NBER WORKING PAPER SERIES

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Working Paper 18165
http://www.nber.org/papers/w18165

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2012

We gratefully acknowledge the leadership and support of our Bloom Township, Chicago Heights, and Chicago Public School District partners. We also thank Bruno Frey, Marcin Siwicki, and Esjay Yoo for their valuable contributions to the experimental design. Alec Brandon, Patrick Fitz, Trevor Gallen, Sean Golden, David Herberich, Wooju Lee, Ian Muir, Joseph Seidel, Phuong Ta, and Timo Vogelsang provided truly outstanding research assistance. The project was made possible by the generous financial support of the Children First Fund, the Kenneth and Anne Griffin Foundation, the Rauner Family Foundation, and the Spencer Foundation. This research has been conducted with IRB approval. Please direct correspondence to Sally Sadoff. Please direct correspondence to Sally Sadoff. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 18165
June 2012, Revised June 2013
JEL No. C9,C93,H75,I20

ABSTRACT

Research on behavioral economics has established the importance of factors such as reference dependent preferences, hyperbolic discounting, and the value placed on non-financial rewards. To date, these insights have had little impact on the way the educational system operates. Through a series of field experiments involving thousands of primary and secondary school students, we demonstrate the power of behavioral economics to influence educational performance. Several insights emerge. First, we find substantial incentive effects from both financial and non-financial incentives on test scores. Second, we find that non-financial incentives are considerably more cost-effective than financial incentives for younger students, but were less effective with older students. Third, and perhaps most importantly, consistent with hyperbolic discounting, all motivating power of the incentives vanishes when rewards are handed out with a delay. Since the rewards to educational investment virtually always come with a delay, our results suggest that the current set of incentives may lead to underinvestment. Fourth, in stark contrast to previous laboratory experiments, we do not see an increased response of effort when rewards are framed as losses. Our findings imply that in the absence of immediate incentives, many students put forth low effort on standardized tests, which may create biases in measures of student ability, teacher value added, school quality, and achievement gaps.

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1 Introduction

Behavioral economics has now gone beyond mere academic curiosity, touching nearly every field in economics. Theorists are recognizing behavioral regularities that lie outside of the standard paradigm in their models, empiricists are taking new behavioral predictions to the lab and field, and policymakers are increasingly recognizing the power of psychology when crafting new legislation. One area where behavioral economics has made only limited inroads, however, is in education circles. This is puzzling since it is an area where the insights gained from behavioral economics might be especially great. In this study, we use a series of field experiments to explore how behavioral economics can be leveraged to better understand a key question in education: given the large and increasing returns to schooling, why is achievement among many students so low? Are some students making optimization "mistakes" that lead to underinvestment in education?

A key feature of the education investment function is that in order to experience the long run returns to schooling, students must make sustained investments in human capital that require exerting effort on tasks that often have relatively low returns in the near term, such as paying attention in class, completing a daily assignment or focusing on a low stakes test. This structure motivates several of the central hypotheses about why students might underinvest in education, particularly those in low-income families and low-achieving schools.[1]

First, these students may undervalue the returns to education (e.g., Eckstein and Wolpin 1999). Second, they may have high discount rates (e.g., Oreopoulos 2007). Third, students may not fully understand the education production function (e.g.,

[1]See Wilson (1987) for further discussion of the effects of lack of exposure to role models in education.

Fryer 2011a).[2] As we discuss in further detail below, we examine each of these potential sources of underinvestment using incentive designs motivated by insights from behavioral economics.

Previous research has focused on increasing students' estimates of the financial returns to education through both information (e.g., Nguyen 2008, Jensen 2010) and offers of performance-based financial incentives.[3] Even with more accurate estimates, however, students may continue to *undervalue* these returns due to limited experience with educational rewards or limited understanding of monetary value.

To address undervaluation of returns, we draw on two areas of behavioral economics: loss aversion and non-monetary rewards. A large literature demonstrates that some people exhibit reference-dependent preferences wherein they respond to losses more strongly than gains (e.g., Tversky and Kahneman 1991). Particularly for students with limited exposure to educational returns, endowing them with a performance-based reward *in advance* of the incentivized task may increase their utility of keeping the reward and motivate greater effort. We also build on a growing area of research demonstrating the motivational power of non-financial rewards (e.g., Kosfeld and Neckermann 2011). These could be especially powerful among younger children who have limited experience with money (e.g., Webley 2005).

A second driver of underinvestment may be individual time preferences. Numerous studies find that children and adolescents tend to exhibit high discount rates and

[2]In a similar vein, recent studies have explored how increased information and understanding can improve decision-making in school choice (Hastings and Weinstein 2008), college enrollment (Dynarski and Scott-Clayton 2008, Bettinger et al 2012), and financial planning (Hastings and Mitchell 2011).

[3]Recent incentive programs have conditioned monetary rewards on a variety of measures including school enrollment, attendance, behavior, grades, test performance, and matriculation. Examples include Progresa in Mexico, which offered incentives for school enrollment and attendance (Schultz 2004, Behrman et al. 2005). A similar conditional cash transfer program was instituted in Colombia (Barrera-Osorio et al. 2011). Other programs have based rewards on overall school performance (see Angrist et al. 2006, Levitt et al. 2010, Leuven et al. 2010, Fryer 2011a).

have difficulty planning for the future (e.g., Gruber 2001, Bettinger and Slonim 2007, Steinberg et al. 2009). Again, we draw on an insight from behavioral economics that some people have hyperbolic time preferences, overweighting the present so much that future rewards are largely ignored (e.g., Strotz 1955, Laibson 1997). Such preferences can lead to underinvestment when (as in education) the returns to achievement are largely delayed.[4]

Previous research has reduced the delay in educational returns by offering financial incentives to students that are paid at the end of a month or school term. Typically, these incentive programs have remained in place for an extended period of time (e.g. a school year) with the goal of affecting student behavior over the duration of that time horizon. However, if students are sufficiently myopic, they will respond more strongly to rewards with very short time horizons compared to incentives extending over several months or years. We therefore compare rewards offered *immediately* after the incentivized task to rewards offered with a small delay (of one month).

Finally, students may have a limited understanding of the education production function. That is, they do not know what types of effort will increase achievement. If this is the case, then students may not improve in response to performance-based incentives even if they are motivated to do so.[5] We design a clean test of this hypothesis by offering students a reward for performance that is announced immediately before the incentivized task (with no advance notice). This allows us to isolate the role of effort in the production function – avoiding confounding due to discount rates, opportunity costs, planning failures, or human capital accumulation (e.g., studying for the test). We also vary the size of the reward in order to separate lack of ability

[4]Previous studies find a negative correlation between hyperbolic discount rates and educational outcomes (Kirby et al. 2002, Kirby et al. 2005). Similarly, Mischel et al. (1989) find that measures of ability to delay gratification in early childhood are predictive of longer-term academic achievement.

[5]Or, there may be a great deal of uncertainty in the production function, which will lead to the same outcome (see Fryer 2011a for further discussion).

from lack of motivation to exert productive effort in response to incentives.

We test our incentive designs using field experiments conducted over multiple sites and years. This permits a glimpse of behavior not only within the experimental period but for months afterwards. Our field experiments include over 6,000 elementary and high school students in three school districts in and around Chicago. The typical study reports findings from a single experiment without any replications to examine transferability to different settings and scales. This paper addresses both questions by studying the impact of various incentive designs in several settings, among a wide age range of students, and in school districts of very different size. Thereby, we ensure that our results are robust to different settings and to scaling the intervention up to entire school-districts.[6]

In our baseline setup, students are offered cash or non-pecuniary rewards for an improvement in test scores. The tests last between 15 - 60 minutes, yielding a high hourly wage (particularly in the highest financial incentive group) that is likely quite salient among our subject pool of low-income children and adolescents. We investigate the effectiveness of low and high financial incentives ($10, $20) and compare these to the impact of non-monetary rewards in the form of a trophy for achievement. These incentives are presented in either the gain or the loss domain, and are offered either immediately after the test or with a delay (a month after the test).

We find that incentives substantially affect student performance. Both financial and non-financial incentives have a significant impact on performance, improving test scores by about a tenth of a standard deviation. These effect sizes are comparable to those achieved through a one standard deviation increase in teacher quality (e.g.,

[6]In a similar vein, Braun et al. (2011) test a single performance pay incentive among 2,600 students in 59 schools and seven states. Fryer (2011a) reports on a series of financial incentive programs carried out in a number of large American school districts (but does not compare different incentive designs within a single setting).

Rockoff 2004, Rivken et al. 2005) or a one-third reduction in class size (Krueger 1999).

In addition, several insights emerge from our behavioral interventions. Younger children are insensitive to the size of the monetary rewards we offer, suggesting limited understanding of these returns; at the same time, they are highly responsive to non-financial rewards, making them particularly cost effective among this group of students. We also find evidence that students have very high discount rates: in contrast to immediate incentives, non-immediate incentives (rewarded a month after the test) have no effect. Finally, students are responsive to high powered incentives but not to low powered incentives, suggesting that they understand the education production function for this task but require sufficient motivation to exert effort. In contrast to the large literature based on laboratory experiments, framing incentives as losses does not systematically alter the response to the rewards.

The design also allows us to uncover some of the underlying heterogeneities that drive the overall effectiveness of reward schemes: younger children are more responsive than older children; effects are somewhat stronger among boys than girls; and overall, the incentives work substantially better on math than on reading tests. Contrary to a widespread concern, we do not find that incentives have a detrimental effect on performance in subsequent tests.

Our results suggest that in the absence of immediate incentives, many students put forth low effort on the standardized tests that we study. These findings have important implications for policymakers because standardized assessment tests are often high-stakes for teachers and principals (e.g., as determinants of school resources), but low-stakes for the individual students choosing to exert effort on the test.

Relatively lower baseline effort among certain groups of students can create important biases in measures of student ability, teacher value added, school quality, and

achievement gaps.[7] For example, the impact of incentives on the performance of the low income minority students in our study would reduce the black-white test score gap by about 20-25 percent (Fryer 2011b). Understanding the extent to which test score gaps are due to lower effort rather than lower ability is crucial for the design of effective educational interventions: the former requires an intervention that increases student motivation, the latter requires an intervention that improves student knowledge and skills.

In addition, the diagnostic tests in our experiments are similar in nature to many of the low-stakes tasks students must engage in daily in order to accumulate human capital. If delays in rewards reduce student effort in our context, it would seem likely that the typical pattern of delayed rewards in the educational setting (e.g., increased earnings associated with school attainment accrue only with lags of years or even decades) induces sub-optimal effort in general.

The remainder of the paper is organized as follows. Section II summarizes the previous work that motivates our incentive design. Section III describes the experimental design and implementation. Section IV discusses the main results and potential sources of heterogeneity. Section V concludes with a discussion of the broader implications of the findings.

[7]Baumert and Demmrich (2001) and Braun et al. (2011) make a similar argument based on their findings and review the literature on achievement gaps due to differential motivation. In a similar vein, Jacob (2005) uncovers evidence that differential effort on the part of students can explain the otherwise puzzling divergence over time in the performance of students in Chicago Public Schools (CPS) on high-stakes versus low-stakes tests. It appears that CPS teachers and administrators became increasingly successful over a period of years at convincing students to take the high-stakes test seriously, but that same effort did not spill over to the low stakes state-administered tests.

2 A Motivating Framework

In part to address the potential sources of underinvestment discussed above – undervaluation of returns, high discount rates, and limited understanding of the production function – there has been growing interest from educators and policymakers in the use of performance-based incentives. While the results of previous programs have varied across settings, incentives have generally been associated with modest positive improvements in student performance. We incorporate insights from behavioral economics into the incentive design in order to better identify sources of low educational investment and to strengthen the impact of performance-based rewards.

Although the incentive structure and performance measures of previous programs have varied, they tend to share the following features. First, they offer rewards as *gains*. That is, students can only receive and experience the reward after exerting effort and meeting the performance criteria. Second, they primarily employ monetary rewards. Third, the incentives are typically announced well in advance of the incentivized task with a delay between the time students must exert effort and the time they receive rewards.

We build on a large literature demonstrating behavioral anomalies, such as the endowment effect (Thaler 1980), status quo bias (Samuelson and Zeckhauser 1988), and observed divergences of willingness to pay and willingness to accept measures of value (Hanemann 1991). These examples of reference-dependent decision making are broadly consistent with a notion of *loss aversion*, an insight gained from Kahneman and Tversky's (1979) prospect theory. If a student is loss averse in our context, then the negative utility she receives from a loss of x is greater in magnitude than the positive utility she receives from a gain of x (for any positive x). Thus optimal effort will increase when rewards are framed as losses rather than gains.

We design incentives in the loss domain aimed at increasing student attachment to the reward, a primary mechanism of loss aversion (e.g., Ariely et al. 2005). Students receive the reward before the incentivized test (and keep it during testing); they sign a form stating that the reward is theirs and that they are responsible for it, and they describe what they will do with the reward if their performance allows them to keep it. While similar framing mechanisms have been widely explored in the lab, ours is among the first studies to experimentally test loss aversion in the field.[8]

A more recent branch of behavioral economics has explored the effectiveness of non-financial rewards (e.g., Frey 2007, Bradler et al. 2013, Ashraf et al. 2012). Such rewards derive their motivational power from a variety of mechanisms including status, self-image concerns, and relative performance feedback that have been shown to affect behavior.[9] These types of non-pecuniary benefits may be particularly potent in the context of recognition for performance in school (e.g., Azmat and Iriberri 2010).

Thus, in contrast to the standard prediction, some students may be willing to exert more effort for a trophy worth $3 than they are for $3 in cash. Non-pecuniary incentives are also attractive because they are already commonly used in schools, which tend to be more comfortable rewarding students with trophies, certificates, and prizes than they are with using cash rewards. Despite their widespread prevalence,

[8]Previous field experiments have tested the effect of the loss frame in marketing messages on product demand (Ganzach and Karsahi 1995, Bertrand et al 2010). In the context of incentives, as far as we know, Hossain and List (2012) is the only previous experimental study to test loss aversion in the field, finding that framing bonuses as losses improves the productivity of teams in a Chinese factory. In studies run concurrently to ours, Fryer et al. (2012) find that framing bonuses as losses improves teacher performance while List and Savikhin (2013) find no framing effects for student incentives to make healthy food choices. Krawczyk (2011) tests the effect of framing on risk taking on a final exam and finds no effect – the study does not examine the effect of framing on effort or overall performance.

[9]See Ball et al. (2001) and Huberman et al. (2004) on status; Blanes i Vidal and Nossol (2011), Tran and Zeckhauser (2012) and Barankay (2011) on relative performance feedback; and, Ariely et al. (2009) and DellaVigna et al. (2012) on image motivation and social pressure. For individuals who care about status and a positive self-image, non-pecuniary gifts carry additional utility when they remind oneself and others of a special achievement of the individual (see, e.g., Loewenstein and Issacharoff 1994 on the trophy value of rewards and Bénabou and Tirole 2006 on self-signaling).

however, the effectiveness of non-financial incentives is largely untested – particularly in terms of cost-effectiveness relative to monetary rewards.[10]

Finally, we address the role of timing of rewards. As we discussed above, students may have time preferences that lead to planning failures and underinvestment of effort in human capital. Most previous programs that have awarded incentives based on test performance have announced the incentive well in advance of the test using high school exit and achievement exams in Israel (Angrist and Lavy 2009) and Texas (Jackson 2010), and standardized tests for elementary/middle school students in Kenya (Kremer et al. 2009), India (Berry 2013), Ohio (Bettinger 2010), and New York City (Fryer 2011a). In the settings most similar to ours, Bettinger (2010) finds that incentives of up to $20 have a significant impact on third through sixth graders' performance in math but no impact on reading, social science, or science; and, Fryer (2011) finds no effect on math or reading test scores of offering incentives of up to $30 to fourth graders and $60 to seventh graders.

Studies that have announced incentives immediately before the test have typically distributed rewards with a delay. The evidence on such delayed rewards is mixed. O'Neil et al. (1996, 2004) find that delayed financial incentives can increase eighth grade test scores but have no effect on twelfth grade test scores, even at very high levels (up to $100 on a 10 question test).[11] In a similar design, Baumert and Demmrich (2001) find no effects of financial incentives on ninth grade test scores. These studies also find no treatment effects from non-financial incentives including feedback, ranking, goal setting, achievement certificates, and test scores counting towards classroom

[10] See for example Kohn (1999) for a review of the use of non-financial incentives. As we discuss below, O'Neil et al (1996) and Baumert and Demmrich (2001) test both financial and non-financial incentives for test performance.

[11] O'Neil et al. (2004) also offered an immediate incentive of up to $20 based on answering 2 practice questions correctly. However, all participants in both the incentive and control groups answered the questions correctly, precluding any measurement of a treatment effect.

grades.

In our experiments, we announce the rewards immediately before the incentivized test (with no advance notice) and in non-delayed treatments distribute rewards immediately after the test.[12] This ensures that students are choosing one-time effort in the immediate period only – i.e., there is no delay between investment decision-making, effort exertion and receipt of rewards. This feature allows us to ignore time discounting of effort, opportunity costs, and planning failures as well as human capital gains that may accrue from effort in previous periods (e.g., studying for the test). We can therefore isolate the extent to which student effort is responsive to incentives.

Within this structure, we vary both the size and timing of the incentives. Varying the size of the reward allows us to distinguish whether students are unable to respond to incentives (because they have a limited understanding of the production function) or whether they are unwilling to respond to incentives (because effort costs are high relative to the size of the reward). We also compare the effects of immediate rewards to the effects of delayed rewards (distributed one month after the test). This allows us isolate the extent to which discounting affects students' effort investment decisions.

3 Experimental Design

The field experiment was carried out in five waves in three low-performing school districts in and around Chicago: Bloom Township (Bloom), Chicago Heights (CH), and Chicago Public Schools (CPS).[13] The first wave was conducted in winter and

[12]To our best knowledge, a study produced concurrently to ours - Braun et al. (2011) - is the only other study to announce the incentive immediately before the test and distribute the reward immediately after the test. They offer a performance-based incentive of up to $30 to eighth and twelfth graders on a low stakes standardized test and find positive and significant treatment effects compared to a control group which received no incentive and a "fixed incentive" group which received $20 regardless of performance.

[13]Bloom Township and Chicago Heights are small school districts with approximately 3,000 students each. In contrast, CPS is the third largest school district in the U.S. with approximately

spring 2009 among high school sophomores at one high school in Bloom. The second wave took place in spring 2010 with a new cohort of Bloom sophomores. The third wave also took place in spring 2010 among 3rd-8th graders in seven schools in Chicago Heights. The final waves scaled up the Bloom and Chicago Heights experiments and were conducted among 2nd-8th graders in 26 CPS schools in fall 2010 and winter 2011.

The field experiment took place during regularly scheduled sessions of standardized diagnostic tests. These are low-stakes tests that students do not generally prepare for or have any external reason to do well on. Students take the tests three times a year in the fall, winter, and spring. They are computer-based and last between 15-60 minutes with students' results available immediately after the test ends.[14]

In each session, immediately before testing began, the test administrator announced the incentive and told students that they would receive the reward immediately (or a month) after the test ended if they improved upon their score from a prior testing session.[15] Immediately after the test ended, we handed out rewards to qualifying students, except in the case of delayed rewards which were distributed a month after testing.[16] Students received no advance notice of the incentives prior to the testing sessions.[17]

400,000 students.

[14]In Bloom, the experiment took place during the STAR Reading Assessment, which is adaptive and lasts about 15 minutes. In Chicago Heights, the experiment took place during the math portion of the ThinkLink Predictive Assessment Series, which lasts about 30 minutes (students there take the test four times per year including a pre-test at the beginning of the year). In CPS, the experiment took place during either the math or reading portion of the Scantron Performance Series, which each last about 60 minutes. Students are not time-constrained on any of the tests.

[15]The score students were told to improve upon in Bloom 2009 was fall 2008, in Bloom 2010 was fall 2009, in CH was winter 2010, in CPS 2010 was spring 2010, and in CPS 2011 was fall 2010.

[16]In CPS, about one-fifth of classes did not complete testing in a single session due to time constraints. In these cases, we returned to the school after every student had completed the test. Excluding these classes from the analysis does not affect the results.

[17]One week before testing, we sent home a consent form to parents stating that we would like their child to participate in a study to be conducted during the upcoming test, and that their child could receive financial or non-financial (where applicable) compensation for their participation. We did

Incentivized students were offered one of the following rewards: financial low ($10 cash), financial high ($20 cash) or non-financial (trophy). In the loss condition (financial high and non-financial) students received the reward at the start of the testing session and were informed that they would keep the reward if they improved and that they would lose the reward if they did not improve. Students also filled in a sheet confirming the receipt of the reward (and in CPS indicated on the form what they planned do with it) and kept the reward at their computer during testing. In the control groups, the test administrator either did not make any announcement (control - no statement) or encouraged students to improve on the test but did not offer any incentive to do so (control - statement).[18] This allows us to test whether there are effects due to the presence of the experimenters (we did not attend "no statement" treatments) or of merely requesting that the student improve. Scripts for the different treatments can be found in Appendix A. An overview of the treatments conducted is presented in Table 1.[19]

We randomized at the level of English class (Bloom) or school-grade (CH and CPS) and blocked the randomization on average baseline score, school (CH and CPS),

not specify the incentives and we sent the same consent form to the treatment and control groups. In Bloom and Chicago Heights, parents only needed to sign the consent form if they did *not* want their child to participate in the study. Less than 1% of parents opted out by returning the form. In CPS, parents needed to sign the consent form in order for their child to participate. 57% of parents returned the signed consent form prior to the fall 2010 wave and 71% of forms were returned prior to the winter 2011 wave. In order to participate, students in all sessions that we attended also signed a student assent form immediately before they took the test. All students opted into the study by signing the assent form. The analysis only includes students who met the consent criteria prior to treatment.

[18]In Chicago Heights, a second financial low (comparison) treatment and a second control-statement (comparison) treatment added a statement that we would compare a student's improvement to three other students with similar past scores. The non-financial treatment added a statement that we would take a photo of qualifying students and post it in their school. In CPS, control - statement students were additionally told (as incentivized students were) that they would learn their scores either immediately or with a one month delay (control - statement - delayed) after testing.

[19]The various waves included additional incentive treatments. To keep the analysis tractable, this paper reports the results from those incentives that are common across the settings. Information on the additional treatments and their results are available upon request.

grade (CH and CPS), and race/ethnicity (CH).[20] In cases where students participated in two testing sessions (Bloom 2009 and CPS 2010/2011), we re-randomized for the second session.[21] Thus, some students received the same treatment in both sessions, while others received a different treatment in the two sessions. In the two cases where students received incentives in a previous session (Bloom spring 2009 and CPS winter 2011) there was no reason for students to expect the experiments to continue, or if the experiments did continue, that they would receive a particular incentive. It is possible, however, that students anticipated there would be incentives in their second testing session. The results presented below are robust to restricting the sample to first-time incentives (i.e., students in their first testing session and those in their second session who were in the control group in the first session).

Tables 2a-2c report summary statistics by treatment group for pre-treatment characteristics in Bloom (2009 and 2010), Chicago Heights (2010), and CPS (2010 and 2011). The pre-treatment characteristics include baseline score on the tested subject, grade (CH and CPS), test subject (CPS), and the following demographics: gender, race/ethnicity, free or reduced price lunch status, and (in CH and CPS) eligibility for an Individualized Education Plan (IEP).[22] While the groups are generally balanced, the tables indicate the presence of some significant differences between individual incentive and pooled control (statement and no statement) groups as well as some imbalance in the overall distribution of students across treatments, with standard

[20]In Bloom, we blocked on average class baseline reading score. If the baseline score was not available, we blocked classes by their track: regular, remedial, or honors. In Chicago Heights and CPS, we blocked on average school-grade baseline math and reading scores.

[21]In the second CPS wave, we additionally blocked on treatment received in the first wave, math and reading scores in the first wave, and treatment received in a separate intervention that took place between the two waves.

[22]Baseline test score is a standardized pre-treatment test score. In Bloom 2009, fall 2008 serves as the baseline. In Bloom 2010, fall 2009 serves as the baseline. In Chicago Heights, winter 2010 serves as the baseline. In CPS, spring 2010 serves as the baseline. Eligibility for free/reduced lunch is a proxy for family income. Individualized Education Plans (IEPs) provide additional services to struggling students. IEP status was not available for Bloom students.

errors clustered by class (Bloom) or school-grade (CH and CPS).

In Bloom (Table 2a) there are no significant differences between the control and individual incentive groups. There is however some imbalance in the overall distribution of black students and students eligible for free or reduced price lunch. In Chicago Heights (Table 2b) the only individually significant differences are the proportion of black and Hispanic students in the non-financial treatment. There is also overall imbalance in baseline test scores and the distribution of Hispanic students across treatments. Finally, in CPS (Table 2c) the various treatment groups are balanced on average grade and baseline score (the immediate rewards non-financial incentive group has higher baseline scores than control significant at the $p < 0.1$ level). There are individually statistically significant differences (both positive and negative) in the proportion of math tests, as well as demographic measures in some groups. The only overall imbalance is in the proportion female across treatments. As shown below, the results are robust to including controls for baseline performance and other pre-treatment characteristics.

4 Results

Table 3 reports our basic results, pooling across grades, subjects, and schools, for all of our treatments in which the rewards were delivered immediately (as opposed to with a one month delay).[23] The dependent variable in all regressions is test score improvement (in standard deviation units) with standard errors clustered by class (Bloom) or school-grade (CH and CPS).[24] Column (1) presents treatment effect es-

[23] An analysis of the individual sessions and settings yields similar results.

[24] Improvement is measured as the difference between the standardized outcome score and the standardized score students were told to improve upon. Scores are standardized to have mean zero and standard deviation equal to 1. In Bloom, we standardize scores within each testing period using the full sample of Bloom students. In Chicago Heights, we standardize scores within each grade

timates absent any controls except for the session of the experiment. The second column adds controls for baseline score in the tested subject (score, score squared and score cubed), past treatment (incentives received in a previous session for Bloom spring 2009 and CPS winter 2011 students), test subject, school and grade (for CH and CPS students), teacher fixed effects (for Bloom students), and demographics (gender, race/ethnicity, free/reduced lunch eligibility, and (in CH and CPS) IEP status).[25] The omitted category in every regression is the pooled control (statement and no statement) group. There are no significant differences in performance between the control subgroups and pooling does not affect the results. This suggests that the treatment effects are due to the incentives rather than the presence of the experimenters or the mere encouragement to improve.

Result 1: Large and immediate monetary incentives lead to test score improvements, small monetary incentives do not

The first result that emerges from Table 3 is the power of large and immediate financial incentives to increase test scores. The point estimates of the $20 incentives (framed either as a gain or a loss) are consistently positive and statistically significant at conventional levels, with improvements ranging from 0.106 - 0.132 standard deviations. The large effects of these relatively modest financial incentives suggest that at baseline this population of students puts forth low effort in response to low (perceived) returns to achievement on standardized tests. The magnitude of the impact is equivalent to about 5 months' worth of learning on the test.[26] In contrast,

using the full sample of Illinois students. In CPS, we standardize scores within each grade, subject, and testing period using the full population of CPS students.

[25]In CPS winter 2011, we additionally control for whether a student received treatment in a separate intervention that took place between the two CPS waves.

[26]The month equivalent measure is based on the STAR Reading Assessment Instructional Reading Level. The Instructional Reading Level (IRL) is the grade level at which a student is at least 80% proficient. An IRL score of 6.6 (the average fall baseline score for Bloom 10th graders) indicates

however, we see little or no impact from the $10 incentives. As far as we know, ours is the first study to demonstrate that student responsiveness to incentives is sensitive to the size of the reward. One interpretation is that, at least for some students, effort costs may be relatively high.[27] Together these results provide evidence that students understand the production function for this task but require sufficient motivation to exert effort.

Result 2: Non-financial incentives also impact performance

Turning to our first behavioral intervention, we compare the effects of non-pecuniary rewards to the effects of both low and high monetary rewards, which allows us to price out the effects of non-financial incentives. The point estimates for non-pecuniary rewards (framed either as a gain or a loss) are only slightly smaller than those for the $20 treatment and much larger than those from the $10 treatment. Typically, the material cost of non-financial incentives is low – in our case, one trophy cost approximately $3. Hence, non-financial incentives are a potentially much more cost effective way of improving student performance than is paying cash. As we discussed above, non-pecuniary incentives are also attractive because schools tend to be more comfortable rewarding students with trophies, certificates, and prizes than they are with using cash incentives.

Result 3: Incentives framed as gains and losses have a similar impact

Our second behavioral intervention built on the large literature demonstrating the power of framing for influencing choices, especially in the gain/loss space. The

that a student is reading at the equivalent of 6th grade and 6 months (with 9 months in a school year).

[27]It may also be the case that relatively low financial incentives crowd out intrinsic motivation yielding smaller net effects. We address this concern below.

bottom two rows of Table 3 report the estimates for our "loss" treatments: one using a financial incentive, the other a prize. The coefficients are slightly larger than those from the analogous "gain" treatments in the top two rows, but are not statistically different. Given that incentives framed as losses do not induce greater test score gains in this context, the more standard gain frame is probably preferable from a policy perspective. Schools tend to be more comfortable offering students rewards rather than threatening to take them away – in part because, as the researchers witnessed, students often express distress at having to return their rewards.

Result 4: Rewards provided with a delay have no impact on student performance

Perhaps the most striking and important finding of our study is that delayed rewards proved completely ineffective in raising test scores, as shown in Table 4. The structure of the table matches that of Table 3, except that the coefficients reported correspond to treatments in which the rewards were given to the students only after a one month delay.[28] In stark contrast to virtually all of the rewards bestowed immediately after the test, all but one of the coefficients on the delayed reward treatments are negative, with the estimates not statistically significant. The effects of the pooled delayed treatments are significantly different from the analogous pooled immediate treatments at the $p < 0.01$ level. The divergence between the immediate and delayed rewards reflect either hyperbolic discounting or enormously high exponential discount rates (i.e. over 800 percent annually).

While these findings are consistent with previous research highlighting the high discount rates of children, it poses a challenge for educators and policymakers. Typ-

[28]Delayed rewards were only implemented in the CPS 2010 wave; hence, the regressions only include observations from that wave and do not include session controls. All the regressions control for the analogous immediate incentive treatments. In the loss condition with a delay, students returned the reward at the end of testing. A month after testing we re-distributed rewards to qualifying students.

ically, the results of the state-wide assessments are only available 1-2 months after the administration of the tests, making it difficult to provide immediate rewards for performance. More broadly, if similar discount rates carry over to other parts of the education production function, our results suggest that the current set of incentives may be leading to underinvestment in human capital.

In results $5-7$ below, we investigate heterogeneous treatment effects. Table 5 reports results for the immediate incentives split by age, test subject, and gender (the first column replicates the results for the full sample from Table 3).[29]

Result 5: Younger students respond more to incentives, especially non-financial incentives

The second and third columns of Table 5 estimate treatment effects for elementary grades and for middle/secondary grades, respectively.[30] The first result to emerge is that younger students are more responsive to incentives with large positive impacts in all treatments. In contrast, only the financial loss incentive yields significant positive effects among older students. Second, younger children are less incentive sensitive. In middle and high school grades the impact of the \$10 and \$20 incentives are significantly different with the low financial incentive actually decreasing performance. Unlike older students, elementary students do not respond differentially to low and high financial rewards. They are also particularly responsive to non-financial incentives, improving by about 0.2 standard deviations (an effect size twice as large as treatment estimates in the full sample).

[29]For space, we only present regressions that include the full set of covariate controls. Regressions that include only session controls yield similar results and are available upon request.

[30]Elementary school students include 2nd-5th graders in Chicago Heights and CPS. Middle school students include 6th-8th graders in Chicago Heights and CPS. Secondary school students include 10th graders in Bloom.

These results are sensible from a number of perspectives: younger children are less familiar with cash, might receive higher utility from the type of prize we were offering, and are also more likely to overestimate the value of non-financial rewards (for example, one third grader announced her estimated value of the $3 trophy to be $20). Together our findings suggest that among children with a limited understanding of monetary returns, non-financial rewards can be particularly effective at addressing underinvestment in education.

Result 6: Math scores respond much more strongly than reading scores

The fifth and sixth columns of Table 5 present treatment effects on math and reading tests respectively. The gains in math are very large ranging from roughly $0.1 - 0.3$ standard deviations. The math point estimates are all positive and mostly significant. With the notable exception of the low financial incentives treatment, the reading coefficients are also positive. In every case, however, the positive impact is smaller for reading than for math and these differences are significant in the financial incentive treatments. The most likely explanation for this result is that math scores are more sensitive to effort than reading. And, indeed, it is often the case that educational incentives have a greater impact on math than reading (e.g., Decker et al. 2004, Rockoff 2004, Jacob 2005, Dobbie and Fryer 2011).

Result 7: Suggestive evidence that boys are more responsive than girls

The final columns of Table 5 present results separately for boys and girls. Across the board, we see larger responses to our interventions for boys relative to girls. The biggest gaps emerge with low financial stakes and in the non-financial treatments, where gender differences are significant at the 10% level. Our findings with respect to gender are consistent with a wealth of prior research that shows boys tend to be

more sensitive to short-term incentives than girls, which may be due in part to gender differences in time preferences.[31]

Result 8: The introduction of rewards does not crowd out future effort

The use of financial incentives in the education context has been sharply criticized. Theoretically, the most compelling of these criticisms is that extrinsic rewards crowd out intrinsic motivation, rendering such approaches ineffective in the short run, and potentially detrimental in the long run if intrinsic motivation remains low after the monetary incentives have been removed.[32] However, on tasks where intrinsic motivation is already low or zero, external rewards are less likely to have such negative long-term effects.[33] It is also worth noting that several studies have tracked student performance after incentives are removed and generally find that students who received incentives continue to outperform the control group (see, e.g., Bettinger and Slonim 2007, Barrera-Osorio et al. 2011, Kremer et al. 2009, Levitt et al. 2010).[34]

We similarly explore whether the incentives have a detrimental impact on subsequent test performance. The richness of our design also permits us to learn whether spillovers differ between financial and non-financial incentives. Table 6 explores two different dimensions along which temporary incentives might distort future outcomes.

[31]Evidence on the effect of incentives by gender is mixed with longer term studies tending to find larger effects on girls (e.g. Angrist et al. 2009, Angrist and Lavy 2009) and shorter term studies finding larger effects among boys, particularly in the context of competition (Gneezy et al.2003, Gneezy and Rustichini 2004). Bettinger and Slonim (2007) and Castillo et al. (2011) find that boys are more impatient than girls.

[32]While this argument applies to extrinsic rewards in any form, monetary incentives are considered particularly insidious to intrinsic motivation.

[33]For further discussion see reviews by e.g., Eisenberger and Cameron 1996, Camerer and Hogarth 1999, Deci et al. 1999, Kohn 1999, Cameron and Pierce 2002. Frey and Oberholzer-Gee (1997) present a formal model and evidence from a field study of motivation crowding-out in an economic context.

[34]Additionally, Bettinger and Slonim (2007) find no evidence that their test performance incentive program erodes elementary school students' intrinsic motivation measured using student and teacher surveys.

The first two columns report the impact of the various treatments on test scores from a subsequent non-incentivized test taken in the same testing period, i.e. just hours or days later. Any increase or decrease in scores on this test would come only from an altered level of effort exerted on the test. Columns (3) and (4) report the impact of exposure to treatment today on test scores in the same subject, but when taking the exam in the next testing period, months later.[35] In neither case do we find strong evidence of either positive or negative spillovers from our incentive treatments. Roughly half the point estimates are positive and half negative, with only one of the coefficients statistically significant. There is some evidence suggesting that low financial incentives might have negative spillovers, as in Gneezy and Rustichini (2000a, 2000b).[36] There is similarly weak, but suggestive, evidence that the non-financial loss treatment generated positive spillovers on performance.

5 Conclusion

This study examines potential sources of underinvestment in education using various incentive schemes inspired by recent findings within behavioral economics. We explore the short-term effects of incentives on student effort and performance, varying the size and type of the rewards as well as their framing. We also offer rewards both immediately and with a delay. We find considerable promise for adding behavioral

[35]In columns (1) and (2), we regress math (reading) treatment on reading (math) improvement in the same period (fall 2010 or winter 2011 in CPS, spring 2010 in CH) for students who received treatment on their first subject test. In columns (3) and (4), we regress the student's treatment on her improvement on a standardized test taken in the subsequent period, controlling for any subsequent treatments when necessary (winter or spring 2011 in CPS, spring 2009 in Bloom). Improvement in CPS spring 2011 was measured with respect to winter 2011. Controls for past treatment include CPS fall 2010 treatment for CPS winter 2011 in column (2) and CPS spring 2011 in column (4); and whether a student received treatment in a separate intervention that took place between the two CPS waves for CPS winter 2011 in columns (2) and (4) and CPS spring 2011 in column (4).

[36]There is also suggestive evidence that the $10 incentive can have a negative impact on the incentivized test itself, with negative estimated effects in several subgroups including older students, girls and reading tests (Table 5).

insights to the educational policymaker's toolkit. Most importantly, seeking immediacy of rewards is fundamental in the presence of the discounting we observe, since most rewards from educational investment come only with great delay. This can potentially lead to dramatic underinvestment in the low stakes tasks students must engage in daily in order to accumulate human capital. Without near term incentives baseline effort among many students is likely to be low.

While there is concern that these types of incentives will crowd out intrinsic motivation, extrinsic rewards can also be used to foster intrinsic motivation and habit formation (Lepper et al 1973, Cameron et al 2005, Bettinger 2010). In affluent families parents often provide children with these kinds of near term rewards including positive feedback, praise, and explicit incentives. Low-income parents in contrast are less likely to offer their children incentives for effort and achievement (Gottfried et al 1998), which may contribute to teachers' struggles to maintain student motivation and focus in low-income schools. If immediate rewards increase students' estimated utility returns to education, then properly structured extrinsic rewards could potentially build (rather than crowd out) intrinsic motivation.

Such short-term rewards can also address problems related to planning failures and limited understanding of the production function. Students may not know the steps to take in order to improve their achievement on a test that is six months away. However, they may be able to effectively respond to performance-based incentives on interim tasks such as learning the daily lesson, completing homework, or focusing on a practice test.

Continuing to apply important elements of behavioral economics to issues within education can directly aid practitioners in need of fresh solutions to the urban school problem. Such behavioral insights can strengthen the impact and the cost-effectiveness of interventions in education. They can also be used as a stepping stone for empiricists

and experimentalists alike, who with the rich array of naturally-occurring data and experimental opportunities are in a unique position to examine theories heretofore untestable. Clearly, however, theory and empirical work must work symbiotically – there have been fewer theoretical advances that combine the best aspects of behavioral insights with issues germane to education. In this spirit, we hope that our study stimulates new work combining psychology and economics that not only deepens our understanding of empirical issues related to education, but also deepens our understanding of the important theoretical questions facing the field.

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Table 1: Overview of the Treatments

	Bloom		Chicago Heights	CPS	
	2009	2010	2010	2010	2011
Control - No statement	X				X
Control - Statement		X	X[a]	X[b]	X
Financial Low ($10)	X		X[a]	X	
Financial High ($20)	X	X	X	X	X
Non-Financial (Trophy)			X	X	X
Financial Loss		X		X	X
Non-Financial Loss				X	X
Financial Delayed				X	
Non-Financial Delayed				X	
Financial Loss Delayed				X	
Non-Financial Loss Delayed				X	
Grade level	Secondary		Elementary/Middle	Elementary/Middle	
Test subject	Reading		Math	Math or Reading	

Note: Financial Loss, Financial Delayed and Financial Loss Delayed all received Financial High ($20) incentives. Non-Financial Loss, Non-Financial Delayed and Non-Financial Loss Delayed all received Non-Financial (trophy) incentives.

[a] Control and Financial Low ($10) are each pooled with treatments that add a statement that a student's improvement will be compared to three other students with similar past scores (see Appendix A for scripts). The comparison statement did not significantly affect test performance at the 10% level.

[b] Control - Statement is pooled with Control - Statement - Delayed which states that students will learn their scores"one month after the test" instead of "immediately after the test" (see Appendix A for scripts). The delayed statement did not significantly affect test performance at the 10% level.

Table 2a: Baseline Characteristics by Treatment Group: Bloom

	Control	Financial Low	Financial High	Financial Loss	F-Test p-value
Observations	240	143	288	125	
Baseline Test Score	0.133	0.132	−0.074	0.289	0.543
	(0.965)	(0.893)	(0.972)	(1.035)	
Female	0.533	0.517	0.472	0.464	0.659
	(0.500)	(0.501)	(0.500)	(0.501)	
Black	0.585	0.413	0.586	0.449	0.029
	(0.494)	(0.494)	(0.493)	(0.500)	
Hispanic	0.314	0.448	0.331	0.390	0.284
	(0.465)	(0.499)	(0.471)	(0.490)	
Free or Reduced	0.729	0.720	0.737	0.822	0.097
Price Lunch	(0.446)	(0.450)	(0.441)	(0.384)	

Note: The table reports group means pooling the Bloom 2009 and Bloom 2010 waves. Standard deviations are reported in parentheses. Baseline score is standardized within testing period to have mean zero and standard deviation one using the full sample of Bloom students. The joint F-test measures the probability that the means are equal to one another, clustering by class. Asterisks indicate a difference of means (compared to control with standard errors clustered by class) significant at the 10/5/1 percent level.

Table 2b: Baseline Characteristics by Treatment Group: Chicago Heights

	Control	Financial Low	Financial High	Non-Financial	F-Test p-value
Observations	160	165	29	69	
Baseline Test Score	−0.457	−0.510	−0.421	−0.682	0.025
	(0.772)	(0.781)	(1.078)	(0.775)	
Grade	6.200	5.133	5.414	5.072	0.803
	(2.070)	(1.446)	(1.402)	(1.229)	
Female	0.513	0.497	0.448	0.449	0.965
	(0.501)	(0.502)	(0.506)	(0.501)	
Black	0.513	0.461	0.310	0.290*	0.368
	(0.501)	(0.500)	(0.471)	(0.457)	
Hispanic	0.363	0.461	0.621	0.623**	0.038
	(0.482)	(0.500)	(0.494)	(0.488)	
Free or Reduced Price Lunch	0.863	0.891	0.897	0.928	0.204
	(0.345)	(0.313)	(0.310)	(0.261)	
Individualized Education Plan (IEP)	0.074	0.069	0.034	0.101	0.857
	(0.263)	(0.255)	(0.186)	(0.304)	

Note: The table reports group means. Standard deviations are reported in parentheses. Baseline score is standardized within grade to have mean zero and standard deviation one using the full sample of Illinois students. The joint F-test measures the probability that the means are equal to one another, clustering by school-grade. Asterisks indicate a difference of means (compared to control with standard errors clustered by school-grade) significant at the 10/5/1 percent level.

Table 2c: Baseline Characteristics by Treatment Group: Chicago Public Schools (CPS)

	Control	*Immediate Rewards*					*Delayed Rewards*				
		Financial Low	Financial High	Non-Financial	Financial Loss	Non-Financial Loss	Financial High	Non-Financial	Financial Loss	Non-Financial Loss	F-Test p-value
Observations	2910	105	644	545	715	706	85	82	30	74	
Baseline Test Score	−0.007	0.058	0.098	0.148*	−0.026	0.051	0.036	−0.036	−0.173	0.152	0.616
	(0.882)	(0.871)	(0.852)	(0.898)	(0.865)	(0.854)	(0.816)	(0.859)	(0.862)	(0.950)	
Grade	5.109	5.619	5.351	5.209	4.752	5.021	5.235	4.793	4.833	4.838	0.977
	(1.858)	(1.361)	(1.714)	(1.948)	(1.937)	(1.896)	(0.811)	(1.577)	(1.599)	(2.054)	
Subject – Math	0.307	0.238	0.253	0.178	0.274	0.358	0.353	0.171	0.000***	0.000***	0.740
	(0.461)	(0.428)	(0.435)	(0.383)	(0.446)	(0.480)	(0.481)	(0.379)	(0.000)	(0.000)	
Female	0.508	0.686***	0.547	0.508	0.585***	0.482	0.518	0.432*	0.367	0.541	0.000
	(0.500)	(0.466)	(0.498)	(0.500)	(0.493)	(0.500)	(0.503)	(0.498)	(0.490)	(0.502)	
Black	0.983	0.990	0.978	0.993**	0.989	0.983	0.976	0.988	0.933	1.000***	0.312
	(0.130)	(0.098)	(0.146)	(0.085)	(0.105)	(0.129)	(0.152)	(0.111)	(0.254)	(0.000)	
Free or Reduced Price Lunch	0.988	1.000***	0.989	0.984	0.992	0.988	1.000***	1.000***	1.000***	1.000***	0.932
	(0.108)	(0.000)	(0.105)	(0.125)	(0.089)	(0.110)	(0.000)	(0.000)	(0.000)	(0.000)	
Individualized Education Plan (IEP)	0.092	0.089	0.100	0.089	0.113	0.101	0.050	0.123	0.069	0.048	0.595
	(0.289)	(0.286)	(0.300)	(0.285)	(0.317)	(0.301)	(0.219)	(0.331)	(0.258)	(0.216)	

Note: The table reports group means pooling the CPS 2010 and CPS 2011 waves. Standard deviations are reported in parentheses. Baseline score is standardized within grade, subject and testing period to have mean zero and standard deviation one using the full sample of CPS students. The joint F-test measures the probability that the means are equal to one another, clustering by school-grade. Asterisks indicate a difference of means (compared to control with standard errors clustered by school-grade) significant at the 10/5/1 percent level.

Table 3: Treatment Effects on Test Score Improvement

	(1)	(2)
Financial Low	0.005	0.006
	(0.046)	(0.045)
Financial High	0.113***	0.106***
	(0.038)	(0.033)
Non-Financial	0.045	0.099**
	(0.041)	(0.043)
Financial Loss	0.132***	0.123***
	(0.042)	(0.037)
Non-Financial Loss	0.095**	0.103**
	(0.043)	(0.040)
Session	Yes	Yes
Other Covariates		Yes
Observations	6844	6844
Classes/School-Grades	226	226

Note: The table reports OLS estimates for treatment effects on test score improvement in standard deviation units for pooled waves in Bloom, Chicago Heights and CPS. Robust standard errors clustered by class in Bloom and by school-grade in Chicago Heights and CPS are reported in parentheses. The omitted category is the pooled control group. Column (1) controls for session. Column (2) adds controls for standardized baseline score on the tested subject (score, score squared and score cubed), past treatment, school, grade, teacher, test subject and demographics (gender, race/ethnicity, free/reduced lunch status and IEP status), where applicable. Asterisks indicate significance at the 10/5/1 percent level.

Table 4: Effect of Delayed Rewards

	CPS	
	(1)	(2)
Delayed Financial High	-0.078	-0.071
	(0.095)	(0.110)
Delayed Non-Financial	-0.171*	-0.121
	(0.093)	(0.087)
Delayed Financial Loss	-0.005	0.154
	(0.086)	(0.149)
Delayed Non-Financial Loss	-0.096*	-0.088
	(0.054)	(0.092)
Immediate Incentive Treatments	Yes	Yes
Other Covariates		Yes
Observations	1362	1362
School-Grades	81	81

Note: The table reports OLS estimates for treatment effects on test score improvement in standard deviation units for the CPS 2010 wave. Robust standard errors clustered by school-grade are reported in parentheses. The omitted category is the pooled control group. Column (1) controls for immediate incentive treatments (financial, non-financial, financial loss and non-financial loss). Column (2) adds controls for standardized baseline score on the tested subject (score, score squared and score cubed), school, grade, test subject and demographics (gender, race/ethnicity, free/reduced lunch status and IEP status). Asterisks indicate significance at the 10/5/1 percent level.

Table 5: Heterogeneous Treatment Effects

	All	*Age*			*Subject*			*Gender*		
	Students	Elementary	Middle/ Secondary	p-value	Math	Reading	p-value	Male	Female	p-value
Financial Low	0.006	0.164	−0.102**	0.032	0.159**	−0.137***	0.000	0.075	−0.054	0.079
	(0.045)	(0.115)	(0.050)		(0.065)	(0.049)		(0.059)	(0.056)	
Financial High	0.106***	0.150***	0.069*	0.214	0.306***	0.058*	0.001	0.118***	0.088**	0.533
	(0.033)	(0.055)	(0.038)		(0.071)	(0.035)		(0.045)	(0.036)	
Non-Financial	0.099**	0.149**	0.077	0.466	0.082	0.072	0.920	0.153***	0.048	0.079
	(0.043)	(0.071)	(0.070)		(0.087)	(0.055)		(0.054)	(0.051)	
Financial Loss	0.123***	0.128**	0.160***	0.629	0.338***	0.072**	0.005	0.162***	0.086**	0.197
	(0.037)	(0.055)	(0.040)		(0.093)	(0.035)		(0.052)	(0.043)	
Non-Financial Loss	0.103**	0.252***	−0.066	0.000	0.097*	0.086*	0.877	0.151***	0.054	0.099
	(0.040)	(0.047)	(0.055)		(0.054)	(0.049)		(0.057)	(0.041)	
Session	Yes	Yes	Yes		Yes	Yes		Yes	Yes	
Other Covariates	Yes	Yes	Yes		Yes	Yes		Yes	Yes	
Observations	6844	3462	3382		2050	4794		3302	3540	
Classes/School-Grades	226	118	122		93	173		226	226	

Note: The table reports OLS estimates for treatment effects on test score improvement in standard deviation units for various subgroups: elementary (grades 2-5 in Chicago Heights and CPS), middle/secondary (grades 6-8 in Chicago Heights and CPS and grade 10 in Bloom), math (Chicago Heights and CPS), reading (Bloom and CPS), male and female. Columns (4), (7) and (10) report p-values resulting from a test of equal coefficients between age, test subject and gender groups, respectively. Robust standard errors clustered by class in Bloom and by school-grade in Chicago Heights and CPS are reported in parentheses. The omitted category is the pooled control group. All regressions control for session, standardized baseline score on the tested subject (score, score squared and score cubed), past treatment, school, grade, teacher, test subject and demographics (gender, race/ethnicity, free/reduced lunch status and IEP status), where applicable. Asterisks indicate significance at the 10/5/1 percent level.

Table 6: Treatment Effects on Future Tests

	Subsequent Subject Same Test Session		Same Subject Subsequent Session	
	(1)	(2)	(3)	(4)
Financial Low	0.054	-0.102	-0.136	-0.058
	(0.074)	(0.084)	(0.084)	(0.086)
Financial High	0.072	-0.016	-0.024	0.005
	(0.056)	(0.045)	(0.061)	(0.056)
Non-Financial	0.038	-0.036	-0.056	-0.010
	(0.070)	(0.050)	(0.068)	(0.047)
Financial Loss	0.066	0.047	0.069	0.032
	(0.066)	(0.049)	(0.053)	(0.044)
Non-Financial Loss	0.038	0.094**	0.037	0.078
	(0.056)	(0.047)	(0.067)	(0.049)
Session	Yes	Yes	Yes	Yes
Subsequent Treatment			Yes	Yes
Other Covariates		Yes		Yes
Observations	4238	4238	5502	5502
Classes/School-Grades	166	166	180	180

Note: The table reports OLS estimates for treatment effects on test score improvement in standard deviation units for the subsequent subject (Chicago Heights and CPS) and the subsequent test session (Bloom 2009 wave and CPS). Robust standard errors clustered by class in Bloom and by school-grade in Chicago Heights and CPS are reported in parentheses. The omitted category is the pooled control group. All columns control for session. Columns (3)-(4) include controls for treatment (if any) on the subsequent test. Columns (2) and (4) add controls for baseline score on the tested subject (score, score squared and score cubed), past treatment, school, grade, teacher, test subject and demographics (gender, race/ethnicity, free/reduced lunch status and IEP status), where applicable. Asterisks indicate significance at the 10/5/1 percent level.

A Appendix: Administrator Scripts

A.1 Bloom

Common to all treatments

To the teacher:

Please read the following statement to your students immediately before they begin the STAR test (after you have given them your regular instructions for testing):

Bloom 2009

Financial Low ($10) You are about to take the STAR Reading Assessment. You also took the STAR Reading Assessment in the fall. If your score on the STAR today is higher than your score in the fall, you will receive $10. You will be paid at the end of the test.

Please fill out your name, signature and date on the assent form. You will turn this in at the end of the test.

Financial High ($20) You are about to take the STAR Reading Assessment. You also took the STAR Reading Assessment in the fall. If your score on the STAR today is higher than your score in the fall, you will receive $20. You will be paid at the end of the test.

Please fill out your name, signature and date on the assent form. You will turn this in at the end of the test.

Bloom 2010

Control - Statement

You are about to take the STAR Reading Assessment. You also took the STAR

Reading Assessment in the fall. Please try to improve your score from the fall.

Financial High ($20) You are about to take the STAR Reading Assessment. You also took the STAR Reading Assessment in the fall. If your score on the STAR today is higher than your score in the fall, you will receive $20. You will be paid at the end of the test.

Please fill out your name, signature and date on the assent form. You will turn this in at the end of the test.

Financial Loss ($20) You are about to take the STAR Reading Assessment. You also took the STAR Reading Assessment in the fall. Please try to improve your score from the fall.

In front of you is an envelope that contains $20. Please open the envelope to confirm that there is $20 inside. [*Wait for students to open envelope and sign confirmation form.*]

If you improve your score from the fall, you will get to keep the $20. If you do not improve your score from the fall, you will not get to keep the $20. You will have to return the $20 immediately after the test.

A.2 Chicago Heights

Common to all treatments

To the teacher:

Please read the following statement to your students immediately before they begin the ThinkLink test (after you have given them your regular instructions for testing):

Control - Statement

You are about to take the ThinkLink Learning test. You also took ThinkLink in the winter. Please try to improve your score from the winter.

Control - Statement - Comparison

You are about to take the ThinkLink Learning test. You also took ThinkLink in the winter. Please try to improve your score from the winter. We will compare your improvement to 3 other students who had the same score as you in the winter.

Financial Low ($10)

You are about to take the ThinkLink Learning test. You also took ThinkLink in the winter. Please try to improve your score from the winter. If you improve your score from the winter, you will receive $10. You will be paid in cash immediately after the test.

Financial Low ($10) - Comparison

You are about to take the ThinkLink Learning test. You also took ThinkLink in the winter. Please try to improve your score from the winter. We will compare your improvement to 3 other students who had the same score as you in the winter. If you improve your score from the winter, you will receive $10. You will be paid in cash immediately after the test.

Financial High ($20)

You are about to take the ThinkLink Learning test. You also took ThinkLink in the winter. Please try to improve your score from the winter. If you improve your score from the winter, you will receive $20. You will be paid in cash immediately after the

test.

Non-Financial (Trophy)

You are about to take the ThinkLink Learning test. You also took ThinkLink in the winter. Please try to improve your score from the winter. If you improve your score from the winter, you will receive this trophy and we will post a photo like this of you in the class. [*SHOW SAMPLE TROPHY AND PHOTO.*] You will receive the trophy and be photographed immediately after the test.

A.3 Chicago Public Schools (CPS)

Common to all treatments

To the teacher:

Please read the following statement to your students immediately before they begin the Scantron test (after you have given them your regular instructions for testing):

CPS 2010

Control - Statement

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring. You will learn your score immediately after the test.

Control - Statement - Delayed

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring. You will learn your score one month after

the test.

Financial Low ($10)

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring. If you improve your score from the spring, you will receive $10. You will learn your score and be paid in cash immediately after the test.

Financial High ($20)

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring. If you improve your score from the spring, you will receive $20. You will learn your score and be paid in cash immediately after the test.

Financial High ($20) - Delayed

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring. If you improve your score from the spring, you will receive $20. You will learn your score and be paid in cash one month after the test.

Financial Loss ($20)

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring.

You are being given an envelope that contains $20. Please open the envelope to make sure that there is $20 inside. Please sign the form that says that this is your $20. And write down what you will do with your $20. [*Wait for students to open envelope*

and complete the confirmation form.]

If you improve your score from the spring, you will get to keep your $20. If you do not improve your score from the spring, you will have to return your $20. You will learn your score and whether you get to keep your $20 immediately after the test

Financial Loss ($20) - Delayed

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring.

You are being given an envelope that contains $20. Please open the envelope to make sure that there is $20 inside. Please sign the form that says that this is your $20. And write down what you will do with your $20.[*Wait for students to open envelope and complete the confirmation form.*]

If you improve your score from the spring, you will get to keep your $20. If you do not improve your score from the spring, you will have to return your $20. You will learn your score and whether you get to keep your $20 one month after the test.

Non-Financial (Trophy)

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring. If you improve your score from the spring, you will receive this trophy [*SHOW SAMPLE TROPHY*]. You will learn your score and receive the trophy immediately after the test.

Non-Financial (Trophy) - Delayed

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring. If you improve your score from the spring, you will receive this trophy [*SHOW SAMPLE TROPHY*]. You will learn your score

and receive the trophy one month after the test.

Non-Financial Loss (Trophy)

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring.

You are being given a trophy. Please sign the form that says that this is your trophy. And write down what you will do with your trophy. [*Wait for students to complete the confirmation form.*]

If you improve your score from the spring, you will get to keep the trophy [*SHOW SAMPLE TROPHY*]. If you do not improve your score from the spring, you will have to return your trophy. You will learn your score and whether you get to keep your trophy immediately after the test.

Non-Financial Loss (Trophy) - Delayed

You are about to take the Scantron test. You also took Scantron in the spring. Please try to improve your score from the spring.

You are being given a trophy. Please sign the form that says that this is your trophy. And write down what you will do with your trophy. [*Wait for students to complete the confirmation form.*]

If you improve your score from the spring, you will get to keep the trophy [*SHOW SAMPLE TROPHY*]. If you do not improve your score from the spring, you will have to return your trophy. You will learn your score and whether you get to keep your trophy one month after the test.

CPS 2011

Control - Statement

You are about to take the Scantron test. You also took Scantron in the fall. Please try to improve your score from the fall. You will learn your score immediately after the test.

Financial High ($20)

You are about to take the Scantron test. You also took Scantron in the fall. Please try to improve your score from the fall. If you improve your score from the fall, you will receive $20. You will learn your score and be paid in cash immediately after the test.

Financial Loss ($20)

You are about to take the Scantron test. You also took Scantron in the fall. Please try to improve your score from the fall.

You are being given an envelope that contains $20. Please open the envelope to make sure that there is $20 inside. Please sign the form that says that this is your $20. And write down what you will do with your $20. [*Wait for students to open envelope and complete the confirmation form.*]

If you improve your score from the fall, you will get to keep your $20. If you do not improve your score from the fall, you will have to return your $20. You will learn your score and whether you get to keep your $20 immediately after the test

Non-Financial (Trophy)

You are about to take the Scantron test. You also took Scantron in the fall. Please try to improve your score from the fall. If you improve your score from the fall, you

will receive this trophy [*SHOW SAMPLE TROPHY*]. You will learn your score and receive the trophy immediately after the test.

Non-Financial Loss (Trophy)

You are about to take the Scantron test. You also took Scantron in the fall. Please try to improve your score from the fall.

You are being given a trophy. Please sign the form that says that this is your trophy. And write down what you will do with your trophy. [*Wait for students to complete the confirmation form.*]

If you improve your score from the fall, you will get to keep the trophy [*SHOW SAMPLE TROPHY*]. If you do not improve your score from the fall, you will have to return your trophy. You will learn your score and whether you get to keep your trophy immediately after the test.