ECON 21110
Applied Microeconometrics
Winter 2022
Lecture 6
Difference-in-Differences (DiD)

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This Lecture

- The Evaluation Problem

$$
\begin{aligned}
& \mathbb{E}\left[Y_i \mid D_i = 1\right] - \mathbb{E}\left[Y_i \mid D_i = 0\right] \\
= \quad & \underbrace{\mathbb{E}\left[Y_{1i} - Y_{0i} \mid D_i = 1\right]}_{TT} + \underbrace{\mathbb{E}\left[Y_{0i} \mid D_i = 1\right] - \mathbb{E}\left[Y_{0i} \mid D_i = 0\right]}_{Selection\ bias}
\end{aligned}
$$

- We want to estimate the causal effect: the average treatment effect on the treated (TT) – solve selection (bias) problem
- If we do not have:
  - random assignment into treatment (i.e. a RCT) to balance *unobservables* (Lecture 3)
  - a valid IV (Lecture 4) nor credible RD design (Lecture 5)
- cannot use MLR to control for *all* relevant *observables* (Lecture 2)
- We can use an evaluation method which relies on selection on *unobservables* in the sense that sometimes it is more reasonable to assume that *changes* in variables (rather than *levels*) move in parallel:
  - **Wooldridge (2016) Chapter 13.2**
  - Angrist & Pischke (2015) "MM" Chapter 5
  - Angrist & Pischke (2009) "MHE" Chapter 5.2

Evaluation using "Difference-in-Differences" (DiD)

- Data from **pooled cross sections** provides us with an alternative way of estimating causal effects, which is often useful for policy evaluation
- Suppose some policy is enacted for a certain group of individuals, geographical units, or firms. For example:
  - Job training programs for which only a subset of job seekers qualify
  - Changes in electoral rules that only pertain to small municipalities
  - New regulation that only pertain to firms above a certain size
- The group exposed to the policy is called **treatment group** and the group not exposed **control group**
- Using "difference-in-differences" (DiD) implies that we compare the difference in outcomes in the treatment and control groups **before** and **after** treatment
- The data requirement is only that we have cross-section data for the treatment and control groups before and after the policy

Evaluation using only "Before-After" (BA)

- Very often a policy change affects part of the population and we want to estimate the causal effect of the policy change
- Suppose we compare the outcome for the *treated* group ($D_i = 1$) before and after treatment
- First difference: we only explore the time-variation before and after the policy change. That is, the Before-After (BA) estimator:

$$Y_{it} = \beta + \delta Post_t + \eta_{it}$$

- Causal effect only if $\mathbb{E}[\eta_t \mid Post_t] = 0$; i.e. if $\mathbb{E}[Y_{0i,pre}] = \mathbb{E}[Y_{0i,post}]$
  - There are no other average differences in *unobservables* before and after the policy change
  - Very strong assumption! What about time-effects or age-effects?

Evaluation using Difference-in-Differences (DiD)

- A solution is to use a control group to "difference out" other factors and isolate the policy effect
- The Difference-in-Differences (DiD) estimator compares the before-after change in outcomes of the treated units ($D_i = 1$) to the before-after change in the outcomes of the untreated units ($D_i = 0$)
- If $\mathbb{E}[Y_{0i,post} - Y_{0i,pre} \mid D_i = 1] = \mathbb{E}[Y_{0i,post} - Y_{0i,pre} \mid D_i = 0]$ then the DiD estimator recovers TT:

$$
\begin{aligned}
&\mathbb{E}[Y_{i,post} - Y_{i,pre} \mid D_i = 1] - \mathbb{E}[Y_{i,post} - Y_{i,pre} \mid D_i = 0] \\
&= \mathbb{E}[Y_{1i,post} - Y_{0i,pre} \mid D_i = 1] - \mathbb{E}[Y_{0i,post} - Y_{0i,pre} \mid D_i = 0] \\
&= \mathbb{E}[Y_{1i,post} - Y_{0i,post} \mid D_i = 1] + \\
&\quad \mathbb{E}[Y_{0i,post} - Y_{0i,pre} \mid D_i = 1] - \mathbb{E}[Y_{0i,post} - Y_{0i,pre} \mid D_i = 0] \\
&= \mathbb{E}[Y_{1i,post} - Y_{0i,post} \mid D_i = 1]
\end{aligned}
$$

- Able to estimate the causal effect of D on Y

Identifying Assumption: Difference-in-Differences (DiD)

- Identifying assumption:

$$\mathbb{E}\left[Y_{0i,post} - Y_{0i,pre} \mid D_i = 1\right] = \mathbb{E}\left[Y_{0i,post} - Y_{0i,pre} \mid D_i = 0\right]$$

- The estimate is an unbiased estimator of the causal effect if the average change in the outcome variable would have been the same for the two groups *without* the treatment
  - This is known as the **parallel trend** assumption

The DiD Estimator

- We can write the DiD-estimator as:

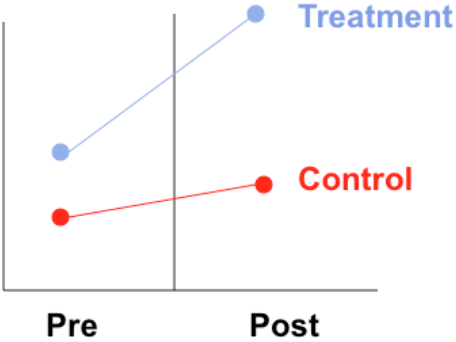
$$Y_{it} = \beta_0 + \delta_0 POST_t + \beta_1 D_{it} + \delta_1 POST_t * D_{it} + U \qquad (13.10)$$

- i is the cross-section unit and t is the time period
- $POST_{it}$ is a binary variable that takes the value 1 in the post-period and 0 in the pre-period
- D_{it} is an indicator variable and takes the value 1 if the treatment group and 0 if the control group
- The table below summarizes the predicted values:

	Before	After	After - Before
Control	β_0	$\beta_0 + \delta_0$	δ_0
Treatment	$\beta_0 + \beta_1$	$\beta_0 + \delta_0 + \beta_1 + \delta_1$	$\delta_0 + \delta_1$
Treatment - Control	β_1	$\beta_1 + \delta_1$	δ_1

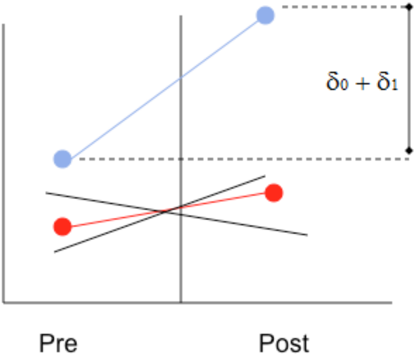
- We could add control variables to (13.10). In this case, equation (13.11) gives the difference in means controlling for these other variables

Difference-in-Differences (DiD)
Graphical illustration



First-Difference: Before-After (BA) estimator
Graphical illustration

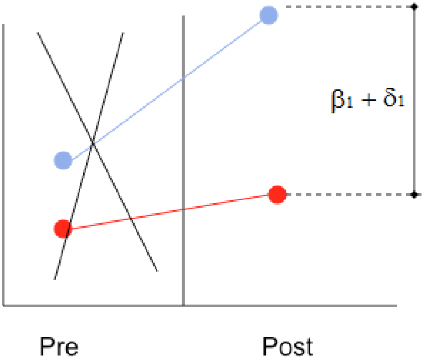
Identifies TT if no time (or age) effects: $\mathbb{E}[Y_{0i,pre}] = \mathbb{E}[Y_{0i,post}]$
i.e. if $\delta_0 = 0$



Effect of program using only pre- & post- data from T group (ignoring general time trend)

Cross-Section difference using only POST data
Graphical illustration

Identifies TT if no selection bias: $\mathbb{E}[Y_{0i} \mid D_i = 1] = \mathbb{E}[Y_{0i} \mid D_i = 0]$
i.e. if $\beta_1 = 0$



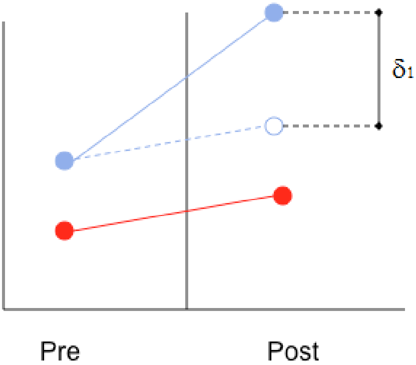
Effect of program using only T & C comparison from post-intervention (ignoring pre-existing differences between T & C groups)

Difference-in-differences (DiD)

Graphical illustration: Identifying Assumption

- Whatever happened to the control group is what would have happened to the treatment group in absence of the policy

$$\mathbb{E}\left[Y_{0i,post} - Y_{0i,pre} \mid D_i = 1\right] = \mathbb{E}\left[Y_{0i,post} - Y_{0i,pre} \mid D_i = 0\right]$$



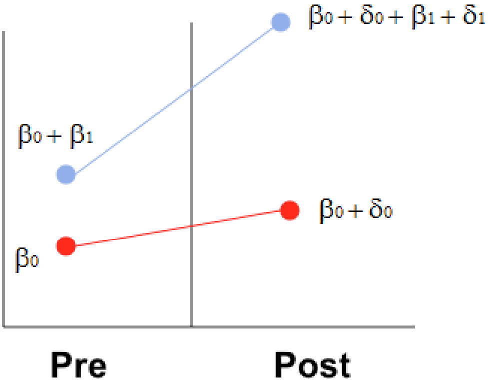
Effect of program difference-in-difference (taking into account pre-existing differences between T & C and general time trend)

Difference-in-Differences (DiD)
Graphical illustration and Regression

- The DiD estimator δ_1 from:

$$Y_{it} = \beta_0 + \delta_0 POST_t + \beta_1 D_{it} + \delta_1 POST_t * D_{it} + U$$

identifies the average causal effect TT=$\mathbb{E}\left[Y_{1i,post} - Y_{0i,post} \mid D_i = 1\right]$



Difference-in-Differences (DiD)
Graphical illustration and Regression

Difference-in-Differences (DiD)

- The important identifying assumption is that $\mathbb{E}\left[U_{it} \mid D_{it}, POST_{it}\right] = 0$
  - No other interaction between time and treatment group except for the treatment we study
- Thus, changes in unobservables over time affect both groups in the same way
- Fixed group effects capture unmeasure differences between treated and nontreated

Assess the credibility of the DiD estimator

- How do we check that we have **parallel trends**?
  - Test other periods $\rightarrow$ there should be no effects
  - Test other groups (alternative control or treatment group) $\rightarrow$ there should be no effects
  - Test other outcomes $\rightarrow$ there should be no effects
- Use more periods
  - In theory only 3 but in practice more
  - establish a pre-trend and extrapolate into post- treatment period
- The composition of individuals/units within the group changes over time
  - Control for observable characteristics
  - Remember to cluster the standard errors on group level

Assess the credibility of the DiD estimator

Targeting based on differences

- A pre-condition of the validity of the DiD assumption is that the program is not implemented based on the pre-existing differences in outcomes
- Example:
  - "Ashenfelter dip": It was common to compare wage gains among participants and non-participants in training programs to evaluate the effect of training on earnings. Ashenfelter & Card (1985) note that training participants often experience a dip in earnings just before they enter the program (which is presumably why they did enter the program in the first place). Since wages have a natural tendency to mean reversion, this leads to an upward bias of the DiD estimate of the program effect
  - In the case of DiD that combine regional and eligibility variation: Often the regional targeting is based upon the situation of the group of eligible people; e.g. locate a bank in the villages where the poor are worse of. Easy to check that this will lead to negative DiD in the absence of the program, if villages differ in terms of distribution of wealth

Assess the credibility of the DiD estimator

- **Functional form dependence:**
  - When average levels of the outcome Y are very different for controls and treatments before the policy change, the magnitude or even sign of the DiD effect is very sensitive to the functional form used
- **Long-term response versus reliability trade-off:**
  - DiD estimates are more reliable when you compare outcomes just before and just after the policy change because the identifying assumption (parallel trends) is more likely to hold over a short time-window. With a long time window, many other things are likely to happen and confound the policy change effect
  - However, for policy purposes, it is often more interesting to know the medium or long term effect of a policy change

Assess the credibility of the DiD estimator

- **Inference**
  - The observations in the control and the treatment group may tend to move together over time. In other words, there may be a common random effect at the time*group level. In this case, the standard error of the DiD estimator should take into account this correlation: we have in effect less information than we think
  - Stata offers a correction of the standard error with the command `cluster`

Example: Friedberg (1998) and Wolfers (2006)
Did Unilateral Divorce Laws Raise Divorce Rates?

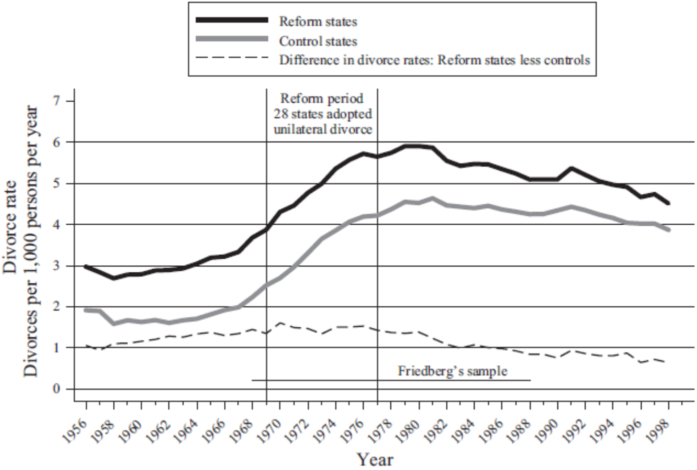
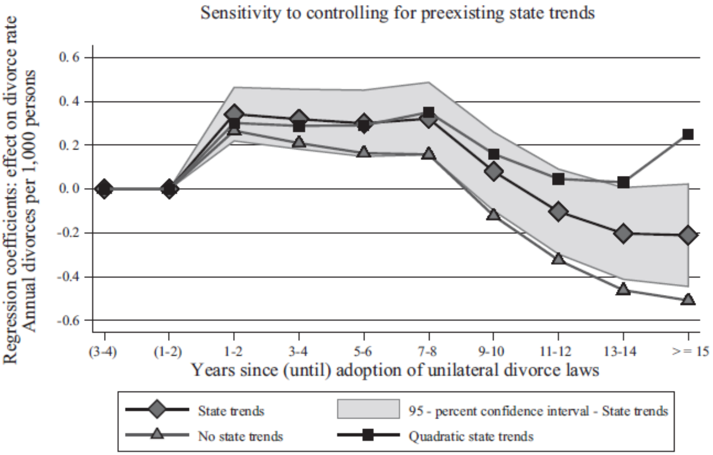


FIGURE 1. AVERAGE DIVORCE RATE: REFORM STATES AND CONTROLS

Example: Friedberg (1998) and Wolfers (2006)
Did Unilateral Divorce Laws Raise Divorce Rates?



All regressions control for state and year fixed effects. For details, see table 2.

FIGURE 3. RESPONSE OF DIVORCE RATE TO UNILATERAL DIVORCE LAWS

Example: Friedberg (1998) and Wolfers (2006)
Did Unilateral Divorce Laws Raise Divorce Rates?

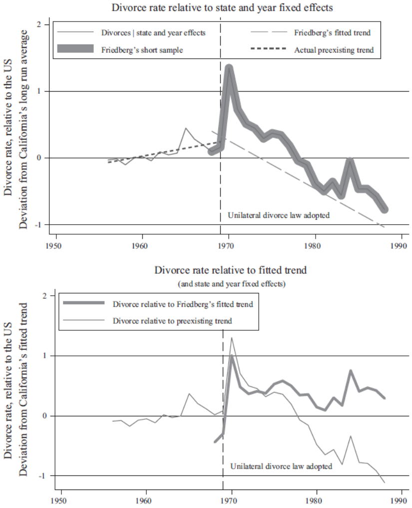


FIGURE 5. CALIFORNIA'S DIVORCE RATE

Fixed Effects (FE) and Difference-in-Differences (DiD)

- The fixed effects (FE) estimator assumes that treatment is as good as randomly assigned conditional on the observed covariates, time, and unobserved fixed effects:

$$\mathbb{E}\left[Y_{0it} \mid D_{it}, X_{it}, A_i, t\right] = \mathbb{E}\left[Y_{0it} \mid X_{it}, A_i, t\right] = \alpha + \beta X_{it} + \lambda_t + \gamma A_i$$

- The difference-in-differences (DiD) estimator makes a very similar assumption, but conditions on a *group* level s instead of an *individual* level i effect

$$\mathbb{E}\left[Y_{0ist} \mid D_{it}, X_{it}, A_s, t\right] = \alpha + \beta X_{it} + \lambda_t + \gamma A_s$$

  where s could be, for example, a state
- Sufficient for any treatment that happens at the state-time level
- While the basic estimation strategy is the same, the data requirements are much less. We do not need repeated observations on unit i; i.e. a panel. Repeated cross-sections sampling from the same aggregate units s are sufficient

Difference-in-Differences (DiD)

Suppose we have two states, two time periods, and treatment occurs only in state 1 in in $t = 1$

$$
\begin{aligned}
Y_{ist} &= \alpha + \beta A_s D_{st} + \gamma A_s + \mu \lambda_t + \varepsilon_{ist} \\
a &\equiv \mathbb{E}\left[Y_{ist} \mid D_{ist} = 1, \lambda_t = 1, A_s = 1\right] = \alpha + \beta + \gamma + \mu \\
b &\equiv \mathbb{E}\left[Y_{ist} \mid D_{ist} = 0, \lambda_t = 0, A_s = 1\right] = \alpha + \gamma \\
c &\equiv \mathbb{E}\left[Y_{ist} \mid D_{ist} = 0, \lambda_t = 1, A_s = 0\right] = \alpha + \mu \\
d &\equiv \mathbb{E}\left[Y_{ist} \mid D_{ist} = 0, \lambda_t = 0, A_s = 0\right] = \alpha
\end{aligned}
$$

The DiD estimator is given by:

$$
\begin{aligned}
&\beta_{DiD} \\
&= (a - b) - (c - d) \\
&= \left(\mathbb{E}\left[Y_{ist} \mid D_{ist} = 1, \lambda_t = 1, A_s = 1\right] - \mathbb{E}\left[Y_{ist} \mid D_{ist} = 0, \lambda_t = 0, A_s = 1\right]\right) \\
&\quad - \left(\mathbb{E}\left[Y_{ist} \mid D_{ist} = 0, \lambda_t = 1, A_s = 0\right] - \mathbb{E}\left[Y_{ist} \mid D_{ist} = 0, \lambda_t = 0, A_s = 0\right]\right) \\
&= ((\alpha + \beta + \gamma + \mu) - (\alpha + \gamma)) - ((\alpha + \mu) - \alpha) = \beta
\end{aligned}
$$

Example: Minimum Wages (Card, 1992)

- Card (1992) exploits regional variation in the federal minimum wage by estimating:

$$Y_{ist} = \gamma_s + \lambda_t + \beta(A_s * D_t) + \varepsilon_{ist}$$

  where the variable A_s is a measure of the fraction of teenagers likely to be affected by a minimum wage increase in each state and D_t is a dummy for observations after 1990, when the federal minimum increased from \$3.35 to \$3.80
- A_s measures the baseline (pre-increase) proportion of each state's teen labor force earning less than \$3.80 fraction affected variable, assumed a good predictor of the wage changes induced by an increase in the federal minimum data from two periods – before and after – in this case 1989 and 1992, 51 states, and thus 102 state-years
- For two periods, one can estimate $dY_{is} = \alpha + \beta(dA_s) + d\varepsilon_{is}$

Example: Minimum Wages (Card, 1992)

Table 5.2.2: Regression-DD estimates of minimum wage effects on teens, 1989 to 1992

Explanatory Variable	Equations for Change in Mean Log Wage: (1)	(2)	Equations for change in Teen Employment-Population Ratio: (3)	(4)
1. Fraction of Affected Teens	0.15 (0.03)	.14 (0.04)	0.02 (0.03)	-.01 (0.03)
2. Change in Overall Emp./Pop. Ratio	–	0.46 (0.60)	–	1.24 (0.60)
3. R-squared	0.30	0.31	0.01	0.09

Notes: Adapted from Card (1992). The table reports estimates from a regression of the change in average teen employment by state on the fraction of teens affected by a change in the federal minimum wage in each state. Data are from the 1989 and 1992 CPS. Regressions are weighted by the CPS sample size by state and year.

Granger Test

- When $T > 2$, DiD lends itself to a tests for causality in the spirit of Granger (1969)
- The Granger idea is to see whether causes happen before consequences and not vice versa
- Suppose the policy variable of interest changes at different times in different states
- In this context, Granger causality testing means a check of whether, conditional on state and year effects, past treatments predict future outcomes while future treatment does not predict past outcomes

Example: Employment Protection (Autor, 2003)

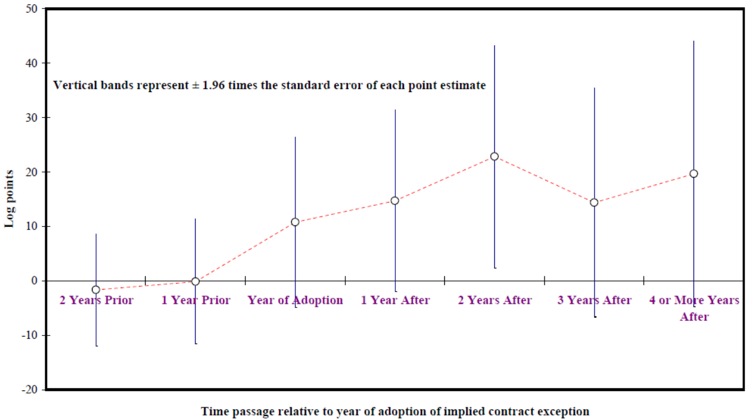
- If the treatment causes the outcome but not vice versa, then *leads* should not matter in the regression:

$$Y_{ist} = \gamma_s + \lambda_t + \sum_{\tau=0}^{m} \beta_{-\tau} D_{s,t-\tau} + \sum_{\tau=0}^{q} \beta_{\tau} D_{s,t+\tau} + \delta X_{ist} + \varepsilon_{ist}$$

  which allows for m lags and q leads ("anticipatory effects") of the treatment

- Autor (2003) implements the Granger test in an investigation of the effect of employment protection on firms use of temporary help
- Employment protection is a type of labor law - promulgated by state legislatures or, more typically, through common law as made by state courts - that makes it harder to fire workers

Example: Employment Protection (Autor, 2003)



Synthetic Control Methods

- In some cases, treatment and potential control groups do not follow parallel trends
  $\rightarrow$ Standard DiD estimator would lead to biased estimates
- Abadie & Gardeazabal (2003) pioneered a synthetic control method when estimating the effects of the terrorist conflict in the Basque Country using other Spanish regions as a comparison group
  - Card (1990) implicitly used a very similar approach in his Mariel boatlift paper investigating the effect of immigration on employment of natives
- The basic idea behind synthetic controls is that a combination of units often provides a better comparison for the unit exposed to the intervention than any single unit alone

SCM: Terrorism and Growth (Abadie & Gardeazabal, 2003)

- They want to evaluate whether Terrorism in the Basque Country had a negative effect on growth
- They cannot use a standard DiD method because none of the other Spanish regions followed the same time trend as the Basque Country
- They therefore take a weighted average of other Spanish regions as a synthetic control group
- The optimal weights they get are: Catalonia: 0.8508, Madrid: 0.1492, and all other regions: 0

SCM: Terrorism and Growth (Abadie & Gardeazabal, 2003)

TABLE 3—PRE-TERRORISM CHARACTERISTICS, 1960'S

	Basque Country (1)	Spain (2)	"Synthetic" Basque Country (3)
Real per capita GDP[a]	5,285.46	3,633.25	5,270.80
Investment ratio (percentage)[b]	24.65	21.79	21.58
Population density[c]	246.89	66.34	196.28
Sectoral shares (percentage)[d]			
Agriculture, forestry, and fishing	6.84	16.34	6.18
Energy and water	4.11	4.32	2.76
Industry	45.08	26.60	37.64
Construction and engineering	6.15	7.25	6.96
Marketable services	33.75	38.53	41.10
Nonmarketable services	4.07	6.97	5.37
Human capital (percentage)[e]			
Illiterates	3.32	11.66	7.65
Primary or without studies	85.97	80.15	82.33
High school	7.46	5.49	6.92
More than high school	3.26	2.70	3.10

Sources: Authors' computations from Matilde Mas et al. (1998) and Fundación BBV (1999).

[a] 1986 USD, average for 1960–1969.

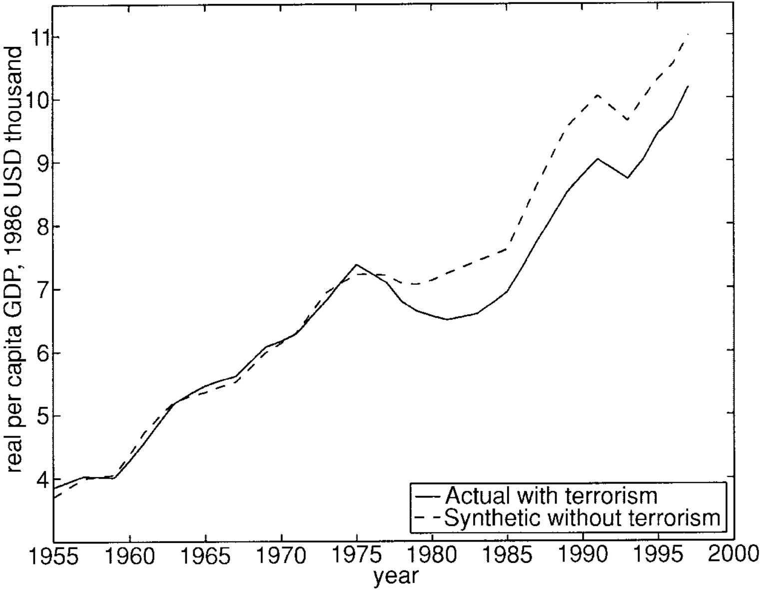
[b] Gross Total Investment/GDP, average for 1964–1969.

[c] Persons per square kilometer, 1969.

[d] Percentages over total production, 1961–1969.

[e] Percentages over working-age population, 1964–1969.

SCM: Terrorism and Growth (Abadie & Gardeazabal, 2003)



SCM: Terrorism and Growth (Abadie & Gardeazabal, 2003)