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A Bitter Aftertaste

How State Aid Affects Recipient Firms and Their Competitors in Europe

Luis Brandao Marques and Hasan Toprak

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A Bitter Aftertaste: How State Aid Affects Recipient Firms and Their Competitors in Europe

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ABSTRACT: Industrial policy is once again at the forefront of the policy debate around the world. However, state aid is a contentious issue in the European Union given the need to maintain a level playing in its single market. This paper estimates the effects of state aid between 2016 and 2023 on listed nonfinancial firms in Belgium, France, Germany, the Netherlands, Spain, and the United Kingdom (until 2020) using a high-frequency identification approach to address endogeneity. It finds that firms that receive state aid increase employment and revenue, but not investment or labor productivity. Moreover, it finds that there are adverse spillover effects to competing firms that significantly undo any positive own effects. These findings suggest that, should there be a case for providing state aid to firms in the European Union, this should be done at the European level instead of the member state level to mitigate adverse spillovers. Pooling resources and competitively allocating aid across the Union could preserve market competition, encourage firm entry, and ensure a more efficient distribution of funds.

JEL Classification Numbers:	G30, L25, L52, O52
Keywords:	Industrial policy; firm performance; state aid; spillovers
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Executive Summary

The Resurgence of Industrial Policy: Implications and Insights from European State Aid

Industrial policy has reemerged as a significant economic strategy, but its empirical evaluation remains limited. This paper contributes to the growing body of the literature on industrial policy by assessing the impact of national state aid on firm outcomes across Europe.

Our analysis provides robust evidence that state aid can temporarily boost firm-level employment and revenue of the recipient firms. However, these benefits are short-lived, with no discernible impact on firm investment or productivity. Moreover, state aid creates adverse spillovers for competitor firms, which ultimately outweigh the positive effects on recipient firms after two years. These findings underscore the importance of carefully weighing the potential benefits of industrial policy against its broader economic consequences, especially regarding the playing field.

In theory, government intervention in specific sectors can address persistent market failures, particularly when first-best solutions are infeasible. Examples include addressing climate change (where carbon pricing may fall short), strengthening supply-chain resilience (where firms do not sufficiently internalize the broader effects of their on- and off-shoring decisions), and enhancing economic security. In such cases, vertical policies like state aid may have a role to play, especially when horizontal policies (e.g., to foster fundamental research and development (R&D) or to build human capital and upskill the labor force) prove insufficient.

However, even when a theoretical case exists, the risks of government failure must be carefully considered. Identifying market failures is inherently complex, and governments often lack the information or incentives needed to implement effective interventions. Moreover, governments can be captured by industry interests and end up supporting losing firms or sectors instead of those with high growth potential. Proper quantification of the overall welfare implications of state aid, while critical, lies beyond the scope of this paper.

Policy Implications and Recommendations

Our findings caution against implementing state aid at the national level without accounting for its effects on the single market. National-level policies risk distorting competition within Europe, creating an uneven playing field that favors domestic firms over competitor firms from other European Union (EU) member states. This, as our analysis suggests, can lead to net aggregate losses for the EU, as the negative spillovers from state aid often surpass its short-term benefits for recipient firms.

To mitigate these risks, we propose that state aid, if justified by well-identified externalities and the absence of less distortionary tools, be coordinated at the EU level. Pooling resources and competitively allocating aid across the Union could preserve market competition, encourage firm entry, and ensure a more efficient distribution of funds. An EU-level approach would better capture gains across value chains that span multiple

member states, reduce the risk of entrenched interests favoring incumbents, and address disparities in fiscal capacity among member countries.

Moreover, the single market enables firms to achieve economies of scale that would be difficult to realize otherwise, particularly in an era of increasing geoeconomic fragmentation. A coordinated EU-level industrial policy could safeguard these advantages while reducing the unintended consequences of fragmented national interventions.

Future Research Priorities

Although our findings highlight key dynamics of state aid, they also underscore the need for further research into its broader economic implications. Aggregating firm-level effects to the macroeconomic level is challenging and may overlook critical general equilibrium effects. For instance, state aid-induced increases in labor demand could elevate wages, potentially dampening employment responses.

A comprehensive evaluation of state aid's net impact on EU employment, investment, productivity, and overall welfare should be a priority for future research. This would provide policymakers with the evidence needed to design industrial policies that balance economic objectives with the integrity of the single market.

A Bitter Aftertaste: How State Aid Affects Recipient Firms and Their Competitors in Europe *

Luís Brandão-Marques and Hasan Toprak

December 11, 2024

Abstract

Industrial policy is once again at the forefront of the policy debate around the world. However, state aid is a contentious issue in the European Union given the need to maintain a level playing field in its single market. This paper estimates the effects of state aid between 2016 and 2023 on listed nonfinancial firms in Belgium, France, Germany, Italy, the Netherlands, Spain, and the United Kingdom (until 2020) using a high-frequency identification approach to address endogeneity. It finds that firms that receive state aid increase employment and revenue, but not investment or labor productivity. Moreover, it finds that there are adverse spillover effects to competing firms that significantly undo any positive own effects. These findings suggest that, should there be a case for providing state aid to firms in the European Union, this should be done at the European level instead of the member state level to mitigate adverse spillovers. Pooling resources and competitively allocating aid across the Union could preserve market competition, encourage firm entry, and ensure a more efficient distribution of funds.

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1 Introduction

The use of state aid and and other forms of industrial policy remains common in both advanced and developing economies and, if anything, has made a comeback in recent years (Evenett et al., 2024).[1] Traditionally, state aid and other government interventions targeted to specific sectors, technologies, or companies were meant to encourage innovation, exports, economic growth, economic diversification, and regional cohesion (Tirole, 2023). The recent return of industrial policy has come on the back of concerns about climate change, geoeconomic fragmentation, security and geopolitical risks, and deindustrialization (Juhász et al., 2024), as well as the Covid-19 pandemic. In the European Union (EU), for example, state aid increased from 0.5 percent of GDP in 2012 to nearly 1.5 percent of GDP in 2022, although half of the latter is pandemic- and Russia-Ukraine war-related aid. And this increase has happened despite the common view among economists that industrial policy, except in very well identified circumstances, is not worth pursuing.

In theory, governments may have a case to intervene in a particular sector to address persistent market failures when first-best solutions are not feasible. For example, in instances in which first-best policies are insufficient, unavailable, or too costly, industrial policy might have a role to play. Industrial policy can address externalities like economies of agglomeration associated with labor market depth, shared infrastructure, and the diffusion of information by supporting the creation or maintenance of sectoral clusters (Tirole, 2018). Other examples of market failures that can potentially be addressed with industrial policy include learning externalities, increasing returns, asymmetric information (e.g., when firms do not have complete information to ensure supply chain resilience), market access externalities (e.g., "missing pioneers" problem)[2] or imperfect competition

[1]Industrial policy is an example of a vertical policy intervention in which state aid is provided to individual firms or sectors with the overarching goal of changing the sectoral composition of the economy. This stands in contrast with horizontal policies that aim to improve the general business environment for all firms and industries in the economy, such as by developing physical infrastructure, cutting red tape, improving education and vocational training, or strengthening governance and institutions. Industrial policy measures include state aid to companies in the form of subsidies, grants, guarantees, or loans, tax breaks and other incentives, special licensing arrangements, custom public services to certain firms or sector, and even trade tools like tariffs, local content requirements, and export restrictions.

[2]The "missing pioneers" problem occurs when access to external markets is too costly for the first mover but, once one firm enters the new market, access to other firms becomes less costly. This externality seems to be significant for small economies but not for large ones (Wei et al., 2021).

(i.e., by promoting entry to industries with few players and significant returns to scale; see the infant industry argument in Harrison and Rodríguez-Clare, 2010). In practice, however, the track record of industrial policy has been lackluster. Corruption and lobbying, incompetent bureaucracies and insufficient effort by civil servants because of low public sector wages, or lack of superior information by technocrats relative to the market can imply that government failures can cause more harm than the market failures that industrial policy is supposed to address (Wei et al., 2023).[3] Moreover, in the context of Europe, the differing capacities of member states to implement industrial policies can exacerbate disparities and distort the level playing field within the single market.

Importantly, empirical evaluations of industrial policy struggle to find beneficial effects and much of the evidence is either anecdotal or fails to account for endogeneity (Lane, 2020), with some exceptions (e.g., Criscuolo et al., 2019). Moreover, measurement challenges ranging from definitional issues (e.g., which subsidies qualify as state aid), lack of reliable data, the simultaneous use of multiple tools that changes over time, and the use of the same tool for other purposes besides industrial policy make it difficult to identify the effects of industrial policy at the firm, sector, or country level (Bown, 2024; Juhász et al., 2024). Overall, there is a significant gap between the theoretical literature discussing the potential costs and benefits of industrial policy and the empirical literature assessing its effects, especially at the firm level.

This paper tries to fill this gap by estimating the effects of state aid in Belgium, France, Germany, Italy, the Netherlands, Spain, and the United Kingdom on employment, revenues, investment, and productivity of recipient and nonrecipient firms. Together, these countries account for the majority of economic activity and state aid in Europe. The paper uses public data on state aid at the firm level from the State Aid Transparency Database maintained by the European Commission and considers the identification problems caused by the inherent endogeneity of industrial policy because of reverse causality and omitted variables.[4] To address endogeneity, in addition to using firm-level data, we

[3]The weak performance of industrial policy also resonates with the "Public Choice" view that such interventions are bound to fail because government officials lack the necessary information and face incentive problems (e.g., political constraints and coordination issues) that prevent them from delivering (Munger, 2022).

[4]There is reverse causality in state aid because grants or loans can be assigned to firms due to gov-

adopt a high-frequency approach common in the macroeconomics literature–but, as far as we know, never used in the empirical industrial organization literature–by measuring the excess equity returns of the recipient firms on the dates in which state aid is announced,[5] thus capturing the nonanticipated component of industrial policy. Since this reaction has not been anticipated by the market, then it cannot be happening because of any expectation of future firm performance, which addresses reverse causality. For the same reasons, it cannot be correlated with any firm characteristics that may explain future firm performance (assuming some degree of market efficiency), thus addressing the potential omitted variables bias.

We find that firms that receive state aid mildly increase their employment (0.3 percent) and revenues (0.6 percent) in the following year, but not investment or productivity. The effects are stronger for firms that are smaller, have less liquidity and lower debt service capacity, and are younger. We do not find that firms that spend more in R&D as a proportion of the their sales have different responses than the rest. We take these findings as suggesting that state aid may overcome firm-level financial constraints but do not seem to be related to firms' ability to innovate. Importantly, the paper finds that there are adverse spillovers from state aid. According to our findings, granting state aid to some firms decreases employment, revenues, and productivity of the nonrecipient firms in the same sector, although there is no effect on investment. In what is another novel finding in this paper, these adverse spillovers are at least half as large as the the own-firm effects after one year of aid being awarded. The adverse spillover effects are also more long-lasting and more than overturn the initial positive effects after two years. The adverse spillovers are also more significant in sectors with more market concentration. Moreover, since state aid does not increase own firm productivity, but still has important negative spillovers, a large-scale program of state aid to firms in Europe would not contribute to raising productivity in the continent. Our findings are robust to various potential sources of misspecification.

ernments expecting them to perform poorly (picking losers) or well (choosing winners), in which case standard regression analysis would, respectively, underestimate or overestimate the potential beneficial effects of industrial policy.

[5]See Cochrane and Piazzesi (2002) and Bauer and Swanson (2023) for examples of high-frequency identification of monetary policy shocks.

We contribute to the empirical literature on the effects of industrial policy. Our paper relates the most to Criscuolo et al. (2019) who, like we do, take into account the problem of endogeneity in their analysis.[6] They find that state aid increases employment at the regional and firm level, but only for small firms. We also find that smaller firms increase employment more than large ones in response to state aid. Our paper also relates to Aghion et al. (2015), according to whom industrial policies in China that are directed to more competitive sectors and that reward competition tend to increase firm-level productivity. However, unlike our paper, neither study looks at the potential spillovers from industrial polices to firms excluded from such measures. But, like Aghion et al. (2015), we find that industrial policy is less likely to be counterproductive in more competitive industries.

This paper is organized as follows. Section 2 describes the data and shows some stylized facts. Section 3 covers the empirical methods and identification strategy. The results are discussed in Section 4 and Section 5 concludes.

2 Data

State aid to firms is "an advantage in any form whatsoever conferred by national public authorities to undertakings on a selective basis" (Treaty on the Functioning of the European Union, 2008). This straightforward definition, however, hides significant heterogeneity in the modalities of state aid across countries and over time. For example, during the Covid-19 pandemic and energy shock after Russia's invasion of Ukraine, much of state aid was aimed at keeping companies afloat during a serious but likely transient shock. In other times, state aid was provided to support strategically important activities (e.g., semiconductors or batteries for electric vehicles). The former case of state aid, to some extent, fits into the concept of counter cyclical policy (i.e., bankruptcy protection) and is less likely to affect the level-playing field, while the latter type of state aid can be explained with an infant industry and future autonomy argument.

The paper uses firm-level data on state aid in the EU. State aid data come from the

[6]Other studies that try to identify causal effects of industrial policy measures using random variation in place-based policies as instruments or with a regression discontinuity design are Mesquita Gabriel et al. (2022), Cerqua and Pellegrini (2014) and Dechezleprêtre et al. (2023).

European Commission's State Aid Transparency Database and are provided by EU member states in compliance with the European transparency requirements for state aid. The dataset includes all state aid measures for agriculture, industries, and services for which the European Commission has either adopted a formal decision or received summary information from member states for measures that are exempt from EU authorization under block exemptions. In general, state aid is not compatible with the European Union Treaty as it goes against the single market and is banned or requires exceptional justification. The latter aid requires prior notification and approval by the European Commission. However, the Commission can adopt so-called Block Exemption Regulations for State aid and declare certain categories of state aid compatible with the Treaty if they fulfill certain conditions, as well as exempt them from the notification requirement and approval. All aid that is less than €300,000 over three years is exempt from the ban on state aid.[7]

The data identify the beneficiary firms, the dates in which aid was announced, the amount of aid, and the type of aid. Although all EU member states are covered by the database, for this study we only use aid awarded between 2016 and 2023 to listed firms from Belgium, France, Germany, Italy, the Netherlands, Spain, and the United Kingdom (until 2020).

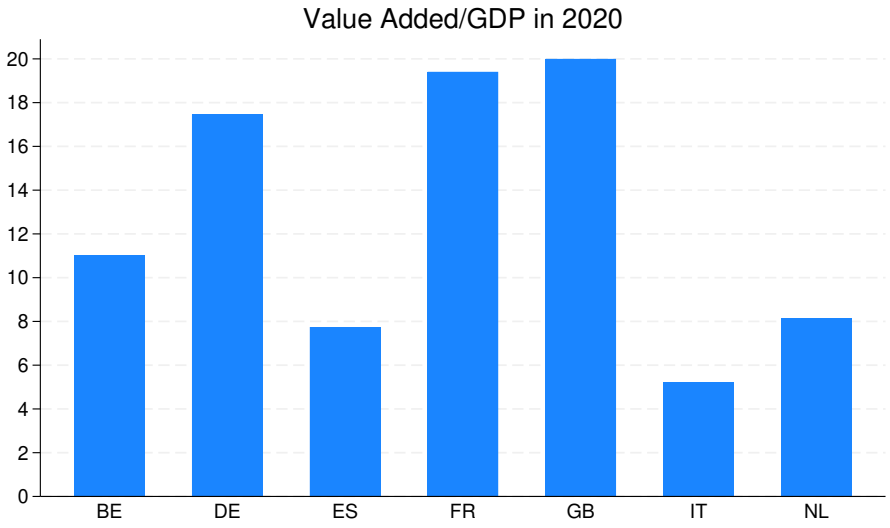
As of 2022, state aid in the countries we cover accounts for [85] percent of total reported state aid cases and 78 percent of the value of awarded state aid in the EU. In the countries in our sample, in 2022, state aid ranged from 1.1 percent of GDP in the Netherlands and Belgium to 1.8 percent of GDP in Germany. Also as of 2022, depending on the country, the main objectives of state aid in the data are environmental protection and energy savings, regional development, and promotion of R&D and research and innovation (R&I). By far, the most common instruments of aid are grants and interest rate subsidies, followed by tax benefits.

Firm outcomes and characteristics are collected at the annual frequency. Firm-level accounting and employment data come from Orbis and are defined in Table A1. The accounting data are cleaned to remove anomalous values and missing data and are deflated so that they reflect real quantities. Non-capital monetary variables are converted from

[7]See `https://competition-policy.ec.europa.eu/state-aid/legislation/regulations_en`.

nominal to real variables and denominated in 2015 constant U.S. dollars, using deflators from various sources (OECD, Eurostat, CEIC database, and government websites). For capital variables (e.g., investment), we use the World Bank's World Development Indicators (WDI) investment deflators at the country level. Although listed firms are only a fraction of all the firms in the European Union that receive state aid, we do not use nonlisted firms for most of our empirical exercises because accounting and employment data are not available to same extent as for listed ones and because our definition of state aid shock uses equity return data (see Section 3). Equity returns (log prices in first differences at the daily frequency) in excess of the market daily average return are from Bloomberg.

Figure 1: Sample as Share of GDP

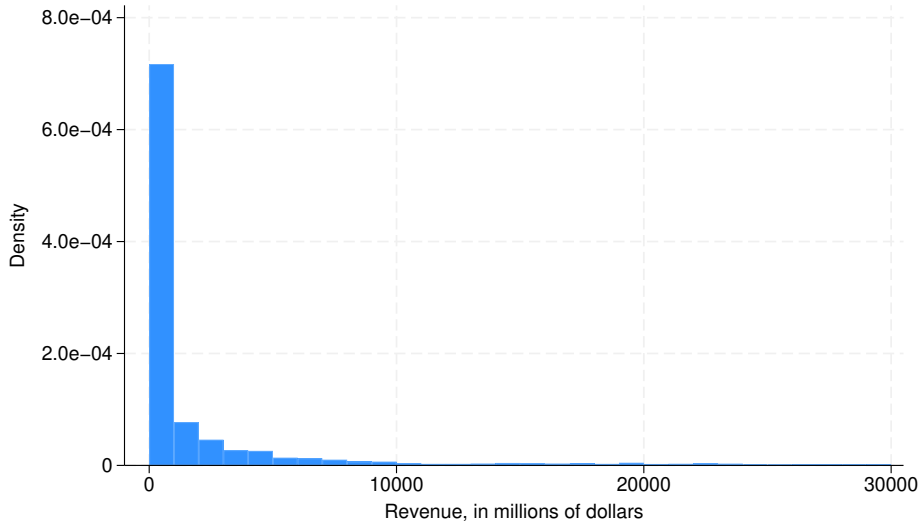


In 2020, the last year for which our data also includes the United Kingdom, the total firm value added in our sample varies between 5 percent in Italy and almost 20 percent of GDP in France and the United Kingdom (Figure 1). This speaks to our sample being representative.

The average size of the firms in our sample is large, with average revenues of 2.4 billion euros and more than 10 thousand workers (Table 1). Still, the sample is very skewed in terms of firm size, with a few very large firms in terms of revenues with the largest firm having more than 341 billion euros in revenues (Figure 2). For this reason, in our econometric specifications (see 3) we work mostly with variables in logs, adjust for

outliers, and winsorize the data when appropriate.

Figure 2: Distribution of Firms by Sales



3 Methodology

3.1 Identification of State Aid Shocks

State aid, like other forms of industrial policy, is implemented in response to the perceived need to enact some structural transformation in the economy and as the response to some identified shortcoming in the current configuration of firm outcomes. Hence, like many other policies, state aid is endogenous in the sense that causality flows both ways between firm outcomes and aid, and because omitted variables are likely to generate biases. There is reverse causality in state aid because grants, loans or any other form of government aid can be assigned to firms because governments expect firms to perform poorly (picking losers) or well (choosing winners). For example, governments are often seen providing aid to firms that were likely to succeed even without help, or granting subsidies to ailing industries. In either case, standard regression and correlation analyses would, respectively, underestimate or overestimate the potential beneficial effects of industrial policy.

Endogeneity is especially troublesome in studies that use aggregate data (Harrison and Rodríguez-Clare, 2010). Moreover, because of the difficulty in building appropriate aggregate counterfactuals, empirical studies that use microdata, like ours, seem inherently more appealing (Lane, 2020). Hence, several recent studies try to identify causal effects of

industrial policy measures using random variation in place-based policies as instruments (Criscuolo et al., 2019; Mesquita Gabriel et al., 2022) or in a regression discontinuity design (Cerqua and Pellegrini, 2014; Dechezleprêtre et al., 2023).

This paper proposes an identification approach that is similar to what is used in the macroeconomics literature of policy shock identification (Cochrane and Piazzesi, 2002; Bauer and Swanson, 2023), but which has never been used in the empirical industrial organization literature, to the best of our knowledge. This identification approach first assumes that financial markets have the same information about state aid as the government and understand the the government's reaction function. Second, it assumes that the systematic component of a policy measure is anticipated by financial markets and fully reflected in prices before the policy is announced. Any asset price reaction that is observed after (but sufficiently close to) the announcement–a surprise–must then be exogenous (i.e., a return surprise should capture an exogenous variation in state aid). Finally, it assumes that the effects of systematic and non-systematic (i.e., surprises) policy changes are the same and, therefore, measuring the effect of the exogenous component of such policy actions is sufficient to draw conclusions about causal effects.

We measure our firm-specific state aid surprise as the daily return on the stock of each firm in the sample (in excess of the average daily market return) on the date of granting the aid. This date is the day when the legal right to receive the aid is conferred to the beneficiary under the applicable national legal regime. The state aid shock is then defined in percent and can be either positive (the firm received more aid than expected) or negative (the aid was less than anticipated by markets). Figure 3 shows that our policy shock measure is symmetric around zero, with most surprises being small.

Moreover, our state aid shock is not correlated with other observable firm characteristics. For example, the association between the shock and firm size shows no discernible pattern (Figure 4). We take this evicence as indicative that our shock is indeed exogenous and supports a causal interpretation of our results.

We define the state aid spillovers variable in a similar fashion. For each firm, the state aid spillover is the average of state aid shocks (weighted by firm assets) in all other countries in a given year and in the same industry (at NACE level 4). This variable

Figure 3: Statistical Distribution of the State Aid Shock

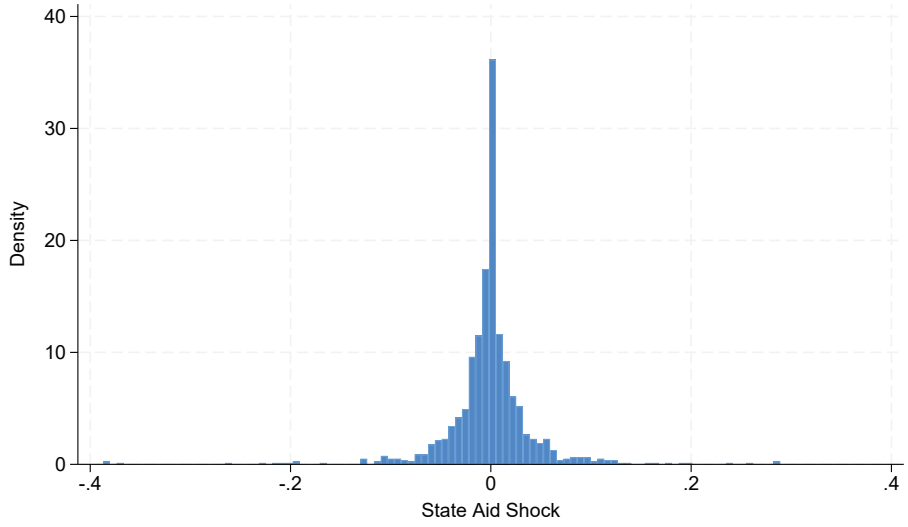
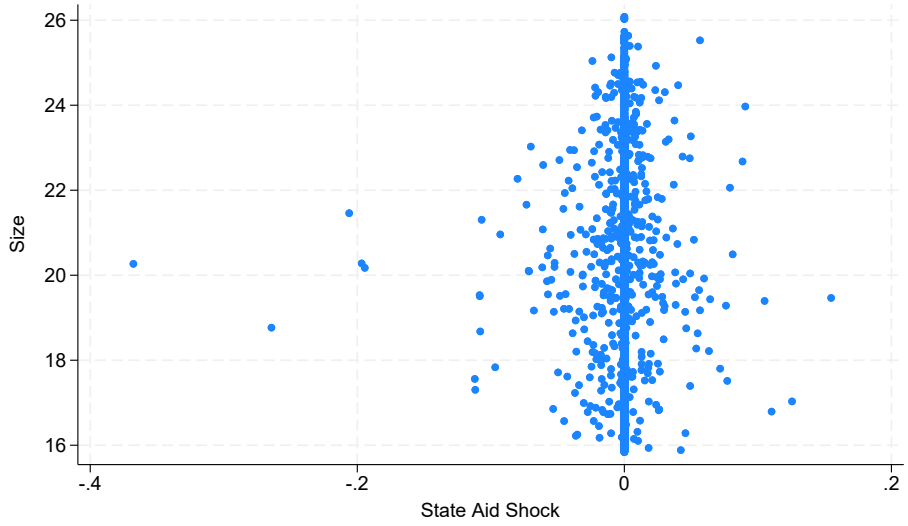


Figure 4: State Aid Shock and Firm Size



intends to capture the effect on a given firm of state aid provided to other firms in the same industry.

Our study has several advantages when it comes to the identification of causal effects. First, it relies on microdata, which ameliorates to some extent the reverse causality problems that plague aggregate data studies. Second, it refers to precise policy interventions, identifying the time of the intervention, the type of intervention (mostly grants), and the recipient and does not bundle different types of intervention (e.g., tariff, nontariff) registered over time (see Lane, 2020, for a discussion of this problem). Third, since we are only tracking the effects of state aid over seven years (2016-2023), we do not have the problem

of studying the effects of industrial policy under different motivations as these change only gradually (although the Covid-19 crisis could have been one of those watershed moments in which the motivation of policies changes dramatically; we address this issue in Section 4.3).

3.2 Econometric Specification

We have three benchmark specifications. The first specification for each outcome variable y tracks the effect of state aid on the recipient firm and is as follows:

$$y_{i,c,t+1} - y_{i,c,t} = \alpha^y + \beta_o^y \times SA_{i,c,t} + \gamma^y \times \mathbf{X}_{i,c,t} + \varepsilon_{i,c,t}, \tag{1}$$

where $y_{i,c,t+1}$ is the log of employment, the log of total revenue, investment in fixed assets (the log change in total assets), or the log of productivity of firm i, in country c, in year $t+1$. Productivity is the firm's value added per worker. SA is the sum of state aid shocks observed for firm i in year t, and $\mathbf{X}$ is a vector of controls that include firm size (log of total assets) and year and firm fixed effects. The second specification tries to measure the effects of state aid on nonrecipient firms in the same sector (i.e., spillovers) and takes the following form:

$$y_{i,c,t+1} - y_{i,c,t} = \alpha^y + \beta_s^y \times SAS_{i,c,t} + \gamma^y \times \mathbf{X}_{i,c,t} + \varepsilon_{i,c,t}, \tag{2}$$

where SAS is the state aid spillover variable and all other variables and parameters have the same meaning as above. The final benchmark specification measures own-effects and spillovers at the same time in case they are correlated and is the following:

$$y_{i,c,t+1} - y_{i,c,t} = \alpha^y + \beta_o^y \times SA_{i,c,t} + \beta_s^y \times SAS_{i,c,t} + \gamma^y \times \mathbf{X}_{i,c,t} + \varepsilon_{i,c,t}. \tag{3}$$

In separate robustness exercises, we control for total debt (in logs), working capital, and sector-specific time trends as well. For all specifications, standard errors are clustered at the firm level since state aid is awarded at this level and it is, therefore, the aggregation level of the treatment assignment (see Abadie et al., 2022).

4 Results

4.1 Own effects

The estimates of the coefficients in equation (1) are presented in Table 3. Firm employment and revenues improve after state aid is awarded, but not firm-level investment or productivity, for which the effect is not statistically significant. The finding that state aid is positively associated with firm employment but not firm productivity is in line with what Branstetter et al. (2023) find for state aid in China. Moreover, like us, Criscuolo et al. (2019) find a causal effect of state aid on employment but not productivity. An explanation for state aid not improving the productivity of recipient firms is that it is not necessarily limited to correcting market failures. However, it is possible that well-designed state aid to correct market failures, including agglomeration externalities, raises firm productivity, as implied by Hodge et al.'s 2024 structural model of industrial policy in Europe.

However, even for the specifications for which we find statistically significant effects of state aid, the economic significance of these estimates is small. In response to a one standard deviation shock (1 percent excess return), firm-level employment is estimated to increase by 0.3 percent for treated firms (see summary statistics for the sample of treated firms in Table 2), while revenue increases by 0.6 percent. Since the economic significance of these estimates is hard to judge, we convert the state aid shock to euros. As the average firm in the sample has a market capitalization of about 7 billion euros, a 1.6 percent excess return is a little over 100 million euro. This means that 100 million euros worth of unanticipated state aid (in net present value) would lead the average firm to increase employment by 0.3 percent of 10,009 or 30 employees, and annual revenue by 0.6 percent of 2.45 billion euros or 15 million euros.

In terms of firm employment, we find significantly smaller effects of state aid than what is found by Criscuolo et al. (2019) for a one standard deviation increase in their measure of state aid (4.7 percent instead of 0.3 percent). A possible explanation for the different magnitude of our findings compared to theirs is that the policies their study are more horizontal in nature than the state aid included in our dataset. Another explanation is

that the average firm size in our sample is much larger than theirs and, as they document in their study, only small firms increase employment once they benefit from state aid. To test this hypothesis we reestimate a modified version of equation (1) but now with an interaction of the state aid shock with firm size (total assets). Specifically, we estimate the following equation for employment, revenue, productivity, and investment:

$$y_{i,c,t+1} - y_{i,c,t} = \alpha^y + \beta_1^y \times SA_{i,c,t} + \beta_2^y \times Size_{i,c,t} + \beta_3^y \times SA_{i,c,t} \times Size_{i,c,t} + \varepsilon_{i,c,t}. \quad (4)$$

The results in Table 4 confirm that, in our sample, smaller firms also benefit more, with the estimated increase in employment for the smallest firms (10th percentile) being 0.96 percent and statistically significant and -0.39 percent for the largest firms (90th percentile) but not statistically significant, which is in line with Melcangi's 2018 findings regarding employment-cash flow sensitivity.[8] We also find that the effect of state aid on firm revenues decreases with firm size, but not the effect on firm investment or productivity.

There are at least two possible reasons for the findings that firm size matters for the effects of state aid (Criscuolo et al., 2019). First, it is possible that smaller firms are more likely to be financially constrained than larger ones and, for that reason, respond more once they receive aid. On the other hand, it could be that large firms are better able to manipulate governments through lobbying and other rent-seeking behavior and obtain aid without the obligation of creating jobs or investing. Unfortunately, both hypothesis are difficult to test. On the one hand, financial constraints are not observable and indirect measures have been criticized in the literature for being ambiguous and only very weakly related to firms' inability to raise external funds (Farre-Mensa and Ljungqvist, 2015). Still, size is often pointed out in the literature as being negatively correlated with financial constraints and, if that is the case, our findings suggest that financial constraints could increase the effect of state aid.[9] Moreover, the theory linking investment-cash flow

[8]If the disbursement of state aid can happen much later than the announcement, this could reduce estimates of the effects of the state aid shock for financially constrained firms that cannot borrow against the announced aid. However, assuming that firm size is an acceptable proxy for financial constraints, we still find significant results for these firms.

[9]Kaplan and Zingales (1997) and Whited and Wu's 2006 indexes, two widely used measures of financial constraints, load positively on leverage, and negatively on firm size, age, and cash (i.e., in their sample, leverage correlates positively with a firm being financially constrained, with the opposite being true for cash and age).

sensitivity to financial constraints is ambiguous or non-monotonic at best (see discussion below and Kaplan and Zingales, 1997, 2000). On the other hand, political influence has many dimensions that are not easily captured by a single variable.

We explore additional firm characteristics that may affect the magnitude and significance of the effect of state aid on firm performance: liquidity, debt-service capacity (i.e., financial risk), age, and R&D intensity. To work through the mechanisms that may determine the sensitivity of firm investment (and related to it, firm employment and revenue), and considering the way we measure our state aid shock as an equity return surprise, it is helpful to think of state aid as an unexpected increase in the firm's cash-flow. A high investment-cash flow sensitivity may occur because of the higher cost of external finance compared to internal finance, or internally generated cash-flows, either because of asymmetric information or agency costs (see Hovakimian, 2009). We can think of state aid as similar to internal finance because its cost for the firm should be lower than that of external finance (i.e., debt or equity issuance), with a greater wedge between the cost of internal and external funds increasing the attractiveness of applying for state aid (see the model in Criscuolo et al., 2019, for example). A higher marginal propensity to hire in response to an increase in the financially constrained firm's cash-flow would work through a similar channel: unconstrained firms should be already operating at an optimal size and should not hire more on the face of positive cash-flow shock, while constrained ones should have an incentive to increase their (suboptimal) scale of operations and increase employment (see Melcangi, 2018).

In theory, firms with more liquidity and greater ability to service debt should investment more for the same amount of investment opportunities (Tirole, 2010). Firm financial risk could change the way state aid affects firm employment, revenues, and investment through risk shifting: firms with higher leverage and/or a lesser ability to service debt have a greater incentive to undertake risky projects as they gamble for resurrection. Everything else equal, this would suggest a greater investment-cash flow sensitivity and, therefore, a greater effect of state aid for more leveraged firms. Another channel would be that firms with more leverage have better investment opportunities, which would also increase their investment-cash flow sensitivity. Whether the sensitivity of investment to

cash-flow is higher for firms with more leverage or financial risk[10] is, however, not well-grounded in theory. Still, we estimate equation (4) but with the state aid shock interacted with EBIDTA-to-long-term-debt instead of size.[11] The results in Table 5 show that more leveraged firms (i.e., with lower ability to service debt) respond more to state aid , with the interaction of state aid and debt-servicing capacity being negative and statistically significant for employment, revenue, and investment, but not productivity.

Next, we use a measure of liquidity–the ratio of sales to total liabilities–as the variable for the interaction with state aid.[12] We expect liquidity to have the opposite effect than leverage: firms with more liquidity are less likely to be financially constrained (though this is not certain since financially constrained firms also value cash more and accumulate cash balances to insure against liquidity problems; see Denis and Sibilkov, 2009; Hovakimian, 2009) and they likely have fewer profitable investment opportunities. The results in Table 6 show that it is indeed the case: the sensitivities of firm employment, revenues, and investment to state aid decrease with firm liquidity, while the sensitivity productivity to state aid does not change (i.e., it is negative but not statistically significant).

In addition, we interact state aid with a firm's age to capture the propensity to innovate. Firm age is known to be inversely related to innovation output (Hansen, 1992). The results in Table 8 show that, indeed, younger firms increase employment and revenues more than older firms (but not investment or productivity). For example, a one-year old firm increases employment by 0.65 percent while a 40-year old firm (the average firm age in our sample) increases it by only 0.4 percent. However, like firm size, firm age is also often used as a measure of firms being financially constrained (Hadlock and Pierce, 2010). To check if the results in Table 8 are capturing the greater propensity of younger firms to

[10]We take firm's debt service capacity as an inverse measure of financial risk since, everything else equal, firms with less debt coverage with internally generated firms (i.e., earnings) should have a smaller distance to default (Merton, 1974).

[11]EBITDA is earnings before interest, taxes, and depreciation and amortization, and measures the firm's ability to generate cash. A low ratio of EBITDA to long-term debt means that the firm may find it difficult to meet its financial obligations, such as interest payments and taxes. Long-term debt is a better measure of leverage than short-term debt because it is more stable and predictable, thus better representing a firm's financial health and ability to service debt over the cycle.

[12]We avoid using cash-at-hand as a measure of liquidity because the two are ambiguously related and could capture other firm characteristics. On the one hand, firms that are more likely to undergo liquidity shortages may want to hold more cash at hand to self insure. On the other hand, holding large amounts of cash on the balance sheet may reflect lack of investment opportunities.

innovate or the higher chance of being financially constrained, we also check the results using the interaction of state aid with R&D intensity (the ratio of R&D spending to firm sales). R&D intensity should be high in more innovative firms and those that have high expected growth and also more investment opportunities. This would suggest that firms that spend more in R&D should benefit more from state aid. Moreover, as mentioned above, firm age correlates negatively with the likelihood of a firm being financially constrained, which could make also them more sensitive to state aid. The results in Table 7 show that firms with higher R&D intensity do not increase employment, revenue, and sales differently than the other firms in response to state aid. The absence of a interaction between state aid and R&D intensity against a significant interaction between state aid and firm age suggests that our results are capturing firm-level financial constraints and not a greater ability to produce innovative products or processes.

4.2 Spillovers to Competitors

The estimation of equation (2) yields the results in Table 10, which show that state aid given to other firms in the same sector and different countries adversely affects non-recipient firms' employment, revenue, and productivity. For all regressions, except for investment, the estimates of the coefficients of SAS are statistically significant. When we include both SA and SAS (specification 3), we obtain similar results: state aid increases employment and revenues, but the adverse spillovers partially overturn these effects, while being clearly negative for firm productivity. In fact, while state aid does not increase firm productivity, the adverse spillover effect is large and significant.

The finding that the adverse spillover effects overturn the beneficial own effects is even stronger if we look at a 2-year horizon. As shown in Table 12, the effects of state aid on recipient firms' outcomes are short-lived and not significant after two years. The spillovers from state aid, however, are even more adverse and remain highly statistically significant. If we cumulate the own- and spillover effects of state aid one and two years ahead (i.e., add the coefficients in Tables 11 and 12), we find that the initially positive effects of state aid on firm employment and revenues are more than compensated by the adverse spillover effects.

To the best of our knowledge, this is a novel result using firm-level data and targeted government aid. A few other studies have also found adverse spillovers from regional aid in the form of space-based grants or tax breaks on employment in non-treated areas that are geographically close or economically similar (see Hanson and Rohlin, 2013; Einiö and Overman, 2020, for the United States and the United Kingdom, respectively). However, these studies use aggregate data rather than firm-level data and examine policy interventions with very different goals (i.e., promoting employment and business creation in economically depressed areas) than what our sample captures.

Finally, we test the hypothesis that the effects of state aid depend on the degree of product market competition that firms face. We calculate the Herfindahl-Hirschman index at the NACE 4-digit level using revenue data for the firms in our sample, plus all the nonlisted firms in the same country and sector for which we could get firm-level data. We then interact it with the state aid spillover variable to obtain the estimates in Table 13. The results show that firms in less competitive markets suffer greater adverse state aid spillovers than those in sectors with lower market concentration. These results provide additional support to the hypothesis that industrial policy stands a better chance of having a positive impact in a more competitive environment, which has also been proposed and tested by Aghion et al. (2015), albeit through a different mechanism (i.e., spillovers instead of direct effects on recipient firms).

4.3 Robustness

We check the robustness of our results in several ways. First, we perform a placebo test in which we replace the state aid shock with the change in excess stock returns of the recipient firms one week before the state aid is announced.[13] Unless the announcement happened to be anticipated because of leaks or some other superior information by market participants (in which case there would not have been a change in excess returns on the announcement date and, therefore, the state aid shock would have been zero), we should not expect any effect of the placebo shock on firm outcomes. The results in Table 14

[13] A placebo test is a form of supplementary analysis conducted to evaluate the credibility of the results from the primary analysis (Athey and Imbens, 2017). A placebo test performs a pseudo-causal analysis in which the treatment effect is a priori known to be zero. A rejection of the null hypothesis in the placebo test would suggest that the primary causal analysis is not credible.

show that this is indeed the case: for all outcome variables, the coefficient of the state aid shock has estimates that are not statistically different than zero. We take these findings as supporting the validity of our identification strategy because equity price movements in days with state aid announcement have predictive power for firm outcomes while those in other days do not.

Second, we tried different compositions of the sample of firms. Specifically, we estimated equations (1)-(3) dropping firms from the United Kingdom to check if our results could be driven by Brexit or by our data for British forms ending in 2020 (i.e., before the Covid-19 pandemic). We also estimated those equations including firms from the agricultural, financial, and real estate sectors to dispel the notion that sample selection is driving the findings. In both cases, the results (unreported) are similar to those reported here.

Third, to check if the results on spillovers could be driven by unobservable country effects correlated with our measure of state aid spillovers, we changed its definition to include firms in same sector and country instead of only firms from different countries. The results, not reported here, were also unchanged.

Fourth, we checked if our results could be due to the extraordinary state aid disbursed during the Covid-19 pandemic, a period during which economic activity was abnormally low. We did this by adding a double interaction of our state aid shock with a Covid-19 dummy in specification (1). The results, not shown here but available upon request, show that for all dependent variables the interaction of state aid with the Covid-19 dummy was not significant (i.e., the effects of state aid were not different before and after Covid-19). Unfortunately, because of the short time dimension of our data, the overall effect of state aid ceased to be significant as well.

Fifth, we also checked if the type of aid could matter for the effects of state aid on recipient firms' outcomes. For this effect, we estimated equation (1) using the same definition of state aid shock but separately for grants and loans (i.e., interest rate subsidies). The results were similar for both types of aid. Moreover, in separate robustness exercises, we control for total debt (in logs), working capital, and sector-specific time trends as well, with the results (available from the authors) being quantitatively similar.

Finally, we checked if the state aid spillover effects could be present because of our sample including only listed firms. This is because listed firms have different characteristics than private ones: listed firms tend to be larger, older, use more fixed assets, are more leveraged, and have lower returns on assets and equity than nonlisted firms. Moreover, listed and nonlisted firms behave differently, with the former investing less and being less responsive to changes in investment opportunities than the latter (Asker et al., 2014), or at least being sensitive to different types of investment opportunities (for example, Dougal and Rettl, 2021, find that listed firms' investment rates react more to changes in industry-level investment opportunities while nonlisted firms respond more to local investment opportunities). We address this issue by including in our sample both listed and nonlisted firms. Unfortunately, our measure of state aid shock is only defined for listed firms since it relies on excess equity returns. This prevents us from estimating specifications (1) and (3), but we can still estimate the spillover effects of state aid awarded to listed firms in the same sector for both listed and nonlisted firms (specification 2). The results, in Table 16, show that the spillovers of state aid remain significantly negative for employment, revenues, and investment, but nor for productivity. We take this evidence as broadly supporting the robustness of our findings, especially when it comes to firm employment and revenue.

5 Conclusion

Industrial policy has been making a comeback. However, the empirical verification of its effects is still lacking. This paper contributes to the empirical literature on industrial policy by estimating the effects of state aid on firm outcomes in Europe.

We find robust evidence that state aid can have a positive effect on firm employment and revenue, but such effects are short-lived. We do not find any effects on firm investment or productivity. Moreover, the adverse spillovers from aid awarded to competitor firms are more than sufficient to overturn the positive own-firm effects after two years. This is not enough to say that the industrial policy does not have other benign effects that are not captured in our analysis.

In theory, governments may have a case to intervene in a particular sector to address

persistent market failures when first-best solutions are not feasible. This could include market failures associated with climate change (for which a first-best solution exists–carbon pricing–but may be insufficient), supply chain resilience, and economic security. So, in instances where horizontal policies are unlikely to be sufficient, a vertical policy like state aid might have a role to play.

However, even if there is a theoretical case for industrial policies to correct market failures, this must always be weighed against the likelihood and severity of government failures. For example, market failures are often difficult to identify and, even then governments may be incapable of implementing effective interventions because of insufficient information or misaligned incentives. A discussion of these issues and a proper quantification of the overall welfare implications of state aid is very important but beyond the scope of this paper.

Our findings advise caution when implementing state aid in Europe at the country level and without considering the effects on the single market. Domestic state aid that favors domestic firms over competitors from other member states risks distorting competition in the EU's single market. This is because state aid has spillovers to nonrecipient firms operating in the same sector that, over time, surpass the short-term benefits to recipient firms.

For this reason, we suggest that some level of coordination of state aid at the EU level could be advantageous. For example, pooling funds and then awarding the aid competitively within the EU could go a long way toward preserving market competition (by encouraging firm entry and not favoring domestic incumbents) while increasing efficiency in the allocation of funds. An EU-level industrial policy instead of a domestic one, if warranted, could also better leverage gains across the whole value chain, which is likely to encompass several EU countries, reduce the risk of capture by incumbents,[14] and overcome the problems created by uneven fiscal space among EU member states (Pinkus et al., 2024). Mitigating the adverse spillovers of domestic state aid is also important because the single market allows firms in the European Union to reap economies of scale otherwise difficult to tap into, especially in a context of geoeconomic fragmentation.

[14]We use the argument in favor of EU-level competition policy in Gutierrez and Philippon (2022) to support this option.

Although the adverse spillovers from domestic state aid could result in net aggregate losses for the EU, our paper, by design, cannot present conclusive evidence to support this hypothesis. This is because the aggregation of results from firm-level data (which help to identify causal effects) to the economy level is fraught with difficulties and could miss important general equilibrium effects. For example, these general equilibrium effects will likely dampen firm-level responses in terms of employment as an economy-wide labor demand increase because of state aid would raise the equilibrium wage (Sraer and Thesmar, 2018). A comprehensive assessment of the net effects of state aid on EU employment, investment, productivity, or welfare should be an important avenue for future research.

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Table 1: Summary Statistics

	Observations	Mean	Median	Std. Deviation	Minimum	Maximum
State Aid	7408	0.000	0	0.010	-0.368	0.155
State Aid Spillovers	7408	0.000	0	0.031	-0.337	0.869
Employment	7408	10009	1035	33790	20	596452
Revenue	7408	2446	266.5	6286	5.022	48464
Sales	6331	2417	252.7	6244	0.000	47673
Value Added	6883	942.5	103.7	2756	0.029	49122
Investment	6101	162.7	3.796	2342.2	-18606	137969
Productivity	6883	141821	92138	339622	428.7	18800000
Size	7408	19.80	19.6	2.160	15.835	26.08
Age	7408	39.64	27.5	36.90	0.000	273
R&D Intensity	1827	0.056	0.024	0.099	0.000	1.427
Cash Flow-to-Current Liabilities	7314	0.399	0.287	0.710	-6.511	16.84
Interest-to-Cash Flow	4074	0.199	0.135	0.169	0.050	0.899
Interest-to-EBITDA	3995	0.179	0.123	0.150	0.050	0.891
EBITDA-to-Debt Service	4616	1.616	1.351	1.338	-0.995	4.995
Sales-to-Liabilities	6220	3.304	2.942	1.894	0.214	12.00

Table 2: Summary Statistics for Treated Firms

	Observations	Mean	Median	Std. Deviation	Minimum	Maximum
State Aid Shock	494	0.001	0.000	0.053	-0.368	0.450
State Aid Spillover	494	0.000	0.000	0.003	-0.029	0.028
Employment	494	24737	2660	57005	21	385000
Revenue (€ millions)	494	8851	807	21455	1	202692
Sales (€ millions)	494	8679	771	21135	0	202386
Value Added (€ millions)	459	2943	286	7626	1	60090
Investment (€ millions)	494	288	4	6949	-63022	115321
Productivity (€ per worker)	459	147732	94112	382901	10577	7810338
Size (log of total assets)	494	20.813	20.760	2.553	15.429	26.776
Debt-to-Assets	493	0.923	0.955	0.118	-0.465	1.000
R&D Intensity	352	0.027	0.001	0.075	0.000	0.803
Cash at Hand (€ millions)	494	0.293	0.153	0.750	0.000	10.972

Table 3: Effects of State Aid on Recipient Firms

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	0.273*	0.590*	0.440	-0.006
	(0.159)	(0.311)	(0.346)	(0.726)
Size	0.099***	0.053	0.044	-0.258***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	6004	6008	6008	5378
Within R2	0.044	0.076	0.009	0.030

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effects of State Aid on Firms by Size

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	4.947**	8.515**	4.598	4.910
	(2.198)	(4.106)	(5.607)	(10.479)
State Aid X Size	-0.233**	-0.393*	-0.206	-0.243
	(0.106)	(0.200)	(0.275)	(0.523)
Size	0.098***	0.052	0.044	-0.258***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	6004	6008	6008	5378
Within R2	0.044	0.077	0.009	0.030

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effects of State Aid on Firms by Debt-Servicing Capacity

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	0.312*	0.621*	0.560	0.062
	(0.163)	(0.318)	(0.355)	(0.762)
State Aid X EBITDA-to-LongTermDebt	-0.016***	-0.036***	-0.031***	-0.102
	(0.002)	(0.002)	(0.006)	(0.120)
EBITDA-to-LongTermDebt	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Size	0.094***	0.035	0.057**	-0.279***
	(0.024)	(0.037)	(0.029)	(0.053)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	5278	5282	5282	4853
Within R2	0.049	0.094	0.020	0.032

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effects of State Aid on Firms by Liquidity

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	0.638**	2.243***	1.619*	0.275
	(0.276)	(0.818)	(0.834)	(1.422)
State Aid X Sales-to-Liabilities	-0.141*	-0.575**	-0.412*	-0.151
	(0.075)	(0.239)	(0.230)	(0.385)
Sales-to-Liabilities	-0.006	-0.026***	-0.011*	-0.043***
	(0.004)	(0.007)	(0.006)	(0.011)
Size	0.099***	0.062	0.059*	-0.316***
	(0.026)	(0.041)	(0.032)	(0.059)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	5010	5025	5025	4476
Within R2	0.041	0.073	0.013	0.038

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Effects of State Aid on Firms by R&D Intensity

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	0.175	0.381*	0.202	-1.471
	(0.230)	(0.226)	(0.371)	(1.935)
State Aid X R&D Intensity	-3.831	-5.330	-4.095	4.678
	(6.173)	(6.430)	(5.225)	(17.649)
R&D Intensity	-0.465	-0.995**	-0.582*	-0.521
	(0.283)	(0.434)	(0.333)	(0.811)
Size	0.107**	0.113*	0.031	-0.153
	(0.044)	(0.063)	(0.063)	(0.116)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	1502	1501	1501	1309
Within R2	0.062	0.111	0.030	0.018

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Effects of State Aid on Firms by Firm Age

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	0.654*	2.063***	1.019	1.217
	(0.340)	(0.689)	(0.771)	(1.281)
State Aid X Age	-0.006*	-0.021***	-0.008	-0.023
	(0.003)	(0.007)	(0.008)	(0.017)
Age	-0.017***	0.008*	-0.011***	0.043***
	(0.003)	(0.004)	(0.004)	(0.007)
Size	0.097***	0.056	0.033	-0.264***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	5970	5973	5973	5347
Within R2	0.044	0.077	0.009	0.031

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Effects of State Aid on Firms by Firm Age and Size

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	4.666**	7.288*	4.174	3.335
	(2.103)	(3.901)	(5.379)	(10.303)
State Aid X Size	-0.206**	-0.267	-0.161	-0.108
	(0.099)	(0.190)	(0.264)	(0.531)
State Aid X Age	-0.004	-0.019***	-0.007	-0.022
	(0.003)	(0.007)	(0.007)	(0.018)
Age	-0.017***	0.008*	-0.011***	0.043***
	(0.003)	(0.004)	(0.004)	(0.007)
Size	0.097***	0.056	0.033	-0.264***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	5970	5973	5973	5347
Within R2	0.044	0.078	0.009	0.031

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Spillovers of State Aid on Other Firms

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid Spillover	-0.133*	-0.246**	0.039	-0.522**
	(0.071)	(0.117)	(0.155)	(0.256)
Size	0.099***	0.053	0.044	-0.258***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	6004	6008	6008	5378
Within R2	0.044	0.077	0.009	0.031

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Effects of State Aid on Firms and Spillovers

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	0.268*	0.580*	0.441	-0.040
	(0.158)	(0.310)	(0.347)	(0.728)
State Aid Spillover	-0.132*	-0.244**	0.041	-0.522**
	(0.072)	(0.117)	(0.155)	(0.256)
Size	0.099***	0.053	0.044	-0.258***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	6004	6008	6008	5378
Within R2	0.044	0.077	0.009	0.031

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Effects of State Aid with 2-Years Later

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid	-0.081	-0.126	0.432	-2.024
	(0.686)	(0.304)	(0.414)	(1.292)
State Aid Spillover	-0.208*	-0.460***	-0.283	-0.374*
	(0.116)	(0.177)	(0.189)	(0.203)
Size	0.040	-0.052	-0.178***	-0.252***
	(0.034)	(0.042)	(0.041)	(0.065)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	4637	4647	4647	4092
Within R2	0.034	0.027	0.032	0.027

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Effects of State Aid on Firms and Spillovers by Market Concentration

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
SA	0.270*	0.578*	0.439	-0.050
	(0.158)	(0.311)	(0.346)	(0.725)
SAS	0.112	0.305	-0.144	0.058
	(0.156)	(0.248)	(0.373)	(0.392)
SAS X HHI	-0.408*	-0.943**	0.312	-0.977
	(0.231)	(0.391)	(0.766)	(0.618)
HHI	-0.020	0.029	0.007	0.052
	(0.022)	(0.035)	(0.031)	(0.055)
Size	0.098***	0.054	0.044	-0.256***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	6004	6008	6008	5378
Within R2	0.045	0.077	0.009	0.031

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Placebo Test of the Effects of State Aid

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
Randomized State Aid	0.100	0.334	0.135	0.298
	(0.110)	(0.255)	(0.147)	(0.399)
Randomized State Aid Spillover	0.000	0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)
Size	0.099***	0.053	0.044	-0.258***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	6004	6008	6008	5378
Within R2	0.044	0.076	0.009	0.030

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 15: State Aid and Covid-19 Pandemic

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
SA	0.191	0.418*	-0.081	-0.599
	(0.177)	(0.238)	(0.329)	(0.910)
SA X I(Pandemic)	0.133	0.291	0.883*	0.790
	(0.239)	(0.424)	(0.476)	(1.020)
Size	0.099***	0.053	0.044	-0.258***
	(0.022)	(0.035)	(0.027)	(0.047)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	6004	6008	6008	5378
Within R2	0.044	0.076	0.009	0.030

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 16: Spillovers of State Aid to Other Listed and Nonlisted Firms

	(1) Employment	(2) Revenue	(3) Investment	(4) Productivity
State Aid Spillovers	-0.264***	-0.183***	-0.147***	0.048
	(0.029)	(0.054)	(0.030)	(0.033)
Size	0.037***	0.052***	-0.018**	-0.042***
	(0.005)	(0.014)	(0.007)	(0.006)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
N	289,372	430,924	430,924	198,427
Within R2	0.027	0.048	0.005	0.052

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A Data Definitions and Sources

Table A1: Variable definitions

Name	Definition
State Aid Shock	Percentage movement in a company's stock price that exceeds the overall market movement on the official announcement date.
State Aid Spillover	sum of state aid shocks in the same NACE2.0 4-digit sector but in different countries, adjusted by the size of the firms.
Employment	Total number of employees included in the company's payroll.
Investment	Change in total assets between two periods.
Sales	Net sales.
Revenue	Total operating revenues (Net sales + Other operating revenues+ Stock variations). The figures do not include VAT. Local differences may occur regarding excises taxes and similar obligatory payments for specific market of tobacco and alcoholic beverage industries..
Value Added	Profit for period + Depreciation + Taxation + Interests paid + Cost of employees.
Productivity	Value added per worker.
Size	Total Assets = Fixed assets + Current assets.
Liquidity	Sales Coverage Ratio = Sales / Liabilities.
Debt Servicing Capacity	EBIDTA / Long-Term Debt.
Debt-to-Asset	(Total assets - Issued Share Capital) / Total Assets.
Cash	Cash and cash equivalent.
Age	Number of years since firm's inception.
R & D Expenses	Total amount of expenses on research and development activities.
R&D Intensity	Ratio of R & D expenses to sales.
Market Concentration (HHI)	Herfindahl–Hirschman index using revenues at the NACE 4-digit level.

Note: The variables are based on Orbis but differ from the original Orbis dataset due to various data cleaning processes. Additionally, the financial variables are deflated versions provided by this dataset. Non-capital monetary variables are converted from nominal to real variables, denominated in 2015 constant U.S. dollars, using deflators from various sources (OECD, Eurostat, CEIC database, government websites). For capital variables, we use the World Bank's World Development Indicators (WDI) investment deflators at the country level.

PUBLICATIONS