

Effect of Police on Crime & Economic Circumstances on Crime

Police, Economic Circumstances, and Crime

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Outline for Today

1. Discuss Jigsaw Papers
2. Summarize Yang (2017)
3. Summarize Palmer, Phillips, Sullivan (2019)



Cheng and Long (2018)



The French Quarter (FQ) Task Force

- The paper uses DiD
- Compares policing policy changes in the French Quarter in New Orleans and how they affected crime
- It compares the FQ before and after the task force was implemented to other neighborhoods during the same period
- The policy change is the creation of the "French Quarter Task Force" (FQTF)



The French Quarter (FQ) Task Force: Background

- Funded by Sidney Torres, a local entrepreneur
- They would drive around the FQ in these tiny cars to more quickly respond to reported crimes or issues (which could be reported via an app), and to otherwise patrol the FQ
- Cheng and Long study three phases:
 1. Pre-FQTF
 2. FQTF under private management
 3. FQTF under public management

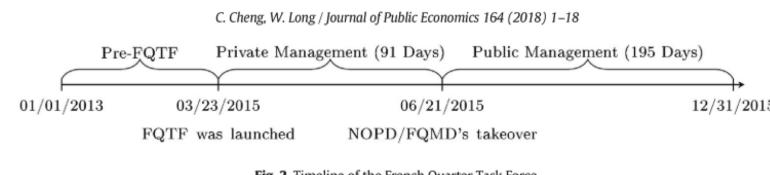


Fig. 2. Timeline of the French Quarter Task Force.

Check and Long: Treatment

- They study **susceptible crimes**.
- **susceptible crimes** are those that could be affected by the presence of the FQTF patrols:
 - Robbery
 - Aggravated Assault
 - Burglary
 - Theft (larceny and auto theft)

Check and Long: Falsification Test

- They use non-susceptible crimes (**homocide**)—crimes that should not be affected by FQTF—as a falsification test
- A falsification test is a way to see if the results you are finding in your study could potentially be spurious (i.e., false)
- Test an outcome that you think you should not change at all
- If there is good reason that it shouldn't change due to the treatment, and you find an effect, it may suggest problems with your DiD approach
 - E.g., parallel trends (aka “common trends”) assumption doesn’t hold

Check and Long: Methodology

- The methodology is a difference-in-differences:
- Compare FQ during the three phases:
 - Before FQTF
 - During FQTF – private management
 - During FQTF – public management
- To the 70 other neighborhoods in New Orleans over the same time period.

Check and Long: Results

- They find that the increased police presence from the FQTF reduced robberies by 37.4%, aggravated assaults by 16.9%, and thefts by 13%
- However, the program was more effective under private management rather than public management (NOPD)
- This may be because the private management structure provided more incentives for police to be productive

Di tella and Schargrodsky (2004)



Introduction

- The paper uses the change in policing after a terrorist attack as a **natural experiment**
- They study the effect of change in policing on crime
- The main Jewish center in Buenos Aires, Argentina was targeted by a terrorist attack in July 1994
- After the attack, all Jewish institutions in Argentina received police protection
- The paper leverages this change in policy in a Difference-in-Differences design
- They compare blocks next to the Jewish centers before and after increase in police presence to similar areas during the same period
- Treatment groups = block with a Jewish inst
+ one block away + two blocks away
- Control groups = 2+ blocks from Jewish institution

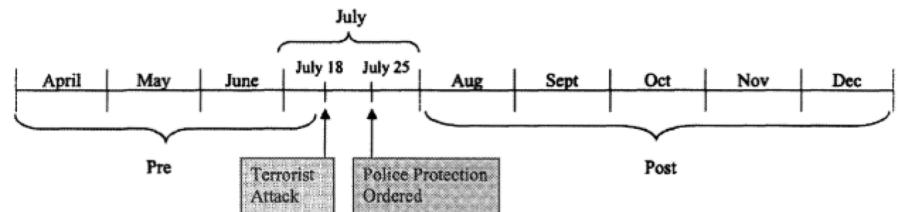


FIGURE 1. TIMELINE OF EVENTS

Key Assumption

- The key assumption is that the allocation of police outside Jewish centers and synagogues is exogenous to the crime trends in those areas
- They study the effect on crimes like property theft
- So for there to be an endogeneity concern, it would have to be the case that there was increased property theft right near synagogues, and the police were allocated in an endogenous way to curb that
- This seems highly unlikely
- The allocation of police here has nothing to do with LOCAL crime trends in property crime, etc., local to the area right by Jewish centers

Results

VOL. 94 NO. 1

DI TELLA AND SCHARGRODSKY: DO POLICE REDUCE CRIME?

123

TABLE 3—THE EFFECT OF POLICE PRESENCE ON CAR THEFT

	Difference-in-difference			Cross section	Time series
	(A)	(B)	(C)		
Same-Block Police	-0.07752*** (0.022)	-0.08007*** (0.022)	-0.08080*** (0.022)	-0.07271*** (0.011)	-0.05843*** (0.022)
One-Block Police		-0.01325 (0.013)	-0.01398 (0.014)	-0.01158 (0.010)	-0.00004 (0.013)
Two-Blocks Police			-0.00218 (0.012)	-0.00342 (0.009)	0.01701 (0.010)
Block fixed effect	Yes	Yes	Yes	No	Yes
Month fixed effect	Yes	Yes	Yes	Yes	No
Number of observations	7,884	7,884	7,884	4,380	3,816
R ²	0.1983	0.1984	0.1984	0.0036	0.1891

Notes: Dependent variable: number of car thefts per month per block. Least-squares dummy variables (LSDV) regressions. Car thefts that occurred between July 18 and July 31 are excluded. Column (D) excludes observations for the preattack period (April through July). Column (E) excludes observations for the blocks that are more than two blocks away from the nearest protected institution. Huber-White standard errors are in parentheses.

*** Significant at the 1-percent level.

- They find that the increased police presence reduced car theft on the same block of the jewish center
- No effect in blocks one and two blocks away
- What is “Cross section”?
- This is the naïve comparison of not using the pre-period data
- Compare treatment to control group in the post period
- This could create bias if there are fixed average differences between same-block and one/two blocks, however this generates only a smaller effect estimate

Results

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- What is “Time series”?
- This is the naïve comparison of not using a control group.
- Just look at crime before and after
- Could be biased from existing time trends. In this case the estimate decreases.
- Perhaps this is because there was an existing trend of decreasing crime over time?

Results

- The authors find that the police presence has a significant effect on reducing car thefts, but only right by the Jewish centers that got police protection
- The effects don't seem to occur outside of adjacent blocks, so the effects dissipate significantly with geography
- This could be due to the nature of this policing, which was more like armed guards near the entrance, and less like proactive policing where the police patrol around

Dur And Vollaard (2019)



RCT on waste disposal enforcement

- This is a randomized control trial (RCT) where the researchers worked with police to randomize a trash enforcement policy
- They picked 56 trash disposal sites in a city in the Netherlands. They randomized those sites into treatment and control
- **Control** = no change in policing policy
- **Treatment** = illegal disposed of trash bags got a warning label applied to the bags that noted that the bag was disposed of illegally and that there is a fine for this
- Thus the “treatment” is more saliency policy enforcement of laws

RCT on waste disposal enforcement

R. Dur, B. Vollaard / Journal of Environmental Economics and Management 93 (2019) 208–220



Fig. 1. Garbage bag disposal container (left) and paper and glass disposal container (right), featuring officers.

RCT on waste disposal enforcement

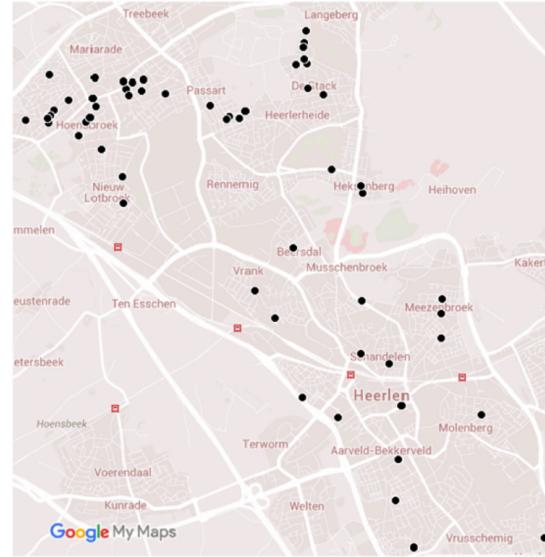


Fig. 2. Map of the area including the 56 container locations.

- The message reads “Found by law enforcement. Fine: at least 90 euros.

Treatment vs. Control

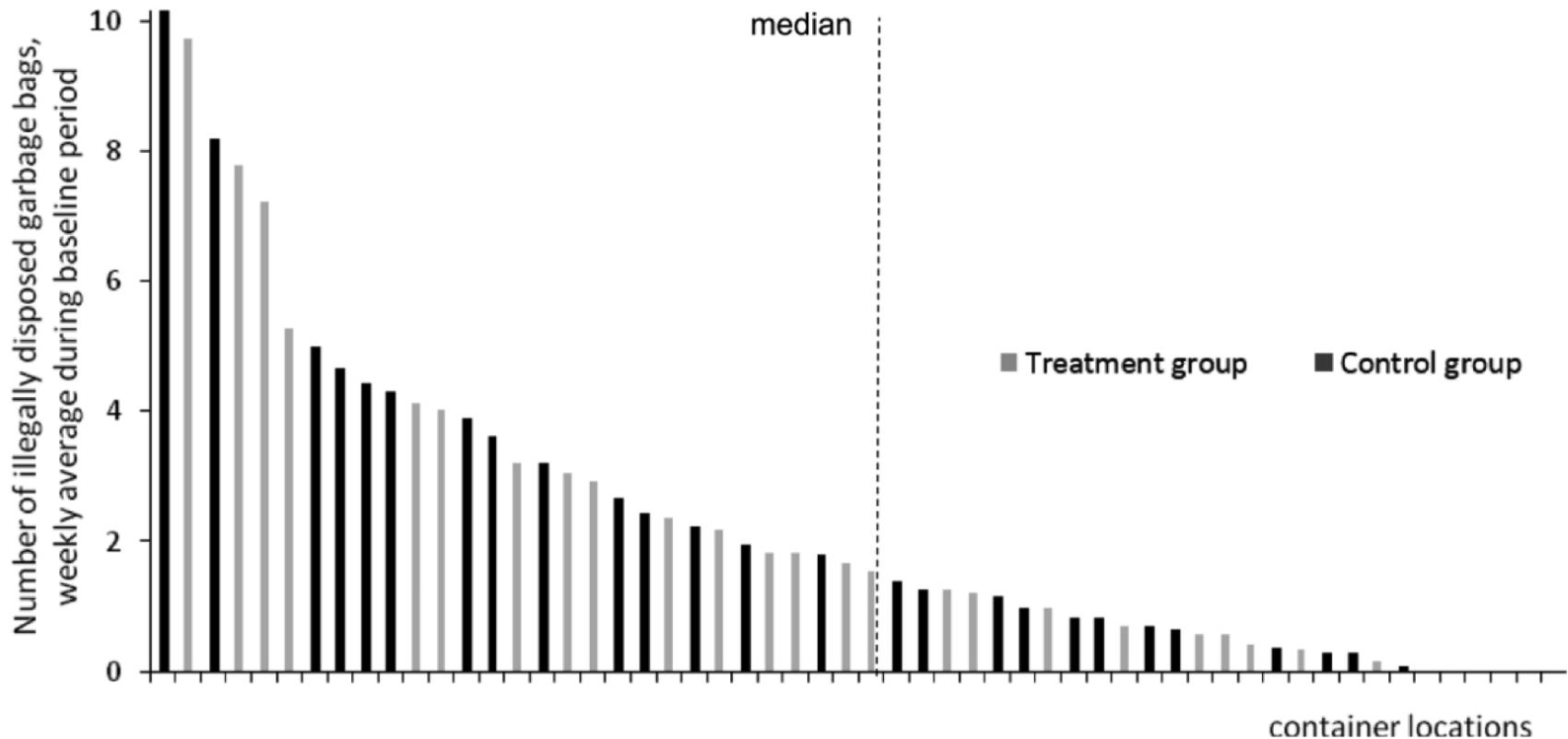


Fig. 4. Average number of illegally disposed garbage bags per location per week, pre-treatment period.

Results

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215

Table 1
Baseline characteristics and randomization check.

	Treatment locations	Control locations	P-value difference
Illegally disposed garbage bags (m ³)	0.22 (0.29)	0.21 (0.30)	0.84
Illegally disposed garbage (m ³) ^a	0.61 (0.76)	0.62 (0.76)	0.87
Number of searched bags	0.65 (1.74)	0.41 (1.61)	0.28
Number of detected offenders	0.11 (0.49)	0.06 (0.31)	0.42
Number of container locations	28	28	
Number of observations	112	112	

Note. Observations by container location and week. Standard deviation between parentheses. Baseline period is August 11–September 7, 2013.

^a Includes garbage bags, disposed household items, and paper and glass.

3.5. Randomization check

Results

Table 2

The effect of warning labels on illegal disposal of garbage.

Dependent variable: rate of illegally disposed garbage	(1) Overall	(2) By am/pm round	(3) By type of location	(4) By pre-treatment level
Treatment†	−0.29* (0.17)			
Treatment * a.m. round		−0.55** (0.25)		
Treatment * p.m. round		0.10 (0.11)		
Treatment * garbage bag disposal locations			−0.50* (0.30)	
Treatment * glass/paper disposal locations			−0.10 (0.15)	
Treatment * cleanest locations				−0.41 (0.28)
Treatment * messiest locations				−0.11 (0.11)

Note. (†) Here and in all following instances, 'Treatment' is defined as $T_i P_t$, treatment group multiplied by treatment period. Observations by container location and week. Number of observations is 504. Between parentheses standard errors clustered by container locations. Not shown are estimation results for location-fixed effects and week-fixed effects. Further, column (3) includes the interaction between the indicator variable for the type of location and the treatment period; column (4) the interaction between the indicator variable for an above-median baseline illegal disposal and the treatment period.

*p < 0.10; **p < 0.05; ***p < 0.01.

Levitt (1997)



Using Police Hiring During Election Cycles as a Natural Experiment

- Levitt uses the fact that police hiring is often used as a political tool during election cycles
- Levitt wanted to try to break the endogeneity loop, where crime affects allocation of police, by leveraging the fact that more police are hired during electoral cycles, before mayoral/municipal and gubernatorial elections
- If this increase in policing is quasi-random and not endogenous, then it provides useful treatment variation to isolate the effect of police on crime, while ignoring the back channel of crime affecting the allocation of police

Police Hiring: Election Cycles vs Non-Election Cycles

276

THE AMERICAN ECONOMIC REVIEW

JUNE 1997

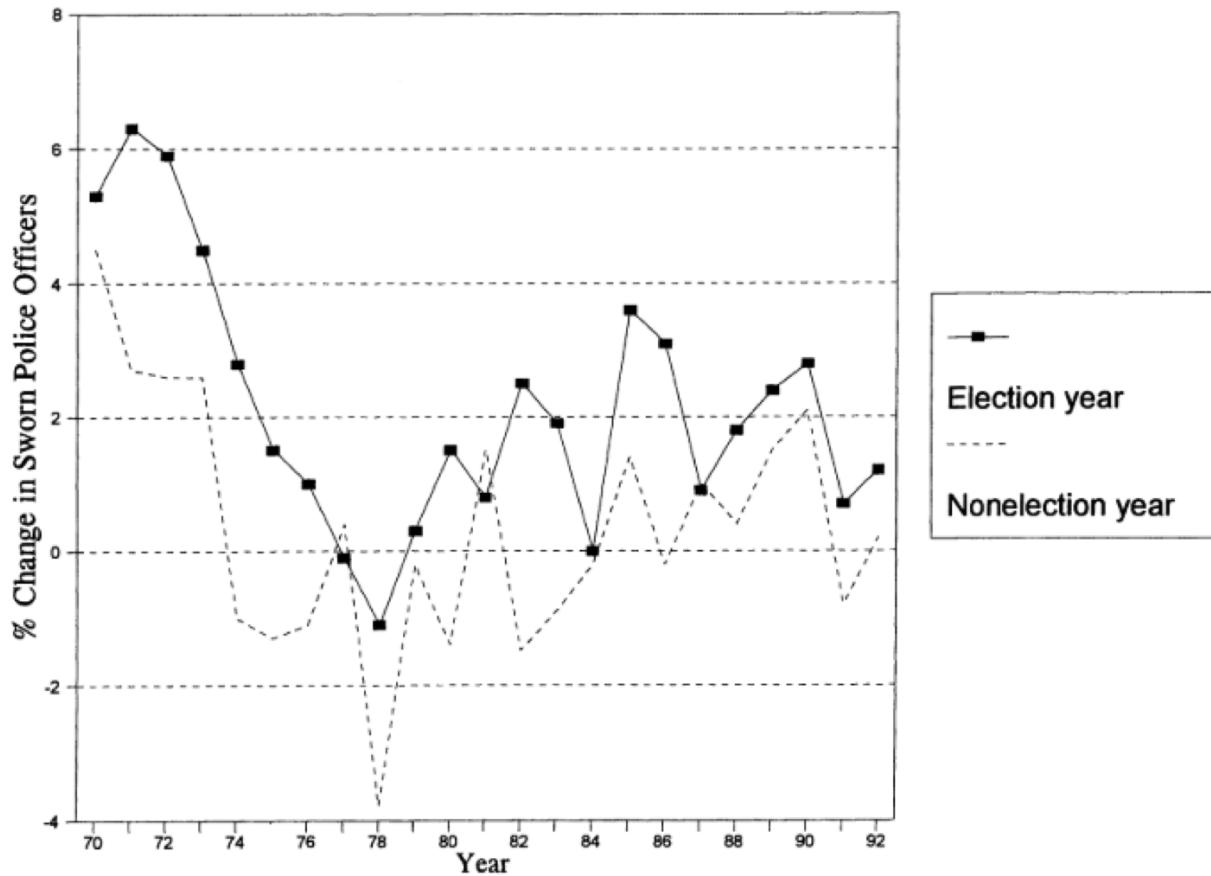


FIGURE 2. YEARLY CHANGES IN SWORN POLICE (ELECTION YEARS VERSUS NONELECTION YEARS)

Instrumental Variable (IV) Approach

- Levitt uses an instrumental variable (IV) approach to estimate the effect of police on crime
- The IV approach is a way to try to break the endogeneity loop
- The idea is to find an instrumental variable (an IV) that is related to your X variable (# of police) but is only related to your Y variable (crime) through its effect of the IV on X
- I.e. the IV cannot have an independent effect on Y
- The IV can only affect Y through X

Instrumental Variable (IV) Approach

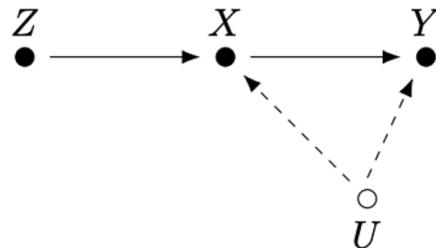


Figure 3: Instrumental Variables

- This is a directed acyclic graph (DAG) that shows the relationship between the IV, the X variable, and the Y variable
- $Z = \text{IV}$ (election cycles)
- $X = \# \text{ of police}$
- $Y = \text{crime}$
- $U = \text{confounding factors that affect crime}$
- In this case, we should have an arrow going from Y to X

Results

TABLE 3—ESTIMATES OF THE ELASTICITY OF VIOLENT CRIME RATES WITH RESPECT TO SWORD POLICE OFFICERS

Variable	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) LIML
In Sworn officers per capita	0.28 (0.05)	-0.27 (0.06)	-1.39 (0.55)	-0.90 (0.40)	-0.65 (0.25)	-1.16 (0.38)
State unemployment rate	-0.65 (0.40)	-0.25 (0.31)	-0.00 (0.36)	-0.19 (0.33)	-0.13 (0.32)	-0.02 (0.33)
In Public welfare spending per capita	-0.03 (0.02)	-0.03 (0.02)	-0.03 (0.02)	-0.03 (0.02)	-0.02 (0.02)	-0.03 (0.02)
In Education spending per capita	0.04 (0.07)	0.06 (0.06)	0.02 (0.07)	0.03 (0.07)	0.05 (0.06)	0.03 (0.06)
Percent ages 15–24 in SMSA	1.43 (1.00)	-2.61 (3.71)	-1.47 (4.12)	-2.55 (3.88)	-2.02 (3.76)	-1.50 (3.86)
Percent black	0.010 (0.003)	-0.017 (0.011)	-0.034 (0.015)	-0.025 (0.013)	-0.022 (0.012)	-0.031 (0.013)
Percent female-headed households	0.003 (0.006)	0.007 (0.023)	0.040 (0.030)	0.023 (0.027)	0.018 (0.025)	0.033 (0.027)
Data differenced?	No	Yes	Yes	Yes	Yes	Yes
Instruments:	None	None	Elections	Election * city-size interactions	Election * region interactions	Election * region interactions
P-value of cross-crime restriction on police elasticity	<0.01	<0.01	0.09	0.13	0.33	0.28

Notes: Dependent variable is $\Delta \ln$ crime rate per capita for one of the four violent crimes (murder and nonnegligent manslaughter, rape, robbery, and aggravated assault), except in column (1) where log-levels, rather than log-differences, are used. Right-hand-side variables also are differenced in columns (2)–(6). Estimates are obtained estimating all crime categories jointly, allowing for a city-fixed effect across crime rates and heteroskedasticity across crime categories. The reported parameter estimates are constrained to be the same across all violent crime. Corresponding results for property crime are reported in Table 4. Number of observations is 1,136 per crime category. Crime-specific year dummies, region dummies, and city-size indicators also are included in all regressions. The reported coefficient for sworn officers is the sum of the contemporaneous and once-lagged coefficients. In columns (3)–(6), sworn officers are treated as endogenous. Column (3) instruments using mayoral and gubernatorial election-year indicators. Column (4) instruments using interactions between the city-size indicator variables and mayoral and gubernatorial elections. Columns (5) and (6) instruments using interactions between region dummies and mayoral and gubernatorial elections. The last row of the table reports the p-value of the restriction that the effect of sworn officers is identical across all four crime categories.

- The main estimates are in columns (3) to (6)
- These use the IV approach
- Columns (1) and (2) just estimate the general effect of police on crime, without using the instrumental variable
- We are concerned that the estimates from (1) and (2) are biased due to endogeneity
- Using IV makes the estimate much more negative -> police have a big effect on reducing crime

Results

- Levitt finds that the increase in police hiring during electoral cycles is associated with a substantial reduction in violent crime
- The impacts on property crime are smaller

Sullivan and O'Keeffe (2017)



The One Where the NYC Police Goes on Strike

- In 2014-2015, like now, there was intense political discussions around police brutality, racism, violent protests, and how policing policy should be changed
- NYPD stopped proactive policing (systematic and aggressive enforcement of low-level violations) in late 2014 to early 2015
- This was a “work slowdown” for seven weeks to try to show how valuable NYPD was.
- (Narrator: they did not show this)
- This is a great paper to study what the effects of proactive policing are. Should we believe the “broken window” theory, or is the narrative that proactive policing increases - criminality correct?

The One Where the NYC Police Goes on Strike

- The researchers compare crime before, during, and after this “work slowdown” to crime levels during the same time of the year in a prior year
- This is like a DiD, so it comes down to if the seasonal pattern in crime would have been the same in May 2013 to May 2014 (control) compared to May 2014 to May 2015 (treated) had “treatment” not occurred
- Key results
- The figures I’ll show you -> the work slowdown did reduce proactive policing
- The table I’ll show you -> this seems to have caused a decrease in complaints of more serious crimes

Results

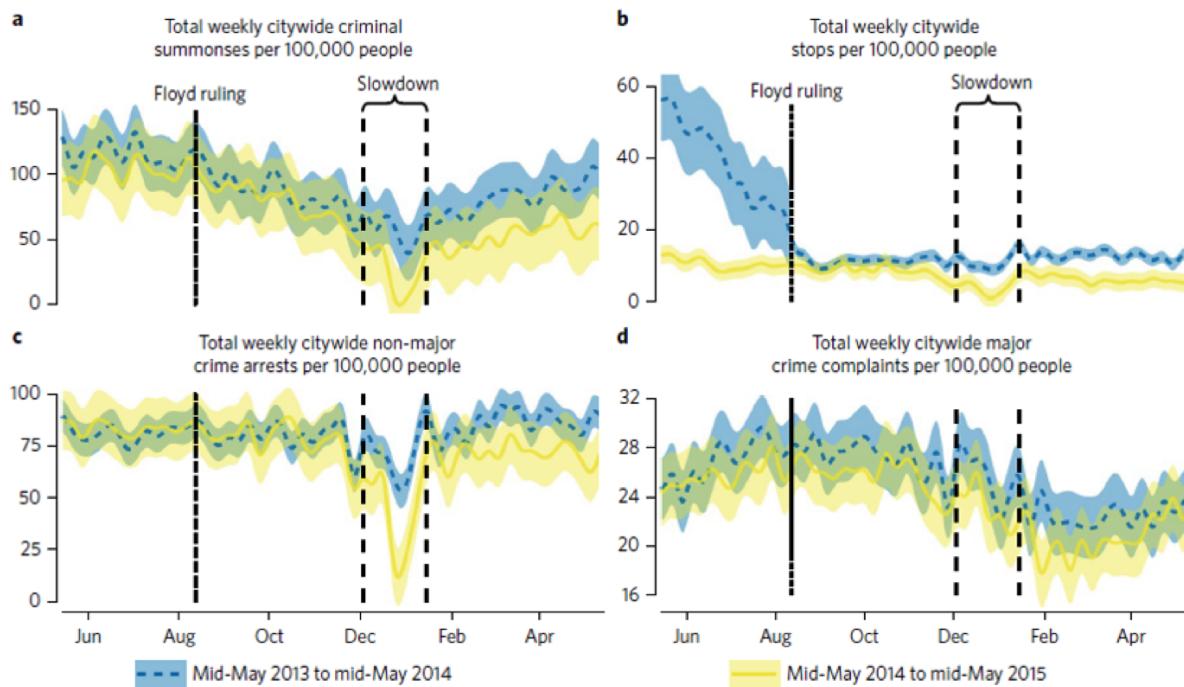


Fig. 1 | Temporal variation in policing and crime complaints in NYC. a-d, Graphs showing total weekly citywide activity over time. The titles refer to y axes; the x axis is time; the original unit is one week, but days are plotted. The line colours and types correspond to different series: the dashed blue lines run from 15 May 2013 to 14 May 2014; the solid yellow lines run from 15 May 2014 to 14 May 2015. The blue and yellow lines are from a natural cubic spline fit through all weekly citywide data points (aggregated from 76 precincts), with each week being a knot. Fifty-two knots are plotted per series per model, derived from an original 7,904 precinct-week observations per variable. The long-dashed black lines delineate the NYPD slowdown weeks (1 December to 19 January), which is the primary comparison period of interest between the two series. The short-dashed black lines indicate the calendar day of the 'Floyd versus City of New York' ruling, 12 August. The shaded ribbons represent one standard deviation in the variable above and below the interpolated value. For models a, c and d, separate standard deviations are calculated by series (N per series per model = 52). In model b, separate standard deviations in per capita stop, question and frisks are calculated for the 13 weeks before, and 39 after, the 12 August 2013 'Floyd' ruling in the first (blue) series, and for all 52 points in the second (yellow) series. Criminal summonses are misdemeanour and summary offences. Major crimes are murder, rape, robbery, felony assault, burglary, grand larceny and grand theft auto; non-major crimes are all other arrestable crimes.

Results

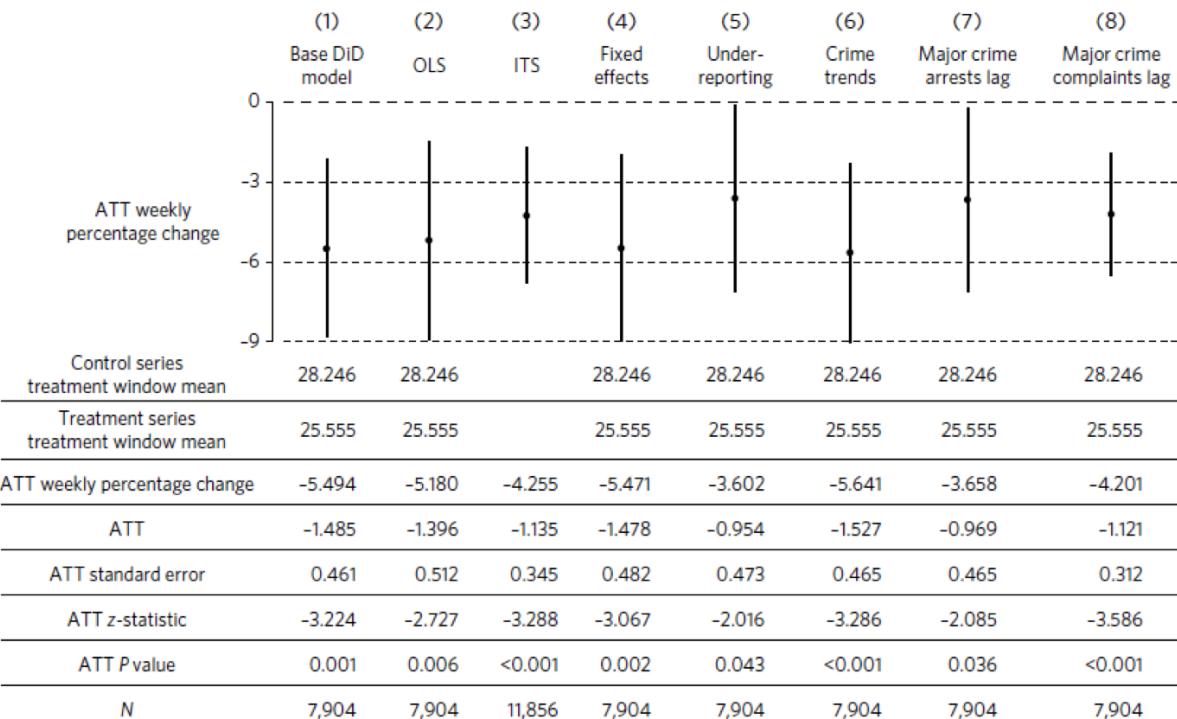


Fig. 3 | Effect of slowdown on major crime complaints. The outcome variable is the number of major crime complaints per week per precinct. All models (1)–(8) use negative binomial (NB2) regression, except (2), which uses ordinary least squares (OLS). For models using difference-in-differences (DiD), (1), (2) and (4)–(8), the series and treatment windows are the same as those in Fig. 2. The ITS model (3) specifies the ‘Intervention’ as starting on 30 November 2014, and the ‘Post-intervention’ period beginning on 19 January 2015. All models use all covariates described in the text for the base specification of model (1), except models (4) and (5), which exclude time-invariant predictors. Model (3) adds month dummies, and (4) and (5) add precinct dummies. Model (5) adds misdemeanour and violation complaints, and (6) adds the percentage change in weekly precinct major crime complaints between 2012 and 2011, and 2013 and 2012. Model (7) adds a one-week lag of major crime arrests, and (8) adds a one-week lag of major crime complaints. Standard errors for all models except (2) are calculated using the delta method, where the gradient is the exponentiated ‘Intervention’ coefficient. For more information, see the note for Fig. 2.

Concluding Thoughts on Policing and Crime



Comparing External Validity

- An important way to compare these studies is based on external validity
- Do they tell us about policing or police policy more broadly?
- Or are they very specific case studies that are only externally valid for similar cases?
- How broadly can we apply the lessons learned from this paper?

Comparing External Validity

- My hot take is that the papers can be sorted in this way, from most externally valid to least externally valid:
 1. Levitt (uses variation that tells us about the effects of police in general, uses a national set of data)
 2. Sullivan and O'Keeffe (only issue is that it's NYC only, and NYC could be unique)
 3. Cheng and Long (NOLA is rather unique, as is the FQ)
 4. Di Tella and Schargrodsy (this isn't policing so much as armed guards outside synagogues)
 5. Dur and Vollaard (it's basically a case study of trash enforcement practices in a western European city)

Comparing Exogeneity

- Another important way to compare these papers is in terms of how exogenous the treatment variation is
 - Was the variation in police or policy random? Close to random? Or could it have been endogenous to something?
- Cheng and Long – FQ Task Force – For this paper it comes down to the parallel trends assumption since this is a DiD
 - E.g., were pre-existing time trends in the FQ similar to those in the control group neighborhoods?
 - Were any other changes going on that differentially impacted one neighborhood over others over time?
- It's a big subjective as to if the assumption holds or not

Comparing Exogeneity

- Di Tella and Schargrodsy – Terrorist Attack
 - This paper leverages a fairly random event – a terrorist attack
 - The terrorist attack led to the gov’t adding police outside of synagogues
 - This is a DiD study, so the parallel trends assumption is important again
 - The “treatment” here is likely exogenous to LOCAL crime trends in those neighborhoods
 - So, I don’t have concerns that blocks adjacent to a synagogue and blocks just a bit further away, for example, had different trends in crime
 - Seems like a pretty good “natural experiment”

Comparing Exogeneity

- Dur and Vollaard – Trash Enforcement RCT
 - This is a randomized control trial (RCT), i.e. an experiment, that seems well done (e.g., properly randomized)
 - This paper uses the most exogenous treatment variation for that reason

Comparing Exogeneity

- Levitt – More police hired before elections
- Levitt exploits the fact that more police were hired before mayoral/municipal elections
- To some extent, this hiring is exogenous, since it is related to electoral cycles
- However, the increase in hiring due to an electoral cycle could vary in an endogenous way
- E.g., pre-election hiring increases more in higher crime areas? In red states? Etc
- Given this, while Levitt may use treatment variation that is more exogenous than more naïve approaches, there are still some concerns

Comparing Exogeneity

- Sullivan and O'Keeffe – NYC police strike and effect on serious crime
- NYPD had a seven week “work slowdown”, where proactive policing was reduced
- The researchers compare crime before, during, and after this “work slowdown” to crime levels during the same time of the year in a prior year
- This is like a DiD, so it comes down to if the seasonal pattern in crime would have been the same in May 2013 to May 2014 (control) compared to May 2014 to May 2015 (treated) had - “treatment” not occurred
- Was the seasonal pattern and trend in crime the same in May 2013 to May 2014 as it would have been in May 2014 to May 2015, absent the work slowdown?
- Seems relatively likely and the figures show similar trends, with the effects during the work slowdowns being clear outliers
- Generally seems exogenous

Comparing Exogeneity

- My hot take is that the papers fall in this range from most exogenous to least:
 1. Dur and Voollaard (it's an RCT!)
 2. Di Tella and Schargrodsy
 3. Sullivan and O'Keefe
 4. Cheng and Long (to be clear, this paper maybe only has mild concerns)
 5. Levitt (there are endogeneity concerns and other economists have brought this up)

Measuring the Effect of Economic Circumstances on Crime



Yang (2017)



Palmer, Phillips, Sullivan (2019)

