within the first twenty percent of days in the month to keep the child eligible for the grain and misrecord many of the future days. For children on the cusp of meeting the 80 percent requirement, teachers can wait until the very last day to inflate attendance. Finally, for children in between these extremes, exactly how long a teacher can wait depends on how likely the child is to attend school in the remainder of the month. Students with high attendance rates are unlikely to require assistance allowing teachers to wait to use their discretion while poorly attending students would require early intervention to keep them eligible.

We posit a dynamic model of teacher behavior, where the teacher has to predict not only the child's attendance on future days in the month but also her own behavior. Imagine it is the second to last day of the month, a child is absent and the teacher must decide whether to misrepresent his attendance. From past experience, she predicts the probability that he attends on a given day is a_t; from her records, she notes that he has been marked as present p_t times prior to today. The subscript t denotes the day of the month, from 1 to T. Since there are only two observations left this month, she should not exaggerate his attendance if p_t is less than or equal to $0.8T-3$, because he has no chance of receiving the grain, or if p_t is greater than or equal to $0.8T$, because he is already guaranteed the grain. If p_t equals $0.8T-1$, she can guarantee him the grain by misrepresenting the child's attendance today or by waiting until the next day and misreporting his attendance if he is absent then. Since there is a cost to inflating attendance and waiting allows for the possibility that the child will be present, she should take the second option.

She does not need to misrepresent today's attendance since she will have the option to do so tomorrow. On the other hand, if p_t is equal to $0.8T-2$, the teacher must misrepresent his attendance today and if he is absent tomorrow, misrepresent it then as well, if the child is to get the grain,. If she does not do so today, he is disqualified. Earlier in the month, the teacher makes a similar calculation with the additional condition that the expected number of times she will have to misrepresent attendance to guarantee the child the grain may be too costly.

To model the entire month, we build a system of finite Bellman equations specifying a value function that depends on the child's attendance, allowing the teacher to misrepresent attendance if the child is absent. Let c be the cost of exaggerating a child's attendance on a single day and let G be the benefit to the teacher if a child receives the grain that month. Finally let β be the discount rate. In the calculations that follow, we take the discount rate to be 1 since the length of a period is only a single day.

The Bellman equations for period T, the last day of the month, are straightforward. If the child is present, the value function is simply whether the child will receive the grain after taking into account that day's observation. If the child is absent, the value function measures whether the child receives the grain, allowing the teacher to misrepresent that day's attendance for a cost. Using superscripts P and A for present and absent, the value functions can be written as follows:

$$V_T^P(p_T) = V\left(p_T + 1\right)$$

$$V_T^A(p_T) = \max_{m_t \in \{0,1\}} \left\{-cm_T + V\left(p_T + m_T\right)\right\}$$

$$\text{where } V(p) = \begin{cases} G & if \ \dfrac{p}{T} \geq 0.8 \\ 0 & else \end{cases}$$

where p_T is the number of past days the child attended school according to the roster and m_T is a choice variable that indicates whether the teacher misrepresents the child's attendance.

On any previous day, the teacher must consider whether the child will attend school in the future, what costs she will incur misrepresenting the child's attendance and whether the child will finally receive the grain. If the child is present on a given day, the Bellman equation is equal to the discounted expected value of the same equation for the following day; 'expected' because it depends on whether the child is present the following day. If the child is absent, the teacher gets to choose how to record his attendance, which impacts the parameters at time t + 1. Thus, the value functions can be written recursively as follows

$$
\begin{aligned}
V_t^P(p_t, a_t, d_t) &= \beta E\left(V_{t+1}\left(p_{t+1}, a_{t+1}, d_{t+1}\right)\right) \\
&= \beta a_t V_{t+1}^P\left[p_t + 1, (a_t d_t + 1)/(d_t + 1), d_t + 1\right] + \beta(1 - a_t) V_{t+1}^A\left[p_t + 1, (a_t d_t + 1)/(d_t + 1), d_t + 1\right] \\
V_t^A(p_t, a_t, d_t) &= \max_{m_t \in \{0,1\}} \left\{-cm_t + E\left(V_{t+1}\left(p_{t+1}, a_{t+1}, d_{t+1}\right)\right)\right\} \\
&= \max_{m_t \in \{0,1\}} \left\{-cm_t + \beta a_t V_{t+1}^P\left[p_t + m_t, a_t d_t/(d_t + 1), d_t + 1\right] + \beta(1 - a_t) V_{t+1}^A\left[p_t + m_t, a_t d_t/(d_t + 1), d_t + 1\right]\right\}
\end{aligned}
$$

where t takes on any integer value in [1, T-1], a_t is the teacher's updated perception of the probability the child attends school, d_t is the number of days elapsed in the year and p_t is as defined above, but on day t. We keep track of d_t in order to update the attendance probability, a_{t+1}. Note that the parameters at time t + 1 (p_{t+1}, a_{t+1} and d_{t+1}) differ between the two equations.

Denote $\tilde{V}_t^A(m; p_t, a_t, d_t)$ to be the absent value function for a given m. This provides a convenient representation of the value of misrepresenting attendance when a child is absent. We define the incentive to inflating attendance on day t, I_t, as the difference between the value function when the teacher exaggerates the child's attendance and when the teacher does not.

$$
\begin{aligned}
I_t(p_t, a_t, d_t) &= \tilde{V}_t^A(m = 1; p_t, a_t, d_t) - \tilde{V}_t^A(m = 0; p_t, a_t, d_t) \\
&= -c + E\left(V_{t+1}\left(p_t + 1, a_t d_t/(d_t + 1), d_t + 1\right)\right) - E\left(V_{t+1}\left(p_t, a_t d_t/(d_t + 1), d_t + 1\right)\right)
\end{aligned}
$$

Solving the model in closed form is straightforward but tedious due to the large number of cases; instead we solve the model numerically.[14] After first discussing the decision rule and the variation in the incentives to misrepresent attendance that arise from this model, we will describe the empirical specifications used to test for these patterns in the data.

Note that student behavior is an integral part of the teacher's problem; the teacher must predict whether the child will attend school in the future. We do not explicitly model the reputational cost, which could result in a response of the child's attendance patterns to the

[14] We leave a structural estimation of this model to future work, focusing here on the implications of the model.

teacher's behavior itself. To justify this modeling decision, however, we do not need to assume that students do not respond to whether their teachers misrepresent attendance. The assumption that teachers do not anticipate this is sufficient, but also not necessary. Since all specifications will include teacher fixed effects or student fixed effects to account for unobservable variation in the cost of misrepresenting attendance, we simply have to assume that this reputation cost does not vary across different days we see the same student.

B. The Decision Rule and Additional Variation in the Incentives

Following the intuition above, teachers should only misrepresent attendance on days when the student needs that day's attendance and perfect future attendance in order to earn the grain: I_{T-n}, the incentive on day T – n, is positive only when the child needs exactly n+1 days of future attendance, including that day, to make the threshold. If the child needs fewer than n+1 days, the teacher should wait, allowing for the possibility that the child attends enough that the teacher will not have to adjust his attendance. If the child needs more days, the child has already been disqualified. However, requiring n+1 days is not a sufficient condition. Early in the month and for a sufficiently large cost, c, the teacher may anticipate having to inflate attendance for a particular child too many times that she deems it too costly to award him the grain.

There is a fair amount of variation in I_{T-n} besides this straightforward decision rule. For ease of exposition, we divide the large number of possible situations into four cases. The first is the only case in which I_{T-n} can be positive: when the student needs exactly n+1 days of future attendance to make the 80% cut-off. In this case, variation in I_{T-n} comes from the expected number of exaggerations necessary to guarantee the student the grain. The higher the number of future exaggerations necessary, the smaller the net benefit to the teacher of having awarded the child the grain. Thus, in this case, I_{T-n} is decreasing in the number of days left in the month and increasing in the teacher's perception of the child's future attendance rate, a_t.

In the second and third cases, I_{T-n} is negative but does not vary with the number of days left or the child's attendance rate. The second occurs when the student needs more than n+1 days of future attendance to make the 80% threshold. The third case occurs when the student has already passed the 80% threshold for the entire month (note: this can only happen close to the end of the month).

The fourth and final case is when the student needs fewer than n+1 days of future attendance to make the 80% threshold. I_{T-n} is negative because teachers should wait until the child needs their help, but it is often greater than $-c$ since the child may eventually need the teachers help to receive the grain. In this case, the relationship between I_{T-n} and a_t has an inverted-U shape. This derives from the fact that an increase in a_t increases the likelihood that the child could have earned the grain on his own. At low values of a_t, an increase in a_t renders it less wasteful to misrepresent attendance, increasing I_{T-n}, because the child is more likely to earn the grain (it is not as costly for the teacher to guarantee the child the grain). At higher values of a_t, an increase in a_t renders it more wasteful to misrepresent attendance since the child may have earned the grain on his own. Appendix A elaborates on these implications from the model.

V. Empirical Specifications

To test for responses to these grain-related incentives, we use several empirical specifications. These specifications allow us to test four sets of hypotheses. First, we use the aggregated data to distinguish the observed pattern of mis-recordings from measurement error and then graphically depict the pattern of mis-representation over the course of the month, showing that teachers seem to be consistently moving children from around 80 percent to above 80 percent attendance. Second, analyzing the data by month, we show that teachers do, on average, award the grain to students who should not receive it. Third, within each month, we show that teachers misrepresent attendance more for children who are still eligible for the grain and at the end of the month, when they know which students they can easily push over the threshold. Importantly, we relate the teacher propensity to lie about student attendance on a daily basis to the various cases described in the model. Finally, we show that the propensity of teachers to exaggerate attendance is correlated with certain student characteristics, suggesting that teachers are relaxing incentives for preferred students.

In Section VI, we use the aggregate monthly data to show that children receive the grain despite having low monitored attendance rates. The most basic test is to examine monitored and roster attendance rates for large discrepancies:

$$\Pr(gotgrain_{ijm} = 1) = f(\alpha + \beta \cdot monitoredattendance_{im} + \nu_{jm} + \varepsilon_{ijm}) \qquad (1)$$

where $gotgrain_{ijm}$ is an indicator for whether child i received the grain from teacher j in month m, $monitoredattendance_{im}$ is a measure of child i's monitored attendance in month m, and ν_j is a teacher-month or student fixed effect. The teacher-month or student fixed effects ensure that these results are not driven by teachers who always misrepresent attendance (even if just in certain months) or teachers who always misrepresent attendance for their favorite students. If teachers are following program rules, monitored attendance should explain most of the variation in when the grain is awarded. Since we do not observe the grain distribution itself, we proxy for $gotgrain_{ijm}$ with a dummy variable for whether the child's teacher-taken roster attendance rate that month was at least 80%. Recall that this strategy suffers from a type of sampling error: we only have noisy measures of monitored monthly attendance due to the small number of days per month the monitor visited the classroom. We run simulations to account for this.

Next we turn to the daily attendance data. It is important to note that the analysis of the daily choice to over-report student attendance has two distinct advantages. First, this allows us to search for monthly patterns to the teachers' behavior and for responses to the child's attendance that month. Since the within month incentives created by the grain program are unique from other incentives faced by teachers, responses can be attributed to the grain program. Second, the direct comparison of the teachers' and monitors' daily data allows us to distinguish the effects of the grain program on teacher behavior even when the program creates the same pattern of incentives for student attendance. The incentive to misrepresent attendance for a particular child on a given day if that child is absent is positively correlated with the incentive for that child to attend school. When we compare roster and monitored daily attendance records for an absent child, we look for teacher responses to the grain over and above the student's response.[15]

However, this approach has one caveat. To meaningfully compare daily attendance records, we must assume that teachers make the decision to misrepresent a particular day's attendance on that day itself. Since attendance records are turned in only at the end of the month, it would make sense for teachers to hide their behavior by spacing out the exaggerations throughout the month. Note that the monthly attendance comparisons above do not require this assumption. Regardless, we do not believe this is a significant concern. First, teachers take

[15] We do find monthly patterns in student attendance: children attend school more towards the end of the month and when they are more likely to earn the grain. For this reason, we focus on days a child is absent.

attendance in pen and by writing “A” or “P”, not by simply adding a check mark to an empty cell. Thus, revising a previous day’s attendance requires visibly altering the earlier record which would look suspicious if done often. Second, we believe the cost of misrepresenting attendance is small enough that teachers would not need to be secretive. Finally, the patterns observed in the data themselves belie this argument. If teachers did revise records retrospectively, it would look as if teachers misrepresented attendance randomly, and they would only have done so for students who eventually got the grain. We find both that they misrepresent attendance with varying propensity throughout the month and that they inflate attendance for some students who will eventually not get the grain. Only 86% of students whose attendance teachers are seen to misrepresent will end up receiving the grain.

We begin by examining each instance a teacher decides whether or not to misreport a child’s absence using the following specification

$$\Pr(misrepresent_{ijt} = 1) = f(\alpha + \beta_0 \cdot eligible_{it} + \nu_{jt} + \varepsilon_{ijt}) \qquad (2)$$

where $misrepresent_{ijt}$ is a dummy variable for whether teacher j exaggerates child i's attendance on day t (conditional on the child being absent on day t), and $eligible_{it}$ is a measure of whether the child is still eligible for the grain given his past attendance this month. We first use a broad measure of continued eligibility that includes all children who have not missed more than 20% of the month already. We next separate out students who have already earned the grain; if teachers are sophisticated about their decisions to misrepresent attendance for the grain, they should mark an absence accurately for a child who has already earned it.

Since there is little information on the cost of overstating a child’s attendance, we proxy for this cost with fixed effects at different levels: teacher-day or student. When we include student fixed effects, we also include month fixed effects. These fixed effects account for the possibility that some teachers may face lower costs than others on certain days or for certain students. These costs may be due to a psychic cost of lying, principals who monitor attendance, auditors from the district’s administration or the reputational cost described above. The teacher-day fixed effects ensure that these results are not driven by teachers who misrepresent attendance for many children on a given day because the teacher herself was absent. Similarly, the student fixed effects ensure that our results are not driven by teachers who misrepresent attendance only for certain students. The results are driven by teachers who misrepresent attendance for a particular child when it might help that child achieve the grain but not at other times.

Note that a response on the part of the teacher to a child's attendance rate in general is not sufficient evidence of a response to the grain: a child's attendance rate may affect the other incentives described above, such as advancement to the next grade, or the cost of such actions may depend on a child's attendance rate. However, teacher behavior in response to child attendance in that particular month, when controlling for the child's average attendance rate or a student fixed effect, strongly suggests a response to the grain.

If teachers are sophisticated, their propensity to exaggerate attendance should depend on how many opportunities the child will have to attend in the future. We estimate the following:

$$\begin{aligned}\Pr(misrepresent_{ijt}=1) = f\,(&\alpha+\beta_1\cdot eligible_{it}+\beta_2\cdot daysleft_t \\ &+\beta_3\cdot eligible_{it}\cdot daysleft_t+\nu_{jt}+\varepsilon_{ijt})\end{aligned} \tag{3}$$

where $daysleft_t$ is the number of school days left in the month.[16] We would expect β_3 to be negative since teachers should misrepresent attendance closer to the end of the month and this should be stronger for children who can still be helped.

Finally, we test the implications of the model described above by estimating:

$$\begin{aligned}\Pr(misrepresent_{ijt}=1) = f\,(&\alpha+\delta_1\cdot needsperfectatt_{it}+\delta_2\cdot alreadyearned_{it} \\ &+\delta_3\cdot wasteeffort_{it}+\nu_j+\varepsilon_{ijt})\end{aligned} \tag{4}$$

where $needsperfectatt_{it}$, $alreadyearned_{it}$ and $wasteeffort_{it}$ are indicator variables for the first, third and fourth cases described above, respectively. The decision rule and variation in I_{T-n} described above lead us to predict that δ_1 should be positive, since this is the only case in which I_{T-n} is positive (i.e. children need that day's record and perfect future attendance). δ_3 should be positive as well, since it is less costly to exaggerate attendance in this case (the child is likely to

[16] We use the number of days left instead of the number of days passed because the total number of school days in each month varies due to public and religious holidays.

earn the grain but not guaranteed to earn it) then in the reference category (the child is already disqualified). δ_2 should be zero since the cost of misrepresenting attendance when a child is already guaranteed the grain is assumed to be the same as in the omitted case. We include either teacher fixed effects or student fixed effects.[17]

We can also directly calculate an estimate for I_{T-n}. The model has four state variables: t, p_t, a_t and d_t. After discretizing the state-space, it is straightforward to solve the model recursively and calculate numerical estimates of important functions. We solve for the various value functions and I_{T-n} under different assumptions about the daily cost to misrepresenting attendance relative to the benefit of a child receiving the grain.[18]

To test whether teachers respond to how I_{T-n} varies with the child's previous attendance rate and the number of days left in the month, we estimate specification (4) including interactions of the three indicator variables described above with the child's past attendance record and the number of days left in the month.

A final way to demonstrate that teachers misrepresent attendance in response to the incentives created by the grain program is to estimate the following specification

$$\Pr(misrepresent_{ijt} = 1) = f(\alpha + \pi \cdot I_{T-n} + \nu_j + \varepsilon_{ijt}) \tag{5}$$

where I_{T-n} is taken directly from the numerical solution to the model. Recall that this calculation depends on the size of the parameter c; we will test the sensitivity of our results to the calibration of c. A positive π confirms that teachers misrepresent attendance in response to the grain, even when we control for teacher or student fixed effects.

VI. Empirical Results: Aggregate and Monthly Attendance Data

[17] We include teacher instead of teacher-day fixed effects in these regressions to allow us to test other predictions of the model, e.g. that the incentive to misrepresent attendance increases towards the end of the month.

[18] Recall that teachers must predict whether the child will attend school in the future. We estimate the teacher's perception of the child's future attendance rate based on a weighted average of monitored attendance and a prior expectation of attendance set at 80%. Our results are qualitatively robust to variations on this assumption, such as assuming a prior of 0 or 100%. This prior is to avoid giving a child a zero probability of attending school after observing one absence out of one observation and similarly for a probability of one. The weight on the monitored attendance is the proportion of the school year elapsed.

In this section we analyze the monthly attendance patterns in the data. One advantage to focusing on the data from this angle is that we do not need to assume that teachers make the decision about whether or not to misrepresent attendance on that day itself. The primary disadvantage is that we have only 3-5 days of monitored attendance records per month creating noise in our estimates of monthly attendance rates. We proceed as follows. First, we formally test the hypothesis that students receive the grain too often given their observed attendance patterns. Second, we plot the students' average attendance rates each month and show that there are too few students below and close to 80 percent attendance and too many above the cut-off. Finally, we use the parametric model in equation (1), to document that children are in fact receiving the grain when they should not have.

A. Aggregate Trends

As discussed previously, classes are quite large and with such young children, it is likely that teachers make mistakes. The summary statistics described above suggest this is unlikely to explain the large number of discrepancies. Teachers are much more likely to mark an absent child present (43%) than a present child absent (2%). It could be, instead, that teachers may fill in the attendance records at the end of the month, suffer from recall issues and employ a "when in doubt, mark present" strategy. The fact that 93% of the days we can compare match perfectly (171,067 out of 184,662) suggests this is unlikely as well.

Comparing the monthly attendance rates between the teachers' and the monitors' data suggests that teachers award the grain too often. Of the student-months that were awarded the grain, 20 percent were months in which the monitors' data recorded less than 80 percent attendance. Some discrepancy between the measures should be expected because there are only at most 5 monitored attendance observations for each month, and we would expect that just through random chance, the monitors would take attendance on days when qualifying students are absent. For example, in a month with 20 days, a child attending 80 percent of the time will be absent on 4 days. If monitors randomly visit on two days the child is present and three days the child is absent, the child will appear to not have earned the grain when in fact he had. So, the question is whether the observed discrepancy is too high to be due to such random chance.

Therefore, we calculate the distribution of this summary statistic under the null hypothesis that teachers record attendance accurately. Treating each child's monitored attendance record in a month as a binomial variable with 3-5 trials and probability of success equal to the child's teacher-taken attendance rate that month, we calculate the probability that a child whose teacher-taken attendance rate is greater than 80% attends fewer than 80% of the monitored days. Then, we treat whether this child deserved the grain as a Bernoulli random variable. Using the central limit theorem, we calculate the mean and standard deviation of this summary statistic to be 8.93% and 0.12%. It is therefore extremely unlikely that the 20% discrepancy in whether a child who received the grain actually deserved it is due to sampling error from how the monitor data was collected.[19]

B. Density Plots

The above summary statistics demonstrate that teachers inflate attendance records systematically. To look more closely at the distribution of monthly attendance rates, we plot kernel densities of the various records available in Figure 1. The solid line plots the smoothed distribution of monthly attendance rates calculated from the roster attendance, along with a 95% confidence interval created by bootstrapping the plot using a subsample of 10,000 with replacement (the intervals are very small). We use a Gaussian kernel and a bandwidth of 0.05; the shape of each plot is robust to different kernels and bandwidths, as well as the local linear density estimator (Cheng, Fan and Marron 1997). The dashed line plots the density of monthly attendance rates calculated from the monitored attendance. Compared to the teachers' records, the monitors' attendance rates show that the teachers record too few students as having just less than 80 percent attendance and too many having just more than 80 percent attendance.

While this is consistent with the incentives of the grain program, we need to be careful that this difference does not result solely from the discrete nature of the monitors' data. Because monitors only sample 3-5 days a month, there are only 11 possible attendance rates (0, 20%, 25%, 33%, 40%, 50%, 60%, 67%, 75%, 80%, 100%), and the data will naturally show masses at these points. The most straightforward way of gauging the severity of this problem is to reduce the observed teachers' data to the same level as the monitors'. We do this by only using three to five randomly chosen days of roster data so that the structure of the two data sets is identical,

[19] Another approach is to isolate cases where a child has missed enough of the monitored days that even if he was present all other days in the month, he could not have earned the grain. For example, if the monitor visited a classroom five times in a month with 21 days and the child was absent all five days, he would be disqualified for sure. Out of the 261 such instances in our data, 22 (8.4%) student-months received the grain anyway. This is a very weak lower bound on the extent to which misreporting affects the grain distribution.

making any remaining difference reflect only the teachers' behavior. This process is done for each month and repeated 100 times; the results are plotted as the dotted line in Figure 1, along with the 95 percent confidence interval. As expected, it show a mass right below 80 percent, but the plot of this simulated data is still well below the monitor's plot prior to 80 percent and above it just after 80 percent, suggesting that teachers are in fact "moving" children with true attendance rates below 80 percent over the cut-off by mis-recording their absences.

Another strategy is to use the opposite approach – instead of making the teachers' data more discrete, make the monitors' data more continuous. This is not possible on a monthly level, but if teachers have an incentive to exaggerate attendance by month, this will result in exaggerations by year. If we aggregate the monitored data to the student-year level, the underlying distribution of both the monitors' and teachers' data will be similarly continuous. Figure 2 plots the results of this exercise, including the 95 percent confidence intervals. As before, the dashed line is the monitors' data and the solid line is the teachers' data. As in Figure 1, the monitors' data suggests that teachers are reporting too few students at around 80 percent attendance and too many students above 80 percent attendance. The dotted line is again the results of the simulation on the roster data, and at this level of aggregation it is very similar to the teachers' roster data. This both validates the strategy used in Figure 1 and documents that the monitor data is sufficiently continuous that the only differences result from teacher behavior.

Note that the yearly attendance records do not display a discontinuity at 80%, confirming that the shape (but not the magnitude) of the monitored attendance arises from the discreteness of the data. There are many reasons we may not expect to see a break. First, while a break at 80% makes sense in the monthly data due to the monthly grain threshold, there is no reason to expect a break at 80% in the yearly data. Second, we would only expect a break at 80% in the monthly data if teachers calculate exactly which children are right below the threshold and adjust their attendance to push them above it. Since the average class has 50 students and teachers are busy with many administrative duties other than taking attendance, it is more likely that they just 'eyeball' a child's past attendance. In addition given a small discrete number of days in each month (varying from 16 to 26), it is numerically not easy to push a student from just under 80% to just over 80%. Similarly, if teachers are making their decisions each day and not all at the end of the month, there will be children the teacher cannot push above the threshold; an extreme example is a child who is absent the first 25% of the month and then attends the last 75%

continuously. Finally, recording too many students right above the 80% threshold may look suspicious and any general misreporting for reasons other than the grain makes it unlikely that we would observe sharp differences in reported attendance above and below 80% attendance.

Finally, we can also take advantage of the fact that some teachers never misreport the attendance of students and plot the pervious distributions separately for teachers that do and do not misreport attendance.[20] This is done in Figures 3 and 4 respectively. Figure 3 is almost identical to Figure 1. Figure 4, however, is not. The two plots suggest that the difference in magnitude between the simulated and monitored distributions comes from teachers who are seen to inflate attendance. Of course, these plots are merely suggestive, because teachers who were seen to misrepresent attendance are likely to differ from those who were not. Note that in Figure 4 the monitors' attendance distribution is identical to the simulated roster data which confirms both that these teachers are not inflating attendance (and not simply affected by the monitors' presence) and that the simulation process correctly approximates the monitoring data. Second, the shape of the curves is also identical to the shape of the simulated attendance data in both Figures 1 and 3, validating the use of these simulated distributions as a comparison distribution.

C. Empirical Regressions using Monthly Data

We can also analyze the monthly attendance records parametrically using equation (1). The results are presented in Table 2. Columns 1-5 estimate linear probability estimates while columns 6-7 estimate conditional logit models (we only show these with student fixed effects to save space – the results from other specifications are very similar). Columns 1-2 and 6 test the conditional correlation between whether a child earned the grain, as measured by his attendance rate from roster data, and the percent of monitored days the child was present. All three regressions suggest strongly, and encouragingly, that a child with greater monthly attendance is more likely to receive the grain. The student fixed effects ensure that our results are not driven by students who consistently receive the grain or consistently fail to receive the grain.

We expect the relationship between the attendance rate and receiving the grain to be positive, but the question is whether it is too small. To gauge this, we want to compare the observed coefficients in columns 1 and 2 with the coefficient we would obtain under the null

[20] Out of the 436 teachers, 360 are in the former group with approximately 13000 students and 46 in the latter with approximately 1500 students.

hypothesis that teachers are recording attendance accurately. This coefficient depends on the underlying distribution of students' attendance rates, but can be directly estimated by using the attendance rate from the teachers' roster data in equation (1) rather than monitors' data. The resulting coefficient is 1.45 and is presented in column 5. Based on the estimate in column 2, in a month where a particular child has a 10% higher monitored attendance rate, that child will be 6% more likely to receive the grain relative to other months. If teachers recorded attendance perfectly accurately and we had monitored data everyday, this statistic would be closer to 14.5%.

Unfortunately, this benchmark coefficient is, in fact, too large. As in the plots of the figures, the fact that we only have 3-5 observations of students' true attendance data each month means that we have a noisy estimate of the students' true attendance rate. Under the null hypothesis of accurate teacher reporting, this is a classic case of measurement error in the independent variable and we would expect the resulting coefficient to be biased downward due to attenuation. To separate out the issue of sampling, we follow the simulation procedure described in the previous section to re-estimate equation (1) under the null hypothesis that teachers record attendance accurately, but with the same number of observations each month as we have for the monitors' data. We bootstrap this estimate, along with the R-squared statistic, 1000 times to construct a 99% confidence interval and the results are presented in Panel B.

As shown in column 2, the coefficient on the monitors' attendance rates is much lower than what one would expect from simple measurement error alone. The mean coefficient in Panel B, is 0.88 and the estimate of 0.63 is lower than the lower bound on the 99 percent confidence interval. This means that the students' actual attendance rate – formally the only determinant of whether or not students should receive the grain – explains too little of whether or not the student gets the grain in a given month. Consistent with this, the explained variance of the regressions is too low – the R-squared statistic of 0.6 is also below the lower bound of the 99 percent confidence intervention of the bootstrapped R-squared statistic.

A similar strategy is to estimate how much of the variation in who receives the grain can be explained by an indicator variable for whether a child's monitored attendance rate is less than 80% (columns 3-4 and 7 in Table 2). This may, in fact, be a more accurate estimate if the relationship between receiving the grain and the child's true attendance rate were sufficiently non-linear. In this case, the true relationship should be negative one since falling below 80 percent attendance should disqualify the student from receiving the grain, but we must also take into account measurement error as before. We do this in the same way, and present the bootstrapped results in Panel B. These results also suggest that not deserving the grain significantly reduces the probability a child receives the grain; however, we see that while a child who does appear to deserve the grain earns it about 95% of the time (the constant), not deserving the grain only reduces this probability by 23-35 percentage points. From the confidence intervals presented in panel B, we can conclude that this discrepancy is unlikely to be due to error in our measure of the child's monthly monitored attendance. A child who would appear to not deserve the grain should be 39-50% less likely to actually receive it with 99% certainty. These results confirm that teachers respect the rules in the grain program enough to ensure that children below 80% are less likely to receive the grain, but suggest that the child's attendance alone does not determine whether a teacher awards the child the grain.

VII. Empirical Results: Daily Attendance Data

A. Comparing Daily Attendance Records

The results above indicate that in recording attendance inaccurately, teachers affect the distribution of the grain. We are able to rule out the possibility that the discrepancies in daily attendance records are due solely to random, mean zero, mistakes. However, the results above do not rule out the explanation that teachers tend to exaggerate attendance for reasons other than the grain program. To differentiate between these reasons, we look to see whether teachers misrepresent attendance more for children who are still eligible for the grain based on roster data and assess how these patterns vary over the course of the month. We do this by examining each instance a teacher decides how to record a child's absence. Panel C of Table 1 presents summary statistics for the measures of eligibility used in this section.

The results in Table 3 demonstrate that the teachers' behavior does respond to variation in the value of misrepresenting attendance due to the grain program, even though their behavior is not consistent with all the nuanced variation in the incentives created. Panel A tests whether teachers misrepresent attendance more for children who are still eligible for the grain, where eligibility is calculated based on roster attendance using equation (2). We estimate these regressions with linear probability models due to the number of fixed effects we want to include.[21] Columns 1-2 use a broad measure of eligibility including all students who have not been recorded absent for 20% or more days of the month before today.[22] Estimating specification (2) with either teacher-day or student fixed effects demonstrates that teachers are 12%-39% more likely to misrepresent attendance for children who are still eligible for the grain.

We next separate out students who have already earned the grain (i.e. attended enough days) in columns 3 and 4 from those who still need to attend school (have not missed more than 20% of the month but have not yet been present for 80%). If teachers are sophisticated about their decisions to misrepresent attendance for the grain, they should mark an absence accurately for a child who has already earned it. We find that teachers are 11-38% more likely to lie for a student when he or she still needs to attend school than when that same student is below this threshold (the omitted group). However, we find that teachers continue to exaggerate the attendance of children even after it is no longer helpful in awarding them the grain.

This latter result may be due to many factors. Teachers do, for example, have general incentives to inflate attendance. It may also be, however, due to the fact that teachers spend very little time recording attendance and are unlikely to calculate whether each child has achieved 80% attendance. In particular, since student-days can only ever be in this category close to the end of the month, this result could stem from a tendency to misrepresent attendance more at that time. In addition, if there is a cost to misrepresenting attendance, it is likely smaller for children who attend frequently; if anyone asks, other students and other teachers are likely to have seen that student around. Given the small cost of misrepresenting attendance, it is likely teachers do not believe it is worth their time to prevent some lies from being wasted on children who have already earned the grain. It could also be that these children have more than 80% attendance due to past instances when the teacher has exaggerated attendance for them this month; the teacher may be in the habit of misrepresenting this student's attendance. Note that such a habit could only explain the teacher's behavior in a given month, since the student fixed effects control for a general tendency for teachers to lie for particular students. To ensure that the results in columns

[21] Results are very similar if we use a conditional logit model, but we have to drop the month fixed effects in order for the model to converge.

[22] Note that if teachers attempted to give the grain to all children, then no child should ever have missed more than 20% of the month, rendering this test ineffective. However, this is unlikely given that approximately 40% of student-months do not earn the grain: while it is unclear how large the cost to misrepresenting attendance is, it is certainly large enough to prevent teachers from ensuring that all students receive the grain.

1-2 were not driven by children above the threshold, we re-estimate these regressions excluding those children (columns 5-6), and find similar results.

We next look for a monthly pattern to the teachers' decisions to misrepresent attendance. Columns 1-4 in Panel B estimate variants of equation (3).The first column shows that teachers are more likely to misrepresent attendance when there are fewer days left in the month. Columns 2-3 include a measure of eligibility and an interaction with the number of days left in the month; column 3 excludes children already above the 80% threshold. While both main effects are of the expected sign (teachers are more likely to mark absent children present when they are still eligible for the grain and closer to the end of the month), the interaction is not significant. In column 4, we find similar results when we include an indicator for the students who have already earned the grain.

Finally, in column 5 we include an additional measure of past roster attendance (whether the child has perfect roster attendance up to today) and interactions with the number of days left. In column 6, we include fixed effects for the number of days left to ensure that these results are not driven by nonlinearities in the monthly variation in incentives. We find that for a child who still needs to attend in order to earn the grain, perfect past attendance increases the teacher's tendency to exaggerate attendance. This makes sense because it is something teachers can scan easily. However, perfect past attendance does not affect teacher behavior for students who have already earned the grain. In addition, the interaction of eligibility and the number of days left is negative and significant, suggesting that teachers respond more to monthly variation in incentives for children who still need to attend. This interaction is not significant for children who have already earned the grain (even for those with perfect past attendance), although it is negative and large in magnitude. Surprisingly, the interaction with children who still need positive attendance but have perfect past attendance is positive and significant. One possible explanation is that teachers may be more willing to misrepresent attendance for children with perfect past attendance even earlier in the month.

All in all, the results in Table 3 suggest that teachers are more likely to misrepresent attendance when the incentives created by the grain program are higher, but they may not be very sophisticated in their decisions. It is likely the cost to recording attendance inaccurately is so low that teachers do not feel the need to economize their behavior precisely.

B. Within-Month Variation in the Grain Incentives

The tests and results described above provided evidence that teachers manipulate attendance in such a way that it affects the distribution of grain and that they respond to monthly variation in the incentives to misrepresent attendance that could only have come from the grain program. While most of the results confirm these conclusions, we can improve upon them by constructing an estimate of the gain from misrepresentation that is closer to the specification suggested by our model. The theoretical model described earlier in the paper gave us clear predictions about how the incentives to misrepresent attendance should vary with respect to the number of days left in the month, the number of days of attendance the child needs and the child's attendance rate. As described in Section IV, we construct an empirical estimate of the value derived from misrepresentation. Summary statistics for the variables associated with the dynamic model can be found in panel C of Table 1.

Table 4 presents the results from our first test of the model's predictions (specification 4). The first two columns test the prediction that the incentive to misrepresent attendance depends on an interaction between the number of days left in the month, n, and the number of days of attendance a child needs to earn the grain, r. The omitted category is the second case, when the child can no longer earn the grain, $r > n+1$.[23] Relative to this case, the benefit to misrepresenting attendance is theoretically indistinguishable in the "already earned" case (the third case, $r = 0)$. The benefit is greater in the other two cases: the first, "needs perfect attendance", where $r = n+1$ – the only case where the benefit is positive – and the fourth, "waste effort", where $r < n+1$. Recall that in this last case, the net benefit to misrepresenting attendance is negative but greater than in the omitted category since the teacher may simply be mistiming her actions. Columns 1 and 2 present the results from estimating specification (4). While teachers respond positively to the first and fourth case as predicted, they also respond positively to the already earned case. This is similar to the finding above that teachers continue to misrepresent attendance even for children who no longer need the assistance.[24]

Columns 3-4 include interactions with the number of days left in the month and control for the child's perceived attendance rate. Theoretically, a greater number of days left should reduce the teacher's propensity to exaggerate attendance, but only the first case. The main effects

[23] The +1 is because n does not include today's attendance record.

[24] Recall that we included teacher instead of teacher-day fixed effects in this table. The results in columns 1 hardly change when we include the further disaggregated fixed effect.

are unaffected by the inclusion of these other variables; teachers misrepresent attendance more for children in the first, third and fourth case relative to the second even with these controls. Teachers also seem to respond to the number of days left in the month even for children who are already disqualified from the grain, although they are more responsive to the number of days left in the month for the first case, as predicted. Finally, teachers misrepresent attendance more for children with higher attendance rates all else equal, as we would expect. Columns 5-6 include interactions with the child's perceived attendance rate. The results demonstrate that, as with days left, teachers respond to the child's attendance rate – they misrepresent attendance more for children with higher attendance rates – but that they do not discriminate correctly between the different cases. In the fourth case, the results confirm the parabolic relationship between the child's attendance rate and the incentive to misrepresent attendance.

These results confirm our conclusion that teachers misrepresent attendance intentionally and in response to monthly patterns that could only derive from the grain program. However, they do not respond precisely or in a sophisticated manner to all the nuances in the incentives to misrepresent attendance for the grain.

Another strategy for demonstrating that teachers respond to the nonlinearities in the incentives to misrepresent attendance is to explicitly calculate the net benefit to misrepresenting attendance, I_{T-n}. In Table 5, we estimate specification (5) where we demonstrate that teachers are more likely to misrepresent attendance for a particular child on a particular day when I_{T-n} is greater. The calculation of I_{T-n} comes from the numerical solution to the dynamic programming problem described above for each instance a child is absent. To perform this calculation, we must first calibrate the cost of exaggerating a child's attendance on a given day, c, and the benefit to the teacher if a child receives the grain, G. Because each period is only a day, we assume the discount rate is equal to one. Since the values of c and G are important only relative to each other, we normalize G to 1 and vary the cost. We consider conservative lower and upper bounds for c. Note that teachers are not willing to misrepresent attendance to make a child who rarely attends school eligible for the grain, suggesting a non-zero value for c. For an upper bound, note that teachers are seen misrepresenting attendance three times within a single month for some children. This suggests a cost of misrepresenting attendance of less than 0.33. We calibrate the cost to equal 0.3 and test how sensitive our results are to this assumption.

The top panel of Table 5 estimates this specification using teacher-day fixed effects while the bottom panel uses student fixed effects. These fixed effects ensure that the results are not driven by a teacher who misrepresents attendance for a particular child or for all children on a particular day. Both panels estimate a linear probability model, but the results are robust to employing a conditional logit model. Teachers respond strongly to variation in this net benefit to misrepresenting attendance and this result is not sensitive to our calibration of the daily cost. Recall that the incentive to misrepresent attendance comes from a highly non-linear relationship between three variables: the number of days left in the month, the number of days of attendance the child needs to earn the grain and the child's future attendance. Controlling linearly for these other variables (not shown) does not detract from the main effect; teachers respond to the nonlinearities. Since this variation could only come from the grain threshold, we conclude that teachers do misrepresent attendance in response to the grain distribution.

VIII. Do Teachers Assist Certain Students in Getting the Grain More than Others?

Once we establish that teachers respond to the grain program, the next natural question is if they use their discretion to help certain students more than others. In Tables 2-5, we tested for the joint significance of all student fixed effects and found them to be significantly different from zero in all cases. Including the student fixed effects explain more of the variation in when children receive the grain or when teachers misrepresent attendance, suggesting that teachers may misrepresent attendance for some children more than others. We explore this question by including demographic characteristics and interactions of these traits with key right hand side variables in the specifications set forth above. We include an indicator for a Muslim child in a non-Muslim school, dummy variables for caste, gender and age and a normalized score on a test in mathematics and language administered at the beginning of the school year.

There are two important points worth noting. First, any test of how student characteristics affect a teacher's decision to misrepresent attendance will potentially suffer from omitted variables bias since the characteristics are not independent of other factors correlated with a teachers' propensity to lie. In other words, if we demonstrate that teachers misrepresent attendance more for children of a particular caste or religion, this need not be due to the teacher's preferences for those children, but may be due to another unobserved characteristic of the student correlated with the observed characteristic. We will not, for example, be able to rule out whether parents in a particular group are more likely to harass (or bribe) the teacher into exaggerating their children's attendance or whether children in this group are richer or poorer. The fact that we find differential misreporting for girls and boys suggests that it is not the first reason: assuming parents send their daughters and sons to the same school, it is unlikely they bribe or harass the teacher for their daughters but not their sons. Similarly, as argued above, children for whom a teacher is seen to misrepresent attendance tend not to earn the grain in every month; one would expect those parents to continue to be the most likely to harass or bribe the teachers.

Second, the question of the causal relationship is largely moot. The question we need to answer to inform the debate on discretion is whether teachers use their *local* information, or information that is not available to the government, in a welfare-improving or reducing way. Many of the characteristics we include (religion, caste, age, gender) are potentially known by the government; if the government wanted to make it easier for children of a particular caste to receive the grain, it could have simply made the threshold for those children lower than 80%. If teachers are responding to these characteristics, it could be argued, these cannot be in accordance with the goals of the government. However, the government may not want to openly discriminate with different rules and may find it difficult to administer such a system. We provide these tests and describe the results since they are suggestive and indicative of the teachers' motivations. These tests demonstrate that the behavior of teachers and administrators within incentive schemes needs to be given more careful consideration.

Following Table 2, we first include student characteristics in the empirical regressions using monthly data (see Table 6). In column 1, we present the estimates from a regression of monitored attendance rates on student characteristics simply to provide summary information on attendance patterns of students of different religions, castes and genders. Some results are as we would expect – children who do better on a test administered at the beginning of the school year have higher attendance rates subsequently. Muslim children in non-Muslim schools have lower attendance rates than other students in their schools. Other results are more surprising: boys are less likely to attend school than girls. While children of higher castes tend to have lower attendance rates, these results are not significant. All regressions in Table 4 also contain age dummies; while some of them are significant, there is no clear pattern.

Columns 2-3 examine what factors affect whether a child earns the grain, according to roster attendance, conditional on measures of his monitored attendance rate. If the student's actual attendance rate was monitored perfectly for the entire month and if teachers were strictly following the 80% rule, student characteristics would not affect whether a child receives the grain. The p-values at the bottom of the table demonstrate that student characteristics have significant predictive power for which children receive the grain. Conditional on measures of monitored attendance, Muslim children (in non-Muslim schools) are less likely to receive the grain by about 1.7% and boys are less likely to receive the grain by about 1.3%. Again, children of higher castes are less likely to receive the grain, but these coefficients are not significant. Smarter students (as measured by the test score) are more likely to receive the grain; a child one standard deviation above the mean is 2% more likely to receive the grain.

Since student characteristics do matter, it seems likely that teachers are misrepresenting attendance differentially since it is unlikely that monitored attendance is systematically biased. Note that column 1 shows that monitored attendance patterns mirror which children are more likely to receive the grain. If boys are less likely to attend school in general, teachers may not misrepresent attendance for boys as much because they believe it is unlikely these boys will receive the grain and not because of a preference for girls. To examine this possibility, we include interactions of student characteristics and the dummy variable for whether the child does not appear to merit the grain (column 5) and student fixed effects (column 6). Column 5 separates out students who appear to earn the grain and those who do not. Among those who did not deserve the grain (the interacted regressions), both boys and Muslim children are still less likely to earn the grain by 3.4% for boys and 6.8% for Muslim children in non-Urdu schools. Children of higher castes are also less likely to receive the grain by about 6-9 percentage points. Finally, children with better pre-test scores are more likely to earn the grain even when they appear not to deserve it by about 2.4% per standard deviation.

In addition, the main variables indicate that among children who deserve the grain, those with higher pre-test scores and children of a middle caste are marginally more likely to receive it. This is surprising since teachers rarely mark present children absent, but could be due to error in the measure of monitored attendance rates. Another explanation is that some of the exaggeration is not for the purpose of helping children receive the grain or that teachers miscalculate. Column 6 rules out the possibility that these explanations are driving our main results by including student fixed effects. Our results regarding boys and children with better test scores remain, the coefficients on higher castes are larger in magnitude and more significantly negative, but the coefficient on Muslim children is no longer significant. These results confirm that even when we compare two months of attendance for the same student, whether he receives the grain when he is less likely to have earned it depends on his caste, gender, pre-test score and possibly religion.[25]

Finally, we next present results from estimating specifications (4) and (5) with student characteristics in Table 7.[26] The inclusion of these characteristics does not diminish the main effects of the grain-related variables. The student characteristics are usually jointly significant (see the p-values at the bottom of the table).; from the individual coefficients, we affirm our previous results that teachers assist girls and children with higher pre-test scores more than other children in receiving the grain.[27] These regressions include teacher-day fixed effects; we find similar results with teacher or teacher-month fixed effects (results not shown). The results are also robust to including additional measures of the grain incentives from Table 4 and for other calibrations of the daily cost from Table 5.

In this section, we have provided some suggestive evidence that teachers manipulate attendance records to help girls and children of lower castes receive the grain more than boys and children of higher castes. They are also more likely to help children who do better on tests administered at the beginning of the year. Teachers are certainly influenced by student characteristics (the main effects), but their response to the grain incentives are not very sensitive to these characteristics (the interactions, which are insignificant).

IX. Conclusion

In this paper, we studied two sets of attendance records to determine whether teachers misrepresent student attendance in response to the incentives created by a school meals program. We modeled teachers' responses to the monthly threshold that determines which children receive the free grain from the government and the results indicate that teachers do respond. They misrepresent attendance more where there is a greater likelihood that doing so will help the child receive the grain when otherwise he would not have. This behavior allows teachers significant power in determining which students receive the grain, since they choose how much to inflate a child's attendance. In turn, this affects the incentives faced by each child. We find that,

[25] Regressing the fixed effects estimated in all these tables on the student characteristics themselves consistently supports the finding that teachers misrepresent attendance more for children with higher pre-test scores and for girls.
[26] We also interact the estimates in Table 4 and find similar results.
[27] The student traits interacted with grain variables are not significant when we include student fixed effects.

conditional on actual attendance, teachers misrepresent attendance more for female students and for students who perform better on a test in mathematics and language at the beginning of the year. There is also evidence that teachers misrepresent attendance more for children of lower castes and less for Muslim students.

The potential importance of this behavior can be illustrated using the grain program itself. The Central Government of India created the NSPE to improve the nutritional status of school children as well as to increase school attendance rates among poor children – goals shared by almost all conditional transfer programs. However, because the program relies on the records kept by teachers, the teachers can and do manipulate those records to change the allocation of the transfers. Specifically, teachers exaggerate attendance, thereby providing free grain to some children who most likely do not meet the requirements.

Within such an incentive scheme, there is an obvious trade-off to this behavior. Since more children get the grain, the manipulated distribution possibly improves the nutritional status of more children, but at the cost of reducing the incentives to attend school. Without taking the teachers' behavior into account, the program will not achieve the balance between these two ends that policy makers had intended.

Since teachers have more information about the individual children, however, allowing them some degree of discretion could be efficient. They may, for example, use local information to target children who are in greater need of the grain. They may also be able to award the grain to children in months in which the students' absences were beyond the child's control, thereby allocating more grain while preserving the incentives created by the original requirements.

Since we cannot know what the teachers know, it is difficult to determine whether their actions are serving the goals of the program or undermining them. The teachers' sensitivity to certain student characteristics sheds some light on this matter. In India, as in much of the developing world, there is a big push to bring female children into the classroom and increase their attendance. The fact that teachers misrepresent attendance more for girls may reflect an effort to increase female school participation. In addition, teachers misrepresent attendance more for children of the lower caste, which may also be in accordance with the goals of the program since these castes have traditionally been disadvantaged. The fact that teachers misrepresent attendance less for Muslim children could be due to the religious conflict in Mumbai, but we cannot rule out other reasons teachers may help these children less. For example, one teacher interviewed indicated that the Muslim families who send their children to her mixed religion school are wealthier than the other families; if true, teachers could be discriminating based on income and not religion. Nevertheless, this behavior is most likely not in accordance with the program administrator's goals. This last result suggests that agent discretion in a setting with pervasive ethnic hostility may provide additional opportunities to discriminate.

Understanding why teachers misrepresent attendance and whether they respond to incentives created by the grain subsidy is important in order to judge the effects of school meal programs. In designing welfare programs that rely on the participation of local agents, it is also important to take into account how the structure of the program may affect the behavior of these agents. In some circumstances, agents may be able to use local information to better target public welfare programs, but they may also respond to local prejudices.

X. References

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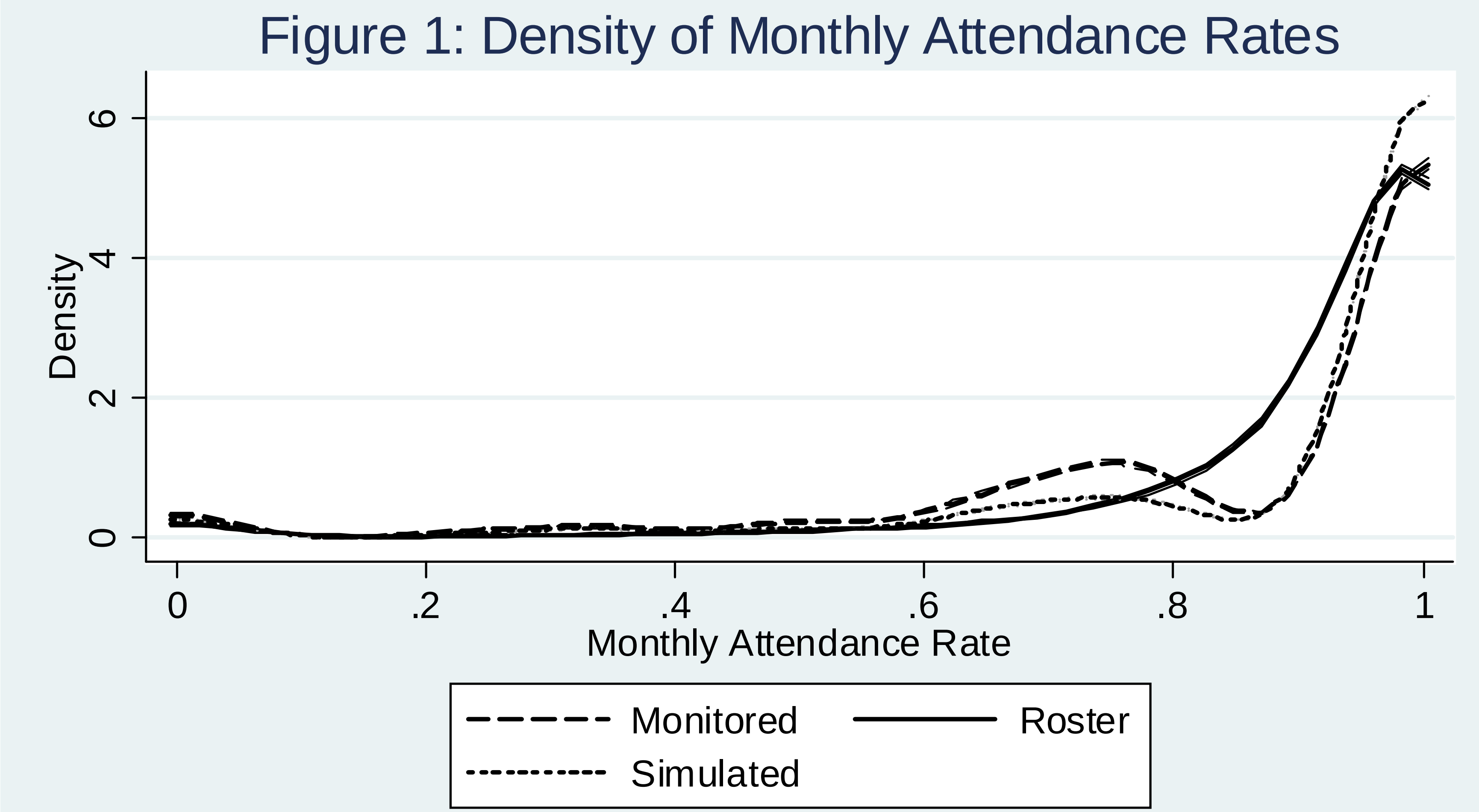
XI. Appendix A

This appendix elaborates on the testable hypotheses presented in section IVB. In the first case, when a student needs exactly n+1 days of future attendance to earn the grain on day T – n, I_{T-n} decreases with the number of days left in the month (since the teacher may have to inflate attendance many more times) and increases with the the child’s predicted attendance rate (since the teacher is less likely to have to inflate attendance many more times). Figures A1, A2 and A3 plot I_{T-n} with respect to a_t, for n equal to 2, 3 and 5, respectively. The solid lines, representing this first case, show that I_{T-n} is increasing in a_t. Note that in Figures A2 and A3, I_{T-n} is negative for very low values of a_t since it is too costly to give the child the grain.

In the second case, where the student requires more than n+1 days of future attendance, and the third case, when the child has already surpassed the 80% threshold for the entire month, I_{T-n} is negative and constant at $-c$ since the child will either receive (second case) or not receive (third case) the grain regardless of what inflation costs the teacher incurs. The dotted line in Figure A1 shows that I_{T-n} is constant in both these cases.

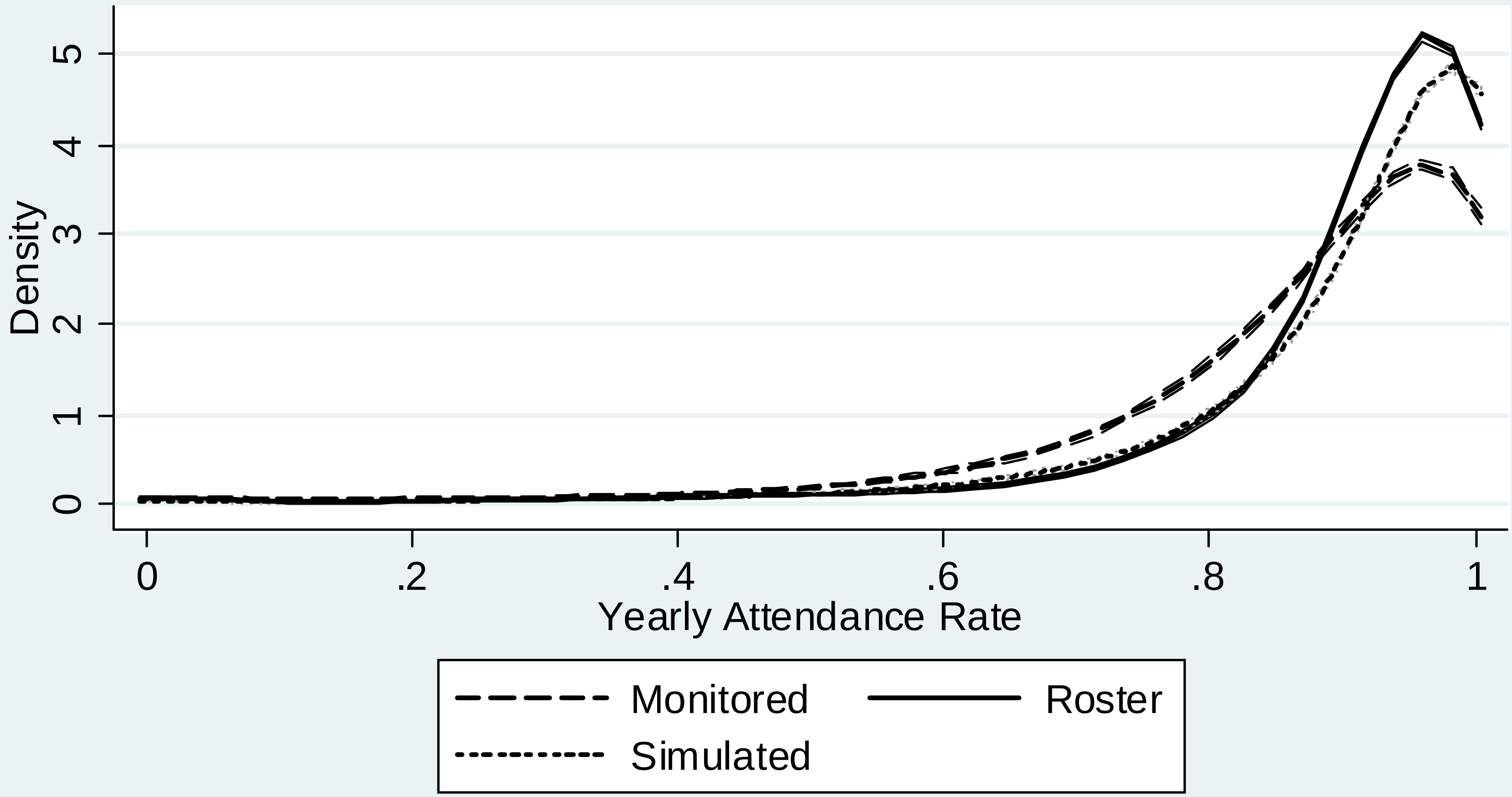
In the fourth case, when the student needs fewer than n+1 days of future attendance to make the 80% threshold, I_{T-n} is negative because teachers should wait until the child hits the first (needs her help for certain) or third (already earned the grain) cases. Variation arises in I_{T-n} in this case because of two opposing forces that derive from the probability the child will receive the grain and the likelihood the teacher will misrepresent the child's attendance in the future. For a child who will earn the grain (either because he has a high attendance rate or because it is sufficiently cheap for the teacher to adjust her records), I_{T-n} is decreasing in a_t. If the child is very likely to attend in the future, misrepresenting attendance today is very costly because the teacher may never have needed to misrepresent his attendance. If the child is less likely to attend in the future, the teacher is simply mistiming the exaggerations. Denote r to be the number of days of future attendance the child needs to earn the grain. The dashed line in Figure A1 demonstrates this force: a child for whom $c = 0.4$ and $r = 1$ is certain to earn the grain since the expected cost of misrepresenting attendance (c multiplied by the probability the child is absent the next day) is less than the value of the grain. Thus, I_{T-n} is decreasing in a_t. The two dashed lines in Figure A2 confirm this prediction: I_{T-n} is decreasing in a_t conditional on r. Note that the short dashed line ($r = 1$), is always below the long dashed line ($r = 2$). This is because the child is more likely to earn the grain himself, rendering the teacher's action wasted, when $r = 1$ than when $r = 2$. In other words, it is more likely the child will attend at least one of the future days than two of them.

However, an increase in a_t also increases the probability the child will earn the grain (both on his own merits and because the teacher does not have to incur too many costs), which in turn increases I_{T-n} because this probability rises faster when the child only needs $r-1$ days of future attendance (the teacher having already adjusted one day's records) than when he needs r days of future attendance. For a child with a very low value of a_t, a teacher is no longer simply mistiming the exaggerations because the child may not earn the grain at all, in which case the teacher's action was wasted. Overall, an increase in a_t increases the likelihood that the child could have earned the grain on his own; at low values of a_t it becomes less wasteful to misrepresent a child's attendance because it is less likely to be too costly to guarantee the child the grain and at higher values of a_t, the action becomes more wasteful since he would have earned the grain on his own. The two dash-dot combination lines in Figure A3 demonstrate this non-monotonic relationship between a_t and I_{T-n} for higher values of r. Note that, while I_{T-n} is always negative, it is often greater than $-c$ since the child may eventually receive the grain.



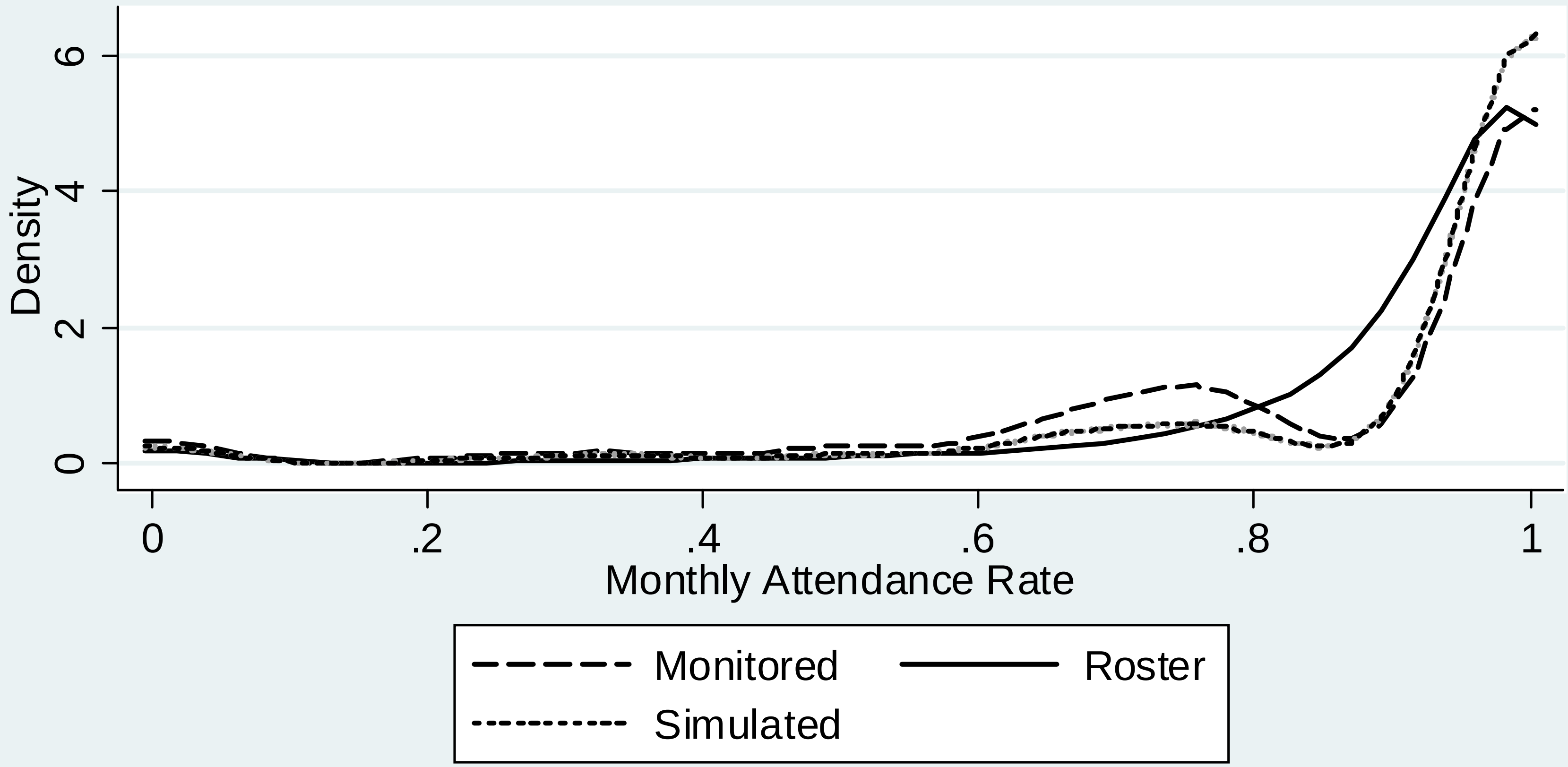
Note: This graph plots the kernel densities of roster and monitored monthly attendance rates separately using a Gaussian kernel and a bandwidth of 0.05. Because of the measurement error problems due to the sampling of days for direct monitoring of attendance (see Section VI.B for a more detailed explanation), we also plot the distribution of simulated data at the same frequency of monitored data, but under the assumption that teachers do not misrepresent attendance. All plots include 95% confidence intervals.

Figure 2: Density of Yearly Attendance Rates



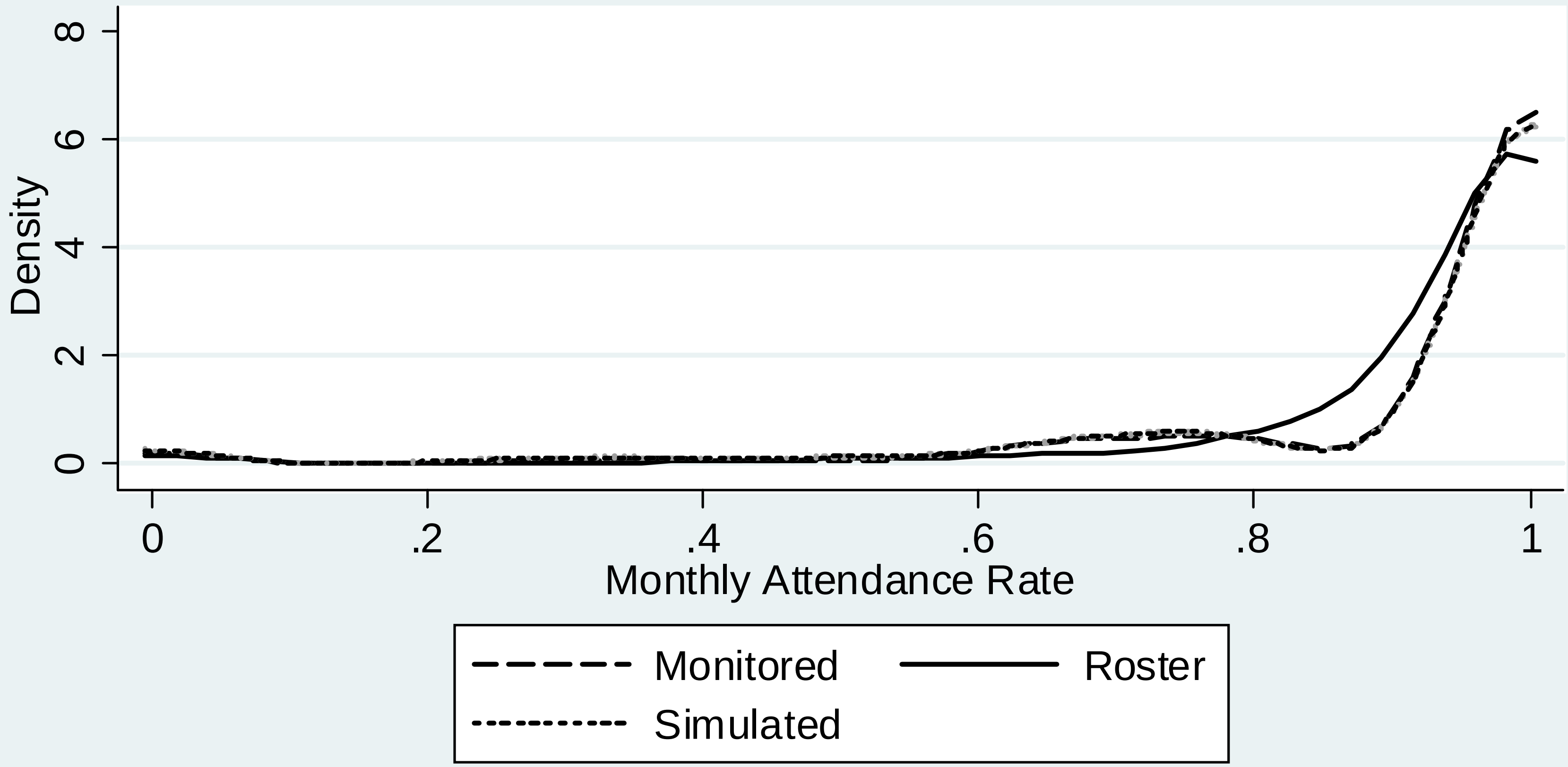
Note: This graph plots the kernel densities of roster and monitored yearly attendance rates separately using a Gaussian kernel and a bandwidth of 0.05. Because of the measurement error problems due to the sampling of days for direct monitoring of attendance (see Section VI.B for a more detailed explanation), we also plot the distribution of simulated data at the same frequency of monitored data, but under the assumption that teachers do not misrepresent attendance. All plots include 95% confidence intervals.

Figure 3: Density of Monthly Attendance Rates for Teachers Seen to Inflate Attendance



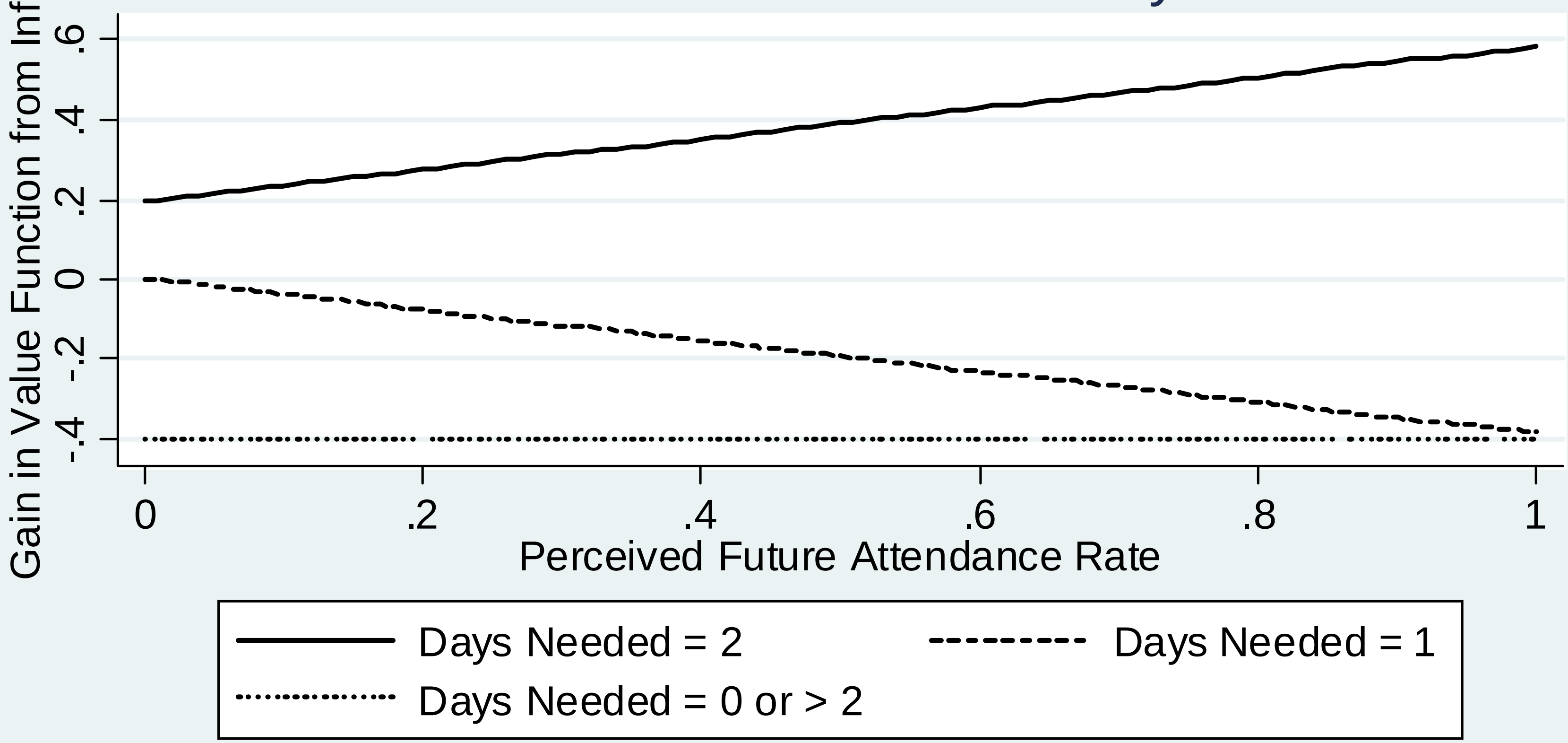
Note: This graph plots the kernel densities of roster and monitored monthly attendance rates separately using a Gaussian kernel and a bandwidth of 0.05. This figure only includes records from teachers who inflate attendance on any matched day. Because of the measurement error problems due to the sampling of days for direct monitoring of attendance (see Section VI.B for a more detailed explanation), we also plot the distribution of simulated data at the same frequency of monitored data, but under the assumption that teachers do not misrepresent attendance.

Figure 4: Density of Monthly Attendance Rates for Teachers Who Never Inflate Attendance



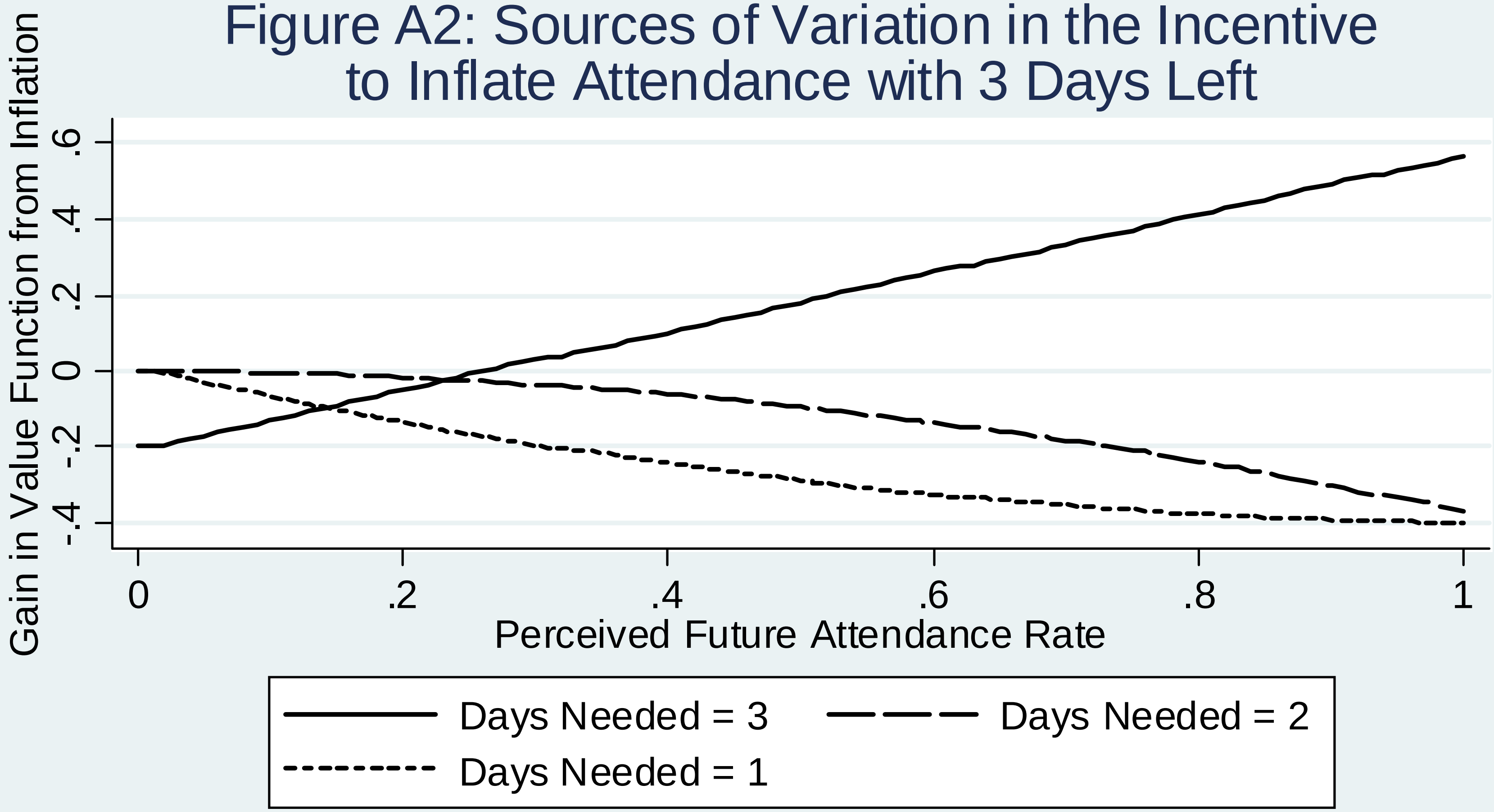
Note: This graph plots the kernel densities of roster and monitored monthly attendance rates separately using a Gaussian kernel and a bandwidth of 0.05. This figure only includes records from teachers who are never seen to inflate attendance. Because of the measurement error problems due to the sampling of days for direct monitoring of attendance (see Section VI.B for a more detailed explanation), we also plot the distribution of simulated data at the same frequency of monitored data, but under the assumption that teachers do not misrepresent attendance.

Figure A1: Sources of Variation in the Incentive to Inflate Attendance with 2 Days Left



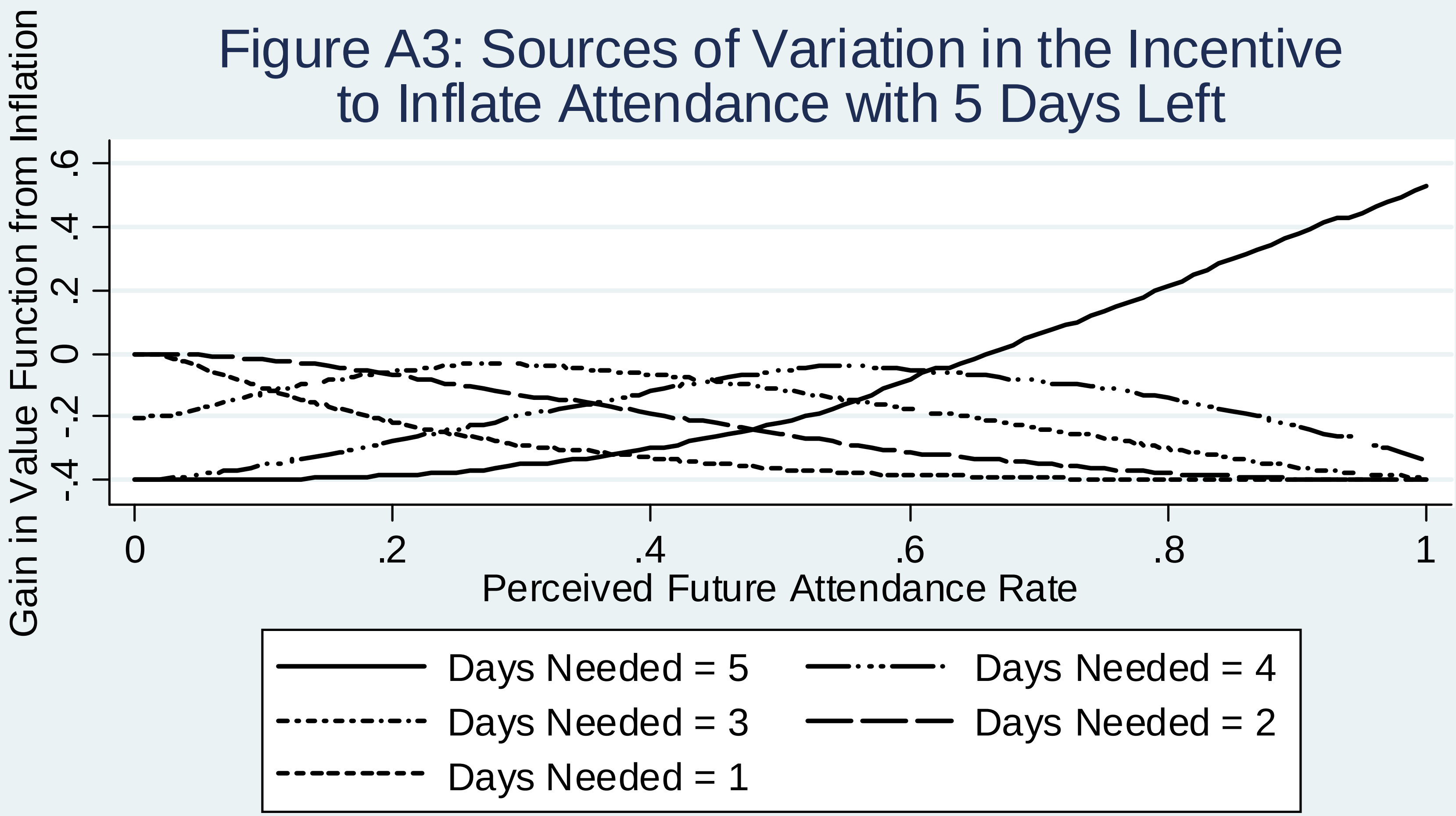
Note: This graph plots the net benefit a teacher receives from misrepresenting attendance. This value is estimated by solving the dynamic programming model numerically and calculating the change in the value function if the teacher inflates a child's attendance. This value is graphed against the teacher's perception of the child's future attendance rate. We assume a daily exaggeration cost of 0.4 and that 30 days have passed in the year in order to update the child's attendance rate. See Section IV for a detailed explanation of the model and Appendix A for a detailed explanation of the graph.

Figure A2: Sources of Variation in the Incentive to Inflate Attendance with 3 Days Left



Note: This graph plots the net benefit a teacher receives from misrepresenting attendance. This value is estimated by solving the dynamic programming model numerically and calculating the change in the value function if the teacher inflates a child's attendance. This value is graphed against the teacher's perception of the child's future attendance rate. We assume a daily exaggeration cost of 0.4 and that 30 days have passed in the year in order to update the child's attendance rate. See Section IV for a detailed explanation of the model and Appendix A for a detailed explanation of the graph.

Figure A3: Sources of Variation in the Incentive to Inflate Attendance with 5 Days Left



Note: This graph plots the net benefit a teacher receives from misrepresenting attendance. This value is estimated by solving the dynamic programming model numerically and calculating the change in the value function if the teacher inflates a child's attendance. This value is graphed against the teacher's perception of the child's future attendance rate. We assume a daily exaggeration cost of 0.4 and that 30 days have passed in the year in order to update the child's attendance rate. See Section IV for a detailed explanation of the model and Appendix A for a detailed explanation of the graph.

Table 1: Summary Statistics

	Obs	Mean	Standard Deviation	Min	Max
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Panel A: Student characteristics					
Child in Urdu School	15519	0.400	0.490	0	1
Muslim Child in Non-Urdu School	15519	0.075	0.264	0	1
Brahmin Caste	15519	0.063	0.243	0	1
Kshatriya Caste	15519	0.106	0.308	0	1
Vaishnav Caste	15519	0.013	0.111	0	1
Shudra Caste	15519	0.041	0.199	0	1
Unknown Caste	15519	0.777	0.416	0	1
Male	15519	0.478	0.500	0	1
Normalized Pre-Test Score	15519	0.009	0.991	-2.056	3.474
Age	13785	8.701	1.351	5	15
Panel B: Student-month data					
Roster attendance rate	76965	0.905	0.196	0	1
Monitored attendance rate	50310	0.859	0.252	0	1
Child had >= 80% roster attendance	76965	0.874	0.332	0	1
Child had >= 80% monitored attendance	50310	0.721	0.448	0	1
Panel C: Student-absence data					
Misrepresented attendance	24192	0.430	0.495	0	1
Still eligible: still in the running	24192	0.738	0.440	0	1
Needs help: still in the running but has not already earned the grain	24192	0.678	0.467	0	1
Already earned: has already earned the grain (Case 3)	24192	0.060	0.238	0	1
Needs help plus perfect attendance this month	24192	0.302	0.459	0	1
Already earned plus perfect attendance this month	24192	0.045	0.207	0	1
Days Left in Month	24192	11.476	6.225	1	26
Needs Perfect Attendance (Case 1)	24192	0.069	0.253	0	1
Waste Effort (Case 4)	24192	0.592	0.492	0	1
Perceived Attendance Rate	24192	0.729	0.169	0	0.992
Gain with cost=0.20	24192	-0.112	0.159	-0.2	0.8
Gain with cost=0.25	24192	-0.159	0.148	-0.250	0.750
Gain with cost=0.30	24192	-0.207	0.141	-0.3	0.7
Gain with cost=0.35	24192	-0.255	0.136	-0.350	0.650
Gain with cost=0.40	24192	-0.304	0.132	-0.4	0.6

Note: This table displays summary statistics of all the variables used in the following tables. Panel A describes student characteristics, panel B presents summary information about monthly attendance rates according to both the teacher-taken (roster) and monitored records. Panel C summarizes variables that derive from the variation in the incentives to misrepresent attendance according to the child's past attendance record and the number of days left in the month. See Section IV and V for more details about the variables in panel C.

Table 2: Empirical Tests of Whether Inaccurate Attendance Records Affect Grain Distribution

Dependent Variable:	Child Had >= 80% Roster Attendance						
	Linear Probability					Conditional Logit	
					Benchmark		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Panel A							
Monitored Attendance	0.795 ***	0.632 ***				5.468 ***	
	(0.017)	(0.024)				(0.194)	
Monitored Attendance < 80%			-0.345 ***	-0.229 ***			-2.383 ***
			(0.012)	(0.012)			(0.077)
Recorded Attendance					1.447 ***		
					(0.026)		
Constant	0.183 ***	0.324 ***	0.964 ***	0.945 ***	-0.423 ***		
	(0.015)	(0.026)	(0.003)	(0.017)	(0.024)		
Additional Fixed Effects	Teacher XMonth	Student	Teacher XMonth	Student	Student	Student	Student
p-value of joint test of f.e.	0.000	0.000	0.000	0.000	0.000		
Number of Observations	49742	49742	49742	49742	49742	14013	14013
R-squared	0.446	0.604	0.330	0.561	0.780		
Panel B							
From Simulated Trials:							
Coefficient:	0.97	0.88	-0.49	-0.4		7.15	-2.99
99% Confidence Interval	(0.96-0.98)	(0.87-0.9)	(-0.48--0.5)	(-0.39--0.41)		(6.75-7.54)	(-2.86--3.15)
p-value	0.000	0.000	0.000	0.000		0.000	0.000
R-squared	0.54	0.67	0.43	0.62			
99% Confidence Interval	(0.53-0.55)	(0.67-0.68)	(0.42-0.44)	(0.62-0.63)			
p-value	0.000	0.000	0.000	0.000			

Note: This table displays estimates of equation (1). All observations are at the student-month level. The dependent variable is whether or not the student was assigned to receive the grain from his or her teacher while the primary independent variable in columns 1, 2, 3, 4, 6, and 7 is the students' attendance record as measured through the direct monitoring of attendance. Because the exact magnitude of the coefficient on monitored attendance is difficult to predict under the null hypothesis that teachers accurately record attendance, column 5 provides a benchmark regression in which the probability of a student receiving grain is regressed on the students' teacher recorded attendance record. Columns 2 and 4-5 also include month fixed effects. Because of the measurement error problems due to the sampling of days for direct monitoring of attendance (see Section VI.C for a more detailed explanation) Panel B provides estimates of the coefficients on monitored attendance in Panel A under the assumption that teachers never misreported attendance and that attendance is taken at the same rate as the monitored attendance data. The estimates are the mean and 99 percent confidence intervals from a 1,000 iteration bootstrap procedure. Columns 1-5 are estimated using a linear probability model while columns 6 and 7 are estimated using a conditional logit model. Robust standard errors clustered by teacher are in parenthesis. Observations are only used if there were at least 3 dates matched between monitored and roster attendance data for a particular student-month. *** 1%, ** 5%, * 10%.

Table 3: Empirical Tests of Whether Teachers Respond to Grain Incentives

Dependent Variable: Misrepresent

Panel A	**(1)**		**(2)**		**(3)**		**(4)**		**(5)**		**(6)**	
Still Eligible	0.387	***	0.118	***								
	(0.019)		(0.015)									
Needs Help					0.378	***	0.114	***	0.373	***	0.104	***
					(0.019)		(0.015)		(0.019)		(0.014)	
Already Earned					0.449	***	0.221	***				
					(0.030)		(0.026)					
Additional Fixed Effects	Teacher XDay		Student		Teacher XDay		Student		Teacher XDay		Student	
p-value of joint test of f.e.	0.000		0.000		0.000		0.000		0.000		0.000	
Number of Observations	24192		24192		24192		24192		22729		22729	
R-squared	0.65		0.68		0.65		0.68		0.64		0.68	

Panel B	**(1)**		**(2)**		**(3)**		**(4)**		**(5)**		**(6)**	
Still Eligible			0.178	***								
			(0.026)									
Needs Help					0.155	***	0.166	***	0.194	***	0.219	***
					(0.027)		(0.027)		(0.030)		(0.031)	
Already Earned							0.223	***	0.267	***	0.307	***
							(0.041)		(0.064)		(0.066)	
Needs Help * Perfect Attendance									0.072	**	0.061	*
									(0.035)		(0.034)	
Already Earned * Perfect Attendance									0.051		0.038	
									(0.070)		(0.070)	
Daysleft	-0.002	***	-0.007	***	-0.007	***	-0.007	***	-0.007	***		
	(0.001)		(0.001)		(0.001)		(0.001)		(0.001)			
Daysleft * Still Eligible			-0.001									
			(0.002)									
Daysleft * Needs Help					-0.0001		-0.0002		-0.005	**	-0.007	***
					(0.002)		(0.002)		(0.002)		(0.002)	
Daysleft * Already Earned							-0.013		-0.004		-0.030	
							(0.018)		(0.049)		(0.048)	
Daysleft * Needs Help * Perfect Attendance									0.010	***	0.011	***
									(0.002)		(0.002)	
Daysleft * Already Earned * Perfect Attendance									-0.010		0.007	
									(0.051)		(0.051)	
Additional Fixed Effects	Student		Student		Student		Student		Student		Daysleft Student	
p-value of joint test of f.e.	0.000		0.000		0.000		0.000		0.000		0.000	
Number of Observations	24192		24192		22729		24192		24192		24192	
R-squared	0.68		0.68		0.69		0.68		0.7		0.7	

Note: This table displays estimates of equation (2) in Panel A and (3) in Panel B. All observations are at the student-day level and are limited to days on which the child is absent. The dependent variable is whether or not the teacher marks the child as present despite the child's absence while the primary independent variables include various measures of the child's eligibility for the grain: "still eligible" is if the child is not yet disqualified, "needs help" is if the child needs further days of attendance to earn the grain and "already earned" is if the child has already qualified for the grain, while the omitted group is those who are already disqualified by missing 20% or more of the total number of days in the month. Panel B includes interactions of these eligibility measures with the number of days left in the month. Columns 5-6 in panel A and 3 in panel B excludes children who have already earned the grain, i.e. attended more than 80% of the month. All columns include month fixed effects. All columns estimate a linear probability model; similar results are obtained with conditional logit. See Section VII.A for a detailed explanation of the results. Robust standard errors clustered by teacher are in parenthesis. *** 1%, ** 5%, * 10%

Table 4: Empirical Tests Derived from Variation in the Incentives to Misrepresent Attendance

Dependent Variable: Misrepresent												
Panel A	**(1)**		**(2)**		**(3)**		**(4)**		**(5)**		**(6)**	
Needs Perfect Attendance	0.161	***	0.038	*	0.304	***	0.182	***	0.429	***	0.355	***
	(0.017)		(0.021)		(0.035)		(0.048)		(0.084)		(0.102)	
Already Earned	0.446	***	0.245	***	0.380	***	0.269	***	0.826	***	0.592	***
	(0.023)		(0.028)		(0.043)		(0.058)		(0.089)		(0.141)	
Waste Effort	0.335	***	0.140	***	0.352	***	0.217	***	0.341	***	0.095	
	(0.015)		(0.016)		(0.023)		(0.031)		(0.067)		(0.079)	
Daysleft					-0.009	***	-0.006	***	-0.009	***	-0.006	***
					(0.001)		(0.001)		(0.001)		(0.001)	
Daysleft					-0.014	***	-0.010	***	-0.016	***	-0.012	***
* Needs Perfect Attendance					(0.003)		(0.004)		(0.003)		(0.004)	
Daysleft					-0.010		-0.014		-0.006		-0.011	
* Already Earned					(0.014)		(0.018)		(0.013)		(0.018)	
Daysleft					0.000		-0.002		0.000		-0.001	
* Waste Effort					(0.001)		(0.002)		(0.001)		(0.002)	
Perceived Attendance Rate					0.263	***	0.015		0.277	***	0.054	
					(0.030)		(0.043)		(0.035)		(0.043)	
Perceived Attendance Rate									-0.151	*	-0.226	**
* Needs Perfect Attendance									(0.091)		(0.106)	
Perceived Attendance Rate									-0.569	***	-0.449	***
* Already Earned									(0.104)		(0.162)	
Perceived Attendance Rate									-0.028		0.591	***
* Waste Effort									(0.176)		(0.227)	
Perceived Att. Rate Squared									0.045		-0.573	***
* Waste Effort									(0.132)		(0.184)	
Additional Fixed Effects	Teacher		Student		Teacher		Student		Teacher		Student	
p-value of joint test of f.e.	0.000		0.000		0.000		0.000		0.000		0.000	
Number of Observations	24192		24192		24192		24192		24192		24192	
R-squared	0.401		0.681		0.420		0.685		0.421		0.686	

Note: This table displays estimates of equation (4). All observations are at the student-day level and are limited to days on which the child is absent. The dependent variable is whether or not the teacher marks the child as present despite the child's absence while the primary independent variables include indicators for the different possible interactions between the number of days left in the month, n, and the number of days of attendance a child needs to earn the grain. The omitted category is when the child is already disqualified from the grain (case 2 as described in Section IV.B). "Needs perfect attendance" is when the child needs exactly n+1 days of future attendance to earn the grain (case 1), "Already Earned" is when the child has already earned the grain (case 3) and "Waste Effort" is when the child may still earn the grain but it is not yet imperative for the teacher to misrepresent the student's attendance (case 4). Columns 3-6 include the number of days left in the month and the child's perceived attendance rate along with interactions with the above cases. All columns estimate a linear probability model; similar results are obtained with conditional logit. See Section IV for a detailed explanation of the model and VII.B for a detailed explanation of the results. Robust standard errors clustered by teacher are in parenthesis. *** 1%, ** 5%, * 10%.

Table 5: Estimates of Responses to the Grain Distribution Using a Numerical Solution

Dependent Variable:	Misrepresent				
Cost:	0.2	0.25	0.3	0.35	0.4
Panel A	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Incentive to Misrepresent	0.129 ***	0.200 ***	0.268 ***	0.331 ***	0.386 ***
	(0.027)	(0.028)	(0.031)	(0.033)	(0.035)
Additional Fixed Effects	Teacher XDay	Teacher XDay	Teacher XDay	Teacher XDay	Teacher XDay
p-value of joint test of f.e.	0.000	0.000	0.000	0.000	0.000
Num. of Obs.	24192	24192	24192	24192	24192
R-squared	0.590	0.592	0.593	0.595	0.596
Panel B					
Incentive to Misrepresent	0.040	0.080 **	0.119 ***	0.156 ***	0.186 ***
	(0.036)	(0.039)	(0.041)	(0.043)	(0.044)
Additional Fixed Effects	Student	Student	Student	Student	Student
p-value of joint test of f.e.	0.000	0.000	0.000	0.000	0.000
Num. of Obs.	24192	24192	24192	24192	24192
R-squared	0.675	0.675	0.675	0.675	0.676

Note: This table displays estimates of equation (5). All observations are at the student-day level and are limited to days on which the child is absent. The dependent variable is whether or not the teacher marks the child as present despite the child's absence while the primary independent variable is the net benefit a teacher receives from misrepresenting attendance. This value is estimated by solving the dynamic programming model numerically and calculating the change in the value function if the teacher inflates a child's attendance, I_{T-n}, as defined in Section IV.A. Different columns estimate this value assuming different daily costs to misrepresenting attendance. Panel B includes month fixed effects. All columns estimate a linear probability model; similar results are obtained with conditional logit. See Section IV for a detailed explanation of the model and VII.B for a detailed explanation of the results. Robust standard errors clustered by teacher are in parenthesis. *** 1%, ** 5%, * 10%.

Table 6: Empirical Tests of Whether Teachers Favor Certain Types of Students

Dependent Variable:	Monitored Attendance	Child Had >= 80% Roster Attendance			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Monitored Attendance		0.785 ***			
		(0.017)			
Monitored Attendance < 80%			-0.338 ***	-0.308 ***	-0.139 ***
			(0.012)	(0.042)	(0.040)
Muslim Student in	-0.041 ***	-0.010	-0.017 *	0.002	
Non-Urdu School	(0.010)	(0.008)	(0.010)	(0.007)	
Brahmin	-0.010	-0.009	-0.014	0.006	
	(0.010)	(0.008)	(0.010)	(0.008)	
Kshatriya	-0.013	-0.003	-0.005	0.011	
	(0.009)	(0.007)	(0.008)	(0.008)	
Vaishnav	0.012	0.007	0.013	0.020 *	
	(0.011)	(0.010)	(0.013)	(0.011)	
Unknown Caste	-0.008	-0.008	-0.011	-0.012	
	(0.008)	(0.006)	(0.008)	(0.008)	
Male	-0.013 ***	-0.008 ***	-0.013 ***	-0.003	
	(0.003)	(0.003)	(0.003)	(0.003)	
Normalized Pre-test Score	0.027 ***	0.015 ***	0.021 ***	0.013 ***	
	(0.002)	(0.002)	(0.002)	(0.002)	
Monitored Attendance < 80%				-0.068 **	-0.020
* Muslim Student in Non-Urdu School				(0.031)	(0.034)
Monitored Attendance < 80%				-0.087 **	-0.146 ***
* Brahmin				(0.042)	(0.044)
Monitored Attendance < 80%				-0.064	-0.090 **
* Kshatriya				(0.040)	(0.043)
Monitored Attendance < 80%				-0.035	-0.120 *
* Vaishnav				(0.064)	(0.069)
Monitored Attendance < 80%				0.009	-0.068 *
* Unknown Caste				(0.042)	(0.041)
Monitored Attendance < 80%				-0.034 ***	-0.027 **
* Male				(0.009)	(0.012)
Monitored Attendance < 80%				0.029 ***	0.024 ***
* Normalized Pre-test Score				(0.006)	(0.008)
P-values from F-tests of:					
Student characteristics		0.000	0.000	0.000	
Student char. and interactions				0.000	0.001
Additional Fixed Effects	Teacher XMonth	Teacher XMonth	Teacher XMonth	Teacher XMonth	Student
p-value of joint test of f.e.	0.000	0.000	0.000	0.000	0.000
Number of Observations	50310	49742	49742	49742	49742
R-squared	0.152	0.448	0.335	0.338	0.562

Note: Column 1 in this table presents estimates from a regression of monitored attendance rates on student characteristics. Columns 2-5 displays estimates of equation (1) with student characteristics. All observations are at the student-month level. The dependent variable in columns 2-5 is whether or not the student was assigned to receive the grain from his or her teacher while the independent variable is the students' attendance record as measured through the direct monitoring of attendance. This table differs from table 2 because of the inclusion of student characteristic controls and interactions of these characteristics with grain-related measures of monitored attendance. All columns include student-age fixed effects and column 5 includes month fixed effects. All columns estimate a linear probability model; similar results are obtained with conditional logit. See Section VIII for a detailed explanation of the results. Robust standard errors clustered by teacher are in parenthesis. Observations are only used if there were at least 3 dates matched between monitored and roster attendance data for a particular student-month. *** 1%, ** 5%, * 10%.

Table 7: Empirical Tests Derived from Variation in the Incentives to Misrepresent Attendance

Dependent Variable: Misrepresent		Cost for Bellman Calculation		
		0.25	0.3	0.35
Panel A	**(1)**	**(2)**	**(3)**	**(4)**
Needs Perfect Attendance	0.182 ***			
	(0.020)			
Already Earned	0.425 ***			
	(0.031)			
Waste Effort	0.411 ***			
	(0.020)			
Incentive to Misrepresent		0.176 ***	0.246 ***	0.311 ***
		(0.027)	(0.030)	(0.032)
Muslim Student in	0.002	-0.004	-0.004	-0.004
Non-Urdu School	(0.016)	(0.021)	(0.021)	(0.021)
Brahmin	-0.016	-0.018	-0.018	-0.018
	(0.024)	(0.030)	(0.030)	(0.030)
Kshatriya	0.011	0.014	0.015	0.015
	(0.021)	(0.027)	(0.027)	(0.027)
Vaishnav	-0.005	0.025	0.025	0.025
	(0.031)	(0.039)	(0.039)	(0.039)
Unknown Caste	-0.007	-0.011	-0.011	-0.011
	(0.021)	(0.027)	(0.027)	(0.026)
Male	-0.014 *	-0.023 **	-0.023 **	-0.023 **
	(0.007)	(0.009)	(0.009)	(0.009)
Normalized Pre-test Score	0.003	0.014 ***	0.014 ***	0.014 ***
	(0.004)	(0.005)	(0.005)	(0.005)
P-values from F-tests of:				
All student characteristics	0.000	0.000	0.000	0.000
Additional Fixed Effects	Teacher XDay	Teacher XDay	Teacher XDay	Teacher XDay
p-value of joint test of f.e.	0.000	0.000	0.000	0.000
Number of Observations	22443	22443	22443	22443
R-squared	0.663	0.601	0.602	0.603

Note: This table displays estimates of equation (4) in column 1 and (5) in columns 2-4. All observations are at the student-day level and are limited to days on which the child is absent. The dependent variable is whether or not the teacher marks the child as present despite the child's absence while the independent variables are derived from the grain incentives (see the notes to tables 4 and 5 for a more detailed explanation). This table differs from tables 4 and 5 because of the inclusion of student characteristics and interactions of these characteristics with measures of the grain-related incentives. All columns include student-age fixed effects. All columns estimate a linear probability model; similar results are obtained with conditional logit. See Section VIII for a detailed explanation of the results. Robust standard errors clustered by teacher are in parenthesis. *** 1%, ** 5%, * 10%.