

Broken or Fixed Effects?*

Charlie Gibbons

Juan Carlos Suárez Serrato[†]

Mike Urbancic

Department of Economics
University of California, Berkeley

November 13, 2008

Abstract

Fixed effects are commonly employed in applied microeconomics to “control for” differences among subpopulations in the data. This framework may fail to capture the true underlying relationships between these groups and, resultingly, would not produce an estimate that is invariant to the composition of the sample under examination. To illustrate this point, we first examine the structure of fixed effects using a variance decomposition approach. Then, turning to data on the returns to education, we find that the distribution of the ages of the individuals in the sample greatly alters the estimated returns to education despite the inclusion of fixed effects. Interacting the education parameter with the age fixed effects is sufficient to generate stable estimates.

PRELIMINARY DRAFT—COMMENTS ARE APPRECIATED.

*We would like to thank Jas Sekhon for his guidance in this project. Additionally, we appreciate the input of seminar participants at UC Berkeley, especially Rocio Titunik and Erin Hartman, and at the Institute for Humane Studies conference, including Jesse Shapiro, for their comments and suggestions. We also thank attendees of the 2008 AEA Pipeline Conference at UCSB, including Rodney Andrews, Henning Bohn, Yolanda Kodrzycki, Trevon Logan, Fernando Lozano, Todd Sorenen, and Doug Steigerwald, for their comments and suggestions. Of course, the usual disclaimers apply.

[†]Corresponding author; jcsuarez@berkeley.edu

1 Introduction

Fixed effects are commonly applied throughout the empirical literature, especially in microeconomic studies. They provide a standard means to “control for” differences among subgroups within the population under investigation. The implicit expectation is (or ought to be) that the inclusion of these effects generates estimates of the parameters of interest identical to those produced by analyzing each subgroup independently. Put differently, the estimates should not depend upon the composition of the sample, as judged by the frequency of observations from each subgroup. Unfortunately, this result imposes severe assumptions upon the true data generating process; these assumptions are outlined in the first two sections of the paper. Happily, however, this expectation is testable and our regressions correctable; these propositions are illustrated in the two latter sections.

The literature on the returns to education provides an attractive touchstone for these points. This topic, of course, is of practical policy importance. Additionally, we can use the influential works of Acemoglu and Angrist (2000) and Lochner and Moretti (2004) to anchor our approach. These papers use data from the U.S. Census and are comprised of millions of observations, providing ample opportunity to identify and correct problems arising from misspecification. Our caveats and approach, however, can be applied to a broad array of questions in applied microeconomics.

Acemoglu and Angrist (2000) attempt to identify the effect of education on wages (the private returns to schooling), while Lochner and Moretti (2004) try to capture the reduction in crime as a result of education (the social returns).¹ These papers both employ similar models built around fixed effects for demographic characteristics.² Implicitly, when economists employ fixed effects, our goal is to achieve the qualities of a difference-in-differences approach, but attained via a single expression; indeed, a fully-interacted fixed effects model is precisely a difference-in-differences model.

In particular, fixed effects claim to provide stability in the parameter of interest irrespective of the underlying distribution of the fixed effects covariates; *i.e.*, the parameter should be estimated by within-, not between-group variation. The models for education employ fixed effects to control for, among other covariates, age, a factor that has great influence on the estimated effect of educa-

¹Acemoglu and Angrist also examine the social returns to education, specifically the effect of education on economic growth. We do not explore this aspect of their inquiry in this paper.

²These papers also employ instrumental variables to account for endogeneity issues; see Gibbons, Suárez Serrato and Urbancic (2008).

tion. Fixed effects are only successful if they capture the true underlying relationships between the covariates. If the specification is incorrect, the estimates can be highly susceptible to the choice of the population under examination. In our study, the distribution of ages in the sample greatly affects the estimate of the returns to education.

We investigate the use of fixed effects regression in four parts. First, we illustrate the consequences of incorrect specification with a simplified model. We further refine this argument by considering a more general variance decomposition analysis. As an empirical application, we look for stability in the estimates of the returns to education by comparing results from regressions employing age fixed effects to those produced via separate regressions for each age group. After finding unstable parameter estimates in these models, we successfully generate stability by interacting the education variable with the fixed effects.

2 Theoretical Underpinnings of Fixed Effects

The following two subsections provide first a simplified example and later a full variance decomposition illustration of the assumptions made by fixed effects estimators.

2.1 A Simplified Model

In this section, we review the econometric assumptions of fixed effects and relate these results to common intuitions. Using the approach of Lochner and Moretti as an example, a simplified version of the OLS model would regress a binary variable indicating incarceration (p_i) on an individual's education (e_i) and an age dummy (A_i), thus permitting two age groups. The proposed model is

$$p_i = \alpha + \beta e_i + \gamma A_i + \epsilon_i. \tag{1}$$

The OLS estimator for β is

$$\beta = \frac{\text{Cov}(e_i, p_i)\text{Var}(A_i) - \text{Cov}(A_i, p_i)\text{Cov}(e_i, A_i)}{\text{Var}(e_i)\text{Var}(A_i) - [\text{Cov}(e_i, A_i)]^2}.$$

Fixed effects are typically described as “controlling” for the variables that they represent. For example, the fixed effects in this model are assumed to prevent the coefficient on education

from fluctuating based upon the ages of the individuals included in the model. Hence, if we restrict our sample to only the individuals in the first age group, our estimate for β should be identical to that obtained by examining the entire population. Because the estimate of β is a function of the covariance between age and education, however, this parameter is not necessarily robust to changes in the ages in the sample.

Some assumptions will make this parameter robust to changes in the age distribution. First, the covariance between education and age must be zero. Also, age must not affect the joint distribution of education and imprisonment. Restated, the fixed effects cannot be correlated with any of the other regressors in the model and their contribution can only be a level effect on the dependent variable. If these conditions do not hold in the data, then the fixed effects will not remove age trends from the estimate of β , contrary to our usual intuition and desires.

2.2 A Variance Decomposition Illustration

We extend the model given in Equation 1 by permitting a full set of age fixed effects and for e_i to become a row vector of variables, \mathbf{x}_{ia} , where i remains the index of the individual and a indexes the age group of that individual.³ Our regression becomes

$$p_{ia} = \alpha + \mathbf{x}'_{ia}\beta + \gamma_a + \epsilon_{ia}. \quad (2)$$

This model can be rewritten in two ways.⁴ First, it can be expressed in terms of means across units in each age group a :

$$\bar{p}_a = \alpha + \bar{\mathbf{x}}'_a\beta + \bar{\epsilon}_a + \gamma_a, \quad (3)$$

where

$$\bar{p}_a = \frac{1}{\#\{i \in a\}} \sum_{i \in a} p_{ia},$$

³We are abusing notation here—the a index really should be $a(i)$. We follow the proposed notation for clarity.

⁴It should be stated that a full exposition of the differences between fixed and random effects approaches is beyond the scope of this inquiry. We direct the reader to Hsiao (2003) as an example of such a discussion. The following discussion is based upon Hsiao (2003) and Greene (2003).

with $\bar{\mathbf{x}}_a$ and $\bar{\epsilon}_a$ defined analogously. The model can also be expressed in terms of deviations from age group means:

$$p_{ia} - \bar{p}_a = (\mathbf{x}_{ia} - \bar{\mathbf{x}}_a)' \beta + \epsilon_{ia} - \bar{\epsilon}_a. \quad (4)$$

All three models could be estimated consistently, if not efficiently, via least squares procedures, given that Equation 2 correctly models the data generating process. The differences in the estimation of β lie in the moment conditions and the variance and covariance matrices used in each specification.

For Equation 2, these matrices are

$$S_{xx}^f = \sum_a \sum_{i \in a} (\mathbf{x}_{ia} - \tilde{\mathbf{x}})' (\mathbf{x}_{ia} - \tilde{\mathbf{x}}) \quad (5a)$$

$$S_{xy}^f = \sum_a \sum_{i \in a} (\mathbf{x}_{ia} - \tilde{\mathbf{x}})' (y_{ia} - \tilde{y}), \quad (5b)$$

where

$$\tilde{\mathbf{x}} = \frac{1}{\#\{i\}} \sum_a \sum_{i \in a} \mathbf{x}_{ia},$$

with \tilde{y} defined analogously as full sample means. These are the full sample variances and covariances and the application of these matrices generates the OLS estimator.

For Equation 3, we have the conditions

$$S_{xx}^b = \sum_a (\bar{\mathbf{x}}_a - \tilde{\mathbf{x}})' (\bar{\mathbf{x}}_a - \tilde{\mathbf{x}}) \quad (6a)$$

$$S_{xy}^b = \sum_a (\bar{\mathbf{x}}_a - \tilde{\mathbf{x}})' (\bar{y}_a - \tilde{y}). \quad (6b)$$

These equations difference each age group's mean from the sample mean; these conditions calculate the *between-group* variation. This formulation is akin to a random effects panel data model.

Lastly, for Equation 4, we have

$$S_{xx}^w = \sum_a \sum_{i \in a} (\mathbf{x}_{ia} - \bar{\mathbf{x}}_a)' (\mathbf{x}_{ia} - \bar{\mathbf{x}}_a) \quad (7a)$$

$$S_{xy}^w = \sum_a \sum_{i \in a} (\mathbf{x}_{ia} - \bar{\mathbf{x}}_a)' (y_{ia} - \bar{y}_a), \quad (7b)$$

These equations compare the realizations of an individual to the average of his age group; these conditions specify the *within-group* variation. This strategy is known as least squares dummy variable (LSDV) estimation and forms the basis of the fixed effects model.

Algebraic manipulations of Equations 5, 6, and 7 reveal that the full sample variance and covariance matrices are the sums of the within- and between-group variances and covariances; that is,

$$S_{xx}^f = S_{xx}^w + S_{xx}^b \quad (8a)$$

$$S_{xy}^f = S_{xy}^w + S_{xy}^b. \quad (8b)$$

The OLS estimate of β in the full sample is

$$\hat{\beta}^f = [S_{xx}^f]^{-1} S_{xy}^f = [S_{xx}^w + S_{xx}^b]^{-1} [S_{xy}^w + S_{xy}^b].$$

Realizing that $S_{xy}^w = S_{xx}^w \hat{\beta}^w$ and $S_{xy}^b = S_{xx}^b \hat{\beta}^b$, we obtain

$$\hat{\beta}^f = F \hat{\beta}^w + (I - F) \hat{\beta}^b, \quad (9)$$

where I denotes the identity matrix and

$$F = [S_{xx}^w + S_{xx}^b]^{-1} S_{xx}^w.$$

The OLS estimate is a weighted sum of the within- and between-group estimates of β , where the weight is directly related to the variance of the covariates in the respective regressions.⁵

In the model of Equation 3, identification requires that the fixed effects be independent of the other covariates; in our case, the fixed effects must be independent of education. In Section 4 we propose a model with fixed effects that have an explicit dependence upon education. As a simpler

⁵The generalized least squares estimator is the best linear unbiased estimator, but this point is ancillary to our discussion. Once again, the interested reader is directed to Hsiao (2003).

example of this dependence, assume the following model, based upon the model of Equation 2:

$$\begin{aligned} p_{ia} &= \alpha + \mathbf{x}'_{ia}\beta + \gamma_a + \epsilon_{ia} \\ \gamma_a &= \mathbf{x}'_{ia}\gamma + \eta_a, \end{aligned}$$

where η_a is distributed $N(0, \sigma_\eta)$. It can be shown that

$$\mathbb{E}(\hat{\beta}^w) = \beta, \text{ but } \mathbb{E}(\hat{\beta}^b) = \beta + \gamma \text{ and } \mathbb{E}(\hat{\beta}^f) = \beta + (I - F)\gamma.$$

This result arises from the fact that the between-group estimator does not control for the correlation among the fixed effects and the \mathbf{x}_{ia} .⁶ Also, to the extent that there is between-group variation (*i.e.*, $S^b_{xx} \neq 0$) and that F is not the identity matrix, the OLS estimate will also be biased, as it places non-zero weight upon the between-group estimate. Hence, failing to account for the correlation between the fixed effects and education biases the estimates.

3 Fixed Effects and Estimates of the Returns to Education

The education literature considered here uses U.S. Census data on black and white men from 1960, 1970, and 1980. Note that, while these files identify incarcerated respondents, they do not provide information on the crimes committed. Additionally, the Census data only identify individuals in prison at the time of the Census and not individuals that have ever been to prison or have committed crimes. To use incarceration as a proxy for crime *per se*, Lochner and Moretti assume that education does not affect the probabilities of arrest or incarceration or sentence length.⁷ Since younger men are more likely to be incarcerated than older men at a given time, there is a significant age trend in these data. In examining private returns to education, log weekly wages is the dependent variable.

Our approach, based upon Lochner and Moretti (2004), employs a base set of explanatory variables in each specification. Fixed effects for age (categorized into 14 dummies spanning three-year intervals: 20–22, 23–25, ...), state of birth, state of residence, and year are included. Some specifications also include cohort of birth (broken into decade-long dummies to account for year of

⁶This forms the basis of Mundlak's critique of random effects models.

⁷Lochner and Moretti offer a detailed examination of these limitations.

birth in addition to age: 1904–1913, 1914–1923, ...) and state of residence interacted with year effects.⁸

3.1 OLS Estimates

Acemoglu and Angrist and Lochner and Moretti first estimate the effect of education on crime via OLS regressions as a benchmark, temporarily ignoring the endogeneity of educational choice (we do not consider the effects of endogeneity *per se* here; see Gibbons, Suárez Serrato and Urbancic (2008)). The results of the OLS regressions are reported in Table 1. For the incarceration regressions, the parameter estimates are the change in percentage points of an individual’s propensity to be incarcerated at the time of the Census for each additional year of education. In the wage regressions, the estimates are the change in an individual’s log weekly wage for an additional year of education.

Acemoglu and Angrist recognize the importance of the age structure in their study; they limit their sample to men aged 40–49 “because they are on a relatively flat part of the age-earnings profile. This makes it easier to control for the effect of individual education on earnings” (Acemoglu and Angrist 2000). Lochner and Moretti use 14 dummies each spanning a three year age interval to account for similar concerns in their study. Their sample includes men aged 20–60, corresponding to the second line of Table 1a.⁹ To examine the efficacy of the age fixed effects, we run each of our crime and wage regressions on samples that include all men over 20, those 60 or younger (corresponding to the data set employed by Lochner and Moretti), those under age 45, and, lastly, individuals under age 30.

In the incarceration regressions, a comparison across the age groups shows that, as the sample is restricted to younger individuals, the parameter estimates of the effect of education on crime increase by a factor of 3 for blacks and 4 for whites (see Table 1a). The estimates are higher

⁸These specifications correspond to those used in Lochner and Moretti, but are broadly consistent with the approach of Acemoglu and Angrist as well. We use the same approach for studying both wages and crime to unite these strands and illustrate the desired methodological points, rather than perform strict replications. Since we add additional instruments to our analysis, we do not use the data provided openly by either pair of authors; instead, we acquire the raw data from their source. See Appendix I for a thorough data description. Also, as we note in the text, for some regressions, we use region, rather than state, of residence. In this case, we use the regions defined by the Census Bureau—northeast, north-central, south, and west. The use of regions rather than states does not alter the results.

⁹To emphasize the note to the tables of the paper, we deviate from convention and indicate significance at the 5% level using a single star and at the 1% level using a pair.

for blacks than for whites in all four age groups. Both these trends can be seen clearly in Figure 1.¹⁰

The trends in the parameter estimates in the wage regressions are comparatively stable; they are nearly flat for blacks across all age subsamples and only fall sharply for the youngest whites (see Table 1b). Wages for both whites and blacks appear to respond similarly to changes in education (exempting the youngest whites, for whom the effect appears smaller).

Table 1: OLS estimates

(a) Effect of education on imprisonment			(b) Effect of education on wages		
	Whites	Blacks		Whites	Blacks
All ages	-0.078** (0.002)	-0.298** (0.012)	All ages	0.052** (0.000)	0.051** (0.001)
60 or younger	-0.095** (0.002)	-0.364** (0.014)	60 or younger	0.052** (0.001)	0.053** (0.001)
45 or younger	-0.139** (0.004)	-0.547** (0.019)	45 or younger	0.045** (0.001)	0.056** (0.001)
30 or younger	-0.238** (0.007)	-0.917** (0.028)	30 or younger	0.020** (0.001)	0.052** (0.001)

Notes: Parameters for the incarceration regressions are in percentage terms. For all regressions, standard errors are clustered by state-year and appear in parentheses. Two stars indicate significance at the 1% level. See Table 3a and Table 3b in Appendix II for detailed descriptions and additional specifications for the incarceration and wage regressions respectively.

Correcting the instability seen in the parameter estimates is crucial to making reliable policy assessments. The fact that the estimate varies systematically depending upon the subpopulation under analysis reveals that the policy prescription will be confounded by the population used to generate the estimated effect. Any choice of population could be considered arbitrary, however. It is for this reason that we seek to find a model that generates stable parameter estimates across subsamples.

3.2 Linear Probability Model Estimates

Linear probability models are well-suited to modeling the effects of explanatory variables on a binary outcome variable, as is the case in the incarceration regressions. While binary choice models are not the prime focus of the paper, we employ these models to determine if their non-linear struc-

¹⁰The additional fixed effects included in specifications 2 and 3 in Table 3a do not eliminate the age trend.

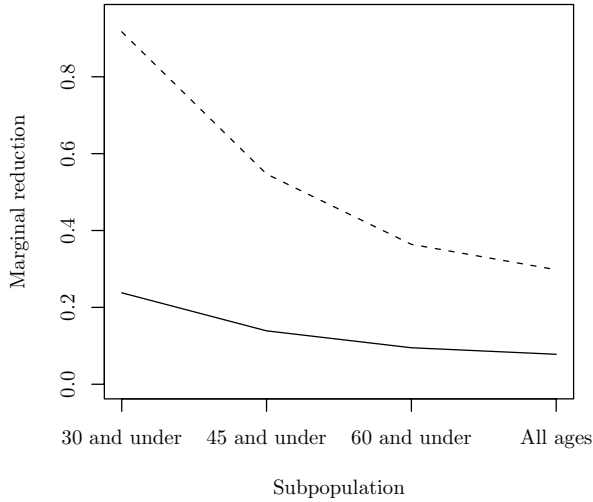


Figure 1: Reduction in criminal propensity due to a marginal increase in education for whites (solid) and blacks (dashed) across age subsamples

ture combats the parameter instability seen in the OLS estimates.¹¹ Results of this specification implemented in OLS, probit, and logit models are presented in Table 2.

The marginal effects of education in both the probit and logit models follow a similar pattern to the OLS results in Table 1a; as the sample reduces to a younger subset of respondents, the parameter estimates increase. Additionally, the logit estimates are uniformly lower than those in the probit models, which are uniformly smaller than those of OLS. These patterns hold for both blacks and whites.

While the coefficient estimates themselves are far more stable for both groups, this is not a satisfactory solution to the issue that we raised for two reasons.¹² First, since the parameter estimate and the standard error are not separately identifiable, it is unclear if the scale of the estimates actually remains the same across the models; ratios of pairs of coefficients across the subsamples could be used to examine this point. But, even if the estimates and their scale are constant among the subsamples, we would still not be satisfied; for it is the marginal effects parameter that is ultimately employed in welfare analysis and hence stability in this parameter itself is our goal.

¹¹Since Lochner and Moretti use a multitude of fixed effects in their regressions, direct applications of linear probability models yield quasi-perfect separation. To alleviate this issue, we use region, rather than state, of residence dummies while retaining the full set of state of birth dummies.

¹²We thank Jesse Shapiro for suggesting this line of inquiry.

Table 2: Probit and Logit estimates for the effect of education on imprisonment

WHITES				
	Probit		Logit	
	Coefficient	Marginal effect	Coefficient	Marginal effect
All ages	-0.080** (0.001)	-0.041** (0.001)	-0.212** (0.002)	-0.034** (0.001)
Below 60	-0.081** (0.001)	-0.061** (0.001)	-0.213** (0.002)	-0.051** (0.001)
Below 45	-0.089** (0.001)	-0.092** (0.001)	-0.223** (0.002)	-0.079** (0.001)
Below 30	-0.105** (0.001)	-0.147** (0.002)	-0.242** (0.002)	-0.122** (0.002)
BLACKS				
	Probit		Logit	
	Coefficient	Marginal effect	Coefficient	Marginal effect
All ages	-0.067** (0.002)	-0.239** (0.006)	-0.147** (0.003)	-0.189** (0.005)
Below 60	-0.068** (0.002)	-0.320** (0.007)	-0.148** (0.003)	-0.270** (0.006)
Below 45	-0.075** (0.002)	-0.490** (0.011)	-0.154** (0.003)	-0.426** (0.009)
Below 30	-0.092** (0.002)	-0.770** (0.017)	-0.174** (0.004)	-0.656** (0.014)

Notes: Estimates and standard errors (in parentheses) are multiplied by 100. Two stars indicate significance at the 1% level. The dependent variable is a dummy equal to 1 if the individual is in prison (a) or log weekly wages (b). All specifications contain dummies for age category (20–22, 23–25, . . .), year, state of birth, and state of residence. Regressions for blacks include a dummy for individuals in the south who turned 14 in 1958 or later to account for the impact of *Brown v. Board of Education*.

4 Interacting Fixed Effects

Intuition and common practice would lead us to believe that fixed effects would “control for” age in these regressions. Unfortunately, the results in Tables 1, 3a, and 3b reveal that strong age trend effects remain in the parameter estimates. Fixed effects are only appropriate if they correctly model the data generating process; namely, the assumptions set forth in Section 2 hold. Our results show that the data deviate greatly from these necessary relationships, resulting in substantial variability in the parameter of interest depending upon the age distribution of the sample.

A more flexible model may achieve a stable parameter estimate across age subsamples. As we saw in Section 2.2, to generate an unbiased estimate using within-group variation, we can add interaction terms between the parameter of interest and the fixed effects. This will correct the

specification of our model, yielding the desired properties; specifically, the effect of education for each age group should be constant across subsamples. Figure 2 illustrates the marginal reduction in the probability of incarceration for each subsample by age, more precisely, the derivative of our criminal propensity regression with respect to education by age group. This figure shows that the interacted model generates this stability. This figure is analogous to Figure 1, but incorporates age-specific education parameters. This series of near-horizontal lines demonstrates that these estimates are robust to changes in the population under examination and the resulting age distribution of the sample.

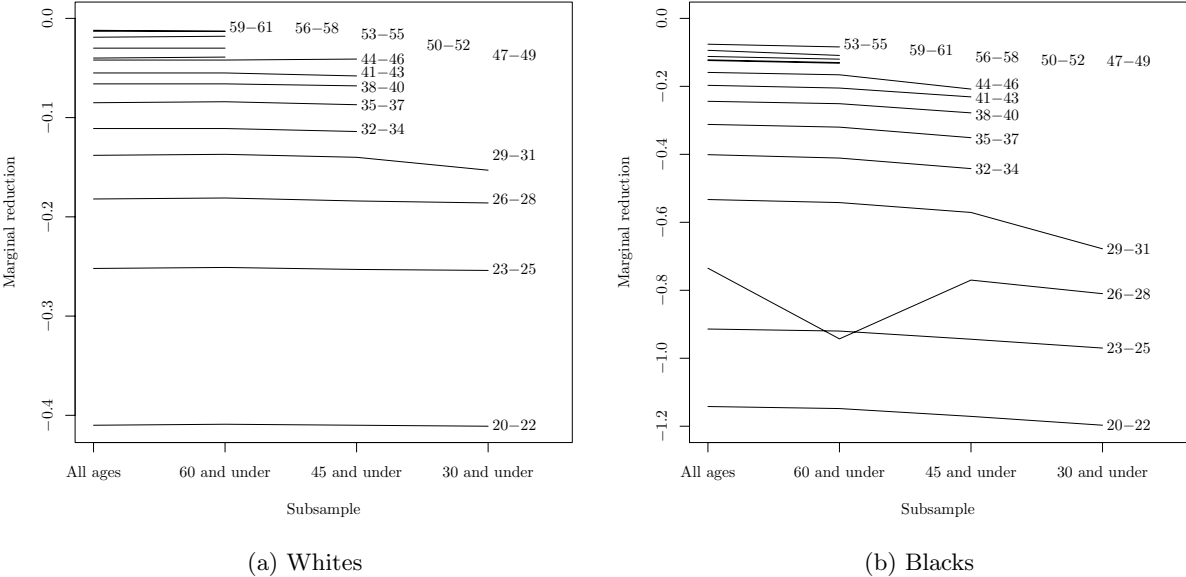


Figure 2: Marginal effect of education on crime by age across age subsamples

These age-specific parameter estimates reveal that the effect of education varies with age. Figure 3 shows the reduction of an individual’s propensity to commit crime due to a marginal increase in educational attainment across age groups. Note the resemblance between these curves and the patterns reflected in Figure 1; the interaction terms capture the instability that plagues the original model.

Finally, additional flexibility can be added by running the regression separately for each age group; here every parameter, not only education, can vary by age. Ideally, this model would generate the same parameter estimates as the more parsimonious age-education interaction model

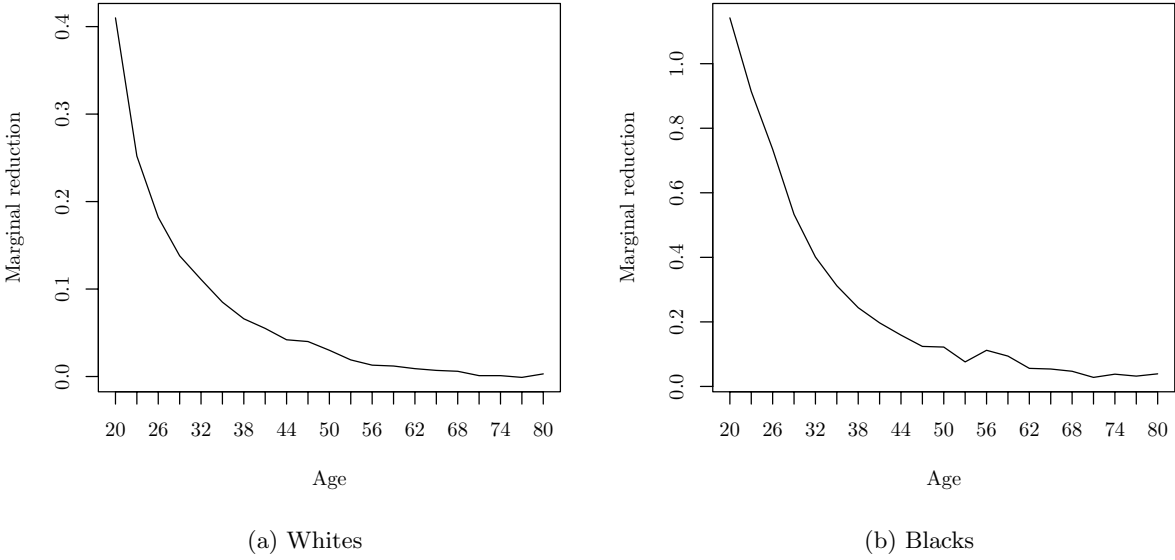
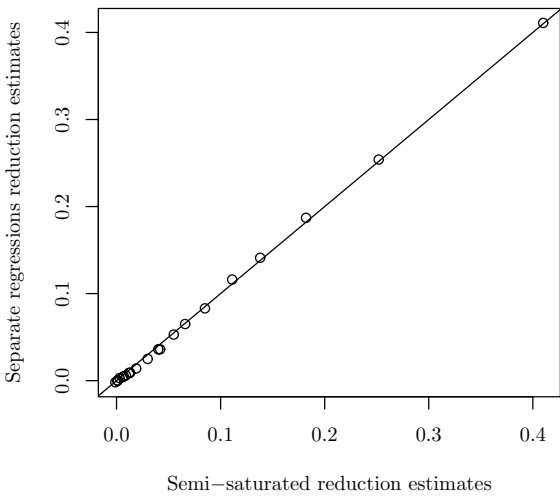


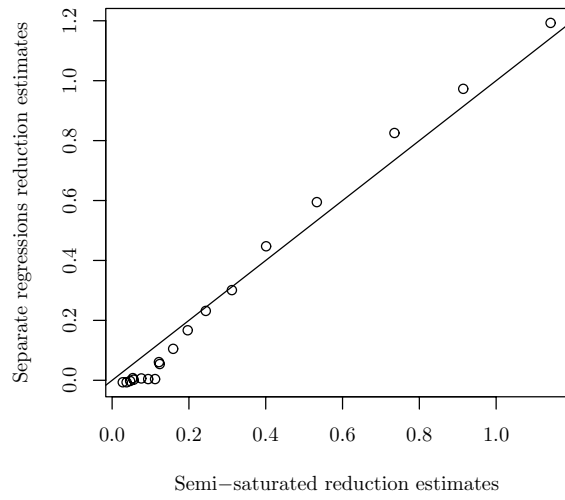
Figure 3: Reduction in criminal propensity due to a marginal increase in education across ages

outlined above. Figure 4 plots estimates of the reduction in criminal propensity from a marginal increase in education for the semi-saturated age-education interaction model on the x -axis and the estimates from separate regressions for each age group on the y -axis; each point represents an age group. The models generate identical results if the points lie on the 45-degree line. The semi-saturated model performs quite well for both groups, especially for whites, in relation to regressions run separately for each age group.

Recalling from Table 1b that the estimates in the wage regressions were relatively stable as compared to those in the incarceration regressions, one would be tempted to believe that the effect of education on wage would be relatively constant across age groups. This homogeneity would yield parameter estimates that do not vary with the underlying age distribution in the sample, precisely the empirical result that we found. But Figure 5 (a wage-effect analogue to Figure 3) reveals that this assumption is inaccurate. Acemoglu and Angrist worry about a non-linear age-earnings profile, but this relationship would be neatly captured by fixed effects if age only altered the level of wages and was independent of other covariates. Our analysis shows that the age-earnings relationship makes estimation more difficult because of the correlation between age and education. A more accurate condition that Acemoglu and Angrist seek is that the marginal effect of education is the



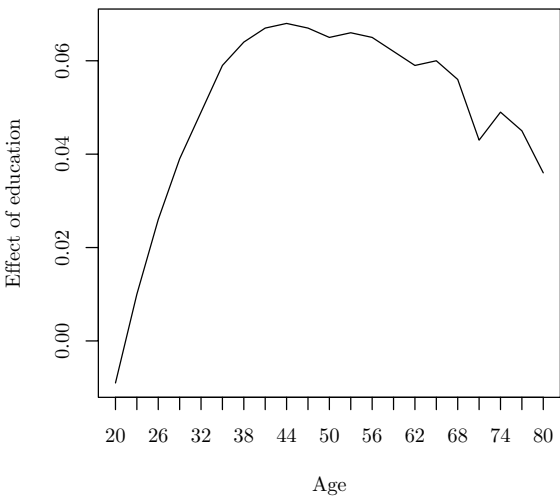
(a) Whites



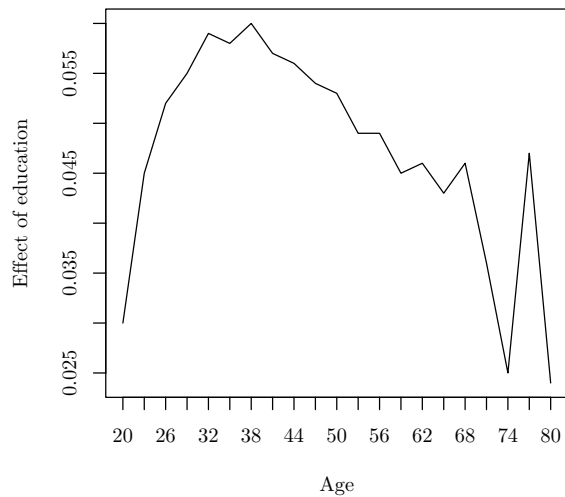
(b) Blacks

Figure 4: Reduction in criminal propensity due to a marginal increase in education in semi-saturated and separate regressions

same across age groups.



(a) Whites



(b) Blacks

Figure 5: Change in log weekly wage due to a marginal increase in education across ages

While older individuals weaken the estimated effect of education on crime, they strengthen

the wage effect. These relationships likely hold because, while older individuals are less likely to commit a crime and thus be imprisoned, they are also likely to have more work experience and resultingly higher wages. The parameter instability arises when these effects are not simply level effects, but rather are correlated with education. Older individuals that are imprisoned are likely incorrigible; while young men may be incarcerated for youthful indiscretions, the elderly in prison are more likely to possess deep-seated criminal propensities. Similarly, older men in the workforce likely remain there because of their high earning power. In other words, these relationships arise from endogeneity.¹³

A glance across the tables and figures above suggests that the marginal effect of education does vary across age subsamples, but we should employ a test of statistical, rather than mere optical, significance. The most straightforward and familiar test would be an F -test of the joint significance of the age-education interaction terms in these regressions. In the criminal propensity regression, the F -statistics for whites and blacks are 147.50 and 58.59 respectively and the values for the wage regressions are 259.84 and 9.55. The critical value for significance at the 95% level is 1.57, suggesting that these interactions are jointly highly significant in all cases. This is especially notable in the wage regression because the across-subsample estimates from Table 1b suggest stability, a result now invalidated both by graphical and statistical evidence.

Clearly, relationships between explanatory variables can confound estimates of the effect of interest and result in parameters that are highly dependent upon the sample under examination. Table 1 suggests that, by dividing the entire sample into subsamples, a researcher can detect these relationships. But Figure 5 shows that, even if a parameter is relatively stable across some subgroups, this does not imply the absence of a confounding relationship. Inclusion or exclusion of interaction terms can be justified using a joint F -test of their significance.

5 Conclusion

Fixed effects are a commonly employed in applied microeconomics to account for differences between subgroups in our analyses. We do not consider, however, whether these elements truly capture the

¹³See Gibbons, Suárez Serrato and Urbancic (2008) for a discussion of the application of instrumental variables in this context. Endogeneity, of course, is only one explanation. Any deviation of this model from the necessary implications of the fixed effect structure outlined in Section 2 can lead to trends in the parameter estimates.

relationships among these groups. Ideally, these structures would produce a stable parameter estimate—*i.e.*, one that is not dependent upon the composition of the sample under examination and similar estimates would be generated by running separate regressions for each subgroup. For success in this endeavor, the fixed effects must be uncorrelated with the other regressors and they must manifest themselves as purely level effects in the dependent variable.

By examining separate age groups in Lochner and Moretti's (2004) study of the effects of education on crime, we show that fixed effects can be ineffective in removing trends from the parameter of interest. Following a parallel approach with Acemoglu and Angrist's (2000) study reveals that parameter stability does not imply pure level effects in the fixed effects parameters. Caution must be exercised when employing these structures and a careful examination of the results may suggest improvements to our models. Interacting fixed effects with the parameter of interest can be quite effective in generating stable estimates and an F -test can be used to test these additional parameters for joint significance.

Unfortunately, the assumptions underlying the general application of fixed effects and regressions more generally are hidden and unconsidered. We have provided means for examining parameter stability *ex post* and for correcting estimates that do not generate this result. By examining our estimates more carefully and correcting those deemed deficient, we can produce more reliable and informative parameter estimates.

References

- Acemoglu, Daron and Joshua Angrist. 2000. “How Large are Human Capital Externalities? Evidence from Compulsory Schooling Laws.” *NBER Macroeconomics Annual* 15:9–59.
- Gibbons, Charles E., Juan Carlos Suárez Serrato and Mike Urbancic. 2008. “LATE for School: Instrumental Variables and the Returns to Education.” *Working Paper* .
- Greene, William. 2003. *Econometric Analysis*. Fifth ed. Prentice Hall.
- Hsiao, Cheng. 2003. *Analysis of Panel Data*. Second ed. Cambridge University Press.
- Lochner, Lance and Enrico Moretti. 2004. “The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports.” *American Economic Review* 94(1):155–189.

Appendix I Data

Lochner and Moretti (2004) use data from the 1960–1980 Censuses in their study. Because we extend their analysis, specifically by the introduction of two additional instruments, we cannot use the version of their data that they make publicly available. Instead, we recollect the data from the Integrated Public Use Microdata Series (IPUMS) at the University of Minnesota. We add quarter of birth for all the Census observations and identify the minimum employment age requirement applicable during their youth. The compulsory schooling age and minimum employment age data are only provided from 1914 forward.¹⁴ Hence, Lochner and Moretti limit their observations to individuals between the ages of 20 and 60 in the 1960–1980 Censuses. We expand this set by including ages 60–70 in the 1970 and 60–80 in the 1980 Censuses.¹⁵ Due to computational limitations, Lochner and Moretti use a 90% sample of their Census file in their paper.¹⁶ Since the seed of this sample was not retained, we are not able to perfectly replicate their sample in our data.¹⁷ Our study takes advantage of increased computing power to examine the entire sample plus the additions listed above. These additions make maximum use of the instruments available.

We follow Lochner and Moretti in analyzing the regression models separately for blacks and whites. This division controls for the inequalities in education experienced by the two races. The regressions for blacks include a dummy variable indicating if the respondent was a black man born in the South who turned 14 in 1958 or later. This incorporates changes in education quality resulting from the Supreme Court decision *Brown v. Board of Education*.¹⁸

Appendix II Regression results

¹⁴Compulsory schooling and minimum employment age data are found in Acemoglu and Angrist (2000).

¹⁵We also add ages 20–50 from 1950, but most individuals in the 1950 Census do not have educational attainment records. When we include those that do have attainment variables, our results do not change markedly. We cannot utilize the 1990 Census because incarceration status is not made publicly available by the Census bureau.

¹⁶Personal communication with the author.

¹⁷In our paper, when we refer to the Lochner-Moretti sample, we are referring to our approximation to their sample; specifically, all black and white men in the 1960–1980 Censuses aged 20–60 born in the contiguous 48 states (Alaska and Hawaii were not part of the union until 1959).

¹⁸We define the south as those states whose Congressmen supported the Southern Manifesto in protest of the *Brown v. Board* decision, namely Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

Table 3: Regression results of the effect of education using alternative function forms

(a) Effect of education on imprisonment				(b) Effect of education on wages			
WHITES				WHITES			
	OLS				OLS		
	(1)	(2)	(3)		(1)	(2)	(3)
All ages	-0.078** (0.002)	-0.078** (0.002)	-0.078** (0.002)	All ages	0.052** (0.000)	0.052** (0.000)	0.052** (0.000)
60 or younger	-0.095** (0.002)	-0.095** (0.003)	-0.095** (0.003)	Below 60	0.052** (0.001)	0.051** (0.001)	0.051** (0.001)
45 or younger	-0.139** (0.004)	-0.139** (0.004)	-0.140** (0.004)	Below 45	0.045** (0.001)	0.044** (0.001)	0.044** (0.001)
30 or younger	-0.238** (0.007)	-0.238** (0.007)	-0.239** (0.007)	Below 30	0.020** (0.001)	0.019** (0.001)	0.019** (0.001)
BLACKS				BLACKS			
	OLS				OLS		
	(1)	(2)	(3)		(1)	(2)	(3)
All ages	-0.298** (0.012)	-0.297** (0.012)	-0.298** (0.012)	All ages	0.051** (0.001)	0.051** (0.001)	0.050** (0.001)
60 or younger	-0.364** (0.014)	-0.364** (0.015)	-0.366** (0.015)	Below 60	0.053** (0.001)	0.052** (0.001)	0.051** (0.001)
45 or younger	-0.547** (0.019)	-0.549** (0.019)	-0.554** (0.019)	Below 45	0.056** (0.001)	0.056** (0.001)	0.055** (0.001)
30 or younger	-0.917** (0.028)	-0.922** (0.028)	-0.930** (0.028)	Below 30	0.052** (0.001)	0.051** (0.001)	0.050** (0.001)
State of residence	X	X	X	State of residence	X	X	X
Cohort of birth dummies		X	X	Cohort of birth dummies		X	X
State of residence × year			X	State of residence × year			X

Notes: Standard errors are clustered by state-year and appear in parentheses. The specifications are applied to different age groups as noted. The dependent variable is a dummy equal to 1 if the individual is in prison (a) or log weekly wages (b). All specifications contain dummies for age category (20–22, 23–25, ...), year, state of birth, and state of residence. Specification (2) contains cohort of birth dummies (1904–1913, 1914–1923, ...). Specification (3) contains dummies for cohort and state of residence crossed by year. Regressions for blacks include a dummy for individuals in the south who turned 14 in 1958 or later to account for the impact of *Brown v. Board of Education*. A single star indicates significance at the 0.05 level; two stars indicate significance at the 0.01 level.