Effects of College Quality: Occupation and Major as Mechanisms of Earnings Increases

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Abstract

This paper contributes to the literature on the causal effect of college quality on student labor market outcomes by exploring mechanisms behind earnings impacts, with a focus on choice of major and occupation. Using data from the NLSY97 and a "selection on observed variables" identification strategy with a rich set of covariates, I find that students who attend high-quality colleges sort into higher-paying occupations. These large and statistically significant effects of college quality on predicted earnings of occupation can account for over half of the earnings return to college quality. On the contrary, predicted earnings by major are not found to be affected by college quality in any economically meaningful or statistically significant way. I apply the framework from Oster (2019) to probe my results for sensitivity, and find that they are robust to a variety of plausible assumptions about the nature of selection on unobserved variables.

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1 Introduction

Choosing a college is one of the largest human capital decision made by many students and their families, and colleges vary vastly in their quality. Previous literature has explored impacts of college quality on earnings but still relatively little is known about the mechanisms that lead to these earnings impacts. This paper quantifies how college quality affects students' propensity to sort into high-earning majors or occupations. Results show that occupation choice is an important driver of earnings gains from college quality: students who attend higher quality colleges are substantively and statistically significantly more likely to work in higher-paying occupations. The magnitude of this increase in predicted earnings of occupation is over half that of the increase in earnings; I estimate that attending a college that is 10 percentiles higher in the college quality distribution increases annual earnings by $1,427 and increases annual predicted earnings of occupation by $768. In a sharp contrast, college quality has no discernible effect on the probability of choosing a higher-paying major.

I rely on a "selection on observed variables" strategy, taking advantage of the rich set of observed characteristics provided by the National Longitudinal Survey of Youth (NLSY) 1997 cohort. I examine sensitivity to this assumption using the framework developed by Oster (2019) and find that my main conclusions hold under a variety of assumptions about the nature of potential selection on unobserved characteristics. I also explore heterogeneity in outcomes with respect to sex, parental education, and ability.

The rest of the paper proceeds as follows: Section 2 reviews related literature. Section 3 lays out a theoretical framework for understanding the effects of college quality on outcomes. Section 4 describes the data used, as well as the measures of college quality and predicted earnings of major/occupation. Section 5 presents the empirical models and identification strategies. Section 6 presents results, and section 7 concludes.

2 Related Literature

This paper contributes to the active literature of college quality's effects on labor market outcomes. It closely relates to Dillon and Smith (2020), a paper that uses the NLSY 1979 and 1997 cohorts to examine the question of how college quality and student-college match[1] affect graduation rates and earnings. Using a "selection on observed variables" identification strategy, they find positive effects of college quality on earnings across the entire student ability distribution, with additional evidence of complementarities between student ability and college quality for some outcomes. These effects are long lasting, persisting until 10-11 years after the college decision for individuals in the 1997 cohort (which is as far as the data allows analysis) and until 30-31 years after the college decision for those in the 1979 cohort.

In addition to Dillon and Smith (2020), there are a number of other papers that analyze the impacts of college quality (Dale and Krueger, 2002; Black et al., 2005; Hoekstra, 2009; Dale and Krueger, 2014; Zimmerman, 2014; Ge et al., 2018; Mountjoy and Hickman, 2020; Smith et al., 2020). The literature seems to agree on a positive causal effect of college quality on earnings for at least some groups, although the magnitude and affected group vary by study. There is also strong support for college quality affecting graduation rates, and some evidence for other outcomes. However, there is little evidence on mechanisms of earnings increases. A better understanding of these mechanisms can help us understand *why* some colleges have causal effects on earnings which can inform policy.

This study shines light on how choice of occupation and major are affected by college quality, and quantifies these channels as mediators for earnings increases. Although there have been studies examining the large differences in returns to various majors (see Altonji et al. (2016) for an overview), there is less work examining the role of college quality in determining major choice. Arcidiacono et al. (2016) consider the interaction of college and major in determining students' outcomes. They focus on differences in returns

[1] "Student-college match" refers to interaction effects of student ability and college quality.

between STEM (science, technology, engineering, and math) and non-STEM majors at various University of California campuses. Their results show that less academically-prepared minorities at higher quality colleges are less likely to persist in STEM majors, lowering their overall returns to their institution-major combination. Overall, I find that attending a high-quality college does not translate into sorting into a higher-paying major. However, I do find some evidence that higher-ability students see a larger effect of college quality on major choice, which helps reconcile my results with Arcidiacono et al. (2016). Ost et al. (2019) uses matched employer-employee administrative data from Ohio to decompose major and institutional (i.e. college) premia into individual- and firm-specific components. In line with my findings, they find that firm fixed effects are much more important in explaining institutional premia in wages than major fixed effects. Majors are more important in explaining individual-specific premia. This paper complements theirs by providing evidence for a causal effect of college quality on occupational sorting.

3 Framework for Understanding College, Major and Occupation Choice

This section presents an informal way to conceptualize how college quality may affect my outcomes of interest. It is inspired by the simple model of major choice presented in Altonji et al. (2016). I assume rational and forward-looking agents. Students first sort into colleges, then majors, then occupations. The choice of where (and whether) to attend college depends on many things including ability, information, and preferences. I plausibly control for the factors that affect this sorting through my identification strategy described in detail in section 5.

After choosing a college, students choose among available majors. This choice depends on future expected utility over various occupations, human capital (general and major-specific), information (about one's ability and preferences for various majors as well as the careers they lead to), and preferences. The occupation choice after leaving college is similar. Chosen occupation (and the set of feasible options) depends on earnings, major, human capital (general and occupation-specific), a noisy signal of ability observed by employers, information, and preferences. There are several channels through which college quality could enter these decisions. First, college quality could affect information at both the major choice stage and the occupation choice stage through better advising and access to networks (of peers, alumni, faculty with industry connections, etc.). Second, college quality will affect the noisy signal that is sent to potential employers. A college that is more selective or prestigious may strengthen the signal of its students and increase their choice set of occupations. Finally, a higher quality college will increase one's general and major-/occupation-specific human capital.

4 Data and Measurement

4.1 Data

I use the National Longitudinal Survey of Youth (NLSY) 1997, a survey in the United States that began interviewing participants aged 12 to 16 in 1997. They were interviewed every year until 2011, after which they have been interviewed every 2 years. I use the representative sample supplemented by the over-samples of Black and Hispanic individuals and use probability of inclusion weights constructed by the NLSY. The NLSY is a good data source for studying effects of college quality since it includes a rich set of variables that can help control for selection into colleges of various qualities. It contains demographics, family background characteristics, and several measures and proxies for cognitive and non-cognitive ability. Using the restricted-access geocode data provides additional information about individuals' home counties, as well as the names of all post-secondary institutions attended. The NLSY is also an excellent data set for studying labor market outcomes, since it keeps detailed records of respondents' employment histories including information about their occupations. Although measurement error is a concern in any survey data set (especially when measuring occupation), improvements in the survey design have greatly reduced measurement error in occupation in NLSY97 as compared to NLSY79, one of the main reasons that I focus my analysis on the later cohort.[2]

One disadvantage of the NLSY is its modest sample size. I include individuals who hold a high school degree or GED, began attending a four-year post-secondary institution by age 21, and have non-missing measures of student ability, college quality, and the relevant outcome (earnings, major, or occupation). This leaves me with a sample of less than 2000 individuals for all outcomes analyzed. Another limitation of the NLSY97 is its lack of full earnings trajectories, since respondents are still in their mid-thirties. So earnings measures 6-7 years and (to a lesser extent) 10-11 years after beginning college should be thought of as noisy measures of permanent income.

4.2 Measure of College Quality

To measure college quality and student ability, I use the same measures employed by Dillon and Smith (2020). The college quality measure is a continuous index obtained by principal component analysis from several measures that proxy for college quality: SAT/ACT of entering students[3], undergraduate student-faculty ratio, applicant rejection rate, and average salary of faculty engaged in instruction. As discussed in Black and Smith (2006), using a composite of several measures decreases measurement error of college quality. However, this strategy likely does not completely eliminate measurement error, the remaining of which would attenuate estimates toward zero. For ease of interpretation, I convert the raw principal component scores to percentiles of the enrollment-weighted distribution of four-year colleges. For all outcomes analyzed, I use the first college attended.[4]

[2]Essentially, before 1994, respondents were asked every year to describe their occupation which was then coded to occupations. Differences in descriptions/interpretations often led to respondents being coded as switching occupations even if they remained in the same position from year to year. Beginning in 1994 (and thus before any interviews with the 97 cohort), respondents were first read their description from the previous year and asked if it still applied, and only required to describe their occupation again if they indicated that they had switched occupations within the past year (U.S. Bureau of Labor Statistics, 2021).

[3]Specifically, I take the midpoint of the 25th and 75th percentiles of SAT/ACT scores reported by each college.

[4]I use first college in order to make a weaker assumption about the exogeneity of college quality on outcomes (discussed further in section 5) and view transfers as intermediate outcomes. However, results using the last college instead of the first college are similar.

4.3 Measures of (Predicted) Earnings

Earnings are measured by a survey question in the NLSY that asks respondents to report their total earnings for the previous year. Following Dillon and Smith (2020), I average earnings in two-year intervals and use the observed year when one year is missing (or zero) within an interval. If neither year in an interval has nonzero earnings reported I omit it. All earnings are measured in real 2010 dollars.

I construct measures of the predicted earnings by major and occupation in the 2009 American Community Survey (ACS).[5] I regress yearly earnings on indicators for each occupation (major) and use the coefficient on each occupation (major) as its measure of predicted earnings, controlling for race, ethnicity, and sex. This is very similar to the strategy used by Katz et al. (2020) in their analysis of a sectoral employment program's effect on the average earnings of participants' occupation.[6] In this regression, I restrict the sample to individuals who are approximately the same age as the NLSY sample at the times that they are answering the earnings questions. Since I measure respondents' actual earnings 6-7 and 10-11 years after beginning college, I also choose these timeframes from the ACS for predicted earnings by occupation/major. For example, for the predicted earnings measures 6-7 years after beginning college, I include individuals in the ACS who are between the ages of 23 and 27.[7] These variables are also measured in real 2010 dollars.

Major is measured in 29 categories. I always use the last major that individuals had while attending any college, and for simplicity ignore minors or double majors.[8] Occupation is measured using 2002 Census 3-digit classification codes, which is the finest

[5]I access the ACS data through IPUMS (Ruggles et al., 2019). I use the year 2009 since it is the first year in which the ACS asks about field of study in which individuals earned their degree. This is not ideal since respondents in the NLSY97 graduated from high school several years prior. I cannot check the correlation between 2009 and earlier years (closer to when individuals in the NLSY attended college) for any variable that requires major, but I do investigate the correlation between predicted earnings by occupation. The correlation between 2003 and 2009 is 0.84, so I do not believe using 2009 would meaningfully affect my results. Occupation codes used in the ACS and NLSY are the same, but major codes vary slightly. I manually matched the NLSY majors that did not have an exact match in the ACS categorization. These matches are in appendix C.

[6]The differences between my specification and the one is Katz et al. (2020) are that they use the most recent 2013-2015 ACS samples, and do not control for race, ethnicity, or sex.

[7]98% of my sample started college between the ages of 17 and 20.

[8]In the case of double majors, respondents indicate which one is their primary major.

measure of occupation available in the NLSY. There are 266 unique occupations in my sample.[9]

5 Empirical Strategy

For each outcome, I estimate the equation:

$$Y_i = \beta_0 + \beta_1 Q_i + \beta_x \vec{X}_i + \varepsilon_i \tag{1}$$

where Q_i is college quality, measured as described in section 4.2, and $\vec{X}_i$ is a vector of conditioning variables. I estimate equation (1) as a baseline specification for all outcomes.

One of the main challenges in addressing the effects of college quality lies in the selection problem: students are not assigned to colleges at random, and if there is a simple correlation between high quality colleges and positive labor market outcomes it is not clear if that is because the college causes the outcomes or because the types of students who would be successful in the labor market anyway are attending high quality colleges. To address this issue, I use a "selection on observed variables" identification strategy, following Dillon and Smith (2020). Specifically, the assumption that underlies my identification strategy is that, conditional on the variables included in X_i, college quality Q_i is independent of the error term, ε_i. In equation form, $E(\varepsilon_i|Q_i, X_i) = E(\varepsilon_i|X_i)$. Because of the rich data provided by the NLSY, this is a reasonable identification strategy in this setting. My conditioning set includes variables that measure or reasonably proxy for many factors that prior literature has found to affect college choice.

One of the greatest threats to identification is unobserved student ability. Higher-ability students may be more likely to attend high-quality colleges and could also be more likely to succeed in the labor market. Thus, I include several measures of student ability.

[9] I also present sensitivity analysis using the broader 2-digit classification codes. To find the occupation of the NLSY respondent, I consider any occupation within the two year window being considered (i.e. either 6-7 years after starting college or 10-11 years after staring college), choosing the occupation closest to the midpoint when there is a switch within the two years. If two occupations are held simultaneously, I take the one with more hours worked. If there is still a tie, I take the one with the higher wage.

My primary student ability measure is the first principal component of eight sections of the Armed Services Vocational Aptitude Battery (ASVAB), a test administered to most NLSY participants. It is measured as percentiles in the enrollment-weighted distribution of college starters in my sample. Other measures of ability include high school GPA and SAT scores. I add controls that capture non-cognitive skills that may not be captured by grade or exam scores, such as whether the student has been suspended from school or was rated as "uncooperative" by their interviewer. I also include several demographic measures, parental education and family income, and county-level measures to control for neighborhood influences. See appendix A for a full list of conditioning variables.[10]

However, because there still may exist some selection bias that has not been controlled for in my conditioning set, I perform sensitivity analysis following Oster (2019). Her paper closely follows and extends Altonji et al. (2005) by proposing a framework to draw qualitative conclusions or bounds for coefficients of interest in the presence of some unobserved confounding variables. In section 6.2, I apply this framework under a set of various assumptions suggested by Oster (2019) to give several ways of interpreting my results. All qualitative conclusions drawn from the baseline specification hold.

6 Results

6.1 Main Specification

Table 1 shows the results of estimating equation (1). For clarity, estimates from all coefficients besides college quality are suppressed. For earnings, I present estimates for outcomes realized at two different points in time, which both measure the number of years since respondents began college. Because the respondents of the NLSY97 are still young enough that they haven't necessarily reached their permanent income, I put more interpretive weight on the later estimates (10-11 years after starting college) and view the earlier estimates (6-7 years after starting college) as a robustness check and an indication

[10] See section 5 of Dillon and Smith (2020) for more evidence of the validity of this identification strategy.

of earning trajectories. College quality is standardized between 0 and 100, so estimates reflect the increase in earnings from a attending a college with 1 percentage point higher quality. Thus, the estimate 10-11 years after starting college of 142.77 implies that attending a college that is 10 percentage points higher in the quality distribution raises annual earnings by about $1,427, on average.[11] This estimate is highly statistically significant: a test of the null hypothesis that the coefficient on college quality is equal to zero has a p-value of less than 0.001. The estimate of the impact of college quality on earnings 6-7 years after starting college implies that going to a college that is 10 percentage points higher on the college quality distribution increases earnings by around $835, on average. These two estimates together show a stable and increasing positive effect of college quality on earnings.[12]

The next four columns explore possible mechanisms for this earnings increase: choosing a major or occupation that generally earns more. Focusing on the predicted earnings by major, my estimates reveal that there is no evidence for major choice as a mechanism for earnings increases. At both 6-7 years and 10-11 years after beginning college, estimates of the effect of college quality on predicted earnings by major are small and statistically insignificant. In contrast, the impact of college quality on working in a high-paying occupation is shown to be important, especially in the longer time horizon. The sixth column shows that attending a college 10 percentiles higher in the quality distribution raises predicted earnings of occupation by $768, which explains around half of the overall estimated increase in earnings. In the shorter run, occupation choice looks to be less consequential for earnings effects.

[11] To give a sense of what a 10pp increase in the college quality distribution is, note the following colleges in each decile of the distribution: 95: University of Rochester (New York), 85: University of Wisconsin - Madison, 75: University of Colorado - Denver, 65: Xavier University (Ohio), 55: California State University - Long Beach, 45: North Dakota State University, 35: University of Mississippi, 25: Indiana State University, 15: Unity College (Maine), 5: Texas Southern University.

[12] However, the increase is not statistically significant.

6.2 Robustness

As discussed in section 5, I implement the framework of Oster (2019) to check how robust effects are to the presence of unobservable confounding variables. I focus on estimates 10-11 years after starting college for this analysis. There are three parameters of importance in this framework. First, β_1 is the coefficient of interest, which in my case represents the effect of college quality on earnings or predicted earnings by major/occupation. Second, δ is the proportion of selection on unobservables relative to selection on observables (e.g., $\delta = 1$ if the selection on observables is equal to the selection on unobservables; $\delta = 0.5$ if there is twice as much selection on observables as on unobservables.) This is the primary parameter that is varied in Altonji et al. (2005) and is used to answer the question, "What would the ratio of selection on unobservables to selection on observables need to be in order for the entire causal effect of the coefficient of interest to be entirely due to selection bias?" Third, R_{max} is "the R-squared from a hypothetical regression of the outcome on treatment and both observed and unobserved controls" (Oster, 2019). Oster notes that in many empirical settings this is likely to be less than one, due to measurement error or expected idiosyncratic variation in the outcome. She suggests using $R_{max} = 1.3 \times \widetilde{R}$ where $\widetilde{R}$ is the R^2 from my main specification. She obtains this from an empirical application of her framework on a set of published articles that use randomized data. She argues that since we know that effects using randomized data are causal (as long as the data was correctly randomized), we should pick a value of R_{max} for which most of these results should survive her adjustment procedure that sets $\delta = 1$. Under $R_{max} = 1.3 \times \widetilde{R}$, 90 percent of the randomized results that were significantly different from zero are still significantly different from zero if we assume that $\delta = 1$. Practically, setting $R_{max} = 1.3 \times \widetilde{R}$ can be interpreted as an assumption that unobservables explain 30 percent as much variation in the outcome as the observables.

Thus, in my first robustness analysis in Table 2, I fix $R_{max} = 1.3 \times \widetilde{R}$ across all columns (as a reminder, $\widetilde{R}$ is the R^2 from my main specification). In the first, third, and fifth column, I additionally fix $\delta = 1$. This means that I am assuming that the selection on unobservables is equal to the selection on observables, then ask what the effects of college quality on earnings and predicted earnings of major/occupation would be. This represents a lower bound for the estimates; it is a conservative estimate in this case since the NLSY97 contains such a rich set of observed variables. In the second, fourth, and sixth columns, I instead fix $\beta = 0$, and ask how much selection the unobservables would have to explain (relative to the observables) for college quality to have no effect on the outcome. In each column, the estimated parameter is in bold.

Results show that the qualitative conclusions hold that college quality matters for earnings and occupation, even if there is considerably more selection on unobservables than observables. For the earnings (occupation) estimates, there would have to be 54% (65%) more selection on unobservables than observables in order for the effect to be zero. This seems unlikely in this setting with very rich data. The estimates for β show that if selection on observables and unobservables were equal, the effects of college quality on earnings and occupation would be considerably smaller, but would still give the same qualitative result. Specifically, they imply that a 10 percentile increase in college quality would increase earnings and predicted earnings of occupation by \$674 and \$420, respectively. For both outcomes, these are around half the size of the coefficients estimated in the baseline specification (\$1427 and \$768 for a 10pp increase in college quality). Because of the large set of controls included in the main specification, the degree of selection on unobservables is likely considerably less than the selection on observables, implying the true value is likely closer to the "upper bound" shown in Table 1 than the lower bound estimated here. As for the effects of college quality on students sorting into higher-paying majors, although the lower bound of β remains above 0, it is economically small.

I present additional robustness checks in appendix D. Continuing with the Oster framework in Table D.2, I present the R_{max} values that are consistent with zero effect sizes under equal selection on observables and unobservables. Table D.3 shows the effect of college quality on predicted earnings of occupation if occupation is measured at the Census 2-digit level as opposed to the 3-digit level. Both of these tables support my main findings. Finally, I present results from models that allow for nonlinear effects of college quality in appendix E. These results show that the baseline specification where college quality enters linearly is not a bad approximation in most cases, but suggest that there may be steeper increases in returns from college quality towards the top of the college quality distribution.

6.3 Heterogeneity

Next I look for heterogeneous effects along three dimensions: sex, parental education, and ability. Because of my relatively small sample sizes, I interact college quality with each subgroup and estimate pooled coefficient for the other conditioning variables. I also take a coarse approach to measuring parental education and ability. I group students parents' into either having a HS degree or less, or at least some college. Parental education is measured as the parent with the highest degree. Ability is broken into above or below the median of the sample, as measured by my main ability measure described in section 5. I also look for heterogeneity by whether students completed a BA. By conditioning on BA completion, I am estimating these results under a different conditional independence assumption. That is, in order for these results to be interpreted as causal, my conditioning set must not only control for selection into initial college choice but also into degree completion. Therefore, I view my results from this analysis as suggestive. For the heterogeneous effects, I focus on the longer time horizon, only presenting estimates for outcomes 10-11 years after respondents start college.

Table 3 shows heterogeneous earnings effects for sex, parental income, and ability, focusing on the longer time horizon. P(CQ,Int) gives the p-value from a joint test of significance of college quality and college quality interacted with the subgroup, while P(Int) gives the p-val of a t-test of the interaction alone. Because I estimate pooled coefficients for all covariates except college quality and the varying subgroups, N(subgroup) denotes the number of observations in each subgroup analyzed while Obs gives the overall size of the estimating sample. The first four columns reveal that there is not much evidence of heterogeneous earnings returns to college quality along sex or parental education, but the last two columns indicate evidence of larger returns for higher ability students. This supports the findings of Dillon and Smith (2020), although, like them, I cannot reject the null that the interaction term between college quality and student ability is equal to zero.

Table 4 presents the same analysis for predicted earnings of major, and reveals interesting heterogeneity with regard to parental education. Lower parental education students who attend higher quality colleges are more likely to sort into higher-paying majors, while there is no effect for students whose parents have at least some college. Perhaps the additional advising resources that come with high-quality colleges are more valuable to students who are not able to get as much advice about college from their parents. There is also some suggestive evidence that men, as well as higher ability students, experience a stronger college quality effect on major choice. However, in all three cases the joint test of statistical significance of college quality and its interaction do not reject the null of no effects at the 5 percent level[13], and for sex and ability the interaction alone is not significant. Finally, table 5 gives some suggestive evidence for higher returns of college quality to predicted earnings of occupation for high-ability students and students with lower parental education. But again, none of the interaction terms are statistically significant.

Finally, I consider whether the effects of college quality differ by BA completion status. Table 6 shows effects for the subgroup of students who obtained a BA at some point in the 10 years since starting college. Table 6 shows that the results remain relatively unchanged for earnings and predicted earnings of major. However, the effect of college quality on predicted earnings of occupation falls quite a bit: from \$762 to \$536 for a 10 percentile increase in college quality. This implies that part of the gains in predicted earnings of occupation from attending a high-quality college rely on students persisting to graduation. This result appears to be driven by a stark difference between graduates and non-graduates in the likelihood of entering an occupation in management or professional fields. In table 7, I present the distribution of students over very broad occupation categories. Thirty-eight percent of students who do not complete a BA end up with a management or professional occupation, as opposed to nearly 80 percent for those with a

[13]The null is rejected at the 10 percent level for parental education.

BA. However, as noted in section 5, limiting the sample to only college graduates imposers a stronger conditional independence assumption which complicates interpretation of the results. I view these results as suggestive and so do not place much interpretative weight on the exact numbers.

7 Conclusion

This paper analyzes the effects of college quality on earnings and predicted earnings of major and occupation. Results provide clear support for a positive effect of college quality on working in a high-paying occupation, with large and statistically significant effects of college quality on predicted earnings of occupation that survive under several assumptions about the presence of selection on unobservable characteristics. Conversely, I do not find evidence for an effect of college quality on choosing a high-paying major.

More work is needed to understand the relationship between college quality and students' choices of major and occupation. Although I highlighted several potential channels through which college quality affects these outcomes, it would be interesting to explore which are most important to the finding that college quality increases predicted earnings by occupation. If information and advising are important, this could inform policy for colleges and directions for government funding. Information experiments as in Wiswall and Zafar (2015) may be able to provide further evidence about how students make occupation choices in addition to major choices. It will also be important to provide more evidence on how earnings and predicted earnings evolve over the full life-cycle.

8 Tables

Table 1: College Quality Effects on Earnings and Predicted Earnings of Major and Occupation

	Earnings		Predicted Earnings			
			Major		Occupation	
	6-7 years	10-11 years	6-7 years	10-11 years	6-7 years	10-11 years
College Quality	83.54	142.77	-4.73	10.14	23.17	76.82
	(19.78)	(29.76)	(6.67)	(8.53)	(12.69)	(17.83)
P-val	<0.001	<0.001	0.478	0.235	0.068	<0.001
Obs	1,828	1,694	1,814	1,814	1,896	1,750
R-sq	0.08	0.13	0.10	0.09	0.05	0.09

Notes: College Quality shows the estimate of the effect of college quality on each outcome at the top of the table from estimating equation (1). Years represents the number of years since starting college for which the outcome is being measured. Standard errors in parentheses. P-val gives the p-value of t-tests under the null of each coefficient = 0. The variables listed in appendix A are included but the coefficient and standard error estimates are suppressed.

Table 2: College Quality Effects on Earnings and Predicted Earnings of Major and Occupation, Sensitivity using Oster bounds (10-11 years after starting college)

	Earnings		Predicted Earnings			
			Major		Occupation	
R_{max}	0.169	0.169	0.123	0.123	0.112	0.112
δ	1	**1.54**	1	**2.03**	1	**1.65**
β	**67.36**	0	**5.98**	0	**42.02**	0

Notes: For each column, the number in bold is the estimate subject to the other two numbers being fixed. Each row reflects one parameter of the Oster framework: R_{max} is the maximum R-squared that could be achieved if all observed and unobserved controls were included, δ is the ratio of selection on unobservables relative to selection on observables, and β is the coefficient on college quality if selection on unobservables is adjusted for according to R_{max} and δ.

Table 3: College Quality Effects on Earnings, Heterogeneity by Sex, Parental Education, and Ability (10-11 years after starting college)

	Gender		Parental Ed.		Ability	
	Female	Male	HS or less	Some College	Low	High
College Quality	132.67	156.24	163.19	135.98	109.74	171.56
	(35.34)	(46.63)	(51.88)	(34.66)	(32.77)	(38.19)
P(CQ,Int)	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
P(Int)	0.675	0.675	0.656	0.656	0.124	0.124
N(subgroup)	940	753	421	1,254	902	791
Obs	1,693	1,693	1,675	1,675	1,693	1,693
R-sq	0.13	0.13	0.13	0.13	0.13	0.13

Notes: College Quality shows the estimate of the effect of college quality on earnings for each subgroup listed at the top of the table, defined as described in the text. Standard errors in parentheses. P(CQ,Int) gives the p-value of a joint test of significance of college quality and college quality interacted with the subgroup under the null of each coefficient = 0. P(Int) give the p-value of a t-test of the interaction term alone under the null of each coefficient = 0. Estimates are obtained by interacting college quality with each subgroup; pooled coefficients are estimated for all other conditioning variables. N(subgroup) gives the number of observations in each subgroup and Obs gives the overall number of observations in the estimating sample. The variables listed in appendix A are included but the coefficient and standard error estimates are suppressed for clarity.

Table 4: Effects of College Quality on Predicted Earnings of Major, Heterogeneity by Sex, Parental Education, and Ability (10-11 years after starting college)

	Gender		Parental Ed.		Ability	
	Female	Male	HS or less	Some College	Low	High
College Quality	0.77	21.16	36.15	-0.07	1.49	17.23
	(9.91)	(12.34)	(15.83)	(9.15)	(9.82)	(10.39)
P(Q,Int)	0.222	0.222	0.071	0.071	0.214	0.214
P(Int)	0.152	0.152	0.037	0.037	0.151	0.151
N(subgroup)	1,017	795	442	1,348	968	844
Obs	1,812	1,812	1,790	1,790	1,812	1,812
R-sq	0.10	0.10	0.10	0.10	0.10	0.10

Notes: College Quality shows the estimate of the effect of college quality on predicted earnings of major for each subgroup listed at the top of the table, defined as described in the text. Standard errors in parentheses. P(CQ,Int) gives the p-value of a joint test of significance of college quality and college quality interacted with the subgroup under the null of each coefficient = 0. P(Int) give the p-value of a t-test of the interaction term alone under the null of each coefficient = 0. Estimates are obtained by interacting college quality with each subgroup; pooled coefficients are estimated for all other conditioning variables. N(subgroup) gives the number of observations in each subgroup and Obs gives the overall number of observations in the estimating sample. The variables listed in appendix A are included but the coefficient and standard error estimates are suppressed for clarity.

Table 5: Effects of College Quality on Predicted Earnings of Occupation, Heterogeneity by Sex, Parental Education, and Ability (10-11 years after starting college)

	Gender		Parental Ed.		Ability	
	Female	Male	HS or less	Some College	Low	High
College Quality	79.26	73.70	113.29	63.02	97.04	59.03
	(21.10)	(25.68)	(39.41)	(19.23)	(20.92)	(21.56)
P(Q,Int)	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
P(Int)	0.853	0.853	0.233	0.233	0.103	0.103
N(subgroup)	985	765	432	1,300	945	805
Obs	1,750	1,750	1,732	1,732	1,750	1,750
R-sq	0.09	0.09	0.08	0.08	0.09	0.09

Notes: College Quality shows the estimate of the effect of college quality on predicted earnings of occupation for each subgroup listed at the top of the table, defined as described in the text. Standard errors in parentheses. P(CQ,Int) gives the p-value of a joint test of significance of college quality and college quality interacted with the subgroup under the null of each coefficient = 0. P(Int) give the p-value of a t-test of the interaction term alone under the null of each coefficient = 0. Estimates are obtained by interacting college quality with each subgroup; pooled coefficients are estimated for all other conditioning variables. N(subgroup) gives the number of observations in each subgroup and Obs gives the overall number of observations in the estimating sample. The variables listed in appendix A are included but the coefficient and standard error estimates are suppressed for clarity.

Table 6: Effects of College Quality on Earnings and Predicted Earnings of Major and Occupation, Conditional on Graduating with a BA (10 years after starting college)

	Earnings	Predicted Earnings	
		Major	Occupation
College Quality	147.33	11.19	53.57
	(35.58)	(9.86)	(20.38)
P-val	<0.001	0.257	0.009
Obs	1,166	1,265	1,193
R-sq	0.12	0.11	0.08

Notes: College Quality shows the estimate of the effect of college quality on each outcome at the top of the table from estimating equation (1), where the sample is restricted to individuals who completed a bachelor's degree. Standard errors in parentheses. P-val gives the p-value of t-tests under the null of each coefficient = 0. The variables listed in appendix A are included but the coefficient and standard error estimates are suppressed for clarity.

Table 7: Occupation Distribution by BA Status

Broad Occupation Group	Holds BA				Total	
	No		Yes			
	No.	%	No.	%	No.	%
Management, Professional, and Related	154	37.6%	875	79.6%	1029	68.2%
Service	108	26.5%	85	7.8%	193	12.8%
Sales and Office	63	15.5%	96	8.7%	159	10.6%
Farming, Fishing, and Forestry	3	0.7%	1	0.1%	4	0.3%
Construction, Extraction, Maintenance, and Repair	36	8.8%	18	1.6%	54	3.6%
Production, Transportation, and Material Moving	44	10.8%	25	2.2%	69	4.6%
Total	408	100.0%	1100	100.0%	1508	100.0%

Notes: Table gives the distribution of workers into the broadest Census-defined occupation categories, by BA completion status. The first two columns give the number and percent of workers in each occupation group for 4-year college starters who did not complete a BA. The next two columns give the number and percent of workers in each occupation group who graduated with a BA, and the final two columns give the overall distribution unconditional on BA completion status.

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Appendices

A Conditioning Set

1. College quality, as described in section 4.2
2. Student ability, as described in section 5
3. ASVAB 2 percentile: second principal component of Armed Service Vocational Aptitude Battery
4. High school GPA percentile
5. SAT/ACT percentile
6. Indicators for participating in petty anti-social behavior by 8th grade: ever suspended from school, ever intentionally destroyed or damaged someone else's property, or ever stolen something worth $50 or less
7. Indicator for being rated uncooperative by the NLSY interviewer
8. Indicator for having sex by age 15
9. Sex
10. Race/ethnicity
11. Family income quartile
12. Number of siblings
13. Parental education
14. Indicator for U.S. region
15. Indicator for living within Metropolitan Statistical Area in 1997
16. Percent of adults with college degree in home county
17. Median income of home county

B Major Categorization

1. Agriculture/Natural resources
2. Anthropology
3. Architecture/Environmental design
4. Area studies or Ethnic studies
5. Biological sciences
6. Business management
7. Communications
8. Computer/Information science
9. Criminology
10. Economics
11. Education
12. Engineering
13. English
14. Fine and applied arts
15. Foreign languages
16. History
17. Home economics
18. Mathematics
19. Nursing
20. Philosophy
21. Physical sciences
22. Political science and government
23. Pre-dental, Pre-med, and Pre-vet
24. Pre-law
25. Psychology
26. Sociology
27. Theology/religious studies
28. Nutrition/Dietetics
29. Hotel/Hospitality management

C Major Matching ACS to NLSY

Table C.1: Manual Matching of Majors between NLSY and ACS

NLSY	ACS
Agriculture/Natural Resources	Agriculture Environment and Natural Resources
Architecture/Environmental Design	Architecture
Area Studies Ethnic Studies	Area, Ethnic, and Civilization Studies
Education	Education Administration and Teaching
Foreign languages	Linguistics and Foreign Languages
Pre-Law	Law
English	English Language, Literature, and Composition
Biological Sciences	Biology and Life Sciences
Mathematics	Mathematics and Statistics
Philosophy	Philosophy and Religious Studies
Theology/Religious studies	Theology and Religious Vocations
Fine and Applied Arts	Fine Arts
Pre-vet Pre-med Pre-dental Other Health Professionals	Medical and Health Sciences and Services
Home Economics	Cosmetology Services and Culinary Arts
Nutrition/Dietetics	Nutrition Sciences
Anthropology	Anthropology and Archaeology
Hotel/Hospitality Management	Hospitality Management

Notes: Matching decisions make by the author. All majors not listed had exact word matching between NLSY and ACS categorizations.

D Robustness

In this section, I supplement the sensitivity analysis in section 6.2 with two additional robustness checks. Table D.2 shows the third and final way of implementing the Oster (2019) framework. Here, I fix the degree of selection on unobservables to be equal to that of observables (δ=1) and the effect of college quality to be zero ($\beta = 0$). For these assumptions to hold, the first row shows the scale that would be needed relative to the R^2 from my baseline specification. The second row shows the value of R_{max} that this produces. In all cases, the baseline R^2 would need to be multiplied by at least 1.5, implying that unobservables must explain at least half as much variation as observables. This seems unlikely given the extensive set of covariates included in the baseline specification.

Table D.2: College Quality Effects on Earnings and Predicted Earnings of Major and Occupation, Sensitivity using Oster framework (10 years after starting college)

	Earnings	Predicted Earnings	
		Major	Occupation
R^2 Multiplier	**1.520**	**1.620**	**1.590**
R_{max}	**0.198**	**0.153**	**0.140**
δ	1	1	1
β	0	0	0

Table D.3 examines the sensitivity of predicted earnings of occupation estimates to the categorization of occupation. In the main results, I use the 2002 Census 3-digit occupation codes but here I used the broader 2-digit codes. Results are not meaningfully changed in any way.

Table D.3: College Quality Effects on Predicted Earnings of Occupation, Sensitivity using Coarser Occupation Measure

	6 years	10 years
College Quality	28.26	71.39
	(10.72)	(15.23)
P-val	0.008	<0.001
Obs	1,898	1,759
R-sq	0.05	0.08

E Nonlinearity

In this section, I explore nonlinearity in the effect of college quality on earnings and predicted earnings of occupation. I implement two different specifications - one that has a polynomial in college quality and one that includes dummies for each quartile of college quality. For the polynomial specification, I estimate:

$$Y_i = \beta_0 + \beta_1 Q_i + \beta_2 Q_i^2 + \beta_4 A_i + \beta_x X_i + \varepsilon_i \tag{2}$$

As discussed in Dillon and Smith (2020), polynomials can be less stable around the edges of the data, so I also implement a specification with dummies for each quartile of the college quality distribution. Thus, for robustness, I estimate:

$$Y_i = \beta_0 + \beta_1 Q_2 + \beta_2 Q_3 + \beta_3 Q_4 + \beta_4 A_i + \beta_x X_i + \varepsilon_i \tag{3}$$

where the bottom quartile is the omitted category.

Table E.4 displays the marginal effects of college quality on earnings and predicted earnings of occupation when college quality enters as a second-degree polynomial. The final row of table E.4 shows the p-value of a test where the null is that the coefficient on the second-degree term is equal to zero. For all outcomes, the null is not rejected at the 5 percent level, implying that nonlinear effects may not be very important in this context. However, there is some suggestive evidence that effects increase more rapidly towards the top of the college quality distribution, especially for earnings 10 years after starting college. For example, focusing on the second column in table E.4, at the 50th percentile of the college quality distribution, the point estimate of 147.41 implies that increasing college quality by 10 percentiles increases earnings by $1,427, on average. However, at the 80th percentile, this point estimate jumps to $2,770 for a 10 percentile increase in college quality, on average.

Table E.5 shows the results from estimating equation (3), where the bottom quar-

Table E.4: Marginal Effects of College Quality on Earnings and Predicted Earnings by Occupation (Polynomial)

	Earnings		Expected Earnings Occ	
	6 years	10 years	6 years	10 years
p10	7.18	-25.33	11.68	67.60
	(52.31)	(84.45)	(34.30)	(49.83)
p20	26.96	17.86	17.42	71.12
	(39.74)	(63.97)	(26.46)	(38.79)
p30	46.74	61.04	23.17	74.65
	(28.40)	(45.22)	(19.26)	(28.57)
p40	66.52	104.23	28.91	78.17
	(20.44)	(31.43)	(13.71)	(20.44)
p50	86.30	147.41	34.66	81.70
	(20.32)	(30.36)	(12.33)	(17.59)
p60	106.08	190.60	40.40	85.22
	(28.15)	(42.97)	(16.21)	(22.16)
p70	125.86	233.78	46.14	88.74
	(39.44)	(61.34)	(22.80)	(31.02)
p80	145.64	276.97	51.89	92.27
	(51.99)	(81.67)	(30.40)	(41.52)
p90	165.42	320.15	57.63	95.79
	(65.07)	(102.80)	(38.40)	(52.68)
Observations	1,828	1,694	1,895	1,750
R-squared	0.062	0.116	0.033	0.074
$Pr(Q = 0, Q^2 = 0)$	0.000	0.000	0.019	0.000
$Pr(Q^2 = 0)$	0.154	0.053	0.503	0.770

Notes: Marginal effects are calculated from the college quality coefficients in equation (2). Years represents the number of years since starting college for which the outcome is being measured. Standard errors in parentheses. The variables listed in appendix A are included but the coefficient and standard error estimates are suppressed for clarity. The final two rows give the p-values from Wald tests that the coefficients on college quality are jointly equal to zero, where the latter excludes the main effect of college quality to test whether the second-order term is equal to zero.

Table E.5: Effect of College Quality on Earnings and Earning Propensity of Major and Occupation (CQ quartiles)

	Earnings		Occupation	
	6 years	10 years	6 years	10 years
Q2	570	1,290	1,012	2,576
	(1,141)	(1,838)	(811)	(1,117)
Q3	3,076	3,357	1,297	2,937
	(1,276)	(2,002)	(861)	(1,213)
Q4	4,715	9,800	2,069	6,000
	(1,620)	(2,424)	(967)	(1,398)
Obs	1828	1694	1895	1750
R-sq	0.06	0.11	0.03	0.07
$(Quartiles = 0)$	0.009	<0.001	0.183	<0.001
$(Q1 = Q2)$	0.617	0.483	0.212	0.021
$(Q2 = Q3)$	0.065	0.313	0.739	0.755
$(Q3 = Q4)$	0.323	0.013	0.403	0.019

Notes: College Quality Quartile shows the estimate of the effect of each college quality on each outcome at the top of the table from estimating equation (3), where the omitted quartile is the bottom one. Years represents the number of years since starting college for which the outcome is being measured. Standard errors in parentheses. The variables listed in appendix A are included but the coefficient and standard error estimates are suppressed for clarity. The fourth row from the bottom gives the p-value from Wald test that the coefficients on college quality are jointly equal to zero. The final three rows give p-values from tests where the null is coefficients on each adjacent pair of quartiles is equal to zero.

tile of the college quality distribution is omitted. These results again imply that the linear specification is a good approximation, with the exception of the top of the college quality distribution. The final 3 rows of table E.5 give p-values of tests of the null that each pair of adjacent quartile coefficients are equal. In both outcomes 10-11 years after beginning college, the null is rejected that the difference between the third and fourth quartile of the college quality distribution is equal to 0. In the predicted earnings of occupation, the null is also rejected that the first and second quartile are equal. An interpretation of the magnitudes of these estimates is as follows: focusing on the last column, the point estimate for Q4 implies that going from the bottom quartile of the college quality distribution to the top quartile raises one's predicted earnings of occupation by \$6,000. The corresponding point estimate for Q3 implies that moving from the bottom quartile to the third quartile only increases predicted earnings of occupation by \$2,937.

Taken together, these results show that the baseline specification where college quality enters linearly is not a bad approximation in most cases. Evidence suggests that the main departures from linearity are at the top of the college quality distribution. It's important to keep in mind that my measure of college quality is ordinal, not cardinal, so it may be that the driver of these results is that there are larger gaps in the underlying latent college quality variable between colleges at the top of the distribution than those at the bottom. However, it is still valuable to learn that the differences between the causal effects of college quality on earnings and predicted earnings of occupation are larger at the top of the college quality distribution than towards the middle or bottom.