Effectiveness of State Aid on Firm Performance: Evidence from the EU*

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Abstract

Despite renewed interest in industrial policy, empirical evidence on state aid effectiveness remains limited. We examine the short-run effects of large state aid awards across 22 EU Member States, linking firm-level data from Orbis with publicly disclosed awards granted in 2017–2018. Using a generalized difference-in-differences design, we estimate effects on investment, employment, and productivity. State aid significantly increases capital investment, particularly among SMEs and firms receiving regional development aid, which are groups more likely to face financial constraints. Effects are strongest in Central and Eastern Europe. However, we find no corresponding improvements in productivity or employment within two years. These findings suggest that while state aid effectively stimulates investment in the short run, productivity gains may take longer to materialize, and efficiency gains could be obtained from better targeting and EU-wide coordination.

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1 Introduction

Industrial policy has returned to the forefront of economic policymaking in Europe. Both the level of state aid spending and the intensity of the debate have risen sharply in recent years. While some governments, notably those of France and Germany, advocate for ambitious subsidy programmes to secure strategic sectors and enhance the EU's competitiveness, others express concern over market distortions and wasteful subsidy races. Yet despite this renewed interest, systematic evidence on the effectiveness of modern state aid in industrialised economies remains limited. In particular, we lack comprehensive insights into what types of aid actually produce the desired changes in firm behaviour and performance, if at all. This paper contributes to filling this gap. We study the short-run effects of large state aid awards across five major policy objectives—regional development, SME support, R&D, environmental protection, and sectoral development—using harmonised firm-level data and beneficiary-level information on aid awards from 22 EU Member States.

We compile a novel dataset that combines all large state aid awards mandated for public disclosure with firm-level data from Bureau van Dijk's Orbis database. Our focus is on awards exceeding €500,000 granted in 2017 and 2018, before the onset of the COVID-19 pandemic. Aid awards above this threshold must be reported publicly. We exclude later years primarily because during the pandemic the number of smaller State Aid approvals increased substantially. These smaller awards are not reported consistently individually and could therefore pollute our control group. Using a generalised difference-in-differences design with staggered adoption, we estimate the dynamic effects of aid on investment, employment, and total factor productivity (TFP).

Most State Aid above the threshold has the primary purpose to support investment rather than employment or current expenditures. We find that, on average, state aid meets its purpose by inducing a sizeable and immediate increase in fixed tangible investment, measured by a change in fixed tangible assets over the previous period's stock. This effect is common across a variety of objectives but particularly pronounced for aid allocated to SMEs, and aid aiming to strengthen regional development and R&D. The results make sense. SMEs and firms receiving aid for regional development, on average, benefit more from State Aid because they are more likely to be financially constrained. Indeed, the impact of State Aid is particularly strong in Central and Eastern Europe. The weaker impact of aid with an environmental objective might be explained by a greater share of replacement investments, which would leave our measure of changes in assetsconstant.

The additional investments that State Aid generates should, in turn, raise productivity. However, we do not observe this effect over our two-year horizon following the approval of the aid. Instead, capital deepening unaccompanied by revenue gains lowers total factor productivity. Productivity declines are most pronounced for SMEs. One reason may be that firms with financial constraints would like to invest more but are unable to do so. As a result, their marginal productivity of capital would be higher before they receive the aid. Once State Aid helps loosen their financial constraints, productivity falls to its optimal level. The impact on employment of higher investment is ex ante ambiguous. We only observe a weak, statistically insignificant positive impact.

Our results suggest that state aid can raise investments but that effects on productivity are likely to take more time. Differences in marginal effects suggest that State Aid stimulates fixed tangible investment more when provided to financially constrained firms. Such firms are not uniformly distributed across the EU. This suggests that efficiency gains could be obtained from better coordination of funds within the EU.

Our paper relates to several papers. Most related to this study is Brandão-Marques (2024) that look at firm level effects of state aid using the same database but a different sample than we do. They focus on listed firms in Belgium, France, Germany, the Netherlands, Spain, and the United Kingdom and find no effects on firm investment, productivity and only minor and short-lived effects on employment. We look at a much larger set of firms and countries including those where firms could plausibly face higher credit constraints. We find that SMEs and Central and Eastern European countries drive the positive effects that we find on firm investment. When we constrain our sample to the listed firms in their set of countries, we also find no effects on firm investment. In that way, our studies complement each other in showing differential impacts state aid can have depending on firm characteristics and financial frictions they face.

Canzian et al. (2025) use state aid data from Spain and Italy to analyse the impact of Covid-19 aid on recipient firms. They focus on national registries that have the universe of all aid awards without a threshold, but such registries only exist for Italy, Spain and Poland. Using the same econometric approach as we do, they find significant benefits for micro-firms, including increased investments and mitigating effects on turnover decline due to Covid-19 shock. Our paper focuses instead on pre-crisis times of more standard measures of government intervention, on the effects of large state aid award, and includes 22 EU countries. Duso et al. (2021) conduct a retrospective study of state aid control in the German broadband market, finding that well-designed public subsidy schemes can enhance competition, increase coverage, and lower prices.

A number of influential studies have used regional-level data to evaluate the effects of EU structural funds allocated for regional development. Becker et al. (2010) and Becker et al. (2013) find that such transfers raise regional GDP and investment, but that these effects are highly heterogeneous—largely concentrated in regions with stronger institutions and higher human capital. In related work, Becker et al. (2012) show that transfer intensity frequently exceeds efficiency-maximizing levels and that a reallocation of funds could generate stronger aggregate growth. More recently, Becker et al. (2018) show that positive effects tend to fade once eligibility is lost, with growth reverting toward pre-treatment trends. These studies offer important insights into the aggregate effects of place-based transfers, and we complement this work by examining firm-level responses to state aid. Moreover, by analysing a broader set of countries and policy objectives, we are able to assess whether the impacts found in these regional studies generalise to other types of industrial support and geographic contexts.

2 Institutional setting

Industrial policies lie at the heart of the origins of European integration. Today, however, such policies are largely left to national authorities, with the Union focusing on a regulatory role rather than taking on an active role in shaping them.

The legal basis of the EU's regulatory framework is Article 107 of the Treaty on the Functioning of the European Union (TFEU), which generally prohibits state aid. State aid in EU law refers to selective advantages granted by national governments to specific firms or sectors that could distort competition, independently of its form (eg, grants, tax advantages, loans) and independently of the type of expenditure that is being supported (eg, investment or current expenditure).

The EU's strict control over state aid stems from its commitment to maintaining a level playing field in the Single Market and prevent wasteful subsidy wars. The Directorate-General for Competition (DG-COMP) oversees the enforcement of these rules, ensuring Member States do not undermine fair competition in the Single Market through public interventions that could favour some businesses over others.

Despite the general prohibition outlined in Article 107, the TFEU allows exceptions for specific cases where state aid is considered beneficial to broader societal or economic goals. These exceptions include aid for disaster relief, regional development, cultural and heritage conservation, and projects of common European interest. Over time, new exceptions have been added to reflect shifting priorities, such as promoting environmental sustainability and energy efficiency.

In practice, state aid remains a significant tool for national governments. Even with strict

EU regulations, Member States collectively spent an average of €77.5 billion annually on state aid between 2000 and 2019. This figure rose to €131.9 billion in 2019—equivalent to 0.94% of the EU's GDP. No breakdown is available by type of expenditure that is supported, but secondary law shows a clear preference for investment expenditure so that aid is primarily given out for capital expenditures (eg, GBER, Regional Aid Guidelines).

National governments can allocate state aid to firms through two primary mechanisms.

The first is de minimis aid, which refers to small-scale support not exceeding €200,000 per company over a three-year period. This type of aid is exempt from notification requirements and does not need to be reported to the European Commission. It is designed to reduce administrative burdens while allowing governments to provide limited, targeted assistance.

For aid exceeding this threshold, Member States must notify the European Commission. The Commission is responsible for assessing whether the proposed aid measure complies with EU rules and is compatible with the Single Market. When submitting an aid measure, governments must specify key details, including the instruments used (e.g., grants or guarantees), eligible sectors, maximum allocation amounts, the aid's objectives, and its timeframe. Certain objectives, such as environmental protection or energy efficiency, are typically fast-tracked for approval, while others may require more extensive investigation.

The data we use for our analysis come from these notification procedures and are managed by DG-COMP. These records offer a detailed view of allocation of large state aid awards across Member States [1]. We now turn to a more detailed explanation of the data sources and methodology used in our study.

[1] https://competition-policy.ec.europa.eu/document/download/8ede121f-56d4-4b81-9fa7-9f8b499e1554_en?filename=state_aid_procedures_factsheet_en.pdf

3 Data

3.1 State aid data

We use data on large state aid awards granted to undertakings across all EU Member States, mandated to be publicly available under EU law.

The backbone of our database is the *Transparency Award Module* (TAM)—an online portal run by the European Commission's Directorate-General for Competition (DG-COMP) that collects all awards above €500,000 for 23 of the 27 EU countries. For the remaining four Member States—Spain, Romania, Poland, and Slovenia, we use the equivalent national transparency registers, and harmonise their variables to the TAM format. Disclosure of these large awards is a mandatory part of the State Aid Modernisation programme, adopted in 2014 and implemented in July 2016. Failure to comply may lead to the Commission demanding the reinstatement of the aid (Commission and for Competition, 2016).

The disclosed data includes the name and identification number of the beneficiary, the objective of the award, the amount granted, the instrument used (e.g., grants, loans, guarantees), the authority granting the aid, and the identification number of the aid measure. Importantly, these reported amounts reflect aid *granted*, which does not necessarily imply that the aid was paid out in full or at all.

Although TAM and national registries focus on aid above €500,000, some Member States or local authorities report smaller awards. To ensure consistency across Member States and to minimise the likelihood that firms that received but did not disclose state aid become part of out control group, we restrict our analysis to aid exceeding this threshold [2]

Our analysis focuses on state aid distributed in 2017 and 2018, following the full implementation of the State Aid Modernisation programme. Aid granted during 2020 and

[2] An important question is which type and share of state aid we are missing by using the TAM data, which includes disproportionally large aid awards. To answer this question, we analysed the Italian national registry, which records all state aid regardless of size. Our findings suggest that the aid not reported in TAM are indeed below €500,000, primarily small-scale, often involving tax advantages for employment or training purposes. We provide the full analysis in Appendix 8.1.

beyond is excluded, as the COVID-19 pandemic led to a surge in aid allocations that differs significantly from pre-pandemic trends. Pandemic-related aid often overlapped with other disclosed objectives, such as SME support or green transition, complicating analysis of the impact of State Aid granted for specific objectives. Furthermore, companies that secured aid for non-crisis related measures were arguably different than those that receive aid in normal times. To maintain the common trends assumption critical to our generalised difference-in-differences approach, we focus on the relatively stable pre-pandemic period of 2017-2018.

State aid is categorised into objectives defined by the European Commission, including agriculture, environmental protection, research and innovation, regional development, and social support. For our analysis, we exclude aid unrelated to firm-level investment and performance, such as aid for agriculture, fisheries, natural disaster relief, cultural heritage conservation, services of general interest (e.g., childcare, postal services, social housing). We also exclude closure aid and rescue and restructuring aid, as these forms of support are rare and narrowly targeted at addressing firm-specific crises, such as coal mine closures. In our dataset, these categories account for just 87 instances above €500,000, primarily given out to coal mines in Poland.

3.2 Firm-Level Data

For the outcome variables, we use balance sheet and income statement data from Bureau van Dijk's Orbis database. These firm-level financial and operational variables are matched to the state aid database we construct. Details on the matching process for each country are provided in 8.2.

After the matching procedure, we retain data for 22 EU Member States: Austria, Belgium, Croatia, Czechia, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Lithuania, the Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, and Sweden. For the remaining five Member States[3], the national identifiers in the TAM database could not be matched to Orbis. The 22 countries included in our dataset represent 98.5% of the EU's GDP.

3.3 Matched Data

Out of a total of 20,228 undertakings that received state aid exceeding 500,000 EUR, we successfully matched 16,258 firms to Orbis, achieving a match rate of 80.37%. This leaves 3,970 aid recipients unmatched, who are therefore excluded from our detailed firm-level analysis. An overview of the matching rates by country, and details of the matching procedure for Germany, which proved more problematic, are in Annex **Section 8.2**.

After the matching, we filter out firms that do not have the variables that we use for our dependent variables as well as propensity score computations. Specifically, we exclude firms that do not have information on sector, employment, total and fixed assets, age, real value added, liquidity, debt, profit margins. We also drop any observations that have less than five employee on average. These steps leave us with 6,722 treated firms and 1,109,006 control firms.

While the dataset is rich and provides detailed information on state aid and firm-level outcomes, it has some limitations that could affect the analysis. First, we do not observe state aid below the €500,000 threshold, meaning that firms classified as part of the control group may have received smaller amounts of aid. Our findings should be interpreted as effects of *large aid awards*, as opposed to receiving aid. That said, it is possible that some larger awards that are close to the threshold are excluded from our dataset.

This omission has implications for potential bias in our estimates. If the treatment effect is consistently of the same sign across the distribution of aid amounts, the exclusion of smaller aid amounts would likely lead to a downward bias in our estimates, as we would be underestimating the overall effect of aid. On the other hand, if the treatment effect is

[3]Bulgaria, Cyprus, Latvia, Luxembourg, and Malta

heterogeneous, with smaller aid amounts having a negative effect that becomes positive as aid amounts increase or vice verse, our estimates could be subject to an upward bias in the absolute values. While the first case seems more plausible, we would need to be cautious with interpretation for this reason.

Another limitation arises from the lack of treatment status information prior to July 2016, when the State Aid Modernisation programme was implemented. As a result, we cannot observe whether firms classified as treated in our analysis had already received aid in the pre-treatment period. If firms that received aid after 2016 had also benefited from earlier aid, their outcomes in the pre-treatment period might reflect the effects of this prior support, violating the common trends assumption. We would ideally start our sample in 2017 and start the analysis of state aid awards that come 2-3 years after. Due to Covid crisis however, this is not a possibility.

Despite these limitations, the structure of our data and methodology mitigates the potential biases to some extent. The large control group helps dilute the impact of any unobserved aid among control firms. Moreover, the ability to examine pre-treatment differences in trends would allow us to see if the fact that we do not observe pre-2016 period is in fact a major concern.

4 Effectiveness of State Aid

4.1 Key variables

Our key outcome variables of interest are firm investment, employment growth, and productivity.

The firm investment rate is constructed as:

$$inv_{it} = \frac{\text{tangible assets}_{it} - \text{tangible assets}_{i,t-1}}{\text{tangible assets}_{i,t-1}} \tag{1}$$

where assets_{it} denotes the firm's tangible fixed assets at time t. These include tangible fixed assets, excluding current and financial assets. The investment rate, therefore, measures the growth in fixed assets relative to the previous year's assets.

We define the employment growth rate as:

$$emp\_growth_{it} = \frac{\text{nr employees}_{it} - \text{nr employees}_{i,t-1}}{\text{nr employees}_{i,t-1}} \tag{2}$$

where employees_{it} represents the number of employees at the firm in year t.

To measure productivity, we compute the log of total factor productivity (TFP) as the residuals from a Cobb-Douglas production function in which the coefficients for capital and labour are allowed to vary by sector. Specifically, we estimate:

$$\log(y_{ist}) = \alpha_s + \alpha_s^k \log(k_{ist}) + \alpha_s^l \log(l_{it}) + \varepsilon_{ist} \tag{3}$$

where y_{it} denotes the firm's value-added, k_{ist} represents capital, l_{it} is labour input, and ϵ_{ist} is the residual. The coefficients are sector-specific, accounting for differences in production technology across industries. The residuals ε_{ist} are then used as the firm-level log TFPR.

For state aid reception, we construct a binary indicator variable that equals one if a firm receives state aid with an aid component exceeding €500,000, and zero otherwise. While we observe the amount of aid granted in the dataset, we choose to use an indicator variable rather than the continuous aid amount for two main reasons.

First, the dataset may not reliably capture the actual size of the aid relevant to the firm. Each award is associated with two values: the nominal amount and the aid component. However, only the aid component is consistently reported, as the nominal amount is not required for submission. Furthermore, calculating the aid component can be challenging, particularly for complex instruments such as guarantees, repayable advances, interest rate subsidies, or tax advantages. Unlike grants, where the aid component is straightforward, these instruments require assumptions about project success probabilities and other financing sources enabled by the aid. Since the calculation process varies across the thousands of granting authorities in the EU, we cannot verify the accuracy or consistency of the reported aid components.

Second, even if the aid component were consistently and correctly computed, it varies across instruments. While the aid component may be comparable within a single instrument type, our analysis must account for a wide range of instruments, which vary significantly in their prevalence across countries. Using a binary indicator allows us to abstract from these differences and focus on the broader impact of receiving state aid.

4.2 Identification strategy

Our goal is to estimate the causal effect of receiving state aid on firm performance. In the context of industrial policy, there is an inherent selection into treatment. In the EU, governments typically design aid measures to target specific sectors or types of activities. Firms apply for aid under these measures, and their applications are assessed and approved by the relevant authorities. As a result, both the decision to grant aid to a firm and the amount of aid granted are inherently selective. Our empirical strategy must account for this selection into treatment.

To address this, we rely on a generalised difference-in-differences design with staggered adoption, also referred to as an event study design. Specifically, we estimate the following regression model:

$$y_{it} = \alpha + \sum_{k=-5}^{2} \beta_k \, \mathrm{Aid}_{ik} + \phi_i + \theta_t + \varepsilon_{it}, \tag{4}$$

where y_{it} represents the outcome variable of interest, such as investment rate, employment growth, or TFP; ϕ_i denotes firm fixed effects, and θ_t represents year fixed effects. The variable Aid_{ik} is a binary indicator equal to one if it is k years since the firm received aid, and zero otherwise. For the control group, Aid_{ik} is always zero. The coefficients $\beta_{k\geq 0}$, capture the causal effects of state aid at different time periods relative to treatment, whereas $\beta_{k<0}$ capture differences in trends in outcomes prior to treatment.

For $\beta_{k\geq 0}$ to represent the causal effects of state aid on firm performance, certain assumptions must hold. These assumptions fall into two broad categories. The first set concerns the counterfactual: how firm outcomes would have evolved in the absence of state aid. The second category relates to the aggregation of heterogeneous effects into the coefficients on relative time dummies, which has been extensively studied in recent literature on difference-in-differences with staggered adoption. The fact that we have only two years of treatment and a very large control group alleviates concerns on negative weights. For robustness, we have conducted the analysis with the estimators that tackle this problem such as the event study estimator developed by Sun and Abraham (2021), and find no differences in results across our specification and theirs.

Below, we state the key assumptions for identifying the causal effects, adopting the terminology and expressions from Wooldridge (2021).

Assumptions on the Counterfactual

Assumption 1 (Conditional No Anticipation, Staggered) *Conditional on treatment and control variables, there are no differences in outcomes between treated and never-treated firms prior to treatment. Specifically, for cohorts r (where $r = \infty$ represents the never-treated group) and cohort indicators d_r, we require:*

$$\mathbb{E}[y_t(r) - y_t(\infty)|d_r = 1, \mathbf{x}] = 0, \quad t < r \tag{5}$$

Assumption 2 (Conditional Common Trends, Staggered) *The average trend in untreated potential outcomes does not depend on treatment status. Formally, with $\mathbf{d}$ the vector of cohort dummies:*

$$\mathbb{E}[y_t(\infty) - y_1(\infty)|\mathbf{d}, \mathbf{x}] = \mathbb{E}[y_t(\infty) - y_1(\infty)|\mathbf{x}], \quad t = 2, ..., T \tag{6}$$

The first assumption ensures that, conditional on covariates, there are no differences in the outcomes of treated and never-treated firms in the pre-treatment period. This assumption pertains to realised outcomes and rules out systematic differences between the treated and control groups before treatment. The second assumption pertains to counterfactual outcomes, requiring that untreated potential outcomes follow the same time trends across treatment groups and the never-treated group. While the first assumption addresses realised pre-treatment outcomes, the second assumption concerns unobserved counterfactual trends.

Pre-Treatment Trends and Anticipation Effects. The dynamic regression model allows us to test for differences between the control and treated groups in the pre-treatment period. If significant differences exist right before the treatment kicks in, this would suggest violations of the no-anticipation assumption, as firms may adjust their investment or employment behaviour in anticipation of receiving aid. By examining pre-trends, we can evaluate the plausibility of the no-anticipation assumption and determine whether treated and control firms are on comparable trajectories before treatment, which we do.

Common Trends Assumption. The common trends assumption, by contrast, is inherently untestable since it concerns counterfactual outcomes that are never realized. However, the absence of pre-treatment differences in trends lends credibility to the assumption. Furthermore, we include firm fixed effects (ϕ_i) in our model to control for time-invariant characteristics, such as sector, size, and location, that may influence both treatment selection and outcomes.

We argue that the large number of countries in our sample aids identification, as firms in the control group from one country serve as plausible counterfactuals for treated firms in another.

One concern is the presence of unobservable shocks contemporaneous to aid reception, such as demand or supply changes, that affect both the likelihood of receiving aid and firm outcomes. If aid granting authorities target firms experiencing these shocks, this could mean that the common trends would not have hold in the absence of state aid even if we do not observe any pre-treatment trend differences. To address this, we include sector-year and country-year interaction terms in the regression to account for time-varying shocks that are specific to a sector or country to complement our baseline analysis. We run this robustness check with a random subsample of our main sample due to computational burden.

By including firm fixed effects, year fixed effects, and sector- and country-specific time interactions, our approach accounts for many potential sources of bias. The dynamic specification allows us to assess pre-treatment trends and detect any systematic differences before aid receipt. While we cannot fully eliminate concerns related to unobservable shocks, our identification strategy is robust to several common threats and provides credible estimates of the causal effects of state aid on firm performance.

4.3 Propensity Score Matching and Sample Construction

Whilst our difference-in-differences design with firm and year fixed effects controls for time-invariant firm characteristics and aggregate time trends, we further strengthen our identification strategy through propensity score matching. This approach serves two purposes: first, it ensures that our treated and control firms are comparable on observable pre-treatment characteristics; second, it restricts our analysis to the region of common support where treated and control firms have overlapping propensity score distributions.

Matching Procedure For our main specifications, we calculate propensity scores for the full sample using all available firms across the 22 EU Member States. We estimate separate propensity scores for six treatment definitions: general treatment, environmental aid, R&D aid, regional aid, SME aid, and sectoral aid. In all of these cases, we restrict the treatment to firms that received awards exceeding €500,000 under a measure.

For each treatment definition, we estimate propensity scores using a logistic regression model. The propensity score model takes the form:

$$\Pr(D_i = 1|\mathbf{X}_i) = \Lambda(\mathbf{X}_i'\boldsymbol{\beta}) \tag{7}$$

where D_i is the treatment indicator, $\mathbf{X}_i$ is a vector of pre-treatment covariates, and $\Lambda(\cdot)$ is the logistic cumulative distribution function.

The vector of matching covariates $\mathbf{X}_i$ includes firm characteristics measured as averages over the pre-treatment period (2013–2016): firm age, firm size (categorical indicators based on employment), industry (NACE Rev. 2 one-digit sector), log of employment, log of total assets, log of revenue value added, debt-to-total-assets ratio, liquidity ratio, and profit margin percentage.

We use subclassification matching to generate weights that we subsequently use in our regression analysis.

In addition to the sample-wide propensity scores, we recognise that institutional contexts, administrative capacity, and the design of state aid measures vary considerably across EU Member States. To account for this heterogeneity, we calculate country- or regional group-specific propensity scores to use then in our country-specific regressions that we do later for robustness.

We define country groupings based on sample size as well as geopgraphic and institutional similarity. Countries with at least 100 treated firms in the main objectives are treated as separate groups (e.g., Hungary, Spain, Portugal, Poland, Czech Republic, Romania, Italy, Germany, Sweden). Countries with smaller samples are grouped by region: CEE small (Slovakia, Croatia, Slovenia, Estonia, Lithuania), Western (France, Belgium, Netherlands, Austria, Ireland), and Nordic (Finland, Denmark).

Common Support and Sample Trimming An important concern is to have comparable treated and control units. To address this, we impose a common support restriction. Specifically, we trim the sample by dropping control firms with propensity scores below the 1st percentile of the treated firms' propensity score distribution, and dropping treated firms with propensity scores above the 99th percentile of the control firms' propensity score distribution.

This trimming procedure ensures that we only compare treated firms to control firms with similar observable characteristics. It removes extreme propensity score values where the overlap between treatment and control is limited, thereby reducing bias from comparing fundamentally different types of firms.

We apply this trimming procedure separately for each treatment definition. In our main analysis using general treatment, we trim based on the general propensity score. When we analyse objective-specific effects (e.g., environmental aid only), we trim based on the objective-specific propensity scores. When we estimate effects within country or regional groupings, we trim based on the country-specific propensity scores.

In our regression specifications, we incorporate the propensity score matching in two ways. First, we weight observations using the subclassification weights generated by the matching procedure, which ensures that the treated and control groups are balanced on the vector of observable covariates $\mathbf{X}_i$. Second, we restrict the sample to the region of common support by dropping observations that fall outside the trimmed propensity score range, as described above.

4.3.1 Propensity score results: selection into treatment

To estimate the likelihood of firms receiving State Aid—our treatment variable—we implemented a series of propensity score regressions using a unified specification across four policy objectives: overall aid, environmental aid, R&D aid, and regional aid. The analysis was conducted on firm-level data aggregated up to the year 2016, ensuring temporal consistency and avoiding post-treatment bias.

The right-hand side (RHS) of the regression equations was standardised across all models to ensure comparability. It included firm age (and its square), size category, sector (based on 1-digit NACE codes), regional development status, listing status, capital intensity (logtransformed and winsorised), value added, debt ratio (capped at 1), liquidity (current assets - stocks, relative to current liabilities), and profit margin. Winsorisation at the 1st and 99th percentiles was applied to key financial ratios to mitigate the influence of outliers.

We estimated the model using logistic regressions with White robust standard errors. The resulting propensity scores represent the estimated probability of treatment for each firm under each policy objective.

Term	Environment	Overall	R&D	Regional
Age	-0.032***	-0.028***	-0.027**	-0.006
Áge^2	0.001***	0.000***	0.000	-0.000
Debt (% of total assets)	-0.865***	-0.787***	-0.960***	-0.630***
Liquidity	-0.078***	-0.051***	-0.012	-0.081***
Profit margin (% of turnover)	-0.018***	-0.004**	-0.019***	0.028***
Listing status: Listed	-0.179	0.521**	0.462	1.066**
Listing status: Unlisted	0.548*	0.387**	-0.204	0.956**
Log real value added	0.563***	0.521***	0.537***	0.324***
Log capital intensity	0.587***	0.336***	0.189***	0.294***
Cohesion region type: Transition	-0.277*	-0.857***	-0.540***	-0.947***
Cohesion region type: More developed	-0.303***	-1.425***	-0.640***	-2.672***
Agriculture	-0.045	0.599***	-0.685**	1.611***
Fishing	-0.482***	0.758***	0.732***	1.887***
Mining	0.269	0.738***	0.470*	0.884***
Manufacturing	-1.776***	-0.799***	-1.077***	0.248
Utilities	-2.103***	-0.072	-1.101***	1.365***
Construction	-2.177***	-0.046	0.404	0.469*
Trade	-2.366***	0.079	0.937***	0.528*
Hotels, restaurants	-2.606***	-0.863***	-0.732**	-0.054
Transport	-1.999***	-0.176	-2.919**	0.805**
Small	0.119	0.714***	0.761***	1.154***
Medium	1.085***	1.388***	1.561***	1.972***
Large	1.412***	1.706***	1.814***	2.366***

Table 1: Impact of firm characteristics on receiving State Aid, by type of aid. For Cohesion classification, the reference group is the less developed regions, for the sector, Other Services, and for the size group, the reference is the micro enterprises.
Significance levels: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 1 summarises the regression output. Several factors consistently influence the likelihood of treatment across policy objectives. Younger firms and those with lower liquidity and debt ratios are less likely to receive aid, while larger firms show a significantly higher probability of selection. Sectoral affiliation plays a notable role: firms in agriculture, fishing, and mining are more likely to be treated, whereas those in manufacturing and utilities tend to be less likely. Regional development status also matters, with firms in transition or more developed regions showing lower treatment probabilities. Evidently this effect is particularly strong for State Aid granted with the objective of regional development. More capital intensive firms, and those in mining, are more likely to receive state aid for environmental purposes, including decarbonisation. Listed status is positively associated with aid receipt in some models, particularly for regional and R&D objectives. Overall, the results suggest that firm size, sector, and financial structure are key determinants of selection into treatment.

5 Empirical Results

This section quantifies the short-run effects of large European state-aid awards on firm-level outcomes. We begin by pooling all aid objectives to establish an average treatment effect. Section 5.2 then explores heterogeneity across the five principal aid objectives under which we observe most aid awards, which are environmental aid, R&D, and regional development aid.

Throughout, coefficients are normalised to zero at $k = -1$. The remaining coefficients measure deviations from the outcome level differences observed one year before the aid award.

5.1 Baseline Results

Our baseline results show significant and large positive effects on firm investment in tangible fixed assets. Firms that received aid invest 5.8 percentage points more in fixed assets in the year in which they receive aid, and 7 percentage points more in the following year. Two years after the aid reception, we observe that the investment rate is back to the same trend as in the control group, although it is important to note that a part of it is due to the baseline effect of the previous year's capital accumulation being higher. The coefficients on the years prior to the aid reception for firm investment are not only insignificant, but also very close to zero. These results are robust to different propensity score calculations as well as leaving them out. Figure 1 shows the pre and post treatment coefficients. The regression tables are in the **Appendix 8.3**

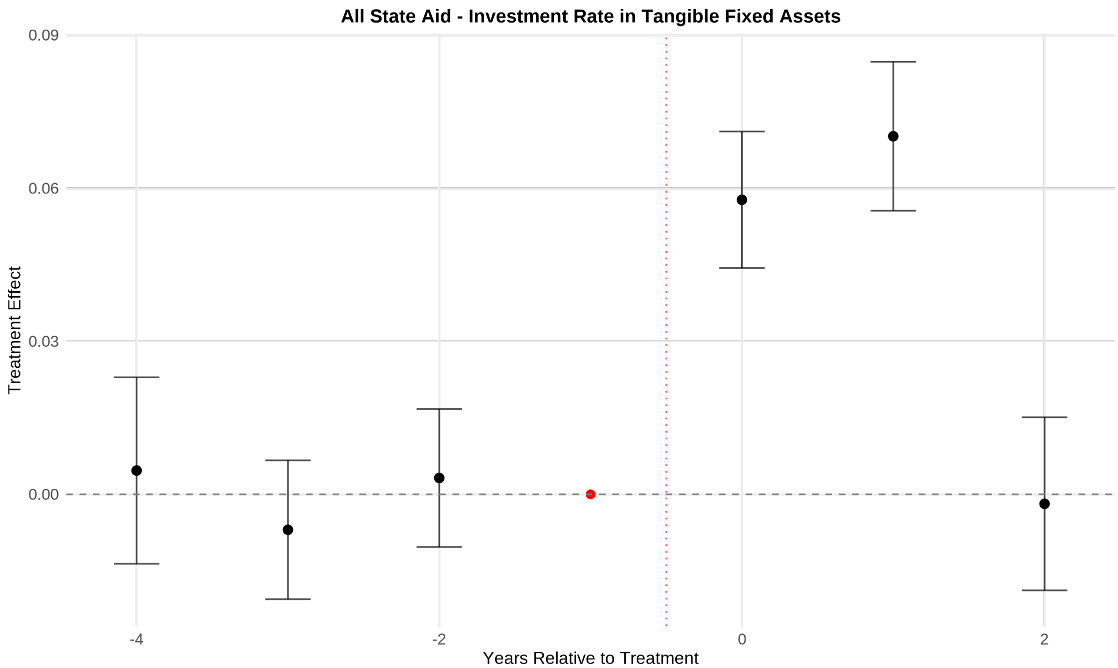


Figure 1: Changes in firm level investment in tangible fixed assets for all state aid awards

We do not see any strong effects on employment growth: the coefficients are both insignificant and an order of magnitude smaller than the coefficients for investment rate. This finding is not surprising given that our focus is on large aid awards that are often given for capital investments. However, one goal often cited in state aid measures is job creation. Our results do not give support to the claim there are spillovers to job creation that follow from state aid even if the focus is not employment.

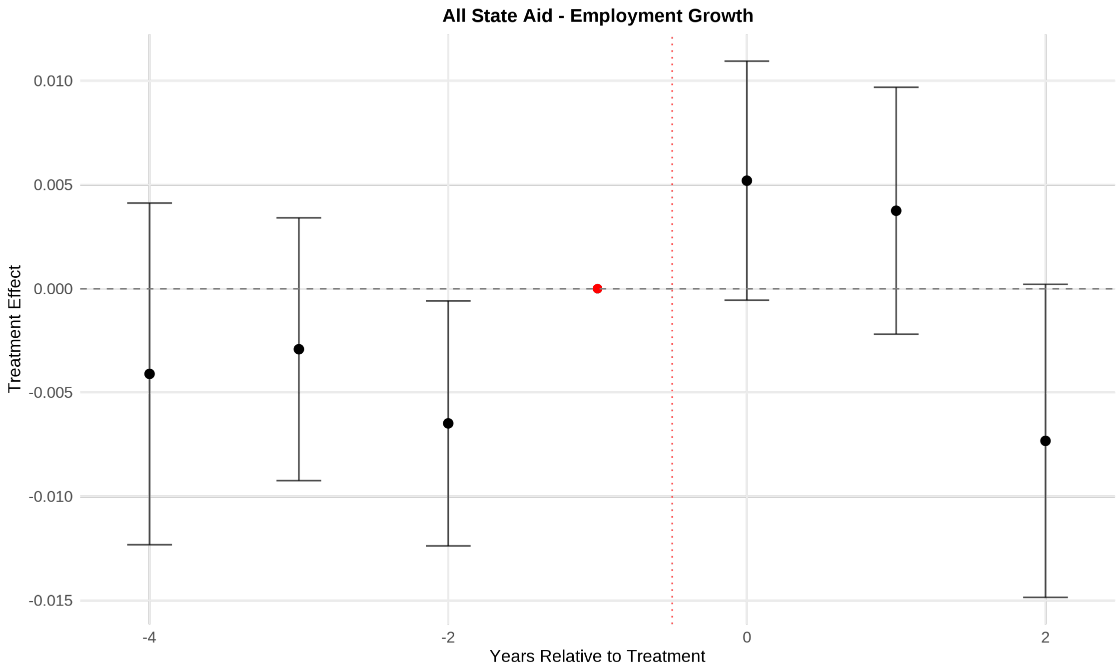


Figure 2: Changes in employment growth rates for all state aid awards

Finally, we see a sizeable and persistent decline in log TFPR of the firms that receive aid as shown in Figure 3. The results imply that the aid recipients' productivity measured by revenue declines by 1 to 3 percentage points in the years following aid reception. Taken together with our findings on firm investment, this result suggests that the firms are not able to increase their revenue commensurately to their capital stock, at least on the short run. We run regressions on revenue divided by total labour force and then revenue divided by total capital, and we confirm that the decline is entirely due to the decrease in the latter.

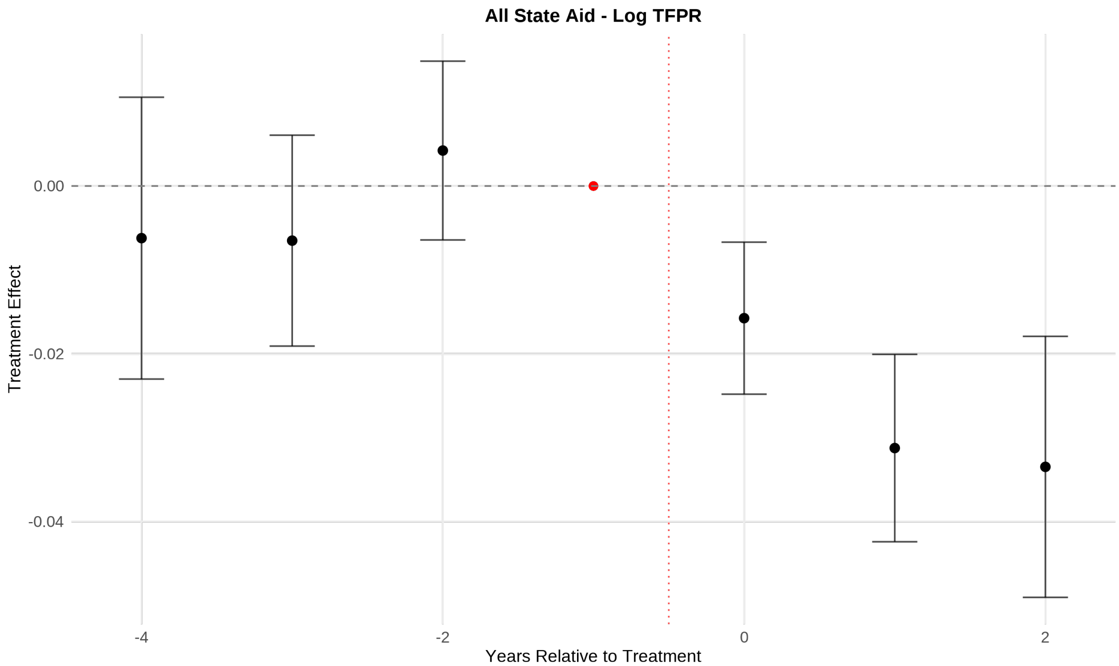


Figure 3: Changes in log TFPR for all state aid awards

The full regression results can be seen in Table 5 in the Appendix.

5.2 Differential Effects by Aid Objectives

Next, we analyse the effects of state aid for broad objective categories. We use the mapping provided by the DG-COMP to match granular aid objectives to broader themes of regional development, research & development, environmental aid including energy savings, sectoral development and SME support. Since there were virtually no employment and training aid above the threshold, we exclude these two themes. As it can be seen from Table 2, there are also fewer firms that received aid for sectoral development and SME support. Sectoral development comprises of very large state aid awards and are therefore rarer. Examples include aid for broadband infrastructure, and aid for airport or port infrastructures. SME aid on the other hand consists of smaller aid amounts, hence only a small share of SME aid crosses the threshold of reporting. We nevertheless report the results for these two objectives as well for completeness, but would caution against drawing any policy conclusions from these results due to small sample size.

Table 2: Number of Treated Firms by State Aid Objective

State Aid Objective	Number of Treated Firms
Regional Development	2,530
Research & Development	2,138
Environment	1,725
Sectoral Development	350
SME Support	214

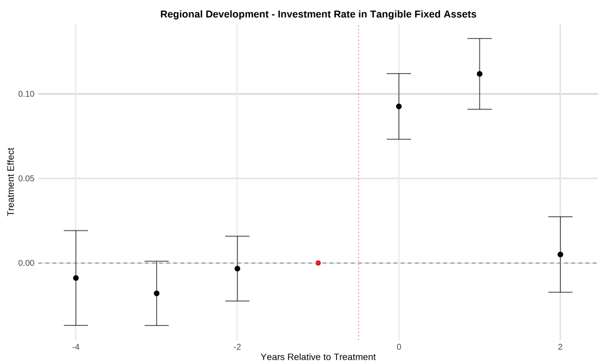
Note: Some firms received aid under multiple objectives and appear in multiple categories.

The breakdown of results by objectives reveal that regional development programmes and R&D aid drive most of the results for the increases in firm investments and decline in TFPR. Firms that received R&D aid increased their investment in fixed assets by 3 and 4 percentage points in the years following aid reception. This increase in investment is accompanied by 2 percentage point decline in firm productivity based on revenue in those two years. Since R&D projects take long time to materialise, it is not unexpected that the capital investments made by these firms are not immediately turned into revenue increases.

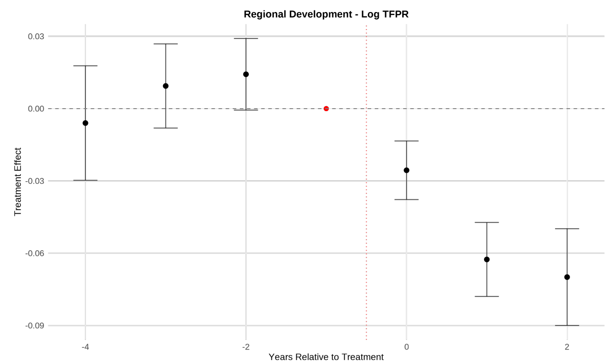
Regional development programmes have the largest effects on firm investments, with aid recipients investing 9 and 11 percentage points more than the control group in the first two years. However, the decline in TFPR is also large and significant, with negative effects persisting for three post-treatment years we consider. One would have expected that investments carried out under regional development programmes would materialise their effects faster than R&D projects. The fact that the decline is significant and persistent raises the question of whether state aid distorts the capital demand of the firms suboptimally.

On the other hand, we do not see any meaningful effects for sectoral development, environmental aid and SME support. As mentioned above, there are few instances of SME support and sectoral development aid, hence our null results should not be taken as conclusive for the effects of such programmes.

As for environmental aid, it is not necessarily a negative sign that there are no effects

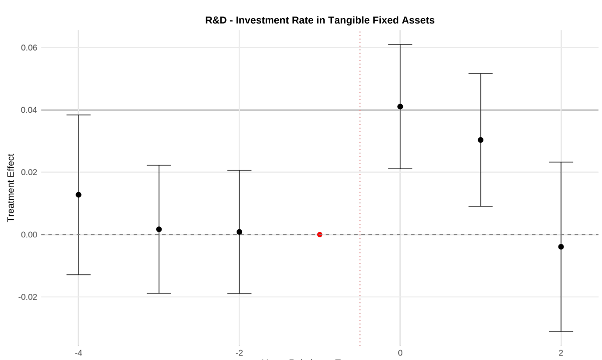


(a) Regional Development: Investment in Tangible Fixed Assets

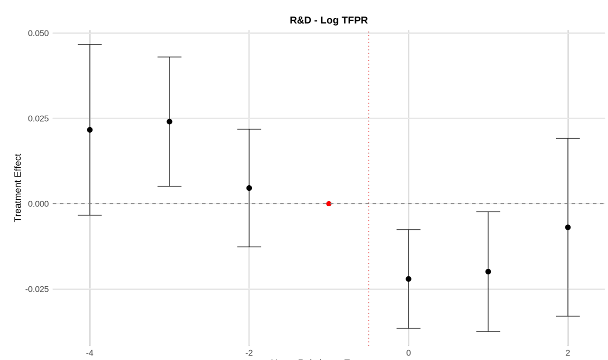


(b) Regional Development: Log TFPR

Figure 4: Event Study Results for Regional Development Aid



(a) R&D Aid: Investment in Tangible Fixed Assets



(b) R&D Aid: Log TFPR

Figure 5: Event Study Results for Regional Development Aid

on the outcomes we consider here. Indeed, the key aim of environmental aid is not to boost investment, revenue or employment but to decrease emissions, and switch to alternative means of production. If anything, one could anticipate a negative effect on the outcomes we consider here. Our results suggest that firms that received environmental aid fared just as well as the control group in these dimensions.

5.3 Country-Specific Analysis

To examine heterogeneity in state aid effectiveness across countries and to conduct a robustness analysis where treated firms are compared to firms in the same country or regions, we conduct separate event study analyses for each country or regional group. This approach addresses two key concerns. First, it allows us to compare treated firms with control firms from the same institutional and economic environment, reducing concerns about unobserved country-specific co-founders. Second, it helps identify whether the pooled estimates mask substantial variation in treatment effects across countries.

We compute propensity scores separately within each country or regional group rather than using a pooled specification with country interactions. This approach ensures proper common support and covariate balance within each country, as the distribution of firm characteristics and the propensity to receive aid may differ substantially across countries.

Our sample selection proceeds as follows. We first assess whether individual countries have sufficient numbers of both treated and control firms to support stand-alone analysis. Countries meeting this threshold are analysed separately: Czech Republic, Germany, Hungary, Italy, Poland, Romania, Spain, Portugal, and Sweden. For smaller countries with limited sample sizes, we create regional groups to maintain statistical power: Western European countries (France, Belgium, Netherlands, Austria, Ireland), Nordic countries (Finland, Denmark), and other Central and Eastern European countries (Slovakia, Croatia, Slovenia, Estonia, Lithuania). This results in 12 country/regional groups.

For the three most populated objectives, environmental, R&D, regional development, and each country/region, we estimate the propensity score using the same set of pre-treatment covariates as in the pooled analysis. We then trim the sample to ensure common support by dropping treated firms below the 1st percentile and control firms above the 99th percentile of the propensity score distribution. For sectoral development and SME support, as the total number of awards in these categories i mall, we do not have the power to estimate 12 different regressions.

Full regression tables for all country-objective combinations are not included in the present paper as they consist of 108 separate regressions, but they are available at request. Here, we present an overview of key findings.

Environmental Aid Unlike in the pooled regressions, we find consistent positive effects on investment in some countries, particularly in Germany, where we observe sustained increases of 4.6 to 6.2 percentage points across all post-treatment periods. Hungary and Portugal exhibit even larger effects though these are less persistent, and limited to one year. Czech Republic and Poland display modest effects of 11.3 and 7.2 percentage points respectively in the year following the aid reception.

In contrast, environmental aid shows limited effects on employment across all countries, with no significant positive impacts observed, whereas the productivity effects are mixed. Southern European countries and Western European countries show largely null results for all outcomes.

R&D Aid shows positive investment effects in several Central and Eastern European countries such as Poland, Hungary and Romania, with effects ranging from 10 to 16 percentage points in Hungary and Poland and reaching 44.6% percentage point in Romania. Interestingly, whereas we had found consistent decline in productivity in our pooled regressions for all aid, the decline in productivity is either short-lived and small as in Poland and Hungary, and close to zero and insignificant for Romania. We also observe positive employment effects in remaining CEE countries and Hungary.

Regional Development Aid generates the largest and cleanest investment effects, particularly in Central and Eastern Europe. Poland exhibits the most robust results with investment increases of approximately 19 percentage points both in the year firms receive aid and the following year. Romania shows the highest magnitude effects with investment increasing by 24 and 32 percentage points in the first two years post-treatment, along with positive employment gains of 5 and 4 percentage points though these come with significant productivity declines of magnitude of 7 to 12 percentage points..

Other CEE countries demonstrate substantial investment gains, particularly one year after receiving aid with an increase in investment of 27 percentage points. Hungary's results show strong investment effects of 15 percentage points and are accompanied by productivity declines. Germany shows modest effects of 8 percentage points only in the year following aid reception.

Expectedly, Sweden, other Nordic countries and Western countries exhibit null effects as these countries do not have a high concentration of regional development aid. All Southern European countries, Italy, Spain, Portugal, also exhibit null effects.

Several patterns emerge from this heterogeneity analysis. First, investment effects are generally strongest and most consistent for regional development aid. Second, Central and Eastern European countries consistently show larger treatment effects than Western or Southern European countries across all three objectives. Third, employment effects are generally small or null across all objectives and countries, with the notable exception of positive employment gains from regional ad R&D aid in Romania and Hungary.

The geographic concentration of effects in CEE countries likely reflects both institutional factors as these countries were primary targets of EU Cohesion Policy during this period and economic factors, including lower baseline capital stocks, higher financial frictions, and greater potential for catch-up growth.

5.4 Heterogeneity by firm characteristics

We then analyse the effects of state aid separately by firm size. Specifically, we define SMEs as firms that had a pre-2017 average employment below 250. We run the regressions separately for the sample of SMEs and large firms. The full results are in Appendix 8.4.

The key observation is that SMEs respond much more strongly to state aid than large firms. For general aid, SMEs increase their investment rate by 7.3 and 8.6 percentage points in the first two years following aid receipt, compared to only 1.7 percentage points for large firms, which quickly dissipates. SMEs also exhibit a 0.8 percentage point increase in employment growth in the year following aid reception, while large firms show no significant employment response. In terms of TFPR, both groups experience similar declines of approximately 3–4 percentage points, despite the substantially stronger investment effects for SMEs.

When we break down the results by objectives, we find that R&D aid is effective in boosting investment only for SMEs, with no significant effects for large firms. Regional aid generates the largest effects, spurring investment increases of 12.2 and 15.3 percentage points for SMEs in the first two years following aid receipt, alongside modest employment gains of 1.2–1.3 percentage points. Notably, environmental aid shows no significant investment effects for either group.

An important observation is that across multiple objectives, large firms exhibit significant negative pre-trends in investment, and sometimes employment, whereas SMEs generally do not. These pre-trends raise selection concerns for large firm recipients but provide greater confidence in the causal interpretation of the SME results.

6 Discussion

The findings of this study provide nuanced insights into the effectiveness of large state aid awards in the European Union. The most robust and consistent effect observed is a significant increase in fixed tangible investment among recipient firms, particularly SMEs and those located in regions targeted for development. This supports the view that state aid can alleviate financial constraints and stimulate capital formation where market failures are most acute. However, the absence of corresponding short-term gains in productivity and employment suggests that the benefits of increased investment may not be immediately realised in broader firm performance metrics. This lag may reflect the time required for new capital to be integrated into productive processes or for innovation-driven investments to yield measurable returns.

A key aspect emerging from the heterogeneity analysis is the variation in state aid effectiveness across countries and firm types. Central and Eastern European countries, where firms tend to face greater financial frictions and lower baseline capital stocks, exhibit the strongest investment responses. Similarly, SMEs benefit more than larger firms, both in terms of investment and, to a lesser extent, employment. These findings highlight the importance of targeting in state aid policy: directing resources towards firms and regions with the greatest constraints can maximise the marginal impact of public support. Conversely, the limited effects observed in more developed Western and Southern European countries may indicate diminishing returns to state aid in contexts where financial markets are deeper and firms are less constrained.

The lack of short-run productivity gains, and in some cases even declines in measured TFP, warrants careful interpretation. One plausible explanation is that capital deepening outpaces revenue growth in the immediate aftermath of aid receipt, temporarily depressing productivity ratios. It is also possible that the types of investments supported—such as replacement of existing assets or compliance-driven upgrades—do not translate directly into higher output or profitability in the short term. These dynamics underscore the need for a longer-term perspective when evaluating the success of state aid interventions.

The present analysis has several limitations. First, the focus on large aid awards (above €500,000) means that the results may not generalise to smaller-scale interventions, which are more prevalent but less systematically reported. Second, the inability to observe pre-2016 treatment status introduces potential bias if firms classified as treated had previously received aid. Third, the relatively short post-treatment window (two years) may be insufficient to capture the full trajectory of productivity and employment effects.

In ongoing research, we are examining additional factors that may shape the effectiveness of state aid, including the size of aid thresholds, the quality of governance, and the level of government responsible for allocation, and which may lie at the basis of the substantial heterogeneity that we observe across countries. The proportion of financial support relative to total investment expenditure can influence recipient firms' incentives: while higher subsidy rates may, in some cases, dampen the drive to ensure project success, they may also be essential for enabling investment when financial constraints are particularly severe. The quality of governance—both in the region where the granting authority is based and where recipient firms operate—is likely to play a crucial role, with stronger institutions expected to enhance the responsiveness of firms to state aid. Furthermore, the administrative capacity of local governments may affect project selection: while local authorities might face resource constraints in identifying and managing eligible projects, they may also possess a more nuanced understanding of local investment needs, potentially leading to more effective targeting of aid.

7 Conclusion

This paper provides new evidence on the short-run effects of large state aid awards on corporate investment, employment, and productivity across 22 EU Member States. The results demonstrate that state aid is effective in stimulating fixed tangible investment, especially among SMEs and in regions with greater financial constraints. However, these investment gains do not translate into immediate improvements in productivity or employment, suggesting that the benefits of state aid may take longer to materialise and may depend on complementary factors such as the size of aid thresholds, the quality of governance, and the level of government responsible for allocation.

The policy implications are twofold. First, enhancing the targeting of state aid both geographically and by firm type can increase its effectiveness and efficiency. Second, greater EU-wide coordination of subsidy programmes could help mitigate the risk of subsidy races and ensure that resources are allocated where they yield the highest returns.

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8 Appendix

This appendix provides supplementary analyses and methodological details to support the main findings of the paper. It first assesses the representativeness of the EU Transparency Award Module (TAM) database in **Section 8.1** for Italy, which is the only country that publishes all state aid awards in a national register in addition to publishing in TAM. For Italy, we conclude that TAM is only missing a small proportion of the awards above €500,000. **Section 8.2** shows the matching results and provides background on the matching of the State Aid awards to ORBIS for Germany. **Section 8.3** presents baseline event-study results on the impact of state aid on firm outcomes, followed by disaggregated analyses by firm size and policy objective, including environmental protection, R&D, regional development, sectoral programmes, and SME support, in **Section 8.4**.

8.1 Contrasting TAM with Italian national registry

EU law mandates Member States to report all state aid above €500,000. 23 countries are publish through the *Transparency Award Module* (TAM) database. TAM has the potential to be a valuable source of information for researchers to conduct analysis of EU-wide industrial policies, but the reporting threshold raises concerns on its coverage. Here, we assess completeness and representativeness of the TAM by comparing it with Italy's *Registro Nazionale degli Aiuti di Stato* (RNA), a comprehensive national registry of all state aid awards. Italy's dual reporting system allows for a best-case assessment of TAM's quality and coverage.

Overall Coverage Our comparison between TAM and the Italian National Registry reveals that TAM captures only a very small fraction of total aid awards, but that the small number of awards it contains represents a large share of the total state aid expenditure. Figure 6 shows that TAM captures only 1.8% of total aid awards on average across 2017-2023. This very low count coverage confirms that the vast majority of state aid awards in Italy fall below TAM's reporting requirements, consistent with the prevalence of small-scale support for SMEs and de minimis aid schemes.

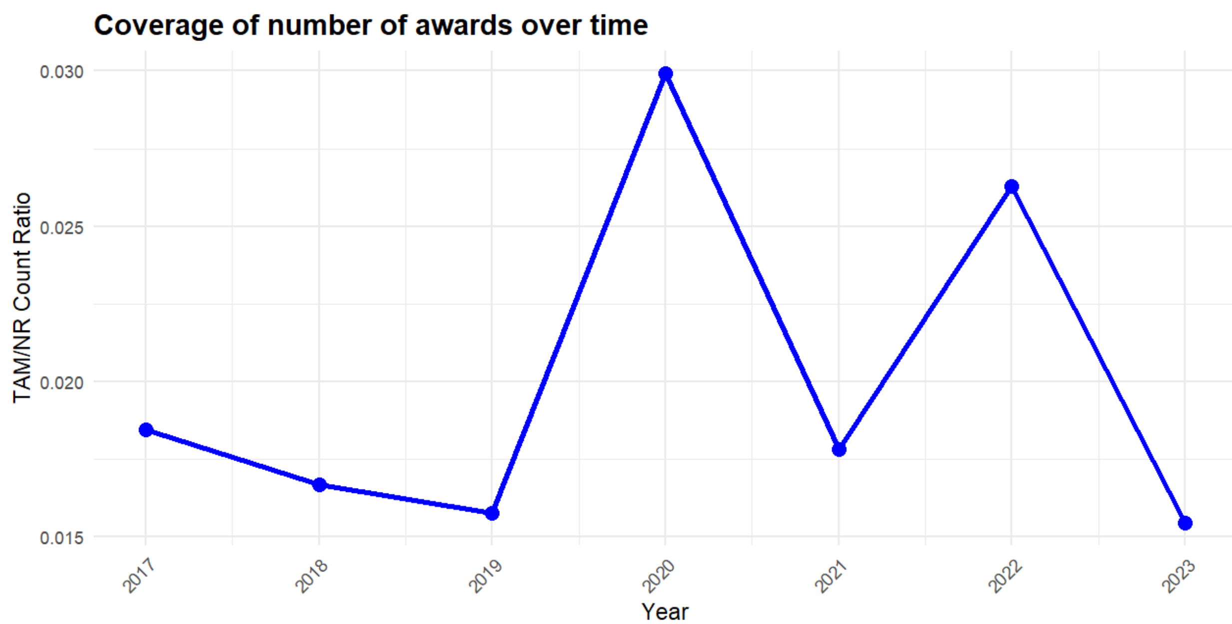

Figure 6: Share of total number of awards in TAM as a fraction of all the state aid awards in Italy over time

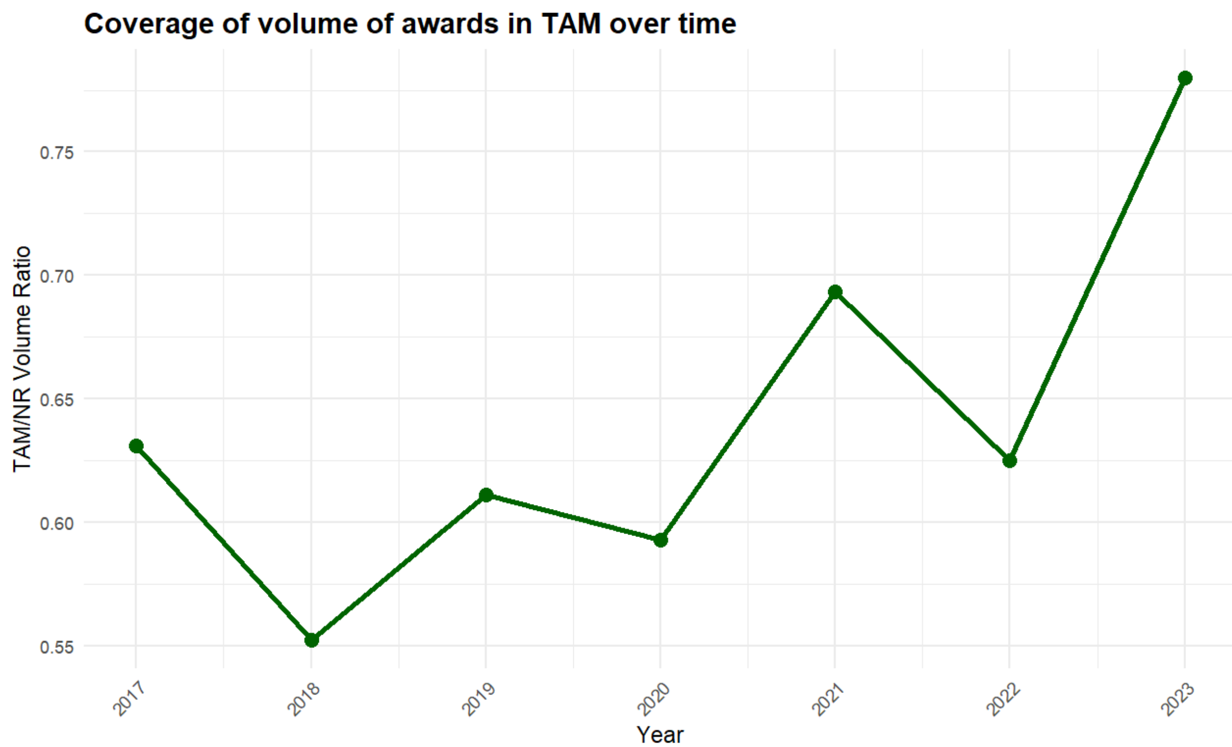

Figure 7: Share of volume of awards in TAM as a fraction of all state aid expenditure in Italy over time

However, Figure 7 presents a markedly different picture: TAM captures 59.4% of total aid volume on average. This volume coverage substantially exceeds that of total number of aid awards, suggesting that TAM fulfils its primary objective of ensuring transparency for major state aid measures. We can observe a spike in coverage of aid volume in 2023 after reporting threshold has been lowered to €100,000. These baseline findings suggest that TAM can be a valuable source of information for large awards, industrial policies and projects, and increasingly so, as the threshold for reporting has been lowered.

Table 3: TAM Coverage by Policy Objective

Policy Objective	Count Coverage (%)	Volume Coverage (%)	
		vs Aid Element	vs Nominal Amount
Remedy for a serious disturbance	2	66.7	66.1
Environmental protection	8.1	80.7	69.2
Research, development and innovation	23.6	79.3	56.8
Regional development	2.7	50	42.6
Compensation of damages	2.2	77.1	67.4
Sectoral development	0.3	13.9	10.7
Culture and heritage conservation 4.1	37.9	25.9	
SMEs including risk capital	0.2	0.5	0.7
Training	1.2	6	6
Rescue & Restructuring	0.0	54.8	40.2
Employment	0.2	1.5	1.5

Coverage by Policy Objectives Table 3 displays the award numbers and volume captured in TAM as a fraction of those in National Registry.

This analysis reveals systematic variation in TAM's representativeness across different aid categories. Research, development and innovation emerges as the best-represented policy area, with TAM capturing 23.6% of awards by count and between 57% and 79% of total volume. This pattern suggests that R&D aid is structured as large awards that exceed TAM's reporting thresholds, making it well-suited for analysis using TAM data.

Environmental protection exhibits a different pattern, with high volume coverage (at least 80%) but lower count coverage (8.1%). This indicates that environmental aid consisting primarily of large, concentrated investments such as infrastructure projects and green transition schemes are captured, while smaller environmental initiatives remain below reporting thresholds.

Aid under regional development schemes follow a similar pattern. While TAM contains only a small percentage of aid distributed for regional development, the awards it includes represent half of the state aid expenditure given out for this policy objective.

Employment aid represents the opposite extreme, with TAM capturing only 0.2% of awards by count and 1.5% by volume. This near-complete absence from TAM reflects the small-scale nature of most employment interventions, which usually take the form of payroll tax reductions and hence fall well below the €100,000–€500,000 reporting thresholds. The picture is similar for SME support (0.2% count, 0.5% volume), confirming that aid targeted at small and medium enterprises remains largely omitted in TAM data. Likewise, most of training aid is not captured in TAM.

Coverage by Aid Amounts To assess compliance with EU transparency requirements, we examine coverage rates for aid awards that theoretically should appear in TAM based on the applicable reporting thresholds. We define an aid award as being recorded in TAM if we observe an exact match on five key identifying variables: beneficiary fiscal code, aid objective, state aid measure number, aid instrument, and date of granting.

For the pre-July 2023 period, when the threshold was 500,000, we find that 92.3% of aid awards with aid elements exceeding this threshold appear in TAM, meaning that 7.7% of awards above the threshold are missing from the transparency database. At least a part of this could be due to our matching since it relies on exact matches of state aid measure numbers, objectives, instruments and fiscal codes.

Overall, the mandatory reporting seems to be reasonably complete. Taken together with the findings of the previous section, we could say that TAM is representative not for the aid received by large undertakings, but for large aid awards.

8.2 Matching results

For most countries, we matched above 80% of the state aid awards contained in TAM or national registers to ORBIS (Table 4). For Germany, where the TAM did not consistently include a standard firm identifier or tax ids, we matched what we could using tax ids. Where these were missing, we used the - often partial - information on the registry number as a blocking variable, narrowing the pool of potential matches, and then to confirm matches by comparing firm names using a similarity algorithm.

Country	Nr of aid recipients	Nr in Orbis	Match rate
PT	650	640	98.46
HU	1223	1204	98.45
FI	346	332	95.95
LT	191	182	95.29
AT	125	118	94.40
ES	1608	1514	94.15
RO	382	359	93.98
SK	213	196	92.02
SI	46	42	91.30
PL	1188	1060	89.23
CZ	809	719	88.88
DK	386	341	88.34
BE	621	548	88.24
EE	88	77	87.50
SE	816	678	83.09
IT	1368	1134	82.89
IE	163	121	74.23
GR	299	219	73.24
HR	193	141	73.06
DE	3163	2236	70.69
FR	1969	1229	62.42
NL	2744	1699	61.92

Table 4: Match rate of recipient firms fom state aid databases to Orbis, only firms that received aid above the threshold

8.3 Baseline Results

Table 5: Event Study: All State Aid Effects on Firm Outcomes

	Investment Rate in Tangible Fixed Assets (1)	Employment Growth (2)	Log TFPR (3)	Revenue per Employee (4)	Revenue over Total Capital (5)
t = -4	0.005	-0.004	-0.006	-0.026***	0.052***
	(0.009)	(0.004)	(0.009)	(0.008)	(0.016)
t = -3	-0.007	-0.003	-0.007	-0.021***	0.037***
	(0.007)	(0.003)	(0.006)	(0.006)	(0.011)
t = -2	0.003	-0.006**	0.004	-0.004	0.020**
	(0.007)	(0.003)	(0.005)	(0.005)	(0.008)
t = 0	0.058***	0.005*	-0.016***	0.005	-0.077***
	(0.007)	(0.003)	(0.005)	(0.004)	(0.008)
t = 1	0.070***	0.004	-0.031***	0.010*	-0.168***
	(0.007)	(0.003)	(0.006)	(0.005)	(0.011)
t = 2	-0.002	-0.007*	-0.033***	0.009	-0.168***
	(0.009)	(0.004)	(0.008)	(0.007)	(0.016)
Observations	3710076	3710076	3710076	3710076	3710076
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Reference period: t = -1 (normalized to zero)

Standard errors clustered at firm level

8.4 Results by firm size

Table 6: Event Study: All State Aid Effects by Firm Size

	SMEs (emp < 250)			Large Firms (emp ≥ 250)		
	Investment Rate	Employment Growth	Log TFPR	Investment Rate	Employment Growth	Log TFPR
	(1)	(2)	(3)	(4)	(5)	(6)
t = -4	0.014	0.001	-0.010	-0.026***	-0.012**	-0.023*
	(0.011)	(0.005)	(0.010)	(0.010)	(0.005)	(0.013)
t = -3	-0.004	-0.002	-0.008	-0.033***	-0.010***	-0.027***
	(0.008)	(0.004)	(0.008)	(0.007)	(0.003)	(0.009)
t = -2	0.003	-0.005	0.005	-0.010	-0.010***	-0.016**
	(0.008)	(0.004)	(0.006)	(0.007)	(0.003)	(0.008)
t = 0	0.073***	0.005	-0.018***	0.017***	0.006*	-0.005
	(0.008)	(0.004)	(0.006)	(0.006)	(0.003)	(0.007)
t = 1	0.086***	0.008**	-0.035***	0.002	-0.005	-0.030***
	(0.009)	(0.004)	(0.007)	(0.007)	(0.003)	(0.008)
t = 2	0.008	-0.003	-0.039***	-0.034***	-0.011**	-0.032***
	(0.011)	(0.005)	(0.009)	(0.009)	(0.004)	(0.011)
Observations	3813902	3813902	3813902	136384	136384	136384
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Reference period: t = -1 (normalized to zero)

Standard errors clustered at firm level

Table 7: Event Study: Effects of Environmental Aid by Firm Size

	SMEs (emp $<$ 250)			Large Firms (emp $\geq$ 250)		
	Investment Rate	Employment Growth	Log TFPR	Investment Rate	Employment Growth	Log TFPR
	(1)	(2)	(3)	(4)	(5)	(6)
t = -4	0.002	-0.022*	-0.036	-0.037***	0.002	-0.034
	(0.022)	(0.012)	(0.031)	(0.014)	(0.007)	(0.023)
t = -3	0.004	-0.016*	-0.077***	-0.031***	-0.001	-0.061***
	(0.017)	(0.009)	(0.022)	(0.010)	(0.005)	(0.014)
t = -2	-0.002	-0.014*	-0.018	-0.019**	-0.001	-0.036***
	(0.018)	(0.008)	(0.019)	(0.009)	(0.004)	(0.011)
t = 0	0.005	-0.013	0.030**	-0.002	0.009**	-0.009
	(0.018)	(0.008)	(0.015)	(0.009)	(0.004)	(0.011)
t = 1	0.023	-0.004	-0.012	-0.010	0.002	-0.020
	(0.019)	(0.008)	(0.020)	(0.010)	(0.005)	(0.014)
t = 2	-0.002	0.015	0.009	-0.019*	0.006	-0.038**
	(0.022)	(0.011)	(0.024)	(0.011)	(0.006)	(0.018)
Observations	2993998	2993998	2993998	111864	111864	111864
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Reference period: t = -1 (normalized to zero)

Standard errors clustered at firm level

Table 8: Event Study: R&D Effects by Firm Size

	SMEs (emp < 250)			Large Firms (emp ≥ 250)		
	Investment Rate	Employment Growth	Log TFPR	Investment Rate	Employment Growth	Log TFPR
	(1)	(2)	(3)	(4)	(5)	(6)
t = -4	0.045**	0.004	0.053***	-0.017	-0.013*	-0.015
	(0.020)	(0.009)	(0.018)	(0.015)	(0.007)	(0.021)
t = -3	0.022	-0.006	0.027*	-0.019	-0.014**	0.003
	(0.016)	(0.007)	(0.014)	(0.012)	(0.006)	(0.015)
t = -2	0.011	0.003	-0.006	-0.014	-0.011**	0.018
	(0.016)	(0.007)	(0.012)	(0.011)	(0.006)	(0.014)
t = 0	0.060***	0.004	-0.033***	0.017	0.006	-0.006
	(0.016)	(0.007)	(0.010)	(0.011)	(0.005)	(0.013)
t = 1	0.054***	0.011	-0.016	-0.001	-0.002	-0.025
	(0.017)	(0.007)	(0.012)	(0.013)	(0.006)	(0.015)
t = 2	0.015	-0.014	-0.008	-0.042**	-0.010	-0.014
	(0.021)	(0.009)	(0.019)	(0.016)	(0.008)	(0.019)
Observations	3510203	3510203	3510203	117915	117915	117915
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Reference period: t = -1 (normalized to zero)

Standard errors clustered at firm level

Table 9: Event Study: Regional Development Effects by Firm Size

	SMEs (emp < 250)			Large Firms (emp ≥ 250)		
	Investment Rate	Employment Growth	Log TFPR	Investment Rate	Employment Growth	Log TFPR
	(1)	(2)	(3)	(4)	(5)	(6)
t = -4	0.008	0.003	-0.025	-0.061**	-0.025**	-0.006
	(0.019)	(0.008)	(0.016)	(0.024)	(0.012)	(0.020)
t = -3	-0.013	-0.002	0.010	-0.048***	-0.018**	-0.014
	(0.014)	(0.007)	(0.012)	(0.014)	(0.007)	(0.017)
t = -2	0.001	-0.012**	0.018*	-0.004	-0.020***	-0.009
	(0.014)	(0.006)	(0.010)	(0.015)	(0.006)	(0.014)
t = 0	0.122***	0.012**	-0.031***	0.043***	-0.003	-0.008
	(0.015)	(0.006)	(0.009)	(0.013)	(0.006)	(0.011)
t = 1	0.153***	0.013**	-0.071***	0.009	-0.011	-0.044***
	(0.015)	(0.006)	(0.011)	(0.015)	(0.007)	(0.014)
t = 2	0.022	0.006	-0.093***	-0.042**	-0.030***	-0.047**
	(0.017)	(0.007)	(0.014)	(0.017)	(0.008)	(0.020)
Observations	3890621	3890621	3890621	118280	118280	118280
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01

Reference period: t = -1 (normalized to zero)

Standard errors clustered at firm level

Table 10: Event Study: Sectoral Development Effects by Firm Size

	SMEs (emp $<$ 250)			Large Firms (emp $\geq$ 250)		
	Investment Rate	Employment Growth	Log TFPR	Investment Rate	Employment Growth	Log TFPR
	(1)	(2)	(3)	(4)	(5)	(6)
t = -4	-0.009	-0.041*	-0.033	0.016	-0.011	-0.014
	(0.059)	(0.025)	(0.038)	(0.017)	(0.009)	(0.048)
t = -3	-0.015	-0.001	-0.059	0.002	0.000	-0.036
	(0.049)	(0.022)	(0.048)	(0.015)	(0.009)	(0.044)
t = -2	0.001	-0.025	0.014	0.014	0.004	-0.013
	(0.051)	(0.023)	(0.051)	(0.014)	(0.009)	(0.026)
t = 0	0.027	-0.047**	0.038	-0.007	0.009	0.012
	(0.036)	(0.019)	(0.033)	(0.011)	(0.009)	(0.028)
t = 1	0.026	-0.015	0.049	-0.001	0.009	-0.004
	(0.045)	(0.021)	(0.038)	(0.014)	(0.009)	(0.040)
t = 2	-0.013	-0.077**	0.043	-0.042	-0.009	-0.038
	(0.052)	(0.034)	(0.063)	(0.031)	(0.014)	(0.087)
Observations	3172343	3172343	3172343	126720	126720	126720
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Reference period: t = -1 (normalized to zero)

Standard errors clustered at firm level

Table 11: Event Study: SME Support Effects by Firm Size

	SMEs (emp $<$ 250)			Large Firms (emp $\geq$ 250)		
	Investment Rate	Employment Growth	Log TFPR	Investment Rate	Employment Growth	Log TFPR
	(1)	(2)	(3)	(4)	(5)	(6)
t = -4	-0.025	-0.049**	0.028	0.330	0.028	0.036
	(0.045)	(0.022)	(0.040)	(0.244)	(0.025)	(0.035)
t = -3	-0.024	-0.020	-0.045	0.058	0.072*	-0.085**
	(0.038)	(0.017)	(0.042)	(0.045)	(0.038)	(0.033)
t = -2	-0.010	-0.038**	0.002	0.138**	0.051	-0.014
	(0.039)	(0.015)	(0.028)	(0.064)	(0.044)	(0.045)
t = 0	0.083**	-0.015	-0.016	0.040	0.001	-0.015
	(0.035)	(0.015)	(0.026)	(0.030)	(0.024)	(0.029)
t = 1	0.035	-0.002	-0.070**	0.015	0.010	0.037
	(0.040)	(0.016)	(0.035)	(0.044)	(0.032)	(0.026)
t = 2	-0.062	-0.019	-0.053	0.083	-0.020	0.024
	(0.052)	(0.020)	(0.047)	(0.145)	(0.079)	(0.076)
	(6957.575)	(3145.924)	(8128.213)			
Observations	3715305	3715305	3715305	117746	117746	117746
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Reference period: t = -1 (normalized to zero)

Standard errors clustered at firm level