EVALUATING PUBLIC SUPPORTS TO THE INVESTMENT ACTIVITIES OF BUSINESS FIRMS: A META-REGRESSION ANALYSIS OF ITALIAN STUDIES

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Abstract

This paper presents a meta-regression analysis of recent micro-econometric evaluations of enterprise and innovation policies implemented in Italy. We categorise 478 programme impacts from 43 studies, all obtained using methods that are appropriate for causal inference in observational settings, and analyse which programme, study and estimate characteristics are associated with higher probability of success net of unobserved heterogeneity at the study level. We find that several types of programmes yield non-negligible probability of success and that the outcome variable used to measure programme impact matters. If there exist any differential in probability of success between the government levels that may deliver the programmes, this differential is favourable to regional governments.

JEL classification: H53, L52, L53

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We thank the authors who responded to our survey for their co-operation and assistance. This article is in the early stages of circulation. Despite our best efforts to include in the analysis all studies that appeared in Italy in recent years, we might unintentionally have missed this goal. In case you are aware of any evaluation studies that have not been considered here, please help us to fill these lacks.
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"*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).

1. Introduction

The use of public funding to foster different types of private investment is a common practice in many countries. For those belonging to the EU, the European Commission has established, since long time, specific guidelines in order to avoid that national (and regional) supports to business companies hamper competition. Moreover for the current programming period of the European Structural Funds, 2014-2020, the Commission requires an ex-post evaluation, based upon counterfactual methods, of the policy measures providing financial aids to private firms. Such an obligation to scrutinise how this portion of tax-payers money is spent has probably raised the cheers of some Italian would-be experts and opinion leaders: finally, there will be the chance to proof that, in Italy, public incentives to business firms are most of the times a waste of money and, thus, should be drastically reduced (Giavazzi et al., 2012).

Actually, the foes of public incentives to enterprises do not know or guiltily neglect that in our country, over the last decade, the number of evaluations concerned with this topic was remarkable. In fact, considering the time span that goes from 2003 to 2015, we found 43 published studies on the effectiveness of public incentives to the investment activities of Italian firms. It must be stressed that this number is confined to the micro-evaluation analyses carried out in compliance with the methodological standards of the so-called "econometrics of programme evaluation" (Imbens and Wooldridge, 2009) and, as such, fully satisfy the above mentioned requirement of the European Commission. [1]

Only a scant minority of these empirical works reports negative effects or no effect at all of the provided incentives, a finding that clashes with the liberalist vulgate invoking a retreat from public supports to private firms. However, as stressed by Stanley (2008) a simple vote-counting of studies (by distinguishing those reporting "positive", "insignificant" or "negative" effects) could be misleading. [2] First of all, "statistically significant results are often treated more favourably by researches, reviewers and/or editors; hence, larger, more significant effects are over-represented. [...] Without some correction for publication bias, a literature that appears to contain a large empirical effect offers little, if any, reason for accepting this effect." (Stanley, 2008, p. 104). Moreover, studies using larger

[1] If we had considered also ex-post evaluations based on less demanding methods (such as those based on interviews of beneficiaries) the number of studies would have almost doubled.

[2] Examples of vote-counting analyses concerned with the effects of R&D supports to business firms can be found in García-Quevedo (2004) and Parsons and Phillips (2007).

samples of firms are likely to find more statistically significant results (either positive or negative) than those based on smaller samples (Card et al., 2010). Finally and most importantly, rather than simply establishing what is the prevailing effect, a more interesting question for both researchers and policy makers is whether there are some factors (such as the chosen estimation technique, the type of incentive, the targeted beneficiaries, etc.) that increase the probability of such an effect. In this respect, literature reviews could provide useful insights. [3] However, each survey contains a degree of subjectivity because the reviewer chooses the studies to be included and, although she tries to be as much comprehensive as possible, she attaches different weights to the selected works in order to identify the reasons of why contrasting findings are likely to emerge.

The approach that attempts to consistently address all the above issues is that of Meta-Regression Analysis (MRA), that is a "regression analysis of regression analyses" (cf. Stanley and Jarrell, 1989, p. 299). Being based on a quantitative exam of the literature, MRA allows one to test whether there are publication biases, as well as if the results change with the model specification and estimation method. Moreover, from a policy perspective, the approach is useful to identify whether the change or the probability of a given outcome (e.g. increase of investment expenditures, improvement of firm performance, etc.) is affected by some features of the policy measure (e.g. types of incentives, eligible beneficiaries, public bodies managing the intervention, etc.).

Garcia-Quevedo (2004), Negassi and Sattin (2014), Castellacci and Mee Lie (2015), Gaillard-Ladinska et al. (2015) apply this method for analysing the effects of public incentives on the R&D activities of business firms, while Kluve (2010) and Card et al. (2010) perform a MRA for some active labour market policies implemented, respectively, in Europe and worldwide.

In this paper we apply a MRA to the already mentioned empirical studies that have estimated the effects of public support to the investment activities of Italian firms. To our knowledge, this is the first application of a MRA to such type of micro-evaluation studies. In order to achieve a sufficiently high number of observations we have considered 43 published works, providing about 470 estimates, concerned with the impacts of different public incentives (subsidies, soft loans, tax credits, public loan guarantees) on different kinds of outcomes (*inter alia*, expenditures on R&D as well as other categories of tangible and intangible investment, innovation activities, debt consolidation, firm performance in terms of employments, sales or productivity). Because of the wide spectrum of outcome variables taken into consideration, we perform a MRA by using as dependent variable a binary indicator equal to one when the public support has generated a "significantly positive" result. Thus, as in Garcia-Quevedo (2004), Kluve (2010) and Card et al. (2010) the analysis refers to the sign and statistical significance of the policy effects. In a further step, we plan to carry out a MRA also for

[3] Recent surveys of international studies evaluating the effects of R&D subsidies or tax incentives are Zùniga-Vicente et al. (2014) and Becker (2015). On the same topic, Potì (2010) reviewed the results arising from Italian studies.

the size of the effects which, however, will be limited to a subset of studies characterised by homogenous (and, thus, comparable) outcome variables.

2. Data

In order to collect a relevant sample of evaluation studies on the effectiveness of public incentives to the investment activities of Italian firms we performed a literature search on the most popular search engines (Scopus, Google Scholar and Google). We carefully investigated the reference lists of the studies retrieved and, eventually, we asked information to colleagues affiliated to the main national associations of (applied) economists (SIE, SIEPI, AISRe). To facilitate the comparability of the studies, we selected only those papers adopting the methodological tools of the econometrics of programme evaluation (Imbens and Wooldridge, 2009) or other methodologies that are suitable to identify causal effects (e.g. Heckman-style approaches, Marginal structural models, etc.). Since these methods were primarily thought for the identification of treatment effects in the presence of independent observations (e.g. under the Stable Unit Treatment Value Assumption; cf. Rubin 1980; Imbens and Rubin, 2015), they were used so far for evaluating the incentives to individual enterprises. Therefore, we have excluded from our analysis policies targeting groups of firms, innovation poles, and other types of temporary associations or consortia, for which these methods are not applicable. Finally, we have considered both articles published on academic journals or books and unpublished studies (e.g. working papers), written from 2000 on. The choice of including studies appeared in outlets other than scientific journals was made not only for the sake of completeness, but also to guard against publication bias.

The result is a database including 43 studies. Given that most of them contain more than one estimate, the total number of estimates amounts to 903. Some of these papers (23) also estimate the effects as a function of particular firms' characteristics. However, in this first stage of our analysis, we will disregard these additional estimates, by focusing on the average effects (average treatment effect, ATE or average treatment effect on the treated, ATT) referred to the whole sample of firms considered in each study. Note that, depending on the identifying assumptions that lie behind the methodologies adopted in the studies under scrutiny, treatment effect estimates can represent either global or local effects. This distinction must be recalled because some methods, such as the RDD or instrumental variables, can only return local effects (respectively, at the threshold of an assignment variable or for so-called compliers) of potentially high internal validity, while other methods (e.g. difference in differences, matching) can return treatment effects referred to the overall sample but with the potential loss of some internal validity (Imbens and Rubin, 2015).

After having excluded the heterogeneity effects, the number of papers we focus on is 43, which include 478 estimates. More than half of these papers (28) have been published in scientific journals.

For each of the observed studies we consider the following variables: average effect, type of policy, policy level at which the intervention is implemented, target of the interventions, type of incentive, year in which the programme is implemented, type of outcome on which treatment effects are estimated, time of estimated impact, number of firms involved, and basic methodology used for estimation. Table 1 provides some descriptive statistics of the observed variables. As highlighted in the table, some of these variables are constant at the level of articles, while others are related to single estimates.

The average effect of the policy (be it global or local) represents the dependent variable we focus on. Given that the examined studies consider policies of different types and take into account different outcome variables, such effects are far from being homogenous. Therefore, in order to compare them, we dichotomise the ATT or ATE and define a variable *significantly positive* equal to one when the effect found by the observed study is positive and statistically significant, and zero otherwise. As shown by the table, 91% of studies report at least one significantly positive treatment effect estimate. Moreover, about one third of overall estimates report significantly positive treatment effects.

The studies considered in our MRA evaluate three broad types of policies: i) R&D incentives; ii) investment subsidies, and iii) bank loans accompanied by public guarantees. The former type is the most frequently analysed, while the last one is considered by 5 papers only. As shown in Table 2, the majority of studies evaluating the effects of bank loans report significantly positive results, while the evidence supporting the beneficial effect of other types of policy seems to be lower. However, only a small percentage of estimates report the existence of a negative effect of the programmes.

The policies evaluated in the different studies have been implemented both at national and at regional level. These latter studies mostly refer to policies that have been implemented in central or northern Italian regions.[4] In addition, we also consider some papers that do not refer to a specific policy, but use survey data in order to understand whether public support leads to positive results. In most of these cases, the surveys do not help identifying what is the level of government responsible for the delivery of policies and other details of the programme actually analysed. [5]

As for the target of the interventions, we distinguished the policies aimed at supporting micro, small and medium-sized firms from those including also large firms among their beneficiaries. This distinction makes sense only for R&D and investment subsidies, because loan guarantees are provided to SMEs only.

[4] These regions are Tuscany (6 studies), Emilia Romagna (4), Piedmont (2), Trento Province (2), Sardinia, Sicily, Umbria (1 each). One study does not report the name of the region where the evaluated program took place.

[5] There are 7 studies using survey data. However, for two of these papers the authors are able to identify the specific program in which firms have participated and consequently what is the government level that is responsible for the implementation of such program.

Table 1. Some descriptive statistics of the studies considered in the meta-regression analysis

	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
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 (from 2013 on)	0.149	0.209
earlier	0.851	0.791
Number of observations	478	43

Notes. Group mean refers to the between-study mean of the within-study means. All variables, with the sole exception of *n. of firms involved in estimation* are binary variables.

Table 2. Effects of the different types of programmes (number of estimates) by sign and statistical significance

Type of programme	Significantly positive	Insignificant	Significantly negative	Total
R&D	76 (28.5%)	183 (68.5%)	8 (3.0%)	267 (100%)
Investment subsidies	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 types of incentives used in the observed programmes are soft loans (with zero or reduced interest rate, that can be, but not always are, combined with a public guarantee), non-repayable grants or tax credits. As shown in Table 3, both R&D support and investment subsidies mobilise all type of instruments while bank credit is, by definition, based on the provision of soft loans.

Table 3. Type of programme and type of incentives used by the policymakers (number of estimates)

Type of incentives	R&D	Investment subsidies	Bank loans	Total
Unspecified or mixed	51 (19.1%)	1 (0.6%)		52 (10.9%)
Soft loans	88 (33.0%)	3 (1.8%)	47 (100.0%)	138 (28.9%)
Grants	124 (46.4%)	141 (86.0%)		265 (55.4%)
Tax credits	4 (1.5%)	19 (11.6%)		23 (4.8%)
Total	267 (100.0%)	164 (100.0%)	47 (100.0%)	478 (100.0%)

Studies largely differ in terms of the outcome variable they use. In addition, each study usually presents more than one outcome variable. When specifying our simplified binary outcome variable, we are aware of the fact that we are summarising results that relate to different dimensions of firms' activities. Some of these may be more directly affected by the subsidy, while others may be influenced in an indirect manner only. For example, the R&D literature discusses the distinction between effects on inputs, outputs or firm behavior (Cerulli, 2010). The first concerns R&D expenditures and possibly R&D employees, while the others refer either to the results of the innovative process or to side effects of learning stemming from that process, which take place after the subsidy. Such distinction is not usually provided by the literature on other areas of policy intervention (e.g. investments or bank loans). However, it may be useful to try to distinguish the type of effects. In order to do so we created a binary variable that takes value of 1 if the effects are measured on the variables that are more directly affected by the public support. In the case of R&D such variables can be R&D investments or employees. In the case of investment subsidies it can be the amount of investment in fixed or intangible capital, while in the case of bank credit it can be the reduction of the interest rate on the aggregate debt, long-term debt growth or variables signalling debt consolidation. As shown in Table 1, about 30% of the estimates are related to such types of variables, while the remaining part refers to variables measuring indirect or side-effects.

Less than half of the estimates refer to the simultaneous effects of the programmes, while the remaining part focus on short, medium or even relatively long-term effects (4 years or more from the end of the programme).The latter are more frequent if the estimation concerns indirect effects.
Matched difference-in-difference and matching are the most commonly used methods of counterfactual evaluation, followed by difference-in-difference. Only few studies use regression discontinuity designs, instrumental variables or other approaches. The majority of policies evaluated have been implemented before 2005.

3. Methodology

As previously explained, the treatment effect estimates, denoted in what follows by the subscript i, that were drawn from the articles under scrutiny were transformed in order to build a dichotomous outcome:

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

The probability of a positive outcome is a function of the covariates introduced in the previous section: $E(y_i|x_i)$=Pr$(y_i = 1|x_i)$. Remember that x_i includes covariates that can refer both to estimates, x_i^E, and to studies, x_i^S .
This expectation must be estimated by means of a generalised linear model, which requires the specification of a nonlinear function such as $g\{(\Pr(y_i = 1|x_i) = \beta_0 + \beta x_i\}$, where $g(.)$ is a particular link function and the right-hand term is referred to as its linear predictor (Agresti, 2015). A popular choice of link function is the logit link.
Now, in our setting each estimate *i* belongs to a study *s*, usually including other estimates *j* reporting, for example, treatment effects on alternative response variables, or estimating the same treatment effect by means of different empirical strategies. If estimates *i* and *j* are drawn from the same study they cannot be considered independent, with the reasons for possible correlation very often being unobserved to the researcher. In our case, one could hypothesise that this correlation could be due 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. Unfortunately, we cannot go beyond conjectures: since these aspects are not observable through available covariates. We can only try to have an idea of their joint influence on our dichotomous response.
Depending on how much emphasis one wants to place on these terms of unobserved heterogeneity in the analysis, two approaches can be envisaged.
A first one consists of estimating the probability that the response is 1 only as a function of the observable covariates, without accounting for unobserved study heterogeneity:

$$logit\{\Pr(y_i = 1|x_i)\} = \beta_0^M + \beta^M x_i \ ,$$

where the coefficient vector β^M represents the change in the log odds ratio of having a significantly positive treatment effect estimate for a one unit increase in the predictor. Then, the direct influence of each single covariate on the probability of having a significantly positive treatment effect estimate can be computed by applying:

$$\Pr(y_i = 1|x_i) = \frac{\exp(\beta_0^M + \beta^M x_i)}{1+\exp(\beta_0^M + \beta^M x_i)} \ .$$

Note that that the influence we are talking about here is on the marginal, or population-averaged, probability of having a significantly positive treatment effect estimate (hence the superscript M on the coefficients). The issue of correlation within clusters of estimates belonging to the same study does not distort coefficients but it does affect their standard errors, which end up being underestimated. This is why, under these circumstances, the use of a cluster-robust (at the study level) estimator of the standard error is recommended (Cameron & Miller, 2015).

An alternative approach consists of estimating the probability of a positive outcome as a function of the observable covariates and a term of unobserved heterogeneity at the study level, u_s , which captures intra-cluster correlation: $logit\{\Pr(y_{is} = 1|x_{is}, u_s\}$. The term u_s can be thought either as a study fixed effect or as a random effect, a distinction that is familiar in panel-data econometrics. For our cross-sectional setting we opt for random effects since the variables we are interested in are both at the estimate and study level, the latter being incompatible with study fixed effects which would explain alone all differences between studies (Snijders & Bosker, 2012).

A straightforward way to specify the relationship in our setting is that of a random-intercept multilevel model (Skrondal & Rabe-Hesketh, 2004; Snijders & Bosker, 2012):

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

where the coefficient vector β^C represents 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 the term u_s, which in turn refers to the random error component for the deviation of the intercept of a group from the overall intercept β_0^C. Once we compute probabilities, the term u_s is also there:

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

which returns a conditional (hence the superscript C on the coefficients), or study-specific probability of having a significantly positive treatment effect estimate. 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 zero, a value which, as will be seen shortly, represents 'by construction' the mean of all random effects.

All this entails that, if unobserved study heterogeneity plays a role, $\beta^C \neq \beta^M$ and $\Pr(y_i = 1|x_i) \neq \Pr(y_{is} = x_{is}, u_s)$ since: i) the vector β^C is estimated net of the random study-level errors, while β^M is not; ii) the study-specific term u_s also contributes to probability. Quite obviously, the equivalences $\beta^C = \beta^M$ and $\Pr(y_i = 1|x_i) = \Pr(y_{is} = x_{is}, u_s)$ are recovered when unobserved study heterogeneity does not exist, i.e. when, for each pair of studies l and m, we have that $u_l = u_m$.

In order to estimate the group-specific deviation from the overall intercept, we must hypothesise that it follows some particular distribution. The usual prior is that $u_s \sim N(0, \sigma_u^2)$, which amounts to assuming that random errors are normally distributed with zero mean (at their mean value there is no deviation from the overall intercept) and unknown variance. The estimation of the variance parameter σ_u^2 relies on the maximum likelihood approach. Once having estimated this variance 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.

4. Model specification

A key decision relates to the specification of our meta-regression model(s), both in terms of the covariates to be included and the functional form that the linear predictor should take in order to return a potentially interesting set of results.

As for the covariates, all of them have been illustrated in Section 2. Here, it is important to recall that some of them are usually not specified in studies using survey data, namely the government level delivering the programme (whether it is national or regional) and the type of targeting underlying this programme (*inter alia*, whether it is for SMEs only or also for larger firms). One possibility consists of fixing an unspecified category in these variables, but the latter is very likely to coincide with that indicating survey (i.e. non administrative) data. In order to avoid these complications, we prefer to focus separately on two estimation samples: the larger one comprises all 478 available estimates but relies on a smaller set of (meaningful) covariates; the smaller one comprises 430 estimates for which full information is available, including covariates detailing the programme.

As for the functional form of the linear predictor, it should of course be established whether the inclusion of the term of unobserved study heterogeneity makes sense. Further options to be evaluated regard if the linear predictor should be merely additive or if it is useful to introduce interactions between some of the variables. An interaction looks particularly attractive in our context: that involving the type of programme, the timing of treatment effects, and the type of variable on which the treatment effect is estimated.

To sum up, we specify two models for the linear predictor:

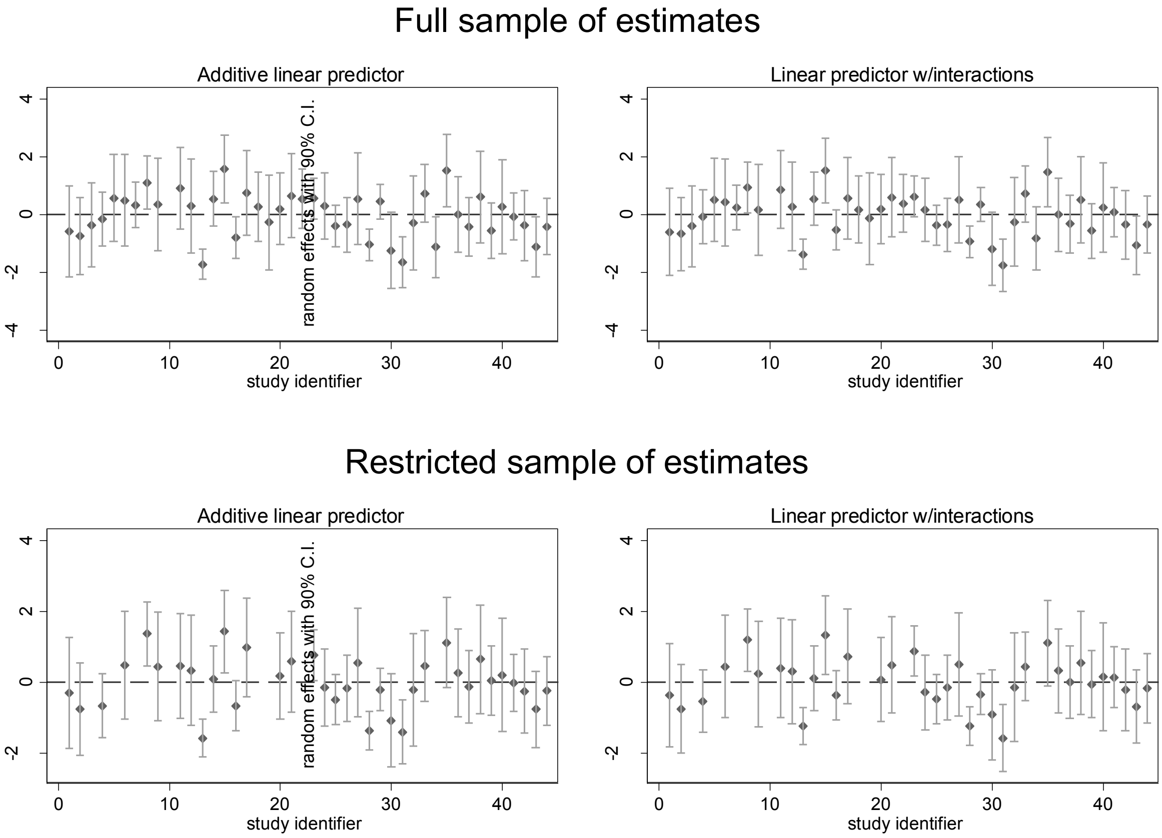
- the first specification is merely additive: it enables us to assess whether a multilevel model is appropriate and, if so, to estimate conditional probabilities of success for each variable of interest;
- the second specification sees the programme type interacted with the timing of treatment effects and the type of outcome variable: it enables us to highlight whether the conditional probability of success for each type of programme is higher depending on the time span in which the outcome is measured as well as on the fact that the outcome is more or less directly affected by the programme.

5. Results

The presentation and discussion of the results proceed as follows. First we assess whether a multilevel, random-intercept logit model is more appropriate for the analysis that the simple logit model. Second, we briefly comment on the coefficient estimates resulting from the most appropriate model and explain what enables us to deem that our analysis is very unlikely to suffer from publication bias. Third, we use these coefficients to predict the average probabilities of success associated with each covariate, as shown in Section 3. Last, in Section 6, we compute the probability of success descending from the joint action of particular combination of covariates that identify some of the most common policy schemes.

In order to establish if unobserved study heterogeneity represents a negligible issue we estimate its variance parameter σ_u^2 and compare the multilevel model to the ordinary logit without σ_u^2 by means of a likelihood-ratio (LR) test. Both with the full and restricted samples (see Table 4), the parameter σ_u^2 is rather large suggesting that there is a non-negligible unobserved study heterogeneity. In fact, the highly significant LR test confirms that σ_u^2 is statistically different from zero and, therefore, the multilevel model is preferable.

Figure 1 – Study random effects accompanied by their 90% CIs



Notes. The full sample comprises 43 studies; the restricted sample comprises 36 studies.

The sets of estimated study-level random effects are shown in Figure 1, separately for the full and restricted samples and for alternative specifications of the linear predictor.

While many of these effects lie close to the mean value of zero and are surrounded by such a level of uncertainty that makes it difficult to establish whether they imply any substantial deviation from the overall intercept, the random effects associable to other studies are farther from the mean and, despite uncertainty, are very likely to entail deviations of either positive or negative sign.

Based on the previous point we will not display, in what follows, the results of the ordinary logit model, which are relegated in the Appendix together with other results documenting the process of model specification. In this regard, it should be pointed out that the only interaction that is advisable to introduce in the model is that between the type of programme and the type of outcome variable. In fact, the coefficients on all interactions that involve the timing of effects are never statistically different from zero.

Table 4 reports the full set of coefficients for each sample and model specification. The coefficients refer to log odds, therefore they cannot be directly interpreted as probability changes, although their sign and magnitude suggest what happens at the level of probability. Each marginal effect on the log odds requires to be commented keeping in mind what constitutes the configuration of covariates at the baseline (i.e. when they take the value of zero): the estimate is drawn from an R&D programme implemented in the late 2000s , providing repayable loans, representing the treatment effect estimated by means of a DID approach on an outcome that is observed well after treatment receipt and not directly affected by the programme, at the fictional sample size of zero; finally, it is contained in a study that did not appear on a scientific journal. With the full sample, this baseline estimate is based on survey data, while in the restricted sample it is inevitably based on administrative data and refers to a national programme targeted to all firms irrespective of their size.

Now, with respect to the baseline configuration, the marginal effect on log odds is always positive if the treatment effect estimate refers to an outcome that is likely to be directly affected by the programme. This is not a surprising result; if anything is surprising is the fact that about 70% of the estimates do not focus on this type of outcome. This positive effect attenuates when an investment, instead of an R&D support, is taken into account.

The publication status of a study does not affect the log odds and, therefore, it is not related to a higher probability of having significantly positive treatment effects. In the full sample, the marginal effect on log odds is positive if the estimate is based on an administrative data bases proving treatment receipt, instead of being self reported as it occurs in survey data.

Further elements emerge only in the restricted sample. Here we find some evidence (model R2) that, if our baseline observation shifts from a national to a regional level of government, an improvement of the log odds is possible. On the contrary, if it shifts from a programme with no participation restriction to another one that targets SMEs only, we have a negative marginal effect on the log odds.

As it can be seen in Table 4 (see also Table 5), the coefficient on the number of firms considered in the estimation is always very close to zero and statistically non significant. This is an important result, in that it shows that the probability of having significantly positive treatment effect estimates does not increase with sample size, as would be expected if there was any publication bias. This simple approach for evaluating if publication bias constitutes a serious threat in meta-analyses relying on discrete or ordinal response variables was put forward in Card et al. (2010).

An identical assessment must be conducted with respect to the opposite situation where the response is one if the treatment effect estimate is significantly negative. All coefficients obtainable on the number of firms in the presence of the full and restricted samples and by means of the alternative specifications illustrated above are summarised in Table 5. Looking at the figures we can conclude that 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.

Table 4. Coefficient estimates

	FULL SAMPLE		*RESTRICTED SAMPLE*	
	(F1) R.I. Logit (additive)	(F2) R.I. Logit (w/interaction)	(R1) R.I. Logit (additive)	(R2) R.I. Logit (w/interaction)
FIXED PART				
R&D (base)	0 (.)	0 (.)	0 (.)	0 (.)
bank credit	0.725 (1.114)	1.100 (1.190)	1.736 (1.537)	2.713 (1.670)
investments	0.737 (0.648)	0.983 (0.643)	1.273* (0.747)	1.741** (0.774)
national (base)			0 (.)	0 (.)
regional			1.140 (0.696)	1.181* (0.674)
targets all firms (base)			0 (.)	0 (.)
targets SMEs only			-1.331 (0.903)	-1.835* (0.980)
loan (base)	0 (.)	0 (.)	0 (.)	0 (.)
grant	0.117 (0.697)	0.103 (0.688)	-0.620 (0.878)	-0.672 (0.875)
tax credit	0.607 (1.144)	0.467 (1.121)	0.0988 (1.419)	-0.122 (1.408)
unspecified or mixed	1.859* (0.958)	1.937** (0.951)	2.134 (1.860)	2.523 (1.877)
other outcome (base)	0 (.)	0 (.)	0 (.)	0 (.)
directly affected outcome	1.296*** (0.352)	2.344*** (0.725)	1.311*** (0.373)	2.909*** (0.954)
N. of firms	-0.00000757 (0.0000202)	-0.00000809 (0.0000194)	-0.000000258 (0.0000217)	-0.00000168 (0.0000210)
DID (base)	0 (.)	0 (.)	0 (.)	0 (.)
RDD	1.151 (0.885)	1.369 (0.860)	1.372 (0.969)	1.753* (0.962)
matched DID	-0.242 (0.792)	-0.0331 (0.771)	-0.445 (0.923)	-0.248 (0.899)
matching	0.916 (0.729)	1.042 (0.711)	1.222 (0.919)	1.472 (0.905)
other method	0.329 (0.998)	0.502 (0.992)	0.357 (1.407)	0.629 (1.386)

Table 4. Coefficient estimates (continued)

	FULL SAMPLE		*RESTRICTED SAMPLE*	
	(F1) R.I. Logit (additive)	(F2) R.I. Logit (w/interaction)	(R1) R.I. Logit (additive)	(R2) R.I. Logit (w/interaction)
implemented in late 2000s (base)	0 (.)	0 (.)	0 (.)	0 (.)
implemented earlier	0.630 (0.841)	0.819 (0.829)	0.945 (0.951)	1.329 (0.968)
survey data (base)	0 (.)	0 (.)		
administrative data	1.859 (1.145)	2.591** (1.220)		
lagged estimate (base)	0 (.)	0 (.)	0 (.)	0 (.)
simultaneous estimate	-0.555 (0.340)	-0.502 (0.338)	-0.549 (0.364)	-0.510 (0.364)
appeared in other outlet (base)	0 (.)	0 (.)	0 (.)	0 (.)
published in journal	-0.571 (0.614)	-0.507 (0.592)	-0.103 (0.724)	-0.159 (0.704)
R&D # directly affected outcome (base)	0 (.)	0 (.)	0 (.)	0 (.)
bank credit # directly affected outcome		-1.145 (1.036)		-1.644 (1.211)
investments # directly affected outcome		-1.493* (0.843)		-2.057** (1.040)
Overall intercept	-3.154** (1.563)	-4.418** (1.716)	-2.088 (1.502)	-2.858* (1.513)
RANDOM PART				
σ_u^2	1.194 (0.557)	1.039 (0.517)	1.093 (0.594)	0.957 (0.548)
LR test vs. logistic regression	25.72***	17.90***	14.62***	11.71***
Observations	478	478	430	430
Studies	43	43	36	36
AIC	543.6	544.4	486.1	485.8
Log likelihood	-254.8	-253.2	-225.1	-222.9

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

Table 5. 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*
Ordinary Logit (additive)	-0.0000046	-0.0000073	0.0000123	-0.0000160
	(0.0000151)	(0.0000147)	(0.0000168)	(0.0000254)
R.I. Logit (additive)	-0.0000076	-0.0000003	0.0000123	-0.0000160
	(0.0000202)	(0.0000217)	(0.0000231)	(0.0000285)
R.I. Logit (w/interaction)	-0.0000081	-0.0000017	0.0000140	-0.0000148
	(0.0000194)	(0.0000210)	(0.0000237)	(0.0000286)

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

Rather than continue to comment on coefficients that need to be transformed in a nonlinear fashion in order to return probabilities, it is convenient to consider Table 6, which presents the average adjusted probability predictions for the covariates of major interest. While fitting these values, we neutralise the influence of unobserved study heterogeneity by fixing all random effects at their mean value of zero. Average adjusted predictions can be interpreted as the level that the probability of success takes on average as a consequence of a one-unit shift in a given covariate, all the other covariates left at the level that is actually observed in the data (Williams, 2012). They provide us with an idea on the association of each predictor, considered in isolation, with the probability of success.

In the table, probability predictions are accompanied by their 90% confidence intervals (standard errors and confidence intervals are computed using the Delta method), which enable us to evaluate not only whether these probabilities are significantly positive, but also to what extent they are different from one another. The rule is that two probabilities are statistically different at a given level of confidence if their intervals do not overlap. Again, we find that the type of outcome considered matters and that estimates referred to programmes targeted to all firms tend to enjoy a higher probability of being significantly positive. We also observe a probability gap with respect to the level of government delivering the programme in favour of the regional level, although the mean difference very slightly misses the 90% significance requirement. Finally, we may add that different programme types are not associated with systematically different probabilities of success.

Table 6. Conditional average adjusted probability predictions for selected covariates; random effects fixed at their mean value of zero

	FULL SAMPLE		*RESTRICTED SAMPLE*	
	(F2) R.I. Logit	(F3) R.I. Logit w/interaction	(R1) R.I. Logit	(R2) R.I. Logit w/interaction
R&D	0.347 [0.218,0.477]	0.368 [0.251,0.484]	0.290 [0.137,0.442]	0.317 [0.172,0.462]
bank credit	0.500 [0.143,0.858]	0.526 [0.156,0.895]	0.653 [0.239,1.067]	0.760 [0.422,1.097]
investments	0.503 [0.330,0.676]	0.477 [0.301,0.653]	0.556 [0.373,0.738]	0.555 [0.385,0.726]
national			0.297 [0.171,0.424]	0.281 [0.166,0.397]
regional			0.513 [0.381,0.644]	0.501 [0.372,0.630]
targets all firms			0.461 [0.350,0.572]	0.451 [0.349,0.553]
targets SMEs only			0.229 [0.0621,0.397]	0.176 [0.0527,0.299]
loan	0.356 [0.172,0.541]	0.344 [0.164,0.524]	0.494 [0.233,0.755]	0.483 [0.227,0.740]
grant	0.379 [0.268,0.490]	0.364 [0.257,0.471]	0.367 [0.255,0.479]	0.349 [0.243,0.455]
tax credit	0.479 [0.141,0.818]	0.438 [0.114,0.762]	0.515 [0.129,0.900]	0.458 [0.0903,0.826]
unspecified or mixed	0.730 [0.489,0.970]	0.733 [0.499,0.967]	0.862 [0.572,1.152]	0.891 [0.657,1.125]
other outcome	0.328 [0.235,0.420]	0.309 [0.223,0.395]	0.349 [0.244,0.455]	0.339 [0.241,0.437]
directly affected outcome	0.618 [0.479,0.757]	0.679 [0.548,0.810]	0.643 [0.486,0.800]	0.732 [0.604,0.861]
lagged estimate	0.486 [0.368,0.603]	0.461 [0.347,0.576]	0.499 [0.358,0.640]	0.473 [0.338,0.608]
simultaneous estimate	0.368 [0.267,0.468]	0.356 [0.261,0.451]	0.381 [0.270,0.493]	0.366 [0.262,0.471]
R&D # other outcome		0.226 [0.118,0.333]		0.196 [0.0920,0.300]
R&D # directly affected outcome		0.718 [0.500,0.935]		0.733 [0.475,0.991]
bank credit # other outcome		0.446 [0.0425,0.850]		0.700 [0.309,1.091]
bank credit # directly affected outcome		0.709 [0.401,1.018]		0.875 [0.685,1.065]
investments # other outcome		0.420 [0.243,0.596]		0.511 [0.342,0.680]
investments # directly affected outcome		0.613 [0.402,0.824]		0.678 [0.498,0.857]

Notes. 90% confidence intervals in brackets.

6. The probability of 'success' of typical enterprise and innovation policy schemes

Based on the models previously estimated on the restricted sample, in particular on the most complete specification R2, it may be interesting to go beyond considering covariates as if they were isolated and conclude our discussion showing the predicted probability of success associated with particular combinations of covariates, corresponding to some of the most common schemes in the area of enterprise support and innovation policies: i) an R&D-grant scheme targeted to all firms irrespective of their size; ii) a scheme providing SMEs with public guarantees on their bank loans; iii) an investment-grant scheme for all firms. By 'success' we mean, again, a significantly positive treatment effect. Additional aspects worth to emphasise refer to the government level delivering the programme and to whether the effect of the programme is measured in terms of direct outcomes (such as R&D or investment expenses) or indirect results (such as employment or sales growth).

The results of this final exercise illustrate how this meta-analysis can provide useful insights to the debate on industrial policies. They are shown in Table 7.

Table 7. Conditional average adjusted probability predictions for the most common policy schemes; random effects fixed at their mean value of zero

Policy scheme	*(A)* *whatever level*	*(B)* *national level*	*(C)* *regional level*	*(C - B)* *difference*
DIRECTLY AFFECTED OUTCOME				
RD grant for all firms	0.732^{***} (0.070)	0.596^{**} (0.232)	0.813^{***} (0.083)	0.217 (0.145)
Guaranteed loan for SMEs only	0.715^{***} (0.161)	0.575^{***} (0.215)	0.799^{***} (0.145)	0.224 (0.139)
Investment grant for all firms	0.675^{***} (0.112)	0.527^{***} (0.146)	0.764^{***} (0.116)	0.238^{*} (0.131)
OTHER OUTCOME				
RD grant for all firms	0.188^{***} (0.061)	0.100^{*} (0.056)	0.245^{***} (0.083)	0.145^{*} (0.080)
Guaranteed loan for SMEs only	0.461^{**} (0.214)	0.309 (0.203)	0.557^{**} (0.233)	0.248^{*} (0.137)
Investment grant for all firms	0.501^{***} (0.105)	0.346^{***} (0.115)	0.599^{***} (0.126)	0.253^{*} (0.137)

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

In all the three schemes the probability of positive treatment effects is statistically significant, and tends to be particularly high on the outcomes that programmes are directly trying to modify. This occurs irrespective of the level of government delivering the programme (column *A*) but also accounting for this characteristic (columns *B* and *C*).

The probability of positive treatment effects associated with regional programmes is remarkably higher than that of national programmes in our sample, whatever scheme and outcome are considered. Besides being positive, the difference in probability ($C - B$) is often, although not always statistically, different from zero.

In particular, when the effect is measured on outcomes that the programmes are intended to directly modify, the difference in probability between government levels is surely positive for investment-grant schemes, while in the other two cases slightly misses standard significance requirements. If other outcomes are taken into account, the difference in favour of the regional level of government is always significantly positive. This kind of evidence leads to conclude that if there is any difference between government levels, this difference tends to be against the national level.

7. Concluding remarks

By considering 43 micro-evaluation studies providing 478 estimates of the effects exerted by public incentives on business investment activities, this paper represents the first attempt to perform a Meta-Regression Analysis of Italian enterprise policies. We take into account different policies aimed at supporting R&D activities, tangible and intangible investments, and the receipt of bank loans. Due to such heterogeneity of policies and outcomes, the type of treatment effect we take as our response variable is given by a binary variable equal to one when the public support determined a significantly positive effect (as opposed to both insignificant and negative ones).

We find that the occurrence of positive effects is not affected by the number of firms considered in the empirical analysis as well as by whether the study was published in a journal: accordingly, there is no reason to believe that publication bias affects our estimates. In general, probability of success seems to be somewhat higher when the policy measure is not targeted on SMEs only. The most striking finding of our meta-analysis is that a positive effect of the policy is more likely to emerge when the measured outcome is directly targeted or immediately affected by the policy measure. Indeed, depending on the type of programme, the occurrence of positive treatment effects increases when the outcome variables refer to R&D expenditures or R&D employees, amount of capital investment, receipt of favourable bank loans or lower interest rates, rather than to other indicators of firm performance. This finding is not surprising. In fact, although effects on the latter type of outcomes are often hoped for by policymakers, they may emerge only after a rather uncertain chain of events, which is difficult to assess.

With respect to some common policy schemes, our findings show their probability of success is non-negligible, especially if evaluated on outcomes that are directly targeted or immediately affected by the schemes themselves. If there exist any differential in probability of success between the government levels that may deliver the programmes, this differential is favourable to regional governments. As a possible explanation for this result, it can be argued that regional policymakers, being particularly aware of the specific features and behaviour of local firms, are able to design and implement more effective policy measures than their national counterparts. In addition to that, however, is should be recalled that the studies on regional programmes considered in our analysis mostly refer to northern and central Italian regions, which, according to European standards, enjoy a decent quality of government and administration (see Rodríguez-Pose and Garcilazo, 2015).

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