

ONLINE APPENDIX

Information Frictions and Skill Signaling in the Youth Labor Market

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A.1 Additional Labor Market Results

A.1.1 Earnings

The main text shows that letters of recommendation increase employment in the short term and earnings in the longer term. A natural question is whether the earnings increase comes from additional part-time employment or from shifting people into high-paying or full-time work. Although we can not observe hours to know for sure, we can look at the full earnings distribution by treatment group to get a sense for where the shifts in the distribution occur. Panel A of Figure A.8 shows the full raw earnings distribution (with the single extreme outlier top-coded, as in the main text). Because the treatment effects are small relative to the scale of all earnings over 4 years, it is hard to see them in Panel A (although it is clear that there is a control outlier, which helps to explain the sensitivity to different skewness adjustments discussed below).

Panel B of Figure A.8 zooms in on the bottom of the distribution, under \$25,000 in 4-year earnings, to make the treatment effect more visible (the distributions above \$25,000 are quite similar). This figure suggests that the bulk of the treatment effect comes from moving people near zero up to earning between \$2,000 and \$5,000. Over 4 years, this pattern is most easily explained by treatment youth having an additional part-time job. Figure 4 in the main text shows that this change was not just over the summer; employment and earnings effects are similar in summer quarters and other quarters.

There is also a smaller shift away from earnings between \$5,000 and \$8,000 and into earnings between \$20,000 and \$24,000. These higher earnings are consistent with more

persistent part-time work, or potentially a year of minimum wage, full-time work. Overall, the earnings distributions suggest that the letters help people enter the labor force or shift to somewhat more or better-paying work; they are not shifting youth into high-paying, full-time jobs.

The main text reports annual and cumulative earnings results for two functional forms of the earnings variable: raw (with one extreme outlier observation top-coded) and winsorized at the 99th percentile. Because we pre-specified that we would explore other adjustments for skewness, Table A.1 shows other transformations of the raw dollar amounts, including an alternative winsorization (at the 99.5th percentile), log earnings with different intercepts added to assess how much the infinite proportional change from 0 matters [$\log(\text{earnings} + 0.1, 1, 10, \text{ or } 100)$], and the inverse hyperbolic sine transformation.

The alternative winsorization in Panel A makes very little difference relative to the results in the main text. The other panels show that, as expected given that there are treatment effects on the extensive margin, the decision about what to add to the 0s does change the point estimates somewhat. Because the biggest change on the employment margin is in year 1, results in year 1 are most sensitive to what is added to 0. The results range from a 9.5 percent increase to a 30 percent increase in year 1 earnings, driven by the fact that so many people are moved off of 0, where the proportional change is undefined. Since fewer people are moved off of 0 for the cumulative earnings measures, those results are more sensible in magnitude, ranging from a 7 percent to a 12 percent increase in earnings over the four years. We emphasize the 4.9 percent increase in the main text, both because the winsorized results were our primary pre-specified outcome and because it is clear that the logged results are sensitive to how we handle the 0s.

A.1.2 Spell Length

The fourth column of Table 3 in the main text shows that treatment increases the average spell length among the first 3 (non-missing) spells. We argue that this result is an indication of improved job match quality among treatment youth relative to control youth. One reason to care about this result is that it pushes against the hypothesis that letters drive employers to inefficiently update (e.g., as might happen if previous applicants with letters were always stellar employees and employers incorrectly believe that any applicant with a letter will be similarly stellar). If employers did inefficiently update in this way, we might expect them to be more willing to hire treatment youth, but then to quickly fire them after learning that they were not as high productivity as expected, which would create inefficient churn. The fact that spell length increases with treatment, however, suggests that letters' signals instead help employers to successfully identify good matches.

The main text reports that across the 3 spells underlying Table 3, there is no treatment-control difference in the number of censored spells, despite treatment spells starting earlier. Table A.2 provides additional evidence on this pattern by looking separately at each of these spells. Each panel shows results for a different job spell, with spell 1 being the spell started the earliest, spell 2 being the spell started next, and so on. If spells are started in the same quarter, we assign the longer spell the lower spell number. We count any spell with at least one quarter occurring in the post-letter period. Youth must have a given spell number to appear in each panel, so the sample becomes more selected as the spell number rises (about 60 percent of the sample has a third spell). The first column reports treatment effects on the length of each spell, defined as the number of consecutive quarters worked at the same employer. The treatment effect on the length of individual spells is always positive, but imprecisely estimated when broken down by individual spell.

The control means suggest why differential censoring may not be a problem for these early spells: even the earliest spell has an average length of just under 4 quarters, so only 7 percent of them are censored (defined as a youth working at an employer in the last quarter we observe in the data). Censoring rises to about a quarter of third spells. We stop at spell 3 to avoid too much further censoring, and because the average number of spells in the sample is just over 3.

As shown in the second column, none of the censoring is significantly different by treatment group, suggesting that differential censoring is not biasing our spell length results. The last 3 columns of the table confirm that the results are robust to looking only at spells that are not censored. We report treatment effects on whether a spell lasts at least 2, 3, or 4 quarters, conditional on observing all the quarters. There is no evidence that letters are creating bad matches, with all but one of the point estimates positive. Overall, analysis at the individual spell level is a bit imprecise, which leads us to average these spell lengths (and report the censoring result across all 3 spells) in the main text.

A.1.3 Employer Type

Tables A.3 and A.4 separate employment and earnings effects by type of employer. Because the letter came on DYCD letterhead (the agency that runs the SYEP), it is possible that the letter increased the rate at which youth reapplied to the SYEP or engaged with future summer or term-time work where DYCD was the employer of record.

Table A.3 shows that this is not a main driver of our results. It reports labor market results separately for DYCD and for all other employers. The only significant increase in employment is at non-DYCD employers, meaning that the letters increased employment outside of the SYEP agency. Earnings impacts are directionally much larger at non-DYCD

employers, on the order of 5 rather than 1 percent.

Table A.4 shows in what types of industries letter recipients work. The classification across industry clusters is based on Gelber, Isen, and Kessler (2016), which groups industries that are over-represented in SYEP, like childcare and landscaping (cluster 1) separately from industries that are under-represented in SYEP, such as retail and food service (cluster 2). Letters directionally increase employment in both types of industries, only marginally significant in year 1 for cluster 2, with earnings increases concentrated in cluster 2 jobs. This pattern suggests that the letters are helping young people shift to jobs outside of the industries that they were most likely to be exposed to through SYEP. Given the evidence from Gelber, Isen, and Kessler (2016), which found that working in cluster 1 jobs results in lower overall earnings than the cluster 2 jobs, the patterns here are consistent with treatment youth using their letters to shift towards higher-paying industries.

A.2 Supervisor Ratings

Panel A of Figure A.9 shows the overall distribution of ratings that supervisors assigned to both treatment and control youth. As discussed in the main text, we designed the survey to maximize the information we would have available to produce recommendation letters, not to ensure that treatment and control youth would be treated equally on the survey. As such, we asked about each treatment youth first, on the same page as we asked supervisors to decide whether to produce a letter. After the supervisor had seen all treated youth, we then asked them a single question about the overall performance of each control youth—all on the same page—making it clear the control youth were not eligible for letters. This aspect of our design makes it possible that supervisors might use different decision rules across treatment and control youth when assessing whether to give a rating and what rating to give.

Indeed, treatment youth are significantly less likely to have been rated by a supervisor (66 versus 71 percent had a rating, $p < 0.001$). Panel B of Figure A.9 shows that treatment youth have a more compressed ratings distribution, with missing mass on both the highest and lower rating categories. This pattern might indicate that supervisors take the letters seriously, so are less likely to give very top marks when they know their responses will be included in a letter, but also less likely to give someone the lowest marks (perhaps to be kind to the youth, since supervisors did not know our exact decision rule for when not to send a bad letter).

Despite the potential for selection into having a rating, observable characteristics are generally still balanced in the sample with non-missing ratings, with a joint F-test (including the actual rating) failing to reject equality across all observables ($p = 0.609$). Table A.5, however, which breaks out the balance tests for youth receiving low versus high ratings,

shows that there is some imbalance within the group that receives low ratings ($p = 0.101$). Since breaking out the results by rating group is central to understanding whether employers are using the letters as signals to accurately update their beliefs, the potential for selection within the rating groups is of concern.

Because of the dramatic difference in having a rating and the small imbalance on observables for those with low ratings, the main text focuses on the subsample of rated youth on complete surveys. Table A.6 shows the equivalent balance tests for the subsample of youth who appeared on a fully completed survey (i.e., where the employer rated every youth on the survey). Although this is a selected group, full survey completion limits the scope for treatment and control youth to be differentially selected into getting a rating. Indeed, the difference in receiving a rating is much smaller in this sample: 31.6 percent for treatment youth and 32.5 percent for controls ($p = 0.066$). And, as the table shows, observables are entirely balanced within each rating group. (Panel C of Figure A.9 suggests there may still be some differences in exact ratings, but only across the ratings that are all classified as “high” in our regressions.) As a result, this is the subsample we use to assess how treatment effects vary by rating in the main text.

Despite our concern about the potential for selection, for completeness, Table A.7 shows the main labor market effects for everyone with a rating, without limiting the sample to completed surveys as in the main text. The patterns are fairly similar to the results in the main text, with the high-rated group showing significantly positive employment effects, especially in the early years, and much more positive earnings impacts than the low-rated group. The earnings point estimates are a bit smaller than in the main text and so not statistically significant outside of year 1, though they still generally grow over time for the high-rated group. In this sample, the low-rated group (where there is the most observable imbalance) has somewhat more positive employment effects, but still has negative earnings point estimates.

We have also tested whether treatment effects on applying to our job posting are different for those with a high versus low rating. Given that this limits an already reduced sample ($N = 4,000$) to those with ratings ($N = 2,783$, when we use all ratings), and then splits the sample into groups, this is not a highly powered test. The difference in the intent-to-treat effects for the high-rated group relative to the low-rated group (i.e., the interaction effect between treatment and being highly rated) is $\beta = 0.008$, $p = 0.721$, with a control rate of application for the low group of 0.078. The difference for the IV is $\beta = 0.018$, $p = 0.748$. So while it is possible that receiving a letter had a more positive effect on job search behavior for highly-rated youth, we cannot reject the null that both effects were zero.

A.3 Heterogeneity

Tables A.8 through A.15 show treatment effects for different subgroups of youth. Because of the number of hypothesis tests across these tables and the limited statistical power, we do not emphasize the statistical significance of any particular result. However, we pre-specified an interest in these divisions as exploratory, so we report the basic patterns here.^{1a}

A.3.1 Mechanisms and Heterogeneity

It is tempting to use basic cuts of the data to help understand the mechanisms driving our main effects. But theory makes clear that single cuts of the data may not be enough. Consider the prediction from the statistical discrimination literature that those with fewer available signals should benefit more from a new signal. That might tempt us to interpret heterogeneity by whether someone ever worked, for example, as a test of statistical discrimination, if we think having no work history means there is more uncertainty about performance.

Importantly, however, as Pallais (2014) proves, theoretical predictions about heterogeneity for these groups are not clear cut. It is only *conditional on ability* that signals should have a bigger effect for those with more uncertainty. If those without signals (e.g., those who have never worked) also have lower average productivity, it is not evident that signals should help that group more. If the letters more often reveal that those with no work history are less prepared for work, we should not expect the signal to improve labor market success.

Given our setting, there are a number of other factors that also vary by subgroup: whether supervisors generate a letter, how strong the letter is, whether workers are looking for work, and whether they decide to use a letter in their applications. To help interpret our subgroup effects, we report the first stage by group, and we summarize application and letter use behavior by group in Table A.16. That said, we emphasize that the many different factors that vary by subgroup make it hard to convert treatment heterogeneity into a clear mechanism story. Doing so would likely require significant assumptions about the structure of the job search process. In addition, as our pre-analysis plan anticipated, we are not well-powered for heterogeneity tests. As a result, while we report subgroup effects—to aid in comparisons to prior work and because descriptive patterns of subgroup results help speak to general questions about labor market inequality—we are cautious not to over-interpret

1a. We add two divisions that were not pre-specified: whether someone is in our education sample and whether they had worked prior to the summer of the SYEP. The former both helps to check whether labor market effects differ for the sample underlying the main education results and provides a rough cut by whether individuals are still in high school (though some of our sample is in high school but not in our education sample, because, e.g., they attend schools that are not in our education data).

these patterns.

A.3.2 Heterogeneity by Subgroups

Table A.8 compares labor market impacts for those who are and are not in the expected in high school sample. Both groups respond positively to the letters. The employment effects are slightly more persistent for those in the education sample, though cumulative earnings impacts are almost identical.

Table A.9 shows effects for those under 18 and those 18 and over at the time of application. Employment point estimates are slightly larger and earnings estimates slightly smaller for those under 18, but both sets of effects are statistically indistinguishable from the effects for older youth.

Table A.10 shows labor market impacts separately for young people who did or did not have any prior work experience (measured as appearing in the UI data) before the SYEP summer. Point estimates are larger and only statistically significantly different from zero for the group that had previous work experience, which is a similar finding as in Pallais (2014). This result is perhaps more consistent with the possibility that employers are using the letters to help identify those likely to be higher performers, rather than to just improve their priors about those with the least available information.

Table A.11 shows results separately for White and non-White youth. The latter group includes youth who are Black, Hispanic, Asian, and Mixed Race/Other in the SYEP data. All the main labor market effects are concentrated among non-White youth, with cumulative earnings effects marginally different from each other.

Tables A.12 and A.13 further break down the main labor market results separately by race and ethnicity subcategories (ITT and IV, respectively). They show that the employment impact is driven by somewhat larger effects for Asian and Hispanic youth, and to a lesser extent those in the Other category, with earnings effects suggestively larger as well. The likelihood of getting a letter is higher for these groups than for Whites (see first column of Table A.13), but even among compliers, the program impacts are larger for Asian, Hispanic, and Other youth. However, as in the main results, we are under-powered to detect group differences; we cannot reject the null that effects are the same across all groups.

Table A.14 shows that female SYEP participants are significantly more likely to receive a letter, with female compliers having suggestively larger employment effects in year 1. In contrast, earnings effects are quite similar by gender; if anything, men have slightly larger point estimates for earnings. The initially larger employment effect for women is consistent with the Abel, Burger, and Piraino (2020) result that the employment benefits of recommendation letters in South Africa were concentrated among women. But unlike in that

setting, young women in NYC do not face the same difficulty finding work relative to young men; indeed, consistent with broader U.S. patterns of young women outperforming their male counterparts, employment rates for women are considerably higher than for men in our sample. The fact that there are larger effects for women both in settings where priors are likely to favor and to disfavor women suggests that the effect is not simply about statistical discrimination, since priors should go in the opposite direction across settings. Additionally, our longer-term results suggest overall effects are fairly similar across gender.

Table A.15 shows effects by neighborhood economic mobility. Using the Opportunity Insights “upward mobility” data (<https://opportunityinsights.org/data/>), we use each individual’s zip code to assign their neighborhood an average income rank for children whose parents were in the 25th percentile of the national household income distribution. Opportunity Insights provides these data at the Census Tract level. We use the Zip Code Tabulation Area (ZCTA) crosswalk to map Census Tracts onto zip codes, which is the geographic information we have on our sample. In cases of multiple Census Tracts falling within a given ZCTA, we use the average upward mobility value (i.e., the unweighted mean across all upward mobility values that fall within the ZCTA). We divide the youth into those who live in areas with above and below median mobility, with median defined in-sample. Table A.15 shows labor market impacts for these two groups. There are positive effects for both those living in above-median and below-median neighborhoods, with early employment effects suggestively larger in places with below-median mobility, but earnings effects suggestively larger in places with above-median mobility.

A.3.3 Information on Letters by Subgroup

To help interpret the patterns of results by subgroup, Table A.16 shows some additional information about the letters for the different subgroups discussed in the previous section. The table shows the treatment group only, since they were the only ones eligible for a letter. The first column shows the proportion of each group that was sent a letter (i.e., having a supervisor agree to produce one and receiving ratings high enough to generate a letter); this summarizes the information shown in the “first stage” column of the separate heterogeneity results. The second column is conditional on the first, showing average overall employee rating on a scale from 1–7 for those who were sent a letter. The third column shows the proportion of each group that submitted an application in response to our job application, conditional on being one of the 2,000 treatment youth randomly selected to receive the job advertisement. The fourth column, conditional on the third, shows the proportion of the applicants that uploaded a letter of recommendation (ours or any other) as part of their application.

There is significant variation both in letter receipt and in average ratings. Non-white, female, in high school, previously-employed, and below-median neighborhood mobility youth are all more likely to receive a letter. But the higher rate of letter receipt does not always correspond with stronger letters, on average. For example, despite larger labor market impacts, non-White youth have significantly lower average ratings conditional on receiving a letter than their White counterparts. And they do not use the letter more frequently; their rate of letter usage is about 6 percentage points lower than the White youth who applied to our job posting, although the small sample size limits how well we can differentiate the groups. The basic pattern of results suggests that the larger labor market effects for non-White youth are likely to be driven by how employers respond, even to slightly weaker letters, rather than big differences in how the groups use the letters.

The only significant differences in letter usage are between those who were or were not in our education sample at the time of SYEP application, and relatedly, those who were under 18 versus 18 and older. This likely helps to explain the bigger employment point estimates for our education sample, who were much more likely to use the letter on our job application than those who were not expected in our school data.

A.4 Additional Education Results

A.4.1 Descriptive Statistics

Table A.17 shows descriptive statistics and treatment-control balance for our education sample. On average, students in our education sample are about 16 years old, 45 percent male, 42 percent Black, 31 percent Hispanic, 14 percent Asian, and 8 percent White. They are in 10th grade on average, attending about 90 percent of the days they are enrolled, and earning a C-plus average. Over 60 percent of them had not worked in UI-covered jobs prior to the SYEP. The table also shows that across all baseline characteristics, treatment and control groups are jointly balanced ($p = 0.149$). It is worth noting that there is some chance imbalance on GPA and on the proportion of the sample that is White; although the differences are substantively small (-0.39 on a 100-point GPA scale and 1 percentage point more likely to be White), they are statistically significant. As a result, the exact magnitude of the education results are somewhat more sensitive to how covariates are included in the regressions (see Appendix Section A.5). However, none of our substantive conclusions are sensitive to covariate choice.

A.4.2 Joint Work and Graduation Outcomes

In the main text, we note that there is evidence that the decrease in on-time graduation is driven by the same youth who are pulled into the labor force. This claim comes from examining the relationship between educational attainment and labor force involvement within the same individual. We define a set of mutually exclusive joint outcome indicators: working and graduating, never working but graduating, working and not graduating, and never working and not graduating. We define these indicators for all three of our education attainment measures: on-time graduation, ever graduating, and graduating or still attending school.

The treatment effects across these outcomes allow us to assess whether any potential shifts in educational attainment occur among the same group that experiences shifts in employment. Table A.18 shows the results. The third column of Panel A shows that there is a significant increase in the proportion of people who work but do not graduate on time of about 2.3 percentage points (16.6 percent) for compliers. Since everyone has to appear in one and only one of the columns, the other columns' estimates show where the marginal work-but-not-graduate-on-time group comes from. The shift to the third column appears to be spread across the other categories, with the biggest shifts from reductions in the number of people who both work and graduate on time, as well as those who neither work nor graduate on time. Although the results in the other columns are not significant, the point estimates suggest that some of those shifted by the letter just add work on top of what would have already been a failure to graduate on time. But for others, the letters seem to prevent them earning their on-time diploma.

Panel B, which measures whether people ever graduate and work, suggests that the decline in on-time graduation may not be permanent. There are no significant changes in work/ever graduate categories. The point estimate for working but not graduating is about a third as large as in Panel A, and there is also a positive point estimate for both working and graduating. The combination of Panels A and B is what drives our conclusion in the main paper that it is the shift into the labor force that slows down graduation, but that it appears most of the slowed-down students will eventually graduate.

Panel C provides some caution, though. By including continued school attendance as part of the dependent variable, it aims to capture what happens to students who are either too old to show up in the graduation data (graduating after their 6-year cohort) or too young to have reached their final graduation outcome. It suggests that there is still a letter-driven increase in working but not persisting in school. While about half of this shift appears to come from people who would otherwise not have worked or graduated (as indicated by the negative

point estimate in column 4), the other half seems to shift from groups that would otherwise have persisted (columns 1 and 2). In combination with Panel B, this might suggest that at least some of the students who could eventually graduate are not still attending school. Longer-term follow-up is needed to assess what these students' final outcomes will be; it is not uncommon for people at the margin of graduating to leave school temporarily and return later.

Nonetheless, these results suggest some caution about encouraging youth at the margin of school completion to join the labor force. The following section further explores this margin by splitting students by baseline academic achievement.

A.4.3 Explaining the Decline in On-time Graduation: Heterogeneity by GPA

In the main text, it is not entirely clear how seriously to take the marginal decline in on-time graduation, since no other educational outcomes show significant declines. If letters are truly slowing down graduation, we might expect to see the mechanisms through which that happens in some of our educational performance measures. In this section, we assess whether there is real concern that the increase in labor market participation prevents a subgroup of youth from the educational progress they would otherwise make. We do this by examining heterogeneity that should be closely related to whether youth are on the margin of graduation: baseline GPA. We split the education sample by whether students are over or under the median GPA in the baseline year (for non-missing GPAs only, $n = 17,732$, median GPA = 80.85).

Table A.19 shows the main education outcomes by GPA, focusing on the IV to conserve space, and Table A.20 shows the corresponding labor market outcomes, including the first stage. Above-median GPA students show no significant changes in education outcomes. But the top row of Table A.19 demonstrates that letters do, in fact, harm the educational progress of the below-median GPA students. They have lower year 1 enrollment (by 2.5 percentage points, or 2.6 percent), perhaps indicating that receiving a letter in the fall of the academic year deters some students from returning to school the following semester. Those that remain in school have significantly lower GPAs (by 0.85 points on a 100 point scale, or 1.2 percent). And though the increase in credits attempted is not statistically significant, it is positive, suggesting some of the drop in GPA might result in retaking courses, which could slow down graduation. Indeed, the decline in on-time graduation is larger and more statistically significant in this subgroup (5.7 percentage points, or 7.6 percent).

As in the main sample, the point estimate on whether below-GPA students ever graduate is considerably smaller than for on-time graduation (-0.02 compared to -0.06), suggesting

that at least some of those who are delayed catch up and eventually graduate. But overall school persistence and on-time college enrollment also have negative point estimates, so final conclusions may need to wait until everyone has had time to either graduate or leave school more permanently.

Consistent with the idea that it is increased labor force participation driving the educational changes, Table A.20 shows that the below-median students have a significantly larger increase in employment in year 1 that remains substantively large but not significant in years 2 and 3, with a significant increase on the intensive margin of work (number of quarters worked) in year 2. Above-median GPA students still benefit from letters, but largely with higher earnings rather than more employment. Table A.21 confirms that the changes in joint outcomes are also concentrated among the below-median students, including declines in persistence. So the bigger boost into the labor market appears likely to be pulling these marginal students out of school.

From a policy perspective, these results provide some caution against the recent push for governments to offer year-round work opportunities to students who might not otherwise obtain term-time jobs. Contrary to results using natural variation in work during school, our results suggest that pushing students into work could slow down the educational progress of lower-performing students. Whether this shift is welfare enhancing depends on how long earnings increases last, how that compares to the cost of extra school years, and whether any of the marginal students are deterred from finishing high school (which likely has a large negative impact on future earnings).

A.5 Robustness to Different Covariate Choices

The main text uses the post-double-selection LASSO (Belloni, Chernozhukov, and Hansen 2014a, 2014b; Belloni et al. 2012) to choose which covariates are included in each regression, as we pre-specified in our pre-analysis plan. For robustness, this section shows two different alternatives: including no covariates other than the cohort indicator needed for treatment to be conditionally random (i.e., controlling for randomization strata), and including all covariates that we feed into the post-double-selection process.

For employment outcomes, the covariates we feed into the lasso include indicators for: being male; being employed in each of the 2nd through 6th years prior to randomization; the earnings quartile of the pre-randomization year earnings; never being employed pre-SYEP; self-reporting being in high school, college, or being a high school graduate; being 15–16, 17–18, 19–20, or 21 and older; being part of the Ladders for Leaders program; being Hispanic, Asian, White, Other, or having missing race/ethnicity; not being matched to

the education data; and being in the expected in high school sample.^{2a}For the education outcomes, covariates we feed into the lasso include indicators for: being in grade 8 or under, grade 10, grade 11, or grade 12; being in deciles 1 through 9 of prior year GPA or missing GPA; being in quartiles 2 through 4 of the share of enrolled days attended; being male; being employed in each of the 2nd through 6th years prior to randomization; the earnings quartile of the pre-randomization year earnings; never being employed pre-SYEP; self-reporting being in high school, college, or being a high school graduate; being 15–16, 17–18, 19–20, or 21 and older; being part of the Ladders for Leaders program; and being Hispanic, Asian, White, Other, or having missing race/ethnicity.

Tables A.22 and A.23 show alternative results for labor market and education effects, respectively, controlling either for no covariates, other than the randomization stratum indicator needed for conditional independence, or all covariates. These tables lead to the same conclusions as the main tables. Because of the imbalance in several education baseline covariates discussed in section A.4.1, the point estimates on GPA and graduation measures become somewhat larger and more significant in specifications without covariate controls.

A.6 Comparing Our Main Sample and Everyone on a Survey

The main text focuses on the sample of youth who were on a survey that a supervisor started, a group that we pre-specified as being of special interest in our pre-analysis plan. This excludes 25,813 young people who were only on surveys that no one started. Since none of these individuals could possibly have been treated if assigned to treatment, everyone in this group is effectively a never-taker. Since we are able to observe this fact for both treatment and control youth on these surveys, we exclude them from our main analysis to help with power.

This section provides some additional information on who is excluded from the sample and the implications for our analysis. Table A.24 compares our main control group to everyone who was on an unopened survey (treatment and control) on baseline characteristics. Given that assignment to supervisors was not random, it is not surprising that young people whose supervisors did not start the survey are observably different than those in our main sample. Table A.24 shows that our main sample is younger, less Black and less White (more Hispanic and Asian), more likely to still be in high school, and generally less engaged in the labor force pre-randomization than those on unopened surveys.

Table A.25 shows the same comparison but for outcome measures rather than baseline

2a. Ladders for Leaders is a special application-based program within the broader SYEP.

characteristics (which is why we only use the control group for those on a started survey). The table indicates that our control group continues to be less involved in the labor market than those on unopened surveys during the outcome period, but more engaged and successful in school. There is, however, no significant difference in job application behavior, consistent with the argument in the main text that differences in employment status do not affect the decision of whether to apply to our job.

Given the observable differences between our main sample and those on unopened surveys, our estimates are most externally valid for the group that would look most like those in our main sample: young people whose supervisors fill out the surveys when asked, without any requirement to do so. It is possible that forcing supervisors to fill out surveys for their employees could generate somewhat different effects, given that the population of youth affected would be observably different.

Table A.26 shows the main employment and earnings results for the full sample of everyone on a survey, rather than our main sample of everyone on a started survey. As we would expect from the inclusion of almost 26,000 additional never-takers, the estimates are somewhat less precise than our main results. But the patterns are quite similar and still statistically significant at the 0.1 level: an increase in year 1 employment that fades out over time, and an increase in earnings that grows in both levels and proportions over time to an additional \$1,470 (5.3 percent) in cumulative earnings.

A.7 Additional Figures and Tables

Figure A.1: Example Supervisor Survey Invitation Email

Dear Judd Kessler,

Thank you for your participation in the 2017 Summer Youth Employment Program (SYEP), run by the New York City Department of Youth and Community Development.

For the second year, we are running a "letter of recommendation" program. As part of this program, **we are asking you to complete a very short survey** about some of the youth who worked for you this summer (the survey should take about 1 minute per selected youth).

Positive responses will be turned into letters of recommendation for the youth. We expect these letters to help youth capitalize on their experience working for you this summer.

To join employers like you in participating, please click on this personalized link by **a week from tomorrow, Friday, October 20th: [Take the survey.](#)**

If you have any questions about the program, please see a further description on our website [here](#).

If you have additional questions, you can contact our academic partners: Judd B. Kessler (judd.kessler@wharton.upenn.edu) at the University of Pennsylvania and Sara Heller (hellersa@sas.upenn.edu).

Sincerely,

SYEP Team

Follow the link to opt out of future emails:
[Click here to unsubscribe](#)

Figure A.2: Screen Shots from Beginning of Supervisor Survey

Judd Kessler, thank you for participating in the 2017 Summer Youth Employment Program, run by the New York City Department of Youth and Community Development.

For the second year, we are conducting a "letter of recommendation" program. We are asking you, and employers like you, to answer a very short survey about some of the youth who worked for you this summer. If you rate a youth positively, your responses will be turned into sentences and put into a recommendation letter from you on DYCD letterhead. The youth can then show this letter to future educators and potential employers. (If you are interested, see a sample letter [here](#).)

So that we can ask you about the correct youth, please confirm the following information:

My name is Judd Kessler.

Yes

No

I supervised or worked with summer youth employees at the University of Pennsylvania.

Yes

No

The following youth from University of Pennsylvania have been randomly selected to participate in the program. Please select which youth you supervised or worked with this summer. If you did not supervise any youth this summer, please leave these boxes unchecked.

Sara Heller

Andre Padilla

William Schmidt

Fernando Willis

You have indicated that you supervised the following youth who have been randomly selected to participate in this program.

Sara Heller
Andre Padilla
William Schmidt
Fernando Willis

For each youth, we will ask you for an overall rating and give you the option to create a letter of recommendation for that youth.

If you choose to create a letter for a particular youth, you will also have the option to include your contact information so that their potential future employers can reach you as a reference.

To be a reference for one or more of the youth listed above, please provide a phone number and/or email address so that potential future employers know how to reach you. (We will only ask for your information once, but we'll ask you if you want to be a reference for each youth separately. So even if you provide contact information here, it will only be included in letters for the youth you select.)

Phone number

Email address



Figure A.3: Screenshot of Control Youth Rating on Supervisor Survey

While the following youth are not part of the program, our records indicate that they worked at the University of Pennsylvania, and we are curious how they did. Please answer the following question about overall performance for any youth you supervised or worked with this summer. **Please leave blank for any youth you did not supervise this summer.**

Overall, how would you rate the following youth as an employee?

	Very poor	Poor	Neutral	Good	Very good	Excellent	Exceptional
Patti Dennis	<input type="checkbox"/>						
Otis Elliott	<input type="checkbox"/>						
Juanita Guerrero	<input type="checkbox"/>						
Russell Higgins	<input type="checkbox"/>						
Ed White	<input type="checkbox"/>						



Figure A.4: Example Cover Letter to the Letter of Recommendation



November 1, 2017

Sara Heller
123 Fake Street
New York, NY 10003

Dear Sara,

This past summer you participated in a New York City summer program. This letter contains five copies of a letter of recommendation your supervisor wrote for you. [You should also have received a link to an electronic copy at [Student Email], in case you want to have an electronic version or print out more of copies of the letter.]

This year, some participants were included in a "letter of recommendation" program. You were included in this program, and your employer gave us feedback that could help you get a job or show your teachers your strengths. We hope you will show your letter of recommendation to your teachers, your guidance counselor, and potential employers (for example, by including it in job applications).

If you have any questions about the program, please see a description on our website here: https://www1.nyc.gov/assets/dycd/downloads/pdf/FAQs_Pilot_2017.pdf

If you have additional questions, you can contact our academic partners: Judd B. Kessler (judd.kessler@wharton.upenn.edu) at the University of Pennsylvania, and Sara Heller (hellersa@sas.upenn.edu).

Sincerely,

DYCD Team

The New York City Department of Youth and Community Development (DYCD) invests in a network of community-based organizations and programs to alleviate the effects of poverty and to provide opportunities for New Yorkers and communities to flourish.

Empowering Individuals • Strengthening Families • Investing in Communities

Notes: This cover letter accompanied five copies of the recommendation sent to youth. The text in brackets appeared when we had an email address on file for the youth.

Figure A.5: Example Job Advertisement Email



Youth Job Opening!

Dear Sara:

A professor at the University of Pennsylvania is looking for former NYC summer job program participants, like you, to **apply for a short-term and flexible job.**

This is an opportunity to **earn money and gain work experience** while helping improve future youth employment programs.

Those hired will **not** need to be on site at the University of Pennsylvania. All tasks and duties necessary to the position will be completed remotely (on an Internet capable computer or by mail).

Qualifications:

- Responsible
- Self-motivated
- Enthusiastic approach
- Some work experience preferred

Compensation for the job is \$15/hour.

If you are **Sara Heller**, click this link to apply (**application due by March 30th**):

[Click here for your personal job application](#)

All others who are interested can click this link for more information and to apply:

[General job application](#)

Figure A.6: Job Application Prompts to Upload Supporting Documents and to be Considered for More Selective Job

If you have supporting documents (e.g. resume or other documents that might strengthen your application), please upload below. (You may upload up to THREE files):

Drop files or click here to upload

Drop files or click here to upload

Drop files or click here to upload

In addition to the regular job that pays \$15/hour, there is a second job that pays \$18/hour. The second job is more selective and so requires a stronger application. If you are interested in also being considered for this second, more-selective job, please click the box below.

Yes, please consider me for the second, more-selective job (\$18/hour) as well as the regular job (\$15/hour).

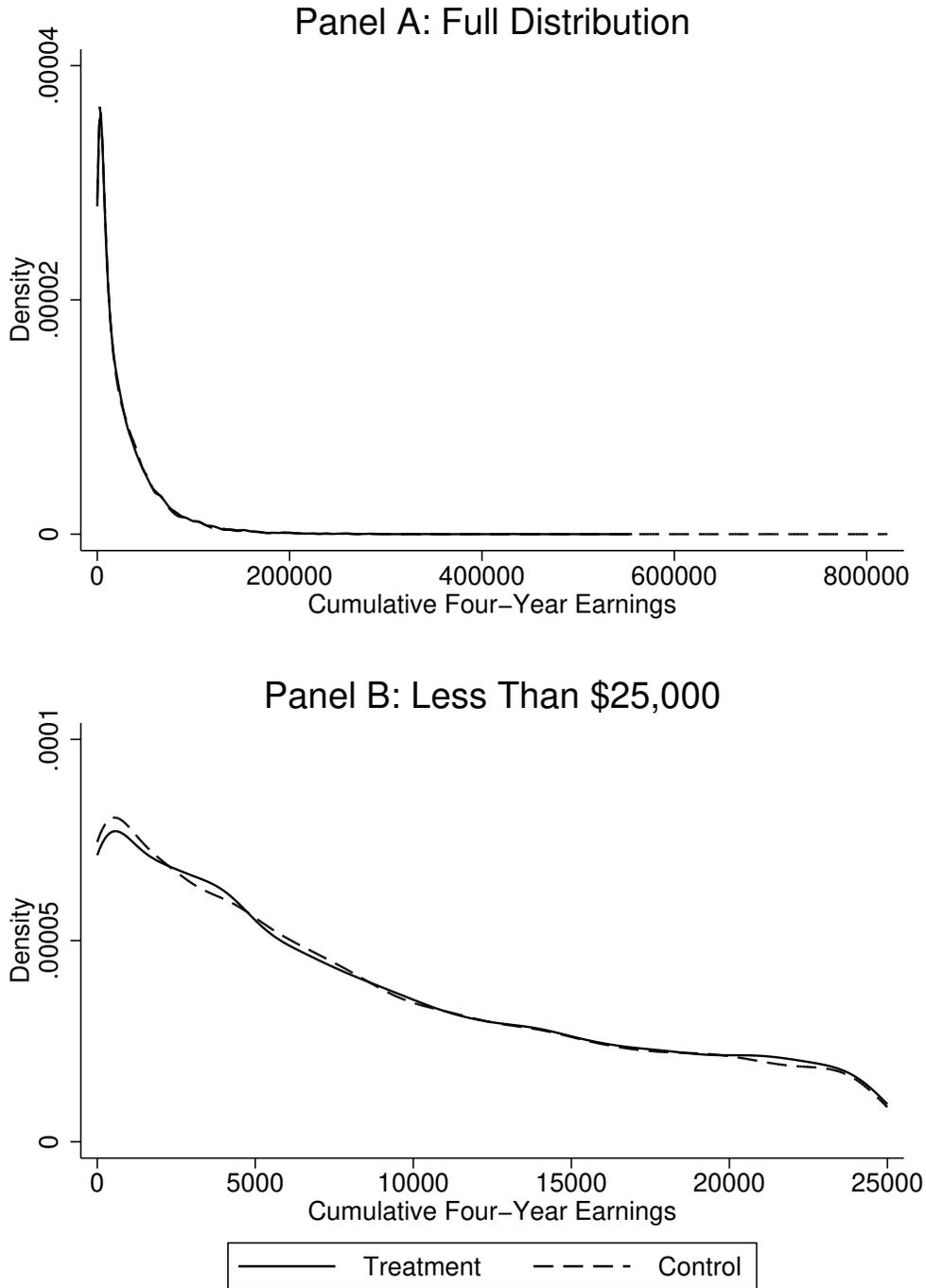
Figure A.7: Available 4th- to 6th-Year Graduation Data Relative to Randomization, by Grade and Study Cohort

Pre-Randomization Grade	8th	9th	10th	11th	12th
2016 Study Cohort					
Year Relative to Randomization	N = 268 994 1,313 1,459 149				
-1 (graduated by 8/2016)					4th
1 (by 8/2017)				4th	5th
2 (by 8/2018)			4th	5th	6th
3 (by 8/2019)		4th	5th	6th	
4 (by 8/2020)	4th	5th	6th		
5 (by 8/2021)	5th	6th			
2017 Study Cohort					
	N = 1,177 3,543 4,984 5,249 578				
-1 (by 8/2017)					4th
1 (by 8/2018)				4th	5th
2 (by 8/2019)			4th	5th	6th
3 (by 8/2020)		4th	5th	6th	
4 (by 8/2021)	4th	5th	6th		
		= Included in graduation measures			
		= Not old enough to observe			

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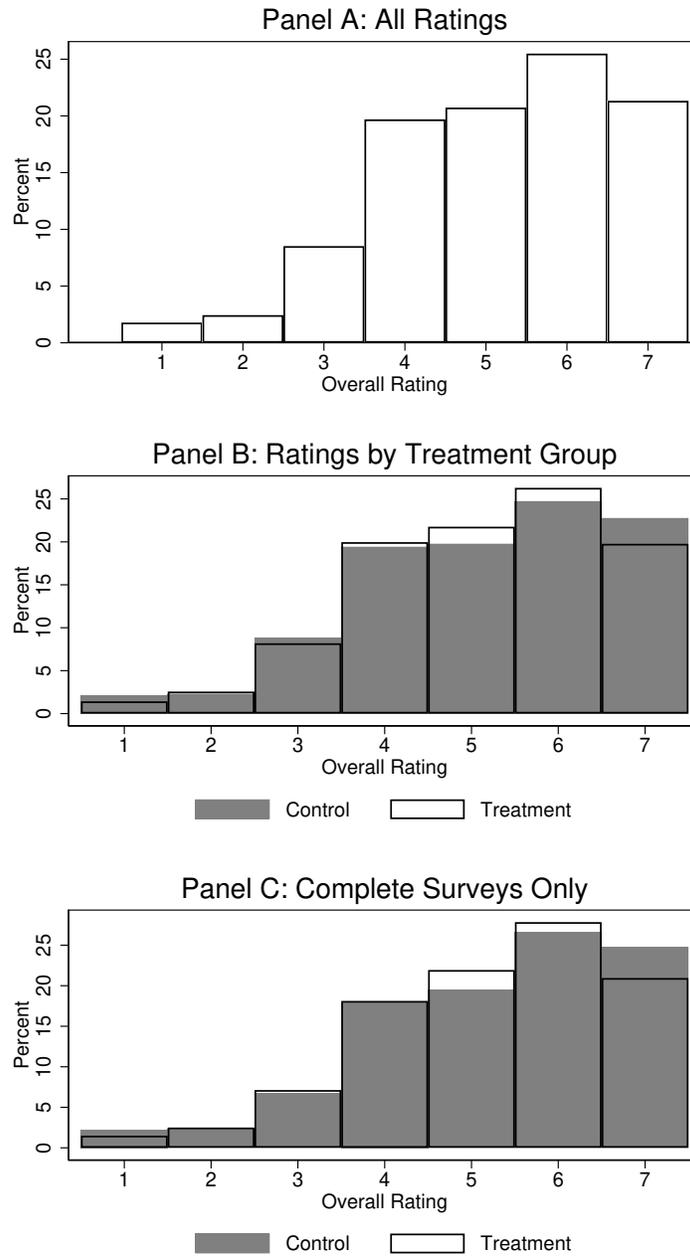
Notes: Figure shows when 4th-, 5th-, and 6th-year graduation outcomes are observed for students in each pre-randomization grade level by study cohort. Black boxes define our main “expected in high school” sample, for whom at least on-time graduation is observed. Gray boxes show the graduation outcomes that are not yet observed in our data. Only 12th graders who had not graduated prior to letter distribution are included in these samples, so they are all recorded as not having graduated by their 4th year graduation date in year -1.

Figure A.8: Cumulative Earnings Distribution



Notes: Figure shows the distribution of total earnings for treatment and control groups over 4 years, with one extreme outlier in one quarter (over \$3 million) top-coded to equal the next highest quarterly amount in the data prior to summing over all quarters. Panel A shows the full distribution. Panel B zooms in on the lower end of the distribution to make treatment-control differences visible.

Figure A.9: Distribution of Supervisor Ratings



Notes: $N = 29,877$ for all surveys and $13,911$ for completed surveys. Figure shows distribution of non-missing supervisor ratings for everyone (Panel A), separately by treatment group (Panel B), and by treatment group just for youth on fully-completed surveys (Panel C). Our main analysis maps categories 1–4 to “low” and categories 5–7 to “high.”

Table A.1: Earnings Impacts across Different Skewness Adjustments

Year	1	2	3	4	Cumulative
Panel A: Winsorized at 99.5th Percentile					
ITT	57.96 (43.16)	104.37 (71.94)	128.83 (96.65)	214.72* (128.38)	544.52** (277.26)
CM	3532	5925	7378	9927	26852
Sent Letter (IV)	149 (106.66)	267 (177.76)	330 (238.80)	546.06* (317.24)	1348.83** (685.56)
CCM	3682	6132	7554	10239	27661
Panel B: Log(Earnings + 0.1)					
ITT	0.125*** (0.042)	0.073 (0.044)	0.042 (0.048)	0.016 (0.050)	0.048 (0.031)
CM	4.92	5.44	4.86	5.36	8.68
Sent Letter (IV)	0.309*** (0.104)	0.18 (0.110)	0.109 (0.119)	0.04 (0.124)	0.12 (0.077)
CCM	4.94	5.56	5.01	5.55	8.76
Panel C: Log(Earnings + 1)					
ITT	0.095*** (0.033)	0.059* (0.035)	0.035 (0.038)	0.013 (0.040)	0.042* (0.026)
CM	5.61	6.08	5.67	6.09	8.86
Sent Letter (IV)	0.236*** (0.081)	0.145* (0.087)	0.091 (0.095)	0.033 (0.100)	0.105* (0.063)
CCM	5.64	6.18	5.79	6.25	8.94
Panel D: Log(Earnings + 10)					
ITT	0.066*** (0.024)	0.045* (0.026)	0.028 (0.029)	0.012 (0.031)	0.035* (0.021)
CM	6.30	6.73	6.48	6.83	9.04
Sent Letter (IV)	0.164*** (0.058)	0.111* (0.064)	0.069 (0.071)	0.028 (0.076)	0.088* (0.051)
CCM	6.34	6.81	6.58	6.95	9.11
Panel E: Log(Earnings + 100)					
ITT	0.038** (0.015)	0.031* (0.017)	0.02 (0.020)	0.01 (0.021)	0.028* (0.016)
CM	7.03	7.40	7.31	7.59	9.24
Sent Letter (IV)	0.095** (0.037)	0.076* (0.043)	0.05 (0.048)	0.02 (0.053)	0.070* (0.039)
CCM	7.07	7.47	7.38	7.68	9.30
Panel F: Asinh(Earnings)					
ITT	0.104*** (0.035)	0.063* (0.038)	0.037 (0.041)	0.015 (0.043)	0.044 (0.027)
CM	6.09	6.58	6.12	6.56	9.50
Sent Letter (IV)	0.258*** (0.088)	0.155* (0.094)	0.097 (0.102)	0.036 (0.107)	0.109 (0.067)
CCM	6.12	6.69	6.25	6.73	9.58

Notes: N = 43,409. Winsorization in Panel A recodes each quarter's highest earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.2: Spell Length and Censoring

	Total Spell Length	Spell Censored	Lasts at Least 2 Qtrs	Lasts at Least 3 Qtrs	Lasts at Least 4 Qtrs
Spell 1					
ITT	0.0331 (0.0365)	-0.0024 (0.0025)	0.0019 (0.0045)	0.0046 (0.0047)	0.0004 (0.0045)
CM	3.72	0.07	0.61	0.45	0.35
IV	0.0825 (0.0897)	-0.0060 (0.0061)	0.0047 (0.0111)	0.0114 (0.0116)	0.0013 (0.0111)
CCM	3.93	0.07	0.63	0.49	0.38
N	40088	40088	39537	39159	38914
Spell 2					
ITT	0.0412 (0.0279)	-0.0046 (0.0037)	0.0021 (0.0054)	0.0078 (0.0053)	0.005 (0.0048)
CM	2.71	0.15	0.57	0.36	0.25
IV	0.10 (0.0678)	-0.0111 (0.0091)	0.0053 (0.0131)	0.0193 (0.0130)	0.0124 (0.0117)
CCM	2.78	0.16	0.59	0.38	0.26
N	34228	34228	32737	31769	31126
Spell 3					
ITT	0.0149 (0.0274)	-0.0070 (0.0051)	0.0009 (0.0063)	0.0023 (0.0064)	-0.0024 (0.0059)
CM	2.51	0.24	0.60	0.37	0.25
IV	0.0370 (0.0660)	-0.0172 (0.0123)	0.0023 (0.0152)	0.0051 (0.0153)	-0.0062 (0.0141)
CCM	2.56	0.25	0.61	0.38	0.26
N	26099	26099	23849	22545	21556

Notes: Total Spell Length conditions on youth having a spell. Censored is an indicator for working in a spell in the last quarter observed. Indicators for at least X quarters are conditional on observing at least X quarters in the data. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Labor Market Effects for DYCD and Non-DYCD Employers

Year	DYCD					Non-DYCD Employers				
	1	2	3	4	Cumulative	1	2	3	4	Cumulative
Panel A: Employment										
ITT	0.0049 (0.0046)	0.0034 (0.0041)	-0.0022 (0.0020)	0.0000 (0.0024)	0.0003 (0.0046)	0.0088** (0.0040)	0.0009 (0.0043)	0.0023 (0.0044)	0.0014 (0.0045)	0.0037 (0.0035)
CM	0.4158	0.2619	0.052	0.0698	0.5227	0.4254	0.5654	0.6252	0.6494	0.8269
Sent Letter (IV)	0.0124 (0.0114)	0.0081 (0.0101)	-0.0054 (0.0050)	0.0000 (0.0059)	0.001 (0.0114)	0.0221** (0.0100)	0.0023 (0.0106)	0.0063 (0.0108)	0.0041 (0.0110)	0.0091 (0.0085)
CCM	0.419	0.253	0.056	0.070	0.531	0.429	0.588	0.638	0.664	0.836
Panel B: Earnings, Winsorized at 99th Percentile										
ITT	1.22 (10.68)	3.35 (9.94)	-4.48 (5.16)	2.63 (4.87)	9.50 (34.59)	58.46 (43.54)	99.84 (72.60)	132.97 (96.82)	212.58* (128.62)	506.15* (274.77)
CM	810	572	117	131	2527	2724	5353	7261	9796	25134
Sent Letter (IV)	2.28 (26.42)	7.94 (24.56)	-11.03 (12.78)	6.48 (12.06)	26.88 (85.42)	144.72 (107.56)	253.76 (179.44)	341.78 (239.22)	540.39* (317.84)	1290.77* (678.88)
CCM	870	574	128	128	2592	2816	5563	7426	10111	25905

N = 43,409. DYCD shows employment and earnings at employers with the FEIN of the agency that runs the SYEP. Non-DYCD shows all other employment. Winsorization in Panel B recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.4: Labor Market Effects by Industry Cluster

Year	SYEP-Related Industries (Cluster 1)					Other Industries (Cluster 2)				
	1	2	3	4	Cumulative	1	2	3	4	Cumulative
Panel A: Employment										
ITT	0.0047	0.0063	0.0001	-0.0013	0.0017	0.0071*	0.0013	0.0029	0.0000	-0.0007
	(0.0047)	(0.0047)	(0.0042)	(0.0043)	(0.0042)	(0.0040)	(0.0044)	(0.0046)	(0.0047)	(0.0042)
CM	0.5243	0.4407	0.2832	0.3092	0.7252	0.3104	0.4246	0.468	0.4879	0.7043
Sent Letter (IV)	0.0117	0.0151	0.0003	-0.0029	0.004	0.0180*	0.0036	0.0076	0.0000	-0.0013
	(0.0116)	(0.0116)	(0.0104)	(0.0108)	(0.0104)	(0.0099)	(0.0110)	(0.0114)	(0.0116)	(0.0104)
CCM	0.537	0.443	0.301	0.33	0.743	0.307	0.435	0.468	0.493	0.713
Panel B: Earnings, Winsorized at 99th Percentile										
ITT	17.06	-2.41	-54.79	77.61	36.25	35.34	104.35	193.36**	158.52	487.14**
	(29.72)	(48.31)	(68.03)	(91.12)	(193.74)	(39.11)	(64.54)	(83.22)	(109.27)	(236.85)
CM	1645	2242	2580	3576	10043	1853	3614	4689	6224	16380
Sent Letter (IV)	46.17	-5.44	-137.75	190.07	102.83	88.04	264.07*	487.93**	403.5	1233.96**
	(73.43)	(119.46)	(168.22)	(225.21)	(478.99)	(96.71)	(159.56)	(205.75)	(270.10)	(585.23)
CCM	1802	2441	2849	3804	10887	1855	3603	4542	6229	16240

N = 43,409. Industry definition follows the cluster definitions in Gelber, Isen, and Kessler 2016. SYEP-related include employment in industries that are over-represented among summer jobs in the program. Other industries are those under-represented in summer jobs. Winsorization in Panel B recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.5: Balance for All Rated Youth by Rating Group

	Control	Treatment	Test of	Control	Treatment	Test of
	Low	Low	Difference	High	High	Difference
N	5062	4632		10425	9768	
Age	17.14	17.06	0.084	17.25	17.25	1.000
Male	0.449	0.448	0.935	0.414	0.417	0.753
Black	0.492	0.500	0.419	0.382	0.371	0.118
Hispanic	0.292	0.294	0.836	0.284	0.287	0.678
Asian	0.099	0.091	0.224	0.147	0.159	0.015
White	0.069	0.070	0.875	0.140	0.137	0.566
Other Race	0.049	0.045	0.392	0.047	0.045	0.598
In High School	0.782	0.787	0.549	0.739	0.734	0.371
HS Graduate	0.046	0.043	0.469	0.040	0.040	0.768
In College	0.133	0.132	0.950	0.204	0.208	0.390
Not in UI Data	0.006	0.006	0.862	0.003	0.003	0.440
Never Employed Pre-SYEP	0.461	0.489	0.007	0.438	0.437	0.861
Ever Worked, Year -4	0.144	0.129	0.026	0.159	0.159	0.963
Earnings, Year -4	258	254	0.875	332	341	0.715
Ever Worked, Year -3	0.254	0.236	0.037	0.274	0.282	0.178
Earnings, Year -3	492	463	0.375	613	630	0.591
Ever Worked, Year -2	0.424	0.403	0.035	0.450	0.454	0.524
Earnings, Year -2	974	862	0.017	1104	1124	0.612
Ever Worked, Year -1	0.964	0.978	0.000	0.989	0.993	0.012
Earnings, Year -1	2169	2101	0.209	2478	2520	0.334
No Education Match	0.094	0.089	0.399	0.131	0.130	0.846
In HS Sample	0.488	0.494	0.542	0.440	0.441	0.826
Joint F-test	F(24, 9587) = 1.382, p=.101			F(24, 19643) = .711, p=.846		

Notes: Sample includes all youth with employer rating (N = 29,887, 256 youth missing race/ethnicity). Low includes rating categories 1–4; High includes rating categories 5–7. Test of difference reports the p-value from a regression of each characteristic on a treatment indicator within that rating group, controlling for a cohort indicator and using standard errors clustered on individual.

Table A.6: Balance by Rating Group, Fully Completed Surveys

	Control	Treatment	Test of	Control	Treatment	Test of
	Low	Low	Difference	High	High	Difference
N	2209	2092		4833	4777	
Age	17.09	17.09	0.919	17.26	17.23	0.453
Male	0.440	0.439	0.937	0.400	0.409	0.352
Black	0.505	0.535	0.053	0.388	0.381	0.481
Hispanic	0.277	0.258	0.178	0.286	0.292	0.491
Asian	0.117	0.111	0.573	0.165	0.178	0.090
White	0.051	0.047	0.465	0.114	0.105	0.145
Other Race	0.050	0.049	0.874	0.047	0.044	0.461
In High School	0.785	0.783	0.846	0.735	0.736	0.941
HS Graduate	0.042	0.041	0.808	0.037	0.031	0.141
In College	0.137	0.139	0.890	0.211	0.216	0.550
Not in UI Data	0.005	0.007	0.595	0.003	0.004	0.352
Never Employed Pre-SYEP	0.481	0.482	0.942	0.450	0.463	0.222
Ever Worked, Year -4	0.134	0.125	0.390	0.150	0.149	0.963
Earnings, Year -4	238	232	0.887	326	308	0.578
Ever Worked, Year -3	0.242	0.234	0.538	0.259	0.266	0.394
Earnings, Year -3	439	447	0.864	596	582	0.749
Ever Worked, Year -2	0.407	0.394	0.415	0.435	0.426	0.388
Earnings, Year -2	909	803	0.110	1041	1035	0.908
Ever Worked, Year -1	0.970	0.979	0.065	0.989	0.991	0.333
Earnings, Year -1	2075	2007	0.361	2427	2358	0.244
No Education Match	0.083	0.078	0.552	0.111	0.114	0.621
In HS Sample	0.498	0.493	0.736	0.460	0.455	0.660
Joint F-test	F(24, 4264) = .889, p=.618			F(24, 9471) = .862, p=.658		

Notes: Sample includes all youth on a fully completed survey (N=13,911, 167 youth missing race/ethnicity). Low includes rating categories 1–4; High includes rating categories 5–7. Test of difference reports the p-value from a regression of each characteristic on a treatment indicator within that rating group, controlling for a cohort indicator and using standard errors clustered on individual.

Table A.7: Employment and Earnings Effects by Rating, On Any Survey

	Y1	Y2	Y3	Y4	Cumulative	
	Employment					
ITT, Low Ratings	0.0135 (0.0089)	0.0001 (0.0089)	-0.0101 (0.0092)	0.0099 (0.0094)	0.0097* (0.0054)	
ITT, High Ratings	0.0122** (0.0059)	0.0100* (0.0060)	0.0068 (0.0063)	0.0008 (0.0063)	0.0003 (0.0035)	
P-value, test of diff.	0.906	0.358	0.131	0.418	0.145	
CM, Low	0.679	0.711	0.653	0.664	0.915	
CM, High	0.724	0.732	0.659	0.694	0.931	
	First Stage					
IV, Low Ratings	0.3067*** (0.0068)	0.0435 (0.0290)	0.0002 (0.0290)	-0.0331 (0.0301)	0.0324 (0.0307)	0.0316* (0.0177)
IV, High Ratings	0.7529*** (0.0043)	0.0161** (0.0079)	0.0132* (0.0079)	0.0089 (0.0084)	0.001 (0.0084)	0.0003 (0.0047)
P-value, test of diff.	0.000	0.362	0.665	0.179	0.323	0.088
CCM, Low	0.65	0.726	0.685	0.660	0.895	
CCM, High	0.719	0.733	0.664	0.700	0.931	
	Earnings, Winsorized at 99th Percentile					
ITT, Low Ratings	0.41 (84.53)	2.43 (140.43)	-25.25 (181.03)	-126.73 (237.26)	-142.21 (518.51)	
ITT, High Ratings	116.09* (65.73)	175.34 (109.88)	71.40 (148.49)	293.61 (198.99)	642.02 (428.95)	
P-value, test of diff.	0.280	0.332	0.68	0.175	0.244	
CM, Low	3202	5418	6598	8629	23884	
CM, High	3778	6292	7942	10752	28874	
	First Stage					
IV, Low Ratings	0.3067*** (0.0068)	-17.41 (274.99)	12.81 (457.96)	-82.44 (590.58)	-407.86 (774.24)	-464.03 (1691.22)
IV, High Ratings	0.7529*** (0.0043)	151.68* (87.20)	234.65 (145.95)	94.80 (197.18)	387.30 (264.17)	852.53 (569.70)
P-value, test of diff.	0	0.558	0.644	0.776	0.331	0.461
CCM, Low	3204	5562	6676	8836	24273	
CCM, High	3804	6324	8039	10853	29161	

Notes: N = 29,887. Sample includes all youth with ratings, regardless of whether supervisor completed all the ratings on the survey. Low includes rating categories 1–4; High includes rating categories 5–7. Earnings winsorization recodes each quarter’s highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.8: Employment and Earnings Effects by Expected in HS Sample

		Y1	Y2	Y3	Y4	Cumulative
Employment						
ITT, Expected in High School		0.0145** (0.0065)	0.0126* (0.0064)	0.0066 (0.0066)	0.0058 (0.0066)	0.0048 (0.0039)
ITT, Not Expected in High School		0.0113** (0.0052)	0.0002 (0.0053)	-0.0002 (0.0057)	-0.0028 (0.0059)	0.0012 (0.0032)
P-value, test of diff.		0.698	0.139	0.441	0.33	0.472
CM, Exp. in HS		0.635	0.677	0.611	0.673	0.913
CM, Not Exp. in HS		0.755	0.756	0.683	0.689	0.930
First Stage						
IV, Expected in High School	0.4138*** (0.0049)	0.0351** (0.0158)	0.0304* (0.0155)	0.0169 (0.0161)	0.0141 (0.0159)	0.0119 (0.0094)
IV, Not Expected in High School	0.3966*** (0.0044)	0.0285** (0.0132)	0.0005 (0.0135)	-0.0004 (0.0143)	-0.0069 (0.0148)	0.0029 (0.0082)
P-value, test of diff.	0.010	0.745	0.146	0.42	0.333	0.471
CCM, Exp. in HS		0.643	0.68	0.611	0.678	0.917
CCM, Not Exp. in HS		0.743	0.770	0.706	0.713	0.929
Earnings, Winsorized at 99th Percentile						
ITT, Expected in High School		15.95 (40.95)	76.62 (76.23)	160.69 (104.62)	236.27* (142.76)	543.09* (291.48)
ITT, Not Expected in High School		95.15 (71.46)	125.07 (115.40)	102.31 (154.25)	193.45 (203.27)	545.72 (446.95)
P-value, test of diff.		0.336	0.726	0.754	0.863	0.996
CM, Exp. in HS		2097	3889	4952	7124	18077
CM, Not Exp. in HS		4727	7620	9399	12261	34158
First Stage						
IV, Expected in High School	0.4138*** (0.0049)	47.67 (99.06)	203.25 (184.31)	413.50 (252.57)	607.46* (346.02)	1323.39* (705.21)
IV, Not Expected in High School	0.3966*** (0.0044)	236.58 (179.71)	315.61 (290.65)	258.62 (388.61)	486.65 (511.35)	1370.80 (1125.81)
P-value, test of diff.	0.010	0.358	0.744	0.739	0.845	0.972
CCM, Exp. in HS		2275	4063	4954	6907	18171
CCM, Not Exp. in HS		4895	7922	9796	13119	35842

Notes: N = 43,409 (19,714 expected in high school data, 23,695 out of school or not expected in later education data). Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.9: Employment and Earnings Effects by Age

		Y1	Y2	Y3	Y4	Cumulative
		Employment				
ITT, Under 18		0.0137**	0.0087	0.0056	0.0054	0.0045
		(0.0055)	(0.0054)	(0.0057)	(0.0055)	(0.0033)
ITT, 18 and Over		0.0110*	0.0002	-0.0023	-0.0066	-0.0002
		(0.0060)	(0.0063)	(0.0068)	(0.0071)	(0.0039)
P-value, test of diff.		0.734	0.306	0.369	0.185	0.347
CM, Under 18		0.645	0.685	0.601	0.677	0.916
CM, 18 and Over		0.798	0.780	0.735	0.691	0.934
	First Stage					
IV, Under 18	0.4027***	0.0341**	0.0216	0.0143	0.0133	0.0114
	(0.0042)	(0.0136)	(0.0134)	(0.0140)	(0.0138)	(0.0081)
IV, 18 and Over	0.4072***	0.0267*	0.0004	-0.0049	-0.0163	-0.0009
	(0.0055)	(0.0148)	(0.0155)	(0.0166)	(0.0175)	(0.0095)
P-value, test of diff.	0.508	0.712	0.302	0.378	0.184	0.325
CCM, Under 18		0.647	0.695	0.613	0.684	0.917
CCM, 18 and Over		0.783	0.787	0.747	0.719	0.936
		Earnings, Winsorized at 99th Percentile				
ITT, Under 18		45.28	60.70	127.61	147.54	387.72
		(35.72)	(65.56)	(90.20)	(122.61)	(251.03)
ITT, 18 and Over		80.62	184.34	133.07	328.55	826.31
		(100.70)	(161.77)	(213.61)	(279.18)	(622.25)
P-value, test of diff.		0.741	0.478	0.981	0.553	0.513
CM, Under 18		2120	3962	4977	7326	18404
CM, 18 and Over		5979	9329	11542	14438	41501
	First Stage					
IV, Under 18	0.4027***	116.87	150.74	338.71	375.06	984.70
	(0.0042)	(88.61)	(162.85)	(223.49)	(304.75)	(623.29)
IV, 18 and Over	0.4072***	201.90	452.80	328.53	836.23	2003.98
	(0.0055)	(247.08)	(397.48)	(524.70)	(684.86)	(1528.46)
P-value, test of diff.	0.508	0.746	0.482	0.986	0.539	0.537
CCM, Under 18		2243	4180	5060	7311	18813
CCM, 18 and Over		6135	9467	11788	15225	42687

Notes: N = 43,409 (27,500 under 18, 15,909 age 18 and up). Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.10: Employment and Earnings Effects by Pre-SYEP Work Experience Status

		Y1	Y2	Y3	Y4	Cumulative
		Employment				
ITT, Never Worked		0.0068 (0.0068)	0.0052 (0.0067)	0.0005 (0.0068)	0.0037 (0.0067)	0.0028 (0.0043)
ITT, Ever Worked		0.0176*** (0.0050)	0.0064 (0.0051)	0.005 (0.0056)	-0.0009 (0.0058)	0.0033 (0.0028)
P-value, test of diff.		0.201	0.888	0.601	0.602	0.922
CM, Never Worked		0.588	0.634	0.557	0.646	0.892
CM, Ever Worked		0.793	0.790	0.727	0.711	0.947
	First Stage					
IV, Never Worked	0.3950*** (0.0049)	0.0173 (0.0173)	0.0132 (0.0170)	0.0022 (0.0171)	0.0094 (0.0170)	0.0067 (0.0110)
IV, Ever Worked	0.4121*** (0.0045)	0.0428*** (0.0121)	0.0155 (0.0123)	0.0121 (0.0136)	-0.0024 (0.0140)	0.0074 (0.0068)
P-value, test of diff.	0.010	0.226	0.912	0.649	0.591	0.953
CCM, Never Worked		0.601	0.646	0.573	0.652	0.896
CCM, Ever Worked		0.775	0.795	0.735	0.732	0.946
		Earnings, Winsorized at 99th Percentile				
ITT, Never Worked		36.57 (37.83)	6.37 (74.50)	9.42 (101.95)	170.96 (143.66)	267.40 (283.22)
ITT, Ever Worked		69.35 (72.48)	177.93 (115.96)	232.79 (155.12)	253.07 (202.61)	747.62* (448.61)
P-value, test of diff.		0.689	0.213	0.228	0.741	0.365
CM, Never Worked		1745	3461	4459	6869	16547
CM, Ever Worked		4993	7941	9766	12429	35282
	First Stage					
IV, Never Worked	0.3950*** (0.0049)	103.08 (95.48)	34.28 (188.20)	43.97 (257.23)	469.71 (363.49)	671.08 (715.99)
IV, Ever Worked	0.4121*** (0.0045)	170.13 (175.90)	433.03 (281.41)	558.67 (376.46)	600.76 (491.28)	1822.65* (1089.21)
P-value, test of diff.	0.010	0.738	0.239	0.258	0.830	0.377
CCM, Never Worked		1920	3848	4878	7018	17673
CCM, Ever Worked		5123	8000	9721	12850	35810

Notes: N = 43,409 (23,718 with work experience prior to the SYEP summer, 19,691 youth who never worked prior to the SYEP summer). Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.11: Employment and Earnings Effects for Minority and White Youth

		Y1	Y2	Y3	Y4	Cumulative
		Employment				
ITT, Minority		0.0134*** (0.0044)	0.0065 (0.0044)	0.005 (0.0046)	0.0028 (0.0047)	0.0045* (0.0027)
ITT, White		0.0049 (0.0114)	-0.0009 (0.0122)	-0.0098 (0.0128)	-0.0074 (0.0129)	-0.0086 (0.0076)
P-value, test of diff.		0.486	0.566	0.279	0.461	0.102
CM, Minority		0.6929	0.7228	0.6606	0.6901	0.9228
CM, White		0.7514	0.6941	0.5698	0.617	0.9169
	First Stage					
IV, Minority	0.4188*** (0.0036)	0.0321*** (0.0106)	0.0156 (0.0105)	0.0125 (0.0111)	0.0067 (0.0112)	0.0103 (0.0064)
IV, White	0.2973*** (0.0088)	0.0163 (0.0385)	-0.0045 (0.0412)	-0.0327 (0.0433)	-0.0249 (0.0437)	-0.0287 (0.0255)
P-value, test of diff.	0.000	0.691	0.636	0.313	0.485	0.138
CCM, Minority		0.691	0.730	0.665	0.700	0.921
CCM, White		0.752	0.712	0.621	0.646	0.951
		Earnings, Winsorized at 99th Percentile				
ITT, Minority		75.47* (45.72)	138.63* (76.32)	190.64* (101.51)	322.79** (133.81)	773.64*** (289.22)
ITT, White		-55.08 (131.93)	-129.91 (213.57)	-292.07 (296.57)	-351.56 (402.44)	-839.26 (874.60)
P-value, test of diff.		0.350	0.236	0.123	0.112	0.080
CM, Minority		3500	5926	7339	9726	26560
CM, White		3662	5636	7202	10354	27023
	First Stage					
IV, Minority	0.4188*** (0.0036)	185.55* (109.08)	339.82* (182.13)	467.05* (242.24)	786.77** (319.28)	1846.53*** (690.67)
IV, White	0.2973*** (0.0088)	-192.20 (444.91)	-454.73 (720.07)	-977.60 (1001.75)	-1186.32 (1358.32)	-2865.06 (2952.51)
P-value, test of diff.	0.000	0.410	0.285	0.161	0.157	0.120
CCM, Minority		3611	6064	7421	9915	27038
CCM, White		4243	6466	8299	11567	30803

Notes: N = 43,019 (37,653 minority youth including Black, Hispanic, Asian, and Mixed Race/Other, 5,366 White youth). 390 observations are dropped due to missing race/ethnicity. Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.12: Employment and Earnings Effects by Race/Ethnicity, ITT

	Y1	Y2	Y3	Y4	Cumulative	Y1	Y2	Y3	Y4	Cumulative
	Employment					Earnings, Winsorized at 99th Percentile				
ITT, White	0.0049 (0.0114)	-0.0007 (0.0122)	-0.0099 (0.0128)	-0.0073 (0.0129)	-0.0084 (0.0075)	-54.07 (132.00)	-128.06 (213.69)	-291.08 (296.68)	-349.92 (402.37)	-834.28 (874.83)
ITT, Black	0.0078 (0.0064)	0.0044 (0.0063)	0.0011 (0.0067)	-0.0099 (0.0067)	-0.0001 (0.0037)	6.42 (65.54)	74.21 (107.32)	53.49 (140.97)	27.77 (179.37)	198.60 (395.84)
ITT, Hispanic	0.0165** (0.0077)	0.0066 (0.0077)	0.0198** (0.0080)	0.0124 (0.0081)	0.0093** (0.0047)	203.13** (83.64)	213.19 (138.10)	283.35* (171.42)	380.19* (225.46)	1095.38** (498.19)
ITT, Asian	0.0252** (0.0121)	0.0026 (0.0121)	-0.0079 (0.0126)	0.0242* (0.0125)	0.0063 (0.0073)	-2.12 (108.48)	61.98 (200.40)	500.69 (309.80)	1114.00** (441.67)	1811.90** (884.08)
ITT, Other	0.010 (0.0194)	0.0338* (0.0192)	-0.0185 (0.0200)	-0.0074 (0.0204)	0.0043 (0.0120)	103.15 (200.40)	427.22 (319.18)	-42.74 (452.16)	368.94 (589.43)	951.71 (1269.50)
P-value, all equal	0.671	0.64	0.117	0.073	0.296	0.318	0.565	0.321	0.099	0.157
CM, White	0.751	0.694	0.570	0.617	0.917	3662	5636	7202	10354	27023
CM, Black	0.715	0.744	0.676	0.708	0.931	3551	5917	7275	9191	25971
CM, Hispanic	0.686	0.718	0.656	0.684	0.916	3668	6325	7478	9766	27269
CM, Asian	0.643	0.675	0.614	0.650	0.914	2956	5195	7122	11301	26837
CM, Other	0.685	0.704	0.682	0.685	0.916	3512	5577	7630	9817	26577

Notes: N = 5,366 White, N = 17,636 Black, N = 12,427 Hispanic, N = 5,578 Asian, and N = 2,012 Mixed Race/Other. 390 observations dropped due to missing race/ethnicity. Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means. P-value from test of null hypothesis that all treatment effects are equal across groups. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.13: Employment and Earnings Effects by Race/Ethnicity, IV

		Y1	Y2	Y3	Y4	Cumul.	Y1	Y2	Y3	Y4	Cumul.
	First Stage	Employment					Earnings, Winsorized at 99th Percentile				
IV, White	0.2973*** (0.0088)	0.0162 (0.0385)	-0.0039 (0.0412)	-0.0333 (0.0433)	-0.0246 (0.0437)	-0.0283 (0.0255)	-191.62 (445.14)	-453.07 (720.36)	-978.84 (1002.05)	-1183.07 (1358.11)	-2861.66 (2953.12)
IV, Black	0.4039*** (0.0052)	0.0194 (0.0157)	0.0108 (0.0155)	0.0034 (0.0165)	-0.0244 (0.0167)	-0.0002 (0.0092)	24.02 (162.06)	198.01 (265.35)	149.00 (348.52)	91.52 (443.42)	495.10 (979.76)
IV, Hispanic	0.4152*** (0.0062)	0.0396** (0.0186)	0.0157 (0.0185)	0.0475** (0.0193)	0.0299 (0.0196)	0.0224** (0.0114)	486.56** (201.36)	508.28 (332.35)	676.96 (412.47)	907.86* (542.62)	2633.74** (1199.97)
IV, Asian	0.4830*** (0.0094)	0.0521** (0.0250)	0.0055 (0.0250)	-0.0153 (0.0261)	0.0500* (0.0259)	0.0131 (0.0151)	9.06 (224.45)	151.61 (415.00)	1064.39* (642.79)	2344.75** (915.64)	3758.13** (1833.46)
IV, Other	0.3925*** (0.0155)	0.0252 (0.0496)	0.0857* (0.0493)	-0.0457 (0.0511)	-0.0189 (0.0523)	0.011 (0.0308)	271.80 (512.13)	1101.66 (817.49)	-76.36 (1156.50)	982.90 (1509.06)	2391.00 (3251.01)
P-value, all equal	0.000	0.802	0.649	0.133	0.078	0.340	0.350	0.606	0.395	0.134	0.222
CCM, White		0.752	0.711	0.622	0.645	0.951	4242	6465	8300	11564	30799
CCM, Black		0.723	0.764	0.699	0.74	0.938	3736	6296	7765	9704	27520
CCM, Hisp.		0.680	0.722	0.637	0.685	0.91	3681	6357	7439	10001	27458
CCM, Asian		0.629	0.667	0.617	0.626	0.899	3077	5128	6197	10307	24832
CCM, Other		0.692	0.692	0.710	0.704	0.918	3876	5324	8405	9888	27502

Notes: N = 5,366 White, N = 17,636 Black, N = 12,427 Hispanic, N = 5,578 Asian, and N = 2,012 Mixed Race/Other. 390 observations dropped due to missing race/ethnicity. Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CCM shows control complier means. P-value from test of null hypothesis that all treatment effects are equal across groups. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.14: Employment and Earnings Effects by Gender

		Y1	Y2	Y3	Y4	Cumulative
		Employment				
ITT, Male		0.0044 (0.0066)	0.0098 (0.0067)	0.0054 (0.0069)	0.0016 (0.0070)	0.0028 (0.0044)
ITT, Female		0.0190*** (0.0052)	0.0029 (0.0052)	0.0011 (0.0056)	0.0008 (0.0056)	0.0031 (0.0029)
P-value, test of diff.		0.083	0.409	0.629	0.935	0.964
CM, Male		0.658	0.659	0.585	0.615	0.894
CM, Female		0.733	0.766	0.699	0.731	0.943
	First Stage					
IV, Male	0.3962*** (0.0051)	0.0111 (0.0166)	0.0249 (0.0168)	0.014 (0.0173)	0.0039 (0.0177)	0.007 (0.0111)
IV, Female	0.4106*** (0.0044)	0.0462*** (0.0128)	0.007 (0.0127)	0.0032 (0.0136)	0.002 (0.0135)	0.0071 (0.0070)
P-value, test of diff.	0.031	0.094	0.396	0.62	0.931	0.993
CCM, Male		0.675	0.666	0.601	0.633	0.898
CCM, Female		0.713	0.773	0.706	0.742	0.942
		Earnings, Winsorized at 99th Percentile				
ITT, Male		50.05 (62.35)	155.43 (106.86)	155.04 (144.91)	246.22 (194.84)	637.00 (418.06)
ITT, Female		63.87 (59.27)	66.31 (96.98)	108.40 (129.47)	191.24 (170.58)	475.58 (370.00)
P-value, test of diff.		0.872	0.537	0.810	0.832	0.772
CM, Male		2968	4963	6416	8675	23111
CM, Female		3952	6642	8096	10861	29640
	First Stage					
IV, Male	0.3962*** (0.0051)	133.37 (157.18)	401.58 (269.46)	411.13 (365.11)	636.41 (491.50)	1617.41 (1055.15)
IV, Female	0.4106*** (0.0044)	160.27 (144.31)	170.37 (236.12)	272.34 (315.34)	481.08 (415.27)	1155.60 (901.22)
P-value, test of diff.	0.031	0.900	0.519	0.773	0.809	0.739
CCM, Male		3195	5161	6676	8963	24054
CCM, Female		4033	6830	8186	11157	30255

Notes: Notes: N = 43,409 (18,539 male youth, 24,870 female youth). Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.15: Employment and Earnings Effects by Neighborhood: Above/Below Median in Opportunity Insights Upward Mobility Ranking

		Y1	Y2	Y3	Y4	Cumulative
Employment						
ITT, Below Median		0.0144** (0.0058)	0.0098* (0.0057)	0.0079 (0.0060)	0.0016 (0.0061)	0.0036 (0.0035)
ITT, Above Median		0.0112* (0.0058)	0.0018 (0.0059)	-0.0021 (0.0062)	0.0006 (0.0063)	0.0023 (0.0036)
P-value, test of diff.		0.699	0.332	0.25	0.907	0.799
CM, Below Median		0.696	0.729	0.660	0.695	0.924
CM, Above Median		0.706	0.711	0.640	0.669	0.921
First Stage						
IV, Below Median	0.4182*** (0.0047)	0.0345** (0.0138)	0.0235* (0.0137)	0.0192 (0.0144)	0.0036 (0.0146)	0.0079 (0.0083)
IV, Above Median	0.3903*** (0.0047)	0.0288* (0.0150)	0.0047 (0.0152)	-0.0046 (0.0159)	0.0019 (0.0161)	0.0063 (0.0092)
P-value, test of diff.	0.000	0.781	0.358	0.266	0.937	0.896
CCM, Below Median		0.700	0.741	0.672	0.711	0.928
CCM, Above Median		0.693	0.714	0.651	0.681	0.919
Earnings, Winsorized at 99th Percentile						
ITT, Below Median		54.02 (60.66)	151.67 (99.13)	64.28 (127.17)	83.12 (164.10)	371.79 (362.56)
ITT, Above Median		62.04 (61.45)	57.01 (104.33)	194.70 (145.73)	352.70* (197.76)	722.90* (420.14)
P-value, test of diff.		0.926	0.511	0.500	0.294	0.527
CM, Below Median		3587	6006	7239	9270	26141
CM, Above Median		3476	5844	7518	10591	27570
First Stage						
IV, Below Median	0.4182*** (0.0047)	133.43 (144.87)	369.80 (236.69)	161.28 (303.70)	210.11 (391.74)	889.18 (866.46)
IV, Above Median	0.3903*** (0.0047)	167.16 (157.48)	157.30 (267.32)	520.99 (373.30)	927.76* (506.98)	1873.76* (1077.22)
P-value, test of diff.	0.000	0.875	0.552	0.455	0.263	0.476
CCM, Below Median		3774	6289	7762	10028	27882
CCM, Above Median		3581	5957	7321	10457	27397

Notes: N = 43,408 (21,860 below median, 21,548 above median, 1 observation missing zip code). Uses within-sample median of Opportunity Insights “upward mobility” index: the average percentile rank for children whose parents were in the 25th percentile of the national income distribution. We map Census tract-level data onto participant zip code, see text for details. Earnings winsorization recodes each quarter’s highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses.
 * p<0.1, ** p<0.05, *** p<0.01

Table A.16: Letter Information and Application Behavior for Treatment Group by Subgroup

	Has Letter	Average Rating	Applied to Our Job	Submitted Letter
White	0.296	6.09	0.073	0.222
Non-White	0.420	5.66	0.083	0.158
Black	0.404	5.54	0.087	0.167
Hispanic	0.416	5.68	0.071	0.130
Asian	0.483	5.85	0.121	0.200
Male	0.396	5.62	0.077	0.162
Female	0.410	5.75	0.086	0.167
In HS Sample	0.412	5.61	0.082	0.230
Not in HS Sample	0.398	5.77	0.082	0.111
Under 18	0.403	5.64	0.084	0.226
18 and Over	0.407	5.79	0.079	0.052
Above Median in OI Rank	0.390	5.80	0.077	0.167
Below Median in OI Rank	0.418	5.60	0.086	0.163
Never Employed Pre-SYEP	0.395	5.61	0.077	0.214
Ever Employed Pre-SYEP	0.412	5.77	0.086	0.128
High Rating	0.753	6.04	0.093	0.250
Low Rating	0.307	3.92	0.074	0.167

Notes: Means shown for treatment group only, $N = 21,714$ (except for high/low rating, which is limited to those with a rating, $N = 14,400$). Average rating conditional on being sent a letter, $N = 8,780$; application probability conditional on being invited to apply, $N = 2,000$ (1,346 for rating categories); and submission probability conditional on applying, $N = 164$ (116 for rating categories). Median OI Rank is the within-sample median of the Opportunity Insights “upward mobility” percentile rank. All differences in having a letter and in average ratings between two groups (i.e., White/Minority, Male/Female, High School/Not in HS, Under/Over 18, Above/Below median OI rank, Never/Ever Employed Pre-SYEP, and High/Low Ratings) are statistically different except for having a letter between those under and over 18. None of the differences in application or letter submission rates are significantly different except for the high school and age differences in submitting the letter.

Table A.17: Education Descriptive Statistics

	N	Control Mean	Treatment Mean	Test of Difference
		Education Sample		
Age	19714	15.96	15.95	0.357
Male	19714	0.452	0.445	0.344
Black	19656	0.426	0.424	0.854
Hispanic	19656	0.309	0.307	0.821
Asian	19656	0.139	0.137	0.794
White	19656	0.074	0.084	0.009
Grade Level	19714	10.04	10.03	0.344
Share Enrolled Days Present	19714	0.902	0.899	0.169
Missing GPA	19714	0.100	0.101	0.848
GPA (100 point scale)	17732	79.73	79.34	0.033
In College Sample	19714	0.903	0.903	0.998
Not in UI Data	19714	0.008	0.010	0.411
Never Employed Pre-SYEP	19714	0.614	0.621	0.284
Ever Worked, Year -4	19714	0.041	0.040	0.688
Earnings, Year -4	19714	64.93	81.84	0.247
Ever Worked, Year -3	19714	0.134	0.134	0.916
Earnings, Year -3	19714	169.01	180.68	0.497
Ever Worked, Year -2	19714	0.305	0.304	0.889
Earnings, Year -2	19714	411.70	402.85	0.629
Ever Worked, Year -1	19714	0.958	0.960	0.565
Earnings, Year -1	19714	1545.18	1534.16	0.604
Joint F-test	F(37, 19063) = 1.242, p=.149			

Notes: Table shows non-missing summary statistics for the expected in high school sample (see text for details). Test of difference reports the p-value from a regression of each characteristic on a treatment indicator, controlling for a cohort indicator and using standard errors clustered on individual.

Table A.18: Joint Employment and School Attainment Outcomes

Panel A: On-Time Graduation				
	Ever Work, On-time Grad	Never Work, On-time Grad	Ever Work, Not On-time	Never Work, Not On-time
ITT	-0.0044 (0.0050)	-0.0018 (0.0030)	0.0096** (0.0042)	-0.0032 (0.0026)
CM	0.736	0.049	0.177	0.037
Sent Letter (IV)	-0.0101 (0.0120)	-0.0052 (0.0073)	0.0231** (0.0101)	-0.0075 (0.0062)
CCM	0.777	0.050	0.139	0.034
Panel B: Any Graduation				
	Ever Work, Graduated	Never Work, Graduated	Ever Work, Not Graduated	Never Work, Not Graduated
ITT	0.0022 (0.0048)	-0.0028 (0.0031)	0.0029 (0.0039)	-0.002 (0.0025)
CM	0.781	0.053	0.132	0.034
Sent Letter (IV)	0.0052 (0.0117)	-0.0077 (0.0075)	0.0072 (0.0094)	-0.0049 (0.0060)
CCM	0.811	0.054	0.106	0.03
Panel C: Any Graduation or Continued Attendance				
	Ever Work, Persisted	Never Work, Persisted	Ever Work, Not Persisted	Never Work, Not Persisted
ITT	-0.0012 (0.0049)	-0.0021 (0.0032)	0.0064* (0.0038)	-0.0025 (0.0024)
CM	0.795	0.055	0.118	0.031
Sent Letter (IV)	-0.0028 (0.0117)	-0.0063 (0.0077)	0.0154* (0.0092)	-0.0062 (0.0057)
CCM	0.827	0.056	0.089	0.028

Notes: $N = 19,714$. Analysis conducted on the main education sample (non-charter 8th–12th graders in the pre-randomization year, see text for details). First stage for this subsample is 0.413. Panel A shows whether someone ever worked during the 4-year follow up and whether they graduated on-time (i.e., 4th-year graduation). Panel B shows whether someone ever worked during the 4-year follow up and whether they ever graduated (i.e., 4th-, 5th-, or 6th-year graduation). Panel C shows whether someone ever worked during the 4-year follow up and whether they either graduated or had positive days attended in the last year of our data. CCM shows control complier means, which may not total to 1 across categories due to estimation error in the IV and the inclusion of different sets of covariates in the post double-selection LASSO. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.19: Education Effects by GPA

	Ever Enrolled Y1	% Enrolled Days Present Y1	GPA Y1	Credits Attempted Y1-4	% Credits Earned Y1-4	Graduated On-Time	Ever Graduated	Graduated or Still Attending	On-time College
IV, Below	-0.0247*	-0.0083	-0.8497*	0.1626	-0.002	-0.0569**	-0.0175	-0.0232	-0.0329
Median GPA	(0.0148)	(0.0141)	(0.4957)	(0.4681)	(0.0164)	(0.0232)	(0.0216)	(0.0212)	(0.0264)
IV, Above	-0.0016	0.001	-0.3397	-0.0176	-0.0038	-0.001	0.0014	0.0019	0.0004
Median GPA	(0.0049)	(0.0054)	(0.3136)	(0.1984)	(0.0063)	(0.0082)	(0.0074)	(0.0072)	(0.0148)
P, test of diff.	0.141	0.54	0.385	0.724	0.921	0.023	0.407	0.265	0.271
CCM, Below	0.945	0.772	72.984	18.914	0.776	0.753	0.83	0.845	0.543
CCM, Above	0.993	0.937	89.624	18.109	0.976	0.974	0.977	0.978	0.885

Notes: N = 17,732, all in main education sample who are not missing baseline GPA (8,868 above median, 8,864 below). Median GPA cut-off is 80.85. Credits attempted and % credits earned equal 0 for those not in school. On-time graduation equals 1 for public school, non-charter students who graduate within 4 years and do not transfer to GED programs or other districts. Ever graduated adds any 5th- and 6th-year graduation observed during the follow-up period. Graduated or still attending equals 1 if student either graduated or has positive days attended in most recent academic year. College enrollment is only measured within 6 months after a student's on-time graduation date, regardless of graduation status. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are shown in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.20: Employment and Earnings Effects by GPA

		Y1	Y2	Y3	Y4	Cumulative
Panel A: Employment						
	First Stage					
IV, Below Median	0.3704***	0.0642**	0.0409	0.0409	0.0099	0.025
GPA	(0.0072)	(0.0260)	(0.0253)	(0.0260)	(0.0264)	(0.0153)
IV, Above Median	0.4615***	-0.0107	0.0184	-0.0141	0.0261	-0.0033
GPA	(0.0076)	(0.0208)	(0.0205)	(0.0215)	(0.0208)	(0.0118)
P, test of diff.	0.000	0.025	0.49	0.104	0.632	0.143
CCM, Below		0.64	0.705	0.654	0.696	0.919
CCM, Above		0.693	0.696	0.638	0.684	0.939
Panel B: Earnings						
IV, Below Median		-43.66	188.02	544.23	537.75	807.55
GPA		(195.96)	(365.87)	(444.48)	(605.06)	(1348.27)
IV, Above Median		28.90	16.99	238.30	1055.54**	1234.33
GPA		(141.89)	(244.72)	(336.28)	(478.33)	(958.11)
P, test of diff.		0.765	0.699	0.584	0.503	0.797
CCM, Below		2432	4583	5961	7688	21084
CCM, Above		2405	4210	4795	6653	18167
Panel C: Earnings, Winsorized at 99th Percentile						
IV, Below Median		-34.81	317.96	529.69	442.99	1304.21
GPA		(171.32)	(327.38)	(436.42)	(589.36)	(1202.26)
IV, Above Median		51.63	28.74	242.02	965.64**	1240.49
GPA		(129.78)	(237.55)	(328.22)	(460.33)	(929.01)
P, test of diff.		0.687	0.475	0.599	0.486	0.967
CCM, Below		2422	4436	5970	7732	20529
CCM, Above		2367	4198	4775	6700	18118
Panel D: Number of Quarters Worked						
IV, Below Median		0.042	0.132*	0.104	0.025	0.307
GPA		(0.073)	(0.079)	(0.085)	(0.091)	(0.230)
IV, Above Median		0.028	-0.004	0.021	0.054	0.097
GPA		(0.058)	(0.065)	(0.071)	(0.074)	(0.189)
P, test of diff.		0.883	0.183	0.454	0.805	0.481
CCM, Below		1.44	1.65	1.88	1.95	6.92
CCM, Above		1.43	1.71	1.71	1.93	6.78

Notes: N = 17,732, all in main education sample who are not missing baseline GPA (8,868 above median, 8,864 below). Median GPA cut-off is 80.85. Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.21: Joint Employment and School Attainment Outcomes by GPA, IV

Panel A: On-Time Graduation				
	Ever Work, On-time Grad	Never Work, On-time Grad	Ever Work, Not On-time	Never Work, Not On-time
IV, Below Median GPA	-0.0404*	-0.0169*	0.0643***	-0.0094
	(0.0241)	(0.0101)	(0.0231)	(0.0120)
IV, Above Median GPA	-0.0051	0.0048	0.0034	-0.0019
	(0.0136)	(0.0112)	(0.0072)	(0.0042)
P, test of diff.	0.202	0.149	0.012	0.557
CCM, Below	0.706	0.047	0.214	0.035
CCM, Above	0.922	0.052	0.016	0.009
Panel B: Any Graduation				
	Ever Work, Graduated	Never Work, Graduated	Ever Work, Not Graduated	Never Work, Not Graduated
IV, Below Median GPA	0.0058	-0.0229**	0.0191	-0.0023
	(0.0229)	(0.0109)	(0.0206)	(0.0113)
IV, Above Median GPA	-0.0041	0.0049	0.0007	-0.0019
	(0.0132)	(0.0112)	(0.0063)	(0.0042)
P, test of diff.	0.707	0.076	0.394	0.971
CCM, Below	0.773	0.057	0.146	0.025
CCM, Above	0.926	0.052	0.014	0.009
Panel C: Any Graduation or Continued Attendance				
	Ever Work, Persisted	Never Work, Persisted	Ever Work, Not Persisted	Never Work, Not Persisted
IV, Below Median GPA	-0.0009	-0.0220*	0.0256	-0.0033
	(0.0227)	(0.0112)	(0.0203)	(0.0110)
IV, Above Median GPA	-0.0051	0.0065	0.0012	-0.0036
	(0.0131)	(0.0112)	(0.0060)	(0.0041)
P, test of diff.	0.872	0.074	0.251	0.979
CCM, Below	0.786	0.058	0.133	0.023
CCM, Above	0.928	0.051	0.012	0.01

Notes: N=17,732, all in main education sample who are not missing baseline GPA (8,868 above median, 8,864 below). Median GPA = 80.85. First stage for below median GPA = 0.370, for above median GPA = 0.462. Panel A shows whether someone ever worked during the 4-year follow up and whether they graduated on-time (i.e., 4th-year graduation). Panel B shows whether someone ever worked during the 4-year follow up and whether they ever graduated (i.e., 4th-, 5th-, or 6th-year graduation). Panel C shows whether someone ever worked during the 4-year follow up and whether they either graduated or had positive days attended in the last year of our data. CCM shows control complier means, which may not total to 1 across categories due to estimation error in the IV and the inclusion of different sets of covariates in the post double-selection LASSO. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.22: Labor Market Effects, Alternative Covariates

Year	1	2	3	4	Cumulative
Panel A: No Covariates					
Employment					
ITT	0.0117*** (0.0044)	0.0049 (0.0043)	0.0026 (0.0045)	0.0009 (0.0045)	0.0025 (0.0026)
CM	0.701	0.72	0.65	0.682	0.922
Sent Letter (IV)	0.0289*** (0.0108)	0.0122 (0.0106)	0.0065 (0.0113)	0.0023 (0.0110)	0.0062 (0.0063)
CCM	0.7	0.73	0.663	0.697	0.925
Earnings, Winsorized at 99th Percentile					
ITT	44.12 (51.84)	100.2 (81.53)	138.52 (106.39)	243.75* (138.57)	547.88* (320.38)
CM	3532	5925	7378	9927	26852
Sent Letter (IV)	109.11 (128.19)	247.81 (201.58)	342.56 (263.07)	602.82* (342.60)	1354.95* (792.06)
CCM	3722	6151	7542	10183	27655
Panel B: All Covariates					
Employment					
ITT	0.0125*** (0.0041)	0.0058 (0.0041)	0.003 (0.0043)	0.0012 (0.0044)	0.0028 (0.0025)
CM	0.701	0.720	0.65	0.682	0.922
Sent Letter (IV)	0.0309*** (0.0101)	0.0144 (0.0102)	0.0074 (0.0107)	0.0029 (0.0108)	0.0069 (0.0062)
CCM	0.698	0.728	0.662	0.696	0.924
Earnings, Winsorized at 99th Percentile					
ITT	53.04 (43.02)	103.78 (71.82)	131.62 (96.34)	219.40* (128.09)	528.91* (276.64)
CM	3532	5925	7378	9927	26852
Sent Letter (IV)	131.15 (106.32)	256.62 (177.53)	325.45 (238.17)	542.50* (316.64)	1307.82* (683.88)
CCM	3700	6143	7559	10243	27702

Notes: N = 43,409. Panel A shows results with no covariates other than cohort indicator. Panel B uses all available covariates (see text) rather than post-double-selection LASSO-selected covariates that are used in the main results. Winsorization in Panel B recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.23: Education Effects, Alternative Covariates

	Ever Enrolled Y1	% Enrolled Days Present	GPA Y1	Credits Attempted Y1-4	% Credits Earned Y1-4	Graduated On-Time	Ever Graduated	Graduated or Still Attending	On-time College
Panel A: No Covariates									
ITT	-0.003 (0.003)	-0.004 (0.004)	-0.463** (0.184)	0.187 (0.144)	-0.002 (0.004)	-0.014** (0.006)	-0.006 (0.005)	-0.009* (0.005)	-0.015** (0.007)
CM	0.946	0.829	80.128	18.958	0.818	0.785	0.834	0.851	0.672
Sent Letter (IV)	-0.008 (0.008)	-0.009 (0.009)	-1.113** (0.445)	0.452 (0.350)	-0.006 (0.011)	-0.034** (0.014)	-0.015 (0.013)	-0.022* (0.012)	-0.035** (0.017)
CCM	0.961	0.861	82.556	18.233	0.856	0.846	0.877	0.895	0.742
Panel B: All Covariates									
ITT	-0.002 (0.003)	0.001 (0.003)	-0.135 (0.098)	0.076 (0.100)	0.002 (0.003)	-0.007* (0.004)	-0.001 (0.004)	-0.004 (0.004)	-0.005 (0.006)
CM	0.946	0.829	80.128	18.958	0.818	0.785	0.834	0.851	0.672
Sent Letter (IV)	-0.004 (0.007)	0.004 (0.007)	-0.324 (0.235)	0.183 (0.241)	0.006 (0.007)	-0.017* (0.010)	-0.002 (0.010)	-0.01 (0.010)	-0.011 (0.013)
CCM	0.957	0.848	81.767	18.502	0.844	0.828	0.864	0.883	0.718
N	19714	19714	18237	19714	19714	19714	19714	19714	17810

Notes: Analysis conducted on main education sample. Panel A shows results with no covariates other than cohort indicator. Panel B uses all available covariates (see text) rather than post-double-selection LASSO-selected covariates that are used in the main results. Credits attempted and earned equal 0 for those not in school. On-time graduation equals 1 for public school, non-charter students who graduate within 4 years and do not transfer to GED programs or other districts. Ever graduated adds any 5th- and 6th-year graduation observed during the follow-up period. Graduated or still attending equals 1 if student either graduated or has positive days attended in most recent academic year. College enrollment is only measured within 6 months after a student's on-time graduation date, regardless of graduation status. CM shows control means; CCM shows control complier means. Standard errors clustered on individual are shown in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.24: Baseline Characteristics, Unopened Surveys versus Main Control Group

	Unopened Surveys	Control	Test of Difference
N	25813	21695	
Age	17.24	17.17	0.002
Male	0.427	0.427	0.894
Black	0.437	0.409	0.000
Hispanic	0.246	0.289	0.000
Asian	0.082	0.129	0.000
White	0.188	0.124	0.000
Other Race	0.047	0.049	0.746
In High School	0.746	0.755	0.014
HS Graduate	0.050	0.044	0.003
In College	0.174	0.173	0.677
Not in UI Data	0.008	0.006	0.078
Never Employed Pre-SYEP	0.429	0.450	0.000
Ever Worked, Year -4	0.170	0.153	0.000
Earnings, Year -4	322	303	0.203
Ever Worked, Year -3	0.293	0.266	0.000
Earnings, Year -3	609	574	0.091
Ever Worked, Year -2	0.459	0.437	0.000
Earnings, Year -2	1093	1052	0.146
Ever Worked, Year -1	0.962	0.965	0.062
Earnings, Year -1	2331	2334	0.719
No Education Match	0.185	0.126	0.000
In HS Sample	0.409	0.454	0.000
Joint F-test	F(24, 45597) = 35.492, p=0		

Notes: Table tests difference of means between all youth in unopened surveys (excluded from our main sample) and our control group (on an opened survey). Test of difference controls for cohort indicator and uses cluster-robust standard errors. 496 youth are missing race/ethnicity.

Table A.25: Outcomes, Unopened Surveys versus Main Control Group

	Unopened Surveys	Control	Test of Difference
Panel A: Labor Market Outcomes			
N	25813	21695	
Employment Y1	0.715	0.701	0.000
Employment Y2	0.715	0.720	0.278
Employment Y3	0.640	0.650	0.143
Employment Y4	0.666	0.682	0.000
Employment Cumulative	0.919	0.922	0.210
Earnings Y1	3617	3532	0.135
Earnings Y2	6157	5925	0.006
Earnings Y3	7561	7378	0.032
Earnings Y4	10050	9927	0.332
Earnings Cumulative	27500	26852	0.031
Joint F-test, Employment Outcomes	$F(8, 45597) = 6.482, p=0$		
Panel B: Education Outcomes			
N	10564	9857	
Enrolled Y1	0.934	0.946	0.000
Perc. Days Present Y1	0.808	0.829	0.000
GPA Y1	79.03	80.13	0.000
Credit Attempted Y1-4	18.68	18.96	0.054
Perc. Credits Earned Y1-4	0.795	0.817	0.000
Graduated On-time	0.751	0.785	0.000
Ever Graduated	0.801	0.834	0.000
Graduated or Still Attending	0.817	0.851	0.000
On-time College	0.627	0.666	0.000
On-time College	0.635	0.672	0.000
Joint F-test, Education Outcomes	$F(6, 18093) = 10.185, p=0$		
Panel C: Job Application Outcomes			
N	636	2000	
Clicked Link	0.090	0.104	0.294
Started Application	0.075	0.089	0.288
Uploaded Any File	0.047	0.053	0.586
Included Letter of Rec	0.003	0.004	0.745
Checked Selective Box	0.039	0.053	0.137
Joint F-test, Job App Outcomes	$F(5, 2630) = .822, p=.534$		

Notes: Table tests difference of means between all youth in unopened surveys (excluded from our main sample) and our control group (on an opened survey), separately for employment outcomes and subset of youth in education sample. $N = 18,396$ for college test. To avoid using the smallest available sample and highly correlated outcomes for joint F-test, the labor market test excludes cumulative outcomes and the education joint test includes 5 high school outcomes and the indicator for graduating or still attending. Test of difference controls for cohort indicator and uses cluster-robust standard errors.

Table A.26: Employment and Earnings Effects, On Any Survey

Year	1	2	3	4	Cumulative
Panel A: Employment					
ITT	0.0063*	0.0033	0.0018	0.0018	-0.0003
	(0.0032)	(0.0033)	(0.0034)	(0.0035)	(0.0020)
CM	0.707	0.719	0.647	0.675	0.922
Sent Letter (IV)	0.0246*	0.0131	0.0081	0.0071	-0.0015
	(0.0128)	(0.0129)	(0.0135)	(0.0137)	(0.0079)
CCM	0.704	0.73	0.662	0.692	0.932
Panel B: Earnings					
ITT	25.24	65.20	122.14	152.78	362.76
	(39.80)	(62.78)	(82.37)	(110.42)	(240.23)
CM	3635	6102	7529	10124	27408
Sent Letter (IV)	111.34	256.36	505.73	626.43	1426.84
	(156.53)	(247.02)	(324.06)	(434.43)	(945.49)
CCM	3796	6232	7472	10301	27874
Panel C: Earnings, Winsorized at 99th Percentile					
ITT	15.67	58.78	113.44	174.43*	373.51*
	(34.50)	(58.03)	(77.50)	(102.44)	(222.77)
CM	3574	6017	7430	9954	27075
Sent Letter (IV)	63.01	231.26	447.05	681.39*	1470.27*
	(135.76)	(228.33)	(305.00)	(403.16)	(876.71)
CCM	3768	6168	7438	10104	27539

Notes: N = 69,222. Sample includes all youth on any survey, regardless of whether any supervisor opened the survey. Earnings winsorization recodes each quarter's highest earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01