THE LONG-RUN IMPACTS OF MEXICAN-AMERICAN SCHOOL DESEGREGATION

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100 Word Abstract

We present the first quantitative analysis of the impact of ending *de jure* segregation of Mexican-American school children in the United States by examining the effects of the 1947 *Mendez v. Westminster* court decision on long-run educational attainment for Hispanics and non-Hispanic whites in California. Our identification strategy relies on comparing individuals across California counties that vary in their likelihood of segregating and across birth cohorts that vary in their exposure to the *Mendez* court ruling based on school start age. Results point to a significant increase in educational attainment for Hispanics who were fully exposed to school desegregation.

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> ***"In response to the demand, a small frame building was erected behind the main elementary school building. Here all the Mexican and Mexican-American children were housed."***
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> --Elis Tipton, Principal of the San Dimas Elementary School during the 1940s. (As cited in Hendrick, 1975, p. 207)

I. Introduction

The rise in majority-minority communities driven by demographic trends and residential segregation have again placed public attention on the issue of school segregation, with a new focus on Latinx groups who are now among the most segregated in states like California (Orfield and Ee 2014).[1] Although the driving force of current school segregation outcomes may be regarded as *de facto* in nature, much can be learned from the impacts of eliminating *de jure* segregation, as with the extensive studies of the impact of court-ordered desegregation in the U.S. South on the education of African-Americans (Guryan 2004; Johnson 2011).[2] At the same time, the literature has paid relatively little attention to Mexican segregation, despite historical accounts which point to widespread segregation of Mexican-Americans throughout the southwestern United States in the early to mid-20th century, and the local establishment of so-called "Mexican schools" (Wollenberg 1976).[3] Our paper provides the first quantitative analysis of the impact of ending *de jure* segregation of Mexican-American school children by examining the effects of the 1947 *Mendez v. Westminster* decision on long-run educational attainment of Hispanics in California.

The *Mendez* decision, which originated as a class-action suit challenging Mexican school segregation by parents of Mexican children in Orange County, California, is often described as an important precursor to the landmark 1954 case, *Brown v. Board of Education* (Meraji 2014; Kandil 2016). Indeed, the *Mendez* decision was the first successful constitutional challenge to segregation in federal courts on the basis that segregation of Mexican students violated the equal protection clause of the 14th Amendment, an argument

[1] Logan and Parman (2017) trace the dramatic rise in residential segregation across the U.S. .

[2] We use the terms African-Americans and Blacks interchangeably throughout as well as the terms Latinx, Latino, and Hispanic interchangeably throughout

[3] We use the terms Mexican-American and Mexican interchangeably to refer to individuals of Mexican ethnic origin, regardless of citizenship.

that would reemerge in the *Brown* case several years later (Valencia 2005). However, just as prior studies have suggested that the full legal impacts of *Brown* were not felt until 10 years later (Reber 2005), the question of whether the end of *de jure* Mexican segregation brought about by *Mendez* ultimately led to improvements in educational outcomes for Mexican-Americans is an empirical one which we investigate here.

One important difference between examining the impact of desegregation of Mexican children in California also suggests why the subject has been underexamined in the literature. Unlike some states in the American South, where official documents differentiate between schools for Black and white children, segregation policy in California was primarily decided at the local level, and though widespread in certain areas, official documentation of the extent and geographic patterns of segregation are limited in comparison. Nevertheless, historical sources suggest that segregated schools were prevalent in areas with relatively high shares of Hispanic populations (Wollenberg 1976; Bowman 2001). Thus, we leverage an intent-to-treat difference-in-differences research design that compares counties with a high Hispanic to non-Hispanic population ratio in 1940 to counties with a relatively low ratio, and across birth cohorts that started school after *Mendez* relative to birth cohorts that started school before the *Mendez* ruling. Analogous to those studies which have examined the impacts of school desegregation for African-Americans (see for example, Guryan 2004; Johnson 2011; Reber 2010; Card and Kruger 1992; Ashenfelter, Collins, and Yoon 2006), we examine educational attainment (completed schooling) outcomes, but our primary focus is the Hispanic population. To provide further weight to our identification strategy, we check for parallel trends prior to treatment impacts by way of a placebo experiment on the birth cohorts who should have been unaffected by *Mendez* based on their school start age. We also trace out impacts by birth year across counties well before and after the *Mendez* ruling to see the full extent of desegregation impacts on cohorts across treated and comparison areas based on school start age.

Our results suggest that the impact of school desegregation in 1947 was significant, leading to an increase of almost 0.9 years of schooling for Hispanics in the cohorts who started school after *Mendez* relative to cohorts born 10 years prior. As the latter group may have been partially affected by *Mendez*, we also estimate impacts relative to an older cohort, born 10-20 years prior, and find even larger effects on the order of 1.9 years of schooling,

which can be compared with an average of 9.2 years for the oldest birth cohorts. Finally, we examine impacts on non-Hispanic whites using the same identification strategy and find evidence of a decline in educational attainment of non-Hispanic whites in birth cohorts that began school after *Mendez* in counties that were more likely to be segregated, relative to those cohorts who began school prior to *Mendez* in counties that were less likely to be segregated. Given that these changes occurred relatively quickly, these facts point to the likelihood of a movement toward a more equitable resource distribution within counties across students from different ethnic groups following the end of *de jure* segregation.[4] However, in the absence of data on school resources explicitly designated as "Mexican" or "white" within counties, we cannot decisively pin down the mechanism.

The remainder of this paper proceeds as follows. Section II provides background on the history of Mexican-American segregation in the Southwest and the *Mendez* court decision. Section III summarizes the data set used and Section IV describes in detail the empirical strategy. Section V presents the results and Section VI concludes with a discussion of potential mechanisms and avenues for future research.

II. Background: Mexican School Segregation and the *Mendez v. Westminster* Decision

The roots of segregation in the American Southwest can be traced back as far as the mid-nineteenth century when the territory was annexed by the United States at the close of the Mexican-American war (Valencia 2005).[5] However, the rise of segregated schools in the region is typically linked to the increased waves of Mexican migration to the American Southwest in the early 20th century and the coinciding rise in prejudice toward individuals of Mexican origin (Wollenberg 1976). By 1927, nearly 10 percent of the state's school children were of Mexican descent, and in some counties, they were closer to 20 or 30 percent of the total school population (Wollenberg 1976, p. 110-11). As for African-Americans in the

[4] School resources have been an important avenue of exploration in the literature on segregation in the South and its impact on African-American educational outcomes (Carruthers and Wannamaker 2017, Margo 1990).

[5] While California was once governed by Mexico, Mexicans who remained there at the conclusion of the Mexican-American war were granted full U.S. citizenship under the Treaty of Guadalupe Hidalgo (1848, Articles VIII and IX).

U.S. South, an underlying pattern of residential segregation emerged that was often codified into law and reinforced through violence and harassment (Donato, Menchaca, and Valencia 1991; Cook, Logan, and Parman 2017). School boundaries were often drawn so that Mexican children could only attend the local Mexican school, and white children were granted transfers out of the Mexican school zone, thus solidifying segregation in the schools (Gonzalez 1985).

However, it is important to note that segregation in Mexican schools was never universally implemented in the same way throughout California. The practice was not part of the state's education code and thus its implementation varied at the local level with some school districts choosing not to separate children, or segregating only younger children (Wollenberg 1976). This variation in treatment status and the failure to document the extent of segregation on a large scale,[6] make economic analysis of the impact of Mexican desegregation challenging and thus necessitate the use of the intent-to-treat research design we pursue here.[7] Nevertheless, by 1931, 85 percent of California schools reported segregating Mexican students at the classroom or school level (Hendrick 1977, as cited in Donato, Menchaca, and Valencia 1991), thus underscoring the fact that segregation was widespread throughout California.

The justification for segregation was often rooted in racial discrimination which was sometimes spelled out explicitly, as with those who wished to characterize Mexicans as Indians, a group which could be legally segregated at the time (Hendrick 1977, as cited in Donato, Menchaca, and Valencia 1991). Similar justifications were made on the basis of cleanliness, intellectual ability, and the otherwise inferiority of Mexicans who were deemed to require special instruction to acculturate to American standards (Carter 1970). Economic interests also played a role in segregation as employers resisted offering education that would jeopardize their primary source of cheap labor (Gonzalez 1985). One of the more salient legal justifications was that language deficiencies on the part of Mexican children required segregation to improve their English language skills, despite the fact that Mexican-

[6] Indeed, Hendrick (1975, p. 213) describes how Los Angeles school officials explicitly stopped recording the race of students and employees.

[7] This marks an important distinction between segregation of African-Americans in the South and the Mexican experience in the Southwest.

American children who did not speak Spanish were also segregated (Donato, Menchaca, and Valencia 1991). Nevertheless, the underlying motivation was clear to the extent that segregation in schools mirrored the society at large where barriers commonly existed to separate Mexicans and whites in facilities such as parks, movie theaters, and swimming pools (Arriola 1995; Kandil 2016).

In this context, the struggle to desegregate schools began in the 1930s in Texas and California when the legality of segregating Mexican school children began to be challenged in the courts to varying success at the local level (for a review see Valencia 2005). In 1945, Gonzalo Mendez and four other Mexican-American parents sued four Orange County school districts in federal court on behalf of their children and 5,000 other children of "Mexican and Latin descent," arguing that segregation violated their constitutional rights (Valencia 2005; *Mendez* 1947). Unlike prior suits, *Mendez* gained significant attention for being the first to claim that the doctrine of "separate but equal" violated the equal protection clause of the 14th Amendment (Gonzalez 1990)—an argument that would resurface in the 1954 *Brown* decision.

However, there are reasons to believe the impact of *Mendez* stopped short of the legal precedent later to be set by *Brown*, and thus might have been more muted. First, an injunction against Mexican segregation was issued in a Federal District court and while the defendants initially resisted its implementation, the ruling was ultimately upheld at the Ninth U.S. Circuit Court of Appeals in 1947 (Gonzalez 1985). The case never reached the Supreme Court because the school district declined to appeal, thus leading some to believe that the ruling might only be locally relevant (Arriola 1995). Second, segregation was ultimately overturned in *Mendez* based on its opposition to existing California law (*Westminster* 1947), rather than a direct rejection of the doctrine of "separate but equal" espoused in *Plessy v. Ferguson* (Valencia 2005). Nevertheless, the controversy highlighted in *Mendez* led legislators to swiftly repeal any remaining school segregation laws in 1947 (Wollenberg 1976), with the support of Governor Earl Warren, who would later serve as Supreme Court justice during the *Brown* decision.[8] Thus, it is reasonable to believe that, in

[8] The participation of the National Association for the Advancement of Colored People (NAACP) and Thurgood Marshall, who helped argued against the legality of segregation in the *Mendez* case, also mark important linkages with

the wake of the *Mendez* decision, California school officials would have understood segregation to be in violation of the law and historical surveys taken at the time suggest that integration efforts were immediately put into place (Gonzalez 1985; Wollenberg 1976).

III. Data Source and Descriptive Statistics

The main data set used in this analysis comes from the five percent Public Use Samples of the 1980, 1990, and 2000 U.S. decennial censuses as made available through IPUMS (Ruggles, et al. 2015). Throughout portions of the analysis, we consider Hispanic and non-Hispanic white individuals who were born in and reside in California, with birth years spanning 1921-1945, where Hispanics are individuals who identify as Hispanic/Latino, regardless of race. Non-Hispanic whites are individuals who identify as white on the race question, but not as Hispanic/Latino.[9] Note that our data restrictions imply that, at the time they are surveyed, all individuals in our sample are well beyond the age when they would likely have completed schooling. Also note that we include a broader classification of Hispanics, rather than restricting attention exclusively to Mexican-Americans, because it is believed that, in general, all individuals identifying as Hispanic would have been treated similarly to Mexican-Americans in the era of segregation.[10] Moreover, focusing on Hispanics born in California in the 1920s through 1940s, as we do here, necessarily excludes immigrants from Central and South America that arrived in the U.S. in the latter half of the 20th century. Thus, it is reasonable to expect that our Hispanic sample consists primarily of individuals of Mexican descent.[11]

To identify likely segregated areas, we use data from the complete count 1940 U.S. Census as made available through the Integrated Public Use Microdata Series (Ruggles, et al.

Brown. Marshall would later serve as counsel for the African-American plaintiffs in *Brown* before the Supreme Court (Valencia 2005).

[9] Individuals identifying with other racial groups are dropped from the sample. Unfortunately, the group of non-Hispanic Blacks in the sample is too small for meaningful analysis.

[10] Indeed, the plaintiffs in *Mendez* asserted that segregated schools were attended by children of "Mexican and Latin descent" (*Mendez* 1947). Focusing the empirical investigation on the larger group of Hispanics also avoids the issue of whether Mexican-Americans would identify themselves as Mexican/Mexican-American or Hispanic, *per se*. The main results are qualitatively similar if we restrict the sample to Mexican-Americans. These results are available upon request.

[11] Even today, Mexican/Mexican-Americans are overwhelmingly the major demographic origin group within the set of individuals identifying as Hispanic (Motel and Patten 2012).

2015).[12] Since there are no official records of which areas were separating Mexican and Mexican-American students into separate schools and classrooms, we leverage historical accounts which indicate segregation was most prevalent in counties with a high share of Hispanics in the population (Wollenberg 1976, Bowman 2001). To make these notions concrete, we calculate the Hispanic to non-Hispanic population ratio by county for all California counties in 1940 and define counties as having a high likelihood of being segregated (high segregation, henceforth *HiSeg*) if they are above the 75th percentile of this distribution (approximately 0.09). For comparison, we define counties as having a low likelihood of being segregated (low segregation, henceforth *LoSeg)* if they are below the 25th percentile in the same distribution (about 0.02)[13], and drop counties which lie between the 25th and 75th percentiles from the analysis.[14] Note that our definitions are consistent with historical evidence on segregation: Orange County, from which the *Mendez v. Westminster* case originated, is in the *HiSeg* group, as well as the counties associated with those towns having segregated schools which are cited in historical sources (Bowman 2001).[15] Our own calculations from tabulations of the California State Department of Education (1932) indicate that the percent of registered minors who were classified as Mexican was also significantly higher in our main sample of *HiSeg* versus *LoSeg* counties (17.58% and 0.95% on average, respectively), which is consistent with our classification.

As a first step of descriptive analysis, Figure 1 traces out average educational attainment by birth year for Hispanics and non-Hispanic whites in *HiSeg* and *LoSeg* counties. Several interesting points are illuminated from the figure, to be investigated in regression

[12] For robustness, we have also used the 1930 Census to define our high- and low-likelihood segregation counties. The advantage of this approach is that "Mexican" was listed as a race category in 1930, however, we prefer to use the 1940 Census because it is the closest to the period prior to *Mendez*. Event study analyses using the 1930 Census categories are available upon request (Appendix Figures A3 through A8).

[13] For robustness, we have also used the upper and lower thirds of the distribution as the thresholds defining high- and low-likelihood segregation counties and have found similar results which are available upon request (Appendix Tables A3 and A4).

[14] For the main analytical sample, high segregation (*HiSeg*) counties include: Fresno, Kings, Madera, Merced, Monterey, Orange, Riverside, San Bernadino, San Luis Obispo, Santa Barbara, Tulare, Ventura, and Yolo. Low segregation (*LoSeg*) counties include: Butte, El Dorado, Humboldt, Shasta, and Sonoma. More *HiSeg* counties are identified than *LoSeg* counties because the empirical strategy uses the complete count 1940 Census to make this distinction, but *LoSeg* counties are less likely to be named in the public use 1980-2000 Censuses.

[15] Two notable exceptions are Los Angeles and San Diego counties where the Hispanic-to-non-Hispanic population ratios fall just below the *HiSeg* level, and are thus dropped from the main analysis here, for consistency with the empirical method. This suggests that our main point estimates are likely to be conservative, relative to what we would obtain if we were able to measure segregation directly.

analysis below. First, the dramatic rise in the educational attainment of Hispanics in *HiSeg* counties can be observed for students who were school age or younger at the time of the *Mendez* decision. At the same time, for older Hispanics in *HiSeg* counties, who were already in their 20s at the time of *Mendez*, educational attainment hovers around 9 years of education on average. In comparison, Hispanics in *LoSeg* counties have educational attainment closer to 12 years on average, and hovers around that level for all cohorts, before and after *Mendez*. This constitutes suggestive evidence that Hispanics in segregated counties were the main beneficiaries of the *Mendez* decision and, though these are only summary statistics, also provide some suggestive support for the parallel trends assumption behind the intent-to-treat difference-in-differences research design, to be assessed more rigorously below.

Figure 1 also shows that non-Hispanic whites have higher educational attainment than Hispanics in both the segregated and non-segregated counties for all cohorts. But the gap between non-Hispanic whites and Hispanics from older cohorts is substantially larger in *HiSeg* counties compared with the analogous gap in *LoSeg* counties. Moreover, while Hispanics in *LoSeg* counties have substantially higher years of education than Hispanics in *HiSeg* counties, this pattern is reversed for non-Hispanic whites, where the highest educational attainment can be seen in *HiSeg* counties, for all except the cohorts that were fully exposed to desegregation. These details provide suggestive support for the hypothesis that older Hispanics in *HiSeg* counties were profoundly disserved by the educational system available to them, which would be consistent with a segregated school system that advantaged non-Hispanic whites in *HiSeg* counties over earlier periods.

It is also noteworthy that the gap between non-Hispanic whites in *HiSeg* versus *LoSeg* counties is much smaller than that between Hispanics in *HiSeg* and *LoSeg* counties. This would also suggest the existence of a more consistent school system in place for non-Hispanic whites across counties, and thus segregated schools for Hispanics in *HiSeg* counties. Finally, the gap between non-Hispanic whites in *HiSeg* and *LoSeg* counties is relatively small throughout the cohorts observed, with a small increase in educational attainment in *LoSeg* counties relative to *HiSeg* evident for the youngest cohorts who would have been fully exposed to *Mendez*. This suggests the possibility of an educational disadvantage to non-Hispanic whites in *HiSeg* counties toward the end of the period, and also with resources being shifted toward Hispanics and away from non-Hispanic whites in *HiSeg* counties after

Mendez. This will be explored in subsequent analysis where we investigate the impacts of desegregation on non-Hispanic whites as an extension below.

To make these differences concrete, Table 1 describes the main Hispanic sample and illustrates the difference-in-differences empirical strategy in summary statistics for the outcomes of interest, using the primary treatment and comparison groups. Consistent with the evidence from Figure 1, we see a large difference in completed education and likelihood of junior high and high school completion rates across *HiSeg* and *LoSeg* counties. For older cohorts (1931 through 1935 birth years) that began school prior to *Mendez*, they amount to 10.2 versus 12 years of schooling, respectively). The same difference is much smaller for younger cohorts (1941 through 1945 birth years) that began school after the *Mendez* ruling (11.7 versus 12.8 years of schooling, respectively). Thus, the difference-in-differences estimate using only these raw averages across groups, suggests a substantial impact of desegregation on Hispanics of 0.7 years of completed schooling, or about 7 percent, relative to the average in *HiSeg* counties among cohorts that began schooling prior to Mendez. Similar impacts are observed for the junior high school and high school completion outcomes, though only the difference-in-differences estimated impacts on years of schooling and junior high school completion outcomes are statistically significant at the 5% and 1% levels, respectively, in these summary statistics. The regression analysis will bolster this analysis by including additional controls for birth year and county fixed effects, as well as checking for robustness to pre-existing trends.

IV. Empirical Strategy

To analyze the effect of Mexican-American school desegregation on educational outcomes, we estimate the following regression model by Ordinary Least Squares:[16]

$$(1) \quad Y_{ict} = \alpha + \pi \cdot (HiSeg_c \cdot PostMendez_t) + X_{ict} \cdot \beta + \mu_c + \delta_t + \varepsilon_{ict}$$

[16] Note the empirical focus on Hispanics includes Mexican-Americans, as discussed in Section III.

where the dependent variable, Y_{ict}, is an educational outcome for individual *i* from county *c* born in year *t* and the sample is limited to men and women from the 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (above the 75% level for all 1940 counties) or very low (below the 25% level for all 1940 counties). $HiSeg_c$ is an indicator variable for Hispanic to non-Hispanic population ratio in county *c* being above the 75% level of all California counties based on the 1940 complete count U.S. Census. $PostMendez_t$ is a binary variable that is equal to 1 if individual *i* is born between the years 1941 to 1945 (ages 2 to 6 at the time of the *Mendez* decision, i.e., fully exposed to desegregation for all school years), and zero otherwise. In the main results, the sample includes 1941-1945 birth years as well as individuals born 10 years prior, i.e., 1931-1935 birth years (ages 12-16 at the time of *Mendez*), and thus the primary comparison group consists of individuals who likely completed elementary school prior to *Mendez*. Note that omitting the intervening birth years (1936-1940) who would have been partially treated in elementary years allows for a more precise comparison of the intervention, in the style of Duflo (2001). X_{ict} is a vector of control variables that includes an indicator for female, and indicators for 1990 and 2000 Census observations, respectively. Lastly, all regression specifications include county of residence fixed effects, μ_c, and birth year fixed effects, δ_t.[17] ε_{ict} is an i.i.d. error term, assumed to be independent within, but not necessarily across counties, so we cluster standard errors at the county level.

This difference-in-differences research design compares the educational attainment of younger Hispanic cohorts who were *fully treated* by the *Mendez* court ruling (cohorts born between 1941 and 1945) to individuals from older cohorts who started school prior to the *Mendez* ruling, based on their likelihood of attending segregated or non-segregated schools prior to the ruling (*HiSeg* versus *LoSeg* counties). One feature of the empirical approach is that it compares Hispanic cohorts that are relatively close in age (birth years 1941 through 1945 and birth years 1931 through 1935) and are thus likely to face similar circumstances aside from desegregation, allowing us to better isolate the impact of treatment. However, one concern with this strategy is that the comparison group might also be *partially treated*

[17] Note that the *Post Mendez* cohort dummy is collinear with the birth year fixed effects just as the *HiSeg* dummy is collinear with the county fixed effects and thus both are dropped from the model.

by the intervention, since the oldest among them would still be teenagers at the time of the *Mendez* ruling. Nevertheless, it is reasonable to expect that the impact on this comparison group would be smaller relative to the treatment group if one expects greater returns from earlier educational investments, and that the cost of switching schools would be greater to those who were already enrolled in school at the time of the *Mendez* decision. Thus, estimates using this comparison group likely represent a lower bound on the impact of the desegregation treatment. To obtain an upper bound on the impact of the treatment, subsequent analysis uses an older comparison group (birth years 1921 through 1930) that would likely have finished schooling at the time of the *Mendez* ruling, and thus would not have benefited from the desegregation treatment during their schooling years.

The identification strategy assumes that no other contemporaneous factors can explain the differences observed over this time period, after controlling for birth year fixed effects, which would account for shocks common to individuals across counties born in the same birth year (e.g., wars and national economic shocks), and county fixed effects, which would purge the estimates of time-invariant differences common to all individuals in a given county (e.g., poverty and ethnic attitudes). It also requires that individuals in the treatment (*HiSeg*) and comparison (*LoSeg*) groups would have maintained parallel trends in the absence of the treatment (desegregation around the time of *Mendez*). To bolster support for this assumption, as a robustness check, we run a placebo experiment where we replace the birth cohorts in the above analysis with cohorts who would all likely have completed schooling prior to *Mendez,* replacing the $PostMendez_t$ indicator in equation (1) with a placebo cohort indicator (birth years 1926-1930) and where the comparison group is a slightly older cohort (birth years 1921-1925).

To assess the evidence for pre-existing trends, we also trace out differences in educational outcomes for individuals from *HiSeg* and *LoSeg* counties across a range of birth years that includes individuals who would likely have completed schooling prior to Mendez. At the same time, these event studies allow us to investigate the plausibility of partial impacts for individuals who were already in school at the time of Mendez. These point estimates are noisy due to the smaller sample sizes in isolating treatment effects by birth year in our sample, but provide some indication of stable trends prior to *Mendez* as well as evidence for partial benefits accruing to Hispanics who were in school at the time of the

Mendez ruling. While our primary research question focuses on the impact of desegregation on Hispanic educational outcomes, as an additional extension, we also investigate the impacts of desegregation on non-Hispanic whites using the primary research design laid out above, but where the sample is limited to non-Hispanic whites.

V. Results and Discussion

Table 2 presents our results from estimating equation (1) on the Hispanic and alternative samples. As shown in column (1) of panel A, the difference-in-differences coefficient in the specification with years of education as the dependent variable is statistically significant at the 1% level and suggests that school desegregation increased years of education for Hispanics by 0.88 years. When compared with the average years of education for Hispanics in *HiSeg* counties prior to *Mendez* (10.23 years), this amount to an increase of about 8.6 percent. This relatively large increase suggests that improvements in schooling may have led to increases in certification, so the remaining columns (2) and (3) of panel A analyze whether Hispanics were more likely to complete junior high (at least 8 years of schooling) and high school (12 years of schooling or more). These difference-in-differences coefficients are also statistically significant at the 1% and 5% levels, respectively, and suggest an increase in the probability of junior high school completion of about 14.5 percentage points and high school completion of about 9.3 percentage points for Hispanics, or about 18.4 % and 19.4%, respectively, relative to their baseline levels of educational attainment prior to *Mendez*.[18]

The primary concern with our empirical strategy is that it relies on the assumption that Hispanics in *HiSeg* and *LoSeg* counties would have maintained parallel trends in the absence of the treatment. To bolster support for this assumption, panel B of Table 2 checks for pre-existing trends by testing whether Hispanics in *HiSeg* counties experienced any improvements in educational attainment relative to *LoSeg* counties in a sample of cohorts that completed schooling prior to *Mendez*—groups that should all be *too old* to benefit from schooling desegregation. Thus, the placebo treatment cohort in the analysis shown in panel

[18] Results estimated separately for Hispanic men and women are available upon request (Appendix Table A1) and show positive impacts for both groups.

B of Table 2 is a cohort that is slightly younger than the placebo comparison cohort, but where neither cohort should benefit from treatment. If our main results were purely driven by differential growth rates in educational attainment over time across both the *HiSeg* and *LoSeg* counties, then we should observe that the point estimates in the placebo analysis are also positive, otherwise this evidence casts doubt on the spurious interpretation of the main results. Indeed, what we observe is that there are no statistically significant differences in years of education (column 1 of panel B) and the junior high school outcome (column 2 of panel B), and only a marginally statistically significant difference in the high school outcome (column 3 of panel B), but where all coefficients are negative. This confirms that, after accounting for the controls in the regression model, there was no pre-existing positive growth trend in *HiSeg* counties relative to *LoSeg* counties prior to desegregation, and is thus consistent with the causal interpretation of the desegregation treatment shown in panel A.

Another way to validate the use of our identification strategy is to present a full event study analysis showing the difference-in-differences coefficient estimates for individual birth years, that is, replacing the $PostMendez_t$ indicator in equation (1) with a birth year indicator. This exercise can also be used to gauge whether there is evidence for partial treatment for cohorts who may have begun schooling prior to *Mendez* but who may not have completed schooling by the time of desegregation. In practice, this exercise is challenging due to small sample sizes at the single birth year level, however, we present the event study graph for the junior high school outcome in Figure 2.[19] Note also that this graph covers a large range of birth years, covering 1921-1945, where the reference category is the 1931 birth year (age 16 at the time of the *Mendez* decision). Since these graphs go from desegregated to segregated eras and consider both fully treated cohorts (i.e., began school after the *Mendez* decision), partially treated cohorts (i.e., in school during the *Mendez* decision), and untreated cohorts, it is perhaps unsurprising that we observe what appears to be three emergent areas of the graph. For the oldest cohorts, who were in their 20s at the time of *Mendez*, we see a relative disadvantage for Hispanics in *HiSeg* counties relative to *LoSeg* counties and this trend is relatively stable over time. For those cohorts who were in their teens (ages 12-16) at the time of *Mendez*, and who might be thought of as partially

[19] The years of education and high school outcome event study analyses are qualitatively similar and can be found in the Appendix (Figures A1 and A2).

treated by desegregation, we see a stable trend around zero relative to the reference category. The latter group is the comparison group in the main results (shown in Table 2), and is marked as "Old Group" in Figure 2. Finally, for those birth years that were fully exposed to desegregation, as a consequence of starting school after *Mendez* (our $PostMendez_t$ cohort in equation (1), noted as "Young Group" (ages 2-6) in Figure 2), we see a positive impact relative to the reference category. While confidence intervals are wide due to small sample sizes at the birth-year level, we view this event study graph as consistent with the overall research design in showing that the difference across *HiSeg* and *LoSeg* counties followed a stable pattern until the advent of desegregation. It also serves to highlight the likelihood of partial treatment exposure for cohorts in school at the time of the *Mendez* decision, and thus suggests that the estimates in Table 2 are likely to be a conservative lower bound estimate of the impact of *Mendez*.[20]

As an extension, we examine whether there are any spillover effects of desegregation using the same identification strategy and regression model in equation (1), but now limiting the sample to non-Hispanic whites.[21] These results, shown in Table 2 of panel C, suggest statistically significant decreases associated with desegregation for non-Hispanic whites in terms of educational attainment, junior high school completion, and high school completion (coefficient estimates -0.39, -0.01, -0.03, respectively), but point estimates are much smaller in magnitude relative to the positive impacts for Hispanics shown in panel A. This relative decline in educational outcomes for non-Hispanic whites in high segregated counties relative to non-Hispanic whites in low segregated counties after school desegregation, mirrors patterns seen in Figure 1 as discussed above. Moreover, these findings would be consistent with school resources being shifted from non-Hispanic whites to Hispanics after school desegregation in high segregated counties, but absent data on school resources within

[20] Event study graphs for Hispanic and non-Hispanic whites when *HiSeg* and *LoSeg* counties are defined using the 1930 Census are available upon request (Appendix A3 through A8) and show positive impacts of *Mendez* on Hispanics and negative impacts on non-Hispanic whites in *HiSeg* counties as well as evidence of stable trends prior to *Mendez*.

[21] Note that any degree of ethnic attrition, whereby individuals were classified as Hispanic at school-age, but no longer identify as Hispanic in later years, would likely yield an upward bias in the estimates for non-Hispanic whites, assuming those individuals would have responded to the desegregation treatment by increasing their educational attainment. This is a reasonable assumption, given evidence that Mexican-American ethnic attritors (classified by measures of ancestry, but not by self-identification) have higher educational attainment than the population that identifies as Mexican-American (Duncan and Trejo 2011). Thus, to the extent that we find negative estimates for the non-Hispanic white population, they are conservative as to the likely impact of desegregation on this group.

schools explicitly identified as Mexican or white-only, we cannot definitively say whether this is the mechanism driving the results from above. Nevertheless, the fact that estimated impacts for non-Hispanic whites are much smaller in magnitude than for Hispanics suggest that desegregating schools improved schooling for Hispanics with relatively small impacts on non-Hispanic whites.

Finally, to provide an upper bound on our estimates of the impact of school desegregation on Hispanics, we examine treatment impacts of school desegregation relative to the older cohorts who completed schooling prior to *Mendez* by estimating the regression model in equation (1) with an alternative comparison group (birth years 1921-1930). These results provide an upper bound on the impact of desegregation on the educational attainment of Hispanics which can be compared with the lower bound of treatment impacts found by using the partially treated comparison group presented in Table 2. The results of this alternative analysis are presented in Table 3 and show that, as expected, treatment impacts are much larger when we consider a comparison group that should not have been affected by *Mendez* at all. This amounts to 1.9 years of additional years of education, a 23 percentage point increase in the likelihood of completing junior high, and an 18 percentage point increase in the likelihood of competing high school. Since these can all be compared to even lower baseline averages for educational attainment in *HiSeg* counties for treated groups prior to treatment, they represent even larger effective increases in educational attainment for treated groups. Table 3 panels B and C present these results separately by gender and show statistically significant impacts of desegregation on both men and women. Thus, these estimates suggest similar impacts for men and women with regards to educational attainment, after taking into account that women in older cohorts have lower educational attainment than men, but suggest somewhat larger impacts on junior high and high school completion rates for women.[22]

[22] Extended results for non-Hispanic whites show negative impacts for men and women (Appendix Table A2), similar to the results in Table 2 panel C. Results using alternative definitions of *HiSeg* and *LoSeg* counties based on the 1930 Census population distribution are available upon request (Appendix Tables A3 and A4) and show impacts of similar magnitude and statistical significance.

VI. Conclusion

This paper provides the first quantitative analysis of the effect of Mexican-American school desegregation by examining the impact of the 1947 *Mendez v. Westminster* decision on long-run educational outcomes of Hispanics in California. While identifying segregated schools is challenging due to a widespread lack of official designations, by comparing individuals in counties which were more likely to have segregated schools with individuals in counties which were less likely to have segregated schools across birth cohorts that started school after *Mendez* relative to birth cohorts that started school prior to the ruling, we have been able to provide quantitative evidence on its impacts. In particular, the results here show that desegregation significantly improved the educational attainment of Mexican-Americans, with some evidence of partial benefits for students already in school at the time of the *Mendez* decision, and even larger effects for students who began school after *Mendez*. At the same time, similar analysis on non-Hispanic white groups suggests there may have been some negative impacts associated with school desegregation, which would be consistent with distributing resources more equitably across racial and ethnic groups after desegregation.

By providing the first quantitative estimates of desegregating Mexican-American schools, this work has made a significant contribution to the literature, but is limited in a number of ways. First, we are not able to directly tie individuals to schools, and most notably, schools and classrooms lack official designations as "Mexican/Mexican-American only" or "white only" even when this was the practice. Ideally, individual data would indicate the school of attendance over time and school quality measures such as teacher education, experience, wages, and student-teacher ratios would be available throughout the period for schools and classrooms that were initially classified as segregated or not. Unfortunately, lacking systematic historical data on these classifications and associated measures of resource allocation makes such investigations difficult, and require a more nuanced approach. Nevertheless, pinning down the mechanisms underlying the increase in educational attainment for Hispanics following desegregation is an important area for future research, in particular given the increased levels of segregation experienced by Latinx students today.

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Table 1: Sample Descriptive Statistics on Mexican-American Educational Attainment by Intent-to-Treat Status

	Panel A: Treatment Group, 1941-1945 Birth Cohorts (Ages 2-6 at Mendez Decision)			Panel B: Comparison Group, 1931-1935 Birth Cohorts (Ages 12-16 at Mendez Decision)			
	Segregation:			Segregation:			
	High	Low	$\Delta^{treatment}$	High	Low	$\Delta^{comparison}$	$\Delta^{treatment} - \Delta^{comparison}$
Years of Education	11.705	12.797	-1.092***	10.228	12.026	-1.798***	0.706**
	[2.808]	[2.530]	(0.217)	[3.736]	[2.248]	(0.267)	(0.344)
Jr. High (8+ years of education)	0.932	0.979	-0.047***	0.792	0.974	-0.182***	0.135***
	[0.251]	[0.144]	(0.013)	[0.406]	[0.160]	(0.020)	(0.024)
High School (12+ years of education)	0.673	0.811	-0.138***	0.482	0.701	-0.219***	0.081
	[0.469]	[0.393]	(0.034)	[0.500]	[0.461]	(0.053)	(0.063)
Number of observations	3,237	143	3,380	2,164	77	2,241	5,621

Notes: High segregation (HiSeg) indicates that the Hispanic to non-Hispanic population ratio is above the 75% level of all California counties based on 1940 full-count Census. Low segregation (LoSeg) indicates that the Hispanic to non-Hispanic population ratio is below the 25% level of all California counties based on 1940 full-count Census. Sample is limited to men and women from 5% samples of 1980, 1990, and 2000 Censuses with birth cohorts between 1941 and 1945 (treatment group) or between 1931 and 1935 (comparison group). Standard deviations in brackets. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: The Impact of Mendez v. Westminster School Desegregation Decision on Educational Attainment

	Panel A: Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	0.880**	0.145***	0.093**
	(0.331)	(0.034)	(0.044)
Average of Dependent Variable in HiSeg Group Prior to Mendez	10.23	0.79	0.48
Observations	5,621	5,621	5,621
R^2	0.10	0.09	0.07
	Panel B: Placebo Sample (Hispanic Birth Cohorts 1921-1930)		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Placebo Post-Mendez Cohort	-0.319	-0.01	-0.133*
	(0.415)	(0.064)	(0.069)
Average of Dependent Variable	9.17	0.70	0.36
Observations	3,423	3,423	3,423
R^2	0.07	0.08	0.04
	Panel C: Non-Hispanic White Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	-0.386***	-0.009**	-0.025**
	(0.078)	(0.004)	(0.010)
Average of Dependent Variable	13.44	0.99	0.90
Observations	27,812	27,812	27,812
R^2	0.05	0.01	0.02
Additional controls in panels A, B, and C:			
Birth year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

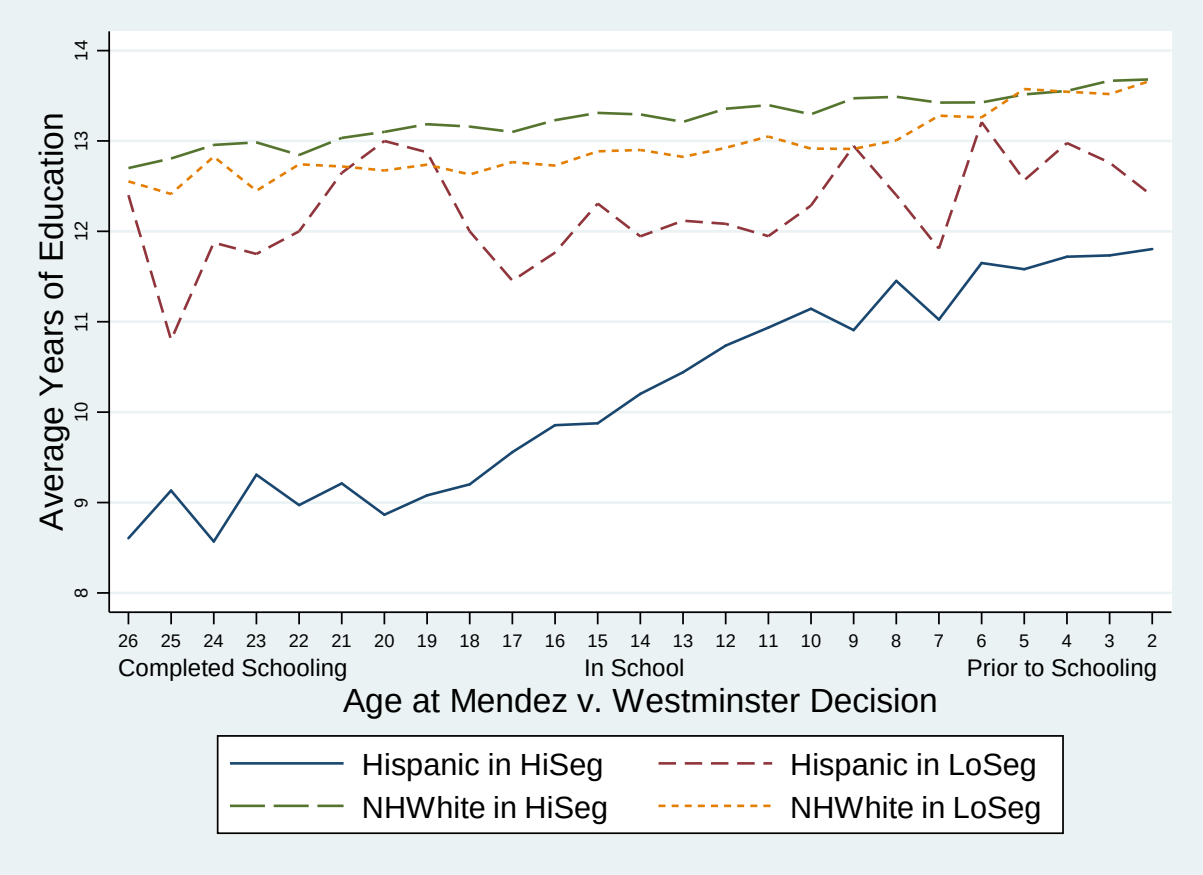
Notes: High segregation (HiSeg) indicates that the Hispanic to non-Hispanic population ratio is above the 75% level of all California counties based on 1940 full-count U.S. Census. Post-Mendez Cohort is an indicator for birth year being 1941 or later. Other controls include indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively, in addition to fixed effects noted in Table 2. Sample is limited to men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (above the 75% level for all 1940 counties: high segregation) or very low (below the 25% level for all 1940 counties: low segregation). Samples in panels A and C includes only those individuals with birth cohorts between 1941 and 1945 (treatment group) or birth cohorts between 1931 and 1935 (comparison group). Sample in panel B includes only those individuals with birth cohorts between 1926 and 1930 (placebo treatment cohort) or birth cohorts between 1921 and 1925 (placebo comparison group). Robust standard errors, clustered at county level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Impact of Mendez v. Westminster with Alternative Comparison Group
Post-Mendez Cohorts (1941-1945 Birth Cohorts) Relative to 1921-1930 Birth Cohorts

	Panel A: Full Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	1.900***	0.229***	0.184***
	(0.330)	(0.041)	(0.030)
Average of Dependent Variable, 1921-30 Cohorts	9.17	0.7	0.36
Observations	6,803	6,803	6,803
R^2	0.17	0.14	0.13
	Panel B: Male Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	2.011***	0.212***	0.125**
	(0.339)	(0.046)	(0.045)
Average of Dependent Variable, 1921-30 Cohorts	9.44	0.72	0.39
Observations	3,218	3,218	3,218
R^2	0.17	0.13	0.14
	Panel C: Female Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	1.937***	0.244***	0.244***
	(0.420)	(0.054)	(0.043)
Average of Dependent Variable, 1921-30 Cohorts	8.95	0.67	0.33
Observations	3,585	3,585	3,585
R^2	0.16	0.15	0.13
Additional controls in panels A, B, and C:			
Birth year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

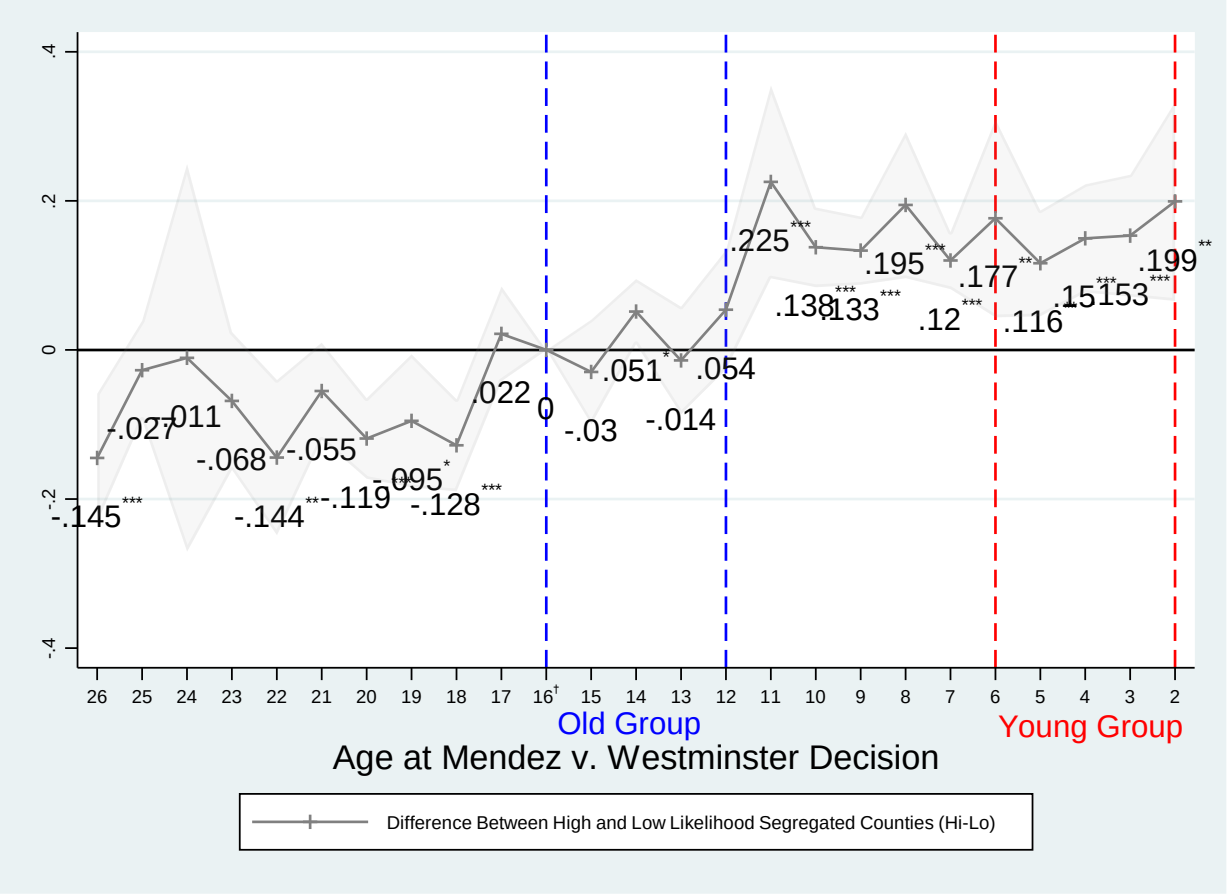
Notes: High segregation (HiSeg) indicates that the Hispanic to non-Hispanic population ratio is above the 75% level of all California counties based on 1940 full-count Census. Post-Mendez Cohort is an indicator for birth cohort being 1941 or later. Other controls include indicator for female (panel A only), and in all panels an indicator for 1990 Census observation and indicator for 2000 Census observation, respectively, in addition to fixed effects noted in Table 3. Sample is limited to Hispanic men and women from 5% samples of 1980, 1990, and 2000 Censuses in California whose birth years are between 1941 and 1945 (treatment group) and birth years between 1921 and 1930 (comparison group), and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (above the 75% level for all 1940 counties: high segregation) or very low (below the 25% level for all 1940 counties: low segregation). Robust standard errors, clustered at county level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Educational Attainment in High and Low Segregated Counties by Age at *Mendez v. Westminster* School Desegregation Decision



Notes: Sample is limited to men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1940 counties: high segregation, HiSeg), or very low (i.e., below the 25% level for all 1940 counties: low segregation, LoSeg).

Figure 2: Event Study Analysis - Educational Attainment for Hispanics, Junior High School Outcome



Notes: Sample is limited to Hispanic men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1940 counties: high segregation) or very low (i.e., below the 25% level for all 1940 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.

APPENDIX

Appendix Table A1: Extended Results--Impact of Mendez v. Westminster on Hispanic Educational Attainment for Post-Mendez Cohorts (1941-1945 Birth Cohorts) Relative to 1931-1935 Birth Cohorts

	Panel A: Full Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	0.880**	0.145***	0.093**
	(0.331)	(0.034)	(0.044)
Average of Dependent Variable			
Observations	5,621	5,621	5,621
R^2	0.101	0.090	0.068
	Panel B: Male Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	1.084***	0.156***	0.134
	(0.297)	(0.029)	(0.079)
Average of Dependent Variable			
Observations	2,718	2,718	2,718
R^2	0.096	0.094	0.062
	Panel C: Female Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	0.719*	0.135**	0.065
	(0.372)	(0.059)	(0.044)
Average of Dependent Variable			
Observations	2,903	2,903	2,903
R^2	0.094	0.092	0.066
Additional controls in panels A, B, and C:			
Birth year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

Notes: High segregation (HiSeg) indicates that the Hispanic to non-Hispanic population ratio is above the 75% level of all California counties based on 1940 full-count Census. Post-Mendez Cohort is an indicator for birth cohort being 1941 or later. Other controls include indicator for female (panel A only), and in all panels an indicator for 1990 Census observation and indicator for 2000 Census observation, respectively, in addition to fixed effects noted in table. Sample is limited to Hispanic men and women from 5% samples of 1980, 1990, and 2000 Censuses in California whose birth years are between 1941 and 1945 (treatment group) and birth years between 1931 and 1935 (comparison group), and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (above the 75% level for all 1940 counties: high segregation) or very low (below the 25% level for all 1940 counties: low segregation). Robust standard errors, clustered at county level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A2: Impact of Mendez v. Westminster on Non-Hispanic Whites with Alternative Comparison Group Post-Mendez Cohorts (1941-1945 Birth Cohorts) Relative to 1921-1930 Birth Cohorts

	Panel A: Full Non-Hispanic White Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	-0.304**	-0.004	-0.030**
	(0.116)	(0.006)	(0.012)
Average of Dependent Variable			
Observations	36,029	36,029	36,029
R^2	0.059	0.014	0.030
	Panel B: Male Non-Hispanic White Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	-0.314*	-0.002	-0.032
	(0.166)	(0.007)	(0.020)
Average of Dependent Variable			
Observations	17,398	17,398	17,398
R^2	0.060	0.017	0.041
	Panel C: Female Non-Hispanic White Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	-0.311***	-0.006	-0.028**
	(0.093)	(0.008)	(0.010)
Average of Dependent Variable			
Observations	18,631	18,631	18,631
R^2	0.041	0.012	0.024
Additional controls in panels A, B, and C:			
Birth year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

Notes: High segregation (HiSeg) indicates that the Hispanic to non-Hispanic population ratio is above the 75% level of all California counties based on 1940 full-count Census. Post-Mendez Cohort is an indicator for birth cohort being 1941 or later. Other controls include indicator for female (panel A only), and in all panels an indicator for 1990 Census observation and indicator for 2000 Census observation, respectively, in addition to fixed effects noted in Table 3. Sample is limited to non-Hispanic white men and women from 5% samples of 1980, 1990, and 2000 Censuses in California whose birth years are between 1941 and 1945 (treatment group) and birth years between 1921 and 1930 (comparison group), and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (above the 75% level for all 1940 counties: high segregation) or very low (below the 25% level for all 1940 counties: low segregation). Robust standard errors, clustered at county level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A3: The Impact of Mendez v. Westminster School Desegregation Decision on Educational Attainment with Alternative High and Low Segregation Definitions

	Panel A: Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	0.702**	0.114***	0.089**
	(0.302)	(0.032)	(0.041)
Average of Dependent Variable			
Observations	12,742	12,742	12,742
R^2	0.072	0.061	0.053
	Panel B: Placebo Sample (Hispanic Birth Cohorts 1921-1930)		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Placebo Post-Mendez Cohort	-0.398	-0.015	-0.143*
	(0.399)	(0.056)	(0.069)
Average of Dependent Variable			
Observations	8,674	8,674	8,674
R^2	0.040	0.046	0.021
	Panel C: Non-Hispanic White Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	-0.420***	-0.012***	-0.036***
	(0.077)	(0.003)	(0.011)
Average of Dependent Variable			
Observations	51,191	51,191	51,191
R^2	0.048	0.005	0.015
Additional controls in panels A, B, and C:			
Birth year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

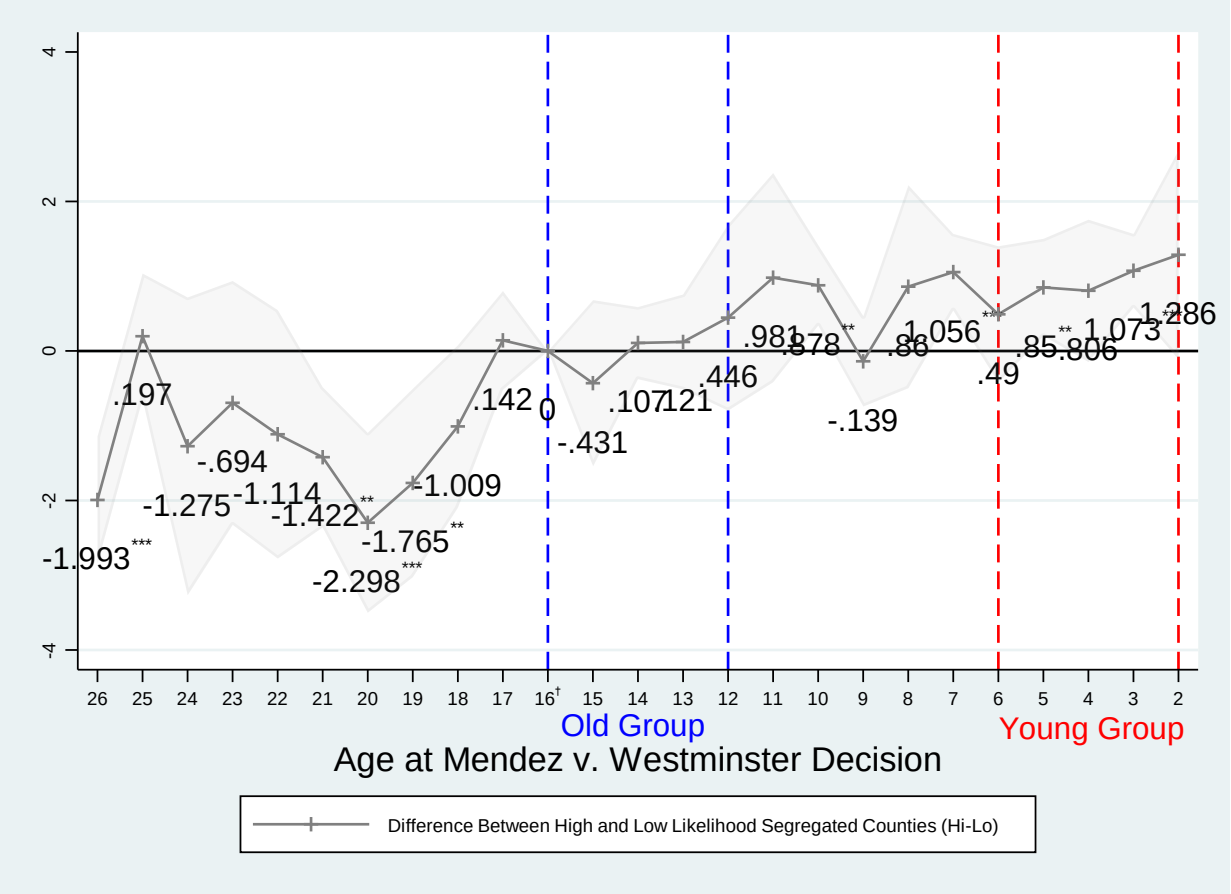
Notes: High segregation (HiSeg) indicates that the Hispanic to non-Hispanic population ratio is above the 67% level of all California counties based on 1940 full-count U.S. Census. Post-Mendez Cohort is an indicator for birth year being 1941 or later. Other controls include indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively, in addition to fixed effects noted in Table 2. Sample is limited to men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (above the 67% level for all 1940 counties: high segregation) or very low (below the 33% level for all 1940 counties: low segregation). Samples in panels A and C includes only those individuals with birth cohorts between 1941 and 1945 (treatment group) or birth cohorts between 1931 and 1935 (comparison group). Sample in panel B includes only those individuals with birth cohorts between 1926 and 1930 (placebo treatment cohort) or birth cohorts between 1921 and 1925 (placebo comparison group). Robust standard errors, clustered at county level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A4: The Impact of Mendez v. Westminster School Desegregation Decision on Educational Attainment with Alternative High and Low Segregation Definitions for Post-Mendez Cohorts (1941-1945 Birth Cohorts) Relative to 1921-1930 Birth Cohorts

	Panel A: Full Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	1.621***	0.185***	0.175***
	(0.296)	(0.039)	(0.027)
Average of Dependent Variable			
Observations	16,162	16,162	16,162
R^2	0.127	0.098	0.111
	Panel B: Male Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	1.687***	0.170***	0.115***
	(0.294)	(0.043)	(0.040)
Average of Dependent Variable			
Observations	7,596	7,596	7,596
R^2	0.125	0.093	0.115
	Panel C: Female Hispanic Sample		
	(1)	(2)	(3)
	Years of education	Jr. High School	High School
High Segregation x Post-Mendez Cohort	1.706***	0.197***	0.239***
	(0.374)	(0.050)	(0.041)
Average of Dependent Variable			
Observations	8,566	8,566	8,566
R^2	0.121	0.106	0.104
Additional controls in panels A, B, and C:			
Birth year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes

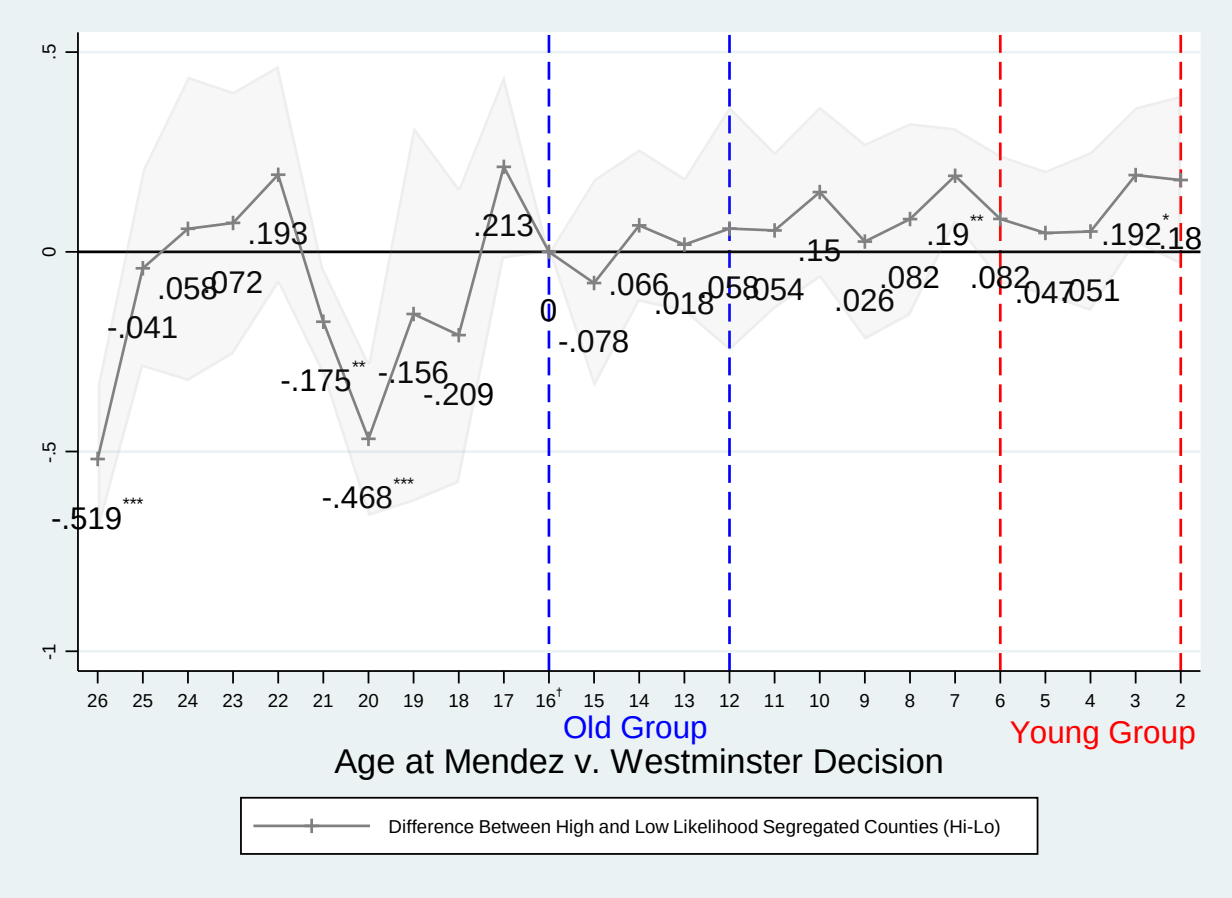
Notes: High segregation (HiSeg) indicates that the Hispanic to non-Hispanic population ratio is above the 67% level of all California counties based on 1940 full-count U.S. Census. Post-Mendez Cohort is an indicator for birth year being 1941 or later. Other controls include indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively, in addition to fixed effects noted in Table 2. Sample is limited to men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (above the 67% level for all 1940 counties: high segregation) or very low (below the 33% level for all 1940 counties: low segregation). Samples in panels A and C includes only those individuals with birth cohorts between 1941 and 1945 (treatment group) or birth cohorts between 1931 and 1935 (comparison group). Sample in panel B includes only those individuals with birth cohorts between 1926 and 1930 (placebo treatment cohort) or birth cohorts between 1921 and 1925 (placebo comparison group). Robust standard errors, clustered at county level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figure A1: Event Study Analysis - Educational Attainment for Hispanics, Years of Education Outcome



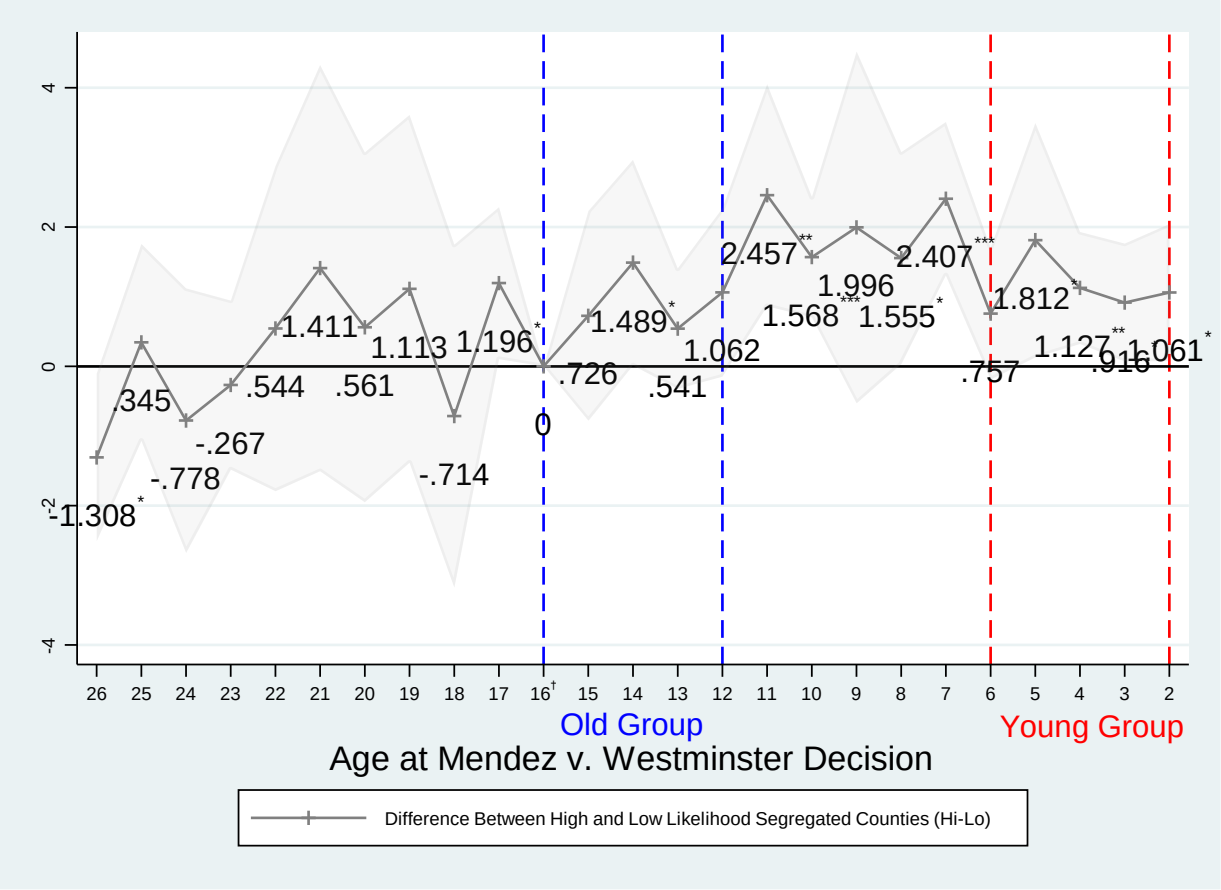
Notes: Sample is limited to Hispanic men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1940 counties: high segregation) or very low (i.e., below the 25% level for all 1940 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.

Appendix Figure A2: Event Study Analysis - Educational Attainment for Hispanics, High School Outcome



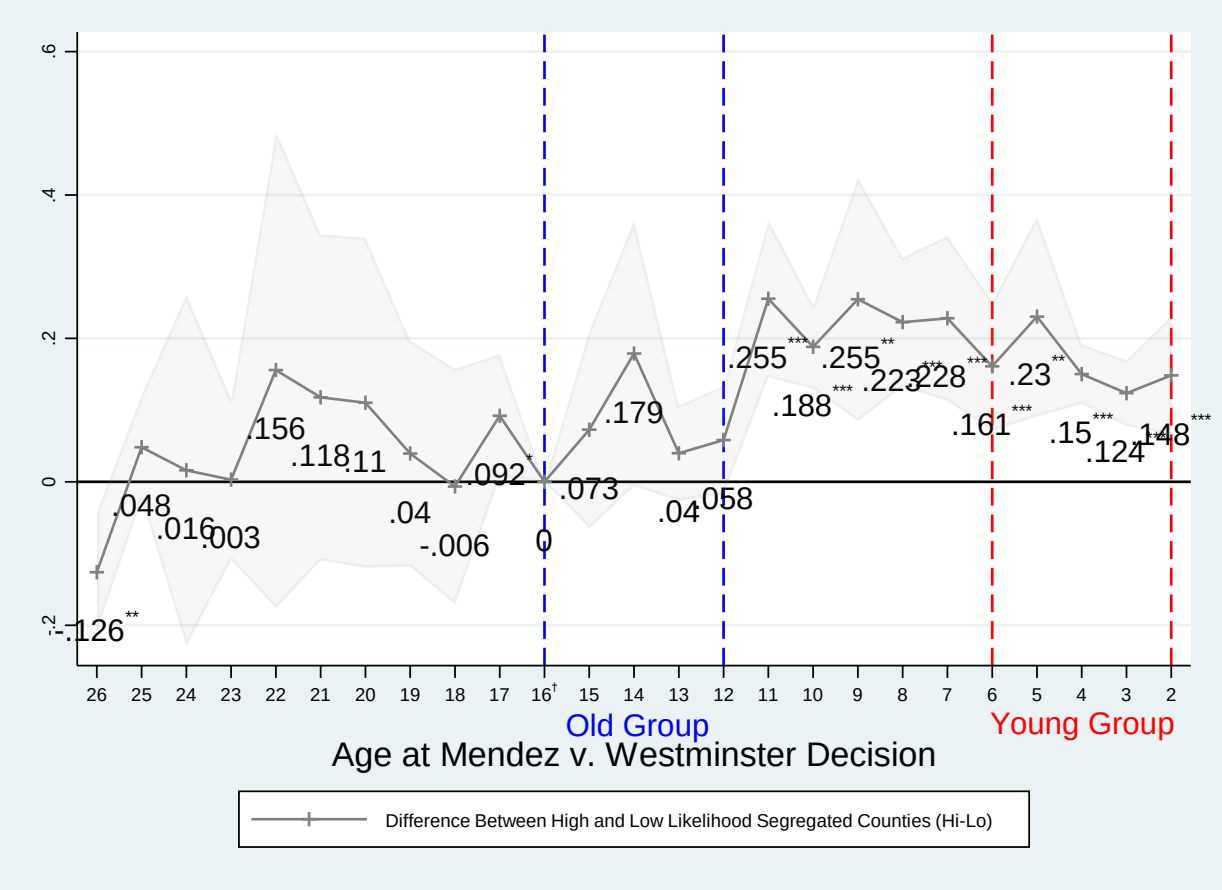
Notes: Sample is limited to Hispanic men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1940 counties: high segregation) or very low (i.e., below the 25% level for all 1940 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.

Appendix Figure A3: Event Study Analysis with 1930 County Segregation Data-Educational Attainment for Hispanics, Years of Education Outcome



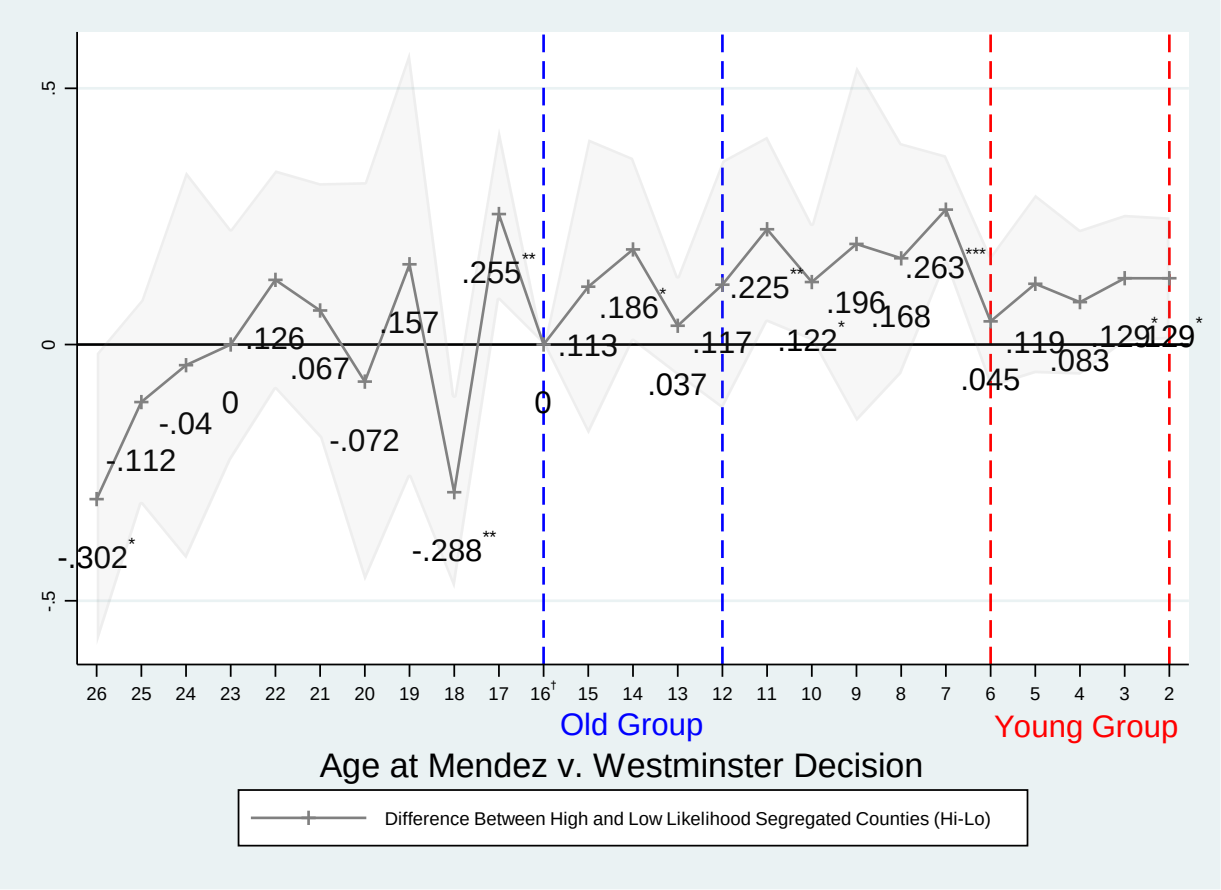
Notes: Sample is limited to Hispanic men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1930 counties: high segregation) or very low (i.e., below the 25% level for all 1930 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.

Appendix Figure A4: Event Study Analysis with 1930 County Segregation Data-Educational Attainment for Hispanics, Junior High School Outcome



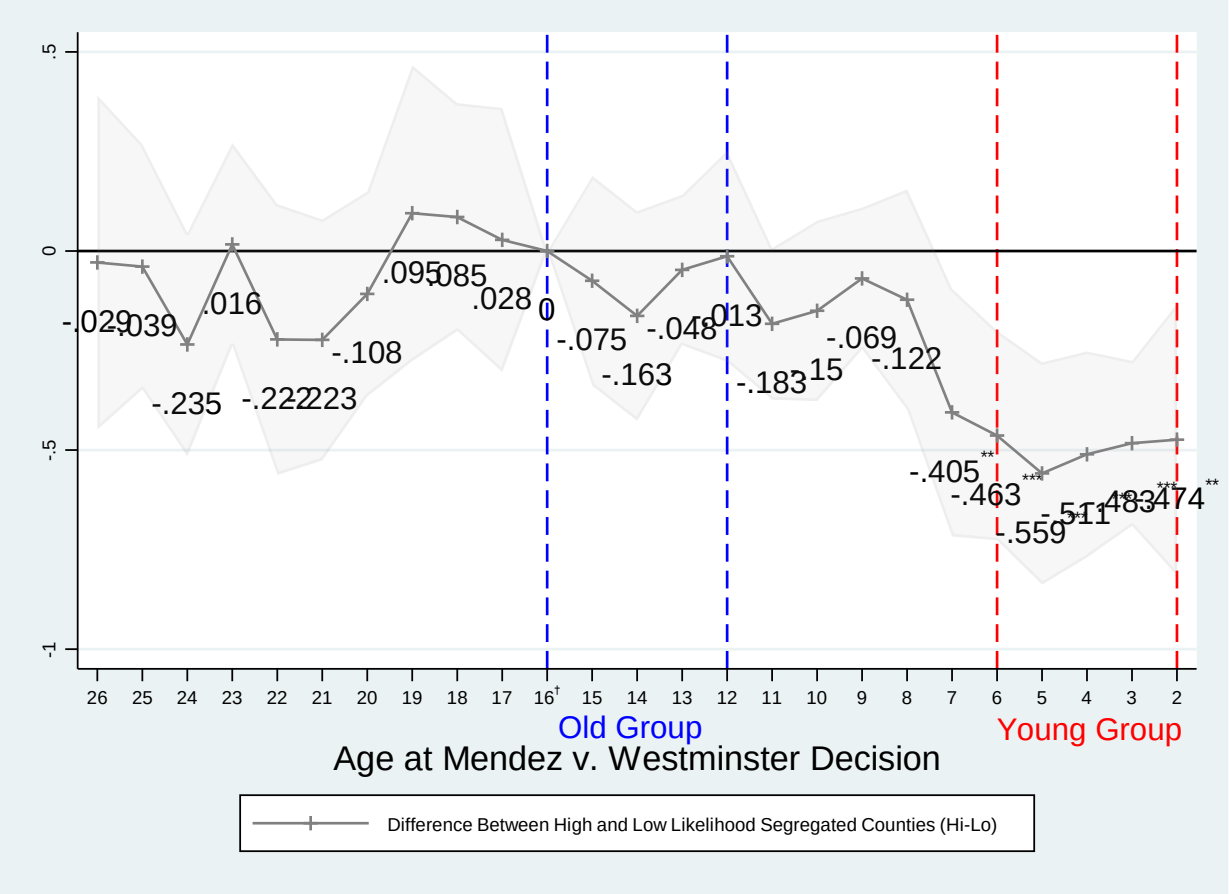
Notes: Sample is limited to Hispanic men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1930 counties: high segregation) or very low (i.e., below the 25% level for all 1930 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.

Appendix Figure A5: Event Study Analysis with 1930 County Segregation Data-Educational Attainment for Hispanics, High School Outcome



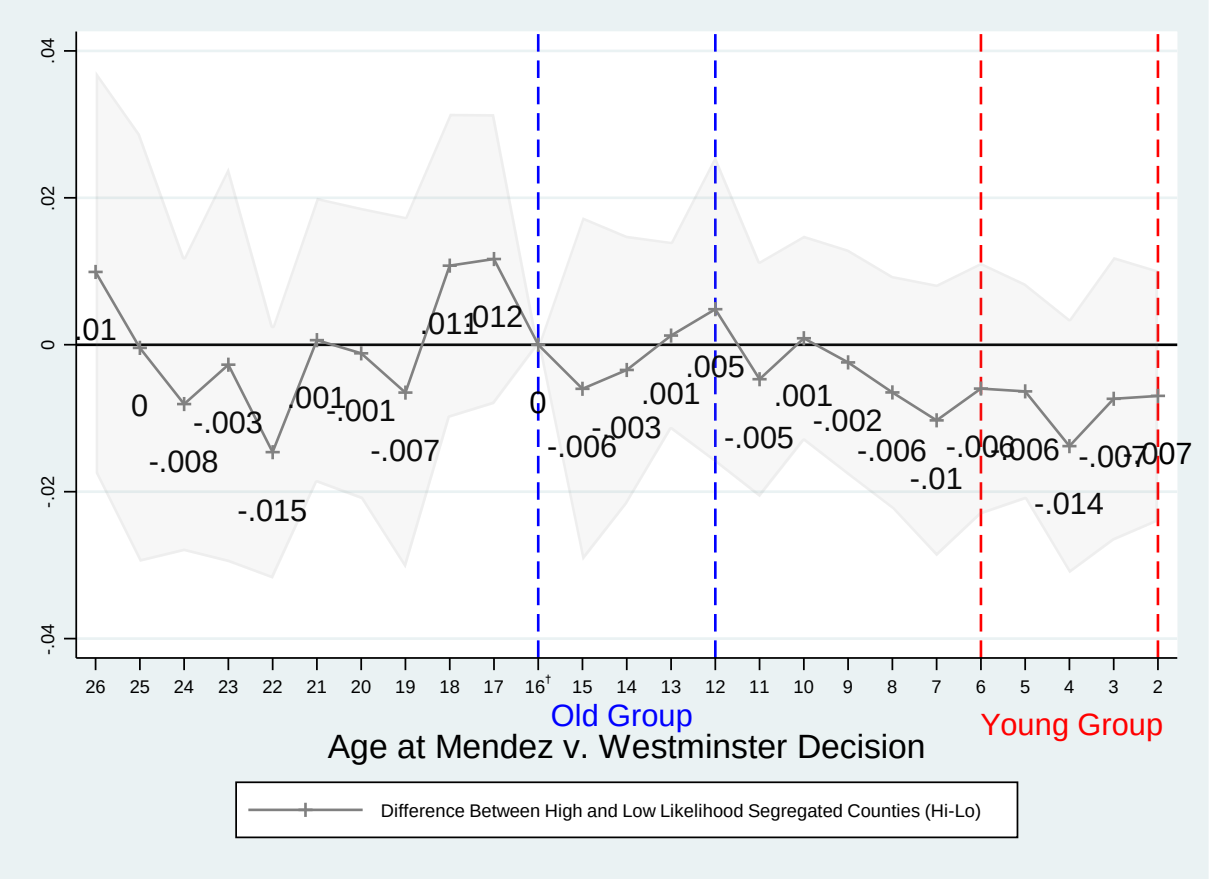
Notes: Sample is limited to Hispanic men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1930 counties: high segregation) or very low (i.e., below the 25% level for all 1930 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.

Appendix Figure A6: Event Study Analysis with 1930 County Segregation Data-Educational Attainment for Non-Hispanic Whites, Years of Education Outcome



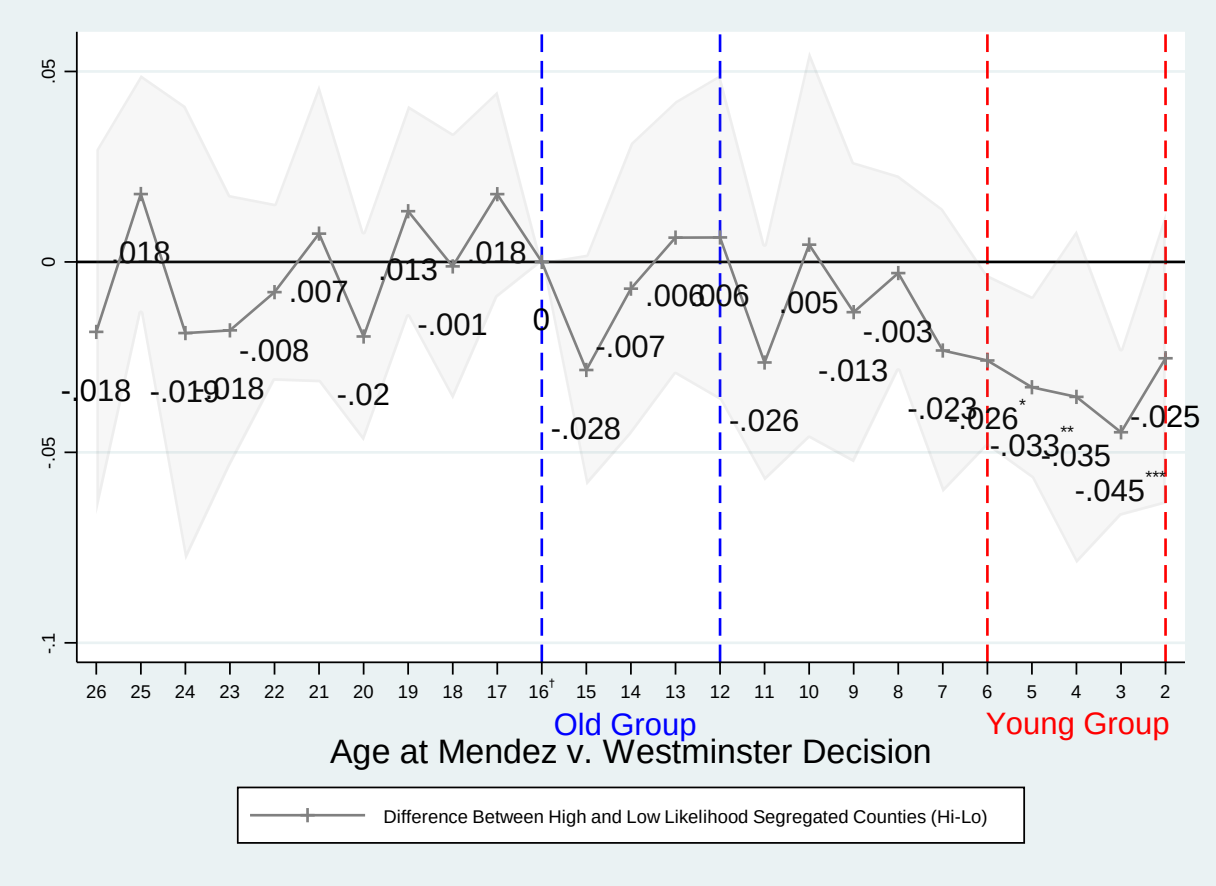
Notes: Sample is limited to non-Hispanic white men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1930 counties: high segregation) or very low (i.e., below the 25% level for all 1930 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.

Appendix Figure A7: Event Study Analysis with 1930 County Segregation Data-Educational Attainment for Non-Hispanic Whites, Junior High School Outcome



Notes: Sample is limited to non-Hispanic white men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1930 counties: high segregation) or very low (i.e., below the 25% level for all 1930 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.

Appendix Figure A8: Event Study Analysis with 1930 County Segregation Data-Educational Attainment for Non-Hispanic Whites, High School Outcome



Notes: Sample is limited to non-Hispanic white men and women from 5% samples of 1980, 1990, and 2000 Censuses who were born in California and who reside in a county where the Hispanic to non-Hispanic population ratio was either very high (i.e., above the 75% level for all 1930 counties: high segregation) or very low (i.e., below the 25% level for all 1930 counties: low segregation). Graph shows the difference-in-differences coefficient estimate on birth year interacted with an indicator for Hispanic to non-Hispanic population ratio in county being above the 75% level of all California counties based on 1940 full-count Census. All regression models also include birth year fixed effects, county fixed effects, indicator for female, indicator for 1990 Census observation and indicator for 2000 Census observation, respectively. Shaded areas indicate the 90% confidence intervals, where standard errors are clustered at county level.