Model Specifications for Estimating Labor Market Returns to Associate Degrees: How Robust Are Fixed Effects Estimates?

A CAPSEE Working Paper

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April 2017

Abstract

Recently, studies have adopted fixed effects modeling to identify the returns to college. This method has the advantage over ordinary least squares estimates in that unobservable, individual-level characteristics that may bias the estimated returns are differenced out. But the method requires extensive longitudinal data and involves complex specifications, raising the possibility that results are sensitive either to sample restrictions or to alternative specifications. Also, the extra requirements might not be justified if results from fixed effects models are broadly similar to those from conventional ordinary least squares models. In this paper we review results from fixed effects models of the earnings gains from completing an associate degree relative to non-completion for community college students. We examine both sampling restrictions and specification issues. We find results to be sensitive to assumptions about missing earnings data and to how time trend specifications are modeled. However, we find no substantively meaningful differences between estimates using fixed effects models and ordinary least squares methods. A main benefit of fixed effects models—controlling for unobservable student characteristics—should be weighed against the difficulty in interpreting coefficients and more intensive data requirements. On the other hand, a distinct advantage of fixed effects models is that they allow for analysis of earning profiles over the period from before to after college. Given the large fluctuations in earnings over this period, this advantage may be significant in yielding evidence on the full returns to college.

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1. Introduction

For many decades, the labor market returns to college have been estimated using the Mincerian Ordinary Least Squares (OLS) functional form (Barrow & Malamud, 2015). Although robust and straightforward to interpret, the well-known limitation of this form is that it does not permit causal interpretation: evidence of higher earnings for degree holders may reflect unobserved ability or motivation (Rouse, 2007). One response to this limitation is the use of fixed effects (FE) models (since Jacobson, LaLonde, & Sullivan, 2005). Fixed effects models estimate the gains from college as the individual-level increase in earnings between time periods (usually quarters of years) when individuals have an award compared with time periods when they do not have an award. The advantage of this approach is that within-person characteristics that are typically unobserved (e.g., ability) should be differenced out of the identified returns. This fixed effects approach has been made possible by the availability of large-scale student-level administrative datasets from postsecondary education systems (see Figlio, Karbownik, & Salvanes, 2015). On average, conclusions from these analyses correspond with the OLS consensus: more intensive postsecondary education has effects on earnings that are positive, economically meaningful, sustained over the career and business cycle, and consistent across states and institutions.[1] These conclusions do not hold for all college pathways but are robust for associate degrees (see Belfield & Bailey, 2017).[2]

However, FE models may have some methodological drawbacks. In a review of studies across social sciences, Sobel (2012, p. 527) concludes that "researchers … have not defined the effects they are attempting to estimate or understood the types of conditions that must be met in order to estimate treatment effects…. This can lead to incorrect conclusions and misguided policy recommendations." We identify two potential drawbacks. One is that FE model specifications are significantly more complicated and involve more variables than basic OLS specifications. Notably, FE models involve assumptions about time trends and earnings trajectories of students during and after college (Henderson, Polachek, & Wang, 2011). Results may therefore be sensitive to model specifications. The other issue is that FE models are more data intensive: they require data before, during, and after college. If these data are missing or unavailable (e.g., for students with no work experience before they enter college), then results may be biased. Even as FE models should eliminate biases from individual-level unobservable characteristics, the sample restrictions and model specification complexities may render these models non-robust. Moreover, if their results are broadly equivalent to results from OLS models, it may not be worthwhile to collect more intensive data and run more complex FE models.

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[1] By more intensive, we mean that the student either accumulates more credits or earns an award. Evidence for these claims is reviewed in the synthesis by Belfield and Bailey (2017). See also Barrow and Malamud (2015) and Oreopoulos and Petronijevic (2013).

[2] Earnings gains from shorter term certificates dissipate a few years after exiting college. Gains from associate degrees in liberal arts or general studies are quite low; although if these students transfer to a four-year institution the returns are high (see Dadgar &Trimble, 2015; Liu, Belfield, & Trimble, 2015).

In this review paper we investigate the robustness of FE models. First, we describe the key differences between OLS and FE models. Next, we examine how results from FE models vary across different sample restrictions and model specifications. Then we examine whether Mincerian OLS models yield different results than FE estimations. For these analyses we use administrative data from North Carolina and summarize evidence from other statewide datasets. To clearly illustrate sampling and functional form sensitivities we focus on the returns to completion relative to non-completion of associate degree programs for students starting at community college.[3] We conclude by summarizing the advantages of applying FE models.

2. Model Specifications

Mincerian OLS Earnings Specifications

Following Mincer (1974), a large volume of studies has estimated the labor market returns to college. These studies applied Ordinary Least Squares (OLS) regression models of earnings against highest level of attainment, controlling for work experience and other covariates:

$$Y_t = \alpha + \theta EDUC_{t-k} + \delta EXP_t + \varphi EXP_t^2 + \beta X_{t-j} + \gamma Z_{t-k} + \varepsilon \tag{1}$$

In equation (1), earnings Y (or log earnings) at quarter t are a function of: college education, $EDUC$, represented as, for example, awards or credits; work experience and its square, EXP and EXP^2; a vector of attributes of college (e.g., sector), X; and a vector of pre-college personal and ability-related characteristics, Z. The coefficient θ represents an estimate of the earnings premium from education obtained in college (or the percentage increase if the variables are expressed in log form).[4]

The Mincerian OLS approach has several strengths. OLS results are easy to report and interpret. The dependent variable earnings is easily expressed (e.g., at age t, in a particular calendar year, r years after first-time enrollment, or s years after completion). Also, this expression is separate from inference as to how these average treatment effects apply to alternative populations or alternative measures of the dependent variable (Carneiro, Heckman, & Vytlacil, 2011). It is easy to check model specifications as well: preferably, the distribution of

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[3] Certificate awards and diploma awards have different meanings across states. For students who start at community college, the returns to bachelor's degrees are complicated by transfer status.

[4] This earnings premium approximates to the return to college under strict assumptions about the cost of college. Our focus here is on the labor market gains from college and not the net returns.

covariates in treatment and control groups should be similar, and this can be tested straightforwardly; and goodness-of-fit can be clearly evaluated from *R*-squared statistics.[5]

However, this Mincerian approach only yields an unbiased estimate of the gains from college if all other variables that are correlated with education and that determine earnings are included in the vectors *X* and *Z*. But most OLS models cannot control for all determinants and so are subject to omitted variable bias. If these omitted variables vary positively with education, the coefficient θ will be an inflated estimate of the gains to education (Barrow & Malamud, 2015).[6] The main concern is that those who attend and complete college have higher ability; omission of ability will therefore bias the labor market gains upward. In most empirical studies where ability measures are available, θ coefficients are lower but are typically still positive and statistically significant.[7] Hence, even as ability does affect earnings, it does not fully or straightforwardly explain the gains to postsecondary education (see also Belfield & Bailey, 2017).

Other biases may be significant. For example, Webber (2016) identifies a sizeable influence of non-cognitive characteristics such as an individual's locus of control and self-esteem. However, these characteristics may themselves be endogenous to either earnings (persons with high earnings may then report higher self-esteem) or education (those with degrees may report higher self-esteem, see de Araujo & Lagos, 2013). Another bias may be self-selection: students who value higher earnings correspondingly self-select into fields of study that yield higher earnings, or students enroll in education until the returns reach a threshold (Melguizo & Wolniak, 2012; Griliches, 1977). Overall, it is not certain that OLS approaches are substantially biased in ways that make the coefficient estimates irrelevant for policy decisions.[8]

As a response to the possible biases associated with OLS approaches, a range of econometric techniques have developed. These techniques include instrumental variables estimation, natural experiments, and estimation using biases on unobservables (Altonji, Elder, &

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[5] Goodness of fit for cross-sectional data on annual and quarterly earnings is approximately 0.1–0.3 (Bahr, Dynarski, Jacob, Kreisman, Sosa, & Wiederspan, 2015; Liu et al., 2015; direct analysis of Current Population Survey [CPS]).

[6] Early analyses justified the use of Mincerian OLS estimation on the grounds that positive and negative biases offset each other (Griliches, 1977). Gelbach (2016) demonstrates how the specific influence of each additional covariate may be misinterpreted. Here we are interested in the most valid estimate of the returns to education, not the measured influence of each covariate.

[7] Using data from the 2000 follow-up of the National Education Longitudinal Survey (NELS), Marcotte (2010) finds that controlling for school quality and academic ability reduces returns to associate degrees by 19 percent for men, but that it increases them by 10 percent for women. Using the National Longitudinal Survey of Youth 1997 (NLSY97), Scott-Clayton and Wen (2017) estimate the returns to associate degrees versus non-completers, which are 14 percent lower with full controls. Using NLSY79, Agan (2013) estimates OLS returns for associate degrees that are 30 percent lower with a full array of cognitive, non-cognitive, and family background characteristics. For Texas, Andrews, Li, and Lovenheim (2014) find that ability bias reduces the returns by approximately one-half and that college intentions reduces the returns for men (by 33 percent) but has no effect for women. For Virginia, controlling for demographic and personal characteristics *increases* the returns (Xu & Fletcher, 2016). For bachelor's degrees in North Carolina, Liu et al. (2015) find the reverse: the controls increase returns for men but decrease them for women. In all cases, the coefficients on education are statistically significant.

[8] In their detailed analysis, Carneiro et al. (2011, p. 2779) concluded that "[s]ome marginal expansions of schooling produce marginal gains that are well below average returns. For other policies associated with other marginal expansions, the marginal gains are substantial."

Taber, 2005; Rouse, 2007). Discussion of these alternatives is beyond the scope of this paper.[9] Here, we focus on a particular alternative technique that is increasingly being applied: fixed effects estimation.

Fixed Effects Specifications

The FE model specification for individual i in quarter of time t is given as:

$$Y_{it} = \alpha + \theta AWARD_{it} + \beta ENROLLED_{it} + \gamma TIME_{it}$$
$$+ \delta TIME_{it} \times DEMOG + \varphi Q_{it} + \mu_i + \eta_t + \varepsilon_{it} \quad (2)$$

In equation (2), Y_{it} is quarterly earnings from Unemployment Insurance (UI) records; *AWARD* is a binary indicator for possession of a degree, certificate or diploma in that quarter; *ENROLLED* refers to quarters in which the student is enrolled in college; *TIME* captures the effect of time on earnings; the interaction *TIME × DEMOG* captures variations in the effect of time with respect to individual characteristics; and *Q* is a vector of other terms included in some specifications (e.g., whether a student transfers to another college). The three remaining terms are: μ_i, which represents individual fixed effects (a dummy variable for each individual in the sample) and so controls for all individual fixed-over-time characteristics; η_t, which represents quarter (e.g., the first quarter of 2011) fixed effects and so controls for quarter-specific macroeconomic circumstances; and ε_{it}, which is the error term.

The advantage of the FE model of equation (2) is that it eliminates bias from time-invariant unobservable characteristics by differencing out person-specific mean earnings from observed quarterly earnings.

However, there are several concerns with FE specifications. One concern is that results from FE specifications—and how these are interpreted—may be sensitive to model specification. Key temporal variables as represented by *TIME* can be modeled in various ways, to capture an individual's age, economic conditions in each quarter, time enrolled, time since completion or exit, and economic events such as the Great Recession. Given that awards take different durations to complete (and that dropouts spend less time in college than award holders), these *TIME* variables and η_t may affect how the coefficients on *AWARD* should be interpreted (see Couch & Placzek, 2010). For example, dropouts will have had a longer time in the labor market than award recipients; their mean value for time since completion will therefore be greater.[10] Interpretation of FE results becomes even more challenging when *TIME* is interacted with demographic characteristics. Also, models vary with respect to the variables included in

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[9] It is difficult to summarize the empirical literature using these alternatives, but the literature does not clearly establish that OLS estimates are strongly biased. For example, Carneiro et al. (2011, Table 5) estimate the returns to one year of college at 0.0836 using OLS and 0.0951 applying a linear instrumental variables approach.

[10] For analysis of the effects of more rapid time to completion on earnings gaps, see Flores-Lagunes and Light (2010).

DEMOG and *Q*: specifications include gender, race/ethnicity, and socioeconomic status; they also include some quasi-fixed attributes (such as intent when entering college and first-semester GPA), and some time-varying attributes (e.g., credits per semester or cumulative credits per semester). Similarly, these variables are not independent of degree completion (students with only 10 cumulative credits cannot obtain an associate degree, for example).[11] Finally, education credentials may be modeled in various ways: for an individual who first obtains a certificate and subsequently an associate degree, the dummy variable for certificate may be toggled off (zero) when the degree is awarded or it may remain as a binary indicator. Thus far, no consistent baseline functional form has been established; this makes comparisons across studies difficult.[12]

Another concern is that these FE models—because they are more data intensive—impose more restrictions on the composition of the estimation sample. FE models typically impose data and sampling constraints so that the earnings gains can be accurately identified. One constraint is that individuals should have work experience prior to enrolling in college (so that within-person latent labor market productivity can be controlled for). But many students progress straight from high school to college without relevant work experience, and some others may have worked in jobs that do not correspond to their future human capital and career aspirations. Also, FE models may be less valid for persons who enroll in college later in their career as a reaction to their otherwise low expected future earnings; selection into college is different for younger versus older workers (Jepsen & Montgomery, 2012). A related constraint is that the dataset must cover a long period of time, including the period before college, during college, and at least five years after exit from college. The influence of missing data (before, during, or after college) may therefore be uncertain.

Moreover, FE specifications do not address all omitted variable bias. They do reduce omitted variable bias from fixed unobservable characteristics (e.g., latent ability). Also, they reduce endogeneity bias, i.e., bias because relatively low expected wages at y_t induce college enrollment at y_{t+1} (for all enrollments in college). Specifically, FE models require that there are no time-varying unobservable shocks correlated with educational attainment and labor market outcomes that differentially affect college completion. One particular shock—the student's realized performance in college and its implications for future earnings—is likely to be highly salient. Indeed, given low completion rates across the two-year sector, it seems likely that many students do change their effort and motivations after they first enroll in college.

This concern raises questions as to the validity of FE estimates and the importance of time trend specifications. Although FE estimation has been extensively used in program evaluation, its application to education–earnings gradients may not be valid. As emphasized by

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[11] Students who work while enrolled may experience slower earnings growth than students who do not work and secure full-time work only after graduation. This disparity may be greater for students who work in jobs that complement their studies.

[12] For example, in examining returns across college students in Texas, Andrews et al. (2014, 2016) estimate the returns based on individual-level residuals. First, earnings are regressed on year, quarter, and high school cohort indicators; the residuals from this equation are used as the dependent variable for the FE model. As discussed below, recent statewide models differ from one of the first studies by Jacobson et al. (2005).

Sobel (2012), there are issues over how coefficients should be interpreted given the model specification.

Figure 1, showing earnings data from Ohio from Minaya and Scott-Clayton (2017), illustrates the challenge for FE estimation. Figure 1 shows the paths of earnings for associate degree completers and for persons who do not complete an award. These paths are very different in several respects. Before entry, the earnings paths are similar, but on entry to college the average earnings of completers are much lower than those of non-completers; this deficit may be an endogenous response to focusing on college rather than work because there is a higher probability of completing the degree. During college, degree recipients have much lower earnings: they are specializing in college more intensively than non-completers. Finally, post-college the earnings gap grows significantly for associate degree recipients. By including pre-, during, and post-college periods, FE models are attempting to identify the returns to education from a model of this entire trajectory.

Figure 1: Quarterly Earnings by Quarter Since First Entry (Women, Ohio)
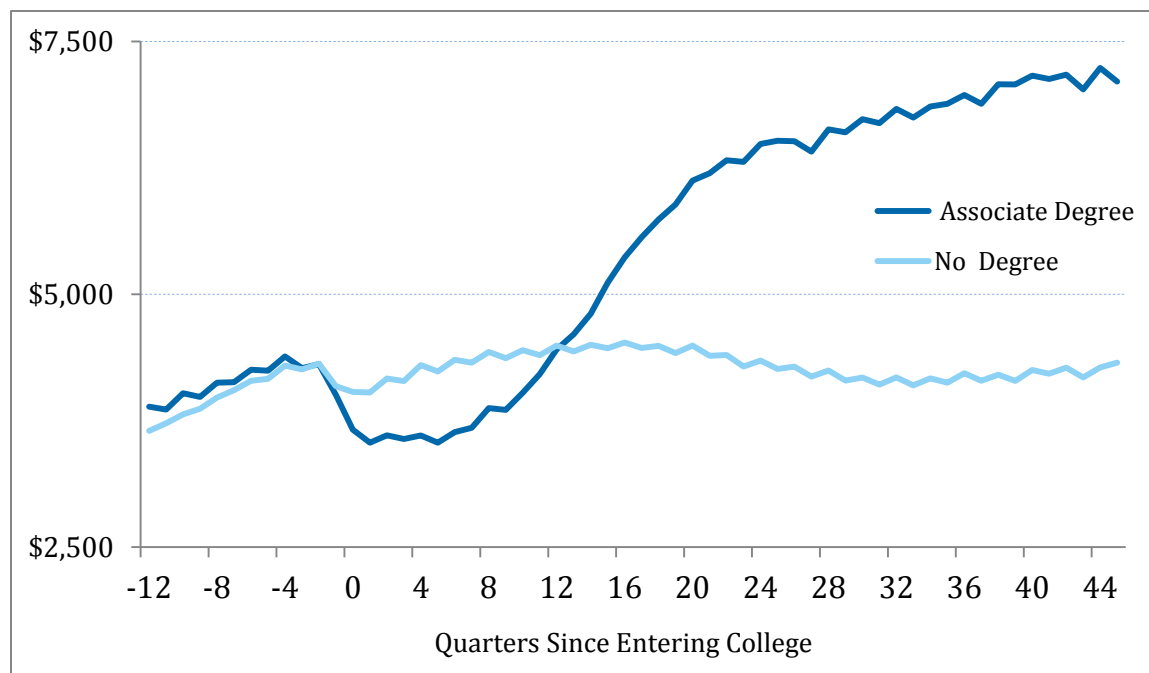
Source: Minaya and Scott-Clayton (2017).

FE models are identifying gains in earnings from the post-college gap in earnings. These models typically have several implicit assumptions. First, there should be immediate impacts of college awards on earnings (i.e., it is appropriate to include all quarters after the college award as reflecting degree-level labor market skills); this seems unlikely given that graduates are searching for work that best fits their award and that this search may take time. Second, the earnings at time t should not dependent on earnings at time $t - 1$; this assumption may also be

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questioned given that the profiles show individuals willing to sacrifice current income for greater future income (and having been unemployed in previous quarters is almost certainly going to influence wages in subsequent quarters). Finally, education at time t should be assumed to be independent of earnings at time $t - 1$; yet, it seems plausible that, if earnings at time $t - 1$ are unexpectedly high, an individual will not go back to college to increase earnings. More generally, ability, motivation, and latent productivity are unlikely to be constant over time: students who discover that they can do well in college may be motivated to study harder.[13] Therefore, following Dynarski, Jacob, and Kreisman (2016), we investigate time trend effects across FE models in detail.

3. Testing the Robustness of Earnings Equations

To test for the robustness of FE models we distinguish two sets of empirical issues: those related to functional form and those related to sampling. We recognize that identification challenges can be addressed in multiple ways. For example, in testing for robustness with respect to the ages of students, the specification might include more age-related covariates or divide the sample into age groups for subgroup estimation. Also, we recognize that the results will vary across subgroups of students (e.g., by race/ethnicity); our focus in this review is not on the heterogeneity of results across these subgroups.

We perform a full set of sample and functional form sensitivity tests for the FE model using data from students who first enrolled in the North Carolina Community College System (NCCCS) in 2002–04. Full details on this dataset are given in Liu et al. (2015). We begin by estimating the expected gains for an associate degree completer over a non-completer from a baseline model. This baseline excludes persons with zero earnings.

The baseline estimates for North Carolina over non-completers are $1,161 for men and $1,932 for women in quarters when the individual's highest award is an associate degree. Relative to average quarterly earnings of $7,983 (men) and $6,478 (women) in the final year of follow-up, this return is statistically significant; it equates to an earnings premium of 15 percent for men and 30 percent for women. This estimated gain is precisely estimated: the standard error is approximately $50. We then compare this baseline estimate to alternative samples and functional forms.

In addition, we summarize the range of robustness checks and sensitivity tests applied in other studies that rely on large-scale transcript datasets matched to UI earnings. For a review of the overall results from these studies, see Belfield and Bailey (2017). These studies apply fixed effects models in different ways and then apply sensitivity checks in correspondingly different

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[13] Also, there are issues of interpretation as to how well the data fit the model based on goodness-of-fit tests. There are goodness-of-fit statistics for FE models, but these are more difficult to interpret relative to OLS R-squared statistics, not least because model specifications vary significantly.

ways—so the robustness of the results across studies is not easy to summarize. Therefore, instead of calculating differences in results across specifications, we summarize the magnitude and sign of each robustness check.

Finally, we compare the FE results with OLS specifications that attempt to identify an equivalent construct—the expected steady-state earnings gains from completing an associate degree. To make this comparison, we need to specify a comparable estimation approach. As for our main test, we calculate estimates directly using administrative data from North Carolina; we also summarize results from the available specifications in related studies from other states. Given the motivation for FE models—that unobservable characteristics positively associated with educational attainment are positively associated with earnings—we expect that the OLS estimates will be considerably higher than the FE estimates. Again, however, we caution that for the comparison to be valid the two estimates must be measuring the same construct.

4. Results From Sensitivity Tests

FE Models: Sensitivity to Sample Inclusion Criteria

We investigate how sensitive the baseline result is to variations in sample inclusion criteria. The baseline estimate is given in column 1 of Table 1, and the alternatives are shown in columns 2–7. Overall, Table 1 shows that the returns to associate degree holders compared to community college students who do not complete an award are large and always statistically significant across the sample specifications.

Time window. FE models require longitudinal data over a long window of time. For example, the North Carolina estimates are based on transcript information for students who entered college in 2003, ostensibly for a two-year degree. The earnings data are from the period from the 1990s up to 2013; that is, there is more than a decade's worth of earnings data. Even this coverage may be too brief: a student entering college in 2003 who earns a bachelor's degree may not fully enter the labor market until 2009; this would yield at most four years of earnings data. Therefore, it is important to investigate whether the time windows for analysis matter and how much.[14]

Overall, the evidence shows that the time frame for analysis does matter in two respects. First, college students do not immediately get full-time employment in a job matching their skills. For displaced workers in Washington State, Jacobson et al. (2005, Table 2) estimate that first-quarter earnings "gains" are negative for trained workers. Second, the growth in earnings for associate degree holders is steeper than for non-completers. Almost all studies identify this steeper trajectory (see, e.g., Jaggars & Xu, 2016). Both these factors mean that results from FE models will be sensitive to the number of quarters available in the dataset.

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[14] OLS estimation only requires the year of earnings and an indicator for college education in the past.

Table 1: Quarterly Earnings Gain Fixed Effects Estimation: Sample Specifications for North Carolina

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Excluding Bachelor's			
		Excluding Low Wage	Excluding Age < 20	Bachelor's Degrees	Excluding Transfers	Including Zero Wage	Shorter Window
	Baseline						
Men							
Associate degree vs. no award	1,161	1,152	1,140	1,159	1,432	1,513	1,121
	(43)***	(43)***	(53)***	(45)***	(56)***	(42)***	(49)***
Coefficient vs. baseline		-1%	-2%	0%	+23%	+30%	-3%
R-squared	0.159	0.151	0.327	0.129	0.124	0.073	0.148
N persons	*93,430*	*93,164*	*45,637*	*81,272*	*46,555*	*104,353*	*93,016*
N quarters	*1,992,497*	*1,967,562*	*931,067*	*1,749,367*	*1,164,127*	*3,618,605*	*1,779,409*
Women							
Associate degree vs. no award	1,932	1,920	2,114	1,991	2,254	2,291	1,815
	(30)***	(30)***	(45)***	(31)***	(40)***	(29)***	(34)***
Coefficient vs. baseline		-1%	+9%	+3%	+17%	+19%	-6%
R-squared	0.191	0.183	0.359	0.141	0.135	0.110	0.154
N persons	*141,683*	*141,356*	*58,358*	*121,919*	*68,373*	*149,470*	*140,826*
N quarters	*3,255,979*	*3,214,126*	*1,277,737*	*2,820,617*	*1,780,395*	*5,202,582*	*2,538,107*

Note. All first-time-in-college NCCCS students (academic years 2002–03 to 2004–05) with earnings up to 2012. Model as per equation (1): Award; Dip4; Quarter; QuarterSqd; Race*Quarter; Age25*Quarter; Race*QuarterSqd; and Age25*QuarterSqd. Constant term included. Award status toggled for highest award. Quarter is calendar quarter (e.g., 2002Q3). Low wage is < $750 per quarter pre-college. Transfer is quarter when student is in a transfer college. Shorter window is two years shorter follow-up. Robust standard errors reported in parentheses.

*$p < .1$. **$p < .05$. ***$p < .01$.

Table 2: Quarterly Earnings Gain Fixed Effects Estimation: Model Specifications for North Carolina

	(1) Baseline	(2) Cubic	(3) Extra Time Variables	(4) Dips Precollege	(5) Post- College Dummy	(6) Transfer Dummy	(7) Award x Enrolled	(8) Award Hold
Men								
Associate degree	1,161	1,152	1,161	1,152	1,174	1,166	1,260	848
	(43)***	(43)***	(43)***	(43)***	(43)***	(43)***	(45)***	(42)***
Coefficient vs. baseline		-1%	0%	-1%	1%	0%	9%	-27%
R-squared	0.159	0.160	0.159	0.159	0.161	0.160	0.159	0.158
N persons	*93,430*	*93,430*	*93,430*	*93,430*	*93,430*	*93,430*	*93,430*	*93,430*
N quarters	*1,992,497*	*1,992,497*	*1,992,497*	*1,992,497*	*1,992,497*	*1,992,497*	*1,992,497*	*1,992,497*
Women								
Associate degree	1,932	1,923	1,932	1,927	1,937	1,928	2,053	1,583
	(30)***	(30)***	(30)***	(30)***	(30)***	(30)***	(31)***	(30)***
Coefficient vs. baseline		0%	0%	0%	1%	0%	7%	-18%
R-squared	0.191	0.193	0.191	0.191	0.193	0.191	0.192	0.188
N persons	*141,683*	*141,683*	*141,683*	*141,683*	*141,683*	*141,683*	*141,683*	*141,683*
N quarters	*3,255,979*	*3,255,979*	*3,255,979*	*3,255,979*	*3,255,979*	*3,255,979*	*3,255,979*	*3,255,979*

Note. All first-time-in-college NCCCS students (academic years 2002–03 to 2004–05). Model (1): as per equation (1): Award; Dip4; Quarter; QuarterSqd; Race*Quarter; Age25*Quarter; Race*QuarterSqd; and Age25*QuarterSqd. Award status toggled for highest award. Constant term included. Quarter is calendar quarter (e.g., 2002 Q3). Model (2): Baseline and QuarterCubed; Race*QuarterCubed; Age25*QuarterCubed; and Age25*QuarterCubed. Model (3): Baseline and Seasons (3). Model (4): Baseline and Indicator (no longer in college). Model (5): Baseline except Dip4 replaced by Indicators (two quarters before enrollment). Model (6): Baseline and Indicator (Transfer student) and Interaction (Transfer x enrolled). Model (7): Baseline and Interactions (award x enrolled). Model (8): Baseline except Award indicator held for all quarters when award held regardless of other awards. Robust standard errors reported in parentheses.

*p < .1. **p < .05. ***p < .01.

Table 3: Robustness Checks on Returns to Associate Degrees (Several State Studies)

	Washington		Michigan		Kentucky		California	Colorado	Ohio
	Female	**Male**	**Female**	**Male**	**Female**	**Male**	**Pooled**	**Pooled**	**Pooled**
Baseline FE including time trend	6.5%	2.1%	$2,363	$1,484	$2,346	$1,441	$1,650	$2,200	$2,439
Sample selection									
Age 25+ on entry; worked pre-entry	---	---							
Age 20–50								+++	n.d.
Excluding younger persons					+	n.d.			
Including younger persons			n.d.	n.d.					
Short window (years post-college)			n.d.	n.d.			--		---
Excluding enrolled in college five years later	--	+			+	+	+++	n.d.	
Adjusting for enrollment of non-completers								n.d.	
Excluding zero credits in college			n.d.	-			n.d.		
Only employed			+	+					
Including missing wages pre-entry	+	+							
Including strong labor market attachment			n.d.	n.d.				+	
Including weak labor market attachment					n.d.	+			
Specifications									
Baseline excluding time trends/interactions	+++	+++	n.d.	n.d.				n.d.	n.d.
Difference estimator			--	n.d.				--	
Adjusting for Ashenfelter Dip (1)	n.d.	n.d.			n.d.	n.d.	n.d.		-
Adjusting for Ashenfelter Dip (2)	n.d.	+	n.d.	n.d.	n.d.	-			

Note. See Appendix Table A1 for full results. *Sources:* Dadgar and Trimble (2015) (Washington); Bahr et al. (2015) (Michigan); Jepsen et al. (2014) (Kentucky); Bahr (2016) (California); Turner (2016) (Colorado); Minaya & Scott-Clayton (2017) (Ohio).

n.d. = estimate within 5% of baseline estimate.
+/- = estimate between 5–10% of baseline estimate.
++/-- = estimate between 10-20% of baseline estimate.
+++/--- = estimate more than 20% different from baseline estimate.

The size of the distortion from having too brief a window is unclear. For Colorado, Turner (2016) finds that the earnings gains in the initial quarters after exit from college are a good approximation to the long-run benefits; earnings gains are also robust with a shorter window for community college students in Michigan (see Table 3 above). For North Carolina, column 7 of Table 1 shows only a modest negative effect from truncating the follow-up period. Returns are 3 percent lower (men) and 6 percent lower (women) if the follow-up is shortened by two years (to a maximum of seven years since first enrollment).[15] However, the analysis in Table 1 compares a moderate time frame with a short time frame. The clearest test comes from analysis of Ohio students: Minaya and Scott-Clayton (2017) calculate that the returns are 25 percent higher if the window of analysis is extended from 5 to 8 years of earnings data post-college (Minaya & Scott-Clayton, 2017, final column of Table 2). Analyses with earnings data of less than 5 years therefore appear to be significantly underestimating the long-run returns to college.

Pre-college work experience. FE models identify the earnings gains from a degree based on deviations from individual-level earnings over the period pre-college and during college. For persons who enter community college almost immediately after high school, any pre-college earnings are unlikely to be a good guide to their expected earnings post-college. It seems likely that, for these persons, the *AWARD* coefficient will be biased upward, capturing the value of the degree and employment in a job that corresponds to their latent productivity. By contrast, for older students who had significant work experience prior to college, the *AWARD* coefficient should more accurately reflect the direct change in skills—and hence earnings—from having a degree.

One test is to see how returns vary if the sample is restricted to older workers. Applying an age restriction does reduce the sample size for analysis. In Washington State, 49 percent of community college students are aged 19 or under at first enrollment (Dadgar & Trimble, 2014), so the analysis sample is reduced by 51 percent; in Michigan, the analysis sample is reduced by 20 percent (Bahr et al., 2015); and in North Carolina the sample is reduced by 51–58 percent.

However, the effect on the *AWARD* coefficient is not affected significantly. Column 3 of Table 1 shows results for North Carolina: excluding persons aged under 20 has a trivial impact for men and a modestly positive impact for women. Given that the sample is reduced by over half, the overall effect is surprisingly small. As summarized in Table 3, studies from other states find a mixed pattern. For Kentucky, excluding persons aged under 20 has a small effect: it reduces returns negligibly for men but increases them slightly for women. Similarly, in Ohio, the differential effect of age is very small. In Washington (in some specifications) returns to associate degree holders are substantially lower if younger persons are excluded (and this is also the case for some specifications in Michigan, not reported in Table 3). However, earnings gains are higher in California for older workers (aged 20 or over on first entry); and in Virginia (Xu &

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[15] This decline was also found by Liu et al. (2015) using OLS estimations and the 2002–3 cohort. But the decline in earnings is non-linear: returns are 13 percent (men) and 15 percent (women) lower if we shorten the follow-up by three years. Also, truncating the follow-up by one year reduces the returns by only 2 percent (men) and 3 percent (women).

Fletcher, 2016) returns to an associate degree are higher for those aged over 25 (not reported in Table 3). A confounding factor is that wage dispersion increases with age: point estimates of returns to awards are less precise for older workers.

In addition, we re-estimate equation (1) by excluding persons with zero (or very low) pre-college earnings. This is a direct test of the influence of pre-college work experience. As shown in column 2 of Table 1, excluding persons with low pre-college wages has a trivial impact on the earnings gains. Under various specifications, this result has also been found in Michigan, Kentucky, and Washington states (Bahr et al., 2015; Dadgar & Trimble, 2014; Jepsen, Troske, & Coomes, 2014).[16]

Zero earnings. FE models rely on data from across at least one decade; results for individuals with high proportions of missing data or intermittent labor market participation over this period may be biased. Data may be missing for many reasons (including incomplete records, out-of-state migration, zero earnings coded as missing, and exemption from UI coverage). These reasons may be collinear with education: if degree completion increases labor force participation rates, then assigning zeroes as missing data will underestimate the returns to college.[17]

As shown in column 1 of Table 1, the baseline model for North Carolina excludes quarters with no earnings data. Including all quarters for each person and assigning missing earnings as zeroes does make a substantial difference. As shown in column 6, the returns to an associate degree are 30 percent higher among men and 19 percent higher among women if we include zero earnings compared to baseline. Evidence from other states is less emphatic (Table 3): adjusting for labor market attachment has a small positive effect on estimated returns. These weaker results are surprising given the positive association between education and labor market participation.[18]

A related consideration is the sample of students who remain in college beyond a reasonable time frame (e.g., greater than 150 percent of "normal" time to completion). Durations in college have been growing longer (Bound, Lovenheim, & Turner, 2010). These persons may still be in college because they have not been able to find work, i.e., their earnings are effectively zero. Also, they may have insufficient years of work from which to identify differences in the growth in earnings post-college. As a robustness check, these persons may be excluded from the sample. As shown in Table 3, however, this exclusion criterion has inconsistent impacts: in Washington, Kentucky, and Colorado, there is a very slight difference in the returns to an

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[16] For Washington, Dadgar and Trimble (2014) report coefficients for associate degrees of 0.102 (men) and 0.088 (women) with a sample only for those with prior earnings data; including all persons yields similar coefficients of 0.114 (men) and 0.074 (women) (M. Trimble, personal communication, July 20, 2015).

[17] It is not possible to distinguish quarters with zero earnings from quarters with missing earnings in the North Carolina dataset. Some preferred specifications do include persons with zero earnings (e.g., those by Dadgar & Trimble, 2015; Jepsen et al., 2014); others exclude zero earnings (e.g., Bahr et al., 2015; Jaggars et al., 2016; Liu et al., 2015).

[18] In Michigan, returns to associate degrees appear lower when the sample is only those who are employed. Using NLSY97, estimates for worker-only samples yield gains from associate degrees that are 14 percent lower than estimates for all persons (Scott-Clayton & Wen, 2017, Table 3).

associate degree; in California, returns are significantly higher when the sample excludes persons who are enrolled in community college for more than five years. Overall, there is no strong evidence that the trend toward longer durations in college has an impact on the estimation of returns.

Student transfers. Another concern is missing student-level data on college attainment. National Student Clearinghouse transfer data do not cover all colleges that students might transfer to, and some colleges do not submit reports on where their students had previously enrolled (see Dynarski, Hemelt, & Hyman, 2015). Overall, lack of coverage in the transfer data affects only a small subset of students: almost all large public institutions report their transfers to the National Student Clearinghouse (and coverage is increasing over time). But lack of coverage may affect clusters of students in for-profit colleges, and coverage does vary by state: some states cover only two thirds of relevant postsecondary institutions. Uncovered students who transfer to other colleges will have more—unobserved—postsecondary human capital (either credits or a credential); the returns to observed degrees may therefore be biased upward when transfer information is missing. Another group for whom data is missing is reverse transfer students, those who transfer from a four-year to a two-year college to earn an associate degree; these students are not typically included in datasets that are collated based on first-semester enrollment at a college.

The results in column 5 of Table 1 show that, for North Carolina, excluding transfer students significantly increases the estimated returns to an associate degree.[19] When transfer students are excluded, the returns to an associate degree are increased by 23 and 17 percent for men and women respectively. This relationship also holds for Kentucky: Jepsen et al. (2014) identify significant differences depending on whether or not transfer students are included. Notably, including transfer students decreases estimated returns because the time window of earnings data is not long enough. (We are not aware of robustness checks on transfer students in other state studies). However, for North Carolina at least, the returns to an associate degree are not substantially affected if students who subsequently earn a bachelor's degree are excluded. By inference, students who transfer but do not complete a bachelor's degree have significantly lower earnings, and results may be sensitive to the size of this group. Looking across all transfer students, though, there is no evidence that this group, regardless of whether they complete any award, have higher ability than students who earn an associate degree.[20]

Overall, results are sensitive to whether transfer students are included in the sample. Modeling the pathways of transfer students therefore appears to be an important concern when estimating the returns to college, although the reasons for this remain unclear.

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[19] Relevant to our comparisons above, North Carolina data has near complete data coverage at the student-level (over 90 percent, Dynarski et al., 2015, Table 2).

[20] Using NLSY79, Agan (2013, Table 3) estimates that for associate degree recipients who transfer, the private internal rate of return is 11 and 16 percent for men and women, respectively, if they earn a bachelor's degree, and 31 and 25 percent if they do not. For those with an associate degree who do not transfer, the internal rate of return is 22 and 32 percent.

FE Models: Sensitivity to Model Specification

Next we consider functional form differences within the fixed effects specification. For North Carolina, variations in returns across alternative functional forms are reported in Table 2 above; the alternatives are again compared to the baseline estimate, given in column 1. Also, for other states, variations are summarized in the bottom panel of Table 3 (above) relative to the baseline in the first row of Table 3 (also see Appendix Table A1). As with sample restrictions, some alternative functional form specifications have a small influence on the estimated returns. There are some important exceptions, although in these cases we must be cautious because the interpretation to be placed on the coefficient has changed.

Ashenfelter dip. Earnings show a clear drop (Ashenfelter dip) before students enter college, raising the possibility that college enrollment is endogenous to future earnings paths. Conventionally, the Ashenfelter dip refers to the circumstance in which persons who experience negative earnings shocks are more likely to subsequently enroll in college. All the statewide studies do show earnings dips prior to college entry. However, this dip may be endogenous, i.e., the student may be giving up a job in order to enroll more intensively in college. As shown in Appendix Figure A1, the Ashenfelter dip is much larger for students who eventually complete an associate degree relative to those who do not complete a degree. Students who complete a degree may be voluntarily trading off work for college.

In FE models, attempts to address the Ashenfelter dip do not change the estimated gains from completing an associate degree. As shown in column 4 of Table 2, the point estimate of returns changes by only 1 percent. As summarized in Table 3, the studies have attempted six alternative specifications to adjust for the Ashenfelter dip: only two yield slightly different estimates, and these differences are not consistent in sign. Overall, functional forms to model pre-entry behavior do not appear to be influential.

Modeling earnings trajectories. As discussed above, a key challenge is how to model time trends of earnings growth for degree holders relative to a baseline earnings growth. It seems plausible that individuals who obtain degrees are on different earnings trajectories both before and after college.[21]

A simple approach would be to include more varied specifications of independent *TIME* variables. As shown in Table 2 (columns 2–6), alternative specifications that have little impact on the returns to awards are: including cubic time trends; extra time variables (season, year) and inclusion of indicators for quarters either after college or as a transfer student. These alternatives affect the coefficients by less than 2 percent. These findings correspond to results from other states (see Table 3).[22] For Kentucky, Jepsen et al. (2014) find that adding time trends reduces coefficients by approximately 4 percent. Thus, more complex specifications to account for time

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[21] Time trends while in college appear less influential (see Jaggars & Xu, 2016). For Ohio, Bettinger and Soliz (2016, Tables 3 and 4) estimate returns that are statistically equivalent for students who work intensively when in college.

[22] For Connecticut, Crouch, and Placzek (2010) find slightly higher wage growth when time trends are included.

do not affect the results. However, estimated returns are significantly different if award receipt is interacted with quarters of enrollment. As shown in column 7 of Table 2, this interaction increases the returns to an associate degree by 9 and 7 percent for men and women respectively. This is further evidence that the returns may be increasing with time since exit from college.

In analysis for Michigan, Dynarski et al. (2016) investigate a range of alternative estimations to more accurately capture time trend differences between degree completers and non-completers. One approach is to control for earnings growth by including a measure of lagged earnings to account for initial productivity. This inclusion has a significant impact on returns: the gains from an associate degree fall from a baseline of $1,129 to $394 when lagged earnings are included (Dynarski et al., 2016, Table 3). A more comprehensive model specification includes an individual time trend; this captures differences in growth trends across individuals. However, returns to an associate degree increase by 17 percent when individual time trends are included (Dynarski et al., 2016, Table 4). Lastly, in looking at pre-college differential trends in earnings, Dynarski et al. (2016, Figure 1) show distinct earnings paths for students who ultimately earn an associate degree. Specifically, these students experience relatively sharp declines in earnings before entering college; for those who do not earn an award, the pre-college earnings trajectory is consistently flat. This finding is more evidence that a simple FE model is unlikely to account for variations in earnings trajectories across student groups. That is, FE models need to account for pre-college dips in earnings, earnings trends when in college, and differential earnings growth after exiting college. However, the implications of this finding are less clear (in part because, as noted above, direct attempts to address Ashenfelter dip effects have little influence). If associate degree holders are experiencing exogenous and relatively sharp declines in earnings before they enter college, this suggests that they are less advantaged than other student groups and that the post-college earnings gains are biased downward. Alternatively, a decline in earnings before entering college may indicate that these students are more motivated to complete a degree.[23]

Analysis using highest award. In some FE specifications the *AWARD* variable is toggled on (equals one) for the first award and then toggled off (equals zero) when the individual receives a higher award. The coefficient on each award is therefore interpreted as the return only when that award is the highest award the individual possesses at that time. By contrast, the alternative specification is to keep the *AWARD* variable toggled on for the entire time period after each award has been received.

There is a significant difference in the results depending on which specification is used. For North Carolina, column 8 of Table 2 shows the difference in results if the award is toggled on permanently post-award compared to the specification where the award is toggled on and then off. The former specification (always toggled on) yields returns to associate degrees that are

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[23] In their modeling of displaced workers in Washington state, Jacobson et al. (2005) include as an independent variable the reciprocal of quarters since exiting college (see also Turner, 2016). This approach affirms that degree holders have different earnings trajectories over non-completers. However, the magnitude of the effect cannot be easily interpreted relative to the baseline model.

sharply reduced (by 27 and 18 percent for men and women respectively). However, the interpretation of the *AWARD* coefficient is different across the two specifications—the coefficients need to be added up, and this information is not clearly available—so we cannot conclude that FE models are not robust.

Overall Sensitivity

Looking across the models, we identify two important sampling issues (transfer and zero wages), two salient functional form issues with implications for interpreting the returns to college (award/enrollment interactions and award-holding), and one important specification issue (the type of time-trend estimator).

For North Carolina, we can calculate the upper and lower bounds for estimates of the returns to college, adjusting for sampling and functional form issues. A lower bound estimate arises by applying the model where award-holding is toggled on/off: with this model the quarterly gains from an associate degree are conservatively estimated at $848 (men) and $1,538 (women) above mean quarterly earnings. This estimate is a premium of 11 and 24 percent over earnings in the last year of follow-up. One upper bound estimate arises from a model including zero earners: with this model the quarterly earnings gains are $1,513 (men) and $2,291 (women). However, we can pool all modeling assumptions that upwardly effect the earnings estimates; that is, we can estimate the returns across all persons (zero and non-zero earnings) who do not transfer to a four-year college, with award interacted with enrollment status in each quarter. With this maximal upper bound, the returns to an associate degree increase to $2,007 (men) and $2,917 (women), which amounts to a premium of 25 and 45 percent over mean quarterly earnings in the last year of follow-up.

Comparison of FE Model and OLS Model Estimates

We now compare results from FE models with those from OLS estimates. The intent is to estimate the "steady-state" quarterly earnings gain from completion of an associate degree over non-completion. The estimates are calculated as the best estimate from each state study (approximately 8–10 years after first-time enrollment in college). However, they are not from directly equivalent samples, and they do not account for salient variations in sampling or functional form.[24]

Comparative estimates across several state analyses are summarized in Table 4. There are some modest differences in estimated returns. There is no clear indication that OLS estimates are substantially inflated over FE estimates. In five states (Michigan, Kentucky, Virginia, North Carolina, and Ohio), the gap between OLS and FE estimates is modest (less than a 15 percent difference); in two states (Michigan for women and Kentucky), the FE estimate is actually

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[24] Also, our comparisons assume that the coefficients from FE models are correctly interpreted with respect to time trend effects. As discussed above, we cannot be certain that the influences of all *TIME* covariates are correctly considered when interpreting the *AWARD* coefficients.

greater than the OLS estimate. Washington is the only state that clearly shows OLS estimates substantially higher than FE estimates.

Table 5 explores the differences between OLS and FE models in more detail with direct comparisons for North Carolina. These estimates use identical samples and are directly comparable (to be conservative, we apply a very parsimonious OLS specification controlling for race/ethnicity, age on entry, and months of work [squared]). Broadly, the results are similar. For three of these models (baseline, including zeroes, and excluding transfers), the coefficients are significantly higher using the FE model than the OLS model. For the fourth model (award held), the coefficients for the OLS model and the FE model are very similar (within 2–5 percent). We emphasize that the OLS model is very parsimonious and does not control for characteristics such as ability, which we would expect to lead to inflated OLS coefficients. However, the robustness and consistency of results between FE and OLS models is not surprising given that, as noted above, other studies that have sought to address ability bias have not found substantially different results.

Table 4: Quarterly Earnings Gain Estimations for Associate Degree Completers Over Non-Completers: OLS and FE Models (Several State Studies)

	OLS Estimation	FE Estimation	(FE-OLS)/OLS
Pooled			
Virginia	$2,989	$2,881	-4%
Ohio	$2,439	$2,313	-5%
Women			
Michigan [a]	0.319	0.327	+2%
Kentucky	$2,290	$2,363	+3%
North Carolina	$2,136	$1,907	-11%
Washington	$1,051	$600	-43%
Men			
Michigan [a]	0.272	0.118	-14%
Kentucky	$1,349	$1,484	+10%
North Carolina	$1,115	$1,113	0%
Washington	$914	$400	-56%

Note. Coefficient amounts not adjusted to constant dollars across states. *Sources:* Dadgar and Trimble (2015) (Washington); Bahr et al. (2015) (Michigan); Jepsen et al. (2014) (Kentucky); Bahr (2016) (California); Minaya & Scott-Clayton (2017) (Ohio). Jaggars and Xu (2016) (Virginia).

[a] Coefficients on ln(earnings) regressions; percent gap calculated based on mean quarterly earnings.

Table 5: Quarterly Earnings Gain Estimations: OLS and FE Models for North Carolina

	(1) Baseline		(2) Including Zero Earnings		(3) Excluding Transfers		(4) Award Hold	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Men								
Associate degree	1,206 (61)***	1,579 (46)***	927 (43)***	1,509 (42)***	1,435 (75)***	1,928 (59)***	1,206 (61)***	1,170 (45)***
R-squared	0.243	0.176	0.456	0.074	0.267	0.141	0.243	0.176
N persons	*61,233*	*61,233*	*103,479*	*103,479*	*36,412*	*36,412*	*61,233*	*61,233*
N quarters		*2,125,043*		*3,601,101*		*1,273,116*		*2,125,043*
Women								
Associate degree	2,120 (34)***	2,430 (32)***	1,809 (26)***	2,292 (29)***	2,559 (39)***	2,820 (40)***	2,120 (34)***	2,018 (31)***
R-squared	0.228	0.217	0.414	0.110	0.250	0.174	0.228	0.214
N persons	*98,937*	*98,937*	*148,117*	*148,117*	*55,049*	*55,049*	*98,937*	*98,937*
N quarters		*3,449,282*		*5,174,049*	*0.250*	*1,939,276*		*3,449,282*

Note. All first-time-in-college NCCCS students (academic years 2002–03 to 2004–05). OLS earnings measured as average quarterly earnings in 2012. FE earnings quarterly from 2002–12. All earnings in 2010 dollars. OLS estimation controls for months of work experience (squared), calendar year (2), and race/ethnicity (3). FE estimation using baseline model as per equation (1) in Table 1. Standard errors in parentheses.

*** $p < 0.01$.

Table 5 does show that OLS models do identify the returns to awards less precisely (standard errors are wider). But in all cases the returns are statistically significantly different from zero at the 1 percent level. Also, the explained variation in earnings appears slightly higher for OLS models than FE models.[25] With respect to efficiency and goodness of fit, FE models do not appear to have a clear advantage over OLS models.

Finally, the results from Table 5 help reconcile this equivalence with the main finding from this review of FE results. The main finding is that results from FE models do vary with respect to some sample selections and functional forms; yet, FE results and OLS results are not significantly discrepant. As shown in Table 5, OLS results also vary with respect to sample selections.[26] Thus, OLS results stay generally equivalent to FE results.

5. Other Empirical Problems

Overall, the results from across the FE specifications and relative to OLS specifications appear robust and consistent. Here we consider if other empirical problems are more salient. One concern is missing earnings data. The Unemployment Insurance (UI) earnings data for a given state typically do not include all workers; they exclude independent contractors, military personnel, some federal personnel, and those working in the informal sector. Separately, there may also be migration out of state; UI records typically do not capture out-of-state earnings. In practice, around 10 percent of all workers do not have reported earnings (zero or a positive amount) over the full period of analysis.[27] It is unclear how salient this missing data bias is; the available evidence suggests it is probably modest. In a direct comparison with survey data, Wallace and Haveman (2007) found that UI data yielded *lower* returns to college and that the biases were smaller for those with steady jobs (such as college-educated workers). Recently, the U.S. Department of Education examined the bias from using UI data compared to tax return data (Executive Office of the President of the U.S., 2015). The difference in college-level average earnings across the two sources of data was less than 1 percent.[28]

The studies analyzed here do not address the problem of missing earnings due to out-migration.[29] If students who migrate out of state are those with the most job offers, then there

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[25] Goodness-of-fit tests are harder to interpret and have not been reported consistently. For example, Liu et al. (2015) report a goodness-of-fit R-squared of 0.19; using a similar specification, Dadgar and Trimble (2015) report 0.59–0.72. These are different test statistics of course, but there is little guidance on what values are reasonable.

[26] For example, Minaya and Scott-Clayton (2017, Appendix Table A2) find that when including zeroes for earnings, OLS results in Ohio are $2,313; when excluding them, OLS results are $1,988.

[27] Missing earnings data may generate bias for program-level analyses: college programs in which the occupational intent is self-employment will show relatively low returns if missing earnings are counted as zero earnings.

[28] The U.S. Department of Education selected tax return data from W-2s (not 1099s) and only for persons who remained within the state in which they attended college. This selection effect reduced the tax sample by 38 percent.

[29] Only one statewide administrative dataset includes UI records for individuals who migrate out of state: for Virginia, results separated by migration status are not available, although the pooled results are similar to those from other states that do not include out-of-state workers (Belfield & Bailey, 2017, Table 2).

may be a positive correlation between out-migration and degree attainment: the degree serves as stronger signal of productivity to a wider labor market. The returns to degrees may therefore be biased downward if out-of-state employment is missing. In direct analysis of migration impacts using NLSY97, Scott-Clayton and Wen (2017) find contrary results for associate degree holders. Associate degree completion does not appear to influence out-migration, but out-of-state migrants with associate degrees earn less than persons who stay in-state. Yet, the negative effect of out-migration is modest: it reduces earnings gains from an associate degree by approximately 6 percent.[30]

More generally, locational decisions may matter. As earnings are higher in urban areas, if degree holders are more likely to live in cities, this will bias upward the returns to college (Black, Kolesnikova, & Taylor, 2009; Doyle & Skinner, 2016; Moretti, 2013). Moretti (2013) estimates that the four-year college wage premium is approximately one-fifth lower after adjusting for differences in cost-of-living between graduates and high school completers. This locational effect may also apply to community college students.

Another consideration is whether it is appropriate to model earnings gains separately from returns net of college costs. Earnings gains and college costs may co-vary; identifying earnings gains alone may therefore provide an incomplete estimate of the net returns. If earnings gains and college costs positively co-vary, then the net returns may be very stable across student groups. Altonji and Zimmerman (2017) directly link evidence on earnings gains to the costs of college and find that more expensive programs yield higher gains such that returns overall are compressed. Another factor is the economic cycle: this may be an important influence on the returns to associate degrees (Cappelli, 2015). The difference in earnings between degree holders and non-completers does appear stable over the economic cycle (Belfield, 2015, Scott-Clayton & Wen, 2017). But there is a dip in earnings and employment probabilities for students graduating during a recession (Herschbein, 2012; Oreopoulos, von Wachter, & Heisz, 2012.) This dip reduces the returns to college net of tuition and fees (but only assuming that students enrolled in courses with the unchanged tuition and fees).

6. Conclusions

There is considerable concern that college is no longer a good investment. It is therefore important that estimates of the earnings gains from college are robust. The above analysis shows that fixed effects models are robust to most sampling restrictions and most functional form specifications. But there are some exceptions. Results vary depending on how missing wages are modeled and on how educational attainment of transfer students is addressed. Results also vary depending on how time trends are modeled. Nevertheless, the conclusion that there are positive

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[30] Calculated from Scott-Clayton and Wen (2017, Tables 1 and 5) based on an out-migration rate of 30 percent.

returns to associate degrees over non-completion remains valid. Indeed, alternative plausible specifications indicate that the returns are even higher than conventionally estimated.

Fixed effects models do offer advantages over OLS estimation. They directly address biases due to individual-level omitted variables—even as this does not appear to be especially influential. As well, fixed effects models allow for new interpretations of time trends in earnings, including the effects of working while enrolled. Of course, these new interpretations rely on significantly more data than is available in cross-sectional datasets. In fact, the above studies suggest that the ideal window of data post-college needs to be very wide, covering at least five years after a student has exited college. OLS estimation of earnings premia requires relatively little data and, over several decades of application, has proved very robust. Alternative methods (such as quasi-experiments and propensity score matching) have typically found results that correspond to those from OLS estimation, and this also appears to be the case for fixed effects models (Barrow & Malamud, 2015; Carneiro et al., 2011). There are advantages from having large-scale longitudinal data, but these advantages are modest. Overall, fixed effects models yield similar results to OLS estimates.

Finally, even as fixed effects models appear to offer only a modest advantage in reducing bias and increasing precision, they do have a distinct advantage: they allow for analysis of earnings profiles over the period from before to after college. Given the large fluctuations in earnings over this period, this advantage may be significant in yielding evidence on the full returns to college. Students need to know that the end result is strongly positive (high earnings gains *x* years after leaving college with a degree) and that the journey to get there (leaving the workforce and progressing through college) is not economically burdensome. Fixed effects models shed new light on this journey.

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Appendix

Table A1: Robustness Checks on Returns to Associate Degrees

	Washington (Percent gains)		Michigan (Dollar gains)		Kentucky (Dollar gains)		California	Colorado	Ohio
	Female	**Male**	**Female**	**Male**	**Female**	**Male**	**Pooled**	**Pooled**	**Pooled**
Baseline FE including time trend	6.5%	2.1%	$2,363	$1,484	$2,346	$1,441	$1,650	$2,200	$2,439
Sample selection									
Age 25+ on entry; worked pre-entry	2.0%	-3.0%							
Age 20–50							$2,195		$2,510
Excluding enrolled in college five years later	5.9%	2.7%			$2,850	$1,850	$2,899	$2,226	
Including missing wages pre-entry	6.9%	2.4%							
Including younger persons			$2,230	$1,562					
Excluding younger persons					$3,150	$1,400			
Excluding zero credits in college			$2,337	$1,276			$1,565		
Only employed			$2,667	$1,660					
Short window (years post-college)			$2,346	$1,367			$1,196		$1,822
Including strong labor market attachment			$2,210	$1,381				$2,449	
Including weak labor market attachment					$2,350	$1,650			
Adjusted for enrollment of non-completers								$2,151	
Specifications									
Baseline excluding time trends/interactions	8.9%	7.2%	$2,440	$1,325			$1,643	$2,307	
Difference estimator			$1886	$1506				$1,439	
Adjusting for Ashenfelter Dip (1)	6.7%	2.4%			$2,350	$1,400	$1,662	$2,074	
Adjusting for Ashenfelter Dip (2)	6.7%	2.8%	$2396	$1,554	$2,350	$1,350			

Sources: Dadgar and Trimble (2015) (Washington); Bahr et al. (2015) (Michigan); Jepsen et al. (2014) (Kentucky); Bahr (2016) (California); Turner (2016) (Colorado); Minaya & Scott-Clayton (2017) (Ohio).

Figure A1: Percentage Decline in Earnings From Two Quarters Prior to College Entry to Quarter of College Entry (Kentucky, Ohio)