The Economics of European Regions: Theory, Empirics, and Policy

Dipartimento di Economia e Management

Co-funded by the Erasmus+ Programme of the European Union

Project funded by

European Commission Erasmus + Programme –Jean Monnet Action

Project number 553280-EPP-1-2015-1-IT-EPPJMO-MODULE

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November 19, 2019

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Natural and quasi-experiment in economics

- Natural experiments studies examine outcome measures for observations in treatment groups and comparison groups that are **not randomly assigned**.
- "**Good** natural experiments are studies in which there is a **transparent exogenous** source of variation in the explanatory variables that determine the treatment assignment" (Meyer, 1995).
- A natural experiment is an empirical study in which units exposed to the experimental and control conditions are determined by nature or by other factors **outside the control of the investigators**, but the process governing the exposures arguably resembles random assignment.
- Thus, natural experiments are observational studies and are **not controlled** in the traditional sense of a randomized experiment.

Natural and quasi-experiment in economics (cont.)

- A natural experiment induced by policy changes, government randomization, or other events may allow a researcher to **clearly defined** exposure involving a well defined subpopulation (and the absence of exposure in a similar subpopulation) such that variation in outcomes may be plausibly attributed to the exposure.

- This occurrence is especially useful in situations in which estimates are **ordinarily biased** because of endogenous variation due to **omitted variables** or **selection**.

  $\Rightarrow$ a natural experiment may allow the study of the effects of exogenous variation in an explanatory variable that is in other situations endogenously related to the outcome.

Natural and quasi-experiment in economics (cont.)

Example: the effect of social insurance programs on labour supply.

- It is *difficult to distinguish* the effects of an individual's benefit entitlement from the effects of past labour supply and earnings that typically determine *that* benefit entitlement.
- Previous earnings are highly correlated with future earnings and the payoff to work.
- Thus, studying the effects of these social insurance programs on employment and earnings, it may be *difficult to separate* the independent influence of earnings history from benefit generosity.
- This problem is exacerbated by the use of proxies for the relevant earnings and benefit variables so that idiosyncratic and potentially exogenous variation in the benefit variables is often lost.

$\Rightarrow$ Many studies have examined changes in social insurance benefits that applied to **certain** groups **but not** others.

Threats to validity - Meyer (1995) & Campbell (1957)

These threats to validity are problems that may undermine the causal interpretation of empirical studies.

Threats to internal validity

Whether is possible to validly draw the conclusion that *within the context* of the study the differences in the outcome were caused by the differences in the relevant explanatory variables.

Threats to external validity

Whether the effects found in an experiment can be *generalized* to different individuals, context, and outcomes.

Threats to internal validity

1. **Omitted variables**: events, other than the experimental treatment, occurring between pre-intervention ad post-intervention observations that provide alternative explanations for the results (*confounding*).
2. **Trends in outcomes**: processes within the units of observations producing changes as a function of passage of time per se (e.g., inflation, ageing, wage growth).
3. **Misspecified variances**: the overstatement of the significance of statistical tests due to effects such as the omission of group error terms that indicate that outcomes for individual units are correlated.
4. **Mismeasurement**: changes in definitions or survey methods that may produce changes in the measured variables.
5. **Political economy**: endogeneity of policy changes due to governmental responses to variables associated with past or expected future outcomes.

Threats to internal validity (cont.)

6. **Simultaneity**: endogeneity of explanatory variables due to their joint determination with outcomes.
7. **Selection**: assignment of observations to treatment groups in a manner that leads to correlation between assignment and outcomes in the absence of treatment.
   Selection based on time-invariant individual characteristics $\Rightarrow$ panel FE or RE.
8. **Attrition**: differential loss of respondent from treatment and control groups.
9. **Omitted interactions**: differential trends in treatment and control groups or omitted variables that change in different ways for treatment and control groups.

Threats to external validity

1. **Interaction of selection and treatment**: the treatment group may not be representative of certain population, or the treatment may be different from that which one would like to examine.
2. **Interaction of setting and treatment**: the effect of the treatment may differ across geographic or institutional settings.
3. **Interaction of history and treatment**: the effect of the treatment may differ across time periods.

The research design

The main goals of the research design used in natural experiment are:

- Finding variation in the key explanatory variables that is exogenous: researcher should seek to find variation that is driven by factors that are *clearly identified and understood*.
- Finding comparison groups that are comparable: the possibility of omitted variables, trends in outcomes, omitted interactions, etc., places a burden on researcher to examine comparability of groups.
- Probing the implications of the hypotheses under test.

Study designs commonly used in natural experiment:
• the one group before and after design;
• the before and after design with an untreated comparison group.

The one group before and after design

Consider the equation:

$$y_{it} = \alpha + \beta d_t + \epsilon_{it}$$

where:

- y_{it}: outcome of interest for unit i ($i = 1, ..., N$), in period t ($t = 0, 1$);
- d_t: dummy for being in the treatment group

$$\begin{cases} d_t = 1 & \text{if } t = 1 \\ d_t = 0 & \text{if } t = 0 \end{cases}$$

- β: causal effect of the treatment on the outcome for treated.

Example:

• treatment group defined by the variation of another variable: the level of minimum wage

• outcomes: employment.

The one group before and after design (cont.)

The key identifying assumption of this model is that, in the absence of treatment, β would be 0 $\Rightarrow$ no difference in the mean of those in group 0 and those in group 1.

This condition is typically written as $E\left[\epsilon_{it}|d_t\right] = 0$.

If this condition hold, un unbiased estimate of β is given by:

$$\hat{\beta}_d = \bar{y}_1 - \bar{y}_0 \tag{1}$$

where:

- $\bar{y}_1$: average of outcome of individuals in the post-treatment period;
- $\bar{y}_0$: average of outcome of individuals in the pre-treatment period.

The one group before and after design (cont.)

Equation (2) can be estimated using pooled data from the two time periods.
If individual data are used, the samples could be different in the two periods $\Rightarrow$ repeated cross-section.

This method can be used only under **very special** circumstances: strong evidence that the two groups would have been *comparable* over time *in the absence of the treatment*.

One way to asses the importance of threats to internal validity is to examine the outcomes for *similar groups that did not receive the treatment* but would presumably be *subject* to these influences as well.

The before and after design with an untreated comparison group

Often data are available for the time period before and after the treatment for a group that does not receive the treatment but experiences some or all of the other influences that affect the treatment group.

When such a group is present $\Rightarrow$ *difference in differences* design.

Difference in differences

Consider the equation:

$$y_{it}^{j} = \alpha + \alpha_1 d_t + \alpha^1 d^j + \beta d_t^j + \epsilon_{it}^{j}$$

where:

- y_{it}^{j}: outcome of interest for unit i ($i = 1, ..., N$), in group j ($j = 0, 1$) in period t ($t = 0, 1$);
- d^j: dummy for being in the treatment group:

$$\begin{cases} d^j = 1 & \text{if } j = 1 \\ d^j = 0 & \text{if } j = 0 \end{cases}$$

- d_t: dummy for being in the post-treatment period:

$$\begin{cases} d_t = 1 & \text{if } t = 1 \\ d_t = 0 & \text{if } t = 0 \end{cases}$$

- d_t^j: dummy for being in the treatment group in post-treatment period:

$$\begin{cases} d_t^j = 1 & \text{if } j = 1 \text{ and } t = 1 \\ d_t^j = 0 & \text{otherwise} \end{cases}$$

- β: causal effect of the treatment on the outcome for treated.

Difference in differences (cont.)

The key identifying assumption of this model is that, in the absence of treatment, β would be 0 $\Rightarrow$ no difference in the mean of those in group 0 and those in group 1.

This condition is typically written as $E\left[\epsilon_{it}^{j} | d_{t}^{j}\right] = 0$.

If this condition hold, un unbiased estimate of β is given by:

$$\hat{\beta}_{dd} = (\bar{y}_1^1 - \bar{y}_0^1) - (\bar{y}_1^0 - \bar{y}_0^0) \tag{2}$$

where:

- $\bar{y}_1^1$: average of outcome of treated group in the post-treatment period;
- $\bar{y}_0^1$: average of outcome of treated group in the pre-treatment period;
- $\bar{y}_1^0$: average of outcome of control group in the post-treatment period;
- $\bar{y}_0^0$: average of outcome of control group in the pre-treatment period.

Difference in differences

$$y_{it}^{j} = \alpha + \alpha_1 d_t + \alpha^1 d^j + \beta d_t^j + \epsilon_{it}^{j}$$

- α_1: captures the way that **both groups** ($j = 0$ and $j = 1$) are influenced by **time**;
- α^1: captures **time-invariant difference** in overall means **between** the two groups.

Difference in differences: Card and Krueger (1994)

- Suppose we want to asses the effect of minimum wages on employment (a classic question in labour economics).
- In a competitive labour market, increases in the minimum wage would move us up a downward-sloping labour demand curve
  $\Rightarrow$ employment would fall (perhaps hurting the very workers minimum wage)
- Card an Krueger (1994) analyse the effect of a minimum wage increase in New Jersey using a difference-in-differences methodology.

Card and Krueger (1994): research design

- On April 1992, New Jersey (NJ) raised the state minimum wage from \$4.25 to \$5.05.
- Card and Krueger collected data on employment of about 400 fast food before (February) and after (November) the minimum wage increase in NJ both in NJ and Pennsylvania (PA).
- PA's minimum wage stayed at \$4.25.

Difference in differences strategy (Angrist and Pischke, 2009)

Define the potential outcomes:

- $Y(1)_{ist}$: employment at restaurant i , state s, time t with a high w^{min}.
- $Y(0)_{ist}$: employment at restaurant i , state s, time t with a low w^{min}.

In practice we only see one or the other. The observed outcome Y_{ist} (omitting the subscript *obs*) is defined as:

$$Y_{ist} = Y(0)_{ist} + (Y(1)_{ist} - Y(0)_{ist})\, D_{st}$$

where D_{st} is a dummy for high-minimum wage states and periods (treatment indicator).

Difference in differences strategy (Angrist and Pischke, 2009)

Assume that:

- the treatment effect, τ is constant: $\tau = Y(1)_{ist} - Y(0)_{ist}|s, t$;
- an additive structure for the potential outcome in the **no-treatment** state:

$$E\left[Y(0)_{ist}|s,t\right] = \gamma_s + \lambda_t$$

$\Rightarrow$ in the absence of a minimum wage change, employment is determined by the sum of a time-invariant state effect γ_s and a year effect that is common across states λ_t.

Then, the **observed** employment Y_{ist} can be written as:

$$Y_{ist} = \gamma_s + \lambda_t + \tau D_{st} + \epsilon_{ist} \quad (3)$$

Difference in differences strategy (Angrist and Pischke, 2009)

- In New Jersey:
  - Employment in February is:
    $E\left[Y_{ist}|s=NJ,t=Feb\right]=\gamma_{NJ}+\lambda_{Feb}$
  - Employment in November is:
    $E\left[Y_{ist}|s=NJ,t=Nov\right]=\gamma_{NJ}+\lambda_{Nov}+\tau$
  - the difference between November and February is:
    $E\left[Y_{ist}|s=NJ,t=Nov\right]-E\left[Y_{ist}|s=NJ,t=Feb\right]=\lambda_{Nov}+\tau-\lambda_{Feb}$
- In Pennsylvania:
  - Employment in February is:
    $E\left[Y_{ist}|s=PA,t=Feb\right]=\gamma_{PA}+\lambda_{Feb}$
  - Employment in November is:
    $E\left[Y_{ist}|s=PA,t=Nov\right]=\gamma_{PA}+\lambda_{Nov}$
  - the difference between November and February is:
    $E\left[Y_{ist}|s=PA,t=Nov\right]-E\left[Y_{ist}|s=PA,t=Feb\right]=\lambda_{Nov}-\lambda_{Feb}$

Difference in differences strategy (Angrist and Pischke, 2009)

- The difference in differences strategy amounts to comparing the change in employment in NJ to the change in employment in PA.
- The population difference in differences:

$$E\left[Y_{ist}|s = NJ, t = Nov\right] - E\left[Y_{ist}|s = NJ, t = Feb\right] +$$
$$-E\left[Y_{ist}|s = PA, t = Nov\right] - E\left[Y_{ist}|s = PA, t = Feb\right] = \tau$$

is the **causal effect** of interest.

- This is estimated using the sample analogue of the population means.

Difference in differences: Card and Krueger (1994)

Variable	PA (i)	NJ (ii)	Difference, NJ − PA (iii)
	Stores by state		
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (0.51)	−2.89 (1.44)
2. FTE employment after, all available observations	21.17 (0.94)	21.03 (0.52)	−0.14 (1.07)
3. Change in mean FTE employment	−2.16 (1.25)	0.59 (0.54)	2.76 (1.36)

Table 3. Card and Krueger 1994. Standard errors in parenthesis.

Surprisingly, employment rose in NJ relative to PA after the minimum wage change.

Difference in differences with regression

- As seen previously we can estimate the difference in differences estimator in a regression framework.
- Advantages:
  - We can control for other variables which may reduce the residual variance (leading to smaller standard errors).
  - It is easy to calculate standard errors.
  - It is easy to include multiple periods.
  - We can study treatments with different treatment intensity (e.g., varying increases in the minimum wage for different states).
- The typical regression model in the difference in differences design is:

$$y_{it} = \beta_1 + \beta_2 \text{Treat}_i + \beta_3 \text{Post}_t + \beta_4 \left(\text{Treat} * \text{Post}\right)_{it} + \epsilon_{it} \qquad (4)$$

where:

  - Treat: a dummy for being in the treatment group
  - Post: post-treatment dummy

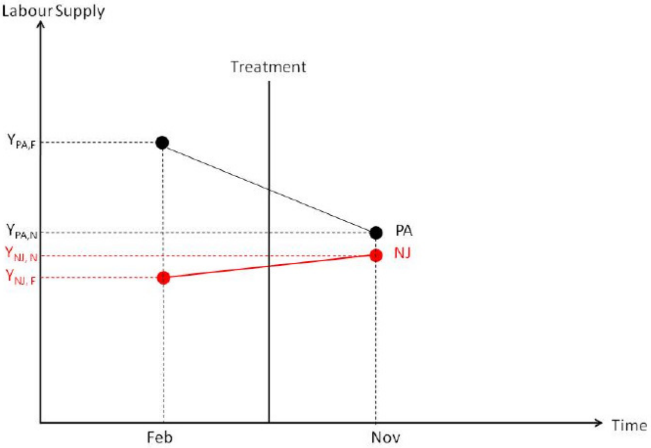
Difference in differences with regression - Card and Krueger

- In the Card and Krueger case the equivalent regression model would be:

$$Y_{ist} = \alpha + \gamma NJ_s + \lambda d_t + \tau \underbrace{(NJ_s * d_t)}_{D_{st}} + \epsilon_{ist}$$

  - NJ_s: a dummy which is equal to 1 if the observation is from NJ.
  - d_t is a dummy which is equal to 1 if the observation is **from** November (post).
- This equation takes the following values (saturated model):
  - $\alpha = E[Y_{ist}|s = PA, t = Feb] = \gamma_{PA} + \lambda_{Feb}$
  - $\gamma = E[Y_{ist}|s = NJ, t = Feb] - E[Y_{ist}|s = PA, t = Feb] = \gamma_{NJ} - \gamma_{PA}$
  - $\lambda = E[Y_{ist}|s = PA, t = Nov] - E[Y_{ist}|s = PA, t = Feb] = \lambda_{Nov} - \lambda_{Feb}$
  - $\tau = E[Y_{ist}|s = NJ, t = Nov] - E[Y_{ist}|s = NJ, t = Feb] +$
    $-E[Y_{ist}|s = PA, t = Nov] - E[Y_{ist}|s = PA, t = Feb]$

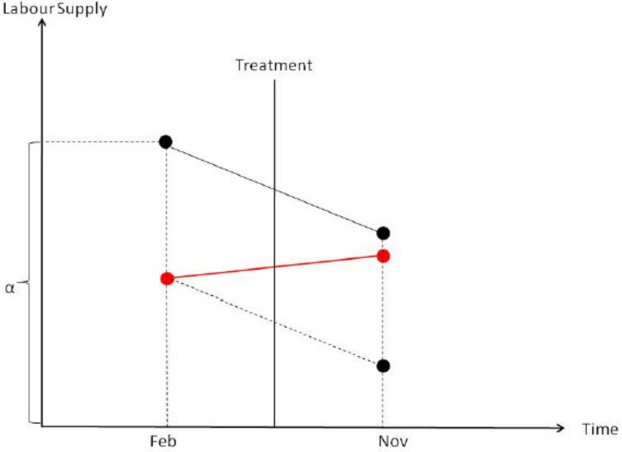
Graph - Observed Data



Source: Waldinger.

Graph - DD

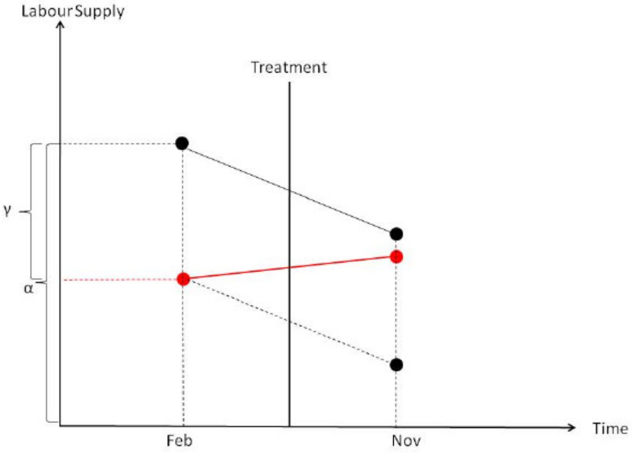
$$Y_{ist} = \alpha + \gamma NJ_s + \lambda d_t + \tau(NJ_s * d_t) + \epsilon_{ist}$$



Source: Waldinger.

Graph - DD

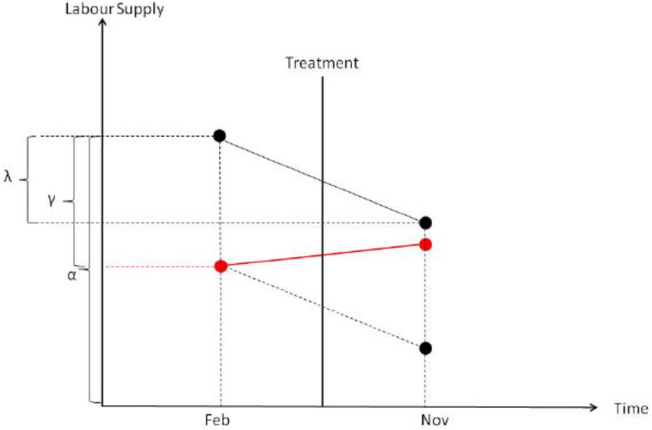
$$Y_{ist} = \alpha + \gamma NJ_s + \lambda d_t + \tau(NJ_s * d_t) + \epsilon_{ist}$$



Source: Waldinger.

Graph - DD

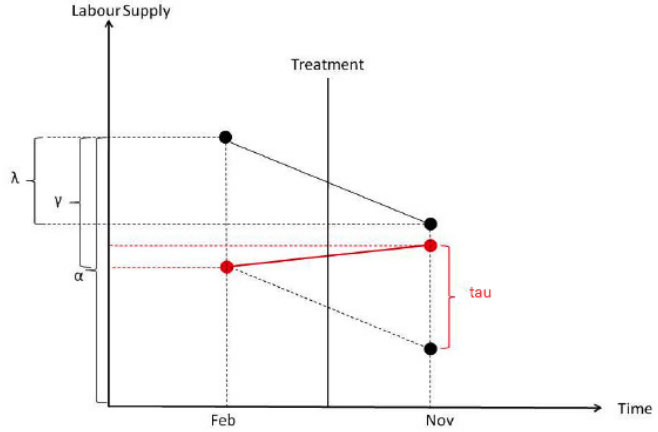
$$Y_{ist} = \alpha + \gamma NJ_s + \lambda d_t + \tau(NJ_s * d_t) + \epsilon_{ist}$$



Source: Waldinger.

Graph - DD

$$Y_{ist} = \alpha + \gamma NJ_s + \lambda d_t + \tau(NJ_s * d_t) + \epsilon_{ist}$$



Source: Waldinger.

Internal validity threats

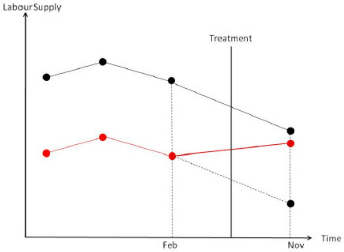
- Several of the internal validity threats are reduced by this approach.
- Influences as changes in other state laws and labour market conditions (omitted variables) and changes in survey methods are reduced by the use of the **untreated** comparison group.
- Also the importance of trends in employment (trends in outcomes) is reduced or eliminated.
- Attrition does not appear in cross-section studies, but ones needs to examines if the samples are selected over time from comparable population.

Internal validity threats (cont.)

- One of the main threat to validity in this design is the possibility of an interaction between treated and post-treatment (omitted interactions).
- Changes in other state laws or *macroeconomic conditions* may **influence the groups in different ways**.
- A *recession* may have a disproportionated effect on one income group or in one state compared to another.
- The difference in differences design is more plausible when the untreated group is **very similar** to the treatment group
  $\Rightarrow$ check differences in mean characteristics.
  $\Rightarrow$ including the characteristics to adjust for **observable** differences
- A favourable situation for difference in differences design is when the outcomes of control group is **close** to that for the treatment group during the **before** period
  $\Rightarrow$ common (parallel) trend.

Key assumption of any DD design: common trends

- The key assumption for any DD design is that the outcome in treatment and control group would follow the **same time trend** in the **absence** of the treatment.
- This does **not** mean that they have to have **the same mean** of the outcome!
- Common trend assumption is difficult to verify but one often uses pre-treatment data to show that the trends are the same.



Multiple pre- post-intervention periods

- Using data form several pre-intervention or post-intervention periods allows to examine various validity threats.
- For example, to understand if the results are drove by seasonality (omitted variables) in the outcome measures.
- Better to check for parallel trends.

Regression DD including leads and lags

- An easy way to analyse pre-trends is by including lags into the DD model.
- On the other hand, leads can be included to analyze whether the treatment effect **changes over time** after treatment.
- The estimated regression would be:

$$Y_{ist} = \gamma_s + \lambda_t + \sum_{r=-q}^{-1} \tau_r D_{sr} + \sum_{r=0}^{m} \tau_r D_{sr} + X_{ist} + \epsilon_{ist}$$

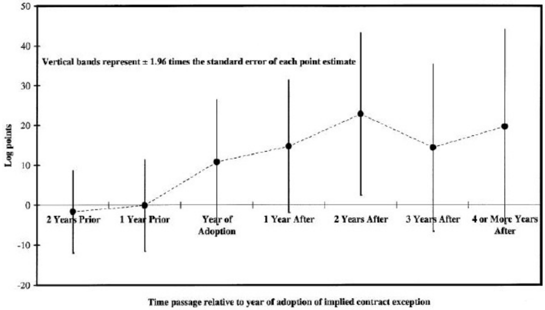
where:

- treatment occurs in year 0
- includes q lags or anticipatory effects
- includes m leads or post treatment effects.

Regression DD including leads and lags: Author (2003)

- Autor (2003): both leads and lags in a DD model to analyse the effect of increased EPL on the firm's use of temporary contracts.
- US employers can usually hire and fire workers whenever wished.
- Some states courts have made some exceptions and, consequently, raised EPL.
- Different states have passed these exceptions at different points in time.
- Usually a normalization for the adoption year to 0 is used.
- Autor (2003) analyses the effect of these exceptions on the use of temporary contracts.

Author (2003): results



Source: Author (2003).

- The lags are very close to 0, showing no evidence for anticipatory effects (evidence in favour of the common trends assumption).
- The leads show that the effect increases during the years of the treatment (although is relatively constant and not so significant!).

Other issues in DD design

- Standard Errors: the variable of interests often only vary at a group level (state) and is serially correlated
  $\Rightarrow$ standard errors are underestimated (Moulton, 1990; Bertrand, et al. 2004)
- Synthetic Control Methods: when treatment and potential control groups do not follow parallel trends.
- Placebo test: as robustness checks for confounding effects.

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