The Effects of Letters of Recommendation in the Youth Labor Market[*]

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Abstract

Youth employment has been near historic lows in recent years, and racial gaps persist. We test whether informational frictions contribute to these outcomes by running a large-scale field experiment involving over 43,000 youth in New York City aimed at providing credible information on young job applicants. We build software that allows employers to quickly and easily produce letters of recommendation for randomly selected youth who worked under their supervision during a summer youth employment program. We then send these letters to nearly 9,000 youth. Being sent a letter generates a 3 percentage point (4.4 percent) increase in employment the following year, with both employment and earnings increases persisting over the two-year follow-up period. By posting our own job advertisement, we document that while treatment youth do use the letters in applications, there is no evidence of a supply-side response (i.e., no increased job search, motivation, or confidence); effects appear to be driven by demand-side responses. Labor market benefits accrue primarily to racial and ethnic minorities, suggesting frictions may contribute to racial employment gaps. But improved employment may also hamper on-time high school graduation. Evidence that letters help improve job match quality suggests that expanding the availability of credible signals about young workers, especially non-White applicants who are no longer in school, could improve the efficiency of the youth labor market.

1   INTRODUCTION

Youth employment lags farther behind adult employment than can be explained by school engagement, and it recovers much more slowly in the wake of major shocks. Even a decade after the Great Recession, youth employment rates in summer—when teenagers are most likely work—were still hovering near their sixty-year low (DeSilver 2021). And the Covid-19 pandemic disproportionately harmed youth labor market prospects at the outset, in part because of the concentration of young people in the food service and retail sectors (Inanc 2020; Kochhar and Barroso 2020). Employment gaps are particularly bleak for minority youth. Unemployment and disconnection rates are 70–80 percent higher for Black and Latino youth than for white youth (Bureau of Labor Statistics 2020; Spievack and Sick 2019), a pattern of disproportionate labor market involvement that is not new (Sum et al. 2014). That youth, and young people of color in particular, have such difficulty securing employment is quite troubling given the range of evidence suggesting that labor force attachment during adolescence and young adulthood affects employment and wages for decades (Baum and Ruhm 2016; Kahn 2010; Neumark 2002; Oreopoulos, Wachter, and Heisz 2012).

Half a century of active labor market programs have tried to address these issues, often with relatively costly efforts to improve human capital or provide job search assistance. Despite some success in developing countries and in U.S. sector-focused training (Card, Kluve, and Weber 2018; Katz et al. 2020; Crépon and Van Den Berg 2016), the frequency with which training programs fail to improve youth employment in the U.S. raises the possibility that important frictions may limit young people's access to the labor market. Their short or nonexistent work histories may leave little way to credibly signal future productivity. Employers may engage in statistical or animus-driven discrimination based on age, class, or race, or they may view participation in training programs as a negative signal itself. Alternatively, on the supply side, young people may lack the social networks, knowledge, or motivation to convert their experience into better employment outcomes.

In this paper, we explore the potential role of informational frictions in the youth labor market by testing an intervention designed to mitigate them. We run a large-scale field experiment with over 43,000 youth in New York City (NYC). We partner with the NYC Summer Youth Employment Program (SYEP), which employs city youth to work over the summer months, to provide a random subset of youth participants with letters of recommendation from their supervisors. To make letter production on this scale feasible, we invite program supervisors to complete a survey tool, developed by our research team, that automatically turns their survey responses on individual participants into full-text letters of recommendation. When supervisors agree to produce a letter of recommendation and pro-

vide high enough ratings of a worker, that treatment youth receives a digital copy and five hard copies of the letter.[1]

We follow treatment and control youth in administrative data for two years after treatment to explore their labor market and educational outcomes. Across a pilot after the summer of 2016 and a full-scale study after the summer of 2017, a total of 43,409 SYEP participants are in our main study sample.[2] We measure employment and earnings using unemployment insurance data from the New York State Department of Labor. We observe educational outcomes, which could be directly affected by the letters or indirectly via changes in labor force involvement, using data from the NYC Department of Education.

Partnering with the NYC SYEP provides an ideal environment to assess the role of frictions in the youth labor market. SYEPs are large social programs that provide paid work to youth—often low-income and minority youth—during the summer months. For about half of these youth, SYEP participation is their first experience in the labor market. Consequently, SYEP participants are representative of the groups likely to face barriers in their attempts to capitalize on their early work experience. Indeed, while prior literature has found that SYEPs improve important outcomes including criminal justice involvement and mortality, multiple randomized controlled trials suggest they do not have consistently positive average effects on future employment (Davis and Heller 2020; Gelber, Isen, and Kessler 2016; Modestino 2019).[3]

Our results suggest a sizable impact of the letter of recommendation intervention. We find that being sent a letter increases the likelihood that a young person is employed by over 3 percentage points in the year after receiving the letter, a 4.5 percent increase relative to the 70 percent of their control group counterparts who work.[4] Employment effects persist over time, with impacts remaining positive and statistically significant over the cumulative two-year follow-up period; youth who are sent a letter are 2 percentage points more likely to have a job over the next two years, a 2.3 percent increase relative to the control complier mean of 84 percentage points. Cumulative earnings are at least 4 percent higher for those sent the letter (p=0.10), with different adjustments for skewness suggesting considerably bigger and more statistically significant effects (up to 23 percent, p<0.05).

1. Our intervention builds on the findings of Pallais (2014) and Abel, Burger, and Piraino (2020), both of which explore the role of labor market frictions by providing information on workers to potential employers. We discuss our study in relation to this literature below.

2. Our empirical strategy involves stacking panels for the two cohorts, so youth can appear in the data more than once. In total, we analyze the data of 41,633 unique individuals.

3. This is in spite of the fact that one of the goals of SYEPs is to improve the long-term employment outcomes of participants (Gelber, Isen, and Kessler 2016).

4. This effect is 250% as large as prior estimates of the effect of the summer program itself on employment. Gelber, Isen, and Kessler (2016) finds that the NYC SYEP increased employment by 1.2 percentage points in the post-program year by encouraging youth to participate in SYEP again.

That simply providing a few pieces of paper improves employment and earnings suggests an important role for information frictions in the youth labor market. Our treatment could be mitigating frictions on either the supply side or the demand side. On the supply side, letters may give youth information about what makes them valuable to employers and the confidence to apply to jobs; on the demand side, letters may give employers a clearer signal about the abilities of a particular youth or make a youth's application more salient.

To assess the mechanism driving our results, we ran an additional data-collection exercise to measure job-seeking behavior among a subset of our sample. We invited 4,000 participants from both treatment and control groups to apply for a short-term online job working for us. The applications we received confirmed that youth use the letter: Treatment youth were 267% more likely to submit a letter of recommendation as part of their application (4.5 percent of control applicants and 16.5 percent of treatment applicants included a letter in their application). Since only about 40 percent of letter recipients actually use them on our application, our main IV effect of being sent a letter likely understates the impact of letters shown to potential employers. Back-of-the-envelope calculations—which involve scaling our employment estimates by the rate that letters are submitted as part of our job application—suggest that actually using the letter is associated with up to a 15 percent increase in employment in the first year and about $1,400 in additional earnings (also about 15 percent) over two years.

Youth in our treatment group were no more likely to apply for our job and no more likely to check a box asking to be considered for a more-selective, higher-paying opportunity, suggesting that our employment effects are not being driven by increased motivation, job search, or confidence.[5] That there was no detectable difference in application behavior among treatment and control youth suggests that the letters work by changing how employers view applicants, rather than how applicants behave. In the presence of demand-side frictions, finding additional ways to communicate credible information about youth applicants to potential employers may help youth succeed in the labor market.

Consistent with this idea, heterogeneous treatment effect analysis of our employment and earnings data reveal that employment and earnings increases are concentrated among youth who are more highly rated by their SYEP employers, suggesting an important role for information transmission in the letters. We also find that the labor market benefits accrue primarily to minority youth, suggesting that this group may face larger frictions in the youth labor market.

5. While it is possible that the increased outside employment among treatment youth affected the decision to apply to our job, application rates did not differ either between treatment and control youth who were employed elsewhere during the quarter we solicited applications, or between treatment and control youth who were not employed elsewhere.

Given that employers seem to respond to letter content, one might worry that letters could lead employers to *incorrectly* update beliefs about candidates. If employers' priors about who obtains letters lead them to believe applicants will be higher productivity employees than they actually are, short-term increases in employment could represent bad matches and be followed by increased churn. Because we can track how many consecutive quarters employees work at the same employer, we can test this question directly. We find no evidence of increased turnover; treatment youth work more quarters at the same number of jobs, such that some job spells get longer. This set of findings suggests the letters are helping workers and employers make more successful job matches.

Lastly, we collected information on educational outcomes in addition to employment data for two reasons. First, letters could have a direct educational effect if shown to teachers or guidance counselors: other work has shown that teachers' and other adults' beliefs about young people directly affect their outcomes, even when the information that changed those beliefs is fictitious (Rosenthal and Jacobson 1968; Bertrand and Duflo 2017). Letters could also help with college applications if young people have few other sources of recommendations. Second, working during high school could pull young people out of school. There is a general consensus in the current literature that working more than 20 hours is harmful, but working a small amount has little effect on schooling (Buscha et al. 2012; Staff, Schulenberg, and Bachman 2010; Monahan, Lee, and Steinberg 2011; Baum and Ruhm 2016; Ruhm 1997). However, the lack of exogenous variation in this literature means that it is still unclear whether a shock to employment would have a causal effect on school success.

We find few significant changes in education outcomes for the nearly 20,000 youth in our study who we can observe in New York City high schools. But among those for whom we can observe graduation, we find that letters of recommendation may slow down—but do not appear to stop—high school graduation, by pulling people into the labor market. There is suggestive evidence, however, that a subset of minority students may not just slow down graduation but also fail to graduate because of the increased employment. Longer-term data is needed to assess what happens once everyone is old enough to be observed six years after the start of high school. In the meantime, policy efforts to provide employers with credible information on minority high school students may minimize the risk of substitution away from school by targeting those not on the margin of graduating. For the average high school student, the welfare implications of efforts to reduce informational frictions in the labor market may depend on how future career trajectories are affected by the value of additional work experience and the cost associated with a longer time spent in high school.

While our employment effects are large, they are consistent with past research that suggests providing even a small amount of information about job-seekers can be quite pow-

erful. In response to fictitious applications in audit studies (Agan and Starr 2018; Kaas and Manger 2012) and to the suppression of information in the real labor market (Bartik and Nelson 2019; Doleac and Hansen 2020), employers show less discrimination when they have more information about candidates. Providing performance information has also been shown to increase short-term employment in two different kinds of labor markets. Pallais (2014) randomly hires nearly 1,000 workers on an online labor market platform (oDesk), provides randomly selected workers with more-detailed public performance reviews, and finds that workers with no prior experience benefit on the platform from being hired and rated, while those with prior work experience benefit from the detailed reviews over the next two months. Abel, Burger, and Piraino (2020) find that encouraging a subset of 1,300 job seekers in South Africa to secure letters of recommendation increases job search and survey measures of being employed among women, but not overall, after three months.

We build on this prior work by exploring the impact of letters of recommendation among 43,000 young people in a large, urban U.S. labor market. The setting expands the study of information frictions to an environment where, unlike oDesk or South Africa, employers can potentially access a range of richer signals about youth applicants (e.g., more widespread, visible employment histories or knowledge about local high schools and GPAs), but also face higher hiring costs (e.g., a minimum wage and more burdensome paperwork). Using administrative data, we can observe employment and earnings at jobs across New York State for two years after treatment, as well possible spillovers on high school outcomes for the study youth who are also enrolled in New York City high schools. These features also allow us to explore specific questions unanswered by the prior literature, such as whether the short-term increase in employment is a temporary result of encouraging bad matches.

Our study provides new evidence that information frictions do prevent young people, especially people of color, from securing successful employment. Given that employers seem to value credible information about applicants, finding additional ways to provide personalized information could help improve labor market outcomes among low-income, minority populations like those in our study. Such interventions might be best targeted at those who are not on the margin of graduating on time to avoid harming educational attainment.

The impact of scaling up efforts to facilitate letters of recommendation (or other credible signals) will depend on general equilibrium effects that we cannot directly measure within our study. Welfare effects of expanding letter distribution in general equilibrium could go in either direction. It is possible that reducing information frictions could increase overall employment by helping employers fill vacancies they would otherwise have left open in the face of too much uncertainty. It is also possible that youth with recommendation letters may simply displace those without them (although we view this as unlikely to have happened

within the context of our control group, given that there are about one million 15- to 24-year-olds in the NYC labor market and we sent fewer than 9,000 letters across two years). Even the welfare implications of full displacement are not obvious, however, since policymakers may value the distributional changes or efficiency gains from better matches, even if there were no net change in employment.

Additional research on exactly how letters change employers' decision-making process would help to predict the welfare consequences of broader efforts to facilitate credible productivity signals. For now, this study provides new evidence on the role of informational frictions in depressing employment and earnings for young people, which potentially limit the impact of programs designed to improve their skills and future labor market outcomes. Fortunately, it may not be particularly costly to reduce these frictions by communicating to employers about applicants' strengths.

2 Setting, Experiment, and Data

2.1 Setting

We partner with the New York City Summer Youth Employment Program (NYC SYEP), implementing our experiment with youth who participated in the summer of 2016 or the summer of 2017. The NYC SYEP is administered by the NYC Department of Community Development (DYCD). Since a post-Great Recession minimum enrollment of 29,416 youth, enrollment has grown steadily to nearly 70,000 youth in 2017. In our program years, the NYC SYEP provided youth with 6 weeks of paid work during July and August. All NYC residents aged 14–24 were eligible to apply for the SYEP program, though 40% of eventual participants were aged 16–17. Participants in the program were provided with jobs with private sector (45%), non-profit (41%), and public sector (14%) employers. The NYC SYEP directly pays youth for their work with their matched employers at the New York State (NYS) minimum wage ($11.00 in 2017). Youth payroll totaled $83 million in 2017, or roughly $1,200 per youth participant. The NYC SYEP had a total program cost of $127 million in 2017. Over 80% of this cost was funded by the City of New York, with a majority of its remaining funding coming from NYS (see *SYEP Annual Summary* 2017).

2.2 Letter of Recommendation Experiment

We received SYEP data from DYCD on a subset of participants from the 2016 NYC SYEP (n=16,478) and all of the participants in the 2017 NYC SYEP (n=66,763). The program data identified each youth's summer work site and the supervisor or supervisors for the youth at that work site. Using these data, we limited our sample in several ways. First, since we

needed to contact supervisors to ask them to complete the letter of recommendation survey, we excluded youth supervised by someone without an email address in the data. Second, we excluded some youth at large work sites to avoid making the survey unmanageable for a single supervisor. 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. We applied the same restriction for the control youth in the survey.[6] In total, this left a sample of 69,222 SYEP participants who were included on at least one survey. Figure 1 traces through this and the subsequent steps of how youth moved through the study.

To generate recommendation letters, we built a survey tool that sent a personalized survey to each supervisor asking about the youth who they supervised that summer (i.e., the youth linked to them in the DYCD data).[7] The email inviting each supervisor to participate explained the letter of recommendation program, included a link to the personalized survey tool, and encouraged them to participate (a sample of the email from 2017 is shown as Appendix Figure A.1). Supervisors were given approximately two weeks to complete the survey, and we sent up to two reminder emails to supervisors who had not yet completed it. 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).

The survey began with a brief explanation for supervisors that if they rated a youth positively enough, their responses to the survey questions might be used to construct letters of recommendation. A link to an example letter was provided to aid in the explanation. Respondents were then asked to confirm that they had been a SYEP supervisor during the preceding summer (see screens at the start of the survey in Appendix Figure A.2). Once the supervisor confirmed being a supervisor, they were shown the list of treatment youth linked to them in the DYCD data, listed alphabetically by last name.[8] Supervisors selected which youth they had directly supervised and were asked a set of questions about each youth

6. 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.

7. The data did not link every youth to a single supervisor. 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. 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. In the survey, we asked supervisors to confirm the youth that 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.

8. Note that confirming one's identity and position as an SYEP supervisor is how we count "starting" the survey, a definition that is relevant below.

(supervisors were asked about the youth they selected in random order). The survey asked supervisors for an overall rating on the youth's performance and whether they would be willing to answer questions that would turn into a letter of recommendation for the youth (see Figure A.2 for screenshots of the survey). If they were willing, they were also asked if they wanted to include their contact information on the letter of recommendation to serve as a reference. They then rated the youth on several attributes, shown in Figure 2.

After the supervisors answered questions about treatment youth, they were asked one question each about control youth—the same question about the overall rating on the youth's performance—all on one screen (see Appendix Figure A.3). They were told that these youth would not be included in the letter of recommendation program.

A total of 5,760 supervisors (1,333 in 2016 and 4,427 in 2017, about 40 percent of all supervisors we emailed) opened the survey and made it to the screen where they could start rating treatment youth. In total, 43,409 young people were on a started survey, 29,887 (69 percent) of whom were given an overall rating by employers (5,978 in 2016 and 23,909 in 2017).

The software we built for this project converted the supervisors' survey responses on treatment youth into formatted letters of recommendation populated with sentences for each youth attribute. For each positively rated attribute, the letter included a dynamically constructed sentence. For example, if in response to the question "How was $< youth\ name >$ at communicating?" the supervisor selected "Very effective," a sentence would appear in the letter that read: "$< Youth\ name >$ was a very effective communicator." Whereas, if the supervisor selected "Not effective" or "Somewhat effective" in response to that question, the sentence about communication would not be included in the letter.

We assigned each attribute to a potential paragraph. If the supervisor rated the youth positively enough on enough attributes to construct a particular paragraph, the paragraph was included in the letter. As long as two paragraphs could be included, the letter was generated for the youth. This procedure ensured that any letters of recommendation our survey tool generated had enough positive things to say about the youth to provide a positive letter that would not be too sparse. Our software produced letters of recommendation as PDFs on official DYCD letterhead. The letters ended with "Sincerely," followed by the name of the supervisor and work site. A short note in the footer of the letter described our letter of recommendation pilot program. Figure 3 shows a sample letter.

In total, we generated and sent 8,780 letters (1,805 in 2016 and 6,975 in 2017). We uploaded digital copies of these letters to Dropbox with a link sent to the youth for whom emails were known (1,737 in 2016 and 6,720 in 2017).[9] In addition, we mailed five physical

9. About 56 percent of letter recipients clicked the link in their email to view the letter digitally. Many

copies of the letters via USPS to each youth along with a cover letter providing context and suggested uses for the letter (see Appendix Figure A.4 for a sample cover letter; similar text was sent to youth via email along with the link to the soft copy of the letter).[10] All letters of recommendation were sent in time for winter holiday hiring in the year after SYEP participation (letters were sent to youth in early-December 2016 for the 2016 cohort and in mid-November 2017 for the 2017 cohort).

2.3 Job Application Data

To understand the mechanisms through which letters of recommendation might impact labor market outcomes of treatment youth, we advertised a job to a subset of the youth in our data, solicited job applications, and hired youth ourselves. We composed a job listing for a one-time, remote paid work assignment, emailed the job listing to 4,000 randomly selected subjects from our 2017 cohort, and observed their job application behavior. The sample was evenly split among treatment and control youth from the letter of recommendation experiment (i.e., youth who had been eligible and ineligible to receive the letter of recommendation) who also had an email address in the data so we could send them the job application.

The job 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 an application was included at the bottom of the job description (see the email invitation sent to youth with the job description in Appendix Figure A.5).

Youth who clicked the link in the email were taken to a job application that asked a few standard contact, background, and employment experience questions. Our application also provided an optional space to upload up to three "supporting documents (e.g. resume or other documents that might strengthen your application)." The application did not explicitly mention uploading letters of recommendation, but it would have been easy for youth to upload the soft copy of the letter of recommendation provided to them in our experiment (see the screenshot of this prompt in Appendix Figure A.6).[11] This upload interface allowed us to measure whether youth provided supporting materials—including a letter of recommendation—with their applications and to assess whether this differed by

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SYEP youth create an email solely for the purpose of the online application and then abandon it, so some letter recipients may not have seen the email containing the link to the digital copy of the letter.

10. Of the 8,780 letters mailed to youth, 127 were returned as undeliverable.

11. We intentionally avoided explicitly mentioning a letter of recommendation to see if youth in our study would choose to upload a letter without a specific prompt to do so. We saw this as realistic to job applications in practice where a youth could choose to provide a potential employer with a letter of recommendation even if one was not specifically requested.

treatment and control youth.

Finally, to assess the confidence of youth in our study, we gave applicants the opportunity to check a box on the application to be considered for a more selective, higher-paying position ($18/hour). The application told them explicitly that being considered for the more selective position would not affect their chances at being selected for the regular job.

All applicants who submitted an application that included their name, email address, and at least 1 additional field were hired.[12] 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.[13] Workers were instructed to finish everything they could within a two-hour time frame. All youth who initiated the job-task (n=232) were paid for two hours of work via a mailed gift card (so our job does not appear in the administrative data on employment and earnings).

2.4 SYEP Administrative Data

Administrative data from the NYC SYEP comes from the NYC DYCD, which runs the program. We received data on a subset of participants of the 2016 NYC SYEP and all participants of the 2017 NYC SYEP. The data on SYEP participants include identifiers (e.g., name, date of birth, and social security number) that allow us to match to various data sources; demographics (e.g., gender, race, and education status) that allow us to test for balance across treatment and control; and contact information (e.g., mailing address and email address) that we used to send letters of recommendation to treatment youth. We define racial/ethnic categories based on the self-reported categories in the application, making the classifications mutually exclusive (e.g., "White" only captures non-Hispanic Whites). We also received information on the work site where the youth worked for the summer and information about the supervisors at that work site, including name and email address. We use the information on work site and supervisor to send the letter of recommendation surveys.

2.5 NYS Department of Labor Data

We obtained earnings and employment data from the New York State Department of Labor (NYSDOL). Data came from NYSDOL's quarterly Unemployment Insurance (UI) dataset,

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12. 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 open response questions in addition to fulfilling the requirements for the standard job.

13. Youth hired for the more selective job were asked additional open response questions that required more thoughtful consideration.

which covers formal sector employment, excluding self-employment or farming income. The data include employer name, employer FEIN, employer address, employer NAICS, and amount paid 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 percent of SYEP youth in our letter of recommendation experiment were matched to the NYSDOL data with no difference between treatment and control youth ($\beta = 0.001, p = 0.128$).[14]

We use data from Q1 (January–March) of 2010 through Q4 (October–December) of 2019. This window provided considerable baseline data as well as two years of outcome data after letters were sent to SYEP participants in our treatment group for each study cohort.[15]

2.6   NYC Department of Education Data

Education data come from the NYC Department of Education (DOE).[16] 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 2019–2020 school years. 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 main 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, 7,643 had no active enrollment within our 2015-2020 data. These students were largely old enough to have left school prior to 2015 (their average age at randomization is 19.7), though some may have transferred to private or non-NYC districts prior to the start of our data. This leaves 69.9% of our main sample with at least some education information in the data, with no treatment-control difference ($\beta = -0.003, p = 0.436$).

14. 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.

15. Letters were sent in Q4 (October–December) of 2016 or 2017, depending on cohort. Consequently, we have additional quarters of data for the youth in the 2016 cohort, but we limit the analysis to the period we can observe for full years for both cohorts, and we stop prior to any influence from Covid-19.

16. 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 are also never linked to DOE data.

3 Method of Analysis

This section discusses how we perform the analysis in this paper. In Section 3.1, we describe our sample definitions and our outcomes of interest for each data source. In Section 3.2, we describe our empirical approach, including our regression specifications. In all sections, we note cases where we deviated from our pre-analysis plan with accompanying explanations for these choices.[17]

3.1 Sample Definitions and Outcomes

3.1.1 Labor Market Sample

Our main sample to explore labor market outcomes consists of the 43,409 SYEP participants who were on a survey that a SYEP supervisor started (i.e., the SYEP participant 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.[18]

We pre-specified this subsample of youth on a started survey as a key sample of interest, because neither treatment nor control youth on *unopened* surveys could have actually received treatment. This kind of non-compliance mechanically reduces statistical power and is orthogonal to treatment status, so we focus on the subsample with a first stage of 0.404 (rather than the first stage of 0.254 when we include youth on unopened surveys).[19] As a result, the treatment effect of receiving a letter of recommendation in our main sample is representative of the population of youth whose supervisors both had an up-to-date email address in the DYCD data and were willing to click on an invitation to participate in the letter of recommendation program. The estimates from this sample of youth might differ from the treatment effect on the broader sample of all SYEP youth, because different types

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17. The pre-analysis plan can be found at https://osf.io/8zwdr/

18. It also excludes the 14,019 youth who were never placed on a survey due to our eligibility restrictions. We randomized all SYEP participants to construct the surveys and apply our eligibility screens (e.g., to ensure a survey had no more than 30 treatment or 30 control youth or to remove those assigned to supervisors with missing email addresses). These youth were never on a survey for reasons unrelated to treatment status, so were never intended to be part of the study sample.

19. While we pre-specified this subsample as a key sample of interest, our main sample included all SYEP participants that we randomized, because we did not anticipate that over one third of the sample would be on unopened surveys. For completeness, we present and discuss results for this larger sample in Appendix Section A.7. We choose to emphasize the results from our smaller sample in the main text, because the power gains from focusing on this subsample give better insight into the effect of the letter of recommendations on the sample of youth who might actually have been eligible to be treated, given the actions of their supervisors.

of youth are placed into jobs with different types of supervisors.[20]

Since supervisor non-response was driven by an inability to reach supervisors by email or by a lack of supervisor interest or capacity to complete the survey, limiting our analysis to this sample does not interfere with the integrity of random assignment (i.e., until the supervisors reached the substantive survey questions, they had no way of knowing which youth would be included in the survey or which youth would be in the treatment or control groups). As expected, Table 1 shows that our main sample is balanced across treatment and control youth.

3.1.2 Labor Market Outcomes

We pre-specified a primary focus on annual earnings, winsorized to deal with outliers, along with alternative methods to adjust for skewness as robustness checks. We pre-specified an indicator for any employment as a secondary outcome. Our main analysis shows employment and earnings in the first year after randomization (4 quarters including the quarter the letters were sent), the second year, and cumulatively, winsorized at the 99.5^{th} percentile, as well as log(earnings+1). Since the +1 transformation effectively manufactures the proportional change from zero, in Appendix Section A.1.1 we also show robustness to alternative transformations (winsorization at the 99th percentile, adding 0.1, 10, and 100 to earnings prior to logging, and the inverse hyperbolic sine).

We also pre-specified exploratory analyses on: (1) the number of jobs and length of jobs to assess job stability and match quality, and (2) the industry of employment to assess whether letters help youth find jobs in which they now have experience (i.e., those over-represented in SYEP jobs) or whether the letters help market youths' skills to the higher-paying industries that are under-represented in SYEP jobs (see a discussion of these industry definitions in Gelber, Isen, and Kessler (2016)). For (1), we define a job spell as all consecutive quarters worked at the same employer. Other outcomes related to spell length and industry are discussed in Appendix A.1.3.

3.1.3 Job Application Sample

We randomly selected 2,000 control youth and 2,000 treatment youth from our main sample to invite to apply to our job application.[21] Table A.5 shows baseline balance for this subsample. Although the vast majority of baseline covariates are balanced, we note that

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20. Appendix Section A.7 shows that youth who were on unopened surveys are indeed observably different than the youth in our control group of opened surveys on demographics and employment outcomes, although not in their likelihood of applying to our job posting. As such, it is possible that forcing supervisors to rate youth might have somewhat different effects than those we estimate here.

21. We also invited 1,000 youth from unopened surveys (i.e., outside of our main sample) to ensure that job application behavior was not dramatically different for the youth excluded from our main sample.

the treatment group in this subsample is significantly more Hispanic by chance (33 percent in the treatment group versus 29 percent in the control group). As we show in Appendix Section A.5, labor market impacts for Hispanic youth are larger than for other groups. As a result, the point estimates for employment and earnings are considerably larger for this sample, although the smaller sample size makes the estimates imprecise (see Table A.6).

3.1.4 Job Application Outcomes

For the sub-sample of individuals we randomly selected to receive our job application advertisement, we pre-specified three key outcomes: whether someone applied, whether they uploaded a letter, and whether they checked the box to apply to a more selective job (as a measure of confidence).

Observing whether there is a treatment-control difference in application rates helps us to test whether there is a supply-side job search response behind any potential changes in labor market outcomes. The proclivity to opt into consideration for the "more selective" job (i.e., with higher pay that requires a stronger application) tests for treatment-control differences in self-efficacy and motivation or in beliefs about the probability of being hired. Whether applicants uploaded a letter provides an implied "first stage" on use of a letter in a job application. We can use this implied first stage to back out an implied LATE for the compliers who are not directly observed in our data: those who actually use the letter in a job application due to random assignment.

We also report two additional outcomes to provide a more complete picture of job application behavior: whether someone clicked the link to view the job application (regardless of whether they applied), and whether someone uploaded any file (e.g., CV, transcript, or anything else) in support of their application.

3.1.5 Rated Youth Sample

To distinguish whether the letters provide a general signal or convey useful information about worker productivity, we report labor market impacts separately for youth who receive low (rating category 1-4) versus high (rating category 5-7) overall ratings from supervisors on the question about overall performance, asked of both treatment and control youth.[22] Unlike our main sample, however, there is the potential for selection into who receives a rating based on supervisor behavior in the survey (e.g., whether the supervisor completes the survey or stops part-way through). Because the survey was designed to maximize the number of letters generated, treatment youth were listed first, along with a longer, multi-page set of questions; control youth were all listed at the end of the survey on a single page, with check

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22. Low ratings are "Very Poor," " Poor," "Neutral," and "Good." High ratings are "Very Good," "Excellent," and "Exceptional."

boxes that allowed the supervisor to quickly answer the single overall quality question about each control youth. The different positioning and survey content for treatment and control youth could change the probability a supervisor rated a particular youth. Additionally, supervisors were told (and could decide whether) their responses would be turned into a letter for treatment youth, but not for control youth. The possibility of sending a letter may itself lead supervisors to make different decisions about whether to rate a youth or which rating to give. Because of both differences, we would not necessarily expect the distribution of treatment and control youth to be identical conditional on having a rating or receiving a particular rating.

Appendix Table A.8 shows that, in fact, treatment youth are significantly less likely to have received a rating than control youth (66 versus 71 percent). Although the distribution of baseline characteristics is not statistically different across groups conditional on having a rating, it is significantly different for youth receiving a low rating (Table A.9 shows that treatment youth receiving a low rating are observably different from control youth receiving a low rating, perhaps because supervisors were more hesitant to give low ratings when a letter might be produced than when they knew it would not).

To minimize the role of selection introduced by whether a youth is rated, when we explore treatment effects by ratings, we focus on the sub-sample of youth who were on a survey in which the supervisor rated every treatment youth and every control youth in the survey. There are 13,911 youth who were on such a survey. Since everyone is rated, these surveys leave no room for treatment-control differences in who is rated within the survey. Appendix Tables A.11 and A.12 indeed show that while treatment youth are 1 percentage point less likely to appear on a completed survey overall, the observable distribution of baseline characteristics is similar both overall and within rating groups for this sub-sample. Appendix Section A.3 shows that results are similar when using all youth with a rating.

3.1.6 Education Sample

Because we knew much less about what education data would be available to us at the time of pre-specification, the education analysis is where we deviate most from our pre-analysis plan. As reported above, about 70 percent of our sample has any active record in the DOE records during the period our data cover (2015-2020). But in practice, many of these students either graduated or left school prior to our 2016 and 2017 study years. And while charter school students do appear in DOE data as having active records, DOE does not share any information about school engagement, performance, or graduation for charter school students with outside researchers. Because of the amount of missing data, including on individual elements like GPA and college enrollment even within individuals that have some educational

records, we leave the analysis of separate educational outcomes to Appendix Section A.4. Instead, we focus the main text on the most substantively important high school outcome, and the one that provides the greatest contribution to the question of how work matters for schooling: high school graduation. We note that because this focus was not pre-specified, it should be considered an exploratory analysis.

We limit our graduation analysis to students for whom we could plausibly measure graduation. Graduation is reported only in the academic years that correspond to a student's on-time (4th), 5th, or 6th year graduation cohort; it is missing for students who transfer to a charter school; move out of district; fall under another exclusion, such as having an individualized education plan (IEP); or were not in a 4th–6th year graduating cohort between 2015–2016 and 2019–2020. To avoid conditioning the graduation sample on what could potentially be an outcome (e.g., transferring in or out of the District), we restrict the sample to those most likely to be observed in the graduation data based only on pre-randomization characteristics.

In particular, we limit attention to students who were enrolled in grades 9–12 in the year prior to randomization, were not enrolled in a charter school at the end of the pre-randomization year, and who had not yet graduated. This is the sample who we expect to be in a DOE high school in the year after SYEP (if they do not transfer or leave school) and who are of the age where we could potentially observe their graduation. We count any graduation outcome within 3 years of randomization. This always allows us to observe on-time (4th year) graduation outcomes and allows us to observe some youth out to the 6th year of high school, after which the DOE stops reporting graduation data. This group 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. This sample restriction leaves us with 14,726 students in our graduation analysis, with no treatment-control difference on being in this sample ($\beta = -0.005, p = 0.379$).

3.1.7 Education Outcomes

We use two main indicators of high school graduation: whether someone graduated on-time (in 4 years) during our follow-up data and whether someone graduated within 6 years in the same time frame.[23] There are 38 students who are part of our graduation sample but missing

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23. Since our graduation data end in the 2019-20 school year, graduation is observed even after the 2 years of follow-up we use for our employment analysis. We define our graduation indicators to include any graduation within 3 years for two reasons. First, we observe these data for both study cohorts. Second, our interest is in eventual graduation, and graduation status in the third year still reflects schooling progress during the earlier two years when labor market involvement is measured.

a graduation record, likely because they joined a group excluded from state graduation counts after randomization (e.g., transferred out of district). Since these individuals did not receive a diploma from NYC DOE, we assign them 0s for graduation.[24]

Results on the two graduation indicators separately, as well as other high school performance measures and on-time college enrollment, are in Appendix Section A.4. For the main text, we focus on whether working more slows down or stops high school graduation. To measure this relationship, we define a set of mutually exclusive joint outcome indicators: working and graduating, never working but graduating, working and not graduating, and never working nor graduating. We define these indicators both for on-time graduation and for ever graduating. The pattern across these outcomes will allow us to assess whether any potential shifts in graduation occur among the same group that experiences shifts in employment.

3.2 Analytical Method

3.2.1 Main Analysis

We begin with an intent-to-treat (ITT) analysis by regressing each outcome variable on a treatment indicator and baseline covariates. The analysis equation is of the form:

$$Y_{ij} = \alpha + \beta T_i + \gamma X_{it-1} + \epsilon_{it}$$

where Y_{it} is an outcome for individual i at time t, T_i is an indicator for random assignment to treatment, and X_{it-1} is a vector of covariates measured at or before the time of random assignment. As pre-specified, we use a post-double-selection LASSO to select which covariates to include in each regression (Belloni, Chernozhukov, and Hansen 2014a, 2014b; Belloni et al. 2012).[25] We always include an indicator variable for study cohort, since randomization occurred separately by study year. Because 1,776 individuals appear more than once in the data, we cluster our standard errors on individual as identified by SSN in the SYEP data.

Not every treatment youth on a started survey actually received a letter, either because: (1) they were on a survey answered by someone who was not their direct supervisor or by

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24. DOE discharge codes suggest there is no treatment effect on whether students transfer out of the District ($\beta = 0.003$, $p = 0.322$ for transfers, with a control mean of 0.033). The on-time college enrollment measure discussed in the appendix, which records if someone is enrolled in an out-of-state college 6 months after their on-time graduation date, also suggests that differential mobility is unlikely to be a problem for employment results; treatment youth are no more likely to leave New York State for college ($\beta = -0.002$, $p = 0.692$ with a control mean of 0.064).

25. We implement this with the Stata commands pdslasso and ivlasso (Ahrens, Hansen, and Schaffer 2020). See Appendix Section A.8 for a list of the covariates we offer the LASSO, and for results without any covariates or with all covariates as robustness checks.

someone who did not provide ratings for them, (2) their ratings were not good enough to receive a positive letter, or (3) they never received either the mailed or emailed letter we sent. As a result of this kind of non-compliance, the ITT will understate the effect of actually receiving a letter. In addition, we cannot observe who actually uses the recommendation letters in practice, so we cannot directly estimate the effect of letter use on compliers. Instead, we do two things to provide a sense of the effect's magnitude for those who are actually treated. First, we use random assignment as an instrument for whether a youth was sent a letter. Since we perfectly observe whether every youth was sent a researcher-generated letter, we can estimate this treatment-on-the-treated effect for everyone. We report control complier means as a baseline measure to assess proportional changes for compliers (Kling, Liebman, and Katz 2007).

Second, we use our job application data to estimate the proportion of treatment youth who actually use the letter in practice, and we scale the ITT effect by this estimate of letter usage. Because our job posting is not representative of all job applications, this extrapolation involves a strong assumption that letter use in the rest of the labor market looks like letter use in our job application. This assumption could fail in two ways: either because it is easier to remember the letter or submit the letter in our application than in other real-world applications (in which case we would likely understate this TOT effect), or because treatment changes the composition of who applies to our job posting by changing whether youth are employed when we send our advertisement (the direction of which depends on employment treatment effects). We discuss the interpretation issues further, below, but in general we consider this a rough approximation of the effect of actually using the letter rather than a direct estimate.

3.2.2 Heterogeneity analysis

Although we pre-specified at the outset that we would not have enough statistical power to differentiate heterogeneous treatment effects, we follow our pre-analysis plan in conducting exploratory analyses based on the characteristics most likely to affect youth's labor market prospects. For all heterogeneity tests, we report the ITT, the first stage, and the IV separately for each group to show how much of ITT differences are from different rates of receiving a letter and how much are from different responses conditional on being sent a letter.

The main text focuses on separating labor market impacts for White and minority youth, defined as any non-White self-classification, including Black, Hispanic, Asian, and Other. To help identify whether employers are responding to the specific information in the letters, we also test for heterogeneity across supervisor ratings. If the letters are successfully communi-

cating specific information, we would expect that providing letters with higher ratings would generate larger labor market benefits.

Appendix Section A.5 breaks down effects by specific racial/ethnic group, and it reports heterogeneity on the other pre-specified categories: age, gender, school enrollment (as self-reported on the application to SYEP), and neighborhood. The appendix also explores heterogeneity by previous work experience to see if the effects are limited to those who lack other signals of an ability to get a job on their applications.

4 Results

4.1 Summary Statistics

Table 1 shows average pre-randomization characteristics for the treatment and control groups.[26] Study participants reflect the population that participates in NYC's SYEP. On average, they are just over 17 years old, about 43 percent male, largely identify as minorities (only 12.5 percent list being White on their application), and 75 percent report being in high school. About 45 percent of participants never appear in the UI data prior to their participation in SYEP, but during the SYEP year, 97 percent work, earning an average of just under $2,400.

4.2 Labor Market Effects

Table 2 reports the main labor market effects. Panel A shows that being assigned to the treatment group increases employment rates by 1.3 percentage points (1.8 percent relative to the control mean of 70 percent) during the year following letter distribution.[27] Actually being sent the letter increases year 1 employment by 3.1 percentage points (4.5 percent relative to the control complier mean). The point estimates in the second year after letter distribution are still positive but smaller and not statistically significant. However, the increase in employment does is still significant over the cumulative two-year follow-up: being sent a letter increases net two-year employment rates by 2 percentage points (2.3 percent).

It is likely that the employment effect will fade out eventually, since almost all control youth will eventually work in the formal labor market at least once. But earnings changes would not necessarily fade out if the letter is helping set youth on a better employment trajectory or find better jobs. Panels B and C report program effects on winsorized earnings in dollars and log(earnings+1), respectively. The treatment effect grows in levels over the two years observed, though effects are somewhat noisy. Those sent a letter earned a total of

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26. As indicated in Section 3.1.1, no more differences are significantly different than would be expected by chance, nor are they jointly significant ($p = 0.267$).

27. Letters were sent in December of 2016 and November of 2017, so we include the final quarter of each year as part of year 1.

$433 more over the two-year follow-up period ($p = 0.101$), a 4.4 percent increase. The $log + 1$ transformation is more precise, with Panel C showing a significant 18.6 percent increase in cumulative earnings and significant increases in both years 1 and 2.

Because there is a treatment effect on the extensive margin, the results may be sensitive to how we handle the proportional change at $0. Table A.1 shows alternative level, log, and asinh transformations. The results suggest that the magnitude of the change is somewhat sensitive to functional form, ranging up to 23 percent, but generally statistically significant. We focus on the 4.4 percent estimate in the main text both because it is the pre-specified primary outcome and because it is the most conservative estimate. Across robustness checks, the evidence suggests that the letters of recommendation generated a sizable increase in earnings.

Table 3 digs more deeply into the UI records to understand how labor market outcomes are improving. The first column shows that treatment youth work in 0.05 more quarters (0.11 for letter recipients) than their control counterparts. The last column shows that conditional on working at all (i.e., for those selected into work), treatment youth find jobs sooner than control youth (0.12 quarters sooner for letter recipients). Together, this pattern suggests that the letters help youth find jobs sooner, but do not merely substitute early work for later work; they work more than they would have otherwise.

One concern about this pattern is the possibility that supervisors could be over-updating their beliefs about youth, interpreting the letters as a stronger quality signal than they actually are. If so, we might expect increased churn, with treatment youth getting hired and fired more frequently than controls. The rest of Table 3 suggests this is not the case: there is no increase in the number of job spells treatment youth have. The point estimate on the number of jobs (including 0s) is quite close to the overall employment estimate, suggesting it is driven by the change at the extensive margin. And conditional on working at all (column 3), which is a selected sample, the point estimate is a precise zero. As we would expect for young people who start working earlier and work more in the same number of jobs, Appendix Section A.1.2 shows that some spells get longer. This provides further evidence that letters are not reducing—and in fact may be increasing—the quality of the job match.

4.3 Mechanisms

4.3.1 Assessing Changes in Labor Supply

A key question about the observed increase in labor market success among treatment youth is whether the letters increase labor supply by increasing youth job search intensity or confidence or whether the letters increase labor demand by changing beliefs about applicants with letters, or increasing the salience of those applicants, among employers. By distributing our

own job posting to 4,000 treatment and control youth, we are able to generate some evidence on why the letters increase employment and earnings and to approximate a first stage for actually using the letter. Appendix Section A.2 shows that we have baseline balance within this sample and shows the main employment and education results for this group.

Table 4 suggests that supply-side responses (increased job search, motivation, or confidence) are unlikely to be driving the labor market improvements. We find no evidence that treatment youth are more likely to click on the application link or actually apply to our posting.[28] The second column shows that 8.8 percent of the control group and 8.2 percent of the treatment group applied to our job. We also find no evidence that the letter increased confidence among applicants conditional on applying; treatment youth are no more likely to volunteer for the more selective job than control youth (see the last column of Table 4, which, adjusting for application rates, translates into 60 percent of control applicants and 51 percent of treatment applicants checking the box to apply for the more selective job).

Of course, it is possible that even though the letters did not change the rate at which young people applied to our job, they could have changed the composition of who applied. Since treatment youth were more likely to be employed in the formal labor market, their interest in our short-term, online job may have been directly affected by treatment, even though we framed the job as flexible enough to be compatible with other work. That said, we cannot reject the null that observables are jointly unrelated to treatment status among applicants, suggesting this is not likely to be the case.[29] Additionally, even if we condition on not being employed elsewhere during the quarter the job application was distributed (a selected group), there is still no significant difference in application rates or our confidence measure ($\beta = -0.01$, $p = 0.351$ for applying and $\beta = -0.01$, $p = 0.132$ for checking the selective box), so the lack of supply-side behavior change does not appear to be due to treatment youth being more likely to be employed already. Overall, the evidence from our job application suggests that labor market improvements are coming from employers responding to letters of recommendation, not from changes in youth's application behavior or confidence.

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28. The "applied" variable here measures whether a youth entered enough information in the application for us to know who filled out the application form. We define "applied" this way because we hired people even if they did not answer all the questions on the application. There is also no treatment-control difference in fully completing the application.

29. As Appendix Section A.2 shows, there is some imbalance (on two individual characteristics) within the job application sample, despite being jointly similar. As such, we test for differences between the treatment and control individuals who applied for our job, conditional on being in our application sample, by interacting each baseline covariate with an indicator for whether the individual applied, regressing treatment on all covariates and these interactions, then testing the hypothesis that all interaction coefficients are jointly 0. The p-value of this test is 0.14.

4.3.2 Assessing the First Stage

As an important check on whether treatment youth actually use the letters we send them—a necessary condition for employers to be able to respond to the letters—the final two columns of Table 4 show treatment effects on the files job applicants uploaded in their application to our job posting. There is no detectable change by treatment in the probability that youth upload some form of supporting material. But there is a dramatic change in whether youth upload a letter of recommendation. Only 0.4 percent of the control group submits a letter, including zeros for those who do not apply (conditional on applying, this translates to 4.5 percent of control applicants submitting a non-intervention letter with their application). Treatment youth are two and a half times more likely to submit a letter of recommendation than the control group: 1.4 percent of all those invited to apply submit a letter (16.5 percent conditional on applying). Since about 40 percent of treatment youth actually received a letter, this implies that about 41 percent of letter recipients use them when they apply to a job (16.5 percent relative to 40 percent).

The observed rates at which letters are used can also benchmark how big employment responses are for youth who actually use their letters. If we very make the (admittedly strong) assumptions that the difference in letter use we observe in our job application applies to the entire sample and that treatment and control youth apply to jobs at the same rate, then the implied first stage for letter use is a 12 percentage point increase (4.5 versus 16.5 percent among applicants). Scaling our main ITT effects by this first stage would in turn imply that the employment increase for those who use the letter is about 15 percent relative to baseline in the first year, and 8 percent over two years, with an additional $1,400 in earnings over that time. If we think that it might be easier to use the letter in our application than in typical job applications (e.g., because receiving a job advertisement that references SYEP and having a screen to upload supporting material reminds treatment youth about the letter or primes them to use it more than in a typical job application) then we are likely overstating the number of treatment youth who used a letter relative to controls. In that case, our 12 percentage point first stage would be an overestimate, and the actual TOT would be even larger than our calculations here suggest.

4.3.3 Assessing Changes in Labor Demand

The evidence so far suggests that employers are the ones responding to an increased use of letters of recommendation in the job applications of treated youth. A final mechanism question is how those letters affect their hiring behavior. It is possible that because letters are infrequently included in typical job applications, it is the presence of the letter itself—regardless of content—that makes an application more salient, resolves some basic

uncertainty about whether an applicant is likely to show up at all, or overcomes statistical discrimination. Alternatively, employers may be using the content of the letter to try to discern something more nuanced about future employee reliability or productivity.

Although we did not send letters where SYEP supervisors included too few positive comments about the youth they supervised, there is still variation in how positive supervisors were in their letters that allows us to assess whether employers respond to letter content. Table 5 shows employment and earnings effects separately for youth who received low ratings (categories 1–4, corresponding to "Very Poor," "Poor," "Neutral," and "Good") and high ratings (categories 5–7, corresponding to "Very Good," "Excellent," and "Exceptional").[30] Highly rated youth were much more likely to receive a letter (81 percent versus 33 percent). So the ITT differences between the groups reflect both differences in letter receipt and differences in outcomes conditional on being sent a letter, although the substantive pattern of results is relatively similar for both the ITT and TOT.

The first result of note is that the ratings do seem to capture attributes that matter in the labor market. Looking at the control means, low-rated youth are 6 percent less likely to work during year 1 (67 versus 72 percent employed), though they catch up to high-rated youth over time. They also earn just under $1,500 (14.6 percent) less over 2 years. Second, we find that, cumulatively, the low-rated group has net employment effects close to 0 and cumulative earnings point estimates that are negative but with huge standard errors. In contrast, the high-rated group has employment and earnings effects that grow over time, such that they are driving basically all of the net positive effects of the treatment. It appears, then, that employers are using the substance of the letters to identify those who are likely to be highly productive employees, but who might not otherwise be noticed during the hiring process. The group of young people who did not impress their SYEP supervisors as much may need more intensive investment in improving skills to reap long-term gains.

4.4 Work and Graduation

Education results on engagement and performance outcomes are in Appendix Section A.4. In general, there is little evidence that letters improve student performance in school (e.g., by changing teacher or guidance counselor beliefs or encouraging college application). While none of the treatment effects are statistically significant, there is one pattern that becomes significant in several subgroups and alternative covariate specifications: on-time (4-year) graduation shows a substantively important decline, while the point estimates on ever grad-

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30. Note that if youth received an overall rating less than "Good," the paragraph that included text about the overall rating was not printed in the letter. These letters were still produced, though, as long as enough other attributes were rated positively enough.

uating (including delayed graduation), graduating or still attending, and enrolling in college on time, are an order of magnitude smaller. This pattern provides suggestive evidence that recommendation letters increase employment while slowing down—but not stopping—high school graduation for those still in school, on average.

A natural hypothesis is that our employment and education effects are driven by the same group of youth: that by pulling young people into the labor force, the letters make it harder for them to complete their high school education on time, leading them to spend additional time in school. Table 6 tests this hypothesis by showing treatment effects for the joint outcomes of working and graduating, never working but graduating, working but not graduating, and never working nor graduating. Panel A shows these outcomes using on-time graduation. Panel B shows these outcomes using whether someone ever graduated, on time or otherwise.

The pattern of results is consistent with letters reducing on-time graduation—but not eventual graduation—for the same group of youth who also have positive employment impacts from the letters. Panel A shows that treatment significantly decreases the proportion of youth who graduate on time without working by 2.4 percentage points for letter recipients (19 percent). Treatment also generates a corresponding increase, of roughly similar size (2.6 percentage points, or 21 percent), in the proportion of youth who work but do not graduate on time. Since there are no significant changes in the other two categories, this is suggestive of a shift from graduating on time without work to working but failing to graduate on time.

By contrast, Panel B shows that for ever graduating, there is no significant change in the proportion of youth who work but do not graduate. Rather, treatment generates a significant 2.5 percentage point decrease in the proportion of letter recipients who never work but graduate, and an offsetting increase (2.2 percentage points) in the proportion who both work and graduate. In other words, the letters seem to encourage employment among those who ever graduate. The point estimates among non-graduates show a similar pattern of a negative point estimate for not working with an offsetting positive one for working. These estimates, however, are substantially smaller and not statistically significant.

In sum, for the subset of study youth for whom we have graduation data, this joint outcome analysis suggests that the shock to employment generated by the letters slows down graduation, but does not stop it. These results provide a useful contribution to the literature on working during school, which typically has been unable to measure on-time graduation and has hit a ceiling effect when analyzing overall graduation (Buscha et al. 2012; Staff, Schulenberg, and Bachman 2010; Monahan, Lee, and Steinberg 2011; Ruhm 1997; Baum and Ruhm 2016). The welfare implications of the graduation slow down—whether the slightly longer high school career outweighs the benefits of the additional work experience and earnings—

depends on how the letters affect the trajectory of future longer-term outcomes. Additional related results, which suggest there may be substantive differences in both employment and schooling impacts by race and ethnicity, are in the following section.

It is worth emphasizing that only about 34 percent of our overall sample is in this analysis, due to the smaller set of youth who are a relevant age and for whom we have education data. The rest of the youth in our study are either too young for us to observe graduation, are out of school already, or are enrolled in schools that are not in our data. Those not in our high school data still have significant increases in year 1 employment as well as much larger point estimates on earnings than the high school sample (see Appendix Section A.4). So, from a policy perspective, it may be feasible to focus on mitigating information frictions that impact youth who are not still in high school, or at least students not on the margin of graduating on-time.

4.5   Heterogeneity

Table 7 shows that, while we do not have the statistical power to differentiate between the two groups, the labor market effects of the letters appear to be driven largely by racial and ethnic minorities. The letters have no significant effect on White youth, who show negative but imprecise point estimates from the letters. Effects are only positive and statistically different from zero for the non-White (Black, Hispanic, Asian, and other) youth in the sample.[31]

The first-stage results suggest part of the difference in the ITT effects is that minority youth are much more likely to be sent a letter (42 percent versus 30 percent for minority youth and White youth respectively). This difference may have to do with differential selection into the SYEP in the first place, since only 13 percent of our SYEP participants are White, or with differences in the kinds of SYEP supervisors for whom minority and White youth work. But the IV results show that, even conditional on being sent a letter, the point estimates for employment and earnings are much bigger for minority youth than for White youth. One might wonder whether the larger TOT reflects differences in letter quality; perhaps letters for minority youth matter more because they are stronger letters. However, we observe that the opposite is true: conditional on receiving a letter, White youth receive ratings that are 0.43 points higher (on our 7-point scale) than minority youth, with no significant differences in whether they use the letter on our job application (see Appendix Section A.6 for descriptions of how letters and job application behavior differ by subgroup). Consequently, it seems that the letters sent to minority youth have a particularly powerful

31. Appendix Section A.5 shows results separately for the individual racial and ethnic groups, as well as for other subgroups of interest.

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effect because of how employers respond to them.

Table 8 shows some important implications of this heterogeneity for the educational effects of the letters by separately analyzing the joint outcomes from Table 6 for White and minority students. Consistent with the idea that it is the increased employment that slows down graduation, Panel A shows that the shift from graduating on-time without working to failing to graduate on-time but working is most pronounced among minority students. The substantive pattern is fairly similar among White students, but the small number of them means the changes are not statistically different from zero.

Racial patterns start to diverge, however, when looking at whether youth ever graduate (Panel B). Here, it appears that the letters do, in fact, increase work at the expense of graduation for minority students. The 3.7 percentage point decrease in graduating but not working among letter recipients no longer shifts entirely to working and graduating for minorities. That point estimate is positive (1.5 percentage points), but not statistically significant. Instead, the increase in the proportion of minorities who work but do not graduate remains significant (1.1 percentage points). Of course, not everyone in the data has had time to reach their 6-year graduation date, so it is possible that the affected students will still eventually graduate.[32] At least for the current follow-up period, however, it appears that letters may induce a small proportion of minority youth not to finish school.

White students, on the other hand, show an insignificant but substantively large decline in the proportion who work without ever graduating (explaining the lack of an average change in this outcome). They appear to shift to graduating without working, raising the possibility that although the letters do not increase employment among White youth, they may encourage those students' school progress. Unfortunately, with only 1,211 White students observed in this graduation data, our sample is too small to differentiate these changes from noise.

5   Discussion

The fact that sending youth a few copies of a letter and an email with a link to that letter increased employment rates by 4 percent—and perhaps as much as 15 percent for those who actually use the letter—suggests that there are, in fact, frictions in the labor market for youth, and minority youth in particular, that are relatively low cost to overcome. We do not find differences in job-seeking behavior among treatment youth other than using the letter, suggesting that employers are the ones responding to the additional information contained in the letters. This interpretation is bolstered by results showing that higher performing

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32. Table A.16 shows that, combined with the higher proportion of minority youth who both work and graduate, there is no significant overall decline in graduating among minority students so far.

youth get a larger labor market benefit from the letters.

We also find that recommendation letters lead to a decline in on-time graduation, and possibly overall graduation for minority youth, driven by substitution toward work on the margin. In addition to being important for any future policy decisions about letters of recommendation or other signals about youth, this finding also speaks to the literature on the impact of working during high school. Our letters provide a plausibly random shock to working during high school, which appears to extend the time spent in high school. To assess the welfare implications of this letter-generated substitution, we would need to make some strong assumptions about how long the increase in earnings will last and how that compares to the longer time in high school. It seems likely that the net effect may not be beneficial, at least for the subset of youth whose overall educational attainment may decline, rather than just be delayed. Reducing employment frictions is most likely to have a net benefit for those who have already finished their high school careers or are not currently on the margin of graduating.

Overall, the labor market results indicate that employers respond to credible information about youth, such that finding additional ways to provide them with personalized information about an individuals' strengths could help improve labor market outcomes among low-income, largely minority populations like those in our study. For social programs looking to help youth capitalize on their training experience, this is an important insight.

A natural question is how broadly this finding applies—whether we are documenting that letters help overcome the particular stigma associated with SYEP participation or that, more broadly, at least some youth unemployment is due to frictions surrounding the availability of information about young applicants. The answer to this question rests partly on whether the employers in our data knew that youth applicants were SYEP participants, which would be necessary for the stigma story. While we cannot observe that directly, we can take a hint from the applications that youth submitted in response to our job ad. In those applications, only 22 percent of applicants self-identify as a SYEP participant in either their list of work experience or their résumé. Given that almost 80 percent of job applicants would not appear to employers as SYEP participants—and that the recommendation letters came on letterhead from the agency that runs the program, increasing the salience of the SYEP—it seems plausible that the frictions we document are more general than SYEP-related beliefs among employers.

Despite the positive effects of the letters on labor market outcomes, our findings do not necessarily imply that policymakers should try to give everyone letters of recommendation. Our estimates are for youth who receive letters when survey responses are voluntary and responses are positive enough, and effects may differ outside of this population. In addition,

any effort to generate more widespread use of credible signals like letters of recommendation could result in displacement; youth with letters might gain jobs, but at the expense of those who would otherwise have taken those jobs.

Such displacement and general equilibrium effects are worth considering as part of efforts to scale up such programs. There are several conditions under which a scaled-up version could be beneficial, even with considerable displacement. If policymakers valued equity, then transferring job opportunities to those farther down the income distribution or to historically marginalized groups might be socially beneficial. Alternatively, even with no net change in employment, letters could generate efficiency gains by helping employers and employees find better matches. And, if employers end up leaving some vacancies open in the face of too much uncertainty about applicants, a widespread information-sharing intervention might increase overall employment. Finally, there could also be general equilibrium effects on the supply side; if young people understand that they may receive helpful recommendation letters, they may work harder in their jobs, generating additional productivity as well as better letters to which future employers will respond more positively.

Even in partial equilibrium, our experiment establishes that information frictions prevent minority young people from getting jobs they could otherwise succeed in. Further research into the precise way employers update their beliefs or substitute across workers in response to efforts to mitigate these frictions would be a productive next step in assessing the most effective way to leverage our findings into higher youth employment.

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Tables and Figures

Table 1: Descriptive Statistics

	Control Mean	Treatment Mean	Test of Difference
N	21,695	21,714	
Age	17.2	17.2	0.641
Male	0.427	0.427	0.991
Black	0.409	0.411	0.805
Hispanic	0.289	0.289	0.944
Asian	0.129	0.130	0.734
White	0.124	0.125	0.756
Other Race	0.049	0.045	0.080
In High School	0.755	0.751	0.339
HS Graduate	0.044	0.042	0.202
In College	0.173	0.180	0.081
Not in UI Data	0.009	0.011	0.128
Never Employed Pre-SYEP	0.450	0.457	0.113
Ever Worked, Year -4	0.153	0.149	0.210
Earnings, Year -4	318	320	0.882
Ever Worked, Year -3	0.267	0.266	0.840
Earnings, Year -3	585	585	1.000
Ever Worked, Year -2	0.437	0.435	0.627
Earnings, Year -2	1072	1050	0.412
Ever Worked, Year -1	0.966	0.966	0.798
Earnings, Year -1	2379	2368	0.722
No Education Match	0.126	0.123	0.359
In HS Sample	0.454	0.454	0.938
Joint F-Test	F(24, 41632) = 1.16, p=.267		

N = 43,409. 390 youth missing race/ethnicity and 1 missing education. 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 2: Labor Market Effects

Year	1	2	Cumulative
Panel A: Employment			
ITT	0.0127***	0.0058	0.0079**
	(0.0041)	(0.0041)	(0.0034)
CM	0.701	0.72	0.841
Sent Letter (IV)	0.0313***	0.0144	0.0195**
	(0.0102)	(0.0102)	(0.0083)
CCM	0.697	0.728	0.841
Panel B: Earnings, Winsorized at 99.5th Percentile			
ITT	60.03	110.1	168.66
	(45.89)	(73.23)	(106.86)
CM	3579	5964	9543
Sent Letter (IV)	154.11	281.4	433.17
	(113.40)	(180.95)	(264.02)
CCM	3729	6162	9894
Panel C: Log(Earnings + 1)			
ITT	0.095***	0.059*	0.075**
	(0.033)	(0.035)	(0.030)
CM	5.61	6.08	7.33
Sent Letter (IV)	0.234***	0.146*	0.186**
	(0.081)	(0.087)	(0.073)
CCM	5.64	6.18	7.39

N = 43,409. Winsorization in Panel B recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. 73 observations adjusted in year 1, 254 in year 2, and 269 cumulatively. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 3: Amount and Timing of Work

	Num Quarters Worked	Num of Job Spells	Num of Job Spells if >0	Time to First Qtr Worked
ITT	0.045**	0.019	0.002	-0.048**
	(0.022)	(0.014)	(0.014)	(0.020)
CM	3.46	1.98	2.36	2.19
Sent Letter (IV)	0.111**	0.046	0.006	-0.119**
	(0.054)	(0.034)	(0.034)	(0.048)
CCM	3.59	1.98	2.35	2.18
N	43409	43409	36647	36647

Notes: Spells defined as consecutive quarters with earnings from same employer. Time to first quarter conditional on spells > 0. CM shows control means; CCM shows control complier means. Regression includes baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 4: Job Application Effects

	Clicked Link	Applied	Checked Selective Job Box	Uploaded Any File	Included Letter of Rec
ITT	-0.007	-0.006	-0.01	0.003	0.010***
	(0.009)	(0.009)	(0.007)	(0.007)	(0.003)
CM	0.103	0.088	0.053	0.052	0.004
Sent Letter (IV)	-0.02	-0.019	-0.027	0.006	0.024***
	(0.024)	(0.022)	(0.017)	(0.018)	(0.007)
CCM	0.138	0.123	0.082	0.065	0.009

Notes: N = 4,000. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 5: Labor Market Effects for Youth with High and Low Employer Ratings

	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Low Ratings	0.0250*	-0.0146	0.002	63.24	-163.28	-98.75
	(0.0133)	(0.0134)	(0.0110)	(129.42)	(211.89)	(308.35)
ITT, High Ratings	0.013	0.0238***	0.0174**	106.42	338.72**	437.84*
	(0.0087)	(0.0087)	(0.0070)	(99.38)	(164.96)	(237.14)
P-value, test of diff.	0.455	0.016	0.235	0.791	0.062	0.168
CM, Low	0.673	0.721	0.836	3109	5409	8518
CM, High	0.715	0.720	0.846	3729	6251	9979

	First Stage					
IV, Low Ratings	0.3301***	0.0756*	-0.0442	0.0056	190.62	-506.09	-311.39
	(0.0103)	(0.0405)	(0.0405)	(0.0332)	(391.67)	(641.82)	(933.60)
IV, High Ratings	0.8108***	0.0161	0.0293***	0.0212**	130.92	413.65**	535.92*
	(0.0057)	(0.0108)	(0.0108)	(0.0087)	(122.57)	(203.44)	(292.50)
P-value, test of diff.	0.000	0.155	0.08	0.649	0.884	0.172	0.387
CCM, Low		0.61	0.756	0.821	3012	5942	8950
CCM, High		0.713	0.717	0.843	3626	6079	9714

Notes: To avoid selection into who is rated within a survey, sample includes only youth who were on a survey where employer rated all listed youth (n = 13,911). Low includes rating categories 1-4; high includes 5-7. Test of difference shows p-value for null hypothesis that treatment effects are equal in low- and high-rated groups. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table 6: Effects on Joint Employment and Graduation

	Panel A: Any On-Time Graduation			
	Ever Work, On-time Grad	Never Work, On-time Grad	Ever Work, Not On-time Grad	Never Work, Not On-time Grad
ITT	0.0023	-0.0096*	0.0111**	-0.0045
	(0.0063)	(0.0050)	(0.0044)	(0.0032)
CM	0.693	0.116	0.147	0.044
Sent Letter (IV)	0.007	-0.0238**	0.0258**	-0.0109
	(0.0143)	(0.0115)	(0.0101)	(0.0072)
CCM	0.716	0.124	0.122	0.039
	Panel B: Any Graduation			
	Ever Work, Graduated	Never Work, Graduated	Ever Work, Not Graduated	Never Work, Not Graduated
ITT	0.0108*	-0.0089*	0.0032	-0.0044
	(0.0062)	(0.0049)	(0.0046)	(0.0034)
CM	0.681	0.108	0.160	0.052
Sent Letter (IV)	0.0249*	-0.0216*	0.0076	-0.01
	(0.0141)	(0.0112)	(0.0106)	(0.0078)
CCM	0.703	0.116	0.136	0.044

N=14,726. Analysis on all those expected to be observed based on pre-program grade of enrollment (see text for details). Panel A shows whether someone ever worked during the two-year follow up and whether they graduated on-time during the data. Panel B shows whether someone ever worked and whether they ever graduated, regardless of timing. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table 7: Labor Market Effects for Minority and White Youth

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Minority		0.0134***	0.0066	0.0090**	79.03	149.27*	227.66**
		(0.0044)	(0.0044)	(0.0036)	(48.13)	(77.52)	(112.60)
ITT, White		0.0048	-0.0019	-0.0031	-70.27	-162.15	-230.22
		(0.0114)	(0.0122)	(0.0096)	(144.77)	(218.80)	(328.57)
P-value, test of diff.		0.483	0.513	0.236	0.328	0.18	0.187
CM, Minority		0.6932	0.7229	0.839	3540	5958	9498
CM, White		0.7518	0.6949	0.851	3754	5702	9457
IV, Minority	0.4188***	0.0319***	0.0158	0.0214**	194.27*	365.42**	557.35**
	(0.0036)	(0.0106)	(0.0105)	(0.0086)	(114.85)	(184.98)	(268.66)
IV, White	0.2973***	0.0157	-0.0077	-0.0112	-241.61	-563.96	-798.88
	(0.0088)	(0.0385)	(0.0412)	(0.0323)	(488.27)	(737.83)	(1107.98)
P-value, test of diff.	0.000	0.685	0.58	0.329	0.385	0.222	0.234
CCM, Minority		0.692	0.729	0.839	3644	6082	9728
CCM, White		0.753	0.715	0.865	4406	6611	11011

Notes: N = 37,653 Minority youth and 5,366 White youth, with 390 observations dropped due to missing race/ethnicity. Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. 69 observations adjusted in year 1, 237 in year 2, and 252 cumulatively. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table 8: Joint Employment and Graduation Outcomes for Minority and White Youth

		Panel A: Any On-Time Graduation			
		Ever Work, On-time	Never Work, On-time	Ever Work, Not On-time	Never Work, Not On-time
ITT, Minority		-0.0069	-0.0134**	0.0244***	-0.0037
		(0.0075)	(0.0053)	(0.0059)	(0.0035)
ITT, White		-0.0076	0.0142	-0.0118	0.0071
		(0.0232)	(0.0199)	(0.0137)	(0.0088)
P-value, test of diff.		0.976	0.182	0.015	0.254
CM, Minority		0.688	0.114	0.153	0.046
CM, White		0.752	0.142	0.084	0.023
	First Stage				
IV, Minority	0.4420***	-0.0156	-0.0311***	0.0545***	-0.0083
	(0.0061)	(0.0170)	(0.0119)	(0.0134)	(0.0078)
IV, White	0.3485***	-0.0218	0.0367	-0.0366	0.0204
	(0.0192)	(0.0667)	(0.0573)	(0.0395)	(0.0253)
P-value, test of diff.	0.000	0.928	0.247	0.029	0.278
CCM, Minority		0.734	0.130	0.100	0.037
CCM, White		0.801	0.094	0.113	0
		Panel B: Any Graduation			
		Ever Work, Graduated	Never Work, Graduated	Ever Work, Not Graduated	Never Work, Not Graduated
ITT, Minority		0.0066	-0.0157***	0.0111**	-0.0008
		(0.0071)	(0.0054)	(0.0051)	(0.0032)
ITT, White		-0.0071	0.0171	-0.0125	0.0057
		(0.0228)	(0.0201)	(0.0122)	(0.0082)
P-value, test of diff.		0.567	0.116	0.075	0.460
CM, Minority		0.732	0.121	0.109	0.038
CM, White		0.766	0.143	0.070	0.021
IV, Minority		0.0149	-0.0366***	0.0245**	-0.0026
		(0.0161)	(0.0122)	(0.0115)	(0.0073)
IV, White		-0.0206	0.0445	-0.0381	0.0133
		(0.0656)	(0.0579)	(0.0354)	(0.0237)
P-value, test of diff.		0.600	0.171	0.093	0.520
CCM, Minority		0.757	0.137	0.076	0.030
CCM, White		0.813	0.086	0.101	0

Notes: N = 13,457 Minority youth and 1,211 White youth, with 58 observations in graduation data dropped due to missing race/ethnicity. CM shows control means; CCM shows control complier means (rounded to 0 if estimate is negative). Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Figure 1: Experimental Flow Chart
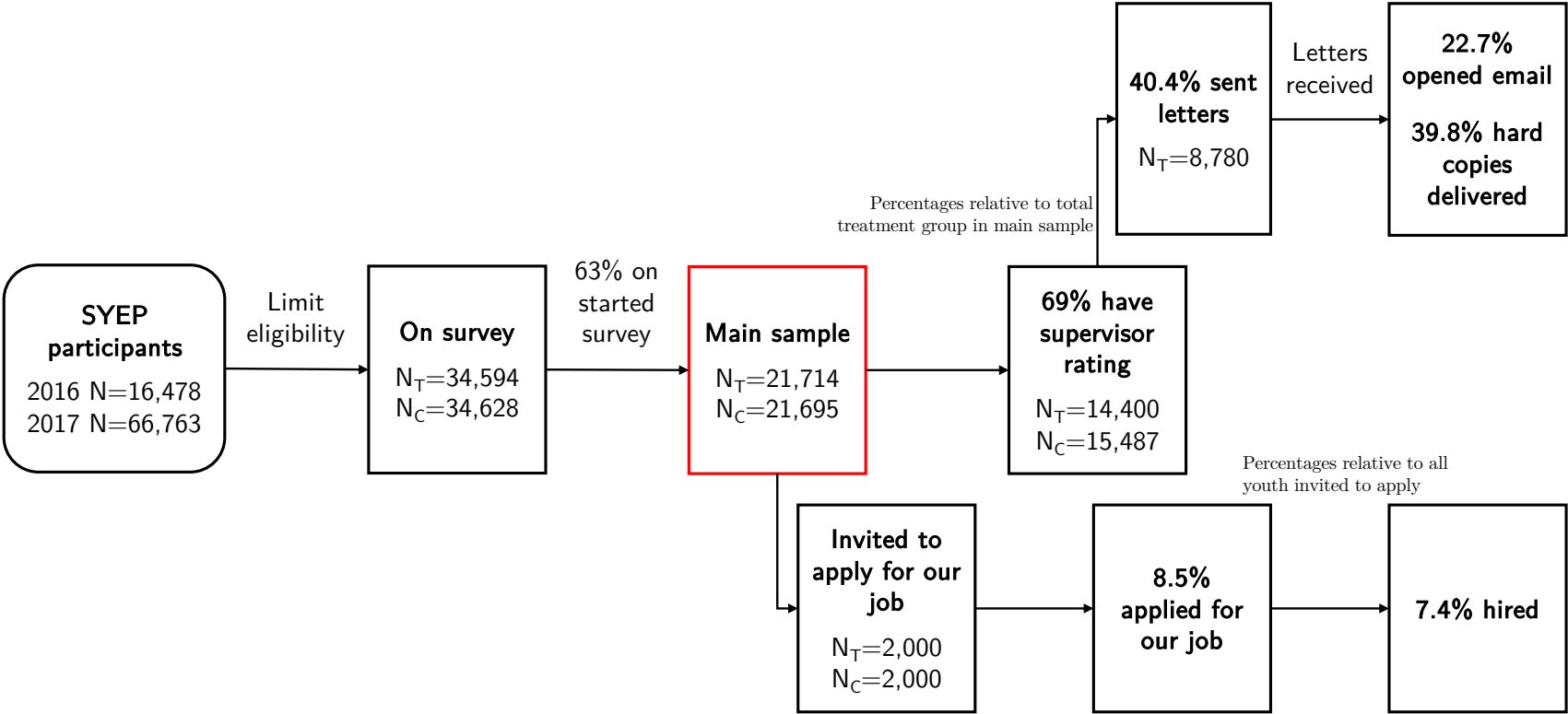
Figure 2: Screen Shots from Treatment Youth Questions on Employer Survey

Sara Heller

We are interested in how Sara Heller performed while working for you as part of the New York City Summer Youth Employment Program.

Overall, how would you rate Sara Heller as an employee?

| Very poor | Poor | Neutral | Good | Very good | Excellent | Exceptional |

If you would like to create a letter of recommendation for Sara Heller, please click >>, below, to continue. If you would prefer not to recommend this youth, please select "No" below and then click >>.

I would like to create a letter of recommendation for Sara Heller.

◉ Yes
○ No

Sara Heller

How often did Sara Heller arrive on time for work?

| Never | Sometimes | Usually | Almost Always | Always |

How often did Sara Heller complete work-related duties in a timely manner?

| Never | Sometimes | Usually | Almost Always | Always |

How was Sara Heller at communicating?

| Not effective | Somewhat effective | Effective | Very effective | Incredibly effective |

How was Sara Heller at following instructions?

| Very poor | Poor | Neutral | Good | Excellent |

Which of these describe Sara Heller? Please select all that apply.

☐ Takes initiative
☐ Trustworthy
☐ Respectful
☐ Works well in teams
☐ Good at responding to criticism
☐ Responsible

Given enough resources, would you hire Sara Heller as a regular employee?

○ Yes, I would
○ No, I would not

Figure 3: Example Letter of Recommendation

NYC
Department of
Youth & Community
Development

November 1, 2017

To Whom It May Concern:

Sara Heller worked for me at the Wharton School during the summer of 2017. Overall, Sara was an exceptional employee.

With regard to reliability, Sara was always on time to work. Sara always completed work related tasks in a timely manner.

When it came to interpersonal interaction, Sara was an incredibly effective communicator. Sara was excellent at following instructions.

In addition to Sara's other strengths, Sara takes initiative, is trustworthy, is respectful, works well in teams, is good at responding to constructive criticism, and is responsible.

Given the resources, I would hire Sara as a full-time employee. I invite you to contact me if you would like more information. I can be reached at 215-898-7696 or judd.kessler@wharton.upenn.edu.

Sincerely,

Judd Kessler
The Wharton School

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

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Note: This recommendation letter is part of a pilot program being run by the New York City Department of Youth and Community Development. Some youth were randomly selected to be part of the pilot. These youth were eligible to receive a letter of recommendation, which reflects supervisor feedback about each individual's job performance.

ONLINE APPENDIX

The Effects of Letters of Recommendation in the Youth Labor Market

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A.1 Additional Labor Market Results, Main Sample

A.1.1 Employment and Earnings

The main text reports annual and cumulative earnings results for two functional forms of the earnings variable: winsorized at the 99.5th percentile and log(earnings+1). Table A.1 shows other transformations of the raw dollar amounts, including an alternative winsorization (at the 99th percentile), alternative intercepts added to earnings prior to logging (0.1, 10, and 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 estimate somewhat. Cumulative earnings increases for compliers range from 9.5 to 23 percent, all significant at the 5 percent level. The results suggest that although there is some uncertainty about how big the proportional change in earnings is, the basic conclusion of a significant increase in earnings is robust to different functional form choices. We emphasize the 4 percent earnings increase from the winsorized version in the main text both because it was our pre-specified primary outcome, and because it is the most conservative result.

Some additional nuance in the pattern of labor market results is shown in Figure A.7, which breaks down both the employment and earnings results by quarter rather than by year. Although the quarterly results are fairly imprecise, two general patterns are of interest. First, we would expect the cumulative employment effect to go to zero over time: almost everyone in the sample is likely to work at some point, leaving no more room for letters to change whether someone ever worked. But as Panel A shows, it still appears that treatment youth work more in each quarter when measured non-cumulatively; in every quarter the point estimate is positive. Second, the effects are not concentrated during the summer quarters (quarters 3 and 7, as indicated by the much higher control means listed at the bottom of the graph). Rather, it appears that the letters increase employment throughout the year. Similarly, Panel B shows that the earnings effects also do not fade out over time. Point estimates are increasing in dollar amounts, while remaining proportionally similar relative to the growing control means.

A.1.2   Spell Length

The main text argues that the mechanism underlying the employment improvements involves employers updating their beliefs. If employers' previous experience is that only extraordinary employees include letters of recommendation in their applications, there could be a risk that employers over-update (i.e., believe treated applicants will be more productive workers than they actually will be). Table 3 in the main text shows that the recommendation letters increased the number of quarters worked and decreased the time until the first quarter worked without increasing the number of job spells, which implies that youth must be working longer in their jobs.

Table A.2 directly confirms that youth are working longer without switching jobs, providing further evidence that the letters are not generating worse matches. 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 forth. 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. 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 the first and second spells is positive, but only statistically significant for the second spell, which increases by 0.11 quarters or about 6 percent for letter recipients.

Because part of the treatment effect is to help youth find jobs more quickly, it is possible that total spell length is biased from differential censoring at the time our data end; that is,

we may observe treatment spells for more quarters after their start than we do for control spells. The second column tests this possibility by using an indicator for whether a spell is censored by the end of our data (i.e., whether a youth was still working at an employer in the last quarter we have in the data). Point estimates are all substantively quite small and not statistically significant, suggesting that differential censoring is not biasing our spell length results; average spells are short enough that we observe all of them despite the earlier start for treatment youth. The last 3 columns of the table confirm that the results are robust to looking only at spells that are not censored, reporting treatment effects on whether a spell lasts at least 2, 3, or 4 quarters conditional on observing all the quarters. Overall, there is no evidence that letters are creating bad matches, but rather they appear to be generating longer job durations.

A.1.3   Employer Type

Tables A.3 and A.4 separate employment and earnings effects by the 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 or engaged with future SYEP activities or other term-time work where DYCD was the employer of record.

Table A.3 shows this is not the main driver of the program effects by reporting 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 the SYEP agency. Earnings impacts are directionally much larger at non-DYCD employers, on the order of 5 rather than 0.5 percent, though too imprecise to draw strong conclusions.

Table A.4 explores what types of industries letter recipients work more in. The classification across industry clusters is based on Gelber, Isen, and Kessler (2016), which groups industries that are more commonly part of SYEPs like childcare and landscaping (cluster 1) separately from industries that are under-represented in SYEPs such as retail and food service (cluster 2). Directionally, letters seem to increase employment in both types of industries, with earnings increases concentrated in cluster 2 jobs. This pattern suggests that the letters are helping young people find jobs even outside of the industries that were most likely to be their SYEP job.

A.2   Additional Job Application Results

Table A.5 shows that observable baseline characteristics are balanced for the randomly selected subsample to whom we sent our job advertisement. No more differences are significant than we would expect by chance, and the joint F-test fails to reject the null hypothesis that

the groups are the same ($p = 0.215$). Notice, however, that there is some chance imbalance on the proportion of the subsample that is Hispanic, with 32.8 percent of the treatment group and 29 percent of the control youth in this category ($p = 0.01$).

As we show below, labor market impacts are biggest for Hispanic youth. As a result, the chance imbalance on ethnicity means that the labor market effects are somewhat larger in our job application subsample than in our main sample, despite being a randomly selected subset (see Table A.6). Consistent with the argument in the main text that the increase in employment is what slows down progress in school, point estimates on education outcomes are also more negative in the job application subsample (see Table A.7), though not distinguishable from zero in part from the reduced sample size.

A.3 Differences by Employer Ratings

Table A.8 shows the treatment-control difference on baseline characteristics for the subset of our main sample that received a rating from a supervisor. 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 on the same page. This aspect of our design makes it likely that supervisors would use different decisions rules when assessing whether to give a particular youth a rating.

Indeed, the top row of Table A.8 shows that treatment youth are significantly less likely to have been rated by a supervisor (66 versus 71 percent had a rating). Despite the potential for selection into having a rating, the other observable characteristics are generally still balanced, with the joint F-test failing to reject equality across all observables ($p = 0.605$). Table A.9, 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 ($p = 0.094$).

Because of the dramatic difference in having a rating and the small imbalance on observables for those with low ratings, we focus on the subsample of rated youth on complete surveys in the main text. But Table A.10 shows that the pattern of heterogeneity in labor market impacts across rating categories is quite similar to those shown in the main text, even when all youth with an employer rating are included.

We have additionally tested whether treatment effects on applying to our job posting are different for those with a high versus low rating. Given that this limits an already reduced sample (N = 4,000) to those with ratings (N = 2,783), 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.

Tables A.11 and A.12 show the equivalent balance tests for the subsample of youth who appeared on a fully completed survey (i.e., where the employer rated every youth on the survey). Although this is a selected group, full survey completion limits the scope for treatment and control youth to be differentially selected into getting a rating. Indeed, the difference in receiving a rating is much smaller in this sample: 31.6 percent for treatment youth and 32.5 percent for controls ($p = 0.066$), driven only by chance differences on how many treatment and control youth were listed on the complete surveys. But as the tables show, observables are entirely balanced within this sample and within each rating group. As a result, this is the subsample we use to assess how treatment effects vary by rating in the main text.

A.4 Education Analysis

A.4.1 Sample Definitions

Because we wrote our pre-analysis plan prior to our conversations with DOE about what data would be available, and prior to matching to our study sample to assess data coverage, our education results are where we deviate most from our PAP. We initially expected to test 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 (other than the selected group that takes Regents exams); and DOE measures graduation and college enrollment only for particular cohorts at particular times.

So instead of forcing different patterns of missing outcomes into a single index, we instead present results for a similar set of outcomes, but separately by outcome for groups defined by data availability. In particular, we define three samples for the education analysis: those expected in high school records, in graduation records, and in college records.

We wish to avoid missing data from students who had already left school, transferred, or attended charter schools. But we can not define our sample based on whether they have

schooling records during outcome years, since treatment could affect enrollment. Instead, we define our high school sample using only baseline characteristics. We identify students who were in public, non-charter schools, attending grades 8–12 in the pre-randomization year, but had not graduated by the August prior to the academic year the study took place. This is the group we would expect to see in high school records if they progressed through high school without transferring or dropping out (we explain how we handle missing data for each outcome in the next section). This "expected in high school" sample leaves us with 19,714 students in our main education analysis, with treatment and control youth equally likely to appear ($\beta = -0.002, p = 0.676$).

For inclusion in the graduation sample, which also underlies the joint outcome analysis in the main text, we limit the "expected in high school" sample to those who were enrolled in a grade that could have reached 4th-, 5th-, or 6th-year graduation prior to the end of our graduation records (the 2019-20 academic year). This effectively excludes pre-randomization 8th graders from our "graduation analysis" sample, since they are not old enough to have graduated by the end of our data period. Note that the graduating cohort is defined by the official 9th grade cohort to which a student belongs per state standards. We do not directly observe which graduation cohort a student is in if they are not in our graduation records, so we limit the sample sample based on whose pre-randomization grade puts them in a graduating cohort for which we would have an observed outcome, if the student were to graduate on time relative to their pre-randomization grade. That means that even students who transferred to other districts during the outcome period will remain in our data; we discuss their outcome definition below. There is no treatment-control difference in the probability of appearing in the graduation sample ($\beta = -0.005, p = 0.379$).

DOE captures post-secondary enrollment data at a single point in time, 6 months after a student reaches their on-time graduation date (i.e., only on-time graduates can have on-time college enrollment). This information is based on data from the National Student Clearinghouse and from CUNY. Because of the timing of this measure, the post-secondary enrollment analysis makes one additional limitation relative to the graduation sample: it also excludes pre-randomization 12th-graders from the "college analysis" sample, since their on-time graduation date makes their college outcome a baseline characteristic (measured just before our letters were distributed). We also have treatment-control balance on the probability of being in this sample ($\beta = -0.002, p = 0.756$).

A.4.2   Outcomes

Because missing data grows over time as students graduate or drop out, we focus our high school outcome analysis on the academic year of random assignment, for which the spring

semester is an outcome (letters were distributed in November or December). To measure high school engagement, we use an indicator for any enrollment, the number of days enrolled, the share of enrolled days actually attended, and GPA on a 100-point scale. We assign 0 days present and 0 days enrolled to students in the expected in high school sample who are missing after treatment, and we analyze non-missing GPAs only. Since there is treatment-control balance on whether someone is in the enrollment, attendance, and GPA data, alternative imputations of missing data would not change results.

To measure on-time graduation, we create an indicator for whether a student in the graduation sample earned a diploma (local, Regents, or Advanced Regents) by her 4th year after initially entering 9th grade.[1a] To measure any graduation, we create an indicator for whether a student earned a diploma at any point during our follow-up period. Note that the data only include graduation information if a student's 4th, 5th, or 6th year for graduation falls between Fall 2015 and Spring 2020. If someone only reached their on-time graduation year during the last year of the data, we will not observe if they graduate after that point, though they will contribute to the "ever graduated" measure. We assign anyone who is part of the graduation sample but missing graduation information a 0 for not receiving a diploma from the NYC DOE. As we report in the main text, there is no treatment effect on transferring, so these 0s will not change the results. We also define a final indicator to measure school persistence, defined as 1 if someone has either graduated or is still in school. This will help to capture those who are still working toward a degree but run into the end of the data before their 6th year for graduation.

We measure any post-secondary enrollment as whether someone is enrolled in a 2- or 4-year institution 6 months after what would have been their on-time graduation date (which is the only timing available in the data). We do not count participation in vocational or public service post-secondary activities as college enrollment. As with graduation, we assign a 0 from anyone who is part of the college sample but missing from the post-secondary data.

A.4.3   Descriptive Statistics

Table A.13 shows descriptive statistics for our "expected in high school" sample. On average, students 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 ($p = 0.151$).

1a. As described above, the 9th grade cohort is determined by the state of New York, so cohorts are defined in the graduation records even when we do not observe the initial 9th grade year in our data.

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 slightly more sensitive to how covariates are included in the regressions (discussed below). However, none of the substantive conclusions change regardless of covariate choice.

A.4.4 Employment Results by Inclusion in the Education Sample

Table A.14 compares labor market impacts for those who are and are not in this expected high school sample. Both groups respond positively to the letters. The employment effects are slightly larger for those in the education sample, though earnings impacts are slightly smaller. None of the differences is statistically significant.

A.4.5 Treatment Effects on Education Outcomes

Table A.15 shows the ITT and TOT impacts of the letters of recommendation on education outcomes for the subset of youth described above. The first four columns show that for students who we expect to be in a DOE high school, in the year we send letters to youth there are no significant changes in enrollment, days enrolled, the share of days enrolled actually attended, or GPA during the academic year. Point estimates are relatively small, with confidence intervals ruling out treatment effects more than 1 percent in either direction.

We explore longer-term educational outcomes in the other columns. While none of the treatment effects is statistically significant, we highlight one pattern that becomes significant for the non-White subgroup and in alternative covariate specifications: on-time (4-year) graduation shows a substantively important decline, while the point estimates on ever graduating (including delayed graduation), graduating or still attending, and enrolling in college on time, are an order of magnitude smaller. The fact that on-time college attendance is not declining in the same way that on-time graduation is suggests that the slow-down in high school completion is among the students who are not going directly to college.

We conclude that there is little evidence that letters improve student performance in school (e.g., by changing teacher or guidance counselor beliefs or encouraging college application). They may, however, slow down progress towards a diploma, likely by pulling marginal students out of school and into the labor force, as discussed in the main text.

Table A.16 shows the same minority/White breakdown for education outcomes as the main text shows for labor market outcomes. The decline in on-time graduation is entirely concentrated among minority high school students, for whom there is a marginally significant

1.7 percentage point (2 percent) decline.[2a] The fact that the negative effect of the letters on education outcomes are concentrated in the same group that sees a positive effect of the letters on labor market outcomes further corroborates the hypothesis that the increase in labor market engagement is generating some decrease in school engagement.

A.5 Heterogeneity

A.5.1 Pre-Specified Categories: Race/Ethnicity, Gender, High School, Age, and Neighborhood

Tables A.17 through A.25 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 attend to the basic patterns here.

Tables A.17 and A.18 show additional labor market results separately for the same division between White and non-White study participants as in the main text. Table A.17 shows the different earnings skewness transformations, and Table A.18 shows impacts on the amount of time and number of spells worked. Consistent with the main results on race/ethnicity, all of these results show that the main labor market effects are concentrated among minority youth. Although we do not generally have the statistical power to distinguish the two groups, all outcomes are statistically differentiable from zero just for non-White participants.

Tables A.19 through A.21 further break down the main labor market results separately by race and ethnicity subcategories. They show that employment impact is driven by somewhat larger effects for Asian and Hispanic youth, with earnings effects suggestively larger for Hispanic youth. Both groups are more likely to get a letter (the first stage in the second panel of each table is larger), but even among compliers the effects are larger for these minority youth.

A similar pattern holds for women (Table A.22), with female SYEP participants significantly more likely to receive a letter, and with compliers having suggestively larger employment effects despite similar or smaller cumulative earnings effects. This finding 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

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2a. The significance of the TOT estimate just barely crosses the $p = 0.1$ threshold, differing from the marginally significant ITT due to small differences from covariate adjustment.

male counterparts, employment rates for women are considerably higher than for men in our NYC sample. The fact that there are larger effects for women in both settings where priors are likely to favor or disfavor women may suggest that the effect is not simply about statistical discrimination.

Table A.23 shows effects separately for youth who report still being in high school or not at the time of application.[3a] Employment effects are suggestively bigger for those still in high school at the time of SYEP application, although earnings effects are more similar across the groups.

Table A.24 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 types of outcomes are far from significantly different than older youth.

Lastly, Table A.25 shows effects by neighborhood economic mobility. Using the Opportunity Insights "upward mobility" 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—the unweighted mean across all upward mobility values that fall within the ZCTA. Then we divide the youth into those who live in areas with above and below median mobility, with median defined in-sample). Table A.25 shows labor market impacts for these two groups. There are positive effects for both both those living in above- and below-median neighborhoods, with employment and earnings impacts suggestively larger in places with below-median mobility.

A.5.2 Previous Work Experience

Tabe A.26 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. Although this was not a pre-specified subgroup of interest, we show these results as an exploration into potential mechanisms. In theory, if statistical discrimination is the sole driver of program impacts, we might expect to see bigger effects for the group with more uncertainty about their productivity, i.e., those without other work histories.

3a. Note that this is different than the sample that is expected to be in high school within the education data, on which we report below. Here we use applicants' self-reports, so that we have an education status for everyone, regardless of enrollment in NYC's public school system.

In fact, we see the opposite: point estimates are larger and only differentiable from zero for the group that had previous work experience. As with the gender heterogeneity in the previous section and the rating heterogeneity in the main text, this seems more consistent with the possibility that employers are using the letters to help identify those likely to be higher performers, not just improve their priors about those with the least available information.

A.6 Information on Letters by Subgroup

To help interpret the patterns of results by subgroup, Table A.27 shows some additional information about the letters for the different subgroups in the previous sections. The entire 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., received a rating high enough and had a supervisor agree to produce one); 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 ad. And the fourth column, conditional on the third, shows the proportion of the applicants that uploaded a letter of recommendation (ours or any other).

There is significant variation both in letter receipt and in average ratings. Non-white, female, non-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. To focus on the minority-White difference in the main text, minorities actually 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 actually 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 effects for minorities 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 high school at the time of SYEP application, and relatedly, those who were under 18 versus 18 and older. This likely helps to explain the bigger labor market point estimates for high school youth, who were much more likely to use the letter on our job application than those who were not in school.

A.7 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 of special interest in our pre-analysis plan. This excludes 25,813 young people who were on a survey 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.

This section provides some additional information on who is excluded from the sample and the implications for our analysis. Tables A.28 to A.30 compare our main control group to everyone who was on an unopened survey (treatment and control) on baseline characteristics, main outcome measures, and job application behavior.[4a]

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.28 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.29 shows 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. Table A.30 shows no significant difference on job application behavior, consistent with the argument in the main text that employment status does 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.

Tables A.31 and A.32 show our main results without excluding those on unopened surveys. As expected given that this adds solely untreated individuals regardless of treatment status (i.e., massively increases the non-compliance in the sample), effects are uniformly smaller and less significant. The main sample's point estimates are within the confidence intervals of these point estimates, also consistent with the fact that the inclusion of unopened

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4a. We invited an additional subset of young people not in our main sample to apply as a check on whether their job application behavior was different.

surveys just adds noise.

A.8   Robustness to Different Covariate Choices

The main text uses the post-double-selection LASSO (Belloni, Chernozhukov, and Hansen 2014a, 2014b; Belloni et al. 2012) to choose which covariates are included in each regression. 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, covariates 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 a high school graduate; being 15-16, 17-18, 19-20, or 21 and up; being part of the Ladders for Leaders program; being Hispanic, Asian, White, other, or having missing race/ethnicity; not being matched to the education data; and being in the expected high school sample. For the education outcomes, covariates 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 a high school graduate; being 15-16, 17-18, 19-20, or 21 and up; being part of the Ladders for Leaders program; and being Hispanic, Asian, White, other, or having missing race/ethnicity.

Both Tables A.33 and A.34 with no covariates and Tables A.35 and A.36 with all covariates show substantively the same conclusions as the main results. Because of the imbalance in several education baseline covariates discussed in section A.4, the point estimates on GPA and on-time graduation become somewhat larger and more significant with these different covariates included. It is for partly this reason that we take the decline in on-time graduation seriously as a main result, even though it is not statistically significant on average in our main results.

A.9 Additional Figures and Tables

Figure A.1: Example Employer 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

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Figure A.2: Screen Shots from Beginning of Employer 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: Screen Shots from Control Youth Rating on Employer Survey

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

NYC
**Department of
Youth & Community
Development**

November 1, 2017

Sara Heller
123 Arbor Street
New York City, NY 92323

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

Figure A.5: Example Job Advertisement Email

Youth Job Opening!

Dear **Sara**:

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

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

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

Qualifications:

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

Compensation for the job is $15/hour.

If you are **Sara Heller**, click this link to apply **(application due by March 30th)**:
Click here for your personal job application

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

A-19

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: Labor Market Effects by Quarter

Panel A: Employment

Panel B: Earnings
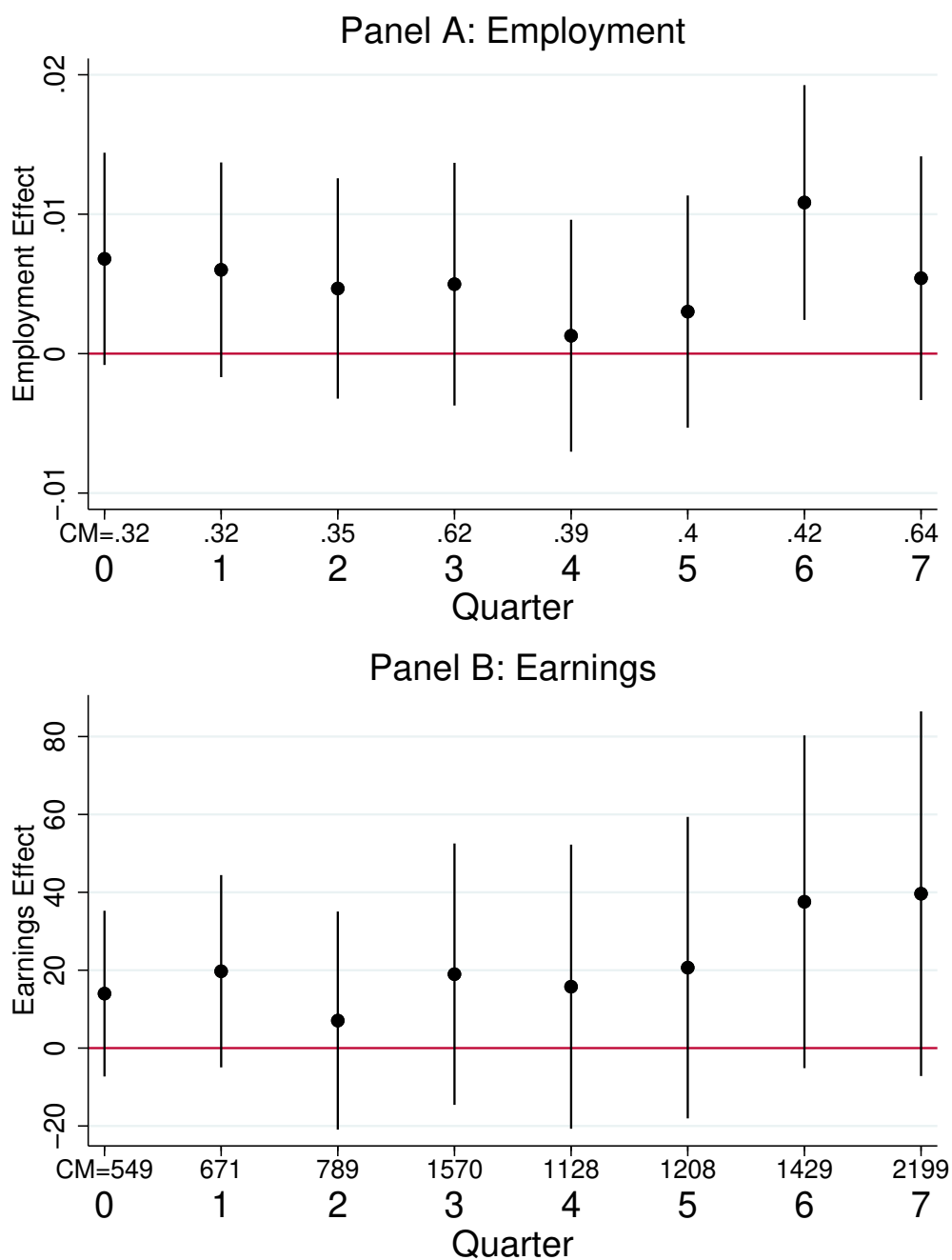
Note: Figures shows treatment effects on employment and earnings by quarter, with 95 percent confidence intervals calculated from cluster-robust standard errors. CM below axis displays the control mean in that quarter. All effects from regressions that include baseline covariates.

Table A.1: Earnings Impacts across Different Skewness Adjustments

Year	1	2	Cumulative
	Panel A: Winsorized at 99th Percentile		
ITT	57.80	109.62	166.88
	(44.86)	(70.87)	(103.63)
CM	3567	5913	9479
Sent Letter (IV)	148.61	280.14	426.40*
	(110.87)	(175.12)	(256.06)
CCM	3718	6112	9833
	Panel B: Log(Earnings + 0.1)		
ITT	0.124***	0.073	0.093**
	(0.042)	(0.044)	(0.037)
CM	4.92	5.44	6.97
Sent Letter (IV)	0.306***	0.18	0.230**
	(0.104)	(0.110)	(0.092)
CCM	4.94	5.56	7.02
	Panel C: Log(Earnings + 10)		
ITT	0.066***	0.045*	0.057**
	(0.024)	(0.026)	(0.022)
CM	6.30	6.73	7.7
Sent Letter (IV)	0.163***	0.112*	0.141**
	(0.058)	(0.064)	(0.055)
CCM	6.34	6.81	7.76
	Panel D: Log(Earnings + 100)		
ITT	0.038**	0.031*	0.039**
	(0.015)	(0.017)	(0.016)
CM	7.03	7.41	8.10
Sent Letter (IV)	0.094**	0.077*	0.095**
	(0.037)	(0.043)	(0.038)
CCM	7.07	7.47	8.15
	Panel E: Asinh(Earnings)		
ITT	0.104***	0.063*	0.080**
	(0.035)	(0.038)	(0.032)
CM	6.10	6.58	7.91
Sent Letter (IV)	0.256***	0.156*	0.198**
	(0.088)	(0.094)	(0.079)
CCM	6.12	6.69	7.97

Notes: N = 43,409. Winsorization in Panel A recodes each quarter's earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.2: Spell Length and Censoring

	Total Spell Length	Spell Censored	Lasts at Least 2 Qtrs	Lasts at Least 3 Qtrs	Lasts at Least 4 Qtrs
			Spell 1		
ITT	0.0238	-0.0052	0.0010	0.0053	-0.0003
	(0.0289)	(0.0044)	(0.0048)	(0.0052)	(0.0051)
CM	3.22	0.23	0.63	0.47	0.37
IV	0.0592	-0.0129	0.0025	0.0134	-0.0002
	(0.0704)	(0.0107)	(0.0117)	(0.0125)	(0.0122)
CCM	3.41	0.25	0.65	0.50	0.41
N	36647	36647	33546	32626	31957
			Spell 2		
ITT	0.0435**	0.0011	0.0024	0.0135*	0.0180**
	(0.0187)	(0.0059)	(0.0072)	(0.0077)	(0.0074)
CM	1.99	0.49	0.63	0.38	0.26
IV	0.1058**	0.0023	0.0054	0.0324*	0.0425**
	(0.0453)	(0.0143)	(0.0171)	(0.0184)	(0.0177)
CCM	1.99	0.50	0.64	0.39	0.25
N	25203	25203	18003	15788	14154
			Spell 3		
ITT	-0.0043	0.0065	-0.0042	-0.0055	-0.0031
	(0.0208)	(0.0085)	(0.0105)	(0.0115)	(0.0110)
CM	1.77	0.55	0.61	0.35	0.22
IV	-0.0111	0.0154	-0.0124	-0.0132	-0.0073
	(0.0499)	(0.0203)	(0.0250)	(0.0276)	(0.0262)
CCM	1.81	0.56	0.64	0.39	0.23
N	12820	12820	8660	6903	5604

Notes: Total spells conditional on having that spell. Indicators for at least X quarters conditional on observing for at least X quarters. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

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

	DYCD			Non-DYCD Employers		
Year	1	2	Cumulative	1	2	Cumulative
	Panel A: Employment					
ITT	0.0046	0.0033	0.0027	0.0088**	0.0008	0.0018
	(0.0046)	(0.0041)	(0.0046)	(0.0040)	(0.0043)	(0.0041)
CM	0.4157	0.262	0.5011	0.4256	0.5655	0.6218
Sent Letter (IV)	0.0119	0.0079	0.007	0.0221**	0.0021	0.0045
	(0.0114)	(0.0101)	(0.0114)	(0.0100)	(0.0106)	(0.0101)
CCM	0.419	0.253	0.507	0.429	0.588	0.639
	Panel B: Earnings, Winsorized at 99.5th Percentile					
ITT	0.92	3.04	4.11	61.05	105.82	166.86
	(10.68)	(9.94)	(16.66)	(46.23)	(73.89)	(108.01)
CM	810	572	1382	2770	5391	8161
Sent Letter (IV)	1.54	7.22	9.45	150.90	268.63	421.21
	(26.41)	(24.56)	(41.20)	(114.23)	(182.63)	(267.00)
CCM	870	574	1444	2861	5593	8453

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 earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.4: Labor Market Effects by Industry Cluster

Year	SYEP-Related Industries (Cluster 1)			Other Industries (Cluster 2)		
	1	2	Cumulative	1	2	Cumulative
	Panel A: Employment					
ITT	0.0049	0.0063	0.0026	0.0071*	0.001	0.0007
	(0.0047)	(0.0047)	(0.0045)	(0.0040)	(0.0044)	(0.0044)
CM	0.5243	0.4407	0.6495	0.3105	0.4256	0.4869
Sent Letter (IV)	0.0119	0.015	0.0061	0.0181*	0.003	0.0022
	(0.0116)	(0.0116)	(0.0111)	(0.0099)	(0.0110)	(0.0108)
CCM	0.536	0.443	0.665	0.307	0.436	0.496
	Panel B: Earnings, Winsorized at 99.5th Percentile					
ITT	19.01	3.09	21.53	34.22	108.76*	135.06
	(31.19)	(49.22)	(71.96)	(40.93)	(65.37)	(95.57)
CM	1658	2261	3919	1882	3637	5518
Sent Letter (IV)	51.02	8.13	60.24	85.19	275.02*	341.83
	(77.07)	(121.69)	(177.89)	(101.20)	(161.58)	(236.30)
CCM	1812	2451	4262	1881	3621	5521

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 earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.5: Descriptive Statistics, Job Application Sample

	Control Mean	Treatment Mean	Test of Difference
N	2000	2000	
Age	17.1	17.2	0.205
Male	0.421	0.444	0.151
Black	0.405	0.385	0.196
Hispanic	0.290	0.328	0.010
Asian	0.140	0.125	0.160
White	0.125	0.124	0.928
Other Race	0.040	0.038	0.789
In High School	0.759	0.753	0.686
HS Graduate	0.045	0.043	0.758
In College	0.173	0.173	0.967
Not in UI Data	0.005	0.013	0.011
Never Employed Pre-SYEP	0.448	0.456	0.611
Ever Worked, Year -4	0.149	0.166	0.129
Earnings, Year -4	294	343	0.425
Ever Worked, Year -3	0.278	0.279	0.944
Earnings, Year -3	605	626	0.777
Ever Worked, Year -2	0.439	0.445	0.726
Earnings, Year -2	1122	1093	0.759
Ever Worked, Year -1	0.976	0.968	0.105
Earnings, Year -1	2519	2496	0.842
No Education Match	0.136	0.136	0.963
In HS Sample	0.451	0.449	0.899
Joint F-test	F(24, 3975) = 1.216, p=.215		

N = 4,000. Test of difference reports the p-value from a regression of each characteristic on a treatment indicator, using standard errors clustered on individual.

Table A.6: Labor Market Effects, Job Application Sample

Year		1	2	Cumulative
		Panel A: Employment		
	ITT	0.0209	0.015	0.0069
		(0.0136)	(0.0135)	(0.0109)
	CM	0.691	0.725	0.849
Sent Letter (IV)		0.0532	0.034	0.0176
		(0.0348)	(0.0345)	(0.0278)
	CCM	0.663	0.718	0.855
		Panel B: Earnings, Winsorized at 99.5th Percentile		
	ITT	319.86*	440.54*	779.72**
		(163.75)	(253.32)	(376.35)
	CM	3637	6129	9766
Sent Letter (IV)		780.27*	1121.22*	1870.48*
		(416.45)	(645.57)	(956.04)
	CCM	3388	5627	9046
		Panel C: Log(Earnings + 1)		
	ITT	0.187*	0.157	0.122
		(0.109)	(0.116)	(0.096)
	CM	5.55	6.14	7.41
Sent Letter (IV)		0.477*	0.401	0.311
		(0.277)	(0.295)	(0.245)
	CCM	5.35	6.01	7.38

Notes: N = 4,000. Winsorization in Panel B recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.7: Education Effects, Job Application Sample

	Ever Enrolled Y1	Days Enrolled Y1	Share Days Present Y1	GPA Y1	On-time Graduation	Ever Graduated	Graduated or Still Attending	On-time College
ITT	-0.0021	-0.2837	-0.0015	-0.0245	-0.0202	-0.0081	-0.0185	-0.0229
	(0.0102)	(1.7969)	(0.0089)	(0.3514)	(0.0146)	(0.0138)	(0.0138)	(0.0217)
CM	0.940	149.9	0.830	79.82	0.851	0.899	0.911	0.748
N	1800	1800	1800	1678	1237	1237	1237	1187
Sent Letter (IV)	-0.005	-0.6684	-0.0035	-0.0576	-0.0451	-0.0181	-0.0414	-0.0486
	(0.0240)	(4.2331)	(0.0209)	(0.8250)	(0.0327)	(0.0309)	(0.0309)	(0.0488)
CCM	0.958	153.1	0.860	81.84	0.908	0.928	0.951	0.797
N	1800	1800	1800	1678	1237	1237	1237	1187

Notes: Analysis on all those expected to be observed based on pre-program grade of enrollment (see text for details) who were also randomly selected to be part of our job application sample. On-time graduation equals 1 for public school, non-charter students who graduate within 4 years and do not transfer to GED or other district. Ever graduated includes any 5- and 6-year graduation observed during the follow-up period. College enrollment only measured within 6 months after a student's on-time graduation date, regardless of graduation status. Regressions include baseline covariates. Robust standard errors in parentheses. * p¡0.1, ** p¡0.05, *** p¡0.01

Table A.8: Balance for Those with Employer Ratings

	Control Mean	Treatment Mean	Test of Difference
Has Rating	0.714	0.663	0.000
N	15487	14400	
Overall Rating	5.18	5.16	0.286
Age	17.22	17.19	0.346
Male	0.426	0.427	0.857
Black	0.418	0.413	0.369
Hispanic	0.287	0.289	0.683
Asian	0.131	0.137	0.105
White	0.117	0.115	0.746
Other Race	0.047	0.045	0.365
In High School	0.753	0.751	0.628
HS Graduate	0.042	0.041	0.837
In College	0.180	0.184	0.419
Not in UI Data	0.005	0.005	0.901
Never Employed Pre-SYEP	0.446	0.454	0.164
Ever Worked, Year -4	0.155	0.149	0.213
Earnings, Year -4	320	326	0.787
Ever Worked, Year -3	0.268	0.268	0.991
Earnings, Year -3	582	589	0.803
Ever Worked, Year -2	0.442	0.438	0.527
Earnings, Year -2	1077	1065	0.691
Ever Worked, Year -1	0.981	0.988	0.000
Earnings, Year -1	2415	2438	0.565
No Education Match	0.119	0.117	0.577
In HS Sample	0.455	0.458	0.625
Joint F-test	F(25, 28919) = .901, p=.605		

Top row shows regression of an indicator for having an employer rating on treatment for full sample (n=43,409). Rest of table uses subset of main sample with non-missing employer ratings (N=29,887, 256 youth missing race/ethnicity). 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.9: Balance for Those with Employer Ratings 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	0.009	0.009	0.902	0.003	0.004	0.696
Never Employed Pre-SYEP	0.461	0.489	0.007	0.438	0.437	0.871
Ever Worked, Year -4	0.145	0.129	0.024	0.159	0.159	0.964
Earnings, Year -4	272	269	0.935	343	353	0.728
Ever Worked, Year -3	0.255	0.236	0.037	0.274	0.282	0.177
Earnings, Year -3	507	477	0.432	618	642	0.487
Ever Worked, Year -2	0.424	0.403	0.037	0.450	0.454	0.523
Earnings, Year -2	995	879	0.024	1117	1153	0.397
Ever Worked, Year -1	0.965	0.979	0.000	0.989	0.993	0.014
Earnings, Year -1	2200	2134	0.269	2519	2583	0.216
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	$F(24, 9587) = 1.397$, p=.094			$F(24, 19643) = .725$, p=.831		

Sample includes all youth with employer rating (N=29,887, 256 youth missing race/ethnicity). Low includes rating categories 1-4; high includes 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.10: Labor Market Effects for Youth with High and Low Employer Ratings, All Rated Youth

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Low Ratings		0.0136	0.0006	0.0075	12.68	26.53	34.89
		(0.0089)	(0.0089)	(0.0073)	(88.92)	(143.15)	(208.89)
ITT, High Ratings		0.0123**	0.0099*	0.0101**	138.15*	187.37*	324.33**
		(0.0059)	(0.0060)	(0.0047)	(70.52)	(112.15)	(164.20)
P-value, test of diff.		0.905	0.383	0.765	0.269	0.376	0.276
CM, Low		0.673	0.721	0.836	3109	5409	8518
CM, High		0.715	0.720	0.846	3729	6251	9979
	First Stage						
IV, Low Ratings	0.3067***	0.0438	0.0017	0.024	22.96	92.65	114.00
	(0.0068)	(0.0290)	(0.0290)	(0.0238)	(289.28)	(466.86)	(681.24)
IV, High Ratings	0.7529***	0.0162**	0.0131*	0.0134**	181.27*	251.10*	431.32**
	(0.0043)	(0.0079)	(0.0079)	(0.0063)	(93.59)	(148.98)	(218.01)
P-value, test of diff.	0.000	0.359	0.704	0.666	0.602	0.746	0.657
CCM, Low		0.642	0.71	0.802	3180	5343	8525
CCM, High		0.713	0.733	0.851	3576	6241	9818

Notes: Sample includes all youth who were rated on a survey (N = 29,887). Low includes rating categories 1-4; high includes 5-7. Test of difference shows p-value for null hypothesis that treatment effects are equal in low- and high-rated groups. Winsorization in Panel B recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.11: Balance for Those with Employer Ratings, Fully Completed Surveys

	Control Mean	Treatment Mean	Test of Difference
On Complete Survey	0.325	0.316	0.066
N	7042	6869	
Overall Rating	5.22	5.20	0.498
Age	17.21	17.19	0.609
Male	0.413	0.418	0.492
Black	0.425	0.428	0.713
Hispanic	0.283	0.282	0.854
Asian	0.150	0.157	0.193
White	0.094	0.087	0.139
Other Race	0.048	0.045	0.491
In High School	0.751	0.750	0.910
HS Graduate	0.038	0.034	0.173
In College	0.188	0.193	0.478
Not in UI Data	0.005	0.005	0.649
Never Employed Pre-SYEP	0.460	0.468	0.311
Ever Worked, Year -4	0.145	0.142	0.642
Earnings, Year -4	321	304	0.595
Ever Worked, Year -3	0.254	0.257	0.683
Earnings, Year -3	560	559	0.974
Ever Worked, Year -2	0.426	0.417	0.257
Earnings, Year -2	1013	993	0.648
Ever Worked, Year -1	0.983	0.988	0.029
Earnings, Year -1	2358	2304	0.317
No Education Match	0.102	0.103	0.860
In HS Sample	0.472	0.467	0.556
Joint F-test	F(25, 13666) = .697, p=.865		

Top row shows regression of indicator for whether an individual was on a fully completed survey on treatment for full sample (n=43,409). Rest of table uses subset of main sample who were on a fully completed survey (N=13,911, 167 youth missing race/ethnicity). 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.12: 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	0.007	0.008	0.877	0.004	0.004	0.597
Never Employed Pre-SYEP	0.481	0.481	0.969	0.450	0.462	0.222
Ever Worked, Year -4	0.134	0.125	0.392	0.150	0.149	0.963
Earnings, Year -4	268	266	0.974	346	320	0.527
Ever Worked, Year -3	0.242	0.234	0.540	0.259	0.266	0.394
Earnings, Year -3	466	479	0.838	603	594	0.854
Ever Worked, Year -2	0.407	0.395	0.435	0.435	0.426	0.388
Earnings, Year -2	933	839	0.224	1050	1060	0.851
Ever Worked, Year -1	0.971	0.980	0.048	0.989	0.991	0.327
Earnings, Year -1	2102	2063	0.650	2475	2410	0.342
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	$F(24, 4264) = .897$, p=.607			$F(24, 9471) = .91$, p=.588		

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 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.13: Descriptive Statistics, Expected in High School Sample

	N	Control Mean	Treatment Mean	Test of Difference
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 Graduation Sample	19714	0.750	0.744	0.379
In College Sample	19714	0.711	0.709	0.756
Not in UI Data	19714	0.013	0.015	0.275
Never Employed Pre-SYEP	19714	0.614	0.621	0.271
Ever Worked, Year -4	19714	0.041	0.040	0.688
Earnings, Year -4	19714	77	96	0.342
Ever Worked, Year -3	19714	0.134	0.134	0.933
Earnings, Year -3	19714	180	193	0.555
Ever Worked, Year -2	19714	0.305	0.304	0.865
Earnings, Year -2	19714	431	421	0.682
Ever Worked, Year -1	19714	0.959	0.960	0.614
Earnings, Year -1	19714	1579	1563	0.594
Joint F-test		F(37, 19063) = 1.239, p=.151		

Notes: Analysis on all those expected to be observed based on pre-program grade of enrollment (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.14: Employment and Earnings Effects for Our Expected in HS Sample

		Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Expected in HS Data		0.0144**	0.0126**	0.0098*	9.74	77.99	80.75
		(0.0065)	(0.0064)	(0.0054)	(43.58)	(77.30)	(107.19)
ITT, Not Expected in HS Data		0.0112**	0.0001	0.0065	104.26	136.85	241.59
		(0.0052)	(0.0053)	(0.0042)	(75.95)	(117.94)	(174.80)
P-value, test of diff.		0.696	0.132	0.624	0.281	0.677	0.433
CM, Exp. In HS		0.636	0.677	0.810	2120	3908	6028
CM, Not Exp. In HS		0.755	0.756	0.866	4794	7675	12469
	First Stage						
ITT, Expected in HS Data	0.4138***	0.0349**	0.0306**	0.0237*	31.73	208.38	239.09
	(0.0049)	(0.0158)	(0.0155)	(0.0130)	(105.45)	(186.83)	(259.02)
ITT, Not Expected in HS Data	0.3966***	0.0282**	0.0002	0.0157	259.84	345.29	604.01
	(0.0045)	(0.0132)	(0.0135)	(0.0106)	(191.00)	(297.03)	(440.06)
P-value, test of diff.	0.01	0.743	0.139	0.632	0.296	0.697	0.476
CCM, Exp. in HS		0.644	0.680	0.819	2302	4072	6375
CCM, Not Exp. In HS		0.743	0.770	0.860	4960	7964	12924

Notes: Analysis on all those expected to be observed based on pre-program grade of enrollment (see text for details). Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.15: Education Effects

	Ever Enrolled Y1	Days Enrolled Y1	Share Days Present Y1	GPA Y1	On-time Graduation	Ever Graduated	Graduated or Still Attending	On-time College
ITT	-0.0019	-0.3152	0.0014	-0.1300	-0.0066	0.0003	-0.0019	0.0005
	(0.0030)	(0.5444)	(0.0028)	(0.0988)	(0.0043)	(0.0041)	(0.0040)	(0.0062)
CM	0.946	150.0	0.829	80.13	0.809	0.858	0.884	0.697
N	19714	19714	19714	18237	14726	14726	14726	13999
Sent Letter (IV)	-0.0045	-0.7681	0.0033	-0.3025	-0.015	0.0009	-0.0044	0.0014
	(0.0072)	(1.3126)	(0.0067)	(0.2367)	(0.0099)	(0.0095)	(0.0092)	(0.0141)
CCM	0.957	153.1	0.849	81.75	0.839	0.876	0.905	0.721
N	19714	19714	19714	18237	14726	14726	14726	13999

Notes: Analysis on all those expected to be observed based on pre-program grade of enrollment (see text for details). 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 includes any 5- or 6-year graduation for the same group if it occurs during the follow-up period. College enrollment only measured within 6 months after a student's on-time graduation date, regardless of graduation status. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.16: Education Effects for Minority and White Youth

	First Stage	Ever Enrolled Y1	Days Enrolled Y1	Share Days Present Y1	GPA Y1	On-time Graduation	Ever Graduated	Graduated or Still Attending	On-time College
ITT, Minority		-0.0017	-0.2870	0.0017	-0.1389	-0.0077*	-0.0005	-0.0033	-0.0004
		(0.0032)	(0.5791)	(0.0029)	(0.1041)	(0.0046)	(0.0044)	(0.0043)	(0.0065)
ITT, White		-0.0000	-0.5683	0.0021	0.0172	0.0058	0.0081	0.0114	0.0076
		(0.0082)	(1.4672)	(0.0079)	(0.3317)	(0.0102)	(0.0096)	(0.0098)	(0.0190)
P-value, test of diff.		0.997	0.859	0.963	0.653	0.232	0.422	0.170	0.692
CM, Minority		0.943	149.3	0.822	79.50	0.801	0.853	0.879	0.686
CM, White		0.974	158.6	0.909	87.70	0.893	0.909	0.934	0.824
	First Stage								
IV, Minority	0.4193***	-0.004	-0.6575	0.004	-0.3361	-0.0172	-0.0010	-0.0073	-0.0009
	(0.0052)	(0.0076)	(1.3777)	(0.0070)	(0.2456)	(0.0105)	(0.0100)	(0.0097)	(0.0147)
IV, White	0.3407***	-0.0046	-1.5909	0.0062	0.0202	0.0174	0.0255	0.0337	0.0255
	(0.0166)	(0.0242)	(4.3503)	(0.0234)	(0.9784)	(0.0295)	(0.0277)	(0.0283)	(0.0545)
P-value, test of diff.	0.000	0.980	0.838	0.930	0.724	0.271	0.370	0.171	0.641
CCM, Minority		0.955	152.5	0.843	81.18	0.834	0.873	0.903	0.713
CCM, White		0.987	160.4	0.907	88.99	0.892	0.898	0.917	0.806

Analysis on all those expected to be observed based on pre-program grade of enrollment (see text for details). 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 includes any 5- or 6-year graduation for the same group if it occurs during the follow-up period. College enrollment only measured within 6 months after a student's on-time graduation date, regardless of graduation status. For those in high school sample, 18,238 minority youth and 1,553 White youth, with 58 observations dropped due to missing race/ethnicity. For graduation sample, 13,539 minority youth and 1,214 White youth, with 58 observations dropped due to missing race/ethnicity. For college sample, 12,852 minority youth and 1,158 White youth, with 56 observations dropped due to missing race/ethnicity. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.17: Earnings Impacts Across Different Skewness Adjustments by Race/Ethnicity

Year	1	2	Cumulative	1	2	Cumulative
	White			Minority		
	Panel A: Winsorized at 99th Percentile					
ITT	-66.60	-115.62	-180.29	76.39	142.25*	218.02**
	(139.80)	(208.70)	(314.35)	(47.28)	(75.37)	(109.75)
CM	3725	5589	9314	3530	5920	9450
Sent Letter (IV)	-230.03	-407.23	-631.33	187.92*	348.55*	534.18**
	(471.52)	(703.61)	(1059.81)	(112.82)	(179.85)	(261.86)
CCM	4353	6402	10750	3638	6051	9692
	Panel B: Log(Earnings + 0.1)					
ITT	0.039	-0.017	-0.032	0.131***	0.084*	0.106***
	(0.115)	(0.131)	(0.105)	(0.045)	(0.048)	(0.040)
CM	5.37	5.10	6.99	4.85	5.48	6.95
Sent Letter (IV)	0.127	-0.077	-0.117	0.313***	0.200*	0.252***
	(0.388)	(0.442)	(0.354)	(0.108)	(0.114)	(0.095)
CCM	5.51	5.38	7.26	4.88	5.57	7.00
	Panel C: Log(Earnings + 10)					
ITT	0.017	-0.009	-0.017	0.070***	0.052*	0.065***
	(0.064)	(0.076)	(0.063)	(0.025)	(0.028)	(0.024)
CM	6.52	6.51	7.68	6.27	6.76	7.70
Sent Letter (IV)	0.055	-0.042	-0.063	0.167***	0.125*	0.154***
	(0.217)	(0.257)	(0.213)	(0.061)	(0.067)	(0.058)
CCM	6.65	6.70	7.88	6.31	6.82	7.75
	Panel D: Log(Earnings + 100)					
ITT	0.007	-0.005	-0.007	0.041**	0.036*	0.043***
	(0.040)	(0.050)	(0.044)	(0.016)	(0.019)	(0.017)
CM	7.13	7.25	8.05	7.01	7.42	8.10
Sent Letter (IV)	0.022	-0.026	-0.029	0.097**	0.086*	0.103***
	(0.136)	(0.169)	(0.147)	(0.038)	(0.044)	(0.040)
CCM	7.25	7.39	8.20	7.05	7.47	8.14
	Panel E: Asinh(Earnings)					
ITT	0.031	-0.014	-0.027	0.110***	0.072*	0.091***
	(0.097)	(0.112)	(0.090)	(0.038)	(0.041)	(0.034)
CM	6.47	6.29	7.92	6.04	6.61	7.91
Sent Letter (IV)	0.102	-0.064	-0.099	0.262***	0.173*	0.218***
	(0.328)	(0.377)	(0.304)	(0.091)	(0.097)	(0.082)
CCM	6.60	6.54	8.17	6.07	6.70	7.96

Notes: N = 43,019, 390 youth excluded for missing race. Winsorization in Panel A recodes each quarter's earnings to the 99th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.18: Amount and Timing of Work by Race/Ethnicity

	Num Quarters Worked	Num of Job Spells	Num of Job Spells if >0	Time to First Qtr Worked
ITT, Minority	0.050**	0.021	0.001	-0.058***
	(0.023)	(0.015)	(0.015)	(0.022)
ITT, White	-0.009	-0.007	0.000	0.015
	(0.059)	(0.036)	(0.035)	(0.049)
P-value, test of diff.	0.354	0.458	0.971	0.175
CM, Minority	3.49	1.99	2.38	2.18
CM, White	3.17	1.90	2.23	2.28
IV, Minority	0.120**	0.051	0.005	-0.138***
	(0.056)	(0.036)	(0.036)	(0.051)
IV, White	-0.030	-0.030	0.000	0.050
	(0.201)	(0.120)	(0.117)	(0.163)
P-value, test of diff.	0.470	0.520	0.965	0.273
CCM, Minority	3.59	1.98	2.35	2.20
CCM, White	3.52	2.00	2.31	2.03
N	43019	43019	36300	36300

Notes: Spells defined as consecutive quarters with earnings from same employer. Time to first quarter conditional on spells > 0. CM shows control means; CCM shows control complier means. Regression includes baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.19: Employment and Earnings Effects for Black and Non-Black Youth

		Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Black		0.0078	0.0046	0.0053	27.10	90.31	129.12
		(0.0064)	(0.0063)	(0.0050)	(68.65)	(108.70)	(158.37)
ITT, Non-Black		0.0160***	0.0065	0.0097**	82.28	120.83	193.93
		(0.0054)	(0.0054)	(0.0045)	(61.30)	(98.22)	(143.45)
P-value, test of diff.		0.324	0.824	0.509	0.549	0.835	0.761
CM, Black		0.715	0.743	0.857	3575	5937	9512
CM, Non-Black		0.691	0.703	0.829	3560	5920	9480
	First Stage						
IV, Black	0.4039***	0.0194	0.0115	0.0128	75.15	238.00	340.56
	(0.0052)	(0.0157)	(0.0155)	(0.0124)	(169.76)	(268.75)	(391.50)
IV, Non-Black	0.4045***	0.0399***	0.0157	0.0239**	205.83	300.39	483.33
	(0.0043)	(0.0133)	(0.0135)	(0.0111)	(151.48)	(242.76)	(354.53)
P-value, test of diff.	0.926	0.321	0.838	0.507	0.566	0.863	0.787
CCM, Black		0.723	0.764	0.867	3728	6294	9995
CCM, Non-Black		0.677	0.703	0.822	3708	6025	9756

Notes: N = 17,636 Black youth and 25,383 non-Black youth with 390 observations dropped due to missing race/ethnicity. Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.20: Employment and Earnings Effects for Hispanic and Non-Hispanic Youth

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Hispanic		0.0162**	0.0069	0.0120*	189.39**	210.23	387.52*
		(0.0077)	(0.0077)	(0.0064)	(88.21)	(139.92)	(206.24)
ITT, Non-Hispanic		0.0113**	0.0053	0.0063	8.15	67.60	79.40
		(0.0049)	(0.0049)	(0.0039)	(53.62)	(85.85)	(124.66)
P-value, test of diff.		0.595	0.862	0.45	0.079	0.385	0.201
CM, Hispanic		0.687	0.718	0.830	3726	6365	10092
CM, Non-Hispanic		0.706	0.720	0.845	3501	5749	9250
First Stage							
IV, Hispanic	0.4152***	0.0390**	0.0164	0.0285*	453.64**	501.29	926.77*
	(0.0062)	(0.0186)	(0.0186)	(0.0154)	(212.35)	(336.73)	(496.49)
IV, Non-Hispanic	0.4000***	0.0285**	0.013	0.0156	27.26	180.69	216.22
	(0.0039)	(0.0122)	(0.0122)	(0.0098)	(133.98)	(214.50)	(311.42)
P-value, test of diff.	0.039	0.635	0.878	0.479	0.089	0.422	0.225
CCM, Hispanic		0.681	0.721	0.826	3759	6396	10184
CCM, Non-Hispanic		0.702	0.731	0.846	3695	6023	9710

Notes: N = 12,427 Hispanic youth and 30,592 non-Hispanic youth with 390 observations dropped due to missing race/ethnicity. Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.21: Employment and Earnings Effects for Asian and Non-Asian Youth

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Asian		0.0249**	0.0023	0.0143	-12.54	83.83	59.05
		(0.0121)	(0.0121)	(0.0101)	(116.75)	(206.75)	(286.56)
ITT, Non-Asian		0.0109**	0.0062	0.0069*	70.73	111.50	184.82
		(0.0044)	(0.0044)	(0.0035)	(49.75)	(78.21)	(115.01)
P-value, test of diff.		0.275	0.762	0.486	0.512	0.9	0.684
CM, Asian		0.643	0.675	0.807	3010	5256	8266
CM, Non-Asian		0.709	0.726	0.846	3649	6026	9675
IV, Asian	0.4830***	0.0516**	0.0047	0.0297	-13.50	194.02	154.14
	(0.0094)	(0.0250)	(0.0250)	(0.0210)	(241.52)	(428.13)	(593.11)
IV, Non-Asian	0.3928***	0.0278**	0.0157	0.0175*	184.22	291.11	481.28
	(0.0035)	(0.0111)	(0.0112)	(0.0090)	(126.60)	(199.01)	(292.61)
P-value, test of diff.	0	0.384	0.687	0.594	0.468	0.837	0.621
CCM, Asian		0.63	0.667	0.79	3129	5169	8324
CCM, Non-Asian		0.708	0.739	0.85	3823	6313	10130

Notes: N = 5,578 Asian youth and 37,441 non-Asian youth with 390 observations dropped due to missing race/ethnicity. Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.22: Employment and Earnings Effects for Male and Female Youth

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Male		0.0045	0.0097	0.004	44.40	154.23	197.37
		(0.0066)	(0.0067)	(0.0056)	(66.40)	(108.86)	(158.56)
ITT, Female		0.0188***	0.0029	0.0109***	71.69	77.20	147.27
		(0.0052)	(0.0052)	(0.0041)	(62.94)	(98.67)	(144.19)
P-value, test of diff.		0.09	0.423	0.326	0.765	0.600	0.815
CM, Male		0.658	0.659	0.802	3015	4997	8012
CM, Female		0.733	0.766	0.870	4000	6684	10684
IV, Male	0.3962***	0.0113	0.0245	0.01	118.63	398.51	520.97
	(0.0051)	(0.0166)	(0.0168)	(0.0142)	(167.44)	(274.52)	(399.62)
IV, Female	0.4106***	0.0457***	0.0071	0.0263***	179.64	197.12	369.99
	(0.0044)	(0.0128)	(0.0127)	(0.0100)	(153.23)	(240.24)	(351.06)
P-value, test of diff.	0.031	0.101	0.410	0.347	0.788	0.581	0.776
CCM, Male		0.675	0.667	0.812	3243	5195	8434
CCM, Female		0.713	0.773	0.862	4079	6858	10944

Notes: Notes: N = 18,539 male youth and 24,870 female youth. Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.23: Employment and Earnings Effects by Self-Reported High School Enrollment

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, High School		0.0149***	0.0107**	0.0095**	66.8671*	99.95	170.2714*
		(0.0050)	(0.0049)	(0.0041)	(39.50)	(66.42)	(94.49)
ITT, Non-High School		0.0059	-0.0091	0.003	44.36	137.33	180.36
		(0.0070)	(0.0075)	(0.0056)	(141.85)	(217.48)	(324.66)
P-value, test of diff.		0.291	0.026	0.348	0.879	0.869	0.976
CM, High School		0.662	0.696	0.822	2492	4473	6965
CM, Non-High School		0.822	0.795	0.898	6934	10566	17499
IV, High School	0.3992***	0.0375***	0.0267**	0.0237**	174.5338*	261.27	435.8011*
	(0.0038)	(0.0124)	(0.0122)	(0.0102)	(98.93)	(166.25)	(236.77)
IV, Non-High School	0.4204***	0.0137	-0.0214	0.0071	94.97	356.81	451.78
	(0.0067)	(0.0166)	(0.0177)	(0.0133)	(336.41)	(515.82)	(769.45)
P-value, test of diff.	0.006	0.253	0.026	0.323	0.821	0.86	0.984
CCM, High School		0.658	0.698	0.823	2565	4549	7114
CCM, Non-High School		0.81	0.816	0.891	7058	10757	17815

Notes: N = 32,703 high school youth and 10,706 non-high school youth (with 1 observation missing education coded as non-high school). Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.24: Employment and Earnings Effects by Age

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Under 18		0.0136**	0.0086	0.0085*	45.02	59.21	104.14
		(0.0055)	(0.0054)	(0.0045)	(37.98)	(66.43)	(92.54)
ITT, 18 and Over		0.0109*	0.0003	0.0066	87.43	191.01	298.05
		(0.0060)	(0.0063)	(0.0048)	(107.02)	(165.19)	(245.86)
P-value, test of diff.		0.745	0.317	0.773	0.709	0.459	0.46
CM, Under 18		0.645	0.686	0.815	2145	3985	6129
CM, 18 and Over		0.798	0.780	0.886	6067	9395	15462
IV, Under 18	0.4027***	0.0337**	0.0213	0.0212*	115.81	155.71	269.76
	(0.0042)	(0.0136)	(0.0134)	(0.0112)	(94.22)	(164.67)	(229.81)
IV, 18 and Over	0.4072***	0.0266*	0.0006	0.0157	218.86	485.31	721.01
	(0.0055)	(0.0148)	(0.0155)	(0.0119)	(262.60)	(405.77)	(603.82)
P-value, test of diff.	0.510	0.724	0.313	0.738	0.712	0.451	0.485
CCM, Under 18		0.647	0.695	0.821	2272	4198	6472
CCM, 18 and Over		0.783	0.786	0.876	6211	9516	15710

Notes: N = 42,409. Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.25: Employment and Earnings Effects by Neighborhood: Above/Below Median in Opportunity Insights Upward Mobility Ranking

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Above Median		0.0111*	0.0016	0.0032	64.18	55.26	115.83
		(0.0058)	(0.0059)	(0.0048)	(66.53)	(106.74)	(155.19)
ITT, Below Median		0.0143**	0.0100*	0.0126***	56.21	164.90	220.49
		(0.0058)	(0.0057)	(0.0047)	(63.27)	(100.37)	(147.04)
P-value, test of diff.		0.694	0.307	0.159	0.931	0.454	0.624
CM, Above Median		0.706	0.711	0.840	3537	5898	9435
CM, Below Median		0.696	0.729	0.841	3621	6029	9650
IV, Above Median	0.3903***	0.0285*	0.004	0.0082	172.51	152.86	321.62
	(0.0047)	(0.0150)	(0.0152)	(0.0123)	(170.54)	(273.50)	(397.67)
IV, Below Median	0.4183***	0.0343**	0.0239*	0.0299***	138.76	401.58*	539.28
	(0.0047)	(0.0138)	(0.0137)	(0.0112)	(151.10)	(239.64)	(351.08)
P-value, test of diff.	0.000	0.775	0.331	0.193	0.882	0.494	0.682
CCM, Above Median		0.693	0.714	0.837	3645	6015	9664
CCM, Below Median		0.700	0.741	0.844	3805	6293	10099

Notes: N = 42,408 (1 observation missing zip code). Above/below median defined as the within-sample median of the 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 study participant zip code, see text for details. Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.26: Employment and Earnings Effects by Pre-SYEP Work Experience Status

	First Stage	Employment Y1	Employment Y2	Employment Cumulative	Earnings Y1	Earnings Y2	Earnings Cumulative
ITT, Ever Worked		0.0177***	0.0065	0.0117***	72.08	184.32	253.99
		(0.0050)	(0.0051)	(0.0038)	(77.80)	(118.37)	(176.63)
ITT, Never Worked		0.0065	0.005	0.0035	39.60	10.91	58.45
		(0.0068)	(0.0067)	(0.0058)	(38.28)	(75.09)	(100.48)
P-value, test of diff.		0.183	0.862	0.237	0.708	0.216	0.336
CM, Ever Worked		0.793	0.790	0.895	5074	8000	13073
CM, Never Worked		0.589	0.635	0.774	1749	3471	5220
IV, Ever Worked	0.4120***	0.0431***	0.0157	0.0280***	175.05	448.45	622.09
	(0.0045)	(0.0121)	(0.0123)	(0.0092)	(188.64)	(287.28)	(428.80)
IV, Never Worked	0.3951***	0.0164	0.0127	0.0086	106.76	46.19	145.86
	(0.0049)	(0.0173)	(0.0170)	(0.0147)	(96.63)	(189.66)	(254.18)
P-value, test of diff.	0.011	0.207	0.885	0.263	0.747	0.242	0.34
CCM, Ever Worked		0.775	0.794	0.883	5209	8057	13267
CCM, Never Worked		0.601	0.647	0.789	1920	3846	5772

Notes: N = 23,731 youth with work experience prior to the SYEP summer and 19,678 youth who never worked prior to the SYEP. Earnings winsorization recodes each quarter's earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.27: 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
Minority	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 High School	0.399	5.64	0.084	0.198
Not in HS	0.421	5.85	0.077	0.053
Under 18	0.403	5.64	0.084	0.226
18 and Over	0.407	5.79	0.079	0.052
Above Median in OI Rank	0.390	5.80	0.077	0.167
Below Median in OI Rank	0.418	5.60	0.086	0.163
Never Employed Pre-SYEP	0.395	5.61	0.077	0.214
Ever Employed Pre-SYEP	0.412	5.77	0.086	0.128
High Rating	0.753	6.04	0.093	0.250
Low Rating	0.307	3.92	0.074	0.167

Notes: Means shown for treatment group only, N = 21,714 (except for high/low rating, which is limited to those with a rating, N = 14,400). Average rating conditional on being sent a letter, N=8,780; application probability conditional on being invited to apply, N=2,000 (1,346 for rating categories); and submission probability conditional on applying, N=164 (116 for rating categories). Median OI Rank is the within-sample median of the Opportunity Insights "upward mobility" percentile rank. All differences in having a letter and in average ratings between two groups (white/minority, male/female, high school/not high school, under/over 18, above/below OI rank, never/ever worked, and high/low ratings) are statistically different except for having a letter between 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.28: 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	0.011	0.009	0.039
Never Employed Pre-SYEP	0.429	0.450	0.000
Ever Worked, Year -4	0.170	0.153	0.000
Earnings, Year -4	333	318	0.443
Ever Worked, Year -3	0.293	0.267	0.000
Earnings, Year -3	616	585	0.174
Ever Worked, Year -2	0.459	0.437	0.000
Earnings, Year -2	1113	1072	0.182
Ever Worked, Year -1	0.962	0.966	0.042
Earnings, Year -1	2379	2379	0.775
No Education Match	0.185	0.126	0.000
In HS Sample	0.409	0.454	0.000
Joint F-test	F(24, 45597) = 35.48, p=0		

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

Table A.29: 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	0.707	0.001
Employment Y2	0.715	0.722	0.256
Employment Cumulative	0.843	0.844	0.417
Earnings Y1	3665	3600	0.155
Earnings Y2	6190	6015	0.009
Earnings Cumulative	9855	9615	0.020
Joint F-test, Employment Outcomes	$F(5, 45597) = 5.959$, p=0		
Panel B: Education Outcomes			
N	10564	9857	
Enrolled Y1	0.934	0.946	0.000
Days Enrolled Y1	146.8	150.0	0.000
Share Days Attended Y1	0.808	0.829	0.000
GPA Y1	79.03	80.13	0.000
On-time Graduation	0.777	0.809	0.000
Ever Graduated	0.828	0.858	0.000
Grad or Still in School	0.856	0.884	0.000
On-time College	0.665	0.697	0.000
Joint F-test, Y1 and Ever Graduated Outcomes	$F(5, 16304) = 167.419$, p=0		

Notes: Table tests difference of means between all youth in unopened surveys (excluded from our main sample) and our control group (on an opened survey), separately for employment outcomes and subset of youth in expected HS sample. N=15,386 for graduation tests and 14,543 for college test. To avoid using smallest available sample and highly correlated outcomes for joint F-test, education joint test includes 4 high school outcomes and ever graduated indicator. Test of difference controls for cohort indicator and uses cluster-robust standard errors.

Table A.30: Job Application Behavior, Unopened Surveys versus Main Control Group

	Unopened Surveys	Control	Test of Difference
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 F(5, 2630) = .822, p=.534			

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

Table A.31: Labor Market Effects, On Any Survey

Year	1	2	Cumulative
	Panel A: Employment		
ITT	0.0062*	0.0034	0.0029
	(0.0032)	(0.0033)	(0.0027)
CM	0.707	0.719	0.843
Sent Letter (IV)	0.0244*	0.0136	0.0111
	(0.0128)	(0.0129)	(0.0104)
CCM	0.704	0.729	0.849
	Panel B: Earnings, Winsorized at 99.5th Percentile		
ITT	23.34	65.37	89.04
	(36.70)	(58.97)	(85.73)
CM	3619	6052	9671
Sent Letter (IV)	92.75	257.2	350.06
	(144.43)	(232.05)	(337.29)
CCM	3791	6186	9977
	Panel C: Log(Earnings + 1)		
ITT	0.043*	0.034	0.028
	(0.026)	(0.028)	(0.023)
CM	5.66	6.08	7.35
Sent Letter (IV)	0.168*	0.135	0.109
	(0.102)	(0.110)	(0.092)
CCM	5.71	6.20	7.47

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 earnings to the 99.5th percentile of all quarterly earnings before summing across years. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.32: Education Effects, On Any Survey & In Expected HS Sample

	Ever Enrolled Y1	Days Enrolled Y1	Share Days Present Y1	GPA Y1	On-time Graduation	Ever Graduated	Graduated or Still Attending	On-time College
ITT	-0.0008	-0.0625	0.001	-0.0576	-0.0055	0.0012	0.0009	0.0006
	(0.0025)	(0.4527)	(0.0023)	(0.0804)	(0.0035)	(0.0034)	(0.0033)	(0.0050)
CM	0.942	148.9	0.822	79.76	0.799	0.847	0.874	0.687
N	30278	30278	30278	27868	22723	22723	22723	21533
Sent Letter (IV)	-0.0025	-0.2313	0.0037	-0.2145	-0.0192	0.0042	0.0037	0.0028
	(0.0092)	(1.6760)	(0.0084)	(0.2900)	(0.0124)	(0.0119)	(0.0117)	(0.0177)
CCM	0.955	152.6	0.848	81.66	0.843	0.873	0.897	0.719
N	30278	30278	30278	27868	22723	22723	22723	21533

Notes: Sample includes all youth on any survey, regardless of whether any supervisor opened the survey, if they were expected to be observed based on pre-program grade of enrollment (see text for details). 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 includes any 5- or 6-year graduation for the same group if it occurs during the follow-up period. College enrollment only measured within 6 months after a student's on-time graduation date, regardless of graduation status. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.33: Labor Market Effects, No Covariates

Year		1	2	Cumulative
		Panel A: Employment		
	ITT	0.0115***	0.0048	0.0070**
		(0.0044)	(0.0043)	(0.0035)
	CM	0.701	0.72	0.841
Sent Letter (IV)		0.0284***	0.0119	0.0172**
		(0.0108)	(0.0106)	(0.0086)
	CCM	0.7	0.731	0.843
		Panel B: Earnings, Winsorized at 99.5th Percentile		
	ITT	41.81	102.64	144.46
		(54.62)	(82.90)	(126.80)
	CM	3579	5964	9543
Sent Letter (IV)		103.41	253.84	357.25
		(135.05)	(204.98)	(313.49)
	CCM	3780	6190	9970
		Panel C: Log(Earnings + 1)		
	ITT	0.084**	0.049	0.066**
		(0.036)	(0.038)	(0.032)
	CM	5.61	6.08	7.33
Sent Letter (IV)		0.208**	0.122	0.162**
		(0.089)	(0.093)	(0.079)
	CCM	5.67	6.21	7.41

N = 43,409. Winsorization in Panel B recodes each quarter's 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 cohort indicator only. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.34: Education Effects, No Covariates

	Ever Enrolled Y1	Days Enrolled Y1	Share Days Present Y1	GPA Y1	On-time Graduation	Ever Graduated	Graduated or Still Attending	On-time College
ITT	-0.0033	-0.8474	-0.0038	-0.4625**	-0.0156**	-0.0059	-0.0074	-0.0118
	(0.0033)	(0.6344)	(0.0038)	(0.1842)	(0.0065)	(0.0058)	(0.0053)	(0.0078)
CM	0.946	150.0	0.829	80.13	0.809	0.858	0.884	0.697
N	19714	19714	19714	18237	14726	14726	14726	13999
Sent Letter (IV)	-0.0081	-2.0549	-0.0093	-1.1128**	-0.0360**	-0.0135	-0.017	-0.0272
	(0.0080)	(1.5393)	(0.0092)	(0.4445)	(0.0150)	(0.0133)	(0.0123)	(0.0180)
CCM	0.961	154.4	0.861	82.56	0.860	0.890	0.918	0.749
N	19714	19714	19714	18237	14726	14726	14726	13999

Analysis on all those expected to be observed based on pre-program grade of enrollment (see text for details). 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 includes any 5- or 6-year graduation for the same group if it occurs during the follow-up period. College enrollment only measured within 6 months after a student's on-time graduation date, regardless of graduation status. CM shows control means; CCM shows control complier means. Regressions include baseline covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.35: Labor Market Effects, All Covariates

Year	1	2	Cumulative
Panel A: Employment			
ITT	0.0124***	0.0058	0.0077**
	(0.0041)	(0.0041)	(0.0034)
CM	0.701	0.72	0.841
Sent Letter (IV)	0.0306***	0.0143	0.0190**
	(0.0101)	(0.0102)	(0.0083)
CCM	0.698	0.728	0.841
Panel B: Earnings, Winsorized at 99.5th Percentile			
ITT	55.14	109.8	164.93
	(45.77)	(73.12)	(106.75)
CM	3579	5964	9543
Sent Letter (IV)	136.33	271.48	407.81
	(113.11)	(180.73)	(263.85)
CCM	3747	6172	9919
Panel C: Log(Earnings + 1)			
ITT	0.093***	0.058*	0.073**
	(0.033)	(0.035)	(0.030)
CM	5.612	6.085	7.332
Sent Letter (IV)	0.229***	0.144*	0.181**
	(0.081)	(0.087)	(0.073)
CCM	5.64	6.19	7.40

N = 43,409. Winsorization in Panel B recodes each quarter's 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 all available covariates (see text) rather than post-double-selection LASSO-selected covariates. Standard errors clustered on individual in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A.36: Education Effects, All Covariates

	Ever Enrolled Y1	Days Enrolled Y1	Share Days Present Y1	GPA Y1	On-time Graduation	Ever Graduated	Graduated or Still Attending	On-time College
ITT	-0.0018	-0.3201	0.0015	-0.1354	-0.0075*	-0.0007	-0.0021	0.0004
	(0.0030)	(0.5425)	(0.0028)	(0.0983)	(0.0043)	(0.0041)	(0.0040)	(0.0062)
CM	0.946	150.0	0.829	80.13	0.809	0.858	0.884	0.697
N	19714	19714	19714	18237	14726	14726	14726	13999
Sent Letter (IV)	-0.0043	-0.7724	0.0036	-0.3243	-0.0171*	-0.0017	-0.0049	0.001
	(0.0072)	(1.3079)	(0.0067)	(0.2353)	(0.0099)	(0.0094)	(0.0092)	(0.0141)
CCM	0.957	153.1	0.848	81.77	0.841	0.878	0.905	0.721
N	19714	19714	19714	18237	14726	14726	14726	13999

Analysis on all those expected to be observed based on pre-program grade of enrollment (see text for details). 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 includes any 5- or 6-year graduation for the same group if it occurs during the follow-up period. College enrollment 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 all available covariates (see text) rather than post-double-selection LASSO-selected covariates. Standard errors clustered on individual in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$