Conditional Cash Penalties in Education: Evidence from the Learnfare Experiment

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Abstract

Wisconsin’s influential Learnfare initiative is a conditional cash penalty program that lowers a family’s welfare benefit when covered teens fail to meet school attendance requirements. A 10-county random-assignment evaluation of this Learnfare initiative suggested that it had little or no effect on school enrollment and attendance. This study presents a reanalysis of the data from this Learnfare evaluation. In Milwaukee County, the Learnfare procedures were poorly implemented and the random-assignment process failed to produce balanced baseline traits. However, the data from the nine remaining counties indicate that Learnfare increased enrollment and attendance by 4 percent (effect sizes = 0.08) and the hypothesis of a common treatment effect sustained throughout the six-semester study period could not be rejected. These treatment effects were largely concentrated among those who were teen parents or school dropouts at baseline. For example, among baseline dropouts, Learnfare increased enrollment by 31 percent (effect size = 0.29) and attendance by 23 percent (effect size = 0.18).

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“Eighty percent of success is showing up.” - Woody Allen

1 Introduction

The recent growth in economic inequality and the well-established importance of education for economic success have created a focused interest in identifying specific policies and practices that can promote the human-capital accumulation of at-risk youth. Some of the most fundamental antecedents to the development of cognitive achievement involve a student’s basic non-cognitive traits like academic engagement and motivation. However, the deterioration of family environments in recent decades and the relative lack of corresponding school and community supports may disadvantage the most needy children with respect to the development of these instrumentally relevant traits. Concerns like these have motivated a growing number of rigorously evaluated initiatives that seek to leverage academic engagement and improve student achievement through the use of explicit and targeted cash incentives. Several recent studies have focused on the effects of providing cash incentives linked directly to the test scores and course performance of K-12 and post-secondary students in developed nations (e.g., Angrist and Lavy 2008, Angrist, Lang, and Oreopolous 2009, Bettinger 2009, Leuven, Oosterbeek, and van der Klaauw, forthcoming, and Richburg-Hayes et al. 2009). In developing countries, the proliferation of “conditional cash transfer” (CCT) programs have provided family-based financial incentives for school attendance and the utilization of social services (e.g., Handa and Davis 2006).

In this study, I examine the effects of Wisconsin’s influential Learnfare program. Learnfare, which began in 1988, linked the generosity of a family’s welfare grant to whether teens covered by this grant met basic school attendance targets. This program has been enormously influential in shaping other state
policies. Thirty-eight states currently take advantage of the flexibility created by the 1996 Federal welfare reforms to craft similar policies that link school attendance and welfare receipt (Education Commission of the States, 2007).

The design of Learnfare resembles that of conventional conditional cash transfer (CCT) programs like Mexico’s PROGRESA (currently called Oportunidades) in linking a family-based grant to attendance targets. However, it also differs in a potentially important program detail. CCT policies like PROGRESA provided a cash grant upon meeting program requirements. In contrast, Learnfare could be termed a conditional cash penalty (CCP) program in that it reduces an extant welfare grant for failure to meet program requirements. Because of the evidence that people exhibit an asymmetric aversion to income losses (Kahneman and Tversky 1979), this aspect of Learnfare policies may amplify its behavioral effects.

Notably, Learnfare also differs from other recently studied incentive programs in developed countries by leveraging family involvement instead of directly targeting students with cash incentives. One other design feature of Learnfare is also noteworthy. The psychological literature on the use of extrinsic rewards in education suggests that such rewards can unintentionally reduce intrinsic motivation when students feel they are likely to lack the capacity to meet the requirements. By targeting the comparatively simple issue of school attendance rather than achievement targets, Learnfare may be particularly unlikely to generate these unintended consequences.

However, despite these potentially compelling design features, a 10-county random-assignment evaluation of Wisconsin’s Learnfare program suggested that it had at best modest and short-term effects on its targeted enrollment and attendance outcomes (Frye and Caspar 1997). In this study, I re-examine the data from that random-assignment study. In particular, I exploit panel-based
econometric specifications based on pooling the available enrollment and attendance data from the six-semester study period. These specifications increase the statistical precision of the estimated treatment effects. Furthermore, they provide a unified framework for assessing the impact of study attrition and the quality of the random assignment results. This research design also provides a framework for formal hypothesis tests related to the dynamic treatment effects of Learnfare assignment (i.e., distinguishing short and long-term effects).

The results of this analysis indicate that, in Milwaukee county, the county-based random-assignment procedures did not produce balanced baseline traits. Specifically, teen parents were significantly more likely to be exempted from Learnfare’s requirements. Furthermore, legal challenges weakened the Learnfare requirements in this county while logistical challenges related to the accurate tracking of attendance data made the program comparatively slow and capricious. For these reasons, this analysis focuses largely on the nine remaining counties that participated in the study where the program implementation was relatively good and the random-assignment procedures appear to have performed well.

The results based on these counties indicate that random assignment to the Learnfare restrictions generated statistically significant improvements in school enrollment and attendance of approximately 4 percent (effect sizes = 0.08). Attrition from the study compromises the statistical power of inferences about the longer-term effects of this random assignment. However, the hypothesis that these treatment effects were the same throughout the study period cannot be rejected. Furthermore, the estimated treatment effects of Learnfare were particularly large for teens who were identified as dropouts at baseline (i.e., 31 and 23 percent increases in enrollment and attendance, respectively). This study concludes with a discussion of the cost-effectiveness of Learnfare policies.
and by underscoring the significant policy design and implementation lessons from Wisconsin’s experience with Learnfare.

2 Financial Incentives for Students

The notion that financial incentives will influence behavior in the expected directions is commonplace in economics. In contrast, an extensive literature in psychology (Deci, Koestner, and Ryan 2001) that began with a classic laboratory experiment by Deci (1971), suggests that extrinsic rewards in education can substantially undermine student performance by decreasing their intrinsic interest in the targeted tasks. However, Cameron (2001) argues that this interpretation conflates the heterogeneous effects of extrinsic rewards for individuals with high and low levels of initial intrinsic motivation. When students lack intrinsic motivation, external incentives can improve academic outcomes (Cameron and Pierce 2002). However, for students who already possess intrinsic motivation, there is evidence that external rewards can be harmful. In a review of this literature, Camerer and Hogarth (1999) also underscore the importance of whether the task targeted with financial incentives is “effort responsive.” With regard to both of these concerns, Learnfare would appear to be well designed. Because Learnfare applies only to economically disadvantaged families (i.e., those receiving welfare), it may target teens with comparatively low baseline levels of intrinsic motivation. And, because Learnfare is linked to school attendance and not academic performance, most covered teens should feel they have the capacity to avoid the financial penalties.

A surprisingly large number of recent random-assignment evaluations have examined the effects of extrinsic education-related awards in field settings. Per-

\footnote{Writing from an economics perspective, Bénabou and Tirole (2003) explicate the determinants of intrinsic and extrinsic motivation in a principal-agent model where agents infer information about themselves and the task at hand from principal’s provision of encouragement and rewards (i.e., the “looking-glass self”).}
haps, the most well-known of these program evaluations involves Mexico’s seminal conditional cash transfer (CCT) program, which was originally called PROGRESA. This program, which has been replicated in multiple countries, provided cash payments to parents every two months conditional on children meeting school attendance goals. Evaluations of this program found that it generated significant improvements in school enrollment as well as other outcomes (e.g., Skoufias and McClafferty 2001). In a random-assignment study conducted in Kenya, Kremer et al. (2004) provided financial awards (i.e., cash grants and school fees) to adolescent girls who met test-score targets. This treatment increased test scores by 0.15 standard deviations and exhibited program externalities in that it also increased the academic performance of boys (who were ineligible) and girls with low baseline scores (who were unlikely to earn rewards).

Several of the studies conducted in developed nations have focused on post-secondary students. For example, Angrist, Lang, and Oreopolous (2009) evaluated the direct and interactive effects of financial rewards linked to GPA performance and academic support services for first-year students at a large Canadian university. The financial rewards, particularly in combination with the offer of support services, improved the performance of female students but not male students. Leuven, Oosterbeek, and van der Klaauw (forthcoming) evaluated the effect of providing cash rewards of different sizes to students at the University of Amsterdam who completed their first-year credit requirements. They found that these rewards improved the performance of students whose measured performance in high school mathematics was high but lowered the performance of students whose prior mathematics achievement was weaker, an effect interpreted as consistent with the degradation of intrinsic motivation. A third random-assignment, post-secondary study (Richburg-Hayes et al. 2009) evaluated the effects of providing financial rewards to parents planning to attend or already
attending a community college in Louisiana. These financial incentives, which were linked to enrollment and GPA targets, improved the number of credits earned, longer-term college persistence as well as measures of motivation.

Two other recent random-assignment studies in developed countries evaluated the effects of financial incentives at elementary and secondary levels. Angrist and Lavy (2008) examine the effects of a school-level policy providing cash incentives for Israeli students to complete a matriculation certificate required for post-secondary schooling. The results of this cluster-randomized trial indicate that cash incentives increased the performance of girls but had no effects on boys. Bettinger (2009) presents an evaluation of cash incentives linked to performance on standardized tests for elementary-school students in a low-income section of eastern Ohio. These incentives increased scores in mathematics (effect size = 0.15) and did not lower measures of intrinsic motivation but had no detectable effects on reading, social science, and science scores. Similar K-12 studies (Medina 2008, Vargas 2009) are ongoing in several cities where student-level financial incentives are linked to attendance and grades (Washington, DC), test scores (New York City), and grades alone (Chicago).

In addition to these recent studies, six other random-assignment studies evaluated programs, which, like Learnfare, linked the threat of financial sanctions to school attendance. Campbell and Wright (2005) argue that two of these programs (Maryland’s Primary Prevention Initiative and Delaware’s A Better Chance program) particularly resembled Wisconsin’s seminal Learnfare program in that they targeted teen welfare recipients and relied primarily on the threat of sanctions rather than an expansion of case-management or support services. These two programs appeared to have negligible effects on school enrollment and attendance (Stoker and Wilson 1998, Fein et al. 2001). The four other programs (i.e., the Teenage Parent Demonstration Program, Ohio’s Learn-
ing, Earning, and Parenting Program, California’s Cal-Learn Demonstration Project, and San Diego County’s School Attendance Demonstration Project) largely targeted teen parents on welfare and blended the threat of sanctions with program features such as intensive case management, support services and financial bonuses for performance. Evaluations of these initiatives suggest that they did increase school enrollment and, to a lesser extent, attendance (Maynard 1993, Bos and Fellerath 1997, Mauldon et al. 2000, and Jones et al. 2002). However, Campbell and Wright (2005) suggest that these comparative results imply that financial sanctions are less likely to be effective when used in isolation from related services and case management.

Taken as a whole, the field-experimental literature on extrinsic rewards in education provides virtually no evidence that such policies have unintended negative consequences, contradicting the concerns that have dominated the lab-experimental literature from psychology. However, the evidence that extrinsic rewards and penalties are consistently effective in promoting targeted outcomes is decidedly mixed. This pattern of robust treatment effects and null findings suggests that program-design details, implementation quality and participant targeting are important policy parameters. In the next section, I describe Wisconsin’s Learnfare program in more detail.

3 Wisconsin’s Learnfare Program

In mid 1980’s, the state of Wisconsin was in the vanguard of states that utilized increased Federal flexibility (i.e. waivers) to experiment with the design and implementation of its welfare programs. Wisconsin’s “first wave” of waiver demonstrations both reduced the work disincentives for welfare recipients and expanded existing job-search and training requirements to the mothers of preschool children. However, the “centerpiece of the first round of Wisconsin initia-
tives” (Wiseman 1996) was the new Learnfare policy that linked welfare receipt to the school attendance of covered teens. The philosophical motivation for these changes was rooted in an interpretation of social-contract theory (e.g., Mead 1986) which argues that the receipt of welfare creates an implicit obligation for the recipient to undertake activities (e.g., employment, job training, and school attendance) that can break cycles of economic dependency.

Learnfare required that teens in families receiving welfare, including teen parents, attend school regularly if they had not graduated from high school or completed an equivalency degree. Specifically, school attendance records were reviewed upon initial application for welfare and twice a year thereafter. Teens who were not enrolled in school (and who had not graduated from high school, completed an equivalency degree or shown good cause) were removed from their family’s welfare grant until school enrollment was established.

If a review indicated that an enrolled teen had 10 or more unexcused full-day absences in a semester, they were designated as having poor attendance and were subjected to monthly monitoring. Families on monthly monitoring received monthly notices that reminded them of Learnfare’s attendance requirement and offered services designed to assist with school-attendance problems. However, when monthly monitoring indicated that a student had more than 2 unexcused, full-day absences in a month, the family was informed that it would face a 1-month benefit sanction unless it could show good cause for the absences. The amount of the sanction depended on the family’s status. For example, the sanction for a single-parent with two children would be approximately $80 per month while, for a teen parent living alone, the sanction would be $190 (Quinn and Magill 1994). According to Frye and Caspar (1997), these sanction amounts

\[\text{However, Wisconsin secured a waiver from Federal requirements for assessment and identification of supportive services prior to sanctioning. Wisconsin was also exempted from Federal requirements for a “conciliatory procedure” to resolve disputes prior to sanctioning, though a 1990 court decision restored some “due process” requirements (Quinn and Magill 1994).} \]
generally ranged from $60 to $190.

Learnfare was implemented for teen parents and 13-14 year olds in March of 1988 and extended to all covered teens by September 1988 (Etheridge and Perry 1993). Governor Tommy Thompson advocated the early implementation of Learnfare, which was subsequently characterized as an “administrative disaster (Wiseman 1996) because of the difficulties of establishing new, reliable and accurate links between schools and welfare offices for attendance monitoring. While the quality of Learnfare monitoring had largely improved throughout the state by the time of the random-assignment evaluation, Milwaukee County is a notable exception. This county contains both the largest school district in the state (Milwaukee Public Schools) and roughly 50 percent of the state’s Learnfare-eligible population (Frye and Caspar 1997).

Milwaukee County effectively had a separate set of Learnfare procedures that included an additional attendance verification check that delayed the time that lapsed between attendance violations and benefit sanctions. This procedure was adopted in 1992 as a part of a settlement to a lawsuit (Kronquist v. Whitburn), which alleged that Learnfare procedures violated due process because of the exceptionally poor quality of the attendance data in Milwaukee County schools. These procedures created an “appreciably longer” time between poor attendance and a sanction (Frye and Caspar 1997). Outside of Milwaukee County, poor attendance could trigger a processed sanction in as little as 2 months. In Milwaukee County, the lapsed time to a sanction would be at least twice as long. Furthermore, as a practical matter, a 1995 review found that the average time between poor attendance and the resulting sanction was actually 6.6 months in Milwaukee Public Schools (Frye and Caspar 1997). This review also found that poor data quality and processing errors in Milwaukee Public Schools led to false negatives: the absence of sanctions in situations when the school attendance
of covered teens failed to meet Learnfare standards. Because of these concerns, both the primary experimental evaluation of Learnfare and this re-analysis treat Milwaukee County separately from the other participating counties.

4 A Random-Assignment Learnfare Evaluation

The Federal waivers that allowed Wisconsin to introduce a policy like Learnfare also required that comprehensive evaluations were conducted. An early non-experimental evaluation based on administrative data from six school districts prior to and after the introduction of Learnfare (Pawasarat, Quinn, and Stetzer 1992) found no evidence that Learnfare improved school attendance. The quality of these inferences was hotly debated by state officials and the evaluation team (Quinn and Magill 1994). Nonetheless, the report in question acknowledged itself that “Given the limitations of the control group populations and problems of identifying AFDC and non-AFDC teen parents, the Learnfare hypothesis testing lacks the strength of an experimental design using random assignment.” However, a subsequent evaluation (Frye and Caspar 1997), which did utilize random assignment, indicated that the Learnfare program had at most short-term school-participation effects for certain sub-groups (Education Week, 1997). That random-assignment evaluation is the focus of the re-analysis presented here.

4.1 Study Description

The random-assignment evaluation of Learnfare was based on data from 10 counties. These 10 counties were chosen from Wisconsin’s 72 counties by a procedure that sought both representativeness of the statewide Learnfare population and a balance of other programmatic concerns. Specifically, counties with fewer than 125 Learnfare teenagers were excluded from consideration because
of the impracticality of monitoring attendance for small numbers of welfare recipients (Frye, Caspar, and Merrill 1992). Other counties (with the exception of Milwaukee County) were excluded because they were participating in a contemporaneous evaluation of the Parental and Family Responsibility program, which influenced the incentives of teen mothers receiving welfare to marry and abstain from having further children (Hoynes 1997, page 133). These exclusions left 29 counties as potential participants in the Learnfare evaluation. Ten counties were randomly selected from this pool with probabilities proportional to their share of the statewide Learnfare population (Milwaukee, Brown, Douglas, Eau Claire, Kenosha, La Crosse, Marathon, Marinette, Portage, and Racine). However, stratification insured the participation of 3 rural counties (i.e., Marathon, Portage, and Marinette).

Between March of 1993 and April of 1994, 3,205 teenagers from these 10 counties were selected for the study. Selection into the study occurred at the time when a teenager was scheduled to be introduced to Learnfare. This usually occurred when a member of an ongoing AFDC case turned 13 or when a new AFDC case opened.3 Study participants had to meet the basic requirements for the Learnfare program: aged 13 to 19, either a parent or living with natural or adoptive parents, and having neither graduated from high school or completed an equivalency degree. Teens with a sibling who had been on the AFDC case and aged 13 to 19 during the previous 12 months were excluded from the study (Frye, Caspar, and Merrill 1992). Once baseline data had been collected and a teen had been determined as eligible for the study, they were randomly assigned a treatment status. A statewide specialist was available to review the eligibility determination and to conduct the random assignment. However, another option was for county staff to make these designations (Frye, Caspar, and Merrill 1992).

3A teenager who had not previously been participating in Learnfare could also enter the study upon moving to the home of a parent receiving welfare support.
Teens assigned to the treatment received the usual introduction to Learnfare and were subject to its sanctions. Those assigned to the control group were not introduced to Learnfare and were exempted from its restrictions for the duration of the study.\footnote{One potential issue with welfare demonstrations of this sort is that the limited duration of the study may bias the inferences towards finding no effect by weakening the treatment contrast (e.g., Hoynes 1997).}

For each study participant, school enrollment and attendance data were collected over a six-semester study period (i.e., spring 1993 through fall 1995). Table 1 illustrates the basic panel structure of the available data by showing the number of study participants by month of entry and the number of subjects with valid attendance data by each of the six available semesters. This table also suggests the extent of attrition from the study population. In the absence of attrition, we would expect to see 3,205 observations for each of the last four study semesters.\footnote{Attrition is defined here as the absence of attendance data and any indication that the teenager completed high school or an equivalency degree.} However, the number of observations with valid data drops from 2,893 in the spring of 1994 to 2,325 in the fall of 1995. The analysis presented here addresses whether this attrition compromises the internal and external validity of the evaluation results. However, another key point is that the lost of nearly 30 percent of the observations by the end of the study period compromises the statistical precision associated with isolating longer-term treatment effects.

4.2 Baseline Data and Treatment-Control Balance

Table 2 presents descriptive statistics on nine baseline traits of the 3,205 study participants, separately for Milwaukee County and the other nine counties and by treatment status. These measures include binary indicators for sex, race, and ethnicity. They also include age measured in years and binary indicators for being “over age” for their grade (e.g., 15+ years old while in grade 8, 16+
years old while in grade 9, etc.), a teen parent, and a school dropout. Nearly 80 percent of the participating teens from Milwaukee County were Black or Hispanic while under 4 percent were Asian. In the other nine counties, over 13 percent of the participants were Asian and just under 25 percent were Black or Hispanic. However, the remaining baseline traits were relatively similar across Milwaukee County and the remaining counties. For example, 15 to 17 percent of participants were defined as school dropouts when they entered the study. And 17 to 19 percent of participants were identified as teen parents at baseline.

The fundamental rationale for using random assignment to choose the Learnfare status of these study participants was to break the correlation that might otherwise exist between the determinants of the outcomes under study and assignment to Learnfare. However, it is possible (though unlikely) that, merely by chance, random assignment fails to balance the observed and unobserved traits of study participants across the treatment and control conditions. Furthermore, in the Learnfare evaluation, county officials (as opposed to a trained state officer) had the autonomy to conduct the random assignment by themselves (Caspar, Frye, and Merrill 1992). This potential decentralization of the random assignment process suggests the possibility that the fidelity of the procedures could have been inconsistent or even subject to some discretion.

A straightforward way to assess the quality of the random-assignment results is to examine whether the observed baseline traits appear to differ across those assigned to the treatment and control groups. Table 2 presents the probability values from t tests of such treatment-control comparisons. The results for Milwaukee County indicate that black participants were significantly less likely to be subjected to Learnfare’s restrictions while Hispanics were significantly more likely. Furthermore, within Milwaukee County, there were weakly significant differences in the likelihood of being “over age” and a teen parent
across the treatment and control conditions. Specifically, both those who were over age and those who were teen parents were more likely to be exempted from Learnfare’s restrictions.

In contrast, in the other nine study counties, baseline traits appear to be well balanced across the treatment and control conditions. Auxiliary regressions that model treatment status as a function of all of these baseline traits imply similar results. Within Milwaukee County, such regressions suggest that teen-parent status has a particularly robust negative effect on being assigned to Learnfare. However, outside of Milwaukee County, these baseline traits are neither individual nor jointly significant determinants of treatment status.

One candidate explanation for the treatment-control imbalance observed in Milwaukee County is that it simply occurred by chance (i.e., an unintended randomization “failure”). Another possibility is that this pattern reflects discretion on the part of the state or county officers who identified each participant’s treatment assignment. More specifically, it may be that, in order to protect certain study participants like teen mothers (98 percent of the participants identified as parents were female) from the comparatively high likelihood of a Learnfare sanction, officials in Milwaukee County were more likely to designate them as being in the control group which was not subject to potential sanctions. However, both the source of this non-random assignment and the direction of the implied bias in the estimated treatment effects are unknown.

The remaining analysis will focus largely on the nine other counties where the treatment-control balance suggests that the random assignment procedures worked well. An additional rationale for this focus is the evidence that the Learnfare sanctions were implemented with higher fidelity (i.e., more quickly and accurately) outside of Milwaukee County.
4.3 Study Attrition

One of the innovations in this reanalysis of the Learnfare evaluation turns on using the panel nature of the available data both to improve statistical precision and to provide a formal framework for testing the dynamic effects of the treatment assignment. In the absence of attrition, the enrollment and attendance outcomes of each of the 3,205 study participants should have been observed in each of 4 to 6 semesters (Table 1). This structure implies that the pooled teen-by-semester panel data set has 16,263 potential observations. However, for roughly 15 percent of these potential observations, neither evidence of high school completion nor valid attendance data could be identified.

The main source of study attrition appears to be due to teens moving either within or out of the state so that their attendance and enrollment data could not be tracked. To the extent this attrition is non-random it could compromise the external validity of this experimental evaluation by shaping the sample on which inferences are based. However, if treatment assignment influences attrition, the internal validity of the experimental evaluation is also in jeopardy. For example, if treatment families (i.e., those subject to Learnfare’s restrictions) with teens likely to dropout of school were more likely to move because of their treatment status, it would create an upward bias in the estimated treatment effects on enrollment and attendance. A straightforward way to address the empirical relevance of these concerns is to model attrition status, $A$, as a function of treatment assignment, $T$, and baseline observables, $X$. A generalized panel-based specification for these auxiliary regressions takes the following form:

$$A_{icms} = \alpha + \gamma T_{icms} + \beta X_{icms} + \eta_c + \theta_m + \delta_s + \varepsilon_{icms}$$  \hspace{1cm} (1)

where $\eta$, $\theta$, and $\delta$ respectively represent county, entry month and semester fixed effects and $\varepsilon$ represents a mean-zero error term for teen $i$ in county $c$ who en-
tered the study in the month-year combination $m$ and is observed in semester $s$. The standard errors in this specification are adjusted for heteroscedasticity clustered at the county/entry-month level. This approach generates the most conservative inferences (i.e., the largest standard errors) among alternative heteroscedasticity corrections. To examine the robustness of the results, some specifications exclude the month and semester fixed effects (i.e., $\theta_m$ and $\delta_s$) while other specifications instead introduce fully general interactions between the county, entry-month, and semester fixed effects (i.e., $\eta_c$, $\theta_m$, and $\delta_s$).

Table 3 presents the key results of these attrition models for the teens. These results consistently indicate across specifications that introduce entry-month and semester fixed effects (i.e., $\theta_m$, $\delta_s$, and $\theta_m \times \delta_s$) that several baseline traits (e.g., minority status, age, teen parent, and school dropout) imply statistically significant increases in the likelihood of attrition. The magnitudes of these effects are particularly notable with respect to the dropout effect. The mean attrition rate among non-dropouts is 13 percent. Being a dropout at baseline more than doubles the probability of study attrition. These patterns indicate that the external validity of the Learnfare evaluations is somewhat compromised by the attrition of study participants. However, the results in Table 3 also consistently indicate that treatment status is unrelated to the likelihood of attrition. These results provide evidence that attrition is unlikely to bias the interval validity of the treatment and control comparisons.

### 4.4 Outcome Measures

The outcome measures constructed for teens whose enrollment and attendance status could be determined are those used in the primary evaluation (Frye and Caspar 1997). School enrollment is measured by the number of months in the semester for which enrollment was verified. This measure varies from 0 to 4.5
in increments of 0.5. The attendance rate identifies the fraction of school days in the teen’s school district for which the student was in attendance. A third measure identifies the fraction of school days for which the student had an unexcused full-day absence. These measures are not fully symmetrical because of excused student absences. Identifying the comparative effects of Learnfare on the attendance rate and the rate of unexcused absences provides a direct way to assess whether Learnfare generated genuine increases in attendance or merely increased the use of excused absences. While most of the analysis presented here utilizes these measures, the results based on alternative outcome measures (e.g., no enrollment, full-time enrollment, no unexcused absences, etc.) are also presented.

An important feature of the enrollment and attendance measures used in the primary evaluation is that they not defined in semesters after a teen completed high school. Most of the study participants (i.e., slightly more than half) were 13 years old when they entered the study so they did not have sufficient time for the typical period of high school completion during the study window. Therefore, these data do not provide a strong test of whether the Learnfare restrictions improved the probability of completing high school. However, inferences based on preferred specifications suggest that random assignment to the Learnfare restrictions had a positive, though not quite statistically significant (p-value = 0.134), effect on high school completion.

This pattern of positive treatment effects implies that the primary evaluation’s approach of eliminating high school completers from the enrollment and attendance analysis biases the estimated treatment effect downward. This issue can be understood as a form of non-random attrition. If Learnfare increased the probability of finishing high school, study participants for whom the policy

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6In the nine study counties (i.e., excluding Milwaukee County), only 13 percent of the student-by-semester observations were identified as high school completers.
had its targeted effects are systematically excluded from the analysis. To address the empirical relevance of this issue, some of the results presented here are based on attendance and enrollment data that reflect simple imputations for those who completed high school. More specifically, missing enrollment and attendance measures are conservatively imputed for high school completers using the latest observation for which a recorded measure was available (i.e., the “last observation carry forward” procedure used by Krueger (1999) and Dee (2004)).

4.5 Replicating Frye and Caspar (1997)

Before moving to an independent analysis of the Learnfare data from the nine other counties, this section establishes an important baseline by describing and replicating the key evaluation results reported by Frye and Caspar (1997). This primary evaluation estimated the effects of random assignment to Learnfare on the 3 enrollment and attendance measures (i.e., months enrolled, rate of attendance, rate of unexcused absences) using separate cross-sections of study participants defined by whether they were in their first, second, third, or fourth study semester. So, for example, the “first-semester” results are based on pooling outcome data from the spring 1993, fall 1993 and spring 1994 semesters.

I report regression results based on the same sample selection and a similar regression specification in Table 4. These results are similar to those reported by Frye and Caspar (1997). For the study participants from Milwaukee County, random assignment to Learnfare appears to have had small and statistically insignificant effects on enrollment and attendance across all 3 outcome measures and regardless of the length of time in the study.8 Outside of Milwaukee County, 7These results parallel those reported in Table 14 of Frye and Caspar (1997). The sample sizes match exactly for all 24 subgroups. However, the estimated treatment effects reported here differ slightly because of modest differences in the regression controls. For example, the results in Table 4 condition on unrestrictive county and semester fixed effects. 8The fourth-semester enrollment result for Milwaukee County suggests that Learnfare reduced enrollment. This weakly significant effect suggests the harmful effects of cash incentives on intrinsic motivation. However, the poor treatment-control balance for the study partici-
where the randomization procedures appear to have performed well, Learnfare appears to have generated significant increases in enrollment and attendance (e.g., a 3 percentage-point increase in attendance) but only in either the first or second semester.

This apparent lack of persistent treatment effects is the basis for the widespread view that Learnfare did not have meaningful effects on its targeted outcomes. However, this interpretation may be inaccurate because an analysis based on the cross-sections in Table 4 fails to exploit the statistical precision made available by the panel structure of the available study data. Furthermore, a panel-data approach to this analysis would also provide a framework for explicit tests of whether the treatment effects have statistically significant differences across semesters. While it is true that the estimated treatment effects appear to decline by the number of semesters in the study, these longer-term effects are also estimated with comparatively less precision because attrition from the study reduces the number of observations observed for multiple semesters. And the lack of precision associated with longer-term effects may be meaningful. For example, the 95-percent confidence intervals for the fourth-semester treatment effects for each of the 3 outcome variables includes the corresponding first-semester point estimate. Statistical tests based on the pooled data can indicate more formally whether the data reject the hypothesis of a common treatment effect across the length of time in the study.

5 Intent-to-Treat Estimates

The basic econometric specification applied to the pooled nine-county data from the Learnfare evaluation takes the following form:

pants from Milwaukee County suggests that these inferences lack internal validity.
\[ Y_{icms} = \alpha + \gamma T_{icms} + \beta X_{icms} + \eta_c + \theta_m + \delta_s + \varepsilon_{icms} \] (2)

As in the attrition analysis, some results are based on specifications that introduce unrestricted interactions between the county, entry month and semester fixed effects (i.e., \( \eta_c, \theta_m, \) and \( \delta_s \)). Furthermore, the standard errors are adjusted for clustering at the county/entry-month level.\(^9\)

### 5.1 Baseline Results

Table 5 reports the estimated \( \gamma \) from multiple specifications of equation (2) and for each of the three outcome measures. The first three specifications introduce county, entry-month, and semester fixed effects. The next three specifications in Table 8 condition on interactions between these fixed effects while the final specification conditions on fixed effects unique to each county, entry-month, and semester observation.

These results consistently indicate that random assignment to the Learnfare program increased both enrollment and attendance. In the preferred specifications, the implied increase in months enrolled is approximately 0.117 while the increase in the attendance rate is approximately 0.024 percentage points. Both of these estimated increases are approximately 3 percent of the control group mean and 7 percent of a standard deviation. Alternatively, these full-sample treatment estimates imply 2 additional days of enrollment and attendance per semester.\(^10\) The Learnfare effects on the rate of unexcused absences and the attendance rate are fairly symmetrical, which suggests that Learnfare did not

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\(^9\) This approach appears to generate the most conservatively large measures of precision. Based partly on Monte Carlo evidence, Angrist and Pischke (2009) recommend a rule of thumb based on choosing the largest of the standard errors from alternative corrections. Clustering based on county/entry-month cells implies a fairly large number of clusters (i.e., \( 9 \times 14 = 126 \)), so the finite-sample bias in such cluster adjustments is not a concern.

\(^10\) The assumption of 20 school days in a month implies that 0.117 additional months is a 2.3 day increase. The assumption of 90 school days in a semester implies that a 0.024 increase in the attendance rate is 2.1 days.
merely increase the number of absences that were excused.

Tables 6, 7, and 8 identify, for each of the three outcome measures, how the effects of Learnfare evolved by participants’ length of time in the study. More specifically, the indicator for random assignment to the Learnfare treatment is interacted with binary indicators for whether the participant is in their first through sixth semester of study participation. All of these specifications condition on fixed effects unique to interactions between the entry month and the current semester (i.e., main effects for length of time in study) so that the dynamic treatment interactions of interest are identified.

These results generally suggest that the treatment-induced increases in enrollment and attendance are largest in the first two semesters of study participation. By the fourth semester, the Learnfare treatment effects appear to have fallen by roughly half and to have become statistically indistinguishable from zero. However, the conventional view that Learnfare had at most short-term effects appears to be overdrawn. The fourth-semester effects are generally within just one standard error of the larger first and second-semester effects. More directly, across all four specifications and each of the three dependent variables, the hypothesis that the treatment has the same effect by length of time in the study cannot be rejected.

One potentially serious drawback in how the outcomes measures were constructed for this study is that the enrollment and attendance measures were no longer defined for those who had completed high school. Most study participants were sufficiently young that they were highly unlikely to complete high school during the study period. However, those assigned to Learnfare were somewhat more likely to complete high school during the study period. The fact that the outcome measures were no longer defined for high school completers constitutes a form of non-random attrition that should create a downward bias in the
estimated effects of Learnfare.

Table 9 provides evidence on the empirical relevance of this attrition by reporting the estimated effects of Learnfare on the enrollment and attendance measures with imputations for high-school completers. As noted earlier, these outcome measures were imputed for high-school completers merely by identifying their last recorded measure. The treatment effects based on these measures are somewhat larger, implying a 3.9 percent increase in enrollment (effect size = 0.088) and a 3.7 percent increase in attendance (effect size = 0.081). As with the unadjusted measures, the hypothesis that the Learnfare treatment had similar effects by length of time in the study cannot be rejected.

5.2 Alternative Outcome Measures

The results based on the outcome measures used by the Frye and Caspar’s (1997) seminal evaluation suggest that Learnfare increased enrollment and attendance by somewhat more than 2 days per semester. However, the treatment effects defined for these continuous measures may be misleading because of the skewed and bimodal nature of these variables. Figures 1, 2, and 3 illustrate this concern by showing the kernel density estimates for each outcome measure and by treatment status. Figures 1 and 2 suggest that, for those assigned to the treatment, the probability mass for these enrollment and attendance is concentrated in higher values. Similarly, Figure 3 indicates that, for those assigned to Learnfare, the rate of unexcused absences tend to be concentrated in the lower values.

These figures suggest that a more natural way to interpret the effects of Learnfare would be to identify how it influences the probability that the enrollment and attendance measures exceed a given value. Table 10 reports the key results of such an exercise using a preferred specification and 22 different
binary outcome measures defined for each enrollment and attendance measure and multiple cut points.

The results indicate that Learnfare increased the probability of full-time enrollment (i.e., months enrolled equal to 4.5) by 2.6 percentage points (i.e., 3.9 percent). Similarly, Learnfare increased the probability of having any enrollment for the entire semester (i.e., months enrolled > 0) by 2.6 percentage points (i.e., 2.9 percent).

The attendance results indicate that Learnfare did not generate statistically significant increases in the probability of perfect or near-perfect attendance. However, Learnfare did increase the probability that the attendance rate exceeded 85 percent by 4.7 percentage points (i.e., an 8.2 percent increase). Similarly, Learnfare increased the probability that the rate of unexcused absences would be less than 30 percent by 3.2 percentage points (i.e., a 4 percent increase). Overall, these full-sample results indicate that Learnfare had fairly similar effects on months enrolled throughout the distribution of that variable. However, the treatment-induced increases in attendance largely implied an increased likelihood of attending school more than 80 to 85 percent of the time.

5.3 Treatment Heterogeneity

Table 11 presents the estimated effects of Learnfare for each of the three outcome measures and sub-groups of study participants defined by baseline traits. These results suggest that Learnfare had similar effects among males and females and larger effects among Hispanic and Black participants. However, the reduction in sample sizes implies that none of these sub-group treatment effects are statistically distinguishable from zero. The more striking result from Table 11 is that the enrollment and attendance effects of Learnfare appear to be largely concentrated among those who were teen parents at baseline and those
who were dropouts at baseline. Specifically, the marginal effects of Learnfare on enrollment are roughly 7 times larger for these two sub-samples than they are for the full-sample. Similarly, the effects of Learnfare on attendance are roughly 4 times larger for these sub-groups than for the full sample.

Table 12 explores the effects of Learnfare for the sample of those who were dropouts at baseline in more detail. More specifically, the results in Table 12 indicate that Learnfare increased the number of months enrolled by 31 percent (i.e., 0.589/1.875). Similarly, Learnfare increased the attendance rate among baseline dropouts by 23 percent (i.e., 0.080/.342). An alternative way to put the magnitude of these effects into perspective is to compare them to the estimated enrollment and attendance gaps among those who were baseline dropouts and those who were not. These results imply that Learnfare closed the enrollment gap between baseline dropouts and school attendees by 75 percent. Learnfare closed the corresponding attendance gap by a third. As with the full sample, the hypothesis that these treatment effects were the same with regard to the length of time in the study cannot be rejected.

The kernel density estimates in Figures 4, 5, and 6 provide non-parametric evidence that assignment to the Learnfare treatment clearly shifted Learnfare participants towards increased enrollment and attendance. However, those density graphs also underscore the bimodal nature of the outcomes measures for those who were baseline dropouts. Table 13 presents the estimated treatment effects for the binary outcome measures based on different cut points in the enrollment and attendance measures.

As with the full-sample analysis, these results indicate that Learnfare had consistent effects along the extensive margin (i.e., the months enrolled measure). For example, Learnfare increased the probability of full-time enrollment among baseline dropouts by 25 percent (i.e., 0.080/0.317) and the probability of having
any enrollment by 37 percent (i.e., 0.194/0.521).

However, the effects of Learnfare on the intensive margin (i.e., attendance) were more modest for baseline dropouts. For example, Learnfare did not create a statistically significant increase among baseline dropouts in the probability of an attendance rate above 80 percent. However, Learnfare did increase the probability of an attendance rate above 60 percent by 11.6 percentage points (i.e., a 33 percent increase).

6 Conclusions

In this paper, I reanalyzed the data from an experimental evaluation of Wisconsin’s seminal Learnfare program. The influential Learnfare program sanctioned the welfare benefits of families where covered teens did not meet school attendance requirements. The results presented here indicate that Learnfare was effective in improving the targeted school enrollment and attendance measures. In fact, the benefits of Learnfare in promoting school attendance were concentrated among some of the most at-risk students (i.e., those who were teen parents or dropouts at baseline).

The design features of Learnfare are distinct from other recent and ongoing initiatives to provide students with financial incentives for academic performance. For example, unlike the recent student-incentive programs in developed countries, Learnfare leveraged family-based financial incentives to improve student outcomes (as in the conditional cash transfer programs that have proliferated in developing countries). Second, Learnfare provided sanctions against an existing transfer rather than rewards. In the presence of loss aversion, this aspect of Learnfare should amplify its behavioral impact and would constitute a novel example of using “choice architecture” to amplify the effects of a policy (Thaler and Susstein 2008). Third, the extant psychological literature suggests
that, to avoid harming intrinsic motivation, financial incentives should be based on requirements that participants feel they have the capacity to meet (i.e., tasks which are “effort responsive”). Learnfare may have been particularly likely to satisfy this condition because it targeted attendance rather than grades or test performance.

The apparent effectiveness of Learnfare suggests that these unique design parameters merit further scrutiny and consideration. However, another notable and important lesson from Wisconsin’s Learnfare experience involves the serious implementation challenges that occurred within Milwaukee County. The failure of the random assignment procedures within Milwaukee County to balance the baseline traits of study participants across the treatment and control states strongly qualifies any conclusions based on the experimental evaluation that occurred there. Nonetheless, the comparative difficulty of producing timely and accurate attendance data within Milwaukee County serve as a reminder that any policy linking financial incentives tied to school attendance is likely to require high-performance data systems that can provide quick and accurate feedback to students and their families. The growing sophistication of data systems in public schools may, therefore, provide an important complement to future policies like Learnfare.

However, any future consideration of Learnfare-like policies should also consider how a program of extrinsic rewards compares to other rigorously evaluated policy alternatives. For example, the “What Works Clearinghouse” maintained by the Institute of Education Sciences has identified other effective dropout prevention programs (e.g., ALAS, Check and Connect) that rely on intensive case management rather than financial incentives. The comparative desirability of such programs is an open question whose answer is likely to depend in part on the amount of intrinsic motivation that exists in the targeted population.
However, two other policy-relevant criteria for comparing dropout prevention programs are cost-effectiveness and scalability. With respect to these desiderata, Learnfare policies may provide an interesting contrast to initiatives that focus on case management and support services. Initiatives that focus on extrinsic incentives are likely to be less labor-intensive than case-management programs and Wisconsin’s experience provides some evidence of efficacy for a mature policy that was implemented at scale statewide.
References


## Table 1 - Study Participants by Month of Entry and Semester of Available Data

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<th>Spring 1994</th>
<th>Fall 1994</th>
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Notes: The treatment and control columns identify the mean value of the baseline trait by treatment status and county. The p-value refers to a two-tailed t-test of the null hypothesis that the treatment and control values are the same.
Table 3 - Determinants of Study Attrition

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Notes: These regression results are based on student-by-semester panel data from the nine non-Milwaukee counties (n=6,028). The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level.

† \(p<0.01\), † † \(p<0.05\), * \(p<0.1\)
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<td>0.0576</td>
<td>1,676</td>
</tr>
<tr>
<td>Months Enrolled</td>
<td>4</td>
<td>-0.0887*</td>
<td>0.0489</td>
<td>1,582</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>1</td>
<td>0.0015</td>
<td>0.0120</td>
<td>1,930</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>2</td>
<td>-0.0111</td>
<td>0.0114</td>
<td>1,827</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>3</td>
<td>-0.0060</td>
<td>0.0126</td>
<td>1,648</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>4</td>
<td>-0.0196</td>
<td>0.0123</td>
<td>1,561</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>1</td>
<td>0.0006</td>
<td>0.0108</td>
<td>1,930</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>2</td>
<td>0.0098</td>
<td>0.0094</td>
<td>1,827</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>3</td>
<td>0.0119</td>
<td>0.0137</td>
<td>1,648</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>4</td>
<td>0.0193</td>
<td>0.0119</td>
<td>1,561</td>
</tr>
<tr>
<td><strong>Other Counties</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Months Enrolled</td>
<td>1</td>
<td>0.1087</td>
<td>0.0925</td>
<td>1,146</td>
</tr>
<tr>
<td>Months Enrolled</td>
<td>2</td>
<td>0.1230*</td>
<td>0.0667</td>
<td>1,074</td>
</tr>
<tr>
<td>Months Enrolled</td>
<td>3</td>
<td>0.0492</td>
<td>0.0844</td>
<td>949</td>
</tr>
<tr>
<td>Months Enrolled</td>
<td>4</td>
<td>0.0052</td>
<td>0.0843</td>
<td>868</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>1</td>
<td>0.0294\dagger</td>
<td>0.0136</td>
<td>1,102</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>2</td>
<td>0.0188</td>
<td>0.0133</td>
<td>1,024</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>3</td>
<td>0.0024</td>
<td>0.0159</td>
<td>925</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>4</td>
<td>0.0135</td>
<td>0.0165</td>
<td>846</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>1</td>
<td>-0.0259*</td>
<td>0.0132</td>
<td>1,102</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>2</td>
<td>-0.0116</td>
<td>0.0140</td>
<td>1,024</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>3</td>
<td>-0.0025</td>
<td>0.0165</td>
<td>925</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>4</td>
<td>-0.0115</td>
<td>0.0162</td>
<td>846</td>
</tr>
</tbody>
</table>

Notes: The ITT estimates condition on the eight baseline observables and semester FE. The standard errors are adjusted for heteroscedasticity clustered at the county/entry-month level.

\dagger p<0.01, \dagger p<0.05, \ast p<0.1
<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Months Enrolled</td>
<td>0.0920∗</td>
<td>0.0878∗</td>
<td>0.0881∗</td>
<td>0.0890∗</td>
<td>0.1176†</td>
<td>0.1170†</td>
<td>0.1168†</td>
</tr>
<tr>
<td></td>
<td>(0.0497)</td>
<td>(0.0481)</td>
<td>(0.0482)</td>
<td>(0.0484)</td>
<td>(0.0529)</td>
<td>(0.0532)</td>
<td>(0.0555)</td>
</tr>
<tr>
<td>Attendance Rate</td>
<td>0.0212†</td>
<td>0.0208†</td>
<td>0.0202∗</td>
<td>0.0202∗</td>
<td>0.0234†</td>
<td>0.0230†</td>
<td>0.0239†</td>
</tr>
<tr>
<td></td>
<td>(0.0104)</td>
<td>(0.0102)</td>
<td>(0.0102)</td>
<td>(0.0103)</td>
<td>(0.0110)</td>
<td>(0.0110)</td>
<td>(0.0114)</td>
</tr>
<tr>
<td>Rate of Unexcused Absences</td>
<td>-0.0192∗</td>
<td>-0.0189∗</td>
<td>-0.0181∗</td>
<td>-0.0182∗</td>
<td>-0.0223∗</td>
<td>-0.0219∗</td>
<td>-0.0224∗</td>
</tr>
<tr>
<td></td>
<td>(0.0108)</td>
<td>(0.0106)</td>
<td>(0.0106)</td>
<td>(0.0107)</td>
<td>(0.0114)</td>
<td>(0.0114)</td>
<td>(0.0119)</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Entry Month FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Semester FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Month-Semester FE</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>County-Semester FE</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>County-Month FE</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>County-Month-Semester FE</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Notes: These regression results are based on student-by-semester panel data from the nine non-Milwaukee counties. The sample size is 4,862 for the enrollment model and 4,697 for the attendance models. All models condition on the eight baseline observables. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level. The control-group means for the three dependent variables are 3.517, 0.728, and 0.208, respectively.

∗ p<0.01, † p<0.05, * p<0.1
Table 6 - Intent-to-Treat (ITT) Estimates By Semesters in Study Months Enrolled

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment x 1st Semester</td>
<td>0.0878*</td>
<td>0.1148†</td>
<td>0.1076*</td>
<td>0.1191†</td>
</tr>
<tr>
<td></td>
<td>(0.0510)</td>
<td>(0.0547)</td>
<td>(0.0544)</td>
<td>(0.0596)</td>
</tr>
<tr>
<td>Treatment x 2nd Semester</td>
<td>0.1420†</td>
<td>0.1689†</td>
<td>0.1664†</td>
<td>0.1414*</td>
</tr>
<tr>
<td></td>
<td>(0.0652)</td>
<td>(0.0685)</td>
<td>(0.0695)</td>
<td>(0.0765)</td>
</tr>
<tr>
<td>Treatment x 3rd Semester</td>
<td>0.0747</td>
<td>0.1093</td>
<td>0.1126</td>
<td>0.1000</td>
</tr>
<tr>
<td></td>
<td>(0.0846)</td>
<td>(0.0880)</td>
<td>(0.0895)</td>
<td>(0.1004)</td>
</tr>
<tr>
<td>Treatment x 4th Semester</td>
<td>0.0453</td>
<td>0.0829</td>
<td>0.0896</td>
<td>0.0881</td>
</tr>
<tr>
<td></td>
<td>(0.0893)</td>
<td>(0.0919)</td>
<td>(0.0909)</td>
<td>(0.1010)</td>
</tr>
<tr>
<td>Treatment x 5th Semester</td>
<td>0.0554</td>
<td>0.0771</td>
<td>0.0783</td>
<td>0.1268</td>
</tr>
<tr>
<td></td>
<td>(0.0889)</td>
<td>(0.0899)</td>
<td>(0.0897)</td>
<td>(0.0988)</td>
</tr>
<tr>
<td>Treatment x 6th Semester</td>
<td>0.2508</td>
<td>0.2384</td>
<td>0.2233</td>
<td>0.1485</td>
</tr>
<tr>
<td></td>
<td>(0.3215)</td>
<td>(0.3468)</td>
<td>(0.3317)</td>
<td>(0.3780)</td>
</tr>
</tbody>
</table>

County FE, Yes No No No
Month-Semester FE, Yes Yes Yes No
County-Semester FE, No No Yes No
County-Month FE, No Yes Yes No
County-Month-Semester FE, No No No Yes

\[ R^2 \] 0.495 0.511 0.517 0.544
\[ p-value (H_0: \gamma_1 = ... = \gamma_6) \] 0.8211 0.8630 0.8836 0.9972

Notes: These results are based on the nine non-Milwaukee counties (n=4,862). All models condition on the eight baseline observables. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level. The p-value refers to an F-test for the equality of the treatment effects across semesters.

† p<0.01, † p<0.05, * p<0.1
Table 7 - Intent-to-Treat (ITT) Estimates By Semesters in Study: Attendance Rate

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment x 1st Semester</td>
<td>0.0338†</td>
<td>0.0371†</td>
<td>0.0368†</td>
<td>0.0362†</td>
</tr>
<tr>
<td></td>
<td>(0.0141)</td>
<td>(0.0150)</td>
<td>(0.0147)</td>
<td>(0.0158)</td>
</tr>
<tr>
<td>Treatment x 2nd Semester</td>
<td>0.0230*</td>
<td>0.0251*</td>
<td>0.0249*</td>
<td>0.0204</td>
</tr>
<tr>
<td></td>
<td>(0.0125)</td>
<td>(0.0132)</td>
<td>(0.0132)</td>
<td>(0.0144)</td>
</tr>
<tr>
<td>Treatment x 3rd Semester</td>
<td>0.0081</td>
<td>0.0142</td>
<td>0.0140</td>
<td>0.0100</td>
</tr>
<tr>
<td></td>
<td>(0.0152)</td>
<td>(0.0155)</td>
<td>(0.0157)</td>
<td>(0.0170)</td>
</tr>
<tr>
<td>Treatment x 4th Semester</td>
<td>0.0183</td>
<td>0.0224</td>
<td>0.0231</td>
<td>0.0289</td>
</tr>
<tr>
<td></td>
<td>(0.0166)</td>
<td>(0.0168)</td>
<td>(0.0166)</td>
<td>(0.0178)</td>
</tr>
<tr>
<td>Treatment x 5th Semester</td>
<td>0.0108</td>
<td>0.0125</td>
<td>0.0122</td>
<td>0.0248</td>
</tr>
<tr>
<td></td>
<td>(0.0201)</td>
<td>(0.0202)</td>
<td>(0.0204)</td>
<td>(0.0231)</td>
</tr>
<tr>
<td>Treatment x 6th Semester</td>
<td>0.0328</td>
<td>0.0209</td>
<td>0.0111</td>
<td>0.0034</td>
</tr>
<tr>
<td></td>
<td>(0.0588)</td>
<td>(0.0606)</td>
<td>(0.0592)</td>
<td>(0.0665)</td>
</tr>
<tr>
<td>County FE</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Month-Semester FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>County-Semester FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>County-Month FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>County-Month-Semester FE</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>R²</td>
<td>0.437</td>
<td>0.462</td>
<td>0.468</td>
<td>0.498</td>
</tr>
<tr>
<td>p-value (H₀ : γ₁ = ... = γ₆)</td>
<td>0.8408</td>
<td>0.8680</td>
<td>0.8542</td>
<td>0.7624</td>
</tr>
</tbody>
</table>

Notes: These results are based on the nine non-Milwaukee counties (n=4,697). All models condition on the eight baseline observables. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level. The p-value refers to an F-test of the equality of the treatment effects across semesters.

‡ p<0.01, † p<0.05, * p<0.1
Table 8 - Intent-to-Treat (ITT) Estimates By Semesters in Study:
Rate of Unexcused Absences

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment x 1st Semester</td>
<td>-0.0299</td>
<td>-0.0347</td>
<td>-0.0349</td>
<td>-0.0342</td>
</tr>
<tr>
<td></td>
<td>(0.0134)</td>
<td>(0.0143)</td>
<td>(0.0141)</td>
<td>(0.0151)</td>
</tr>
<tr>
<td>Treatment x 2nd Semester</td>
<td>-0.0160</td>
<td>-0.0196</td>
<td>-0.0189</td>
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</tr>
<tr>
<td></td>
<td>(0.0132)</td>
<td>(0.0139)</td>
<td>(0.0140)</td>
<td>(0.0154)</td>
</tr>
<tr>
<td>Treatment x 3rd Semester</td>
<td>-0.0078</td>
<td>-0.0144</td>
<td>-0.0143</td>
<td>-0.0106</td>
</tr>
<tr>
<td></td>
<td>(0.0158)</td>
<td>(0.0162)</td>
<td>(0.0165)</td>
<td>(0.0181)</td>
</tr>
<tr>
<td>Treatment x 4th Semester</td>
<td>-0.0164</td>
<td>-0.0219</td>
<td>-0.0221</td>
<td>-0.0279</td>
</tr>
<tr>
<td></td>
<td>(0.0167)</td>
<td>(0.0168)</td>
<td>(0.0164)</td>
<td>(0.0176)</td>
</tr>
<tr>
<td>Treatment x 5th Semester</td>
<td>-0.0129</td>
<td>-0.0145</td>
<td>-0.0140</td>
<td>-0.0247</td>
</tr>
<tr>
<td></td>
<td>(0.0204)</td>
<td>(0.0202)</td>
<td>(0.0203)</td>
<td>(0.0225)</td>
</tr>
<tr>
<td>Treatment x 6th Semester</td>
<td>-0.0526</td>
<td>-0.0401</td>
<td>-0.0304</td>
<td>-0.0267</td>
</tr>
<tr>
<td></td>
<td>(0.0664)</td>
<td>(0.0691)</td>
<td>(0.0664)</td>
<td>(0.0759)</td>
</tr>
</tbody>
</table>

County FE                     | Yes     | No      | No      | No      |
Month-Semester FE             | Yes     | Yes     | Yes     | No      |
County-Semester FE            | No      | No      | Yes     | No      |
County-Month FE               | No      | Yes     | Yes     | No      |
County-Month-Semester FE      | No      | No      | No      | Yes     |
\( R^2 \)                     | 0.461   | 0.483   | 0.490   | 0.519   |
p-value (\( H_0 : \gamma_1 = ... = \gamma_6 \)) | 0.8602  | 0.8627  | 0.8375  | 0.7125  |

Notes: These results are based on the nine non-Milwaukee counties (n=4,697). All models condition on the eight baseline observables. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level. The p-value refers to an F-test of the equality of the treatment effects across semesters.

\( \dagger p<0.01, \ddagger p<0.05, * p<0.1 \)
Table 9 - Intent-to-Treat (ITT) Estimates By Semester, Outcome Measures with Imputations for HS Graduates

<table>
<thead>
<tr>
<th></th>
<th>Enrollment (1)</th>
<th>Attendance Rate (2)</th>
<th>Rate of Unexcused Absences (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment</td>
<td>0.1379†</td>
<td>-0.0270*</td>
<td>-0.0271*</td>
</tr>
<tr>
<td>Treatment x 1st Semester</td>
<td>-0.1195†</td>
<td>-0.0380†</td>
<td>-0.0361†</td>
</tr>
<tr>
<td>Treatment x 2nd Semester</td>
<td>-0.1339*</td>
<td>-0.0209</td>
<td>-0.0153</td>
</tr>
<tr>
<td>Treatment x 3rd Semester</td>
<td>-0.1394</td>
<td>-0.0139</td>
<td>-0.0168</td>
</tr>
<tr>
<td>Treatment x 4th Semester</td>
<td>-0.1313</td>
<td>-0.0311</td>
<td>-0.0320</td>
</tr>
<tr>
<td>Treatment x 5th Semester</td>
<td>-0.1414</td>
<td>-0.0283</td>
<td>-0.0317</td>
</tr>
<tr>
<td>Treatment x 6th Semester</td>
<td>-0.3396</td>
<td>-0.0443</td>
<td>-0.0597</td>
</tr>
<tr>
<td>( \gamma_i )</td>
<td>0.512</td>
<td>0.727</td>
<td>0.209</td>
</tr>
<tr>
<td>N</td>
<td>5,158</td>
<td>5,158</td>
<td>4,956</td>
</tr>
<tr>
<td>R²</td>
<td>0.509</td>
<td>0.509</td>
<td>0.509</td>
</tr>
<tr>
<td>p-value (H₀: ( \gamma_1 = ... = \gamma_6 ))</td>
<td>0.9947</td>
<td>0.7511</td>
<td>0.6097</td>
</tr>
</tbody>
</table>

Notes: These results are based on the nine non-Milwaukee counties with enrollment and attendance imputed for high school completers using their last observed value. All models condition on the eight baseline observables and fixed effects specific to each county/entry-month/semester cell. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level. The p-value refers to an F-test for the equality of the treatment effects across semesters.

\( \dagger \) p<0.01, \( \ddagger \) p<0.05, * p<0.1
Figure 1: Months Enrolled by Treatment Status

Figure 2: Attendance Rate by Treatment Status

Figure 3: Rates of Unexcused Absences by Treatment Status
| Binary outcome variable          | Estimated Effect | Standard Error | $\hat{Y}_i|T_i = 0$ |
|----------------------------------|------------------|----------------|-----------------|
| Months enrolled $= 4.5$          | 0.0261∗          | 0.0143         | 0.671           |
| Months enrolled $\geq 4.0$       | 0.0269∗          | 0.0137         | 0.702           |
| Months enrolled $\geq 3.0$       | 0.0252∗          | 0.0134         | 0.757           |
| Months enrolled $\geq 2.0$       | 0.0276†          | 0.0115         | 0.806           |
| Months enrolled $\geq 1.0$       | 0.0235∗          | 0.0129         | 0.856           |
| Months enrolled $> 0$            | 0.0260∗          | 0.0134         | 0.885           |
| Attendance rate $= 1.0$          | 0.0052           | 0.0146         | 0.110           |
| Attendance rate $\geq 0.95$      | 0.0306           | 0.0221         | 0.297           |
| Attendance rate $\geq 0.90$      | 0.0409∗          | 0.0233         | 0.472           |
| Attendance rate $\geq 0.85$      | 0.0474†          | 0.0200         | 0.578           |
| Attendance rate $\geq 0.80$      | 0.0445†          | 0.0180         | 0.652           |
| Attendance rate $\geq 0.70$      | 0.0304∗          | 0.0158         | 0.727           |
| Attendance rate $\geq 0.60$      | 0.0225           | 0.0138         | 0.776           |
| Attendance rate $\geq 0.50$      | 0.0246∗          | 0.0132         | 0.800           |
| Rate of unexcused absences $= 0$ | 0.0275           | 0.0216         | 0.438           |
| Rate of unexcused absences $\leq 0.05$ | 0.0380∗   | 0.0210         | 0.619           |
| Rate of unexcused absences $\leq 0.10$ | 0.0227   | 0.0173         | 0.699           |
| Rate of unexcused absences $\leq 0.15$ | 0.0275∗   | 0.0155         | 0.731           |
| Rate of unexcused absences $\leq 0.20$ | 0.0300†    | 0.0149         | 0.753           |
| Rate of unexcused absences $\leq 0.30$ | 0.0316†    | 0.0133         | 0.786           |
| Rate of unexcused absences $\leq 0.40$ | 0.0265†    | 0.0134         | 0.804           |
| Rate of unexcused absences $\leq 0.50$ | 0.0243∗    | 0.0127         | 0.818           |

Notes: These results are based on the nine non-Milwaukee counties. All models condition on the eight baseline observables and county/entry-month/semester FE. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level.

† $p<0.01$, ‡ $p<0.05$, * $p<0.1$
<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Full Sample</th>
<th>Female</th>
<th>Male</th>
<th>Minority</th>
<th>Parent at baseline</th>
<th>Not a parent at baseline</th>
<th>Dropout at baseline</th>
<th>Enrolled at baseline</th>
</tr>
</thead>
<tbody>
<tr>
<td>Months enrolled</td>
<td>0.0890*</td>
<td>0.0794</td>
<td>0.0606</td>
<td>0.1575</td>
<td>0.6426†</td>
<td>0.0217</td>
<td>0.5890†</td>
<td>0.0350</td>
</tr>
<tr>
<td></td>
<td>(0.0484)</td>
<td>(0.0692)</td>
<td>(0.0704)</td>
<td>(0.1153)</td>
<td>(0.2440)</td>
<td>(0.0441)</td>
<td>(0.1935)</td>
<td>(0.0451)</td>
</tr>
<tr>
<td>Attendance rate</td>
<td>0.0202*</td>
<td>0.0174</td>
<td>0.0170</td>
<td>0.0322</td>
<td>0.0881*</td>
<td>0.0148</td>
<td>0.0803*</td>
<td>0.0142</td>
</tr>
<tr>
<td></td>
<td>(0.0103)</td>
<td>(0.0158)</td>
<td>(0.0164)</td>
<td>(0.0206)</td>
<td>(0.0523)</td>
<td>(0.0112)</td>
<td>(0.0405)</td>
<td>(0.0101)</td>
</tr>
<tr>
<td>Rate of unexcused absences</td>
<td>-0.0182*</td>
<td>-0.0154</td>
<td>-0.0158</td>
<td>-0.0348</td>
<td>-0.1069*</td>
<td>-0.0114</td>
<td>-0.1007†</td>
<td>-0.0100</td>
</tr>
<tr>
<td></td>
<td>(0.0107)</td>
<td>(0.0156)</td>
<td>(0.0171)</td>
<td>(0.0223)</td>
<td>(0.0605)</td>
<td>(0.0111)</td>
<td>(0.0422)</td>
<td>(0.0103)</td>
</tr>
</tbody>
</table>

Notes: These results are based on the nine non-Milwaukee counties. The models condition on the other seven baseline observables, county fixed effects, and fixed effects specific to each semester/entry-month cell. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level.

† p<0.01, † p<0.05, * p<0.1
<table>
<thead>
<tr>
<th></th>
<th>Enrollment</th>
<th>Attendance Rate</th>
<th>Rate of Unexcused Absences</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Treatment</td>
<td>0.5890†</td>
<td>-</td>
<td>0.0803*</td>
</tr>
<tr>
<td></td>
<td>(0.1935)</td>
<td>(0.0405)</td>
<td>(0.0422)</td>
</tr>
<tr>
<td>Treatment x 1st Semester</td>
<td>-</td>
<td>0.5955†</td>
<td>0.1124*</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>(0.2397)</td>
<td>(0.0597)</td>
</tr>
<tr>
<td>Treatment x 2nd Semester</td>
<td>-</td>
<td>0.7207†</td>
<td>0.0236</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>(0.3025)</td>
<td>(0.0551)</td>
</tr>
<tr>
<td>Treatment x 3rd Semester</td>
<td>-</td>
<td>0.3652</td>
<td>0.0500</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>(0.3934)</td>
<td>(0.0605)</td>
</tr>
<tr>
<td>Treatment x 4th Semester</td>
<td>-</td>
<td>0.7653†</td>
<td>0.1478†</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>(0.4002)</td>
<td>(0.0652)</td>
</tr>
<tr>
<td>Treatment x 5th Semester</td>
<td>-</td>
<td>0.3784</td>
<td>0.0860</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>(0.3939)</td>
<td>(0.0725)</td>
</tr>
<tr>
<td>Treatment x 6th Semester</td>
<td>-</td>
<td>1.2006</td>
<td>0.1731</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>(1.5374)</td>
<td>(0.2585)</td>
</tr>
</tbody>
</table>

Notes: These results are based on baseline dropouts from the nine non-Milwaukee counties. All models condition on the eight baseline observables, fixed effects specific to each county, and fixed effects specific to each county/entry-month cell. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level. The p-value refers to an F-test for the equality of the treatment effects across semesters.

† p<0.01, † p<0.05, * p<0.1
Figure 4: Months Enrolled by Treatment Status, Baseline Dropouts

Figure 5: Attendance Rate by Treatment Status, Baseline Dropouts

Figure 6: Rates of Unexcused Absences by Treatment Status, Baseline Dropouts
Table 13 - Intent-to-Treat (ITT) Estimates: Alternative Outcomes Measures, Baseline Dropouts

| Outcome                                | Estimated Effect | Standard Error | \( F | T_{i} = 0 \) |
|----------------------------------------|------------------|----------------|----------------|
| Months enrolled = 4.5                  | 0.0802*          | 0.0423         | 0.317          |
| Months enrolled ≥ 4.0                  | 0.1256†          | 0.0495         | 0.347          |
| Months enrolled ≥ 3.0                  | 0.1324‡          | 0.0463         | 0.389          |
| Months enrolled ≥ 2.0                  | 0.1340‡          | 0.0453         | 0.442          |
| Months enrolled ≥ 1.0                  | 0.1395‡          | 0.0428         | 0.482          |
| Months enrolled > 0                    | 0.1935‡          | 0.0481         | 0.521          |
| Attendance rate = 1.0                  | -0.0055          | 0.0324         | 0.076          |
| Attendance rate ≥ 0.95                | -0.0312          | 0.0396         | 0.200          |
| Attendance rate ≥ 0.90                | -0.0183          | 0.0432         | 0.252          |
| Attendance rate ≥ 0.85                | 0.0194           | 0.0437         | 0.283          |
| Attendance rate ≥ 0.80                | 0.0251           | 0.0439         | 0.310          |
| Attendance rate ≥ 0.70                | 0.0792*          | 0.0465         | 0.338          |
| Attendance rate ≥ 0.60                | 0.1155†          | 0.0488         | 0.345          |
| Attendance rate ≥ 0.50                | 0.1256‡          | 0.0473         | 0.352          |
| Rate of unexcused absences = 0        | 0.0069           | 0.0492         | 0.203          |
| Rate of unexcused absences≤0.05        | 0.0278           | 0.0416         | 0.276          |
| Rate of unexcused absences≤0.10       | 0.0315           | 0.0407         | 0.307          |
| Rate of unexcused absences≤0.15       | 0.0664           | 0.0466         | 0.321          |
| Rate of unexcused absences≤0.20       | 0.0923†          | 0.0462         | 0.324          |
| Rate of unexcused absences≤0.30       | 0.1213†          | 0.0464         | 0.345          |
| Rate of unexcused absences≤0.40       | 0.1268‡          | 0.0464         | 0.352          |
| Rate of unexcused absences≤0.50       | 0.1283‡          | 0.0474         | 0.362          |

Notes: These results are based on baseline dropouts from the nine non-Milwaukee counties. All models condition on the eight baseline observables. The standard errors are reported in parentheses and adjusted for heteroscedasticity clustered at the county/entry-month level. The p-value refers to an F-test for the equality of the treatment effects across semesters.

\[ \hat{\theta}_{i} \] p<0.01, † p<0.05, * p<0.1