APPENDIX A. ANALYSES ON SELECTION INTO OUR SAMPLE

Using the 2016 Law Enforcement Agency Roster ("LEAR"; Bureau of Justice Statistics 2017) data, we assess the representativeness of the 36 police departments (PDs) in our final sample in term of the population size they serve and the number of full-time sworn officers. A few notes about our final sample are:

1. The SOP dataset originally includes information on 55 municipal PDs. However, only 36 out of the 55 PDs have the traffic stop data from 2015, along with the necessary information for our analysis, such as the date and specific location of each stop. Six of those 36 PDs adopted the PDI in May 2015.
2. The PDI was adopted by 19 local PDs in May 2015 nationwide, along with 2 county PDs. The SOP dataset includes information on 7 of these 19 local PDs.[1]
3. The 2016 LEAR dataset has a sample of 10,554 local city or county PDs. Among these, 135 PDs serve cities where population is greater than 0.2 million, and 3,336 PDs serve cities with more than 10,000 people.

Table A1 shows regressions that examine a given department's log-transformed population and log-transformed number of full-time sworn officers (based on 2013 Law Enforcement Management and Administrative Statistics (LEMAS); Bureau of Justice Statistics 2015)[2] to predict its inclusion in the SOP dataset and the PDI adoption, respectively. We do not find evidence that PDs that were included in the SOP dataset are different from those that were not included in statistically or substantively significant ways, after controlling for state fixed effects (Models 1-3). When state fixed effects are not considered (Models 4-6), PDs associated with larger cities are more likely to be included in the SOP dataset.

Table A1. Logit Estimation of SOP and PDI Sample

Outcome	In the SOP?						PDI Adoption			
	All	>10k	>200k	All	>10k	>200k	All		SOP	
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
log(Population)	1.9	1.8	0.60	2.0***	2.0**	1.1	-0.16	-1.9	0.28	0.23
	(1.3)	(1.3)	(1.9)	(0.60)	(0.60)	(0.68)	(0.75)	(1.5)	(0.44)	(0.48)
log(FT Sworn Officer)	0.04	0.06	0.008	-0.36	-0.35	-0.58	1.4*	2.1		
	(1.1)	(1.1)	(1.5)	(0.52)	(0.52)	(0.52)	(0.69)	(1.3)		
Constant				-25.2***	-25.1***	-11.5+	-10.5*	8.8	-5.1	-4.6
				(4.6)	(4.6)	(5.9)	(5.3)	(11.7)	(5.8)	(6.2)
State FE	Y	Y	Y	N	N	N	N	N	N	N
Standard-Errors		State								
Observations	946	639	65	1,864	1,216	128	1,864	32	38	36
Squared Correlation	0.37	0.35	0.09	0.15	0.15	0.02	0.10	0.11	0.009	0.007
Pseudo R^2	0.51	0.46	0.07	0.42	0.37	0.02	0.37	0.12	0.01	0.008
BIC	274.5	266.6	144.1	209.6	208.2	150.6	138.1	37.6	43.2	39.4

[1] Our primary analyses are based on 36 police departments, with six participating in the PDI and the remaining 30 not participating. The six "treated" departments have traffic stop data for the entire year 2015 from the SOP dataset. However, the dataset for Oakland PD (California) only partially covers the year 2015 (i.e., stop data for months from October to December are missing) and is therefore excluded from the analysis.

[2] 2013 LEMAS sample was collected on 2,327 local police departments, 895 sheriffs' offices, and the 50 state agencies. All agencies employing > 100 sworn officers are to be included, and smaller ones are sampled from size-based strata.

Models 7 through 10 compare those that adopted the PDI versus those that did not, using a sample of PDs in both the 2016 LEAR and 2013 LEMAS datasets. In this sample, PDI adoption seems more likely when the number of sworn officers increases in the department (Model 7). Models 8 through 10 restrict the sample to the PDs covered by SOP. Due to some missing information on officer sizes in the 2013 LEMAS dataset, Model 8 is based on 32 PDs. In models where we do not control officer sizes, Model 9 includes the entire sample of 38 PDs including Oakland (CA) and Owensboro (KY), while Model 10 includes the final sample of 36 PDs in our final analysis of traffic stops excluding the two departments without the complete data for the year 2015 (see Table A2). In these models, neither population nor officer size shows a significant association with PDI adoption.

Among the 36 PDs we analyze in our main analyses, 6 PDs have adopted the PDI, while the remaining 30 have not. The t-test of the average population size between the two groups yielded little evidence of statistically or substantively meaningful difference (t = 0.24, p = 0.81). Neither did the t-test of the average full-time sworn officers reveal evidence of difference (t = 0.57, p = 0.59). Figure A1 visualizes the distribution of PDs in terms of population, officer size, and whether they are within the SOP dataset and adopted the PDI.

Figure A1. PDI and SOP (PDs Serving Population > 200k)

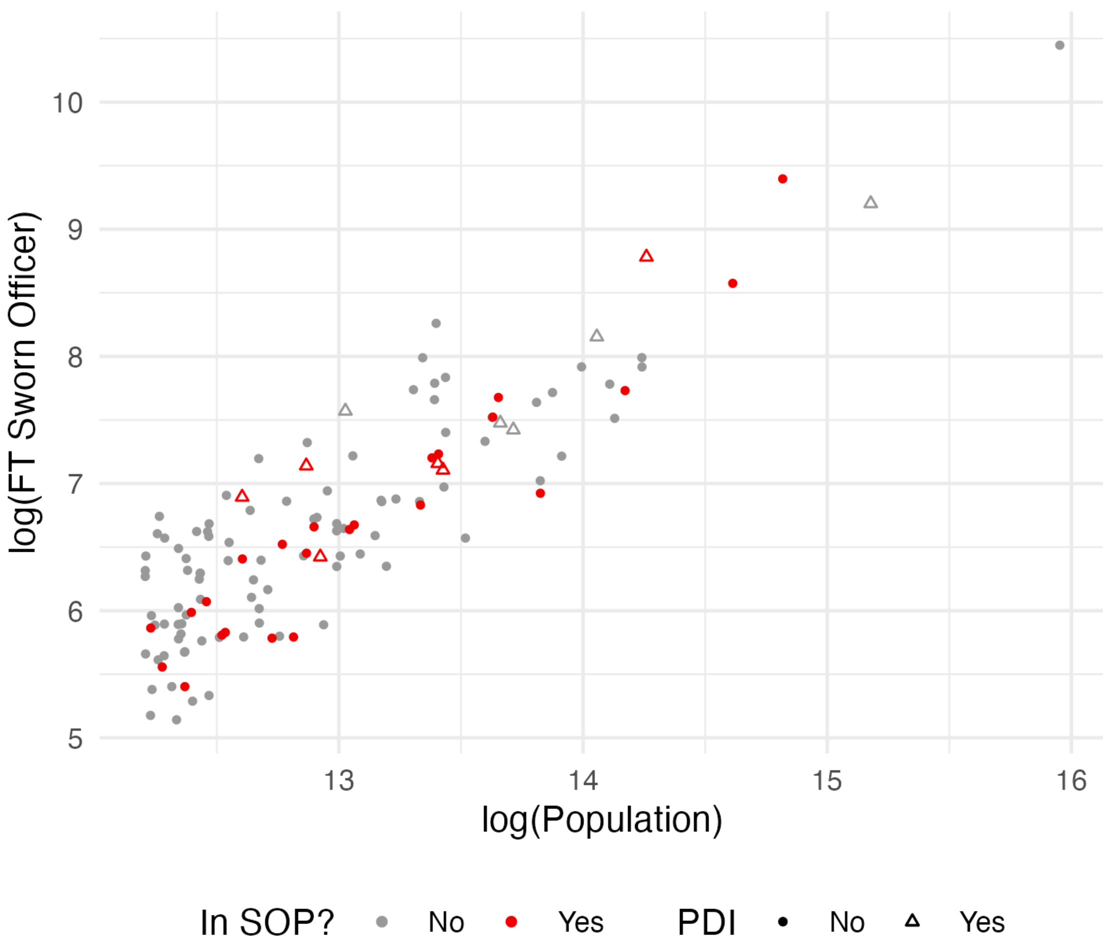


Table A2. Sampled Police Departments in the Final Sample

State	City	Police Department	Population	FT Sworn	PDI	SOP	Analysis
AZ	Mesa	Mesa PD	462,092	764		Y	Control
AZ	Gilbert	Gilbert PD	235,430	222		Y	Control
CA	Oakland	Oakland PD	409,994	616	Y	Y	[1]
CA	Richmond	Richmond PD	108,464	185	Y		
CA	Bakersfield	Bakersfield PD	367,406	328		Y	Control
CA	Los Angeles	Los Angeles PD	3,906,772	9,920	Y		
CA	Long Beach	Long Beach PD	471,123	792		Y	Control
CA	Santa Ana	Santa Ana PD	336,462	325		Y	Control
CA	San Bernardino	San Bernardino PD	214,588	259		Y	Control
CA	San Francisco	San Francisco PD	850,294	2,158		Y	Control
CA	San Jose	San Jose PD	1,009,679	1,016		Y	Control
CO	Aurora	Aurora PD	350,948	680		Y	Control
CO	Denver	Denver PD	665,353	1,383		Y	Control
CT	Hartford	Hartford PD	124,943	468		Y	Control
GA	Atlanta	Atlanta PD	454,363	1,940	Y		
ID	Idaho Falls	Idaho Falls PD	58,606			Y	Control
IL	Chicago	Chicago PD	2,724,121	12,042		Y	Control
IN	Fort Wayne	Fort Wayne PD	257,172	433		Y	Control
IN	Indianapolis	Indianapolis Police	858,238		Y		
KS	Wichita	Wichita PD	387,493	634		Y	Control
KY	Owensboro	Owensboro PD	58,659			Y	[1]
KY	Louisville	Louisville Metro PD	677,710	1,220	Y	Y	Treatment
LA	New Orleans	New Orleans PD	387,113	1,261	Y	Y	Treatment
MN	Saint Paul	Saint Paul PD	297,984	606		Y	Control
NC	Charlotte	Charlotte-Mecklenburg PD	856,916	1,766	Y		
ND	Grand Forks	Grand Forks PD	55,438			Y	Control
NJ	Camden	Camden County PD	76,892		Y	Y	Treatment
NV	Henderson	Henderson PD	274,121	333		Y	Control
NY	Albany	Albany City PD	98,595	327		Y	Control
OH	Columbus	Columbus PD	830,811	1,849		Y	Control
OH	Cincinnati	Cincinnati PD	297,671	986	Y	Y	Treatment
OK	Oklahoma City	Oklahoma City PD	617,975	926		Y	Control
OK	Tulsa	Tulsa PD	399,556	780		Y	Control
PA	Philadelphia	Philadelphia City PD	1,559,062	6,515	Y	Y	Treatment
SC	Columbia	Columbia PD	134,124	376	Y		
TN	Nashville	Metro Nashville PD	647,689	1,342		Y	Control
TN	Knoxville	Knoxville PD	184,362	405	Y		
TX	San Antonio	San Antonio PD	1,428,465	2,278		Y	Control
TX	Plano	Plano PD	277,822	340		Y	Control
TX	Dallas	Dallas PD	1,272,396	3,478	Y		

TX	Houston	Houston PD	2,219,933	5,295		Y	Control
TX	Lubbock	Lubbock PD	241,826	398		Y	Control
TX	Austin	Austin PD	903,924	1,673	Y		
VA	Hampton	Hampton PD	136,590	281	Y		
VA	Newport News	Newport News PD	182,374	404	Y		
VT	Burlington	Burlington PD	42,252			Y	Control
VT	Rutland	Rutland City PD	16,044		Y		
WA	Seattle	Seattle PD	663,410	1,285	Y	Y	Treatment
WA	Tacoma	Tacoma PD	204,722	352		Y	Control
WI	Madison	Madison PD	245,788			Y	Control
				N =	19	38	6 Tr. and 30 Ctrl.

[1] While the Oakland Police Department in California and the Owensboro Police Department in Kentucky are included in the SOP data, they do not have complete observations for the entire year of 2015, so they are excluded from our empirical analyses.

APPENDIX B. POST-HOC ANALYSES AND ROBUSTNESS CHECKS

This appendix provides a series of robustness checks to supplement the results reported in the main draft.

Verifying Parallel Pre-Trend Assumption

In order to address the concern on the parallel pre-trend assumption, we conduct a sensitivity test as suggested by Rambachan and Roth (2023) to assess how sensitive our findings are to a possible violation of the parallel pre-trend assumption. Figure B1 illustrates results from this analysis, which indicate that our findings remain robust even in the presence of such violations to the extent that relative magnitude bounds (i.e., $\bar{M}$) are smaller than 0.6.

Excluding Potential Outlier Departments

We here conduct the same analyses as main ones reported in the main text, but excluding one of the six treated police departments at a time. By doing so, we assess how their respective exclusion might influence the overall trend—whether the initial decline in the overall number of stops per capita and the relative number of stops per capita in neighborhoods with more Black residents disappears when the longer time period is considered.

While most of our findings are replicated in this set of analyses, one specification that excludes the Philadelphia Police Department (PPD) warrants further attention. In Table B1 Panel E, i.e., without the PPD in the sample, the shorter-term impacts of the PDI on the number of stops per capita are significantly negative in Models 1 through 5, which resemble those from the main analyses using the whole sample. However, there is no statistically or substantively significant evidence that the number of stops per capita increases over the long term, as reported in the main analysis. Nonetheless, the broader pattern of recovery is replicated in other specifications that exclude one of the remaining five treated police departments. Figure B4 Panel E visually presents this overall pattern.

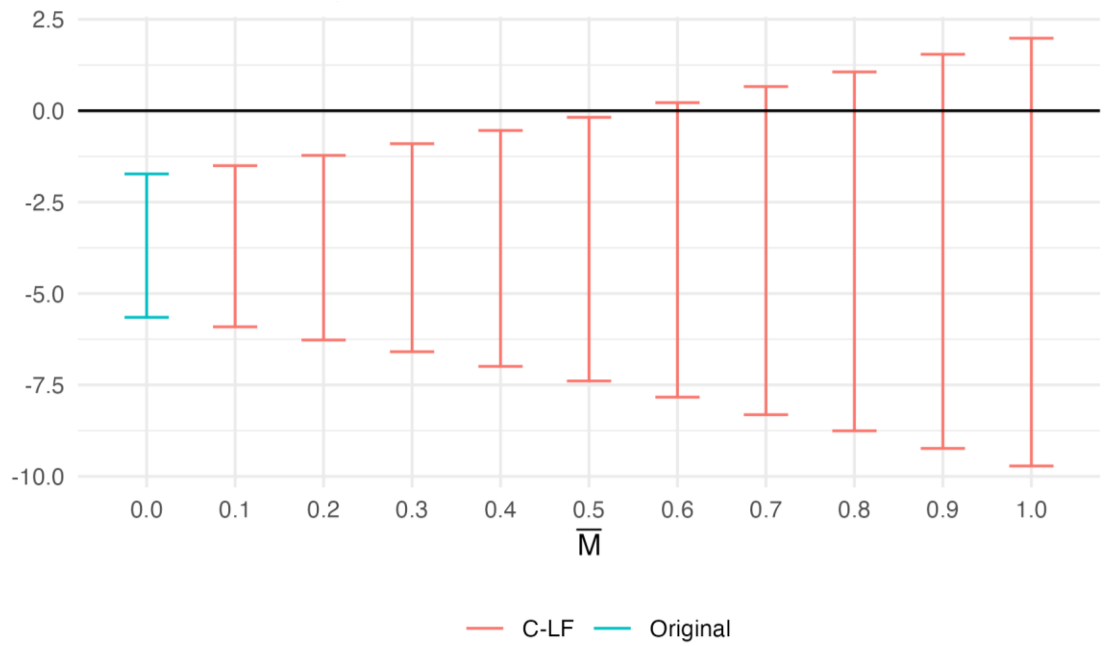
A similar takeaway may be drawn for results on the relative number of stops in neighborhoods with more Black residents—i.e., that the initial decline in the number of stops disappears over time while the number does not increase due to the PDI. In Table B2 Panel E, again without the PPD in the sample, the triple interaction between the DID estimator and the standardized Black ratio is consistently negative across the board, though only at marginal levels of significance ($p < 0.1$) at the longest time window.

These findings imply that the PPD may have played a disproportionate role in driving the longer-term effects in the main analyses. This seems to stem in part from the fact that PPD is one of the bigger departments (i.e., departments patrolling more tracts) with more heterogeneous tracts (i.e., tracts with especially few or more Black residents than those from other departments). For the former, Figure B5 illustrates the number of unique tracts associated with each jurisdiction for the police department. The PPD covers well over 300 tracts, significantly more than other treated police departments of smaller size, such as Camden, NJ, and Louisville, KY. For the latter, Figure B2 displays the distribution of tracts with more or less Black residents within each police department. It is worth noting that we do not use the raw Black ratios (as shown in Panels A and C in Figure B2), but instead, we demean and standardize the raw Black ratios within each police department to consider more "Black" tracts in each city's context (as depicted in Panels B and D in Figure B2). Figure B2 illustrates that the PPD covers more tracts with more Black residents in the sample, which resulted in a relatively greater impact on the overall outcomes in our main analyses.

This post-hoc analysis should be interpreted with respect to a perhaps obvious trade-off. On the one hand, our main analyses compare all treated departments with all control departments to avoid idiosyncrasies from a single department skewing the results. When seasonality and other department-specific characteristics are likely idiosyncratic to each department (as shown in Figure B6), this analytical decision seems reasonable, as in most other analyses using difference-in-differences frameworks that aggregate treatment and control *group*. But on the other hand, an inevitable downside is that we lose some of heterogeneity associated with the PDI's effects on each department, although we do capture some such heterogeneity by examining departments with more or fewer stops per capita (i.e., analyses reported in Table 3). Also, another possible drawback from such aggregated analyses is the influence of outliers. For us, while the PPD appears to influence the overall pattern more than other treated departments, the overall pattern seems to remain largely consistent even excluding the PPD.

The key takeaway from these analyses is that the erosion of initial compliance does not appear to be driven by a single outlier department. Across all specifications, this pattern is consistent. The longer-run results, however, warrant a more careful distinction. In the full sample, the treatment coefficients eventually turn positive and reach statistical significance, suggesting that the initial compliance effect fades and then reverses. When PPD is excluded, while the trajectory still tends in the direction of reversal, this pattern does not emerge as clearly. The leave-one-out analyses, therefore, suggest that while the erosion of initial compliance is robust, the evidence for a longer-run reversal should be interpreted with that caveat in mind.

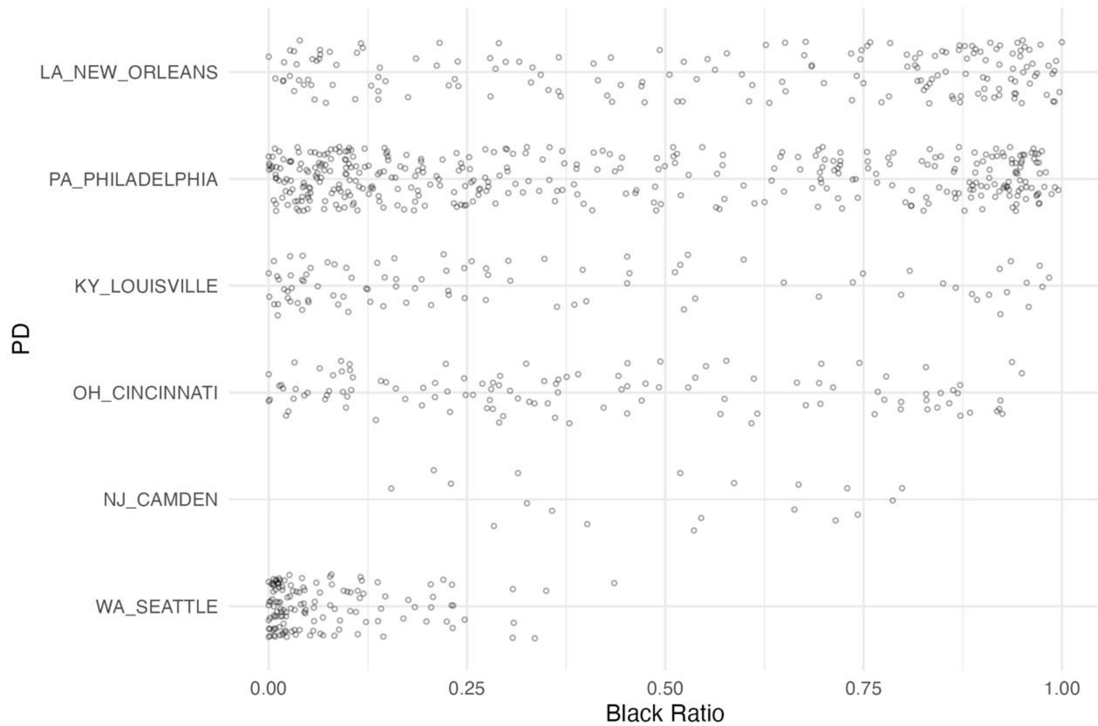
Figure B1. Sensitivity Test for Post-treatment Parallel Trends



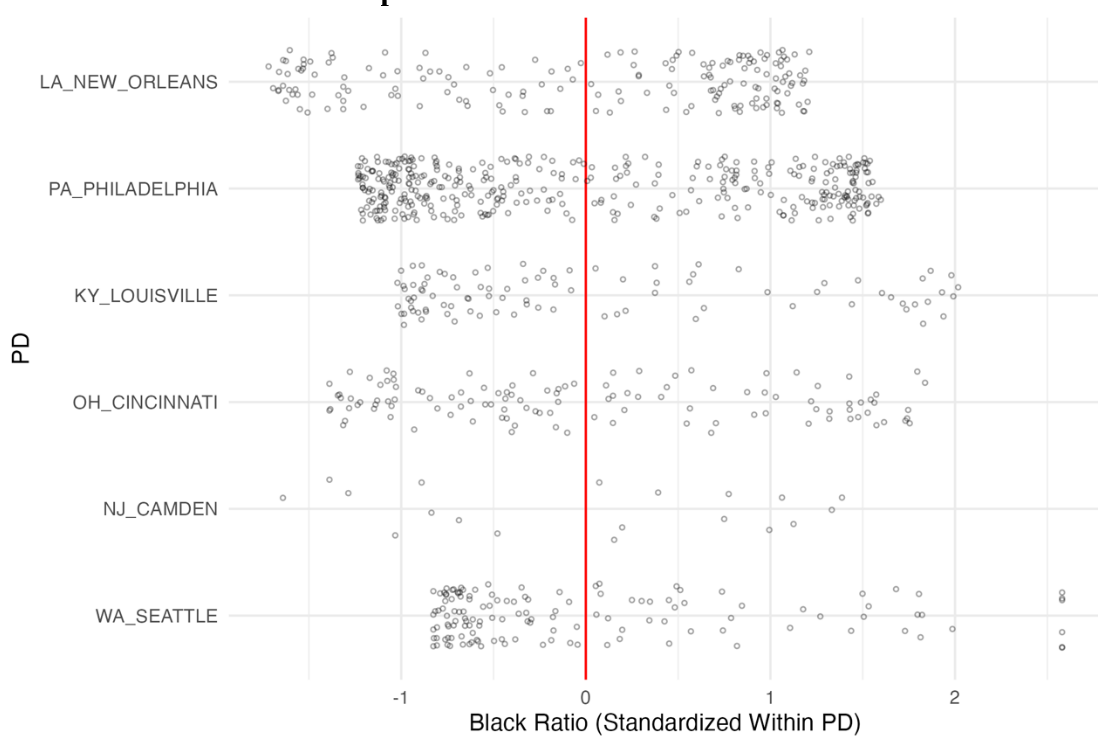
Notes: The figure presents the result of a sensitivity test for the parallel trends assumption using the approach suggested by Rambachan and Roth (2023), based on a panel event study. X-axis indicates the deviation in the slope of the post-treatment trend from the linear extrapolation of the pre-trend. Y-axis shows the 95% confidence intervals for the point estimates for the PDI effects. This analysis suggests that the critical threshold for the significant treatment effects is $\overline{M}$= 0.6. That is, if $\overline{M}$ is equal to or less than 0.5, a negative effect of the PDI on the number of per capita traffic stops is observed.

Figure B2. Heterogeneity of Police Departments in the Distribution of More or Less Black Tracts

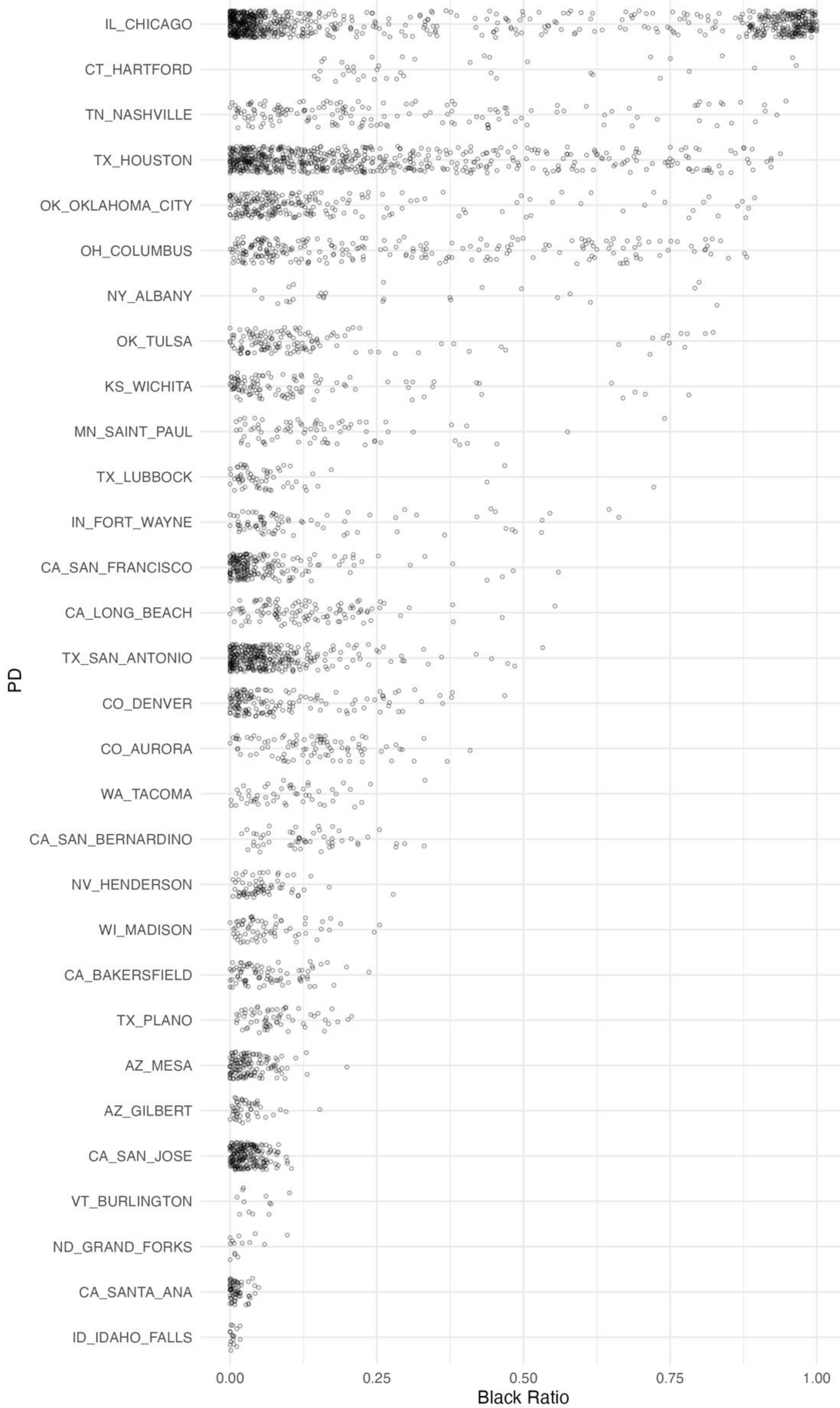
Panel A. Treated Police Departments and the Black Resident Ratios



Panel B. Treated Police Departments and the Standardized Black Resident Ratios



Panel C. Control Police Departments and the Black Resident Ratios



Panel D. Control Police Departments and the Standardized Black Resident Ratios

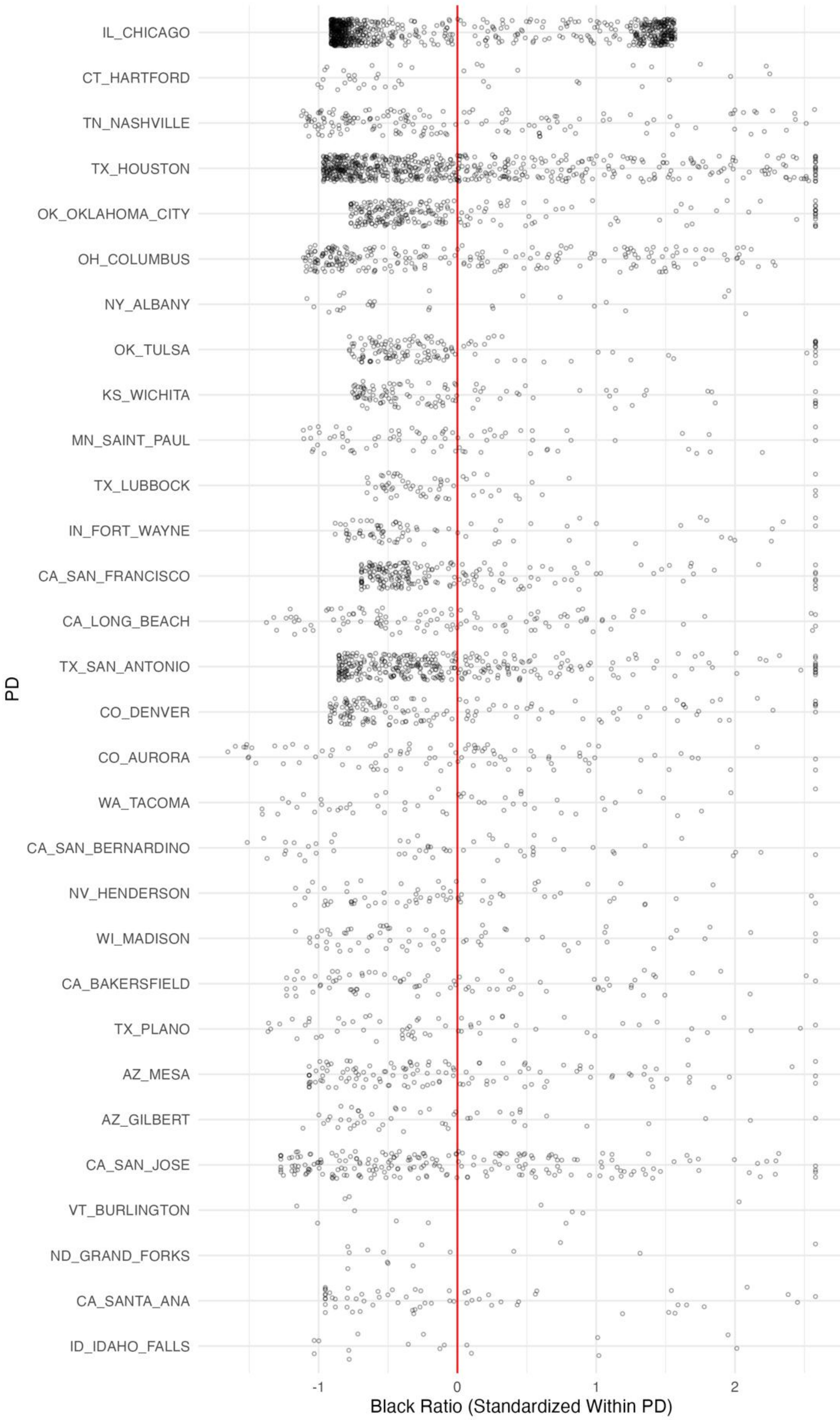
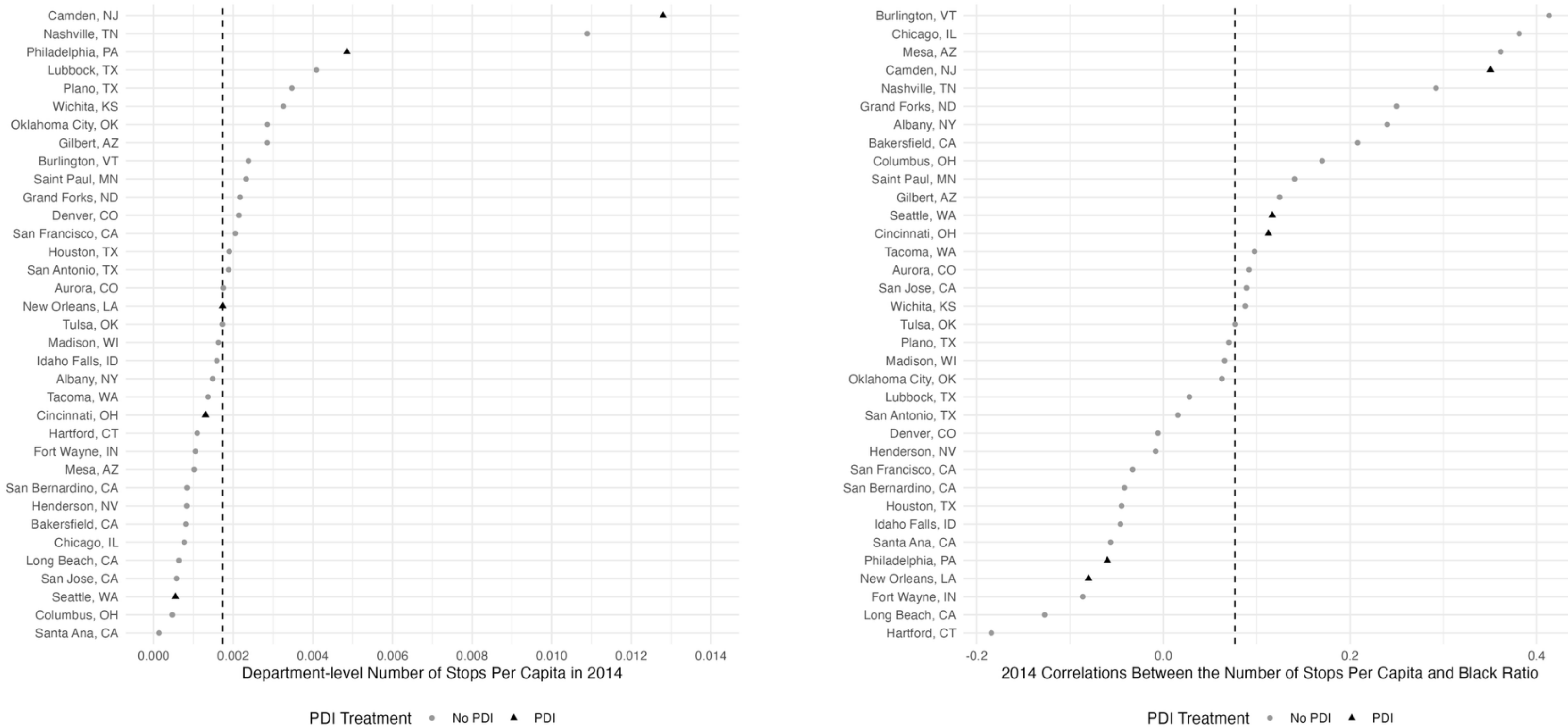


Figure B3. Different Patterns of Public Service Provision and Allocation Prior to the Police Data Initiative (PDI)

A. Number of Traffic Stops Per Capita in 2014

B. Correlations Between Traffic Stops Per Capita and the Proportion of Black Residents in 2014 (“Imbalance Score”)



Note: Data for this figure come from 35 police departments (PDs) in the final sample. One police department is dropped as it does not have observations from 2014: Louisville PD in Kentucky. Black triangles (treatment): 5 PDs that participated in the Police Data Initiative (PDI); grey circles (control): 30 PDs that did not participate in the PDI. The dotted line illustrates the median of the number of stops per capita and the correlations, respectively.

Figure B4. Event Study Plots

The figures below are based on an event study estimation where we incorporate a set of interaction terms between the treatment indicator and the week indicators, with one week before the PDI serving as the reference category. The analysis is based on the observations up to six weeks before and up to 30 weeks after the PDI. The sample consists of 30 control police departments and all treated police departments except for one, as specified in the title of each panel.

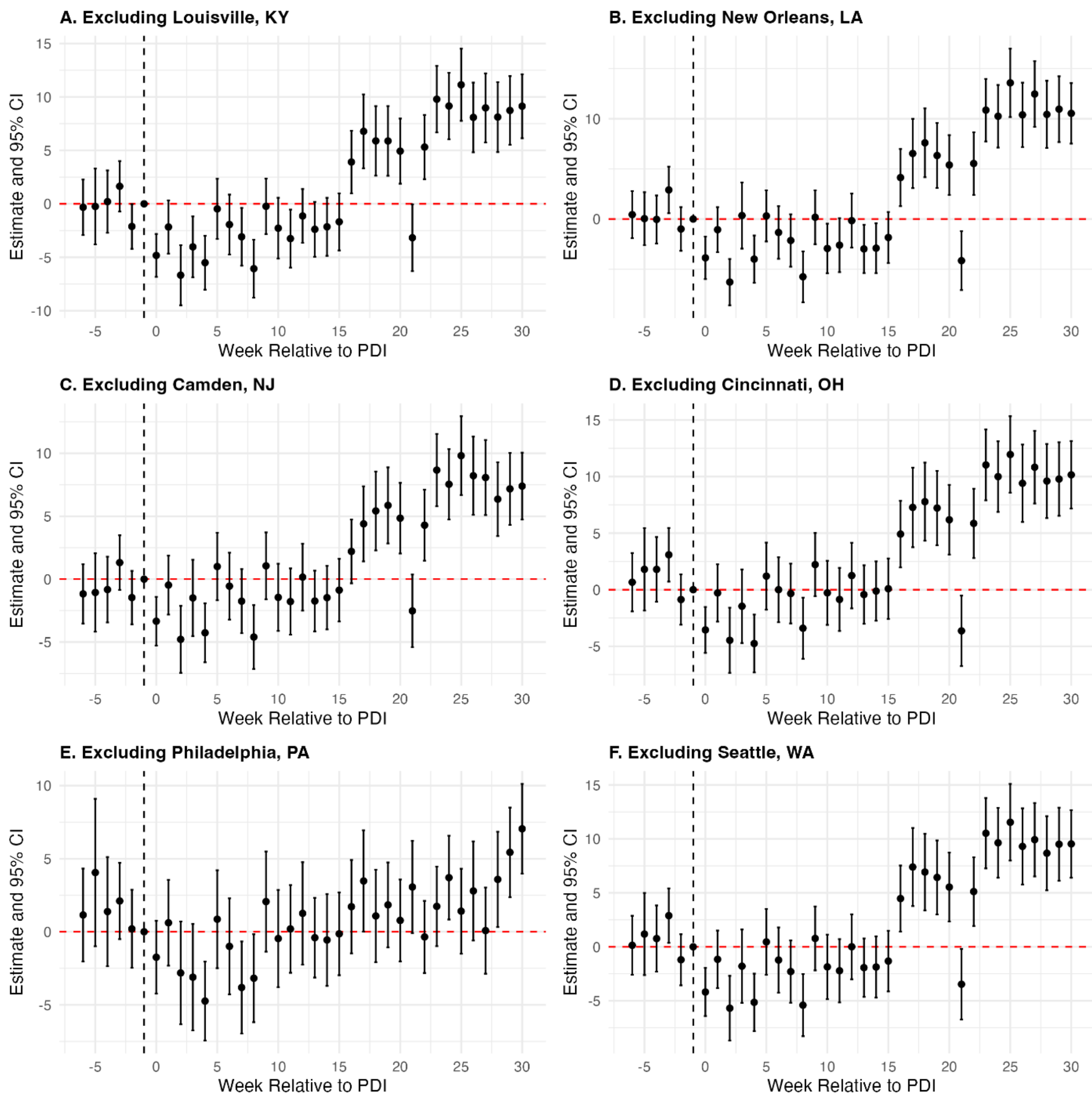


Figure B5. Number of Tracts Associated with Each Treated and Control Police Departments

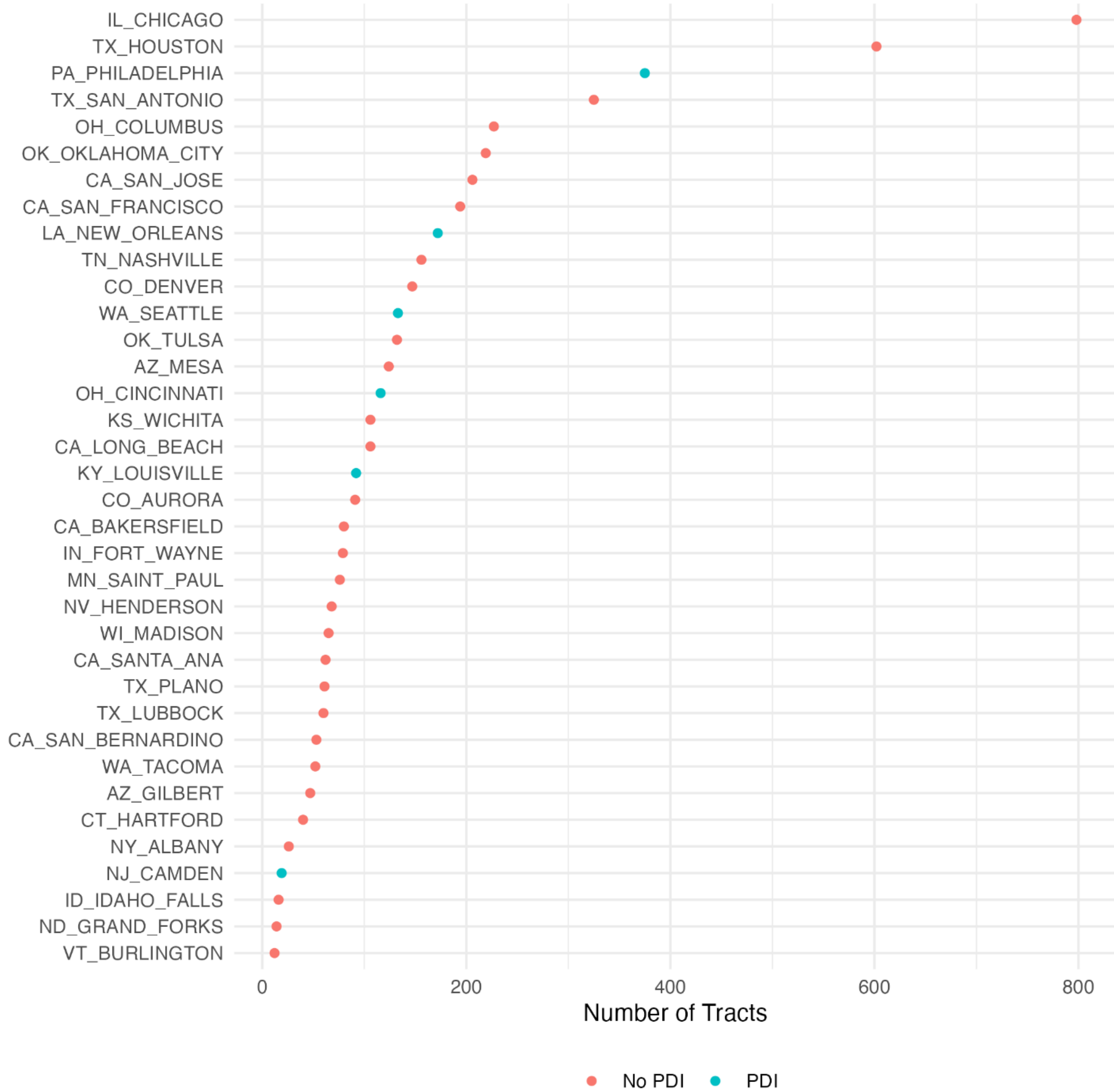


Figure B6. Distribution of the Raw Number of Stops Per Capita

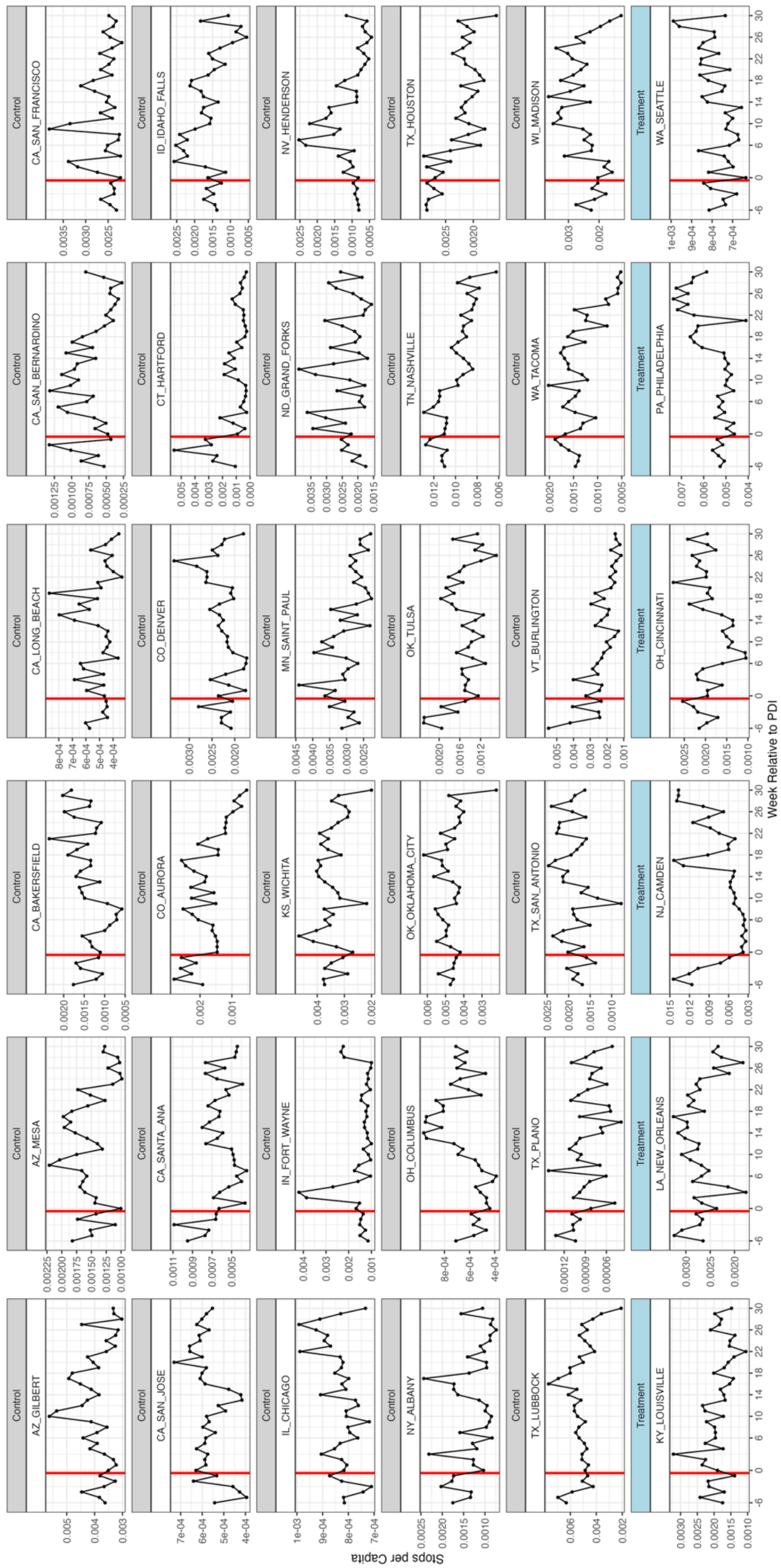


Table B1. OLS Estimation of the PDI Effect on the *Amount* of Traffic Stops Per 10,000 People

The tables below are based on a regression estimation of the model specified in equation (1). The sample consists of 30 control police departments and all treated police departments except for one, as specified in the title of each panel. Clustered robust standard errors in parentheses. + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Panel A. Excluding Louisville, KY

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Weeks	10 Weeks	14 Weeks	18 Weeks	22 Weeks	26 Weeks	30 Weeks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-3.52***	-3.25***	-2.94***	-1.51*	-0.66	0.88	1.91*
	(0.84)	(0.80)	(0.77)	(0.75)	(0.79)	(0.81)	(0.82)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	65780	86020	106260	126500	146740	166980	187220
N of Tracts	5059	5059	5059	5059	5059	5059	5059

Panel B. Excluding New Orleans, LA

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Weeks	10 Weeks	14 Weeks	18 Weeks	22 Weeks	26 Weeks	30 Weeks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-2.66**	-2.80**	-2.74**	-1.38+	-0.64	1.07	2.31**
	(0.91)	(0.86)	(0.83)	(0.80)	(0.83)	(0.84)	(0.84)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	64740	84660	104580	124500	144420	164340	184260
N of Tracts	4979	4979	4979	4979	4979	4979	4979

Panel C. Excluding Camden, NJ

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Weeks	10 Weeks	14 Weeks	18 Weeks	22 Weeks	26 Weeks	30 Weeks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-1.45*	-1.34*	-1.16+	-0.22	0.45	1.73*	2.51***
	(0.66)	(0.63)	(0.61)	(0.63)	(0.65)	(0.68)	(0.70)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Stops	66729	87261	107793	128325	148857	169389	189921
N of Tracts	5132	5132	5132	5132	5132	5132	5132

Panel D. Excluding Cincinnati, OH

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Weeks	10 Weeks	14 Weeks	18 Weeks	22 Weeks	26 Weeks	30 Weeks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-2.98***	-2.46**	-2.10**	-0.83	-0.19	1.25	2.25**
	(0.87)	(0.83)	(0.80)	(0.77)	(0.80)	(0.82)	(0.83)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	65468	85612	105756	125900	146044	166188	186332
N of Tracts	5035	5035	5035	5035	5035	5035	5035

Panel E. Excluding Philadelphia, PA

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Weeks	10 Weeks	14 Weeks	18 Weeks	22 Weeks	26 Weeks	30 Weeks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-3.19***	-3.06***	-2.60**	-2.05*	-1.72*	-1.32	-0.82
	(0.95)	(0.91)	(0.88)	(0.80)	(0.82)	(0.83)	(0.81)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	62101	81209	100317	119425	138533	157641	176749
N of Tracts	4776	4776	4776	4776	4776	4776	4776

Panel F. Excluding Seattle, WA

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Weeks	10 Weeks	14 Weeks	18 Weeks	22 Weeks	26 Weeks	30 Weeks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-3.31***	-3.13***	-2.87***	-1.48+	-0.74	0.80	1.83*
	(0.90)	(0.85)	(0.82)	(0.80)	(0.83)	(0.86)	(0.86)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	65247	85323	105399	125475	145551	165627	185703
N of Tracts	5018	5018	5018	5018	5018	5018	5018

Clustered robust standard errors in parentheses. + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table B2. OLS Estimation of the PDI Effect on the *Allocation* of Traffic Stops Per 10,000 People

The tables below are based on a regression estimation of the model specified in equation (2). The sample consists of 30 control police departments and all treated police departments except for one, as specified in the title of each panel. Clustered robust standard errors in parentheses. + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Panel A. Excluding Louisville, KY

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Wks	10 Wks	14 Wks	18 Wks	22 Wks	26 Wks	30 Wks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-3.53***	-3.26***	-2.95***	-1.52*	-0.67	0.87	1.90*
	(0.84)	(0.80)	(0.77)	(0.75)	(0.79)	(0.81)	(0.82)
Post × Black Ratio	0.81**	0.85**	0.67*	0.67*	0.67*	0.59+	0.56+
	(0.29)	(0.29)	(0.26)	(0.28)	(0.29)	(0.30)	(0.30)
Treatment × Post × Black Ratio	-1.33+	-1.65*	-1.36*	-0.51	0.21	1.30+	1.98*
	(0.71)	(0.69)	(0.66)	(0.68)	(0.72)	(0.77)	(0.81)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	65780	86020	106260	126500	146740	166980	187220
N of Tracts	5059	5059	5059	5059	5059	5059	5059

Panel B. Excluding New Orleans, LA

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Wks	10 Wks	14 Wks	18 Wks	22 Wks	26 Wks	30 Wks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-2.68**	-2.82**	-2.75***	-1.39+	-0.65	1.06	2.30**
	(0.91)	(0.86)	(0.83)	(0.80)	(0.83)	(0.84)	(0.84)
Post × Black Ratio	0.81**	0.85**	0.67*	0.67*	0.67*	0.59+	0.56+
	(0.29)	(0.29)	(0.26)	(0.28)	(0.29)	(0.30)	(0.30)
Treatment × Post × Black Ratio	-1.38+	-1.80*	-1.46*	-0.63	0.06	1.24	2.02*
	(0.75)	(0.73)	(0.70)	(0.72)	(0.77)	(0.83)	(0.87)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	64740	84660	104580	124500	144420	164340	184260
N of Tracts	4979	4979	4979	4979	4979	4979	4979

Panel C. Excluding Camden, NJ

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Wks	10 Wks	14 Wks	18 Wks	22 Wks	26 Wks	30 Wks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-1.47*	-1.36*	-1.18+	-0.23	0.44	1.72*	2.51***
	(0.66)	(0.63)	(0.61)	(0.63)	(0.65)	(0.68)	(0.70)
Post × Black Ratio	0.81**	0.85**	0.67*	0.67*	0.67*	0.59+	0.56+
	(0.29)	(0.29)	(0.26)	(0.28)	(0.29)	(0.30)	(0.30)
Treatment × Post × Black Ratio	-0.84	-1.20*	-0.96+	-0.28	0.34	1.38*	2.01**
	(0.61)	(0.59)	(0.57)	(0.61)	(0.65)	(0.70)	(0.74)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	66729	87261	107793	128325	148857	169389	189921
N of Tracts	5132	5132	5132	5132	5132	5132	5132

Panel D. Excluding Cincinnati, OH

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Wks	10 Wks	14 Wks	18 Wks	22 Wks	26 Wks	30 Wks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-3.00***	-2.47**	-2.11**	-0.84	-0.20	1.24	2.24**
	(0.87)	(0.83)	(0.80)	(0.77)	(0.80)	(0.82)	(0.82)
Post × Black Ratio	0.81**	0.85**	0.67*	0.67*	0.67*	0.59+	0.56+
	(0.29)	(0.29)	(0.26)	(0.28)	(0.29)	(0.30)	(0.30)
Treatment × Post × Black Ratio	-1.12	-1.45*	-1.13+	-0.31	0.34	1.43+	2.11*
	(0.73)	(0.70)	(0.67)	(0.69)	(0.74)	(0.79)	(0.83)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	65468	85612	105756	125900	146044	166188	186332
N of Tracts	5035	5035	5035	5035	5035	5035	5035

Panel E. Excluding Philadelphia, PA

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Wks	10 Wks	14 Wks	18 Wks	22 Wks	26 Wks	30 Wks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-3.20***	-3.08***	-2.62**	-2.06**	-1.73*	-1.34	-0.84
	(0.96)	(0.91)	(0.88)	(0.80)	(0.82)	(0.83)	(0.81)
Post × Black Ratio	0.81**	0.85**	0.67*	0.67*	0.67*	0.59+	0.56+
	(0.29)	(0.29)	(0.26)	(0.28)	(0.29)	(0.30)	(0.30)
Treatment × Post × Black Ratio	-1.25+	-1.66*	-1.55*	-1.45*	-1.44*	-1.24*	-1.02+
	(0.69)	(0.67)	(0.63)	(0.58)	(0.59)	(0.59)	(0.58)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	62101	81209	100317	119425	138533	157641	176749
N of Tracts	4776	4776	4776	4776	4776	4776	4776

Panel F. Excluding Seattle, WA

Outcome	Stops Per Capita						
Pre Period	6 Weeks						
Post Period	6 Wks	10 Wks	14 Wks	18 Wks	22 Wks	26 Wks	30 Wks
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment × Post	-3.32***	-3.14***	-2.88***	-1.49+	-0.75	0.78	1.81*
	(0.90)	(0.85)	(0.82)	(0.80)	(0.83)	(0.85)	(0.86)
Post × Black Ratio	0.81**	0.85**	0.67*	0.67*	0.67*	0.59+	0.56+
	(0.29)	(0.29)	(0.26)	(0.28)	(0.29)	(0.30)	(0.30)
Treatment × Post × Black Ratio	-1.19	-1.60*	-1.32+	-0.52	0.17	1.29	1.99*
	(0.73)	(0.71)	(0.68)	(0.70)	(0.75)	(0.80)	(0.84)
Tract FE	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y
N of Obs	65247	85323	105399	125475	145551	165627	185703
N of Tracts	5018	5018	5018	5018	5018	5018	5018