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Incentive Effects of R&D Tax Incentives

– A Meta-Regression Analysis Focusing on R&D Tax Policy Designs

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Despite the growing literature on the effectiveness of R&D tax incentives, little is known about differing design aspects of the underlying R&D tax policies. In this paper, I apply sophisticated meta-regression methodology to separate the distinct provisions through which various R&D tax policies affect firms' R&D expenditure. My results indicate on average larger input additionality effects of hybrid tax regimes compared to volume-based schemes, while incremental tax measures seem to provide the lowest incentives for firms. My findings are particularly important for policy makers optimizing the design of an R&D tax policy.

Keywords: R&D Tax Policy, Design Aspects, Input Additionality Effects, Meta-Regression Analysis

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1. Introduction

Economic growth is largely determined by research and development (R&D) (Schumpeter, 1942; Solow, 1957; Romer, 1986; Lucas, 1988). However, due to market failure, from a society point of view the incentives to invest in R&D are too small as overall economic returns significantly exceed private returns (Arrow, 1962). The European Union set an ambitious target of increasing R&D spending to three percent of GDP as part of its Lisbon Strategy and following-up Strategy Europe 2020. As a response, 30 of the 35 OECD member states have already implemented various tax instruments to stimulate firms' R&D spending over the last decades (OECD, 2018).

The design of R&D tax incentives varies substantially among countries, ranging from tax credits to enhanced tax allowances and accelerated depreciations (European Commission, 2014). Hereby, tax incentives apply either to all qualifying R&D expenditures (volume-based), to the additional amount of R&D expenditure exceeding a given base level (incremental) or to a mixture of both (hybrid). Additionally, in some countries, immediate cash refunds, carrybacks, or carryforwards of unused tax benefits are supposed to ensure an incentive effect for loss-making firms.

While a large body of empirical studies provides evidence for positive effects of R&D tax incentives on R&D expenditure using firm and country level datasets, little is known about alternative design aspects although this is a critical issue in policy debates. As the general generosity of R&D tax regimes across countries is primarily driven by the design of the offered tax incentives, it is reasonable to expect that firms may respond differently to varying aspects of an R&D tax policy (Elschner et al., 2011; Thomson, 2013; Appelt et al., 2016). Therefore, my study contributes to the literature by examining the impact heterogeneity of diverse tax incentive provisions among countries using meta-regression analysis (MRA).

Prior MRA by Gaillard-Ladinska et al. (2015) and Castellacci and Lie (2015) analyze the effectiveness of R&D tax credits across industrial sectors. This paper extend earlier meta-regression analyses by Gaillard-Ladinska et al. (2015) and Castellacci and Lie (2015) in three ways. First, I update the Castellacci and Lie (2015) and Gaillard-Ladinska et al. (2015) meta-samples and focus on the latest literature by sampling studies published between 1993 and 2019.[2] Second, following Feld and Heckemeyer (2011), I utilize sophisticated meta-regression methodology in order to identify the best meta-regression model for my data at hand. Finally, I broaden the scope of my study by including additional moderator variables that refer particularly to the design aspects of R&D tax incentives. Taking advantage of the large cross-country variation in the underlying primary literature, I explain the heterogeneity in effect estimates through different design provisions, such as the scope of a tax instrument.

My MRA results reveal that various design choices result indeed in heterogeneous incentive effects for firms. More precisely, I find on average stronger additionality effects in countries with hybrid schemes, followed by volume-based tax regimes, while countries with incremental designs seem to provide the poorest incentives for firms. This is an interesting result especially for policy makers who are continuously improving the design aspects of R&D tax incentives.

The remainder of this study is structured as follows. Section 2 gives a review of literature on the effectiveness of R&D tax policy. Section 3 discusses the methodology of MRA, briefly presents the underlying meta-sample, and summarizes the identified moderator variables. The results of my MRA are displayed in Section 4. Section 5 investigates whether my main results are robust to various meta-subsamples. Finally, Section 6 concludes.

[2] I am grateful to Fulvio Castellacci and Christine Mee Lie for providing their database.

2. A Review of the Literature

2.1 The Effectiveness of R&D Tax Policy

R&D tax incentives have become an increasingly popular instrument available to policy makers to stimulate firms' private R&D expenditure. The introduction of tax incentives among countries has encouraged researchers to evaluate the effectiveness of an R&D tax policy. Most studies measure the input additionality effect of tax incentives by determining the increase in private R&D expenditure induced by the R&D tax policy.

In their comprehensive review on the effectiveness of R&D tax incentives, Hall and Van Reenen (2000) distinguish two key approaches for measuring the (causal) effect of R&D tax policies on R&D expenditures: The structural and the direct approach. The structural approach relies on the neoclassical investment model formalized by Jorgenson (1963) and Hall and Jorgenson (1967) in which an R&D capital stock is explained by the user cost of R&D. The R&D user cost incorporates tax incentives, wages as well as interest, inflation and depreciation rates (Hall and Van Reenen, 2000, 459). Numerous empirical studies postulate significant positive effects of decreasing R&D user cost on firms' R&D spending (e.g Billings et al., 2001; Paff, 2004, 2005; Harris et al., 2009; Mulkay and Mairesse, 2003, 2008, 2013). However, in the structural approach it is notoriously hard to control for all potentially confounding variables.

Recently, the direct approach has been increasingly utilized in the literature. It compares the increase in R&D expenditure of a treatment group with that of a control group. Group identification of firms can be based on their eligibility (potential treatment group) and non-eligibility (potential control group) for R&D tax instruments (see e.g. Hægeland and Møen, 2007; Duguet, 2012). However, this approach potentially suffer from an endogenous selection bias as user firms of R&D tax incentives may differ systematically from non-user firms. Accordingly, selection into the treatment group is not random, but may be due to firm characteristics or the design of the R&D tax measure. Therefore, the average change in firms' R&D expenditure cannot solely be attributed to the R&D tax instruments in force.

To mitigate endogeneity issues, some studies exploit the difference-in-difference (DiD) approach (e.g. Paff, 2005; Bozio et al., 2014; Cantabene and Nascia, 2014; Guceri, 2015, 2018; Guceri and Liu, 2019). This method compares the outcome of interest between firms with similar firm characteristics before and after an exogenous change in the R&D tax incentive scheme affecting only one (treatment) group of firms. An additional increase in R&D expenditure for the treatment group is solely attributable to the R&D tax policy, unless the common trend assumption is violated. Some other papers estimate the impact of R&D tax incentives using non-parametric matching techniques which balance the distribution of the treatment and control group (see e.g. Huang and Yang, 2009; Corchuelo and Martínez-Ros, 2010; Yang et al., 2012; Kobayashi, 2014 and Holt et al., 2016). However, to be effective, matching requires that differences between treatment and control groups manifest in observable variables (Pearl, 2009).

2.2 Design Aspects of R&D Tax Policy

The design aspects of R&D tax policies differ considerably among countries (European Commission, 2014). Generally, R&D tax incentives either relate to R&D expenditure (input-based) or the income generated by intellectual property (output-based). In my meta-analysis, I focus on the former as most research analyzes input-based provisions.

Input-based incentives are considered within a wide set of instruments: R&D tax credits, enhanced allowances, accelerated depreciations or cash refunds and credits for loss-making firms.[3] Some countries use a mix of several tax measures and combine tax credits with enhanced

[3] The enhanced allowance is commonly combined with a cash credit which is a refundable tax credit for SMEs in a loss position.

allowances or accelerated depreciations to decrease both, the firms' taxes due, as well as the firms' tax base. Countries ensure an incentive effect of R&D tax instruments even for firms in a loss position by either allowing immediate cash refunds or by providing for carrybacks or carryforwards to future income years.

An R&D tax policy can be further classified according to its scope. Tax measures are implemented incrementally if they cover only an increase in R&D expenditures compared to an initial level, volume-based if they apply to all qualifying R&D expenditures or hybrid if they combine the two previous schemes. As volume-based regimes are less complex and reach a larger target group, some countries have recently adopted or switched to volume-based systems.[4] However, an incremental R&D tax policy may minimize the deadweight loss through forgone tax revenue due to R&D that would have been done without any R&D tax incentives. Lokshin and Mohnen (2012) evaluate the Dutch volume-based tax credit scheme during the years 1996 and 2004, and measure a deadweight loss of 85 percent of forgone tax revenue. Contrary, Lester and Warda (2014) find that both, incremental and volume-based incentives, exhibit a similar cost-effectiveness.

As seen above, a large body of literature analyzes the effectiveness of R&D tax incentives relying on micro data. This literature rarely investigates whether firms' responses to tax incentives may vary with design aspects (i.e. distinct scopes), and if so, the results are mixed (Klassen et al., 2004; European Commission, 2014; Spengel et al., 2017).

[4] E.g., France had an incremental scheme until 2003, a hybrid scheme between 2004 and 2007 and a volume-based scheme from 2008 onwards. However, Italy replaced its volume-based scheme by an incremental scheme in 2011.

3. Meta-Analysis Methodology

The studies reviewed in Section 2 vary considerably with respect to samples, methods and particularly the design of the R&D tax incentive taken into account. Therefore, it is not surprisingly that findings on the effectiveness of R&D tax treatments are inconsistent or even contradictory. Meta-analysis is a methodology to quantitatively combine a strand of literature, and analyze the heterogeneity in effect estimates across primary studies (Stanley, 2001). The contribution of my MRA is to separate various design aspects through which an R&D tax policy affects firms' R&D expenditure.

3.1 Measuring the Effectiveness of R&D Tax Policy

Results of meta-analyses are only meaningful if effect estimates are consistent across the underlying primary studies (Stanley, 2001). Therefore, I only consider studies on the effectiveness of the R&D tax policy that estimate the additionality ratio according to the following model:[5]

$$RD_{it} = \beta_0 + \beta_1 Eligibility_{it} + \beta X_{it} + \varepsilon_{it} \text{ (1)}$$

The primary studies underlying my MRA regress the (log) R&D expenditure of firm i in period t (RD_{it}) on a dummy variable indicating the eligibility to the R&D tax policy scheme ($Eligibility_{it}$) and a vector of firm level controls (X_{it}).[6] The coefficient of interest β_1 (expected to be positive) captures the additionality ratio which is the dependent variable in my MRA.

[5] The study by Kasahara et al. (2014) uses the effective tax rate as explanatory variable and is therefore excluded from my meta-sample.

[6] Some primary studies relying on the direct approach consider R&D intensity (R&D expenditures scaled by assets or sales) as input measure. In an unreported robustness test, I investigate whether my results hold if I drop effect estimates using R&D intensity as the dependent variable. My results remain unchanged.

3.2 Selection of Literature

To locate appropriate studies, I use common databases and search engines such as JSTOR, Elsevier ScienceDirect Books and Journal database, IDEAS, and Google Scholar. The central keywords used in the electronic literature research were “Research and Development”, “Tax Policy”, “Tax Credits”, “Tax Incentives”, “Additionality Ratio”, and “Effectiveness”. While Castellacci and Lie (2015) and Gaillard-Ladinska et al. (2015) cover studies published from 1992 through 2013 and 2006 through 2014, respectively, I consider papers published between 1993 and 2019 in my meta-sample. I only select primary studies that are based on firm level datasets and provide sufficient information concerning standard errors or t-statistics.[7] The last update of my meta-sample was carried out in March 2019.

As selecting one single effect estimate per primary study is quite subjective and results in a small sample size and less heterogeneity among effect estimates, I included multiple effect estimates from each primary study as long as there is a substantial difference regarding the model specification, method or sub-sample.[8] Such multiple sampling is commonly used in meta-analyses (Feld and Heckemeyer, 2011; Castellacci and Lie, 2015; Belz et al., 2017).

Table 1 in the Appendix outlines all primary studies in my meta-sample and provides an overview on the underlying countries and obtained effect estimates (i.e. number, mean, minimum, maximum and standard deviation). Studies are highlighted with an “n” if they were not subject to the meta-analyses by Castellacci and Lie (2015) or Gaillard-Ladinska et al. (2015).[9] As shown in Table 1, my total meta-sample contains 18 primary studies and 244 effect estimates. The number of effect estimates taken from each primary study ranges from 1 in Billings and Fried (1993) to 72 in Ho (2006). As expected, all arithmetic means of effect

[7] The study of Aralica and Botric (2013) does not provide sufficient information and is therefore excluded from my meta-sample.

[8] I do not sample effect estimates in robustness analysis and for specific sub-samples of firms that are beyond the scope of my study, e.g. liquidity constrained firms, firms in the service industry or low-concentration sectors.

[9] Differences in primary studies’ means arise because I sample effect estimates relying on a published version of the primary study and I do not consider effect estimates for specific sub-samples.

estimates per primary study in column 5 are positive and vary from 0.00065 in Berger (1993) to 3.72 in Ho (2006), indicating that effect estimates differ considerably across primary studies. However, the effect estimates vary far less within primary studies since the standard deviations in the last column of Table 1 range far below 1.0, except for the study by Ho (2006). In addition, my meta-sample shows a large geographical variation as it covers effect estimates for 10 different countries. I exploit this country heterogeneity to separate the effects of various policy design aspects.

3.3 Meta-Regression Analysis

I rely on MRA to explain the heterogeneity among effect estimates. Therefore, my meta-regression model considers moderator variables which describe various characteristics within and between primary studies that may account for the variation across effect estimates (see Section 3.4 for moderator variable description). I apply the following fixed effects (FE) meta-regression model:

$$\hat{e}_{is} = \alpha + \beta_1 \cdot x_{is} + \varepsilon_{is} \ (2)$$

In equation (2) the dependent variable is the reported effect estimate $\hat{e}_{is}$ (i.e. additionality ratio) of regression $i = 1, \dots, I$ found in primary study $s = 1, \dots, S$. x_{is} is a vector with moderator variables, and α specifies the intercept. In MRA, the FE model assumes that all primary studies share a common true effect, while differences among effect estimates are solely due to the sampling error.

If heterogeneity among effect estimates is only partially explained by the moderator variables, a random or mixed effects (ME) model is more suitable. The ME meta-regression model is the following:

$$\hat{e}_{is} = \alpha + \beta_1 \cdot x_{is} + \vartheta_{is} + \varepsilon_{is} \ (3)$$

where ϑ_{is} captures the unexplained heterogeneity and all other variables as defined above.

The standard errors of effect estimates vary substantially due to different attributes (i.e. sample sizes) in the underlying primary studies. Hence, the residuals are clearly heteroskedastic. To account for heteroscedasticity, both equations (2) and (3) are weighted by the inverse variances of effect estimates as analytical weights (Stanley, 2008). In the case of the FE meta-regression model, the analytical weights are the squared standard errors sampled from primary studies. For the ME meta-regression model, the weights are estimated relying on the residual maximum likelihood (REML) technique. Beyond accounting for heteroscedasticity, controlling for precision corrects for low quality effect estimates as imprecise estimates are given a lower weight in the MRA (Stanley and Doucouliagos, 2012).

I consider multiple effect estimates from each primary study in my meta-sample which bears the risk of within-study dependency. The integration of moderator variables does not account for the possibility of dependency of effect estimates due to unobserved study level heterogeneity.[10] To correct for autocorrelation researchers suggest the use of unbalanced panel or multilevel models (Stanley and Doucouliagos, 2012).[11] To identify the best meta-regression model for my data at hand, I apply model specification testing proposed by Feld and Heckemeyer (2011).

3.4 Moderator Variables

Building on the literature review in Section 2, I identify various moderator variables that refer to the definition of the dependent variable, the type of sample, the econometric method, and most importantly to the design choice of an R&D tax policy. Thus, I code binary dummy

[10] In this setting, autocorrelation (within-study dependency) may be present, hence, the error terms are not independently and identically distributed.

[11] The panel structure is unbalanced as the number of effect estimates vary across study.

variables referring to the study and regression level. Table 2 in the Appendix summarizes the moderator variables along with their summary statistics for my meta-sample. Table 2 indicates that each moderator variable is based on a sample of at least 30 effect estimates which allows for a reliable empirical analysis. The summary statistics show that there is indeed variability among primary effect estimates as the means vary sharply between various moderator variables.

3.4.1 Definition of Dependent Variable

As the distribution of firms' R&D expenditure is largely skewed, primary literature mainly specifies the logarithm of R&D spending as dependent variable, while limiting the sample to observations with strictly positive values on R&D expenditure. However, explicitly omitting observations with zero values may induce endogenous selection bias.

For this reason, I code a binary moderator variable: *Logarithm*. The moderator variable *Logarithm* is equal to one if a primary regression considers the logarithm of the dependent variable, and zero otherwise. The regression coefficient on *Logarithm* measures the marginal impact of applying this definition in primary regressions relative to the omitted definition (*Level*), ceteris paribus.

3.4.2 Firm Characteristics: Size and Industry

The seminal work by Schumpeter (1942) hypothesized the dominance of larger firms in the technological process through greater market power leading to better financial and human resources. Theory and empirical evidence suggest that SMEs are less likely to innovate as they face liquidity constraints due to asymmetric information (Czarnitzki and Hottenrott, 2011). Hence, one might expect that the effectiveness of an R&D tax policy is stronger for SMEs than for large firms due to the provision of additional liquidity. However, empirical evidence on heterogeneous additionality effects by firm size is mixed (e.g. Koga, 2003; Baghana and Mohnen, 2009; Corchuelo and Martínez-Ros, 2010; Kobayashi, 2014). Therefore, I introduce a moderator variable for effect estimates based on a sub-sample of small firms (*SMEs*).

According to Castellacci and Lie (2015), the impact of an R&D tax credit varies among sectors, possibly because firms across industries differ largely regarding their innovation strategies and technological performances. Their results suggest on average smaller additionality effects for high-tech firms. I categorize whether effect estimates are related to a sub-sample of high-tech firms (*High-Tech Firms*).

3.4.3 Econometric Method

Primary studies further differ regarding the acknowledgement of endogeneity issues. While early studies apply simple OLS regression models, more recent empirical literature considers the issue of endogeneity by using matching methods or by exploiting the panel structure of data (DiD approach).[12]

To take this into account, I introduce a dummy variable indicating the econometric method used. *Diff-in-Diff* is one if the underlying primary regression applies the DiD approach, and zero otherwise. Studies estimating the DiD mostly include firm and year fixed effects into their regression equation. This is important since firm and year fixed effects control for unobservable (time-invariant) firm characteristics (e.g. managerial ability, attitude to risk or culture) and time trends (e.g. overall business cycle, technology shocks or changing legislation) that could affect firms' R&D expenditure. I expect smaller effect estimates for studies exploiting the DiD since the inclusion might reduce cross-sectional and time-series variation.

[12] Due to multicollinearity, the moderator variable indicating primary regressions applying matching techniques has been excluded.

3.4.4 Design of R&D Tax Policy

The generosity of an R&D tax policy depends on its design which varies considerably among countries (Elschner et al., 2011; Thomson, 2013; Appelt et al., 2016). Based on the literature review in Section 2, it is reasonable to expect that firms respond differently to varying aspects of an R&D tax policy. Therefore, I include various moderator variables to separate the incentive effects of different design aspects.

I introduce three moderator variables relating to the scope of countries' tax regimes: *Incremental Scheme, Hybrid Scheme* and *Volume-Based Scheme.* The moderator variable *Enhanced Allowance* indicates whether the underlying effect estimate refers to a country offering enhanced allowances (one), rather than to a country implementing *Tax Credits* (zero). Moreover, I include a moderator variable regarding the existence of a refund of unused tax benefits: *Immediate Cash Refund* that equals one if an effect estimate relates to a country that grants an immediate cash refund, and zero otherwise.

3.4.5 Publication Bias

Primary studies as well as meta-analyses are often subject to the issue of publication bias as statistically insignificant and unexpected effect estimates often remain unpublished. Funnel plots map the effect estimates against its precision as measured by the inverse of the estimates' standard errors to detect the presence of a publication bias (Egger et al., 1997). Primary studies with small sample sizes and larger standard errors (i.e. low precision) scatter widely at the bottom of the diagram, whereas studies with large sample sizes and thus higher precision are distributed at the top of the diagram. In the absence of publication bias, the effect estimates are expected to spread randomly around the common true effect.

Obviously, the asymmetric funnel plot in Figure 1 in the Appendix indicates the presence of a publication bias in the primary literature since the funnel plot shows an elongated right tail with a missing left-hand side as only a few primary studies report negative additionality ratios.

To address the issue of publication bias more formally, I include the *Standard Error* of effect estimates into my meta-regression model which is a common method in MRA (e.g. Feld and Heckemeyer, 2011; Castellacci and Lie, 2015; Belz et al., 2017). The funnel asymmetry test (FAT) is a t-test of the regression coefficient on *Standard Error*, indicating the presence of a publication bias (Egger et al., 1997). The precision effect test (PET) identifies whether there is a true empirical effect beyond publication bias (Stanley, 2008).

4. Meta-Regression Results

Table 4 in the Appendix displays the main results of my MRA for effect estimates relying on the direct approach according to equation (1). The dependent variable is the additionality ratio taken from primary studies, and the independent variables are various characteristics within and between primary studies that may account for the variation across effect estimates. Each meta-regression coefficient indicates the impact of the moderator variables on the effectiveness of R&D tax policy, ceteris paribus. For moderator variable descriptions see Section 3.4 and Table 2 in the Appendix. As the Q-test in Table 3 in the Appendix indicates excess heterogeneity among effect estimates and the Breusch-Pagan LM test does not provide evidence for unobserved study level heterogeneity, my MRA in Table 4 relies on ME pooled WLS. Table 4 shows that correlation between the moderator variables is a minor issue in my MRA as each variance inflation factor (VIF) is far below 10.00.

The main contribution of my MRA is to separate the incentive effects of varying design aspects. The regression coefficients on *Incremental Scheme* are significantly negative, while those on *Volume-Based Scheme* and *Hybrid Scheme* are both positive and on the moderator variable *Hybrid Scheme* significant at the 1 percent level. More precisely, hybrid schemes provide on average stronger incentives for firms, while the additionality effects in countries with incremental measures are on average lower in the underlying primary literature. A plausible explanation for this finding is that many firms already have high levels of R&D expenditure and may find it more challenging to raise their R&D expenditure even further (Castellacci and Lie, 2015). Therefore, incremental designs may distort firms' R&D processes as the timing of R&D investment is determined by maximizing tax advantages rather than optimizing the R&D strategy (Hollander et al., 1987). In addition, incremental schemes tend to provide less additional liquidity to firms and are more complex than hybrid and volume-based measures, resulting in higher administrative costs. The latter may discourage firms from applying for such incentives if the (perceived) application costs exceed expected tax benefits (Appelt et al., 2016).

In column (4) and (5), I introduce the moderator variables *Enhanced Allowance* and *Cash Refund* which account for the generosity of an R&D tax regime. The coefficients on *Enhanced Allowance* in Table 4 are negative and highly significant, indicating that enhanced allowances seem to have on average lower incentive effects in comparison to tax credits. This result is reasonable since enhanced allowances only reduce the taxable base, while tax credits decrease a firms' tax liability. The overall amount of the tax benefit may be lower for such tax base measures because it depends additionally on the statutory tax rate a firm faces (Spengel and Wiegard, 2011). The moderator variable *Immediate Cash Refund* separates the incentive effects of refundability provisions for loss-making firms. The coefficients on *Immediate Cash Refund* are positive and significant in column (5), suggesting on average higher incentive effects of immediate cash refunds compared to carry-over or even no refundability provisions. The R&D literature argues that liquidity constraints due to asymmetric information are a major economic justification for providing tax incentives (e.g. Spengel and Wiegard, 2011; Pfeiffer and Spengel, 2017). However, if financial restrictions are of concern, it is even more likely to be in a low profit or loss position when carrying out R&D, so that the tax incentives would not have any immediate effects (Appelt et al., 2016). Consequently, the higher additionality effects may be because cash refunds raise the liquidity for loss-making firms immediately.

As explained before, I consider a moderator variable relating to the definition of the dependent variable in the underlying primary regressions.[13] The coefficients on *Logarithm* are positive but only significant in column (2) and column (3). As the results are not robust across the various specifications, I am not able to confirm the expected bias induced by transforming the dependent variable and omitting observations with zero values.

Thereafter, I distinguish between firm size groups by adding the moderator variable *SMEs*. Surprisingly, I do not find systematic differences between effect estimates based on a sub-sample of SMEs and those relying on any other (sub-)sample. Regarding the moderator variable *High-Tech Firms* that controls for industry differences, I obtain the following result: The regression coefficients on *High-Tech Firms* in Table 4 are indeed significantly negative in column (2), column (3) and column (5). This finding confirms the results of Castellacci and Lie (2015), showing that the effect estimates for a sub-sample of high-tech firms are still lower in the underlying primary literature.

As expected, the underlying econometric method is a source of heterogeneity in effect estimates. The coefficients on the moderator variable *Diff-in-Diff* are negative and highly significant throughout my specifications, except for column (5). These findings imply that primary studies using the DiD approach to address endogeneity obtain on average smaller effect estimates in comparison to primary studies that use other methods. My findings supports the

[13] In a robustness test in Table 6, I investigate whether my results hold on a meta-subsample of effect estimates relying on a log-linear specification. My results remain unchanged.

prediction that studies not considering endogeneity issues tend to overestimate the effects of R&D tax policy on R&D expenditure.

At last, Table 4 documents that the regression coefficients on the *Sampling Error* are highly significant throughout all specifications, indicating the presence of a publication bias in the underlying primary literature on the effectiveness of R&D tax policy after controlling for heterogeneity. However, there is a true empirical effect beyond publication bias as the intercept is significantly positive, except for specification (5). Overall, my MRA results point out that the design choices among R&D tax policies seem to have an important impact on the obtained additionality effects in the primary literature. In the next Section, I investigate this pattern further.

5. Robustness Analysis

I check whether my main results in Table 4 are robust to limiting the analysis to various meta-subsamples. As indicated by Table 1, several estimates in the study of Ho (2006) can be seen as outliers: E.g. the largest estimate in this study is 36.43, whereas the largest estimate in all other studies is 1.5. Therefore, I exclude effect estimates based on a linear specification found in the primary study by Ho (2006).[14] As the Breusch-Pagan LM test in Table 3 indicates unobserved study level heterogeneity and the Hausman test rejects the random effects multilevel model, I allow autocorrelation between effect estimates per primary study (within-study dependency) and cluster standard errors on the study level. However, the regression results in Table 5 must be interpreted with caution since the number of study clusters with 18 studies is rather small. Despite this, the coefficients remain qualitatively unchanged.

[14] After dropping effect estimates relying on a linear regression the number of effect estimates is 37 and the arithmetic mean is 0.12.

In Table 6, I only consider effect estimates from a log-linear functional form. In each specification, I estimate a FE pooled WLS meta-regression without clustering standard errors because the number of study clusters is too small. The main results do not qualitatively change. This robustness check can be further used to specify the true empirical effect beyond publication bias as effect estimates based on a log-linear specification are commonly interpreted as the percentage change in R&D expenditure due to the R&D tax policy. While a hybrid tax incentive increase firms' R&D expenditure by roughly 40 percent, a volume-based scheme raise R&D expenditure by 12.7 percent and an incremental scheme by only 1 percent in the underlying primary literature.

6. Conclusion

Tax incentives have become a major policy tool to stimulate firms' private R&D expenditure. Despite the extensive literature which provides profound insights into the input additionality of R&D tax policies based on firm level data, the heterogeneity of incentive effects between countries due to various design aspects is not sufficiently taken into account in this stream of literature. The aim of my study is to shed further light on various design aspects through which an R&D tax policy affects firms' R&D expenditure. Therefore, I set up a meta-sample containing 18 studies and 244 additionality ratios published between 1993 and 2019. To investigate my research question, I apply sophisticated meta-regression methodology after identifying the best meta-regression model for my data at hand. My main results indicate on average the largest incentive effects of hybrid tax regimes, followed by volume-based schemes, while obtained additionality ratios seem to be lower for countries with incremental tax measures. Additionally, I discover significant publication bias in the underlying strand of literature. My study has important implications for policy makers, as my findings show that firms respond differently to varying R&D tax policy designs.

Appendix

Figure 1: Funnel plot.

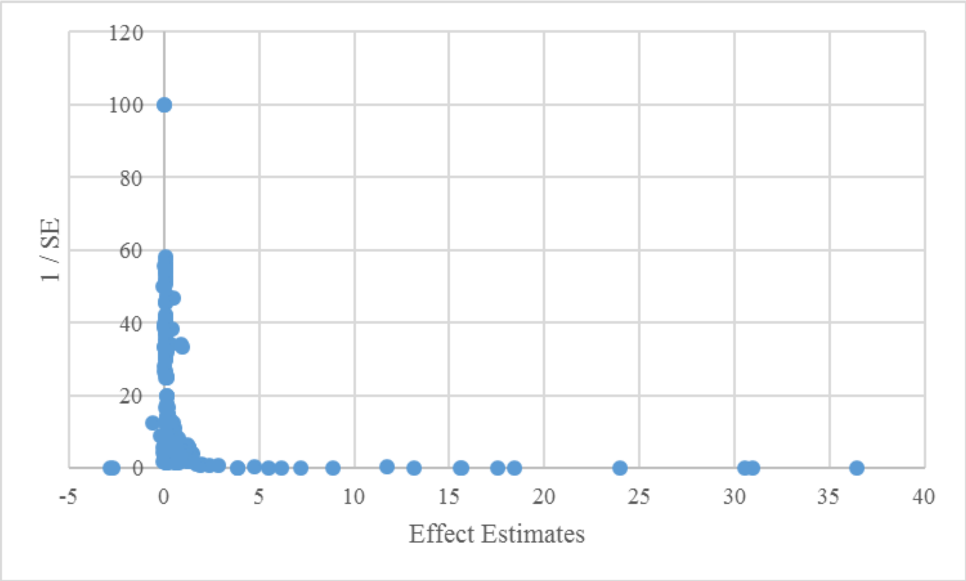

Table 1: Primary Studies Relying on the Direct Approach (Additionality Ratio).

	Author	**Country**	**Effect Estimates**				**Std. Dev.**
			N	**Mean**	**Min**	**Max**	
1	Agrawal et al. (2017)[n]	Canada	3	0.18	0.17	0.18	0.01
2	Berger (1993)	USA	2	0.00	0.00	0.00	0.00
3	Billings and Fried (1999)	USA	1	0.02	0.02	0.02	-
4	Billings et al. (2001)	USA	2	0.19	0.14	0.25	0.08
5	Bozio et al. (2014)	France	7	0.10	0.05	0.17	0.04
6	Cantabene and Nascia (2014)[n]	Italy	2	0.17	0.15	0.19	0.03
7	Corchuelo and Martínez-Ros (2010)	Spain	18	0.66	0.12	0.87	0.19
8	Duguet (2012)	France	50	0.08	0.03	0.14	0.03
9	Guceri (2015)[n]	UK	10	0.19	0.12	0.23	0.04
10	Guceri (2018)[n]	UK	10	0.17	0.14	0.21	0.03
11	Guceri and Liu (2019)[n]	UK	6	0.31	0.28	0.38	0.04
12	Hægeland and Møen (2007)	Norway	18	0.99	-0.57	1.50	0.52
13	Ho (2006)	USA	72	3.72	-2.81	36.43	7.99
14	Holt et al. (2016)[n]	Australia	10	0.40	0.13	0.92	0.28
15	Huang and Yang (2009)	Taiwan	9	0.81	0.57	1.04	0.16
16	Kobayashi (2014)[n]	Japan	15	1.16	0.80	1.43	0.17
17	Paff (2005)	USA	6	0.34	0.33	0.35	0.01
18	Yang et al. (2012)	Taiwan	3	0.50	0.23	0.72	0.25
	Total Meta-Sample	-	244	1.40	-2.81	36.43	4.59

[n] Primary study was not subject to prior meta-analyses by Castellacci and Lie (2015) or Gaillard-Ladinska et al. (2015).

Table 2: Summary Statistics of Moderator Variables.

Moderator Variables	Description	Descriptive Statistics		
		No.	Mean	Std. Dev.
Definition of Dependent Variable				
Logarithm	= 1 if primary regression uses the logarithm of dependent variable, and 0 otherwise.	135	0.553	0.498
Type of Sample				
SMEs	= 1 if primary regression relies on a sub-sample of SMEs, and 0 otherwise.	70	0.287	0.453
High-Tech Firms	= 1 if primary regression relies on a sub-sample of high-tech firms, and 0 otherwise.	47	0.193	0.395
Econometric Method and Specification				
Diff-in-Diff	= 1 if primary regression uses DiD approach, and 0 otherwise.	67	0.275	0.447
Design of R&D Tax Policy				
Incremental Scheme	= 1 if a country to which the respective effect estimate refers has adopted an incremental scheme, and 0 otherwise.	133	0.545	0.499
Hybrid Scheme	= 1 if a country to which the respective effect estimate refers has adopted a hybrid scheme, and 0 otherwise.	49	0.201	0.401
Volume-Based Scheme	= 1 if a country to which the respective effect estimate refers has adopted a volume-based scheme, and 0 otherwise.	62	0.254	0.436
Enhanced Allowance	= 1 if a country to which the respective effect estimate refers has adopted an enhanced allowance, and 0 otherwise.	30	0.123	0.329
Immediate Cash Refund	= 1 if a country to which the respective effect estimate refers has adopted an immediate cash refund, and 0 otherwise.	35	0.143	0.351
Publication Bias				
Standard Error	Standard errors of effect estimates.	244	1.219	5.574

Table 3: Tests to Identify the Best Meta-Regression Model.

Specification	No Excess Heterogeneity?	No Unobserved Study Level Heterogeneity?	No Correlation between RE and Moderator Variables?
	Cochran's Q-Test	Breusch-Pagan LM Test	Hausman Test
		<u>Table 4: Total Meta-Sample</u>	
(1)	1524.44	0.00	-
	p = 0.000	p = 1.000	
(2)	1553.41	0.00	-
	p = 0.000	p = 1.000	
(3)	1563.13	0.00	-
	p = 0.000	p = 1.000	
(4)	1452.70	0.00	-
	p = 0.000	p = 1.000	
(5)	1371.75	0.00	-
	p = 0.000	p = 1.000	
		<u>Table 5: Meta-Sample without Outliers</u>	
(1)	2326.27	81.43	20.52
	p = 0.000	p = 0.000	p = 0.000
(2)	2205.31	220.18	17.19
	p = 0.000	p = 0.000	p = 0.002
(3)	2698.96	400.62	20.39
	p = 0.000	p = 0.000	p = 0.000

Notes: p-values are given below the test statistics.

Table 4: Results of MRA.

Moderator Variables	*Incremental Schemes* (1)	*Hybrid Schemes* (2)	*Volume-Based Schemes* (3)	*Generosity* (4)	*Generosity* (5)
Definition of Dependent Variable					
Logarithm	0.038	0.144***	0.191***	0.048	0.070
	(0.046)	(0.040)	(0.049)	(0.047)	(0.043)
Type of Sample					
SMEs	-0.011	0.021	0.051	0.013	-0.059
	(0.038)	(0.036)	(0.043)	(0.043)	(0.041)
High-Tech Firms	0.042	-0.142***	-0.141**	0.001	-0.135**
	(0.058)	(0.051)	(0.067)	(0.060)	(0.052)
Econometric Method					
Diff-in-Diff	-0.230***	-0.114**	-0.252***	-0.187***	0.015
	(0.043)	(0.046)	(0.052)	(0.047)	(0.049)
Design of R&D Tax Policy					
Incremental Scheme	-0.306***			-0.329***	
	(0.041)			(0.043)	
Hybrid Scheme		0.354***			0.436***
		(0.047)			(0.047)
Volume-Based Scheme			0.044		
			(0.053)		
Enhanced Allowance				-0.152***	-0.149***
				(0.054)	(0.050)
Immediate Cash Refund				0.018	0.256***
				(0.053)	(0.051)
Publication Bias					
Standard Error	1.533***	1.495***	1.693***	1.508***	1.537***
	(0.153)	(0.155)	(0.164)	(0.154)	(0.148)
Constant	0.342***	0.041*	0.079***	0.358***	0.025
	(0.043)	(0.023)	(0.027)	(0.044)	(0.022)
Number of Observations	244	244	244	244	244
Number of Primary Studies	18	18	18	18	18

VIF	1.50	1.51	1.75	1.76	1.78

Notes: The dependent variable is the additionality ratio found in primary studies (direct approach). All moderator variables are coded as binary dummy variables, except for *Standard Error*. In each specification, I use ME pooled weighted least squares (WLS). The analytical weights are estimated relying on REML technique. ***, **, * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors in parentheses.

Table 5: Robustness Analysis.

Moderator Variables	*Incremental Schemes* (1)	*Hybrid Schemes* (2)	*Volume-Based Schemes* (3)
Definition of Dependent Variable			
Logarithm	0.012	0.059	0.069
	(0.019)	(0.040)	(0.073)
Type of Sample			
SMEs	0.039	0.065	0.080
	(0.094)	(0.078)	(0.106)
High-Tech Firms	-0.069***	-0.123**	-0.142
	(0.020)	(0.048)	(0.086)
Design of R&D Tax Policy			
Incremental Scheme	-0.170*		
	(0.098)		
Hybrid Scheme		0.275***	
		(0.061)	
Volume-Based Scheme			0.030
			(0.102)
Publication Bias			
Standard Error	2.146***	2.281***	2.583***
	(0.699)	(0.481)	(0.499)
Constant	0.171*	0.000**	0.000
	(0.098)	(0.000)	(0.000)
Number of Observations	209	209	209
Number of Primary Studies	18	18	18
VIF	1.78	1.37	1.61

Notes: The dependent variable is the additionality ratio found in primary studies (direct approach). I drop effect estimates relying on a linear specification found in Ho (2006). All moderator variables are coded as binary dummy variables, except for *Standard Error*. In each specification, I use FE pooled weighted least squares (WLS). The analytical weights are squared sampling errors. ***, **, * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors in parentheses and are clustered on study level to control for autocorrelation (within-study dependency).

Table 6: Robustness Analysis.

Moderator Variables	*Incremental Schemes* (1)	*Hybrid Schemes* (2)	*Volume-Based Schemes* (3)
Type of Sample			
SMEs	0.068***	0.071***	0.087***
	(0.012)	(0.012)	(0.012)
High-Tech Firms	-0.051***	-0.117***	-0.136***
	(0.011)	(0.010)	(0.011)
Design of R&D Tax Policy			
Incremental Scheme	-0.208***		
	(0.010)		
Hybrid Scheme		0.337***	
		(0.014)	
Volume-Based Scheme			0.049***
			(0.010)
Publication Bias			
Standard Error	1.385***	1.853***	2.001***
	(0.118)	(0.113)	(0.116)
Constant	0.219***	0.063***	0.078***
	(0.008)	(0.006)	(0.006)
Number of Observations	135	135	135
Number of Primary Studies	13	13	13
VIF	1.69	1.43	1.44

Notes: The dependent variable is the additionality ratio found in primary studies (direct approach). I only consider effect estimates relying on a log-linear specification. All moderator variables are coded as binary dummy variables, except for *Standard Error*. In each specification, I use FE pooled weighted least squares (WLS). The analytical weights are squared sampling errors. ***, **, * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors in parentheses.

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