

Cause or Effect? Turnout in Hispanic Majority-Minority Districts*

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Abstract

Hispanic voters appear to turnout in greater numbers when co-ethnic incumbents are on the ballot, suggesting a participation benefit to majority-minority (MM) districts. We find that this turnout effect is spurious, at least in the elections we study. When drawing minority districts, California redistrictors sorted higher participating minorities into MM districts in the 1990, 2000, and 2010 redistricting cycles, biasing subsequent comparisons of minority participation. To overcome this problem, we exploit variation induced by the redistricting process through a difference-in-difference design to study Hispanic turnout in California before and after apportionment. We match census blocks moved to districts represented by Hispanic incumbents to blocks that do not move, using a new hierarchical matching algorithm. Our approach allows for a placebo test to validate our design, and sensitivity analyses to assess the robustness of the findings to bias. Contrary to previous research, we find that being moved to a district with an Hispanic incumbent has little impact on Hispanic participation.

1 Introduction

In spite of significant advances in minority representation and socioeconomic status, participation by minority voters still lags considerably behind that of other groups in the U.S. This inequality has prompted political scientists to look beyond traditional socioeconomic explanations, and focus on shared race or ethnicity between voters and candidates to explain variation in minority turnout and registration (Barreto 2007; Barreto, Segura, and Woods 2004; Bobo and Gilliam 1990; Gay 2001a). A central finding in this work is that minority citizens represented by co-ethnic incumbents are more likely to participate in elections. Since most minority incumbent legislators are elected in majority-minority (MM) legislative districts, this has been interpreted as evidence that the creation of MM districts causes an increase in minority turnout.¹

We call into question the causal interpretation of this participation effect with an analysis of Hispanic turnout in California. Two features make California an ideal case. Hispanics are especially likely to exhibit co-ethnic effects due to strongly-held cultural, linguistic, and political identities (Barreto 2007; Shaw, De La Garza, and Lee 2000). Moreover, California is the state with the greatest experience with majority-Hispanic redistricting, which has prompted many scholars before us to focus on it (e.g., Barreto 2007; Barreto, Segura, and Woods 2004; Barreto, Villarreal, and Woods 2005; Gay 2001b; Shaw, De La Garza, and Lee 2000).

We analyze California's congressional elections before and after the 2000 redistricting cycle. We find that redistricting plans that create or preserve minority-concentrated voting areas to safely elect minority candidates, consistently induce systematic differences between MM districts and non-MM districts on many electorally-relevant characteristics. Particularly troublesome, our analysis of California's last three redistricting cycles (1990, 2000, and 2010) shows that areas that are placed in MM districts have higher minority participation in *prior* elections than areas placed in non-MM districts. Studies that fail to control for these systematic differences originating during redistricting are liable to mistake co-ethnic representation as the *cause* of minority voting, rather than the consequence of higher-participating minority areas being more likely to become part of

¹We use the term majority-Hispanic (MH) to refer to MM districts where the majority of the population is Hispanic.

MM districts in the first place.

To overcome this general methodological problem, we use a research design directly based on the variation induced during redistricting. Scholars have utilized the process of redrawing legislative districts to study the personal vote ([Ansolabehere, Snyder, and Stewart 2000](#); [Desposato and Petrocik 2003](#); [Sekhon and Titiunik 2012](#)).² We are the first to propose a redistricting design to study the effects of MM districts or the ethnicity of candidates. The design exploits the fact that redistricting alters the ethnicity of the incumbent and the electorate for some voters but not others. In particular, we compare voters moved to a new incumbent and district to voters whose incumbent ethnicity remains unchanged after redistricting.

Voters who are redistricted, however, are not randomly selected. Thus, we control for differences in the observable characteristics used in the redistricting process to determine which blocks of voters will join new MM districts. We develop a new hierarchical matching approach that finds comparable redistricted and not-redistricted blocks when these are nested within multiple jurisdictions (i.e., state assembly, senate, congress), to obtain balance on crucial pre-determined covariates across blocks and districts simultaneously. Using this design, we analyze an outcome where we expect a zero effect by construction, providing a way to evaluate whether biases are removed by hierarchical matching. Finally, we use Hispanic turnout and registration before and after redistricting to estimate difference-in-differences, correcting for any biases that may remain in baseline participation after matching.

Contrary to prior research, we find that being moved to an Hispanic incumbent and district has little effect on either Hispanic voter turnout or registration in subsequent elections. We find a small decline in non-Hispanic turnout, though this effect just misses statistical significance. Finally, we conduct sensitivity tests to evaluate whether observed differences on pre-redistricting covariates could account for any participation effects recovered before and after matching ([Rosenbaum 2002](#)). The sensitivity analysis is consistent with our main findings of no turnout benefits associated with

²[Ansolabehere, Snyder, and Stewart \(2000\)](#) used redistricting to estimate the incumbency advantage by comparing vote share in the new parts of districts to that in the unchanged parts. [Sekhon and Titiunik \(2012\)](#) showed the original redistricting designs were flawed, proposed new designs, and derived conditions to identify causal effects.

co-ethnic representation.

2 The Minority Empowerment Hypothesis

Majority-minority districting under the Voting Rights Act (VRA) dramatically expanded minority access to political offices.³ Indeed, over the last half-century, the number of Hispanic members of Congress grew seven-fold, from 4 (0.7%) to 31 (7.1%) (U.S. Census Bureau 2012). This expansion of minority incumbents led scholars to study whether MM districts also lead to an increase in minority voter turnout.

According to the traditional resource model, people participate in elections when they can afford the costs of doing so and when the benefits outweigh these costs (e.g., Wolfinger and Rosenstone 1980). Recently, scholars have emphasized the ethnicity of candidates as contributing unique electoral costs and benefits (Barreto 2007; Bobo and Gilliam 1990; Gay 2001a; Tate 2003). Having a co-ethnic candidate seek and win political office may give minorities a psychological benefit by signaling that their minority group has political influence. As a consequence of this ‘empowerment’, levels of trust, efficacy, and political knowledge may increase amongst co-ethnics, and lead to their greater vote-participation (Banducci, Donovan, and Karp 2004; Bobo and Gilliam 1990; Gilliam 1996).⁴

The early focus in this work has been on uncovering the empowerment effects of black politicians, producing largely mixed results. In an influential paper, Bobo and Gilliam (1990) show that blacks living in cities with a black mayor are more politically engaged than those whose mayor is Non-Hispanic White (NHW). Conversely, in a study of eight states, Gay (2001a) finds that being

³A decade following passage, Congress amended the VRA in 1975 to include Hispanics as a protected minority group. Additional amendments in 1982, upheld in *Thornburg v. Gingles* (1985), barred redistricting plans that diluted minority voting strength even if drawn without an explicitly discriminatory purpose. Subsequently, the VRA was interpreted as mandating MM districts whenever feasible to expand minority representation (Gay 2001b).

⁴We follow previous literature and focus on voter turnout as a fundamental measure of minority empowerment (Banducci, Donovan, and Karp 2004; Barreto 2010, 2007; Barreto, Segura, and Woods 2004; Barreto, Villarreal, and Woods 2005; Bobo and Gilliam 1990; Gay 2001a,b; Gilliam and Kauffman 1998; Griffin and Keane 2006; Grofman and Reynolds 1996; Leighley 2001; Shaw, De La Garza, and Lee 2000; Tate 1991). Voting is of course not the only important form of political empowerment, but it is a necessary condition for a fully inclusive and responsive political system.

represented by a black legislator lowers NHW turnout, but has no positive effect on black turnout. [Tate \(2003\)](#) and [Griffin and Keane \(2006\)](#) also find little evidence that candidates' race affects minority participation.

Some scholars suggest Hispanic, rather than black voters may be especially responsive to co-ethnic representation ([Barreto 2010, 2007](#); [Barreto, Villarreal, and Woods 2005](#); [Shaw, De La Garza, and Lee 2000](#)). Many Hispanic voters share a common language and heritage that elicits a sense of distinctness from other American groups ([Barreto 2010](#)). Because of this feature, Hispanic leaders may campaign along co-ethnic cleavages, reinforcing the connection between co-ethnicity and minority mobilization ([Leighley 2001](#)). Finally, Spanish surnames on the ballot or in campaign materials may provide informative cues about a candidate's policy record, encouraging Hispanics to show up or vote down-ballot by reducing their information costs ([McConnaughy et al. 2010](#)).

Accordingly, [Barreto, Villarreal, and Woods \(2005\)](#) show that high-density Hispanic precincts in Los Angeles have higher turnout when Hispanic candidates contest elections. [Barreto \(2007\)](#) similarly finds in five major cities that Hispanic mayoral candidates on the ballot increases rates of Hispanic voter turnout. [Shaw, De La Garza, and Lee \(2000\)](#) validate self-reported turnout from a survey of Hispanics in California, Florida and Texas, and find that turnout increases as a result of co-ethnic mobilization by Hispanic groups. And [Barreto, Segura, and Woods \(2004\)](#) find a strong association between residing in multiple, overlapping majority-Hispanic (MH) legislative districts and Hispanic turnout rates among registered voters in California. However, using national voter registration data, [Fraga \(2012\)](#) finds that Hispanic representation is not correlated with greater Hispanic turnout, suggesting the association does not generalize across all House districts.

An alternative hypothesis is that MM districts *lower* minority turnout by reducing electoral competition, dampening the benefits to participating in minority-concentrated areas ([Brace et al. 1995](#); [Gay 2007](#); [Guinier 1994](#)). There is little support in the extant literature for the low-competition hypothesis. Although a variety of countervailing effects are possible, we show that the way MM districts are drawn remains a critical inferential issue in determining which, if any, of these forces are causes of minority participation.

3 Racial Redistricting and its Consequences for Studying Minority Participation

We now show that, during each of the last three cycles (1990, 2000, and 2010), California redistricted higher-turnout Hispanic voters into new or existing MH U.S. House districts, as the number of these districts expanded from two to ten.⁵ We also discuss some possible mechanisms that could drive these differences in pre-redistricting minority turnout. Despite our focus on California, these mechanisms are general incentives or legal restrictions created by the VRA that may shape the racial redistricting process in other states.

3.1 Empirical Evidence of Bias

Our main analysis is drawn from data on the 2000 California redistricting plan for U.S. House districts. After the 2000 redistricting, raw minority turnout rates are higher in majority-Hispanic than in non-Hispanic districts.⁶ For example, in 2004, mean Hispanic turnout in non-MH congressional districts was 4.5 percentage points higher than in MM districts. But this raw difference is misleading since MH districts differ from non-MH districts in many features, most importantly Hispanic voting age population (HVAP). After adjusting for the proportion of HVAP over total voting age population (or P-HVAP), we see that the mean 2004 Hispanic turnout rate is 4.3 percentage points higher in MH than non-MH districts.⁷ Figure 1 displays a Quantile-Quantile plot (QQ-plot) of Hispanic turnout in 2004, comparing blocks in majority-Hispanic congressional dis-

⁵Most California's MM districts were created in 1990 from Republican efforts to use the new legal environment to expand party influence in the state (Kousser 1997). These districts were subsequently consolidated under Democratic majorities through incumbent protection plans in 2000 and 2010 (McDonald 2004; Pierce and Larson 2011). We describe these processes in more detail in the Supplemental Appendix.

⁶Turnout outcomes are measured throughout as a proportion of total (non-)Hispanic registration in 2000. Registration in 2000, as opposed to in 2002, is unaffected by the 2000 redistricting—which occurs after the 2000 election. Registration outcomes are measured throughout as a proportion of the (non-)Hispanic voting age population in 2000.

⁷Mean differences are calculated after matching on P-HVAP at the 2000 census block. The estimand is average treatment effect on the treated defined as being in a MH House district with an Hispanic incumbent. The analogous differences for 2000 and 2002 are respectively, 2.4% and 2.6%. Using the same matches, differences in turnout over HVAP across MH and non-MH seats are 7.1%, 4.9%, and 7.6% in 2000, 2002, and 2004.

tricts to blocks in non-majority-Hispanic districts.⁸ The QQ-plot shows that, for blocks with equal proportion of Hispanic adults, there is a significant and positive association between Hispanic turnout and majority-Hispanic status.⁹

The critical question is whether these Hispanic participation differences are *caused* by residing in a majority-Hispanic district with an Hispanic representative, or whether these arise as a result of pre-existing differences among voters in MH and non-MH districts. The evidence we present strongly suggests the latter. In Figure 2, we display a QQ-plot of Hispanic turnout in the 2000 election, comparing ‘treated’ blocks, those that *will be moved* from a congressional district represented by a NHW incumbent to a district represented by an Hispanic incumbent, to ‘control’ blocks, those that keep the same NHW incumbent before and after redistricting.¹⁰ We find that blocks that will be moved have a mean Hispanic *prior* turnout rate that is 4.2 percentage points higher than blocks that will not be moved. Further, the QQ-plot reveals that this bias persists across the whole distribution.¹¹ This shows that, given equal P-HVAP and *before the redistricting process occurs*, Hispanic voters who will be redistricted into a district with an Hispanic incumbent vote at systematically higher rates than Hispanic voters who will be left in their old NHW-incumbent district.

Bias persists not only in 2000, but also in 1990 and 2010. We replicate our analysis for 1990, when most of California’s MH districts were created, and 2010, when a non-partisan citizens commission maintained these districts. Figure 3(a) and Figure 3(b) present QQ-plots for Hispanic registration over HVAP for the 1990 and 2010 redistricting, respectively, and Figure 3(c) presents a QQ-plot for Hispanic turnout (over HVAP) in 2010. These plots show that blocks moved to districts represented by Hispanic politicians consistently have higher rates of previous Hispanic

⁸QQ-plots display quantiles of one empirical distribution against quantiles of another, where deviations off the 45-degree line indicate differences in the distributions.

⁹Similar patterns emerge when comparing blocks simultaneously in three overlapping MH districts (state assembly, senate, congress) to blocks in three non-MH districts, following Barreto, Segura, and Woods (2004). See Supplemental Appendix.

¹⁰Defining treatment using majority-Hispanic (50%) status leads to nearly-identical results, as nearly all majority-Hispanic districts are represented by an Hispanic incumbent, and nearly all Hispanic incumbents represent majority-Hispanic districts.

¹¹The bias is also substantial when comparing Hispanic voters across overlapping legislative districts with multiple Hispanic incumbents. See Supplemental Appendix.

registration (by 4% to 5%) and turnout (by 3%) than those remaining with NHW incumbents in both 1990 and 2010. In combination, this provides overwhelming evidence that California's racial redistricting process induces significant differences in prior participation across MM and non-MM districts, a finding particularly worrisome for studies that have used these races to analyze minority empowerment. This bias is particularly illustrative since it emerges not only when MM districts were originally created, but also at each cycle in which they were maintained and expanded. Moreover, bias appears regardless of whether redistricting is driven by Republican (1990), Democratic (2000), or non-partisan (2010) efforts.

3.2 Bias Generating Mechanisms

Although the empirical evidence in California of bias from racial redistricting is clear, there is a paucity of research on exactly why it arises. We are amongst the first to consider the systematic features of blocks that predict their later inclusion in MM districts during redistricting. We explore here the degree to which many of the major actors in California have a stake in locating higher participating minorities into MM districts. We find that officials may do so to comply with the non-minority-dilution provisions in the VRA, as well as to help secure incumbent reelection. While these are not exhaustive explanations for how this bias arises, they do point to common features of the redistricting process that potentially influence mapdrawing across many states with significant minority populations.¹²

The Department of Justice (DOJ) enforcement of preclearance in Section 5 of the VRA provides states with a strong incentive to avoid redistricting plans that lower minority participation in districts represented by minority incumbents. Consequently, covered states may aim to redistrict higher participating minorities into MM districts to avoid retrogression or vote dilution challenges ([California Citizens Redistricting Commission 2011](#)). Section 5 of the VRA requires certain states and counties to preclear changes to legislative boundaries by the DOJ to ensure these do not have

¹²See Supplemental Appendix for a discussion of other potential selection mechanisms, including geographical and legal constraints on MM districting that may indirectly contribute to prior differences in minority participation.

a ‘retrogressive effect’, “‘leav(ing) minority voters with less chance to be effective in electing preferred candidates than they were’ under the prior districting plan” ([League of United Latin American Citizens v. Perry 2006](#), p. 34). According to its guidelines, in addition to concentration of minority population, the DOJ broadly analyzes minority electoral behavior, specifically “differing rates of electoral participation within discrete portions of a population”, to establish whether a proposed plan will be retrogressive ([Department of Justice 2011](#), p.7471).¹³

Allocating lower-participating minorities to districts represented by minority incumbents could be considered retrogressive under these criteria, given the expectation that the candidate of choice of minority voters is the minority incumbent. Perhaps the clearest evidence of this emerged in the 2010 Texas redistricting plan, which failed to obtain preclearance due to attempts to move lower-turnout Hispanic voters into the Hispanic-represented 23rd House district. The courts found these efforts to be retrogressive and implemented their own maps, highlighting clear legal and political incentives for states to redistrict higher-turnout minorities into minority-represented districts ([Texas v. United States 2012](#)). In California, four counties are subject to preclearance (Kings, Merced, Monterey and Yuba), which covers Hispanic voters in four congressional districts (and nine assembly and senate districts).¹⁴ Plans that fail to sustain levels of Hispanic participation in these counties risk a DOJ objection and possible judicial intervention, providing a strong deterrence incentive to allocate high-participating minorities to MM districts ([Hasen 2006](#)).

Moreover, incumbents of both parties may support moving higher participating minorities into MM districts, given the commitment to create or maintain minority legislative seats, since this can help secure their reelection. Hispanic incumbents want high-turnout Hispanic areas to be in their districts, and Republicans want such areas to be removed from their districts, since these voters tend to elect Democrats ([Lopez and Taylor 2012](#)). Some NHW Democrats may prefer not to move high-

¹³The guidelines have several passages where turnout is considered explicitly: “Election history and voting patterns within the jurisdiction, voter registration and turnout information, and other similar information are very important to an assessment of the actual effect of a redistricting plan” ([Department of Justice 2011](#), p. 7471).

¹⁴Since these counties and their districts are included in the redistricting plan, the entire state plan must be precleared ([Cain, MacDonald, and Hui 2006](#)). The recent Supreme Court decision in *Shelby v. Holder*, however, has clouded whether these counties will remain covered in the next redistricting, barring Congressional action to amend the overturned formula in Section 4.

turnout Hispanic areas from their district to a MM district, particularly when this hurts reelection. However, evidence suggests that the Democrats designed the 2000 California plan precisely to balance the electoral goals of Democratic incumbents, while maintaining the voting strength of Hispanics in MM districts, to minimize this potential tradeoff (McDonald 2004). Overall, these incentives may encourage bipartisan log-rolling since protecting incumbents through redistricting is one thing both Democrats and Republicans usually agree on (Bickerstaff 2007).¹⁵

4 Redistricting as Research Design

We propose a novel research design aimed at evaluating the effects of being placed in a district represented by an Hispanic incumbent on Hispanic turnout, explicitly accounting for the biases introduced during redistricting. Our research design exploits the variation brought about by the redistricting process, and builds on previous work by Ansolabehere, Snyder, and Stewart (2000) and Sekhon and Titiunik (2012), who used a related design to study the incumbency advantage. Redistricting induces both temporal and cross-sectional variation, since voters vote both before and after redistricting, and some voters are moved to a different district while others are not. Our research design exploits this variation to estimate the effect of co-ethnic incumbency on Hispanic turnout.

Our unit of analysis is the census block, and we let $T_i^{WH} = 1$ if block i is moved from a congressional district represented by a NHW incumbent to one represented by an Hispanic incumbent just before election t , and $T_i^{WH} = 0$ if block i stays in a district represented by a NHW incumbent before and after election t . We define $Y_0(i, t)$ to be the outcome of block i if $T_i^{WH} = 0$, and $Y_1(i, t)$ the outcome if $T_i^{WH} = 1$. We only observe the *realized* outcome, $Y(i, t) = Y_0(i, t) \cdot (1 - T_i^{WH}) + Y_1(i, t) \cdot T_i^{WH}$. Our parameter of interest is the average treat-

¹⁵Though the recent case in Texas suggests these incentives may depend on the balance of party control in the state, or on opportunities to engage in partisan gerrymandering.

ment effect on the treated:

$$ATT^{WH} \equiv E [Y_1(i, t) - Y_0(i, t) | T_i^{WH} = 1] . \quad (1)$$

If redistricting was randomized, we could estimate ATT^{WH} as the unconditional turnout differences between voters moved from a NHW to an Hispanic incumbent and voters left with a NHW incumbent before and after redistricting. Of course, redistricting is far from random, with several population characteristics considered when drawing district boundaries. Fortunately, we have the same characteristics that public officials use in redistricting (i.e., census information, election results, past turnout, registration).¹⁶ The crucial assumption is that, controlling for these characteristics, non-redistricted (control) blocks are a valid comparison group to infer the turnout rates that would have occurred after redistricting among redistricted (treated) blocks, if the incumbent in treated blocks had not changed.

This assumption that treatment and control groups are comparable after controlling for observable characteristics X , called the “selection on observables” assumption, is defined formally as

$$E [Y_0(i, t) | T_i^{WH} = 1, X] = E [Y_0(i, t) | T_i^{WH} = 0, X] . \quad (2)$$

This assumption is sufficient to estimate the conditional version of our effect of interest

$$\begin{aligned} ATT(X)^{WH} &= E [Y_1(i, t) - Y_0(i, t) | T_i^{WH} = 1, X] \\ &= E [Y(i, t) | T_i^{WH} = 1, X] - E [Y(i, t) | T_i^{WH} = 0, X] . \end{aligned}$$

$ATT(X)^{WH}$ can then be averaged over the distribution of X in the treatment group to recover the unconditional parameter ATT^{WH} .

We show below that the restriction in (2) may be too strong given the evidence for California.

¹⁶We also have software used during redistricting in California.

In other words, redistricted voters may not be comparable to voters whose previous incumbent remains unchanged, even if they have similar values in X . Since we observe participation rates before and after redistricting, we replace (2) with a more plausible assumption. This assumption requires that, controlling for X , average participation in redistricted and non-redistricted blocks would have had similar trends if redistricting had not occurred. In other words, the *change* in Hispanic turnout we observe among control blocks whose incumbent does not change is the change in Hispanic turnout that treated blocks would have had if their incumbent had not changed. This assumption is defined formally as

$$E[Y_0(i, t) - Y_0(i, t - 1) | T_i^{WH} = 1] = E[Y_0(i, t) - Y_0(i, t - 1) | T_i^{WH} = 0]. \quad (3)$$

In contrast to Assumption (2), Assumption (3) requires preredistricting participation data to recover causal estimates (Abadie 2005). Under (3), our parameter of interest can be recovered by estimating *difference-in-differences*, comparing the difference between treatment and control in the *change* in Hispanic turnout before ($t - 1$) and after (t) redistricting:

$$\begin{aligned} & E[Y(i, t) - Y(i, t - 1) | T_i^{WH} = 1, X] - E[Y(i, t) - Y(i, t - 1) | T_i^{WH} = 0, X] \quad (4) \\ &= E[Y_1(i, t) - Y_0(i, t) | T_i^{WH} = 1, X] \\ &= ATT(X)^{WH}. \end{aligned}$$

4.1 Hierarchical Matching

Legislatures simultaneously redistrict overlapping congressional, assembly and state senate jurisdictions. We focus analysis on movements of blocks between congressional districts, since we expect these elections to be most salient for voters. However, we only compare treated and control voters within the same (pre-redistricting) triplet of congressional, assembly and senate districts, as defined in the 1990 redistricting plan. We limit our analysis in this way because, as Barreto, Segura, and Woods (2004) show, areas represented by Hispanic incumbents at all three

levels are systematically different from areas represented by fewer or no Hispanic incumbents. This restriction ensures that variation in features of prior state and House legislative districts does not affect our inferences.

Due to the hierarchical nature of redistricting, we develop a multilevel algorithm that uses a genetic optimizer to match blocks moved from a NHW to an Hispanic congressional incumbent to unmoved blocks based on the baseline covariates used during redistricting (Diamond and Sekhon 2012; Sekhon 2011). Genetic matching utilizes a nearest-neighbor algorithm to create treated-control pairs that minimize distances in multivariate space. The algorithm prioritizes matches that reduce differences across matched-units on the most dissimilar characteristics, gradually increasing the similarities (*balance*) on each covariate in X over successive generations. Hierarchical genetic matching uses a constrained optimization that limits the available matches to be within the same legislative district “triplet”, minimizing within-triplet distances. Matched pairs *within* each triplet are selected to maximize balance *across* all triplets. Our study of minority turnout is the first use of this hierarchical matching algorithm in empirical research.

5 Data Sources and Measurement

We analyze data from California’s 2000 redistricting plan collected from the California State-wide Database (SWDB) for the 1998 to 2004 period, using 2000 census block as our unit of analysis. Census blocks are the lowest level of geography used in redistricting (Cain, MacDonald, and Hui 2006). Focusing on blocks allows us to replicate the same redistricting selection process used in 2000. We also can track the same unit over time, something not possible if we analyzed other aggregated geographies. For 1998 and 2000, the SWDB includes turnout and registration figures for 2000 census blocks. However, for 2002 and 2004, participation data are only available at the 2010 census block, and thus must be converted to 2000 census block. We take a standard approach, using Census Block Relationship Files to convert participation data at the 2010 census-

block to the 2000 census-block level.¹⁷

In addition to total turnout and registration counts, the SWDB data contains turnout and registration by age, party affiliation and ethnicity. Importantly, Hispanic turnout figures are constructed from block-geocoded and surname-matched individual records in California voter registration files.¹⁸ The SWDB also includes vote returns for congressional, assembly, state senate, U.S. senate, presidential, and other elections. For incumbent ethnicity indicators, we use the *Hispanic Americans in Congress* website, maintained by the Congressional Research Service. Finally, to measure important covariates, we merged block-level data from the 2000 Census Summary File 3 to the SWDB dataset.¹⁹ Every block in the dataset was assigned its corresponding congressional, assembly and state senate district according to the redistricting plan effective for each general election between 1998 and 2004.

Our unit of analysis is the census block, the fundamental level redistrictors use in their software, data and algorithms to draw maps. However, our participation measures originate directly from individual-level voter files through SWDB surname matching. As a result, we measure Hispanic participation with much greater accuracy than would be possible from ecological inference using aggregated demographics.²⁰ Yet, to assess the effects of racial redistricting, we must aggregate individual-level data to blocks, since legislatures redistrict census-block geographies and not individuals, and use the same information utilized by redistrictors to control for biases that emerge from the process.²¹

¹⁷See Supplemental Appendix for details and robustness checks for our block-conversion procedure.

¹⁸See Supplemental Appendix for details on the SWDB surname-matching approach to measure participation rates by ethnicity.

¹⁹The block-level Summary File 3 variables are approximate because the smallest geographical unit for which the Summary File 3 is reported is the block-group level. Census variables include population by age, race or ethnicity, and language spoken at home, employment status, place of birth, and education level.

²⁰See Barreto (2007), Barreto, Villarreal, and Woods (2005), and Gay (2001a) for studies that use some ecological inference to estimate minority turnout from aggregate data.

²¹This aggregation step is analogous to an individual-level regression approach where the marginal effects of residing in multiple MM districts are estimated as the aggregate mean differences in participation rates. However, this regression approach may be biased because individual-level predictors are not directly used to determine the composition of districts and are only imperfectly related to the aggregate information that redistrictors use.

6 Results

Before estimation, we construct two datasets from California’s more than 300,000 blocks, by trimming and matching to reduce demographic, socioeconomic and electoral dissimilarities across minority-redistricted geographies. Importantly, post-redistricting participation was never used in constructing these datasets.²² To create BASELINE, we exclude open-seat House races in 2000 or 2002, and races with a defeated incumbent in 2000.²³ Throughout, we define treatment blocks as having a NHW incumbent in the 2000 congressional election and an Hispanic incumbent in the 2002 and 2004 elections. For BASELINE, we define control blocks as having a NHW congressional incumbent in elections from 2000 to 2004. Finally, we match each treated block to a control with the most similar P-HVAP, resulting in 4,687 matched-pairs.²⁴

Overall, blocks differ greatly in minority population, with 49% P-HVAP in treatment compared to 29% in control. While matching on P-HVAP eliminated this difference, treatment and control groups in BASELINE still differ in nearly every other observable characteristic. Table 1 presents p -values from difference-in-means t -tests and Kolmogorov-Smirnov tests (ks -tests) on redistricting characteristics across treatment and control. The first three columns show that considerable imbalances remain even after controlling for P-HVAP in BASELINE. For instance, treatment blocks have higher income, a greater proportion of naturalized citizens, and significantly higher rates of previous Hispanic and total registration, which all positively correlate with Hispanic participation.

Due to remaining imbalances in BASELINE, we construct MATCHED by imposing additional restrictions and conducting hierarchical matching. First, we restrict controls to have not only a NHW congressional incumbent from 2000 to 2004, but the *same* incumbent in these elections.²⁵ Second, we drop blocks in each 2000 *assembly-senate-congress* “triplet”, where the number of

²²A detailed description of each dataset is presented in the Supplemental Appendix.

²³Relaxing restrictions for open seats and freshman incumbents does not affect our results.

²⁴Pre-matching restrictions discard about 100,000 blocks, resulting in 4,802 blocks and 197,354 control blocks. From this, we match on P-HVAP to produce BASELINE and match on the full “conditioning set” (listed in Table 1) to produce MATCHED. We lose 115 pairs from BASELINE and 16 pairs from MATCHED converting 2010 to 2000 blocks, with final samples of 9,374 and 856 blocks.

²⁵This discards about 50,000 (25%) additional controls.

control blocks was less than twice the number of treated blocks.²⁶ To further reduce imbalances in important covariates, we drop treated blocks with pre-redistricting variation outside the common support of controls in X within each triplet, shrinking the number of treatments from 4,687 in BASELINE to 428 in MATCHED. These more than 4,000 blocks were discarded because the available controls were too dissimilar in their pre-treatment characteristics to provide quality matches.²⁷

The last step for MATCHED was to match these 428 treatments to comparable controls using hierarchical matching. First, treatment and controls were matched *exactly on triplet indicator*, ensuring that the characteristics of lower districts and the ethnicity of their incumbents cannot affect our inferences. Treatment and controls *within each triplet* then were matched on important characteristics denoted the “conditioning set” in Table 1. Finally, hierarchical matching selected matched-pairs within triplets that maximize balance *across* triplets. As seen in Table 1, hierarchical matching in MATCHED made treatment and control blocks much more similar in their characteristics used during redistricting. All mean differences on the *conditioning set* are statistically insignificant (smallest p -value is 0.71) and the entire distributions are more similar than in BASELINE as measured by ks -tests (although the smallest p -value is 0.01).²⁸

6.1 Main Results

The effect of being redistricted from a NHW to an Hispanic incumbent on Hispanic participation (or ATT^{WH}) can be estimated under Assumption (2) or Assumption (3). Although both assumptions are untestable, we can assess which assumption may be more appropriate for this

²⁶For example, before redistricting, a treated block could be contained simultaneously in Congressional district 45, Assembly district 68, and State Senate district 34. Such a block would be identified as a member of the “cd45-ad68-sd34” triplet. This treated block could only be matched to control blocks within the “cd45-ad68-sd34” triplet, and only triplets with twice as many controls as treateds are retained during matching. This restriction ensures a large enough reservoir of control blocks to match to treatment blocks within each triplet.

²⁷After trimming and matching, our final dataset retains blocks in 92% of all 138 districts with NHW incumbents, and 81% of all 31 districts represented by an Hispanic incumbent, across the three jurisdiction-levels. See Supplemental Appendix for more details.

²⁸Although we discard many blocks, balance tests show that our MATCHED dataset is sufficiently powered to discern mean differences as small as 2% at $p < 0.05$ (e.g., Democratic Registration in 1998 and 2000, Proportion of the Population Aged 25 to 44), and 1% at $p < 0.1$ (e.g., Proportion Population Female).

study. Before redistricting, treatment and control units face the same House, assembly and state senate incumbents. If assumption (2) holds, no significant differences should be observed between would-be treatment and would-be control blocks on pre-redistricting Hispanic turnout or registration, controlling for characteristics used during redistricting. This observation suggests a “placebo test”, which evaluates whether controlling for X is enough to recover this true zero effect on prior participation, as required for valid cross-sectional comparisons.

The results of this placebo are shown in Table 2. Just controlling for P-HVAP in BASELINE, there is a significant difference in 2000 Hispanic turnout and registration *before redistricting occurs*. In spite of having the same U.S. House incumbent in 2000 and similar proportions of Hispanic adults, other differences between would-be treatments and would-be controls result in differences in prior Hispanic participation. The bias is not small: in the treatment group, Hispanics vote at a rate of 65%, but only at 60% among controls. Treatment blocks in BASELINE also have significantly higher Hispanic registration, and significantly higher non-Hispanic turnout and registration.

The placebo results indicate that trimming and hierarchical matching in MATCHED greatly reduce this redistricting-induced bias. In Table 2, average Hispanic turnout in 2000 is 69% among treatments and 68% among controls, a difference statistically indistinguishable from zero (p -value of 0.52).²⁹ The average difference in Hispanic registration shrinks as well, although still statistically significant (p -value of 0.04). Non-Hispanic registration rates are now very similar, and non-Hispanic turnout is statistically indistinguishable from zero. The QQ-plot in Figure 4 presents placebo results for 2000 Hispanic turnout, comparing would-be treatment and would-be control blocks in MATCHED. Although turnout quantiles are much more similar, some differences remain. As shown in the figure, would-be treatments are still slightly larger than would-be controls, particularly in the lower quantiles of the distributions. Taken together, these results suggest that procedures implemented in MATCHED were successful in removing much of the bias. However, some bias remains, as evidenced by still significant differences in Hispanic registration and small

²⁹We also cannot reject the null that the treatment and control groups have the same prior turnout distribution with a ks -test p -value of 0.21.

differences in Hispanic turnout.

We thus focus on estimating *difference-in-differences* defined in equation (4). Table 3 shows the *difference* in participation for treatments and controls, differencing before and after redistricting. As can be seen, the 2004-2000 differences and the 2002-2000 differences in Hispanic turnout generally are small or negative across every analysis. In BASELINE, the 2002-2000 differences actually show a greater *drop-off* in Hispanic turnout in treated than in control blocks in the midterm election following redistricting. Though by the 2004 general election, treated blocks exhibit a small, positive difference (4.6%) in turnout above control blocks.³⁰

After conditioning on important covariates in MATCHED, however, all turnout differences become statistically indistinguishable between treatment and control for both the 2004 general (1.4%, p -value is 0.84) and the 2002 midterm (1.3%, p -value is 0.75).³¹ This evidence strongly suggests that Hispanic citizens in comparable blocks do *not* vote in greater numbers by virtue of being moved to districts represented by co-ethnic politicians. Moreover, the finding is bolstered by the fact that we actually recover inconsistent (both positive and negative) results in BASELINE, simply by restricting our comparisons to blocks that *could have been redistricted* to MM districts with Hispanic incumbents.³² In addition, we find no co-ethnic representation effect on Hispanic registration for either the 2002-2000 or the 2004-2000 differences.

The most consistent finding is that being moved to an Hispanic representative has a small negative effect on non-Hispanic turnout. The 2002-2000 differences in non-Hispanic turnout across treated and control are -6.2% in BASELINE and -5.6% in MATCHED. Similar results are found for the 2004-2000 differences, with -14.5% and -8.2% differences for treated and control in BASELINE and MATCHED. The BASELINE results reach statistical significance ($p < 0.001$) and are large in magnitude, however, the results in MATCHED are smaller and fail to obtain statistical significance

³⁰Cross-sectional analysis recovers similar null results. After matching, the difference in 2002 Hispanic turnout between treated and control blocks is 2.7% (p -value of 0.47), and the difference in 2004 is 2.4% (p -value of 0.74).

³¹We also look at 2006-2000 differences and find similar null effects.

³²To address concerns about generalizability to competitive settings, we replicate our analysis for districts where Hispanic candidates ran in *open-seat* races in 2002 (Congressional Districts 21 and 39). The open seat analysis confirms our main findings, providing little evidence that the opportunity to elect new Hispanic candidates in open races increases Hispanic turnout or registration. See the Supplemental Appendix for more details.

at the $p < 0.1$ level. Again, the findings for non-Hispanic registration are inconsistent.

6.2 Sensitivity to Observed Imbalances

In observational work on minority empowerment, researchers assume that units with similar values of predetermined characteristics X , have the same probability of being represented by Hispanic incumbents, justifying a comparison of treated and control blocks with similar X . Since we observe much of the redistricting process, including the data used by redistrictors, we think this assumption is plausible here. However, it may not be possible to find such similar units *in a particular sample*. Indeed, as we show for California, it is quite difficult to find redistricted and non-redistricted blocks with similar values in their pre-redistricting covariates, even when these share the same prior incumbent and legislative-district histories. This lack of common support across treated and control groups can lead to biased inferences.

In such cases, researchers often are forced to make a tradeoff: trim observations to eliminate imbalances *or* assume imbalances on X are ignorable (i.e., uncorrelated with Hispanic participation). Since we find it implausible for the placebo differences in 2000 turnout (4.2%) and registration (9.4%) in BASELINE to be uncorrelated with later participation, we trim and match to reduce this bias when estimating the co-ethnicity effect. Yet, these estimates are only valid for the small subset of treatment and control blocks that are comparable, and do not tell us what effect we would observe if we could eliminate all pre-redistricting differences for the whole population. Thus, there is a potential tradeoff between the validity and generality of our findings.

To address this tradeoff, we develop a sensitivity analysis that tests whether the observed imbalances that remain in X across treated and control could account for the positive Hispanic participation ‘effects’ we observe, *regardless of whether any blocks are discarded or matched*. In the test, we estimate the probability of block i being redistricted given the X – the *propensity score* –, before and after matching. We then construct a measure of “overt” bias from this score. We transform the propensity score into an odds-ratio Γ , which is the median predicted odds of receiving treatment among treated blocks, divided by the median predicted odds of receiving treatment among control

blocks. This information is then used in a [Rosenbaum \(2002\)](#) sensitivity analysis, which tests the null hypothesis of no effect in the presence of bias of magnitude up to Γ .³³ If estimates are sensitive to imbalances (i.e., we fail to reject the null hypothesis of no effect in the presence of overt bias), we conclude that the participation differences we observe could have emerged merely due to bias, and not to any co-ethnicity effects.

Our estimated Γ indicates that treated and control blocks become more comparable as we trim and match the data. Γ shrinks from 2.10 in BASELINE to 1.24 in MATCHED, which is closer to what we would expect under random redistricting ($\Gamma = 1$). This analysis provides additional evidence *against* a co-ethnicity effect for the entire population of blocks in California. In both MATCHED and BASELINE, we consistently fail to reject the null hypothesis of no effect of being moved to an Hispanic incumbent on subsequent Hispanic registration *and* turnout for both 2004-2000 and 2002-2000, given remaining pre-redistricting differences. P-values for BASELINE 2004 and 2002 registration and turnout differences are all greater than 0.99, and p-values for MATCHED 2004 and 2002 registration and turnout differences are all greater than 0.30 – see details in Section 6 of Supplemental Appendix. This means we find no minority turnout effects, given the degree of overt bias that remains in the way blocks were assigned new Hispanic congressional incumbents in the 2000 redistricting cycle, regardless of whether any blocks are discarded.

7 Conclusion

Majority-minority districting brings unquestionable benefits to minority representation in the U.S. Yet, there is an ongoing debate over whether MM districts, and the minority representatives that are typically elected in these districts, have any positive effect on minority participation. Contrary to previous work, we find that being represented by a co-ethnic incumbent has little impact on Hispanic turnout or registration in elections following California’s 2000 redistricting. We show that the non-random way in which minority-represented legislative districts are constructed results

³³See Supplemental Appendix for further details on the sensitivity test.

in a spurious positive correlation between MM districts and minority turnout in the state that has received the greatest scholarly attention on the topic. We analyze the behavior of Hispanic voters who are most likely to exhibit co-ethnic empowerment according to previous research. Though more work is needed to assess the generality of this null finding, the lack of effects for Hispanics suggests that other racial or ethnic groups are similarly unlikely to experience any co-ethnic participation benefits. [Keele and White \(2011\)](#) use our design to study co-ethnic turnout in black MM districts, finding similar null results.

We characterize significant biases that persist in estimating the effects of co-ethnic legislators on minority turnout. We also discuss possible mechanisms that could explain why higher participating minorities get redistricting to majority-minority districts, notably to help protect incumbents or avoid VRA preclearance objections. Though these are possible ways that racial redistricting could create spurious differences so pronounced in California, we do not assert these constitute an exhaustive theoretical account. While we focus on one state, we develop a logic of racial redistricting that predicts similar biases may emerge elsewhere, which will be an issue not only in studies using redistricting as a research design, but in any study examining the association between co-ethnic representation and minority participation. Additional work is needed to assess how general this bias is and precisely what factors explain it.³⁴ Researchers, as a matter of course, should verify whether any such process is at play in future work on minority participation. Further research is also needed to clarify whether null results for California generalize to other states and political offices, given spurious differences originating in the construction of minority jurisdictions.

In drawing broader conclusions from our findings, however, we note that our results cannot address the effects that a significant reduction in the number of MM districts would have on Hispanic participation. For instance, Hispanic voters may judge the fairness of the political system in part based on the degree of Hispanic representation in Congress or in state legislatures. Any policy change that had the effect of dramatically reducing the number of elected minority representatives

³⁴Intriguingly, [Fraga \(2012\)](#) finds *no* association between Hispanic candidacy and co-ethnic participation across House districts nationally. We argue that previous significant California results were driven by the selection process we uncover. [Fraga's \(2012\)](#) null result could be driven by a combination of selection processes across states, with some redistrictors moving *lower turnout* Hispanics into MM districts, similar to the 2010 Texas plan.

(for instance, the elimination of MM districts), could erode minorities' trust in the legitimacy of the political system, which could then cascade into significant declines in minority participation. Alternatively, the resulting outrage could increase minority political mobilization. The research design and evidence we consider here is not well-suited to address this type of global effects of major changes in descriptive representation, but neither are any of the previous studies of the effects of co-ethnicity on minority participation. Our findings do speak directly to these previous results, showing that majority-Hispanic districts do not have positive effects on Hispanic turnout stemming from marginal (rather than total) changes to Hispanic representation.

Our study also makes important contributions to research design. We develop a unique approach to reduce biases in estimating empowerment effects amongst voters residing in overlapping minority districts. Our design exploits variation induced during redistricting to identify blocks of minority voters differing as little as possible except MM district-assignment. We develop a novel hierarchical matching method that constrains comparisons so blocks are only matched if they share the same prior legislative incumbents *and* are similar in characteristics used during redistricting. We validate our design through a placebo on pre-redistricting participation outcomes not used during matching. We then estimate difference-in-differences to adjust for unobserved constant differences. These are weaker identification assumptions than previous work has relied on. Of course, we are using observational methods, and biases may remain.

Finally, we develop a remedy for the frequent trade-off researchers make in trimming observations to improve balance and retaining external validity. To find comparable units, we discarded 91% of blocks moved to new Hispanic incumbents. A possible objection is that an empowerment effect could be present in the whole population, but not in our trimmed subsample. We conduct sensitivity analysis to consider this possibility. The tests show that a zero effect cannot be rejected given the considerable imbalances in observable characteristics, regardless of whether any block data was discarded. This type of empirically-oriented sensitivity analysis can help future scholars in dealing with similar trade-offs between the generality and validity of their findings.

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Figure 1: QQ Plot of 2004 Hispanic Turnout for California Blocks In and Not In Hispanic Majority-Minority Districts – Matched on P-HVAP

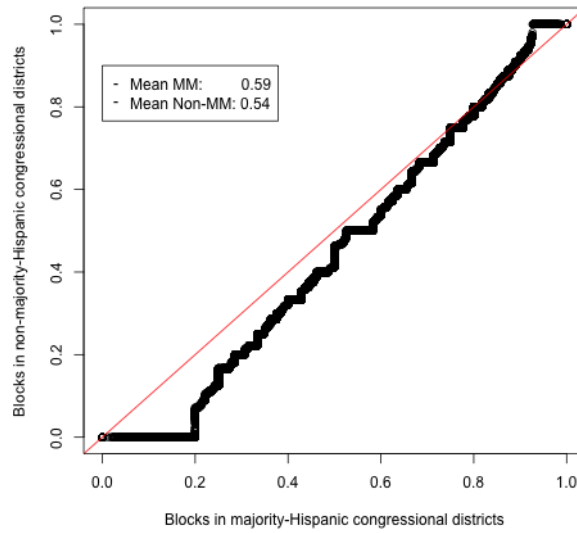


Figure 2: QQ Plots of 2000 Hispanic Turnout Rates for California Blocks To Be Moved From NHW Incumbents To Hispanic Incumbents After Redistricting– Matched on P-HVAP

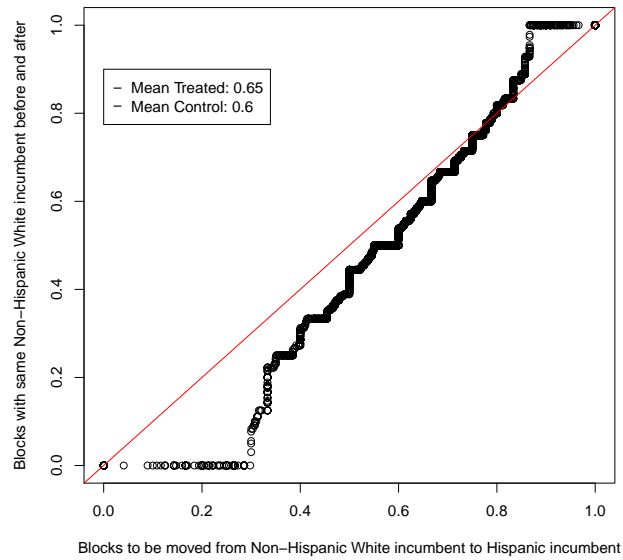
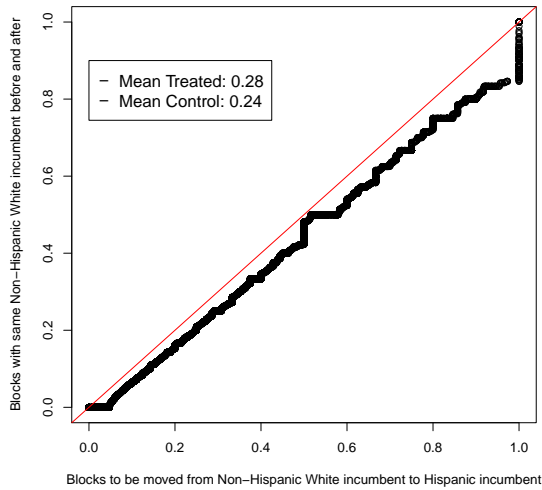
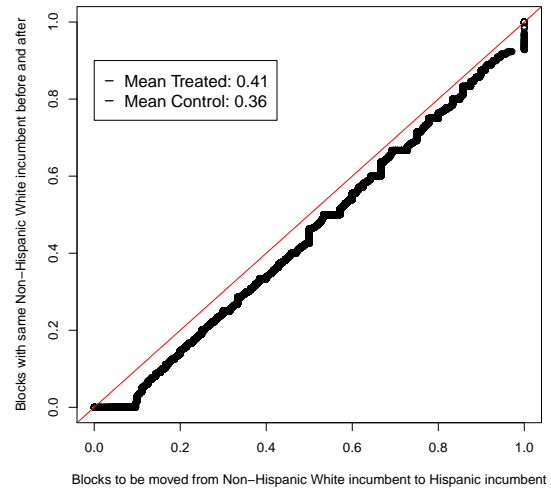


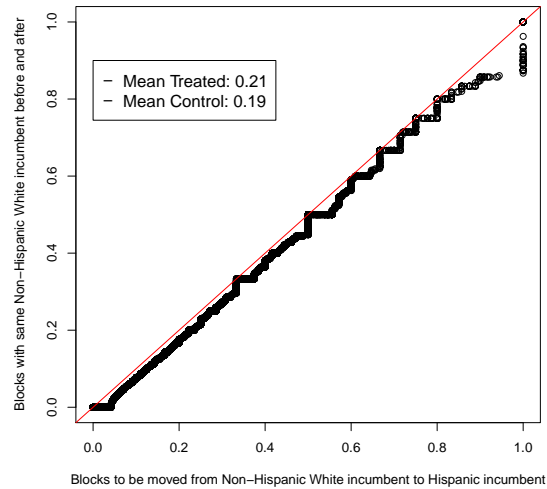
Figure 3: QQ Plots of 1990 and 2010 Hispanic Participation for California Blocks To Be Moved From NHW Incumbents To Hispanic Incumbents After Redistricting in 1990 or 2010 – Matched on P-HVAP



(a) 1990 REGISTRATION



(b) 2010 REGISTRATION



(c) 2010 TURNOUT

Figure 4: QQ Plots of 2000 Hispanic Turnout Rates for California Blocks To Be Moved From NHW Incumbents To Hispanic Incumbents After Redistricting – Matched on Conditioning Set

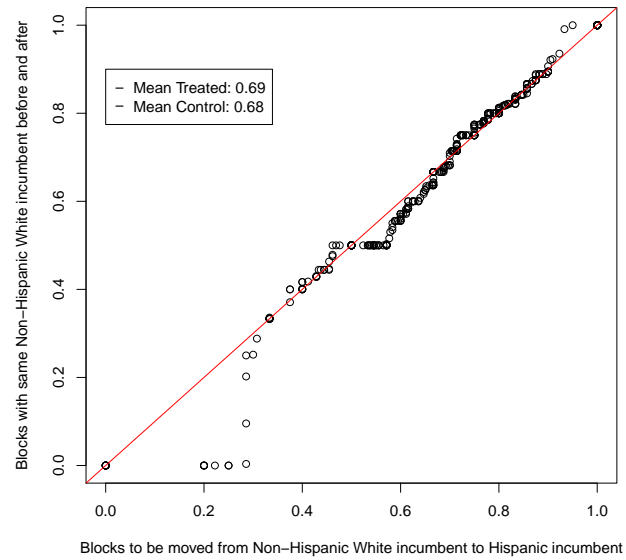


Table 1: Covariate Balance for Baseline and Matching Analysis

| | BASELINE | | | MATCHED | | |
|------------------------------|----------|-------------|------------|---------|-------------|------------|
| | Mean | P-value | | Mean | P-value | |
| | Tr - Co | μ -test | ks -test | Tr - Co | μ -test | ks -test |
| CONDITIONING SET | | | | | | |
| Voting Age Population (VAP) | 9.79 | 0.00 | 0.00 | 1.48 | 0.78 | 0.79 |
| Black VAP | 0.02 | 0.00 | 0.00 | 0.00 | 0.75 | 0.99 |
| Hispanic VAP | 0.00 | 1.00 | 1.00 | 0.00 | 0.77 | 0.16 |
| HH Income 39k | -0.06 | 0.00 | 0.00 | 0.00 | 0.89 | 0.10 |
| HH Income 40k to 74k | 0.02 | 0.03 | 0.00 | 0.00 | 0.97 | 0.04 |
| HH Income \geq 100k | 0.02 | 0.17 | 0.00 | 0.00 | 0.95 | 0.01 |
| Pop Highschool or Less | -0.01 | 0.00 | 0.00 | 0.01 | 0.71 | 0.06 |
| Pop Foreign | 0.04 | 0.00 | 0.00 | 0.00 | 0.90 | 0.10 |
| ADDITIONAL COVARIATES | | | | | | |
| Pop Non-Citizen | 0.00 | 0.06 | 0.00 | 0.00 | 0.76 | 0.08 |
| Pop Naturalized Citizen | 0.04 | 0.00 | 0.00 | 0.00 | 0.82 | 0.00 |
| Registration Total 1998 | 0.03 | 0.00 | 0.00 | 0.02 | 0.14 | 0.11 |
| Registration Hispanic 1998 | 0.03 | 0.00 | 0.00 | 0.00 | 0.38 | 0.13 |
| Registration Democrat 1998 | -0.01 | 0.02 | 0.00 | 0.02 | 0.05 | 0.00 |
| Registration Republican 1998 | 0.02 | 0.00 | 0.00 | -0.01 | 0.60 | 0.79 |
| Registration Total 2000 | 0.04 | 0.00 | 0.00 | 0.02 | 0.08 | 0.02 |
| Registration Democrat 2000 | 0.01 | 0.00 | 0.00 | 0.02 | 0.05 | 0.00 |
| Registration Republican 2000 | 0.00 | 0.97 | 0.00 | 0.00 | 0.68 | 0.07 |
| Dem. Vote U.S. Senate 1998 | -0.13 | 0.00 | 0.00 | -0.04 | 0.25 | 0.41 |
| Dem. Vote Governor 1998 | -0.13 | 0.00 | 0.00 | -0.05 | 0.17 | 0.02 |
| Dem. Vote U.S. House 1998 | -0.12 | 0.00 | 0.00 | -0.06 | 0.08 | 0.41 |
| Dem. Vote President 2000 | 0.01 | 0.41 | 0.00 | 0.00 | 0.92 | 0.07 |
| Dem. Vote U.S. Senate 2000 | -0.01 | 0.19 | 0.00 | 0.00 | 0.84 | 0.07 |
| Dem. Vote U.S. House 2000 | -0.05 | 0.00 | 0.00 | -0.01 | 0.56 | 0.07 |
| Pop Female | 0.02 | 0.00 | 0.00 | 0.01 | 0.09 | 0.00 |
| Pop 25 to 44 Years | -0.01 | 0.00 | 0.00 | -0.02 | 0.00 | 0.00 |
| Pop 45 to 59 Years | 0.00 | 0.00 | 0.00 | 0.00 | 0.60 | 0.57 |

Table 2: Intermediate Analysis: 2000 Cross-Sectional Results

| | 2000 | |
|---|---------|---------|
| | Tr - Co | P-value |
| BASELINE | | |
| Hispanic Turnout | 0.042 | 0.00 |
| Hispanic Registration | 0.094 | 0.00 |
| Non-Hispanic Turnout | 0.012 | 0.00 |
| Non-Hispanic Registration | 0.030 | 0.00 |
| MATCHED | | |
| Hispanic Turnout | 0.011 | 0.52 |
| Hispanic Registration | 0.038 | 0.04 |
| Non-Hispanic Turnout | 0.009 | 0.41 |
| Non-Hispanic Registration | -0.017 | 0.29 |
| P-values are standard t-test probabilities. | | |

Table 3: Difference-in-Difference Estimates: 2004-2000 and 2002-2000 Difference Results

| | 2004 | | 2002 | |
|---------------------------|---------|---------|---------|---------|
| | Tr - Co | P-value | Tr - Co | P-value |
| BASELINE | | | | |
| Hispanic Turnout | 0.046 | 0.02 | -0.034 | 0.00 |
| Hispanic Registration | 0.012 | 0.10 | 0.009 | 0.20 |
| Non-Hispanic Turnout | -0.145 | 0.00 | -0.062 | 0.00 |
| Non-Hispanic Registration | -0.013 | 0.07 | 0.003 | 0.63 |
| MATCHED | | | | |
| Hispanic Turnout | 0.014 | 0.84 | 0.013 | 0.75 |
| Hispanic Registration | 0.032 | 0.21 | -0.008 | 0.76 |
| Non-Hispanic Turnout | -0.082 | 0.24 | -0.056 | 0.10 |
| Non-Hispanic Registration | 0.033 | 0.15 | 0.037 | 0.11 |

P-values are OLS with Huber-White standard errors and interacted time fixed effects.