SAPIENZA
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Workshop AISRe – ABC

The frontier of the debate in Regional and Urban Economics

Evaluation of territorial policies

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Agenda

- A brief review of counterfactual evaluation methods (from Ichino)
- Policy Evaluation and Spillover
- The Stable Unit Treatment Value Assumption (Sutva)
- A general framework to relax the SUTVA in regional policy analyses
- *Example : Beyond the SUTVA: how policy evaluations change when we allow for interactions among firms (Cerqua and Pellegrini, 2014)*

Counterfactual evaluation is about causality

- Counterfactual impact evaluations borrow a technique (control and comparison groups) from the medical sciences. These are useful where a given policy instrument is applied to a large number of units: in regional policy this means firms and innovation support.
- The basic idea in counterfactual framework is to estimate the casual effect of a policy
- Some questions about causality:

- Does aspirin reduce the risk of heart attacks?
- Does an additional year of schooling increase future earnings?
- Does public subsidies increase investments and employments?

The answers to these questions involve the identification and measurement of causal links.

Counterfactual framework (Rubin's model)

We have a population of units; for each unit we observe a variable D (treatment) and a variable Y (outcome).
We observe that D and Y are correlated. Does correlation imply causation?
In general no, because of:

- confounding factors;
- reverse causality.

- D_i is the treatment status:
  $D_i = 1$ if unit i has been exposed to treatment;
  $D_i = 0$ if unit i has not been exposed to treatment.
- Y_i (Di) indicates the potential outcome according to treatment:
  $Y_i(1)$ is the outcome in case of treatment;
  $Y_i(0)$ is the outcome in case of no treatment;

Counterfactual framework (Rubin's model)

The observed outcome for each unit can be written as:

$$Y_i = D_i Y_i(1) + (1 - Di) Y_i(0)$$

Causal effect.

For a unit i, the treatment D_i has a causal effect on the outcome Yi_i if the event $D_i = 1$ instead of $D_i = 0$ implies that $Y_i = Y_i(1)$ instead of $Y_i = Y_i(0)$.

In this case the causal effect of D_i on Y_i is

$$\Delta_i = Y_i(1) - Y_i(0)$$

The identification of this effect is logically impossible: The Fundamental Problem of Causal Inference.

It is impossible to observe for the same unit i the values $D_i = 1$ and $D_i = 0$ as well as the values $Y_i(1)$ and $Y_i(0)$ and, therefore, it is impossible to observe the effect of D on Y for unit i (Holland, 1986).

Solution: randomization

Randomized experiments
Consider two random samples C and T from the population. Since by construction these samples are statistically identical to the entire population we can write:
and

$$E\{Y_i(0)|i \in C\} = E\{Y_i(0)|i \in T\} = E\{Y_i(0)\}$$
$$E\{Y_i(1)|i \in C\} = E\{Y_i(1)|i \in T\} = E\{Y_i(1)\}.$$

Substituting it is immediate to obtain:

$$E\{\Delta_i\} \equiv E\{Y_i(1)\} - E\{Y_i(0)\}$$
$$= E\{Y_i(1)|i \in T\} - E\{Y_i(0)|i \in C\}.$$

Randomization solves the Fundamental Problem of Causal Inference because it allows to use the control units C as an image of what would happen to the treated units T in the counterfactual situation of no treatment, and vice-versa.

Linear model and selection

$$Y_{it} = \beta X_{it} + U_{it} \qquad t<k$$
$$Y_{it} = \beta X_{it} + \alpha D_i + U_{it} \quad t>k$$

Y_{it} = outcome

X_{it} = observable covariates

β = parameters

D_i =dummy variable (1 if treated, 0 if not treated)

α = homogeneous policy effect

U= unobservables, with zero mean : E(U)=0

The estimation of α

(Average Treatment Effect, ATE)

$$E(\Delta)=E(\alpha) = E(Y_{it} \mid D=1) - E(Y_{it} \mid D=0)$$

$$= E(\beta X_{it} + \alpha D_i + U_{it} \mid D=1)$$

$$-E(\beta X_{it} + U_{it} \mid D=0)$$

$$= \alpha + E(U_{it} \mid D=1) - E(U_{it} \mid D=0)$$

And, without selection,

$$E(U)=E(U_{it} \mid D=1) = E(U_{it} \mid D=0)=0$$

If absence of selection bias: E(U,D)=0

la

The policy effect is:

t>k

$$\overline{Y}_t^{(1)} = \sum_i \frac{Y_{it} D_i}{D_i} \quad ; \overline{Y}_t^{(0)} = \sum_i \frac{Y_{it} (1 - D)_i}{(1 - D)_i}$$

$$\hat{a} = \overline{Y}_t^{(1)} - \overline{Y}_t^{(0)}$$

IF selection bias?

If unobservables is: $U_{it} = a_i + v_{it}$,

Where $E(a|D=1) \neq E(a|D=0)$

And $E(v|D=1)=E(v|D=0) = 0$ therefore

$[E(U_{it}|D=1) - E(U_{it}|D=0)]=$

$[E(v_{it}|D=1)+ E(a_i|D=1)] - [E(v_{it}|D=0)+ E(a_i|D=0)]=E(a|D=1)-E(a|D=0) \neq 0$

And the OLS estimation is biased

$Y_{it} = \beta X_{it} + \alpha_i D_i + U_{it}$

the bias in the estimation of α_i depends on the correlation between D and U

The quasi-experimental approaches

- If randomization is not possible, we use different methods in order to reproduce a plausible ex ante randomization using ex post data :
- Differences-in-differences, assuming that unobservables are constant in time, and therefore can be eliminated by a fixed effect estimator
- Matching, assuming that the differences between treated and not treated derive only from observables. Conditioning on observables, the two sample (the treatment) are random
- Regression discontinuity design: around the discontinuity we have a local randomization
- Instrumental variables: there is a variable that is a factor of randomization and does not affect outcome

The evaluation of regional policies

- Basically, regional policy in European countries. has been aimed at influencing the regional allocation of investments and employment, in order to increase competitiveness, self-sustaining growth, and new employment in low income regions
- Regional industrial policies promote local aggregations of firms, that produce positive local agglomeration externalities, or spatial externalities or spillovers. This is an important channel for generating income and growth
- Several regional policies generate spillovers and spatial externalities, especially investment, innovation and R&D regional policies
- However, spillover effects and spatial externalities are neglected in the evaluation of regional policies

Why the evaluation without exernalities?

- Problem: the standard evaluation methods are based on the Rubin Model, that relies on the Stable Unit Treatment Value Assumption (SUTVA),
- i.e. the model assumes away any possible interactions among firms due to the policy (see Rubin, 1986).
- The reason is that is not possible assumes that the treated firms behaviour as the not treated ones in absence of treatment, if the not treated are in some way affected by the treatment
- There are several situations in which SUTVA is clearly not plausible; this is true especially for the regional policies, that have a clear spatial effect.
- however, severe empirical difficulties in disentangling the spillover effects from more relevant confounding factors have hindered the relaxation of the SUTVA.

Literature on regional policies' spillovers

The lack of an extensive literature on the spatial effect of incentives is rather curious, because several industrial policies, especially the policies oriented to the growth of underdeveloped regions, are designed for generating spatial externalities.

. A summary of the rationale for the supply of localization incentives is presented in Glaeser (2001):

- Externalities linked to the demand and supply of goods and services;
- Agglomeration externalities, due to income production generating ex post high regional public income by local taxes;
- Ex post appropriation. The firm's fixed resources create an immobility which means that it is easy prey for a taxing authority.
- Tax discrimination for attracting firms in presence of firms with different levels of demand for different locations. Local governments will charge different tax rates (and will give tax incentives) to different firms depending on how much they want to locate in the region;
- Corruption and influence.

Literature on regional policies' spillovers

However, a few recent studies have tried to gauge spillover effects:

Ham, J.C., Swenson, C., Imrohoroglu, A., Song, H., 2011. Government Programs Can Improve Local Labor Markets: Evidence from State EZs, Federal Empowerment Zones and Federal Enterprise Community. Journal of Public Economics 95, 779–797.

Hanson, A., Rohlin, S., 2013. Do spatially targeted redevelopment programs spillover? Regional Science and Urban Economics 43 (1), 86–100.

De Castris M., Pellegrini G., 2012. "Evaluation of Spatial Effects of Capital Subsidies in the South of Italy". Regional Studies 46 (4), 525–538.

Criscuolo, C., Martin, R., Overman, H., Van Reenen, J., 2012. "The Causal Effects of an Industrial Policy". NBER Working Paper, 17842.

Cerqua A., Pellegrini G., 2014 " Spillovers and Policy Evaluation" ,in F. Mazzola, D. Musolino e V. Provenzano (a cura di), Reti, Nuovi settori e sostenibilità, FrancoAngeli, 2014, ISBN 978-88-917-0887-8, pp. 353-370. (english *version:Beyond* the SUTVA: how policy evaluations change when we allow for interactions among firms)

Example

Let us imagine a scenario in which a classical regional policy, like a selective business incentives policy, brings about negative (positive) spillover effects on unsubsidised firms located in the vicinity of one or more subsidised firms belonging to the same sector of activity.

In order to estimate the causal effect of the policy on subsidised firms, traditional analyses (the ones relying on the SUTVA) identify the unsubsidised firms located on the eligible territory as those firms with the most similar features in respect of the subsidised firms. Nevertheless, in presence of spillovers, even a perfect control of the selection bias will not suffice to prevent a biased ATT estimate.

Reason: also the not treated firms are affected by the policy

Consequences: 1) **upward (downward) estimate of the ATT**

2) **no estimates of the spillover effects**

The main potential spillovers

The cross-sectional substitution. This externality occurs when subsidised firms take some of the investment opportunities that unsubsidised firms would have exploited in the absence of the policy.

The crowding-out effect. This is in act if the additional investment of the subsidised firms has crowded-out of the market unsubsidised firms.

The agglomeration effect { on unsubsidised firms. / on new entrants

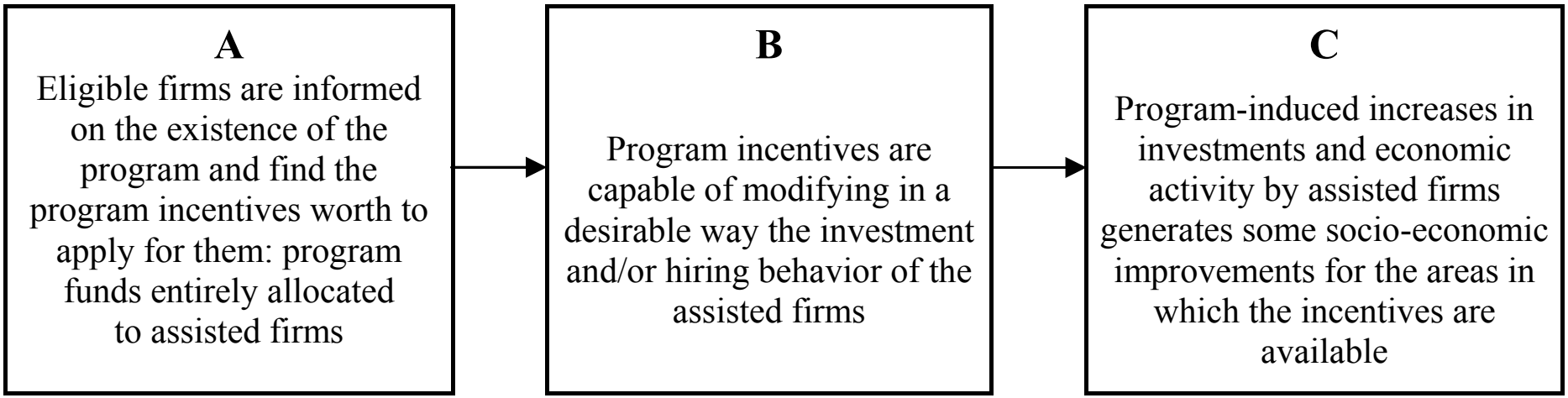
Due to the very limited information on how spillovers spread, we will turn to assumptions that allow retrieving an estimate of two spillover parameters:

i) **The Average Spillover effect on the Affected (ASA)** contrasts the positive agglomeration effect on unsubsidised firms with the cross-sectional substitution;

ii) **The Average Spillover effect on New Entrants (ASNE)** contrasts the crowding-out effect and the agglomeration effect on business births.

Spillover and regional policy: micro and macro evaluation (figure from Bondonio)

Figure 1: *Causal links from business incentives to desirable socio-economic outcomes*



The Stable Unit Treatment Value Assumption

The **SUTVA** holds if the causal impact of the subsidies on a firm does not depend on:

1) the intensity of the subsidies

2) the subsidies that other firms receive, including competitors.

The evaluation strategies based on the SUTVA do not model how firms affect each other's but assume that even if they interact, the subsidies received by one or more of these firms do not influence the future outcomes of the other interacting firms.

This assumption seems particularly strong especially when we talk about competing firms.

A general framework to relax the SUTVA in industrial or regional policy analyses

- Consider a group of firms indexed by $i=1,\ldots, N$.
- Let the random variable D_i denote a treatment indicator which is equal to 1 if treatment is received by firm i and 0 otherwise.
- Let $\boldsymbol{D}\equiv(D_1,\ldots,D_i,\ldots,D_N)$ represent the treatment assignment for all firms.
- Following Hong and Raudenbush (2013), we describe the potential outcomes for firm i as a function of the firm i's own treatment assignment (D_i), the treatment assignment of other firms ($\boldsymbol{D}_{-i}$), as well as the assignment of the focal firm to a different intensity of treatment (j).
- For firm i with intensity of the treatment j, the potential outcome is denoted by $Y_i(\boldsymbol{D},j)$
- The SUTVA is the special case: $Y_i(\boldsymbol{D},j) = Y_i(D_i)$

A general framework to relax the SUTVA in industrial or regional policy analyses

- Let each firm have its own set of influence i made up of all the firms that might affect firm's i potential outcomes and of which treatment assignment is represented by Di. Moreover, let $\Pr(q \in i)$ be a function of a vector Z.

- **ASSUMPTION 1**: There exists only 1 version of the treatment (1st part of the SUTVA),

  i.e. j = constant, $\forall i$;

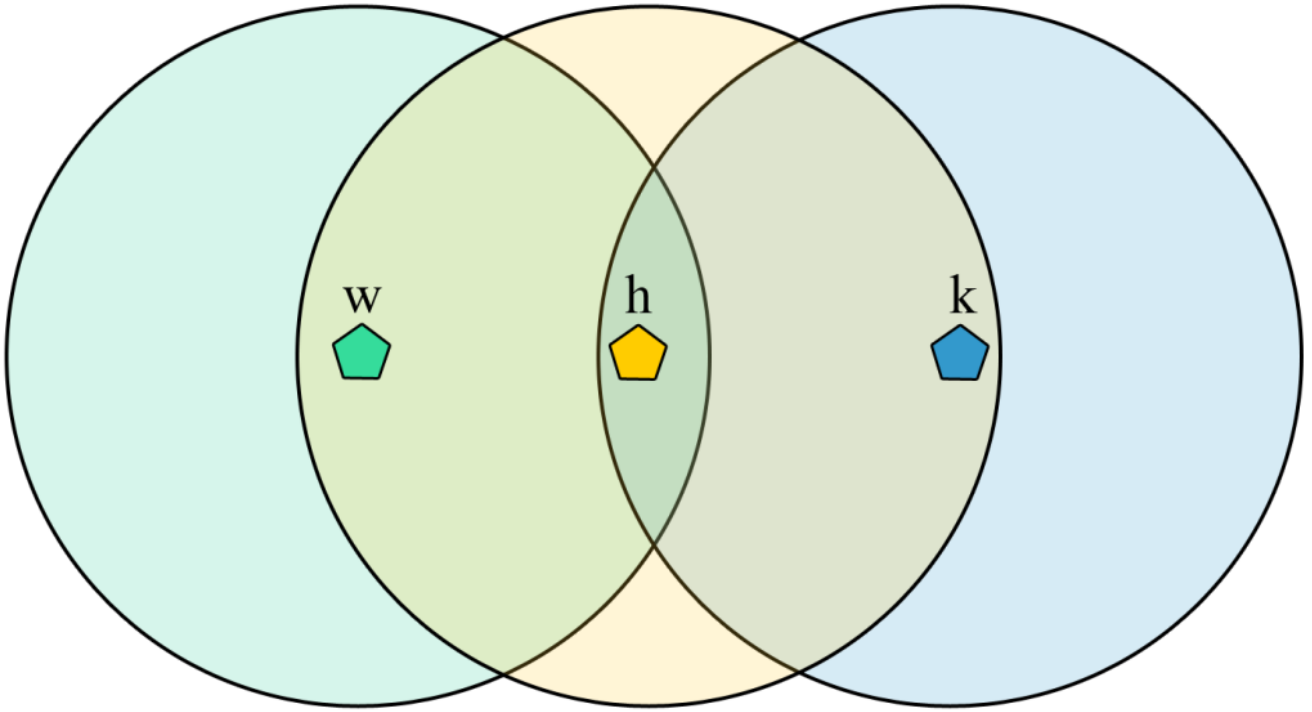
A general framework to relax the SUTVA in industrial or regional policy analyses

- **ASSUMPTION 2**: Firm *i* might interact only with firms belonging to ***i***, so there are $2^{\#i+1}$ potential outcomes for firm *i*, and individual causal effects may be defined as a comparison between any two of them: $Yi(Di,\mathbf{Di})$ versus $Yi(Di',\mathbf{D(i)'})$; $Di,Di' \in \{0,1\}$, and $\mathbf{Di,Di'} \in \{0,1\}^{\#i}$.

- **ASSUMPTION 3**: Second order spillovers are negligible, i.e. even if firm's *h* potential outcome, with $h \in \mathbf{k}$, is affected by the treatment of one or more firms in ***k*** this will not affect firm's *w* potential outcome when $w \in \mathbf{h}$ and $w \notin \mathbf{k}$.

A general framework to relax the SUTVA in industrial or regional policy analyses

Example of the latter assumption:



Example of the latter assumption:

- Focusing on the potential outcomes of firms h and w, $Y_h(D_h, D_w, D_k)$ and $Y_w(D_w, D_h)$, the latter assumption rules out that the possible influence on Y_h of the subsidy received by firm k, affects Y_w.

A general framework to relax the SUTVA in industrial or regional policy analyses

- **Definition 1**. The treatment effect for the subsidised firm *r*:

  $$Y_r(D_r=1,\mathbf{D_{(r)}})-Y_r(D_r=0,\mathbf{D_{(r)}=0})$$.

- **Definition 2**. The spillover effect for the unsubsidised firm *v*:

  $$Y_v(D_v=0,\mathbf{D_{(v)}\neq 0})-Y_v(D_v=0,\mathbf{D_{(v)}=0})$$.

- We might have used "policy effect" instead of "treatment effect" to remark that such effect might depend both on the subsidies received by firm *r* and on the subsidies received by other firms in its set of influence ***(r)***.

A general framework to relax the SUTVA in industrial or regional policy analyses

- Because of the fundamental problem of causal inference the aim is to estimate two average effects:
- **Definition 3.** The Average Treatment effect on the Treated (ATT):

$$E[Y_i(D_i=1,\mathbf{D_{(i)}})-Y_i(D_i=0,\mathbf{D_{(i)}=0})|\mathbf{D}]$$

- **Definition 4.** The Average Spillover effect on the Affected (ASA):

$$E[Y_i(D_i=0,\mathbf{D_{(i)}\neq 0})-Y_i(D_i=0,\mathbf{D_{(i)}=0})|\mathbf{D}]$$

- Note: The counterfactual scenario for the ATT does not consist merely in changing the assignment for firm *i* from $D_i=1$ to $D_i=0$ but also in removing the subsidy to all the other firms that belong to ***(i)*** if necessary, i.e. ***D(i)*** is changed to the null vector if $\mathbf{D(i)\neq 0}$.
- Alternative: $E[Y_i(D_i=1,\mathbf{D_{(i)}})-Y_i(D_i=0,\mathbf{D_{(i)}})|\mathbf{D}]$

A general framework to relax the SUTVA in industrial or regional policy analyses

- The **SUTVA** is a special case in which $Y_i \perp \boldsymbol{D}_{(i)} \; \forall \, i$, i.e. the future outcome of a firm does not depend on the treatment received by the firms belonging to its set of influence ⇒

  ATT: $E[Y_i(D_i=1)-Y_i(D_i=0)|\boldsymbol{D_i=1}]$

  ASA: *no spillover effects*

A general framework to relax the SUTVA in industrial or regional policy analyses

Table 1
Differences in the groups of firms in the proposed framework with respect to the traditional framework.

	Proposed framework	Traditional framework (SUTVA)
Treated Group	N_T = # of treated firms	N_T = # of treated firms
Affected Group	N_A = # of affected untreated firms	$N_A = \emptyset$
Control Group	N_C = # of not affected untreated firms	N_C = # of untreated firms

where, $N = N_T + N_A + N_C$

How to substitute the SUTVA? Cerqua and Pellegrini (2014)

If data could reveal the extension of spillovers we would come up with a structural model that takes into account the relationships between the firms and how they interact. However, this appears to be an utopian endeavour, not least because of the remarkable firms' heterogeneity.

Consequently, we turn to assumptions that inevitably are a priori and should still be considered fairly strong; nevertheless, those assumptions will allow partially relaxing the SUTVA and retrieving rough estimates of the spillover effects:

a) **Only intrasectoral spillovers**;

b) **Spillovers spread within a certain distance**.

According to a recent survey (Drucker, 2012), the majority of studies on the extent of agglomeration economies report that externalities exist primarily in a narrow band surrounding a spillover generator, while a strong decay can be observed after a few miles.

How to substitute the SUTVA? (5)

In order to estimate the ATT and the spillover effects we resort to a recent matching techniques: the **coarsened exact matching (CEM)**. The idea of the CEM is to temporarily coarsen each conditioning variable into substantively meaningful groups, exact match on these coarsened data, and then retain only the original (uncoarsened) values of the matched data (see Iacus et al., 2011). We combine the CEM with the difference-in-differences estimator (DiD)

⇒ **CEM-DID**

In order to estimate the ATT, each subsidised firm is matched with one or more unsubsidised firms:

a) belonging to the same sector of activity;

b) having similar values of the conditioning variables;

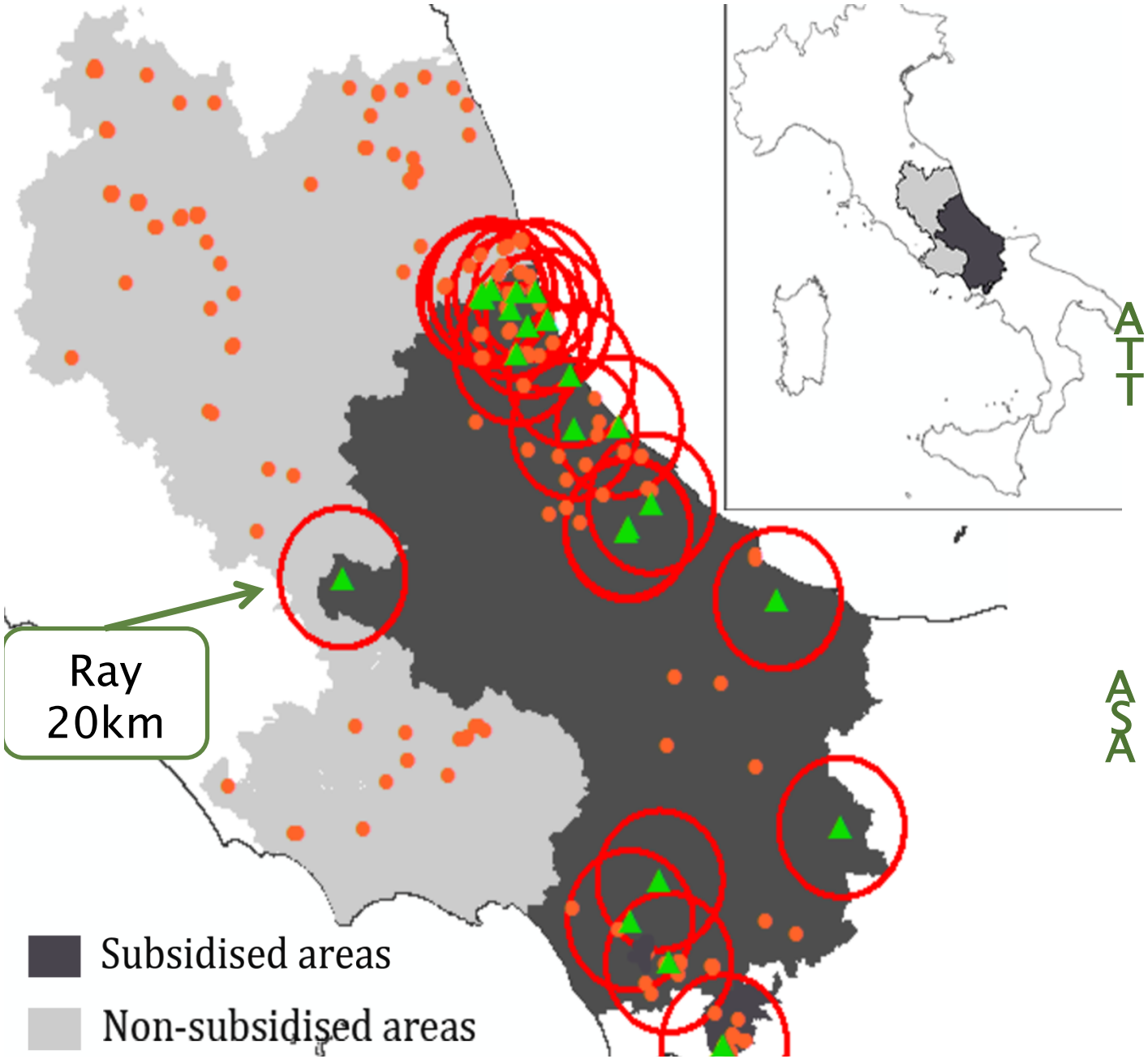
c) located in an area not interested by spillover effects but not too far away.

How to substitute the SUTVA? (6)

Conditioning variables:

1. the growth rate of tangible capital from 1993-1995;
2. the turnover in 1995;
3. the number of workers in 1995;
4. the ROE in 1995;
5. the number of neighbours with a maximum distance of 10km;
6. 13 dummy variables that subdivide the manufacturing firms in 14 subgroups according to the Istat ATECO 2002 classification.

How to substitute the SUTVA? (7)



Manufacture of textile and wearing apparel

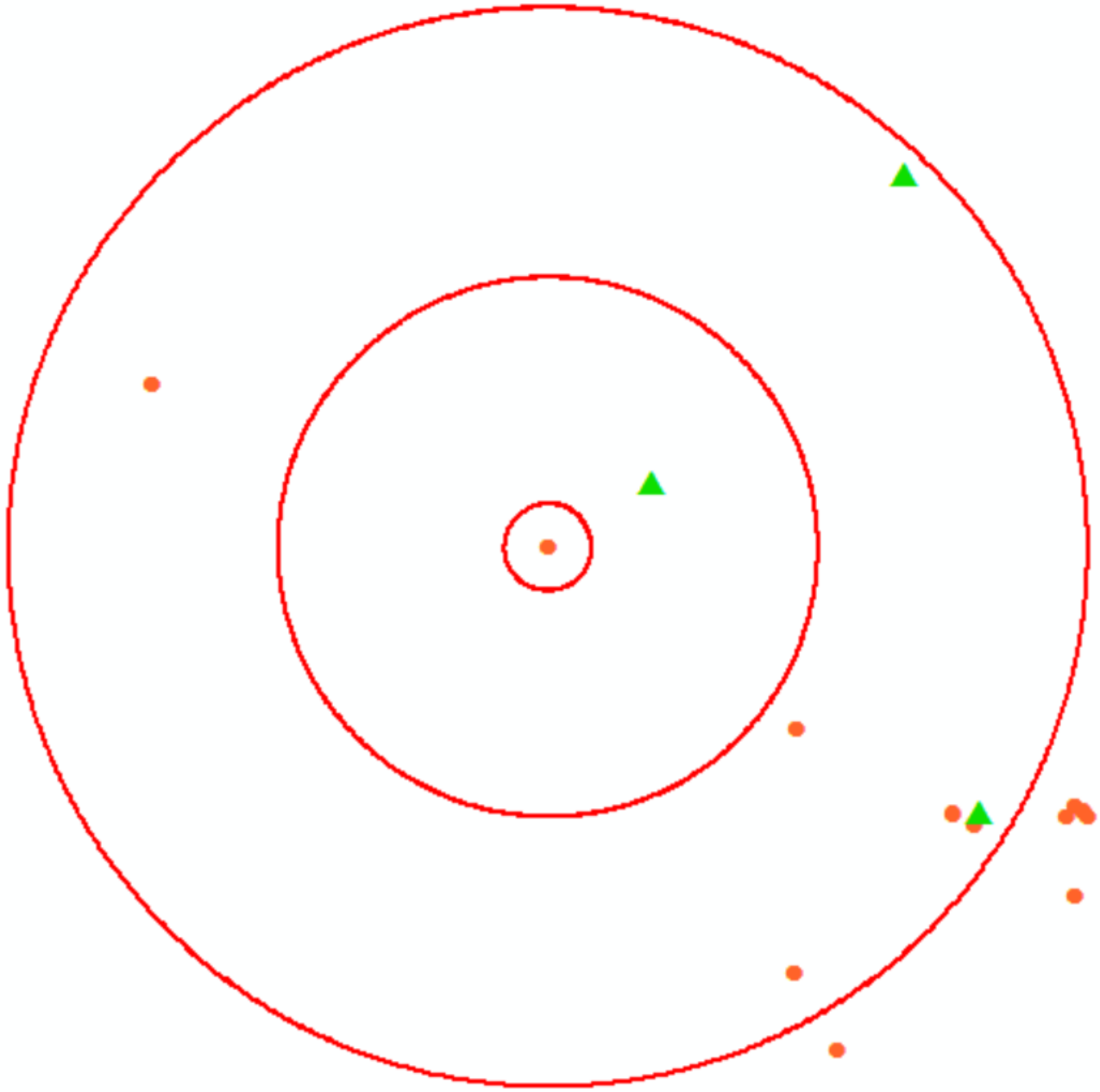
ATT

- Treated
- Non-treated and unaffected

ASA

- Non-treated but affected
- Non-treated and unaffected

How to substitute the SUTVA? (8)



Application

In our application we evaluate the **Italian Law 488/92 (L488)**, which has been the main policy instrument for reducing territorial disparities in Italy during the period 1996-2007. L488 operates in the less-developed areas of Italy, i.e. the areas designed as Obj. 1, 2 or 5b for the purpose of EU Structural Funds. L488 allocates grants on capital account through a rationing system on the basis of regional competitive auctions.

The investment projects are ranked on the basis of five criteria:

1) the share of owners' funds on total investment;
2) the new job creation by unit of investment;
3) the ratio between the subsidy requested by the firm and the highest subsidy applicable;
4) a score related to the priorities of the region in relation to location, project type and sector;
5) a score related to the environmental impact of the project.

Application (2)

The analysis refers to the period 1995-2001 and focuses on the four L488 auctions that were concluded by 2001. Data relative to the auctions derive from two different datasets:

a) the administrative L488 dataset of the Ministry of Economic Development;

b) a financial statement dataset, collecting data from AIDA and other sources of financial information.

Application (3)

By linking the L488 dataset with the financial statement dataset, we reconstruct a merged dataset for the period from 1995-2001 and after a complex process of cleaning and merging we have 2,213 manufacturing firms (code D of the NACE 2002 classification) that were localised in the areas under analysis.

Of these firms:

- 200 firms were subsidised
- 679 make up the group of affected untreated firms
- 1,334 make up the control group (not affected untreated firms).

Pre-treatment differences

Table 2: Pre-treatment differences between treated/affected and controls

	Variables	Treatment/ Affected Group	Averages computed without using the CEM weights		Averages computed after using the CEM weights	
			Control Group	Difference	Control Group	Difference
Treated	Tangible Capital 1995	2,313	935	(1,378)***	1,570	(743)*
	Turnover 1995	8,093	4,012	(4,081)***	6,838	(1,255)
	Growth rate of tang. cap. 93-95	21.72	17.62	(4.10)	17.67	(4.05)
	ROE in 1995	17.83	9.65	(8.18)***	10.02	(7.81)**
	Nb. of workers in 1995	42.60	24.25	(18.35)***	37.13	(5.47)
	Nb. Of neighbours in 10km	6.52	9.53	(-3.01)*	5.53	(0.99)
	Added value in 1995	2,168	1,064	(1,104)***	1,636	(532)*
	Liabilities in 1995	7,519	3,508	(4,011)***	5,849	(1670)
Affected (first mile)	Tangible Capital 1995	1,411	929	(482)**	1,066	(345)
	Turnover 1995	4,712	3,633	(1,079)*	4,060	(652)
	Growth rate of tang. cap. 93-95	10.67	12.45	(-1.78)	12.77	(-2.10)
	ROE in 1995	6.95	6.78	(0.17)	6.97	(-0.02)
	Nb. of workers in 1995	27.51	22.23	(5.28)*	26.83	(0.68)
	Nb. Of neighbours in 10km	13.43	6.09	(7.32)***	7.86	(5.57)***
	Added value in 1995	1,114	980	(134)	1,163	(-49)
	Liabilities in 1995	4,459	3,204	(1,255)***	3,740	(719)
Affected (between 1 mile and 10 km)	Tangible Capital 1995	791	786	(5)	740	(51)
	Turnover 1995	2,807	3,423	(-616)	2,918	(-111)
	Growth rate of tang. cap. 93-95	16.45	16.67	(-0.22)	14.43	(2.02)
	ROE in 1995	8.93	7.03	(1.90)	6.50	(2.43)
	Nb. of workers in 1995	17.31	21.64	(-4.33)**	18.33	(-1.02)
	Nb. Of neighbours in 10km	11.02	5.35	(5.67)***	7.91	(3.11)**
	Added value in 1995	706	908	(-202)**	782	(-76)
	Liabilities in 1995	2,657	3,008	(-351)	2,583	(74)
Affected (between 10 and 20 km)	Tangible Capital 1995	887	706	(181)**	716	(171)
	Turnover 1995	2,831	3,383	(-552)	3,447	(-616)
	Growth rate of tang. cap. 93-95	11.05	20.39	(-9.34)***	11.93	(-0.88)
	ROE in 1995	5.81	11.04	(-5.23)***	6.40	(-0.59)
	Nb. of workers in 1995	18.79	21.54	(-2.75)	20.42	(-1.63)
	Nb. Of neighbours in 10km	7.98	17.80	(-9.82)***	11.49	(-3.51)**
	Added value in 1995	803	894	(-91)	860	(-57)
	Liabilities in 1995	2,930	2,812	(118)	3,023	(-93)

Results

Table 3: ATT estimates

Bandwidth	CEM-DiD	CEM-DiD without the 4 strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of 4 strata
Yearly growth rate of tangible capital	8.94 (1.29)***	9.25 (1.34)***	10.24 (1.92)***
Yearly growth rate of turnover	3.08 (0.70)***	2.78 (0.71)***	1.78 (0.98)*
Absolute employment change for each firm	8.16 (1.58)***	7.07 (1.59)***	8.17 (2.28)***
Nb. matched subsidised firms	191	181	181
Nb. controls	787	783	783

Results (2)

Table 4: ASA estimates

	Bandwidth	CEM-DiD	CEM-DiD without the strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of the strata in the previous step
Within 1 mile	Yearly growth rate of tangible capital	0.67	0.25	1.37
		(1.48)	(1.47)	(2.14)
	Yearly growth rate of turnover	-0.02	0.08	-0.17
		(0.83)	(0.84)	(1.32)
	Absolute employment change for each firm	-1.93	-1.96	-1.80
		(0.94)**	(0.95)**	(1.42)
	Nb. matched affected firms	168	158	158
	Nb. controls	615	609	609
Between 1 mile and 10 km	Yearly growth rate of tangible capital	0.93	0.43	-0.72
		(1.21)	(1.25)	(1.95)
	Yearly growth rate of turnover	-0.76	-0.59	-0.69
		(0.69)	(0.71)	(1.06)
	Absolute employment change for each firm	-0.64	-0.60	-0.23
		(0.98)	(1.03)	(1.44)
	Nb. matched affected firms	249	235	235
	Nb. controls	811	806	806
Between 10 and 20 km	Yearly growth rate of tangible capital	-1.23	-1.33	-0.77
		(1.27)	(1.31)	(1.62)
	Yearly growth rate of turnover	0.16	-0.14	-0.25
		(0.69)	(0.69)	(1.02)
	Absolute employment change for each firm	-0.51	-0.57	-0.94
		(1.01)	(1.04)	(1.56)
	Nb. matched affected firms	236	227	227
	Nb. controls	924	921	921

Results (3)

ASNE

Our strategy to infer the prevailing spillover effect consists of looking at the ratio of the number of new entrants to the number of firm exits in certain areas. Considering new entrants (closing-down firms) located within a limited distance from the closest treated firm belonging to the same sector as new entrants (closing-down) affected firms and new entrants (closing-down firms) with no treated firms belonging to the same sector within the same limited distance as new entrants (closing-down) not affected firms it is possible to compare the aforementioned ratio for the 2 groups of firms.

We find some evidence of crowding-out effects that have prevailed over agglomeration effects for the affected firms with at least 1 treated in their closest set of influence.

Results (4)

Table 5: ATT estimates by number of neighbours

	Bandwidth	CEM-DiD	CEM-DiD without the strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of the strata in the previous step
Treated with a number of neighbours smaller than the median of their sector	Yearly growth rate of tangible capital	10.87 (1.87)***	10.87 (1.95)***	10.62 (2.73)***
	Yearly growth rate of turnover	5.38 (0.95)***	4.88 (0.98)***	4.48 (1.38)***
	Absolute employment change for each firm	10.12 (2.04)***	8.27 (1.99)***	8.31 (3.07)***
	Nb. matched subsidised firms	111	105	105
	Nb. controls	509	507	507
Treated with a number of neighbours larger than the median of their sector	Yearly growth rate of tangible capital	5.12 (2.01)**	7.76 (2.23)***	3.07 (3.16)
	Yearly growth rate of turnover	-0.22 (1.03)	-0.54 (1.12)	-2.05 (1.48)
	Absolute employment change for each firm	3.28 (2.67)	6.08 (2.70)**	6.20 (4.74)
	Nb. matched subsidised firms	74	65	65
	Nb. controls	247	241	241

Conclusions

Our novel approach allows evaluating the presence and the extent of micro spillover effects. We are able to consistently estimate the ATT and the ASA at the same time. We find nil or weak statistically significant spillovers with respect to investment, and turnover; however, we find negative employment spillovers that are statistically significant. This finding emphasises that **the ATT on itself is not a sufficient parameter to evaluate the effectiveness of a business incentives policy.**

The combined assessment of the 3 parameters suggests that **capital subsidies engender a growth process in the eligible area in terms of investment and employment (however the latter is partially determined to the detriment of affected firms).**

Possible explanations

In the factor market there is labour mobility (at least within a small area). Besides, in the goods market firms located in the same area compete on the same job-market

Capital is a very deep-rooted factor (at least in the short-run). Besides, in the goods market firms located in the same area often do not compete on the same product market

The combined assessment of the 3 parameters suggests that **capital subsidies engender a growth process in the eligible area in terms of investment and employment (however the latter is partially determined to the detriment of affected firms).**