Part 4: Extensions

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As a reminder, I chose the following paper:

Hanna, R., Kreindler, G., & Olken, B. A. (2017). Citywide Effects of High-Occupancy Vehicle Restrictions: Evidence from the Elimination of '3-in-1'in Jakarta (No. w23295). National Bureau of Economic Research.

Main Result of the paper

As a reminder, the main results of the paper are reproduced in cloumns (2) and (4) in Panel A below. The coefficient of interest is in Policy Lifting. Comparing the Policy Lifting coefficient to the Control mean in columns (2) and (4) will give us the 45% and 85% increases in traffic delays.

Panel A: Delay on 3-in-1 Road (Sudirman)

	(1)	(2)	(3)	(4)	(5)	(6)
Time Interval	6 - 7 a.m.	7 - 10 a.m.	10 a.m. - 4:30 p.m.	4:30 - 7 p.m.	7 - 8 p.m.	8 p.m. - 6 a.m.
Policy Lifting	-0.00	0.98***	0.55**	2.48***	1.98***	0.05
	(0.05)	(0.07)	(0.23)	(0.30)	(0.34)	(0.08)
Northbound	0.24***	0.12	-0.98***	-1.48***	-2.01***	-0.08
	(0.01)	(0.12)	(0.16)	(0.26)	(0.36)	(0.05)
Observations	264	792	1,720	670	270	2,656
Control mean	1.92	2.14	2.98	2.84	3.59	1.87

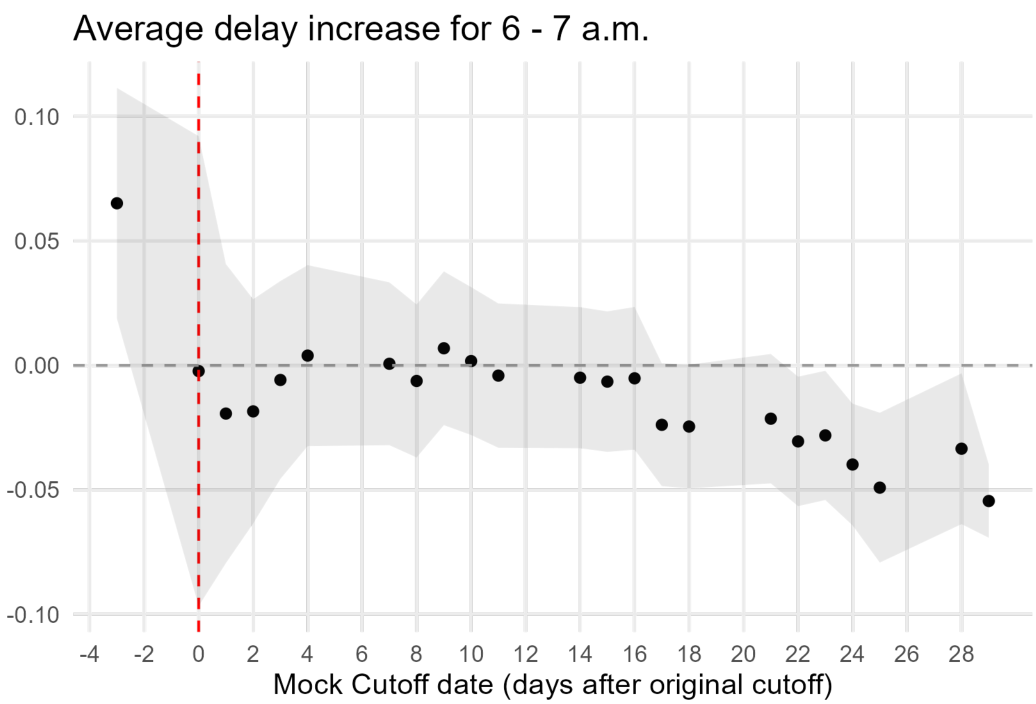
The results in the table employ a pre-post approach, comparing traffic congestion levels before and after the policy elimination, according to the following regression:

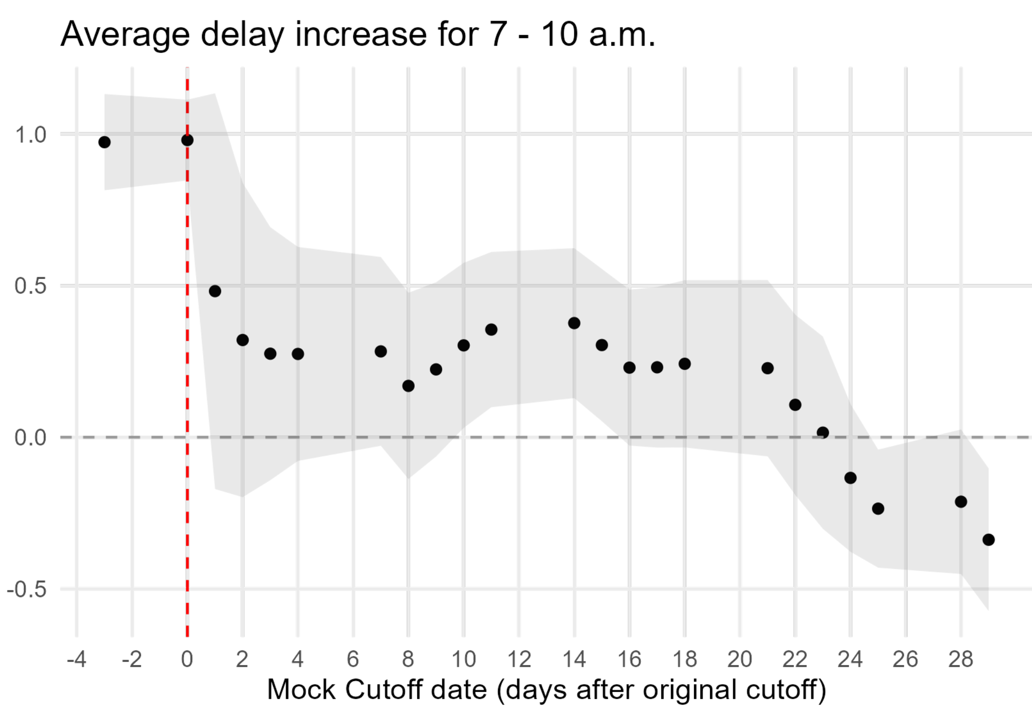
$$delay_{idh} = \alpha + \beta \cdot post_d + \gamma \cdot north_i + \varepsilon_{idh}$$

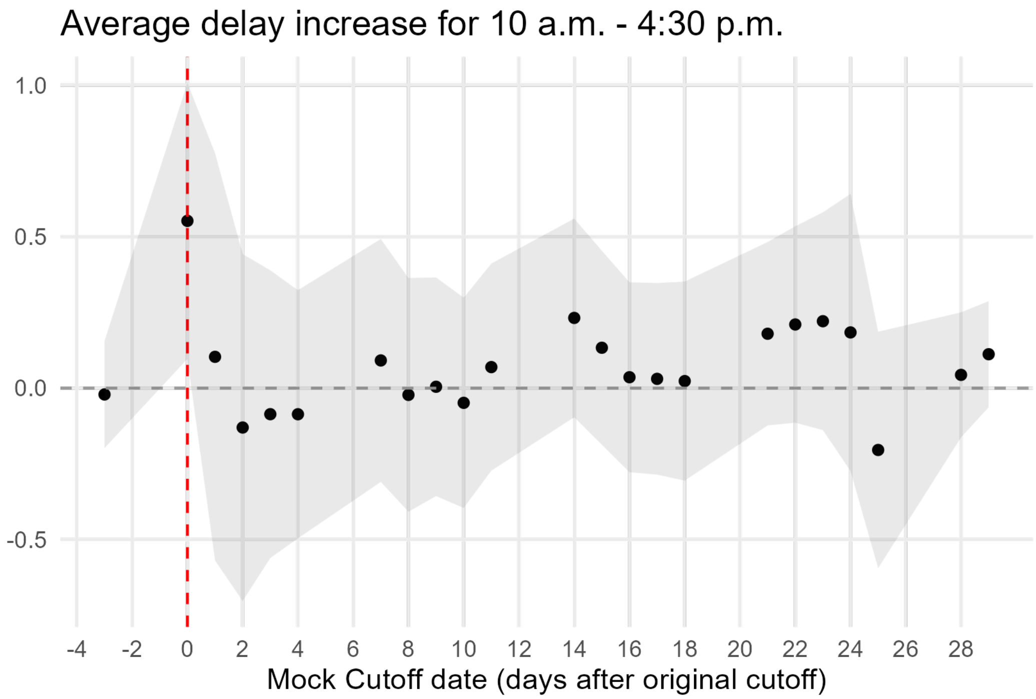
Where *delay* is the average travel delay in minutes per kilometer for segment (traffic route) i, on date d and for departure time h, *north* is an indicator for whether segment i is Northbound (to control for directional peak traffic routes), and *post* is an indicator for dates after the lifting of the HOV policy (Policy Lifting). β is the difference in average delays after the policy is lifted. Standard errors are clustered by date and direction.

Extension

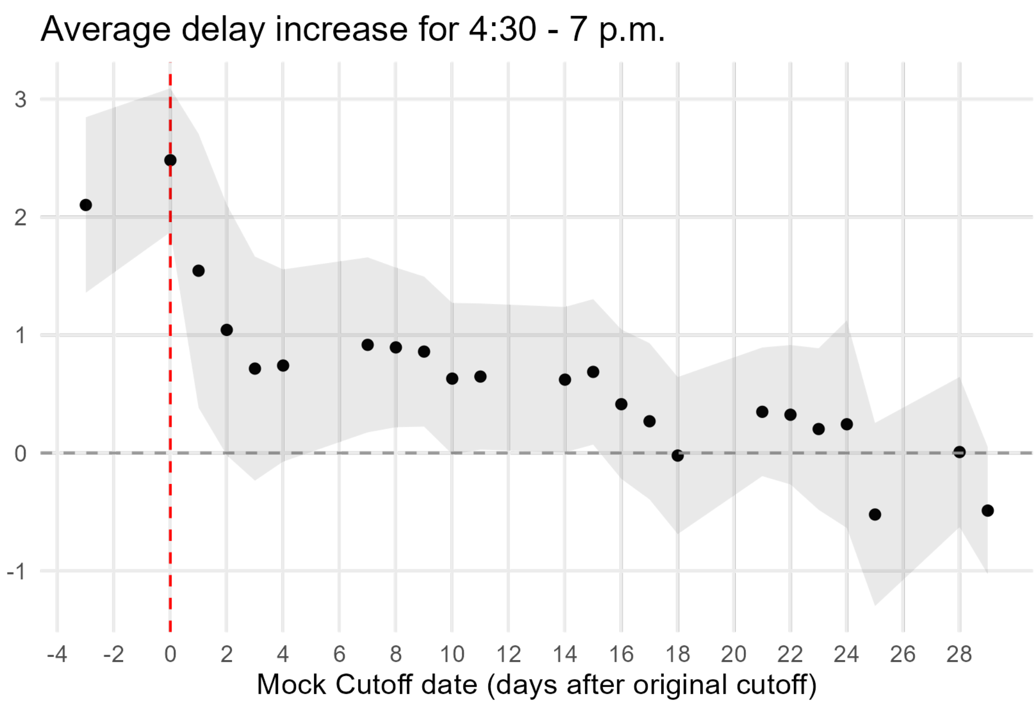
I propose a robustness check for Panel A by rerunning all the regressions while changing the policy lifting date to whatever dates we have in our sample. This amounts to a placebo test of the treated data: I reran the 6 regressions above 25 times (for each date in our sample) and made a coefficient plot for each Time Interval. You can see the results on the next page. If the robustness check is passed, we would expect to see non-significant treatment effects for other cutoff dates. We should especially pay attention to the 7 - 10 a.m. and 4:30 - 7 p.m. time intervals, where the main effects come from.


Average delay increase for 6 - 7 a.m.
0.10
0.05
0.00
-0.05
-0.10
-4 -2 0 2 4 6 8 10 12 14 16 18 20 22 24 26 28
Mock Cutoff date (days after original cutoff)

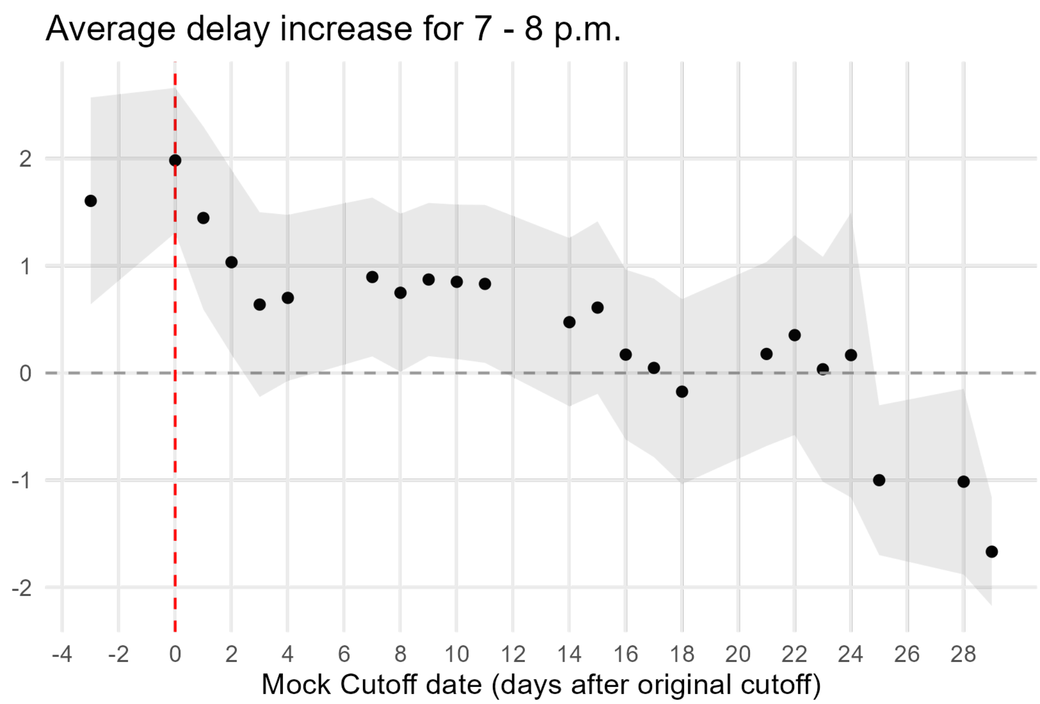

Average delay increase for 7 - 10 a.m.
1.0
0.5
0.0
-0.5
-4 -2 0 2 4 6 8 10 12 14 16 18 20 22 24 26 28
Mock Cutoff date (days after original cutoff)

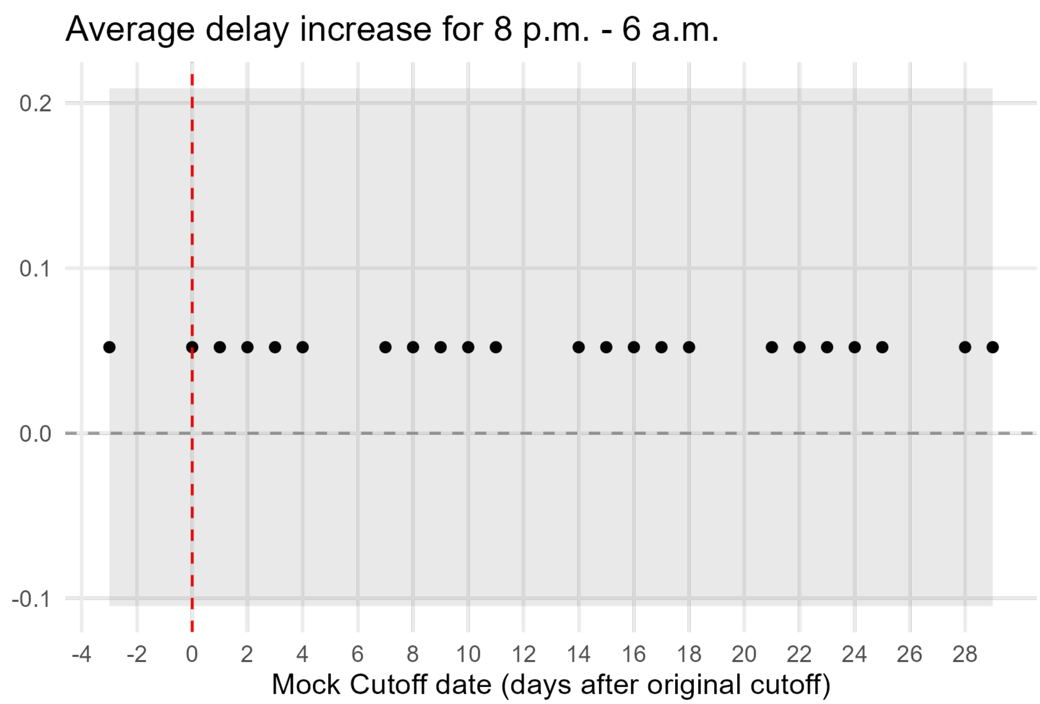


Average delay increase for 10 a.m. - 4:30 p.m.
1.0
0.5
0.0
-0.5
-4
-2
0
2
4
6
8
10
12
14
16
18
20
22
24
26
28
Mock Cutoff date (days after original cutoff)



Average delay increase for 4:30 - 7 p.m.
3
2
1
0
-1
-4
-2
0
2
4
6
8
10
12
14
16
18
20
22
24
26
28
Mock Cutoff date (days after original cutoff)





As you can see, the results seem robust when taking mock cutoff dates after the actual policy lifting date (except for very few false positives from 7 - 10 a.m. and 10 a.m. - 4:30 p.m.). Because there is too little information they collected for the period before the policy (up to 4 days before the policy lifting), we get some false positives before the treatment happens. This is a potential weak point about the paper, and the main reason why the authors try another approach by collecting historical Google Maps data, as opposed to the "live" data they queried on those days.