Participatory Democracy and Voter Turnout: Evidence from Participatory Budgeting

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Abstract

This paper examines the impact of local participatory democracy initiatives on individual voter turnout in ordinary elections. While such initiatives aspire to create more activated citizens, there is still limited empirical research to validate these claims. We link participants in New York City's new Participatory Budgeting (PB) process to their state voter file records to test whether PB increases participants' likelihood of voting in regular elections. We use coarsened exact matching to identify similar voters from council districts where PB was not implemented. Comparing PB voters to similar individuals who were not exposed to PB, we find that engaging with participatory budgeting increased individuals' probability of voting by an average of 7.5%. In addition, we find that these effects are greater for those who often have lower probabilities of voting – young people, lower educated and lower income voters, black voters, and people who are the minority race of their neighborhood.

1 Introduction

The "participation hypothesis" suggests that the act of participating in local democratic activities encourages further engagement in other spheres of civic life and supports healthy democratic systems (Finkel 1985; Gastil et al. 2008; Mansbridge 1995; Pateman 1970). The belief in the power of practicing participation has driven innovation and investment in local participation initiatives around the world (World Bank 2008; Norris 2011 chapter 12; Pateman 2012; White House 2013). However, the empirical basis for this hypothesis is less well-established than the common conviction in its merit. As even Mansbridge (1995) has acknowledged, "[p]articipation does make better citizens. I believe it, but I can't prove it. And neither can anyone else" (p. 1). The familiar difficulties of isolating relationships in observational studies of political life is not the only challenge here. Studying the effects of civic participation in one venue on democratic political engagement in another venue is particularly vulnerable to problems of selection bias, in which people who opt in to participation may be the type of people who are just in general more 'participatory'. Nevertheless, since Mansbridge's statement that the participation hypothesis is unprovable, some scholars have attempted to isolate causal estimates of the effect of prior participation on subsequent participation, usually turnout in future formal elections (Gastil et al. 2008; Gerber et al. 2003; Plutzer 2002). Yet studies directly linking new participation in small-scale democratic innovations to future political participation remain few in number. This paper makes a direct contribution to this gap in the empirical literature on participatory democracy, examining the effect of individual involvement in a recent participatory innovation, participatory budgeting in New York City, on subsequent voter turnout. This is the first research project in North America to directly link engagement in participatory budgeting to future validated political activity.

Participatory budgeting (PB) is a process where everyday people come up with ideas for and vote on how to spend public money. By getting people directly involved in issues that affect their community, many hope that this form of participatory democracy will have broader impacts on the civic and political life of communities, including on electoral participation. Indeed, some youth in Boston report that as a result of participating in PB, they are more likely to vote in regular elections (Grillos 2014: 25). Many PB participants from other localities say that they are more likely get involved in their

communities as a result of PB (Crum, Salinas, Weber 2013; Jovanovich and Russell 2016). Yet existing data on this question is relatively sparse, and ultimately relies on self-reported data about future behaviors.

In this paper, we use New York State voter file data to measure the effect of PB participation on voter turnout in regular elections. We link individual participants in PB in New York City to their state voter records in order to assess, via logistic difference-in-difference regression models, whether people who participate in PB are subsequently more likely to vote in regular elections. To help isolate the effect of PB, we use coarsened exact matching (CEM) to match PB voters to otherwise similar voters in districts where people did not have the opportunity to participate in PB. Comparing PB voters to similar individuals that we would expect to have participated in PB if they had the chance, we find that engaging with participatory budgeting increased individuals' predicted probability of voting by over 7% on average. We also find that the positive effects vary across different sub-populations, with stronger effects for those groups often documented as having a lower propensity to vote: increases in future electoral participation are greater for people under 30, people from less well-educated or lower-income neighborhoods, and people who are not members of the majority race of their neighborhood. Additionally, we find that black voters see the largest effects and white voters the smallest effects from PB.

The paper proceeds as follows: First we offer a brief introduction to participatory budgeting, in particular as implemented in New York City. We provide an empirical and theoretical justification for our hypothesis that participatory budgeting participating may be associated with an increase in regular voter turnout. Along the way, we also review other factors known to influence individual voter turnout, as key elements to control for when identifying the non-PB comparison group and in our final analysis. Next, we describe our data and the procedures for matching PB voters to similar non-PB voters. Finally, we present the results of our analysis, including detailed breakdowns of the expected effect from PB for specific sub-groups in our population, including breakdowns by individual and neighborhood characteristics.

2 Background

2.1 Participatory Budgeting

Participatory budgeting has grown in the US since it first came to Chicago in 2009. Since then, over 433,000 people have voted across over 30 cities in the US and Canada, directly deciding how to spend almost $300 million in public money. In PB, potential budget items are directly collected from residents, rather than generated by government officials. Through a process of deliberation and analysis, the collected budget ideas are developed and refined by fellow residents, in partnership with government staff. A narrowed list of proposals are placed on a ballot for other community members to directly vote for which items should be funded. Winning projects are then implemented by the local government. While the PB process can be applied to almost any type of budget, most cases of PB in North America have been applied to portions of the capital budgets, often distributed by city council districts or neighborhoods. In such cases, rather than the city council members deciding on how to spend discretionary money in their district, they use a PB process to give decision-making power over this spending to their city residents.

Each local PB process has its own rules for who is eligible, but in North America, people without traditional voting rights - non-citizens, people under the voting age, or those disenfranchised due to felony conviction - are usually eligible to vote in PB. Authorization to vote or participate in PB typically has minimal requirements, making it easy for people to partake in the process. Since the process is unfamiliar for many residents, those implementing the PB process go to lengths to publicize it, including street outreach in public places, text message and phone recruitment, advertisements, and other methods.

Many scholars have examined PB as a form of participatory democracy with the ability to reshape governance institutions in more democratic ways (Fung and Wright 2003; Cabannes 2004). It is a way to create avenues for civic participation and political voice for underrepresented or marginalized people (Baiocchi 2003), though the depth of these effects are not guaranteed (Baiocchi and Ganuza 2014). While the participants of PB in the US tend to be more highly educated, there also often is an

over-representation of black voters and low-income voters, relative to the neighborhood in which it takes place (Hagelskamp et al. 2016: 6).

In general, research on participatory budgeting has been mostly descriptive, providing accounts of the process as well as documenting the composition of who participates. There has been less systematic investigation on the measurable impacts of PB on individuals and their communities (Wampler et al. 2018). The few projects that have examined PB's measurable impacts include studies exploring PB's impacts on the structure of civic communication and collaboration patterns (Johnson 2017), numbers of civil society organizations (Wampler 2012), and health care spending and infant mortality rates (Gonçalves 2014; Touchton and Wampler 2014). Lerner and Schugurensky (2007) document the ways that PB educates individuals with new knowledge and skills and shifts attitudes and practice, serving as schools of citizenship that may support future civic or political engagement.

PB first came to New York City in 2011 when it was introduced by four city council members, and has since been implemented in 31 of the 51 districts.[1] There is a central city-wide steering committee that oversees and supports PB across the city, though a district's implementation ultimately depends on the local city council member's staff. In 2018, the typical district PB allocates about $1 million, and the average project costs almost $263,000. Winning projects range from new school facilities, to new park renovations, to security cameras, to street repairs, and more.[2] City council districts are on average about 164,000 people. Turnout for PB has grown from its first year at an average of 1,496 voters across four districts in 2011, to 3,335 voters across 30 districts in 2017 (see Figure 1). PB voters in New York City are diverse. Surveys conducted throughout the first 4 years of PB in New York City have described PB voters as broadly representative of their districts in terms of racial composition and household income (Community Development Project 2012, 2013, 2014, 2015). One exception was the common pattern that PB voters often reported higher levels of education than their districts in general (Community Development Project 2015).

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[1]For more information on participatory budgeting in New York City, particularly in the earlier years of its implementation, see the accounts in Gilman (2013), Johnson (2017), Community Development Project (2015), and Su (2017).

[2]With the exception of one district, only capital projects are eligible for PB funding, not operations funding.
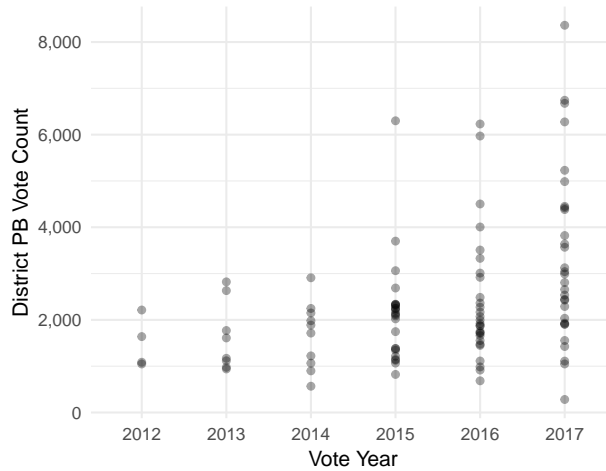
Figure 1: Number of voters per City Council District, by year.

2.2 Participatory democracy and voter turnout

One of the core aspects of participatory democratic theory is the idea that participation in local or everyday venues acts as a "school of democracy," teaching the practices and habits that build capable citizens and form the foundation for future participation. (Mansbridge 1999; Pateman 1970). This participatory practice can come in many forms (see Rowe and Frewer 2005), which could have differential effects, but the core idea is that starting to participate in one venue creates a virtuous cycle that can lead to more participation in other venues. Many democratic theorists, including Pateman (1970), Bachrach and Botwinick (1992), and even Verba, Scholzman and Brady (1995) emphasized participation in 'non-political' spaces like workplaces or churches as important precedents for democratic political engagement. Much of of the contemporary push for expanded opportunities for participatory decision-making locally have been initiated directly as a response to concerns with the decline of traditional venues for learning participation.

Reflecting the central importance of voting in modern democracies, and building on extensive work on the determinants of voter turnout (of which Verba et al. are one example), a primary area of interest for exploring the secondary effects of participation is how it does or does not expand voter participation in regular elections. Dvořák et al. (2017) find that direct democracy in the form of citizen initiatives and referenda in the Czech Republic led to increased subsequent turnout in the short

term. Measuring (self-reported) behavior in the months following participation, Gastil et al.(2010) and Jacobs et al. (2009) both find that individual voter turnout increases after individuals engage in deliberative participation (in courtroom juries or issue deliberations, respectively). Gastil et al. (2010), in particular, find that the effect on turnout increases with more intense forms of jury deliberation. While the specific psycho-social mechanism of motivating effects from participation requires more research, activating one's sense of civic duty has been shown to have positive effects on voter turnout (Gerber et al. 2008; Blais and Achen 2018).

2.3   Factors that affect turnout

We expect participation in a PB process to be associated with an increase in the likelihood of voting in regular elections. To assess the degree to which PB participation is indeed associated with stronger expectations of turning out to vote, we also need to account for the many other factors that have been documented as affecting whether people show up at the polls. The primary confounding factor in determining whether participation causes people to vote more is that people who are more likely to participate in other avenues of civil society are also more likely to vote in regular elections. This introduces a kind of selection bias, where the people who show up to participatory budgeting events are already more likely to turn out to vote. Existing literature on voter behavior provides several empirically-supported explanations for voter turnout, which we account for in our matching and regression models below.

One of the most consistent predictors of voter turnout is one's prior voting history (Cancela and Geys 2016). Denny and Doyle (2009) find that voting in a previous election increases the probability of voting in a subsequent election by 13%. People are often habitual voters or habitual non-voters (Plutzer 2002). Fowler (2002) estimates that half of voters either always vote or always abstain. A meta-analysis by Cancela and Geys (2016) shows that 86% of studies measuring the effect of past turnout on subsequent turnout found a positive and statistically significant effect (p. 267). Gerber et al. (2003) attempt to estimate a causal effect of voting habit on subsequent elections. Their study finds a 45% greater chance of voting among those registered voters who had voted the year prior compared to

those who did not.

Education level also is a consistent predictor of voter turnout. More education correlates with increased voter turnout with "law-like regularity" (Sondheimer and Green 2010: 174). Some have argued that education is not the mechanism that affects voter turnout, but rather is a proxy for other factors that cause people to be more likely to vote (Kam and Palmer 2008; Tenn 2007). On the other hand, Sondheimer and Green (2010) draw on experimental and quasi-experimental data to attribute causal effects of education on voter turnout.

Race of the voter as well as the context in which they live have been shown to be related to voter turnout. Many find that being a member of the majority racial group in a district has a positive effect on turnout (Fraga 2016a; Fraga 2016b; Barreto et al. 2004), while Geys' (2006) meta-analysis finds that higher shares of ethnic and racial minorities are negatively correlated with turnout.

Finally, turnout is often higher in competitive races. Indeed it is one of the strongest predictors in aggregate level turnout (Franklin 2004). Meta-analysis suggests that closeness of an election is positively associated with turnout (Geys 2006). There are competing explanations of why this is the case. One common explanation is the Downsian argument that people are more likely to vote when they believe that they might cast a decisive ballot (see Matsusaka and Palda 1993). Others argue that close elections induce more elite mobilization and campaigning (Cox and Munge 1989). Regardless of the underlying causal mechanism, existing literature identifies competitiveness as a largely consistent predictor of voter turnout.

3   Data and Method

To test our hypothesis that PB participation translates to increased electoral participation, we use individual level participatory budgeting voter data, acquired from the central Participatory Budgeting New York City body and local city council district offices. Through our partnership with NY Civic Engagement Table, we use Catalist to access the NY state voter file and other individual level data to link PB voters with their turnout histories where possible. We then use coarsened exact matching to identify a comparison group of approximately identical voters from districts where PB is not available.

This enables us to estimate the effect of participating in PB by comparing the changes in the formal voting behavior of PB voters during the time period before and after participating PB , compared to the voting behavior during the same time period of similar people with similar past voting behavior who did not have the opportunity to participate in PB. To make these comparisons, we use logistic difference-in-difference models to estimate predicted probabilities of voting for PB voters. We additionally estimate and present predictions from models which condition the effect of PB by different demographic and neighborhood characteristics to explore the extent to which the effect of PB varies for different sub-populations.

3.1   Data

This analysis uses individual and census tract level data for registered voters, including both those who did and those who did not also vote in PB. The individual data is provided by Catalist through the NY Civic Engagement Table and their partnership with Participatory Budgeting Project and PBNYC. This includes a voter's name, full voting history, age, and address, which come directly from the New York State Voter File. Catalist additionally includes an estimated race variable using a proprietary algorithm, which has been used with high success in previous research (Fraga 2016a; Fraga 2016b; Ansolabehere and Hersh 2012). It uses names, local census information, and commercial data to estimate a voter's race.

   Along with the individual level data on voting history, age, race, and geo-located address (which is used to identify the election district and census tract), we include census tract-level variables from the American Community Survey (US Census Bureau 2015). When matched with individual level data, localized contextual data has been used as proxies for missing individual data, such as income (Hersh and Nall 2016). We include median household income and percent of the population with different levels of educational attainment as a proxy for important unmeasured individual traits of income and education. We also include the percent of the population that is white, to account for the effects of racial composition, as well as whether a voter's estimated race matches the majority race of their census tract (Fraga 2016b). Construction of the sample population for both PB and non-PB voters, including

the matching process, is described below.

3.1.1 PB voters

Identifying PB voters and matching them to their voting histories was the most challenging part of data collection. For the first several years of PB, some districts and central city staff collected basic identifying information about voters, including name and date of birth, usually as part of the process of validating voters' eligibility. To create our dataset, staff at the Participatory Budgeting Project obtained the PB voter lists for districts who held a PB process in New York City from 2013-2016. PBP staff contacted all 27 city council districts who held a participatory budgeting process in 2015 to request their list of voters in their respective districts. 15 districts replied, but only two districts had multiple years of voter lists. The remainder only had records for the 2016 voting cycle.

From all city sources, we were able to identify 10,030 PB voters in the state voter file. It is important to note that PB voting procedures are deliberately more inclusive than traditional electoral processes, and extend voting rights to youth, non-citizens, and felons who may not be eligible to vote in regular elections, and thus will not be found in the voting file. For those we could not link to the voter file, we do not know for certain whether this is because they were not registered to vote, ineligible to register, or we could not match them for some other reason (for example an error in a PB voter's recorded name or date of birth).

An additional complicating factor is that different districts first implemented PB at different points in time, and voters may or may not vote in multiple years of PB. In the first years, voter lists were not held in a central location, but rather by local district offices that varied in the extent of their PB record-keeping. In order to minimize bias from misidentifying the year people first voted in PB, we only included voters for whom we could be confident that we had data all the way back to the first year of PB voting in their district. This included all voters from districts 23, 30, 35, 36, 39, and 40.[3] Finally, for this analysis, we limited our sample to individuals with a current voting address within

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[3]In addition, a small subset of voters whose votes were recorded in 2012 in centrally held records, but who were not definitely assigned to a PB district are also included, as the first PB votes in the city occurred in 2012 and thus we are confident of recording these voters' initial experience with PB.

New York City. This left us with a final PB voter sample size of 7,656 people.

3.1.2   Non-PB voters

Non-PB voters were identified from the New York State voter file via Catalist. Having obtained the full voter file, we excluded all voters in districts that had implemented PB as of 2017 as well as any voters in non-PB districts who were listed as a PB voter in our files from the city (this could happen in the case of someone moving from a PB to non-PB district during the time frame of this study). We excluded all voters from districts that had implemented PB to minimize bias from two possible sources: 1) without full PB voter records in all PB districts, we cannot ensure that a voter who would match a PB voter did not themselves vote in PB (which would bias any estimated effect of PB downward) and 2) a non-PB voter in a PB district has demonstrable differences from a PB voter (by virtue of opting out from the PB process). The important comparison is between PB voters and *voters who would be the type to get involved in PB* if it were an option available to them. 20 of the 51 New York City Council districts have never implemented PB. Voters from these 20 districts make up the population of potential matches with the PB voters, as described in the matching process below.
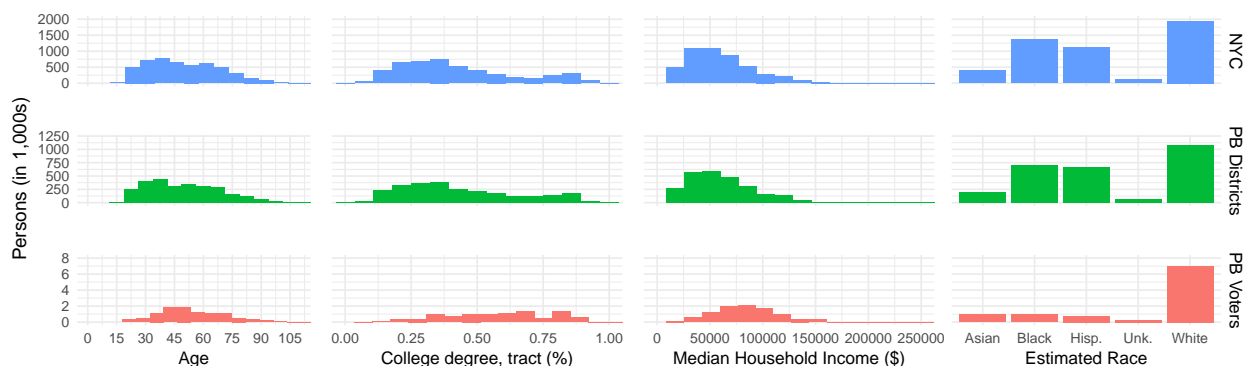

Figure 2: Characteristics of registered voters in New York City as a whole, PB districts in NYC, and PB voters whose data was collected and included in our sample.

Figures 2 and 3 illustrate the general characteristics of NYC registered voters as a whole, voters in PB districts (regardless of their participation in PB), and the PB voters that made it into our final sample. While PB districts in general resemble the city as a whole, PB voters in our sample tend to come from more well educated neighborhoods and are more likely to be white than the general NYC
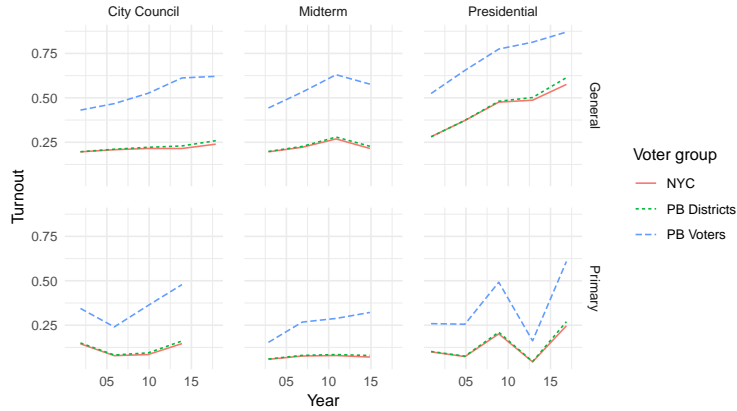
Figure 3: Voter turnout of registered voters in New York City as a whole, PB districts in NYC, and PB voters whose data was collected and included in our sample.

voter population. The matching method described below ensures that our analysis compares our PB voters to similar non-PB voters elsewhere in the city.[4]

3.2 Matching

The objective of this analysis is to understand the effect of participation in PB on future electoral participation. We cannot directly compare PB voters' behavior to the counterfactual scenario in which they did not experience PB. Instead, we use matching methods to construct a control group of registered voters in districts where PB is not available that is balanced with our 'treatment group' of PB voters on all covariates that are expected to predict voting and/or participation in PB. This restricts our analysis to a comparison only with those individuals who would be likely to to participate in PB if given the chance. While matching methods do not replicate experimental control, such an approach gives us the best opportunity to estimate the observable impact of participatory budgeting with purely observational data.[5]

Given our large set of potential matches and high level of overlap in population characteristics between our PB voters and the whole city voting population, we use one-to-one coarsened exact matching (Iacus et al. 2012), a technique that directly matches two subjects on a set of relevant covariates,

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[4]See Appendix for detail on demographic balance of PB and non-PB voters before and after matching.

[5]An extensive literature on statistical matching methods exists. For a good review of different methods, see King et al. 2011; Iacus et al. 2012; or Stuart 2010.

Table 1: Match Criteria

	Individual-level	Tract/Election District-level
Age	Age[i]	-
Gender	Male/Female[ii]	-
Race	White, Black, Asian, Latinx, Unknown[ii] Membership in majority racial group in census tract [ii]	Tract: % pop. identified as white[iii]
Voting History	Indicator for each 2008-2012 election[ii] Count of votes cast 2000-2007 [v]	-
Education	-	Tract: % pop. with college degree[iv]
Income	-	Tract: Median household income[iii]
Electoral competitiveness	-	Election District: % Margin of victory, top vote getting race in each ED 2014 & 2016[vi]

[i]Matching ages within 5 year intervals (15-19, 20-24, 25-29 etc.) [ii]Exact match. [iii]Match within quintiles (Bottom 20% of tracts, next 20-40% percent of tracts, etc). [iv] Match above and below median percent with a college degree. [v]Match within low, medium, high competition districts (stratified by bottom 25th, middle 50th, and upper 75th percentiles) in 2014 primary and general and 2016 presidential primary and general. Note that earlier election district-level vote results are not available from the NYC Board of elections. ED-level data are only publicly available for 2014 onward. [vi] Voters' total ballots cast in primary and general elections from 2000 - 2007 are separately summed (maximum possible 8 votes for each general and primary counts. Match is by votes in group of 0, (1, 2), (3, 4), (5,6) or (7,8) total votes.

reducing model dependence and ensuring balance on even a large number of covariates (such as the many predictors of individual turnout).[6] A match is only achieved if they are effectively identical on the specified covariates. The match is "coarse," because while some variables like gender are easy to match exactly, continuous variables like age and income are more difficult to match exactly, and thus acceptable ranges for matching are determined.

For this analysis, we match on a combination of individual and census-tract level variables expected to be associated with PB participation and/or voter turnout. The variable measurement and the matching strata are defined in Table 1. Using these criteria, we are able to successfully match 3,636, or 47%, of our 7,656 PB voters to a non-PB voter. We can increase our match rate substantially by dropping the census tract level variables and relaxing the coarseness of the match, but were not willing to accept the level of imbalance in contextual neighborhood factors that resulted. After matching, we

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[6]With many more controls than PB voters, we randomly sample one matching control individual for every PB voter in each strata.

have a dataset including 152,712 voter-election observations. The goal here is to find an adequate sample of comparable voters, pruning our data to be balanced between treatment and control groups, not simply to find a nearest match for every PB voter.[7]

3.3 Effect Estimation Method

We implement a difference-in-differences design for the binary outcome of whether an individual voted or not in a given election. We use logit models that include random effects to account for the hierarchical clustering of observations within individuals and districts. In addition to the indicators of membership in the 'treatment' group (PB voters) and whether an election occurred before or after an individual's first PB vote (the equivalent of the treatment*time interaction in a two period difference-in-difference design), we include dummy variables for type of election and election years, as well as including individual and tract level controls.

The base model thus takes the form:

$$Pr(y_i = 1) = \text{logit}^{-1}(\gamma PB_i + \lambda_y + \delta A_{iy} + X_i'\beta + \vartheta M_v + \alpha_D + \alpha_i)$$

where

$$\alpha_D \sim N(0, \sigma_D), \text{for } D = 1, ..., 48$$

$$\alpha_i \sim N(0, \sigma_i), \text{for } i = 1, ..., 7,272$$

In this notation, PB_i is the indicator of a individual's membership in the group of PB (treatment) or non-PB (control) voters , λ_y is the fixed effect estimate for each year capturing the baseline time trend, A_{iy} is the interaction between treatment group membership and an election occurring after an

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[7]As a robustness check, we also replicated the regression analysis below for four different levels of match granularity. Substantive model results were stable even with a much more granular exact match cutpoints that provided a highly exact match for only 2443 (32%) of all PB voters.

individual's first PB vote, making δ the difference-in-difference estimate: the expected effect of PB. X_i is a vector of individual covariates, including age, gender, race, membership in the local majority racial group, neighborhood median household income, percent of the population with college degrees in the neighborhood. M_v are fixed effects for the type of election, general, primary, or presidential primary, for each individual vote observation. α_D and α_i are the random effects for district D and individual i.

We also do not assume that the effect of PB is constant across all groups within the population. To test the heterogeneity of effects among groups, we also fit a series of models including separate interaction terms between the indicator for an election taking place after first voting in PB and various sub-group classifications, including by race, youth, gender, high/low education, and high/low median household income. These interaction models take the form:

$$Pr(y_i = 1) = \text{logit}^{-1}(\gamma PB_i + \lambda_y + \delta A_{iy} + \eta A_{iy}S_i + \tau_{Siy} + X_i'\beta + \vartheta M_v + \alpha_D + \alpha_i)$$

These interaction models include the same general terms as the base model above, with the additions of the necessary interaction terms for the "triple difference" estimates of the conditional effect of PB on different sub-groups. $A_{iy}S_i$ is the interaction term of the 'after PB' indicator with the sub-group, with η providing the estimate of the conditioned effect of PB for the covariate of interest. τ_{Siy} is the interaction of the sub-group variable with the year fixed effect dummies, accounting for the sub-group-specific time trends.

Effect estimates from logit models, especially when interactions between variables are included, require additional interpretation to be meaningful in terms of direct quantities of interest. All results from models below are thus presented in the form of changes in the predicted probability of turnout. Tables of regression coefficients and significance tests are available in the appendix. Confidence intervals for predicted probabilities were generated by simulation.

4 Results

This analysis demonstrates a strong and statistically significant effect of voting in participatory bud-geting on the probability of voting in subsequent regular elections. On average, participation in PB is associated with an increase of 7.5% in an individual's predicted probability of voting, all else being equal.[8] A positive significant effect of PB is evident in a minimal model including only the indicators necessary for a basic difference-in-difference analysis. This substantial positive effect is robust to the inclusion of individual, election, and tract-level covariates. Table 2 presents the model estimates of both the minimal difference-in-difference model and the models including each level of additional covariates.[9] Note that membership in a PB-implementing district has no significant association with voter turnout, validating the effectiveness of the matching method for identifying a comparison group with equal baseline likelihood of voting.

The coefficient estimates demonstrate a robust positive effect of PB. We illustrate the magnitude of this effect with predicted probabilities of voting, since direct substantive interpretation of the logit difference-in-difference coefficients is not straightforward. We calculate predicted probabilities of voting with and without hypothetical participation in PB to illustrate the magnitude of the effect of PB. Figure 4 illustrates the difference in the predicted probability of voting in the general elections that have occurred since PB was first implemented in New York City. This figure compares the estimated turnout for non-PB voters in two scenarios - the real world where they did not participate in PB and the counterfactual alternative where these voters had participated in a PB process in their own district. As Figure 4 shows, depending on the baseline predicted turnout for different election years, the substantive average effect of PB varies from just over 6% increase in the probability of turning out in the 2016 general election to over an increase of over 16% points in the 2014 and 2017 elections.

Logit models appropriately model the outcome of interest as a binary outcome (voted/did not vote). This modeling choice imposes a reasonable non-linearity on any effect of PB: If a person already

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[8]7.5% is the marginal effect of the post-treatment variable in the best-fitting difference-in-difference model, without including any interactions of the post-treatment indicator with other covariates.

[9]Model selection processes used in-sample percent correctly predicted and AIC/BIC criteria to identify and exclude appropriate transformations and redundant variables, such as a census-tract flag for majority white population (this variable was not informative once race and flag for majority race membership was included).

Table 2: Individual voter turnout difference-in-difference regression results: no interactions

	Minimal	Demog.	Tract	Majority Match
	(1)	(2)	(3)	(4)
PB district	−0.335	−0.162	−0.097	−0.110
	(0.233)	(0.173)	(0.151)	(0.152)
After PB	0.696*	0.674*	0.675*	0.675*
	(0.029)	(0.029)	(0.029)	(0.029)
Primary election	−2.387*	−2.412*	−2.412*	−2.412*
	(0.019)	(0.019)	(0.019)	(0.019)
Pres. Primary	−2.499*	−2.545*	−2.545*	−2.545*
	(0.032)	(0.032)	(0.032)	(0.032)
Black		−1.154*	−0.968*	−0.885*
		(0.124)	(0.125)	(0.128)
Asian		−2.112*	−1.963*	−1.922*
		(0.088)	(0.090)	(0.092)
Hispanic		−1.518*	−1.398*	−1.244*
		(0.125)	(0.126)	(0.137)
Race Unknown		−1.669*	−1.563*	−1.382*
		(0.215)	(0.214)	(0.223)
Female		0.071	0.064	0.066
		(0.048)	(0.048)	(0.048)
Age in years		0.094*	0.091*	0.089*
		(0.009)	(0.009)	(0.009)
Age^2		−0.0003*	−0.0003*	−0.0003*
		(0.0001)	(0.0001)	(0.0001)
18+ at vote		−7.726*	−7.728*	−7.732*
		(1.009)	(1.009)	(1.009)
% college educated			1.899*	1.821*
			(0.268)	(0.270)
Median HH income			−0.037*	−0.038*
			(0.012)	(0.012)
Majority Race				0.219*
				(0.074)
Constant	1.242*	−1.929*	−2.557*	−2.674*
	(0.204)	(0.277)	(0.281)	(0.284)
Year fixed effects?	Yes	Yes	Yes	Yes
Observations	152712	152712	152712	152712
AIC	119811.700	115189.600	115139.600	115132.900
BIC.	119970.600	115428.100	115398.000	115401.200

Note: Difference-in-difference regression results from multilevel mixed effect logistic models of individual turnout in a given election, including random effects for individual and council districts. Standard errors reported in parentheses and statistical significance at $p < 0.05$ indicated by *.
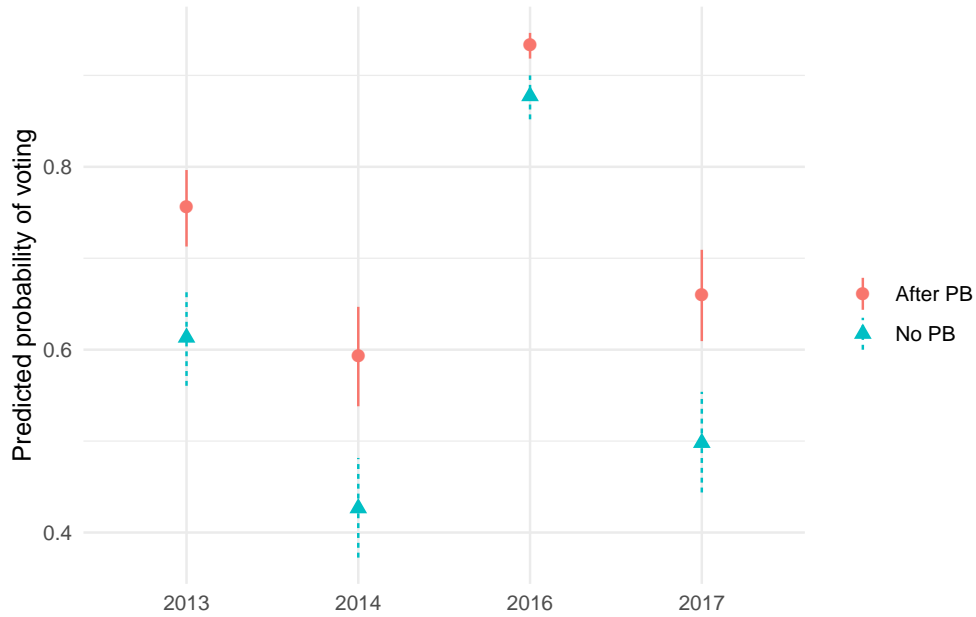
Figure 4: Predicted probability of voting in general elections, with 95% confidence intervals. Predictions for non-PB voters, showing effect of hypothetical participation in PB vote. All other variables set to their means. 95% confidence intervals generated by simulation.

has a history of voting in every election, PB cannot have a very large impact on their probability of voting, because they no longer have any room to improve. On the other hand, a person who votes only occasionally has the potential of a much more substantial impact on their future behavior. This non-linear effect is why, in Figure 4, the difference between voting with and without PB is smaller in the 2016 presidential election, compared to the other lower turnout years. Presidential election years like 2016 already see higher turnout in general, so the effect of PB is less evident. This implies a potentially stronger impact from PB during off-cycle election years when turnout is typically lower for most voters.

4.1   Sub-group Breakdowns

The foundational model presented above assumes that the effect of PB is the same across all different types of voters. This is an assumption that can be directly assessed by including interactions between the effect of PB and specific sub-groups of interest in the population, effectively making the effect of PB conditional on other variables, for example a voter's race or the median income of households in

their immediate neighborhood. We explore the conditional effect of PB across several different group breakdowns: by race, gender, youth voters (under 30),[10] and neighborhood characteristics of voters including income and education. Figure 5 shows the differential impact of PB within each of these sub-group comparisons. The x-axis shows the expected change in the probability of voting for typical non-PB voters, were they to have participated in a PB process prior to the general election in 2016. We fit separate models conditioning the effect of PB on each variable in turn.
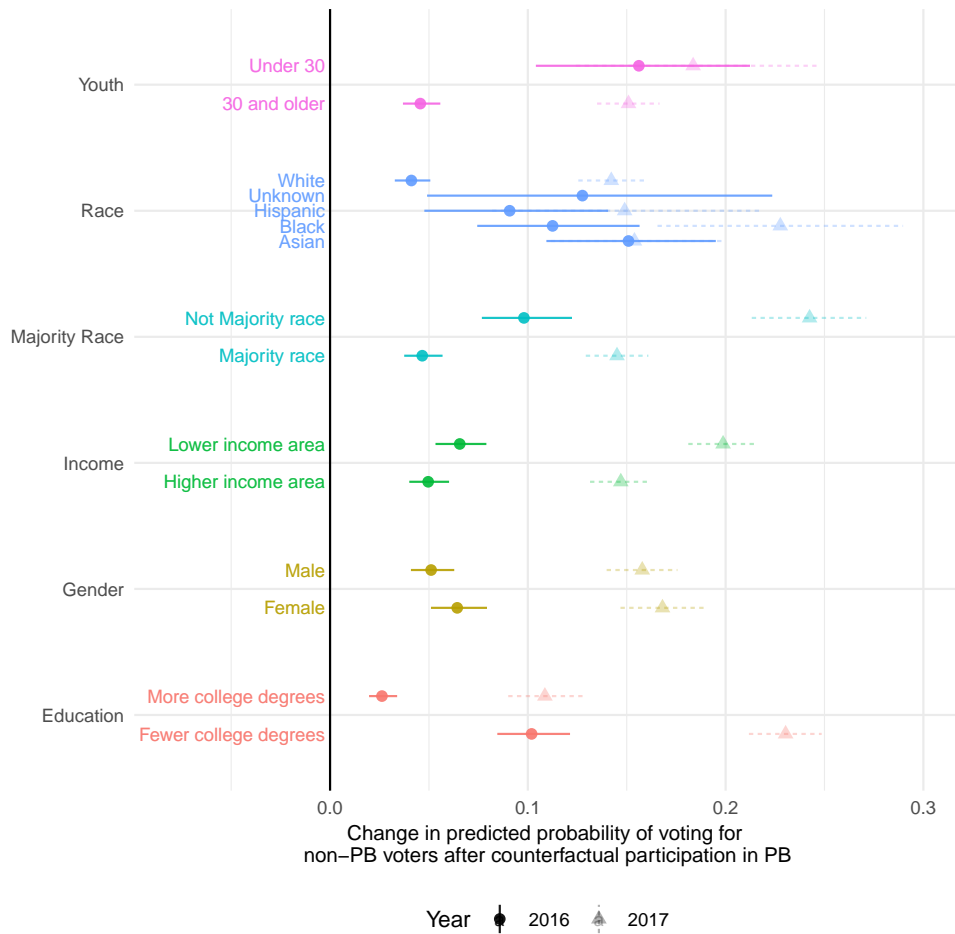

Figure 5: Change in predicted probabilities of voting in the 2016 and 2017 general elections with and without counterfactual participation in PB, with effect conditional on sub-group. Change in predicted probability of voting in 2016 are showed by bold circles, while the change in predicted probably of voting in 2017 are shown in grayed out triangles, for comparison. Interaction effects for each grouping variable estimated in separate models, including all base terms as covariates. All covariates other than highlighted subgroups for each model are set to their means for prediction. Majority match is the exception: it is set to its mean within the specified racial group for race interactions. 95% confidence intervals generated by simulation. See Table A.1 in the appendix for details of model estimates.

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[10]Voters are flagged as 'youth' voters if they were under 30 at the time of analysis

The effect of PB remains consistently significant and positive even when allowed to interact with each key covariate. Within this generally positive effect, however, as Figure 5 shows, PB can indeed have substantially and significantly different effects for different sub-groups. The prevalence of post-secondary education in the census tract and membership in the majority race in the census tract are the two variables with the strongest moderating influence on the effect of PB. Individuals in census tracts with fewer college educated people (as a percentage of the population) or who are not members of the majority racial group in the tract see large increases in the effect of PB - translating to an *additional* increase in the probability of voting in the 2016 general election of about 8% and 5%, respectively.

Other conditional influences on the effect of PB include the the contrast between older and younger voters and median household income of the voter's census tract. Younger voters, while having a lower predicted probability of voting, demonstrate a significantly and substantially larger effect from PB, while voters in lower income areas also experience slightly but significantly larger effects from PB. Figure 5 shows that different racial groups see statistically significantly different effects from PB, with white voters demonstrating the smallest effect and black and Asian voters showing the largest effects. The large expected differences in the probability of voting with and without PB are not only a result of the interaction of race with PB (the interaction with race in the model is only significant for black voters compared to white). This variation illustrates how the overall effect of PB is amplified more for minority racial groups because of their generally lower probabilities of voting, especially when a member of a minority within their local area. Whites are generally more likely to vote than any other racial group in most elections, while black and Asian voters are significantly less likely to vote. These differences allows PB to have greater leverage among Asian and black voters than white voters. There is no evidence for differential effects of PB on the basis of gender.

For comparison, Figure 5 also shows the estimated effect of PB in the 2017 general election (shown by the grayed out triangles). This was a local election, with overall lower turnout across all groups. This figure highlights how the strongest moderating effects (majority racial group membership, prevalence of post-secondary education, and local median household income) continue to support different total increase in predicted probability of voting as a result of PB. In this election where all groups are less

likely to vote,the effect of PB is more similar across racial groups, although black voters still have a significantly stronger effect from PB than white and Hispanic voters.

5 Discussion

These results point to a robust positive impact from voting in PB on the likelihood of someone turning out to vote in a future regular election. This effect persists across different types of participants, including across racial groups, gender, age groups, and people from more or lower income or less well-educated neighborhoods. These results present a confirmation of participants' own direct accounts of increased motivation to get involved in other kinds of citizen activity following their experiences with PB (Lerner and Schugurensky 2006; Gilman 2016).

While this analysis has been conducted making the most of available data, the results are of course subject to a range of limitations. The first is that we cannot generalize these effects beyond the population of voters likely to vote in PB. Participatory budgeting has so far been an opt-in process; similar effects on future participation may not necessary apply in settings where participation was mandatory. That being said, our sample of PB voters does include many individuals without a history of voting who begin to vote following their participation in PB, lending some support to expectations that the civic learning in PB can support an increased propensity to vote even among those who do not have a previous history of voting. Second, the study is limited to New York City, and different cities may yield different findings. Finally, our analysis is restricted to districts that had robust data collection processes. We know that there is diversity in the implementation of PB across New York districts and across cities that have chosen to implement PB (Johnson 2017), and some instances of PB may not have the same results. It is possible that the successful data collection practices of the districts who had voter lists to share with us indicate unusual styles of PB implementation that mean their voters' experiences of PB may not generalize to other council districts. It will be important to replicate the analysis presented here in other settings where individual voter data is available.

Participatory budgeting processes can create an intersection between 'less political' volunteering and civic engagement and formal political behaviors. With its community focus and emphasis on public co-design of projects with direct impact on local neighborhoods, PB is uniquely positioned to bring in residents who may not typically vote or engage in formal electoral politics. Intentional outreach efforts combined with low registration barriers help to bring more people easily into the

process. This paper offers the first direct analysis of the impact of PB engagement on increased voter mobilization in the United States, contributing to the broader research on how instances of civic participation can serve as gateways to additional political activity . In New York City at least, it appears that PB has been able to act as an effective bridge between civic participation and voter activation, particularly for those groups who often display lower voter turnout rates.

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A Appendix

A.1 Results: Sub-group interaction model estimates

Table A.1: Individual voter turnout difference-in-difference regression results: including sub-group interactions for 'triple-difference' results

	Base	*Race	*Maj. Match	*Gender	*Education	*Income	*Youth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PB district	−0.11 (0.15)	−0.14 (0.15)	−0.13 (0.15)	−0.11 (0.15)	−0.18 (0.15)	−0.15 (0.15)	−0.05 (0.15)
After PB	0.67* (0.03)	0.65* (0.03)	1.00* (0.06)	0.69* (0.04)	1.43* (0.09)	1.24* (0.09)	0.66* (0.03)
Primary election	−2.41* (0.02)	−2.44* (0.02)	−2.43* (0.02)	−2.41* (0.02)	−2.45* (0.02)	−2.43* (0.02)	−2.42* (0.02)
Pres. Primary	−2.55* (0.03)	−2.59* (0.03)	−2.57* (0.03)	−2.55* (0.03)	−2.59* (0.03)	−2.57* (0.03)	−2.55* (0.03)
Black	−0.89* (0.13)	−1.11* (0.15)	−0.84* (0.13)	−0.88* (0.13)	−0.84* (0.13)	−0.84* (0.13)	−0.96* (0.13)
Asian	−1.92* (0.09)	−2.99* (0.12)	−1.90* (0.09)	−1.92* (0.09)	−1.88* (0.09)	−1.89* (0.09)	−2.01* (0.09)
Hispanic	−1.24* (0.14)	−1.71* (0.17)	−1.19* (0.14)	−1.25* (0.14)	−1.19* (0.14)	−1.20* (0.14)	−1.35* (0.13)
Race Unknown	−1.38* (0.22)	−2.69* (0.32)	−1.32* (0.22)	−1.38* (0.22)	−1.34* (0.23)	−1.34* (0.22)	−1.44* (0.22)
Female	0.07 (0.05)	0.07 (0.05)	0.07 (0.05)	0.13* (0.06)	0.07 (0.05)	0.07 (0.05)	0.09 (0.05)
Age in years	0.09* (0.01)	0.10* (0.01)	0.09* (0.01)	0.09* (0.01)	0.09* (0.01)	0.09* (0.01)	0.20* (0.01)
Age^2	−0.00* (0.00)	−0.00* (0.00)	−0.00* (0.00)	−0.00* (0.00)	−0.00* (0.00)	−0.00* (0.00)	−0.00* (0.00)
18+ at vote	−7.73* (1.01)	−7.65* (1.01)	−7.73* (1.01)	−7.73* (1.01)	−7.69* (1.01)	−7.71* (1.01)	−6.88* (1.02)
% college educated	1.82* (0.27)	1.84* (0.27)	1.89* (0.27)	1.82* (0.27)	3.00* (0.28)	1.96* (0.27)	1.74* (0.27)
Median HH income	−0.04* (0.01)	−0.04* (0.01)	−0.04* (0.01)	−0.04* (0.01)	−0.04* (0.01)	0.01 (0.01)	−0.04* (0.01)
Majority Race	0.22* (0.07)	0.22* (0.07)	0.70* (0.09)	0.22* (0.07)	0.25* (0.07)	0.24* (0.07)	0.17* (0.07)
After PB * Black		0.31* (0.14)					
After PB * Asian		0.15 (0.11)					
After PB * Hispanic		0.04 (0.16)					
After PB * Unknown		0.33 (0.29)					
After PB * Majority match			−0.38* (0.07)				
After PB * Female				−0.03 (0.06)			
After PB * % college					−1.11* (0.14)		
After PB * Median HH. inc.						−0.06* (0.01)	
Age < 30							0.99* (0.24)
After PB * Age < 30							0.28 (0.15)
Constant	−2.67* (0.28)	−2.72* (0.29)	−3.15* (0.29)	−2.72* (0.29)	−3.42* (0.29)	−3.22* (0.29)	−5.94* (0.36)
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subgroup time trends?	None	Race	Maj. Match	Gender	Education	Income	Youth
Observations	152712	152712	152712	152712	152712	152712	152712
AIC	115132.90	114059.90	114560.50	115089.60	113929.00	114514.90	114624.20
BIC	115401.20	114725.60	114928.10	115457.20	114286.70	114872.60	115001.70

Note: "Triple difference" regression results, including interactions conditioning treatment effect by designated covariates, from multilevel mixed effect logistic models of individual turnout in a given election, including random effects for individual and council districts. Standard errors reported in parentheses and statistical significance at $p < 0.05$: *.

A.2 Online Appendix: Matching Specification Assessment

Matching successfully improves balance on all covariates, except for competitiveness, illustrated in Figure A.1. Observe that the sample after matching (the "adjusted" sample in the figures below) has mean differences on nearly all covariates at or near zero. One exception is the mean difference balance on the electoral competitiveness measures. Means are highly sensitive to outliers in election competitiveness scores, which results in comparatively high standardized mean differences that persisted across quintile matching. Restricting match cutpoints to exclude outliers improves match balance on these measures, but substantially cuts down the number of matching cases (from 3,646 to 2,443 matched). As the correlation between electoral competitiveness and turnout is low within these New York City elections (bivariate association competitiveness and turnout at the individual level is minimal to non-existent for all elections with competitiveness data), we accept the imbalance from outliers in the match in order to maintain a more broadly representative sample across other measures.

We replicated the regression analyses reported in the main paper above with the datasets produced from more restricted matching specifications (including one that restricts matching with outliers), and the substantive results of the analysis remain largely consistent. Figure A.2 illustrates the relative stability of coefficient estimates as the granularity of match criteria is increased.

Matching successfully trims the population of 'control' cases to be more efficiently comparable to the PB cases. However, our strict matching specifications do mean that over 50% of PB voters are not, in fact, matched to a unique non-PB voter elsewhere in the city. Figure A.3 illustrates the difference between PB voters who are successfully matched and those that are not. In general, we were more successful at matching white or younger and those from more well-educated or lower income census tracts. No major demographic groups were unmatched, but it is worth noting that larger proportions of the populations for which our models predicted the strongest impacts were not included in the analysis.

Disaggregating the analysis of the differences between PB voters who are successful matched and those who are not, we do see that a higher proportion of white voters are successfully matched. This difference is strongest when comparing the match rate of established voters. Fewer non-white voters
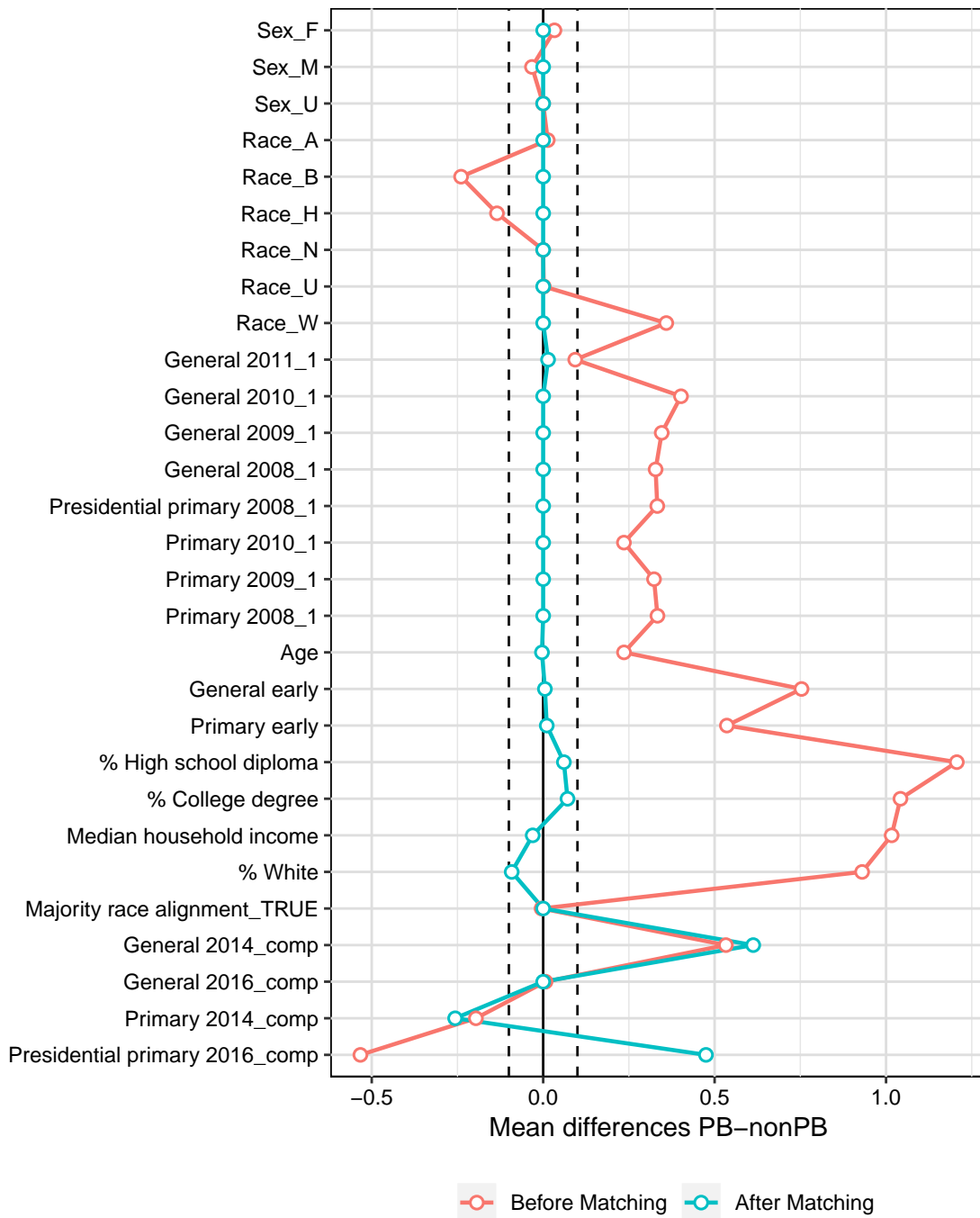
Figure A.1: Mean differences (literal difference in proportion for binary outcomes like vote turnout, standardized mean difference for continuous variables like age or % white) between PB and non-PB observations. 'Before matching' reports the difference between PB and non-PB voters in the entire voterfile prior to matching, 'after matching' sample reports the mean differences of the one-to-one matched sample. The selected match criteria substantially decreases imbalance on most measures as defined by difference in means, with the exception of competitiveness measures in the 2014 and 2016 elections, which are sensitive to outliers remaining in the sample.
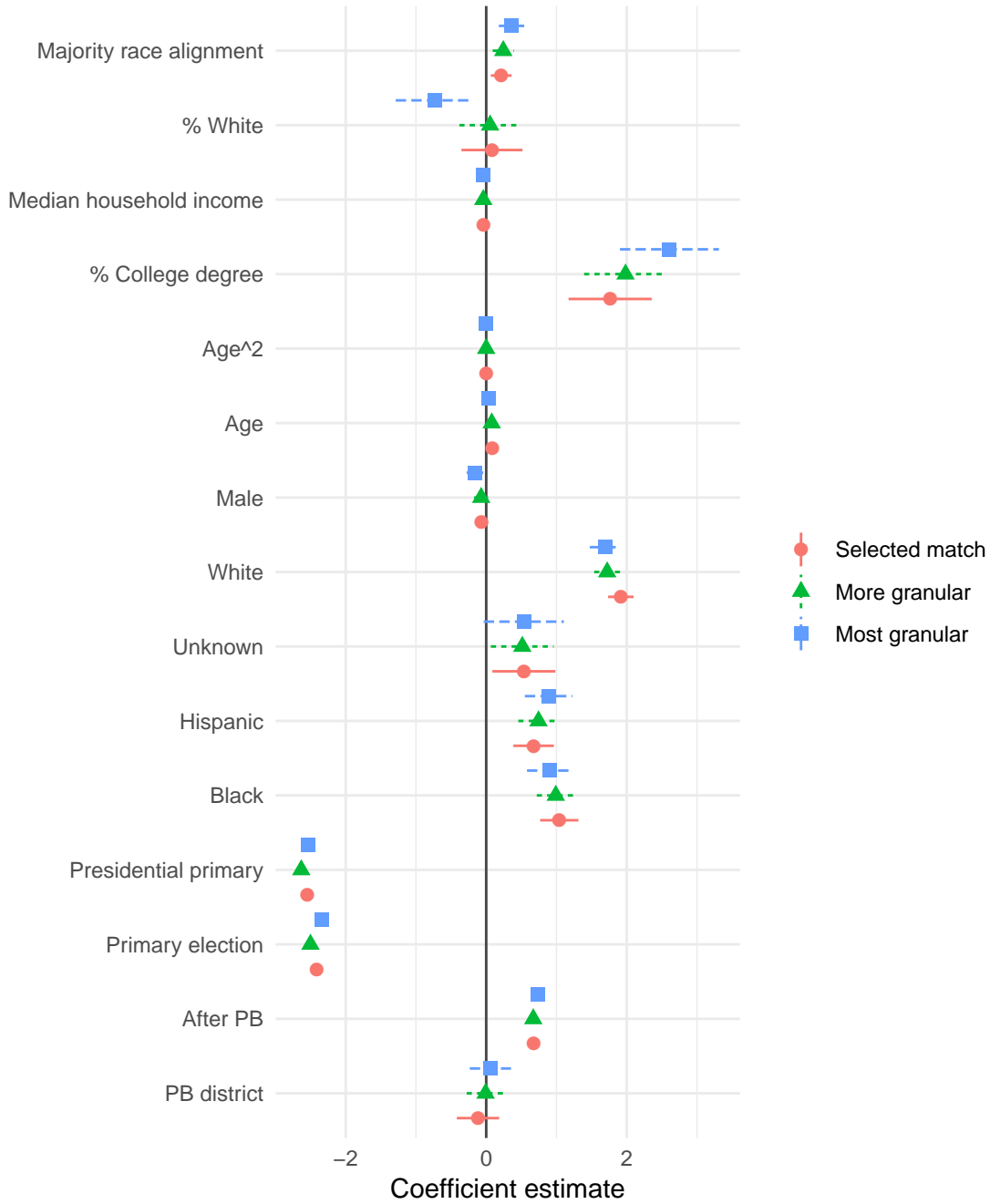
Figure A.2: Coefficient estimates for the base model (replicating Model 4 from Table 2 in the main text), fit on datasets generated with increasingly granular (restrictive) match criteria. 95% confidence intervals reported. Note that estimates of the Intercept and flag for age-based voter eligibility at time of election are excluded to restrict plot range and improve readability of the estimates for coefficients of interest. Main results are robust to increasing granularity of match criteria.
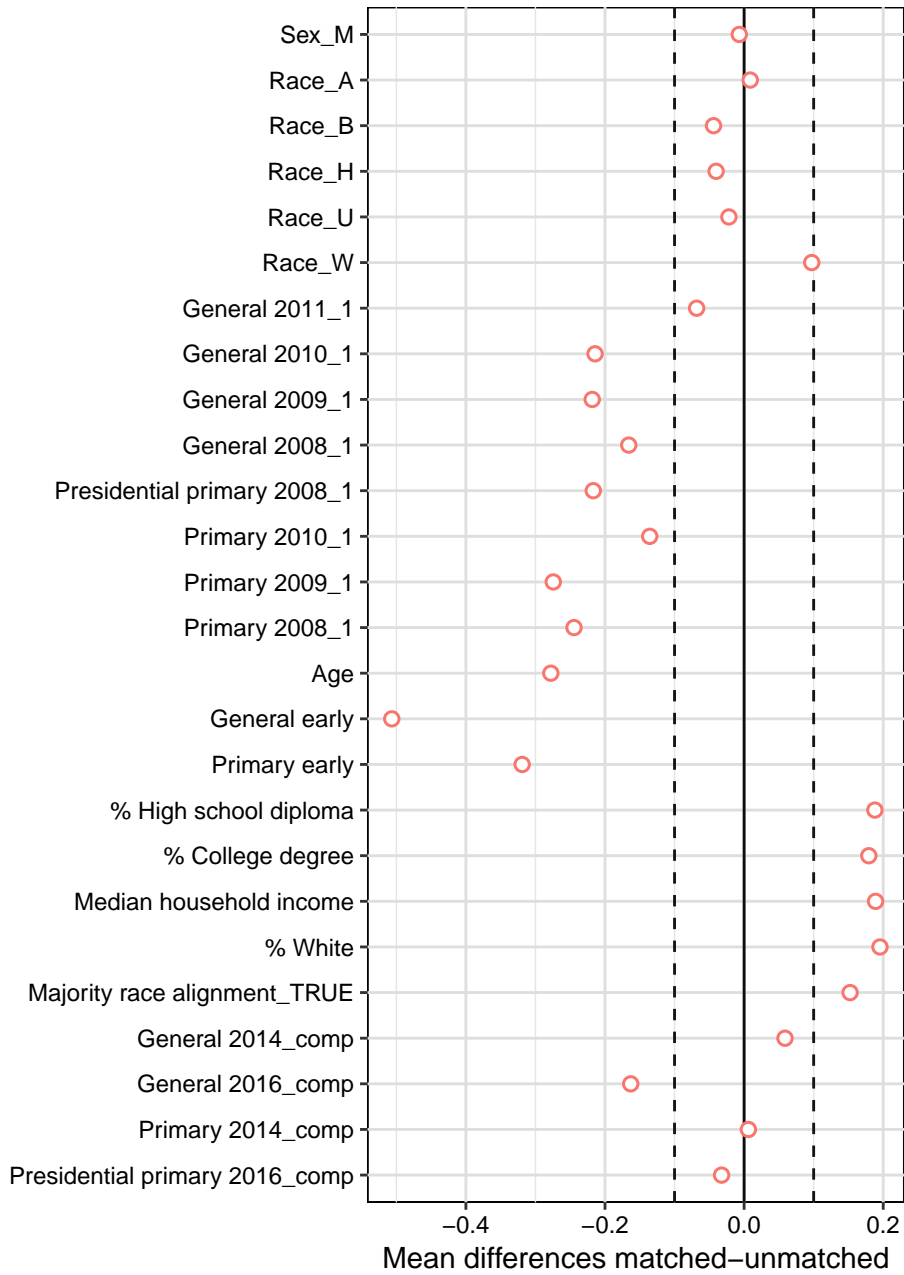
Figure A.3: Mean differences (literal difference in proportion for binary outcomes like past vote turnout, standardized mean difference for continuous variables like % white) between PB voters to make it into the selected matched sample, and those who were not successfully matched to a non-PB voter.

with a history of previous voting make it into the matched sample. This is likely due in large part to the fact that the relative proportion of experienced voters is higher among PB voters than the general population of registered voters. The skewness of the frequency of past votes is particularly pronounced for non-white voters, leaving the pool of potential voters proportionately smaller for these groups.