Appendix: Spoiled Ballots as a Response to Limitations on Choice and Influence

A Descriptive Statistics

Table A.1: Descriptive Statistic for the Percentage of Invalid Votes

		N	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
2001	Full Sample	35,799	0.000	2.174	3.734	4.264	5.568	80.851
	$< 3{,}500$	33,279	0.000	2.041	3.579	4.066	5.405	80.851
	$\geq 3{,}500$	2,520	1.513	4.140	5.370	6.888	7.038	51.029
2008	Full Sample	34,914	0.000	1.907	3.279	3.706	4.863	41.639
	$< 3{,}500$	32,527	0.000	1.818	3.216	3.583	4.831	40.769
	$\geq 3{,}500$	2,387	0.000	2.996	3.868	5.382	5.213	41.639
2014	Full Sample	35,845	0.000	2.020	3.529	5.230	5.570	66.391
	$< 1{,}000$	26,307	0.000	1.550	2.957	3.448	4.721	64.000
	$\geq 1{,}000$	9,538	0.000	3.597	5.170	10.145	16.109	66.391
2020	Full Sample	34,358	0.000	1.504	2.817	4.259	4.710	73.239
	$< 1{,}000$	24,632	0.000	1.064	2.316	2.798	3.865	73.239
	$\geq 1{,}000$	9,726	0.529	2.775	4.358	7.960	12.181	70.083

B Complete Results

Table B.1: RD Effect of Fortified CLPR on the Percentage of Invalid Votes, 2001-2020

Election	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2001	3.408*	[1.036, 5.780]	0.00002	0.116	802	484
2008	2.851*	[0.521, 5.180]	0.00169	0.125	1,369	612
2014	9.785*	[8.477, 11.093]	1.01e-82	0.062	6,009	2,745
2020	7.556*	[6.477, 8.634]	7.61e-73	0.045	3,900	2,294

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the municipal elections. Estimate is the average treatment effect at cutoff with local linear polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

C Validity Checks

Table C.1: RD Effect of Fortified PR on Percentage of Invalid Votes in National Assembly Elections, 2002-2017

National Assembly Election (Municipal Election)	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2002 (2001)	0.093	[-0.138, 0.323]	0.29891	0.167	1,315	658
2012 (2008)	0.119	[-0.131, 0.368]	0.22114	0.120	1,267	589
2017 (2014)	-0.017	[-0.268, 0.233]	0.86062	0.037	2,710	1,798
2017 (2020)	0.035	[-0.193, 0.263]	0.69106	0.036	2,835	1,914

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the lower chamber election. Estimate is the average treatment effect at cutoff with local linear regression with triangular kernel and MSE-optimal bandwidth. Column 1 shows the lower chamber election year and the municipal election year in parentheses. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth.

Table C.2: RD Effect of Fortified PR on Percentage of Invalid Votes in Presidential Elections, 2002-2017

Presidential Election (Municipal Election)	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2002 (2001)	0.077	[-0.089, 0.244]	0.23093	0.096	624	417
2012 (2008)	-0.155	[-0.359, 0.049]	0.05034	0.078	687	392
2017 (2014)	0.126	[-0.049, 0.302]	0.06424	0.042	3,222	1,985
2017 (2020)	-0.123	[-0.313, 0.066]	0.09356	0.029	2,155	1,622

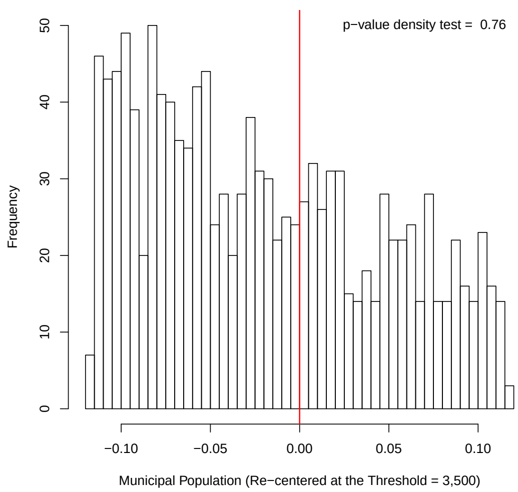
Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the presidential election. Estimate is the average treatment effect at cutoff with local linear regression with triangular kernel and MSE-optimal bandwidth. Column 1 shows the presidential election year and the municipal election year in parentheses. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth.

Table C.3: RD Effect of Fortified PR on Unemployment Rate, 2002-2017

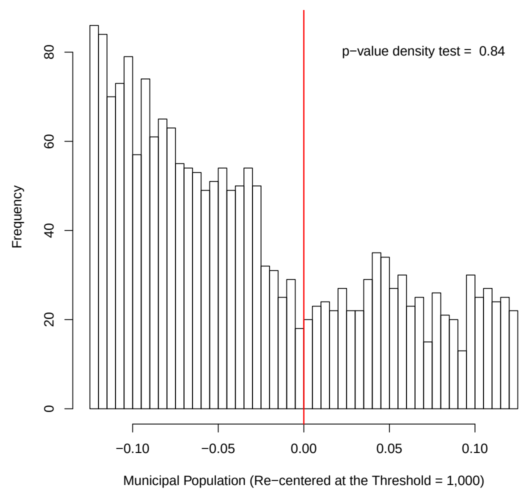
Unemployment Rate (Municipal Election)	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2006 (2001)	0.131	[-1.146, 1.409]	0.79099	0.169	1,330	664
2008 (2008)	0.045	[-1.223, 1.313]	0.92669	0.161	2,146	744
2015 (2014)	0.182	[-0.496, 0.859]	0.49006	0.047	3,858	2,178
2015 (2020)	-0.240	[-0.938, 0.457]	0.37471	0.038	2,991	1,978

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the municipal unemployment rate. Estimate is the average treatment effect at cutoff with local linear regression with triangular kernel and MSE-optimal bandwidth. Column 1 shows the year in which the unemployment rate was measured and the municipal election year in parentheses. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth.

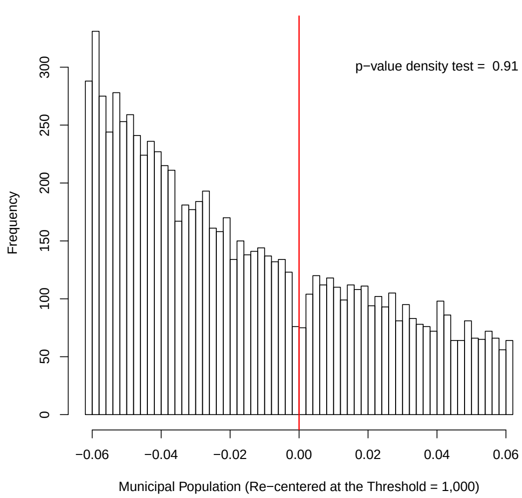
Figure C.1: Histogram of Population (Re-centered at the threshold)—French Municipal Elections, 2001-2020



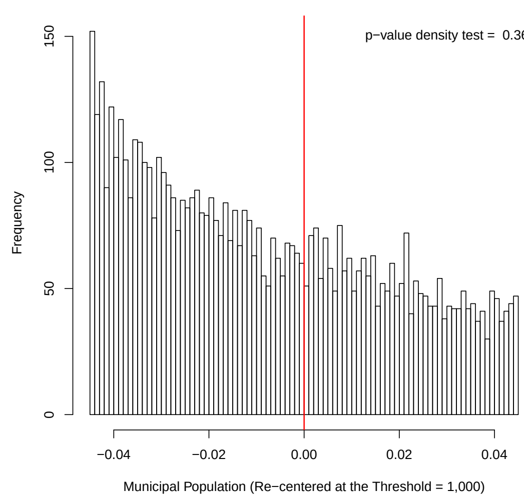
(a) 2001



(b) 2008



(c) 2014

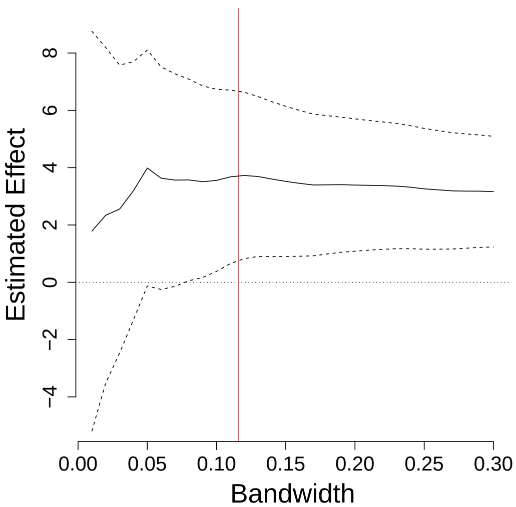


(d) 2020

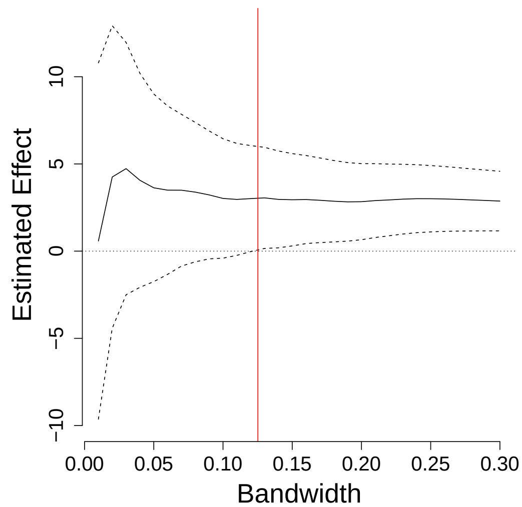
Note: Red vertical line represents the threshold. $p-$value for manipulation test using the local polynomial density estimator proposed by Cattaneo et al. (2019).

D Sensitivity Analysis

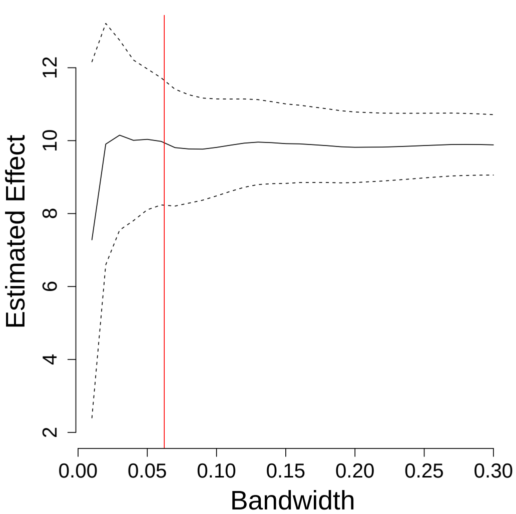
Figure D.1: Bandwidth Sensitivity—French Municipal Elections, 2001-2020



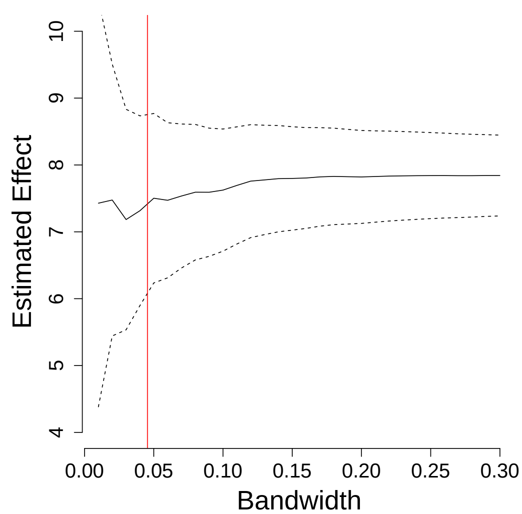
(a) 2001



(b) 2008



(c) 2014



(d) 2020

Note: Red vertical line represents the MSE-optimal bandwidth. Dashed horizontal lines represent 0.99% robust confidence intervals.

E Alternative Specifications

Table E.1: RD Effect of Fortified CLPR on the Percentage Invalid Votes, 2001-2020—Second-Degree Polynomial

Election	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2001	3.547*	[0.922, 6.172]	0.00050	0.189	1,591	726
2008	2.984†	[-0.018, 5.987]	0.01046	0.158	2,069	737
2014	10.029*	[8.060, 11.998]	2.54e-39	0.061	5,875	2,706
2020	7.472*	[6.221, 8.723]	2.04e-53	0.066	6,930	2,997

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the municipal elections. Estimate is the average treatment effect at cutoff with local second-order polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

Table E.2: RD Effect of Fortified CLPR on the Percentage Invalid Votes, 2001-2020—Third-Degree Polynomial

Election	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2001	3.603*	[0.794, 6.413]	0.00095	0.271	3,097	921
2008	3.043†	[-0.380, 6.466]	0.02203	0.203	3,518	876
2014	10.296*	[7.680, 12.911]	3.74e-24	0.062	6,029	2,751
2020	7.357*	[5.719, 8.994]	5.63e-31	0.064	6,615	2,927

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the municipal elections. Estimate is the average treatment effect at cutoff with local third-order polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

Table E.3: RD Effect of Fortified CLPR on the Percentage Invalid Votes, 2001-2020—Fourth-Degree Polynomial

Election	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2001	3.763^{*}	[0.486, 7.041]	0.00310	0.291	3,647	969
2008	$3.637^{\dagger}$	[-0.569, 7.843]	0.02591	0.210	3,832	900
2014	10.235^{*}	[7.286, 13.184]	3.92e-19	0.076	8,380	3,185
2020	7.303^{*}	[5.390, 9.216]	7.94e-23	0.072	7,829	3,163

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the municipal elections. Estimate is the average treatment effect at cutoff with local fourth-order polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

F Blank and Null Votes RDD

Table F.1: RD Effect of Fortified CLPR on the Percentage Blank and Null Votes, 2020

	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
% of Blank	1.618^{*}	[1.251, 1.985]	6.95e-30	0.038	3072	1998
% of Null	6.007^{*}	[5.226, 6.788]	2.43e-87	0.065	6762	2954

Note: Running variable is the municipal population re-centered at the threshold. Outcomes are the percentage of blank and null votes in the 2020 municipal elections. Estimate is the average treatment effect at cutoff with local linear polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

G Municipalities that Changed their Electoral System because of Population Increase or Decrease

In this Appendix, we analyze municipalities that changed their electoral system because of an increase or a decrease in their population between elections. To do so, we subset our data to two sets of municipalities: those where the electoral system changed between the 2001 and 2008 elections and those where the electoral system changed between the 2014 and 2020 elections.

We found that 92 municipalities had their electoral system modified between 2001 and 2008 elections due to their population. All these municipalities moved from MNTV to Fortified CLPR system. In the period between 2014 and 2020, we encountered 500 municipalities where the electoral system changed. Of these 500 municipalities, 97 moved from the Fortified CLPR to MNTV, whereas 403 moved from the MNTV to Fortified CLPR.

Using these data, we ran two models in which we regressed the *Invalid Votes* on an indicator for *Fortified CLPR*. Given that population defined the assignment of the electoral system and it is likely related to BNS votes, we controlled for the (logged) population in the models. Lastly, we included municipality and year fixed effects. Note that the year fixed effects are perfectly collinear with *Fortified CLPR* in the sample for 2001-08. Thus, we excluded this fixed effect from this model.

The results in Table G.1 show that the coefficient for *Fortified CLPR* is positive in both samples, indicating that municipalities had a larger percentage of invalid votes in elections where *Fortified CLPR* was used. The coefficient for the 2001-2008 sample is only statistically different from zero at $p < 0.1$. Nevertheless, it is worth noting the small sample size in this sample.

Table G.1: Effect of Fortified CLPR on % of Invalid Votes

	Dependent variable:	
	% of Invalid Votes	
	2001-2008	2014-2020
	(1)	(2)
Fortified CLPR	3.714^{*}	8.027^{***}
	(2.059)	(0.679)
Control for Population	Yes	Yes
Municipality FE	Yes	Yes
Year FE	–	Yes
Observations	184	1,000
R^2	0.543	0.633

Note: Table's entries are coefficients from linear regression models (OLS). Clustered robust standard errors by municipality in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

H Fuzzy RDD

Table H.1: Fuzzy RD Effect of Fortified CLPR on the Percentage Invalid Votes, 2001-2020

Election	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2001	3.488*	[1.065, 5.911]	0.00020	0.116	802	484
2008	2.851*	[0.521, 5.180]	0.00161	0.125	1,369	612
2014	9.901*	[8.583, 11.220]	2.45e-83	0.062	6,009	2,745
2020	7.556*	[6.477, 8.634]	7.61e-73	0.045	3,900	2,294

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the municipal elections. Estimate is the average treatment effect at cutoff with local linear polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

I RDD effect of Fortified CLPR on Turnout

Table I.1: RD Effect of Fortified CLPR on Turnout, 2001-2020

Election	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2001	0.010	[-0.016, 0.035]	0.33292	0.140	1,028	569
2008	0.011	[-0.019, 0.042]	0.33907	0.101	988	496
2014	-0.013†	[-0.028, 0.001]	0.02010	0.047	3,964	2,241
2020	-0.017	[-0.042, 0.008]	0.07521	0.031	2,331	1,691

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the turnout in the municipal elections. Estimate is the average treatment effect at cutoff with local linear polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, *p*-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

J Other Treatments

One final challenge to our approach is that the 3,500 and 1,000 thresholds are used to change other policies in France. Eggers et al. (2018) show that the 3,500 threshold is used to change eight other policies, and the 1,000 threshold is employed to change two other policies (see Table J.1). Some of these treatments *might* affect the percentage of BNS votes, we do not see clear causal paths between some of them and our outcome of interest. Specifically, one might argue that some voters understand the introduction of gender parity as a bad policy, leading them to cast more BNS votes. Similarly, perhaps voters in municipalities with more politicians (large council and more deputy mayors) are more dissatisfied with politics than those where there are fewer politicians. Lastly, one might be able to make the case that voters are more inclined to protest in municipalities where politicians receive higher compensations (salary and amount of paid leave). It is difficult to make the case that outsourcing scrutiny and the need for committees to be proportional might spawn protest. Below we explain why we believe that these treatments do not cause the increase in invalid votes presented in the main paper.

Table J.1: Other Treatments at the 3,500 and 1,000 Thresholds

	Pre-Reform (3,500 inhabitants)	Post-Reform (1,000 inhabitants)
Electoral System (MNTV vs. Fortified PR)	X	X
Gender parity	X	X
Salary of mayor and deputy mayors	X	X
Council Size	X	
Max. number of deputy mayors	X	
Outsourcing scrutiny	X	
Council must debate budget prior to vote	X	
Committees follow PR principle	X	
Amount of paid leave for council work	X	

Source: Compiled by Eggers et al. (2018) using the French Legal Code.

We start our analysis evaluating whether there is a discontinuity, in the percentage of invalid, in the elections pre-reform at the 1,000 threshold and post-reform at the 3,500 threshold. All the estimates in Table J.2 are statistically insignificant at $p < 0.01$. However, we do notice a statistically significant discontinuity at $p < 0.05$ at the 1,000 threshold in 2008. Before the 2014 electoral reform, the 1,000 threshold was used to change the maximum salary of mayors and deputy mayors.[1] Perhaps, this discontinuity can be attributed to this policy. The fact that the estimate in 2001 is statistically insignificant, however, indicates that if the change in salary of mayors and deputy mayors causes invalid vote, this relationship is not consistent over time. Moreover, the RD effects in the body of the paper at the 1,000 threshold are much larger than the one estimated at the 1,000 threshold in 2008 (9.785% in 2014 and 7.556% in 2020). Given that the estimated effect in 2008 is lower than 1%, the probability that the change in the salary of mayors and deputy mayors is the main reason for our observed effect is likely very low.

Table J.2: RD Effect of Fortified CLPR on the Percentage Invalid Votes, 2001-2020—Using 1,000 Threshold for 2001 and 2008 and the 3,500 Threshold for 2014 and 2020 Elections

Election	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2001	0.271	[-0.285, 0.827]	0.20921	0.064	5,470	2,423
2008	$0.510^{\dagger}$	[-0.007, 1.028]	0.01107	0.028	2,395	1,573
2014	-0.081	[-2.781, 2.619]	0.93842	0.103	855	581
2020	0.917	[-1.444, 3.278]	0.31706	0.101	905	586

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the municipal elections. Estimate is the average treatment effect at cutoff with local linear polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

Table J.2 also shows that there is no discontinuity at the 3,500 threshold in either 2014 or 2020. Only the assignments of electoral system and gender parity were modified between 2008 and 2014. The lack of statistical significance for the estimates at the 3,500 in 2014 and 2020 indicate that the remaining treatments are unrelated to the percentage of invalid votes. In other words, after the change in the assignment of fortified CLPR and gender parity to 1,000 inhabitants, there is no evidence of a discontinuity in invalid votes in the municipalities that are close to the 3,500 cutpoint, even though this cutpoint continued to be used to change the other seven policies.

[1] *Code général des collectivités territoriales - Article L2123-23*. The maximum salary of mayors and deputy mayors is calculating by multiplying the public service remuneration in the election-year by an index. The threshold is used to change this index. See `https://www.collectivites-locales.gouv.fr/regime-indemnitaire-des-elus` for more details. Access on March, 25, 2020.

In addition to the evidence presented in Table J.2, we leverage the 2014 reform to estimate difference-in-discontinuity regression model (Grembi et al., 2016). This design allows us to isolate the effect of fortified CLPR from the other treatments that also occur at the 1,000 and 3,500 thresholds. Table J.3 has the estimated effect of fortified CLPR. We see that the difference-in-discontinuity effect of fortified CLPR is positive in both thresholds. That is, the use of a fortified CLPR system increased the percentage of invalid votes when it was enforced at the 3,500 threshold and also when it started to be enforced at the 1,000 threshold.

Table J.3: Difference in Discontinuity Effect of Fortified CLPR on the Percentage Invalid Votes—French Municipal Elections 2008 and 2014

Threshold	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
1,000	9.625*	[8.400, 10.848]	1.41e-90	0.048	9,956	2,255
3,500	3.058*	[0.448, 5.667]	0.00252	0.130	966	2,420

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the municipal elections. Estimate is the average treatment effect at cutoff using a linear estimator (OLS). Columns 3-7 are: 99% clustered-robust confidence intervals by municipality, p-value, main optimal bandwidth, control and treatment observations within the bandwidth. * $p < 0.01$; † $p < 0.05$.

Considering that gender parity is only enforced in municipalities that also use a fortified CLPR system, the previous analysis cannot rule out the possibility that gender parity causes invalid votes. One way to check for this possibility would be to use data for elections before the establishment of gender parity in 1999. However, data for municipal elections before the use of gender parity are not available. Even with this limitation, there are some reasons to believe that gender parity is not causing the increase in invalid votes.

According to Lambert (2001), in 1996, three years before the approval of the rule, 74% of the electorate favored the creation of gender quotas. The same author reports that opinion polls fielded in 2001 revealed that two voters out of three were receptive to the idea of having a female mayor in their town (Lambert, 2001). Additionally, except for municipal elections in cities that use MNTV, gender parity is enforced in all elections since 1999 (Bird, 2004). As a result, if voters are more likely to cast BNS ballots because of gender parity, we should observe an increase in invalid votes in all elections in France after the implementation of such a rule. The data in Table J.4 suggest that the percentage of invalid votes did not increase after the enforcement of gender parity. In fact, there is a decrease in the percentage of invalid votes in legislative and European elections in the first contest after the establishment of the gender parity. Although there is an increase in the case of cantonal/departmental elections, this figure starts to decrease in the second election.

Table J.4: Invalid Votes in European, Legislative, and Cantonal/Departmental Elections in France, 1984-2019

European		Legislative		Cantonal-Departmental	
Year	%	Year	%	Year	%
2019	4.53	2017	2.22	2015	4.89
2014	4.01	2012	1.58	2011	2.95
2009	4.30	2007	3.42	2008	2.60
2004	3.30	2002	4.35	2004	4.11
1999	5.93	1997	4.90	2001	4.79
1994	5.35	1993	5.30	1998	4.56
1989	2.80	1988	2.00	1994	2.73
1984	3.50	1986	4.30	1992	4.88

Source: Ministry of the Interior and IDEA.

K BNS votes in the Second Round

In this Appendix, we show RD estimates for BNS votes where second rounds were held. At the time we collected these data, second round results were only available for the 2008 and 2014 election. The results in Table K.1 indicate null estimates for the effect of Fortified CLPR on BNS votes in second rounds. That is, we do not detect a discontinuity when using data for the second rounds.

Although we can only speculate about the reasons behind this result, we can imagine that voters treat second rounds differently than first rounds. There are reasons to think that BNS ballots may increase in the second round of an MNTV election and decrease in the second round of a Fortified-CLPR election. First, in MNTV systems, the pool of candidates is likely different in many municipalities. Because candidates who received the absolute majority of votes in the first round are elected, voters who turn out for the second round have a smaller set of options from which to choose. Frustrated by this, they may cast more BNS ballots. Second, in Fortified CLPR systems, voters are aware that because of the seat bonus (fortification) a plurality of the vote is enough to give the majority of the seats in the legislature to a list. Voters who decide to turn out may want to avoid victory by their less preferred alternative. As a result, they may be more inclined to cast a valid ballot in the second round than they were in the first. Again this is speculating on our part, and it would require a completely different research design to address.

Table K.1: RD Effect of Fortified CLPR on Invalid Votes-Second Round, 2008-2014

Election	Estimate	99% CI	p-value	h	n_{co}	n_{tr}
2008	-1.391	$[-4.318, 1.535]$	0.22075	0.120	307	131
2014	0.871	$[-2.690, 4.431]$	0.52884	0.037	368	73

Note: Running variable is the municipal population re-centered at the threshold. Outcome is the percentage of invalid votes in the municipal elections. Estimate is the average treatment effect at cutoff with local linear polynomial regression with triangular kernel and MSE-optimal bandwidth. Columns 3-7 are: 99% robust confidence intervals, p-value, main optimal bandwidth, control and treatment observations within the bandwidth.

L Simulations: Proportionality in MNTV and Fortified CLPR Systems

In the body of the paper, we provide anecdotal evidence that, relative to MNTV, Fortified CLPR produces disproportional results because of the seat bonus for the top vote-getting party. Ideally, we would be able to calculate the Gallagher Index of disproportionality for each municipality and compare the results produced by the electoral systems (Gallagher, 1991). Unfortunately, we are unable to do this due to data limitations. French election authorities do not report on which list or to which parties candidates in the MNTV election belong. As a result, calculating something like the Gallagher index is not tractable. Calculating the measure at the individual candidate level would not be very revealing. Winning candidates could have vote shares ranging from 50% + 1 to 100%. However, their individual seat shares would be constant at 1/M. The index that would result would obscure vast differences.

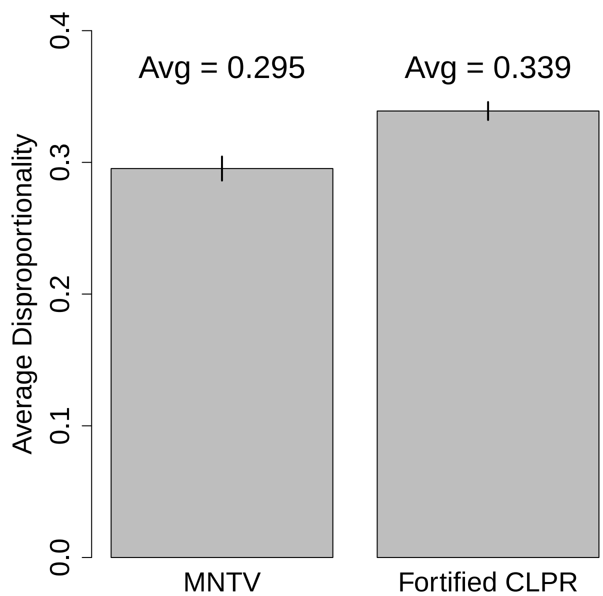
Thus, in order to provide evidence that Fortified CLPR generates a high level of disproportionality, we chose to rely on simulations. We generate 1,000 votes with preferences distributed normally ($\mu = 0$, $sd = 1$) in a municipality with M = 15 — the magnitude for municipalities with 1,000 voters. Then, we randomly sample N parties from a uniform distribution ($a = -2$ and $b = -2$), where $N = \{5, 6, 7, 8, 9, 10\}$. For MNTV systems, we let parties field C candidates, where C is a number between 5 (inclusive) and 15 (inclusive) — all parties field the same number of candidates in a given iteration. Note that the specific values of N and C are randomly defined in each iteration (all values have the same probability of occurrence). Candidates are sampled from uniform distributions where a and b are the mid-points between parties' positions. Voters cast a ballot for the party in Fortified CLPR (candidate in MNTV) who is closest to them.[2] Votes for candidates in MNTV are pooled to the party level to calculate each party's seat share. Lastly, we compute the Gallagher index of disproportionality, which varies from 0 (perfect proportionality) to 1 (perfect disproportionality). We repeat this process 1,000 times, using vote share in the first

[2]Distances are calculated using $\sqrt{(V_i - P_j)^2}$, where V is the voter's position and P is the party/candidate's position.

round and the final seat share.

Figure L.1 shows the results from our simulations. Both systems produce high levels of disproportionality. However, on average, Fortified CLPR leads to a higher level of disproportionality between vote and seat distributions than MNTV does. The difference is, on average, equal to 0.043 and it is statistically different from zero ($p < 2.2e-16$).

Figure L.1: Simulation Results— Disproportionality in MNTV and Fortified CLPR



Note: Vertical bars represent 99% confidence intervals.

References

Bird, K. (2004). The Effects of Gender Parity in Elections: The French Case. In J. Gaffney (Ed.), *The French Presidential and Legislative Elections of 2002*. Routledge.

Eggers, A. C., R. Freier, V. Grembi, and T. Nannicini (2018). Regression Discontinuity Designs Based on Population Thresholds: Pitfalls and Solutions: RDD Based on Population Thresholds. *American Journal of Political Science 62*(1), 210–229.

Gallagher, M. (1991, March). Proportionality, disproportionality and electoral systems. *Electoral Studies 10*(1), 33–51.

Grembi, V., T. Nannicini, and U. Troiano (2016). Do Fiscal Rules Matter? *American Economic Journal: Applied Economics 8*(3), 1–30.

Lambert, C. (2001). French Women in Politics: The Long Road to Parity. https://www.brookings.edu/articles/french-women-in-politics-the-long-road-to-parity/.