


RESEARCH ARTICLE  

Making sense of voting “habits”: Applying the process model of behavior change to a series of large-scale get-out-the-vote experiments

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Abstract

I apply a new theoretical framework to voting to more cohesively bridge the economic cost-benefit model of voting with the psychology-motivated voting-as-a-habit literature. This new theoretical frame gives greater clarity as to how a vote in one election might beget a vote in another election, while yielding testable predictions as to which circumstances are more favorable for developing turnout persistence. To test these predictions, I make use of a novel dataset consisting of nine large-N, door-to-door voter mobilization field experiments in various election contexts (with ~1.8 million voters in total). Consistent with prior empirical research, my analysis finds that being nudged to vote in one election leads to increased turnout four years later. But the main contribution of this paper is that the theoretical framework’s predictions and the corresponding empirical results make sense of turnout persistence heterogeneities that have been detected in certain prior empirical studies but not others.

Introduction

There is consensus among empiricists in American politics that voting is habit-forming (Aldrich, Montgomery, and Wood, 2011; Coppock and Green, 2016; Denny and Doyle, 2009; Franklin and Hobolt, 2011; Garcia Bedolla and Michelson, 2012; Gerber, Green and Shachar, 2003; Meredith, 2009; Solvak and Vassil, 2018). But there are key components of the theoretical framework that are, as yet, underdeveloped. For one, a voting “habit” does not satisfy psychologists’ definition of habit as an action that becomes automatic from continual repetition and a stable

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  This article has earned badges for transparent research practices: Open Data and Open Materials. For details see the [Data Availability Statement](#).

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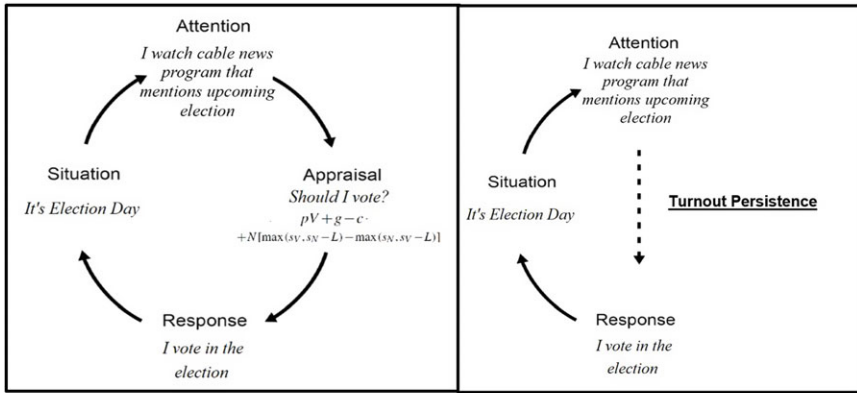
context (Danner, Aarts, and De Vries, 2008; Lally and Gardner, 2013; Moors and De Houwer, 2006; Wood, Tam, and Witt, 2005; Wood, Quinn, and Kashy, 2002) – the long gaps of time between elections and different election contexts preclude voting from becoming truly automatic (Dinas, 2012). More pressing is that there is insufficient existing research (for an overview, see Coppock and Green, 2016) to provide guidance why voting is habit-forming in some circumstances but not in others. To address these issues, I apply a new theoretical framework, the Process Model for Behavior Change (PMBC) (Duckworth and Gross, 2020), to voting in order to more cohesively bridge the economic cost-benefit model of voting with the psychology-motivated voting-as-a-habit literature.

This new theoretical frame gives greater clarity as to how a vote in one election might beget a vote in the future, while yielding testable predictions as to which circumstances are more favorable for developing turnout persistence. The PMBC first requires that an individual's attention be triggered by some environmental cue about Election Day. Then, the individual deliberates whether to vote, which can be represented by a cost-benefit equation (e.g., DellaVigna et al., 2016; Riker and Ordeshook, 1968). However, the PMBC framework also notes that this deliberative stage can be skipped via a heuristic shortcut. To increase the likelihood of triggering a heuristic shortcut in downstream elections, the attention cues should be similar to that of the last election that individual voted in. As such, turnout persistence across different election types (presidential elections vs. local elections) and saliencies (competitive vs. non-competitive) should be less likely. Finally, heuristic shortcuts are made when the individual obtains positive reinforcement for their past behavior. So, in an election context, an individual may feel that they made the “right” decision in turning out to vote if their preferred candidate won.

To test these predictions, I make use of a novel dataset consisting of nine door-to-door voter mobilization field experiments in various election contexts (with ~1.8 million voters in total). Consistent with prior empirical research, my analysis finds that being nudged to vote in one election does lead to increased turnout four years later. But the main contribution of this paper is that the PMBC predictions and the corresponding empirical results make sense of turnout persistence heterogeneities that have been detected in certain prior empirical studies but not others: some studies have found that being turned out to vote in a low-salience election upstream still led to downstream turnout persistence (Coppock and Green 2016; Garcia Bedolla and Michelson 2012) but other studies did not (Hill and Kousser 2016; Michelson 2003). In the same vein, Coppock and Green (2016), Hill and Kousser (2016), Michelson (2003), Green and Shachar (2000) found stronger turnout persistence in elections of the same type (e.g., a midterm election followed by a presidential election), while Garcia Bedolla and Michelson (2012) and Gerber, Green, and Shachar (2003) did not.¹

In the largest experimental analysis of turnout persistence to date, I find that being induced to vote in *low*-salience elections does not lead to voting in downstream *high*-salience elections, but being induced to vote in *high*-salience elections had large turnout persistence effects in downstream *high*-salience

¹For a detailed analysis of existing research through the PMBC lens, please see the Supplementary Materials (SM), Sections 1 and 2.



Adapted from Duckworth and Gross (2020, p. 40)

Figure 1. PMBC model as applied to voting with (right) and without turnout persistence (left).

elections. Similarly, I find that turnout persistence exists for elections of the same type but not for disparate elections. Finally, I find suggestive evidence that voting in an election where one’s (likely) preferred candidate won is associated with larger turnout persistence effects.

The theory

The Process Model of Behavior Change (PMBC) (Duckworth and Gross, 2020) allows us to make sense of persistent voting behaviors and why they occur when they do. The PMBC stipulates that any behavior occurs as part of a recursive cycle of four stages (Figure 1, left). First, we encounter a situation (e.g., it is Election Day). Some element(s) of the situation demand our attention (e.g., seeing news coverage of the upcoming election). We appraise the situation (i.e., apply our personal cost-benefit equation² to determine if voting is worth the effort) and pick the appropriate response (i.e., vote or not). But this model also emphasizes that because appraisal is cognitively taxing, there exist shortcuts past the Appraisal Stage (Figure 1, right). To be specific, this cognitive shortcut economizes “cognitive effort . . . because we have responded the same way in the same context and gotten a similar reward” (Duckworth and Gross 2020, 41). Past voting *causing* future voting would mean voters are sidestepping the Appraisal Stage and *not* weighing the costs and benefits of voting.

While repetition is necessary to make voting habitual, because the PMBC is a process-driven model, we can focus on the mechanism behind the *development* of shortcuts past the Appraisal Stage. We do not need to adjudicate whether persistent voting is a “habit” in the strict psychological sense.³ Rather, one can construe this shortcut as a failure to deliberate over whether an action is worth taking (e.g., “I voted in the last election and I was happy that I did, so I may as well vote again”).

²For more details, see SM, Section 1.

³For a more details, see SM, Section 2.

This is distinct from a deliberative thought process that has the individual thinking about how much time they'll wait in line at the polling place, the times someone praised them for voting, etc. The cognitive shortcut is a failure to even cursorily engage in the specifics of a cost-benefit assessment. And so, the PMBC model allows us to identify three elements that are necessary for voting to persist:

1. An individual's attention must be triggered by some environmental cue about Election Day (i.e., the Attention Stage).
2. Previous voting experience must validate that the voting calculus in the Appraisal Stage was correct. (i.e., the cost-benefit calculation should clearly show that the voter made the "right" choice in showing up at the polls.)
3. There must be positive reinforcement as voting behavior is repeated for the voter to begin to skip the Appraisal Stage and show up at the polls as a cognitive shortcut.

These necessary conditions yield three⁴ testable predictions:

Prediction 1: *Voters induced to vote in one election type (e.g., presidential) will exhibit stronger turnout persistence in future elections of the same type.* To bypass the Appraisal Stage, the contextual cues in the Attention Stage need to be similar to previous instances where the outcome of the Situation Stage was positive. While the difference in election types is most clear between local and federal elections, even presidential and midterm elections are different in salience,⁵ who votes,⁶ how campaigns spend money,⁷ and which voters campaigns target (Garcia Bedolla and Michelson, 2012). Although early experimental voting-as-a-habit literature did not anticipate differences across election types (e.g., Green and Gerber, 2002; Gerber, Green, and Shachar 2003), empirical differences in turnout persistence across election contexts were detected but limited by sample size (e.g., Garcia Bedolla and Michelson 2012); larger-scale analyses presented empirical evidence of stronger turnout persistence across elections of the same type but did not present a theory to explain the peculiarity (Coppock and Green 2016). As such, though election type incorporates many interrelated characteristics, it is prominent in the turnout persistence literature and presents a replicable benchmark for PMBC predictions.

Prediction 2: *Voters induced to vote in a low-salience election will not exhibit stronger turnout persistence in higher-salience elections.*⁸ This prediction is closely related to Prediction 1, but isolates salience from other characteristics inherent in election types. Election salience has been used extensively in

⁴There is a fourth highly exploratory prediction on how household income can affect turnout persistence included in SM, Section 5.3.

⁵Presidential elections have an information environment that is more crowded than that of midterms – driven at least partly by substantial differences in campaign spending (e.g., Jackson 2000, 2002).

⁶For instance, younger voters are much less likely to vote in the midterms than in presidential elections (Leighley and Nagler, 2018).

⁷For instance, both 2012 presidential campaigns allocated substantially more money to campaign ads than the average Senate campaign in 2014 (Koerth, 2018).

⁸I did not have sufficient data to test the converse (cf., Gerber, Green, and Shachar (2003), Cutts, Fieldhouse, and John (2009) and a subset of Garcia Bedolla and Michelson's (2012) experiments).

developing a theory of voter behavior (e.g., Arceneaux and Nickerson, 2009; Malhotra et al. 2011), as it proxies how much election information and advertising a voter is exposed to (e.g., Hernandez, Anduiza, Rico, 2021). Prior research on turnout persistence in this context has produced inconsistent results (e.g., Bedolla and Michelson’s (2012) downstream Complier Average Causal Effects ranged from -0.82 to 1.59). The absence of turnout persistence after low-salience upstream elections has generally been ascribed to methodological limitations, such as insufficient sample size or upstream effects that were too small (see Coppock and Green’s (2016) review).

The PMBC can help us make sense of these inconsistencies. The contextual cues in high-salience and low-salience information environments are likely different: talking to a local candidate who is canvassing the neighborhood for the first time is a novel cue, whereas a co-worker talking about the upcoming presidential election may be a recurring cue.⁹ Novel cues are more likely to trigger a re-appraisal of the cost-benefit equation and thus make heuristic decision-making less likely.

Prediction 3: *Voting in an election where one’s chosen candidate won should be positively associated with turnout persistence.* The PMBC model stipulates that positive reinforcement is key to strengthening the heuristic shortcut past the Appraisal Stage. As such, casting a vote in an election where your preferred candidate wins should provide a larger “warm glow” than individuals who voted and their candidate lost.¹⁰ Since a larger warm glow term makes it more likely that the outcome of the cost-benefit analysis in the Appraisal Stage is positive, the individual would get some positive reinforcement, which should make subsequent voting in an election with similar context cues more likely.¹¹ The impact of election outcome on turnout persistence has generally not been addressed in prior research,¹² but this prediction is broadly consistent with the “winner effect” literature, which finds that voters’ sentiments about the democratic process improve when the participant had voted for the winning candidate (e.g., Sinclair, Smith, and Tucker, 2018).

Evaluating the predictions empirically

To assess these predictions, I build on Garcia Bedolla and Michelson’s pioneering (2012) research on the impacts of real-world GOTV programs. I partnered with a US labor organization that runs door-to-door outreach programs with millions of

⁹Campaigns recognize these differences: “[i]n high-salience elections, campaigns target unlikely voters out of the belief that everyone else is going to vote without their encouragement, whereas in low-salience elections they assume the opposite and focus on those voters who have reliably voted in the past” (Arceneaux and Nickerson, 2009; p. 5).

¹⁰Some voters may experience a warm glow from the act of voting alone (e.g., third-party voters), but we are unable to observe the utility a voter assigns to their experience at the polls. Instead, we must rely on observable differences between election contexts and a set of assumptions about how those differing election contexts might reasonably affect voter utility. For a more extensive discussion of the “warm glow” variable in the calculus of voting, see SM, Section 1.

¹¹There are alternative theoretical frameworks that speak to these same predictions (see the Discussion and Conclusions section).

¹²The one exception is Fujiwara, Meng, and Vogl (2016), but they address this prediction indirectly (via perception of pivotality rather than actual election outcome).

working-class Americans. The organization's sampling criteria were based on their election goals and logistical availability.^{13,14} The organization randomized a percentage of their program sample to an uncontacted control group in nine separate field experiments. The experiments were conducted in 8 different states and included five different election contexts, with a total of over 1.8 million households. For a summary of the experiments, see Table 1.¹⁵ All subjects were matched to subsequent elections' voter files.

These data were initially analyzed as part of a consulting project but the subgroups associated with the predictions in this paper were never assessed. Because I am not able to provide objective of evidence that this is the case, I decided that submitting a pre-analysis plan may appear misleading. For a comprehensive discussion of my decision to not preregister, please see SM, Section 4. In lieu of a pre-analysis plan, I attempt to maximize data and analytical transparency and emphasize the need to replicate these findings.

The initial upstream effect appears small at just 0.3 percentage points ($p < 0.001$), but it is nearly identical to effects found in similarly high-salience election contexts (Gerber and Green, 2019; see SM, Section 5.1 for more details). To analyze turnout persistence, I use the analysis strategy in Coppock and Green (2016), where V_1 is defined as voting in the upstream election and V_2 is voting in a downstream election, Z is an indicator denoting whether or not the individual was assigned to receive a doorknock GOTV treatment at time 1. The main estimand of interest is a Complier Average Causal Effect (CACE) – the effect of voting in an upstream election on downstream voting among those who vote because they receive the GOTV doorknock. The estimator is thus $\widehat{CACE} = \frac{\hat{E}[V_{2i}|Z_i=1] - \hat{E}[V_{2i}|Z_i=0]}{\hat{E}[V_{1i}|Z_i=1] - \hat{E}[V_{1i}|Z_i=0]}$ for every household i in the experiment. This is estimated via two-stage least squares as is standard practice (Angrist, Imbens, and Rubin, 1996; Landau and Emsley, 2020).

Since the initial canvasses were conducted anywhere from 2014 to 2017, the only downstream election for which I can use all of the data is the 2018 general election.^{16,17} When I regress data from all experiments (with experiment fixed effects and robust standard errors) in a 2SLS regression with voting in the 2018 general election as the instrumented variable, I estimate a CACE of 0.57 ($p = 0.004$).^{18,19} In other words, of those voters successfully turned out upstream, over 57% turned out again in the 2018 general election. However, the key contribution of this study is that the variety of election contexts and the large

¹³The geography had to be dense enough to warrant the cost of door-to-door canvassing.

¹⁴Since PMBC is designed to be broadly portable across different types of people and contexts, PMBC predictions need not be tested in a nationally representative sample.

¹⁵For more details on the data used, please see SM, Section 3.

¹⁶2018 primary turnout was not available as part of this project.

¹⁷It is possible to analyze downstream CACEs for 2016 primary and general elections for a smaller subset of my data, as seen below.

¹⁸With and without covariates.

¹⁹Since the initial upstream effect is small, it is important to check for weak instruments (e.g., Andrews, Stock, and Sun 2019). For a full analysis of weak instruments, see SM, Section 5.2. In short, one experiment, Washington 2014, had an upstream treatment effect indistinguishable from zero ($p = 0.95$) and is omitted from all subsequent analyses, as it would bias turnout persistence upward. Note that including the Washington data does not substantially change the reported results.

Table 1. Overview of all experiments

Election		Control	Treatment	Total	What's on the ballot?	Population Turnout Rate	Sample Turnout Rate
2014 Midterm Election (Iowa)	<i>N</i>	29,054	28,937	57,991	US Senate, US Congress, State-level offices, 3 ballot initiatives	53%	72%
	%	50.1	49.9	100			
2014 Midterm Election (Illinois)	<i>N</i>	267,630	268,096	535,726	US Senate, US Congress, State-level offices, 2 ballot initiatives	49%	76%
	%	49.96	50.04	100			
2014 Midterm Election (Michigan)	<i>N</i>	224,905	224,828	449,733	US Senate, US Congress, State-level offices, 3 ballot initiatives	42%	74%
	%	50.01	49.99	100			
2014 Midterm Election (New York)	<i>N</i>	75,679	176,579	252,258	US Congress, State-level Offices, 3 ballot initiatives	33%	49%
	%	30	70	100			
2014 Midterm Election (Washington)	<i>N</i>	24,004	95,765	119,769	US Congress, 4 ballot initiatives	54%	63%
	%	20.04	79.96	100			
2015 Pennsylvania Supreme Court General Election	<i>N</i>	16,494	32,784	49,278	3 Supreme Court Seats	27%	60%
	%	33.47	66.53	100			
2015 Philadelphia Mayoral Democratic Primary Election	<i>N</i>	52,291	153,690	205,981	Democratic Mayoral Primary	26%	57%
	%	25.39	74.61	100			
2016 Presidential Election (North Carolina)	<i>N</i>	14,544	122,488	137,032	Presidential Election battleground, US Senate, US Congress, State-level offices	69%	68%
	%	10.61	89.39	100			
2017 Gubernatorial Election (Virginia)	<i>N</i>	7,114	60,857	67,971	State-level offices	48%	75%
	%	10.47	89.53	100			

sample size allow for the testing the circumstances in which turnout persistence is more (or less) likely to occur. Table 2 summarizes my predictions, my empirical findings, and the empirical findings of previous experimental studies that looked at similar populations of voters.

Prediction 1: *Voters induced to vote in one election type will exhibit stronger turnout persistence in future elections of the same type.* In my data, there are four cases where the upstream and downstream election is of the same type (federal midterm).²⁰ In Table 3, I present the initial upstream turnout effect in the first column; in the next two columns, I show downstream CACEs of disparate election types (midterm-on-presidential-primary and midterm-on-presidential), and in the final column, I present the CACE of like election types (midterm-on-midterm). Downstream voting persistence among compliers was small-to-null in the presidential elections,²¹ but was very strong in the following midterm election (96% of voters who were successfully turned out by the canvass voted four years later ($p = 0.006$)). My data, therefore, comport with the PMBC model, Coppock and Green (2016), and the pioneering observational work by Aldrich, Montgomery, and Wood (2011).

Prediction 2: *Voters induced to vote in a low-salience election will not exhibit stronger turnout persistence in higher-salience elections.* A common proxy for election salience is actual turnout on Election Day (e.g., Coppock and Green, 2016; Rolfe, 2012). The 2015 PA Supreme Court election and the 2015 Philadelphia Mayoral Primary clearly qualify with turnout well under 30%.²² As seen in Table 4, the canvass in Pennsylvania and Philadelphia had an upstream treatment effect size that was about three times as large as that of the rest of the experiments (0.85 percentage points versus 0.26 percentage points); nevertheless, we find that there were no lasting downstream effects among compliers (the first row cell in light gray). In contrast, nearly 90% of participants induced to vote by the initial GOTV contact in the remaining states turned out downstream in the 2018 midterm election ($p = 0.004$). These results are highly robust to other definitions of salience (e.g., no president/governor/senate race on ballot); see SM, Section 5.4 for more details.

Prediction 3: *Voting in an election where one's chosen candidate won should be positively associated with turnout persistence formation.* While we are not able to determine how positive an experience a voter had at the polls, we do have one observable variable: who won the election. Although it is not possible to know who a given participant voted for, the targeting criteria of our partner organization are overwhelmingly Democratic. So, for this analysis, I remove registered Republicans

²⁰I omit the 2016 North Carolina presidential experiment from this comparison as I do not have data for the 2020 presidential election. That said, the downstream effect in North Carolina on the following midterm election was not significant ($p = 0.277$), but the magnitude was high (0.70). It is likely this analysis is underpowered and so the results do not provide clear evidence against or in support of Prediction 1.

²¹Note that there is a somewhat higher (but non-significant) CACE in the presidential primary, which may be driven by lower baseline turnout as predicted in Coppock and Green (2016).

²²Local media coverage of these election has called turnout "low" (Holmberg, 2015) and "bad" (Kerkestra 2015).

Table 2. Summary of predictions and results across multiple studies

Study	Ternovski (2023) <i>N</i> = ~1.8M 9 experiments	Coppock and Green (2016) <i>N</i> = ~1.2M 3 experiments	Fujiwara, Meng, and Vogl (2016) <i>N</i> = ~50k 1 natural experiment	Hill and Kousser (2016) <i>N</i> = ~150k 1 experiment	Garcia Bedolla and Michelson (2012) <i>N</i> = ~133k 14 experiments	Michelson (2003) <i>N</i> = ~3k 1 experiment	Gerber, Green, and Shachar (2003) <i>N</i> = ~25k 1 experiment
P1: Voters induced to vote in one election type will exhibit stronger turnout persistence in future elections of the same type.	Yes	Yes	Yes (mixed)*	Yes**	No**	Yes**	No**
P2: Voters induced to vote in a low-salience election will <u>not</u> exhibit stronger turnout persistence in higher salience elections.	Yes	No	N/A	Yes	No	Yes	N/A
P3: Voting in an election where one’s chosen candidate won should be positively associated with turnout persistence.	Yes	N/A	No	N/A	N/A	N/A	N/A

* = One set of studies did yield results that were consistent with my prediction, but a second set of studies did not. The authors believe the second set failed to replicate due to inadequate statistical power. ** = These studies only had data on the effect of voting in one election type upstream on voting in a different type of election downstream. Thus, we are unable to determine if the effects they find are higher for elections of the same type (i.e., we have no counterfactual within study).

Table 3. Individuals who were successfully turned out to vote in 2014 midterm elections continued to vote in 2018 midterm but not in 2016 presidential elections

	Upstream 2014 General Election (1st Stage)	2016 Presidential Primary Election	2016 Presidential General Election	2018 Midterm General Election
Midterms Only (IA, IL, MI, NY)	0.0024*** (0.0007)	0.4246 (0.3176)	0.2212 (0.2255)	0.959** (0.3506)

First column denotes the upstream ITT effect estimated via OLS regression. Subsequent columns estimate CACE for different downstream elections. Robust standard errors in parentheses. *** = p -value ≤ 0.001 , ** = $p \leq 0.01$.

Table 4. Salience and downstream CACEs

			2018 Midterm General Election
Lowest Projected Turnout + No Federal Race on Ballot	Low Salience	(PA, Philly)	0.0085*** (0.0021)
	High Salience	(All others)	0.0026*** (0.0007)
			-0.0736 (0.2241)
			0.8829** (0.3068)

This table examines the impact of voting in the elections described in each row on downstream turnout in the 2018 midterm election. Robust standard errors in parentheses. *** = p -value ≤ 0.001 , ** = $p \leq 0.01$.

and assume individuals with missing²³ registration data prefer the Democratic candidate to win. If this assumption is violated, we will be *less* likely to detect an effect in elections where the Democratic candidate won, which makes for a conservative test of Prediction 3.

I find that participating in an upstream election where the Democratic candidate won was associated with persistent downstream effects among compliers four years later and no turnout persistence in states where the Republican won (see Table 5). One concern is that the election outcome may not be truly exogenous, but Eggers et al. (2015) found compelling evidence that the outcomes of close elections are as-if random.²⁴ Three of the four states in this analysis were top-of-the-ticket tossups (IL 2014, MI 2014, NC 2016) and Iowa 2014 had a competitive down-ballot race.²⁵ It is also possible that this result is driven by a difference in voter identification laws (i.e., more stringent voter ID requirements may make voters' experience at the polls less positive). However, the voting identification laws are similar across the two groups of states.²⁶ Still, these results are only based on a comparison of two sets of

²³Party registration is missing for all voters in IL and MI, so limiting the sample to only registered Democrats is not feasible.

²⁴As a result, outcomes of close races have been used to estimate causal impacts of incumbency (e.g., Ariga et al. 2016; Kendall and Rekas 2012; Lee 2008).

²⁵The results hold even if I omit Iowa 2014 (NC 2016 CACE = -0.02 , $p = 0.989$).

²⁶In Iowa and Michigan, an ID is requested and, if it is not presented, voters must sign an oath or affidavit verifying their identity and are allowed to cast a regular ballot; North Carolina and Illinois both do not require any form of ID to vote (NCSL 2023).

Table 5. Successfully being turned out to vote in the elections where the voters’ (likely) preferred candidate won makes those voters more likely to vote in 2018 midterm election²⁷

	Upstream Election (1st Stage)	2018 Midterm General Election
Dem Win (IL 2014, MI 2014)	0.0019* (0.0008)	1.3969* (0.6173)
Dem Loss (IA 2014, NC 2016)	0.0087*** (0.0026)	0.2013 (0.2856)

First column denotes the upstream ITT effect estimated via OLS regression. Subsequent columns estimate CACE for different downstream elections. Robust standard errors in parentheses. ** = $p \leq 0.001$, * = $p \leq 0.01$, * = $p \leq 0.05$.

experiments and there may be other unobserved confounders.²⁸ For instance, one plausible confounder is the baseline turnout propensities across these two groups are markedly different (see SM, Table 7.5). Given these data limitations, the analysis of Prediction 3 is the most exploratory and should be replicated before generalizing from this result.

Discussion and conclusions

My results indicate that the PMBC theoretical framework may provide additional insight into why turnout persistence forms in some contexts but not others. Its predictions validate in a novel large-scale dataset, but since the predictions were not preregistered before the data were collected, it is imperative to replicate these results in future studies and assess some of the alternative explanations that might yield the same results.

One possible alternative explanation for turnout persistence, particularly for Prediction 1 (persistence across similar election types) is raised by what Rogers and Frey (2014) termed “rip currents,” where compliance to an upstream GOTV intervention leads to subsequent attention from campaigns and non-profits. It’s not that there is a permanent change to an individual’s cost-benefit equation or the act of voting creates a self-reinforcing intrinsic impetus to vote; rather, the rip currents hypothesis claims that the people who are successfully nudged to the polls by a campaign are subsequently targeted by more campaigns and non-profits with nudges that are similarly successful in inducing these individuals to vote in downstream elections.

The few existing studies on this subject suggest that this is unlikely to be the main driver behind turnout persistence. Turnout persistence isn’t more pronounced in battleground states (Coppock and Green, 2016) and since battleground states attract more campaign activity, those voters who are successfully mobilized in an upstream

²⁷Since the election context is different in the 2017 Virginia experiment, I exclude it from this comparison. However, adding the VA data results in a highly similar CACE of 1.31 ($p = 0.02$).

²⁸There is evidence that the Dem Win subgroup suffers from a weak instrument (see SM, Section 5.2). As is recommended in e.g., Staiger and Stock (1997), I use Anderson-Rubin weak-instrument-robust tests. These tests find evidence of turnout persistence in the Dem Win subsample ($p = 0.001$) and no turnout persistence in the Dem Loss subsample (p -value = 0.503). This is consistent with the reported CACEs.

election should receive more campaign attention under the rip current hypothesis.²⁹ Rogers et al. (2017) explicitly analyzed whether campaigns, in downstream elections, were more likely to contact participants who were in an upstream treatment group than participants in an upstream control group. The study merged all subjects of a 2011 GOTV experiment to two databases of 2016 campaign contact information.³⁰ While Rogers et al. (2017) did find modest increases in downstream contact of the 2011 treatment group, the differences were so small that they concluded that turnout persistence “cannot plausibly be attributed to the treatment and control groups’ differential exposure to mobilization activity.” (p. 92).³¹

That said, repeated campaign contact may be key to building voting persistence in the PMBC model: the question is whether the subsequent campaign contact affects the Appraisal Stage (the cost-benefit equation of voting) or simply calls attention to an upcoming election. Future research should attempt to disentangle these effects; one possible design would randomize continual informational treatments (e.g., identical text messages reminding an individual of an upcoming Election Day every election) versus a one-off, heavy-touch GOTV intervention. PMBC would predict that while the information treatment should have a lower upstream effect, compliers in the informational treatment should develop stronger turnout persistence years later (as compared to compliers in the heavy-touch condition).

An alternative explanation for Prediction 2 (weak turnout persistence after mobilization in low-salience elections) is that voters in low-salience elections were already high-propensity voters and they “complied” with the treatment because they were reminded of a lesser known election (e.g., Dale and Strauss, 2009). Since these are already high-propensity voters, they may be more likely to encounter ceiling effects downstream.³² However, downstream 2018 midterm turnout in the control groups of the 2015 PA and the 2015 Philadelphia studies (low-salience elections) was 72.3% versus 73.8% among the control groups in the high-salience elections. This indicates that the high-salience and low-salience groups had comparable baseline turnout propensities downstream. In other words, we see that voters had high turnout treatment effects upstream in low-salience elections (perhaps because they were reminded of that election), but those compliers did not continue to vote in subsequent elections. On the other hand, compliers *with comparable baseline turnout propensities downstream* who were successfully turned out in more high-salience elections upstream, continued to vote in the 2018 midterm at very high

²⁹Coppock and Green (2016) also provide an overview of other studies that looked at subsequent campaign contact across upstream treatment conditions; Dinas (2012) and Green, McGrath, and Aronow (2013) found very modest effects of treatment on subsequent campaign contact.

³⁰The first database was maintained by the Obama for America campaign (which ran Barak Obama’s campaign in 2008 and 2012) and the second, by Catalist, a Democratic-leaning data clearinghouse that tracked voter contact data of several large Democratic-leaning non-profits (Rogers et al., 2017).

³¹The only sizable increase came in the form of direct mail (8.1 percentage points more in the treatment group), which is unlikely to explain the entirety of turnout persistence. A meta-analysis of direct mail outreach finds a 0.5 percentage point impact; even the most effective intervention (social pressure) has, on average, only a 2 percentage point impact on turnout (Green and Gerber 2019; p. 214).

³²The issue of ceiling effects could also affect the analysis in Prediction 1. I address this possibility in SM, Section 6.

rates. It is, however, possible that the same treatment script works differently in low-salience versus high-salience elections. It may be that in low-salience elections, the noticeable reminder (Dale and Strauss, 2009) resonates with voters (without developing habit), but in high-salience elections, it is plan-making (which may help develop habit).

PMBC also overlaps with existing theoretical frameworks – particularly in regard to Prediction 3 (being more likely to vote in a subsequent election after one’s preferred candidate won). For one, the winning candidate may be able to provide more resources to that voter. Even if there is a lack of “rip currents” (i.e., subsequent campaign contact), the winning campaign may increase that voter’s resources by implementing policies favorable to the voter³³ or otherwise lowering the cost of voting for supporters but not opponents. Another explanation may come from the robust “winner effect” literature, where voters who voted for the winning candidate feel more strongly about electoral and democratic legitimacy, perception of fairness, political efficacy, and other variables more closely related to the Appraisal Stage than habit (e.g., Sinclair, Smith, and Tucker, 2018). A better test of Prediction 3 might involve nonpartisan, low-salience ballot initiatives – are supporters of such winning ballot initiatives more likely to exhibit turnout persistence?

Another related alternative explanation is that voting starts a self-reinforcing process (e.g., Rogers and Frey 2014). This explanation is related to the informational reductions in transaction costs (e.g., learning the best time to go to one’s local polling place), inflating the warm glow of voting (as in Sinclair, Smith, and Tucker, 2018), or the social benefits of voting. Under this explanation, the economic model is sufficient to explain turnout persistence without the need for PMBC. However, it is difficult to reconcile this mechanism with the finding that turnout persistence fades faster for some upstream interventions (e.g., social pressure (Davenport et al., 2010)) but not others (Coppock and Green, 2016). If turnout persistence is always driven by a self-reinforcing adjustment of costs, we would expect that the specifics of the initial motivation for voting shouldn’t affect the longevity of effects.³⁴

There may still be other explanations that do not comport with the PMBC model and in no way should my empirical assessment be viewed as conclusive evidence that turnout persistence is explained solely by the PMBC theoretical framework. The goal of this paper is to present a new theoretical framework with empirical evidence illustrating this framework’s value. Future studies should directly test the predictions that come out of my application of PMBC to turnout persistence. Specifically, I predict that to increase the chances of developing turnout persistence, the following conditions should hold. First, the contextual cues in one election should be as similar as possible in future elections. This implies that organizations tasked with increasing turnout may want to attempt using the same prompt to remind voters that it’s Election Day from one election to the next. It’s also important that voters successfully persuaded to turn out to the polls have a net positive experience voting. This makes it more likely that a voter will begin to skip the

³³As recent studies like Costa (2021) show, despite recent increases in partisan polarization, voters still vote according to policy preferences.

³⁴For a more comprehensive discussion of how different intervention types – and particularly social pressure – might affect turnout persistence, please see SM, Sections 2 and 3.

Appraisal Stage and adopt a cognitive shortcut in response to a contextual cue. The implication is the voters who did not have a positive experience voting (e.g., long lines, their preferred candidate lost) may benefit from receiving a different contextual cue next election to stop the development of a non-voting habit.

Supplementary material. To view supplementary material for this article, please visit <https://doi.org/10.1017/XPS.2023.25>

Data availability statement. The data, code, and any additional materials required to replicate all analyses in this article are available at the Journal of Experimental Political Science Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/G07AO1>

Competing interests. Data were shared after the contractual relationship between the author and the partner organization was concluded. As such, there were no conflicts of interest.

Ethics statement. A de-identified dataset was shared by a partner organization with the author. As such, since there is no Personally Identifiable Information (PII), this was not human subject research and did not require IRB review.

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