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Testing the Participation Hypothesis: 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, using the example of participatory budgeting (PB). Such initiatives often aspire to create more activated citizens, but there is still limited empirical research validating these claims. We link participants in New York City's participatory budgeting 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 8.4 percentage points. 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.

Keywords: Participatory budgeting, participatory democracy, voter turnout

Do individual experiences of participation in one setting reinforce participatory behavior elsewhere? 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. 2010; Mansbridge 1995; Pateman 1970). The belief in the power of participation has driven innovation and investment in local participatory initiatives around the world (World Bank 2008; Norris 2011; Pateman 2012; White House 2013). However, the empirical basis for this hypothesis is less well-established. As Mansbridge (1995) has stated, "[p]articipation does make better citizens. I believe it, but I can't prove it. And neither can anyone else" (p. 1). Making an empirical case for or against the participation hypothesis is challenging in the best of circumstances, since the question is particularly vulnerable to problems of selection bias: people who opt in to participation may be the type of people who are just in general more "participatory." Nevertheless, some studies have attempted to isolate causal estimates of the effect of prior participation on subsequent participation (e.g. Gastil et al. 2008; Gerber et al. 2003; Plutzer 2002), yet these remain few in number. This paper makes a direct contribution to the empirical literature on participatory democracy by examining the effect of individual involvement in participatory budgeting on subsequent voter turnout in New York City. This is the first research project in North America to directly examine the relationship between engagement in participatory budgeting and future validated political activity.

Participatory budgeting (PB) is a process where everyday people come up with ideas for, discuss, and vote on how to spend public money. By getting people directly involved in consequential decision making about issues that affect their community, many hope that this form of participatory democracy will strengthen the broader civic and political life of communities, including on electoral participation. These local participatory innovations build on traditional hopes of participatory democrats, with explicitly stated goals to "transform democracy" (in Vallejo, CA) or to "open up government [and] expand civic engagement" (New York City).[1] Evaluations of PB pro-

[1]These goals were explicitly stated in the official rule books for PB in each of these cities (City of Vallejo 2013 and PBNYC 2013). These goals have not been not idle words hidden in formal documents; themes of political transformation and democratic renewal have pervaded the outreach materials, framing of meetings and public events, and the explicit hopes for PB shared between observers and practitioners in the field.

cesses around the U.S. provide accounts of survey respondents reporting that they are more likely get involved in their communities as a result of PB (Crum, Salinas, Weber 2013; Jovanovich and Russell 2016), or even, as demonstrated in surveys of youth participants in Boston, that they are more likely to vote in regular elections (Grillos 2014: 25). Despite the prevalence of both anecdotal accounts of increased political participation as well as survey data on self-reported increased motivation to participate, rigorous empirical data of actual observed political behavior remains sparse.

In this paper, we move beyond the self-reporting of voters' intentions to directly assessing whether involvement in participatory budgeting is associated with a *change* in voters' observed behavior over time. The participatory budgeting process in New York City presents an opportunity to systematically test the participation hypothesis, since PB in New York City is district-based, and there is variation across city council districts in when they start their first PB process or whether they opt out of implementing it at all. In partnership with the city staff coordinating the processes around the city, we create a unique dataset by linking individuals in New York City who voted in their district PB process to their state voter records. With this data we 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. Matching techniques have been used to draw causal inferences from observational data (King et al. 2011, Iacus et al. 2012). 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 8.4 percentage points on average. While we find as a baseline that a district's first implementation of PB is associated with a 1.8 percentage point increase in non-PB voters' likelihood of voting, the effect of directly participating in PB is substantially greater. We also find that the positive effects vary across different sub-populations, with stronger effects for those groups who often have a lower propensity to vote: 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 positive effects from PB, while white voters experience the smallest effects.

This paper starts with a brief introduction to participatory budgeting and make the case for using New York City as an analytic focus. We then provide an empirical and theoretical context for our hypothesis that participation in PB may be associated with an increase in regular voter turnout. 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. We then 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. Finally, we conclude with some lessons for research and practice in participatory democracy.

Participatory budgeting

Participatory budgeting is a set of structured processes for residents to make direct decisions on how to spend public money. PB has grown rapidly in the US since it first came to Chicago in 2009. Each year, new cities embark on a participatory budgeting process for the first time and nearly all existing cities continue the practice. Currently, over 834,000 people have voted across over 65 cities in the US and Canada, directly deciding how to spend almost $371 million in public money (Participatory Budgeting Project 2020). 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 then placed on a public ballot where other community members can 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. While this is the most common format, it has also been used in public schools by students, and in 2018 New York City voters approved the expansion of PB to the first city-wide process in the country.

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 and advertisements. Voting is accessible, with translated materials to learn about projects usually available at voting sites (or online, in cities where online voting is an option).

The growth of PB in New York City provides a unique setting in which to evaluate PB's potential effectiveness as a strategy to activate citizenship practices. With several years of increasingly many council districts implementing PB, New York City provides an opportunity for structured examination of changes in voter behavior as subsets of new voters around the city are introduced to the innovative participatory process. PB first came to New York City in 2011 when it was introduced by four city council members, and has since been incrementally implemented in 31 of the 51 districts.[2] There is a central city-wide steering committee that oversees and supports PB across the city, though a district's implementation and approach ultimately has depended on the local city council member's staff.

In 2018, a typical NYC district allocated about $1 million to PB, and the average project cost almost $263,000. Winning projects have ranged from new school facilities, to new park reno-

[2]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).

vations, to security cameras, to street repairs, and more.[3] City council districts are on average about 164,000 people. Turnout for PB has grown from its first year in 2011, when it was only four districts with an average of 1,496 voters per district, to 30 districts in 2017 with an average of 3,335 voters per district (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), although often reporting higher levels of education than their districts in general (Community Development Project 2015).

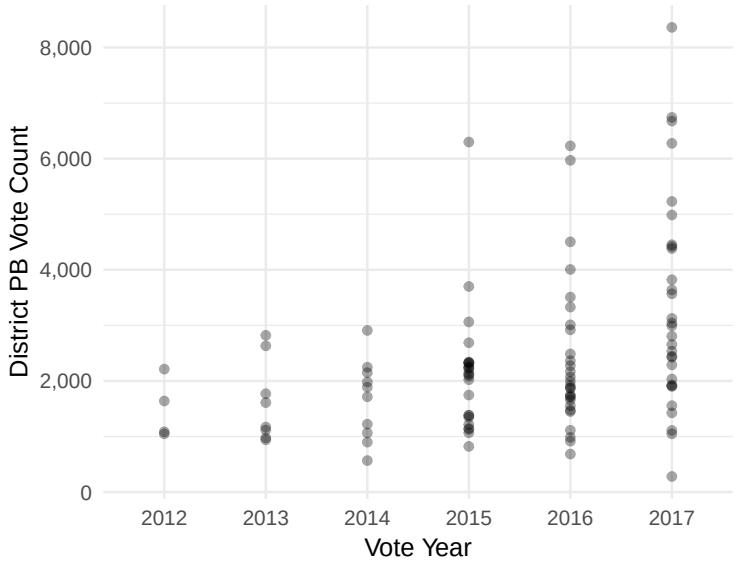

Figure 1: Number of voters per City Council District, by year.

Engaging citizens and increasing public participation is an explicit goal of many PB processes, and 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). PB has been reported as a way to create avenues for civic participation and political voice for underrepresented or marginalized people (Baiocchi 2003), though the depth of these effects is not guaranteed (Baiocchi and Ganuza 2014). While the participants of PB in the US tend to be more highly educated than the local population, there also often is an over-representation of black voters

[3]With the exception of one district, only capital projects are eligible for PB funding, not operations funding.

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

Most research on participatory budgeting and its democratic features has been 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.

These studies of PB tend to focus on the phases of the process prior to the final vote. These are the most intense stages of the process, as participants come up with project ideas and develop them into workable proposals through a process of deliberation and consultation with city staff across multiple events. While it is in these phases where people have the most sustained engagement and potential for learning and exchange, it is also where the fewest people actually participate.[4] The time commitment is an obstacle for many with family and employment constraints, and the extended engagement and political negotiation may not appeal to everyone. While learning how to speak the language of government is one of the skills participants learn, it can also alienate people (Talpin 2011). However, there is less research on those who engage with PB at the least costly level of participation: voting. Over the course of nine days, the voting process touches many more people, as city staff, local organizations, and others actively recruit voters. Drawing in members of the public in with a straightforward question "How would you spend $1,000,000?," organizers bring the voting to where the public is: grocery stores, libraries, subway stations, and other public spaces, including, in NYC, online. Voters are given information about what PB is, told

[4]In New York City the "delegates," who develop and deliberate on the proposed projects, represent on average 2% of the total voting body in a District.

that their vote will help decide which project to fund in their community, and are invited to vote for whichever projects they think would best benefit the neighborhood. This low barrier voting effort, grounded in explicit goals of community benefit, provide a low-cost opportunity for individuals to participate in an activity that will impact how money is spent in their community. It is the behaviors of these high impact but low intensity voters who we examine in our study.

The participation hypothesis and voter turnout

As described above, PB is generally understood to be an innovation in the tradition of participatory democracy. One of the core tenets 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; Barber 1984; Pateman 1970). Participatory practice can come in many forms (see Rowe and Frewer 2005) that could have differential effects, but the core idea is that starting to participate in one setting creates a virtuous cycle that can lead to more participation elsewhere. Many democratic theorists, including Pateman (1970), Barber (1984), and Verba, Scholzman and Brady (1995) emphasized participation even in 'non-political' spaces like workplaces or churches as important precedents for democratic political engagement. With perceived declines in traditional spaces for participation and civic learning (Putnam 2000, Macedo et al. 2005), practitioners and scholars creatively identified alternative opportunities for engagement and participatory practice to counterbalance perceived democratic declines and strengthen civil society (Fung and Wright 2003; Norris 2011 ch. 12, Baiocchi et al 2011). Much of this innovation has remained grounded, like PB, in the idea that participation, community engagement, and deliberation among ordinary citizens can be a pathway to activated democratic practice.

Empirical evidence for this hypothesis of the positive effects of participation is mixed, with a variety of studies across a range of settings and specifications. Even scholars who agree that active engagement is a requirement for a thriving democracy see potential risks from individuals'

experiences of participation: some political participation carries with it risks of conflict, messy compromise, and unpleasant feelings that collectively may depress, rather than enhance, people's willingness to get involved (e.g. Hibbing and Theiss-Morse 2002, Eliasoph 1998). Indeed, there is an extensive thread of research in public administration literature detailing how traditional public meetings and ineffectual engagement processes can generate negative or demobilizing results for participants (Nabatchi and Leighninger 2015, Ch 2; Nabatchi and Amsler 2014; Irvin and Stansbury 2004).

In recent years many studies have worked to develop an empirical understanding of the conditions when and how participatory activities can live up to the expectations of them. Much of this research has focused on the crafting of careful deliberative events, where people come together to debate a particular policy issue in a controlled setting, with a focus on whether and how participating in these events increases various types of political efficacy, under the assumption that both personal and political efficacy are prerequisites for participation.[5] Even within and across these controlled deliberative settings, results are mixed. Myers et al. (2018) finds no increase in self-reported willingness to participate after taking part in a deliberative event, while Morrell (2005) finds no increase in general political efficacy from deliberation, but some increase in people's feelings of efficacy around their own ability to deliberate. Nabatchi (2010) finds only some support for the positive effects on external efficacy after deliberating, with no significant changes to internal efficacy. Using self-reports of participation rather than efficacy measures, a methodologically careful study out of Sweden finds no "spillover" of participatory activity across contexts (Adnam 2008).

A number of studies have offered clearer evidence for positive individual effects from participation. Along with classic work by Verba, Schlozman, and Brady (1995) relating participation in a range of community spaces to voter turnout and other political action, more recent work has highlighted potential for positive participation feedback loops. Gastil et al. (2010) offer one of

[5]Political efficacy is generally an outcome of interest because it is available as a self-reported characteristic, and, importantly, is seen as being an important predictor of further political participation (Gastil and Xenos 2010; Abramson and Aldritch 1982; Finkel 1985).

the most compelling empirical accounts in support of the participation hypothesis, providing clear evidence, based on actual court and voting records, linking participation in jury deliberation with actual increases in jurors' probability of voting. Jacobs et al. (2009) find from self reported surveys that engagement in both general political discussion and structured deliberative activities increases participation in other settings; however they are unable to replicate this positive effect in pre-post surveys of participants in a deliberative forum about Social Security. More recently, Boullianne et al. (2020) demonstrate a causal pathway by which participating in a single extended deliberation on climate change strengthens subsequent participation in civic and political life, based on self-reports from panel surveys before, soon after, and over two years after the event.

A notable observation from reviewing these disparate conclusions is that there are many different kinds of participation, and that the type of experiences (including costs to participants, structure of interaction, opportunity for connection rather than conflict, and the general framing of the event) may all have an impact on the outcomes for participants. While the specific psychological or social mechanisms driving the participation hypothesis require further study, some research has demonstrated that activating one's sense of civic duty may have positive effects on voter turnout (Gerber et al. 2008; Blais and Achen 2018). Reinforcement of civic duty is an important element of the framing and context of most participatory budgeting processes. This reinforcement of norms of civic duty may combine with the directly observable impact of one's participation, such as when PB voters see a project they voted for funded and implemented. This makes PB more analogous to jury participation, with its real work impact and civic framing (Gastil et al. 2010, Ch. 7), rather than deliberative events, where regardless of the duration and quality of the deliberation that a participant may experience, the results do not go on to directly shape polices or practices (Myers 2018 et al.; Nabatchi 2010).

Methodologically, many studies of the participation hypothesis draw on a combination of formal experimental design (Morell 2005) or one-off pilot projects with pre-post surveys (Nabatchi 2010; Boullianne et al. 2020). Experiments serve as the "gold standard" of causal inference by helping the researcher control for selection bias and other confounding factors, but also limit what

can possibly be studied due to few practical opportunities for research in real world contexts. Many studies, whether experimental or observational, also often rely on self-reported or prospective accounts of participation, rather than observed behavior (Crum, Salinas, Weber 2013; Jovanovich and Russell 2016; Grillos 2014; Myers et al. 2018). In contrast, voting is both a directly observable action as well as centrally important in modern democracies and a key indicator of a healthy citizenry. While this risks conflating "willingness and opportunity" to participate (Myers et al. 2018), it allows us to see what people actually do and what impact their actions may have after having a prior participatory experience. Across the existing evidence for the participation hypothesis, Gastil and collaborators' work with jurors remains the exception that follows participants' actual political behavior. Our study aims to balance the causal rigor of experimental designs with the external validity of real-world observational studies, while maintaining a strongly empirical focus on observed behavior. We focus on the effects of participation on subsequent voting behavior as seen in the real world, and use matching methods to simulate a experimental control to draw stronger causal inferences with observational data.

Factors that affect turnout

Our primary hypothesis is that participation in a PB process will be associated with a higher probability of voting in regular elections. To assess the degree to which this is true, we also need to account for the many other factors that affect 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 potential selection bias, where the people who show up to participatory budgeting events are already more likely to turn out to vote. Thus, to understand any effect of PB, we must account for individuals' baseline expectation of turning out. 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), with some estimates suggesting that half of voters either always vote or always abstain (Fowler 2002). Gerber et al. (2003) attempt to estimate a causal effect of voting habit on subsequent elections and find a 45% greater chance of voting among those registered voters who had voted the year prior compared to those who did not. 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).

Along with past voting behavior, education and race are also important predictors 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 racial context in which they live have also 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, competitiveness is an important predictor to control for in our models of turnout.

Methodology

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 New York 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 was not available. This enables us to estimate the effect of participating in PB by examining the changes the formal voting behavior before and after an individual first votes in PB, compared with similar individuals living in a district without PB available. To do this, 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.

Data

This analysis uses individual, census tract, and election district level data for registered voters, including both those who did and those who did not vote in PB. The individual level data is provided by Catalist, a voter data service, 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 geo-located address (used to identify the election district and census tract), 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 from Catalist, 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.

We also include measures of competitiveness for each election cycle from 2008 through 2017. As election district level data for elections prior to 2014 is not available from either the New York City or New York State Boards of Elections,[6] we instead assigned each voter the full set of major political districts for which regular elections are held (City Council, Congress, Assembly, and State Senate), using historically appropriate boundaries files for both 2000 and 2010 census districts. Only PDF summaries for historical elections are available from the NYC Board of Elections, so we wrote a webscraper to extract each race's results from the hundreds of election recap PDFs available from the NYC BOE website (vote.nyc). From this, we were able to identify, for any given voter, the margin of victory of the most competitive major race on their ballot for any given election.

PB voters

We identify PB voters from voter lists maintained at the district and central city offices. This data set includes only individuals who actually voted on the final ballot of a PB process. While PB has many moments of participation, including more intense and recurring deliberative events for a small set of highly engaged volunteers, our study focuses on the larger population involved in the 'lightest touch' of the PB process: voting on the final project proposals. For the first several years of PB, some districts and central city staff collected basic identifying information about voters,

[6]Two different cycles of Freedom of Information Act requests failed to elicit machine readable election results or results at the election district (the equivalent of precinct in most other jurisdictions).

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 (PBP) obtained the 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.

From all city sources, we were able to match 10,030 PB voters to their records in the state voter file, from nearly 19,000 individual PB records. 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 districts began implementing 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.[7] Finally, for this analysis, we limited our sample to individuals with a current (as of data collection in 2018) address located within New York City. This left us with a final PB voter sample size of 7,797 people.

[7]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.

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 had never implemented PB as of 2017. 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

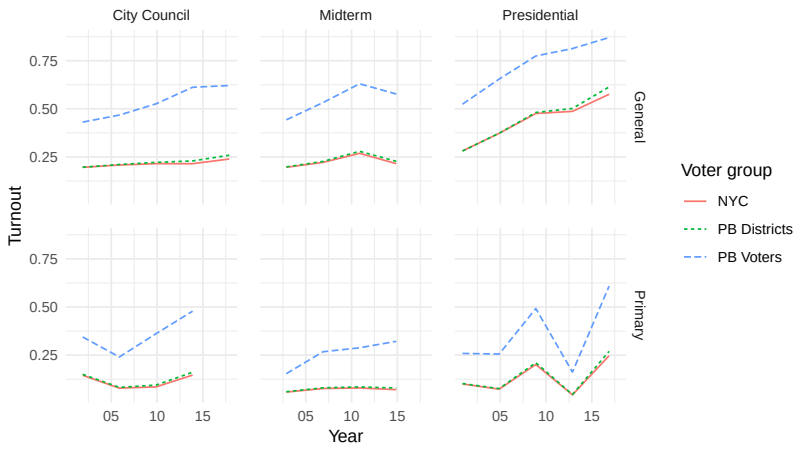

Figure 3: Voter turnout of registered voters in New York City as a whole, PB districts in NYC, and PB voters whose data was collected and included in our sample.

NYC voter population. The matching method described below ensures that our analysis compares our PB voters to similar non-PB voters elsewhere in the city.[8]

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 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.[9]

Given our large set of potential matches and high level of overlap in most population characteristics between our PB voters and the whole city voting population, we use one-to-one coarsened

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

[9] 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-level
Age	Age[i]	-
Gender	Male/Female[ii]	-
Race	White, Black, Asian, Latinx, Unknown[ii] Membership in majority racial group in census tract [ii]	% pop. identified as white[iii]
Voting History	Indicator for each 2008-2012 election[ii] Count of votes cast 2000-2007 [v]	-
Education	-	% pop. with college degree[iv]
Income	-	Median household income[iii]
Electoral competitiveness	% Margin of victory for race with most votes for a given individual and election (excluded from primary analysis)[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] 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. [vi] This variable is excluded from the match used for primary analysis. Match is by high or low competition districts, with high competition defined as lower than median margin of victory (competition generally shows a high level of clustering, with most voters clustered at the median with a long tail of more competitive races). Many elections have very little within-city variation in competitiveness because a national, state, or city-wide election dominates all election districts; to speed computation only elections with observed variability in competitiveness across the city were included as match criteria.

exact matching (Iacus et al. 2012), a technique that directly matches two subjects on a set of relevant covariates, reducing model dependence and ensuring balance on even a large number of covariates (such as the many predictors of individual turnout).[10] A match is only achieved if they are effectively "the same" based on specified thresholds. 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

[10]With many more controls than PB voters, we randomly sample one matching control individual for every PB voter in each strata. An alternative would be to allow many matches from the control population and then use appropriate weights in the subsequent regressions. With our relatively large treatment case sample size, one-to-many matching with weights actually provides a larger sample than is necessary to fit reliable and informative models, while significantly increasing complexity and computation time.

and the matching strata are defined in Table 1. Using these criteria, we are able to successfully match 5,362, or 69%, of our 7,797 PB voters to a non-PB voter., resulting in a final sample of 10724 including non-PB voters. The goal 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. This choice of match thresholds balances the trade-offs between closer similarity between PB and non-PB voters in the analysis, and preserving the representativeness of the PB population included in the analysis. 96% of the starting set of PB voters match a non-PB voter exactly on age group, gender race, and recent voter history. Adding neighborhood (census tract), competitiveness, and/or city council district characteristics progressively restricts the set of of PB voters with a positive match that can be included in the sample.

The sample included in the primary analysis below uses only individual and tract level variables in the match procedure. We control for individual electoral competitiveness history in regression models but excluded the variable from the matching process. Even with a minimal binary high/low competition cutoff and relaxing the granularity of the other individual and tract level measures, ensuring a match across multiple historical election cycles (effectively adding at least 8 additional parameters to match), dramatically reduced PB sample size, with only 2,061, or 26% of PB voters having an exact match in competitiveness history. Competitiveness is of course an important driver of turnout, and is included as a control variable in all regression models. We also replicate the primary analysis within the subsample of only voters matched on competiveness and observe an even stronger effect of PB within this sample, further supporting evidence that the effect of PB is not merely an artifact of more competitive election cycles. Our online appendix provides a detailed illustration of the stability of the effect of PB on voter turnout across many different matching specifications, as well as illustrating the balance assessments of our match choice.

Understanding that PB is an intervention which certain council members opt into, we also considered matching on a number of council district measures (including district level demographics and council member characteristics including incumbency status and level of competition they face in each city election cycle). However, it is clear that with only 51 districts, the unique profile of

each district is challenging to match to any other single district. The fact of district idiosyncrasy is unavoidable in this observational study and while we include district random effects in the regression analysis, we cannot simply abstract away all the elements of potential bias from district factors confounding PB's effects. Matching on district characteristics imposes significant constraints on sample sizes (under 10% of PB voters matched), resulting in unacceptable imbalance between PB voters included in and excluded in the analysis sample (while not substantially impacting the main effect of PB, as documented in the appendix). Instead of matching on district, we include secondary analyses which compare PB voters to non-PB voters *in their same district* as well as the comparison of non-PB voters in PB districts to voters in the same non-PB districts that are used to construct the comparison group for the primary analysis. The first analysis effectively controls for the district effects by removing the district level variation (but increases known selection bias in the comparison of PB voters to non-PB voters). The second comparison in comparison provides a 'placebo' test, assessing how much of the apparent effect of PB observed for PB voters may in fact just be an artifact of their districts or council members.

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. In other words, we compare the *changes* in voting behavior over time for PB and non-PB individuals, after controlling both for membership in the PB 'treatment' group and time trend effects.[11]. We use hierarchical 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, tract, and competition controls.

[11] See Angrist and Pischke (2008) for a introduction to difference-in difference models of increasing complexity.

The base model thus takes the form:

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

where

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

$$\alpha_i \sim N(0, \sigma_i), \text{for } i = 1, ..., 10724$$

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 individual's first PB vote, making δ the difference-in-difference estimate: the expected effect of PB. $\mathbf{X_i}$ 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. ξC_{iv} are the competitiveness scores reported for voter i in election v. 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:

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

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 from the model variance-covariance matrix.

Results

This analysis demonstrates a strong and statistically significant effect of voting in participatory budgeting on the probability of voting in subsequent regular elections. On average, participation in PB is associated with a marginal increase of 8.4% in an individual's predicted probability of voting, all else being equal.[12] 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.[13] 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

[12] 8.4% (95% CI: 7.8-9%) 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, calculated with the R `margins` package.

[13] 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	Compet.	Matched Comp.
PB district	−0.395	−0.076	−0.063	−0.099	0.053
	(0.208)	(0.165)	(0.152)	(0.160)	(0.284)
After PB	0.653*	0.628*	0.629*	0.700*	1.559*
	(0.024)	(0.024)	(0.024)	(0.026)	(0.124)
Primary election	−2.375*	−2.392*	−2.392*	−2.218*	−2.143*
	(0.016)	(0.016)	(0.016)	(0.017)	(0.085)
Pres. Primary	−2.442*	−2.481*	−2.481*	−2.574*	−2.555*
	(0.026)	(0.026)	(0.026)	(0.029)	(0.126)
Black		−0.965*	−0.757*	−0.769*	0.231
		(0.098)	(0.102)	(0.106)	(0.429)
Asian		−2.088*	−1.920*	−1.940*	−1.244*
		(0.075)	(0.078)	(0.081)	(0.385)
Hispanic		−1.271*	−1.071*	−1.095*	−1.137
		(0.097)	(0.107)	(0.111)	(0.586)
Race Unknown		−1.526*	−1.300*	−1.304*	−1.565*
		(0.155)	(0.163)	(0.169)	(0.760)
Female		0.038	0.035	0.037	−0.030
		(0.040)	(0.040)	(0.041)	(0.197)
Age in years		0.134*	0.129*	0.132*	0.049
		(0.007)	(0.007)	(0.008)	(0.030)
Age^2		−0.001*	−0.001*	−0.001*	−0.0001
		(0.0001)	(0.0001)	(0.0001)	(0.0003)
18+ at vote		−7.659*	−7.662*	−7.584*	−18.499
		(1.007)	(1.007)	(1.007)	(409.613)
% college educated			1.794*	1.744*	2.895*
			(0.228)	(0.238)	(0.914)
Median HH income			−0.031*	−0.032*	−0.026
			(0.010)	(0.010)	(0.048)
Majority Race			0.140*	0.151*	−0.096
			(0.062)	(0.064)	(0.308)
Competitiveness				−0.852*	−0.748*
				(0.048)	(0.226)
Constant	1.683*	−2.620*	−3.300*	−3.166*	−2.370*
	(0.183)	(0.246)	(0.249)	(0.261)	(0.885)
Year fixed effects?	Yes	Yes	Yes	Yes	Yes
Observations	193032	193032	193032	173648	9613
Akaike Inf. Crit.	166756.500	160682.400	160604.500	144000.700	6819.564
Bayesian Inf. Crit.	166898.900	160906.100	160858.800	144262.400	7006.007

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 *. Reduced N in 'Compet.' model is a result of removing subsets of year-election observations that were not linked to election results for that election district (for example not every district had elections in every primary cycle). 'Matched comp.' reports results from the full model replicated in the smaller sample with full match control on competitiveness.

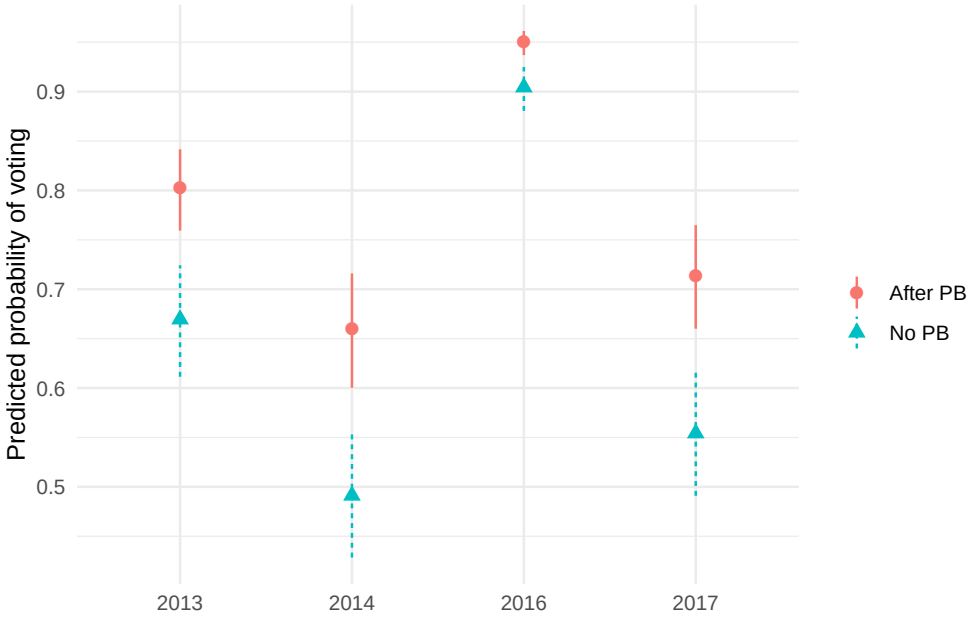

Figure 4: Predicted probability of voting in general elections, with 95% confidence intervals, using the full model including competitiveness controls. Predictions for non-PB voters, showing effect of hypothetical participation in PB vote. All other variables set to their means, representing the 'typical' voter only conditioned on PB participation. 95% confidence intervals generated by simulation from model variance-covariance matrix.

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 nearly a 5% increase in the probability of turning out in the 2016 general election to an over 16% increase in the lower-turnout 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 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.

Robustness to match specification and alternate comparison methodology

The positive effect of PB is remarkably robust to a very wide range of matching specifications. The online appendix provides a detailed overview of both the match balance tradeoffs, as well as demonstrating the stability of PB's effect across many different configurations of matching criteria. As discussed above and further in the appendix, we started out with an intention to include electoral competitiveness as a primary match criteria. Full matching on competitiveness came at too high a cost of sample size and representativeness and, importantly, did not substantially alter our main effect. In fact, replicating the regression models in the restricted competitiveness samples substantially increased the magnitude of the effect, resulting in a marginal effect of a 15.4% (CI: 12.8-18%) increase in the probability of turning out to vote after participating in PB rather than the 8.4% from the main model. See the last column Table 2 for a full comparison of estimated effects for this more restrictive match sample.

The other crucial set of conditions for which we were not able to establish a fully balanced and representative sample through matching were the council district level variables (i.e. district demographics and council member characteristics). As with competitiveness, our main effect of PB is robust in the smaller samples resulting from including district characteristics in the match. In order to better differentiate to what extent the effect of PB is simply an artifact of idiosyncrasies of PB districts or council members, we conducted an an additional pair of robustness tests. We first compared PB voters and non-voters *within PB districts*; if the apparent effect of PB in our

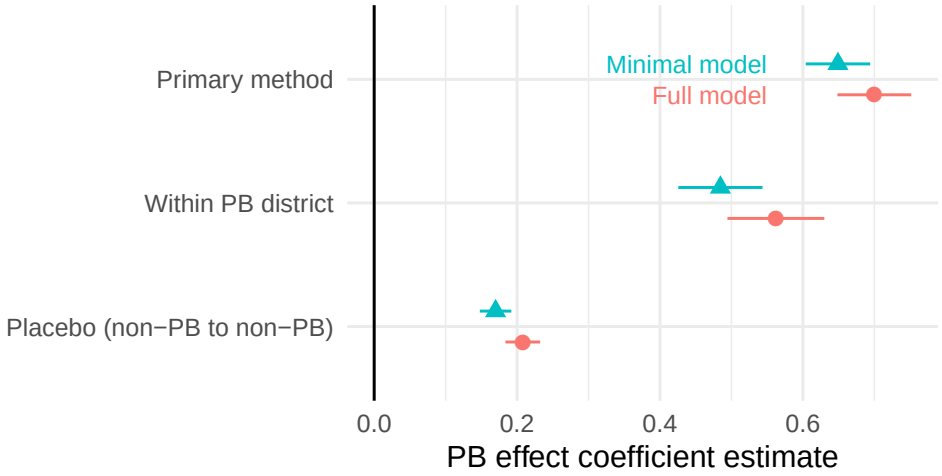

Figure 5: PB effect coefficient estimates for our primary method in contrast to the within-district and non-PB comparison from the same difference-in-difference model specification used in the minimal and full models described reported above. The start date of PB for the PB district voters in the placebo models is defined as the date PB was first implemented in the district. Points are model point estimates, ranges are the 95% confidence intervals of that estimate. Confidence intervals are tighter for the placebo models as sample sizes of non-PB voters were greater.

primary models is an artifact of the district, we should see the effect of PB disappear. Second, we compared non-PB voters in PB districts to similar voters in non-PB districts. In this case, if the apparent effect of PB participation is confounded by district effects that are correlated with the introduction of PB, we should see this effect persist even for these "placebo" non-voters who were not in fact known to be exposed to the treatment.[14]

As Figure 5 illustrates, results of these tests largely support the primary model evidence of an effect of PB. The placebo model, while still showing a significant positive effect, is significantly smaller, with coefficients hovering around 0.2 rather than 0.7, corresponding to a much smaller marginal increase in probability of voting of closer of 1.8% (rather than 8.4%). This demonstrates that voting in PB has an effect on turnout that is independent of the increase associated with living in a district after it implements PB. Meanwhile, the within-PB-district model, shows moderated but still substantial effects of PB.[15]

[14]Thanks to an anonymous reviewer for suggesting this 'placebo' framing of a non-PB comparison group.

[15]Note, it is not entirely clear what the causal story behind the small district-level effect of PB may be. It could be that the effect of PB has has absorbed district characteristics correlated with the introduction of PB or it could be that the assorted effects of PB on community networks and civil society produce spillover effects that mean even non-voters are more activated in subsequent election cycles.

Effects by subgroups

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 in 2018), and neighborhood characteristics of voters including income and education. Figure 6 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.

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 6 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 youth status are the two variables with the clearest moderating influence on the effect of PB. Individuals in census tracts with fewer college educated people (as a percentage of the population) or who were not older than 30 at the time of the 2016 vote 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 5% and over 15%, respectively. Minority race membership and lower income neighborhoods demonstrate small positive moderating effects, with additional increases in probability of voting associated with PB participation of around 1.5% and 0.7%. There is no evidence for differential effects of PB on the basis of gender.

Within our sample, predicted turnout effects demonstrate statistically significantly different effects associated with PB participation for different racial groups. When PB participation combines with the diverging baseline turnout rates of different racial groups, the total observed effects vary substantially. Figure 6 shows that different racial groups see statistically significantly different

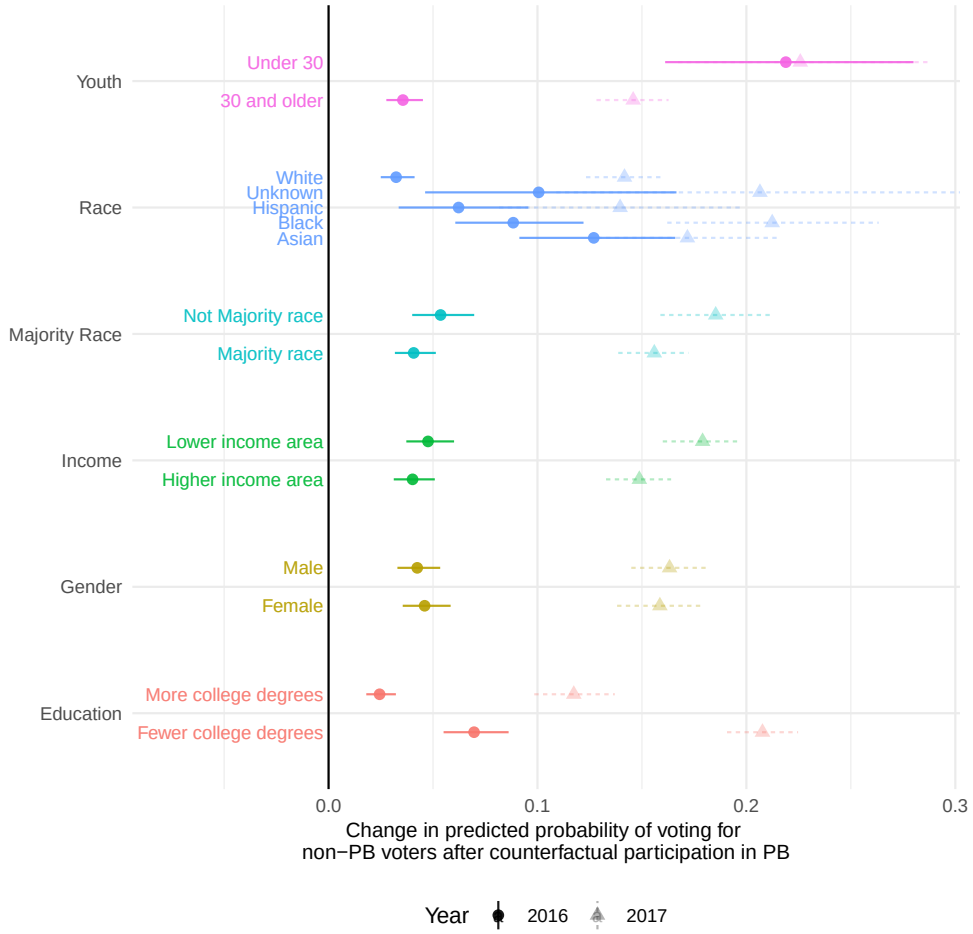

Figure 6: Change in predicted probabilities of voting in the 2016 and 2017 general elections with and without counterfactual participation in PB, with effect conditional on sub-group. Change in predicted probability of voting in 2016 are shown by bold circles, while the change in predicted probability of voting in 2017 are shown by faded 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.

effects from PB, with white voters demonstrating the smallest effect and Asian and black 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, but also a consequence of the higher baseline voting rates of the white voters in our sample (within the matched sample, white voters had between 10-23% higher probabilities of voting in the 2016 general election than voters in other racial groups). This variation illustrates how the overall effect of PB may be amplified for populations with lower general probabilities of voting. These differences allow PB to have greater

leverage among Asian and black voters than white voters. While voting in PB has a consistently positive effect on subsequent electoral participation, for all populations and cuts across the data, the specific stories about sub-group outcomes are more dependent on the setting and conditions of this study: PB in communities with different baseline patterns of engagement will also include different sub-populations who may receive greater impact from participatory interventions like PB.

As a further comparison, Figure 6 also shows the estimated effect of PB in the 2017 general election. 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 increases 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, with only a marginal tendency for black voters to demonstrate stronger effects from PB.

Discussion and Conclusion

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 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 for New York City, 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, since participatory budgeting has so far been an opt-in process. 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, suggesting that civic learning can take place among non-voters. Second, while New York City allows for many

advantages in terms of large-scale data analysis, the generalizability of these findings to different kinds of cities and PB models requires further research. 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 other cities that have chosen to implement PB (Johnson 2017), and PB as implemented in other settings may not have the same results. For these reasons, it will be important to replicate the analysis presented here in other settings where individual voter data are available.

As well as these limitations imposed by the study's specific New York City context, there are additional constraints on generalization resulting from practical methodological considerations. While we did our best to maximize the representativeness of the set of PB voters used in this analysis, our main analysis still cuts out about 30% of the potential PB population. No major demographic groups or types of voters were unmatched, but there were some types of voters who were less likely to have a successful match and for whom these results may have more limited generalizability: in particular voters with a longer history of turning out to vote, older voters, and voters in lower income or less well-educated census tracts.[16]. In an effort to preserve empirical support across more of our PB population, we opted not to match on competitiveness for our main analysis, instead controlling for this measure in our regression models. In this case we accepted reintroducing regression assumptions about competitiveness in exchange for more overall more representative data.

This analysis provides a clear contribution to the body of empirical evidence outlining whether and how participatory activity in one setting can support subsequent political participation. This study is distinctive in two key features. First, we limit our setting and evidence to observed real world behavior, rather than relying on self-reported past or projected future actions. Second, the voting in PB is different from many of the deliberative and high-intensity exercises that have dominated much of the recent empirical literature on participation effects — many of which report null findings (Morrell 2005, Myers et al. 2018, Nabatchi 2010). In these events, people may talk about

[16]See the online appendix for more detail on the balance of in- and out-of-sample PB voters

political issues, but their discussions end there – the discussion does not link directly to real-world consequences for participants or their communities. The PB vote provides a contrast both in intensity of engagement and in real-world relevance; the projects that PB voters choose represent physical investments in their communities. In the PB vote, we have an example of a participation opportunity that is both consequential and accessible to many people, while remaining explicitly tied to values of political responsibility and participatory citizenship. In the 'mixed bag' of evidence for participation's feedback effects, this study affirms the positive findings of other scholars who have identified that activities embedded in people's real worlds (such as the everyday discursive participation of Jacobs et al. 2009) and linked explicitly to civic responsibilities (as with the jury service in Gastil et al. 2010) can have real impacts on their continued democratic participation.

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.

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
PB district	−0.10 (0.16)	−0.12 (0.16)	−0.11 (0.16)	−0.10 (0.16)	−0.16 (0.16)	−0.14 (0.16)	−0.11 (0.16)
After PB	0.70* (0.03)	0.67* (0.03)	0.79* (0.05)	0.68* (0.04)	1.20* (0.08)	1.09* (0.08)	0.68* (0.03)
Primary election	−2.22* (0.02)	−2.23* (0.02)	−2.22* (0.02)	−2.22* (0.02)	−2.22* (0.02)	−2.22* (0.02)	−2.22* (0.02)
Pres. Primary	−2.57* (0.03)	−2.61* (0.03)	−2.59* (0.03)	−2.58* (0.03)	−2.59* (0.03)	−2.58* (0.03)	−2.57* (0.03)
Competitiveness	−0.77* (0.11)	−1.00* (0.13)	−0.77* (0.11)	−0.77* (0.11)	−0.77* (0.11)	−0.76* (0.11)	−0.82* (0.11)
Black	−1.94* (0.08)	−2.95* (0.11)	−1.94* (0.08)	−1.94* (0.08)	−1.94* (0.08)	−1.94* (0.08)	−2.01* (0.08)
Asian	−1.10* (0.11)	−1.52* (0.14)	−1.10* (0.11)	−1.10* (0.11)	−1.10* (0.11)	−1.09* (0.11)	−1.15* (0.11)
Hispanic	−1.30* (0.17)	−1.66* (0.22)	−1.31* (0.17)	−1.30* (0.17)	−1.30* (0.17)	−1.30* (0.17)	−1.34* (0.17)
Race Unknown	−0.85* (0.05)	−0.85* (0.05)	−0.86* (0.05)	−0.85* (0.05)	−0.83* (0.05)	−0.85* (0.05)	−0.84* (0.05)
Female	0.04 (0.04)	0.04 (0.04)	0.04 (0.04)	0.10 (0.05)	0.04 (0.04)	0.04 (0.04)	0.05 (0.04)
Age in years	0.13* (0.01)	0.14* (0.01)	0.13* (0.01)	0.13* (0.01)	0.13* (0.01)	0.13* (0.01)	0.22* (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	−7.58* (1.01)	−7.52* (1.01)	−7.58* (1.01)	−7.59* (1.01)	−7.57* (1.01)	−7.57* (1.01)	−6.86* (1.01)
% college educated	1.74* (0.24)	1.79* (0.24)	1.77* (0.24)	1.74* (0.24)	2.26* (0.25)	1.83* (0.24)	1.74* (0.24)
Median HH income	−0.03* (0.01)	−0.03* (0.01)	−0.03* (0.01)	−0.03* (0.01)	−0.03* (0.01)	−0.02 (0.01)	−0.03* (0.01)
Majority Race	0.15* (0.06)	0.14* (0.06)	0.58* (0.08)	0.15* (0.06)	0.15* (0.06)	0.16* (0.06)	0.11 (0.06)
After PB * Black		0.22* (0.11)					
After PB * Asian		0.13 (0.10)					
After PB * Hispanic		−0.10 (0.13)					
After PB * Unknown		0.18 (0.21)					
After PB * Majority match			−0.11 (0.06)				
After PB * Female				0.03 (0.05)			
After PB * % college					−0.76* (0.13)		
After PB * Median HH. inc.						−0.04* (0.01)	
Age < 30							0.72* (0.20)
After PB * Age < 30							0.55* (0.14)
Constant	−3.17* (0.26)	−3.16* (0.26)	−3.51* (0.26)	−3.21* (0.26)	−3.45* (0.27)	−3.31* (0.26)	−5.67* (0.32)
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subgroup time trends?	None	Race	Maj. Match	Gender	Education	Income	Youth
Observations	173648	173648	173648	173648	173648	173648	173648
Akaike Inf. Crit.	144000.70	143250.10	143739.20	143972.70	143794.10	143925.20	143550.80
Bayesian Inf. Crit.	144262.40	143833.80	144081.40	144314.90	144126.20	144257.40	143903.00

Notes: "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 $p < 0.05$: *.

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