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The Earnings and Income Mobility Consequences of Attending a Historically Black College/University: Matching Estimates From 2015 U.S. Department of Education College Scorecard Data

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Abstract

This article considers the labor market consequences of attending a Historically Black College/University (HBCU). With 2015 U.S. Department of Education College Scorecard Data, we use a matching estimator to identify and estimate the treatment effect of HBCU attendance on median earnings, earnings relative to a high school graduate, and income relative to that of the household at the time of initial enrollment, 6 and 10 years after attendance. Our treatment effect parameter estimates suggest that once we account for the differential return to college majors, the urban wage premium, and the proportionality/dependence of the labor market return of Black student college attendees on the share of a college/university's student population that is Black, there is a long-run earnings premium associated with HBCU attendance. In addition, for HBCUs in general, we find that there is a population of students who would realize a positive labor market premium—as high as approximately 42%—and earn more than a high school graduate if they were to attend an HBCU. With respect to intergenerational income mobility, we find that HBCU attendance enables their

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actual and potential attendees to move to a higher quantile of income relative to their households in the long run.

Keywords
Historically Black Colleges and Universities, labor market returns, intergenerational income mobility, treatment effect, matching estimator

Introduction

At least since Solnick (1990), economists and social scientists have viewed Historically Black Colleges and Universities (HBCUs) as labor market interventions that matter for the wages and earnings of attendees and graduates. Other and subsequent analyses of the labor market impact of HBCU attendance and/or graduation have produced a range of findings. Relative to non-HBCUs, attendance and/or graduation from an HBCU have been found to have no impact on wages and/or earnings (Ehrenberg & Rothstein, 1994; Fitzgerald, 2000; Robinson & Albert, 2008), a positive impact (Burnim, 1980; Constantine, 1995; Mykerezi & Mills, 2008; Price, Spriggs, & Swinton, 2011; Strayhorn, 2008; Wood & Palmer, 2017), or a negative impact (Fitzgerald, 2000; Fryer & Greenstone, 2010; Thomas & Zhang, 2005). The analysis of Espinosa, Kelchen, and Taylor (2018) suggests that HBCUs may also enable intergenerational mobility in that attendees/graduates reach quantiles in the income distribution that exceed those of the households in which they exited upon entry into college. In this article, we consider the labor market earnings and intergenerational income mobility impact from attending an HBCU. With U.S. Department of Education College Scorecard data on aggregated cohorts of college/university attendees, we use a matching estimator to estimate the effects of HBCU attendance on labor market earnings and a measure of intergenerational income mobility 6 and 10 years after attendance.

Our inquiry contributes to an understanding of the labor market and intergenerational mobility consequences of HBCU attendance and their efficacy as active labor market interventions (Crepon & Van den Berg, 2016). Fryer and Greenstone (2007) concluded that while HBCUs may have conferred unique labor market advantages to attendees in the early 1970s, by the 1990s, there was a substantial wage penalty, and that in contemporary times, HBCU attendance appears to retard Black progress. This suggests that HBCUs, as labor market treatments, are no longer effective, and, as institutions that aspire to enable human capital investments, possibly fail in generating benefits that exceed costs for attendees. Our inquiry is based on data in which labor market outcomes of HBCU attendees are captured 6 and 10 years after initial attendance, and into the initial decades of the 21st century. This enables a determination of the long-run labor market consequences of HBCU attendance and the extent to which, as labor market interventions, HBCUs merit attention for additional private and public resources that would enable them to be more successful as active labor market interventions.[1]

The remainder of this article is organized as follows: "Methodology and Data" section provides a discussion of the data and methodology. We deploy a potential outcomes approach (Rubin, 2011) that enables a parameterization of the HBCU treatment effect, which is identified within a matching estimator where selection into the treatment is independent of potential outcomes. The results are reported in Section "Results," and the "Conclusion" section concludes.

Methodology and Data

We parameterize and estimate the treatment effect of attending an HBCU within the Rubin causal framework of potential outcomes (Rubin, 2011). Following Imbens (2004), a sample is characterized by (Y_i, X_i, T_i), where the Y_i is outcomes for the treated and untreated states of $Y(1)$ and $Y(0)$, respectively; the X_i is covariates measuring individual characteristics; and the T_i is treatment indicators. For M potential matches for treated observations, the imputed potential outcomes are $\hat{Y}_i(0) = Y_i$ if $T_i = 0$, $\hat{Y}_i(0) = (1/M)\sum_{j \in l_{m(i)}} Y_j$ if $T_i = 1$, $\hat{Y}_i(1) = (1/M)\sum_{j \in l_{m(i)}} Y_j$ if $T_i = 0$, and $Y_i(1) = Y_i$ if $T_i = 1$, where $l_{m(i)}$ is an index l for $T_l \neq T_i$ that satisfies $\sum_{j|T_j \neq T_i} 1[\| X_j - X_i \| \leq \| X_l - X_i \|] = m \in M$. In general, the indicator function $l(\cdot)$ selects and matches observations in the control group that are the mth closest with respect to the distance norm $\| \cdot \|$.

In a sample of N observations with N_1 treated and N_0 controls, a matching estimator for the population average treatment effect (Abadie, Drukker, Herr, & Imbens, 2004) is

$$\tau^P = \frac{1}{N}\sum_{i=1}^{N}\left[\hat{Y}_i(1) - \hat{Y}_i(0)\right],$$

where τ^P is the treatment effect for a randomly assigned member of the population. To the extent that individuals can select in or out of treatment, the effect of the treatment on the subpopulation of treated and control observations may be of interest. As such, the population treatment effect for those actually treated and not treated respectively are

$$\tau_T^P = \frac{1}{N_1}\sum_{i:T_i=1}\left[\hat{Y}_i(1) - \hat{Y}_i(0)\right],$$

$$\tau_C^P = \frac{1}{N_0}\sum_{i:T_i=0}\left[\hat{Y}_i(1) - \hat{Y}_i(0)\right].$$

If assignment to the treatment is independent of the outcomes, then conditional on the X_i, τ^P, τ^T, and τ_T^C are identified. In this context, matching approximates a

randomized experiment, as treated and control observations are similar in observable characteristics. If observable characteristics are correlated with unobservable characteristics, matching estimators also enable causal estimates of treatment effects as treatment and control observations differ only in their assignment to treatment—or treatment status (Stuart, 2010).

In contrast to matching on a propensity score, our treatment effect estimator matches on covariates. Relative to matching on a propensity score—a scalar indicating the probability of receiving treatment—matching on covariates provides a better approximation to a fully blocked randomized experimental design (King & Nielsen, 2016). This enables treatment effect parameter estimates that are less model dependent, and with less bias (Imai, King, & Nall, 2009; Imai, King, & Stuart, 2008).

The data are from the 2015 U.S. Department of Education College Scorecard, which consists of linked college/university data on average student and institutional characteristics from several sources, including the Integrated Postsecondary Education Data System, the National Student Loan Data System, and the U.S. Treasury Department's federal earnings and tax records.[2] College Scorecard earnings data for federally aided students are based on linked earnings data from administrative tax records maintained by the U.S. Department of the Treasury. Earnings are aggregated and de-identified estimates at the institution level, and include measures such as the mean and median of the earnings distribution of Title IV federally aided students 6 and 10 years after initial attendance/enrollment. Earnings are measured as the sum of wages and deferred compensation from all W-2 forms received for each individual, plus self-employment earnings from Schedule SE. The Treasury Department wage data are of particular value as they enable the construction of median labor market earnings, the share who earn more than a typical high school graduate who enrolled/attended a college/university and received federal financial aid (e.g., Pell grants and/or subsidized student loans), and total household income of parents at the time enrolled in college/university.

We consider those institutions that are nonprofit and award at least a baccalaureate degree. Our analysis thus enables a consideration of the HBCU treatment effect for the typical attendee or graduate, based on median earnings and the percentage of students earning more than the typical high school graduate—US$25,000—both in constant 2014 dollars, 6 and 10 years after initial attendance/enrollment, absolutely, and relative to household income during initial college/university enrollment.[3] It is important to note that unlike Price et al.'s (2011) study, which considered the labor market consequences of HBCU attendance/graduation for only Black students, our results will inform the labor market consequences of HBCU attendance for an attendee of any race or ethnicity. We are not able to estimate the treatment effect of HBCU attendance just for Black attendees as the 2015 College Scorecard data are not captured across individuals by racial classification, but instead by individuals across institutions in which individual outcomes by race cannot be measured.

Given the labor market outcomes of interest, we estimate treatment effects by matching on covariates in the College Scorecard data that, in our view, best enable the

identification of the treatment effect of interest. As HBCU attendees and applicants are, relative to non-HBCU attendees and applicants, less prepared academically (Freeman & Thomas, 2002; Kim & Conrad, 2006), and more financially disadvantaged (Bennett & Xie, 2003), we use, for each college/university, the average SAT equivalent score and the median household income of students as matching covariates. To the extent that our matching specifications omit relevant variables, particularly unobservables, the inclusion of the SAT score can be a good proxy for unobservables that matter in the decision to attend a particular college/university (Dale & Krueger, 2002, 2014; Rothstein, 2004). College Scorecard data only capture data for college attendees, and the returns to schooling can differ by attendance and degree completion—the sheepskin effect (Bilkic, Gries, & Pilichowski, 2004; Trostel & Walker, 2004) which can vary by race and gender (Bitzan, 2009). As such, we also match on the institutional full-time student retention rate, and 150% completion rate to mitigate the bias in measured labor market earnings that can result from an inability to distinguish between college attendees and graduates in the College Scorecard data.[4]

Results

Table 1 reports a statistical summary of all covariates constructed from the 2015 College Scorecard data. Given the differences in the number of observations due to missing observations, parameter estimates of the effects of HBCU attendance on the labor market outcomes of interest could be subject to bias if the pattern of missing observations is not completely random. Table 2 reports the results of testing whether or not the core outcome covariates are missing completely at random (MCAR), or if there is covariate-dependent missingness (CDM). The tests are that of Li (2013) based upon that of Little (1988).[5] The null hypothesis under MCAR is rejected, whereas under CDM, it cannot be rejected. As the CDM test conditions on the matching covariates, This suggests that estimating treatment effect parameters on only complete cases due to missing observations in the College Scorecard data will not introduce any bias (Li, 2013).

As indicated in Table 1, the 2015 College Scorecard data only provide data with the covariates of interest for only 59 HBCUs, which is less than 3%, approximately, of the total number of observations in the data. However, as our matching estimator identifies the causal effect of HBCU attendance based upon comparing treated observations with a "nearest neighbor" who did not receive the HBCU treatment in the entire sample, the 59 HBCU observations are sufficient for identifying the causal effect of HBCU attendance, given selection into the treatment being conditional upon observables (Black, 2015). As the 2015 College Scorecard data comprise of all students who attended college in receipt of some form of federal assistance, our estimated treatment parameters are based upon a significant fraction of the U.S. college student population.

Tables 3 to 5 report the estimates of three treatment effects associated with attending HBCUs and elite HBCUs on our three measures of labor market outcomes based

Table 1. Covariate Summary.

	All colleges/ universities	Non-HBCUs	HBCUs
Median earnings of students working and not enrolled 6 years after entry (2014 dollars)	33,886.5	34,317.7	23,449.1
SD	8,590.8	8,451.9	4,230.2
No. of observations	1,487	1,428	59
Median earnings of students working and not enrolled 10 years after entry (2014 dollars)	42,826.9	43,271.5	32,159.3
SD	10,065.7	9,962.4	5,737.5
No. of observations	1,475	1,416	59
Share of students earning over US$25,000 six years after entry (2014 dollars)	0.6497	0.6578	0.4532
SD	0.1086	0.1012	0.0967
No. of observations	1,487	1,428	59
Share of students earning over US$25,000 ten years after entry (2014 dollars)	0.7529	0.7583	0.6241
SD	0.0837	0.0794	0.0824
No. of observations	1,475	1,416	59
Ratio of median earnings of student working and not enrolled 6 years after entry to median household income of students (2014 dollars)	0.5359	0.5397	0.4475
SD	0.1204	0.1208	0.0682
No. of observations	1,464	1,405	59
Ratio of median earnings of student working and not enrolled 10 years after entry to median household income of students (2014 dollars)	0.6756	0.6783	0.6116
SD	0.1255	0.1264	0.0758
No. of observations	1,453	1,394	59
Average SAT equivalent score of students admitted	1,061.6	1,069.7	859.3
SD	129.9	125.1	76.3
No. of observations	1,531	1,472	59
Median household income of students (2014 dollars)	63,609.9	64,057.5	52,797.9
SD	11,043.6	10,925.8	7,987.1
No. of observations	1,484	1,425	59
Full-time student retention rate of institution	0.7549	0.7594	0.6519
SD	0.1188	0.1177	0.0952
No. of observations	1,403	1,344	59
150% completion rate of institution	0.5424	0.5513	0.3409
SD	0.1757	0.1722	0.1283
No. of observations	1,397	1,338	59

(continued)

Table 1. (continued)

	All colleges/ universities	Non-HBCUs	HBCUs
Large city (population > 250,000)	0.197	0.193	0.305
SD	0.398	0.395	0.464
No. of observations	1,531	1,472	59
Percentage of undergraduates that are Black American	0.129	0.099	0.846
SD	0.178	0.098	0.162
No. of observations	1,485	1,426	59
State-supported institution	0.387	0.379	0.576
SD	0.487	0.485	0.498
No. of observations	1,531	1,472	59
Percentage of degrees awarded in agriculture, agriculture operations, and related programs	0.007	0.006	0.009
SD	0.038	0.038	0.022
No. of observations	1,485	1,426	59
Percentage of degrees awarded in natural resources and conservation	0.008	0.008	0.002
SD	0.024	0.024	0.005
No. of observations	1,485	1,426	59
Percentage of degrees awarded in architecture and related services	0.004	0.004	0.003
SD	0.033	0.033	0.010
No. of observations	1,485	1,426	59
Percentage of degrees awarded in area, ethnic, cultural, gender, and group studies	0.004	0.004	0.001
SD	0.011	0.012	0.002
No. of observations	1,485	1,426	59
Percentage of degrees awarded in communication, journalism, and related programs	0.036	0.036	0.053
SD	0.038	0.038	0.046
No. of observations	1,485	1,426	59
Percentage of degrees awarded in communications technologies, technicians and support services	0.002	0.002	0.003
SD	0.015	0.015	0.019
No. of observations	1,485	1,426	59
Percentage of degrees awarded in computer and information sciences and support services	0.019	0.019	0.024
SD	0.040	0.041	0.021
No. of observations	1,485	1,426	59

(continued)

Table 1. (continued)

	All colleges/ universities	Non-HBCUs	HBCUs
Percentage of degrees awarded in personal and culinary services	0.001	0.001	0
SD	0.008	0.008	0
No. of observations	1,485	1,426	59
Percentage of degrees awarded in education	0.069	0.069	0.068
SD	0.079	0.080	0.062
No. of observations	1,485	1,426	59
Percentage of degrees awarded in engineering	0.031	0.031	0.018
SD	0.093	0.095	0.037
No. of observations	1,485	1,426	59
Percentage of degrees awarded in engineering technologies and engineering-related fields	0.009	0.008	0.015
SD	0.029	0.029	0.030
No. of observations	1,485	1,426	59
Percentage of degrees awarded in foreign languages, literature, and linguistics	0.009	0.009	0.002
SD	0.017	0.005	0.030
No. of observations	1,485	1,426	59
Percentage of degrees awarded in family, consumer, and human sciences	0.009	0.008	0.018
SD	0.023	0.023	0.029
No. of observations	1,485	1,426	59
Percentage of degrees awarded in legal professions and studies	0.003	0.003	0.001
SD	0.023	0.024	0.0002
No. of observations	1,485	1,426	59
Percentage of degrees awarded in English language, literature, and letters	0.028	0.028	0.028
SD	0.027	0.027	0.028
No. of observations	1,485	1,426	59
Percentage of degrees awarded in liberal arts and sciences, general studies and humanities	0.054	0.055	0.029
SD	0.128	0.131	0.049
No. of observations	1,485	1,426	59
Percentage of degrees awarded in library science	0.001	0.001	0
SD	0.001	0.001	0
No. of observations	1,485	1,426	59
Percentage of degrees awarded in mathematics and statistics	0.011	0.011	0.014
SD	0.013	0.013	0.013
No. of observations	1,485	1,426	59

(continued)

Table 1. (continued)

	All colleges/ universities	Non-HBCUs	HBCUs
Percentage of degrees awarded in military technologies and applied sciences	0.001	0.001	0.001
SD	0.002	0.001	0.001
No. of observations	1,485	1,426	59
Percentage of degrees awarded in multidisciplinary and interdisciplinary studies	0.018	0.018	0.011
SD	0.034	0.037	0.029
No. of observations	1,485	1,426	59
Percentage of degrees awarded in parks, recreation leisure and fitness studies	0.025	0.025	0.025
SD	0.035	0.035	0.036
No. of observations	1,485	1,426	59
Percentage of degrees awarded in theology and religious vocations	0.024	0.024	0.002
SD	0.112	0.115	0.011
No. of observations	1,485	1,426	59
Percentage of degrees awarded in physical sciences	0.015	0.015	0.019
SD	0.031	0.032	0.029
No. of observations	1,485	1,426	59
Percentage of degrees awarded in science technologies and technicians	0.001	0.001	0.001
SD	0.005	0.005	0.001
No. of observations	1,485	1,426	59
Percentage of degrees awarded in psychology	0.061	0.060	0.074
SD	0.050	0.049	0.053
No. of observations	1,485	1,426	59
Percentage of degrees awarded in homeland security, law enforcement, firefighting, and related protective services	0.028	0.027	0.073
SD	0.058	0.057	0.065
No. of observations	1,485	1,426	59
Percentage of degrees awarded in public administration and social service professions	0.018	0.017	0.035
SD	0.040	0.039	0.038
No. of observations	1,485	1,426	59
Percentage of degrees awarded in social sciences	0.061	0.061	0.076
SD	0.074	0.074	0.073
No. of observations	1,485	1,426	59
Percentage of degrees awarded in construction trades	0.001	0.001	0
SD	0.006	0.006	0
No. of observations	1,485	1,426	59

(continued)

Table 1. (continued)

	All colleges/ universities	Non-HBCUs	HBCUs
Percentage of degrees awarded in mechanic and repair technologies and technicians	0.001	0.001	0
SD	0.014	0.015	0
No. of observations	1,485	1,426	59
Percentage of degrees awarded in precision production	0.001	0.001	0
SD	0.005	0.005	0
No. of observations	1,485	1,426	59
Percentage of degrees awarded in transportation and materials moving	0.003	0.003	0.001
SD	0.031	0.032	0.004
No. of observations	1,485	1,426	59
Percentage of degrees awarded in visual and performing arts	0.056	0.057	0.027
SD	0.119	0.122	0.020
No. of observations	1,485	1,426	59
Percentage of degrees awarded in health professions and related programs	0.128	0.130	0.082
SD	0.195	0.198	0.098
No. of observations	1,485	1,426	59
Percentage of degrees awarded in business, management marketing, and related support services	0.173	0.173	0.182
SD	0.135	0.136	0.095
No. of observations	1,485	1,426	59
Percentage of degrees awarded in history	0.019	0.019	0.016
SD	0.019	0.019	0.019
No. of observations	1,485	1,426	59

Source. 2015 U.S. Department of Education College Scorecard.
Note. HBCUs = Historically Black Colleges and Universities.

on a simple specification that matches only on the SAT score, household income, and retention/student characteristics.[6] This simple specification presumes that ability and need drive, at least in part, the college admission and/or persistence decision (Singell, 2002; Stewart, Lim, & Kim, 2015). Separate treatment parameter estimates for elite HBCUs—Hampton, Howard, Morehouse, Spelman, Xavier—are provided as there is evidence that the labor market returns are different for these elite HBCUs (Fryer & Greenstone, 2010; Price et al., 2011).[7] For each outcome of interest, similar to Price et al. (2011), we estimate the sample average treatment effect (τ^P or SATE), the sample average treatment effect on the treated (τ_T^P or SATT)—those individuals who actually received the treatment of attending an HBCU—and the sample average

Table 2. Missing Completely at Random and Covariate-Dependent Missingness Tests.

Missing completely at random	
No. of observations	1,488
$\chi^2_{25} = 20.31$	
p-value	.061
Covariate-dependent missingness	
No. of observations	1,365
$\chi^2_{112} = 46.41$	
p-value	.115

Note. The test for missing completely at random and covariate-dependent missingness is that of Cheng (2013) based upon the test proposed by Little (1988). For the covariates of interest y_i, it is assumed that $\mathbf{y} \sim N(\mu, \Sigma)$. Let $\bar{\mathbf{y}}_{oj}(p_j \times 1)$ be the observed sample average for the jth missing value data pattern for $j \in J$, and let μ_{oj} and Σ_{oj} be the $p_j \times 1$ dimensional mean vector and the $p_j \times p_j$ covariance matrix of the observed components of the jth missing value pattern, respectively. Finally, let $\gamma_i = (\gamma_{i1},\ldots,\gamma_{ip})^T$ be the p-dimensional binary indicator vector of whether each observation in y_i is observed (e.g., $\gamma_{ik} = 1$ if y_{ik} is observed, and zero otherwise), and let $\mathbf{I}_j \subseteq [1,2,\ldots,n]$ be the index set of pattern j in the sample, where $n_j = |\mathbf{I}_j|$, and $\sum_{j-1}^{J} n_j = n$. Little's χ^2 test statistic for MCAR with $\sum_{j=1}^{J} p_j - p$ degrees of freedom is

$$d_o^2 = \sum_{j=1}^{J} n_j \left(\bar{\mathbf{y}}_{oj} - \mu_{oj}\right)^T \Sigma_{oj}^{-1} \left(\bar{\mathbf{y}}_{oj} - \mu_{oj}\right).$$

If the data are MCAR, d_o^2 is a test statistic for the null hypothesis:

$$H_o : \mathbf{y}_{o,i} \big| \gamma_i \sim \mathcal{N}\left(\mu_{oj}, \Sigma_{oj}\right).$$

If H_o is rejected, the y_i cannot be viewed as MCAR. The $\bar{\mathbf{y}}_i$ is the sample mean values of the covariates and Σ_{oj} is the estimated variance-covariance matrix.
Little's χ^2 test statistic for CDM with $q(\Sigma_{j=1}^{J} p_j - p)$ degrees of freedom is

$$d_o^2 = \sum_{j=1}^{J} n_j \Sigma_{i \in \mathbf{I}} \left(\bar{\mathbf{B}}_{oj}\mathbf{x}_i - \mathbf{B}_{oj}\mathbf{x}_i\right)^T \Sigma_{oj}^{-1} \left(\bar{\mathbf{B}}_{oj}\mathbf{x}_i - \mathbf{B}_{oj}\mathbf{x}_i\right),$$

where $\mathbf{x}$ is a covariate vector, $\mathbf{B}_{oj}$ is a $p \times q$ matrix of coefficients from $\mathbf{y} = \mathbf{B}\mathbf{x} + \varepsilon$ whose rows correspond to the j th missing pattern, and $\bar{\mathbf{B}}_{oj}$ is the ordinary least squares estimator of $\mathbf{B}_{oj}$.
If the data are CDM, d_o^2 is a test statistic for the null hypothesis:

$$H_o : \mathbf{y}_{o,i} \big| \gamma_i \mathbf{x}_i \sim \mathcal{N}\left(\mathbf{B}_{oj}, \Sigma_{oj}\right).$$

If H_o is rejected, the y_i cannot be viewed as CDM.

treatment effect on the controls (τ_C^P or SATC)—an estimate of how the treatment would have affected the nontreated individuals, had they received the treatment of attending an HBCU. All parameter estimates are based on four Mahalanobis distance nearest neighbor matches with replacement, as there is evidence that matching parameter estimates are robust when selecting between one and four matches with replacement (Imbens, 2004).[8]

Table 3 reports treatment effect parameter estimates for the logarithm of median earnings. The SATE is only significant for elite HBCUs 10 years after attendance. The

Table 3. The Effect of Attending a HBCU on Median Labor Market Earnings.

Outcome	Six years after attendance	Ten years after attendance	Six years after attendance	Ten years after attendance
HBCU type	All	All	Elite	Elite
Treatment effect				
τ^{P} (SATE)	.033	.116	.032	.071
	(.040)	(.041)[a]	(.026)	(.030)[b]
τ^{P}_{T} (SATT)	−.216	−.152	.017	.118
	(.024)[a]	(.023)[a]	(.044)	(.042)[a]
τ^{P}_{C} (SATC)	.045	.128	.032	.071
	(.042)	(.043)[a]	(.029)	(.030)[b]
No. of observations	1,364	1,356	1,364	1,356
No. of matches	4	4	4	4

Note. Standard errors are in parentheses. HBCU = Historically Black Colleges and University; SATE = sample average treatment effect; SATT = sample average treatment effect on the treated; SATC = sample average treatment effect on the controls.
[a]Significant at the .01 level.
[b]Significant at the .05 level.

SATT is negative and statistically significant for all HBCUs at both 6 and 10 years after attendance, and positive and statistically significant for elite HBCUs 10 years after attendance. The SATC is positive and significant for all, and elite HBCUs 10 years after attendance. For HBCUs in general, these estimates suggest that while those who actually receive the treatment suffer an earnings penalty, the positive and statistically significant sign for SATC 10 years after attendance suggests that there is a population of students not at HBCUs, who would realize an earnings premium, if they were to attend an HBCU. In the case of elite HBCUs, all three treatment parameters are positive and statistically significant 10 years after attendance. This suggests that although there is a relative labor market penalty for actual HBCU attendance, for elite HBCU attendees, relative to all colleges/universities, there is a long-run labor market earnings premium.

Treatment effect parameter estimates for the logarithm of the share of students earning more than US$25,000 are reported in Table 4. The SATE and SATC are never statistically significant. The SATT is negative and statistically significant for all HBCUs 6 and 10 years after attendance, but is positive and statistically insignificant for elite HBCUs in each instance. This suggests that for HBCUs in general, but not for elite HBCUs, relative to all colleges/universities, actual attendance lowers the likelihood that investing in a college education will increase earnings beyond that of a typical high school graduate.

Table 5 reports treatment effect parameter estimates for the logarithm of the ratio of median earnings of students to median household income at the time they initially attended the institution of record in the College Scorecard data—a measure of intergenerational income mobility. The SATE is never statistically significant. For HBCUs

Table 4. The Effect of Attending a HBCU on Share of Students Earning More Than US$25,000.

Outcome	Six years after attendance	Ten years after attendance	Six years after attendance	Ten years after attendance
HBCU type	All	All	Elite	Elite
Treatment effect				
τ^{P} (SATE)	.045	.030	.013	−.021
	(.040)	(.025)	(.027)	(.019)
τ_T^{P} (SATT)	−.242	−.108	.017	.027
	(.025)[a]	(.015)[a]	(.045)	(.028)
τ_C^{P} (SATC)	.058	.037	.014	−.021
	(.041)	(.026)	(.027)	(.019)
No. of observations	1,364	1,356	1,364	1,356
No. of matches	4	4	4	4

Note. Standard errors are in parentheses. HBCU = Historically Black Colleges and University; SATE = sample average treatment effect; SATT = sample average treatment effect on the treated; SATC = sample average treatment effect on the controls.
[a]Significant at the .01 level.

in general, the SATT is negative and statistically significant at 6 and 10 years after attendance, but positive and statistically significant for elite HBCUs 10 years after attendance. The SATC is positive and statistically significant for all HBCUs 10 years after attendance. This suggests that actual attendees of elite HBCUs realize a relative intergenerational income mobility advantage 10 years after attendance, and actual attendees of HBCUs in general do not. However, the SATC at 10 years after attendance is positive for HBCUs in general, suggesting that there is a population of attendees who could attend a typical HBCU, and realize a relative intergenerational income mobility advantage.

The statistically significant treatment parameter estimates in Table 3 to 5 are of practical significance. As the outcomes are logarithm transformed, the significant treatment parameters imply attending an HBCU has a double-digit effect on the labor market outcome under consideration. For example, the SATT results for all HBCUs 10 years after attendance in Table 3 suggest actual HBCU attendees suffer a 15% penalty in median earnings, approximately. Based upon the mean reported for median earnings 10 years after attendance in Table 1, this translates into a earnings penalty of approximately US$6,000. This is in contrast to the SATT results for attending an elite HBCU, where attendance translates into an earnings premium of approximately 12% or US$5,000.

While based on a parsimonious and sensible treatment specification that matches on ability, need, and student retention/persistence characteristics, the treatment parameter estimates reported in Table 3 to 5 may be biased for at least three reasons that can possibly be remediated within the College Scorecard Data. First, there are differential returns to college majors (Altonjii, Blom, & Meghir, 2012; Kirkeboen, Leuven, &

Table 5. The Effect of Attending a HBCU on the Ratio of Median Earnings of Student to Median Household Income.

Outcome	Six years after attendance	Ten years after attendance	Six years after attendance	Ten years after attendance
HBCU type	All	All	Elite	Elite
Treatment effect				
τ^{P} (SATE)	.017	.091	.020	.055
	(.023)	(.026)[a]	(.025)	(.049)
τ_{T}^{P} (SATT)	−.211	−.135	.016	.120
	(.023)[a]	(.019)[a]	(.044)	(.056)[b]
τ_{C}^{P} (SATC)	.028	.101	.020	.055
	(.023)	(.027)[a]	(.025)	(.049)
No. of observations	1,364	1,356	1,364	1,356
No. of matches	4	4	4	4

Note. Standard errors are in parentheses. HBCU = Historically Black Colleges and University; SATE = sample average treatment effect; SATT = sample average treatment effect on the treated; SATC = sample average treatment effect on the controls.
[a]Significant at the .01 level.
[b]Significant at the .01 level.

Mogstad, 2016) along with evidence that HBCUs have comparative advantages in producing particular majors such as those in the sciences (Sharpe, 2016). Second, an urban wage premium exists (Baum-Snow & Pavan, 2011; Yankow, 2006). Finally, there is evidence that college students prefer to attend colleges where most of the student body shares their own race and ethnicity (Butler, 2010), and for Black college graduates, the labor market return appears to be an increasing function of the share of the student population that is Black (Price et al., 2011). Failure to match on covariates that reflect these considerations could introduce some bias in treatment effect parameter estimates, as the treatment and/or the outcomes under consideration are possibly correlated with college major, geographical population size/density, and the share of a college/university's student population that is Black.

Tables 6 to 8 report treatment parameter estimates that include the matching covariates for the estimates in Tables 3 to 5, plus augmentation with additional covariates from the College Scorecard data that can potentially mitigate any bias in treatment parameter estimates resulting from a failure to account for the differential return to college majors, the urban wage premium, and the proportionality/dependence of the college labor market return of Black students on the share of a college/university's student population that is Black. These additional matching covariates—their summary reported in Table 1—include a binary variable for the college/university being in city with a population that exceeds 250,000, the percentage of a college/university's undergraduate population that is Black American, and the percentage of degrees awarded across 36 categories of college majors. We also include a binary variable for the college/university being a state-supported institution, as these college/universities,

Table 6. The Effect of Attending a HBCU on Median Labor Market Earnings.

Outcome	Six years after attendance	Ten years after attendance	Six years after attendance	Ten years after attendance
HBCU type	All	All	Elite	Elite
Treatment effect				
τ^P (SATE)	.225	.402	.075	.096
	(.024)[a]	(.027)[a]	(.025)[a]	(.029)[a]
τ^P_T (SATT)	−.106	.117	.316	.043
	(.045)[b]	(.041)[b]	(.052)[a]	(.041)
τ^P_C (SATC)	.239	.415	.074	.098
	(.025)[a]	(.028)[a]	(.025)[a]	(.029)[a]
No. of observations	1,364	1,356	1,364	1,356
No. of matches	4	4	4	4

Note. Standard errors are in parentheses. HBCU = Historically Black College/University.
[a]Significant at the .01 level.
[b]Significant at the .05 level.

Table 7. The Effect of Attending a HBCU on Share of Students Earning More Than $25,000.

Outcome	Six years after attendance	Ten years after attendance	Six years after attendance	Ten years after attendance
HBCU type	All	All	Elite	Elite
Treatment effect				
τ^P (SATE)	.273	.401	.078	.029
	(.032)[a]	(.021)[a]	(.026)[a]	(.018)
τ^P_T (SATT)	−.167	.035	.287	−.044
	(.045)[b]	(.027)	(.059)[a]	(.028)
τ^P_C (SATC)	.293	.417	.078	.031
	(.033)[a]	(.022)[a]	(.026)[a]	(.018)
No. of observations	1,364	1,356	1,364	1,356
Number of matches	4	4	4	4

Note. Standard errors are in parentheses. HBCU = Historically Black College/University.
[a]Significant at the .01 level.
[b]Significant at the .05 level.

relative to private colleges/universities, are more likely to offer degree programs with a practical emphasis tied to particular jobs in the labor market (Brint, Riddle, Turk-Bicakci, & Levy, 2005).

Table 6 reports treatment effect parameter estimates for the logarithm of median earnings; in contrast to the estimates in Table 3, the SATE is always positive and statistically significant. The SATT is only negative and significant in one instance—for all HBCUs 6 years after attendance—but otherwise positive and statistically

Table 8. The Effect of Attending a HBCU on the Ratio of Median Earnings of Student to Median Household Income.

Outcome	Six years after attendance	Ten years after attendance	Six years after attendance	Ten years after attendance
HBCU type	All	All	Elite	Elite
Treatment effect				
τ^{P} (SATE)	.275	.454	.067	.089
	(.043)[a]	(.044)[a]	(.025)[a]	(.047)[b]
τ^{P}_{T} (SATT)	−.089	.134	.311	.039
	(.042)[b]	(.040)[a]	(.052)[a]	(.055)
τ^{P}_{C} (SATC)	.292	.468	.066	.089
	(.045)[a]	(.045)[a]	(.025)[a]	(.047)[b]
No. of observations	1,364	1,356	1,364	1,356
Number of matches	4	4	4	4

Note. Standard errors are in parentheses. HBCU = Historically Black College/University.
[a]Significant at the .01 level.
[b]Significant at the .05 level.

significant for all and elite HBCUs. The SATC is always positive and significant for all and elite HBCUs. For HBCUs in general, these estimates suggest that while those who actually receive the treatment suffer an earnings penalty 6 years after attendance, it is always positive for elite HBCUs, and for HBCUs in general, it is positive after 10 years of attendance. The positive and significant sign for SATC 10 years after attendance suggests that there is a population of students not at HBCUs, who would realize an earnings premium, if they were to attend an HBCU. In the case of elite HBCUs, all three treatment parameters are positive and significant 10 years after attendance. This suggests that while there is a relative labor market penalty for HBCU attendance 6 years after attendance, this is not true for elite HBCUs, and this earning penalty disappears, translating into an earning premium for all HBCU attendees 10 years after attendance.

The SATT parameter estimates in Table 6 also suggest that for all HBCUs, there is a substantial earnings premium of approximately 12%, 10 years after attendance. In addition, the positive and significant SATC for all HBCUs suggests that there is a population of students who would realize a positive relative labor market premium as high as approximately 42%—10 years after attendance—if they were to attend an HBCU. For this subpopulation, the estimated SATC suggests that attending an HBCU would have resulted in a relative labor market premium in 2015, which is not consistent with Fryer and Greenstone's (2010) finding of a declining and negative post-1990 labor market return for HBCU attendees.

Treatment effect parameter estimates for the logarithm of the share of students earning more than US$25,000 are reported in Table 7. The SATE is positive and statistically significant in all but one case—elite HBCUs 10 years after attendance. The

SATT is negative and statistically significant for all HBCUs 6 years after attendance, and positive and statistically significant for elite HBCUs 6 years after attendance. The SATC is always positive and statistically significant except in one instance—10 years after attending an elite HBCU. This suggests that for elite HBCU attendees, relative to all colleges/universities, actual attendance increases the likelihood that investing in a college education will increase earnings beyond that of a typical high school graduate, 6 years after attendance. For HBCUs in general, attendees fare worse than high graduates 6 years after attendance, but this difference disappears 10 years after attendance. When statistically significant, the SATC is always positive, suggesting that there is a population of students not at HBCUs, who would realize earnings higher than the typical high school graduate if they were to attend an HBCU.

Table 8 reports treatment effect parameter estimates for the logarithm of the ratio of median earnings of students to median household income at the time they initially attended the institution of record in the College Scorecard data—a measure of intergenerational income mobility. The SATE, SATT, and SATC are all positive and statistically significant. The always positive and statistically significant SATC suggests that there is a population of attendees who could attend a typical HBCU and realize a relative intergenerational income mobility advantage.

The treatment parameter estimates in Table 8 complement the recent analysis of Espinosa et al. (2018) with respect to HBCU attendance and intergenerational income mobility. For all HBCUs, the positive and significant SATT 10 years after attendance suggests that these HBCUs enable their attendees to move to a higher quantile of income relative to their households in the long run. The positive and statistically significant SATC 6 and 10 years after attendance suggests that HBCUs could realize more income mobility among attendees than they currently enable, as there is a population of students who would realize intergenerational income mobility if they were to attend an HBCU.

Conclusion

This article considered the labor market earnings and intergenerational income mobility impact from attending an HBCU. With U.S. Department of Education College Scorecard data on aggregated cohorts of college/university attendees, we use a matching estimator to estimate the effects of HBCU attendance on labor market earnings and a measure of intergenerational income mobility 6 and 10 years after attendance. Our results suggest that once we account for the differential return to college majors, the urban wage premium, and the proportionality/dependence of the labor market return of Black student college attendees on the share of a college/university's student population that is Black, there is a long-run earnings premium associated with HBCU attendance. In addition, for HBCUs in general, we find that there is a population of students who would realize a positive labor market premium—as high as approximately 42%—and earn more than a high school graduate if they were to attend an HBCU. With respect to intergenerational income mobility, we find that HBCU attendance enables

their actual and potential attendees to move to a higher quantile of income relative to their households in the long run.

Similar to Price et al. (2010), our results suggest that HBCUs, as labor market interventions, continue to have a compelling educational justification. While the findings of Fryer and Greenstone (2007, 2010), and Dale and Krueger (2014) suggest that the labor market returns to HBCU attendance are in secular decline, our treatment parameter estimates from 2015 U.S. Department of Education College Scorecard data suggest otherwise. For HBCUs in general, our findings suggest that the labor returns to attendance, even when negative 6 and 10 years for actual attendance, could be rendered positive if they were able to recruit individuals from the subpopulation of controls in our data, who did not attend, but would have realized a labor market premium if they had, as the estimated treatment effect for the controls is almost always positive and statistically significant. This subpopulation of potential HBCU attendees could include individuals who are not the traditional constituency of HBCUs such as Asians and Latinos (Maramba, Palmer, Yull, & Ozuna, 2015). In this context, our findings suggest that HBCUs are successful in generating human capital investments where the costs exceed the benefits for attendees. Given the history of HBCUs receiving unequal resources (Ortega & Swinton, 2018), our results suggest that HBCUs merit attention for additional private and public resources that would enable them to be more successful as active labor market interventions for a wide variety of students, and not just their historical constituency—the descendants of American Negro slaves.

There are at least three limitations of our results that merit attention. First, our treatment effect parameter estimates are identified if assignment to the treatment is only a function of the observable characteristics of individuals. If there are unobservables that matter for attending an HBCU, our treatment effect parameter estimates could be biased. However, as Imbens (2004) shows, this need not cause bias if the unobservables are unrelated to the outcome of interest. In our analysis, we are confident that the covariates we match on capture, say, the unobservable marginal costs of investing in human capital, which could matter. As such, we are confident that our parameter estimates identify the effect, possibly causal, of HBCU attendance on labor market outcomes.

Second, unlike Price et al. (2011), which considered the labor market consequences of HBCU attendance/graduation for only Black students, given the limitations of the 2015 Scorecard data that do not permit measurement of individual race/ethnicity characteristics, our results inform the labor market consequences of HBCU attendance for an attendee of any race or ethnicity. To the extent that HBCUs confer distinct psychological advantages upon Black students that boost labor market earnings (Price et al., 2011), the treatment effects reported here may not precisely inform how effective, or not, HBCU attendance is as a labor market treatment for Black students—the historical constituency of HBCUs. However, HBCUs are no longer, and have not been for some time, the only choice for Black students, and several have in the recent decades become less Black in their student population (Allen, Jewell, Griffin, & Wolf, 2007; Gasman, 2013; Henry & Closson, 2010).[9] In this context, the treatment effect parameter estimates reported here are relevant for a college and university admissions/enrollment environment in which HBCUs are potential

educational options and labor market interventions for students from a wide variety of racial and ethnic backgrounds.

Finally, our results make no distinctions between treated and untreated institutions with respect to geography or gender. To the extent that these factors are important for post-attendance labor market outcomes, the treatment effect estimates reported here could be biased. However, to the extent that individuals with similar measured characteristics are also similar on unmeasured characteristics, and/or if these unmeasured characteristics are unrelated to measured characteristics (Imbens, 2004), the treatment effect parameter estimates reported here are unbiased.

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Notes

1. In contrast to Fryer and Greenstone (2007), our approach utilizes data that provide a more exact measure of earnings, based on what is reported on official tax returns, and captures the labor market outcomes of Historically Black College/University (HBCU) attendees in more contemporary times—Fryer and Greenstone's analysis ends in the late 1990s.
2. College Scorecard data are publicly available at https://collegescorecard.ed.gov
3. In the Scorecard data, US$25,000 corresponds approximately to the median earnings/wages of workers age 25 to 34 years with only a high school diploma.
4. The 150% completion rate is the percentage of an entering degree-seeking cohort in 4-year public higher education institutions that complete a postsecondary degree within 6 years.
5. We implement these tests in Stata 15.0 with the mcartest command.
6. All parameter estimates were implemented in Stata 15.0 with the nnmatch command (Abadie, Drukker, Herr, & Imbens, 2004). As the simple matching estimator can be biased when matching is not exact on the covariates, we bias-adjust our treatment parameter estimates via the approach of Abadie and Imbens (2011), which nnmatch allows.
7. Hampton, Howard, Morehouse, Spelman, and Xavier are defined as elite based upon being ranked in the top five of all HBCUs in 2018 by *U.S. News and World Report*.
8. The Mahalanobis distance between two covariate vectors $\mathbf{X}_i$ and $\mathbf{X}_j$ is $d(i,j) = ([\mathbf{X}_i - \mathbf{X}_j]'S^{-1}[\mathbf{X}_i - \mathbf{X}_j])^{1/2}$, where $S = \text{cov}(\mathbf{X}_i, \mathbf{X}_j)$. Mahalanobis distance matching on the closest control observations with replacement enables a treatment effect estimator with the lowest conditional bias (Abadie & Imbens, 2006; Rubin, 1979).
9. For example, Gasman (2013) notes that Asian American and White enrollment at HBCUs has increased by double-digit percentages in recent decades.

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