Parental Control and College Attendance among East Asian and Mexican American Youth from Immigrant Families

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ABSTRACT

Drawing on data from three waves of the National Longitudinal Study of Adolescent Health (Add Health), the present study examines the association between parental control over children's daily activities and college attendance among the two largest immigrant ethnic groups in the United States: Mexican and East Asian Americans. The major goal of this research is to assess the extent to which the Asian-Hispanic gap with respect to an immigrant advantage in educational attainment is attributable to differences in experiences of restrictive parenting practices. I rely on propensity score matching techniques based on a counterfactual analysis framework to strengthen causal inferences about parental control effects and also conduct a sensitivity analysis of result robustness. Consistent with prior findings for other educational outcomes, my study identifies an advantage from having immigrant parents with respect to college attendance, with the advantage being more salient among East Asian youth. Yet propensity score matching estimates suggest that parental controls over children's daily lives tend to have no effect on the likelihood of college attendance across ethnicity and parental nativity, except that a negative effect is found among children of East Asian immigrants. The phenomenon of East Asian youth benefiting more than Mexican American youth from having an immigrant family background is thus unlikely to be driven by differences in experiences in parental control.

Due to massive waves of immigration, living with immigrant parents has become a common life experience for American youth. Indeed, recent estimates suggest that approximately one in five school-age youth reside in an immigrant family in the United States, including about half of Hispanic youth and 90 percent of Asian youth (Kao & Thompson 2003). As education is a well-established route to economic success, understanding how youth from immigrant families fare in school is of considerable interest for the inquiry into immigrant adaptation. Yet prior research on racial/ethnic gaps in education has produced a fascinating puzzle: whereas children of Asian immigrants tend to academically outperform their otherwise similar co-ethnic peers with native-born parents, Hispanic youth are generally not found to enjoy the same academic benefit from having immigrant parents, despite comparable socioeconomic handicaps both groups face (Glick and White 2004; Kao 2004; Kao and Tienda 1995).[1]

Why might having an immigrant family background bring more of an educational benefit for Asians than for Hispanics? One possible explanation lies in disparities in parenting styles between Hispanic and Asian immigrant families. Both anecdotal and scholarly accounts suggest that parental controls over children's daily behaviors, for example, may play a role in Asian children's academic success (Chao 1994, 2001; Kao 2004). Such controls may buffer potential risk factors for academic failure both within and outside the family. Yet previous research reveals that Hispanic and Asian youth tend to experience many commonalities in family processes, including restrictive parental authority (e.g. Fuligni 1998). It thus follows that seemingly homogeneous immigrant families may function disparately in the sense that, with respect to educational outcomes,

[1] In a separate dissertation chapter I use multilevel modeling techniques to investigate the contribution of variation in school quality to these group differences in educational outcomes.

the consequences of parental control can vary across groups. In other words, even if levels of parental control are similar in Asian and Hispanic families, the effect of a restrictive family environment on academic achievement may itself differ.

Prior studies of the nexus between parental control and children's educational outcomes, however, suffer from a number of notable shortcomings. First of all, most findings relate to pan-ethnic aggregates such as "Hispanics" or "Asians," thereby potentially concealing important ethnic variations within these groups (Kao & Thompson 2003; Zhou 1997). Secondly, the existing literature has focused on school performance measures such as grades, test scores, and high school dropout, with less attention to educational outcomes at the post-secondary level. Studying post-secondary outcomes is crucial given the fact that, in tandem with a fundamental shift toward the post-secondary level in educational inequality along racial lines (Mare 1995; Kao and Thompson 2003), the most remarkable gap in earnings today exists between college graduates and those without a college degree (Levy and Murnane 1992). Finally, and perhaps most importantly, the use of observational data in almost all prior studies raises serious concerns with causality of the observed associations between parental control and youth educational outcomes. Conventional multivariate regressions are known to be subject to selection bias due to *unobserved* confounders. Yet they may also be flawed in controlling for *observed* characteristics to the extent that covariates supposed to be "held equal" are indeed significantly heterogeneous in the analyzed sample.[2]

The present study, which addresses each of these limitations of prior work, investigates how parental control affects children's likelihood of college attendance among East Asians (Chinese, Korean, and Japanese) and Mexicans, two of the largest

[2] See below for a detailed discussion of this issue.

immigrant ethnic groups in the United States. Drawing on data from the National Longitudinal Study of Adolescent Health (Add Health), I ask *whether group differences in the experience of parental control over daily life can explain the greater immigrant advantage observed among East Asian-origin youth relative to Mexican youth.* To improve my ability to make causal inferences about parental control effects, I resort to propensity score matching techniques based on a counterfactual analysis framework, and also conduct a sensitivity analysis of the robustness of my results to the potential existence of unobserved confounding variables. Finally, substantial missing data on covariates are formally handled by simultaneously imputing relevant covariates through an iterative algorithm that incorporates randomness. The imputation is necessary given that sample sizes within groups under study (see below) tend to be small and given the potential for biased estimates of parental control effects due to selectivity in patterns of missing data.

BACKGROUND

Parental Control and Educational Outcomes among Children of Immigrants

Immigrant families in the Untied States are commonly thought to be more "traditional," characterized particularly by restrictive authority of parents, than is true of the American mainstream (Chao 2000; Fuligni 1998). Potential destructive impacts of authoritarian parenting upon child development have been widely acknowledged by psychologists (e.g. Dornbusch et al. 1987; Lamborn et al. 1991). Yet childrearing practices such as night curfews and limitations on whom to hang around with may also serve to shield children from risk factors for school failure, including gang crime and negative peer influences prevailing in many neighborhoods where immigrants live (Zhou 1997). More importantly, the unique socialization setting of immigrant-parentage children may lead them to be less rebellious toward parental controls over their lives. A growing literature attributes outstanding educational accomplishments of children from immigrant families to their tendency to respect parental authority (Kao and Thompson 2003).

Yet prior studies remain inconclusive as to whether, concerning educational outcomes, parental controls are necessarily a positive factor in youth's lives. In particular, practical implications and consequences of parental control may vary across immigrant generation and race/ethnicity (Chao 1994; Dornbusch 1989; Hirschman, Lee, and Emeka 2004). Using data from the National Education Longitudinal Study (NELS), for example, Kao (2004) reports racially differentiated *ceteris paribus* effects of parental dominance over decision-making within the family on high school grade point averages, with parental control over rules such as curfew timing associated with better school performance for Asian children but not for Hispanics. She thus concludes that Asian children tend to "react more favorably towards increased parental control" (p.447). An analysis of Korean American adolescents, however, does not suggest any positive correlation between parental control and grade point averages, suggesting possible further heterogeneity within such broad categories as "Asians" (Kim and Rohner 2002).

Highlighting the role of parental control, the present study sheds new light upon the Asian-Hispanic achievement puzzle by scrutinizing it at the immigrant generational level. I choose to compare two culturally distinctive ethnic groups, East Asian and Mexican Americans, which also rank as the two largest immigrant groups in the United

States. On the one hand, Chinese, Korean, and Japanese share an arguably similar Confucianist tradition, which places parental authority and filial obedience in the center of civility and highly values education. On the other hand, Mexican Americans stand unparalleled in term of population size and socioeconomic disadvantage vis-à-vis any other major Hispanic group (Perlmann and Waldinger 1997). Recognizing an omission of comparative studies of the relationship between parental control and post-secondary educational outcomes in the literature, the present study targets college attendance and examines variations in this relationship across ethnicity and parental nativity. [3]

Assessing Parental Control Effects Using Observational Data

When the key explanatory variable is a "manipulable" factor such as parental control over children's daily activities instead of, for example, gender, the concern about its *causal* effects arises. In observational studies of the influence of parental control upon academic achievement, children reporting high levels of parental control may also tend to be distinctive in terms of other family and individual characteristics, which may be measured or unmeasured. To borrow language from experimental studies, children are not randomly assigned to families differentiated by the "treatment," in this case to families with differing levels of parental control. Thus, to obtain unbiased estimates of parental control effects on academic achievement, it is critical that observational studies control for potential confounders such as parental social status and children's prior academic performance, which may affect both the treatment (parental control) and the outcome (academic achievement).

[3] Note that in this study I adopt a simplified measure of generation status, namely parental nativity, making distinction only between children with immigrant parent(s) and those born to two native parents.

Although a comprehensive review of the advantages and disadvantages of various strategies for accounting for confounding factors is beyond the scope of this paper, it is worth highlighting a possible key methodological problem in the conventional multivariate regression approach. If, for simplicity, parental control is measured dichotomously (e.g. any control vs. no control), the coefficient for this variable obtained from multivariate regression estimators amounts to an average effect over the entire range or density of the (observed) covariate vector (Smith 1997). Nevertheless, this estimate can be biased when the distribution of covariates significantly differs between children with and without parental control. The bias stems from the fact that the conventional multivariate regression approach does not automatically ensure the condition of common support (Bryson, Dorsett, and Purdon 2002; Smith 1997). To wit, the two groups will be practically incomparable if, for certain combinations of covariate values, only children with parental control or only those without can be found. Consequently, multivariate regressions tend to perform poorly in "controlling for" observed covariates, despite further biases resulting from unobserved confounders.

The present study utilizes an alternative to standard multivariate regressions--propensity score matching--to estimate parental control effects (Rubin 1973). This method starts with estimating a propensity score for each observation, which can simply be the fitted probability of receiving "treatment" (i.e. parental control) based on a binary-response model (e.g. probit) [4] with relevant determinants of parental control as predictors. The next and crucial step is to match each "treated" case with one or more "untreated" case(s) with similar propensity scores. The idea is that, if the *balancing property* is satisfied, treated cases and their untreated match(es) will be observationally identical with

[4] Note that the treatment variable is still assumed to be dichotomous.

respect to the multivariate distribution of observed characteristics, conditional on propensity scores (Rosenbaum and Rubin 1983b). Put in a different way, the "assignment" of treatment is now considered to be independent of the observed characteristics as predictors of parental control. It is worth emphasizing that, in the meantime, the common support condition can be explicitly imposed (Bryson et al. 2002; Caliendo and Kopeinig 2005; Ham, Li, and Reagan 2006). Specifically, matching is only conducted over the overlapping region of the distributions of propensity score in the treated and untreated groups, ensuring that, conditional on observed characteristics, the probability of being in each group is non-zero.[5] The original sample is thus refined by retaining only treated and untreated cases that are matched while discarding all unmatched ones. Finally, the "cherry-picked" pairs forming the matched sample allow one to calculate a non-parametric *average effect of treatment on the treated* (ATT) in a counterfactual sense (Harding 2003; Morgan 2004; Rosenbaum and Rubin 1983b).

Propensity score matching offers certain methodological strengths over conventional multivariate regressions. Matching, which mimics the randomization in true experiments, gains its conceptual clarity by explicitly sorting out *comparable* treated and untreated cases, with the treatment effect defined for those receiving the treatment.[6] Such a counterfactual framework lends arguably solid grounds for drawing causal inferences about the association of educational outcomes with parental control (Holland 1986; Smith 1997). Matching on a univariate scalar summary of covariates, namely the propensity score, is also parsimonious relative to comparable methods of exact matching. Finally, use of matching and non-parametric estimation circumvent to some degree inextricable

[5] See also Lechner (2000) for caveats about this approach.
[6] Conversely, the treatment effect can also be defined for those receiving no treatment if the matching procedure is modified accordingly.

issues of model specification that haunt all multivariate regressions. Bear in mind, nevertheless, that propensity score matching only tackles selection bias due to observed characteristics (Dehejia and Wahba 1999; Rubin 1997). To obtain well-founded causal inferences about the treatment effect, it is thus necessary to ascertain how robust propensity score matching estimates are in the presence of unobserved confounders.

DATA AND ANALYTIC STRATEGY

The Add Health Data and Missingness on Covariates

The data used are from the three waves (1995, 1996, and 2001) of the National Longitudinal Study of Adolescent Health (Add Health), a school-based study of health-related behaviors of American adolescents. The Add Health sample at wave I consists of approximately 20,000 seventh to twelfth graders, including 640 East Asian[7] and 1,850 Mexican youth (Bearman, Jones, and Udry 1997). The analysis is limited to 448 East Asians and 1308 Mexicans who participated in the last follow-up survey in 2001.

The matching procedure takes advantage of the longitudinality of the Add Health study design. The outcome, college attendance, is based on information collected at the last wave (2001). Parental control is indicated by the adolescent's report at wave II (1996) as to whether he or she was allowed to make independent decisions regarding the following: (1) the time he/she must be home on weekend nights, (2) the people he/she hangs around with, (3) what he/she wears, (4) how much television he/she watches, (5) which television programs he/she watches, (6) what time he/she goes to bed on week

[7] Over four hundred of them come from a Chinese over-sample.

nights, and (7) what he/she eats.[8] All family and child characteristics used to predict parental control are assessed at the time of the baseline survey (1995), thus minimizing potential problems of causal ordering. Family characteristics include poverty status, parental education, intact family status, public assistance, and parental aspirations toward child's college attendance. Child characteristics include gender, maturity (at least 18 years old), Peabody Picture Vocabulary Test (AHPVT) score, and last year's average grade (See Appendix 1 for construction of variables).

As with other nationally representative surveys, the Add Health contains only relatively small samples of Mexicans and East Asians. The limitation on sample size is further exacerbated by missing data on a large number of variables. The missingness stems mainly from two sources: nonresponse to particular survey items and sample attrition over time. Specifically, the response rate is 78.9 percent for the baseline interview, and approximately 70 percent of the original Mexican and East Asian samples were re-interviewed at two follow-ups.[9] I impute missing values of all covariates on which at least one percent of the observations are missing.[10] This is accomplished through the STATA program *-ice* (Royston 2004, 2005a, 2005b), which implements an iterative procedure called multivariable regression switching (see Appendix 2 for details). All subsequent analyses are thus based on the imputed data.[11]

Matching on Propensity Scores and ATT

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[8] The battery of items has good reliability (Cronbach's alpha 0.65).
[9] The whole Add Health sample follows almost identical pattern of attrition over time (Bearman et al. 1997)
[10] See Table A1 in Appendix 2. After imputation on such covariates, only less than 20 cases need to be excluded due to missingness on the nonimputed variables.
[11] Indeed, the study parallel to the one presented here cannot proceed if the original data are used, since the sample size will simply be too small for the subgroup of native-parentage East Asians. For the other three subgroups, propensity score matching estimates of parental control effects turn out to be quite different. This may be a sign of selectivity in patterns of missing data in the Add Health data (see also Allison 2001).

A dichotomous treatment variable is created by distinguishing children who were granted autonomy on everything (no control) from those subject to at least some parental control. Note that as the former group turns out to be the minority in the sample, I code "no control" as 1 and, accordingly, define the treatment as *removal of parental control*. Matching is conducted separately within each of the four strata identified by both ethnicity (Mexican vs. East Asian) and parental nativity (immigrant vs. native born), thus allowing for heterogeneous treatment effects across subgroups (Caliendo and Kopeinig 2005).

Propensity scores as fitted probabilities of receiving the "treatment" (no parental control) are attained using probit regression.[12] Predictors include relevant covariates that theoretically or empirically affect both parental control and college attendance, provided that the balancing property is not violated (see Bryson et al. 2002; Caliendo and Kopeinig 2005; Heckman et al. 1997; Rubin and Thomas 1996).[13] Dehejia and Wahba's (2002) iterative algorithm, which checks the balancing property with respect to individual covariates, provides further guidelines for model specification.[14] In brief, the range of estimated propensity scores is divided into equally spaced blocks such that, within each block, the average score does not significantly differ between the treatment and untreated group. Once equality of propensity score holds within each block, balance on individual covariates is then gauged through conventional *t*-tests.[15]

[12] As Smith (1997) suggests, any other binary-response model will also suffice because the purpose is only to sort the cases.
[13] See below.
[14] The algorithm is partially implemented by a STATA program, *pscore* (Becker and Ichino 2002, p.360)
[15] When one or more covariates are not balanced, a less parsimonious model is recommended (p.360). For instance, higher-order and interaction terms are likely candidates to be added.

I next apply *a nearest-neighbor matching procedure* in which each case receiving treatment (no parental control) is matched with one closest available untreated case.[16] The common support condition is ensured by limiting the matching exclusively to the intersection of the distributions of propensity score for treated and untreated groups.[17] In view of its central importance to propensity score matching techniques, I rely on multiple indices and tests to assess matching quality. T-tests are used to test for equality of means for each individual covariate between the treated cases and untreated matches. For each covariate, I further assess reduction in the standardized bias, defined as the difference in means between treated and untreated group as a percentage of the square root of the arithmetic average of their variances (Rosenbaum and Rubin 1985). Two overall measures of covariate balance are also calculated before and after matching. One is the pseudo-R^2 as a goodness-of-fit indicator for the probit regression fitting propensity scores. Pseudo-R^2 should be fairly small when the matched sample is used to re-estimate propensity scores, the reason being that, if matching succeeds in achieving balance on the multivariate distribution of covariates, covariates are no longer expected to have much power predicting membership in the treatment group (Caliendao and Kopeinig 2005). Similarly, likelihood ratio tests on the joint significance of covariates should be rejected for the matched sample if the balancing property is satisfied for the matched sample.

Average effect of treatment on the treated (ATT) and its variance[18] are generally given by formula (1) and (2) below (Becker and Ichino 2002).[19] Since both the outcome and treatment are binary in this case, the matched sample can be represented by a 2×2

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[16] All cases are first sorted by propensity scores.
[17] See Caliendao and Kopeinig (2005) for an extended discussion on the issue of common support.
[18] The variance will be attained through bootstrapping.
[19] Both matching and calculation of ATT are implemented by the STATA program *–psmatch2* (Leuven and Sianesi 2003)

table. Accordingly, alternative measures of treatment effect such as odds ratio and χ^2 statistic can be calculated.[20] As shown below, odds ratios provides a convenient way to assess potential influences of omitted/unobserved covariates. Resulting estimates are thus compared across four strata in search for patterns of ethnic/generational differences.

$$ATT = \frac{1}{N^T}\sum_{i\in T}\left(Y_i^T - \sum_{j\in C(i)} w_{ij} Y_j^C\right) = \frac{1}{N^T}\sum_{i\in T} Y_i^T - \frac{1}{N^T}\sum_{j\in C} w_j Y_j^C \qquad (1)$$

$$\begin{aligned} Var(ATT) &= \frac{1}{\left(N^T\right)^2}\left\{\sum_{i\in T} Var\left(Y_i^T\right) + \sum_{j\in C}\left(w_j\right)^2 Var\left(Y_j^C\right)\right\} \\ &= \frac{1}{N^T} Var\left(Y_i^T\right) + \frac{1}{\left(N^T\right)^2}\sum_{j\in C}\left(w_j\right)^2 Var\left(Y_j^C\right) \qquad (2) \end{aligned}$$

Where T *= Treated,* C *= Control (untreated), and weights* $w_j = \sum_i w_{ij}$

Sensitivity Analysis

Considering that propensity score matching only handles imbalances on *observed* characteristics, the present study further gauges the robustness of my results against unobserved confounders by following Harding's (2003) procedure, which is designed for situations in which both the outcome and treatment are binary. Suppose that there exists a latent confounder also measured dichotomously. The logic is to re-estimate the treatment effect in the form of odds ratio and its confidence interval for hypothetical combinations of the effects, expressed as odds ratios, of the confounder upon parental control and college attendance (See Appendix 3 for details). Such a sensitivity analysis is particularly

[20] Note that this two-way table has already accounted for pre-treatment covariates.

important for the present study, given the fact that the Add Health contains only limited measures of family socioeconomic status. Among other things, information on family wealth and assets, which may be a crucial factor for college attendance, is entirely absent.

RESULTS

Sample means of variables by ethnicity and parental nativity are displayed in Table 1.[21] As shown in this table, the Add Health data also evince the aforementioned Hispanic-Asian achievement puzzle. Among East Asians, chances of attending college are appreciably higher for children born to immigrant parents as opposed to native ones, [22] despite seemingly alike family socioeconomic status. The overwhelming majority (90%) of East Asian youth born to immigrants attend college, while the likelihood declines by nearly one third for those from native families. A smaller gap emerges when high school grades are considered. In contrast, Mexican youth as a whole suffer from low rates (<50%) of college attendance and relatively poor grades, regardless of parental nativity. Multivariate regression analyses further reveal that, other things being equal, children of Mexican immigrants indeed outperform their native-parentage co-ethnics in terms of college attendance (odds ratio=1.93, p<0.001) but not grades. [23] Yet immigrant family background is associated with not only higher grades (p<0.01), but also much higher chances of attending college (odds ratio=4.58, p<0.001) among East Asian youth.

[21] All tables presented here are based on the imputed data. Yet the means calculated for the original data are highly similar (not shown here).
[22] The difference is significant at the 0.001 level.
[23] Covariates include all family and children characteristics in Table 1 except parental control.

It can also be seen from Table 1 that Mexican immigrant parents are most controlling, while the other three groups appear to be nearly indistinguishable with respect to parental control. For all Mexican and East Asian groups, the majority of parents impose some rules on children's daily lives, such as weekend night curfew and restrictions on TV watching. Yet Mexican immigrant families are expected to have slightly higher levels of such controlling practices.

(Insert Table 1 here)

As mentioned above, one of the strengths of propensity score matching hinges on its ability to create comparable matches. Therefore, it is vital to ensure that matching on propensity scores achieves balance on the distributions of covariates predicting membership in the treatment group. As Dehejia and Wahba' s (1999) algorithm indicates, a simple additive probit model suffices to balance all covariates with respect to the first-order moment when the common support holds.[24] Overall measures of matching quality are presented in Table 2. For Mexican youth with immigrant parents, for instance, 210 *treated* cases (no parental control) find a match, such that only 420 (55%) out of 769 cases will be used for estimating treatment effect. Table 2 shows that pseudo-R^2 of the probit model fitted for the matched sample is as low as 0.002 relative to 0.086 before matching, a sign of overall balance on the covariates. The likelihood ratio test also offers supportive evidence that after matching covariates are no longer jointly significant in predicting the probability of treatment. Likewise, other three groups also see significant improvements in the balance on covariates after matching. Yet it is worth indicating that the improvement appears to be most remarkable for the group with the largest size,

[24] This is the case across all subgroups, leading to a universal model specification.

namely Mexican youth with immigrant parents. Examining balance on individual covariates further shows that the nearest-neighbor matching performs fairly well in removing differences in covariate means between treated and untreated groups (not presented here).[25] When covariate variance is taken into account, nonetheless, matching quality becomes less satisfactory. In particularly, matching on propensity score indeed augments the standardized bias for a few covariates in the two East Asian groups. [26]

(Insert Table 2 here)

Table 3 presents estimates of the treatment effect of *no* parental control on college attendance by ethnicity and parental nativity. For children of Mexican immigrants, those free of parental control (treated) appear to be somewhat more likely to attend college than are comparable children whose parents impose rules on their daily activities. However, the difference is far from significant. Native-parentage Mexican children are outstanding in that the probability of attending college appears to be completely independent of parental control.[27] As for East Asian children with native-born parents, lack of parental control is associated with a greater probability of college attendance (63%, vs. 53% for those experiencing at least some parental control), although this effect is statistically insignificant.

(Insert Table 3 here)

[25] The only exception is age for the group "Mexicans with native parents".
[26] Specifically, an increase of standardized bias occurs for sex of East Asian with immigrant parent(s) and public assistance for those with native-born parents. It is likely due to the relatively small sample sizes of these two groups (see relevant discussions below).
[27] Note that this "complete" independence may be an artifact caused by the sample being used. Yet the basic pattern should remain intact given that there is no significant parental control effect for this group.

Only for children of East Asian immigrants does parental control manifest a significant, albeit marginally, effect on the likelihood of college attendance. The average effect of treatment on the treated (ATT) equals 0.072 (t=1.89). Put in a counterfactual analysis fashion, chances of attending college are about 7 percent higher for these East Asian children now enjoying full daily autonomy than if their parents otherwise impose restrictions upon their daily activities, such as limited access to TV and weekend night curfew. It is worth noting that fully 96 percent of East Asian are expected to attend college. The treatment effect of (no) parental control can also be represented by two alternate measures, odds ratio (3.28, *p*=.046) and χ^2 (4.36, *p*=.037), which are calculated from the matched sample. I further fit a corresponding multivariate logistic regression using the unmatched sample in order to see to what extent matching on propensity score alters the results. The regression-based parental control effect turns to be slightly smaller (odds ratio=3.02) and statistically insignificant (*p*=0.138), implying a modest bias in comparison with the propensity score matching estimate.

Table 4 further presents a sensitivity analysis of the robustness of the parental control effect, expressed in terms of odds ratios (=3.28, *p*=. 046), found among children of East Asian immigrants. The table consists of re-estimated odds ratios, p-values (in parenthesis), and 95% confidence intervals under hypothetical scenarios of the effects of a dichotomous unobserved confounder on parental control (Γ) and college attendance (Δ) in terms of odds ratios.[28] As illustrated, the negative effect of parental control found among children of East Asian immigrants tends to be fairly robust when the marginal effects of the confounder are modest (Γ<2, Δ<2). Yet the bias is generally expected to

[28] Note that although the odds ratio (=3.28) should remain unchanged if the confounder either does not affect parental control (Γ =1.0) or does not affect college attendance (Δ =1.0), cells in the first row and first column are not identical due to rounding and possibly small sample size.

increase as the effects of the confounder on parental control and college attendance turn more significant jointly. When the confounder equally and strongly affects both parental control and college attendance, with an odds ratio of 4 as shown in the last cell, for example, the "true" effect of parental control will be as low as 1.97 and no longer statistically significant (p=. 277). Yet the estimates presented in Table 4 ought to be interpreted with extra cautions, given that the sample size is very small (n=194).

(Insert Table 4 here)

DISCUSSION

Educators and scholars have long been concerned with the underachievement of Hispanic students in the United States, nearly half of them Mexican Americans (Livingston and Kahn 2002; Mare 1995; Martinez, DeGarmo, and Eddy 2004). In view of similar immigrant roots, Asian Americans seem to be a model for academic success that is readily available to follow. Yet our knowledge about the success story of Asian Americans remains mixed. At the core of many debates lies the question whether the "Asian children achievement myth" is simply a product of socioeconomic advantages possessed by Asian immigrant families. From a policy-making perspective, it may bear significant implications if, indeed, Asian immigrants manage to overcome socioeconomic handicaps through certain parenting practices to help their offspring do well in school.

The present study investigates causal effects of one type of parental agency, control over children's daily behaviors, on the likelihood of college attendance among American youth of East Asian and Mexican descent. Consistent with prior studies based on NELS (e.g. Glick and White 2004), the Add Health data also evidence an advantage from having immigrant parents in terms of college attendance, with the advantage being more salient among East Asian youth.

Yet propensity score matching estimates suggest that this phenomenon is unlikely to be driven by differences in the experiences in parental control across groups. Imposing restrictive rules on children's daily lives is expected to have no positive effect at all, and, indeed, seems to be harmful to the educational attainment East Asian youth from immigrant families. Hence, my finding conflicts with previous studies such as Kao (2004). It is worth reemphasizing the distinctiveness of my study compared with most prior ones, that is, the present study targets two culturally distinctive ethnics as opposed to broad, loosely defined groups such as Hispanics and Asian. Part of the discrepancy in findings may result from the fact that Mexican and East Asian Americans are different from other Hispanic and Asian groups with respect to family processes.

The implications of these results are at least twofold. On the one hand, even within supposedly distinctive immigrant families, controlling childrearing practices tend to lead to no positive, if not damaging, effects on children as far as participation in post-secondary education is concerned. It seems to echo the well-established negative influence of authoritarian upbringing on child wellbeing in the context of the broader U.S. population. It is also possible that parental control over children's daily behaviors no longer provides academic benefits when children become mature and conceivably more independent. Put differently, children living in immigrant families may respond to restrictive parenting differently at different developmental stages. In light of the specific design of the present study, it is more precise to state that the influence of controlling parenting practices during late adolescence tends to be limited or sometimes negative.

On the other hand, the results call into doubt the popular stereotype that Asian cultures are so "distinctive" that Asian children tend to respond favorably to authoritarian parents in terms of academic achievement. As demonstrated by this study, children of East Asian immigrants indeed stand as the only group who would be harmed academically by controlling parenting. It thus follows that East Asian youth's success in obtaining a college education derives from something other than presence of parental control. As Chao (1994) suggests, standard measures of parental control designed for American society may not be sufficient to capture cultural subtleties of child-rearing and parent-child relationship within Asian immigrant families. Future research needs to explore more culture-specific measures of parenting in order to decode the Asian success story. There is ethnographic evidence, for example, that Chinese parents tend to be highly involved in children's choice of college and vigorously seek assistance and information through social networks (Louie, 2001).

Yet the present study is subject to a few limitations which should be addressed in future work. First of all, it is important to point out that the propensity score matching estimates in this study rely on small samples. As Rubin (1997) suggests, propensity score matching estimator performs better with large data sets. When the sample size is small, "substantial imbalances of some covariates may be unavoidable despite subclassification using a sensibly estimated propensity score" (p.7). Tests of matching quality in this study demonstrate that, with small data sets in combination with a sizable number of predictors of propensity score, it will be still possible to achieve balance on all such pre-treatment characteristics with respect to the first-order moment of the distribution (e.g. mean). However, it is inevitably difficult to also achieve balance on the distribution of predictors with respect to higher-order moments (see discussion on standardized bias above). Despite all its theoretical elegance, the propensity score matching estimator thus becomes less desirable in the case of small data sets to the extent that the counterfactual framework underpinning it entails observationally comparable treated and untreated matches.

Secondly, this study adopts a dichotomous measure of parental control constructed from multiple indicators under the implicit assumption that there exists a uni-factor structure underlying these indicators. This *a priori* assumption may be flawed, however. Recent psychological literature has identified two intrinsically disparate types of parental control: psychological control and behavioral control. The former reflects a coercive and manipulative mentality, while behavioral control refers to parental monitoring and limit setting (Barber, Olsen, and Shagle 1994; Silk et al. 2003; Steinberg 1990; Steinberg et al. 1992). Accordingly, a distinction is deliberately made between behavioral control and lack of autonomy granting. Whereas lack of autonomy reflects parental "exclusion of children from outside influences and opportunities," behavioral control aims at "socialization and behavioral regulations" (p. 116). In view of this body of psychological literature, a refined measure of parental control entails initially determining the latent structure of available measures by use of, for example, confirmatory factor analysis (see e.g. McNeal 1999).

Finally, neighborhood characteristics and peer influences are not explicitly incorporated in the current analysis. As discussed above, controlling parenting practices could be deliberate response to vicious neighborhood and negative peer influences. For future research, one way to account for the interplay between such contextual factors and parental control is to stratify the sample by contextual characteristics (e.g. crime rates), allowing parental control effects to vary by contexts differentiated by such characteristics.

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Table 1. Sample Means of Variables (Imputed)

	Mexican		East Asian	
Variable	Immigrant Parent (58.8 %)	Native Parents (41.2 %)	Immigrant Parent (65.0 %)	Native Parents (35.0%)
College Attendance	46.9 %	45.1 %	90.0 %	64.3 %
Parental Control (vs. No Control) *	71.4 %	63.6 %	63.2 %	64.3 %
Family Poverty Status*				
Income-to-needs ratio <1	30.7 %	16.7 %	7.9 %	7.0 %
1-3.9	65.5 %	69.20 %	14.1 %	11.5 %
4+	3.8 %	14.1 %	33.3 %	43.3 %
Parental Education				
Less than high school	76.2 %	34.0 %	12.7 %	1.9 %
High School/GED	10.5 %	25.6 %	22.7 %	24.8 %
Some College	10.5 %	28.2 %	21.0 %	33.8 %
College Degree	2.7 %	12.2 %	43.6 %	39.5 %
Public Assistance*	31.5 %	33.2 %	16.2 %	20.4 %
Parental Aspiration (1-5)*	4.1	3.9	4.5	4.1
Intact Family	83.2 %	57.5 %	89.7 %	70.7 %
Sex (=male)	50.3 %	49.0 %	54.6 %	51.6 %
Age (at least 18 years old)	23.4 %	18.2 %	14.8 %	12.1 %
Average Grade (0-4)*	2.6	2.6	3.2	2.9
AHPVT score*	89.9	95.9	104.8	102.4
N	769	539	291	157

*Imputed

Table 2. Overall Quality of Propensity Score Matching

			Pseudo R^2		LR (χ^2)			
	# Matched Pairs	% Matched	Before Matching	After Matching	Before Matching	$p > \chi^2$	After Matching	$p > \chi^2$
Mexican, IP (n=769)	210	0.55	0.086	0.002	79.2	0.000	1.0	1.000
Mexican, NP (n=539)	188	0.70	0.099	0.027	70.2	0.000	14.2	0.288
East Asian, IP (n=291)	97	0.67	0.124	0.021	47.3	0.000	5.6	0.935
East Asian, NP (n=157)	54	0.69	0.121	0.033	24.8	0.016	5.0	0.931

NOTE: IP – Immigrant parent(s), NP – Native-born Parents

Table 3. Effects of Treatment (No Parental Control) on College Attendance by Parental Nativity, Mexican and East Asian

	# Matched Pairs	% College Attendance No Control (Treated)	% College Attendance Parental Control (Untreated)	χ^2	*Odds ratio*	*ATT*	*t*
Mexican, IP (n=769)	210	49.1	43.8	1.16 (0.282)	1.23 (0.282)	0.052	1.08
Mexican, NP (n=539)	188	43.1	43.1	0.00	1.00	0.000	0.00
East Asian, IP (n=291)	97	95.9	88.7	4.36 (0.037)	3.28 (0.046)	0.072	1.89
East Asian, NP (n=157)	54	63.0	55.6	0.61 (0.433)	1.36 (0.434)	0.074	0.78

NOTE: IP – Immigrant parent(s), NP – Native-born Parents
ATT – Average Effect of Treatment on the Treated

Table 4. Robustness of Parental Control Effects on College Attendance in the Matched Sample, East Asian Children with Immigrant Parent(s)

	Δ=1.0	Δ=1.2	Δ=1.8	Δ=2.0	Δ=4.0
Γ=1.0	3.28 (0.046)	3.28 (0.047)	3.30 (0.046)	3.30 (0.046)	3.29 (0.048)
	(1.019, 10.552)	(1.018, 10.560)	(1.019, 10.690)	(1.019, 10.690)	(1.011, 10.712)
Γ=1.2	3.29 (0.046)	3.25 (0.049)	3.21 (0.052)	3.19 (0.053)	3.09 (0.062)
	(1.020, 10.585)	(1.008, 10.470)	(.990, 10.321)	(.986, 10.330)	(.944, 10.091)
Γ=1.8	3.24 (0.051)	3.22 (0.052)	3.10 (0.060)	2.94 (0.074)	2.70 (0.102)
	(.996, 10.519)	(.990, 10.449)	(.952, 10.070)	(.901, 9.604)	(.822, 8.865)
Γ=2.0	3.24 (0.059)	3.08 (0.062)	2.91 (0.078)	2.91 (0.078)	2.63 (0.112)
	(.995, 10.569)	(.944, 10.051)	(.889, 9.530)	(.889, 9.530)	(.799, 8.661)
Γ=4.0	3.41 (0.050)	3.36 (0.052)	2.75 (0.105)	2.75 (0.105)	1.97 (0.277)
	(.999, 11.653)	(.989, 11.439)	(.809, 9.368)	(.809, 9.368)	(.580, 6.694)

NOTE: *Γ* - effect of unobserved confounder on parental control (in odds ratio)
Δ – effect of unobserved confounder on college attendance (in odds ratio)

APPENDIX 1: *Measurement of Family and Child Characteristics*

Family and child characteristics are assessed at the first survey wave (1995), ensuring that they are all antecedent to the treatment--(removal of) parental control. I select an array of important family SES measures, including parental education, poverty status, and receipt of public assistance. Parental education is captured by the highest of parents' educational attainment (less than high school, high school diploma/GED, some college, or college degree(s)). I determine families' poverty status by an income-to-needs ratio, i.e. total family income relative to the official U.S. government poverty threshold for a given family composition (US Bureau of the Census, 1996; see also Sweeney and Wang 2004). An indicator of receiving public assistance is coded 1 if the family received any form of public assistance (e.g. SSI, AFDC, and food stamps) during the past month and 0 otherwise. I also control for family structure using intact family status (two parents vs. not). Finally, level of parental aspirations toward child's college education, a well-established factor for academic achievement, is also included as a predictor of treatment.

Child's characteristics consist of gender, maturity, previous academic performance, and intelligence. Specifically, the child is considered mature if he or she was at least 18 years old at the time of the baseline interview. I measure child's prior academic performance using last year's average grade over math, English, science, and social sciences.[29] It is worth noting that, conceivably, parents may switch to a more restrictive parenting style by increasing control over children if the latter are not performing well in school (Muller 1998). Finally, the score on the Add Health Peabody Vocabulary Test (AHPVT) serves as proxy for child's intelligence.

[29] Grades on each subject were initially coded using a 4-point scheme, ranging from 1 to 4 with 1 standing for "D & F", 2 for "C", 3 for "B", and 4 for "A".

APPENDIX 2: *Imputation on Missing Values of Covariates*

Table A1 shows covariates on which at least one percent of the East Asian or Mexican sample are missing. Approximately one quarter of the cases are missing on the treatment variable for both samples largely because of sample attribution by the time of the first follow-up survey (1996), while high levels of missing data on family SES measures, such as poverty status and receipt of public assistance, result from relatively low response rates of relevant survey questions at the baseline interview.

Table A1. Missingness on Covariates by Ethnicity

	Percent Missing (%)	
Variable	Mexican	East Asian
Parental control (treatment)	24.6	23.7
Family Poverty Status	37.2	38.8
Parental Education	20.4	29.5
Parental Aspiration	2.5	1.6
Public Assistance	22.5	31.0
Average Grade	3.8	2.0
AHPVT score	6.1	8.0
N	1,308	448

Missing values on such variables are imputed simultaneously through an iterative algorithm termed "switching regression" (Royston 2004; van Buuren et al. 1999). The algorithm first initializes all variables by replacing each missing value with a random draw from the observed values. Variables are then imputed in turn, with a specific univariate imputation procedure being applied to each variable. Each such univariate imputation procedure implements a regression model appropriate to the relevant variable as the response variable. If, for example, parental education (categorical) is being imputed, an ordered logit model will be applied with predictors being all other covariates (not limited to the ones with missing values) [30] and the outcome variable, in this case college attendance. It is worth emphasizing that the imputation accommodates randomness in estimating the regression coefficients by making random draws from the posterior distribution of the residual standard deviation. As Allison (2004) points out, introducing additional variation through random draws from the posterior distribution of parameters can significantly improve the imputation quality, especially with small sample sizes or substantial missing data. In particular, inclusion of the outcome variable as a predictor in imputing covariates is justified by this randomness to the extent that, indeed, dependent variable is essential for getting unbiased estimates of the regression coefficients (Allison 2004; Paul et al. 2003).

Once all variables with missing values are imputed, this switching regression process will be iterated multiple times for estimates to converge. The present study sets the number of iteration to 20. [31] Note that this iterative procedure as a whole (e.g. a 20-iterationn one) can further be repeated to attain multiple imputed datasets, which leads to *multiple imputation* in Allison's (2004) sense. Yet the goal of imputation for the current analysis is restricted to creating one single "complete" dataset by implementing the switching regression algorithm only once.

[30] It is also allowed to incorporate additional covariates that are considered to be strong predictors.
[31] As Royston (2004) illustrates, 5 iterations or so may indeed suffice.

APPENDIX 3: *Sensitivity Analysis*

When both outcome and treatment are measured dichotomously, the matched sample can be represented by a two-way table analogous to Table A2, which implies an association between college attendance and parental control in the form of odds ratio ([B/(A+B)]/[D/(C+D)]). Expressing the association as odds ratio not only provides an alternative estimate of treatment effect, but also allows one to assess the robustness of propensity score matching estimates in the presence of unobserved characteristics, which affect both the outcome and treatment. For the current study, I follow Harding's (2003) procedure to examine "how the point estimate and its confidence interval change under the presence of an unobserved covariate" (Harding 2003, pp.691-2; see also Rosenbaum and Rubin 1983). Specifically, this sensitivity analysis re-estimates the treatment effect for alternative combinations of hypothetical effects, expressed as odds ratio, of the unobserved characteristic on parental control and college attendance (pp.692-4).

Table A2: Observed Table for Matched Cases

	No College	College
No Treatment (Parental control)	A	B
Treatment (No control)	C	D

Suppose that the unobserved characteristic is also measured dichotomously. With this simplistic assumption, the effects of an unobserved confounder (U) on parental control (treatment) and college attendance (outcome) will be given by two latent tables (see Table A3-1), where n_1 - n_8 are unknown parameters as counts yet to be determined.

Nevertheless, these two latent tables cannot be observed. Instead, we will only be able to observe the aggregate table (Table A3-2), which is equivalent to Table A2.

Table A3-1: Latent Tables (w/ unobservable covariate U)

		No College	College
U=0	Control	n_1	n_2
	Treatment	n_3	n_4

		No College	College
U=1	Control	n_5	n_6
	Treatment	n_7	n_8

Table A3-2: Observed Table for Matched Cases

	No College	College
Control	$n_1+n_5=A$	$n_2+n_6=B$
Treatment	$n_3+n_7=C$	$n_4+n_8=D$

I denote the effect of the unobserved variable, *U*, on the treatment (parental control) as Γ and the effect on college attendance as Δ, both of which are expressed as odds ratio. With a further assumption that the three-way interaction between parental control, college attendance, and *U* is zero, we can determine all the unknown parameters (n_1 - n_8) under different scenarios of Γ and Δ by solving the system of equations as follows. [32] It is worth noting that Equation (9) is indeed redundant since only 8 parameters need to be determined for this system.

$$n_1 + n_5 = A \qquad (1)$$
$$n_2 + n_6 = B \qquad (2)$$
$$n_3 + n_7 = C \qquad (3)$$
$$n_4 + n_8 = D \qquad (4)$$
$$n_1 + n_2 + n_3 + n_4 = n_5 + n_6 + n_7 + n_8 \qquad (5)$$
$$(\ln n_1 + \ln n_6) - (\ln n_2 + \ln n_5) = \ln \Delta \qquad (6)$$
$$(\ln n_3 + \ln n_8) - (\ln n_4 + \ln n_7) = \ln \Delta \qquad (7)$$
$$(\ln n_1 + \ln n_7) - (\ln n_3 + \ln n_5) = \ln \Gamma \qquad (8)$$
$$(\ln n_2 + \ln n_8) - (\ln n_6 + \ln n_4) = \ln \Gamma \qquad (9)*$$

NOTE: *Γ* - effect of unobserved confounder on parental control (in odds ratio)
Δ – effect of unobserved confounder on college attendance (in odds ratio)

Once the counts n_1 - n_8 in Table A3-1 are all known, a new, adjusted treatment effect can be obtained by fitting a logistic regression based such grouped data. One of the advantages of this approach, as Harding (2003) indicates, lies in its ability to calculate both adjusted treatment effects and corresponding confidence intervals.

[32] This can be done using such software as Mathematica and Matlab.