Economics of Crime
Deterrence: Introduction and diff in diff

1 Empirical tests of deterrence

Key questions:

- Does deterrence work?
- How to measure specific, general and marginal deterrence? Can we disentangle between them?
- What role doest incapacitation play?
- What other factors affects crime (supply of offenders, unemployment)? Can we capture them to filter them out?
- Which policies are effective in combating crime (police, prevention etc)?

Hurdles to overcome for the estimation:

1. **Flawed measures of crime.**

   Police statistics: A lot of crime goes underreported. Prague 2000: 96% of car thefts, 73% of bike thefts, 68% of burglaries, 46% of robberies and 41% of small thefts are reported.

   It is in the interest of the police to not report crime. Police actively discourages people to report crimes that are hard to investigate. Police then looks more effective in investigation the remaining crimes.

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Definition of offenses varies across jurisdictions and over time. (Its hard to compare countries)

Victimization surveys (ICVS) - Uncover some hidden crimes. General problems with retrospective information (how much can you remember?)

2. **You only learn who's committing crime on the subsample of offenders that get arrested.**

   Selection bias in estimation. The most proficient criminals are never caught.

3. **Measures of deterrence**

   We would like to have a person/crime specific measure (because of measure X, Y persons did choose to not commit the crime). In fact we have a number of people arrested, imprisoned etc for each crime.
   Very hard to distinguish between deterrence and incapacitation. Pure incapacitation gives results observationally equivalent with the economic model, even though there is no deterrence.

4. **Endogeneity of deterrence and other criminal justice variables.**

   More crime => more expenditure on police, tougher punishments etc.

   More crime => lower probability of apprehension holding the police resources constant (remember the $C(p, O)$ function).

   So there appears to be a negative relationship but it has nothing to do with behavioral response of criminals.

5. **Channels of responses**

   Say you observe that lower p leads to less crime. But does that mean that deterrence work? If deterrence does not work but incapacitation does, we would still observe a negative relationship. Higher p => more criminals behind bars => less crime, but no change in behavior.

Broad empirical approaches:

1. Estimating directly the relationship between O and measures/proxies for p, f on data aggregated at some regional (county, state, city) level. Variation: changes in p, f across places and over time.

2. Exploiting legislative changes or changes in other policies that shift deterrence. "Quasi-natural" experiments. Data at aggregated at regional level.

   (a) policy changes (diff-in-diff research design)

(b) instrumental variables

(c) event studies

3. Individual data - initially not much success here, very hard to get sensible measures of deterrence at the individual level, measures of crime also problematic.
   Self reports - accuracy problems.
   Arrests - affected by deterrence as well.
   Recently they started to emerge, e.g. Draga, Galbiati, and Vertova (2009) on Italian prison pardons (and yet, cannot distinguish between deterrence and the criminogenic effect of time in prison).

1.1 Traditional region-level studies

First pioneers with flawed methodology. Estimating aggregate changes in crime as a response of change in enforcement.

1.1.1 Ehrlich 1973 - classical piece, first attempt in literature

Takes U.S. states in years 1940, 1950, 1960 (three data-points for each state). Estimates simultaneous equations using OLS.

His main regression is:

$$\begin{aligned}\log(Y/N) &= \alpha + \alpha \log P + \alpha \log F + \log X + \varepsilon \\ \log P &= \beta \log F + \beta \log(E/N)_{t-1} + \beta \log(Y/N)_{t-1} + \log X + u\end{aligned}$$

Also says there should be an extra equation

$$\log(E/N)_t = \log L + \gamma \log(E/N)_{t-1} + \gamma \log(Y/N)_{t-1} + v$$

But does not actually run this in his 2SLS.

- P is constructed as the number of offenders imprisoned per offenses known.
- F is measured as the average time served by offenders in state prisons.
- L ... loss from crime
- E ... police expenditures .

Results: sizable negative elasticities (crime decreases strongly to a with a change police expenditure by 1%)

Some problems:

1. The 2SLS should kick out some of the "viscous circle" correlation between P and F. If sentences are higher, then there is a more people serving them in the prisons and vice versa. There is a strong direct correlation and causality. Yet the estimates are bigger in absolute terms compared to OLS.
2. There is variation across states, but where does this come from? Model provides no answer to a different starting conditions and different developments. No guarantee that the simultaneous equations capture all causal relationships. Get huge negatives in both equations - is that the true relationship?

3. Aggregation problem: police expenditure is aggregated, though it should rather be offense-specific. There is no way do distinguish the source of police spending variations. Additional data are impossible to obtain.

4. A lot of follow-up literature improved the original article: Easy improvements: Just using panel data and panel estimation techniques (fixed effects) reduces the magnitude of estimates by about 1/3 to 1/2. (There are obviously unobservable shocks so that states have both high crime and low P).

1.2 Exploiting policy interventions - Difference-in-differences

1.2.1 Kessler and Levitt (1999) - Sentence Enhancements

A simple illustration of the difference-in-differences framework. Using a "quasi-natural experiment" distinguishing deterrence and incapacitation - pure test of deterrence.

Proposition 8 - popular initiative passed by the voters of California in 1982, mandated substantial increases in sentences for repeat criminals. Limits for prison sentences determined by the statute, chosen by the judge upon conviction. Base sentence based on the actual offense, can be enhanced for criminals with certain criminal history.

Before: Sentences were enhanced by 3 years for a violent felony offender for each prison term served by that offender for a violent felony; or 1-year for each prior non-violent prison term, whichever is greater.

After: Sentences enhanced by 5 years for a "serious" felony offender for each prior conviction for a serious felony; or 1-year for each prior prison term for any offense, whichever is greater.

Additional provisions: Eliminating the limit that criminal histories only count for the past 10 years, prohibiting judges to allowing defendants to serve the enhancement concurrently with some other sentence.

The new enhanced sentences apply only to newly sentenced criminals: the incapacitation effect is not immediate, shows up years later (after the new criminals finish the term that they would have served without the enhancement). On the other hand, the deterrent effect is immediate, so if there is an immediate decline in crime, it should be due to the deterrence effect. Several years later, crime should decline even further - the incapacitation effect kicks in.

Key idea is to use a newly established policy disturbing the previously known and almost steady state to show the different impacts on deterrence. Proposition 8 sound also very cheap on paper to implement.

Basic diff-in-diff for eligible crimes:
California vs the rest of U.S., in years 1979-81 and 1981-85, then the long-term trend added in to the model. Why we might have doubts about it? In California situation before the measure was way worse then average of U.S. and become way better then average after the measure. Does the pre-trend in CA mean that the actual impact is even stronger? Or that CA was just peaking, and would have stopped?

"Diff-in-diff-in-diff"
Some crimes were not affected by Provision 8, we can estimate a difference in differences between them and different states in the us. Main issue of the results is substitution towards other crimes, that is unmeasurable.

1.2.2 Shepherd 2002 - Truth in Sentencing Laws

A "typical" diff-in-diff study. Individual regions (states) implement a given policy at different points in time. Diff-in-diff at the regional level: very common set-up. There is a policy change (more or less similar across regions) adopted by some (not all) regions at various points in time. In ideal settings, this setup should enable to estimate the causal relationship of the policy by comparing two regions with similar characteristics before of after the change. In reality there are a lot of problems to be overcome for persuasive result (mainly why was the region chosen for the policy in the first place).

In the regression framework, diff-in-diff is set up in as a dummy for the policy plus year and region dummies. Other dummies and fixed effect are removing all other variance from the model, leading to demeaning issues (are we sure, that we capture the right source of variance?).

Issue with the results 1: Evidence that policing intensity has increased as well (Table 5), imposed sentences got longer (plus the TIS requirement on top of that). Both are included in the regression, yet raises suspicion the TIS laws associated with some other tough-on-crime policies. One way out: just estimate the "black-box" (TIS dummy) - we would have a believable estimate of the overall effect of a "package" of TIS-related policies, on the other hand not knowing which mechanism is driving the effect. Using data on micro level would maybe enable to exploit differences in counties.

Issue with the results 2: The data is county-level data until 1996. Most states implemented the TIS around 1994. Very short time window to show full effects. On the other hand, the effects are only due to deterrence, not incapacitation (like in Kessler and Levitt (1999)). But a longer follow-up period would greatly improve the study.

1.2.3 Levitt 1998 - Juvenile Crime and Punishment

More creative use of difference-in-differences, but basic idea is the same. Comparing two population groups that receive different treatment, and cohorts (groups of persons defined by being the same age at one time) progress from one treatment to another as they age.

Simple test of deterrence hypothesis: **Juveniles are subject to a different criminal law than adults.** This gives two sources of variation:

- Change in the punitiveness as the person reaches adulthood. Policies can have a different effects based on age (correlated with socioeconomic status and cognitive abilities) of the offenders.
- Variation across states in relative punitiveness of juveniles and adults. U.S. states differ severely in age of consent, drinking age and regulations enabling to but juveniles on trial as adults.

Legal background of the differences:

- Criminal histories deleted after reaching adulthood (Why, doesn't the public have the right to know?)
- Shorter prison sentences (standard is half the term with maximal cap, release after reaching legal age in some cases).
- Different correctional facilities and rehabilitation efforts.

Data: **no direct measure of the number of crime committed by an age group**, only total number of offenses committed and arrests and imprisonments by age group. We see only proxies of the things we want to measure. They construct additional measures as explained variables for regressions:

Measure of juvenile crime:

$$crime_{cst} = \frac{arrests_{cst}}{arrests_{st}} * crime_{st}$$

Using the change in legal treatment at age 18.
Relative Punitiveness:

$$\frac{AdultPrisoners/AdultCrime}{JuvenilePrisoners/JuvenileCrime}$$

More conventional regression approach - 2 standard measures of deterrence (custody rate):

$$JuvCustody = \frac{JuvenileInCustody}{JuvenileCrime}$$

$$JuvCustody = \frac{JuvenileInCustody}{JuvenileInPopulation}$$

First measure suffers from division bias. There is a mechanical negative correlation between number of people in custody and number of crimes. There is a tendency to overestimate the relationship.

Second measure suffers from omitted variable bias, "resource saturation". On average, the adult custody rate is 1.42 that of the juvenile rate.

Notice that this study suffers from what most of the studies will inevitably will, trying to estimate relationship of interest by proxy variables. There proxy variables might capture the relationship of interest or might not, persuasiveness depends on number of robustness checks and writing prowess of the author.

1.2.4 Conclusions on the diff-in-diff set-up

- Very popular in the 1990's and early 2000, still have a valid use in clearly defined circumstances.
- Today there is a higher standard for believing the estimates. Just diff-in-diff is not enough. The concerns of endogeneity and trends are way too strong, need to be addressed and solved by attempting the following:
  - we usually don't have an instrument (instrumental variables) for adoption of these laws (also, usually the sample is too small, time series are not ideal for IV)
  - we need finer variation (diff-in-diff-in-diff - eligible crimes, subgroups of population affected, etc. to control for state-year effect...)
  - pre-trends are fairly easy estimated by having year-specific treatment dummies for years before as well as after

Conceptual issues:

- What are they really estimating? Using time series and highly aggregated data with proxy measures, our estimates is inevitably polluted by all other factors affecting variables of interest, not just the policy effect. We ideally need a causal model of various policy effects and have the data to filter out all conjoined effects (one effect of the policy can increase, another decrease variable of interest).
- Can we get a clean estimate of the effects of a particular policy? Policy should be predesigned with this in mind. If not we need to capture as much data we can, that might be impossible and expensive.
- Heavily dependent on current institutional context, limited usefulness to infer expected effects of the same policy in other places or context, or of alternative deterrence policies.
- Doesn't estimate general parameters useful. Its heavily depended on used design.

1.3 Individual-level studies - observational data

These are generally ideal for any type of research. Lots of observations, allows for controlling many factors at the individual level, even when the intervention/variation is at the level of district, population subgroup, etc.

In crime literature, there are datasets tracking the recidivists, or one is sometimes able to construct individual criminal (arrest) histories from arrest data; longitudinal surveys. These data can still contain a selection and survivorship biases typical to all crime data.

Best case: having variation in deterrence at the individual level.

1.3.1 Drago, Galbiati and Vertova (2009): Italian amnesty and deterrence

Italian amnesty of 2006: released almost 40 percent of prisoners. Institutional design:

1. Sentences for all prisoners reduced by 3 years: those who had less than 3 years remaining were released immediately (a vast majority), others had to serve the original sentence 3 years shorter.

2. This left a ”residual sentence”.

3. W hen convicted of a crime in the future, the offender had to serve the new sentence for the new crime plus the “residual sentence” forgiven for the old crime.

This created next-to-random variation in the punishment facing different individuals. Conditional on the original sentence of 2 years, being released with a residual sentence of 2 months or 22 months was a matter of only the time when the offender entered prison. It is key to condition on the original sentence - offenders with a short residual sentence are disproportionately those with short original sentence, hence less serious offenders, less with different recidivism probabilities.

Requirement for randomness: There could not be any systematic differences between entry cohorts 2 and 22 months before the amnesty. They are possible and very likely, the Amnesty was anticipated, debated in the Parliament and known to public and possibly also to prisoners. Hence there could be some changes upon entry and release in anticipation over the amnesty.

To prove randomness, authors to a formal check in table Table 1, comparing the offenders with above and below the median of the original sentence. Identical populations on observables (this is important for any inference!)

Data: administrative records on the released prisoners, almost 26,000 individuals in total, followed for 7 months. Key variable: whether they were re-arrested during the 7-month period (measure of recidivism).

Regression is very simple (equation 1), 1 years sentence - 0.16 percentage points reduction in re-offending. Large implied elasticity (0.74)

Excellent variation, and yet - cannot distinguish between deterrence and the criminogenic effect of time in prison.

Reading list for this chapter:

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Národohospodářská fakulta VŠE v Praze