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ABSTRACT

Canonical human capital theories posit that education, by enhancing worker skills, reduces the likelihood that a worker will be laid-off during times of economic change. Yet, this has not been demonstrated causally. We link administrative education records from 1987 through 2002 to nationally representative surveys conducted before and after COVID-19 onset in Barbados to explore the causal impact of improved education on job loss during this period. Using a regression discontinuity (RD) design, we show that females (but not males) who score just above the admission threshold for more selective secondary schools attain more years of education than those that scored just below (essentially holding initial ability fixed). We then find that these same females are much less likely to have lost a job after the onset of COVID-19. We show that these effects are not driven by labor supply decisions, fertility or access to child care, or selection into more resilient sectors and occupations. Because employers observe incumbent worker productivity, these patterns are inconsistent with pure education signaling, and suggest that education enhances worker skill.

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I Introduction

It is well-documented that workers with more years of education tend to experience fewer job losses during economic downturns (Farber, 2005, 2015). Most recently, job losses during the COVID-19 pandemic were concentrated among workers with less education (CPS 2021; Bottan et al. 2020). However, because higher productivity workers may select into education and professions, this could reflect either selection or the causal impacts of education. We provide some of the first evidence on the extent to which education *causally* influences job losses during times of economic disruption using exogenous school assignments in Barbados and longitudinal survey data linking individuals from primary school through adulthood before, during, and after the COVID-19-induced recession.

During the COVID-19 pandemic many businesses dissolved and others made significant adjustments as demand for in-person services fell and many nations went into some form of government-mandated shutdown (Fetzer et al., 2020; Bartik et al., 2020). This resulted in considerable and sudden job losses worldwide beginning in February 2020. In the U.S., the unemployment rate went from 3.5% in February 2020 to 14.7% just 8 weeks later (BLS, 2020). In Latin America almost half of households had a member who lost a job at the onset of the pandemic (Bottan et al., 2020). This unprecedented event provides an opportunity to examine the *causal* role that education may play in protecting workers from job loss during times of economic contraction.

There are non-trivial challenges to understanding the causal impact of education on recessionary job losses. It requires (a) longitudinal data linking individuals to their educational attainment and subsequent labor market outcomes, (b) exogenous variation in educational attainment, and (c) multiple measures of labor market participation for these same individuals before, during, and after a recession. We overcome these issues by matching individuals' administrative education records from 1987 through 2002 to nationally representative surveys conducted before, during, and after the onset of COVID-19 in Barbados.

To uncover causal relationships, we follow Beuermann and Jackson (2022) and exploit the exogenous assignment of students to secondary schools caused by the centralized school assignment mechanism. A serial dictatorship algorithm (Abdulkadiroglu and Sonmez, 1998) assigns students to schools based on their scores in a standardized national exam taken at the end of primary school (BSSEE) and their ranked list of school preferences. This creates a test score cut-off for each secondary school above which applicants are admitted and below which they are not. This feature allows us to use a regression discontinuity (RD) design to isolate the causal effect of education (holding underlying pre-schooling ability fixed) on subsequent outcomes.

We link administrative BSSEE data covering the full population of secondary school applications and assignments for sixteen years (1987 through 2002) to the 2016 Barbados Survey of

Living Conditions (BSLC) and focus on sixteen cohorts aged 25 or older at the time of the survey. To track outcomes related to the recession, we link individual students in the BSSEE and the BSLC to two new waves of surveys conducted during the COVID-19 pandemic. We are able to track labor market outcomes before, during and after the COVID-19 recession for the same nationally representative sample of individuals for whom we have exogenous secondary-school assignments.

As in Beuermann and Jackson (2022), females who score just above an admission threshold for a more-selective school are 25 percentage-points more likely to attend university and attain on average 2.935 more years of education than those with scores just below (p-value$<$0.01). In contrast, there is no increase for men. Looking at job loss in our follow-up data, *these same females* with scores just above an admission threshold are about 36 percentage-points (p-value$<$0.05) less likely to have lost a job during the recession than those with scores just below. We implement several empirical tests to establish that the effects we present are causal, not driven by sample non-response bias, and are robust to several alternative specifications.

To show the importance of using data straddling an economic downturn for measuring protective effects, we estimate similar models of job loss during other periods. During a period of regular growth and during recovery, scoring above the threshold does not affect job loss. We also estimate effects on employment during each of these time periods (as opposed to job loss) and find that attending a preferred school disproportionately affects employment only for females during the recession. This reinforces that our reduced job loss impacts are driven by increased education for females, and underscores the fact that detecting employment effects of education can be challenging in tight labor markets. Indeed, existing work examining the effect of education on employment status *in general* often finds no systematic significant effects (e.g., Riddell and Song 2011; Grosz 2020; Duflo et al. 2021).

We also explore mechanisms. The results are driven by layoffs as opposed to labor-supply decisions. The protective effects of education are related to worker's attributes as opposed to job characteristics. The results are not driven by more-educated women sorting into recession-proof sectors and occupations—i.e., those sector-occupations that experienced less employment disruptions during the onset of the COVID-19 pandemic, by more educated women having increased access to childcare or reduced fertility, or by higher costs of laying off senior workers (who may incidentally be more highly educated). Overall, the patterns indicate a protective role of education *per se* during downturns.

We make several contributions. We show that the well-established negative relationship between educational attainment and job loss is not all selection and somewhat reflects the causal protective effect of education. Also, our examination of protective effects during a downturn (as opposed to employment more broadly) allows us to speak to the broader human capital vs. signaling debate. Under pure signalling (Spence, 1973), students who score just above or below an admission threshold would be equally productive. If so, because firms likely lay-off incumbent workers based on actual productivity (Gibbons and Katz, 1991; Berger, 2018), among those with the same productivity, education would be unrelated to job losses. Accordingly, our patterns are inconsistent with a pure signalling model and suggest that education enhances worker skills. Finally, our results provide empirical support for key predictions from canonical work (e.g., Schultz 1975; Gibbons and Katz 1991) that, *if education enhances human capital*, workers with more education, should be less susceptible to job losses during times of adjustment.

II The Barbados Context and Data

At the end of primary school, students register to take the Barbados Secondary School Entrance Examination (BSSEE) and submit a list of ranked secondary school choices to the Ministry of Education. There are 24 public secondary schools. Students are then ranked by their BSSEE score and gender. Individual school capacity by gender is pre-determined. The centralized mechanism assigns the highest ranked student to her first choice. It then moves on to the second and treats her similarly. The procedure continues until it reaches a student whose first choice is full. At that point, it tries to assign the student to her second choice. If full, to the third choice and so on. Once this student has been assigned to a school, the algorithm moves on to the next person. This mechanism is strategy- proof: truthfully ranking schools is a weakly dominant strategy (Dubins and Freedman, 1981; Roth, 1982).[1] As described in Section III below, we exploit this mechanism to uncover the causal effects of preferred school attendance on educational attainment and adult labor market outcomes.

Administrative Data: We collected the BSSEE data for the full population of students who applied to a public secondary school in Barbados between 1987 and 2002.[2] Following Beuermann and Jackson (2022), these cohorts are the focus of our analyses given that they were above 25 years old when labor market outcomes were tracked for the first time in 2016.[3] These data include each student's name, date of birth, gender, primary school attended, parish of residence, total score on the BSSEE exam, and the ranked list of secondary schools the student wished to attend.

2016 Survey Data: Our outcomes of interest come from three individual-level panel surveys. The first is the 2016 Barbados Survey of Living Conditions. This survey is a large, parish-level representative two-percent survey of the population collected between February 2016 and January

[1] This mechanism is strategy- proof when students are allowed to rank every school, which is true within our context for BSSEE cohorts 1987-1996. For BSSEE cohorts 1997-2002, the number of choices was restricted to 9. As shown in Beuermann and Jackson (2022), this change did not affect overall school rankings and most students did not fill their preference lists. Therefore, truthful ranking remained a dominant strategy (Haeringer and Klijn, 2009; Pathak and Sönmez, 2013).

[2] Around 91 percent of secondary students in Barbados are enrolled in the public education system.

[3] By age 25, 99 percent of all individuals had completed their formal schooling. Therefore, this population is suitable to measure educational attainment and labor market outcomes.

2017. Beuermann and Jackson (2022) matched this survey at the individual level with the BSSEE administrative data, achieving a 90 percent match rate among surveyed persons.

2020 Survey Data: To measure labor market resilience amidst the COVID-19 pandemic, we implemented two *additional* telephone surveys focused on the same sample as the 2016 Barbados Survey of Living Conditions. The first survey was executed in May 2020 and collected data on labor market outcomes before the pandemic (February 2020) and after its onset (May 2020). The second survey was collected in November 2020 and captures labor market outcomes during a period when mobility-restrictions were relaxed. We achieved an overall response rate of 52 and 47 percent respectively, with respect to the 2016 survey.[4]

Importantly, we show that our 2020 survey results likely generalize to the full population. First, the distributions of baseline characteristics available in the BSSEE administrative data are similar for the full population and the surveyed sample (Table A.1). Second, admission to a preferred school is unrelated to responding the survey and subsequently being matched with the BSSEE administrative data (Section III). Third, the impacts on the likelihood of preferred school attendance and school environments are similar in both the full population and among those who are linked to the 2020 survey (Section III).

Summary statistics are presented in Table 1. Before secondary school enrollment, females had higher BSSEE test scores than males (panel A). As adults, females completed, on average, about two additional years of schooling and were more likely to hold a university degree than males (panel B). While female employment rates were below males, average monthly gross wages were significantly higher for employed females (panel B). The pandemic generated large declines in employment (panel C). Employment fell by roughly 20 percentage points between February and May 2020. However, by November 2020, after the government relaxed some of the mobility restrictions, employment levels were almost at pre-pandemic levels. Of the roughly 20 (25) percent of females (males) lost their job between February and May 2020, about 55 (75) percent of females (males) were employed by November 2020– mainly due to reemployment in the same pre-pandemic job.

III Empirical Strategy

The centralized assignment mechanism creates a test score cutoff above which applicants to each school are admitted and below which they are not. The cutoffs are not known to parents and can vary from year to year. Also, parents do not know their test score when making choices – making the cutoffs very difficult to game. If nothing else differs among those scoring just above and below the cutoff, any sudden change in outcomes as students' BSSEE score goes from below to above the cutoff for a preferred school can be attributed to attending that preferred school (Hahn et al. 2001). We exploit the discontinuity in the admission probability through the cutoff by

[4] This is in line with the typical response levels in phone-based surveys.

estimating the following two-stage least-squares (2SLS) model:

$$Attend_{ijt} = \pi \cdot Above_{ijt} + f_1(BSEE_{it}) + X_{ijt}\gamma_1 + C_{1,jt} + \varepsilon_{1,ijt} \quad (1)$$

$$Y_{ijt} = \beta \cdot \hat{Attend}_{ijt} + f_2(BSEE_{it}) + X_{ijt}\gamma_2 + C_{2,jt} + \varepsilon_{2,ijt} \quad (2)$$

In the first stage (1) we predict whether individual i attends school j at time t, $Attend_{ijt}$, as a function of scoring above the cutoff for preferred school j at time t, $Above_{ijt}$, and controls.[5] $f_1(BSEE_{it})$ is a smooth function of the incoming BSSEE score fully interacted with the $Above_{ijt}$ indicator.[6] We also include parish of residency fixed effects and gender (X_{ijt}). Following Jackson (2010) and Pop-Eleches and Urquiola (2013), we stack the data across all application pools into a single cutoff, recenter BSSEE scores at each respective cutoff, and include cutoff fixed effects ($C_{1,jt}$). The cutoff fixed effects ensure that all comparisons are among students who applied to the same school in the same year. In the second stage (2), the outcome of interest (Y_{ijt}) is a function of predicted preferred school attendance and all controls from Equation (1). The second stage excluded instrument is $Above_{ijt}$. Because the same individual can enter the data for multiple cutoffs, the standard errors are clustered at the student level.[7] To improve precision, we exploit all available observations. However, in Section IV.2, we show that our main results are robust to alternative bandwidth restrictions.

The identifying assumption is that nothing other than the change in admission probability changes in a discontinuous manner through the cutoff. We test this assumption in several ways. Following McCrary (2008), we test for a discontinuity in density through the cutoff and find no discontinuity in the full population or the survey sample (Table A.2, panel A). As an additional test for smoothness, we compute predicted outcomes (using all the available covariates captured at BSSEE registration as predictors) and test for whether scoring above the cutoff is associated with any significant change on these fitted outcomes. Consistent with no gaming of the cutoffs, Table A.3 shows that there is no discernible relationship between scoring above the cutoff and the predicted outcomes. In contrast, scoring above a cutoff increases the likelihood of attending a preferred school by about 80-82 percentage points in the full population and 86 percentage points in the survey sample (both effects being statistically equivalent – Table A.2, panel B).[8]

[5] We code the attended school as the one in which the student was enrolled in the last year (ie. fifth year) of secondary studies. For those who leave school early, we use the assigned school. See Beuermann and Jackson (2022) for details.

[6] We model $f_1(BSEE_{it})$ with a 3rd-order polynomial. However, as shown in Section IV.2, our results are robust to alternative polynomial orders.

[7] In our context, this approach is equivalent to heteroskedasticity-robust estimated standard errors allowing for off-diagonal non-zero terms in the variance-covariance matrix when the same individual enters the data for more than one cutoff. Kolesár and Rothe (2018) show this to be a more conservative approach than also clustering estimated standard errors at the level of the running variable.

[8] As individuals can appear for multiple cutoffs, regression models have more observations than individuals.

To provide further evidence that our 2020 survey results are likely representative of the population, we show no change in the likelihood of being matched to and then responding to the survey through the cutoffs (Table A.2, panel C). We also show that attending a preferred school increases peer quality (average BSSEE scores) by about 0.25 standard deviations, and also decreases heterogeneity in peer quality with lower cohort sizes (Table A.2, panel D). Importantly, and consistent with the first stage results, the estimated effects on school environments are statistically indistinguishable between the population and the survey sample – further evidence that the survey results likely generalize to the full population.

Although our sample is not only representative of the full population on average, but also within the boundaries of the school assignment cutoffs; it is also the case that the sample is small. Therefore, conventional inference based on analytically estimated heteroskedasticity-robust standard errors, might lead to over-rejection of the null hypothesis of no effect (Duflo et al. 2008). To account for this, we follow Rosenbaum (2002) and implement a randomization inference procedure for hypothesis testing. For each studied outcome, we randomly assigned placebo cutoffs to each school-cohort and estimated reduced-form effects across 2,000 iterations. We then compute the percentile of the real effect within the empirical CDF of the placebo effects and report it with each of our estimated effects. While inference is based on this procedure, we also report the analytical standard errors to give a sense of the precision of our estimates.

IV Results and Discussion

The top two panels of Figure 1 show sharp jumps in the likelihood of attending the preferred school at the eligibility threshold. These jumps are similar for both sexes around 86 percentage points (Table A.2). The middle panels of Figure 1 show that there is a discontinuous jump in the years of schooling for females (left) but not for males (right). In panel A of Table 2, we present 2SLS estimates of preferred school attendance (β) from equation (2), which are consistent with the graphical evidence. Among women, attending a preferred school increases years of schooling by 2.9 (p-value$<$0.01).[9] This coincides with a 24.5 percentage points increase in the likelihood of completing a university degree (p-value$<$0.01). In contrast, for men, the point estimates on years of schooling (-0.899 years) and attendance to university (-13.5 percentage points) are negative and significantly different from the positive impacts on females.

IV.1 Effects on employment and job loss

We leverage the COVID-19 pandemic shock to provide novel evidence on the causal protective role of education in labor markets. Between 2016 and February 2020, male and female employment was stable at just above and below 80 percent, respectively (Table 1). For both sexes, employment

[9]The estimates of these effects cover a range of values: the 95 percent confidence interval indicates that attending a preferred school lead to between 0.98 years and 4.88 additional years of schooling in the case of women.

fell sharply in May 2020 by roughly 20 percentage points and recovered to almost pre-pandemic levels by November 2020. This provides a unique setting to analyze how attending a preferred school, by inducing higher educational attainment among females, might have differentially impacted the trajectory of employment for females and males.

Effects on Employment: These impacts vary across labor market conditions and follow different dynamics by gender. Table 2 reports estimates of β from equation (2) using the employment status as outcome for women and men before, during, and after the pandemic-induced shock. In 2016 and February 2020, when employment rates were about 80 percent, both females and males who attended preferred schools were more likely to be employed. However, for neither group can we reject that the effect is zero. In contrast, in May 2020 when the employment rates were much lower (about 58 percent for women and 66 percent for men), the effects for men and women diverge. Females who scored just above the admission cutoff for a preferred school were 66 percentage points more likely to be employed than those who score just below (p-value$<$0.01), while males were somewhat less likely to be employed (p-value$>$0.1).[10] In November 2020, after the government lifted the economic and mobility restrictions, the difference in the impacts on employment for men and women substantially narrowed.

The results indicate that attending a preferred school does not significantly impact employment for men. In contrast, for women, when the economy is strong, attending a preferred school is weakly associated with higher employment, while when the economy is weak, attending a preferred school significantly increases employment. The differential effects on employment by gender mirror the differential effects on educational attainment—suggesting that the channel through which attendance at a preferred school impacts employment is greater educational attainment. While this pattern of effects on employment over this time period suggests possible impacts of attending a preferred school on job loss, it could also reflect differences in hiring practices during a strong versus a weak economy (Forsythe, 2021). Next, we examine the impacts on job loss.

Effects on Job Loss: One of the primary benefits of our longitudinal data that straddles the recession is that it allows for a direct examination of job loss. Job loss is particularly interesting, because unlike employment decisions, employers make decisions about which workers to retain after observing worker productivity.

We present visual evidence of causal impacts on job loss between February and May 2020 in the lower panel of Figure 1. There is a sharp drop in job loss right at the cutoff for women (left), while there is no such shift for men (right). The regression estimates (Panel C of Table 2), indicate that women who scored just above the admission threshold for a preferred school were 35.9 percentage points less likely to have lost a job between February 2020 and May 2020 (p-value$<$0.05), while

[10] While the employment impact for women is clearly positive, it includes a range of values with a 95 percent CI of [0.31;1.00].

the impact for males, although positive, is not significant.[11] The effects on job loss among women can account for roughly 55 percent of the employment effects during May 2020. The results are quantitatively and qualitatively similar when we use 2016 employment status as the baseline for computing job loss.[12]

Overall, the results suggest a causal role for education in reducing job loss; females who attended their preferred school achieved higher levels of education and were less likely to experience employment disruptions during the recession.[13] They also suggests that the differences in the impacts of attending a preferred school on employment during the recession are largely due to declines in job losses among more educated females and not due to different hiring practices during the recession. Note that there are neither substantial nor significant effects on job losses before the pandemic—i.e., between 2016 and February 2020 (Panel C of Table 2), and that the effects are small and statistically insignificant when we analyze job losses between February and November 2020. These results indicate that education may play a larger role in employment during recessions. We explore mechanisms in sections IV.3 and V.

IV.2 Robustness checks

We demonstrate the robustness of our estimates in a variety of ways. We show that our main point estimates for women and men are largely stable irrespective of the bandwidth and BSSEE (the running variable) polynomial specifications (Figures A.1 and A.2). Even though we show that the survey is representative of the full population (Table A.1), that there is no differential response rate through the cutoffs (Table A.2), and that first stage estimates and estimated impacts on school environments are the same for the full population and the surveyed sample (Table A.2); we also show that our results are the same in models that do not use population weights (Table A.4). In addition to showing smoothness through the cutoffs to demonstrate the validity of the RD model (Tables A.2 and A.3), the evidence presented demonstrates that our results reflect causal impacts.

IV.3 Mechanisms

Voluntary or involuntary job disruptions: Our measure of job loss identifies individuals who were not employed in May 2020 during the recession but were employed in pre-pandemic periods. As such, our measure of job loss captures workers' decisions to withdraw from the labor market amid the crisis as well as layoffs. We collected data on the reasons for changes in employment

[11] The job loss impact for females, while robustly negative, conveys a 95 percent CI of [-0.64;-0.08].

[12] Beuermann and Jackson (2022) find that attending to a preferred school had significant positive effects on peer quality and negative effects on health risks (proxied by obesity). These impacts were similar for women *and* men, which ameliorates concerns that the differential impacts on job loss are driven by heterogeneous effects on health and social capital.

[13] Conversely, although estimated with less precision, men who attended their preferred school exhibit lower educational attainment (see Panel A of Table 2) and are more likely to experience a job loss during the crisis.

status between February and May 2020. Panel A of Table 3 shows that, for women, attending a preferred school reduces the likelihood of a layoff and has no impact on job disruptions associated with worker-initiated separations. Note that the 34.6 percentage point decline in the likelihood of experiencing a layoff accounts entirely for the observed impact on job loss. This result underscores the protective effects of education: women who attended their preferred school obtained substantially more education and thus were less prone to suffer job displacements during the recession.[14]

Fertility and access to child care: We are also able to rule out individual labor-supply decisions related to fertility as drivers of our results. It has been widely documented that the COVID-19 crises disproportionately affected women (Alon et al., 2020; Fabrizio et al., 2021) and that, due to the suspension of child care services, women had to withdraw from the labor force (Russell and Sun, 2020). If education reduced long-term fertility or improved women's access to childcare help from outside the household, we would expect that women who attended preferred schools and obtained more education would have been less likely to leave the labor force in order to take care of children. However, we find no evidence in support of this mechanism. Panel B of Table 3 shows that there are no impacts of attending to a preferred school on the probability of having children younger than 6 years old, on the probability of having school-age children, or the probability of receiving external help with children during the pandemic.[15]

Selection into sectors and occupations: While the results above indicate that education provides protection against layoffs, there are two possible channels through which education could reduce layoffs during recessions. First, education could increase productivity and employers may choose to retain high-productivity workers during recessions. Second, workers with more years of education could select into jobs that are more recession-proof. Unfortunately, we are not able to directly test the first channel in our data. Although not a perfect test of the second channel, we do examine the extent to which the observed job losses are similar to job losses experienced within the worker's pre-pandemic sector of employment and occupation. That way we explore the extent to which pre-pandemic selection into sectors and occupations might drive our results.

First, we identified an individual's sector and occupation *before* the pandemic using data from 2016 and February 2020. Second, we computed the job loss of all *other* individuals in the same pre-recession sector-occupation bin—i.e., the average job loss in the same sector and occupation, while excluding the contribution of person i to avoid mechanical correlation. The resulting sector-occupation job loss is well aligned with actual job loss. A regression of actual job loss on sector-

[14] In addition, for men, attending to a preferred school marginally increases the probability of being laid off (p-value<0.1). This is consistent with lower academic achievement among males who attended their preferred school (Table 2).

[15] This is consistent with Beuermann and Jackson (2022) who found that preferred school attendance reduced teen motherhood (which possibly mediated the higher levels of educational attainment for women) but did not affect the likelihood of giving birth by age 25 or overall fertility.

occupation job loss yields a slope of 0.99 (see Figure A.3, panel A) and an R-squared of 0.21. In addition, sector-occupation job loss is associated with relevant predictors of employment disruptions during the pandemic, such as jobs that require personal contact with other individuals (see Figure A.3, panel B).[16]

If the effects on job loss that we observe are completely attributable to differences in the sectors and occupations in which the females who attended their preferred schools were employed before the pandemic, the coefficient on the *sector-occupation* job loss would be equal to that on *actual* job loss. At the other end of the spectrum, evidence of no impact of attending a preferred school on sector-occupation job loss would suggest that the reduced job loss for females during recessions is not due to differential selection into sectors and occupations. Instead, it would suggest that women who attended a preferred school and acquired higher levels of human capital are more productive workers.

We present impacts of attending a preferred school on sector-occupation job loss in Panel C of Table 3. For females, who experience large reductions in actual job loss, the effect on *sector-occupation* job loss can not be distinguished from zero. This suggests that the reduced job loss we document is not driven by differential selection into sectors or occupations by those who have more years of education.

Last in, first out: Studying job loss in a sample of workers who had been in the labor market for several years before the pandemic and acquired years of tenure implies that employers had learned their workers' productivity. However, there may be other mechanisms besides productivity that could rationalize our results. For example, Forsythe (2021) documents that firms preferentially hire experienced workers during periods of high unemployment and Buhai et al. (2014) show that workers with relatively lower seniority within the firm are more likely to experience layoffs—i.e., the last in, first out rule. This could be due to loyalty or higher firing costs due to labor market regulations.[17] If worker seniority is correlated with years of education, then our results could be driven by higher firing costs as opposed to more-educated women being more productive. This is unlikely in our setting. Panel D of Table 3 shows that although attending a preferred school increases women's educational attainment, it does not have statistically discernible impacts on pre-pandemic tenure. Thus, one can think of our results as evidence of a link between increased educational attainment and job loss, holding worker tenure constant.

[16] The availability of teleworking possibilities has also been associated with lower incidences of employment disruptions. Although pre-pandemic incidence of teleworking in Barbados was relatively low with about 10 percent of individuals reporting that their February 2020 job allowed regular teleworking, all pre-pandemic teleworking is concentrated in sector-occupations with relatively low incidence of job losses (below 33 percentage points).

[17] In Barbados, an employee is eligible for severance payments after continuously working 104 weeks for 21 hours or more per week.

V Signaling versus Human Capital

Our results bring novel empirical evidence to the long-standing debate of whether the returns to education reflect human capital or are purely a signal of ability. The literature on employer learning argues that while employers make personnel decisions at the point of hire based on a signal (because worker ability is not directly observed by employers before employment), they make subsequent decisions regarding their incumbent workers based on actual observed productivity (Altonji and Pierret, 2001; Arcidiacono et al., 2010). Consistent with this theory, wages tend to be more closely related to measures of ability over time. The empirical evidence suggests that employers learn worker ability relatively quickly such that the signaling component is mostly eliminated within three to six years (Aryal et al., 2019; Bordón and Braga, 2020; Lange, 2007). Because women who scored just above and below an admission threshold have the same ability *prior to secondary school*, their higher educational attainment would not protect them from job losses under a pure signalling story. As such, our findings are inconsistent with education as purely a signal of worker ability. While layoffs could be affected by signalling among workers with low tenure, in our data, almost all of the respondents are past the three to six year window of tenure. As such, for the overwhelming majority of the sample, any decisions made by employers should not be affected by the signal of worker ability but should reflect worker productivity. For this reason, our results are consistent with the human capital accumulation theory of education.[18]

VI Implications for Recovery

Our setting also allows us to analyze the effects of attending a preferred school during a period of normal economic growth and a recovery period. Panel E of Table 3 shows no evidence that attending a preferred school increases the likelihood of being reemployed in November 2020. This suggests that less educated females reentered the labor force at similar rates as more educated females. However, they seem to have done so at lower pay. Due to reluctance to provide information on wages and individual earnings, we collected data on total monthly household income.[19] Panel F of Table 3 shows that, before the crisis (in January 2020), attending a preferred school may increase family income – but these effects are not statistically significant (possibly due to lack of power). In contrast, we find large positive and significant effects of attending a preferred school on total

[18]Another possibility is that preferred school attendance could have improved social networks in the labor market that could serve as employment protection. Beuermann and Jackson (2022) show that both females and males experienced improved social networks in the labor market equally. In addition, preferred school attendance might have also affected other aspects of human capital that could explain the reduced job losses for females. Beuermann and Jackson (2022) also show that long-term health was positively affected for both females and males evenly. Therefore, these mechanisms are unlikely to account for the differential job losses between females and males.

[19]However, the individuals that we study are significant contributors to household income and, therefore, changes in their earnings are important determinants of it. Indeed, in February 2020, 70 percent of our sample contributed at least with half of household monthly income and 90 percent with at least a third of it.

monthly household income for females in April 2020, July 2020, and October 2020.[20] The positive impact on household income in April 2020 reflects the fact that women who attended a preferred school were less likely to lose their job during the crisis. The significant positive impact on household income in October 2020, after much employment had recovered, implies that women with less education reentered the labor market at lower wages. This suggests that the recession reduced the economic well being of women with less education in two ways. First, women with less education were more likely to experience job loss and the associated loss of income during the recession. Second, women with less education were more likely to reintegrate into the labor market at lower wages. If these wage differentials persist or compound over time, this has important implications for longer-term inequality of labor market earnings. More generally, the results show that the labor market returns to education are not constant, but are larger during economic downturns.

VII Conclusions

It is well-documented that more educated workers tend to fare better during recessions (Farber, 2005, 2015). Canonical models in labor economics posit that this *may be*, in part, because education - by enhancing ability – is protective against job loss (e.g., Schultz 1975; Gibbons and Katz 1991). However, this notion has not been tested *causally*. Leveraging exogenous school assignments and data straddling the COVID-19 related recession, we provide novel evidence that this relationship reflects a causal effect of education.

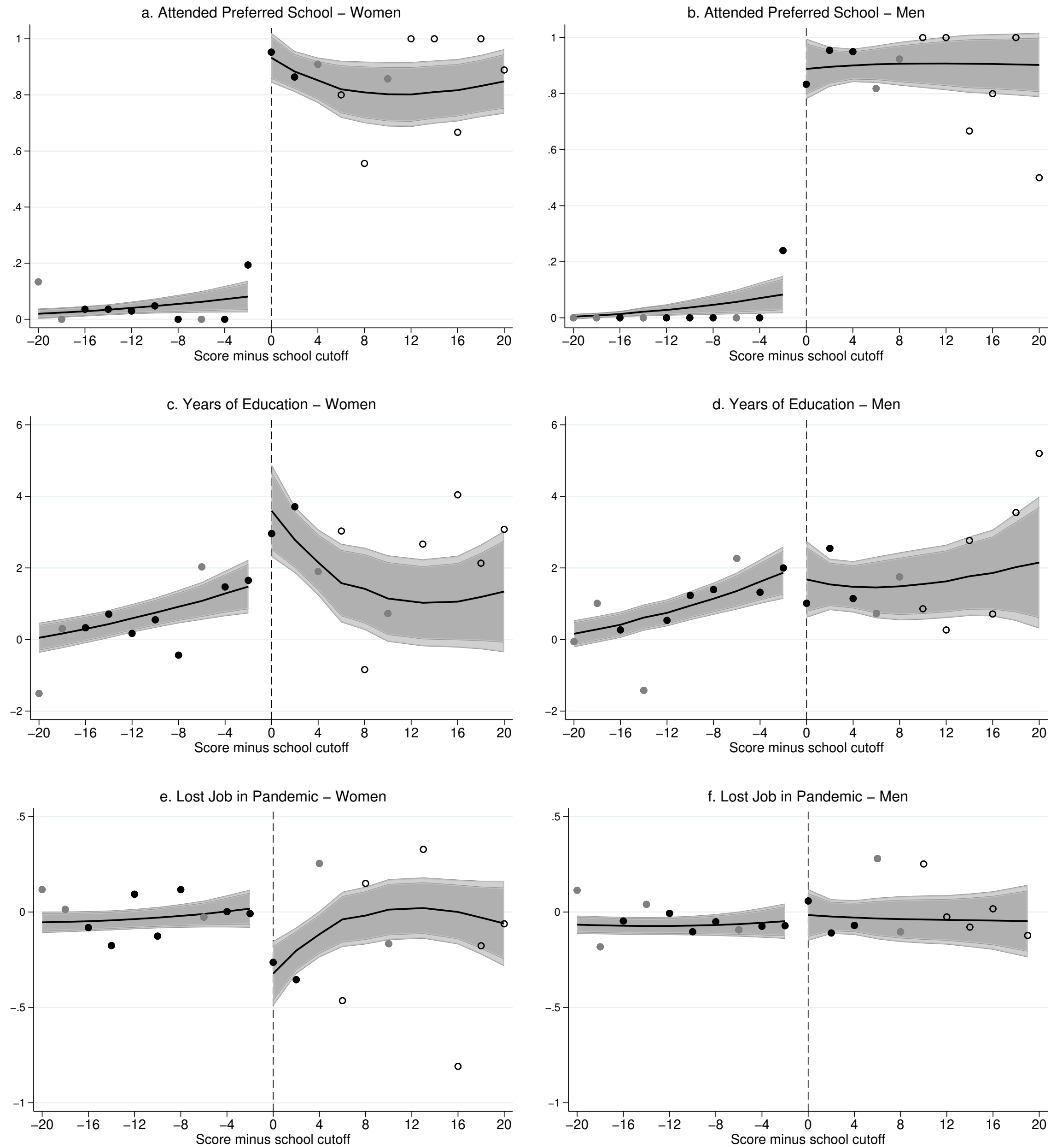
We find that females who attended their preferred school attained more years of education and were also much less likely to have experienced a job loss during the COVID-19 recession. We show that these effects are not driven by voluntary labor supply decisions, fertility or access to child care, or selection into more resilient sectors and occupations. This is compelling evidence that education plays a causal role in protecting workers from job losses during economic downturns – supporting the predictions in the canonical work.

Because our sample largely includes individuals with relatively high pre-pandemic tenure (9 years on average), layoff decisions would have been most likely made based on productivity rather than a signaling component of education (Altonji and Pierret, 2001; Arcidiacono et al., 2010; Aryal et al., 2019; Bordón and Braga, 2020; Lange, 2007). As such, our findings are unlikely to be driven by signalling, and therefore suggest that education causally boosts adult productivity.

While our work provides novel insights on the importance of education for individuals with test scores close to the admission threshold for a preferred school, our estimates do not include any general equilibrium effects of the impact of increasing education for an entire population.

[20] The May 2020 survey collected total household income for January and April, 2020. The November 2020 survey collected income for July and October 2020.

Figure 1: Discontinuities Through Assignment Cutoffs



Notes: Panels a-b: Y-axis represents the likelihood of having attended a preferred school. Panels c-f: Y-axis represents residuals from regressing the outcome on cutoff and parish of residence fixed effects. The X-axis is the BSSEE score relative to the cutoff. Circles are means corresponding to 2-point bins of the relative score. Black (Gray) [Hollow] circles include more than 20 (between 10 and 20) [less than 10] observations. The solid lines are generated by fitting a third degree polynomial of the relative score fully interacted with the 'Above' indicator. The 90 (95) percent confidence interval of the fitted polynomial is presented in dark (light) gray. Fitted polynomials and confidence intervals were generated using all available observations but, for visualization purposes, the figures show windows of +/-20 points (+/- 0.8sd) of the relative score.

Table 1: Summary Statistics

	Women	Men
	(1)	(2)
Panel A: Baseline administrative data (11 years old)		
Standardized BSSEE score	0.149	-0.104
	(0.943)	(1.033)
Panel B: 2016 Survey data (25 - 40 years old when surveyed)		
Years of education	12.999	11.173
	(4.394)	(4.314)
University degree	0.298	0.170
	(0.447)	(0.380)
Employed	0.789	0.825
	(0.403)	(0.380)
Monthly gross wage (2016 US$)	1,408	1,263
	(1,137)	(830)
Panel C: 2020 Survey data (29 - 44 years old when surveyed)		
Employed in Feb 2020	0.764	0.869
	(0.409)	(0.351)
Employed in May 2020	0.576	0.657
	(0.494)	(0.475)
Employed in Nov 2020	0.720	0.849
	(0.442)	(0.380)
Lost job (Feb 2020 - May 2020)	0.198	0.245
	(0.416)	(0.420)
Reemployed in Nov 2020	0.109	0.185
[in any job]	(0.340)	(0.366)
Reemployed in Nov 2020	0.097	0.121
[in same job as February 2020]	(0.330)	(0.308)
Individuals	114	133

Notes: This table displays summary statistics of individuals from the matched 2020 survey data covering BSSEE cohorts 1987-2002. Statistics are weighted by the inverse of sampling probability to reflect survey design. Standard deviations are reported in parentheses below the means. Employed individuals include those who answer positively to the following question: During the past 7 days, did you work in a paid job or a business, including a household business, even if only for one hour?

Table 2: 2SLS Effects on Educational Attainment and Employment

	Women	Men	(1) = (2)
	(1)	(2)	(3)
Panel A: Educational Attainment			
Years of education	2.935***	-0.899	<0.01
	(0.994) [100]	(0.786) [19]	
University degree	0.245***	-0.135	0.02
	(0.124) [100]	(0.101) [7]	
Panel B: Employment			
Employed in 2016	0.100	0.079	0.89
	(0.110) [73]	(0.085) [78]	
Employed in Feb 2020	0.266	0.099	0.33
	(0.148) [94]	(0.096) [82]	
Employed in May 2020	0.664***	-0.123	<0.01
	(0.177) [100]	(0.150) [22]	
Employed in Nov 2020	0.221	-0.046	0.12
	(0.149) [88]	(0.099) [39]	
Panel C: Job loss			
Lost job (Feb 2020 - May 2020)	-0.359**	0.162	0.01
	(0.144) [3]	(0.129) [89]	
Lost job (2016 - May 2020)	-0.330+	0.143	0.03
	(0.166) [4]	(0.145) [85]	
Lost job (2016 - Feb 2020)	-0.050	-0.040	0.94
	(0.105) [32]	(0.094) [33]	
Lost job (Feb 2020 - Nov 2020)	-0.099	0.102	0.07
	(0.099) [22]	(0.059) [85]	
Observations	652	699	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. The percentiles of the estimated effects within the distributions of 2,000 placebo cutoff effects are shown in brackets. p-values are computed as follows. (a) p-value<0.01: if the percentile of the true effect within the CDF of the placebo effects is 1 or 100; (b) p-value<0.05: if the percentile of the true effect within the CDF of the placebo effects locates within [2,3] or [98,99]; (c) p-value<0.1: if the percentile of the true effect within the CDF of the placebo effects locates within [4,5] or [96,97]. All regressions include interactions between the BSSEE 3rd order polynomial and the 'Above' indicator, cutoff fixed effects, and sociodemographic controls (student gender and parish fixed-effects). Regressions are weighted by the inverse of sampling probability to reflect survey design. Column (3) reports the p-value of a test for the equality of estimates reported in columns (1) and (2). *** p<0.01, ** p<0.05, + p<0.1.

Table 3: 2SLS Effects on Mechanisms

	Women	Men	(1) = (2)
	(1)	(2)	(3)
Panel A: Effects on layoffs and voluntary employment disruptions			
Lost job - Laid off (Feb 2020 - May 2020)	-0.346**	0.238+	<0.01
	(0.141) [3]	(0.115) [97]	
Lost job - Other reasons (Feb 2020-May 2020)	-0.014	-0.076	0.30
	(0.029) [33]	(0.057) [18]	
Panel B: Effects on fertility and access to child care			
Have member <6 yr old	-0.060	-0.177	0.65
	(0.205) [35]	(0.165) [12]	
External help with <6 yr (Feb 2020)	0.046	-0.128	0.23
	(0.071) [64]	(0.125) [8]	
External help with <6 yr (Apr-May 2020)	0.002	-0.004	0.91
	(0.045) [48]	(0.031) [39]	
Have school age member (6-17 yr old)	0.007	0.051	0.84
	(0.188) [54]	(0.109) [65]	
Panel C: Effects on selection into recession-proof occupations			
Sector-occupation job losses (Feb 2020 - May 2020)	0.085	0.090	0.98
[by Feb 2020 sector-occupation]	(0.131) [75]	(0.202) [68]	
Sector-occupation job losses (2016 - May 2020)	-0.142	-0.302	0.68
[by 2016 sector-occupation]	(0.247) [26]	(0.265) [11]	
Panel D: Effects on tenure at pre-pandemic job			
Tenure in years at job (Feb 2020)	0.122	1.867	0.56
	(2.294) [46]	(1.895) [80]	
Panel E: Effects on reemployment in November 2020			
Reemployed in Nov 2020	-0.161	0.078	0.15
	(0.106) [14]	(0.121) [77]	
Panel F: Effects on total household income			
Log HH income (Jan 2020)	0.246	-0.287	0.30
	(0.333) [83]	(0.388) [22]	
Log HH income (Apr 2020)	1.239**	0.210	0.23
	(0.603) [98]	(0.641) [66]	
Log HH income (Jul 2020)	0.687**	0.164	0.30
	(0.322) [99]	(0.389) [71]	
Log HH income (Oct 2020)	0.935***	0.239	0.14
	(0.320) [100]	(0.347) [77]	
Observations	652	699	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. The percentiles of the estimated effects within the distributions of 2,000 placebo cutoff effects are shown in brackets. p-values are computed as follows. (a) p-value<0.01: if the percentile of the true effect within the CDF of the placebo effects is 1 or 100; (b) p-value<0.05: if the percentile of the true effect within the CDF of the placebo effects locates within [2,3] or [98,99]; (c) p-value<0.1: if the percentile of the true effect within the CDF of the placebo effects locates within [4,5] or [96,97]. Sectors include 8 categories: Government; Agriculture, forestry and fishing; Accommodation and food services; Construction; Wholesale and retail trade; Finance and insurance; Transportation, storage and communications; Manufacturing industry. Occupations include the following 6 categories: Manager; Professional; Clerical support worker; Service and sales worker; Plant and machine operator; Elementary occupation. These 48 sector-occupation bins show an average size of 28 observations with a minimum (maximum) bin size of 6 (106) observations. All regressions include interactions between the BSSEE 3rd order polynomial and the 'Above' indicator, cutoff fixed effects, and sociodemographic controls (student gender and parish fixed-effects). Regressions are weighted by the inverse of sampling probability to reflect survey design. Column (3) reports the p-value of a test for the equality of estimates reported in columns (1) and (2). *** p<0.01, ** p<0.05, + p<0.1.

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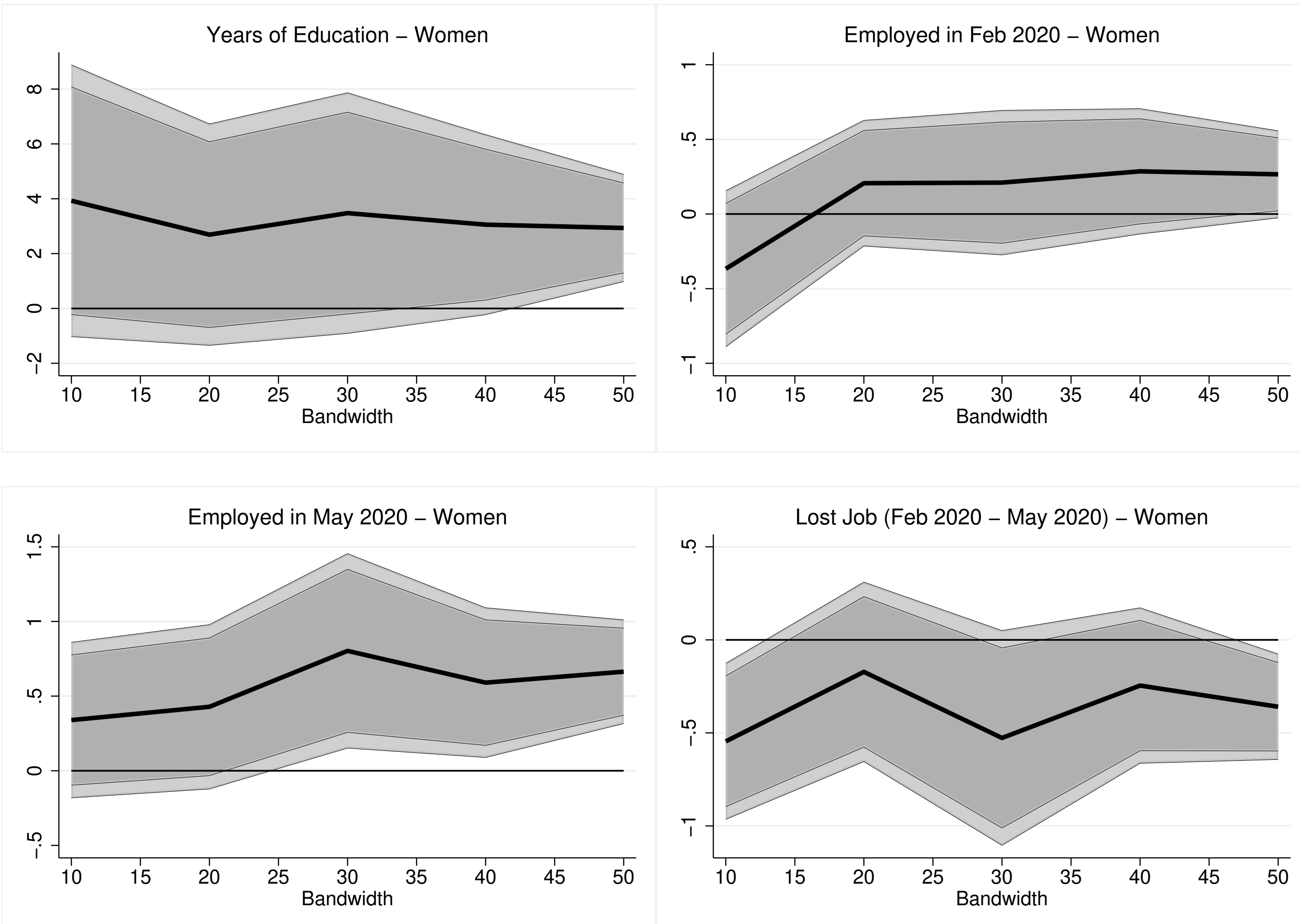
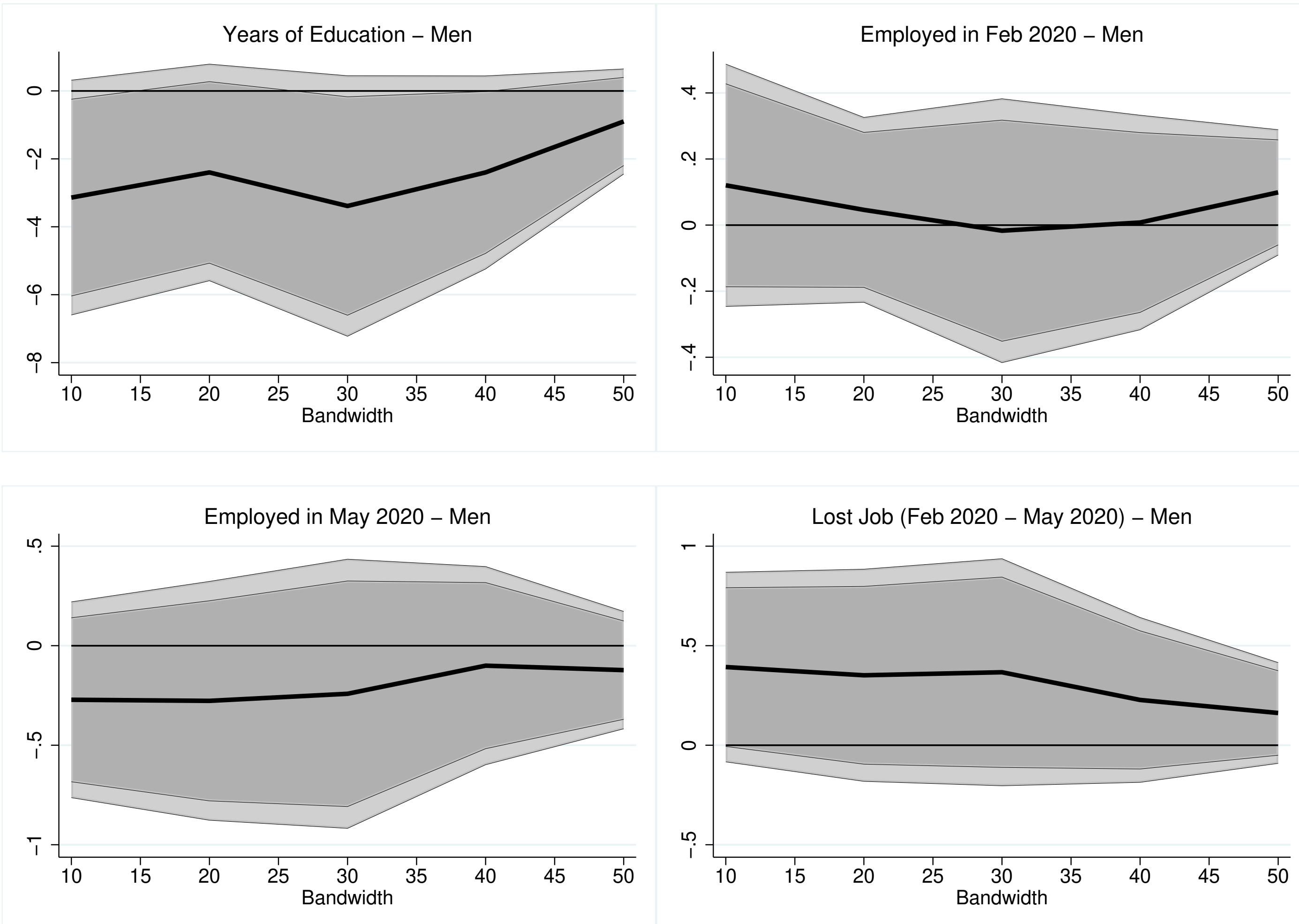
VIII Appendix

Figure A.1: 2SLS Effects by Alternative Bandwidths and Polynomial Specifications (Women)



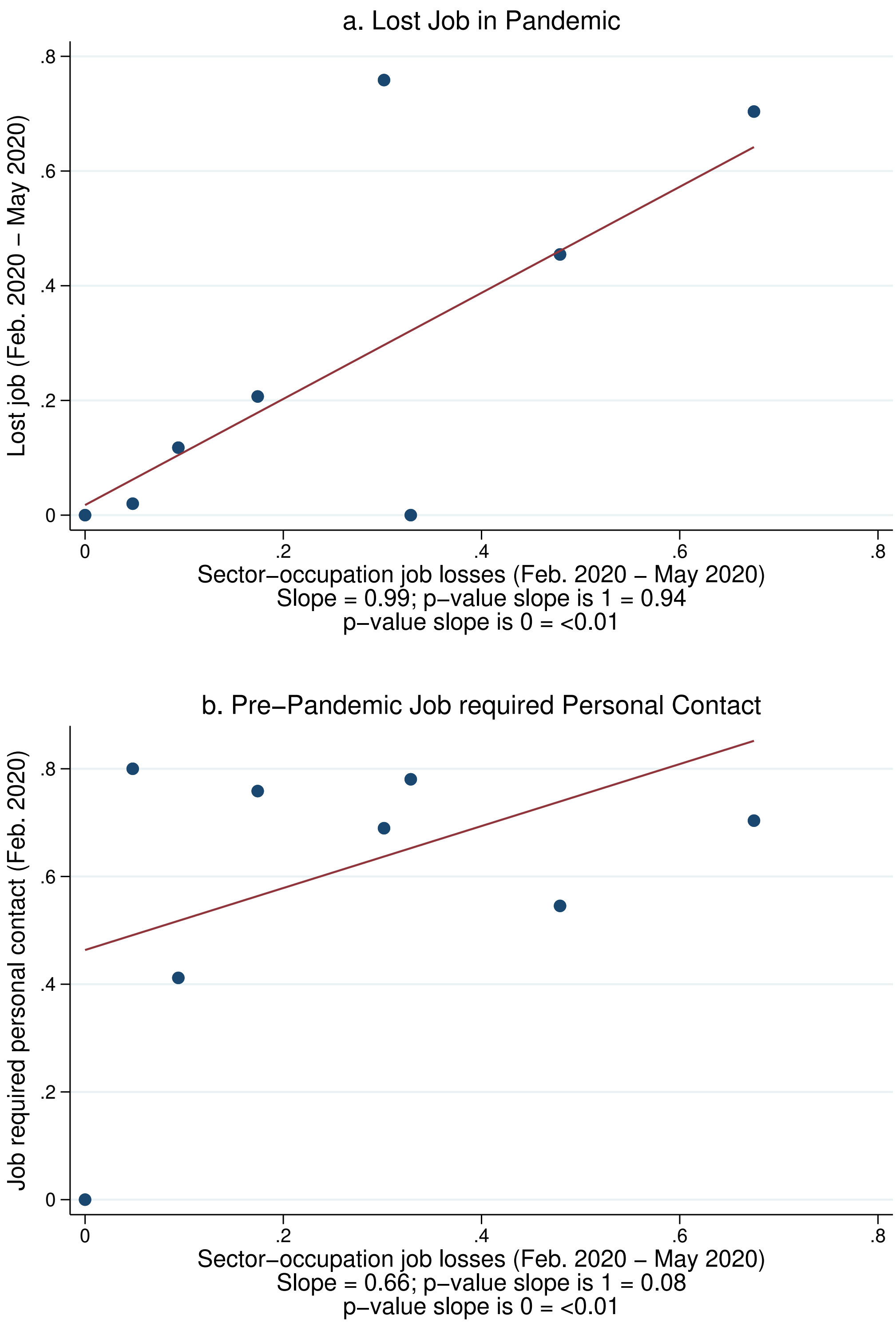
Notes: This figure depicts estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). The estimated 2SLS effects are reported for the following bandwidths [polynomial specifications]: +/- 10 (+/- 0.4sd) [linear polynomial]; +/- 20 (+/- 0.8sd) [quadratic polynomial]; +/-30 (+/-1.2sd) [cubic polynomial]; +/-40 (+/-1.6sd) [cubic polynomial]; and +/-50 (+/-2sd) [cubic polynomial]. The 90 (95) percent confidence interval of the estimated effects is presented in dark (light) gray.

Figure A.2: 2SLS Effects by Alternative Bandwidths and Polynomial Specifications (Men)



Notes: This figure depicts estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). The estimated 2SLS effects are reported for the following bandwidths [polynomial specifications]: +/- 10 (+/- 0.4sd) [linear polynomial]; +/- 20 (+/- 0.8sd) [quadratic polynomial]; +/-30 (+/-1.2sd) [cubic polynomial]; +/-40 (+/-1.6sd) [cubic polynomial]; and +/-50 (+/-2sd) [cubic polynomial]. The 90 (95) percent confidence interval of the estimated effects is presented in dark (light) gray.

Figure A.3: Sector-Occupation Job Losses



Notes: Panel a: Y-axis represents actual job losses experienced between February 2020 and May 2020. Panel b: Y-axis represents the likelihood of having a job that required personal contact with other individuals during February 2020. The X-axis represents sector-occupation job loss based on the sector-occupation reported for February 2020. The solid line represents a linear fit. Data has been grouped in eight bins represented by the solid circles.

Table A.1: Survey Representativeness

Sample:	Population	Matched Survey	(1) = (2)
	(1)	(2)	(3)
Panel A: Sociodemographics			
Female	0.498	0.464	0.36
	(0.500)	(0.500)	
Month of birth: Jan - Mar	0.241	0.246	0.95
	(0.428)	(0.430)	
Month of birth: Apr - Jun	0.222	0.288	0.07
	(0.415)	(0.448)	
Month of birth: Jul - Sep	0.250	0.209	0.25
	(0.433)	(0.409)	
Month of birth: Oct - Dec	0.287	0.258	0.41
	(0.453)	(0.446)	
Panel B: Selectivity of Secondary School Choices (mean BSSEE score of incoming class)			
Choice 1	1.236	1.189	0.48
	(0.621)	(0.591)	
Choice 2	1.026	0.988	0.78
	(0.649)	(0.605)	
Choice 3	0.938	0.834	0.04
	(0.598)	(0.556)	
Choice 4	0.710	0.543	0.02
	(0.628)	(0.678)	
Choice 5	0.455	0.334	0.07
	(0.648)	(0.646)	
Choice 6	0.237	0.182	0.73
	(0.703)	(0.694)	
Choice 7	0.049	-0.049	0.31
	(0.742)	(0.750)	
Choice 8	-0.053	-0.087	0.72
	(0.785)	(0.822)	
Choice 9	-0.154	-0.183	0.57
	(0.813)	(0.877)	
Panel C: Parish of Residency (before admission to secondary school)			
Parish 1	0.023	0.019	0.63
	(0.150)	(0.215)	

cont'd. Table A.1. Survey Representativeness

Parish 2	0.043	0.030	0.20
	(0.204)	(0.239)	
Parish 3	0.065	0.048	0.29
	(0.246)	(0.260)	
Parish 4	0.038	0.028	0.42
	(0.192)	(0.197)	
Parish 5	0.038	0.041	0.85
	(0.191)	(0.232)	
Parish 6	0.382	0.407	0.53
	(0.486)	(0.462)	
Parish 7	0.084	0.117	0.25
	(0.278)	(0.291)	
Parish 8	0.077	0.065	0.38
	(0.267)	(0.285)	
Parish 9	0.046	0.043	0.90
	(0.209)	(0.232)	
Parish 10	0.174	0.189	0.65
	(0.379)	(0.362)	
Individuals	62,755	247	

Notes: Sample corresponds to BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed for the first time in 2016). Column (1) reports means and standard deviations of the not surveyed population (i.e., individuals not covered by our survey but that we observe in the administrative BSSEE data). Column (2) reports means and standard deviations of individuals who were surveyed in May and November 2020 and matched with the BSSEE administrative dataset. Estimates in column (2) are weighted by the inverse of sampling probability to reflect survey design. Column (3) reports the p-value of a test for the equality of means reported in columns (1) and (2) adjusting for BSSEE cohorts fixed effects.

Table A.2: Validity of Identification Strategy, Survey Representativeness, and School Environments

Estimation Sample:	Population		Matched Survey		(1) = (3)	(2) = (4)	(3) = (4)
	Women	Men	Women	Men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Cutoff manipulation test							
Differential density	0.476	-1.564	-0.967	-0.140			
[p-value]	[0.634]	[0.118]	[0.333]	[0.888]			
Panel B: First Stage							
Attended preferred school	0.792***	0.823***	0.858***	0.865***	0.14	0.32	0.91
	(0.005)	(0.005)	(0.043)	(0.043)			
Panel C: Survey Response Rate - 2SLS							
Responded survey [over full population]	-0.00162	0.00006					
	(0.00119)	(0.00118)					
Responded survey [over 2016 survey sample]	0.028	0.004					
	(0.045)	(0.042)					
Panel D: School Environments Effects - 2SLS							
Peers BSSEE score	0.255***	0.251***	0.295***	0.221***	0.42	0.49	0.31
	(0.023)	(0.013)	(0.056)	(0.046)			
BSSEE coef. of variation	-0.009***	-0.010***	-0.009**	-0.013***	0.98	0.47	0.43
	(0.002)	(0.001)	(0.004)	(0.004)			
Cohort size	-9.006***	-14.419***	-12.975**	-29.073***	0.66	0.26	0.09
	(2.739)	(2.162)	(5.080)	(6.804)			
Observations	185,560	187,319	652	699			
Sociodemographics	Yes	Yes	Yes	Yes			
BSSEE cubic spline	Yes	Yes	Yes	Yes			
Cutoff fixed effects	Yes	Yes	Yes	Yes			
Sampling weights	No	No	Yes	Yes			

Notes: Panel A reports the results of the McCrary (2008) cutoff manipulation test. Panel B reports estimated coefficients on the 'Above' indicator resulting from a reduced form model as in equation (1) of the text. Panels C and D report estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. Sociodemographic controls include student gender and parish fixed-effects. Regressions in columns (3) and (4) are weighted by the inverse of sampling probability to reflect survey design. Column (5) reports the p-value of a test for the equality of estimates reported in columns (1) and (3). Column (6) reports the p-value of a test for the equality of estimates reported in columns (2) and (4). Column (7) reports the p-value of a test for the equality of estimates reported in columns (3) and (4). Sample corresponds to BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed for the first time in 2016). *** $p<0.01$, ** $p<0.05$, + $p<0.1$.

Table A.3: Reduced Form Estimates on Predicted Outcomes

	Women	Men	(1) = (2)	Prediction R2
	(1)	(2)	(3)	(4)
Panel A: Predicted educational attainment				
Predicted: Years of education	-0.356	-0.130	0.84	0.56
	(0.946)	(0.551)		
Predicted: University degree	-0.077	-0.016	0.47	0.36
	(0.075)	(0.038)		
Panel B: Predicted employment				
Predicted: Employed in 2016	0.114	-0.026	0.23	0.56
	(0.097)	(0.066)		
Predicted: Employed in Feb 2020	0.014	0.034	0.84	0.46
	(0.089)	(0.050)		
Predicted: Employed in May 2020	0.146	-0.016	0.20	0.46
	(0.102)	(0.075)		
Predicted: Employed in Nov 2020	0.084	-0.034	0.30	0.44
	(0.102)	(0.065)		
Panel C: Predicted job loss				
Predicted: Lost job (Feb 2020 - May 2020)	-0.101	0.104	0.09	0.42
	(0.088)	(0.081)		
Predicted: Lost job (2016 - May 2020)	0.028	0.044	0.88	0.38
	(0.085)	(0.066)		
Predicted: Lost job (2016 - Feb 2020)	0.079	-0.003	0.24	0.37
	(0.065)	(0.034)		
Predicted: Lost job (Feb 2020 - Nov 2020)	-0.063	0.108	0.17	0.60
	(0.113)	(0.066)		
Observations	652	699		
BSSEE cubic spline	Yes	Yes		
Cutoff fixed effects	Yes	Yes		

Notes: Columns (1) and (2) report estimated coefficients on the 'Above' indicator resulting from reduced form models as in equation (1) of the text. Left hand side variables are predicted outcomes. The predictors include all available covariates captured at BSSEE registration: year and month of birth, selectivity of school choices, and parish of residence. Estimated standard errors in parenthesis are clustered at the individual level. Sample corresponds to BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed for the first time in 2016). Estimates in columns (1) and (2) are weighted by the inverse of sampling probability to reflect survey design. Column (3) reports the p-value of a test for the equality of estimates reported in columns (1) and (2). Column (4) reports the adjusted coefficient of determination of the prediction regression for each outcome. *** $p<0.01$, ** $p<0.05$, + $p<0.1$.

Table A.4: Unweighted 2SLS Effects on Educational Attainment and Employment

	Women	Men	(1) = (2)
	(1)	(2)	(3)
Panel A: Educational Attainment			
Years of education	2.370***	-0.792	0.01
	(0.884)	(0.724)	
University degree	0.179+	-0.134	0.03
	(0.108)	(0.098)	
Panel B: Employment			
Employed in 2016	0.076	0.043	0.81
	(0.102)	(0.088)	
Employed in Feb 2020	0.135	0.115	0.90
	(0.122)	(0.106)	
Employed in May 2020	0.521***	0.012	0.01
	(0.142)	(0.140)	
Employed in Nov 2020	0.125	-0.048	0.30
	(0.137)	(0.103)	
Panel C: Job loss			
Lost job (Feb 2020 - May 2020)	-0.344**	0.021	0.05
	(0.137)	(0.121)	
Lost job (2016 - May 2020)	-0.246+	-0.056	0.31
	(0.133)	(0.136)	
Lost job (2016 - Feb 2020)	0.011	-0.081	0.44
	(0.075)	(0.098)	
Lost job (Feb 2020 - Nov 2020)	-0.112	0.089	0.13
	(0.104)	(0.082)	
Observations	652	699	

Notes: This table reports estimated 2SLS coefficients on ‘Attend’ a preferred school using ‘Above’ as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. All regressions include interactions between the BSSEE 3rd order polynomial and the ‘Above’ indicator, cutoff fixed effects, and sociodemographic controls (student gender and parish fixed-effects). Regressions are unweighted. Column (3) reports the p-value of a test for the equality of estimates reported in columns (1) and (2). *** p<0.01, ** p<0.05, + p<0.1.