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Closing the Age Gap in Voter Turnout: The Promise of Election Reform, And the Rising Threat of Youth Voter Suppression

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By

Charlotte Hill

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Committee in charge:

Professor Amy Lerman, Chair
Professor Sarah Anzia
Professor Gabriel Lenz

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Abstract

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Young Americans consistently vote at lower rates than older Americans, particularly seniors. What explains this age-turnout gap, and which interventions, if any, can help reduce it?

My dissertation argues that voting costs are distributed unequally across age groups, and that this unequal distribution plays a central role in explaining the age-turnout gap. I present empirical evidence that when certain voting costs are alleviated via the introduction of voter registration reforms, youth voting rates disproportionately increase: same-day registration laws boost youth turnout by 3.2 to 7.3 percentage points, and automatic voter registration is associated with a 6.3 percentage-point increase in youth turnout. Older age groups see much smaller effects.

Using data from an original, nationally representative survey, I show that young Americans face higher voting costs than older age groups. In turn, these higher costs predict lower registration and turnout rates. Compared to seniors, today's youth are less informed about the voting process and how to research candidates and issues; struggle more to find the time to vote, to plan ahead to vote, and to balance voting with other life tasks; face greater transportation issues and tradeoffs between voting and earning money; have greater difficulty with the parts of voting that cannot be done online; disproportionately think mail voting is a hassle; and are less likely to own the documentation they need to register and vote. When asked directly, youth are significantly more likely than older Americans to say that registration and voting are difficult. After adjusting for race and ethnicity, gender, education, and family income, being young (relative to being a senior) is a large and statistically significant predictor of facing voting costs. I also find that young people are the least-informed of any age group about their state's policies on same-day registration, early voting, mail voting, and policies around updating voter registration.

Considering the recent rise in youth-targeted voter suppression, I present data from a randomized controlled survey experiment, which finds that informing young people about efforts to limit their ability to vote does not significantly increase their voting intentions. Notably, it does among youth with a strong sense of age identity—but most young people do not identify especially strongly with their age group. Previously documented backlashes to voter suppression may have been larger because they targeted more cohesive or "important" identity groups. Unless young people begin seeing their age as a more important aspect of their identity, we should not expect youth-targeted suppression to spark the same countermobilization.

For Louis, Marie, and Little Peach

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The phrase “it takes a village” is typically used in the context of child-rearing, but in my experience, it holds just as true for dissertation-writing. I’m fortunate to have been supported by my own village of teachers, friends, and family these last few years.

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CHAPTER 1: THE AGE-TURNOUT GAP

One of the most consistent findings in political science is that young Americans do not vote at high rates. More concerning than their absolute turnout levels is the discrepancy between voting rates *across* age groups: young Americans consistently vote less often than older Americans, particularly seniors (e.g., Miller and Shanks 1996; Rosenstone and Hansen 1993; Wolfinger and Rosenstone 1980). In the 2016 presidential election, only 46 percent of eligible young adults between the ages of 18 to 29 turned out to vote, whereas close to 71 percent of citizens 65 years and older voted—a gap of nearly 25 percentage points (File 2017). Youth turnout was higher in 2020, with approximately 50 percent of young people voting (CIRCLE 2021); however, exit polls suggest that seniors saw an even *larger* turnout boost, exacerbating the participation gap (Noah 2020). Even in the historically high-turnout midterm election of 2018, youth turnout topped out at 35.6 percent, compared to 66 percent for senior voters (Misra 2019).

This turnout disparity leads the policy priorities and preferences of young adults to be underrepresented in American politics. The characteristics and preferences of the electorate shape both who gets elected and which policies they prioritize and implement (Anzia 2014, Bertocchi et al. 2017, Fumagalli and Narciso 2012, Griffin and Newman 2005, 2013, Lijphart 1997). Despite a growing recognition that politicians are not always responsive to changing public preferences (Achen and Bartels 2016, Lenz 2012), political leaders do seem to adopt popular stances "on any issue where the public's views are clear cut" and to adjust their votes in anticipation of public sentiment (Schickler 2018, p. 124).

Young Americans tend to have different candidate preferences than their elders; in an August 2019 poll, a plurality of young people (24%) listed Bernie Sanders as their top choice for president in 2020, while 41 percent of older Americans strongly preferred frontrunner Joe Biden (Pew Research Center 2019c). They are more liberal than seniors on a range of issues, including racial equality, immigration, LGBT rights, climate change, gun policy, and marijuana (Doherty 2018). They also prioritize issues differently: young voters care more about the environment and the treatment of LGBT individuals and racial and ethnic minorities, whereas older voters care disproportionately about Supreme Court appointments, Social Security, terrorism, foreign policy, and trade (Doherty et al. 2016). Young people can also be important conduits to immigrant constituencies (CIRCLE 2018) whose vote choices are often distinct from those of the broader electorate (e.g., Gimpel 2010, Lopez et al. 2018, Preuhs 2019).

When young people fail to turn out, elected leaders have little cause to prioritize young people's issues and vote in line with their preferences. The problem is not simply that "kids these days" vote at low rates, but that age-based inequality in political participation has serious repercussions for whose voices are heard and taken seriously in local, state, and federal halls of power.

Today's age-turnout gap also poses problems for the *future* representativeness of American democracy. It is not guaranteed that young people will eventually embrace voting as they approach middle age. Low participation begets further low participation. As a consequence of not voting, young people fail to enact youth-targeted programs that might motivate them to turn out in future elections. By contrast, older Americans have secured policy victories such as Social Security that they then seek to protect in subsequent elections, helping to sustain high participation rates (Campbell 2003; Weir et al 1988, p. 25–26). If young people come to believe that the political system is unresponsive to people like them, this may further depress turnout

rates; given young people's already-high levels of political distrust (Citrin and Stoker 2018) and low satisfaction with regime performance (Norris 2011), this possibility is particularly concerning. Because voting—and, by extension, *not* voting—is habitual and persistent (Coppock and Green 2016, Fujiwara et al. 2016, Gerber et al. 2003, Meredith 2009), these trends could persist as today's young people grow older. Identifying interventions to establish patterns of voting early in life, then, is critical to ensuring that individuals turn out not just in their younger years but also as they age.

While we have become accustomed to young people not voting, there are good reasons to expect youth to vote in high numbers. Perhaps most obviously, their financial and physical well-being is inextricably linked to policy outcomes: Social Security risks insolvency before they can reap the benefits of their paycheck contributions, student loan debt is skyrocketing, global warming promises to unsettle the ecosystem young people inherit from older generations, and school shootings threaten college students' personal safety. Young people also participate in other forms of political activity, such as protest, social media engagement, and volunteerism, at high levels (Norris 2011, Strama 1998, Strama 2003). Some see these high levels of non-electoral political participation and civic engagement and reasons to be more sanguine about low youth turnout (e.g., Dalton 2009, Zukin et al. 2006), but they are no substitute for voting. Ultimately, politicians do not get protested, tweeted, or volunteered out of office.[1]

Perhaps the most commonly offered explanation for low youth turnout is that young people are simply uninterested in politics (e.g., Wattenberg 2015). But the vast majority of young Americans believe that voting is important (Hartig 2018) and intend to vote in elections (Holbein and Hillygus 2020). Yet young people follow through on their voting intentions at far lower rates than older adults, particularly senior citizens (Quintelier and Blais 2015).

To summarize: young people risk suffering unfavorable political outcomes by refraining from voting, are willing and capable when it comes to other forms of political participation, and broadly *want* to vote. Why, then, do they still turn out at far lower rates than older Americans—and what, if anything, can be done to address this disparity?

Theories of Voting

To begin understanding why young people do not vote at high rates, it is instructive to first examine why, in general, people choose to vote. Early socio-psychological models generalized that individuals whose psychological traits and life experiences led them to feel efficacious and care about politics were more likely to participate in elections, while institutional or practical barriers held some individuals back (Campbell et al. 1960; also see discussion in Aytaç and Stokes 2019, ch. 2). In the 1950s, rational choice theory posited a more straightforward "calculus of voting," in which an individual's likelihood of turning out hinges on a rational cost-benefit analysis: she is more likely to vote as the benefits she gains from voting go up and/or the costs she incurs from voting go down (Downs 1957).

While this fundamental premise—that people vote when the benefits of doing so outweigh the costs—is uncontroversial, the specific "calculus of voting" developed by Downs has been widely criticized for failing to predict voter turnout. In this calculus, benefits (B) are strictly defined as the differential utility gained from one's preferred candidate winning over their competition. This term is modified by p, the perceived probability that their vote would actually make a difference in which candidate got elected. If pB fails to outweigh the costs a

[1] There is some evidence that protest can directly affect policy outcomes (Madestam et al. 2013), but politicians still have little electoral incentive to care about protesters who are unlikely to vote.

voter incurs from voting (*C*), then the individual will abstain from casting a ballot. Taken to its logical conclusion, this calculus predicts that a rational voter, recognizing both how miniscule an effect his vote will have on election outcomes and also that voting is at least somewhat costly, will optimize his utility by choosing to abstain from voting altogether. Yet clearly, people do vote. This so-called "paradox of voting" has plagued rational choice for the past half-century.

In response to this paradox, scholars began searching for other benefits that individuals might get from voting. Most prominently, Riker and Ordeshook (1968) add in a *D* term encompassing a range of "positive satisfactions" one might gain from voting: the satisfaction of "compliance with the ethic of voting," of "affirming allegiance to the political system," of "affirming a partisan preference," of "deciding, going to the polls, etc.," and of "affirming one's efficacy in the political system" (28). Other scholars posit that people may get "relational goods" from voting (Uhlaner 1989), perhaps turning out in order to feel connected with and accepted by their community (Abrams et al. 2010, Sinclair 2012). Schuessler (2000) argues that people may vote not in order to bring about specific electoral outcomes at all but, rather, "to express who they are by attaching themselves to such outcomes" (p. 5). Ferejohn and Fiorina (1974) turn the calculus of voting on its head, arguing that voters are not utility maximizers so much as regret minimizers, voting in order to forestall the extreme regret they would feel were the election to end in a tie, where their vote could have swung the outcome in favor of their preferred candidate. Importantly, all of these scholars focus their attention on the benefits individuals may gain from voting (or the regret they may feel from abstaining). The costs incurred from voting are generally presumed constant.

The resource theory of political participation offers a rare focus on the *costs* component of the voting calculus. Rather than asking why individuals decide to vote, Brady, Verba, and Schlozman (1995) outline three plausible explanations for why individuals might *not* participate in politics—"because they can't, because they don't want to, or because nobody asked" (p. 271)—and focus on the first explanation ("they can't), as it is normatively the most problematic. They hypothesize that some people cannot participate because they lack the resources to overcome the costs of political engagement. Three resources, in particular, should help voters overcome these costs: money, time, and civic skills. In the specific domain of voting (as opposed to other forms of participation), Brady et al. find that having more time is a significant predictor of turnout, while money and civic skills matter far less. Notably, resource theory does not attempt to enumerate the actual costs of voting, nor does it examine the distribution of costs across subgroups. Indeed, the general assumption among resource theory scholars is that voting costs, especially relative to the costs involved in other forms of political activity, are relatively low (Verba et al. 1995).

New work by Aytaç and Stokes (2019) introduces a new element to the voting calculus: the cost of abstention. In this calculus, an individual's probability of voting is a function of the benefits she perceives herself gaining from voting, the costs she perceives herself suffering from voting, *plus* the costs she anticipates incurring if she *abstains* from casting a ballot.

Why Do Young People Vote at Lower Rates?

Research into the puzzle of the age-turnout gap has generally followed the trajectory of the broader voter turnout literature. Scholars have identified myriad features of young people that might lead them to perceive lower benefits of voting (or, per Ferejohn and Fiorina's argument, lower costs of abstaining) than older people, while far fewer studies have delved into the (disproportionate) costs young people might face during the voting process.

In the socialization literature, it is generally assumed that newly eligible 18-year-olds are politically inactive. Youth must be socialized *into* political activity through the development of their "civic orientations": political efficacy, political knowledge, and political interest (Beck and Jennings 1982). Partisan affiliations are seen as an especially important motivating force that develops over time, hence contributing to the age gap; "partisanship is what bonds voters to campaigns, and the sense of party identification is more firmly entrenched among older Americans who have had multiple opportunities to cast partisan votes" (Iyengar and Jackman 2003, p. 2).[2]

To explain why young people might begin from a place of political inactivity, researchers often focus on the particular characteristics associated with young adulthood that might result in low turnout. Perhaps most importantly, young people are preoccupied with a range of important events and changes happening in their lives, such as "obtaining an education, finding a mate, and establishing a career" (Strate et al. 1989, p. 443). These preoccupations prevent them from devoting time and energy to political matters (Kinder 2006), ultimately depressing their turnout levels relative to older age groups.

Research indicates that life cycles do matter for voter turnout. In one highly cited study, Strate, Parrish, Elder, and Ford (1989) find that "age-related processes," measured using indicators of "civic competence, community attachment, church attendance, the strength of political party attachments, government responsiveness, and family income," can explain about half of the turnout gap between young and older Americans. Yet it remains unclear *why* life cycles are so strongly linked to voter turnout. One popular theory is that as people grow older, they reach certain maturation milestones—such as starting a full-time job, getting married, buying a home, and having kids—that bring them into contact with civic groups, which in turn foster their political skills and pressure them into civic activity (Smets 2016, Kinder 2006). Despite their low voting levels, however, young people protest and volunteer at rates comparable to those of other age groups, leading one to wonder why reaching maturation milestones should be essential for voting but not for other types of civic activity. Moreover, the age gap in turnout is much larger in the United States than in other Western countries (Holbein and Hillygus 2020, ch. 1), whose citizens tend to follow similar life trajectories (Goerres 2007, p. 119).[3] There may be something particular about the way the U.S. structures its electoral processes that magnifies the impact of life cycles on turnout.

Some argue that recent generations are particularly susceptible to low turnout, perhaps because they have not been properly socialized into the voting process (Franklin et al. 2004), have grown up without being called to action by a global crisis such as World War II (Patterson 2002), or are taking longer to reach critical maturation milestones (Smets 2016). Notably, none of these explanations sheds light on the persistent age gap across decades.

Others have highlighted the importance of the aging process itself. Goerres (2007), for instance, presents a scenario in which young people begin with a less than 50% chance of voting, and so many individuals do not vote. However, because voting is habitual, those few people who do vote become regular voters. This process continues each election year, with past voters

[2] Recent polling data challenges this argument; when asked to define their party preference, the vast majority of young Americans see themselves as Democrats or Republicans, or as leaning toward one of the two major parties, rather than as strict independents with no party preference (Maniam and Smith 2017)

[3] From Goerres: "Similarities include a strong emphasis on chronological age as a social marker, a linear (rather than cyclical) perception of the life course from birth to death and an individualistic outlook that permits very different social roles along the life course (Albert and Cattell 1994, 64–65; Wilson 2000, 27)."

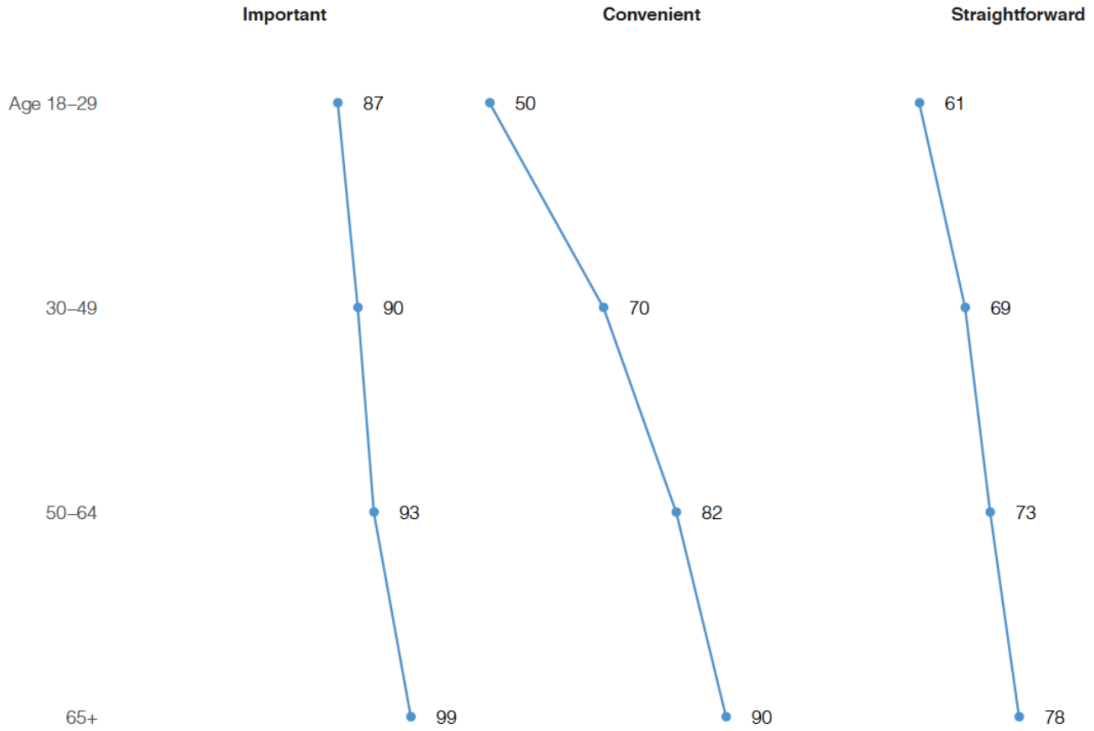
turning out again, and past non-voters maintaining a less-than-fifty-percent (but greater than zero) chance of voting. After enough elections, most people eventually cast a ballot and join the ranks of habitual voters. This argument, while interesting, portrays non-voters as automatons, equally likely to cast a ballot each time an election arises, regardless of the benefits they perceive from voting and the costs they anticipate incurring. We are left searching for an explanation that can be reconciled with existing theories of voter turnout.

Perhaps the most popular explanation for the age-turnout gap is that young people are not especially interested in politics, an idea with roots in early sociological research. For decades, scholars studying political behavior have found young people to be less politically interested than their older counterparts (e.g., Converse and Niemi 1971)—though because this research often used *turnout* as a proxy for political interest (e.g., Glenn and Grimes 1968), it risks offering a tautological explanation for young people's low voting rates.

Recent research calls into question whether today's young people do, indeed, lack interest in politics or the desire to participate. Holbein and Hillygus analyze data from the American National Election Survey and find that "most young people are politically interested and motivated" (p. 10), leading them to conclude that "the narrative of disinterested young citizens... simply is not empirically true." While young and old Americans do differ in their levels of political interest, the age gap is about 10 percentage points, compared to a 30-50 percentage-point gap in participation. In other words, differential interest may explain part of the turnout gap, but not the whole thing. The authors then point to several other data sources, including the General Social Survey, UCLA's annual survey of first-year college students, and a Pew Research Center analysis, all of which support the conclusion that young adults care about politics (p. 11).

Yet when it comes to perceptions of the electoral process, the similarities across age groups end. A nationally representative Pew Research Center panel survey in 2018, conducted online on a randomly selected sample of more than 10,000 U.S. adults in late September and early October of 2018, found that young people are more likely than older adults to struggle with the voting process. When presented with several potential descriptors of voting, young people were about 12 percentage points less likely than seniors to agree that voting is "important," but they were a full 40 percentage points less likely to agree that voting is convenient and 16 percentage points less likely to say that voting is straightforward (Figure 1).

Figure 1: Young Adults Less Likely to Find Voting Convenient or Straightforward



Source: Author's analysis of Pew Research Center American Trends Panel Wave 38 [Data file]. Retrieved from https://www.pewresearch.org/politics/dataset/american-trends-panel-wave-38/.

The Pew survey also finds that young people are more likely than older Americans to personally expect voting to be difficult and to say election rules make it too hard to register and vote. These data suggest that *costs*, not just benefits, may be a critical factor in the turnout disparity between young and old Americans.

The Costs of Voting

Surprisingly little scholarly attention has been paid to the costs of voting (but see Blais et al. 2019, discussed in more detail in the next chapter). Many researchers have assumed that voting is fairly easy, "no more costly than many other kinds of intermittent activities [people] undertake" (Niemi 1976, p. 116; see also Ferejohn and Fiorina 1974, Riker and Ordeshook 1968). Others have questioned this assumption of low costs. Aldrich (1993), after noting that costs were often omitted from models entirely or measured only indirectly, suggested that "if good measures of costs were available in surveys they would also show strong effects" (1993, 257)." Indeed, Downs' original economic theory of voting (1957) assumed that the information costs of voting—the effort expended in learning about candidates and the electoral process more broadly—were high. As Sanders (1980) laments, the modern notion of low voting costs "bears little or no relationship to the costs of information gathering and processing emphasized by Downs; nor does it acknowledge the physical costs of registering and getting to the polls on a workday-costs measured in time, money, and opportunities foregone" (p. 856). Moreover, even if the absolute costs of voting are quite low, they can still have a large impact on a person's likelihood of turnout if their perceived benefits of voting are also small (Niemi 1976, p. 117).

A small number of studies have explored heterogeneity in the voting costs borne by different socio-economic and demographic groups. Once again, the early voting turnout literature was better on this front; several prominent scholars in the 1970s posited differential costs as an explanation for why low-income individuals turned out at lower rates than their richer counterparts (Frey 1971, Niemi 1976, Tollison and Willett 1973). Other early studies noted that varying costs of travel might impact someone's likelihood of voting (Downs 1957, Niemi 1976, Sanders 1980). In the years since the voting franchise was expanded to include people of color, costs have often been higher for non-white individuals, particularly African Americans, whose turnout increased dramatically after a series of barriers to black registration and voting were dismantled by the Voting Rights Act (Rosenstone and Hansen 1993).

More recent research has found that residents in minority-majority precincts face longer wait times on Election Day (Pettigrew 2017; Stewart 2012). Brady and McNulty (2011) highlight how individuals living farther from their polling place faced higher costs of voting in person,[4] and Haspel and Knotts (2005) find that small differences in how far individuals live from the polls have an outsize impact on their likelihood of voting. In addition, multiple studies find that higher residential mobility increases the effort required to remain registered to vote (Ansolabehere et al. 2012, Highton 2000, Squire et al. 1987)—an especially important finding, given young people's disproportionately high mobility rates (US Census Bureau 2016). Yet no study has specifically examined how a range of voting costs might vary by age, or how variation in those costs predicts registration and turnout rates. If young people face higher voting costs than older age groups, this could help explain why young people are far less likely to vote.

Can Electoral Reforms Increase Youth Turnout?

Grassroots advocates focused on closing the age-turnout gap often begin from this premise, arguing that young people face meaningful costs to participation, and policies that reduce these costs will result in higher turnout. They advocate for election law reforms that they expect to increase young people's voting rates, such as automatically opting people into the voter registration system, letting people register and vote on the same day, and making Election Day a national holiday (e.g., Wong et al. 2018, EveryAction 2018, Bains 2019).

Yet some scholars examining the effects of these and other voting reforms have been less optimistic about their turnout-boosting potential (e.g., Berinsky 2005, Fitzgerald 2005, Highton 2004, Nagler 1991, Traugott 2004, Yoder et al. 2021). Adam Berinsky has been perhaps the most outspoken critic of the potential for cost-reducing reforms to improve the representativeness of the electorate. In an influential 2005 paper on "the perverse consequences of electoral reform in the United States," Berinsky argues that, contrary to the hopes of reform advocates—and the expectations of many earlier political scientists—voting reforms do not make the electorate more representative. In fact, they do the opposite: "reforms designed to make voting 'easier' magnify the existing socioeconomic biases in the composition of the electorate" (p. 472). He holds that it is not enough to ask *whether* a reform makes the electorate bigger. We must also ask *how* it expands the electorate: by mobilizing new voters who had previously not participated in elections, or by retaining transient voters who had voted inconsistently. This is because new voters are systematically different from transient voters: they are typically lower income, lower

[4] Interestingly, when people's polling places exogenously shifted to a more distant location, older voters were more likely to switch to absentee voting, while younger voters were more likely to abstain from voting entirely (p. 116). The authors theorize that this difference is due to learning about the electoral system: "older people (whom we can presume have learned about the voting system) substitute absentee voting for polling place voting" (p. 126).

education, and underrepresented in the political system. If a reform expands the electorate not by bringing in new voters but, rather, by making occasional voters more consistent in casting a ballot, this could actually exacerbate turnout inequalities, not fix them. Reviewing several studies on the consequences of four voting reforms—all-mail voting, early voting, absentee voting, and internet voting—Berinsky concludes that this is, in fact, the case: "Rather than stimulating the unengaged, who are relatively deficient in political and economic resources, these reforms retain engaged voters… Thus, although electoral reforms may increase turnout, they do so by ensuring that politically engaged voters continue to come to polls election after election" (p. 473).

Importantly, Berinsky does not appear to include young people in his definition of "underrepresented groups." For instance, he highlights one study's finding that Election-Day registration (EDR), which allows individuals to register immediately before voting on Election Day, does not improve representativeness—yet in a footnote, he acknowledges that EDR *did* improve equality of representation by both age and mobility status (p. 489).

Moreover, Berinsky focuses on a set of reforms that were never especially promising for improving representation. All four reforms are only helpful for registered voters—that is, individuals who had enough information and other resources to navigate the first big step in the voting process. Absentee voting and early voting are "opt-in" reforms, where voters must learn about and decide to take advantage of them; this makes them more likely to benefit higher-information individuals who are aware of the voting policies in their state. The reforms are also future-oriented: they help prospective voters make an alternative voting plan in advance of Election Day. As such, they are well-suited to people who pay early attention to elections and can envision their Election-Day schedules well in advance—a description that does not easily map onto underrepresented voting groups, especially young people. The internet voting research Berinsky examines is from Arizona's Democratic primary in 2000, a year in which voting access was dramatically higher in wealthy, white, and well-educated families (Pew Research Center 2015a). It should come as no surprise that these groups were therefore more likely to cast online ballots; indeed, the Arizona Democratic Party was eventually sued for discriminating *against* low-income and minority voters by encouraging people to vote online (Kantor 2000). Again, the reform's effect on the age-turnout gap is not discussed. Perhaps the most concerning claims about the perverse effects of voting reforms concern all-mail voting—yet recent research suggests this policy change, when implemented with an eye toward improving turnout equity, could disproportionately *benefit* younger people (Bonica et al. 2020).

A 2016 essay by Berinsky takes the argument against voting reforms even further, arguing that laws aimed at reducing registration and voting costs are unlikely to boost turnout at all. While the relationship between higher costs and lower turnout may have held decades ago, he argues, it no longer does—largely because registration and voting have ceased being especially costly activities. "The changes over the last 40 years have greatly reduced the direct costs of registration and voting," Berinsky writes. Specifically, laws like the 1993 National Voter Registration Act (NVRA), popularly known as the "Motor-Voter Act" for allowing individuals to register to vote at their local Department of Motor Vehicles or via mail, have brought registration costs down dramatically. With the process already this easy, reducing costs any further will do little to increase voting rates.

Berinsky cites several studies that support this claim. Martinez and Hill (1999) find that the NVRA did not significantly increase overall state-level turnout. In a subsequent study of the same law, Brown and Wedeking (2006) determine that the NVRA did succeed at increasing *registration* rates, but a disproportionate number of these new registrants did not go on to vote—

suggesting that registration costs were not the main barrier to their electoral participation. Kousser and Mullin (2007) find that the quasi-random assignment of some California voters to vote by mail can decrease turnout in general elections. And Burden et al. (2014) show that spreading out voting opportunities over an early-voting window does not drive higher aggregate turnout. Indeed, they find that early voting may *reduce* voting rates, because pro-voting information and social pressure campaigns are no longer centered around a single day of action, Election Day, and are instead spread thinly over several days or weeks. Taking these findings together, Berinsky concludes that "the balance of evidence is clear: lowering the direct costs of voting does little if anything to increase turnout."

However, this once again fails to account for a number of studies that *do* find significant turnout effects of cost-reducing policies (see, e.g,. the thorough review in Larocca and Klemanski 2011). For instance, all-mail voting has been found multiple times over to increase turnout (Bonica et al. 2020, Gronke et al. 2007, Karp and Banducci 2000). Election Day registration also consistently drives up turnout (Brians and Grofman 2001, Hanmer 2009, Knack 2001, Leighley and Nagler 2009). More recent research finds that pre-registration laws, which allow individuals younger than 18 to pre-register to vote, significantly increase voter turnout by up to 5 percentage points (Holbein and Hillygus 2017), and that internet voting can boost voting rates by more than 3 percentage points (Goodman and Stokes 2018).

To be sure, not all electoral changes regularly result in more voting. Absentee voting reforms are not especially promising: permanent no-excuse absentee voting has a positive but insignificant impact on turnout (Gronke et al. 2007), while non-permanent no-excuse absentee has been found to have either a positive (Wolfinger and Hoffman 2001, Leighley and Nagler 2009) or non-existent (Fitzgerald 2005, Gronke et al. 2007, Yoder et al. 2021) effect on turnout, depending on the study. Research on early voting consistently finds no significant effect on turnout, with the reform's benefits primarily reserved for those who already vote (Fitzgerald 2005, Gronke 2004).[5] But this is precisely why it is risky to paint voting reforms with too broad a brush: depending on the cost they aim to reduce, and who currently bears the burden of that cost, they may boost turnout, have little effect, or even shrink voting rates—and these effects can vary across subgroups.

Berinsky also sidesteps the rapid rise of another type of election law not yet discussed: the suppressive kind, intended to *restrict* voter turnout (Ross and Spencer 2019, Wang 2012). The most commonly investigated form of modern voter suppression is the requirement that individuals present personal identification when attempting to vote. The most stringent states require individuals to present a valid photo ID—often one issued by a government agency—in order to vote. While a robust debate has broken out in the literature on the turnout effects of strict photo ID laws (Grimmer et al. 2018, Hajnal et al. 2017), there has emerged some consensus that, at a minimum, these policies disproportionately burden racial and ethnic minority groups (Hajnal et al. 2018). If suppressive laws threaten to shrink turnout, it stands to reason that there is some potential for cost-reducing laws to *raise* turnout levels.

[5] Notably, Burden et al.'s (2014) argument is not that early voting succeeds in reducing costs yet remains ineffective, but rather that early voting inadvertently increases *indirect* costs for voters, thereby driving down voting rates. In other words, election reforms might still work at getting people to vote—but only if they actually do, in fact, make voting easier for those people.

A New Framework for Thinking About Election Reforms

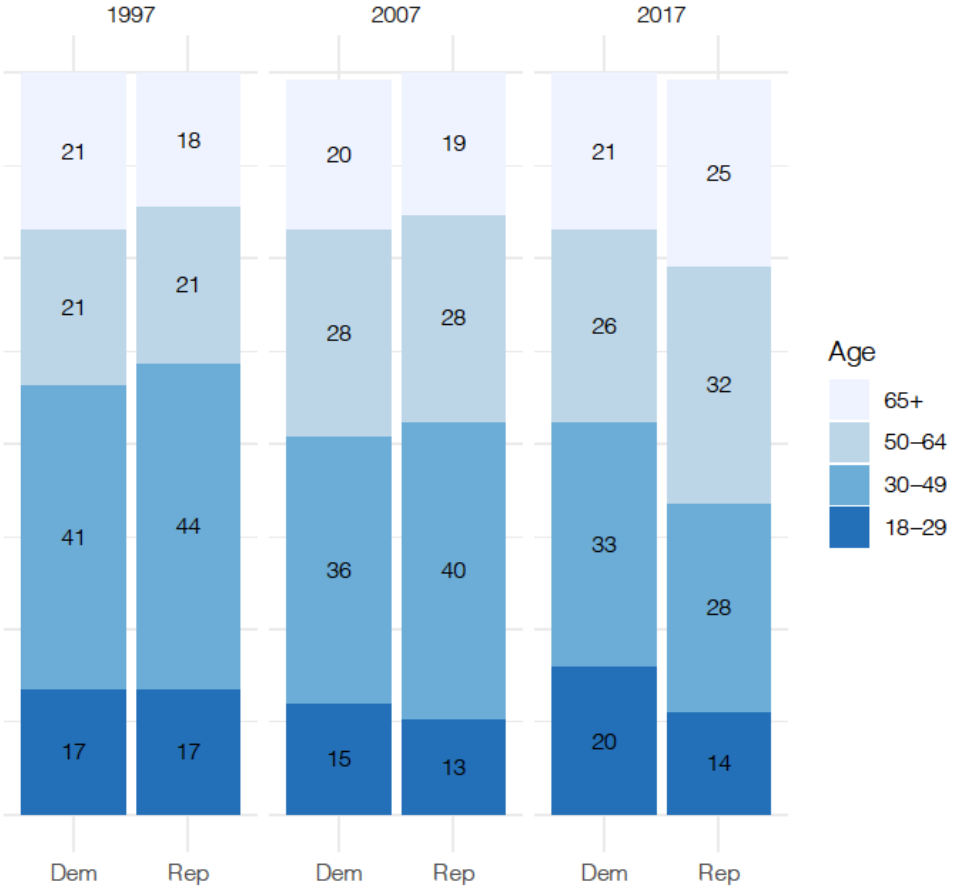
All told, a thorough read of the election law literature shows that certain reforms intended to make voting easier do meaningfully increase voter turnout for some groups, while others do not—and still others may have perverse, negative consequences. More complicated still, individual reforms can be impactful for some groups while leaving others relatively unaffected. What matters is how the costs that the reform alleviates are distributed across groups (or, in the case of voter suppression, how the costs the reform *imposes* are distributed).

Consider a population consisting of two equally sized groups, Group A and Group B. Group A faces a large voting cost, C, while Group B does not. Now imagine that a voting reform eliminates C. Overall turnout will go up, but only due to changes in Group A's voting calculus. Group B will be unaffected, and the aggregate effect of the reform will appear more modest than the effect for Group A alone.

This dissertation presents a new approach for thinking about the potential of a given voting law to shrink (or, conversely, widen) the age-turnout gap—one that attempts to reconcile the ambitious claims of pro-reform advocates and the weaker-than-expected empirical results found by election law scholars. It begins with acknowledging that different voting reforms are intended to ease (or impose) different voting costs, and that not all voting costs are equally borne by voters; some age groups are disproportionately burdened. Moreover, voters differ in how easily they can *take advantage* of each reform; in particular, opt-in reforms are harder for low-information voters to learn about and use. To properly anticipate the effect of a voting reform on youth turnout, then, we must identify *which voting costs* are impacted by the reform—noting Burden et al.'s (2014) warning that both direct and indirect costs should be taken into account whenever possible—and also identify *which age groups* are most likely to bear those costs. We should also empirically examine the reform's effect on turnout *for each age group*, in order to uncover any potentially heterogeneous treatment effects by age. In this way, we can develop a more nuanced understanding of how election laws affect age-based turnout inequality.

These heterogeneous effects by age are important in their own right. But understanding them has become even more important in recent years, as the two major parties have sorted by age. In the late 1990s, the Republican and Democratic parties contained a similar mix of age groups. Registered voters between 18 and 29 were equally represented in both parties, and seniors were slightly overrepresented on the Democratic side. But as Figure 2 depicts, over the past twenty years, the Republican Party has aged. Today, of registered voters who identify as either Democratic or Democratic-leaning, 20 percent are 18-29 years old, and another 33 percent are between 30 and 49, with a total of 53 percent under the age of 50. By contrast, among registered voters who identify with or lean toward the Republican Party, just 42 percent are under fifty: 14 percent are 18-29 years old, and 28 percent are between 30 and 49.

Figure 2: Age Group Representation In Major Political Parties



Source: Nationally representative Pew Research Center poll of registered voters (Doherty et al. 2018).

As age groups sort into parties, we should expect politicians to enact election reforms with an eye toward increasing turnout among "their" voters—and reducing turnout on the other side. Looking only at the aggregate turnout effects of these policies may mask their substantively important effects on subgroups. By looking for heterogeneous effects by subgroup, we maximize our chances of identifying the very effects that lawmakers hope to engender with these policy changes.

Dissertation Outline

In the following chapters, I employ this approach to understand how several election reforms affect the age-turnout gap. In Chapter 2, I examine the turnout effects of three reforms with particular promise for boosting youth engagement: same-day registration, automatic voter registration, and internet voting. I first present new empirical data from quasi-experimental research conducted in partnership with Jake Grumbach at the University of Washington evaluating the age-specific turnout effects of two policies that reduce the costs of voter registration: same-day registration, which lets people register and vote on the same day, and automatic voter registration, which automatically registers individuals who interface with participating government agencies. I show that these reforms disproportionately benefit young people, boosting their turnout relative to other groups. Meanwhile, early voting—a reform that reduces costs primarily borne by older Americans—does not appear to increase youth turnout. I

also summarize the results of several existing studies on the effect of internet voting, all of which find that being able to vote online has an especially large turnout effect for young people.

In Chapter 3, I delve into the theory behind why certain reforms might disproportionately benefit young people. Specifically, I explore the question of how voting costs vary by age group, shedding light on the specific costs that—if mitigated by new voting policies—hold the greatest promise of shrinking the age-turnout gap. I begin by reviewing past research on voting costs and then present the results of a nationally representative survey of young and older Americans, conducted in partnership with the Berkeley Institute for Young Americans. I show that voting costs are unequally distributed by age, and that higher costs predict lower registration and turnout rates. Compared to seniors, today's youth are less informed about the voting process and how to research candidates and issues. They struggle more to find the time to vote, to plan ahead to vote, and to balance voting with other life tasks. Youth face greater transportation issues when voting, face larger tradeoffs between voting and earning money, and have greater difficulty with the parts of voting that cannot be done online. They also disproportionately think mail voting is a hassle, and they are less likely to own the documentation they need to register and vote. When asked directly, youth are significantly more likely than older Americans to say that registration and voting are difficult. After adjusting for race-ethnicity, gender, education, and family income, being young (relative to being a senior) is a large and statistically significant predictor of voting costs. I also find that young people are the least-informed of any age group about their state's policies on same-day registration, early voting, mail voting, and whether individuals must update their voter registration after moving.

In Chapter 4, I turn my focus to youth voter suppression—an effort to shrink the youth franchise even further via imposing costly registration and voting burdens that disproportionately affect young people. Those who hope to close the age-turnout gap must reckon with this increasingly concerning phenomenon. Not only is youth voter suppression becoming more common and potentially demobilizing young people—particularly of color (Rogowski and Cohen 2015)—but it is also increasingly difficult to stop, as it is typically perpetrated by Republican-controlled state legislatures with little electoral incentive to reverse course. Absent well-enforced federal action limiting suppression, these states may see widening age-turnout gaps over time. However, research on the impacts of *race-based* voter suppression hints at a path forward for combatting the harmful consequences of these policies: framing restrictive voter laws as attempts to reduce Black voters' turnout can have the opposite effect, inducing psychological reactance and anger that can motivate individuals to participate at higher rates. Drawing on these findings, I explore whether informing young people about youth-targeted voter suppression can mitigate the negative effects of these policies and actually *increase* youth political participation.

The results from a pilot study are not especially encouraging. I find that informing young people about youth suppression increases their average levels of anger and psychological reactance and indirectly boosts their intentions to vote in an upcoming election. However, after adjusting for anger and reactance, the direct effect of this information on intentions to vote is *negative*, leading to a statistically insignificant total effect. (The effect is still larger in magnitude for young people than for older individuals—an argument for replicating the study with a larger sample.) Treatment effects are especially large for young people who strongly identify with their age group, suggesting that youth voter suppression could spark backlash in the future if young people developed a greater sense of age-group identity, similar to the identities held by racial and partisan groups.

I conclude the dissertation with a discussion of my findings and future research opportunities. Drawing on original survey data, I highlight potential election reforms that are especially promising for alleviating young people's voting costs and reducing turnout inequalities across age groups. I discuss the challenge of tradeoffs between participation and security, paying special attention to the rise of internet voting, which initial research suggests would make turnout more equal across age groups but also vastly increase security risks. Finally, I highlight the need for researchers and advocates to identify other interventions—policy, communications, or otherwise—that will combat the concerning rise of youth-targeted voter suppression.

A caveat to the reader: this dissertation is not, and does not attempt to be, a thorough account of why young people vote at low rates, or of what changes are necessary to fully close the age-turnout gap. The broad extant literature on low youth turnout makes abundantly clear that there is no single reason why young Americans vote less than older Americans, and any study that attempts to argue as much is unlikely to succeed. Rather, my goal is to understand whether unequally distributed voting costs play a meaningful role in the age-turnout gap, and how addressing these costs—via both institutional reforms and, in the case of voter suppression, public information campaigns—can help shrink disparities in voting rates across age groups.

Dissertation Contributions

My dissertation is complementary to, but distinct from, new work by Holbein and Hillygus (2020). We depart from the same place: a concern that high voting costs prevent many young people from participating in elections. Holbein and Hillygus convincingly argue that young people are interested in politics and motivated to vote, but that they are frequently unable to overcome obstacles standing in their way of voting. They also posit (but do not empirically show) that voting costs are especially high for young people, particularly college students. Holbein and Hillygus champion teaching young people the non-cognitive skills—self-regulation and sociability—necessary to overcome voting costs, perhaps through an applied learning-oriented civics curriculum. In contrast, my dissertation focuses less on how to help young people surmount barriers to voting, and more on how to reduce these barriers in the first place via policy change.[6]

For their part, Holbein and Hillygus do argue in a later chapter that an additional way to boost young people's voting rates, beyond improving their non-cognitive skills, is to lower their voting costs. The chapter provides evidence that certain cost-reducing reforms such as same-day registration and pre-registration increase youth turnout, whereas other reforms such as no-fault absentee voting have little to no effect. While my analysis of the effect of voting reforms on youth turnout reaches the same conclusion, it differs in one key way. Whereas Holbein and Hillygus focus on the effect of reforms on youth voting, I compare the turnout effects of voting reforms across age groups, allowing me to draw conclusions about the effect on the composition of the broader electorate.

My dissertation provides a valuable update to the resource model of political participation originally put forth by Brady et al. (1995). According to this model, there are three key resources required to overcome costs of political participation: time, money, and civic skills. These resources are distributed unevenly across socioeconomic and demographic groups, resulting in lower political involvement by resource-poor individuals. However, the model does not account

[6] While Holbein and Hillygus explore this solution as well, they do so secondarily. The bulk of their book is devoted to building non-cognitive skills in young Americans, especially those not yet of voting age.

for an unequal distribution of costs themselves. There are two ways in which voting can be more costly for Person A than for Person B: 1) if Person A has fewer resources necessary to complete the many steps of the voting process, or 2) if Person B must expend more resources—more time, more money, or more mental energy—to complete these tasks. A voting system that requires re-registering and locating a new polling place after every change in residence is inherently more costly for frequent movers, as they must expend more resources to stay registered, just as a system that requires mailing in a ballot is more costly for voters who grew up in the digital era, as they must expend more mental energy—and, often, more time and money—to understand the postal system, procure stamps, and locate a dropbox. The resource model should account for these unequal costs, recognizing that some groups will need more resources than others just to turn out at equal rates.

This dissertation also brings a new focus on how voters *perceive* voting costs. As Blais et al. articulate in a recent paper, the voter turnout literature is "remarkably silent on the role of factors that hold citizens from voting" (2019, p. 1). While scholars have explored whether "facilitating reforms" aimed at making voting easier (i.e., less costly) increase turnout, they have not sufficiently explored the extent to which voters perceive voting as costly in the first place. Blais et al. probe this question by surveying citizens in five countries (Canada, France, Germany, Spain, and Switzerland) with questions about the ease or difficulty of voting and then use this data to predict voter turnout. They find that perceived costs are predictive of voter turnout, but few citizens believe that voting is costly in the first place.

I take a different approach to measuring individual citizens' voting costs, recognizing that social desirability bias, insufficient effort in responding, and sheer habituation to voting costs can lead respondents to state that voting is easier for them than it is in reality. Further differentiating my research, I survey American citizens, who are not included in Blais et al.'s study. There is reason to suspect that Americans perceive greater voting costs than their Canadian and European counterparts, as U.S. voter turnout lags far behind that of other developed nations (DeSilver 2018). I again depart from Blais et al. by including registration costs in my conceptualization of voting costs. Not only is registration a key prerequisite to casting a ballot in most states, but it likely accounts for a disproportionately large portion of the cost of voting; as Wolfinger and Rosenstone originally argued, "Registration is usually more difficult than voting, often involving more obscure information and a longer journey at a less convenient time, to complete a more complicated procedure. Moreover, it must usually be done before interest in the campaign has reached its peak" (1980, p. 61).

Finally, my research places special emphasis on the need to study voter turnout at the level of population subgroups. Various groups have their own historical patterns of voting behavior; African Americans vote at high rates and fairly consistently for Democrats, older Americans vote frequently and more conservatively, and so forth. Groups also differ in their resources, lifestyles, and behaviors, all of which can affect how often they turn out and which candidates and causes they support. Politicians respond to these patterns by passing voting laws aimed at facilitating the turnout of the groups most likely to support them, or vice versa. Accordingly, there is every reason to expect that both our present voting system and future voting reforms would heterogeneously impact subgroups, and that measuring average effects will mask this important variation. These heterogeneous effects shape who constitutes the electorate, ultimately impacting which candidates win elections, which policies they prioritize, and how they vote on those policies.

This work has several policy implications. First, with limited agenda space, state and federal actors interested in boosting youth turnout must weigh the respective benefits of various electoral reforms (e.g., same-day registration, automatic voter registration, online registration, early voting, vote centers, mobile voting, etc.), all of which can be expected to impact young people differently. Moreover, campaigns and grassroots groups attempting to mobilize young voters must choose where to spend their resources: on getting young people registered, turning them out, or trying to change laws to structurally alter either of these processes. By clarifying the unique impact that various voting barriers have on young Americans, my dissertation will help policymakers think through which reforms are most likely to reduce voting costs and ultimately drive up youth turnout.

More broadly, my dissertation reframes the evergreen public conversation on voter turnout around how certain groups benefit from the structure of existing election laws, while other groups do not. While many scholars and public intellectuals readily acknowledge that election barriers have been erected to prevent people of color from voting, there is less recognition of the insidious (and perhaps unintentional) ways in which voting laws depress turnout among low-education, low-information, economically disadvantaged, highly mobile, and young Americans. To this end, my dissertation points toward reforms that will help not just youth, but any group that lacks the resources to easily register and vote under current electoral regimes.

This is especially important given recent economic, technological, and political changes in America. Financial security is an important prerequisite to voting (allowing one to feel comfortable taking time off work, to afford the cost of transportation to the polls, etc.), yet post-Great Recession, it is in short supply. Economic shifts toward part-time, flexible labor, without accompanying subsidies for childcare and enforced employee protections for missing work due to voting, leave Americans increasingly burdened by a cumbersome voting process. Meanwhile, technological shifts habituate potential voters to easy online action, while even the lowest-friction electoral systems still require voters to mail in their ballots. All these changes are occurring while public trust declines (Rainie and Perrin 2019) and local political news coverage disappears (Associated Press 2019), leaving people especially disconnected from government. In other words, for many Americans, the costs of voting are rising, while the perceived benefits of voting are shrinking. To the extent that democracy survives on public participation, this suggests that America's very system of government is on shaky ground.

CHAPTER 2: REGISTRATION REFORMS BOOST YOUTH TURNOUT

Can election reforms improve turnout among young people? Prior studies have thoroughly investigated the effect of specific voting reforms, such as vote by mail (Berinsky et al. 2001, Karp and Banducci 2000, Kousser and Mullin 2007, Southwell 2004, 2009, Southwell and Burchett 2000), absentee voting (Karp and Banducci 2001, Patterson and Caldeira 1985), early voting (Gronke et al. 2007, Richardson and Neeley 1996, Stein and García-Monet 1997), and "motor voter" laws (Franklin and Grier 1997, Knack 1995, 1999, Martinez and Hill 1999, Wolfinger and Hoffman 2001). Other research has investigated differences in voter participation across age groups (e.g., Bhatti et al. 2012, Wattenberg 2015). However, there has been less focus on how voting reforms may affect age groups differently.

In this chapter, I present data showing that same day registration (SDR) improves voter turnout among young people, and that automatic voter registration (AVR), a newer policy reform that is quickly gaining traction across states, is positively associated with higher youth voting rates. Both laws lower the costs of a major barrier to young potential voters: the registration process. Young people's life circumstances make traditional registration uniquely costly. They are more likely to change residential addresses, requiring them to update their registration. They less frequently use government offices that provide registration materials. They have not yet developed habits of voting and may not know where or how to register. SDR laws should make voting less costly for these young voters by combining registering and voting into a single act (Wolfinger et al. 2005), while AVR laws should essentially eliminate registration costs for individuals who interact with government agencies involved in the voter registration process.

The bulk of this chapter focuses on SDR, a reform that has been adopted by a large number of states over a broad period of time, allowing for robust estimation of causal effects on youth turnout. The data presented here around the impact of SDR laws was collected and analyzed in collaboration with Jake Grumbach, a professor of political science at the University of Washington. We find that SDR laws have a positive turnout effect for all age groups, but they disproportionately increase voting rates for young people. Next, I turn to a cross-sectional analysis of the impact of AVR laws. This research, again conducted with Jake Grumbach, finds that AVR is positively associated with turnout overall, but especially for young voters.

Same-Day Registration and Turnout by Age Group

Same-day registration (SDR) allows eligible individuals to register and cast their vote on the same day. Since its original implementation in Maine in 1973, SDR has been adopted by twenty additional states, plus the District of Columbia (Table 1). In nearly all of these states, individuals can register and vote on Election Day; the one exception is North Carolina, which only allows individuals to register and vote on the same day in the lead-up to an election (NCSL 2019). In 2012, nearly ten percent of voters used SDR to cast their ballot (Rogers and Carbó 2013). Descriptively, SDR states tend to have higher turnout than non-SDR states. In the 2012 presidential election, for instance, average turnout was more than ten percentage points higher in states that allow SDR.

Table 1: Varieties of Same-Day Registration

Election Day Only	Election Day & Early/Absentee Voting	Early/Absentee Voting Only
1. Connecticut 2. District of Columbia 3. Idaho 4. Maine 5. Minnesota 6. New Hampshire 7. Wisconsin 8. Wyoming	1. California 2. Colorado 3. Hawaii 4. Illinois 5. Iowa 6. Maryland 7. Michigan 8. Montana 9. Nevada 10. Utah 11. Vermont 12. Washington	1. New Mexico[7] 2. North Carolina 3. Ohio[8]

Source: Authors' analysis of National Conference of State Legislatures data (NCSL 2019).

A substantial body of research has estimated the effect of SDR on overall turnout; for instance, Burden et al. find that SDR "marginally increas[es] turnout if the window for registration is sufficiently long" (2014, p. 26). Much of this research has focused on Election Day registration states—that is, the subset of states that only allow same-day registration on the final day of the election. These studies generally indicate that EDR laws have a positive effect on turnout (Fenster 1994, Highton 2004, Highton and Wolfinger 1998, Knack 2001). However, this research has not been updated to reflect the increasing number of states with SDR laws. Eleven of the 21 states with SDR (plus Washington, DC) enacted their laws in 2012 or later (NCSL 2019), and, to the best of my knowledge, no published study includes data covering this time period.

Variation in the effect of SDR across demographic groups is less understood. A small number of existing studies suggest that removing registration barriers boosts youth turnout. One study found that youth turnout in EDR states is 14 percentage points higher than in non-EDR states in presidential elections and 4 percentage points higher in midterm congressional elections (Fitzgerald 2003).[9] Similar effects have been seen with other voting reforms that lower barriers to registration; pre-registering 16- and 17 year-olds to vote, for instance, increases the probability that youth will vote by up to 5 percentage points (Holbein and Hillygus 2017). Most importantly for our study, Leighley and Nagler (2014, ch. 4) compare the aggregate turnout of age groups before and after the implementation of EDR, finding that turnout of young voters increases significantly more than that of older voters.

[7] Beginning January 1, 2021, qualified voters can register and vote on Election Day.

[8] From 2005 to 2014, Ohio had in place a so-called "Golden Week," during which voters could register and vote on the same day. This week—technically six days—resulted from the overlap of two other voting provisions: voter registration that ended 30 days before the election, and same-day registration that started 35 days before the election. During the 6 days of overlap, voters could both register and vote on the same day.

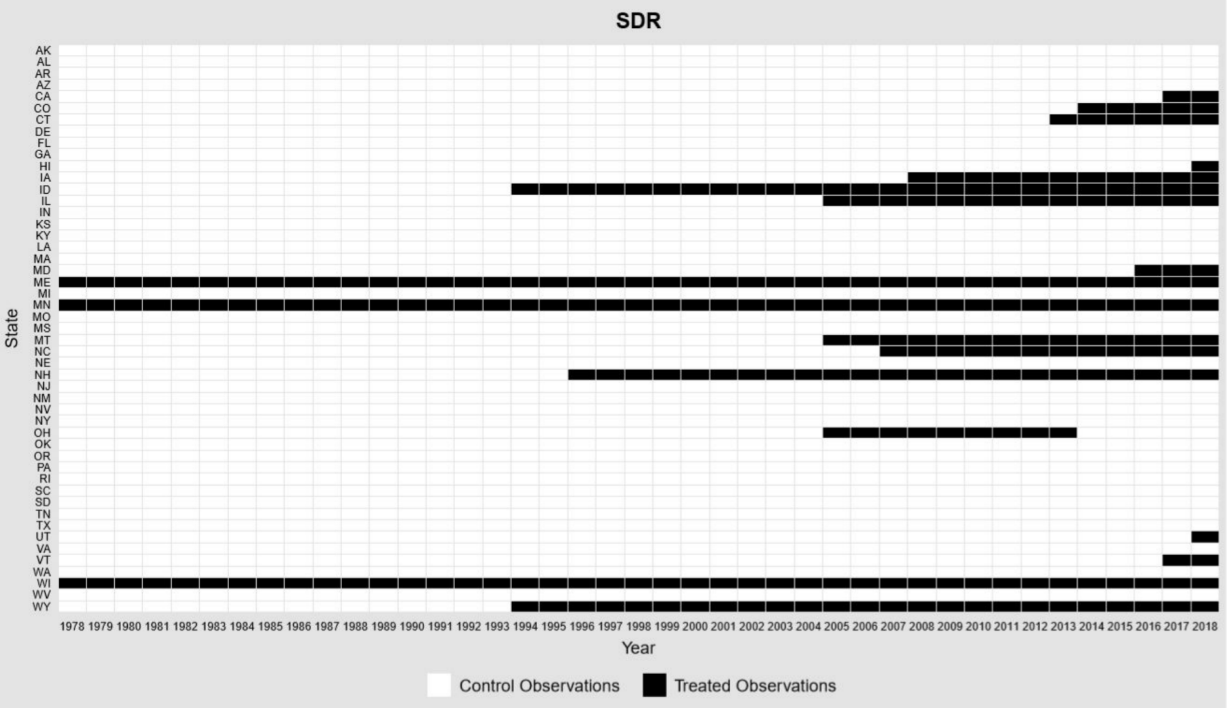
[9] Our study differs from Fitzgerald's. We increase the sample size (from n = 1,718 to n = 1.6 million individual observations) and use estimation strategies beyond cross-sectional regression.

In addition to covering a longer and more recent time period than earlier studies, we make a number of additional contributions to provide a comprehensive analysis of SDR and age. First, we theorize mechanisms behind an age-conditional effect of SDR. Second, we offer a wide array of statistical models, using a variety of both individual-level and aggregate data. Third, we investigate additional heterogeneity by election type. And finally, we investigate the potential downstream effects of SDR on election and policy outcomes.

Methods

We collect data on state SDR laws from the National Conference of State Legislatures (NCSL 2019). Data on state early voting and no-fault absentee voting laws are extended from Boehmke and Skinner (2012) and Grumbach (2018); voter ID data also come from the NCSL. Our data cover the years 1978 through 2018.

Figure 1: Implementation of SDR in the U.S. States



We collect data for the dependent variable, voter turnout, from the Census Current Population Survey (CPS) Voter Supplement. The CPS Voter Supplement is a biennial survey of approximately 60,000 households,[10] which affords us a large sample for quite precise estimates. Our individual-level models use over 1.6 million observations.[11] The CPS data also contain the age variables necessary to estimate the effect of election laws on the turnout of different age

[10] The CPS is administered every month in order to track unemployment and other labor market dynamics. Biennially, the CPS produces the Voter Supplement in November with survey questions related to voting.

[11] Like all prominent self-reported measures of voter turnout, the CPS turnout question is known to suffer from over-reporting. However, studies suggest that this over-reporting is unlikely to introduce bias to estimates of the relationship between election laws and turnout (e.g., Highton 2004; Burden et al. 2014, p. 101). Following convention (e.g., Burden et al. 2014, p. 101), we code individuals who refuse to answer as non-voters. As a robustness check, we replicate the main analysis excluding these individuals in Appendix Table A8. The results are consistent.

groups. The CPS measures specific yearly age. In our main analyses, we group individuals into conventional age categories: 18-24, 25-34, 35-44, 45-54, 55-64, 65 and above.[12]

While the CPS is the canonical dataset for studies of election law and turnout (Alvarez et al. 2011, Nagler 1991), as a robustness check, we replicate our analyses with Cooperative Congressional Election Study (CCES) data.[13] Despite the CCES's shorter time frame, smaller number of observations, and sampling issues (Grimmer et al. 2018), the results are consistent.

Electoral reform does not happen in a vacuum; confounding variables may lead states to both implement SDR and have higher voter turnout. Most studies of the effect of SDR on turnout have used traditional OLS and maximum likelihood estimation with controls for demographic characteristics that might affect turnout (e.g., Brians and Grofman 2001, Highton 1997, Knack and White 2000). Burden et al. (2014) augment their regression analysis with matching and difference-in-differences analysis to mitigate the threat of confounders.

Our main estimates come from a difference-in-differences (diff-in-diff) design, which exploits variation within states across time.[14] We supplement this with a matching design, which exploits variation in election laws across states within election years.[15]

Our diff-in-diff design tests the within-state relationship between the laws and voter turnout. Here we aggregate data to the state level.[16] The diff-in-diff design eliminates time-invariant confounders across states. We begin with a traditional two-way fixed effect regression model. Imai and Kim (2017, p. 17) note that a two-way fixed effects model does not provide an average treatment effect, but rather "a weighted average of unit-specific ATEs where the weights are proportional to the within-unit variance of the treatment assignment variable." In our case, this means that the two-way fixed effects model more heavily weights states that switch to SDR near the middle of our time-series. To account for this, we augment the model with a weighted fixed effects (WFE) estimator (Imai and Kim 2017). Because the weighting procedures of WFE reduce statistical precision considerably and our effective sample size is small, we primarily use it as a substantive robustness check.

Second, we use a matching design, which compares differences in turnout between demographically similar individuals in SDR and non-SDR states in the same election. The matching strategy pairs each treatment unit to a similar control unit, avoiding comparing

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[12] We use age categories because the conditional effect of election laws may not vary linearly by age. An alternative strategy is to use a continuous age variable with quadratic and/or cubic terms. The results are substantively consistent. We opt for the age categories for purposes of substantive clarity. The groups are of roughly comparable population size, except for the 18-24 category, which is a smaller group in the population (9.5% of the U.S. population in the 2010 Census, compared to 13.5% on average for the other groups).

[13] The CCES data begins in 2006. Survey respondents are matched to state voter files to construct the validated turnout variable. While the CCES data offer a valuable robustness check, there are important drawbacks that make the CPS preferable for our main analyses. The method used to match CCES respondents to voter file records varies by state and time (Grimmer et al. 2018). Furthermore, the CCES sample size is smaller in each election year, and when combined with its limited coverage across time, the sample size in analyses is reduced by over an order of magnitude. The limited time frame poses additional problems for our difference-in-difference estimation.

[14] Specifically, within-state changes in turnout in SDR versus non-SDR years are compared to within-state changes in states that do not implement SDR.

[15] For the matching regressions, we provide a placebo analysis based on those of Grimmer et al (2018), in which we compare turnout in states *before* they implement SDR with turnout in states that *never* implement SDR after adjusting for demographic covariates. We find that young people's turnout does not vary. However, older individuals' turnout is slightly higher in pre-SDR states than never-SDR states (Appendix Table A6).

[16] A difference-in-difference design at the county level is also possible, but this is less feasible for our analysis of the effect by age. Age is much more strongly correlated with county than state of residence.

individuals who are very likely or unlikely to live in a SDR state; for instance, SDR states had smaller concentrations of racial minorities than non-SDR states until the 2010s. We use genetic matching, which generates many potential matches of control units to treated units until covariates are balanced across the treatment and control groups (Diamond and Sekhon 2013). Specifically, within each age group and election year, we match individuals by race, gender, and continuous measures of age, income, and education.[17] A paired t-test estimates the average treatment effect on the treated (ATT) for the matched sample.[18] We report statistics on covariate balance in Appendix Table A5. Only four of the 66 age group-covariate combinations significantly differs between treatment and control units at the $p<0.05$ level, indicating strong balance on observables.

Both of the estimation strategies—matching and diff-in-diff—have different strengths and weaknesses in identifying the causal effect of election laws on turnout. The matching design must locate and adjust for all of the relevant differences between individuals in SDR and non-SDR states that could affect their likelihood of turning out to vote (the selection on observables assumption). Difference-in-differences compares turnout in SDR elections to turnout in non-SDR elections within a state (e.g., the difference in turnout in Iowa before and after its 2007 implementation of SDR), while also holding constant all national characteristics of the particular election year. If SDR implementation is not correlated with other turnout-related factors, this approach may be preferable to comparing Iowans in 2008 (for example) to individuals in another state without SDR. However, this assumption of no time-variant confounders is not always appropriate; bias occurs if an unobserved factor about a state changes in ways that are correlated with the timing of its implementation (or non-implementation) of SDR. Through these multiple design strategies (including non-parametric tests), the inclusion of carefully selected controls at different levels of analysis, and replication in two distinct data sets, we improve on previous estimation strategies.

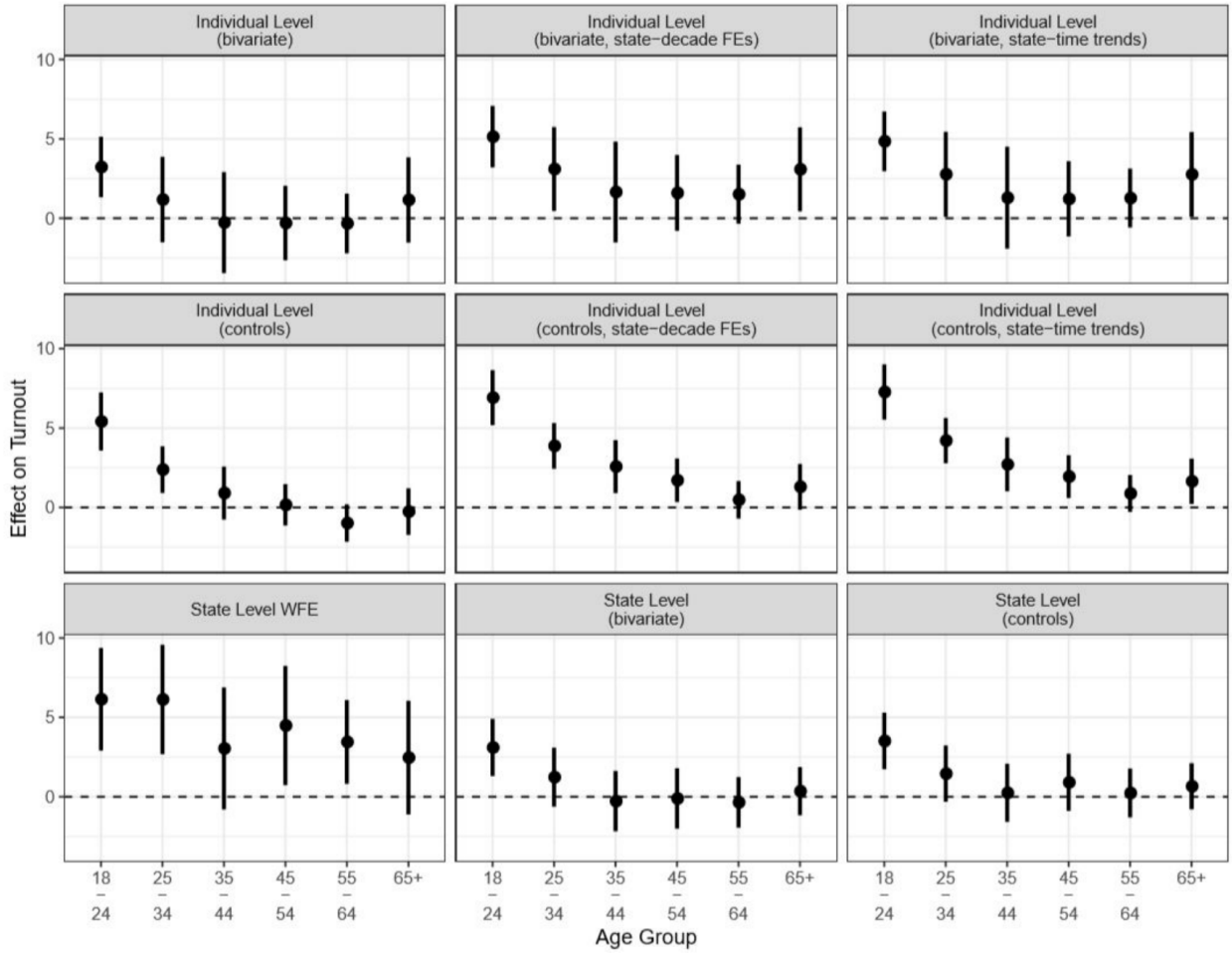
Results

Figure 2 plots the effect of SDR from separate difference-in-differences model specifications along with 95% confidence intervals. For 18-24 year-olds, the individual level models show a 3.23 and 5.41 percentage point increase in turnout for the bivariate and covariate-adjusted specifications, respectively. When including state-decade fixed effects or state × linear time trends, the marginal effects range from 4.84 to 7.27. The aggregate state level models show effects of 3.10 (bivariate) and 3.51 percentage-points (controls). By contrast, SDR effects for individuals ages 25 and over, and especially for those 35 and over, are smaller within 16 specifications. Estimates for groups 35 and over range from -0.99 to 4.49. With the exception of the WFE specification, the 18-24 coefficient is significantly greater than each of the coefficients for groups 35 and over ($p<0.05$).

[17] We exact match on race and gender.

[18] Unlike an average treatment effect $E[Y(1) - Y(0)]$, an ATT is the average difference between only the observed treated units and their counterfactuals $E[Y(1) - Y(0) \mid T=1]$.

Figure 2: Difference-in-Differences Effect on Turnout by Age



Note: All models include state and year fixed effects. State-decade FE specifications include three fixed effects for each state (1978-1990, 1992-2004, and 2006-2018). State-time trends specifications also include state × year fixed effects. State level models use aggregated state level data (N=980 for each age group model). Individual level covariates include race, gender, income, and education.State level covariates include percent white, percent black, percent Asian, poverty rate, and percent college graduates or above. Full regression results are presented in Appendix Tables A1-A4. Robust standard errors are clustered by state. In addition to heteroskedasticity, WFE standard errors allow for arbitrary autocorrelation.

The WFE specification shows a 6.14 percentage point effect of SDR on the turnout of 18-24 year-olds, with a very similar estimate for 25-34 year-olds. As expected, the traditional fixed effects specifications produce estimates with considerably smaller variance than WFE. In turn, although the SDR effect is again greatest for young voters, the estimates are not significantly greater than those of 45-54 year olds $p<0.05$ level. As an additional robustness check, we provide a lagged dependent variable model in Appendix Table A8.

Table 2: Matching Estimates of SDR Effect by Age

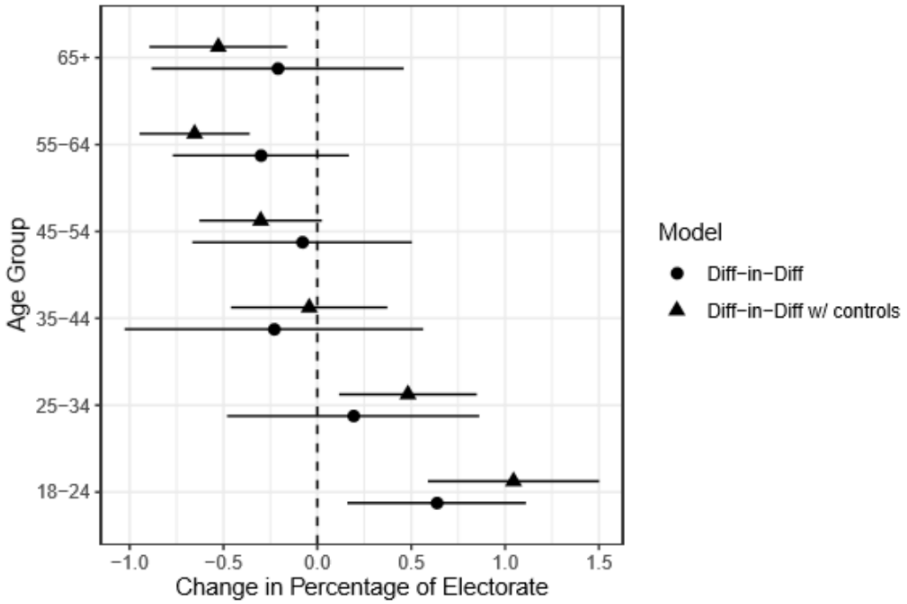
Age Group	ATT Estimate	95% CI
18-24	0.063	(0.056, 0.071)
25-34	0.059	(0.052, 0.066)
35-44	0.060	(0.053, 0.066)
45-54	0.048	(0.042, 0.055)
55-64	0.043	(0.036, 0.050)
65+	0.047	(0.041, 0.053)

Our matching analysis finds that SDR increases turnout for all age groups, but most for younger voters. Table 2 reports ATT estimates of the effect of SDR on the probability of turnout by age group, along with 95% confidence intervals. While the estimates are positive for all age groups, they are largest for the youngest voters, reaching a high of 6.3 percentage points for 18-24-year-olds. The matching results show one important difference from the diff-in-diff results presented above: whereas our diff-in-diffs suggested that the SDR effect is significantly greater for 18-24 year-olds than for any other age group, the matching estimates show an effect for 18-24 year-olds that is virtually identical to the effect for 25-34 year-olds. Still, the SDR effects for 18-24 year-olds and 25-34 year-olds are significantly greater than the effects for every other age group.

We also predict the change in the age composition of the U.S. electorate if all states were to allow for SDR. We first estimate the predicted probability of voting by age group under counterfactual scenarios of all states with SDR and no states with SDR (using the individual level bivariate and control specifications in Figure 2). For each counterfactual, we then weight these probabilities by the number of individuals of each age group in the population from the 2010 Census. We divide this estimate (the number of voters from each age group) by the total number of voters to estimate each group's percentage of the electorate.

Figure 3 plots the difference in percentage of the electorate from each age group under full SDR and no SDR, along with 95% confidence intervals around the predictions. The shares of 18-24 year-olds and 25-34 year-olds in the electorate both increase under SDR. Mechanically, this also means that older voters make up a smaller part of the electorate under SDR. It appears that universal statewide adoption of SDR would make the electorate younger.

Figure 3: Change in Age Composition of Electorate Under SDR



A younger electorate could have major consequences for both election results and policy outcomes. Previous research has estimated the impact of universal turnout on election outcomes; since non-voters lean slightly more Democratic than voters, universal turnout would likely increase Democratic vote shares by 1.5 percentage points in Senate races (Citrin et al. 2003, Sides et al. 2008). However, because *young* non-voters are even more likely to lean Democratic, expanded SDR would likely change nearly as many election outcomes as universal turnout—including, quite possibly, the result of the 2016 presidential election. The non-SDR state of Michigan, for example, is home to nearly one million 18-24 year-olds. If additional voters from SDR were to have voted in the same patterns as real 2016 Michigan voters of their age groups (and if SDR did not meaningfully change other critical election factors such as the geography of turnout), our difference-in-differences estimate implies a counterfactual vote swing for Hillary Clinton of between 19,000 and 28,000 votes, larger than Donald Trump's victory margin in Michigan of 10,704 votes.[19]

Automatic Voter Registration and Turnout by Age Group

Similarly to same-day registration, automatic voter registration (AVR) aims to boost turnout by reducing the costs of registration for potential voters. The core concept is straightforward: any eligible individual who interfaces with a participating state government agency—primarily the DMV—is automatically enrolled to vote, rather than being required to "opt in" by proactively filling out a registration card.[20] Substantial variation still exists across AVR regimes: for instance, most AVR states offer voters a chance to opt out of registration on the "front end"—that is, while interacting with a government agency responsible for registering voters, such as the DMV—while others only allow voters to opt out on the "back end," typically

[19] We take the partisanship of presidential vote by age group from the CCES data. The substantive point stands when using exit poll estimates instead. Overall, our prediction is relatively conservative given the greater effect of SDR on young people in presidential elections shown earlier.

[20] Notably, the AVR label is sometimes misapplied to states with easy opt in processes, such as New Mexico, Utah, and Colorado (until Colorado implemented a true opt-out AVR system in 2020).

via a postcard mailed to them after they have been automatically registered. States also vary in which government agencies serve as automatic registration sites; all AVR states but Alaska automatically register eligible individuals who interact with the DMV, but about half also register people at additional agencies such as health and social service departments (NCSL 2021).

Oregon passed the first-ever AVR bill in 2015. Immediately after the law was implemented in early 2016, Oregon's average monthly voter registration rate more than tripled (Brater 2016). In the 2016 general election, Oregon's AVR system turned out 98,000 new voters, more than 40,000 of whom were otherwise unlikely to have registered and voted—disproportionately young people and individuals living in less urban, lower income, and more racially diverse communities (Griffin et al. 2017). Inspired by Oregon's ostensible success, nineteen additional states, plus the District of Columbia, have adopted their own AVR policies over the past five years. The rapid propagation of AVR has been driven partly by proponents' conviction that removing voter registration barriers will increase turnout, especially among typically low-propensity voters. Yet no comprehensive study has been completed of AVR's effectiveness at boosting turnout among specific demographic groups.

Past studies of other voting reforms, especially those focused on lowering the cost of registration, provide good reason to expect AVR to boost *aggregate* turnout; for example, voting rates are significantly higher in states with "motor voter" registration, a predecessor of AVR, than in other states (Franklin and Grier 1997). But AVR's turnout effect may be different across age groups. McGhee and Romero (2019) use both difference-in-differences and synthetic control identification strategies to analyze AVR's effect on young people's voter registration rates. They find mixed, though generally positive, results. Their diff-in-diff model finds a 5.1 percentage-point increase in registration rates for youth (compared to a 2.1 percentage-point increase for all individuals). The synthetic control analysis finds average treatment effects for young people ranging from +9.4 percentage points in Oregon to -2.6 percentage points in Connecticut. Ten of twelve states show a positive AVR effect for young people, but no treatment effect rises to traditional levels of statistical significance.

It is not immediately obvious that AVR should disproportionately boost the turnout rates of young adults. As discussed in an earlier section, young people do likely face higher registration costs than older individuals; to the extent that AVR reduces these costs, it may indeed have a greater effect on turnout for young people. Then again, as shown in Table 3, young adults are far less likely than Americans in their 50s and 60s, and slightly less likely than those in their 70s and older, to have a valid driver's license (Sivak and Schoettle 2016). If young people are interacting less often with the DMV than other groups, AVR might actually be *poorly* suited to boosting youth registration and turnout rates.

Table 3: Licensed drivers as a percentage of their age-group population

Age	1983	2008	2011	2014
16	46.2	31.1	27.5	24.5
17	68.9	50.0	45.0	44.9
18	80.4	65.4	69.3	60.1
19	87.3	75.5	69.3	69.0
20-24	91.8	82.0	79.7	76.7
25-29	95.6	86.3	87.5	85.1
30-34	96.5	90.6	89.1	86.6
35-39	94.9	91.7	90.2	87.9
40-44	92.2	91.9	91.6	89.1
45-49	92.5	93.0	91.9	90.5
50-54	91.4	94.2	92.2	91.2
55-59	88.2	94.9	93.2	91.8
60-64	83.8	95.9	92.7	92.1
65-69	79.2	94.0	93.0	91.4
70+	55.0	78.4	79.2	79.0

Source: Sivak and Schoettle's (2016) analysis of Federal Highway Administration data.

There is clearly a need for additional research that sheds light on the relationship between AVR laws and turnout across age groups.

Methods

We collect data on state AVR laws from the National Council of State Legislatures (NCSL 2021) and from the Brennan Center for Justice at New York University Law School (Brennan Center 2020). For our dependent variable, voter turnout, we again use the canonical dataset in the study of election reforms and turnout: the CPS Voter Supplement, a biannual survey of over 80,000 individuals.[21] We limit our analysis to states with AVR laws in advance of the 2018 midterm elections. This was the only election year in which a large number of states were using AVR and for which CPS Voter Supplement data is presently available. Moreover, the 2020 election suffered the simultaneous shocks of the Covid-19 pandemic, myriad legal challenges to absentee and mail-voting legal provisions, and widespread problems of voter suppression and election disinformation, all of which likely shifted voting access and

[21] As a robustness check, we provide analogous models using voter file data. We do not use these estimates for our main results because voter files do not have representative samples of non-registrants, leading to bias in studies of turnout (Nyhan, Skovron, and Titiunik 2017).

participation within states and demographic subgroups. Table 4 shows the full list of states with AVR, with the states included in this 2018-focused analysis shaded in gray.

Table 4: States with AVR

State	AVR Approved	AVR Implemented
Alaska	November 2016	March 2017
California	October 2015	April 2018
Colorado	2017	February 2017
Connecticut	May 2016	2020
District of Columbia	December 2016	June 2018
Georgia	2016	September 2016
Illinois	August 2017	July 2019[22]
Maine	June 2019	Anticipated 2022
Maryland	April 2018	July 2019
Massachusetts	August 2018	January 2020
Michigan	November 2018	September 2019
Nevada	November 2018	January 2020
New Jersey	April 2018	November 2018[23]
New York	December 2020	Anticipated 2023
Oregon	March 2015	January 2016
Rhode Island	July 2017	June 2018
Vermont	April 2016	January 2017
Virginia	April 2020	July 2020
Washington	March 2018	July 2019
West Virginia	April 2016	Anticipated July 2021

Source: Author's consolidation of data from the National Conference of State Legislatures (2021) and the Brennan Center (2020).

Our estimation strategy is a cross-sectional analysis, the most common methodological approach to measuring the relationship between election laws and turnout. As Hanmer (2009)

[22] The AVR law set an implementation deadline of July 2018, but the state did not first offer AVR until July 2019 (Tareen 2019). The Brennan Center argues that AVR had still not been effectively implemented in the state as of early 2020 (Brennan Center 2020).

[23] New Jersey began implementing its AVR law on November 1, 2018, just 5 days in advance of the November 6 general election. Because AVR works primarily by registering people who interact with the DMV for other reasons, and because relatively few people likely interacted with the DMV on those 5 days, we do not include it in our analysis of AVR's effect on 2018 turnout.

and others discuss, this methodological approach is vulnerable to the possibility of endogeneity affecting turnout estimates. States adopt election reforms wholesale for their constituents, and they may do so for reasons directly related to voter turnout. For instance, states with a history of embracing widespread political participation may be oriented toward expanding the electorate even further by adopting an electoral reform that promises to boost voting rates, whereas states with a history of voter suppression may be inclined not to embrace such a reform. Comparing the turnout of these two types of states, and asserting that any differences are due to the presence or absence of the reform, would be to ignore the potentially more important variable of the state's political orientation toward voter participation and to therefore obtain biased estimates of the reform's impact on voting rates.

We take this concern seriously but, for several reasons, still believe our cross-sectional approach is best-suited for an analysis of AVR's effect on turnout. First, the states that had implemented AVR by 2018 represent a broad range of political climates and orientations toward electorate-expanding reforms. At the time, AVR states included California and Oregon—liberal states with a history of adopting other cost-reducing policies such as SDR and all-mail voting—but also Alaska and Georgia, conservative states that have not fully embraced either of these other reforms.[24] AVR existed in the Northeast (Vermont, Connecticut, Rhode Island), South (District of Columbia, Georgia), Southwest (Colorado), and West (Alaska, California, Oregon). Some states with AVR had a history of high voter turnout (e.g., Oregon), while others had struggled with low turnout (e.g., Georgia). Additionally, most historically high-turnout states had not adopted AVR. There is also significant variety in the ways in which states adopted their AVR policies: some passed AVR laws through the legislature, others (Connecticut and Georgia) created AVR policies through administrative agencies, and Alaska adopted AVR at the ballot box via the initiative process.

We also control for a range of factors that might be associated with both the presence of an AVR law and higher turnout, such as demographic factors (race, age, income, education, and gender), election characteristics (whether the election featured a U.S. Senate or gubernatorial race), and the presence of other voting laws (SDR, early voting, and no-fault absentee voting). In doing so, we reduce the probability that any positive association between AVR and turnout is in fact due to a confounding variable.

The results show an insignificant relationship between AVR and overall turnout of approximately one half of one percentage point. However, we find substantial heterogeneity by age: AVR has a large positive association with turnout of young individuals but is unrelated or even negatively associated with turnout of older individuals. The results point to the importance of exploring the impacts of voting reforms by age group.

Results

We first present the results of an OLS regression examining the overall relationship between AVR and turnout (Table 5). Holding constant demographic factors (race, age, income, education, and gender), election characteristics (whether the election featured a U.S. Senate or gubernatorial race), and the presence of other voting laws (SDR, early voting, and no-fault absentee voting), we find that individuals in AVR states were one percentage point more likely to vote in 2018 than individuals in non-AVR states, a modest but statistically significant increase.

[24] Alaska permits same-day registration for presidential and vice presidential elections only.

Table 5: AVR and Turnout

	Dependent variable:			
	Pr(Voted)			
	(1)	(2)	(3)	(4)
AVR	0.014**	0.010*	0.012*	0.010*
	(0.004)	(0.005)	(0.005)	(0.005)
SDR		0.037***	0.036***	0.023***
		(0.004)	(0.004)	(0.004)
Early Voting		-0.052***	-0.051***	-0.030***
		(0.004)	(0.004)	(0.004)
No-Fault Absentee		0.015***	0.017***	0.020***
		(0.004)	(0.004)	(0.004)
Senate Election			0.013***	0.007
			(0.004)	(0.004)
Gubernatorial Election			-0.003	-0.017***
			(0.004)	(0.004)
Constant	0.542***	0.537***	0.530***	0.189***
	(0.002)	(0.003)	(0.005)	(0.008)
Demographic Covariates	No	No	No	Yes
Observations	88,749	85,915	85,915	85,915
R^2	0.0001	0.003	0.003	0.139
Adjusted R^2	0.0001	0.003	0.003	0.138
Residual Std. Error	0.498 (df = 88747)	0.498 (df = 85910)	0.498 (df = 85908)	0.462 (df = 85895)
F Statistic	9.828** (df = 1; 88747)	60.430*** (df = 4; 85910)	42.330*** (df = 6; 85908)	727.789*** (df = 19; 85895)
Note:				*p<0.05; **p<0.01; ***p<0.001

To estimate the effect of AVR on specific age groups, we interact an individual's age with the presence of an AVR law in their state. We find that the effect of AVR on the probability of voting varies considerably by age: AVR is associated with a 6.3 percentage-point increase in voter turnout among individuals ages 18-24, while for individuals 65 and over, AVR is associated with a 2.0 percentage-point *decrease* in turnout. Results are robust to the inclusion of individual-level Census weights (Appendix Table A7).

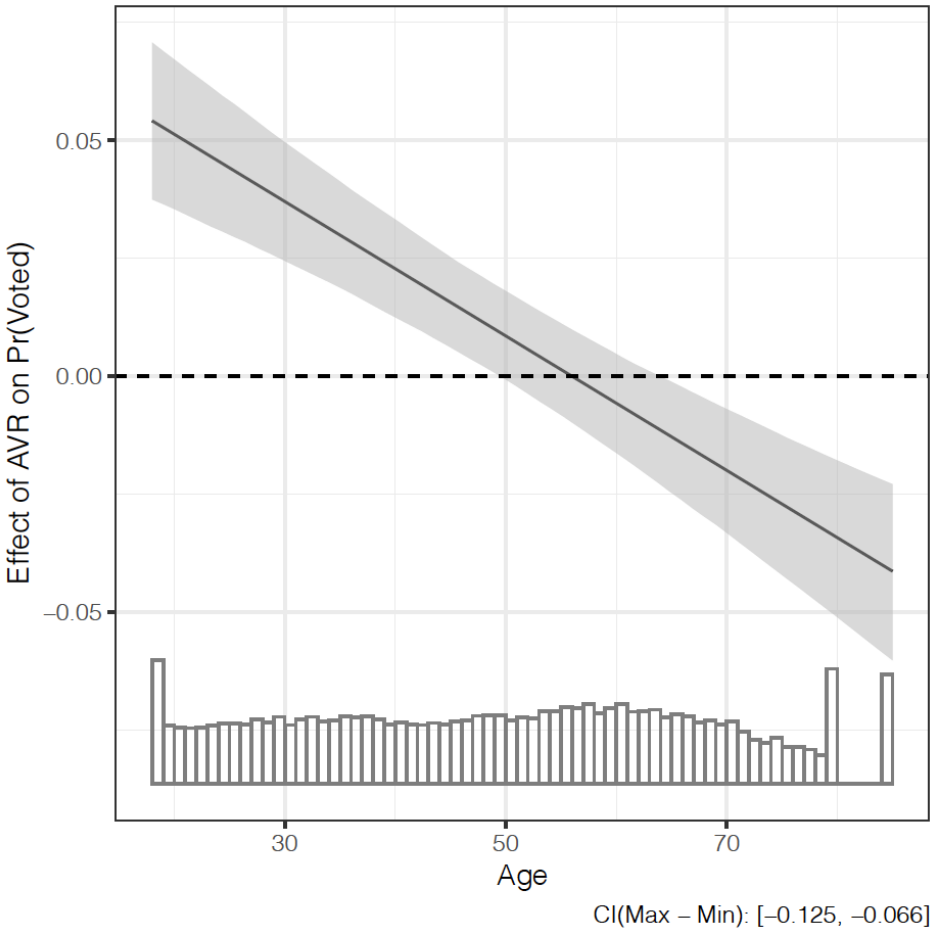
Table 6: AVR and Turnout By Age

	Dependent variable:			
	Pr(Voted)			
	(1)	(2)	(3)	(4)
AVR	-0.006	-0.005	-0.004	-0.020*
	(0.009)	(0.009)	(0.009)	(0.009)
Age 18-24	-0.362***	-0.359***	-0.359***	-0.344***
	(0.007)	(0.007)	(0.007)	(0.007)
Age 25-34	-0.254***	-0.254***	-0.254***	-0.305***
	(0.006)	(0.006)	(0.006)	(0.006)
Age 35-44	-0.166***	-0.166***	-0.166***	-0.228***
	(0.006)	(0.006)	(0.006)	(0.006)
Age 45-54	-0.099***	-0.098***	-0.098***	-0.151***
	(0.006)	(0.006)	(0.006)	(0.006)
Age 55-64	-0.044***	-0.044***	-0.044***	-0.074***
	(0.006)	(0.006)	(0.006)	(0.005)
SDR		0.037***	0.037***	0.023***
		(0.004)	(0.004)	(0.004)
Early Voting		-0.042***	-0.042***	-0.030***
		(0.004)	(0.004)	(0.004)
No-Fault Absentee		0.011**	0.012**	0.020***
		(0.004)	(0.004)	(0.004)
Senate Election			0.011**	0.007
			(0.004)	(0.004)
Gubernatorial Election			-0.003	-0.017***
			(0.004)	(0.004)
AVR:Age 18-24	0.080***	0.063***	0.063***	0.083***
	(0.016)	(0.016)	(0.016)	(0.016)
AVR:Age 25-34	0.067***	0.045**	0.045**	0.059***
	(0.013)	(0.014)	(0.014)	(0.014)
AVR:Age 35-44	0.042**	0.024	0.024	0.039**
	(0.014)	(0.014)	(0.014)	(0.014)
AVR:Age 45-54	0.018	0.014	0.014	0.028*
	(0.014)	(0.014)	(0.014)	(0.014)
AVR:Age 55-64	-0.005	0.0001	-0.0002	0.005
	(0.013)	(0.014)	(0.014)	(0.013)
Constant	0.666***	0.660***	0.654***	0.193***
	(0.004)	(0.004)	(0.006)	(0.008)
Demographic Covariates	No	No	No	Yes
Observations	88,749	85,915	85,915	85,915
R^2	0.051	0.054	0.054	0.139
Adjusted R^2	0.050	0.054	0.054	0.139
Residual Std. Error	0.485 (df = 88737)	0.485 (df = 85900)	0.485 (df = 85898)	0.462 (df = 85890)
F Statistic	429.170*** (df = 11; 88737)	349.741*** (df = 14; 85900)	306.668*** (df = 16; 85898)	578.219*** (df = 24; 85890)
Note:				*p<0.05; **p<0.01; ***p<0.001

Note: Demographic covariates include race, gender, income, and education.

The magnitude of the association between AVR and turnout decreases as individuals age. AVR is associated with a 3.9 percentage-point turnout increase for individuals 25-34, a 1.9 percentage-point increase for those 35-44, and a 0.8 percentage-point increase for those 45-54. For individuals ages 55-64, AVR is associated with a 1.5 percentage-point decrease in turnout, though the difference between this and the 2 percentage-point decrease for seniors is statistically insignificant. Figure 4 plots the marginal effect of AVR by age, with age modeled linearly.

Figure 4: AVR and Turnout by Age



Conclusion

These analyses show that same-day registration and automatic voter registration have the potential to meaningfully shrink the turnout disparity between young and older Americans. Two quasi-experimental analyses find that same-day registration leads to higher turnout across the board but especially for younger individuals, increasing their share of the electorate. A cross-sectional analysis finds a large positive association between AVR and youth turnout, and smaller—and sometimes even negative—associations between AVR and older individuals.

What might explain this negative relationship between AVR and older age groups? We suspect this may be a statistical artifact—that when the data exists to conduct a more sophisticated causal analysis of AVR's effect on turnout by age, this relationship will disappear. However, if it does hold across analyses, we can offer at least one explanation. If the presence of AVR in a state leads political campaigns and voter outreach groups to shift some of their limited resources to mobilizing new registrants (who are disproportionately young), and to deprioritize mobilizing the highest-propensity voters (who are disproportionately old), this could result in a small turnout drop for the oldest voters.

Clearly, more data beyond this cursory look is needed to causally identify AVR's effect on turnout by age. It could be that the states that adopted AVR in advance of the 2018 election were systematically different from those that did not in ways not captured by this cross-sectional

analysis. Examining a single election year, in an admittedly tumultuous political environment, is insufficient to determine the effect of AVR on turnout over the long run. And grouping all AVR states together ignores the substantial heterogeneity in AVR regimes between states. Future research should incorporate individual-level turnout data from the 2020 general election, use a broader range of methods, and explore heterogeneous turnout effects across types of AVR states.

The findings presented here point to unequally distributed voting costs being an important factor in turnout disparities across age groups. If a voting reform reduces or eliminates a voting cost, and if turnout then goes up more for young people than for other age groups, it stands to reason that young people were disproportionately burdened by that voting cost in the first place. The next chapter provides additional support for this idea. Using data from an original, nationally representative survey, I find that a range of voting costs are higher for younger Americans than for older age groups, and individuals who face higher costs are less likely to vote.

CHAPTER 3: YOUNG PEOPLE FACE HIGHER VOTING COSTS

One reason for the age-turnout gap may be that voting is more costly for young people than for other age groups. The past chapter showed that when certain voting barriers are reduced or eliminated, turnout goes up disproportionately for young people. In particular, same-day registration and automatic voter registration are associated with greater turnout among young adults more than among other age groups.

Yet we know surprisingly little about how voting costs might vary by age. This is a clear oversight. Understanding the ways in which young people are especially burdened by the voting process is essential for diagnosing shortcomings of our current voting system—and for identifying the specific policy changes most likely to increase youth turnout in the future.

To explore how voting costs may be unequally distributed across age groups, I surveyed voting-age Americans about their experiences with registering and voting. I find that young people face higher barriers to voting than other age groups. Compared to seniors, today's youth are less informed about the voting process and how to research candidates and issues; struggle more to find the time to vote, to plan ahead to vote, and to balance voting with other life tasks; face greater transportation issues and tradeoffs between voting and earning money; have greater difficulty with the parts of voting that cannot be done online; disproportionately think mail voting is a hassle; and are less likely to own the documentation they need to register and vote. When asked directly, youth are significantly more likely than older Americans to say that registration and voting are difficult. A series of linear regressions finds that, after adjusting for race-ethnicity, gender, education, and family income, being young (relative to being a senior) is a large and statistically significant predictor of voting costs. In turn, these voting costs are significant predictors of voting, with information costs the strongest predictors: people who report lacking registration and voting information are up to 57 percentage points less likely to have voted in 2018.

One path to reducing voting costs for young people is for policymakers to make the registration and voting process easier. I asked respondents to indicate how much easier voting would be if various voting reforms—same-day registration, early voting, mail voting, automatic voter registration, updating people's registration after they move, and internet voting—were adopted. On average, all ages believe that these voting reforms would make things easier for them, with young people slightly more likely than seniors to believe that internet voting and same-day registration, in particular, would make things easier.

These results suggest that reducing the specific voting costs that disproportionately affect young people could dramatically shrink the age-turnout gap. Yet the real-world effects of cost-reducing election reforms are typically more modest. Even same-day registration, which eliminates the cost of voter registration, only boosts youth turnout by between 3 and 7 percentage points. This presents a puzzle: if high voting costs prevent young people from voting, then why does lowering those costs not lead to even larger turnout gains? One plausible explanation may be that young people are largely unaware of the cost-reducing voting reforms enacted in their state and therefore less likely to take advantage of them.

I explore this possibility by asking survey participants to answer a series of factual questions about their state's registration and voting laws. I find that, on average, young people are the least-informed of any age group about their state's policies on same-day registration, early voting, mail voting, and whether individuals must update their voter registration after

moving. Regression analyses again confirm that young age is a significant predictor of lower information about state voting laws.

In the following sections, I explore reasons why young people may face higher voting costs than older voters, especially seniors, and discuss challenges with past approaches to measuring voting costs. I then lay out my new approach to measuring costs and present the methods and sampling strategy used in my survey. Finally, I present survey results and discuss their implications.

Why Young People Face Higher Voting Costs

Broadly speaking, individuals could face higher voting costs in two ways. First, they might have to expend disproportionately more resources (e.g., mental effort, free time, or money) than other people in order to register and vote. In other words, they might face higher voting *barriers* than other people. For example, we know that on average, Black and Hispanic Americans face longer voting lines than white Americans. Casting a ballot therefore "costs" them a greater expenditure of time. In other cases, the barriers to voting might be constant across all individuals, but some people might have fewer *resources* to overcome these barriers. Two people may face equally long voting lines, but if one person has less free time, waiting to vote will be more costly for them.

I hypothesize that young people—and especially today's young people—have certain attributes that both lower their starting level of voting resources and also require them to expend relatively more resources than other age groups in order to vote. Following the existing literature on youth turnout, these can be separated into life-cycle and cohort-specific factors: life-cycle factors relate to being at a certain stage of the life cycle, whereas cohort factors relate to being a member of a time-specific birth cohort.

Life-Cycle Factors

Young people tend to share certain characteristics as a result of being in the same life stage. For starters, given the minimum voting age of 18, they are more likely to be new voters. Relative to older individuals who have previous experience voting, it likely takes more mental effort for them to learn how to register, find and travel to their registration site, and (correctly) fill out their registration paperwork. Once registered, it should again take more mental effort to learn about the voting process, develop a voting plan, find and travel to their polling place, and successfully complete their ballot. New voters may also not know how to go about deciding which candidates and issues to support. They may, for instance, be unaware of common voting resources such as voter guides and advocacy group endorsements.

Young adulthood is typically associated with a series of major life changes, including finishing secondary school, moving out of the family home, and finding new friends and romantic partners. These activities each take a substantial investment of time and may leave young people feeling busier than their older (and more settled) counterparts, raising their opportunity cost of taking time to register and vote. Notably, young people expect voting to take a significant amount of time; a 2012 poll (CIRCLE 2012, p. 4-5) found that nearly 40 percent of young adults expected voting on Election Day to take between 21 and 60 minutes, while another 10 percent thought it would take at least an hour. Researchers have also found an association between shorter polling hours and lower youth turnout (Kawashima-Ginsberg et al. 2009, p. 4), suggesting that schedule restraints prevent some youth from voting. By virtue of being in college or overrepresented in part-time jobs, young people may also have less consistent schedules than

other age groups, making it harder to plan ahead to vote. Relatedly, they may struggle to balance voting and registration with all the other things on their plate; one recent study found that young people have worse work-life balance than other age groups (Nagle 2019).

Youth workers might also perceive a greater opportunity cost from taking the time to register and vote, for two reasons: First, young people are overrepresented among hourly wage workers; while young adults ages 19-25 make up just 9 percent of the U.S. population (Kaiser Family Foundation 2018), they constitute 20 percent of wage earners. In contrast to salaried workers who earn a consistent amount each pay period regardless of the specific number of hours they work, wage workers forgo income if they spend time they could be working on other, non-work tasks. Exacerbating this issue, young people are disproportionately likely to work in the "gig economy," where they could theoretically work at any given moment of the day. Second, young people generally have less personal wealth than older Americans, making up a full 50 percent of minimum-wage earners (Bureau of Labor Statistics 2018). The marginal dollar they forgo by taking time to register and vote constitutes a larger percentage of their income; this should make it more costly for them to participate.

Because they are poorer, young people likely also have a harder time shouldering the real financial costs of registering and voting. They may need to purchase the official documents needed to obtain a valid voter ID (Thompson 2012), pay to print out registration forms, buy stamps for a mail-in ballot (Armitage 2018), and pay for bus, taxi, or ride-share transportation to and from registration and voting sites. All told, being low-income would be expected to negatively impact youth registration and turnout rates.

Compared to older Americans, young people are also disproportionately mobile. Individuals between the ages of 18 and 29 change addresses more than twice as frequently as those over the age of 30 (US Census Bureau 2016). Many relocate for college just as they become eligible to vote; in one study, more than half of people between the ages of 18 and 21 who reported having moved in the previous year cited education or schooling as a major reason for relocating (Taylor et al. 2008). Unless they are moving within a state with automatic voter registration and they update their home address with their state, these young people must re-register to vote every time they move (Ansolabehere et al. 2012, Holbein and Hillygus 2020, Kaid et al. 2007, Squire et al. 1987).

Yet having to re-register is just one component of the increased costs that follow from relocation. Individuals who move addresses may also have to exert more effort to learn a host of new information relevant to voting in their new neighborhood, including any differences in registration and voting rules, where to find the closest registration site and polling place, how to navigate to and from those registration and voting sites, and, if they moved to a new jurisdiction, background on the candidates and issues on their ballot.[25]

New research from Holbein and Hillygus (2020) suggests that young people have underdeveloped "non-cognitive skills" such as conscientiousness and grit—and that this is a feature consistent across generations, not specific to "kids these days." These skills are necessary to overcome not only traditional voting barriers but also unanticipated obstacles like inclement weather (Fujiwara et al. 2016, Gomez et al. 2007, Henderson and Brooks 2016) or long voting lines (Pettigrew 2016). A voting system that demands participants persevere through numerous setbacks should be more costly for those who lack this capacity.

[25] While several scholars have highlighted the link between youth mobility and low registration rates, none have explicitly incorporated higher youth mobility into a broader theory of heterogeneous voting costs by age or examined other ways that mobility may affect voting costs.

Despite today's young Americans earning college degrees at higher rates than previous generations, the youngest voting-eligible adults (ages 18-24) still have lower levels of formal education than older Americans (US Census Bureau 2020). Educational attainment is strongly associated with voter turnout. Burden (2009) offers three explanations for this relationship: education "provides people with skills to make sense of the political world," "makes for easier navigation of voter registration requirements and other impediments to voting," and "socialize[s] a sense of civic duty and expose[s] them to elite recruitment efforts" (p. 542). College students tend to turn out at much higher rates than non-college youth; turnout rates for young people with a bachelor's degree usually range from 60 to 80 percent (Godsay et al. 2012). Yet only 37 percent of Americans under 30 have a bachelor's degree (US Census Bureau 2019a). For young adults without any college experience, turnout is around 35 percent—about half that of college graduates (Godsay et al. 2012).

Cohort Factors

Although the age gap in U.S. turnout has existed for decades, evidence suggests it has grown in recent years (Smets 2010). Cohort-specific factors may help explain this growing gap.

Today's young people, commonly referred to as Millennials and Gen Z, grew up in the internet age. These "digital natives" are used to completing bureaucratic tasks through online portals, many of which provide streamlined automated or "one-click" services. For example, about 70 percent of 18-29-year old smartphone owners have used their phone to do online banking, compared with only 57 percent of the overall population (Smith 2015). Young people may find it difficult to navigate a voting system that still requires paper forms, long waits between registration deadlines and Election Day, and in-person trips to the voting booth or post office.

Relatedly, Millennials and Gen Z-ers are also less comfortable using the postal service. A recent USPS report found that young people receive less mail, pick up their mail less frequently, and use the mail to pay their bills less often than members of older generations (USPS 2018). Young people also hold "strong preferences for electronic alternatives to mailed correspondence and bill payment" and believe that sending mail is "conceptually easy" but a "laborious" process. Presumably, young people are also less likely to own stamps, which they may need (or at least *think* they need) to cast a ballot by mail (Armitage 2018). These findings suggest that young people might find it disproportionately difficult to participate in mail-based elections programs, such as those that let people mail in their registration forms or vote by mail. With more states considering all-mail elections, this possibility is particularly concerning.

The current cohort of young people is also driving less. A University of Michigan study found that the percentage of young adults (ages 20-24) with driver's licenses declined steadily from 92% in 1983 to just 77% in 2014. Older Americans, by contrast, drive at far higher rates; in 2014, more than 92% of individuals ages 60-64 had a license (Sivak and Schoettle 2016). The two most common reasons why young adults forgo driving speak to their limited resources: 37% cited being too busy or not having enough time to get a license, and 32% cited the cost of vehicle ownership and maintenance (Schoettle and Sivak 2013). Unsurprisingly, young people are also overrepresented among public transportation users (Anderson 2016).

Young people's shift away from driving has several potential ramifications for their turnout. First, when they skip visiting the DMV to get a license, they cannot be asked to register to vote by a DMV employee; this should lower the overall youth registration rate. Second, their marginal costs of traveling to registration and voting sites should increase; this may help explain

why, in one study of young non-voters, 30 percent cited transportation issues as a reason why they did not vote (CIRCLE 2018). Third, young people without driver's licenses may fear getting turned away at the polls due to lack of valid voter ID, even if their jurisdiction does not actually require a driver's license to vote (Ember 2018).

Another reason why today's young people may face higher voting costs is that they disproportionately belong to Black, Hispanic, Asian, and other minority racial and ethnic groups. The current cohort of young Americans is the most diverse in our country's history (Fry and Parker 2018). Previous research suggests that people of color face greater barriers to voting than whites (Pettigrew 2017, Rosenstone and Hansen 1993, Stewart 2012). (In the regression tables included in Appendix B, I repeatedly find that people of color face higher voting costs than white people, even after controlling for family income and education.) Racial-ethnic groups may also face qualitatively *different* voting costs as a result of engaging differently with registration and voting processes. According to a recent analysis of US Census data by the Voter Participation Center, Black youth are less likely to register online and more likely to vote in person than young Asians or whites (Lake et al. 2017). When asked why they didn't vote, Black youth are disproportionately likely to cite being uninterested or feeling their vote does not matter; by contrast, young Latinos are more likely to cite being busy, and young Asian Americans are more likely to cite being away from home or out of town (p. 63-64). When explaining why they are not *registered* to vote, young Asian Americans are more likely to say they do not meet residency requirements, whereas Latino youth disproportionately say they do not know where or how to register (p. 127).

Measuring Costs of Voting

Higher voting costs have long been recognized as reducing the likelihood that individuals will participate in elections. But the costs of voting have largely gone unmeasured in past research on voter turnout. A recent paper by Blais, Daoust, Dassonneville, and Péloquin-Skulski (2019) identifies just one previous study that attempts to directly measure voting costs in aggregate (Blais 2000), along with two studies that highlight variations in the cost of waiting in line to vote (Pettigrew 2017, Stewart 2012). To address this clear shortcoming of the literature, Blais and coauthors conduct two surveys to measure people's perceived costs of voting. Costs are broken into two components: the direct costs of voting, and the information costs associated with learning about parties and candidates and deciding for whom to vote; registration costs are not measured.

In one of the surveys, fielded in British Columbia and Quebec, subjects are asked to rate, on a scale from 1-10, how easy or difficult it is to go to the polling station (a measure of direct costs) or to make up their mind about the parties or party leaders (a measure of information costs). In the second survey, conducted in Canada, France, Germany, Spain and Switzerland, subjects are asked how easy or difficult it is to vote, using a four-point scale ranging from very difficult to very easy.

This study has several limitations, each of which highlights the need for an additional survey measuring voting costs. To begin with, the authors do not survey United States citizens or collect sufficient data to analyze how costs vary across age (or other demographic) groups. The study also does not attempt to measure registration costs, despite the fact that registration is a prerequisite for voting across the U.S.[26]

[26] The one exception is North Dakota, the only state without a registration requirement.

Another serious limitation of the Blais et al. study is that it relies on subjects to accurately assess and report the costs they face. The authors justify this approach to measuring costs by arguing that "what matters is whether individual citizens perceive voting to be easy or difficult" (p. 2). Certainly, perceptions of cost matter; an individual who thinks voting will be hard will be less likely to vote than someone who anticipates an easy voting experience, all else being equal. But people's *perceptions* of voting costs, especially as measured via a single question, may not fully capture the costs of registering and voting. Survey respondents may exert insufficient effort when answering the question, thinking only about the most salient aspects of the voting process—perhaps the actual act of casting a ballot at their polling place—and failing to incorporate other costs that are less salient, such as the time and energy required to locate their polling place, find a good time to vote, weigh various transportation options, and so forth. This should downwardly bias the average cost of voting reported by respondents.

Moreover, even if respondents do take the time to think through every step of the voting process, they may be so habituated to certain voting hassles, such as waiting in line at their polling place or buying stamps for their mail-in ballot, that they altogether fail to incorporate these into their calculation of voting costs. Finally, social desirability bias could also downwardly bias survey results: if respondents personally find voting difficult but attribute this to some individual shortcoming, they may feel embarrassed, leading them to respond to the cost question in a way they believe is more socially acceptable.[27]

A New Measurement Approach

To address these potential pitfalls and more effectively estimate the relationship between voting costs and turnout, I take a different approach to measuring costs. In a nationally representative survey, I asked respondents questions about the specific barriers they face at different stages of the registration and voting process, as well as the resources they have at their disposal to overcome those barriers. This more detailed and comprehensive approach avoids the problems associated with more typical, summary measures of voting difficulty that I have just described.

In an initial set of Likert-style questions, I asked whether respondents know the steps they need to take to register and vote, the location of their polling place, how to find information about candidates and issues on their ballot, and how to fill out a ballot. Individuals who lack this information are said to face "information costs" when registering or voting—that is, they must exert additional effort to learn the information they need to participate in elections. I next asked a series of questions aimed at uncovering ways in which young people might face disproportionate barriers related to time, scheduling, and work-life balance: whether they struggle to balance registering and voting with the other things they have to get done, to plan ahead to register and vote, and to find the time to register and vote.

Additional questions addressed whether transportation issues make it hard for them to register and vote, whether registration and voting use up time they could spend earning money instead (a measure of the opportunity cost of voting), and whether they already own the voter ID documentation needed to register and vote. I also asked about specific aspects of the voting

[27] Qualitative interviews conducted by Holbein and Hillygus (2020) suggest that social desirability bias could lead people to say voting is easier than it actually is. In their interviews, individuals appear to interpret difficulties with voting as a reflection of some personal shortcoming, rather than a structural problem. It is quite plausible that these individuals would report that the voting process is somewhat or very easy, even if they personally find it difficult to navigate.

process that may disproportionately burden young people: whether respondents feel that sending a registration form or ballot through the mail is a hassle, and whether they struggle to complete the parts of registering and voting that can't be done online. I supplemented these questions with more typical measures of voting costs, asking individuals to rate how easy or difficult registration and voting are for them personally.[28]

In addition to questions about voting costs, respondents were also asked a series of questions about registration and voting laws. First they were asked to identify, to the best of their knowledge, their state's approach to various aspects of elections, including registration deadlines and mail voting. Then they were presented with a series of potential voting reforms—including allowing people to register on Election Day at the polls, allowing people to vote ahead of Election Day, automatically registering all eligible citizens to vote, automatically updating voter registration when people move, conducting elections entirely by mail, and allowing people to vote online—and asked whether each reform would make things easier or harder for someone like them.

Respondents were also asked for their registration and voting history, whether they intend to vote in the 2020 presidential election, and for whom they intend to vote. YouGov, who recruited the sample of respondents, also provided demographic information for each respondent, including age, race and ethnicity, sex, educational attainment, and family income. This allowed me to control for these factors when investigating the relationship between age and voting costs, as well as to explore heterogeneous effects across subgroups.

Sampling and Data Collection

The survey was sponsored by the Berkeley Institute for Young Americans (BIFYA) and fielded by the polling firm YouGov between April 29 and May 13, 2020.

YouGov interviewed 2,448 voting-age American citizens, oversampling on young (18-29) and old (65+) individuals in order to improve statistical precision when comparing findings across age groups. Interviewed individuals were then matched to a nationally representative sampling frame constructed from the 2016 American Community Survey 1-year sample, resulting in a final sample of 2,270 respondents. These individuals were then weighted to the ACS sampling frame using propensity scores that took into account their age, gender, race/ethnicity, years of formal education, and region, and then post-stratified according to these propensity scores. Finally, the weights were post-stratified on 2016 presidential vote choice and a four-way stratification of gender, age, race, and education, in order to produce the final weights.[29] These careful sampling and weighting protocols make it possible to generalize my survey findings to the broader universe of voting-age Americans.

Variables were recoded and rescaled for easier interpretation of survey results. Respondents were categorized into six age groups (18-24, 25-29, 30-39, 40-49, 50-64, and 65 and older), five racial/ethnic groups (white, Black, Hispanic, Asian, and other), and four

[28] Due to space limitations, I did not ask respondents about two important attributes described earlier in this paper that should affect voting costs: whether they have moved recently, and whether they lack a driver's license. Both theory and common sense support the idea that moving and lacking a driver's license should increase one's voting costs and reduce their likelihood of turning out, and other researchers have already documented that young people are more mobile than older age groups and less likely to hold a driver's license.

[29] More information on the matching, stratification, and weighting procedures used to conduct the BIFYA/YouGov survey can be obtained by emailing Charlotte Hill at charlottehill@berkeley.edu.

education groups (high school or less, some college, bachelor's, and post-graduate).[30] All Likert-scale items were converted to binary variables to make it easier to interpret results.

I coded data on state election laws based on publicly available information from the National Conference of State Legislatures.[31] Individuals who correctly identified a policy their state had adopted were coded as being "correctly informed" about that policy. Individuals who incorrectly identified their state's policy were coded as being "misinformed." Those who were not sure of their state's policy were coded as being "unsure."

Results

Costs of Registration and Voting

For each of the thirteen indicators of registration and voting costs included in the survey, young people reported facing higher voting barriers and having access to fewer voting resources than other age groups, particularly senior citizens.

Respondents were asked to read a series of statements and to answer whether they agreed or disagreed using a 7-point scale. For ease of interpretation, dependent variable values were re-coded on a three-point scale: 0 = disagree, 0.5 = neither agree nor disagree, and 1 = agree.

Information Costs

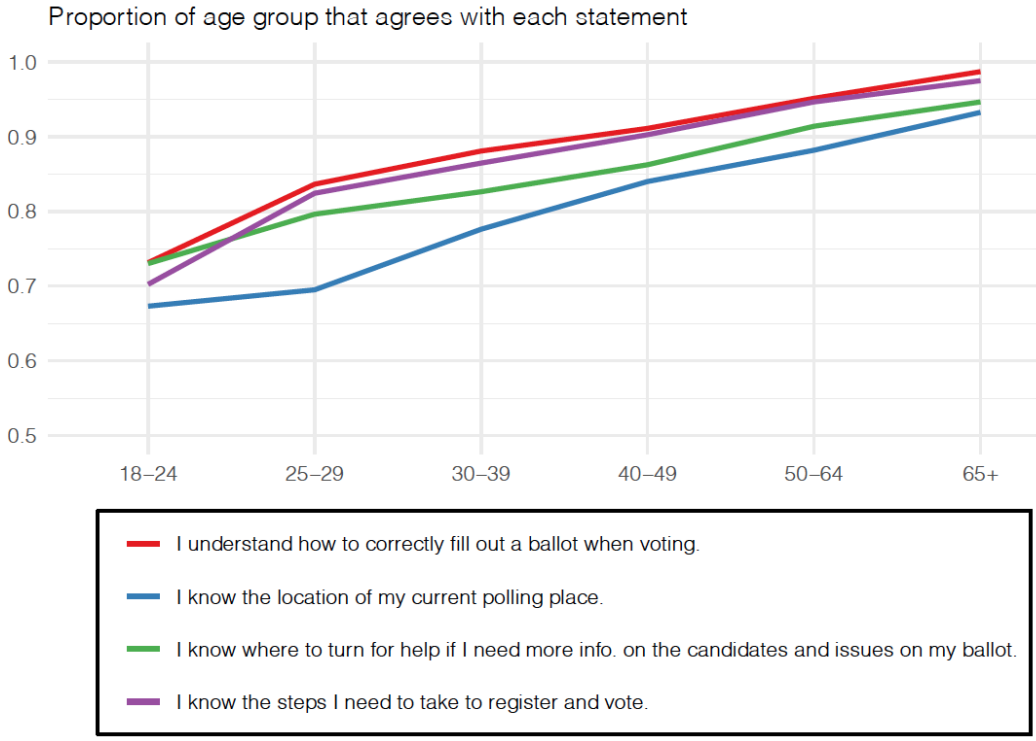
The first set of statements had to do with the costs of learning how to vote—both how to navigate the electoral process, and also how to make informed voting choices.

1. I understand how to correctly fill out a ballot when voting.
2. I know the steps I need to take to register and vote.
3. I know where to turn for help if I need more information on the candidates and issues on my ballot.
4. I know the steps I need to take to register and vote.

[30] One important reason for dividing young people into two age groups (18-24 and 25-29) was to better capture any differences in voting costs that might stem from their being at meaningfully different stages in the life cycle.

[31] State laws were coded according to their status at the time of the survey (April 29-May 13, 2020); since then, several states have changed their voting policies, especially around mail and absentee voting.

Figure 1: Young People Less Likely to Have the Information They Need to Vote



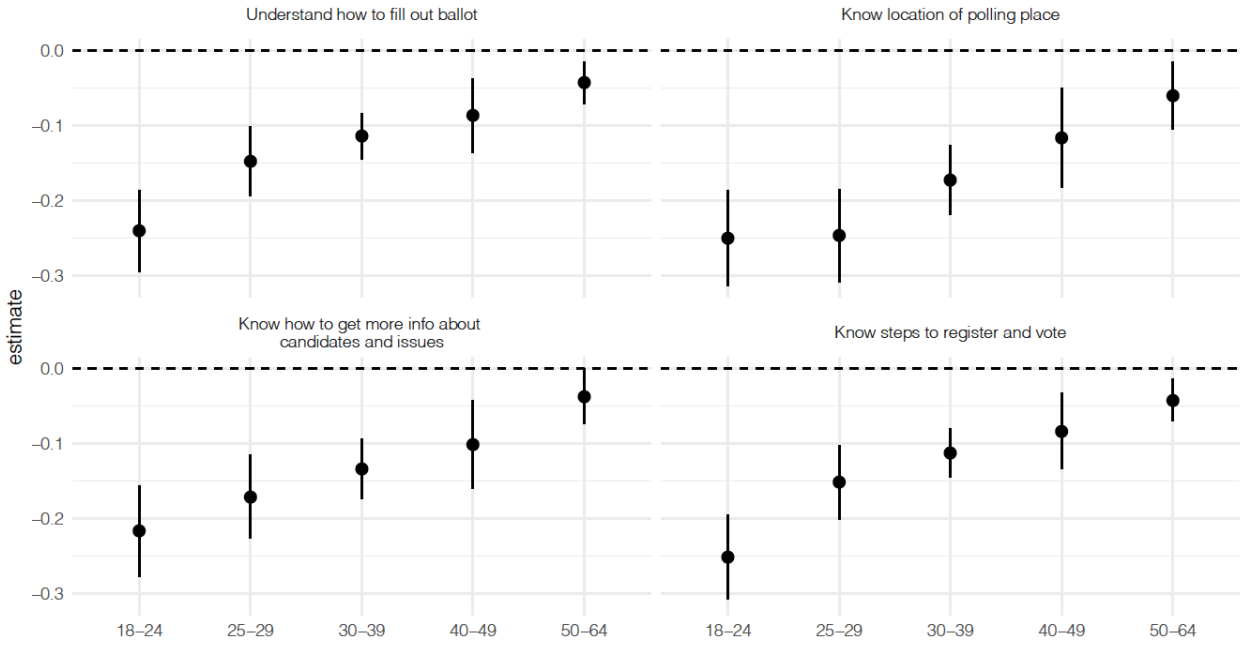
All age groups tend to agree with these statements. However, young people are less likely to agree than older people, as shown in Figure 1. The parallels across measures are striking: the youngest people (ages 18 to 24) are the least likely to agree with each of the four measures of voting information, while voting information is at its highest levels for people 65 and older.

A series of linear regressions confirm that young people have significantly lower levels of voting information than people 65 and older, as shown in Figure 2 (full regression results are shown in Appendix Table B15).[32] After adjusting for race and ethnicity, educational attainment, gender, and family income, every age group is significantly less likely than seniors to know key voting information.

[32] There is debate over whether linear regressions or ordinal logit (or probit) regressions are best-suited for analyzing results of Likert survey items (e.g., Bishop and Herron 2015; Knapp 1990). To help address this concern, I first recode all Likert scale-type dependent variables as binary outcomes, eliminating issues over how to interpret *spacing* between levels while preserving some of the information conveyed by the *order* of the levels. Per Hellevik (2009), I then use linear regression to regress these binary outcomes on a series of covariates. As a robustness check, all models were also run using ordinal logistic regression. All substantive results remained the same. Full results of the ordinal logistic regressions can be found in Appendix Tables A1-A14.

Figure 2: Young Adults Least Likely to Have the Information They Need to Vote

Estimated effect of age on likelihood of agreeing with statement, relative to people 65 and older.



Note: Estimates and 95% confidence intervals are from a generalized linear regression on age group, with controls for educational attainment, gender, race and ethnicity, and family income.

Information barriers are highest for 18- to 24-year-olds, who are 23 percentage points less likely than seniors to understand how to correctly fill out a ballot, 24 percentage points less likely to know the location of their polling place, 21 percentage points less likely to know where to turn for help to find more information on the candidates and issues on their ballot, and 24 percentage points less likely to know the steps they need to take to register and vote. When these four measures are combined into a scale, the average 18-24-year-old scores 22 percentage points lower on voting information than the average senior. Substantively, the magnitude of these relationships—in either individual or scale form—are larger than those for race, education, gender, or family income alone.

Time and Scheduling Costs

Next, respondents were asked to read a set of statements about time and scheduling barriers to voting.

1. Around election time, I have difficulty balancing registering and voting with all the other things I have to get done.
2. My schedule is so unpredictable that it's hard to plan ahead to register and vote.
3. It's hard for me to find the time to register and vote.

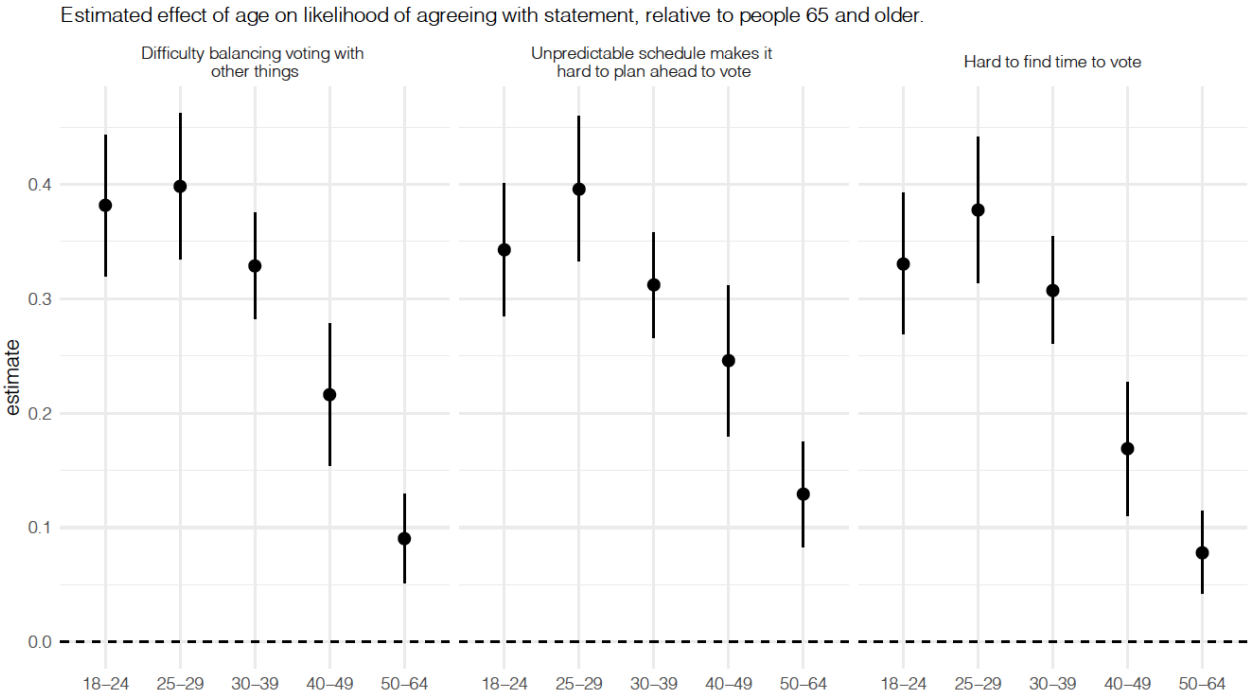
While all age groups tend to disagree with these statements, young people are much more likely to report facing these particular voting barriers than older people, as shown in Figure 3. Around 4 in 10 people in their twenties agree that they have difficulty balancing voting with other tasks, that unpredictable schedules make it hard to plan to vote, and that it is hard to find

the time to vote. Agreement declines with age, with 95 percent of individuals 65 and older disagreeing with these statements.

Figure 3: Young Adults Face Greater Time and Scheduling Barriers to Voting

After adjusting for race and ethnicity, education, sex, and income, being young is a significant predictor of higher time, scheduling, and work-life balance costs (Figure 4; see full regression results in Appendix Table A16). Relative to those 65 and older, young adults ages 18-24 and 25-29 are more likely to have difficulty balancing registering and voting with other tasks (+38 and +40 percentage points, respectively), to say it is hard to find the time to register and vote (+33 and +38 percentage points), and to say their schedule is so unpredictable that it's hard to plan ahead to register and vote (+34 and +40 percentage points). As before, these relationships are substantively much larger than those for race, education, income, and gender.

Figure 4: Young Adults Face Greater Time and Scheduling Barriers to Voting



Note: Estimates and 95% confidence intervals are from a generalized linear regression on age group, with controls for educational attainment, gender, race and ethnicity, and family income.

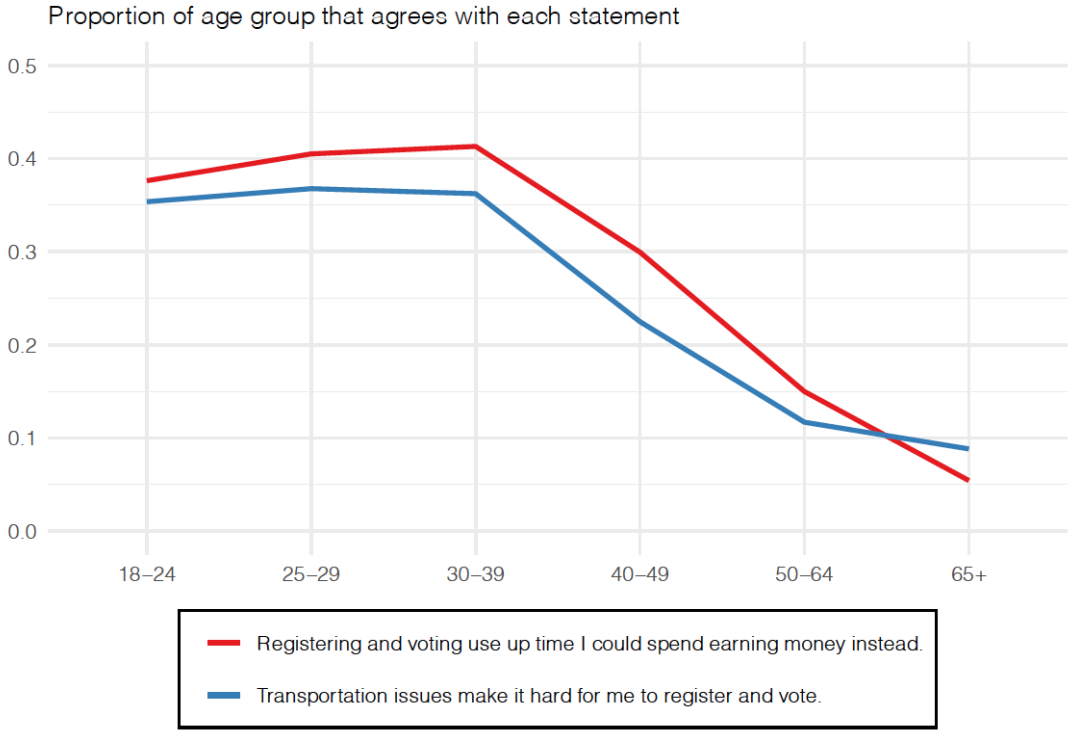
Transportation and Opportunity Costs

Two additional statements addressed the transportation and opportunity costs associated with voting. Because voters are not technically required to pay to vote, these costs represent key ways in which voters must draw on personal resources—or forego the opportunity to increase their resources—in order to vote.

1. Registering and voting use up time I could spend earning money instead.
2. Transportation issues make it hard for me to register and vote.

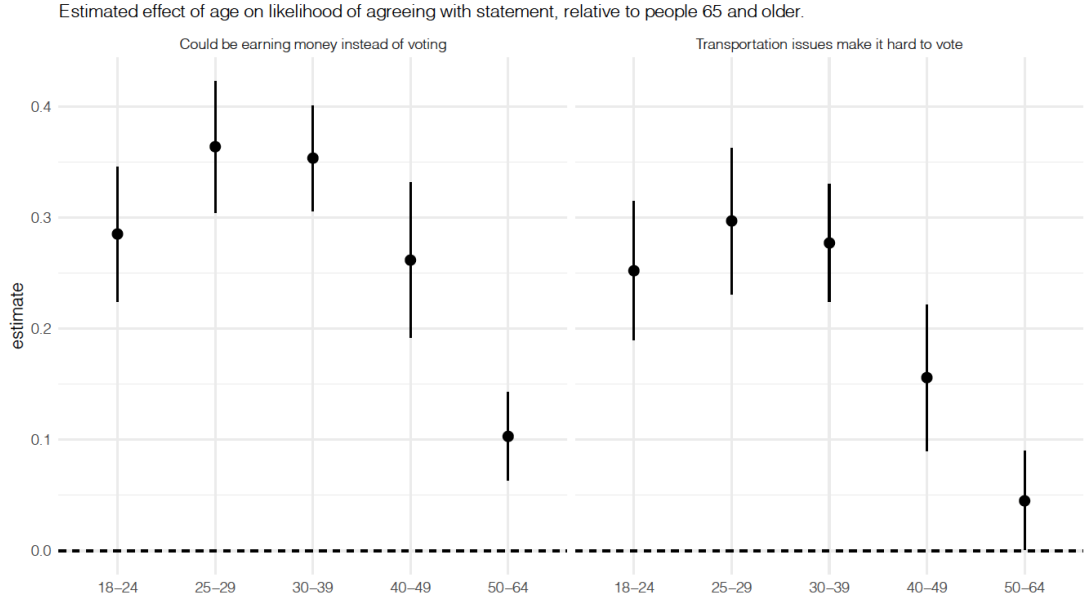
Figure 5 shows a similar pattern to the results presented earlier. Young people are most likely to agree with both of these statements: around 4 in 10 of people in their twenties and thirties face transportation and opportunity costs, compared to fewer than 1 in 10 seniors.

Figure 5: Young Adults More Burdened By Transportation and Opportunity Costs



Differences in transportation and opportunity costs by age group are statistically significant, as shown in Figure 6. After standard controls, young Americans ages 18-24 and 25-29 are between 25 and 30 percentage points more likely to agree that transportation issues make it hard for them to register and vote and between 29 and 36 percentage points more likely to agree that registration and voting use up time they could spend earning money instead. People in their 30s are similarly more likely to agree with these statements. Again, these effect sizes are much larger in magnitude than the effects for race, education, gender, and income, as shown in Appendix Table B17.

Figure 6: Young People More Burdened by Transportation and Opportunity Costs



Note: Estimates and 95% confidence intervals are from a generalized linear regression on age group, with controls for educational attainment, gender, race and ethnicity, and family income.

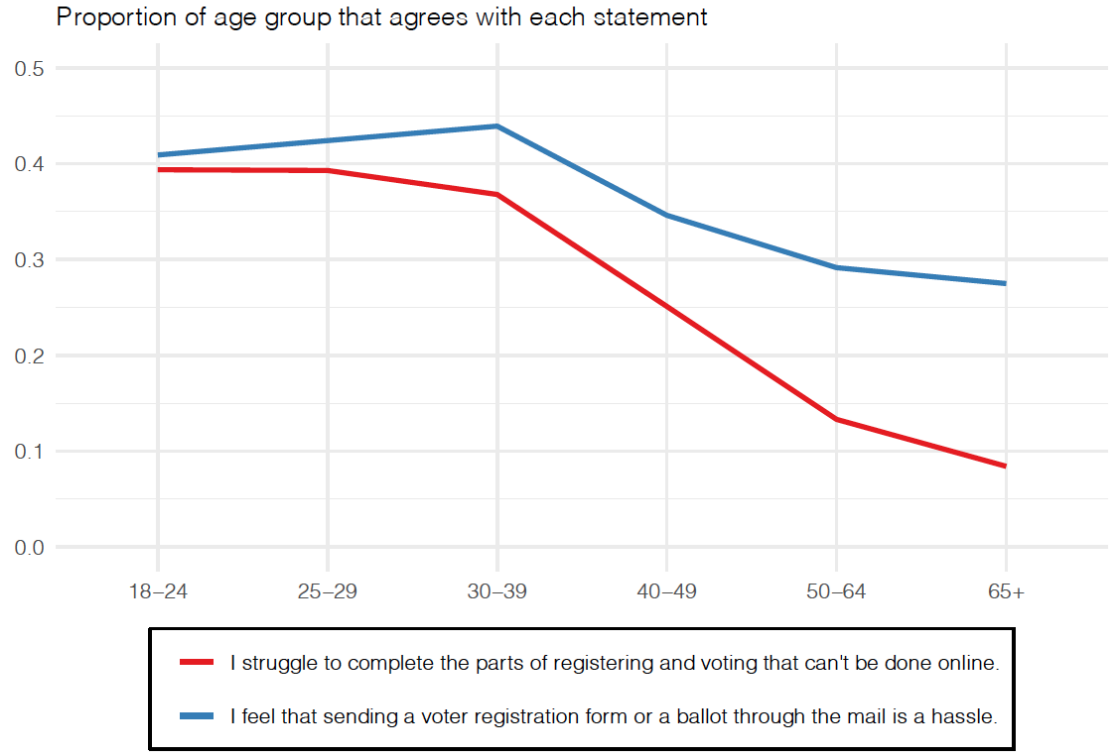
Voting Procedure Costs

Respondents were also presented with two statements about America's voting procedures.

1. I struggle to complete the parts of registering and voting that can't be done online.
2. I feel that sending a voter registration form or ballot through the mail is a hassle.

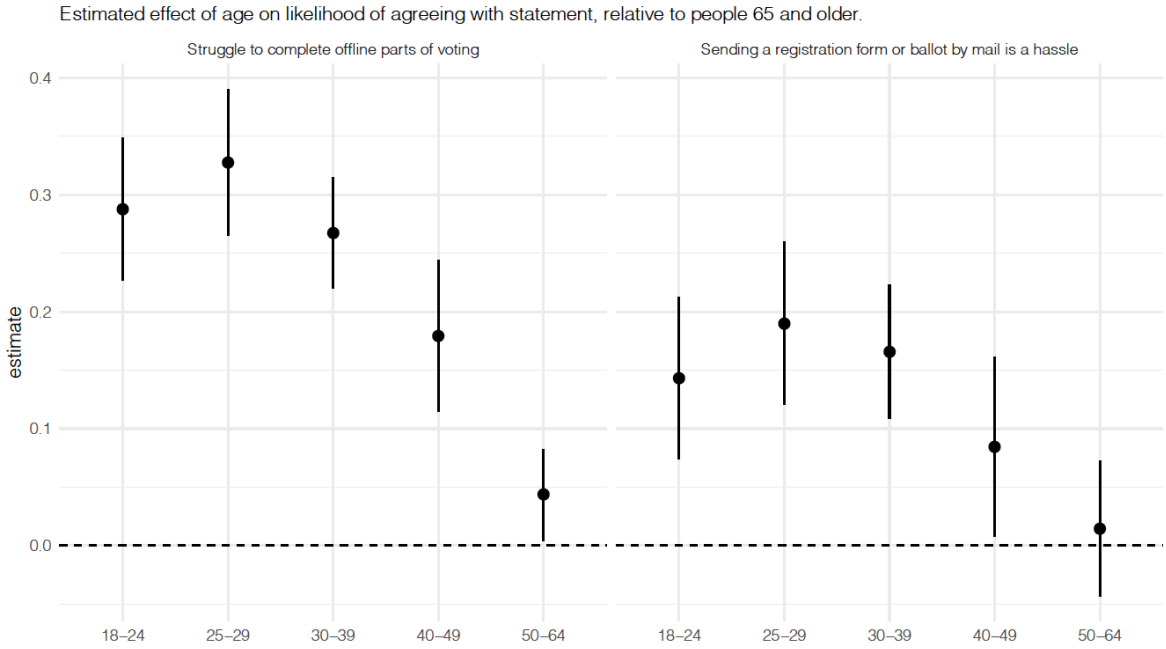
Young people once again report higher agreement with these statements, as shown in Figure 7. Around 40 percent of 18 to 24-year-olds, 25 to 29-year-olds, and 30 to 39-year-olds feel burdened by having to complete some registration and voting tasks offline and by the mail-in voting process. Agreement falls somewhat for those in their forties and reaches its lowest levels for people in their fifties and older, with seniors far less likely than young adults to report struggling with offline and mail voting procedures.

Figure 7: Young Adults More Burdened by Offline Voting Procedures, Mail Voting



After adjusting for standard controls, these differences by age are statistically significant and large in magnitude, as shown in Figure 8 (see Appendix Table B18 for full regression results). 18- to 24-year-olds are 29 percentage points more likely to struggle to complete the parts of registering and voting that can't be done online and 14 percentage points more likely to feel that sending a registration form or ballot through the mail is a hassle. Costs appear to be even higher for 25- to 29-year-olds, who are 33 and 19 percentage points more likely than seniors to struggle with offline and mail-based voting procedures, respectively. Results are similar for people in their thirties, as well.

Figure 8: Young Adults More Burdened by Offline Voting Procedures, Mail Voting

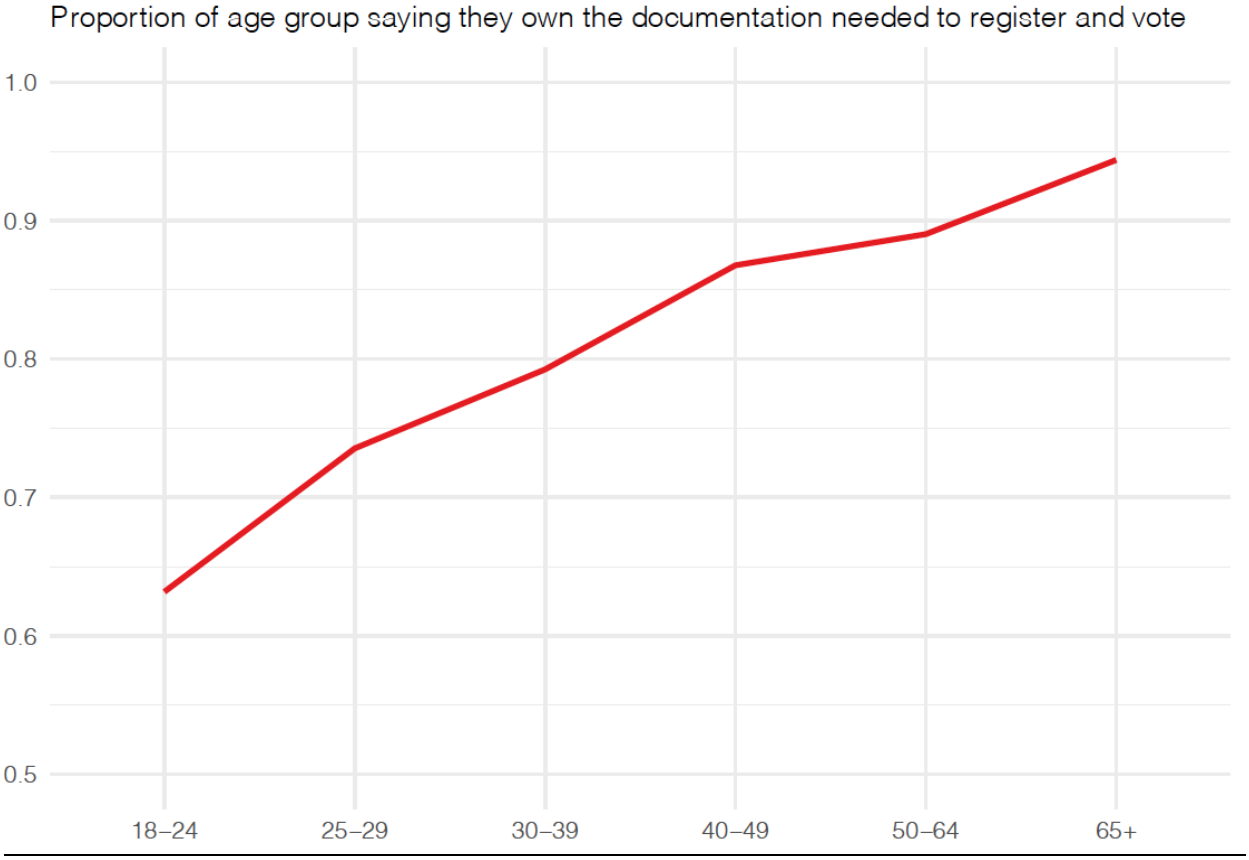


Note: Estimates and 95% confidence intervals are from a generalized linear regression on age group, with controls for educational attainment, gender, race and ethnicity, and family income.

Voter Identification Costs

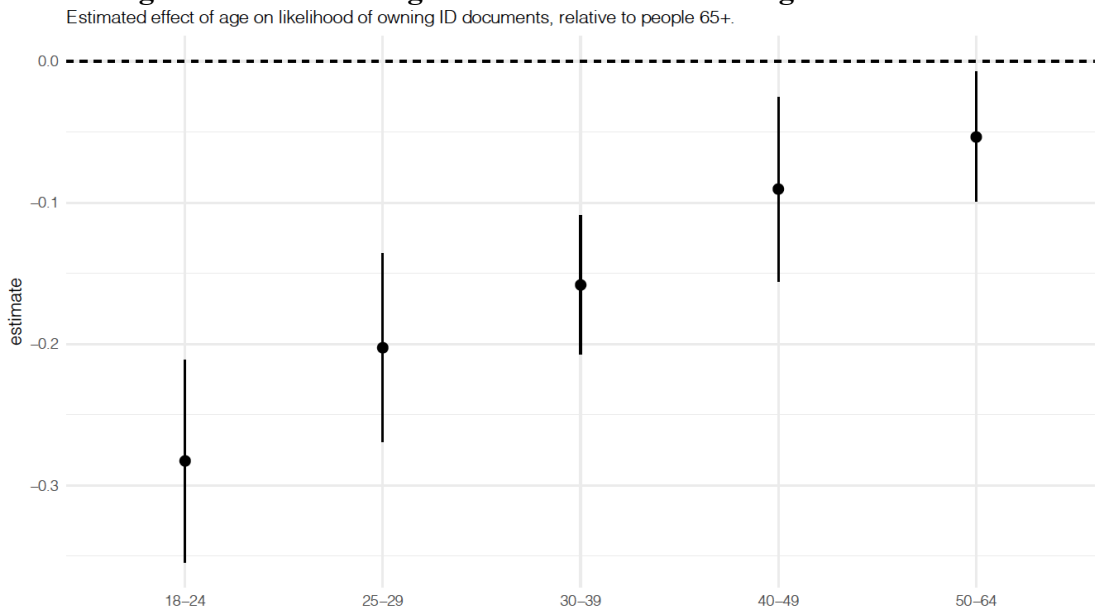
Respondents were next asked whether they own the documentation they believe they need to register and vote. Figure 9 shows that just 63 percent of young people said they "probably" or "definitely" already have this documentation, compared to nearly 95 percent of seniors.

Figure 9: Young Adults Less Likely to Own Voter ID Documentation



After adjusting for race, education, gender, and income, young adults between 18 and 24 are a full 28 percentage points less likely than seniors to say they have the voter ID documentation they believe they need to register and vote, as shown in Figure 10. Young adults ages 25-29 are around 20 percentage points less likely than seniors to have this documentation. These differences are statistically significant (Appendix Table B19).

Figure 10: Effect of Age On Likelihood of Owning ID Documents

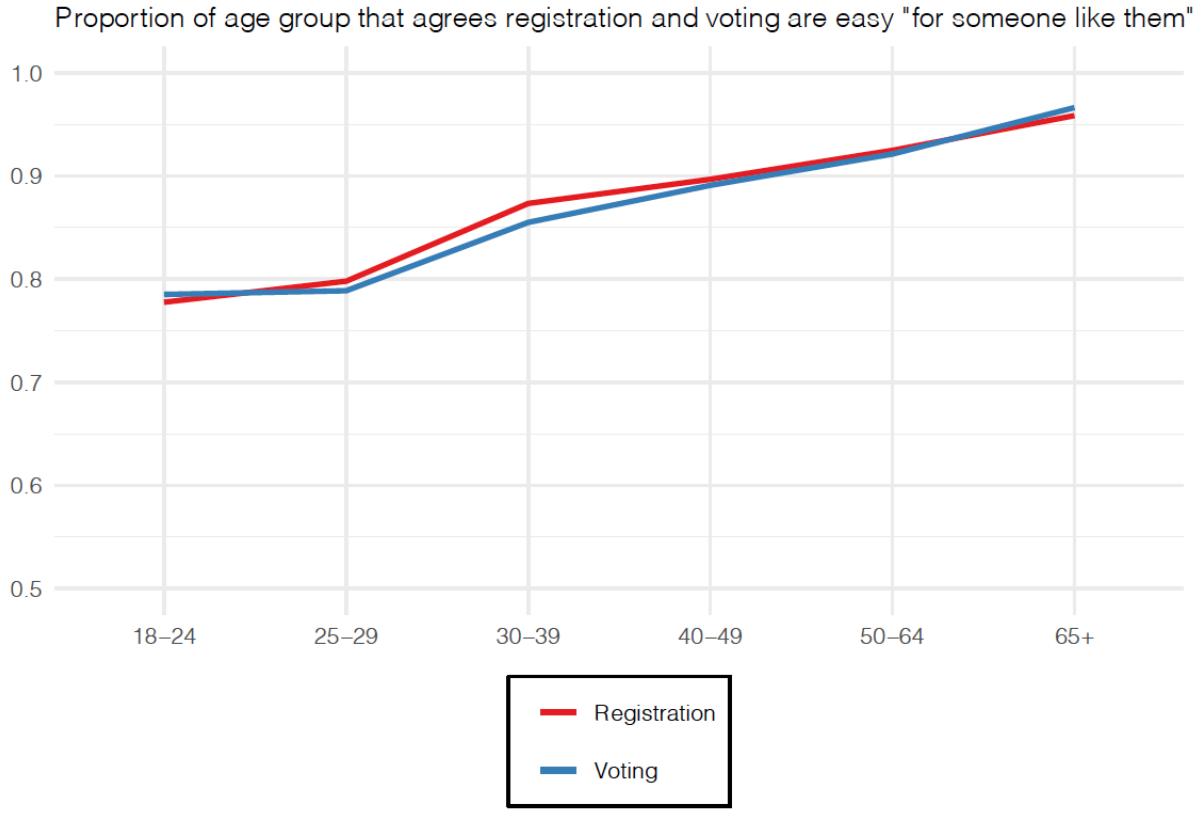


Note: Estimates and 95% confidence intervals are from a generalized linear regression on age group, with controls for educational attainment, gender, race and ethnicity, and family income.

Perceived Ease of Voting

After being asked these questions about voting and registration information and barriers, respondents were asked to reflect on "how easy or difficult it is for someone like you to register to vote" and "to vote." Overall, most people answered that registration and voting are easy (Figure 11). However, answers once again differed across age groups. Just under 80 percent of people between 18 and 24 say that registration and voting are easy for someone like them, compared to more than 95 percent of people 65 and older, a 15 percentage-point gap.

Figure 11: Young Adults Less Likely to Agree that Registration and Voting Are Easy



Notably, unregistered individuals are much less likely than their registered counterparts to find registration and voting easy (Figure 12).[33] This is not especially surprising: those who perceive the registration process as easy are more likely to complete it. Yet the gap in perceived ease between registered and unregistered people is much larger among youth. More than 90 percent of registered youth believe voter registration and voting are easy, compared to just 60 percent of unregistered young people who feel the same. For seniors, this gap between registrants and unregistered people remains, but it is much smaller (17 percentage points for registering and 11 percentage points for voting).[34]

This difference by age suggests that costs may be a more important reason for the registration gap among young people than among older people. That is, younger people may be more likely to remain unregistered because they find the registration process complicated or difficult, whereas older people may perceive the registration process as relatively easy but stay unregistered for other reasons, such as a lack of interest.

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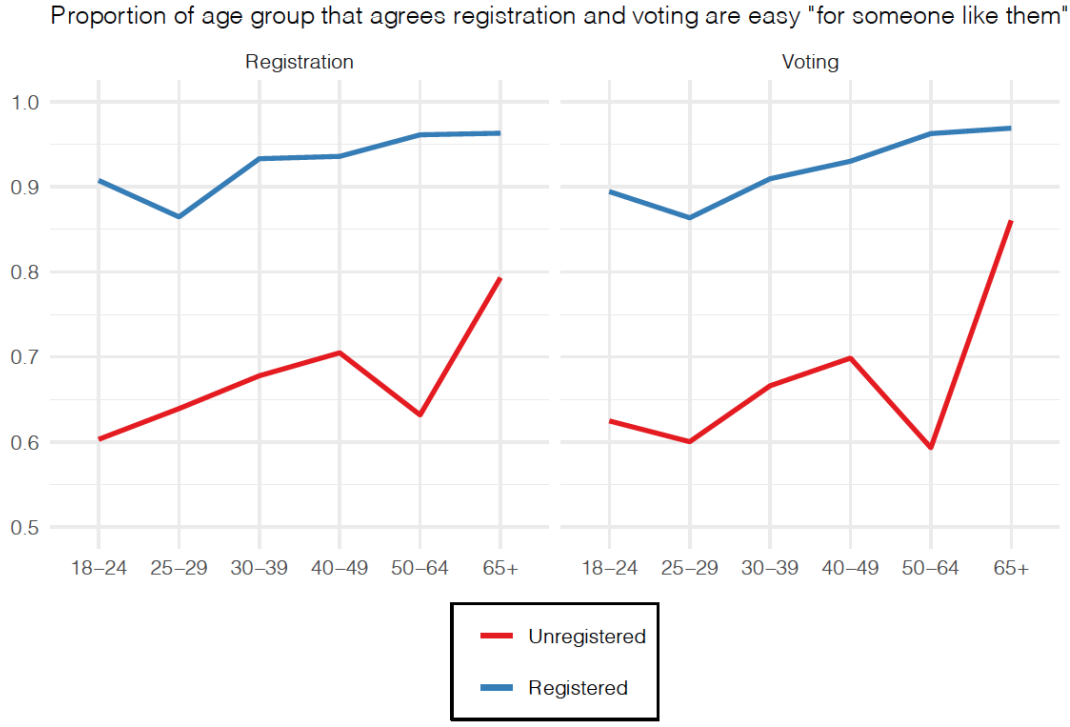
[33] I also analyze how each individual voting cost varies by registration status. When data are stratified by registration status, voting costs appear higher for unregistered young people than for their registered counterparts. Unregistered youth are less likely than registered youth to have any of the four types of voting information (Appendix Figure B1) and more likely to face every other voting barrier measured by the survey (Appendix Figure B2). However, perhaps due to the relatively small sample size of this survey, differences in reported costs by the interaction of age and registration status do not reach the level of statistical significance.

[34] Despite the relatively small sample size, a regression analysis interacting age with registration status confirms a statistically significant difference ($p<.05$) in perceived ease of voting between unregistered and registered young adults and a nearly significant difference ($p<.10$) in perceived ease of registration (Appendix Table B20).

Figure 12: Ease of Registering and Voting Varies by Registration Status and Age



Yet even after controlling for registration status, young Americans are still less likely than older people to perceive registration and voting as easy. In other words, registration status alone does not account for why young people disproportionately believe voting is difficult.

Age, Education, Race, and Voting Costs

To what extent can the differences in voting costs across age groups be explained by young people's lower levels of formal education and greater racial diversity? Some evidence on these questions can be found in the linear regression results presented in the previous section. After accounting for education and race, voting costs tend to be higher for young people than for seniors. For every voting barrier explored above, adjusting for race and education only slightly reduced the coefficient on age.[35] In other words, being young appears to have a meaningfully large and statistically significant effect on the cost of voting, above and beyond the direct effects of race and education.

To shed additional light on this question, I predict young people's voting costs for two scenarios: one in which they have their existing levels of racial diversity and educational experience, and one in which they share the racial and educational composition of senior citizens.[36] Interestingly, shifting from their true racial and educational makeup to that of seniors

[35] Data to this effect is available upon request.

[36] For this analysis, I first calculate young people's and seniors' average weighted proportions of white, Black, Asian, Hispanic, and 'other' members. I do the same for educational attainment. I then use these average proportions to predict average voting costs for 18-24-year-olds and for seniors 65+ according to the following model:

$$cost \sim \beta1*age\_group + \beta2*race\_black + \beta3*race\_hispanic + \beta4*race\_asian + \beta5*race\_other + \beta6*some\_college + \beta7*bachelors + \beta8*post\_grad$$

has only a small effect on predicted costs; for instance, when young people adopt the characteristics of seniors, they become only 2 percentage points more likely to have the information they need to register and vote, leaving a 22 percentage-point gap between age groups.

One interpretation of these findings is that race and education play only a limited role in shaping how voting costs are distributed across age groups. Yet both race and education matter immensely for at least one critical step in the voting process: registration. After adjusting for age, gender, and family income, both race and education continue to be meaningfully large and statistically significant predictors of whether a person registers to vote. Compared with white individuals, Black, Hispanic, and Asian people are 11, 12, and 19 percentage points less likely to be registered, respectively. Compared to those with only a high school education or less, people with some college experience, a bachelor's degree, or a postgraduate degree are 12, 13, and 12 percentage points *more* likely to be registered (Appendix Table B21). Registration is, in turn, an important predictor of voting costs, with unregistered Americans consistently facing higher costs than their registered counterparts. To the extent that being more racially diverse and lower-education directly reduces young people's registration rates, this may have the downstream effect of increasing their costs at later stages of the voting process.

Predicting Registration and Voter Turnout

For each of the voting cost indicators identified above, young people appear to be disproportionately burdened, relative to older age groups. But how much do these costs actually matter? That is, how predictive are these costs when it comes to real-world voting behavior?

I use each voting cost indicator to predict the likelihood of turning out to vote in the 2018 midterm election, as well as currently being registered to vote. For all indicators except one—considering mail voting to be a hassle—voting costs are significant predictors of turnout, after adjusting for age, race, education, gender, and income. (Including age as a control variable is important; otherwise, we could not know whether a significant relationship between voting costs and turnout was in fact due to other age-related factors.) Similarly, all voting cost indicators, including thinking mail voting is a hassle, predict a lower likelihood of being registered to vote (Appendix Figures B3-B4).

Information Costs

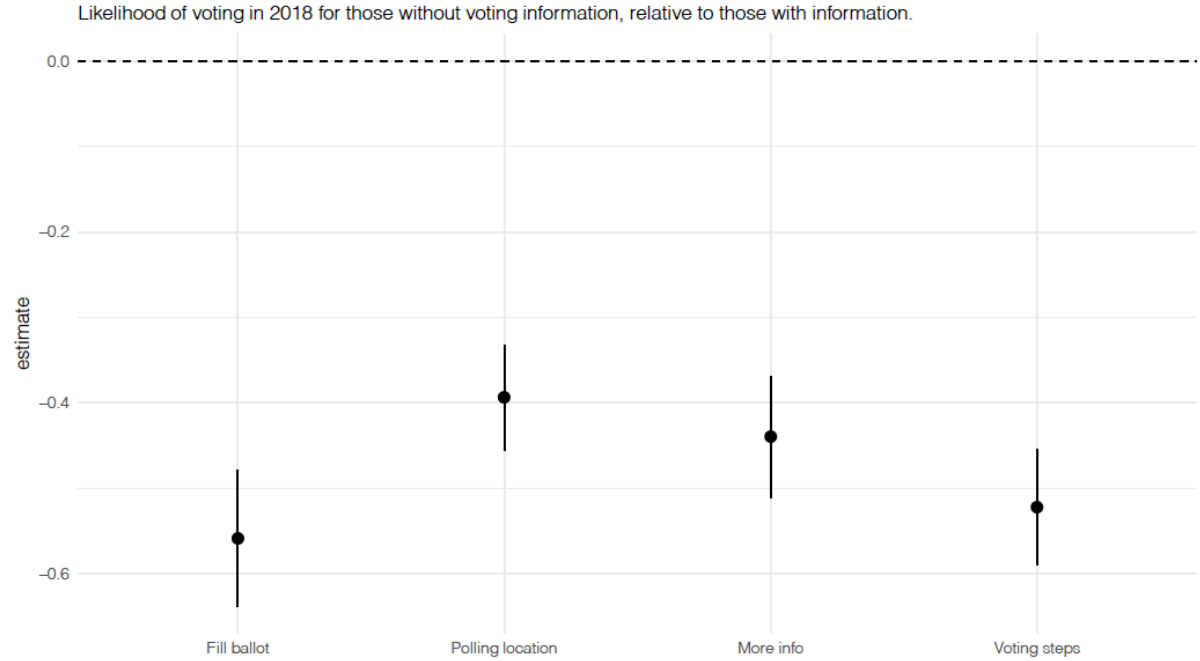
Information costs are the biggest predictor of low turnout. As Figure 13 shows, people who do not know the steps of registering and voting are 53 percentage points less likely to have voted in 2018 than those who report knowing this information. People who do not know the location of their polling place are 40 percentage points less likely to have voted. Not knowing where to find more information about candidates and issues decreases their likelihood of voting by 45 percentage points. And not knowing how to fill out a ballot correctly is associated with a 57 percentage-point decrease in the likelihood of voting.

In the original model predicting voting likelihood using age, race, gender, educational attainment, and family income, being 18-24 is associated with a 53 percentage-point decrease in the likelihood of voting, relative to being a senior. Being 25-29 is associated with a 37 percentage-point decrease. Separately adjusting for each information cost shrinks the effect of

I first calculate true cost levels, plugging in each age group's true race and education proportions. I then construct counterfactuals for each age group using the other group's race and education proportions. Finally, for each age group, I compare its true predicted costs against its counterfactual costs.

age for these groups by between 15 and 30 percent. For instance, adjusting for knowledge of how to correctly fill out a ballot reduces the coefficient of "age 18-24" from -0.53 to -0.40, a 25% reduction.

Figure 13: People Who Lack Voting Information Less Likely to Have Voted in 2018



Note: Estimates and 95% confidence intervals are from a generalized linear regression on a binary variable representing whether an individual lacks voting information, with controls for age, educational attainment, gender, race and ethnicity, and family income. The binary variable is coded as 0 for individuals who agree that they know the voting information in question, 0.5 for individuals who neither agree nor disagree, and 1 for individuals who disagree.

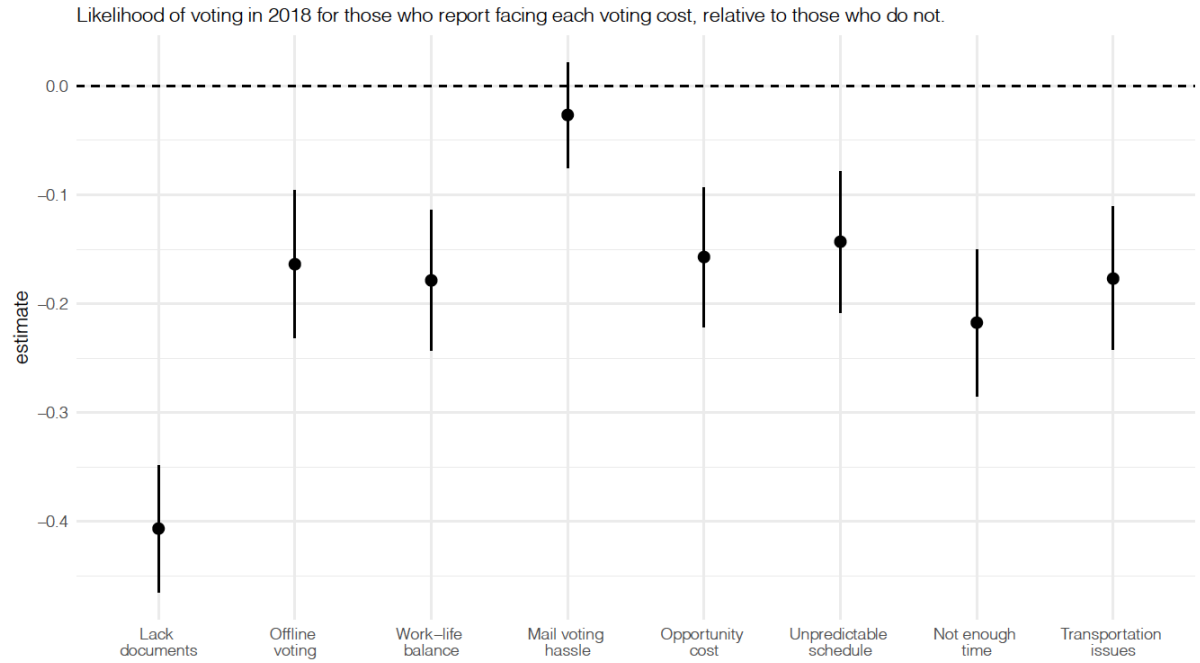
Other Voting Costs

Other voting costs also predict lower turnout (Figure 14). After adjusting for standard controls including age, people who struggle with work-life balance are 19 percentage points less likely to have voted. Not having enough time is associated with a 23 percentage-point decrease in the likelihood of having turned out. An unpredictable schedule predicts a 15 percentage-point decrease. Struggling with transportation reduces one's likelihood of having voted in 2018 by 19 percentage points; people who report facing a financial opportunity cost to voting are 16 percentage points less likely to have voted. Those who say they struggle with the offline parts of voting are 17 percentage points less likely to have voted than those who do not. Most notably, those who report lacking the documentation they need to vote are a full 42 percentage points less likely to have voted in 2018 than those with proper documentation.

The one exception has to do with perceptions of mail voting. Before adjusting for age, the belief that mail voting is a hassle predicts a 9 percentage-point decrease in turnout likelihood—but once age is added in as a predictor, this effect drops to 3 percentage points and loses statistical significance. This could suggest that efforts to make mail voting less complicated for voters may be less consequential than other cost-reducing reforms. However, it is important to remember that voter turnout was measured in 2018, before the pandemic-induced rapid spread of mail voting throughout the U.S. It may be that individuals' struggles with mail voting were not

strongly associated with turnout in 2018 because voters had other straightforward options for how to cast a ballot, but that, in 2020, this cost took on new significance.

Figure 14: Higher Voting Costs Predict Lower Likelihood of Having Voted in 2018



Note: Estimates and 95% confidence intervals are from a generalized linear regression on a binary variable indicating whether an individual faces a given voting cost, with controls for age, educational attainment, gender, race and ethnicity, and family income. The binary variable is coded as 0 for individuals who disagree that they face the cost in question, 0.5 for individuals who neither agree nor disagree, and 1 for individuals who agree.

Adjusting for these voting costs shrinks the relationship between being young and turning out to vote. These reductions range from 6% to 27%, depending on the cost. At the low end, adjusting for a lack of transportation or for facing an opportunity cost to voting reduces the coefficient on being 18-24 by 6%. At the high end, adjusting for not having enough time to vote shrinks the coefficient on being 25-29 by 27%. One way to interpret these changes is that the voting costs themselves, as opposed to some other characteristic of young age, seem to be doing "the work" of reducing turnout. Once the costs are controlled for, age is less effective at predicting voting rates.

Expectations of Voting Reforms

To what extent do today's young people believe that registration and voting reforms would make things easier for them? To answer this question, respondents were asked to reflect on a series of voting reforms and mark how much easier or harder they think each would make things for them. To help respondents understand the likely impact of each policy, and to avoid a situation in which respondents simply supported policies favored by their chosen political party, I described these reforms in detail instead of listing them under their common names (e.g., "letting people vote in advance of Election Day" instead of "early voting"). Below is the list of policies as described in the survey; the common names listed in parentheses were not included.

1. “Letting people register to vote on Election Day at their polling place” (same-day registration)
2. “Letting people vote in advance of Election Day” (early voting)
3. “Sending all voters a ballot that they can cast by mail or in person” (mail voting)
4. “Automatically registering eligible people to vote when they interact with the DMV and other government offices” (AVR)
5. “Automatically updating people’s voter registration when they move” (updating registration after moving)
6. “Letting people vote over the internet” (internet voting)

While I expect most people believe these reforms would make things easier for them—a general reflection of broad popular support for voting reforms—I anticipate greater youth support for reforms that would reduce costs disproportionately borne by young adults. As shown earlier in this paper, young people face greater time, transportation costs, and opportunity costs of voting. The three registration-focused reforms described above—SDR, AVR, and updating registration after moving—would reduce these costs by eliminating the need for at least some voters to travel to a registration site such as the DMV and fill out paperwork. They would also reduce the need to plan ahead and balance voter registration with other tasks, two things that young people disproportionately struggle with. I also expect young people to be more likely to believe that internet voting would make things easier, as they report higher levels of difficulty with offline voting. (Internet voting would also reduce time and transportation costs, further benefiting young people.)

By contrast, I anticipate young adults to be *less* likely than older Americans to believe mail voting would make things easier. Youth disproportionately find voting by mail to be a hassle—despite new research indicating that, once implemented, youth turnout can increase under all-mail voting regimes (Bonica et al. 2020). Similarly, I anticipate that young people will not be more likely than seniors to expect early voting to make things easier for them. Early voting is generally seen as benefiting those with predictable lives, who can take advantage of the policy to plan ahead to vote on the most convenient day. It is less beneficial for those whose chaotic schedules prevent them from planning ahead—that is, young people. Supporting this contention, empirical research suggests that early voting primarily boosts turnout for older Americans (Lyons and Scheb 1999).

Figure 15: All Age Groups Believe Voting Reforms Will Make Things Easier

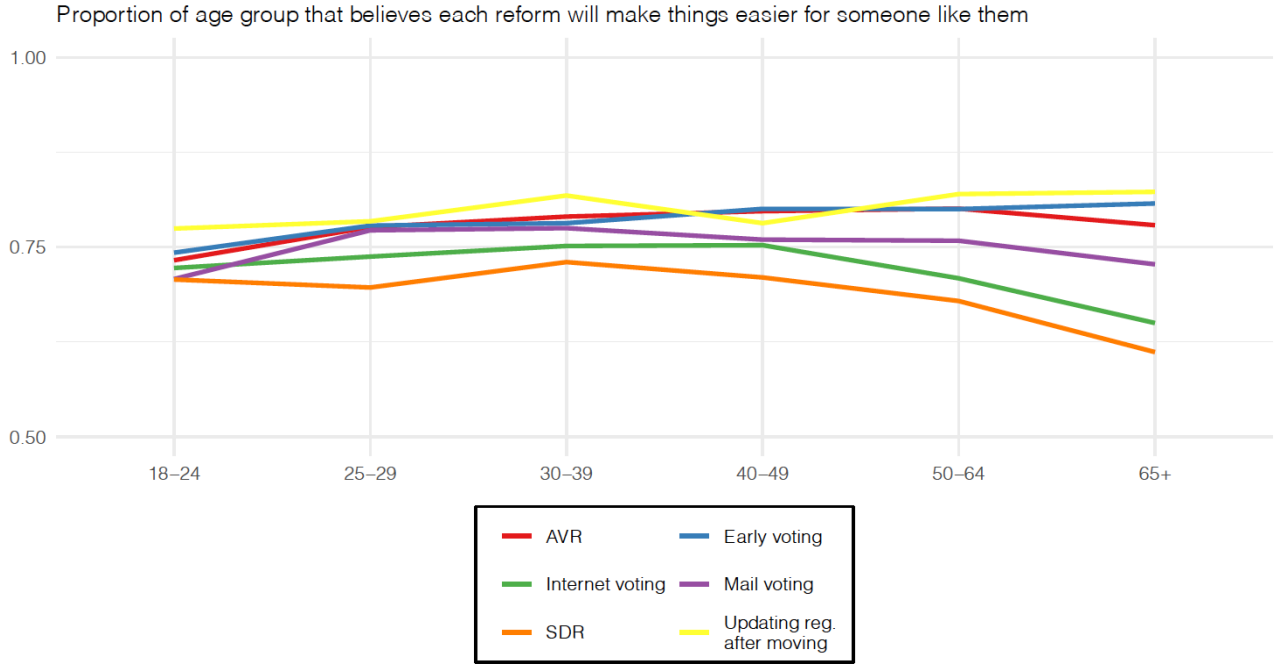


Figure 15 shows attitudes toward each policy broken out by age group. On average, all ages believe that each voting reform would make things easier for them. Young people are slightly more likely than seniors to believe that internet voting and same-day registration would make things easier; otherwise, responses are quite similar across age groups.

There do appear to be differences by registration status, though these differences are not statistically significant (likely due to the survey's small sample size). Interestingly, for reforms focused on registration, young people who are already registered (and who therefore are less likely to benefit from easier registration policies) are *more* likely than unregistered youth to think the reforms would make things easier. The same is true for voting-focused reforms: young registrants are more likely to perceive a benefit from laws that reduce the costs of voting, while unregistered youth are less likely to think these reforms would make things easier.

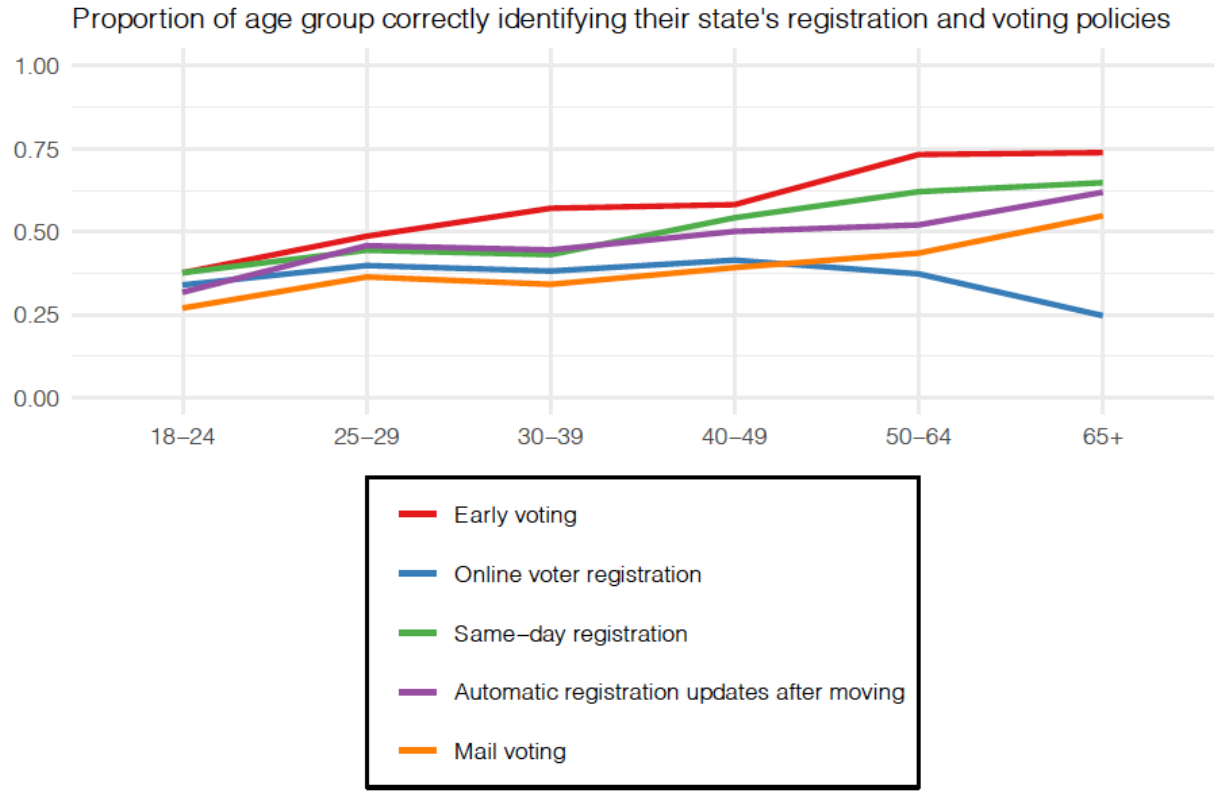
Knowledge of Voting Laws

I next asked survey participants a series of factual questions about their state's registration and voting laws. Lack of knowledge about state voting policies can be problematic in at least two cases. First, people might think their state's voting process is *easier* than it is, leaving them unprepared to complete onerous voting steps when the time comes and ultimately driving down turnout. Second, they might think the process is *harder* than it is, discouraging them from participating. If young people do not know when their legislators adopt policies that make voting easier, this could help explain why voting reforms are sometimes ineffective at increasing youth turnout.

To minimize the risk of people looking up the correct answers before responding, I prefaced these factual questions with an explicit request: "We are trying to get a general sense of what people know about these rules, so please answer to the best of your knowledge, without looking up any additional information." Responses were recoded as binary outcomes for each voting law, with incorrect or "unsure" answers coded as 0 and correct answers coded as 1.

By and large, young people are least likely of any age group to correctly identify their state's voting laws (Figure 16). Fewer than half of 18-24 and 25-29-year-olds can accurately name their state's policy on early voting, same-day voter registration, automatically updating people's voter registrations after they move, and mail voting. The one exception is online voter registration, where young people are slightly *more* likely than seniors (and about as likely as other age groups) to correctly identify their state's policy. Even then, however, only a minority of young people know their state's policy.

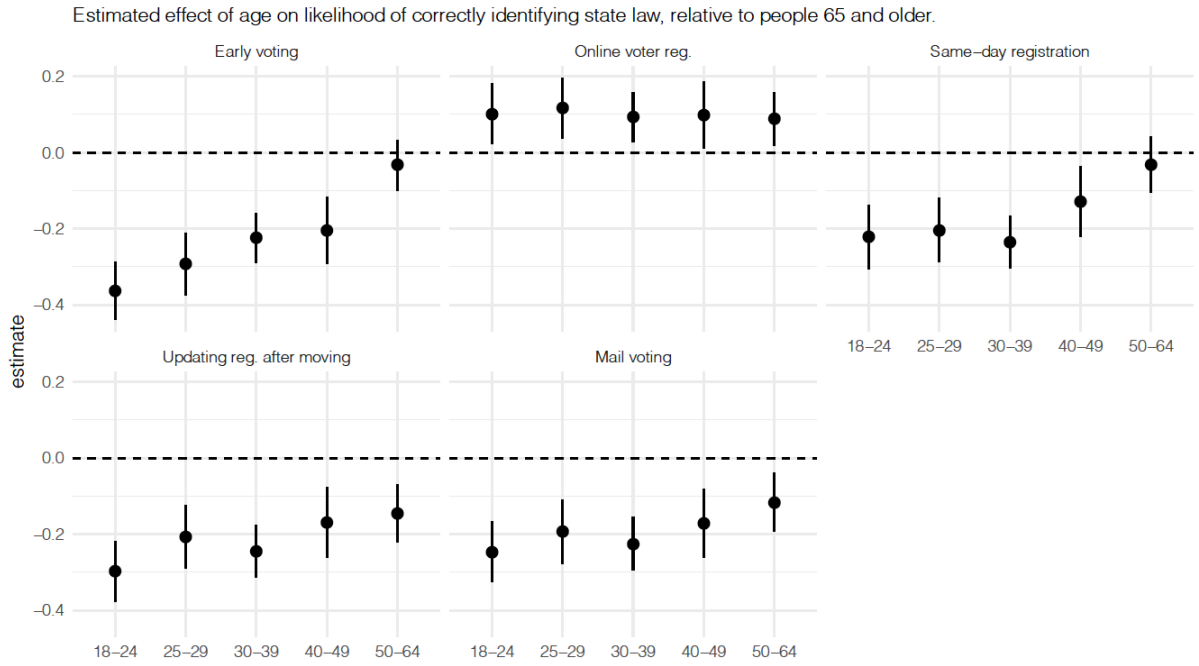
Figure 16: Young Adults Less Likely to Know Their State's Voting Laws



Note: Both incorrect and "unsure" responses coded as 0.

After adjusting for race, education, gender, and family income, young people ages 18-24 are significantly less likely than seniors to correctly identify their state's rules on same-day registration (-22 percentage points), early voting (-36 percentage points), updating registration after moving (-30 percentage points), and mail voting (-25 percentage points), as shown in Figure 17. Conversely, young people are 10 percentage points *more* likely than seniors to correctly identify their state's policy on online voter registration (OVR).

Figure 17: Young Adults Differ from Older Adults In Knowledge of Voting Laws



Note: Estimates and 95% confidence intervals are from a generalized linear regression on age group, with controls for educational attainment, gender, race and ethnicity, and family income.

Knowledge of Voting Laws by Age and Type of State

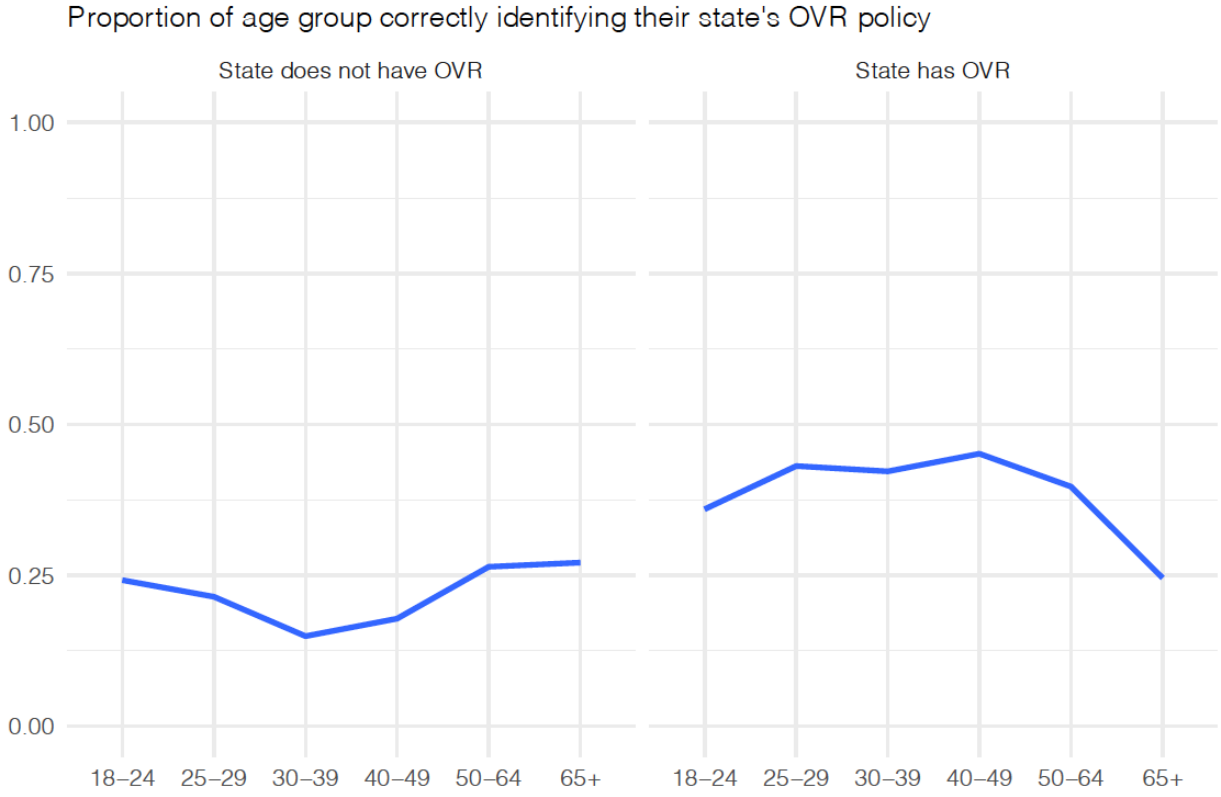
These aggregate levels of voting law knowledge mask interesting variation between types of state: those that have adopted the laws in question, and those that have not. This variation matters. In states that have passed laws to make voting easier, prospective voters often need to know these policies exist before being able to take advantage of them. If many young people think the voting process is harder than it is, this could drive down turnout—and lead researchers to conclude that cost-reducing reforms are relatively ineffective at boosting youth voting rates. Meanwhile, many young people live in states *without* these policies on the books. If they mistakenly believe these policies *do* exist—or even if they are simply unsure about their state's laws—they might fail to properly engage in their state's more cumbersome voting process, under the false expectation that an easier process is available to them. For example, an uninformed voter in states *without* same-day registration might wait to register until Election Day, at which point it will be too late.

In the following section, I analyze each age group's awareness of voting laws by the type of state—those with cost-reducing reforms on the books, and those without them.

Online Voter Registration

Figure 18 shows how voter knowledge varies between states with and without OVR. Overall, most voters are uninformed about their state's policy on OVR: in states without OVR, only a quarter of young people and seniors alike recognize that voters cannot register online. In states *with* OVR, well under half of people realize they *can* register online.

Figure 18: Knowledge of State OVR Policies Varies by Age and Type of State



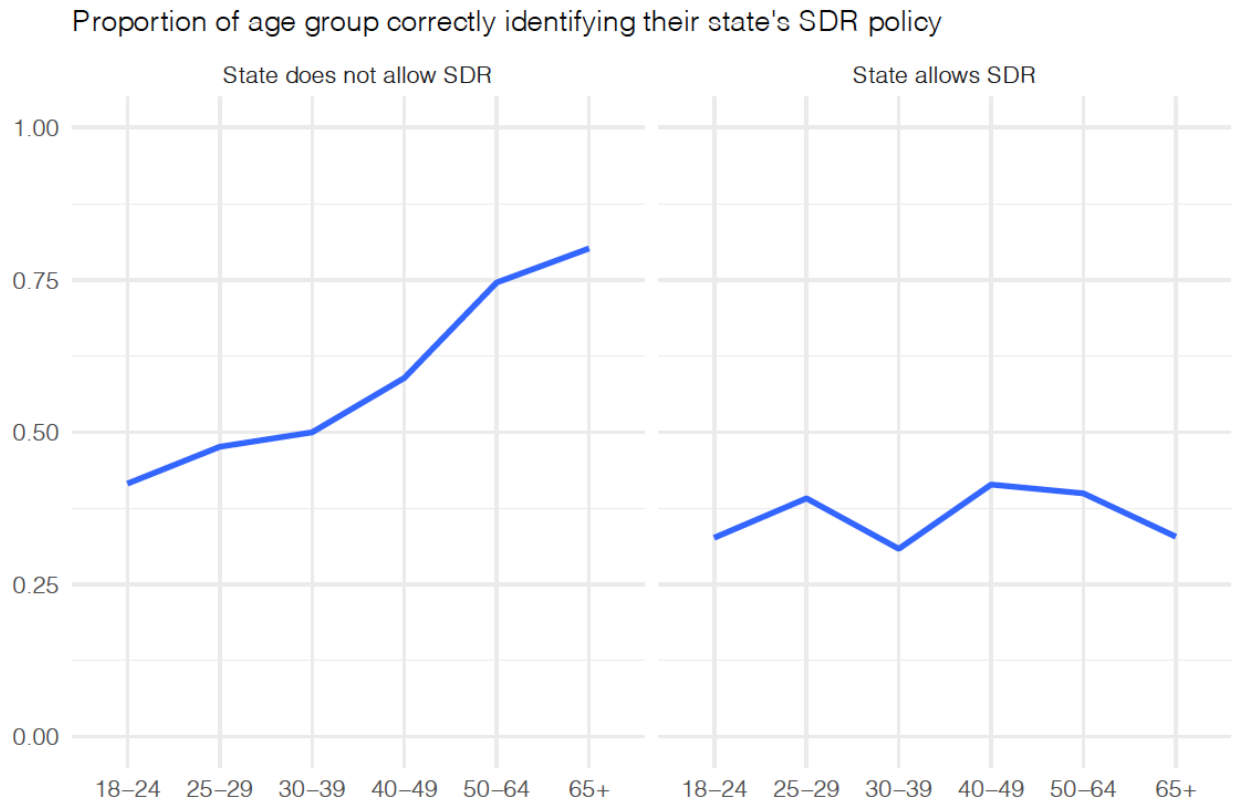
Note: Incorrect and "unsure" responses coded as 0.

That said, there do appear to be some differences in the relationship between a voter's age and their ability to correctly identify their state's approach to online registration. In states that require voters to register in person or by mail (no OVR), young and old people are slightly better than middle-aged Americans at answering correctly. In states that allow online registration, young and middle-aged individuals are most likely to answer correctly, while seniors are the least informed. It may be that seniors are less likely overall to believe their state has OVR, perhaps because they registered to vote before online registration portals existed. (While nearly 4 in 5 states allow for OVR, most have only adopted the reform in the past decade.) Young people, meanwhile, are more likely to have tried to register online in recent years and are therefore better acquainted with their state's OVR policy.

Same-Day Registration

Young people are the least informed of any age group about their state's policy on same-day registration (SDR). As Figure 19 shows, however, this difference between age groups only exists in states without SDR, where young people are only half as likely as seniors to know their state's policy. Most young people in these states are not aware that their state has a firm registration deadline in place. In states that do allow people to register and vote on the same day, young people are about as likely as other age groups to know this fact. This is not saying much: in these states, only a minority of people in *every* age group know their state allows for same-day registration.

Figure 19: Knowledge of State SDR Policies Varies by Age, Type of State

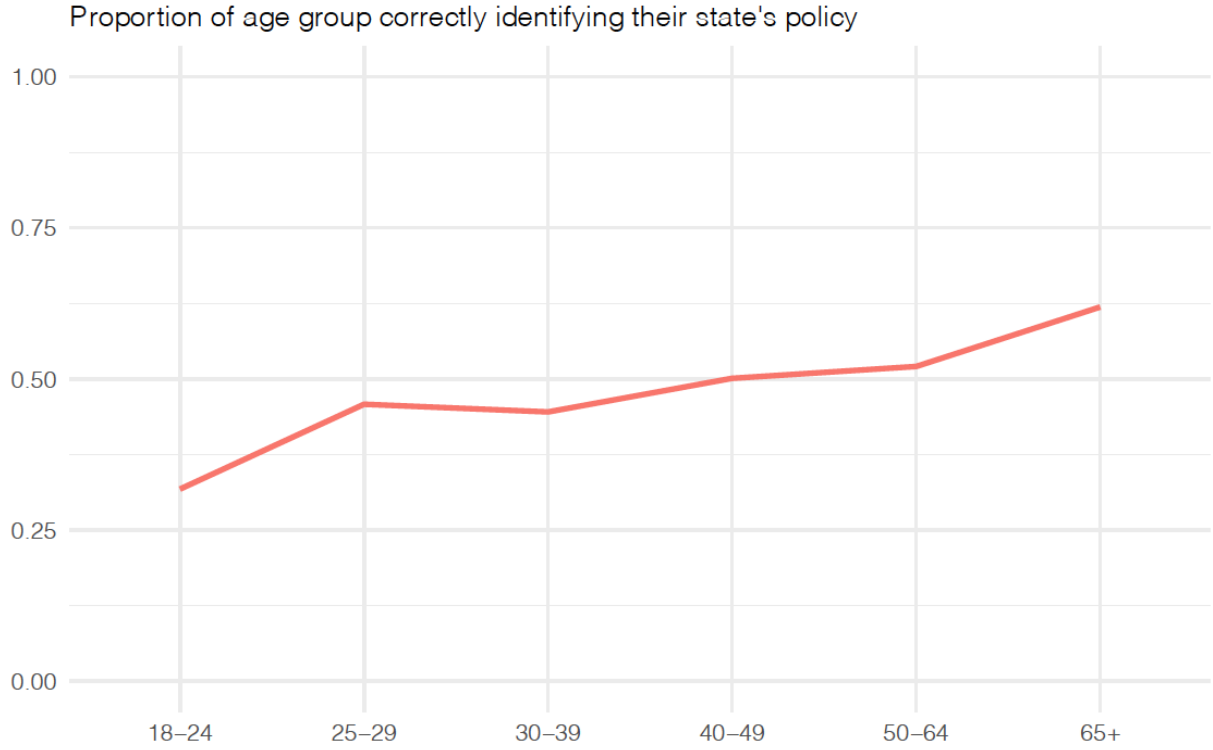


Note: Incorrect and "unsure" responses coded as 0.

Updating Registration After Moving

Respondents were asked to identify their state's policy on whether individuals who have recently moved must update their registration before being allowed to vote. In reality, there is no variation in this policy across states; every state requires voters to update their registration after moving (except North Dakota, which does not have voter registration). Yet only about half of people are aware of this requirement. As Figure 20 shows, young people are the least informed; only about a third of young Americans recognize they must update their registration after moving. Seniors are twice as likely to correctly identify their state's policy.

Figure 20: Most Young Adults Do Not Know They Must Update Registration After Moving



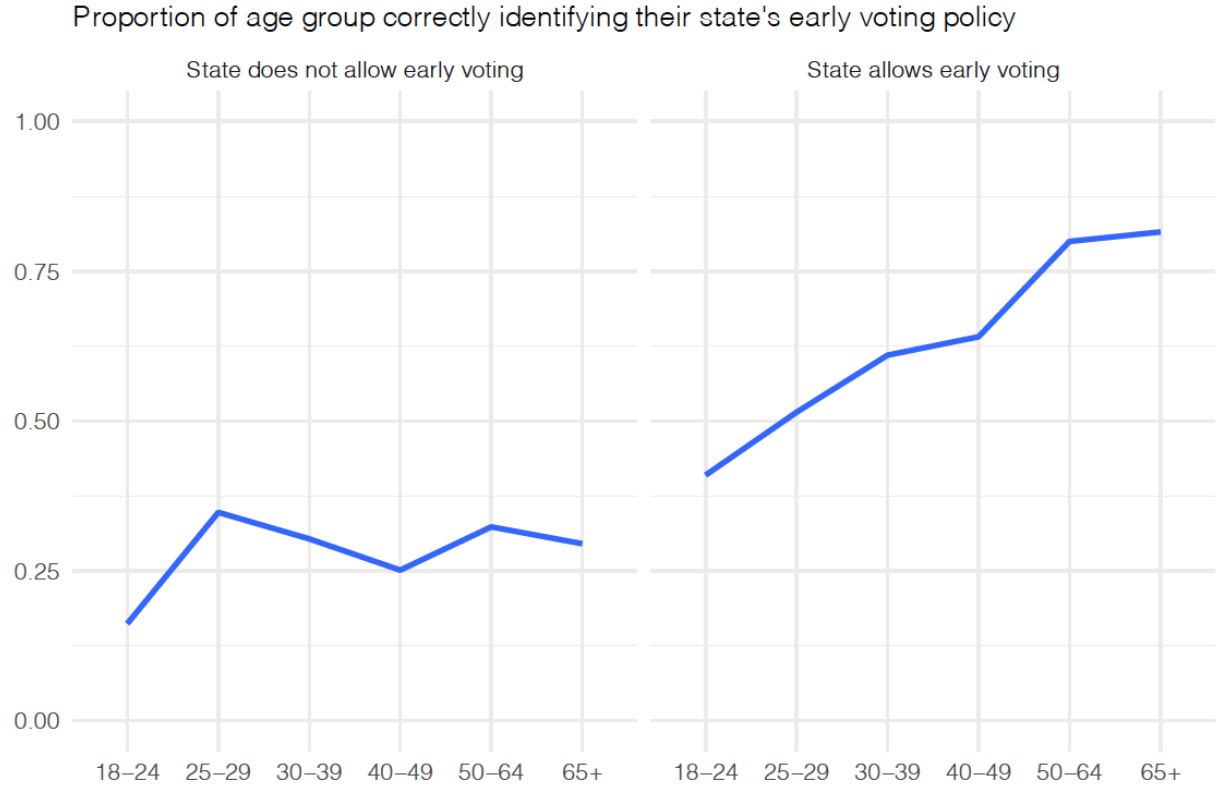
Note: Incorrect and "unsure" responses coded as 0.

Early Voting

Awareness of state early voting laws varies dramatically by both age and type of state (Figure 21). In early voting states, only 41 percent of young people are aware their state allows people to cast their ballot before Election Day, compared with 82 percent of seniors.

Awareness is generally much lower in states that do not allow for early voting: only a third of Americans in these states recognize that early voting is not permitted. Once again, youth are the least informed: only 16 percent of 18- to 24-year-olds say early voting is not allowed in their state, compared with between 25 and 35 percent of older age groups.

Figure 21: Knowledge of State Early Voting Policies Varies by Age, Type of State

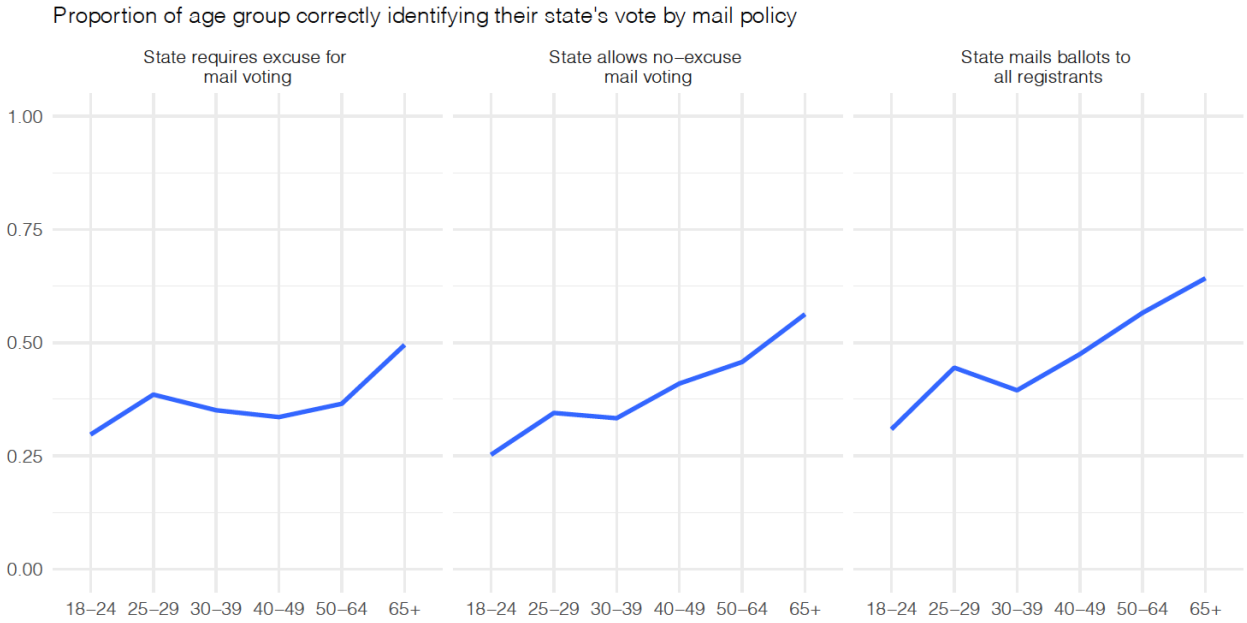


Note: Incorrect and "unsure" responses coded as 0.

Mail Voting

Young people know the least about their state's policy on voting by mail (Figure 22). Whether their state requires an excuse for mail voting, allows people to vote by mail without providing an excuse, or mails ballots to all registered voters, young people are less likely than others—especially seniors—to correctly identify their state's policy: Between 25 and 30 percent of people ages 18-24 know their state's policy on mail voting, compared to between 49 and 64 percent of those 65 and older. Notably, overall awareness of vote-by-mail laws is generally low across age groups and types of state.

Figure 22: Young Adults Consistently Less Likely to Know State's Policy on Mail Voting



Note: Incorrect and "unsure" responses coded as 0.

Discussion

In this chapter, I showed that voting costs are unequally distributed by age. Young people face higher barriers to voting: they have less information about the voting process and about the candidates and issues on their ballot, less time and flexibility to engage in registration and voting, and less ability to balance voting with the other things going on in their lives. They disproportionately face transportation issues when registering and voting, and they are more likely to face a tradeoff between participating in elections and earning money. The largely offline nature of voting presents a larger struggle for youth than for older Americans, as do both registering to vote and voting by mail. Young people are also less likely to have the voter ID documentation they think they need to vote—a particularly concerning finding, given the dramatic increase in strict voter ID laws in recent years (NCSL 2020). All but one of the cost indicators measured here is a significant predictor of whether an individual voted in the 2018 midterm elections.

Importantly, this chapter is not an exhaustive look at voting costs. Measuring *every* cost of voting may be an impossible endeavor; every action requires some costly expenditure of energy, and trying to catalog every cost would prove extremely time-consuming.[37] These survey results leave open the question of whether young people face higher costs *overall*, relative to older age groups. Yet when asked how easy or difficult registration and voting are for someone like them, young people are more likely to rate these processes as difficult than any other age group. These direct measures of voting costs are imperfect—but they do hint that young people might experience systematically greater voting costs.

Of course, age is not the only predictor of voting difficulty. Consistently, I find that race and education also have a large bearing on voting costs. People of color and less-educated

[37] It could be, for instance, that there are *emotional* costs to being politically engaged that vary by age. One recent study found that teenagers are disproportionately likely to say politics causes them stress and has damaged close relationships (Oosterhoff 2019).

individuals are more likely to face barriers to voting—and to have fewer resources to overcome those barriers. Because today's youth are more racially diverse and have less formal education than older people, this further increases voting costs for young Americans. However, as I note earlier, these differences alone do not fully account for differences in costs across age groups.

In the vast majority of cases, young people lack essential information about their state's voting laws. They are also less informed than older people—though awareness of voting laws is distressingly low across age groups. This finding is surely disheartening for those who see voting reforms as a panacea for boosting turnout. Particularly concerning is that young people in states with more *restrictive* voting laws are especially likely to think cost-reducing reforms are already in place; for instance, youth in states without same-day registration are more likely than older voters to think they can register and vote on Election Day. Might a significant number of young adults be expecting to vote on Election Day, only to realize too late that they were supposed to register ahead of time?

Low knowledge about state voting laws has a silver lining, however: the relatively small turnout effects of cost-reducing electoral reforms found by past scholars may be due in good measure to lack of public awareness, not to any inherent flaw or shortcoming of the policies themselves. Of course, because this data is cross-sectional, we cannot know for certain how stable these responses would be over time. Yet the fact that the survey was conducted during a national election year—in the late spring of 2020, near the height of the Democratic Party primaries—suggests that even a highly salient election with widespread voter mobilization efforts is insufficient to remedy differences in voting information across age groups.

The survey results presented here drive home that there is no universal voting experience. Some groups face greater barriers to electoral participation than others, while others have less time, money, information, and other resources to complete the steps of registering and casting a ballot. Young people appear to face especially high voting costs, relative to older voters. It is unsurprising, then, that youth tend to turn out at lower rates and ultimately have less power within the political system. To shrink the age-turnout gap and improve the representativeness of America's elections, it is critical that we identify which voting barriers disproportionately burden young people—and then prioritize the election reforms that directly target those barriers.

CHAPTER 4: BACKLASH TO YOUTH VOTER SUPPRESSION

As the previous chapter showed, one promising approach to shrinking the age-turnout is to directly reduce those voting costs that disproportionately affect young people. 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.[38] 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, 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

[38] 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 chapter, 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.

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 chapter, 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.[39] 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 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.

[39] 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.

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 Democractic 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.[40] 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.[41]

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 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

[40] Coppock & McClellan (2019) show that experimental results replicated on Lucid samples match closely with benchmark results obtained using national probability samples.

[41] 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.

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.[42]

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.[43] 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.

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.

[42] 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.

[43] 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.

Table 1: Youth Treatment Increases Perception of Youth Voter Suppression

Regression Results

	Dependent variable:				
	youth targeted by suppression				
	18-29	30-39	40-49	50-64	65+
generic treatment	0.010	-0.007	-0.050*	0.030	0.019
	(0.022)	(0.023)	(0.027)	(0.020)	(0.021)
youth treatment	0.031	0.040*	0.078***	0.113***	0.120***
	(0.022)	(0.023)	(0.026)	(0.020)	(0.022)
constant	0.518***	0.496***	0.471***	0.405***	0.380***
	(0.016)	(0.016)	(0.019)	(0.014)	(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).[44] 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).

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.[45] 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.

[44] 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).

[45] 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.

Table 2: Youth Treatment Increases Respondent Anger

Regression Results

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

Table 3: Youth Treatment Increases Respondent Reactance

Regression Results

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

Voting intentions

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

Regression Results

	Dependent variable:				
	vote in 2022				
	18-29	30-39	40-49	50-64	65+
generic treatment	0.005	-0.012	-0.015	-0.009	0.015
	(0.021)	(0.023)	(0.025)	(0.019)	(0.019)
youth treatment	0.011	-0.007	-0.012	-0.012	0.006
	(0.021)	(0.023)	(0.024)	(0.019)	(0.019)
constant	0.696***	0.761***	0.809***	0.850***	0.899***
	(0.015)	(0.017)	(0.017)	(0.014)	(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.[46] 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; 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.[47] Mechanically, the indirect effect is identified

[46] This is also known as "inconsistent" or "competitive" mediation (Koschate-Fischer and Schwille 2018).

[47] This indirect mediation effect is also known as the average causal mediation effect (ACME).

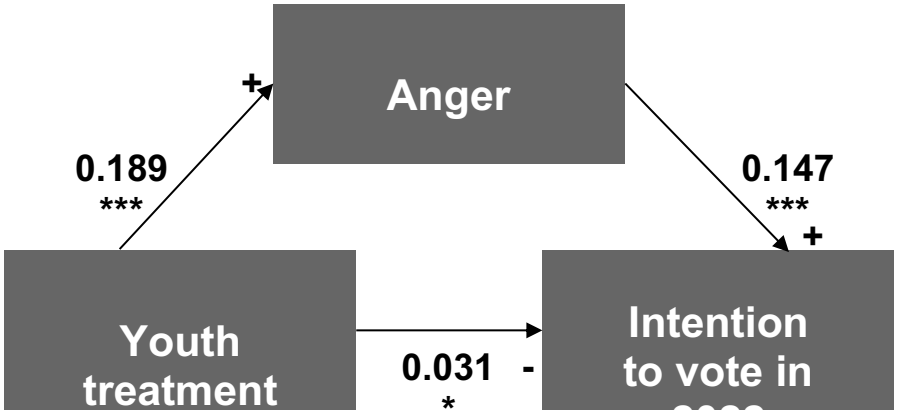
by first calculating the relationship between treatment and the mediator in question, then by calculating the relationship between the mediator and the outcome, and finally by multiplying these two numbers. The direct effect, by contrast, is the effect of treatment on the outcome after controlling for 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.

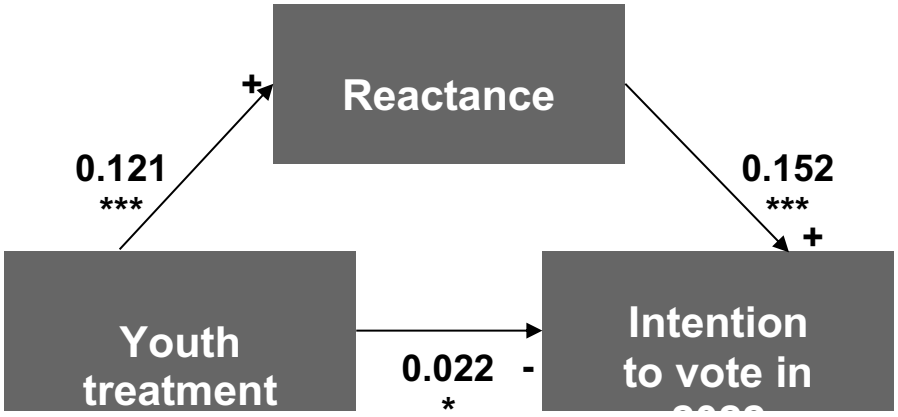
Figure 1: Anger Mediates Effect of Treatment on Voting Intentions



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)*(.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.[48] The treatment's AIE via anger was 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.

[48] 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 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.

Table 5: Mediation Effects Vary By Age Group

	anger			*reactance*		
age group	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 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

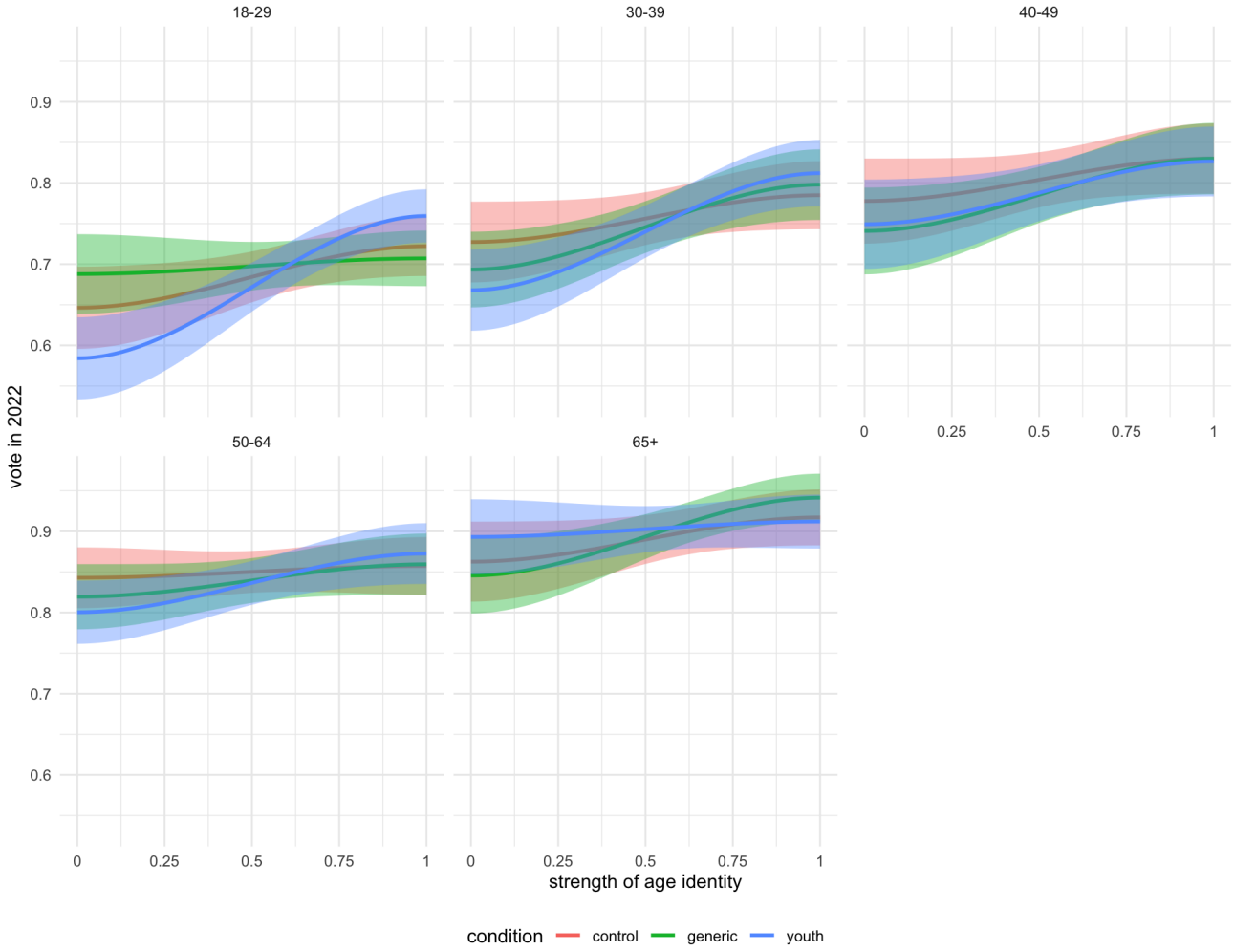
Regression Results

	Dependent variable:				
	vote in 2022				
	18-29	30-39	40-49	50-64	65+
generic treatment	0.042	-0.034	-0.037	-0.023	-0.017
	(0.035)	(0.034)	(0.039)	(0.028)	(0.034)
youth treatment	-0.062*	-0.059	-0.029	-0.042	0.030
	(0.036)	(0.036)	(0.038)	(0.028)	(0.033)
strong age ID	0.076**	0.058*	0.052	0.015	0.054*
	(0.031)	(0.033)	(0.035)	(0.027)	(0.029)
generic treatment:strong age ID	-0.057	0.047	0.037	0.025	0.041
	(0.043)	(0.046)	(0.050)	(0.038)	(0.041)
youth treatment:strong age ID	0.099**	0.087*	0.025	0.058	-0.036
	(0.044)	(0.047)	(0.049)	(0.039)	(0.041)
constant	0.646***	0.727***	0.778***	0.843***	0.863***
	(0.025)	(0.025)	(0.027)	(0.020)	(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	-0.092	0.029	-0.008	-0.065
	(0.066)	(0.066)	(0.077)	(0.063)	(0.075)
youth treatment	-0.059	-0.116*	0.099	-0.118*	0.070
	(0.061)	(0.067)	(0.069)	(0.063)	(0.082)
warmth	0.249***	0.112*	0.252***	0.118**	0.068
	(0.059)	(0.065)	(0.071)	(0.058)	(0.070)
generic treatment:warmth	-0.105	0.117	-0.069	-0.005	0.111
	(0.087)	(0.090)	(0.103)	(0.085)	(0.097)
youth treatment:warmth	0.108	0.155*	-0.149	0.144*	-0.083
	(0.082)	(0.091)	(0.094)	(0.085)	(0.104)
constant	0.517***	0.684***	0.632***	0.768***	0.847***
	(0.045)	(0.047)	(0.053)	(0.043)	(0.056)
Observations	1,086	967	782	1,219	836
Note:					*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

	Dependent variable:				
	vote in 2022				
	White	Black	Asian	Hispanic	Other
generic treatment	-0.012	-0.014	0.019	0.134***	-0.013
	(0.011)	(0.031)	(0.040)	(0.044)	(0.065)
youth treatment	-0.005	-0.006	-0.050	0.126***	-0.025
	(0.011)	(0.029)	(0.042)	(0.046)	(0.063)
constant	0.825***	0.756***	0.743***	0.649***	0.704***
	(0.008)	(0.021)	(0.029)	(0.033)	(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.

CHAPTER 5: LOOKING FORWARD

What can be done to shrink the age-turnout gap? This dissertation offers one clear answer: figure out which parts of voting are more costly for young people than for older age groups, and make them easier. This can be done through legislative and non-legislative means.

Legislative strategies for shrinking the age-turnout gap

Same-day registration is a great example of a reform that, while not targeted directly at young people, has the effect of boosting youth turnout relative to that of other age groups. Young adults struggle more with voter registration: as my survey results from chapter 3 show, youth are less informed about the registration process, feel busier than other age groups, struggle more to balance work with other tasks like registering and voting, experience greater opportunity costs from registering and voting, change addresses (and therefore have to re-register) more frequently—the list goes on. Make it easier for *everyone* to register by letting people sign up to vote right before casting their ballot, and as I show in chapter 2, young people—because they face higher registration barriers to begin with—turn out at disproportionately higher rates.

Other registration-stage reforms are also promising for boosting youth voting rates and reducing age-based turnout inequalities, but not without caveats. The most popular of these is automatic voter registration (AVR), which can dramatically reduce registration costs by enrolling eligible individuals without requiring them to even *want* to register. However, it typically does so only for eligible voters who interact with their state's Department of Motor Vehicles (or equivalent agency). As I note in chapter 2, today's young people are becoming licensed drivers at lower rates than past generations. If young people underrepresented at the DMV relative to older age groups, then even if youth face greater registration costs, DMV-based automatic voter registration may do little to close the age-turnout gap. Indeed, in some circumstances, the reform could inadvertently worsen turnout inequalities. A straightforward solution is to expand AVR touchpoint agencies to include sites that disportionately serve young people. One could imagine, for instance, taking the list of young adults who apply for federal student aid and using it to register eligible people to vote, as we currently do to register young men for the Selective Service (Seago 2019).

Offering 16- and 17-year-olds the opportunity to pre-register to vote could be especially effective at closing the age-turnout gap. This is because pre-registration is only available to young people, not to older age groups. The question is therefore not *whether* pre-registration will increase young people's share of the electorate, but by how much. Past research has found that pre-registration increases youth turnout by up to 5 percentage points (Holbein and Hillygus 2017), but there is likely room for improvement here, if we assume that many young people in pre-registration states are unaware the opportunity to pre-register exists. Improvements to existing pre-registration regimes could also help drive up the reform's effectiveness. For instance, states could mandate that public high schools give all students the opportunity to pre-register to vote, as the California legislature proposed in 2019 (Shapiro 2019).[49] My current research explores this idea further, examining the causal impact of teaching eligible high school students about pre-registration on their registration and voting rates.

What about internet voting? This increasingly popular reform has been used in national elections in Estonia, in Swiss canton elections, and in at least three U.S. jurisdictions, which have

[49] California Governor Gavin Newsom proceeded to veto the bill.

adopted mobile voting for overseas military voters (Mearian 2019). Despite serious security risks—many election security experts claim that due to the possibility of hacking and coercion, internet voting should not be used in government elections (Newell 2021)—future pilot programs are almost certain to proliferate, especially as traditional voting machines increasingly require costly repairs (Koebler and Gault 2018) that federal funding is insufficient to cover (Newman 2019). Understanding the potential turnout ramifications of mobile voting is critical as this policy reform gains traction. As I show in chapter 3, young people are nearly 30 percentage points more likely than seniors to struggle to complete the parts of voting that cannot be done online. It stands to reason that allowing all individuals to vote online would disproportionately boost youth turnout.

Past research has repeatedly shown that, by and large, young people take advantage of internet voting at higher rates than older people. According to one review of 22 empirical studies of internet voting (Serdult et al. 2015), young people are generally more likely to vote online than older people.[50] But while internet voting could help reduce the age-turnout gap in the near term, this effect may be short-lived. Schlozman et al. (2010) and Serdult et al. (2015) both show that after controlling for overall internet usage, young people are *not* more likely to take advantage of online voting. In other words, it may be that internet voting currently boosts youth turnout because today's young people are disproportionately online. Once these digitally savvy youth grow older and replace their internet-wary elders—that is, once internet use becomes more even across age groups—more internet voting's disproportionate effect on youth turnout may disappear.

Non-legislative strategies for shrinking the age-turnout gap

Unfortunately, reforming state election laws is not always possible. In many states with Republican-controlled legislatures, electorate-expanding reforms are unlikely to pass. This is especially likely to be the case for policies that make voting easier for young people, who tend to support Democratic candidates and liberal policies. Republican control of state government is strongly associated with democratic backsliding (Grumbach 2021), and virtually all recent instances of youth voter suppression have occurred at the hands of Republican politicians.

In some of these states, it may be possible to reform the voting process via ballot initiative.[51] But when an initiative-based strategy is not feasible for legal, political, or financial reasons, democracy advocates might consider another approach: alleviate voters' information costs. These can be addressed outside the policy-making process entirely via direct outreach to young voters. As I show in chapter 3, young people face greater information costs than older people: they are less likely to know how or where to register and vote, and they tend not to know their state's policies around registration deadlines, early voting, and mail voting. Targeting young adults with this sort of practical information could help them more successfully navigate the voting process, without requiring lawmakers to take action that may be against their electoral interests.

A more challenging question is what, if anything, can be done to counter the effects of youth-targeted voter suppression. The results from my survey experiment in chapter 4 suggest

[50] In the US context, the probability of taking advantage of internet voting was highest for middle-aged individuals rather than for the youngest age group (p. 34). These results should be taken with a grain of salt, however, as they come from a limited set of studies of Arizona's 2000 and 2004 Democratic primary elections. A later study found that older people are disproportionately unwilling to vote online (Fowler 2019).

[51] See Maine's recent experience of passing and upholding ranked choice voting at the ballot.

that, unlike more traditional forms of voter suppression broadly targeted at Democrats or people of color, youth-targeted suppression may not generate meaningful voter backlash. While the evidence suggests that young people *might* participate more in response to information about youth suppression, this increase is relatively small and not statistically distinguishable from zero. Further research with a larger and/or more representative sample would help determine whether this information does, in fact, have any effect on voting intentions, and whether those intentions translate into real-world turnout. Until then, those trying to bolster youth turnout should not be particularly optimistic about the potential of information about youth suppression to mobilize young voters. Researchers should make a strong effort to quickly identify other interventions—policy, communications, or otherwise—that will more definitively combat youth suppression.

A new framework for thinking about election laws

This dissertation began as an exploration of how changing election laws impact youth turnout. In the intervening years, democracy has suffered several high-profile and deeply concerning blows: restrictions of voting access at the hands of state legislatures (Boschma 2021), a violent attack on the nation's Capitol, an attempt by the majority of Republican members of the House of Representatives to overturn the results of the 2020 presidential election, and nearly 400 bills with restrictive voting provisions proposed in the first four months of 2021 alone (Brennan Center 2021).

To fortify democracy, many liberal politicians and activists have pushed for the passage of the For the People Act, popularly known as H.R.1. This omnibus legislation contains myriad provisions, but the most often discussed are a set of reforms aimed at making voting easier, especially for historically disadvantaged and disenfranchised groups—including young people. Perhaps unsurprisingly, the intense national focus on H.R.1 has prompted a fierce debate in political circles around whether these reforms will indeed meaningfully affect turnout. Arguably the most prominent journalist challenging the power of electoral reforms is Nate Cohn at the *New York Times*, who wrote in a recent piece that "expanding voting options to make it more convenient hasn't seemed to have a huge effect on turnout or electoral outcomes" (Cohn 2021). Much of his argument was in turn based on the work of Professor Adam Berinsky, whose read of the election reform literature I discussed—and challenged—in my introductory chapter. Meanwhile, democracy reform advocates are increasingly worried that national Democratic officials are not taking seriously enough the GOP's threats to voting access and electoral integrity (Brownstein 2021).

The lack of national consensus around the impact of election reforms on turnout highlights a serious shortcoming in the political science literature: half a century after scholars began attempting to chronicle the effects of election law changes, we still lack a shared framework for how to anticipate, measure, and discuss these effects. Too often, our discussion of reforms boils down to two sides: yes, they boost turnout, or no, they have no effect. Advocates of either position can cherrypick research that is grounded in a particular setting, sample, or historical moment, but that often lacks sufficient external validity to be applied more broadly—or, as I show in the introductory chapter, they can often point to research conducted at such a broad level that it obscures important heterogeneity at the subgroup level. Accurately anticipating the effects of new election laws requires a close examination of *which costs* these laws reduce (or, in the case of voter suppression, increase) and *which groups* disproportionately bear these costs—as this will in good measure determine which groups are most affected.

These are certainly not the only factors that should be investigated. As Barry Burden et al. (2014) have convincingly argued, reforms can have unanticipated indirect consequences that affect turnout in ways advocates never envisioned. The most notable example here is early voting, which makes voting easier but also makes Election Day less salient, especially for low-propensity voters. As a result, it does little to boost aggregate turnout, and it may inadvertently exacerbate socioeconomic biases in the electorate. Similarly, voter suppression laws can sometimes induce a strong-enough backlash that turnout among targeted groups actually goes up—or at least remains steady—rather than precipitously dropping (Biggers 2019, Valentino and Neuner 2017).

While grappling with these less obvious possibilities will improve our prognostications, it is of course impossible to tell the future. To understand the true impact of an election law, we must wait until it has been implemented and then measure its real-world effects. My research points to the importance and value of conducting this research at the subgroup level. At a time of dramatic differences in political access and influence across demographic groups, it is not enough to measure electorate-wide turnout effects. We must instead attempt to understand how reforms will shape *who* votes, so we can identify which reforms hold the most promise for increasing political equality.

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APPENDIX A

Table A1: Full Individual Level Difference-in-Differences Model Results

term	estimate	std.error	statistic	p.value	model
(Intercept)	0.61	0.01	60.54	0	Bivariate
sdr	0.01	0.02	0.72	0.47	Bivariate
age_group18.24	-0.36	0.01	-59.77	0	Bivariate
age_group25.34	-0.21	0.01	-35.7	0	Bivariate
age_group35.44	-0.1	0.01	-16.21	0	Bivariate
age_group45.54	-0.04	0.01	-7.34	0	Bivariate
age_group55.64	0.01	0	1.31	0.2	Bivariate
sdr:age_group18.24	0.02	0.01	2.13	0.04	Bivariate
sdr:age_group25.34	0	0.01	0.01	0.99	Bivariate
sdr:age_group35.44	-0.01	0.02	-0.88	0.38	Bivariate
sdr:age_group45.54	-0.01	0.01	-1.22	0.23	Bivariate
sdr:age_group55.64	-0.01	0.01	-1.55	0.13	Bivariate
(Intercept)	0.26	0.01	22.59	0	Controls
sdr	0	0.01	-0.22	0.83	Controls
age_group18.24	-0.42	0.01	-77.54	0	Controls
age_group25.34	-0.33	0	-81.08	0	Controls
age_group35.44	-0.22	0	-59.02	0	Controls
age_group45.54	-0.15	0	-39.84	0	Controls
age_group55.64	-0.07	0	-18.83	0	Controls
factor(race)200	0.05	0.01	6.55	0	Controls
factor(race)300	-0.05	0.01	-4.43	0	Controls
factor(race)650	-0.16	0.03	-5.2	0	Controls
factor(race)651	-0.18	0.01	-19.6	0	Controls
factor(race)652	-0.13	0.02	-6.87	0	Controls
factor(race)700	-0.09	0.02	-3.75	0	Controls
sex	0.02	0	13.66	0	Controls
faminc	0.01	0	52.03	0	Controls
educ	0.02	0	56.18	0	Controls
sdr:age_group18.24	0.06	0.01	6.09	0	Controls
sdr:age_group25.34	0.03	0.01	3.53	0	Controls
sdr:age_group35.44	0.01	0.01	1.37	0.18	Controls
sdr:age_group45.54	0	0.01	0.64	0.53	Controls
sdr:age_group55.64	-0.01	0.01	-1.21	0.23	Controls

Table A2: Full Individual Level Difference-in-Differences Model Results (State-Decade FEs)

term	estimate	std.error	statistic	p.value	model
(Intercept)	0.76	0.01	142.2	0	Bivariate
sdr	0.03	0.02	1.56	0.13	Bivariate
age_group18.24	-0.36	0.01	-59.51	0	Bivariate
age_group25.34	-0.22	0.01	-35.89	0	Bivariate
age_group35.44	-0.1	0.01	-16.06	0	Bivariate
age_group45.54	-0.04	0.01	-7.24	0	Bivariate
age_group55.64	0.01	0	1.41	0.16	Bivariate
sdr:age_group18.24	0.02	0.01	2.08	0.04	Bivariate
sdr:age_group25.34	0	0.01	0.01	0.99	Bivariate
sdr:age_group35.44	-0.01	0.02	-0.88	0.38	Bivariate
sdr:age_group45.54	-0.01	0.01	-1.22	0.23	Bivariate
sdr:age_group55.64	-0.02	0.01	-1.65	0.1	Bivariate
(Intercept)	0.22	0.01	21.96	0	Controls
sdr	0.01	0.01	0.87	0.39	Controls
age_group18.24	-0.42	0.01	-75.95	0	Controls
age_group25.34	-0.32	0	-75.74	0	Controls
age_group35.44	-0.22	0	-54.25	0	Controls
age_group45.54	-0.14	0	-35.58	0	Controls
age_group55.64	-0.06	0	-16.27	0	Controls
factor(race)200	0.05	0.01	5.83	0	Controls
factor(race)300	-0.06	0.01	-5.09	0	Controls
factor(race)650	-0.19	0.03	-7.41	0	Controls
factor(race)651	-0.17	0.01	-13.82	0	Controls
factor(race)652	-0.11	0.02	-6.98	0	Controls
factor(race)700	-0.09	0.02	-5.05	0	Controls
sex	0.02	0	12.83	0	Controls
faminc	0.01	0	37.31	0	Controls
educ	0.02	0	57.2	0	Controls
sdr:age_group18.24	0.06	0.01	6.36	0	Controls
sdr:age_group25.34	0.03	0.01	3.5	0	Controls
sdr:age_group35.44	0.01	0.01	1.5	0.14	Controls
sdr:age_group45.54	0	0.01	0.59	0.56	Controls
sdr:age_group55.64	-0.01	0.01	-1.36	0.18	Controls

Table A3: Full Individual Level Difference-in-Differences Model Results (State-Time Trends)

term	estimate	std.error	statistic	p.value	model
(Intercept)					Bivariate
sdr	0.03	0.01	2.32	0.02	Bivariate
age_group18.24	-0.36	0.01	-59.46	0	Bivariate
age_group25.34	-0.21	0.01	-35.52	0	Bivariate
age_group35.44	-0.1	0.01	-16.14	0	Bivariate
age_group45.54	-0.04	0.01	-7.3	0	Bivariate
age_group55.64	0.01	0	1.34	0.19	Bivariate
year	0	0	81.05	0	Bivariate
sdr:age_group18.24	0.02	0.01	2.17	0.04	Bivariate
sdr:age_group25.34	0	0.01	0.01	0.99	Bivariate
sdr:age_group35.44	-0.01	0.02	-0.9	0.37	Bivariate
sdr:age_group45.54	-0.02	0.01	-1.27	0.21	Bivariate
sdr:age_group55.64	-0.01	0.01	-1.57	0.12	Bivariate
(Intercept)					Controls
sdr	0.02	0.01	1.85	0.07	Controls
age_group18.24	-0.42	0.01	-76.88	0	Controls
age_group25.34	-0.32	0	-80.08	0	Controls
age_group35.44	-0.22	0	-58.58	0	Controls
age_group45.54	-0.15	0	-39.68	0	Controls
age_group55.64	-0.07	0	-18.84	0	Controls
factor(race)200	0.05	0.01	6.53	0	Controls
factor(race)300	-0.05	0.01	-4.18	0	Controls
factor(race)650	-0.17	0.03	-6.09	0	Controls
factor(race)651	-0.17	0.01	-14.01	0	Controls
factor(race)652	-0.11	0.01	-7.29	0	Controls
factor(race)700	-0.11	0.02	-6.58	0	Controls
sex	0.02	0	13.66	0	Controls
faminc	0.01	0	51.45	0	Controls
educ	0.02	0	56.76	0	Controls
year	0	0	32.36	0	Controls
sdr:age_group18.24	0.06	0.01	6.32	0	Controls
sdr:age_group25.34	0.03	0.01	3.52	0	Controls
sdr:age_group35.44	0.01	0.01	1.23	0.23	Controls
sdr:age_group45.54	0	0.01	0.43	0.67	Controls
sdr:age_group55.64	-0.01	0.01	-1.29	0.2	Controls

Table A4: Full State Level Difference-in-Differences Model Results

term	estimate	std.error	statistic	p.value	group	model
(Intercept)	0.31	0.01	21.26	0	18-24	Bivariate
sdr	0.03	0.01	3.46	0	18-24	Bivariate

(Intercept)	-0.2	0.16	-1.44	0.15	18-24	Controls
sdr	0.04	0.01	3.9	0	18-24	Controls
white	0.05	0.12	0.54	0.59	18-24	Controls
black	0.07	0.14	0.64	0.53	18-24	Controls
asian	0.3	0.13	3.69	0	18-24	Controls
povrate	0	0	2.8	0.01	18-24	Controls
educ	0.02	0.01	3.74	0	18-24	Controls
(Intercept)	0.43	0.01	27.13	0	25-34	Bivariate
sdr	0.01	0.01	1.28	0.2	25-34	Bivariate
(Intercept)	-0.6	0.2	-3.56	0	25-34	Controls
sdr	0.01	0.01	1.53	0.13	25-34	Controls
white	0.34	0.16	2.55	0.01	25-34	Controls
black	0.32	0.19	2.06	0.04	25-34	Controls
asian	0.38	0.15	3.31	0	25-34	Controls
povrate	0	0	3.29	0	25-34	Controls
educ	0.04	0.01	7.15	0	25-34	Controls
(Intercept)	0.51	0.02	34.49	0	35-44	Bivariate
sdr	0	0.01	-0.31	0.75	35-44	Bivariate
(Intercept)	-0.47	0.15	-3.13	0	35-44	Controls
sdr	0	0.01	0.28	0.78	35-44	Controls
white	0.42	0.14	3.14	0	35-44	Controls
black	0.4	0.17	2.61	0.01	35-44	Controls
asian	0.52	0.15	4.14	0	35-44	Controls
povrate	0	0	3.06	0	35-44	Controls
educ	0.03	0	7.34	0	35-44	Controls
(Intercept)	0.54	0.01	36.46	0	45-54	Bivariate
sdr	0	0.01	-0.13	0.9	45-54	Bivariate
(Intercept)	-0.46	0.19	-2.76	0.01	45-54	Controls
sdr	0.01	0.01	1.03	0.3	45-54	Controls
white	0.45	0.18	2.78	0.01	45-54	Controls
black	0.56	0.2	3.16	0	45-54	Controls
asian	0.53	0.18	3.56	0	45-54	Controls
povrate	0	0	3.53	0	45-54	Controls
educ	0.03	0	8.38	0	45-54	Controls
(Intercept)	0.61	0.01	43.91	0	55-64	Bivariate
sdr	0	0.01	-0.43	0.67	55-64	Bivariate
(Intercept)	-0.29	0.23	-1.5	0.14	55-64	Controls
sdr	0	0.01	0.28	0.78	55-64	Controls
white	0.43	0.24	2.2	0.03	55-64	Controls
black	0.57	0.24	2.82	0	55-64	Controls
asian	0.38	0.22	2.33	0.02	55-64	Controls
povrate	0	0	2.69	0.01	55-64	Controls
educ	0.03	0	9.1	0	55-64	Controls
(Intercept)	0.63	0.01	50.98	0	65+	Bivariate
sdr	0	0.01	0.46	0.65	65+	Bivariate
(Intercept)	-0.26	0.2	-1.38	0.17	65+	Controls
sdr	0.01	0.01	0.89	0.37	65+	Controls
white	0.56	0.19	3.08	0	65+	Controls
black	0.45	0.2	2.32	0.02	65+	Controls

asian	0.58	0.21	3.31	0	65+	Controls
povrate	0	0	0.91	0.36	65+	Controls
educ	0.02	0	8.43	0	65+	Controls

Table A5: Matching Balance Statistics

Covariate	Mean Treated	Mean Control	T-Test p-value	Age Group
Age	20.919	20.916	0.133	18-24
Income (31 categories)	20.663	20.67	0.029	18-24
Education (32 categories)	19.173	19.169	0.339	18-24
Female	0.508	0.508	1	18-24
White	0.891	0.891	1	18-24
Black	0.057	0.057	1	18-24
Native American	0.011	0.011	1	18-24
Asian/Pacific Islander	0.002	0.002	1	18-24
Asian Only	0.02	0.02	1	18-24
Asian/Hawaiian	0.002	0.002	1	18-24
Other	0.002	0.002	1	18-24
Age	29.542	29.541	0.871	25-34
Income (31 categories)	22.064	22.066	0.311	25-34
Education (32 categories)	21.417	21.415	0.442	25-34
Female	0.512	0.512	1	25-34
White	0.906	0.906	1	25-34
Black	0.051	0.051	1	25-34
Native American	0.01	0.01	1	25-34
Asian/Pacific Islander	0.001	0.001	1	25-34
Asian Only	0.019	0.019	1	25-34
Asian/Hawaiian	0.002	0.002	1	25-34
Other	0.001	0.001	1	25-34
Age	39.513	39.513	1	35-44
Income (31 categories)	23.694	23.698	0.04	35-44
Education (32 categories)	21.595	21.598	0.585	35-44
Female	0.512	0.512	1	35-44
White	0.912	0.912	1	35-44
Black	0.045	0.045	1	35-44
Native American	0.009	0.009	1	35-44
Asian/Pacific Islander	0.001	0.001	1	35-44
Asian Only	0.02	0.02	1	35-44
Asian/Hawaiian	0.002	0.002	1	35-44
Other	0.001	0.001	1	35-44
Age	49.501	49.503	0.443	45-54
Income (31 categories)	24.19	24.192	0.11	45-54
Education (32 categories)	21.376	21.371	0.102	45-54
Female	0.512	0.512	1	45-54
White	0.913	0.913	1	45-54
Black	0.046	0.046	1	45-54
Native American	0.008	0.008	1	45-54
Asian/Pacific Islander	0.001	0.001	1	45-54
Asian Only	0.02	0.02	1	45-54
Asian/Hawaiian	0.001	0.001	1	45-54
Other	0	0	1	45-54
Age	59.254	59.254	0.715	55-64

Income (31 categories)	23.216	23.219	0.158	55-64
Education (32 categories)	21.058	21.053	0.174	55-64
Female	0.51	0.51	1	55-64
White	0.922	0.922	1	55-64
Black	0.043	0.043	1	55-64
Native American	0.008	0.008	1	55-64
Asian/Pacific Islander	0.001	0.001	1	55-64
Asian Only	0.018	0.018	1	55-64
Asian/Hawaiian	0.002	0.002	1	55-64
Other	0.001	0.001	1	55-64
Age	73.798	73.795	0.478	65+
Income (31 categories)	20.017	20.022	0.032	65+
Education (32 categories)	19.546	19.549	0.244	65+
Female	0.556	0.556	1	65+
White	0.933	0.933	1	65+
Black	0.037	0.037	1	65+
Native American	0.006	0.006	1	65+
Asian/Pacific Islander	0	0	1	65+
Asian Only	0.017	0.017	1	65+
Asian/Hawaiian	0.001	0.001	1	65+
Other	0	0	1	65+

Table A6: Placebo Test for Matching Design

Age Group	*ATT Estimate*	*95% CI*
18-24	-0.007	(-0.013, 0.000)
25-34	-0.003	(-0.009, 0.003)
35-44	0.006	(0.000, 0.012)
45-54	0.003	(-0.004, .009)
55-64	0.010	(0.003, 0.017)
65+	0.015	(0.009, 0.021)

Table A6 shows the results of a placebo test for the matching design. The test involves comparing turnout in SDR states during the years *before SDR implementation* to turnout in states that *never implemented SDR*. A successful test would show no difference in turnout between similar young people in pre-SDR and never-SDR states. If we find large differences in turnout, the analysis likely suffers from omitted variable bias or endogeneity. We find that turnout in pre-SDR states is modestly greater for older but not for younger voters. The placebo estimate for 18-24 year-olds is a precisely estimated zero.

Table A7: AVR and Turnout by Age (Weighted)

	Dependent variable:			
	Pr(Voted)			
	(1)	(2)	(3)	(4)
AVR	-0.007	-0.011	-0.009	-0.020*
	(0.009)	(0.010)	(0.010)	(0.009)
Age 18-24	-0.348***	-0.347***	-0.347***	-0.335***
	(0.007)	(0.007)	(0.007)	(0.006)
Age 25-34	-0.249***	-0.249***	-0.249***	-0.298***
	(0.006)	(0.006)	(0.006)	(0.006)
Age 35-44	-0.155***	-0.154***	-0.154***	-0.217***
	(0.006)	(0.006)	(0.006)	(0.006)
Age 45-54	-0.094***	-0.094***	-0.094***	-0.148***
	(0.006)	(0.006)	(0.006)	(0.006)
Age 55-64	-0.042***	-0.043***	-0.043***	-0.075***
	(0.006)	(0.006)	(0.006)	(0.006)
SDR		0.035***	0.035***	0.022***
		(0.004)	(0.004)	(0.004)
Early Voting		-0.038***	-0.038***	-0.027***
		(0.004)	(0.004)	(0.004)
No-Fault Absentee		0.010*	0.011**	0.016***
		(0.004)	(0.004)	(0.004)
Senate Election			0.009*	0.003
			(0.004)	(0.004)
Gubernatorial Election			-0.006	-0.013***
			(0.004)	(0.004)
AVR:Age 18-24	0.059***	0.056***	0.055***	0.076***
	(0.015)	(0.016)	(0.016)	(0.015)
AVR:Age 25-34	0.048***	0.045**	0.044**	0.058***
	(0.014)	(0.014)	(0.014)	(0.013)
AVR:Age 35-44	0.017	0.014	0.014	0.031*
	(0.014)	(0.014)	(0.014)	(0.014)
AVR:Age 45-54	0.014	0.014	0.013	0.027*
	(0.014)	(0.014)	(0.014)	(0.014)
AVR:Age 55-64	-0.008	-0.007	-0.007	0.002
	(0.014)	(0.014)	(0.014)	(0.013)
Constant	0.662***	0.659***	0.657***	0.190***
	(0.004)	(0.004)	(0.006)	(0.008)
Demographic Covariates	No	No	No	Yes
Observations	88,749	85,915	85,915	85,915
R^2	0.050	0.052	0.052	0.138
Adjusted R^2	0.050	0.052	0.052	0.137
Residual Std. Error	24.683 (df = 88737)	25.006 (df = 85900)	25.006 (df = 85898)	23.855 (df = 85890)
F Statistic	429.002*** (df = 11; 88737)	338.948*** (df = 14; 85900)	296.994*** (df = 16; 85898)	571.368*** (df = 24; 85890)
Note:				*p<0.05; **p<0.01; ***p<0.001

Table A8: Bivariate Lagged Dependent Variable Models (Robustness Check)

Age Group	Treatment	Estimate	SE
18-24	SDR	0.065	0.01
25-34	SDR	0.044	0.011
35-44	SDR	0.027	0.012
45-54	SDR	0.026	0.01
55-64	SDR	0.022	0.009
65+	SDR	0.032	0.006
18-24	Early Voting	-0.009	0.01
25-34	Early Voting	-0.017	0.01
35-44	Early Voting	-0.019	0.008
45-54	Early Voting	-0.017	0.007
55-64	Early Voting	-0.015	0.007
65+	Early Voting	-0.002	0.007

Note: Estimates are from bivariate models using aggregated state level data (N=980), controlling for the age group's turnout in year t - 4 (for consistency of election type). Robust standard errors are clustered by state.

APPENDIX B

Table B1: Ordinal Logistic Regression ("Fill ballot")

	Value	Std. Error	t value	p value
age_50-64	-1.392145115	0.423017010	-3.2909909	0.001
age_40-49	-2.148826524	0.445922922	-4.8188295	0.000
age_30-39	-2.516756987	0.379780451	-6.6268735	0.000
age_25-29	-2.765122581	0.399191588	-6.9268057	0.000
age_18-24	-3.225047272	0.389602844	-8.2777816	0.000
race_Black	-0.893892518	0.228832203	-3.9063231	0.000
race_Hispanic	-0.637206895	0.189326030	-3.3656592	0.001
race_Asian	-1.192684618	0.268050714	-4.4494738	0.000
race_Other	-0.659712023	0.293320530	-2.2491164	0.025
educ_Some college	0.628075488	0.169517848	3.7050700	0.000
educ_Bachelor's	1.307995043	0.248872580	5.2556816	0.000
educ_Post-grad	1.385388608	0.319297938	4.3388586	0.000
female	0.299797606	0.151446935	1.9795555	0.048
faminc_new	0.001023006	0.001966817	0.5201326	0.603

Table B2: Ordinal Logistic Regression ("Polling location")

	Value	Std. Error	t value	p value
age_50-64	-0.729407077	0.259446220	-2.811400	0.005
age_40-49	-1.206321154	0.294776566	-4.092324	0.000
age_30-39	-1.623261675	0.227430774	-7.137388	0.000
age_25-29	-1.998228492	0.245476860	-8.140191	0.000
age_18-24	-1.930948908	0.242559852	-7.960711	0.000
race_Black	-0.233627044	0.183451422	-1.273509	0.203
race_Hispanic	-0.221366926	0.156873071	-1.411121	0.158
race_Asian	-0.822916549	0.238067204	-3.456657	0.001
race_Other	-0.592018344	0.289051306	-2.048143	0.041
educ_Some college	0.321965503	0.136637470	2.356349	0.018
educ_Bachelor's	0.785045332	0.178315857	4.402555	0.000
educ_Post-grad	1.151251048	0.246422775	4.671853	0.000
female	0.247266231	0.120778210	2.047275	0.041
faminc_new	0.001927176	0.001571342	1.226452	0.220

Table B3: Ordinal Logistic Regression ("More info")

	Value	Std. Error	t value	p value
age_50-64	-0.510536538	0.267870887	-1.905905	0.057
age_40-49	-1.155869810	0.305419280	-3.784535	0.000
age_30-39	-1.488859283	0.232261224	-6.410279	0.000
age_25-29	-1.687861697	0.258081810	-6.540026	0.000
age_18-24	-1.818635203	0.253666472	-7.169395	0.000
race_Black	-0.494516753	0.214900918	-2.301138	0.021
race_Hispanic	-0.298854204	0.175226643	-1.705529	0.088
race_Asian	-0.704260957	0.258095756	-2.728681	0.006
race_Other	-0.805291589	0.267817268	-3.006870	0.003
educ_Some college	0.585547670	0.145817611	4.015617	0.000
educ_Bachelor's	1.108548768	0.192021601	5.773042	0.000
educ_Post-grad	1.408117833	0.260635229	5.402638	0.000
female	0.449356244	0.133376904	3.369071	0.001
faminc_new	-0.002922275	0.001624867	-1.798470	0.072

Table B4: Ordinal Logistic Regression ("Voting steps")

	Value	Std. Error	t value	p value
age_50-64	-0.9551933048	0.3527400	-2.7079249	0.007
age_40-49	-1.5063619941	0.3893513	-3.8689018	0.000
age_30-39	-1.9609968891	0.3106103	-6.3133681	0.000
age_25-29	-2.1898406758	0.3339467	-6.5574551	0.000
age_18-24	-2.6562003269	0.3241305	-8.1948474	0.000
race_Black	-0.6347496389	0.2203766	-2.8802953	0.004
race_Hispanic	-0.3253739465	0.1786596	-1.8211945	0.069
race_Asian	-0.9993557927	0.3050651	-3.2758777	0.001
race_Other	-0.2964282475	0.3583247	-0.8272615	0.408
educ_Some college	0.4420581740	0.1633794	2.7057157	0.007
educ_Bachelor's	1.0642181478	0.2265352	4.6978044	0.000
educ_Post-grad	1.3577331450	0.3150528	4.3095411	0.000
female	0.4191046962	0.1471752	2.8476584	0.004
faminc_new	-0.0009072333	0.0018752	-0.4838062	0.629

Table B5: Ordinal Logistic Regression ("Not enough time")

	Value	Std. Error	t value	p value
age_50-64	1.205683373	0.259812186	4.6405959	0.000
age_40-49	1.860044459	0.274172248	6.7842186	0.000
age_30-39	2.594080705	0.235649102	11.0082350	0.000
age_25-29	2.919794314	0.254567088	11.4696457	0.000
age_18-24	2.665551974	0.255983535	10.4129821	0.000
race_Black	0.260068121	0.190337025	1.3663559	0.172
race_Hispanic	0.635398901	0.142934150	4.4453960	0.000
race_Asian	0.949744162	0.254487023	3.7319945	0.000
race_Other	0.321432537	0.284070889	1.1315223	0.258
educ_Some college	-0.544182689	0.134967998	-4.0319387	0.000
educ_Bachelor's	-0.729593836	0.171638501	-4.2507586	0.000
educ_Post-grad	-0.892341959	0.244215557	-3.6539112	0.000
female	-0.069551199	0.116410991	-0.5974625	0.550
faminc_new	-0.004109405	0.001565737	-2.6245823	0.009

Table B6: Ordinal Logistic Regression ("Unpredictable schedule")

	Value	Std. Error	t value	p value
age_50-64	1.574376562	0.264680504	5.9482151	0.000
age_40-49	2.329653771	0.272357328	8.5536666	0.000
age_30-39	2.661329219	0.235021136	11.3237867	0.000
age_25-29	3.046646489	0.253364327	12.0247650	0.000
age_18-24	2.791865293	0.246603134	11.3212888	0.000
race_Black	0.231470999	0.172541445	1.3415386	0.180
race_Hispanic	0.537016077	0.142496377	3.7686297	0.000
race_Asian	0.800003319	0.213595667	3.7454099	0.000
race_Other	0.420145580	0.272668514	1.5408658	0.123
educ_Some college	-0.371378339	0.127081125	-2.9223721	0.003
educ_Bachelor's	-0.533709703	0.167155652	-3.1928906	0.001
educ_Post-grad	-0.775575228	0.242032685	-3.2044235	0.001
female	-0.036799502	0.112047959	-0.3284263	0.743
faminc_new	-0.003193009	0.001404696	-2.2730951	0.023

Table B7: Ordinal Logistic Regression ("Struggle to balance")

	Value	Std. Error	t value	p value
age_50-64	1.200659674	0.245498011	4.8907104	0.000
age_40-49	2.045001260	0.250856020	8.1520916	0.000
age_30-39	2.609154540	0.214375018	12.1709822	0.000
age_25-29	2.932545529	0.237718146	12.3362292	0.000
age_18-24	2.838062219	0.234890524	12.0824892	0.000
race_Black	0.224680479	0.174397128	1.2883267	0.198
race_Hispanic	0.563207121	0.147666182	3.8140562	0.000
race_Asian	0.741365638	0.237683252	3.1191329	0.002
race_Other	-0.096896291	0.260374878	-0.3721415	0.710
educ_Some college	-0.408971165	0.131042727	-3.1208994	0.002
educ_Bachelor's	-0.327431555	0.159006339	-2.0592359	0.039
educ_Post-grad	-0.517126092	0.234698948	-2.2033592	0.028
female	0.038468878	0.112143510	0.3430326	0.732
faminc_new	-0.003753378	0.001442759	-2.6015272	0.009

Table B8: Ordinal Logistic Regression ("Transportation issues")

	Value	Std. Error	t value	p value
age_50-64	0.523324650	0.235956526	2.2178859	0.027
age_40-49	1.305852107	0.253336349	5.1546180	0.000
age_30-39	1.942354989	0.211236243	9.1951786	0.000
age_25-29	2.062950927	0.227481422	9.0686567	0.000
age_18-24	1.808338131	0.222285547	8.1352034	0.000
race_Black	0.466134170	0.174248383	2.6751133	0.007
race_Hispanic	0.490481110	0.144178502	3.4019018	0.001
race_Asian	0.733640043	0.220101150	3.3331949	0.001
race_Other	0.472212680	0.292378039	1.6150758	0.106
educ_Some college	-0.613202925	0.128275667	-4.7803526	0.000
educ_Bachelor's	-1.026708654	0.168686115	-6.0865037	0.000
educ_Post-grad	-1.007688972	0.242569271	-4.1542318	0.000
female	-0.040978983	0.113494170	-0.3610669	0.718
faminc_new	-0.002047524	0.001481012	-1.3825166	0.167

Table B9: Ordinal Logistic Regression ("Could be earning money")

	Value	Std. Error	t value	p value
age_50-64	1.239342272	0.229708261	5.3952882	0.000
age_40-49	2.166807552	0.254239035	8.5227178	0.000
age_30-39	2.629619017	0.209306738	12.5634704	0.000
age_25-29	2.672341593	0.222898662	11.9890428	0.000
age_18-24	2.276660393	0.231479647	9.8352508	0.000
race_Black	0.438346134	0.161218603	2.7189550	0.007
race_Hispanic	0.398079577	0.148174930	2.6865515	0.007
race_Asian	0.785559798	0.222861907	3.5248725	0.000
race_Other	0.269541593	0.261314282	1.0314844	0.302
educ_Some college	-0.279554397	0.128383396	-2.1774965	0.029
educ_Bachelor's	-0.534650914	0.163578716	-3.2684626	0.001
educ_Post-grad	-0.435977643	0.231897801	-1.8800422	0.060
female	0.179881629	0.109175940	1.6476307	0.099
faminc_new	-0.001078373	0.001393722	-0.7737363	0.439

Table B10: Ordinal Logistic Regression ("Offline parts of voting")

	Value	Std. Error	t value	p value
age_50-64	0.452658862	0.204308358	2.2155670	0.027
age_40-49	1.338544663	0.228552214	5.8566252	0.000
age_30-39	1.821645440	0.179742294	10.1347624	0.000
age_25-29	2.102230114	0.202020181	10.4060402	0.000
age_18-24	1.897355654	0.202321612	9.3779188	0.000
race_Black	0.312155504	0.174068824	1.7932878	0.073
race_Hispanic	0.584779106	0.143228585	4.0828380	0.000
race_Asian	0.793211429	0.240623077	3.2964894	0.001
race_Other	0.072004861	0.273245980	0.2635166	0.792
educ_Some college	-0.448165839	0.127817680	-3.5062899	0.000
educ_Bachelor's	-0.585520213	0.154936555	-3.7790966	0.000
educ_Post-grad	-0.846920415	0.235889725	-3.5903235	0.000
female	0.116819888	0.108863193	1.0730889	0.283
faminc_new	-0.002666329	0.001429154	-1.8656693	0.062

Table B11: Ordinal Logistic Regression ("Mail voting a hassle")

	Value	Std. Error	t value	p value
age_50-64	0.0964859370	0.15438427	0.62497257	0.532
age_40-49	0.4120325547	0.19019518	2.16636699	0.030
age_30-39	0.7940144489	0.14014242	5.66576823	0.000
age_25-29	0.9121413102	0.16304841	5.59429738	0.000
age_18-24	0.6872338685	0.16708606	4.11305326	0.000
race_Black	-0.0670160303	0.15122668	-0.44314953	0.658
race_Hispanic	0.1635675012	0.13267082	1.23288225	0.218
race_Asian	-0.2125117693	0.22507476	-0.94418302	0.345
race_Other	0.1049919175	0.23044244	0.45561016	0.649
educ_Some college	-0.3304763058	0.11197648	-2.95130102	0.003
educ_Bachelor's	-0.1830128555	0.13853698	-1.32103971	0.186
educ_Post-grad	-0.1550634490	0.18001051	-0.86141330	0.389
female	0.2602509100	0.09498054	2.74004462	0.006
faminc_new	-0.0001242671	0.00126871	-0.09794754	0.922

Table B12: Ordinal Logistic Regression ("Voter documentation")

	Value	Std. Error	t value	p value	p value
age_50-64	0.0964859370	0.15438427	0.62497257	0.532	0.532
age_40-49	0.4120325547	0.19019518	2.16636699	0.030	0.030
age_30-39	0.7940144489	0.14014242	5.66576823	0.000	0.000
age_25-29	0.9121413102	0.16304841	5.59429738	0.000	0.000
age_18-24	0.6872338685	0.16708606	4.11305326	0.000	0.000
race_Black	-0.0670160303	0.15122668	-0.44314953	0.658	0.658
race_Hispanic	0.1635675012	0.13267082	1.23288225	0.218	0.218
race_Asian	-0.2125117693	0.22507476	-0.94418302	0.345	0.345
race_Other	0.1049919175	0.23044244	0.45561016	0.649	0.649
educ_Some college	-0.3304763058	0.11197648	-2.95130102	0.003	0.003
educ_Bachelor's	-0.1830128555	0.13853698	-1.32103971	0.186	0.186
educ_Post-grad	-0.1550634490	0.18001051	-0.86141330	0.389	0.389
female	0.2602509100	0.09498054	2.74004462	0.006	0.006
faminc_new	-0.0001242671	0.00126871	-0.09794754	0.922	0.922

Table B13: Ordinal Logistic Regression ("Ease of registration")

	Value	Std. Error	t value	p value
age_50-64	-0.787361226	0.280192915	-2.8100683	0.005
age_40-49	-1.100986592	0.346525197	-3.1772194	0.001
age_30-39	-1.477866049	0.243467176	-6.0700833	0.000
age_25-29	-2.085600173	0.267756717	-7.7891610	0.000
age_18-24	-2.005124934	0.258112081	-7.7684273	0.000
race_Black	-1.008986074	0.216480056	-4.6608731	0.000
race_Hispanic	-0.957043504	0.177640694	-5.3875240	0.000
race_Asian	-0.893214170	0.293021111	-3.0482929	0.002
race_Other	-0.994266286	0.286902588	-3.4655187	0.001
educ_Some college	0.758024844	0.165343110	4.5845566	0.000
educ_Bachelor's	0.827172564	0.217511762	3.8028866	0.000
educ_Post-grad	0.676590111	0.294569464	2.2968780	0.022
female	0.105871494	0.144525346	0.7325462	0.464
faminc_new	-0.002431592	0.001923254	-1.2643114	0.206

Table B14: Ordinal Logistic Regression ("Ease of voting")

	Value	Std. Error	t value	p value
age_50-64	-1.120638858	0.302816866	-3.7007148	0.000
age_40-49	-1.670060107	0.347974821	-4.7993705	0.000
age_30-39	-2.037826260	0.259802006	-7.8437665	0.000
age_25-29	-2.458496577	0.282660753	-8.6976934	0.000
age_18-24	-2.338625011	0.278622213	-8.3935340	0.000
race_Black	-0.854599217	0.215253941	-3.9701908	0.000
race_Hispanic	-0.744386001	0.171568600	-4.3387077	0.000
race_Asian	-0.862926945	0.313098009	-2.7560921	0.006
race_Other	-1.166046647	0.294526937	-3.9590492	0.000
educ_Some college	0.547752585	0.160398865	3.4149405	0.001
educ_Bachelor's	0.870943252	0.218354410	3.9886680	0.000
educ_Post-grad	1.414102313	0.307939976	4.5921362	0.000
female	0.215844293	0.141444841	1.5259962	0.127
faminc_new	0.001582686	0.002033971	0.7781262	0.436

Table B15: Generalized Linear Regression Results (Information Costs)

Regression Results

	Dependent variable:				
	Fill ballot	Polling location	More info	Voting steps	Voting info. scale
	(1)	(2)	(3)	(4)	(5)
Age 50-64	-0.038***	-0.055**	-0.033*	-0.036**	-0.038***
	(0.014)	(0.023)	(0.019)	(0.014)	(0.014)
Age 40-49	-0.085***	-0.115***	-0.101***	-0.083***	-0.097***
	(0.025)	(0.034)	(0.030)	(0.026)	(0.023)
Age 30-39	-0.112***	-0.170***	-0.133***	-0.112***	-0.129***
	(0.015)	(0.024)	(0.020)	(0.017)	(0.014)
Age 25-29	-0.148***	-0.247***	-0.172***	-0.154***	-0.177***
	(0.023)	(0.032)	(0.028)	(0.025)	(0.021)
Age 18-24	-0.234***	-0.242***	-0.212***	-0.244***	-0.222***
	(0.027)	(0.032)	(0.031)	(0.029)	(0.022)
Black	-0.088***	-0.030	-0.064**	-0.066**	-0.058**
	(0.027)	(0.029)	(0.031)	(0.028)	(0.023)
Hispanic	-0.054***	-0.026	-0.031	-0.019	-0.028
	(0.020)	(0.026)	(0.024)	(0.020)	(0.017)
Asian	-0.103***	-0.124***	-0.065*	-0.114**	-0.103***
	(0.036)	(0.048)	(0.039)	(0.046)	(0.033)
Other race	-0.045*	-0.082*	-0.101**	-0.025	-0.056**
	(0.026)	(0.046)	(0.040)	(0.031)	(0.026)
Some college	0.055***	0.044**	0.068***	0.037**	0.053***
	(0.016)	(0.021)	(0.019)	(0.017)	(0.014)
Bachelor's	0.093***	0.101***	0.117***	0.086***	0.102***
	(0.016)	(0.023)	(0.019)	(0.017)	(0.015)
Post-grad	0.084***	0.127***	0.123***	0.087***	0.106***
	(0.016)	(0.024)	(0.019)	(0.017)	(0.015)
Female	0.019	0.033*	0.046***	0.029**	0.032***
	(0.013)	(0.017)	(0.015)	(0.013)	(0.011)
Family income	0.0002	0.0004*	-0.0002	-0.00000	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Constant	0.954***	0.877***	0.894***	0.941***	0.914***
	(0.011)	(0.021)	(0.019)	(0.013)	(0.012)
Observations	1,961	1,956	1,962	1,960	1,913
Note:					*p<0.1; **p<0.05; ***p<0.01

Table B16: Generalized Linear Regression Results (Time and Scheduling Costs)

Regression Results

	Dependent variable:		
	Difficulty balancing	Hard to find the time	Unpredictable schedule
	(1)	(2)	(3)
Age 50-64	0.090***	0.078***	0.129***
	(0.020)	(0.018)	(0.023)
Age 40-49	0.216***	0.169***	0.246***
	(0.032)	(0.030)	(0.034)
Age 30-39	0.329***	0.308***	0.312***
	(0.023)	(0.024)	(0.024)
Age 25-29	0.398***	0.377***	0.396***
	(0.033)	(0.032)	(0.032)
Age 18-24	0.381***	0.331***	0.343***
	(0.031)	(0.031)	(0.029)
Black	0.029	0.038	0.030
	(0.028)	(0.030)	(0.029)
Hispanic	0.107***	0.115***	0.099***
	(0.029)	(0.027)	(0.028)
Asian	0.133***	0.160***	0.135***
	(0.051)	(0.052)	(0.048)
Other race	-0.008	0.052	0.058
	(0.036)	(0.040)	(0.042)
Some college	-0.055***	-0.073***	-0.054***
	(0.020)	(0.020)	(0.021)
Bachelor's	-0.037	-0.089***	-0.068***
	(0.025)	(0.024)	(0.026)
Post-grad	-0.055*	-0.096***	-0.088***
	(0.030)	(0.027)	(0.030)
Female	0.010	-0.001	-0.004
	(0.017)	(0.017)	(0.017)
Family income	-0.001***	-0.001***	-0.001***
	(0.0002)	(0.0002)	(0.0002)
Constant	0.066***	0.081***	0.070***
	(0.016)	(0.015)	(0.016)
Observations	1,969	1,964	1,960
Note:			*p<0.1; **p<0.05; ***p<0.01

Table B17: Generalized Linear Regression Results (Transportation & Opportunity Costs)

Regression Results

	Dependent variable:	
	Transportation issues	Earning money instead
	(1)	(2)
Age 50-64	0.045**	0.103***
	(0.023)	(0.020)
Age 40-49	0.156***	0.262***
	(0.034)	(0.036)
Age 30-39	0.277***	0.353***
	(0.027)	(0.024)
Age 25-29	0.297***	0.364***
	(0.033)	(0.030)
Age 18-24	0.252***	0.285***
	(0.032)	(0.031)
Black	0.076**	0.069**
	(0.032)	(0.029)
Hispanic	0.086***	0.069**
	(0.028)	(0.028)
Asian	0.119**	0.141***
	(0.049)	(0.049)
Other race	0.076*	0.040
	(0.045)	(0.040)
Some college	-0.100***	-0.041**
	(0.021)	(0.021)
Bachelor's	-0.149***	-0.072***
	(0.024)	(0.025)
Post-grad	-0.133***	-0.054*
	(0.030)	(0.031)
Female	-0.004	0.035**
	(0.018)	(0.017)
Family income	-0.0004*	-0.0003
	(0.0002)	(0.0002)
Constant	0.141***	0.050***
	(0.023)	(0.016)
Observations	1,957	1,961
Note:		*p<0.1; **p<0.05; ***p<0.01

Table B18: Generalized Linear Regression Results (Offline Voting Tasks)

Regression Results

	Dependent variable:	
	Not online	Mail hassle
	(1)	(2)
Age 50-64	0.043**	0.015
	(0.020)	(0.029)
Age 40-49	0.179***	0.085**
	(0.033)	(0.039)
Age 30-39	0.267***	0.166***
	(0.024)	(0.029)
Age 25-29	0.328***	0.190***
	(0.032)	(0.036)
Age 18-24	0.288***	0.143***
	(0.031)	(0.035)
Black	0.044	-0.022
	(0.030)	(0.032)
Hispanic	0.105***	0.036
	(0.027)	(0.030)
Asian	0.130***	-0.058
	(0.050)	(0.049)
Other race	0.017	0.027
	(0.039)	(0.049)
Some college	-0.072***	-0.065***
	(0.021)	(0.023)
Bachelor's	-0.087***	-0.031
	(0.023)	(0.029)
Post-grad	-0.099***	-0.023
	(0.030)	(0.037)
Female	0.026	0.057***
	(0.017)	(0.020)
Family income	-0.001**	-0.00005
	(0.0002)	(0.0003)
Constant	0.114***	0.271***
	(0.020)	(0.024)
Observations	1,958	1,962
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table B19: Generalized Linear Regression Results (Voter ID Documentation)

Regression Results

	Dependent variable:
	Voter ID documentation
Age 50-64	-0.053**
	(0.023)
Age 40-49	-0.091***
	(0.033)
Age 30-39	-0.158***
	(0.025)
Age 25-29	-0.203***
	(0.034)
Age 18-24	-0.283***
	(0.037)
Black	-0.103***
	(0.035)
Hispanic	-0.174***
	(0.032)
Asian	-0.175***
	(0.057)
Other race	-0.043
	(0.036)
Some college	0.109***
	(0.023)
Bachelor's	0.149***
	(0.023)
Post-grad	0.153***
	(0.023)
Female	0.038**
	(0.018)
Family income	-0.0005*
	(0.0003)
Constant	0.901***
	(0.018)
Observations	1,962
Note:	*p<0.1; **p<0.05; ***p<0.01

Figure B1: Voting Information Varies by Registration Status, Age

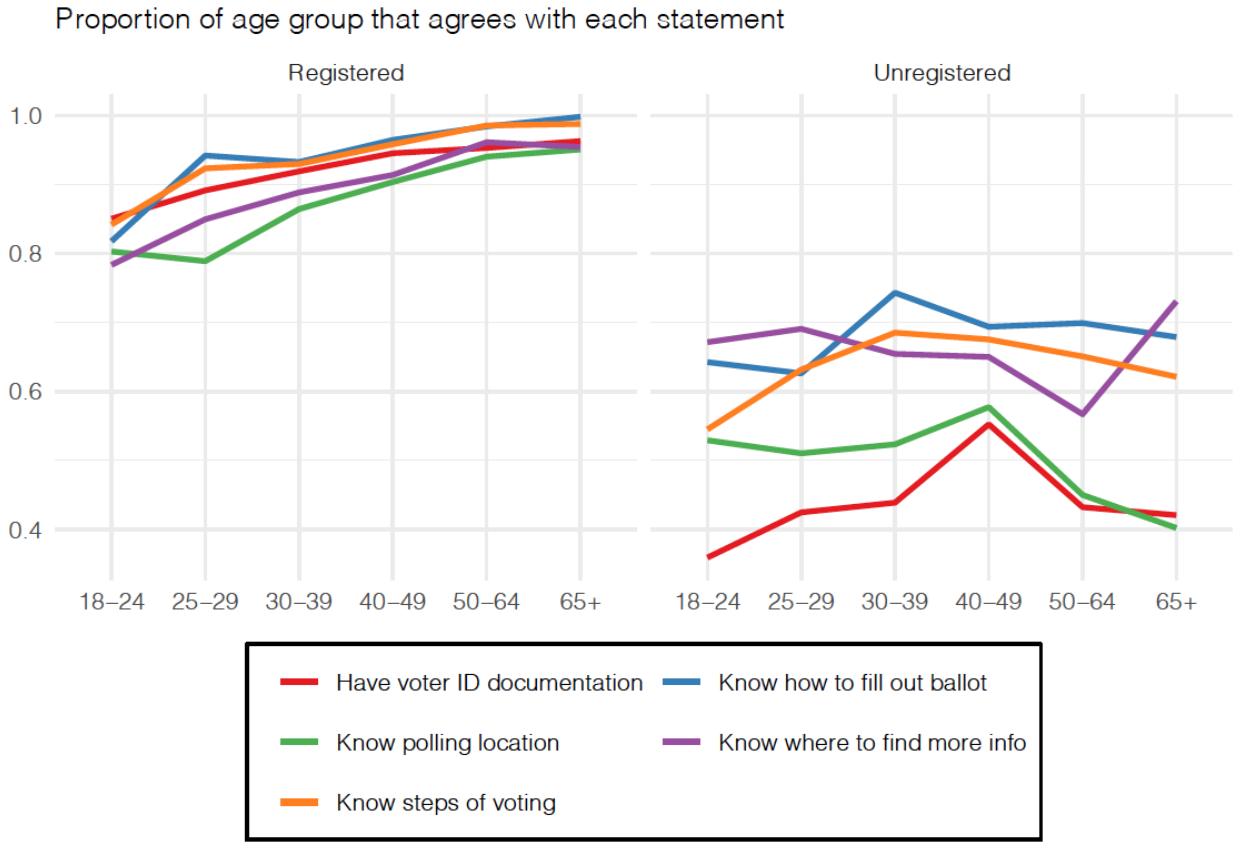


Figure B2: Voting Barriers Vary by Age, Registration Status

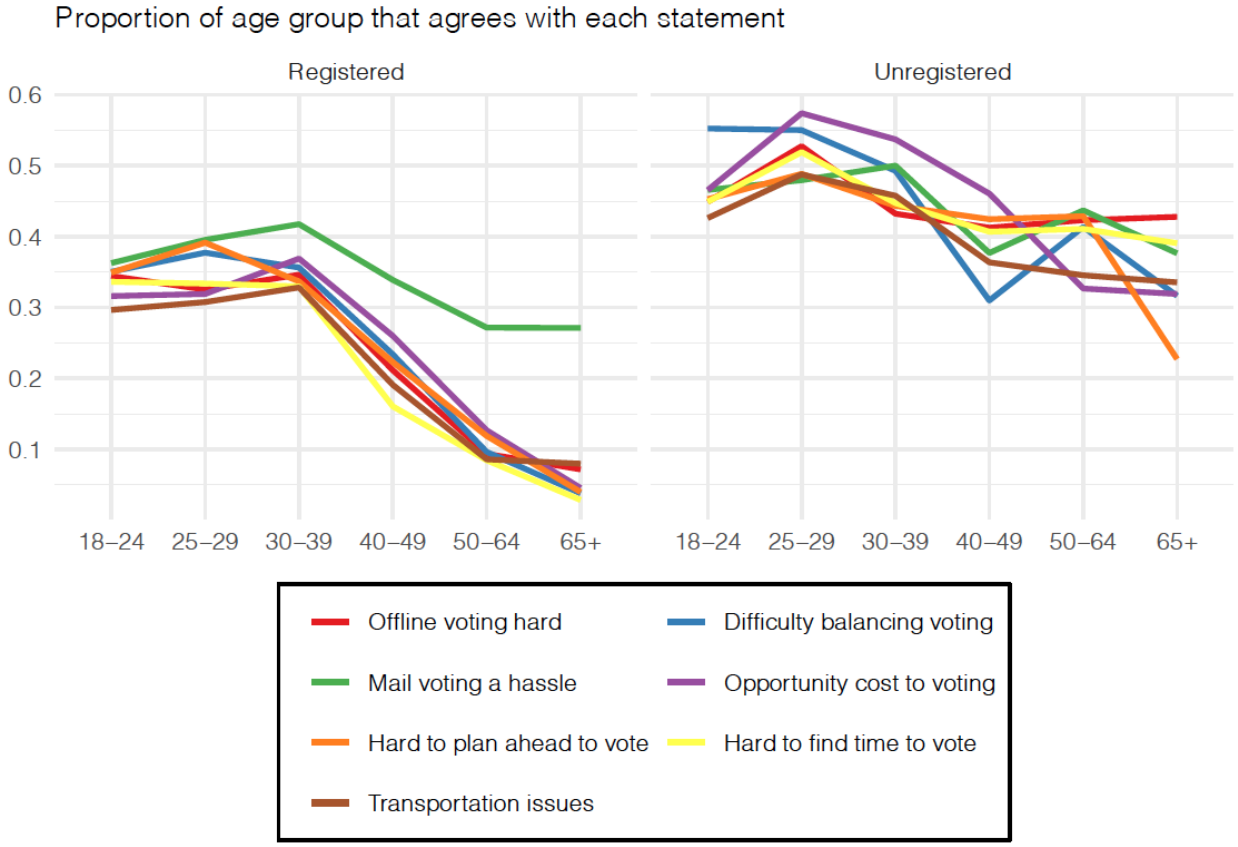


Table B20: Generalized Linear Regression Results (Registration and Voting Are Easy)

Regression Results

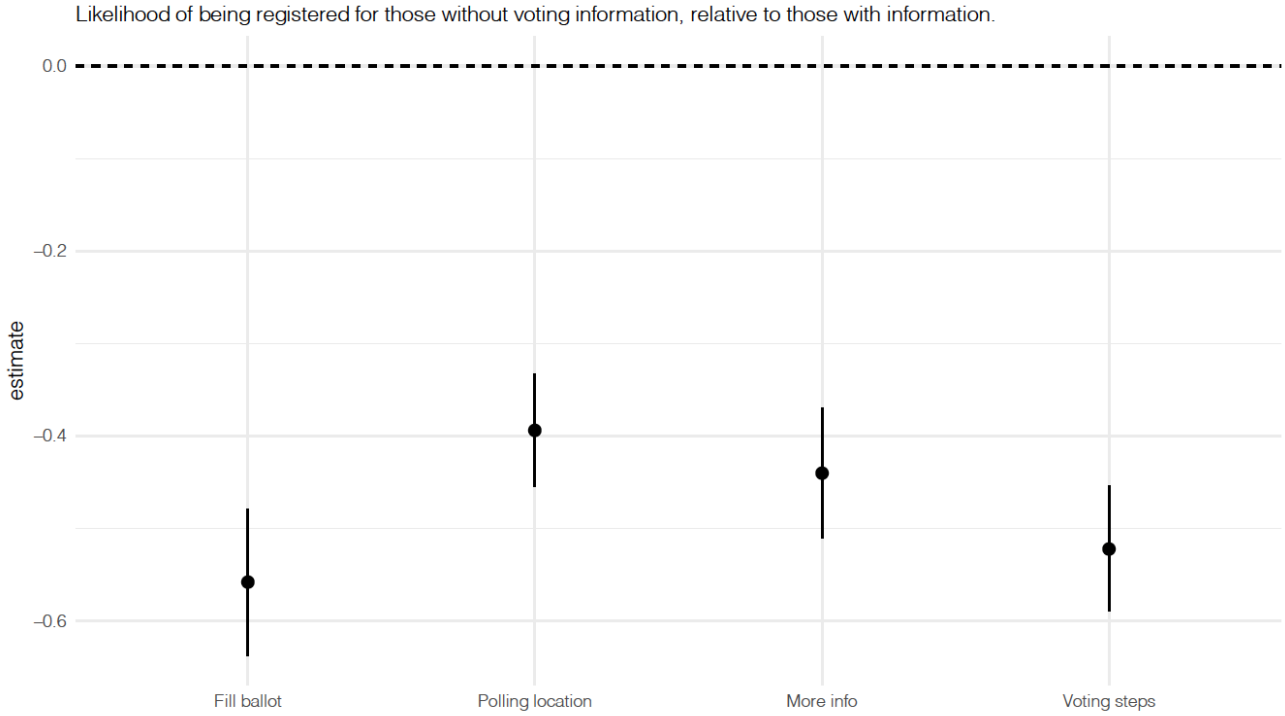
	Dependent variable:	
	Registration easy	Voting easy
	(1)	(2)
age_50-64	-0.213**	-0.281***
	(0.106)	(0.103)
age_40-49	-0.135	-0.168*
	(0.112)	(0.101)
age_30-39	-0.136	-0.169**
	(0.088)	(0.076)
age_25-29	-0.211**	-0.281***
	(0.092)	(0.081)
age_18-24	-0.221**	-0.236***
	(0.090)	(0.078)
registered	0.120	0.100
	(0.078)	(0.065)
race_Black	-0.072***	-0.061**
	(0.027)	(0.028)
race_Hispanic	-0.066***	-0.040**
	(0.022)	(0.020)
race_Asian	-0.052	-0.056
	(0.039)	(0.040)
race_Other	-0.085***	-0.129***
	(0.032)	(0.039)
educ_Some college	0.040**	0.017
	(0.016)	(0.016)
educ_Bachelor's	0.040**	0.039**
	(0.017)	(0.018)
educ_Post-grad	0.027	0.068***
	(0.023)	(0.017)
female	-0.009	0.003
	(0.013)	(0.013)
faminc_new	-0.0002	0.0002
	(0.0002)	(0.0002)
age_50-64:registered	0.201*	0.268**
	(0.107)	(0.104)
age_40-49:registered	0.099	0.113
	(0.114)	(0.103)
age_30-39:registered	0.091	0.090
	(0.088)	(0.077)
age_25-29:registered	0.101	0.168**
	(0.097)	(0.085)
age_18-24:registered	0.164*	0.168**
	(0.093)	(0.082)
Constant	0.854***	0.866***
	(0.079)	(0.066)
Observations	1,875	1,848
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table B21: Generalized Linear Regression ("Registered to Vote")

```
               Estimate Std. Error t value Pr(>|t|)
(Intercept)    0.9000807  0.0180499  49.866  < 2e-16 ***
age_50-64     -0.0853894  0.0224037  -3.811 0.000142 ***
age_40-49     -0.1880005  0.0349630  -5.377 8.47e-08 ***
age_30-39     -0.2228094  0.0244964  -9.096  < 2e-16 ***
age_25-29     -0.3045105  0.0349226  -8.720  < 2e-16 ***
age_18-24     -0.4088480  0.0357859 -11.425  < 2e-16 ***
race_black    -0.1116281  0.0350589  -3.184 0.001475 **
race_hispanic -0.1237578  0.0309871  -3.994 6.74e-05 ***
race_asian    -0.1857000  0.0590086  -3.147 0.001674 **
race_other     0.0381039  0.0350844   1.086 0.277584
some_college   0.1183955  0.0226454   5.228 1.89e-07 ***
bachelors      0.1333513  0.0253587   5.259 1.61e-07 ***
post_grad      0.1223909  0.0278370   4.397 1.16e-05 ***
female         0.0760287  0.0182306   4.170 3.17e-05 ***
faminc_new    -0.0004302  0.0002605  -1.651 0.098859 .
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

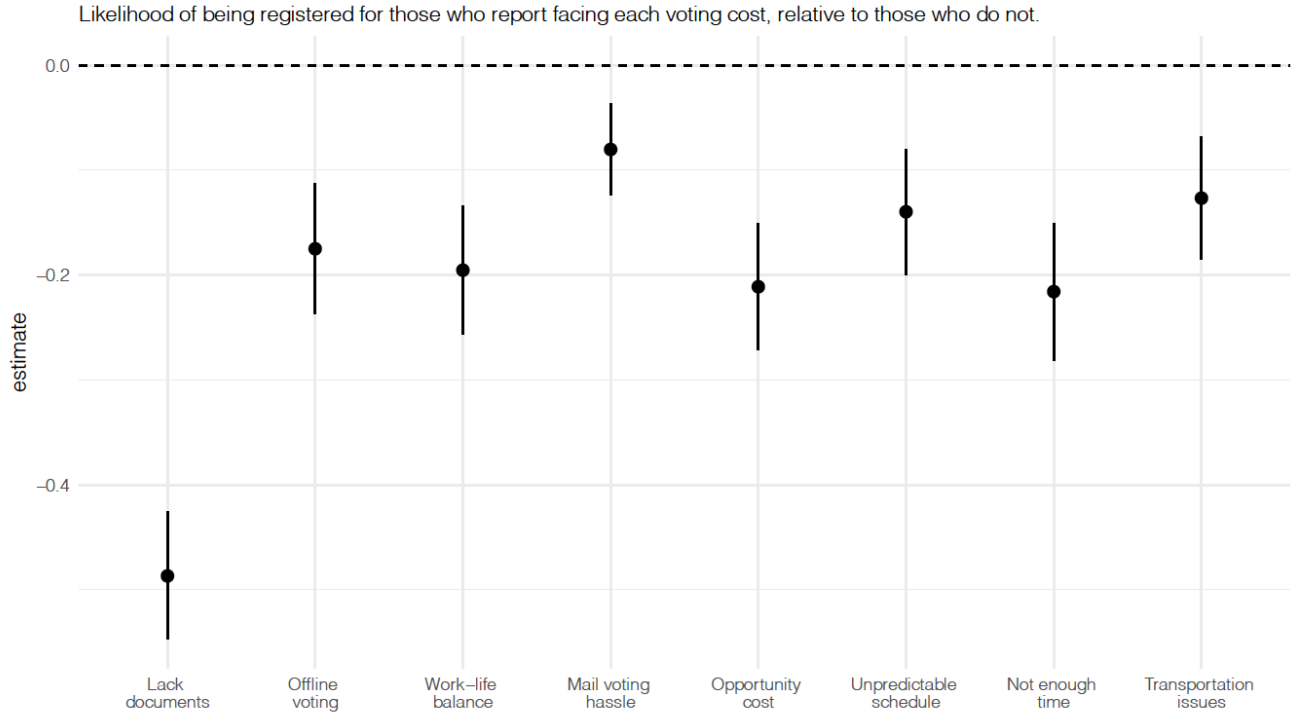
Race and education are significant predictors of registration status.

Figure B3: People Who Lack Voting Info. Significantly Less Likely to Be Registered to Vote



Note: Estimates and 95% confidence intervals are from a generalized linear regression on age group, with controls for educational attainment, gender, race and ethnicity, and family income.

Figure B4: Higher Voting Costs Predict Lower Likelihood of Being Registered to Vote



Note: Estimates and 95% confidence intervals are from a generalized linear regression on age group, with controls for educational attainment, gender, race and ethnicity, and family income.

APPENDIX C

Table C1: Balance Summary Across All Treatment Pairs

	Type	Max.Diff.Un
age_group_18-24	Binary	0.0071
age_group_25-29	Binary	0.0109
age_group_30-39	Binary	0.0310
age_group_40-49	Binary	0.0272
age_group_50-64	Binary	0.0205
age_group_65+	Binary	0.0112
female	Binary	0.0155
educ_group_HS or less	Binary	0.0303
educ_group_Some college	Binary	0.0285
educ_group_Bachelor's	Binary	0.0415
educ_group_Postgraduate	Binary	0.0163
income_group_Less than $50K	Binary	0.0172
income_group_$50-99K	Binary	0.0563
income_group_$100-149K	Binary	0.0325
income_group_$150K or more	Binary	0.0065
race2_Asian	Binary	0.0120
race2_Black	Binary	0.0008
race2_Mixed Race	Binary	0.0053
race2_Native/Indian	Binary	0.0031
race2_Other	Binary	0.0063
race2_Pacific Islander	Binary	0.0041
race2_White	Binary	0.0066
hispanic	Binary	0.0009

Sample sizes

	control	generic_treat	youth_treat
All	973	975	969

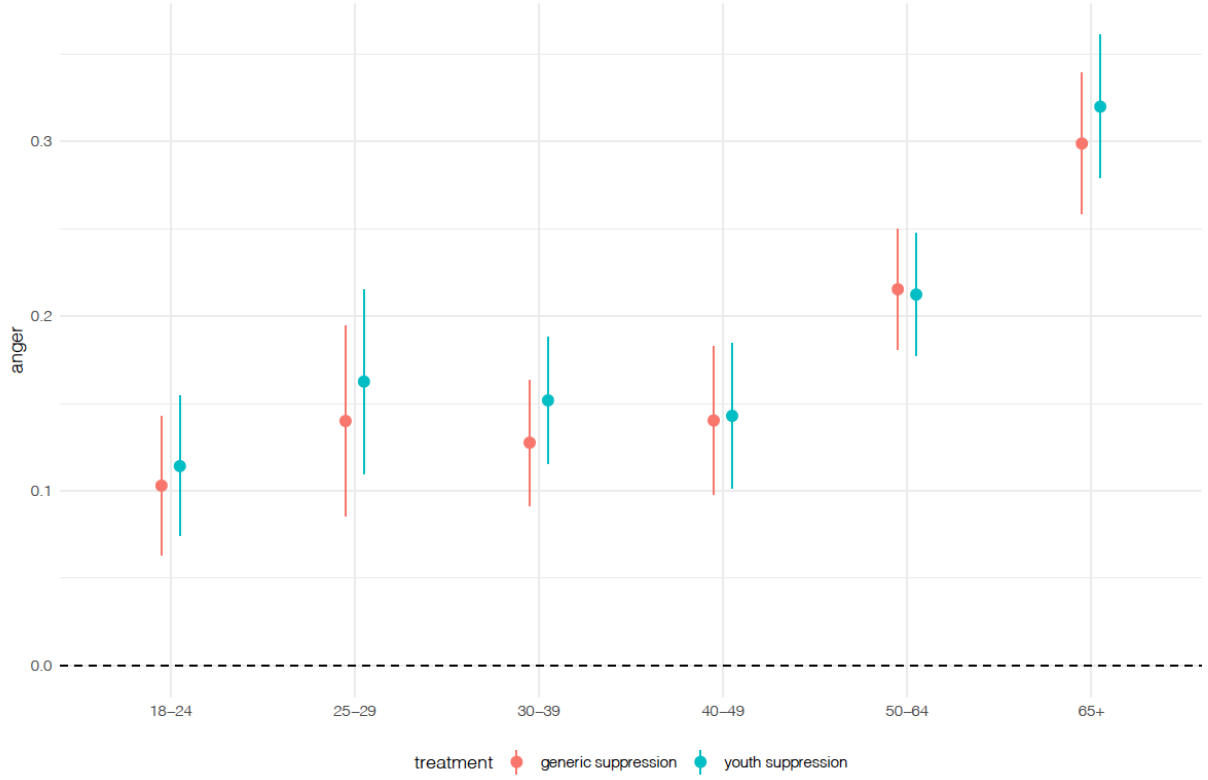
Table C2: Pairwise T-Tests of Anger Among Control Group Respondents by Age Group (with Bonferroni Correction)

	.y.	group1	group2	n1	n2	p	p.signif	p.adj	p.adj.signif
*	<chr>	<chr>	<chr>	<int>	<int>	<dbl>	<chr>	<dbl>	<chr>
1	anger	65+	18-24	277	216	0.000000049	****	0.00000074	****
2	anger	65+	25-29	277	140	0.0013	**	0.015	*
3	anger	18-24	25-29	216	140	0.149	ns	1	ns
4	anger	65+	30-39	277	318	0.00014	***	0.0021	**
5	anger	18-24	30-39	216	318	0.038	*	0.567	ns
6	anger	25-29	30-39	140	318	0.792	ns	1	ns
7	anger	65+	40-49	277	259	0.00044	***	0.0067	**
8	anger	18-24	40-49	216	259	0.036	*	0.542	ns
9	anger	25-29	40-49	140	259	0.727	ns	1	ns
10	anger	30-39	40-49	318	259	0.906	ns	1	ns
11	anger	65+	50-64	277	408	0.063	ns	0.947	ns
12	anger	18-24	50-64	216	408	0.000029	****	0.00044	***
13	anger	25-29	50-64	140	408	0.045	*	0.682	ns
14	anger	30-39	50-64	318	408	0.024	*	0.358	ns
15	anger	40-49	50-64	259	408	0.045	*	0.673	ns

Table C3: Pairwise T-Tests of Reactance Among Control Group Respondents by Age Group (with Bonferroni Correction)

.y.	group1	group2	n1	n2	p	p.signif	p.adj	p.adj.signif
<chr>	*<chr>*	*<chr>*	*<int>*	*<int>*	*<dbl>*	*<chr>*	*<dbl>*	*<chr>*
reactance	65+	18-24	278	217	0.000233	***	0.00349	**
reactance	65+	25-29	278	140	0.00819	**	0.123	ns
reactance	18-24	25-29	217	140	0.581	ns	1	ns
reactance	65+	30-39	278	318	0.00195	**	0.0293	*
reactance	18-24	30-39	217	318	0.369	ns	1	ns
reactance	25-29	30-39	140	318	0.848	ns	1	ns
reactance	65+	40-49	278	259	0.0136	*	0.204	ns
reactance	18-24	40-49	217	259	0.189	ns	1	ns
reactance	25-29	40-49	140	259	0.561	ns	1	ns
reactance	30-39	40-49	318	259	0.62	ns	1	ns
reactance	65+	50-64	278	408	0.316	ns	1	ns
reactance	18-24	50-64	217	408	0.00233	**	0.035	*
reactance	25-29	50-64	140	408	0.0451	*	0.677	ns
reactance	30-39	50-64	318	408	0.0182	*	0.273	ns
reactance	40-49	50-64	259	408	0.0886	ns	1	ns

Figure C1: Effect of Treatment on Anger by Age Group



Estimates and 95% confidence intervals are from OLS regressions of anger on treatment condition.

Table C4: Interaction Effect of Treatment and Democratic Affiliation

Regression Results

	Dependent variable:				
	vote in 2022				
	18-29	30-39	40-49	50-64	65+
generic treatment	-0.020	-0.057*	0.026	-0.014	0.030
	(0.030)	(0.033)	(0.035)	(0.026)	(0.025)
youth treatment	0.006	-0.003	-0.021	-0.035	0.013
	(0.029)	(0.033)	(0.034)	(0.026)	(0.026)
Democrat	0.096***	0.078**	0.111***	0.060**	0.078***
	(0.029)	(0.033)	(0.034)	(0.027)	(0.027)
generic treatment:Democrat	0.026	0.086*	-0.068	0.013	-0.034
	(0.041)	(0.045)	(0.049)	(0.038)	(0.038)
youth treatment:Democrat	-0.003	-0.003	0.031	0.051	-0.021
	(0.041)	(0.046)	(0.048)	(0.038)	(0.039)
constant	0.651***	0.721***	0.749***	0.823***	0.865***
	(0.020)	(0.023)	(0.025)	(0.018)	(0.018)
Observations	1,086	969	785	1,219	837
Note:					*p<0.1; **p<0.05; ***p<0.01

Table C5: Three-Way Interaction Effect of Treatment, Democratic Affiliation, and Belief that Youth Typically Support Democrats (by Age Group)

Regression Results

	Dependent variable:				
	vote in 2022				
	18-29	30-39	40-49	50-64	65+
generic treatment	-0.062*	-0.084**	0.025	-0.025	0.091**
	(0.036)	(0.039)	(0.043)	(0.035)	(0.036)
youth treatment	-0.013	-0.010	-0.014	-0.057	0.055
	(0.036)	(0.039)	(0.042)	(0.035)	(0.037)
Democrat	0.022	0.067	0.129**	0.028	0.152***
	(0.045)	(0.048)	(0.053)	(0.040)	(0.039)
youth Democrats	-0.010	0.107**	0.137***	0.102***	0.162***
	(0.041)	(0.049)	(0.051)	(0.036)	(0.036)
generic treat:Democrat	0.058	0.067	-0.137*	0.083	-0.096*
	(0.064)	(0.069)	(0.075)	(0.055)	(0.055)
youth treat:Democrat	0.024	0.048	0.018	0.135**	-0.068
	(0.062)	(0.070)	(0.074)	(0.055)	(0.055)
generic treat:youth Democrats	0.137**	0.074	0.009	0.019	-0.114**
	(0.063)	(0.069)	(0.070)	(0.050)	(0.050)
youth treat:youth Democrats	0.056	0.021	0.006	0.047	-0.074
	(0.061)	(0.069)	(0.071)	(0.051)	(0.052)
Democrat:youth Democrats	0.111*	-0.039	-0.089	0.029	-0.138**
	(0.061)	(0.068)	(0.071)	(0.054)	(0.054)
generic treat:Democrat:youth Democrats	-0.127	-0.015	0.096	-0.110	0.115
	(0.089)	(0.096)	(0.102)	(0.076)	(0.076)
youth treat:Democrat:youth Democrats	-0.065	-0.087	0.00002	-0.146*	0.085
	(0.086)	(0.098)	(0.101)	(0.076)	(0.077)
constant	0.654***	0.686***	0.696***	0.776***	0.779***
	(0.025)	(0.028)	(0.032)	(0.024)	(0.026)
Observations	1,086	969	785	1,219	837
Note:					*p<0.1; **p<0.05; ***p<0.01

Table C6: Interaction Effect of Treatment and Warmth Toward Youth

Regression Results

	Dependent variable:				
	anger				
	18-29	30-39	40-49	50-64	65+
generic treatment	0.034	0.068	0.011	0.136**	0.093
	(0.053)	(0.053)	(0.069)	(0.058)	(0.080)
youth treatment	0.056	0.193***	0.060	0.007	0.070
	(0.049)	(0.054)	(0.061)	(0.058)	(0.087)
warmth	-0.036	-0.035	-0.154**	-0.069	-0.143*
	(0.048)	(0.052)	(0.063)	(0.053)	(0.075)
generic treatment:warmth	0.113	0.086	0.181**	0.113	0.273***
	(0.070)	(0.073)	(0.091)	(0.078)	(0.104)
youth treatment:warmth	0.109*	-0.061	0.116	0.288***	0.326***
	(0.066)	(0.073)	(0.083)	(0.078)	(0.111)
constant	0.450***	0.423***	0.505***	0.413***	0.445***
	(0.036)	(0.038)	(0.047)	(0.039)	(0.059)
Observations	1,082	964	779	1,217	833
Note:					*p<0.1; **p<0.05; ***p<0.01

Table C7: Mediation Effects of Treatment on Voting Intentions (by Perception of Being Targeted by Voting Restrictions)

	perceived target of voting restrictions		
age group	Avg. indirect effect	Avg. direct effect	Total effect
18-29	0.0112***	-0.0003	0.0113
30-39	0.0043	-0.0107	-0.0064
40-49	0.0038	-0.0152	-0.0114
50-64	0.0022	-0.0154	-0.0132
65+	0.0018	0.0051	0.0069
Significance codes: p<0.001 = ***, p<0.01 = **, p<0.05 = *			

Given 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 only at respondents' perception that other people are trying to restrict their own ability to vote in elections. That is, do respondents believe that they themselves are being targeted by suppression? This measure already makes up one half of the reactance index (the other half being anger), but it could possibly serve as a mediator on its own. In other words, it could be that, for some respondents, a shift in this perception alone increases their intention to vote in 2022, regardless of whether they also experience an increase in anger.

This mediation analysis finds that the perception of being targeted by suppression does mediate the relationship between treatment and voting intentions, but only for the youngest respondents. The indirect effect was 1.2 percentage points for those 18-29 ($p<.001$). For older age groups, the indirect effect was smaller and statistically indistinguishable from zero. The bootstrapped average direct effect of perceived targeting on voting intentions was very small and did not reach conventional levels of statistical significance for any age group.

Survey instrument

People are very busy these days and many do not have time to follow what goes on in the government. We are testing whether people read questions. To show that you've read this much, answer both "extremely interested" and "very interested."

- ☐ Extremely interested
- ☐ Very interested
- ☐ Moderately interested
- ☐ Slightly interested
- ☐ Not interested at all

As you know, people have different identities. They think of themselves as young adults, middle-aged, etc. We would like to ask you how you think about yourself. Which of these age groups do you most identify with?

- o Young adults
- o Middle-aged adults
- o Older adults

How strongly do you identify with being a [young adult / middle-aged adult / older adult]?

- o Extremely strongly
- o Very strongly
- o Moderately strongly
- o Not very strongly
- o Not at all strongly

We'd like to get your feelings toward some groups of people. For each group listed below, please mark how warm (favorable) or cold (unfavorable) you feel toward them.

How warm or cold do you feel toward [young people / old people / Black people / white people / Republicans / Democrats]?

- o Very warm
- o Warm
- o Slightly warm
- o Neutral
- o Slightly cold
- o Cold
- o Very cold

If you had to guess, would you say that young people today usually vote for Republicans or for Democrats, or is it about half and half?

- o Usually for Republicans
- o Usually for Democrats
- o About half and half
- o Not sure

Some people seem to follow what's going on in government and politics most of the time, whether there's an election going on or not. Others aren't that interested.

In general, how often do you follow what's going on in government and politics?

- o Most of the time
- o Some of the time
- o Only now and then
- o Almost never
- o Not sure

Are you currently registered to vote at your current address, registered at a different address, or not currently registered?

- o Registered at current address
- o Registered at different address
- o Not currently registered
- o Not sure

In talking to people about the 2020 presidential election, we often find that a lot of people were not able to vote because they weren't registered, they were sick, or they just didn't have time. Did you vote in the 2020 presidential election?

- o Yes
- o No
- o Not sure

Please closely read the following story. When you are finished reading, proceed to the next question.

Control condition:

As 2022 Midterm Elections Approach, Campaigns Say It Is All About Turnout

Candidates are changing strategy to get more people voting in elections.

Political campaigns will spend millions of dollars next year trying to persuade voters around the country to choose their candidate over all others. However, strong evidence now suggests the biggest determinant of who wins is not the candidate with the best message, but the candidate who can most effectively turn out his or her base.

Midterms are all about base mobilization of partisan voters, not changing minds. In fact, one recent study by a political scientist at Michigan State University found that only 6% of voters changed parties between 2012 and 2016. This finding has led some to suggest that investing in persuasion is an enormous waste of money, relative to increasing turnout among one's existing supporters.

Several campaigns are changing tactics as a result of this research. **Political candidates are investing more in digital advertisements, text messages, and other voter mobilization strategies.** Many organizations are also publicizing information about how to register to vote.

Research suggests that these methods can be effective. James Hall, a political consultant, puts it this way: "If we won't change your mind with an attack ad, colorful yard sign, or witty campaign slogan, we should focus our resources instead on getting our voters to the polls."

Generic suppression condition:

As 2022 Midterm Elections Approach, Campaigns Say Voter Suppression May Reduce Turnout

Restrictive election laws could prevent many Americans from exercising their right to vote.

Political campaigns will spend millions of dollars next year trying to persuade voters around the country to choose their candidate over all others. However, strong evidence now suggests the biggest determinant of who wins is not the candidate with the best message, but the candidate who can most effectively turn out his or her base.

Midterms are all about base mobilization of partisan voters, not changing minds. In fact, one recent study by a political scientist at Michigan State University found that only 6% of voters changed parties between 2012 and 2016. This finding has led some to suggest that investing in persuasion is an enormous waste of money, relative to increasing turnout among one's existing supporters.

This may also explain why some politicians are promoting new laws that could restrict voting access for their opponents. **State lawmakers have already introduced 253 bills this year with provisions critics say suppress turnout**—including closing polling places, limiting early voting, and banning the use of certain ID cards to meet voter identification requirements. Many states are also making it harder to register to vote.

Research suggests that these new voting laws prevent many legally eligible people from voting. James Hall, a political consultant, says these laws will make a difference: "My best guess is that these laws will prevent thousands of Americans from exercising their right to vote. It's an absolute outrage."

Youth suppression condition:

As 2022 Midterm Elections Approach, Campaigns Say Voter Suppression May Reduce Youth Turnout

Restrictive election laws could prevent many young Americans from exercising their right to vote.

Political campaigns will spend millions of dollars next year trying to persuade young voters around the country to choose their candidate over all others. However, strong evidence now suggests the biggest determinant of who wins is not the candidate with the best message, but the candidate who can most effectively turn out his or her base.

Midterms are all about base mobilization of partisan voters, not changing minds. In fact, one recent study by a political scientist at Michigan State University found that only 6% of voters changed parties between 2012 and 2016. This finding has led some to suggest that investing in persuasion is an enormous waste of money, relative to increasing turnout among one's existing supporters.

This may also explain why some politicians are promoting new laws that could restrict voter access for their opponents. **Young adults are increasingly the targets. State lawmakers have already introduced 253 bills this year with provisions critics say suppress youth turnout**—including closing campus polling places, limiting early voting at colleges and universities, and banning the use of student ID cards to meet voter identification requirements. Many states are also making it harder for young adults to register to vote.

Research suggests that these new voting laws keep many legally eligible young people from voting. James Hall, a political consultant, says the laws will make a difference: "My best guess is that these laws will prevent thousands of young Americans from exercising their right to vote. It's an absolute outrage."

Generally speaking, how did the story you just read make you feel? Please tell us how much you felt each of the following emotions while reading the story.

	Not at all	A little	Somewhat	Very much	Extremely
Angry	o	o	o	o	o
Outraged	o	o	o	o	o
Disgusted	o	o	o	o	o

Annoyed	o	o	o	o	o
Hopeful	o	o	o	o	o
Cheerful	o	o	o	o	o
Pleased	o	o	o	o	o

How much do you agree or disagree with the following statement? "There are people out there trying to restrict my ability to vote in elections."

- o Strongly agree
- o Agree
- o Slightly agree
- o Neither agree nor disagree
- o Slightly disagree
- o Disagree
- o Strongly disagree

Do you favor or oppose state and federal governments passing stricter election laws, even if that makes it harder for some eligible citizens to vote?

- o Strongly favor
- o Favor
- o Slightly favor
- o Neither favor nor oppose
- o Slightly oppose
- o Oppose
- o Strongly oppose

How likely are you to vote in the 2022 midterm election?

- o Extremely likely
- o Moderately likely
- o Slightly likely
- o Neither likely nor unlikely
- o Slightly unlikely
- o Moderately unlikely
- o Extremely unlikely

In the next 12 months, how likely is it that you will go to any political meetings, rallies, speeches, dinners, or things like that, either in person or virtually?

o Extremely likely
o Moderately likely
o Slightly likely
o Neither likely nor unlikely
o Slightly unlikely
o Moderately unlikely
o Extremely unlikely

In the next 12 months, how likely is it that you will give money to a political leader, party, or organization?

o Extremely likely
o Moderately likely
o Slightly likely
o Neither likely nor unlikely
o Slightly unlikely
o Moderately unlikely
o Extremely unlikely

How interested are you in volunteering some of your time to raise awareness about voter suppression laws, so that people know what they need to do in order to be able to vote in future elections?

o Extremely interested
o Very interested
o Somewhat interested
o Not too interested
o Not at all interested

Voter fraud occurs when an individual voter intentionally engages in illegal behavior such as voting twice, voting when they are not eligible, or voting in a jurisdiction where they do not live.

How common would you say voter fraud is in elections today?

o Extremely common
o Very common
o Somewhat common
o Slightly common
o Not at all common

Voter suppression is a strategy used to influence the outcome of an election by discouraging or preventing specific groups of people from voting.

How common would you say voter suppression is in elections today?

o Extremely common
o Very common
o Somewhat common
o Slightly common
o Not at all common

How often do you think each of the following groups is targeted by voter suppression?

	Never	Rarely	Sometimes	Often	Almost always
Young people	o	o	o	o	o
Black/African American people	o	o	o	o	o
Hispanic/Latino people	o	o	o	o	o
Democrats	o	o	o	o	o
Old people	o	o	o	o	o
White people	o	o	o	o	o
Republicans	o	o	o	o	o
Independents	o	o	o	o	o
Christians	o	o	o	o	o

You answered that you are interested in volunteering to raise awareness about stricter voting laws. You can click here now to sign up to volunteer with the nonpartisan organization Let America Vote. Next, click the arrow to continue the survey.

Please select the color "yellow" below to demonstrate that you are paying attention to the instructions in this survey.

o Yellow

- Green
- Red