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Redistricting and the Causal Impact of Race on Voter Turnout

Bernard L. Fraga, Indiana University

Recent work challenges traditional understandings of the link between race and voter turnout, suggesting that there is limited evidence of increased minority voting due to co-ethnic representation and majority-minority districts. Here I examine 65.3 million registration records from 10 states to trace individual-level participation before and after the 2012 round of redistricting, testing whether a shift in congressional representation, candidacy, and/or district ethnic composition affected an individual's decision to participate. Separating results for non-Hispanic white, black, Latino, and Asian American registrants, I find that individuals change their behavior in response to ethnoracial context, with African Americans more likely to vote when assigned to majority-black districts with black candidates or incumbents. White and Asian registrants also turn out in higher numbers when a co-ethnic candidate is on the ballot, but Latinos may be less likely to vote in the short term when assigned to majority-Latino districts.

The Voting Rights Act of 1965 (VRA) removed many formal barriers to African American political participation. With a substantial increase in black voter registration, attention soon turned to ensuring African American (and later Latino, Asian American, and Native American) votes would have an impact on outcomes (Gerken 2001; Grofman, Handley, and Niemi 1992). Underlying such efforts was the notion that electoral institutions, especially district boundaries, can modify the representation and influence that individuals have in the political process; in short, the political context in which one votes matters. A substantial literature demonstrates that changing the way districts are configured affects which racial/ethnic groups can elect co-ethnics to office (Branton 2009; Bullock and Gaddie 2009; Canon 1999; Epstein and O'Halloran 1999; Grofman, Handley, and Lublin 2001; Lublin 1997) and may sway in policy outcomes (Grose 2011; Swain 1993). Does mass political participation follow suit?

The study that follows examines how ethnoracial political context, understood as the racial/ethnic background of politicians and the composition of the districts they emerge from, affects an individual's propensity to turn out to vote. Existing

work addressing this issue has reached few firm conclusions: Minority candidates and elected officials seem to stimulate co-ethnic turnout in some studies (Barreto 2007, 2010; Bobo and Gilliam 1990; Griffin and Keane 2006; Rocha et al. 2010; Washington 2006; Whitby 2007) but have little effect in other work, except to depress turnout for the (non-Hispanic) white majority (Gay 2001; Henderson, Sekhon, and Titiunik 2013; Keele and White 2011). Mixed findings also appear when studying district demographic composition as distinct from minority officeholding (Barreto, Segura, and Woods 2004; Brace et al. 1995; Fraga 2015; Henderson et al. 2013). In addition, the trade-off between internal and external validity inherent in these analyses makes it difficult to assess the causal effect of district ethnoracial context on voter turnout.

I leverage the very process giving rise to these ethnoracial contexts, redistricting, to provide a clearer picture of the behavioral impact of race on voter turnout. Under certain circumstances, longitudinal analyses intersected by redistricting can estimate causal effects (Ansolabehere, Snyder, and Stewart 2000; Dunning 2012; Sekhon and Titiunik 2013). Here I examine congressional redistricting taking place in 10 states for 2012 elections, making use of a voter file database

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Data and supporting materials necessary to reproduce the numerical results in the paper are available in the *JOP* Dataverse (<https://dataverse.harvard.edu/dataverse/jop>). An online appendix with supplementary material is available at <http://dx.doi.org/10.1086/683601>.

to study individual-level voter turnout before and after incumbents, potential candidates, and constituents reacted to a new set of district boundaries. Tracing rates of participation for 65,315,053 eligible voters, I use information on each person's registration address to compare citizens who shared the same district in previous elections but find themselves in contrasting ethnorracial contexts as a result of the redistricting process. Providing distinct results for individual white, black, Latino, and Asian registrants, and incorporating detailed data on the background of congressional candidates, what results is the first comprehensive effort to quantify the immediate causal impact of redistricting, co-ethnic representation, and racial/ethnic context on individual turnout nationwide.

For African Americans, Asian Americans, and whites, I find that registrants who are assigned to a district where their ethnic group is a majority of the population, or where co-ethnics are on the ballot, are more likely to participate in the subsequent election. These results are robust to exact matching pretreatment covariates across treatment and control conditions, including turnout in the 2006, 2008, and 2010 elections. While a small part of the overall turnout equation, prevailing interpretations of the role of race in voter turnout appear validated for these groups. That said, I also demonstrate that Latinos are significantly less likely to vote when redistricted into majority-Latino districts, again with results robust to exact matching on covariates that would otherwise bias our results. Such a finding, while also appearing as a short-term effect of shifts in ethnorracial context in other studies, should encourage future work that explores intergroup heterogeneity in racial effects and their persistence over time. Taken as a whole, however, the analysis indicates that rates of participation can be manipulated by the legislation and/or political elites that determine electoral boundaries.

RACE, RACIAL CONTEXT, AND PARTICIPATION

Today we continue to witness a relationship between an individual's race or ethnicity and their likelihood of turning out to vote (Barreto 2010; Lublin et al. 2009).¹ Traditional socioeconomic explanations, while informative for some ethnic groups in past election cycles (Leighley and Vedlitz 1999; Verba and Nie 1972), have proved insufficient to explain persistent low turnout for Latinos and Asians relative

to whites (Conover 1984; Jang 2009; Lien 1994; Shaw, de la Garza, and Lee 2000) and fail to explain a decline, or even reversal, of the black-white turnout gap in recent presidential elections (Wheaton 2013). Instead, scholars have turned to theories rooted in electoral institutions or context, particularly circumstances affected by the use of race in the redistricting process, to explain differences in participation associated with a citizen's racial or ethnic background.

The most common theoretical link between race and voter turnout continues to be Bobo's and Gilliam's (1990) analysis of black political participation in contrasting mayoral contexts. In what they termed the "empowerment" framework, Bobo and Gilliam assert that minority groups with "significant representation and influence in political decision making" should have increased sociopolitical participation, including voter turnout (378–79). With black empowerment operationalized as residing in a city with a black mayor, they find increased turnout for individual African Americans in the 1980 and 1984 election relative to black respondents without co-ethnic mayors. Though rooted in African Americans' response to co-ethnic officeholding in large cities, Bobo and Gilliam (1990) suggest their theory should apply to other groups, specifically Latinos, and how whites would respond to losing co-ethnic leadership (389).

Subsequent research making use of empowerment theory focused on minority officeholding or candidacy as indicators of empowerment and, thus, higher minority turnout. Tate (1991) found Jesse Jackson's 1984 presidential candidacy increased black participation among survey respondents, while Griffin and Keane (2006), Washington (2006), and Rocha et al. (2010) found similar impacts with survey samples of blacks and Latinos descriptively represented in Congress and state legislatures. Yet we know that citizens often over-report or "misremember" the turnout decision (Ansolabehere and Hersh 2012; Silver, Anderson, and Abramson 1986). Furthermore, African Americans and Latinos may be especially likely to over-report in contexts associated with minority empowerment due to social pressure (Fraga 2015; McKee, Hood, and Hill 2012). When using official election returns, researchers find little evidence of increased participation in heavily black (Brace et al. 1995; Gay 2001) or Latino (Brace et al. 1995) precincts when co-ethnic legislators ran for reelection, though turnout did drop in white precincts in the presence of African American incumbents (Gay 2001).² On the other hand, Barreto (2007) found that Latino mayoral candidacy stimulated turnout in majority-Latino precincts in five cities, and Whitby (2007) found increased county-level

1. Though the 2012 Current Population Survey (CPS) reported African American voter turnout had exceeded white turnout for the first time (Wheaton 2013), in 2010 48.6% of eligible whites reported voting, while black turnout lagged behind at 43.5%. The same year, 31.2% of eligible (citizen) Latinos voted, and 30.8% of Asian Americans participated. 2010 CPS Data available at <http://dx.doi.org/10.3886/ICPSR31082.v1>.

2. Gay does provide evidence for a short-term, positive impact of co-ethnic associated empowerment on turnout pointing to "entrenched" black

black turnout as a result of black representation on county councils.

Minority legislators are more likely to be elected from majority-minority districts, presumably because racial bloc voting leads to the majority group gaining representation (Lublin 1997; Swain 1993; Thernstrom and Thernstrom 1997). Subsequent to the 1982 VRA amendments and the *Thornburg v. Gingles* (1986) decision, legislators were compelled to produce districting plans that enhanced the ability of minorities to elect candidates of their own choosing through the creation of such districts. As a result, some authors have operationalized empowerment as simple presence in a majority-minority district, finding that residence in electoral jurisdictions with higher proportions of African Americans and Latinos is associated with higher turnout for black and Latino individuals, respectively (Barreto et al. 2004; Fraga 2015). Candidates running in majority-minority districts likely have a strong incentive to focus get-out-the-vote efforts on minorities (Leighley 2001; Rosenstone and Hansen 1993); while minority candidates may be better equipped to mobilize co-ethnic constituents (Barreto 2010; McConnaughy et al. 2010; Rocha et al. 2010; Shaw et al. 2000), elites of any race will likely tailor strategies to district conditions (Uhlener 1989). Thus, we could imagine the primary mechanism by which turnout increases for minority groups is not an individual-level sense of empowerment brought by co-ethnic representation but rather a response to the “empowering” mobilization activities of election-seeking politicians.

While mixed when using aggregate data, the current state of the literature would suggest a relationship between individual-level minority turnout and residence in a district with minority candidates, incumbents, or majority population. But does an individual’s presence in these contexts *cause* higher turnout? As an alternative, we could imagine individuals choose to live in places where they feel that their ethnic group is dominant (Boustan 2011), especially those who may be most likely to be involved in politics. Establishing causality with observational data is notoriously difficult (Morgan and Winship 2007), but Sekhon and Titiunik (2013) demonstrate that the redistricting process can be used to estimate causal effects with electoral data. Leveraging California’s 2002 round of redistricting, Henderson et al. (2013) study the effect of Latino incumbents on Latino and white registrants but see no significant impact of placement in a Latino-held congressional district on Latino or white turnout. Keele and White (2011) also use redistricting to study black

and white turnout in Georgia and North Carolina after placement in a majority-black district and find no evidence of increased black turnout and minimal evidence of decreased white turnout. Thus a causal basis for increased minority participation resulting from co-ethnic representation, candidacy, or majority-minority districts has not been established, despite theoretical and correlational evidence for such a relationship.

RESEARCH DESIGN

The research design I use focuses on estimating the causal impact of congressional district ethnoracial context on voter turnout. Thus, I use a modified version of the methodology proposed by Sekhon and Titiunik (2013), and first implemented to study race and turnout by Henderson et al. (2013) and Keele and White (2011), to examine changes in turnout attributable to co-ethnic incumbency, general election candidacy, or the racial/ethnic composition of the district. Based on past theoretical and empirical understandings, I test the following hypotheses:

- H1.** Individuals are more likely to turn out to vote when assigned to congressional districts with a co-ethnic incumbent, *ceteris paribus*.
- H2.** Individuals are more likely to turn out to vote when assigned to congressional districts where a co-ethnic candidate seeks office, *ceteris paribus*.
- H3.** Individuals are more likely to turn out to vote when assigned to congressional districts where their ethnic group composes a majority of the voting-age population, *ceteris paribus*.

As noted previously, past work has operationalized ethnoracial context through the race of incumbents, candidates, or the demographic composition of legislative districts. While all sharing reference to Bobo’s and Gilliam’s (1990) empowerment framework, distinct mechanisms may be captured by each ethnoracial context “treatment” listed above. For instance, Bobo’s and Gilliam’s (1990) “representation and influence” may be best captured by hypothesis 1, which addresses co-ethnic representation. Co-ethnic candidates, studied via hypothesis 2, may also do outreach to their ethnic constituents and thus increase psychological engagement with the election at hand (Barreto 2010, 7). Finally, mobilization by election-seeking politicians may indicate that district composition is key, as featured in hypothesis 3 (Fraga 2015). In practice, these contextual factors are highly correlated, as the presence of each is likely to be influenced by, if not directly attributable to, the use

representation as one possible explanation for limited increases in African American participation.

of race in the congressional redistricting process (Canon 1999).³ Therefore, a methodological strategy taking advantage of redistricting is likely to help us understand when and for whom ethnoracial context affects participation, whichever way we operationalize the concept. Below I describe my research design in greater detail and how it addresses three key barriers previous work has faced when examining ethnoracial context and political behavior.

Ignorability

Prior studies of racial context and voter turnout are generally unable to identify the causal impact of co-ethnic incumbency, candidacy, or district demographics. One reason is the inability of most observational analyses to account for outside factors that could influence our results in a systematic fashion (Dunning 2012). Should such confounders persist, we may wrongly attribute a change in outcomes (or lack thereof) to the features we are investigating. When using survey data in a regression context, we often include “control” variables in an attempt to account for traits that differ between individuals; when we fail to do so, we say the result may be subject to omitted variable bias. In the potential outcomes framework, this has been labeled selection bias (Morgan and Winship 2007) or a violation of the ignorability assumption of no difference between “treatment” and “control” populations, who should be similar in all respects aside from variation on our factor of interest (Rubin 1978).⁴ How might such confounders appear in the study of race and participation, and how can we better account for these bias-inducing factors?

A key source of selection bias when analyzing the impact of district ethnoracial context is, not surprisingly, “self-selection:” individuals may choose to live in contexts of empowerment.⁵ In an experiment, we would randomly assign individuals to live in a treatment district, and compare their turnout rate to individuals in a control district. We could then be certain that, on average, there are no observable or unobservable differences between populations based on treat-

ment status. As such an action is implausible, I instead leverage the redistricting process, which has the potential for *as-if* random assignment.⁶ Since most citizens are not in direct control of the boundary-making process, redistricting largely removes self-selection as a source of bias. Redistricting has been used to make causal inferences regarding the incumbency advantage (Ansolabehere et al. 2000; Sekhon and Titiunik 2013) in addition to the impact of district context on turnout. Comparing individuals who resided in the same district before redistricting, but were later assigned to different districts for the election after redistricting, we can plausibly identify the average effect of a change induced by the redistricting process on individual behavior.⁷

Though perhaps having features similar to random assignment, especially from the perspective of individuals receiving treatment, redistricting is not itself a random process. Political elites establish the geographic boundaries that determine assignment to treatment and control conditions. Historically, partisan state legislators established congressional district boundaries, within equal population constraints established in *Reynolds v. Sims* (1964). The Voting Rights Act of 1965, along with subsequent amendments and court rulings, also produced mandates that continue to influence the redistricting process (Levitt 2010). While state legislators should not be seen as unconstrained when setting electoral boundaries,⁸ it is worth considering elite manipulation of districts as a possible factor influencing treatment selection. Henderson et al. (2013) provide evidence from both statistical analyses and elite interviews demonstrating that those in charge of the 2002 California redistricting process placed high-turnout, heavily Latino census blocks into majority-Latino districts.⁹ On the other hand, evidence has emerged suggesting that Texas Republicans “intentionally” placed heavily Latino blocks with low rates of turnout in 2006, 2008, and 2010 into majority-Latino districts for 2012 (Li 2012). Even after dealing with self-selection, nonrandom processes may thus affect our results (Rubin 2008).

3. As the racial identity or makeup of incumbents, candidates, or jurisdictions is not itself randomly assigned, this study does not isolate the impact of these specific factors from other district-level contextual attributes that may be associated with ethnoracial context. Instead, the study examines individual-level responses to “exposure” to the set of attributes that accompany a randomly-assigned change in ethnoracial context (see Sen and Wasow 2016). Robustness checks offered in the appendix address alternative hypotheses and causal pathways, however. I thank an anonymous reviewer for suggesting this clarification.

4. See the appendix for associated mathematical notation.

5. This is along with any number of plausible, yet difficult to measure, nonignorable differences between those who choose to live in one congressional district versus another.

6. Scholars who leverage redistricting may classify their analyses as natural experiments (Dunning 2012, 44–45, 59–60). While the design I outline below does share some features of a randomized controlled trial, I ask readers to note the advantages of the design over previous research, while keeping in mind that the analysis is fundamentally observational rather than experimental.

7. See the appendix for associated notation and a detailed illustration of how district boundaries may intersect across redistricting plans.

8. For instance, when examining the incumbency advantage, Ansolabehere et al. (2000) find little evidence that legislators are able to skew districts systematically in a fashion that would favor their proximate interest: reelection (23–24).

9. For 2012, an independent redistricting commission crafted districts in California, potentially eliminating this source of bias.

To deal with nonrandom assignment, past work uses a matching algorithm to match treated units to control units.¹⁰ While making treatment and control groups more comparable on potential confounders, the “selection on observables” assumption inherent in a matching design may miss any number of unobservable pretreatment differences between groups. One way of addressing this problem is to estimate the “difference in differences” (DID), comparing turnout pre- and post-redistricting for both treatment and control groups, and assuming a parallel trend in the dependent variable absent treatment (Keele and Minozzi 2013). With multiple observations for the same individual over time, I exact match individuals on previous turnout, ensuring balance on unobservable characteristics correlated with my (lagged) outcome of interest, and making the initial “difference” for the DID model precisely 0 (Athey and Imbens 2006; Keele and White 2011).¹¹ I also exact match registrants on age group, gender, and, inherent in the design of the study, race/ethnicity and pre-2012 congressional district.

Modifiable areal unit problem

If we are interested in understanding individual responses to district ethnoracial context, an analysis that makes use of aggregate data will face the modifiable areal unit problem (MAUP). Acknowledged as a difficulty in establishing causality since at least the 1930s (Gehlke and Biehl 1934), the MAUP may be described as “a geographic manifestation of the ecological fallacy where inference based on data aggregated to a particular set of geographical regions may change if the same data are aggregated to a different set of geographical regions” (Gotway Crawford and Young 2004). Early work demonstrated that the relationship between levels of aggregation and inferences about the size and/or direction of causal effects are difficult to establish, and hence account for, *ex ante* (Fotheringham and Wong 1991; Openshaw 1984). More recent work proposes that the only way to “solve” the MAUP is to use individual-level data that can be aggregated to the level of interest to the researcher (Weeks

2004).¹² The MAUP suggests mean turnout in aggregate units likely serves as an unreliable indicator of individual behaviors within those units, and thus I use geocoded individual-level data culled from the voter file in my analysis. While treatment or control designation for each individual is assigned at the district level, the inferences made in this study are about the impact of these district-level features on individual behavior.¹³

An inference strategy using redistricting creates further complications, however. In the idealized redistricting methodology, some individuals are “moved” to a different district, while others are “left” in their previous district. As Sekhon and Titiunik (2013) note: “Redistricting induces variation in at least two dimensions: a time dimension, as voters vote both before and after redistricting, and a cross-sectional dimension, as some voters are moved to a different district while others stay in the district they originally belonged to” (3). However, the reality of redistricting is often far more nuanced. Consider figure 1, which displays California congressional districts in the Los Angeles area for the 2010 and 2012 elections. While California did not gain or lose any congressional seats as a result of Census 2010-derived apportionment, nearly all of the district boundaries shifted in both size and shape; it is difficult to determine which of the 2010 districts correspond to the 2012 districts, as all districts have changed shape and all voters face a “different” district in some sense.

Phrased in terms of the MAUP, the areal units we would like to compare have been modified with introduction of the 2012 plan. Unless we define our treatment and control groups in some other fashion, we will not be able to establish whether differences are attributable to the shift in areal units or a treatment of interest. Since it is insufficient to analyze “movers” versus “stayers,” I instead use treatment designation to establish my comparison populations. Restricting comparison to those who were in the same district prior to redistricting, I consider treated individuals to be those assigned to a 2012 district with a co-ethnic incumbent (hypothesis 1), candidate (hypothesis 2), or where their ethnic group comprises a majority of the voting-age population (hypothesis 3). Control individuals are assigned to districts without these features. Such a method of defining treatment and control populations

10. Henderson et al. (2013) use a hierarchical genetic matching algorithm to match treated census blocks to control blocks, trimming 91% of blocks that did not have a treated pair comparable on observable traits. Owing to the magnitude of the individual-level data, I do not face such a stark trade-off between internal and external validity.

11. Exact matching on previous turnout has two additional benefits: (a) turnout at $t - 1$ has been shown to be an excellent predictor of participation at t (Plutzer 2002) and (b) evidence suggests aggregate measures of previous turnout are used in the treatment assignment process by redistrictors (Henderson et al. 2013; Li 2012). Both of these benefits provide support for the untestable assertion that $(Y^1, Y^0) \perp D \mid X$. See the online appendix for more details of the matching procedure.

12. King (1997) proposes ecological inference as a solution, which also involves making inferences about individual behavior rather than comparing aggregate results directly.

13. However, if the focus of the study was on discerning which level or type of context impacts behavior (Cho and Rudolph 2008) or how various populations change their behavior when in close proximity, it would be important to test for effects at varying degrees of aggregation. For an application of such a strategy to racial context and political behavior, see Hersh and Nall (2015).

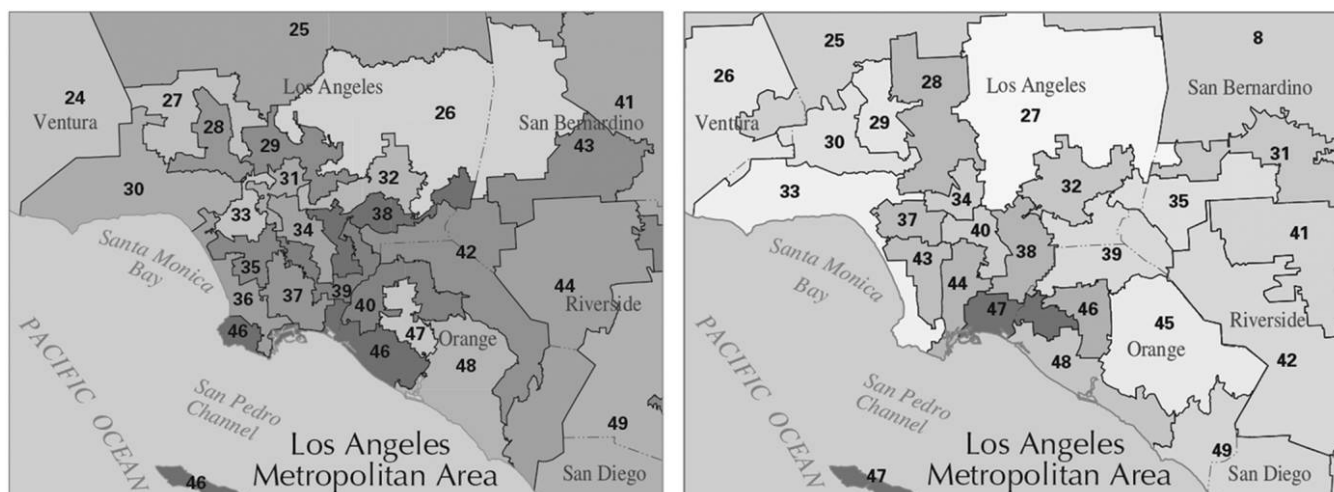


Figure 1. Los Angeles area congressional districts, 2010 and 2012 elections. Maps reflect boundaries for 112th Congress (left) and 113th Congress (right). These maps were produced by the US Geological Survey and are available at <http://nationalmap.gov/smallscale/printable/congress.html>. A color version of this figure is available online.

means that multiple comparisons may be available for groups of voters with the same 2010 district but also permits us to avoid assumptions about the persistence of certain district-level contextual factors across redistricting plans.¹⁴

External validity

Most existing analyses of the impact of minority candidacy or district composition on participation are conducted on a small number of geographies and focus on a single group's behavior. Limitations in data availability, especially at the state level, drive the decision to sacrifice some measure of external validity for the sake of internal validity (Nicholson-Crotty and Meier 2002). Yet theories of race and voter turnout developed via analysis of one ethnic group often do not generalize to other groups (Leighley and Vedlitz 1999). In this study, I analyze the behavior of non-Hispanic whites, African Americans, Latinos, and Asian Americans separately but for the same election and with the same broad universe of cases. While on its face this provides my results with greater external validity than past work, it is important to consider exactly how the analysis accounts for possible heterogeneity in effects across groups or geographies.

In 2010, there were 22 states where a single nonwhite group made up a majority of at least one congressional district. Of these states, I analyze individual-level turnout in Alabama, Arizona, California, Florida, Illinois, Mississippi, New York, North Carolina, South Carolina, and Texas.¹⁵ To-

taling 201 congressional districts for 2012, these 10 states also contain the majority of all nonwhite members of Congress, minority congressional candidates, and minority voters. In contrast, Henderson et al. (2013) examine the impact of California's 2002 redistricting on Latino and white turnout in 40 districts (though only 10 remain after matching), while Keele and White (2011) study black and white turnout in eight North Carolina and Georgia districts. Though the ideal analysis would cover at least all 22 states with majority-minority districts, this study addresses a far greater variety of contexts than previous work combined.

The fundamental unit of analysis in this work is the individual, who is part of either the treatment or control group based on the ethnoracial context found within her assigned congressional districts. To compare turnout between these groups, I designate each combination of 2012 treatment and control districts as a unique case or trial, not assuming *ex ante* that we should see consistent effects within the entire state or 10-state sample. Exact matching and analysis occurs at the level of these trials, but results are then combined to provide an overall average treatment effect on the treated across all trials in the study (Hedges and Vevea 1998). To aggregate results, the intuitive solution may be to calculate the mean of the average treatment effects on the treated found in each trial. However, to account for heterogeneous treatment effects and the possibility that trials with a greater number of individuals will provide more informative estimates of the overall treatment effect, I weight

14. See the appendix for associated mathematical notation and a diagram of treatment-control assignment with multiple comparisons.

15. The states selected provide geographic variation consistent with the distribution of majority-minority districts, information about states

formerly covered by Section 5 of the Voting Rights Act, and variation in partisan contexts. Other states were excluded due to limitations in data availability.

each trial by the inverse of the variance found when comparing treatment and control groups.¹⁶ This generates a weighted mean of the overall average treatment effect on the treated and represents the expected treatment effect for an individual registrant residing within one of the 10 states I analyze.

DATA

The goal of this study is to ascertain how voter turnout is influenced by co-ethnic officeholding, candidacy or the demographic composition of congressional districts. Information about individual-level participation is extracted from a voter file database developed by Catalist, LLC, a data vendor to political campaigns.¹⁷ Catalist acquires registered voter lists from every state, imputes all data available from the list, and then organizes and combines registration data with official election-level turnout indicators to produce approximately 225 million individual-level entries as of July 2013. While state registration lists are dynamic, with voters appearing and then disappearing as a result of list purges, Catalist maintains unique records for each individual who was registered by fall 2006 even when they are removed from state lists.¹⁸ Thus, from the perspective of a researcher interested in turnout, the data form a panel tracking every voter nationwide since 2006.¹⁹ I include only registrants whose registration date is on or before November 8, 2006, and who reside in one of the 10 states in the study, yielding 65,315,053 registrants. I then select those who were registered continuously in the same state until at least November 6, 2012, and thus were eligible to vote within the state in the 2006, 2008, 2010, and 2012 general elections. The dependent variable takes a value of 1 if the registrant voted in the 2012 general election, 0 otherwise.

Of course, the voter file contains useful indicators aside from turnout and registration date. Registrants are assigned

to precincts through their registration address, such that each individual's pre- and post-redistricting congressional district can be ascertained.²⁰ Also, registrants specify their gender and date of birth, which are added to turnout in 2006, 2008, and 2010 as conditioning variables for exact matching across treatment and control groups.

If we are interested in individual-level turnout by race, however, we need to go beyond what is usually listed in the voter file. Past studies have made use of southern states' efforts to record the race/ethnicity of registrants (Keele and White 2011; Whitby 2007).²¹ In California, where voter registration forms do not query race, last name matching to Spanish-surname lists has become a standard method of distinguishing Latinos from non-Latinos (Barreto et al. 2004; Henderson et al. 2013). Expanding on such an approach, Catalist, through a contract with CPM Ethnic, uses first, middle, and last name matching, census block contextual data, commercial information, and registrant age to predict the race/ethnicity of every registrant nationwide. Nearly every voter is predicted as either (non-Hispanic) white, black, Latino, Asian, or Native American, with 91.4% accuracy when compared to self-reports in the 2012 Cooperative Congressional Elections Study (Fraga 2015).²² Thus, the proprietary method used by Catalist and CPM Ethnic is both rooted in well-understood principles of individual race prediction (Elliott et al. 2008) and highly effective when compared to self-report.²³ I test my three key hypotheses separately for white, black, Latino, and Asian voters, making use of the individual-level race coding provided in the database.

To test hypotheses 1 and 2, I combine turnout data with detailed information on candidates for Congress. Names and election results were drawn from Federal Elections Commis-

16. Such a procedure is common when aggregating results from multiple randomized controlled trials, and is well suited to within-sample hypothesis testing (Hedges and Vevea 1998; Stanley and Doucouliagos 2015). See the appendix for associated mathematical notation and a discussion of alternative aggregation techniques.

17. Further details about the vendor may be found in Ansolabehere and Hersh (2012) and Fraga (2015). I am grateful to the Indiana University College of Arts and Sciences for funding access to the Catalist data.

18. If one were to independently acquire a state's voter file today and examine turnout in the 2010 election, many voters would not appear on the list as they have been dropped from the rolls. Thus, longitudinal analysis of individual-level registration or turnout is a great challenge to researchers wishing to avoid contracting with a third-party organization, despite the public availability of the voter file.

19. For each general election from 2006 to 2012, the number of individual records in the Catalist database indicating that the registrant turned out to vote is within 1% of the official voter count as maintained by McDonald (2012).

20. While Catalist preserves unique records for each individual at the state level, those who move within a state in the six-year timespan I examine are associated with the districts pertaining to their most recent registration address.

21. These states are AL, FL, GA, LA, NC, and SC. MS and TN do not require voters to list their race, but provide space for this purpose on the registration form.

22. Hersh (2015) also reports 87.3% overall accuracy when comparing self-report from the 2008 CCES to Catalist's race/ethnicity indicators. However, both Fraga (2015) and Hersh (2015) note that Catalist's predictions of individual race are less reliable than self-reported race available in the aforementioned southern states, and that non-Hispanic white respondents are substantially more likely to be correctly identified in the Catalist database than black, Latino, or Asian individuals.

23. As noted in Ansolabehere and Hersh (2012), Catalist placed second in a national name-matching contest. Information regarding the algorithm used by CPM Ethnic may be found at <http://cpm-technologies.com/cpmEthnic.html>. For an analysis of the (limited) ways in which misprediction of individual race can affect estimates of voter turnout at the district level, see Fraga (2015).

sion reports on all two-party congressional general election candidates from 2006, 2008, 2010, and 2012. The race/ethnicity of each candidate was then coded as either white, black, Latino, Asian, or Native American, using statements made by candidates, membership in ethnic caucuses or organizations, ethnic advocacy foundations, news articles, and both archived and live candidate websites. To examine hypothesis 1, treatment is defined as assignment to a district with a co-ethnic incumbent for 2012. The control condition is assignment to a district without a co-ethnic incumbent. All individuals who had a co-ethnic incumbent in 2006, 2008, or 2010 are removed from the analysis, such that the treatment condition represents a “change” and the control condition “no change” in terms of incumbent race. For the co-ethnic candidate treatment, hypothesis 2, treated individuals are those assigned to district where at least one co-ethnic Democratic or Republican general election candidate was on the ballot in 2012. The control condition is placement in a district without a co-ethnic candidate. Similar to the restriction used to test hypothesis 1, individuals who had co-ethnic candidates in 2006, 2008, or 2010 are not included in the analysis.²⁴

Hypothesis 3, which examines the impact of assignment to a district where the individual’s ethnic group is in the majority, makes use of Census 2010 voting-age population (VAP) data broken down by racial/ethnic group.²⁵ For each district, I calculate the proportion of the VAP that is non-Hispanic white, black, or Latino, then for each group of voters, evaluate whether turnout is higher when assigned to a majority-white, majority-black, or majority-Latino district. To ensure treatment assignment induces a veritable change in ethnoracial context, the results provided here only include registrants who resided in a district where their ethnic group was less than 30% of the district’s population before redistricting, and where a comparison can be made to con-

trol registrants who remained in a district that was also less than 30% co-ethnic.²⁶

RESULTS

Past work has found significant differences between the pre-redistricting attributes and behaviors of individuals “moved” to a new district, versus those remaining in the “old” district. In the research design outlined above, I instead define treatment and control groups on the basis of three contextual factors of interest, co-ethnic incumbency, co-ethnic candidacy, and district demographics. Tables 1, 2, and 3 indicate how treatment and control groups differ in terms of voter turnout in 2010 and 2012 elections, for each of the three treatment conditions and broken down by racial/ethnic group.²⁷

Examining the “baseline” statistics in table 1 first, where treatment is defined as assignment to a district with a co-ethnic incumbent, note that there are differences in turnout in the 2010 election for individuals who later formed the treatment and control groups. For instance, white and African American turnout pretreatment was slightly higher for whites who were later assigned to a jurisdiction with a white or black incumbent, respectively, while Latino and Asian turnout was lower for those who were later assigned to districts with co-ethnic representatives. Remember that all of the individuals in the treatment and control groups were in the same districts and had the same incumbents in 2010; if redistricting was truly random, we should see no significant difference in 2010 turnout. In other words, we may consider turnout in 2010 as a “placebo test” for the effect of redistricting, as participation in the 2010 election is a lagged version of the dependent variable (Sekhon and Titiunik 2013).²⁸ Table 1 indicates we should be concerned about nonrandom treatment assignment, at least for whites and Latinos. Turnout in the 2012 election, our quantity of interest as the election was held under the new district boundaries, would indicate that white and black turnout increased and Latino and Asian turnout decreased when not accounting for turnout propensity in 2010.

The “baseline” columns in table 2, where treatment is defined as placement in districts with co-ethnic candidates,

24. While the empirical analysis is restricted to those who were identified as residing in pre-2012 districts without a co-ethnic incumbent, candidate, or majority, some portion of a district’s population may be composed of those who moved into the pre-2012 district between 2006 and 2010. If these within-state movers moved from districts that had a co-ethnic incumbent, candidate, or majority, the “novelty” of the post-redistricting context may be reduced for those who were assigned to a treated district. Under these conditions, within-state movers may skew my estimated treatment effects toward zero.

25. I use the voting-age population rather than the citizen voting-age population or voting-eligible population, as the voting-age population is the only one of these quantities that was available for all subgroups and was measured at the same time (April 2010) for both pre- and post-2012 district boundaries. Outside of Hawaii, there were no majority-Asian congressional districts based on the 2010 Census. As a result, I do not examine hypothesis 3 for Asian American registrants.

26. The appendix offers an analysis of the impact of district demographics when operationalizing ethnic composition as a continuous variable; results are comparable across the two techniques.

27. Tables listing treatment and control group balance on age group, gender, turnout in 2006, and turnout in 2008 may be found in the appendix. All of these covariates were exact matched in the “matched” condition, along with turnout in 2010.

28. That said, tables 1–3 do not account for heterogeneity in treatment effects across district pairs. See the appendix for an alternative identification strategy that may also serve as a placebo test.

Table 1. Differences in Voter Turnout, Co-Ethnic Incumbent Treatment

	Baseline			Matched		
	Treatment	Control	<i>p</i> -value	Treatment	Control	<i>p</i> -value
White registrants						
<i>N</i>	668,781	2,077,275		667,643	2,065,943	
District pairs	148	148		148	148	
% Turnout 2010	54.13	51.65	.03	54.13	54.13	1.00
% Turnout 2012	65.46	64.25	.25	65.46	66.24	.43
Black registrants						
<i>N</i>	363,794	1,461,162		363,441	1,455,953	
District pairs	173	173		173	173	
% Turnout 2010	49.67	49.14	.57	49.63	49.63	1.00
% Turnout 2012	71.51	70.10	.14	71.55	70.65	.36
Latino registrants						
<i>N</i>	156,389	783,060		155,769	780,278	
District pairs	112	112		112	112	
% Turnout 2010	39.20	42.02	.09	39.12	39.12	1.00
% Turnout 2012	57.01	59.32	.08	57.03	56.92	.92
Asian registrants						
<i>N</i>	56,715	105,336		56,608	105,020	
District Pairs	24	24		24	24	
% Turnout 2010	55.36	58.67	.29	55.58	55.58	1.00
% Turnout 2012	65.39	68.86	.21	65.61	66.73	.68

Note. For the “Baseline” condition, “% Turnout 2010” serves as a “placebo test” for no difference pre-redistricting between treatment and control groups. In the “Matched” condition, 2010 turnout has been exactly matched such that there is balance across treatment conditions, along with a series of other demographic variables. A table listing balance on all variables can be found in the appendix. The *p*-value reflects *t*-test for aggregate differences between district pairs on 2010 and 2012 turnout, respectively.

indicate substantially lower Latino turnout pre-redistricting for those who were later assigned to districts with Latino candidates. However, large pretreatment differences in 2010 turnout for other groups do not manifest in table 2, and an initial look at turnout in the 2012 election would only suggest a decrease in turnout for Latinos.

Recall that the third hypothesis tests the causal impact of a shift in district demographics on voter turnout, with treatment defined as placement in a majority-white, black, or Latino district for white, black, and Latino registrants, respectively. Table 3 displays pretreatment differences in 2010 voter turnout even larger than what was witnessed for the co-ethnic incumbent treatment, as a placebo test of no difference in white or Latino turnout pre-redistricting clearly fails. Here, whites assigned to a majority-white district have higher 2010 turnout, on average, and Latino individuals with significantly lower prior turnout are more likely to end up in a majority-Latino district for 2012. Turnout in the election following redistricting mirrors these differences, thus providing further evidence that nonrandom assignment via re-

districting would bias our results (Henderson et al. 2013; Li 2012).

Exact matching removes pre-treatment differences across treatment and control conditions. The “matched” columns in tables 1, 2, and 3 indicate perfect balance on 2010 turnout and a set of other covariates was achieved with minimal trimming of observations, whether defined by the number of individuals (*N*) or district pairs.²⁹ Across groups and treatment conditions, less than 1% of registrants were removed to achieve balance.³⁰ Yet correcting for prior turnout shifts our

29. A placebo test of no effect in prior elections demonstrates that redistricting is also associated with differential turnout in the 2008 and 2010 elections, even after matching on participation in 2006 and both 2006 and 2008, respectively. This is consistent with the “Baseline” imbalance displayed in tables 1–3, and strong evidence that the matched results are superior estimates of the true effects of interest.

30. Exact matching was conducted via the MatchIt package in R (Ho et al. 2007). District pairs with fewer than 100 registrants in the treatment and control conditions were excluded from the analysis. Their inclusion does not change the results substantially, but estimation of the variance

Table 2. Differences in Voter Turnout, Co-Ethnic Candidate Treatment

	Baseline			Matched		
	Treatment	Control	<i>p</i> -value	Treatment	Control	<i>p</i> -value
White registrants						
<i>N</i>	81,183	155,296		81,177	155,259	
District pairs	8	8		8	8	
% Turnout 2010	48.01	49.37	.66	48.00	48.00	1.00
% Turnout 2012	67.89	66.77	.70	67.89	67.52	.85
Black registrants						
<i>N</i>	821,925	1,630,949		817,262	1,622,318	
District pairs	273	273		273	273	
% Turnout 2010	49.81	48.99	.33	49.77	49.77	1.00
% Turnout 2012	69.53	69.61	.92	69.58	69.73	.86
Latino registrants						
<i>N</i>	441,898	1,076,985		438,541	1,072,644	
District Pairs	169	169		169	169	
% Turnout 2010	41.92	44.48	.09	41.82	41.82	1.00
% Turnout 2012	59.52	61.06	.20	59.52	59.24	.81
Asian registrants						
<i>N</i>	126,590	199,507		125,548	196,818	
District Pairs	95	95		95	95	
% Turnout 2010	42.15	42.82	.75	42.00	42.00	1.00
% Turnout 2012	58.60	58.93	.84	58.57	58.47	.95

Note. For the “Baseline” condition, “% Turnout 2010” serves as a “placebo test” for no difference pre-redistricting between treatment and control groups. In the “Matched” condition, 2010 turnout has been exactly matched such that there is balance across treatment conditions, along with a series of other demographic variables. A table listing balance on all variables can be found in the appendix. The *p*-value reflects *t*-test for aggregate differences between district pairs on 2010 and 2012 turnout, respectively.

expectations regarding the impact of redistricting on voter turnout. Matched results in tables 1–3 now show little evidence that residence in districts with minority incumbents, candidates, or a large minority population depresses black, Latino, or Asian turnout when aggregating the mean level of turnout in each condition. However, some district pairs have far more individuals than others and may be more informative estimates of the average treatment effect on the treated. Recalling the earlier discussion of external validity, I account for possible heterogeneity in treatment effects across districts by considering each 2012 district pair to be a trial and combining results through inverse variance weighting. Figures 2, 3, 4, and 5 display the core results graphically, with each trial weighted by its inverse variance in the construction of a weighted mean difference in turnout, which again serves as the overall average treatment effect on the treated (ATT) for each treatment and ethnic group. The standard errors

within treatment and control groups is unreliable with small sample sizes (Dunning 2012, 171–72).

used to construct displayed 95% confidence intervals also make use of inverse variance weighting.³¹

Though not the focus of past work on race-based districting and voter turnout, the non-Hispanic white population has been included in prior analyses (Brace et al. 1995; Gay 2001; Henderson et al. 2013; Keele and White 2011). In those studies, white turnout was often found to decrease slightly when residing in a majority-minority (and hence, nonwhite majority) district. Figure 2 indicates that, before exact matching, white registrants will be more likely to turn out to vote when they have a white incumbent, candidate, or are assigned to a white-majority district for the first time. However, after matching, only the result for co-ethnic candidates is robust, as on average white turnout is 1.46 percentage points higher when comparing whites with the same pretreatment characteristics but assigned to contrasting ethnoracial contexts for 2012. Such a result is consistent with the correlation found by Fraga (2015) and may indicate that candidate race is particularly salient to white

31. See the appendix for more details regarding this procedure.

Table 3. Differences in Voter Turnout, Majority-(Group) District Treatment

	Baseline			Matched		
	Treatment	Control	<i>p</i> -value	Treatment	Control	<i>p</i> -value
White registrants						
<i>N</i>	195,825	686,529		195,665	681,596	
District pairs	50	50		50	50	
% Turnout 2010	52.74	46.39	.00	52.74	52.74	1.00
% Turnout 2012	62.06	58.53	.04	61.99	63.29	.40
Black registrants						
<i>N</i>	164,841	656,503		164,580	654,941	
District pairs	78	78		78	78	
% Turnout 2010	48.68	47.13	.35	48.54	48.54	1.00
% Turnout 2012	68.53	66.83	.31	68.57	67.83	.67
Latino registrants						
<i>N</i>	154,432	553,107		154,100	549,380	
District Pairs	99	99		99	99	
% Turnout 2010	38.90	45.35	.00	38.84	38.84	1.00
% Turnout 2012	57.15	63.71	.00	57.15	58.74	.29

Note. For the “Baseline” condition, “% Turnout 2010” serves as a “placebo test” for no difference pre-redistricting between treatment and control groups. In the “Matched” condition, 2010 turnout has been exactly matched such that there is balance across treatment conditions, along with a series of other demographic variables. A table listing balance on all variables can be found in the appendix. The *p*-value reflects *t*-test for aggregate differences between district pairs on 2010 and 2012 turnout, respectively. Results for Asian registrants not provided, as there are no majority-Asian districts in the data set.

voters after an extended period without co-ethnic representation. White turnout may decrease slightly when assigned to districts with white incumbents or majority-white districts (−0.17 and −0.46, respectively), at least when forcing balance across treatment and control groups on factors that may influence turnout.

Increased black turnout in contexts of African American officeholding gave rise to empowerment theory, though more recent work has called the causal basis of those findings into question. Leveraging the redistricting process, figure 3 confirms a significant impact of black officeholding on voter turnout. On average, African American registrants were 0.84 percentage points more likely to participate in the 2012 election when assigned to a congressional district with a black incumbent member of Congress, relative to black registrants who had the same 2010 district, prior turnout rate, and demographic profile. Interestingly, a similar effect is not found for black congressional candidacy, with the overall ATT neither substantively nor statistically significant. Assignment to a majority-black district, however, increases turnout at a rate comparable to gaining co-ethnic representation (+0.75).

In 2012, nearly every majority-black district also had an African American incumbent; we should not be surprised that operationalization of ethnoracial context through ei-

ther of these factors leads to substantively similar results. Yet a co-ethnic candidate does not appear to be enough to stimulate black turnout. What about in places where an African American candidate is likely to win office, even if the candidate is not an incumbent? At the bottom of figure 3, I provide a conditional average treatment effect on

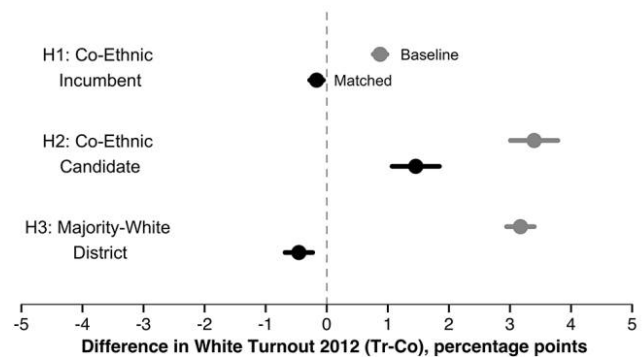


Figure 2. Overall average treatment effect on the treated (ATT), white registrants. Points indicate the difference in turnout between the treatment group and control group when using co-ethnic incumbency, candidacy, or a majority-white district to define treatment. Results reflect inverse variance weighted means of the average treatment effect on the treated, with a 95% confidence interval extending outward. Gray points denote the baseline (unmatched) analysis, black points the results after exact matching individuals across treatment and control conditions on previous turnout, age, and gender.

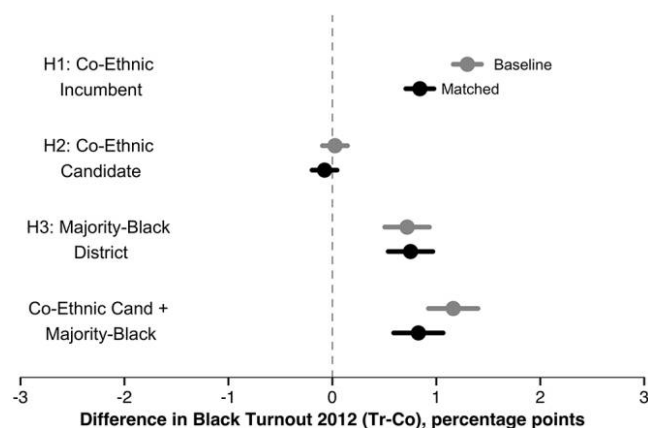


Figure 3. Overall average treatment effect on the treated (ATT), black registrants. Points indicate the difference in turnout between the treatment group and control group when using co-ethnic incumbency, candidacy, or a majority-black district to define treatment. Results reflect inverse variance weighted means of the average treatment effect on the treated, with a 95% confidence interval extending outward. Gray points denote the baseline (unmatched) analysis, black points the results after exact matching individuals across treatment and control conditions on previous turnout, age, and gender. “Co-Ethnic Cand + Majority-black” is the weighted conditional effect (CATT) of assignment to a majority-black district with an African American candidate.

the treated (CATT), restricting treated districts to those where a co-ethnic ran for office and the district was majority-black.³² Turnout increases 0.83 percentage points when comparing treatment and control registrants under such conditions, indicating that black voters will likely respond with increased turnout when co-ethnics seek office in heavily black districts. Thus, the power of “representation and influence” as turnout stimuli appears robust (Bobo and Gilliam 1990), as African Americans are more likely to vote when able to elect or reelect a candidate of their choosing.

Latinos are the focus of the most comprehensive prior analyses of the impact of VRA-associated redistricting on minority turnout (Barreto et al. 2004; Henderson et al. 2013). Indeed, authors have asserted that Latinos may be most likely to respond to co-ethnic officeholding or candidacy, due to shared linguistic as well as cultural ties (Barreto 2010; Shaw et al. 2000). Figure 4 tests hypotheses regarding Latino officeholding, candidacy, and presence in heavily Latino districts, finding no evidence of a positive, immediate impact of any of these factors on a Latino registrant’s likelihood of voting. A null finding, perhaps due to limited case selection, would not be new (Brace et al. 1995; Henderson et al. 2013). Instead, I find evidence of a slight decrease in turnout for Latinos resulting from placement in a district with a Latino incumbent (−0.33 percentage points), no significant

32. Control districts are <30% black and did not have a black candidate seek office from 2006 to 2012.

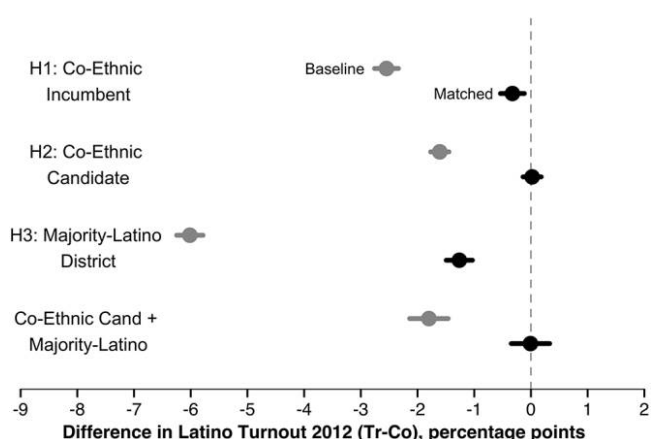


Figure 4. Overall average treatment effect on the treated (ATT), Latino registrants. Points indicate the difference in turnout between the treatment group and control group when using co-ethnic incumbency, candidacy, or a majority-Latino district to define treatment. Results reflect inverse variance weighted means of the average treatment effect on the treated, with a 95% confidence interval extending outward. Gray points denote the baseline (unmatched) analysis, black points the results after exact matching individuals across treatment and control conditions on previous turnout, age, and gender. “Co-Ethnic Cand + Majority-Latino” is the weighted conditional effect (CATT) of assignment to a majority-Latino district with a Latino candidate.

result for Latino candidacy, and a substantial negative impact of being assigned to a Latino-majority district (−1.26 points). Consistent with tables 1–3, unmatched results indicate quite clearly that our results would be biased toward even lower 2012 turnout for Latinos in the treatment group, which may be the result of nonrandom selection by redistricting commissions (Henderson et al. 2013; Li 2012). Yet even after accounting for 2006, 2008, and 2010 turnout, a negative effect persists.

The conditional average treatment effect shown at the bottom of figure 4 also suggests isolating the combined effect of co-ethnic candidacy and assignment to a majority-Latino district does not increase Latino turnout on average. Thus, we may have reason to be skeptical of the applicability of traditional formulations of empowerment theory to the Latino population. In particular, the more robust negative impact of majority-Latino districts may suggest a demobilizing impact of being “moved” to a majority-Latino district. The distinct strategies employed in such districts may be not be effective at mobilizing Latinos whose previous district was heavily non-Latino.³³

Asian Americans are the fastest growing ethnic group in the United States, approximately 5% of the total population as of 2014. However, few analyses have analyzed

33. For example, Abrajano (2010) notes that Spanish-language ads may reduce turnout among English-speaking Latinos (91).

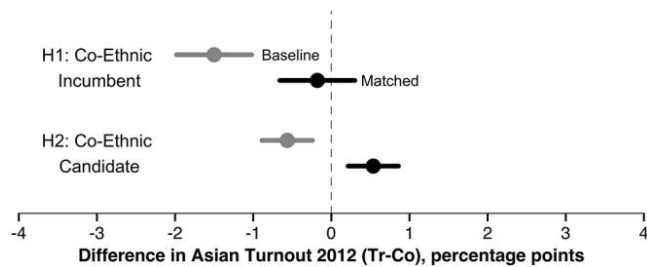


Figure 5. Overall average treatment effect on the treated (ATT), Asian Registrants. Points indicate the difference in turnout between the treatment group and control group when using co-ethnic incumbency or candidacy to define treatment. Results reflect inverse variance weighted means of the average treatment effect on the treated, with a 95% confidence interval extending outward. Gray points denote the baseline (unmatched) analysis, black points the results after exact matching individuals across treatment and control conditions on previous turnout, age, and gender.

patters in Asian political participation, and almost no studies explore whether or not Asian Americans are more likely to vote in contexts of descriptive representation (Fraga 2015; Wong et al. 2011). That said, in 2012 six Asian American members of Congress were elected in the 10 states I examine. Figure 5 provides an initial examination of the impact of co-ethnic incumbency and candidacy for the approximately 325,000 Asian Americans in the study, most of whom are in California. While Asian American voter turnout appears lower in contexts of descriptive representation/candidacy, after matching most of this effect disappears, and indeed, a measurable (+0.54 percentage points) increase in Asian voter turnout under conditions of Asian American candidacy is found under the matched condition. Contrasting the Asian American population with other minority groups, we see signs of a pattern more similar to that of African Americans than Latinos, though the disparity between unmatched and matched results indicates that future researchers should consider whether low-turnout Asian registrants are selectively redistricted as well.

CONCLUSION

Existing work on race and voter turnout suggests that ethnoracial context will increase minority political participation but cannot establish a causal basis for such a claim. Broadening the scope of the analysis to include whites, African Americans, Latinos, and Asians concurrently, I find substantial evidence that co-ethnic incumbency, candidacy, and/or assignment to a majority-minority does indeed influence voter turnout. For African Americans, the immediate impact of being assigned to a district with a black incumbent and/or a black majority is a measurable boost in voter turnout (+0.75 to +0.83 percentage points). White and Asian American registrants also participate more when

co-ethnic candidates are on the ballot (+1.46 and +0.54 percentage points, respectively). However, in the election following redistricting, Latinos are significantly less likely to vote when they have a Latino incumbent (-0.33 percentage points) or are assigned to a heavily Latino district (-1.26 percentage points). Though these effects are small in magnitude, note that such shifts in turnout are roughly equal to half of the black-white difference in voting rates for the 2012 election (File 2013). Furthermore, consider the impact of plausible campaign activities on voter turnout. The effects I find are roughly equivalent to receiving an impersonal contact encouraging a registrant to vote, with the low end approximating a text message (Malhotra et al. 2011) or Facebook notification (Bond et al. 2012) and the high end a piece of direct mail providing information about election issues and candidates (García Bedolla and Michelson 2012) or a simple reminder to vote (Gerber, Green, and Larimer 2008). While not as powerful as forms of personal contact that tap into civic identity (García Bedolla and Michelson 2012), the impact of ethnoracial context may be similar to, or even reflect, a modest change in campaign mobilization by parties and politicians.

Making use of individual-level data and the 2012 round of redistricting, I provide greater accuracy and avoid difficulties that have clouded our understanding of the role of race on voter turnout. The 10-state sample I examine also provides a greater measure of generalizability than previously possible. Despite these advances, the results above raise questions that give direction to future work. On its face, the most surprising finding may be the demobilizing impact of assignment to majority-Latino districts for Latino registrants. What explains this lower Latino turnout? Could a lack of viable competition in heavily Latino districts lead to lower quality incumbents, a story aligning with the analysis of candidate gender in Anzia and Berry (2011)³⁴ Might campaign strategies in majority-Latino districts lead to a less informed or less participatory Latino electorate (Abrajano 2010)? Researchers should continue to probe the root causes and persistence of this pattern but perhaps reconsider the novelty of this finding as well. Most notably, Barreto et al. (2004) uncover a similar short-term impact. Though the focus of their study was not on the effect of redistricting, they found Latinos living in majority-Latino districts created after the 1992 round of redistricting were 1.6 percentage points less likely to vote in the 1996 election than other Latinos (Barreto

34. However, removal of the least competitive congressional districts provides similar results to those shown in figure 4 above. See the appendix for more details and additional robustness checks.

et al. 2004, 72), an effect size comparable to the two-year impact found in this study. By the 1998 and 2000 elections, however, they observed that turnout was substantially higher for Latinos in heavily Latino districts.

The immediate effect of the 2012 redistricting process on individual turnout may also differ from long-term impacts for other groups. Barreto et al.'s (2004) short-term decrease in Latino turnout is offset by a substantially larger increase in turnout in later years. On the other hand, Tate (1991, 1994) and Gay (2001) hypothesized that their null finding for black turnout was due to long-term descriptive representation, which suggests the increase in African American participation found above may not persist years into the future. Tate and Gay suggest the limited competitiveness of majority-minority districts and the limited ability of co-ethnic representatives to affect change engenders a reversal of the empowerment effect as voters become despondent.³⁵ That said, these aggregate level viewpoints would seem to challenge the established wisdom that participation itself is habit-forming at the individual level (Fowler 2006; Gerber, Green, and Shachar 2003). Future work should thus extend the individual-level causal framework provided above to future elections, with the possibility (or indeed, expectation) that the immediate effect of a shift in ethnoracial context may differ from long-term results.³⁶

Taken together, the findings from the present study suggest that voter turnout is influenced by the largely modifiable actions of elected officials, independent redistricting commissions, and others who guide the redistricting process. Unlike other turnout-related factors, such as socioeconomic status (Verba, Scholzman, and Brady 1995), campaign activity (Rosenstone and Hansen 1993), or social pressure (Gerber et al. 2008), the composition of electoral jurisdictions is almost entirely shaped by forces within the control of public policy. Indeed, every 10 years, states across the country are required to make decisions that we now know to have a demonstrable impact on who votes.³⁷ Measures such as the Voting Rights Act continue to affect those decisions, especially in the realm

of jurisdiction-level ethnoracial context as examined above. Yet subsequent to the *Shelby County v. Holder* (2013) decision, the preclearance provisions of the VRA were deactivated until Congress can establish a new formula for judging which jurisdictions deserve heightened scrutiny. A renewed focus on persistently low minority voter turnout may make its way into a revised coverage formula (Jackson 2014). Given the impact of ethnoracial context on participation, we may be approaching an era where debates regarding majority-minority districting and substantive representation (Epstein and O'Halloran 1999; Lublin 1997) should be joined by a discussion of participatory outcomes with an earlier provenance.

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35. Though again, removal of the least competitive congressional districts provides similar results to those shown in figure 3. See the appendix for details.

36. Given the increase in African American turnout witnessed in the 2012 election (File 2013), it may be especially relevant to examine whether effects persist when President Obama is not on the ballot.

37. For comparison, hotly debated election reforms such as early voting, strict photo ID requirements, and election day registration only increase self-reported turnout by 1–2 percentage points on average (McDonald, Shino, and Smith 2015).

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