



# DIFFERENCE IN DIFFERENCES & PANEL DATA

## Technical Track Session III

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These slides constitute supporting material to the *Impact Evaluation in Practice Handbook* : Gertler, P. J.; Martinez, S., Premand, P., Rawlings, L. B. and Christel M. J. Vermeersch, 2010, *Impact Evaluation in Practice: Ancillary Material*, The World Bank, Washington DC ([www.worldbank.org/ieinpractice](http://www.worldbank.org/ieinpractice)). The content of this presentation reflects the views of the authors and not necessarily those of the World Bank.

# Structure of this session

- 1 When do we use Differences-in-Differences? (*Diff-in-Diff* or *DD*)
- 2 Estimation strategy: 3 ways to look at *DD*
- 3 Examples:
  - Extension of education services (*Indonesia*)
  - Water for life (*Argentina*)

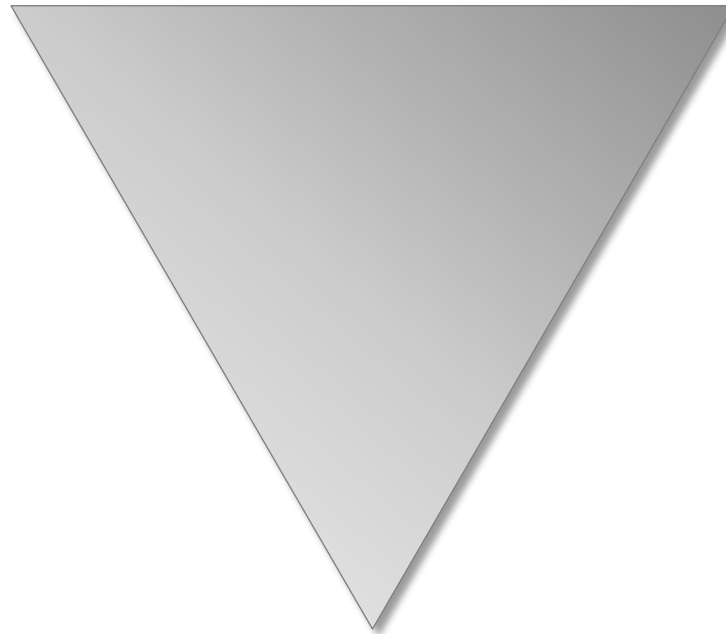
# 1 When do we use DD?

- We can't always randomize  
E.g. Estimating the impact of a "past" program
- As always, we need to identify
  - which is the group affected by the policy change ("*treatment*"), and
  - which is the group that is not affected ("*comparison*")
- We can try to find a "natural experiment" that allows us to identify the impact of a policy
  - E.g. An unexpected change in policy
  - E.g. A policy that only affects 16 year-olds but not 15 year-olds
  - In general, exploit **variation** of policies in time and space
- The quality of the comparison group determines the quality of the evaluation.

# 2 3 ways to look at DD

In a "Box"

Graphically



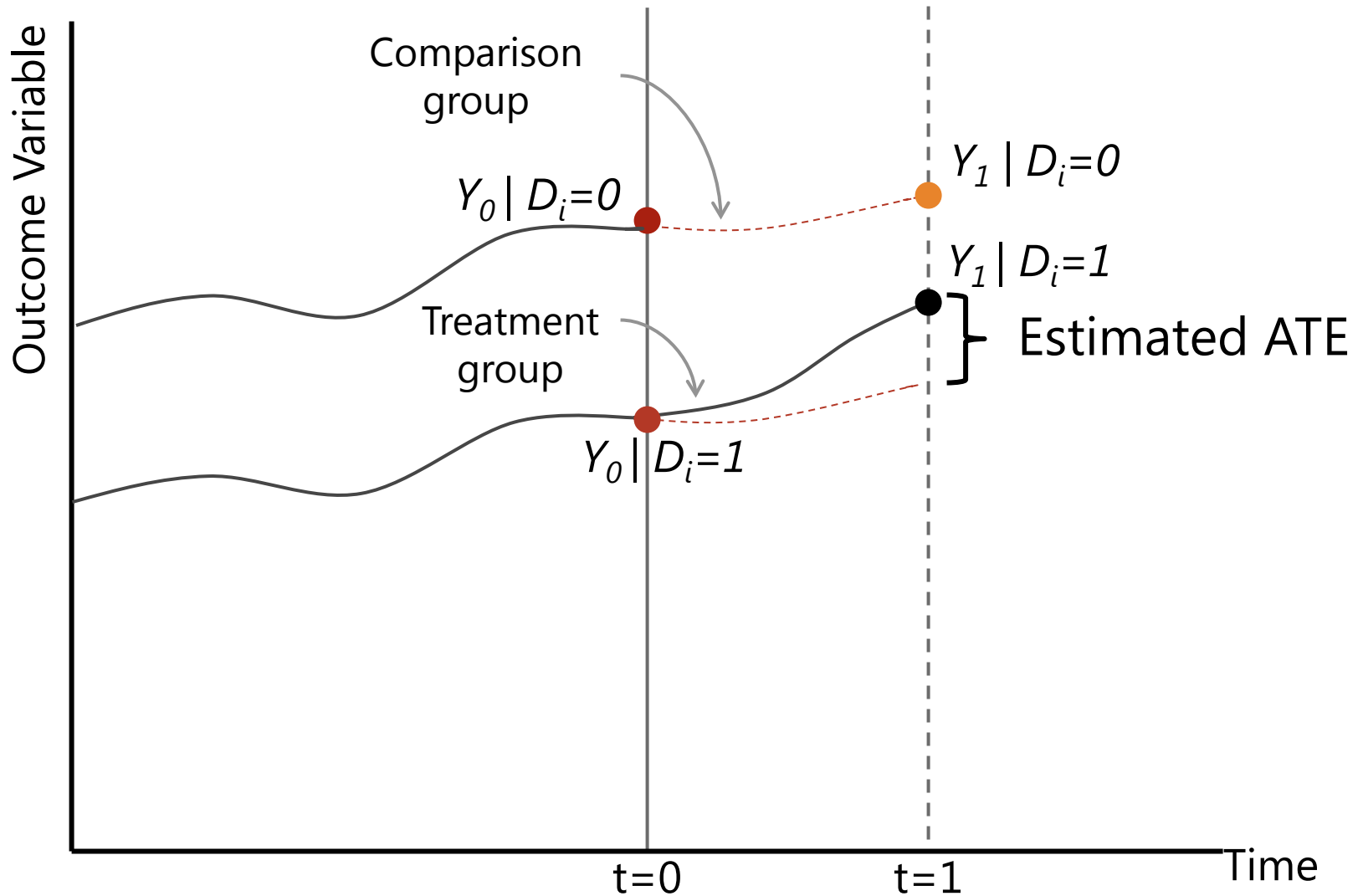
In a Regression

# The "box"

	<b>Group affected by the policy change</b> <i>(treatment)</i>	<b>Group that is not affected by the policy change</b> <i>(comparison)</i>
After the program start	$Y_1   D_i=1$	$Y_1   D_i=0$
Before the program start	$Y_0   D_i=1$	$Y_0   D_i=0$
<b>Difference</b>	$(Y_1   D=1) - (Y_0   D=1)$	$(Y_1   D=0) - (Y_0   D=0)$

$$DD = [(Y_1 | D=1) - (Y_0 | D=1)] - [(Y_1 | D=0) - (Y_0 | D=0)]$$

# Graphically



$$DD = [(Y_1 | D=1) - (Y_0 | D=1)] - [(Y_1 | D=0) - (Y_0 | D=0)]$$

# Regression (for 2 time periods)

$$Y_{it} = \alpha + \beta.1(t = 1) + \gamma.1(D_i = 1) + \delta.1(t = 1).1(D_i = 1) + \varepsilon_{it}$$

⇓

$$E(Y_{i1} | D_i = 1) = ???$$

$$E(Y_{i0} | D_i = 1) = ???$$

$$E(Y_{i1} | D_i = 0) = ???$$

$$E(Y_{i0} | D_i = 0) = ???$$

⇓

$$\begin{aligned} DD &= (E(Y_{i1} | D_i = 1) - E(Y_{i0} | D_i = 1)) - (E(Y_{i1} | D_i = 0) - E(Y_{i0} | D_i = 0)) \\ &= ??? \end{aligned}$$

$$DD = [(Y_1 | D = 1) - (Y_0 | D = 1)] - [(Y_1 | D = 0) - (Y_0 | D = 0)]$$

# Regression (for 2 time periods)

$$Y_{it} = \alpha + \beta.1(t = 1) + \gamma.1(D_i = 1) + \delta.1(t = 1).1(D_i = 1) + \varepsilon_{it}$$

⇓

$$E(Y_{i1} | D_i = 1) = \alpha + \beta.1 + \gamma.1 + \delta.1.1 + E(\varepsilon_{i1} | D_i = 1) = \alpha + \beta + \gamma + \delta$$

$$E(Y_{i0} | D_i = 1) = ???$$

$$E(Y_{i1} | D_i = 0) = ???$$

$$E(Y_{i0} | D_i = 0) = ???$$

⇓

$$\begin{aligned} DD &= (E(Y_{i1} | D_i = 1) - E(Y_{i0} | D_i = 1)) - (E(Y_{i1} | D_i = 0) - E(Y_{i0} | D_i = 0)) \\ &= ??? \end{aligned}$$

$$DD = [(Y_1 | D = 1) - (Y_0 | D = 1)] - [(Y_1 | D = 0) - (Y_0 | D = 0)]$$



# Regression (for 2 time periods)

$$Y_{it} = \alpha + \beta \cdot 1(t = 1) + \gamma \cdot 1(D_i = 1) + \delta \cdot 1(t = 1) \cdot 1(D_i = 1) + \varepsilon_{it}$$

⇓

$$E(Y_{i1} | D_i = 1) = \alpha + \beta \cdot 1 + \gamma \cdot 1 + \delta \cdot 1 \cdot 1 + E(\varepsilon_{i1} | D_i = 1) = \alpha + \beta + \gamma + \delta$$

$$E(Y_{i0} | D_i = 1) = \alpha + \beta \cdot 0 + \gamma \cdot 1 + \delta \cdot 0 \cdot 1 + E(\varepsilon_{i0} | D_i = 1) = \alpha + \gamma$$

$$E(Y_{i1} | D_i = 0) = \alpha + \beta \cdot 1 + \gamma \cdot 0 + \delta \cdot 1 \cdot 0 + E(\varepsilon_{i1} | D_i = 0) = \alpha + \beta$$

$$E(Y_{i0} | D_i = 0) = \alpha + \beta \cdot 0 + \gamma \cdot 0 + \delta \cdot 0 \cdot 0 + E(\varepsilon_{i0} | D_i = 0) = \alpha$$

⇓

$$DD = (E(Y_{i1} | D_i = 1) - E(Y_{i0} | D_i = 1)) - (E(Y_{i1} | D_i = 0) - E(Y_{i0} | D_i = 0))$$

$$= (\beta + \delta) - \beta$$

$$= \delta$$

$$DD = [(Y_1 | D = 1) - (Y_0 | D = 1)] - [(Y_1 | D = 0) - (Y_0 | D = 0)]$$

# If we have more than 2 time periods/groups:

We use a regression with fixed effects for time and group...

$$Y_{it} = \alpha + \sum_{\tau=1}^T \beta_{\tau} \cdot 1(t = \tau) + \sum_{i=1}^I \gamma_i \cdot 1(i = i) + \delta \cdot D_{it} + \varepsilon_{it}$$

where  $D_{it}$  is the intensity of the  $D$  treatment in group  $i$  in period  $t$ .

# Identification in DD

- The identification of the treatment effect is based on the inter-temporal variation between the groups.
- *i.e.* Changes in the outcome variable  $Y$  over time, that are specific to the treatment groups.
- *i.e.* Jumps in trends in the outcome variable, that happen only for the treatment groups, not for the comparison groups, exactly at the time that the treatment kicks in.

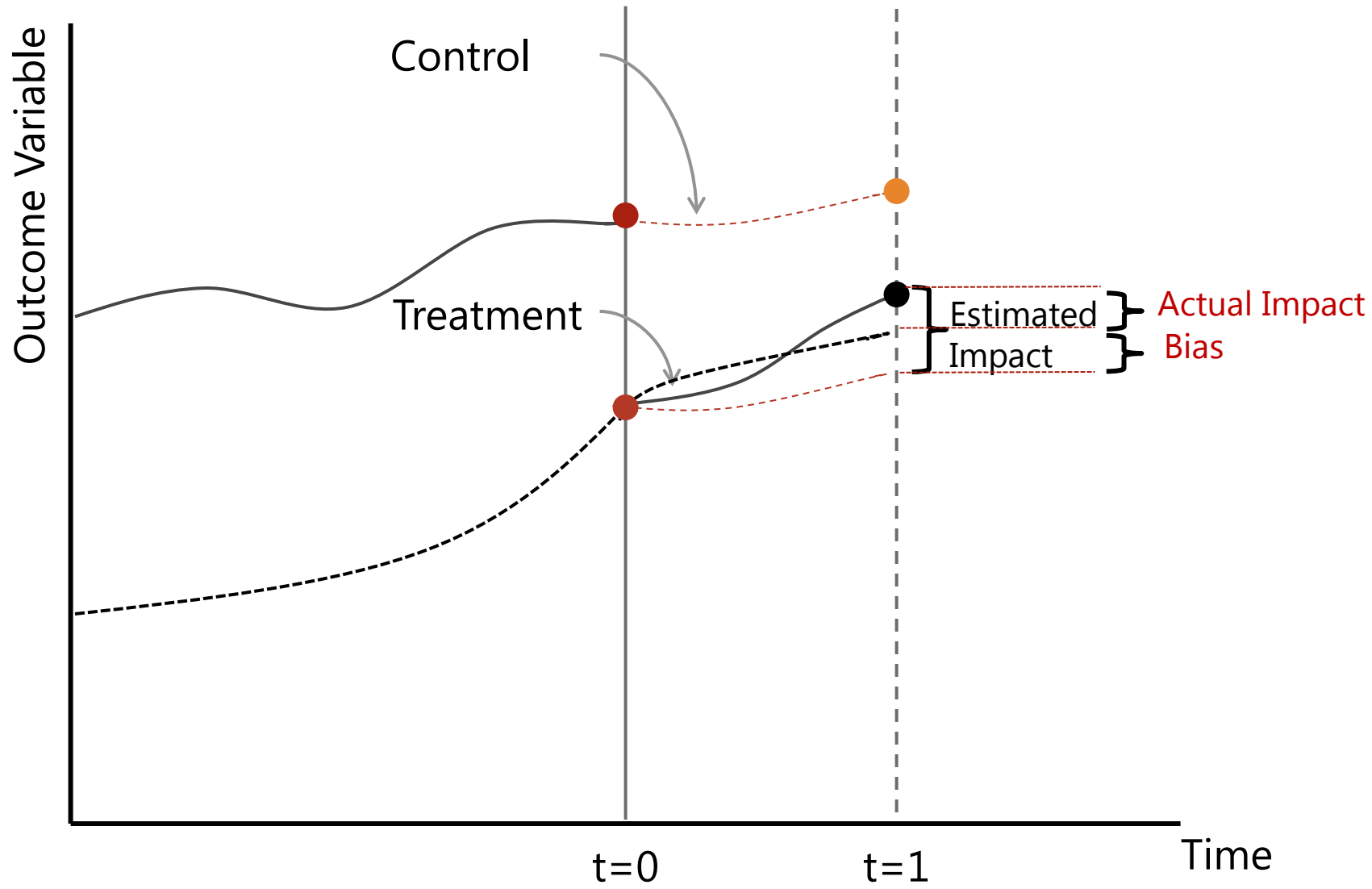
# Warnings

- DD/ fixed effects control for:
  - Fixed group effects. *E.g. Farmers who own their land, farmers who don't own their land*
  - Effects that are common to all groups at one particular point in time, or “common trends”. *E.g. The 2006 drought affected all farmers, regardless of who owns the land*
- Valid only when the policy change has an **immediate impact** on the outcome variable.  
If there is a delay in the impact of the policy change, we need to use lagged treatment variables.

# Warnings

- DD attributes any differences in trends between the treatment and control groups, that occur at the same time as the intervention, to that intervention.
- If there are other factors that affect the difference in trends between the two groups, then the estimation will be **biased!**

# *Violation of Equal Trend Assumption*



# Sensitivity analysis for diff-in-diff

- Perform a “placebo” DD, i.e. use a “fake” treatment group
  - Ex. for previous years (e.g. Years -2, -1).
  - Or using as a treatment group a population you know was NOT affected
  - If the DD estimate is different from 0, the trends are not parallel, and our original DD is likely to be biased.
- Use a different comparison group
  - The two DDs should give the same estimates.
- Use an outcome variable  $Y_2$  which you know is NOT affected by the intervention:
  - Using the same comparison group and treatment year
  - If the DD estimate is different from zero, we have a problem

# Frequently occurring issues in DD

- Participation is based in difference in outcomes prior to the intervention. E.g. "*Ashenfelter dip*": selection into treatment influence by transitory shocks on past outcomes (Ashenfelter, 1978; Chay et al., 2005 ).
- If program impact is heterogeneous across individual characteristics, pre-treatment differences in observed characteristics can create non-parallel outcome dynamics (Abadie, 2005).
- Similarly, bias would occur when the size of the response depends in a non-linear way on the size of the intervention, and we compare a group with high treatment intensity, with a group with low treatment intensity
- When outcomes within the unit of time/group are correlated, OLS standard errors understate the st. dev. of the DD estimator (Bertrand et al., 2004).



**Example 1**

**Schooling and labor market  
consequences of school construction  
in Indonesia: evidence from an unusual  
policy experiment**

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Esther Duflo, MIT  
**American Economic Review, Sept 2001**

# Research questions

School infrastructure → Educational achievement?

Educational achievement → Salary level?

What is the economic return on schooling?

# Program description

- **1973-1978:** The Indonesian government built 61,000 schools *equivalent to one school per 500 children between 5 and 14 years old*
- The enrollment rate increased from 69% to **85%** between 1973 and 1978
- The number of schools built in each region depended on the number of children out of school in those regions in 1972, before the start of the program.

# Identification of the treatment effect

There are 2 sources of variations in the intensity of the program for a given individual:

- By region

There is variation in the number of schools received in each region.

- By age

- Children who were older than 12 years in 1972 did not benefit from the program.
- The younger a child was 1972, the more it benefited from the program –because she spent more time in the new schools.

# Sources of data

- 1995 population census.  
Individual-level data on:
  - birth date
  - 1995 salary level
  - 1995 level of education
- The intensity of the building program in the birth region of each person in the sample.
- **Sample:** men born between 1950 and 1972.

# A first estimation of the impact

## Step 1: Let's simplify the problem and estimate the impact of the program.

- We simplify the intensity of the program:  
*high or low*
- We simplify the groups of children affected by the program
  - *Young* cohort of children who benefitted
  - *Older* cohort of children who did not benefit

# Let's look at the average of the outcome variable "years of schooling"

## Intensity of the Building Program

Age in 1974	High	Low	
2-6 (young cohort)	8.49	9.76	
12-17 (older cohort)	8.02	9.4	
<b>Difference</b>	<b>0.47</b>	<b>0.36</b>	<b>0.12 DD</b> <b>(0.089)</b>

# Let's look at the average of the outcome variable "years of schooling"

## Intensity of the Building program

Age in 1974	High	Low	Difference
2-6 (young cohort)	8.49	9.76	<b>-1.27</b>
12-17 (older cohort)	8.02	9.4	<b>-1.39</b>
			<b>0.12 DD</b> <b>(0.089)</b>



# Placebo DD

(Cf. p.798, Table 3, panel B)

## Idea:

- Look for 2 groups whom you know did not benefit, compute a DD, and check whether the estimated effect is 0.
- If it is NOT 0, we're in trouble...

## Intensity of the Building Program

Age in 1974	High	Low	
12-17	8.02	9.40	
18-24	7.70	9.12	
<b>Difference</b>	<b>0.32</b>	<b>0.28</b>	<b>0.034 DD</b> <b>(0.098)</b>

## Step 2: Let's estimate this with a regression

$$S_{ijk} = c + \alpha_j + \beta_k + \gamma.(P_j.T_i) + \delta.(C_j.T_i) + \varepsilon_{ijk}$$

with

$S_{ijk}$  = education level of person  $i$  in  
region  $j$  in cohort  $k$

$P_j$  = 1 if the person was born in a region with a  
high program intensity

$T_i$  = 1 if the person belongs to the "young" cohort

$C_j$  = dummy variable for region  $j$

$\beta_k$  = cohort fixed-effect

$\alpha_j$  = district of birth fixed-effect

$\varepsilon_{ijk}$  = error term for person  $i$  in region  $j$  in cohort  $k$

# Step 3: Let's use additional information

We will use the intensity of the program in each region:

$$S_{ijk} = c + \alpha_j + \beta_k + \gamma \cdot (P_j \cdot T_i) + \delta \cdot (C_j \cdot T_i) + \varepsilon_{ijk}$$

where  $P_j$  = the intensity of building activity in region j

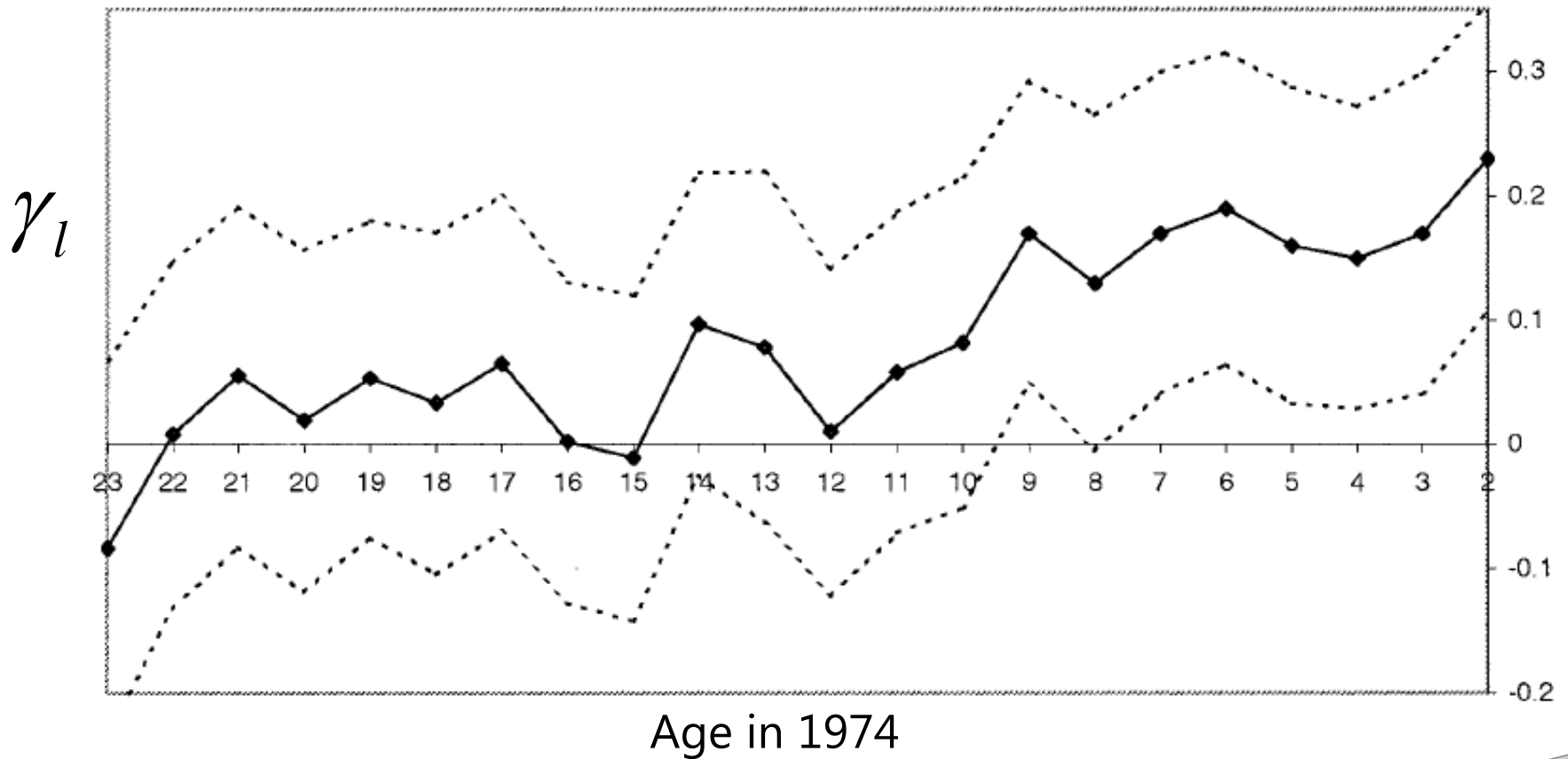
$C_j$  = a vector of regional characteristics

We estimate the effect of the program for each cohort separately:

$$S_{ijk} = c + \alpha_j + \beta_k + \sum_{l=2}^{23} \gamma_l \cdot (P_j \cdot d_i) + \sum_{l=2}^{23} \delta_l C_j T_i + \varepsilon_{ijk}$$

where  $d_i$  = a dummy variable for belonging to cohort i

# Program effect per cohort



# For $y$ = Dependent variable = Salary

	Log(wages)		
	Level of program in region of birth		
	High (4)	Low (5)	Difference (6)
<i>Panel A: Experiment of Interest</i>			
Aged 2 to 6 in 1974	6.61 (0.0078)	6.73 (0.0064)	-0.12 (0.010)
Aged 12 to 17 in 1974	6.87 (0.0085)	7.02 (0.0069)	-0.15 (0.011)
Difference	-0.26 (0.011)	-0.29 (0.0096)	0.026 (0.015)
<i>Panel B: Control Experiment</i>			
Aged 12 to 17 in 1974	6.87 (0.0085)	7.02 (0.0069)	-0.15 (0.011)
Aged 18 to 24 in 1974	6.92 (0.0097)	7.08 (0.0076)	-0.16 (0.012)
Difference	0.056 (0.013)	0.063 (0.010)	0.0070 (0.016)

# Conclusion

- **Results:** For each school built per 1000 students;
  - The average educational achievement increase by **0.12- 0.19 years**
  - The average salaries increased by **2.6 – 5.4 %**
- Making sure the DD estimation is accurate:
  - A placebo DD gave **0** estimated effect
  - Use various alternative specifications
  - Check that the impact estimates for each age cohort make sense.

# References

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**THANK YOU**





# Q & A

**Example 2**  
**Water for Life:**  
**The Impact of the Privatization of**  
**Water Services on Child Mortality**

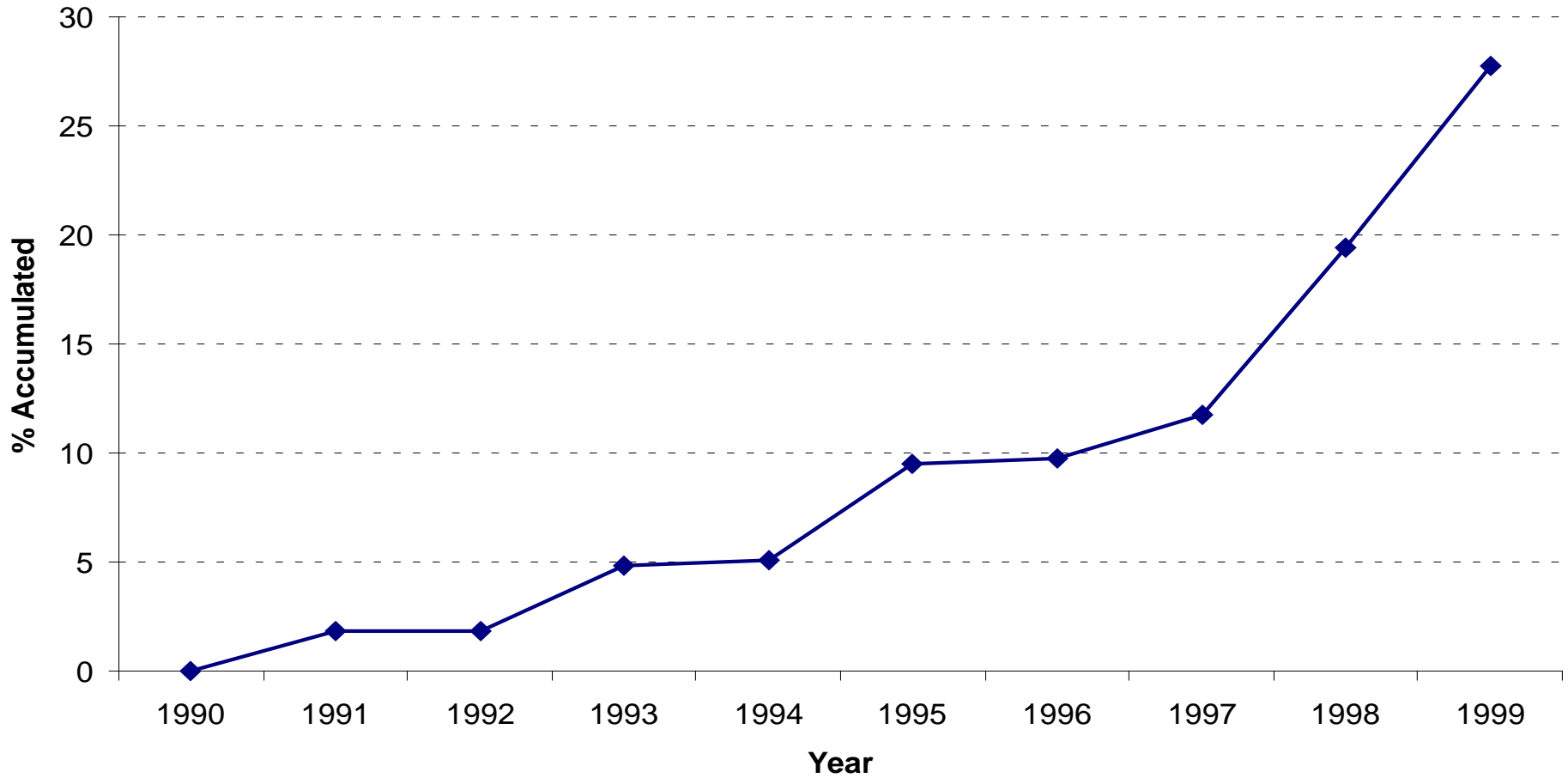
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Sebastián Galiani, Universidad de San Andrés  
Paul Gertler, UC Berkeley  
Ernesto Schargrodsky, Universidad Torcuato Di Tella  
**JPE (2005)**

# Changes in water services delivery 1990-1999

Type of provision methods	Number of municipalities	%
Always public	196	39.7%
Always a not-for-profit cooperative	143	28.9%
Converted from public to private	138	27.9%
Always private	1	0.2%
No information	16	3.2%
<b>Total</b>	<b>494</b>	<b>100%</b>

# Figure1: Percentage of Municipalities with Privatized Water Systems



# Use “*outside*” factors to determine who privatizes

- The political party that governed the municipality
  - Federal, Peronist y Provincial parties: **allowed privatization**
  - Radical party: **did not allow privatization**
- Which party was in power/whether the water got privatized did not depend on:
  - Income, unemployment, inequality at the municipal level
  - Recent changes in infant mortality rates

# Regression

$$y_{it} = \alpha dI_{it} + \beta x_{it} + \lambda_t + \mu_i + \varepsilon_{it}$$

*where*

$y_{it}$  = infant mortality rate in munic.  $i$  in year  $t$

$dI_{it}$  = dummy variable that takes value 1 if

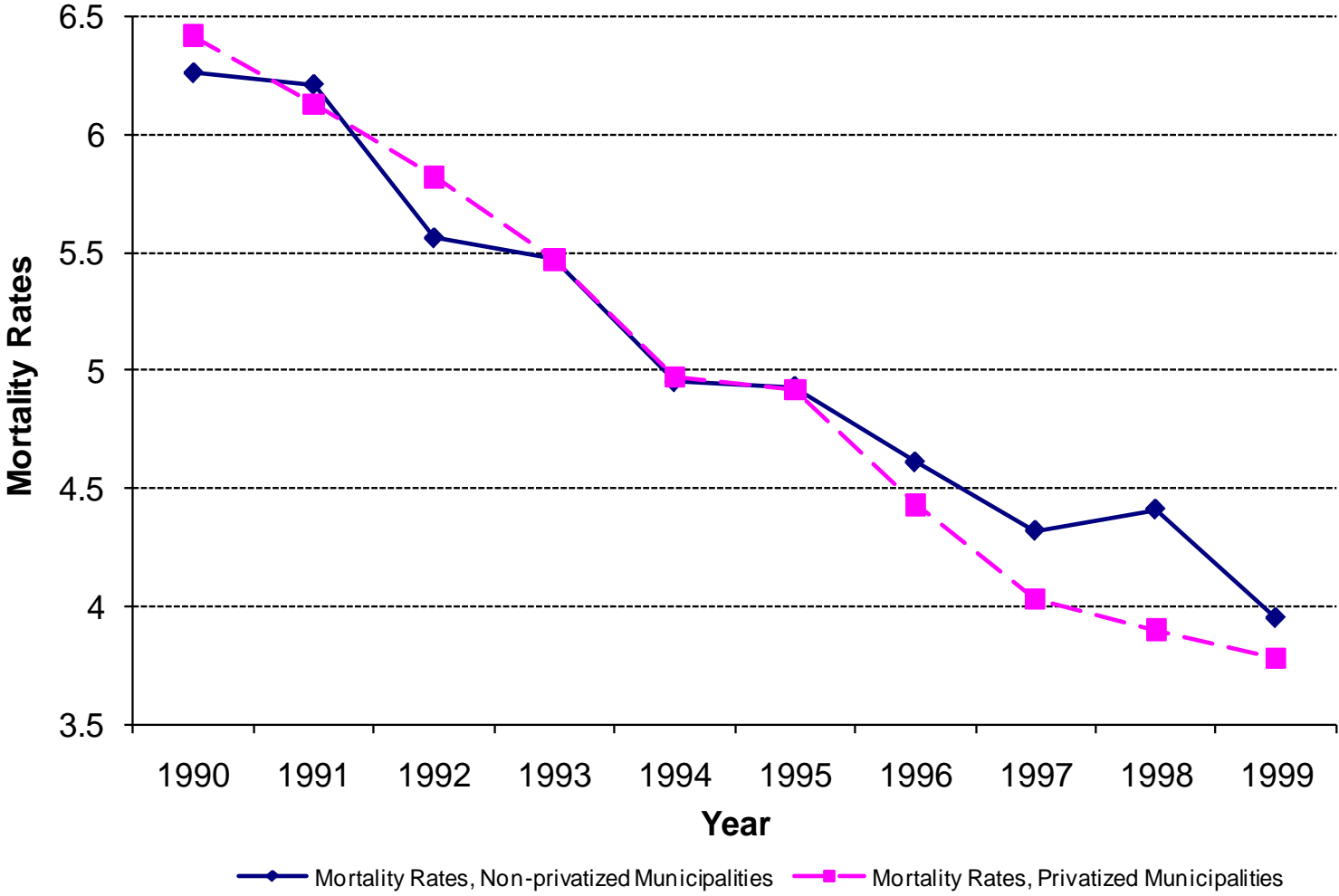
municipality  $i$  has private water provider in year  $t$

$x_{it}$  = vector of covariates

$\lambda_t$  = fixed time effect

$\mu_i$  = fixed municipality effect

**Figure 4: Evolution of Mortality Rates for Municipalities with Privatized vs. Non-Privatized Water Services**



# DD results: Privatization reduced infant mortality

	Full Sample			Common Support			Matched
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Private Water (=1)	- 0.33 **	- 0.32 *	- 0.29 *	- 0.54 ***	- 0.54 ***	- 0.53 ***	- 0.60 ***
% $\Delta$ in Mortality	- 5.3 %	- 5.1 %	- 4.5 %	- 8.6 %	- 8.6 %	- 8.4 %	- 10.0 %
Real GDP/Capita		0.01	0.01		0.01	0.01	
Unemployment Rate		- 0.56	-0.64		-0.78	-0.84	
Inequality (Gini)		5.17 *	5.09 *		3.05	3.05	
Public Spending/Cap		- 0.03	- 0.04		-0.07 *	- 0.07 *	
Radical Party (=1)			0.48 *			0.17	
Peronist Party (=1)			- 0.20			- 0.17	
F-Stat Municipal FE	13.84***	11.92***	11.51***	10.39***	8.65***	8.32***	
F-Stat for year FE	55.03***	19.88***	18.25***	52.25***	15.59***	12.98***	



# Sensitivity analysis

1

Check that the trends in infant mortality were identical in the two types of municipalities **before** privatization

- You can do this by running the same equation, using only the years before the intervention – the treatment effect should be zero for those years
- Found that we cannot reject the null hypothesis of equal trends between treatment and controls, in the years before privatization

2

Check that privatization only affects mortality through reasons that are logically related to water and sanitation issues.

For example, there is no effect of privatization on death rate from cardiovascular disease or accidents.

# Impact of privatization on death from various causes

## DD on common support

	1990 Mean Mortality Rate	Estimated Impact Coefficients	%Δ in Mortality Rate
Infectious and parasitic diseases	.565	-.103 (.048)** [.055]* {.068}	-18.2
Perinatal deaths	2.316	-.266 (.105)** [.107]** {.123)**	-11.5
All other causes in aggregate	2.565	-.082 (.114) [.101] {.109}	-3.2
All other causes disaggregated:			
Accidents	.399	-.004 (.057)	...
Congenital anomalies	.711	-.022 (.056)	...
Skin and soft-tissue diseases	.000	.000 (.001)	...
Blood and hematologic diseases	.024	-.002 (.008)	...
Nervous system disorders	.163	.025 (.026)	...
Cardiovascular diseases	.236	.006 (.030)	...

# Privatization has a larger effect in poor and very poor municipalities than in non-poor municipalities

<b>Municipalities</b>	<b>Average mortality per 100, 1990</b>	<b>Estimated impact</b>	<b>% change in mortality</b>
Non-poor	5.15	0.105	...
Poor	7.15	-0.767***	-10.7%
Very poor	9.46	-2.214***	-23.4%

# Conclusion

Using a combination of methods, we found that:

- Privatization of water services is associated with a reduction in infant mortality of 5-7%.
- The reduction of mortality is:
  - Due to fewer deaths from infectious and parasitic diseases.
  - Not due to changes in death rates from reasons not related to water and sanitation
- The largest decrease in infant mortality occurred in low income municipalities.



**THANK YOU**

