Effect of Police on Crime & Economic Circumstances on Crime

Police, Economic Circumstances, and Crime

Hussain Hadah (he/him)
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Tulane University

Outline for Today

1. **Discuss Jigsaw Papers**

2. **Summarize Yang (2017)**

3. **Summarize Palmer, Phillips, Sullivan (2019)**

Tulane University

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

C. Cheng, W. Long / Journal of Public Economics 164 (2018) 1–18

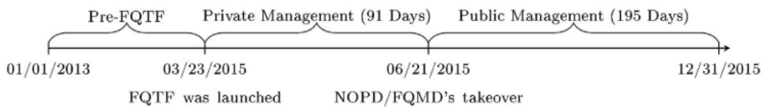

Fig. 2. Timeline of the French Quarter Task Force.

Check and Long: Treatment

- They study susceptible crimes.
- susceptible crimes are those that could be affected by the presence of the FQTF patrols:
  - Roberry
  - Aggravated Assault
  - Bulglary
  - 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 expirement
- 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

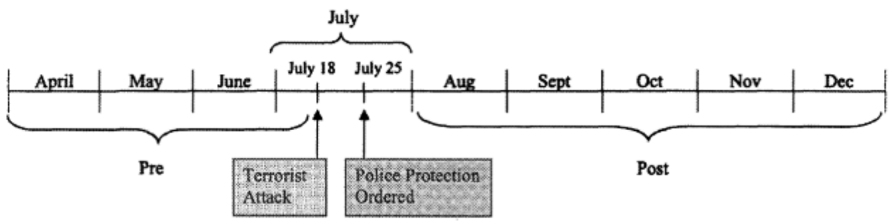

FIGURE 1. TIMELINE OF EVENTS

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 their 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)	(D)	(E)
Same-Block Police	−0.07752***	−0.08007***	−0.08080***	−0.07271***	−0.05843***
	(0.022)	(0.022)	(0.022)	(0.011)	(0.022)
One-Block Police		−0.01325	−0.01398	−0.01158	−0.00004
		(0.013)	(0.014)	(0.010)	(0.013)
Two-Blocks Police			−0.00218	−0.00342	0.01701
			(0.012)	(0.009)	(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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*** Significant at the 1-percent level.

- 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

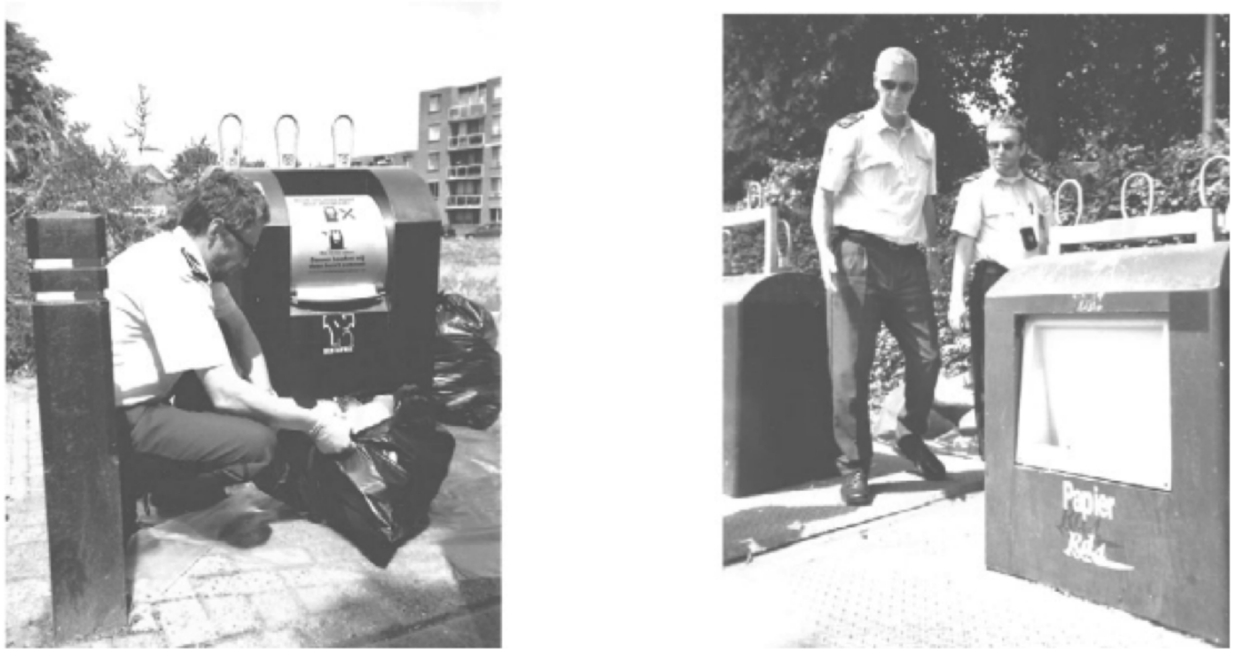


Fig. 1. Garbage bag disposal container (left) and paper and glass disposal container (right), featuring officers.

RCT on waste disposal enforcement

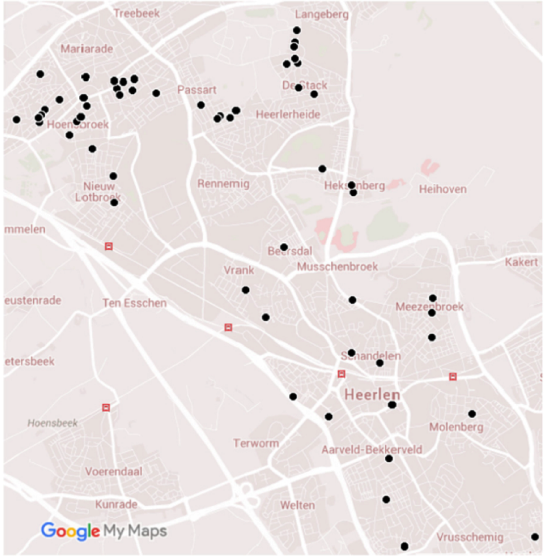


Fig. 2. Map of the area including the 56 container locations.

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

Treatment vs. Control

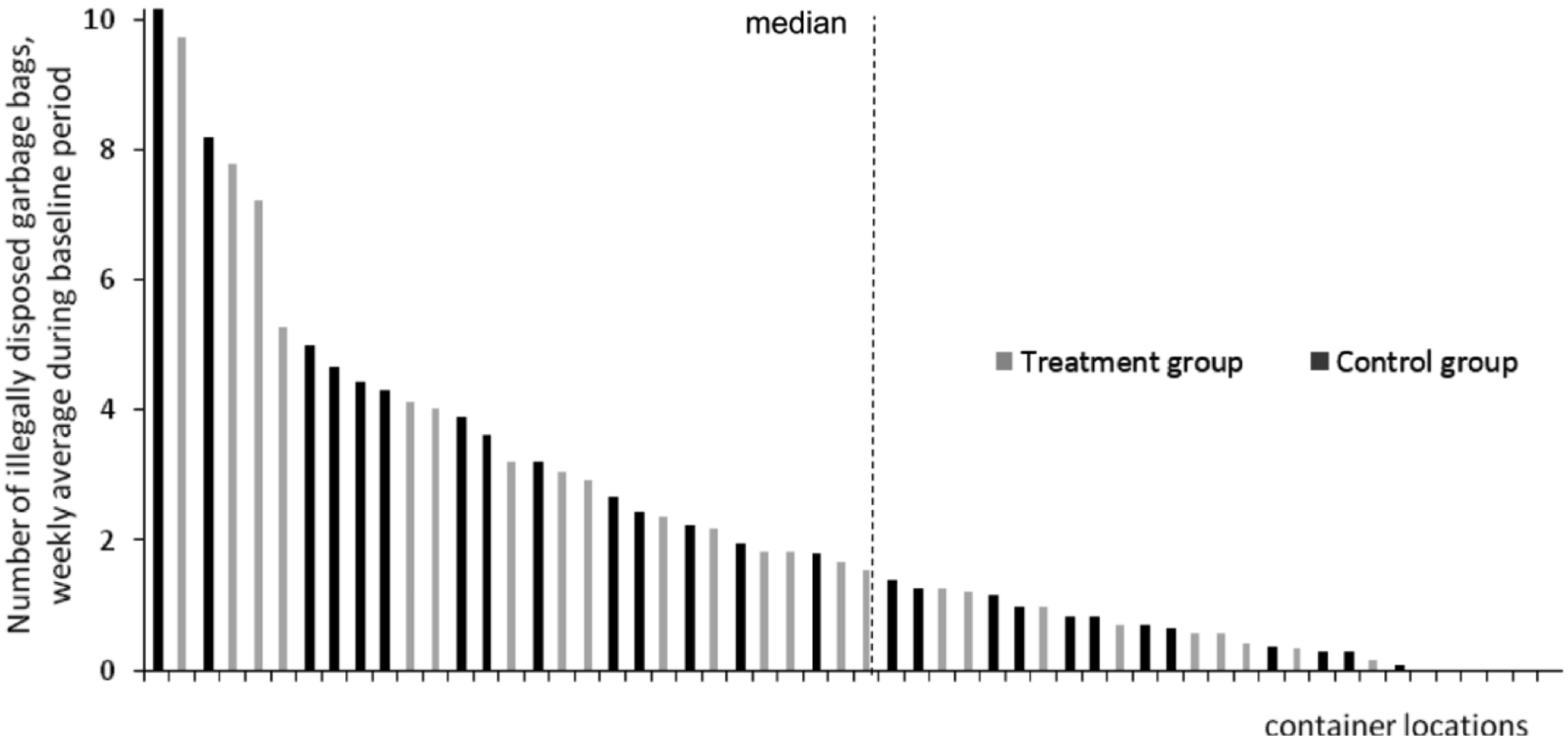


Fig. 4. Average number of illegally disposed garbage bags per location per week, pre-treatment period.

Results

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

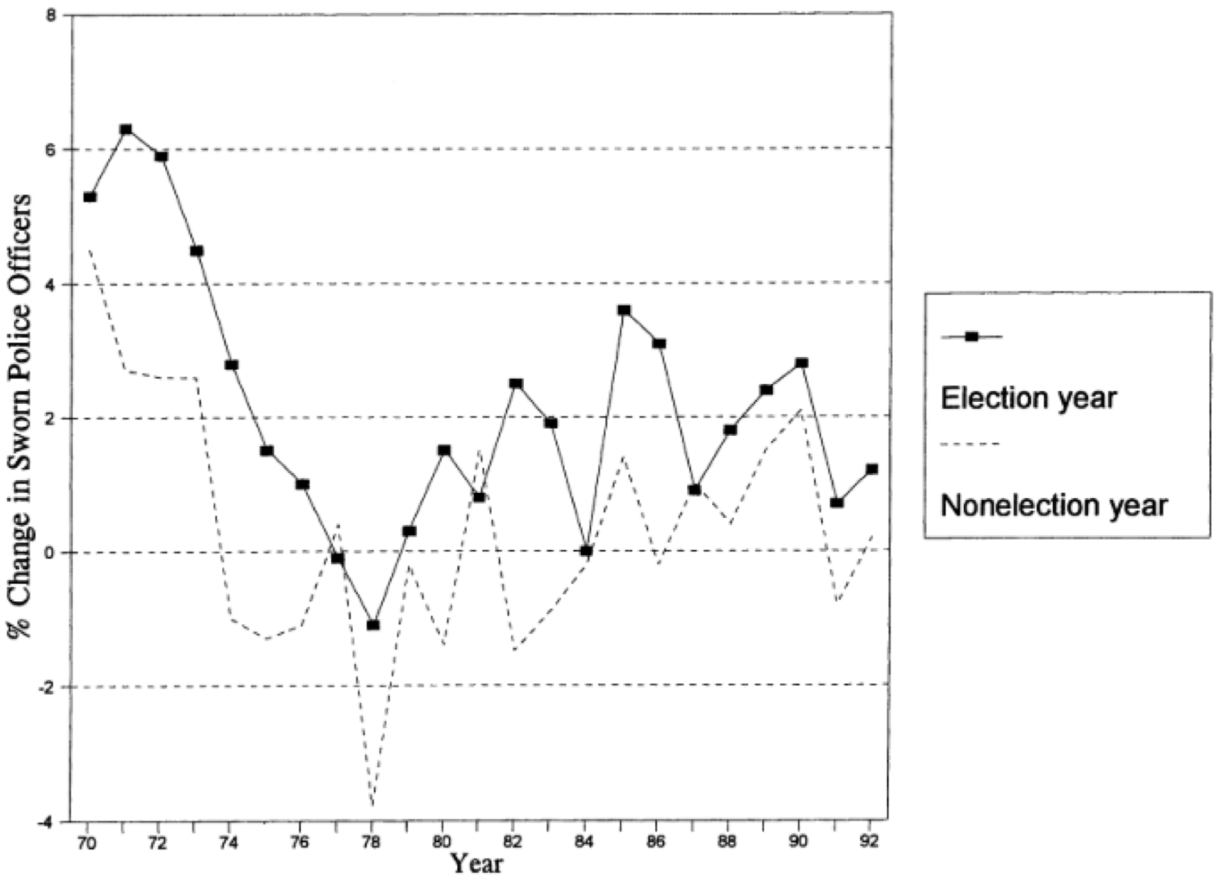


FIGURE 2. YEARLY CHANGES IN SWORN POLICE (ELECTION YEARS VERSUS NONELECTION YEARS)

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 it 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

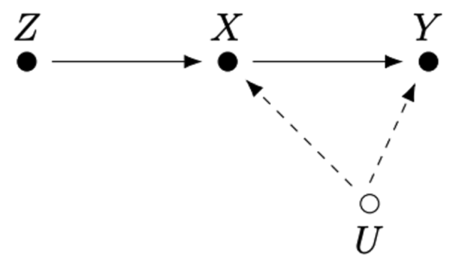


Figure 3: Instrumental Variables

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

Results

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

Variable	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) LIML
ln 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)
ln 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)
ln 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 Δln 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 as 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

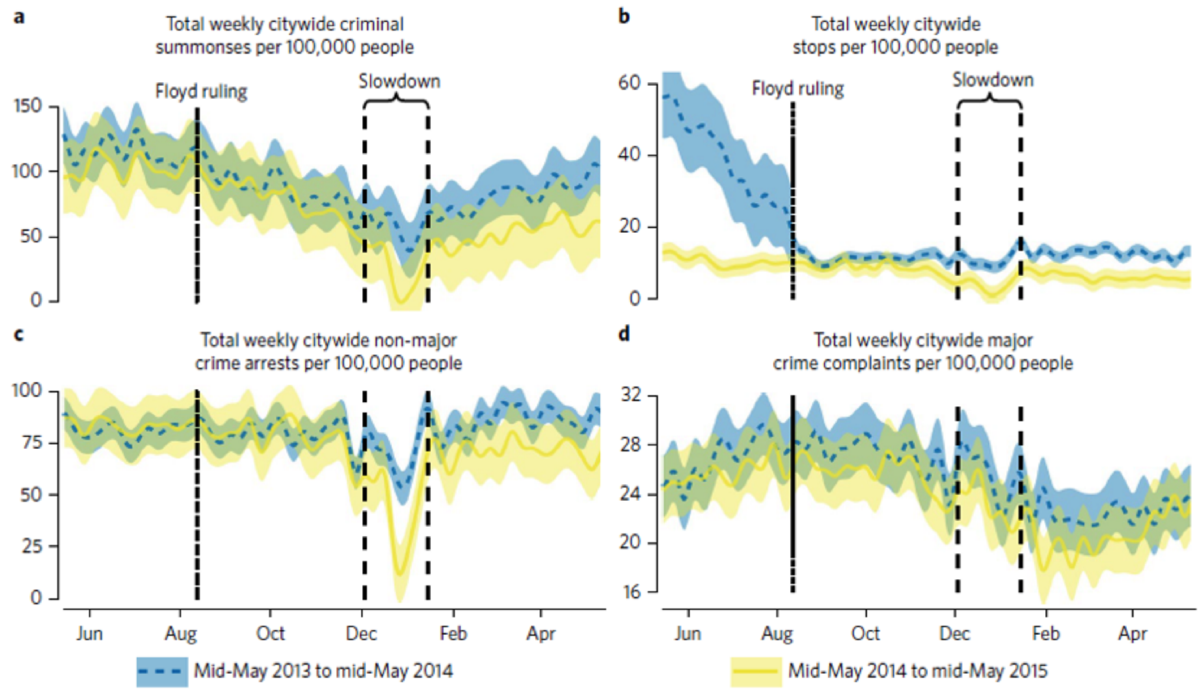
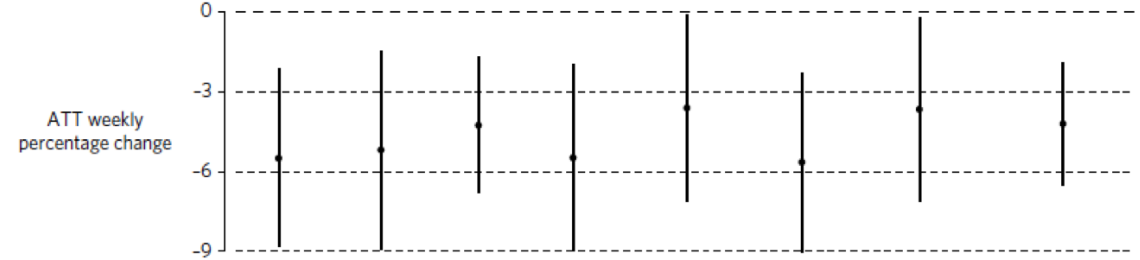


Fig. 1 | Temporal variation in policing and crime complaints in NYC. a–d, Graphs showing total weekly citywide activity over time. The titles refer to *y* axes; the *x* axis is time; the original unit is one week, but days are plotted. The line colours and types correspond to different series: the dashed 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



	(1) Base DiD model	(2) OLS	(3) ITS	(4) Fixed effects	(5) Under-reporting	(6) Crime trends	(7) Major crime arrests lag	(8) Major crime complaints lag
Control series treatment window mean	28.246	28.246		28.246	28.246	28.246	28.246	28.246
Treatment series treatment window mean	25.555	25.555		25.555	25.555	25.555	25.555	25.555
ATT weekly percentage change	-5.494	-5.180	-4.255	-5.471	-3.602	-5.641	-3.658	-4.201
ATT	-1.485	-1.396	-1.135	-1.478	-0.954	-1.527	-0.969	-1.121
ATT standard error	0.461	0.512	0.345	0.482	0.473	0.465	0.465	0.312
ATT *z*-statistic	-3.224	-2.727	-3.288	-3.067	-2.016	-3.286	-2.085	-3.586
ATT *P* value	0.001	0.006	<0.001	0.002	0.043	<0.001	0.036	<0.001
N	7,904	7,904	11,856	7,904	7,904	7,904	7,904	7,904

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 be 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 Schargrodsky (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 Schargrodsky – 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 exogeneous treatment variation for that reason

Comparing Exogeneit

- 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 then 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 exogeneous

Comparing Exogeneity

- My hot take is that the papers fall in this range from most exogeneous to least:

1. Dur and Voollaard (it's an RCT!)
2. Di Tella and Schargrodsky
3. Sullivan and O'Keeffe
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

Tulane University

Yang (2017)

Abstract

Abstract: “This paper estimates the impact of local labor market conditions on criminal recidivism using administrative prison records on four million offenders released from 43 states between 2000 and 2013. Exploiting the timing of each offender’s release from prison, I find that being released to a county with higher low-skilled wages significantly decreases the risk of recidivism. The impact of higher wages on recidivism is larger for both black offenders and first-time offenders, and in sectors that report being more willing to hire ex-offenders. These results are robust to individual- and county-level controls, such as policing and corrections activity, and do not appear to be driven by changes in the composition of released offenders during good or bad economic times.”

Summary Statistics

- This is a summary statistics table showing you what her data looks like
- This one shows facts about how often people return to prison (recidivate)

Table 1
Distribution of time until return to prison.

	No. of obs	Probability of return to prison in			
		≤ 1 year	≤ 2 years	≤ 3 years	≤ 5 years
All prisoners	4,029,781	0.146	0.227	0.268	0.304
Demographics					
White	1,888,533	0.139	0.216	0.254	0.289
Black	1,491,470	0.148	0.240	0.288	0.331
Hispanic	701,319	0.139	0.202	0.230	0.252
Male	3,501,023	0.151	0.235	0.278	0.315
Female	527,741	0.113	0.172	0.202	0.230
Age under 25	825,430	0.204	0.311	0.362	0.404
Age 25–40	1,974,349	0.143	0.224	0.266	0.304
Age over 40	1,229,591	0.112	0.174	0.207	0.235
Less HS degree	1,326,984	0.136	0.227	0.275	0.322
HS degree	1,064,684	0.126	0.200	0.238	0.273
College degree	27,073	0.077	0.124	0.150	0.180
Prior felony incarceration	662,673	0.153	0.230	0.270	0.307
No prior felony	2,148,616	0.141	0.221	0.261	0.297
Type of offense					
Violent offense	979,874	0.139	0.219	0.260	0.296
Property offense	1,120,922	0.178	0.268	0.311	0.349
Drug offense	1,168,453	0.131	0.209	0.250	0.285
Reason for first prison spell admittance					
Court commitment	3,279,972	0.136	0.214	0.253	0.288
Parole revocation	199,508	0.211	0.328	0.383	0.427
Probation revocation	322,983	0.194	0.292	0.341	0.385
Reason for first prison spell release					
Discretionary parole	1,177,321	0.166	0.260	0.302	0.335
Mandatory parole	767,042	0.236	0.336	0.382	0.415
Shock probation	415,490	0.126	0.218	0.266	0.308
Expiration of sentence	1,069,258	0.049	0.101	0.138	0.180

Notes: This table presents descriptive statistics for the unconditional probabilities of returning to prison for the full sample of prisoners released between 2000–2013 in 43 states.

Summary Statistics

- This is another summary statistics table, showing what her sample looks like
- E.g., what is the demographic and educational make-up of her sample?
- What kind of offenses were committed?

Table 2
Summary statistics of prisoners released 2000–2013.

	Offender sample		Offender-quarter sample	
Variable	Mean	SD	Mean	SD
NCRP data				
White	0.498	0.500	0.502	0.500
Black	0.393	0.488	0.391	0.488
Hispanic	0.197	0.398	0.201	0.401
Male	0.869	0.337	0.864	0.342
Female	0.131	0.337	0.136	0.342
Age at release	34.383	10.636	34.802	10.658
Less HS degree	0.511	0.500	0.516	0.500
HS degree	0.410	0.492	0.406	0.491
Some college	0.063	0.243	0.063	0.244
College degree	0.010	0.102	0.011	0.103
Prior felony incarceration	0.236	0.424	0.230	0.420
Violent offense	0.245	0.430	0.243	0.429
Property offense	0.280	0.449	0.269	0.444
Drug offense	0.292	0.455	0.301	0.459
Number of counts	1.234	1.314	1.225	1.302
Total sentence (years)	4.718	6.123	4.709	6.222
Time served (years)	2.161	3.289	2.173	3.287
Court commitment	0.831	0.375	0.838	0.368
Parole revocation	0.051	0.219	0.048	0.214
Probation revocation	0.082	0.274	0.079	0.270
Discretionary parole	0.306	0.461	0.284	0.451
Mandatory parole	0.199	0.399	0.192	0.394
Shock probation	0.108	0.310	0.107	0.309
Expiration of sentence	0.278	0.448	0.312	0.463
Missing crime Characteristics	0.006	0.078	0.007	0.083
Missing race	0.059	0.236	0.057	0.232
Missing Hispanic	0.116	0.320	0.123	0.329
Missing education	0.356	0.479	0.344	0.475
Missing prior	0.302	0.459	0.305	0.460
Labor market variables (in logs)				
Low-skilled wages	7.369	0.149	7.369	0.151
Low-skilled construction wages	7.454	0.204	7.451	0.204
Low-skilled manufacturing wages	7.512	0.200	7.515	0.201
Low-skilled transportation wages	7.380	0.179	7.380	0.180
Low-skilled finance wages	7.676	0.230	7.679	0.232
Low-skilled professional services wages	7.617	0.231	7.622	0.232
Low-skilled management wages	7.630	0.302	7.638	0.305

Notes: This table presents summary statistics on the full sample of released prisoners from 2000–2013 from 43 states. The offender sample contains one observation per prisoner and labor market summary statistics are presented for the quarter of release. The offender-quarter sample contains one observation for each quarter out of prison.

Methodology

- Yang's general approach is a version of a difference-in-differences
- The idea to compare people released from prison in the same county in good economic conditions versus bad economic conditions
- Yang measures economic conditions through wages in low skilled jobs
- These are the jobs that are most likely to hire those with criminal records
- By looking at people within the same county, during times with higher vs. lower wages, it removes any bias for the fixed differences between counties
    - Recidivism rates and other factors may be different between counties
- Comparisons between, rather than within counties would be more of an "apples to oranges" comparison
- Like other DiD examples, where there are fixed differences that exist between groups

Methodology

- An assumption is required for Yang's approach to provide an unbiased estimate of the causal effect of local economic conditions on crime
- The assumption is that when comparing those within the same county in good and bad economic times, there are no differences other than the different economic circumstances
- The ideal would be like a randomized control trial (RCT) $\rightarrow$ higher/lower wages are randomly assigned over time

Methodology

- Obviously, that's not possible
- But hopefully there are no important differences between good and bad economic times other than the economy
- Otherwise the treatment and control groups would be different. The key example of possible differences are that the types of people released during good economic times, within the same county, could differ from those released during bad economic times, within the same county
- While some of this can be controlled for in the regression analysis (i.e. control variables), any differences that are not controlled for could cause bias

Results

- This is the main results table
- Results show that if the low-skill wage is higher, then recidivism decreases (hence the negative coefficient)
- Results are very similar even when control variables are added
- Other results:
    - Blacks, non-Hispanics, younger people, those with less education, men, and those with less time served are more likely to recidivate

Table 4
Main results.

	(1)	(2)	(3)
Log low-skill wage	−0.436***	−0.435***	−0.462***
	(0.057)	(0.060)	(0.060)
Black		0.133***	0.159***
		(0.008)	(0.009)
Not Hispanic		0.240***	0.223***
		(0.023)	(0.021)
Female		−0.304***	−0.309***
		(0.014)	(0.009)
HS degree		−0.066***	−0.077***
		(0.016)	(0.017)
Some college		−0.131***	−0.151***
		(0.016)	(0.016)
College degree		−0.294***	−0.301***
		(0.027)	(0.027)
Age at release		−0.049***	−0.044***
		(0.004)	(0.000)
No prior felony		−0.516***	−0.469***
		(0.038)	(0.047)
Time served (years)			−0.012***
			(0.004)
Observations	34,872,568	34,872,568	34,872,568
Defendant controls	No	Yes	Yes
Crime controls	No	No	Yes

Notes: This table presents proportional hazard estimates for the sample of prisoners released between 2000–2013 in 43 states. Each column represents a separate regression. Column 2 adds controls for defendant demographics: race, ethnicity, gender, age, age squared, highest graded completed, prior felony incarceration indicator. Column 3 adds controls for crime and prison characteristics: main offense type, number of convicted counts, total sentence imposed, type of prison admission, type of facility, reason for release, time served, time served squared. All specifications include year and county fixed effects. Standard errors are clustered at the county level.
*** significant at 1 percent level.

Results

Table 5
Results by industry.

	(1)	(2)	(3)	(4)	(5)	(6)
Construction log low-skill wage	−0.164***					
	(0.040)					
Manufacturing log low-skill wage		−0.231***				
		(0.060)				
Transportation log low-skill wage			0.007			
			(0.040)			
Finance log low-skill wage				0.089***		
				(0.035)		
Prof. services log low-skill wage					−0.064	
					(0.048)	
Management log low-skill wage						0.018
						(0.026)
Other log low-skill wage	−0.308***	−0.291***	−0.470***	−0.584***	−0.422***	−0.585***
	(0.080)	(0.067)	(0.069)	(0.069)	(0.079)	(0.086)
Observations	34,823,482	34,713,772	34,574,189	31,979,852	32,710,100	28,660,000
Defendant controls	Yes	Yes	Yes	Yes	Yes	Yes
Crime controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents proportional hazard estimates for the sample of prisoners released between 2000–2013 in 43 states. Each column represents a separate specification controlling for industry specific county-level log wages and log wages in all other industries. I consider three low-skilled sectors most willing to hire ex-offenders: construction; manufacturing; and transportation, and three high-skilled sectors least willing to hire ex-offenders: finance and insurance; professional, scientific, and technical services; and management of companies and enterprises. Specifications include demographic, crime, and prison characteristics. All specifications include year and county fixed effects. Standard errors are clustered at the county level.
*** significant at 1 percent level.

Results: Heterogeneity

Table 6
Results by offender demographics.

	All	Male	Female	White	Black	<25	25 to 40	>40
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log low-skill wage	−0.462***	−0.463***	−0.480***	−0.364***	−0.539***	−0.415***	−0.430***	−0.502***
	(0.060)	(0.061)	(0.097)	(0.052)	(0.096)	(0.074)	(0.062)	(0.069)
3 year recidivism	0.268	0.278	0.202	0.254	0.288	0.362	0.266	0.207
Observations	34,872,568	30,139,485	4,721,248	16,465,378	12,982,650	6,612,160	17,130,434	11,127,386
Defendant controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crime controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents proportional hazard estimates for subsamples of prisoners released between 2000–2013 in 43 states. Each column represents a separate specification. Specifications include demographic, crime, and prison characteristics. All specifications include year and county fixed effects. Standard errors are clustered at the county level.
*** significant at 1 percent level.

Table 7
Results by criminal history and crime type.

	Prior felony	No prior	Violent	Property	Drug
	(1)	(2)	(3)	(4)	(5)
Log low-skill wage	−0.227**	−0.690**	−0.471***	−0.461***	−0.445***
	(0.096)	(0.079)	(0.086)	(0.067)	(0.069)
3 year recidivism	0.270	0.261	0.260	0.311	0.250
Observations	5,533,463	18,762,280	8,454,298	9,353,063	10,496,821
Defendant controls	Yes	Yes	Yes	Yes	Yes
Crime controls	Yes	Yes	Yes	Yes	Yes

Notes: This table presents proportional hazard estimates for subsamples of prisoners released between 2000–2013 in 43 states. Each column represents a separate specification. Specifications include demographic, crime, and prison characteristics. All specifications include year and county fixed effects. Standard errors are clustered at the county level.
*** significant at 1 percent level.
** significant at 5 percent level.

Palmer, Phillips, Sullivan (2019)

Abstract

Abstract: “Does emergency financial assistance reduce criminal behavior among those experiencing negative shocks? To address this question, we exploit quasi-random variation in the allocation of temporary financial assistance to eligible individuals and families that have experienced an economic shock. Chicago's Homelessness Prevention Call Center (HPCC) connects such families and individuals with assistance, but the availability of funding varies unpredictably. Consequently, we can determine the impact of temporary assistance on crime by comparing outcomes for those who call when funds are available to those who call when no funds are available…

What do they do?

- Linking this call center information to arrest records from the Chicago Police Department, we find some evidence that total arrests fall between 1 and 2 years after the call
- For violent crime, police arrest those for whom funds were available 51% less often than those who were eligible but for whom no funds were available.
- Single individuals drive this decrease.
- The decline in crime appears to be related, in part, to greater housing stability—being referred to assistance significantly decreases arrests for homelessness-related, outdoor crimes such as trespassing

What do they do?

- However, we also find that financial assistance leads to an increase in property crime arrests
- This increase is evident for family heads, but not single individuals;
- The increase is mostly due to shoplifting; and the timing of this increase suggests that financial assistance enables some families to take on financial obligations that they are subsequently unable to meet
- Overall, the change in the mix of crime induced by financial assistance generates considerable social benefits due to the greater social cost of violence”

Call Volumes

- The researchers use “eligible calls”, which are the people who are eligible, based on the HPCC’s criteria, for the assistance
- For these people it’s almost a coin toss if they get the funding

Table 1
Call volume, HPCC, January 20, 2010-September 14, 2012.

Sample composition	N	% funds available	# prior calls	Proportion with a prior call
All calls	200,661	5.4	0.7	0.31
Eligible calls	14,819	47.9	1.1	0.47
First call within last week	12,880	48.1	0.9	0.41
First call within last six months	8655	50.0	0.3	0.15
First call since June 2009	7222	49.8	0.0	0.00

Notes: The sample restrictions for each row include the restrictions imposed in all rows above it. For example, the sample in the third row that is restricted to first calls in the last week is also restricted to eligible calls.

Funding is Random

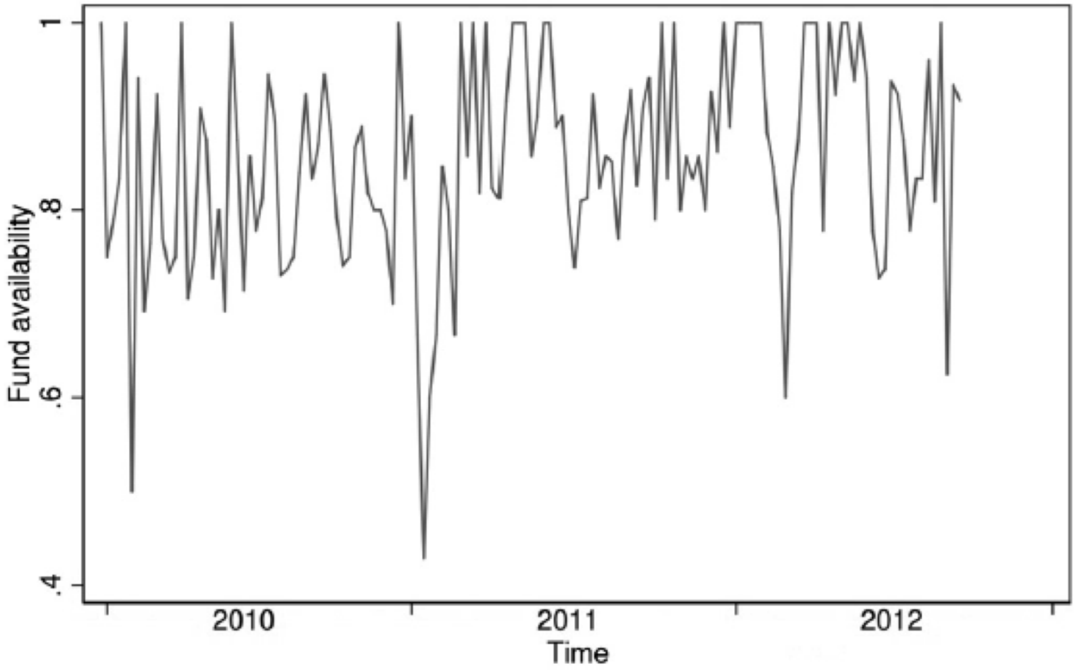


Fig. 2. Fund availability rate, by week, eligible callers to the HPCC. *Notes*: This figure is similar to Evans et al. (2016), but for a slightly different sample. Sample includes all eligible callers from 2010 to 2012 who are seeking rent assistance with need amounts between \$300 and \$900, who are non-veterans, who neither receive housing subsidies nor request more than one month of rent, who report both Social Security Numbers and family-scaled incomes below twice the poverty line, and who are not homeowners ($N = 2035$). The fund availability rate is the frequency of fund availability to those eligible callers who call within that week.

Sample of Callers

Table 3
Mean characteristics of eligible, first-time callers for all types of assistance.

Dependent variable	Control group mean	Adjusted difference
Ever arrested before call	0.32	0.0074
Arrested 1 year before call or less	0.053	0.010[a]
Arrested 1 year before call or less – Violent	0.010	0.0020
Arrested 1 year before call or less – Property	0.0069	0.0025
Arrested 1 year before call or less – Drugs	0.0099	0.0011
Arrested 1 year before call or less – Other	0.021	0.0031
Female	0.83	−0.035[c]
White, non-Hispanic	0.063	0.011[a]
Black, non-Hispanic	0.89	−0.013[a]
Other, non-Hispanic	0.041	0.00045
Hispanic	0.072	0.00099
Age	40.8	−0.73[c]
Number of adults in caller's household	1.43	−0.021
Number of minors in caller's household	1.51	−0.072[b]
Percentage in ZIP code with HS degree (standardized)	0.00098	−0.019
Labor force participation rate in ZIP code (standardized)	−0.013	0.011
Unemployment rate in ZIP code (standardized)	0.0080	−0.018
Median age in ZIP code (standardized)	−0.0053	0.0047
Monthly housing cost in ZIP code (thousands, standardized)	0.014	−0.030
Median household income in ZIP code (thousands, standardized)	0.011	−0.015
Fraction black in ZIP code (standardized)	0.0054	−0.015
Fraction white in ZIP code (standardized)	0.00084	0.0060
Fraction other races in ZIP code (standardized)	−0.017	0.032
Applying due to benefit loss	0.12	−0.0055
Applying due to inability to pay bills	0.049	−0.010[b]
Applying due to exiting shared housing	0.058	0.0038
Applying to flee abuse	0.012	0.0014
Applying due to job loss	0.25	−0.0025
Monthly income (thousands)	1.08	−0.038[b]
Receiving SNAP benefits	0.69	−0.0083
Receiving child support	0.057	−0.0024
Receiving earned income	0.50	−0.0085
Receiving SSI	0.18	−0.0045
Receiving income from TANF	0.085	0.0054
Receiving unemployment payments	0.14	0.012
Receiving other income sources	0.082	−0.0076
Living situation: rent housing	0.84	−0.012
Living situation: shared housing	0.13	0.012
Shelter inhabitancy in past 18 months	0.047	0.014[b]
N	4328	8655

Notes: Results are for our main sample. The second column shows the coefficient on fund availability from a regression of the listed baseline characteristics on a fund availability dummy and controls for fund-specific restrictions.
[a] Significant at 10%; based on heteroskedasticity-robust standard errors.
[b] Significant at 5%; based on heteroskedasticity-robust standard errors.
[c] Significant at 1%; based on heteroskedasticity-robust standard errors.

Main Results

Table 4
OLS estimates of the effect of fund availability on arrests.

	(1) 1 year	(2) 2 years	(3) 3 years
Effect on all arrests	−0.0099[a]	−0.0080	−0.0031
	(0.0058)	(0.0071)	(0.0078)
Control group mean	0.055	0.087	0.108
Effect on violent arrests	−0.0087[c]	−0.0086[b]	−0.0086[a]
	(0.0033)	(0.0041)	(0.0046)
Control group mean	0.017	0.028	0.037
Effect on property arrests	0.0021	0.0052	0.010[c]
	(0.0024)	(0.0032)	(0.0037)
Control group mean	0.007	0.015	0.019
Effect on drug arrests	−0.00039	−0.0018	−0.0023
	(0.0026)	(0.0033)	(0.0039)
Control group mean	0.012	0.020	0.026
Effect on other arrests	0.0010	−0.0027	−0.0013
	(0.0042)	(0.0054)	(0.0061)
Control group mean	0.024	0.042	0.055
Controls for characteristics related to fund availability	Yes	Yes	Yes
Controls for other observable characteristics	Yes	Yes	Yes
N	8655	8655	8655

Notes: Results are for our main sample of eligible first-time calls within the last six months for rent, security deposit, utility, and other assistance, January 20, 2010–September 14, 2012. See text for additional restrictions. Each cell shows the coefficient on funds availability from a separate regression. The outcome is a dummy for being arrested for the listed type of crime within the listed time frame. Calendar and fund availability controls include linear controls

- Effect are strongest (more statistically significant) for violent arrests
- E.g., one year after getting the funding, violent arrests are 0.0087 lower
- Compared to average rate (control group mean of 0.017), this is a decrease of about 50%!!!

Main Results

- There is an increase in property arrests three years later, due to getting the funding
- The authors argue that this may be that when the families get the funding, they get requests for that money, and they overcommit on who they promise to give money to
- This could lead to an incentive to commit shoplifting once those “debts” catch up

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OLS estimates of the effect of fund availability on arrests.

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[a] Significant at 10%.
[b] Significant at 5%.
[c] Significant at 1%.

More Results

Table 4
OLS estimates of the effect of fund availability on arrests.

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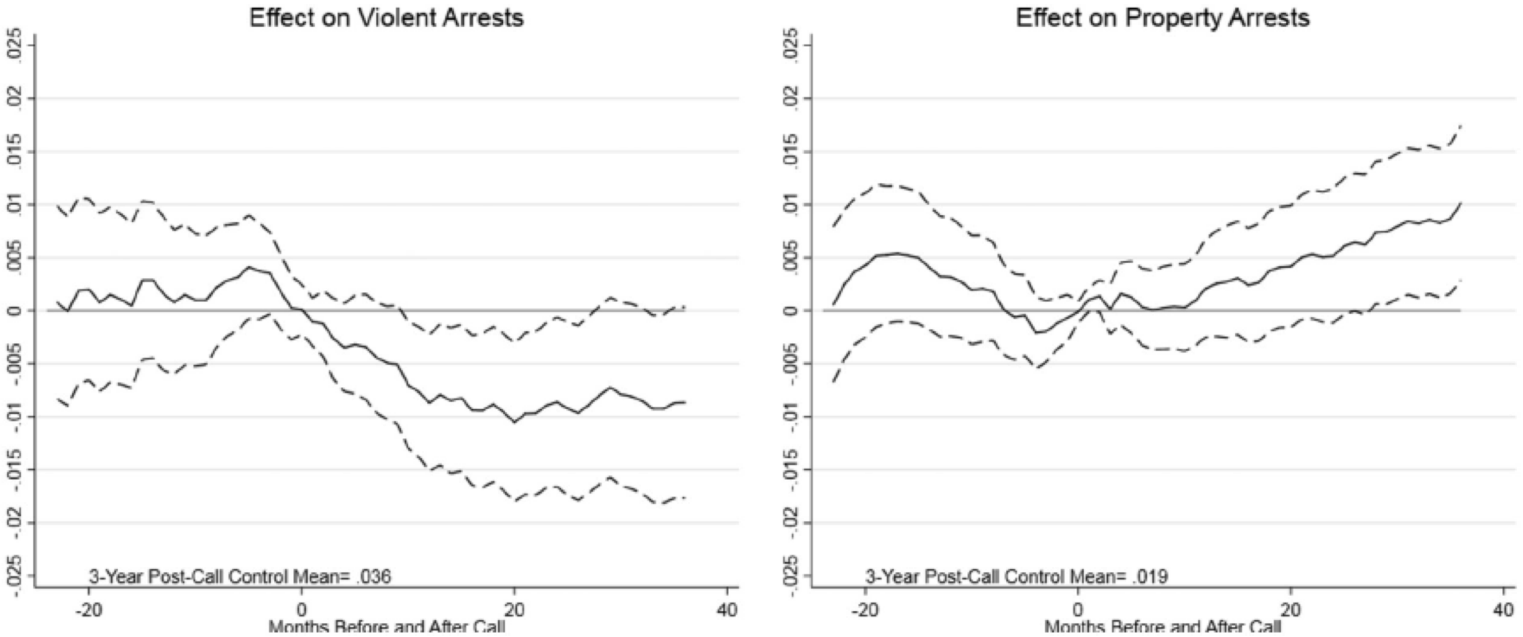
Effects on Single Individuals vs Families



Practice Questions

What could be on the quiz?

- You may be wondering what quiz/exam questions on this content might be like
- One question on Yang, one on Palmer et al
- If you want feedback on these practice problems, then please submit by Sunday