Econometrica Supplementary Material

SUPPLEMENT TO "VOTING TECHNOLOGY, POLITICAL RESPONSIVENESS, AND INFANT HEALTH: EVIDENCE FROM BRAZIL"

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APPENDIX A: DESCRIPTION OF VOTING TECHNOLOGIES

THE INTERFACE OF THE EV TECHNOLOGY is a small screen and a set of keys closely resembling a touch-tone phone with the addition of three colored buttons, as Figure 1 illustrates. Panel B depicts the initial screen a voter faces. It provides an instruction to vote for a state legislator (*deputado estadual*). Once the voter types the number of his choice of candidate, her information (photo, name, and party) appears on the screen, as depicted on Panel C. A voter can then "confirm" his vote by pressing the green button or "correct" his vote using the orange button. The former option casts a vote and the latter restarts the process and returns to the initial screen. There are two ways a residual vote can be cast with EV. One is by pressing the "blank" (white) button. Another one is by typing a number that does not correspond to a candidate. Doing so will lead to the screen depicted on Figure A1. Pressing the "confirm" button at this point will lead to a residual vote.

There are at least three features of the electronic technology that can reduce residual votes, especially from less educated voters. First, there is the introduction of visual aids (candidate photographs). Second, the machine provides instant feedback to the voter. When he types a wrong number, the machine informs him (Figure A1). Note that even a completely illiterate voter that could not read the "wrong number" message on the screen would notice that a picture has not appeared on the screen. Obviously, a piece of paper cannot provide this type of feedback.

Third, the technology guides the user through the many votes (state legislator, federal legislator, senator, governor, president) he has to cast. It also informs the voter when all votes are cast, making it less likely that he forgets to vote for one office. Moreover, with paper ballots, a voter may misvote by, for example, writing the number of a state legislator in the federal legislator box. The electronic device also makes salient how many digits are needed for a vote so that even a completely illiterate voter can take this as a cue for which vote he hast to cast. For example, Figure 1 depicts the machine requesting the user for a 5-digit number, which has to be the case of a state legislator (as federal legislators have 4-digit and other offices use 2-digit numbers).

In principle, another possible explanation for the reduction in residual votes is due to voting technology affecting electoral fraud at the vote-counting stage. However, this is unlikely to be the case. There is virtually no evidence of electoral fraud related to the electronic system, which is perceived as honest by

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FIGURE A1.—Residual vote under the electronic technology.

98% of Brazilians and usually presented as an international case study of trustworthy government technology.[1] While there is evidence of vote-tallying fraud during the military rule (1964–1985) and the democratic transition period, these are much less likely to be prevalent during the 1998 election analyzed here. Moreover, it must be noted that to explain the *entirety* of effects (i.e., all additional valid votes), fraud under paper ballots would require tampering with more than 10% of all ballots cast (and up to 15%–20% in high-illiteracy municipalities), which would add to millions of pieces of paper nationwide and make it unlikely to go undetected. Moreover, "ballot-stuffing" is also unlikely to explain the entirety of results, since it would generate differences in official turnout between municipalities below and above the cutoff.

Finally, one possible way to assess the possibility of fraud would be to estimate EV's effect on the vote shares of parties that one could presume are in a position to engage in fraud. However, these are difficult to define in a non-ad hoc manner. While focusing on parties controlling national, state, or local governments would be a priori reasonable, there is little variation to be exploited in such exercise. The main center-right party alliance that controlled the federal congress was also in power in the vast majority of municipalities around the cutoff. Hence, estimating effects on the vote share of parties in power would be primarily to capture these party-specific effects and would reveal little about the possibility of fraud.

APPENDIX B: ADDITIONAL RDD RESULTS

This appendix provides two sets of additional results regarding the effect of electronic technology on valid votes. First, the effect of EV on residual (invalid) votes is decomposed by its effect on "blank" and "null" voting. Second,

[1]This figure from 2008 was cited in Avgerou, Ganzaroli, Poulymenakou, and Reinhard (2009), which also presented the electronic voting system and the Brazilian electoral authority as a success story in fostering citizens' trust in information technology and government institutions.

the estimated effects on valid voting in elections for other offices are presented (lower chamber of federal congress, federal senate, governor, and president). The evidence provided in this appendix supports the interpretation that EV increases valid voting by facilitating ballot operation and diminishing errors in cast votes.

A residual (i.e., not valid) vote is classified as either blank ("*branco*") or null ("*nulo*"). When a paper ballot is in use, a vote is considered blank when nothing is written on the ballot, and it is considered as null when the number or name on the ballot cannot be assigned to any candidate. With electronic technology, a null vote is cast when the voter confirms a vote for a number that does not correspond to a candidate, and a blank vote is cast by pressing the "blank" button on the machine.[2]

Panel A of Table A-I presents the estimated treatment effects of EV on blank and null votes separately, with each measure as a share of the total turnout (which equals the sum of valid and invalid votes). The results are estimated with the exact same procedure of those reported in Table II and described in Section 2. The estimates indicated that EV reduces null voting by an amount equivalent to 8 p.p. of total turnout, with some minor variation depending on specification and bandwidth used. Blank votes, on the other hand, are reduced by about 4 p.p. of total turnout. Part of the larger effect on null votes can be explained by its higher prevalence under paper ballots (13.8% of voters left their paper ballots blank and 9.3% cast a null vote). The results above are consistent with the interpretation that some voters were unable to properly fill the paper ballots (since a possible reaction for someone who is challenged by the task is to leave it blank).

Panel B of Table A-I reports the estimated effects of EV on the ratio between valid votes and turnout. The first line reports the same estimates provided in Table II for the state legislature, and the following lines present the similar estimated effects for elections for federal congress (lower chamber and senate), state governor, and federal president. Recall that these five separate races occurred simultaneously, and hence the turnout in all races is the exact same.

Panel B indicates that the effect of EV is larger in the proportional representation races where a paper ballot requires writing down the name or number of the candidate (lower chamber of congress and state legislature) than in the plurality races where a paper ballot involves checking a box (senate, governor, and president). Since writing a name/number is presumably more difficult than checking a box, especially for the less educated, the results support the interpretation that the electronic technology facilitates the task of voting. The effects on valid voting for the lower chamber of federal congress are larger than

[2]This description applies to the case of election under proportional representation rules, such as the ones for state legislature. In the case of plurality elections where a paper ballot requires checking a box, a blank vote occurs when no candidate is checked, and a null vote when multiple candidates are checked.

TABLE A-I

ADDITIONAL TREATMENT EFFECTS OF ELECTRONIC VOTING[a]

	Full Sample Mean	Pre-Treat. Mean	IKBW {obs}	(1)	(2)	(3)
Panel A: Effects on Null and Blank Votes, State Legislature Election						
Null Votes/Turnout	0.136	0.134	12,605	−0.087	−0.086	−0.084
	[0.056]	(0.008)	{286}	(0.009)	(0.010)	(0.015)
Blank Votes/Turnout	0.109	0.093	12,659	−0.037	−0.036	−0.040
	[0.041]	(0.005)	{287}	(0.007)	(0.008)	(0.012)
Panel B: Effects on Valid Votes/Turnout, by Office						
State Legislators	0.755	0.780	11,873	0.118	0.121	0.124
	[0.087]	(0.013)	{265}	(0.015)	(0.016)	(0.025)
Federal Congress	0.704	0.689	11,546	0.212	0.210	0.205
(Lower Chamber)	[0.092]	(0.015)	{259}	(0.017)	(0.030)	(0.019)
Federal Congress	0.666	0.704	14,287	0.074	0.087	0.100
(Senate)	[0.106]	(0.012)	{342}	(0.016)	(0.020)	(0.030)
State Governor	0.721	0.749	14,795	0.069	0.078	0.082
	[0.097]	(0.011)	{352}	(0.014)	(0.017)	(0.026)
President	0.767	0.796	17,386	0.016	0.021	0.031
	[0.068]	(0.007)	{446}	(0.011)	(0.013)	(0.021)
Bandwidth:	—			IKBW	10,000	5000
Observations:	5281			—	229	116

[a]Robust standard errors in parentheses, standard deviations in brackets, number of observations in curly brackets—{}. The unit of observation is a municipality. Each figure in columns (1)–(3) is from a separate local linear regression estimate with the specified bandwidth. The pre-treatment mean is the estimated value of the dependent variable for a municipality with 40,500 registered voters that uses paper ballot (based on the specification on column 1). The IKBW column provides the Imbens and Kalyanaraman (2012) optimal bandwidth (capped at 20,000) and the associated number of observations. Details on the dependent variables in the text.

the ones for the state legislature. Comparison of the "pre-treatment means" in Table A-I indicate that this difference could be due to the fact that valid voting in federal elections is smaller than in state elections. The effects are particularly small (and statistically insignificant) for presidential elections. A possible explanation would be that since the presidential race dominates popular and media attention, voters are particularly better prepared to cast their votes for president, even when using the more error-prone paper ballot.

APPENDIX C: ROBUSTNESS CHECKS ON RDD ESTIMATES

This section provides further results regarding the validity of the RDD estimates of the effects of EV presented in Section 2. First, it provides evidence against the manipulation of the position of the 40,500-voter threshold and also of the forcing variable (number of registered voters). Second, it briefly discusses the presence of other discontinuous assignment rules across Brazilian municipalities, and why they are unlikely to affect the results.

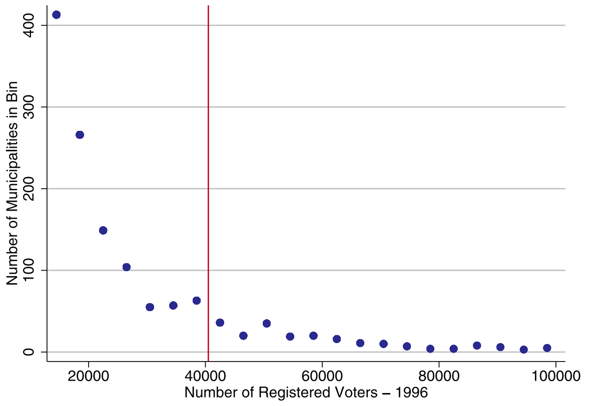

FIGURE A2.—Testing manipulation of forcing variable. Each marker represents the number of municipalities in a 4000-voter bin. The vertical line marks the 40,500-voter threshold.

C.1. *Manipulation of the Forcing Variable and Threshold Position*

To test the possibility of strategic manipulation, Figure A2 plots the distribution of number of voters as of 1996 (the forcing variable in the RDD) across municipalities. Each marker represents the number of municipalities that fall into a 4000-voter interval. For example, the circle to the immediate right of the vertical line represents the number of municipalities that have between 40,500 and 44,500 registered voters.

If strategic manipulation has taken place, it would likely reflect in a jump around the threshold. For example, voter registration was encouraged in municipalities that would be close to the left of the threshold; then the number of municipalities just above the threshold would probably be unusually large compared to the number of municipalities just below it, creating a discontinuity in the distribution. The same logic would apply to deterrence of registration. Figure A2, however, shows that the distribution of municipalities is smooth around the threshold, providing no evidence of manipulation. This is, as discussed in Section 2, expected since the forcing variable (number of voters in 1996) was measured and determined before the announcement of the threshold (in 1998).

Another potential threat to validity would occur if the position of the threshold was manipulated, for example, if the federal electoral authority could have chosen the 40,500-voter cutoff in order to extend EV in a state that had a large number of municipalities just above this cutoff. To test if the threshold was manipulated based on state characteristics, I check if it is possible to predict the voting technology used in a municipality (i.e., on which side of threshold it is) with the information on to which state it belongs.

The test is performed in two steps. First, I compute the predicted values from a regression of a dummy indicator for electronic vote use (i.e., being to the

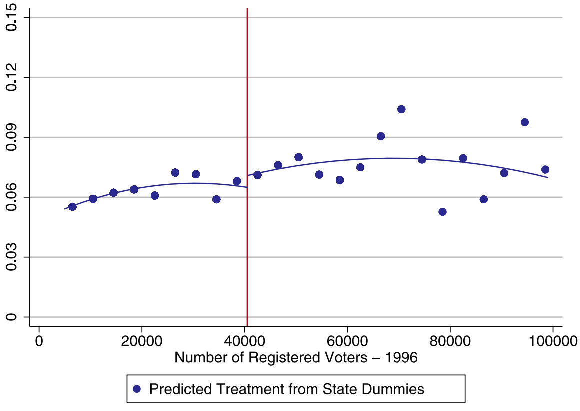

FIGURE A3.—Testing manipulation of the threshold position. Each marker represents the average value of the variable in a 4000-voter bin. The continuous lines are from a quadratic fit over the original ("unbinned") data. The vertical line marks the 40,500-voter threshold.

right of the threshold) against a full set of state dummies (using municipal-level data for 1998). Second, I test if the relationship between these predicted values and the forcing variable (number of voters) is smooth around the threshold. Figure A3 provides the graphical evidence on this second step: the predicted value evolves continuously around the threshold.

The evidence in Figure A3 is supportive of nonmanipulation of the threshold's position. If the cutoff was chosen in order to target a particular state, then information on which state a municipality is located in (captured by the state dummies) would predict EV and lead to a "jump" around the threshold. In other words, nonmanipulation implies that the probability of a municipality "just below" and "just above" the threshold being in a particular state should be, on average, the same. Figure A3 shows that this is the case.[3]

C.2. *Other Discontinuous Assignments*

To the best of my knowledge, there is only one discontinuous assignment across Brazilian municipalities that, like the one used in this paper, is based on the number of registered voters. It occurs in municipal elections that do not occur simultaneously with the state legislature elections analyzed in this paper. Municipalities with more than 200,000 voters should use runoff rules instead of plurality to elect its mayors. Given that 200,000 is far from the 40,500-voter

[3]Note also that by using predicted values from all state dummies, this test has more power than the alternative of testing if every individual state has a continuous distribution of municipalities around the threshold. The rationale behind this procedure is similar to that of, in a randomized trial context, testing if all available covariates can predict treatment status, as opposed to checking for treatment effects on individual covariates.

threshold in this paper and that municipal and state elections are never held in the same year, there is no reason to believe that it can confound the results.[4]

There are other discontinuous assignments based on a municipality's population (and not number of voters, as the one used in this paper). All but one were created *after* the 1998 election, and hence cannot account for the results.[5] The exception is a multi-threshold rule regarding the distribution of federal funds to municipal governments. Since this rule has been present throughout the whole period analyzed in this paper, any confounding effects it could have on the results should also be seen in the placebo tests using the 1994 and 2002 elections. Since the behavior of valid voting is smooth around the 40,500-voter threshold in those two elections, it is unlikely that the assignment of federal funds confounded the results.[6]

APPENDIX D: EFFECTS ON PARTY SEAT SHARES

Table A-II provides the effects of EV on a party's representation in the state's legislative assembly. Effects are provided for the ten most represented

TABLE A-II

EFFECT ON PARTY SEAT SHARES[a]

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
Sample (Terms):		1994–1998 (Paper–Disc.)	1998–2002 (Disc.–Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
PCdoB	0.010	−0.002	0.019	−0.011	0.008
	[0.016]	(0.003)	(0.013)	(0.007)	(0.007)
		{0.564}	{0.232}	{0.170}	{0.340}
PDT	0.066	0.089	−0.066	0.077	0.011
	[0.054]	(0.049)	(0.041)	(0.042)	(0.018)
		{0.064}	{0.300}	{0.084}	{0.528}
PFL	0.150	−0.023	−0.025	0.001	−0.024
	[0.106]	(0.086)	(0.031)	(0.046)	(0.046)
		{0.814}	{0.466}	{0.994}	{0.654}

(*Continues*)

[4] Fujiwara (2011), Chamon, de Mello, and Firpo (2009), and Gonçalves, Madeira, and Rodrigues (2008) exploited this RDD to estimate the effects of runoff systems.

[5] Population-based discontinuous assignments created in the 2000s include regulations for the size of municipal legislatures and the wages of its members, and also restrictions on the use of firearms by municipal police forces. Ferraz and Finan (2009) exploited the RDD regarding legislator wages.

[6] Litschig and Morrison (2013) and Brollo, Nannicini, Perotti, and Tabellini (2013) exploited this discontinuity to estimate effects of government grants.

TABLE A-II—*Continued*

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
Sample (Terms):		1994–1998 (Paper–Disc.)	1998–2002 (Disc.–Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
PL	0.052	−0.025	0.030	−0.028	0.003
	[0.048]	(0.044)	(0.032)	(0.024)	(0.030)
		{0.588}	{0.508}	{0.274}	{0.954}
PMDB	0.170	0.038	0.078	−0.020	0.057
	[0.096]	(0.082)	(0.041)	(0.046)	(0.049)
		{0.696}	{0.106}	{0.686}	{0.362}
PPS	0.022	−0.028	0.014	−0.021	−0.007
	[0.030]	(0.016)	(0.021)	(0.015)	(0.012)
		{0.082}	{0.526}	{0.190}	{0.612}
PSB	0.042	0.010	0.052	−0.021	0.031
	[0.056]	(0.022)	(0.027)	(0.020)	(0.013)
		{0.714}	{0.060}	{0.348}	{0.054}
PSDB	0.112	−0.054	−0.099	0.023	−0.077
	[0.096]	(0.063)	(0.045)	(0.047)	(0.030)
		{0.466}	{0.054}	{0.654}	{0.068}
PT	0.094	0.030	−0.087	0.058	−0.029
	[0.062]	(0.027)	(0.022)	(0.022)	(0.012)
		{0.290}	{0.004}	{0.026}	{0.010}
PTB	0.070	0.003	−0.025	0.014	−0.011
	[0.064]	(0.098)	(0.020)	(0.049)	(0.052)
		{0.982}	{0.224}	{0.746}	{0.814}
Other Parties	0.214	−0.038	0.110	−0.074	0.036
	[0.116]	(0.103)	(0.067)	(0.039)	(0.079)
		{0.680}	{0.214}	{0.080}	{0.658}
N (State-Terms)	—	54	54	—	—
N (States/First-Diffs)	—	27	27	—	—

[a]Standard errors clustered at the state level in parentheses. Standard deviations in brackets. p-values based on Cameron et al. (2008) cluster-robust wild-bootstrap in curly brackets—{}. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}). Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.

parties, and the smaller parties are aggregated into the "other parties" categories. Parties are referred to by their local acronyms. Table A-II follows the same pattern used for Table IV. The only parties that follow the sign-switch pattern and have a significant implied average effect (column 3) are the PT—Partido dos Trabalhadores (Workers' Party) and the PDT—Partido Democrático Trabalhista (Democratic Labor Party), two traditionally left-wing parties.

TABLE A-III
EFFECT ON LEVELS OF SPENDING[a]

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
Sample (Terms):		1994–1998 (Paper–Disc.)	1998–2002 (Disc.–Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Health Care Spending (2000 *reais* per capita)	99.66 [82.95]	40.705 (14.650) {0.000}	−105.670 (63.533) {0.194}	73.187 (32.932) {0.054}	−32.483 (31.385) {0.366}
Total Spending (2000 *reais* per capita)	963.33 [475.223]	−64.783 (148.472) {0.710}	−545.042 (304.307) {0.212}	240.130 (190.841) {0.276}	−304.913 (139.267) {0.136}
Total Revenue (2000 *reais* per capita)	955.15 [476.41]	−102.113 (162.058) {0.572}	−534.818 (305.174) {0.228}	216.353 (19.943) {0.352}	−318.466 (13.548) {0.092}
Net Interest Payments (2000 *reais* per capita)	4.79 [5.75]	10.935 (35.880) {0.772}	−18.459 (18.161) {0.380}	14.697 (21.320) {0.554}	−3.762 (18.243) {0.848}
N (State-Terms)	—	54	54	—	—
N (States/First-Diffs)	—	27	27	—	—

[a]Standard errors clustered at the state level in parentheses. Standard deviations in brackets. p-values based on Cameron et al. (2008) cluster-robust wild-bootstrap in curly brackets—{}. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}). Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.

APPENDIX E: EFFECTS ON LEVELS OF SPENDING

Table A-III repeats the estimation provided in Table IV using levels of spending (instead of budget shares or logarithms). The unit of measurement is a 2000 *real* per capita. It provides estimates for health care spending, which largely corroborate those in Table IV (although standard errors are somewhat larger). It also provides results on total spending (including transfers and interest payments and debt amortization) and total revenue (including taxes and transfers). Since yearly data on debt positions are not available for the whole period, Table A-III provides estimates on net interest payments to address this issue. The sign-switch pattern is not observed in total spending and revenue, and the estimated coefficients are not significant, although their magnitude is large compared to health spending.

APPENDIX F: CLASSIFICATION OF STATE PUBLIC SPENDING

The FINBRA database collects budget information that state governments report to the federal Ministry of Finances. This is the data source used to construct state-level spending on health care used throughout this paper. The reports break down state spending across multiple categories. Throughout the 1991–2010 period analyzed in this paper, it is only possible to construct at most eight consistently defined categories of spending.[7] Additionally, a residual category name "other spending" also has to be constructed in order to (i) account for years in which there is spending classified simply as "other" and (ii) incorporate categories that appear only in certain years of the sample (e.g., "foreign relations"). Interest payments and debt amortization fall in the "other spending" category, which likely explains its sizable budget share. The categories used here match well with another attempt to build comparable state spending by category.[8] While the results on share of budget spent on health care are provided in Table IV, Table A-IV provides the results for the other categories of spending, using the same pattern of Table IV. The sample averages also provide a breakdown of the average state budget. The largest category, and the only one that tracks the sign-switch pattern of EV's introduction (with an implied average effects that is statistically significant) is "administration and planning." This is essentially the "overhead" of government operations, making an exact interpretation of this effect difficult. States also spend a large share of budget on education. This is mostly at the secondary level (and at the post-secondary in some states), given municipalities are the main providers of primary education in the period, which may explain why the enfranchisement of less educated voters does not lead to more state spending on education. Public safety is also a sizable category, since most police forces in Brazil are funded by state governments. Finally, it must be noted that "social assistance" may be inflated by including government employees' pensions.

APPENDIX G: ADDITIONAL RESULTS ON PRENATAL VISITS

Table A-V presents results on the share of (uneducated) mothers with 0 or 1–6 prenatal visits. It follows the same format as Panel C of Table IV in the text, which presents results for mothers with 7+ prenatal visits. The results indicate that there is no association between EV use and the share of mothers with 0 prenatal visits, with small (and insignificant) estimates. On the other hand, there is a clear sign-switch and a negative impact of EV on the number of mothers with 1–6 visits. The size of this negative impact is comparable to the positive impact on the share of 7+ visits, suggesting that EV increased prenatal visits mainly at the intensive margin.

[7] While in later years in the sample spending is further specified (e.g., primary vs. secondary education), in earlier years only wider categories (e.g., total spending on education) is provided.

[8] Data provided the Instituto de Pesquisa Economica Aplicada on www.ipeadata.gov.br.

TABLE A-IV

EFFECTS BY CATEGORIES OF STATE SPENDING[a]

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98} - \theta^{02})/2$	$(\theta^{98} + \theta^{02})/2$
Sample (Terms):		1994–1998 (Paper–Disc.)	1998–2002 (Disc.–Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Administration and Planning	0.181	−0.072	0.121	−0.097	0.025
	[0.097]	(0.044)	(0.083)	(0.043)	(0.052)
		{0.126}	{0.192}	{0.084}	{0.686}
Social Assistance	0.108	−0.016	−0.053	0.018	−0.035
	[0.054]	(0.017)	(0.024)	(0.015)	(0.015)
		{0.316}	{0.074}	{0.274}	{0.034}
Education	0.176	−0.005	0.016	−0.010	0.005
	[0.037]	(0.014)	(0.015)	(0.011)	(0.009)
		{0.708}	{0.324}	{0.626}	{0.554}
Judiciary	0.063	0.008	−0.024	0.016	−0.008
	[0.023]	(0.020)	(0.011)	(0.012)	(0.010)
		{0.726}	{0.048}	{0.326}	{0.540}
Legislative	0.036	−0.002	0.015	−0.009	0.007
	[0.016]	(0.008)	(0.011)	(0.006)	(0.007)
		{0.878}	{0.318}	{0.266}	{0.524}
Public Safety	0.074	0.002	−0.017	0.009	−0.008
	[0.026]	(0.015)	(0.024)	(0.010)	(0.018)
		{0.922}	{0.564}	{0.348}	{0.752}
Transportation	0.053	0.010	−0.004	0.007	0.003
	[0.049]	(0.017)	(0.036)	(0.017)	(0.023)
		{0.606}	{0.910}	{0.658}	{0.2968}
Other Categories	0.210	0.036	−0.025	0.030	0.005
	[0.086]	(0.031)	(0.036)	(0.031)	(0.013)
		{0.316}	{0.536}	{0.426}	{0.654}
N (State-Terms)	—	54	54	—	—
N (States/First-Diffs)	—	27	27	—	—

[a]Standard errors clustered at the state level in parentheses. Standard deviations in brackets. p-values based on Cameron et al. (2008) cluster-robust wild-bootstrap in curly brackets—{}. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}). Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.

APPENDIX H: ASSESSING THE ROLE OF OUTLIERS I: GRAPHICAL ANALYSIS OF STATE-LEVEL RESULTS

Figures A4 and A5 present a graphical representation of the main results provided in Table IV. Since the estimations include a set of region-time dummies, the graphical analysis is based on residuals of a regression of the (change

TABLE A-V

EFFECT OF PRENATAL VISITS (MOTHERS WITH LESS THAN 8 YEARS OF SCHOOLING)[a]

				Linear Combinations	
Parameter:		θ^{98}	θ^{02}	$(\theta^{98}-\theta^{02})/2$	$(\theta^{98}+\theta^{02})/2$
Sample (Terms):		1994–1998 (Paper–Disc.)	1998–2002 (Disc.–Electr.)		
	Sample Avg.	(1)	(2)	(3)	(4)
Share With 0 Visits	0.099	−0.040	−0.088	0.021	−0.065
	[0.092]	(0.075)	(0.054)	(0.056)	(0.032)
		{0.682}	{0.226}	{0.678}	{0.146}
Share With 1 to 6 Visits	0.537	−0.082	0.111	−0.090	0.017
	[0.123]	(0.054)	(0.035)	(0.031)	(0.034)
		{0.178}	{0.004}	{0.004}	{0.596}
N (State-Terms)	—	54	54	—	—
N (States/First-Diffs)	—	27	27	—	—

[a]Standard errors clustered at the state level in parentheses. Standard deviations in brackets. p-values based on Cameron et al. (2008) cluster-robust wild-bootstrap in curly brackets—{}. The unit of observation is a state-electoral term. Each row reports the estimation of equations (6) and (7) using the specified dependent variable. Each figure in columns (1) and (2) is from a separate regression, providing the coefficient on the share of voters living above the cutoff (S_i) on the 1998 and 2002 first-differences, respectively (θ^{98} and θ^{02}). Columns (3) and (4) report the specified linear combination of these parameters. Region-time effects are included.

in) variables against the region dummies. Hence the slopes in the fitted lines match the point estimates presented in Table IV.

The top left graph of Figure A4 plots the (residual of) share of voters living in municipalities above the cutoff on the x-axis and the *differences* in the (residual of) valid vote ratio observed in 1994 and 1998 (the paper-only and discontinuity elections) on the y-axis. A clear positive relationship is observed, as expected since the variable in the horizontal axis has a *positive* association with EV in this period. The middle graph on the top row plots the relationship between the *exact same variable on the x-axis* and the 1998–2002 *differences* in valid voting (i.e., between the discontinuity and electronic elections). Since in this case the share of voters living in municipalities above the cutoff is *negatively* associated with EV, the relationship changes sign, as expected.

As discussed in the paper, this sign switch in the slopes of the fitted curves is interpreted as the causal effect of EV on valid votes. The top right plot on the figure then pools the data from both previous graphs. It plots the first differences for both the 1994–1998 and 1998–2002 against the changes in the share of electorate using EV. Since change in EV use equals S_i in 1994–1998 period and $1-S_i$ in the 1998–2002 period, the bottom graph is the same of overlapping the "mirror image" of the middle graph (i.e., the same plot with $1-S_i$ instead of S_i in the x-axis) over the left graph. This exercise allows for a graphical evaluation of the similarity of the slopes in the previous graphs. In other words,

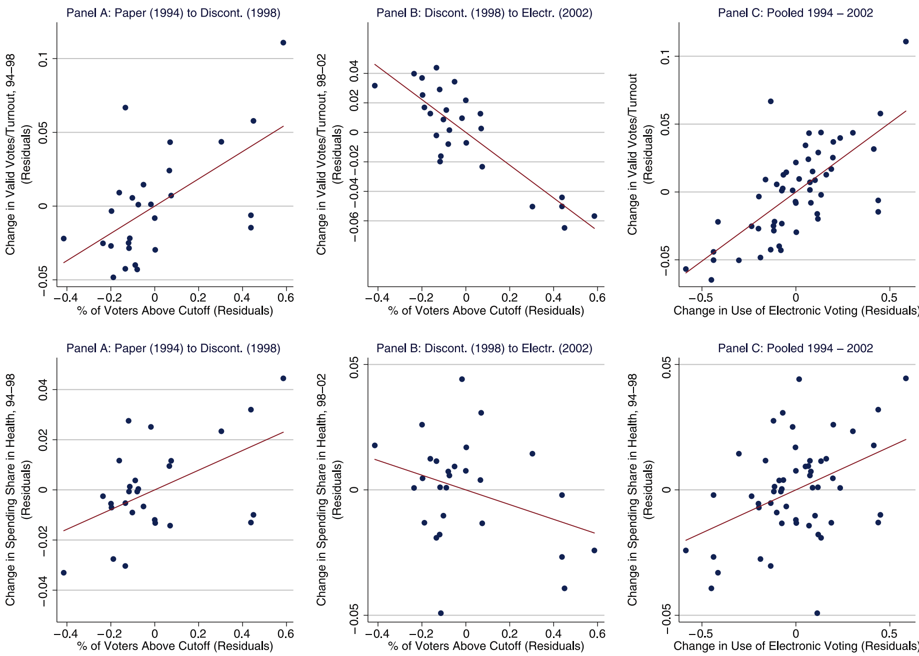

FIGURE A4.—Electronic voting phase-in, valid votes, and health spending.

the left, middle, and right plots are the graphical counterparts of columns (1), (2), and (3) of Table IV. The "tighter" fit on the left graph exemplifies why the pooled estimates (column 3) are usually more precisely estimated than the ones in columns (1) and (2).

The second row of Figure A4 repeats the exercise for the change in health care spending. Figure A5 does the same for prenatal visits and low-birth weight. In all cases, a graphical sign switch and an association between EV and the outcomes can be observed, especially in the plots to the right, which pool the data and allow a more precise estimation.

APPENDIX I: ASSESSING OUTLIERS II: LEAVE-ONE-STATE-OUT ESTIMATES

To assess the role of outliers in driving the results on, Figure A6 depicts the estimated effect of EV on the valid votes to turnout ratio, sequentially dropping each state of the sample. Each marker represents the estimated θ from equation (8), dropping the designated state (the horizontal axis uses states' official acronyms). The bars represent 95% confidence intervals based on Cameron et al.'s (2008) cluster-robust wild-bootstrap. The vertical line represents the effect using the whole sample (reported in Table IV).

Figures A7, A8, and A9 repeat the exercise for health care spending, prenatal visits, and low-birthweight prevalence. The results are, in general, robust to

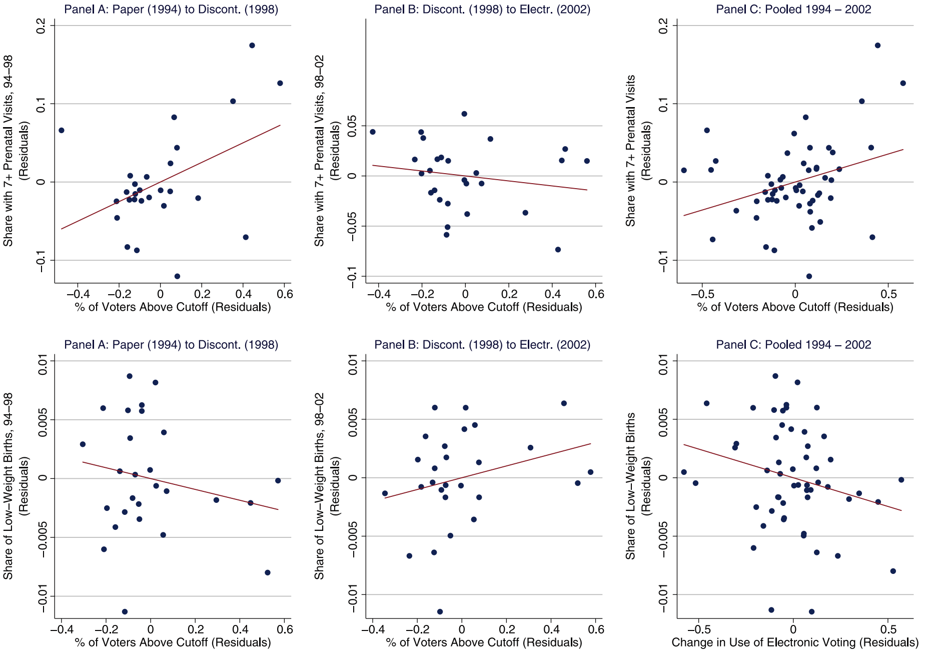

FIGURE A5.—Electronic voting phase-in, prenatal visits, and birthweight.

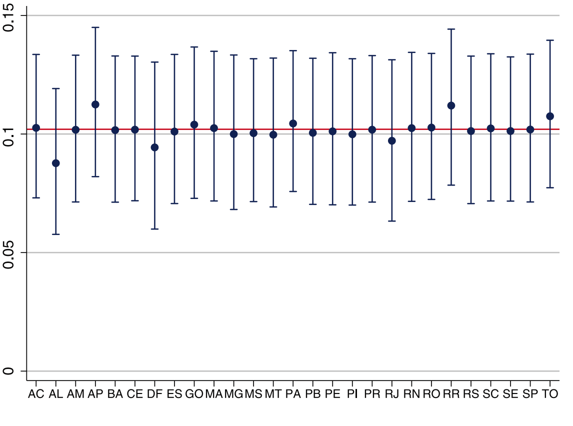

FIGURE A6.—Leave-one-state-out estimates: valid votes/turnout. Each marker represents the estimated effect of electronic voting using the state sample (equation (8)), dropping the specified state from the sample. Confidence intervals based on Cameron et al.'s (2008) cluster-robust wild-bootstrap. Horizontal line represents estimated effect based on full sample.

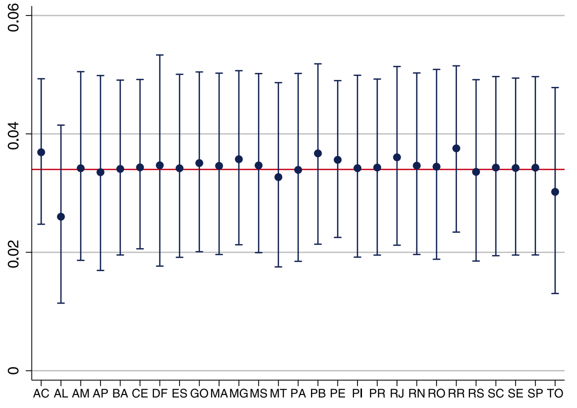

FIGURE A7.—Leave-one-state-out estimates: share of health care spending. Each marker represents the estimated effect of electronic voting using the state sample (equation (8)), dropping the specified state from the sample. Confidence intervals based on Cameron et al.'s (2008) cluster-robust wild-bootstrap. Horizontal line represents estimated effect based on full sample.

dropping individual observations; the only case where there is some sensitivity in the results to some states is the number of prenatal visits: dropping Amapá reduces the estimated effect (although the original estimate is still in its confidence interval), while dropping Roraima increases the effect. Both states are quite similar, being small, relatively unpopulated, and situated in the Amazon ecosystem. Most importantly, both did not follow the discontinuous rule (and hence are dropped from the estimation in column 10 of Table VI): given that

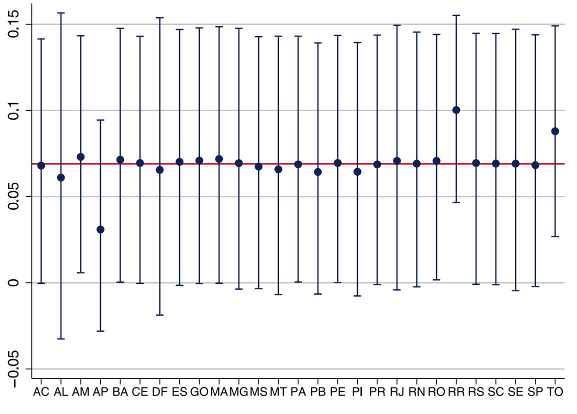

FIGURE A8.—Leave-one-state-out estimates: share with 7+ prenatal visits. Each marker represents the estimated effect of electronic voting using the state sample (equation (8)), dropping the specified state from the sample. Confidence intervals based on Cameron et al.'s (2008) cluster-robust wild-bootstrap. Horizontal line represents estimated effect based on full sample.

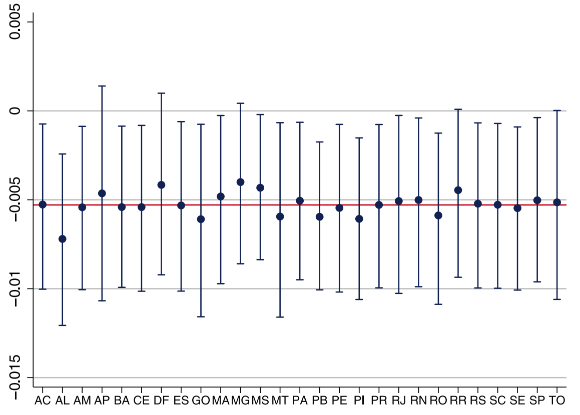

FIGURE A9.—Leave-one-state-out estimates: share with low-weight births. Each marker represents the estimated effect of electronic voting using the state sample (equation (8)), dropping the specified state from the sample. Confidence intervals based on Cameron et al.'s (2008) cluster-robust wild-bootstrap. Horizontal line represents estimated effect based on full sample.

the main text already discusses dropping these states and highlights that the effects on visits are noisier than on other outcomes, the main conclusions in the main text remain.

APPENDIX J: TIMING OF EFFECTS

To maintain the statistical inference as conservative as possible in the presence of a relatively small number of states, the effects reported in the text aggregate yearly data into electoral-term aggregates. In order to assess more precisely the dynamics of the effects, I estimate the following regression using yearly data for state i at year t:

$$y_{it} = \alpha_t + \sum_{k=1996}^{2006} \pi_k S_i 1\{t = k\} + \gamma_t + \beta X_{it} + \varepsilon_{ie}. \tag{A1}$$

The set of π_k hence maps the relationship between voters above the cutoff (S_i) and the outcome of interest y_{it} over time. This equation is a generalization of equation (8) for yearly data: it also includes state and time effects, and uses only region-time effects as controls (X_{it}). The estimation uses the 1995–2006 years like equation (8), with π_{1995} omitted to avoid collinearity (recall the 1995–1998 legislature is elected in 1994, etc.). If equation (9) is aggregated to electoral-term (four-year) level data, the differences in π_k map into θ^{98} and θ^{02}.

The results for share of health care spending are provided in Figure A10. The markers represent the estimated π_k for the designated year; the estimates are rescaled (i.e., a constant is added) in order to make the 1995–1998 π's

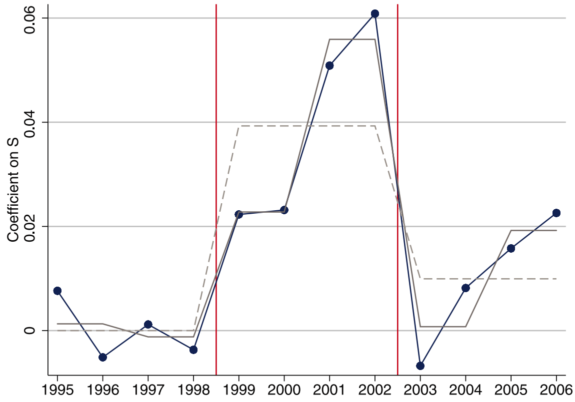

FIGURE A10.—Coefficient of share above cutoff (S), by year: share of health care spending. Each marker represents the coefficient of the share of voters residing above cutoff (S) interacted with the specified year (equation (9)). Solid (dashed) lines plot two-(four-)year averages. See text for more details.

average equal to zero. The solid line without markers repeats the analysis using data at the bi-yearly level (e.g., 1995–1996 averages, 1997–1998 averages, etc.), and the dashed lines the one using four-year averages. Hence, the first jump in the dashed lines represents θ^{98}, while the second one represents θ^{02} (both reported in Table IV). The vertical lines represent the 1998 and 2002 elections.

Figure A10 indicates a stability in the relationship between S_i and health spending, but that jumps immediately after the election, and drops after the 2002 election. The timing is fairly sharp: the switching occurs immediately after elections. This highlights how unlikely it is for a mean reversing shock or omitted variable to drive the results: it has to affect a certain type of states exactly in 1999, and switch its behavior exactly in 2003.

The solid and dashed lines essentially smooth out the effects, and highlight the degree of noise in yearly data that drives jumps across within-election years. Hence, I avoid assigning specific interpretation to, say, the jump between 2000 and 2001.

Figure A11 repeats the exercise for prenatal visits. The jump between the 1995–1998 and 1999–2002 periods is clear and stable (e.g., no pre-EV trends), while the reduction in 2003–2006 is less pronounced. This can be seen in the estimated θ^{98} and θ^{02} and is discussed in the text. Figure A12 repeats the exercise for birthweight. There is a little more yearly noise in these estimates, which gets smoothed out once one focuses on the two-year averages, especially when comparing 1999–2002 and 2003–2006 periods.

Finally, I reiterate the noisy nature of the data: most π_k are not statistically different from the previous and subsequent π_k (but they are when aggregated to four-years and tested in a manner to simulate the results in Table IV). Hence, I avoid assigning an interpretation to jumps in specific years.

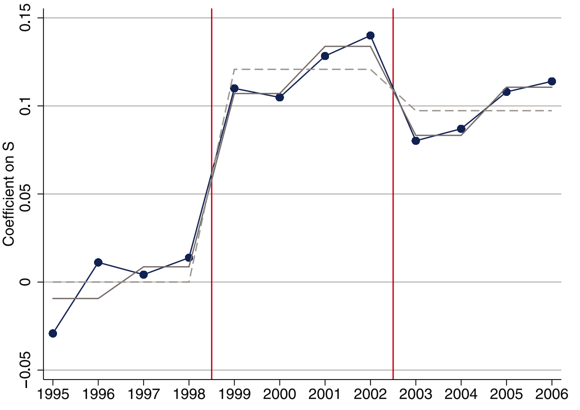

FIGURE A11.—Coefficient of share above cutoff (S), by year: share with 7+ prenatal visits. Each marker represents the coefficient of the share of voters residing above cutoff (S) interacted with the specified year (equation (9)). Solid (dashed) lines plot two-(four-)year averages. See text for more details.

APPENDIX K: INSTRUMENTAL VARIABLE ESTIMATES

To assess the extent to which the results are driven by variation in S_i due to the presence of larger cities or very small municipalities, this section provides results based on the following 2SLS procedure. The second-stage regression is similar to equation (8):

$$y_{ie} = \alpha_e + \theta_{IV} S_i \cdot Term_e^{98} + \gamma_i + \beta X_{ie} + \varepsilon_{ie}, \tag{A2}$$

FIGURE A12.—Coefficient of share above cutoff (S), by year: share with low-weight births. Each marker represents the coefficient of the share of voters residing above cutoff (S) interacted with the specified year (equation (9)). Solid (dashed) lines plot two-(four-)year averages. See text for more details.

with $(S_i \cdot Term_e^{98})$ instrumented according to the following first stage:

$$S_i \cdot Term_e^{98} = \alpha_e^{1st} + \lambda Z_i^j \cdot Term_e^{98} + \gamma_i^{1st} + \beta^{1st} X_{ie} + \varepsilon_{ie}^{1st}, \tag{A3}$$

where $Z_i^j = \frac{\text{Number of voters in municipalities with electorate between 40,500 and 40,500}+j\text{ in state }i}{\text{Number of voters in municipalities with electorate between 40,500}-j\text{ and 40,500}+j\text{ in state }i}$.

Hence, the share of voters living above the cutoff (S_i) is instrumented with the share of voters living above the cutoff in a window (of size j) around the cutoff. Note that if j is large enough, Z_i becomes S_i. Hence, the instrument focuses on the shape of (within-state) distribution of municipality size around the cutoff. Intuitively, it "throws away" the variation related to presence of very large or very small municipalities. For example, if a shock to very large municipalities is driving the results, the 2SLS estimates should differ from those from the main text.

There are two important choices to be made in constructing the Z_i^j. The first is the choice of j. While intuitively very small j are desirable, they will yield less predictive instruments, and may indeed lead to biases due to weak instruments. I address this issue by estimating effects with multiple sizes of j, from 5000 to 500,000. The second issue is how to assign an instrument to the states that did not follow the discontinuity, which are assigned $S_i = 1$ in the text.[9] One possibility, and that keeps the IV estimate closer to the analysis in the text, is to assign them a $Z_i^j = 1$. The estimates provided in Table A-VI are based on this choice.

Each row of Table A-VI provides the estimated λ and θ_{IV} for the four main outcomes (those reported on Table VI) and the designated j. The results are very similar to those in Table IV, regardless of j's size, except that the effect on prenatal visits is usually larger. The first-stage effect is strong for all windows, although it should be highlighted that in small-j cases, that is likely driven by assigning $Z_i^j = 1$ for the four states not following the discontinuity. This is an important caveat in interpreting this result: the small j are likely driven by the variation from those states.

Another choice for dealing with states that did not follow the discontinuity is to drop them from the estimation. Table A-VII repeats the analysis from Table A-VI, doing so. However, in the cases of smaller j, the first stage is weak, to an extent that the weak instruments became an issue interpreting the results. Using the "rule of thumb" of an excluded instrument F-stat larger than ten to rule out sizable weak instrument bias, only the cases with j of 40,000 and higher are valid. In those, the estimates from the main text are mostly corroborated (if anything, the effects on visits and birthweight are larger).[10] More importantly, the cases where j is small (below 30,000) are clearly uninformative about the results.

[9]Note that the main text also uses dropping these states as a robustness check.

[10]Note that, in the case of single instrument, the excluded instrument F-stat is the square of its t-stat, so a ratio between reported coefficient and standard error of below 3.16 would fail the rule of thumb.

TABLE A-VI

IV ESTIMATES OF THE EFFECT OF ELECTRONIC VOTING[a]

Outcome:				Birth Outcomes (Mothers w/o Primary Schooling)	
Window	First-Stage	Valid Votes/ Turnout	Share of Health Care Spending	Share w. 7+ Prenatal Visits	Share w. Low-Weight Births (×100)
500,000	0.822	0.100	0.041	0.089	−0.640
	(0.078)	(0.019)	(0.007)	(0.047)	(0.265)
	{0.000}	{0.026}	{0.000}	{0.053}	{0.016}
200,000	0.772	0.106	0.039	0.086	−0.696
	(0.061)	(0.018)	(0.007)	(0.042)	(0.291)
	{0.000}	{0.004}	{0.000}	{0.032}	{0.010}
100,000	0.740	0.105	0.038	0.084	−0.575
	(0.050)	(0.018)	(0.007)	(0.042)	(0.268)
	{0.000}	{0.004}	{0.000}	{0.050}	{0.038}
50,000	0.680	0.103	0.032	0.087	−0.612
	(0.041)	(0.018)	(0.010)	(0.043)	(0.266)
	{0.000}	{0.006}	{0.002}	{0.056}	{0.018}
40,000	0.673	0.104	0.033	0.088	−0.602
	(0.040)	(0.018)	(0.009)	(0.043)	(0.258)
	{0.000}	{0.006}	{0.000}	{0.042}	{0.018}
30,000	0.679	0.103	0.033	0.095	−0.615
	(0.048)	(0.019)	(0.011)	(0.043)	(0.264)
	{0.000}	{0.012}	{0.019}	{0.008}	{0.034}
20,000	0.677	0.101	0.032	0.105	−0.397
	(0.079)	(0.020)	(0.012)	(0.046)	(0.279)
	{0.000}	{0.020}	{0.076}	{0.020}	{0.168}
10,000	0.658	0.102	0.029	0.107	−0.332
	(0.105)	(0.020)	(0.014)	(0.046)	(0.268)
	{0.000}	{0.014}	{0.190}	{0.080}	{0.268}
5000	0.559	0.111	0.026	0.111	−0.631
	(0.083)	(0.019)	(0.014)	(0.043)	(0.393)
	{0.000}	{0.006}	{0.212}	{0.012}	{0.112}
N	81	81	81	81	81

[a]Standard errors clustered at the state level in parentheses. p-values based on Cameron et al. (2008) cluster-robust wild-bootstrap in curly brackets—{}. The unit of observation is a state-electoral term. Each column reports the estimation of equation (10) using the specified dependent variable. Each figure is from a separate 2SLS regression using a sample covering three electoral terms, with the instrument with the window (j) specified in the row. See text for further information on the construction of instruments.

A third possibility would be to include the four states not following the discontinuity with their actual Z_i^j (not setting it to 1 or dropping them). In this case, the instrument is always weak, since most of these states have small Z_i^j. Hence, I avoid estimating the second stage. Finally, I highlight that the main

TABLE A-VII

IV ESTIMATES OF THE EFFECTS OF ELECTRONIC VOTING (RESTRICTED SAMPLE)[a]

				Birth Outcomes (Mothers w/o Primary Schooling)	
Outcome: Window	First-Stage	Valid Votes/ Turnout	Share of Health Care Spending	Share w. 7+ Prenatal Visits	Share w. Low-Weight Births (×100)
500,000	0.715	0.134	0.055	0.103	−0.933
	(0.164)	(0.020)	(0.024)	(0.050)	(0.603)
	{0.000}	{0.000}	{0.032}	{0.000}	{0.148}
200,000	0.678	0.141	0.043	0.085	−1.163
	(0.111)	(0.016)	(0.018)	(0.033)	(0.627)
	{0.000}	{0.000}	{0.026}	{0.000}	{0.002}
100,000	0.709	0.139	0.041	0.083	−0.734
	(0.085)	(0.015)	(0.019)	(0.028)	(0.564)
	{0.000}	{0.000}	{0.000}	{0.000}	{0.164}
50,000	0.670	0.138	0.018	0.102	−0.984
	(0.048)	(0.015)	(0.015)	(0.028)	(0.606)
	{0.000}	{0.000}	{0.386}	{0.000}	{0.002}
40,000	0.655	0.140	0.022	0.106	−0.932
	(0.040)	(0.014)	(0.014)	(0.031)	(0.566)
	{0.000}	{0.000}	{0.234}	{0.000}	{0.002}
30,000	0.525	0.142	0.018	0.145	−1.131
	(0.177)	(0.019)	(0.026)	(0.069)	(0.672)
	{0.000}	{0.000}	{0.608}	{0.000}	{0.002}
20,000	0.363	0.143	0.011	0.208	−0.238
	(0.278)	(0.031)	(0.048)	(0.166)	(0.891)
	{0.424}	{0.346}	{0.878}	{0.000}	{0.854}
10,000	0.311	0.145	−0.002	0.240	0.082
	(0.219)	(0.029)	(0.053)	(0.157)	(1.101)
	{0.242}	{0.176}	{0.898}	{0.001}	{0.944}
5000	0.277	0.161	−0.005	0.206	−1.097
	(0.148)	(0.030)	(0.046)	(0.114)	(1.025)
	{0.178}	{0.094}	{0.896}	{0.000}	{0.296}
N	69	69	69	69	69

[a]Standard errors clustered at the state level in parentheses. p-values based on Cameron et al. (2008) cluster-robust wild-bootstrap in curly brackets—{}. The unit of observation is a state-electoral term. Each column reports the estimation of equation (10) using the specified dependent variable. Each figure is from a separate 2SLS regression using a sample covering three electoral terms, with the instrument with the window (j) specified in the row. See text for further information on the construction of instruments. The sample excludes states that did not follow the discontinuous assignment rule.

issue here is not the presence of four states not following the rule, but the fact that when j is "too small," it becomes unpredictive of S_i. More intuitively, the correlation between the distribution of municipalities above/below the cutoff in a "small" window around it is not a strong enough predictor of the distribution above/below the cutoff in general, and hence the roll-out of EV, to allow its use as an instrument.

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