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REAL 15-T-6 August, 2015

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Abstract: This paper offers a meta-analysis of the controversial impact of EU structural funds on the growth of the recipient regions. It identifies the factors that explain the heterogeneity in the size of 323 estimates of their impact recorded in 17 econometric studies. Differences are found to come from several data characteristics, from controlling for the endogeneity of the funds and from the presence of some regressors in the primary studies. We complement this approach by an ordered probit model to uncover the factors that affect the probability of estimating a significantly positive return of the funds.

Keywords: meta-analysis, hierarchical model, multivariate regression, regional growth, structural funds, public policy

JEL codes: R58, O47, C30

Section 1- Introduction

In the European Union, every programming period sees around one-third of the budget devoted to various regional cohesion policies. Since their implementation in the 1970's, a large set of studies and reports focus on measuring their impact on the economy of the recipient localities, regions, and countries. They are selected because of their low levels of relative per capita GDP, high unemployment rate, low density, and recessive industry. While some contributions in the academic literature are generally supportive of the continuation of such policies (e.g. CAPPELEN *et al*., 2003; ESPOSTI and BUSSOLETTI, 2008; BEUGELSDIJK and EIJFFINGER, 2005; GARCIA-SOLANES and MARÍA-DOLORES, 2001), others cast doubts about their actual efficacy (e.g. DALL’ERBA and LE GALLO, 2008; and some estimates of DALL'ERBA and LE GALLO, 2007, PUIGCERVER-PEÑALVER, 2007, and of BOUAYAD-AGHA *et al.*, 2011), highlight their conditional efficacy (e.g. EDERVEEN *et al*., 2002, 2006; RODRIGUEZ-POSE and FRATESI, 2004; DALL'ERBA and LE GALLO, 2007; ESPOSTI and BUSSOLETTI, 2008), or conclude that they act negatively on growth (FAGERBERG and VERSPAGEN, 1996; and some estimates of PUIGCERVER-PEÑALVER, 2007, and of BOUAYAD-AGHA *et al*., 2011). In our opinion, understanding what factors explain the differences in the estimated impact of regional policies and whether actual practical changes can be implemented is especially important now that sluggish economic growth among European Union members and recent rounds of bailouts have undermined the availability of public funding for regional cohesion purposes.

This paper relies on published and unpublished literature that econometrically estimates the regional growth impact of structural funds, the most important tool of EU cohesion policies, and identifies the sources of heterogeneity in the estimated impact. The focus is solely on econometric studies because it is impossible to combine the outcomes of individual case studies (such as VENABLES and GASIOREK, 1999, who focus on the Tagus bridge in Lisbon, Portugal) since they are too specific to the project under investigation. The results of model simulations (e.g. PEREIRA, 1994; BEUTEL, 1995) are disregarded also because there are too few of them, and they exclusively focus on four peripheral countries. Many other papers are not considered because they do not rely on a sufficiently homogenous definition of the funds (e.g. they use proxy or dummies), the focus is on another dependent variable, or another source of incomparability that will be described further below. As a result, our meta-database is composed of 17 manuscripts offering 323 estimates in total.

The decision to use the meta-analysis framework, first introduced by GLASS (1976), is due to its capacity to combine the results of several existing studies and summarize their outcome. In addition, it controls for differences/similarities within and between studies and identifies whether the former come from sampling (e.g. size and time period of the sample) or non-sampling (e.g. estimation process and regressors used) characteristics. This process takes place in the frame of a meta-regression which measures the role of the study characteristics by explaining the differences among study outcomes. As mentioned in DE DOMINICIS *et al*. (2008), it allows a more complete picture of an existing literature than traditional qualitative or narrative approaches.

Meta-analysis started in the field of medical sciences (LIPSEY and WILSON, 1993; SHAPIRO and SHAPIRO, 1982; SMITH and GLASS, 1977), but was adopted fairly early in social sciences, education (KULIK *et al*.,1980; BERNARD *et al*., 2004; SOSA *et al*., 2011), and economics (STANLEY, 1998, 2004; CARD and KRUEGER, 1995; VAN DEN BERGH *et al*., 1997; ABREU *et al*., 2005; DOBSON *et al.,* 2006; BANZHAF and

SMITH, 2007). It has also been used in the unpublished document of [deleted to maintain anonymity in review process] which discusses the same topic of structural funds and regional growth. However, the current study differs from the previous study in a number of important ways. First, the previous work relies on 218 estimates because of the smaller number of studies (12) that were available at the time. Second, some of the key moderators used here to explain the heterogeneity among the estimated impact of the funds are not considered in their study. Third, they do not account for dependence between effect sizes coming from the same study.

The remainder of this paper is organized as follows: section 2 begins with a short description of the theoretical backgrounds commonly used to measure the impact of EU structural funds on regional growth. It continues with a description of some of the econometric challenges and their solution associated to this literature. Section 3 reports the way the primary estimates have been collected from the existing literature and explores whether they display the presence of heterogeneity and of a publication bias. Section 4 presents the meta-regression models as well as the moderators used to explain the variance in the collected effect sizes. Section 5 presents the estimation results and discusses the factors that significantly affect the magnitude of the estimated impact of the funds. We complement them with an ordered probit model to uncover the factors that influence the probability of estimating a significantly positive return of the funds. Finally, section 6 presents the conclusions.

Section 2–Growth theories and econometric methods

2.1. Theories

The predicted impact of European structural funds relies upon economic growth theory. Three approaches are commonly used to understand the role of public investments in stimulating growth. The traditional approach is the neoclassical growth framework that relies on the assumptions of decreasing returns to capital and constant and exogenous rate of technological progress. Structural funds correspond to public investments allocated to a capital scarce region, hence they increase the growth rate of the recipient area which experiences faster convergence towards its steady-state level but for a short period of time only (SOLOW, 1956; SWAN, 1956). The growth rate does not change in the long-run due to the decreasing nature of the returns to capital. This holds true with investments in human capital as well (MANKIW *et al.*, 1992).In a neoclassical setting, only changes in the exogenous rate of technological progress modify the steady-state growth rate. The hypothesis of decreasing marginal return to capital is not reconcilable with the assumptions of the second strand of the literature, namely the endogenous growth theory. Based on the assumptions of constant returns to capital (at the regional level), endogenous technological progress and local externalities, endogenous growth models assume that new investments in public capital increase the marginal product of private capital. This fosters capital accumulation and growth in the recipient region in the long-run (ROMER, 1986, 1990; BARRO, 1990; ASCHAUER, 1989). However, the empirical paradox pinpointed by JONES (1995a, 1995b) according to which total factor productivity remains constant in spite of new expenditure in R&D and human capital has given birth to the semi-endogenous growth theory (JONES, 1995b; SEGERSTROM, 1998). Based on the idea of decreasing returns to scale in the production of knowledge, these models assume that total factor productivity growth depends on the exogenous growth rate of the population because it determines the R&D employment growth rate.

Neither the neoclassical northe endogenous growth theories are specific enough about the type of public capital that is funded, yet the largest share of structural funds (around 1/3) finances transportation infrastructures. They reduce transportation costs, hence they have consequences on the economic growth of the recipient regions in ways that cannot be captured in any of the previous growth theories. As such, we turn to the third strand of economic growth theory, namely the new economic geography (KRUGMAN, 1991; FUJITA *et al*., 1999). In this growth theory, new transportation infrastructures lead to different degrees of improvement in accessibility and economic development in the region(s) where they are implemented (VICKERMAN 1996; BOARNET, 1998). When new (interregional) transportation infrastructures connect regions of different levels of income, companies and workers may delocate from the poor region to the rich one to benefit from agglomeration economies (MARSHALL, 1890; KRUGMAN, 1991).This process can be self-reinforcing when the presence of localized technology spillovers is conducive to growth as indicated in the models of BALDWIN and MARTIN (2004) and BALDWIN *et al*. (2004) who combine new economic geography and endogenous growth theories. Yet, interregional transportation infrastructures are more often the rule than the exception in the European highway and railway networks (VICKERMAN *et al*., 1999). As a result, while interregional transportation infrastructures increase the accessibility of several regions, their gains will always be relatively higher in the richest one (VICKERMAN *et al*., 1999).

2.2. Econometric methods

In spite of these three strands of economic growth theory, the empirical literature relies almost exclusively on the neoclassical beta-convergence model à la BARRO AND SALA-I-MARTIN (1991) to econometrically assess the impact of structural funds on per capita income growth. This feature is an advantage in a meta-analysis as it makes the estimates of the primary studies homogeneous conceptually. Specifically, the (cross-section) model most commonly used in the literature to measure the elasticity of the funds derives from the beta-convergence model specified in MANKIW *et al.* (1992, p. 423) but with variations in the number and specification of the regressors:

$$\frac{1}{T-t0}(\ln(y_T)-\ln(y_{t0}))=g=\alpha\iota_n+\ \beta_0\ln(y_{t0})+\ \beta_1\ln(s)+\ \beta_2\ln(n+g+\delta)+$$
$$X\,\beta_3+\ \beta_4 SF+\varepsilon with\ \varepsilon\sim N(0,\sigma_\varepsilon^2 I_n)(1)$$

where the dependent variable (g) is the annual growth rate of per capita GDP in region *i* over the period t0 – T,y_{t0} is the initial level of per capita GDP, *s*is the average gross domestic savings rate, *n* is the population growth rate, *g* is the exogenous rate of technological progress, δ is the rate of depreciation, *X* is a matrix of additional variables that maintain the steady state of each economy constant and *SF* stands for the structural funds. ε is the error term with the usual properties. Most studies report a negative and significant estimate of β_0 which validates the convergence assumption brought to the fore by the neoclassical growth model. In the current paper, wefocus on the effect size of the average annual growth rate with respect to structural funds, i.e. the coefficient β_4. As described in section 3, our meta-analysis is based on compiling comparable measurements of this coefficient (STANLEY, 2001) which reflects the level of efficiency of the funds in promoting growth in the recipient areas.

Note that one gets a different marginal effect when an interaction term is added to specification (1). For instance, when EDERVEEN *et al.* (2006) evaluate whether the funds are *conditionally* effective on the quality of the institutions that rule the recipient region, they add a term such as $\beta_5 SF * institutions$ to the regressors of equation (1). The marginal effect then becomes $\partial g/\partial SF = \beta_4 + \beta_5 institutions$. In this situation, we measure the effect at the mean of the interacted term when possible (e.g. the mean of '*institutions*' in the case above).

While most of thestudies measure the variables in the matrix X at the initial time period to prevent endogeneity, the funds are sometimes measured over the growth period. This leads to a problem of reverse causality as the funds are, in part, allocated based on past relative levels of regional per capita income (DALL'ERBA and LE GALLO, 2008). This problem has also been highlighted recently by BRÜCKNER (2013) in an international context. The more recent studies in our database have dealt with this issue by usingpast levels of structural funds (such as MOHL and HAGEN, 2010), other instrumental variables (such as in DALL'ERBA and LE GALLO, 2008;) or ARELLANO and BOND's (1991) estimator (ESPOSTI and BUSSOLETTI, 2008). Differences in the treatment of endogeneity among primary studies will be treated in section 5 of this paper.

As access to structural funds data has become more available, several authors have decided to assess the impact of the funds in the frame of a panel data model. Such a specification provides them with more information and data variability. This allows control over unobserved heterogeneity and reduces problems of collinearity among the expanatory variables. The literature uses standard methods for panel data estimation:

fixed effect modelas well as the GMM estimator developed by ARELLANO and BOND (1991). No panel-data study uses a random effect approach which, in the frame of a neoclassical growth model, implies that the individual effects are correlated with some regressors. This would lead to endogeneity (ISLAM, 2003; ABREU *et al*., 2005; ESPOSTI and BUSSOLETTI, 2008).

Increasing interest in new economic geography (KRUGMAN, 1991; FUJITA *et al*., 1999) and advances in the field of spatial econometrics (ANSELIN, 1988; LE SAGE and PACE, 2009), have led to four studies which have investigated the impact of the funds, not only on the growth rate of the targeted regions, but also on the one of their neighbors (DALL'ERBA and LE GALLO, 2007, 2008; BOUAYAD-AGHA *et al*., 2011; MOHL and HAGEN, 2010). It allows them to proxy for interregional backward and forward linkages, technology spillovers, commuting across regions, and to refute the traditional assumption of independence of the error terms (CRESPO CUARESMA and FELDKIRCHER, 2013).

Section 3-Primary studies

The collection process of the primary studies was performed to avoid missing any relevant empirical estimates. The goal is to reduce the potential biases due to any nonrandom selection. The following sampling criteria were used to search for all relevant estimates. First, we searched the Economic Literature Index (EconLit), ISI web of Knowledge and Google search for any reference on 'European growth', 'structural fund', 'European regional policy', and'European regional cohesion'. Our search led to a very large number of results.We selected the studies written in English[1] and eliminated those that use proxies for structural funds such as public (national or regional) investments in education or in transportation. Their amount and allocation criteria are not based on European directives, sothey poorly reflect European regional development efforts. We also eliminated the studies that use a dependent variable other than per capita income growth, such as KYRIACOU and ROCA-SAGALES (2012), who measure the effect on regional disparities within countries, or MARTIN and TYLER (2006), who focus on job creation; use a theoretical framework that is not the neoclassical growth model (DALL'ERBA *et al.*, 2009, and VARGA and VELD, 2011, estimate an endogenous growth model) or rely on a different modeling framework such as the stochastic frontier methodology used in GOMEZ-GARCIA et al. (2012) or the generalized propensity score estimation of BECKER et al. (2012). We also removed studies such as BECKER *et al.* (2010), RAMAJO *et al*. (2008), LLUSSÁ and MÁRIO LOPES (2010), and some measurements in ESPOSTI (2007) because they do not use the actual amounts of structural funds among their regressors[2]. We also disregarded the estimates of FAYOLLE

[1]As noticed by an anonymous referee, studies written in other European languages exist. The limitation to papers written in English is only due to the capacity of the authors to extract information from such primary studies.

[2] They use a binary variable for recipients vs. non-recipients (BECKER *et al.*, 2010; ESPOSTI, 2007) or use different growth regressions by eligibility status (RAMAJO *et al.*, 2008; LLUSSÁ and MÁRIO LOPES, 2010). Here, we keep the measurements of ESPOSTI (2007) based on the actual allocation of the funds only. The latter contribution

and LECUYER (2000) and some estimates in PUIGCERVER-PEÑALVER (2007) because they are based on the regional allocation of the funds relatively to the Community average. Similarly, the work of GARCIA-SOLANES and MARÍA-DOLORES (2001) is excluded as regional growth is measured relatively to the sample average. Note also that econometric studies providing local estimates, as LE GALLO *et al*. (2011), or focusing on the regions of one country only (e.g. PERCOCO, 2005) could not be considered since all the other studies measure the overall impact on the sample of EU regions.

After screening the list of relevant articles according to the above criteria, we also individually checked the studies they refer to in their bibliography. Working papers that have led to a publication have naturally been removed from our sample to avoid double-counting. It leads to a meta-database composed of 17 studies. The first transformation needed to make them even more comparable consists in adjusting the estimates so that they all report the marginal effect on the *yearly* growth rate. Only two studies relying on a five-year growth rate had to be adjusted as such. Secondly, the functional form is an important element to consider as it leads to a different interpretation of the marginal effect of the funds. While most of the studies rely on a linear model or on a log-log model (9 and 5 articles respectively), 2 articles use a log-lin model and 1 uses a lin-log model. The latter two cases report few estimates and the semi-elasticity they represent ($(\Delta Y/Y)/\Delta X$) or $\Delta Y/(\Delta X/X)$) can be transformed to an elasticity ($(\Delta Y/Y)/(\Delta X/X)$) when the average value of X (for log-lin) or of Y (for lin-log) is reported in the article.

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demonstrates clearly that using a dummy variable or actual expenses leads to different results.

This process guaranteesthe completeness, homogeneity and comparability of the population under investigation, i.e., 323 estimates of the impact of structural funds on regional growth. Among them, 77 are marginal effects based on an interaction termzsuch as $\partial g/\partial SF = \beta_4 + \beta_5 z$. Since the primary studies report the measurement of the mean of the interacted term z in 65 cases, the total effect evaluated at the mean is β_4 in 258 cases and it is $\beta_4 + \beta_5 z$ in 65 cases.

Table1 illustrates the composition of our meta-sample and reports the year of publication, publication status, number of observations in each study, andthe functional form used. We also report some basic statistics on the estimates (minimum, maximum, mean, and standard deviation) as well as the percentage of estimates in each of the following three categories: positive and significant, non-significant, negative and significant. We note that the majority is published work (14 out of 20), and relies on a panel approach (13 out of 17). Overall, the estimates range from -7.586 to 6.294, have an average value of 0.174, and are mostly non-significant (71.5% of the estimates). Among the significant estimates, the positive ones are almost twice as numerous as the negative ones (18.3% vs. 10.2%).

[Table 1 here]

We complement these basic statistics with a Cochran's Q-test. It allows us to formally detect the significant presence of heterogeneity among primary observations (BERLIN *et al*., 1989). The null hypothesis is that all studies share a common grand mean effect size. The test statistic is given by:

$$Q = \sum_{i=1}^{k} w_i T_i^2 - \frac{(\sum_{i=1}^{k} w_i T_i)^2}{\sum_{i=1}^{k} w_i},(2)$$

where $w_i = 1/v_i$ is the reciprocal of the estimated variance $v_i$for the effective size T_i of the i^{th} subject (this information comes from the primary studies) and Q follows a χ^2 distribution with k-1 degrees of freedom. If the calculated Q value is greater than the pre-set critical value, we reject the null hypothesis of homogeneity.

The result for the overall sample indicates that the primary studies report heterogeneous effect sizes as Q=1186 with p-value=0.000. Several factors are potential sources of this heterogeneity. Since table 1 indicates that the functional forms differ across studies, we decide to run the same statistic but on the sub-sample of the 165 lin-lin estimates and of the 147 log-log estimates[3]. The results are Q= 593 (p-val. = 0.000) and Q= 597 (p-val. = 0.000) respectively. It indicates that even the estimates sharing the same functional forms are heterogenous.

A second possible source of heterogeneity is the potential publication bias of the primary studies (ROTHSTEIN *et al*., 2005). If a bias is present, it is reflected in the results of the meta-analysis. It is particularly relevant in a sample like ours that combines published and unpublished manuscripts and it could also be present within each of the two groups. The bias comes from the researchers, the editor, or the reviewers of the journal who decide not to submit or publish an article because of unfavorable results or their significance level (GREENWALD, 1975). DAVIS (1971) and DICKERSIN (2006) also speak about rejection when empirical results contradict theory, the intellectual position of the editor/reviewers, or well-established knowledge.

In order to formally test the presence of a publication bias, we use the 'test of funnel

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[3] 11 semi-elasticities are not included as the primary studies do not report the average value of X or of Y.

asymmetry' developed by EGGER *et al.* (1997). It consists in running a linear regression between the ratio 'effect size/standard error'and'1/standard error', and then testing whether the intercept of the regression line differs significantly from zero. A significant intercept indicates the presence of a publication bias. The estimated intercept for the meta-sample is 0.506, with a p-value = 0.000, indicating the significant presence of a publication bias. When applied to the two sub-samples defined by functional form, the results are 0.453 (p-val. = 0.081) and 0.736 (p-val. = 0.000) for the lin-lin and log-log estimates. As such, only the linear estimates indicate the absence of a publication bias.

In order to explore further the sources of heterogeneity in the sign and magnitude of the effect sizes while accounting for the variation that is observed and can be collected easily from the primary studies, the next sections offer a multivariate meta-regression analysis.

Section 4-Fixed-effects model, mixed-effects model and hierarchical model

The fixed effects and mixed effects regression models are commonly used in meta-analysis to control for the heterogeneity in the primary estimates. The fixed effects model assumes that the variability among the effect sizes can be fully explained by a set of moderator variables that account for differences in the characteristics across study *i*:

$T_i = \beta_0 + \beta_1 x_{i,1} + \cdots + \beta_k x_{i,k} + \varepsilon_i$ with $\varepsilon_i \sim N(0, \mathrm{v}_i)$(2)

Where x_1 ... x_k are the study characteristics, β_1 ... β_k are the regression coefficients, ε_i is the error term andv_i is the estimated variance of the effect sizes collected from the primary studies, *i* = 1,2,…,k refers to the indices for the estimated effect sizes.

In the mixed effect model the variability beyond the sampling error is derived partly from a systematic factor, as in the fixed effect model, and partly from random sources:

$T_i = \beta_0 + \beta_1 x_{i,1} + \cdots + \beta_k x_{i,k} + \mu_i + \varepsilon_i$ with $\varepsilon_i \sim N(0, \mathrm{v}_i)$ and $\mu_i \sim N(0, \tau^2)$(3)

Both the fixed-effects and the mixed-effects models allow the true effect size and its precision to vary across regressions in the primary studies. However, the mixed effects model also assumes that not all heterogeneity is observable. It allows for the presence of residual heterogeneity by assuming that the underlying effects follow a normal distribution around the effects predicted by the covariates (SUTTON *et al*., 2000).

One potential drawback of the above models is their assumption that the estimated effect sizes are independently distributed no matter whether they come from the same or different studies. The traditional assumption of independence can be violated when two (or more) effect size estimates come from the same study. This means they are based on the same sample of data, which introduces dependence at the sampling level (STEVENS and TAYLOR, 2009). The sampling dependence can be accounted for by appropriate estimation of the sampling covariance matrix (GLESER and OLKIN, 1994). In our case, the 323 observations in our meta-analysis database are not from 323 independent studies, but are all nested within 17 studies. In order to verify if accounting for this type of dependence modifies our conclusions, we complement the above models with a two-level hierarchical model that considers first the within-study variation and second the between-study variation (GOLDSTEIN, 2003; RAUDENBUSH and BYRK, 2001).

Following the notation used by DOMINICIS *et al.* (2008), the two-level hierarchical model is:

$T_{ij} = \beta_{0j} + \beta_1 x_{i,1} + \cdots + \beta_k x_{i,k} + \varepsilon_{ij}$,

$\beta_{0j} = \beta_0 + \mu_j$, with $\varepsilon_{ij} \sim N(0, \mathrm{v}_i)$ and $\mu_j \sim N(0, \tau^2)$, (4)

where i is the individual observations nested in study j, ε_{ij} represents the error term at measurement level, v_i is the estimated variance of the effect sizes from the collected studies, μ_j is the error term at the study level shared by all measurements within the same study.

As in DOBSON *et al.* (2006), we find that it would be impossible to take into account all the conditioning variables used in the primary studies given the limited size of our sample and that several of them can be found in some individual studies only. As a result, we focus below on the most commonly used conditioning variables and use additional dummies to capture differences in study design (data and estimation characteristics). Controlling for all sources of heterogeneity is anyway unnecessary as it would only capture study differences that are already taken into account in the study fixed effects of the hierarchical model.

We decide to group the moderators (regressors) we include in every regression into three classes. The first class concerns the *data characteristics,* which include information about:

-the publication status (published or unpublished) as the Q statistics above suggest it may be a source of heterogeneity and several contributions have already indicated that the magnitude and precision of the estimates are correlated with the publication status (GREENWALD, 1975; EGGER *et al.*, 1997).

- the degree of freedom.

- the area of study (more or less than EU12) as studies performed on a sample that excludes the Southern and East European countries generally conclude to a greater degree of cohesion and efficiency of the funds.

-the type of spatial unit used (country vs. regions) as it is well-known the spatial scale used for the analysis influences the conclusions (OPENSHAW and TAYLOR, 1979)

- the definition of the funds (fund/GDP vs. other) in order to differentiate the ways the primary studies normalize the allocation of the funds.

- the functional form used (linear, semi-elasticity vs. elasticity) as the three forms are found in the primary studies and we are especially interested in figuring out if there is a significant difference between estimates based on linear vs. log-log models. These two functional forms constitute the bulk of the estimates (see table 1).

-whether the funds are for objective 1 regions as historically the largest share of structural funds has been allocated to so-called objective 1 regions selected upon their level of per capita GDP being below 75% of the European average.

- the time lag between the average allocation of the funds and the average of the growth period as several primary studies use a lag to remove potential problems of simultaneous causation and recognize that public investments do not act instantaneously on growth.

- the number of years included in the allocation of the funds. Studies based on an average of several years are less sensitive to the cyclical effect of each year's allocation.

- initial year of the growth period (pre- vs. post-1994). It allows us to test the existence of a structural break in the capacity of the funds to promote growth. 1994 is chosen as it corresponds to the beginning of the 1994-1999 programming period during which more than 2.5 times the previous (1989-1993) level of funds was allocated.

- whether the study was written/published before or after the median year (2007) of our sample. This variable allows us to test whether more recent studies benefit from the experience built in the past literature. For instance, more recent studies pay a much greater degree of attention to issues of endogeneity of the funds and spatial autocorrelation than earlier studies. If not controlled for, both issues affect the magnitude and precision of the estimates.

The second class of moderators concerns the *estimation characteristics*, that is information on the estimation methods. Here, we distinguish the least squares methods (OLS, GLS, LSDV) from the others (ML, GMM, 2SLS). While OLS and ML are equivalent in most simple regressions, they are not equivalent in the presence of spatial autocorrelation. This means ML is not part of the reference group. The other two moderators in this class indicate whether instruments (IV) were used to account for the endogeneity of the funds and whether a fixed effect approach was used. As mentioned in section 2, panel data studies cannot use a random effect approach in a neoclassical growth model. Finally, we measure the role of controlling for spatial dependence as it is increasingly recognized that the funds have effects beyond the boundaries of the recipient areas. It is a dummy with value 1 when the presence of externalities and feedback effects has been accounted for by spatial econometric means in the primary study.

1. The third class of moderators refers to the *presence of regressors* other than structural funds. The estimated effectiveness of the funds is also conditional upon such characteristics in the primary studies (EDERVEEN *et al*., 2002, 2006; RODRIGUEZ-POSE and FRATESI, 2004; ESPOSTI and BUSSOLETTI, 2008). They include the presence/absence of a national dummy variable, of the initial per capita GDP, of variables capturing the characteristics of the economic structure (e.g. share of workers in agriculture), employment or population, public investments or infrastructure stock, human capital or investments in education or research and development, corruption/institutional quality and the presence of an interaction term.

In essence, our results will suggest that the use of the above data characteristics, estimation characteristics and moderators produce smaller/greater estimates of β_4 on average in the primary studies. Except for the few continuous variables we use, our estimates can also be understood as measuring the bias that exist from excluding the associated control or choosing the alternative (in parenthesis in table 2) in the primary study.

Note that we are aware that the interpretation of some of the above dummy variables is not necessarily the same for different studies. For example, which country- or region-specific characteristics are captured by ‘Fixed effects’ depends on which other regressors are already included in the primary study. Similarly, the type of IV used is conditional upon other existing regressors. However, it is impossible to account for such a large degree of heterogeneity across primary studies without compromising the degree of freedom and the quality of our estimates.

Section 5. Meta-regression results

Table 2 presents the results of the regressions for the fixed effect model (column 1) and the mixed effect model (column 2) where the 323 estimates are considered independent and for the hierarchical linear model where they are not (column 3). Indeed, the study fixed effects included in the latter model controls for differences across studies.

[table 2 here]

The magnitude, sign and precision level of our estimates are comparable across all three models. The results indicate that the first significant moderator is ‘publish’. It is a dummy variable that takes 1 when the primary study is published and 0 if not. The coefficient indicates that, on average, published studies report an impact that is lower than unpublished studies. This result corroborates the publication bias found earlier for our overall sample. The second significant moderator is 'area of study'. It is a dummy variable that takes the value 1 when the area of study is less than EU12 and 0 if not. The coefficient indicates that, on average, the impact of the funds on growth is greater in samples considering 'less than EU12' countries than in samples based on 'EU12 or more' countries[4]. This result is not surprising considering that the poor regions of the Southern countries that enlarged the European Union from 9 to 12 members consumed a large share of the structural funds, yet they did not necessarily catch-up with their average national income or with the European average (DALL'ERBA and HEWINGS, 2009). While we do not find any significant difference between studies performed at the country or regional level, there is one between estimates based on the funds/GDP vs. any other form of normalization (funds/population or just funds). The former leads to estimates that are slightly higher on average.

We do not find any significant difference due to the functional form used, which supports our choice of working with the whole sample. It is not in contradiction with the results of the Q statistics above as they indicated heterogeneity across estimates of the same functional form but not across forms. Our next significant moderator is 'objective 1'. It is a dummy variable that takes value 1 when the funds are explicitly allocated to objective 1 regions. The difference in the estimated impact of such funds compared to non-objective 1 funds is significant but is very small (less than 0.000). Our results indicate also that the

[4]No study in our meta-database considers more than the EU 15 regions.

immediate impact of the funds is greater than its delayed impact although not by much. This argument is in tune with RODRIGUEZ-POSE (2000) and BOLDRIN and CANOVA (2001) where these authors see, at least in the first rounds of EU cohesion policies, a strategy targeted more towards short-term income support and redistribution than long-term sustainable development. We do not find any significant impact on heterogeneity of the number of years included in the allocation of the funds but both the initial year of the growth period and the year of composition/publication of the primary study matter. They are dummy variables with value 1 for early periods and 0 for the more recent periods. Several factors could explain the role of the beginning of the growth period: the presence of business cycles that render the funds more efficient over some periods of time, an increase in the amounts allocated over each programming period (following the enlargement to the South, the 1994-1999 period saw a significant increase in funding for regional development compared to the past), or the presence of a 'learning effect' in the allocation and use of the funds as advanced by RODRIGUEZ-POSE and NOVAK (2013) recently. The authors justify it with a 'more appropriate expenditure of the Cohesion funds, due to a progressive shift in their expenditure priorities' as well as a 'strengthening of the principle of partnership' with local and regional authorities (p.32). We believe that the significant presence of a time trend in the year of publication or composition of the manuscript indicates a 'learning effect' too, although of a different nature. More recent studies can rely on a larger literature providing additional expertise on the topic and on the appropriate statistical techniques to pay attention to, among other, spatial autocorrelation and the endogenous nature of the funds. Both effects can affect the magnitude and the precision of the estimates.

Next, we test whether several estimation characteristics used in the primary studies influence the estimated impact of the funds on growth. We discover that controlling for the endogeneity of the funds leads to estimates that are lower on average. It is the only significant characteristic in the second class of moderators.

Finally, we test the role of the regressors included in the primary studies where they control for observed heterogeneity. In our study they correspond to a dummy variable with value 1 when it is present in the primary study and 0 otherwise. We find that three moderators are significant at the 5% level. They are 'human capital or investment in education or R&D', 'corruption/institutional quality' and 'interaction term'. The first variable leads to an effect size that is lower on average. Its presence across many studies reflects the dominance of the augmented Solow growth model that includes the presence of a proxy for human-capital accumulation (MANKIW *et al*., 1992). EDERVEEN *et al*. (2006) is the contribution that explores the role of the second variable the most among the four studies that do so. Not surprisingly, they conclude that the effectiveness of the funds is conditional upon the level of corruption/institutional quality of the recipient area. Compared to studies that do not control for this characteristic, their estimates conclude to a lower effect size on average. Finally, when it comes to the 'interaction term', we refer the reader to the primary studies to find the exact definition of the 17 variables the funds have been interacted with in 77 cases. On average, the presence of an interaction term leads to a higher estimated impact of the funds in the primary studies.

When comparing the three models, we find that the coefficient estimates are very similar in magnitude and precision. It is confirmed in the similarity of the models' fit values (log-likelihood, AIC and R* - the Pearson correlation test between the fitted and observed values) and can be explained by the value of τ^2 being zero in the mixed and hierarchical models[5]. As a result, the heterogeneity detected in the distribution of the effect sizes is entirely observable whether it comes from the differences in study design, estimation processes, moderators used in the primary studies or from the variance of the effect sizes they estimate.

Finally, we complement the above models with an ordered probit model that presents the advantage of accounting for *both* the effect size of the dependent variable and whether it is significant or not in the primary studies (KOETSE *et al.*, 2009; CARD *et al.*, 2010). In this approach, the dependent variable takes on a value of 0 for the 'significant positive estimates' (when $T_i/\sqrt{v_i}$ is greater than 1.96), 1 for the 'significant negative estimates' (when $T_i/\sqrt{v_i}$ is smaller than -1.96) and 2 for the 'non-significant estimates' (when $|T_i/\sqrt{v_i}|$ is smaller than 1.96). In this model the errors are assumed to be normally distributed with variance 1 (GREENE, 2012, p. 788). The results appear in the last column of Table 2. All the significant and negative estimated coefficients indicate the variables that increase Prob (y=0 | x). They also decrease Prob (y=2 | x) while their impact on the middle category, Prob (y=1 | x), is more ambiguous as described in GREENE (2012, p.789). The opposite can be said about the significant and positive estimated coefficients. Our results indicate that the variables that increase the probability of a positive and significant estimated impact of the funds are the use of a functional form other than elasticity and the presence of a variable controlling for the level of

[5] We find the same results for the hierarchical model when we consider the studies written by the same author(s) as one. We still have 323 estimates in this case but only 12 independent studies. Complete results available from the authors upon request.

‘Employment or population’ in the primary study. We also find that the probability of concluding to an efficient impact of the funds decreases with increasing years of lag between allocation and growth, which indicates the immediate rather than long-run impact of EU cohesion policies (RODRIGUEZ-POSE, 2000; BOLDRIN and CANOVA, 2001); when the funds are divided by GDP; when spatial autocorrelation is controlled for (DALL’ERBA and LE GALLO, 2008) and when the original model captures the ‘economic structure’ of the recipient area.

Section 6. Conclusion

The capacity of structural funds to promote regional economic growth has been controversial for decades. Both economic theory and empirical applications are not unanimous about their role on growth; yet structural funds are an important part of the European integration project and the evaluation of their impact matters for both the recipients and the payers. This paper takes stock of the large number of studies that measure econometrically the impact of the funds on growth and select among them those that offer comparable effect sizes. It leads to 17 studies that offer 323 marginal effects. Not surprisingly, a Cochran’s Q statistic indicates that they are heterogeneous. It is true even when they are grouped by functional form (lin-lin vs. log-log). A significant publication bias is detected in the overall sample of primary estimates and among those that measure an elasticity, which could also explain their heterogeneity.

We investigate the sources of their heterogeneity further by means of several weighted regression models (fixed-effects model, mixed-effects model and hierarchical model). While they all assume that part of the heterogeneity is due to differences in the data characteristics, estimation methods and choice of regressors in the primary studies, they each model the variance of the omitted variables differently. Yet, they all lead to very similar estimates, which proves the robustness of our results and that all the heterogeneity detected among the effect sizes is observable. They indicate that several differences in the data characteristics are at the origin of the heterogeneity found in the primary estimates. Among them, we find that the publication status influences the size of the estimates. We also note the presence of a 'learning effect' in the sense that studies focusing on more recent years conclude to a larger impact of the funds, which suggests the way of allocating and using them has become more efficient. Furthermore, our results indicate that the differences in functional forms used in the primary studies do not have a significant impact on the size of the estimates.

Controlling for endogeneity and for three types of regressors ('human capital or investment in education or R&D', 'institutional quality' and 'interaction term') in the original studies also lead to significant differences in the primary estimates. The latter are characteristics of the recipient regions that condition the effectiveness of the funds.

Finally, the complement the usual meta-analytic approach by running an ordered probit model to uncover the factors that affect the probability of estimating a significantly positive impact of the funds. To our knowledge, this endeavor had never been done before.

These results suggest that future researchers working on EU regional development policies should be aware of the possible econometric bias and associated erroneous conclusions that come with their choice of study design and regressors. On the other hand, it is now clear that there are many aspects of the study such as the functional form and some estimation characteristics they should not be too worried about since they do not affect significantly the size of the estimates on average. In addition, future researchers will be able to rely on a larger literature than the first contributors to this field and this 'learning effect' has proven not negligible.

Given the long-lasting interest for improving the effectiveness of the funds, we believe that future contributions should devote more attention to estimating the impact of the funds in the frame of theories and models other than the neoclassical beta-convergence model. For instance, DALL'ERBA *et al.* (2009) and VARGA and VELD (2011) offer an approach based on an endogenous growth model but many more contributions are needed. Another exciting development in the evaluation of the funds is the use of a counterfactual methodological approach based on the regression discontinuity design as in BECKER *et al*. (2010, 2013) and PELLEGRINI *et al*. (2013). The authors build on the allocation rule of Objective 1 funds to compare the effect on the regions with a per capita GDP level just below the eligibility threshold (75% of EU average) with the per capita GDP of the regions just above since they did not get this type of funding. Last but not least, more attention could be given to locally weighted estimates of the funds as in LE GALLO *et al.* (2011). Their main contribution is to provide coefficient estimates for every single region, as opposed to the average impact for the entire sample, as is currently done in the literature. It helps them identify the regions where the funds have had a positive and significant impact and allows them to reconsider the 'one size fits all' approach that has dominated the allocation process and the empirical literature so far.

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Table 1: Characteristicsof the primary studies

Study[a]	Pub. type	No. of est.	Functional form	Effect size estimate						
				Min	Max	Mean	St. dev.	% sig. &Neg.	% Non-sig	% sig. &Pos.
Akcomak S. (2008)	T	12	Lin-Lin	0.004	0.080	0.044	0.029	0.0	91.7	8.3
Bahr C. (2008)	PD	13	Lin-Lin	-0.001	0.157	0.063	0.040	0.0	38.5	61.5
Beugelsdijk M. and Eijffinger S. (2005)	PD	4	Lin-Lin	-1.431	0.32	-0.258	0.815	0.0	75	25
Bouayad-Agha S., Turpin N. and Vedrine L. (2011)	PD	18	Log-Log	-0.005	0.020	0.006	0.008	16.7	83.3	0.0
Bouvet F. (2005)	T	4	Log-Log	0.020	0.270	0.105	0.113	0.0	25	75
Cappelen, A., Castellacci, F., Fagerberg, J., and Verspagen, B.(2003)	PD	3	Lin-Lin	0.005	0.007	0.006	0.001	0.0	0.0	100
Dall'erba S. and Le Gallo J. (2008)	PD	3	Lin-Lin	-0.010	0.002	-0.004	0.006	0.0	100	0.0
Dall'erba S. and Le Gallo J. (2007)	PD	28	Lin-Lin	-0.002	0.007	0.000	0.002	14.3	71.4	14.3
Ederveen, S., Gorter, J., Mooij, R. and Nahuis, R (2002)	WP	3	Log-Lin	-0.350	0.700	0.123	0.533	33.3	33.3	33.3
Ederveen, S., de Groot, H. and Nahuis, R. (2006)	PD	31	Log-Log	-0.026	0.062	0.008	0.022	0.0	100	0.0
Esposti R. (2007)	PD	8	Lin-Lin	0.000	0.000	0.000	0.000	0.0	62.5	37.5
Esposti R. and Bussoletti S. (2008)	PD	4	Log-Log	0.139	0.414	0.226	0.129	0.0	100	0.0
Fagerberg J. and Verspagen B. (1996)	PD	2	Lin-Lin	-0.417	-0.225	-0.321	0.136	100	0.0	0.0
Mohl P. and Hagen T. (2010)	PD	90	Log-Log	-0.009	0.011	0.000	0.004	18.9	54.4	26.7
Puigcerver-Peñalver M.-C. (2007)	PD	6	Log-Lin	-1.343	0.001	-0.448	0.602	50	50	0.0
Rodriguez-Pose A. and Fratesi U. (2004)	PD	92	Lin-Lin	-7.586	6.294	0.484	2.184	3.2	85.9	10.9
Rodriguez-Pose A. and Novak K. (2013)	PD	2	Lin-Log	0.021	0.369	0.195	0.247	0.0	50	50
Total		323		-7.586	6.294	0.174	1.504	10.2	71.5	18.3

Notes: [a] Publication year is reported in parentheses; PD stands for published papers, WP stands for working papers, T for thesis

Table 2: Meta-regression results (overall sample)

Moderator variables	Fixed effects	Mixed effects	Hierar-chical	Ordered probit
Constant	0.187 (0.003)	0.187 (0.003)	0.187 (0.003)	
Data characteristics				
Publication status: published (unpublished)	-0.047 (0.045)	-0.047 (0.045)	-0.047 (0.045)	1.453 (0.052)
Degree of freedom†	-0.001 (0.180)	-0.001 (0.178)	-0.001 (0.180)	0.726 (0.081)
Area of Study: Less than EU12 (EU12 or more)	0.037 (0.008)	0.037 (0.008)	0.037 (0.008)	0.037 (0.008)
Spatial units: country (regions)	0.006 (0.560)	0.006 (0.562)	0.006 (0.560)	0.914 (0.377)
Fund definition: Fund/GDP (other)	0.068 (0.044)	0.068 (0.044)	0.068 (0.044)	2.224 (0.050)
Functional form: lin-lin (log-log)	-0.002 (0.639)	-0.002 (0.639)	-0.002 (0.639)	-2.502 (0.022)
Functional form: semi-elasticity (log-log)	0.003 (0.912)	0.003 (0.913)	0.003 (0.912)	-1.948 (0.014)
Recipient regions: Objective 1 regions (other)	0.000 (0.002)	0.000 (0.003)	0.000 (0.002)	0.100 (0.606)
Time lag: number of years†	-0.001 (0.000)	-0.001 (0.000)	-0.001 (0.000)	0.253 (0.004)
Years of allocation †	-0.001 (0.826)	-0.001 (0.826)	-0.001 (0.826)	0.186 (0.083)
Initial year of growth period: pre-1994 (post-1994)	-0.098 (0.001)	-0.098 (0.001)	-0.098 (0.001)	-0.214 (0.593)
Early study: written pre-2007 (recent study: written post-2007)	-0.026 (0.019)	-0.026 (0.019)	-0.026 (0.019)	-1.547 (0.171)
Estimation characteristics				
Estimation method: other (least squares methods)	0.031 (0.341)	0.031 (0.341)	0.031 (0.341)	-1.817 (0.066)
Endogeneity (no endogeneity)	-0.002 (0.001)	-0.002 (0.001)	-0.002 (0.001)	-0.250 (0.355)
Fixed effects approach (no effect)	-0.023 (0.348)	-0.023 (0.348)	-0.023 (0.348)	0.334 (0.611)
Spatial autocorrelation	-0.031 (0.339)	-0.031 (0.339)	-0.031 (0.339)	1.935 (0.040)
Presence of regressors				
National dummy variable	0.000 (0.232)	0.000 (0.233)	0.000 (0.232)	-0.262 (0.563)
Initial per capita GDP	-0.033 (0.494)	-0.033 (0.494)	-0.033 (0.494)	-0.506 (0.328)
Economic structure	0.000 (0.055)	0.000 (0.058)	0.000 (0.055)	1.958 (0.011)

Employment or population	0.022 (0.307)	0.022 (0.307)	0.022 (0.307)	-2.025 (0.006)
Public investment or infrastructure stock	-0.002 (0.537)	-0.002 (0.537)	-0.002 (0.537)	0.285 (0.652)
Human capital or investment in education or R&D	-0.102 (0.000)	-0.102 (0.000)	-0.102 (0.000)	-0.916 (0.202)
Corruption/Institutional quality	0.040 (0.000)	0.040 (0.000)	0.040 (0.000)	1.106 (0.082)
Interaction term	0.010 (0.000)	0.010 (0.000)	0.010 (0.000)	0.863 (0.086)
Threshold from 'Positive significant' to 'Negative significant'				-0.699 (0.516)
Threshold from 'Negative significant' to 'Non-significant'				-0.274 (0.799)
n	323			
Log-Likelihood	473.392	0.163	473.392	-208.73
AIC	-896.785	0.163	-894.785	469.459
R*	0.163	0.163	0.163	0.357

Notes: The fixed effect model is estimated by Maximum Likelihood, the mixed effect mode is estimated by Restricted ML, the hierarchical model is estimated by Iterative Restricted ML. In the latter model, level-1 number of estimates is 323; level-2 number of studies is 17. The ordered probit model is a special case of the generalized linear model which is estimated by Maximum Likelihood. The dependent variable is the marginal effect of the funds on regional growth taken from the primary studies while it is a categorical variable indicating whether an estimate is significant positive, significant negative or non-significant in the ordered probit model. All moderator variables enter the regression as dummies, except those labeled with a '†' which are continuous variables. The omitted category for dummy variable appears in brackets below the name of the moderator variable. The p-values are reported in parenthesis below the coefficient estimates. R* is the Pearson coefficient of correlation between the fitted and the observed dependent variable. It is based on the pseudo-R described in McFADDEN (1973) for the ordered probit model.