

# Information about Youth Voter Suppression Fails to Induce Significant Voter Backlash Effect

Charlotte Hill\*

September 3, 2021

Word Count: 8,221

## Abstract

In this paper, I show that information about ongoing attempts to disenfranchise young Americans induces anger and psychological reactance, which in turn increases people's intentions to vote. However, after controlling for anger and reactance, information about youth suppression also has a negative *direct* impact on people's turnout intentions. These indirect and direct results effectively cancel each other out, resulting in a statistically insignificant total effect on turnout in general, and youth turnout in particular. I also discover useful information about which types of people are most affected by youth suppression information: treatment effects were larger for those who personally identify with or care more about young people. Party affiliation, and the perception that young people share one's political party preferences, surprisingly had little moderating effect. Ultimately, because the total effect of treatment on youth turnout was minimal and statistically insignificant, I argue that those hoping to counter the effects of youth suppression laws should explore other counter-mobilization strategies.

---

I thank Sarah Anzia, Jake Grumbach, Hakeem Jefferson, Amy Lerman, and Gabe Lenz. All remaining errors are my own.

\* Goldman School of Public Policy, UC Berkeley. [charlottehill@berkeley.edu](mailto:charlottehill@berkeley.edu).

## Introduction

Researchers have extensively documented a large and persistent gap in turnout rates across age groups (e.g., Miller and Shanks 1996; Rosenstone and Hansen 1993; Wolfinger and Rosenstone 1980). In the 2020 presidential election, only 51.4 percent of eligible young adults between the ages of 18 to 24 turned out to vote, whereas 76 percent of citizens ages 65-74 voted—a gap of nearly 25 percentage points (US Census 2021). Even in the historically high-turnout midterm election of 2018, turnout of 18-29-year-olds topped out at 35.6 percent, compared to 66 percent for senior voters (Misra 2019).

One promising approach to shrinking the age-turnout gap is to directly reduce those voting costs that disproportionately affect young people, such as registration costs. Yet this typically requires passing election reforms through friendly state legislatures—and in recent years, many state policymakers have not only signaled an unwillingness to consider such reforms but actively moved in the opposite direction, adopting laws that make it *harder* for young people to register and vote. Can anything be done to mitigate the effect of these restrictive policies on youth turnout?

The past decade has been marked by a wave of youth suppression laws, primarily implemented by Republican-controlled legislatures.<sup>1</sup> In North Carolina, elected officials responded to college students voting disproportionately for Democratic candidates in 2012 by closing campus polling places, removing students from local voter rolls, and introducing a Senate bill that “would prevent parents from claiming their college-aged children as dependents if their child registers to vote in the county where they go to school instead of their home county” (Now Foundation 2014). In Texas, the Republican legislature passed a law to end early voting at temporary or mobile sites, effectively eliminating early voting on college campuses; Democrats sued in response, claiming the move was aimed at suppressing youth turnout (Goldenstein 2019). In Wisconsin, students can only use their college ID to satisfy the state’s voter identification requirement if the ID card displays the date it was issued, has an expiration date no more than two years after the issuance date, and includes the student’s signature, and if the student also shows proof of residency and college enrollment (Jervis 2020). In total, seven states with strict voter ID laws do not accept student ID cards as valid proof of identity (Campus Vote Project 2019).

Youth voter suppression—primarily but not solely directed at college students—has been documented by journalists (e.g., Levin 2020, Williams 2020, Wines 2019), legal scholars (e.g., Fearon-Maradey 2014, Bromberg 2018), and advocacy organizations (e.g., Leach and Cohen 2020, Lee 2014, Rock the Vote 2020). Many political leaders also appear to recognize that voter suppression is a real and growing threat to youth voting: in the lead-up to the 2020 general election, Democratic members of Congress introduced a new bill to protect youth voting rights,

---

<sup>1</sup> The question of how to define voter suppression is a difficult and longstanding one. A core dilemma is whether one should only consider a legislature’s *intention* in passing a law or also acknowledge the law’s practical *effects* in terms of reducing the turnout of a particular group. Another is whether a distinction should be drawn between *proactive* suppression brought on by a new policy, in contrast to *passive* suppression that stems from failing to change a particular policy that has disproportionately negative impacts on young people. In this paper, I loosely define youth voter suppression as a proactive legal change that has either the intention or the real-world effect of disproportionately reducing the turnout of young people.

citing “efforts to disenfranchise youth” that “could have lasting effects for decades to come” as the motivation behind the legislation (Janfaza 2020).

Most scholarship on voter suppression focuses on how it dilutes or restricts votes by racial and ethnic minorities (e.g., Bentele and O’Brien 2013, Burden 2018, Hajnal et al. 2017). While this is a critically important line of inquiry, there is room to expand this research agenda to explore the impact of suppressive policies on youth turnout. Suppression may play a role in low youth voting rates, especially in states and districts with more extreme suppressive measures in place. For example, one recent study found that election environments with stricter voting laws have twice as large a negative effect on young voters as they do on older individuals, all else being equal (Juelich and Coll 2020).

To the extent that laws aimed at restricting youth turnout are indeed effective, is there a way to combat their effects outside the legislative process? That is, in jurisdictions with legislators who stand to benefit from reducing youth voting rates, and who therefore are incentivized to keep suppressive laws on the books, is it possible to mitigate the turnout-depressing effects of youth suppression policies?

One intriguing possibility is to spark sufficient backlash against these laws that young people actually turn out in *higher* numbers after the laws are enacted. Such a backlash effect has been documented among other voting blocs: Democrats (Valentino and Neuner 2017) and Black voters (Biggers 2019). It may be that informing young Americans about ongoing attempts to suppress the youth vote induces a backlash effect that compensates for whatever depressive turnout effects these laws would otherwise have. Moreover, if other age groups sympathize sufficiently with young people, or if they see young people as part of a shared identity group such as their political party, they may similarly be activated and turn out at higher rates—though this could also have the unintended consequence of exacerbating the age-turnout gap.

In this paper, I present the results of a survey experiment, which finds that information about ongoing attempts to disenfranchise young Americans induces anger and psychological reactance, which in turn increases people’s intentions to vote. However, after controlling for anger and reactance, information about youth suppression also has a negative *direct* impact on people’s turnout intentions. These indirect and direct results effectively cancel each other out, resulting in a statistically insignificant total effect on turnout. I also discover useful information about which types of people are most affected by youth suppression information: treatment effects were larger for those who personally identify with or care more about young people. Party affiliation, and the perception that young people share one’s political party preferences, surprisingly had little moderating effect.

Ultimately, because the total effect of treatment on youth turnout was minimal and statistically insignificant, I argue that those hoping to counter the effects of youth suppression laws should explore other counter-mobilization strategies. Inducing youth backlash is unlikely to be a winning strategy.

Anger, Psychological Reactance, and Voter Backlash

Two prominent studies have hypothesized a relationship between information about voter suppression and voter backlash. They posit that when people encounter information about voter suppression, they experience psychological states of arousal, which in turn motivate them to engage in more political participation.

In Valentino and Neuner's 2017 study, people who read about voter ID laws through a "voter disenfranchisement" frame feel angry, and this anger leads them to intend to vote at higher rates. Past research has found that anger can lead individuals to expend greater energy and resources in order to overcome threats (Huddy et al. 2007, Lerner and Keltner 2001), that group threats can trigger strong emotional responses (Groenendyk and Banks 2014), and, perhaps most importantly, that anger can lead individuals to participate in politics at higher rates (Valentino et al. 2011). Valentino and Neuner find that the anger response varies by party, with strong Democrats feeling much angrier in response to the disenfranchisement frame than strong Republicans. Interestingly, the backlash effect among Democrats appears even stronger when voter suppression is framed as an attempt to reduce *Black* voter turnout. The authors attribute these results to group identity: Democrats, recognizing that the targets of voter suppression tend to be left-leaning, feel angry that "their side" is being harmed.

In a separate set of field experiments, Biggers (2019) finds that framing suppressive actions as targeting African Americans leads Black voters to experience more *psychological reactance*—a negative emotional reaction in response to the feeling that someone is taking away your choices or freedom, typically measured as "an amalgamation of anger and negative cognitions" (Reynolds-Tylus et al. 2020). In the psychology literature, multiple studies have shown that public health messages discouraging certain harmful activities such as smoking (Grandpre et al. 2003) and drinking alcohol (Albarracin et al. 2004, Bensley and Wu 1991) can spark backlash in subjects, leading them to engage in even more undesired behavior. Political scientists have been slow to incorporate reactance into their understanding of political behavior; the few studies that do measure reactance do so in the context of examining whether social pressure outreach actually alienates some voters, counterproductively driving down turnout (e.g., Gerber et al. 2008, Mann 2010, Panagopoulos 2013).

Notably, Biggers finds only limited evidence of this reactance translating into real-world changes in voting behavior. In the first of three experiments, Biggers identified a positive and statistically significant treatment effect on Black voters. In two subsequent experiments, however, there was no significant effect. There are several ways to interpret these findings. It could be that reactance has little impact on political participation, and the first experiment's findings were simply a statistical anomaly. Alternatively, perhaps the second and third experiments had a lower rate of treatment delivery.<sup>2</sup> It could also be that subjects in the second and third experiments—conducted years after the first wave of voter ID laws—had already experienced backlash and been converted into voters. This would help explain why Biggers found a positive treatment effect in his first study, shortly after voter ID laws were initially

---

<sup>2</sup> To induce reactance, Biggers mailed experimental subjects a postcard that, among other language, contained a short paragraph framing voter ID laws as a "deliberate assault on the voting rights of minorities, the elderly, and low-income and young voters." While the language itself is strong, there is no way to know how many people read it—or even saw the postcard briefly.

passed. Further research is clearly needed to understand under what conditions, and for which populations, information about voter disenfranchisement leads to greater political participation.

### *The moderating role of groups and identity*

There are several factors that potentially moderate a backlash effect on turnout. Group identity is first among them. A long literature has found that partisan identities and racial identities are meaningful, strong, and consequential for political attitudes and behaviors (e.g., Chong and Rogers 2005, Greene 1999, Huddy et al. 2015, Leighley and Vedlitz 1999), and a perceived threat to these identity groups might induce negative reactions and spur greater participation.

Which group identities are most likely to matter for backlash to youth suppression? One obvious answer is age identity. While we know very little about this particular identity, new research from Trachtman, Anzia, and Hill (2021) finds that Americans do indeed identify with their age groups. For young people in particular, age identity is comparable in strength to partisan identity—though partisan identity is not as strong for young people as for older age groups. Moreover, young individuals with stronger age identities are more likely to vote and engage in other forms of participation. But how young people respond to attempts to restrict their ability to vote, and how strength of age identity moderates this response, has not been explored. Voter suppression frames might only produce backlash when centered on an important shared group identity—and it is unclear whether age-based identities, like “youth,” are as strong as race-based identities like “Black.” It may be that young people with a stronger sense of age identity experience greater backlash to youth suppression frames than those with weaker age identity.

Party identity could moderate the backlash effect as well. Valentino and Neuner (2017) find that self-identified Democrats are much more angry about voter disenfranchisement—an understandable response, given that disenfranchising laws are typically adopted by Republican lawmakers, at least partly in an effort to reduce Democratic vote-share. And yet, the authors also find that Democratic respondents feel most angry when disenfranchising laws are framed not as targeting *Democratic* voters, but as targeting *Black* voters. Valentino and Neuner offer one explanation for this finding: many Democrats are aware that Black voters are a key part of the Democratic voting coalition. Accordingly, an attempt to suppress Black voters can still feel to non-Black Democrats like an attack on their own identity group. Whether this phenomenon applies in the case of information about *youth* suppression, rather than the suppression of Black voters, likely depends on whether people perceive young people as being more closely aligned with the Democratic Party. That is, if Valentino and Neuner are correct in their interpretation of their results, we should expect youth suppression information to have the biggest effect on Democrats who understand that young people tend to *also* tend to be Democratic.

There is another possible explanation for why the authors find that information about voter suppression has a larger effect on Democrats than Republicans: perhaps Democrats are more likely to care about Black people and feel angry on their behalf, even if they do not associate Black people with their own identity group. In other words, an individual need not necessarily belong to the aggrieved identity group in order to be angered and mobilized by its suppression. Along these lines, Valentino and Neuner find in a separate cross-sectional survey

that those with high symbolic racism (SR) scores feel far less angry toward voter ID laws than those with lower SR scores. If how individuals feel toward the suppressed group also matters, then information about *youth* suppression may be particularly effective at sparking backlash among those who feel warmly toward young Americans, regardless of their own age.

There may also be important heterogeneity based on an individual's race. There is a long history of people of color being impacted by voter suppression efforts, and today's young people disproportionately belong to minority racial and ethnic groups. For these reasons, people of color may connect more strongly with—and therefore react more strongly to—information about youth disenfranchisement.

In short, there are multiple pathways through which youth suppression information might affect participation across groups. To understand how this information will shape not only individual behavior but also the overall composition of the electorate, we must examine the responses of voters across age and racial groups.

## Methods

### *Sample*

To explore these ideas, I ran a survey experiment measuring the extent to which information about youth voter suppression evokes voter backlash, and whether effects were particularly strong for young people. The experiment was fielded between April 27 and 28, 2021, on a U.S. Census-balanced convenience sample using Lucid, an online survey sampling firm.<sup>3</sup> A total of 4,898 respondents completed the survey. Respondents were approximately 51% female and 74% white. Around 55% had attended some or no college, and 51% reported a household income of less than \$50,000 a year.<sup>4</sup>

### *Experimental Design and Treatments*

Respondents were block-randomized on the basis of age, race (white vs. non-white), and educational attainment (no college vs. at least some college) to one of three news conditions: one control condition, and two treatments focusing on the role of changes to voting laws in the 2022 midterm election. The randomization was successful, with balance across all demographic covariates (Appendix Table C1).

Treatment and control messages were presented as short newspaper articles modeled after the messages originally used in Valentino and Neuner's 2017 study on Democratic backlash to voter ID laws. All three conditions followed the same format: two paragraphs emphasized the importance of increasing voter turnout (relative to persuading swing voters) for the 2022

---

<sup>3</sup> Coppock & McClellan (2019) show that experimental results replicated on Lucid samples match closely with benchmark results obtained using national probability samples.

<sup>4</sup> Two attention checks were included in the survey in an effort to improve data quality and demographic reliability (Aronow et al. 2020). More than 99% of respondents passed at least one attention check, and 80% of respondents successfully completed both attention checks. The substantive interpretation of the results below did not change when the dataset was limited to only those who passed both attention checks.

midterm elections, one paragraph discussed how this information was shifting campaigns' political strategies, and a final paragraph featured a quote from a political consultant about the electoral impact of this new strategy. Key lines were bolded to increase the likelihood of successful experimental manipulation. In order to avoid deception, I designed the articles to include only accurate information about the details, scope, and consequences of voter suppression. Full wording of each condition can be found in Appendix C.

Respondents assigned to the control condition read an article carefully crafted to make salient the topics of elections, campaign strategy, and political competition, while not discussing voting laws or disenfranchisement. The first two paragraphs—the same for individuals assigned to all conditions—stated that campaigns were spending millions of dollars to persuade voters, but that data suggested mobilizing base voters would be more effective. The third paragraph stated that campaigns were therefore changing tactics and investing in “digital advertisements, text messages, and other voter mobilization strategies,” and the fourth quoted a political consultant advocating for mobilization over persuasion.

The two treatment conditions discussed how changes to voting laws could depress turnout. Each condition was designed to be increasingly specific about the targets and potential consequences of these legal changes, in order to test which elements of the article drove any changes in anger, reactance, and intended participation. The first treatment, the generic “suppression” condition, said that these laws would likely have a disenfranchising effect, but did not specify any particular identity group as the target or victim. It named four voting law changes that some politicians were introducing in order to reduce turnout among their opponents: closing polling places, limiting early voting, banning the use of certain ID cards to meet voter identification requirements, and making it harder to register to vote. In the concluding paragraph, the political consultant anticipated that these laws would prevent thousands of Americans from exercising their right to vote and called this “an absolute outrage.”

The second treatment, the “youth suppression” condition, added to the generic suppression condition that *young people* were the intended targets of these laws and would be negatively impacted by them. The legal changes were described as closing *campus* polling places, limiting early voting *at colleges and universities*, banning the use of *student* ID cards to meet voter identification requirements, and making it harder for *young adults* to register to vote. The political consultant was quoted as saying that these laws would prevent thousands of *young Americans* from exercising their right to vote.

### *Measures*

Before treatment, respondents were asked a series of questions about several beliefs and attitudes that could potentially moderate the strength of the treatment effect: strength of age identity, warmth toward other groups, and perceptions of young people's political partisanship. Age identity was measured by first asking individuals which age group they identified with—*young adults*, *middle-aged adults*, or *older adults*—and then asking them how strongly they identified with this age group. Exact wording of all survey questions can be found in Appendix C.

The group warmth measure was modified from Payne et al.'s (2010) approach to measuring explicit prejudice. To measure perceptions of young people's partisanship, I asked

respondents whether, if they had to guess, they would say that young people today usually vote for Democrats or Republicans. Basic demographic information—age, gender, race, ethnicity, educational attainment, household income, political party affiliation, region, and zip code—were supplied by Lucid.

A number of outcomes were measured post-treatment, beginning with two potential mediators of voter backlash: anger and psychological reactance. To measure anger, I used Valentino and Neuner's (2017) approach, asking respondents how strongly the story they read made them feel a number of emotions. Responses ranged from "not at all" to "extremely" on a 5-point scale. Subjects' responses for four emotions—angry, outraged, disgusted, and annoyed—were then combined into an index that was re-coded to range from 0 to 1.

To measure reactance, I departed from the traditional psychological method, which simply adds a respondent's anger score to their negative thoughts score. While I originally adopted this approach in an earlier pilot study, it ultimately appeared insufficient for capturing reactance in this context. Typically, reactance measures are used in psychological research in response to treatments that explicitly try to get subjects to curtail bad behaviors—for example, messages emphasizing how bad smoking is. Then experimenters measure negative thoughts and anger, combine them, and use that index score as a measure of the reactance that subjects experienced. This approach, while reasonable for those types of experiments, is difficult to translate into a context in which individuals are *learning* about a freedom-restricting attempt but not actually being *subjected* to the attempt in real time. The researcher can ask subjects whether they feel angry and negative toward what they read about ongoing attempts at voter suppression, but whether respondents who feel those sentiments perceive their *own* freedom as under threat remains unclear.<sup>5</sup>

I instead measured reactance by first asking subjects to state how much they agreed or disagreed that there were people trying to restrict their own ability to vote in elections, with answers ranging from "strongly disagree" to "strongly agree" on a 7-point scale. Answers were re-coded to range from 0-1 and then combined with scores from the anger measure. The index was again re-coded to range from 0-1 for ease of interpretation.

Political participation was measured by asking respondents how likely they were to vote in the 2022 midterm election. Response options ranged from extremely unlikely to extremely likely on a 7-point scale, and answers were re-coded to range from 0-1.<sup>6</sup> I also measured two other potentially important outcomes that could be affected by the message frames: beliefs about the prevalence of voter suppression in general, and beliefs about how often specific groups (e.g., young people) are targeted by voter suppression laws.

---

<sup>5</sup> Alternatively, we could adopt the psychologists' approach and put an anti-voting message directly in front of experimental subjects; however, doing so would not only present clear ethical concerns but also lack external validity, as most suppression is carried out via policy change rather than overt anti-voting language.

<sup>6</sup> Three additional political participation questions asked how likely the respondent was to attend a political meeting in the coming year (either in person or virtually), how likely they were to give money to a political individual or group in the coming year, and how interested they were in volunteering to raise awareness of voter suppression. Results for these measures are available upon request.



## Results

### *Perceptions of suppression*

Reading the hypothetical news article about youth voter suppression did not change overall beliefs about how common voter suppression is in elections. It did, however, generally increase respondents' perception that *young people* are targeted by suppressive voting laws, though this change was only statistically significant for older age groups (Table 1). Respondents ages 65 and older were the most impacted: seniors who received the youth treatment were 12 percentage points more likely to agree that young people are sometimes, often, or almost always targeted by voter suppression, compared to seniors in the control group ( $p < .01$ ). There was no corresponding significant effect on young respondents. This is possibly because they started out more likely than other age groups to believe young people were targeted by suppression: among those assigned to the control condition, 52% of young adults thought youth were targeted, compared to just 38% of seniors.

**Table 1: Youth Treatment Increases Perception of Youth Voter Suppression**

		Regression Results				
		<i>Dependent variable:</i>				
		youth targeted by suppression				
		18-29	30-39	40-49	50-64	65+
generic treatment	0.010 (0.022)	-0.007 (0.023)	-0.050* (0.027)	0.030 (0.020)	0.019 (0.021)	
youth treatment	0.031 (0.022)	0.040* (0.023)	0.078*** (0.026)	0.113*** (0.020)	0.120*** (0.022)	
constant	0.518*** (0.016)	0.496*** (0.016)	0.471*** (0.019)	0.405*** (0.014)	0.380*** (0.015)	
Observations	1,085	969	785	1,218	838	

*Note:* \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$

### *Anger and reactance*

The youth treatment significantly increased respondents' anger and reactance (Tables 2 and 3).<sup>7</sup> Contrary to expectations, the effects were largest for older respondents. The difference in treatment effects for young versus old subjects was striking: for 18-29-year-olds, the youth treatment caused an 13.3 percentage-point bump in anger; by contrast, for those 65 and older, the treatment increased anger by 32.0 percentage points ( $p < .01$ )—an effect almost three times as large. Similarly, the youth treatment boosted reactance for 18-29-year-olds by 10.3 percentage points, compared to 19.7 percentage points for seniors ( $p < .01$ ).

<sup>7</sup> The youth treatment was only slightly more effective than the generic treatment, and differences in effect sizes by age group were not significantly different from one another (Appendix Figure C1).

These differences could be partly explained by different starting levels of anger and reactance across age groups. Young respondents in the control condition reported higher baseline levels of both anger and reactance.<sup>8</sup> Even if anger and reactance were held constant at the levels reported by young respondents in the control group, however, the youth treatment would still have a larger effect for older individuals.

**Table 2: Youth Treatment Increases Respondent Anger  
Regression Results**

<i>Dependent variable:</i>					
anger					
	18-29	30-39	40-49	50-64	65+
generic treatment	0.116*** (0.016)	0.128*** (0.018)	0.140*** (0.022)	0.215*** (0.018)	0.299*** (0.021)
youth treatment	0.133*** (0.016)	0.152*** (0.019)	0.143*** (0.021)	0.212*** (0.018)	0.320*** (0.021)
constant	0.424*** (0.012)	0.399*** (0.013)	0.397*** (0.015)	0.365*** (0.012)	0.336*** (0.015)
Observations	1,082	966	782	1,217	834
<i>Note:</i>	* p<0.1; ** p<0.05; *** p<0.01				

**Table 3: Youth Treatment Increases Respondent Reactance  
Regression Results**

<i>Dependent variable:</i>					
reactance					
	18-29	30-39	40-49	50-64	65+
generic treatment	0.096*** (0.015)	0.090*** (0.018)	0.092*** (0.022)	0.150*** (0.018)	0.179*** (0.022)
youth treatment	0.103*** (0.015)	0.090*** (0.018)	0.093*** (0.021)	0.126*** (0.018)	0.197*** (0.023)
constant	0.506*** (0.011)	0.494*** (0.013)	0.485*** (0.015)	0.456*** (0.013)	0.440*** (0.016)
Observations	1,082	966	782	1,217	834
<i>Note:</i>	* p<0.1; ** p<0.05; *** p<0.01				

### *Voting intentions*

<sup>8</sup> A series of pairwise t-tests confirms this difference (Appendix Tables C2–C3). The pairwise t-tests are conducted using a Bonferroni correction, which adjusts for the fact that when testing multiple hypotheses, there is an increased chance of observing a rare event and therefore incorrectly rejecting a null hypothesis.

The total effect of the youth treatment on intentions to vote ranged from around +1.1 to -1.2 percentage points, depending on the age group. Younger respondents saw the biggest boost (Table 4). However, none of the treatment effects by age group reached conventional levels of statistical significance. In other words, we cannot reject the null hypothesis that exposure to information about youth suppression had *no* overall effect on young people’s—or any age group’s—average intentions to vote in 2022. Despite this result, a series of mediation analyses does suggest a strong positive, *indirect* relationship between information about youth suppression and intentions to vote in 2022.

**Table 4: Youth Treatment Has Insignificant Total Effect on Voting Intentions**

<b>Regression Results</b>					
<i>Dependent variable:</i>					
vote in 2022					
	18-29	30-39	40-49	50-64	65+
generic treatment	0.005 (0.021)	-0.012 (0.023)	-0.015 (0.025)	-0.009 (0.019)	0.015 (0.019)
youth treatment	0.011 (0.021)	-0.007 (0.023)	-0.012 (0.024)	-0.012 (0.019)	0.006 (0.019)
constant	0.696*** (0.015)	0.761*** (0.017)	0.809*** (0.017)	0.850*** (0.014)	0.899*** (0.014)
Observations	1,086	969	785	1,219	837

*Note:* \* p<0.1; \*\* p<0.05; \*\*\* p<0.01

### Anger and reactance as mediators

The traditional causal-steps approach to mediation would hold that, because the total effect of treatment on voting was insignificant, no further exploration of a mediation effect is necessary—there is simply no effect to mediate (Baron and Kenny 1986). However, other scholars have shown that even in the absence of evidence of a total effect of treatment, there can still exist a significant mediated relationship between the independent variable of interest (in this case, the youth treatment) and the outcome (voting) (e.g., Bollen 1989, p. 52, Koschate-Fischer et al. 2018, Kraemer et al. 2002, p. 879-880, and Rucker et al. 2011).

This situation could arise from a number of factors, including less precise measurement of the treatment and outcome variables than the mediating variable, a stronger relationship between treatment and mediating variable than between the treatment and outcome variable, and an insufficiently large sample size (see Rucker et al. 2011 for further discussion). Another possible explanation—one particularly relevant here—is the existence of a suppressor effect, in which the *direct* effect of treatment on the outcome is oppositely signed from the *indirect* effect of treatment via the mediator.<sup>9</sup> MacKinnon et al. (2000) cite the example of the interrelationships between stress, coping, and mood. The direct effect of stress on mood is presumably negative;

<sup>9</sup> This is also known as “inconsistent” or “competitive” mediation (Koschate-Fischer and Schwillle 2018).

however, the effect of stress on coping is likely positive, as is the effect of coping on mood. As a result, the indirect effect of stress on mood, via the coping mediator, is positive. Because the direct and indirect effects are oppositely signed, the total effect (defined as the sum of the direct and indirect effects) may appear very small, or even statistically indistinguishable from zero. As I show below, it appears that the suppressor effect may be at play in my mediation results, resulting in an insignificant total effect despite the presence of significant direct and indirect effects.

Recent scholars have advocated for using the product of coefficients approach to mediation. This approach involves deprioritizing the total effect and instead directly calculating an indirect mediation effect: in this case, the youth treatment's *indirect* effect on intentions to vote in 2022 that goes through anger or reactance.<sup>10</sup> Mechanically, the indirect effect is identified by first calculating the relationship between treatment and the mediator in question, then by calculating the relationship between the mediator and the outcome, and finally by multiplying these two numbers. The direct effect, by contrast, is the effect of treatment on the outcome after controlling for the mediator in question. By using the *mediator* R package, I was able to use bootstrapped samples to identify average direct and indirect effects of both hypothesized mediators, as well as p-values and confidence intervals for each.

On average, the youth treatment does have a positive and significant *indirect* effect on the likelihood of voting: exposing people to information about youth suppression increases their reported anger and psychological reactance, and in turn, this higher anger and reactance increases their intentions to vote in the future. However, youth suppression information also seems to have a negative *direct* impact on overall intentions to vote, and these indirect and direct effects effectively cancel each other out. Notably, these patterns vary by age group, a phenomenon I discuss in detail below. Both anger and reactance were found to play a significant mediating role between treatment and voting intentions, though anger appeared to be a stronger mediator overall than reactance.

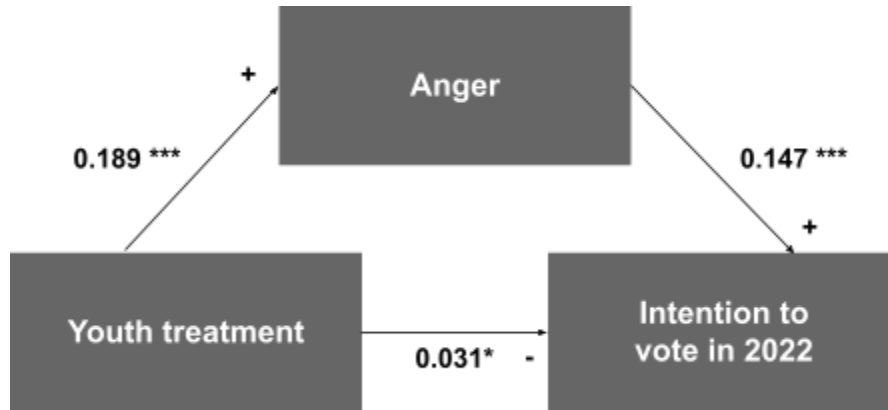
As Figure 1 illustrates, the regression coefficient between treatment and anger (.189) was significant, as was the regression coefficient between anger and intention to vote in 2022 (.147). The indirect effect was therefore  $(.189) * (.147) = .028$ . I tested the significance of this effect using bootstrapping procedures. Unstandardized indirect effects were computed for each of 1,000 bootstrapped samples, and the 95% confidence interval was computed by determining the indirect effects at the 2.5th and 97.5th percentiles. The bootstrapped unstandardized indirect effect was .028, and the 95% confidence interval ranged from .021 to .03. Thus, the indirect effect was statistically significant ( $p < .001$ ).

We can also see in Figure 1 that the direct effect of the youth treatment on voting intentions was approximately the same size as the indirect effect via anger, but negative (-.31,  $p < .05$ ). As a result, the total effect is not statistically distinguishable from zero.

### **Figure 1: Anger Mediates Effect of Treatment on Voting Intentions**

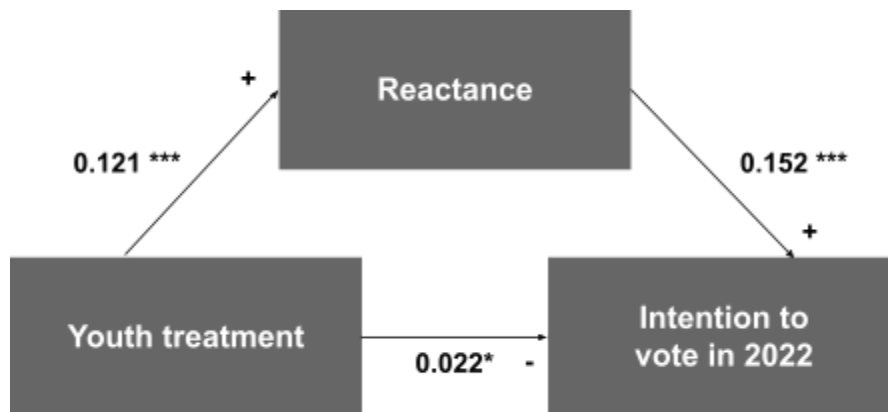
---

<sup>10</sup> This indirect mediation effect is also known as the average causal mediation effect (ACME).



Turning to reactance, the regression coefficient between treatment and reactance (.121) was significant (Figure 2), as was the regression coefficient between reactance and intention to vote in 2022 (.152). The indirect effect was  $(.121) \times (.152) = .0183$ . Using the same bootstrapping procedures described above, I tested the significance of this indirect effect and identified a 95% confidence interval. The bootstrapped unstandardized indirect effect was .0183, and the 95% confidence interval ranged from .014 to .02, indicating that the indirect effect was statistically significant ( $p < .001$ ). Meanwhile, the direct effect of treatment on voting intentions was once again negative ( $-.022$ ,  $p < .05$ ), explaining the insignificant total effect of treatment on voting intentions.

**Figure 2: Reactance Mediates Effect of Treatment on Voting Intentions**



### Mediation effects by age group

Importantly, the mediated effect of youth suppression information appears to vary by age. Table 5 reports the bootstrapped average indirect effect (AIE), average direct effect (ADE), and treatment effect (TE) by age group for both mediators.<sup>11</sup> The treatment's AIE via anger was

<sup>11</sup> Given these findings—that the indirect effects of treatment on turnout intentions are larger for seniors when mediated by anger and larger for youth when mediated by reactance—I conducted a final mediation analysis looking

largest for seniors and smallest for young adults (Table XX)—perhaps unsurprisingly, given that the youth treatment boosted anger more for seniors than for other age groups. However, the ADE was also largest and *negatively* signed for older adults. As a result, the *total* effect of treatment on voting intentions was largest for young people—though neither the ADE nor TE for any age group was statistically distinguishable from zero.

In other words, it could be that treatment does in fact have a disproportionately positive impact on turnout for the youngest respondents, and that a larger sample size or more precise measurement (or both) would reveal this finding. Further research is needed to determine if this is the case.

**Table 5: Mediation Effects Vary By Age Group**

age group	<i>anger</i>			<i>reactance</i>		
	Avg. indirect effect	Avg. direct effect	Total effect	Avg. indirect effect	Avg. direct effect	Total effect
18-29	0.0128**	0.0012	0.0140	0.0221***	-0.0098	0.0123
30-39	0.0257***	-0.0318	-0.0061	0.0212***	-0.0273	-0.0062
40-49	0.0140*	-0.0272	-0.0131	0.0131***	-0.0264	-0.0133
50-64	0.0336***	-0.0453*	-0.0117	0.0169***	-0.0288	-0.0119
65+	0.0445***	-0.0389.	0.0055	0.0176**	-0.0122	0.0054

Significance codes: p<.001 = \*\*\*, p<.01 = \*\*, p<.05 = \*, p<.1 = .

By contrast, the youth treatment’s indirect effect via reactance was largest for young people. For respondents ages 18-29 and 30-39, the AIE was 2.2 percentage points (p<.001), while for older age groups, it was consistently smaller. As with anger, the direct effect was largest and *negatively* signed for older respondents. The average total effect was therefore largest and only positive for young adults. But again, neither the ADEs or the TEs by age group reached statistical significance.

Age identity

As expected, strength of age identity moderated the youth treatment’s impact. I coded respondents as having either high or low age identity strength, with high-age ID individuals those who reported identifying with their age group either extremely or very strongly. Table XX shows that the interaction between youth treatment and strong age ID was 9.9 percentage points (p<.05). Substantively, this means that the youth treatment was especially effective at boosting voting intentions among young people with strong age identities. For slightly older

---

only at respondents’ perception that people are trying to restrict their own ability to vote in elections. Respondents 18-29 were the only ones with a significant AIE (.0116, p<.001). I report the full results in Appendix Table C7.

respondents—those ages 30 to 39—the interaction between youth treatment and age ID strength was also large and marginally significant (8.7 percentage points,  $p < .1$ ). For older age groups, this interaction was both smaller in magnitude and did not reach statistical significance.

**Table 6: Age Identity Moderates Treatment Effect**

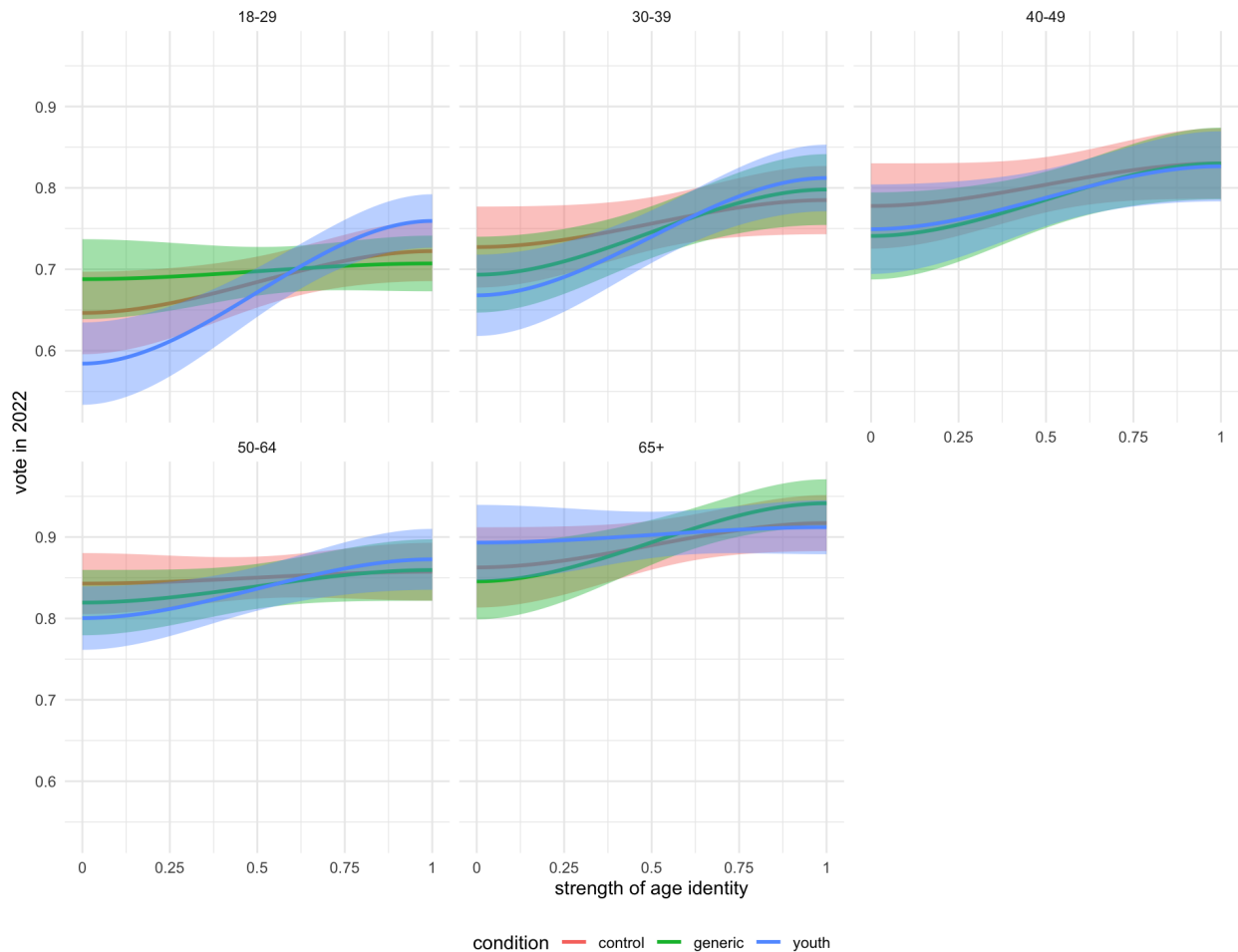
<b>Regression Results</b>					
	<i>Dependent variable:</i>				
	vote in 2022				
	18-29	30-39	40-49	50-64	65+
generic treatment	0.042 (0.035)	-0.034 (0.034)	-0.037 (0.039)	-0.023 (0.028)	-0.017 (0.034)
youth treatment	-0.062* (0.036)	-0.059 (0.036)	-0.029 (0.038)	-0.042 (0.028)	0.030 (0.033)
strong age ID	0.076** (0.031)	0.058* (0.033)	0.052 (0.035)	0.015 (0.027)	0.054* (0.029)
generic treatment:strong age ID	-0.057 (0.043)	0.047 (0.046)	0.037 (0.050)	0.025 (0.038)	0.041 (0.041)
youth treatment:strong age ID	0.099** (0.044)	0.087* (0.047)	0.025 (0.049)	0.058 (0.039)	-0.036 (0.041)
constant	0.646*** (0.025)	0.727*** (0.025)	0.778*** (0.027)	0.843*** (0.020)	0.863*** (0.024)
Observations	1,086	969	785	1,217	837

*Note:* \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$

These results make intuitive sense. Young people who strongly identify as such should be more affected by youth suppression information than young people who identify weakly with their age group.

Figure 3 visualizes the relationship between treatment, age group, age ID strength, and intentions to vote in 2022. Age ID strength is shown on the x-axis, and intention to vote is shown on the y-axis. Across age groups, treatment has a larger effect on average voting intentions for those with strong age ID. But this relationship is much stronger, and only statistically significant, for the youngest respondents.

**Figure 3: Age Identity Disproportionately Moderates Treatment for Young Adults**



### Partisanship and co-partisanship

The youth treatment did not increase Democratic respondents' intentions to vote more than non-Democrats in any age group (Appendix Table C4). Similarly, there was no disproportionate effect for Democratic respondents who believed that young people were their co-partisans (Appendix Table C5). This latter finding suggests that, to the extent that learning about youth suppression drives people to want to vote, the reason for this effect is not simple political self-interest.

### Warmth toward young people

As expected, the youth suppression treatment was particularly effective at boosting voting intentions for respondents who feel warmly toward young people (Table 7). This moderating effect was only positive and (marginally) significant, however, for middle-aged respondents ( $p < .1$ ). This makes some sense; younger respondents do not need to feel particularly warm toward people their age to be angered by youth suppression, as they themselves are the targets.



Older respondents, by contrast, may need further reason to care about suppression, such as positive feelings toward the targeted group.

Why no significant moderating effect for seniors—especially since, as shown in Appendix Table C6, those who felt warmly toward youth did experience greater *anger* in response to treatment? It could be that there simply was not as much opportunity for treatment to move seniors’ voting intentions. Seniors started out far more likely than other age groups to intend to vote: nearly 85 percent of seniors assigned to the control group and low in warmth intended to vote.

**Table 7: Warmth Toward Young People Moderates Treatment Effect**

Regression Results					
	Dependent variable:				
	vote in 2022				
	18-29	30-39	40-49	50-64	65+
generic treatment	0.079 (0.066)	-0.092 (0.066)	0.029 (0.077)	-0.008 (0.063)	-0.065 (0.075)
youth treatment	-0.059 (0.061)	-0.116* (0.067)	0.099 (0.069)	-0.118* (0.063)	0.070 (0.082)
warmth	0.249*** (0.059)	0.112* (0.065)	0.252*** (0.071)	0.118** (0.058)	0.068 (0.070)
generic treatment:warmth	-0.105 (0.087)	0.117 (0.090)	-0.069 (0.103)	-0.005 (0.085)	0.111 (0.097)
youth treatment:warmth	0.108 (0.082)	0.155* (0.091)	-0.149 (0.094)	0.144* (0.085)	-0.083 (0.104)
constant	0.517*** (0.045)	0.684*** (0.047)	0.632*** (0.053)	0.768*** (0.043)	0.847*** (0.056)
Observations	1,086	967	782	1,219	836
<i>Note:</i> *p<0.1; **p<0.05; ***p<0.01					

### Racial group

While my sample size was not large enough to sufficiently power analyses by both age *and* race, I did examine how the effect treatment varied by respondents’ racial group alone. Table 8 shows that the youth treatment had a significant positive effect of 12.6 percentage points for Hispanic voters (p<.01), though effects for all other racial groups were smaller, negatively signed, and statistically insignificant. That said, the generic treatment had an even larger positive impact on Hispanic voters’ turnout intentions. To the extent that inducing backlash could help remedy longstanding inequities in voter turnout, speaking more generally about voter suppression rather than focusing on young people may be more effective at boosting Hispanic turnout.

**Table 8: Treatment Boosts Voting Intentions Among Hispanic Respondents**  
**Regression Results**

	<i>Dependent variable:</i>				
	vote in 2022				
	White	Black	Asian	Hispanic	Other
generic treatment	-0.012 (0.011)	-0.014 (0.031)	0.019 (0.040)	0.134*** (0.044)	-0.013 (0.065)
youth treatment	-0.005 (0.011)	-0.006 (0.029)	-0.050 (0.042)	0.126*** (0.046)	-0.025 (0.063)
constant	0.825*** (0.008)	0.756*** (0.021)	0.743*** (0.029)	0.649*** (0.033)	0.704*** (0.047)
Observations	3,633	545	281	223	150

*Note:* \* p<0.1; \*\* p<0.05; \*\*\* p<0.01

## Discussion

This experiment aimed to determine whether giving people—especially young people—information about youth-targeted voter suppression increased their anger and psychological reactance and, ultimately, their intentions to participate in American politics. At first glance, the results were not particularly promising. Exposure to information about youth suppression did not have a significant overall effect on intentions to vote—not for young people, and not for any other age group.

The youth suppression treatment did, however, *indirectly* increase some people’s likelihood of voting, by way of increasing their anger and reactance. This indirect effect via anger was largest for seniors, while for reactance, it was largest for youth. But after adjusting for these emotional states, youth suppression information also had a negative (albeit statistically insignificant) *direct* effect on turnout intentions for older age groups. As a result, the *total* impact of youth suppression information may indeed be largest for the youngest respondents. In both the anger and reactance mediation analyses, the total effect of treatment on turnout intentions was only positive for those ages 18-29—though because these results failed to reach conventional levels of statistical significance, further research with a larger sample size or stronger treatment (or both) is necessary.

What might explain these negative direct effects of youth suppression information on voting intentions? One plausible explanation is that reading about efforts to restrict youth voting left a metaphorical bad taste in people’s mouths, turning them off from political engagement altogether. After all, it probably takes a certain type of person to hear about “dirty” politics and want to participate *more*, not less.

As expected, information about youth voter suppression disproportionately boosted turnout intentions for certain types of individuals. Age identity strength played a large

moderating role, but only for young people: when young individuals with strong age identities read about youth being targeted by voter suppression, their voting intentions increased at higher rates than youth with lower age identities. Meanwhile, strength of age identity had no bearing on treatment effects for older age groups.

But individuals needed not personally identify as young to be affected by information about youth suppression. Simply feeling warmly toward young people also increases the treatment's impact on voting intentions. Upon hearing that young people were being targeted by restrictive voting laws, middle-aged people who felt warmly toward youth expressed greater intentions to turn out than middle-aged people who didn't favor young people as much. (Seniors exhibited similar emotional responses to treatment, but their voting intentions did not shift as dramatically, perhaps because they were already much more likely to vote than middle-aged adults.)

Intriguingly, youth suppression information did not increase intentions to vote more for Democrats than for Republicans. Similarly, it did not seem to matter much whether individuals believed young people shared their political party preferences. In other words, the backlash effect—to the extent that it exists—did not seem driven by partisan self-interest but, rather, by an identification with or concern for the targeted group.

These party-based results are noticeably different from those found by Valentino and Neuner (2017), who find that Democrats are especially mobilized by voter ID laws. What might explain this difference? To start, young individuals have been found to identify less strongly with their age group than Democrats identify with their party (Trachtman et al. 2021). It is clear that youth with stronger age identities respond more to treatment—but if most young people do not identify particularly strongly with being young, this may translate into a relatively weak overall treatment effect. By contrast, if Democrats typically identify strongly with their party, then framing voter suppression efforts as targeting their party could be much more impactful. Future research might explore whether priming youth identity increases the effectiveness of youth suppression information at driving turnout.

As noted earlier, the youth treatment did not have a significant total effect on respondents' intentions to vote in the 2022 midterm elections. It could be that the treatment was simply ineffective at mobilizing people. Alternatively, it could be that the Lucid survey respondents were already highly likely to vote in 2022, relative to the general public, leaving them little "room for improvement" on this measure. The data supports this possibility, as 78% of study participants reported voting in 2020—a full 12 percentage points higher than the general public (Schaul et al. 2020). Young Lucid respondents reported especially high voting rates compared to their general-public counterparts: nearly 62% of 18- to 29-year-old respondents reported voting, compared to the 50 percent of 18- to 29-year-olds in the general public who actually voted (CIRCLE 2021). It could be, then, that treatment would indeed boost young people's intentions to vote, but only for those who were not already highly likely to participate. To explore this possibility, the experiment could be conducted on a nationally representative sample of young people, as well as paired with a field experiment to measure how information about youth suppression changes real-world voting outcomes.

A separate outstanding question is whether the increasing tendency of young people to vote for Democratic candidates will shift these findings over time. While 61% of youth voted for Joe Biden in the 2020 general election, compared to just 37% who voted for Donald Trump (CIRCLE 2021), only about half (52%) of respondents in my experiment believed that young people tend to vote for Democrats. While I did not find that perceived co-partisanship moderates treatment effects, this could be because individuals are not particularly confident in their perception of young people's voting tendencies. If current trends hold and youth continue supporting Democrats over Republicans—and if this translates into a more widely held belief that young people are Democratic—perceptions of co-partisanship might coalesce, solidify, and begin meaningfully moderating treatment effects. Of course, if this primarily increased treatment effects for *older* respondents, this could actually worsen rather than mitigate the age-turnout gap.

Overall, the results of this experiment are not particularly promising for those who hope voter backlash will keep turnout stable in the face of youth-targeted voter suppression. The core idea of voter backlash is that when individuals learn their group, or a group they care about, is being suppressed, the emotional arousal they experience in response motivates them to vote at higher rates. In this case, while young respondents did feel some anger and reactance in response to learning their age group was being suppressed, this did not translate into a substantial or statistically significant overall uptick in intentions to vote—at least, not one that can be detected with this particular group of respondents and limited sample size. As anti-democratic lawmakers across the country propose and pass laws aimed at driving down young people's electoral participation, those political organizations and campaigns that want to *keep* young people voting would do well to consider alternative approaches to counter-mobilization.

## References

- Albarracín, D., G. T. Kumkale, and B. T. Johnson. 2004. "Influences of Social Power and Normative Support on Condom Use Decisions: A Research Synthesis." *AIDS care* 16(6): 700–723.
- Aronow, Peter Michael, Joshua Kalla, Lilla Orr, and John Ternovski. 2020. "Evidence of Rising Rates of Inattentiveness on Lucid in 2020." <https://osf.io/preprints/socarxiv/8sbe4/> (April 6, 2021).
- Bensley, Lillian S., and Rui Wu. 1991. "The Role of Psychological Reactance in Drinking Following Alcohol Prevention Messages." *Journal of Applied Social Psychology* 21(13): 1111–24.
- Bentele, Keith G., and Erin E. O'Brien. 2013. "Jim Crow 2.0? Why States Consider and Adopt Restrictive Voter Access Policies." *Perspectives on Politics* 11(4): 1088–1116.
- Biggers, Daniel R. 2019. "Can the Backlash Against Voter ID Laws Activate Minority Voters? Experimental Evidence Examining Voter Mobilization Through Psychological Reactance." *Political Behavior*. <https://doi.org/10.1007/s11109-019-09587-0> (April 9, 2021).
- Bromberg, Yael. 2018. "Youth Voting Rights and the Unfulfilled Promise of the Twenty-Sixth Amendment." *University of Pennsylvania Journal of Constitutional Law* 21: 1105.
- Burden, Barry C. 2018. "Disagreement over ID Requirements and Minority Voter Turnout." *The Journal of Politics* 80(3): 1060–63.
- Campus Vote Project. 2019. "Student ID as Voter ID." *Fair Elections Center*. <https://www.campusvoteproject.org/student-id-as-voter-id> (November 21, 2020).
- Chong, Dennis, and Reuel Rogers. 2005. "Racial Solidarity and Political Participation." *Political Behavior* 27(4): 347–74.
- CIRCLE. 2020. "Election Week 2020: Young People Increase Turnout, Lead Biden to Victory." *Center for Information and Research on Civic Learning and Engagement*. <https://circle.tufts.edu/latest-research/election-week-2020> (January 3, 2021).
- Coppock, Alexander, and Oliver A. McClellan. 2019. "Validating the Demographic, Political, Psychological, and Experimental Results Obtained from a New Source of Online Survey Respondents - Alexander Coppock, Oliver A. McClellan, 2019." *Research & Politics*. <http://journals.sagepub.com/doi/full/10.1177/2053168018822174> (December 12, 2020).
- Doherty, Carroll. 2018. "The Generation Gap in American Politics." *Pew Research Center for the People and the Press*.

<https://www.people-press.org/wp-content/uploads/sites/4/2018/03/03-01-18-Generations-release2.pdf> (November 20, 2019).

- Doherty, Carroll, Jocelyn Kiley, and Bridget Johnson. 2016. *2016 Campaign: Strong Interest, Widespread Dissatisfaction*. Pew Research Center.  
<https://www.people-press.org/wp-content/uploads/sites/4/2016/07/07-07-16-Voter-attitudes-release.pdf>.
- Fearon-Maradey, Sarah. 2014. "Disenfranchising America's Youth: How Current Voting Laws Are Contrary to the Intent of the Twenty-Sixth Amendment." *University of New Hampshire Law Review* 12: 289.
- Flores, Alejandro, and Alexander Coppock. 2018. "Do Bilinguals Respond More Favorably to Candidate Advertisements in English or in Spanish?" *Political Communication* 35(4): 612–33.
- Gerber, Alan S., Donald P. Green, and Christopher W. Larimer. 2008. "Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment." *American Political Science Review* 102(1): 33–48.
- Goldenstein, Taylor. 2019. "Democrats Sue Texas over Mobile Voting Ban." *Houston Chronicle*.  
<https://www.houstonchronicle.com/politics/texas/article/Democrats-sue-Texas-over-mobile-voting-ban-14584810.php> (November 21, 2020).
- Graham, Matthew H. 2020. "Self-Awareness of Political Knowledge." *Political Behavior* 42(1): 305–26.
- Grandpre, Joseph et al. 2003. "Adolescent Reactance and Anti-Smoking Campaigns: A Theoretical Approach." *Health Communication* 15(3): 349–66.
- Greene, Steven. 1999. "Understanding Party Identification: A Social Identity Approach." *Political Psychology* 20(2): 393–403.
- Groenendyk, Eric W., and Antoine J. Banks. 2014. "Emotional Rescue: How Affect Helps Partisans Overcome Collective Action Problems."  
<https://onlinelibrary.wiley.com/doi/abs/10.1111/pops.12045> (April 9, 2021).
- Hajnal, Zoltan, Nazita Lajevardi, and Lindsay Nielson. 2017. "Voter Identification Laws and the Suppression of Minority Votes." *The Journal of Politics* 79(2): 363–79.
- Hartig, Hannah. 2018. "In Their Own Words: Why Some People Find Voting Difficult." *Pew Research Center*.  
<https://www.pewresearch.org/fact-tank/2018/11/05/in-their-own-words-why-some-people-find-voting-difficult/> (September 13, 2019).

- Hill, Charlotte. 2020. "Young People Face Higher Voting Costs and Are Less Informed About State Voting Laws." Berkeley Institute for Young Americans.  
[http://youngamericans.berkeley.edu/wp-content/uploads/2020/08/Hill\\_BIFYA\\_Working\\_Paper\\_08\\_08\\_2020.pdf](http://youngamericans.berkeley.edu/wp-content/uploads/2020/08/Hill_BIFYA_Working_Paper_08_08_2020.pdf).
- Holbein, John B., and D. Sunshine Hillygus. 2016. "Making Young Voters: The Impact of Preregistration on Youth Turnout." *American Journal of Political Science* 60(2): 364–82.
- . 2019. *Making Young Voters: Converting Civic Attitudes into Civic Action*. Cambridge: Cambridge University Press.
- . 2020. *Making Young Voters: Converting Civic Attitudes Into Civic Action*. Cambridge University Press.
- Huddy, Leonie, and Alexa Bankert. 2017. "Political Partisanship as a Social Identity." In *Oxford Research Encyclopedia of Politics*, Oxford University Press.  
<https://oxfordre.com/politics/view/10.1093/acrefore/9780190228637.001.0001/acrefore-9780190228637-e-250> (December 12, 2020).
- Huddy, Leonie, Lilliana Mason, and Lene Aarøe. 2015. "Expressive Partisanship: Campaign Involvement, Political Emotion, and Partisan Identity." *American Political Science Review* 109(1): 1–17.
- Janfaza, Rachel. 2020. "Lawmakers Introduce New Bill to Protect Youth Voting Rights." *CNN*.  
<https://www.cnn.com/2020/08/13/politics/legislation-youth-voting-rights/index.html> (November 21, 2020).
- Jervis, Rick. 2020. "Youth Voter Turnout Shows Suppression Isn't Stopping Young Americans." *USA Today*.  
<https://www.usatoday.com/story/news/politics/elections/2020/10/31/young-voters-suppression-tactics-early-voting-election-record-numbers/6071293002/> (November 21, 2020).
- Juelich, Courtney L., and Joseph A. Coll. 2020. "Rock the Vote or Block the Vote? How the Cost of Voting Affects the Voting Behavior of American Youth: Part of Special Symposium on Election Sciences." <https://journals.sagepub.com/doi/abs/10.1177/1532673X20920265> (April 9, 2021).
- Leach, Emily, and Brent J. Cohen. 2020. "REPORT: Young People Are the Pathway to Victory in 2020." *Generation Progress*. <https://genprogress.org/youthvote/> (November 30, 2020).
- Lee, Erin Ferns. 2014. *Enfranchising America's Youth*. Project Vote.  
<http://www.projectvote.org/wp-content/uploads/2014/05/POLICY-PAPER-Enfranchising-Americas-Youth-May-9-2014.pdf>.

- Leighley, Jan E., and Arnold Vedlitz. 1999. "Race, Ethnicity, and Political Participation: Competing Models and Contrasting Explanations." *The Journal of Politics* 61(4): 1092–1114.
- Lerner, Jennifer S., and Dacher Keltner. 2001. "Fear, Anger, and Risk." *Journal of Personality and Social Psychology* 81(1): 146–59.
- Levin, Dan. 2020. "Student Voting Surges Despite Efforts to Suppress It." *The New York Times*. <https://www.nytimes.com/2020/10/29/us/student-voting-surge.html> (November 21, 2020).
- Mann, Christopher B. 2010. "Is There Backlash to Social Pressure? A Large-Scale Field Experiment on Voter Mobilization." *Political Behavior* 32(3): 387–407.
- Miller, Warren E., and J. Merrill Shanks. 1996. *The New American Voter*. Cambridge, Mass: Harvard University Press.
- Misra, Jordan. 2019. "Behind the 2018 U.S. Midterm Election Turnout." *The United States Census Bureau*. <https://www.census.gov/library/stories/2019/04/behind-2018-united-states-midterm-election-turnout.html> (October 26, 2019).
- Now Foundation. 2014. "Voter Suppression Targets Women, Youth and Communities of Color (Issue Advisory, Part One)." *National Organization for Women*. <https://now.org/resource/voter-suppression-targets-women-youth-and-communities-of-color-issue-advisory-part-one/> (November 21, 2020).
- Panagopoulos, Costas. 2013. "Positive Social Pressure and Prosocial Motivation: Evidence from a Large-Scale Field Experiment on Voter Mobilization." *Political Psychology* 34(2): 265–75.
- Pew Research Center. 2016. "Mechanical Turkers: Young, Well-Educated and Frequent Users." <https://www.pewresearch.org/internet/2016/07/11/turkers-in-this-canvassing-young-well-educated-and-frequent-users/> (December 12, 2020).
- . 2019. "Most Democrats Are Excited by 'Several' 2020 Candidates - Not Just Their Top Choice." *Pew Research Center for the People and the Press*. <https://www.people-press.org/2019/08/16/most-democrats-are-excited-by-several-2020-candidates-not-just-their-top-choice/> (November 4, 2019).
- Postmes, Tom, S. Alexander Haslam, and Lise Jans. 2013. "A Single-Item Measure of Social Identification: Reliability, Validity, and Utility." *British Journal of Social Psychology* 52(4): 597–617.



- Quick, Brian L. 2012. "What Is the Best Measure of Psychological Reactance? An Empirical Test of Two Measures." *Health Communication* 27(1): 1–9.
- Reynolds-Tylus, Tobias, Elisabeth Bigsby, and Brian L Quick. 2020. "A Comparison of Three Approaches for Measuring Negative Cognitions for Psychological Reactance." *Communication Methods and Measures*: 1–17.
- Rock the Vote. 2020. "About Rock the Vote." <https://www.rockthevote.org/about-rock-the-vote/> (November 30, 2020).
- Rosenstone, Steven J, and John Mark Hansen. 1993. *Mobilization, Participation, and Democracy in America*. New York: Longman.
- Schaul, Kevin, Kate Rabinowitz, and Kate Mellnik. 2020. "2020 Turnout Is the Highest in over a Century." *Washington Post*. <https://www.washingtonpost.com/graphics/2020/elections/voter-turnout/> (January 3, 2021).
- Trachtman, Samuel, Sarah Anzia, and Charlotte Hill. 2021. "Age Group Identity and Political Participation."
- US Census Bureau. 2021. "2020 Presidential Election Voting & Registration Tables Now Available." *The United States Census Bureau*. <https://www.census.gov/newsroom/press-releases/2021/2020-presidential-election-voting-and-registration-tables-now-available.html> (September 3, 2021).
- Valentino, Nicholas A., and Fabian G. Neuner. 2017. "Why the Sky Didn't Fall: Mobilizing Anger in Reaction to Voter ID Laws." *Political Psychology* 38(2): 331–50.
- Williams, Asiah. 2020. "Of Course Republicans Want to Suppress the Youth Vote | The Nation." <https://www.thenation.com/article/politics/youth-vote-suppression/> (November 21, 2020).
- Wines, Michael. 2019. "The Student Vote Is Surging. So Are Efforts to Suppress It. - The New York Times." <https://www.nytimes.com/2019/10/24/us/voting-college-suppression.html> (November 21, 2020).
- Wolfinger, Raymond E., and Steven J. Rosenstone. 1980. *Who Votes?* New Haven: Yale University Press.
- Zukin, Cliff et al., eds. 2006. *A New Engagement? Political Participation, Civic Life, and the Changing American Citizen*. Oxford ; New York: Oxford University Press.