Evaluating public support to the investment activities of business firms: A meta-regression analysis of Italian studies

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Getting into the debate

"*Much of the political debate surroundings such programmes remains at the level of ideology. [...] Yet as social scientists we have an obligation to try to brings facts to bear on these debates. [...] the social productivity of these programmes is fundamentally an empirical question*." (Jaffe, 2002, p. 23).

- We focus on Italy, which is relatively disregarded by existing reviews and where the debate about the effectiveness of industrial policies is occasional and based on partial evidences
- We analyse the available evaluation studies – whose number has grown fast in recent years - through a systematic review of the available literature and a meta-analysis
- We introduce some novelty in the MRA by considering unobserved study heterogeneity

Previous MRAs on enterprise and innovation policy

- Garcia-Quevedo (2004) on R&D subsidies (39 empirical studies*74 estimates) (IT: 1)
  - Ys are dummies for positive effect or for crowding-out
  - None of the observed study characteristics has an influence on the probability of a positive result; weak evidence of crowding out

- Negassi and Sattin (2014) (60*625) (IT: 3); Castellacci and Mee Lie (2015) (34*404) (IT:1) ; Gaillard-Ladinska et al (2015) (16*82 + 9*95) (IT: 0) on tax incentives/tax credits for R&D
  - y is the is the effect of tax credit on R&D investment (additionality ratio or user cost elasticity)
  - tax credit increases R&D expenditures particularly in the high-technology industry (1)
  - the additionality effect of R&D tax credits is stronger for SMEs, firms in the service sectors, and firms in low-tech sectors (2)
  - a reduction in the user cost of capital of ten percent raises stock of R&D capital by 1.3 percent and flow of R&D expenditure by 2.1 percent; the presence of a tax incentive scheme is associated with seven percent more R&D expenditure (3)

Systematic review, meta-analysis and MRA

(1) to perform a comprehensive review of the evidence, extract data from the studies that are included in the review and categorise the available information

(2) to combine data to produce a summary result of the systematic review

(3) to perform the meta-analysis, and, in particular:

- to avoid the simple vote count (publication bias)
- to assess the influence of some programme or study characteristics on the probability of particular results (e.g. probability of positive treatment effects)
- to test whether the influence found in the sample of studies under scrutiny is caused by something other than mere random chance

Articles and estimates

Most programme evaluation studies of economic and social programmes report several treatment effect estimates that can differ in terms of

- outcomes of interest (e.g. investment, employment, probabilities), these outcomes being expressed in different measurement units
- estimand (e.g. ATE, ATT), that may refer to difference in levels or in variations
- identification assumptions and consequent estimation methodology
- samples involved in estimation
- subsamples involved in estimation and/or to which specific estimates refer to (e.g. heterogeneity of effects)

Traditional MRA approaches (Stanley, 2008) are mostly thought for cases where outcomes are uniform between and also within studies (e.g. variation of R&D investment)

If not so: separate analysis depending on outcome?

Card et al. (2010) face this problem with active labour market policies: they set out a strategy to conduct MRA with binary or ordinal "summary" outcomes

Data

- ✓ 43 published and unpublished articles written from 2000 on * 478 estimates, adopting the tools of the conterfactual approach (Imbens and Wooldridge, 2009)

- ✓ Outcome variable: treatment effect

$$y_i = \begin{cases} 1 & \text{if the estimate is significantly positive} \\ 0 & \text{otherwise} \end{cases}$$

- ✓ Predictors: type of incentives, policy level at which the intervention is implemented, target of the interventions, year in which the programme is implemented, type of outcome on which treatment effects are estimated, timing of estimated impact, number of firms involved in the estimation, basic methodology used for estimation, publication status of article, …

Outcome variable and some predictors are measured at the level of estimates, while other predictors are defined /constant at the study level!

Each study usually contains a number of estimates (11 on average)

Vote counts

Type of programme	Significantly positive	Insignificant	Significantly negative	Total
R&D	76 (28.5%)	183 (68.5%)	8 (3.0%)	267 (100%)
Investment	59 (36.0%)	87 (53.0%)	18 (11.0%)	164 (100%)
Bank loans	26 (55.3%)	16 (34.0%)	5 (10.6%)	47 (100%)
Total	161 (33.7%)	286 (59.8%)	31 (6.5%)	478 (100%)

The meta-regression model

- We are interested in the probability that the response is 1 as a function of: i) the predictors $\boldsymbol{x}_i$ and ii) a term of unobserved heterogeneity at the study level u_s $E(y_i|\boldsymbol{x}_i, u_s)=Pr(y_i= 1 |\boldsymbol{x}_i, u_s)$
- u_s is important as observations from a same study cannot be assumed independent!
- Therefore, we estimate the following random-intercept logit multilevel model

$$logit\{\Pr(y_{is} = 1|\boldsymbol{x}_{is}, u_s)\} = \beta_0^C + \boldsymbol{\beta}^C \boldsymbol{x}_{is} + u_s$$

  where coeffcients β^C represent the change in the log odds ratio of having a significantly positive treatment effect estimate for a one unit increase in the predictor, conditional on u_S. The latter refers to the random error component for the deviation of the intercept of a group from the overall intercept.

- By means of the following nonlinear transformation we can use coefficients to compute probabilities

$$\Pr(y_{is} = 1|\boldsymbol{x}_{is}, u_s) = \frac{\exp(\beta_0^C + \boldsymbol{\beta}^C \boldsymbol{x}_{is} + u_s)}{1 + \exp(\beta_0^C + \boldsymbol{\beta}^C \boldsymbol{x}_{is} + u_s)}$$

On unobserved study heterogeneity

- could be due, for example, to the unobserved ability of the authors in framing the study or obtaining credible estimates, or also it might depend on their determination to search for particular results

- explanations of u_s can be only hypothetical, since it captures the "joint average" influence on *Y* exerted by all aspects that are not represented by observable predictors

- in order to assess the study-specific deviation from the overall intercept, we usually hypothesise that $u_s \sim N(0, \sigma_u^2)$ i.i.d

- once having estimated variance σ_u^2 we test whether it is significantly different from zero. Intuitively, the idea is that the greater this variance, the less negligible unobserved study heterogeneity is. Random effects can be then predicted by Empirical Bayes methods

- if one is interested in probability computations that are net of the term of unobserved study heterogeneity, these can be obtained by fixing all u_s at their mean value of zero

Two groups of estimates

- In studies using survey data, some of the previous variables are not specified (government level delivering the programme, type of targeting underlying this programme, …).

- Instead of fixing an unspecified category in these variables (which would coincide with that indicating data source) we specify two different groups:
  - The whole group of 43 studies, including 478 available estimates and a smaller set of covariates that are specified for all estimates;
  - A smaller group of 36 studies NOT using survey data, including 430 estimates and the complete set of covariates characterizing them

Results: Coefficient estimates

Baseline:
R&D programme
late 2000s
repayable loans
DID approach
Outcome observed well after treatment receipt
Outcome is not directly affected by this type of programme
Survey data
Study did not appear on a scientific journal

Restricted sample:
administrative data
national programme
all firms

	FULL SAMPLE	*RESTRICTED SAMPLE*
FIXED PART		
R&D (base)	0	0
	(.)	(.)
bank credit	1100	2713
	(1190)	(1670)
investments	0.983	1.741**
	(0.643)	(0.774)
national (base)		0
		(.)
regional		1.181*
		(0.674)
targets all firms (base)		0
		(.)
targets SMEs only		-1.835*
		(0.980)
loan (base)	0	0
	(.)	(.)
grant	0.103	-0.672
	(0.688)	(0.875)
tax credit	0.467	-0.122
	(1121)	(1408)
unspecified or mixed	1.937**	2523
	(0.951)	(1877)
other outcome (base)	0	0
	(.)	(.)
directly affected outcome	2.344***	2.909***
	(0.725)	(0.954)
N. of firms	-0.00000809	-0.00000168
	(0.0000194)	(0.0000210)
DID (base)	0	0
	(.)	(.)
RDD	1369	1.753*
	(0.860)	(0.962)
matched DID	-0.0331	-0.248
	(0.771)	(0.899)
matching	1042	1472
	(0.711)	(0.905)
other method	0.502	0.629
	(0.992)	(1386)

Results: Coefficient estimates (cont)

	FULL SAMPLE	*RESTRICTED SAMPLE*
implemented in late 2000s (base)	0	0
	(.)	(.)
implemented earlier	0.819	1329
	(0.829)	(0.968)
survey data (base)	0	
	(.)	
administrative data	2.591**	
	(1220)	
lagged estimate (base)	0	0
	(.)	(.)
simultaneous estimate	-0.502	-0.510
	(0.338)	(0.364)
appeared in other outlet (base)	0	0
	(.)	(.)
published in journal	-0.507	-0.159
	(0.592)	(0.704)
R&D # directly affected outcome (base)	0	0
	(.)	(.)
bank credit # directly affected outcome	-1145	-1644
	(1036)	(1211)
investments # directly affected outcome	-1.493*	-2.057**
	(0.843)	(1040)
Overall intercept	-4.418**	-2.858*
	-1716	-1513
RANDOM PART		
σ^2_u	1039	0.957
	(0.517)	(0.548)
LR test vs. logistic regression	17.90***	11.71***
Observations	478	430
Studies	43	36
AIC	544.4	485.8
Log likelihood	-253.2	-222.9

Interaction: programme type * type of outcome variable

No publication bias

Coefficient for the number of firms involved in estimation when the response variable is (A) a significantly positive or (B) a significantly negative treatment effect

(A) Significantly positive		*(B) Significantly negative*	
FULL SAMPLE	*RESTR. SAMPLE*	*FULL SAMPLE*	*RESTR. SAMPLE*
-0.0000081	-0.0000017	0.0000140	-0.0000148
(0.0000194)	(0.0000210)	(0.0000237)	(0.0000286)

The increase in sample size is associated …

- neither with a higher probability of having significantly positive effects
- nor with a higher probability of having significantly negative effects

which enables us to deem that our analysis is very unlikely to suffer from publication bias

Results for some common policy schemes

A. R&D grant, targeting both small and larger firms
B. Guaranteed loan for SMEs only
C. Investment grant, targeting both small and larger firms

We fix predictors at particular values representing policy schemes, we also fix all u_s at their mean value of zero

We predict probabilities of success depending on the fact that:

- the outcome variable which the treatment effect refers to is a variable that the programme in question is intended to modify in a direct way
- the government level delivering the programme is national or regional

R&D grants for all firms

Average adjusted probability predictions; random effects fixed at zero

	(A) whatever level	(B) national level	(C) regional level	(C - B) difference
DIRECTLY AFFECTED OUTCOME	0.732***	0.596**	0.813***	0.217
	(0.070)	(0.232)	(0.083)	(0.145)
OTHER OUTCOME	0.188***	0.100*	0.245***	0.145*
	(0.061)	(0.056)	(0.083)	(0.080)

Notes. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Guaranteed loans for SMEs

Average adjusted probability predictions; random effects fixed at zero

	(A) whatever level	(B) national level	(C) regional level	(C - B) difference
DIRECTLY AFFECTED OUTCOME	0.715***	0.575***	0.799***	0.224
	(0.161)	(0.215)	(0.145)	(0.139)
OTHER OUTCOME	0.461**	0.309	0.557**	0.248*
	(0.214)	(0.203)	(0.233)	(0.137)

Notes. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Investment grants for all firms

Average adjusted probability predictions; random effects fixed at zero

	(A) whatever level	(B) national level	(C) regional level	(C - B) difference
DIRECTLY AFFECTED OUTCOME	0.675***	0.527***	0.764***	0.238*
	(0.112)	(0.146)	(0.116)	(0.131)
OTHER OUTCOME	0.501***	0.346***	0.599***	0.253*
	(0.105)	(0.115)	(0.126)	(0.137)

Notes. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Provisional conclusions and future steps

- Probability of some success is non negligible
- More positive effects when the outcome variable is directly affected by the policy
- There is no evidence about the weaknesses of the regional policy. However, we have to consider that evaluations are mostly referred to regions having a decent quality of government (Rodríguez-Pose and Garcilazo, 2015)

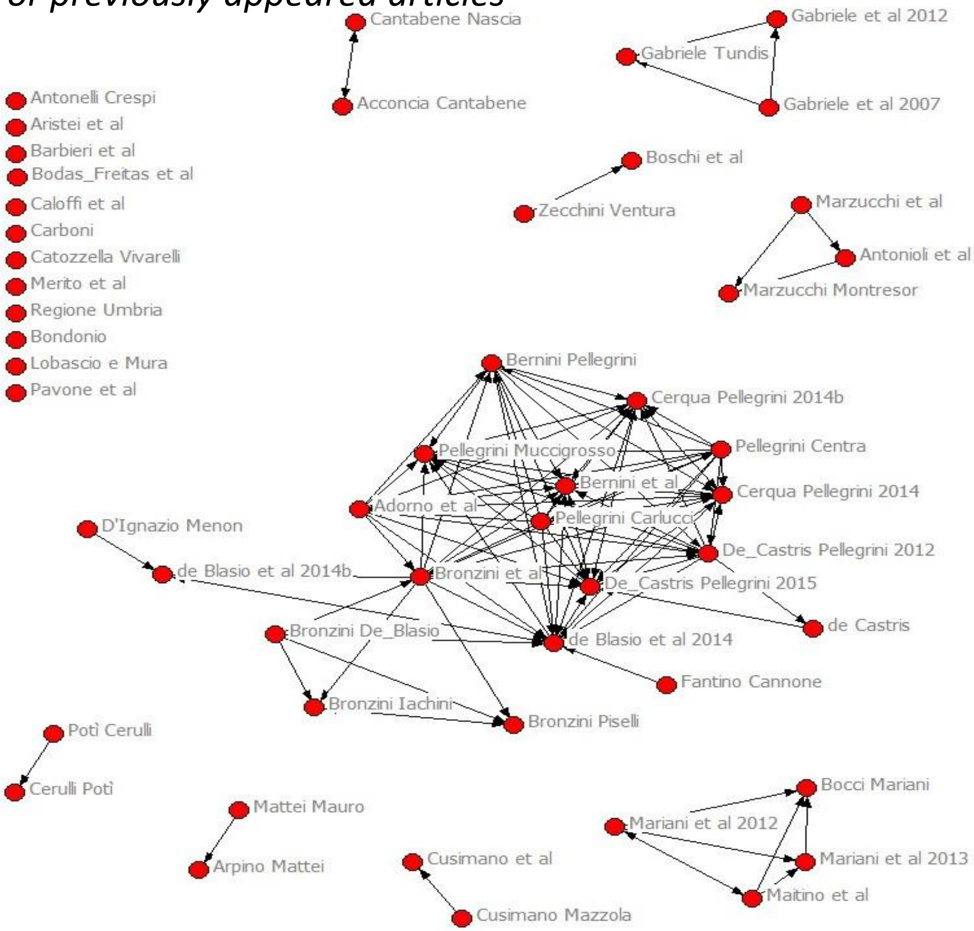
What we are doing right now ...

- Add more than 400 estimates relative to treatment effects in meaningful subgroups (e.g. advantaged / disadvantage firms)
- Also: inclusion of a few additional evaluation studies appeared in the last few months
- On a subset of estimates expressed in the same measurement unit, we carry out a more traditional MRA with a model for the magnitude of treatment effects

What we are doing right now (with C. Bocci)

Random effects have been so far assumed as independent from one another. However, it can be viewed as unrealistic to assume independence between studies, for example between those sharing co-authors.

Relationship btw articles sharing at least 1 coauthor, each article receives influence only from contemporary or previously appeared articles



The Neigbours' strategy (spatial)

Estimates are nested into studies, while studies are no longer independent as they may receive influence from neighbouring studies

Build an adjacency matrix **W** where studies are neighbours if they share some co-authors. This matrix describes how r.e. from neighbouring articles are related

Hypothesise and model random effects *v* with a simultaneously autoregressive (SAR) structure. Now, the random term is $v=\rho \mathbf{W} v+u$, where

--- **W** the adjacency matrix

--- *u* is a normally distributed random component $u_s \sim N(0,\sigma_u^2)$

--- ρ is a spatial autoregressive coefficient. It defines the strenght of the "spatial" relationship described by **W**

In sum, we need to estimate ρ and σ_u^2

Thank you

for any comment or suggestion!

	At the level of estimates	At the level of studies
	Mean	Group mean
Response variable: treatment effect is significantly positive	0.337	
At least one treatment effect is significantly positive		0.907
Variables that are constant within studies		
Study was published in a journal	0.536	0.651
Study uses administrative rather than survey data	0.900	0.837
Programme type		
R&D	0.559	0.512
investments	0.343	0.372
bank loans	0.098	0.116
Variables that are not always constant within studies		
Outcome directly affected by the programme	0.297	0.356
Non simultaneous treatment effect	0.609	0.442
N. of firms involved in estimation	4158	5086
Target firms		
Target all firms	0.776	0.605
Target SMEs only	0.140	0.244
unspecified	0.084	0.151

	At the level of estimates	At the level of studies
	Mean	Group mean
Government level delivering the programme		
national	0.362	0.430
regional	0.554	0.419
unspecified or mixed	0.084	0.151
Incentive type		
unspecified or mixed	0.109	0.197
loan	0.289	0.201
grant	0.554	0.528
tax credit	0.048	0.074
Basic methodology used for estimation		
DID	0.201	0.205
RDD	0.098	0.128
matched DID	0.425	0.209
matching	0.218	0.322
other	0.059	0.136
Year of the programme		
late 2000s	0.149	0.209
earlier	0.851	0.791
Number of observations	478	43