Supplemental Appendix for "The Ripple Effect: The Political Consequences of Proximal Contact with Immigration Enforcement"

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1 Summary Statistics

1.1 Full Sample

Table A.1: Summary Statistics (Full Sample)

	N	Mean	SD	Weighted Mean	Weighted SD	Min.	Max.
Protest	1643.00	0.16	0.37	0.12	0.33	0.00	1.00
Proximal Contact	1643.00	0.33	0.47	0.24	0.43	0.00	1.00
Perceived Injustice	1643.00	4.83	2.25	4.34	2.28	0.00	9.00
Woman	1643.00	0.66	0.47	0.52	0.50	0.00	1.00
Foreign	1643.00	0.12	0.33	0.08	0.27	0.00	1.00
Latino	1643.00	0.25	0.43	0.14	0.34	0.00	1.00
Black	1643.00	0.25	0.43	0.15	0.36	0.00	1.00
Asian	1643.00	0.25	0.43	0.04	0.20	0.00	1.00
White	1643.00	0.25	0.43	0.67	0.47	0.00	1.00
Age (18-29)	1643.00	0.26	0.44	0.14	0.34	0.00	1.00
Age (60+)	1643.00	0.20	0.40	0.29	0.46	0.00	1.00
Income (20-39k)	1643.00	0.17	0.38	0.16	0.37	0.00	1.00
Income (40-59k)	1643.00	0.19	0.39	0.19	0.39	0.00	1.00
Income (60-79k)	1643.00	0.16	0.37	0.18	0.39	0.00	1.00
Income (80-99k)	1643.00	0.11	0.32	0.11	0.32	0.00	1.00
Income (100-150k)	1643.00	0.16	0.36	0.18	0.39	0.00	1.00
Education (High School)	1643.00	0.18	0.38	0.18	0.39	0.00	1.00
Education (post-HS)	1643.00	0.80	0.40	0.80	0.40	0.00	1.00
Democrat	1643.00	0.49	0.50	0.42	0.49	0.00	1.00
Independent	1643.00	0.25	0.43	0.27	0.44	0.00	1.00
Republican	1643.00	0.23	0.42	0.29	0.46	0.00	1.00
Other Party	1643.00	0.03	0.16	0.02	0.13	0.00	1.00

1.2 Racial Sub-samples

Table A.2: Summary Statistics (White Sample)

	N	Mean	SD	Weighted Mean	Weighted SD	Min.	Max.
Protest	413.00	0.09	0.29	0.08	0.28	0.00	1.00
Proximal Contact	413.00	0.20	0.40	0.18	0.39	0.00	1.00
Perceived Injustice	413.00	3.94	2.18	3.90	2.18	0.00	9.00
Woman	413.00	0.62	0.48	0.51	0.50	0.00	1.00
Foreign	413.00	0.02	0.15	0.03	0.16	0.00	1.00
Age (18-29)	413.00	0.09	0.28	0.09	0.28	0.00	1.00
Age (60+)	413.00	0.43	0.50	0.37	0.48	0.00	1.00
Income (20-39k)	413.00	0.15	0.36	0.15	0.35	0.00	1.00
Income (40-59k)	413.00	0.19	0.39	0.18	0.39	0.00	1.00
Income (60-79k)	413.00	0.18	0.39	0.19	0.39	0.00	1.00
Income (80-99k)	413.00	0.12	0.32	0.12	0.32	0.00	1.00
Income (100-150k)	413.00	0.20	0.40	0.21	0.41	0.00	1.00
Education (High School)	413.00	0.15	0.36	0.17	0.37	0.00	1.00
Education (post-HS)	413.00	0.84	0.37	0.83	0.38	0.00	1.00
Democrat	413.00	0.37	0.48	0.33	0.47	0.00	1.00
Independent	413.00	0.29	0.45	0.29	0.45	0.00	1.00
Republican	413.00	0.33	0.47	0.37	0.48	0.00	1.00
Other Party	413.00	0.01	0.11	0.01	0.11	0.00	1.00

Table A.3: Summary Statistics (Latino Sample)

	N	Mean	SD	Weighted Mean	Weighted SD	Min.	Max.
Protest	413.00	0.21	0.41	0.23	0.42	0.00	1.00
Proximal Contact	413.00	0.53	0.50	0.52	0.50	0.00	1.00
Perceived Injustice	413.00	4.93	2.36	5.01	2.33	0.00	9.00
Woman	413.00	0.70	0.46	0.53	0.50	0.00	1.00
Foreign	413.00	0.13	0.34	0.28	0.45	0.00	1.00
Age (18-29)	413.00	0.35	0.48	0.23	0.42	0.00	1.00
Age (60+)	413.00	0.09	0.29	0.11	0.32	0.00	1.00
Income (20-39k)	413.00	0.21	0.41	0.19	0.39	0.00	1.00
Income (40-59k)	413.00	0.21	0.41	0.19	0.39	0.00	1.00
Income (60-79k)	413.00	0.16	0.37	0.21	0.41	0.00	1.00
Income (80-99k)	413.00	0.11	0.31	0.11	0.32	0.00	1.00
Income (100-150k)	413.00	0.14	0.35	0.14	0.35	0.00	1.00
Education (High School)	413.00	0.21	0.41	0.20	0.40	0.00	1.00
Education (post-HS)	413.00	0.75	0.43	0.72	0.45	0.00	1.00
Democrat	413.00	0.49	0.50	0.59	0.49	0.00	1.00
Independent	413.00	0.23	0.42	0.19	0.40	0.00	1.00
Republican	413.00	0.25	0.43	0.19	0.40	0.00	1.00
Other Party	413.00	0.03	0.18	0.02	0.14	0.00	1.00

Table A.4: Summary Statistics (Black Sample)

	N	Mean	SD	Weighted Mean	Weighted SD	Min.	Max.
Protest	406.00	0.21	0.41	0.19	0.39	0.00	1.00
Proximal Contact	406.00	0.30	0.46	0.27	0.44	0.00	1.00
Perceived Injustice	406.00	5.61	2.21	5.56	2.18	0.00	9.00
Woman	406.00	0.70	0.46	0.56	0.50	0.00	1.00
Foreign	406.00	0.03	0.17	0.02	0.15	0.00	1.00
Age (18-29)	406.00	0.37	0.48	0.23	0.42	0.00	1.00
Age (60+)	406.00	0.13	0.33	0.14	0.35	0.00	1.00
Income (20-39k)	406.00	0.20	0.40	0.20	0.40	0.00	1.00
Income (40-59k)	406.00	0.22	0.41	0.20	0.40	0.00	1.00
Income (60-79k)	406.00	0.13	0.34	0.13	0.34	0.00	1.00
Income (80-99k)	406.00	0.09	0.28	0.10	0.30	0.00	1.00
Income (100-150k)	406.00	0.09	0.28	0.11	0.31	0.00	1.00
Education (High School)	406.00	0.26	0.44	0.27	0.44	0.00	1.00
Education (post-HS)	406.00	0.71	0.45	0.70	0.46	0.00	1.00
Democrat	406.00	0.67	0.47	0.66	0.47	0.00	1.00
Independent	406.00	0.21	0.41	0.21	0.41	0.00	1.00
Republican	406.00	0.09	0.29	0.10	0.30	0.00	1.00
Other Party	406.00	0.03	0.18	0.03	0.17	0.00	1.00

Table A.5: Summary Statistics (Asian Sample)

	N	Mean	SD	Weighted Mean	Weighted SD	Min.	Max.
Protest	411.00	0.12	0.33	0.12	0.32	0.00	1.00
Proximal Contact	411.00	0.29	0.46	0.28	0.45	0.00	1.00
Perceived Injustice	411.00	4.86	1.92	4.88	1.89	0.00	9.00
Woman	411.00	0.62	0.49	0.50	0.50	0.00	1.00
Foreign	411.00	0.31	0.46	0.49	0.50	0.00	1.00
Age (18-29)	411.00	0.25	0.43	0.23	0.42	0.00	1.00
Age (60+)	411.00	0.15	0.36	0.17	0.38	0.00	1.00
Income (20-39k)	411.00	0.13	0.33	0.15	0.35	0.00	1.00
Income (40-59k)	411.00	0.13	0.33	0.13	0.34	0.00	1.00
Income (60-79k)	411.00	0.17	0.37	0.15	0.36	0.00	1.00
Income (80-99k)	411.00	0.13	0.34	0.12	0.33	0.00	1.00
Income (100-150k)	411.00	0.20	0.40	0.18	0.39	0.00	1.00
Education (High School)	411.00	0.09	0.28	0.09	0.29	0.00	1.00
Education (post-HS)	411.00	0.91	0.29	0.90	0.30	0.00	1.00
Democrat	411.00	0.45	0.50	0.50	0.50	0.00	1.00
Independent	411.00	0.29	0.45	0.28	0.45	0.00	1.00
Republican	411.00	0.24	0.42	0.20	0.40	0.00	1.00
Other Party	411.00	0.03	0.17	0.03	0.17	0.00	1.00

2 Regression Tables of Main Results

2.1 Association Between Proximal Contact and Perceived Injustice (Figures 1, 2)

Table A.6: Association Between Proximate Contact and Perceptions of Injustice by Race Subsets (Characterizing Figure 1)

	Full	Latino	White	Black	Asian
(Intercept)	3.53^{***}	3.63^{***}	4.16^{***}	4.81^{***}	4.54^{***}
	(0.43)	(0.71)	(0.46)	(0.91)	(0.77)
Prox. Contact	0.60^{***}	0.78^{**}	0.71^{**}	0.24	0.32
	(0.16)	(0.26)	(0.26)	(0.25)	(0.24)
Age (18-29)	-0.11	0.15	-0.22	-0.07	0.33
	(0.20)	(0.30)	(0.38)	(0.25)	(0.30)
Age (60+)	-0.75^{***}	-0.70	-0.82^{***}	-0.28	-0.12
	(0.18)	(0.48)	(0.21)	(0.37)	(0.33)
Income (20-39k)	0.47^{*}	-0.48	0.93^{**}	0.05	-0.19
	(0.22)	(0.42)	(0.34)	(0.35)	(0.39)
Income (40-59k)	-0.12	0.14	-0.20	0.05	-0.04
	(0.24)	(0.37)	(0.37)	(0.36)	(0.33)
Income (60-79k)	0.52^{*}	0.30	0.70	0.16	0.20
	(0.25)	(0.42)	(0.36)	(0.40)	(0.32)
Income (80-99k)	-0.12	-0.35	-0.13	0.53	0.01
	(0.27)	(0.50)	(0.38)	(0.48)	(0.39)
Income (100-150k)	-0.22	0.04	-0.27	0.49	-0.40
	(0.25)	(0.56)	(0.35)	(0.54)	(0.33)
Educ. (HS)	-0.40	-0.01	-1.04^{**}	-0.71	-0.78
	(0.36)	(0.59)	(0.36)	(0.74)	(0.79)
Educ. (Post-HS)	-0.44	-0.39	-1.15^{***}	-0.42	-0.28
	(0.34)	(0.54)	(0.33)	(0.72)	(0.72)
Dem.	1.70^{***}	1.59^{***}	1.81^{***}	1.25^{**}	1.04^{***}
	(0.18)	(0.33)	(0.24)	(0.46)	(0.26)
Ind.	0.74^{***}	0.46	0.66^{*}	1.12^{*}	0.52
	(0.21)	(0.44)	(0.26)	(0.53)	(0.29)
Other Party	0.68	0.10	1.44	-0.40	0.41
	(0.46)	(0.85)	(0.74)	(0.64)	(0.66)
Female	0.21	0.48	0.18	0.12	0.12
	(0.15)	(0.29)	(0.21)	(0.26)	(0.22)
Foreign	0.01	0.18	-0.10	-1.28^{**}	-0.28
	(0.22)	(0.35)	(0.53)	(0.41)	(0.22)
Latinx	0.26				
	(0.18)				
Black	0.91^{***}				
	(0.18)				
AAPI	0.51^{**}				
	(0.18)				
R^2	0.24	0.18	0.24	0.07	0.10
Adj. R^2	0.23	0.15	0.22	0.03	0.06
Num. obs.	1643	413	413	406	411
RMSE	2.46	1.95	3.89	2.08	0.92

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.2 Association Between Proximal Contact and Voting (Figures 3, 4A)

Table A.7: Association Between Proximate Contact and Self-Reported Vote Intention by Race Subsets (Characterizing Figure 2, Panel A)

	Full	Latino	White	Black	Asian
(Intercept)	0.48***	0.27	0.39	0.23	−0.07
	(0.11)	(0.18)	(0.20)	(0.21)	(0.12)
Proximate Contact	−0.04	−0.04	−0.06	−0.00	0.03
	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Unfair Scale	0.00	0.00	−0.01	0.03*	0.03*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Age (18-29)	−0.23***	−0.14*	−0.28**	−0.25***	−0.13*
	(0.05)	(0.07)	(0.10)	(0.06)	(0.06)
Age (60+)	0.12**	0.04	0.11*	0.17*	0.30***
	(0.04)	(0.11)	(0.05)	(0.07)	(0.07)
Income (20-39k)	−0.10	−0.01	−0.15	−0.04	0.07
	(0.05)	(0.10)	(0.09)	(0.08)	(0.08)
Income (40-59k)	−0.07	0.05	−0.14	0.09	0.06
	(0.05)	(0.10)	(0.07)	(0.07)	(0.10)
Income (60-79k)	0.01	−0.07	0.00	0.10	−0.07
	(0.05)	(0.11)	(0.07)	(0.09)	(0.08)
Income (80-99k)	0.06	0.13	−0.01	0.32***	0.06
	(0.05)	(0.13)	(0.07)	(0.09)	(0.09)
Income (100-150k)	−0.05	−0.13	−0.10	0.23*	0.06
	(0.05)	(0.11)	(0.07)	(0.10)	(0.08)
Educ. (HS)	0.16	0.17	0.32	0.16	0.31**
	(0.10)	(0.15)	(0.19)	(0.16)	(0.10)
Educ. (Post-HS)	0.30**	0.30*	0.50**	0.13	0.47***
	(0.10)	(0.14)	(0.19)	(0.16)	(0.08)
Dem.	0.01	0.10	0.04	0.02	−0.12
	(0.04)	(0.08)	(0.06)	(0.10)	(0.07)
Ind.	−0.17***	−0.09	−0.18**	−0.06	−0.24**
	(0.05)	(0.08)	(0.06)	(0.11)	(0.08)
Other Party	−0.33*	0.09	−0.44	−0.30*	−0.19
	(0.13)	(0.19)	(0.25)	(0.15)	(0.22)
Female	−0.06	−0.02	−0.09	0.03	0.00
	(0.03)	(0.06)	(0.05)	(0.06)	(0.05)
Foreign	−0.13*	−0.10	−0.14	−0.14	−0.05
	(0.06)	(0.08)	(0.14)	(0.16)	(0.05)
Latinx	−0.07				
	(0.04)				
Black	−0.04				
	(0.04)				
AAPI	−0.12**				
	(0.05)				
R^2	0.16	0.11	0.18	0.18	0.13
Adj. R^2	0.15	0.07	0.14	0.15	0.09
Num. obs.	1643	413	413	406	411
RMSE	0.56	0.44	0.88	0.45	0.24

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.3 Association Between Proximal Contact and Protest (Figures 3, 4B)

Table A.8: Association Between Proximate Contact and Self-Reported Protest Behavior by Race Subsets (Characterizing Figure 2, Panel B)

	Full	Latino	White	Black	Asian
(Intercept)	0.13	0.16	−0.09	−0.06	0.54
	(0.10)	(0.16)	(0.09)	(0.10)	(0.33)
Proximate Contact	0.19***	0.23***	0.14**	0.29***	0.18***
	(0.03)	(0.05)	(0.05)	(0.05)	(0.04)
Unfair Scale	0.01**	0.02	0.01	0.02*	0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Age (18-29)	0.01	0.01	−0.03	0.05	0.09
	(0.03)	(0.06)	(0.05)	(0.05)	(0.05)
Age (60+)	−0.03	0.00	−0.02	−0.07	−0.08**
	(0.02)	(0.06)	(0.02)	(0.04)	(0.03)
Income (20-39k)	0.00	0.16	−0.02	−0.06	−0.07
	(0.03)	(0.08)	(0.05)	(0.04)	(0.04)
Income (40-59k)	0.01	0.14	−0.03	0.09	−0.04
	(0.03)	(0.07)	(0.05)	(0.06)	(0.05)
Income (60-79k)	0.04	0.13	0.00	0.09	−0.00
	(0.04)	(0.07)	(0.05)	(0.06)	(0.05)
Income (80-99k)	0.07	0.20*	0.04	0.08	0.04
	(0.04)	(0.08)	(0.06)	(0.08)	(0.06)
Income (100-150k)	−0.01	0.23*	−0.07	0.12	0.00
	(0.03)	(0.10)	(0.04)	(0.08)	(0.05)
Educ. (HS)	−0.18	−0.26	0.05	0.11	−0.50
	(0.10)	(0.14)	(0.08)	(0.08)	(0.32)
Educ. (Post-HS)	−0.14	−0.28*	0.11	0.12	−0.51
	(0.10)	(0.13)	(0.08)	(0.07)	(0.32)
Dem.	0.04	0.05	0.09*	−0.15*	−0.00
	(0.03)	(0.06)	(0.04)	(0.07)	(0.04)
Ind.	−0.04	0.03	−0.04	−0.16*	−0.04
	(0.02)	(0.09)	(0.03)	(0.07)	(0.04)
Other Party	−0.06	−0.04	−0.04	−0.17	−0.09*
	(0.03)	(0.10)	(0.04)	(0.11)	(0.04)
Female	0.01	−0.08	0.02	0.09*	0.03
	(0.02)	(0.06)	(0.03)	(0.04)	(0.03)
Foreign	−0.07*	−0.04	−0.13**	0.17	−0.01
	(0.03)	(0.07)	(0.04)	(0.11)	(0.03)
Latinx	0.04				
	(0.03)				
Black	0.04				
	(0.03)				
AAPI	0.02				
	(0.03)				
R^2	0.15	0.18	0.14	0.20	0.16
Adj. R^2	0.14	0.15	0.11	0.17	0.12
Num. obs.	1643	413	413	406	411
RMSE	0.37	0.35	0.53	0.35	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.4 Association Between Proximal Contact x Perceived Injustice and Voting

Table A.9: Interaction Model — Self-Reported Vote Intention by Race Subsets (Characterizing Figure 3)

	Full	Latino	White	Black	Asian
(Intercept)	0.49^{***}	0.30	0.37	0.30	-0.07
	(0.11)	(0.20)	(0.20)	(0.20)	(0.13)
Proximate Contact	-0.06	-0.11	0.04	-0.39^{*}	0.03
	(0.08)	(0.14)	(0.11)	(0.19)	(0.17)
Unfair Scale	-0.00	-0.00	-0.01	0.01	0.03
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Age (18-29)	-0.23^{***}	-0.14^{*}	-0.28^{**}	-0.25^{***}	-0.13^{*}
	(0.05)	(0.07)	(0.10)	(0.06)	(0.06)
Age (60+)	0.12^{**}	0.03	0.11^{*}	0.17^{*}	0.30^{***}
	(0.04)	(0.11)	(0.05)	(0.07)	(0.07)
Income (20-39k)	-0.10	-0.01	-0.15	-0.05	0.07
	(0.05)	(0.10)	(0.09)	(0.08)	(0.08)
Income (40-59k)	-0.07	0.05	-0.14	0.10	0.06
	(0.05)	(0.10)	(0.07)	(0.07)	(0.10)
Income (60-79k)	0.01	-0.07	0.00	0.12	-0.07
	(0.05)	(0.11)	(0.07)	(0.09)	(0.08)
Income (80-99k)	0.06	0.14	-0.01	0.32^{***}	0.06
	(0.05)	(0.13)	(0.07)	(0.08)	(0.09)
Income (100-150k)	-0.05	-0.13	-0.10	0.25^{*}	0.06
	(0.05)	(0.11)	(0.07)	(0.10)	(0.08)
Educ. (HS)	0.16	0.17	0.33	0.16	0.31^{**}
	(0.10)	(0.15)	(0.19)	(0.15)	(0.10)
Educ. (Post-HS)	0.30^{**}	0.30^{*}	0.50^{**}	0.12	0.47^{***}
	(0.10)	(0.15)	(0.19)	(0.15)	(0.08)
Dem.	0.01	0.10	0.04	0.02	-0.12
	(0.04)	(0.08)	(0.06)	(0.10)	(0.07)
Ind.	-0.17^{***}	-0.09	-0.18^{**}	-0.04	-0.24^{**}
	(0.05)	(0.09)	(0.06)	(0.11)	(0.08)
Other Party	-0.33^{*}	0.09	-0.44	-0.31^{*}	-0.19
	(0.13)	(0.19)	(0.25)	(0.15)	(0.22)
Female	-0.06	-0.02	-0.09	0.02	0.00
	(0.03)	(0.06)	(0.05)	(0.06)	(0.05)
Foreign	-0.13^{*}	-0.10	-0.14	-0.14	-0.05
	(0.06)	(0.08)	(0.14)	(0.16)	(0.05)
Latinx	-0.07				
	(0.04)				
Black	-0.04				
	(0.04)				
AAPI	-0.12^{**}				
	(0.05)				
Prox. Contact x Unfair Scale	0.00	0.01	-0.02	0.07^{*}	-0.00
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
R^2	0.16	0.11	0.18	0.20	0.13
Adj. R^2	0.15	0.07	0.14	0.16	0.09
Num. obs.	1643	413	413	406	411
RMSE	0.56	0.44	0.88	0.44	0.24

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.5 Association Between Proximal Contact x Perceived Injustice and Protest (Figure 5)

Table A.10: Interaction Model — Self-Reported Protest by Race Subsets (Characterizing Figure 4)

	Full	Latino	White	Black	Asian
(Intercept)	0.16	0.19	−0.06	−0.06	0.59
	(0.10)	(0.15)	(0.09)	(0.10)	(0.32)
Proximate Contact	0.05	0.15	−0.01	0.26	0.02
	(0.05)	(0.10)	(0.08)	(0.16)	(0.11)
Unfair Scale	0.01	0.01	0.01	0.02	−0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Age (18-29)	0.01	0.01	−0.02	0.05	0.09
	(0.03)	(0.06)	(0.05)	(0.05)	(0.05)
Age (60+)	−0.03	−0.01	−0.02	−0.07	-0.08^{**}
	(0.02)	(0.06)	(0.02)	(0.04)	(0.03)
Income (20-39k)	0.00	0.16^{*}	−0.02	−0.06	−0.07
	(0.03)	(0.08)	(0.05)	(0.04)	(0.05)
Income (40-59k)	0.02	0.14	−0.03	0.09	−0.04
	(0.03)	(0.07)	(0.05)	(0.06)	(0.05)
Income (60-79k)	0.04	0.13	0.00	0.09	−0.01
	(0.04)	(0.07)	(0.05)	(0.06)	(0.05)
Income (80-99k)	0.07	0.20^{*}	0.04	0.08	0.03
	(0.04)	(0.08)	(0.06)	(0.08)	(0.06)
Income (100-150k)	−0.01	0.23^{*}	−0.07	0.12	−0.00
	(0.03)	(0.10)	(0.04)	(0.08)	(0.05)
Educ. (HS)	−0.19	−0.26	0.04	0.11	−0.51
	(0.10)	(0.14)	(0.08)	(0.08)	(0.32)
Educ. (Post-HS)	−0.14	-0.28^{*}	0.11	0.12	−0.52
	(0.10)	(0.13)	(0.08)	(0.07)	(0.31)
Dem.	0.05	0.05	0.10^{*}	-0.15^{*}	−0.00
	(0.03)	(0.06)	(0.04)	(0.07)	(0.04)
Ind.	−0.04	0.03	−0.03	-0.16^{*}	−0.03
	(0.02)	(0.09)	(0.03)	(0.07)	(0.04)
Other Party	−0.06	−0.04	−0.04	−0.17	-0.10^{*}
	(0.04)	(0.10)	(0.04)	(0.11)	(0.04)
Female	0.01	−0.08	0.02	0.09^{*}	0.03
	(0.02)	(0.06)	(0.03)	(0.04)	(0.03)
Foreign	-0.07^{*}	−0.04	-0.13^{**}	0.17	−0.01
	(0.03)	(0.06)	(0.04)	(0.11)	(0.03)
Latinx	0.04				
	(0.03)				
Black	0.04				
	(0.03)				
AAPI	0.02				
	(0.03)				
Prox. Contact x Unfair Scale	0.03^{*}	0.02	0.03	0.01	0.03
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)
R^2	0.15	0.19	0.16	0.20	0.16
Adj. R^2	0.14	0.15	0.12	0.17	0.13
Num. obs.	1643	413	413	406	411
RMSE	0.37	0.35	0.52	0.35	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.6 Vote Intention Model With Continuous Measure

Here, instead of using the binary vote outcome that we construct for those who are 100% likely to vote in the 2018 midterm election, we use the underlying continuous measure from 0 (definitely do not want to vote) to 10 (definitely do want to vote). We find results consistent with the binary measure in that proximal contact and the interaction between proximal contact with perceived injustice are not associated with voting behavior.

Table A.11: Association Between Proximate Contact and Self-Reported Vote Intention by Race Subsets (Continuous Measure)

	Full	Latino	White	Black	Asian
(Intercept)	7.66***	7.01***	6.62***	6.74***	4.74
	(0.54)	(0.93)	(0.84)	(0.89)	(2.59)
Proximate Contact	−0.02	0.21	−0.14	0.00	−0.16
	(0.17)	(0.31)	(0.25)	(0.32)	(0.30)
Unfair Scale	−0.03	0.06	−0.08	0.08	0.10
	(0.04)	(0.07)	(0.06)	(0.07)	(0.07)
Age (18-29)	−1.32***	−0.97**	−1.47**	−1.37***	−0.69
	(0.27)	(0.36)	(0.53)	(0.33)	(0.40)
Age (60+)	0.59***	0.59	0.57**	0.66	0.92**
	(0.16)	(0.35)	(0.21)	(0.36)	(0.32)
Income (20-39k)	−0.55	−0.27	−0.96*	0.43	−0.21
	(0.29)	(0.45)	(0.45)	(0.47)	(0.44)
Income (40-59k)	0.03	−0.36	−0.16	0.78	0.18
	(0.21)	(0.56)	(0.28)	(0.43)	(0.42)
Income (60-79k)	0.22	−0.40	0.09	1.12**	−0.32
	(0.22)	(0.44)	(0.30)	(0.43)	(0.41)
Income (80-99k)	0.10	0.15	−0.31	1.60***	0.03
	(0.24)	(0.46)	(0.35)	(0.42)	(0.39)
Income (100-150k)	0.01	−0.32	−0.24	0.98	0.02
	(0.21)	(0.47)	(0.27)	(0.58)	(0.40)
Educ. (HS)	0.88	0.44	2.27**	0.88	2.78
	(0.52)	(0.82)	(0.84)	(0.77)	(2.56)
Educ. (Post-HS)	1.79***	1.90**	3.22***	0.98	3.48
	(0.49)	(0.73)	(0.78)	(0.74)	(2.51)
Dem.	0.11	0.24	0.17	−0.08	0.02
	(0.19)	(0.33)	(0.25)	(0.40)	(0.35)
Ind.	−0.76***	−0.63	−0.69*	−0.98	−0.67
	(0.23)	(0.41)	(0.28)	(0.51)	(0.38)
Other Party	−1.76*	−0.87	−2.04	−1.79*	−1.11
	(0.76)	(1.21)	(1.55)	(0.82)	(1.01)
Female	−0.42**	−0.59*	−0.40	−0.14	−0.53*
	(0.15)	(0.28)	(0.22)	(0.29)	(0.27)
Foreign	−0.34	−0.47	0.15	−0.68	−0.09
	(0.22)	(0.43)	(0.27)	(0.89)	(0.29)
Latinx	−0.20				
	(0.19)				
Black	−0.18				
	(0.20)				
AAPI	−0.36				
	(0.20)				
R^2	0.19	0.18	0.21	0.17	0.11
Adj. R^2	0.18	0.14	0.18	0.14	0.08
Num. obs.	1643	413	413	406	411
RMSE	2.62	2.13	3.96	2.43	1.18

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.12: Interaction Model — Self-Reported Vote Intention by Race Subsets (Continuous Outcome)

	Full	Latino	White	Black	Asian
(Intercept)	7.75^{***}	6.88^{***}	6.64^{***}	7.21^{***}	4.67
	(0.54)	(0.91)	(0.85)	(0.83)	(2.61)
Proximate Contact	−0.44	0.48	−0.24	-2.60^{**}	0.03
	(0.36)	(0.69)	(0.49)	(0.96)	(0.81)
Unfair Scale	−0.06	0.08	−0.09	−0.01	0.11
	(0.05)	(0.09)	(0.07)	(0.08)	(0.08)
Age (18-29)	-1.31^{***}	-0.96^{**}	-1.46^{**}	-1.33^{***}	−0.69
	(0.27)	(0.36)	(0.53)	(0.32)	(0.40)
Age (60+)	0.59^{***}	0.62	0.57^{**}	0.66	0.92^{**}
	(0.16)	(0.36)	(0.21)	(0.36)	(0.32)
Income (20-39k)	−0.55	−0.28	-0.96^{*}	0.41	−0.20
	(0.29)	(0.46)	(0.45)	(0.46)	(0.44)
Income (40-59k)	0.04	−0.37	−0.15	0.83^{*}	0.18
	(0.21)	(0.57)	(0.28)	(0.42)	(0.42)
Income (60-79k)	0.22	−0.41	0.09	1.23^{**}	−0.31
	(0.22)	(0.45)	(0.30)	(0.43)	(0.41)
Income (80-99k)	0.10	0.13	−0.32	1.64^{***}	0.05
	(0.24)	(0.47)	(0.35)	(0.40)	(0.39)
Income (100-150k)	0.00	−0.32	−0.24	1.07	0.03
	(0.21)	(0.47)	(0.27)	(0.57)	(0.40)
Educ. (HS)	0.87	0.45	2.27^{**}	0.88	2.78
	(0.52)	(0.81)	(0.84)	(0.72)	(2.56)
Educ. (Post-HS)	1.78^{***}	1.91^{**}	3.22^{***}	0.93	3.48
	(0.49)	(0.73)	(0.78)	(0.68)	(2.52)
Dem.	0.12	0.24	0.18	−0.04	0.02
	(0.19)	(0.33)	(0.25)	(0.38)	(0.35)
Ind.	-0.75^{***}	−0.62	-0.69^{*}	−0.89	−0.67
	(0.23)	(0.41)	(0.29)	(0.50)	(0.38)
Other Party	-1.77^{*}	−0.85	−2.04	-1.82^{*}	−1.10
	(0.75)	(1.22)	(1.54)	(0.82)	(1.01)
Female	-0.42^{**}	-0.60^{*}	−0.40	−0.20	−0.53
	(0.15)	(0.29)	(0.22)	(0.28)	(0.27)
Foreign	−0.35	−0.46	0.15	−0.69	−0.09
	(0.22)	(0.42)	(0.27)	(0.89)	(0.29)
Latinx	−0.21				
	(0.19)				
Black	−0.18				
	(0.20)				
AAPI	−0.35				
	(0.20)				
Prox. Contact x Unfair Scale	0.09	−0.05	0.02	0.45^{**}	−0.04
	(0.07)	(0.13)	(0.11)	(0.15)	(0.14)
R^2	0.19	0.18	0.21	0.19	0.11
Adj. R^2	0.18	0.14	0.18	0.16	0.08
Num. obs.	1643	413	413	406	411
RMSE	2.62	2.13	3.96	2.40	1.19

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

3 Mediation Analysis for Perceived Injustice

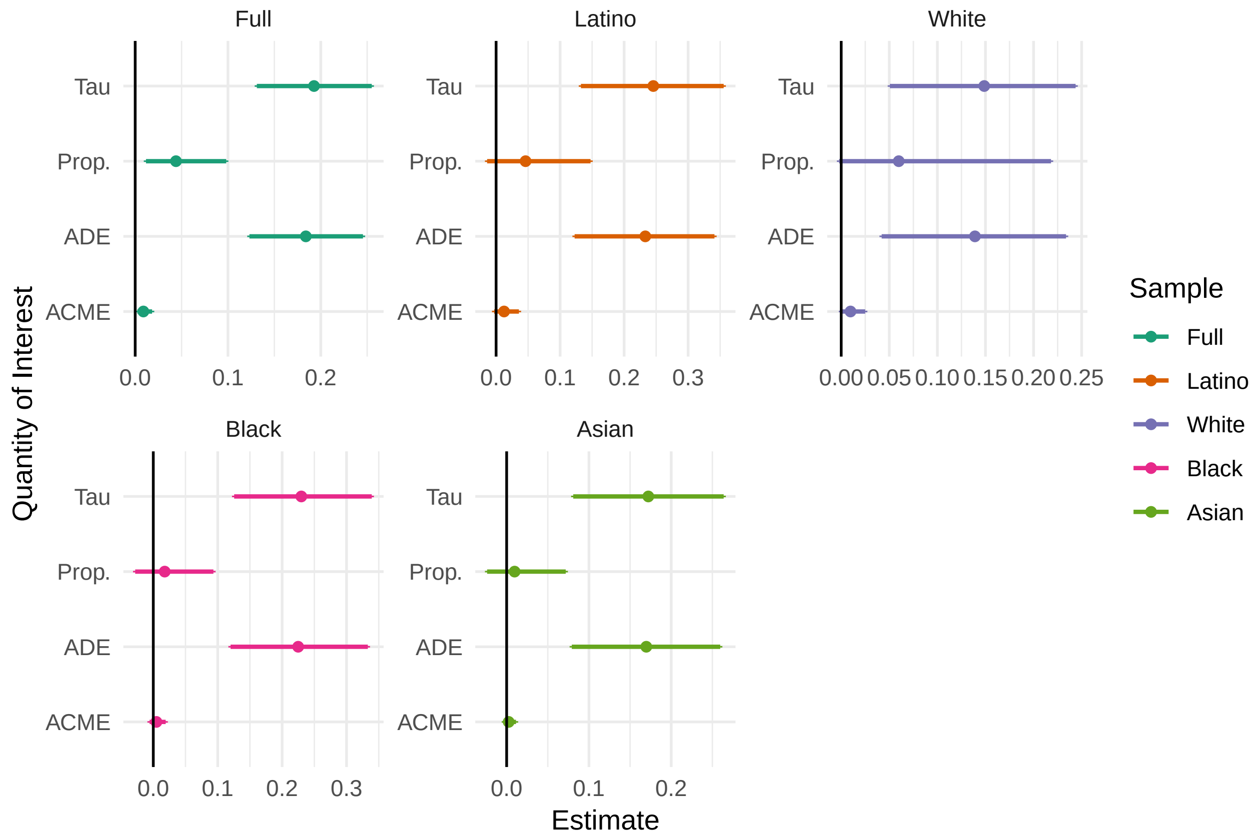

Figure A.1: Mediation Analysis by Subsample

Since perceptions of injustice may be a mechanism by which proximal contact affects non-traditional participation, we conduct a mediation analysis where perceptions of injustice is the intermediate variable between proximal contact and self-reported protest behavior. To do so, we employ the `mediation` $\mathbb{R}$ package which fits models for observed outcome and mediator variables and resimulates model parameters from their sampling distribution in order to derive causal mediation effects, summary statistics, and confidence intervals (Imai, Keele and Tingley, 2010; Imai, Keele and Yamamoto, 2010). The models can be characterized as such:

$$PoI_i = \alpha_1 + \beta_1 ProximalContact_i + \xi_1 \mathbb{X} + \varepsilon_{i1} \tag{1}$$

$$Protest_i = \alpha_1 + \beta_2 ProximalContact_i + \gamma PoI_i + \xi_2 \mathbb{X} + \varepsilon_{i2} \tag{2}$$

Where $\hat{\gamma} \times \hat{\beta}_1$ is the estimated mediation effect and β_2 is the direct effect. Standard errors are White heteroskedastic consistent.

The mediated effects are relatively weak in comparison to the direct effect across all samples. For the full sample, the average mediated effect (AME) is .8 pp, the average direct effect (ADE) is 18 pp, and the total effect is 19 pp. The proportion of the total effect explained by the mediated effect is roughly 4 percent. The mediated effect is statistically distinct from zero given 95% intervals. For the Latino subsample, the AME is 1 pp, while the ADE is 23 pp, and the total effect is 25 pp. The proportion of the total effect explained by the AME is about 5 percent. The AME for Latinos is not statistically significant. For the white subsample, the AME is 1 pp, the

ADE is 14 pp, the direct effect is 15 pp, and the proportion of the direct effect explained by the AME is 6 percent. Again, like the Latino sample, the AME is not statistically significant. For the black subsample, the AME is .4 pp, the ADE is 22 pp, the direct effect is 23 pp, and the proportion explained by the AME is about 2 percent. The AME is not statistically significant. For the Asian subsample, the AME is .2 pp, the ADE is 16.9 pp, and the total effect is 17.2 pp, where the proportion of the total effect explained by the AME is .9 percent. The AME is not statistically significant. This suggests that, assuming conditional ignorability, which is a strong assumption given that results are highly sensitive to confounders that are correlated with the outcome and mediator,[1] the effects of proximal contact with immigration enforcement may not work through perceptions of injustice, and if they do, they do so weakly both substantively and statistically. Instead, given the results explicated by Figure 5, we should conclude that the effects of proximal contact with immigration enforcement are instead moderated by perceptions of injustice.

[1] For instance, a sensitivity analysis suggests that the correlation between ε_1 and ε_2 in models (6) and (7) need only be positively correlated at .1 (a weak correlation) to bring the mediation effect to 0 in the full sample model.

4 Ideology Placebo Tests and Ruling Out Measurement Error in the Dependent Variable

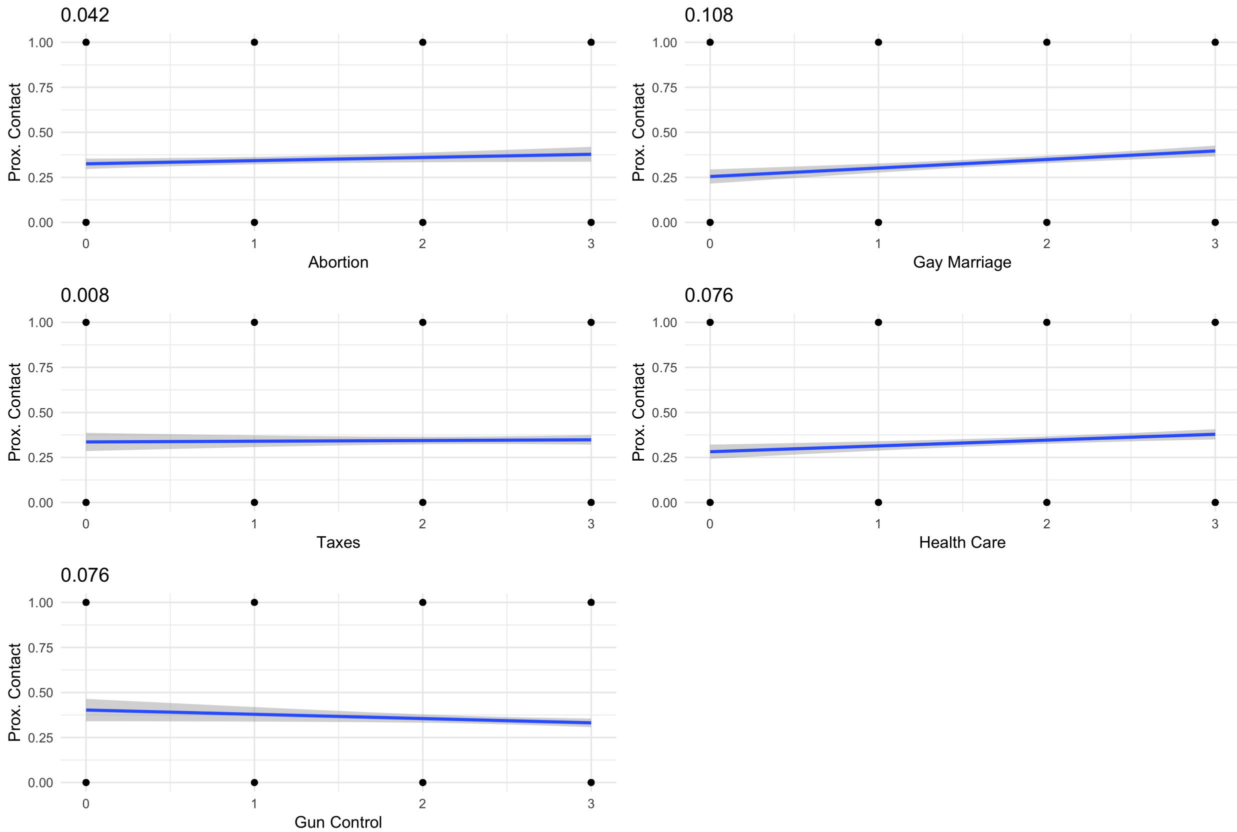

Figure A.2: Raw Correlation Between Ideological Policy Positions and Proximal Contact (Pearson's ρ for each item with respect to Proximal contact is above each individual plot)

One potential concern may be that proximal contact is correlated with partisanship and/or left-leaning ideology, which could be correlated with a number of policy positions that could motivate a respondent's propensity to protest. This concern is particularly important given that the dependent variable asks respondents if, "In the past year and a half, have you taken part in any political protests, marches, or demonstrations?" The question asks if respondents have participated in protests *generally*, not necessarily in protests related to immigration or detention. Therefore, a shortcoming of the question wording is that other ideological preferences unrelated to immigration policy may motivate protest (For instance, protesting attacks on Obamacare), not contact with immigration enforcement itself. To rule out the possibility of both confounding related to partisanship/ideology and measurement error related to the question wording of the outcome, we conduct a number of tests.

First, we observe the raw correlation between ideologically liberal policy preference items that could be correlated with the propensity to protest, particularly during the Trump era, and proximal contact.[2] Figure A.2 shows that the bivariate correlation between proximal contact and each of the liberal policy preferences is relatively weak if not non-existent. Second, we test to

[2]The question wording for these five policy position items asks respondents if they are more or less likely to support a candidate (four radials between much/less likely) for Congress if they support these particular policies: 1)

	Abortion	Gay Marriage	Gun Control	Health Care	Taxes	Protest
(Intercept)	2.05^{***}	1.33^{***}	2.21^{***}	0.51^{*}	1.76^{***}	0.11
	(0.29)	(0.27)	(0.17)	(0.23)	(0.21)	(0.11)
Proximal Contact	0.09	0.01	-0.08	0.05	-0.05	0.18^{***}
	(0.09)	(0.08)	(0.06)	(0.07)	(0.07)	(0.03)
R^2	0.13	0.19	0.12	0.41	0.16	0.17
Adj. R^2	0.13	0.18	0.11	0.40	0.16	0.16
Num. obs.	2045	2045	2045	2045	2045	2045
RMSE	1.16	1.10	0.87	1.00	0.99	0.33

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.13: Treatment Does Not Predict Ideological Preferences and Controlling for Ideological Preferences Does Not Nullify Main Results (Control Covariates Omitted)

see if proximal contact is correlated with five different policy positions that are associated with a liberal ideology controlling for relevant covariates in a multivariate model. We find no statistically significant association between proximal contact and the five policy outcomes where each policy preference is an individual outcome. Moreover, we also do not find that the joint density of the policy preference items (in addition to other relevant control variables) are associated with treatment. This suggests that the effects of ideological policy preferences and exposure to immigration enforcement are decoupled. Second, we ensure that, controlling for various preferences for liberal policies, proximal contact is still associated with a higher propensity to protest.

Abortion: "Put a justice on the supreme court who will strike down Roe v. Wade and make abortion illegal once and for all", 2) **Gay Marriage:** "Wants to ensure that gays and lesbians have the same legal right to get married" 3) **Gun Control:** "Calls for universal background checks before anyone can buy a gun, with no loopholes", 4) **Health Care:** "Wants to expand access to health care and improve and protect Obamacare", 5) **Taxes:** "Wants to stop millionaires and corporations from getting huge tax breaks."

	Proximal Contact
(Intercept)	0.14
	(0.11)
Abortion	0.01
	(0.01)
Gay Marriage	0.00
	(0.01)
Health Care	0.02
	(0.02)
Taxes	−0.01
	(0.02)
Gun Control	−0.02
	(0.02)
R^2	0.14
Adj. R^2	0.13
Num. obs.	2045
RMSE	0.45

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.14: None of the Ideological Issue Positions Predict Treatment

5 Assessing the Moderating Influence of Partisan Identification

	Democrat	Republican	Independent	Other	Party Heterogeneity 1	Party Heterogeneity 2
(Intercept)	0.22	0.06	−0.08	0.65	0.12	0.13
	(0.15)	(0.29)	(0.10)	(0.36)	(0.10)	(0.10)
Proximal Contact	0.26***	0.16*	0.07*	0.19	0.12***	0.17**
	(0.05)	(0.06)	(0.03)	(0.10)	(0.04)	(0.06)
Democrat					0.03	0.02
					(0.02)	(0.03)
Independent						−0.02
						(0.02)
Other					−0.04*	−0.05*
					(0.02)	(0.02)
Contact x Democrat					0.14*	0.09
					(0.06)	(0.07)
Contact x Independent						−0.10
						(0.07)
Contact x Other					0.06	0.01
					(0.12)	(0.13)
R^2	0.18	0.19	0.16	0.57	0.17	0.17
Adj. R^2	0.17	0.16	0.13	0.39	0.16	0.16
Num. obs.	949	489	545	62	2045	2045
RMSE	0.38	0.30	0.25	0.16	0.33	0.33

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.15: Heterogeneity of Proximal Contact by Partisan Identification (Control Covariates Omitted)

Table A.15 characterizes the association between proximal contact and protests subsetted and moderated by partisan identification. The first three models indicate that there is an independent, positive, and statistically significant association between contact with immigration enforcement and self-reported participation in protests or demonstrations for respondents who characterize themselves as Democrats, Republicans, and Independents. In addition, the fourth model indicates that there is an independent, positive association between proximal contact and the propensity to protest. However, this is not statistically significant at $p < .05$, but is still statistically significant at $p < .10$. Partisan identification also appears to moderate the strength of the correlation between proximal contact and participation in protests or demonstrations. The fifth model shows that the partial derivative of proximal contact is stronger for Democrats when independents and Republicans are the reference category. This makes sense theoretically, given that Democrats are more likely to reject punitive immigration policies. However, the sixth model shows that the partial derivative of proximal contact is not stronger for Democrats when the reference category only consists of Republicans, meaning that the moderating influence of identification with the Democratic party is primarily driven by the relatively low influence of proximal contact on the propensity to protest amongst independents (this is also corroborated by the third model on the table, which shows a relatively small independent partial derivative of proximal contact for independents in comparison to both Democrats, Republicans, and even those with an unusual partisan identification).

6 Replicating Main Results Using Logistic Regression

6.1 Replicating Association Between Proximal Contact and Perceived Injustice Using Ordered Logit

Table A.16: Logistic Regression Replication (PoI Outcome)

	Full	White	Latino	Black	Asian
Proximate Contact	0.56^{***}	0.77^{***}	0.65^{**}	0.15	0.32
	(0.09)	(0.12)	(0.20)	(0.21)	(0.40)
AIC	10229.80	6677.77	1459.68	1621.96	456.21
BIC	10387.07	6807.93	1551.63	1716.69	519.71
Log Likelihood	-5087.90	-3314.89	-705.84	-786.98	-204.10
Deviance	10175.80	6629.77	1411.68	1573.96	408.21
Num. obs.	2502	1674	341	383	104

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.16 displays ordered logit coefficient estimates that replicate the results from figures 1 and 2, which assesses the association between proximate contact with immigration enforcement and a respondent's perceived injustice. The coefficient estimates are generated after conditioning for all the covariates described in the data and measurement section, and each model characterizes a different racial/ethnic subsample (with the first model being the full sample). The ordered logit estimates are consistent with the main OLS estimates. Proximate exposure is associated with an increase in perceptions of injustice by 0.56, 0.77, 0.65, 0.15, and 0.32 of a single point on the 0-9 scale for the full, white, Latino, black and Asian samples respectively.[3] Like the OLS results,

[3] It may appear to be the case that the number of observations for the ordered logistic analysis is different than the N for the original OLS analysis, but this is a function of the ordered logistic regression table output displaying

the coefficient estimates are statistically significant for the full, white, and Latino subsamples, but not for the black and Asian subsamples.

the effective sample size after accounting for the population weights. The content of the subsamples is still the same between the OLS and logistic regression analysis.

6.2 Replicating Association Between Proximal Contact and Both Voting and Protest Using Logistic Regression

Table A.17: Logistic Regression Replication (Protest Outcome)

	Full	White	Latino	Black	Asian
Proximate Contact	1.61^{***}	1.52^{***}	1.59^{***}	1.77^{***}	1.64^{*}
	(0.14)	(0.21)	(0.34)	(0.31)	(0.70)
AIC	1382.54	761.71	283.01	292.07	38.13
BIC	1490.63	830.11	351.41	360.18	106.45
Log Likelihood	-671.27	-363.86	-124.51	-129.04	-2.07
Deviance	1481.44	717.45	297.85	290.04	58.38
Num. obs.	1643	413	413	406	411

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.18: Logistic Regression Replication (Vote Outcome)

	Full	White	Latino	Black	Asian
Proximate Contact	-0.21	-0.29	-0.19	-0.04	0.13
	(0.11)	(0.15)	(0.25)	(0.27)	(0.49)
AIC	2752.65	1855.48	418.42	438.72	56.11
BIC	2860.73	1923.88	486.82	506.82	124.42
Log Likelihood	-1356.32	-910.74	-192.21	-202.36	-11.05
Deviance	2924.87	1824.40	432.30	450.00	128.18
Num. obs.	1643	413	413	406	411

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.17 replicates the estimates characterizing the association between proximate contact and protest activity after conditioning on control covariates on figures 3 and 4 in a logistic regression framework. Proximate contact is positively associated with the probability of protest activity across the full, white, Latino, Black, and Asian subsamples. These associations are statistically significant. Conversely, table A.18 replicates the estimates characterizing the association between proximate contact and vote intention after conditioning on control covariates on figures 3 and 4 using logistic regression models. Proximate contact is not associated with the probability of a positive vote intention across all subsamples. Both tables demonstrate that the OLS results characterizing the influence of proximate contact on political participation are not model dependent.

6.3 Replicating Association Between Interaction of Proximal Contact with Perceived Injustice on Both Voting and Protest

Table A.19: Logistic Regression Replication (Protest Outcome, Interactive Model)

	Full	White	Latino	Black	Asian
Proximate Contact	1.65^{***}	1.18^{*}	1.69	2.27^{*}	0.74
	(0.37)	(0.57)	(0.88)	(0.96)	(2.17)
Unfair Scale	0.17^{***}	0.18^{**}	0.15	0.19^{*}	0.01
	(0.05)	(0.07)	(0.13)	(0.10)	(0.28)
Prox. Contact x Unfair Scale	-0.01	0.07	-0.02	-0.08	0.17
	(0.06)	(0.10)	(0.15)	(0.15)	(0.39)
AIC	1384.52	763.48	285.05	293.26	40.22
BIC	1498.01	835.90	357.47	365.38	112.56
Log Likelihood	-671.26	-363.74	-124.52	-128.63	-2.11
Deviance	1481.43	717.02	297.84	289.74	58.19
Num. obs.	1643	413	413	406	411

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.20: Logistic Regression Replication (Vote Outcome, Interactive Model)

	Full	White	Latino	Black	Asian
Proximate Contact	-0.32	0.22	-0.52	-2.20^{*}	0.11
	(0.25)	(0.35)	(0.58)	(0.92)	(1.42)
Unfair Scale	-0.00	-0.03	-0.02	0.06	0.14
	(0.03)	(0.03)	(0.07)	(0.06)	(0.14)
Prox. Contact x Unfair Scale	0.02	-0.11	0.06	0.38^{*}	0.00
	(0.05)	(0.07)	(0.10)	(0.15)	(0.26)
AIC	2754.72	1854.68	419.92	435.45	58.11
BIC	2868.21	1927.10	492.34	507.56	130.45
Log Likelihood	-1356.36	-909.34	-191.96	-199.72	-11.06
Deviance	2924.65	1821.61	431.91	443.41	128.18
Num. obs.	1643	413	413	406	411

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

6.4 Demonstrating Lack of Statistical Significance for Multiplicative Term in Logit Model Does Not Mean Absence of Heterogeneous Partial Derivative

Although table A.20 demonstrates that the injustice scale and proximate contact multiplicative term is not statistically significant in a logistic regression framework (unlike the OLS framework), this does not necessarily mean there are no heterogenous partial derivatives of proximate contact conditional on respondents holding different levels of perceived injustice. Since the slope of the logit link function changes if other covariates in the model besides the covariates of interest change, the partial derivative of proximate contact can still be conditional on the value of other covariates we theoretically define as moderators (Berry, DeMeritt and Esarey, 2010). In our case, the moderator of interest is a respondent's sense of injustice. The logit link function has the steepest slope near the middle, when the probability of protest is near 0.5. Conversely, the slope is flatter when the probability of protest is closer to 0 or 1. The partial derivatve of proximate contact will be greatest when the probability of protest is closer to 0.5, and will decline when changes in other variables, such as a sense of injustice, push the probability of protest closer to 0 or 1. This phenomenon is otherwise known as *compression* (Berry, DeMeritt and Esarey, 2010).

One may argue that heterogeneity based on compression is not theoretically relevant since it is the result of the choice of a particular functional form. We disagree. First, in the logistic regression framework, we care about $Pr(Protest)$, not the latent variable $protest^*$ prior to the logit link transformation, which is unaffected by compression yet influenced only by a multiplicative term and may characterize a data generating process that does not accurately describe the nature of the outcome (binary, probabilistic, constrained between 0 and 1) (Rainey, 2016). Second, compression makes theoretical sense in the context of proximate contact and a sense of injustice. We have already demonstrated that perceived injustice is associated with higher levels of protest activity. Thus, respondents low on the injustice scale will probably be closer to the 0.5 mark for $Pr(Y)$ (since protest activity is still a relatively rare phenomenon). Therefore, a change in the injustice scale will change the partial derivative of proximate contact in a positive (and larger) direction under the logistic regression framework as a result of the logit link function form despite the absence of statistical significance from the product term (Berry, DeMeritt and Esarey, 2010).

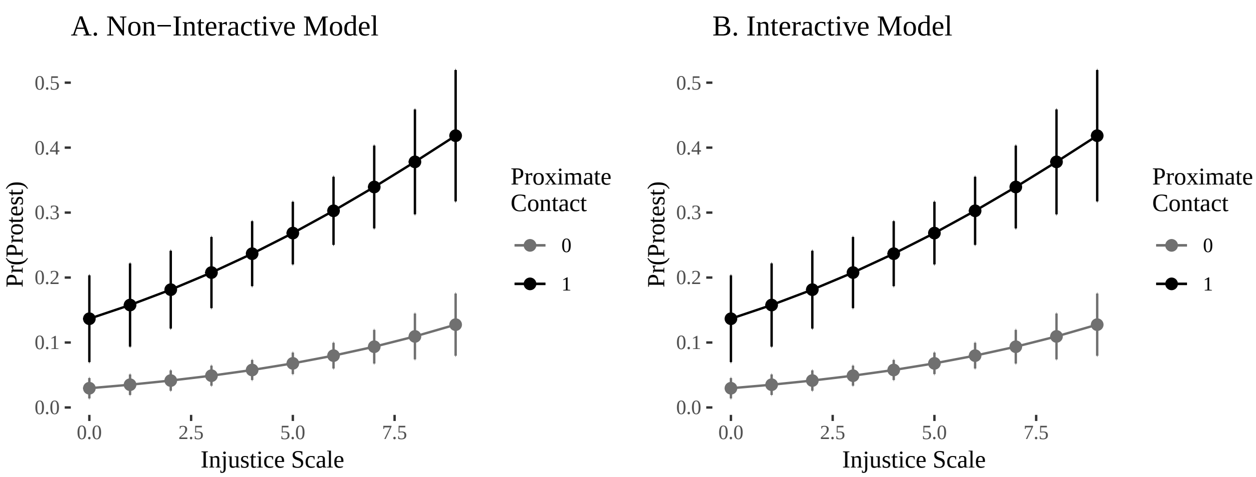

Figure A.3: Predicted Protest Probabilities Across Proximate Contact and the Unfairness Scale under Logistic Regression Framework

To this end, we calculate predicted probabilities of protest activity conditional on different levels of proximate contact and perceived injustice for both an interactive and non-interactive logistic regression model (figure A.3). We use both logistic regression models with and without a product term since a product term in the model can still minimize bias and discriminate against the possibility that the product term undermines a hypothesized interactive association due to compression (Rainey, 2016). The predicted probabilities are virtually the same across the two different models, suggesting that the statistically insignificant interaction on table A.20 has no bearing on the original conclusion from the OLS model. Moreover, the patterns evidenced by the predicted probabilities in the logistic regression framework appear to characterize the same kind of heterogeneity illustrated on figure 5, where proximate contact is more likely to motivate protest activity conditional on respondents having a higher sense of injustice.

In addition to demonstrating that the predicted probabilities with a statistically insignificant product term in a logistic framework are analogous to the interactive patterns described in the original OLS analysis, we compute a "second difference," consistent with the recommendations set forth by Berry, DeMeritt and Esarey (2010), Berry, DeMeritt and Esarey (2016) and Rainey (2016), in order to derive the partial derivative of moving from the lowest to the highest point on the perceptions of injustice scale across proximate contact. The quantity is characterized below:

$$\begin{aligned}\Delta\Delta Pr(Protest) =& [Pr(Protest|ProxContact = 1, PoI = 9, \mathbb{X} = \mu(\mathbb{X})\\ &- Pr(Protest|ProxContact = 1, PoI = 0, \mathbb{X} = \mu(\mathbb{X})]\\ &- [Pr(Protest|ProxContact = 0, PoI = 9, \mathbb{X} = \mu(\mathbb{X})\\ &- Pr(Protest|ProxContact = 0, PoI = 0, \mathbb{X} = \mu(\mathbb{X})]\end{aligned}$$

The second difference captures heterogeneity due to compression in the influence of proximate contact on protest activity based on movement along the perceptions of injustice scale. To derive the second difference, we fit the full interactive model in the logistic regression framework and predict the probability of a respondent participating in a protest conditional on both proximate contact and being on the highest and lowest end of the injustice scale, for a total of 4 predicted probabilities.[4] We then acquire two quantities. The first quantity is the predicted probability of protest conditional on proximate contact and being high on the injustice scale (proximate contact = 1, injustice scale = 9) minus the predicted probability of protest conditional on proximate contact and being low on the injustice scale (proximate contact = 1, injustice scale = 0). The second quantity is the predicted probability of protest conditional on no proximate contact and being high on the injustice scale (proximate contact = 1, injustice scale = 9) minus the predicted probability of protest conditional on no proximate contact and being low on the injustice scale (proximate contact = 0, injustice scale = 0). We then subtract the first quantity from the second quantity to acquire the second difference, which captures the heterogenous partial derivatives of proximate contact conditional on moving from the lowest to the highest value of the injustice scale. We use bootstrapping and resample the original data with 1000 replicates that re-calculate the second difference to generate confidence intervals for statistical inference. We estimate second differences for both a non-interactive and interactive logistic regression specification. The partial derivative of proximate contact conditional on shifting from the lowest to highest value on the perceptions of injustice scale is displayed on table A.21.

Table A.21 displays the estimates, t-statistics, p-values, and confidence intervals at the 95% and 90% level for the second difference computed using a logistic regression model with and without a product term. For the non-interactive model and interactive model, the second difference is a 19 percentage point increase in the predicted probability of protest and is statistically

[4] All other covariates are held at their means.

Table A.21: Second Difference Estimate

	Estimate	t-stat	p-val	Upper (95%)	Lower (95%)	Upper (90%)	Lower (90%)
Without Product Term	0.19	3.36	0.00	0.31	0.08	0.29	0.10
With Product Term	0.19	1.76	0.08	0.40	-0.02	0.37	0.01

significant at the 95% level for the non-interactive model ($p < 0.01$, $t = 3.39$) and at the 90% level for the interactive model ($p < 0.10$, $t = 1.81$). These results are consistent with the original OLS estimates, and that changing the functional form to estimate the probability of protest with a logistic regression model does not change substantive conclusions.

7 Prediction Diagnostics Between OLS and Logistic Regression

7.1 Assessing Prediction Error

7.1.1 Full Sample

Table A.22: % Correctly Predicted (Full Sample)

	OLS	Logit	Modal Outcome
Protest	0.841	0.834	0.841
Vote	0.647	0.647	0.525
Protest (Interactive)	0.841	0.834	0.841
Vote (Interactive)	0.650	0.650	0.525

Table A.23: RMSE (Full Sample)

	OLS	Logit	Modal Outcome
Protest	0.117	0.117	0.159
Vote	0.219	0.219	0.475
Protest (Interactive)	0.116	0.117	0.159
Vote (Interactive)	0.219	0.219	0.475

7.1.2 White Sample

Table A.24: % Correctly Predicted (White Sample)

	OLS	Logit	Modal Outcome
Protest	0.906	0.913	0.906
Vote	0.712	0.722	0.661
Protest (Interactive)	0.908	0.910	0.906
Vote (Interactive)	0.719	0.722	0.661

Table A.25: RMSE (White Sample)

	OLS	Logit	Modal Outcome
Protest	0.073	0.067	0.094
Vote	0.187	0.186	0.339
Protest (Interactive)	0.072	0.067	0.094
Vote (Interactive)	0.187	0.186	0.339

7.1.3 Latino Sample

Table A.26: % Correctly Predicted (Latino Sample)

	OLS	Logit	Modal Outcome
Protest	0.787	0.782	0.792
Vote	0.642	0.642	0.482
Protest (Interactive)	0.787	0.780	0.792
Vote (Interactive)	0.649	0.649	0.482

Table A.27: RMSE (Latino Sample)

	OLS	Logit	Modal Outcome
Protest	0.148	0.145	0.208
Vote	0.225	0.224	0.518
Protest (Interactive)	0.147	0.145	0.208
Vote (Interactive)	0.224	0.223	0.518

7.1.4 Black Sample

Table A.28: % Correctly Predicted (Black Sample)

	OLS	Logit	Modal Outcome
Protest	0.828	0.810	0.788
Vote	0.672	0.675	0.515
Protest (Interactive)	0.828	0.815	0.788
Vote (Interactive)	0.680	0.690	0.515

Table A.29: RMSE (Black Sample)

	OLS	Logit	Modal Outcome
Protest	0.134	0.133	0.212
Vote	0.207	0.207	0.485
Protest (Interactive)	0.134	0.133	0.212
Vote (Interactive)	0.205	0.203	0.485

7.1.5 Asian Sample

Table A.30: % Correctly Predicted (Asian Sample)

	OLS	Logit	Modal Outcome
Protest	0.878	0.883	0.876
Vote	0.625	0.635	0.440
Protest (Interactive)	0.878	0.876	0.876
Vote (Interactive)	0.630	0.635	0.440

Table A.31: RMSE (Asian Sample)

	OLS	Logit	Modal Outcome
Protest	0.091	0.090	0.124
Vote	0.223	0.224	0.560
Protest (Interactive)	0.090	0.090	0.124
Vote (Interactive)	0.223	0.224	0.560

7.2 OLS Predictive Value Range

7.2.1 Min/Max Predicted Value Tables

Table A.32: Min/Max Predicted Values in OLS Models (Protest Outcome)

	Full	White	Latino	Black	Asian
Min	-0.12	-0.20	-0.25	-0.15	-0.13
Max	0.57	0.40	0.73	0.73	0.73

Table A.33: Min/Max Predicted Values in OLS Models (Vote Outcome)

	Full	White	Latino	Black	Asian
Min	-0.29	-0.15	-0.14	-0.17	-0.16
Max	0.97	1.01	0.83	1.11	0.98

Table A.34: Min/Max Predicted Values in OLS Models (Protest Outcome, Interactive Model)

	Full	White	Latino	Black	Asian
Min	-0.12	-0.19	-0.22	-0.15	-0.11
Max	0.65	0.52	0.74	0.74	0.72

Table A.35: Min/Max Predicted Values in OLS Models (Vote Outcome, Interactive Model)

	Full	White	Latino	Black	Asian
Min	-0.29	-0.15	-0.12	-0.20	-0.16
Max	0.98	1.01	0.84	1.09	0.98

7.2.2 Predicted Value Distribution by Subsample (Protest Outcome)

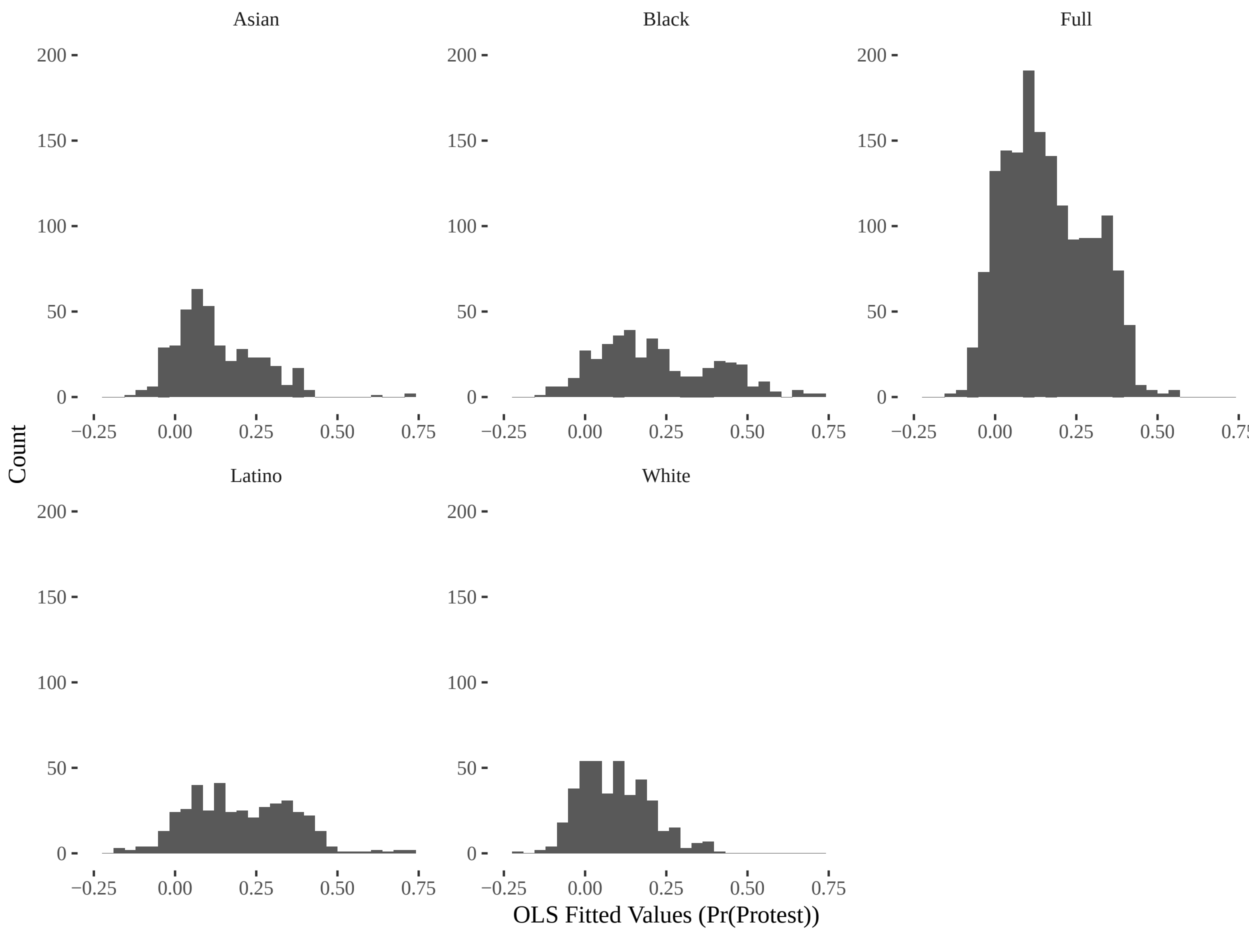

Figure A.4: Predicted Value Distribution by Racial/Ethnic Subsample (Protest Outcome)

7.2.3 Predicted Value Distribution by Subsample (Vote Outcome)

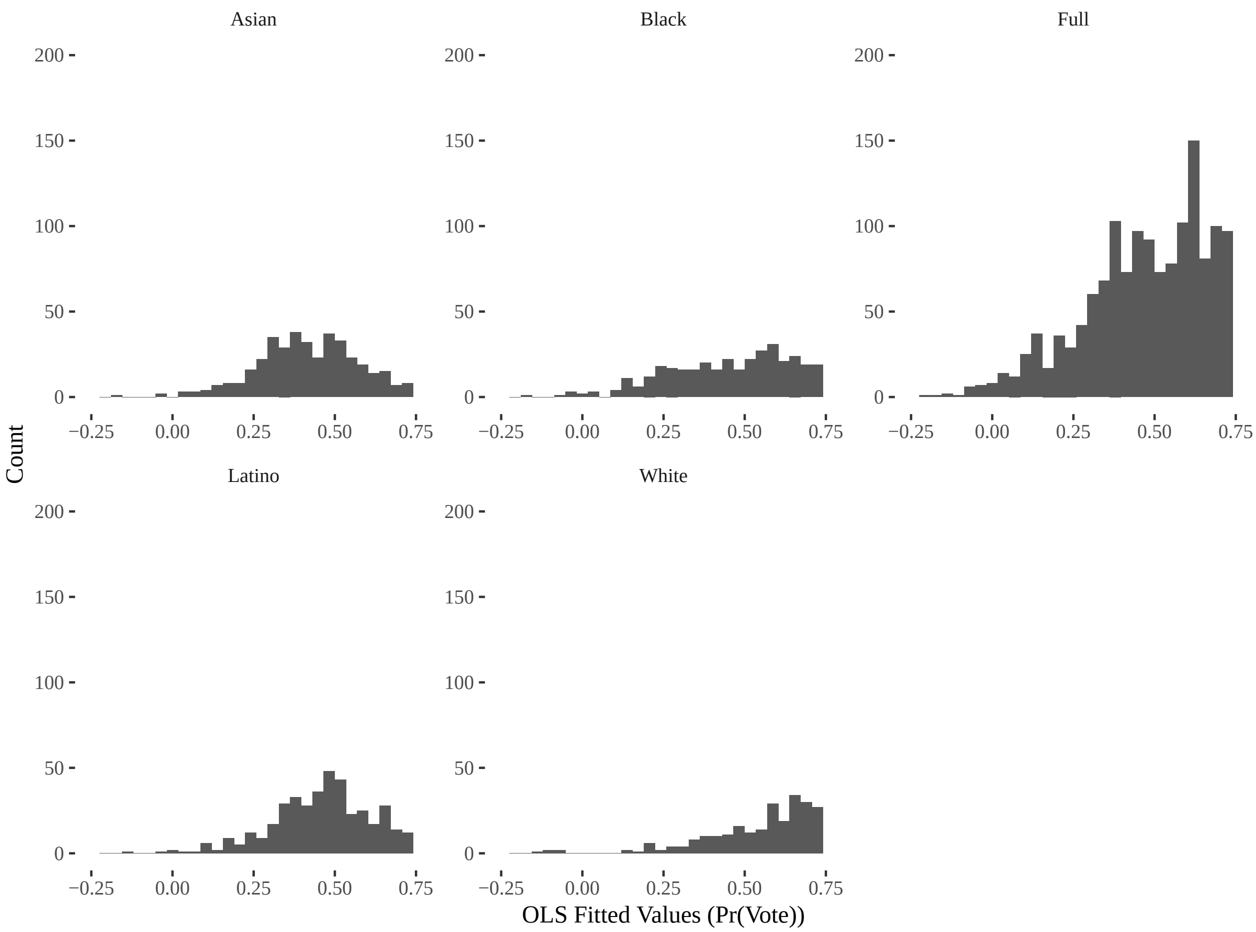

Figure A.5: Predicted Value Distribution by Racial/Ethnic Subsample (Vote Outcome)

7.2.4 Predicted Value Distribution by Subsample (Protest Outcome, Interactive Model)

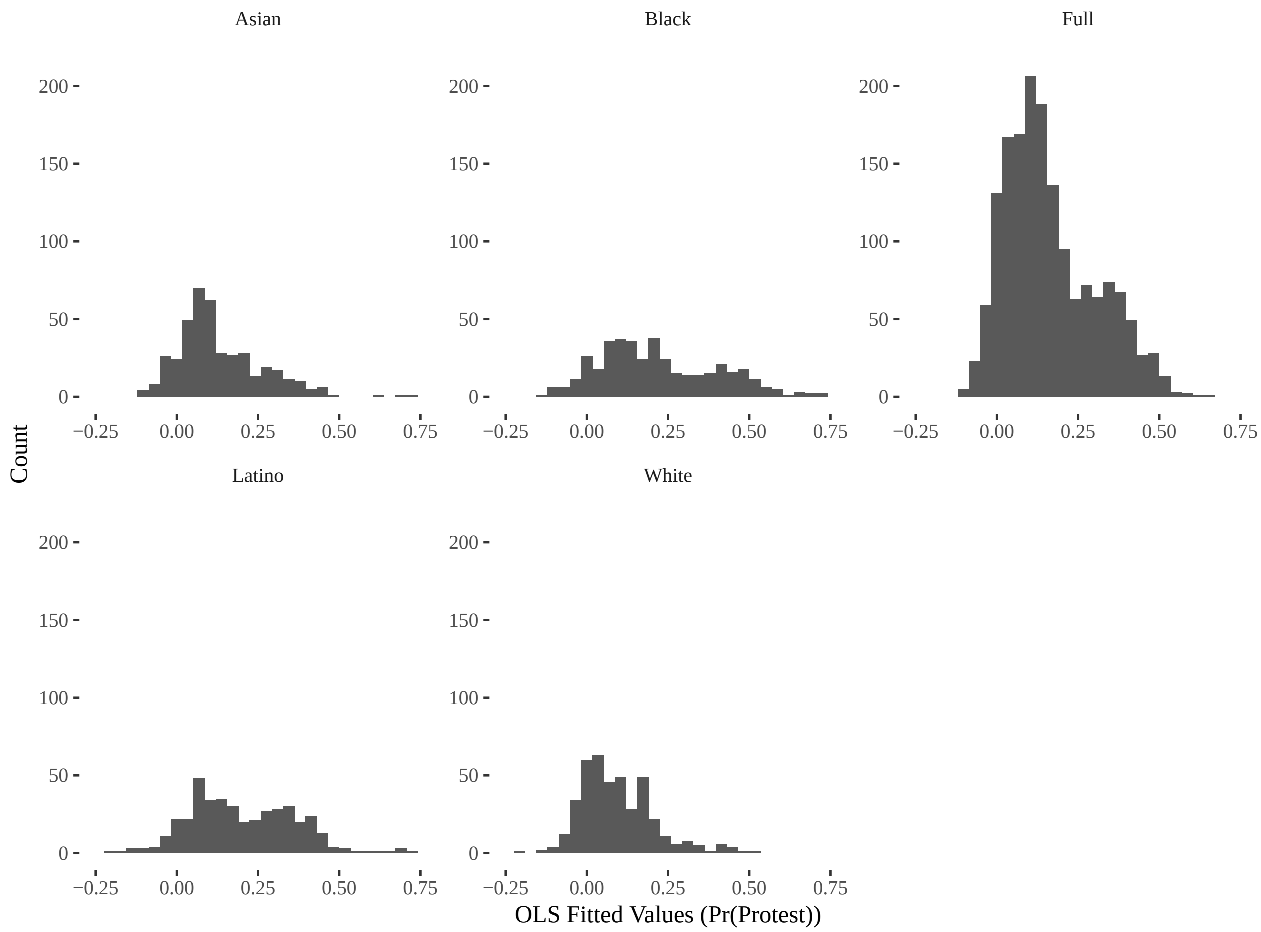

Figure A.6: Predicted Value Distribution by Racial/Ethnic Subsample (Protest Outcome, Interactive Model)

7.2.5 Predicted Value Distribution by Subsample (Vote Outcome, Interactive Model)

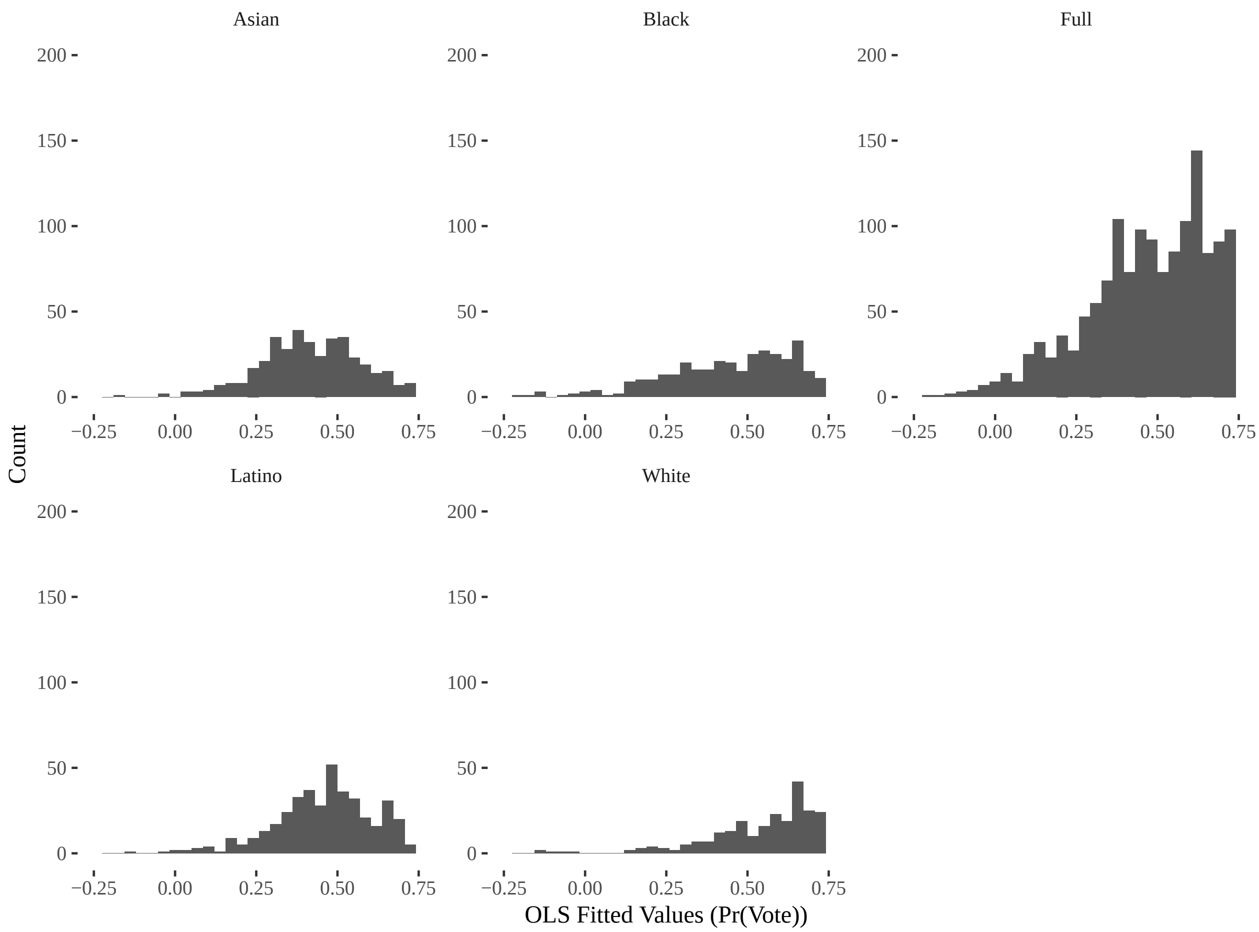

Figure A.7: Predicted Value Distribution by Racial/Ethnic Subsample (Vote Outcome, Interactive Model)

8 Ruling Out Reverse Causality

One concern over our results relates to the way the protest outcome is measured, which asks whether respondents participated in a protest in the last year. It is possible that protest participation resulted in contact with someone undocumented, either by meeting undocumented people who are engaged in political activism or by higher levels of contact with marginalized groups after deciding to engage in political activism. If protest participation preceded contact, our results may simply be the result of a reverse causal dynamic, biasing the direction and strength of the association we derive. Before we conduct further analysis using the 2018 Latino Decisions Midterm Survey, we want to note that we have replicated our results characterizing the association between proximal contact and *prospective,* rather than *retrospective* protest activity using the RWJF 2015 survey. The RWJF 2015 survey includes an item that asks whether the respodnent "intends to participate in a protest," and we find that the association between proximal contact and protest activity using the RWJF data is consistent with our main results in the Midterm survey (see table A.87).

In addition, we conduct two separate tests to rule out the possibility of a reverse causal dynamic affecting our estimates. First, we disaggregate the proximate contact measure and generate separate dummy variables for whether someone knows another friend or family who was "detained and deported" and whether someone knows another friend or family member who is undocumented. Protest participation may result in coming into contact with someone who is undocumented, but not so much an individual who has been detained or deported, unless the detainment/deportation occurred after the fact. Second, we disaggregate both the proximate contact measure into two separate binary variables that characterize whether someone knows a *friend* who is undocumented or was detained/deported and whether someone knows a *family member* who is undocumented or was detained/deported. The key distinction here is disaggregating contact via friendship networks versus contact via familial networks, with the assumption being that protest activity is less likely to result in coming into close contact with someone who is already a family member as opposed to someone who could be a friend.

```
FALSE
FALSE          0     1
FALSE    0 1099    84
FALSE    1  201   259
```

Figure A.8 displays the results from the first test, where we assess the association between knowing someone detained/deported and politicial participation. All models also condition on knowing someone undocumented, so the estimates characterize whether a respondent knows someone detained/deported net of knowing someone undocumented. The x-axis characterizes the racial subsample used to estimate the partial derivative. The y-axis characterizes the size and statistical uncertainty of the partial derivative. The left panel characterizes estimates where the outcome is perceived injustice. The right panel characterizes estimates where the outcome is protest activity. The estimates on these plots are consistent with our main analysis, suggesting that knowing someone detained and deported motivates both perceived injustice and political participation.

We also assess the association between the interaction of knowing someone detained/deported with perceived injustice and the propensity to engage in political protest (See table A.36. All relevant covariates are included in the models.). Generally, with respect to the direction of the interaction, we derive consistent results. However, we do not achieve statistical significant with the

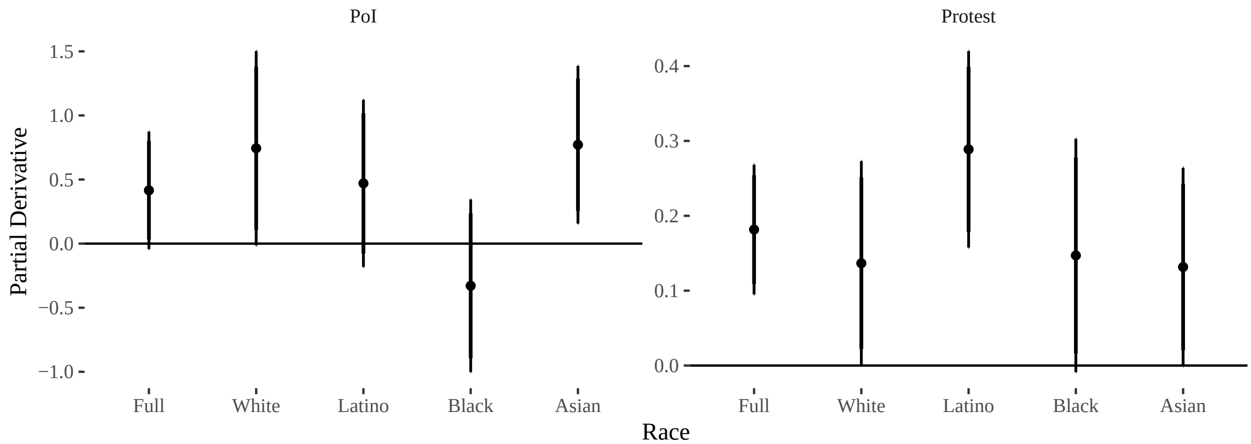

Figure A.8: Ruling out reverse causal dynamic by assessing partial derivatives of knowing someone detained/deported on relevant outcomes

Table A.36: Partial Derivatives Characterizing the Association Between the Interaction of Contact with Perceived Injustice and Protest Activity

	Full	White	Latino	Black	Asian
Detain/Deport x Unfair Scale	0.02	0.03	−0.01	0.05	−0.02
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Know Undoc. x Unfair Scale	0.01	0.02	0.01	−0.06	0.06*
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
R^2	0.18	0.17	0.24	0.22	0.19
Adj. R^2	0.17	0.13	0.21	0.18	0.15
Num. obs.	1643	413	413	406	411
RMSE	0.37	0.52	0.34	0.34	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

exception of the interaction between knowing someone undocumented and perceived injustice among the Asian subsample. Given that the interactions between perceived injustice, knowing someone undocumented, and knowing someone detained/deported are typically in the same direction as the main results, it is possible that the explanation for our statistically significant association in the main estimates may be a function of statistical power. There are not many who have come into contact with individuals who have been detained/deported or are undocumented.[5] Additionally, a statistically significant partial derivative for an interaction typically requires a large sample size.

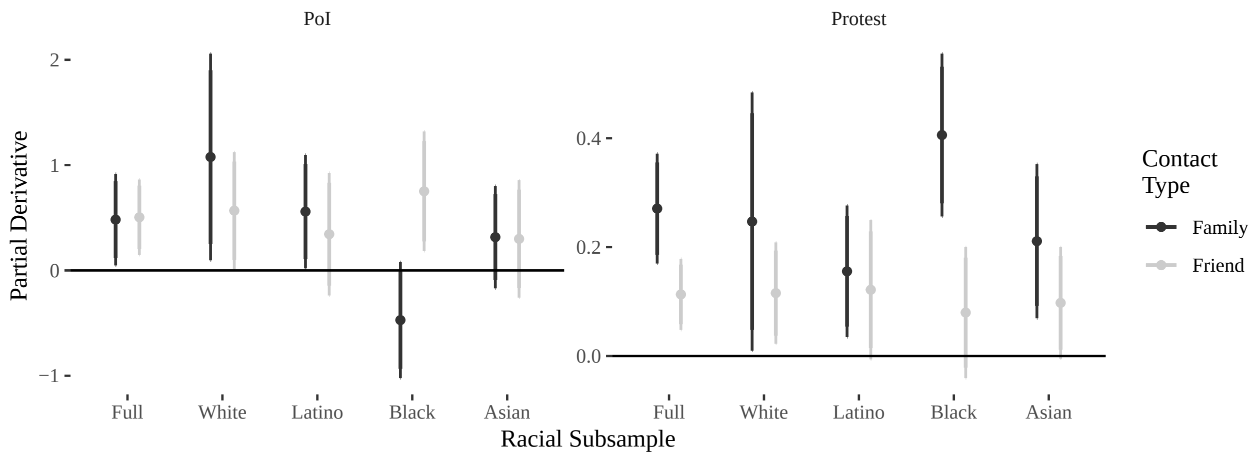

Figure A.9: Ruling out reverse causal dynamic by assessing partial derivatives of knowing a family member who is either undocumented or detained/deported on relevant outcomes

A potential problem with analyzing proximate contact as a measure of knowing someone detained or deported is that one may have participated in protest activity, come into contact with someone undocumented, and then know of their detainment/deportation ex post, which could still make our results characterize a bidirectional rather than unidirectional association. An additional robustness check we use is to assess the association between knowing a family member (as opposed to a family member and/or a friend) who is undocumented or detained/deported and political participation. Figure A.9 characterizes the partial derivatve estimates derived from this kind of analysis. Again, like figure A.8, the x-axis is the racial subsample, the y-axis is the partial derivative size, and each panel characterizes a distinct outcome, with the left being perceived injustice and the right being propensity to protest. Across the board, the type of proximal contact unlikely to be affected by reverse causal processes (e.g. family contact) is positively associated with both perceived injustice and protest in a statistically significant manner consistent with the main results. Moreover, for the full sample, there appears to be a statistically significant difference in partial derivative estimates between familial contact versus friendship contact at the $p < .10$ level, with familial contact increasing the propensity to protest *more* than simply friendship contact.

Table A.37 characterizes the association between the interaction of perceived injustice with different kinds of contact and propensity to protest (all covariates included in the model). Although the partial derivative for the interaction between familial contact and perceived injustice is not statistically significant for the full sample, the sign is in the correct direction. We contend the lack of statistical significance using measures of familial contact may be a function of limited

[5] Those who know someone undocumented or who have been detained/deported are 33% of the sample.

Table A.37: Partial Derivatives of Interaction Between Type of Contact and Perceived Injustice on Protest Activity

	Full	White	Latino	Black	Asian
Prox Fam x Unfair Scale	0.02	0.09	−0.02	0.01	−0.03
	(0.02)	(0.06)	(0.03)	(0.05)	(0.05)
Prox Friend x Unfair Scale	0.02	0.02	0.02	0.04	0.01
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)
R^2	0.16	0.17	0.16	0.24	0.17
Adj. R^2	0.15	0.13	0.12	0.20	0.13
Num. obs.	1643	413	413	406	411
RMSE	0.37	0.52	0.36	0.34	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

sample size rather than the absence of a substantively meaningful effect,[6] especially since the partial derivative is similar in kind to the one found in the main model characterized by table A.10.

[6]The original proximate contact treatment had 33% of survey respondents reporting they knew someone undocumented or detained and deported whereas the familial proximate contact treatment only has 16% of survey respondents reporting they knew a family member who was either undocumented or detained/deported.

9 Ruling Out Potential Dependence Between Prospective Voting and Retrospective Protesting

A concern with the models we specify in the main text to assess the association between proximal contact and voting is that we do not account for prior protest participation. We assess the association between proximal contact and the interaction of proximal contact with perceived injustice conditional on prior protest activity to determine if the results change in a substantively meaningful manner. They do not. The partial derivatives on figure A.10 demonstrate that the association between proximate contact and voter activity is not statistically significant across all sub-samples.[7]

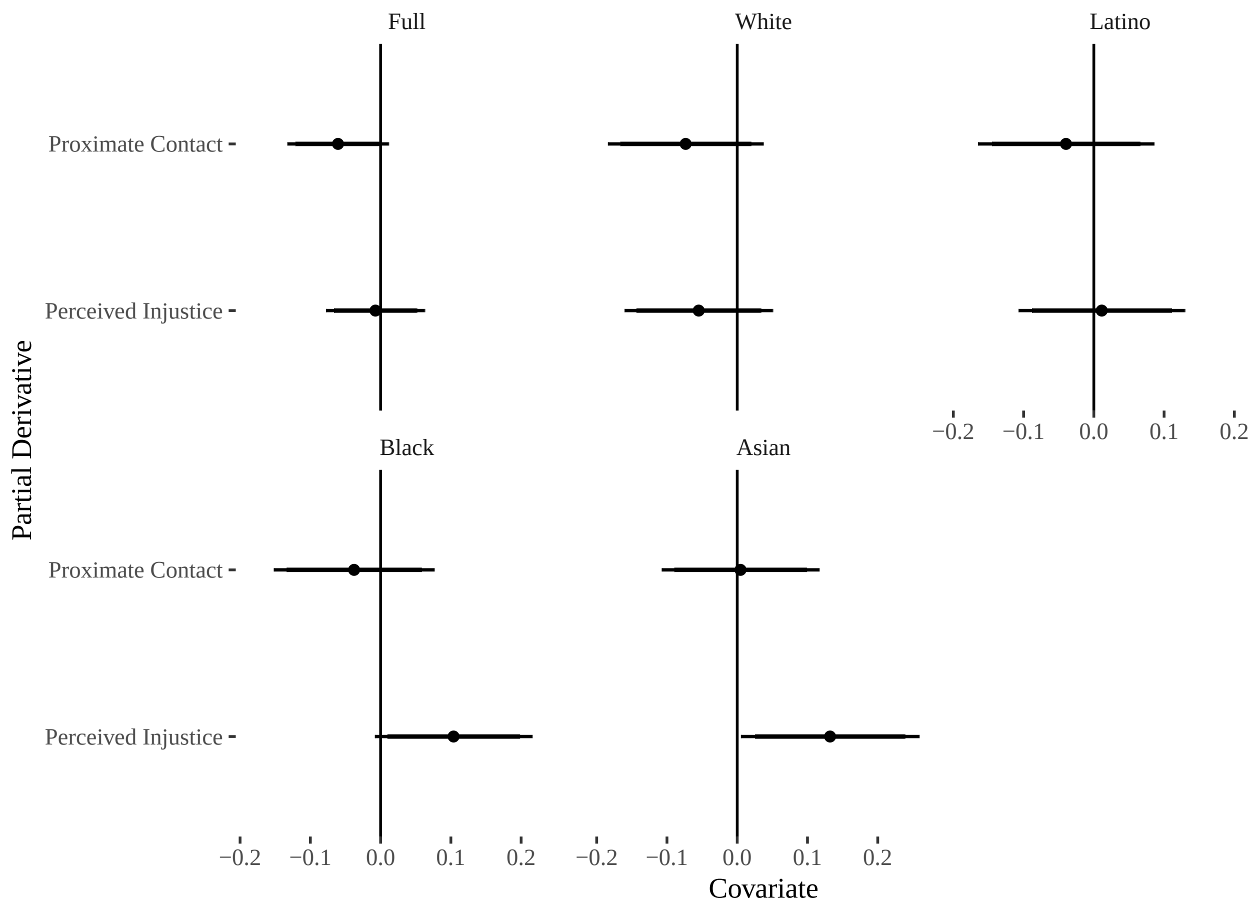

Figure A.10: Accounting for pre-treatment protest activity in vote outcome models

Moreover, figure A.11 demonstrates that the partial derivative for the interaction between proximate contact and perceived injustice is statistically insignificant (and essentially a pure zero) for the full sample. Likewise, there is no statistically significant partial derivative for the interaction for almost all sub-samples with the exception of the black sub-sample, consistent with the main results.

[7]Standardized coefficients displayed. The outcome for figures A.10 and A.11 is vote intention.

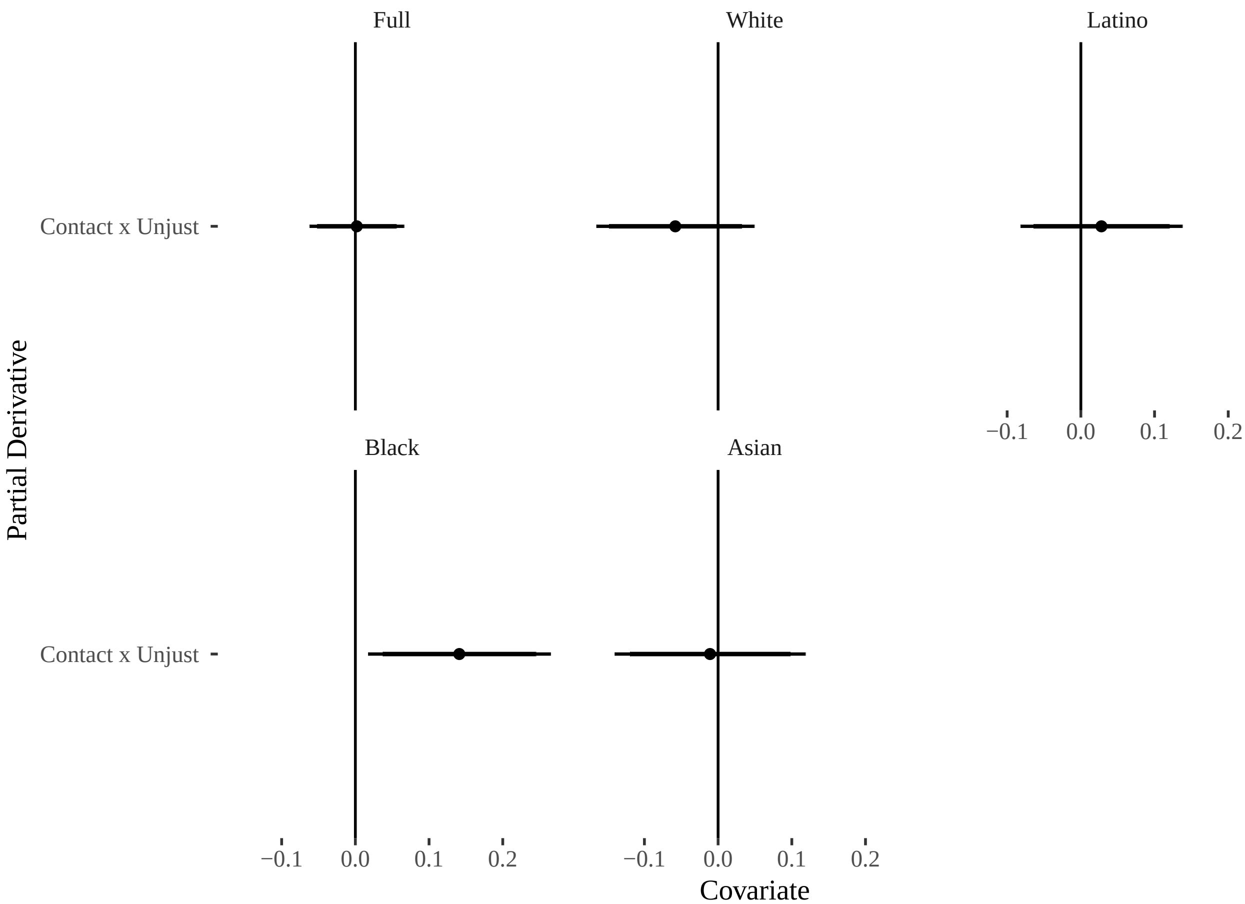

Figure A.11: Accounting for pre-treatment protest activity in vote outcome models with interaction

10 Validating the Proximal Contact Measure and Accounting for Geographic Context

10.1 Assessing if Proximal Contact is Correlated With Contextual Measures of Immigrant Contact and Enforcement

A potential concern over our proximal contact treatment is that those who report contact with immigrants, and undocumented immigrants specifically, may actually have limited contact and are simply signaling that they know someone undocumented. Or, those who report contact may actually have weak ties to their contact given that the context they operate within is typically non-immigrant or composed of demographics that are less tethered to the immigrant experience. Moreover, reporting bias may be exacerbated in the age of Trump, where politically motivated respondents could indicate they know someone undocumented to signal solidarity with immigrant communities.

In light of these concerns, we validate the proximal contact measure by assessing whether it is correlated with covariates that we would think suggest a high degree of contact with immigrant communities and more specifically, undocumented immigrants. Therefore, we assess the bivariate correlation between proximal contact and the proportion of Latinos in a respondent's geographic area of residence, the proportion of foreign-born individuals in a respondent's geographic area of residence, and the proportion of non-citizen individuals in a respondent's geographic area of residence. For these covariates we use both zipcode and county level measures. Moreover, we assess the correlation between the number of cumulative deportation removals through the federal government's Secure Communities program between 2009 and 2015 at the county level.[8] These covariates are theoretically motivated. percent Latino makes sense since most undocumented immigrants are from Latin American countries (and therefore those who are vulnerable to contact with immigration enforcement are mostly Latino).[9] Percent foreign born and percent non-citizen capture the degree to which respondents may be exposed to immigrant populations and perhaps, undocumented people. The number of removals in a county captures the degree to which individuals may have contact with those exposed to immigration enforcement. For this validation exercise, any model with geographic covariates on the right hand side uses clustered standard errors at the relevant level of geographic aggregation (e.g. county, zipcode). We conduct the validation exercise across all racial subsamples.

For the full sample, tables A.38 and A.43 demonstrate a positive correlation between proximal contact and percent Latino, percent foreign born, percent non-citizen, and the logged number of Secure Communities deportations (see figures A.12 and A.17 for a visualization of the bivariate correlation between contextual covariates and proximal contact at the zipcode and county level respectively). The same is true of the white sample (tables A.39 and A.44, figures A.13 and A.19). However, there does not appear to be a correlation between the aforementioned measures of geographic context and proximal contact for the Latino population, the black population, or the Asian population (tables A.40, A.41, A.42, A.45, A.46, and A.47, figures A.14, A.15, A.16, A.20, and A.21). Latino and Asian respondents that are in contexts that are not necessarily immigrant, Latino, nor Asian may have contact regardless of where they reside due to social, as opposed to community-based networks. This does not necessarily invalidate the proximal contact measure, since we may have a stronger prior expectation that Latinos or Asians who live in non-immigrant

[8] We log this covariate after adding 1 to the raw data to ensure that the covariate is defined after log transformation. We thank Rachel Torres and Rene Rocha for sharing the removal data with us.

[9] Although we acknowledge that the proportion of Latinos who are undocumented is relatively small, see https://www.huffpost.com/entry/politico-immigrants-latino_n_3142061.

contexts are going to have a better chance of knowing someone foreign-born or undocumented than a white person who lives in a mostly white or mostly non-immigrant context. It is unclear why there is little correlation between proximal contact and contextual measures that account for contact with immigrants for the black sample. The level of proximal contact for the black sample is relatively high (30% versus 20% for the white sample), so the result is somewhat puzzling. Potential explanations could be that black people in areas with a low number of immigrants may still be more likely to come into contact with immigrants (than say, white people in areas with a low number of immigrants) via particular occupational sectors where there is demographic overlap or with the few Latinos or immigrants that are perhaps more likely to move into black neighborhoods (McClain et al., 2007; Mohl, 2003).

Table A.38: Validating Proximal Contact with Geographic Zipcode Level Correlates (Zipcode cluster SE, Full Sample)

	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	0.004^{***}		
	(0.001)		
% Foreign		0.005^{***}	
		(0.001)	
% Non-Citizen			0.009^{***}
			(0.002)
R^2	0.030	0.021	0.019
Adj. R^2	0.029	0.021	0.018
Num. obs.	1643	1643	1643
RMSE	0.523	0.526	0.526

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.39: Validating Proximal Contact with Geographic Zipcode Level Correlates (Zipcode cluster SE, White Sample)

	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	0.003^{*}		
	(0.002)		
% Foreign		0.006^{*}	
		(0.002)	
% Non-Citizen			0.007
			(0.004)
R^2	0.016	0.020	0.009
Adj. R^2	0.014	0.018	0.007
Num. obs.	413	413	413
RMSE	0.773	0.772	0.776

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.40: Validating Proximal Contact with Geographic Zipcode Level Correlates (Zipcode cluster SE, Latino Sample)

	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	0.000		
	(0.001)		
% Foreign		-0.001	
		(0.002)	
% Non-Citizen			0.001
			(0.004)
R^2	0.001	0.002	0.001
Adj. R^2	-0.002	-0.000	-0.002
Num. obs.	413	413	413
RMSE	0.455	0.454	0.455

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.41: Validating Proximal Contact with Geographic Zipcode Level Correlates (Zipcode cluster SE, Black Sample)

	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	-0.000		
	(0.001)		
% Foreign		-0.000	
		(0.002)	
% Non-Citizen			0.000
			(0.003)
R^2	0.000	0.000	0.000
Adj. R^2	-0.002	-0.002	-0.002
Num. obs.	406	406	406
RMSE	0.430	0.430	0.430

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.42: Validating Proximal Contact with Geographic Zipcode Level Correlates (Zipcode cluster SE, Asian Sample)

	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	0.002		
	(0.001)		
% Foreign		0.002	
		(0.002)	
% Non-Citizen			0.000
			(0.004)
R^2	0.003	0.005	0.000
Adj. R^2	0.001	0.002	-0.002
Num. obs.	411	411	411
RMSE	0.226	0.225	0.226

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.43: Validating Proximal Contact with Geographic County Level Correlates (County Cluster SE, Full Sample)

	Prox. Contact	Prox. Contact	Prox. Contact	Prox Contact
% Latino	0.004^{***}			
	(0.001)			
% Foreign		0.006^{***}		
		(0.001)		
% Non-Citizen			0.012^{***}	
			(0.002)	
Log(Removals + 1)				0.019^{**}
				(0.006)
R^2	0.022	0.020	0.021	0.011
Adj. R^2	0.022	0.019	0.020	0.011
Num. obs.	1643	1643	1643	1629
RMSE	0.525	0.526	0.526	0.529

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.44: Validating Proximal Contact with Geographic County Level Correlates (County Cluster SE, White Sample)

	Prox. Contact	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	0.003*			
	(0.001)			
% Foreign		0.005*		
		(0.002)		
% Non-Citizen			0.010*	
			(0.004)	
Log(Removals + 1)				0.017*
				(0.007)
R^2	0.011	0.015	0.012	0.011
Adj. R^2	0.009	0.013	0.010	0.008
Num. obs.	413	413	413	408
RMSE	0.775	0.774	0.775	0.779

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.45: Validating Proximal Contact with Geographic County Level Correlates (County Cluster SE, Latino Sample)

	Prox. Contact	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	0.000			
	(0.002)			
% Foreign		−0.001		
		(0.004)		
% Non-Citizen			0.001	
			(0.009)	
Log(Removals + 1)				0.004
				(0.016)
R^2	0.000	0.001	0.000	0.000
Adj. R^2	-0.002	-0.002	-0.002	-0.002
Num. obs.	413	413	413	412
RMSE	0.455	0.455	0.455	0.454

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.46: Validating Proximal Contact with Geographic County Level Correlates (County Cluster SE, Black Sample)

	Prox. Contact	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	−0.000			
	(0.002)			
% Foreign		0.001		
		(0.003)		
% Non-Citizen			0.002	
			(0.006)	
Log(Removals + 1)				−0.013
				(0.011)
R^2	0.000	0.001	0.000	0.004
Adj. R^2	-0.002	-0.001	-0.002	0.001
Num. obs.	406	406	406	406
RMSE	0.430	0.429	0.430	0.429

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.47: Validating Proximal Contact with Geographic County Level Correlates (County Cluster SE, Asian Sample)

	Prox. Contact	Prox. Contact	Prox. Contact	Prox. Contact
% Latino	0.002			
	(0.001)			
% Foreign		0.003		
		(0.002)		
% Non-Citizen			0.007	
			(0.005)	
Log(Removals + 1)				0.011
				(0.011)
R^2	0.006	0.007	0.006	0.003
Adj. R^2	0.003	0.004	0.004	0.001
Num. obs.	411	411	411	403
RMSE	0.225	0.225	0.225	0.223

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

10.2 Visual Representation of Bivariate Correlations Between Contextual Covariates and Proximal Contact

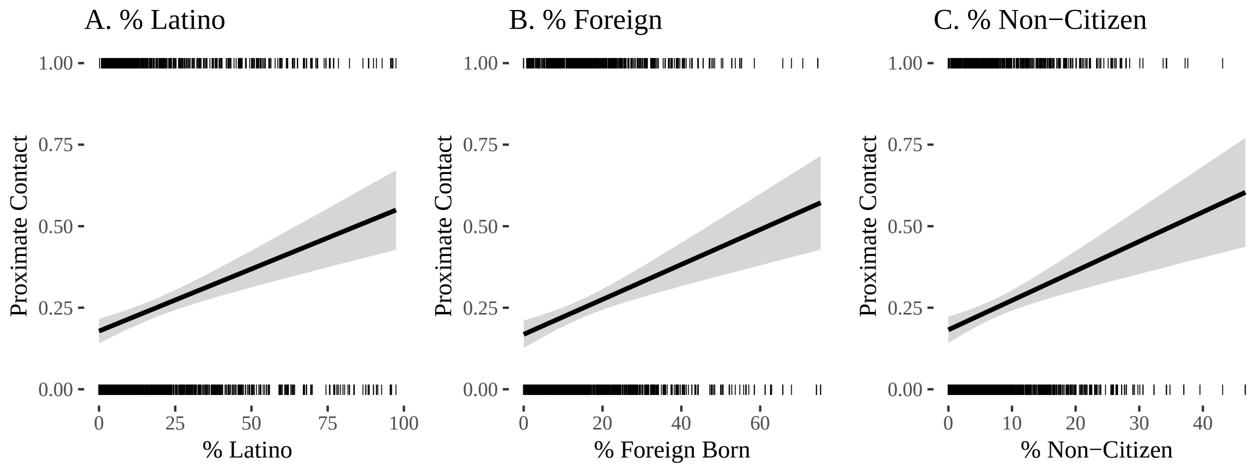

Figure A.12: Bivariate Correlation Between Zipcode Context Covariates and Proximal Contact (Full Sample, Weighted Estimates, HC2 Robust SE)

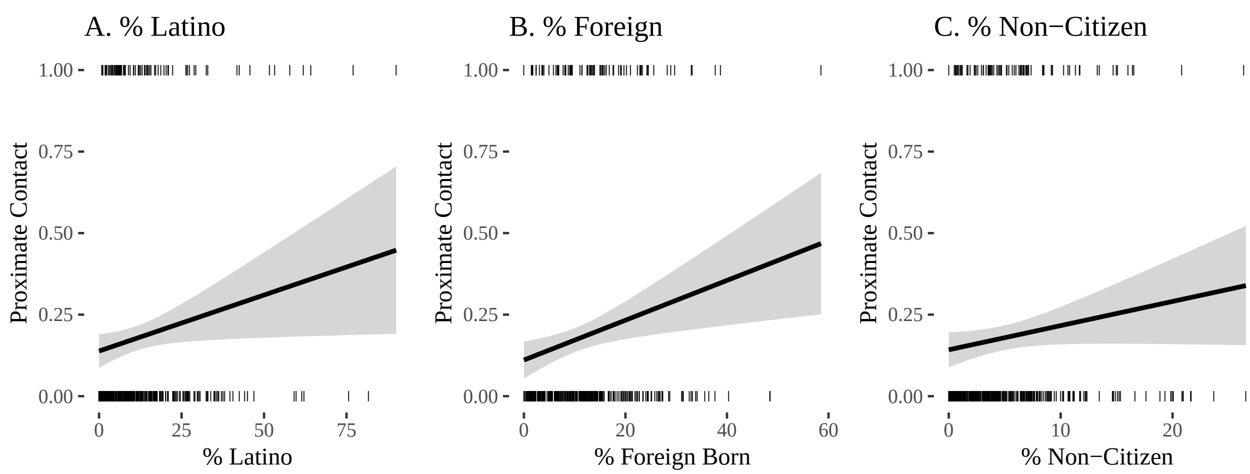

Figure A.13: Bivariate Correlation Between Zipcode Context Covariates and Proximal Contact (White Sample, Weighted Estimates, HC2 Robust SE)

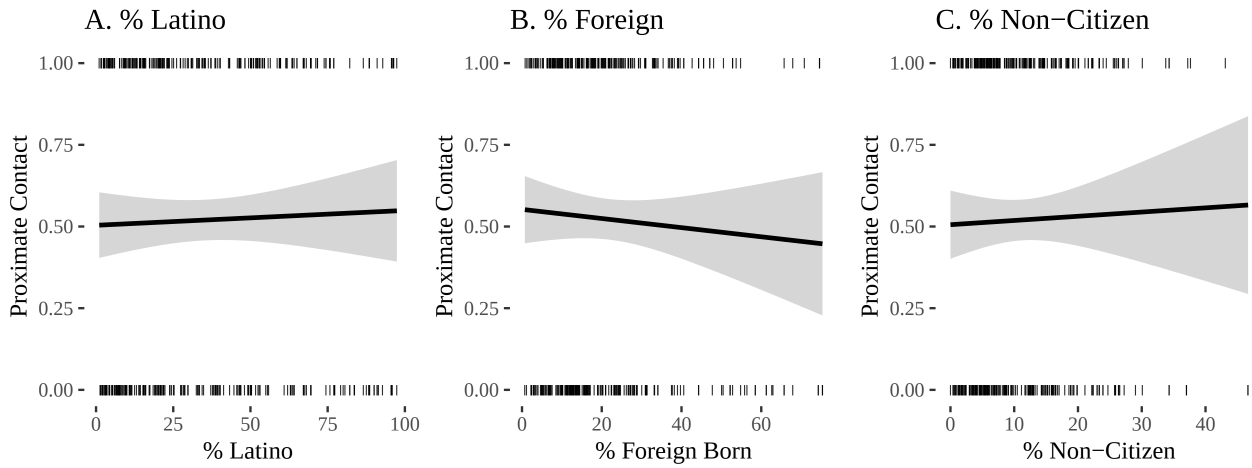

Figure A.14: Bivariate Correlation Between Zipcode Context Covariates and Proximal Contact (Latino Sample, Weighted Estimates, HC2 Robust SE)

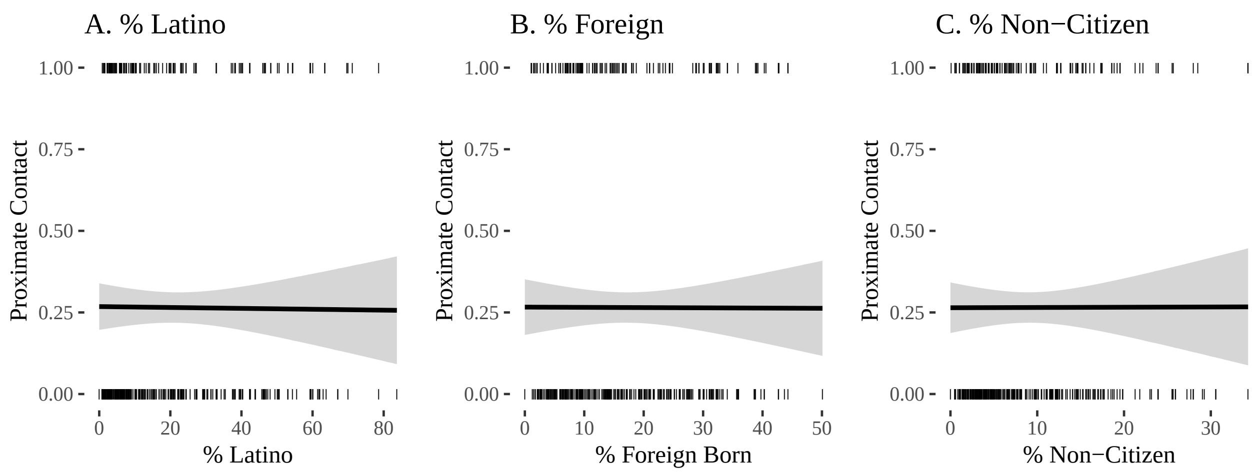

Figure A.15: Bivariate Correlation Between Zipcode Context Covariates and Proximal Contact (Black Sample, Weighted Estimates, HC2 Robust SE)

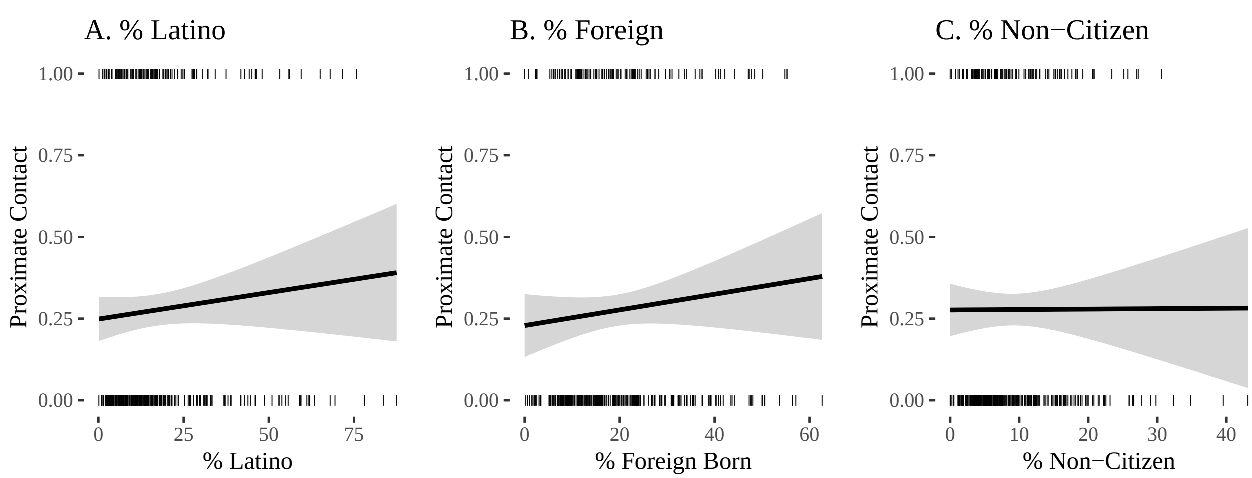

Figure A.16: Bivariate Correlation Between Zipcode Context Covariates and Proximal Contact (Asian Sample, Weighted Estimates, HC2 Robust SE)

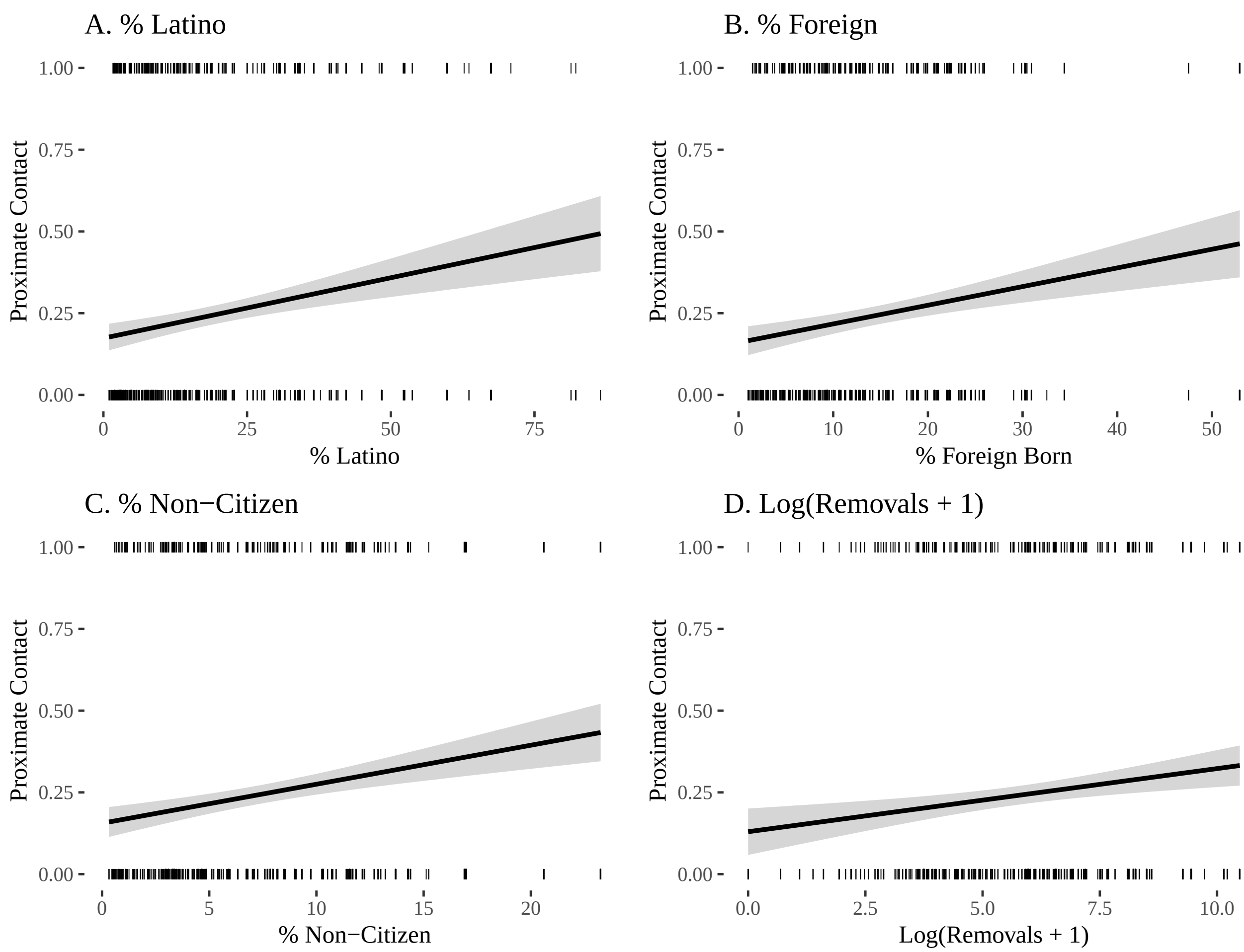

Figure A.17: Bivariate Correlation Between County Context Covariates and Proximal Contact (Full Sample, Weighted Estimates, HC2 Robust SE)

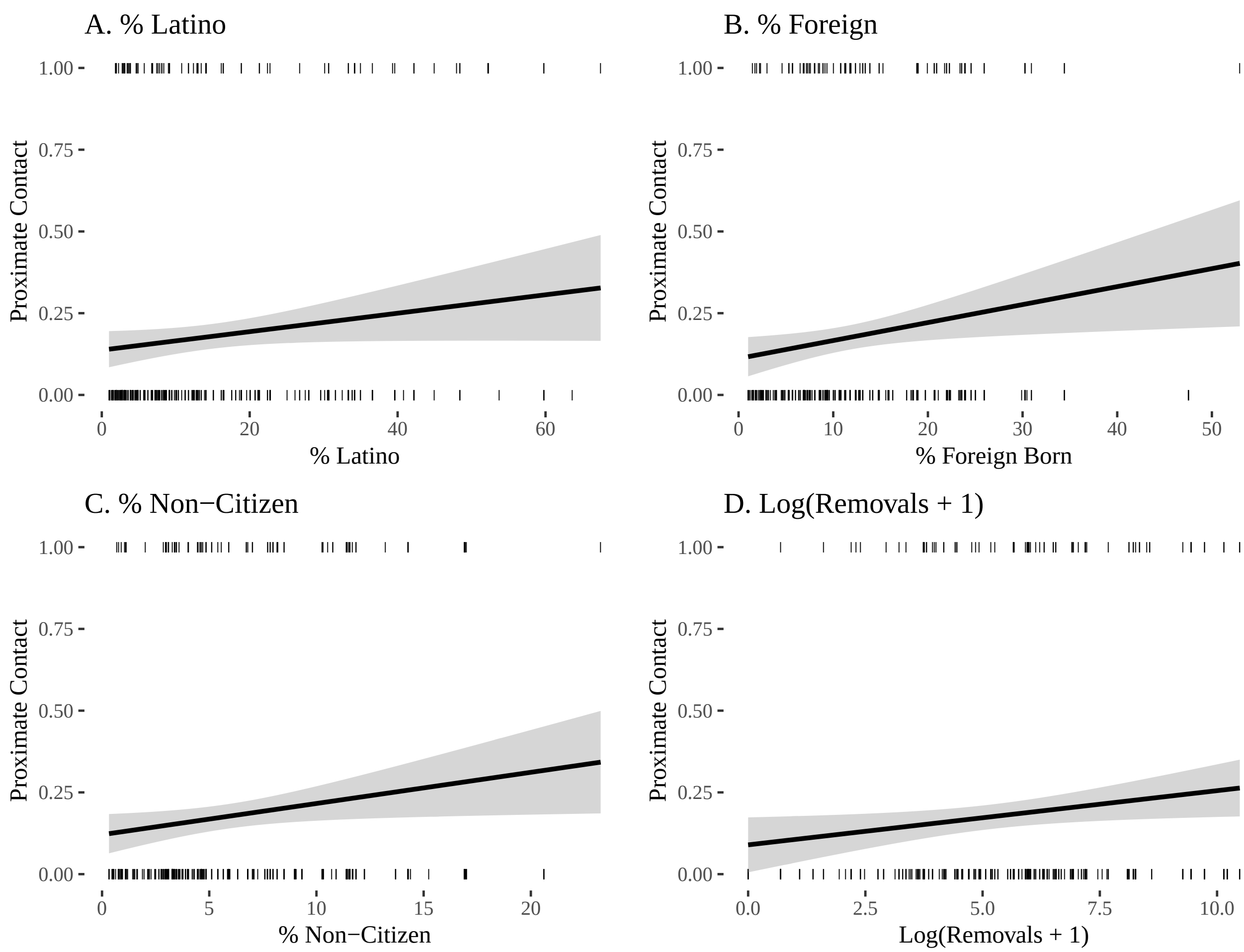

Figure A.18: Bivariate Correlation Between County Context Covariates and Proximal Contact (White Sample, Weighted Estimates, HC2 Robust SE)

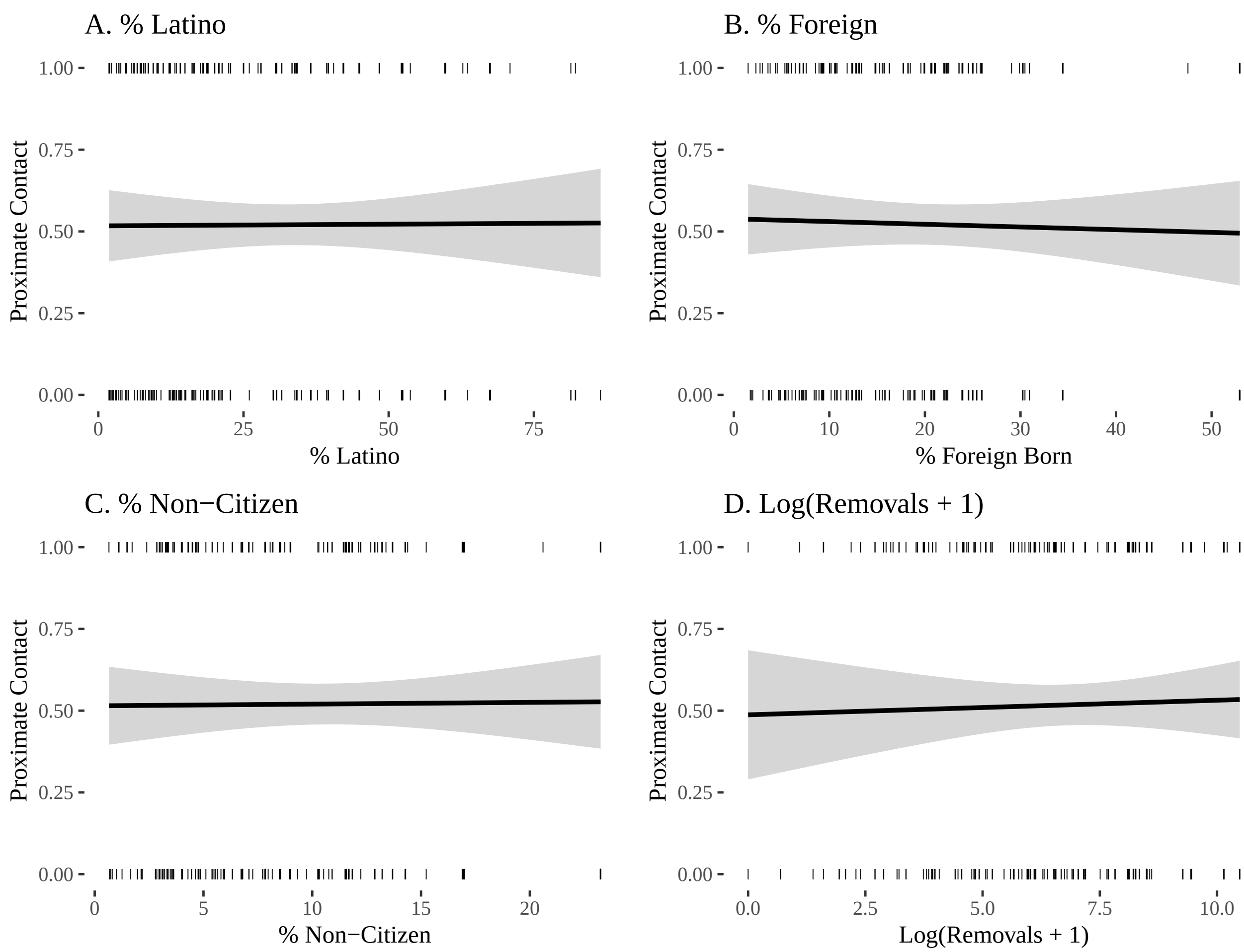

Figure A.19: Bivariate Correlation Between County Context Covariates and Proximal Contact (Latino Sample, Weighted Estimates, HC2 Robust SE)

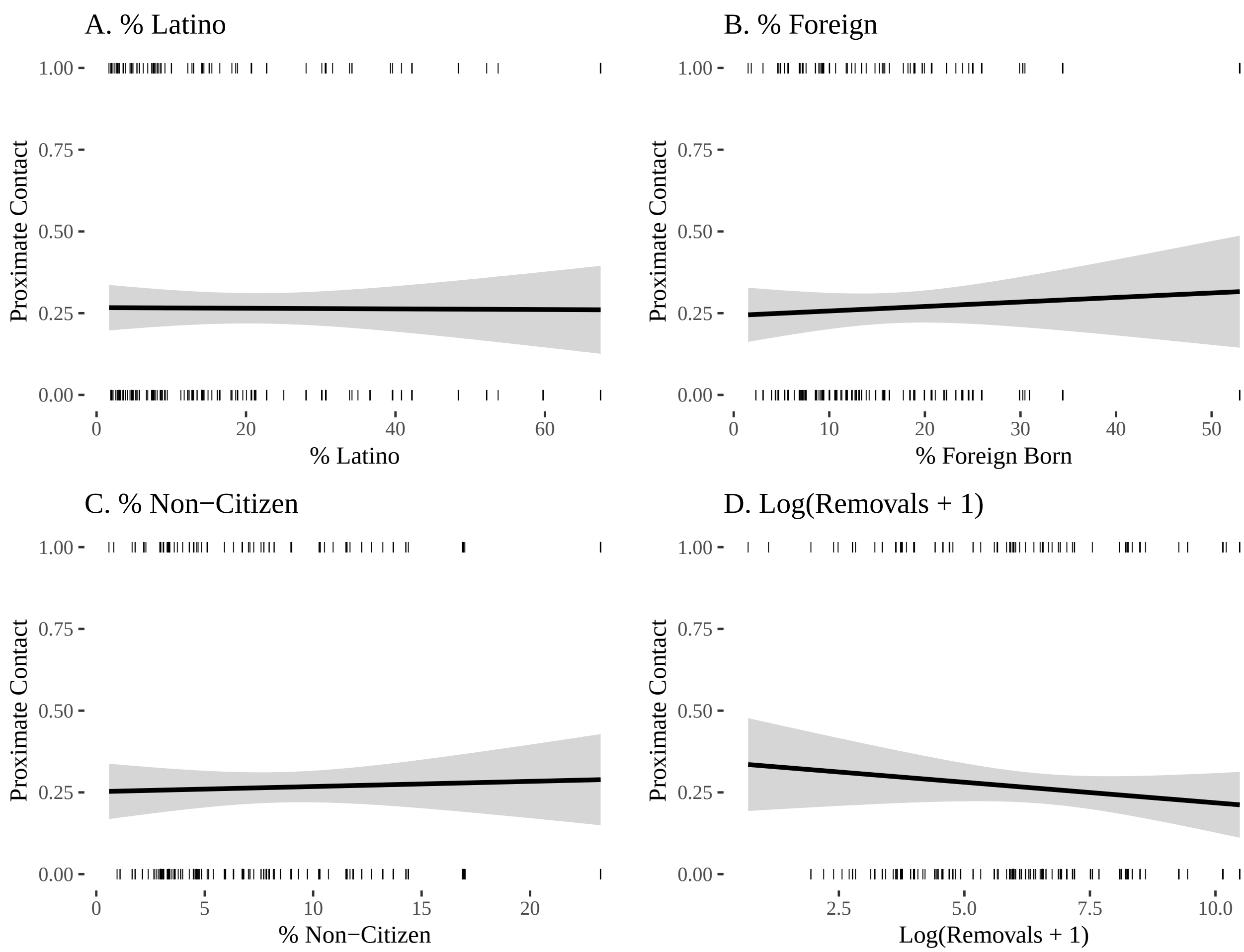

Figure A.20: Bivariate Correlation Between County Context Covariates and Proximal Contact (Black Sample, Weighted Estimates, HC2 Robust SE)

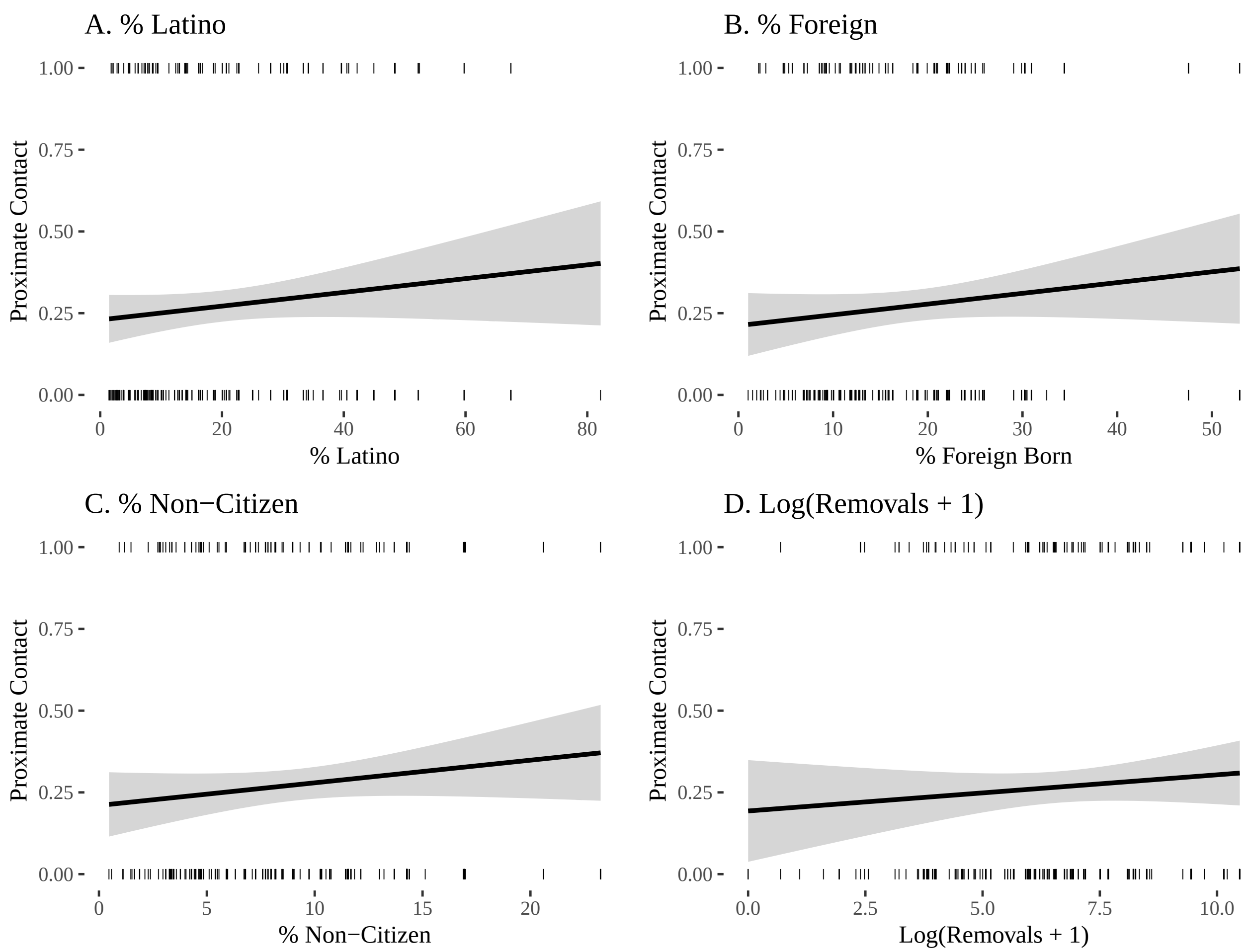

Figure A.21: Bivariate Correlation Between County Context Covariates and Proximal Contact (Asian Sample, Weighted Estimates, HC2 Robust SE)

10.3 Accounting for Contextual Factors by Conditioning on Geographic Fixed Effects

We rule out the possibility that geographically contextual factors may be influencing the associations we derive in the main results. These factors may be important, as demographic context, differences in immigration enforcement across space, and differences in the degree of state and local cooperation with federal immigration authorities may influence both selection into proximal contact and how individuals respond to enforcement. In a cross-sectional framework, we can effectively condition on context with the inclusion of geographic fixed effects. Even with the inclusion of both county and zipcode level fixed effects, we derive partial derivatives that are consistent with the main results. Table A.48 displays the partial derivative of proximal contact with respect to perceived injustice conditional on relevant covariates and county fixed effects. Like the main results, the partial derivative is statistically significant for the Latino and full sample. However, unlike the main results, the partial derivative is statistically insignificant for the white sample, but it is in the correct direction and still substantively larger relative to the Asian or Black sample.

Table A.49 displays the partial derivative of proximal contact with respect to perceived injustice conditional on relevant covariates and zipcode fixed effects. The model characterizing the association between proximal contact and percieved injustice for the white and Asian sample is excluded due to the lack of model identification using zipcode fixed effects. Again, we find results consistent with those in the main text, with proximal contact being positively and statistically significantly associated with perceived injustice in the full sample and the Latino sample (albeit at the $p < 0.10$ level for the Latino sample).

We assess the association between proximal contact and political participation conditional on geographic context. Table A.50 displays the partial derivatives of proximal contact on protest activity conditional on relevant covariates and county fixed effects (the asian sample is excluded due to lack of model identification). Here, we find that the partial derivative is positive and statistically significantly associated with protest activity acrtoss all subsamples. We conduct the same exercise with zipcode level fixed effects on table A.51, and find similar results consistent with the main conclusions of the paper (although we could not identify partial derivatives at the zip code level for whites and Asians).

Additionally, we also ensure that the association between proximal contact and voting behavior is not statistically significant. Tables A.52 and A.53 displays the partial derivatives of proximal contact on voting activity conditional on relevant covariates and both county and zipcode fixed effects (any samples that are excluded are the result of no model identification). Like the main results, we do not find a statistically significant association between proximal contact and voting activity.

We also replicate our results testing for an association between the interaction of proximal contact with perceived injustice on protest activity conditional on geographic context (tables A.54 and A.55). We find that the interaction between proximal contact and perceived injustice is still positive and statistically significant conditional on county fixed effects, but not on zipcode fixed effects (albeit the partial derivative is in the correct direction).

Table A.48: The association between proximal contact and perceived injustice holds after conditioning for county fixed effects

	Full	White	Latino	Black	Asian
Proximal Contact	0.12^{***}	0.09	0.21^{***}	0.02	0.05
	(0.03)	(0.07)	(0.06)	(0.06)	(0.06)
R^2	0.42	0.56	0.49	0.37	0.34
Adj. R^2	0.33	0.28	0.24	0.15	0.07
Num. obs.	1643	413	413	406	411
RMSE	1.02	1.65	0.82	0.87	0.41

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.49: The association between proximal contact and perceived injustice holds after conditioning for zipcode fixed effects (whites and Asians omitted due to lack of model identification)

	Full	Latino	Black
Proximal Contact	0.11^{*}	0.25	-0.01
	(0.06)	(0.13)	(0.10)
R^2	0.79	0.88	0.82
Adj. R^2	0.48	0.38	0.24
Num. obs.	1643	413	406
RMSE	0.90	0.74	0.82

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.50: The association between proximal contact and protest activity holds after conditioning for county fixed effects (asian sample excluded due to lack of model identification)

	Full	White	Latino	Black	Asian
Proximal Contact	0.27^{***}	0.18^{*}	0.33^{***}	0.31^{***}	0.29^{***}
	(0.04)	(0.08)	(0.07)	(0.08)	(0.07)
R^2	0.29	0.43	0.54	0.38	0.38
Adj. R^2	0.18	0.07	0.31	0.16	0.11
Num. obs.	1643	413	413	406	411
RMSE	0.99	1.47	0.86	0.95	0.42

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.51: The association between proximal contact and protest activity holds after conditioning for zipcode fixed effects (whites and asians excluded due to lack of model identification)

	Full	Latino	Black
Proximal Contact	0.29^{***}	0.25^{**}	0.35^{**}
	(0.05)	(0.08)	(0.11)
R^2	0.75	0.93	0.84
Adj. R^2	0.38	0.63	0.30
Num. obs.	1643	413	406
RMSE	0.86	0.63	0.87

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.52: The association between proximal contact and voting activity holds after conditioning for county fixed effects

	Full	White	Latino	Black	Asian
Proximal Contact	-0.04	-0.07	-0.02	0.00	0.02
	(0.04)	(0.06)	(0.07)	(0.06)	(0.07)
R^2	0.34	0.50	0.42	0.42	0.37
Adj. R^2	0.24	0.18	0.13	0.21	0.10
Num. obs.	1643	413	413	406	411
RMSE	1.05	1.72	0.85	0.86	0.48

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.53: The association between proximal contact and voting activity holds after conditioning for zipcode fixed effects (whites not included due to lack of model identification)

	Full	Latino	Black	Asian
Proximal Contact	0.05	0.10	0.04	0.21
	(0.06)	(0.12)	(0.10)	(0.13)
R^2	0.79	0.87	0.86	0.89
Adj. R^2	0.47	0.30	0.39	0.36
Num. obs.	1643	413	406	411
RMSE	0.88	0.76	0.76	0.40

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.54: The association between proximal contact and protest activity holds after conditioning for county fixed effects

	Full	White	Latino	Black	Asian
Contact * Injustice	0.09**	0.08	0.04	0.03	0.09
	(0.04)	(0.07)	(0.06)	(0.08)	(0.07)
R^2	0.30	0.43	0.54	0.38	0.38
Adj. R^2	0.19	0.07	0.31	0.16	0.11
Num. obs.	1643	413	413	406	411
RMSE	0.98	1.47	0.86	0.95	0.42

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.55: The association between proximal contact and protest activity holds after conditioning for zipcode fixed effects (white and Asian sample excluded due to lack of model identification)

	Full	Latino	Black
Contact * Injustice	0.06	0.08	0.03
	(0.05)	(0.10)	(0.08)
R^2	0.75	0.93	0.38
Adj. R^2	0.38	0.63	0.16
Num. obs.	1643	413	406
RMSE	0.86	0.63	0.95

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

10.4 Exploring Heterogeneity by Geographic Context

We explore heterogeneity by context. First, we assess if proximal contact has heterogenous partial derivatives by the number of removals at the county level through Secure Communities, the proportion of removals that are felony criminal removals at the county level through Secure Communities, and the proportion of non-citizens at the zip code level. We conduct this exercise for both the perceived injustice and protest outcomes. Then, we assess triple interactions between perceived injustice, proximal contact, and the aforementioned contextual covariates (i.e. removals, proportion felony removals, and percent non-citizen) with the protest outcome.

Tables A.56, A.57 and A.58 demonstrates no association between interactions of proximal contact with contextual covariates and percieved injustice. Tables A.59, A.60, A.61 nearly demonstrates no association between interactions of proximal contact with contextual covariates and protest activity. One exception is the interaction of the logged number of removals (plus 1) with proximal contact and protest for the black and Asian sample. However, the coefficients are in opposing directions, which is atheoretical. Moreover, we are concerned about interpreting multiplicative terms with covariates at higher levels of geographic aggregation in a small subsample framework. Tables A.62, A.63 and A.64 display triple interactions of proximal contact with perceived injustice with a relevant geographic covariate. There is no heterogeneity across all three contextual/geographic measures, leading us to conclude that context does not play a role in moderating the influence of proximal contact or perceived injustice.

Table A.56: Association between Interaction of Proximal Contact with Logged Removals and Perceived Injustice Conditional on Relevant Covariates (Full Sample, County Cluster SE)

	Full	White	Latino	Black	Asian
Contact x Log(Removals + 1)	−0.00	−0.05	0.15	0.12	−0.03
	(0.07)	(0.12)	(0.11)	(0.11)	(0.11)
R^2	0.24	0.25	0.18	0.07	0.11
Adj. R^2	0.23	0.21	0.15	0.03	0.07
Num. obs.	1629	408	412	406	403
RMSE	2.47	3.89	1.95	2.08	0.91

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.57: Association between Interaction of Proximal Contact with Percent Criminal Removals and Perceived Injustice Conditional on Relevant Covariates

	Full	White	Latino	Black	Asian
Contact x % Criminal	0.01	0.01	−0.00	0.05	0.00
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)
R^2	0.24	0.25	0.18	0.08	0.11
Adj. R^2	0.23	0.21	0.15	0.04	0.07
Num. obs.	1629	408	412	406	403
RMSE	2.47	3.90	1.95	2.07	0.91

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.58: Association between Interaction of Proximal Contact with Percent Non-Citizen and Perceived Injustice Conditional on Relevant Covariates

	Full	White	Latino	Black	Asian
Contact x % Non-Citizen	-0.01	-0.00	-0.03	-0.01	-0.01
	(0.02)	(0.05)	(0.03)	(0.03)	(0.04)
R^2	0.24	0.25	0.18	0.07	0.10
Adj. R^2	0.24	0.21	0.15	0.03	0.06
Num. obs.	1643	413	413	406	411
RMSE	2.46	3.89	1.95	2.09	0.92

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.59: Association between Interaction of Proximal Contact with Logged Removals and Protest Conditional on Relevant Covariates

	Full	White	Latino	Black	Asian
Contact x Log(Removals + 1)	-0.01	-0.01	0.02	-0.06^{**}	0.04^{*}
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
R^2	0.15	0.14	0.19	0.21	0.17
Adj. R^2	0.14	0.10	0.16	0.18	0.14
Num. obs.	1629	408	412	406	403
RMSE	0.37	0.53	0.35	0.34	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.60: Association between Interaction of Proximal Contact with Percent Criminal Removals and Protest Conditional on Relevant Covariates

	Full	White	Latino	Black	Asian
Contact x % Criminal	0.00	0.00	0.01	-0.00	0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
R^2	0.15	0.15	0.20	0.20	0.17
Adj. R^2	0.14	0.11	0.16	0.17	0.13
Num. obs.	1629	408	412	406	403
RMSE	0.37	0.53	0.35	0.35	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.61: Association between Interaction of Proximal Contact with Percent Non-Citizen and Protest Conditional on Relevant Covariates

	Full	White	Latino	Black	Asian
Contact x % Non-Citizen	0.00	0.02*	−0.01	−0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
R^2	0.15	0.16	0.21	0.21	0.16
Adj. R^2	0.14	0.13	0.17	0.17	0.12
Num. obs.	1643	413	413	406	411
RMSE	0.37	0.52	0.35	0.35	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.62: Triple Interaction of Proximal Contact with Perceived Injustice with Log(Removals + 1) and Protest Conditional on Relevant Covariates

	Full	White	Latino	Black	Asian
Contact x Injustice x Log(Removals + 1)	0.01	0.01	0.01	−0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.17	0.17	0.21	0.23	0.18
Adj. R^2	0.15	0.13	0.16	0.18	0.14
Num. obs.	1629	408	412	406	403
RMSE	0.37	0.52	0.35	0.34	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.63: Triple Interaction of Proximal Contact with Perceived Injustice with percent criminal and Protest Conditional on Relevant Covariates

	Full	White	Latino	Black	Asian
Contact x Injustice x % Criminal	0.00	0.00	0.00	0.00	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.16	0.17	0.21	0.21	0.18
Adj. R^2	0.15	0.12	0.17	0.17	0.14
Num. obs.	1629	408	412	406	403
RMSE	0.37	0.52	0.35	0.35	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.64: Triple Interaction of Proximal Contact with Perceived Injustice with percent Non-Citizen and Protest Conditional on Relevant Covariates

	Full	White	Latino	Black	Asian
Contact x Injustice x % Non-Citizen	0.00	0.01	0.00	−0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.16	0.18	0.21	0.22	0.17
Adj. R^2	0.14	0.14	0.17	0.18	0.12
Num. obs.	1643	413	413	406	411
RMSE	0.37	0.52	0.35	0.34	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

10.5 Assessing the Influence of State-Level Sanctuary Laws

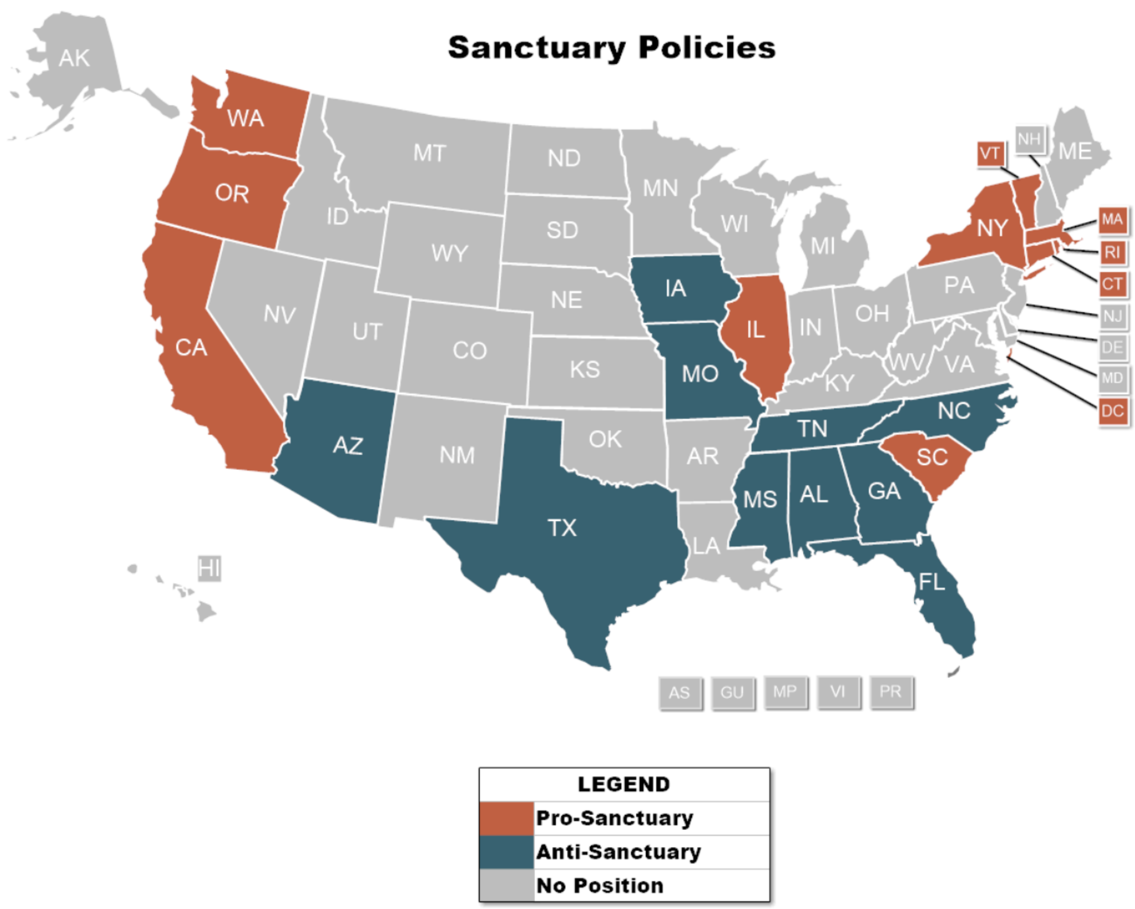

Figure A.22: States by Sanctuary Status in 2019 as per the National Council of State Legislatures (NCSL)

We use data from the National Council of State Legislatures and construct dichotomous measures for whether a state is neutral, pro-sanctuary, or anti-sanctuary with respect to immigration enforcement. Anti-sanctuary states have laws on the books that prohibit localities within their jurisdiction from actively refusing to cooperate with federal authorities in enforcing immigration law (e.g. Texas SB 4). Sanctuary states do the opposite, and even prohibit law enforcement from holding undocumented immigrants as detainees for ICE (e.g. California AB 110).[10] With this data, we assess if the partial derivatives of proximal contact and the interaction of proximal contact with perceived injustice are conditional on whether respondents are from anti-sanctuary states, pro-sanctuary states, or neutral states.

For the most part, we do not find any meaningful sources of heterogeneity across state-level sanctuary law context. Table A.65 displays the partial derivatives for the association between proximal contact and perceived injustice subsetted by state-level sanctuary laws. Column 1 characterizes the sanctuary state subset, column 2 characterizes the neutral state subset, column 3 characterizes the anti-sanctuary state subset. Column 4 characterizes a model using all the data, but where proximal contact is interacted with dichotomous measures for sanctuary and anti-sanctuary states, with neutral states being the reference category.[11] Columns 1-3 demonstrates that proximal contact is still statistically significantly associated with perceived injustice regardless of state-level sanctuary context. Column 4 demonstrates that proximal contact does not have partial derivatives conditional on pro- or anti-sanctuary context relative to a neutral context. Table A.70 displays the partial derivatives for the association between proximal contact and protest activity. Across all sanctuary context subsets, proximal contact is still independently positively and statistically significantly associated with protest activity (columns 1-3). Moreover, there does not appear to be heterogeneity in the partial derivatives for proximal contact conditional on sanctuary context (column 4). On table A.75, the association between proximal contact and voting is displayed. Consistent with the main results, there does not appear to be any association between proximal contact and voting based on different subsets of state-level sanctuary context or conditional on state-level sanctuary context.

Table A.80 displays the partial derivatives of the interaction between proximal contact and perceived injustice across the state-level sanctuary contexts (columns 1-3). It also displays the partial derivatives of triple interactions between proximal contact, perceived injustice, and state-level sanctuary context (column 4). Although it appears that the interactive partial derivative is larger in areas that are neutral with respect to state cooperation with federal immigration authorities, the triple interactions do not indicate that individuals in places with anti- or pro-sanctuary laws are less likely to be influenced by the interaction between proximal contact and perceived injustice.

Table A.85 is the result of a similar exercise as table A.80, but where the outcome is vote intention. Here, it appears to be the case that respondents in anti-sanctuary states are more likely to vote conditional on the interaction between proximal contact and perceived injustice relative to respondents living in neutral states. One potential explanation is that likely voters who may be mobilized by injustice will be more likely to vote to change policy circumstances locally if they understand that there are anti-immigrant laws on the books.[12] The anti-sanctuary

[10] For more details on measurement and the states which have implemented sanctuary laws, see http://www.ncsl.org/research/immigration/sanctuary-policy-faq635991795.aspx. Moreover, see figure A.22, a visual representation of which states are pro- or anti-sanctuary. In our analysis, we code Florida as a neutral state since Florida only recently passed anti-sanctuary legislation in June 2019 (SB 168). Since our survey was fielded in 2018, coding Florida as anti-sanctuary could generate post-treatment bias.

[11] When state-level covariates are on the right hand side of the model, robust standard errors are clustered at the state-level.

[12] Although this is a somewhat problematic explanation given that our outcome is related to the 2018 Midterm Election, with many elections occurring at the federal level. However, it could be the case that voters are also motivated

law being on the books may commpel voters to turn out more than in contexts where there are limited anti-immigrant policies.

In addition, we include tables assessing heterogenous partial derivatives for proximal contact and perceived injustice across context-based samples and conditional on context for each racial subsample. We mostly include these tables for the sake of transparency, however, we hesitate to interpret these results since many of them rely on particularly small sample sizes per contextual subset.

to vote for downballot state-level candidates.

10.5.1 Association Between Proximal Contact and Perceived Injustice

Table A.65: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Perceived Injustice (Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.16^{*}	0.10	0.20^{**}	0.08
	(0.07)	(0.05)	(0.08)	(0.04)
Sanctuary State				0.01
				(0.07)
Anti-Sanctuary State				0.06
				(0.08)
Prox. Contact x Sanctuary				0.08
				(0.11)
Prox. Contact x Anti-Sanctuary				0.11
				(0.06)
R^2	0.24	0.26	0.36	0.24
Adj. R^2	0.21	0.24	0.32	0.23
Num. obs.	480	827	336	1643
RMSE	1.11	1.09	1.03	1.10

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.66: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Perceived Injustice (White Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.21^{*}	0.17^{*}	0.20	0.08
	(0.10)	(0.08)	(0.13)	(0.09)
Sanctuary State				0.06
				(0.11)
Anti-Sanctuary State				0.05
				(0.18)
Prox. Contact x Sanctuary				0.13
				(0.19)
Prox. Contact x Anti-Sanctuary				0.14
				(0.14)
R^2	0.28	0.19	0.42	0.25
Adj. R^2	0.17	0.16	0.30	0.21
Num. obs.	117	213	83	413
RMSE	1.85	1.76	1.61	1.73

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.67: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Perceived Injustice (Latino Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.07	0.13	0.33**	0.18
	(0.13)	(0.09)	(0.10)	(0.09)
Sanctuary State				0.00
				(0.23)
Anti-Sanctuary State				−0.03
				(0.17)
Prox. Contact x Sanctuary				−0.11
				(0.15)
Prox. Contact x Anti-Sanctuary				0.04
				(0.12)
R^2	0.17	0.25	0.36	0.18
Adj. R^2	0.05	0.18	0.25	0.14
Num. obs.	116	190	107	413
RMSE	0.86	0.90	0.77	0.87

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.68: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Perceived Injustice (Black Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.10	0.00	0.26	0.00
		(0.07)	(0.13)	(0.10)
Sanctuary State				−0.08
				(0.11)
Anti-Sanctuary State				0.27*
				(0.10)
Prox. Contact x Sanctuary				0.12
				(0.11)
Prox. Contact x Anti-Sanctuary				0.13
				(0.14)
R^2	0.22	0.06	0.29	0.08
Adj. R^2	0.04	0.01	0.10	0.04
Num. obs.	80	251	75	406
RMSE	1.05	0.90	0.87	0.92

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.69: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Perceived Injustice (Asian Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.07	0.13	−0.16	0.13
	(0.08)	(0.08)	(0.11)	(0.11)
Sanctuary State				−0.03
				(0.07)
Anti-Sanctuary State				0.00
				(0.08)
Prox. Contact x Sanctuary				−0.07
				(0.15)
Prox. Contact x Anti-Sanctuary				−0.17
				(0.17)
R^2	0.13	0.15	0.39	0.10
Adj. R^2	0.04	0.08	0.24	0.06
Num. obs.	167	173	71	411
RMSE	0.45	0.39	0.33	0.41

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

10.5.2 Association Between Proximal Contact and Protest

Table A.70: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Protest (Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.06^{**}	0.11^{***}	0.09^{**}	0.11^{**}
	(0.02)	(0.02)	(0.03)	(0.02)
Sanctuary State				−0.02
				(0.03)
Anti-Sanctuary State				−0.04
				(0.05)
Prox. Contact x Sanctuary				−0.05
				(0.03)
Prox. Contact x Anti-Sanctuary				−0.01
				(0.05)
R^2	0.16	0.17	0.26	0.15
Adj. R^2	0.13	0.15	0.22	0.14
Num. obs.	480	827	336	1643
RMSE	0.36	0.39	0.33	0.37

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.71: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Protest (White Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.00	0.11^{**}	0.08	0.10^{*}
		(0.04)	(0.05)	(0.04)
Sanctuary State				-0.06
				(0.03)
Anti-Sanctuary State				-0.07
				(0.08)
Prox. Contact x Sanctuary				-0.10
				(0.06)
Prox. Contact x Anti-Sanctuary				-0.03
				(0.10)
R^2	0.15	0.21	0.28	0.17
Adj. R^2	0.01	0.14	0.12	0.13
Num. obs.	117	213	83	413
RMSE	0.52	0.57	0.39	0.52

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.72: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Protest (Latino Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.19^{***}	0.10^{**}	0.08	0.10
	(0.04)	(0.04)	(0.05)	(0.04)
Sanctuary State				-0.00
				(0.06)
Anti-Sanctuary State				-0.01
				(0.09)
Prox. Contact x Sanctuary				0.04
				(0.06)
Prox. Contact x Anti-Sanctuary				-0.04
				(0.06)
R^2	0.41	0.23	0.27	0.19
Adj. R^2	0.32	0.16	0.14	0.15
Num. obs.	116	190	107	413
RMSE	0.33	0.32	0.37	0.35

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.73: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Protest (Black Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.10	0.14^{***}	0.10	0.14^{**}
		(0.03)	(0.06)	(0.02)
Sanctuary State				0.05
				(0.06)
Anti-Sanctuary State				-0.01
				(0.06)
Prox. Contact x Sanctuary				-0.02
				(0.04)
Prox. Contact x Anti-Sanctuary				-0.00
				(0.06)
R^2	0.31	0.22	0.36	0.21
Adj. R^2	0.13	0.17	0.19	0.16
Num. obs.	80	251	75	406
RMSE	0.39	0.34	0.33	0.35

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.74: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Protest (Asian Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.12^{***}	0.03	0.12^{*}	0.04
	(0.03)	(0.03)	(0.05)	(0.03)
Sanctuary State				-0.01
				(0.04)
Anti-Sanctuary State				-0.02
				(0.05)
Prox. Contact x Sanctuary				0.08
				(0.04)
Prox. Contact x Anti-Sanctuary				0.09
				(0.06)
R^2	0.29	0.14	0.35	0.17
Adj. R^2	0.21	0.06	0.18	0.13
Num. obs.	167	173	71	411
RMSE	0.15	0.16	0.12	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

10.5.3 Association Between Proximal Contact and Voting

Table A.75: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Voting (Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.00	−0.04	−0.01	−0.03
	(0.03)	(0.03)	(0.04)	(0.03)
Sanctuary State				0.00
				(0.04)
Anti-Sanctuary State				0.06
				(0.03)
Prox. Contact x Sanctuary				0.02
				(0.04)
Prox. Contact x Anti-Sanctuary				0.02
				(0.04)
R^2	0.23	0.14	0.27	0.16
Adj. R^2	0.20	0.12	0.22	0.15
Num. obs.	480	827	336	1643
RMSE	0.53	0.57	0.52	0.56

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.76: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Voting (White Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	−0.01	−0.07	0.04	−0.05
		(0.04)	(0.07)	(0.04)
Sanctuary State				0.00
				(0.04)
Anti-Sanctuary State				0.11
				(0.06)
Prox. Contact x Sanctuary				0.04
				(0.06)
Prox. Contact x Anti-Sanctuary				0.09
				(0.08)
R^2	0.32	0.17	0.31	0.18
Adj. R^2	0.21	0.10	0.15	0.14
Num. obs.	117	213	83	413
RMSE	0.88	0.91	0.82	0.88

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.77: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Voting (Latino Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.10	0.04	-0.11^{*}	0.04
	(0.05)	(0.04)	(0.06)	(0.02)
Sanctuary State				-0.04
				(0.05)
Anti-Sanctuary State				0.17^{*}
				(0.05)
Prox. Contact x Sanctuary				0.01
				(0.07)
Prox. Contact x Anti-Sanctuary				-0.19^{*}
				(0.06)
R^2	0.32	0.17	0.30	0.15
Adj. R^2	0.22	0.10	0.18	0.10
Num. obs.	116	190	107	413
RMSE	0.39	0.43	0.42	0.43

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.78: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Voting (Black Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	-0.08	0.14^{***}	0.09	-0.01
	(0.06)	(0.03)	(0.06)	(0.04)
Sanctuary State				0.05
				(0.08)
Anti-Sanctuary State				-0.02
				(0.08)
Prox. Contact x Sanctuary				-0.02
				(0.08)
Prox. Contact x Anti-Sanctuary				0.08
				(0.11)
R^2	0.30	0.22	0.48	0.19
Adj. R^2	0.13	0.17	0.33	0.15
Num. obs.	80	251	75	406
RMSE	0.46	0.34	0.40	0.45

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.79: Assessing Heterogeneity by State Sanctuary Laws for the Association Between Proximal Contact and Voting (Asian Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.03	0.00	0.01	0.01
	(0.04)	(0.05)	(0.07)	(0.05)
Sanctuary State				0.00
				(0.07)
Anti-Sanctuary State				−0.04
				(0.09)
Prox. Contact x Sanctuary				−0.01
				(0.06)
Prox. Contact x Anti-Sanctuary				0.02
				(0.10)
R^2	0.21	0.11	0.40	0.13
Adj. R^2	0.12	0.03	0.23	0.09
Num. obs.	167	173	71	411
RMSE	0.24	0.25	0.22	0.24

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

10.5.4 Association Between Interaction of Proximal Contact with Perceived Injustice and Protest

Table A.80: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Protest (Fill Sample, Standardized Coefficients, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.06^{**}	0.11^{***}	0.09^{**}	0.11^{**}
	(0.02)	(0.02)	(0.03)	(0.02)
Sanctuary State				-0.02
				(0.04)
Anti-Sanctuary State				-0.04
				(0.05)
Perceived Injustice	0.01	0.07^{***}	-0.00	0.06^{*}
	(0.02)	(0.02)	(0.03)	(0.02)
Prox. Contact x Sanctuary				-0.04
				(0.03)
Prox. Contact x Anti-Sanctuary				-0.01
				(0.05)
Prox. Contact x Injustice	0.00	0.06^{**}	0.01	0.06^{*}
	(0.02)	(0.02)	(0.03)	(0.02)
Injustice x Sanctuary				-0.04
				(0.03)
Injustice x Anti-Sanctuary				-0.05
				(0.03)
Contact x Injustice x Anti-Sanctuary				-0.06
				(0.03)
R^2	0.16	0.19	0.26	0.17
Adj. R^2	0.13	0.17	0.21	0.15
Num. obs.	480	827	336	1643
RMSE	0.36	0.38	0.33	0.37

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.81: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Protest (White Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.00	0.13	0.08	0.12^{*}
			(0.06)	(0.04)
Sanctuary State				-0.07
				(0.04)
Anti-Sanctuary State				-0.08
				(0.09)
Perceived Injustice	0.00	0.08	0.01	0.07
			(0.05)	(0.03)
Prox. Contact x Sanctuary				-0.12
				(0.06)
Prox. Contact x Anti-Sanctuary				-0.05
				(0.10)
Prox. Contact x Injustice	-0.00	0.07	0.00	0.07^{*}
			(0.06)	(0.03)
Injustice x Sanctuary				-0.05
				(0.04)
Injustice x Anti-Sanctuary				-0.08
				(0.04)
Contact x Injustice x Anti-Sanctuary				-0.09
				(0.04)
R^2	0.15	0.24	0.28	0.20
Adj. R^2	0.00	0.18	0.11	0.15
Num. obs.	117	213	83	413
RMSE	0.52	0.56	0.39	0.51

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.82: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Protest (Latino Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.20***	0.09**	0.07	0.09
	(0.04)	(0.03)	(0.05)	(0.04)
Sanctuary State				0.01
				(0.06)
Anti-Sanctuary State				0.00
				(0.08)
Perceived Injustice	0.07	0.03	−0.03	0.05
	(0.04)	(0.03)	(0.06)	(0.03)
Prox. Contact x Sanctuary				0.06
				(0.05)
Prox. Contact x Anti-Sanctuary				−0.03
				(0.07)
Prox. Contact x Injustice	−0.06	0.05	0.01	0.06
	(0.03)	(0.03)	(0.05)	(0.03)
Injustice x Sanctuary				−0.01
				(0.05)
Injustice x Anti-Sanctuary				−0.04
				(0.05)
Contact x Injustice x Anti-Sanctuary				−0.06
				(0.08)
R^2	0.43	0.24	0.27	0.21
Adj. R^2	0.33	0.17	0.13	0.16
Num. obs.	116	190	107	413
RMSE	0.33	0.32	0.37	0.35

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.83: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Protest (Black Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.13^{*}	0.13^{***}	0.03	0.14^{**}
	(0.06)	(0.04)	(0.07)	(0.02)
Sanctuary State				0.08
				(0.06)
Anti-Sanctuary State				-0.01
				(0.08)
Perceived Injustice	-0.03	0.08^{*}	0.06	0.07^{*}
	(0.06)	(0.03)	(0.06)	(0.02)
Prox. Contact x Sanctuary				0.00
				(0.04)
Prox. Contact x Anti-Sanctuary				-0.03
				(0.06)
Prox. Contact x Injustice	-0.07	0.03	0.08	0.01
	(0.06)	(0.04)	(0.06)	(0.04)
Injustice x Sanctuary				-0.09
				(0.04)
Injustice x Anti-Sanctuary				-0.04
				(0.07)
Contact x Injustice x Anti-Sanctuary				0.03
				(0.08)
R^2	0.32	0.22	0.38	0.21
Adj. R^2	0.13	0.17	0.20	0.16
Num. obs.	80	251	75	406
RMSE	0.39	0.34	0.33	0.35

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.84: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Protest (Asian Sample, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.11^{**}	0.03	0.12^{*}	0.03
	(0.03)	(0.03)	(0.05)	(0.03)
Sanctuary State				-0.01
				(0.04)
Anti-Sanctuary State				-0.01
				(0.05)
Perceived Injustice	0.04	0.00	0.04	0.00
	(0.03)	(0.03)	(0.05)	(0.02)
Prox. Contact x Sanctuary				0.08
				(0.04)
Prox. Contact x Anti-Sanctuary				0.09
				(0.06)
Prox. Contact x Injustice	0.02	0.04	0.07	0.05
	(0.03)	(0.03)	(0.06)	(0.03)
Injustice x Sanctuary				0.04
				(0.03)
Injustice x Anti-Sanctuary				0.02
				(0.04)
Contact x Injustice x Anti-Sanctuary				-0.02
				(0.04)
R^2	0.29	0.15	0.37	0.18
Adj. R^2	0.21	0.06	0.18	0.13
Num. obs.	167	173	71	411
RMSE	0.15	0.16	0.12	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

10.5.5 Association Between Interaction of Proximal Contact with Perceived Injustice and Voting

Table A.85: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Voting (Standardized Coefficients, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.00	−0.04	−0.02	−0.03
	(0.03)	(0.03)	(0.04)	(0.03)
Sanctuary State				0.01
				(0.03)
Anti-Sanctuary State				0.04
				(0.03)
Perceived Injustice	−0.01	0.01	0.02	−0.00
	(0.03)	(0.03)	(0.04)	(0.02)
Prox. Contact x Sanctuary				0.02
				(0.04)
Prox. Contact x Anti-Sanctuary				0.01
				(0.04)
Prox. Contact x Injustice	−0.03	−0.01	0.07*	−0.00
	(0.03)	(0.02)	(0.03)	(0.02)
Injustice x Sanctuary				0.00
				(0.05)
Injustice x Anti-Sanctuary				0.02
				(0.05)
Contact x Injustice x Anti-Sanctuary				0.08*
				(0.03)
R^2	0.23	0.14	0.28	0.17
Adj. R^2	0.20	0.12	0.24	0.15
Num. obs.	480	827	336	1643
RMSE	0.53	0.57	0.51	0.55

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.86: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Voting (White Sample, Standardized Coefficients, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	-0.02	-0.08	0.05	-0.06
			(0.07)	(0.06)
Sanctuary State				0.03
				(0.04)
Anti-Sanctuary State				0.11
				(0.07)
Perceived Injustice	-0.03	-0.04	0.05	-0.06
			(0.07)	(0.03)
Prox. Contact x Sanctuary				0.03
				(0.06)
Prox. Contact x Anti-Sanctuary				0.11
				(0.08)
Prox. Contact x Injustice	-0.10	-0.05	0.12	-0.05
			(0.08)	(0.06)
Injustice x Sanctuary				0.05
				(0.08)
Injustice x Anti-Sanctuary				0.10
				(0.11)
Contact x Injustice x Anti-Sanctuary				0.18
				(0.08)
R^2	0.34	0.18	0.34	0.20
Adj. R^2	0.22	0.11	0.18	0.15
Num. obs.	117	213	83	413
RMSE	0.87	0.90	0.81	0.88

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.87: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Voting (Latino Sample, Standardized Coefficients, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.10	0.04	-0.11^{*}	0.03
	(0.06)	(0.04)	(0.06)	(0.02)
Sanctuary State				-0.02
				(0.05)
Anti-Sanctuary State				0.17
				(0.06)
Perceived Injustice	-0.10	0.06	0.04	0.06
	(0.06)	(0.04)	(0.06)	(0.08)
Prox. Contact x Sanctuary				0.02
				(0.06)
Prox. Contact x Anti-Sanctuary				-0.19^{*}
				(0.05)
Prox. Contact x Injustice	-0.01	0.04	0.04	0.02
	(0.05)	(0.04)	(0.05)	(0.03)
Injustice x Sanctuary				-0.15
				(0.08)
Injustice x Anti-Sanctuary				-0.05
				(0.08)
Contact x Injustice x Anti-Sanctuary				0.04
				(0.06)
R^2	0.32	0.18	0.31	0.17
Adj. R^2	0.21	0.10	0.17	0.11
Num. obs.	116	190	107	413
RMSE	0.39	0.43	0.42	0.43

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.88: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Voting (Black Sample, Standardized Coefficients, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	−0.14	−0.03	0.05	−0.02
		(0.04)		(0.05)
Sanctuary State				−0.00
				(0.08)
Anti-Sanctuary State				−0.04
				(0.08)
Perceived Injustice	0.19	0.05	0.05	0.05
		(0.04)		(0.04)
Prox. Contact x Sanctuary				−0.08
				(0.10)
Prox. Contact x Anti-Sanctuary				0.06
				(0.13)
Prox. Contact x Injustice	0.17	0.04	0.05	0.04
		(0.04)		(0.05)
Injustice x Sanctuary				0.11
				(0.06)
Injustice x Anti-Sanctuary				0.02
				(0.06)
Contact x Injustice x Anti-Sanctuary				0.00
				(0.08)
R^2	0.37	0.17	0.48	0.21
Adj. R^2	0.20	0.11	0.33	0.16
Num. obs.	80	251	75	406
RMSE	0.44	0.45	0.40	0.44

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A.89: Assessing Heterogeneity by State Sanctuary Laws for the Association Between The Interaction of Proximal Contact with Perceived Injustice and Voting (Asian Sample, Standardized Coefficients, Standardized Coefficients, Control Covariates Not Displayed)

	Sanctuary	Neutral	Anti-Sanctuary	Full
Proximal Contact	0.02	0.01	0.01	0.02
	(0.04)	(0.05)	(0.07)	(0.05)
Sanctuary State				−0.01
				(0.07)
Anti-Sanctuary State				−0.04
				(0.09)
Perceived Injustice	0.09*	0.07	−0.08	0.08
	(0.05)	(0.06)	(0.08)	(0.05)
Prox. Contact x Sanctuary				−0.02
				(0.06)
Prox. Contact x Anti-Sanctuary				0.01
				(0.09)
Prox. Contact x Injustice	0.03	−0.06	−0.17	−0.04
	(0.05)	(0.06)	(0.09)	(0.06)
Injustice x Sanctuary				0.00
				(0.06)
Injustice x Anti-Sanctuary				−0.12
				(0.10)
Contact x Injustice x Anti-Sanctuary				−0.06
				(0.09)
R^2	0.21	0.12	0.43	0.14
Adj. R^2	0.12	0.03	0.26	0.08
Num. obs.	167	173	71	411
RMSE	0.24	0.24	0.21	0.24

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

11 Out-of-survey Replications

11.1 2015 Robert Wood Johnson Foundation Latino National Health Survey (RWJF) Replication

	Protest 1	Protest 2	Protest 3	Protest 1	Protest 2	Protest 3
(Intercept)	0.34***	0.16***	0.05*	0.42***	0.23***	0.07*
	(0.05)	(0.05)	(0.03)	(0.05)	(0.04)	(0.03)
Proximal Contact 1	0.22***	0.18***	0.06**			
	(0.04)	(0.03)	(0.02)			
Proximal Contact 2				0.18***	0.14***	0.07***
				(0.03)	(0.03)	(0.02)
R^2	0.10	0.08	0.04	0.08	0.07	0.04
Adj. R^2	0.09	0.07	0.03	0.07	0.06	0.03
Num. obs.	1402	1425	1425	1425	1425	1425
RMSE	1.27	0.46	0.26	0.49	0.46	0.26

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Models 1 characterizes the association between *proximal contact 1* and *protest 1*, with *proximal contact 1* capturing only if someone knows another person that has been detained and deported. Models 2-3 characterize the association as Model 1, but the outcome is coded differently. Question 25a of the RWJF survey asks how likely someone is to protest in the future on a 5-point likert scale (from "not at all likely" to "extremely likely"). *protest 1* is coded such that anything that is from "a little likely" to "extremely likely" is coded as 1 and everything else 0. *protest 2* is coded such that anything that is from "moderately likely" to "extremely likely" is coded as 1, and everything else 0. Finally, *protest 3* is coded such that anything that is *not* "extremely likely" is coded as 0. Models 4-6 characterize the same association, but proximal contact (*proximal contact 2*) is instead coded as whether or not someone knows another person that has been detained and deported *and/or* simply knows someone that is undocumented. The RWJF survey only includes Latinx in the sample, and all models include covariates for age, income, education, partisan identification and foreign-born status. All standard errors are HC2 heteroskedastic-robust.

Table A.90: The Association Between Proximal Contact and Protest Replicates in the RWJF Survey

To increase the external validity of our findings, we replicated our analysis in two out-of-sample surveys that ask respondents similar questions regarding proximal exposure to immigration enforcement and their individual propensity to protest. The first survey we replicate our analysis with is the Robert Wood Johnson Foundation (RWJF) Immigration Survey, which is a nationally representative survey of 1493 Latinos age 18+ deployed between January 29th to March 12th 2015. Interviews were conducted via both telephone and web modes and were administered in Spanish or English at the discretion of the respondent. The survey was implemented by Latino Decisions.

The RWJF survey includes a proximal contact item that approximates the proximal contact item on the Latino Decisions Immigration Partners/Midterm Survey. Two items are relevant. The first item asks, *"Now take a moment to think about all the people in your family, your friends, coworkers, and other people you know. This is completely anonymous and no personal information will be shared. Do you happen to know somebody who may be an undocumented immigrant?"* Respondents could respond "Yes," "No," "Don't Know", and "Refused." The second item asks, *"Do you personally know someone who has faced detention or deportation for immigration reasons?"* with the same set of responses. We generate two binary proximal contact treatment variables from these two items. The first proximal contact treatment, characterized as *proximal contact 1* on the table, is the second item where 1 is if the respondent says yes and 0 for every other response. The second proximal

Table A.91: Voting is Not Associated with Proximal Contact in the RWJF Data

	Vote Intent 1	Vote Intent 2
Proximal Contact	0.06	0.07
	(0.08)	(0.04)
R^2	0.12	0.10
Adj. R^2	0.11	0.09
Num. obs.	922	922
RMSE	0.85	0.46

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

contact treatment, characterized as *proximal contact 2* on the table, is inclusive of both the first and second items, where 1 is if the respondent says yes either to the first or second item and 0 for every other response.

The RWJF survey also includes a protest item that approximates our protest outcome. The relevant item says, *"In the future, how likely are you to do any of the following: Participate in a rally march, demonstration, or protest"*, with a 5-point likert scale response from (5) Not at all Likely to (1) Extremely likely, along with a “Don’t Know” response and a “Refused” response. We produce three distinct binary protest outcome variables from this item. *protest 1* is coded such that anything that is from “a little likely” to “extremely likely” is coded as 1 and everything else is coded as 0. *protest 2* is coded such that anything that is from “moderately likely” to “extremely likely” is coded as 1 and everything else is coded as 0. *protest 3* is coded such that anything that is **not** “extremely likely” is coded as 0. The reason we employ several measures to capture those with a strong intention to protest versus those with a weak intention to protest is because a major difference between the RWJF protest item and the Immigration Partners/Midterm Survey item is that the Immigration Partners item asks respondents if they participated in a protest, not if they *intend* to participate in a protest.

Given that the RWJF survey only samples Latinx, the replication presented here only complements the main results of the Latinx subset. We find that the association between proximal contact and self-reported protest is positive and statistically significant. For the low-threshold outcomes (protest 1-2), the point estimates are relatively similar to the point estimates in the main results. However, for the high-threshold outcomes (protest 3), which may better approximate the protest item employed in the main results, the coefficient size decreases. All of the models control for age, income, education, party ID and foreign-born status.

We also assess the association between proximal contact and voting behavior in the RWJF data conditional on all relevant control covariates (see table A.91), given that we do not find an association between proximal contact and vote intention using the Latino Decisions Midterm Survey. Again, we find no statistically significant association between proximal contact and two measures of voter intention.[13]

[13]Vote Intent 1 on table A.91 is measured by using the entire 4-point likert scale in response to an item that asks respondents how likely they are to vote in the next local, state, or national election. Vote Intent 2 on table A.91 is measured by a binary measure of the same 4-point likert scale, with 1 being equal to those who indicated they were “Extremely Likely” to vote in the next election. See section 11.1.1 for more details on question wording for vote intention in the RWJF survey. Respondents who indicated they do not know whether they would participate or those who refused to answer the question were omitted from the analysis.

11.1.1 RWJF Survey Items

Participation Items (English Version)
25. In the future, how likely are you to do any of the following: Extremely Likely (1), Very Likely (2), Moderately Likely (3), A little Likely (4), Not at all Likely (5)? [Don't read: Don't know (88), Refused (99)]
25a. Participate in a rally march, demonstration or protest

- 1) Extremely Likely
- 2) Very likely
- 3) Moderately likely
- 4) A little likely
- 5) Not at all likely

- 88) Don't Know
- 99) Refused

25e. Vote in the next local, state, or national election

- 1) Extremely Likely
- 2) Very likely
- 3) Moderately likely
- 4) A little likely
- 5) Not at all likely

- 88) Don't Know
- 99) Refused

Proximal Contact Items (English Version)
45. Do you personally know someone who has faced detention or deportation for immigration reasons [Should be asked of full sample]?

- 1) Yes, Know of Someone
- 2) No, do not know anyone
- 88) Don't Know
- 99) Refused

43. Now take a moment to think about all the people in your family, your friends, coworkers, and other people you know. This is completely anonymous and no personal information will be shared. Do you happen to know somebody who may be an undocumented immigrant?

- 1) Yes
- 2) No
- 88) Don't Know
- 99) Refused

11.2 2016 Collaborative Multi-Racial Post-Election Survey (CMPS) Replication

	Model 1	Model 2	Model 3	Model 4	Model 5
(Intercept)	0.05	0.02	0.02	−0.02	−0.01
	(0.06)	(0.04)	(0.04)	(0.05)	(0.05)
Prox. Contact	0.04				
	(0.03)				
Know Undoc. 1		0.08^{***}			
		(0.01)			
Know Undoc. 2			0.10^{***}		
			(0.02)		
Prox. Contact + Know Undoc. 1				0.07^{**}	
				(0.02)	
Prox. Contact + Know Undoc. 2					0.08^{***}
					(0.02)
R^2	0.03	0.05	0.06	0.04	0.05
Adj. R^2	0.02	0.04	0.05	0.03	0.04
Num. obs.	2065	2997	2458	2047	1524
RMSE	0.32	0.29	0.30	0.34	0.36

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Model 1 characterizes the association between knowing someone who was detained or deported due to their immigraiton status and self-reported protest activity. In the CMPS, this question is only asked of foreign-born respondents. Therefore, the sample in Model 1 is isolated to solely foreign-born respondents. Models 2 and 3 characterize the association between knowing someone who is undocumented and protest activity. This sample is only asked of Latinx respondents in the sample. There are two different measurements of the treatment. *Know Undoc. 1* codes both "Yes" and "Don't Know" responses as 1 and "No" as 0 due to the possibility of underreporting bias. *Know Undoc. 2* omits all individuals in the sample who report "Don't Know." Models 4 and 5 characterize the association between knowing someone who was detained/deported *and/or* knowing someone who was undocumented. This means the sample includes both US and foreign-born Latinx. Model 1 includes covariates for age, race, income, education, and partisan identification. Models 2-5 include the same covariates, except for race, with the inclusion of a dummy variable for foreign-born status. All standard errors are HC2 heteroskedastic-robust.

Table A.92: The Association Between Proximal Contact and Protest Replicates in CMPS Survey Sample

Another survey that has similar measures as the Immigration Partners/Midterm Survey is the Collaborative Multi-racial Post-Election Survey (CMPS). The CMPS includes a total of 10,145 completed interviews colleected online in a self-administerd format from December 3, 2016 to February 15, 2017. The survey was available to respondents in English, Spanish, Chinese, Korean and Vietnamese. The survey was implemented by Latino Decisions.

The CMPS includes two relevant items that approximate the proximal contact treatment in the main analysis. The first item asks, *"How many people do you know who have been detained or deported for immigration reasons"* and the respondent can provide a count of how many individuals they know that have been detained or deported. The second item asks, *"Now take a moment to think about all the people in your family, your friends, co-workers, and other people you know. Do you happen to know somebody who is an undocumented immigrant? This is completely anonymous, and just for a simple demographic analysis."* The respondent can answer "Yes", "No", or "Don't Know." There are some sampling limitations, however, when employing these two items from the CMPS. Namely, the first item is only asked of Latinx respondents, and the second item is only asked of foreign-born respondents. Once again, this means that the results presented here with respect to the CMPS can only be comparable to the Latinx subset in the main results from the Immigration Partners/Midterm survey. We construct a series of proximal contact treatment variables from these two items. The first, *prox. contact*, is a binary treatment variable based on the first item coded as 1 if the respondent knows at least 1 person that was detained or deported by immigration authorities. The second, *know undoc. 1*, is a binary treatment variable based on the second item coded as 1 for "yes"" or "don't know" and 0 for every other response. "Don't Know" is coded as 1 due to the possibility of underreporting bias. *know undoc. 2*, is coded similarily, however, respondents who report "don't know" are coded as missing, and are thus effectively dropped from the sample. *prox. contact + know undoc. 1* and *prox. contact + know undoc. 2* are binary treatment variables inclusive of knowing someone exposed to immigration enforcement **and/or** knowing someone who is undocumented. We generate these measures of proximal contact to more closely approximate the treatment employed in the main analysis, which measures knowing someone who possesses DACA status *and/or* knowing someone who has been exposed to immigration enforcement,

The CMPS also includes a protest item that is identical to the Immigration Partners/Midterm survey, which asks, "In the last twelve months, have you attended a protest march, demonstration, or rally?" The respondent can reply "Yes", "No", or "Don't Know." Like the item in the Immigration Partners/Midterm survey, the outcome is coded 1 for "Yes" and 0 for all other responses.

The results in the CMPS replication are consistent with the main results of the paper. The association between the pure proximal contact variable (e.g. knowing someone who was deported or detained) and the protest outcome in Model 1 is statistically insignificant ($p < .12$), with a point estimate of 4 percentage points. However, this may be a function of the sample, given that the proximal contact question was only asked of foreign-born respondents in the CMPS. If anything, this is consistent with an analysis in the Immigration Partners/Midterm Survey that subsets the data to foreign-born respondents, which finds a statistically insignificant association between proximal contact and self-reported protest with a point estimate of 11 percentage points ($p < .13$). However, knowing someone undocumented in the Latinx sample has a statistically significant positive relationship with protest activity with a point estimate of 8-10% points depending on how the treatment is measured. In addition, the two treatment measures that account for proximal exposure to immigration enforcement and whether or know a respondent knows someone who is undocumented are positively associated with protest activity and statistically significant. One may pose the objection that the point estimates are relatively distinct from the point estimates in the main analysis. However, given the possibility of sampling variability within a repeated-sample framework, this does not necessarily call into question the validity of the main results.

Table A.93 displays the results characterizing the association between proximal contact and whether a respondent voted in the 2016 election conditional on relevant covariates. Across different measures of proximal contact, there does not appear to be a statistically significant association between proximal contact and the propensity to vote, consistent with the main results using the Latino Decisions 2018 Midterm Survey.[14]

[14]See section 11.2.1 for details on the vote item. The item was only asked of registered voters, which explains the smaller sample size relative to the protest results in the CMPS.

Table A.93: There is No Association Between Proximal Contact and Voting in the 2016 CMPS

	Vote	Vote	Vote	Vote	Vote
Prox. Contact	0.01				
	(0.03)				
Know Undoc. 1		−0.01			
		(0.02)			
Know Undoc. 2			0.00		
			(0.02)		
Prox. Contact + Know Undoc. 1				−0.04	
				(0.04)	
Prox. Contact + Know Undoc. 2					−0.03
					(0.04)
R^2	0.11	0.12	0.11	0.08	0.08
Adj. R^2	0.09	0.11	0.10	0.07	0.06
Num. obs.	932	1815	1541	1105	832
RMSE	0.31	0.23	0.22	0.26	0.25

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

11.2.1 CMPS Survey Items

Protest Item
In the last twelve months, have you attended a protest march, demonstration, or rally?

- 1) Yes
- 2) No

Vote Item
This year a lot of people said they did NOT vote in the election, because they were just too busy, not that interested in politics, or frankly don't like their choices. How about you? Would the official vote records for {INSERT STATE} indicate that you voted in 2016 election, or like many people, did you skip this one?

- 1) Yes, I voted
- 2) No, I did NOT vote

Proximal Contact Items
[L364] Now take a moment to think about all the people in your family, your friends, co-workers, and other people you know. Do you happen to know somebody who is an undocumented immigrant? This is completely anonymous, and just for a simple demographic analysis.

- 1) Yes
- 2) No
- 3) Don't know

[C394] How many people do you know who have been detained or deported for immigration reasons?

11.3 2010 Pew Hispanic Survey Replication

	Pro-Immigrant Protest
(Intercept)	0.08^{*}
	(0.04)
Proximal Contact	0.14^{***}
	(0.03)
R^2	0.06
Adj. R^2	0.05
Num. obs.	1375
RMSE	0.35

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

The model presented above characterizes the association between knowing someone who has been detained or deported by immigration enforcement in the last 12 months and whether or not a respondent reported participating in a protest or demonstration to *support immigrant* rights over the past year. The model includes control covariates for partisan identification, education, age, income, gender, and foreign-born status. Standard errors are heteroskedastic robust.

Table A.94: The Association Between Proximal Contact and Protest Replicates in Pew Hispanic Survey Sample

Another survey with similar items as the Immigration Partners/Midterm Survey is the 2010 Pew Hispanic Survey, a bilingual, dual-frame (cell phone and landline), nationally representative telephone survey of Latino adults residing in the U.S., which was conducted between August 17, 2010 and September 19, 2010. Since this survey is isolated to a Latinx sample, it can only serve as a replication for the Latinx subset results. However, one potential benefit of the Pew Survey results is that the protest item captures political mobilization with the explicit purpose to "support immigrations rights," reducing the possibility of the type of measurement error we may observe in the main results. The treatment variable is slightly different than the treatment employed in the main results. It is not inclusive of an item that characterizes whether or not someone knows someone on DACA or knows someone in their family, friend group, or acquantices that is undocumented. However, the treatment is based on a questions that asks "Do you personally know someone who has been deported or detained by the federal government for immigration reasons in the last 12 months?" with four potential resposes: "Yes," "No," "Don't Know," or "Refused." We generate a binary treatment variable where 1 is "Yes" and 0 is all other options. Including "Don't Know" and "Refused" into the 0 category should not be problematic given that "Don't Know"" and "Refused" jointly constitute 8 observations.

The results hold in the Pew Hispanic Survey, and are still similar to the point estimates in the main results. Proximal exposure to immigration enforcement increases the probability of self-reported participation in pro-immigration protests by 14 percentage points. Given that positive and statistically significant associations between proximal exposure to immigration enforcement and protest activity are consistent across four separate independent samples, this suggests that the sampling distribution centered along the true partial derivative is quite unlikely to include zero.

Like the 2018 Latino Decisions Midterm Survey, we also assess the association between proximal contact and voting behavior in the 2010 Pew Survey. Here, we find no association between proximal contact and vote intention in the 2010 Congressional election, consistent with the main

Table A.95: There is No Association Between Proximal Contact and Voting in the Pew Survey

	Vote
Proximal Contact	-0.04
	(0.03)
R^2	0.31
Adj. R^2	0.30
Num. obs.	1375
RMSE	0.42

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

results.[15]

[15]The vote intention measure is based on two different items in the Pew Hispanic Survey. The first item asks whether respondents are will vote in the upcoming 2010 Congressional election. Then, a follow-up quesiton asks respondents how certain they are that they will vote if they indicated they will vote, with a 3-point likert from "not certain" to "absolutely certain." Those coded as intending to vote indicate both that they plan to vote and that they are "absolutely certain" they will vote. See section 11.3.1 for more details on item wording.

11.3.1 2010 Pew Hispanic Survey Items

Protest Item

55. In the United States in the past year have you participated in any protests or demonstrations to support immigrations rights, or not?

- 1) Yes
- 2) No
- 8) Don't Know
- 9) Refused

Vote Items
26. Thinking ahead to November, do you yourself plan to vote in the Congressional election this November, or not?

- 1) Yes, plan to
- 2) No, don't plan to
- 3) Don't know
- 9) Refused

27. How certain are you that you will vote? Are you absolutely certain, fairly certain, or not certain?

- 1) Absolutely certain
- 2) Fairly certain
- 3) Not certain
- 8) Don't know
- 9) Refused

Proximal Contact Item

33. Do you personally know someone who has been deported or detained by the federal government for immigration reasons in the last 12 months?

- 1) Yes
- 2) No
- 8) Don't know
- 9) Refused

12 Sensitivity Analysis

Table A.96: Robustness Value Table

Treatment	Est.	SE	t-value	$R^2_{Y\sim D\|X}$	RV	$RV_{\alpha=0.05}$
Proximal Contact	0.193	0.019	10.385	0.062	0.227	0.188

To assess the strength of the association between proximal contact and the propensity to protest given the possibility of unobserved confounding, we employ a set of tools by Hazlett and Cinelli (2018) that reparameterizes the omitted variable bias (OVB) framework in terms of partial R^2 values rather than raw regression coefficients. Reparameterization of OVB in this manner has several benefits: 1) It is scale free, meaning it determines how confounding affects the strength of the treatment effect without regard to the measurement of the confounder (e.g. binary versus continuous), 2) it allows an assessment of any number of confounders acting together, perhaps even non-linearly, 3) it is not dependent on the sample size, unlike other tools for assessing the strength of treatment effects to confounding (Hosman, Hansen and Holland, 2010), and 4) it helps to assess sensitivity to extreme scenarios by which $R^2_{Y\sim Z|X,D}$ is 100%, meaning, the confounder explains *all* of the variance in Y. This allows the researcher to isolate an assessment of the degree of omitted covariate *imbalance* between treated and control groups that would nullify the treatment effect given a confounder that explains all of the variation in the outcome.

Table A.97: Covariate Bounds Table

Bound Label	$R^2_{D\sim Z\|X}$	$R^2_{Y\sim Z\|D,X}$
Latino	0.0460	0.0019
Foreign Born	0.0000	0.0032
Democrat	0.0001	0.0031
High Income (80-99k)	0.0003	0.0037

The robustness value table indicates how much joint variation in both the treatment and outcome need to be explained by an unobserved confounder in order to change the sign of the proximal contact coefficient (net of controlling for the unfairness scale, prior personal experience with immigration enforcement, age, education, partisan identification, gender, foreign-born status, and race/ethnicity). In this case, the robustness value that characterizes how much joint variation in the treatment and outcome must be explained by a confounder in order to reduce the coefficient to zero and/or flip the sign of the coefficient is 22%. The robustness value that characterizes how much variation joint variation in the treatment and outcome must be explained by a confounder in order to make the coefficient statistically insignificant given $\alpha = 0.05$ is 19%. The table also characterizes how much variance is explained in the outcome by the treatment conditional on the other covariates (6.2%). Thus, if a confounder explained 100% of the variation in Y (an extreme-case scenario), the treatment effect estimate will be reduced to zero if it also explains 6.2% of the variation in the treatment, and so reference points using theoretically motivated observed confounders could provide some insight into how insulated the treatment is from confounding. Any confounder would not only need to explain 19% of the joint variation in the treatment and outcome, but would need to do so *net* of the joint distribution of covariates controlled for in the main models.

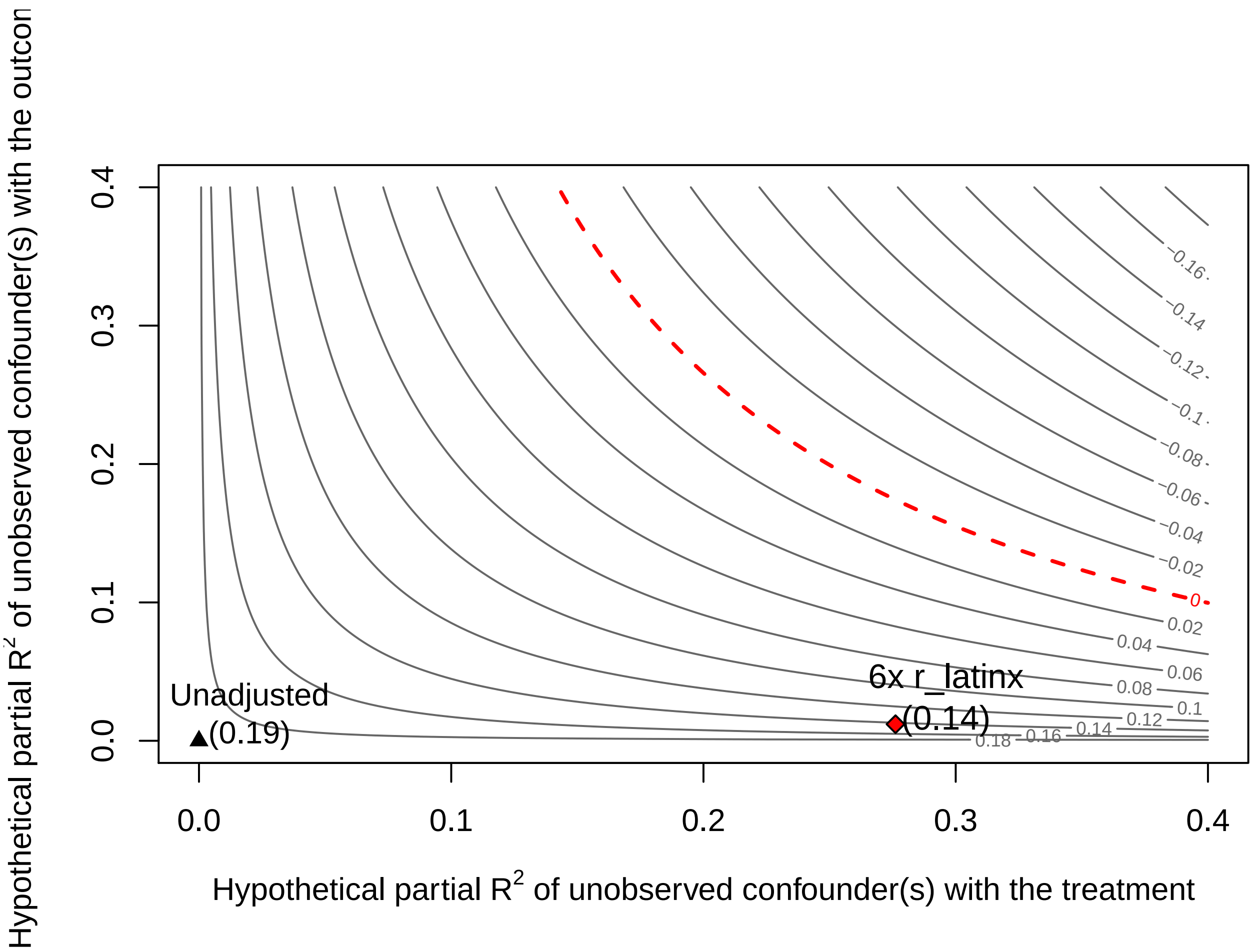

Figure A.23: Contour plot illustrating zero effect given various degrees of confounding

The first plot illustrates the size of the coefficient given various degrees of confounding with respect to both the treatment and outcome. The X-axis characterizes the hypothetical partial R^2 for the imbalance of a confounder between treated and control units whereas the Y axis characterizes the hypothetical partial R^2 for the impact of a potential confounder on the outcome. The contour lines characterize adjusted treatment effect sizes conditional on the degree of confounder imbalance *and* the effect of the confounder.

The plot also illustrates a bounds analysis that could provide a frame of reference for the reader in terms of judging the robustness of the treatment effect to potential confounders. Theoretically, what kind of confounder would likely explain joint variation in the propensity to protest and the likelihood of knowing someone who is undocumented and/or has been detained or deported by immigration enforcement? One such confounder, might be whether or not a respondent is Latino. If a respondent is Latino, they may be more likely to know someone who is undocumented or who has had contact with immigration enforcement given that Latinos constitute a large proportion of undocumented immigrants and may be more likely to engage in non-traditional political participation given their immigration status and the fact that they may feel compelled to respond to policy threat. Given that we may expect Latinx to be a large confounding variable, we may want to use it as a "bound" to understand the strength of the treatment effect of proximal contact. The plot provides strong evidence that the effect size is robust to potential theoretically large confounders. The black triangle on the plot characterizes the unadjusted treatment effect estimate. The red diamond characterizes the adjusted treatment effect estimate if there was a confounder that was *six times the size of whether or not the respondent is Latinx.* In order for Latino to nullify the treatment effect, it must explain *15 times* the level of variation explained in the treatment and outcome than it actually explains. Given our theoretical prior that whether or not a respondent is Latino theoretically matters to a significant extent with respect to the treatment and outcome, this would suggest that confounders big enough to nullify the size of the treatment effect may be unlikely.

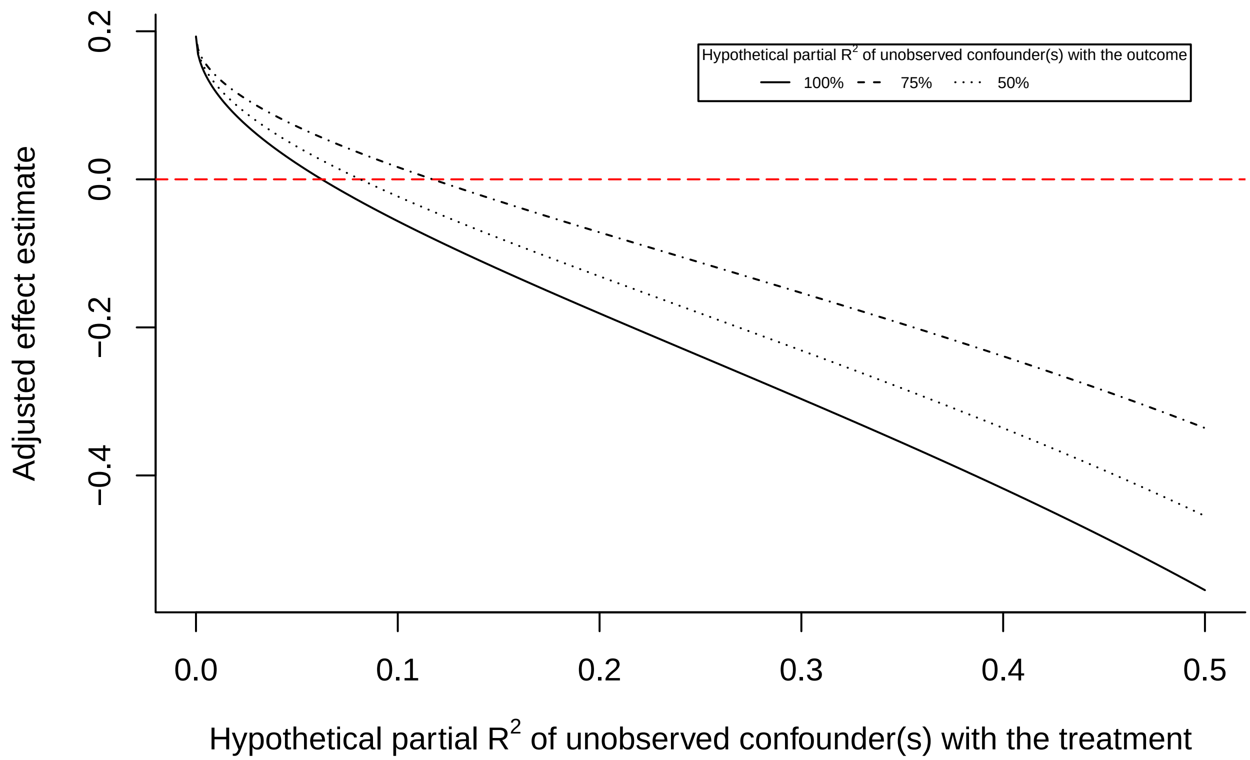

Figure A.24: Contour plot illustrating zero effect given a confounder that explains all variance of Y

The second plot shows the adjusted coefficient for proximal contact given an extreme case scenario where the confounder explains 100% of the variation in the outcome. In this case, whether or not the effect size becomes zero is conditional on how much imbalance the confounder drives in the treatment.

13 Heterogeneity of Proximal Contact by Perceived Injustice — Vote Outcome Plot

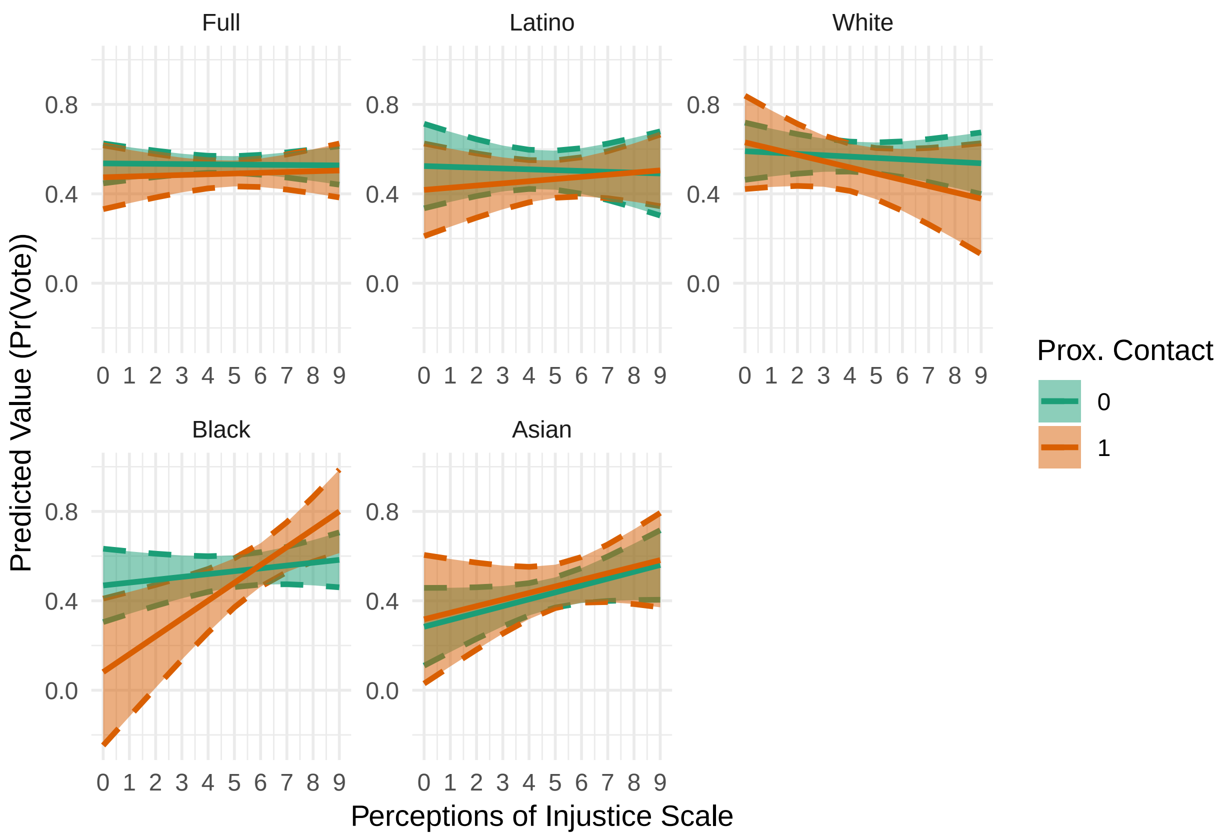

Figure A.25: Pr(Vote) by PoI and Race

There is no association between proximal contact and vote intention moderated by a sense of injustice with the exception of black respondents (Figure A.25), disconfirming hypotheses 1, 2, 3, and 5 but providing some support for hypothesis 4. Black respondents that have experienced exposure to immigration enforcement and possess a stronger sense of injustice are more likely to vote, which could be a function of racial group empathy (Hurwitz, Peffley and Mondak, 2015). However, it is important to note that black respondents without proximal contact are just as likely to vote across all levels of the injustice scale, and their propensity to protest is statistically indistinguishable from black respondents that have had proximal contact and are at the highest level of the injustice scale.[16]

[16]The results are similar if the 0-10 vote intention scale is used.

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