Voting Then, Voting Now: The Long-Term Consequences of Participation in South Africa's First Democratic Election

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In a developing democracy, can elections change the behavior of participating citizens? I study the effect of voting in South Africa's first democratic election in 1994 on future voting and present evidence of the lasting behavioral effects of past participation. Eligibility to participate in 1994 affects future voting by 3 percentage points, with an average treatment effect of actually voting between 3.5 and 8.5 percentage points. Given low turnout rates, these effects account for 7%–20% of the size of the electorate. To explain this result, I propose a theoretical explanation that draws on psychological research—*affective experience habituation*. I argue that persistence (or habituation) in voting behavior is at least partly driven by the creation of associations between first-time voting and positive emotional states. This theory is tested within the context of the 1994 election by exploiting variation in electoral experiences.

Voting is the quintessential democratic behavior. Yet voting is a novel act for the citizens of many new democracies, particularly those who were disenfranchised under colonialism, dictatorship, or one-party rule. A major challenge in new democracies lies in turning these newly enfranchised citizens into voters. One possibility is that, through repeated exposure to elections and democratic processes, citizens may become more likely to vote in the future, more politically engaged and informed, or more supportive of democracy as a system of government (Lindberg 2006a, 2006b). While research in American politics provides some answers to these questions (Green and Shachar 2000; Meredith 2009), research in development is essentially silent on the issue. I demonstrate that, in a new democracy, exposure to, and participation in, an election can increase the likelihood of voting in the future. Voters who participate once are more likely to participate in the future by between 3.5 and 8.5 percentage points. Given low rates of turnout in new democracies, these effects may account for 7%–20% of the size of the electorate.

Studying the effects of electoral participation is a challenging empirical problem.[1] To overcome this, I leverage a natural experiment created by the timing of elections. By comparing the voting behavior of South Africans who were just old enough to vote in South Africa's first democratic election in 1994 with South Africans who were just too young, I isolate the long-term behavioral effects of participation.[2] The elections of April 26 to April 29, 1994, ended hundreds of years of exclusive white rule in South Africa. They introduced 18 million new voters to the electorate, mostly previously disenfranchised black Africans, and roughly 20 million of the 23 million of those eligible voted (Johnson and Schlemmer 1996). Since 1994, electoral turnout steadily declined, plateauing at around 55% of the voting-age population in general elections and much lower in local elections. In 2016, South Africa remains a single-party dominant regime in which the African National Congress (ANC) regularly wins over 60% of the popular vote, controls eight of nine provinces, runs over two-thirds of all municipalities, and is routinely embroiled in corruption scandals that suggest serious abuses of power.

1. The naive relationship between participation from one year to the next is no doubt confounded by numerous unobservable factors.
2. The research design is similar to those used in Cameron et al. (2013), Dinas (2012, 2014), Meredith (2009), and Mullainathan and Washington (2009).

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To help explain the downstream effects of participation in this setting, I introduce *affective experience habituation*, which posits that voting persistence—repeat voting—is driven by the creation of cognitive associations between the act of voting and positive emotional states during an election. The theory predicts that those who vote while in a positive state of mind are more likely to vote in the future but those who vote while negatively disposed may opt out in the future. The unique context of South Africa's 1994 election allows me to present suggestive tests of these predictions, and I find evidence consistent with the mechanism.

The results mirror similar findings in American politics (Meredith 2009), and they fit well with Lindberg's (2006a) suggestion that ensuring elections are perceived as open and fair may be an important tool in the development of a healthy democracy. But the particular mechanism proposed also sounds a warning call: elections that are perceived by voters as unfair, biased, or unsafe may actually serve to reduce long-term participation. This may threaten democracy: if those who are opposed to an incumbent have systematically negative experiences at the polls, while supporters of the incumbent have positive experiences, opposition supporters may opt out of the electorate at differentially high rates. Such behavior may result in a "sorted" electorate, further solidifying the incumbents' power.

Two features of South Africa's 1994 election help extend prior research. First, the election was a major administrative undertaking—the voting population increased nearly 10-fold overnight. Consequently, no registration process was introduced, and citizens and residents were able to vote simply by showing official identification. None of the individuals who participated in 1994 were entered into a voters' roll, and all were required to register for the first time ahead of the next national election in 1999 (Johnson and Schlemmer 1996). This provides a unique opportunity to assess the effect of participation for the population as a whole, while ruling out registration effects.[3] Importantly, while the 1994 election was sui generis, the results replicate elsewhere, both in later elections in South Africa and in other African democracies that underwent transitions—specifically Ghana, Tanzania, and Kenya.

Second, experiences of the 1994 election varied by race. While the end of Apartheid was a moment of liberation for black South Africans after hundreds of years of subjugation, for many white South Africans it represented a step into a frightening unknown (Johnson and Schlemmer 1996; Lodge 1999). The typical emotional states of black and white South Africans who cast their ballots in 1994 were thus markedly different. These experiences can further be contrasted against those of colored South Africans, a group of roughly 5 million people of mixed ethnic origin who were "caught in the middle" during the transition (Ferree 2010; Harris 2016).[4] These differences are well documented qualitatively, and analyses of survey data collected in 1994 corroborate these accounts. Leveraging this variation to explore heterogeneity in the effect of participation provides a good suggestive test of whether affective experience habituation is empirically supported.

As such, I make two major contributions. I present evidence that past participation increases future participation in a major developing democracy. While other studies have found similar effects in the United States or in Britain (Denny and Doyle 2009; Dinas 2012; Green and Shachar 2000; Meredith 2009), no study has investigated such effects in a hegemonic developing democracy, a common regime type. The research design used, while limited in some ways, also provides the first estimates both net of registration effects and not conditional on registration status.

I also introduce and provide exploratory tests of a new psychological mechanism that can partially explain voting persistence. The mechanism is general—it explains the findings presented here and may help to explain similar findings in developed-world contexts. Importantly for the study of developing democracies like South Africa, Ghana, Kenya, Tanzania, and many others, the mechanism suggests key conditions under which voters will continue to participate, as well as conditions under which they may opt out of the electorate.

ELECTIONS IN DEVELOPED AND DEVELOPING DEMOCRACIES

Research in comparative politics suggests that elections in developing democracies rarely have pro-democratic consequences (Carothers 2002; Schedler 2002) and instead are a means by which dominant parties sustain their rule (Gandhi and Przeworski 2009; Schedler 2006). They serve as pressure valves, may enable the regime to monitor citizens' behavior, or can broaden the nexus of elites who depend on the status quo (Gandhi and Przeworski 2007; Geddes 2005; Magaloni 2006, 2008). Similarly, elections may legitimate the regime's existence while sustaining its authority (Hermet, Rose, and Rouquié 1978; Levitsky and Way 2002; Schedler 2010).

Lindberg (2006a, 2006b) observes that elections in developing democracies are not purely the preserve of elites and they may have consequences for citizens at a behavioral

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3. Registration may increase the likelihood of repeat voting, because it reduces the cost of future voting. Any study that wishes to establish the pure effect of participating once on participating again must find ways around this challenge.

4. I thank an anonymous reviewer for highlighting this last point.

level. Elections engage millions of voters in the political process, no matter its flaws. If Lindberg is correct, citizens' beliefs, preferences, and behaviors change through political engagement. Elections may thus have important macropolitical consequences that stem from microbehavioral effects. If—as my results suggest—voting behavior is persistent over time, this provides important insights into how and why the electorate of a developing democracy may grow, shrink, or change. If—as my theory suggests—voter persistence only occurs when electoral experiences are positive, this may have implications for the macropartisanship of the electorate over time.

Despite these insights, no research explores the persistence of electoral participation in the context of a developing democracy, the modal form of electoral democracy worldwide. Prior studies focus on the American and British contexts, which, while highly informative, limit the scope of their findings (Coppock and Green 2015; Cutts, Fieldhouse, and John 2009; Denny and Doyle 2009; Dinas 2012; Gerber, Green, and Shachar 2003; Green and Shachar 2000; Malhotra et al. 2011; Plutzer 2002). Further, little attention is paid to the theoretical properties of repeated participation, or, as these studies often term the phenomenon, "voter habituation" (Aldrich, Montgomery, and Wood 2011).

Within this body of work, one study is closely related to the present study, Meredith's (2009) rigorous exploration of the effects of participation in the United States. Meredith (2009) uses voting-age cutoffs to estimate the effect of first-time voting on future participation. Meredith's (2009) research design has distinct advantages over the design used here, most notably that the exact date of birth of each person in the study is observed. I am still able to offer some other advances. First, this paper generalizes Meredith's (2009) findings to a broader context than just the United States. Second, the nature of the 1994 election in South Africa allows me to isolate the "pure" effect of participation, ruling out mechanical registration effects. While Meredith (2009) provides some evidence that registration effects do not drive his results, he cannot provide much insight into the true magnitude of any "pure" effect net of registration effects. Third, the unique context of the 1994 election allows for a deeper investigation of the psychological mechanisms underpinning habitual voting. In all, our studies are highly complementary in both design and findings.

While all of the studies mentioned above contribute to our understanding of the effects of participation on repeated participation, questions remain unanswered. Empirically, with the exception of Meredith (2009), these studies rely on one of two research designs—they either piggy-back on a field experiment or they leverage panel data and rely on individual fixed effects. Both strategies have potential weaknesses. Piggy-backing off field experiments generates "compound treatments"—it is unclear whether long-term participation is the result of voting, down-stream treatment effects of the experiment itself, or various potential violations of key design assumptions. Estimating treatment effects via fixed effects panel regression is difficult for other reasons, notably that they do not account for time-varying unobserved confounders, such as income, age, marital status, or levels of social capital.

Also unanswered are questions of theory. The participation effect is usually referred to simply as "habituation," but insufficient attention is paid to the assumptions that underpin this interpretation. It is hard to reconcile voting behavior with psychological theories of habituation, which describe a subconscious association between an environment and a responsive action that is developed over repeated encounters with the environment or stimuli (e.g., opening the refrigerator when you enter the kitchen; Aldrich et al. 2011). How a similar effect works with respect to voting is unclear. Further, little is known about the conditions under which such habituation may or may not occur. This paper engages both questions.

Affective experience habituation

What precisely does voter habituation mean, why does it occur, and when may it be absent? While Gerber et al. (2003) and Meredith (2009) speculate about potential channels, they cannot offer much in the way of evidence. I suggest a psychological mechanism that may partially explain the effect. Research in psychology demonstrates that when individuals engage in new tasks, they create implicit subconscious associations between a task and their emotional state (Aarts and Dijksterhuis 2000; Baumeister et al. 2007; Bodenhausen 1993). Starting a new task in a bad mood, for instance, may lower the probability of returning to that task in the future. Conversely, starting a task in a good mood is likely to encourage future engagement. For the remainder of the paper, I term this theory *affective experience habituation*.

Custers and Aarts (2005), through a series of experiments, extend this argument with a model of subconscious affect. They argue that humans tend to be goal-oriented, which manifests itself in conscious and subconscious desires to achieve certain behavioral, mental, or emotional states. Once tasks become associated with certain emotional states, approaching or avoiding these activities becomes, in part, a conscious and subconscious responsive behavior. In the present case, if voting becomes associated with a desirable emotional state, then opportunities to vote are seen as opportunities to enter a desirable emotional state. This raises the probability

that any individual with a positive affect attached to voting would turn up to vote. The converse of this, however, is that when voting becomes associated with negative or undesirable emotional states (like fear, anxiety, or threat), repeated voting becomes less likely (Aarts, Custers, and Holland 2007).

If this is the case, opportunities to vote are seen as an opportunity to enter a negative emotional state, and so avoidance may occur as a result. Prior research suggests that affective (or emotional) factors may influence "political cognition" when and how to interact with the political world. Research has focused on emotions like anxiety and anger, proposing that they may help to explain various political behaviors—in particular judgments about candidates, parties, policies, including habitual outcomes (Marcus, MacKuen, and Neuman 2011; Marcus, Neuman, and MacKuen 2000, Marcus et al. 2005, Valentino et al. 2011). While some of these arguments do consider the creation of habits via affective associations, they focus more on the role of emotions in dynamically evaluating habits and provoking deviations from habitual behaviors toward higher cognitive systems. Recent empirical research in political psychology suggests that emotions can indeed cause affective associations in the early stages of information processing, inducing downstream effects on political preferences (Erisen, Lodge, and Taber 2014).

One testable implication of affective experience habituation is that voter persistence should only occur when elections generate positive associations for voters. In other studies, researchers typically focus on countries in which voting is generally a uniformly positive experience (Cutts et al. 2009; Denny and Doyle 2009; Dinas 2012; Gerber 2003; Green and Shachar 2000; Meredith 2009; Plutzer 2002). In such contexts, polling stations are usually safe and clean, are perceived as friendly pro-social environments, and tend to be free of violence. Further, elections are generally perceived to be free and fair. These feelings may generally imbue voting with positive associations. At the same time, multiple studies in American politics directly address the behavioral effects of polling station experiences. They find that variation in experiences at polling stations—for instance, being asked for a voter ID—can have longer-term behavioral consequences for perceptions of elections and electoral processes (Alvarez, Hall, and Llewellyn 2008; Atkeson and Saunders 2007).[5] Similarly, evidence shows that the context of a voting environment has effects on behavior (Aldrich et al. 2011; Berger, Meredith, and Wheeler 2008), giving further credence to the idea that electoral experiences matter.

Affective experience habituation seems a plausible theoretical explanation of current findings in the literature: voter

persistence occurs, experiences at the polls have downstream behavioral effects, and "habituation" is a goal-driven result of affective associations. I next establish that South Africans had markedly different electoral experiences in 1994. I then establish that habituation does occur in South Africa, but only among black South Africans. For white and colored voters, whose experiences in 1994 were far more negative than blacks', habituation is either reversed (whites) or simply does not occur (coloreds).

The South African context

While race groups are a somewhat crude source of variation, the South African context is one in which they are plausible. First, researchers studying South African political behavior have routinely referred to elections as "racial censuses" (Ferree 2006, 2010; Lodge 1999; Mattes 1995). There is also empirical evidence that South Africans' political opinions varied markedly by race in and around the 1994 elections.

A nationally representative public opinion survey ($n = 2,517$) conducted by the Institute for Democracy in Africa (IDASA) shortly after the 1994 election includes a number of particularly instructive variables (Mattes et al. 1994). One question asked for respondents' evaluation of the trajectory of "overall conditions" in South Africa. A second asked for an evaluation of the trajectory of "economic conditions" in South Africa. Respondents were also asked to rate the performance of F. W. de Klerk's National Party government, the party that explicitly created legal Apartheid and had ruled since 1948. Finally, respondents were asked which party they felt closest to. With these measures I am able to assess how public perceptions in the election period varied by race group.[6]

Table 1 presents row percentages for each race group's responses. Regression analyses presented in the appendix (available online) show, unsurprisingly, that these differences are statistically significant at conventional levels, even when controlling for employment status, income, and age. In general, white South Africans were disproportionately likely to feel highly negative about the future prospects of South Africa: 76% of whites reported that they felt general conditions in South Africa were either getting worse or getting much worse; 75% reported the same for South Africa's economic conditions. Black and colored South Africans, by con-

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5. For a comprehensive review of this literature, see Stewart (2011).

6. Two caveats should be included with these analyses. First, the data were collected shortly after the election (between August 26 and September 16, 1994). Ideally I would use data collected in the run up to the election, but no such data are available. Second, I focus only on three race groups—black, colored, and white—and I do not consider the Indian population. In the IDASA data, this group is extremely small.

Table 1. Public Sentiment and Party Attachment in 1994 by Race Group

	General Conditions in South Africa				
	Getting Much Worse (%)	Getting Worse (%)	Staying the Same (%)	Getting Better (%)	Getting Much Better (%)
Black	16	26	22	27	9
Colored	16	23	26	27	7
White	27	51	15	7	0
	Economic Conditions in South Africa				
	Getting Much Worse (%)	Getting Worse (%)	Staying the Same (%)	Getting Better (%)	Getting Much Better (%)
Black	16	27	28	23	6
Colored	17	26	29	25	3
White	23	52	17	8	0
	Performance of De Klerk Government				
	Very Poor (%)	Poor (%)	Average (%)	Good (%)	Very Good (%)
Black	33	24	27	14	3
Colored	4	13	22	30	30
White	5	7	28	39	22
	National Party Closest Party? (%)				
	No	Yes			
Black	98	2			
Colored	44	56			
White	48	52			
	African National Congress Closest Party?				
	No (%)	Yes (%)			
Black	24	76			
Colored	73	27			
White	99	1			

trast, generally reported more sanguine opinions of South Africa's prospects. Yet coloreds were much more politically similar to whites than blacks—reporting similar levels of closeness to the National Party (NP) and similar assessments of the NP's performance. Half of all black South Africans said that the NP's performance was poor or very poor, and only 2% reported that the NP was the party they felt closest to. I return to these contrasts later, but suffice it to say white South Africans felt dramatically more pessimistic about South Africa's prospects, black South Africans generally felt negative about the NP but positive about the future, while colored South Africans found themselves more in-between—positive about the future but also positive about De Klerk's performance. Given South Africa's profoundly racialized history and the nature of the transition, these results are not surprising (Lodge 1999; Mattes 1995).

Contemporary trends in voting reported by the Independent Electoral Commission of South Africa (IEC) suggest that participation in the 1994 elections may have had important long-term effects. While individual-level administrative data on registration are not readily available, the IEC publicly reports data in 10-year age bands. Ahead of the 2014 national elections, over the age of 40 (in the bands 40–49, 50–59, etc.), registration and actual participation plateau at roughly 93%–97% and 80%–90% of the voting age population, respectively. In the band 30–39, which includes mostly South Africans under the age of 38, who would have been under 18 in 1994, registration is 5% lower, and it falls very rapidly in the 18–29 age band.

DATA AND RESEARCH DESIGN

While these trends are suggestive, this study attempts a more rigorous analysis of voter persistence, which presents an empirical challenge: the relationship between voting once and voting in the future may be confounded. People who turn out to vote in one election probably differ from those who do not on various unobservable characteristics. To address this challenge, I use a common feature in electoral democracies: the legal voting age.

Elections are a tool for assessing the public's preferences, so setting age restrictions on voting is necessary. Excluding young children, for instance, probably ensures that the electorate more accurately represents public sentiment. Yet as the ages of those eligible and ineligible approaches the legal voting age, the decision to include or exclude becomes increasingly arbitrary. While the legal voting age, 18 years in South Africa, is not arbitrary in general, it is locally arbitrary for those whose ages are very close to the threshold. Separating those aged 17 years and 11 months from those aged 18 years is essentially "random."

This provides an opportunity to study the effect of participation on future participation using a regression discontinuity design (RDD). I compare those who were *just old enough* to vote in 1994 with those who were *just too young*. While the data do not allow me to consider respondents by exact date of birth (it was not collected), I can compare two cohorts of South Africans—those born in 1975 and those born in 1977—over three elections, 1999, 2004, and 2009. It would be ideal to have the exact date of birth, but it seems unlikely that the estimated treatment effects could be accounted for by other differences between those born in 1975 and 1977, and I present a range of evidence to probe the credibility of this assumption.[7]

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7. This constraint affects other studies using age discontinuities in developing contexts, e.g., Croke et al. (2016).

Data

The primary analyses draw on data collected by the Human Sciences Research Council (HSRC), South Africa's statutory research agency for the social sciences and humanities. The HSRC collects repeated yearly cross sections of a nationally representative sample of South Africans' views on a range of political, social, and economic topics in their South African Social Attitudes Survey (SASAS; HSRC 2003–2011), a member of the International Social Survey Programme (ISSP). Each round of SASAS provides a representative sample of roughly 3,500–7,000 individuals including both South Africans and noncitizens (noncitizens are excluded in all of my analyses), across all nine of the provinces. Each round of data locates interviews in a subsample of 500 enumeration areas (EAs) drawn from a master sample of 1,000 EAs.

I use data from 2003 through 2011. The full data from these surveys includes 43,948 observations, though the effective sample size is much smaller, as described below. This date range includes retrospective voting measures for the 1999, 2004, and 2009 National General Elections, respectively, the second, third, and fourth national democratic elections in the South Africa's history.[8]

The primary dependent variable is *vote*, drawn from a question in all waves of the survey, which asks, "Which party did you vote for in the last national election?" The options include all the parties who participated in the election, as well as "don't know," "refuse to answer," and "I did not vote." I code $vote_i = 100$ if the respondent selects any party, vote $vote_i = 0$ if the respondent selects "I did not vote," and I exclude the observation if any of the don't know or refusal options were chosen.[9]

Each respondent's age at the time of the April 1994 election ($age94_i$) is established by using the respondent's age at survey and the year and month of each survey. The data

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8. In results not presented here, I explored whether there were diminishing treatment effects over time. While this is true in the intent-to-treat point estimates (which decline from 4% in 1999 to 2.5% in 2004 and to 1.5% in 2009), limited power prevents these from being estimated with much precision.

9. It is well known that survey data often overstate voting, as many people claim to vote when really they did not. If this measurement error is randomly distributed around the threshold (i.e., randomly distributed between 17- and 19-year-olds), the regression discontinuity design will still return unbiased estimates. If overreporting of voting is associated with treatment assignment, this may be problematic. While I cannot directly assess this, I conducted tests for discontinuities in nonresponse choices (results are presented in the appendix), and I find that rates of "don't know" and "refuse to answer" are not predicted by treatment. This is suggestive that measurement error associated with the dependent variable is ignorable.

can then be decomposed by race ($race_i$). In the main analyses, I include only those who were fully excluded before the 1994 election—specifically, black African South Africans—while in later sections I investigate heterogeneity by race group. The relationship between age, treatment, and mean voting for black South Africans is presented in figure 1, with corresponding figures for other race groups in the appendix.[10] In the estimations presented below, and the following discussions, I limit the group from which inferences are drawn to only those who were 17 (control) and 19 (treated) in 1994. Extensive summary statistics are available in the appendix.

Figure 1 shows that voting propensity jumps upward after the cutoff. While the visualized jump appears small, this is due to the range of the y-axis (see inset). It is also partly due to the shape of the conditional expectation functions either side of the cutoff: rather than increasing steadily in age, voting behavior follows a step-wise distribution. The probability of voting is sharply increasing in age for all individuals under the cutpoint for participation in 1994, which may be due to simple ageing effects or repeated voting in later elections. It then levels out instantly and almost entirely. In developed countries, this is not the case; there is typically a gentler slope, and the probability of voting tends to crest around 50 years of age, declining slowly for older voters. In the South African case, the cresting or plateauing point is exactly on the right-hand side of the cut-point for treatment assignment, which varies from age 28 to age 36 depending on the survey year. For those who were eligible to participate in 1994, there is almost no relationship between age and voting.[11] Among those who were not able to participate, the relationship is 60 times stronger.[12] Estimating the variance of voting probability conditional on treatment shows that voting in the control condition (0.25) has more than twice the variance of voting in the treatment condition (0.11). This is strong evidence that the voting behavior is discontinuous at the cut-point.

Research design

The research design isolates the intent-to-treat effect of participation in the 1994 election on participation in later national elections among those who were previously disenfranchised.

Formally, we may think about the potential outcomes for each individual i as:

$$vote_i = \begin{cases} vote_i(1), & \text{if } D_i = 1, \\ vote_i(0), & \text{if } D_i = 0, \end{cases}$$

for $vote_i \in \{0, 1\}$, which indicates whether individual i voted in the most recent election. Here D_i indicates whether individual i voted in 1994, 1 indicating they did and 0 that they did not. This "treatment" itself is not observed, but "encouragement" to treatment is. I observe whether individuals were eligible to vote in 1994, even if I cannot observe whether they did. "Encouragement" can thus be defined as $Z_i \in \{0, 1\}$. Assignment to encouragement is given as a function of an individual's age in 1994 ($age94_i$) and a threshold c:

$$Z_i = \begin{cases} 1, & \text{if } age94_i > c, \\ 0, & \text{if } age94_i < c, \end{cases}$$

where $c = 18$, the legal age of voting. I discard all data where $age94_i = c$, given that I do not have exact birth dates for respondents. Importantly, it is highly unlikely that anyone who was not eligible in 1994 actually did vote, as voters were required to present either photographic identification documents or a waiver approved by the Department of Home Affairs.[13]

The formal assumption that underpins the research design is $\mathbb{E}[vote_i(d)|age94 = 17] = \mathbb{E}[vote_i(d)|age94 = 19]$ for $d = (0, 1)$. This states that had those who were 17 years old in 1994 ("control") participated in the election, they would, on average, have voted in later elections with the same probability as those who were 19 years old in 1994 and voted ("treated"), and vice versa. The research design is only valid if there are no systematic differences between the 17-year-old and 19-year-old cohorts. Such differences could correlate with the outcome variable yielding spurious estimates of the causal effect.

Plausibility of the research design

I use three approaches to assess the plausibility of the research design: balance tests, historical evidence, and placebo tests. First, covariates collected in the survey allow for tests of balance on observable characteristics. In particular, I assess balance on plausibly confounding covariates that may be increasing in age (employment, income, wealth, schooling, health, and marital status), as well as sex. Definitions for all

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10. I exclude Indian South Africans because, while formerly disenfranchised, they were given a limited franchise in the early 1980s, and it is not clear whether the 1994 election was perceived as positive or negative in these communities.

11. A simple regression restricted to those age 19 and over in 1994 gives a coefficient of 0.001, or 0.1% increase per year, increasing only slightly when including a quadratic age term.

12. The coefficient jumps to 0.06, or 6% per year.

13. While there is speculation that some very small number of 16- and 17-year-olds may have voted, the percentage of noncompliers/always-takers in the control is almost certainly well below 0.1%, and so it can be ignored for all intents and purposes. "Noncompliance" is for all practical purposes one-sided. Thus, $\Pr(W_i = 0|D_i = 1) \geq 0$, but $\Pr(W_i = 1|D_i = 0) = 0$ by definition.
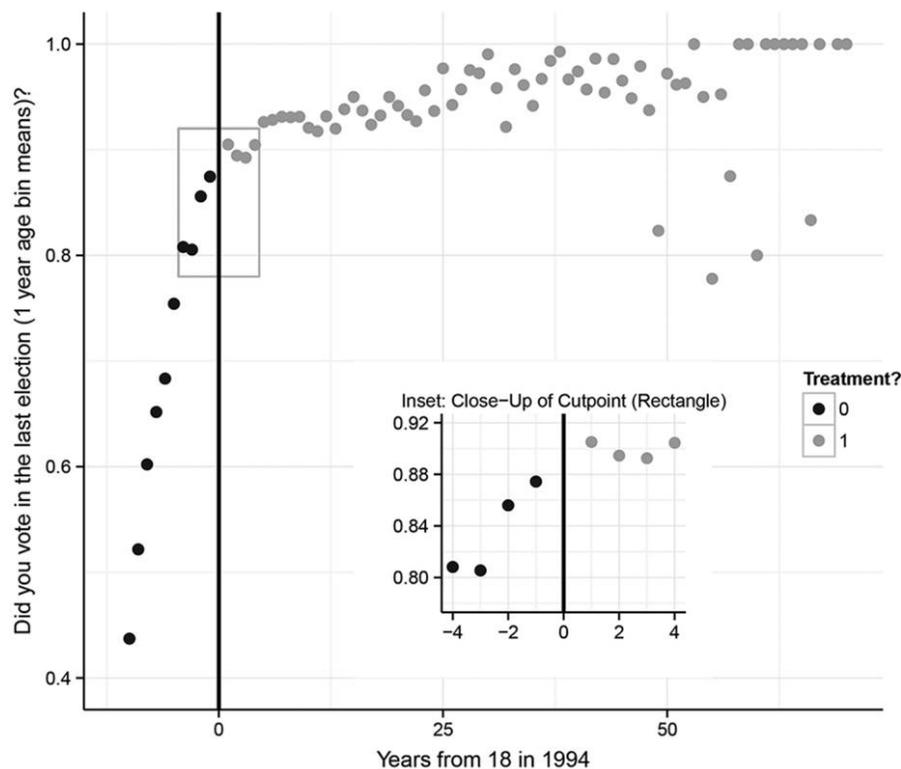
Figure 1. Mean voting by age cohort. This figure shows the mean reported voting by age cohort for black South Africans. The vertical line indicates the cutpoint—the value on the *x*-axis ($Age94_i - 18$) at which treatment is taken. Black points on the left of the line are control; gray points on the right are treated. The inset focuses on the area indicated by the rectangle.

covariates are available in the appendix. Balance is presented in figure 2, which shows the distributions of all covariates for both the treated and control groups. The densities are extremely similar, and, unsurprisingly, statistical tests reveal no difference in either the means or the distributions (for these results, see the balance table in the appendix). In sum, key covariates appear to be very well balanced across treated and control, within a 1 year age window, adding credibility to the assumptions underpinning the design.[14]

While balance tests suggest that the treated and control groups are comparable, there remains the possibility that the cohorts experienced other "treatments." For instance, had major changes to the schooling system affected only those who were 17 in 1994 and not those who were 19, the research design might be threatened. Confounders of this form would require major legislative changes in 1994 that affected either only voters or nonvoters. The nature of the transition in South Africa lends support in this regard. In general, major

legislative changes occurred in the period 1990–91 (with the unbanning of the ANC and the repeal of Apartheid legislation) and 1996 (with the passing of the Constitution). To my knowledge, no legislation that could have differentially affected only 17-year-olds or 19-year-olds was passed in an appropriate period.

Finally, I conducted placebo tests with the same SASAS surveys using varying cut-points in time and the associated observations that fall around those cut-points. Essentially, I pretend that the first democratic election, actually held in 1994, was held in 1992, 1989, 1984, 1979, 1974, or 1969.[15] Using the same method with which I estimate the main effects, I estimate a placebo treatment effect using these placebo cut-points. If confounding differences in cohorts are driving the main results, we would expect the results to replicate across irrelevant time cut-offs. Instead, the placebo

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14. These figures and tests are also presented for white and colored voters in the appendix. I replicated the main results controlling for all these covariates, after multiple imputation using Amelia to handle missingness. The results remain extremely stable and the point estimate barely changes.

15. I choose 1992 as the closest possible date to 1994 that does not include treated and control in placebo treated. Years 1989, 1984, 1979, 1974 and 1969 are the dates of South African elections in which black South Africans were not allowed to participate, and so these are optimal placebo tests for this group—there was an election, but not one in which the sample was eligible to participate.
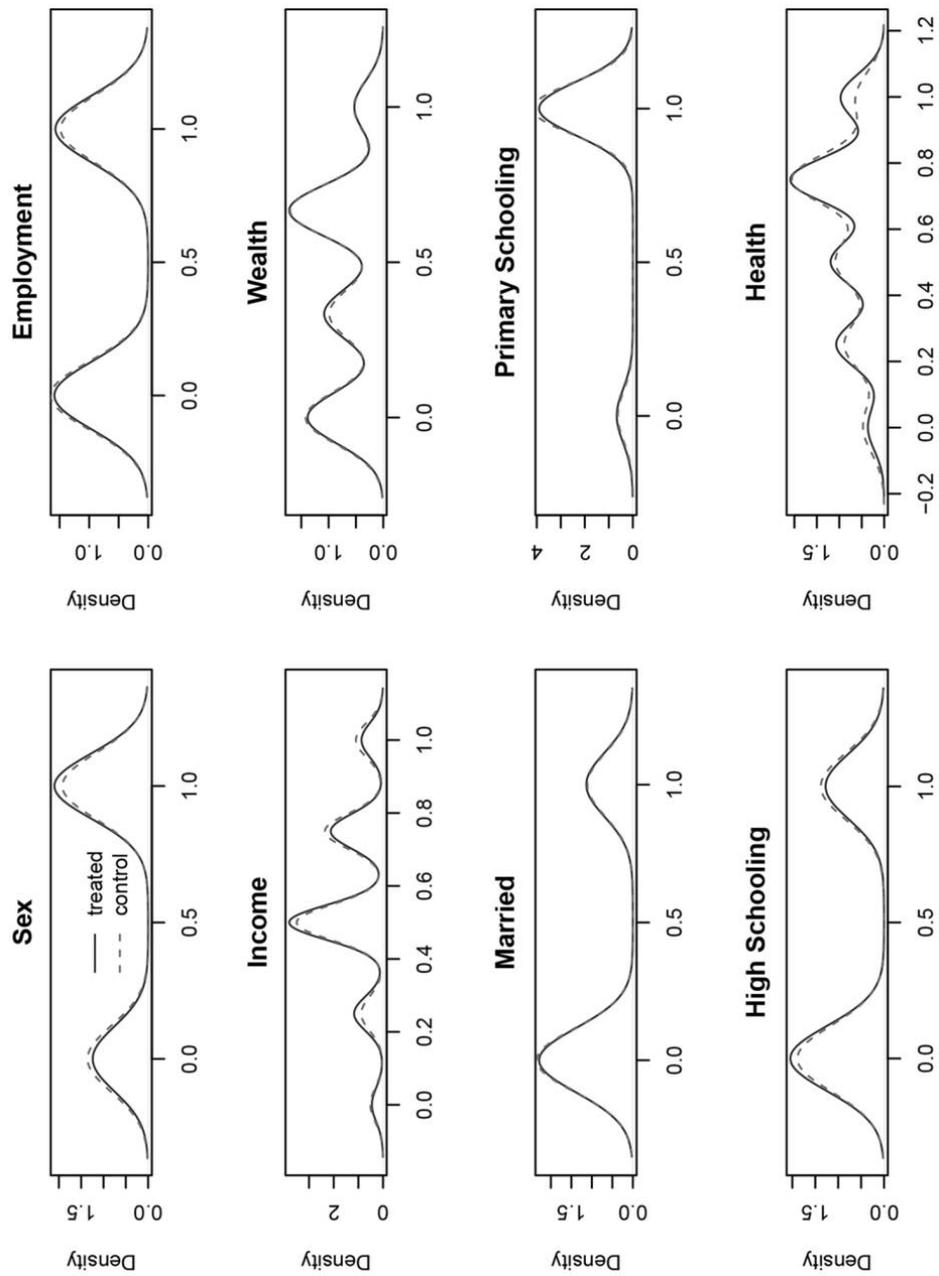
Figure 2. Comparative densities of covariates, 1 year window, black South Africans. This figure shows the densities of all eight covariates, within the 1 year window, for black South Africans. Treated are in dashed lines, control in solid lines.

tests, presented in full in the appendix, show almost no differences across the placebo cut-points and none of similar magnitude to the main effect.

Estimation

To estimate the intent-to-treat effect of participation, I use a linear probability model (LPM) implemented with ordinary least squares:[16]

$$\text{vote}_i = \alpha + \beta D_i + \delta_s + \epsilon_{is}.$$

This estimation strategy also includes survey fixed effects (δ_s), which account for survey-period differences. Essentially, it compares treated and control units within each survey year and averages the effect, eliminating survey year–specific shocks that might generate spurious treatment effects.[17]

As the data are a repeated cross section, there is a time structure. This needs to be accounted for in the estimation of standard errors (Lee and Card 2008). The 2000s are a busy time period in South African political history, and as a result, individual error terms are likely to correlate within each survey time period. For any given survey-year s, the errors for the individuals surveyed that year are likely to correlate in some way. As a result, I cluster standard errors by survey-year, providing nine unique clusters.[18]

RESULTS

How does participation in 1994 affect participation in future elections for newly enfranchised black South Africans? Results comparing 17- and 19-year-olds in 1994 are pre-

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16. The results are essentially the same when estimating the effect using logit. I use the LPM due to ease of interpretability and consistency with fixed effects. One can consider this approach as "semi-parametric" because I focus only on individuals aged 17 or 19 in 1994.

17. While the design is similar to conventional regression discontinuity designs, the discrete measurement of the variable that determines treatment eligibility (by year of birth rather than exact date of birth) means that conventional approaches to regression discontinuities are ill suited here. Fitting highly sensitive and flexible models like a local linear regression infer more from the data when estimating $\text{age}_{94} = 18$ than the data can feasibly provide. Further, the substantial difference in the shapes of the conditional expectation functions below and above the cut-point, as shown in fig. 1, suggests that any approach that globally selects a bandwidth (e.g., conventional local linear regression) will be nonoptimal. See the appendix for a more detailed discussion and for replications of the results using the conventional framework.

18. While conventional cluster robust standard errors are problematic with fewer than 40 clusters, Cameron, Gelbach, and Miller (2008) show that the cluster bootstrap approach is generally free from consistency problems when there are at least 10 blocks or clusters. With 9 clusters the cluster bootstrap should be a roughly consistent estimator of the correct standard errors.

sented in table 2. The results suggest that those who were eligible to vote in 1994 are 3 percentage points more likely to vote than those who were not. Note that, as I discuss later, the result presented here is the intent-to-treat effect (ITT), and it does not take into account the fact that not everyone who could vote in 1994 actually did.

A 3 percentage point increase in the probability of voting is substantively large when turnout is low. Young voters in South Africa routinely turnout at low rates in general elections (Mattes 2012). In the 2009 national election the 18–29 age group participated at around 35% of the eligible voting population; an increased probability of voting of just 3 percentage points at the individual level would increase relative turnout by almost 10% in this age group.

Estimating average effects

While the ITT estimate of 3 percentage points is substantively important, it underestimates the average treatment effect on the treated (ATT), which is a more theoretically interesting quantity. All those who were just old enough to vote in 1994 can be thought of as having been randomly encouraged to take treatment, but not all of them complied with encouragement. In a case of one-sided noncompliance (e.g., none of the control could vote), the ATT is calculated dividing the ITT by the compliance rate. Unfortunately, no records exist of the exact turnout rates among specifically 18- and 19-year-olds in the 1994 election nor is participation in 1994 asked about in the survey. I thus assess potential ATTs for varying levels of compliance given the estimates from the previous section. Using present day and historical turnout rates, I compute lower and upper bounds for the ATT. The results are presented in figure 3.

Overlaid in figure 3 are two vertical lines. The line further to the right represents the turnout rate for the whole country in 1994 (as a percentage of voting age population). The line to the left represents the turnout rate among 18- and 19-year-olds in the 2009 election (again, as a percentage of voting age population). It is highly unlikely that the compliance rate for 19-year-olds in 1994 falls outside of these two bounds. Young voters generally turn out at lower rates than older voters, so it is unlikely that the compliance rate was higher than the national compliance rate in 1994, which was 86%. Turnout has been reasonably stable in the post-1994 elections, so 2009 turnout rate among 18- and 19-year-olds is a plausible lower bound because the turnout rate among 19-year-olds in 1994 was probably higher than the equivalent turnout rate among 19-year-olds in 2009 (roughly 35%). This suggests that the ATT is bounded by 3.44 percentage points at the lower end and 8.46 percentage points at the higher end.

Table 2. Estimated Intent-to-Treat Effect of Past Participation on Future Participation

	Dependent Variable: Vote			
	(1)	(2)	(3)	(4)
Treat	.0307**	.0296**	.0275*	.0263*
	(.0101)	(.0100)	(.0107)	(.0108)
Window	[−1, 1]	[−1, 1]	[−1, 1]	[−1, 1]
Blacks only	Yes	Yes	Yes	Yes
Survey fixed effects	No	Yes	No	Yes
Province fixed effects	No	No	Yes	Yes
R^2	.002	.007	.016	.021

Note. $N = 1{,}288$. The dependent variable ranges from 0 to 100. Square brackets indicate the minimum and maximum values of the forcing variable (age in 1994), recentered around 0. Standard errors clustered by time period are in parentheses.
* $p < .05$.
** $p < .01$.

While the ATT estimates are substantively large, the results are less sanguine than some estimated in some prior studies. For instance, Gerber et al. (2003), who report a 46% treatment effect, or even Denny and Doyle (2009), who provide a more plausible effect estimate of 13%, may overshoot the mark. Yet these results accord almost perfectly with Meredith's (2009) finding of a main effect of between 5% and 7.5%. The contexts of these studies are very different, but the discrepancies in magnitude merit exploration in future research.

Treatment effect heterogeneity by race

As a suggestive test of the theoretical predictions of affective experience habituation, I now consider heterogeneity in treatment effects. The main results focused on the effect of participation among black South Africans, for whom the election was a moment of liberation. As shown earlier, for white South Africans, the emotions attached to the election were highly negative. Many whites still supported the National Party, and 1994 was essentially a moment of submission and immense anxiety. If the affective experience habituation is at work, we should find either negative or null treatment effects for white South Africans.

For colored South Africans, emotional states were more mixed—they were less sanguine than those of blacks but less negative than those of whites. The colored population's history includes a long period of partial enfranchisement, then a period of disenfranchisement, then a period of limited representation in the Tricameral Parliament of the 1980s and a generally compromised political position torn between the Apartheid regime and the ANC (Ferree 2010; Harris 2016). Ahead of the 1994 elections, the colored population

was drawn in multiple directions; see, for example, the earlier evidence that they approved of de Klerk's administration but were still quite positive about South Africa's future. As such, their emotional states should have fallen somewhere between whites and blacks, and a "null" treatment effect seems plausible.[19]

Table 3 presents the main analyses for white South Africans only in column 2 and of colored South Africans only in column 3. For reference, the effect estimates for black South Africans are presented in column 1. In contrast to blacks, participating in 1994 causes white South Africans to vote at lower rates in the future. This suggests that white South Africans were affected very differently by the treatment. This result is consistent with the predictions of the mechanism, that the treatment effect is moderated by associations with the electoral experience. For colored South Africans, participating in 1994 has an essentially zero estimated effect, though it is worth noting that the standard error is quite large. While these tests are not definitive, both results are consistent with the predictions of affective experience habituation.

Alternative explanations

The results so far suggest that participation habituates people to voting but only if they generate positive associations with the act of voting. Yet the main results may stem from a number of explanations over and above, or in place of, affective experience habituation. I assess five alternative channels

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19. The analysis of colored voters was suggested by an anonymous reviewer and had not been conducted prior to hypothesis formation.
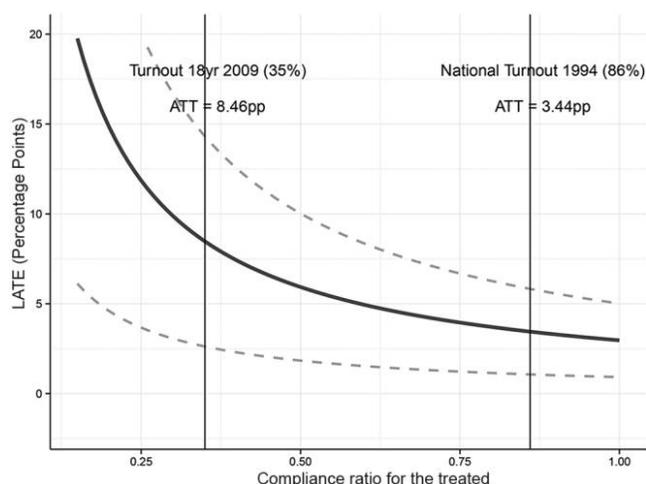
Figure 3. Average treatment effects on the treated for varying levels of compliance. The solid-line curve is the estimated ATT (*y*-axis) for a given level of compliance (*x*-axis). The dashed curves represent the 95% CI for the simulated ATT, given the standard errors of the semiparametric estimate. The vertical lines represent the turn-out rates among the whole country in 1994 (86%) and among 18- and 19-year-olds in the 2009 election (35%). These represent upper and lower bounds for the ATT.

using the same SASAS data: trust in institutions, preference shifts, beliefs about voting, identity changes, and exposure effects.

First, attitudinal shifts might occur when individuals update their trust in institutions. Participation may induce a feeling of active citizenship or ownership of the government. It might also change voters' beliefs about the legitimacy of democracy and democratic institutions, increasing trust in democratic institutions and their propensity to engage with these institutions. I test this channel with *trust in government*, which proxies for general trust in democracy and democratic institutions.

A second possible mechanism is that voting in an election induces voters to sharpen or change their political preferences, thus providing them greater perceived incentive to participate. To test for changes in preferences, I consider voters' political preferences for the political status quo. I measure this through a variable that records respondents' *democratic satisfaction*. This represents a broad assessment of voters' preferences over the contemporary political landscape.

Third, it is possible that voting changes people's beliefs about the act of voting. Active participation on election day implies contact with fellow voters, and, potentially, an active engagement with central democratic themes. In particular, it may suggest to citizens that voting is a social or moral obligation. It may also suggest to them that voting matters, and that expressing one's political beliefs can make a difference. To explore this channel I consider two variables. The first

measures *duty to vote*, which is a respondent's belief that it is everyone's obligation to vote in elections. The second measures whether *voting matters*, which captures a respondents belief that casting a vote can make a difference.

Fourth, identity changes, in the South African case, could have occurred because of the profound and unique importance of the 1994 election. Those who voted may have developed a strong sense of "South African-ness" by engaging in a crucial civic act at an historical moment. I test this explanation with *SA identity*, which measures whether respondents would rather be South African than any other nationality.

Finally, it is plausible that voting in a previous election increases exposure to political parties. For instance, in the United States, researchers speculate that part of the "habitual voting" effect may be due to voters becoming exposed to campaign machinery, as campaigns tend to target registered voters. In the South African case, voters who show up to participate in one time period may have been more strongly exposed to particular parties. In particular, the ANC is the largest and most powerful party—its networks and local structures are the most widespread and well developed, at least in the early and mid-1990s and among black voters. As such, I can test for political exposure effects by simply considering whether treatment has an effect on the probability of voting for the ANC.

The results of these tests are presented in table 4. The first column shows the results for tests of attitude change, the second for preference shifts, the third and fourth for changes in beliefs about voting, the fifth for identity changes, and the sixth for exposure to political parties. The evidence for all four channels is weak. For preference shifts, beliefs about voting, identity changes, and machine politics, the estimated

Table 3. Heterogeneous Effects by Race Group

	Dependent Variable: Vote		
	Black Only	Colored Only	White Only
Treat	2.964**	.139	−10.41*
	(1.009)	(4.553)	(4.671)
Window	[−1, 1]	[−1, 1]	[−1, 1]
Survey fixed effects	Yes	Yes	Yes
N	1,288	210	116
*R*²	.007	.050	.155

Note.   The dependent variable ranges from 0 to 100. Standard errors clustered by year are in parentheses.

* *p* < .05.

** *p* < .01.

Table 4. Alternative Channels

	Dependent Variables					
	Trust Government?	Democratic Satisfaction?	Vote Duty?	Vote Pointless?	SA Identity?	Vote ANC?
Treat	1.785	−.188	.692	1.734	−.333	.526
	(1.405)	(1.902)	(2.161)	(1.950)	(1.590)	(1.430)
Window	[−1,1]	[−1,1]	[−1,1]	[−1,1]	[−1,1]	[−1,1]
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Blacks only	Yes	Yes	Yes	Yes	Yes	Yes
N	1,152	1,151	1,059	1,069	525	1,288
R^2	.037	.075	.027	.042	.011	.058

Note. All dependent variables range from 0 to 100. There are no statistically significant differences in any of these results. Standard errors clustered by year are in parentheses. SA = South African-ness; ANC = African National Congress.
* $p < .05$.
** $p < .01$.

effect sizes tend to be smaller than 1%, and none are statistically significant.

One final alternative explanation is that the estimated effect is a result of the "special" nature of the 1994 election. To rule this out, I replicated the main effect estimate using the 1999 election instead of the 1994 election for black voters. The results are available in the appendix. While a registration effect may partly explain voter persistence as a result of participation in 1999, the magnitudes of the effect estimates are three times larger than the effect estimates for 1994, roughly 9 percentage points. Unless the registration effect is three times larger than affective experience habituation, it seems likely the 1999 effect is a mixture of registration and habituation; the sui generis nature of the 1994 election does not explain the result.

GENERALIZABILITY
Internal generalizability

One issue with regression discontinuity designs is limited generalizeability—the effect estimates apply to those close to the cutpoint. To probe whether the results apply to South Africans more broadly, I consider another point of variation in South Africans' experiences in 1994, election violence. The Uppsala Conflict Data Program's Georeferenced Event Dataset (UCDP GED) provides measures of violent events in South Africa between 1989 and 1994 (Sundberg, Lindgren, and Padskocimaite 2010; Sundberg and Melander 2013). During the long transition to democracy, the Inkatha Freedom Party (IFP), a Zulu nationalist party, was in sustained conflict with the ANC, whose national dominance allowed them to lead the negotiations with the National Party (NP). The violence was primarily guerilla warfare that affected

numerous communities, and this nearly derailed the democratic transition. Between 1989 and 1994, over 12,000 South Africans lost their lives to this conflict (Johnson and Schlemmer 1996; Southall 1994). Importantly, the political violence attenuated dramatically after 1994, which is also evident in the data.

I coded as Violent any current-day political ward in which deaths occurred in the period.[20] I then spatially merged these data with South Africa's electoral geography, which allowed me to study the relationship between violence prior to the 1994 election and turnout among registered voters in the 1999, 2004, and 2009 elections.[21] I use the following regression framework to estimate the association:

$$\text{Turnout}_i = \alpha + \beta \text{Violent}_i + \delta_m + \epsilon_m,$$

for ward i, where δ_m are fixed effects for each municipality m. Standard errors are clustered at the municipality level. This estimation strategy thus compares only wards within the same municipality, and there are on average 10–15 wards per municipality.

While the results should be seen as merely suggestive, table 5 shows that exposure to violence in the run up to the 1994 elections has a lasting negative association with turnout. Those places affected by violence in the 1989–94 period tend to have turnout rates roughly 3 percentage points lower than nonviolent places. Violence in the run up to 1994

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20. While some events are only a single death, the UCDP GED underestimates violence in South Africa in the period. Most cases in which there was a single death were probably more violent. This may imply that the results presented here are conservative, given that many of my "control" units did in fact experience violence.

21. A map illustrating the data is available in the appendix.

Table 5. Violence before 1994 and Future Turnout

	Dependent Variable: Turn-out		
	1999 Election	2004 Election	2009 Election
Violent	−3.59%*	−2.99%[+]	−3.00%*
	(1.71)	(1.52)	(1.27)
Municipality fixed effects	Yes	Yes	Yes
N	2,755	2,799	3,083

Note. The dependent variables range from 0 to 100. All standard errors, clustered by municipality, are in parentheses.

[+] $p < .10$.

* $p < .05$.

probably lowered initial turnout in affected areas, and the reduced-form association is thus consistent with the finding that voting in 1994 affected long-run participation. The results are also consistent with the proposed mechanism—voters exposed to violence had negative experiences in and around the run up to 1994, and they may turn out at lower rates in the future.

External generalizability

This study may have broad implications for sub-Saharan Africa. I briefly consider Ghana, Kenya, and Tanzania as plausible comparison cases, though the findings presented here should be read cautiously. All are former British colonies that underwent multiparty democratizations or extensions of the franchise in the early 1990s. In Ghana, elections were not held from 1979 through 1992, but starting in 1992 elections have been free, fair, and very competitive. Kenya and Tanzania differ from Ghana in that, prior to democratization, there were one-party elections, contested solely by the liberation parties of KANU (Kenya) and CCM (Tanzania).

As in the case of South Africa's 1994 election, Ghana's 1992 elections were largely emancipatory, and while at the time problematic (the opposition did not recognize Rawlings's victory), they have ultimately been perceived positively by voters (Adu-Gyamfi 2014). Since 1992, there have been transitions of power at the executive level, institutions have improved in terms of transparency and credibility, and turnout has remained reasonably high over time (ranging from 64% to 82% of the voting age population). Broadly speaking, elections carry a positive competitive affect in the Ghanaian case.

Much like South Africa, Kenyan politics exhibits clear group-based (ethnicity) voting dynamics, interwoven with policy- and issue-based voting (Bratton and Kimenyi 2008). The transitional elections were received reasonably positively, though many anticipated that KANU would retain power and authority in the multiparty period. The transition was, and politics in Kenya has unfortunately continued to be, relatively violent (Klopp and Zuern 2007). Following brutal violence in 2008, and rising tension in 2016, affect around elections is likely mixed in Kenya.

In Tanzania, 1995 heralded the first multiparty election in the country's history. These first elections were widely perceived as deeply flawed, manipulated by the incumbent CCM, and they have been perceived negatively by many in the country. The CCM has since consolidated its power and now receives over 80% of the vote, and participation has remained low, ranging from 37% to 68% of the voting age population.

Using Afrobarometer data, I am able to repeat the previous analysis for the cases of Ghana, Kenya, and Tanzania. For the Ghanaian case, I use data from the 1999, 2005, 2008, and 2012 rounds of the Afrobarometer; for Kenya, I use data from 2003, 2005, 2009, and 2011, and for Tanzania 2001, 2005, 2008, and 2012.[22] With only four rounds of data accounting for within-year correlations in uncertainty is difficult, and the number of observations close to the threshold—roughly 300 in each sample—renders the exercise underpowered.[23] I implement an estimation strategy that draws on Cattaneo, Frandsen, and Titiunik (2015) and Small, Ten Have, and Rosenbaum (2008).[24] Cattaneo et al. (2015) propose the use of randomization inference in regression discontinuity designs, suggesting that in certain situations units either side of the discontinuity can be treated as though they are units in a randomized trial. Inference is conducted via Fisherian randomization; by permuting treatment repeatedly and comparing a critical statistic (in this case the difference in means), I am able to generate a p-value for the test of the sharp null of no treatment effect. I randomly assign treatment, take the difference-in-means, and repeat the process 10,000 times, comparing the resulting distribution and observed estimate.

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22. The second round of the Afrobarometer lacks questions about voting behavior and is excluded.

23. Using the regression approach (but with robust standard errors rather than cluster-robust standard errors), the results are as follows. For Ghana, the point estimate is 4.179 (p-value = .146), for Kenya it is 2.74 (p-value = .487), and for Tanzania −4.991 (p-value = .123). The point estimates are close to the South African estimates, and it is plausible that with more observations the results would be statistically significant in the regression framework.

24. The average treatment effect is $\hat{\tau} = \mathbb{E}[Y_i|D_i = 1] - \mathbb{E}[Y_i|D_i = 0]$ within a window $= [c, c]$, where $c < 18 < c$.

In the top-left panel of figure 4, in the shaded area, is the previous result for South Africa, analysed by Fisherian inference. In the top right, bottom left, and bottom right panels, in the shaded areas, are the results for Ghana, Kenya, and Tanzania, respectively. The main effect appears to replicate in both Ghana and Kenya (though not statistically significantly so in Kenya). Given the similarities with South Africa for both cases, this suggests that the result may generalize. The Tanzanian case reveals the reverse of the main effect (though the magnitude of the point estimate is similar). In Tanzania, participation in the first democratic election predicts lower levels of participation in the future. This result is consistent with the proposed mechanism—Tanzania, where the first multiparty elections (and subsequent democracy) were less
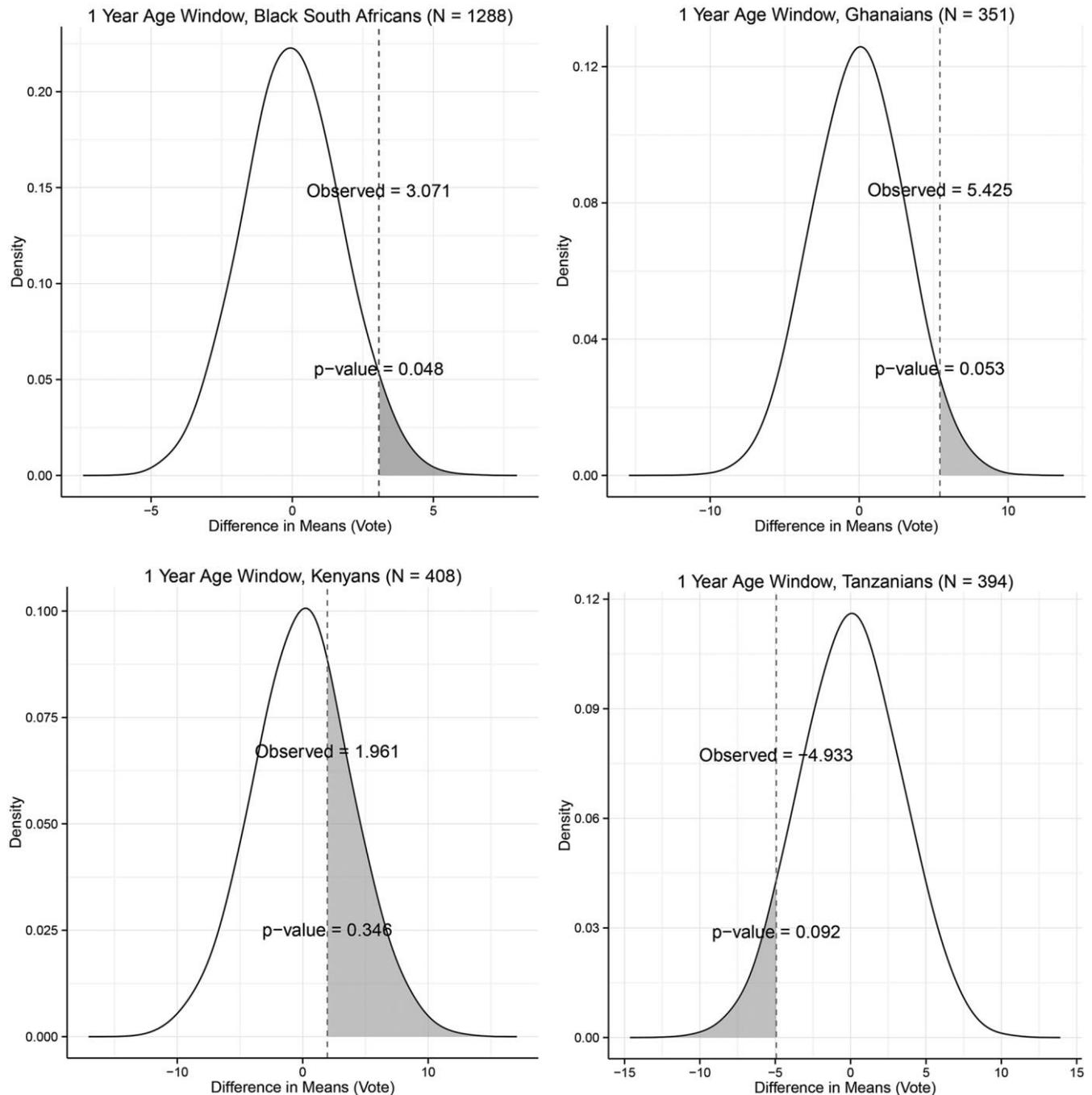

Figure 4. Comparative effects in South Africa, Ghana, Kenya, and Tanzania. These graphs show the empirical distribution of permuted treatment assignment. Top left is South Africa (replication of main result estimates for comparison), top right is Ghana, bottom left is Kenya, and bottom right is Tanzania. Vertical lines give the observed treatment effect from difference-in-means. The p-values are calculated by Fisherian randomization inference.

credible, shows a negative effect. In Ghana, where the initial elections were at least better received and have grown in credibility in the national imagination, there is a positive effect. In the case of Kenya results are more mixed—while it is best not to infer too much from these findings, it is possible that mixed experiences in Kenya yield mixed results, much like the colored community in South Africa.

CONCLUSION

Prior research suggests that elections in developing democracies simply serve to entrench hegemonic elites. This study proposes an alternative view. Elections can affect the long-term political behaviors of ordinary citizens. In particular, I find that among otherwise extremely similar individuals, those who were able to vote in the 1994 elections in South Africa have a higher propensity to vote in future democratic elections than those who were not. The results suggest voting persistence over time, net of any mechanical registration effect. The credibility of these estimates hinge on the comparability of those aged 17 and 19 in 1994, and while I provide a range of evidence that this is plausible, the core assumptions should be studied with care. This notwithstanding the estimated effects corroborate previous research in the United States and Britain, this study presents the first evidence of habituation in a developing democracy. Within this research design and setting, the estimated effect is in the region of 3.5–8.5 percentage points, lower than those found in some other studies. Future research should examine the robustness and origins of this attenuated effect.

This study also suggests a psychological mechanism for understanding this effect. Affective experience habituation, which posits that associations between actions and emotional states drive habituation, accounts for positive voting persistence effects in a range of potential cases. Crucially, affective experience habituation predicts heterogeneous habituation in voters: those who have positive emotional states should habituate, while those who have negative emotional states may cease political participation. By contrasting the effect of participation among those who had, on average, positive emotional dispositions toward the 1994 election with those who, on average, did not, I find evidence consistent with this theory. Further, this study provides suggestive evidence that the result is not driven by alternative channels speculated at by earlier research (Green and Shachar 2000).

It thus appears that elections are not entirely the preserve of the elite in a developing democracy, even one dominated by a single party. Elections appear to have individual-level effects on voters, shaping their political behavior long after they cast their ballots. Yet to sustain the growth of a democratic electorate, voters must have positive electoral experiences, regardless of their political preferences. One important implication of this research is that overwhelmingly negative electoral experiences may actually serve to shrink turnout, a proposition that potentially finds some support in the case of Tanzania. The partisan implications of such effects in noncompetitive democracies are obvious, and troubling. If supporters of the opposition tend to have negative experiences at the polls, the electorate may begin to shape itself over time in favor of the incumbent party. If, however, supporters of the incumbent regime enjoy their electoral experiences, they may consistently turn up at higher rates in the future.

The results of this study should be understood as part of a broader program of research into the behavioral effects of elections on ordinary citizens in developing democracies. Rather than focusing exclusively on the ways in which elites manipulate elections and use them to sustain their authority in developing democracies, researchers should begin to explore the possibility that elections can shape the behavior of ordinary citizens. Voter persistence is merely one such effect; elections may have multiple varied consequences at the behavioral level, many of which may ripple through the macro-electorate.

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