Do 40-Year-Old Facts Still Matter? Long-Run Effects of Federal Oversight under the Voting Rights Act

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Online Appendix

Reading this Appendix is not necessary to understanding the main analysis. All results discussed here are also referenced in the main paper. This section simply provides a more detailed explanation of the paper's robustness analyses and secondary data sources.

I. Data

A. Gallup Survey

ANES survey data is one of few available sources of historical information on political preferences identified by respondent race and county. However, given the limited sample size of the data, specifically among minorities, one may be concerned about the robustness of the party identification by race estimates. Thus, to complement those estimates, I replicate the party affiliation by race analysis presented in Section III using historical Gallup survey micro-data identified by respondent state and race. These results were displayed in Table A7 and Table A8.

Specifically, I responses regarding self-reported Republican affiliation and hypothetical opposition to a black presidential candidate. The latter was based on a question asking "If your party nominated a generally well-qualified man for president, would you vote for him if he happened to a Negro?" and coded as 0 for individuals responding "Yes" and 1 otherwise. Responses to the same question were employed by Kuziemko and Washington (2015) as a consistently measured proxy for racial conservatism, similar to the ANES question regarding government aid to minorities. Thus, micro-data were acquired from the Roper Center for all surveys administered in the sample period including the black president question (i.e., 1961, 1963, 1965, 1967, 1969, 1971, 1978, 1983,

1984, 1987, 1999, 2003) and contain between 1,000 and 3,500 responses in each year.

B. Electoral Rules

Municipality-level data employed in Figure A5 and Table A14 comes from International City/County Municipal Association (ICMA), specifically Municipal Yearbooks published in 1972, 1976, 1981, 1986, 1991, 1996, 2001, 2006 and 2011. These data are in turn based on mail-in surveys administered by the ICMA to municipalities in the one or two years prior to publication. Surveys contain questions regarding forms of government and were sent to cities, towns, townships, villages and boroughs with populations larger than 2,500 (5,000 prior to 1980), as well as other municipalities in the ICMA database. Data from 1981 onwards are available in electronic format and are identified by Census FIPS place codes. For earlier years, information was hand-coded from hard copies. Unfortunately, hard copies do not contain contain place codes. Thus, pre-treatment data was first mapped to place codes by name and state then merged with data from latter years by place code. The final dataset contains between 2,000 to 4,000 municipalities in each sample year.

C. Congressional Ideology

As complement to the Democratic vote share analysis presented in Section III, I estimate the effects of preclearance on Congressional ideology and affiliation using DW-NOMINATE data. DW-NOMINATE is a multidimensional scaling technique which collapses legislative roll-call voting into a two-dimensional ideal point. The first dimension is commonly thought of as a liberal-conservative measure (scaled from -1 to 1) and correctly classifies roll-call votes over 90% of the time. This score is also strongly correlated with ideological measures derived from campaign finances (Bonica, 2014) and Congressional speech, even when controlling for political party (Gentzkow et al., 2016). While the second dimension of DW-NOMINATE historically tracked to policy issues that cut across party lines, Congressional voting since the

1960's has been virtually unidimensional (McCarty et al., 1997). Importantly, DW-NOMINATE scores are specific to a representative-Congress and capture within-person changes over time.

Because preclearance coverage was determined at the county-level, I map each district in each Congress to the county or counties that comprise it. Because district borders frequently change, often drastically, this process was non-trivial. However, it was greatly facilitated by relationship files generously provided by James Snyder. For districts comprised of several counties, this data was supplemented with hand-coded information from the Congressional District Atlas, which publishes detailed district maps overlaid with county borders.

II. Robustness

A. Standard Errors

The paper's main analysis employs standard errors clustered at the state-level. This has been shown to correct for serial correlation in situations with close to 50 clusters (Bertrand et al., 2002). As my main analysis relies on 44 clusters, assumptions about asymptotic convergence may not hold. Thus, I test the robustness of my results to various corrections. In particular, I estimate Equation 1 using heteroskedasticity-robust standard errors, county clustered standard errors, state and year multi-way clustered standard errors, and wild-t cluster bootstrapped standard errors presented in Cameron et al. (2008). These results are shown in Table B2.

In nearly all cases, state-clustered standard errors (Column 1) are actually *smaller* than those produced from other methods for pre-treatment estimates. Thus, the 1968 DD estimate for both voter turnout and Democratic vote share is insignificant at the 5% level with all alternative specifications, providing support for the existence of parallel pre-trends. Importantly, post-treatment significance is maintained in nearly all cases.

B. Placebo Tests

B..1 Randomly-Assigned

The fact that standard errors are actually smaller when clustering by state than when clustering by county or not at all suggests that serial correlation is not an overriding concern. Nonetheless, for robustness' sake, I run a series of 500 simulations estimating Equation 1 with randomly generated treatment groups. Table B3 lists the estimated coefficients of interest for voter turnout in Column 1 and for Democratic vote share in Column 2. All coefficients are insignificant and near to zero.

B..2 Faux-Triggers

While the regression discontinuity analysis and flexible determination controls presented in Section IV suggest otherwise, one may be concerned that the cutoffs and variables used to determine preclearance are correlated with differential time trends. To examine this, I estimate "treatment" effects using placebo treatments around the actual coverage cutoffs. In particular, I restrict the analysis to counties with greater than 5% minority population share in 1970 and either 40-50% 1972 turnout or 50-60% 1972 turnout. For the first sample, I then assign treatment according to a "faux"-trigger at 45%. For the second, I assign treatment according to a false trigger at 55%. These estimates are displayed in Columns 1 and 2 of Table B4, respectively.

Notably, none of the post-treatment estimates on voter turnout are significant at the 5%-level and the majority are of the opposite sign (negative) of my main results. Regarding Democratic vote share, four of 22 post-treatment estimates are significant at the 5%-level. However, three of these four estimates, and 11 of the 22 total post-treatment estimates, are positive, while all of the paper's primary post-treatment estimates were negative. Taken together with the extremely limited sample size, these results suggest that my primary estimates are not confounded by differential trends around the true cutoffs.

C. Falsification Tests

In DD models, there is always concern that the estimated effects reflect some omitted, underlying trend orthogonal to the treatment itself. To test this, I use Equation 1 to estimate the impact of treatment on population size and minority share. Unlike other correlates of turnout like income or education which may be directly influenced through political representation, population growth and migration patterns are perhaps more likely to be "pre-determined." But because voter turnout the ratio of votes cast to eligible voters, any unaccounted trends in population could directly bias the outcome of interest.

As shown in Table B3, Columns 3 and 4, this is likely not a concern here. I find little evidence of differential trends between groups in population size or composition, as all coefficients are insignificantly different from zero and small in magnitude.

D. Alternative Fixed Effects

One strength of this study's design is its ability to control for state-level shocks. In including state-year fixed effects, however, it is possible that the estimated treatment effects are not representative of counties in wholly-covered states (i.e., those in Texas and Arizona). I thus estimate Equation 1 replacing state-year indicators with alternative fixed effects.

Column 1 of Table B6 displays the estimated coefficients with a set of year indicators and a dummy variable for the historical use of poll taxes in certain states prior to their nationwide abolition in 1966. Column 2 instead replaces state-year fixed effects with division-year fixed effects. For both models, the results are nearly identical to those including state-year fixed effects, suggesting both the robustness of my findings and their validity with respect to counties in covered states.

E. Alternative Controls

I also demonstrate robustness to alternative controls for the language restrictions concurrently introduced in the 1975 VRA. Because the presence of bilingual election requirements spanned both treatment and control groups and varied between elections within counties, I included a simply policy indicator to account for their effects in my main estimation. However, one may be concerned that this dummy variable fails to capture the actual policy effect or somehow biases the DD estimates of interest. Thus, I estimate Equation 1 with two alternative specifications. One excludes the bilingual indicator entirely, the other includes a full set of bilingual-year interactions, allowing for time-varying effects of bilingual election requirements. As Columns 1 and 2 of Table B5 show, neither method demonstrably changes the sign, magnitude or significance of the coefficients of interest.

Alternatively, one may be concerned that the estimates are biased by unobserved demographic trends, not otherwise captured by the interactions between year and 1970 population measures. To account for this, I further include time-varying controls for these same demographic characteristics. These estimates, which are nearly identical to my main results, are displayed in Column 3.

F. Alternative Outcomes

This paper's main outcomes of interest are untransformed measures of voter turnout (calculated as the share of votes cast to the voting eligible population) and Democratic vote share (calculated as the share of Democratic votes cast to major party votes cast). Here, I estimate effects on an alternative measure of Democratic support — party vote share measured against *all* presidential votes cast (including those for third-party candidates) — as well as on logged voter turnout and Democratic vote share. These estimates are shown in Table B7. Notably, the direction and significance of all estimates are highly similar to those shown in the paper's primary analysis, supporting the existence of parallel pre-trends and large, persistent post-treatment effects.

III. Other Analysis

A. Voting Rights Act of 1965

As noted in Section II, identifying preclearance's effects from the 1965 VRA is problematic due to the Act's concurrent prohibition of literacy tests. Though excluding areas covered in 1965 from the analysis bolsters the internal validity of turnout estimates, one may be concerned that it also diminishes their external validity, particularly with relation to the Southern states most commonly associated with racial discrimination.

To address these concerns, I estimate treatment effects upon the universe of counties ever subject to preclearance by employing a "stacked" difference-in-differences model and controlling for the presence of literacy tests. In particular, I estimate:

$$\begin{aligned} y_{c,t} =& \delta_c + \delta_{d,t} + \sum_{\tau \neq -1} \beta_\tau relativetime_{\tau,t} \times PC_c \\ &+ \gamma_1 bilingual_{c,t} + \gamma_2 literacytest_{c,t} + \gamma_3 polltax_{c,t} + \epsilon_{c,t}, \end{aligned} \tag{7}$$

where $relativetime_{\tau,t}$ are relative time to treatment dummies equal to 1 if election t is τ elections from the treatment. For example, $relativetime_{1,t}$ is set to 1 in 1968 for counties subject to preclearance starting in 1965, in 1972 for counties subject to preclearance starting in 1970 and in 1976 for counties subject to preclearance starting in 1975, and is set to 0 otherwise. As the inclusion of state-year fixed effects would absorb all the treatment variation from the Southern states wholly-covered by the 1965 VRA, I instead include Census division-year effects $\delta_{d,t}$ and a $polltax_{c,t}$ indicator to control for the historical use of poll taxes in certain states. $literacytest_{c,t}$ accounts for the presence of literacy tests at time t among those discriminatory counties where they were later banned.[43] The coefficients of interest β_τ then represent the

[43]Specifically, $literacytest_{c,t}$ is defined as by Husted and Kenny (1997), such that it equals 1 for all counties covered by the 1965 and 1970 VRAs only in years prior to coverage and is set to 0 otherwise, even among those uncovered counties which employed literacy tests

average change between an election τ periods from treatment and the election just prior to treatment ($\tau = -1$) pooled across all treatment counties, relative to that same change over time among control counties.

The results from estimating Equation 7 with and without flexible demographic and eligibility controls are displayed in Table B8. Though some of the pre-treatment DD estimates ($\tau < 0$) are significant at the 10% level, they are inconsistent in sign and do not indicate obvious pre-treatment trends. Following treatment, I find that turnout in covered areas increased steadily over time. Despite examining three times as many treatment counties as in Section III, these effects are remarkably similar to those presented earlier, which also found maximum turnout gains of around 8 p.p.

Though this analysis does not control for idiosyncratic state-level shocks and adopts an admittedly simplistic approach to accounting for literacy test bans, the fact that these estimates so closely align with the paper's main results demonstrates both the robustness of my findings as well as the consistent and powerful effects of preclearance coverage as applied throughout the country.

(for example, those in Massachusetts and Maine). The reason for this is that literacy tests varied greatly between areas and were most restrictive in those areas specifically targeted by the earlier VRAs.

Table B1: Effect on DW-NOMINATE Scores

PC x Cong.	(2)		(3)	
	DV=1st Dim.		*DV=2nd Dim.*	
87	0.004	(0.188)	-0.171	(0.155)
88	-0.020	(0.171)	-0.036	(0.127)
89	-0.031	(0.141)	0.027	(0.129)
90	-0.003	(0.069)	0.059	(0.103)
91	0.024	(0.059)	0.023	(0.093)
92	-0.026	(0.100)	0.095	(0.078)
93	-	-	-	-
94	0.046	(0.020)	0.050	(0.048)
95	0.017	(0.020)	0.125	(0.050)
96	0.034	(0.035)	0.146	(0.047)
97	0.114	(0.096)	-0.000	(0.117)
98	0.088	(0.067)	0.018	(0.115)
99	0.192	(0.062)	-0.068	(0.123)
100	0.159	(0.043)	-0.099	(0.116)
101	0.109	(0.056)	-0.044	(0.116)
102	0.206	(0.066)	-0.083	(0.128)
103	0.129	(0.066)	-0.047	(0.127)
104	0.051	(0.094)	-0.064	(0.126)
105	0.074	(0.076)	-0.098	(0.142)
106	0.102	(0.072)	-0.069	(0.144)
107	0.178	(0.077)	0.132	(0.124)
108	0.191	(0.074)	0.131	(0.119)
109	0.261	(0.064)	0.146	(0.124)
110	0.186	(0.078)	0.228	(0.139)
111	0.297	(0.088)	0.193	(0.128)
112	0.243	(0.075)	0.043	(0.110)
113	0.254	(0.117)	0.133	(0.114)
Obs.	9,449		9,449	
R-sq.	0.721		0.739	

Notes: Data come from DW-NOMINATE. DD coefficients from estimation of district-level analogue of Equation 1 including county and Census division-year fixed effects displayed. Standard errors clustered at the state-level in parentheses. DW-NOMINATE scores collapse Congressional voting into a two-dimensional ideal point, scaled from -1 to 1. The first dimension broadly measures a district representative's conservatism in a given session of Congress (higher is more conservative). The second dimension historically captured attitudes surrounding policy issues that cut across party lines, but has provided little additional explanatory power since the 1960's. The omitted period is the 93rd session of Congress, which ended in 1975.

Table B2: Alternative Standard Errors

		Standard Errors				p-values
PC x Year	Coef.	cluster state (1)	cluster state, year (2)	cluster county (3)	heterosk. robust (4)	wild-t boot (5)
		Panel A: DV=Voter Turnout				
1960	0.012	(0.014)	(0.003)	(0.013)	(0.019)	0.116
1964	0.006	(0.008)	(0.010)	(0.010)	(0.022)	0.110
1968	0.013	(0.003)	(0.010)	(0.007)	(0.020)	0.068
1972	-	-	-	-	-	-
1976	0.021	(0.005)	(0.003)	(0.009)	(0.019)	0.000
1980	0.043	(0.006)	(0.006)	(0.013)	(0.017)	0.058
1984	0.051	(0.008)	(0.003)	(0.009)	(0.017)	0.054
1988	0.066	(0.008)	(0.003)	(0.011)	(0.016)	0.000
1992	0.061	(0.010)	(0.009)	(0.015)	(0.016)	0.086
1996	0.079	(0.008)	(0.016)	(0.020)	(0.017)	0.136
2000	0.083	(0.010)	(0.017)	(0.021)	(0.016)	0.014
2004	0.081	(0.011)	(0.017)	(0.021)	(0.016)	0.092
2008	0.078	(0.012)	(0.017)	(0.021)	(0.016)	0.132
2012	0.081	(0.014)	(0.014)	(0.018)	(0.016)	0.162
2016	0.078	(0.009)	(0.016)	(0.021)	(0.017)	0.096
		Panel B: DV=Dem. Vote Share				
1960	0.005	(0.026)	(0.018)	(0.024)	(0.032)	0.076
1964	0.018	(0.019)	(0.016)	(0.016)	(0.027)	0.068
1968	-0.000	(0.020)	(0.019)	(0.018)	(0.032)	0.064
1972	-	-	-	-	-	-
1976	-0.026	(0.009)	(0.009)	(0.012)	(0.022)	0.054
1980	-0.002	(0.013)	(0.007)	(0.015)	(0.019)	0.000
1984	-0.027	(0.017)	(0.014)	(0.019)	(0.020)	0.046
1988	-0.031	(0.023)	(0.018)	(0.025)	(0.021)	0.068
1992	-0.049	(0.023)	(0.018)	(0.025)	(0.022)	0.000
1996	-0.048	(0.018)	(0.014)	(0.022)	(0.020)	0.044
2000	-0.053	(0.019)	(0.017)	(0.021)	(0.020)	0.100
2004	-0.046	(0.018)	(0.014)	(0.020)	(0.018)	0.114
2008	-0.035	(0.019)	(0.015)	(0.021)	(0.017)	0.140
2012	-0.025	(0.019)	(0.015)	(0.024)	(0.019)	0.082
2016	-0.012	(0.026)	(0.020)	(0.030)	(0.024)	0.158

Notes: Data come from ICPSR and Dave Leip's Election Atlas. Standard errors calculated with various methodologies in parentheses, p-values from bootstrapping listed in Column (5). Coefficients and state-clusterd standard errors (shown in Column 1) are derived from main estimation results plotted in Figures 3 and 5.

Table B3: Placebo Tests: Random Assignment and Population Measures

	Treatment Placebos				Outcome Placebos			
PC x Year	(1)		(2)		(3)		(4)	
	DV=Turnout		*DV=Dem. Share*		*DV=ln(VAP)*		*DV=% Minority*	
1960	0.000	0.008	-0.001	0.008	-0.010	(0.082)	-0.005	(0.010)
1964	0.000	0.008	0.000	0.007	-0.007	(0.050)	-0.002	(0.006)
1968	0.000	0.005	-0.000	0.007	-0.005	(0.024)	-0.001	(0.003)
1972			-	-	-	-	-	-
1976	0.000	0.005	0.000	0.007	-0.004	(0.008)	-0.000	(0.002)
1980	0.001	0.007	0.000	0.008	-0.016	(0.017)	0.001	(0.004)
1984	0.001	0.007	0.000	0.007	-0.003	(0.017)	0.002	(0.003)
1988	0.001	0.008	0.000	0.008	0.011	(0.021)	0.002	(0.003)
1992	0.001	0.010	0.000	0.009	-0.019	(0.027)	0.005	(0.004)
1996	0.001	0.010	-0.001	0.010	-0.024	(0.033)	0.005	(0.003)
2000	0.001	0.009	-0.001	0.011	0.004	(0.025)	0.004	(0.005)
2004	0.001	0.010	-0.001	0.011	0.020	(0.027)	0.003	(0.004)
2008	0.000	0.010	-0.001	0.012	0.020	(0.028)	0.002	(0.004)
2012	0.001	0.009	-0.001	0.013	0.028	(0.026)	0.003	(0.005)
2016	0.000	0.010	-0.001	0.014	0.027	(0.028)	0.002	(0.006)
PC=	Assigned Randomly				Actual Treatment			
Obs.					37,605		37,605	
R-sq.	500 reps		500 reps		0.994		0.965	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 with randomly assigned treatment status and for outcome placebos displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B4: Placebo Tests: Faux-Triggers

Less x Year	(1)		(2)	
	Panel A: DV=Voter Turnout			
1960	-0.020	(0.023)	-0.022	(0.015)
1964	-0.017	(0.014)	-0.014	(0.010)
1968	-0.027	(0.019)	-0.020	(0.010)
1972	-	-	-	-
1976	-0.018	(0.012)	0.006	(0.007)
1980	-0.007	(0.015)	0.004	(0.010)
1984	-0.015	(0.014)	0.009	(0.012)
1988	-0.011	(0.016)	0.015	(0.011)
1992	-0.010	(0.018)	0.023	(0.013)
1996	-0.015	(0.014)	0.016	(0.013)
2000	-0.018	(0.019)	-0.002	(0.015)
2004	-0.006	(0.020)	-0.001	(0.014)
2008	-0.001	(0.022)	-0.008	(0.014)
2012	-0.008	(0.025)	-0.019	(0.017)
2016	0.006	(0.025)	-0.007	(0.016)
	Panel B: DV=Dem. Vote Share			
1960	-0.018	(0.021)	0.010	(0.010)
1964	-0.034	(0.021)	-0.019	(0.013)
1968	-0.021	(0.016)	-0.008	(0.008)
1972	-	-	-	-
1976	-0.025	(0.014)	-0.013	(0.009)
1980	-0.006	(0.013)	0.006	(0.007)
1984	-0.012	(0.013)	-0.017	(0.009)
1988	-0.008	(0.015)	-0.026	(0.011)
1992	-0.012	(0.014)	-0.015	(0.012)
1996	0.008	(0.015)	0.021	(0.018)
2000	0.026	(0.014)	0.025	(0.020)
2004	0.027	(0.014)	0.016	(0.021)
2008	0.042	(0.017)	-0.008	(0.020)
2012	0.052	(0.019)	-0.000	(0.021)
2016	0.047	(0.022)	0.048	(0.025)
Turnout	40-50		50-60	
Faux-Trigger	<45		<55	
Obs.	1,530		1,805	
R-sq.	1.000		1.000	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 examining counties near the turnout cutoff and using "faux" treatment triggers. Column 1 restricts the sample to high-minority counties with 1972 turnout between 40-50% and sets a placebo trigger at 45%. Column 2 restricts the sample to high-minority counties with 1972 turnout between 50-60% and sets a placebo trigger at 55%. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B5: Alternative Controls

PC x Year	(1)		(2)		(3)	
			Panel A: DV=Voter Turnout			
1960	0.009	(0.015)	0.008	(0.015)	0.007	(0.014)
1964	0.005	(0.008)	0.005	(0.008)	0.003	(0.009)
1968	0.012	(0.003)	0.011	(0.003)	0.011	(0.003)
1972	-	-	-	-	-	-
1976	-0.003	(0.006)	-0.020	(0.014)	0.012	(0.005)
1980	0.014	(0.007)	0.012	(0.008)	0.029	(0.006)
1984	0.025	(0.004)	0.022	(0.006)	0.041	(0.007)
1988	0.035	(0.007)	0.041	(0.011)	0.051	(0.008)
1992	0.033	(0.008)	0.050	(0.011)	0.038	(0.009)
1996	0.044	(0.011)	0.061	(0.008)	0.051	(0.011)
2000	0.050	(0.011)	0.067	(0.011)	0.060	(0.012)
2004	0.043	(0.012)	0.065	(0.012)	0.052	(0.013)
2008	0.039	(0.011)	0.060	(0.013)	0.048	(0.013)
2012	0.046	(0.011)	0.062	(0.015)	0.052	(0.014)
2016	0.048	(0.010)	0.054	(0.009)	0.048	(0.011)
			Panel B: DV=Dem. Vote Share			
1960	0.006	(0.026)	0.006	(0.026)	0.003	(0.030)
1964	0.018	(0.019)	0.018	(0.019)	0.017	(0.021)
1968	0.001	(0.020)	0.000	(0.020)	-0.001	(0.021)
1972	-	-	-	-	-	-
1976	-0.010	(0.005)	-0.013	(0.005)	-0.021	(0.008)
1980	0.015	(0.008)	0.017	(0.009)	0.003	(0.013)
1984	-0.010	(0.013)	-0.011	(0.017)	-0.022	(0.016)
1988	-0.014	(0.022)	-0.011	(0.024)	-0.026	(0.022)
1992	-0.036	(0.022)	-0.038	(0.023)	-0.051	(0.023)
1996	-0.035	(0.016)	-0.048	(0.017)	-0.046	(0.018)
2000	-0.040	(0.017)	-0.060	(0.021)	-0.050	(0.020)
2004	-0.033	(0.019)	-0.045	(0.018)	-0.042	(0.018)
2008	-0.022	(0.022)	-0.033	(0.019)	-0.030	(0.019)
2012	-0.015	(0.021)	-0.031	(0.018)	-0.021	(0.020)
2016	-0.008	(0.027)	-0.023	(0.024)	-0.009	(0.026)
Add'l Ctrls.	-		Bilingual x Year		Demo. (Time-Varying)	
Omitted Ctrls.	Bilingual		-		-	
Obs.	37,606		37,606		37,604	
R-sq.	0.926		0.931		0.933	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 with alternative control specifications displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B6: Alternative Fixed Effects

PC x Year	(1)		(2)	
	Panel A: DV=Voter Turnout			
1960	-0.027	(0.025)	0.015	(0.029)
1964	0.018	(0.024)	0.005	(0.019)
1968	-0.021	(0.010)	-0.015	(0.010)
1972	-	-	-	-
1976	0.062	(0.015)	0.040	(0.014)
1980	0.066	(0.015)	0.049	(0.016)
1984	0.099	(0.022)	0.066	(0.013)
1988	0.102	(0.017)	0.079	(0.014)
1992	0.076	(0.012)	0.061	(0.009)
1996	0.065	(0.014)	0.068	(0.013)
2000	0.061	(0.018)	0.075	(0.015)
2004	0.043	(0.015)	0.067	(0.012)
2008	0.037	(0.016)	0.072	(0.016)
2012	0.037	(0.018)	0.073	(0.021)
2016	0.053	(0.023)	0.069	(0.017)
	Panel B: DV=Dem. Vote Share			
1960	0.025	(0.024)	-0.017	(0.017)
1964	0.011	(0.031)	-0.008	(0.031)
1968	0.057	(0.020)	-0.005	(0.011)
1972	-	-	-	-
1976	0.013	(0.019)	-0.080	(0.023)
1980	-0.017	(0.021)	-0.052	(0.016)
1984	-0.039	(0.011)	-0.060	(0.011)
1988	-0.023	(0.011)	-0.068	(0.016)
1992	-0.046	(0.013)	-0.087	(0.026)
1996	-0.051	(0.020)	-0.076	(0.030)
2000	-0.080	(0.025)	-0.075	(0.032)
2004	-0.072	(0.023)	-0.066	(0.026)
2008	-0.072	(0.016)	-0.055	(0.013)
2012	-0.069	(0.019)	-0.045	(0.015)
2016	-0.016	(0.012)	-0.015	(0.014)
Add'l Ctrls.	Year, Polltax		Division x Year, Polltax	
Omitted Ctrls.	State x Year		State x Year	
Obs.	37,620		37,620	
R-sq.	0.760		0.838	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 with alternative control specifications displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B7: Alternative Outcomes

PC x Year	(1)		(2)		(3)	
	DV=ln(Turnout)		*DV=ln(Dem. Share)*		*DV=Dem. Share (All)*	
1960	0.019	(0.027)	-0.005	(0.040)	0.005	(0.026)
1964	0.008	(0.016)	0.021	(0.015)	0.018	(0.019)
1968	0.020	(0.006)	0.001	(0.029)	-0.011	(0.011)
1972	-	-	-	-	-	-
1976	0.013	(0.011)	-0.066	(0.029)	-0.025	(0.009)
1980	0.048	(0.011)	-0.013	(0.038)	0.001	(0.013)
1984	0.073	(0.022)	-0.078	(0.048)	-0.028	(0.016)
1988	0.090	(0.026)	-0.082	(0.059)	-0.031	(0.022)
1992	0.079	(0.030)	-0.112	(0.056)	-0.039	(0.018)
1996	0.095	(0.020)	-0.106	(0.049)	-0.042	(0.018)
2000	0.105	(0.029)	-0.121	(0.052)	-0.051	(0.020)
2004	0.095	(0.032)	-0.101	(0.048)	-0.047	(0.017)
2008	0.090	(0.034)	-0.070	(0.044)	-0.036	(0.018)
2012	0.095	(0.041)	-0.054	(0.045)	-0.027	(0.019)
2016	0.090	(0.028)	-0.022	(0.053)	-0.013	(0.024)
Obs.	37,606		37,576		37,576	
R-sq.	0.998		0.9		0.921	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 1 for alternative outcomes displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes demographic controls.

Table B8: Counties Covered under All VRA Versions

PC x Rel.Time	(1)		(2)	
	DV=Voter Turnout			
-4	-0.005	(0.029)	0.000	(0.028)
-3	0.012	(0.012)	0.032	(0.017)
-2	-0.040	(0.020)	-0.027	(0.017)
-1	-	-	-	-
0	0.020	(0.019)	0.035	(0.019)
1	0.006	(0.025)	0.022	(0.025)
2	0.024	(0.026)	0.038	(0.024)
3	0.038	(0.026)	0.048	(0.025)
4	0.041	(0.024)	0.045	(0.024)
5	0.040	(0.026)	0.046	(0.026)
6	0.050	(0.024)	0.058	(0.024)
7	0.056	(0.021)	0.060	(0.022)
8	0.052	(0.023)	0.057	(0.024)
9	0.045	(0.024)	0.048	(0.024)
10	0.059	(0.024)	0.056	(0.024)
11	0.067	(0.023)	0.064	(0.023)
12	0.065	(0.024)	0.068	(0.026)
Demo. Ctrls.	-		Yes	
Obs.	46,591		46,533	
R-sq.	0.837		0.847	

Notes: Data come from ICPSR and Dave Leip's Election Atlas. DD coefficients from estimation of Equation 7 displayed. Standard errors clustered at the state-level in parentheses. Observations weighted by voting eligible population. Includes all counties brought under preclearance coverage by the 1965, 1970 and 1975 versions of the VRA. Relative time corresponds to the number of presidential elections before/after treatment.