

# First Time Presidential Voting and Political Trust: Evidence from a Regression Discontinuity Design

**Jin Woo Kim**

Annenberg School for Communication  
University of Pennsylvania  
[jkim@asc.upenn.edu](mailto:jkim@asc.upenn.edu)

**Eunji Kim**

Department of Political Science  
Annenberg School for Communication  
University of Pennsylvania  
[eunjikim@upenn.edu](mailto:eunjikim@upenn.edu)

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## **Abstract**

What do first-time voters learn after participating in a presidential election? Democratic theorists relish political participation for its benefits in promoting a sense of political legitimacy. Yet these benefits are theorized in a small-scale self-government setting, and it is unclear whether similar effects can be found in a representative democracy, where many citizens participate only through voting. We argue that presidential voting typically results in eventual disappointment—either because preferred candidate loses or because the elected president’s big promises are underdelivered—, which may have long-term consequences on political trust. Applying a regression discontinuity design to the ANES time-series data (1974 to 2008), we find that being just eligible to vote in a presidential election undercuts political trust 2 or 4 years down the road by several percentage points—a tendency that is more pronounced under a failing economy. We discuss the implications of our findings for political socialization.

*That's the nature of democracy. It is hard, and sometimes contentious and noisy, and it's not always inspiring. But to the young people who got into politics for the first time, and may be disappointed by the results, I just want you to know, you have to stay encouraged. Don't get cynical. Don't ever think you can't make a difference.*

—Barack Obama on November 9, 2016.<sup>1</sup>

The role of civic participation in a democracy seems sacrosanct. Whether it's a town hall meeting in New England (Tocqueville 1835) or a Yugoslavian industrial enterprise (Pateman 1970), many prominent observers of the political scene have noted the benefits of being part of the democratic decision-making process. Participation not only makes citizens more virtuous, public-minded, and spirited, it also fosters a sense of legitimacy and trust in the political system (see Mansbridge 1999 and Theiss-Morse and Hibbing 2005 for a review). Even political participation at its most passive—voting—is known to have similar effects. Casting a vote can make citizens feel that the decisions of a political system are their own, and that they have a voice in democratic governance (Ginsberg and Weissberg 1978; Nadeau and Blais 1993; Thomson 1970, 62–64).

But voting in a presidential election in a democracy is a far from cheerful experience. The day after the election, half of the nation experiences the pain of political defeat. For the other half of the electorate that cheers the election outcome, politics can be disappointing in the end. The act of voting generates psychological attachment to a candidate and thus higher expectations (Albert and Bandura 1989; Bem 1967; Festinger and Carlsmith 1959), but most campaign promises are underdelivered, and citizens' high expectations are never fully met. So regardless of the election outcome, democracy “contributes little if anything to good moods” and is “often the source of hurt feelings and disappointments” (Lane 1999, 365). Even without quoting grave statistics on how the majority of American citizens is dissatisfied with the nation's system of government (Gallup 2014; Pew

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<sup>1</sup> <https://www.whitehouse.gov/the-press-office/2016/11/09/statement-president>

Research Center 2015), frustration seems to be a default state of mind in democracy, probably more so if citizens pay the cost of voting and are directly “betrayed” by elected presidents. First-time voters in particular may take away the lesson after four years (and throughout their lives) that politicians can’t be trusted.

This paper investigates the casual effects of first-time presidential voting on political trust. Capitalizing on an exogenous variation in voting eligibility from age restrictions,<sup>2</sup> we compare the “control” (under age 18) and “treatment” (over age 18) groups two or four years after the presidential election to examine the *downstream* effect of voting on trust in government. Using a regression discontinuity design (RDD) approach in order to address the potential endogeneity problem that voters and non-voters may have fundamentally different attitudes that affect their level of trust in government, we provide long-overdue evidence that the long-run effect of (first-time) presidential voting on political trust is different from earlier findings on the salutary effects of electoral participation, but is consistent with what one would expect given the American public’s usual disappointment with its elected presidents (Muller 1970; Stimson 1976; Sigelman and Knight 1983; 1985) and the role of presidential economic performance in shaping such trends (Healy and Malhotra 2013; Kernell 1978; Ostrom and Simon 1982).

More importantly, our findings are relevant to the political socialization literature. Political cynicism increases during early adulthood (Neimi and Sobieszek 1977), in a similar way that immigrants become more distrustful as they stay longer in a new country (i.e., Bilodeau and Nevitte 2003; Michelson 2007). Our finding offers a plausible acculturation mechanism that causes young voters to become less trusting of government. To the extent that (1) exercising their right to vote for

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<sup>2</sup> A growing number of recent studies has used voting eligibility as an instrument with which to estimate the causal impact of voting on the subsequent election’s turnout (Meredith 2009; Meredith and Endter 2015), polarization (Mullainathan and Washington 2009), partisan identity (Dinas 2014), and party registration (Kaplan and Mukand 2011), yet the effect of participation on the general level of trust in the government has not been examined.

the first time is a major—if not the biggest—event in a young citizen’s political socialization process *and* (2) most citizens are removed from politics except for voting throughout their lives, the simmering assumption in American political life that politicians are not to be trusted (and that change must come from the outside) might have originated in their first voting experience.

The implications of our findings are mixed. Cynical citizens can still be interested and engaged in politics (de Vreese 2005; de Vreese and Semetko 2002; Norris 1999), and prior evidence on habitual voting suggests that a decreased level of trust does not necessarily continue to discourage young citizens from voting. But to the extent that the functioning of a healthy political system hinges on, if not requires, public trust in government, the role of civic education on the nature of democracy—which is rightfully “not always inspiring” (Obama 2016)—looms large.

### **Voting and Trust in Government**

Civic participation is essential to the functioning of a democracy, and scholars—although they have different views on the *role* of participation—are concerned with its systematic consequences (Conway 1991). The existing research generally agrees that participation has positive effects, such as making citizens more tolerant, informed, and public spirited (e.g., Box 1998; Mason 1982; Mill 1859, 1861; Thompson 1970; Warren 1992). Political participation is also cherished as an instrumental tool that fosters a sense of legitimacy and trust in the political system (e.g., Barber 1984; Morell 1999; Pateman 1970. cf. Brehm and Rahn 1997). Citizens are found to have a higher regard for political processes when they participate in (or are exposed to) ballot initiatives, propositions, and plebiscites (Bowler and Bonovan 2002; Gilens et al. 2001; Hero and Tolbert 2004; Olken 2010), constitution drafting (Eisenstadt et al. 2015), direct elections (Persson et al. 2012; Tavits 2008), or administration such as public hearings and citizen advisory boards (XiaoHu and Wan Wart 2007). There is considerable empirical evidence on how participation breeds trust.

But the often-quoted positive impact of political participation on trust cited above relates to active involvement in decision-making processes and deliberation, not voting, which is “the quintessential form of political participation” in a representative democracy (Wilson et al. 2016, 183). Many citizens participate only by voting, specifically in presidential elections. Although the act of voting is considered “the weakest...expression of the spirit of democracy,” which too often “fixes choices and thereby stultifies the imagination” (Barber 1984, 202; Wilson et al. 2016, 136), prior works nonetheless confirm the positive effects of voting on trust. People’s attitudes toward the government and its responsiveness improve as the result of participating in elections (Clarke and Acock 1989; Clarke and Kornberg 1992; Ginsberg and Weissberg 1978; Finkel 1985). In a similar fashion, Plutzer (2002) notes that the simple occurrence of an election “act[s] to increase citizen affinity for leaders and regime,” as citizens have been socialized to believe that voting legitimizes political authority.

Previous studies typically estimate the *short-term* impact of voting by capturing the effects of voting right after the election (cf. David and Hitt 2016; Finkel 1987). While it may seem reasonable to measure the impacts of voting immediately after the “treatment,” existing evidence on the positive effects of voting is not generalizable beyond a few months after the election. If anything, the consequences of being involved in an election are likely to change once citizens observe how the elected president performs—especially given that presidential popularity usually declines after the inauguration (Sigelman and Knight 1983, 1985; Stimson 1976). To put it somewhat bluntly, customers’ reactions to choosing a service need to be measured after they get to use and evaluate that service (e.g., Johnson and Grayson 2005). Post-election surveys obviously cannot account for the “service evaluation” component of the causal pathway between voting and political attitudes.

This is not to say, of course, that the immediate impacts of electoral participation are unimportant.<sup>3</sup> But in the long run, the causal effects of voting on citizen attitudes toward the government are likely to hinge on whether the chosen leader governs competently through the term; these long-term effects remain unexamined to date.

Furthermore, existing studies that use cross-sectional data suffer from potential endogeneity problems regarding voting decisions. Those who participate in politics may be fundamentally different from those who do not with respect to a variety of unobserved confounders, making it extremely difficult to estimate the causal impact of participation on trust. Indeed, Mansbridge (1999, 315) asserted that this type of methodological challenge would lead to the absence of “highly persuasive positive or negative evidence [of the impact of participation] in the near future,” though she still cautiously predicted that “if we finally parse out what causes what...we will find that participation in a democratic polity makes the participants better citizens.”

We have reason to believe that individuals who might have become “better citizens” by voting in a presidential election may experience eventual frustration and disappointment, which generates systemic consequences. Even though elected presidents at least *attempt* to fulfill their campaign promises with a surprisingly high pledge fulfillment rate (Corrazzini et al. 2014; Petry and Benoit 2009), there are several structural reasons promoting the perception that they under-deliver. First, the media’s well-known negativity bias leads to an asymmetry in coverage of promises that politicians dishonored, while promises kept get substantially less coverage. So “with the news media fixed on government inaptitude and malevolence,” it comes as no surprise that “citizens have to come to evaluate political objects through this lens” (Hetherington 2005, 152), which leads to falsely high estimates of unfulfilled promises.

In addition to what presidents actually do and how the media report on their actions,

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<sup>3</sup> The legitimacy and/or stability of democratic politics hinges on how the supporters of the losing candidate react to the electoral outcome (Nadeau and Blais 1993).

however, the mismatch between expectations and performance seems inevitable precisely because politics is about “selling hope” (O’Shaughnessy 1990). Presidential candidates typically set unrealistically high goals and make prospective claims without qualifications. Indeed, this is what voters want from candidates, as they value optimism over other leadership qualities such as being charismatic or cautious (Malhotra and Margalit 2014). But as campaign rhetoric often soars way above the political reality. Citizens’ post-election expectations of “the man inside the White House...to do something about everything” (Neustadt 1960, 7) is never entirely fulfilled, a gap that tends to expand during the course of the presidency (Raichur and Waterman 1993). Indeed, people’s eventual disappointment with elected presidents is a consistent, well-documented empirical pattern in public opinion scholarship (Genovese 2002; Lowi 1985; Jenkins-Smith et al. 2005; Stimson 1976). Presidential approval ratings usually decline after the “honeymoon” phase (Brody 1991; Muller 1970; Stimson 1976; Sigelman and Knight 1983; 1985), and disappointing presidential performance—especially related to national economic performance—is known to drive such trends (Kernell 1978; Ostrom and Simon 1985). In a nutshell, whether it’s the media’s negativity bias or voters’ unrealistic expectations, the results of presidential voting are ultimately disappointing.

### **Theoretical Expectations**

Considering that political participation is not ultimately a cheerful experience, we think there are two possible micro-level mechanisms that may explain the negative effect of presidential voting on trust. First, for citizens who voted for the losing candidate, the disappointment is immediate. Losers are less willing to bestow legitimacy upon a political system that produced an outcome they actively sought to avert. It has been repeatedly shown across different electoral settings that those who voted for the losing candidate tend to be less supportive and trusting of the political system than those who voted for the winning candidate (Anderson et al. 2005; Craig et al. 2006; Nadeau and Blais 1993, Anderson and Guillory 1997, Norris 1999; Nadeau et al. 2000; Anderson and LoTempio 2002; cf.

Rahn et al. 1999). More importantly, the effect of losing an election persists over time (Anderson et al. 2005), as the fact that they voted for the opponent makes election losers disapprove of the government more in order to decrease the level of cognitive dissonance (Mullainathan and Washington 2009).

But election winners will also eventually experience disappointment and demonstrate lower levels of trust in government, particularly because only those who paid the cost of voting are directly “betrayed” by presidents who fail to deliver on their promises in the end. The act of voting requires both cognitive efforts to make a decision and physical efforts to go to the polling booth (and possibly wait in line for hours). To the extent that actions themselves can drive preferences, not vice versa, the act of voting *itself* can generate psychological commitment to a candidate and higher expectations (Albert and Bandura 1989; Bem 1967; Festinger and Carlsmith 1959). This, in turn, indicates that voting can make disappointment more acute, lowering the general level of trust in government.

A similar mechanism is at play in Mutz’s finding (2009, 443) that the negative e-commerce experiences of first-time buyers result in lower levels of generalized trust: “Although a high level of trust in humankind was not [buyers’] reason for choosing to buy online... people may shift their attitudes toward the trustworthiness of unknown others so that these attitudes are consistent with their demonstrated willingness to purchase online.” By the same token, we suspect that election winners might experience a *greater* loss of trust in government as dissatisfaction arises from increased expectations, with winners feeling relatively more deprived (Abeles, 1976; Gurr, 1970; Hooghe et al. forthcoming; Smith et al 2012).<sup>4</sup>

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<sup>4</sup> This would not happen, of course, if winners avoid disappointing news about presidential performance altogether and continue to trust the government. But this is unlikely, given that partisans are not impervious to “bad” news (Gerber and Green 1999) and that citizens who want to defend the president may also find it *easier* to blame other politicians and the political system (Malhotra 2008), which can decrease general trust in

Not every presidential election is equally disappointing, however. To the extent that the effect of voting undercuts citizens' political trust, the pattern should be more pronounced under an especially disappointing presidency. We focus on national economic performance as the single-most visible indicator of presidential performance from the voters' perspective; it is known to be the most important determinant of presidential approval ratings and election outcomes (Hibbs 1987). We acknowledge that (1) economic conditions are not the only criterion the public uses to evaluate presidents, although it is arguably the most salient one, and (2) the status of the economy may be confounded with other social or political trends. Our aim is not to single out the causal impact of the economy, but rather to examine whether the patterns observed through the prism of the economy are consistent with our argument: that the disappointing end result of electing the incumbent president is the underlying mechanism of the detrimental impact of voting. At a minimum, we should find that the erosive effect of voting is weaker when "things are better."

## Data

We use the American National Election Studies' (ANES) Time Series data to examine the effect of first-time presidential voting on political trust. Since the 1972 election, the voting age in the United States has been set at 18, following the ratification of the Twenty-Sixth Amendment in 1971.

Accordingly, we combine 17 rounds of ANES Time Series surveys conducted between 1974 and 2008 in which the key variables—birth year/month and political trust—are available ( $N = 27,182$ ).

*Voting Eligibility.* Based on information about birth year and month, we calculate each respondent's age on the previous presidential election day, as opposed to age at the time of survey.<sup>5</sup>

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the government. Though not about election winners *per se*, aggregate-level analyses also suggest that citizens' rising expectations of the government's capacity is the reason for the recent decline in political trust in industrialized economies (Dalton 2004; Miller and Listhaug 1999; Orren 1997, cf. Seyd 2015).

<sup>5</sup> Throughout the remainder of this paper, "age" refers to respondents' age at the time of the previous election, which means that by the time they took the ANES surveys, just-eligible voters were 20 years old in mid-term election surveys (e.g., 1978), and 22 years old in presidential election surveys (e.g., 1980).

For example, in the 1974 round, the age of respondents born in October 1954 is  $18 + 1/12 \approx 18.08$  and for those born in December 1954 it is 17.92. Only the former was eligible to vote in the 1972 presidential election, thus qualifying as the “treatment” group.<sup>6</sup>

*Political Trust.* We rescale the ANES Trust in Government Index to vary between 0 and 1, with 1 indicating the highest level of political trust.<sup>7</sup> This index is comprised of four items: (1) “How much of the time do you think you can trust the government in Washington to do what is right?” (2) “Would you say the government is pretty much run by a few big interests looking out for themselves?” (3) “Do you think that people in the government waste a lot of money we pay in taxes?” (4) “Do you think that quite a few of the people running the government are crooked?”

*Economic Performance.* We use per capita real income growth rate as an indicator of presidential performance.<sup>8</sup> Table 1 shows one-year growth rates and two-year averages leading up to the end of each survey year. The economic conditions are categorized as “bad” when per capita real income decreased from the previous year. Eight of the 17 survey years fall into this category. In the remaining nine survey years, per capita real income grew at least by 1.76 percent, providing a clean cut point. The separation between “bad” and “good” economies is clear, looking at two-year averages as well.

[Table 1 here]

## Identification Strategy

To identify the downstream effect of first-time presidential voting on political trust, we use an RD research design that leverages the voting-age requirement. The key intuition behind this empirical approach is that in the narrow vicinity surrounding the voting-age cutoff—e.g., those who are 6

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<sup>6</sup> Since birth *day* is unavailable from the ANES data, we cannot identify the voting eligibility of the small number of respondents ( $N = 42$ ) who turned 18 *sometime* in November of a presidential election year (e.g., the 1954 November cohort in the 1972 election). We drop them from the main analyses presented below. We found very similar results when we included these “partially treated” individuals in the treatment group.

<sup>7</sup> The variable name in the ANES cumulative data file is VCF0656.

<sup>8</sup> <http://www.census.gov/hhes/www/income/data/historical/people/>

months older or younger than the legal voting age at the time of the previous presidential election—the “treatment” and “control” groups should be fairly similar, except that only the former was eligible to vote (see Coppock and Green 2016; Dinas 2014; Meredith 2009; Mullainathan and Washington 2009 for examples of recent research capitalizing on the voting age in an RD framework). This research design can be considered a special case of natural experiment to the extent that these two groups are comparable “as if” they were randomly assigned (Dunning 2012).

*Assumption 1. (Unconfoundedness in the vicinity): Those barely above and below the legal voting age have the same potential outcomes.* Under this assumption, the local average treatment effect (LATE) of voting eligibility can be identified as the mean difference between just-eligible voters and their slightly younger counterparts (Imbens and Lemieux 2008, 621).

Given that the eligible are technically older than the ineligible, although by only a few months, we can use an alternative identifying assumption. *Assumption 2. (Continuity): Potential outcome is a continuous function of age.* In other words, people’s level of trust in government evolves smoothly with respect to their age at the time of their first presidential election. This assumption implies that there is no reason, other than voting eligibility, for trust in government to be a discontinuous function of age at 18. This is weaker than Assumption 1, because it holds even if younger/older citizens have different potential outcomes, so long as the changes in potential outcomes at the cutoff are “smooth.” Under the continuity assumption, the effect of voting eligibility can be captured by a local linear estimator fitting separate linear trends in political trust on either side of the cutoff, and by estimating the size of the gap between the two regression lines (Imbens and Lemieux 2008, 618).

While less sensitive to age-associated biases, accounting for linear associations introduces additional uncertainty, thereby widening standard errors. The mean difference estimator is preferable in terms of precision and simplicity, although it requires a stronger assumption (Dunning 2012). We use both

approaches below. To the extent that these estimators agree with each other, one can be more confident in both (Imbens and Lemieux 2008, 633).<sup>9</sup>

There are three main reasons to think that our identifying assumptions are plausible. First, unlike other applications of RD design (e.g., Lee 2008; Caughey and Sekhon 2011), it is very difficult to think that people would precisely sort themselves to receive treatment; most parents would not decide when to have a child based on whether the child will be eligible to vote in an election coming up in 18 years. Second, both the control and treatment groups are comprised of young adults living in the same social and political environment. Therefore, strong predictors of political trust established in prior research (e.g., Chanley et al. 2000; Citrin and Green 1986), such as economic conditions and political scandals, are essentially held constant. For such factors to cause age-associated biases in our estimates, they should strongly interact with age such that even slightly (say, one year) older people respond more sensitively to bad signals (e.g., failing economy), which then may create a steep decline in political trust during early adulthood. Contrary to this possibility, however, we show below that trends in political trust are flat for young voters on either side of the cut point. Third, the control and treatment groups are reasonably well balanced with respect to a variety of demographic characteristics (e.g., income) and placebo outcomes (e.g. social trust), using the same specifications as our main analyses of treatment effects (see the appendix). For the most part, observed imbalances are substantively small and statistically insignificant. And the estimates barely change when we control for demographic and socio-economic characteristics.

Yet one remaining concern may be that, since those who just turned 18 at the time of the previous presidential election would turn 20 (in the year of the mid-term election surveys) or 22 (in the year of the general election surveys) by the time they are interviewed by the ANES, having been just eligible in the preceding presidential election may have coincided with graduating from a two- or

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<sup>9</sup> Scholars have different preferences between the difference-in-means and local lineal regression estimators (for a discussion, see Dunning 2012; Imbens and Lemieux 2008).

four-year college and being employed 2 or 4 years later. Assumptions 1 or 2 may be undermined if the relationship between voting eligibility and political trust is confounded with life events that discontinuously take place around the time that one turns 20 and/or 22—including entering the professional world. In line with this concern, relatively consistent imbalance was found for occupational status in our balance check. Below, we empirically rule out this possibility, based on analyses exploring the effects of passing the voting-age cutoff in the year of the *midterm* elections. Here, the age compositions of the placebo treatment and control groups are the same as the main test—one group consists of those who have just turned 22 or 20 by the time they take the ANES survey, and the other group consists of those who are slightly younger—but these groups do *not* differ in voting eligibility during the previous *presidential* election. If participating in a presidential election is what leads to a loss of political trust, this placebo cutoff should *not* have the same kind of effects.<sup>10</sup> We show below that this was indeed the case. This is powerful evidence that unobserved differences in life experiences at the age cutoff cannot explain away our key findings.

Finally, our estimators identify the LATE of voting *eligibility* (i.e., intent-to-treat effect), not the effect of voting *per se*. Eligibility discontinuously increases, but never perfectly predicts voting, as many citizens choose to abstain. Thus, we need to rescale the estimates by the turnout rate of just-eligible voters to recover the LATE among compliers (i.e., those who vote when eligible), which is valid under *Assumption 3 (Exclusion Restriction): The observed effect of voting eligibility occurs only through voting*.<sup>11</sup> This assumption would be undermined if eligible voters were more likely to pay attention to coverage of the presidential campaign, or if not voting made them more cynical (see Cappella and

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<sup>10</sup> This placebo test implicitly assumes that the erosive effect of voting is not generalizable to non-presidential elections. We elaborate on this point below.

<sup>11</sup> Statistically, this can be done by fitting a two-stage-least-squares model, whereby the first stage regresses previous voting on eligibility, and the second stage regresses political trust on predicted voting probabilities. In the remainder of this article, however, we focus mainly on the intent-to-treat effects, because several subsets of the ANES data (1978, 1984, 1986) do not provide information on past voting behavior, and self-reported voting is prone to a variety of biases (Berstein, Chadha and Montjoy 2001).

Jamieson 1997; Mutz and Reeves 2005). There are two reasons to think that this possibility is unlikely. First, campaign/media effects are known to decay rather rapidly (Gerber et al. 2011), including the effect of incivility on political trust (Mutz and Reeves 2005). It is therefore difficult to think that exposure to campaigns would last long enough to be detected in 2 to 4 years.<sup>12</sup> Further, this scenario cannot explain the treatment effect heterogeneity we show below, in which first-time presidential voting erodes trust in government only when the economic performance turns out to be disappointing, because the media cannot cover a presidential campaign based on what will happen 2 to 4 years in the future.

## Results

Our findings support the two main hypotheses across different specifications: (1) first-time presidential voting (eligibility) substantially undercuts young citizens' trust in government and (2) the detrimental effect of voting (eligibility) depends on the state of the national economy. Figure 1 summarizes these points; small dots show one-month averages and larger ones show four-month averages. Also plotted in Figures 1 and 2 are “trends” in political trust captured by kernel-weighted local means, fitted, respectively, to both the left and right of the cutoff.

The top panel of Figure 1 demonstrates the average effect of voting eligibility. Although there are some ups and downs, the dots on the left-hand side of the cutoff bounce around the 0.40 mark, while the dots on the right-hand side remain close to 0.35, demonstrating an abrupt change at the voting-age threshold. The local polynomial lines also reveal a discontinuity in political trust at the voting-age threshold, which falls from 0.39 to 0.35. Scaling this gap by an approximate turnout rate of 40% among just-eligible voters would suggest that voting for a presidential candidate during very

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<sup>12</sup> The possibility that voting (not just eligibility) increases subsequent exposure to political information is not inconsistent with Assumption 3. Regardless, we found little evidence of the downstream effects of voting on news exposure.

early adulthood, on average, undermines an individual’s trust in government by  $(0.39-0.35)/0.4 = 10$  percentage points. Another important takeaway of this figure is that the lines are relatively flat on either side of the cutoff, which suggests that potential outcomes in political trust are unlikely to be affected by narrow age differences—empirical evidence of the assumption that slightly younger/older individuals can be valid counterfactuals for each other.

[Figure 1 about here]

The bottom panel graphs the dots and lines *separately* for the years in which the income growth rate in the survey year was greater than zero, and less than zero, illustrating how much the eroding effect of voting depends on the status of the economy. The LATE of voting eligibility under a disappointing presidency, displayed in Panel (2), is twice as large as, with a difference of 0.08 (0.40–0.32). By contrast, when the president’s performance is relatively good (in years with positive real income growth), political trust drops only a little ( $0.38-0.37 = 0.01$ ) at the legal voting age, as shown in Panel (3). In summary, those who were eligible to vote in the preceding presidential election end up being less trusting than those who were ineligible, on average (by 4 percentage points), and the pattern is driven almost entirely by the years in which the national economy was in poor shape.

To provide more formal estimates of the (intent-to-treat) effect of first-time presidential voting, we first estimate the following mean comparison model based on Assumption 1:

$$Trust_i = (h_b < AgeC_i < h_a) \cdot (\alpha + \tau_{MD} \cdot Eligible_i + \epsilon_i), \quad (1)$$

where  $h_b$  and  $h_a$  are the bandwidths below and above the cutoff, which specifies how much data in the vicinity around the cutoff are retained for the estimation;  $AgeC_i$  is the running variable (age in months centered at  $18 \cdot 12 = 216$ );  $\tau_{MD}$  is the mean difference capturing the LATE; and  $\epsilon_i$  is the

error term. Our second specification uses the following local linear regression model, which is based on Assumption 2:

$$Trust_i = (h_b < AgeC_i < h_a) \cdot (\alpha + \tau_{DJ} \cdot Eligible_i + \beta \cdot AgeC_i + \gamma \cdot Eligible_i \cdot AgeC_i + \epsilon_i), \quad (2)$$

where  $\beta$  and  $\beta + \gamma$ , respectively, are the linear slopes on each side of the cutoff; and  $\tau_{DJ}$  is the discontinuous jump (hence the subscript  $_{DJ}$ ) between the two slopes at the cutoff identifying the LATE.<sup>13</sup>

[Table 2 here]

Table 2 reports the ordinary least square (OLS) estimates of Equations 1 and 2. Column 1 draws on all survey years (1974 to 2008), whereas Columns 2 and 3 subset the data by the status of the economy (see Table 1). In Column 4, we report the difference between the estimates in Columns 2 and 3, capturing treatment effect heterogeneity across varying economic performance. As shown, the regression estimates are similar to the graphical analyses, and consistent across different selections of bandwidths and estimation approaches. For example, looking at Row 1, which compares the means between just-eligible and just-ineligible voters who were up to 6 months older or younger than 18, voting eligibility is estimated to undercut political trust by an average of 3.6 percentage points (Column 1), and by 8.5 points under a bad economy (Column 2). Such an effect is

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<sup>13</sup> As noted, we dropped those who turned 18 in November of the previous presidential election year. Therefore, we set up the local linear estimator so that it captures  $\lim_{x \downarrow 0} E[Trust_i | Age_i = 18.08 + x] - \lim_{x \uparrow 0} E[Trust_i | Age_i = 17.92 + x]$  instead of  $\lim_{x \downarrow 0} E[Trust_i | Age_i = 18 + x] - \lim_{x \uparrow 0} E[Trust_i | Age_i = 18 + x]$ . This was done by slightly truncating  $AgeC_i$  (i.e., the running variable); for example, for the 1974 survey, those who were born in September, October, November, and December of 1954 were respectively coded 1.001, 0.001, missing, -0.001, and so on. This approach invokes a (mild) assumption that those who were 17.92 and 18.08 have the same potential outcomes, in addition to Assumption 2. We found very similar results using an alternative operation of the running variable, in which the November cohorts are not dropped, and the same individuals are coded 2 (September), 1 (October), 0 (November), -1 (December), -2 (January, 1995), and so on.

entirely absent when the economy is in better shape (Column 3), which creates a noticeable heterogeneity in treatment effects (8.5 points). The second row, which compares 17 year olds and 18 year olds, tells essentially the same story. Rows 3 to 5 display local linear estimates that use wider bandwidths while controlling for linear trends in political trust on either side, and underweighting observations that are distant from the cutoff with a triangle kernel.<sup>14</sup> Again, the findings consistently show that voting eligibility diminishes political trust in the long run, especially when the country's economic performance is poor.<sup>15</sup>

[Figures 2 and 3 here]

We demonstrate the robustness of the results across other choices of bandwidths in Figures 2 and 3. Figure 1 plots the RD estimates for the difference-in-means model with bandwidths from 1–24 months on either side, along with 95% confidence intervals. As can be seen, our findings vary little across bandwidths, except for very narrow bandwidths (three months or below), the estimates for which are imprecise and somewhat unstable. Figure 2 shows the estimates from the local linear models for bandwidths ranging from 1–10 years.<sup>16</sup> Again, for windows that allow for a reasonable level of precision, the estimates remain stable across different bandwidths.

[Figure 4 here]

The results presented thus far suggest that first-time presidential voting eventually makes citizens more cynical about politics. But the extent to which this finding explains the general understanding of young citizens' current (and future) political attitudes depends on (1) whether the erosive effects are driven mostly by a few unusual election years and (2) whether the effects occurred

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<sup>14</sup> Row 3 includes those aged 14 to 22—i.e., everyone in Figure 1—and Row 4 expands the bandwidth on the right side to 26. Row 5 uses MSE-optimal bandwidths selected for each side (Calonico et al. 2014).

<sup>15</sup> As indicated, the estimates we provide here are intent-to-treat effects, which are necessarily smaller than average treatment effects for compliers. In the appendix, we provide the effects of voting by scaling the effects of voting eligibility by self-reported voting rates in the previous presidential election.

<sup>16</sup> Note that the maximum bandwidth on the left side of the cutoff is 4, because those who were under 14 in the previous presidential election are not included in ANES surveys.

mostly in the distant past. To explore these possibilities, we plot and estimate regression discontinuities for each survey year in panel of Figure 3. Each row represents a time period classified into three categories—(1) 1974 to 1984; (2) 1986 to 1996; and (3) 1998 to 2008—which roughly corresponds to three generations of respondents (Baby Boomers, Generation X, and Millennials) that make up the bulk of the current US adult population. The figure shows that the negative effect of first-time presidential voting on political trust has been more of a rule than an exception, especially in recent years. Voting eligibility led to a decrease in political trust in 11 out of 16 survey years; just-eligible voters reported lower levels of trust in government than just-ineligible voters 9 out of 10 times since 1986 (i.e., for Generation X and Millennials).<sup>17</sup>

[Figure 5 here]

Figure 5 illustrates the generational differences in treatment effects. The figure provides our main estimates for each generation.<sup>18</sup> It shows that first-time presidential voting (eligibility) has had similar effects across generations under a bad economy (Panel 1). Interestingly, however, we find that voting had different cross-generational consequences under a good economy: it had a positive (though statistically insignificant) effect for Boomers, and an insignificant negative effect for Generation X, and a significant negative effect for Millennials (Panel 2). When averaged over the status of the economy, the overall effects are more pronounced for more recent cohorts, ranging from virtually non-existent (-0.4 percentage points for Boomers) to substantial erosion (-9.4 points for Millennials), as visualized in Panel 3. This is because younger generations—especially Millennials—tend to become more cynical after first-time presidential voting regardless of the incumbent president’s economic performance (Panel 4).<sup>19</sup>

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<sup>17</sup> Few of the estimates are statistically significant because very few young voters were sampled in each survey year.

<sup>18</sup> The estimates are based on MSE-optimal bandwidths—analogueous to Row 5 of Table 2.

<sup>19</sup> There can be multiple explanations for the lack of heterogeneity for Millennials. It may be the result of political scandals during the Clinton presidency that may have disappointed first-time voters despite the

Both our theoretical argument and empirical analyses have focused on the consequences of voting for a *presidential* candidate, not voting in general. In light of prior research showing the positive effects of other forms of political involvement (i.e., Warren 1992; Box 1998; Mason 1982, Thompson 1970), the erosion of political trust is likely to occur only when participants are let down eventually. We would argue that other participatory experiences are not typically as disappointing as presidential politics, which tend to cause a more acute sense of electoral defeat—and ultimately a more salient sense of broken promises—than other forms of political participation. Now we empirically establish this boundary condition focusing on the effects of voting in *midterm* elections. There are three good reasons to think that midterm voting would not have the same effects as presidential voting. First, unlike presidents, individual local officials and members of Congress do not *personify* “the government in Washington” or “people running the government,” which is the focus of the Trust in Government index used in this study. Second, while people’s attitudes toward Congress are clearly associated with trust in government (Citrin and Luks 2001), it is well known that Americans distinguish between Congress as a whole and congressmen from their district (Fenno 1978). Third, many Americans do not know who represents them in Congress, let alone how they have performed, because the media focus on the president and key leaders of the major parties.

[Figure 6 here]

Figure 6 reports the RD estimates of *midterm* voting eligibility’s effect on political trust, using the same specifications used for the effect of *presidential* voting. As can be seen, we find no evidence that passing the age cutoff in midterm elections decreases political trust. If anything, midterm voting makes young citizens more trusting, which coincides with the more traditional view in the literature (e.g., Mansbridge 1999), though the estimates are somewhat sensitive to bandwidth sizes, and

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relatively strong economy. It may also be that overall economic growth in the 1990s did not affect young voters’ evaluations of the sitting president because of the rising income inequality. While these are intriguing hypotheses, it is beyond the scope of our research to explore them.

statistically insignificant. Importantly, this result is difficult to reconcile with the alternative explanation that (unobserved) confounding life events change simultaneously at the discontinuity.

## **Discussion and Conclusion**

It is a long-standing and well-accepted premise that active participation in democratic decision making makes participants better citizens (Mansbridge 1999). But too often, what citizens observe after their costly participation in presidential elections—compromises and imperfect solutions to political issues—is off-putting (Durr, Gilmour and Wolbrecht 1997; Hibbing and Theiss-Morse 1995, 147). Leveraging exogenous variation in voting eligibility imposed by age restrictions, we provide casual evidence that first-time presidential voting eligibility has long-run consequences on political trust. Those who were qualified to vote by quasi-random age eligibility display a 4-percentage-point increase in their level of political distrust two and four years after the election. In tandem with economic voting theory, we also find that the impact of first-time voting on distrust is more pronounced under a failing economy, when a president turns out to be more disappointing.

Our counterintuitive findings are consistent with general acculturation effects in political trust, a well-known phenomenon in political socialization processes (Searing et al. 1973; 1976). Early studies on children’s political attitudes repeatedly show that they have remarkable faith in government and political figures, unlike adults (Dawson and Prewitt 1969; Greenstein 1960; Hyman 1959), and overwhelmingly agree with the blanket statement that the president “would *always* want to help me if I needed it” (Hess and Tomney 1967). But these idealistic images change as they become older and more aware of real-world problems (see Neimi and Sobieszek 1977 for a review). Similar effects are observed among immigrants, who have higher expectations of the government in a new country. As immigrants stay longer in the new country and become acculturated to mainstream society, they become increasingly distrustful of the government (Bilodeau and Nevitte 2003; Garcia

1973; Lavariega Monforti and Michelson 2014; Michelson 2007; Röder and Mühlau; Wenzel 2006, 2011). First-time presidential voting is arguably the most salient political learning experience for many young voters, and is typically the default way to be acculturated into the political system. But the lesson they take away, four years after their first election day, is that politics is disappointing and their lives have not changed much. Many studies have explored the causes of prevalent political cynicism in America, where citizens assume the worst of political “insiders” and popular media portray a broken system for which redemption is only possible with the help of outsiders and political novices, yet none has shed light on how the very first political experience of young voters might have played an important role in cultivating such cynicism. Hence, we believe our research contributes to the political socialization literature methodologically and substantively, against the backdrop of the rise of outsiders on the contemporary political scene.

The implications of our findings are not necessarily depressing or sanguine from a normative standpoint. It is worth highlighting that these empirical results cannot tell us whether the decreased level of trust will continue to discourage young citizens from voting, and are *not* incompatible with the idea of habitual voting, a pattern in which first-time voters are more likely to vote again in subsequent elections (i.e., Coppock and Green 2016; Plutzer 2002; Meredith 2015; Meredith and Endter 2015). In reviewing the past literature on political mistrust and participation, de Vreese (2005) concludes that there is only modest empirical evidence to support a direct link between cynicism and voter turnout (see also Leshner and Thorson 2000; Pinkleton, Austin, Zhou, Willoughby, and Reiser 2012). Indeed, several works suggest that citizens who distrust politicians can still be interested in politics and vote, leading to a positive correlation between cynicism and turnout (e.g., De Vreese and Semetko 2002; Jeffres et al 2008). To the extent that it is those who expect more who will be further disappointed, political distrust can instead reflect “the fact that normative ideals toward democracy have become more exigent” or serve as “an indicator for a

healthy and thriving democratic political culture” (Norris 1999). But given that the functioning of representative democracies heavily hinges on public trust (Almond and Verba 1963; Easton, 1975; Hetherington 2005), the way that participation raises people’s expectations, and therefore decreases trust, is rather unfortunate, if inevitable. Even if eroding voters’ sense of trust does not affect subsequent voter turnout *per se*, trust affects civic compliance, electoral choices, and policy preferences, among many other aspects of democratic societies (Chanley, Rudolph, and Rahn 2001; Hetherington 1998; Levi 1998; Scholtz and Lubell 1998; Tyler 1998; Wilkes 2014).

If our findings are a manifestation of a fundamental paradox of modern democratic politics—the inevitable disjuncture between promise and performance (Barger 1984; Stimson 1976)—the role of civic education looms large. It is not that starry-eyed young voters should not raise their expectations at all. But to the extent that politicians’ overpromising is endemic to the democratic process, civic education should teach young citizens to appreciate “the ugliness of democracy,” where problems are difficult, true solutions are nonexistent, and compromising is the norm (Bennett 1997). As Hibbing and Thesis-More rightly noted (1995, 157), “developing this appreciation is something that should be a standard part of the educational experience in a democracy, but apparently is not.” Needless to say, the press can also play a role by probing candidates further and highlighting the conditions under which their plans will lead to the outcomes they promise, and the irreducible uncertainty. Citizens might easily infer that bold campaign promises are merely a campaign device, but for those who take candidates’ promises seriously, in-depth media coverage of the unstated assumptions underlying a proposed policy could mitigate some eventual disappointment at the very least.

“[T]rust is a gamble that others will act responsibly on one’s behalf,” so it may well be “foolish to entrust one’s interests to a proven failure” (Citrin and Luks 2001, 26). But given how

endemic disappointment is to participation in a representative democracy, it is heartening that voters nonetheless continue striving to fail valiantly.

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Table 1: Economic Performance by Survey Year

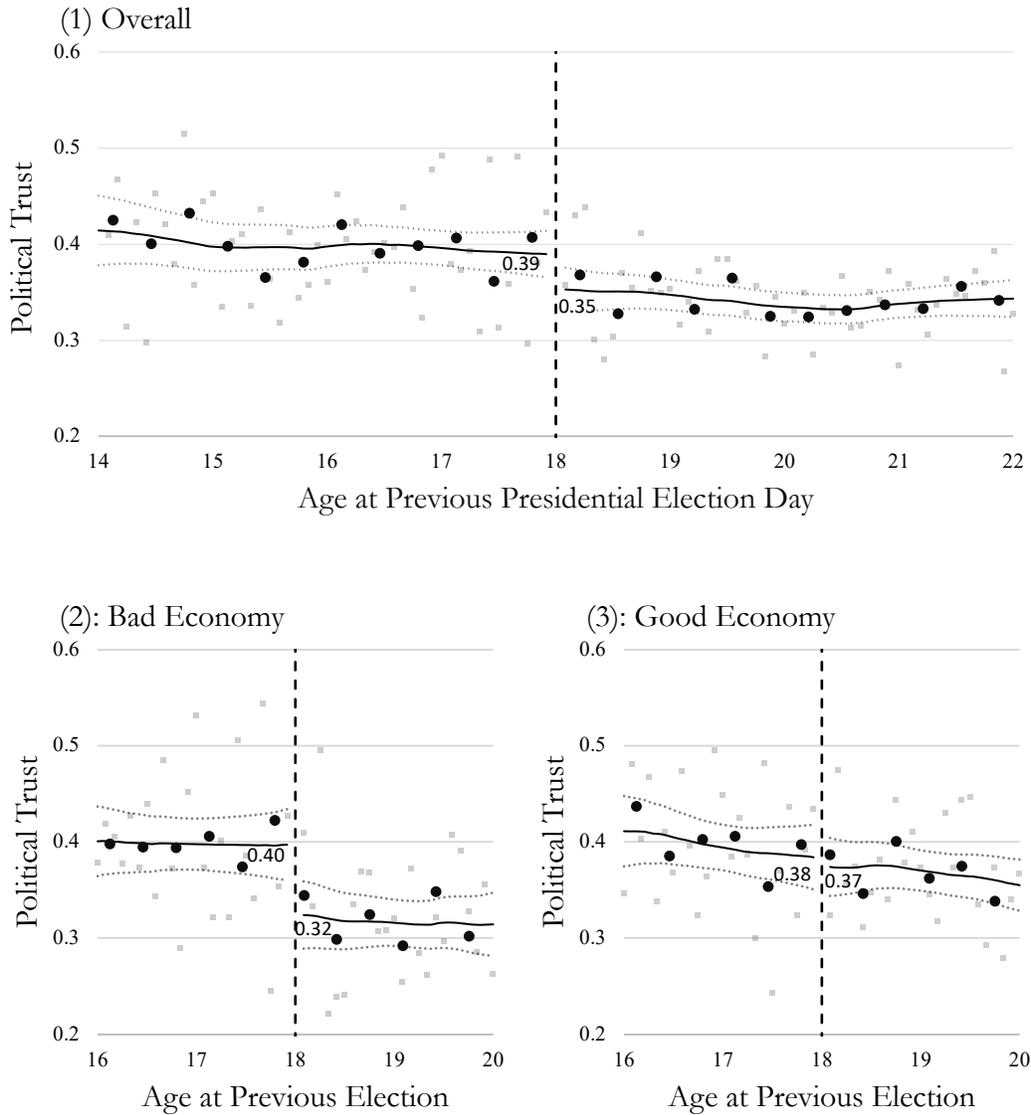
Annual Real Income Growth			
Survey Year		2-Year Average	Coded
2008	-3.12	-2.11	Bad
1990	-2.46	0.11	Bad
1974	-2.42	0.49	Bad
1980	-2.3	-0.44	Bad
2002	-1.84	-1.19	Bad
1992	-0.94	-1.46	Bad
2004	-0.2	-0.16	Bad
1982	-0.08	-0.35	Bad
2000	1.76	2.56	Good
1988	2.16	2.44	Good
1996	2.5	2.06	Good
1994	2.78	3.24	Good
1998	3.17	3.51	Good
1976	3.45	1.81	Good
1986	4.08	3.59	Good
1978	4.42	3.83	Good
1984	4.43	2.95	Good

Table 2: Intent-to-treat Effect of First-time Presidential Voting on Political Trust

Estimator	Bandwidth (Years)	(1) Overall	(2) Bad Economy	(3) Good Economy	(4) Difference	
(1) Difference-in-Means	[-0.5, +0.5]	-0.036 (0.024)	-0.085* (0.038)	-0.000 (0.032)	-0.085* (0.050)	400
(2) Difference-in-Means	[-1, +1]	-0.037* (0.017)	-0.077* (0.026)	-0.008 (0.023)	-0.069* (0.035)	811
(3) Local Linear	[-48, +48]	-0.035* (0.019)	-0.081* (0.029)	-0.001 (0.025)	-0.080* (0.038)	3204
(4) Local Linear	[-48, +96]	-0.033* (0.017)	-0.078* (0.026)	-0.001 (0.022)	-0.077* (0.034)	5306
(5) Local Linear	MSE Optimal	-0.036* (0.019)	-0.084* (0.024)	-0.009 (0.022)	-0.075* (0.033)	6153

\* $p < 0.1$ . OLS estimates. Standard errors are adjusted for survey-year-by-state clusters. N corresponds to Column 1. Rows 1 and 2 are based on Equation 1. Rows 3 to 5 are based on Equation 2. Triangle kernel weights are used for local linear estimates. MSE-optimal bandwidths in Columns 1, 2 and 3 are respectively [-35.1, +115.6], [-47.6, +174.0], and [-44.6, +120.9].

Figure 1: Graphical Illustration of Regression Discontinuity



Note. Dots are binned averages. Small dots show one month averages, and large dots show four-months averages. Solid lines show kernel (Epanechnikov) weighted local means fitted on either side of the cutoff, with dotted lines representing 95% confidence intervals. The x-axes in all three panels are age at previous presidential election. Age during the ANES surveys are 2 to 4 years higher.

Figure 2: Difference-in-Means Estimates across Bandwidths

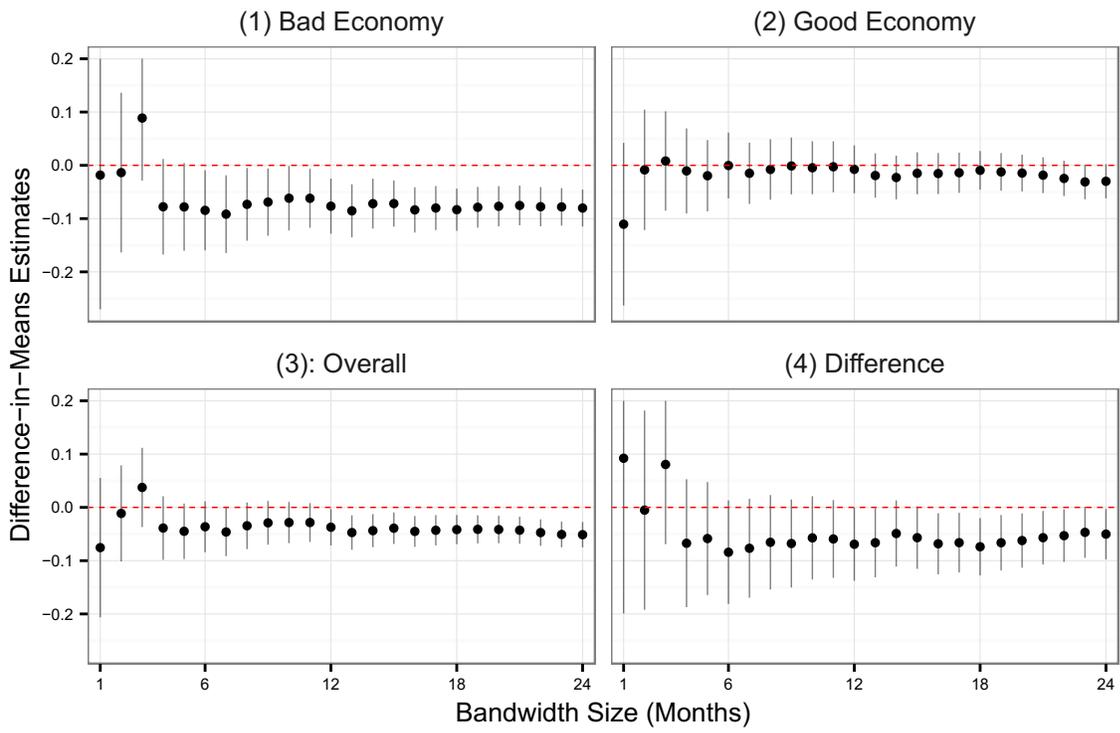
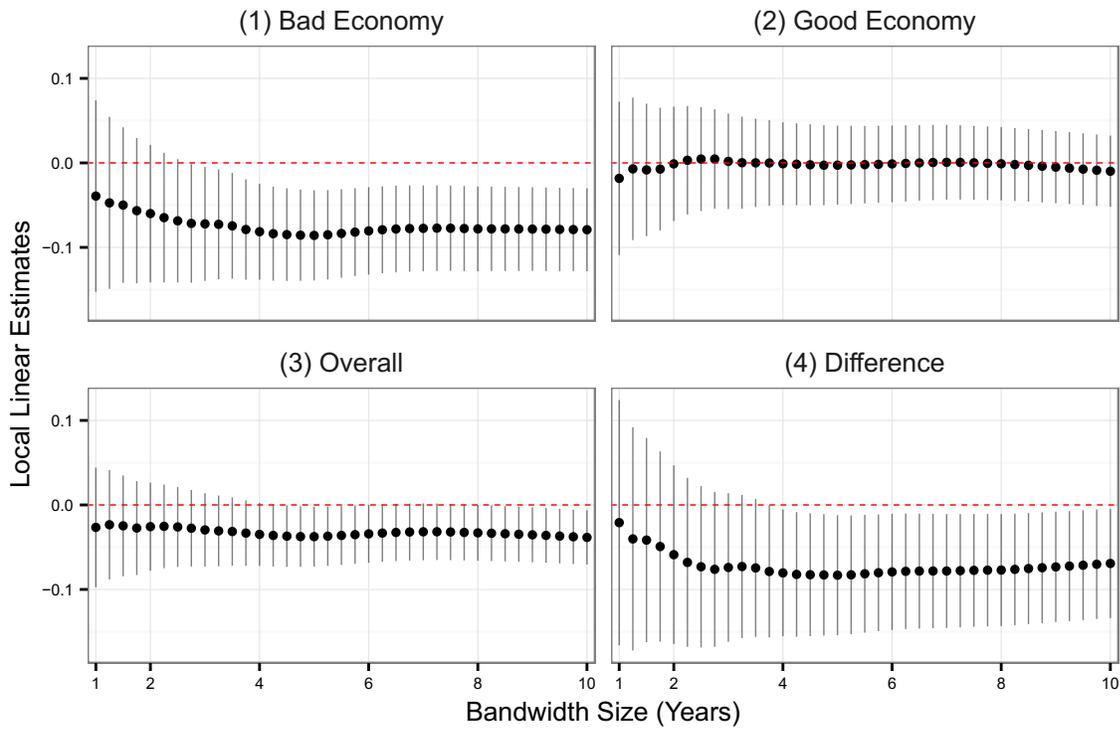
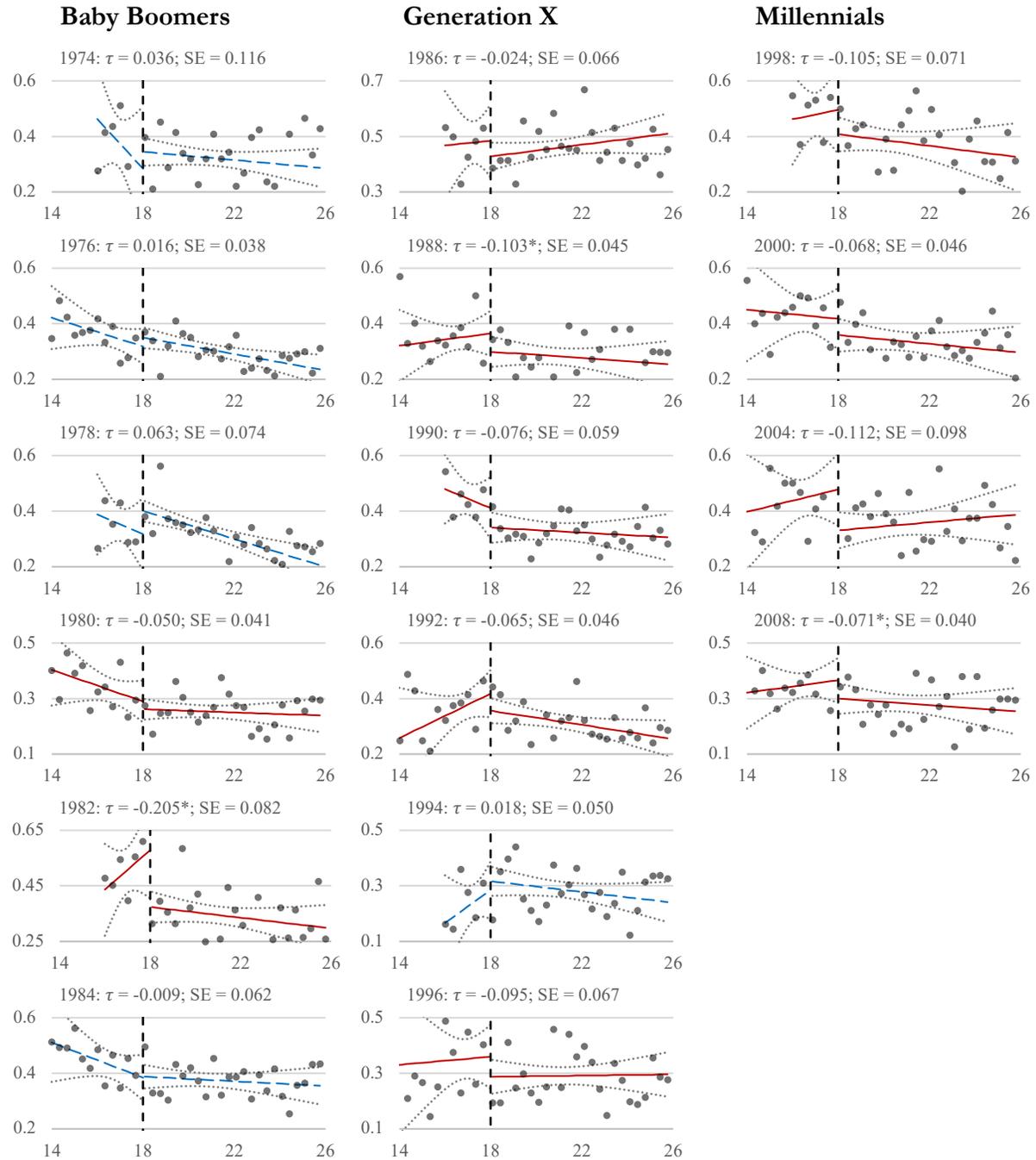


Figure 3: Local Linear Estimates across Bandwidths



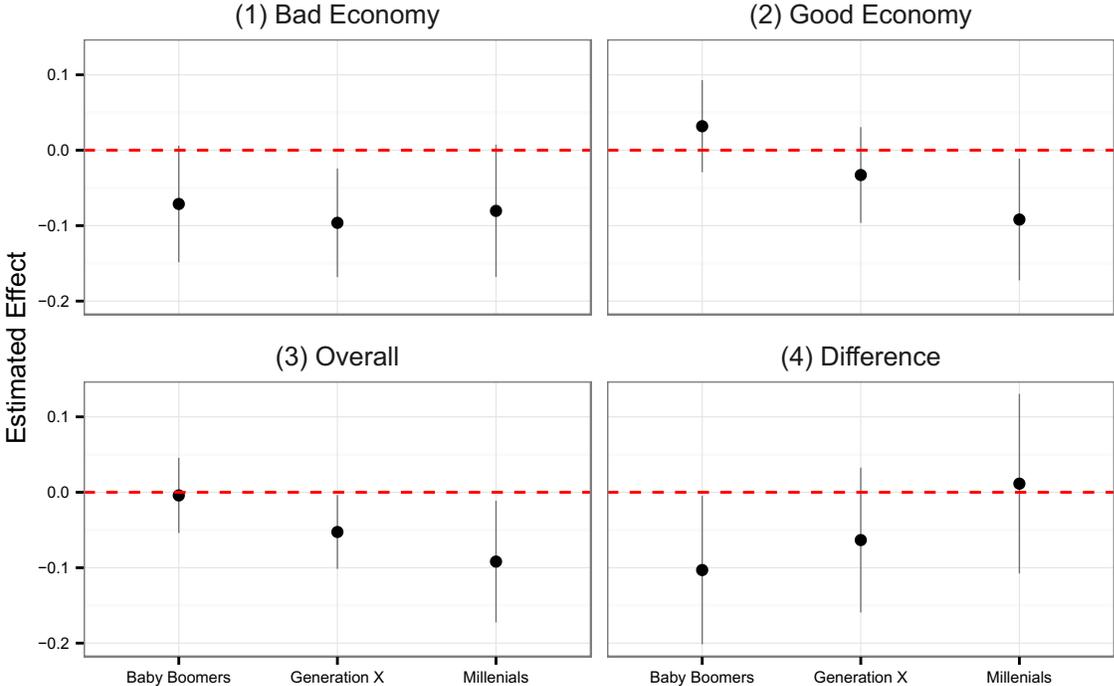
Note. OLS estimates. Dots are point estimates. Bars are 95% confidence intervals.

Figure 4: Regression Discontinuities by Survey Year



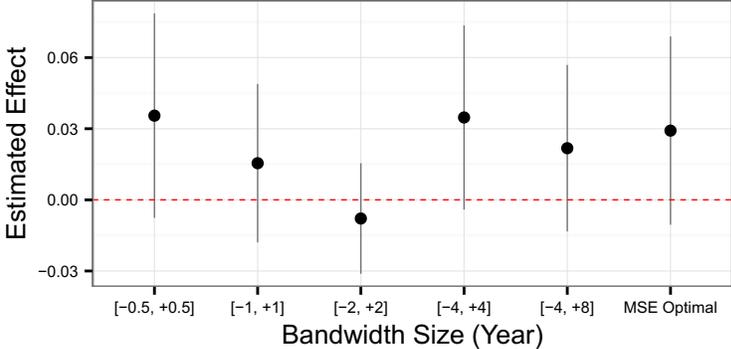
In each graph, Y axis is political trust, and X axis is age at previous presidential election. Triangular kernel-weighted local linear fits with 95% CIs, fitted either side of the cutoff. Solid (red) lines represent discontinuous decrease, and long-dashed (blue) lines represent discontinuous increase. Circles are four-month binned averages. The estimates of  $\tau$  are calculated using MSE optimal bandwidths. 2002 is omitted due to extremely small sample size (i.e., only five observations in the left side of the cutoff).

Figure 5 The Effect of Voting Eligibility by Generation



Note. Triangular kernel-weighted local linear estimates. MSE-optimal bandwidths are used for each model. Dots are point estimates. Bars are 95% confidence intervals.

Figure 6: Effect of *Midterm* Voting Eligibility on Trust by Bandwidths



## Appendix 1: Balance Check

Table A1: Balance Check

Estimator	Bandwidths (Years)	(1)		(2)		(3)		(4)		(5)	
		Diff-in-Means		Diff-in-Means		Local Linear		Local Linear		Local Linear	
		[-0.5, +0.5]		[-1, +1]		[-4, +4]		[-4, +8]		MSE Optimal	
Plausibly Pre-Treat Placebo	College	0.065	(0.051)	0.040	(0.040)	0.011	(0.043)	0.026	(0.036)	0.049	(0.038)
	Income	0.005	(0.050)	0.008	(0.036)	0.008	(0.040)	-0.007	(0.034)	-0.024	(0.039)
	Female	-0.018	(0.048)	-0.015	(0.035)	-0.014	(0.037)	-0.017	(0.032)	0.000	(0.037)
	Black	-0.056	(0.035)	-0.027	(0.026)	-0.031	(0.029)	-0.032	(0.025)	-0.023	(0.027)
	White	0.005	(0.045)	0.015	(0.034)	-0.003	(0.037)	0.015	(0.032)	0.031	(0.034)
	Hispanic	0.029	(0.032)	-0.008	(0.027)	0.017	(0.027)	0.017	(0.027)	-0.005	(0.024)
	South	-0.054	(0.062)	-0.019	(0.056)	-0.030	(0.058)	-0.001	(0.053)	-0.032	(0.053)
	West	0.018	(0.054)	-0.021	(0.045)	-0.004	(0.047)	-0.021	(0.042)	-0.004	(0.045)
	Midwest	0.027	(0.052)	0.027	(0.042)	0.018	(0.045)	0.015	(0.041)	0.025	(0.041)
	East	0.009	(0.046)	0.012	(0.036)	0.016	(0.039)	0.006	(0.036)	0.014	(0.037)
	Church Attendance	-0.028	(0.035)	-0.017	(0.024)	-0.017	(0.027)	-0.001	(0.022)	-0.026	(0.027)
	Christian	0.026	(0.040)	0.003	(0.027)	0.006	(0.030)	0.017	(0.025)	0.002	(0.029)
	Union	0.040*	(0.023)	0.027	(0.018)	0.027	(0.019)	0.009	(0.015)	0.022	(0.016)
	No Occupation	-0.074	(0.048)	-0.061	(0.040)	-0.053	(0.043)	-0.067*	(0.035)	-0.093*	(0.039)
	White Collar Occupation	0.009	(0.048)	0.006	(0.035)	0.004	(0.038)	0.047	(0.031)	0.072*	(0.036)
	Blue Collar Occupation	0.041	(0.026)	0.018	(0.017)	0.014	(0.020)	-0.006	(0.016)	-0.003	(0.016)
	Service Area Occupation	0.024	(0.050)	0.036	(0.037)	0.035	(0.040)	0.026	(0.032)	0.020	(0.036)
	financial	-0.028	(0.041)	-0.023	(0.031)	-0.019	(0.033)	-0.021	(0.027)	0.003	(0.031)
	Married	0.001	(0.045)	0.054*	(0.032)	0.002	(0.036)	0.029	(0.029)	-0.002	(0.038)
	Post-Treat Placebo	Partisan (0= No; 1=Yes)	-0.041	(0.052)	-0.006	(0.036)	-0.006	(0.040)	0.001	(0.034)	-0.058
PID (Scale; 1 = Strong Rep)		-0.063*	(0.030)	-0.012	(0.021)	-0.034	(0.023)	-0.019	(0.019)	-0.037*	(0.021)
Conservative		-0.023	(0.017)	0.004	(0.013)	-0.006	(0.014)	-0.009	(0.012)	-0.022*	(0.013)
Pol. Participation		0.004	(0.017)	0.002	(0.013)	0.007	(0.014)	0.014	(0.012)	0.002	(0.014)
Pol. Interest		-0.023	(0.032)	0.026	(0.024)	-0.001	(0.026)	0.017	(0.022)	0.026	(0.023)
Knowledge (House Majority)		-0.003	(0.050)	0.016	(0.040)	0.004	(0.043)	0.019	(0.034)	-0.002	(0.039)
TV News Exposure		0.043	(0.057)	0.001	(0.039)	0.020	(0.041)	0.027	(0.034)	0.044	(0.036)
Newspaper Exposure		0.049	(0.059)	-0.015	(0.041)	0.003	(0.046)	0.031	(0.038)	0.035	(0.039)
Social Trust		0.013	(0.075)	0.029	(0.052)	0.015	(0.057)	0.021	(0.049)	0.034	(0.048)
Media Trust		0.003	(0.048)	-0.004	(0.035)	-0.022	(0.038)	-0.026	(0.029)	0.005	(0.030)

Note. OLS estimates. Model specifications are analogous to Column 1 of Table 2 in the main text.

## Appendix 2: Average Treatment Effects for Compliers

The estimates we have reported so far are intent-to-treat effects. In Table A2, we provide the local average treatment effects of voting itself, by scaling the effects of voting eligibility by self-reported voting rates at the previous presidential election.

Table A2: Average Treatment Effect on the Treated

Estimator	Bandwidth (Years)	(1) Overall	(2) Bad Economy	(3) Good Economy	(4) Difference	N
Difference-in-Means	[-6, +6]	-0.129 (0.078)	-0.400* (0.204)	0.009 (0.070)	0.408* (0.217)	296
Difference-in-Means	[-12, +12]	-0.136* (0.061)	-0.303* (0.117)	-0.025 (0.067)	0.278* (0.135)	591
Local Linear	[-48, +48]	-0.144* (0.066)	-0.338* (0.140)	-0.029 (0.069)	-0.309* (0.156)	2187
Local Linear	[-48, +96]	-0.147* (0.057)	-0.285* (0.097)	-0.033 (0.068)	-0.252* (0.119)	3833
Local Linear	MSE Optimal	-0.174* (0.057)	-0.337* (0.097)	-0.060 (0.068)	-0.277* (0.119)	5362

First-time presidential voting is estimated to lower political trust on average by 13 to 17 percentage points depending on the specification, and much more so when economic performance is bad (by about 30 to 40 points). Note that these effects are larger than what is expected based on the estimates shown in Table 2. Accounting for a turnout rate of 40 percent would translate the average intent-to-treat effects shown in Table 2 into the average treatment effects on the treated ranging between 8 to 9 percentage points. Two reasons for this discrepancy seem to be that (1) past presidential voting was omitted in several ANES surveys, and the years in which ANES did provide previous voting behavior happened to be the years when voting eligibility had stronger effects, and (2) the turnout gap is likely underestimated (about 33 to 36 percentage points) because some ineligible voters said they voted; for example, 6 percent of 17 year olds indicated that they turned out in the previous presidential election. Given these issues, we focused on intent-to-treat effects in the main text.