## Supplemental Information

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# A Data Availability Across 20 Largest Cities

**Table A1: Data Availability Across Top 20 Most Populated US Cities**

| City | State | Population Size | Crime Data | Call Data | Stop Data | Stop Race Data | Car Accident Data | Arrest Data | Shot Spotter Data | Use of Force Data | Complaint Data | Car Citation Data | SQF Data | Mayor Party | Evidence Of BLM Protest |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| New York City | NY | 8804190 | ✗ | ✓ | ✓ | ✓ | ✓ | ✓ | ✗ | ✓ | ✓ | ✗ | ✓ | Democrat | ✓ |
| Los Angeles | CA | 3898747 | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Chicago | IL | 2746388 | ✓ | ✗ | ✓ | ✓ | ✓ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Houston | TX | 2304580 | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Phoenix | AZ | 1608139 | ✓ | ✓ | ✗ | ✗ | ✗ | ✓ | ✗ | ✓ | ✗ | ✓ | ✗ | Democrat | ✓ |
| Philadelphia | PA | 1608139 | ✓ | ✗ | ✓ | ✓ | ✗ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| San Antonio | TX | 1434625 | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Independent (Progressive) | ✓ |
| San Diego | CA | 1386932 | ✗ | ✓ | ✗ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Republican | ✓ |
| Dallas | TX | 1304379 | ✓ | ✗ | ✗ | ✗ | ✓ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| San Jose | CA | 1013240 | ✗ | ✓ | ✗ | ✗ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Austin | TX | 961855 | ✓ | ✗ | ✓ | ✗ | ✗ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Jacksonville | FL | 949611 | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Republican | ✓ |
| Fort Worth | TX | 918915 | ✓ | ✗ | ✗ | ✗ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Republican | ✓ |
| Columbus | OH | 905748 | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Indianapolis | IN | 897041 | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✓ | ✓ | ✗ | ✗ | Democrat | ✓ |
| Charlotte | NC | 874579 | ✓ | ✓ | ✓ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| San Francisco | CA | 873965 | ✓ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Seattle | WA | 737015 | ✓ | ✓ | ✓ | ✓ | ✗ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Nashville | TN | 715884 | ✓ | ✓ | ✗ | ✗ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |
| Denver | CO | 715522 | ✓ | ✗ | ✓ | ✗ | ✓ | ✗ | ✗ | ✗ | ✗ | ✗ | ✗ | Non-Partisan (Democrat) | ✓ |
| D.C. | N/A | 712816 | ✓ | ✗ | ✓ | ✓ | ✓ | ✓ | ✓ | ✗ | ✗ | ✗ | ✗ | Democrat | ✓ |

Note: Shaded rows denote cities included in study. Population data from U.S. Census (2020).

# B RDiT Tables Associated with in-Text Figures

**Table B2:** RDiT Coefficients Characterizing the Effect of BLM Protests on Policing Activities.

| City | Stops | Coeff | SE | P-Val | N-Val | Effective N | Bandwidth Est. |
|---|---|---|---|---|---|---|---|
| Austin | Traffic | -2.03 | 0.24 | 0.000 | 514 | 56.41 | 28.21 |
| LA | Pedestrian | -1.87 | 0.18 | 0.000 | 697 | 164.25 | 82.13 |
| LA | Traffic | -2.84 | 0.26 | 0.000 | 697 | 117.37 | 58.68 |
| Philly | Pedestrian | -0.19 | 0.04 | 0.000 | 880 | 124.09 | 62.04 |
| Philly | Traffic | -0.58 | 0.04 | 0.000 | 880 | 91.81 | 45.91 |
| Seattle | Terry | -1.58 | 0.21 | 0.000 | 1885 | 170.99 | 85.49 |

*All estimates are specified with a uniform kernel and polynomial degree equal to 1. Standard errors are robust.*

**Table B3:** RDiT Coefficients Characterizing Changes in Officer  Civilian Initiated Calls Following the BLM Protest.

| City | Officer:Civilian Calls | Coeff | SE | P-Val | N-Val | Effective N | Bandwidth Est. |
|---|---|---|---|---|---|---|---|
| LA | Call Difference | -146.43 | 22.96 | 0.000 | 4014 | 96.15 | 48.07 |
| Seattle | Call Difference | -0.43 | 0.04 | 0.000 | 4014 | 118.86 | 59.43 |
| LA | Call Ratio | -474.43 | 123.30 | 0.000 | 513 | 170.24 | 85.12 |
| Seattle | Call Ratio | -0.24 | 0.04 | 0.000 | 513 | 227.64 | 113.82 |

*All estimates are specified with a uniform kernel and polynomial degree equal to 1. Standard errors are robust.*

**Table B4:** RDiT Coefficients Characterizing the Effect of BLM Protests on Policing Quality.

| City | Outcome | Coeff | SE | P-Val | N-Val | Effective N | Bandwidth Est. |
|---|---|---|---|---|---|---|---|
| Austin | Hit rate | 0.04 | 0.01 | 0.00 | 514 | 82.98 | 41.49 |
| LA | Hit rate | 0.00 | 0.01 | 0.88 | 697 | 409.87 | 204.94 |
| Philly | Hit rate | 0.01 | 0.01 | 0.85 | 2341 | 239.23 | 119.62 |
| Seattle | Hit rate | 0.15 | 0.07 | 0.02 | 1885 | 337.01 | 168.50 |
| Austin | Arrest rate | 0.13 | 0.03 | 0.00 | 514 | 62.42 | 31.21 |
| LA | Arrest rate | 0.03 | 0.01 | 0.00 | 697 | 339.91 | 169.95 |
| Philly | Arrest rate | 0.01 | 0.01 | 0.85 | 2341 | 239.23 | 119.62 |
| Seattle | Arrest rate | 0.15 | 0.07 | 0.02 | 1885 | 337.01 | 168.50 |
| Austin | B/W rate ratio | 0.90 | 0.48 | 0.04 | 357 | 64.22 | 32.11 |
| LA | B/W rate ratio | -1.69 | 0.50 | 0.00 | 697 | 236.81 | 118.40 |
| Philly | B/W rate ratio | -4.74 | 1.72 | 0.00 | 2337 | 209.27 | 104.64 |
| Seattle | B/W rate ratio | -6.99 | 3.02 | 0.01 | 339 | 183.84 | 91.92 |

*All estimates are specified with a uniform kernel and polynomial degree equal to 1. Standard errors are robust.*

**Table B5:** RDiT Coefficients Characterizing the Effect of BLM Protests on Crime.

| City | Crime Type | Coeff | SE | P-Val | N-Val | Effective N | Bandwidth Est. |
|---|---|---|---|---|---|---|---|
| Austin | Violent | -0.24 | 0.18 | 0.07 | 6358 | 276.61 | 138.31 |
| Austin | Property | 0.08 | 0.24 | 0.96 | 6358 | 138.35 | 69.18 |
| Austin | Society | -0.23 | 0.06 | 0.00 | 6358 | 277.56 | 138.78 |
| LA | Violent | 0.90 | 0.17 | 0.00 | 3800 | 383.81 | 191.91 |
| LA | Property | 0.61 | 0.20 | 0.00 | 3800 | 189.13 | 94.56 |
| LA | Society | 0.11 | 0.17 | 0.26 | 3800 | 351.43 | 175.71 |
| Philly | Violent | 0.51 | 0.19 | 0.03 | 5263 | 172.81 | 86.40 |
| Philly | Property | 0.50 | 0.30 | 0.05 | 5263 | 284.67 | 142.33 |
| Philly | Society | -0.34 | 0.11 | 0.00 | 5263 | 178.23 | 89.11 |
| Seattle | Violent | 0.04 | 0.27 | 0.80 | 4532 | 150.25 | 75.13 |
| Seattle | Property | -0.72 | 0.35 | 0.02 | 4532 | 179.89 | 89.94 |
| Seattle | Society | -1.34 | 0.18 | 0.00 | 4532 | 274.78 | 137.39 |

*All estimates are specified with a uniform kernel and polynomial degree equal to 1. Standard errors are robust.*

# C Results by demographic composition of smaller geographic unit

**Table C6: Regression discontinuity coefficient difference by beat, Seattle**

| | Coefficient difference | P-value | Bandwidth | Measure | DV |
|---|---|---|---|---|---|
| (1) | −0.878 | .950 | 25 | Income | Terry stops |
| (2) | −0.974 | .946 | 50 | Income | Terry stops |
| (3) | −1.209 | .934 | 100 | Income | Terry stops |
| (4) | 1.460 | .806 | 25 | Nonwhite | Terry stops |
| (5) | 1.069 | .860 | 50 | Nonwhite | Terry stops |
| (6) | 1.166 | .852 | 100 | Nonwhite | Terry stops |
| (7) | −16.107 | .985 | 25 | Income | Calls |
| (8) | −19.631 | .979 | 50 | Income | Calls |
| (9) | −22.031 | .976 | 100 | Income | Calls |
| (10) | 13.948 | .976 | 25 | Nonwhite | Calls |
| (11) | 17.272 | .964 | 50 | Nonwhite | Calls |
| (12) | 20.131 | .947 | 100 | Nonwhite | Calls |

**Table C7: Regression discontinuity coefficient difference by block group, Austin**

| | Coefficient difference | P-value | Bandwidth | Measure | DV | City |
|---|---|---|---|---|---|---|
| (1) | −0.013 | 0.970 | 25 | Income | Terry stops | Austin |
| (2) | −0.012 | 0.971 | 50 | Income | Terry stops | Austin |
| (3) | −0.010 | 0.974 | 100 | Income | Terry stops | Austin |
| (4) | −0.018 | 0.946 | 25 | Nonwhite | Terry stops | Austin |
| (5) | −0.020 | 0.935 | 50 | Nonwhite | Terry stops | Austin |
| (6) | −0.015 | 0.951 | 100 | Nonwhite | Terry stops | Austin |
| (7) | −0.045 | 0.833 | 25 | Black | Terry stops | Austin |
| (8) | −0.036 | 0.844 | 50 | Black | Terry stops | Austin |
| (9) | −0.016 | 0.931 | 100 | Black | Terry stops | Austin |
| (10) | 0.000 | 0.999 | 25 | Income | Crime | Austin |
| (11) | 0.000 | 0.999 | 50 | Income | Crime | Austin |
| (12) | 0.000 | 0.998 | 100 | Income | Crime | Austin |
| (13) | 0.000 | 0.999 | 25 | Nonwhite | Crime | Austin |
| (14) | 0.000 | 0.998 | 50 | Nonwhite | Crime | Austin |
| (15) | 0.000 | 0.996 | 100 | Nonwhite | Crime | Austin |
| (16) | 0.000 | 0.989 | 25 | Black | Crime | Austin |
| (17) | 0.000 | 0.987 | 50 | Black | Crime | Austin |
| (18) | 0.000 | 0.993 | 100 | Black | Crime | Austin |

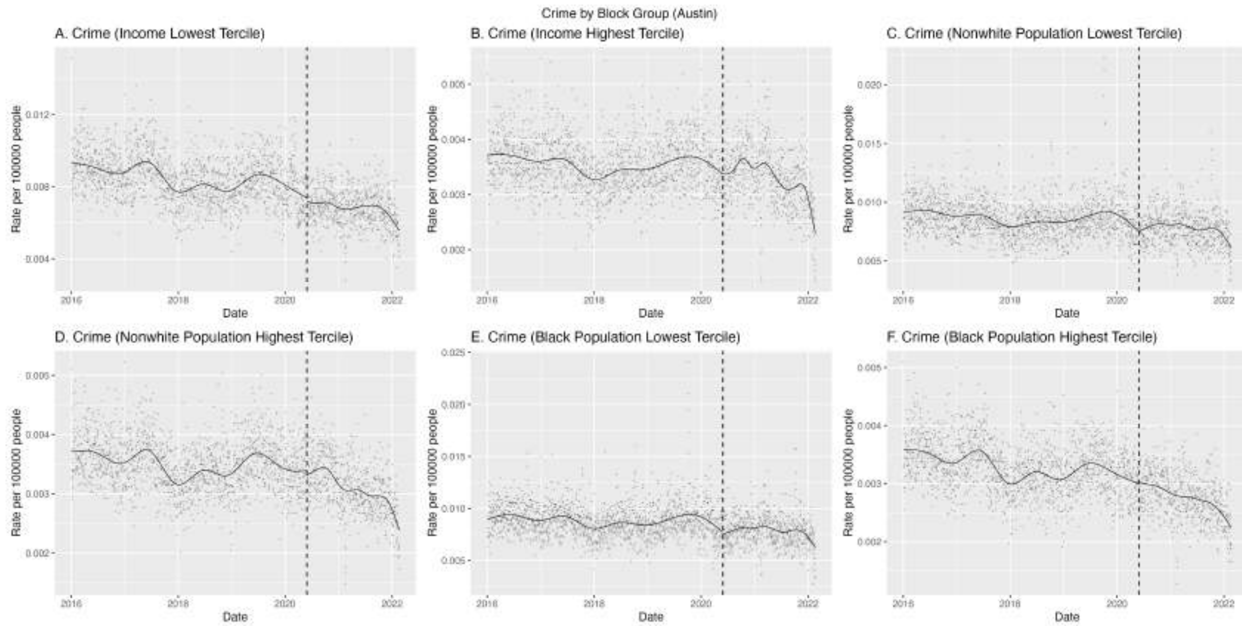


**Figure C1**

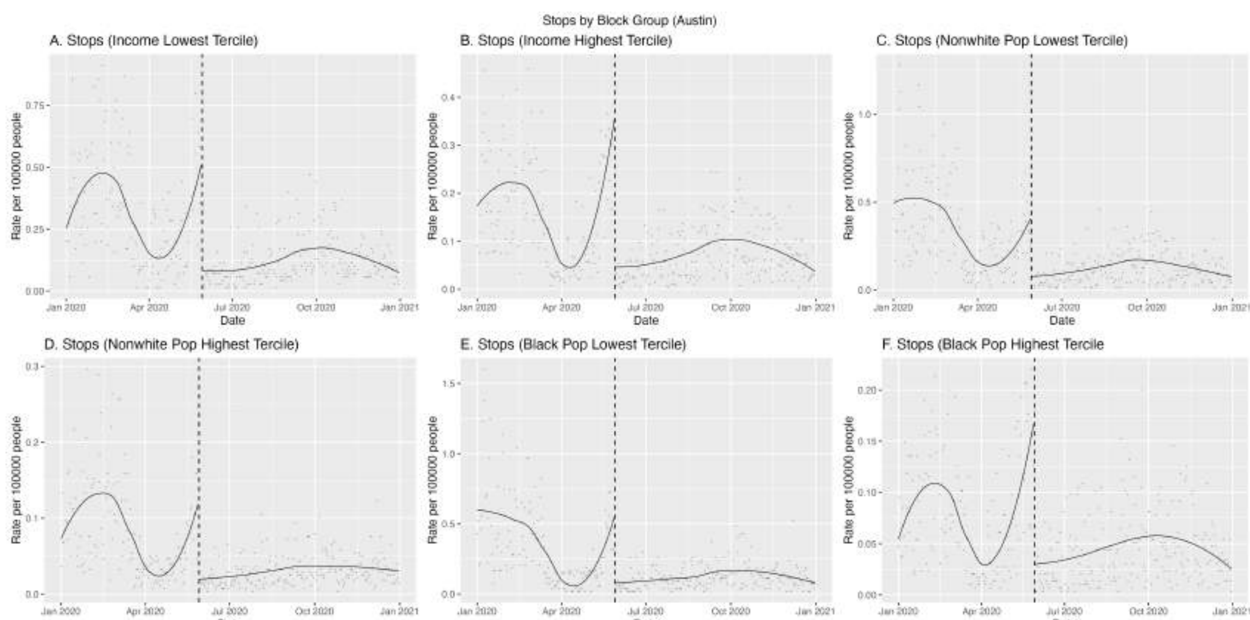


**Figure C2**

**Table C8: Regression discontinuity coefficient difference by block group, LA**

| | Coefficient difference | P-value | Bandwidth | Measure | DV | City |
|---|---|---|---|---|---|---|
| (1) | 0.000 | 0.993 | 25 | Income | Crime | LA |
| (2) | 0.000 | 0.993 | 50 | Income | Crime | LA |
| (3) | 0.000 | 0.997 | 100 | Income | Crime | LA |
| (4) | 0.000 | 0.993 | 25 | Nonwhite | Crime | LA |
| (5) | 0.000 | 0.999 | 50 | Nonwhite | Crime | LA |
| (6) | 0.000 | 0.967 | 100 | Nonwhite | Crime | LA |
| (7) | 0.000 | 0.994 | 25 | Black | Crime | LA |
| (8) | 0.000 | 0.996 | 50 | Black | Crime | LA |
| (9) | 0.000 | 0.998 | 100 | Black | Crime | LA |

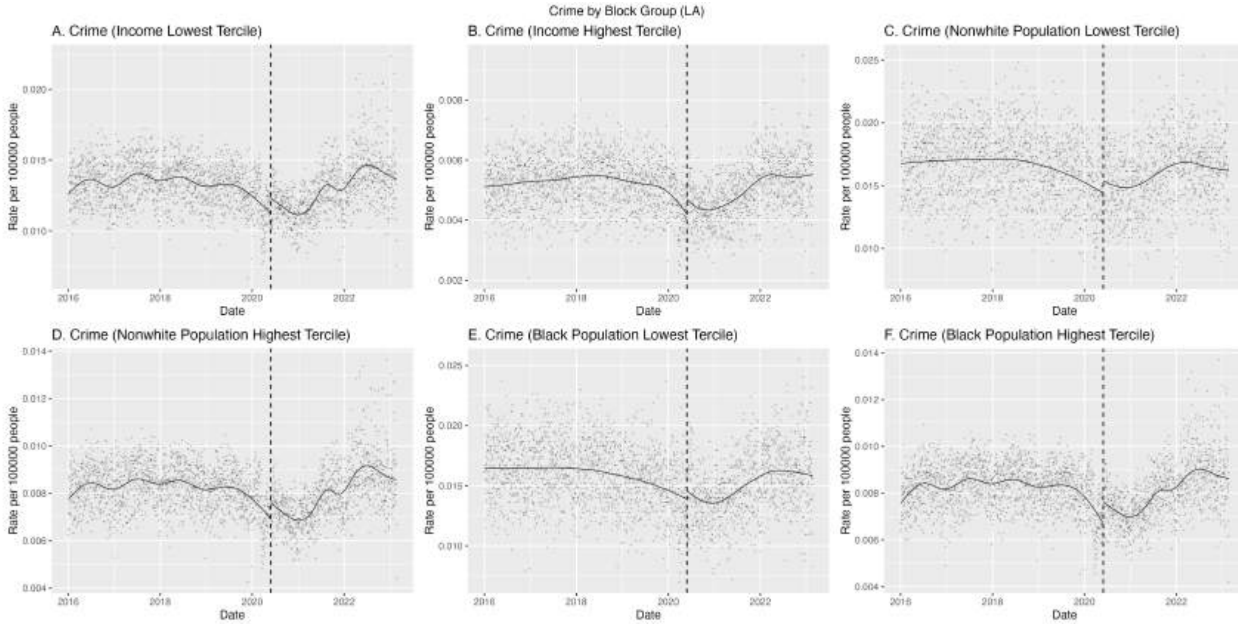


**Figure C3**

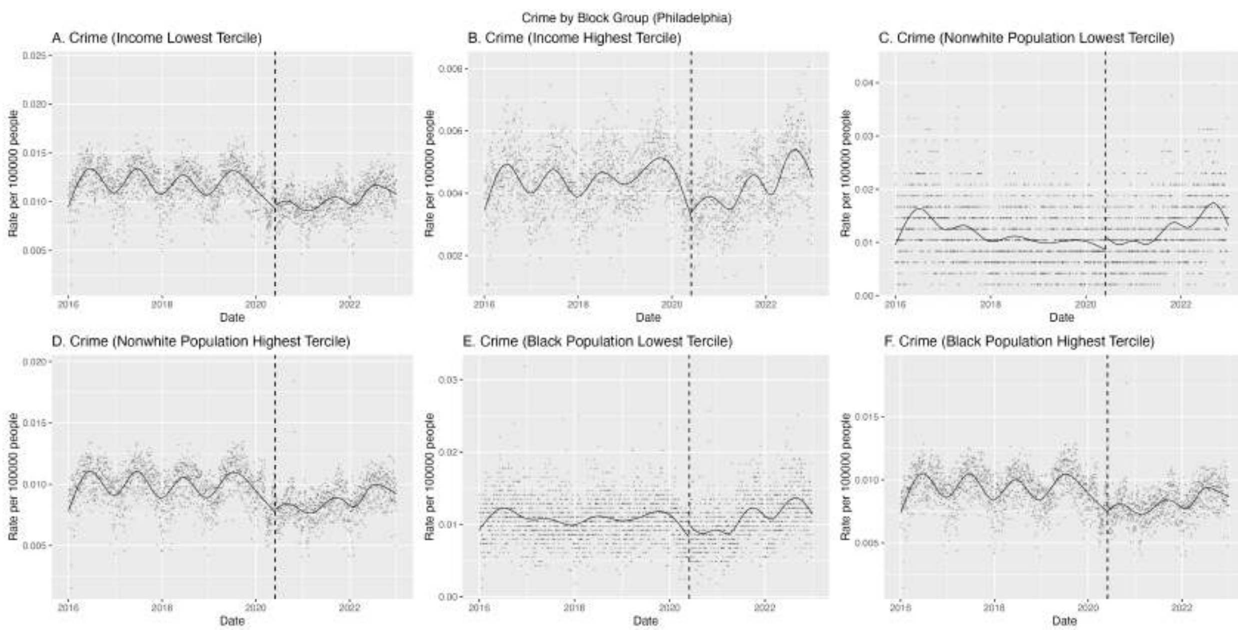


**Figure C4**

**Table C9: Regression discontinuity coefficient difference by block group, Philadelphia**

| | Coefficient difference | P-value | Bandwidth | Measure | DV | City |
|---|---|---|---|---|---|---|
| (10) | −0.007 | 0.932 | 25 | Income | Vehicle stops | Philadelphia |
| (11) | −0.007 | 0.927 | 50 | Income | Vehicle stops | Philadelphia |
| (12) | −0.004 | 0.971 | 100 | Income | Vehicle stops | Philadelphia |
| (13) | 0.023 | 0.780 | 25 | Nonwhite | Vehicle stops | Philadelphia |
| (14) | 0.013 | 0.831 | 50 | Nonwhite | Vehicle stops | Philadelphia |
| (15) | 0.007 | 0.921 | 100 | Nonwhite | Vehicle stops | Philadelphia |
| (16) | 0.008 | 0.875 | 25 | Black | Vehicle stops | Philadelphia |
| (17) | 0.008 | 0.877 | 50 | Black | Vehicle stops | Philadelphia |
| (18) | 0.007 | 0.908 | 100 | Black | Vehicle stops | Philadelphia |
| (19) | 0.001 | 0.998 | 25 | Income | Crime | Philadelphia |
| (20) | −0.001 | 0.999 | 50 | Income | Crime | Philadelphia |
| (21) | −0.001 | 0.999 | 100 | Income | Crime | Philadelphia |
| (22) | 0.003 | 0.972 | 25 | Nonwhite | Crime | Philadelphia |
| (23) | 0.004 | 0.965 | 50 | Nonwhite | Crime | Philadelphia |
| (24) | 0.003 | 0.967 | 100 | Nonwhite | Crime | Philadelphia |
| (25) | 0.002 | 0.980 | 25 | Black | Crime | Philadelphia |
| (26) | 0.003 | 0.967 | 50 | Black | Crime | Philadelphia |
| (27) | 0.002 | 0.968 | 100 | Black | Crime | Philadelphia |

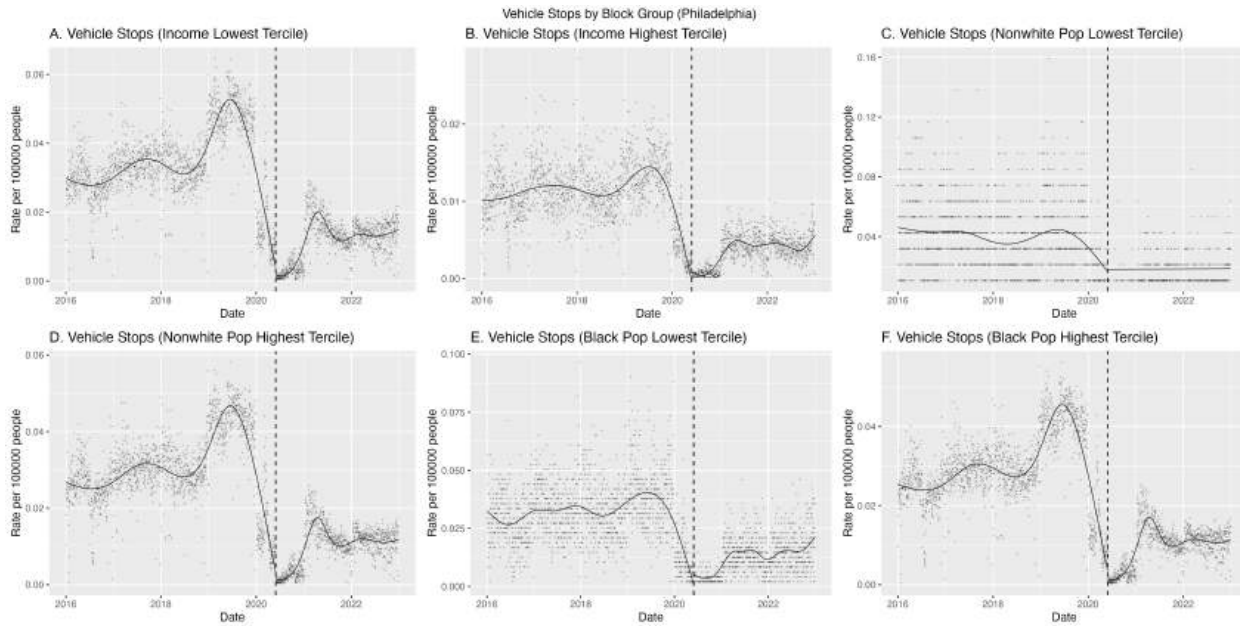


**Figure C5**

# D Efficiency Is Not a Function of Crime

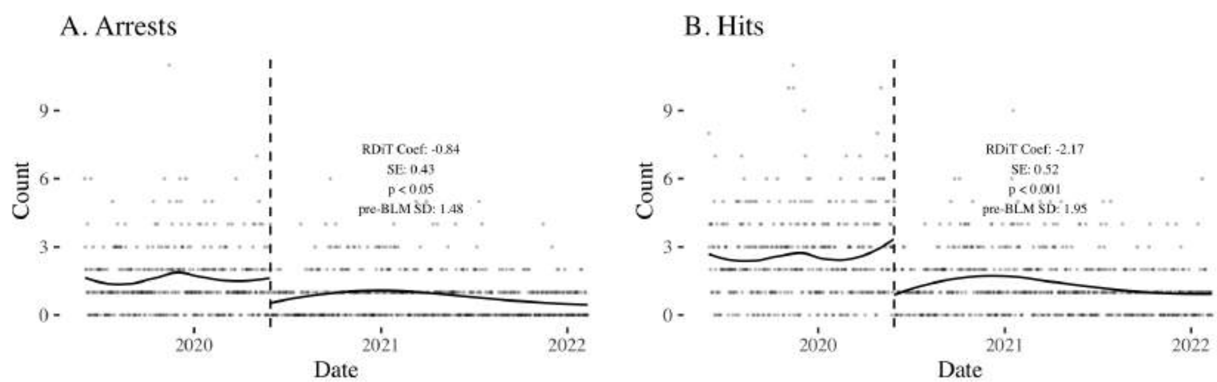


**Figure D6:** Terry Stop Arrest (Panel A) and Hit Counts (Panel B, y-axis) Over Time (x-axis). Loess lines fit on each side of the *BLM protest* discontinuity. Dashed vertical line denotes *BLM protest* onset. Annotations denote RDiT coefficients using a running variable to the 1st degree.

# E Alternative RDiT Specifications

In each figure, the x-axis characterizes the different kernel/polynomial specifications (0 = difference-in-means, 1 = linear polynomial, 2 = quadratic polynomial, 3 = cubic polynomial; Tri. = triangular kernel, Uni. = uniform kernel, Epa. = epanechnikov kernel). The y-axis characterizes the unstandardized RDiT coefficient for each of the respective outcomes (characterized by separate facets). 95% CIs displayed from robust SEs

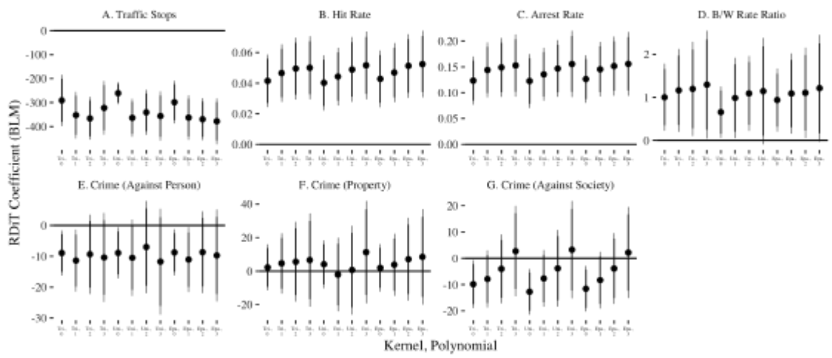


**Figure E7: Alternative RDiT Specifications Across Outcomes: Austin**

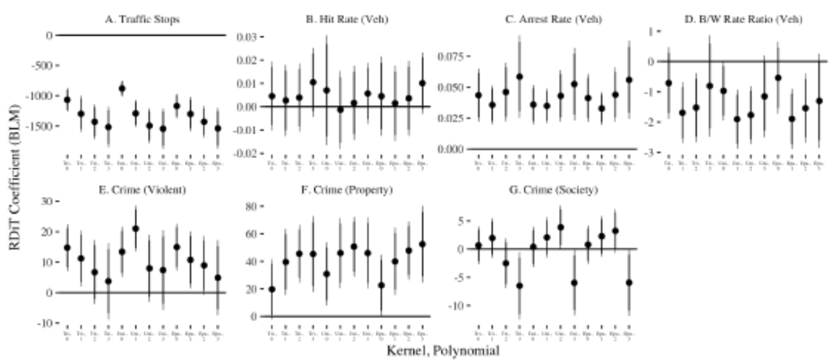


**Figure E8: Alternative RDiT Specifications Across Outcomes: Los Angeles**

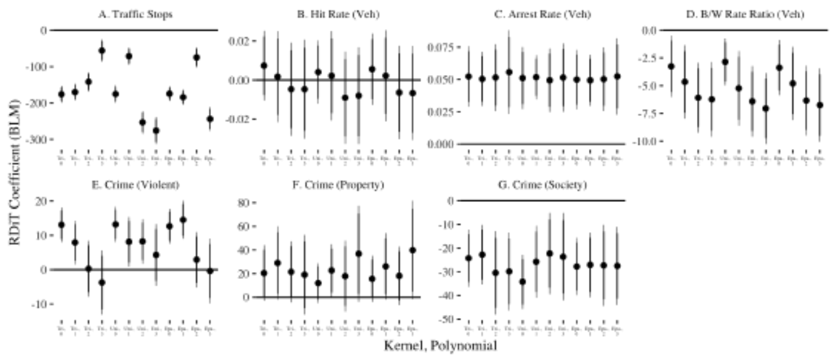


**Figure E9: Alternative RDiT Specifications Across Outcomes: Philadelphia**

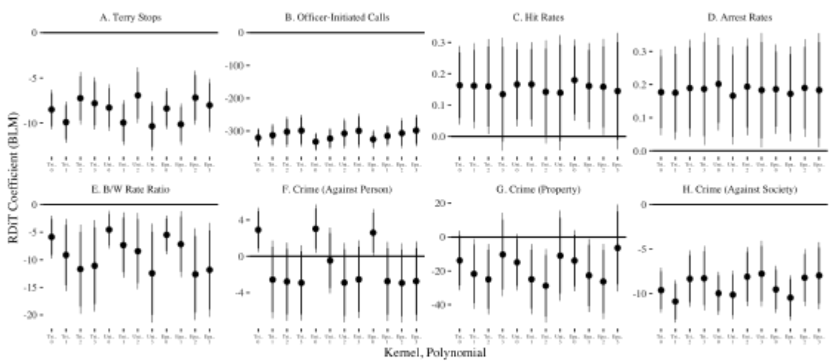


**Figure E10: Alternative RDiT Specifications Across Outcomes: Seattle**

# F Alternative Bandwidths

In each alternative bandwidth plot, the x-axis is the number of days used in the data before and after the *BLM protest*. The y-axis is the unstandardized RDiT coefficient. Each facet characterizes a different kernel (Tri. = triangular, Uni. = uniform, Epa. = Epanechnikov) and polynomial (0, 1, 2, 3) specification. 95% CIs displayed from robust SEs.

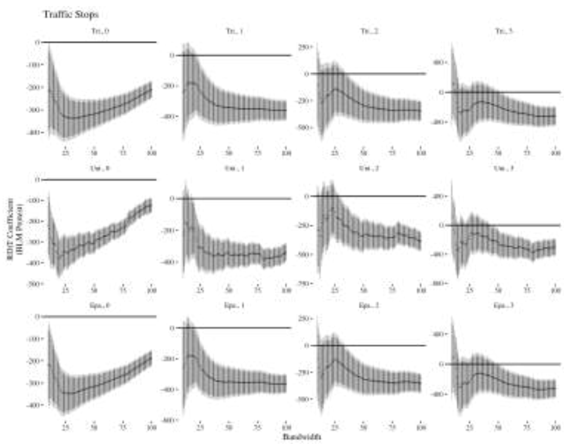


**Figure F11:** Austin, Traffic Stop Outcome

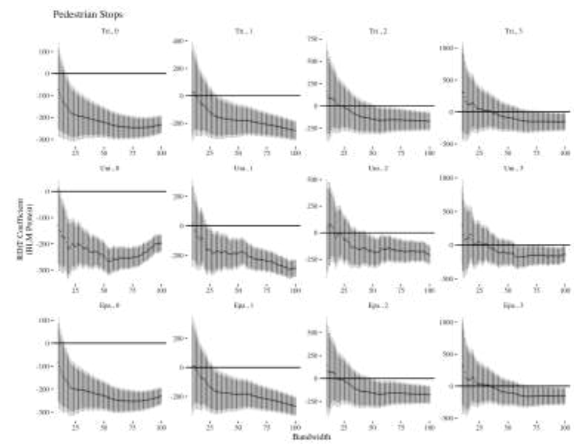


**Figure F12:** Los Angeles, Pedestrian Stops Outcome

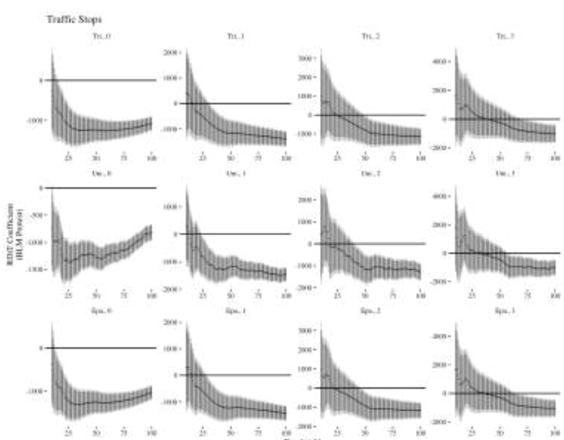


**Figure F13:** Los Angeles, Traffic Stops Outcome

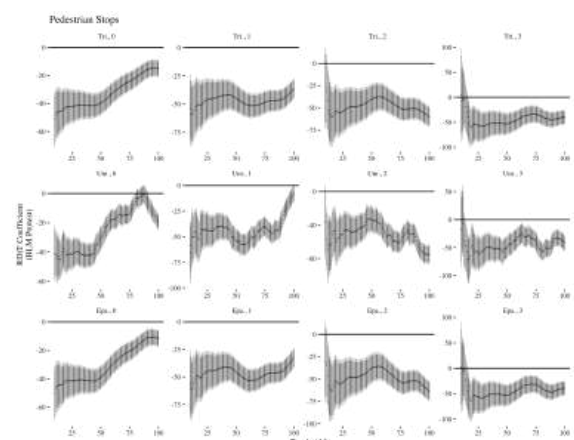


**Figure F14:** Philadelphia, Pedestrian Stops Outcome

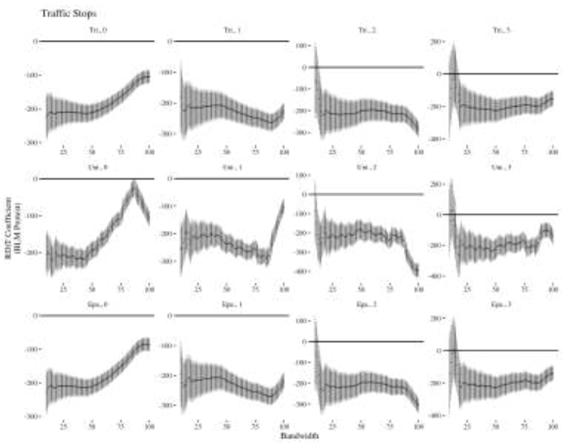


**Figure F15:** Philadelphia, Vehicle Stops Outcome

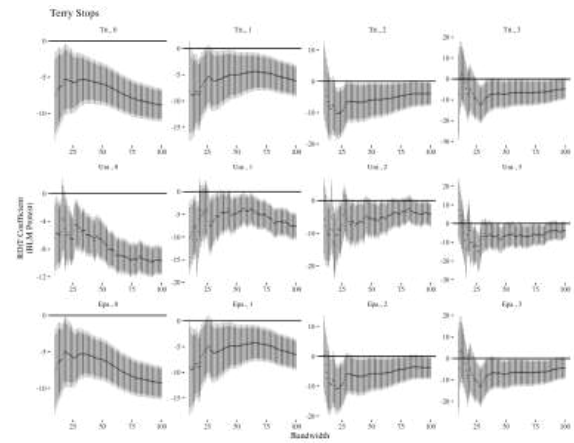


**Figure F16:** Seattle, Terry Stops Outcome

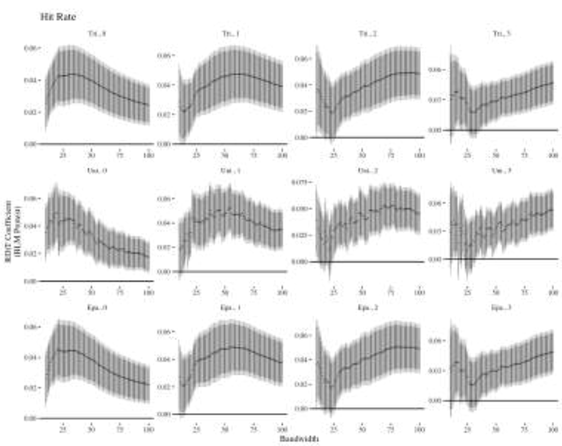


**Figure F17:** Austin, Hit Rates Outcome

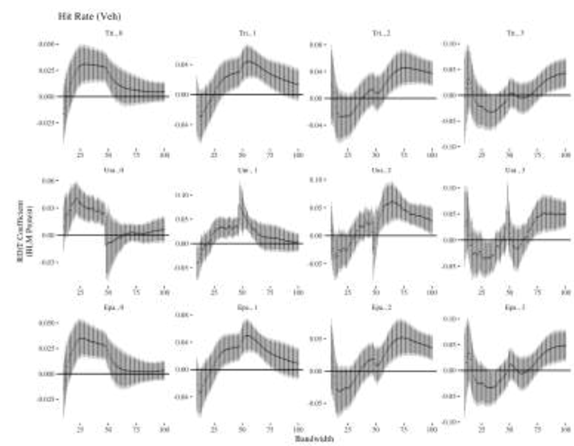


**Figure F18:** Los Angeles, Vehicle Hit Rates Outcome

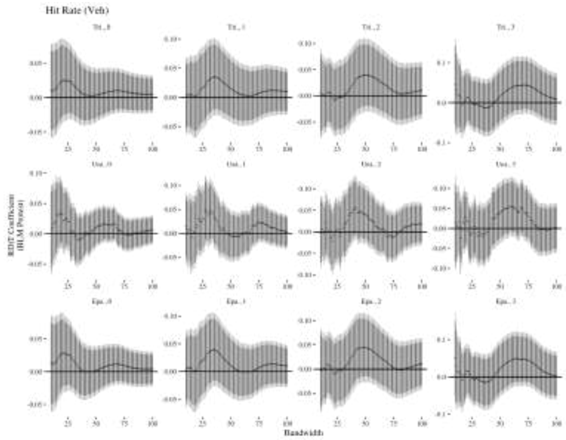


**Figure F19:** Philadelphia, Vehicle Hit Rates Outcome

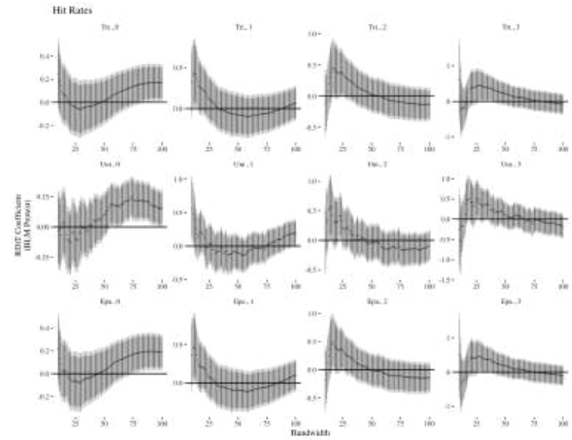


**Figure F20:** Seattle, Terry Stop Hit Rates Outcome

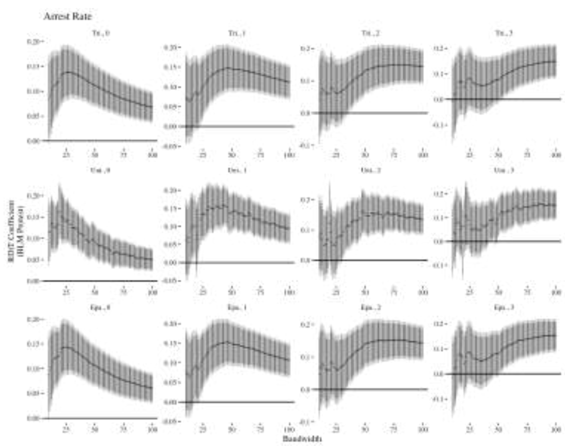


**Figure F21:** Austin, Vehicle Arrest Rates Outcome

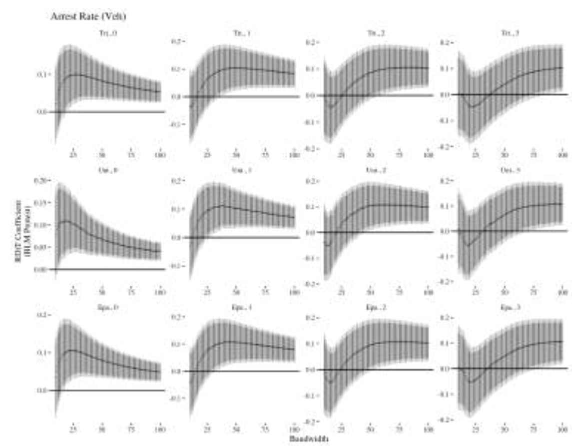


**Figure F22:** Los Angeles, Vehicle Arrest Rate Outcome

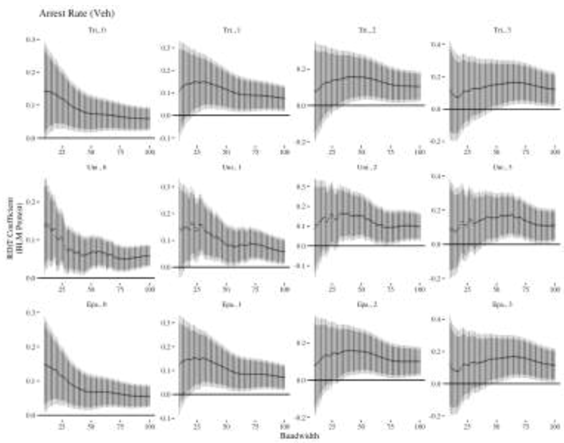

**Figure F23:** Philadelphia, Vehicle Arrest Hit Rates Outcome

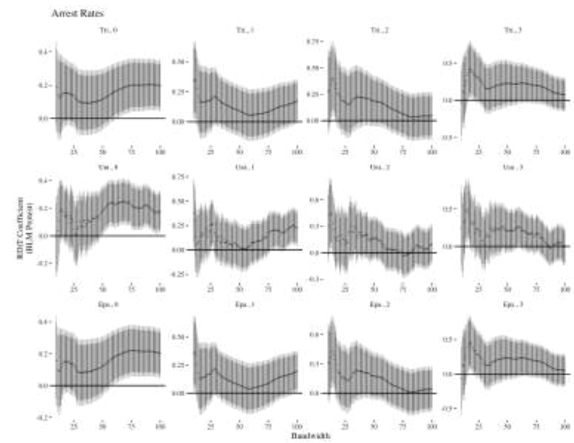

**Figure F24:** Seattle, Terry Stop Arrest Hit Rates Outcome

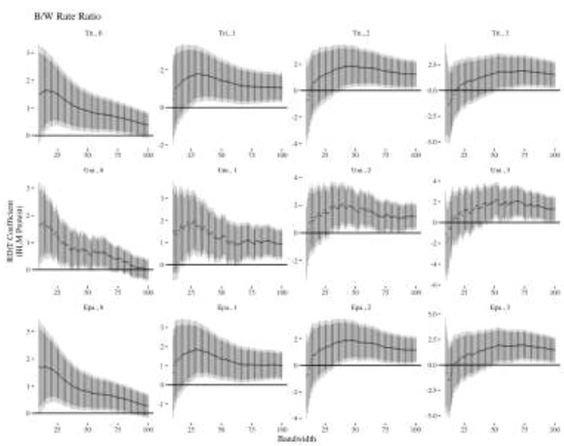

**Figure F25:** Austin, Black/white Vehicle Stop Rate Ratios Outcome

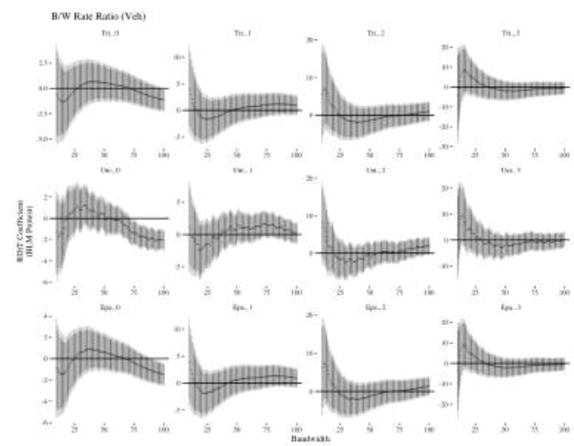

**Figure F26:** Los Angeles, Vehicle Stop Black/white Rate Ratio Outcome

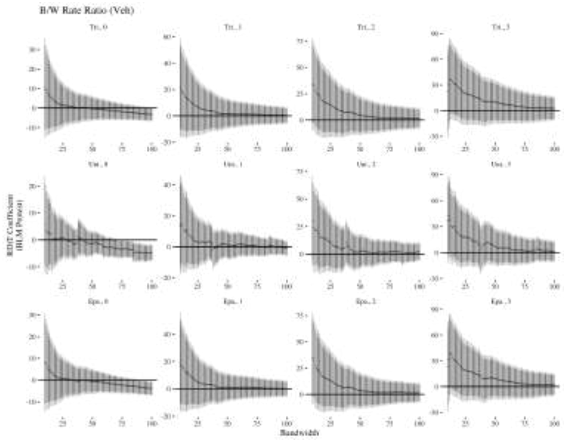


**Figure F27:** Philadelphia, Vehicle Stop Black/white Rate Ratio Outcome

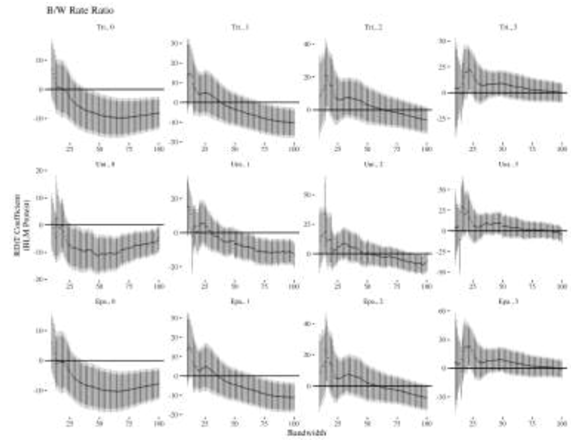


**Figure F28:** Seattle, Black/white Rate Ratio Outcome

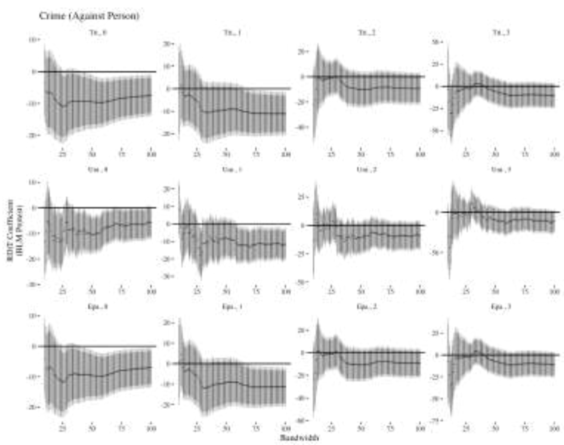


**Figure F29:** Austin, Crimes Against Person Outcome

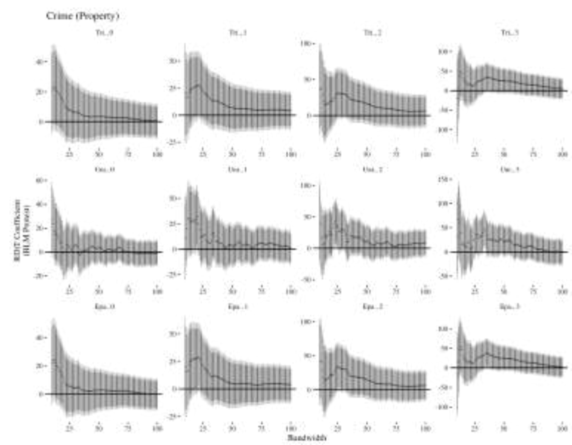


**Figure F30:** Austin, Crimes Against Property Outcome

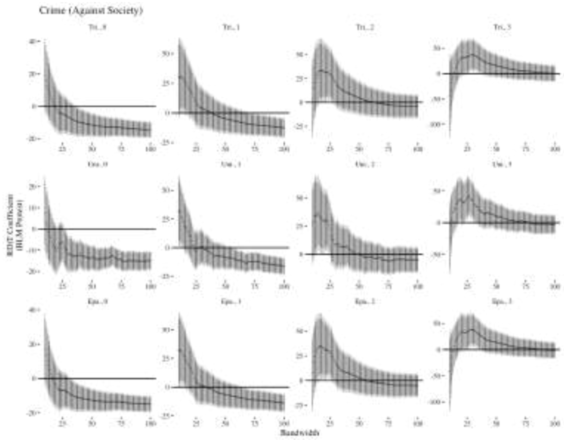


**Figure F31:** Austin, Crimes Against Society Outcome

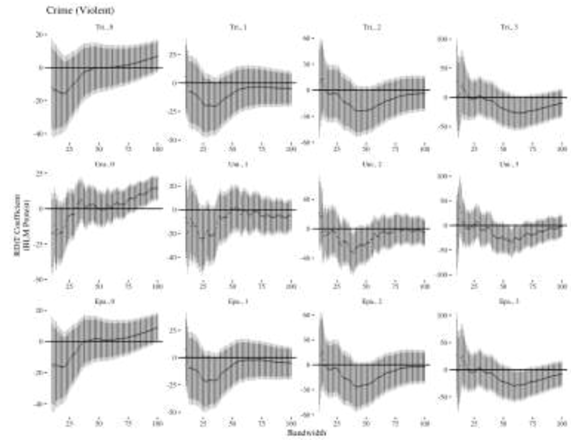


**Figure F32:** Los Angeles, Crimes Against Persons Outcome

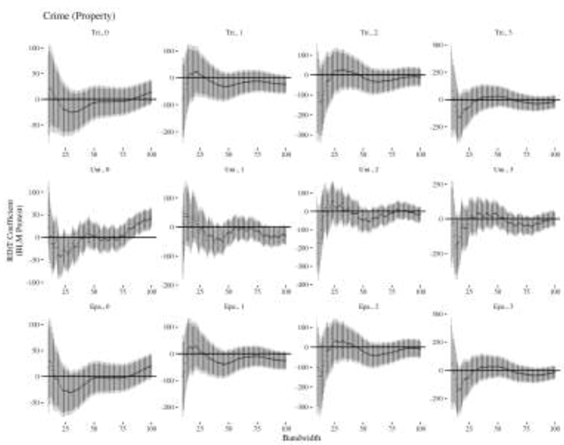


**Figure F33:** Los Angeles, Crimes Against Property Outcome

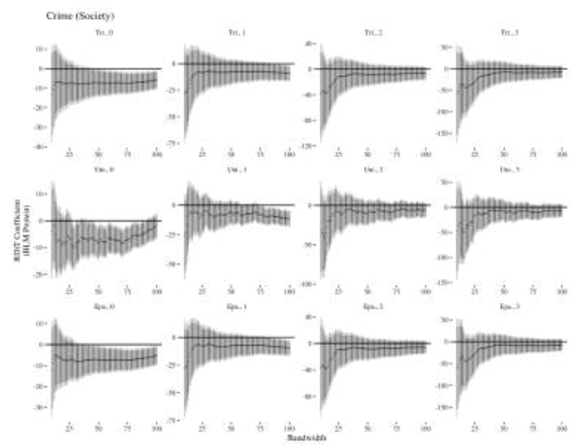


**Figure F34:** Los Angeles, Crimes Against Society Outcome

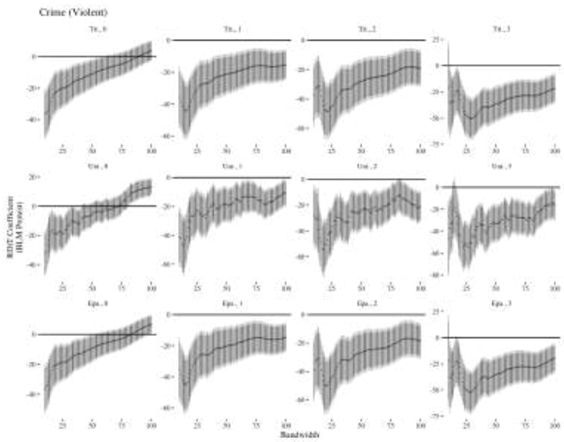


**Figure F35:** Philadelphia, Crimes Against Persons Outcome

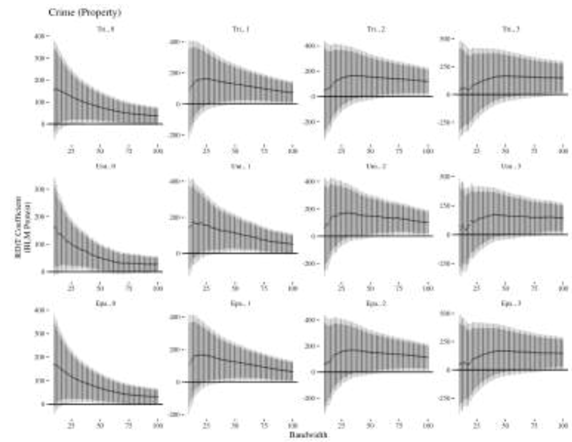


**Figure F36:** Philadelphia, Crimes Against Property Outcome

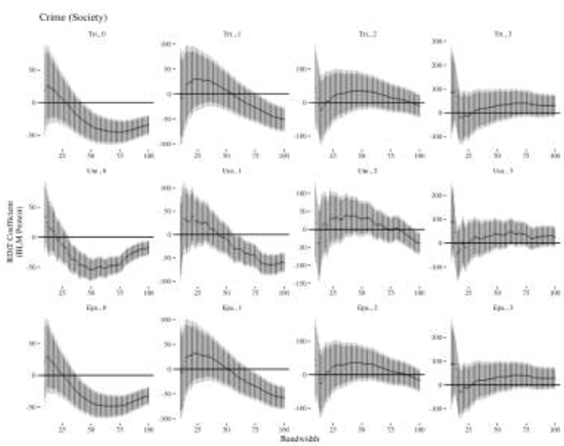


**Figure F37:** Philadelphia, Crimes Against Society Outcome

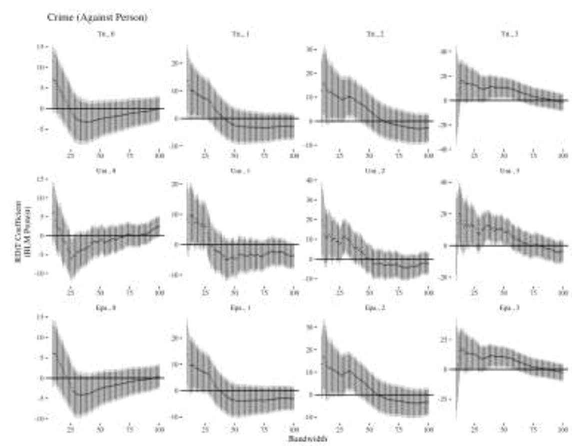


**Figure F38:** Seattle, Crimes Against Persons Outcome

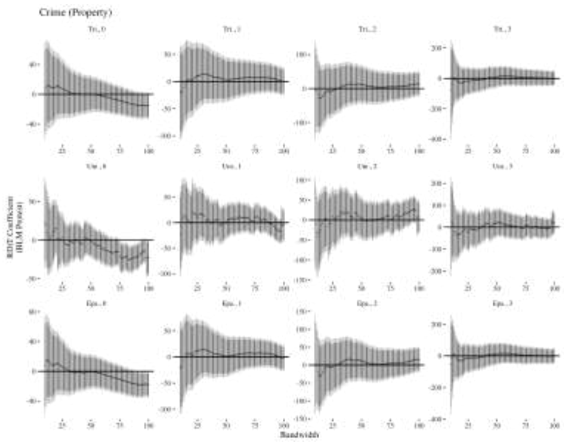


**Figure F39:** Seattle, Crimes Against Property Outcome

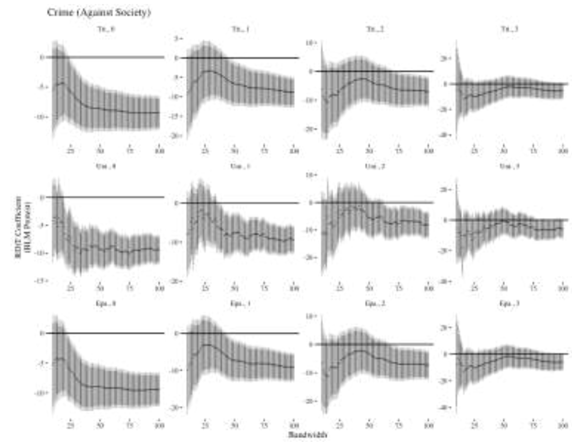


**Figure F40:** Seattle, Crimes Against Society Outcome

# G Long-Term Effects

Figures in this section assess the persistence of decreases in policing activity post-BLM protest. The X-axis is the number of days cut from the time series post-*BLM protest*. Y-axis is the unstandardized RDiT coefficient characterizing the discontinuous effect in the outcome between the time period immediately before the BLM protests and however many days after the onset of the BLM protests.

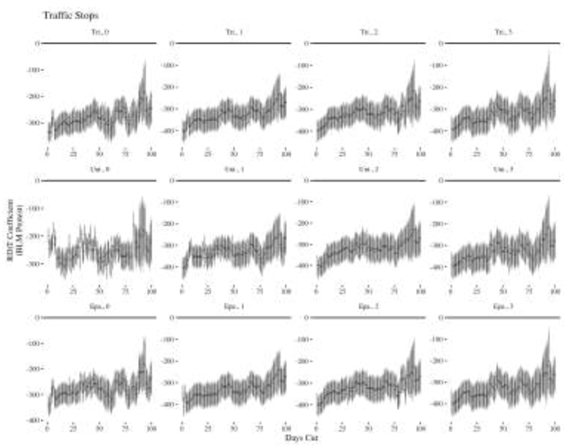


**Figure G41:** Austin, Traffic Stop Outcome

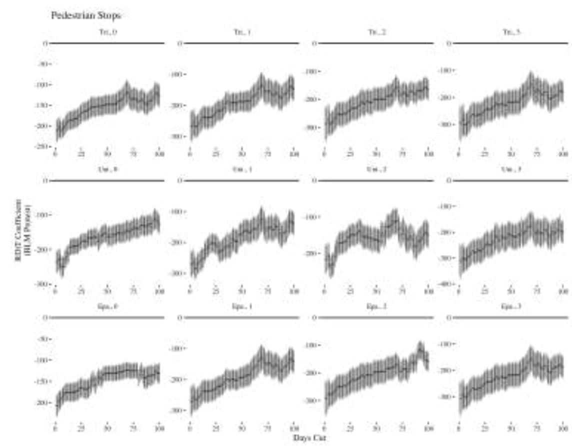


**Figure G42:** Los Angeles, Pedestrian Stop Outcome

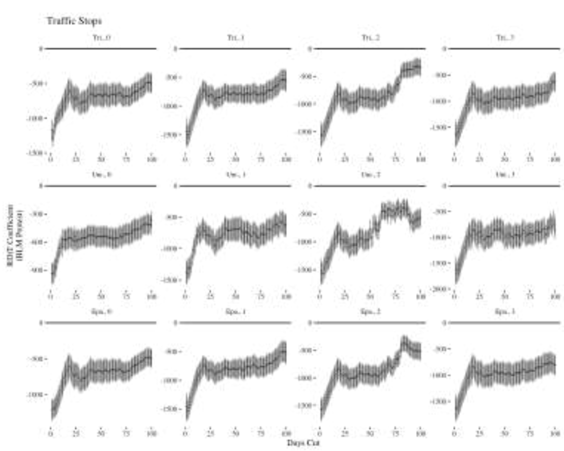


**Figure G43:** Los Angeles, Traffic Stop Outcome

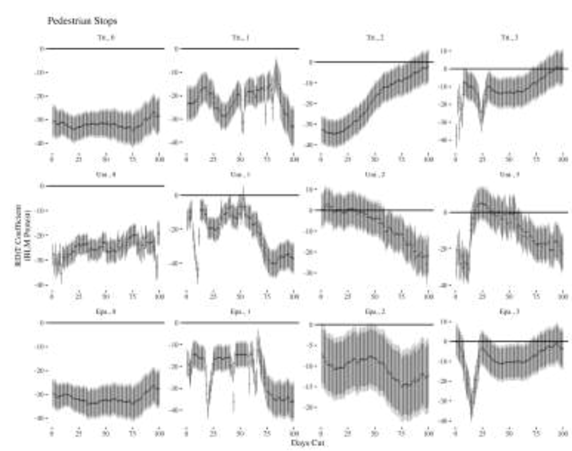


**Figure G44:** Philadelphia, Pedestrian Stop Outcome

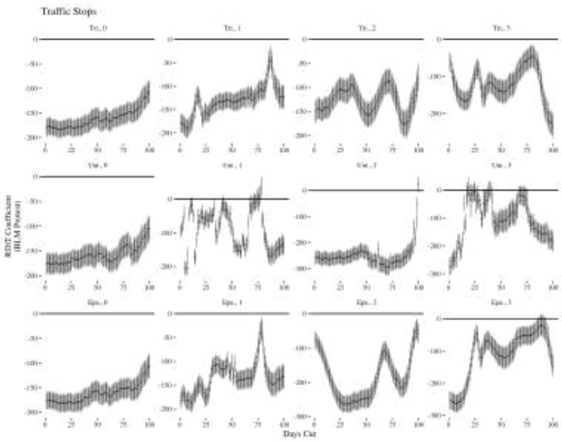

**Figure G45:** Philadelphia, Traffic Stop Outcome

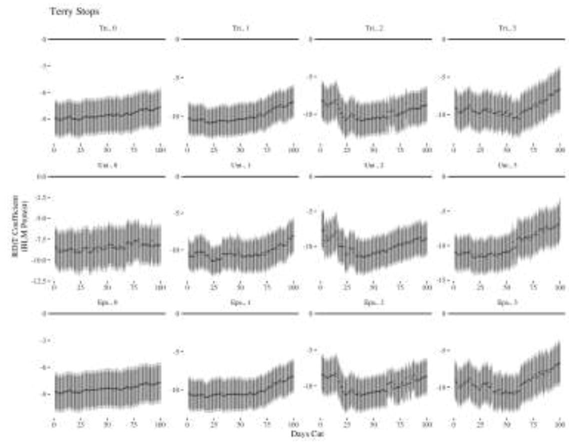

**Figure G46:** Seattle, Terry Stop Outcome

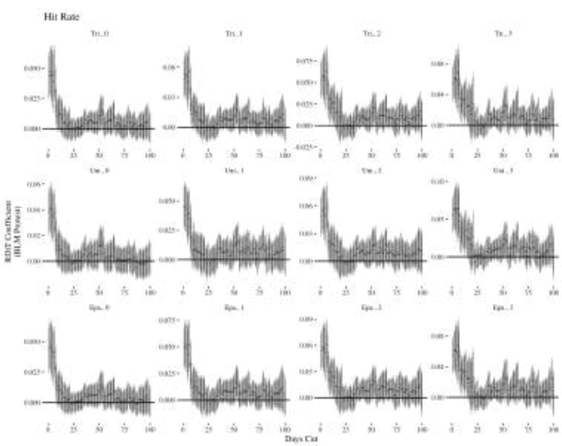

**Figure G47:** Austin, Hit Rate Outcome

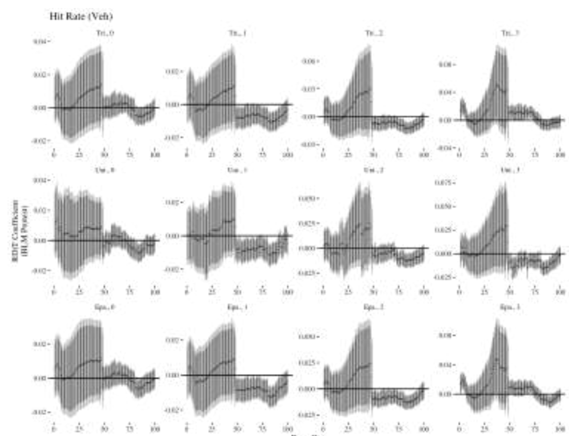

**Figure G48:** Los Angeles, Traffic Hit Rate Outcome

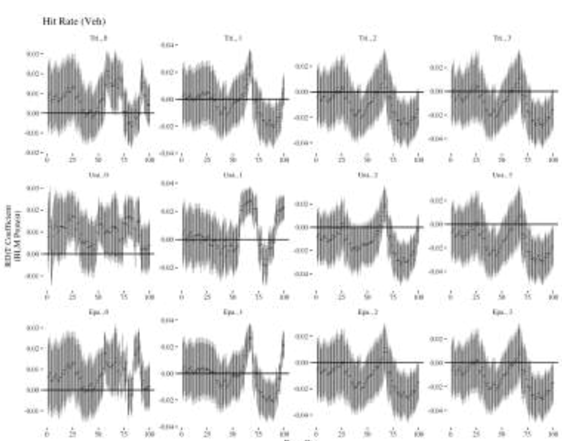

**Figure G49:** Philadelphia, Traffic Hit Rate Outcome

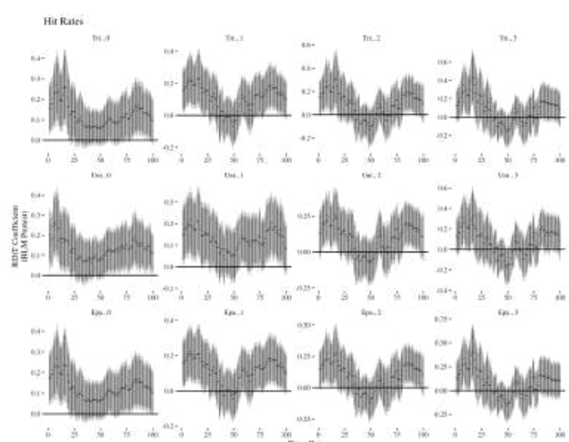

**Figure G50:** Seattle, Terry Hit Rate Outcome

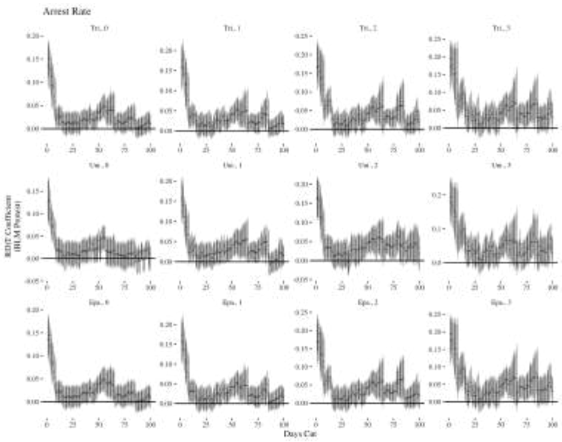

**Figure G51:** Austin, Arrest Rate Outcome

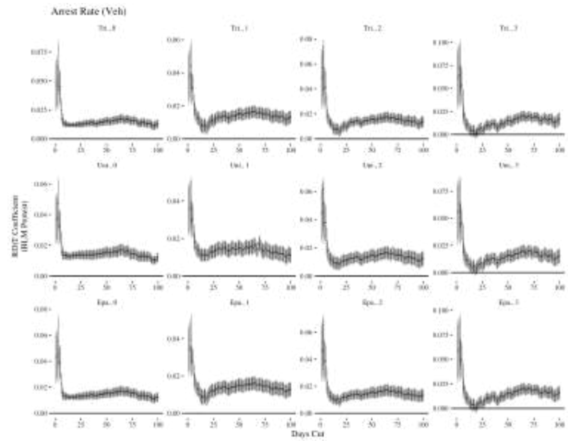

**Figure G52:** Los Angeles, Traffic Arrest Rate Outcome

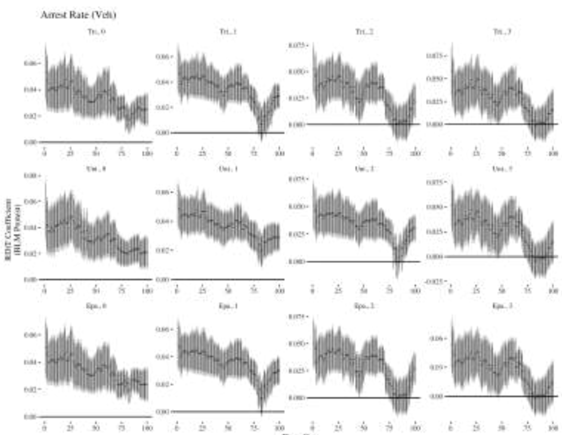

**Figure G53:** Philadelphia, Traffic Arrest Rate Outcome

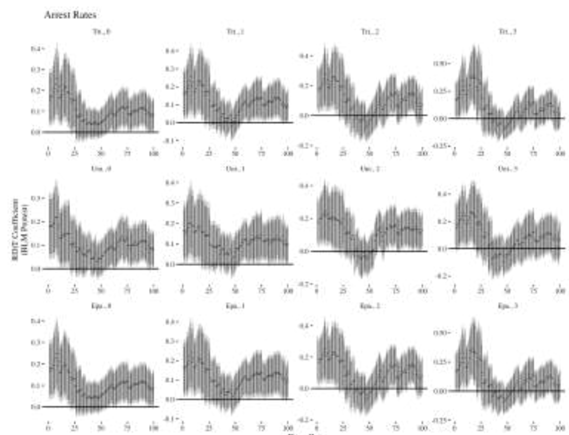

**Figure G54:** Seattle, Terry Arrest Rate Outcome

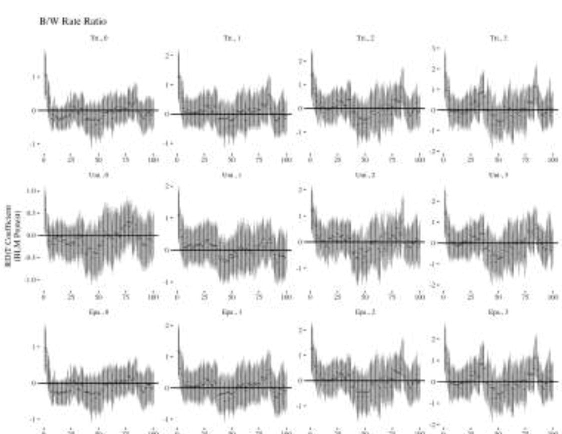

**Figure G55:** Austin, Black/white Traffic Stop Rate Ratio Outcome

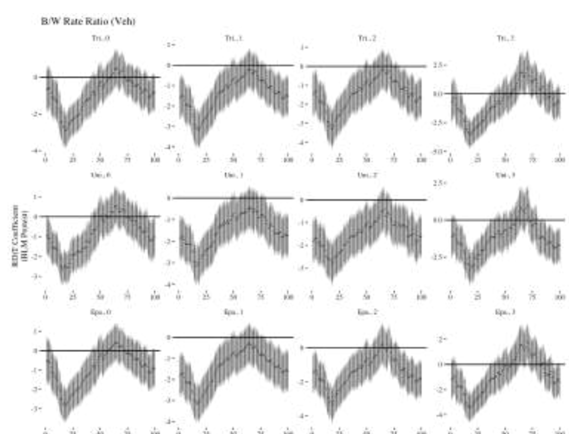

**Figure G56:** Los Angeles, Black/white Vehicle Stop Rate Ratio Outcome

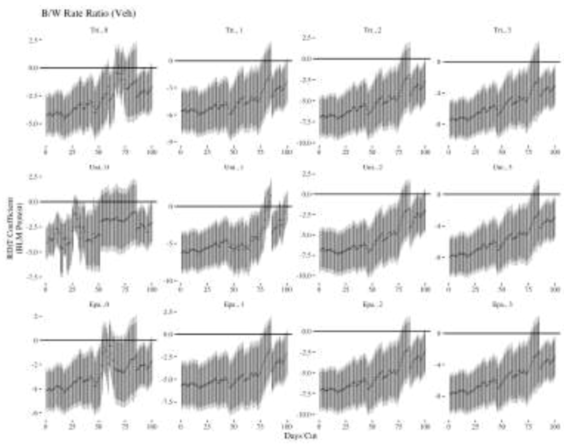

**Figure G57:** Philadelphia, Black/white Traffic Stop Rate Ratio Outcome

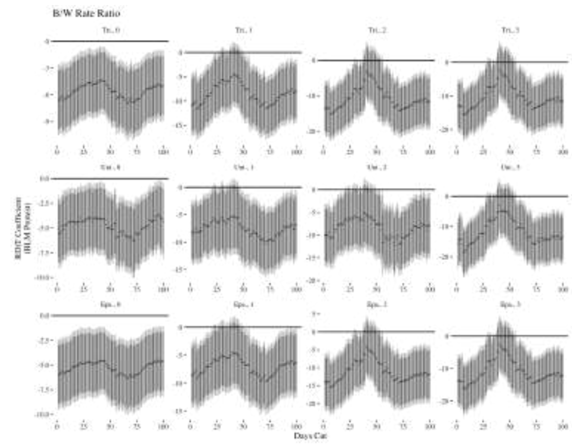

**Figure G58:** Seattle, Black/white Terry Stop Rate Ratio Outcome

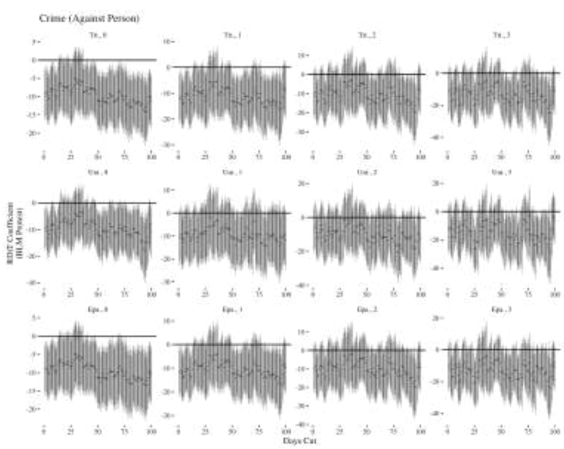

**Figure G59:** Austin, Crimes Against Persons Outcome

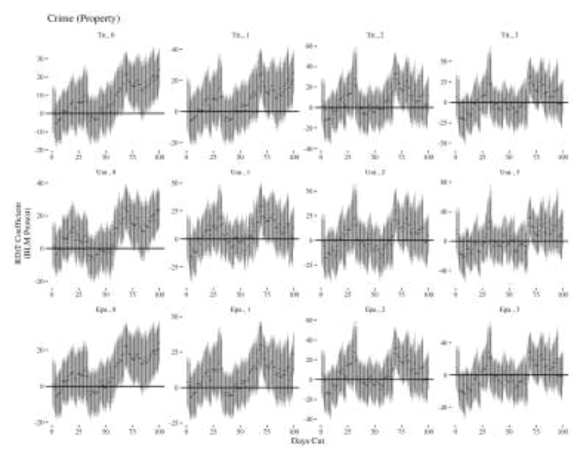

**Figure G60:** Austin, Crimes Against Property Outcome

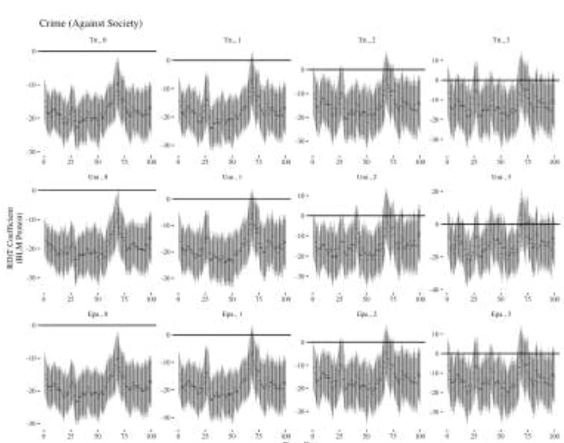

**Figure G61:** Austin, Crimes Against Society Outcome

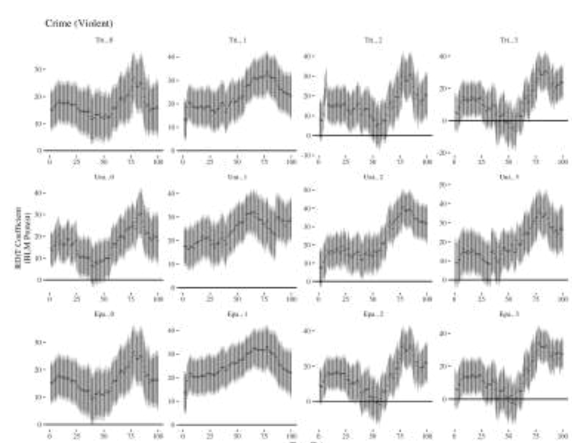

**Figure G62:** Los Angeles, Crimes Against Person Outcome

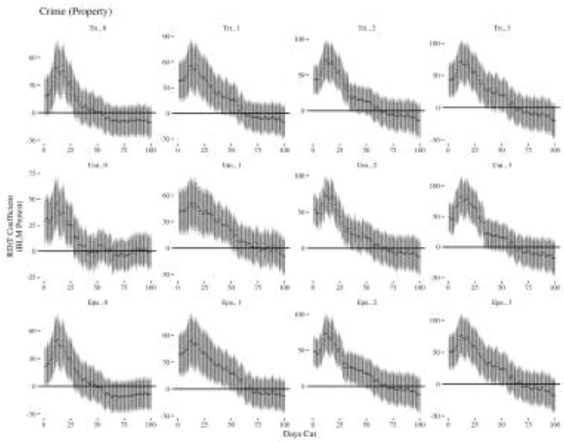

**Figure G63:** Los Angeles, Crimes Against Property Outcome

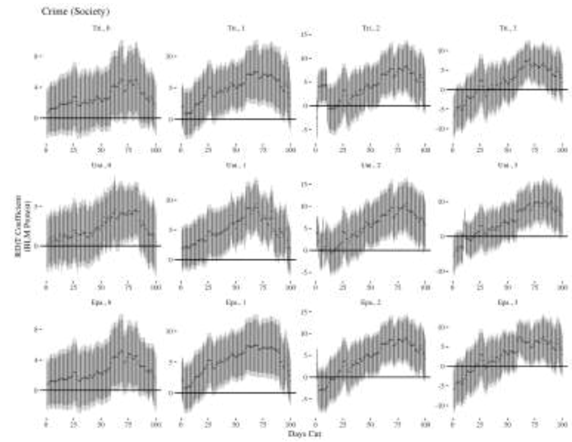

**Figure G64:** Los Angeles, Crimes Against Society Outcome

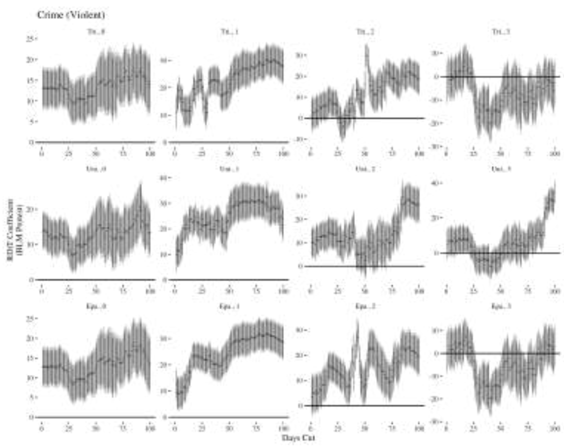

**Figure G65:** Philadelphia, Crimes Against Person Outcome

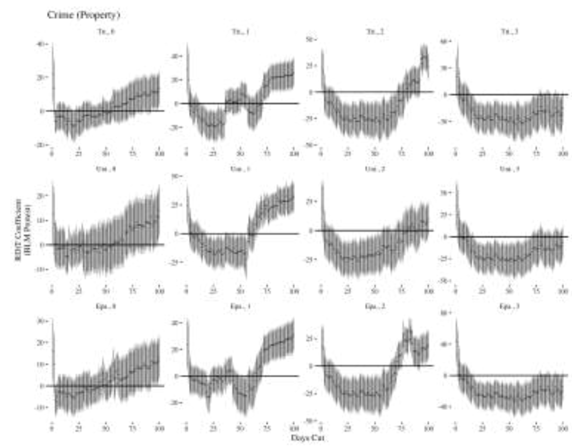

**Figure G66:** Philadelphia, Crimes Against Property Outcome

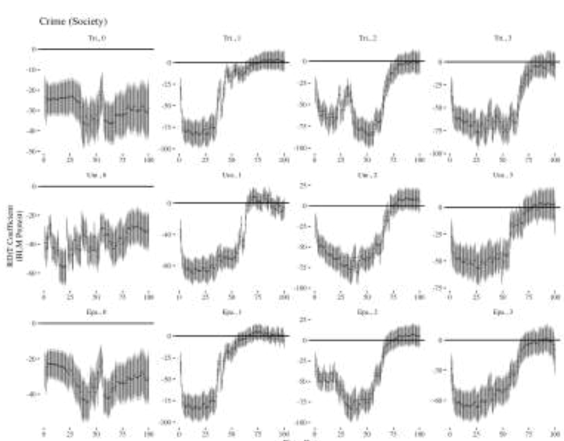

**Figure G67:** Philadelphia, Crimes Against Society Outcome

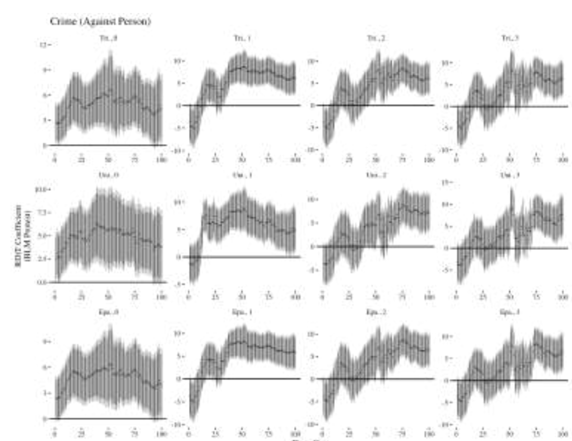

**Figure G68:** Seattle, Crimes Against Persons Outcome

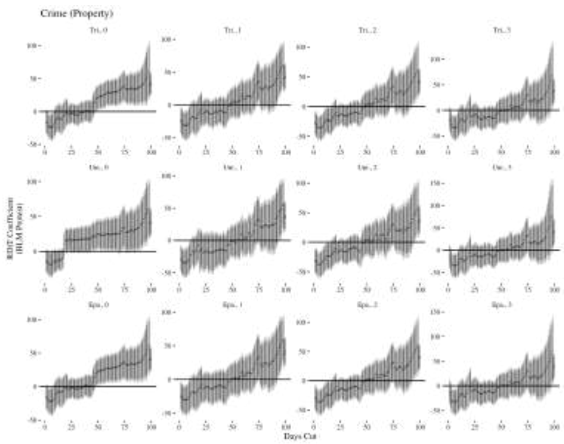


**Figure G69:** Seattle, Crimes Against Property Outcome

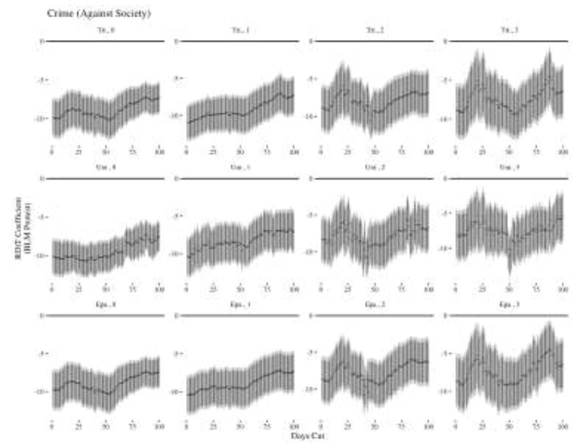


**Figure G70:** Seattle, Crimes Against Society Outcome

# H Seasonal Placebo Test

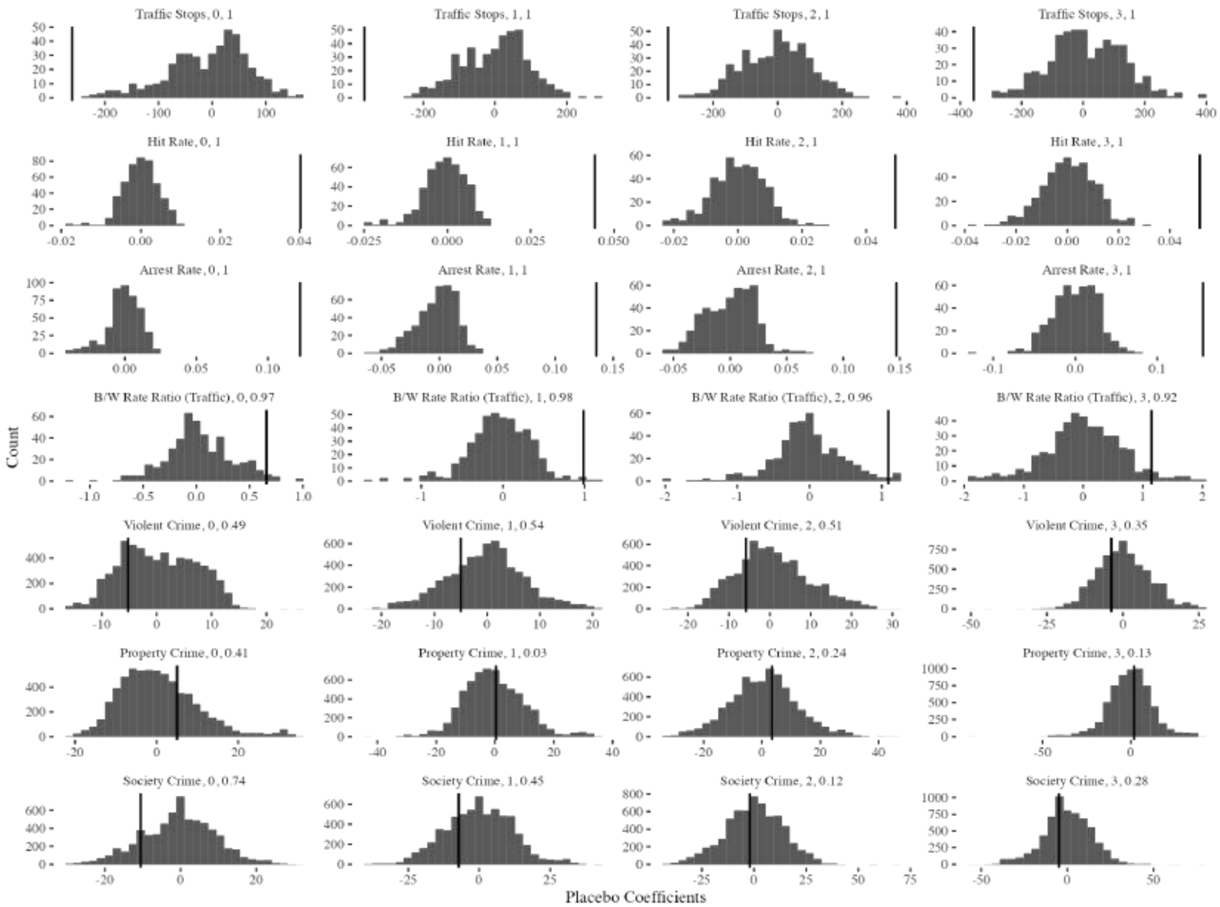


**Figure H71: Temporal Placebo Tests Comparing True Post-BLM Discontinuous Effect to Pre-BLM Discontinuous Effects (Austin).** The x-axis characterizes pre-BLM placebo coefficients. Solid vertical line denotes true post-BLM protest coefficient. Each facet denotes an outcome, polynomial degree, and the proportion of placebo coefficients (converted to absolute value) that the true coefficient (converted to absolute value) is larger than.

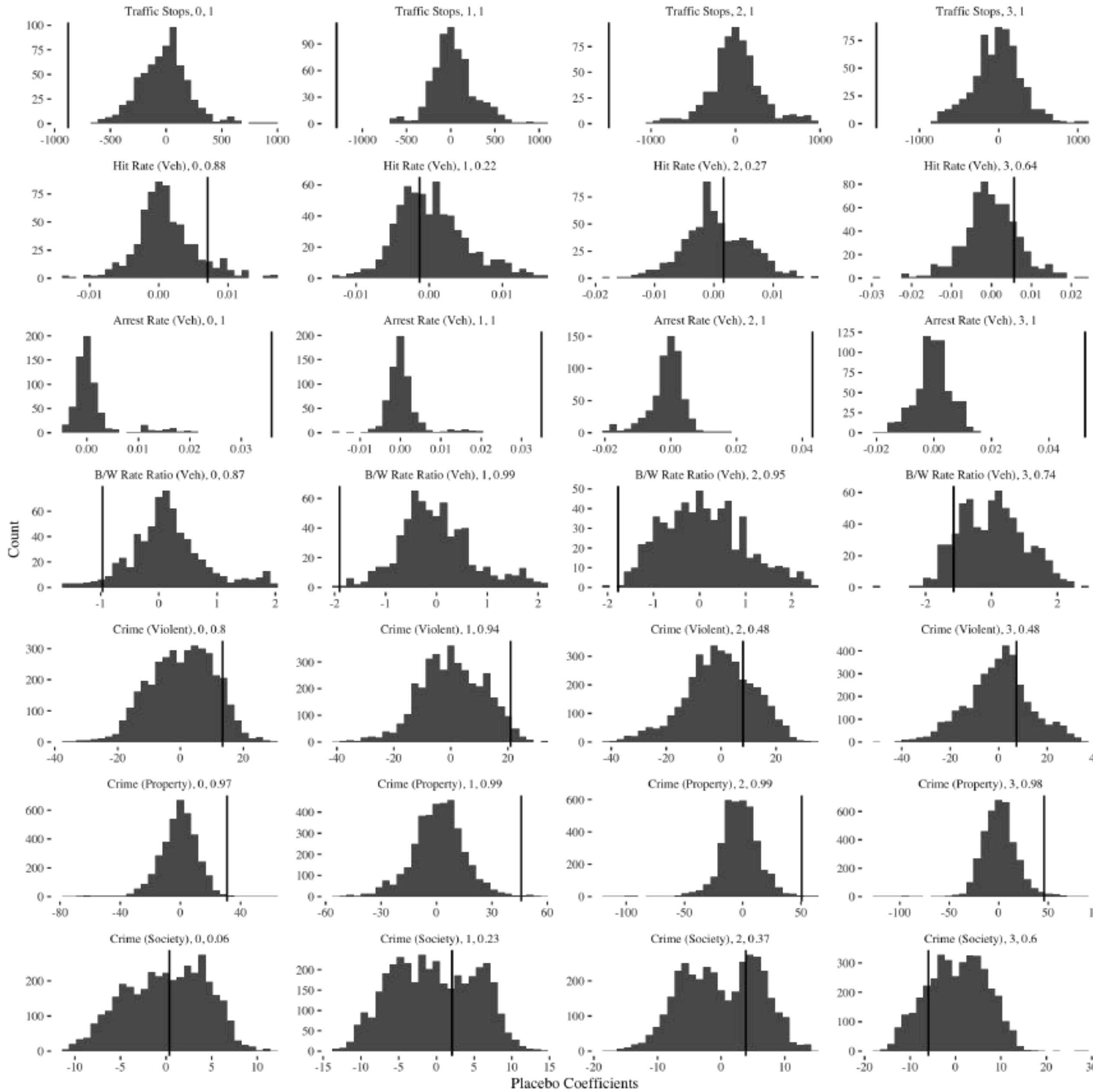

**Figure H72: Temporal Placebo Tests Comparing True Post-BLM Discontinuous Effect to Pre-BLM Discontinuous Effects (Los Angeles).** The x-axis characterizes pre-BLM placebo coefficients. Solid vertical line denotes true post-BLM protest coefficient. Each facet denotes an outcome, polynomial degree, and the proportion of placebo coefficients (converted to absolute value) that the true coefficient (converted to absolute value) is larger than.

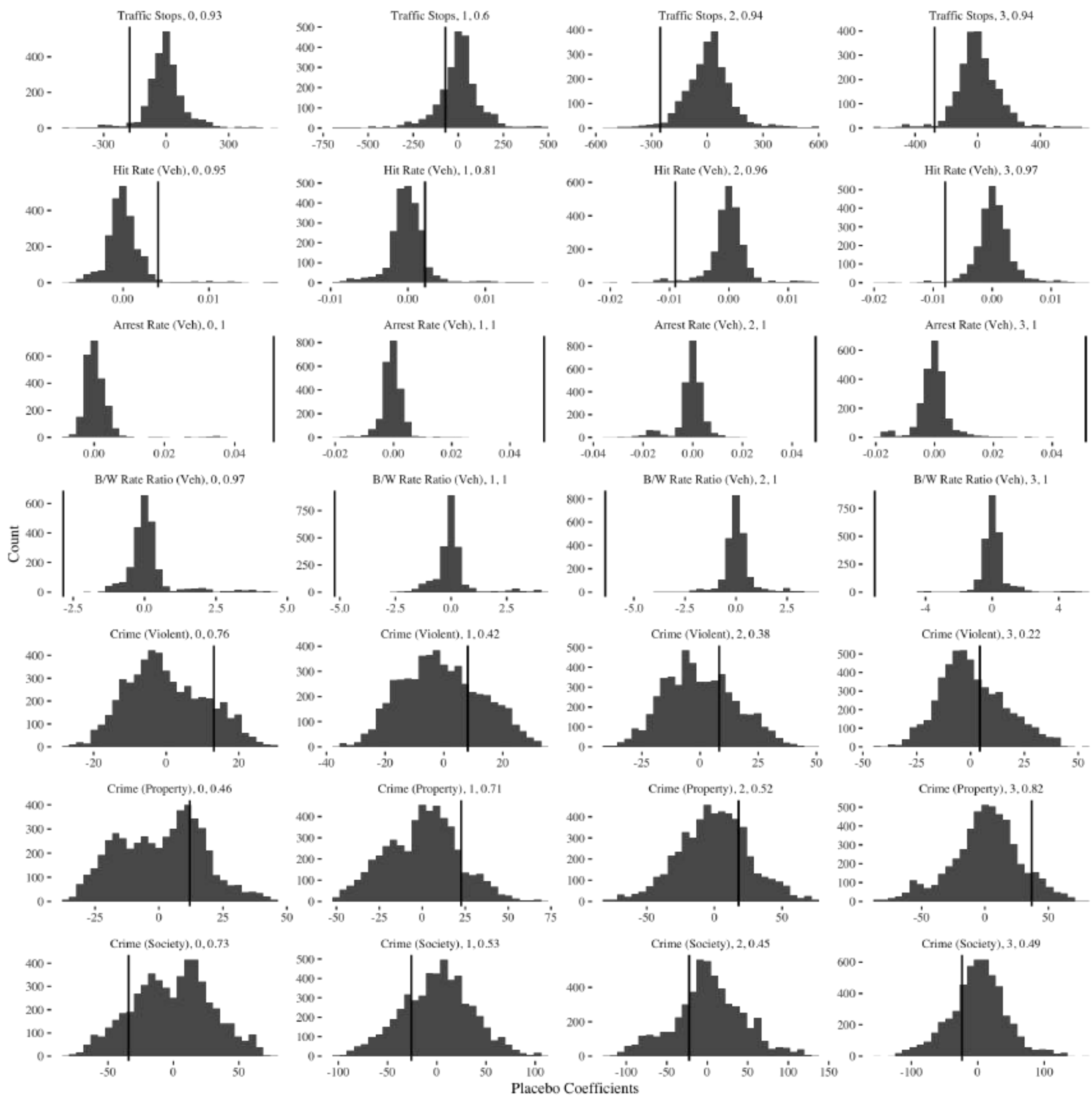


**Figure H73: Temporal Placebo Tests Comparing True Post-BLM Discontinuous Effect to Pre-BLM Discontinuous Effects (Philadelphia).** The x-axis characterizes pre-BLM placebo coefficients. Solid vertical line denotes true post-BLM protest coefficient. Each facet denotes an outcome, polynomial degree, and the proportion of placebo coefficients (converted to absolute value) that the true coefficient (converted to absolute value) is larger than.

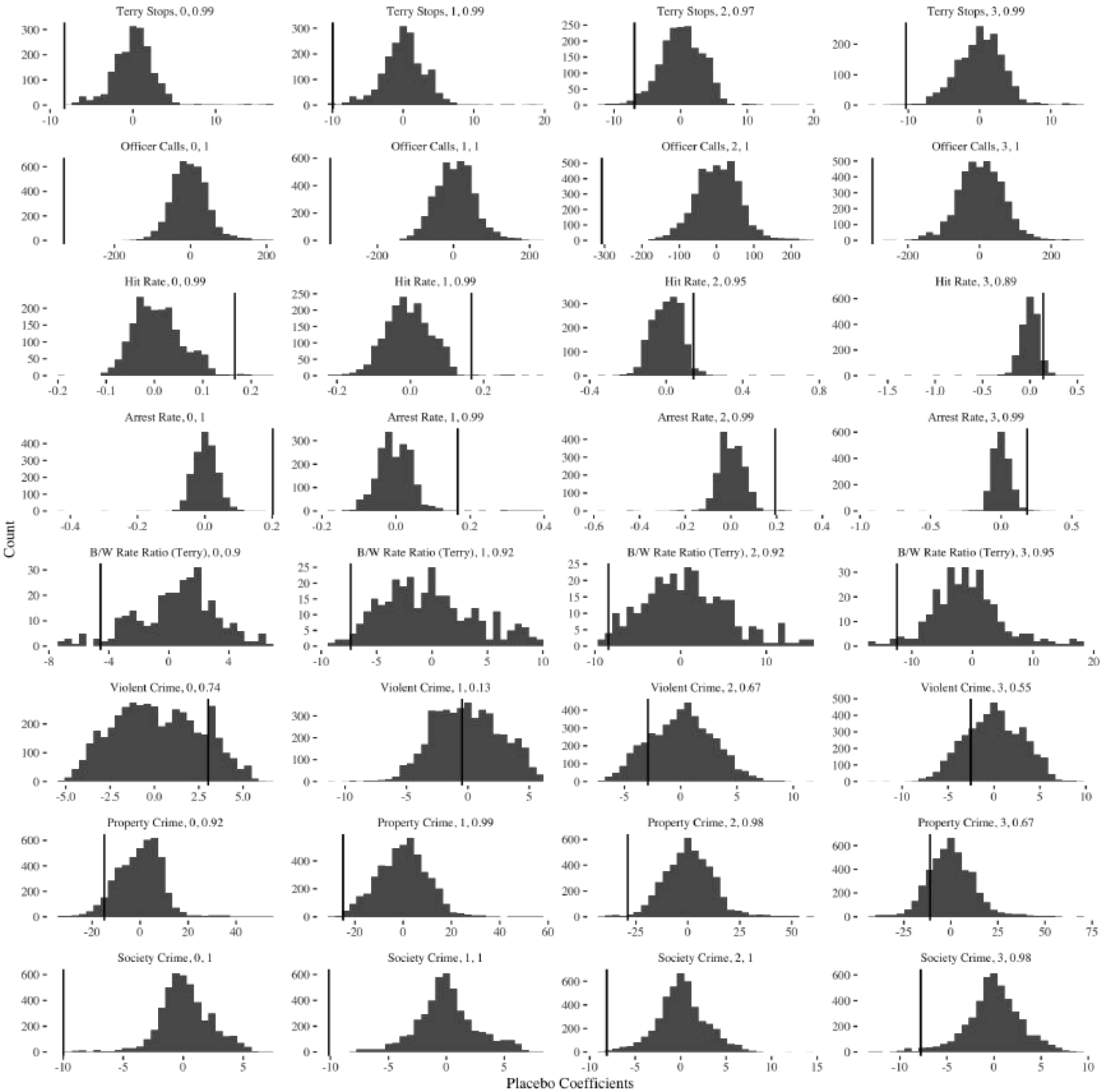


**Figure H74: Temporal Placebo Tests Comparing True Post-BLM Discontinuous Effect to Pre-BLM Discontinuous Effects (Seattle).** The x-axis characterizes pre-BLM placebo coefficients. Solid vertical line denotes true post-BLM protest coefficient. Each facet denotes an outcome, polynomial degree, and the proportion of placebo coefficients (converted to absolute value) that the true coefficient (converted to absolute value) is larger than.

# I San Diego Replication

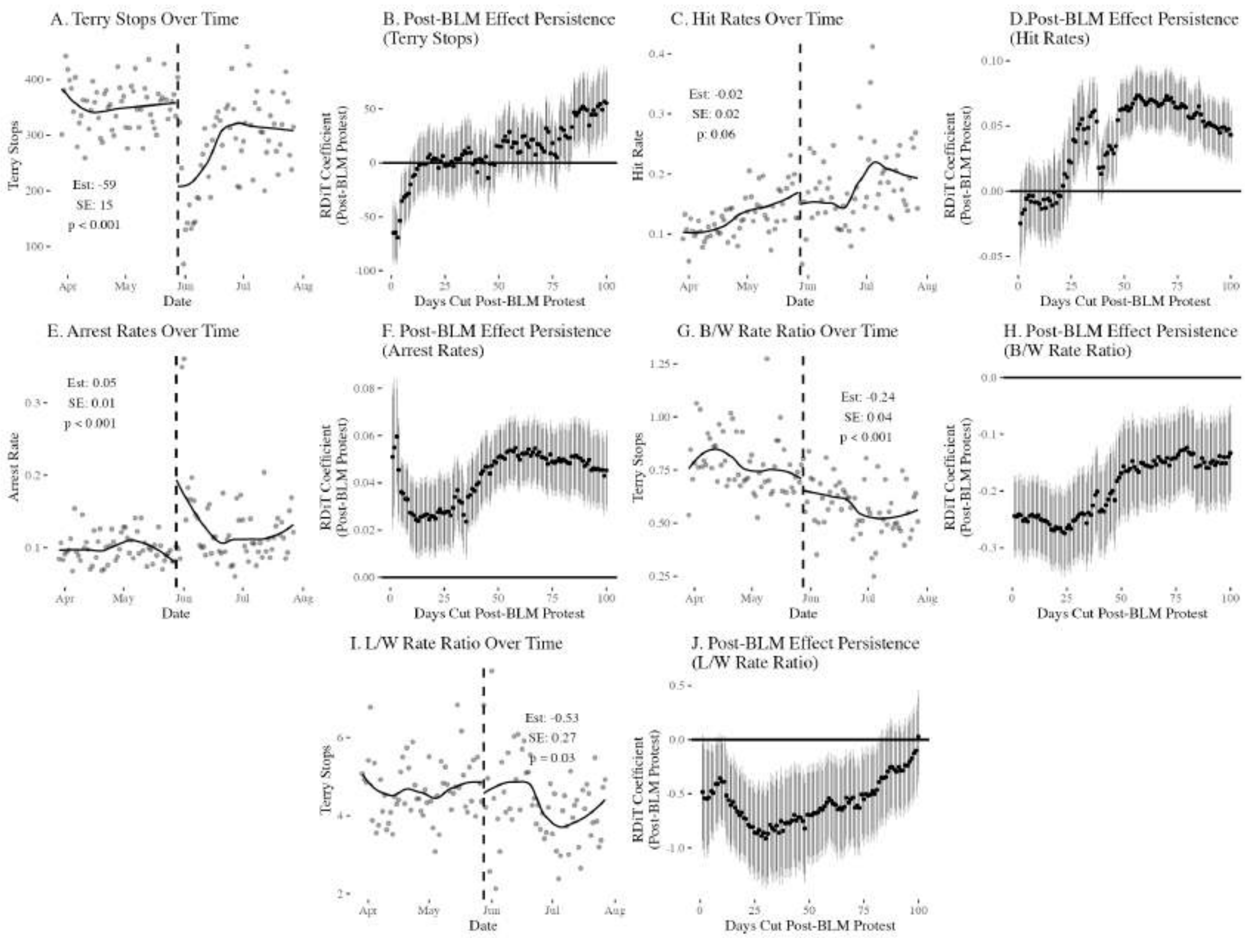

**Figure I75: San Diego Replication.** Panels A, C, E, and G characterize daily terry stops, hit rates, arrest rates, and reported violent crime from 911 calls (y-axis) over time (x-axis). Annotations for Panels A, C, E, and G are RDiT estimates characterizing the discontinuous post-*BLM protest* coefficient for the respective outcomes (linear polynomial, uniform kernel, mean-squared optimal bandwidth selection by Calonico, Cattaneo, and Titiunik (2015)). Panels B, D, F, and H characterize RDiT post-*BLM protest* coefficient after cutting 1-100 days post-*BLM protest* (but keeping days after intact). 95% CIs displayed, robust SEs reported.

Figure I75 characterizes the results from analyzing San Diego stop and 911 call data to test *Hypotheses 1-2*. To test *Hypothesis 1-2*, we collect San Diego Police Department (SDPD) terry stop data from San Diego's open data website.[41] With these data, we generate daily-

[41] https://data.sandiego.gov/datasets/?department=police

level measures of the count of terry stops, hit rates,[42] arrest rates, black/white rate ratios, and Latino/white rate ratios. The San Diego data are aggregated at the stop/person-level. This is because multiple people may be involved in a given stop (e.g. a police officer stopping a group of 3 people). Therefore, the daily time series of terry stops is aggregated from stop-level data whereas the the daily time series of hit rates, arrest rates, and the rate ratios are aggregated from person/stop-level data.

Despite being a Republican-controlled city at the time of the 2020 BLM protests, our results in San Diego are largely consistent with our main results derived from Democrat-controlled cities, with some minor exceptions.

First, consistent with *Hypothesis 1*, SDPD terry stops discontinuously decrease post-*BLM protest*. Descriptively, this is clear in Figure I75, Panel A. RDiT estimates also suggest the *BLM protest* discontinously decreased the number of terry stops by 59, 70% of the pre-*BLM protest* outcome standard deviation (see annotation on Figure I75, Panel A). However, unlike our main results, the *BLM protest* does not appear to persistently reduce SDPD terry stops. After cutting 1-100 days immediately post-*BLM protest* from the data (but leaving data from the days after intact), the decrease in terry stops reverts to the pre-*BLM protest* equilibrium within 15 days (Figure I75, Panel B).

Second, consistent with our main results, we find mixed evidence for *Hypothesis 2a* and *2b*. On the one hand, consistent with *Hypothesis 2b*, there is no discontinuous increase in the hit rate post-*BLM protest* (Figure I75, Panel C). However, consistent with *Hypothesis 2a*, there is a discontinuous increase in the arrest rate (Figure I75, Panel E). RDiT estimates suggest the *BLM protest* increased the arrest rate by 5 percentage points, equivalent to 150% of the pre-*BLM protest* outcome standard deviation. The increase in arrest rates post-*BLM protest* was persistent, suggesting the post-*BLM protest* coefficient is not driven by dynamics intrinsic to the behavior of protesters. The discontinuous increase in arrest rates post-*BLM protest* remained after cutting 1-100 days immediately post-*BLM protest* from the data Figure I75, Panel F).

Third, consistent with our main results, we find additional support for *Hypothesis 2a* analyzing racial disparities in SDPD terry stops. The Black/white and Latino/white stop rate ratios discontinuously decrease post-*BLM protest* (Figure I75, Panels G and I). RDiT estimates suggest the *BLM protest* discontinuously decreased the Black/white and Latino/white stop rate ratios by 0.24 and 0.53 respectively, equivalent to 180% and 71% of the pre-*BLM protest* outcome standard deviations. These decreases were persistent, again suggesting the post-*BLM protest* coefficient is not driven by dynamics intrinsic to the behavior of protestors. The discontinuous decrease in Black/white stop rate ratios remained after cutting 1-100 days immediately post-*BLM protest* from the data Figure I75, Panel H). Likewise, the discontinuous decrease in Latino/white stop rate ratios remained up to 80 days immediately post-*BLM protest* (Figure I75, Panel J).

In summary, like our main results, there is evidence consistent with *Hypothesis 1*, but mixed evidence consistent with *Hypothesis 2a* and *Hypothesis 2b*. On balance, there is a decrease in policing activity, but only briefly. Concomitantly, there is no discontinuous shift

[42] We use the SDPD definition of contraband to measure hit rates conditional on a terry stop. SDPD defines contraband as alcohol, ammunition, cellphones/electronic devices, drug paraphernalia, drugs/narcotics, firearms, money, stolen property, non-firearm weapons (see `https://www.sandiego.gov/sites/default/files/sdpd-ripa-presentation-220124.pdf`).

in hit rates, but there is a discontinuous (and persistent) increase in arrest rates in addition to a discontinuous (and persistent) decrease in racial disparities.

# J Balance Tests

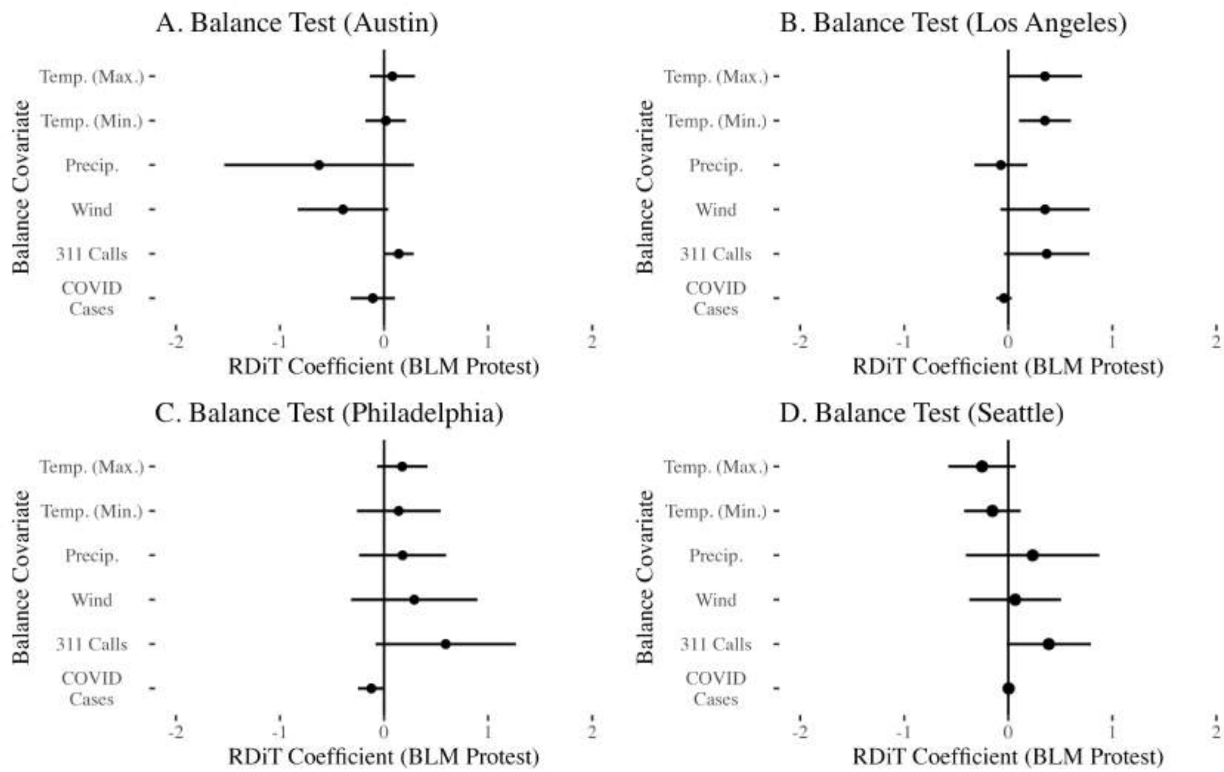


**Figure J76: Balance tests.** Y-axis is the balance covariate, x-axis is the standardized *BLM protest* RDiT coefficient. Daily data on temperature, precipitation, and wind speed for each city (between March 2019 and December 2021) are from the National Oceanic and Atmospheric Administration (NOAA). Daily data on 311 calls are from each city's respective open data websites. Daily data on COVID cases in Austin are from the Texas Department of State Health Services (see: https://www.dshs.texas.gov/texas-respiratory-virus-surveillance-report). Daily data on COVID cases in Los Angeles are from the LA County Department of Public Health (see: http://publichealth.lacounty.gov/media/coronavirus/data/). Daily data on COVID cases in Philadelphia are from the Philadelphia open data website (see: https://opendataphilly.org/datasets/covid-tests-and-cases/). Daily data on COVID cases in Seattle are from the Seattle open data website (see: https://kingcounty.gov/en/dept/dph/health-safety/disease-illness/covid-19/data). 95% CIs displayed from robust SEs.

## J.1 Re-Estimation With Control Covariates

### J.1.1 Depolicing

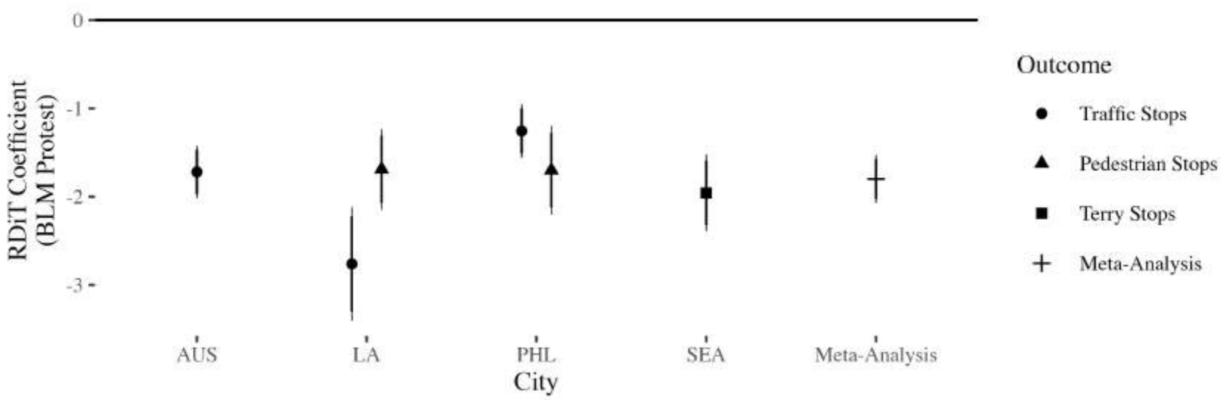


**Figure J77: Standardized RDiT Coefficients Characterizing Effect of BLM Protests (y-axis) on Policing Activity Across Cities (x-axis) With Control Covariate Adjustment.** Shape denotes outcome type across the cities. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. Study-adjusted random effects meta-analytic coefficient on display. 95% CIs displayed derived from robust SEs. All models adjust for daily temperature (minimum, maximum), precipitation, wind speed, 311 calls, and COVID cases.

### J.1.2 Arrest Rates

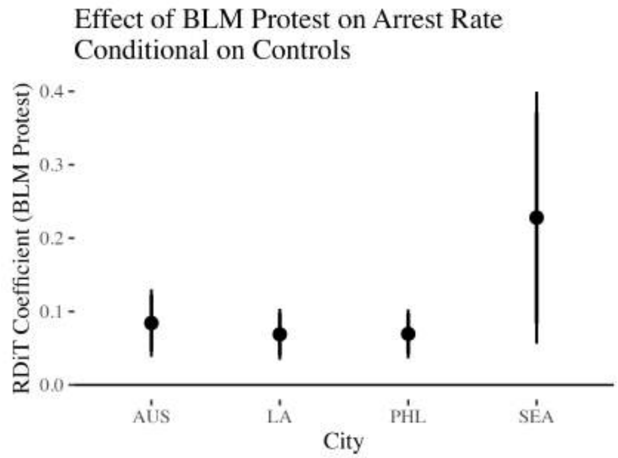


**Figure J78: Standardized RDiT Coefficients Characterizing Effect of BLM Protests (y-axis) on Arrest Rates in Austin, Los Angeles, Philadelphia and Seattle (x-axis) With Control Covariate Adjustment.** All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. 95% CIs displayed derived from robust SEs. All models adjust for daily temperature (minimum, maximum), precipitation, wind speed, 311 calls, and COVID cases.

### J.1.3 Rate Ratios

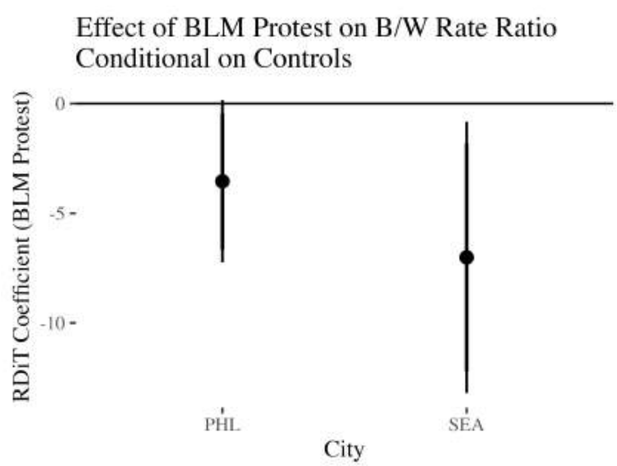


**Figure J79: Standardized RDiT Coefficients Characterizing Effect of BLM Protests (y-axis) on Black/White Rate Ratios in Philadelphia and Seattle (x-axis) With Control Covariate Adjustment.** All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. 95% CIs displayed derived from robust SEs. All models adjust for daily temperature (minimum, maximum), precipitation, wind speed, 311 calls, and COVID cases.

#### J.1.4 Crime

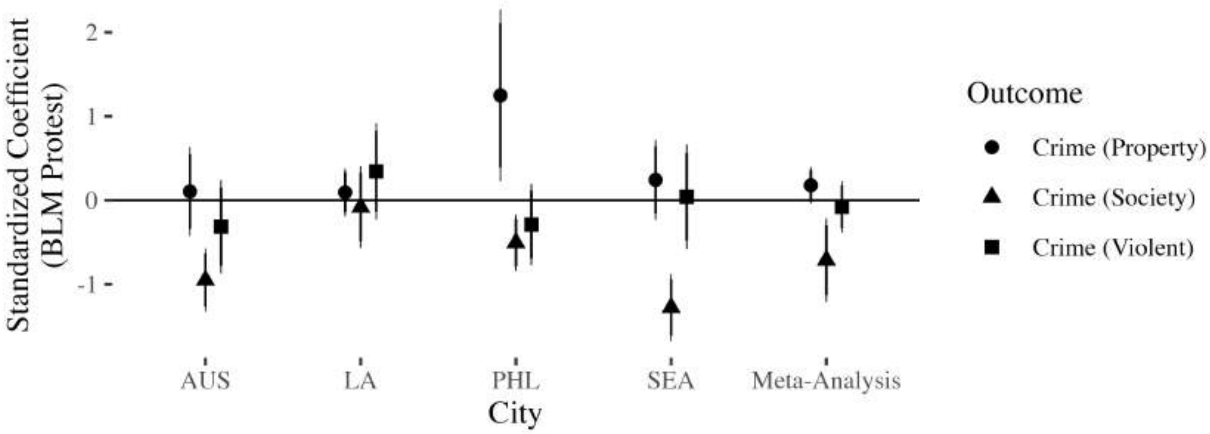


**Figure J80: RDiT Estimates Characterizing Standardized Effect (y-axis) of *BLM Protests* on Crime Across Cities (x-axis) With Covariate Adjustment.** Shape denotes outcome type. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. Study-adjusted random effects meta-analytic coefficient on display. 95% CIs displayed derived from robust SEs.

# K Demonstrating Hit Rates Correlated With Each Other

### Table K10: Different types of hit rates are correlated with each other (Los Angeles)

| | Substances (Mean) (1) | Evidence (Mean) (2) | Evidence (Mean) (3) | Substances (Sum) (4) | Evidence (Sum) (5) | Evidence (Sum) (6) |
|---|---|---|---|---|---|---|
| Weapons (Mean) | $0.60^{***}$ | $0.19^{***}$ | | | | |
| | (0.04) | (0.03) | | | | |
| Substances (Mean) | | | $0.16^{***}$ | | | |
| | | | (0.02) | | | |
| Weapons (Sum) | | | | $0.41^{***}$ | $0.30^{***}$ | |
| | | | | (0.02) | (0.03) | |
| Substances (Sum) | | | | | | $0.42^{***}$ |
| | | | | | | (0.03) |
| $R^2$ | 0.26 | 0.11 | 0.11 | 0.20 | 0.08 | 0.14 |
| N | 1694 | 1694 | 1694 | 1694 | 1694 | 1694 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Weapons (mean) is the probability that a weapon, firearm, or ammunition is identified during a stop. Substances (mean) is the probability alcohol, drugs, or drug paraphernalia is identified during a stop. Evidence (mean) is the probability electronic devices, money, stolen property, or other contraband are identified during a stop. The (sum) outcomes are the count of weapons, substances, and evidence-based contraband recovered.

### Table K11: Different types of hit rates are correlated with each other (Austin)

| | Cash (Mean) (1) | Weapons (Mean) (2) | Weapons (Mean) (3) | Cash (Sum) (4) | Weapons (Sum) (5) | Weapons (Sum) (6) |
|---|---|---|---|---|---|---|
| Substances (Mean) | $0.26^{***}$ | $0.12^{*}$ | | | | |
| | (0.07) | (0.05) | | | | |
| Cash (Mean) | | | 0.01 | | | |
| | | | (0.05) | | | |
| Substances (Sum) | | | | $0.17^{***}$ | $0.12^{***}$ | |
| | | | | (0.05) | (0.03) | |
| Cash (Sum) | | | | | | 0.01 |
| | | | | | | (0.03) |
| $R^2$ | 0.07 | 0.04 | 0.03 | 0.04 | 0.04 | 0.02 |
| Num. obs. | 731 | 731 | 731 | 731 | 731 | 731 |

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

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Substances (mean) is the daily probability that drugs or alcohol are identified during a stop. Cash (mean) is the daily probability that cash is identified during a stop. Weapons (mean) is the daily probability that weapons are identified during a stop. The (sum) outcomes are the daily count of substances, cash, and weapons contraband recovered.

# L Meta-Analysis Justification

Given we analyze the effect of the *BLM protests* on policing activity and crime across four independent cities, we also estimate and present a Hartung-Knapp random effects meta-analytic estimate averaging the *BLM protest* coefficients across the four cities[43] with respect to each outcome of interest.[44] An advantage of our analyses is that we test our hypotheses across four independent police departments whereas prior research often analyzes police behavior with incident-level data in a single city (Ratcliffe and Taylor, 2023; Nix, Huff, et al., 2024). Although single city analyses are valuable, a shortcoming of a single city analysis is that it does not account for the idiosyncratic and localized nature of police departments across the U.S. (Sinclair, Love, and Gutiérrez-Vera, 2021), which may moderate statistical relationships of interest. Therefore, analyzing four cities in addition to a meta-analytic approach may teach us more about the average effect of BLM protests across a diverse set of cities. If coefficients across the four cities are substantively similar with a consistent meta-analytic estimate, we can be more confident city-level idiosyncrasies are not as relevant in explaining our results or conclusions. If coefficients across the four cities are substantively distinct and varied, then the meta-analytic estimate may give us a sense of which effect direction (which could also be statistically null) is dominant on average.

However, we note there are still limitations to our meta-analytic approach. Although evaluating the effects of the BLM protests in four cities is better than one, our meta-analytic conclusions are still confined to what we can observe in the four cities we analyze. Indeed, the reason we can effectively analyze these four cities across our outcomes of interest is because they have made certain data available, but factors correlated with data availability may also moderate our effects (Cook and Fortunato, 2023). For instance, more transparent police departments may also be the types of departments to increase police quality in response to BLM protests.[45] Therefore, we caution readers from making excessively generalizable inferences from our analyses concerning the link between BLM protests, police activity, and public safety.

[43]We do not pool the data into a single dataset and estimate the discontinuous effect of the *BLM protest* on our outcomes of interest due to differences in the data-generating process and outcome measurement across cities (e.g. terry stops vs. traffic stops, or hit rate measurement differences). Although data-generating process differences may pose issues with the meta-analysis, the meta-analytic estimates can still teach us general patterns concerning the effect of the BLM protests.

[44]The Hartung-Knapp random effects approach is advantageous since it adjusts estimates and standard errors in light of study effect heterogeneity, mitigating false positives (IntHout, Ioannidis, and Borm, 2014).

[45]But we do not find the BLM protests universally increased policing quality in our analyses, so we are less concerned about this threat to generalizability.

# M Using Consistent Hit Rate Measure

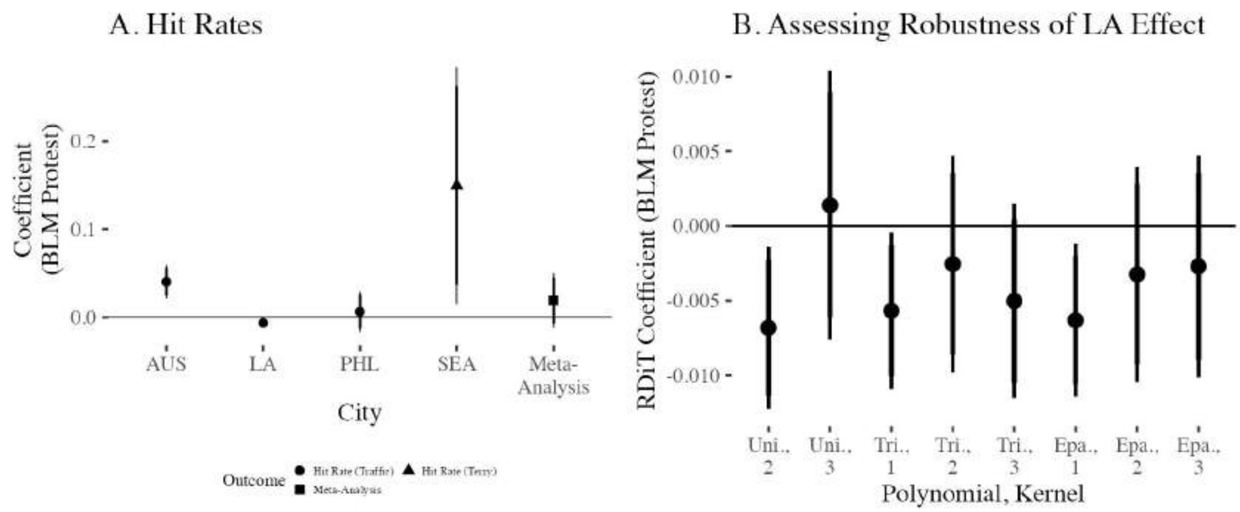


**Figure M81: Re-estimating effect of *BLM protest* on *hit rates* using consistent *hit rate* measures.** Panel A characterizes the BLM protest RDiT effect on hit rates across the cities of interest in addition to a random effects meta-analytic effect (running variable polynomial equal to 1, uniform kernel, using mean-squared optimal bandwidth approach by Calonico, Cattaneo, and Titiunik (2015)). Panel B characterizes the BLM protest RDiT effect on hit rates in Los Angeles using different polynomial and kernel specifications. 95% CIs displayed from robust SEs.

In this Section, we re-estimate the effects of the *BLM protest* on *hit rates* across the cities of interest using more harmonized hit rate outcome measures. Only Austin and Los Angeles allow us to disaggregate different types of contraband, so we generate a common hit rate between these two cities where a "hit" is now defined as identification of a "weapon," "drugs," or "money." Importantly, this "hit" measurement is similar to the way Philadelphia measures hit rates (weapons, drugs, or other contraband), so our hit rate outcomes are relatively harmonized across at least three cities.

Figure M81, Panel A, characterizes the regression discontinuity-in-time effect of the *BLM protest* on the new, harmonized, hit rate measures. The results are largely the same as those in the main text on Figure 3, with the exception being that the BLM protest appears to have decreased the *hit rate* in Los Angeles ($-0.006$, $p < 0.01$) instead of having no effect. However, this finding is not robust and sensitive to model specification. Figure M81, Panel B shows in 5/9 estimates using alternative kernel and polynomial specifications, the effect of the *BLM protest* on hit rates is statistically null. Finally, like the main results on Figure 3, the meta-analytic estimate shows, across the four cities we analyze, the average effect of the BLM protest is also statistically null ($0.02, p = .22$).

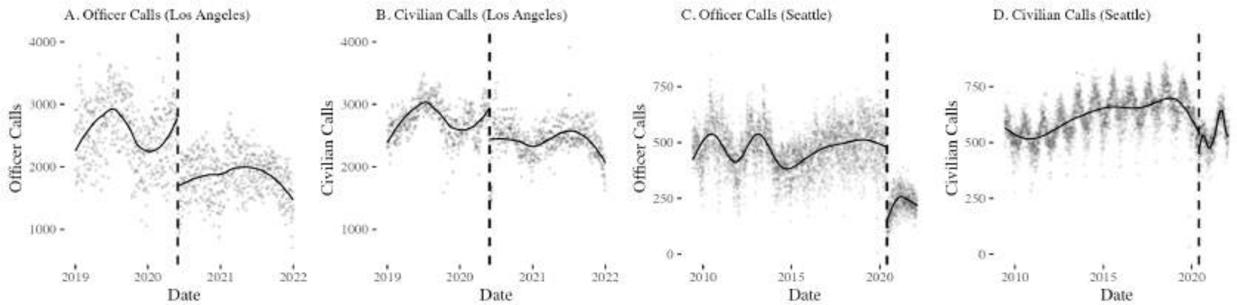


**Figure N82: Officer and Civilian Emergency Calls (y-axis) Over Time (x-axis) in Los Angeles (Panels A-B) and Seattle (Panels C-D)**

# N Is Depolicing Due to Reduced Civilian Demand?

An alternative explanation for the finding that the *BLM protests* decreased police activity is that civilians reduced demand for police services instead of the police restraining their activity. Reductions in civilian demand may be due to individuals staying home during the protest or a reticence to request police intervention brought on by the protests themselves (Ang et al., 2021). To assess this, we leverage 911 call data from two of our four cities: Los Angeles (2019-01-01 to 2021-01-01) and Seattle (2010-01-01 to 2021-01-01).[46] Emergency call data from these cities can be disaggregated between calls initiated by civilians and by police officers. Officer initiated 911 calls are often reports of encountered incidents.[47] Therefore, officer-initiated 911 calls serve as a measure of policing, while civilian-initiated calls serve as a measure of civilian demand for police services. Importantly, our goal is to rule out the possibility that declines in police stops are not wholly accounted for by reduced civilian demand. If we can show that decreases in officer-initiated 911 calls are more substantial and persistent than decreases in civilian-initiated calls, then we have evidence that police restraint is operative in policing patterns net of civilian demand.

Figure N82 shows officer and civilian calls over time. In Los Angeles, officer calls discontinuously and persistently decrease while civilian calls discontinuously decrease, but to a lesser extent than officer calls (Panels A-B). Likewise, in Seattle, officer calls appear to discontinuously decrease post-*BLM protest*, and the decrease persists well into 2020 (Panel C). Conversely, civilian calls discontinuously decrease only slightly post-*BLM protest* (Panel D), and rebound to the pre-protest mean by the end of 2020. Additionally, decreases in officer calls appear more substantial at the discontinuity than decreases in civilian calls.

We conduct a formal test demonstrating that the discontinuous decrease in officer calls post-*BLM protest* was more substantial than the reduction in civilian calls. We assess the discontinuous decrease in the *difference* and *ratio* between officer and civilian calls (Figure N83). If the decrease is negative, then observed reductions in police activity are likely driven primarily by police themselves. We find statistically significant and substantial discontinuous reductions in the officer-civilian call *difference* ($\beta$ = -0.4, SE = 0.04, $p < 0.001$; $\beta$ = -0.24,

[46] Source: `https://data.lacity.org/browse?q=calls%20for%20service&sortBy=relevance` and `https://data.seattle.gov/Public-Safety/Call-Data/33kz-ixgy`

[47] Based on our correspondence with LA and Seattle Open Data.

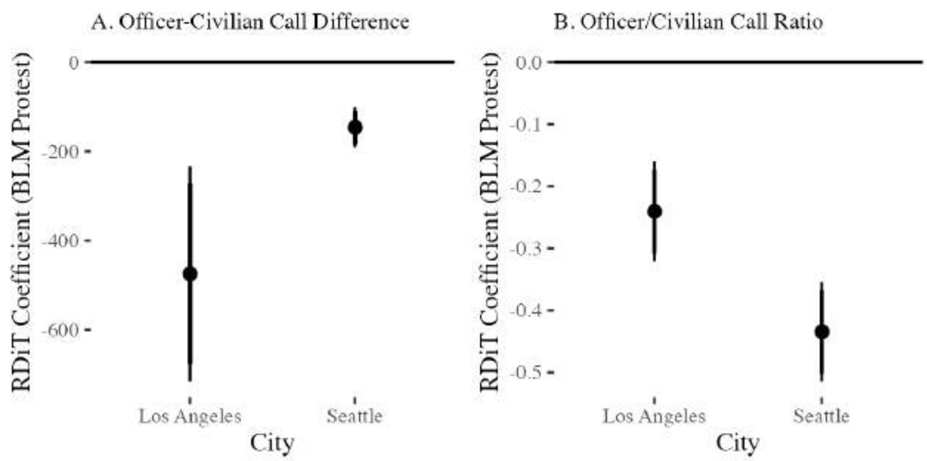


**Figure N83: Assessing If Reductions in Policing Activity are Driven by Civilian Demand.** The y-axis is the post-*BLM protest* RDiT coefficient, the x-axis is the city at use. All estimates are from RD specifications with a uniform kernel,polynomial degree equal to 1, and mean-squared optimal bandwidth selection (Calonico, Cattaneo, and Titiunik, 2015). 95% CIs displayed derived from robust SEs. Regression tables can be found in Appendix Table B3.

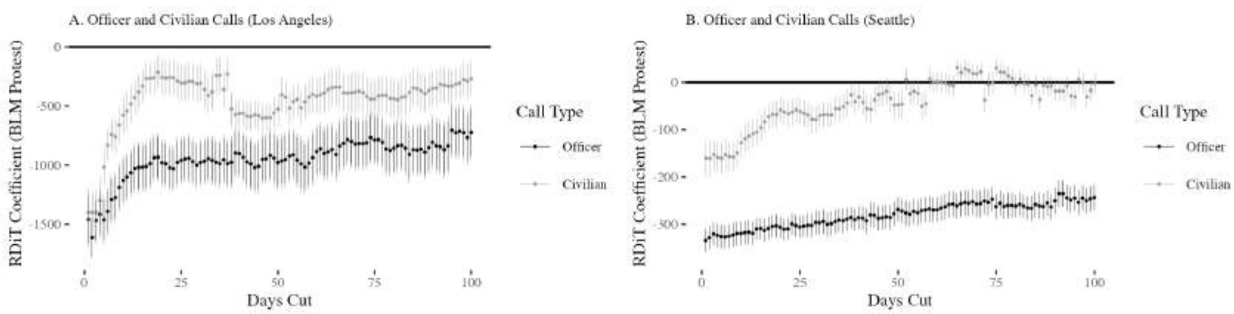


**Figure N84: Assessing Persistence of Reductions in Civilian Demand and Police Activity.** The x-axis is the number of days cut from right-hand side of the discontinuity in the data (but keeping days after intact). The y-axis is the post-*BLM protest* coefficient. Color denotes call type. Estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. 95% CIs displayed derived from robust SEs.

SE = 0.04, $p < 0.001$) and officer/civilian call *ratio* ($\beta$ = -146, SE = 22, $p < 0.001$; $\beta$ = -434, SE = 123, $p < 0.001$) in both Los Angeles and Seattle.

Finally, we assess whether the decrease in officer calls post-*BLM protest* was more *persistent* than the reduction in civilian calls. To do this, we cut data from a specified number of days (1-100) immediately post-*BLM protest*, but keep all data after the cut number of days intact. In both cities, officer calls discontinuously and persistently decrease. But, civilian calls rebound in Los Angeles roughly 20 days post-*BLM protest* and do so within 50 days in Seattle (Figure N84). Coefficient difference tests suggest the decrease in officer calls is statistically lower than the decrease in civilian calls after cutting 1-100 days immediately post-*BLM protest* ($p < 0.05$ in all cases with the exception of the first four days cut in Los

Angeles). In summary, consistent with *Hypothesis 1*, reductions in police activity are present even after accounting for citizen demand.

# O Is the Increase in Arrest Rates Prosocial?

We identify evidence across four cities that the onset of the *BLM protest* increased *arrest rates*. Consistent with *Hypothesis 2a* and our theory, our assumption is that higher *arrest rates* post-*BLM protest* characterize more prosocial policing since police are increasingly engaging in less superfluous stops and identifying arrest-worthy offenses conditional on civilian contact. However, given the arrest rate is the number of daily arrests normalized over the number of daily stops, the increase in the arrest rate could also be a function of reduced discretion in arrest initialization and subsequent increases in the count of arrests post-*BLM protest*. This concern is particularly relevant since police may increase arrest initialization in direct response to protest activity.

Therefore, we decompose the *arrest rate* and assess the RDiT discontinuous effect of the *BLM protest* on arrests and stops. Inconsistent with the notion that the arrest rate increase post-*BLM protest* was a function of increased arrest initialization (perhaps in response to BLM protest activity), we not only find that stops declined post-*BLM protest*, but also the overall number of arrests (Figure P85, Panels A-B). These results suggest we are correct in assuming the increase in arrest rates post-*BLM protest* is likely prosocial and a product of reduced discretionary policing.

Additionally, to the extent we think arrests are less discretionary than stops (because they require at least some explanation of offending activity on part of a police officer to initialize), we should expect a steeper reduction in the stop level post-*BLM protest* relative to the average stop level pre-*BLM protest* than the arrest level post-*BLM protest* relative to the average pre-*BLM protest* arrest level if police are engaging in less discretionary (and therefore ostensibly more prosocial) policing. To evaluate this proposition, we generate two outcomes: 1) an arrest ratio, equal to the number of arrests divided by the mean number of arrests in 2020 pre-*BLM protest*; and 2) a stop ratio, equal to the number of stops divided by the mean number of stops in 2020 pre-*BLM protest*. Consistent with our argument that the BLM protests differentially reduced more discretionary police activity (stops) than less discretionary activity (arrests), the *BLM protest*-induced decline in the stop ratio is statistically larger than the arrest ratio decline for Austin, Los Angeles, and Seattle (Figure P85, Panels C-F). Philadelphia is an exception, where the arrest ratio decline is larger than the stop ratio decline. But, ultimately, given the decline in overall arrests and stops in Philadelphia, these results are not inconsistent with our perspective that the BLM protests largely motivated relatively prosocial policing by reducing discretionary activity and unwarranted police contact *more* than less discretionary activity (i.e. arrest initialization).

We also engage in an additional decomposition of the *arrest rate* outcome to further rule out if the increase in arrest rates is a function of increased discretionary arrests in the aftermath of the BLM protests against BLM protesters. Although Austin, Philadelphia, and Seattle do not allow us to assess different types of arrests by offense severity, the Los Angeles arrest data includes arrest codes that we merge with California Department of Justice data to identify the severity of the arrestable offense,[48] which may correspond to the level of discretion used to initiate an arrest (lower severity = more discretion). The LA arrest data clarifies three types of arrests in order from least to most severe: 1) infraction

[48] To access raw arrest code data, see https://oag.ca.gov/law/code-tables

arrests; 2) misdemeanor arrests; 3) felony arrests. If the BLM protests motivated an increase in low-level arrests against protesters, we might expect 1) the proportion of arrests that are infraction or misdemeanor arrests to increase post-*BLM protest*; and/or 2) the proportion of stops that lead to infraction or misdemeanor arrests to increase post-*BLM protest*. We do not find evidence the proportion of arrests that are infraction arrests increased post-*BLM protest*, but we do find some evidence that the proportion of arrests that are misdemeanor arrests increased post-*BLM protest*. However, after removing arrests that are likely a product of protest activity, we do not find the proportion of arrests that are misdemeanor arrests increased post-*BLM protest* (Figure P86, Panel B). Moreover, we find that the proportion of stops that lead to infraction arrests does not increase post-*BLM protest*, while the proportion of stops that lead to both misdemeanor *and* felony arrests does increase post-*BLM protest* (with or without adjusting for arrests that are likely a product of protest activity) (Figure P86, Panel C). We interpret these results as evidence that the increase in the rate of arrests conditional on a stop in Los Angeles was not entirely driven by the police response to BLM protest activity short of the onset of the BLM protest. Since the proportion of stops leading to high-level arrests (i.e. felonies) increased post-*BLM protest*, it stands to reason that the increase in the *arrest rate* post-*BLM protest*, at least in Los Angeles, was partially driven by an increase in the identification of "arrest-worthy" activity conditional on civilian contact. Although the proportion of stops leading to misdemeanor arrests increased post-*BLM protest*, this does not mean the increase in the arrest rate post-*BLM protest* was necessarily driven by the initialization of superfluous arrests against protesters. The increase in the proportion of stops that lead to misdemeanor arrests is still positive after removing arrests that are likely a function of BLM protest activity from the data, and the proportion of stops leading to very low-level infraction arrests does not shift.

# P Decomposing the Arrest Rate Measure

## P.1 Arrests vs Stops

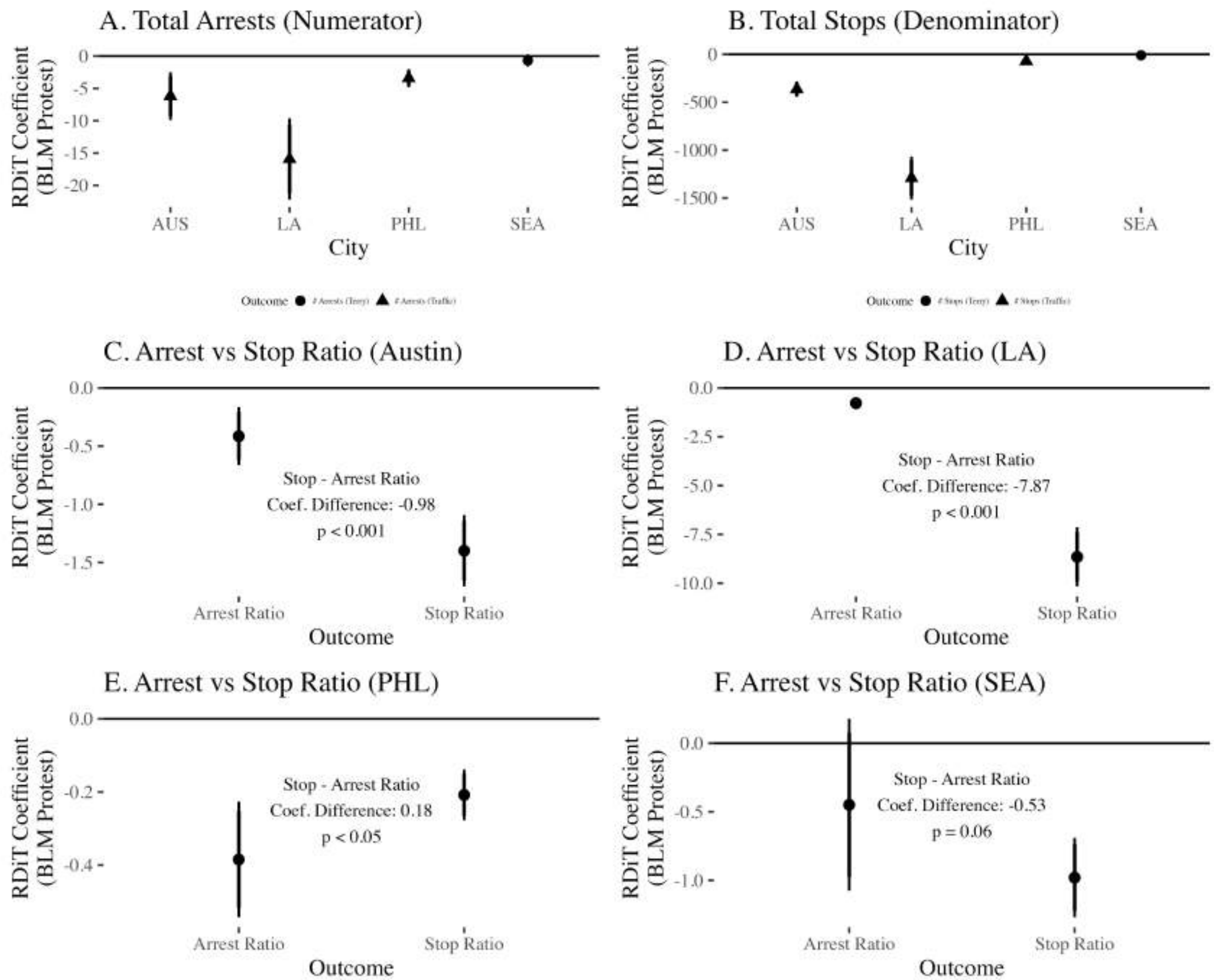


**Figure P85: Decomposing the *arrest rate* outcomes across cities.** Panel A characterizes the RDiT effect of the *BLM protest* on the daily number of arrests (y-axis, the numerator for the *arrest rate* outcome) across cities (x-axis). Panel B characterizes the RDiT effect of the *BLM protest* on the daily number of stops (y-axis, the denominator for the *arrest rate* outcome) across cities (x-axis). Panels C-F characterize the RDiT effect of the *BLM protest* on the daily ratio of the number of arrests (and stops) relative to the pre-*BLM protest* arrest (and stop) count mean between 2020-01-01 to the onset of the BLM protest. Annotations denote the *BLM protest* RDiT coefficient difference tests between the stop and arrest outcomes (stop - arrest). All RDiT estimates use the Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth selection approach with a uniform kernel and the running variable (days to *BLM protest*) to the first polynomial. 95% CIs displayed from robust SEs.

## P.2 Arrest Offense Type

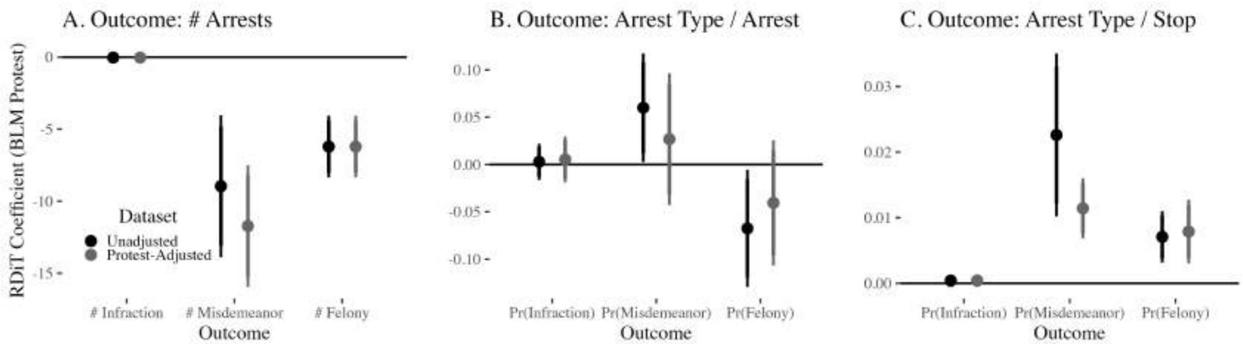


**Figure P86: Decomposing the *arrest rate* in Los Angeles by level of offense severity.** Panel A characterizes the RDiT effect of the *BLM protest* on the number of infraction, misdemeanor, and felony arrests in Los Angeles. Panel B characterizes the RDiT *BLM protest* effect on the proportion of arrests that are infractions, misdemeanors, or felonies in Los Angeles. Panel C characterizes the RDiT *BLM protest* effect on the proportion of stops that lead to infraction, misdemeanor, and felony arrests in Los Angeles. Across all plots, color denotes the inclusion (unadjusted) or exclusion (protest-adjusted) of arrest types that are newly in the top 30 arrest types in the 15 days after the BLM protest (relative to 15 days before), which are nearly all likely protest-related misdemeanor infractions (i.e. looting during state of emergency, burglary during state of emergency, curfew violations, emergency curfew violation, violating protective order, local ordinance violation, recovery of known stolen property, vandalism/property damage). All RDiT estimates use the Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth selection approach with a uniform kernel and the running variable (days to *BLM protest*) to the first polynomial. 95% CIs displayed from robust SEs.

# Q Protest Onset vs. Protest Intensity

A potential issue with our analyses is that the effect of the BLM protests on police activity could primarily be a function of police responses to the intensity of BLM protest activity itself. For example, policing may decline or arrest rates may increase not just because of the *onset* of the BLM protest and concomitant public scrutiny, but also because of police working directly in ways related to the protests themselves (i.e. policing the protests, crowd control, traffic control).

Thus, we conceptually distinguish between BLM protest *onset* and BLM protest *intensity*. Our design estimates the discontinuous effect of the onset, that is, the start, of the BLM protests. However, as mentioned during the discussion of the BLM protest as a bundled treatment in the Estimation Strategy section, BLM protest onset is certainly associated with BLM protest intensity, that is, the number of BLM protests or the number of BLM protesters in the street on a given day across the four cities we analyze. Therefore, it is unclear if the effects we estimate are a function of BLM protest *onset* or *intensity*. The distinction between BLM protest onset and intensity also matters for evaluating long-term effects. Our strategy is to descriptively assess the persistence of the discontinuous effects we identify by removing 1-100 days from our data post-*BLM protest*, but our outcomes may be affected by shifts in BLM protest intensity several days after BLM protest onset, generating bias in our long-term effect estimates.

To this end, we evaluate the effect of BLM protest onset on our outcomes of interest adjusting for BLM protest intensity. We use daily data on BLM protest intensity from the Crowd Counting Consortium during the year 2020. We measure intensity in two ways: 1) the daily count of BLM protests and; 2) the daily number of BLM protesters on the street within the four cities of interest. We merge this data with our policing data for the year 2020.[49] We estimate two post-*BLM protest* effects. The first is a regression-discontinuity-in-time estimate evaluating the short-term effect of the BLM protest on our outcomes of interest adjusting for BLM protest activity. The second is a simple pre-post difference-in-means *BLM protest* estimate comparing our outcomes of interest before and after the BLM protest adjusting for BLM protest intensity and year-quarter fixed effects to partial out seasonal outcome trends for 2020. This second estimation strategy allows us to evaluate the *long-term effects* of BLM protest onset adjusting for daily BLM protest intensity. Overall, across all cities and the two different estimation strategies, we find: 1) BLM protest *onset* decreases police stops across all four cities irrespective of BLM protest *intensity* (and in Philadelphia and Seattle, BLM protest intensity is uncorrelated with stops) (Tables R12-R15); 2) BLM protest *onset* increases the *arrest rate* across all four cities, whereas BLM protest *intensity* is typically uncorrelated with *arrest rates* (Tables R16-R19); 3) BLM protest *onset* decreases the Black/white rate ratio in Seattle and Philadelphia, but BLM protest *intensity* is uncorrelated with the Black/white rate ratio (Tables R20-R21). These results ultimately suggest that our effects are not driven by bundled treatment effects in the form of police responding to the protesters, but rather, the onset of BLM protests have reshaped police activity *independent of the intensity of the BLM protests.*

[49] CCC does not collect BLM protest intensity outside 2020.

# R Adjusting for Protest Intensity

## R.1 Protest Intensity Over time

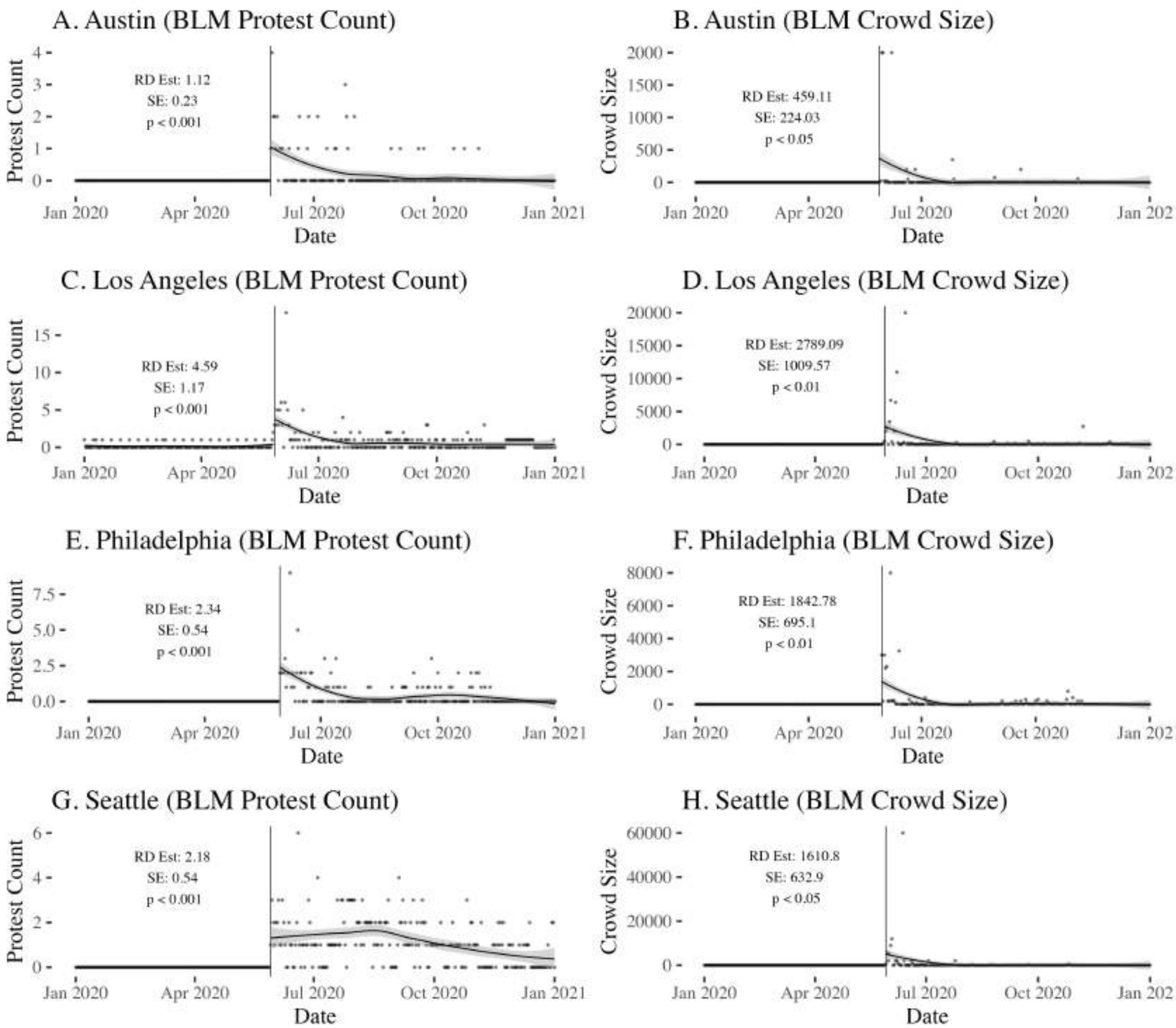


**Figure R87: Protest activity (y-axis) over time (x-axis) in 2020 by city.** Panels A, C, E, and G characterize the number of BLM protests over time in 2020 throughout Austin, Los Angeles, Philadelphia, and Seattle respectively. Panels B, D, F, and H characterize the daily BLM protest crowd size (lower estimate) in 2020 throughout Austin, Los Angeles, Philadelphia, and Seattle respectively. Data on the intensity of BLM protests are from the Crowd Counting Consortium (see: `https://ash.harvard.edu/programs/crowd-counting-consortium/`). Solid vertical line denotes the onset of the BLM protests in the respective cities we analyze. Loess lines fit on each side of the onset of the BLM protests. Annotations denote regression discontinuity-in-time *BLM protest* coefficients (polynomial = 1, uniform kernel). 95% CIs displayed from robust SEs.

## R.2 Demonstrating BLM Protest Onset Affects Police Behavior Net of Protest Intensity

### R.2.1 Stop Outcome

**Table R12: The onset of the BLM protest shifts police behavior net of the intensity of day-to-day protest activity (Austin)**

| | Traffic Stops | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | $-85.90^{***}$ | $-65.43^{*}$ | $-64.17^{*}$ | $-80.17^{**}$ | $-81.08^{**}$ |
| | (23.28) | (30.03) | (29.17) | (26.68) | (25.97) |
| Protest Count | | $-17.79^{**}$ | | $-18.50^{**}$ | |
| | | (6.36) | | (6.47) | |
| Log(Crowd Size + 1) | | | $-8.64^{***}$ | | $-7.87^{**}$ |
| | | | (2.25) | | (2.59) |
| Log(COVID Cases + 1) | | $-3.00$ | $-3.71$ | $-7.61$ | $-7.68$ |
| | | (4.42) | (4.26) | (4.77) | (4.75) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.34 | 0.34 | 0.35 | 0.37 | 0.37 |
| Num. obs. | 366 | 366 | 366 | 366 | 366 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Models 1-3 assess the discontinuous effect of the BLM protest on traffic stops throughout Austin (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Austin from the Crowd Counting Consortium (see: `https://ash.harvard.edu/programs/crowd-counting-consortium/`). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Austin (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from the Texas Department of State Health Services. HC2 robust SEs in parentheses.

## Table R13: The onset of the BLM protest shifts police behavior net of the intensity of day-to-day protest activity (Los Angeles)

| | Traffic Stops | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | $-516.93^{***}$ | $-618.81^{***}$ | $-606.37^{***}$ | $-565.69^{***}$ | $-537.30^{***}$ |
| | (77.33) | (78.61) | (76.59) | (92.91) | (94.07) |
| Protest Count | | $-49.97^{**}$ | | $-41.94^{**}$ | |
| | | (15.86) | | (14.49) | |
| Log(Crowd Size + 1) | | | $-41.01^{***}$ | | $-35.09^{***}$ |
| | | | (8.43) | | (8.46) |
| Log(COVID Cases + 1) | | $-100.17^{***}$ | $-102.38^{***}$ | $-52.85^{***}$ | $-52.72^{***}$ |
| | | (17.19) | (16.98) | (12.66) | (12.61) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.39 | 0.47 | 0.48 | 0.46 | 0.47 |
| Num. obs. | 366 | 366 | 366 | 366 | 366 |

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

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Models 1-3 assess the discontinuous effect of the BLM protest on traffic stops throughout Los Angeles (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Los Angeles from the Crowd Counting Consortium (see: `https://ash.harvard.edu/programs/crowd-counting-consortium/`). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Los Angeles (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from the LA County Department of Public Health. HC2 robust SEs in parentheses.

## Table R14: The onset of the BLM protest shifts police behavior net of the intensity of day-to-day protest activity (Philadelphia)

| | Traffic Stops | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | $-211.04^{***}$ | $-258.17^{***}$ | $-258.59^{***}$ | $-172.42^{***}$ | $-173.32^{***}$ |
| | (17.60) | (35.82) | (35.71) | (10.62) | (10.60) |
| Protest Count | | $-2.57$ | | $-1.29$ | |
| | | (1.74) | | (2.28) | |
| Log(Crowd Size + 1) | | | $-0.99$ | | $-0.31$ |
| | | | (0.97) | | (1.09) |
| Log(COVID Cases + 1) | | $-17.03$ | $-17.04$ | $18.13^{***}$ | $18.16^{***}$ |
| | | (9.33) | (9.33) | (5.39) | (5.39) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.62 | 0.62 | 0.62 | 0.70 | 0.70 |
| Num. obs. | 366 | 366 | 366 | 366 | 366 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Models 1-3 assess the discontinuous effect of the BLM protest on traffic stops throughout Philadelphia (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Philadelphia from the Crowd Counting Consortium (see: `https://ash.harvard.edu/programs/crowd-counting-consortium/`). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Philadelphia (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from Philadelphia Open Data. HC2 robust SEs in parentheses.

## Table R15: The onset of the BLM protest shifts police behavior net of the intensity of protest activity (Seattle)

| | Traffic Stops | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | $-9.95^{***}$ | $-10.18^{***}$ | $-10.08^{***}$ | $-8.84^{***}$ | $-8.86^{***}$ |
| | (0.93) | (1.12) | (1.07) | (0.85) | (0.84) |
| Protest Count | | 0.15 | | 0.04 | |
| | | (0.16) | | (0.15) | |
| Log(Crowd Size + 1) | | | 0.05 | | 0.02 |
| | | | (0.06) | | (0.07) |
| Log(COVID Cases + 1) | | 0.03 | 0.03 | 0.23 | 0.23 |
| | | (0.23) | (0.23) | (0.18) | (0.18) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.40 | 0.40 | 0.40 | 0.40 | 0.40 |
| Num. obs. | 348 | 348 | 348 | 348 | 348 |

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

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Models 1-3 assess the discontinuous effect of the BLM protest on traffic stops throughout Seattle (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Seattle from the Crowd Counting Consortium (see: https://ash.harvard.edu/programs/crowd-counting-consortium/). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Seattle (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from Seattle Open Data. HC2 robust SEs in parentheses.

### R.2.2 Arrest Rate Outcome

## Table R16: The onset of the BLM protest increases the arrest rate, not the intensity of protest activity (Austin)

| | Arrest Rate | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | 0.01 | 0.03 | $0.04^{\dagger}$ | $0.05^{*}$ | $0.05^{*}$ |
| | (0.01) | (0.02) | (0.02) | (0.02) | (0.02) |
| Protest Count | | $0.01^{\dagger}$ | | 0.01 | |
| | | (0.01) | | (0.01) | |
| Log(Crowd Size + 1) | | | 0.00 | | 0.00 |
| | | | (0.00) | | (0.00) |
| Log(COVID Cases + 1) | | $-0.01^{*}$ | $-0.01^{*}$ | $-0.01$ | $-0.01$ |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.01 | 0.06 | 0.05 | 0.06 | 0.06 |
| Num. obs. | 366 | 366 | 366 | 366 | 366 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Models 1-3 assess the discontinuous effect of the BLM protest on arrest rates throughout Austin (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Austin from the Crowd Counting Consortium (see: https://ash.harvard.edu/programs/crowd-counting-consortium/). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Austin (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from the Texas Department of State Health Services. HC2 robust SEs in parentheses.

## Table R17: The onset of the BLM protest increases the arrest rate, not the intensity of protest activity (Los Angeles)

| | Arrest Rate | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | 0.03*** | 0.02** | 0.02** | 0.03** | 0.03* |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Protest Count | | 0.00 | | 0.00 | |
| | | (0.00) | | (0.00) | |
| Log(Crowd Size + 1) | | | 0.00 | | 0.00 |
| | | | (0.00) | | (0.00) |
| Log(COVID Cases + 1) | | −0.00† | −0.00 | −0.00† | −0.00† |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.12 | 0.16 | 0.15 | 0.19 | 0.18 |
| Num. obs. | 367 | 367 | 367 | 367 | 367 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Models 1-3 assess the discontinuous effect of the BLM protest on arrest rates throughout Los Angeles (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Los Angeles from the Crowd Counting Consortium (see: https://ash.harvard.edu/programs/crowd-counting-consortium/). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Los Angeles (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from the LA County Department of Public Health. HC2 robust SEs in parentheses.

## Table R18: The onset of the BLM protest increases the arrest rate, not the intensity of day-to-day protest activity (Philadelphia)

| | Arrest Rate | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | 0.04*** | 0.04*** | 0.04*** | 0.05** | 0.05** |
| | (0.01) | (0.01) | (0.01) | (0.02) | (0.02) |
| Protest Count | | 0.01 | | 0.01 | |
| | | (0.00) | | (0.00) | |
| Log(Crowd Size + 1) | | | 0.00 | | 0.00 |
| | | | (0.00) | | (0.00) |
| Log(COVID Cases + 1) | | 0.00 | 0.00 | 0.00 | 0.00 |
| | | (0.00) | (0.00) | (0.00) | (0.00) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.20 | 0.21 | 0.21 | 0.21 | 0.22 |
| Num. obs. | 367 | 367 | 367 | 367 | 367 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Models 1-3 assess the discontinuous effect of the BLM protest on arrest rates throughout Philadelphia (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Philadelphia from the Crowd Counting Consortium (see: https://ash.harvard.edu/programs/crowd-counting-consortium/). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Philadelphia (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from Philadelphia Open Data. HC2 robust SEs in parentheses.

## Table R19: The onset of the BLM protest increases the arrest rate, not the intensity of day-to-day protest activity (Seattle)

| | Arrest Rate | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | $0.14^{**}$ | $0.14^{*}$ | $0.16^{*}$ | $0.16^{*}$ | $0.19^{*}$ |
| | (0.05) | (0.07) | (0.06) | (0.08) | (0.09) |
| Protest Count | | 0.01 | | 0.01 | |
| | | (0.02) | | (0.02) | |
| Log(Crowd Size + 1) | | | 0.00 | | −0.00 |
| | | | (0.01) | | (0.01) |
| Log(COVID Cases + 1) | | 0.02 | 0.02 | 0.01 | 0.01 |
| | | (0.01) | (0.01) | (0.01) | (0.01) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.03 | 0.03 | 0.03 | 0.05 | 0.04 |
| Num. obs. | 349 | 349 | 349 | 349 | 349 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.10$. Models 1-3 assess the discontinuous effect of the BLM protest on arrest rates throughout Seattle (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Seattle from the Crowd Counting Consortium (see: https://ash.harvard.edu/programs/crowd-counting-consortium/). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Seattle (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from Seattle Open Data. HC2 robust SEs in parentheses.

### R.2.3 Black/white Rate Ratio Outcome

## Table R20: The onset of the BLM protest reduces the Black/white stop rate ratio net of the intensity of protest activity (Philadelphia)

| | Black/white Rate Ratio | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | $-4.22^{***}$ | $-3.38^{**}$ | $-3.29^{**}$ | $-2.50^{*}$ | $-2.36^{*}$ |
| | (1.21) | (1.09) | (1.07) | (1.26) | (1.18) |
| Protest Count | | −0.09 | | −0.13 | |
| | | (0.34) | | (0.32) | |
| Log(Crowd Size + 1) | | | −0.08 | | −0.10 |
| | | | (0.20) | | (0.20) |
| Log(COVID Cases + 1) | | 0.25 | 0.25 | $0.37^{*}$ | $0.36^{*}$ |
| | | (0.27) | (0.27) | (0.17) | (0.17) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.08 | 0.09 | 0.09 | 0.09 | 0.09 |
| Num. obs. | 362 | 362 | 362 | 362 | 362 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.10$. Models 1-3 assess the discontinuous effect of the BLM protest on the Black/white rate ratio rates throughout Philadelphia (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Philadelphia from the Crowd Counting Consortium (see: https://ash.harvard.edu/programs/crowd-counting-consortium/). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Philadelphia (logged plus 1 to ensure identification) from the Crowd Counting Consortium. "COVID Cases" is the number of daily COVID cases using data from Philadelphia Open Data. HC2 robust SEs in parentheses.

**Table R21: The onset of the BLM protest reduces the Black/white stop rate ratio net of the intensity of protest activity (Seattle)**

| | Black/white Rate Ratio | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| BLM Protest | $-4.61^{*}$ | $-6.31^{**}$ | $-7.04^{**}$ | $-4.98^{**}$ | $-6.03^{***}$ |
| | (2.21) | (2.36) | (2.35) | (1.52) | (1.67) |
| Protest Count | | $-0.13$ | | $-0.21$ | |
| | | (0.34) | | (0.33) | |
| Log(Crowd Size + 1) | | | 0.14 | | 0.17 |
| | | | (0.17) | | (0.18) |
| Log(COVID Cases + 1) | | $-1.34^{*}$ | $-1.35^{*}$ | $-1.20^{*}$ | $-1.18^{*}$ |
| | | (0.55) | (0.55) | (0.52) | (0.52) |
| RD Effect? | Y | Y | Y | N | N |
| DIM Effect? | N | N | N | Y | Y |
| Quarter FE | N | N | N | Y | Y |
| $R^2$ | 0.10 | 0.14 | 0.14 | 0.14 | 0.14 |
| Num. obs. | 264 | 264 | 264 | 264 | 264 |

Note: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.10$. Models 1-3 assess the discontinuous effect of the BLM protest on the Black/white rate ratio rates throughout Seattle (running variable polynomial = 1, uniform kernel, using all days during the year 2020). Models 4-5 assess a difference-in-means between days before and after the BLM protest adjusting for year-quarter fixed effects. "Protest count" is the number of BLM protests occurring at the daily-level in Seattle from the Crowd Counting Consortium (see: `https://ash.harvard.edu/programs/crowd-counting-consortium/`). "Crowd size" is the low-end estimate of the daily number of BLM protesters out in Seattle (logged plus 1 to ensure identification) from the Crowd Counting Consortium. HC2 robust SEs in parentheses.

# S Assessing Impact of Intervening Events (Jacob Blake)

## S.1 Effect of Jacob Blake Shooting on BLM Protest Intensity

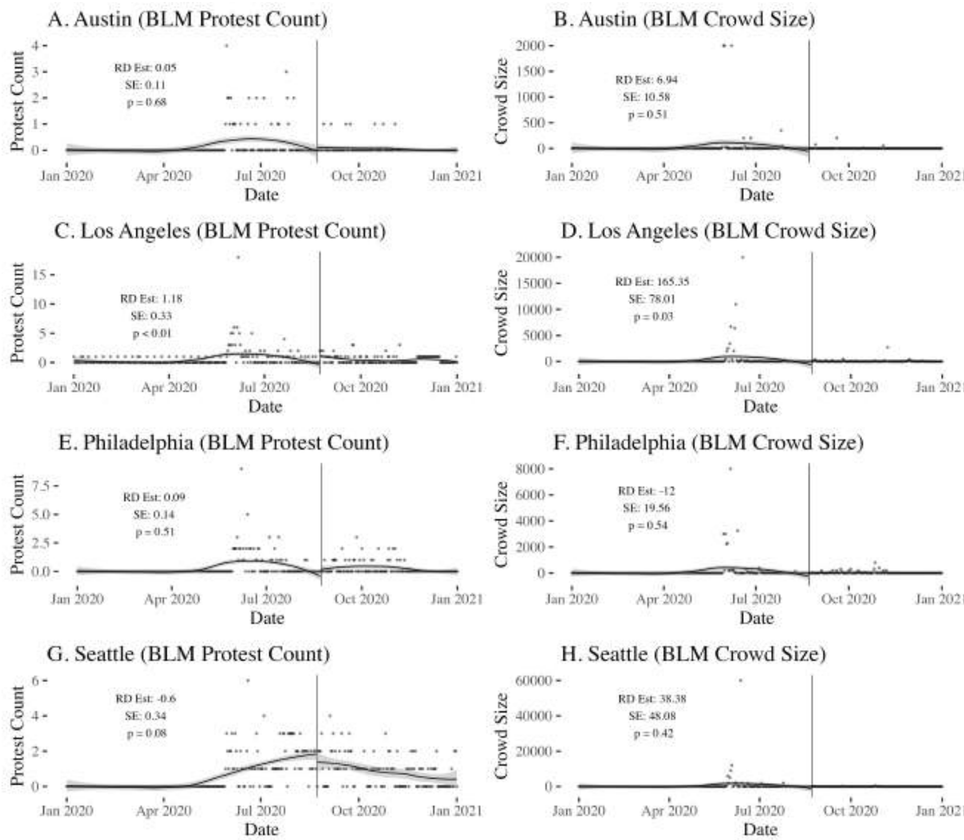


**Figure S88: Protest activity (y-axis) over time (x-axis) in 2020 by city.** Panels A, C, E, and G characterize the number of BLM protests over time in 2020 throughout Austin, Los Angeles, Philadelphia, and Seattle respectively. Panels B, D, F, and H characterize the daily BLM protest crowd size (lower estimate) in 2020 throughout Austin, Los Angeles, Philadelphia, and Seattle respectively. Data on the intensity of BLM protests are from the Crowd Counting Consortium (see: `https://ash.harvard.edu/programs/crowd-counting-consortium/`). Solid vertical line denotes the onset of Jacob Blake's shooting in the respective cities we analyze. Loess lines fit on each side of the onset of Jacob Blake's shooting. Annotations denote regression discontinuity-in-time *BLM protest* coefficients (polynomial = 1, uniform kernel). 95% CIs displayed from robust SEs.

## S.2 Effect of Jacob Blake Shooting on Policing Intensity

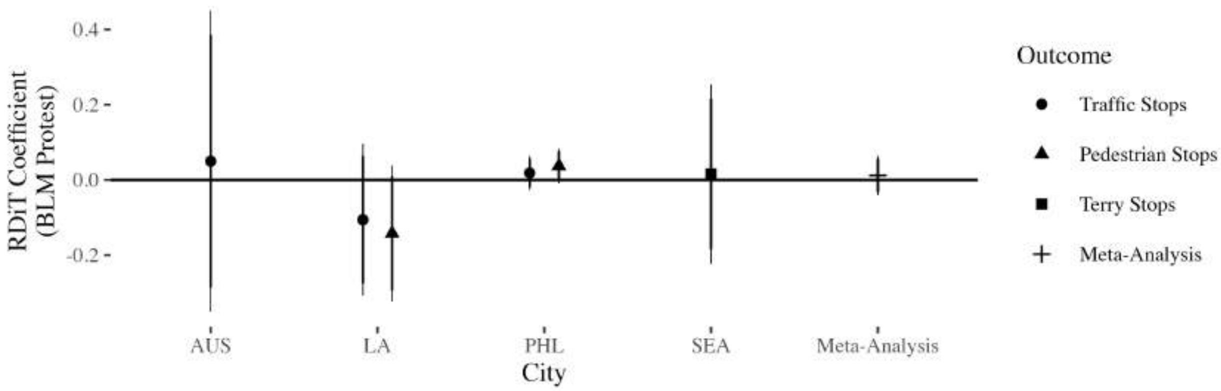


**Figure S89: Standardized RDiT Coefficients Characterizing Effect of Jacob Blake Shooting (August 23, 2020) (y-axis) on Policing Activity Across Cities (x-axis).** Shape denotes outcome type across the cities. All estimates are from RD specifications with a uniform kernel and polynomial degree equal to 1. Study-adjusted random effects meta-analytic coefficient on display. 95% CIs displayed derived from robust SEs.

# T Additional Temporal Placebo Tests

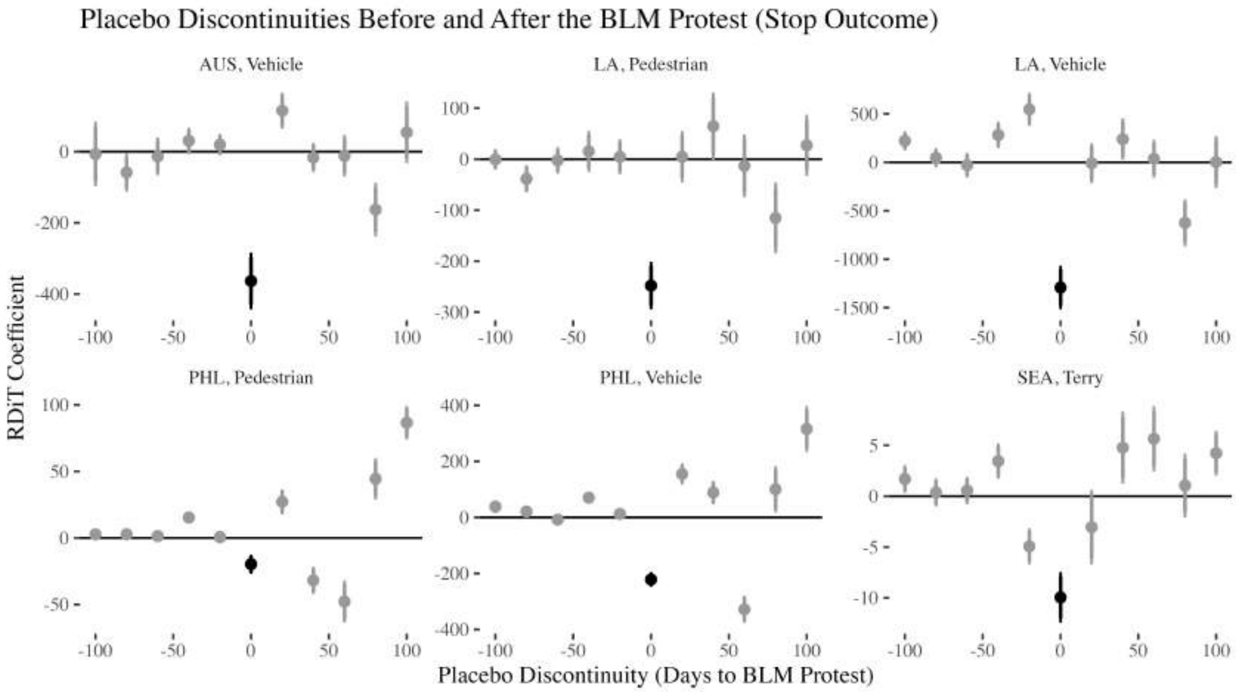


**Figure T90: Placebo tests characterizing the discontinuous effect (y-axis) of cut-points -100 to 100 days before and after the BLM protests (x-axis) on stops across cities.** Each panel refers to a different city and outcome type. Grey-colored coefficient estimates are from discontinuities that are before and after the initial onset of the BLM protest. Black-colored coefficient estimates are from discontinuities characterizing the onset of the BLM protests. All estimates are derived from regression discontinuity-in-time models using Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth approach (running variable polynomial = 1, uniform kernel). 95% CIs displayed from robust SEs.

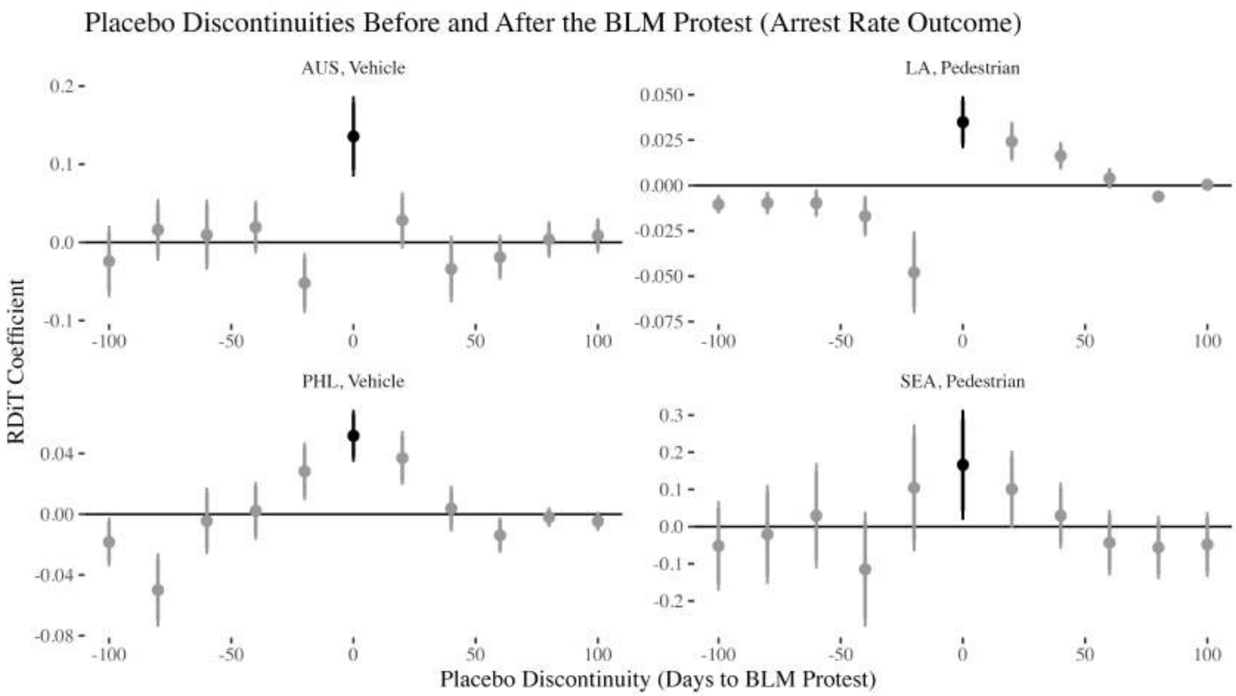


**Figure T91: Placebo tests characterizing the discontinuous effect (y-axis) of cutpoints -100 to 100 days before and after the BLM protests (x-axis) on arrest rates across cities.** Each panel refers to a different city and outcome type. Grey-colored coefficient estimates are from discontinuities that are before and after the initial onset of the BLM protest. Black-colored coefficient estimates are from discontinuities characterizing the onset of the BLM protests. All estimates are derived from regression discontinuity-in-time models using Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth approach (running variable polynomial = 1, uniform kernel). 95% CIs displayed from robust SEs.

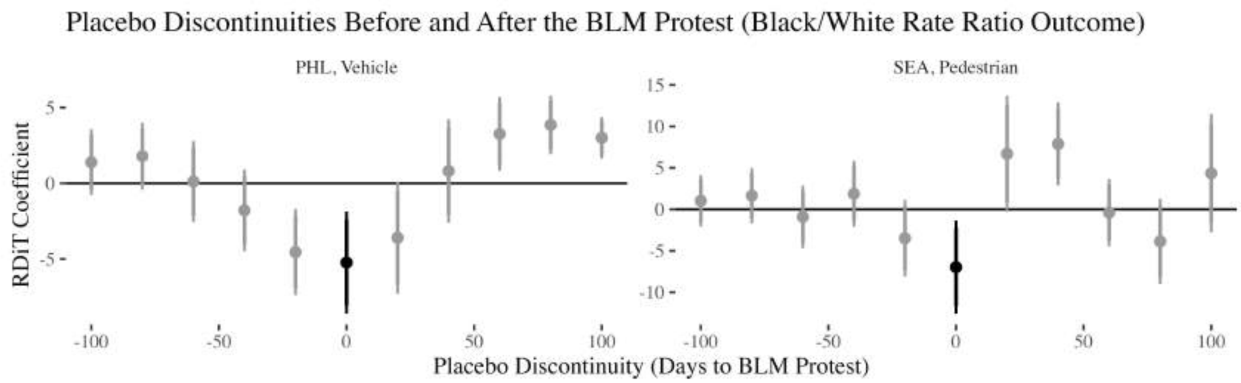


**Figure T92: Placebo tests characterizing the discontinuous effect (y-axis) of cutpoints -100 to 100 days before and after the BLM protests (x-axis) on the Black/white rate ratio across Philadelphia and Seattle.** Each panel refers to a different city and outcome type. Grey-colored coefficient estimates are from discontinuities that are before and after the initial onset of the BLM protest. Black-colored coefficient estimates are from discontinuities characterizing the onset of the BLM protests. All estimates are derived from regression discontinuity-in-time models using Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth approach (running variable polynomial = 1, uniform kernel). 95% CIs displayed from robust SEs.

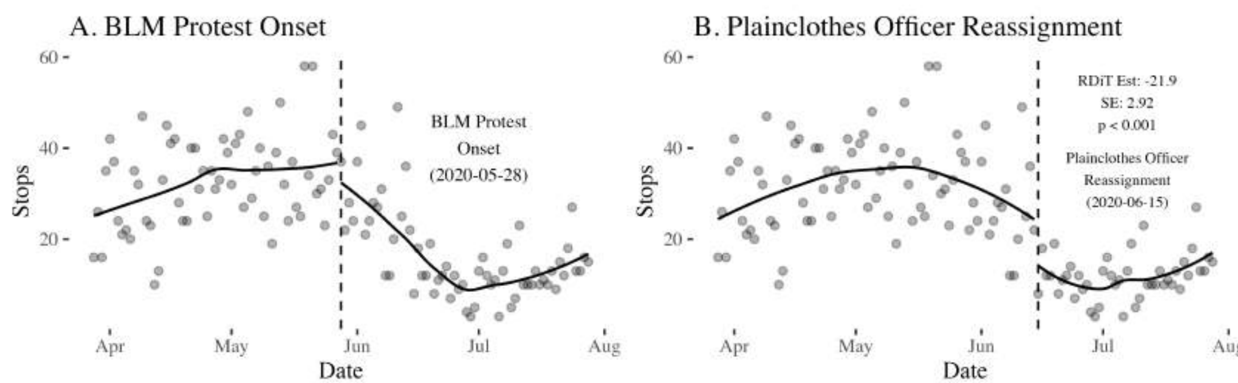


**Figure U93: The BLM protests did not necessarily reduce the number of stop-and-frisks imposed by NYPD (Panel A), but a plainclothes officer dissolution a few weeks after the onset of the BLM protests did discontinuously decrease policing (Panel B).** Dashed vertical line denotes the onset of the BLM protest (Panel A) and the plainclothes officer dissolution (Panel B). Loess lines fit on each side of the onset of the BLM protest and plainclothes officer dissolution respectively. Annotation on Panel B denotes regression discontinuity-in-time estimates characterizing the effect of the reassignment on the count of stop-and-frisks using the Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth approach (running variable polynomial = 1, uniform kernel). Robust SEs reported for RDiT estimates.

# U New York City Replication

Unlike the four cities we analyze in the main text, the relationship between NYPD policing and the BLM protests is fundamentally distinct. In the four cities we analyze, there is a clear, unambiguous, discontinuous decline in policing activity after the onset of the BLM protests. However, in New York City, this is not necessarily the case. Figure U93, Panel A shows that at the moment of the onset of the BLM protests in New York City, there is not a discontinuous decline in stop-and-frisks.[50] However, a few days later, there appears to be a discontinuous decline in stop-and-frisks corresponding to the dissolution and subsequent reassignment of a plainclothes officer unit that prioritized the use of stop-and-frisk to identify criminal activity (Figure U93, Panel B).[51] Police Commissioner Dermot F. Shea reported that he dissolved the unit to "build trust in the community" in the aftermath of weeks of BLM protests against excessive policing. Additionally, if we use an alternative measure of discretionary policing activity, that is reports by officers of engaging in directed patrols in particular places (i.e. officer calls to emergency management), we appear to observe a discontinuous decline in policing activity post-*BLM protest* (Figure U94, Panel C)). Indeed, regression discontinuity-in-time estimates suggest after the plainclothes unit dissolution, stop-and-frisks decline by substantively large 22 stops, 180% of the of the daily pre-reassignment stop standard deviation (12) and 76% of the pre-reassignment stop mean (29). Likewise, regression discontinuity-in-time estimates suggest after the *BLM protest*, there is a discontinuous decline in reports of directed patrols by -2600 ($p < 0.001$), 85% of the pre-*BLM protest* mean and 418% of the outcome standard deviation.[52]

Given qualitative evidence strongly suggests the plainclothes officer dissolution (*POD*) was implemented in response to the 2020 BLM protests and the onset of the *POD* is associated with a discontinuous drop in policing activity (just like the discontinuous drop in policing activity after the initial onset of the BLM protests across the four cities we analyze in the main text), we estimate the effect of the *POD* on our other outcomes of interest measuring policing quality within the stop-and-frisk data and we also estimate the effect of the *POD* and the *BLM protest* on crime to effectively assess how responses to public scrutiny in the aftermath of the 2020 BLM protests may have shifted NYPD behavior and affected public safety.

The RDiT discontinuous effect of the *POD* on stop-and-frisk *hit rates* is negative (-0.06) but only marginally significant ($p < 0.1$),[53] yet substantively large, equivalent to roughly 100% of the pre-*POD* standard deviation in the hit rate (Figure U95, Panel A). Descriptively, the decline in hit rates appears to be a function of a short-term pre-*POD* trend driven by the onset of the BLM protests in early June, suggesting these results may be an artifact of

[50] We use stop-and-frisk data from 2018-2021, publicly available here: https://www.nyc.gov/site/nypd/stats/reports-analysis/stopfrisk.page

[51] https://www.nytimes.com/2020/06/15/nyregion/nypd-plainclothes-cops.html

[52] Just like the main results, the effect of the BLM protest on civilian calls is comparatively small, implying that the reduction in policing we are identifying is not simply a function of reduced civilian demand. Regression discontinuity-in-time estimates suggest post-BLM protest, there was a comparatively smaller decline in 960 civilian calls (only 7% of the pre-BLM protest civilian call mean and 87% of the civilian call outcome standard deviation) relative to the reduction in 2600 reports of directed patrols.

[53] We measure *hit rates* by using the primary way the NYPD defines a "hit" for a stop-and-frisk: identification of a weapon.

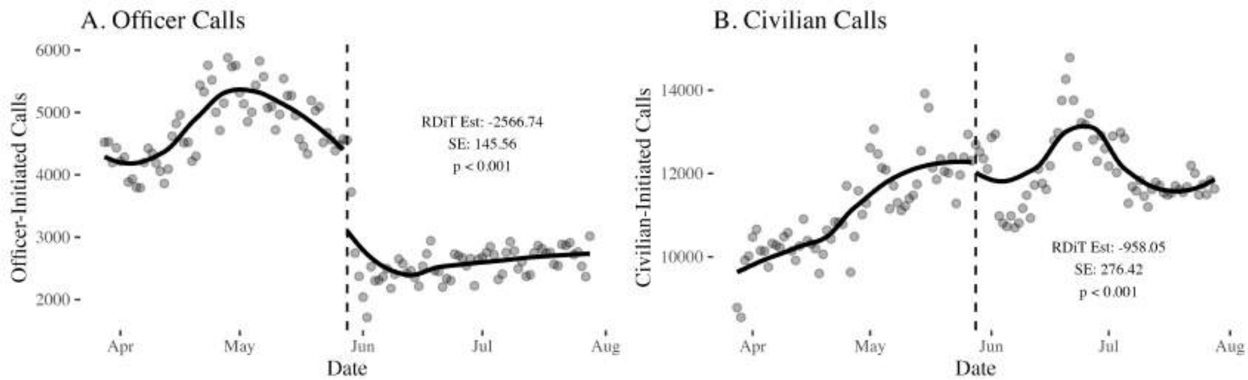


**Figure U94: The BLM protests reduced policing activity in the form of directed patrols (i.e. "officer calls," Panel A) and civilian 911 calls (Panel B).** Panel A characterizes the daily (x-axis) number of officer calls to emergency management to report directed patrols (y-axis) in the two months before and after the onset of the BLM protests. Panel B characterizes the number of daily (x-axis) civilian calls to emergency management (i.e. 911 calls, y-axis) in the two months before and after the onset of the BLM protests. Dashed vertical line denotes the onset of the BLM protest. Loess lines are fit on each side of the BLM protest discontinuity. Annotations denote regression discontinuity-in-time estimates characterizing the effect of the reassignment on the count of stop-and-frisks using the Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth approach (running variable polynomial = 1, uniform kernel). Robust SEs reported for RDiT estimates.

the BLM protests (i.e. a reversion to the mean after identifying weapon contraband among protesters) and not a significant shift in NYPD police tactics. Conversely, the post-*POD* effect on *arrest rates* is statistically insignificant and there is no discontinuity that is easily visually detectable from the descriptive statistics (Figure U95, Panel B).

However, the discontinuous effect of the *POD* on both the Black/white and Latino/white *rate ratio* is negative, statistically significant, and visually detectable from the descriptive statistics (Figure U95, Panels C-D). Post-POD, the Black/white rate ratio declines by -2.7 ($p < 0.001$) and the Latino/white rate ratio declines by -1.27 ($p < 0.001$), equivalent to 88% and 95% of the pre-*POD* Black/white and Latino/white rate ratio standard deviations (3.09, 1.33).

In summary, just like the results in the main text, support for *Hypothesis 2a* is mixed. The *POD* may have reduced *hit rates*, but this effect appears to be the product of a reversion to the mean related to BLM protest activity, not a structural shift in policing activity. Conversely, there is no shift in *arrest rates* before and after the *POD*. However, there is fairly strong support for *Hypothesis 2a* when we analyze the *rate ratio* outcomes. The NYPD Black/white and Latino/white rate ratios discontinuously decline after the *POD*, implying less racially disparate policing after the removal of the plainclothes stop-and-frisk unit.

Finally, we explore the effect of the *POD* and the *BLM protest* on crime using NYPD data on civilian complaints, a relevant question given that the *POD* resulted in a discontinuous decline in stop-and-frisks and the *BLM protest* resulted in a decline in officer-initiated

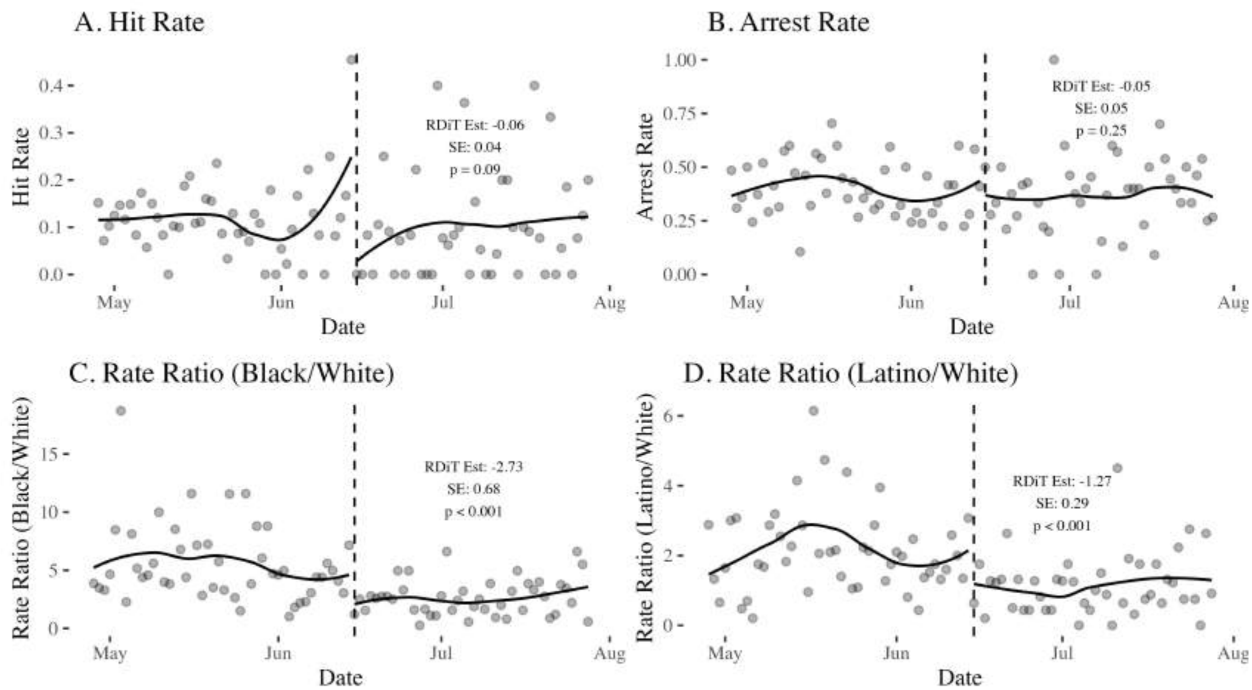


**Figure U95: The plainclothes officer dissolution in the aftermath of the BLM protests had mixed effects on NYPD policing quality.** Each panel characterizes the daily (x-axis) outcome (denoted by panel title) level two months before and after the plainclothes officer reassignment. Dashed vertical line denotes the onset of the plainclothes officer reassignment. Loess lines fit on each side of the reassignment onset discontinuity. Annotations denote regression discontinuity-in-time estimates characterizing the effect of the reassignment on the respective outcomes using the Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth approach (running variable polynomial = 1, uniform kernel). Robust SEs reported for RDiT estimates.

emergency management calls reporting directed patrols. Regression discontinuity-in-time estimates suggest the *POD* did not change against society or property crime (Figure U96, Panels A-B). However, inconsistent with *Hypothesis 3*, there appears to be a discontinuous increase in violent crime by 32 ($p < 0.01$), 74% and 14% of the pre-POD violent crime standard deviation (43) and mean (237) (Figure U96, Panel C). These results imply the decline in policing activity post-*POD* may have had deleterious consequences by reducing a deterrent effect against violent crime. However, it is important to note that any potential increase in crime post-*POD* is not necessarily the direct result of the *BLM protest* onset itself. Indeed, regression discontinuity-in-time estimates suggest *BLM protest* onset motivated a decline in against society crime, no change in violent crime, but an increase in property crime (Figure U96, Panels D-F).

Given the regression discontinuity-in-time estimates suggest the *POD* and *BLM protest* appear to have increased violent and property crime respectively, we put these effects to the test and evaluate their robustness to alternative RDiT kernel and polynomial specifications. Figure U97 characterizes the effect of the *POD* and *BLM protest* on violent and property

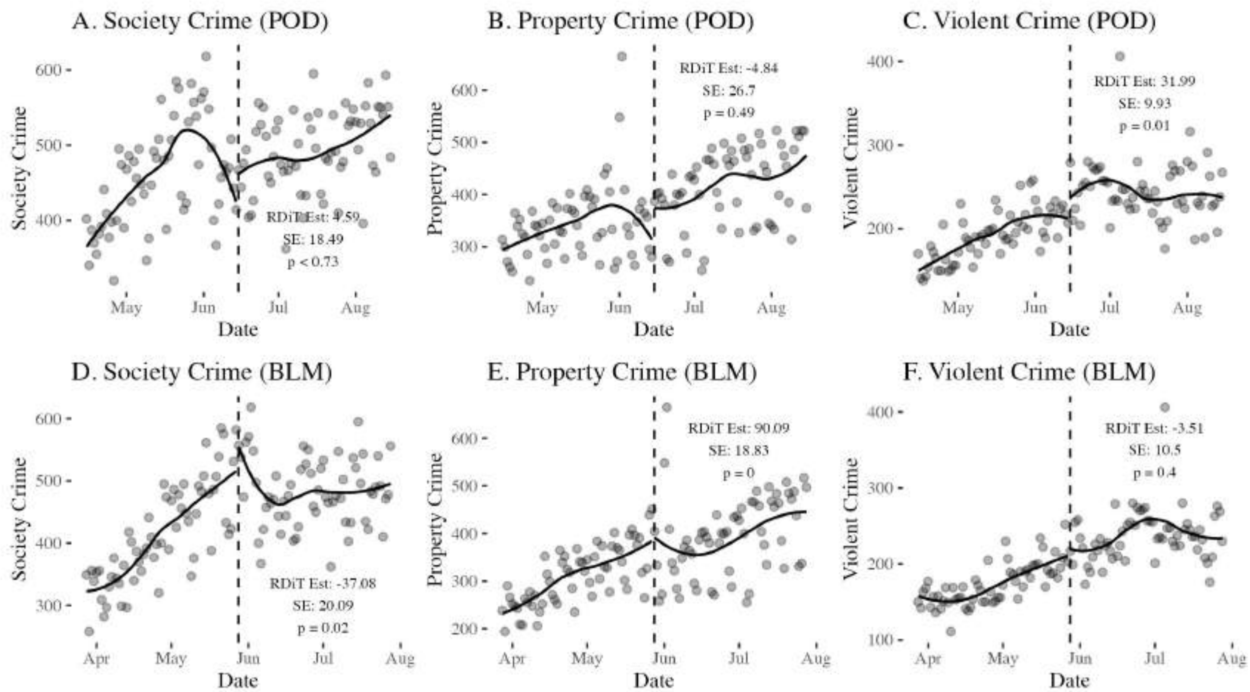


**Figure U96: The effect of the plainclothes officer dissolution (Panels A-C) and the BLM protest (Panels D-F) on crime.** Annotations denote regression discontinuity-in-time estimates characterizing the effect of the reassignment on the respective outcomes using the Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth approach (running variable polynomial = 1, uniform kernel). Robust SEs reported for RDiT estimates.

crime respectively. The effect of the *POD* on violent crime is not robust to alternative kernel and polynomial specifications, with 6 out of 9 specifications being statistically insignificant (Figure U97, Panel A). Likewise, the effect of the *BLM protest* on property crime is highly sensitive to kernel and polynomial specification, with 7 out of 9 specifications being statistically insignificant, and several coefficients in the opposite sign of the original specification we used (polynomial = 1, uniform kernel) (Figure U97, Panel B).

In summary, the empirical evidence in New York City is akin to the conclusions of the main text of the paper. Around the moment of the BLM protest, there was a precipitous decline in policing activity (and in New York City, this decline was partially the product of dissolving a plainclothes officer unit that engaged in high levels of stop-and-frisks). The effect of this decline in policing activity was ambiguous with respect to policing quality. There seems to be an idiosyncratic decline in hit rates (and therefore policing quality) that is not the product of a structural shift in policing, but at the same time, there is a decline in racially disparate policing toward Black and Latino civilians. Finally, the relationship between the BLM protest, the plainclothes officer dissolution, and crime is ambiguous and not sufficiently robust so as to be distinct from zero.

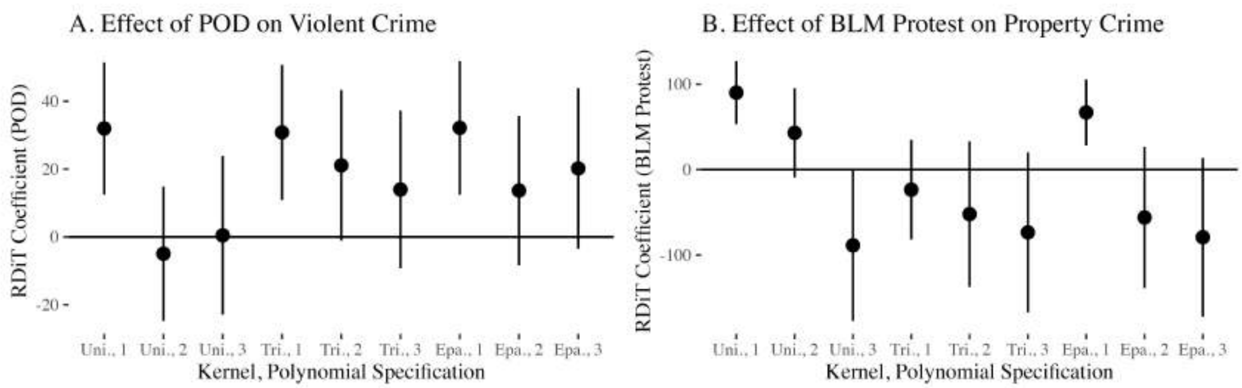


**Figure U97: The effect of the plainclothes officer reassignment on violent crime (Panel A) and the effect of the BLM protest on property crime (Panel B) across different kernel, polynomial specifications (x-axis).** All estimates use the Calonico, Cattaneo, and Titiunik (2015) mean-squared optimal bandwidth approach. 95% CIs from robust SEs displayed.