Economics of Crime
Deterrence - field experiments

1 Empirical tests of deterrence

Key questions:

- Does deterrence work? Can we be sure that we see the deterrence?
- What other factors affect crime (supply of offenders, unemployment)? Can we capture them to filter them out?
- Which policies are effective in combating crime (police, prevention etc)? What are their returns?

1.1 Individual-level studies - field experiments

In econometrics you need some (exogenous) variation. You need to have a theory or knowledge why are variables developing the way they are. Controlled experiments are ideal. Sometimes they are provided by the real world (policy change, policy change affecting different groups).

What if there is no policy change happening? Or you can't observe the variable of interest? Create variation yourself!

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Experiment can be designed to be close to ideal, we create explicit causality. With y as health outcome and X treatment (0 if medication, 1 if placebo, or dosage), assigned **randomly** we just estimate:

$$\begin{aligned} y &= \beta X + e \\ \beta_{RE} &= \frac{E[Y|X=1] - E[Y|X=0]}{E[Y|X=1] - E[Y|X=0]} = \frac{\Delta Y}{\Delta X} \end{aligned}$$

β_{RE} is the effect and we're done.

1) Laboratory experiment

- [+] everything under control
- [+] cheap
- [-] limited transferability of results into the real world
- [-] hardly ever estimate parameters of interest or magnitude of treatment effects

2) Field experiment (Mexico's Progressa, New York subway night police patrols)

- [+] more realistic
- [+] results more relevant for policy questions
- [+] pilot programs sometimes used for policy formulations
- [-] **hugely expensive and time consuming**
- [-] not everything under control
- [-] behavior of people affected by short-term horizon of the experiment
- [-] **often times unethical or illegal**
- Design issue: do people know they are part of an experiment!
  - Inevitable and generally desirable in social policy experiments,
  - Undesirable in deterrence experiments,
- Design issue: how far can you intervene and how far can you measure the behavior?

1.1.1 Kleven, Knudsen, Kreiner, Pedersen, Saez (2011): Unwilling on unable to cheat? Evidence from a tax audit experiment in Denmark

A field experiment on tax evasion. Asks many questions. We focus on "how people respond to an increase in the *perceived* probability of punishment (audit)?"

Usual problems in measuring tax evasion:

- measuring evasion
- measuring probability (objective vs subjective)
- endogeneity of p (mere average wouldn't do)
- getting the variation in p

Denmark taxpayer compliance experiment:

- Random sample of 25,000 employees and 17,800 self-employed.
- over-sampling of taxpayers with more complex tax returns
- Two dimensions of treatment/control:
  - Auditing taxpayers returns in 2007 (2006 income). Unbeknown to taxpayers, but they were informed if the audit found something wrong. Treatment mechanism: $\uparrow$ perceived probability of detection, plus ability of auditors to uncover something wrong.
  - 3 randomly selected groups (by 1/3): no letter, 50% letter, and 100% letter. Sent letters in early 2008, that is, before *reporting* their 2007 income but after the income was earned. The letters warned that the taxpayers returns will be audited with a 50% or 100% probability. The returns were then audited according to these probabilities. Treatment mechanism: letter $\Rightarrow \uparrow$ probability of audit hence detection.
- The researchers observe the pre- and post-experiment returns and audit results. Measure evasion based on the audit adjustments.

Results:

1. Measure of under-reporting based on audit:
   - About 1% of personal income but 16% of self-employment income not reported.
   - Huge distinction between third-party reported and self-reported income (and deductions)

- Within self-employment: the same self-employed do not cheat if the income is third-party reported

2. The effects of past audits:

   - Detection uncovered on past audits: 8491 income, 19
   - Increase in the income reported next year between audited and unaudited group: +2557 DKR increase, and 0.89 percent increase in the probability of reporting higher income
   - Almost all of the effect is in self-reported income
   - Stronger effects if limited to groups that did not receive the threat-of-audit letters afterwards.

3. The effects of threat-of-audit letters:

   - Unfortunately, letters sent to employees only
   - strong effect on average (100 DKR increase)
   - Stronger effect on the prior audit group
   - roughly equal size effect of the 50% threat and 100% threat

Conclusion: Field experiments like this are very useful. Not just academic purposes, but allow detecting the effects of practical enforcement measures, better targeting of enforcement resources, etc.

2 Legalized abortion and crime

2.0.1 Levitt and Donohue (2001) provocative paper, mentioned in Freakonomics

Abortion illegal in the U.S. until 1970's. There were abortions, either illegal transactions (high money cost, health risk) or travels to Europe (high money costs).

1970: five states legalized (New York, California, Washington, Alaska, Hawaii

1973: Supreme Court decision (Roe vs. Wade) legalized abortion nation-wide. Low cost of obtaining abortion afterwards ($80). Large increase in the number of abortions (though not overnight).

Why should it affect crime?

1. The type of person most likely to commit crime is young men aged 17-29. Abortions reduced birth rates => smaller cohort sizes => 17 years later there are fewer people around to commit crimes.

2. **Selection effects**: Children born to disadvantaged mothers (single, poor, living in bad neighborhoods) are more likely to become criminals. These types of mothers are also more likely to benefit from abortions. Also children who do not grow up in a supportive environment (parents who don't love them etc) are more likely to commit crime. Many unwanted children are not born after abortion. New cohorts are made of individuals less likely to commit crimes as adults. Cite quite a lot of evidence on both.

The paper presents evidence consistent with the hypothesis that legalizing abortion lowered crime.

1. National time series (Figure II and III)

2. Early vs late legalizes. In the early legalizing states, crime rates began to fall few years earlier and the drop was more pronounced than in the rest of the country (Table I). Those states also accumulated more abortions.

3. Changes in crime are negatively related to effective abortion rates. The impact of abortion should be gradual - as time passes, a higher fraction of the population of potential criminals were born after abortion. Also the take-up of abortions differed largely across states, and was gradual.

Effective abortion rate:

$$EAR_t = \Sigma_a \frac{Abortions_{t-a}}{LiveBirths_{t-a}} \frac{Arrests_a}{Arrests_{total}}$$

The arrest ratio is a little tricky - if measured as of year t, the number of arrests by a post-legalization cohort a already captures the impact of abortion (true rate should be higher). Ideally one would like to measure on a ratio not affected by abortion (but using the past may miss time shift in the age composition of criminals).

Sensitivity checks.

4. Age-specific arrest rates negatively correlated with the effective abortion rate. Arrest rates measured per number of people aged below 25. If all the impact of abortion would go only through cohort size, the coefficient would still be zero 25 years later. First piece of evidence on the selection effect.

Table VI. Nice check: same regression on the cohorts aged 25+. Still, all the variation is at the state-year level (they have the total effective abortion rate here, not the cohort-specific abortion rate). Cannot control for unobservables that affected a given state in a given year differently from other states, which may be correlated with abortion.

5. Arrests by state and years of age. Do cohorts more affected by abortion commit more crime? Within a state, one should seek reduction in crime rates only for cohorts born after Roe vs. Wade, and higher reductions for later cohorts as those were affected by abortions even more.

$$\ln(Arrests_{stb}) = \beta AbortionRate_{sb} + \gamma_s + \gamma_{tb} + \gamma_{st} + (\gamma_{sa}) + \varepsilon_{stb}$$

Explain the role of all these fixed effects, namely state-year and state-age. Cannot do state-cohort fixed effects since the abortion rate is a fixed number for a given state-cohort. Main advantage: can kick out state-year shocks potentially correlated with arrests.

Table VII. Problem 1: They use raw number of arrests, rather than arrests per capita. So even as they find a negative coefficient it may be solely due to a smaller cohort size. They argue that the population data by age are not reliable enough at the state level.

Table VII. Problem 2: There was a programming error. They claim that they included the state-year effects, but in fact they did not. So still unobservables specific to a state-year could have biased the estimates.

The findings are economically significant: They imply that as much as 50% of the large decline in crime in the U.S. during the 1990's is attributed to abortion.

2.0.2 Foote and Goetz (2005)

The whole paper is about Table VII in Levitt and Donohue, i.e., the evidence for the abortion-crime hypothesis from cohort-specific arrests. Does not dispute any other evidence.

- They discovered the programming error, run the regressions without errors.
- They run the regressions on arrests per capita, not number of arrests.
- Emphasize the state-year effects, or state-age effects.

They elaborate why having the age dimension is important - can eliminate any confounding effects going on at the state-year, or state-age level. Emergence of crack cocaine (somehow undisputed that it did contribute to crime) in the 1980's: came first in large urban centers like CA and NY, also faded away there first. With the state-year, the finding that high abortion states had less crime may as well be "explained" by state-year effects.

Main points of the paper:

1. Clear value in eliminating the error.

2. The per capita regression: Arrests are already a proxy for the number of crimes, adding population brings in additional error => may be too noisy.

3. Using the state-age, state-year dummies: OK in principle but may be asking the data to do too much. Demeaning the data kicks out some unobservables that are common to each state, state-year, or state-age, that are correlated with the abortion measure and affect crime. On the other it kicks out some of the good variation: say in one year, there is an increase in the abortion exposure for all age groups and a reduction in crime. That may have been due to some spurious relationship as well as to a true causal relationship. Still, in the regression with state-year effects, this change is picked-up by the state-year dummy variable and the relationship between abortion and crime is estimated on a differential change (among age groups) in abortions and crimes.

2.0.3 Donohue and Levitt's reply to Foote and Goetz

Two arguments main arguments of the response paper:

- Acknowledge the error, report the results with the error corrected and on the original data, fair comparison
- Argue that the combination of removing too much variation through state-year and state-age fixed effects, plus some measurement error in the population, plus a measurement error in the number of abortions.

Their new results: construct a more precise measure of abortion.

There are two sets of measures on the incidence of abortion by states, they are correlated strongly in the raw data but fairly weekly after partialling out the state, yaer, and state-year fixed effects (Table 3).

Likely sources of measurement error:

- mobility to obtain abortion (so number of abortions divided by the number of residents overstates the true extent of abortion in the early legalizers since women travelled there to obtain abortion). The alternative dataset records both the state of abortion procedure as well as the state of residence.
- mobilty after abortion (about 1/3 of Americans aged 15-24 lives in a different state than they were born in) - they reconstruct the state of residence from the pairwise state mobility table (weakness: those affected by abortion legalization are probably less mobile)
- assigning the abortion year to the year/age of arrest - construct the fraction of how many arrested in year t of age a would have been aborted in years $t-a, t-a-1$, and $t-a-2$.
- also use the alternative source of abortion data as an instrument - would be perfect if the measurement errors in data collection were uncorrelated (they don't do the usual battery of tests; also discuss initially that in fact the measurement errors are likely to be correlated, but later they forget about it and proclaim this to be an instrument).

2.0.4 Lessons and the "big picture":

1. Journalists like to write about sensational things. They loved Levitt and Donohue paper because it looked sensational. They loved Foote and Goetz: Found an error in Levitt, so the whole abortion and crime story is dead. In fact it's not so black and white. Levitt and Donohue present a lot of supporting evidence for the hypothesis, only one piece of evidence got disputed later, not entirely disputed given the replies.

2. How do we find truth in economics? The theory provides predictions, testable hypotheses. The world provides data. The predictions imply that certain patterns, or relationships, in the data are consistent with the theory, different patterns contradict that theory. We then gradually build confidence in that theory by piling out pieces of empirical evidence that are consistent with it, or abandon the theory by gradually piling up evidence that contradicts it, or at least does not provide support. One single paper is never enough to definitely confirm a hypothesis or to definitely shoot it down.

3. Great example of how criticism, repeatability and scientific discourse improves the knowledge - after all, Donohue and Levitt were kicked hard to get sharper abortion data and produce better results.

4. The abortion and crime story (at least in the case of the U.S.) makes inherent economic sense. To falsify the theory as outright wrong would either require a different theory, or to assemble a lot of evidence of reversed relationship, or to empirically reject the "building blocks" (that unwanted or disadvantaged children are more likely to commit crime etc).

5. When we are worried about unobservables, reverse causalities etc we typically have some theory of why they are present, why the unobservables are correlated with our observables etc. Here it's just pure coincidence.

6. The abortion-crime relationship is context-specific. Pop-Eleches (2006) on Romania. Looks at the cohorts born less than 6 months and more than 6 months after the abortion ban, only during the same school year (the first group includes both wanted and unwanted children, the latter only the wanted children). They also competed for the same school, labor market etc resources. Result: on average, the post-ban children had *better* schooling and labor market outcomes. (The data is dirty.)

   Controlling for various parental background variables: they had worse educational outcomes (Estimates a pure effect of unwantedness.)

Reading list for this chapter

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