Online Appendix

Can Political Alignment Reduce Crime?
Evidence from Chile

Carla Alberti[1] Diego Díaz–Rioseco[2] Giancarlo Visconti[3]

Table of contents

[1] Assistant Professor, Political Science Department, Universidad Católica de Chile.
[2] Assistant Professor, School of Government, Universidad Católica de Chile.
[3] Assistant Professor, Department of Political Science, Penn State University.

Appendix A: Continuity Assumptions

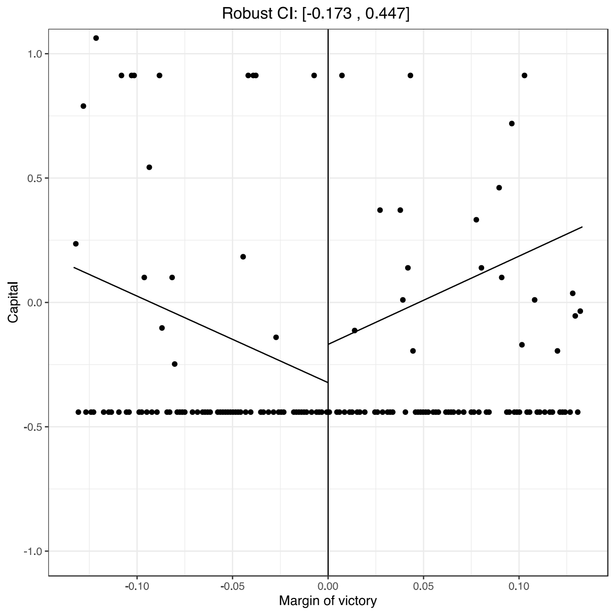
Our design assumes that there are no abrupt changes at the cutoff (except for the treatment). We use the *rdrobust* package to check if political alignment affects different placebo and pretreatment covariates: a binary indicator of being the capital city of a province (capital), the code of the region (region), the code of the province (province), the area of the municipality in square kilometers (area), the log of the population using the 2002 census (population), the proportion of urban population using the 2002 census (urban), literacy using data from the 2003 UNDP (literacy), income per capita from the 2003 UNDP (income), the human development index using data from the 2003 UNDP (HDI), the human development ranking using data from the 2003 UNDP (HDR), the log of the votes cast in 1999 (the election prior to the beginning of our dataset) (votes), the vote share obtained by the right-wing candidate in 1999 (right-wing), the vote share obtained by the left-wing candidate in 1999 (left-wing), the percentage of invalid votes in 1999 (invalid), and the percentage of blank votes in 1999 (blank).

We also use data from the National Socioeconomic Survey (CASEN), which is a household survey conducted by the Ministry of Social Development (MIDESO) every two or three years. This survey represents the main source of socioeconomic data in Chile. In particular, we use the 2003 CASEN survey, which includes information for 68.155 households. We construct three variables from this dataset: unemployment, which is measured as the unemployment rate, where the number of people who are not employed and searching for a job is divided by the labor force (unemployment). Wage income, which is measured as the average household income, which considers income earned from wages and salaries, self-employment, and capital income (wage income). And Inequality. To measure inequality, we calculated the Theil index using a measure of household income that comprises both the household income and monetary transfers from the state (income inequality).

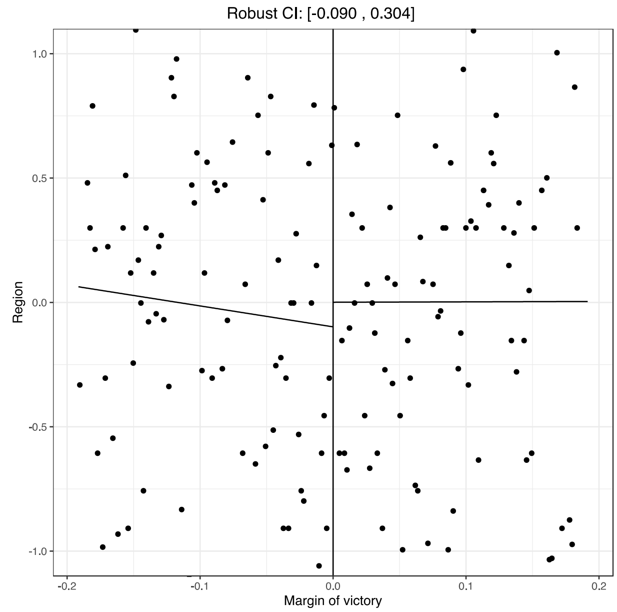
We also check the public expenditure on police the year before the election. Although there is not publicly available data on central government spending, we were able to collect data from the Regional Development Fund’s (RDF) expenditures on security issues, which includes resources to support police activities. These funds are transferred to municipalities by the central government, but through the governor, who is appointed by the president and represents the national executive in each region. As a reminder, although these funds are regional, it is important to note that there are no regional police forces, which is why this fund is a good proxy to gauge public spending on the police in the country. We thus computed the total amount of resources distributed by the RDF for public security purposes (lagged rdf security). We expressed them in Chilean constant pesos and lagged them in one mayoral period.

Finally, we also lagged in one mayoral period our outcomes of interest (i.e., homicides, rapes, assaults, theft, robbery, and robbery by surprise) to check the continuity assumption when using the number of reported crimes before the election.

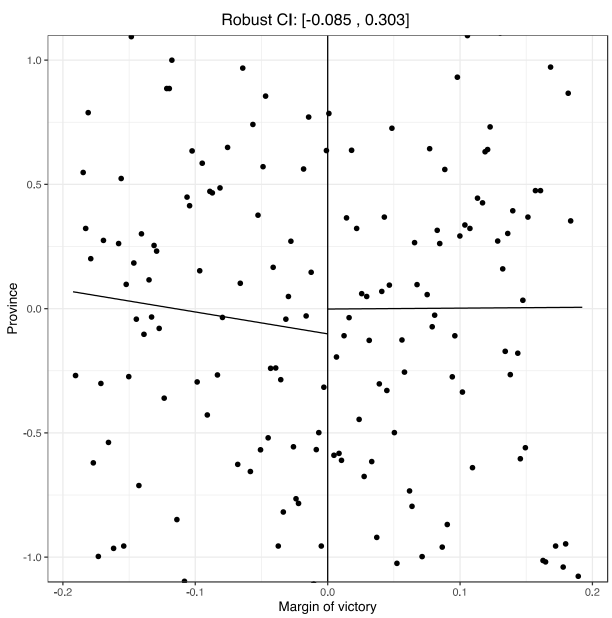
We standardized all the variables. The results show a smooth transition between control and treatment groups for all these variables. The sample size varies across covariates due to missing values. We report the results in figure and table format.



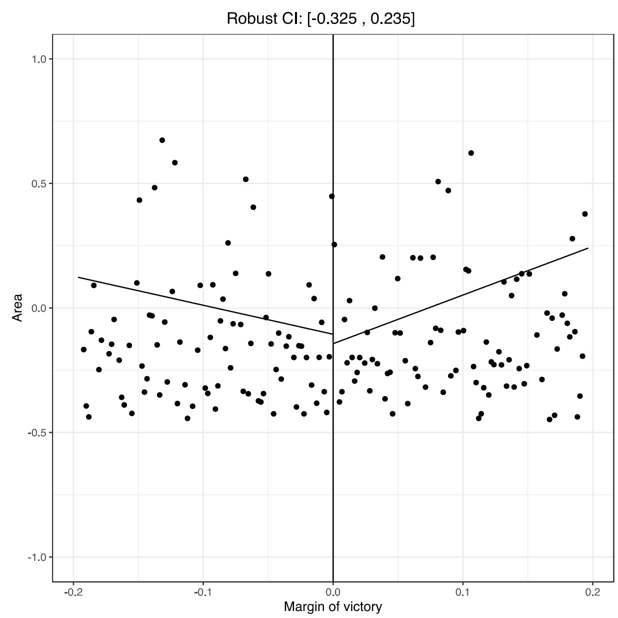
A1.a. Capital



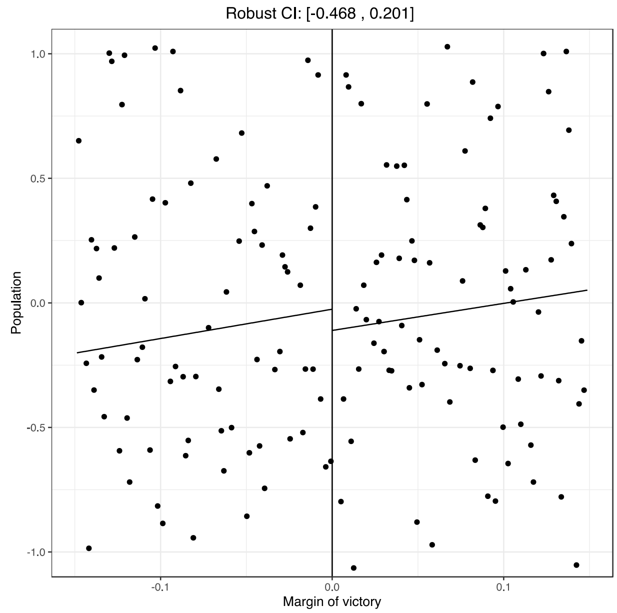
A1.b. Region



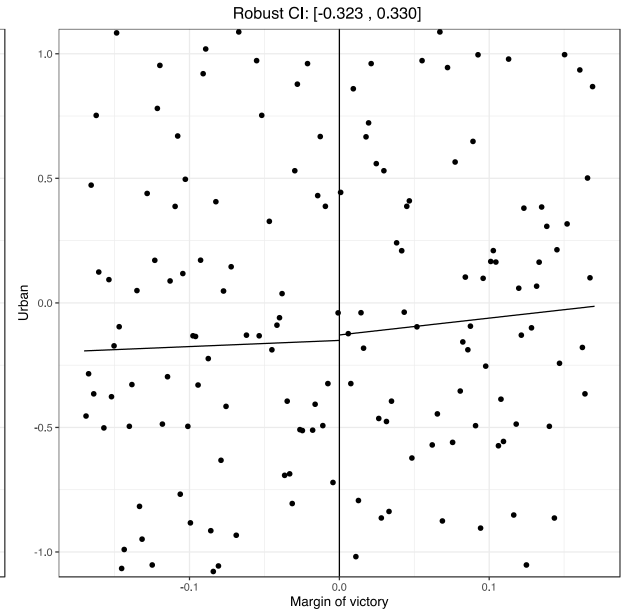
A1.c. Province



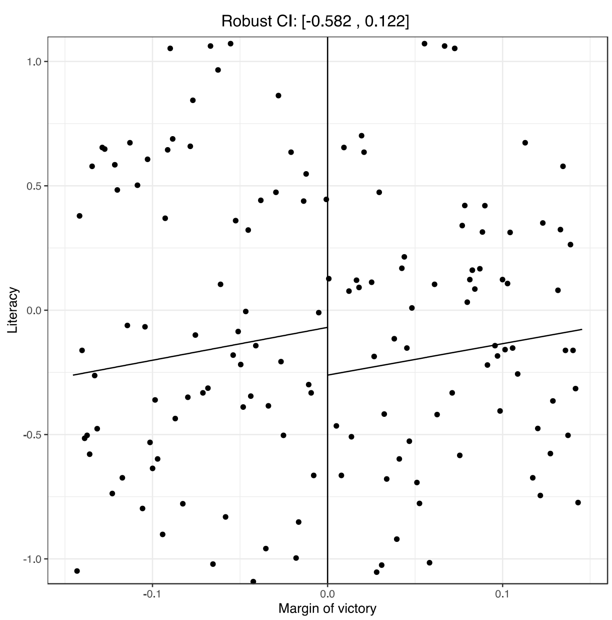
A1.d. Area



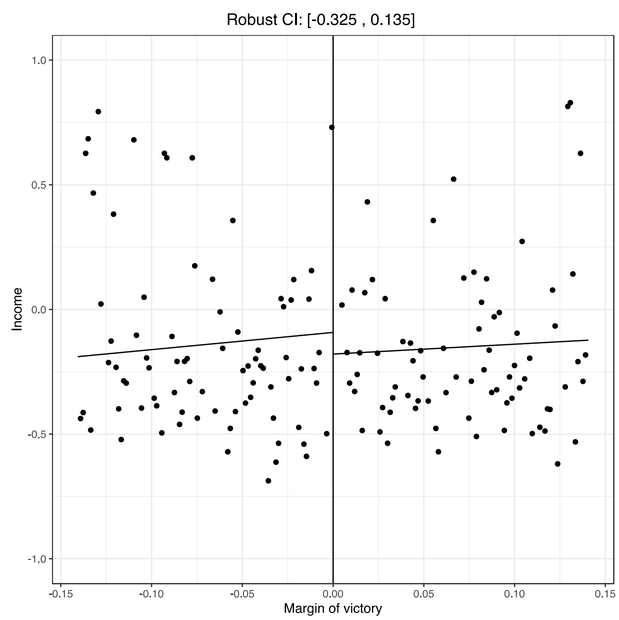
A1.e. Population



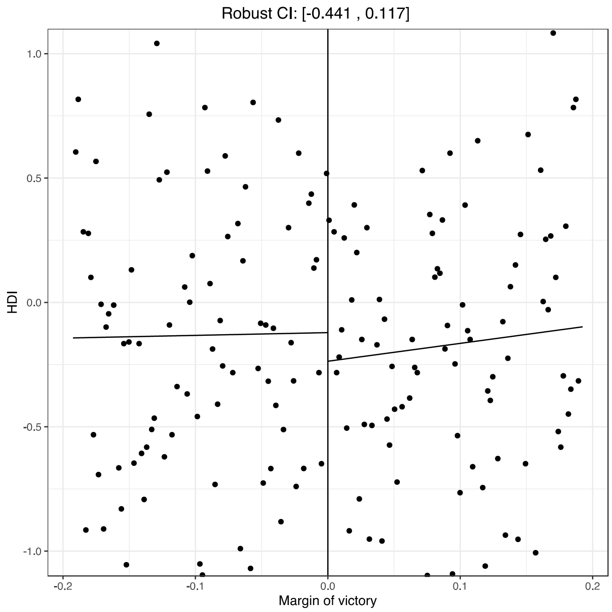
A1.f. Urban



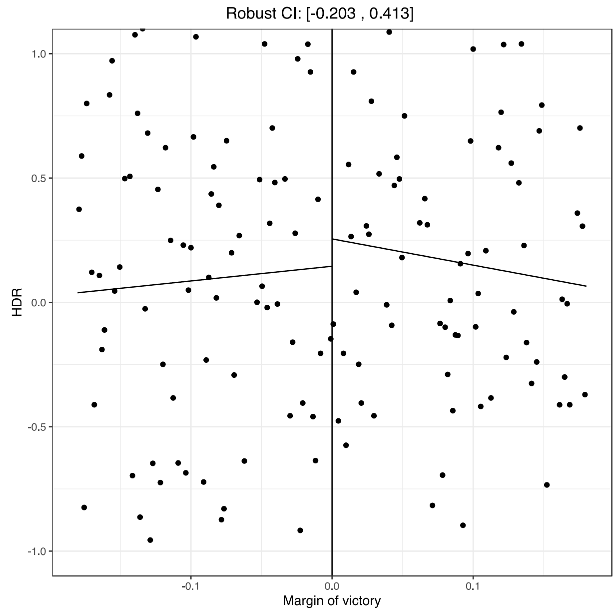
A1.g. Literacy



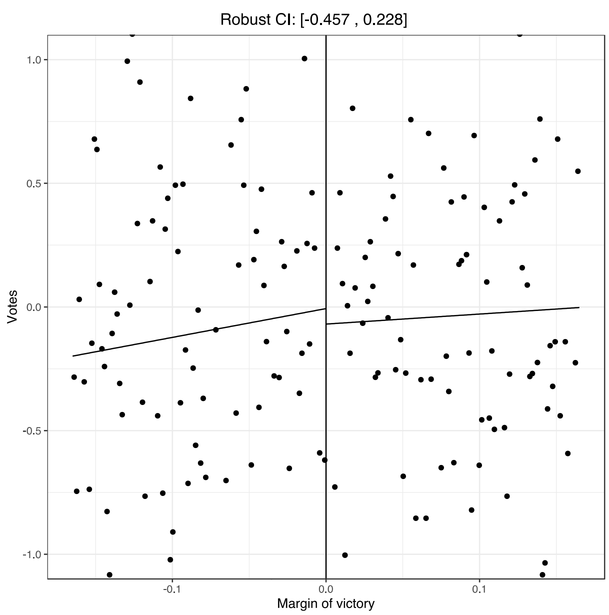
A1.h. Income



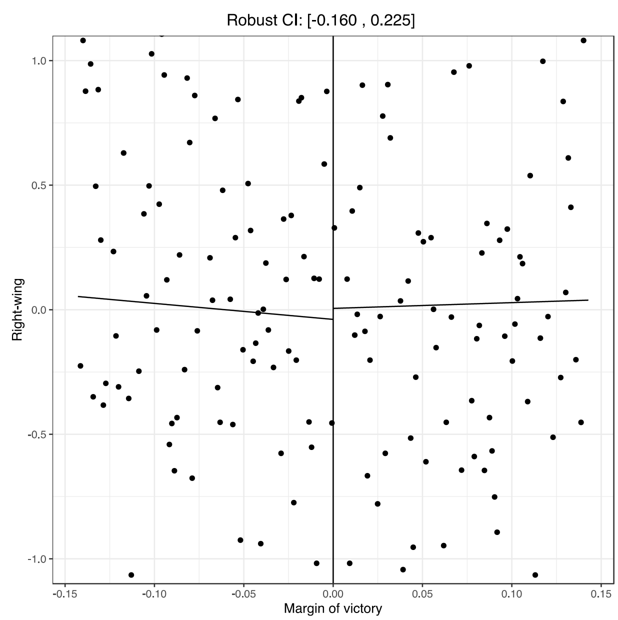
A1.i. HDI



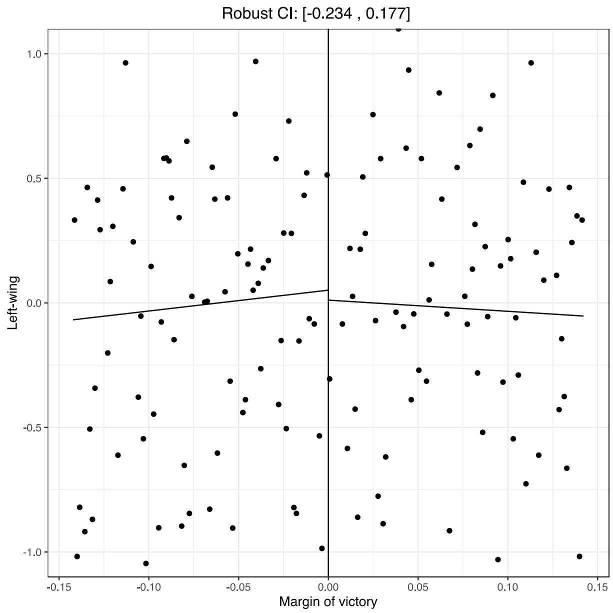
A1.j. HDR



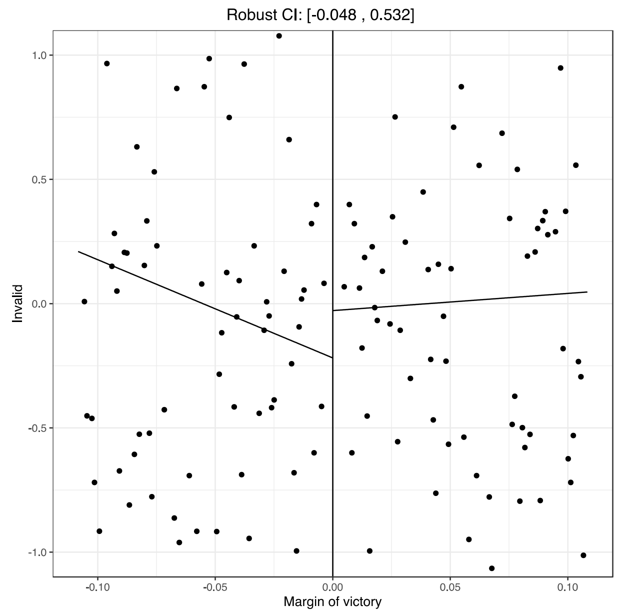
A1.k. Votes



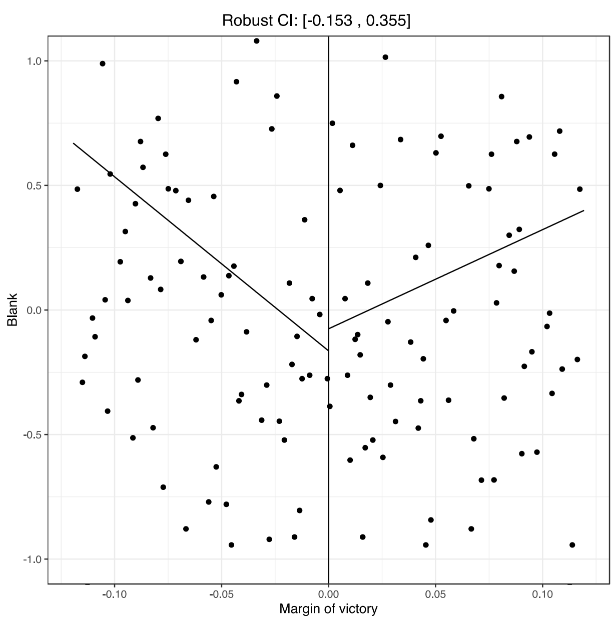
A1.l. Right-wing



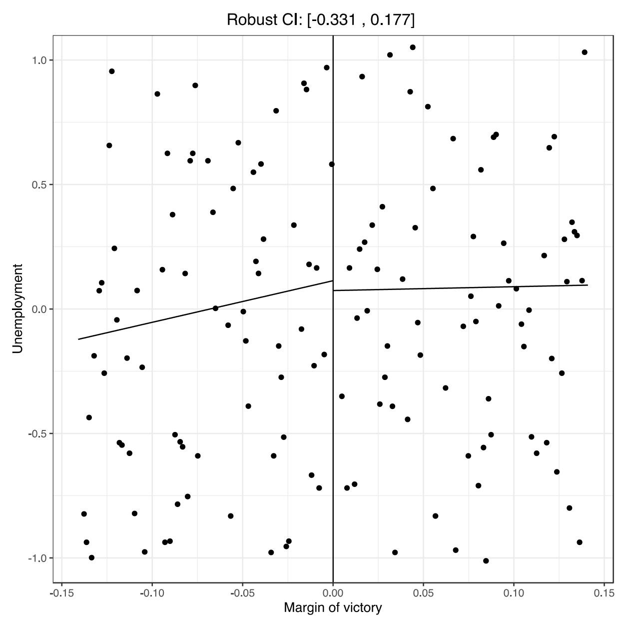
A1.m. Left-wing



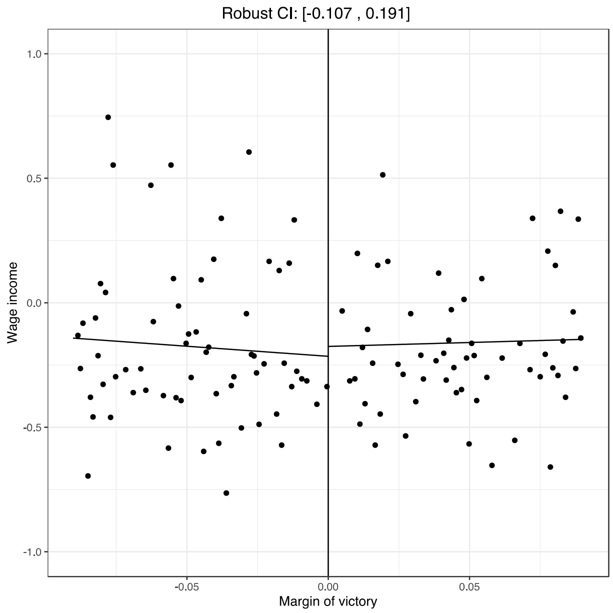
A1.n. Invalid



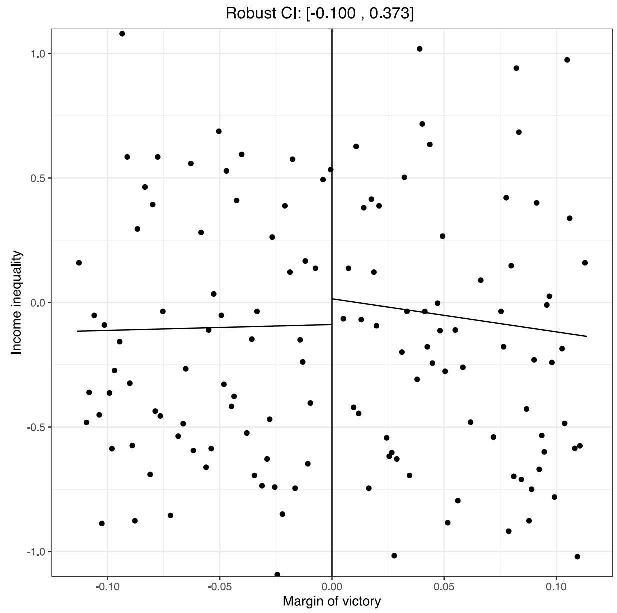
A1.o. Blank



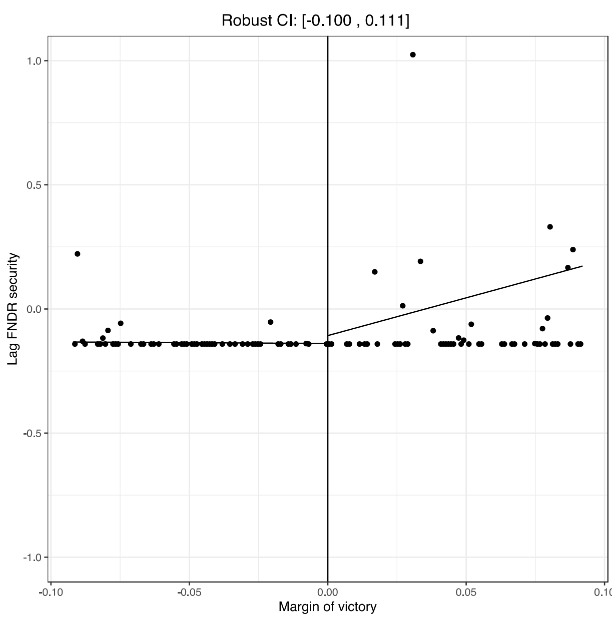
A1.p. Unemployment



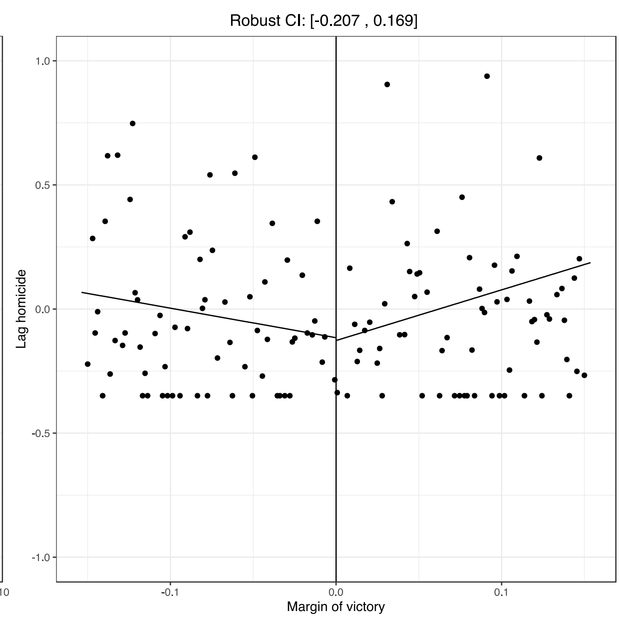
A1.q. Wage income



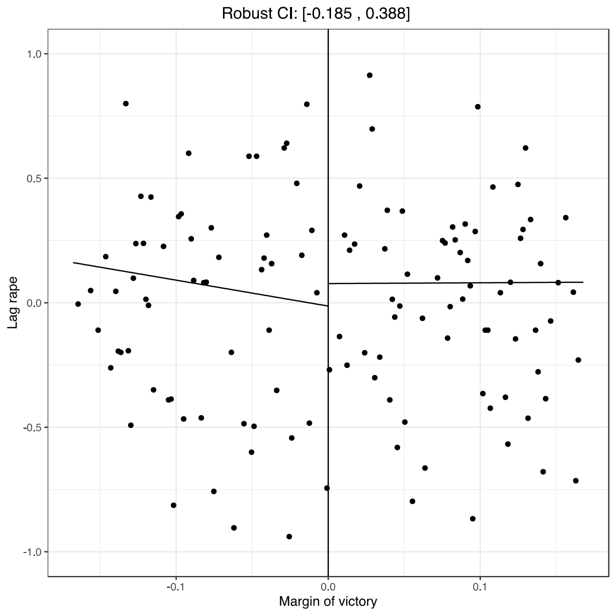
A1.r. Inequality



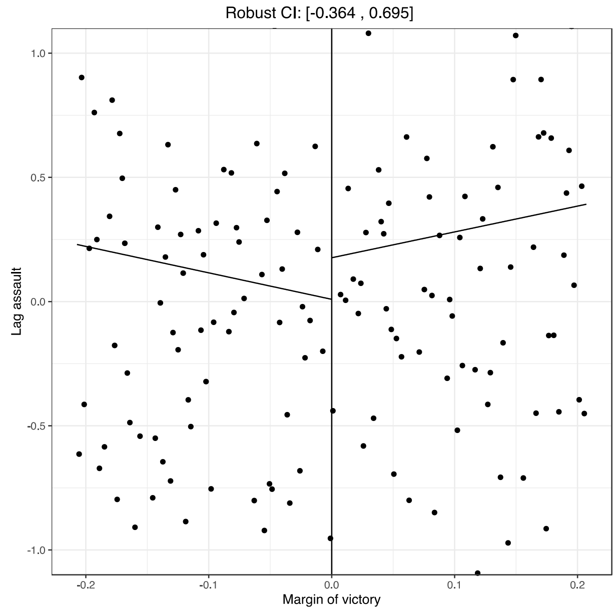
A1.s. Lagged RDF Security



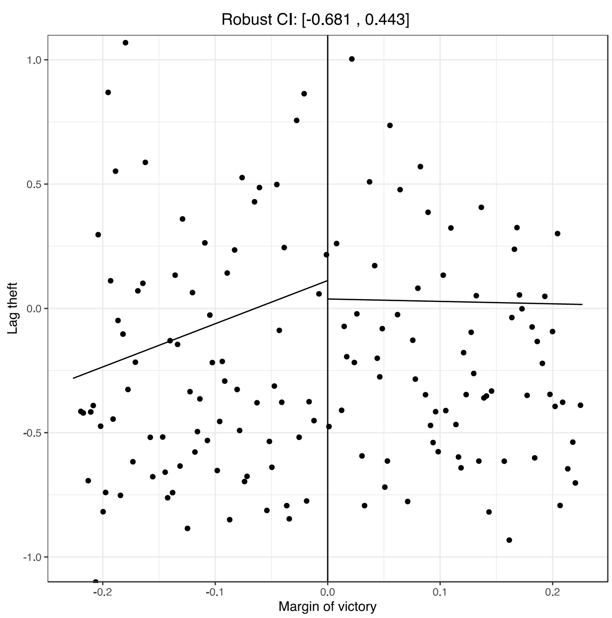
A1.t. Lagged Homicides



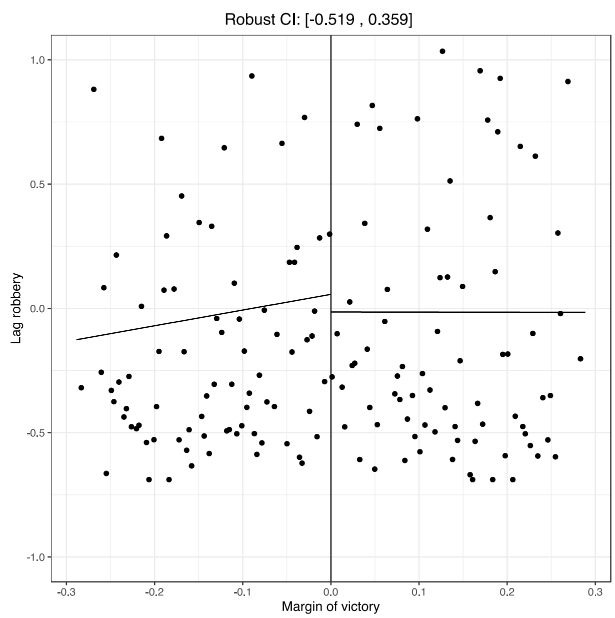
A1.u. Lagged Rape



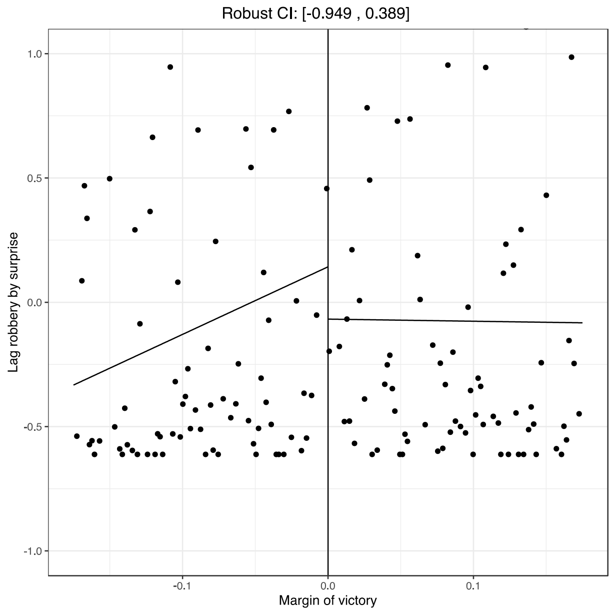
A1.v. Lagged Assault



A1.w. Lagged Theft



A1.x. Lagged Robbery



A1.y. Lagged Robbery by surprise

Figure A1: Effect of political alignment on pretreatment and placebo covariates

Table A1: Effect of political alignment on pretreatment and placebo covariates

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	MSE bandwidth
Capital	0.154	0.385	[-0.173, 0.447]	2099	971	0.133
Region	0.099	0.286	[-0.090, 0.304]	2099	1334	0.191
Province	0.100	0.269	[-0.085, 0.303]	2099	1334	0.192
Area	-0.038	0.753	[-0.325, 0.235]	2099	1350	0.197
Population	-0.085	0.433	[-0.468, 0.201]	2075	1074	0.149
Urban	0.022	0.984	[-0.323, 0.330]	2075	1202	0.170
Literacy	-0.192	0.200	[-0.582, 0.122]	2075	1039	0.145
Income	-0.087	0.416	[-0.325, 0.135]	1863	912	0.141

HDI	-0.115	0.255	[-0.441, 0.117]	2075	1322	0.192
HDR	0.110	0.504	[-0.203, 0.413]	2075	1273	0.180
Votes	-0.063	0.513	[-0.457, 0.228]	2075	1160	0.165
Right-wing	0.044	0.741	[-0.160, 0.225]	2075	1031	0.143
Left-wing	-0.040	0.783	[-0.234, 0.177]	2075	1027	0.142
Invalid	0.190	0.102	[-0.048, 0.532]	2075	779	0.108
Blank	0.089	0.435	[-0.153, 0.355]	2075	838	0.119
Unemployment	-0.040	0.551	[-0.331, 0.177]	1902	936	0.141
Wage income	0.040	0.579	[-0.107, 0.191]	1902	585	0.090
Inequality	0.103	0.257	[-0.100, 0.373]	1902	740	0.114
Lagged RDF Security	0.032	0.914	[-0.100, 0.111]	1012	317	0.092
Lagged Homicides	-0.012	0.845	[-0.207, 0.169]	1012	531	0.154
Lagged Rape	0.090	0.486	[-0.185, 0.388]	1012	566	0.168
Lagged Assault	0.167	0.540	[-0.364, 0.695]	1012	665	0.207
Lagged Theft	-0.074	0.678	[-0.681, 0.443]	1012	698	0.226
Lagged Robbery	-0.071	0.721	[-0.519, 0.359]	1012	771	0.289
Lagged Robbery by surprise	-0.210	0.412	[-0.949, 0.389]	1008	590	0.175

Appendix B: List of Interviewees

Table A2: List of interviewees

Position	Type of Interview	Date of Interview
Mayor, Nueva Mayoría	Semi-structured	November 20th, 2019
Mayor, Nueva Mayoría	Semi-structured	December 19th, 2019
Mayor, Nueva Mayoría	Semi-structured	January 31st, 2020
Mayor, Chile Vamos	Semi-structured	November 4th, 2019
Mayor, Chile Vamos	Semi-structured	December, 23rd, 2019
Mayor, Chile Vamos	Semi-structured	January 21st, 2020
Municipal Security Manager (Nueva Mayoría)	Semi-structured	January 2nd, 2020
Municipal Security Manager (Chile Vamos)	Semi-structured	January 3rd, 2020
Former Under-secretary of Interior and Public Security, Nueva Mayoría	Semi-structured	December 20th, 2019

Appendix C: Comparison of Different Samples

In this section we check the differences between the sample with all the units and the sample of eligible units (i.e., an aligned and a non-aligned mayoral candidate were the most voted for candidates, and candidates have a party affiliation). We aim for standardized differences below 0.2 standard deviation units as evidence of covariate balance (Silber et al. 2013). We evaluate 18 pretreatment or placebo covariates and find standardized differences equal or below one-fifth of a standard deviation for 16 covariates and an average standardized difference of 0.096.

Table A3: Comparing covariates across samples (all units and eligible units)

Covariates	Mean all units	Mean eligible units	Stand. Diff.
Capital	0.16	0.16	0.03
Province	85.47	92.39	0.29
Region	8.30	9.01	0.29
Area	2,184.24	1,707.39	0.12
Population	9.87	10.03	0.18
Urban	0.61	0.61	0.02
Literacy	90.65	90.85	0.05
Income	104,425.20	107,190.20	0.05
HDI	0.69	0.69	0.01
HDR	171.09	170.51	0.01
Votes	9.17	9.34	0.20
Right-wing	0.50	0.51	0.12
Left-wing	0.46	0.45	0.12
Invalid	0.02	0.02	0.06
Blank	0.01	0.01	0.08
Unemployment	0.09	0.09	0.06
Wage	210,419.40	212,016.70	0.02
Inequality	0.40	0.40	0.02

Since the RDD estimates a local treatment effect, it is important to check that the characteristics of the samples generated using optimal bandwidths are not very different from the sample with the eligible units. In the following tables, we compare the mean for 18 pretreatment and placebo covariates across the bandwidth samples used for the six main outcomes. In all cases we see that the units used to estimate the Local Average Treatment Effect (LATE) are not particularly different from all the eligible units (and the latter are not very different from all the units).

Table A4: Comparing covariates across samples (eligible units and homicide bandwidth)

Covariates	Mean eligible units	Mean homicide bandwidth	Stand. Diff.
Capital	0.16	0.16	0.00
Province	92.39	92.80	0.02
Region	9.01	9.05	0.02
Area	1,707.39	1,653.39	0.02
Population	10.03	9.94	0.08
Urban	0.61	0.58	0.14
Literacy	90.85	89.98	0.25
Income	107,190.20	94,879.94	0.14
HDI	0.69	0.68	0.21
HDR	170.51	184.92	0.21
Votes	9.34	9.27	0.08
Right-wing	0.51	0.51	0.05
Left-wing	0.45	0.45	0.05
Invalid	0.02	0.02	0.02
Blank	0.01	0.01	0.13
Unemployment	0.09	0.09	0.07
Wage	212,016.70	196,007.00	0.13
Inequality	0.40	0.40	0.02

Table A5: Comparing covariates across samples (eligible units and rape bandwidth)

Covariates	Mean eligible units	Mean rape bandwidth	Stand. Diff.
Capital	0.16	0.16	0.01
Province	92.39	92.62	0.01
Region	9.01	9.03	0.01
Area	1,707.39	1,680.89	0.01
Population	10.03	9.94	0.08
Urban	0.61	0.58	0.15
Literacy	90.85	89.98	0.25
Income	107,190.20	95,041.69	0.14
HDI	0.69	0.68	0.20
HDR	170.51	184.43	0.20
Votes	9.34	9.26	0.08
Right-wing	0.51	0.51	0.02
Left-wing	0.45	0.45	0.02
Invalid	0.02	0.02	0.04
Blank	0.01	0.01	0.14
Unemployment	0.09	0.09	0.07
Wage	212,016.70	196,156.70	0.12
Inequality	0.40	0.40	0.00

Table A6: Comparing covariates across samples (eligible units and assault bandwidth)

Covariates	Mean eligible units	Mean assault bandwidth	Stand. Diff.
Capital	0.16	0.18	0.07
Province	92.39	91.69	0.03
Region	9.01	8.93	0.03
Area	1,707.39	1,720.40	0.00
Population	10.03	9.96	0.07
Urban	0.61	0.59	0.12
Literacy	90.85	90.19	0.19
Income	107,190.20	95,566.11	0.13
HDI	0.69	0.68	0.18
HDR	170.51	181.43	0.16
Votes	9.34	9.28	0.07
Right-wing	0.51	0.51	0.05
Left-wing	0.45	0.45	0.05
Invalid	0.02	0.02	0.01
Blank	0.01	0.01	0.08
Unemployment	0.09	0.09	0.04
Wage	212,016.70	196,568.80	0.12
Inequality	0.40	0.40	0.00

Table A7: Comparing covariates across samples (eligible units and theft bandwidth)

Covariates	Mean eligible units	Mean theft bandwidth	Stand. Diff.
Capital	0.16	0.15	0.04
Province	92.39	90.42	0.08
Region	9.01	8.81	0.08
Area	1,707.39	1,610.50	0.03
Population	10.03	9.94	0.09
Urban	0.61	0.57	0.20
Literacy	90.85	90	0.24
Income	107,190.20	92,446.75	0.16
HDI	0.69	0.68	0.20
HDR	170.51	184.48	0.19
Votes	9.34	9.27	0.08
Right-wing	0.51	0.51	0.02
Left-wing	0.45	0.46	0.02
Invalid	0.02	0.02	0.03
Blank	0.01	0.01	0.18
Unemployment	0.09	0.09	0.08
Wage	212,016.70	190,255.70	0.16
Inequality	0.40	0.39	0.08

Table A8: Comparing covariates across samples (eligible units and robbery bandwidth)

Covariates	Mean eligible units	Mean robbery bandwidth	Stand. Diff.
Capital	0.16	0.15	0.04
Province	92.39	91.91	0.02
Region	9.01	8.96	0.02
Area	1,707.39	1,681.03	0.01
Population	10.03	9.92	0.11
Urban	0.61	0.57	0.21
Literacy	90.85	89.97	0.24
Income	107,190.20	92,033.41	0.17
HDI	0.69	0.68	0.21
HDR	170.51	185.48	0.21
Votes	9.34	9.24	0.11
Right-wing	0.51	0.51	0.04
Left-wing	0.45	0.45	0.03
Invalid	0.02	0.02	0.01
Blank	0.01	0.01	0.25
Unemployment	0.09	0.09	0.07
Wage	212,016.70	190,042.20	0.17
Inequality	0.40	0.39	0.07

Table A9: Comparing covariates across samples (eligible units and robbery by surprise bandwidth)

Covariates	Mean eligible units	Mean robbery by surprise bandwidth	Stand. Diff.
Capital	0.16	0.15	0.04
Province	92.39	91.10	0.05
Region	9.01	8.88	0.05
Area	1,707.39	1,807.07	0.04
Population	10.03	9.90	0.12
Urban	0.61	0.57	0.20
Literacy	90.85	90.05	0.22
Income	107,190.20	92,505.37	0.16
HDI	0.69	0.68	0.19
HDR	170.51	183.78	0.18
Votes	9.34	9.22	0.12
Right-wing	0.51	0.51	0.01
Left-wing	0.45	0.45	0.01
Invalid	0.02	0.02	0.00
Blank	0.01	0.01	0.25
Unemployment	0.09	0.09	0.05
Wage	212,016.70	190,899.70	0.16
Inequality	0.40	0.39	0.09

Appendix D: **Unclustered Standard Errors and Unstandardized Outcomes**

Table A10: Unclustered standard errors

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	Bandwidth
Homicide	- 0.090	0.483	[-0.486, 0.230]	2099	1157	0.162
Rape	-0.039	0.659	[-0.326, 0.206]	2099	1217	0.171
Assault	-0.055	0.421	[-0.261, 0.109]	2099	1314	0.109
Theft	-0.612	0.000	[-0.960, -0.379]	2099	513	0.077
Robbery	-0.446	0.001	[-0.810, -0.197]	2099	692	0.095
Robbery by surprise	-0.452	0.001	[-0.812, -0.216]	2087	672	0.094

Table A11: Unstandardized outcomes

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	Bandwidth
Homicide	-0.975	0.419	[-4.758, 1.981]	2099	1146	0.162
Rape	-0.875	0.470	[-4.464, 2.060]	2099	1114	0.155
Assault	-10.646	0.531	[-65.852, 33.966]	2099	1362	0.198
Theft	-375.727	0.000	[-648.635, -195.461]	2099	695	0.096
Robbery	-110.916	0.017	[-236.884, -22.772]	2099	779	0.106
Robbery by surprise	-46.565	0.019	[-102.175, -9.148]	2087	799	0.110

Table A12: Unclustered standard errors and unstandardized outcomes

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	Bandwidth
Homicide	-0.968	0.483	[-5.252, 2.482]	2099	1157	0.162
Rape	-0.616	0.659	[-5.118, 3.235]	2099	1217	0.171
Assault	-11.869	0.421	[-56.386, 23.543]	2099	1314	0.186
Theft	-412.063	0.000	[-646.967, -255.425]	2099	513	0.077
Robbery	-128.107	0.001	[-232.865, -56.670]	2099	692	0.095
Robbery by surprise	-56.072	0.001	[-100.846, -26.754]	2087	672	0.094

Appendix E: Burglary

In this section we visually report the effect of political alignment on burglaries.

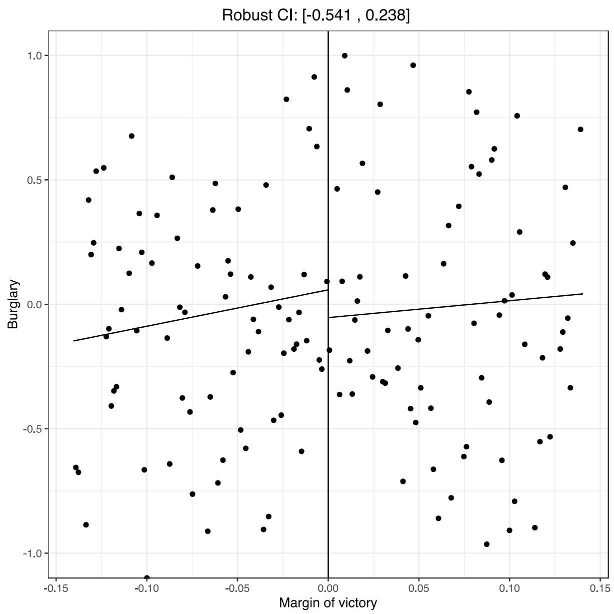


Figure A2: Effect of political alignment on burglary

Appendix F: Displacement Between Municipalities

Our results show that political alignment between the mayor and the president decreases street property crime. These results raise another relevant question: whether alignment is indeed reducing crime or, alternatively, just displacing it to neighboring municipalities. Extant research has studied how crime-reduction interventions can generate immediate spatial displacement of crime to areas near the targeted or treated sites (Bowers and Johnson 2003; Weisburd et al. 2006; Guerrete and Bowers 2009). The most common methodological approach is to use the weighted displacement quotient (WDQ) developed by Bowers and Johnson (2003). To determine the WDQ, we need to define three operational areas: the target area, the buffer areas where crime is more likely to be displaced, and control areas that allow us to check general crime trends (Ratcliffe and Green 2011). However, it is difficult to implement the WDQ within an electoral RDD framework because multiple units are treated at the same time and the displacement quotient does not distinguish between having a treated neighbor or not. The WDQ becomes a more intuitive strategy when studying one particular crime-reduction intervention and not a series of interventions assigned to several units that are sometimes contiguous.

As a result, we provide an approach to evaluate the impact of displacement that is consistent with our original empirical strategy. Specifically, we study the heterogeneous treatment effects of political alignment based on having or not having adjacent aligned municipalities. As an extension of our argument, we expect that treated municipalities should displace crime to non-treated municipalities, which lack improved public infrastructure, but should be less likely to do so to other treated municipalities, as the quality of their public spaces is expected to be similar. We focus on the interaction between the treatment and a binary indicator of having an aligned neighbor, which allows us to learn whether alignment between the mayor and the president has a different effect depending on the status (i.e., aligned or not-aligned) of the neighboring municipalities. We exclude from the analysis municipalities that do not have neighbors (e.g., Eastern Island).

The existence of heterogeneous treatment effects depending on the treatment status of the aligned municipalities can be interpreted as evidence of displacement. In the previous analysis, we used the *rdrobust package*; that approach, however, does not allow us to estimate the heterogenous treatment effects. To implement this analysis, we use the equation A1:[4]

$$Y_{it} = \alpha + \beta_1 T_{it} + \beta_2 A_{it} + \beta_3 T_{it} * A_{it} + \beta_4 M_{it} + \varepsilon_{it} \quad \text{(A.1)}$$

Y corresponds to the street property crimes in standard deviation units in municipality *i* and year *t*. *T* depicts the treatment (units above the cutoff). *A* is a binary indicator of having or not having an aligned adjacent municipality, and the interaction between *T* and *A* represents the change in effect of political alignment depending on having or not having an aligned adjacent municipality. *M* describes the margin of victory. We use the MSE optimal bandwidth to estimate the previous equation and a triangular kernel that assigns more weights to units closer to the cutoff. The table below summarizes the heterogeneous treatment effects of political alignment.

[4] We use the specification for pooled regression described in Lee and Lemiux (2010: 318) that does not include an interaction between the treatment and the running variable. That decision facilitates the interpretability of the results (i.e., the specification does not require a triple interaction between the treatment, the running variable, and the covariate).

Table A13: Heterogenous effects of political alignment

	Street property crimes
Political alignment (β_1)	-0.512***
	(0.117)
Aligned adjacent municipality (β_2)	0.244***
	(0.060)
Interaction (β_3)	-0.046
	(0.139)
MSE bandwidth	0.093
N	665

Note: *p<0.1; **p<0.05; ***p<0.01. We only show the coefficients of interest.

The results are consistent with our theoretical expectations, as they show that political alignment significantly reduces street property crimes in municipalities without an aligned neighbor (β_1). Furthermore, for non-treated municipalities, having an aligned adjacent municipality correlates with having more crime (β_2), which provides evidence of crime displacement from areas with improved public spaces to others with low-quality public infrastructure. Along the lines of our argument, non-treated municipalities are recipients of crime, as criminals prefer to move to municipalities in which they face lower risks of being caught. Finally, the interaction does not show a significant difference in the effect of political alignment across municipalities that have and do not have an aligned neighbor (β_3). This signals that political alignment reduces crime in a municipality regardless of the status of its neighbors.

Appendix G: Timing

One could expect that political alignment would not have the same effect in the first and remaining years of a mayoral term since projects funded by the central government might take time to crystallize. To check this, we modified equation A.1 used before, and instead of interacting the treatment with having an aligned municipality, we interact it with the year of the mayoral term. The first year is the reference category (factor variable). We only report the coefficients of interest. As the outcome, we aggregate the three property crimes that happen in public into a single variable. The table below summarizes the results.

Table A14: Heterogenous effects of political alignment

	Street property crime
Alignment	-0.541***
	(0.090)
Alignment*Second year	-0.031**
	(0.014)
Alignment*Third year	-0.016
	(0.018)
Alignment*Fourth year	-0.048
	(0.097)
MSE bandwidth	0.096
N	695

Note: *p<0.1; **p<0.05; ***p<0.01.

The results show that there is a larger decrease in crime after the first year of a mayoral term, and that the difference is significant at least for the second year (when compared with the first one). However, political alignment already has a substantive effect on reducing crime in the first year. We hold that this is explained by the nature of the discretionary funds studied in this paper: that is, they are easily distributed and quickly used by local governments.

Appendix H: Alternative Mechanisms

It is possible to argue that alignment can impact crime through alternative mechanisms such as the political manipulation of the police, access to the police and courts, improvements in labor market opportunities, or access to social programs. While plausible, we discuss different arguments that might make these explanations less relevant for the Chilean case. However, in countries where these alternative mechanisms operate, we might expect even larger effects of alignment on local criminality, as they might reinforce each other.

The first alternative mechanism is that political alignment could affect policing. Recent studies have explored as to whether the presence of police forces and certain policing strategies contribute to reducing crime. Regarding presence, Blattman et al. (2017) find that increasing state presence (e.g., more police patrols) has modest direct impacts on reducing crime. As to strategies, Mummolo (2018) shows that militarized policing fails to reduce local crime. Alignment might affect the presence of the police, as the central government might be interested in benefiting aligned municipalities and thus increases the deployment of police forces in order to reduce crime. However, in Chile, the central government does not control the police and, therefore, it cannot manipulate the number of officers at the local level. Indeed, the Carabineros are a national and militarized police force, and the sole law enforcement agency responsible for policing at the local level.[5] More importantly, Carabineros' relation with the national government has been traditionally marked by high levels of autonomy, which was enhanced by a series of institutional reforms carried out during Pinochet's dictatorship (1973-1990) (Früling 2010). In effect, the institution has largely remained isolated from civilian control over operations and the development of strategies (Candina 2006). As a former Under-secretary of Interior and Public Security commented, "the central government cannot manipulate police deployment at the municipal level […] It is the Carabineros who determine, using a software, the number of officers required in each locality."[6] Similarly, former Under-Secretary of Carabineros Neftali Carabantes claimed that "the Ministry of Interior does not have […] the power to approve police services, the deployment of operations, the resources associated to services, the distribution of personnel, and control their budget" (El Mostrador 2019). Finally, we contend that, if political alignment were to affect the deployment of police forces, we should expect a decrease in all types of crime in aligned municipalities, as existing research has shown that increasing the number of police contributes to reducing the rates of many of the crimes we study, including those against the person and property-related, as well as those that occur in public and private spaces. For instance, more police presence reduces the rates of crimes such as homicide and robbery (Marvell and Moody 1996; Levitt 1997; Evans and Owens 2007), burglary (Marvell and Moody 1996; Evans and Owens 2007), and assault (Levitt 1997; Evans and Owens 2007). Increasing the number of police may affect different crimes through different mechanisms. For instance, in the cases of robbery and assault, there is a victim that can identify the offender and therefore, increased police effort might make arrest more likely (Levitt 1997). More generally, police presence deters crime, as it increases the probability of arrest and, consequently, the cost of committing a crime (Lin 2009). In our study, however, we find no

[5] Chile has another police, the Investigative Police (*Policía de Investigaciones* – PDI). Nonetheless, the PDI is only in charge of crime investigations, and also centralized at the national level.

[6] Interview by authors, former Under-secretary of Interior and Public Security, Nueva Mayoría, December 20th, 2019.

effect on a series of crimes against the person, including assaults, a crime that is particularly sensitive to policing, which suggests that it is not the police, but rather the improvement of public infrastructure what explains the reduction in street property crime.

Similarly, as existing research on alignment and crime in Mexico and Colombia show, aligned mayors could manipulate the police either to coordinate efforts with the central government to curb criminality or to establish corrupt deals with criminals, enforcing the law selectively. Nonetheless, as noted, the police in Chile is centralized at the national level. Thus, the country does not have subnational police forces, which entails that mayors in aligned municipalities cannot control the police and, therefore, the coordination and corruption mechanisms do not apply. This is further reinforced by the Carabineros' autonomy, as decisions are made by high-ranking officers at the national level. In effect, according to a former Under-secretary of Interior and Public Security, "mayors can request that Carabineros police one area more than others, but it is the police in the end who decide whether they will do it or not."[7] Relatedly, although mayors cannot manipulate the police, there might be informal arrangements between mayors and high-ranking police officers to decide how the Carabineros will enforce the law in a given municipality. Indeed, as Holland (2015, 2017) shows, mayors can request Carabineros not to enforce the law against street vendors. However, alignment does not appear to be a relevant explanatory variable to account for mayors' decisions to either enforce the law or support forbearance. According to Holland's evidence, both aligned and non-aligned mayors in poor municipalities supported forbearance. Similarly, in wealthier districts, aligned and non-aligned mayors enforced the law. For instance, the author provides qualitative evidence of poor municipalities, such as Conchali and Pedro Aguirre Cerda, whose mayors were aligned and non-aligned, respectively,[8] but where forbearance was adopted as a strategy to benefit the poor.

The second mechanism posits that alignment might have an effect on local institutions, such as the police and courts, either improving individuals' access to these institutions, or, alternatively, creating incentives for the police to deter citizens from reporting crime. In both scenarios, however, all types of crime should be affected. Therefore, this argument does not explain the difference between street property crimes and other offenses. We explore this by using crimes at the household level as a placebo analysis, including disturbing the peace (DTP), domestic violence against the elderly (DVE), domestic violence against men (DVM), domestic violence against women (DVW), and domestic violence against children (DVC). Since we do not expect public infrastructure to impact these types of crime, finding an effect would provide evidence that there are other mechanisms at play. The figures below summarize the results. As expected, having an aligned mayor with the president does not affect crimes at the household level, which signals that the mechanisms related to the police and courts do not apply. Consequently, the fact that we only observe a reduction in property crimes that occur on the streets strongly indicates that improvements in public infrastructure, rather than other mechanisms, explain these results.

The third alternative mechanism holds that alignment could help improve individuals' income, as the construction of urban infrastructure could decrease unemployment levels in aligned

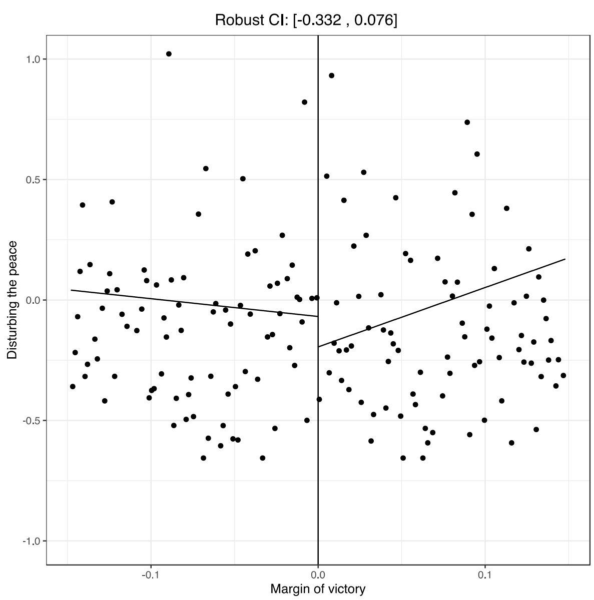
---

[7] Interview by authors, former Under-secretary of Interior and Public Security, Nueva Mayoría, December 20th, 2019.

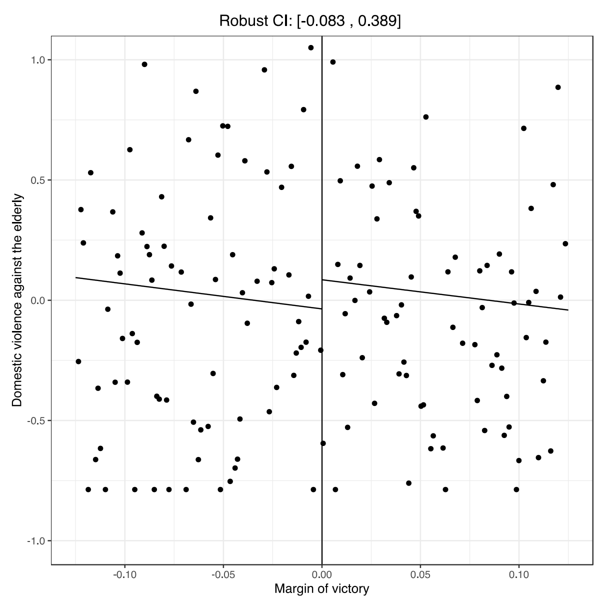
[8] The president at the time these interviews were conducted was right-wing Sebastian Piñera. The mayor of Conchali was Ruben Malvoa, also a right-wing politician, and member of the president's party, Renovacion Nacional, whereas the mayor of Pedro Aguirre Cerda was Claudina Nuñez, a Communist politician.

municipalities by increasing demand for local labor and, thus, reduce criminality. Similarly, aligned municipalities could receive more employment programs, which would decrease crime. Although existing research in economics has found mixed evidence regarding the effect of employment on crime, some studies suggest that improvements in the labor market contribute to reducing crime (e.g., Raphael and Winter-Ebmer 2001; Gould et al. 2002). Since there is no data on unemployment at the municipal level in Chile, we use the number of people who have registered in the municipality's job placement office as a proxy for levels of local unemployment. As observed in the figures below, we find no evidence that alignment has an effect on the number of individuals looking for a job. This suggests that unemployment is not driving the variation in local crime rates.

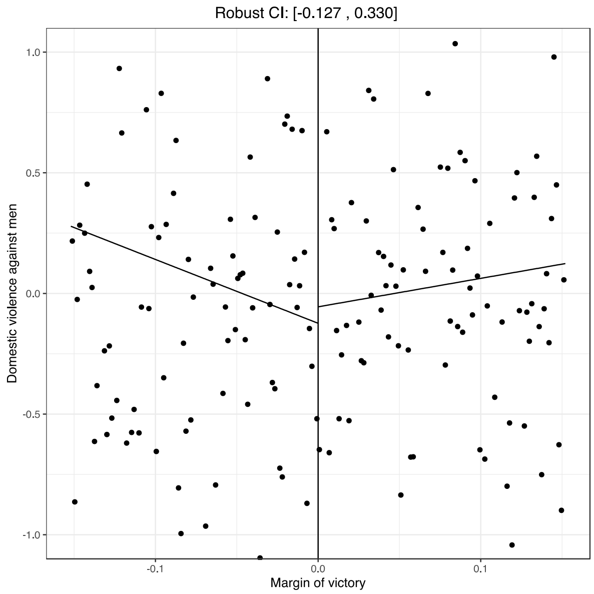
Fourth, alignment could also affect individual income through social programs. For instance, aligned municipalities could receive more social programs than non-aligned ones, thereby increasing individual income and affecting the opportunity cost of committing an economically motivated crime (Gould et al. 2002). The results show that there is no effect of alignment on the municipality's per capita spending on social programs, which signals that this mechanism does not explain our main findings.



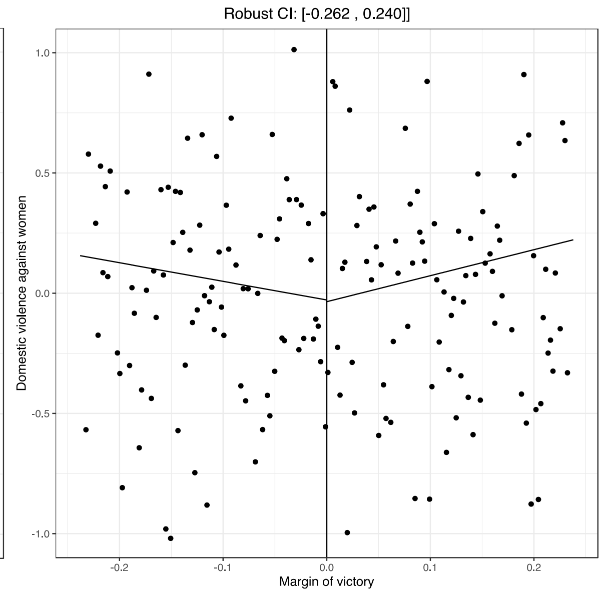
A3.a. DTP



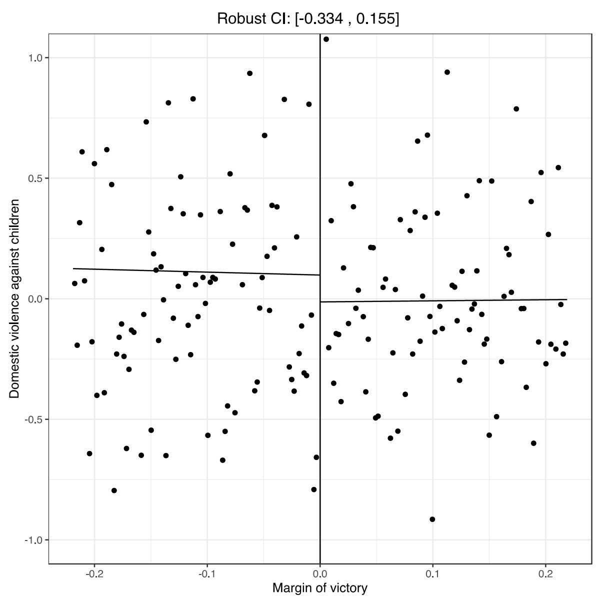
A3.b. DVE



A3.c. DVM

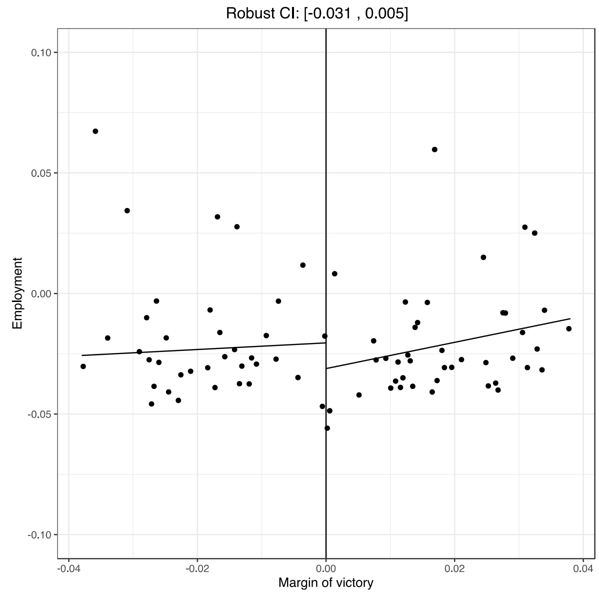


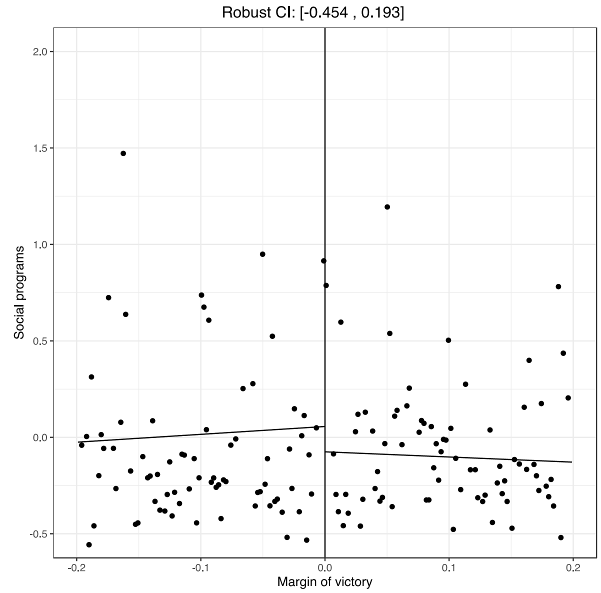
A3.d. DVW



A3.c. DVC

Figure A3: Effect of political alignment on placebo outcomes





A4.a. Employment A4.b. Social programs

Figure A4: Effect of political alignment on alternative mechanisms

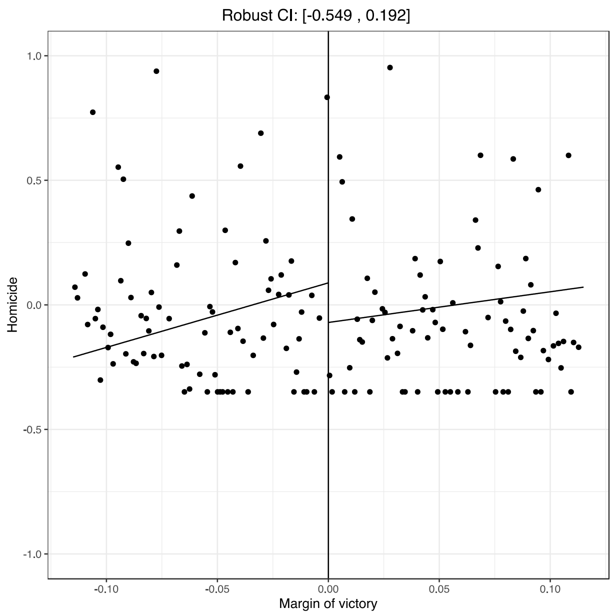
Table A15: Effect of political alignment on placebo outcomes and alternative mechanisms

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	MSE bandwidth
DTP	-0.127	0.219	[-0.332, 0.076]	2099	1082	0.148
DVE	0.122	0.203	[-0.083, 0.389]	2099	895	0.125
DVM	0.068	0.384	[-0.127, 0.330]	2099	1110	0.152
DVW	-0.007	0.933	[-0.262, 0.240]	2099	1509	0.238
DVC	-0.111	0.472	[-0.334, 0.155]	2099	1444	0.219
Employment	-0.011	0.170	[-0.031, 0.005]	1957	259	0.038
Social programs	-0.131	0.429	[-0.454, 0.193]	1111	720	0.199

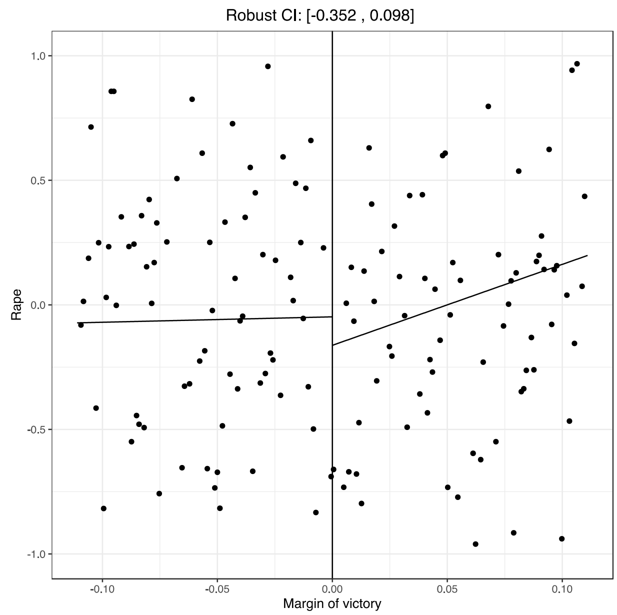
Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.

Appendix I: Other Bandwidths

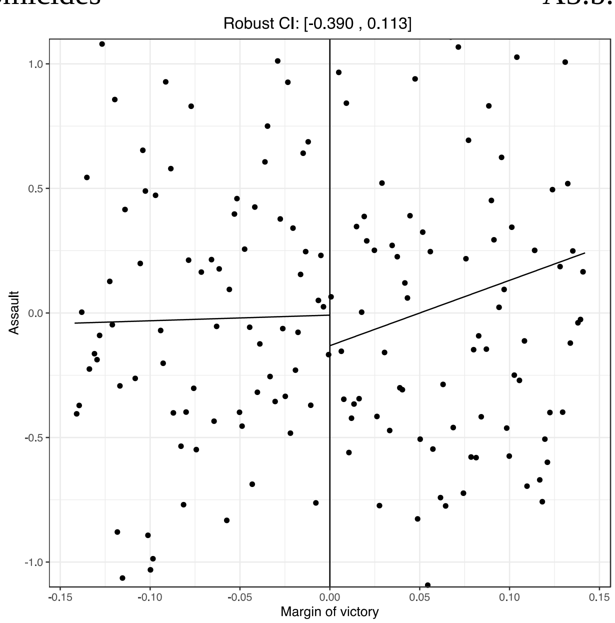
Figures below replicate the results for the main six outcomes using CER-optimal, MSE-sum, and MSE-two selectors. Results are consistent across all bandwidths.



A5.a. Homicides

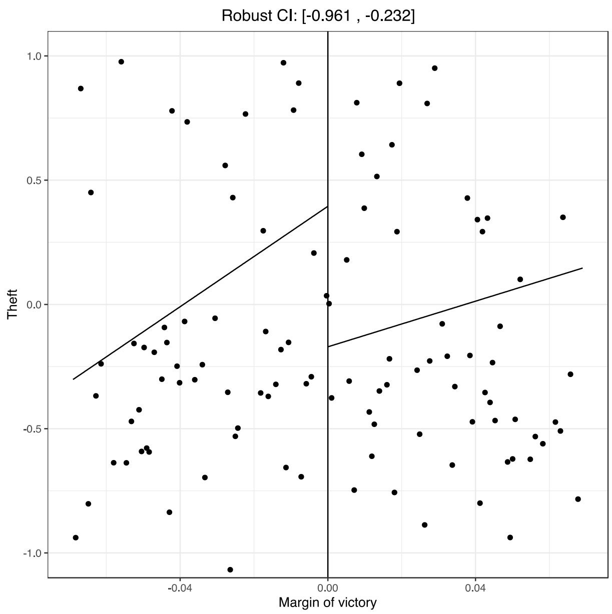


A5.b. Rapes

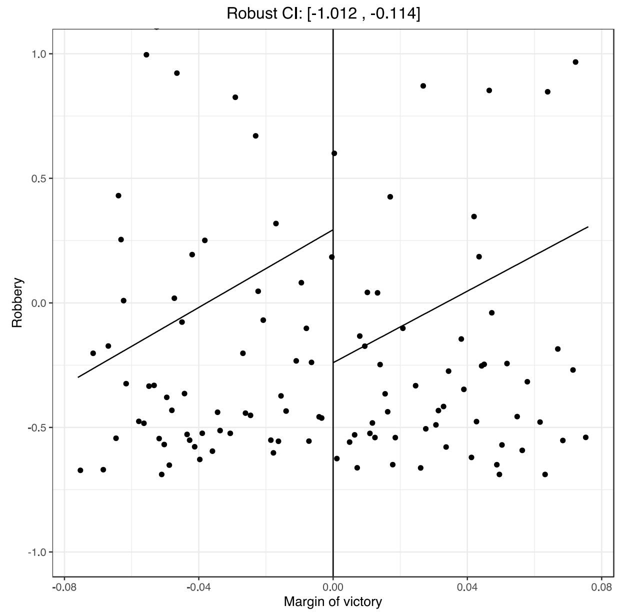


A5.c. Assault

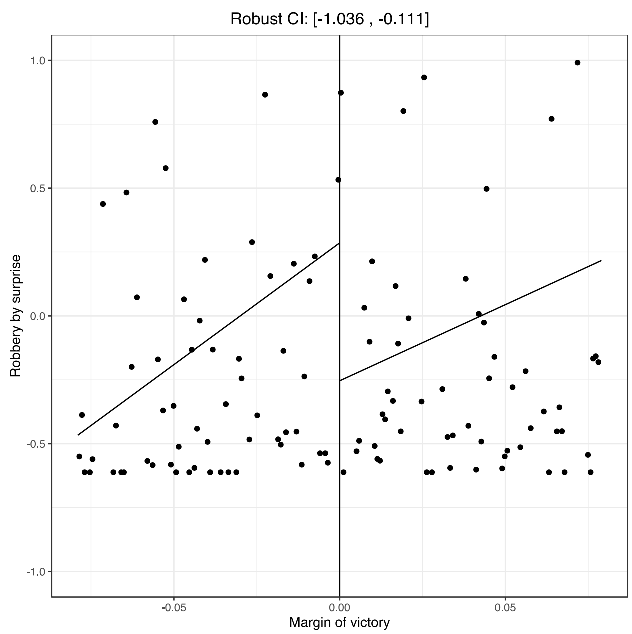
Figure A5: RD plot for political alignment on crimes against the person (CER-optimal bandwidths)



A6.a. Theft



A6.b. Robbery



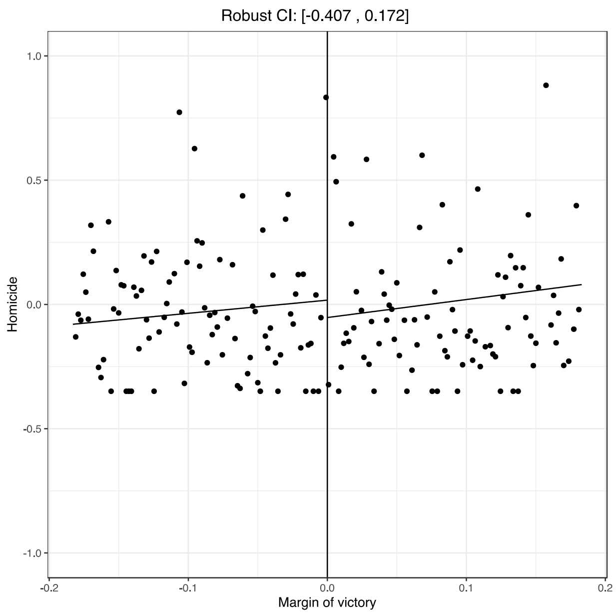
A6.c. Robbery by surprise

Figure A6: Effect of political alignment on street property crime (CER-optimal bandwidths)

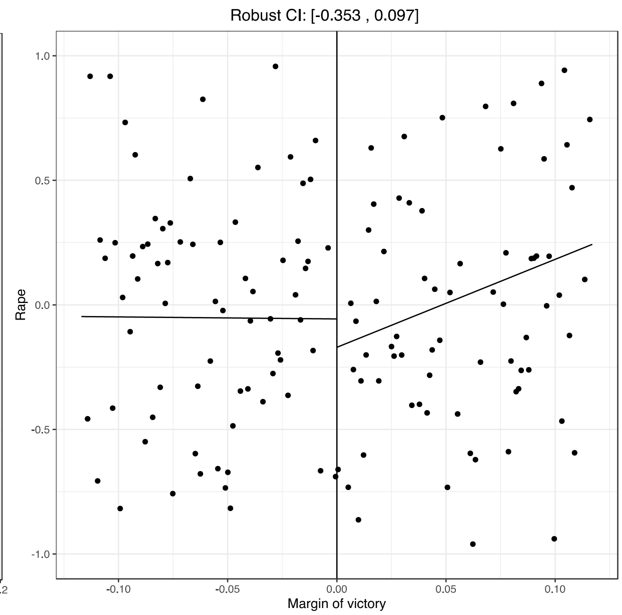
Table A16: Effect of political alignment on crime (CER-optimal bandwidths)

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	CER-optimal bandwidth
Homicide	-0.158	0.344	[-0.549, 0.192]	2099	826	0.115
Rape	-0.114	0.269	[-0.352, 0.098]	2099	811	0.111
Assault	-0.123	0.282	[-0.390, 0.113]	2099	1035	0.142
Theft	-0.563	0.001	[-0.961, -0.232]	2099	485	0.069
Robbery	-0.533	0.014	[-1.012, -0.114]	2099	501	0.076
Robbery by surprise	-0.541	0.015	[-1.036, -0.111]	2087	520	0.079

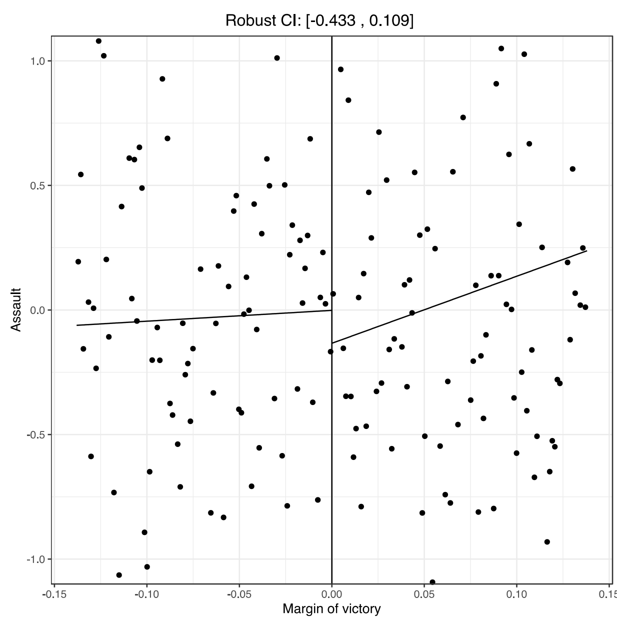
Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.



A7.a. Homicides

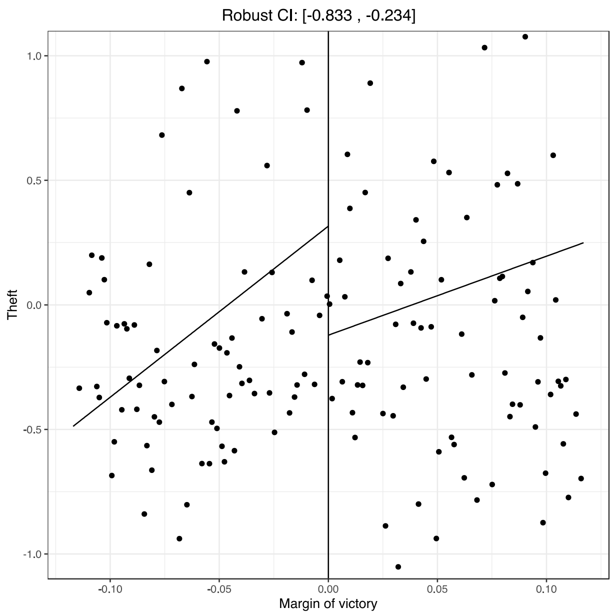


A7.b. Rapes

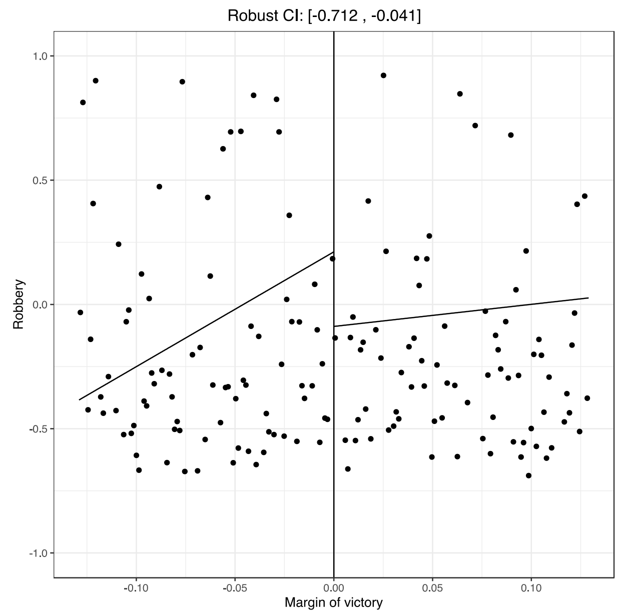


A7.c. Assault

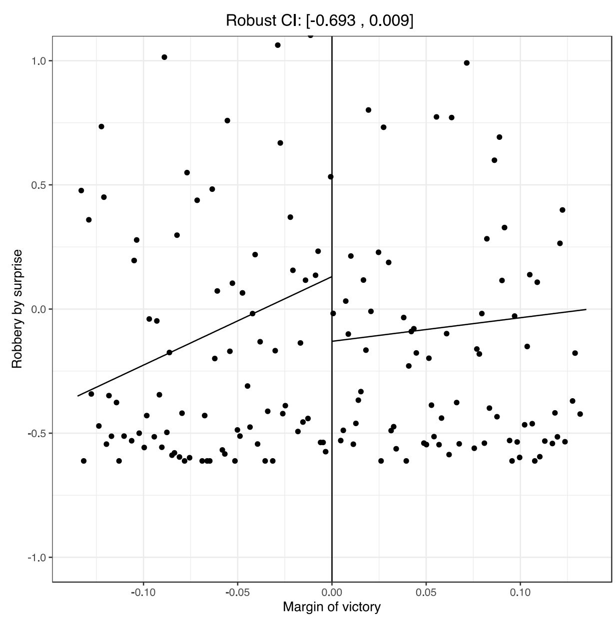
Figure A7: RD plot for political alignment on crimes against the person (MSE-sum bandwidths)



A8.a. Theft



A8.b. Robbery



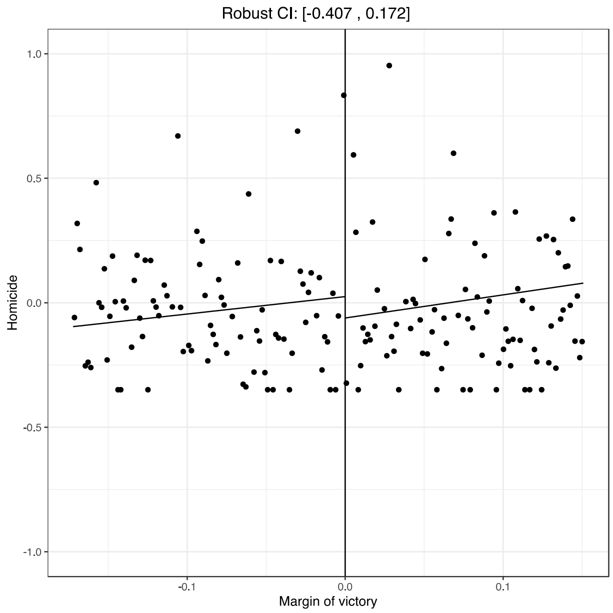
A8.c. Robbery by surprise

Figure A8: Effect of political alignment on street property crime (MSE-sum bandwidth)

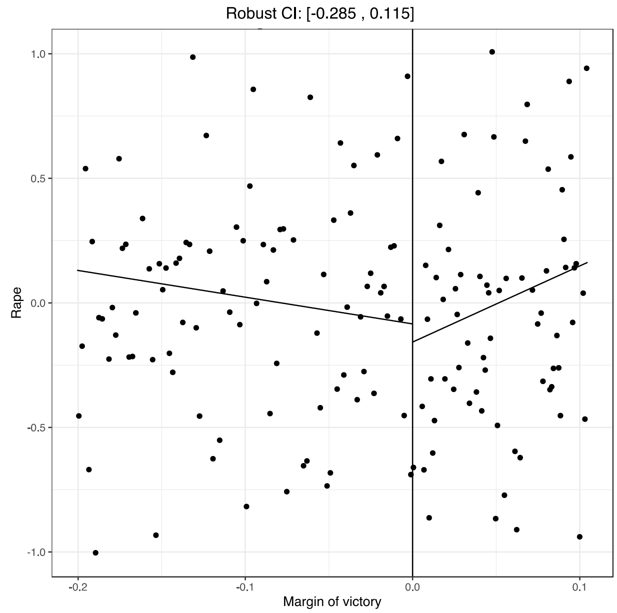
Table A17: Effect of political alignment on crime (MSE-sum bandwidths)

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	MSE-sum bandwidth
Homicide	-0.070	0.427	[-0.407, 0.172]	2099	1309	0.183
Rape	-0.114	0.264	[-0.353, 0.097]	2099	830	0.117
Assault	-0.131	0.242	[-0.433, 0.109]	2099	1007	0.138
Theft	-0.440	0.001	[-0.833, -0.234]	2099	830	0.117
Robbery	-0.301	0.028	[-0.712, -0.041]	2099	927	0.129
Robbery by surprise	-0.262	0.056	[-0.693, 0.009]	2087	967	0.135

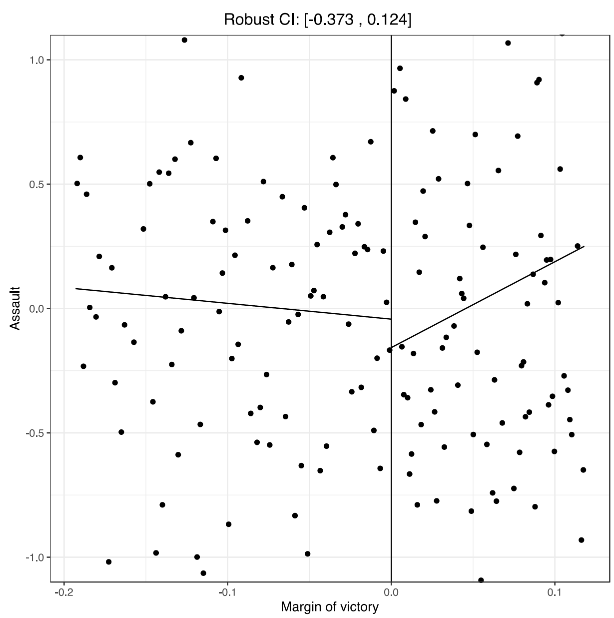
Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.



A9.a. Homicides

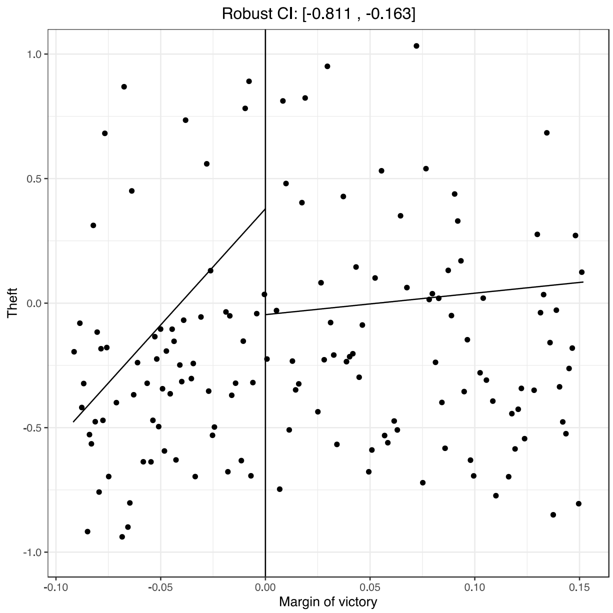


A9.b. Rapes

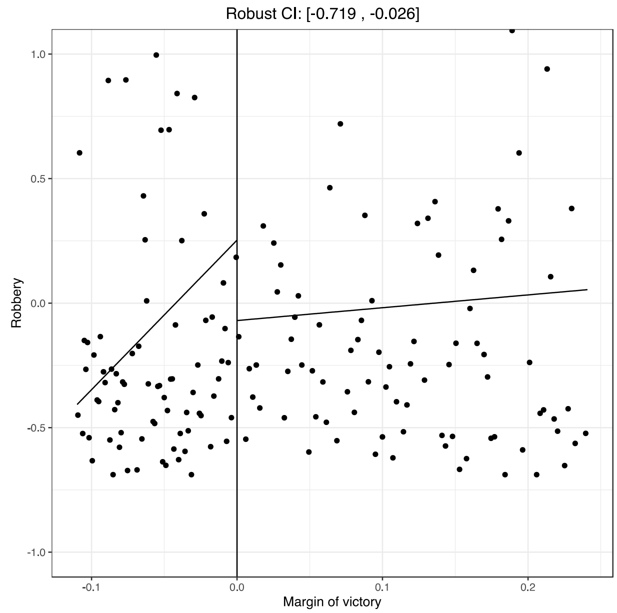


A9.c. Assault

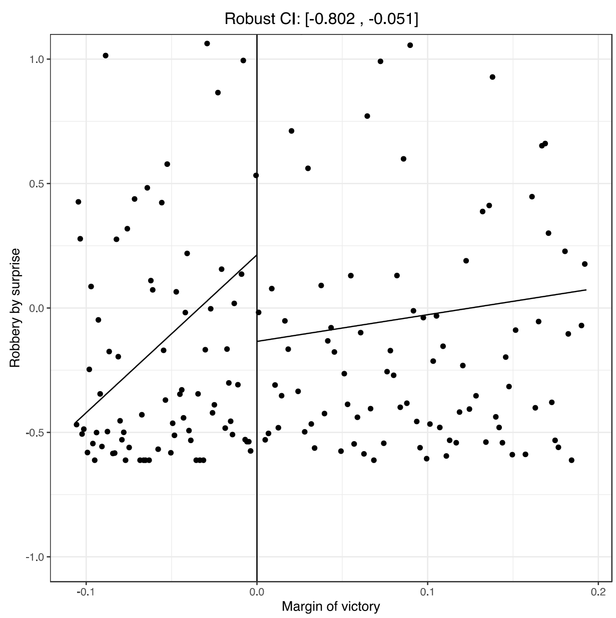
Figure A9: RD plot for political alignment on crimes against the person (Bandwidth = MSE-two selectors)



A10.a. Theft



A10.b. Robbery



A10.c. Robbery by surprise

Figure A10: Effect of political alignment on street property crime (Bandwidth = MSE-two selectors)

Table A18: Effect of political alignment on crime (bandwidth MSE-two selectors)

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	MSE-two selectors bandwidth
Homicide	-0.085	0.403	[-0.427, 0.172]	2099	1163	0.173
Rape	-0.074	0.405	[-0.285, 0.115]	2099	1047	0.202
Assault	-0.113	0.325	[-0.373, 0.124]	2099	1083	0.193
Theft	-0.420	0.003	[-0.811, -0.163]	2099	926	0.093
Robbery	-0.320	0.035	[-0.719, -0.026]	2099	1201	0.112
Robbery by surprise	-0.345	0.026	[-0.802, -0.051]	2087	1072	0.108

Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.

Appendix J: Artificial Cutoffs

We also conduct a falsification test for an effect that we know is absent: the effect of political alignment on crime when using artificial cutoffs. Tables below show that there is not enough evidence to reject the null hypothesis when using one standard deviation below and one standard deviation above the original cutoff (i.e., margin of victory equals to zero) as the new cut point. The standard deviation of the margin of victory is 0.234.

Table A19: Effect of political alignment on crime (Cutoff = 1 SD below original cutoff)

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	MSE bandwidth
Homicides	-0.200	0.269	[-0.660, 0.184]	2099	510	0.146
Rape	0.076	0.539	[-0.216, 0.413]	2099	479	0.138
Assault	0.164	0.149	[-0.064, 0.425]	2099	424	0.124
Theft	-0.144	0.675	[-0.530, 0.343]	2099	323	0.094
Robbery	0.045	0.540	[-0.246, 0.470]	2099	276	0.080
Robbery by surprise	-0.112	0.703	[-0.401, 0.271]	2087	314	0.089

Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.

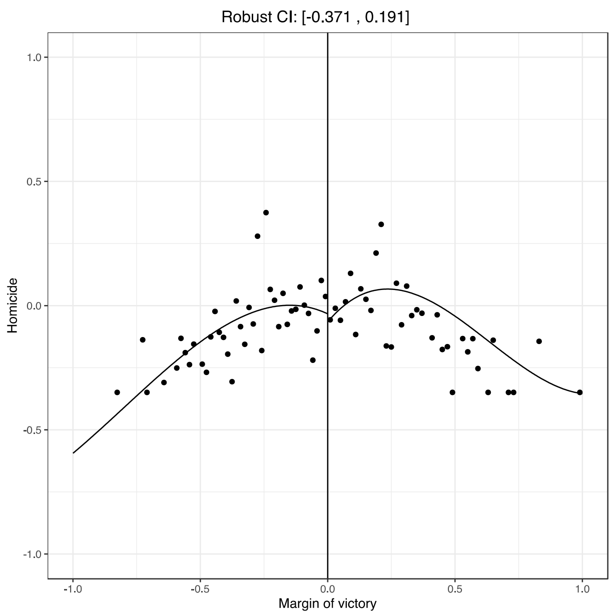
Table A20: Effect of political alignment on crime (Cutoff = 1 SD above original cutoff)

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	MSE bandwidth
Homicides	-0.202	0.055	[-0.395, 0.004]	2099	790	0.154
Rape	-0.271	0.076	[-0.680, 0.033]	2099	639	0.131
Assault	0.051	0.746	[-0.306, 0.427]	2099	519	0.110
Theft	0.104	0.396	[-0.160, 0.406]	2099	800	0.156
Robbery	-0.031	0.724	[-0.351, 0.244]	2099	497	0.106
Robbery by surprise	0.087	0.495	[-0.212, 0.438]	2087	579	0.119

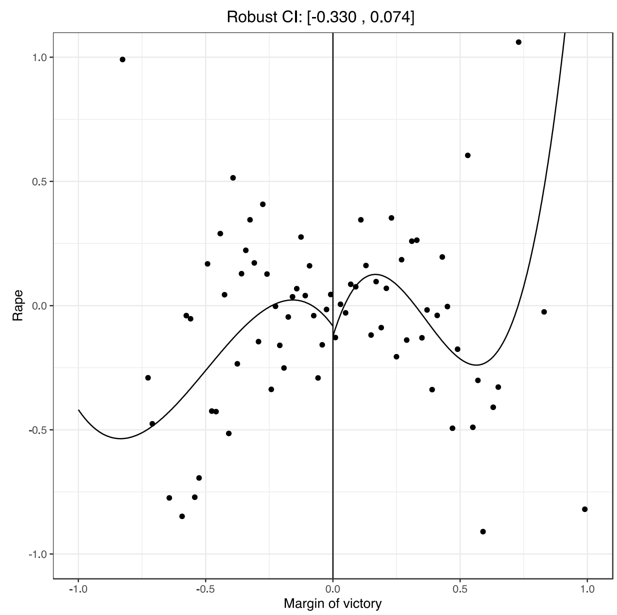
Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.

Appendix K: Global Polynomial

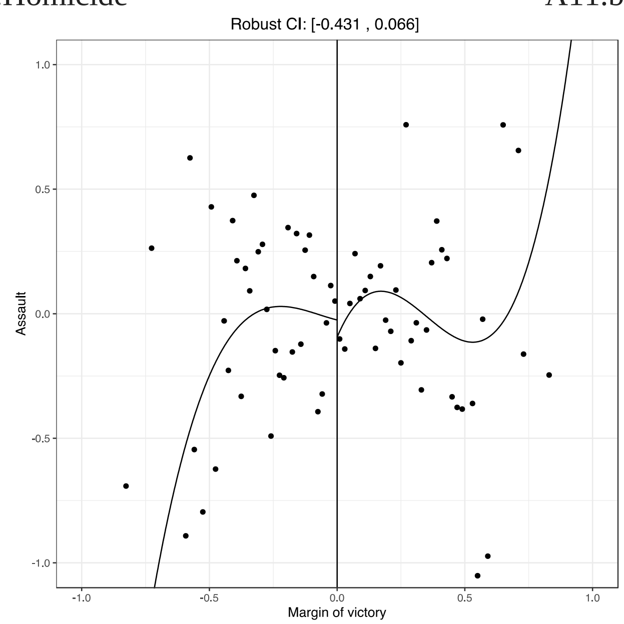
As an additional analysis, we use a global polynomial fit based on a third-order polynomial regression to check the effect of political alignment on our six main outcomes. As we report below, we find the same pattern, that is, there is no impact on crimes against the person but there are clear effects on street property crime.



A11.a.Homicide

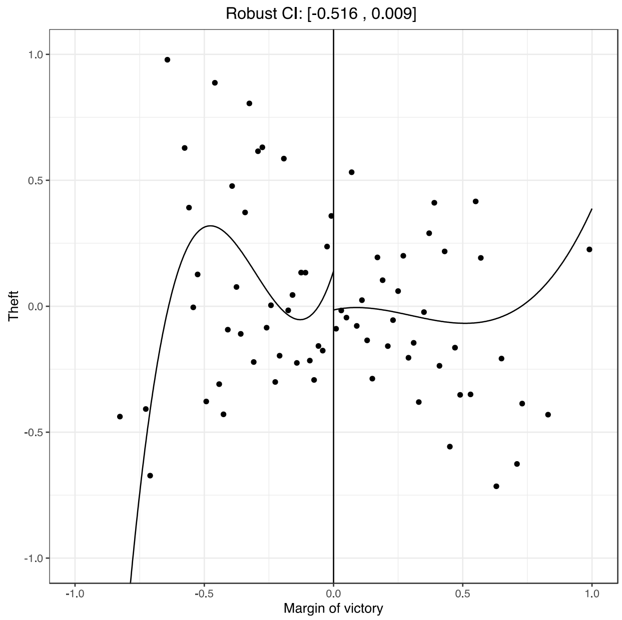


A11.b.Rape

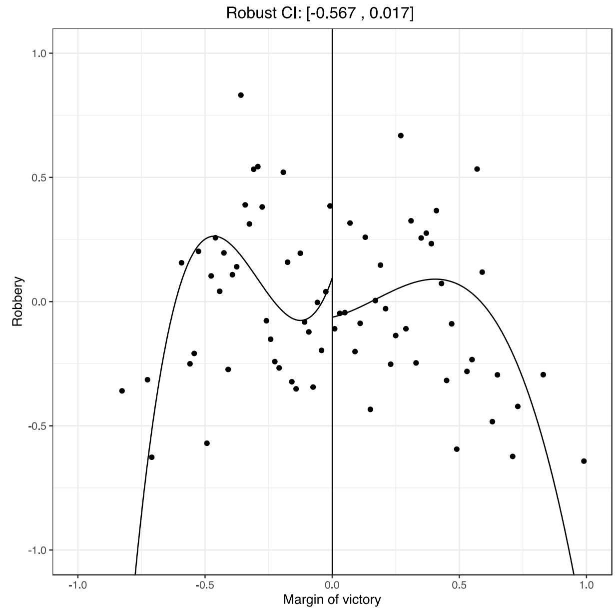


A11.c.Assault

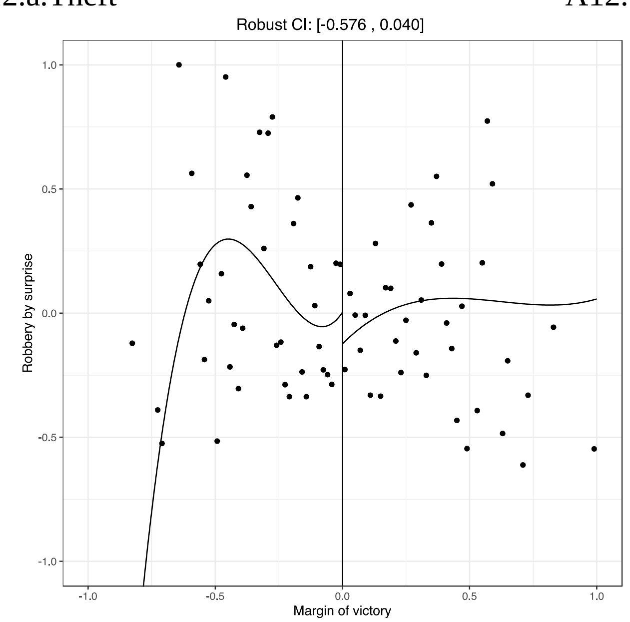
Figure A11: Effect of political alignment on crimes against the person (global polynomial fit based on a third-order polynomial regression)



A12.a.Theft



A12.b.Robbery



A12.c.Robbery by surprise

Figure A12: Effect of political alignment on street property crime (global polynomial fit based on a third-order polynomial regression)

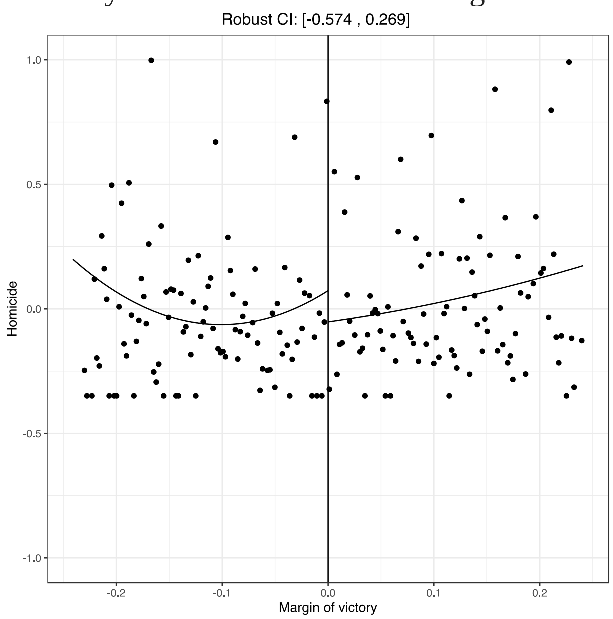
Table A21: Effect of political alignment on crimes (global polynomial)

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	Bandwidth
Homicide	-0.037	0.529	[-0.371, 0.191]	2099	2099	1
Rape	0.023	0.213	[-0.330, 0.074]	2099	2099	1
Assault	0.005	0.150	[-0.431, 0.066]	2099	2099	1
Theft	-0.132	0.058	[-0.516, 0.009]	2099	2099	1
Robbery	-0.145	0.065	[-0.567, 0.017]	2099	2099	1
Robbery by surprise	-0.089	0.088	[-0.576, 0.040]	2087	2099	1

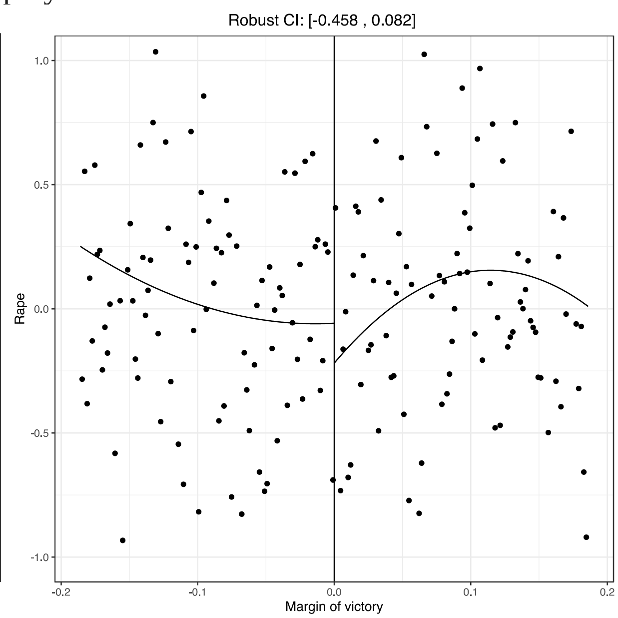
Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.

Appendix L: Local-quadratic and Local-cubic Polynomials

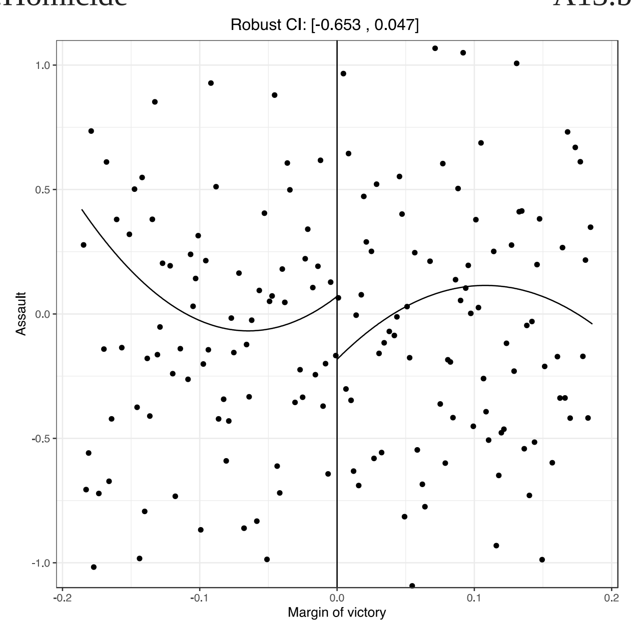
We also use local-quadratic and local-cubic polynomials as robustness checks. The conclusions of our study are not conditional on using different polynomials.



A13a.Homicide

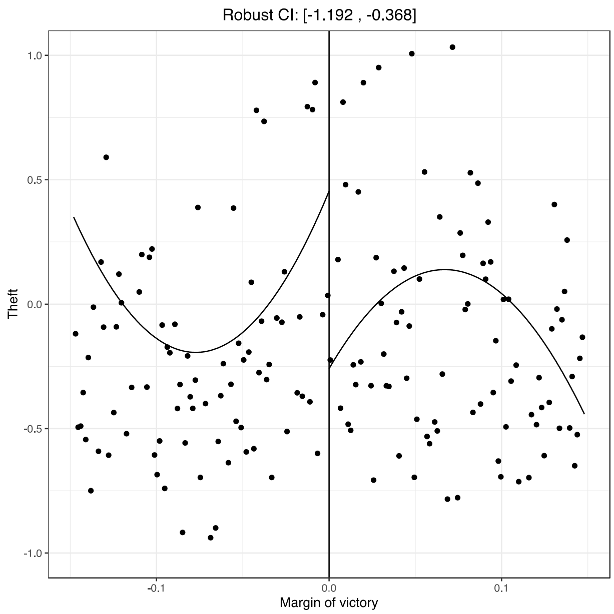


A13.b.Rape

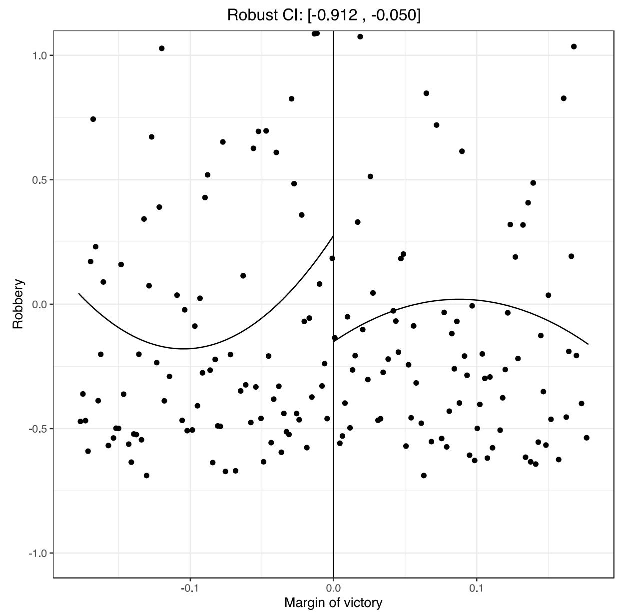


A13.c.Assault

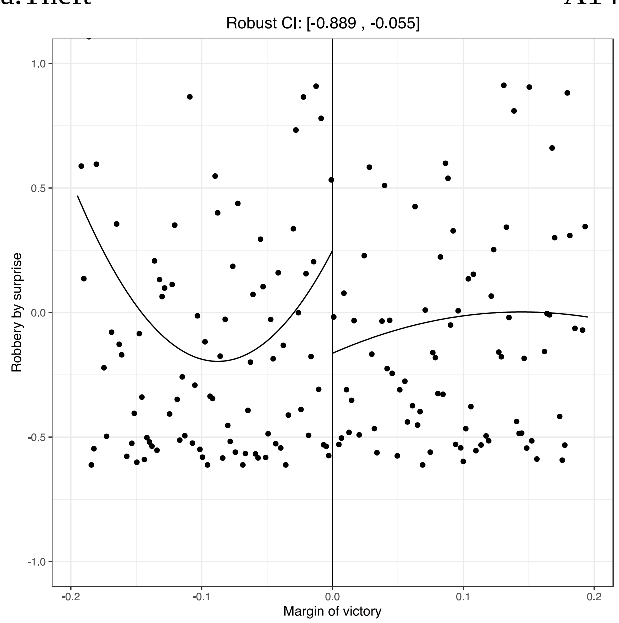
Figure A13: Effect of political alignment on crimes against the person (quadratic polynomial)



A14.a.Theft



A14.b.Robbery



A14.c.Robbery by surprise

Figure A14: Effect of political alignment on street property crime (quadratic polynomial)

Table A22: Effect of political alignment on crime (local quadratic)

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	MSE bandwidth
Homicide	-0.126	0.477	[-0.574, 0.269]	2099	1517	0.241
Rape	-0.160	0.172	[-0.458, 0.082]	2099	1318	0.186
Assault	-0.252	0.090	[-0.653, 0.047]	2099	1318	0.186
Theft	-0.714	0.000	[-1.192, -0.368]	2099	1082	0.148
Robbery	-0.426	0.029	[-0.912, -0.050]	2099	1261	0.178
Robbery by surprise	-0.413	0.027	[-0.889, -0.055]	2087	1334	0.195

Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.

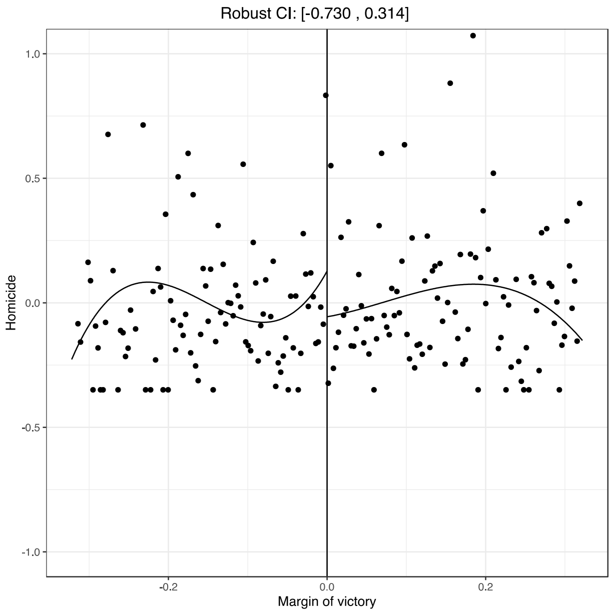


Figure A15.a.Homicide

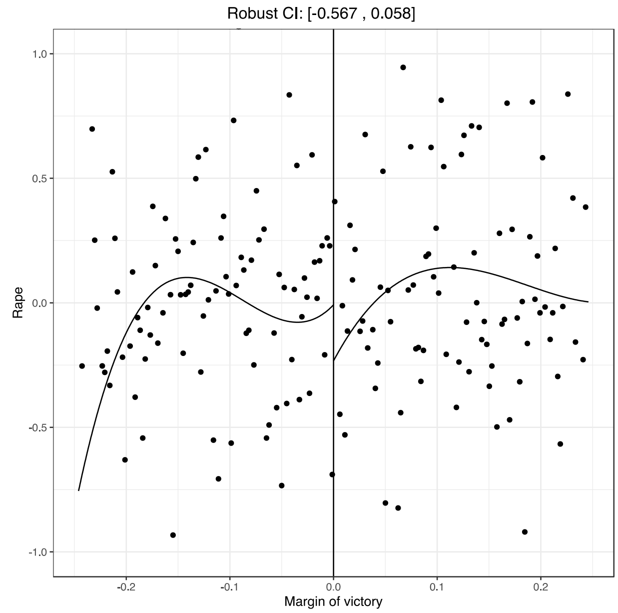


Figure A15.b.Rape

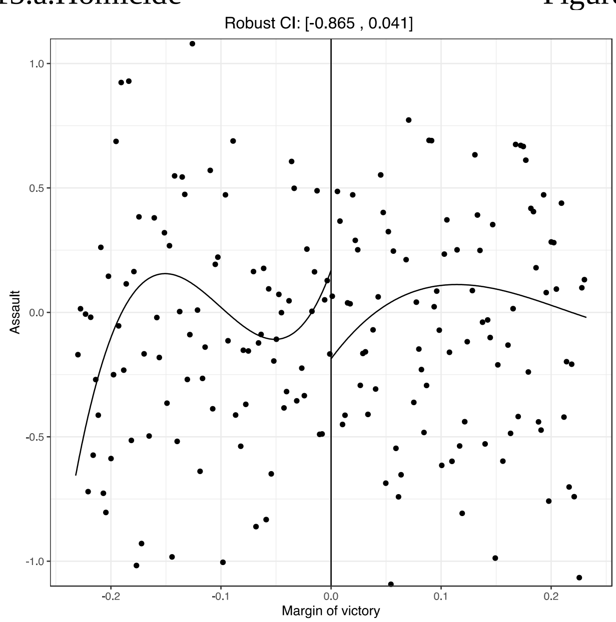
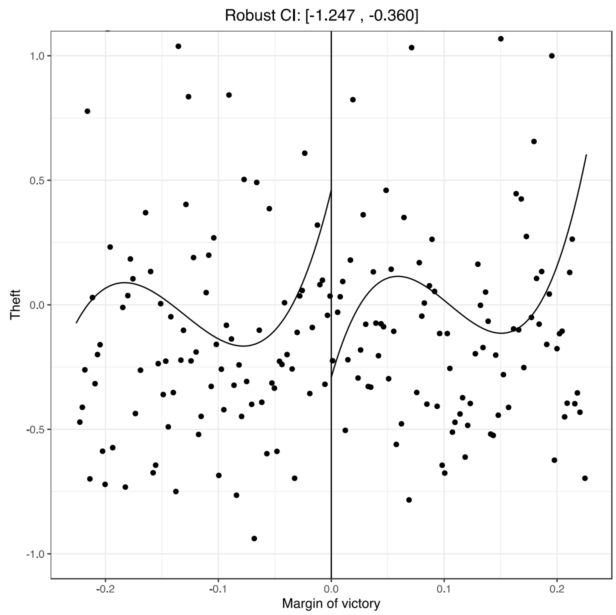
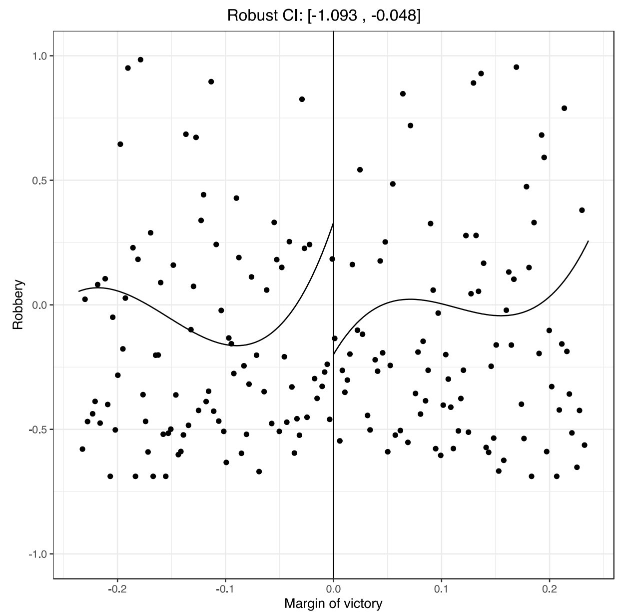


Figure A15.c.Assault

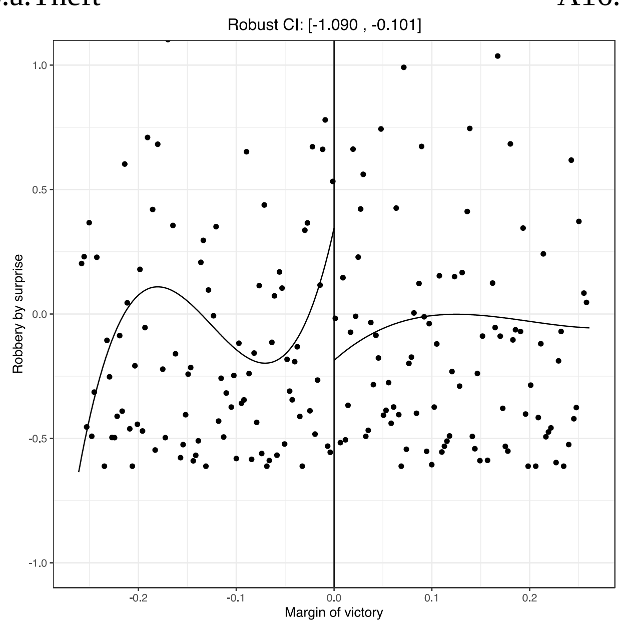
Figure A15: Effect of political alignment on crimes against the person (cubic polynomial)





A16.a.Theft

A16.b.Robbery



A16.c.Robbery by surprise

Figure A16: Effect of political alignment on street property crime (cubic polynomial)

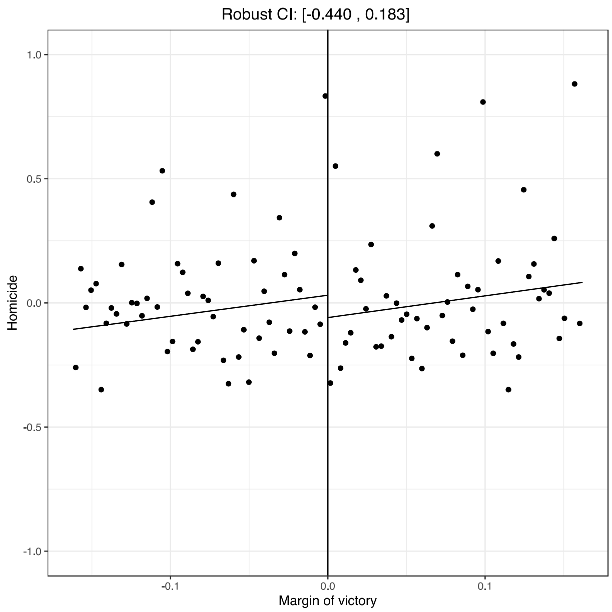
Table A23: Effect of political alignment on crime (local cubic)

	Point Estimate	Robust P-value	Robust 95% Confidence Interval	Overall sample size	Effective sample size	MSE bandwidth
Homicide	-0.183	0.435	[-0.730, 0.314]	2099	1756	0.322
Rape	-0.225	0.110	[-0.567, 0.058]	2099	1540	0.246
Assault	-0.356	0.075	[-0.865, 0.041]	2099	1493	0.232
Theft	-0.753	0.000	[-1.247, -0.360]	2099	1468	0.226
Robbery	-0.530	0.032	[-1.093, -0.048]	2099	1509	0.236
Robbery by surprise	-0.532	0.018	[-1.090, -0.101]	2087	1580	0.261

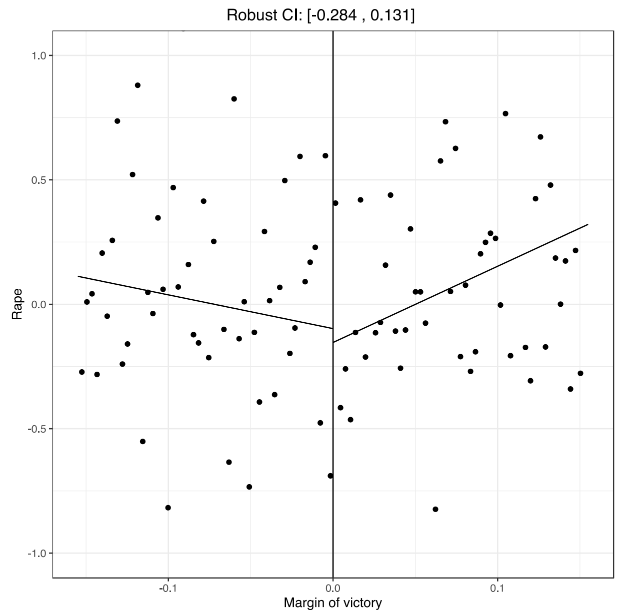
Note: We cluster the standard errors at the municipality-term level (4 years). All outcomes are standardized.

Appendix M: Fewer Bins

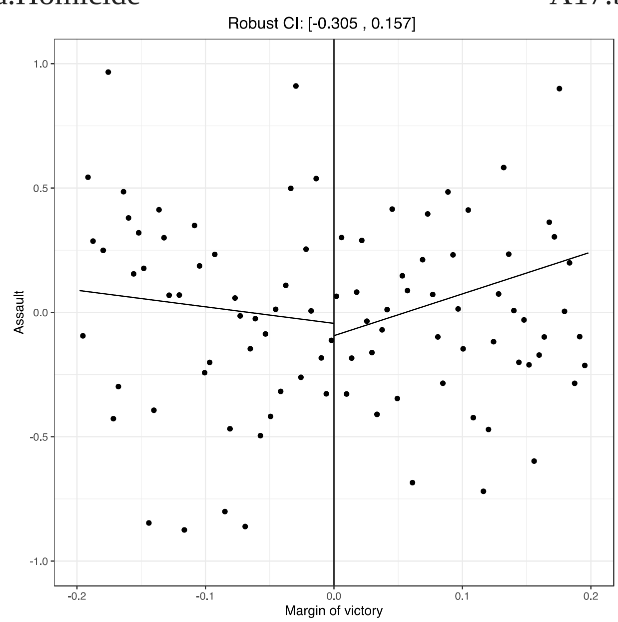
As a final check, we report the main RD plots but using 50 rather than 100 bins.



A17.a.Homicide

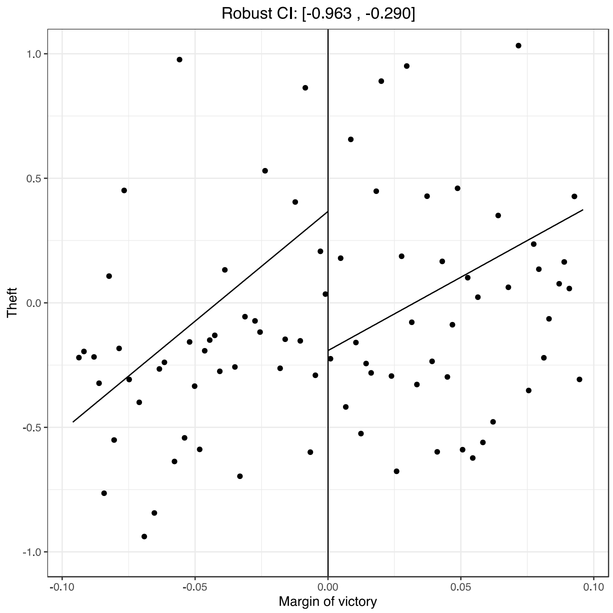


A17.b.Rape

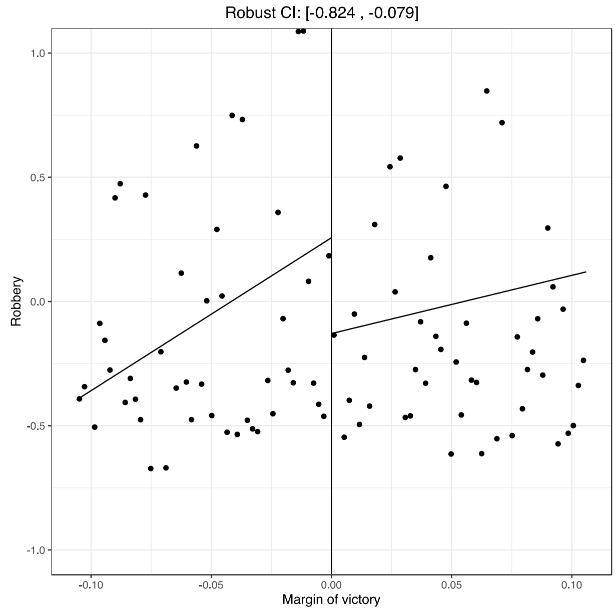


A17.c.Assault

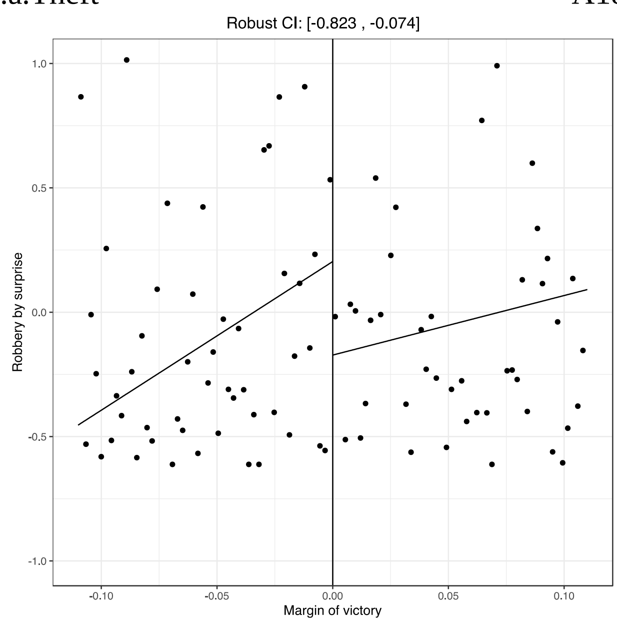
Figure A17: Effect of political alignment on crimes against the person (bins = 50)



A18.a.Theft



A18.b.Robbery



A18.c.Robbery by surprise

Figure A18: Effect of political alignment on street property crime (bins = 50)

Appendix N: Variables Description

Table A24: Description and source of all covariates and outcomes

Variable	**Type**	**Description**	**Source**
Homicide rate	Outcome	Homicides per 100,000 individuals	Centro de estudios y Analisis del Delito
Rape rate	Outcome	Rapes per 100,000 individuals	Centro de estudios y Analisis del Delito
Assault rate	Outcome	Assaults per 100,000 individuals	Centro de estudios y Analisis del Delito
Theft rate	Outcome	Thefts per 100,000 individuals	Centro de estudios y Analisis del Delito
Robbery rate	Outcome	Robbery per 100,000 individuals	Centro de estudios y Analisis del Delito
Robbery by surprise	Outcome	Robbery by surprise per 100,000 individuals	Centro de estudios y Analisis del Delito
Burglary	Outcome	Burglary per 100,000 individuals	Centro de estudios y Analisis del Delito
DTP	Outcome	Disturbing the peace cases per 100,000 individuals	Centro de setudios y Analisis del Delito
DVE	Outcome	Domestic violence against the elderly per 100,000 individuals	Centro de estudios y Analisis del Delito
DVM	Outcome	Domestic violence against men per 100,000 individuals	Centro de estudios y Analisis del Delito
DVW	Outcome	Domestic violence against women per 100,000 individuals	Centro de estudios y Analisis del Delito
DVC	Outcome	Domestic violence against children per 100,000 individuals	Centro de estudios y Analisis del Delito
Social programs	Outcome (causal mechanisms)	Municipal spending in social programs	Sistema Nacional de Información Municipal
Unemployment (2003)	Pretreatment Covariate	Number of people who are not employed and searching for a job.	Caracterización Socioeconómica Nacional, Casen
Wage income (2003)	Pretreatment Covariate	Average household income, which considers income earned from wages and salaries, self-employment, and capital income.	Caracterización Socioeconómica Nacional, Casen

Income inequality (2003)	Pretreatment Covariate	Computed using Theil index, a measure of household income that comprises both the household income and monetary transfers from the state.	Caracterización Socioeconómica Nacional, Casen
Lagged RDF security	Pretreatment Covariate	Regional Development Fund distributed for security purposes, which includes resources to support police activities.	Subsecretaría de Desarrollo Regional (SUBDERE)
Capital	Placebo Covariate	Province capital city	Census data
Region	Placebo Covariate	Region number	Census data
Province	Placebo Covariate	Province number	Census data
Area	Placebo Covariate	Area in km2	Census data
Population (2002)	Pretreatment Covariate	Total population	Census data
Urban (2002)	Pretreatment Covariate	Urban population	United Nations Development Programme
Literacy (2003)	Pretreatment Covariate	Literacy rate	United Nations Development Programme
Income (2003)	Pretreatment Covariate	Income rate	United Nations Development Programme
HDI (2003)	Pretreatment Covariate	Human Development Index	United Nations Development Programme
HDR (2003)	Pretreatment Covariate	Human Development Ranking	United Nations Development Programme
Votes (2000)	Pretreatment Covariate	Total number of votes 2000 presidential election	Servicio Electoral

Right-wing (2000)	Pretreatment Covariate	Vote share right wing candidate 2000 presidential election.	Servicio Electoral
Left-wing (2000)	Pretreatment Covariate	Vote share left wing candidate 2000 presidential election.	Servicio Electoral
Invalid (2000)	Pretreatment Covariate	Invalid votes 2000 presidential election.	Servicio Electoral
Blank (2000)	Pretreatment Covariate	Blank votes 2000 presidential election.	Servicio Electoral
Discretionary funds	Outcome (causal mechanisms)	Discretionary funds distributed by the central goverment	Servicio Electoral
Electricity spending on services provided to the community	Outcome (causal mechanisms)	Spending on electricity for the community	Sistema Nacional de Información Municipal

References

Blattman, Christopher, Donald Green, Daniel Ortega, and Santiago Tobón. 2017. Place-based interventions at scale: The direct and spillover effects of policing and city services on crime. No. w23941. *National Bureau of Economic Research*. Link

Bowers, Kate, and Shane Johnson. 2003. "Measuring the Geographical Displacement and Diffusion of Benefit Effects of Crime Prevention Activity." *Journal of Quantitative Criminology* 19 (3): 275–301.

Cattaneo, Matias D., Nicolás Idrobo, and Rocío Titiunik. 2020. *A practical introduction to regression discontinuity designs: Foundations*. Elements in Quantitative and Computational Methods for the Social Sciences Cambridge University Press.

Evans, William N., and Emily G. Owens. 2007. "COPS and Crime." *Journal of public Economics* 91(1-2): 181-201.

Guerette, Rob, and Kate Bowers. 2009. "Assessing the Extent of Crime Displacement and Diffusion of Benefits: A Review of Situational Crime Prevention Evaluations." *Criminology* 47 (4): 1331–68.

Gould, Eric, Bruce Weinberg, and David Mustard. 2002. "Crime Rate and Local Labor Market Opportunities in the U.S.: 1979–1997." *Review of Economics and Statistics* 84 (1): 45–61.

Holland, Alisha. 2015. "The distributive politics of enforcement." *American Journal of Political Science* 59, no. 2: 357-371.

Holland, Alisha. 2017. *Forbearance as redistribution: The politics of informal welfare in Latin America*. Cambridge University Press.

Levitt, Steven D. 1997. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime." *American Economic Review* 87(3): 270-290.

Lin, Ming-Jen. 2009. "More police, less crime: Evidence from US state data." *International Review of Law and Economics* 29(2): 73-80.

Marvell, Thomas B., and Carlisle E. Moody. 1996. "Specification problems, police levels, and crime rates." *Criminology* 34(4): 609-646.

Mummolo, Jonathan. 2018. Militarization fails to enhance police safety or reduce crime but may harm police reputation. *Proceedings of the National Academy of Sciences* 115 (37): 911-86.

Silber, Jeffrey H., Paul R. Rosenbaum, Richard N. Ross, Orit Even-Shoshan, Rachel R. Kelz, Mark D. Neuman, and Caroline E. Reinke. 2013. "Racial disparities in operative procedure time: the influence of obesity." *Anesthesiology* 119, no. 1: 43-51.

Raphael, Stephen and Rudolf Winter-Ebmer. 2001. “Identifying the Effect of Unemployment on Crime.” *Journal of Law and Economics* 44 (1): 259–84.

Weisburd, David, Elizabeth Groff, and Sue-Ming Yang. 2012. *The Criminology of Place: Street Segments and Our Understanding of the Crime Problem*. New York: Oxford University Press.