FLS 6415 - Causal Inference for the Political Economy of Development

Week 8 - Violence & Difference-in-Differences

Jonathan Phillips

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- Our basic causal inference problem is that confounding makes counterfactual cases implausible (biased)
- If we compare separate treatment and control units when treatment assignment is not random:
  - The control units have different levels of the outcome for many reasons, not just treatment
- If we compare the same unit before and after treatment:
  - Other factors influencing the outcome might also have changed between our measurements (eg. any news event!)

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- We can keep lots of variables fixed if we compare the same unit before and after treatment
- We can measure how much other factors changed over time if we have units that were not exposed to treatment

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- We're now comparing *changes* (differences), not *levels* of the outcome
  - Most confounders affect levels, so this makes our counterfactuals more plausible
    - Eg. different laws affect growth rates, not the change in growth over time
  - And crucially, we can remove confounding even for *unobserved* confounders
  - So Diff-in-Diff is 'better' than controlling or matching, which only eliminate observed (measured) confounding

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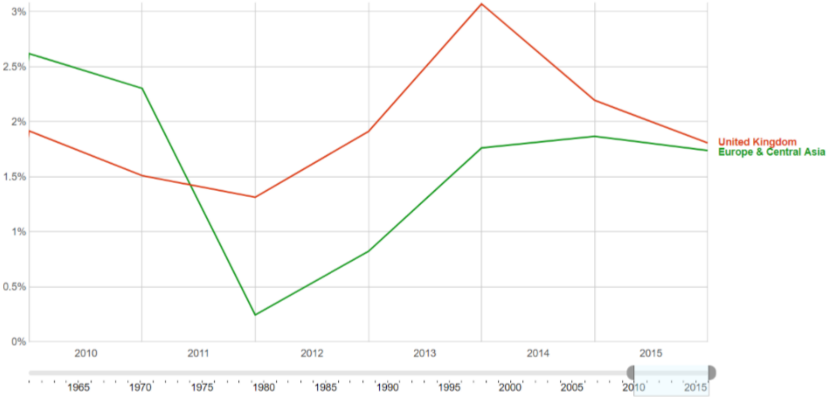
  - Eg. the UK's growth rate was falling even before the Brexit vote, but Europe was improving
- Diff-in-Diff is 'worse' than natural experiments

Difference-in-Differences



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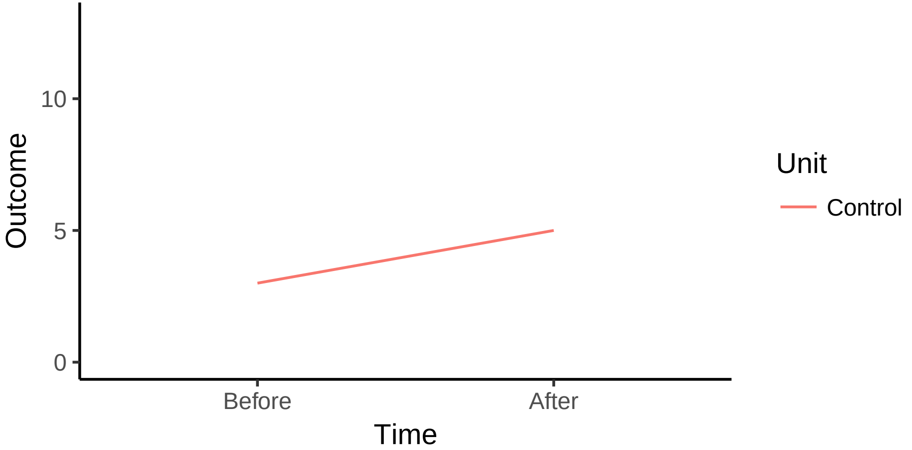

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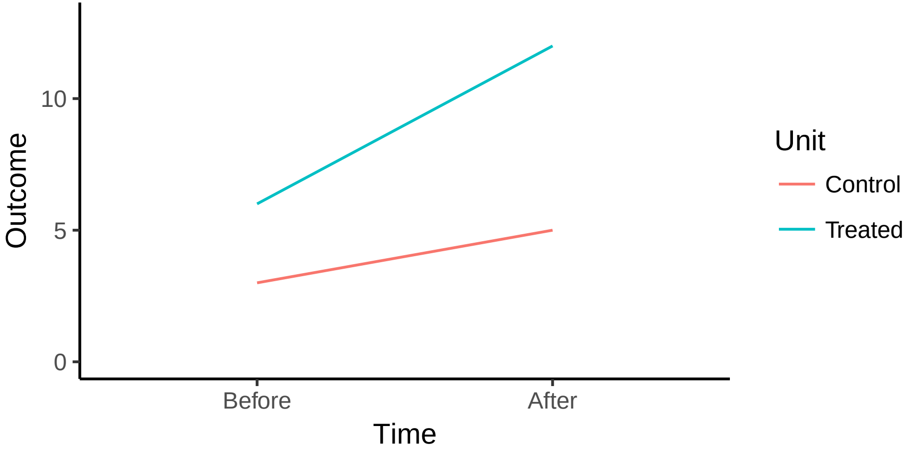

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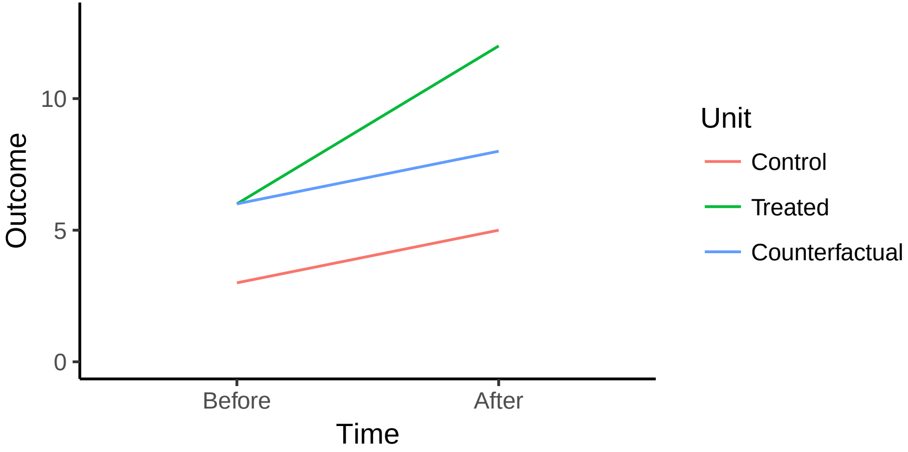

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$$Y_{it} = \alpha + \gamma D_i$$

- The difference-in-differences estimate is just the *interaction* of time and treatment status

$$Y_{it} = \alpha + \gamma D_i + \delta T_t + \beta D_i * T_t$$

- β is our causal effect estimate

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- Difference-in-Differences means:

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- That’s our causal effect

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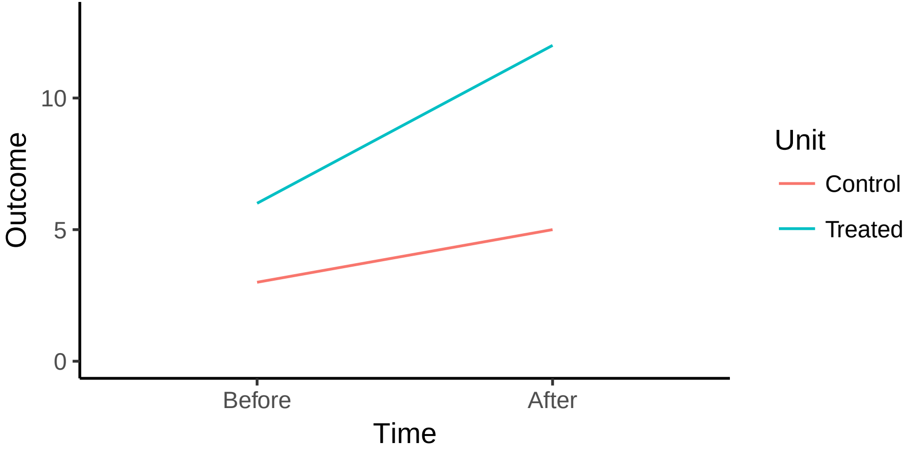
Raw Data:



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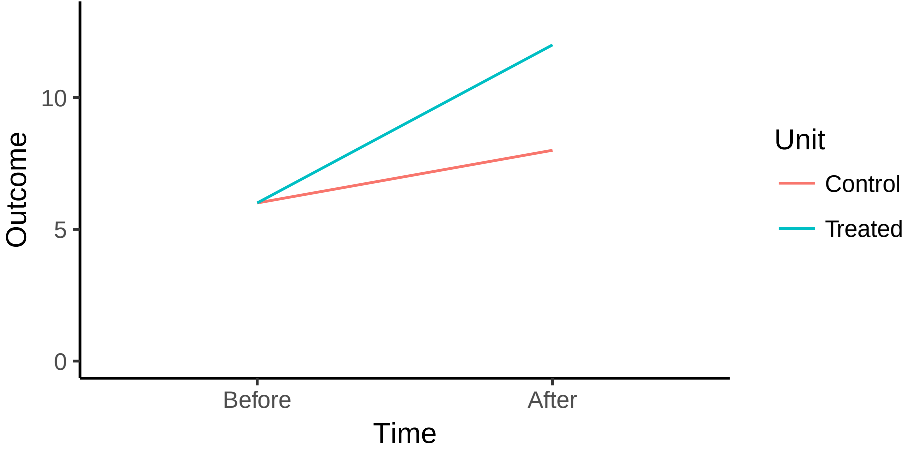
Add a variable (fixed effect) for treated/control:



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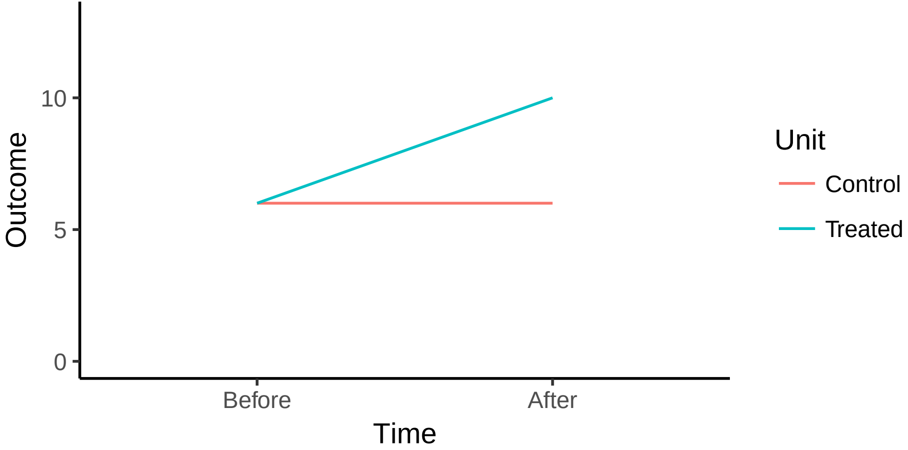

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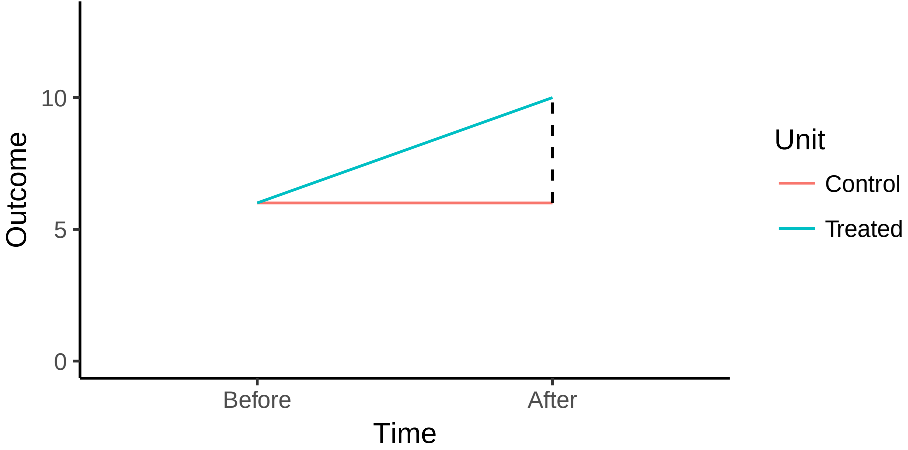

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- One test of this is to check if **pre-treatment trends are parallel**
- Then our counterfactual makes sense

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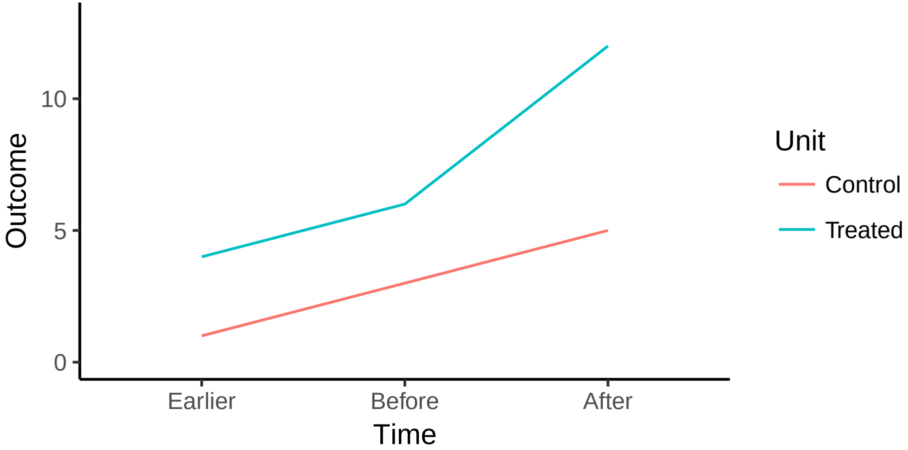

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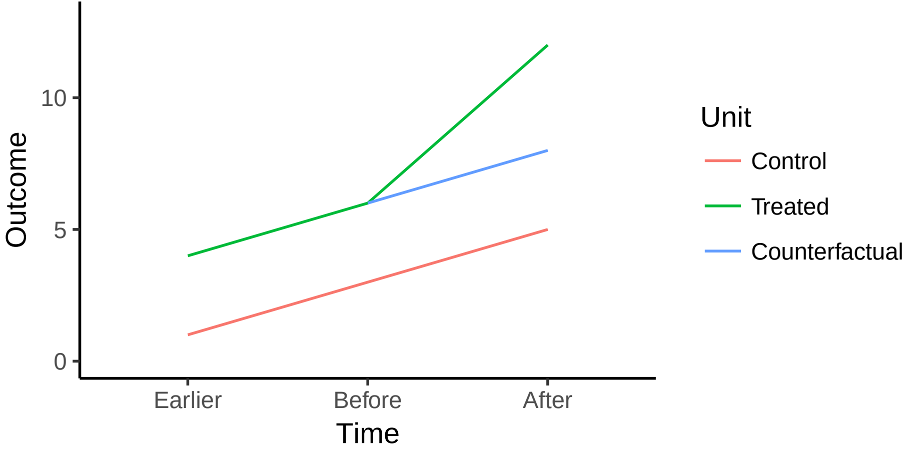

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- Parallel trends (no time-varying confounders) is a difficult assumption
- Selection into treatment is usually not just due to mostly 'fixed' variables (eg. gender) but due to 'time-varying' variables (eg. income, employment etc.)
- Eg. training program participants' income has usually fallen a lot in the past few months

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- Eg. training program participants’ income has usually fallen a lot in the past few months
- A good test is to see if there is an effect from ’placebos’ - testing for treatment effects at times before treatment happened

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- Eg. The UK also announced new rules to regulate the banking sector on the same day as Brexit

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- Bertrand et al (2003):
  - Careful with standard errors
  - Especially if more than two time periods (auto-correlation)
  - So cluster standard errors by each cross-sectional unit (eg. each country)

Dube and Vargas 2008

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    - Reverse causation: Less violence causes more income
    - Confounding: More effective government raises income and lowers violence

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  - Prevents reverse causation: See how violence changes *after* treatment
  - International prices are used for treatment, which are even more ’exogenous’
- Compare changes in violence in coffee-growing areas to changes in violence in non-growing areas
- They go beyond ’before’ and ’after’, using the long-term change in oil/coffee prices themselves (continuous treatment variable)

Dube and Vargas 2008

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Dube and Vargas 2008

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  - Supportive evidence that wages decrease as coffee prices fall and state revenues increase as oil prices rise

Chimeli and Soares 2017

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Chimeli and Soares 2017

- How does an activity being illegal affect violence?
- How did Brazil's ban on mahogany affect homicides?
- What are the barriers to causal inference?
  - Confounders, eg. State capacity
  - Reverse causation, eg. Violence causes associated activities to be outlawed
  - Other evidence only from drugs, which are directly connected to violence

Chimeli and Soares 2017

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Chimeli and Soares 2017

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  - No risk of reverse causation: Change in violence measured after treatment
  - No risk of confounding by 'fixed' (non-time-varying) confounders, eg. state capacity

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  - Repeated measurement before and after treatment
  - No risk of reverse causation: Change in violence measured after treatment
  - No risk of confounding by 'fixed' (non-time-varying) confounders, eg. state capacity
- Comparing the *change* in violence in mahogany-growing areas to the change in violence in non-growing areas

Chimeli and Soares 2017

- **Population:**

Chimeli and Soares 2017

- **Population:** Brazilian municipalities

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- **Population:** Brazilian municipalities
- **Sample:**

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- **Treatment Assignment Mechanism:**

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- **Outcome:** Rate of Homicides

Chimeli and Soares 2017

- Multiple treatment timings:
  - 1st policy change
  - 2nd policy change
  - Reverse treatment: Better policing of mahogany regulations

Chimeli and Soares 2017

- Methodology:

Chimeli and Soares 2017

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$$Homicides_{it} = \gamma_t + \delta_i + \beta(Post - 1998_t * Mahognay_i) + \epsilon_i$$

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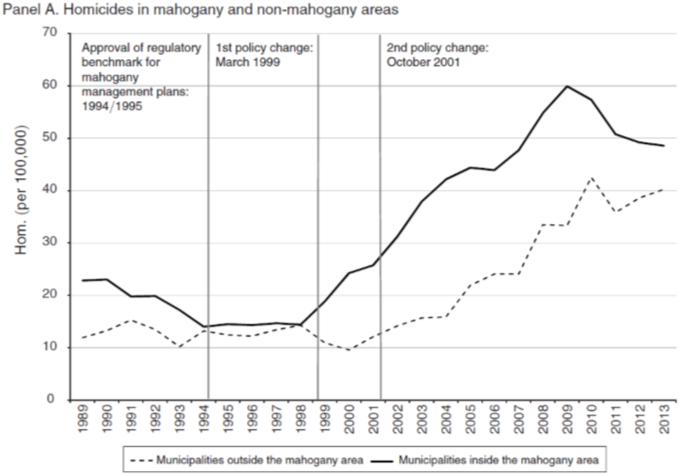
- Apply more complex state-specific trends for covariates to minimize risk of non-parallel trends
- Cluster standard errors by municipality
- Supporting evidence: The 'extra' homicides were the type we'd expect from illegal activity

Difference-in-Differences



Chimeli and Soares 2017

- Interpretation
  - Illegal activity prevents ’peaceful’ contract enforcement
  - Competition between loggers
  - Contract enforcement with buyers
  - Intimidation of communities to not report logging

Bundervoet et al 2008

- What is the impact of (the Burundian) civil war on children's health?

Bundervoet et al 2008

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Bundervoet et al 2008

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- What is the challenge to causal inference here?
  - **Confounding:** Poorly governed places more likely to be at war and have poor health
  - **Selection:** Fighters target poorer places where children have poor health
  - **Confounding:** Older children are more exposed to conflict and have worse height-for-age mechanically

Bundervoet et al 2008

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Bundervoet et al 2008

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  - The war varied in location and timing

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Bundervoet et al 2008

- A difference-in-differences methodology helps because:
  - The war varied in location and timing
  - Eliminate geographical confounding: We compare children born after conflict to those born during conflict
  - Control for secular changes over time: Compare children in provinces with conflict and without

Chimeli and Soares 2017

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- **Population:** Burundi

Chimeli and Soares 2017

- **Population:** Burundi
- **Sample:**

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  - NOTE: They have to exclude places that always or never experienced war. Why?

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- **Outcome:** Childrens' Height-for-Age Z-score

Bundervoet et al 2008

- Methodology:

Bundervoet et al 2008

- Methodology:

$$Height\_for\_Age_{ijt} = \alpha + \gamma_t + \delta_j + \beta(Conflict\_in\_Province_{ij} * Alive\_during\_conflict_{it}) + \epsilon_{ijt}$$

Bundervoet et al 2008

- Methodology:

$$Height\_for\_Age_{ijt} = \alpha + \gamma_t + \delta_j + \beta(Conflict\_in\_Province_{ij} * Alive\_during\_conflict_{it}) + \epsilon_{ijt}$$

- Add province time trends to limit risk of non-parallel trends

Bundervoet et al 2008

- Results:

Bundervoet et al 2008

- Results:
  - Exposure to civil war leads to half a standard deviation lower height-for-age among children

Lyall 2009

- Does indiscriminate violence incite insurgent attacks?

Lyall 2009

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- What is the challenge to causal inference here?

Lyall 2009

- Does indiscriminate violence incite insurgent attacks?
- What is the challenge to causal inference here?
  - **Reverse Causation:** Insurgent attacks encourage state violence
  - **Selection:** States target places where they expect insurgent attacks to start

Lyall 2009

- A difference-in-differences methodology helps:
  - Correct sequencing of Russian artillery, then measuring change in rebel attacks
  - Control for differences between places that did and did not have attacks
- Comparing the change in attacks before and after shelling in shelled vs. non-shelled villages

Lyall 2009

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Lyall 2009

- **Population:** Villages in Chechnya

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- **Population:** Villages in Chechnya
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- **Treatment Assignment Mechanism:** Somewhat random, drunken, but probably a bit strategic
- **Outcome:** Number of rebel attacks within 90 days

Lyall 2009

- Methodology:

Lyall 2009

- Methodology:
  - Balance tests suggest randomization on observables holds, but *not sufficient*
  - Pre-Regression Matching to make sure we're comparing similar shelled and non-shelled villages
    - Should help with ensuring parallel trends
- Then finally a difference-in-differences method

Lyall 2009

- Results:

Lyall 2009

- Results:
  - Shelling a village reduces insurgent attacks by 24%

Lyall 2009

- Results:
  - Shelling a village reduces insurgent attacks by 24%
  - Shelling decreased by 5% in control villages, and 29% in shelled villages

Summary

- Causes of violence:

Summary

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  - Commodity prices
  - Illegal status of activities

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Summary

- Causes of violence:
    - Commodity prices
    - Illegal status of activities
- Effects of violence:
    - Malnutrition in children
    - Fewer counter-attacks by rebels (sometimes...)