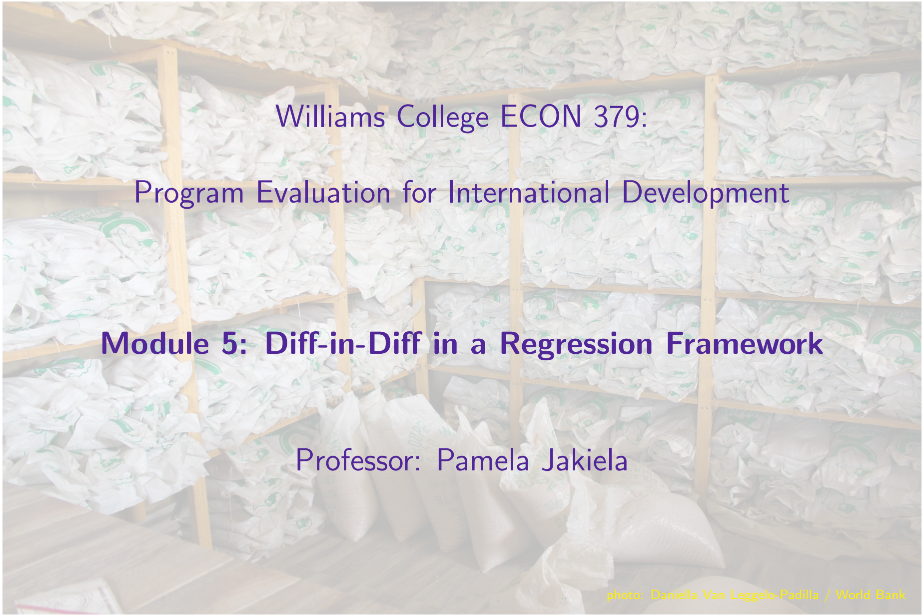

Williams College ECON 379:

Program Evaluation for International Development

Module 5: Diff-in-Diff in a Regression Framework

Professor: Pamela Jakiela

photo: Daniella Van Leggelo-Padilla / World Bank

2×2 Diff-in-Diff Specifications

Difference-in-Differences Estimation

	treatment	comparison	difference
pre	$\bar{Y}^T_{pre}$	$\bar{Y}^C_{pre}$	$\bar{Y}^T_{pre} - \bar{Y}^C_{pre}$
post	$\bar{Y}^T_{post}$	$\bar{Y}^C_{post}$	$\bar{Y}^T_{post} - \bar{Y}^T_{post}$
difference	$\bar{Y}^T_{post} - \bar{Y}^T_{pre}$	$\bar{Y}^C_{post} - \bar{Y}^C_{pre}$	δ_{DD}

Difference-in-Differences Estimation

To implement diff-in-diff in a regression framework, we estimate:

$$Y_{i,t} = \alpha + \beta D_i + \theta Post_t + \delta \left(D_i * Post_t \right) + \varepsilon_{i,t}$$

Where:

- D_i = treatment dummy
- $Post_i$ = dummy for post-treatment period
- $D_i * Post_i$ = interaction term

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Where:

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- $D_i * Post_i$ = interaction term

Panel data: every unit$\times$period data point is an observation

Difference-in-Differences Estimation in Stata

```
. reg y treatment post treatxpost

      Source |       SS           df       MS      Number of obs   =     2,000
-------------+----------------------------------   F(3, 1996)      =     64.75
       Model |   1558.8687         3  519.622901   Prob > F        =    0.0000
    Residual |  16017.7056     1,996  8.02490261   R-squared       =    0.0887
-------------+----------------------------------   Adj R-squared   =    0.0873
       Total |  17576.5743     1,999   8.7926835   Root MSE        =    2.8328

------------------------------------------------------------------------------
           y |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   treatment |  -.1928937   .1791636    -1.08   0.282     -.544261    .1584737
        post |   .0679519   .1791636     0.38   0.705    -.2834154    .4193193
  treatxpost |   2.110153   .2533757     8.33   0.000     1.613244    2.607061
       _cons |   5.231523   .1266878    41.29   0.000     4.983069    5.479977
------------------------------------------------------------------------------
```

Example: Police Reform in Chicago

Chicago police and ACLU agree to stop-and-frisk safeguards

Aamer Madhani USA TODAY
Published 9:14 a.m. ET Aug. 7, 2015 | **Updated 1:34 p.m. ET Aug. 7, 2015**

CHICAGO — The Chicago Police Department and American Civil Liberties Union of Illinois announced Friday that they've come to an agreement on monitoring how officers go about conducting street stops of citizens in the nation's third-largest city.

The deal follows fierce criticism of Chicago police disproportionately targeting minorities for questioning and searches under the controversial "stop and frisk" practice.

Chicago Police Superintendent Garry McCarthy and ACLU officials announced on Friday that they've come to an agreement on reforms to the city's stop-and-frisk policy. *M. Spencer Green, AP*

Under the agreement, police will track all street stops and protective pat-downs — not just those that don't result in an arrest, as they have in the past.

In addition, the city and ACLU have agreed to name an independent consultant, former U.S. magistrate Arlander Keys, who will issue public reports twice a year that detail how the department conducts street stops and recommend policy changes.

The police department also agreed to bolster training of officers to ensure that officers don't use race, ethnicity, gender or sexual orientation when deciding to stop and frisk, and to conduct pat-downs only when reasonably suspicious that a person is armed and dangerous.

The agreement goes into effect immediately.

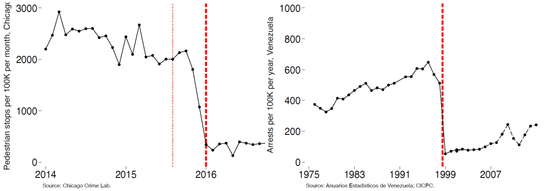
source: USA Today

Example: Police Reform in Chicago

Figure 1: Sharp declines in arbitrary pedestrian stops and arrests

These figures show that the reforms we study, marked in red, caused sharp drops in (a) pedestrian stops in Chicago and (b) arrests in Venezuela. The red dotted line in (a) marks the announcement of the reform.

(a) Chicago (b) Venezuela



source: Hausman and Kronick (2020)

Unexpected policy change in August 2015: police officers had to complete paperwork documenting every “stop-and-frisk” encounter

Example: Police Reform in Chicago

Comparison of (not random) treatment, comparison groups:

- **Treatment group:** Chicago police
- **Comparison group:** all other police departments in Illinois

Data on all **traffic** stops between 2013 and 2018

- Pre-treatment period up through August (or December) 2015
- Outcomes: number/type of stops, resulting citations

Example: Police Reform in Chicago

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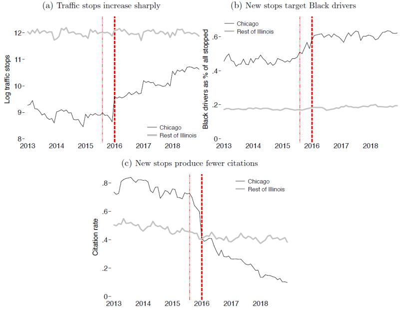
$\Rightarrow$ Notice: many periods of data, not just two (pre/post)

Example: Police Reform in Chicago

Figure 2: The substitution of traffic stops for pedestrian stops in Chicago

Figure (a) shows that the number of traffic stops jumped in Jan. 2016 in Chicago but not in the rest of Illinois; Figure (b) shows that the new stops targeted Black drivers; and Figure (c) shows that the new stops produced fewer citations. Dashed vertical lines mark when the agreement took effect; dotted vertical lines mark when the agreement was announced.



Source: Illinois Department of Transportation.

source: Hausman and Kronick (2020)

Example: Police Reform in Chicago

Regression specification:

$$Y_{g,t} = \alpha_0 + \alpha_1 \text{CHI}_g + \beta_1 \text{POST}_t + \beta_2 \left(\text{CHI}_g \times \text{POST}_t\right) + \varepsilon_{g,t}$$

α_0 = a constant (pre-treatment mean outside Chicago)

α_1 = pre-treatment difference between Chicago, not Chicago

β_1 = post-treatment difference in means outside Chicago

β_2 = diff-in-diff estimate of treatment effect

g = group (Chicago or not) and t = time period

Example: Police Reform in Chicago

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Table A.1: The substitution of traffic stops for pedestrian stops in Chicago

Estimates of Equation 2. The coefficient β_2 captures the difference-in-differences between Chicago and the rest of Illinois, from before to after the ACLU agreement.

	(1) (ln) Traffic Stops	(2) P(Black|Stopped)	(3) P(Citation|Stopped)
α_1: Chicago	-3.083 (0.04)	0.294 (0.005)	0.267 (0.01)
β_1: Post	0.014 (0.02)	0.012 (0.001)	-0.072 (0.006)
β_2: Chicago $\times$ Post	1.182 (0.08)	0.128 (0.006)	-0.441 (0.02)
Constant	11.999 (0.02)	0.175 (0.0007)	0.482 (0.005)
Observations	144	144	144

Robust standard errors in parentheses.

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Using ΔY_i as the Outcome Variable

Interacted specification is equivalent* to first differences:

$$Y_{i,t=2} - Y_{i,t=1} = \eta + \gamma D_i + \epsilon_{it}$$

where:

- $Y_{i,t=2} - Y_{i,t=1}$ = change (pre vs. post) in outcome of interest
- γ = coefficient of interest (the treatment effect)
- η = time trend (average change in comparison group)

* Coefficients will be identical, but standard errors may differ

Example: Minimum Wages and Employment in the Fast-Food Industry

Interacted specification is equivalent* to first differences:

$$\Delta \text{FTE}_i = \eta + \gamma \text{NJ}_i + \epsilon_i$$

where:

- ΔFTE_i = change in full-time employment in restaurant i
- γ = difference in mean change in NJ stores (vs. PA stores)
- η = constant (mean change in FTE in PA)

Example: Minimum Wages and Employment in the Fast-Food Industry

TABLE 4—REDUCED-FORM MODELS FOR CHANGE IN EMPLOYMENT

	Model	
Independent variable	(i)	(ii)
New Jersey dummy	2.33 (1.19)	2.30 (1.20)
Controls for chain and ownership[b]	no	yes
Controls for region[c]	no	no
Standard error of regression	8.79	8.78
Probability value for controls[d]	—	0.34

Notes: Standard errors are given in parentheses. The sample consists of 357 stores with available data on employment and starting wages in waves 1 and 2. The dependent variable in all models is change in FTE employment. The mean and standard deviation of the dependent variable are -0.237 and 8.825, respectively. All models include an unrestricted constant (not reported).

[b]Three dummy variables for chain type and whether or not the store is company-owned are included.

[c]Dummy variables for two regions of New Jersey and two regions of eastern Pennsylvania are included.

[d]Probability value of joint F test for exclusion of all control variables.

source: Card and Krueger (1994)

Continuous Treatment

Flavors of Diff-in-Diff Estimation

First two examples were traditional 2×2 diff-in-diff:

- Treatment binary (both econometrically and conceptually)
  - Police reform in Chicago, but ot elsewhere
  - Minimum wage increase in NJ, but not PA

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We often wish to evaluate policies implemented "everywhere" which may lead to variation in treatment across units in practice

- Duflo (2001): more schools built in some Indonesian regions
- Godlonton and Okeke (2015): Malawi's ban on traditional birth attendants couldn't impact regions where no one uses TBAs

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In such settings, treatment intensity often varies continuously

Example: Traditional Birth Attendants in Malawi

Does a ban on informal health providers save lives? Evidence from Malawi☆

Susan Godlonton [a,b], Edward N. Okeke [c,*]

[a] *Department of Economics, Williams College, United States*
[b] *IFPRI, United States*
[c] *Department of Economics, Sociology and Statistics, RAND, United States*

A R T I C L E   I N F O

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Available online 11 September 2015

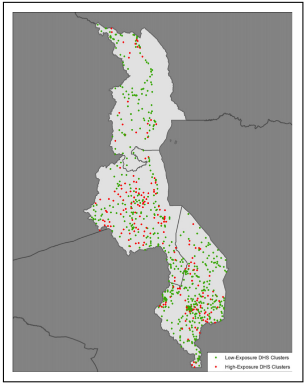
Keywords:
Informal health providers
Government bans
Child mortality

A B S T R A C T

Informal health providers ranging from drug vendors to traditional healers account for a large fraction of health care provision in developing countries. They are, however, largely unlicensed and unregulated leading to concern that they provide ineffective and, in some cases, even harmful care. A new and controversial policy tool that has been proposed to alter household health seeking behavior is an outright ban on these informal providers. The theoretical effects of such a ban are ambiguous. In this paper, we study the effect of a ban on informal (traditional) birth attendants imposed by the Malawi government in 2007. To measure the effect of the ban, we use a difference-in-difference strategy exploiting variation across time and space in the intensity of exposure to the ban. Our most conservative estimates suggest that the ban decreased use of traditional attendants by about 15 percentage points. Approximately three quarters of this decline can be attributed to an increase in use of the formal sector and the remainder is accounted for by an increase in relative/friend-attended births. Despite the rather large shift from the informal to the formal sector, we do not find any evidence of a statistically significant reduction in newborn mortality on average. The results are robust to a triple difference specification using young children as a control group. We examine several explanations for this result and find evidence consistent with quality of formal care acting as a constraint on improvements in newborn health.

source: Godlonton and Okeke (2015)

Example: Traditional Birth Attendants in Malawi



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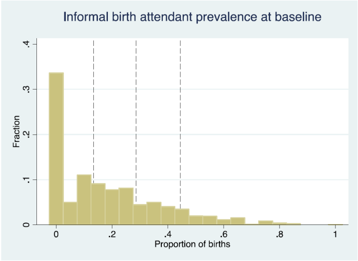

Notes: This figure plots the distribution of DHS clusters by baseline prevalence of informal birth attendant use. Dotted lines from left to right indicate the 50th, 75th, and 90th percentiles respectively.

source: Godlonton and Okeke (2015)

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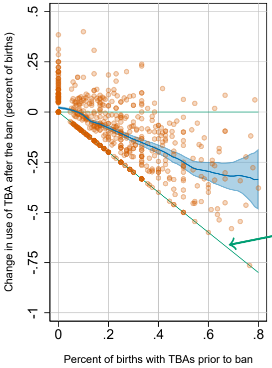
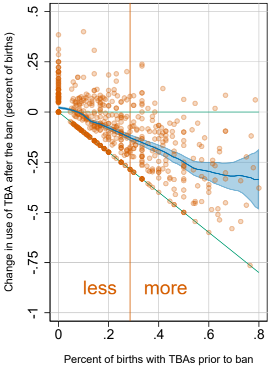


Post-ban mean must be positive

$\Rightarrow$ Decline $\uparrow$ with pre-ban mean

Example: Traditional Birth Attendants in Malawi



Continous treatment ⇒ two options:

- Discretize treatment
- Use continuous variable

Both have pros and cons in terms of power, potential for mis-specification

- ΔY specification = bivariate OLS

Generalizing the Difference-in-Differences Framework

Option 1: partition sample into more/less treated

- Treatment variable is a dummy for more-treated group
  - Coefficient is difference in means between groups

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- Estimand: difference in outcomes from increased treatment

Option 2: treatment as a continuous variable (X_i)

- OLS coefficient is weighted sum of Y_i values
- What is the estimand?

Treatment as a Continuous Variable

OLS coefficient is weighted sum of Y_i values:

$$\beta_{OLS} = \frac{COV(X,Y)}{VAR(X)} = \sum_i Y_i \left(\frac{(X_i - \bar{X})}{\sum_i (X_i - \bar{X})^2} \right)$$

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weights proportional to deviations from mean X

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weights proportional to
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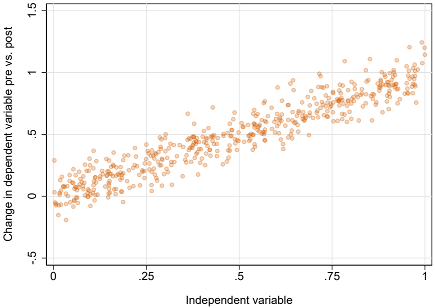
In essence, the treatment group is i with $X_i - \bar{X} > 0$

The Dose-Response Relationship



Is dose-response linear?

$\Rightarrow$ If yes: OLS FTW!

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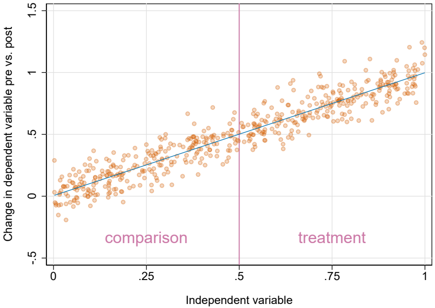

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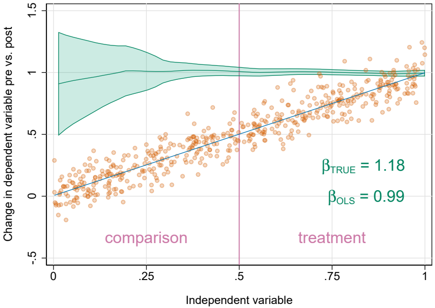

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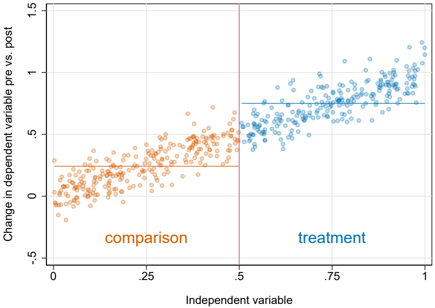
OLS is specified correctly, maximizes statisticaly power

The Dose-Response Relationship



When we discretize X_i:

$\Rightarrow$ Estimating impact of ΔX_i

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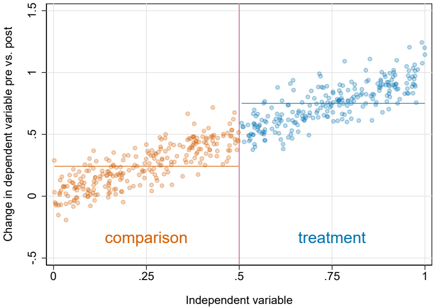

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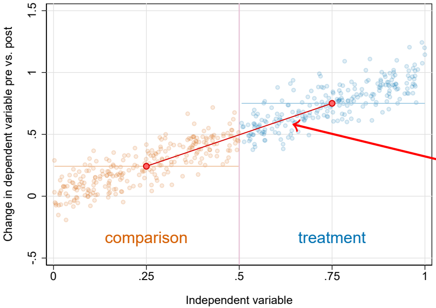

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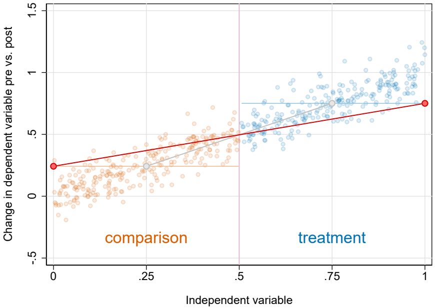

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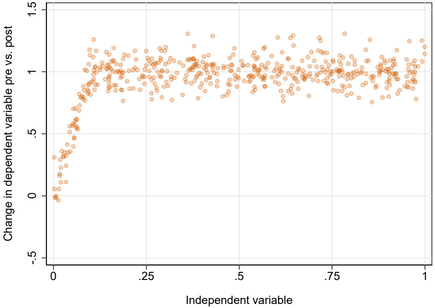
Not what we get from a dummy for "more treated"

$\Rightarrow \beta_{OLS}$ biased toward 0?

When Dose-Response Relationship is Not Linear

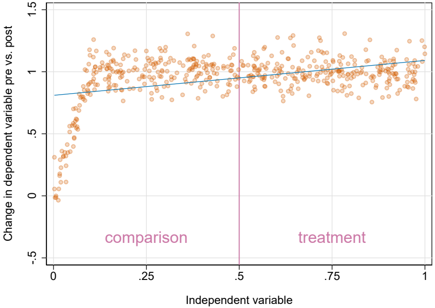


Continuous treatment variable

+ non-linear dose-response

= big trouble

When Dose-Response Relationship is Not Linear



Continuous treatment variable

$+$ non-linear dose-response

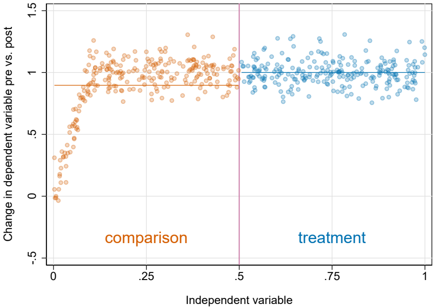
$=$ big trouble

Treatent effect when $X_i > 0$

- Not linear in X_i

$\Rightarrow \beta_{OLS}$ not ATE

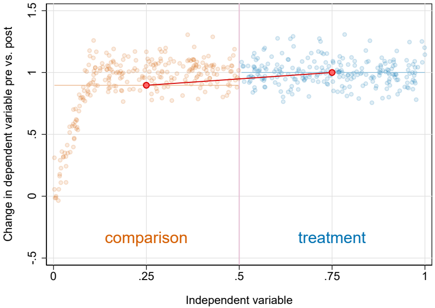
When Dose-Response Relationship is Not Linear



Binary treatment variable:

- Small difference in means
- $\Delta \bar{Y}_T$, $\Delta \bar{Y}_C$ similar
- Treatment effect in $\Delta \bar{Y}_C$

When Dose-Response Relationship is Not Linear



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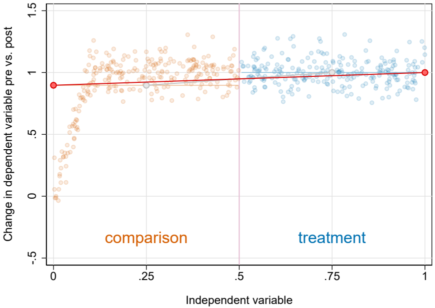
β_{OLS} unbiased estimate of impact of $\bar{X}_C$ to $\bar{X}_T$ shift

$\Rightarrow$ Conservative approach

When Dose-Response Relationship is Not Linear



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$\Rightarrow$ Conservative approach

Flavors of Diff-in-Diff: Summary

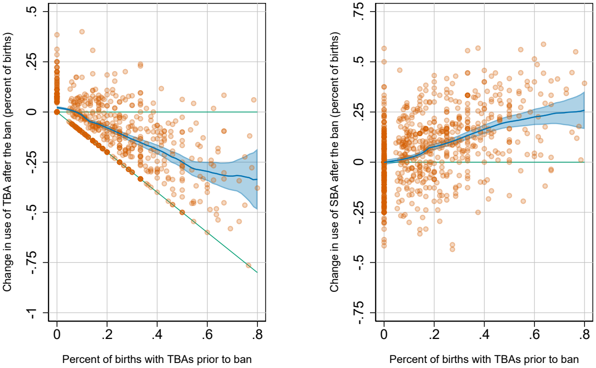
Treatment is fundamentally binary: us a treatment dummy

- Examples: Chicago police reform, NJ minimum wage
- Clear how to interpret, unbiased (w/ common trends)

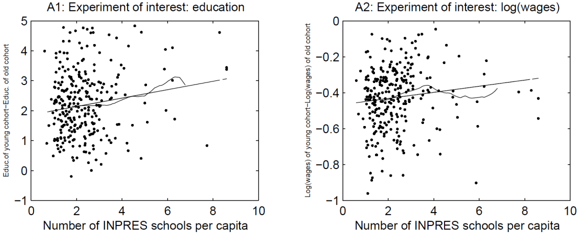
Treatment is fundamentally continuous:

- Use a treatment dummy (discretize treatment)
  - Clear how to interpret coefficient, nature of estimand
  - Understates impacts when "comparison" group is impacted
- Use continuous variation in treatment
  - Increases power when dose-response relationship close to linear
  - May give you garbage when dose-response non-linear

Dose-Response Relationship in Malawi TBAs Paper



Dose-Response Relationship in School Construction Paper



Fixed Effects

Generalized Diff-in-Diff with Fixed Effects

Widely used panel data diff-in-diff specification:

$$Y_{i,t} = \alpha + \gamma D_{i,t} + \delta \left(D_{i,t} \times Post_t \right) + \nu_t + \varepsilon_{i,t}$$

where:

- $D_{i,t}$ = treatment dummy (could be continuous variable)
- δ = diff-in-diff estimate of treatment effect
- ν_t = time-period fixed effects

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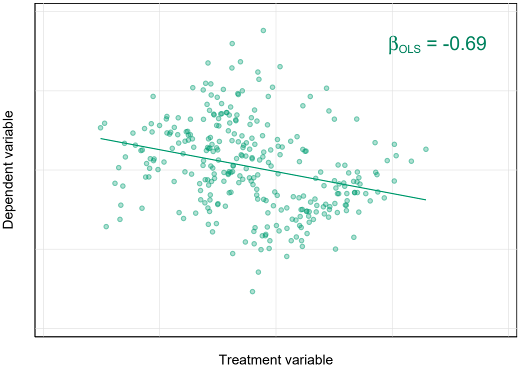
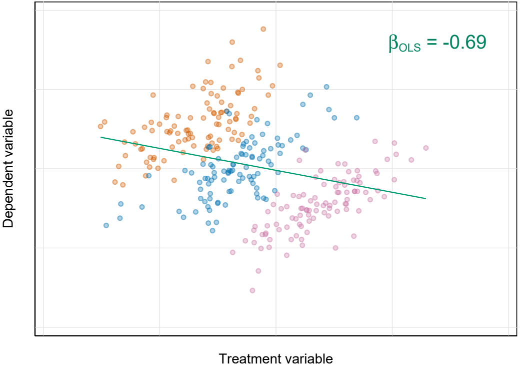
What Do Fixed Effects Do?



What Do Fixed Effects Do?



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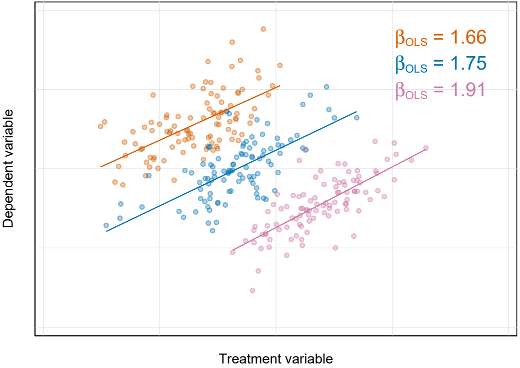

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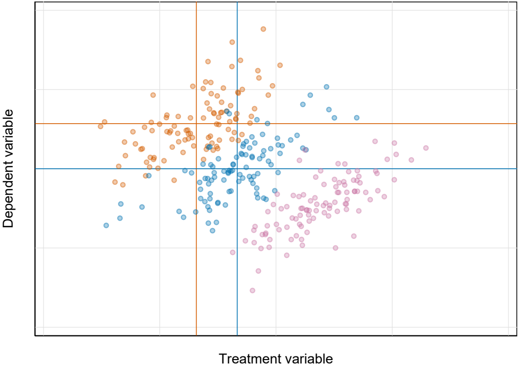

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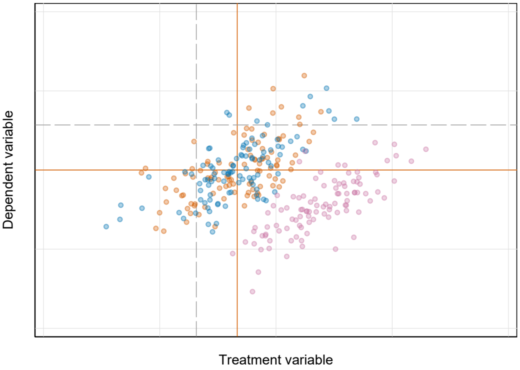

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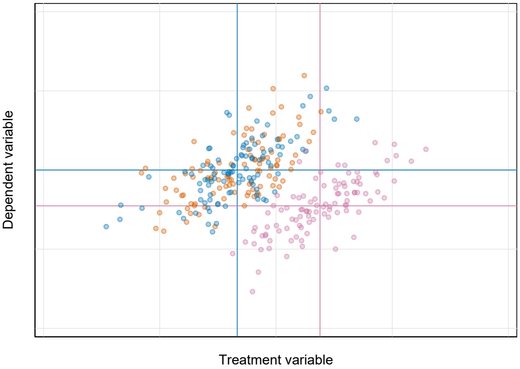

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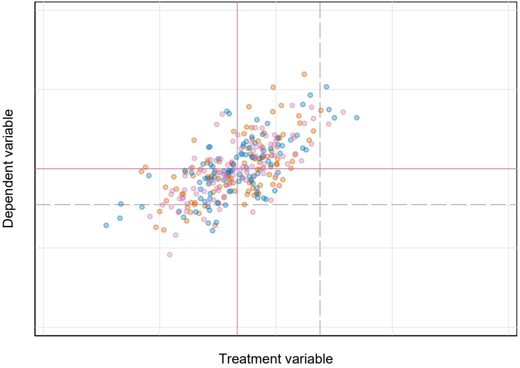

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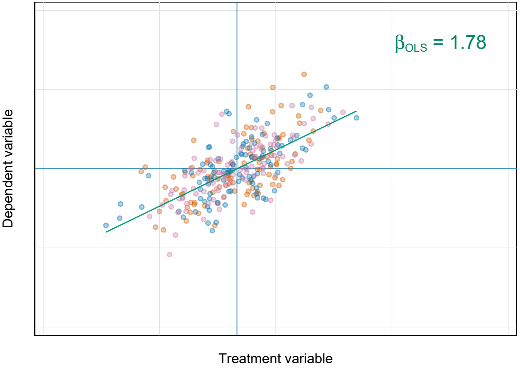

Diff-in-Diff with Time Fixed Effects

	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$
Unit 1	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$
Unit 2	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$
Unit 3	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$
Unit 4	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$
Unit 5	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$	$X_{i,t}$

What is **panel data**?

$\Rightarrow$ Multiple units, over time

Diff-in-Diff with Time Fixed Effects

	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$
Unit 1	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$
Unit 2	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$
Unit 3	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$
Unit 4	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$
Unit 5	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$	$D_{i,t}$

What is **panel data**?

$\Rightarrow$ Multiple units, over time

In a diff-in-diff setup,
some units are treated
after some period of time

Diff-in-Diff with Time Fixed Effects

	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$
Unit 1	0	0	0	0	0
Unit 2	0	0	0	0	0
Unit 3	0	0	0	1	1
Unit 4	0	0	0	1	1
Unit 5	0	0	0	1	1

What is **panel data**?

$\Rightarrow$ Multiple units, over time

In a diff-in-diff setup,
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after some period of time

Diff-in-Diff with Time Fixed Effects

	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$
Unit 1	0	0	0	0	0
Unit 2	0	0	0	0	0
Unit 3	0	0	0	1	1
Unit 4	0	0	0	1	1
Unit 5	0	0	0	1	1
$\bar{D}_t$	0	0	0	0.6	0.6

Time fixed effects:

$\Rightarrow$ Subract off mean $D_{i,t}$

Diff-in-Diff with Time Fixed Effects

	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$
Unit 1	0	0	0	-0.6	-0.6
Unit 2	0	0	0	-0.6	-0.6
Unit 3	0	0	0	0.4	0.4
Unit 4	0	0	0	0.4	0.4
Unit 5	0	0	0	0.4	0.4
$\bar{D}_t$	0	0	0	0.6	0.6

Time fixed effects:

$\Rightarrow$ Subract off mean $D_{i,t}$

Equivalent to regression on:

$\tilde{D}_{i,t} = D_{i,t} - \bar{D}_t$

Diff-in-Diff with Time Fixed Effects

	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$
Unit 1	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$
Unit 2	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$
Unit 3	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$
Unit 4	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$
Unit 5	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$	$\tilde{Y}_{i,t}$
$\bar{Y}_t$	$\bar{Y}_1$	$\bar{Y}_2$	$\bar{Y}_3$	$\bar{Y}_4$	$\bar{Y}_5$

Time fixed effects:

$\Rightarrow$ Subract off mean $D_{i,t}$

Equivalent to regression on:

$\tilde{D}_{i,t} = D_{i,t} - \bar{D}_t$

With dependent variable:

$\tilde{Y}_{i,t} = Y_{i,t} - \bar{Y}_t$

Diff-in-Diff with Time Fixed Effects

Why used time fixed effects (instead of dummy for post-treatment)?

- Fixed effects "soak up" period-specific shocks better
  - Smaller residuals $\Rightarrow$ smaller standard errors $\Rightarrow$ statistical power
- If time fixed effects yield (very) different results: be very afraid
  - Should not change coefficient in a balanced panel

Diff-in-Diff with Time Fixed Effects

Why used time fixed effects (instead of dummy for post-treatment)?

- Fixed effects "soak up" period-specific shocks better
  - Smaller residuals $\Rightarrow$ smaller standard errors $\Rightarrow$ statistical power
- If time fixed effects yield (very) different results: be very afraid
  - Should not change coefficient in a balanced panel

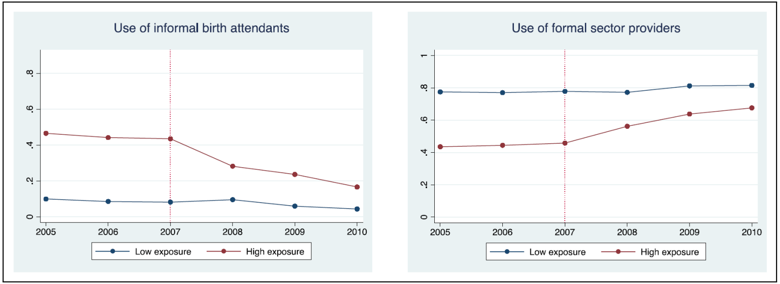
Two-way fixed effects specification:

$$Y_{i,t} = \alpha + \eta_i + \nu_t + \delta D_{i,t} + \varepsilon_{i,t}$$

where η_i is an individual FE, ν_t is a time FE, and δ is DD estimator

Use two-way fixed effects with caution when treatment starts at different times in different units, treatment is continuous, or variance of treatment differs across treated units for other reasons, as we discuss further in the next module.

Example: Malawi's Ban on Traditional Birth Attendants



source: Godlonton and Okeke (2015)

Example: Malawi's Ban on Traditional Birth Attendants

Godlonton and Okeke (2015) estimate regression specification:

$$Y_{i,t} = \alpha_1 + \delta HighExposure_c + \gamma HighExposure_c \times Post_t + X_{ict}\beta + \tau_t + \varepsilon_{ict}$$

where:

- $HighExposure_c$ = indicator for (more) treated clusters (pre-ban use of TBAs above 75$^{\text{th}}$ percentile)
- $HighExposure_c \times Post_t$ = indicator for treated cluster-months
- γ = diff-in-diff estimate of treatment effect
- X_{ict} = set of control variables (eg household size, etc.)
- τ_t = fixed effect for month of birth (eg January 2007)
- ε_{ict} = mean-zero error term

Example: Malawi's Ban on Traditional Birth Attendants

Table 5
What was the effect of the ban on the use of formal and informal sector providers?

Variables	(1)	(2)	(3)	(4)	(5)	(6)
A. Birth attendant is informal attendant						
High exposure × *Post*	−0.189***	−0.190***	−0.184***	−0.187***	−0.154***	−0.188***
	(0.0146)	(0.0130)	(0.0141)	(0.0144)	(0.0126)	(0.0146)
High exposure	0.344***	0.321***	0.318***	0.320***	0.267***	
	(0.0143)	(0.0131)	(0.0123)	(0.0127)	(0.0110)	
Post				0.0134	−0.0655	−0.000915
				(0.0667)	(0.0908)	(0.0679)
Constant	0.0411***	0.0537	0.0512	1.848***	3.525***	0.265***
	(0.00204)	(0.0415)	(0.0410)	(0.284)	(0.440)	(0.0637)
Observations	19,607	18,673	18,673	18,673	12,491	18,673
R-squared	0.138	0.149	0.150	0.148	0.113	0.209
B. Birth attendant is formal sector provider						
High exposure × *Post*	0.145***	0.144***	0.143***	0.146***	0.109***	0.150***
	(0.0157)	(0.0136)	(0.0153)	(0.0152)	(0.0152)	(0.0165)
High exposure	−0.317***	−0.270***	−0.269***	−0.271***	−0.206***	
	(0.0177)	(0.0150)	(0.0152)	(0.0149)	(0.0155)	
Post				0.0660	0.132	0.00746
				(0.0794)	(0.0889)	(0.0974)
Constant	0.808***	0.726***	0.730***	−1.668***	−2.433***	0.446***
	(0.00257)	(0.0431)	(0.0429)	(0.391)	(0.479)	(0.0995)
Controls	No	Yes	Yes	Yes	Yes	Yes
Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
Trimmed data	No	No	No	No	Yes	No
Cluster fixed effects	No	No	No	No	No	Yes
Observations	19,607	18,673	18,673	18,673	12,491	18,673
R-squared	0.088	0.132	0.134	0.131	0.104	0.218

Notes: for Panel A the dependent variable is an indicator for a birth attended by an informal birth attendant. For Panel B the dependent variable is an indicator for a birth attended by a formal-sector provider. Controls include an indicator for male births, an indicator for a multiple birth, birth order, dummies for mother's level of schooling, dummies for mother's age at birth, an indicator for women who are married or living with a partner, dummies for ethnicity and religion, dummies for the partner's educational attainment, distance to the nearest health facility, wealth quintile dummies, and a rural–urban indicator. Each column includes district and year × month fixed effects. Full set of coefficients is not shown to conserve space (see Table A.1). In Column 5, we exclude villages with baseline prevalence of 0 or 1 to account for 'floor' and 'ceiling' effects. Column 6 is equivalent to Column 3 except that district fixed effects have been replaced with cluster fixed effects. *Post* = 1 if birth occurs after December 2007. Standard errors in parentheses are clustered at the district level (there are 27 districts).
*** $p < 0.01$.
** $p < 0.05$.
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Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
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High exposure	−0.317***	−0.270***	−0.269***	−0.271***	−0.206***	
	(0.0177)	(0.0150)	(0.0152)	(0.0149)	(0.0155)	
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	(0.00257)	(0.0431)	(0.0429)	(0.391)	(0.479)	(0.0995)
Controls	No	Yes	Yes	Yes	Yes	Yes
Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
Trimmed data	No	No	No	No	Yes	No
Cluster fixed effects	No	No	No	No	No	Yes
Observations	19,607	18,673	18,673	18,673	12,491	18,673
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Notes: for Panel A the dependent variable is an indicator for a birth attended by an informal birth attendant. For Panel B the dependent variable is an indicator for a birth attended by a formal-sector provider. Controls include an indicator for male births, an indicator for a multiple birth, birth order, dummies for mother's level of schooling, dummies for mother's age at birth, an indicator for women who are married or living with a partner, dummies for ethnicity and religion, dummies for the partner's educational attainment, distance to the nearest health facility, wealth quintile dummies, and a rural–urban indicator. Each column includes district and year × month fixed effects. Full set of coefficients is not shown to conserve space (see Table A.1). In Column 5, we exclude villages with baseline prevalence of 0 or 1 to account for 'floor' and 'ceiling' effects. Column 6 is equivalent to Column 3 except that district fixed effects have been replaced with cluster fixed effects. *Post* = 1 if birth occurs after December 2007. Standard errors in parentheses are clustered at the district level (there are 27 districts).
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source: Godlonton and Okeke (2015)

Example: Malawi's Ban on Traditional Birth Attendants

Table 6
What was the effect of the ban on the use of other substitutes?

Variables	(1)	(2)	(3)	(4)	(5)	(6)
A. Birth attendant is a relative or friend						
High exposure × *Post*	0.0414***	0.0417***	0.0364***	0.0366***	0.0389***	0.0351***
	(0.00694)	(0.00725)	(0.00863)	(0.00918)	(0.0110)	(0.00962)
High exposure	−0.0256***	−0.0424***	−0.0396***	−0.0399***	−0.0496***	
	(0.00836)	(0.00933)	(0.00982)	(0.0101)	(0.0123)	
Post				−0.0476	−0.0367	0.121
				(0.0543)	(0.0812)	(0.0812)
Constant	0.105***	0.186***	0.184***	0.750***	0.251	0.202***
	(0.00151)	(0.0542)	(0.0536)	(0.236)	(0.329)	(0.0642)
Observations	19,607	18,673	18,673	18,673	12,491	18,673
R-squared	0.022	0.041	0.042	0.039	0.042	0.133
B. Birth was unattended						
High exposure × *Post*	0.00281	0.00322	0.00334	0.00247	0.00541	0.00116
	(0.00512)	(0.00491)	(0.00493)	(0.00557)	(0.00543)	(0.00518)
High exposure	0.000257	−0.00614*	−0.00622	−0.00629	−0.00931*	
	(0.00338)	(0.00339)	(0.00369)	(0.00393)	(0.00493)	
Post				0.0110	0.00680	−0.0164
				(0.0474)	(0.0572)	(0.0513)
Constant	0.0306***	0.0184	0.0173	−0.0440	−0.234	0.0319
	(0.000623)	(0.0267)	(0.0265)	(0.158)	(0.200)	(0.0346)
Controls	No	Yes	Yes	Yes	Yes	Yes
Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
Trimmed data	No	No	No	No	Yes	No
Cluster fixed effects	No	No	No	No	No	Yes
Observations	19,607	18,673	18,673	18,673	12,491	18,673
R-squared	0.009	0.033	0.034	0.033	0.038	0.097

Notes: in Panel A (top) the dependent variable is an indicator for a birth attended by a relative or friend while in Panel B (bottom) the dependent variable is an indicator for an unattended birth. Controls include an indicator for male births, an indicator for a multiple birth, birth order, dummies for mother's level of schooling, dummies for mother's age at birth, an indicator for women who are married or living with a partner, dummies for ethnicity and religion, dummies for the partner's educational attainment, distance to the nearest health facility, wealth quintile dummies, and a rural–urban indicator. Each column includes district and year × month fixed effects. Full set of coefficients not shown to conserve space (see Table A.1). In Column 5, we exclude villages with baseline prevalence of 0 or 1 to account for 'floor' and 'ceiling' effects. Column 6 is equivalent to Column 3 except that district fixed effects have been replaced with cluster fixed effects. *Post* = 1 if birth occurs after December 2007. Standard errors in parentheses are clustered at the district level (there are 27 districts).

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High exposure × *Post*	0.0414***	0.0417***	0.0364***	0.0366***	0.0389***	0.0351***
	(0.00694)	(0.00725)	(0.00863)	(0.00918)	(0.0110)	(0.00962)
High exposure	−0.0256***	−0.0424***	−0.0396***	−0.0399***	−0.0496***	
	(0.00836)	(0.00933)	(0.00982)	(0.0101)	(0.0123)	
Post				−0.0476	−0.0367	0.121
				(0.0543)	(0.0812)	(0.0812)
Constant	0.105***	0.186***	0.184***	0.750***	0.251	0.202***
	(0.00151)	(0.0542)	(0.0536)	(0.236)	(0.329)	(0.0642)
Observations	19,607	18,673	18,673	18,673	12,491	18,673
R-squared	0.022	0.041	0.042	0.039	0.042	0.133
B. Birth was unattended						
High exposure × *Post*	0.00281	0.00322	0.00334	0.00247	0.00541	0.00116
	(0.00512)	(0.00491)	(0.00493)	(0.00557)	(0.00543)	(0.00518)
High exposure	0.000257	−0.00614*	−0.00622	−0.00629	−0.00931*	
	(0.00338)	(0.00339)	(0.00369)	(0.00393)	(0.00493)	
Post				0.0110	0.00680	−0.0164
				(0.0474)	(0.0572)	(0.0513)
Constant	0.0306***	0.0184	0.0173	−0.0440	−0.234	0.0319
	(0.000623)	(0.0267)	(0.0265)	(0.158)	(0.200)	(0.0346)
Controls	No	Yes	Yes	Yes	Yes	Yes
Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
Trimmed data	No	No	No	No	Yes	No
Cluster fixed effects	No	No	No	No	No	Yes
Observations	19,607	18,673	18,673	18,673	12,491	18,673
R-squared	0.009	0.033	0.034	0.033	0.038	0.097

Notes: in Panel A (top) the dependent variable is an indicator for a birth attended by a relative or friend while in Panel B (bottom) the dependent variable is an indicator for an unattended birth. Controls include an indicator for male births, an indicator for a multiple birth, birth order, dummies for mother's level of schooling, dummies for mother's age at birth, an indicator for women who are married or living with a partner, dummies for ethnicity and religion, dummies for the partner's educational attainment, distance to the nearest health facility, wealth quintile dummies, and a rural–urban indicator. Each column includes district and year × month fixed effects. Full set of coefficients not shown to conserve space (see Table A.1). In Column 5, we exclude villages with baseline prevalence of 0 or 1 to account for 'floor' and 'ceiling' effects. Column 6 is equivalent to Column 3 except that district fixed effects have been replaced with cluster fixed effects. *Post* = 1 if birth occurs after December 2007. Standard errors in parentheses are clustered at the district level (there are 27 districts).

*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

source: Godlonton and Okeke (2015)

Example: Malawi's Ban on Traditional Birth Attendants

Table 6
What was the effect of the ban on the use of other substitutes?

Variables	(1)	(2)	(3)	(4)	(5)	(6)
A. Birth attendant is a relative or friend						
High exposure × *Post*	0.0414***	0.0417***	0.0364***	0.0366***	0.0389***	0.0351***
	(0.00694)	(0.00725)	(0.00863)	(0.00918)	(0.0110)	(0.00962)
High exposure	−0.0256***	−0.0424***	−0.0396***	−0.0399***	−0.0496***	
	(0.00836)	(0.00933)	(0.00982)	(0.0101)	(0.0123)	
Post				−0.0476	−0.0367	0.121
				(0.0543)	(0.0812)	(0.0812)
Constant	0.105***	0.186***	0.184***	0.750***	0.251	0.202***
	(0.00151)	(0.0542)	(0.0536)	(0.236)	(0.329)	(0.0642)
Observations	19,607	18,673	18,673	18,673	12,491	18,673
R-squared	0.022	0.041	0.042	0.039	0.042	0.133
B. Birth was unattended						
High exposure × *Post*	0.00281	0.00322	0.00334	0.00247	0.00541	0.00116
	(0.00512)	(0.00491)	(0.00493)	(0.00557)	(0.00543)	(0.00518)
High exposure	0.000257	−0.00614*	−0.00622	−0.00629	−0.00931*	
	(0.00338)	(0.00339)	(0.00369)	(0.00393)	(0.00493)	
Post				0.0110	0.00680	−0.0164
				(0.0474)	(0.0572)	(0.0513)
Constant	0.0306***	0.0184	0.0173	−0.0440	−0.234	0.0319
	(0.000623)	(0.0267)	(0.0265)	(0.158)	(0.200)	(0.0346)
Controls	No	Yes	Yes	Yes	Yes	Yes
Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
Trimmed data	No	No	No	No	Yes	No
Cluster fixed effects	No	No	No	No	No	Yes
Observations	19,607	18,673	18,673	18,673	12,491	18,673
R-squared	0.009	0.033	0.034	0.033	0.038	0.097

Notes: in Panel A (top) the dependent variable is an indicator for a birth attended by a relative or friend while in Panel B (bottom) the dependent variable is an indicator for an unattended birth. Controls include an indicator for male births, an indicator for a multiple birth, birth order, dummies for mother's level of schooling, dummies for mother's age at birth, an indicator for women who are married or living with a partner, dummies for ethnicity and religion, dummies for the partner's educational attainment, distance to the nearest health facility, wealth quintile dummies, and a rural–urban indicator. Each column includes district and year × month fixed effects. Full set of coefficients not shown to conserve space (see Table A.1). In Column 5, we exclude villages with baseline prevalence of 0 or 1 to account for 'floor' and 'ceiling' effects. Column 6 is equivalent to Column 3 except that district fixed effects have been replaced with cluster fixed effects. *Post* = 1 if birth occurs after December 2007. Standard errors in parentheses are clustered at the district level (there are 27 districts).

*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

source: Godlonton and Okeke (2015)

Example: Malawi's Ban on Traditional Birth Attendants

Table 7
What was the effect of the ban on newbom deaths?

Variables	(1)	(2)	(3)	(4)	(5)	(6)
A. Child death within the first week						
High exposure × *Post*	−2.939	−2.742	0.319	−0.645	1.712	−0.344
	(3.500)	(3.530)	(3.669)	(3.706)	(4.121)	(3.508)
High exposure	5.311**	5.383*	4.528	4.661	2.850	
	(2.465)	(2.812)	(2.854)	(2.912)	(3.380)	
Post				−25.62	−33.96	−13.10
				(15.37)	(20.75)	(27.30)
Constant	21.39***	8.271	8.075	133.3***	139.7***	5.029
	(0.414)	(9.116)	(9.053)	(34.58)	(35.83)	(14.15)
Observations	35,246	33,748	33,748	33,748	22,317	33,748
R-squared	0.005	0.010	0.010	0.008	0.009	0.037
B. Child death within the first month						
High exposure × *Post*	−4.150	−4.414	−1.316	−1.908	−0.211	−2.760
	(4.242)	(4.274)	(4.369)	(4.515)	(4.603)	(4.337)
High exposure	6.659**	6.292*	5.465	5.428	3.899	
	(3.142)	(3.395)	(3.472)	(3.531)	(3.994)	
Post				−35.98	−54.18	−7.293
				(26.75)	(35.46)	(44.58)
Constant	31.90***	22.13*	21.72*	208.7***	231.5***	21.07
	(0.543)	(11.61)	(11.49)	(41.97)	(54.47)	(15.41)
Controls	No	Yes	Yes	Yes	Yes	Yes
Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
Trimmed data	No	No	No	No	Yes	No
Cluster fixed effects	No	No	No	No	No	Yes
Observations	35,246	33,748	33,748	33,748	22,317	33,748
R-squared	0.005	0.012	0.012	0.010	0.012	0.038

Notes: in Panel A the dependent variable is an indicator for a newborn death within a week of being born. In Panel B the dependent variable is an indicator for a newborn death within a month of being born. Both variables have been scaled to allow coefficients to be interpretable as *X* per 1000 live births. Controls include indicators for male births, first births and young mothers (age < 18), dummies for mother's level of schooling, an indicator for women who are married or living with a partner, dummies for ethnicity and religion, dummies for the partner's educational attainment, distance to the nearest health facility, wealth quintile dummies, and a rural–urban indicator. Each column includes district and year × month fixed effects. Full set of coefficients is not shown to conserve space (see Table A.1). In Column 5, we exclude villages with baseline prevalence of 0 or 1 to account for 'floor' and 'ceiling' effects. Column 6 is equivalent to Column 3 except that district fixed effects have been replaced with cluster fixed effects. *Post* = 1 if birth occurs after December 2007. Standard errors in parentheses are clustered at the district level (there are 27 districts).

*** $p < 0.01$.
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source: Godlonton and Okeke (2015)

Example: Malawi's Ban on Traditional Birth Attendants

Table 7
What was the effect of the ban on newbom deaths?

Variables	(1)	(2)	(3)	(4)	(5)	(6)
A. Child death within the first week						
High exposure × *Post*	−2.939	−2.742	0.319	−0.645	1.712	−0.344
	(3.500)	(3.530)	(3.669)	(3.706)	(4.121)	(3.508)
High exposure	5.311**	5.383*	4.528	4.661	2.850	
	(2.465)	(2.812)	(2.854)	(2.912)	(3.380)	
Post				−25.62	−33.96	−13.10
				(15.37)	(20.75)	(27.30)
Constant	21.39***	8.271	8.075	133.3***	139.7***	5.029
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	(3.142)	(3.395)	(3.472)	(3.531)	(3.994)	
Post				−35.98	−54.18	−7.293
				(26.75)	(35.46)	(44.58)
Constant	31.90***	22.13*	21.72*	208.7***	231.5***	21.07
	(0.543)	(11.61)	(11.49)	(41.97)	(54.47)	(15.41)
Controls	No	Yes	Yes	Yes	Yes	Yes
Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
Trimmed data	No	No	No	No	Yes	No
Cluster fixed effects	No	No	No	No	No	Yes
Observations	35,246	33,748	33,748	33,748	22,317	33,748
R-squared	0.005	0.012	0.012	0.010	0.012	0.038

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Post				−35.98	−54.18	−7.293
				(26.75)	(35.46)	(44.58)
Constant	31.90***	22.13*	21.72*	208.7***	231.5***	21.07
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Controls	No	Yes	Yes	Yes	Yes	Yes
Controls × *Post*	No	No	Yes	Yes	Yes	Yes
District-specific trend	No	No	No	Yes	Yes	No
Trimmed data	No	No	No	No	Yes	No
Cluster fixed effects	No	No	No	No	No	Yes
Observations	35,246	33,748	33,748	33,748	22,317	33,748
R-squared	0.005	0.012	0.012	0.010	0.012	0.038

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*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

source: Godlonton and Okeke (2015)

Example: School Construction in Indonesia

Main empirical specification in Duflo (2001):

$$S_{ijk} = \alpha + \eta_j + \beta_k + \gamma \left(Intensity_j * Young_i \right) + \boldsymbol{C}_j \delta + \varepsilon_{ijk}$$

where:

- S_{ijk} = education of individual i born in region j in year k
- η_j = region of birth fixed effect
- β_k = year of birth fixed effect
- $Young_i$ = dummy for being 6 or younger in 1974 (treatment group)
- $Intensity_j$ = INPRES schools per thousand school-aged children
- $\boldsymbol{C}_j$ = a vector of region-specific controls (that change over time)

Example: School Construction in Indonesia

Dependent Variable: Years of Education

	Obs.	OLS (1)	OLS (2)	OLS (3)
Panel A: Entire Sample				
$Intensity_j * Young_i$	78,470	0.124	0.150	0.188
		(0.025)	(0.026)	(0.029)
Panel B: Sample of Wage Earners				
$Intensity_j * Young_i$	31,061	0.196	0.199	0.259
		(0.042)	(0.043)	(0.050)
Controls Included:				
YOB∗enrollment rate in 1971		No	Yes	Yes
YOB∗other INPRES programs		No	No	Yes

Sample includes individuals aged 2 to 6 or 12 to 17 in 1974. All Specifications include region of birth dummies, year of birth dummies, and interactions between the year of birth dummis and the number of children in the region of birth (in 1971). Standard errors are in parentheses.

Example: School Construction in Indonesia

Dependent Variable: Log Hourly Wages (as Adults)

	Obs.	OLS (1)	OLS (2)	OLS (3)
Panel A: Sample of Wage Earners				
$Intensity_j * Young_i$	31,061	0.0147	0.0172	0.027
		(0.007)	(0.007)	(0.008)
Controls Included:				
YOB$*$enrollment rate in 1971		No	Yes	Yes
YOB$*$other INPRES programs		No	No	Yes

Sample includes individuals aged 2 to 6 or 12 to 17 in 1974. All Specifications include region of birth dummies, year of birth dummies, and interactions between the year of birth dummis and the number of children in the region of birth (in 1971). Standard errors are in parentheses.

Thank you!