# ONLINE APPENDIX

Information Frictions and Skill Signaling in the Youth Labor Market

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## A. Further Details on Supervisor Survey

In determining which youth to assign to which supervisor surveys, DYCD's work-site data did not provide a one-to-one match. Sometimes, multiple supervisors were listed for a single work site, such that it was not clear which youth reported to which supervisor or if a youth reported to multiple supervisors; in these cases, we assumed the latter for the purposes of constructing our survey tool. Consequently, youth could be listed on more than one survey. If more than one supervisor rated a young person, we generated the letter from the survey with the highest rating, breaking ties by prioritizing letters that included employer contact information, and then those with the most positive responses about the youth. Sometimes, a single supervisor was listed for multiple work sites. If the names of the work sites suggested they might be connected (e.g., multiple branches of the same store), we treated them as one work site for the purposes of constructing the survey tool.

As noted in the main text, we limited the number of youth on each survey to keep the survey length manageable. In particular, if any supervisor was linked to more than 30 treatment youth, then we randomly selected 30 treatment youth to be included in the survey. The same restriction applied to controls. To ensure that neither the treatment nor control group exceeded the 30-person-per-survey limit, we randomly assigned treatment and control status prior to making these sample restrictions. Since youth were randomly selected to be excluded, random assignment is still only a function of random variables. For the 2016 cohort, we emailed 3,297 supervisors at the end of September (initial emails went out on 09/29/16). For the 2017 cohort, we emailed 11,877 supervisors in October (initial emails went out on 10/12/17).

In the survey, we asked supervisors to confirm the youth who worked for them and to provide the names of others who might have supervised youth so we could include them in the letter of recommendation program as well. Our main sample in the text includes all SYEP participants who appeared on at least one survey in which the supervisor clicked the link inviting them to take the survey and confirmed on the first page of the survey—prior to viewing which youth were on the survey or what their treatment status was—that they supervised youth that summer. This excludes the 25,813 youth who were randomized and placed on a survey that no supervisor ever opened.

# B. Further Details on Job Task

The job task was described as being with a professor at the University of Pennsylvania who was looking for former NYC summer job participants for a short-term and flexible job. The job description highlighted several qualifications: "responsible," "self-motivated," having an "enthusiastic approach," and offered compensation of $15/hour. A link to an application with a deadline to submit was included at the bottom of the job description. In addition to the 4,000 treatment and control invitees in our main sample, we also invited 1,000 youth from unopened surveys (i.e., outside our main sample) to ensure that job application behavior was not dramatically different for the youth excluded from our main sample.

All those who submitted an application that included their name, email address, and at least 1 additional field were hired. To ensure our hiring for the more selective job was incentive compatible with our instructions about higher selectivity, the youth needed to click the box asking to be considered and needed to complete one or more of the free-response questions in addition to fulfilling the requirements for the standard job.

The job itself was an online survey of multiple-choice questions. These questions asked youth about their experiences job-seeking and considering college, as well as about their career and education goals. At the end of the survey, there were free-response questions about the youth's experience in SYEP. Youth hired for the more selective job were asked additional free-response questions that required more thoughtful consideration. Workers were instructed to finish everything they could within a two-hour time frame. All youth who initiated the job-task (n=227) were paid for two hours of work via a mailed, pre-loaded debit card (so our job does not appear in the administrative data on employment and earnings).

# C. Details on Data Matching and Availability

## 1. *Labor Market Data*

We obtained earnings and employment data from the New York State Department of Labor (NYSDOL). Data come from NYSDOL's quarterly Unemployment Insurance (UI) dataset, which covers formal sector employment, excluding self-employment or farming income. The data include employer name, FEIN, address, NAICS, and amount paid to each worker in each quarter. NYSDOL analysts matched SYEP participants to UI data using social security number. When multiple profiles in the NYSDOL data shared the same social security number, we used name to disambiguate the UI data. In total, 99.3 percent of SYEP youth in our letter of recommendation experiment were matched to the NYSDOL data with no difference between treatment and control youth ($\beta = 0.001, p = 0.209$).

In theory, everyone in our data should have matched to the data, since they were all listed as a SYEP participant during the summer prior to the program. Some of the non-workers may not have matched to the UI data despite having worked due to typographical mistakes or incorrect SSNs. Others may not have ever been paid by SYEP despite being listed as a participant in their data, and so not actually have received any wages to be reported to the UI system. We assume anyone not appearing in the UI data had no employment and zero earnings.

### *2. Education Data*

Education data come from the NYC Department of Education (DOE). The DOE used name, date of birth, and gender to perform a probabilistic match between our study sample and their records between the 2015–2016 and 2020–2021 school years, inclusive. SYEP applicants fail to match because they never appear in the DOE system (e.g., always attended private school), matched to more than one student record (DOE treats multiple matches on the same name and birth date as a non-match), or because typographical errors or name changes prevented identifying a study participant's education records.

Overall, 88 percent of our sample matched to a DOE student record, with no treatment-control difference in match rates ($\beta = -0.003, p = 0.359$). Within the sample that matched to a DOE student record, 7,642 had no active enrollment within our 2015–2021 data. These students were largely old enough to have left school prior to 2015 (their average age at randomization is 19.7), although some may have transferred to private or non-NYC districts prior to the start of our data. This leaves 69.9% of our sample with at least some education information in the data, with no treatment-control difference ($\beta = -0.003, p = 0.442$). At the request of the data provider, when we merge DOE data with the rest of our study data, we exclude the self-reported citizenship status that appears on the SYEP application, so that education outcomes are never linked to citizenship status. SYEP application data also provides spotty information on whether youth live in public housing or are on public assistance; those fields are also never linked to DOE data.

Our definition of the main "expected in high school" education sample excludes students outside of the DOE, pre-randomization dropouts and graduates, and students who temporarily stopped attending public school or had not yet joined the school district in the year before randomization.

### *3. Graduation and Post-Secondary Data Availability*

As discussed in the main text, graduation and post-secondary outcomes are not universally available. Per state standards, DOE only reports graduation in the academic years that correspond to a student's on-time (4th), 5th, or 6th year graduation cohort, even if a student returns to school after their 6th year. Graduation data are missing for students who transfer to a charter school; who move out of district; who fall under another exclusion, such as having an individualized education plan

(IEP); or who were not in a 4th–6th year graduating cohort between fall 2015 and summer 2021.

Appendix Figure A.7 diagrams the available graduation data by grade and study cohort. About 6 percent of students in our education sample are too young to have 5-year graduation recorded, and 25 percent are missing 6-year graduation. These students will have 0s for "ever graduated," although they may still graduate in the future. Additionally, some students may take longer than 6 years to graduate, which (per state standards) is not captured in DOE data. To fill in available information about whether younger and older students are still engaged in school, we use our "school persistence" indicator. Note that the graduating cohort in DOE data is defined by the official 9th grade cohort to which a student belongs per state standards. We do not directly observe which graduation cohort students are in if they are not in our graduation records, so our education sample is defined based on pre-randomization grade rather than official graduating cohort. This means that students who transferred to other districts during the outcome period will remain in our data.

There are 865 youth in our education sample who do not appear in the graduation data, likely because they transferred out of the district or joined a different group excluded from state graduation counts after randomization. Since these individuals did not receive a diploma from NYC DOE, we assign them zeros for graduation. As mentioned in the main text, DOE discharge codes suggest there is no treatment effect on whether students transfer out of the district ($\beta = 0.003$, $p = 0.260$, with a control mean of 0.032). Since we do not observe graduation outside the district, the balance on transfers helps to rule out the possibility of differential mobility biasing the graduation results.

## D. Additional Labor Market Results

### 1. *Earnings Distribution*

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 cannot 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 contributes to 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 largest density of data more visible. 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.

Because of the scale of the earnings distribution relative to the changes in earnings, however, it is difficult to eyeball the pdfs to assess how different the distributions really are (especially at the top end where there is less data). Further detail on the change in earnings distribution comes from quantile regression results shown in Table A.3. Although quantile regression relies on assumptions that may not hold (e.g., no rank-switching), it can at least provide some additional statistical exploration into where letters are shifting the earnings distribution, since the scale of the pdfs makes it difficult to see the changes. The table reports ITT quantile regression results controlling only for the cohort indicator (necessary for treatment to be random) to ensure convergence.

We see a significant increase in percentile 15, which for controls is those earning between \$909 and \$2,024. The treatment quantile is shifted up by \$146, or 7.2 percent ($p = 0.008$). To give a sense for an appropriate scaling factor, although quantile regressions do not scale into LATEs in the same way as OLS, the table also reports the proportion of treatment youth who were sent a letter in this part of the distribution (as defined by control cutoffs): 35.6 percent. The other significant shift in earnings is at the high end of the distribution; there are significant increases at the 85th and 90th percentiles of 2.6 and 3 percent from control earnings of \$54,000-67,000 ($p = 0.071$ and 0.032 respectively), with about 44 percent of treatment youth being sent a letter.

Given the number of hypothesis tests in the table, we may not want to put too much stock in any single quantile result. But the basic pattern—proportionally large increases at the bottom and smaller but substantively important increases at the top—is useful for thinking about what the letters are doing. Given the different effects we see for low-rated and high-rated youth in the main paper, and the results described below, it seems possible that this shift in distributions is driven by low-earning youth moving away from 0 earnings, possibly by gaining work at DYCD (see below). High-earning youth, meanwhile, seem less likely to be moving off 0. But we still see an increase in the higher end of earnings, which is consistent with higher-earning youth working in better jobs for longer spells.

## *2. Earnings Robustness*

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(earnings + 0.1, 1, 10, 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, earnings 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, unsurprisingly, sensitive to how we handle the 0s.

Table A.2 shifts attention away from the 0s by reporting earnings conditional on working. The pattern of results is very similar to the unconditional results: a point estimate that grows over time and is statistically significant in year 4 (IV=$709, a 4.8 percent increase) and cumulatively (IV=$1,362, a 4.6 percent increase). The fact that the point estimates grow over time in both levels and proportionally, even conditional on working, is suggestive that the letters do not simply speed up learning but rather improve match quality. Unless earnings trajectories are convex, giving treatment youth a faster start on the same trajectory would lead to earnings gaps that stay stable or close over time. Growth in treatment effects over time, especially given the control group's linear wage growth in practice, is not consistent with a "faster employer learning" story. Rather, it seems likely to indicate that letters set youth on steeper earnings trajectories, reflecting better jobs or better matches.

### *3. 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.4 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, despite treatment youth finding jobs faster. 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.

## *4. Employer Type*

Tables A.5 and A.6 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.5 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.6 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, but results are only significant in year 1 for cluster 2, with earnings increases concentrated in cluster 2 jobs as well. 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.

## E. 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.7, 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.8 shows 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.9 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.

As mentioned in the main text, we do one additional exercise to assess whether any remaining bias from sample selection into ratings is driving the observed rating heterogeneity. We use the non-missing ratings in the control group to predict rating based on observables for everyone, and then assess heterogeneity by predicted, rather than actual, rating. In particular, we regress the actual (non-missing) rating on all our baseline covariates for the control group, regardless of whether the survey was complete or not ($N = 15,487$). Observables are significant predictors of ratings, as the prediction regression has an F-statistic of 44.66 ($p < 0.001$). But consistent with the argument in the paper that ratings also capture unobservable attributes, the observables isolate a relatively small part of the variation in ratings (adjusted $R^2 = 0.07$).

Because predicted ratings are more condensed than actual ratings, we do not apply our same 1–4 and 5–7 classification. Rather, we preserve the proportion of youth who are highly rated in the control distribution (67.3 percent), and assign the top 67.3 percent of the predicted ratings distribution to have a "high" predicted rating. We then run our labor market regressions using predicted rating rather than actual rating. Note that this procedure introduces a huge amount of measurement error into ratings given the moderate correlation (0.27) between actual and predicted ratings. Nonetheless, it ensures that there can be no differential selection into ratings across control and treatment groups; everyone with the same baseline observables is assigned the same rating.

The results are in Table A.10. While the ITT point estimates are about half as big as our main results, both the IV estimates and the overall patterns are quite similar (though the IV is a bit awkward to interpret, since we do not change who was sent a letter despite changing the underlying ratings). Low-rated youth still show a significant ITT increase in employment in year 1 but no other significant changes. High-rated youth still show significant ITT employment and earnings increases both in year 1 and cumulatively. We have somewhat less precision to differentiate the low-ratings and high-ratings groups, likely due to the introduction of so much measurement error. But the concentration of lasting results in the highly-rated group persists, which leads us to conclude that differential sample selection is unlikely to be driving the ratings heterogeneity we document in the main text.

Finally, we have tested whether treatment effects on applying to our job posting are different for those with (actual) high versus low ratings. 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.

# F. Heterogeneity

Tables A.11 through A.18 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. 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).

## *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 youth 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.19 (see discussion in Section F.3). 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 these patterns.

### *2. Heterogeneity by Subgroups*

Table A.11 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.12 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.13 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.14 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.15 and A.16 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.16), but even among compliers, the program impacts are larger for Asian, Hispanic, and Other youth. However, given the smaller size of each group, we are under-powered to detect group differences; we cannot reject the null that effects are the same across all groups.

Table A.17 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.18 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.18 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.

### *3. Information on Letters by Subgroup*

To help interpret the patterns of results by subgroup, Table A.19 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.

## G. Additional Education Results

### 1. *Explanation of Deviation from Pre-analysis Plan*

As mentioned in the main text, we wrote our pre-analysis plan before we knew what education data would be available or the details of DOE coverage. So our education results are where we deviate most from our pre-analysis plan. We initially expected to use an index that included days present, an indicator for graduating or still being in school, GPA, and standardized test scores when available, plus a separate outcome measuring post-secondary enrollment. In practice, many elements of this index are missing for multiple reasons. Many students are not in school to have attendance, or they attend a school (including charters) where DOE does not share records; we do not have standardized test scores in the data (except for the selected group that takes Regents exams); and DOE measures graduation and college enrollment only for particular cohorts at particular times. Consequently, instead of forcing different patterns of missing outcomes into a single index, we instead present results separately for the outcomes we have.

### 2. *Descriptive Statistics*

Table A.20 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 H). However, none of our substantive conclusions are sensitive to covariate choice.

### 3. *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.21 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.

## *4. 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.22 shows the main education outcomes by GPA, focusing on the IV to conserve space, and Table A.23 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.22 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.23 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.24 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).

## H. Robustness to Different Covariate Choices

The main text uses the post-double selection LASSO (Belloni, Chernozhukov and Hansen, 2014*a*,*b*; 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 (a special application-based program within the broader SYEP); 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.

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.25 and A.26 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 G.2, the point estimates on GPA and graduation measures become somewhat larger and more significant in specifications without covariate controls.

# I. 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.27 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.27 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.28 shows the same comparison but for outcome measures rather than baseline 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. It is difficult to say from the observable differences in youth across the opened and unopened surveys whether effects would be bigger or smaller if supervisors were forced to fill out the surveys. The unopened surveys contained more White youth, for whom we observe smaller labor market effects. But they also had more youth already out of high school, which could diminish graduation crowd-out, and more youth with work experience prior to SYEP, who have directionally larger point estimates on employment and earnings, see Appendix Section F.

Table A.29 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.

## J. Details on Forest Plot

Figure 5 in the main text compares the magnitudes of our key results—at various time horizons—with results from related studies. This section summarizes our process for selecting and standardizing estimates to allow for reasonable, if imperfect, comparisons across settings. An overview is shown in Table A.30, which is discussed further below.

### *1. Estimate Selection*

We aim to include studies that are closest to the effects we estimate: those that isolate the provision of information on job applications in the labor market. This excludes studies where other factors are varied in addition to the provision of information. For example, Autor et al. (2006) and Autor (2017) analyze the effect of temporary help agencies on labor market outcomes. Temp agencies provide a signal of worker skill, but workers also gain on-the-job training, which could impact labor market outcomes. It also excludes studies where information about job search strategies or available opportunities is provided to workers (e.g., Belot, Kircher and Muller, 2019; Groh et al., 2015). Lastly, we exclude studies where applicant information comes in a form that involves additional intermediaries with their own motivations or additional screening before applications are submitted, such as employee referrals, employer-offered screening tests, and outsourcing agencies (e.g., Hoffman, Kahn and Li, 2018; Pallais and Sands, 2016; Stanton and Thomas, 2016). Note that Bassi and Nansamba (2021) is a bit of an edge case: they provide a skill certificate as well as applicant-firm matching. But the matching is random and occurs for both treatment and controls, so the core variation is from the skill certificate.

We do not include audit studies that vary information about race or ethnicity on job applications, in the vein of Bertrand and Mullainathan (2004), since that literature is focused on discrimination and not just skill signals. We do, however, use estimates that leverage variation in whether other characteristics (criminal or credit histories) are allowed to be included in various parts of the application process (Agan and Starr, 2018; Bartik and Nelson, forthcoming; Doleac, 2016), since those estimates are more about how employers use information rather than how they discriminate against racial and ethnic groups. We exclude Kaas and Manger (2012), since the focus of that paper is on varying German versus Turkish names in applications. A subgroup analysis in that paper does show that providing additional information in the form of recommendation letters may reduce discrimination. But because the kinds of applications that included letters also varied in a number of other ways, the test does not isolate the effect of information. As a result, we do not include it in our summary table.

## *2. Details on Calculations*

We want to focus on the effect of information for those who have it, and compliance—who actually receives the information—is often a function of study design. As a result, Figure 5 reports local average treatment effects (LATEs) rather than intent-to-treat effects (ITTs) wherever possible. Abel et al. (2020) and Heller and Kessler (2023) report LATE directly. Abebe et al. (2021), Bassi and Nansamba (2021), and Pallais (2014) report the ITT and the compliance rate. For these estimates, we back out the implied LATE by dividing the ITT by the compliance rate. For Bassi and Nansamba (2021), we use the compliance rate for the treatment group, which is reported as receiving a certificate and showing up to the matched interview.

Our focus on the LATE in each setting helps to adjust for differences in study design that affect take-up (e.g., whether experiments offer skill signals directly to employers or leave room for the treatment group not to participate or receive/use the signal). But using the IV does not make the estimates exactly comparable across studies. As noted in the main text, the LATEs do not necessarily capture the same parameter for the same compliers. For example, in the studies where researchers provide the skill signal directly to employers for everyone who shows up to a training or interview, take-up always involves receiving and using the signal.

In our setting, however, take-up is defined by supervisor behavior: compliers are everyone whose supervisor started a survey, agreed to provide a letter, and rated youth highly enough to send them a letter. Since not everyone who is sent a letter actually receives it or uses it in a job application, our LATE measures the effect of being sent a skill signal in this way, not the effect of receiving or using one. As we discuss in the text, this likely understates the effect of actually using the letter. This difference in what each LATE represents may be another reason that our estimates are suggestively smaller than those in developing economies, where the LATEs more often estimate the effect of using a skill signal.

For all estimates, we calculate 95% confidence interval bounds by multiplying the reported standard error by 1.96 and subtracting (adding) this value from (to) the relevant estimate. For papers reporting an ITT and compliance rate, we then divide the confidence interval bounds by the compliance rate. For the studies that involve state-wide policy changes in available information (Agan and Starr, 2018; Bartik and Nelson, forthcoming; Doleac and Hansen, 2020), everyone is officially treated by the ban. Individual-level non-compliance (i.e., employers asking for information they should not) is typically unobserved. So in those cases, we report the ITTs from the paper.

Because the baseline levels of outcomes are so different across contexts, we aim to make changes more comparable by focusing on percent changes (treatment effect or confidence interval bounds divided by the control group mean). Note that since the other studies in Figure 5 report control group means rather than control complier means, we scale the estimates in Heller and Kessler (2023) by the control group mean as well. As a result, estimates are shown in terms of percent change relative to the control mean. The resulting percent change can be different than those in the main text, and may somewhat misrepresent the percent change for compliers to the extent CCMs are different from CMs. But since few other papers report CCMs, we use the CM for consistency.

### *3. Details on Outcomes in Each Study*

Table A.30 provides additional details on each estimate used in Figure 5. The first column notes that despite some similarities in intent, not all studies examine exactly the same treatment. The top panel of Figure 5 summarizes the effects of different worker skill signals, including skill certifications (Abebe et al., 2021; Bassi and Nansamba, 2021; Carranza et al., 2022), performance evaluations (Pallais, 2014), and ratings forms filled out by former supervisors (Abel, Burger and Piraino, 2020). The bottom panel of Figure 5 summarizes the effects of employers having other information about workers from marginalized groups, including their criminal histories (Agan and Starr, 2018; Doleac and Hansen, 2020) and credit histories (Bartik and Nelson, forthcoming). These effects are identified using variation in state-wide bans that prevent asking for this information at various points in the hiring process.

Figure 5 groups outcomes into four main categories: callback rates, employment, earnings, and graduation. Column 2 of Table A.30 highlights that the outcomes are measured differently across studies. Callback rates are defined as the fraction of applications that receive an interview request or a request for more information (Abel, Burger and Piraino, 2020; Agan and Starr, 2018; Carranza et al., 2022). Employment measures are more variable across studies. They measure the fraction of individuals who: obtained work in the online marketplace Upwork, formerly known as oDesk, over the sample period (Pallais, 2014); obtained any formal sector job in the state over the sample period (Heller and Kessler 2023); were currently in paid- or self-employment at the time of being surveyed (Abel, Burger and Piraino, 2020); were in paid- or self-employment in the week prior to being surveyed (Abebe et al., 2021; Carranza et al., 2022; Doleac and Hansen, 2020); did any paid work in the month prior to being surveyed (Bassi and Nansamba, 2021); or found a job out of unemployment during the sample period (Bartik and Nelson, forthcoming).

Earnings estimates also capture somewhat different measures of earnings across studies. The estimates count earnings from oDesk over the sample period (Pallais, 2014), earnings from all formal sector work over the sample period winsorized at the 99$^{\text{th}}$ percentile (Heller and Kessler 2023), earnings in the week prior to being surveyed (Abebe et al., 2021; Carranza et al., 2022), monthly earnings from respondent's main occupation over the sample period (Abebe et al., 2021), and total earnings in the month prior to being surveyed (Bassi and Nansamba, 2021). The reported 12-month earnings estimates from Abebe et al. (2021) include earnings in the week prior to being surveyed. The 48-month estimates include monthly earnings from the respondent's main occupation. Estimates for Bassi and Nansamba are based on pooled data across 12- and 26-month follow-ups. To make the time pattern in our study more comparable to these other estimates for both employment and earnings, we include some additional estimates in Figure 5 beyond what is reported in the paper's tables (i.e., we include cumulative 3-month and 24-month estimates).

Not all studies report earnings conditional on working. So to improve comparability, all earnings outcomes reported in Figure 5 are unconditional on working. Several papers also provide conditional earnings estimates. Bassi and Nansamba (2021) find that total earnings in the month prior to being surveyed increased by 23% (LATE) for those employed. This effect is larger and more precisely estimated than the unconditional effect included in the figure. Abebe et al. (2021) and Carranza et al. (2022) estimate smaller conditional earnings effects relative to unconditional: a 45% (LATE) and 19% increase in earnings, respectively.

Heller and Kessler (2023) report two high school graduation outcomes—an indicator for whether someone graduated on time (within 4 years, first row) and an indicator for whether someone ever graduated from high school in the observed data (second row). See the description of these outcomes in the main text and appendix for further details on the coverage of these outcomes.

For the papers studying more general state policies that ban information on job applications, the outcomes are somewhat different. These studies estimate a harmful effect of *removing* information about Black applicants' criminal and credit histories on their labor market outcomes, likely driven by statistical discrimination in the absence of information. Since the estimates in the top panel of Figure 5 report the positive effects of *providing* information about applicants, we flip the signs relative to what is reported in these papers. That is, we use estimates of Black callback and employment rates when bans on information ("Ban the Box" and pre-employment credit check bans) are not in place. To accomplish this, we use post-ban Black callback and employment rates, respectively, as "baseline" estimates, and show the effect of moving from this baseline (i.e., the period when employers had less information about applicants) to the period before the ban was implemented (i.e., when employers had more information about applicants).

To be specific, Agan and Starr (2018) report a pre-BTB Black callback rate of 10.7% and a post-BTB Black callback rate of 10.4% (in-text), corresponding to the 3 percentage point reduction in Black callback rates after BTB indicated by the coefficient on *Box* in Table 4. We use the standard error on this estimate to calculate confidence intervals. We use the post-BTB callback rate (10.4%) as the baseline and show the positive effect of moving from the BTB period to the available box pre-period (10.7%). Doleac and Hansen (2020) estimate a pre-BTB Black employment rate of 67.7%. We add this baseline to the coefficient on $Black \times BTB$ in Table 4, Column 5 (-0.034) to obtain the post-BTB Black employment rate (64.3%), which we use as the baseline estimate for our purposes. As before, we show the positive effect (3.4 percentage points) of moving from the post-BTB period to the pre-BTB period. Bartik and Nelson (forthcoming) report a 13.5%, or 3 percentage point, reduction in Black job-finding rates resulting from bans on credit histories. We divide the estimate by 0.03 to back out the Black job-finding rate in the period before the ban (22.22%), which implies a post-ban Black job-finding rate of 19.22%. Dividing the 3 percentage point change by the job-finding rate in the post period yields a 15.6% increase in the job-finding rate, moving from the post-ban period to the pre-ban period. Bartik and Nelson (forthcoming) track outcomes for 36 months before and 48 months after a credit history ban. While we show effects before the ban, we report their full follow-up period of 48 months after the ban. We use the standard error reported by the authors to calculate confidence interval bounds and follow the same procedure to estimate confidence interval bounds in the pre-ban period.

This approach allows us to more readily make comparisons between these papers and the others included in the figure, but it relies on the implicit assumption that moving from more information to less information (no ban to a ban) has the same effect as moving from less information to more information (a ban to no ban). We note that our focus on the effects for the marginalized group means we do not always report the papers' main estimates (e.g., Agan and Starr (2018)) but focus on the race gap, which is more precisely estimated than the impact on Black applicants alone that we report).

We emphasize that despite our efforts to make estimates as comparable as possible, there is still variation in what each estimate represents. The main text touches on some of the implications of the differences, including that our estimates are cumulative, while the other skill signal papers capture labor market outcomes during a given week, month, or 2 months; and that the state information bans capture general rather than partial equilibrium responses.

## 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.

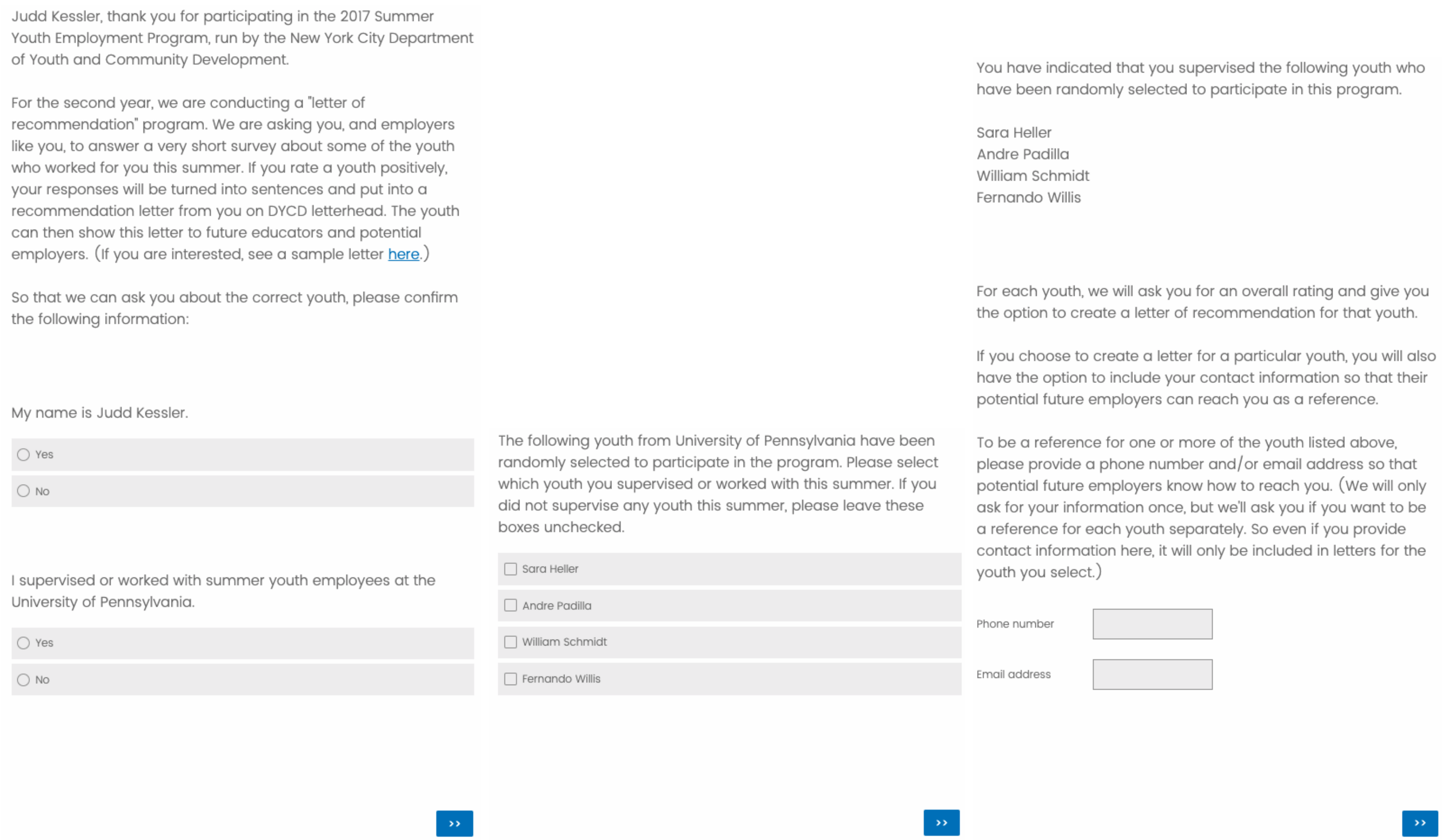
If you have additional questions, you can contact our academic partners: Judd B. Kessler (judd.kessler@wharton.upenn.edu) at the University of Pennsylvania and Sara Heller (hellersa@sas.upenn.edu).

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.

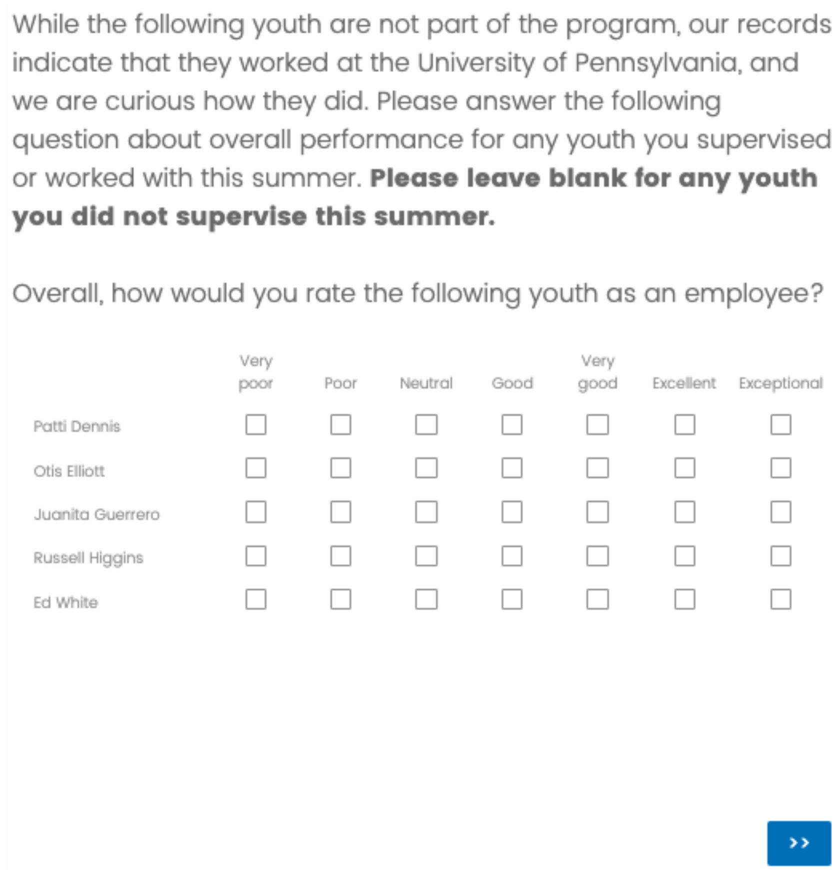
To be a reference for one or more of the youth listed above, please provide a phone number and/or email address so that potential future employers know how to reach you. (We will only ask for your information once, but we'll ask you if you want to be a reference for each youth separately. So even if 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 | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ |
| Otis Elliott | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ |
| Juanita Guerrero | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ |
| Russell Higgins | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ |
| Ed White | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ |

››

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

NYC
Department of
Youth & Community
Development

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

*Note:* 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

Penn
University of Pennsylvania

## 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 |
|---|---|---|---|---|---|
| Year Relative to Randomization | 2016 Study Cohort | | | | |
| | 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 | | | |

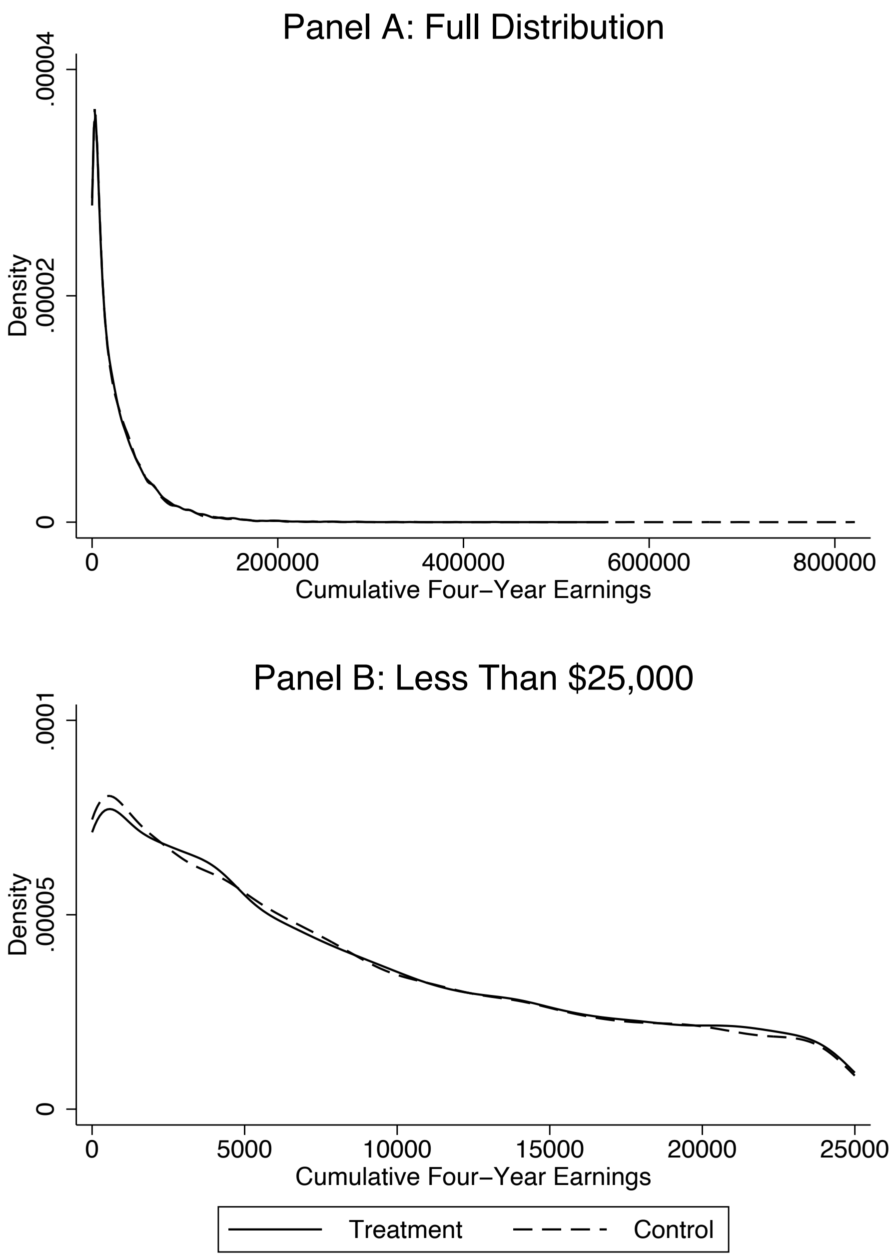
| | 2017 Study Cohort | | | | |
|---|---|---|---|---|---|
| | N = 1,177 | 3,543 | 4,984 | 5,249 | 578 |
| -1 (by 8/2017) | | | | | 4th |
| 1 (by 8/2018) | | | | 4th | 5th |
| 2 (by 8/2019) | | | 4th | 5th | 6th |
| 3 (by 8/2020) | | 4th | 5th | 6th | |
| 4 (by 8/2021) | 4th | 5th | 6th | | |

= Included in graduation measures

= Not old enough to observe

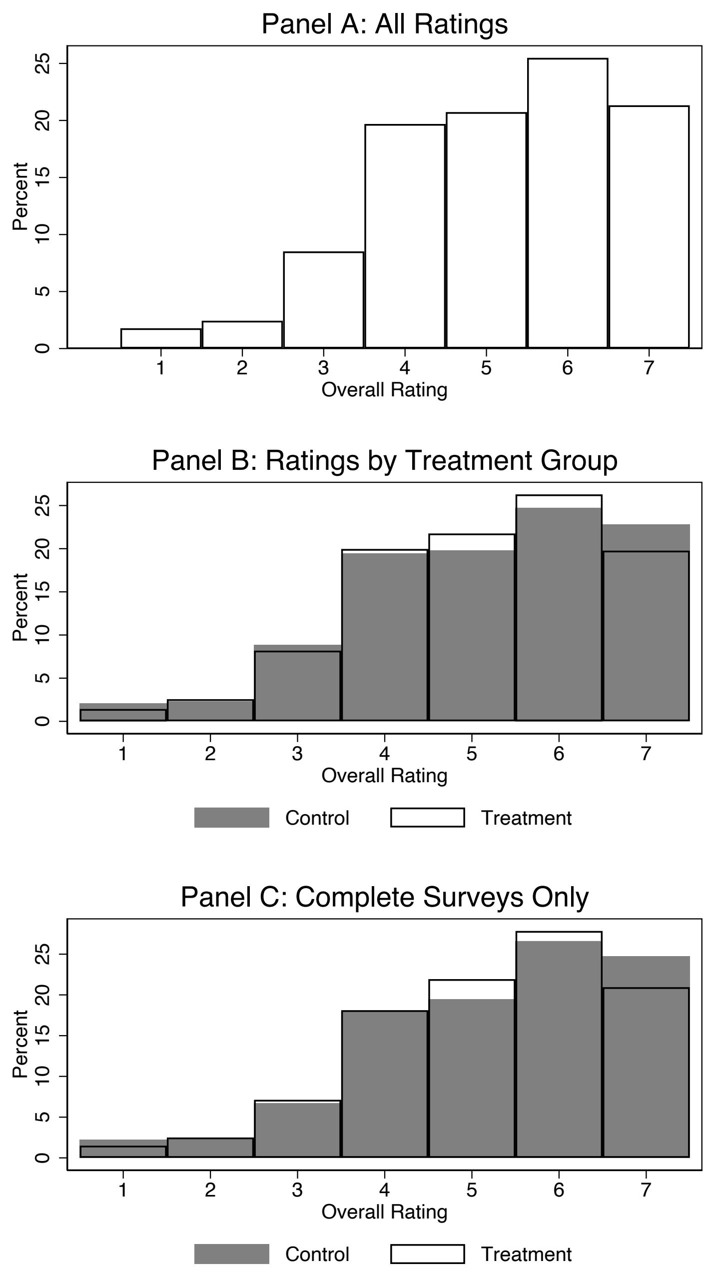
*Note:* Figure shows when 4th-, 5th-, and 6th-year graduation outcomes are observed for students in each pre-randomization grade level by study cohort. Black boxes define our main "expected in high school" sample, for whom at least on-time graduation is observed. Gray boxes show the graduation outcomes that are not yet observed in our data. The only 12th graders included in this sample are those 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




*Note:* 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




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

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.2—Earnings Conditional on Working

| Year | 1 | 2 | 3 | 4 | Cumulative |
|---|---|---|---|---|---|
| ITT | 33.12 | 123.07 | 176.97 | 290.44* | 546.63* |
| | (55.98) | (88.87) | (129.54) | (159.83) | (291.97) |
| CM | 5041 | 8231 | 11349 | 14561 | 29116 |
| Sent Letter (IV) | 82.78 | 300.64 | 440.71 | 708.65* | 1361.65* |
| | (135.13) | (214.44) | (311.77) | (386.23) | (717.25) |
| CCM | 5177 | 8315 | 11329 | 14714 | 29806 |
| N | 30669 | 31357 | 28275 | 29615 | 40088 |

*Note:* Table shows earnings winsorized at the 99th percentile only for the sample with non-zero earnings. CM shows control means; CCM shows control complier means. Regressions include baseline covariates chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.3—Quantile Regression Results for Cumulative Earnings

| Percentile | Control Earnings | ITT, Quantile Regression | Percent Change | P-value, Test of Difference | % T Sent Letter in Bin |
|---|---|---|---|---|---|
| 10 | 909 | 123 | 13.5 | 0.258 | 0.362 |
| 15 | 2,024 | 146 | 7.2 | 0.008 | 0.356 |
| 20 | 3,291 | 152 | 4.6 | 0.152 | 0.356 |
| 25 | 4,500 | 6 | 0.1 | 0.956 | 0.365 |
| 30 | 6,036 | 108 | 1.8 | 0.436 | 0.383 |
| 35 | 7,706 | 203 | 2.6 | 0.238 | 0.402 |
| 40 | 9,727 | 172 | 1.8 | 0.419 | 0.410 |
| 45 | 12,152 | 275 | 2.3 | 0.276 | 0.404 |
| 50 | 14,921 | 205 | 1.4 | 0.480 | 0.428 |
| 55 | 18,330 | 289 | 1.6 | 0.400 | 0.434 |
| 60 | 22,115 | 151 | 0.7 | 0.688 | 0.381 |
| 65 | 26,558 | -94 | -0.4 | 0.827 | 0.415 |
| 70 | 31,511 | 159 | 0.5 | 0.742 | 0.417 |
| 75 | 37,371 | 253 | 0.7 | 0.632 | 0.433 |
| 80 | 44,212 | 973 | 2.2 | 0.127 | 0.420 |
| 85 | 53,670 | 1375 | 2.6 | 0.071 | 0.438 |
| 90 | 67,246 | 1993 | 3.0 | 0.032 | 0.439 |

*Note:* First column defines the percentile of interest. Second column shows the earnings level corresponding to the listed percentile threshold, calculated on the control distribution of raw cumulative earnings. Third column shows the treatment coefficient from a quantile regression of raw earnings on treatment and a cohort indicator. Fourth column shows the implied percent change relative to the percentile cutoff amount. Fifth column shows the p-value on the treatment coefficient, using robust standard errors. Last column shows the proportion of treatment youth who were sent a letter, defining each bin by the control percentile cutoffs, to provide a sense for how compliance changes over the earnings distribution.

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

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.5—Labor Market Effects for DYCD and Non-DYCD Employers

| | DYCD | | | | | Non-DYCD Employers | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Year | 1 | 2 | 3 | 4 | Cumulative | 1 | 2 | 3 | 4 | Cumulative |
| | Panel A: Employment | | | | | | | | | |
| ITT | 0.0049 | 0.0034 | -0.0022 | 0.0000 | 0.0003 | 0.0088** | 0.0009 | 0.0023 | 0.0014 | 0.0037 |
| | (0.0046) | (0.0041) | (0.0020) | (0.0024) | (0.0046) | (0.0040) | (0.0043) | (0.0044) | (0.0045) | (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 | 0.0081 | -0.0054 | 0.0000 | 0.001 | 0.0221** | 0.0023 | 0.0063 | 0.0041 | 0.0091 |
| | (0.0114) | (0.0101) | (0.0050) | (0.0059) | (0.0114) | (0.0100) | (0.0106) | (0.0108) | (0.0110) | (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 | 3.35 | -4.48 | 2.63 | 9.50 | 58.46 | 99.84 | 132.97 | 212.58* | 506.15* |
| | (10.68) | (9.94) | (5.16) | (4.87) | (34.59) | (43.54) | (72.60) | (96.82) | (128.62) | (274.77) |
| CM | 810 | 572 | 117 | 131 | 2527 | 2724 | 5353 | 7261 | 9796 | 25134 |
| Sent Letter (IV) | 2.28 | 7.94 | -11.03 | 6.48 | 26.88 | 144.72 | 253.76 | 341.78 | 540.39* | 1290.77* |
| | (26.42) | (24.56) | (12.78) | (12.06) | (85.42) | (107.56) | (179.44) | (239.22) | (317.84) | (678.88) |
| CCM | 870 | 574 | 128 | 128 | 2592 | 2816 | 5563 | 7426 | 10111 | 25905 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.6—Labor Market Effects by Industry Cluster

| | SYEP-Related Industries (Cluster 1) | | | | | Other Industries (Cluster 2) | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Year | 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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.7—Balance for All Rated Youth by Rating Group

| | Control Low | Treatment Low | Test of Difference | Control High | Treatment High | Test of 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 | | |

*Note:* 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.8—Balance by Rating Group, Fully Completed Surveys

| | Control Low | Treatment Low | Test of Difference | Control High | Treatment High | Test of 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 | | |

*Note:* 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.9—Employment and Earnings Effects by Rating, On Any Survey

| | First Stage | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|
| | | Employment | | | | |
| ITT, Low Ratings | | 0.0135 | 0.0001 | -0.0101 | 0.0099 | 0.0097* |
| | | (0.0089) | (0.0089) | (0.0092) | (0.0094) | (0.0054) |
| ITT, High Ratings | | 0.0122** | 0.0100* | 0.0068 | 0.0008 | 0.0003 |
| | | (0.0059) | (0.0060) | (0.0063) | (0.0063) | (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 |
| IV, Low Ratings | 0.3067*** | 0.0435 | 0.0002 | -0.0331 | 0.0324 | 0.0316* |
| | (0.0068) | (0.0290) | (0.0290) | (0.0301) | (0.0307) | (0.0177) |
| IV, High Ratings | 0.7529*** | 0.0161** | 0.0132* | 0.0089 | 0.001 | 0.0003 |
| | (0.0043) | (0.0079) | (0.0079) | (0.0084) | (0.0084) | (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 | 2.43 | -25.25 | -126.73 | -142.21 |
| | | (84.53) | (140.43) | (181.03) | (237.26) | (518.51) |
| ITT, High Ratings | | 116.09* | 175.34 | 71.40 | 293.61 | 642.02 |
| | | (65.73) | (109.88) | (148.49) | (198.99) | (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*** | -17.41 | 12.81 | -82.44 | -407.86 | -464.03 |
| | (0.0068) | (274.99) | (457.96) | (590.58) | (774.24) | (1691.22) |
| IV, High Ratings | 0.7529*** | 151.68* | 234.65 | 94.80 | 387.30 | 852.53 |
| | (0.0043) | (87.20) | (145.95) | (197.18) | (264.17) | (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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.10—Labor Market Effects Using Non-Missing Controls to Predict All Ratings

| | | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|
| | | Employment | | | | |
| ITT, Low Ratings | | 0.0150** | 0.0023 | 0.0069 | -0.0064 | -0.0014 |
| | | (0.0076) | (0.0076) | (0.0078) | (0.0079) | (0.0052) |
| ITT, High Ratings | | 0.0118** | 0.0074 | 0.0012 | 0.0048 | 0.0047* |
| | | (0.0049) | (0.0049) | (0.0052) | (0.0052) | (0.0027) |
| P-value, test of diff. | | 0.724 | 0.576 | 0.543 | 0.241 | 0.295 |
| CM, Low | | 0.621 | 0.662 | 0.606 | 0.641 | 0.890 |
| CM, High | | 0.740 | 0.748 | 0.672 | 0.702 | 0.938 |
| | First Stage | | | | | |
| IV, Low Ratings | 0.3370*** | 0.0441** | 0.0092 | 0.0217 | -0.0177 | -0.0024 |
| | (0.0056) | (0.0225) | (0.0226) | (0.0230) | (0.0235) | (0.0153) |
| IV, High Ratings | 0.4369*** | 0.0269** | 0.0169 | 0.0025 | 0.0105 | 0.0107* |
| | (0.0041) | (0.0112) | (0.0112) | (0.0119) | (0.0120) | (0.0063) |
| P-value, test of diff. | 0.000 | 0.492 | 0.761 | 0.46 | 0.285 | 0.429 |
| CCM, Low | | 0.613 | 0.678 | 0.603 | 0.667 | 0.895 |
| CCM, High | | 0.728 | 0.746 | 0.684 | 0.708 | 0.934 |
| | | Earnings, Winsorized at 99th Percentile | | | | |
| ITT, Low Ratings | | -37.00 | 55.34 | 81.32 | 99.76 | 200.37 |
| | | (67.08) | (109.80) | (140.42) | (179.73) | (400.00) |
| ITT, High Ratings | | 104.65* | 128.50 | 155.65 | 273.76 | 684.34* |
| | | (55.20) | (92.29) | (126.20) | (169.64) | (363.29) |
| P-value, test of diff. | | 0.103 | 0.609 | 0.694 | 0.481 | 0.37 |
| CM, Low | | 2800 | 4677 | 5723 | 7295 | 20514 |
| CM, High | | 3890 | 6536 | 8188 | 11216 | 29955 |
| | First Stage | | | | | |
| IV, Low Ratings | 0.3370*** | -99.63 | 187.12 | 267.39 | 343.75 | 713.06 |
| | (0.0056) | (198.64) | (324.89) | (415.59) | (532.29) | (1183.04) |
| IV, High Ratings | 0.4369*** | 238.76* | 291.82 | 353.50 | 622.06 | 1553.33* |
| | (0.0041) | (126.33) | (211.31) | (288.95) | (388.18) | (831.61) |
| P-value, test of diff. | 0.000 | 0.151 | 0.787 | 0.865 | 0.673 | 0.561 |
| CCM, Low | | 3071 | 4827 | 5831 | 7366 | 21103 |
| CCM, High | | 3915 | 6626 | 8200 | 11315 | 30150 |

*Note:* N = 43,409. Predicted ratings categories from a regression of rating on observables among controls with non-missing ratings. 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.11—Employment and Earnings Effects by Expected in HS Sample

| | First Stage | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|
| | | Employment | | | | |
| ITT, Expected | | 0.0145** | 0.0126* | 0.0066 | 0.0058 | 0.0048 |
| in High School | | (0.0065) | (0.0064) | (0.0066) | (0.0066) | (0.0039) |
| ITT, Not Expected | | 0.0113** | 0.0002 | -0.0002 | -0.0028 | 0.0012 |
| in High School | | (0.0052) | (0.0053) | (0.0057) | (0.0059) | (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 | 0.4138*** | 0.0351** | 0.0304* | 0.0169 | 0.0141 | 0.0119 |
| in High School | (0.0049) | (0.0158) | (0.0155) | (0.0161) | (0.0159) | (0.0094) |
| IV, Not Expected | 0.3966*** | 0.0285** | 0.0005 | -0.0004 | -0.0069 | 0.0029 |
| in High School | (0.0044) | (0.0132) | (0.0135) | (0.0143) | (0.0148) | (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 | | 15.95 | 76.62 | 160.69 | 236.27* | 543.09* |
| in High School | | (40.95) | (76.23) | (104.62) | (142.76) | (291.48) |
| ITT, Not Expected | | 95.15 | 125.07 | 102.31 | 193.45 | 545.72 |
| in High School | | (71.46) | (115.40) | (154.25) | (203.27) | (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 | 0.4138*** | 47.67 | 203.25 | 413.50 | 607.46* | 1323.39* |
| in High School | (0.0049) | (99.06) | (184.31) | (252.57) | (346.02) | (705.21) |
| IV, Not Expected | 0.3966*** | 236.58 | 315.61 | 258.62 | 486.65 | 1370.80 |
| in High School | (0.0044) | (179.71) | (290.65) | (388.61) | (511.35) | (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 |

*Note:* 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 chosen by the post-double selection LASSO. 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 Age

| | First Stage | 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 |
| 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 |

*Note:* 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 chosen by the post-double selection LASSO. 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 Pre-SYEP Work Experience Status

| | First Stage | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|
| | | Employment | | | | |
| ITT, Never Worked | | 0.0068 | 0.0052 | 0.0005 | 0.0037 | 0.0028 |
| | | (0.0068) | (0.0067) | (0.0068) | (0.0067) | (0.0043) |
| ITT, Ever Worked | | 0.0176*** | 0.0064 | 0.005 | -0.0009 | 0.0033 |
| | | (0.0050) | (0.0051) | (0.0056) | (0.0058) | (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*** | 0.0173 | 0.0132 | 0.0022 | 0.0094 | 0.0067 |
| | (0.0049) | (0.0173) | (0.0170) | (0.0171) | (0.0170) | (0.0110) |
| IV, Ever Worked | 0.4121*** | 0.0428*** | 0.0155 | 0.0121 | -0.0024 | 0.0074 |
| | (0.0045) | (0.0121) | (0.0123) | (0.0136) | (0.0140) | (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 | 6.37 | 9.42 | 170.96 | 267.40 |
| | | (37.83) | (74.50) | (101.95) | (143.66) | (283.22) |
| ITT, Ever Worked | | 69.35 | 177.93 | 232.79 | 253.07 | 747.62* |
| | | (72.48) | (115.96) | (155.12) | (202.61) | (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*** | 103.08 | 34.28 | 43.97 | 469.71 | 671.08 |
| | (0.0049) | (95.48) | (188.20) | (257.23) | (363.49) | (715.99) |
| IV, Ever Worked | 0.4121*** | 170.13 | 433.03 | 558.67 | 600.76 | 1822.65* |
| | (0.0045) | (175.90) | (281.41) | (376.46) | (491.28) | (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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.14—Employment and Earnings Effects for Minority and White Youth

| | First Stage | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|
| | | Employment | | | | |
| ITT, Minority | | 0.0134*** | 0.0065 | 0.005 | 0.0028 | 0.0045* |
| | | (0.0044) | (0.0044) | (0.0046) | (0.0047) | (0.0027) |
| ITT, White | | 0.0049 | -0.0009 | -0.0098 | -0.0074 | -0.0086 |
| | | (0.0114) | (0.0122) | (0.0128) | (0.0129) | (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 |
| IV, Minority | 0.4188*** | 0.0321*** | 0.0156 | 0.0125 | 0.0067 | 0.0103 |
| | (0.0036) | (0.0106) | (0.0105) | (0.0111) | (0.0112) | (0.0064) |
| IV, White | 0.2973*** | 0.0163 | -0.0045 | -0.0327 | -0.0249 | -0.0287 |
| | (0.0088) | (0.0385) | (0.0412) | (0.0433) | (0.0437) | (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* | 138.63* | 190.64* | 322.79** | 773.64*** |
| | | (45.72) | (76.32) | (101.51) | (133.81) | (289.22) |
| ITT, White | | -55.08 | -129.91 | -292.07 | -351.56 | -839.26 |
| | | (131.93) | (213.57) | (296.57) | (402.44) | (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*** | 185.55* | 339.82* | 467.05* | 786.77** | 1846.53*** |
| | (0.0036) | (109.08) | (182.13) | (242.24) | (319.28) | (690.67) |
| IV, White | 0.2973*** | -192.20 | -454.73 | -977.60 | -1186.32 | -2865.06 |
| | (0.0088) | (444.91) | (720.07) | (1001.75) | (1358.32) | (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 |

*Note:* 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 chosen by the post-double selection LASSO. 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 Race/Ethnicity, ITT

| | Y1 | Y2 | Y3 | Y4 | Cumulative | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|---|---|---|---|
| | Employment | | | | | Earnings, Winsorized at 99th Percentile | | | | |
| ITT, White | 0.0049 | -0.0007 | -0.0099 | -0.0073 | -0.0084 | -54.07 | -128.06 | -291.08 | -349.92 | -834.28 |
| | (0.0114) | (0.0122) | (0.0128) | (0.0129) | (0.0075) | (132.00) | (213.69) | (296.68) | (402.37) | (874.83) |
| ITT, Black | 0.0078 | 0.0044 | 0.0011 | -0.0099 | -0.0001 | 6.42 | 74.21 | 53.49 | 27.77 | 198.60 |
| | (0.0064) | (0.0063) | (0.0067) | (0.0067) | (0.0037) | (65.54) | (107.32) | (140.97) | (179.37) | (395.84) |
| ITT, Hispanic | 0.0165** | 0.0066 | 0.0198** | 0.0124 | 0.0093** | 203.13** | 213.19 | 283.35* | 380.19* | 1095.38** |
| | (0.0077) | (0.0077) | (0.0080) | (0.0081) | (0.0047) | (83.64) | (138.10) | (171.42) | (225.46) | (498.19) |
| ITT, Asian | 0.0252** | 0.0026 | -0.0079 | 0.0242* | 0.0063 | -2.12 | 61.98 | 500.69 | 1114.00** | 1811.90** |
| | (0.0121) | (0.0121) | (0.0126) | (0.0125) | (0.0073) | (108.48) | (200.40) | (309.80) | (441.67) | (884.08) |
| ITT, Other | 0.010 | 0.0338* | -0.0185 | -0.0074 | 0.0043 | 103.15 | 427.22 | -42.74 | 368.94 | 951.71 |
| | (0.0194) | (0.0192) | (0.0200) | (0.0204) | (0.0120) | (200.40) | (319.18) | (452.16) | (589.43) | (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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.16—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*** | 0.0162 | -0.0039 | -0.0333 | -0.0246 | -0.0283 | -191.62 | -453.07 | -978.84 | -1183.07 | -2861.66 |
| | (0.0088) | (0.0385) | (0.0412) | (0.0433) | (0.0437) | (0.0255) | (445.14) | (720.36) | (1002.05) | (1358.11) | (2953.12) |
| IV, Black | 0.4039*** | 0.0194 | 0.0108 | 0.0034 | -0.0244 | -0.0002 | 24.02 | 198.01 | 149.00 | 91.52 | 495.10 |
| | (0.0052) | (0.0157) | (0.0155) | (0.0165) | (0.0167) | (0.0092) | (162.06) | (265.35) | (348.52) | (443.42) | (979.76) |
| IV, Hispanic | 0.4152*** | 0.0396** | 0.0157 | 0.0475** | 0.0299 | 0.0224** | 486.56** | 508.28 | 676.96 | 907.86* | 2633.74** |
| | (0.0062) | (0.0186) | (0.0185) | (0.0193) | (0.0196) | (0.0114) | (201.36) | (332.35) | (412.47) | (542.62) | (1199.97) |
| IV, Asian | 0.4830*** | 0.0521** | 0.0055 | -0.0153 | 0.0500* | 0.0131 | 9.06 | 151.61 | 1064.39* | 2344.75** | 3758.13** |
| | (0.0094) | (0.0250) | (0.0250) | (0.0261) | (0.0259) | (0.0151) | (224.45) | (415.00) | (642.79) | (915.64) | (1833.46) |
| IV, Other | 0.3925*** | 0.0252 | 0.0857* | -0.0457 | -0.0189 | 0.011 | 271.80 | 1101.66 | -76.36 | 982.90 | 2391.00 |
| | (0.0155) | (0.0496) | (0.0493) | (0.0511) | (0.0523) | (0.0308) | (512.13) | (817.49) | (1156.50) | (1509.06) | (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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.17—Employment and Earnings Effects by Gender

| | First Stage | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|
| | | | | Employment | | |
| ITT, Male | | 0.0044 | 0.0098 | 0.0054 | 0.0016 | 0.0028 |
| | | (0.0066) | (0.0067) | (0.0069) | (0.0070) | (0.0044) |
| ITT, Female | | 0.0190*** | 0.0029 | 0.0011 | 0.0008 | 0.0031 |
| | | (0.0052) | (0.0052) | (0.0056) | (0.0056) | (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*** | 0.0111 | 0.0249 | 0.014 | 0.0039 | 0.007 |
| | (0.0051) | (0.0166) | (0.0168) | (0.0173) | (0.0177) | (0.0111) |
| IV, Female | 0.4106*** | 0.0462*** | 0.007 | 0.0032 | 0.002 | 0.0071 |
| | (0.0044) | (0.0128) | (0.0127) | (0.0136) | (0.0135) | (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 | 155.43 | 155.04 | 246.22 | 637.00 |
| | | (62.35) | (106.86) | (144.91) | (194.84) | (418.06) |
| ITT, Female | | 63.87 | 66.31 | 108.40 | 191.24 | 475.58 |
| | | (59.27) | (96.98) | (129.47) | (170.58) | (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*** | 133.37 | 401.58 | 411.13 | 636.41 | 1617.41 |
| | (0.0051) | (157.18) | (269.46) | (365.11) | (491.50) | (1055.15) |
| IV, Female | 0.4106*** | 160.27 | 170.37 | 272.34 | 481.08 | 1155.60 |
| | (0.0044) | (144.31) | (236.12) | (315.34) | (415.27) | (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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.18—Employment and Earnings Effects by Neighborhood: Above/Below Median in Opportunity Insights Upward Mobility Ranking

| | First Stage | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|
| | | Employment | | | | |
| ITT, Below Median | | 0.0144** | 0.0098* | 0.0079 | 0.0016 | 0.0036 |
| | | (0.0058) | (0.0057) | (0.0060) | (0.0061) | (0.0035) |
| ITT, Above Median | | 0.0112* | 0.0018 | -0.0021 | 0.0006 | 0.0023 |
| | | (0.0058) | (0.0059) | (0.0062) | (0.0063) | (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*** | 0.0345** | 0.0235* | 0.0192 | 0.0036 | 0.0079 |
| | (0.0047) | (0.0138) | (0.0137) | (0.0144) | (0.0146) | (0.0083) |
| IV, Above Median | 0.3903*** | 0.0288* | 0.0047 | -0.0046 | 0.0019 | 0.0063 |
| | (0.0047) | (0.0150) | (0.0152) | (0.0159) | (0.0161) | (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 | 151.67 | 64.28 | 83.12 | 371.79 |
| | | (60.66) | (99.13) | (127.17) | (164.10) | (362.56) |
| ITT, Above Median | | 62.04 | 57.01 | 194.70 | 352.70* | 722.90* |
| | | (61.45) | (104.33) | (145.73) | (197.76) | (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*** | 133.43 | 369.80 | 161.28 | 210.11 | 889.18 |
| | (0.0047) | (144.87) | (236.69) | (303.70) | (391.74) | (866.46) |
| IV, Above Median | 0.3903*** | 167.16 | 157.30 | 520.99 | 927.76* | 1873.76* |
| | (0.0047) | (157.48) | (267.32) | (373.30) | (506.98) | (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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.19—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 |

*Note:* 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.20—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 | $F(37, 19063) = 1.242$, $p=.149$ | | | |

*Note:* 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.21—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 | -0.0018 | 0.0096** | -0.0032 |
| | (0.0050) | (0.0030) | (0.0042) | (0.0026) |
| CM | 0.736 | 0.049 | 0.177 | 0.037 |
| Sent Letter (IV) | -0.0101 | -0.0052 | 0.0231** | -0.0075 |
| | (0.0120) | (0.0073) | (0.0101) | (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 | -0.0028 | 0.0029 | -0.002 |
| | (0.0048) | (0.0031) | (0.0039) | (0.0025) |
| CM | 0.781 | 0.053 | 0.132 | 0.034 |
| Sent Letter (IV) | 0.0052 | -0.0077 | 0.0072 | -0.0049 |
| | (0.0117) | (0.0075) | (0.0094) | (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 | -0.0021 | 0.0064* | -0.0025 |
| | (0.0049) | (0.0032) | (0.0038) | (0.0024) |
| CM | 0.795 | 0.055 | 0.118 | 0.031 |
| Sent Letter (IV) | -0.0028 | -0.0063 | 0.0154* | -0.0062 |
| | (0.0117) | (0.0077) | (0.0092) | (0.0057) |
| CCM | 0.827 | 0.056 | 0.089 | 0.028 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.22—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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are shown in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.23—Employment and Earnings Effects by GPA

| | First Stage | Y1 | Y2 | Y3 | Y4 | Cumulative |
|---|---|---|---|---|---|---|
| | | Panel A: Employment | | | | |
| 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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.24—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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.25—Labor Market Effects, Alternative Covariates

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

*Note:* N = 43,409. Panel A shows results with no coviarates 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.26—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 | -0.004 | -0.463** | 0.187 | -0.002 | -0.014** | -0.006 | -0.009* | -0.015** |
| | (0.003) | (0.004) | (0.184) | (0.144) | (0.004) | (0.006) | (0.005) | (0.005) | (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 | -0.009 | -1.113** | 0.452 | -0.006 | -0.034** | -0.015 | -0.022* | -0.035** |
| | (0.008) | (0.009) | (0.445) | (0.350) | (0.011) | (0.014) | (0.013) | (0.012) | (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 | 0.001 | -0.135 | 0.076 | 0.002 | -0.007* | -0.001 | -0.004 | -0.005 |
| | (0.003) | (0.003) | (0.098) | (0.100) | (0.003) | (0.004) | (0.004) | (0.004) | (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 | 0.004 | -0.324 | 0.183 | 0.006 | -0.017* | -0.002 | -0.01 | -0.011 |
| | (0.007) | (0.007) | (0.235) | (0.241) | (0.007) | (0.010) | (0.010) | (0.010) | (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 |

*Note:* Analysis conducted on main education sample. Panel A shows results with no coviarates 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.27—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 | | |

*Note:* 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.28—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 | | |

*Note:* 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.29—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 |

*Note:* 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 chosen by the post-double selection LASSO. Standard errors clustered on individual are in parentheses. * p<0.1, ** p<0.05, *** p<0.01

## Table A.30—Forest Plot Details

| Treatment Type | Outcome Desc. | Study Setting | Author(s) (Year) | Follow-Up Period | Location (Orig. Paper) | Compliance Rate | Control Mean | Point Estimate | Standard Error |
|---|---|---|---|---|---|---|---|---|---|
| Skill Signal | Positive employer response | South Africa | Abel et al. (2020) | Varies | Table 1, Col. 1 | | 0.042 | 0.025 $^{\diamond}$ | 0.010 |
| | Positive employer response | South Africa | Carranza et al. (2022) | 2 weeks | Table 4, Col. 2 | | 0.130 | 0.016 | 0.009 |
| | Any platform job over sample period | oDesk | Pallais (2014) | 2 months | Table A2, Col. 2 | 82% | 0.308 | 0.145 | 0.016 |
| | Currently in paid- or self-employment | South Africa | Abel et al. (2020) | 3 months | Table 5, Col. 6 | | 0.134 | 0.037 $^{\diamond}$ | 0.036 |
| | Any formal sector job over sample period | U.S. | Heller and Kessler (2023) | 3 months | New for figure | | 0.318 | 0.017 $^{\diamond}$ | 0.010 |
| | Paid- or self-employment in last week | South Africa | Carranza et al. (2022) | 4 months | Table 1, Col. 1 | | 0.309 | 0.052 | 0.012 |
| | Paid- or self-employment in last week | Ethiopia | Abebe et al. (2021) | 12 months | Table 2, Col. 3 | 48.8% | 0.537 | 0.021 | 0.031 |
| | Any formal sector job over sample period | U.S. | Heller and Kessler (2023) | 12 months | Table 2, Col. 1 | | 0.701 | 0.032 $^{\diamond}$ | 0.010 |
| | Any formal sector job over sample period | U.S. | Heller and Kessler (2023) | 24 months | New for figure | | 0.840 | 0.020 $^{\diamond}$ | 0.008 |
| | Any paid work in last month | Uganda | Bassi and Nansamba (2021) | 26 months | Table 9, Col. 1 | 49% | 0.750 | -0.014 | 0.025 |
| | Paid- or self-employment in last week | Ethiopia | Abebe et al. (2021) | 48 months | Table 2, Col. 7 | 48.8% | 0.657 | 0.029 | 0.032 |
| | Any formal sector job over sample period | U.S. | Heller and Kessler (2023) | 48 months | Table 2, Col. 5 | | 0.922 | 0.007 $^{\diamond}$ | 0.006 |
| | Platform earnings over sample period | oDesk | Pallais (2014) | 2 months | Table A2, Col. 5 | 82% | 59 | 19.59 | 6.03 |
| | Winsorized quarterly earnings | U.S. | Heller and Kessler (2023) | 3 months | New for figure | | 542 | 30.63 $^{\diamond}$ | 25.43 |
| | Earnings in last week | South Africa | Carranza et al. (2022) | 4 months | Table 1, Col. 3 | | 159 | 0.337 $^{\ddagger}$ | 0.074 |
| | Earnings in last week | Ethiopia | Abebe et al. (2021) | 12 months | Table 2, Col. 3 | 48.8% | 739 | 3.36 | 65.67 |
| | Winsorized annual earnings | U.S. | Heller and Kessler (2023) | 12 months | Table 2, Col. 1 | | 3532 | 149.02 $^{\diamond}$ | 106.66 |
| | Winsorized annual earnings | U.S. | Heller and Kessler (2023) | 24 months | New for figure | | 9457 | 413.80 $^{\diamond}$ | 254.84 |
| | Earnings in last month | Uganda | Bassi and Nansamba (2021) | 26 months | Table 9, Col. 2 | 49% | 47 | 3.72 | 3.20 |
| | Earnings in last week | Ethiopia | Abebe et al. (2021) | 48 months | Table 2, Col. 7 | 48.8% | 1217 | 299.47 | 121.38 |
| | Winsorized annual earnings | U.S. | Heller and Kessler (2023) | 48 months | Table 2, Col. 5 | | 26852 | 1348.83 $^{\diamond}$ | 685.56 |
| | Graduated HS on time | U.S. | Heller and Kessler (2023) | $\leq$ 60 months | Table 4, Col. 6 | | 0.785 | -0.016 $^{\diamond}$ | 0.010 |
| | Ever graduated HS | U.S. | Heller and Kessler (2023) | $\leq$ 60 months | Table 4, Col. 7 | | 0.834 | -0.002 $^{\diamond}$ | 0.010 |
| More Info. | Positive employer response | U.S. | Agan and Starr (2018) | 2 months | Table 4, Col. 2 | | 0.104 | 0.003 | 0.015 |
| | Job out of unemployment over sample period | U.S. | Bartik and Nelson (2022) | 48 months | Table 4, Col. 3 | | | -0.135 $^{\ddagger}$ | 0.058 |
| | Paid- or self-employment in last week | U.S. | Doleac and Hansen (2020) | Varies | Table 4, Col. 5 | | 0.677 | -0.034 | 0.015 |

*Note:* Table provides information on each estimate used in Figure 5, including where the estimate is located in the original paper and the compliance rate used to calculate the LATE where applicable. $^{\diamond}$ indicates original estimate is the LATE. $^{\ddagger}$ indicates original estimate already reported as percent change and does not need to be scaled by the control mean. The follow-up period indicates how long the relevant outcome is tracked. See Appendix Section J for further details.