Essays in empirical economics: How state ownership affects corporate finance decisions of commercial companies

Dissertation

zur Erlangung des Doktorgrades

der Wirtschafts- und Sozialwissenschaftlichen Fakultät

der Eberhard Karls Universität Tübingen

vorgelegt von

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aus Schwäbisch Hall

Tübingen

2018

Tag der mündlichen Prüfung:	08.02.2019
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Erklärung an Eides statt

Ich erkläre hiermit, dass ich die zur Promotion eingereichte Arbeit mit dem Titel: "Essays in empirical economics: How state ownership affects corporate finance decisions of commercial companies" mit Ausnahme der in der "Declaration of co-authorship" aufgeführten Einschränkungen selbständig verfasst habe. Ich habe nur die angegebenen Quellen und Hilfsmittel benutzt und wörtlich oder inhaltlich übernommene Stellen als solche gekennzeichnet. Ich versichere an Eides statt, dass diese Angaben wahr sind und dass ich nichts verschwiegen habe. Mir ist bekannt, dass die falsche Abgabe einer Versicherung an Eides statt mit Freiheitsstrafe bis zu drei Jahren oder mit Geldstrafe bestraft wird.

Lukas Jakob

Declaration of co-authorship

I hereby declare that this thesis contains material that resulted from joint research, in particular:

Chapter 3 is joint work by Lukas Jakob and Georg Wamser. The research idea was jointly developed and improved through mutual correspondence and discussion. The estimation strategy was proposed by Georg Wamser and implemented by Lukas Jakob. Both authors contributed equally to the writing of the text.

The research idea behind Chapter 4 is uniquely from Lukas Jakob. The estimation strategy borrows from Georg Wamser's approach in Chapter 3 but is slightly modified by Lukas Jakob, who also did the whole writing.

The research idea behind Chapter 5 is mainly from Lukas Jakob but benefited from useful comments by Sebastian Olschewski. Lukas Jakob implemented the field data chapters 5.2-5.5.The experimental Subchapter 5.6 is joint work with Sebastian Olschewski and Ulrich Schmidt, who greatly contributed in the design and implementation of this Subchapter. The writing was done mostly by Lukas Jakob, with contributions from Sebastian Olschewski in Subchapters 5.1 and 5.6.

The sole author of the introductory and concluding Chapters 1, 2, and 6 is Lukas Jakob.

Lukas Jakob

Previous publication

This thesis includes three original manuscripts, all of which are intended for later publication:

Chapter 3, co-authored with Georg Wamser, is an unpublished manuscript.

Chapter 4 is published as a working paper in the Tübingen Research School of International Taxation.

Chapter 5, co-authored with Sebastian Olschewski and Ulrich Schmidt, is an unpublished manuscript.

Acknowledgements

I especially thank my supervisor Georg Wamser for useful comments and continuous support during this thesis. His support was by no means restricted to the jointly elaborated Chapter 3.

I would also like to thank my co-authors of Chapter 5, Sebastian Olschewski and Ulrich Schmidt. Especially Sebastian Olschewski contributed to all chapters in the form of useful comments and as a willing partner in academic discussion.

Special thanks go to my wife, Maria Gürlich, without whom this thesis would not have been possible. Her useful remarks, ever-open ear, and endless patience were essential during the last three years. I would also like to thank my parents, Jutta and Stefan Jakob, who have supported me throughout my entire life.

Numerous other people have read and commented on early manuscripts. In particular, I thank my brother, Martin Jakob, and my friends, Martin Weidner, Arne Stanneck, and Benjamin Hageloch.

Lukas Jakob

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1 Introduction

The philosopher Thomas Hobbes argued that the state brings order into chaos by imposing a common set of rules on individuals if they forgo some personal freedoms.[1] In modern economic terminology, we can interpret his idea in the context of transaction costs: By sticking to the same legal framework within a state, individuals may forgo some freedoms but can rely on the validity of the framework when conducting transactions with other people. This order increases trust between economic agents and consequently, lowers their transaction cost. What the state is to *public* order, the firm is to *private* order. In his seminal work on the theory of the firm, Coase (1937) argues that firms exist because transaction costs within the firm can be lower than within a market environment. As a consequence, economic agents organize in firms for some activities and conduct spot market transactions for others.

Thus, states and firms both represent different levels of human organization and governance, which have often become intertwined. Under socialism, most companies were part of the state. At other times, some firms – such as the Dutch East India company or the American United Fruit company – acquired state-like powers. While these examples belong to the realm of history, the focus of this doctoral thesis lies on the empirical analysis of a hybrid organizational form, which is widespread in the modern world: State-owned enterprises (SOEs). Modern SOEs operate as companies mostly in market environments and have the state as a substantial shareholder. Therefore, this thesis is settled in the sphere of empirical corporate finance. It uses modern econometric methods to analyze how state ownership affects corporate decisions and perceptions as well as how these feedback into balance sheet metrics. Due to the nature of the government shareholder, the research questions also extend to the fields of public finance and public policy.

Economic interaction of individuals is central to the institution of the firm: Alchian and Demsetz (1972) define the firm as the centralized contractual agent in a team productive process. Economic production is complicated and relies on a vast number of relations between suppliers, producers, workers, and clients. Each economic agent has its obligations and rights within this process. However, it is impossible to define *complete contracts* for such relationships, i.e., contracts that precisely state obligations and rights for all parties in all states of the world. Firms, as institutions of private order, facilitate contracting when costs to use individual market contracts are too high (Williamson, 1973, 2002). These costs derive from transaction costs and market im-

[1] See, for a recent edition, Hobbes and Gaskin (1996).

perfections and lead to hierarchic organizational forms. In public finance, there is a need for public ordering: "Politics is a structure of complex exchange among individuals, a structure within which persons seek to secure collectively their own privately defined objectives that cannot be efficiently secured through simple market exchanges" (Buchanan, 1987). From an institutional perspective, there is little conceptual difference between governments and firms (Tirole, 1994). Indeed, the theory of the firm has many common patterns even with early public order theories of the state: In essence, we can interpret the chaos of Thomas Hobbes as a very high transaction cost that renders a society dysfunctional. Hobbes' imposition of law on citizens restricts their freedom for a common benefit: Order. In modern democracies, citizens delegate legislation to parliaments and governments. The reasoning is very similar compared to the theory of the firm: Transaction costs and market imperfections such as public goods and externalities can lead to prohibitively high costs for the individual. A significant consequence from this is that types of governance (the state, firms, markets, or hybrids such as SOEs) differ structurally in their attributes, costs, and competencies and can handle a different portfolio of transactions efficiently. The most relevant type of governance in this thesis is the firm.

We can understand the firm as an economic institution of contracting and thus go beyond the traditional interpretation of economics as a science focusing on choice only (Williamson, 2002). This helps to describe human actors in more realistic terms. A firm acts like a mini-society with a vast array of norms beyond those centered on the exchange and its immediate processes. The contracting perspective further allows to distinct transactions between faceless instantaneous firm exchanges at equilibrium prices towards situations where the identities of the parties matter (Ben-Porath, 1980).

What defines the identity of a firm? Numerous attributes seem of importance: First, there is the nature of the product the firm provides. Is it a common good or a public good? Second, there is the extent of vertical integration: Does the firm incorporate the whole production process or only a part of it? Third, there is ownership structure: Who holds the residual control rights and what is the purpose of ownership (Grossman and Hart, 1986)? And fourth, there are agency conflicts and the incentive structure within the firm. Thus, analysis of the firm as an institution of private ordering matters precisely because of different firm identities. Study of state ownership is vital because contracts are incomplete and governments still own and operate substantial shares of the economy (Laffont and Tirole, 1991; Tirole, 1994; Hart et al., 1997).

Considering the significant privatizations in the 1980s and 1990s, it is perhaps surprising that governments acquired more assets (USD 1.52 trillion) than they sold (USD 1.48 trillion) over the 2001-2012 period (Fotak and Megginson, 2015). In countries

of the Organization for Economic Cooperation and Development (OECD) alone, the governments own 0.86 trillion USD in commercial minority holdings and 2.41 trillion USD in commercial majority holdings (OECD, 2014b, 2017). These figures do not even take into account the immense state holdings of emerging economies such as China, India, and Brasil.

Numerous theories have tried to explain why the state acts as owner, and why empirical literature identifies substantial differences between SOEs and privately owned firms. Chapter 2.1 introduces the three main theories of state ownership and presents important implications in more detail. Chapter 2.2 summarizes empirical literature along the theoretical considerations and also introduces empirical results from related topics such as politically connected firms.[2] Broadly, we can divide the literature into three periods: Economists conducted a first prominent strand of research before and during the transition period of Eastern Europe and China. A major issue during this period was the privatization of government assets in these countries. As former socialist economies became global economic actors, the focus shifted on state capitalism, often (but not exclusively) centered on Chinese SOEs. With the emergence of large sovereign wealth funds (SWFs), economists have looked at asset allocation and investment policies of the state as an investor. Relatively little research exists, however, on state ownership in market environments with institutionalized competitive ownership neutrality. This thesis extends this last strand of literature by empirically analyzing SOEs in the EU and OECD countries. It contributes to the literature on different levels: First, it answers so far neglected research questions related to previous literature, especially in the context of taxation. Second, it replicates established results using new datasets, which are bigger than in most other studies, and third, it introduces new arguments which could help to explain the established empirical characteristics of SOEs.

This thesis is an empirical work. All content chapters treat topics related to state ownership by developing new hypotheses and testing them using extensive datasets consisting of firm-level balance sheet data (Orbis). To do so, they rely on empirical comparisons between SOEs and private companies to isolate the effect of state ownership. Orbis contains current and historical ownership information from official sources and allows for identification of SOEs. A key issue to address is non-random state ownership, which could bias coefficient estimates. Depending on the research question, I employ either propensity score matching techniques or selection models to address non-random state ownership. Both are state-of-the-art methods in the empirical corporate

[2]Political connections can lead to preferential treatment by authorities and are therefore similar to state ownership.

finance literature and allow to correct for selection and other potentially critical issues such as endogeneity and unobserved heterogeneity (Li and Prabhala, 2007). The particular estimation procedure is described extensively in each content chapter. Besides, each chapter contains a detailed listing of data restrictions and applied data management in the chapter appendix.

The content chapters of this thesis represent independent research articles but are nevertheless closely related both content-wise and methodologically. Chapter 3 deals with debt-related tax incentives under partial state ownership. Chapter 4 focuses on the effective taxation of wholly owned commercial SOEs. Chapter 5 deals with implicit state guarantees and societal bias towards state enterprise. Chapters 3 and 4 share the focus on taxation within a state ownership context. They also use very similar estimation strategies. Results complement each other and contribute to a better understanding of why and how state ownership affects firm taxation. Chapters 3 and 5 relate because Chapter 5 investigates a key assumption of Chapter 3: Do SOEs benefit from preferential access to finance? Methodologically, Chapter 5 differs from Chapters 3 and 4 because at close consideration, a selection approach suits the research question better than a matching approach. The choice of estimation strategy should, however, not be interpreted as a binary choice of appropriateness: Both matching and selection models address data concerns such as selection adequately (Li and Prabhala, 2007). The experimental part of Chapter 5 indicates that the public-good function often attributed to state ownership may introduce a behavioral bias, which complements the more traditional incentive-based hypotheses in the other chapters.

The most crucial result across all studies is that state ownership is not neutral, even in developed market economies. It changes the incentives within and the perception of firms, and these feedback into corporate finance decisions of companies. In the following, I will briefly introduce the research questions of each content chapter and present their central results.

Chapter 3: The impact of taxes on debt financing under partial state ownership

This Chapter is joint work with Georg Wamser and looks at tax sensitivities and debt financing of mixed-ownership firms. In particular, it deals with the question of how partial state ownership affects the tax incentives of private co-owners. Previous studies suggest that state ownership facilitates access to debt financing because of implicit state guarantees to bondholders. Private co-owners of partial SOEs may exploit such guarantees to save taxes and increase the return on equity to private shareholders. We hypothesize that the latter behavior alters the cost-benefit trade-off between debt and equity financing in such a way that debt ratios under shared ownership are set at

a maximum level, irrespective of variations in tax rates. It is the implicit guarantee associated with state ownership that renders the cost of high debt levels less relevant. Therefore, the implicit guarantee primarily determines debt ratios and not the tax rate.

The dataset consists of balance sheet data of joint affiliates between SOEs and multinational enterprises (MNEs) from 22 OECD and EU member countries. We look at long-term leverage ratios of mixed-ownership firms and compare them with suitable privately owned companies. The effect of being partially state-owned is identified by first finding comparable control units of privately held affiliates for each SOE affiliate. The requirements for being accepted as a similar control unit are relatively strict, as matched pairs (of partially state-owned and private affiliates) must operate in the same country and the same sector. We then estimate pair-fixed effects regressions and further control for time-fixed effects, as well as time-varying control variables, which previous literature identifies as relevant determinants of debt ratios.

Our empirical analysis supports the hypothesis of a maximum debt level: We find debt ratios of shared ownership companies to be higher but unresponsive to changes in tax rates. The fact that partially state-owned affiliates are found to use substantially more debt suggests that these firms operate under implicit state guarantees. Previous empirical results relating to weak SOE monitoring let us conclude that privately owned MNEs make use of facilitated access to debt to maximize interest tax shields in such affiliates. Additional results in this Chapter indicate that MNEs do not exploit shared ownership for international tax planning.

We confirm our central finding in many robustness tests, including different matching procedures, alternative outcome measures, and checks where we randomly assign treatment status. The main conclusion of this Chapter is that partially state-owned firms neglect the cost of debt and employ a maximum attainable debt ratio. This suggests that government participation may enable MNEs to reduce tax payments, which may ultimately lead to a loss of revenue to the public owner. A policy implication of this Chapter is that state owners should closely monitor the capital structure of mixed-ownership firms. Otherwise, there could be a hidden and so far neglected cost to the state owner in the form of lower tax receipts.

Chapter 4: Is commercial state ownership tax neutral?

A pillar of the European Union's (EU) single market is the prohibition of state aid: Governments must not discriminate against some firms by offering tax advantages to others. EU regulation aims to ensure competitive neutrality among all commercially active firms. However, national competition authorities monitor adherence to EU competition law to a large extent, which has lead to substantial differences in monitoring quality across member states. Motivated by this institutional setting, this Chapter an-

alyzes the tax neutrality of state ownership in EU member states. Governments could subsidize their SOEs with low taxes out of political considerations (to secure employment) or use taxes to force distributions under dividend-averse managers. The Chapter, therefore, compares three different specifications of the effective tax rate (ETR) of wholly owned commercial SOEs with those of similar private firms. To the best of my knowledge, the Chapter is the first to address the tax neutrality of SOEs in a European context.

The analysis uses single financial statement data from 18 EU member countries and employs selection-on-observables propensity score matching to identify for each SOE a suitable control group of private firms. The control group consists of firms within a relatively strict caliper that also operate in the same country as the SOE. In outcome regressions, I control for group-fixed effects (based on the best matches), a vector of time-varying firm characteristics that have been used in previous studies as well as time, and sector effects. Thus, the estimation approach is similar to Chapter 3 and controls for non-random selection into state ownership.

Results indicate that commercial SOEs pay, on average, higher effective tax rates than comparable privately owned firms. Hence, results do not suggest that governments subsidize commercial SOEs. A likely explanation is the budgetary importance of commercial state ownership, which makes governments force distributions from their commercial companies via tax payments. A complementing explanation may be higher levels of tax-planning in private firms. Additional results indicate that this effect is not equally present in all member states.

The Chapter contains an extensive set of robustness tests. In particular, it analyzes if the central finding extends to variations in model specification and parametrization, consolidated financial statement data, long-term specifications of the effective tax rate, and different estimation techniques. It also verifies if results are a mere statistical coincidence by conducting an extensive falsification test with pseudo-ownership assignment. All these tests confirm the main finding of higher SOE ETRs. The central conclusion of this Chapter is that tax neutrality in the EU's single market remains imperfect towards ownership structure, and the extent of imperfection depends on the member state.

Chapter 5: State ownership, company risk, and societal value

A key feature of state ownership is that bondholders of SOE debt assume implicit state guarantees. Whereas Chapter 3 takes this as an established result, Chapter 5 first analyzes an extensive firm-level data set to see if implicit guarantees are indeed present. Thus, the central part of this Chapter replicates an established result using a much bigger dataset than previous studies, which consists of non-listed firms. The last part of this Chapter reports experimental results and introduces a behavioral argument

that could help to explain lower financing costs for SOEs. The experimental Section is among the first to introduce behavioral arguments into the literature on ownership structure and is joint work with Sebastian Olschewski and Ulrich Schmidt.

The Chapter starts by examining how firm performance affects the cost of debt. It uses financial statement data from 15 EU countries in 2006-2015 and employs an endogenous selection model that allows addressing potential unobserved heterogeneity and endogenous independent variables. The estimation approach differs from Chapters 3 and 4 for several reasons: First, we are mainly interested in coefficient differences between two ownership groups for our profitability variable. Second, we cannot exclude differential impacts depending on ownership for other control variables. Third, we believe it is vital to include unobserved information correlated with selection as an additional control variable to limit endogeneity concerns in our regression models. We achieve the latter by calculating and adding the inverse Mill's ratios from a state ownership selection model. For these reasons, we opt for an estimation strategy that treats SOEs and private firms as two different regimes and controls for endogenous selection into them.

Results show that (i) SOEs, on average, pay less for their debt and (ii) the more profitable a private firm, the less it pays for its debt. Whereas an increase in operating profitability decreases interest rates of private companies, firm profitability is not a relevant determinant for SOE interest rates. The reason behind the second result is that profitability functions as a risk proxy. The relation does not hold, however, for SOEs, which points to implicit guarantees. However, the complete absence of a relationship between our company risk proxy and the cost of finance for SOEs suggests that other factors than risk are relevant, too. This is because, in the EU, commercial SOEs and private firms are subject to the same bankruptcy framework, i.e., risk should also matter for SOEs. The differential result is very robust, even under situations of severe sovereign financial distress. Other sensitivity tests include instrumental variables, variations in the profitability measure, modifications in panel balance, and changes in model specification and parametrization.

Previous literature has identified political bias as a determinant of SOE financing costs. Such a bias could mainly exist with state investors, for instance, state-owned banks. Motivated by the argument that SOEs often exist to address market imperfections, we analyze if social concerns could be an additional factor that provides cheaper financing for state companies. Our experimental approach uses questionnaires to determine investment decisions of participants. They choose between a project that generates a private return only (imitating a private firm) and a project that is less efficient but creates a payoff to society in addition to regular returns (mimicking a state-owned com-

pany). Results indicate that investment into the social but inefficient project is positive in all specifications of the investment decision. We take this finding as evidence that people may favor state ownership in some occasions because of a societal return, even if this is inefficient.

This Chapter confirms previous empirical results on implicit guarantees, which indicate a competitive advantage of commercially active SOEs . Additional experimental results suggest that SOEs may also benefit from a behavioral bias in society. The Chapter is thus among the first to highlight the behavioral implications of ownership, which could be an exciting direction for future research.

The rest of this doctoral thesis proceeds as follows: Chapter 2 provides a brief introduction to theoretical and empirical aspects, which the literature has associated with state ownership. Then follow the three content Chapters 3, 4, and 5 that were briefly described above. The thesis concludes with a summary in Section 6 and the bibliography.

2 State ownership of firms

2.1 Theoretical approaches towards state ownership

Three major theories exist to explain and describe state ownership of companies. Atkinson and Stiglitz (2015) framed the "social view", which suggests that SOEs exist to mitigate market failures and increase overall welfare. For example, private postal operators have no incentive to service remote areas because the profit generated by a few parcels does not cover the costs of delivery. Similar arguments arise for other network industries such as railways and electricity. In such cases, the state as an owner may accept lower profit levels for the sake of providing a specific service. The assumption is then that the non-discriminatory provision of such a service is beneficial to society as a whole. The social view also extends to state ownership of financial institutions. The argument is then that private actors may be reluctant to invest in projects that yield societal benefits but low financial returns. An example is Germany's *Kreditanstalt für Wiederaufbau* which focuses on investments with added value in the environmental and social sphere. The social view is dominant in the European political sphere, where massive public investment funds have been established in recent years.[3] A vital feature

[3]The European Fund for Strategic Investments (EFSI) uses public money to compensate private investors for the increased risk of eligible investment projects. Eligibility rests on criteria with assumed long-term welfare implications such as strategic infrastructure, renewable energy, and others.

of the social view is that it assumes a societal benefit from state ownership that would be absent if companies were private (and therefore profit-maximizing). Chapter 5 analyzes whether the underlying assumption of this view influences firms' cost of finance. It is important to note that often, it is complicated to disentangle commercial and public benefit. Besides, all economic sectors of the EU/OECD countries see private firm activity, which makes it very difficult to determine when state ownership is preferable on welfare grounds.

The second "political view" on state ownership of firms emerged during the transition from planned to market economies. Shleifer and Vishny (1994) postulate that SOEs are a means to achieve the goals of politicians. Such goals may be diverse and range from maximizing employment, securing voters' happiness, or achieving geopolitical agendas. Inefficiency arises because politicians use SOEs to transfer resources to their supporters or use them to pursue private agendas (Cox and McCubbins, 1986; Shleifer, 1998; John et al., 2008). Political agendas are facilitated by the fact that taxpayers – as ultimate owners of SOEs – face an extreme kind of separation between ownership and control. Dinc and Gupta (2011) show that, whereas efficiency gains from privatization are dispersed among the population, the costs (for example in the form of layoffs) are concentrated among a few – especially among workers and politicians of the region the privatized firm operates. Not all agents in society are organized equally, which gives well-organized interest groups – such as workers and politicians – an advantage in deciding resource allocations towards SOEs. The additional cost, for instance in the form of excess labor, is not internalized by anyone with sufficient powers to influence the government. Instead, the general population relies on politicians and civil servants to act in their interest (Bennedsen, 2000). Naturally, politicians may want to mask their vested interests and pretend not to meddle in SOEs, which sometimes makes evidence for the political view difficult to disentangle from conventional agency problems. Nevertheless, plenty of likely anecdotal evidence exists: For example, the European Commission sent a *Statement of Objections* in April 2015 to the Russian SOE *Gazprom* for alleged abuse of its dominance on Central and Eastern European gas supply markets.[4] Another recent potential example is German state bank *Kreditanstalt für Wiederaufbau*'s intervention (on behalf of the German government) to keep the insolvent carrier Air Berlin flying during the pre-election summer holidays in 2017.

The third "agency view" borrows the notion of the social view that SOEs mitigate market failures but integrates arguments of the political view. Banerjee (1997) develops

[4]The statement carried a clear political dimension because it accused Gazprom of abusing its pricing policy to achieve political ends, such as participation of SOEs from Eastern European countries in specific pipeline projects.

a theory of *misgovernance* of how a benevolent government my create misallocation and corruption which rests on the facts that (I) governments respond to market failures, and (II) the government has agents who maximize private benefits and not the general welfare. The agency view explains well the empirical fact that state ownership continues to be widespread in the network industries and why it leads to lower corporate governance quality at the same time (Borisova et al., 2012).

Each view on state ownership is theoretically plausible, but which is the most relevant? Most empirical studies support the political and the agency views, i.e., SOEs lead to a welfare loss caused either by deliberate political actions or by agency conflicts. It is important to note, that it may not always be possible to distinguish the political and agency views empirically. They are also not mutually exclusive. There is, however, little factual evidence in the literature that SOEs raise welfare. Nevertheless, the social view remains very popular in the political sphere and public discussion. In the context of this thesis, all three views are relevant to develop the research questions and interpret the empirical results.

Economic theory as also come up with numerous arguments on how state ownership may affect firm behavior and incentives of the management. I will summarize the most relevant for this thesis in the following.

The first argument is that state ownership induces a soft budget constraint. The term was coined by Kornai (1979, 1980, 1986) to describe persistent shortages in state-dominated socialist economies (exogenous cause) but is now also understood as an endogenous time inconsistency problem (Dewatripont and Maskin, 1995). Hence, it can be generalized as a situation in which a financing entity cannot credibly commit itself not to bail out a creditor or, analogously, re-invest in a project with positive expected profit and sunk cost (Kornai et al., 2003). Consequently, a soft budget constraint may give rise to moral hazard at the creditor level.[5] Soft budget constraints are naturally intimately related to the phenomenon of implicit guarantees, which we can interpret as the perception of soft budget constraints by investors (Kornai et al., 2003). Implicit guarantees potentially affect borrowing costs and financing behavior and are of particular relevance in content Chapters 3 and 5.

A second argument is low-powered incentives. They arise in a situation where a

[5]Laffont and Tirole (1993) argue that the conventional wisdom of only SOEs suffering from soft budget constraints is not as straightforward as it seems because governments have bailed out private firms as well. Lin and Tan (1999) argue that once the state has a strategic interest in a firm (including private ones), a soft budget constraint may arise. An additional aspect that may determine the degree of softness is the closeness between a borrowing company and the lending financial institution (Rizov, 2008). The concept is also not necessarily limited to firms: Numerous studies have argued that country bailouts or quantitative easing programmes carry a moral hazard risk (Vaubel, 1983; De Grauwe, 2013). Consequently, they may soften budget constraints.

transacting party is unable to collect full returns from a transaction (Williamson, 1985; Tirole, 1994; Banerjee, 1997). Low-powered incentives caused by state ownership affect both, the company management and the owning government institution. On the management level, the degree of ownership matters as low-powered incentives will arguably be less relevant in minority-owned listed SOEs (with private co-owners) compared to wholly state-owned firms. Especially in wholly owned SOEs, managers cannot collect share packages as bonus payments because this would amount to privatization. As a consequence, they may care less about firm valuation and stock market performance and prefer to live a "quiet life" (Hicks, 1935; Bertrand and Mullainathan, 2003). The controlling state institutions such as ministries or government agencies may also be affected by low-powered incentives because they too act as fiduciaries for the general public and not as owners. Besides, political fortunes are unlikely to be very sensitive to SOE performance in normal times, which limits politicians' incentives to monitor enterprise management and leads to weaker monitoring of SOEs (Vickers and Yarrow, 1991; Musacchio and Lazzarini, 2012). Nevertheless, politicians may have high-powered incentives concerning politically sensitive SOEs under exceptional circumstances, for instance, if the fate of a specific company could significantly influence an election result (Frant, 1996; D'Souza and Nash, 2017). This is in line with the political view because private benefits are unusually large in such a situation. Differences in management and board incentives between SOEs and private firms are of relevance in content Chapters 3 and 4: Weak monitoring by the state shareholder enables the private co-owners to set maximum debt levels in Chapter 3. Low-powered management incentives to engage in tax planning may partly explain higher ETRs of SOEs.

2.2 Empirical aspects of state ownership

SOEs are essential elements of many national economies (OECD, 2017). Although the intensity of state ownership has ebbed and flowed with the prevailing political doctrine, states have always actively engaged in economic activity. Recently, the trend has been going towards more significant state holdings: Guedhami (2012) reports a clear upwards trend in government investment activities since the late 1980s, which culminates in very high shares of government acquisitions during the Asian crisis of 1998 and the financial crisis of 2008 and 2009. European governments were particularly active buyers, with seven European countries ranging in the top 15.[6] On the sectoral level, most investments were in the finance/real estate sectors as well as utilities. Guedhami (2012) notes that

[6]The countries are the U.K., the Netherlands, Germany, Belgium, Ireland, France, and Luxembourg.

an increasing share in these transactions is due to SWFs, which account for around one-fifth of total purchases by government investors.

State ownership, however, may not always be interpreted as a binary ownership situation irrespective of the ownership share, the owning government institution, and purpose of ownership. For example, a meta-analysis by Djankov and Murrell (2002) shows that commercial SOEs perform better than traditional SOEs (with public policy obligation) and may even – under specific circumstances – be preferable to private ownership. For this reason, all content chapters of this doctoral thesis precisely define state ownership in their respective contexts. Musacchio and Lazzarini (2012) summarize the most frequent manifestations of state ownership, which they sum up as varieties of state capitalism.

First, there is conventional state ownership of a company by a country's central government or regional and local entities. Table 2.2.1 contains numbers from an OECD (2017) survey on company ownership by central governments across the globe. As can be seen, the Chinese central government is a full or majority owner of 51,341 commercial SOEs, with a combined estimated value of 29.2 trillion USD. Commercial state ownership outside China is substantial as well: Survey countries own a full or majority share in 2,467 companies valued at 2,4 trillion USD. Additionally, OECD countries have minority stakes worth 0.86 trillion USD (OECD, 2014b). Often, partial state ownership consists of post-privatization residual holdings, i.e., companies which were initially wholly owned by the state and were privatized to some extent. This pattern is notably present in the network industries of the EU and some OECD countries as well as the former transition economies in Eastern Europe.[7]

Second, there is government ownership for investment purposes. Since 2008, more than 25 countries have created SWFs, which invest in company equity and held a total of 4.5 trillion in 2014 (Fotak and Megginson, 2015). Mostly, these are minority holdings and, at least on paper, serve a portfolio-diversification purpose.

Third, governments sometimes become owners for political reasons, for example when they act as an owner of last resort to preserve jobs or guarantee financial stability in formerly private companies. The OECD (2017) lists several cases in EU countries, where the purchase of government equity could point to political agendas. For example, the German government bought 25 percent of *Commerzbank* in early 2009 to ensure continued lending at the height of the global financial crisis.

Figure 2.2.1 presents the sectoral distribution of commercial centrally owned SOEs according to the OECD (2017). The left pie contains information on company equity

[7]The extent of privatization depends on the sector: For instance, residual holdings in the electricity and gas sector remain higher than in the telecommunications sector (OECD, 2017).

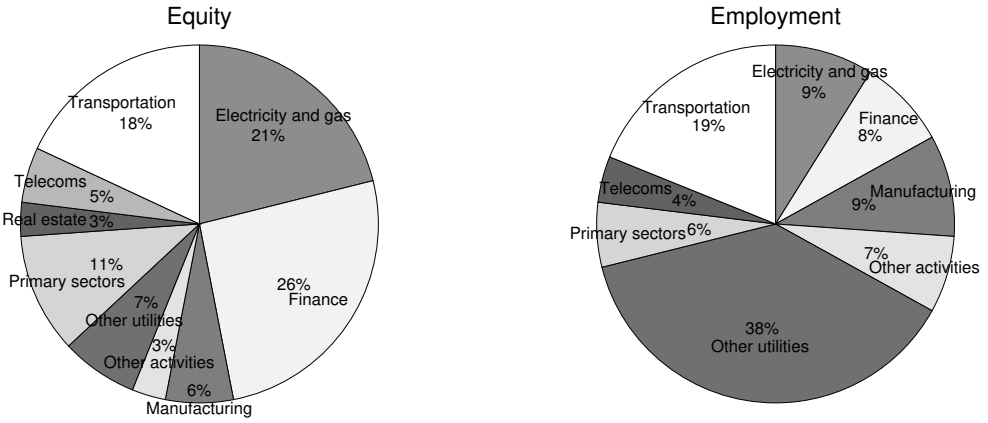
Table 2.2.1: Central SOEs in OECD survey countries

The table presents the number of firms, employees, and market value of commercial majority-owned central government SOEs; the company value is based on year-end estimates in 2015; the data does not include minority-owned SOEs or entities held by regional or municipal administrations; data is taken from the OECD (2017) survey.

Country	**Number of SOEs**	**Number of employees**	**Value USD bn**
Argentina	59	130,776	28
Australia	8	42,607	14
Austria	10	72,491	5
Brazil	134	597,505	145
Canada	44	83,462	30
Chile	25	50,361	21
China	51,341	20,248,999	29,201
Colombia	39	33,033	23
Costa Rica	32	43,013	13
Czech Republic	133	133,826	29
Denmark	21	18,728	14
Estonia	66	26,026	4
Finland	47	72,391	40
France	51	826,967	77
Germany	71	370,440	72
Greece	42	42,927	83
Hungary	370	148,193	9
Iceland	35	3,636	4
India	270	3,284,845	339
Ireland	25	39,079	10
Israel	28	57,114	
Italy	20	499,765	208
Japan	8	256,265	82
Korea	56	147,833	218
Latvia	71	49,962	8
Lithuania	128	40,711	6
Mexico	78	73,686	21
Netherlands	29	110,400	83
New Zealand	37	36,214	29
Norway	55	230,601	108
Poland	126	128,016	16
Slovakia	113	60,471	
Slovenia	37	47,052	13
Spain	51	94,635	37
Sweden	49	124,133	37
Switzerland	4	106,883	45
Turkey	39	438,990	63
United Kingdom	16	153,604	115
Total (excluding China)	2,467	9,238,528	2,408

across sectors and the right pie plots employment shares. Figure 2.2.1 shows that SOEs are present in most relevant economic areas, even though they predominate in network industries such as electricity, gas, and transportation. SOEs are also ubiquitous in the banking industry, where large state development banks have expanded substantially in recent years (Monnet et al., 2014). While the strong presence of SOEs in the network sectors is evidence for the social or agency views, state ownership in financial companies could point to political financing. Hence, the observed prevalence of SOEs does not unequivocally support one of the theoretical views.

Figure 2.2.1: Sectoral distribution of central SOEs (OECD, 2017)



It is important to note that Table 2.2.1 and Figure 2.2.1 only contain data for centrally-owned SOEs. Often, regional and local governments own significant shares in firms as well. For instance, in a recent report on state ownership in Europe, the European Commission reports that in Italy exist around 6,000 local SOEs that employ a total of 500,000 people (2.1 percent of total employment). Data on regional and local SOEs is much scarcer because they are seldom listed on stock exchanges (but may still be commercially active while operating as limited liability companies). The OECD (2017) estimates that only 45 percent of SOEs by value and 25 percent of SOEs by employment are listed on stock exchanges. Hence, the fact that this doctoral thesis draws on an extensive sample that includes ownership information for non-listed firms is a key distinction to previous empirical research in the field.

An extensive literature has analyzed state ownership of companies. Empirical studies focus on firm-level data analysis and interpret results in line with the theoretical approaches outlined in Section 2.1. However, studies have also added many stand-alone effects that extend the fundamental theories of state ownership. The literature is furthermore not exclusively from the economic sphere, as related fields such as business, law, and sociology have contributed significantly. An important issue of empirical work

is the specific SOE definition a study uses because results may well depend on the *type* of state ownership. Most studies analyze data of listed majority-owned commercial SOEs. This doctoral thesis, in contrast, mostly uses non-listed wholly state-owned subsidiaries (except for Chapter 3, which uses joint affiliates of SOEs and MNEs). Even in the latter case, affiliates are often non-listed firms. If firms are not listed, and therefore, less information is easily accessible, implicit guarantees may be more relevant compared to more transparent and publicly traded majority-owned SOEs. Seemingly contradicting results in the ownership literature can often – at closer scrutiny – be attributed to different specifications of ownership and the variables of interest.[8]

In line with the political view on state ownership, many studies focus on the meddling of state owners and vested interests in firms. In this respect, there is also a significant strand of literature focusing on political connections and their effects on firm characteristics and decisions. Though technically not the same thing, SOEs can be interpreted as a subgroup of politically connected firms because they usually have politicians or high-ranking government officials on their supervisory boards.[9] Hence, many results from the political connections literature seem relevant in an SOE context. Kahan and Rock (2011) discuss how – from a legal perspective – a state majority owner has a higher levy to impose its agenda on a firm than a private controlling owner. D'Souza and Nash (2017) present evidence that private benefits, which politicians can extract from state-controlled firms, dominate economic decision making. Political interference comes at a cost to the firm: Nasr et al. (2012) find that the cost of equity of partially privatized SOEs increases with the state share and that this increase is more pronounced if the privatizing government is considered more interventionist. Other studies confirm a negative impact of state ownership on firm value (Shirley and Walsh, 2000; Lin and Su, 2008; Borisova et al., 2012). Faccio (2006) finds that political connections do not unambiguously increase firm value, which moderates results from a previous study drawing on limited data from Indonesia (Fisman, 2001). A large SOE share in the economy may not only affect SOEs themselves but also have implications for other firms: For example, Brandt and Li (2003) show that banks discriminate against private firms to obtain benefits from connected politicians.

[8]One example is the positive bond spread that some studies find for SOEs in non-crisis times (Borisova and Megginson, 2011; Borisova et al., 2015). Both studies use spreads based on corporate bonds instead of standard credit contracts, which increases the distance between creditor and debtor and additionally limits their validity to very large firms that select into issuing corporate bonds.

[9]Faccio (2006) provides a comprehensive overview on political connections around the globe, which she defines as a situation where a substantial owner or key official (CEO, CFO, vice-president, or secretary) is a member of parliament, a minister, or closely related to a politician. This is the case with many central SOEs. In regional or local SOEs, board members may be mayors, or high-ranking officials of the respective government entities.

Political agendas seem to be especially present in state-owned banks: Khwaja and Mian (2005) find that state-owned banks lend more to politically connected firms, even though they have higher default rates. Dinc (2005) shows that state-owned banks increase their lending in election years relative to private banks. He also argues that the problem of political meddling may be stronger in banks than in non-financial companies. Similarly, Sapienza (2004) finds a political dimension in the landing behavior of state-owned banks, which charge lower rates in regions where related political parties are stronger. Nevertheless, state-owned banks may still be more efficient in allocating capital compared to direct transfers by governments (Cull and Xu, 2000).

The rise of SWFs has led to concerns that such investments are politically motivated (Keller, 2009). For example, SWFs could impose conflicting goals on their targets, such as securing investment and knowledge transfer into their jurisdictions. A survey on empirical literature by Fotak and Megginson (2015) argues that SWFs differ substantially in their asset allocation and professionalism across countries. However, concerns about political investment are mostly unfounded. SWFs differ, though, in their investment patterns from other large institutional investors (Chhaochharia and Laeven, 2008). In particular, they (i) invest in companies that are culturally close, (ii) they overinvest in equities and (ii) they invest in firms with large market capitalization. They further tend to chase past returns and hold poorly diversified portfolios both in geographic and industry terms. Especially point (ii) may still indicate a political dimension, targeted at constituents: It is easier to justify investments in big well-known companies with global brands than in diversified and complicated portfolios.

A large empirical literature deals with debt financing of SOEs and relates to the theoretical aspects of implicit guarantees and soft budget constraints. In the privatization context of the 1990s, many studies found evidence that SOEs use more debt than private (or newly privatized) firms.[10] While findings from this period probably relate to the ongoing transition process and institutional particularities, more recent studies have pointed at implicit state guarantees to explain financing cost differences across firms in market economies without a socialist past. For instance, the OECD (2014a) concludes from a big survey that SOEs access debt on the commercial marketplace, but – compared to private firms – at a lower cost. In China, non-traded SOEs have significantly higher leverage and easier access to long-term debt than private or foreign firms (Li et al., 2009). Private firms hold more cash than SOEs – a fact that further supports the presence of soft budget constraints of Chinese SOEs, especially

[10]See, for instance, Megginson et al. (1994), and for surveys Dewenter and Malatesta (2001) and Megginson and Netter (2001). A study by Mykhayliv and Zauner (2013) shows that the problem persists in transition countries where the pace of reform has been slow.

with state-owned banks (Megginson et al., 2014). Capital structure decisions and political interests may be intertwined, as multinational Chinese SOEs have higher leverage ratios as they promote national overseas strategies (Zhang and Zhang, 2016).

There is also evidence that the financing advantage may depend on the economic environment as implicit guarantees may be particularly important during times of economic crisis (Borisova et al., 2015). The authors conclude that in non-crisis times, SOEs may even face higher costs of debt compared to privately-owned firms because bond spreads reflect the lower quality of corporate governance in these firms. Borisova and Megginson (2011) reach a similar conclusion: They look at the effect of post-privatization residual holdings and find that credit spreads increase while the state share decreases but remains positive. Once a firm is entirely privately owned, it faces lower spreads and hence lower borrowing costs. By using corporate bond spreads of listed firms, both studies target a substantially different group of companies than the content chapters of this thesis (large firms that self-select into issuing bonds rather than obtaining bank debt).

Again, results from the literature on politically connected firms seem relevant: Two studies find higher debt levels and a higher bail-out probability for politically connected firms, while they are less profitable when compared to private peers on an accounting basis (Faccio et al., 2006; Faccio, 2010).

When it comes to performance measures such as profitability and efficiency, conventional wisdom has long associated SOEs with worse corporate performance. The theoretical underpinning of these assertions could lie in low-powered incentives of the management. Empirical studies mostly reach similar conclusions: Numerous studies and surveys document significant differences in performance between SOEs and private (or privatized) firms (Megginson and Netter, 2001; Dewenter and Malatesta, 2001; Estrin et al., 2009).[11] In China, Fan et al. (2007) show that partially privatized firms with bureaucrats as CEOs perform worse and suffer from low-quality board monitoring at the same time. The performance impact is not limited to former transition economies: Borisova et al. (2012) find that government ownership leads to lower corporate governance quality in the EU. The relation also holds when state owners are SWFs, i.e., state entities that claim to act as a non-controlling investor and not as a meddling owner. Bortolotti et al. (2015) find that sovereign wealth fund targets suffer from declining return on assets and sales growth three years after sovereign wealth fund investments.

[11] Dinc and Gupta (2011) note that governments are more likely to privatize profitable firms. However, most studies comparing private firms with privatized firms do not control for endogenous privatization decisions. Using an instrumental variables approach, the authors show that significant improvements in profitability and efficiency persist even under endogenous privatization.

In times of crisis, government ownership may also protect firms from losing value (due to a higher bailout probability), at least in countries with good institutions that protect private shareholders from being expropriated (Beuselinck et al., 2017). There is also evidence that SOEs allocate capital less efficiently compared to private firms: Chen et al. (2011) and Chen et al. (2016) show that private conglomerates in China allocate more internal capital to units with better investment projects than state firms do. Chen et al. (2017) find that government ownership decreases investment sensitivity for good projects. Jaslowitzer et al. (2016) look at European data and show that conservatism and stability-seeking characterize investment levels of SOEs. These empirical studies on SOE investment are in line with political investment agendas and a "quiet life" behavior of SOE managers.

The short literature survey above is not exhaustive and focuses only on the most important aspects of state ownership. Therefore, each chapter again reviews relevant studies for the research question at hand – including related literature from other fields of economics, such as debt financing, tax planning, effective taxation, and behavioral economics.

3 The impact of taxes on debt financing under partial state ownership

3.1 Introduction

An extensive literature analyzing SOEs argues that commercial lenders assume implicit state guarantees when providing credit. Consequently, SOEs should be able to issue more debt. Many empirical studies support this hypothesis. The survey of Megginson and Netter (2001) suggests that the privatization of an SOE usually leads to a significantly lower debt-to-asset ratio. Rizov (2008) provides a more general overview of theoretical and empirical work analyzing the consequences of a soft budget constraint on leverage.[12] Two reports by the (OECD, 2012, 2014a) confirm that the pricing behavior of lenders differs if the borrower is an SOE. A very recent paper by Chen et al. (2016) finds that private firms in China have lower leverage than SOEs, suggesting that SOEs have facilitated access to debt financing.

While many SOEs are not wholly state-owned, previous literature has ignored how implicit guarantees change the behavior of private co-owners. Similarly, we know little about how the presence of a state owner, regardless of the ownership share, alters the risk perception of creditors. This is surprising, as mixed ownership is widespread: The OECD (2014b) estimates minority state holdings alone amount to 860 billion USD across member states. In an international context, it is often the case that domestic SOEs and privately owned MNEs (MNEs) have stakes in the same affiliates.[13]

Analyzing the consequences of state guarantees is naturally difficult: They are implicit and therefore unobserved. Although a study by the OECD (2014a) confirms that SOEs (from European or OECD countries) obtain debt in the commercial marketplace, just like privately held firms, commercial credit markets seem to be increasingly influenced by state-owned banks, and this could facilitate access to credit for SOEs (Sapienza, 2004; Dinc, 2005).[14] Private co-owners of mixed-ownership companies may particularly benefit from the facilitated debt access. From their perspective, implicit

[12]Implicit state guarantees are closely related to soft budget constraints (Kornai et al., 2003), which is why numerous studies on the subject are of equal relevance in our context. Theory and empirical work suggest a rise in leverage, the softer the budget constraint, i.e., the stronger the assumed implicit guarantee.

[13]See Norbäck and Persson (2004), arguing that in about 29% of the privatizations in OECD countries the buyer was a foreign firm; see also Gupta (2005).

[14]State-owned banks in Europe increased their assets by at least 30 percent from 2008 to 2012 compared to only a 4-percent increase for private banks (Monnet et al., 2014).

guarantees might change the standard trade-off between tax benefit (deductible interest expense) and cost of financial distress. For example, a private co-owner might pressure managers to set a maximum attainable debt level to avoid unilateral distributions (via tax payments) to the government. This suggests two things. First, debt levels of mixed-ownership firms are higher. Second, debt responses to corporate tax rates in mixed-ownership firms should be less pronounced.

Tax-deductible interest expenses allow firms to avoid tax payments and suggest a positive correlation between business taxes and debt financing. While there is a vast number of studies on the role of taxes for capital structure choice,[15] previous research has not addressed the issue of whether the debt responses of partial SOEs differ with regard to the standard determinants of capital structure. In particular, partial state ownership may alter a firm's debt tax-responsiveness for three reasons. First, the government as a shareholder benefits from both taxes and dividends.[16] However, under shared ownership, it will prefer tax payments to dividends because tax payments are not distributed to all shareholders. Second, private co-owners might take advantage of easier debt access and benefit from maximum tax shields, irrespective of the corporate tax level. Third, agency conflicts could affect the behavior of managers under partial state ownership in a different way compared to private firms, and this might have implications for the decisions about debt financing.

This paper suggests a differential impact of taxes on debt financing, depending on ownership. We particularly provide evidence that mixed-ownership firms use significantly more debt, but are less tax-sensitive than private firms. For the empirical analysis, we use a large micro-level dataset (ORBIS), which allows us to distinguish between affiliates of MNEs in which an SOE is a co-owner and affiliates of MNEs where this is not the case.[17] By comparing affiliates with government participation to similar but privately owned ones, we can identify the differential impact of taxation on capital structure caused by partial state ownership. We thereby show that the well-established effect of taxes on capital structure choice does not hold if the firm is partially state-owned. Instead, leverage levels are higher, irrespective of the tax rate,

[15]Influential contributions to this literature are the ones by Jensen and Meckling (1976), Myers (1977), and Harris and Raviv (1991). Empirical contributions finding a positive effect of taxes on debt financing are the ones by MacKie-Mason (1990), Rajan and Zingales (1995), Desai et al. (2004), Huizinga et al. (2008), Buettner et al. (2009); for a meta-study, see Feld et al. (2013); for surveys, see Myers (2001), and Graham (2003).

[16]This assumes that the firm operates in the same country as the government owner, which is the case for a vast majority of SOEs and also in the sample we use for our empirical analysis.

[17]Our data consists of affiliates with ownership above 50 percent (subsidiaries) and affiliates with ownership below 50 percent. When we use "SOE" in an affiliate context, we always refer to partially state-owned affiliates, that is firms with a mixed MNE-SOE ownership structure.

suggesting significantly higher tax shields for private shareholders. To the best of our knowledge, this is the first paper that makes this distinction when examining the effect of taxes on debt financing.

An unconditional comparison using our data suggests that the average debt-to-asset ratio of partially SOE-held affiliates is about 5.8 percentage points higher than the one of affiliates without SOE ownership. Differences in debt financing at the extensive and the intensive margin drive this discrepancy: The unconditional probability of having positive debt is about 15.4 percent higher for affiliates where an SOE is involved; debt-to-asset ratios of partially state-owned affiliates with positive debt exceed the ones of private firms by an average of 2.5 percent. Another interesting observation from the data is that leverage is hardly correlated with taxes in case of partially SOE-held affiliates, while debt ratios of private firms exhibit a clear positive correlation. This may be surprising given numerous anecdotes about the tax planning and tax avoidance of SOEs, and given that SOEs operating in OECD and EU countries are subject to the same tax treatment as private firms (OECD, 2012).[18]

One of the central issues we need to address in an empirical analysis is that the status of state ownership is not random, and unconditional comparisons (of outcomes) between partially SOE-held and non-SOE-held affiliates will therefore necessarily produce biased estimates. We design our investigation approach in a way that particularly allows us to understand the differential impact of taxes on debt financing, given the non-random assignment of state ownership. To account for the latter, we first match pairs of similar affiliates of MNEs, where each pair involves one partially SOE-held affiliate (treated) and one non-SOE-held affiliate (control). We require these pairs to be active in the same sector and country. The differential impact of taxes is then identified by using time variation in tax incentives, conditional on time-varying determinants of debt financing as well as time and matched-pair-specific effects. For the basic results, and assuming a tax rate of 25%, our estimates suggest that being (partially) SOE-held is associated with an 8 percentage points higher debt ratio. A central finding is that treated affiliates do not respond to taxes at all. This indicates that the classic leverage trade-off becomes weaker or even disappears under state ownership. We additionally show that the intensive margin drives the difference between mixed-ownership and private units. All results are robust to (i) exact matching within country and sector, (ii) alternative regression specifications, (iii) varying specifications of the propensity score, as well as (iv) placebo effects.

Our empirical results are consistent with theories on capital structure and state

[18]Within the EU, state aid legislation prohibits differential treatment of SOEs if they are commercially active.

ownership. First, partial state ownership facilitates access to credit. Second, private firms in mixed-ownership relationships aim at exploiting a maximum attainable debt ratio. Thereby, the private co-owners can avoid unilateral distributions to state shareholders, which count on budgetary contributions from commercial SOEs (European Commission, 2016). Since partial state ownership facilitates access to debt and alters the standard debt trade-off towards higher levels of debt, our findings may also have implications for MNEs and their tax planning activities. In particular, debt as a vehicle to avoid taxes seems to be an attractive tax planning strategy if monitoring by state owners is weak (Megginson and Netter, 2001; Fan et al., 2007). Third, higher leverage helps dividend-averse managers of partially SOEs to increase their leeway for "empire building" and investment in "pet projects" (Cui, 2015b). Fourth, rate of return targets from governments may give managers of partially state-owned affiliates an incentive to use very high levels of debt, irrespective of the tax rate. Fifth, firms may suffer from a leverage ratchet effect, which biases capital structure decisions towards higher debt levels (Admati et al., 2018). This effect is likely to be particularly strong in SOEs because of restrictions on equity issues imposed by the government.

The rest of this paper is organized as follows. Section 3.2 provides all definitions of the variables we use in our analysis and reviews the relevant literature. Section 3.3 explains the econometric approach. Section 3.4 presents descriptive statistics and the main result. Section 3.5 summarizes a broad range of sensitivity tests. Section 3.6 elaborates on implications of our findings. Section 3.7 concludes.

3.2 Taxation, state ownership, and debt

3.2.1 Taxes and debt financing of SOEs

The differential effect of taxes on SOEs has attracted very little attention in the corporate finance literature. Cui (2015b) summarizes three different (not mutually exclusive) perspectives on the issue. First, taxes are irrelevant in the context of wholly-owned SOEs: It does not matter whether the government receives taxes or dividends. However, differential tax treatment matters in mixed-ownership companies: Governments tax the dividends paid to private investors, whereas distributions to the state are not subject to additional tax. Otherwise, it is reasonable to assume that SOE managers are indifferent between tax and dividend payout, assuming the absence of agency conflicts.

Second, there is a perspective called the "condition of neutrality". It derives from the observation that most SOEs operate in market environments where preferential taxation would give SOEs a competitive advantage over private firms. Hence, SOE

taxation is necessary to ensure fair competition. This point of view is particularly strong in the EU, where preferential tax treatment of SOEs would contradict state aid legislation aimed at providing a level playing field in the single market.[19] However, governments sometimes act in response to market failure, and competitive neutrality may consequently not be a sufficient argument to explain SOE taxation.[20] Within an SOE, it may be unclear which activities stem from public service obligations and which are of commercial interest. Moreover, it is hard to say if public service obligations ultimately benefit or hurt an SOE's market activities. Still, SOEs and their private shareholders may benefit from other competitive advantages such as implicit debt guarantees. At the same time, SOEs have been found to be less profitable (Dewenter and Malatesta, 2001; Megginson and Netter, 2001) and less well managed (Borisova et al., 2012).

Third, SOE taxation is a means of forcing distributions under agency conflicts (Cui, 2015b). Note that taxes, in this view, are a substitute for dividend payments to the government in the spirit of Jensen (1986). A firm should pay out dividends to shareholders if the marginal benefit of a payout is higher than that of an additional unit of investment. Managers may be reluctant to pay dividends due to agency conflicts. For instance, they may prefer to invest in projects that produce private benefits, or they pursue "empire building" and "quiet life" strategies. Thus, shareholders have to engage in effective monitoring to mitigate divergent interests of managers, a task that may be harder to accomplish for SOEs (Megginson and Netter, 2001; Musacchio and Lazzarini, 2012). Less effective monitoring is often attributed to a weaker sense of ownership in government agencies in charge of SOE supervision compared to large shareholders in private firms. SOE managers may, therefore, *care more about corporate taxes* than managers of private firms as weaker monitoring and corporate governance increase opportunities for "empire-building" strategies (Cui, 2015b).

Governments in many EU member states use financial gains from their commercially active companies in budgets (European Commission, 2016). Effective tax rates of wholly-owned commercial SOEs reflect this behavior: Jakob (2018) shows that they pay higher effective tax rates in Europe than comparable private firms. In our mixed-ownership study, tax payments increase distributions to state shareholders and lower

[19]The relevant articles are 107-109 in the Treaty on the Functioning of the EU (TFEU). Even though exceptions persist on when SOEs may receive state aid (at arm's-length), Article 90 of the Treaty on the European Community explicitly subjects public undertakings of "general economic interest" to EU competition law.

[20]We see in our data that governments often hold a participating interest in network industries, such as energy companies, airlines, railways, postal services, utilities and other sectors of public interest. Many former state monopolies have been partly privatized over the last decades. However, the comprehensive review of ownership structures undertaken for this study reveal continued government participation in these sectors across all countries included in our sample.

distributions to private owners. Hence, governments have an incentive to *force* high tax payments to increase their distributions over those of private shareholders. In contrast, private shareholders might pressure the management to avoid unilateral distributions towards government owners.

Summing up, mixed-ownership firms face different tax-related capital structure trade-offs compared to private companies. In particular, incentives differ because of (i) implicit state guarantees, (ii) weak monitoring by governments, (iii) bigger agency conflicts, and (iv) forced tax distributions to governments. Note that these points substantially depend on ownership and not on the level of tax rates. As a consequence, the standard cost-benefit capital structure trade-offs no longer apply. The reason is that partial state ownership affects the cost of financial distress as well as the tax benefits of using debt financing. Instead, we propose for mixed-ownership companies:

> **Hypothesis:** *Mixed-ownership SOEs exploit the facilitated access to debt financing and employ a maximum attainable debt ratio to shield private investors from corporate taxes. The optimal capital structure then primarily depends on the presence of implicit guarantees and less on corporate tax rates.*

Our empirical analysis provides a test of this hypothesis by focusing on the differential impact of ownership on leverage, and by examining the tax-responsiveness of debt if firms are in partial state ownership.

Note, finally, that debt financing may help to meet rate-of-return targets set by governments. Such goals are used by three-quarters of all OECD countries to assess the performance of SOEs. With very few exceptions, governments do not link them to capital structure decisions (OECD, 2014a).

3.2.2 Data and the definition of state ownership

Our analysis focuses on MNEs and their affiliates. We use the commercial ORBIS firm-level panel dataset from Bureau van Dijk. ORBIS records balance sheet and income statement data from millions of companies across the globe. An essential advantage of ORBIS is the inclusion of non-listed firms and affiliates, which allows us to analyze debt financing at the non-consolidated micro level (the affiliate level). We should mention, though, that the dataset suffers from a large number of missing values in some variables that are important for this study. This reduces the overall sample size to a significant extent.

We first use information provided by Bureau van Dijk to identify affiliates of SOEs. In ORBIS, a company is classified as state-owned if the state owns a substantial share of at least 25 percent of a company's controlling equity. We define an indicator variable SOE_i, which equals 1 if affiliate i is co-owned by an SOE. If this is not the case and no SOE is involved in the controlling capital of i, we set $SOE_i = 0$. As mentioned above, we focus on MNEs, i.e., firms that have at least one affiliate abroad in which the parent company holds at least 1 percent. In a next step, we manually verify the ownership structure of each affiliate with $SOE_i = 1$, using the historical ownership information available in the online ORBIS database. Thus, we ensure that during the years from 2004 to 2013, an affiliate was always partially owned by an SOE.[21] At the same time, we ensure that the affiliate had a mixed ownership structure by identifying at least one private shareholder in each year. We also make sure that the MNEs and their affiliates with $SOE_i = 0$ are purely private firms by manually checking their historical ownership structure in each year. Affiliates that we find to be partially state-owned (for example, if the owning government holds a share smaller than 25 percent of the parent company) are removed from the dataset in the respective years. We thereby make sure that we compare affiliates of MNEs with state participation to affiliates of MNEs where the state is not involved at all.

The ownership structure of some affiliates cannot be fully determined, as the data include some holding companies for which no ultimate ownership data is available. We assume that such firms are privately owned, which is most likely the case as OECD governments make all their holdings public. Besides, we are ultimately interested in implicit guarantees, for which state ownership has to be publicly known.

Note that our data include small and passive investments by SWFs. In ORBIS, we can easily differentiate between SWF portfolio companies and companies directly owned by the state because SWF companies carry the ownership label "country *via its funds*" compared to "country". SWF portfolio companies tend to be non-substantial and hold less than 10 percent of affiliate equity, which is below our sample threshold for substantial ownership. Cui (2015b) argues that SWF portfolio companies differ from traditional SOEs because the SWF stake is mostly not substantial and will alter firm behavior and incentives to a much lesser extent.[22] In any case, we do not consider SWF portfolios as SOEs, and this is consistent with the political view of state owner-

[21] Around 77 percent of treated firms fulfill our ownership structure requirements for all years in the period considered. We also construct matched pairs for different ownership patterns in the respective years to avoid a survivorship bias in our estimates.

[22] Nevertheless, a body of research has shown that SWF participation does affect the characteristics of target firms. See Fotak and Megginson (2015) for a survey.

ship (Shleifer and Vishny, 1994; Shleifer, 1998). The latter suggests that SOEs may be vehicles of politicians' private interests such as forcing distributions or pursuing employment and investment policies. The state primarily acts as an investor rather than as an owner when acquiring non-controlling positions in firms via its SWF. The aim is then to realize a long-term financial return (Fotak and Megginson, 2015).

The definition of an entity being state-owned or private is crucial. Few studies find a negative correlation between state ownership and debt. For example, the study by Bortolotti et al. (2015) suggests a decrease in leverage of publicly traded firms after investments of state-owned investors. However, they focus on investments in minority stakes, whereas we use affiliates of substantially state-owned SOEs. Borisova and Megginson (2011) find that the cost of debt increases with a decreasing share of government ownership, which implies lower leverage. Entirely privatized firms, in turn, face lower spreads (used by Borisova and Megginson, 2011, as a proxy for the cost of debt) and hence lower borrowing costs. A study by Borisova et al. (2015) suggests that implicit guarantees have only been relevant during the financial crisis (starting in 2008) and that SOEs face higher bond spreads in normal times. These studies appear to contradict previous findings. The focus on bond spreads may, however, not be the most relevant one given that the medium-sized utility affiliate (of the partially state-owned units) is the typical observation in our sample.

It is important to recall that, at this point, the partially state-owned affiliate i, which we will analyze in the empirical part below, is at the same time an independent entity of a private MNE. The affiliate is located somewhere within the EU/OECD member states, but not necessarily in the same country as the SOE.[23] The goal of the empirical analysis is to match (comparable) affiliates that are fully privately owned with mixed-ownership affiliates.

3.2.3 Debt financing

The variable we are interested in is long-term debt, which we define as long-term credit (i.e., maturities of more than one year) divided by total assets. We denote this variable for affiliate i as LEV_{it}. We focus on long-term debt as it should be the choice variable when (i) firms decide on optimal tax shields, and when (ii) implicit state guarantees are more important for long-term financing (which is very likely). Similarly, facilitated debt access at state banks should, in particular, affect long-term funding.[24] We report results for two alternative outcome variables in the sensitivity analysis in Section 3.5.

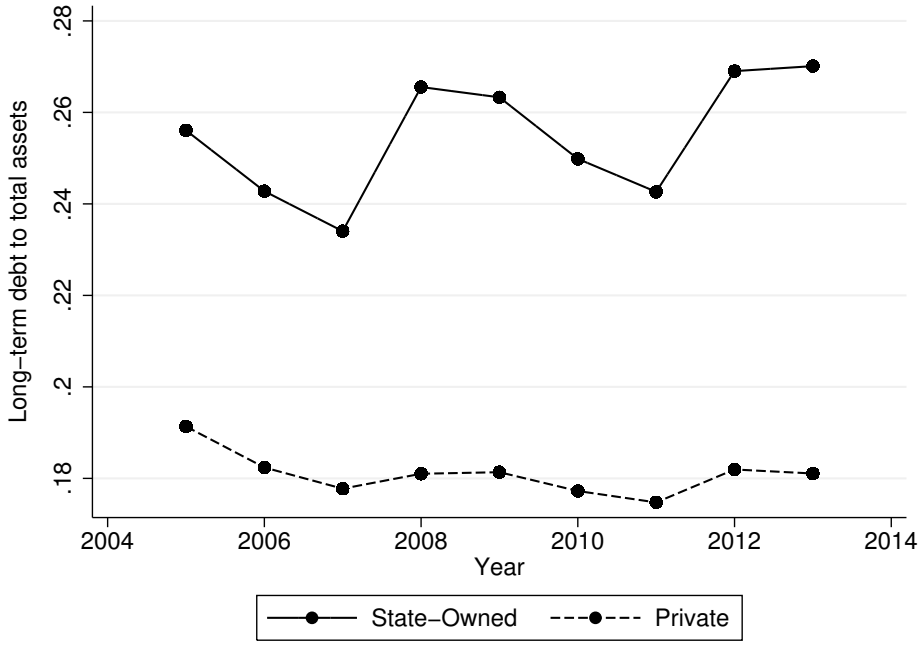
[23] We also estimate our model where SOEs and affiliates operate in the same country.

[24] For instance, European state-owned banks focus on long-term lending (Monnet et al., 2014).

Figure 3.2.1 depicts the outcome variable, LEV_{it}, over time. The solid line shows the average debt ratio of mixed-ownership affiliates ($SOE_i = 1$), the dashed line the average debt ratio for the fully privately-held affiliates ($SOE_i = 0$). While the level of this ratio stays roughly constant over time for the control group, it plummets and subsequently rises again quite significantly on two occasions in the treatment group. The timing seems to suggest a relation to events at the macroeconomic level: The first peak in 2008 coincides with the global financial crisis, the second peak in 2012 may be related to the sovereign debt crisis in the Eurozone. The cyclical responses of debt financing of the SOEs support the results of Borisova et al. (2015). These show that implicit guarantees of SOEs are particularly relevant in times of macroeconomic crisis.

Figure 3.2.1: Yearly average of the outcome variable

Figure 3.2.1 plots yearly averages of our outcome variable LEV_{it} for state-owned and privately owned firms with $LEV_{it} > 0$ from 2005 to 2013. LEV_{it} is defined as long-term credit divided by total assets for firm i in year t.



3.3 Empirical approach

The central goal of our empirical analysis is to learn about a possible differential impact of the corporate tax rate that applies at location k and time t, TAX_{kt}, on debt financing of affiliate i, LEV_{it}. To estimate the differential impact we use interactions of TAX_{kt} and the indicator SOE_i.

First, however, our empirical approach requires identifying pairs of affiliates, where each pair consists of one affiliate that is held by an SOE ($SOE_i = 1$) and one that is not ($SOE_i = 0$). The former unit is called *treated*, and the latter unit is called *control*. Whether affiliate i is assigned to one or the other group depends on vectors of affiliate-i-specific and host-country-k-specific determinants, which we summarize in $\boldsymbol{X}_{0i}$ and $\boldsymbol{Z}_{0k}$, as well as the industry s in which an affiliate operates, ψ_s. We then specify the following linear index:

$$SOE_{it_0} \quad = \quad \boldsymbol{\beta}_1 \boldsymbol{X}_{0it_0} + \boldsymbol{\beta}_2 \boldsymbol{Z}_{0kt_0} + \psi_i + \epsilon_{ikt_0}. \tag{3.1}$$

We use specification (3.1) to predict the propensity $\hat{p}_i$ of i being SOE-held, using a binary probability model (probit). Based on this probability model we produce two vectors of propensity scores: One for the SOE-held affiliates $\hat{\boldsymbol{p}}^{\mathbf{1}}$, and one for the ones that are not SOE-held, $\hat{\boldsymbol{p}}^{\mathbf{0}}$. The time index t_0 indicates that we focus on observations in the initial year ($t_0 = 2005$) in our data.[25]

The next step requires identifying the *nearest neighbor* for each treated unit i. Let c_i^m denote the respective control unit m that we match to the treated unit i. The best match is determined by $c_i^m = \min_{\{m\}}(|\hat{p}_i^1 - \hat{p}_m^0|),\ \forall\ m \neq i$. We start by matching across countries and sectors but also provide results based on exact country and industry matching. This produces pairs of affiliates $\{SOE_i = 1; SOE_m = 0\}$ where one is SOE-held and one is not. Note that we only use observations as controls which are 100 percent owned by an MNE to ensure they are fully privately owned. Once we have identified c_i^m for each treated unit, we estimate

$$\begin{aligned} LEV_{it} = \alpha_1 SOE_i \quad &+ \quad \alpha_2 TAX_{kt} + \alpha_3 SOE_i \times TAX_{kt} \\ &+ \quad \boldsymbol{\alpha}_4 \boldsymbol{X}_{it} + \boldsymbol{\alpha}_5 \boldsymbol{Z}_{kt} + \phi_t + c_i^m + \psi_s + \omega_k + \varepsilon_{iskt}, \end{aligned} \tag{3.2}$$

where LEV_{it} denotes the debt-to-asset ratio of affiliate i in year t ($t = 2005, ..., 2013$), SOE_i indicates treatment status, and TAX_{kt} is the statutory tax rate of country k in year t, the host location of affiliate i. We are mainly interested in the coefficient α_1 of the treatment variable SOE_i and the interaction term $SOE_i \times TAX_{kt}$. In particular, the coefficient α_3 provides an estimate for the differential impact of TAX_{kt} under partial state ownership. It

[25] We lose $t = 2004$ due to first differencing of some variables. If the ownership verification (described above) reveals relevant changes in ownership during the sample period, we remove the observation from the sample for the respective years.

is identified through variation in tax rates over time as we condition on ϕ_t and c_i^m, which denote time, and pair-specific effects. Similarly, ψ_s and ω_k denote sector and country effects, respectively. Note that the index m in c_i^m indicates that the pair fixed effect is based on the best match as determined above. Thus, conditioning on c_i^m means that we remove all cross-sectional differences between affiliate pairs and it allows us to identify the differential impact of being SOE-held by time-averaging over all treatment-control units within each pair.

3.4 Descriptive statistics

For our empirical analysis, we use a mostly balanced panel, in which both treated and control units are observed in almost every year from 2005 to 2013. Our dataset includes affiliates operating in 22 countries, all of which are either OECD or EU member countries. This leads to a total of 1,780 treated and 73,033 control observations over the whole observed time interval. The average SOE parent holds 27 percent of a joint affiliate, and the average non-SOE parent owns 25 percent.[26] The treated units operate only in 12 of the 22 countries, all of which are EU member countries with the exceptions of Norway and South Korea. The countries with the most treated observations are Germany, France, and Belgium. Some countries, like Italy, are not represented in our sample because of missing information in some of the control variables.[27]

Descriptive statistics at the sectoral level suggest that governments are involved in many industries. However, there is more government activity in sectors like transport, electricity, and communications, than in others, like wholesale trade. Table 3.4.1 depicts the relative shares using a sectoral classification based on the first digit of the US-SIC-code. To ensure that sectoral distributions do not drive our results, we include SIC-1 dummies in the estimates of the propensity scores.[28]

[26]The mean ownership share in the paper by Beuselinck et al. (2017) is about 22% (cf. Table 1).

[27]This is not representative, as SOEs play a prominent role in many EU and OECD member states, especially in Italy (European Commission, 2016). Note that the sample composition is not relevant, however, in the sense that cross-country variation is fully taken into account in results where we match treated and control units located in the same country. Moreover, our estimates are robust to a full set of country dummies in the propensity score estimation. To save degrees of freedom in the basic estimate, we follow Borisova et al. (2012) and include only dummies indicating the La Porta et al. (1998) legal origin.

[28]Our results are also robust to exact matching by sector (see below).

Table 3.4.1: Sectoral distribution of state ownership

The table presents the sectoral distribution of partially state-owned and fully privately owned firms; the sectoral distribution is based on 1-digit SIC identifier codes.

SIC-Sector	**MNE**		**SOE**		**Total**	
	Freq.	**Percent**	**Freq.**	**Percent**	**Freq.**	**Percent**
Agriculture, Forestry and Fishing	500	0.68	0	0	500	0.67
Mining	420	0.58	0	0	420	0.56
Construction	3,532	4.84	70	3.93	3,602	4.81
Manufacturing	19,300	26.43	250	14.04	19,550	26.13
Transport, Communications, Electric, Gas	5,600	7.67	700	39.33	6,300	8.42
Wholesale Trade	19,328	26.46	80	4.49	19,408	25.94
Retail Trade	3,070	4.20	20	1.12	3,090	4.13
Finance, Insurance, Real Estate	7,116	9.74	250	14.04	7,366	9.85
Services	13,018	17.82	390	21.91	13,408	17.92
Public Administration	1,149	1.57	20	1.12	1,169	1.56
Total	*73,033*	*100.00*	*1,780*	*100.00*	*74,813*	*100.00*

Table 3.4.2 presents summary statistics for all variables of interest.[29] We are mainly interested in LEV_{it}, the *long-term debt-to-asset ratio* of affiliate i at time t. We remove affiliates where long-term debt exceeds or equals the total assets as corporations in the OECD and EU require some equity.[30] As we would expect, the unconditional correlation between LEV_{it} and SOE_i is positive. Note that the mean of LEV_{it} is smaller than in most studies, which may be for the following reasons: First, we focus on long-term borrowing and exclude any debt with a maturity of less than one year.[31] Second, our dataset includes a large share of zero-leverage firms. To be precise, 42 percent of the affiliates report zero long-term debt, which is higher than the 32 percent reported by Strebulaev and Yang (2013) in their study using US data from 1962 and 2009.[32] We account for the fact that many of our observations have zero debt by estimating an extensive and an intensive margin debt choice in Section 3.5. We additionally use two alternative specifications of outcome as a robustness check in the same section.

The control variables we include in the outcome equation are ones that have been shown to affect the capital structure of firms in previous literature. In particular, we condition on the following variables: TAX_{kt} is the statutory tax rate that applies to entity i at location

[29]Table 3.8.3 in the chapter appendix reports a correlation matrix for the variables used in the regressions. Table 3.8.5 provides data sources and a description of all variables.

[30]When including observations in our empirical analysis with $LEV_{it} = 1$, the results are unchanged, though.

[31]We present estimates including short-term debt in Section 3.5.

[32]The inclusion of short-term debt leads to a zero-leverage share of 35 percent, which is close to Strebulaev and Yang (2013).

k and year t. In most countries, firms can deduct interest payments from their tax base and therefore have a tax-incentive to use debt financing. The data consistently confirm a positive correlation between TAX_{kt} and LEV_{it} (0.0508; see Table 3.8.3 in the chapter appendix). At the same time, governments may want to force distributions via taxation from their SOEs, which is an alternative way of raising revenue under the assumption of a dividend-averse management.

We use the following firm-level variables: First, Graham and Leary (2011) suggest profitability as an important determinant of leverage. We follow Huizinga et al. (2008) and use the return on total assets (ROA_{it}) as a measure of profitability. The reasoning for its inclusion is straightforward: The more profitable a firm, the more likely it will get credit. From the government's perspective, it is arguably more interesting to participate in profitable ventures, or more lucrative to sell stakes in more profitable SOEs (Dinc and Gupta, 2011). Second, the variable $ATANG_{it}$ is the ratio of fixed to total assets. Asset tangibility ($ATANG_{it}$) has been found to be an important determinant of capital structure because tangible assets can be used as collateral to obtain credit (Rajan and Zingales, 1995; Graham and Leary, 2011). Key industries with a large propensity to be state-owned are industries with a high share of fixed assets, such as utilities, airlines, and energy companies. The variable is strongly correlated with both LEV_{it} and SOE_i (see Table 3.8.3).

Table 3.4.2: Descriptive statistics

The table presents summary statistics of the dependent variable LEV_{it}, state ownership SOE_i and the control variables used to estimate equation (3.2); Table 3.4.2 is based on 74,813 observations.

Variable	**Mean**	**Std.Dev.**	**Min.**	**Max.**
LEV_{it}	0.07	0.16	0	0.99
TAX_{kt}	0.30	0.06	0.1	0.40
ROA_{it}	6.81	17.16	-100	99.94
$ATANG_{it}$	0.30	0.29	0	1
$log(TA)_{it}$	9.27	2.03	0	18.03
$log(SALES)_{it}$	9.44	1.90	0	17.59
$CORRF_{kt}$	74.50	14.80	33	97
$INVESTF_{kt}$	72.40	14.21	50	95
$CREDITM_{kt}$	131.35	42.90	30.38	248.94
$GDPG_{kt}$	1.61	2.72	-14.81	11.90
$GDPPC_{kt}$	39,572	10,937	11,623	96,711

Graham and Leary (2011) further suggest to include a measure of firm size. Larger firms have better access to credit. The government is also more likely to intervene if a firm is large. This is because political reasons for ownership become more important with increasing firm size. We use total assets ($log(TA)_{it}$) as a proxy for firm size. We also produce results where

we use the log of total sales as size proxy ($log(SALES)_{it}$), but as we have many missing values in the sales variable, we prefer $log(TA)_{it}$. Using one or the other does not change our findings.

At the country level, beside TAX_{kt}, we include two indicators from the Heritage Foundation, freedom from corruption ($CORRF_{kt}$) and investment freedom ($INVESTF_{kt}$), as well as a proxy for credit market depth ($CREDITM_{kt}$), GDP growth ($GDPG_{kt}$) and GDP per capita ($GDPPC_{kt}$). We take the latter three variables from the World Bank's World Development Indicators database.

3.4.1 Basic result

Based on a probit model and equation (3.1), we first estimate propensity scores for being state-owned. In particular, we use firm size, firm leverage, sales growth, an investment proxy, return on assets, a proxy for credit market size, GDP per capita, GDP growth and an indicator of legal origin based on La Porta et al. (1998) as control variables. Our specification of the propensity score is very similar to the specification used by Borisova et al. (2012), but we additionally include polynomials of the explanatory variables and some additional regressors such as a proxy for investment growth, $\Delta log(FA)_{it}$, where FA denotes the fixed assets of affiliate i. However, some of the variables used by Borisova et al. (2012) are not available in ORBIS. We finally include one-digit SIC-sector dummies. The results of the propensity score estimation are presented in Table 3.8.4 in the chapter appendix.

We then try to find a comparable entirely privately owned affiliate (with $SOE_i = 0$) for each partially state-owned treated affiliate (with $SOE_i = 1$) in the base year 2005. We do this by using the procedure described above (nearest-neighbor-matching based on propensity scores obtained from the probit estimates).[33]

Based on the pairs we then run outcome regressions as indicated in equation (3.2). It is important to note that the estimates include pair fixed effects, which absorb unobserved heterogeneity between the matched pairs but allow us to identify the impact of being partially state-owned. All regressions additionally condition on country-specific effects (by including

[33]We match the pairs using the alternative calipers 0.5, 0.1, 0.01, 0.001, as well as 10 and 1 percent of the propensity score standard deviation. Thus, we apply even stricter caliper criteria than suggested by Austin (2011). The results (of the outcome regressions) are very robust and do not change with calipers. We take this as evidence of a good first step model and proceed with a caliper of 0.5 to maximize our sample size.

country dummies), industry-specific effects (by including one-digit SIC-sector dummies), as well as time-varying affiliate and country controls. Table 3.4.3 provides the basic results. These results are based on 1,481 observations and 92 treated units matched with 92 nearest neighbor control units observed over time.

Table 3.4.3: Basic result

The table presents pair-fixed effects regressions based on $N = 1,481$ observations; the base year for pair matching is 2005; the dependent variable is the long-term debt-to-asset ratio; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	Coeff. *(s.e.)*
SOE_i	0.3351**
	(0.1348)
TAX_{kt}	0.9706**
	(0.3718)
$TAX_{kt} \times SOE_i$	-1.0168**
	(0.4185)
ROA_{it}	0.0003
	(0.0004)
$ATANG_{it}$	0.2437***
	(0.0644)
$CORRF_{kt}$	-0.0027**
	(0.0013)
$INVESTF_{kt}$	0.0001
	(0.0010)
$CREDITM_{kt}$	0.0003
	(0.0003)
$GDPG_{kt}$	-0.0002
	(0.0027)
$log(TA)_{it}$	0.0065
	(0.0088)
Country effects	Yes
Sector effects	Yes

Given a tax rate of 20%, our model predicts a 13 percentage points higher debt ratio for mixed-ownership firms. Note that the indicator SOE_i controls for all remaining unobserved effects within matched pairs. Thus, the effect of TAX_{kt} is identified from changes in the tax over time. For $SOE_i = 0$, a 1-percentage-point increase in TAX_{kt} is associated with an almost 1 percentage point higher debt-to-asset ratio. This implies that the 13 percentage points mixed-ownership effect from above has a tax equivalent of about 13 percentage points (tax differential which has about the same impact). The tax effect is larger than the typical tax

responsiveness found in the previous literature (see Feld et al., 2013, for a meta-study). The additional interaction term $TAX_{kt} \times SOE_i$ suggests that one of the reasons for the finding of a relatively moderate tax elasticity in previous literature may be related to the heterogeneity in tax responses depending on ownership. The negative interaction term implies that the effect of taxes when firms are partially state-owned is virtually zero. This finding appears to contradict empirical studies using Chinese data referred to in Cui (2015a). These studies find a positive relationship between state ownership and tax sensitivity. Instead, our results support the argument that mixed-ownership firms use more debt irrespective of the tax rate and thus the classical cost-benefit trade-off in capital structure choice becomes less relevant. A large part of the following sections will examine how robust our finding of a reduced tax sensitivity of partial SOEs is. What we can conclude from Table 3.4.3 is that there is a differential impact of taxes on debt financing, depending on ownership. One explanation is that private shareholders (in mixed relationships) make use of a maximum attainable debt ratio, which they do not exceed, and no longer respond to marginal changes in taxes.

The effects of other controls are generally in line with what previous studies have found. An increase in the tangible asset share leads to a rise in leverage because fixed assets make better collateral, which firms can pledge against bank loans. The only country-level control that is significant is corruption freedom. The negative relation supports the argument made by Kesternich and Schnitzer (2010), who show that higher levels of corruption discourage the use of equity financing. The other country controls and the firm size proxy are not significant. This may well be due to the country and pair fixed effects included. In addition to the sensitivity analysis presented in the next section, our basic estimates are very robust, and we confirm the estimated effects at very similar significance levels when excluding financial firms (SIC-identifiers starting with "7"), and when using the (log of) sales as an alternative firm size proxy.

3.5 Sensitivity analyses

3.5.1 Exact matching

We first assess the robustness of our main result by providing estimates from exact (country and sector) matching. Table 3.5.1 presents the findings for both models. Exact matching by country yields a similar magnitude and significance level for the coefficient on SOE_i (Column

A) compared to the baseline result. This is not very surprising given that the basic estimates condition on country effects in the outcome equation. Perhaps surprisingly, $ATANG_{it}$ is no longer significant.

Table 3.5.1: Exact matching

The table presents pair-fixed effects regressions based on $N_A = 1,195$, and $N_B = 1,024$ observations, respectively; the base year for pair matching is 2005; sector matches are within three-digit SICs; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: Country *Coeff.* *(s.e.)*	B: Sector *Coeff.* *(s.e.)*
SOE_i	0.3068**	0.2783**
	(0.1363)	(0.1351)
TAX_{kt}	1.1504**	1.2910***
	(0.4601)	(0.3971)
$TAX_{kt} \times SOE_i$	-0.8184*	-0.8222*
	(0.4168)	(0.4503)
ROA_{it}	-0.0010*	-0.0008
	(0.0005)	(0.0007)
$ATANG_{it}$	-0.0142	0.2627***
	(0.0600)	(0.0812)
$CORRF_{kt}$	-0.0022	-0.0020*
	(0.0015)	(0.0012)
$INVESTF_{kt}$	0.0002	-0.0010
	(0.0011)	(0.0010)
$CREDITM_{kt}$	0.0003	0.0003
	(0.0004)	(0.0005)
$GDPG_{kt}$	-0.0008	-0.0020
	(0.0033)	(0.0025)
$log(TA)_{it}$	0.0106	0.0008
	(0.0152)	(0.0087)
Country effects	No	Yes
Sector effects	Yes	No

The right-hand side (Column B) of the table provides the results when utilizing exact matching at the level of three-digit SIC-sectors. The effect of SOE_i remains robust at the five percent level but is now weaker in magnitude (0.2783 compared to 0.3351). The tax sensitivity is confirmed to be substantially smaller for the SOEs, but its total effect remains positive. $ATANG_{it}$ and $CORRF_{kt}$ are estimated with the same sign as in Table 3.4.3, though the coefficients of both variables are smaller now. A potential problem we are facing is that the number of matches becomes relatively small. The results in Column A are based on 78 pairs

compared to the 92 from our main result. When matching within the three-digit SIC-sectors, we only find 67 pairs due to the additional restriction of matching exactly on an affiliate's industry (note, though, that both regressions still use more than 1,000 observations). However, we conclude from Table 3.5.1 that our benchmark results are not biased by possible spurious correlations arising from comparisons across countries or sectors.

3.5.2 Extensive vs. intensive margin

From the descriptive statistics, it is unclear whether the extensive margin (i.e., the determinants of zero vs. positive leverage) drives differential responses or the intensive margin (that is, marginal changes in leverage conditional on non-zero debt). To look at this, we estimate equation (3.2) for both margins separately. Table 3.5.2 presents the results.

Apart from $ATANG_{it}$, which seems to be an important determinant of having positive debt, none of the other variables has a significant impact on the extensive margin. Of course, this does not mean that there are no cross-sectional differences in the use of debt at the extensive margin. However, it appears that all of these differences are captured by the fixed effects approach. The estimates at the intensive margin (right-hand side in Table 3.5.2) confirm all the findings from Table 3.4.3. Only the magnitudes of the effects, as well as the statistical significance, increase compared to the basic findings. This suggests that most of the differential variation in the data happens at the intensive margin.

Given that the OECD finds that SOEs in its member countries access debt almost exclusively in the commercial marketplace (OECD, 2014a), we are not surprised to see descriptive differences in the extensive margin disappear in the panel regressions. The large effects found for the intensive margin point to lower borrowing costs (for the partially state-owned firms), which is in line with an altered cost-benefit trade-off under mixed ownership.

3.5.3 Alternative outcome measures

The very low long-term leverage ratio in our data is a possible source of concern. To address this issue, we estimate our model using two alternative specifications of the debt ratio. First, we include all debt with a maturity of less than one year. The mean leverage is now 17 percent for state-owned and 13 percent for privately owned firms. A total of 35 percent has no short- or long-term debt at all. This zero share is still higher than in most studies, but fairly close to Strebulaev and Yang (2013). Column A of Table 3.5.3 provides the results. While the impact

Table 3.5.2: Extensive and intensive margin of debt financing

The table presents pair-fixed effects regressions based on $N_A = 1,481$ and $N_B = 847$ observations, respectively; the base year for pair matching is 2005; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: Extensive margin *Coeff.* *(s.e.)*	B: Intensive margin *Coeff.* *(s.e.)*
SOE_i	-0.2945	0.4025***
	(0.3335)	(0.1212)
TAX_{kt}	-0.5330	1.4530***
	(0.8406)	(0.4941)
$TAX_{kt} \times SOE_i$	1.3291	-1.3277***
	(1.0350)	(0.3916)
ROA_{it}	-0.0010	0.0003
	(0.0011)	(0.0013)
$ATANG_{it}$	0.3475***	0.2033***
	(0.1197)	(0.0723)
$CORRF_{kt}$	-0.0031	-0.0062***
	(0.0047)	(0.0021)
$INVESTF_{kt}$	0.0009	-0.0000
	(0.0029)	(0.0013)
$CREDITM_{kt}$	-0.0005	0.0003
	(0.0011)	(0.0004)
$GDPG_{kt}$	-0.0059	0.0027
	(0.0080)	(0.0039)
$log(TA)_{it}$	0.0217	0.0019
	(0.0080)	(0.0131)
Country effects	Yes	Yes
Sector effects	Yes	Yes

of TAX_{kt} remains almost unchanged, the effect of SOE_i on the outcome is weaker and less robust, but still significant at the 10% level. The same holds for the interaction term $TAX_{kt} \times SOE_i$. We conclude that partial state ownership primarily facilitates access to long-term debt rather than to short-term debt, a finding that could point to better relationships with long-term lenders, such as state-owned development banks. Second, we define an alternative dependent variable as the total non-current liabilities divided by total assets. The mean of this variable is 0.27 for $SOE_i = 1$, and 0.2 for $SOE_i = 0$.[34] The share of zeros decreases to 9 and 17 percent, respectively. This is because the definition of debt is now broader. For example, the

[34]For data from five sectors in six countries, Vause (2009) reports an average value of this variable of about 0.28.

non-current liabilities include all deferred tax liabilities of more than twelve months. While we believe that the debt definition from above is more appropriate, this alternative measure has certainly the advantage that it includes any form of long-term financial obligations an affiliate has. It is also plausible to argue that total non-current liabilities may be less likely to suffer from reporting errors or missing values since they are a major balance sheet item. Moreover, the alternative leverage ratio is a useful indicator in its own right because the impact of taxation may depend on the level and timing of deferred taxes. For example, a government may allow an SOE to defer tax payments into the long-term future out of political considerations (despite EU competitive neutrality regulation). In doing so, it softens a firm's budget constraint (Schaffer, 1998) – which of course is in the interest of private co-owners. If partial state ownership helps to defer tax payments more easily, the tax sensitivity should further decrease.

Table 3.5.3 reports the estimates with total non-current liabilities divided by total assets as the dependent variable (Column B). The effect of SOE_i is stronger and statistically more significant. While the impact of TAX_{kt} is very similar compared to the magnitude in our baseline model, the negative coefficient of the interaction term becomes larger. This is consistent with the argument made above that deferring taxes becomes easier under state ownership. The coefficient of $ATANG_{it}$ becomes smaller. This is plausible as well, as collateral is essential when raising debt, but it does not influence other items now included in the outcome variable. In contrast to our findings from above, the effects of ROA_{it}, $GDPG_{kt}$, and $log(TA)_{it}$ are now estimated to be statistically significant.

3.5.4 Placebo treatments

In this Section, we present further robustness results showing that "placebo treatments" do not affect our outcome variable. In particular, the aim is to assess whether the predictions reported in earlier sections can clearly be attributed to treatment status. To obtain placebo treatment effects, we first randomly select 178 observations as "treated" in our base year 2005. The 178 observations correspond to the actual number of treated affiliates in the dataset. We then run our pair-fixed effects regression from above and repeat the random assignment procedure 1,000 times. Table 3.5.4 presents the averaged regression statistics, and Figure 3.8.1 in the chapter appendix depicts the empirical cumulative distribution functions (CDF) of the three variables of interest. The vertical lines correspond to the benchmark results from

Table 3.5.3: Alternative outcome measures

The table presents pair-fixed effects regressions based on $N_A = 1,481$ and $N_B = 1,264$ observations, respectively; the base year for pair matching is 2005; outcome in Column A is total short- and long-term debt divided by total assets; outcome in Column B is total long-term liabilities divided by total assets. *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: Including short-term debt *Coeff.* *(s.e.)*	B: Total long-term liabilities *Coeff.* *(s.e.)*
SOE_i	0.2495*	0.4033***
	(0.1393)	(0.1440)
TAX_{kt}	0.9306**	0.9625**
	(0.3715)	(0.4239)
$TAX_{kt} \times SOE_i$	-0.8033*	-1.1678**
	(0.4337)	(0.4643)
ROA_{it}	-0.0030***	-0.0015**
	(0.0006)	(0.0006)
$ATANG_{it}$	0.2010***	0.1234**
	(0.0502)	(0.0527)
$CORRF_{kt}$	-0.0010	-0.0014
	(0.0018)	(0.0017)
$INVESTF_{kt}$	-0.0015*	0.0001
	(0.0008)	(0.0011)
$CREDITM_{kt}$	-0.0001	0.0003
	(0.0004)	(0.0004)
$GDPG_{kt}$	-0.0008	-0.0056*
	(0.0029)	(0.0030)
$log(TA)_{it}$	0.0117	0.0278**
	(0.0093)	(0.0120)
Country effects	Yes	Yes
Sector effects	Yes	Yes

Table 3.4.3.

As can be seen in Table 3.5.4, the average from the 1,000 random treatments is very close to zero, suggesting no treatment effect at all. The distribution of the pseudo-SOE_i coefficients in Figure 3.8.1 reveals that all estimates are below the estimate of 0.3351 from Table 3.4.3. The 5 percent confidence intervals given in the right panel of Figure 3.8.1 show that only a tiny number of random assignments produce a significantly positive coefficient. The average of 1,000 placebo coefficients for TAX_{kt} in Table 3.5.4 has, as expected, a positive sign, but is not statistically significant. The empirical CDF of TAX_{kt} coefficients in Figure 3.8.1 is skewed towards positive values and indicates that firms respond to increases in TAX_{kt} with

Table 3.5.4: Placebo effects

The table presents the average of 1,000 pair-fixed effects regressions based on $n = 178$ randomly assigned treatment observations; the base year for pair matching is 2005; the dependent variable is the long-term debt-to-asset ratio; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	Coeff. *(s.e.)*
SOE_i	-0.0021
	(0.0741)
TAX_{kt}	0.3195
	(0.2530)
$TAX_{kt} \times SOE_i$	0.0071
	(0.2457)

higher debt levels. To a lesser extent, this also holds for 5 percent confidence intervals in the right panel. The averaged coefficient of the interaction term $TAX_{kt} \times SOE_i$ in Table 3.5.4 is not significant either. A closer look at the empirical CDF in Figure 3.8.1 reveals a fairly even distribution around 0 for the coefficient, and a slightly positively skewed CDF for the 5 percent confidence intervals. Summing up, regressions with pseudo-SOEs do not produce significant outcomes for the ownership indicator SOE_i or the interaction term $TAX_{kt} \times SOE_i$, but do imply a significant impact of TAX_{kt} on LEV_{it} as the conventional debt-tax trade-off suggests. We conclude that the effects of SOE_i and $TAX_{kt} \times SOE_i$ from our main result indeed derive from state ownership.

3.5.5 Location of the mixed-ownership affiliate

An important issue in our context might be the location of affiliates. More specifically, the differential impact of taxes on capital structure choice should be stronger if an affiliate operates in the same country as the SOE. A tax planning MNE will try to minimize overall tax payments independently of location but facilitated access to debt could depend on the affiliate operating in the same country as the owning state. Besides, a government owner cares more about tax payments within its jurisdiction than abroad. This may be even more the case if taxes act as a "forcing-distributions" tool as argued by Cui (2015b).

Table 3.5.5 presents our estimates of equation (3.2) when only affiliates are considered

Table 3.5.5: Local SOEs

The table presents pair-fixed effects regressions based on $N = 1,416$ observations; the base year for pair matching is 2005; the dependent variable is the long-term debt-to-asset ratio; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	Coeff. *(s.e.)*
SOE_i	0.4616***
	(0.1393)
TAX_{kt}	1.3169**
	(0.5038)
$TAX_{kt} \times SOE_i$	-1.4357***
	(0.4452)
ROA_{it}	-0.0004
	(0.0005)
$ATANG_{it}$	0.1827***
	(0.0479)
$CORRF_{kt}$	-0.0002
	(0.0017)
$INVESTF_{kt}$	-0.0009
	(0.0011)
$CREDITM_{kt}$	0.0001
	(0.0005)
$GDPG_{kt}$	-0.0012
	(0.0039)
$log(TA)_{it}$	0.0055
	(0.0092)
Country effects	Yes
Sector effects	Yes

that operate in the same jurisdiction as the SOE parent.[35] All variables of interest show a significant increase in magnitude and significance level. However, we confirm that mixed ownership significantly impacts leverage, irrespective of the tax rate. This evidence is consistent with the arguments from above: The facilitated access induced by partial state ownership should lead to a stronger effect of SOE_i (the coefficient is now 0.4545). The tax rate does not matter for mixed-ownership firms, as the cost-benefit trade-off becomes less relevant.

[35]In most but not all cases, the location of the SOE parent coincides with the location of the owning government. A few firms in our sample have various owning governments (mostly France and Belgium) or are firms registered in the OECD by non-OECD governments.

3.6 SOEs and international tax planning

So far, our results have shown that partial state ownership helps MNEs to expand the tax shields of mixed-ownership affiliates. Let us now address the question of whether partially state-owned affiliates are important vehicles for debt shifting within tax-planning MNEs.

It seems that partial state involvement does not discourage firms from making use of preferential tax regimes and tax planning. For example, the European Commission argues that tax rulings granted by Luxembourg to Engie, a French energy giant in partial state ownership (33%), amount to illegal state aid.[36] According to news reports, Engie, as well as other partially-state-owned MNEs such as Eni, Thales, or EDF, have established holdings in the Netherlands to cut their tax bills.[37] A prominent case from Germany is WestLB, a bank that was split up in 2012 and is assumed to have dodged an estimated amount of 600 million euros in taxes between 2006 and 2011.[38]

Previous literature has argued that MNEs often use internal debt to save taxes. This strategy involves lending from firm entities located in tax haven (or low-tax) countries to affiliates located in high-tax countries, where interest payments reduce taxable income. Buettner and Wamser (2013) suggest that optimizing MNEs operate a tax haven affiliate and all lending is provided from that location.[39] The empirical implication is that the minimum tax rate within the firm should negatively correlate with borrowing at other locations. This is because a higher tax in the country in which the MNE operates a lending affiliate reduces the incentives to use internal debt at other locations as the tax savings from providing debt across borders decrease. While, in our data, we cannot distinguish between internal and external debt financing, we would expect that the debt ratio at i increases if the tax at the location of the "lowest-tax affiliate" is cut. For this purpose, we define the variable $MINTAX_{ft} = min(TAX_{kt}) \; \forall \; i \in N^f$, where N^f denotes the total number of affiliates that belong to MNE f.

Table 3.6.1 presents the test of the profit shifting hypothesis. Column A adds only the $MINTAX_{ft}$ variable, whereas Column B also controls for a differential impact depending on

[36] "EU probes French gas firm's Luxembourg tax dealings" (Maurice, 2016).

[37] "French companies set up in Netherlands to cut tax bill" (Reuters, 2013).

[38] "Dividend-Stripping Probe Targets WestLB" (Iwersen and Votsmeier, 2015).

[39] The theoretical argument has been introduced by Mintz and Smart (2004). See also Møen et al. (2018).

ownership. The results suggest the following: In Column A, the effect of $MINTAX_{ft}$ alone does not affect firm leverage significantly. However, once we take the differential impact of $MINTAX_{ft} \times SOE_i$ into account (Column B), the minimum tax rate is estimated with a negative sign. As expected, MNE affiliates with $SOE_i = 0$ adjust their capital structure if the tax rate at the minimum affiliate increases. This does not hold for firms with $SOE_i = 1$, as the interaction between $MINTAX_{ft}$ and TAX_{kt} has a positive sign. While the overall effect of $MINTAX_{ft}$ is still negative, the effect of the minimum tax is less important. This is indirect evidence that tax planning and tax avoidance using internal lending play a less important role under mixed ownership.

The finding is consistent with the other results from above: Affiliates that are partially state-owned exploit maximum debt levels under state guarantees. Beyond that, however, the affiliates do not respond to tax incentives comparable to entirely privately held affiliates.[40]

3.7 Conclusion

We examine the impact of partial state ownership on the debt financing of MNE affiliates with state participation. We find that the well-established impact of profit taxation on capital structure does not hold for mixed-ownership firms, using balance sheet data of affiliates from 22 OECD and EU member countries. Thus, a general finding of our study is that the impact of taxes on debt financing depends on ownership. The partially state-owned affiliates are found to use substantially more debt, which is evidence that these firms operate under implicit state guarantees. We argue that privately owned MNEs make use of facilitated access to debt to maximize interest tax shields in such affiliates. The effect of being partially state-owned is identified by first finding comparable control units of privately held affiliates. The requirements for being accepted as a comparable control unit are relatively strict, as matched pairs (of partially state-owned and private affiliates) must operate in the same country and the same sector. Moreover, our estimation approach accounts for time and pair-fixed effects, as well as a number of time-varying control variables, which are standard in the literature analyzing debt ratios.

We confirm our central finding in many robustness tests, including checks where we randomly assign treatment status. We additionally provide evidence that partial state ownership

[40]We should note that the results presented in Table 3.6.1 are sensitive to the caliper choice in the matching procedure. In particular, the estimates are more robust when smaller calipers are chosen.

Table 3.6.1: Minimum tax affiliates

The table presents pair-fixed effects regressions based on $N_A = 935$ and $N_B = 935$ observations, respectively; the base year for pair matching is 2005; Column A does not differentiate the effect of a change in the minimum tax rate of the group with respect to ownership; Column B controls for a differential impact. *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: Minimum tax rate *Coeff.* *(s.e.)*	B: Differential impact *Coeff.* *(s.e.)*
SOE_i	0.3929**	0.3496*
	(0.1877)	(0.1874)
TAX_{kt}	1.6464***	1.8851***
	(0.4609)	(0.4927)
$TAX_{kt} \times SOE_i$	-1.2540**	-1.6394**
	(0.5772)	(0.6404)
ROA_{it}	0.0004	0.0005
	(0.0005)	(0.0005)
$ATANG_{it}$	0.2724***	0.2943***
	(0.0644)	(0.0661)
$CORRF_{kt}$	-0.0038**	-0.0038**
	(0.0016)	(0.0016)
$INVESTF_{kt}$	0.0007	0.0006
	(0.0008)	(0.0008)
$CREDITM_{kt}$	0.0002	0.0002
	(0.0004)	(0.0004)
$GDPG_{kt}$	0.0004	0.0005
	(0.0012)	(0.0012)
$log(TA)_{it}$	-0.0038	-0.0073
	(0.0110)	(0.0111)
$MINTAX_{ft}$	-0.3003	-0.6797*
	(0.2518)	(0.3738)
$MINTAX_{ft} \times SOE_i$		0.6524*
		(0.3675)
Country effects	Yes	Yes
Sector effects	Yes	Yes

does not relate to other tax planning strategies involving debt financing for reasons of international tax planning. The main finding of our study is that partially state-owned firms neglect the cost of debt and employ a maximum attainable debt ratio. This suggests that government participation may enable MNEs to reduce tax payments, which may ultimately lead to a loss of revenue for the public owner.

3.8 Appendix to Chapter 3

3.8.1 Additional sensitivity analyses

3.8.1.1 Additional controls

We additionally include two controls, which have been used in the literature as determinants of leverage (see Graham and Leary, 2011, for a survey).[41] In Table 3.8.1 we include the investment proxy $\Delta log(FA)_{it}$ already used in the propensity score estimation. Investment levels of a firm can affect the leverage level through several channels. First, credit may have been used to finance investment. Hence, a higher investment level should coincide with higher leverage. Second, many credit contracts restrict investments of a firm through financial covenants. Low investment levels could thus be an indicator for an already highly leveraged firm. Roberts and Sufi (2009) present evidence that one-fourth of U.S. public companies violate such covenants at some point. The ensuing technical default allows lenders to decrease the size of a credit facility or even terminate the contract early, with adverse consequences on firm leverage. But also if no technical default is present, i.e., a covenant has not been breached, lenders may be more cautious in disbursing revolving credit facilities if a firm has very high investment levels due to increased cash flow risk. Column A of Table 3.8.1 presents estimates with investment as an additional control. The results are very similar in magnitude and significance to our basic results for all variables. Hence, our results do not support the idea that investment levels affect affiliate leverage.

Another control is a firm's growth opportunities, denoted by $GROP_{skt}$, which can be an indicator of future profits and should positively affect leverage (Harris and Raviv, 1991). We use the Huizinga et al. (2008) definition as the annual growth rate median of affiliate sales in an affiliate's country and industry. However, the inclusion of growth opportunities does not significantly alter our results. We also conduct robustness checks for inflation, the cost of enforcing formal contracts and financial freedom (not separately reported). None of these additional controls alters the findings in any significant way, but the number of observations becomes substantially smaller because of missing values in these controls.

[41] Note that we prefer the more parsimonious specifications from above to have as many observations as possible.

Table 3.8.1: Investment and growth opportunities

The table presents pair-fixed effects regressions based on $N_A = 1,481$ and $N_B = 847$ observations, respectively; the base year for pair matching is 2005; sector matches are within three-digit SICs; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: Investment *Coeff.* *(s.e.)*	B: Growth opportunities *Coeff.* *(s.e.)*
SOE_i	0.3362**	0.3356**
	(0.1354)	(0.1347)
TAX_{kt}	0.9789**	0.9720**
	(0.3759)	(0.3706)
$TAX_{kt} \times SOE_i$	-1.0221**	-1.0186**
	(0.4206)	(0.4180)
ROA_{it}	0.0003	0.0003
	(0.0004)	(0.0004)
$ATANG_{it}$	0.2469***	0.2440***
	(0.0650)	(0.0644)
$CORRF_{kt}$	-0.0026**	-0.0027**
	(0.0013)	(0.0013)
$INVESTF_{kt}$	0.0001	0.0001
	(0.0010)	(0.0010)
$CREDITM_{kt}$	0.0003	0.0003
	(0.0003)	(0.0003)
$GDPG_{kt}$	-0.0000	-0.0001
	(0.0028)	(0.0027)
$log(TA)_{it}$	0.0064	0.0066
	(0.0088)	(0.0090)
$\Delta log(FA)_{it}$	-0.0077	
	(0.0095)	
$GROP_{skt}$		-0.0039
		(0.0133)
Country effects	Yes	Yes
Sector effects	Yes	Yes

3.8.1.2 Variations in the propensity score estimation

The specification of our propensity score model is very similar to the one in Borisova et al. (2012). Although we believe this specification is very plausible on economic grounds, we re-estimate our model with different propensity scores. As a first step, we include asset tangibility $ATANG_{it}$ as an additional predictor of treatment status and then re-estimate our outcome equation (3.2). Column A of Table 3.8.2 presents the results. The inclusion of asset tangibility increases the magnitude of coefficients and robustness of all variables of interest. The impact

Table 3.8.2: Variations in propensity score estimation

The table presents pair-fixed effects regressions based on $N_A = 1,481$ and $N_B = 847$ observations, respectively; the base year for pair matching is 2005; sector matches are within three-digit SICs; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: Including $ATANG_{it}$	B: Excluding $\Delta log(SALES)_{it}$
	Coeff.	*Coeff.*
	(s.e.)	*(s.e.)*
SOE_i	0.4143***	0.3138**
	(0.1124)	(0.1247)
TAX_{kt}	1.1860***	1.1463***
	(0.3120)	(0.3758)
$TAX_{kt} \times SOE_i$	-1.1936***	-0.9823**
	(0.3498)	(0.3977)
ROA_{it}	-0.0012*	-0.0008*
	(0.0007)	(0.0005)
$ATANG_{it}$	0.1391**	0.1802***
	(0.0609)	(0.0525)
$CORRF_{kt}$	-0.0024*	-0.0010
	(0.0014)	(0.0014)
$INVESTF_{kt}$	0.0010	0.0009
	(0.0011)	(0.0010)
$CREDITM_{kt}$	0.0006	0.0002
	(0.0004)	(0.0003)
$GDPG_{kt}$	-0.0032	-0.0040
	(0.0038)	(0.0028)
$log(TA)_{it}$	0.0116	-0.0057
	(0.0086)	(0.0098)
Country effects	Yes	Yes
Sector effects	Yes	Yes

of the treatment variable SOE_i is now 0.41. An increase in the tax rate TAX_{kt} of one percentage point, ceteris paribus, increases leverage by 1.18 percentage points, an increase in magnitude of roughly 20 percent. Similarly, the effect of the interaction term $TAX_{kt} \times SOE_i$ has increased to the same extent, suggesting that the tax rate does not influence the capital structure of partially state-owned MNEs at all. Firm profitability, asset tangibility, and corruption freedom are significant and remain in line with prior estimation results.

We finally use a specification to estimate equation (3.1) without the sales growth control $\Delta log(SALES)_{it}$. Unfortunately, the ORBIS dataset suffers from many missing values in the total sales variable which we use to compute $\Delta log(SALES)_{it}$. Hence, by excluding the variable at both estimation stages, we can base our model on $N_B = 1,798$ observations, an

increase of about 21 percent. Column B of Table 3.8.2 presents the estimates without sales growth in the first stage. The results look very similar to our baseline estimates.

3.8.2 Additional figures and tables

Table 3.8.3: Correlation matrix

The table presents correlations of the dependent variable LEV_{it}, state ownership SOE_i and the control variables used in the regression models.

	LEV_{it}	SOE_i	TAX_{kt}	ROA_{it}	$ATANG_{it}$	$log(TA)_{it}$	$log(SALES)_{it}$	$CORRF_{kt}$	$INVESTF_{kt}$	$CREDITM_{kt}$	$GDPG_{kt}$	$GDPPC_{it}$
LEV_{it}	1.00											
SOE_i	0.08	1.00										
TAX_{kt}	0.05	0.05	1.00									
ROA_{it}	-0.12	-0.01	-0.04	1.00								
$ATANG_{it}$	0.36	0.14	-0.03	-0.13	1.00							
$log(TA)_{it}$	0.10	0.11	0.15	-0.00	0.30	1.00						
$log(SALES)_{it}$	-0.05	0.04	0.10	0.08	-0.02	0.79	1.00					
$CORRF_{kt}$	0.02	-0.02	0.25	0.04	-0.10	-0.09	-0.07	1.00				
$INVESTF_{kt}$	0.10	0.03	-0.20	0.02	0.02	-0.02	-0.01	0.38	1.00			
$CREDITM_{kt}$	0.09	0.03	0.50	-0.06	0.01	0.20	0.16	0.43	0.13	1.00		
$GDPG_{kt}$	-0.02	-0.00	-0.16	0.08	-0.00	-0.09	-0.06	-0.14	0.01	-0.40	1.00	
$GDPPC_{kt}$	-0.02	-0.00	0.32	0.05	-0.11	-0.09	-0.07	0.78	0.10	0.26	-0.06	1.00

Figure 3.8.1: Distribution of placebo estimates

Figure 3.8.1 depicts empirical cumulative distribution functions for 1,000 coefficient estimates of our variables of interest; treatment was assigned to 178 randomly selected cross-sectional units in each iteration; the first row contains the coefficient estimates and 5 percent confidence intervals (CIs) for SOE_i; the second and third row follow analogously for TAX_{kt} and $TAX_{kt} \times SOE_i$; vertical lines represent the estimates from Table 3.4.3.

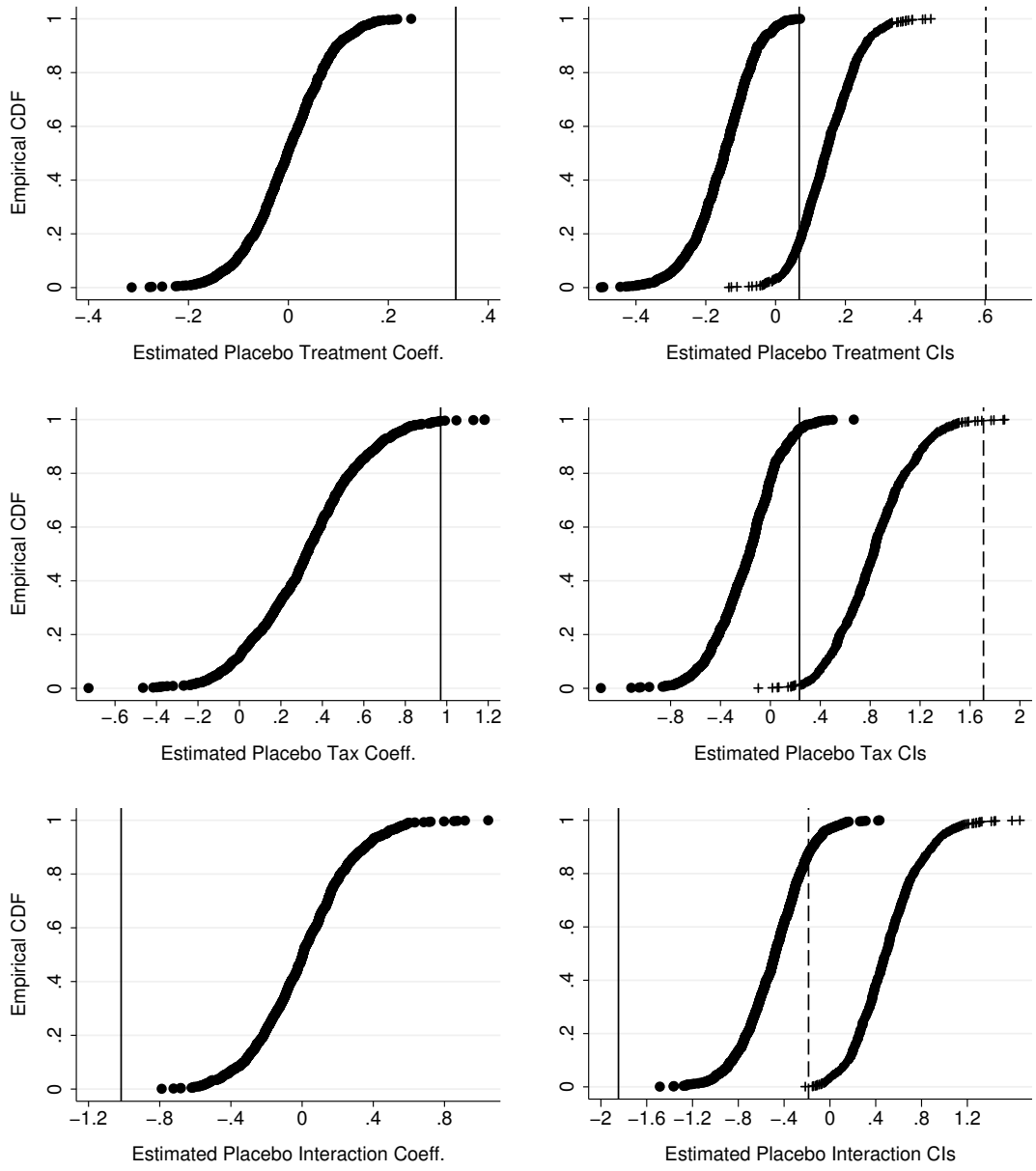


Table 3.8.4: Propensity score estimation

The table presents the propensity score estimation of (3.1) based on $N = 5,827$ observations in $t = 2005$; the dependent variable is the propensity score to be partially state-owned; the CI is the 5 percent confidence interval; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	Coeff. *(s.e.)*
LEV_{it}	4.0600***
	(1.3066)
LEV_{it}^2	-11.6170**
	(4.5815)
LEV_{it}^3	8.8499**
	(3.9179)
ROA_{it}	0.0044
	(0.0047)
ROA_{it}^2	-0.0001
	(0.0001)
ROA_{it}^3	-0.0000*
	(0.0000)
$log(TA)_{it}$	-2.2833***
	(0.6355)
$log(TA)_{it}^2$	0.2576***
	(0.0669)
$log(TA)_{it}^3$	-0.0089***
	(0.0023)
$\Delta log(FA)_{it}$	-0.1605
	(0.1208)
$\Delta log(FA)_{it}^2$	-0.0434
	(0.0527)
$\Delta log(FA)_{it}^3$	0.0040
	(0.0172)
$\Delta log(SALES)_{it}$	0.1806*
	(0.1076)
$\Delta log(SALES)_{it}^2$	0.0098
	(0.0368)
$\Delta log(SALES)_{it}^3$	-0.0089
	(0.0072)
$CREDITM_{kt}$	-0.1718
	(0.2363)
$CREDITM_{kt}^2$	0.0021
	(0.0029)
$CREDITM_{kt}^3$	-0.0000
	(0.0000)
$GDPG_{kt}$	-8.1592***
	(2.5743)
$GDPG_{kt}^2$	2.8761***
	(0.8926)
$GDPG_{kt}^3$	-0.2616***
	(0.0803)
$GDPPC_{kt}$	-0.0017***
	(0.0005)
$GDPPC_{kt}^2$	0.0000***
	(0.0000)
$GDPPC_{kt}^3$	-0.0000***
	(0.0000)
Legal origin: French	4.8292*
	(2.7608)
Legal origin: German	3.8950
	(2.8984)
Legal origin: Scandinavian	2.8027
	(3.2151)
Construction	-0.1805
	(0.5264)
Manufacturing	-0.4067
	(0.4964)
Transport, Communications, Electric, Gas	0.8478*
	(0.4917)
Wholesale Trade	-0.7406
	(0.5119)
Retail Trade	-0.6088
	(0.5976)
Finance, Insurance, Real Estate	0.1393
	(0.5077)
Services	0.1672
	(0.4918)
Constant	30.3277***
	(9.3289)

3.8.3 Variable definitions and data

Table 3.8.5: Variable definitions

Firm level	(Source: Orbis)
LEV_{it}	Long-term debt / total assets of firm i in year t.
SOE_i	Binary indicator of partial state ownership.
$TAX_{kt} \times SOE_i$	Interaction term of SOE_i and TAX_{kt}.
ROA_{it}	Return on assets of firm i in year t.
$ATANG_{it}$	Asset tangibility: fixed assets / total assets of firm i in year t.
$log(TA)_{it}$	Log(total assets) of firm i in year t.
$\Delta log(FA)_{it}$	Investment proxy: log(fixed assets)$_{it} - log$(fixed assets)$_{i,t-1}$.
$\Delta log(SALES)_{it}$	Sales growth: log(turnover)$_{it} - log$(turnover)$_{i,t-1}$.
Country level	
TAX_{kt}	Statutory tax rate of country k in year t.
$CORRF_{kt}$	Corruption freedom in country k and year t (Source: Heritage Foundation).
$INVESTF_{kt}$	Investment freedom in country k and year t (Source: Heritage Foundation).
$CREDITM_{kt}$	Domestic credit provided by banking sector in country k and year t as percentage of GDP (Source: Worldbank).
$GDPG_{kt}$	Annual GDP growth in percent in country k and year t (Source: Worldbank).
$GDPPC_{kt}$	GDP per capita in country k and year t, PPP at constant 2011 international USD (Source: Worldbank).
Legal origin	Legal origin dummy variables of country k based on La Porta et al. (1998).
Sector level	
Sector dummies	Sector dummies are based on 1-digit SIC identifiers.
$GROP_{skt}$	Growth opportunities are defined as in Huizinga et al. (2008): The growth rate median of affiliate sales in an affiliate's industry s, country k and year t.

- BvD definition of SOE:
  - Minimum percentage that must characterize the path from a subject company up to its ultimate owner: 25.01 percent. Hence, a company is considered an SOE if

the government has at least 25 percent direct or indirect control. The path from an SOE to an affiliate is at least 10 percent.

- BvD definition of affiliate:
  - MNE affiliates: Affiliates located in a specific region not ultimately owned but owned by at least 1 percent; may have other shareholders in the foreign country. Extracted for all world regions.
- Only unconsolidated balance sheet information is used (BvD conscode "U1").
- $LEV_{it} \in [0; 1[$. We assume that a fully leveraged firm should be a reporting error since every incorporation form we know requires some equity. The result does not depend on this restriction.
- Only observations with no missing values in LEV_{it} from 2004-2013 are considered.
- Shared SOE-MNE affiliates with a sum of SOE-MNE ownership exceeding 100 percent are dropped as reporting errors.
- Joint affiliates of two SOEs are excluded because we want to focus on joint SOE-MNE affiliates.
- Only wholly-owned MNE subsidiaries are allowed as controls. This ensures that we can unambiguously verify ownership structures.

4 Is commercial state ownership tax neutral?

4.1 Introduction

The EU has led an intense policy and legislative debate on tax planning of MNEs and state aid to such firms.[42] The debate originated from an assessment and corresponding action plan on tax base erosion and profit shifting by the OECD (2013a,b). Governments potentially actively encouraged tax planning strategies by offering attractive conditions in their jurisdictions.[43] A significantly lower tax burden for one firm constitutes a competitive advantage relative to its peers. Therefore, state aid is a key concern to ensure the functioning of the EU's single market and prohibited by its legislation.[44] The European debate has neglected so far whether EU member states' extensive ownership of commercially active SOEs is tax neutral. This paper analyzes whether ETRs of commercially active SOEs differ from those of comparable private firms. It contributes to the existing literature by comprehensively assessing ownership tax neutrality within the EU.

Governments have granted special tax treatment to some private companies in return for investment and employment. Hence, they may grant tax advantages to their own commercially active SOEs out of similar considerations. Such behavior would lead to lower ETRs of SOEs and undermine competition in the single market at the expense of private firms. There are also theoretical arguments why ETRs of SOEs could be higher: First, EU governments receive substantial budgetary contributions from their commercial SOEs (European Commission, 2016). Under agency conflicts, taxes can force distributions from dividend-averse managers (Cui, 2015b). Second, SOE managers may face lower incentives to minimize tax payments than their private-firm counterparts. This is because the owning state is the ultimate beneficiary of both taxes and dividends. Third, public scrutiny on SOE taxation may be higher and tax planning less acceptable in the public eye (Dyreng et al., 2016).

[42] Specific events substantially shaped this debate. For example, a set of internal advance tax rulings of Luxembourgian authorities was leaked to the public in November 2014, revealing how multinational firms have engaged in aggressive tax planning strategies by shifting profits into the grand duchy. A second example is the tax-related record fine imposed on Apple by the European Commission in September 2016.

[43] Throughout this study, I use the term "tax planning" to describe all efforts of a company to reduce its effective tax rate. Hence, I do not distinguish between legal and illegal activities.

[44] Treaty on the Functioning of the EU (TFEU), Article 107.

It is – to some extent – a political decision which firm is state-owned and which firm is not. Hence, ownership is not random but depends on a set of variables, which are partly tricky to observe. Consistent evaluation of whether state ownership affects company ETRs needs to consider non-random ownership in its estimation procedure. Thus, this study follows a selection-on-observables approach and estimates a propensity score of state ownership similar to Borisova et al. (2012). The propensity score identifies a set of similar private firms by nearest neighbor matching, which serves as a control group to construct group-fixed effects. In a subsequent step, I estimate group-fixed-effects regressions that control for a state ownership dummy, a vector of time-varying firm characteristics as well as time and sector effects. Controlling for the group-fixed effect eliminates any unobserved time-constant factors between groups. Matching SOEs and private firms within a country ensures that this fixed effect absorbs relevant unobserved country factors such as tax policy attitudes or tax law enforcement quality (Burgstahler et al., 2006). Such factors can either be exploited by private firms or used to support SOEs (Nicodeme, 2001). This estimation approach consistently identifies the impact of state ownership on effective company taxation in the beta coefficient of the state ownership dummy variable.

This study uses a mostly balanced panel from 2009 to 2015 which forms part of Bureau van Dijk's Orbis database. Orbis contains detailed financial and ownership data from annual single and consolidated company financial statements. An advantage of Orbis is that it includes non-listed firms and takes ownership information from official sources. This substantially increases the sample size compared to studies focusing on manual ownership research and listed firms only. The America-focused tax literature identifies a critical issue of financial statement tax data: Managers have an incentive to present high profit levels to investors and low profit levels to tax authorities (book-tax trade-off). Thus, differences could arise between the profit and tax items in a firm's financial statement (aimed at investors) and the same firm's tax statement (targeted at authorities). The tax statement contains the actual taxes paid, the financial statement not necessarily. The book-tax trade-off is mostly relevant at the consolidated level – which is where the US levies corporate taxes. Differences arise in particular because of reporting incentives and the consolidation process itself. Hanlon (2003) and Mcgill and Outslay (2004) point out the limitations of financial statement data when looking at tax-related issues in the US. These concerns are less relevant in this study: EU member states levy taxes at the single-entity level, which leads to a higher degree of book-tax

conformity (Hanlon and Heitzman, 2010; Watrin et al., 2014). The EU obliges every multi-entity company to present separate financial reports for all its single entities (subsidiaries). My sample consists of unconsolidated single-entity firm-years from EU countries. Deferred taxes cause a large share of book-tax differences, an issue I address in Section 4.5 by reporting outcomes for long-run ETRs similar to Dyreng et al. (2008). Hence, the overall inference of this study is relatively free from book-tax bias.

Empirical results strongly suggest that SOEs in the EU pay higher ETRs than private firms. The primary model predicts a robust markup of 1.6 percentage points using the pre-tax profit ETR as the dependent variable. The magnitude of the effect depends to some extent on the profit measure I use to compute ETRs, which is why this study employs three different ones through all model specifications. At the same time, estimated ETR elasticities to statutory tax rates are persistently inelastic for both ownership groups – suggesting that private firms and SOEs respond to an increase in statutory tax rates with more tax planning. This finding is consistent with empirically established tax planning strategies such as increased debt usage under higher tax rates (see Feld et al. (2013) for a meta-study) and anecdotal evidence suggesting that SOEs engage in tax planning just as private firms do.[45] Several explanations for this central finding seem plausible: First, owning governments may force distributions via tax payments (Cui, 2015a), which leads to higher ETRs of SOEs. This appears particularly credible considering the budgetary contributions of commercial SOEs. Second, tax payments of SOEs may be under closer public scrutiny than private-firm taxation, which has the same effect (Dyreng et al., 2016). Third, SOE managers could pursue a quiet life and refrain from active tax planning, especially at lower statutory rates, i.e., when the opportunity cost in forgone pet projects is low. Descriptive statistics suggest that the ETR markup for SOEs may not be equally strong in all countries. This is not surprising because EU member states have different intensities and regulatory backgrounds of state ownership. The main result of this study is robust to (i) exact matching within country and two-digit NACE2 sector, (ii) variations in the number of matched private cross-sectional units, (iii) changes in model and propensity score specifications, (iv) long-term ETRs as dependent variables, (v) alternative econometric approaches, (vi) the use of consolidated instead of unconsolidated financial data, and (vii) placebo falsification tests.

[45] A well-known case is "sale and leaseback" deals of municipal enterprises, which primarily aim at reducing tax payments.

The contribution of this study is twofold: First, to the best of my knowledge, it is the first to undertake a comprehensive empirical analysis of effective firm-level taxation and state ownership in Europe. In this, it adds effective taxation to an established set of differences between private and state-owned firms. Second, it contributes to the policy discussion on tax planning by showing that effective taxation within the EU's single market is not ownership neutral. Hence, it indicates that private firms may be more successful in reducing their tax liabilities than SOEs, which could adversely affect competitive neutrality.

The rest of this study proceeds as follows: Section 4.2 deals with the ETR measures employed in empirical models, develops testable hypotheses and reviews the relevant literature on taxation and state ownership. Section 4.3 develops the empirical methodology and describes its implementation using the Orbis data set. Section 4.4 looks at descriptive statistics and contains the main estimates. Section 4.5 contains sensitivity analysis. Section 4.6 concludes.

4.2 Definitions: Effective taxation and state ownership

4.2.1 Effective taxation

Hanlon and Heitzman (2010) provide a comprehensive overview of ETR-based indicators used in prior taxation research. This study applies three different specifications of the most common one: The average backward-looking ETR.[46] All three are computed using balance sheet data from Orbis but employ different profit measures. The pre-tax profit ETR is defined as

$$ETR_{it}^{PTP} = \frac{TAX_{it}}{PTP_{it}}, \tag{4.1}$$

where TAX_{it} is firm i's aggregated tax liability in year t and PTP_{it} is its pre-tax profit in the same period.[47] The ETR_{it}^{PTP} is a standard measure in the literature and suggested as such

[46] In this study, the abbreviation "ETR" always refers to the average backward-looking ETR. In addition to simple ETRs, I estimate all models using a second indicator type proposed by Hanlon and Heitzman (2010), namely ETR-tax rate differentials. Results point to similar conclusions and are available upon request.

[47] In Orbis, I divide the item *#taxa* (taxation) by *#plbt* (profit/loss before tax) in equation (4.1), *#oppl* (operating profit) in equation (4.2), and *#ebta* (EBITDA) in equation (4.3), respectively.

in IAS 12.86.[48] A recent study that uses this indicator with a similar data set is Watrin and Thomsen (2016). It is also widely used in actual corporate decision making (Graham et al., 2017). The ETR_{it}^{PTP} is calculated using a company's pre-tax profit, i.e., after subtracting all operating and financial expenses of the firm's ordinary business activity in the given year. An advantage of this specification is that the profit measure should be relatively close to actual taxable income. The literature is, however, not entirely conclusive on which profit measure to use. Nicodeme (2001), for instance, suggests using a firm's operating profit in the denominator because its calculation does not differ much across countries. Hence, as a second ETR specification, I define

$$ETR_{it}^{OPP} = \frac{TAX_{it}}{OPP_{it}}, \tag{4.2}$$

where TAX_{it} are again firm i's total tax expenses in year t and OPP_{it} its operating profit of the same period. I use a third specification based on earnings before interest, tax, depreciation, and amortization (EBITDA):

$$ETR_{it}^{EBI} = \frac{TAX_{it}}{EBI_{it}}. \tag{4.3}$$

With EBI_{it} as the denominator, results are less dependent on national accounting practices and depreciation policies of individual firms (Vause, 2009). There is no single best ETR definition, as each has advantages and drawbacks. It is also important to understand that ETRs based on alternative profit measures capture different things: For example, variations between SOEs and private firms in the ETR_{it}^{EBI} should not originate from deviating depreciation policies, whereas differences in ETR_{it}^{PTP} could arise because of depreciation-based tax planning. Therefore, this study always reports regression results using ETRs (4.1) to (4.3) to avoid relying on a single ETR definition.

A strength of all indicators is that they stem from actual firm data. A shared weakness arises from reliance on accounting tax data, which can differ from actual taxes paid. Companies usually keep at least two sets of books – one for accounting purposes and one for taxation. In the former, they target investors and attempt to present the company in an informative

[48] More specifically, TAX_{it} should consist of current tax expenses minus deferred tax liabilities. Unfortunately, Orbis only provides an aggregate total tax variable which does not allow to identify a firm's deferred tax liabilities separately. All ETRs in this study rely on the total tax variable from Orbis.

way. In the latter, they target tax authorities and attempt to minimize tax exposure. For this reason, numerous studies focusing on US data show that inferences from accounting data on tax-related topics do not always yield reliable results (Graham, 1996; Hanlon, 2003; Mcgill and Outslay, 2004; Dyreng et al., 2008). The fact that a considerable amount of studies have nevertheless attempted to do so stems from the non-disclosure of companies' tax statements.[49] Book-tax differences vary greatly between countries. In Europe, they are significantly lower, especially in single (individual) accounts (Mcgill and Outslay, 2004; Burgstahler et al., 2006; Goncharov and Werner, 2009; Hanlon and Heitzman, 2010). The EU obliges every entity by law to publish these accounts, which constitute the basis to assess taxable income (Watrin et al., 2014). For this reason, I use single financial statements of European firms in all models except for the consolidated model presented in Section 4.5. I conclude that the present study is less susceptible to biased inference than many previous studies using American data.

In addition to book-tax differences, a few other caveats remain: First, a company can reduce tax payments by reporting lower accounting earnings and lower taxable income. This "conforming" tax planning would not show up in the accounting information and cannot be captured by any indicator based on accounting figures (Hanlon and Heitzman, 2010). The same holds for permanent tax rebates, which would not show up in accounting tax data (Buijink et al., 2002). There is a strong reason to believe that permanent tax rebates to commercially active SOEs (or other firms) are mostly absent in the EU because they would fall foul on the EU's state aid legislation.[50] In contrast, the ETR measures in this study capture non-conforming tax planning strategies such as temporary rebates, transfer pricing or amortization techniques. Second, national accounting frameworks could affect results. Within the EU, regulation has harmonized accounting rules to a significant extent.[51] Cross-country differences in accounting practices persist. According to Collins and Shackelford (1995), critical areas of such differences are the treatment of depreciation, goodwill amortization, pension expense and expenses for research and development. Another possi-

[49]Of these, a first group of studies focuses on determinants of companies' ETRs (Zimmerman, 1983; Stickney and McGee, 1982; Gupta and Newberry, 1997) and a second group on cross-country comparisons (Collins and Shackelford, 1995; Buijink et al., 2002).

[50]TFEU, article 107. The introductory examples show that governments have wooed large MNEs with precisely such rebates. However, they may rather be the exception than the rule.

[51]The Fourth and Seventh Company Law Directives harmonized accounting rules to a significant extent and were implemented into national law in the 1980s and 1990s. They oblige publicly traded and private companies to prepare audited financial statements following the directives.

ble source that has gained importance in recent years is the treatment of intangible assets. Differences in the legal framework should be captured to a large extent by the group-fixed effect, which controls for unobserved heterogeneity of SOEs and comparable private firms within a specific country. Firm-level accounting variations within a group are precisely the driving force that could make a private firm's tax strategy more effective than an SOE's. The last caveat is that company taxation is of a dynamic nature. Comparisons using yearly data alone may be misleading (Dyreng et al., 2008). To address this issue, I report results using long-term ETRs in Section 4.5.

4.2.2 State ownership in the EU and hypothesis development

The extent of state ownership in the EU varies from country to country. For example, the United Kingdom has completely privatized its energy and much of its transport sector, whereas SOEs are important players in the same industries in both France and Belgium. In most countries, state ownership is widespread: A recent report by the European Commission (2016) shows that in 13 of the block's 28 member countries the market value of central SOEs exceeds ten percent of GDP, among them France and Italy. The employment share of SOEs relative to the total workforce ranges between two and six percent in the majority of countries but goes up to ten percent in France. Budgetary contributions of SOEs are non-negligible: Finland, for instance, received an average of 1.5 percent of GDP from 2005 to 2014 from its SOEs. Numerous other countries like Sweden, Estonia, Slovakia, and the Netherlands also received distributions in this period exceeding one percent of GDP. These are substantial numbers: In 2016, total government revenue from value-added tax ranged between 3 and 13 percent of GDP in EU member states. High budgetary contributions suggest that many SOEs in EU countries are of commercial nature.

Few studies have looked at the effect of ownership structure on ETRs. A study by Chen et al. (2010) uses S&P 1500 effective book and cash tax rates to analyze whether family-owned firms are more or less tax-aggressive than non-family-owned firms. Their results indicate a lower aggressiveness of family-owned firms.

State ownership could alter company ETRs towards both lower and higher levels. From a regulatory perspective, commercial firm activity should be subject to the same legal framework across all EU member states irrespective of ownership structure. The purpose of this is that EU member states remain autonomous in their asset ownership decisions while ensuring

that private companies or companies from other member states are not discriminated against. The competitive neutrality agenda of the EU leads to the benchmark Hypothesis 1:

Hypothesis 1 *Competitive neutrality in the EU single market extends to effective taxation, and there is no significant difference between SOE and private-firm ETRs.*

The EU single market is, however, still imperfect in many aspects. In theory, the European Commission ensures competitive neutrality and should prevent member states deviating from it. In practice, the Commission delegates a large share of monitoring and implementation of EU competition law to national competition authorities. As a result, oversight and enforcement of EU competition law vary greatly between countries – a key concern of current EU legislative activity.[52] Besides, it would be premature to derive the existence of large state sectors from the non-commercial Atkinson and Stiglitz (2015) argument of market failure alone.[53] Instead, state ownership appears to have a clear political dimension (Shleifer and Vishny, 1994; Shleifer, 1998).[54] For example, about 21 percent of SOEs in Italy offer goods or services without any public service obligation (European Commission, 2016). At least 3,000 Italian SOEs have less than six employees, and in about half of them, there are more directors than workers. If the state controls both the tax authority and the company, conflicts of interest can arise. On the one hand, a government must commit to competitive neutrality in the EU's single market and prevent preferential treatment of any commercially active cooperation – independently of ownership. On the other hand, favorable taxation may be preferred over job losses or private market entry. Moreover, some SOEs have become internationally active, and their success could be a question of national pride. As governments are willing to give privately owned firms special tax treatment for political considerations, they could do the same for their commercially active SOEs. Hence, the political view of state ownership offers an explanation of why governments could be more lenient with their firms, which would lead to lower ETRs of SOEs. This leads to Hypothesis 2:

[52]COM (2017) 142: Proposal for a directive of the European Parliament and of the Council to empower the competition authorities of the member states to be more effective enforcers and to ensure the proper functioning of the internal market.

[53]In this "social" theory of state ownership, the government steps in when private companies would not provide an efficient level of products or services. An example to illustrate this argument is postal services to remote areas, which private companies would not provide because it would incur them a loss.

[54]The political view of state ownership argues that SOEs are vehicles of politicians' private interests such as extracting rents and pursuing employment or investment policies.

Hypothesis 2 *Commercial SOEs pay lower ETRs than comparable private firms because governments subsidize their firms out of political considerations.*

A number of empirical studies using non-European data find effects in this direction (Derashid and Zhang, 2003; Adhikari et al., 2006; Cui, 2015a).

Other empirical findings associated with state ownership offer predictions why ETRs of SOEs could be higher. SOEs have been found to suffer from worse corporate governance, lower profitability, softer budget constraints, and lower labor intensities (Dewenter and Malatesta, 2001; Chen et al., 2011; Borisova et al., 2012; OECD, 2014a; Chen et al., 2017). A possible explanation for the overall worse performance is low-powered incentives of the SOE management, i.e., a situation where returns from a transaction cannot be collected directly by the transacting party (Williamson, 1985; Tirole, 1994; Banerjee, 1997). SOE managers do not own shares in wholly owned SOEs and cannot be rewarded with share options because this would amount to privatization. Hence, SOE managers may benefit from increased firm profit to a lesser extent than private firm managers. Aggressive tax planning is costly, requires effort and poses a potential employment risk in case of discovery, which could lead managers to prefer a "quiet life" (Hicks, 1935; Bertrand and Mullainathan, 2003). Given the fact that executives matter for levels of tax planning (Dyreng et al., 2010), the presence of low-powered incentives of SOE managers compared to private firm managers could explain lower levels of tax planning and higher ETRs of SOEs. A second argument derives from weak monitoring of SOEs' corporate activities (Megginson and Netter, 2001; Musacchio and Lazzarini, 2012).[55] This could aggravate agency conflicts associated with free cash flow (Jensen, 1986) and lead to empire building and pet projects. Taxation is then a useful tool to force distributions from dividend-averse SOE managers (Cui, 2015a). The tax agency is already dealing with tax-optimizing private firms, and taxation of SOEs does not require any additional skills (in contrast to evaluating corporate activities). In case of discovery, the general public would undoubtedly disapprove of SOE tax planning, a fact that managers may take into account (Dyreng et al., 2016). This argument seems especially important when considering the substantial budgetary contributions of commercial SOEs. Taken together, these arguments give rise to Hypothesis 3:

[55] Weaker SOE monitoring could also be the result of low-powered incentives in responsible ministries or government agencies because they act as fiduciaries for the general public and not as owners (Frant, 1996).

Hypothesis 3 *Commercial SOEs pay higher ETRs than comparable private firms because governments force budgetary contributions via tax payments and private firms engage in more tax planning.*

A study that finds higher ETRs for SOEs than for private firms in Chinese data is the one by Wu et al. (2013). Note that Hypotheses 1 to 3 complement each other. The procedure described in Section 4.3 provides a test for their validity.

4.2.3 Identification of ownership in Orbis and sample methodology

The study's sample period ranges from 2009 to 2015 and includes companies with non-missing unconsolidated financial statements in at least two of the seven years.[56] I identify unconsolidated financial statements with the indicators U1 and U2 from Orbis. U1 refers to firms for which only unconsolidated data is available and U2 to firms with both types. A single-entity company is state-owned if the Orbis variable *#ownership type* takes the form "Public authority, state, government". A group subsidiary is state-owned if the ownership path from the group company to the subsidiary is 100 percent and Orbis classifies the group as an SOE. If a company fulfills one of these criteria, the indicator variable is $SOE_i = 1$. A private firm may carry any other ownership label and takes on the indicator variable $SOE_i = 0$. To ensure a firm is truly private, I keep only observations with a single owning entity that does not fall into the SOE categories above.

For many firm-years, I do not observe any ownership information, which reduces the sample size significantly.[57] I partially mitigate this by imputing up to two consecutive years of missing ownership data if the same owner controls an observation before and after the information gap with equal share. This study focuses on state ownership in general and does not differentiate between different owning institutions. If a single entity firm has several direct state owners – say a ministry, a state-owned bank, and a municipality – they are summed up to a total state share. I drop an observation if its total percentage remains below 100 percent, i.e., any private owning entity remains. It is important to note that the sample

[56]The average cross-sectional unit has more than five firm-years, which gives the panel a largely balanced structure.

[57]Missing values originate most likely from data collection and should not correlate with state ownership or any of the control variables because state holdings are public. Therefore, I assume the absence of sample selection based on ownership type.

includes only SOEs operating in the same country as the owning state. Thus, taxes and firm profit ultimately benefit the same state.[58] This could alter ETRs in both directions: On the one hand, it ensures that political arguments for state ownership such as local employment are relevant. On the other hand, it could increase management incentives to live a quiet life without tax planning. I do not impose such an owner-location restriction on private firms in the sample.

The dataset only includes firms operating in the EU because European state aid legislation prohibits any structural taxation difference between commercially active SOEs and private firms. I assume SOEs are of commercial nature for several reasons: First, they exist as separate legal corporations. All German sample SOEs are either registered as *GmbH* (limited liability company) or *AG* (joint stock company), indicating that they do not form part of the general government and remain outside its boundary. Similarly, Italian firms are registered either as S.R.L. (limited liability) or S.P.A. (joint stock company). The same holds for SOEs in other countries, which are either limited liability companies or stock companies. Second, I consider only firms with positive ETRs. This ensures that sample SOEs are taxable entities and therefore of commercial nature. Third, I exclude firms that could be exempt from taxes because they provide non-profit services in the health or social sectors.[59] I identify these firms by their NACE2-categories *O: Public administration, defense, compulsory social security*, *P: Education*, and *Q: Human health and social work activities*. Furthermore, I exclude all financial firms (NACE2 category *K: Financial and insurance activities*) as state-owned banks have a special regulatory status in some member states. If a sample SOE has a partial public service obligation, this does not necessarily affect the taxation variable because, in OECD countries, compensation for such activities mostly consists of direct transfers (OECD, 2014a). Summing up, sample SOEs should be commercially active companies and receive the same tax treatment as private firms.

The focus on EU data is useful from an accounting perspective as sufficient cross-country differences remain while holding underlying accounting standards constant (Burgstahler et al., 2006). Consolidated financial statements using IFRS are obligatory for companies in all EU

[58] In some member states, like Germany or Italy, several levels of government collect corporate taxes. Taxes and profit may, therefore, benefit different levels of government.

[59] For instance, §52, Section 2 no. 3 and 4 of the *Abgabenordnung* in combination with §5 Section 1 no. 9 of the *Körperschaftssteuergesetz* exempts institutions of a purely non-profit character from income taxation in Germany.

countries. For single financial statements, national legislation differs across member states. Orbis contains both IFRS and local GAAP single financial statements with the large majority being local GAAP. All firms in my estimation sample use local GAAP to ensure that the same legal accounting framework applies to each company in country-specific sub-samples.[60]

Figure 4.2.1: Average pre-tax profit ETR by country

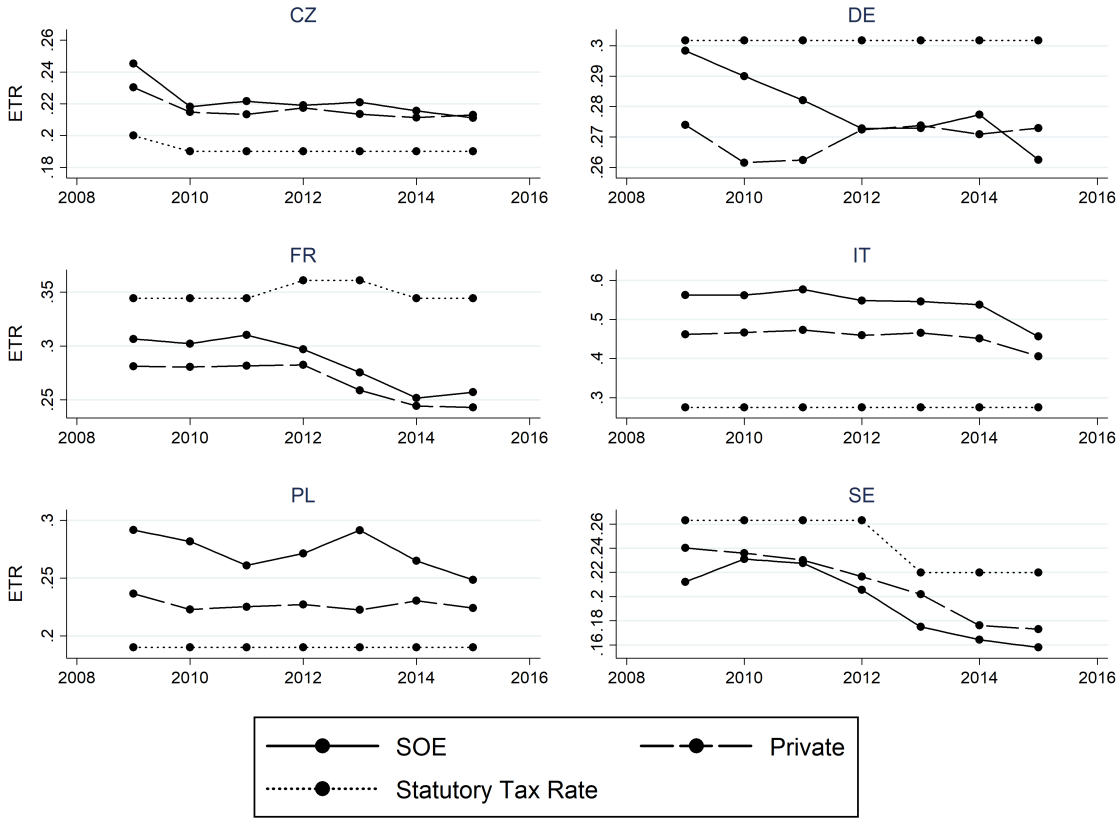


Figure 4.2.1 plots the ETR_{it}^{PTP} from equation (4.1) for both ownership groups in six EU member states.[61] The dotted curve represents the corresponding corporate statutory tax rates $TAXR_{kt}$. The highest statutory tax rate – at around 35 percent – applies in France (FR), followed by Germany (DE) and Italy (IT) at around 30 percent.[62] A general trend is

[60] Appendix Table 4.7.1 presents estimates for the SOE_i variable including IFRS-firms.

[61] Figure 4.2.1 contains the countries with the highest sample number of SOEs. Appendix Figure 4.7.1 plots equivalent curves for Austria (AT), Belgium (BE), Estonia (EE), Finland (FI), Croatia (HR), and Slovenia (SI).

[62] Statutory tax rates are only a crude proxy of individual company taxation because in many countries different levels of government levy taxes. For instance, around half of corporate income tax expenditure in Germany depends on the municipality. As a consequence, $TAXR_{kt}$ necessarily is an

that ETRs for both SOEs and private firms are either constant or declining during the sample period. In the Czech Republic (CZ) and Sweden (SE), the decline coincides with a reduction in statutory rates. In France, the statutory tax rate briefly increased in 2012 and decreased again in 2014. This coincides with a substantial decrease in ETRs starting from the statutory rate increase. Germany, Italy, and Poland (PL) did not change their corporate tax rates in the sample period. The ETR_{it}^{PTP} of SOEs is continuously higher than the one of private firms in France, Italy, and Poland. In the Czech Republic and Germany, the two curves intersect, whereas in Sweden the opposite is the case: SOEs have constantly lower average ETR_{it}^{PTP} than private firms. This pattern also holds for a number of countries plotted in Appendix Figure 4.7.1. Figures 4.2.1 and 4.7.1 suggest that ETR differences depend on the country, which is most probably a consequence of different attitudes towards state ownership and taxation across member states.

Two additional observations can be made in Figure 4.2.1: The first refers to base erosion and profit shifting. A country with strong tax base erosion should have a constant statutory rate and a downward-sloping ETR curve. It is important to recall that the ETR measures in this study are based on financial statement profit and firms face little incentive to under-report these measures (in contrast to tax statement income). Only France has such a pattern for both private firms and SOEs and, interestingly, also has the highest corporate tax rate. In Germany and Poland, the trend is downward-sloping only for SOEs, but not for private firms. Hence, Figure 4.2.1 does not support claims of wide-spread tax base erosion, at least at tax rates levied by the majority of member states. This finding is consistent with observations made in US data by Dyreng et al. (2017). The second observation is that in the Czech Republic, Italy an Poland, ETR curves are higher than statutory rates. Usually, ETRs are lower than statutory tax rates (Vause, 2009; OECD, 2013b). The Orbis variable TAX_{it}, which I use to compute the dependent ETR variables from equations (4.1) to (4.3), represents a firm's total taxes, i.e., it may include other levies such as non-substantial regional taxes.[63] A second explanation is deferred taxes, which could raise the ETR_{it}^{PTP} of both ownership groups in these countries more than in other countries. It is important to recall that this study focuses

average levy for the individual firm and may not include all taxes a firm has to pay.

[63]In Italy, corporate income tax is based on two pillars, the national *imposta sul reddito sulle società* or IRES, and a regional tax, the *imposta regionale sulle attività produttive* or IRAP. Both apply to financial statement profit and are thus reflected in the ETR variable, whereas the statutory tax rate refers only to the IRES because it accounts for the lion's share of corporate tax (PWC, 2016).

on within-country differences between two ownership groups, and the group-fixed effect should absorb any time-constant unobserved effects at the country level. Hence, results should not depend on whether the average ETR_{it}^{PTP} is above or below the corresponding statutory rate.

4.3 Empirical methodology

The main goal of this study is to compare ETRs of SOEs with those of similar private firms in the EU. Hence, the primary effect of interest is the coefficient of the state ownership dummy SOE_i in regressions of the dependent ETR variables from equations (4.1) to (4.3).

In a first step, I identify groups of companies, where each group consists of one company that is state-owned ($SOE_i = 1$) and m companies which are not ($SOE_i = 0 \ \forall \ m$). The m companies should be similar to the SOE to ensure that the group-fixed effect absorbs reasonable cross-sectional differences between groups. Each company i has a propensity to be state-owned that depends on a vector of i-specific determinants $\boldsymbol{X}_{i0}$ and country k-specific determinants $\boldsymbol{Z}_{k0}$. Additionally, the propensity depends on the industry ψ_s, which leads to the specification of the linear probability model index

$$SOE_{it_0} \quad = \quad \boldsymbol{\beta}_1 \boldsymbol{X}_{i0t_0} + \boldsymbol{\beta}_2 \boldsymbol{Z}_{k0t_0} + \psi_s + \epsilon_{ikt_0}, \tag{4.4}$$

to predict the propensity $\hat{p}_{it_0}^{\text{SOE}}$ that company i is state-owned, employing a probit model. The time index t_0 in equation (4.4) indicates that I use only data from the initial year of the sample.[64] Estimating equation (4.4) produces two vectors of probabilistic state ownership: One for SOEs, $\hat{\boldsymbol{p}}^{\mathbf{1}}$, and one for private companies, $\hat{\boldsymbol{p}}^{\mathbf{0}}$.

In a second step, I identify the m nearest neighbors for each SOE. Let c_i^1 denote the respective private firm j which is *closest* to the SOE i. The best match is determined by $c_i^1 = \min_{\{j\}}(|\hat{p}_i - \hat{p}_j^1| < \rho) \ \forall \ j \ \neq \ i$. The caliper ρ represents the maximum difference in propensities between i and j.[65] Identification of the second, third and mth best matches c_i^2, c_i^3 and c_i^m follows analogously. Matches are within country throughout this study to ensure comparability of companies within groups. I also match within two-digit NACE2 sectors, which does not alter results significantly. Matching takes place without replacement,

[64] The initial year is 2010. The year 2009 drops out because of first differencing in control variables.

[65] The standard caliper $\rho = 0.1 * SD^{probit}$ is even stricter than the $\rho = 0.2 * SD^{probit}$ that Austin (2011) suggests. As a robustness check, I also present results using the calipers $0.2 * SD^{probit}$ and $0.01 * SD^{probit}$ in Appendix Table 4.7.1.

which causes a trade-off: On the one hand, a larger m increases the amount of information in the control group and makes economically good comparisons more likely. On the other hand, there may not be sufficient good matches for all SOEs, which leads to the exclusion of some of them. I mitigate this trade-off by presenting model estimates with varying numbers of matched control units.[66] Matching produces groups of companies $\{SOE_i = 1; SOE_i^1 = 0 \ldots, SOE_i^m = 0\}$ where, within each group, one observation is state-owned and m private firms are not.

After identifying $c_i^1 \ \ldots \ c_i^m$ for each SOE, I proceed and estimate

$$Y_{it} = \alpha_1 SOE_i + \boldsymbol{\alpha}_2 \boldsymbol{X}_{it} + \boldsymbol{\alpha}_3 \boldsymbol{Z}_{kt} + \phi_t + c_i^{gm} + \psi_s + \varepsilon_{iskt}, \tag{4.5}$$

where Y_{it} denotes the ETR dependent variables from equations (4.1) to (4.3) of company i in year t ($t = 2010, ..., 2015$). The dummy SOE_i is the main variable of interest, $\boldsymbol{X}_{it}$ indicates a set of firm-level controls, $\boldsymbol{Z}_{kt}$ a set of country-level variables, and ϕ_t, c_i^{gm} and ψ_s denote time, group and sector-specific effects. Note that the index gm in c_i^{gm} indicates the number of best matches used to define the group-fixed effect. Conditioning on c_i^{gm} removes all time-constant cross-sectional differences between company groups. Hence, I can identify the differential impact of being state-owned by time-averaging over all treatment and control units within each group.

4.4 Descriptive statistics and basic results

4.4.1 Descriptive statistics

The final sample consists of 159,398 firm-years, of which 7,612 are SOEs and 151,786 private companies. I observe each company in at least two of the six years of the sample period. The average number of firm-years per company is five, which gives the sample a largely balanced structure. Table 4.4.1 contains summary statistics and Appendix Table 4.7.2 correlations for the dependent variables and controls I use to estimate the propensity score of state ownership (4.4) and the outcome equation (4.5).

The specification of the state ownership probit model (4.4) follows Borisova et al. (2012). On the firm level, I include $log(SALES)_{it}$ as a proxy for firm size, the return on total assets

[66] The main model is based on $m = 3$ control units. Appendix Table 4.7.1 presents results for the variable of interest SOE_i when matching takes place with $m = 1$, $m = 2$ and $m = 5$ private firms.

Table 4.4.1: Descriptive statistics

The table presents summary statistics of the dependent variables ETR_{it}^{PTP}, ETR_{it}^{OPP}, ETR_{it}^{EBI}, state ownership SOE_i, and the control variables used to estimate models (4.4) and (4.5); statistics are based on 159,398 observations.

Variable	**Firm-years**		**State-owned**	**Private**
SOE_i	159,398		7,612	151,786
Dependent Variables	**Mean**	**Std.Dev.**	**Min**	**Max**
ETR_{it}^{PTP}	0.31	0.18	0.00	0.92
ETR_{it}^{OPP}	0.26	0.15	0.00	0.84
ETR_{it}^{EBI}	0.18	0.11	0.00	0.68
Control Variables	**Mean**	**Std.Dev.**	**Min**	**Max**
$log(TA)_{it}$	8.63	1.64	2.71	18.11
$log(SALES)_{it}$	8.78	1.70	0.69	17.24
ROA_{it}	10.10	10.31	0.01	99.78
LEV_{it}	0.14	0.20	0.00	1.00
$ATANG_{it}$	0.25	0.27	0.00	1.00
$log(DEPR)_{it}$	4.79	1.96	0.00	14.84
$\Delta log(FA)_{it}$	0.01	0.51	-7.88	8.79
$\Delta log(SALES)_{it}$	0.03	0.35	-7.27	9.73
$GROP_{skt}$	-0.01	0.10	-2.69	3.15
$TAXR_{kt}$	0.27	0.06	0.10	0.36
$CREDITM_{kt}$	141.32	35.38	37.51	234.02
$GDPG_{kt}$	0.89	1.81	-3.78	7.58
$GDPPC_{kt}$	35,576.02	6,101.70	15,261.58	46,388.29

ROA_{it} as a proxy for profitability, the leverage ratio LEV_{it}, sales growth $\Delta log(SALES)_{it}$ and the change in fixed assets $\Delta log(FA)_{it}$ as investment proxy.[67] On the country level, controls are GDP growth $GDPG_{kt}$, GDP per capita $GDPPC_{kt}$, an indicator of a country's credit market size $CREDITM_{kt}$, and dummy variables representing the La Porta et al. (1998) legal origin. I add a set of sectoral dummies to control for different intensities of state ownership in different sectors. For example, 22 percent of SOEs operate in the NACE2 category *D: Electricity, gas, steam and air conditioning supply* whereas only 0.77 percent of private firms do. A final control in the probit model is the respective dependent ETR variable from the outcome equation (4.5). The probit model also contains all non-binary variables as squared and cubic terms. Appendix Table 4.7.3 contains the probit results for all three ETR specifications.

I winsorize the three dependent variables of outcome equation (4.5), ETR_{it}^{PTP}, ETR_{it}^{OPP}, and ETR_{it}^{EBI} at the top and bottom one percent to make results less dependent on outliers.

[67] Table 4.7.4 contains definitions and sources of all variables.

The first, ETR_{it}^{PTP}, has a mean of 0.31, which is 13 percentage points higher than the mean of the EBITDA-based ETR_{it}^{EBI}. This is because a firm's EBITDA still contains amortization and interest, and is thus higher than pre-tax profit. ETR_{it}^{OPP} has a mean of 0.26 because operating profit lies between the other measures in a firm's profit cascade. Correlations in Appendix Table 4.7.2 follow accordingly: ETR_{it}^{PTP} and ETR_{it}^{OPP} correlate stronger (0.73) than ETR_{it}^{PTP} and ETR_{it}^{EBI} (0.54). All three ETR specifications correlate weakly negatively with the binary state ownership indicator SOE_i. The average ETR_{it}^{PTP} in the sample is higher than the average statutory tax rate $TAXR_{kt}$. Most likely this is because the sample contains a large share of companies operating in high-tax countries such as Germany and Italy.

The choice of covariates for the outcome equation (4.5) follows previous studies by Gupta and Newberry (1997) and Derashid and Zhang (2003). On the firm level, I include the ROA_{it} as a proxy for profitability. The sample's average ROA_{it} is ten percent. Its correlation with SOE_i is -0.09, which suggests that SOEs are less profitable. The second firm-level control is a company's leverage LEV_{it} because interest payments are tax-deductible in all sample countries and thus affect effective taxation. Next is asset tangibility $ATANG_{it}$ – a proxy for capital intensity and thus for the nature of a business. Its correlation with state ownership is 0.25, which is possibly due to the fixed-asset-intensive sectors in which SOEs operate. I use $log(SALES)_{it}$ as a firm size proxy. On the one hand, a bigger firm can be subject to greater scrutiny from the tax administration, on the other hand, it can spend more on tax planning strategies. Whether firm size positively or negatively affects effective taxation is therefore not entirely clear, and several studies do not find the indicator to be a significant predictor at all (Stickney and McGee, 1982; Gupta and Newberry, 1997). Here, $log(SALES)_{it}$ and the alternative size proxy $log(TA)_{it}$ correlate negatively with the dependent variables. The last firm-level variable is depreciation $log(DEPR)_{it}$. Depreciation is tax deductible and therefore reduces the outcome variables. Correlations from Appendix Table 4.7.2 point in this direction. On the sectoral level, I include the growth opportunities indicator $GROP_{skt}$ as in Huizinga et al. (2008). A key advantage of my data is its extension to non-listed firms, which has the side effect of impeding the use of market-to-book ratios that other studies employ. The last covariate is the country-level statutory tax rate. In my sample, corporate tax rates range from ten percent in Bulgaria to 36.1 percent in France. High corporate tax rates may not only increase the dependent variables but also encourage tax planning. In addition to these

controls, I use sectoral dummies, time dummies, and the group-fixed effects, which should absorb any time-constant unobserved heterogeneity on the country and group level.

Table 4.4.2: Country data

The table presents country-level summary statistics of the dependent variables ETR_{it}^{PTP}.

	State-Owned				Private			
Country	n	N	Mean	Median	n	N	Mean	Median
Austria	50	217	0.25	0.25	574	2,552	0.22	0.25
Belgium	7	34	0.27	0.29	390	1,685	0.31	0.32
Bulgaria	3	12	0.09	0.10	411	1,555	0.14	0.13
Czech Republic	202	858	0.21	0.19	3,459	14,851	0.21	0.19
Germany	451	1,983	0.28	0.28	2,916	11,761	0.26	0.28
Estonia	8	35	0.16	0.16	78	322	0.12	0.07
Finland	56	236	0.16	0.15	669	2,920	0.20	0.21
France	118	469	0.30	0.30	8,412	36,966	0.27	0.28
Croatia	28	121	0.25	0.22	973	4,557	0.24	0.21
Hungary	5	25	0.06	0.06	126	578	0.13	0.11
Italy	351	1,399	0.52	0.49	11,849	50,685	0.45	0.41
Latvia	6	26	0.21	0.17	62	236	0.19	0.16
Poland	170	753	0.26	0.22	414	1,851	0.22	0.20
Romania	4	18	0.26	0.21	169	715	0.21	0.17
Sweden	337	1,366	0.20	0.19	4,032	17,592	0.21	0.21
Slovakia	2	6	0.20	0.19	243	1,066	0.26	0.23
Slovenia	13	54	0.19	0.20	380	1,816	0.18	0.17
Total	1,811	7,612	0.25	0.20	35,180	151,786	0.31	0.29

Table 4.4.2 shows the country-specific distribution of SOEs and private firms and also contains country-wise descriptive statistics of the dependent variable ETR_{it}^{PTP}. The distribution of companies across ownership groups and country is not representative. For example, we observe only 118 cross-sectional SOE units in France, compared to 351 in Italy, even though SOEs are no less dominant in the French economy. It is nevertheless plausible to assume that country sample shares are free of selection bias as the dataset only contains information that each firm is legally obliged to publish. Moreover, cross-country variation is fully taken into account by exact country matching. Table 4.4.2 indicates that SOEs have a higher mean ETR_{it}^{PTP} than private firms in 10 of the 17 countries. In six countries, the opposite is the case, and in the Czech Republic, there is no apparent mean difference. This suggests that tax neutrality of ownership depends on the EU member state. In a majority of 10 countries, the mean and median ETR_{it}^{PTP} of SOEs are close, that is, they do not differ by more than 0.01. In this case, the density above and below the mean ETR value is similar. ETR distributions of private companies have a larger density for lower ETR levels in 10 countries (mean > median). This could point to a higher degree of tax-planning by private firms.

4.4.2 Basic result

The main specification of model (4.5) uses three nearest neighbors per SOE to construct the group-fixed effect. Table 4.4.3 presents the conditional results. It contains coefficient estimates for the variable of interest, SOE_i, and all time-varying controls. In addition to group effects, the model also contains sector and time effects, which I do not report separately. The estimates predict a higher dependent variable for SOEs irrespective of its specification. The effect of SOE_i on ETR_{it}^{PTP} (Column A) is 0.0160, which translates into a 1.6 percentage point higher ETR for SOEs. The impact of SOE_i on ETR_{it}^{OPP} is 0.0173 (Column B), and thus – at 1.73 percentage points – slightly stronger in magnitude. The coefficient of SOE_i using ETR_{it}^{EBI} as the dependent variable is only 0.0073 and the weakest (Column C). The effect is statistically significant at the one percent level in Columns A and B, and at the five percent level in Column C.

State ownership has a bigger impact on ETR_{it}^{PTP} and ETR_{it}^{OPP} compared to ETR_{it}^{EBI}, which suggests that amortization plays an important role in tax planning. To see why, recall that ETR_{it}^{PTP} is calculated using pre-tax profit, i.e., after depreciation and interest. Previous literature has related both balance sheet items with tax planning strategies (among many others, see Collins and Shackelford (1995) and Huizinga et al. (2008)). ETR_{it}^{EBI} derives from EBITDA, i.e., profit before interest and depreciation, and leaves less room for tax planning. Consequently, deviating coefficient intensities may point to different depreciation policies between SOEs and private firms.

This main result clearly rejects Hypotheses 1 and 2. Neither do commercial SOEs have similar ETRs on the EU level nor do they benefit from noticeable tax subsidies (which would lower ETRs). Instead, it supports Hypothesis 3. I calculate elasticities of ETR_{it}^{PTP} by ownership groups for different levels of the statutory tax rate $TAXR_{kt}$ to further examine differences between the two ownership groups.[68] Elasticities are almost identical and persistently inelastic for both ownership groups – which could suggest that private firms and SOEs respond to an increase in statutory tax rates with more tax planning. A further indication in this direction is that elasticities are not significant at the 25 percent $TAXR_{kt}$ quantile (a tax rate of 22 percent and elasticities of 0.79 for SOEs and 0.80 for private firms), but highly significant at the 75 percent quantile (a tax rate of 30.18 percent and an elasticity

[68]The statutory tax rate $TAXR_{kt}$ only affects the ETR_{it}^{PTP} significantly, which is why I restrict elasticity analysis to Column A.

Table 4.4.3: Basic result

The table presents group-fixed effects regressions based on $N_A = 12,868$, $N_B = 13,156$, and $N_C = 15,505$ observations, respectively; group-fixed effects are based on 3 nearest neighbors in 2010; matches are within country; standard errors are clustered at the firm level; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: ETR_{it}^{PTP}	C: ETR_{it}^{OPP}	B: ETR_{it}^{EBI}
	Coeff.	*Coeff.*	*Coeff.*
	(s.e.)	*(s.e.)*	*(s.e.)*
SOE_i	0.0160***	0.0173***	0.0073**
	(0.0045)	(0.0040)	(0.0030)
LEV_{it}	0.0167	-0.1479***	-0.0706***
	(0.0112)	(0.0097)	(0.0064)
$log(SALES)_{it}$	-0.0082***	-0.0078***	0.0181***
	(0.0024)	(0.0023)	(0.0016)
$ATANG_{it}$	-0.0196*	-0.0669***	-0.0401***
	(0.0104)	(0.0105)	(0.0071)
$log(DEPR)_{it}$	-0.0038*	0.0007	-0.0272***
	(0.0020)	(0.0019)	(0.0014)
ROA_{it}	-0.0028***	-0.0002	0.0022***
	(0.0003)	(0.0002)	(0.0002)
$GROP_{skt}$	-0.0237*	0.0326**	0.0124
	(0.0141)	(0.0151)	(0.0093)
$TAXR_{kt}$	0.8351***	0.1055	0.1262
	(0.1495)	(0.1335)	(0.0935)
Group effects	Yes	Yes	Yes
Sector effects	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes

of 0.79 for both ownership types). These findings are consistent with empirically established tax planning strategies such as increased debt usage under higher tax rates (see Feld et al. (2013) for a meta-study) and anecdotal evidence suggesting that SOEs engage in tax planning just as private firms do. The main conditional result does not unequivocally support the theory of SOE managers engaging in a quiet life without tax planning (because elasticities are almost identical). Instead, it supports the argument of governments forcing distributions via tax payments (Cui, 2015a) and of closer public scrutiny for SOE taxation (Dyreng et al., 2016). Especially the first seems relevant, given the substantial budgetary contributions some EU governments receive from their commercially active SOEs (European Commission, 2016). Nevertheless, it would be premature to discard the quiet life hypothesis based on this result alone.

The remaining control variables have the predicted effects. A higher debt share LEV_{it}

significantly reduces ETR-specifications with pre-interest profit measures (Columns B and C) and is not significant in Column A. The firm size proxy $log(SALES)_{it}$ has an adverse effect in Columns A and B and a positive effect in Column C. Nevertheless, results do suggest that larger firms pay lower ETRs. Asset tangibility $ATANG_{it}$ and depreciation $log(DEPR)_{it}$ have a negative impact in all specifications except for the insignificant $log(DEPR)_{it}$ coefficient in Column B. Both variables measure capital intensity, which previous studies associated with lower ETRs (Stickney and McGee, 1982; Gupta and Newberry, 1997; Derashid and Zhang, 2003).

The rest of this study will mainly deal with the question of how robust this main finding is. In addition to the analysis presented in Section 4.5, I conduct a broad set of checks. First, I use ETR-tax rate differences as dependent variables. Second, I match within sectors, use one, two, and five nearest neighbors, and change the propensity calipers. Third, I modify the probit model (4.4). Fourth, I include additional controls in the estimation of the outcome equation (4.5) and fifth; I vary the design of the dataset. The benchmark result is robust to all modifications. For more details on these tests, I refer to Appendix Section 4.7.1.

4.5 Sensitivity analysis

4.5.1 Long-term ETR measures

As companies pay taxes regularly, tax planning may not materialize in yearly tax data due to its dynamic nature (Dyreng et al., 2008). Additionally, business cycles can affect yearly tax rates and lead to incorrect coefficient interpretations for longer time periods. Dyreng et al. (2008) propose a "long-run cash effective tax rate" to cope with these issues. Unfortunately, Orbis does not allow to explicitly isolate cash taxes in the specific year from deferred or other taxes. Hence, I cannot reproduce their long-run measure directly. Nevertheless, I compute a similar indicator for each cross-sectional unit as

$$LTETR_i^{PTP} = \frac{\sum_{t=1}^{T} TAX_{it}}{\sum_{t=1}^{T} PTP_{it}}. \tag{4.6}$$

The long-term operating profit and EBITDA specifications, $LTETR_i^{OPP}$ and $LTETR_i^{EBI}$, follow analogously. I impose an additional restriction on the sample by only computing long-run ETRs for firms with at least six years of non-missing data. This ensures that results

capture long-term effective firm taxation but eliminates some countries from the sample. Table 4.5.1 contains country-level mean values for the long-run $LTETR_i^{PTP}$.

Table 4.5.1: Long-term descriptives

The table presents summary statistics and tests for differences in means of the dependent variable $LTETR_i^{PTP}$ by country; the table only contains countries with at least 50 SOE firm-years; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Country	**N**	**Mean** $SOE_i = 1$	**Mean** $SOE_i = 0$	**Difference**	**T-statistic**
Austria	4,722	0.23	0.23	0.0155	1.38
Czech Republic	12,515	0.20	0.20	-0.0054***	-3.37
Germany	19,940	0.27	0.27	0.0045	1.81
Finland	2,601	0.15	0.20	-0.0500***	-8.02
France	31,001	0.30	0.28	0.0151***	4.81
Croatia	4,649	0.23	0.21	0.0096***	2.38
Italy	40,006	0.46	0.40	0.0568***	15.18
Latvia	4,595	0.20	0.16	0.0402***	7.66
Poland	3,186	0.24	0.21	0.0308***	11.43
Sweden	16,376	0.18	0.19	-0.0162***	-7.74
Slovenia	1,802	0.19	0.18	0.0187**	2.83

Long-term rates of both ownership groups relate similarly to short-term ETRs (Table 4.4.2): A trend is that long-term mean values for both SOEs and private firms are lower than annual means. For SOEs, this is the case in ten of eleven countries. For private firms, this holds for eight countries. The last two columns of Table 4.5.1 contain the country-wise differences in long-term ETR means between SOEs and private firms and the t-statistic of a two-sided test. In six countries, the difference is statistically significant and positive, two have positive but insignificant differences, and in three it is significantly negative. Hence, country-level evidence points to somewhat higher long-term ETRs of SOEs, even though this is not the case in all countries.

Next, I estimate the model with pooled OLS by collapsing the panel control variables into time averages. The model also includes sector and country effects, the latter of which captures all country variables such as the statutory tax rate $TAXR_{kt}$.

The estimates are weaker compared to the yearly primary result from Table 4.4.3. The SOE_i coefficient indicates an increase of 1.11 percent for the $LTETR_i^{PTP}$ (Column A). The effect on the $LTETR_{it}^{OPP}$ is positive but not significant (Column B). The impact on the $LTETR_{it}^{EBI}$ is 0.44 percent and significant at the ten percent level (Column C). Summing up, Tables 4.5.1 and 4.5.2 show that the yearly results also hold for long-term dependent variables, even though results are less robust than the benchmark.

Table 4.5.2: Long-run ETRs

The table presents pooled OLS regressions based on $N_A = 19,772$, $N_B = 21,441$, and $N_C = 23,947$ observations, respectively; the dependent variables are defined as indicated in equation (4.6); superscript m indicates that controls are time-averaged; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: $LTETR_i^{PTP}$ *Coeff.* *(s.e.)*	C: $LTETR_i^{OPP}$ *Coeff.* *(s.e.)*	B: $LTETR_i^{EBI}$ *Coeff.* *(s.e.)*
SOE_i	0.0111***	0.0016	0.0044*
	(0.0035)	(0.0031)	(0.0023)
LEV_i^m	-0.0012***	0.0003***	0.0022***
	(0.0001)	(0.0001)	(0.0001)
$log(SALES)_i^m$	0.0117***	-0.1497***	-0.0972***
	(0.0041)	(0.0034)	(0.0026)
$ATANG_i^m$	-0.0114***	-0.0326***	-0.0403***
	(0.0041)	(0.0035)	(0.0026)
$log(DEPR)_i^m$	0.0021***	0.0008	0.0206***
	(0.0008)	(0.0007)	(0.0005)
ROA_i^m	-0.0276	0.0170	0.0220
	(0.0197)	(0.0177)	(0.0136)
$GROP_{sk}^m$	-0.0016**	0.0026***	-0.0220***
	(0.0007)	(0.0006)	(0.0005)
Country effects	Yes	Yes	Yes
Sector effects	Yes	Yes	Yes

4.5.2 Pooled OLS

In this Section, I re-estimate the main model using pooled ordinary least squares (POLS) as an alternative estimation strategy. This ignores the panel structure of the data but offers the advantage of a much bigger sample size. A substantial share of firm-years contains missing values already in controls of the state ownership specification from equation (4.4). The matching procedure further reduces the sample size in case insufficient matches exist for all SOEs. Whereas the basic estimates from Table 4.4.3 use between 12,000 and 16,000 firm-years, POLS allows to draw on 170,000 to 200,000 firm-years. Table 4.5.3 presents the results. All POLS estimates contain country, sector, and time-fixed effects, which I do not report separately.

Estimates of the state ownership coefficient are stronger in magnitude in Columns A-C of Table 4.5.3 than in Table 4.4.3. If a company is state-owned, the model predicts an increase of 2.55 percentage points for the ETR_{it}^{PTP}, a rise of 1.29 percentage points for the

Table 4.5.3: Pooled OLS

The table presents pooled OLS regressions based on $N_A = 172,574$, $N_B = 180,698$, and $N_C = 194,501$ observations, respectively; standard errors are clustered at the firm level; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: ETR_{it}^{PTP} *Coeff.* *(s.e.)*	B: ETR_{it}^{OPP} *Coeff.* *(s.e.)*	C: ETR_{it}^{EBI} *Coeff.* *(s.e.)*
SOE_i	0.0255***	0.0129***	0.0089***
	(0.0036)	(0.0030)	(0.0021)
LEV_{it}	0.0241***	-0.1495***	-0.0854***
	(0.0033)	(0.0027)	(0.0019)
$log(SALES)_{it}$	0.0008	0.0007	0.0221***
	(0.0006)	(0.0006)	(0.0004)
$ATANG_{it}$	-0.0122***	-0.0418***	-0.0387***
	(0.0034)	(0.0030)	(0.0021)
$log(DEPR)_{it}$	-0.0019***	0.0020***	-0.0241***
	(0.0005)	(0.0005)	(0.0004)
ROA_{it}	-0.0026***	-0.0004***	0.0018***
	(0.0001)	(0.0000)	(0.0000)
$GROP_{skt}$	-0.0135***	-0.0017	-0.0040
	(0.0040)	(0.0036)	(0.0026)
$TAXR_{kt}$	0.3908***	0.2614***	0.2094***
	(0.0351)	(0.0314)	(0.0220)
Country effects	Yes	Yes	Yes
Sector effects	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes

ETR_{it}^{OPP}, and an increase of 0.89 percentage points for the ETR_{it}^{EBI}. All SOE_i coefficients are significant at the one percent level. The stronger magnitude of effects could be due to the lack of group-fixed effects, which absorb cross-sectional variation in the primary results. Most control variables are similar to the benchmark results.

Elasticities of the $TAXR_{kt}$ variable are persistently inelastic and do not differ significantly across ownership groups. This supports the arguments from above of closer public scrutiny for SOEs and governments forcing distributions via tax payments. I conclude that the general results are robust to a different estimation technique applied to a very big sample.

4.5.3 Consolidated data

The next robustness test uses consolidated financial statements to estimate equation (4.5). The analysis allows to a certain extent to control for conforming tax planning strategies,

which affect both the numerator and the denominator of the ETR (Watrin and Thomsen, 2016). This is because the TAX_{it} and respective profit variables capture the behavior of the group as a whole. However, it is important to remember that EU member states levy taxes usually on single accounts.

I identify consolidated financial statement in Orbis with the consolidation codes C1 and C2. C1 refers to firms for which only consolidated data is available and C2 to firms with consolidated and unconsolidated data. I use both types and apply the same data management and estimation procedure as above.[69] The sample sizes are much smaller compared to the benchmark estimate from Table 4.4.3 but are still above 1,000 firm-years for all specifications. Table 4.5.4 presents the results.

The general pattern from the basic results does not change. The coefficients of SOE_i have a positive and significant impact on all specifications of the dependent variable. Ceteris paribus, SOE_i increases the ETR_{it}^{PTP} by 3.16 percentage points, the ETR_{it}^{OPP} by 3.12 percentage points and the ETR_{it}^{EBI} by 2.01 percentage points. These magnitudes are stronger compared to the benchmark result from Table 4.4.3.

One possible interpretation of this finding is that private firms use the consolidation process to reduce their tax liabilities, whereas SOEs do so to a lesser extent. Alternatively, there may be subsidiaries or affiliates in low-tax third countries, which the unconsolidated dataset does not contain. Such affiliates would reduce a group's overall ETR. I conclude that the basic result extends to consolidated financial statements of companies registered in EU countries.

4.5.4 Falsification tests

In this Section, I use a falsification test to assess if the predicted effect of state ownership could be random, i.e., a statistical coincidence without causal relation. In doing so, I follow other studies that have used similar falsification tests (De Simone, 2016; Goldbach et al., 2017). In a first step, I randomly assign state ownership $SOE_i = 1$ to the same number of

[69] A few minor differences exist: First, I use IFRS and local GAAP firms. IFRS is the mandatory reporting language for consolidated financial statements in the EU. Nevertheless, some firms chose voluntarily to publish accounts in local GAAP. Orbis sometimes only contains these accounts. Second, I use a probit specification of model (4.4) without the sales growth variable $\Delta log(SALES)_{it}$ due to the lack of convergence otherwise. Third, I use a caliper of $0.2 * SD^{probit}$ instead of $0.1 * SD^{probit}$. Results are robust for both calipers, but the sample size decreases significantly for the 0.1 caliper.

Table 4.5.4: Consolidated financial statements

The table presents group-fixed effects regressions based on $N_A = 1,393$, $N_B = 1,349$, and $N_C = 1,605$ observations, respectively; group-fixed effects are based on 3 nearest neighbors in 2010; matches are within country; standard errors are clustered at the firm level; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: ETR_{it}^{PTP}	B: ETR_{it}^{OPP}	C: ETR_{it}^{EBI}
	Coeff.	*Coeff.*	*Coeff.*
	(s.e.)	*(s.e.)*	*(s.e.)*
SOE_i	0.0316**	0.0312***	0.0201**
	(0.0137)	(0.0115)	(0.0079)
LEV_{it}	0.0531	-0.1565***	-0.0516***
	(0.0337)	(0.0336)	(0.0184)
$log(SALES)_{it}$	-0.0034	0.0075	0.0240***
	(0.0080)	(0.0073)	(0.0047)
$ATANG_{it}$	-0.0662*	-0.0531*	-0.0469**
	(0.0398)	(0.0288)	(0.0185)
$log(DEPR)_{it}$	0.0003	-0.0039	-0.0201***
	(0.0065)	(0.0063)	(0.0041)
ROA_{it}	-0.0038***	-0.0017*	0.0010*
	(0.0009)	(0.0010)	(0.0006)
$GROP_{skt}$	-0.0460	0.0478	-0.0141
	(0.0386)	(0.0381)	(0.0271)
$TAXR_{kt}$	0.6304*	-0.1425	-0.2383
	(0.3442)	(0.2522)	(0.1732)
Group effects	Yes	Yes	Yes
Sector effects	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes

cross-sectional private firms as there are SOEs in the sample.[70] Then, I repeat the estimation procedure from Section 4.3 using the "pseudo" SOEs 1000 times.

Figures 4.5.1 and 4.5.2 contain empirical cumulative distribution functions (CDFs) of the state ownership coefficients and the respective five percent confidence intervals (CIs) for all three ETR definitions ETR_{it}^{PTP}, ETR_{it}^{OPP} and ETR_{it}^{EBI}. The left panel of both figures contains the coefficient CDFs, the right panel the five percent CIs. The vertical lines represent the benchmark results from Table 4.4.3.

In Figure 4.5.1, I assign pseudo-SOE status across countries. Coefficient estimates locate evenly around zero – 50 percent of repetitions suggest a positive, and 50 percent a negative impact of pseudo-ownership on all three ETR definitions. The benchmark coefficients from

[70] As no algorithm is genuinely random, I use a pseudo-random seed based on system time to assign treatment status.

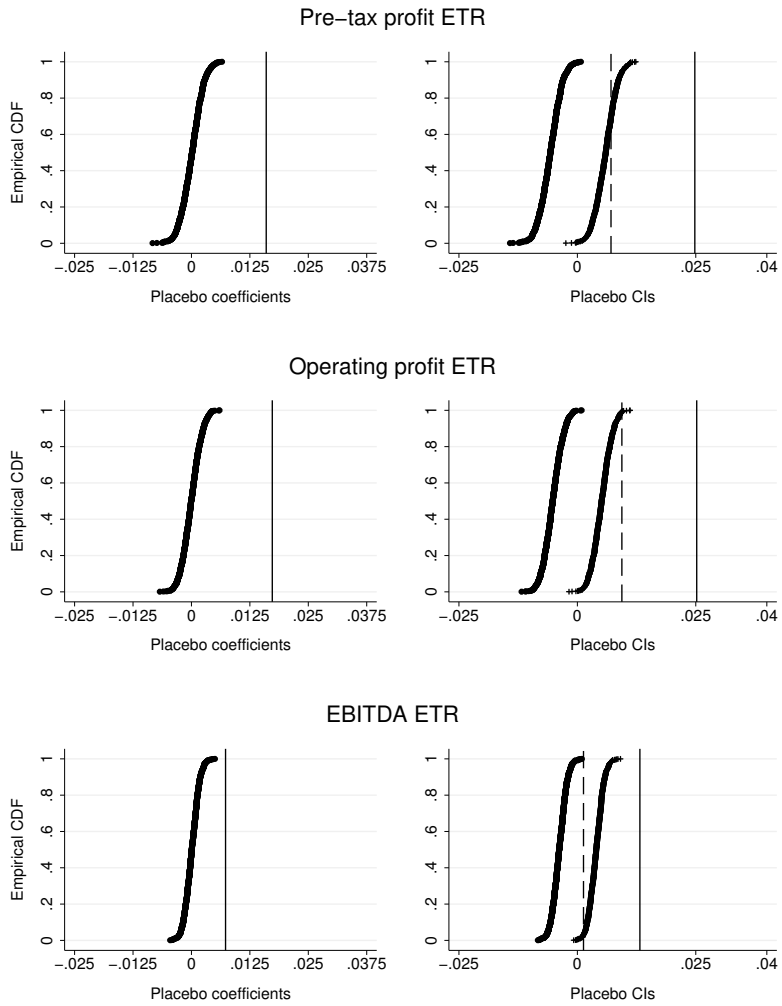


Figure 4.5.1: Cross-country pseudo-SOE assignment

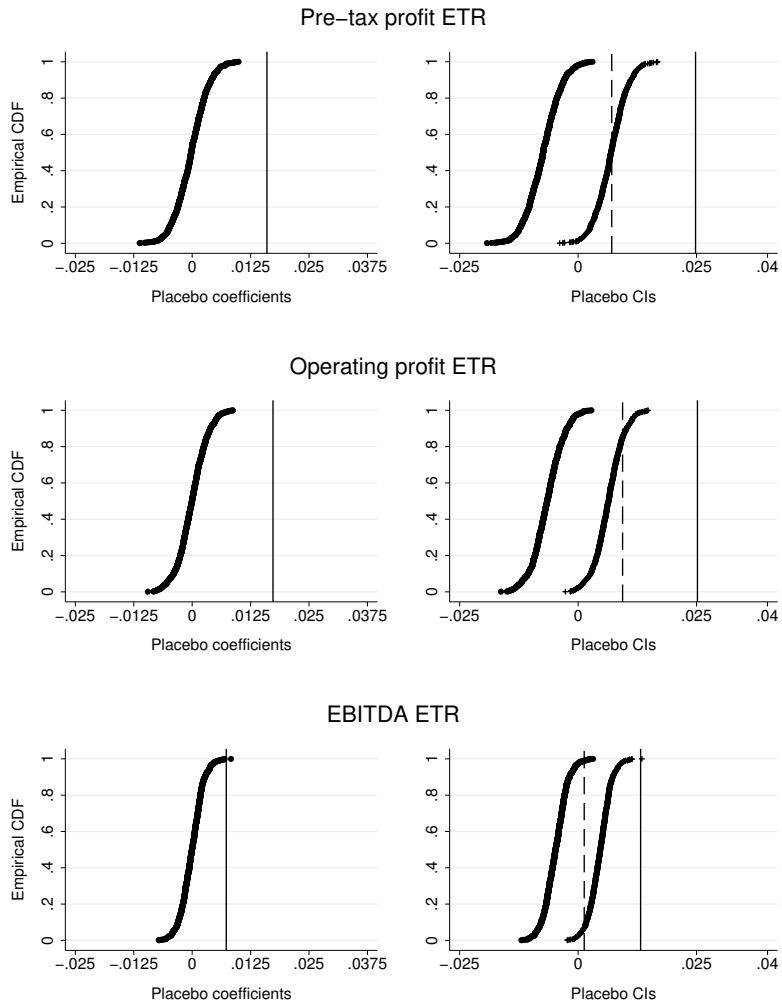


Figure 4.5.2: Within-country pseudo-SOE assignment

Table 4.4.3 are much stronger in magnitude than all estimates with random ownership assignment, suggesting that the effect is not statistical noise. The same holds for the five percent CIs (right panel of Figure 4.5.1): The original estimates produce a much stronger effect than any randomized estimate.

In Figure 4.5.2, I assign random state ownership to the same number of cross-sectional units within countries as there are SOEs in the sample, which leaves the country weights from the basic results in Table 4.4.3 unchanged. The additional constraint has no effect on the empirical CDFs for both the ETR_{it}^{PTP} and the ETR_{it}^{OPP}. In all cases, coefficients are close to zero, and with equal probability above or below zero. For the third dependent variable, ETR_{it}^{EBI}, one pseudo-estimate exists which predicts a stronger statistically robust effect. Recall that the ETR_{it}^{EBI} derives from EBITDA, and thus before the major tax-planning items depreciation and interest of the profit cascade. Therefore, the fact that random estimates are closer to the benchmark for the ETR_{it}^{EBI} suggests that differences in other definitions of the dependent variable relate to these items. I conclude that the effect of state ownership on the dependent variables depends on true ownership and is not a product of statistical noise.

4.6 Conclusion

This study examines the tax neutrality of commercial state ownership in the EU. Results suggest that SOEs in Europe pay, on average, higher effective tax rates than comparable privately owned firms. I attribute this finding mainly to the budgetary importance of commercial state ownership, which makes governments force distributions from their profitable companies via tax payments. A complementing explanation is higher levels of tax-planning in private firms.

The study uses financial statement data from 17 EU member countries and employs selection-on-observables propensity score matching to identify a suitable control group for each SOE. The control group consists of firms within a relatively strict caliper that also operate in the same country as the SOE. This approach controls for non-random selection into state ownership. In the outcome regressions, I additionally control for a vector of time-varying firm characteristics that have been used in previous studies as well as time and sector effects.

I conduct an extensive set of robustness tests. In particular, I analyze if the central finding extends to variations in model specification and parametrization, consolidated financial

statement data, long-term specifications of the effective tax rate, and different estimation techniques. I also verify if results are a mere statistical coincidence by conducting an extensive falsification test with pseudo-ownership assignment.

A central policy implication of my main finding is that tax neutrality in the EU's single market remains imperfect towards wide-spread state ownership, and the extent and direction of imperfection depends on the member state. Therefore, regulators should pay closer attention to tax neutrality topics that may not grab the headlines like tax planning of MNEs does but could distort competitive neutrality to a significant extent.

4.7 Appendix to Chapter 4

4.7.1 Additional sensitivity analyses

The benchmark result in Table 4.4.3 is robust to a large set of additional checks. Table 4.7.1 reports the coefficient estimates of these tests for the variable of interest, SOE_i. Each row represents a variation from the primary result, which the first column specifies in more detail. I divide additional sensitivity analyses into five subgroups, namely variations in the dependent variable specification, changes in the matching procedure, modifications in the probit model (4.4) and the outcome equation (4.5), and modifications in the dataset.

The first line in Table 4.7.1 defines the dependent variable as the difference between the respective ETR and a country's statutory tax rate. The SOE_i variable then measures differences between SOEs and private firms in the ability to undercut statutory taxes. A positive coefficient of the ownership variables indicates a lower ability to undercut statutory rates. Results unambiguously show that this is the case for SOEs.

The next six lines refer to variations in the matching procedure. The first modification presents results when group-fixed effects capture time-constant sector characteristics (instead of the benchmark country characteristics). The reasoning behind this test is the fact that sectoral characteristics may have a significant impact on taxes and ownership structure. Thus, a sector group-fixed effect targets the business activity of firms more precisely. Coefficients remain robust but are less significant. The next three rows contain estimates for five, two and one nearest neighbors (instead of three). All SOE_i coefficients remain significant at the one percent level. The single exception is the coefficient for one nearest neighbor and the ETR_{it}^{OPP} as the dependent variable, which is significant at the five percent level. Furthermore, I vary the caliper, which is the maximum distance a private firm can have in the linear probit index from a treated firm to qualify as a group-fixed effect control. For a looser caliper of 0.2 times the probit standard deviation, estimates increase in magnitude and remain significant. A very strict caliper of 0.01 times the probit standard deviation yields a positive effect only for the ETR_{it}^{PTP}. Note that a caliper of 0.01 times the probit standard deviation is much stricter than the 0.2 factor proposed by Austin (2011). Hence, the matching procedure discards a relatively large share of useful information.

The next test refers to a variation in the probit model (4.4). I exclude the dependent variable to align the specification more with Borisova et al. (2012). Results are stronger in

Table 4.7.1: Additional sensitivity tests

The table presents coefficient estimates for the variable of interest SOE_i; each row represents a modification from the benchmark result in Table 4.4.3; results of covariates are not reported but available upon request; standard errors are clustered at the firm level; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: ETR_{it}^{PTP} *Coeff.* *(s.e.)*	B: ETR_{it}^{OPP} *Coeff.* *(s.e.)*	C: ETR_{it}^{EBI} *Coeff.* *(s.e.)*
Dependent variable			
ETR - tax rate difference	0.0153***	0.0184***	0.0094***
	(0.0048)	(0.0043)	(0.0030)
Matching procedure			
2-digit NACE2 matching	0.0121*	0.0117**	0.0083**
	(0.0064)	(0.0051)	(0.0034)
5 Nearest neighbors	0.0176***	0.0169***	0.0084***
	(0.0047)	(0.0040)	(0.0030)
2 Nearest neighbors	0.0154***	0.0119***	0.0079***
	(0.0045)	(0.0042)	(0.0029)
1 Nearest neighbor	0.0156***	0.0088**	0.0101***
	(0.0048)	(0.0044)	(0.0030)
Caliper 0.2*probit-SD	0.0214***	0.0205***	0.0100***
	(0.0047)	(0.0042)	(0.0030)
Caliper 0.01*probit-SD	0.0144***	0.0065	0.0038
	(0.0051)	(0.0046)	(0.0033)
Variations in Model (4.4)			
Probit ex ETR_{it}	0.0306***	0.0225***	0.0060**
	(0.0051)	(0.0042)	(0.0029)
Variations in Model (4.5)			
Depreciation/sales	0.0160***	0.0167***	0.0018
	(0.0045)	(0.0040)	(0.0031)
Intangible Assets	0.0158***	0.0180***	0.0082***
	(0.0046)	(0.0041)	(0.0031)
Log(Total Assets)	0.0158***	0.0161***	0.0080***
	(0.0044)	(0.0040)	(0.0031)
Dataset specification			
Incl. financial firms	0.0200***	0.0171***	0.0113***
	(0.0045)	(0.0042)	(0.0029)
Incl. IFRS	0.0116***	0.0178***	0.0096***
	(0.0043)	(0.0039)	(0.0029)

magnitude and remain significant.

The next block deals with modifications of the outcome regression (4.5). First, I scale depreciation in a given year by a firm's sales and use this indicator instead of $log(DEPR)_{it}$. The reasoning is that the indicator now controls for relative depreciation in contrast to absolute depreciation. Except for the coefficient of ETR_{it}^{EBI} (Column C), the impact remains unchanged. Second, I include a proxy for intangible assets as an additional control. In doing so, I address another balance sheet item that previous literature associates with tax planning (Collins and Shackelford, 1995). Coefficient estimates of SOE_i do not change significantly. Third, I use $log(TA)_{it}$ instead of $log(SALES)_{it}$ as a size proxy. This increases the sample slightly and mitigates concerns that results depend on the choice of the size proxy. Results show that this is not the case.

The final set of variations deals with data management. First, I include financial firms (NACE2 category K: Financial and insurance activities) to verify if results depend on their exclusion. This is not the case. Second, I include all IFRS firms in the sample. The sample now extends to Spanish firms but is not necessarily homogenous concerning accounting techniques in other countries. The effect of SOE_i is robust to this modification.

Summing up, I conclude that the primary results do neither depend on the definition of the dependent variable, the matching procedure, the specification of the probit or outcome equations, nor on the design of the dataset.

4.7.2 Additional figures and tables

Figure 4.7.1: Average pre-tax profit ETR by country

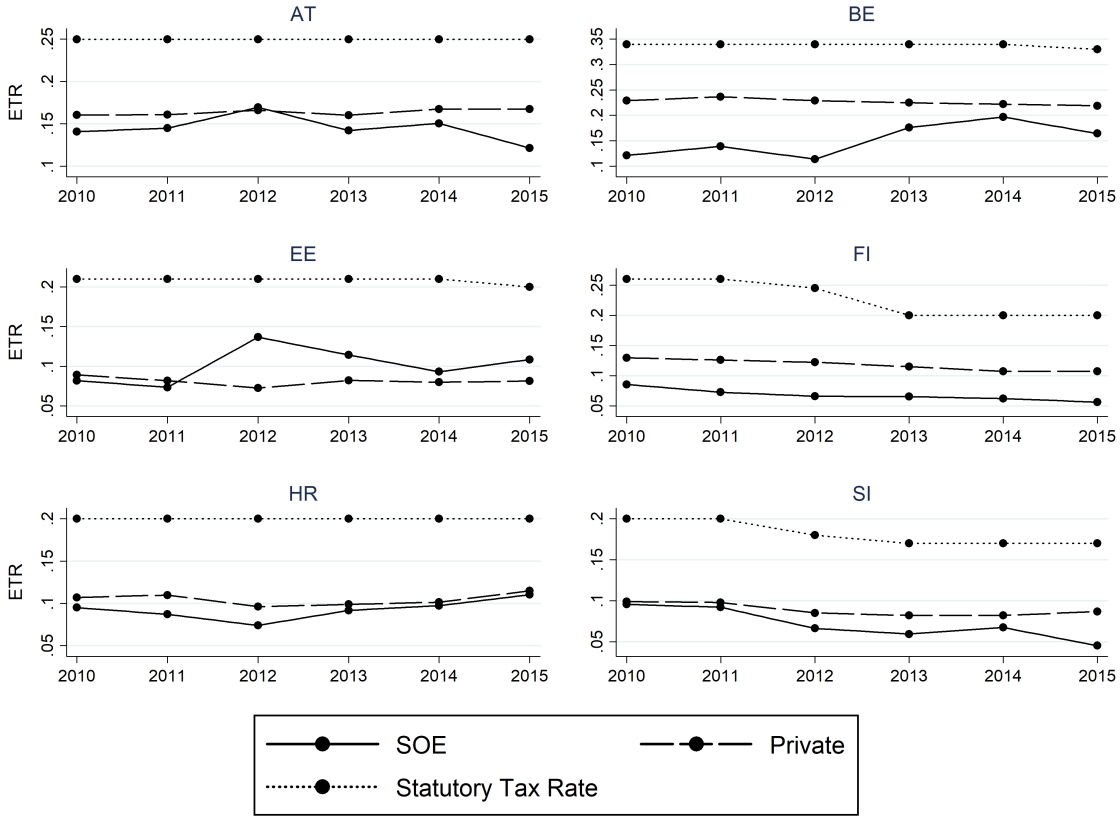


Table 4.7.2: Correlation matrix

The table presents correlations of the dependent variables ETR_{it}^{PTP}, ETR_{it}^{OPP}, ETR_{it}^{EBI}, state ownership SOE_i and the control variables used to estimate models (4.4) and (4.5); correlations are based on 159,398 observations.

	SOE_i	ETR_{it}^{PTP}	ETR_{it}^{OPP}	ETR_{it}^{EBI}	$log(TA)_{it}$	$log(SALES)_{it}$	ROA_{it}	LEV_{it}	$ATANG_{it}$	$log(DEPR)_{it}$	$\Delta log(FA)_{it}$	$\Delta log(SALES)_{it}$	$GROP_{skt}$	$TAXR_{kt}$	$CREDITM_{kt}$	$GDPG_{kt}$	$GDPPC_{kt}$
SOE_i	1.00																
ETR_{it}^{PTP}	-0.02	1.00															
ETR_{it}^{OPP}	-0.05	0.74	1.00														
ETR_{it}^{EBI}	-0.12	0.54	0.81	1.00													
$log(TA)_{it}$	0.20	-0.09	-0.06	-0.09	1.00												
$log(SALES)_{it}$	0.09	-0.12	-0.03	-0.00	0.79	1.00											
ROA_{it}	-0.09	-0.21	0.01	0.25	-0.12	0.07	1.00										
LEV_{it}	0.06	0.05	-0.24	-0.28	0.06	-0.11	-0.26	1.00									
$ATANG_{it}$	0.25	-0.07	-0.22	-0.40	0.18	-0.17	-0.20	0.38	1.00								
$log(DEPR)_{it}$	0.22	-0.09	-0.09	-0.30	0.79	0.68	-0.08	0.12	0.38	1.00							
$\Delta log(FA)_{it}$	-0.01	-0.00	0.00	0.03	0.00	-0.01	-0.00	0.03	0.04	-0.03	1.00						
$\Delta log(SALES)_{it}$	-0.02	0.01	0.01	0.04	-0.03	0.03	0.08	0.01	-0.02	-0.03	0.14	1.00					
$GROP_{skt}$	-0.03	0.00	0.01	0.02	0.02	0.06	0.05	0.01	-0.01	0.03	0.10	0.28	1.00				
$TAXR_{kt}$	-0.07	0.14	0.23	0.28	0.05	0.12	0.04	-0.06	-0.24	-0.03	-0.00	0.00	-0.01	1.00			
$CREDITM_{kt}$	-0.05	0.37	0.35	0.33	0.05	-0.03	-0.07	0.08	-0.09	-0.03	-0.01	0.02	-0.04	0.59	1.00		
$GDPG_{kt}$	0.08	-0.29	-0.24	-0.19	0.11	0.13	0.08	0.03	0.11	0.11	-0.02	-0.01	0.01	-0.08	-0.25	1.00	
$GDPPC_{kt}$	0.03	-0.03	0.01	0.06	0.23	0.20	0.02	0.08	0.01	0.14	-0.01	-0.00	0.02	0.49	0.60	0.23	1.00

Table 4.7.3: Probit regressions of the basic result

The table presents probit results for model (4.4); the predicted propensities of state ownership are used to construct the group-fixed effect of the outcome regressions in Table 4.4.3; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: ETR_{it}^{PTP} *Coeff.* *(s.e.)*	B: ETR_{it}^{OPP} *Coeff.* *(s.e.)*	B: ETR_{it}^{EBI} *Coeff.* *(s.e.)*
LEV_{it}	-1.7343***	-1.6379***	-1.3258***
	(0.4684)	(0.4567)	(0.4376)
LEV_{it}^2	4.1351***	5.0455***	3.5452**
	(1.5912)	(1.5211)	(1.4598)
LEV_{it}^3	-2.8080**	-3.9321***	-2.6152**
	(1.3286)	(1.2545)	(1.2076)
ROA_{it}	-0.0543***	-0.0193***	-0.0190***
	(0.0079)	(0.0035)	(0.0031)
ROA_{it}^2	0.0016***	0.0001	0.0000
	(0.0004)	(0.0001)	(0.0001)
ROA_{it}^3	-0.0000***	0.0000	0.0000**
	(0.0000)	(0.0000)	(0.0000)
$log(TA)_{it}$	-1.7738***	-1.7092***	-1.6952***
	(0.2612)	(0.2672)	(0.2618)
$log(TA)_{it}^2$	0.1792***	0.1734***	0.1730***

Table 4.7.3: Probit regressions of the basic result (continued)

	A: ETR_{it}^{PTP} *Coeff.* *(s.e.)*	B: ETR_{it}^{OPP} *Coeff.* *(s.e.)*	C: ETR_{it}^{EBI} *Coeff.* *(s.e.)*
	(0.0260)	(0.0269)	(0.0262)
$log(TA)_{it}^3$	-0.0053***	-0.0051***	-0.0051***
	(0.0008)	(0.0009)	(0.0008)
$\Delta log(FA)_{it}$	0.0754*	0.0815*	0.1271***
	(0.0443)	(0.0454)	(0.0481)
$\Delta log(FA)_{it}^2$	-0.0351	-0.0503*	-0.0987***
	(0.0244)	(0.0265)	(0.0304)
$\Delta log(FA)_{it}^3$	0.0034	0.0060	0.0114***
	(0.0034)	(0.0037)	(0.0040)
$\Delta log(SALES)_{it}$	-0.1083**	-0.1145**	-0.1428***
	(0.0472)	(0.0452)	(0.0434)
$\Delta log(SALES)_{it}^2$	-0.0241*	-0.0211	-0.0139
	(0.0134)	(0.0148)	(0.0134)
$\Delta log(SALES)_{it}^3$	0.0014	0.0028	0.0025
	(0.0023)	(0.0021)	(0.0020)
$CREDITM_{kt}$	-0.0022	-0.0400	-0.1174**
	(0.0435)	(0.0442)	(0.0472)
$CREDITM_{kt}^2$	0.0000	0.0004	0.0011**
	(0.0004)	(0.0004)	(0.0004)
$CREDITM_{kt}^3$	-0.0000	-0.0000	-0.0000**
	(0.0000)	(0.0000)	(0.0000)
$GDPG_{kt}$	0.2512***	0.2596***	0.2104***
	(0.0490)	(0.0500)	(0.0508)
$GDPG_{kt}^2$	0.0247***	0.0303***	0.0556***
	(0.0085)	(0.0086)	(0.0156)
$GDPG_{kt}^3$	-0.0088***	-0.0101***	-0.0142***
	(0.0022)	(0.0022)	(0.0030)
$GDPPC_{kt}$	-0.0003*	-0.0004**	-0.0006***
	(0.0002)	(0.0002)	(0.0002)
$GDPPC_{kt}^2$	0.0000	0.0000*	0.0000***
	(0.0000)	(0.0000)	(0.0000)
$GDPPC_{kt}^3$	-0.0000	-0.0000*	-0.0000***
	(0.0000)	(0.0000)	(0.0000)
LO: French	-0.2175	-0.5276	-1.6815**
	(0.6146)	(0.6240)	(0.6775)
LO German	0.1821	-0.2840	-1.9399**
	(0.8365)	(0.8520)	(0.9313)
LO Scandinavian	-0.1057	-0.5133	-1.9410**
	(0.7901)	(0.8036)	(0.8729)
NACE2 B	-0.1285	-0.0545	-0.0807
	(0.2510)	(0.2485)	(0.2434)
NACE2 C	-1.0416***	-1.0341***	-1.1003***
	(0.1458)	(0.1425)	(0.1382)
NACE2 D	1.7681***	1.7642***	1.6538***
	(0.1445)	(0.1412)	(0.1367)
NACE2 E	1.3660***	1.3650***	1.3465***
	(0.1473)	(0.1439)	(0.1391)
NACE2 F	-0.2305	-0.2455*	-0.2868**
	(0.1416)	(0.1387)	(0.1341)
NACE2 G	-0.8111***	-0.7911***	-0.7782***
	(0.1415)	(0.1382)	(0.1332)
NACE2 H	0.3167**	0.4031***	
	(0.1417)	(0.1386)	(0.1323)
NACE2 I	-0.3134*	-0.3455*	-0.4104**
	(0.1865)	(0.1837)	(0.1733)
NACE2 J	-0.0806	-0.1121	-0.0974
	(0.1537)	(0.1516)	(0.1448)
NACE2 L	0.2176	0.2463*	0.2051
	(0.1388)	(0.1351)	(0.1303)
NACE2 M	-0.0136	-0.0302	-0.0495
	(0.1445)	(0.1425)	(0.1373)
NACE2 N	0.1204	0.1588	0.1717

Table 4.7.3: Probit regressions of the basic result (continued)

	A: ETR_{it}^{PTP} *Coeff.* *(s.e.)*	B: ETR_{it}^{OPP} *Coeff.* *(s.e.)*	C: ETR_{it}^{EBI} *Coeff.* *(s.e.)*
	(0.1478)	(0.1439)	(0.1381)
NACE2 R	0.7640***	0.8271***	0.7981***
	(0.1805)	(0.1786)	(0.1676)
NACE2 S	0.2424	0.1907	0.2183
	(0.1997)	(0.1990)	(0.1882)
NACE2 U	1.3309	1.4906	0.9155
	(0.9113)	(0.9138)	(0.7608)
ETR_{it}^{PTP}	-1.2246**		
	(0.6063)		
ETR_{it}^{PTP2}	3.8465**		
	(1.7071)		
ETR_{it}^{PTP3}	-1.9253		
	(1.3122)		
ETR_{it}^{OPP}		-2.1783***	
		(0.6451)	
ETR_{it}^{OPP2}		5.6906***	
		(2.0412)	
ETR_{it}^{OPP3}		-2.4843	
		(1.7532)	
ETR_{it}^{EBI}		-4.5698***	
			(0.7556)
ETR_{it}^{EBI2}			17.3336***
			(3.2164)
ETR_{it}^{EBI3}			-15.3592***
			(3.6992)
constant	8.2735***	9.8044***	14.6239***
	(2.9562)	(3.0402)	(3.1906)

4.7.3 Variable definitions and data

Table 4.7.4: Variable definitions

Firm level	(Source: Orbis)
SOE_i	Binary indicator of state ownership.
TAX_{it}	Total financial statement tax payments of firm i in year t.
PTP_{it}	Pre-tax profit of firm i in year t.
OPP_{it}	Operating profit of firm i in year t.
EBI_{it}	Earnings before interest, tax, depreciation and amortization (EBITDA) of firm i in year t.
ETR_{it}^{PTP}	Pre-tax profit effective tax rate (TAX_{it}/PTP_{it}).
ETR_{it}^{OPP}	Operating profit effective tax rate (TAX_{it}/OPP_{it}).
ETR_{it}^{EBI}	EBITDA effective tax rate (TAX_{it}/EBI_{it}).
$log(TA)_{it}$	log(total assets) of firm i in year t.
$log(SALES)_{it}$	log(turnover) of firm i in year t.
ROA_{it}	Return on assets of firm i in year t.
LEV_{it}	Total debt / total assets of firm i in year t.
$ATANG_{it}$	Asset tangibility: fixed assets / total assets of firm i in year t.
$log(DEPR)_{it}$	log(depreciation and amortization) of firm i in year t.
$\Delta log(FA)_{it}$	Investment proxy: $log(\text{fixed assets})_{it} - log(\text{fixed assets})_{i,t-1}$.
$\Delta log(SALES)_{it}$	Sales growth: $log(\text{turnover})_{it} - log(\text{turnover})_{i,t-1}$.
Country level	
$TAXR_{kt}$	Statutory tax rate of country k in year t. (Source: Paying Taxes, PWC).
$CREDITM_{kt}$	Domestic credit provided by banking sector in country k and year t as percentage of GDP (Source: Worldbank).
$GDPG_{kt}$	Annual GDP growth in percent in country k and year t (Source: Worldbank).
$GDPPC_{kt}$	GDP per capita in country k and year t, PPP at constant 2011 international USD (Source: Worldbank).
Legal origin dummies	Legal origin dummy variables of country k based on La Porta et al. (1997).
Sector level	
Sector dummies	Sector dummies are based on NACE2 categories.
$GROP_{skt}$	Growth opportunities are defined as in Huizinga et al. (2008): The growth rate median of subsidiary sales in a subsidiary's industry s, country k, and year t.

- Ownership restriction on SOE part:

  - Historical ownership data base from Orbis: Label "public authority, state, government".
  - 100 percent stand-alone SOE or 100 percent group subsidiary where the group is classified by Orbis as "SOE".

- Ownership restriction on private firms:
  - Historical ownership data base from Orbis: Any other label.
  - 100 percent private stand-alone firms and subsidiaries.

- I consider only *#conscode* "U1" and "U2" subsidiaries, i.e., unconsolidated financial statements. Robustness is performed with "C1" and "C2" in Section 4.5.

- I use only data from EU countries because commercially active SOEs and private firms should receive the same tax treatment.

- A cross-sectional unit needs at least two firm-years in $[2009, 2015]$. The average – at five firm-years – is much higher.

- Only local GAAP firms. Robustness including IFRS firms in the Appendix Section 4.7.

- $\#Taxation > 0$

- $\text{ETR}_{it} \in]0; 1[$. Top and bottom 1 percent are winsorized. This holds for all ETR specifications.

- $\text{TAN}_{it} \in [0; 1]$.

- $\text{LEV}_{it} \in [0; 1[$.

5 State ownership, company risk, and societal value

5.1 Introduction

Do SOEs receive beneficial treatment compared to private competitors? A recent survey by PricewaterhouseCoopers (PWC, 2015) finds that 83 percent of private-sector CEOs believe that state ownership distorts competition, and 67 percent feel that government ownership affects industry regulation and its enforcement. Both responses suggest that state ownership influences the way firms operate and the way they are treated, possibly yielding them a competitive advantage. For instance, empirical observations indicate that SOEs benefit from facilitated access to debt financing (Brandt and Li, 2003; Li et al., 2009; OECD, 2012, 2014a; Chen et al., 2016). Implicit state guarantees on SOE debt are the most common explanation for this phenomenon: Investors assume that SOE debt is less risky. Other studies have pointed to political agendas as a key determinant of SOE financing costs (Brandt and Li, 2003; Zhang and Zhang, 2016). While these factors indeed seem relevant, our results – based on field and experimental data – show that societal bias in the general population towards state enterprise could be another important cause.

In the PwC survey, 37 percent of private sector CEOs agreed that SOEs are beneficial to societal value in some sectors of the economy. Such value may not lie in financial results for the owning citizens alone: Previous literature has shown that SOEs are less profitable and suffer from worse corporate governance than private firms (Dewenter and Malatesta, 2001; Megginson and Netter, 2001; Estrin et al., 2009; Borisova et al., 2012). Instead, consider the argument that SOEs exist to combat market imperfections (Atkinson and Stiglitz, 2015). If this is the case, then SOEs offer some form of less tangible return that goes beyond their financial results in the sense that they internalize market failure. This additional return benefits every constituent. Hence, the community that owns the firm is not only compensated with the dividend of the SOE but also with some form of benefit that is distributed among all citizens.

This paper first analyzes financial data of European companies and finds that SOEs benefit from implicit debt guarantees. However, – assuming rational investors – our regression

analysis points to additional factors influencing financing costs of SOEs in particular. Therefore, we experiment in a second step to evaluate if people are willing to forgo private returns in exchange for a societal benefit. Results indicate that people invest money in a company that is inefficient but yields a societal return even if investing in an alternative option is efficient. We argue that the "societal benefits" assumption of state ownership could be a driving force not only in determining financing costs of SOEs but also in explaining recent increases in equity ownership by governments and tolerance towards persistently worse SOE book performance. Our findings seem especially relevant in countries with large state-bank sectors, where this kind of bias may significantly affect lending policies.

We start by examining the empirical average cost of debt of SOEs and private firms using a large firm-level panel dataset (Orbis) in the years 2008-2015. In the EU, the regulatory framework of competitive neutrality bans preferential treatment (such as explicit debt guarantees) based on specific ownership types for commercial enterprises.[71] As a result, SOEs obtain debt uniquely in the commercial marketplace in OECD and EU countries (OECD, 2014a). Another study on competitive neutrality within the OECD (2012) argues that member countries formally apply the same bankruptcy framework to commercial SOEs than to private firms. Non-performing loans or even defaults of SOEs are a real possibility, which banks should take into account. Nevertheless, it may be difficult for the government to convince market actors, that it does not provide an *implicit* guarantee on the debt (or against default) of a specific SOE (OECD, 2016). Even if lenders assume implicit guarantees but behave otherwise rationally, they should attribute a positive (but lower) default risk to SOEs and adjust loans accordingly. A proxy for default risk is firm profitability (Altman, 1968). Thus, a worse corporate performance should, ceteris paribus, increase individual company risk and therefore a firm's cost of external finance. Our field data analysis investigates this relation for both ownership types.

A key advantage of our data is that we have access to financial statements of non-listed firms. Most other studies focusing on ownership research use data of publicly listed firms. Hence, our data include companies such as municipal property developers and local energy companies that are visible in the local public sphere and to local decision makers. A further advantage is the considerable amount of available historical ownership data, which we observe

[71] We discuss a few exceptions in Section 5.2.1. Generally, Article 107 of the Treaty on the Functioning of the EU prohibits state aid for commercially active SOEs because it could undermine the functioning of the single market. We elaborate further on state aid and access to finance in 5.2.1.

on a yearly basis. This enables us to construct a much bigger sample of firms with verified ownership information than in most previous studies.

State ownership is not random. Therefore, merely estimating a model that treats state ownership as exogenous would lead to biased estimates. This is because some of our control variables most likely correlate with selection into state ownership and not controlling for it would bias their coefficients. To address potential bias, we consider a selection model as the appropriate estimation strategy. It treats state and private ownership as two separate lending regimes and controls for selection into the former. Specifically, we employ the Semykina and Wooldridge (2010) selection approach, which has the additional advantage that it enables us to address unobserved heterogeneity, potentially endogenous explanatory variables, and selection within a simple, coherent framework.

We find that SOEs pay less for their debt than private firms and risk proxies are relevant determinants of debt costs only for the latter. Our basic specification suggests that average interest rates of SOEs across all debt contracts are between 1.1 percentage points (using a two-sided test) and 1.2 percentage points (using regression analysis) lower than those of private companies. Whereas an increase in operating profitability of 1 percentage point decreases interest rates of private companies by 1.36 basis points, firm profitability is not a relevant determinant for SOE interest rates.[72] This finding is very robust, even under situations of severe sovereign financial distress. This result indicates that investors attribute different levels of risk to the two ownership types. However, we believe that implicit state warranties alone are an insufficient explanation for this finding because we would have expected a significant but weaker effect for SOEs compared to private firms.

An established alternative explanation could be political lending, especially since state-owned banks have become increasingly important in European credit markets (Monnet et al., 2014). There is evidence that state lenders act according to political agendas, even if this behavior leads to lower returns or even losses (Sapienza, 2004; Dinc, 2005; Khwaja and Mian, 2005). However, such action need not necessarily be restricted to state banks nor caused by political opportunism: Many studies show that investors are willing to accept a sub-optimal financial performance to pursue social or ethical objectives (see Renneboog et al. (2008) for a survey). Goss and Roberts (2011) show that socially responsible US companies pay, on average, less for their debt. Hence, banks may favor a particular group of companies because

[72] We derive our profitability proxy before financial expenses and thus avoid reverse causality.

of ideological preferences (Brandt and Li, 2003). In our setting, it seems plausible that local banks (often state-owned) feel a special attachment to local community-owned companies, which induces a choice to forgo higher profits by catering to preference voluntarily. We test this hypothesis with an experimental investment task. Participants can choose an investment with a positive externality (mimicking an SOE), even though financial returns are lower than an alternative investment option (emulating a private company). Results show that participants allocate around one-third of their funds to the "SOE" even though this was inefficient. As in our field data analysis, risk considerations seem relevant but not exclusive explanatory factors.

The main result of our study is that SOEs pay less for their debt and risk considerations have a relevant, but not exclusive influence on their financing costs. Hence, this study supports prior research that has shown how government ownership affects the market perceptions of firm value and risk (Borisova et al., 2015; Beuselinck et al., 2017). We add to the literature by demonstrating that societal value considerations may impact their cost of finance. Our study complements previous empirical research relying on political arguments to explain less expensive SOE financing (Sapienza, 2004; Dinc, 2005).

This study proceeds as follows: Section 5.2 defines our outcome variable, the risk proxy, our definition of state ownership, and the institutional background of our data. Section 5.3 contains the econometric approach, the data management for implementation in Orbis as well as descriptive statistics. Section 5.4 presents our basic result. Section 5.5 adds our main robustness checks. Section 5.6 presents the setup and the results of our experimental analysis and Section 5.7 concludes.

5.2 Definitions, hypothesis development and institutional background

5.2.1 Outcome, profitability and state ownership

We start with analyzing firm-level data from Orbis to evaluate the determinants of the cost of debt for SOEs and private firms. In particular, we want to check if both ownership types differ concerning the effect of company risk on the cost of debt. We make use of the general notion that more profitable firms should, on average, pay less for their debt because they

face lower probabilities of bankruptcy (Altman, 1968).[73] Our dependent variable is firm i's average interest rate in the year t:

$$AVIN_{it} = \frac{INTE_{it}}{LTDB_{it} + STDB_{it}}, \tag{5.1}$$

where $INTE_{it}$ is the total interest paid of firm i in year t, $LTDB_{it}$ is total long-term debt (of maturity above one year) and $STDB_{it}$ is short-term debt (of maturity below one year).[74]

Profitability should affect a firm's cost of finance because it is a proxy for the quality of management and the underlying firm-level risk. However, the literature identifies no single best way to measure a firm's profitability. Our main proxy is a firm's return on total assets $OROA_{it}$, calculated as operating profit divided by total assets. In this we follow Altman (1968) and Goss and Roberts (2011). Operating profit is defined as all operating revenues minus all operating expenses. We use $OROA_{it}$ as our main risk proxy because it is free of reverse causality but estimate our model using the pre-tax return on assets as a robustness check.

Throughout this study, we expect a differential impact of profitability on a firm's $AVIN_{it}$, depending on ownership. To be precise, we expect a negative impact of ROA_{it} on $AVIN_{it}$ for private firms because higher profitability decreases the risk of financial distress. We do not expect firm risk to affect the financing costs of SOEs because decision makers (1) assume implicit guarantees, (2) cater for political favors, and (3) are biased to lend to state firms because they assume societal benefits. We summarize this in Hypothesis 1:

Hypothesis 1 *Profitability is an essential determinant of the cost of debt of private firms but is no relevant explanatory variable for the cost of debt of SOEs.*

A firm is included in our main regression if we have data for it in at least five consecutive non-missing years.[75] To determine whether a firm is state-owned or privately owned, we rely on the historical ownership database that forms part of Orbis. It allows us to identify wholly

[73] We focus on the impact of profitability on the cost of assumed target leverage. Hence, we are not interested in the effect of profitability on firms' debt-to-equity ratios, which is ambiguous in the corporate finance literature (Frank and Goyal, 2009).

[74] Appendix Table 5.8.5 contains descriptions and sources of all variables used in this study. Appendix Section 5.8.3 lists all data restrictions.

[75] Our results are robust to a decrease and increase in panel balance. Results are reported in Appendix Tables 5.8.2 and 5.8.3.

owned subsidiaries or stand-alone firms which carry the ultimate ownership label "public authority, state, government". We introduce an ownership indicator $SOE_i = 1$ if a firm falls into this category. Orbis allows us to distinguish privately owned firms by additional ownership groups. For example, we can determine if a firm is family-owned or in the hands of a private equity fund. Numerous studies conclude that these ownership types affect firms' performance and the cost of debt (Anderson and Reeb, 2003; Anderson et al., 2003; Kaplan and Stromberg, 2009). We are interested in singling out the effect of state ownership on a neutral benchmark. Therefore, we limit our private firm sample to the most common and neutral ultimate ownership type "industrial company". Private firms in our sample are thus either stand-alone industrial companies or 100-percent subsidiaries of such firms and carry the ownership label $SOE_i = 0$. These ownership definitions allow us to generate a sample of 6,868 SOE firm-years and 94,345 private firm-years.

5.2.2 Institutional background and our data

The validity of our field-data analysis relies on two crucial institutional assumptions: The first is that only implicit and imperfect guarantees for SOE debt exist. The second is that SOEs must be subject to similar rules and financing conditions than private firms.

According to an OECD (2014a) survey on SOE financing, only the governments of Germany, Poland, and Slovenia offer explicit debt guarantees to some SOEs, but such warranties are either strictly limited to non-commercial objectives or apply to private firms as well. In all other respondent countries, explicit guarantees on SOE debt are non-existent. Within the EU, explicit guarantees to commercial ventures constitute state aid, which is prohibited. For instance, Germany was obliged to abolish explicit guarantees for its savings banks in 2001 (Gropp et al., 2014). From this, we conclude that explicit guarantees are unlikely to be a significant issue in our data.

A study on competitive neutrality within the OECD (2012) finds that member states subject their SOEs to the same regulatory framework, i.e., no significant exemptions from bankruptcy law exist. An exception is state-owned banks such as the German "Kreditanstalt für Wiederaufbau" or the French "Caisse de Dépôts et Consignations". We can easily take account of state banks by excluding all financial and public administration firms from our sample. We also exclude all sectors connected with the passenger railway industry because national railway monopolies have received interest-free loans in some member countries – which

is similar to a full guarantee.[76] We further exclude firms incorporated as non-commercial enterprises such as the German *gemeinnützige Gesellschaft mit begrenzter Haftung (gGmbH)* and their European equivalents. Summing up, we conclude that debt guarantees in our data are imperfect and implicit.

Our focus on EU data provides an important safeguard to ensure similar rules and financing conditions for SOEs and private firms because any structural advantage would fall foul on EU state aid legislation. A few pitfalls, however, remain: SOEs can get state support for activities with a non-trivial public policy obligation. A primary example is again state-owned banks and the railway sector, which we exclude from our sample. According to the OECD (2014a), SOEs in member countries access debt almost exclusively on the marketplace, whereas compensation for public policy obligations takes the form of equity injections or direct transfers in most member states. In some member states, notably the Czech Republic, Finland and Slovenia, state aid is non-existent OECD (2014a). Hence, our dependent variable $AVIN_{it}$ should be mostly unaffected by direct state support. Nevertheless, we exclude all NACE2 categories with no private firm activity from the estimation sample as an additional safeguard.

Financing conditions may also depend on the presence of state-owned banks, which have increasingly penetrated commercial debt markets (Monnet et al., 2014). In Germany, which is the country with the most observations in our sample, state-owned development banks play an active role in lending to SOEs, but also to private companies.[77] In others, such as Sweden, SOEs are not allowed to borrow from government institutions at all (OECD, 2014a). The fact that state ownership of banks may influence credit decisions is well-documented (Sapienza, 2004; Dinc, 2005; Khwaja and Mian, 2005). Politicized lending does, however, not contradict our results because it could – on some occasions – be tantamount to social lending. We address the public service obligation issue by estimating our model using only sectors with a low public service propensity and both SOEs and private firms in Section 5.8.1. Summing up, we conclude that institutional lending conditions in our data are similar across ownership types.

[76] In total, we exclude from all our regressions the NACE2-categories K ("Finance and Insurance"), O ("Public Administration and Defence"), and the 4-digit NACE2 sectors 4212 ("Construction of railways and underground railways"), 4910 ("Passenger rail transport, interurban"), 4931 "Urban and suburban passenger land transport", and 5221 ("Service activities incidental to land transportation").

[77] Our results are robust to the exclusion of all German companies.

Table 5.2.1 shows the distribution of SOEs and private firms across countries. Unfortunately, Orbis contains missing values in many control variables, which reduces the sample size considerably. For example, the relatively low share of SOEs in France is not representative of the French economy.[78] The other big continental European economies are each represented with a considerable share for both ownership groups. In total, our sample covers 15 EU countries. Despite the high number of missing values, two key advantages distinguish our field

Table 5.2.1: Country data

Table 5.2.1 presents the ownership distributions for our estimation sample across countries.

Country	$SOE_i = 1$**: N**	$SOE_i = 1$**: %**	$SOE_i = 0$**: N**	$SOE_i = 0$**: %**	**Total: N**	**Total: %**
Austria	56	0.82	1,184	1.25	1,240	1.23
Belgium	7	0.10	2,741	2.91	2,748	2.71
Czech Republic	333	4.85	2,516	2.67	2,849	2.81
Germany	2,619	38.13	7,565	8.02	10,184	10.06
Spain	909	13.24	20,047	21.25	20,956	20.70
Finland	371	5.40	1,811	1.92	2,182	2.16
France	5	0.07	27,720	29.38	27,725	27.39
Italy	867	12.62	13,901	14.73	14,768	14.59
Latvia	10	0.15	28	0.03	38	0.04
Poland	1,148	16.72	1,486	1.57	2,634	2.60
Portugal	36	0.52	1,444	1.53	1,480	1.46
Romania	5	0.07	207	0.22	212	0.21
Sweden	470	6.84	12,447	13.19	12,917	12.76
Slowenia	22	0.32	932	0.99	954	0.94
Slovakia	10	0.15	324	0.34	334	0.33
Total	6,868	100.00	94,353	100.00	101,221	100.00

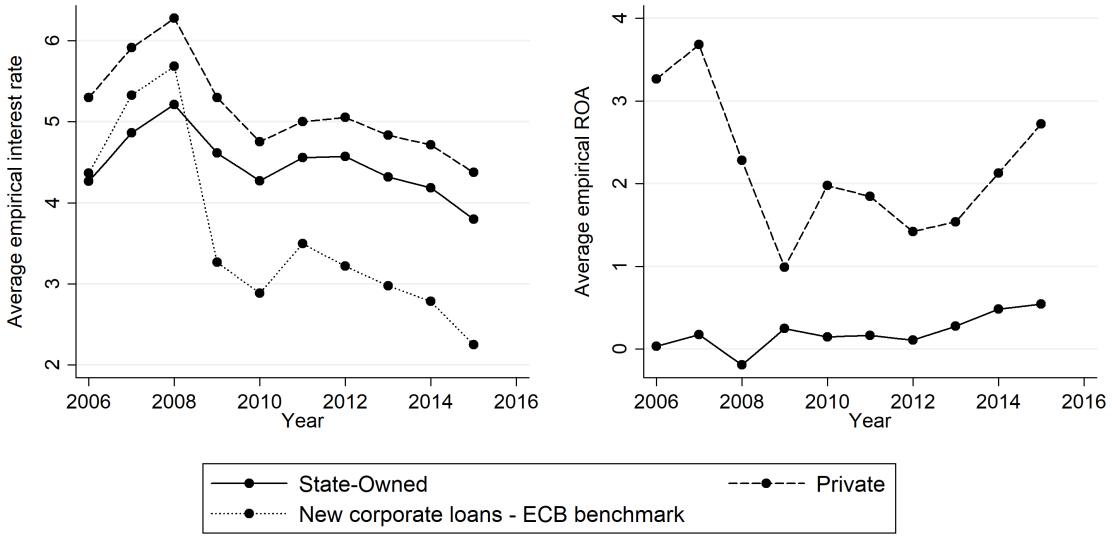
data analysis from many other studies focusing on ownership structure: First, Orbis collects ownership information from official sources, which makes our study less dependent on manual ownership research. Second, our sample size is much larger than in most other studies using manually collected ownership information.

Figure 5.2.1 plots our outcome variable $AVIN_{it}$ and our main variable of interest $OROA_{it}$ over the sample period from 2006-2015. It shows that SOEs pay lower average interest rates in each year (left panel). The average interest rate curves of the two ownership types are very parallel and indicate a persistent difference of around one percentage point between SOEs and private firms in the first three years, which declines to around 0.5 percentage points afterward. The initial increase in average interest rates coincides with tight credit and uncertainty at the height of the financial crisis in 2008. The fall in subsequent years points to improved access to finance, likely caused by better economic conditions in general and expansionary

[78] We assume sample selection in our data set is uncorrelated with our outcome variable and error term since we only use information that is publicly available. More likely, missing values arise because of differences in data-collecting processes of the dataset provider Bureau van Dijk.

Figure 5.2.1: Average interest and operating return on assets

Figure 5.2.1 plots the empirical average interest rate $AVIN_{it}$ and average return on assets ROA_{it} over our sample period 2006-2015.



ECB monetary policy. The third curve in the left panel represents the ECB's benchmark on new corporate loans. It follows a similar pattern than the average interest curves but declines faster in later years. We explain the increasing difference between the ECB benchmark and our indicators with legacy debt. Besides, most companies in our sample are relatively small and probably borrow at higher interest rates than the average firm. The right panel of Figure 5.2.1 shows that SOEs also have lower profitability $OROA_{it}$ than private firms in our sample period. The private firms again exhibit a behavior that is consistent with the financial crisis and weak economic recovery in the EU: Profitability decreases sharply during the crisis years 2008 and 2009 and only recovers from 2014, i.e., after the peak of the Eurozone crisis. SOE profitability is showing a weak but persistent trend upwards, which could reflect ongoing efforts to improve the performance of SOEs across Europe.

5.3 Empirical approach

5.3.1 Econometric methodology

State ownership is not random. Therefore, merely estimating a model that treats state ownership as exogenous would lead to biased estimates. Numerous studies have used matching

methods in the state ownership context (Borisova et al., 2012; Beuselinck et al., 2017). While matching methods offer many advantages, we are particularly interested in coefficient differences across two ownership groups. Under implicit guarantees alone, we would expect a smaller effect of our risk proxy on the cost of SOE debt. We can also not exclude the possibility that other control variables differ in their impact on SOEs and private companies. Besides, our experimental hypothesis states that the general public *perceives* state ownership differently. For these reasons, we consider a selection model as the appropriate estimation strategy, which treats state and private ownership as two separate lending regimes and controls for selection into the former.

Specifically, we employ the endogenous selection model proposed by Semykina and Wooldridge (2010). We assume that two regimes $R^1 \ \forall \ SOE_i = 1$ and $R^2 \ \forall \ SOE_i = 0$ exist and determine the average cost of debt $AVIN_{it}$. Selection into one regime or the other is not random because the government (or the general public) face different social, political and economic incentives to own a specific company or not. Our basic outcome model takes the form:

$$AVIN_{it}^{R} = \boldsymbol{x}_{it}\boldsymbol{\beta}^{R} + c_{i1} + u_{it1}, \tag{5.2}$$

where R indicates whether we are in regime $SOE_i = 1$ or regime $SOE_i = 0$. The vector $\boldsymbol{x}_{it}$ denotes a $1 \times K$ vector of time-varying explanatory variables and $\boldsymbol{\beta}^{R}$ denotes differential coefficient estimates depending on the regime R. The unobserved effect c_{i1} is time-constant and u_{it1} is an idiosyncratic error term. The index 1 denotes that the element belongs to our outcome equation (5.2). Potentially, $E\left(u_{it1}|\boldsymbol{x}_i, c_{i1}\right) \neq 0$ for some element of $\boldsymbol{x}_{it}$ because of endogeneity. In our data set, this may be the case for firm leverage, which affects $AVIN_{it}^{R}$ but could also be affected by it.[79] Additionally, whether firm i is state-owned or privately owned is not random, but depends itself on many factors, which potentially cause selection bias. Estimates of (5.2) that do not take into account unobserved heterogeneity, potential endogeneity and selection could be biased. The Semykina and Wooldridge (2010) approach allows us to address all three issues within a coherent modeling framework.

We start with modeling the selection into state ownership with the following probit equa-

[79] We deal with this possibility in Section 5.8.1.

tion:

$$P\left(s_{it}=1|\boldsymbol{z}_i\right)=\Phi\left(\boldsymbol{z}_{it}\boldsymbol{\delta}_{t2}+c_{i2}\right)=\Phi\left(\boldsymbol{z}_{it}\boldsymbol{\delta}_{t2}+\bar{\boldsymbol{z}}_i\boldsymbol{\xi}_{t2}\right). \tag{5.3}$$

The index 2 denotes that an element belongs to our selection equation (5.3). The $1 \times L\,(L > K)$ vector of instruments $\boldsymbol{z}_{it}$ is assumed to be free of unobserved heterogeneity due to a Mundlak (1978) time-constant fixed effect $c_{i2} = \bar{\boldsymbol{z}}_i\boldsymbol{\xi}_{t2} + a_{i2}$ with $\bar{\boldsymbol{z}}_i = T^{-1}\sum_{t=1}^{T}\boldsymbol{z}_{it}$ and $E\left(a_{i2}|\boldsymbol{z}_i\right) = 0$. The vector $\boldsymbol{z}_{it}$ contains all the exogenous elements of $\boldsymbol{x}_{it}$ and needs at least one additional regressor that affects selection. After estimating equation (5.3) for each year separately, we use the resulting propensities to compute the inverse Mills ratios

$$\hat{\lambda}_{it}^{SOE=1} \equiv \lambda^{SOE=1}\left(\boldsymbol{z}_{it}\hat{\boldsymbol{\delta}}_{t2}+\bar{\boldsymbol{z}}_i\hat{\boldsymbol{\xi}}_{t2}\right) \equiv \frac{\phi\left(\boldsymbol{z}_{it}\hat{\boldsymbol{\delta}}_{t2}+\bar{\boldsymbol{z}}_i\hat{\boldsymbol{\xi}}_{t2}\right)}{\Phi\left(\boldsymbol{z}_{it}\hat{\boldsymbol{\delta}}_{t2}+\bar{\boldsymbol{z}}_i\hat{\boldsymbol{\xi}}_{t2}\right)}, \tag{5.4}$$

$$\hat{\lambda}_{it}^{SOE=0} \equiv \lambda^{SOE=0}\left(\boldsymbol{z}_{it}\hat{\boldsymbol{\delta}}_{t2}+\bar{\boldsymbol{z}}_i\hat{\boldsymbol{\xi}}_{t2}\right) \equiv \frac{\phi\left(\boldsymbol{z}_{it}\hat{\boldsymbol{\delta}}_{t2}+\bar{\boldsymbol{z}}_i\hat{\boldsymbol{\xi}}_{t2}\right)}{1-\Phi\left(\boldsymbol{z}_{it}\hat{\boldsymbol{\delta}}_{t2}+\bar{\boldsymbol{z}}_i\hat{\boldsymbol{\xi}}_{t2}\right)}, \tag{5.5}$$

where $\phi\left(\cdot\right)$ represents the standard normal density and $\Phi\left(\cdot\right)$ the cumulative standard normal distribution.

We then model the unobserved heterogeneity from equation (5.2) analogously to the selection equation as $c_{i1} = \bar{\boldsymbol{z}}_i\boldsymbol{\eta} + a_{i1}$, where $\bar{\boldsymbol{z}}_i = T^{-1}\sum_{t=1}^{T}\boldsymbol{z}_{it}$ and we assume $E\left(a_{i1}|\boldsymbol{z}_{i1}\right) = 0$. Inclusion of the respective inverse Mills ratio to control for endogenous selection leads to our outcome equation:

$$AVIN_{it}^{R} = \boldsymbol{x}_{it}\boldsymbol{\beta}^{R} + \bar{\boldsymbol{z}}_i\boldsymbol{\eta} + \gamma\hat{\lambda_{it}^{R}} + \gamma^{t}\hat{\lambda_{it}^{R}} \times \psi_t + \psi_t + e_{it1},\ t = 1 \ldots T, \tag{5.6}$$

where R indicates the respective terms for either the SOE or the private regime. The interaction term $\hat{\lambda_{it}^{R}} \times \psi_t$ controls for time variation in unobserved effects and ψ_t is a set of time dummies. The inclusion of the inverse Mills ratio $\hat{\lambda_{it}^{R}}$ as an additional regressor is standard in the empirical corporate finance literature (Li and Prabhala, 2007). It allows controlling for omitted variables such as unobserved private information on the management level or implicit state warranties. The fact that we allow the impact of the selection terms to differ across

time (by the inclusion of $\gamma^t \hat{\lambda}_{it}^R \times \psi_t$) enables us to control for time-dependent unobservable variables related to state ownership.

We estimate equation (5.6) by pooled 2SLS and use $\boldsymbol{x}_{it} \equiv \boldsymbol{z}_{it1} \subset \boldsymbol{z}_{it}$, $\bar{\boldsymbol{z}}_i$ and $\hat{\lambda}_{it}^R$ as instruments. The vector $\boldsymbol{z}_{it1}$ is a subset of $\boldsymbol{z}_{it}$ because it excludes the selection-specific instruments used in the probit equation (5.3). In our basic model, we treat all elements of $\boldsymbol{x}_{it}$ as exogenous, which is why $\boldsymbol{z}_{it1}$ and $\boldsymbol{x}_{it}$ contain the same elements. In Section 5.5.1, we report results taking into account potentially endogenous elements of $\boldsymbol{x}_{it}$, which means that $\boldsymbol{x}_{it} \neq \boldsymbol{z}_{it1}$.

It is important to highlight that we estimate (5.6) twice, once for SOEs and once for private firms and include the respective selection adjustment terms. After obtaining estimation results, we apply the parametric error correction procedure suggested in Semykina and Wooldridge (2010). For a detailed description of this procedure, we refer to their study.

5.3.2 Implementation and descriptive statistics

In a first step, we estimate the propensity of state ownership from equation (5.3). We employ a similar set of control variables as Borisova et al. (2012) and Beuselinck et al. (2017), of which Table 5.3.1 contains descriptive statistics (along with descriptive statistics for variables and instruments used to estimate the outcome equation (5.6)).[80]

Table 5.3.1: Descriptive statistics

Table 5.3.1 presents summary statistics of the dependent variable $AVIN_{it}$, the profitability indicator $OROA_{it}$ and the control variables used in the selection and outcome equations; based on 101,221 observations.

Variable	**Mean**	**Std.Dev.**	**Min**	**Max**
$AVIN_{it}$	0.05	0.04	0.00	0.25
$OROA_{it}$	0.04	0.09	-0.54	0.41
LEV_{it}	0.28	0.21	0.00	1.00
$TANG_{it}$	0.31	0.28	0.00	1.00
$log(SALES)_{it}$	9.42	1.67	1.10	17.93
$\Delta log(FA)_{it}$	0.00	0.31	-1.69	2.47
$\Delta log(SALES)_{it}$	-0.00	0.26	-1.74	1.98
$CREDITM_{kt}$	160.53	42.91	37.51	248.94
$GDPG_{kt}$	0.00	0.02	-0.08	0.11
TAX_{kt}	0.30	0.05	0.15	0.39
$INFL_{kt}$	0.01	0.01	-0.01	0.06
$STANG_{st}$	0.28	0.15	0.03	0.77
$LAEF_{it}$	23.12	16.54	0.00	99.71
$GDPPC_{kt}$	36,398.86	5,010.41	17,354.78	45,296.46
$SLEV_{st}$	0.18	0.06	0.03	0.60

[80]The estimation procedure impedes the use of time-constant variables such as a country's legal origin, which is why we cannot use all controls as in Borisova et al. (2012) and Beuselinck et al. (2017). Appendix Table 5.8.4 presents correlations for all variables.

Specifically, we include all variables in the probit estimate which we initially treat as exogenous in our outcome equation (5.6). On the firm level, this is the case for our profitability indicator $OROA_{it}$, a firm's asset tangibility $TANG_{it}$, firm size $log(SALES)_{it}$ and leverage LEV_{it}.[81] On the country level we include a country's credit market size $CREDITM_{kt}$, GDP growth rate $GDPG_{kt}$, the statutory tax rate TAX_{kt} and inflation $INFL_{kt}$. In addition to the exogenous variables from the outcome equation, we include on the firm level an investment proxy $\Delta log(FA)_{it}$ and a sales growth proxy $\Delta log(SALES)_{it}$, as well as GDP per capita $GDPPC_{kt}$ on the country level. The estimation procedure requires us to include at least one additional time-varying instrument that affects selection into state ownership but not the outcome variable $AVIN_{it}$. We use two instruments: First, in the spirit of Laeven and Levine (2009), we use sectoral asset tangibility $STANG_{st}$. It fulfills the economic criteria for good selection instruments because a higher share of fixed assets is typical for network industries, where SOE are particularly common. It is unlikely to have a causal impact on individual firms' financing cost, given that we control for individual asset tangibility $TANG_{it}$. Concerns of reverse causality when using sectoral instruments by Lin et al. (2011) and Lin et al. (2013) seem less of a concern here due to the huge sample size, and the requirement that each sector mean derives from at least two companies.[82] The correlation between the individual and the sectoral tangibility variables from Appendix Table 5.8.4 is 0.59, which suggests multicollinearity is not a strong concern. The second instrument is the labor efficiency indicator $LAEF_{it}$, which we define as a firm's labor costs divided by its operating revenue. The economic reasoning behind the performance ratio $LAEF_{it}$ as an instrument is previous empirical findings, which relate state ownership to less productive workers (Dewenter and Malatesta, 2001). It is also reasonable to assume that it does not directly affect the dependent variable $AVIN_{it}$. The correlation from Appendix Table 5.8.4 is -0.01 and supports this argument. We estimate the probit model for each year separately and use the results to calculate the inverse Mills ratios according to equations (5.4) and (5.5).

In a second step, we estimate our outcome equation (5.6) using the exogenous regressors from above, the respective inverse Mills ratios (including the time interactions), as well as time dummies. Thus, our model contains commonly used firm and country-level control variables

[81] We report results for potentially endogenous firm-level variables in Section 5.5.1.

[82] Only 1 percent of firms are based in sectors with less than 400 firm-years because we calculate sectoral instruments using a much larger sample, i.e., we include observations with missing values that drop out of our selection and outcome equations (5.3) and (5.6).

in bond price regressions.[83] Semykina and Wooldridge (2010) propose to begin with a fixed effects two-stage least squares (FE-2SLS) and test the significance of the selection terms $\hat{\lambda}_{it}^{R}$ and $\hat{\lambda}_{it}^{R} \times \psi_t$ using a Hausmann test. In the absence of selection, FE-2SLS is preferable to the Semykina and Wooldridge (2010) correction procedure, which leads to a larger root mean square errors. However, we can reject the null hypothesis of no selection by a wide margin for both ownership types (the p-values are below 0.0001 for both tests).

5.4 Basic empirical results

Our field data analysis has the primary goals to compare lending rates of SOEs with those of private firms and to test Hypothesis 1. We start with two-sided t-tests to see if the descriptive $AVIN_{it}$ difference between SOEs and private firms is meaningful. Then, we analyze the impact of firm risk, which we proxy with the operating return on assets $OROA_{it}$, on a firm's average interest rate $AVIN_{it}$. If implicit state guarantees are imperfect and the primary driver of lower borrowing costs, we should see a significant impact of $OROA_{it}$ on $AVIN_{it}$ for both ownership types, albeit with a smaller magnitude for SOEs. If the risk proxy $OROA_{it}$ does not affect the borrowing costs of SOEs at all, this is indirect evidence of other factors driving borrowing costs, such as political considerations or social lending.

Table 5.4.1 contains yearly t-tests for differences in $AVIN_{it}$ means between our ownership groups. SOEs pay less for their debt than private firms in all years – the difference is significant at the 1-percent level in each test. The spread is not time-constant: It ranges from a maximum of almost 2.5 percentage points at the height of the financial crisis to a much lower 0.5 percentage points in 2015. SOEs have easier access to debt in times of crisis, a finding that is in line with a study for publicly listed SOEs by Borisova et al. (2015).

Finally, we proceed to estimate equation (5.6) by pooled 2SLS for the two ownership groups separately. It is important to note that the estimates control for selection into the state or private ownership regime due to the inclusion of the inverse Mills ratios. Additionally, they

[83] Similar firm and country controls are suggested by Fama and French (1993), Borisova et al. (2015), and Goss and Roberts (2011). A disadvantage of our data is that we are not able to control for a set of firm and debt-contract specific factors because we do not observe this data. On the firm-level, this is primarily the case for the market-to-book ratio, which we do not observe for our non-traded firms. We believe debt-contract related factors are less of a concern in our case because we are primarily interested in the impact of firm-level determinants on the average cost of debt, i.e., across all debt contracts of a firm. Since our firms are largely medium-sized, we argue that conventional debt contracts should be roughly similar within firms.

Table 5.4.1: Statistical tests

Table 5.4.1 presents tests for differences in means of the dependent variable $AVIN_{it}$ by year; based on 101,221 observations; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Year	**Mean** $SOE_i = 0$	**Mean** $SOE_i = 1$	**Difference**	**T-statistic**
$2007 - 2015$	0.0436	0.0546	-0.0110***	-22.70
2007	0.0417	0.0661	-0.0244***	-6.84
2008	0.0471	0.0712	-0.0241***	-13.43
2009	0.0429	0.0579	-0.0151***	-10.13
2010	0.0428	0.0501	-0.0073***	-5.59
2011	0.0446	0.0543	-0.0097***	-8.05
2012	0.0456	0.0551	-0.0095***	-7.74
2013	0.0432	0.0526	-0.0093***	-7.22
2014	0.0419	0.0510	-0.0091***	-6.74
2015	0.0411	0.0464	-0.0053***	-3.45

contain a Mundlak (1978) fixed effect that absorbs unobserved heterogeneity on the firm-level.

Table 5.4.2 presents our basic regression results. They are based on $N_A = 6,868$ SOE firm-years and $N_B = 94,353$ private firm-years. Firm risk, proxied with firm performance $OROA_{it}$, does not affect the funding rate of SOEs at all (Column A). This suggests that company-associated risk is not a key explanatory factor for average interest rates of SOEs. The finding is remarkable in the sense that we should not observe any explicit state guarantees on this debt and the variable is highly significant for private firms (Column B). For them, a ceteris paribus increase in firm profitability by one percentage point will decrease the average debt cost by 0.0136 percentage points or 1.36 basis points. This is what we expect: The more profitable a firm is, the lower its funding costs should be because of lower default risk. This result is in line with findings by Goss and Roberts (2011) and Borisova et al. (2015).[84] Our basic result confirms Hypothesis 1. The selection term λ_{it}^{SOE} has a significantly negative sign and confirms that ownership is not random.

The effects of other control variables have reasonable interpretations. LEV_{it} negatively affects $AVIN_{it}$ of both SOEs and private firms. This may seem surprising at first because the higher the debt level, the higher the risk of default. It is important to remember that we look at a firms' average interest rate for all long and short-term debt obligations. Firms which can issue much debt are likely to be more creditworthy overall and should have a lower marginal cost for the initial unit of debt. Besides, they may benefit from a flatter interest rate curve. This could lead to lower average interest rates for higher levered firms. $TANG_{it}$

[84]Both studies confirm a negative impact of firm profitability on the cost of debt. Magnitudes, however, differ because of alternative definitions of the dependent variable.

Table 5.4.2: Basic result

The table presents switching regressions based on $N_A = 6,868$ and $N_B = 94,353$ observations, respectively; a firm has non-missing values in at least five consecutive years from 2007 to 2015; the dependent variable is $AVIN_{it}$; estimates take into account endogenous selection into state ownership. Selection is instrumented with sectoral asset tangibility $STANG_{st}$ and the employee efficiency measure $LAEF_{it}$; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: SOE *Coeff.* *(s.e.)*	B: Private *Coeff.* *(s.e.)*
$OROA_{it}$	0.0170	-0.0136***
	(0.0157)	(0.0023)
LEV_{it}	-0.1024***	-0.1303***
	(0.0080)	(0.0020)
$TANG_{it}$	0.0215***	0.0166***
	(0.0065)	(0.0022)
$log(SALES)_{it}$	0.0011	0.0005
	(0.0021)	(0.0006)
$CREDITM_{kt}$	0.0001	0.0000
	(0.0000)	(0.0000)
$GDPG_{kt}$	0.0124	0.0346***
	(0.0335)	(0.0122)
TAX_{kt}	0.0632**	-0.0471***
	(0.0307)	(0.0098)
$INFL_{it}$	-0.0557	-0.0527*
	(0.0640)	(0.0285)
λ_{it}^{SOE}	-0.0123**	
	(0.0060)	
λ_{it}^{PRI}		-0.0003
		(0.0014)
Fixed Effects	Yes	Yes
Time Dummies	Yes	Yes
$\lambda\times$ Time	Yes	Yes

positively affects average interest rates, probably because firms with more fixed assets can afford to take on more expensive debt. An interesting finding is the differential impact of the statutory tax rate TAX_{kt}. While many studies confirm a positive effect of taxes on leverage due to interest tax shields (Feld et al., 2013), the relation between taxes and the average interest rate is not as straight-forward. What matters to tax-planning firms ultimately is the amount of taxes they can offset from their bill using paid interest and not the average cost of their debt. It may be that a firm pays, on average, less for its debt than a comparable firm but offsets a higher share of its tax payments with interest. Hence, the negative impact of TAX_{kt} on $AVIN_{it}$ for private firms (Column B) is not contradicting previous taxation

research. The positive impact of TAX_{kt} on $AVIN_{it}$ for SOEs (Column A) may partly result from an over-representation of low-tax and high average interest countries in the SOE sample (especially Poland).

A surprising result is the negative relation of inflation to our dependent variable $AVIN_{it}$. In normal times, we would expect higher inflation rates to cause higher interest rates. A possible explanation could lie in the combination of exceptional macroeconomic circumstances and unconventional monetary policy of the ECB. We look at average interest rates at a time of initially strong deflationary tendencies during the financial crisis followed by a slight uptick in inflation rates in the years afterward. At the same time, the ECB pursued a very expansive monetary policy to bring interest rates down, which could be responsible for the negative relation observed.

Our main result is very robust. In addition to the analysis presented in Section 5.8.1, we confirm the differential impact of firm profitability $OROA_{it}$ on average interest $AVIN_{it}$ for the following model variations: First, a different firm size proxy log(total assets); second, inclusion of a debt maturity proxy; third, variations in the selection instruments;[85] and forth, variations in the dependent variable interval.[86] Results for all specifications are available upon request.

5.5 Sensitivity analyses

5.5.1 Instrumental variables

In this Section, we address potential reverse causality concerns from our baseline result of the firm-level control variables, and in particular, financial leverage LEV_{it}. Financial leverage could affect our dependent variable $AVIN_{it}$ because the relative share of debt financing could affect the average interest rate a firm pays. However, LEV_{it} could also be affected by $AVIN_{it}$ in the sense that companies only use as much debt as they can afford at their specific average interest rate.

To disentangle these effects, we make use of the specific methodological approach of

[85] We use a set of different employee efficiency measures as well as the country-level variable *public sector quota of GDP* instead of sectoral asset tangibility $STANG_{st}$.

[86] Our main result allows a maximum $AVIN_{it}$ of 25 percent a year and a time average of 15 percent. We alternatively use 15 percent as a yearly maximum for $AVIN_{it}$ as well as ten and five percent as cut-offs for the time average per firm.

Table 5.5.1: Endogenous firm-level controls

The table presents switching regressions based on $N_A = 6,747$ and $N_B = 91,451$ observations, respectively; a firm has non-missing values in at least five consecutive years from 2007 to 2015; dependent variable is $AVIN_{it}$; estimates take into account endogenous selection into state ownership and endogenous firm-level variables LEV_{it}, $TANG_{it}$, and $log(SALES)_{it}$; selection is instrumented with sectoral asset tangibility $STANG_{st}$ and the employee efficiency measure $LAEF_{it}$; firm-level controls are instrumented with sectoral leverage $SLEV_{st}$, $TANG_{i,t-1}$, and $log(SALES)_{i,t-1}$; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: SOE *Coeff.* *(s.e.)*	B: Private *Coeff.* *(s.e.)*
$OROA_{it}$	0.0374	-0.0269***
	(0.0247)	(0.0098)
LEV_{it}	-0.0347	-0.1885***
	(0.0704)	(0.0385)
$TANG_{it}$	0.0234	0.0485***
	(0.0149)	(0.0099)
$log(SALES)_{it}$	0.0015	0.0010
	(0.0031)	(0.0011)
$CREDITM_{kt}$	0.0001	0.0000
	(0.0001)	(0.0000)
$GDPG_{kt}$	0.0330	0.0361**
	(0.0351)	(0.0148)
TAX_{kt}	0.0552*	-0.0444***
	(0.0320)	(0.0133)
$INFL_{it}$	-0.0853	-0.0065
	(0.0760)	(0.0373)
λ_{it}^{SOE}	-0.0096	
	(0.0073)	
λ_{it}^{PRI}		0.0005
		(0.0021)
Fixed Effects	Yes	Yes
Time Dummies	Yes	Yes
$\lambda\times$ Time	Yes	Yes

the Semykina and Wooldridge (2010) framework, which allows us to re-estimate our basic model and instrument the firm-level variables LEV_{it}, $TANG_{it}$, and $log(SALES)_{it}$ using all exogenous information and the instruments sectoral leverage $SLEV_{st}$, as well as the time lags $TANG_{i,t-1}$, and $log(SALES)_{i,t-1}$. By using lagged variables as *instruments* and not as *proxy variables*, we avoid a widespread misconception in empirical social sciences that lagged explanatory variables avoid simultaneity bias (Reed, 2015). We use sectoral leverage $SLEV_{st}$ as an instrument instead of lagged leverage because endogeneity concerns are strongest for this variable. Sectoral leverage is an important determinant of a firm's capital structure (Frank

and Goyal, 2009). Given the fact that 99 percent of sectoral averages draw on more than 400 firm-years, $SLEV_{st}$ should not be a significant determinant of individual average interest rates. We test the validity of these instruments using the Kleibergen-Paap rk LM statistic for under-identification, the Stock-Yogo test for weak instruments, and the Hansen J-statistic for over-identification. All these tests suggest the proposed specification is suitable. Table 5.5.1 presents the results.

Compared to the estimates from Table 5.4.2, the coefficients of $OROA_{it}$ are stronger in magnitude. For SOEs, the model predicts a positive relation, which is not statistically significant (Column A). For private firms, the model suggests that an increase in profitability of 1 percentage point will decrease the dependent variable by 0.0269 percentage points or 2.69 basis points (Column B). The selection term λ_{it}^{SOE} still has a negative sign but fails to meet conventional significance thresholds. The coefficient of instrumented LEV_{it} retains its sign in both columns but is significant at higher magnitude only in Column B. The other significant control variables from Table 5.4.2, $TANG_{it}$, $GDPG_{kt}$, and TAX_{kt}, retain their signs but are less significant in the SOE regression (Column A). In contrast, magnitudes are stronger in the model for private companies (Column B). We conclude from this robustness check that potential reverse causality of firm variables and in particular financial leverage does not fundamentally alter our results from above.

5.5.2 Net income profitability

Throughout this study, we have used a firm's operating profit to calculate the profitability proxy $OROA_{it}$. Even though we believe this is the most appropriate measure because we need not worry about reverse causality, it is not the most commonly used indicator in practitioners' work. Banks, when deciding on credit contracts, evaluate firms using widely reported financial indicators. Thus, the reverse causality of post-financial performance profitability measures may be lower than one might expect. Hence, we re-estimate our baseline model using ROA_{it} as a profitability proxy, which we define as net income divided by total assets. Table 5.5.2 presents the results.

Compared to the baseline result from Table 5.4.2, the coefficient of ROA_{it} has a negative sign but remains statistically insignificant (Column A). The magnitude of the effect for private firms has increased to -0.0357, suggesting a decrease in the average interest rate of 3.57 basis points for a profitability increase of one percentage point (Column B). The effect

Table 5.5.2: Net income return on assets

The table presents switching regressions based on $N_A = 6,749$ and $N_B = 92,057$ observations, respectively; the profitability measure ROA_{it} is a firm's net income divided by total assets; a firm has non-missing values in at least five consecutive years from 2007 to 2015; dependent variable is $AVIN_{it}$; estimates take into account endogenous selection into state ownership; selection is instrumented with sectoral asset tangibility $STANG_{st}$ and the employee efficiency measure $LAEF_{it}$; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: SOE *Coeff.* *(s.e.)*	B: Private *Coeff.* *(s.e.)*
ROA_{it}	-0.0146	-0.0357***
	(0.0158)	(0.0025)
LEV_{it}	-0.1035***	-0.1328***
	(0.0082)	(0.0020)
$TANG_{it}$	0.0194***	0.0137***
	(0.0066)	(0.0022)
$log(SALES)_{it}$	0.0014	0.0013**
	(0.0021)	(0.0006)
$CREDITM_{kt}$	0.0001	0.0000
	(0.0000)	(0.0000)
$GDPG_{kt}$	0.0138	0.0359***
	(0.0338)	(0.0121)
TAX_{kt}	0.0628**	-0.0463***
	(0.0310)	(0.0094)
$INFL_{it}$	-0.0383	-0.0439
	(0.0649)	(0.0284)
λ_{it}^{SOE}	-0.0094*	
	(0.0052)	
λ_{it}^{PRI}		0.0001
		(0.0006)
Fixed Effects	Yes	Yes
Time Dummies	Yes	Yes
$\lambda\times$ Time	Yes	Yes

remains significant at the one percent level. The selection indicator λ_{it}^{SOE} is of slightly weaker magnitude than in the primary result (-0.0094 compared to -0.0123) and remains statistically significant at the ten percent level. Coefficients and significance levels of other controls do not differ much from the benchmark. We conclude from this test that our result is robust to a more commonly used but econometrically inferior specification of firm profitability.

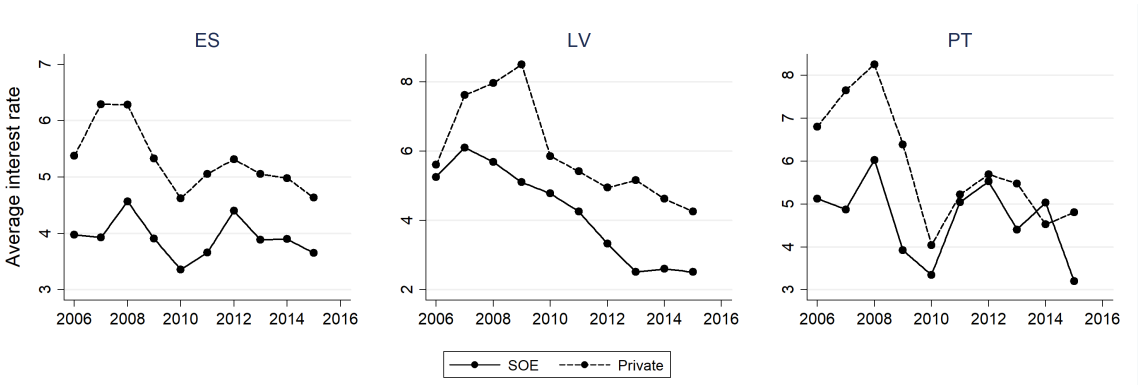
5.5.3 Implicit guarantees during sovereign debt crises

Let us now further evaluate the question whether implicit state guarantees alone offer a sufficient explanation for lower SOE funding rates. In empirical terms, this question has already been answered in the sense that political considerations have been found to affect debt allocations and costs significantly (Sapienza, 2004; Dinc, 2005). Hence, the purpose of this robustness check is not to refute established results on implicit guarantees but to reconsider their predictions in the light of our data. Many studies have argued that state ownership shields companies from adverse effects during financial crises such as debt constraints and reductions in firm value (Borisova and Megginson, 2011; Borisova et al., 2015; Beuselinck et al., 2017). Similarly, Faccio et al. (2006) find that politically connected firms are more likely to be bailed out. What these studies have in common is that they implicitly assume solvency of the respective government because they focus on data from solvent countries or defaults of individual companies in normal economic times. The exceptional situation of public finances in some EU member states during our sample period allows us to analyze the average cost of debt in situations when the governments themselves were cut off from capital markets. This was the case for a total of eight member states who requested financial assistance from the EU, namely Cyprus, Greece, Hungary, Ireland, Latvia, Portugal, Romania, and Spain.[87] During the programs, all countries were under scrutiny from the European Institutions and the International Monetary Fund (IMF). As a consequence, a government's ability to act independently in case of an SOE default was severely constrained. Moreover, program countries usually had to comply with reform and privatization obligations to bring their budget deficits under control. We argue that in such a situation, implicit state warranties should become irrelevant because investors have lost trust in the government's ability to service debt. Figure 5.5.1 plots our dependent variable $AVIN_{it}$ for Latvia, Portugal and Spain. If implicit guarantees are the only explanation for cheaper funding rates, we would expect the difference between the two groups disappear – or become at least much smaller – before the beginning of the respective adjustment programs. Latvia and Portugal requested financial assistance in 2011, whereas Spain only did in 2012. Spain and Portugal experienced significant

[87] Unfortunately, from these countries, Orbis only contains sufficient company ownership data for Latvia, Portugal, and Spain.

Figure 5.5.1: Average interest rates in program countries

Figure 5.5.1 plots the empirical average interest rate $AVIN_{it}$ for Portugal and Spain.



sovereign downgrades from rating agencies prior to and during this period.[88] Figure 5.5.1 reveals that Spanish and Latvian average interest rate spreads between the two ownership groups remain roughly parallel over the whole period (left and central panel). Importantly, there is no significant change in the years of the economic adjustment program, a time when the European Institutions scrutinized the Latvian and Spanish governments, and bail-outs of (non-financial) state firms were less likely. In contrast, the Portuguese spread between SOEs and private firms is much lower in 2010-2014 than in previous years. Specifically, the SOE curve approaches the average interest rate of private companies in 2010 and 2011, i.e., immediately prior and at the beginning of the adjustment program. This finding indicates that implicit guarantees have disappeared.[89]

Table 5.5.3: Country tests

Table 5.5.3 presents tests for differences in means of the dependent variable $AVIN_{it}$ for Latvia, Portugal and Spain during their respective economic adjustment programs; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

Year	**Mean** $SOE_i = 0$	**Mean** $SOE_i = 1$	**Difference**	**T-statistic**
Latvia 2011 – 2014	0.0420	0.0587	-0.0167***	-6.01
Portugal 2011 – 2014	0.0499	0.0534	-0.0035	-0.79
Spain 2012 – 2014	0.0407	0.0512	-0.0105***	-6.95

[88]The rating agency Standard and Poor's downgraded Portugal from A+ in January 2009 to BB in January 2012 – a downgrade of 2 slots. Spain was downgraded from AA+ in January 2009 to BBB- in October 2012 – also a downgrade of 2 slots.

[89]It should be noted that the Portuguese curve is based on a smaller sample of 199 firm-years compared to 678 for Latvia and 2,516 for Spain. These numbers deviate from the ones reported in Table 5.2.1 because they include observations with missing values in other control variables.

Table 5.5.3 presents t-tests for differences in means for all three countries during their adjustment programs. We use yearly data, so the time interval for both tests exceeds the actual time interval of the adjustment programs, a fact that allows for a time lag of the effect. Irrespective of government solvency, Latvia and Spain exhibit significantly lower average interest rates for SOEs than for private firms during their respective adjustment programs. We take this as evidence, that implicit state guarantees alone are unlikely to explain funding rates of SOEs in these countries. In Portugal, the difference is not significant during the adjustment program, a fact that instead points to implicit guarantees.

We conclude that implicit guarantees seem a necessary but not sufficient condition to explain cheaper funding rates of SOEs. Preferential allocation of debt could be a complementing explanation for the non-existence of spreads in Latvia and Spain during their adjustment programs: In a period of recession and falling living standards, the perceived positive externality of public ownership may bias fund allocation more strongly than in economically prosperous times. In Portugal, this argument is less convincing, which could point to different institutional environments.

5.6 Experimental analysis

5.6.1 Experimental hypothesis

So far we have analyzed financial data from European companies. Our results have shown that risk proxies do not significantly affect the average interest rate for SOEs whereas they are a critical determinant for private firms. The most common explanation for this finding is that implicit state guarantees to bondholders render company risk less relevant. However, implicit guarantees alone seem an insufficient justification because such warranties are not explicit and default probabilities of SOEs remain positive. In other words, we would have expected risk proxies to be less important but significant determinants of SOE debt.

Let us now address the question of whether behavioral arguments could offer a complementing explanation for lower SOE financing costs. It is important to remember that we look mostly at local SOEs, which provide a multitude of services affecting the daily life of citizens.[90] Ramsey (1980) gives an example of how state ownership may induce behavioral

[90]Our behavioral argument remains valid for large *national champions*. However, the increased distance between citizens and company may render it less relevant.

distortions: A loss from an SOE could cause a larger decline in the citizens‘ utility than an analogous gain would raise it. The argument rests on the plausible assumption that a large number of welfare projects would dilute additional gains with relatively little impact on individuals whereas a loss would trigger public anger about wasted tax-payers' money.

Any behavioral argument leading to lower funding costs for SOEs would require that citizens attribute some societal benefit to this specific ownership structure. If they price in societal returns, they may be willing to accept lower individual returns for the sake of the overall benefit of society. Do individuals care about the returns of their co-citizens? Since the fundamental result of Kahneman et al. (1986), more than 100 experimental studies have shown that around two-thirds of *dictators* in dictator games voluntarily share a part of their profit with others (Engel, 2011). The same holds for experimental work on public goods, where plenty of studies show that people are willing to take costly action to increase the payoffs of others (Camerer and Fehr, 2004). A paper by Bartling et al. (2015) on social behavior in a market environment reveals that consumers persistently chose socially responsible products to avoid negative externalities on others. This choice is reflected in the composition of product portfolios and the markup of the socially beneficial product. Thus, consumers are willing to incur a higher cost to themselves for the benefit of their co-citizens. In an investment context, Berg et al. (1995) find that self-interest alone cannot explain the investment behavior in their experiment. A study that relates experimental results to SOEs is the one by Reynolds et al. (2009), who find that participants of experiments are more risk-averse when making decisions for others. The authors see this as a reason for conservative investment and innovation policies in state firms.

In our setting, SOEs could have cheaper funding costs because citizens assume they generate a positive externality to others. We summarize this argument in Hypothesis 2:

Hypothesis 2 *Citizens invest in SOEs with low financial returns if they assume an additional return to society – even if this investment behavior is inefficient.*

The inefficiency argument rests on the empirical observation that SOEs have been found to be less profitable and less efficient Dewenter and Malatesta (2001); Megginson and Netter (2001); Estrin et al. (2009).

5.6.2 Experimental methodology and results

To evaluate Hypothesis 2, we conduct an experiment in which participants decide whether to invest in a project that pays a return only to them (mimicking a private company) or a project that pays a return to them, and, additionally, to the general public (thus imitating an SOE).[91] Hence, our setup lets participants assume the role of bank employees, mayors or other officials in board functions of SOEs. Our experimental design is thus a new variation of a classical public goods game, where full investment into the company with externality (the SOE) leads to an inefficient but more egalitarian outcome to the participant pool.

The experiment relies on questionnaires, which ask students 11 questions to allocate 100 Euros between both company types. The participant pool consists of economics students of Kiel University, who had about ten minutes to answer the questionnaires.

In total, we used four questionnaires, which differ by the allocation mechanism and the modeling of the social benefit. Two groups were given the task to distribute their 100 Euros freely between the two companies (A & B). Two other groups had the binary task to chose either one company or the other (C & D). These two groups were each again divided in one group where the social return takes the form of a donation (A & C) and one group where the social return is modeled as a positive externality to citizens (B & D). Otherwise, there is no difference between the four questionnaires, which we summarize in Table 5.6.1.

Table 5.6.1: Questionnaires

Number	**Allocation type**	**Social return is ...**	**N**
A	continuous distribution among both companies	...donated to charity	34
B	continuous distribution among both companies	...positive externality	24
C	binary choice between both companies	...donated to charity	27
D	binary choice between both companies	...positive externality	25

A translation of Questionnaire A is in Appendix Section 5.8.4. The eleven questions in each questionnaire cover different scenarios of risk and return correlations. The purpose of this is to check whether investment in company B (the *SOE*) is persistent and independent of diversification arguments or risk preferences of participants. For instance, we ask all specifications for two types of variances: First, we allow the *private firm* return variance to be

[91] Our experiment is based on hypothetical investments, i.e., no real payoffs were at stake.

larger (which is the case in our field data), and second, we assume equal variances to exclude the possibility of choices resulting from risk aversion. The conventional mean returns are – at five percent for private investment and two percent for the *SOE* investment – the same in all questions and similar to the actual observed mean $OROA_{it}$ from our field data analysis. The *SOE* investment generates an additional return of two percent that goes either to charity or co-citizens. The eleven questions also differ by overall returns of the *SOE* investment: Only in Question 2 and 9 is it an efficient investment, i.e., the conventional and societal expected returns combined equal the expected return of the private investment. Else, the private investment yields an overall higher expected return.

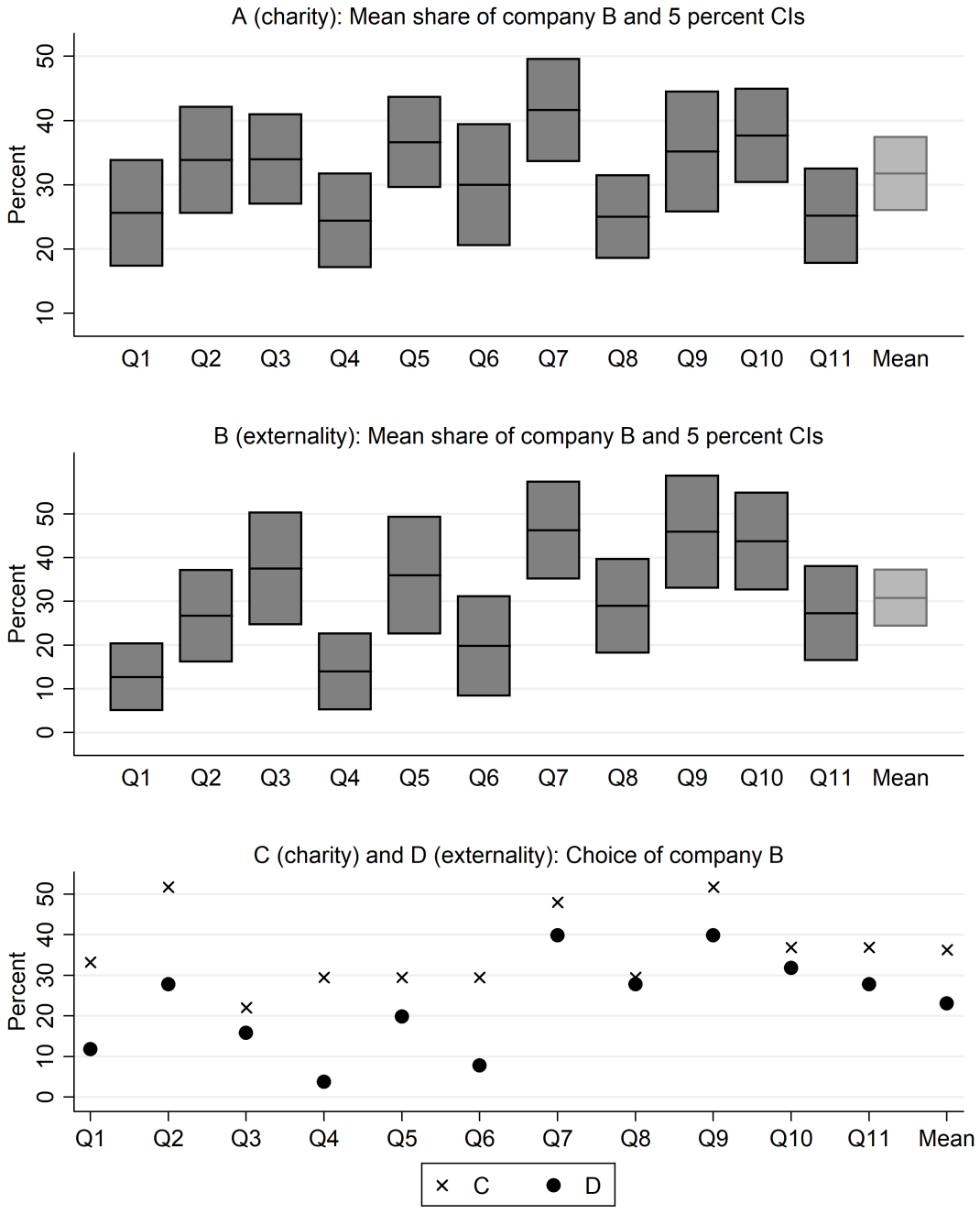
Questions 1 and 2 cover decisions under certainty, and Questions 3-11 under uncertainty. Under certainty, we cover a scenario where investing in the *SOE* is inefficient (Question 1) and a scenario where it is efficient (Question 2). Question 3 deals with uncertain conventional returns for both ownership types and certain societal benefits. Question 4 is similar but assumes equal return variances. Question 5 correlates conventional returns negatively. Question 6 is like Question 5 but again assumes equal return variances. Question 7 and 8 repeat this pattern with a risky societal payoff: Question 7 allows return variances to differ and Question 8 assumes equal variances across ownership groups. In Question 9, the externality remains risky but investing in the *SOE* project becomes efficient because of identical expected returns. Question 10 negatively correlates both risky return types of the *SOE* investment with the risky *private firm* return. Question 11 is the same but again assumes identical variances.

Figure 5.6.1 plots the responses of all questionnaires. The first graph contains results for Questionnaire A, the second graph for Questionnaire B, and the third graph for Questionnaires C and D.

The first two graphs (A and B) plot the fund allocation to the *SOE* investment as a percentage share across all specifications of the questions Q1 to Q11. The horizontal line within each box represents the mean value of the question, and the grey box indicates 5 percent confidence intervals. In both graphs, the participants could freely choose how much they would like to invest in the *SOE* and how much in the *private firm*. The last, light grey box indicates the mean across all questions. Both graphs show that participants put a statistically significant share of their funds into the *SOE* investment in all scenarios. The average investment share of company B across all eleven questions is – at around 30 percent – very similar in Questionnaires A and B. As we would expect, it is also below the 40-60

Figure 5.6.1: Questionnaire results

Figure 5.6.1 plots the results of all four questionnaires A, B, C, and D; the y-axis in the upper two graphs contains the percentage allocation of funds to the *SOE* investment when participants could allocate freely; the lower graph plots the percentage of participants choosing the *SOE* investment in the two binary questionnaires; the x-axis in all graphs plots questions 1-11 and the mean value across all questions.



percent average investment level in standard public goods games, which treat investment in the public good as efficient (Camerer and Fehr, 2004). Under certainty, participants put more into the *SOE* investment if this is efficient (Q2 vs. Q1). An interesting observation is that levels of *SOE* investment are lower under equal variances (Q4 vs. Q3, Q6 vs. Q5, Q8 vs. Q7, and Q11 vs. Q10). This suggests that risk aversion partially drives investment decisions – a result that nicely relates to our field data part where implicit guarantees cater to risk-averse investors. However, even under identical variances (and thus same risk), investments into company B remain significantly positive. As in the field data case, risk is not the only explanatory factor. It does not matter if we model the societal return as a donation to a charity or as a positive externality (Panel A vs. Panel B). Neither does it matter how returns correlate across ownership groups.

The lower graph (Panel C) plots the percentage of participants choosing the *SOE* investment in the two binary questionnaires C and D. In both cases, participants could only invest the full amount in either company A or B. Again, the share of participants that chose the *SOE* is significantly different from zero in all specifications of the investment task. The overall mean of the choice variants C and D is – at around 30 percent – very similar to the average of the allocation variants A and B. Under certainty, participants again invest more in company B if this is efficient (Q2 vs. Q1). We also observe the pattern that investment levels are lower under equal variances (Q4 vs. Q3, Q6 vs. Q5, and Q8 vs. Q7). Under choice, we observe a statistically significant difference between the charity and positive externality framework, with the charity Questionnaire C getting a higher share across all questions. However, this difference is only significant at the ten percent level.

The analysis presented in this Section supports Hypothesis 2. It indicates that participants are willing to forgo a positive amount of private returns in exchange for a social benefit such as charity or returns to co-citizens. While risk arguments remain a vital factor in determining portfolio allocations, societal concerns seem very important as well. This finding could be an additional explanation of why firms with constituent (state) ownership get cheaper funding – especially in countries with large state bank sectors such as Germany.

5.7 Conclusion

This study has introduced a new argument on how state ownership affects companies: If the general public assumes they are of societal value, they will be treated differently than private

companies. Different treatment extends to finance metrics: Our results indicate that decision makers accept inefficiently low returns from SOEs if they offer some value to society.

We start with field data analysis and check how firm performance affects the cost of debt. Results show that the more profitable a private firm, the less it pays for its debt. The reason behind this established result is that profitability functions as a risk proxy. The relation does not hold, however, for SOEs. The most common explanation for the differential impact is implicit state guarantees to bondholders. While risk considerations seem indeed relevant, results indicate that they are not sufficient to explain our finding. We argue that societal concerns could be an additional factor that provides easier financing access for state companies, especially if SOEs exist to combat market imperfections. Using questionnaires, we experiment to determine investment decisions of participants across ownership groups. They choose between a project that generates a private return only (a private firm) and a project that is less efficient but creates a payoff to society in addition to regular returns (a state-owned company). Results indicate that investment into the social but inefficient asset is positive in all specifications of the investment decision. Interestingly, risk considerations also seem relevant in the experimental context but are insufficient to explain investment behavior alone. We take this finding as evidence that citizens prize in societal value of SOEs even to the extent that the presence of societal payoff is more important than overall company performance.

SOEs and private firms are consequently not treated equally. Large multinational organizations like the OECD seem to be aware of that: Plenty of technical reports propose regulations on how to limit distortions in competition caused by state ownership or how to improve the lackluster performance of SOEs. They may all have missed that a fundamental underlying difference is the societal perception of state ownership, which could impede ownership neutrality on behavioral grounds.

5.8 Appendix to Chapter 5

5.8.1 Additional sensitivity analyses

5.8.1.1 Additional restrictions to ensure commercial activity

A key assumption of our ORBIS data analysis is that sample SOEs operate under the same legal framework than private firms. We already take numerous precautions in our baseline result to ensure this is the case. In this robustness check, we enforce even stricter criteria by dropping a broader selection of industries with potential public service obligations. Besides the already excluded categories "K: Finance and Insurance", "O: Public Administration and Defence" and railway sectors, we exclude the NACE2 categories "P: Education" and "Q: Human Health and Social Work". Furthermore, we only include sectors which contain both ownership groups. Thus, we ensure that all remaining SOEs operate in commercial sectors of the economy and, consequently, face the same regulatory framework as private companies. Table 5.8.1 presents the estimation results.

The additional restrictions reduce the sample size significantly, to $N_A = 6,155$ for the SOE model and $N_B = 28,037$ for the private-firm model, respectively. The differential impact of $OROA_{it}$ is unaffected by these additional data restrictions. The coefficient is not significant in the SOE model (Column A) and significant at the one percent level and of similar magnitude than in Table 5.4.2 in the private firm model (Column B). The selection term is significant at the ten percent level and predicts a decrease in average interest rates $AVIN_{it}$ of 1.04 percent (Column A). The coefficient of leverage LEV_{it} is highly significant and similar in magnitude to the baseline result in both models. No major deviation exists for the other significant control variables $TANG_{it}$, $GDPG_{kt}$, and TAX_{kt} from Table 5.4.2. We conclude from this robustness check that differences in the legal framework of SOEs due to public service obligations do not cause our results.

5.8.1.2 Variation in panel balance

Our baseline result from Table 5.4.2 requires that we observe data of each company in at least five consecutive years. This threshold is arbitrary and could influence our results. To exclude this possibility, we re-estimate our outcome equation (5.6) using two different panel structures, namely seven and three consecutive years. Table 5.8.2 contains the results for an

Table 5.8.1: Commercial activity

The table presents switching regressions based on $N_A = 6,115$ and $N_B = 28,037$ observations, respectively; a firm has non-missing values in at least five consecutive years from 2007 to 2015; dependent variable is $AVIN_{it}$; estimates take into account endogenous selection into state ownership; selection is instrumented with sectoral asset tangibility $STANG_{st}$ and the employee efficiency measure $LAEF_{it}$; estimates exclude all sectors with a possible public service obligation and contain only sectors with both ownership types; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: SOE *Coeff.* *(s.e.)*	B: Private *Coeff.* *(s.e.)*
$OROA_{it}$	0.0120	-0.0105***
	(0.0164)	(0.0039)
LEV_{it}	-0.1043***	-0.1136***
	(0.0086)	(0.0033)
$TANG_{it}$	0.0188***	0.0134***
	(0.0070)	(0.0032)
$log(SALES)_{it}$	0.0011	0.0003
	(0.0021)	(0.0009)
$CREDITM_{kt}$	0.0001	0.0000
	(0.0000)	(0.0000)
$GDPG_{kt}$	-0.0082	0.0076
	(0.0347)	(0.0198)
TAX_{kt}	0.0636**	-0.0150
	(0.0318)	(0.0154)
$INFL_{it}$	-0.0755	-0.0030
	(0.0650)	(0.0505)
λ_{it}^{SOE}	-0.0104*	
	(0.0057)	
λ_{it}^{PRI}		0.0004
		(0.0031)
Fixed Effects	Yes	Yes
Time Dummies	Yes	Yes
$\lambda\times$ Time	Yes	Yes

increase in panel balance.

The increase in panel balance leads to a significant drop in the number of observations: The SOE model (Column A) draws on $N_A = 4,074$ observations. The private firm model (Column B) uses $N_B = 28,376$ observations. The two variables of interest $OROA_{it}$ and λ_{it}^{SOE}, have very similar patterns than in the baseline model from Table 5.4.2. Whereas the risk proxy $OROA_{it}$ does not significantly affect the average interest rate $AVIN_{it}$, the coefficient in Column B is -0.0178, suggesting an $AVIN_{it}$ decrease of 1.78 basis points for a one percentage point increase of $OROA_{it}$. The selection indicator λ_{it}^{SOE} predicts a decrease

Table 5.8.2: Increased panel balance

The table presents switching regressions based on $N_A = 4,074$ and $N_B = 28,376$ observations, respectively; a firm has non-missing values in at least seven consecutive years from 2007 to 2015; the dependent variable is $AVIN_{it}$; estimates take into account endogenous selection into state ownership; selection is instrumented with sectoral asset tangibility $STANG_{st}$ and the employee efficiency measure $LAEF_{it}$; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: SOE *Coeff.* *(s.e.)*	B: Private *Coeff.* *(s.e.)*
$OROA_{it}$	0.0234	-0.0177***
	(0.0218)	(0.0046)
LEV_{it}	-0.0963***	-0.1143***
	(0.0099)	(0.0035)
$TANG_{it}$	0.0248***	0.0102***
	(0.0078)	(0.0035)
$log(SALES)_{it}$	-0.0003	0.0004
	(0.0027)	(0.0011)
$CREDITM_{kt}$	0.0001	0.0000
	(0.0001)	(0.0001)
$GDPG_{kt}$	0.0025	0.0365
	(0.0360)	(0.0244)
TAX_{kt}	0.0879**	-0.0224
	(0.0445)	(0.0327)
$INFL_{it}$	-0.0864	0.0016
	(0.0852)	(0.0573)
λ_{it}^{SOE}	-0.0100*	
	(0.0059)	
λ_{it}^{PRI}		0.0011
		(0.0066)
Fixed Effects	Yes	Yes
Time Dummies	Yes	Yes
$\lambda\times$ Time	Yes	Yes

of one percentage point in average interest rates if a firm is an SOE. The pattern of other control variables is mostly similar – except for the TAX_{kt} variable, which is not significant in the private firm model (Column B). We conclude from this robustness check that an increase in panel balance does not significantly alter our results.

Table 5.8.3 presents the results for a decrease in Panel balance. If we include all companies for which we observe data in three consecutive years, the SOE model (Column A) draws on $N_A = 8,785$ observations and the private firm model (Column B) on $N_B = 131,562$ observations. The impact of $OROA_{it}$ remains unchanged to previous results, as does the effect of leverage LEV_{it} and asset tangibility $TANG_{it}$. The selection term λ_{it}^{SOE} retains its

Table 5.8.3: Decreased panel balance

The table presents switching regressions based on $N_A = 8,785$ and $N_B = 131,562$ observations, respectively; a firm has non-missing values in at least three consecutive years from 2007 to 2015; the dependent variable is $AVIN_{it}$; estimates take into account endogenous selection into state ownership; selection is instrumented with sectoral asset tangibility $STANG_{st}$ and the employee efficiency measure $LAEF_{it}$; *** significant at the 1 percent level; ** significant at the 5 percent level; * significant at the 10 percent level.

	A: SOE *Coeff.* *(s.e.)*	B: Private *Coeff.* *(s.e.)*
$OROA_{it}$	0.0211	-0.0115***
	(0.0131)	(0.0019)
LEV_{it}	-0.1089***	-0.1365***
	(0.0074)	(0.0017)
$TANG_{it}$	0.0194***	0.0189***
	(0.0062)	(0.0020)
$log(SALES)_{it}$	0.0014	0.0003
	(0.0019)	(0.0005)
$CREDITM_{kt}$	0.0000	0.0000**
	(0.0000)	(0.0000)
$GDPG_{kt}$	0.0092	0.0442***
	(0.0305)	(0.0110)
TAX_{kt}	0.0397	-0.0443***
	(0.0297)	(0.0087)
$INFL_{it}$	-0.1211**	-0.0516**
	(0.0590)	(0.0246)
λ_{it}^{SOE}	-0.0069	
	(0.0052)	
λ_{it}^{PRI}		0.0007
		(0.0008)
Fixed Effects	Yes	Yes
Time Dummies	Yes	Yes
$\lambda\times$ Time	Yes	Yes

sign but misses significance thresholds by a thin margin. A significant difference between the baseline result from Table 5.5.1 is that inflation $INFL_{it}$ is negatively significant in both models. As pointed out above, this surprising result is most likely due to the exceptional deflationary environment and monetary policy during our sample period.

We conclude from this robustness check that a decrease in panel balance does not change our results significantly.

5.8.2 Additional figures and tables

Table 5.8.4: Correlation matrix

Table 5.8.4 presents correlations of the dependent variable LEV_{it}, state ownership SOE_i and the control variables used in the regression models; based on 101,221 observations.

	SOE_i	$AVIN_{it}$	$OROA_{it}$	LEV_{it}	$TANG_{it}$	$log(TA)_{it}$	$CREDITM_{kt}$	$GDPG_{kt}$	TAX_{kt}	$INFL_{it}$	$STANG_{st}$	$LAEF_{it}$	$\Delta log(FA)_{it}$	$\Delta log(SALES)_{it}$	$GDPPC_{kt}$	$SLEV_{st}$
SOE_i	1.00															
$AVIN_{it}$	0.07	1.00														
$OROA_{it}$	0.06	0.04	1.00													
LEV_{it}	-0.07	-0.26	-0.18	1.00												
$TANG_{it}$	-0.33	-0.10	-0.06	0.30	1.00											
$log(SALES)_{it}$	0.03	0.04	0.06	-0.10	-0.14	1.00										
$CREDITM_{kt}$	0.11	-0.01	-0.08	0.12	-0.09	-0.07	1.00									
$GDPG_{kt}$	-0.06	-0.01	0.08	-0.02	0.07	-0.02	-0.27	1.00								
TAX_{kt}	0.17	0.03	0.01	-0.11	-0.28	0.07	0.36	-0.11	1.00							
$INFL_{it}$	-0.03	0.08	0.01	0.02	0.02	0.06	0.03	0.11	0.03	1.00						
$STANG_{st}$	-0.31	-0.09	-0.04	0.23	0.59	-0.17	0.00	0.04	-0.14	0.00	1.00					
$LAEF_{it}$	-0.05	-0.01	-0.14	-0.08	0.07	-0.24	0.04	-0.01	0.06	-0.03	0.02	1.00				
$\Delta log(FA)_{it}$	0.00	-0.07	0.05	0.02	0.03	0.02	-0.02	-0.03	0.04	0.12	-0.00	-0.00	1.00			
$\Delta log(SALES)_{it}$	0.01	0.01	0.18	-0.00	0.02	0.07	-0.04	0.09	0.02	0.16	0.01	-0.06	0.18	1.00		
$GDPPC_{kt}$	0.04	0.04	0.07	0.08	0.02	0.04	-0.08	0.17	0.19	-0.09	0.03	0.09	0.01	0.03	1.00	
$SLEV_{st}$	-0.20	-0.07	-0.05	0.30	0.42	-0.16	0.08	-0.01	-0.05	0.07	0.73	-0.10	0.02	0.01	0.05	1.00

5.8.3 Variable definitions and data

Table 5.8.5: Variable definitions

Firm level	(Source: Orbis)
SOE_i	Binary indicator: 1 indicates state ownership.
$INTE_{it}$	Total interest paid by firm i in year t.
$LTDB_{it}$	Total long-term debt of firm i in year t (maturity $>$ 1 year).
$STDB_{it}$	Total short-term debt of firm i in year t (maturity $<$ 1 year).
$AVIN_{it}$	Total interest / total debt of firm i in year t.
$OROA_{it}$	Operating profit / total assets of firm i in year t.
ROA_{it}	Net income / total assets of firm i in year t.
LEV_{it}	Total debt / total assets of firm i in year t.
$TANG_{it}$	Asset tangibility: fixed assets / total assets of firm i in year t.
$log(SALES)_{it}$	log(Sales) of firm i in year t.
$LAEF_{it}$	Costs of employees / operating revenue of firm i in year t.
$\Delta log(FA)_{it}$	Investment proxy: $log(\text{fixed assets}_{it}) - log(\text{fixed assets})_{i,t-1}$.
$\Delta log(SALES)_{it}$	Sales growth: $log(\text{turnover}_{it}) - log(\text{turnover})_{i,t-1}$.
Country level	
TAX_{kt}	Statutory tax rate of country k in year t.
$CREDITM_{kt}$	Domestic credit provided by banking sector in country k and year t as percentage of GDP (Source: Worldbank).
$GDPG_{kt}$	Annual GDP growth in percent in country k and year t (Source: Worldbank).
$GDPPC_{kt}$	GDP per capita in country k and year t, PPP at constant 2011 international USD (Source: Worldbank) .
$INFL_{kt}$	Consumer price inflation of country k in year t (Source: Worldbank).
Sector level (4-digit NACE2 code)	
$SLEV_{st}$	Average total debt / total assets of sector s in year t.
$STANG_{st}$	Average asset tangibility: fixed assets / total assets of sector s in year t.

- State ownership:
  - Stand-alone firms and wholly owned subsidiaries that carry the Orbis ultimate ownership label "Public authority, state, government" in year t.

- Private ownership:
  - Stand-alone firms and wholly owned subsidiaries that carry the Orbis ultimate ownership label "Industrial company" in year t.
- We impute up to two years of missing ownership data if the owning entity and the owning share remained unchanged over this period.
- We only use unconsolidated financial data (BvD conscode "U1" and "U2").
- We drop duplicates in terms of id and year.
- We use only data from EU countries. Thus, we ensure a similar regulatory environment is present for all firms.
- Only observations with at least 5 consecutive firm-years in LEV_{it} from 2006-2015 are considered. The result does not depend on this restriction.
- $AVIN_{it} \in [0; 0.25]$. We drop observations with a larger time mean of 0.15. The results do not depend on this restriction.
- $LEV_{it} \in [0; 1[$. We presume that a fully leveraged firm should be a reporting error since every incorporation form we know requires some equity.
- $TANG_{it} \in [0; 1]$.
- The firm-level variables $OROA_{it}$, ROA_{it}, $\Delta log(SALES)_{it}$ and $\Delta log(FA)_{it}$ are winsorized at the top and bottom one percent to make our results less dependent on outliers.

5.8.4 Questionnaire

Remark: The bold information in brackets was not part of the distributed questionnaires.

Investment decisions
Questionnaire A

Instructions
In each situation below you need to take an investment decision. All questions are hypothetical, and you should answer them as if you were investing your own money.

There are two companies: company A and company B. Assume that the companies are identical except for the parameters mentioned explicitly in each question.

There are two types of returns: Normal returns directly increase your hypothetical payoff, whereas a different kind of return will hypothetically be donated to charity. Each investment situation will explicitly state the return share that is going to charity.

You can invest any share of your money in company A or company B. For this purpose, please insert a percentage between 0 and 100 into the corresponding box. Your investment shares for both companies need to add up to 100 in each question (you cannot "save" your money).

1. (Certainty)
Company A: return 5 %.
Company B: return 2 %. Additionally, it donates the certain amount of 2% to a charitable institution.
How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

2. (Certainty, equal returns)
Company A: Return 5 %.
Company B: Return 2.5 %. Additionally, it donates the certain amount of 2.5 % to a charitable institution.
How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

3. (Risk baseline)

Company A: average return 5 %.
Company B: average return 2 %. Additionally, it donates the certain amount of 2% to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	0%	4%	6%	10%
Company B	1%	1.5%	2.5%	3%

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

4. (Risk, same variance)
Company A: average return 5 %.
Company B: average return 2 %. Additionally, it donates the certain amount of 2% to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	4%	4.5%	5.5%	6%
Company B	1%	1.5%	2.5%	3%

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

5. (risk baseline, negative correlation)
Company A: average return 5 %.
Company B: average return 2 %. Additionally, it donates the certain amount of 2% to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	0%	4%	6%	10%
Company B	3%	2.5%	1.5%	1%

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

6. (Risk, same variance, negative correlation)
Company A: average return 5 %.
Company B: average return 2 %. Additionally, it donates the certain amount of 2% to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	6%	5.5%	4.5%	4%
Company B	1%	1.5%	2.5%	3%

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

7. (Risk baseline, risky externality)
Company A: average return 5 %.
Company B: average return 2 %. Additionally, it donates a mean of 2% to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	0%	4%	6%	10%
Company B	1.5% for you 1.5% charity	1.75% for you 1.75% charity	2.25% for you 2.25% charity	2.5% for you 2.5% charity

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

8. (Risk, same variance, risky externality)
Company A: average return 5 %.
Company B: average return 2 %. Additionally, it donates a mean of 2 % to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	4%	4.5%	5.5%	6%
Company B	1.5% for you 1.5% charity	1.75% for you 1.75% charity	2.25% for you 2.25% charity	2.5% for you 2.5% charity

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

9. (Risk, same return and variance, risky externality)
Company A: average return 5 %.
Company B: average return 2.5 %. Additionally, it donates a mean of 2.5% to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	4%	4.5%	5.5%	6%
Company B	2% for you 2% charity	2.25% for you 2.25% charity	2.75% for you 2.75% charity	3% for you 3% charity

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

10. (Risk baseline, negative correlation, risky externality)
Company A: average return 5 %.
Company B: average return 2 %. Additionally, it donates a mean of 2% to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	0%	4%	6%	10%

Company B	2.5% for you 2.5% charity	2.25% for you 2.25% charity	1.75% for you 1.75% charity	1.5% for you 1.5% charity

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

11. (Risk, same variance, negative correlation, risky externality)
Company A: average return 5 %.
Company B: average return 2 %. Additionally, it donates a mean of 2% to a charitable institution.

	state 1	state 2	state 3	state 4
Company A	6%	5.5%	4.5%	4%
Company B	1.5% for you 1.5% charity	1.75% for you 1.75% charity	2.25% for you 2.25% charity	2.5% for you 2.5% charity

How would you like to invest your budget? Please give relative percentage shares.

☐ Company A % ☐ Company B %

6 Concluding remarks

This thesis has focused on a prominent economic feature of the global economy: State ownership of commercial firms. I have argued that state ownership is not only widespread in emerging markets such as China but also in the mature economies of the OECD and the EU. In this context, the novel results of the three content chapters seem of particular relevance because they highlight so far neglected corporate finance aspects of state ownership, which also have implications for public finance due to the interaction of ownership and taxation.

The thesis has looked at three novel research topics that arise from the fact that state ownership is not neutral in the sense that it changes corporate incentives and perceptions, which in turn affect corporate finance decisions. The first two look at tax-related aspects of state ownership and the third focuses on the cost of finance as well as the public-good nature often associated with state ownership.

The first topic looks at tax-related debt incentives of mixed-ownership firms. Mixed ownership refers to affiliates that are partially state-owned and partially owned by MNEs. The Chapter extends previous research on tax-responsiveness of debt, which argues that firms use debt to lower their tax burdens. Is state ownership relevant in this context? Yes, because it changes the incentives of private co-owners. Also, governments are often inefficient in monitoring state companies, which gives private co-owners a substantial managerial levy. Whereas purely private firms face a trade-off between tax payments (depending on the tax rate) and debt-related bankruptcy risk, mixed-ownership firms use very high debt levels irrespective of the tax rate because they rely on implicit state guarantees for that debt. The primary incentive that determines debt levels is the state's participation in the venture, and not the tax rate. As government monitoring is weak, managers and private co-owners exploit this situation and adjust the capital structure to their needs, i.e., at a maximum level. This result has substantial implications for governments: High debt-levels lower tax payments and thus avoid unilateral returns to governments. At the same time, high debt-levels increase the return on invested equity. While this is a good thing for private co-owners, the state may receive fewer distributions due to the tax loss. Thus, contrary to the government's investment purpose, it may lose money from mixed-ownership firms. The bottom line from this content Chapter is that commercial participation in mixed firms may be less beneficial for the government as it believes them to be.

The second topic compares effective tax rates of wholly owned commercial SOEs with those of private firms. In comparison to the first content chapter, Chapter 4 deals with a binary ownership situation between wholly owned subsidiaries. Besides, the research question borrows from the accounting literature because it uses the effective tax rate as the dependent variable, which is a widely used accounting measure. State ownership matters in this context because – ultimately – the state controls both its firms and the tax authority. The political view on state ownership suggests a conflict of interests in the sense that governments could use taxation as a means to subsidize their companies. However, econometric results do not support this hypothesis. Instead, effective tax rates of SOEs are higher than those of private firms. Two main arguments arise to explain this finding: The first is that governments extract distributions via tax payments to cover their budgetary needs. The second is that managers of wholly owned SOEs are arguably more indifferent to tax payments then managers of private (or mixed) firms, which could lead to lower levels of tax planning in SOEs. Again, state ownership is not neutral but leads to robust differences in corporate finance metrics between SOEs and private firms. While governments surely welcome higher tax payments from their companies, this may adversely affect their competitiveness and investment levels, at least in the absence of investment tax breaks. The bottom line from this Chapter is that state ownership is not tax neutral in the EU, which has not achieved its regulatory goal of competitive tax neutrality.

The third topic takes a closer look at the cost of finance of SOEs, which are substantially lower than those of private companies. Starting from the conventional explanation that state ownership induces implicit guarantees, the Chapter provides evidence that company risk is not related to the cost of SOE debt, whereas it increases the financing cost of private firms. Thus, it confirms previous findings on the existence of implicit state guarantees using a much bigger sample of non-listed companies. Nevertheless, results indicate that implicit guarantees are unlikely to be the only explanatory factors for this difference. By referring to the social view of state ownership, the Chapter explores whether the framing of societal benefits caused by state ownership induces a bias in society towards this ownership type. Experimental results point in this direction: Participants consistently invest in a project mimicking an SOE that offers inefficient returns but yields a societal benefit. Hence, behavioral arguments could be an additional explanation for lower financing costs of SOEs. This finding is especially relevant in the light of the persistently worse performance of SOEs: Investors (and more generally –

citizens) may tolerate weaker book performance of state firms because they deduce societal benefits from this ownership type. The bottom line from this last Chapter is that human opinion and prejudice may affect the perception of corporate ownership and could lead to an efficiency loss for overall society.

Summing up, this thesis contributes to the empirical literature on state ownership, capital structure, taxation, and debt financing. It makes use of the social, political and agency view on state ownership and uses them to formulate hypotheses on relevant and so far unaddressed research questions that relate to corporate taxation and financing. Combined results across the content chapters indicate that state ownership is not neutral in the sense that it changes incentives and perceptions of firms, which are reflected in financial statement data. A relevant normative question, though, remains difficult to answer: Should governments own commercial companies? Politicians may answer this question affirmatively by pointing out how state ownership yields better services and more jobs to constituents than profit-maximizing private ownership. They may bear in mind both market imperfections and political agendas, and like the power that state firms can give them. In the light of this doctoral thesis, regulators should think twice: State ownership will not always lead to the expected outcome and comes with many strings attached. As this thesis has shown, such strings are present in the fields of corporate capital structure, tax payments, and the cost of finance – all of which are likely to distort competition. In the EU, we can, therefore, draw one conclusion with near certainty: If regulators aspire towards competitive neutrality for all companies, commercial state ownership distorts this policy goal, no matter how much regulation is passed to create a level playing field.

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