Norwegian School of Economics

Bergen, Fall 2017

NHH

The Impact of Governments on the Performance of State-Owned Enterprises: Evidence from Norway

An empirical study of the government effect on the performance differential between state- and privately owned enterprises

Thomas Scheele Berg

Magnus Bjærum Kvidal

Supervisor: Johan Mellberg

Master thesis in Financial Economics

NORWEGIAN SCHOOL OF ECONOMICS

i. Abstract

This thesis investigates how the performance differential between state-owned enterprises (SOEs) and privately owned enterprises (POEs) is affected by different governments. Theory suggests that through their ability to elect the Board of Directors, the government can influence corporate governance or even utilize SOEs as vehicles to promote political objectives. By using the performance differential as a measure of the extent of deviation from profit maximizing behavior, we compare governments prior to and after three elections in Norway. By examining these elections separately, the research robustness is increased.

Historically, SOEs have not been selected randomly, and generally differ significantly from POEs. Consequently, we apply inverse probability of treatment weighting (IPTW) using propensity scores in order to construct a comparable control group. Using the weighted control group, we have utilized a difference-in-differences model specification to compare the governments. Visual inspections suggest that SOEs and the weighted POEs exhibit parallel trends prior to treatment and we resultantly argue that the results can be interpreted causally.

The results show no evidence that any of the three government changes caused a significant change in the profitability of SOEs compared to POEs. The small treatment group contributes to big standard errors of the coefficients, making it unlikely that we would retrieve statistically robust results even if there actually existed a causal relationship.

1. Introduction

The intention of this study is to shed light on how different governments affect the performance of state-owned enterprises (SOEs) that are said to be profit maximizing. SOEs are indirectly owned by the people, and it is inherently challenging for them to measure a government's ability to run an SOE. This lack of monitoring can cause the incentives of government representatives to be unaligned with the interests of the general population. For instance, previous research has found that governments pursue political objectives that increase their chances of being re-elected on the expense of the welfare of the people in general. We argue that increased access to information regarding how well SOEs perform under different governments will increase the accountability of government officials, and hopefully contribute to aligning their incentives to those of the population.

How SOEs perform compared to POEs has been widely researched with a substantial base of the research indicating that POEs are superior in terms of profitability and productivity. Surprisingly, empirical research on the reasons for this performance differential is limited. Both empirical and theoretical studies suggest that SOEs in general suffer from poorer corporate governance or that the government utilizes SOEs to fulfil certain political objectives - like an attempt to increase employment. As a natural extension to this, it seems reasonable to hypothesize that some governments are more eager to exploit the opportunity of fulfilling certain political objectives or are poorer at corporate governance than others, resulting in reduced performance.

Although the privatization wave of the 80s and 90s has slowed down, some of the biggest companies in several countries remain fully or partly state-owned. One such country is Norway, where privatization and competitive tendering of public services are still widely debated topics, and are among the most polarizing topics between the left- and right wing in the country's politics. The right-wing parties are in general pro privatization whilst the left-wing parties are against. Also, the different parties have different views on how SOEs should be run. For instance, the Conservative Party (2017) states that they will: "Ensure, equal treatment of private and public companies when SOEs operate in an open market", whilst the Labour Party (2017) states that they will: "Ensure moderation in management salary". Further, a Labour Party led government restricted SOEs from using management option programs in 2006 (Norwegian Ministry of Trade, Industry and Fisheries, 2014). Although it is beyond the scope of this paper to go into the details of the party platforms, there is little doubt that these two parties to some extent have opposing views on how SOEs should be run, with the left-wing emphasizing political objectives.

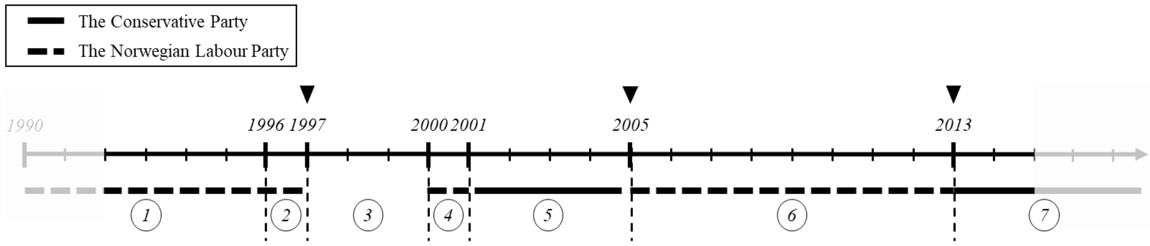
According to previous theoretical studies on public choice theory, the performance differential should increase as the political objectives imposed by the government increase. As the right-wing parties have a more liberal approach to how SOEs should be run, we hypothesize that they should cause a smaller performance differential than the left-wing parties. Similarly, the agency theory supports this view as the left-wing parties wish to impose salary constraints on management in SOEs.

There are two main reasons to why Norway is highly suitable for investigating the research question. Firstly, there are two dominating and opposing political parties where one, and only one, of these has been represented in every government except one since 1992. Second, state ownership remains common despite numerous examples of privatization over the past few decades. According to Norman, Reve and Roland, SOEs' share of value added in Norway is the highest among all countries in the EU and European Economic Area (as cited in Goldeng et al., 2008). A substantial proportion of the value creation from SOEs originates from the oil industry, but they are also present in many other industries. The presence of SOEs across a wide range of industries allows us to find a large number of comparable companies that compete in the same markets. This paper includes both enterprises that are fully state-owned and partially state-owned in the term SOE. A more thorough description of the selection of SOEs is presented in section 5.3. See table VIII in appendix for an overview of SOEs included in the study by industry.

To perform a proper analysis, the ruling period of the government both prior to and after the election should be of some length. Of the six government changes in this period, there are three that fit the research question. The first is the government change that took place in 1997. After seven years of a pure Labour Party minority government, it was replaced in 1997 by a minority government consisting of the Christian Democratic Party, the Centre Party and the Liberal Party which lasted until 2000. The second government change suitable for this study happened in 2005. Prior to this election there had been a minority government since 2001, comprised by the Conservative Party, the Christian Democratic Party and the Liberal Party. This government was followed by eight years of a majority government comprised by the Labor Party, the Socialist Left Party and the Centre Party. The final government change included in the analysis took place in 2013, when a minority government comprised by the

Conservative Party and the Progress Party was formed. Figure I shows the timeline of governments from 1992 to 2015, the period in which we have data.

Figure I. Timeline of Norwegian Governments, 1990-2017



Period		*Government*	*Represented parties*
1.	Nov 1990 – Oct 1996	Gro H. Brundtland's third gov.	Labour Party
2.	Oct 1996 – Oct 1997	Thorbjørn Jagland's gov.	Labour Party
3.	Oct 1997 – Mar 2000	Kjell Magne Bondevik's first gov.	Chrisitian Democratic Party, Centre Party, Liberal Party
4.	Mar 2000 – Oct 2001	Jens Stoltenberg's first gov	Labour Party
5.	Oct 2001 – Oct 2005	Kjell Magne Bondevik's second gov.	Conservative Party, Liberal Party, Christian Democratic Party
6.	Oct 2005 – Oct 2013	Jens Stoltenberg's second gov.	Labour Party, Centre Party, Socialist Left Party
7.	Oct 2013 -	Erna Solberg's gov.	Conservative Party, Progress Party

The figure shows a timeline of the governments in the period 1990-2017. The governments are marked by numbers below the line. The triangles above the line highlight the elections that are relevant for the research question.

Section 2 provides a brief historical review of SOEs and the state's position as an owner of enterprises in Norway. In section 3, we present various theoretical explanations for the performance differential between POEs and SOEs, as well as for variations in this performance differential. Section 4 briefly presents the highlights from previous research on the topic. In section 5, the collection and processing of the data is presented. The experimental design is described in section 6, before the results are presented and discussed in section 7. Section 8 concludes the study, discusses limitations of the methodology and results, and provides recommendations for further research on the topic.

2. Background

Historically, state investments and divestments in companies have been fueled by several political and economic motives, meaning that it is not random which companies that are state-owned. Resultantly, SOEs inhibit certain traits that deviate drastically from the average POE, formed by the objectives of the ownership. Understanding the underlying reasons for why states have invested and divested in certain companies is necessary in order to understand the potential selection issues. Norway has a long history of state ownership, but the state has partly privatized many of its companies through public listings. However, the government still takes part as an active owner, especially through its decisive power at the general assemblies, and hence in appointing the Board of Directors.

During the 1800s and 1900s, state ownership was widely adopted with the ambition to increase investments in public services (Millward, 2005; Toninelli, 2000). Although this was the main purpose of state ownership, governments have also used acquisitions of new firms as a fiscal policy tool with the ambition of stimulating the economy during economic downturns (Rajan & Zingales, 2004). This has contributed to the existence of SOEs in industries way beyond public services. Many of these SOEs have suffered under inadequate management and having to cope with a variety of political objectives (Shirley & Nellis, 1991).

The trend of increasing state ownership was abruptly reversed in the early 1980s as a result of privatization programs initiated by Britain's Thatcher government (Megginson & Netter, 2001). According to Price Waterhouse (1989), as referred to in Megginson and Netter (2001), the objectives of the privatization initiated by the Thatcher government was to; raise revenue for the state, promote economic efficiency, reduce government interference in the economy, promote wider share ownership, provide the opportunity to introduce competition, and subject SOEs to market discipline. The perceived success of the British privatization and economic turmoil, i.e. oil shocks, credit rationing and increasing interest rates, sparked a privatization wave reaching most industrialized countries (Megginson & Netter, 2001; Megginson, 2005). Throughout the 80s and 90s, the height of the privatization wave, there was a significant reduction in the number of SOEs in most countries (Toninelli, 2000; Sheshinski & Lopez-Calva, 2003). Nonetheless, many governments have maintained some of the largest SOEs under their control by keeping minority stakes in these companies (Bortolotti & Faccio, 2009; Capobianco & Christiansen, 2011; OECD, 2005).

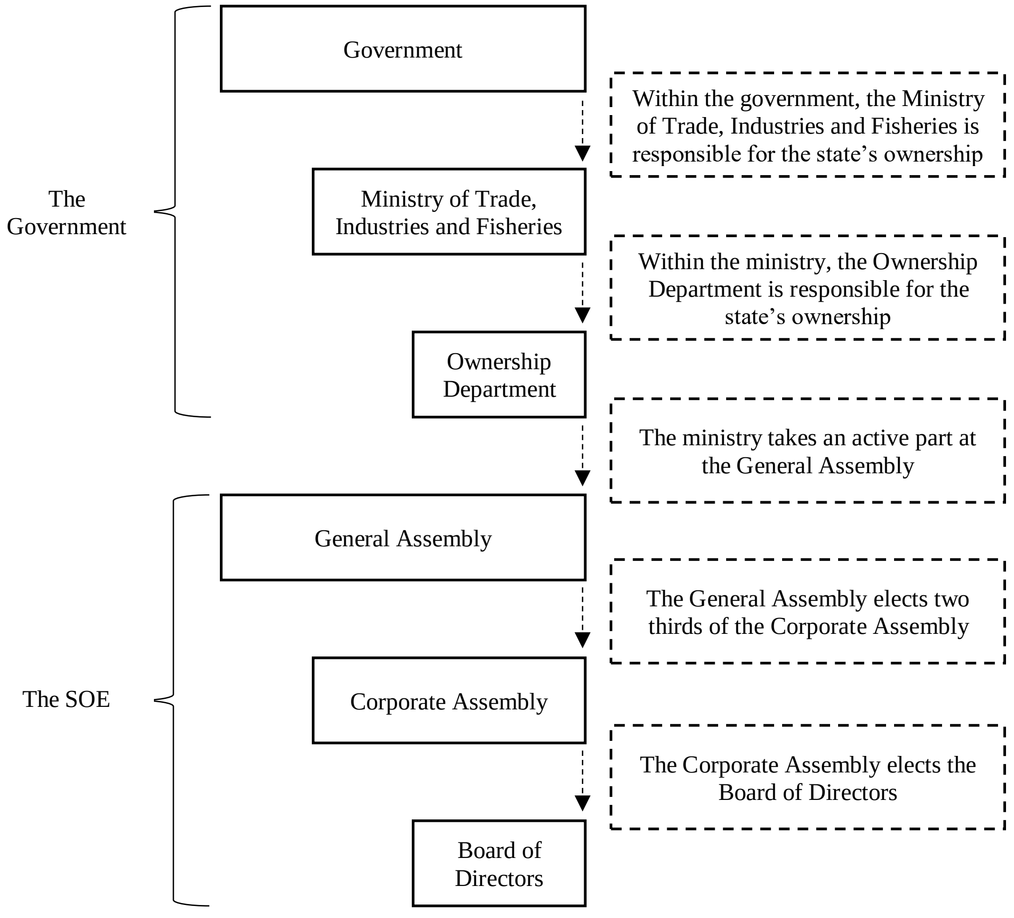
As outlined, the history clearly shows that SOEs have not been randomly selected, with purposes ranging from resolving market failures to achieving certain political objectives. We argue that the political background, and often origin, of SOEs substantiates the hypothesis that SOEs performance can be affected by the government's view on utilizing SOEs for pursuing political objectives. Further, the use of SOEs as political instruments can explain why SOEs can consistently underperform, and still survive.

Most of the SOEs with commercial objectives in Norway are owned through the Ministry of Trade, Industry and Fisheries. Within the political staff, the highest ranked person regarding ownership questions is the Minister of Trade and Industry. The department within the ministry that is responsible for the ownership is the Ownership Department. Among their responsibilities are; (1) contribute to a suitable board composition, (2) contribute to good corporate governance, (3) strategic, analytic and economic follow-up, and (4) follow-up with regard to corporate social responsibility (Norwegian Ministry, Trade and Fisheries, 2017a). It seems clear that the government's execution of these responsibilities could affect the performance of the SOEs.

The responsibility firstly mentioned, contributing to a suitable board composition, is especially important as there is widespread agreement in the literature that the composition of the board may affect the performance of the company[1]. The general assembly elects two thirds of the corporate assembly, which in turn elects the Board of Directors. In most of the companies in which the state holds shares, it holds enough shares to have an effective majority at the general assemblies. Hence, the state may theoretically compose the board in the SOEs as it pleases. For an illustration of the way in which the government formally controls the SOEs, see figure II below.

[1] See for instance Bhagat and Black (1999), Baysinger and Butler (1985), and Perry and Shivdasani (2005).

Figure II. The Norwegian Government's Formal Channel of Influence on SOEs



3. Theory Behind State-Owned Enterprises

Theoretical approaches to investigating state and private ownership has primarily revolved around agency/property rights theory and public choice theory (Villalonga, 2000). Most of the research has been based on the agency/property rights theories, but also discussions based on public choice theory has reached the same overall conclusion; privately owned firms should be more efficient than state-owned firms.

Property rights theory argues that firms with passive owners are likely to have principal-agent problems making them less efficient and less profitable than owner-controlled firms (Furubotn & Pejovich, 1972). The fact that government officials are representatives for the population in general and not direct owners themselves, makes the property rights much more ambiguous in SOEs than in POEs. In addition, these officials are often poorly monitored, which weakens their incentives to improve performance compared to a direct owner and should in theory implicate that SOEs are less efficient than POEs (Alchian, 1965).

The proponents of the public choice theory argue that politicians pursue maximization of their own utility rather than the public's best interest, as the costs of monitoring politicians are too high for the general public (Villalonga, 2000). SOEs can for instance be misused as mechanisms to support governmental pet projects and help climbing politicians (Schleifer & Vishny, 1994). If state ownership does in fact increase deviations from profit maximizing behaviour, the performance of SOEs is likely to suffer (Andrews & Dowling, 1998; Boycko, Shleifer, & Vishny, 1996; Djankov & Murrell, 2002). Recent empirical studies have found support for the public choice theory. For instance, it has been shown that state owned banks increase their lending relative to private banks in emerging markets during election years (Dinc, 2005). Also, Carvalho (2014) found that governments try to increase employment during election years by providing favourable lending from state-owned banks, and Moita and Paiva (2013) found evidence that governments force down prices taken by SOEs in regulated industries during election years.

Similarly, Lazzarini and Musacchio (2015) find that the performance differential between SOEs and POEs increase during election years and economic downturns, when politicians are more tempted to interfere with SOEs. In other words, a considerable amount of research has found that SOEs alter behaviour according to political objectives. The public choice theory substantiates our claim that the performance differential could vary as a result of the government's propensity to pursue political goals on the expense of profit-maximization. In addition, it has been shown that the pursuit of political objectives in addition to commercial objectives complicates the creation of well-functioning incentive contracts (Bai & Xu, 2005; Firth, Fung, & Rui, 2006).

Even though the literature generally concludes that POEs outperform SOEs, it also contains several arguments to why SOEs might be better performers than POEs. It is argued that one of the main benefits of SOEs is that state capital can be more patient than private capital (Beuselinck, Cao, Deloof, & Xia, 2017; Borisova, Brockman, Salas, & Zagorchev, 2012). Furthermore, partial state equity can help SOEs pursue profitable projects when faced with scarce access to capital and other institutional constraints with first-rate access to governmental resources (Inoue, Lazzarini, & Musacchio, 2013; Vaaler & Schrage, 2009).

The theoretical question of interest for this thesis is whether the differences between POEs and SOEs also vary between governments. The personal utility maximization as proposed by public choice theory is likely to be more prevalent among some politicians than others. It is also possible that the politics of some parties to a larger extent allows for such behaviour, making it more prevalent among politicians from certain parties. Likewise, it is a reasonable possibility that politicians from some parties are better at corporate governance, reducing the principal/agent problems that can occur as anticipated by property rights theory. This substantiates our hypothesis that the performance differential is likely to vary between governments.

4. Literature

As a natural response to the privatization wave, the virtues and shortcomings of SOEs compared to POEs have been widely researched over the last two decades, both empirically and theoretically. In most studies, the conclusion has been that SOEs generally underperform compared to POEs. However, meta-reviews conducted by for instance Villalonga (2000), and Shirley and Walsh (2000) find that results to some extent are mixed. Researchers have utilized a variety of different techniques and samples, and it is therefore not surprising that conclusions to some degree are differing. Many of the studies also suffer from considerable weaknesses regarding methodology, especially in creating samples of POEs and SOEs that are comparable. This section will present the findings of the most important literature in the area and evaluate the robustness of their methods.

A meta-review conducted by Villalonga (2000) finds that of the 153 studies previously conducted, 104 are in favor of POEs, 14 are in favor of SOEs, and 35 are neutral. Similarly, Shirley and Walsh (2000) surveyed 52 privatization studies whereof 32 indicate that POEs outperform SOEs, 15 were inconclusive and 5 indicate that SOEs outperform POEs. An interesting side note to this finding is that all of the 5 studies that are in favor of SOEs were based on analyses of monopoly firms in the utility sectors (Villalonga, 2000; Goldeng et al., 2008). Compared to majority SOEs, firms with minority state investment have been relatively understudied (Lazzarini & Musacchio, 2015).

Many empirical studies have found that SOEs are inferior to private companies in terms of efficiency due to lack of incentives and poor monitoring (Boardman & Vining, 1989; Dharwadkar, George, & Brandes, 2000; La Porta & Lopez-De-Silanes, 1999). However, there are methodical issues with most of the studies in the area. Prior to Boardman and Vining (1989), most studies compared the performance of SOEs and POEs that were either natural monopolies, operating in a regulated duopoly or produced products that could not be priced by competitive forces (Boardman & Vining, 1989). Hence, they were unable to investigate the highly interesting question of performance differences in a competitive environment.

Boardman and Vining (1989) compare the performance of purely state-owned enterprises, mixed enterprises (MEs) and POEs among the 500 largest non-U.S. industrial firms, as compiled by Fortune magazine in 1983. The result of their study is consistent with their hypothesis; the performance of POEs is significantly superior to the performance of both MEs and SOEs. For all profitability measures, MEs perform no better, and often worse, than SOEs. However, in terms of sales per employee, MEs perform significantly better than SOEs.

The findings of Boardman and Vining (1989) have been supported by Vining and Boardman (1992), and Dewenter and Malatesta (2001), which utilize similar approaches but with different samples. Even though these studies do not limit their samples to companies operating in monopolies or duopolies, there are clear methodological weaknesses. Due to omitted variable bias, there is no reason to believe that SOEs and POEs are comparable, and the research is often prone to selection bias in the sense that there may be unobserved fundamental reasons why some companies are state-owned, and these reasons also affect performance.

Goldeng et al. (2008) use a comprehensive panel data set containing accounting information for all registered Norwegian companies in the period 1990 to 1999. They find that SOEs perform significantly better than POEs. Depending on the selection criteria applied, POEs typically achieve a ROA which is 8 to 10 percentage points higher than that of the SOEs. Also in this study there are weaknesses in the methodology. Similar to previous studies, the study uses a simple sample selection criterion relying solely on the presence of SOEs in different industries. We argue that this approach suffers from selection bias as it matches companies on industry only, whilst in reality, SOEs and POEs differ on a much wider range of characteristics. Additionally, the authors exclude all companies from certain industries that they assume not to be profit maximizing. However, SOEs that are not profit maximizing occur in variety of industries, also in industries that are not excluded. We will later in the paper describe how we alleviate these issues using IPTW and explicit selection of profit-maximizing SOEs.

In almost all research conducted in the area, including for instance Boardman and Vining (1989), and Goldeng et al. (2008), there are issues regarding endogeneity. There are fundamental reasons why some companies are state-owned, and one such reason is to what extent there is market failure in the particular industry (Megginson & Netter, 2001). The factors affecting the ownership type will in many cases also affect company performance (Megginson & Netter, 2001). Hence, it is difficult to separate the ownership effect on performance. Kole and Mulherin (1997) address this problem specifically by studying a sample of U.S. subsidiaries of German and Japanese companies in which the U.S. government seized 35-100 percent of the outstanding common stock following World War II. As a result, the U.S. government set the corporate policy and elected the management for a period ranging from 1 to 23 years for the companies in question. The authors find no significant differences in accounting performance between the vested companies and a control group. Kole and Mulherin (1997) tie this to the fact that there is little evidence of governments trying to affect investment decisions of the vested firms.

A surprisingly low share of the existing literature has examined the reasons for the performance differential between SOEs and POEs, and how the differential develops over time. One of the few examples of this is a study by Lazzarini and Musacchio (2015) concluding that a general performance gap is not universally present. Creating a cross-country and cross-industry sample of listed SOEs, and comparing this to a control group of listed POEs, the authors find that significant performance gaps occur when the environment changes in such a way that the company needs to adjust their operations. The authors identify two such changes; election years and years of economic downturn. The authors find that after an economic crisis, SOEs' performance decreases relatively more than it does for POEs. Further, they find that majority SOEs underperform in election years in developing countries for all their performance measures except ROA and TFP. The authors suggest that among possible reasons for this performance gap, are attempts by the government to increase employment and to force SOEs to keep prices low.

To our knowledge there is no existing literature on how different governments within a country affect the performance of SOEs. Authors like Dinc (2005), Carvalho (2014), and Moita and Paiva (2013) have all conducted studies comparable to the one of Lazzarini and Musacchio (2015), but these only examine whether performance changes at the time of election. In other words, they do not study whether the subsequent government affects SOEs differently than the previous government.

In previous literature two primary approaches to measure performance related to corporate governance have been used; accounting-based and market-based indicators (Al-Matari, Al-Swidi, & Fadzil, 2014). According to Hutchinson and Gul (2004), and supported by Al-Matari et al. (2014), accounting-based performance measures are preferred when the relationship between corporate governance and firm performance is examined. A meta study by Al-Matari et al. (2014) shows that ROA is by far the most used measure of performance in the corporate governance literature.

One of the primary criticisms against the use of ROA as a performance measure is the backward-looking nature of this measurement. However, for the purpose of this study, this is a desirable trait as we would not want the market's expectations of future performance under other governments to affect the observed performance. Further, our sample consists primarily of companies where market values are not available. May, Yozzo and Regan (2001) emphasize that ROA fails to account for the relative risk of companies. As an example, they point out that specialty stores typically achieve higher ROA than discount stores, but that specialty stores also are associated with greater operating risk due to their narrow assortment. However, to account for this, one would have to use a market-based indicator which is not available for most of the companies. Conclusively, many factors point towards ROA as the most suitable measure of performance for this study.

5. Data

5.1 Background

At the core of answering this paper's research question lies a complete data set of all Norwegian companies from 1992 to 2015 provided by Samfunns- og næringslivsforskning AS (SNF). In 2015 a total of 300,413 companies were included in the database. The rare opportunity of having access to practically the entire population of the country's companies further strengthens our view that Norway is a highly suitable base for investigating the research question.

In most countries, Norway included, the principle of historical cost accounting has been dominating, and fair value adjustments following acquisitions have rarely been made (Goldeng et al., 2008). The advantage of this accounting principle is objectivity and verifiability, but on the other hand, these accounting values do not necessarily reflect a company's true economic performance for a given year. Many writers have warned that rates of return based on accounting data are of low economic significance and can be misleading measures of profitability (Brief and Lawson, 1992). On the other hand, there are obvious benefits of using accounting values. Firstly, it allows us to use the comprehensive data sets provided by SNF. Second, there is no reason to believe that SOEs and POEs are systematically different in terms of accounting practice. Hence, the use of accounting values should not cause any bias.

Data has been delivered to SNF annually from Brønnøysundregistrene through Bisnode D&B Norway AS in cooperation with Menon Business Economics AS (Berner, Mjøs, and Olving, 2016). Due to inconsistencies and changes in accounting policies, SNF has conducted standardization and quality control of the data. The data is structured in 24 annual financial statements files, 24 annual consolidated financial statement files and 24 annual files containing company information. The company information includes a number of company characteristics such as the legal status, ownership type, municipal location and industry.

In the process of compiling these data we have removed all observations that are not present in both an accounting file and the company information file. Additionally, it is necessary to remove certain variables in order to reduce the need for computational power. Please see the following sections for further details on data compiling.

The compiled data set covers all public and private Norwegian companies in the period from 1992 to 2015. This period contains three changes in accounting rules (Berner, Mjøs, and Olving, 2016). The act relating to annual accounts from 1998 was largely a continuation of previous standards, but with a higher degree of detail. Further, IFRS was implemented from 2005, where fair value accounting is used to a larger extent. This paper intends to investigate the response of the performance differential between SOEs and POEs as governments change, and we argue that there should be no systematic differences in how SOEs and POEs within the same industries are affected by such accounting changes. Hence, we will utilize the full time span of data. Further details on the processing and quality controls conducted by SNF can be found in Berner, Mjøs, and Olving (2016).

All the variables used are shown in table I below, including a description and the source.

Table I. Variables Used in the Study

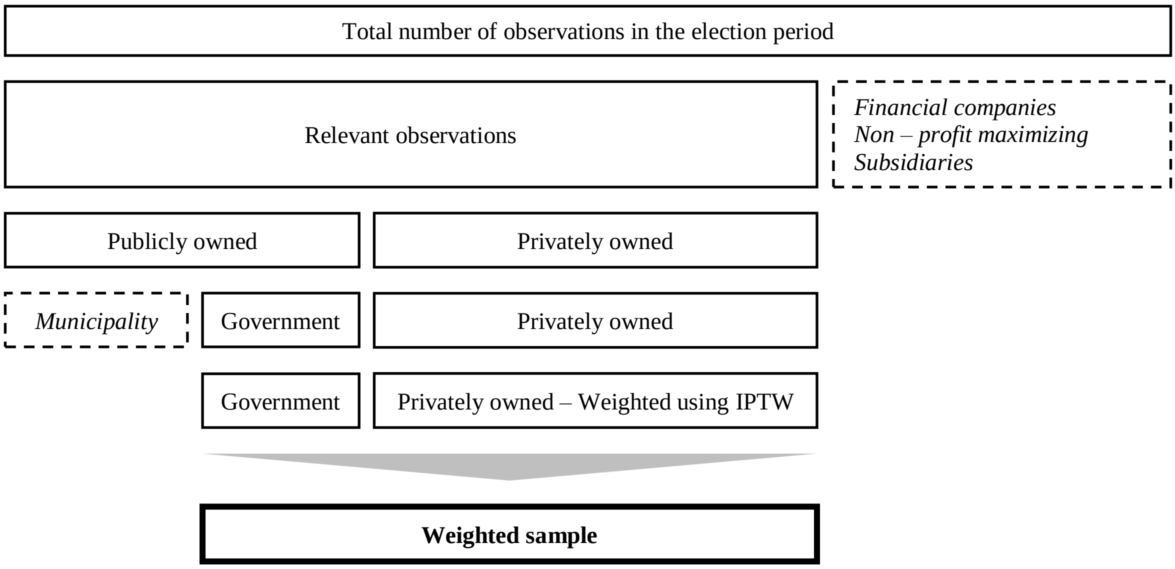
Variable name	**Definition**	**Source**
Age	Number of years since the company was established	SNF: aar, stiftaar
Employees	Number of employees	SNF: ansatte
HHI	Herfindahl Index. Calculated as the sum of the squared market shares by industry based on 5-digit NACE code	SNF: bransjek_07
Ifrs	Equal to one if the company follows IFRS and zero if not	SNF: ifrs
Incominggovernment	Equal to zero for the government prior to the election and one for the government after the election	The Norwegian Government, 2013
Industry	Norwegian 2-digit NACE industry code. Due to change to from sn2002 to sn2007 in 2008, the sn2002 codes has been extrapolated until 2015	SNF: bransjek_02_2s[1]
Location	Geographic region dummies	SNF: landsdel
Marketshare	Share of sales by 5-digit NACE code, sn2007 is extrapolated backwards by SNF	SNF: bransjek_07
Publiclisting	Equal to 1 if the company is listed and zero if not	SNF: bors_aks
ROA	Return on Assets. Operating income divided by average assets in the period	SNF: driftsrs, sumeiend
ROS	Return on Sales. Operating income divided by average sales in the period	SNF: driftsrs, totinn
SOE	The treatment variable. Equal to one if the state holds more than 30% ownership in the company	Various whitepapers[2]
Totassets	Total assets	SNF: sumeiend
Totincome	Total sales	SNF: totinn
Weight	The weight attributed to a company in the regression. Calculated using the IPTW formula	See section 5.4
Year	Accounting year	SNF: aar

1) On the 2-digit NACE level SNF has not conducted backward extrapolation of branjsek_07_2s. Backward extrapolation of bransjek_07_2s would result in missing industry codes for all companies that is not in the dataset after 2008. Hence we have extrapolated bransjek_02_2s from 2009 to 2015 in order to keep the loss of observations to a minimum.
2) (Norwegian Ministry of Trade and Industry, 2002), (Norwegian Ministry of Trade and Industry, 2006), (Norwegian Ministry of Trade and Industry, 2011) and (Norwegian Ministry of Trade, Industry and Fisheries, 2017b). For further details, see, the reference list.

5.2 Removing Irrelevant Observations

The full dataset must be curtailed before it can be used for any thoughtful analysis. The purpose of the sample reduction is primarily to increase the comparability of the SOEs and POEs, but also to remove and fix errors in the data. Figure III illustrates the process of creating the samples for each election.

Figure III. The Sample Construction Process



The figure illustrates how the full sample has been narrowed down to the final weighted sample. Starting with the full sample at the top, we remove the observations with dashed outlines before we weight the privately owned companies using IPTW, resulting in the final sample.

We want to ensure that all companies have a set of purely commercial objectives. We have therefore dropped all observations that are not limited companies, e.g. cooperatives, sole proprietorships etc., from the POE sample. Further, all companies from the financial sector have been dropped due to different accounting standards, resulting in low comparability on our preferred performance measures. Companies with sales revenue below NOK 2.5 million are also dropped, as these are more prone to reporting errors. Finally, all obvious input errors have been altered.

There has also been conducted a number of smaller changes in the dataset. We have removed all subsidiary companies and used consolidated statements for the ultimate owners. Consolidated statements are considered more useful than the separate financial statements when the individual companies are related, and the consolidated statements provide the best means of obtaining a clear picture of the total resources and performance that are under the control of a parent company (Baker, Lembke, & King, 2004).

5.3 Constructing the Treatment Group

Enterprises fully and partly owned by the state are identified using various white papers from the government to the Parliament and State Ownership Reports from the government[2]. Combined, these reports compile all relevant state-owned enterprises. By relevant enterprises we mean enterprises in which the state has considerable influence. We have therefore put a lower limit of the state's ownership at 30 percent of outstanding shares as a criterion. For most companies, an ownership of 30 percent or more gives an effective majority of voting rights at general assemblies. We have also included Aker Solutions ASA, despite the state's indirect ownership of less than 30 percent. The reason for this is that the state bought shares in its holding company Aker Kværner Holding AS with the purpose of preventing the companies in the group from being sold abroad. Thus, the state obviously believes its shares gives it considerable influence.

Our measures of performance are purely quantitative. Hence, it is only meaningful to include SOEs with the primary purpose of profit maximization. As a result of this, we only include enterprises that by the government are categorized with (1) solely commercial objectives (e.g. Entra ASA and Mesta AS) and (2) commercial objectives and objective of maintaining head office functions in Norway (e.g. Telenor ASA and Yara International ASA). To track the government's ownership share in the companies we use annual reports and the Norwegian Centre for Research Data's database "Forvaltningsdatabasen". Please see table VIII in appendix for a complete list of SOEs used in the study.

Previous studies have excluded a number of industries in an attempt to reduce the presence of SOEs that are not profit maximizing, e.g. health care and theatrical services. We argue that this approach is prone to error and provides no guarantee that all non-profit maximizing companies are excluded. In our case, this is not an issue as we have explicitly selected only the SOEs that are profit maximizing as stated by the government in their State Ownership Reports.

[2] (Norwegian Ministry of Trade and Industry, 2002), (Norwegian Ministry of Trade and Industry, 2006), (Norwegian Ministry of Trade and Industry, 2011) and (Norwegian Ministry of Trade, Industry and Fisheries, 2017b). For further details, see, the reference list.

Another important remark is that we are only interested in companies that are owned by the state, and not municipalities or counties. Our hypothesis is that the government mainly influences the SOEs through the Board of Directors, on which they do not have influence if a municipality or county is the owner. In such cases it is the municipal council or county council that appoints the board, and these councils are formed as a result of the municipal elections. Hence, these companies are not relevant and all such observations are excluded.

Table II presents summary statistics for each election year by group. When examining POEs and SOEs, it is apparent that these groups are widely different. There are considerable differences in size, Herfindahl index (HHI) and market share to mention some. Hence, a performance comparison between these groups would be futile as potential results might originate from any differences between these groups in any of the mentioned dimensions. In order to mitigate any bias that might arise from this issue, we apply inverse probability of treatment weighting (IPTW) using propensity scores to construct a comparable control group.

Table II. Summary Statistics of Treatment and Control Group Prior to Weighting

	1997				2005				2013			
	Mean	Std. dev.	Min	Max	Mean	Std. dev.	Min	Max	Mean	Std. dev.	Min	Max
Full sample												
ROA	0.132	0.198	-0.982	0.999	0.128	0.193	-0.982	0.996	0.104	0.184	-0.977	0.994
Assets	66	2,027	0	121,538	100	3,726	0	284,828	128	6,766	0	885,600
Sales	80	2,214	3	142,910	91	4,222	3	393,718	92	4,857	3	637,400
HHI	0.081	0.123	0.005	1.000	0.095	0.164	0.003	1.000	0.091	0.143	0.004	1.000
Market size	169,604	192,895	3	398,397	43,259	60,320	3	411,170	71,762	100,032	3	644,891
Market share	0.023	0.094	0.000	1.000	0.013	0.063	0.000	1.000	0.009	0.060	0.000	1.000
Age	13.08	11.83	3.00	133.00	16.79	15.37	3.00	152.00	17.56	13.91	3.00	160.00
N	7,545				10,845				18,593			
State-Owned Enterprises												
ROA	0.034	0.078	-0.150	0.139	0.130	0.101	0.001	0.365	0.110	0.100	-0.091	0.284
Assets	19,119	40,813	13	121,538	59,354	102,677	69	284,828	98,931	232,835	150	885,600
Sales	19,792	43,816	18	142,910	59,344	123,032	172	393,718	69,185	167,284	324	637,400
HHI	0.480	0.459	0.021	1.000	0.575	0.407	0.009	1.000	0.604	0.340	0.067	1.000
Market size	133,764	178,657	58	398,397	88,604	129,424	322	411,170	91,674	173,192	589	644,891
Market share	0.498	0.477	0.000	1.000	0.626	0.419	0.007	1.000	0.621	0.370	0.006	1.000
Age	20.21	24.27	3.00	92.00	23.18	27.15	5.00	100.00	24.14	25.86	5.00	108.00
N	14				11				14			
Privately Owned Enterprises												
ROA	0.133	0.198	-0.982	0.999	0.128	0.194	-0.982	0.996	0.104	0.184	-0.977	0.994
Assets	30	751	0	58,258	39	773	0	74,609	53	714	0	52,115
Sales	44	933	3	73,554	30	546	3	55,304	40	656	3	67,442
HHI	0.080	0.121	0.005	1.000	0.094	0.163	0.003	1.000	0.090	0.142	0.004	1.000
Market size	169,671	192,925	3	398,397	43,213	60,205	3	411,170	71,747	99,964	3	644,891
Market share	0.022	0.090	0.000	1.000	0.012	0.059	0.000	1.000	0.009	0.057	0.000	1.000
Age	13.07	11.79	3.00	133.00	16.78	15.36	3.00	152.00	17.56	13.90	3.00	160.00
N	7,531				10,834				18,579			

Assets, Sales and Market Size are given in million NOK. Age is given in years. Min and Max show the minimum and maximum values in the sample. Std.dev shows the standard deviation of the observations. Means shows the unweighted mean of the observations.

5.4 Constructing a Comparable Control Group - Inverse Probability of Treatment Weighting

As previously discussed, SOEs have not been selected randomly throughout history, resulting in major differences between SOEs and POEs. This is a critical concern for this study as it entails that the SOEs, among other differences, are over-represented within industries providing public goods, and are typically much larger than the average POE. These differences suggest that the two groups may be affected differently by a number of omitted variables, meaning that they might not exhibit parallel trends in the absence of treatment. Therefore, the full sample of POEs is not a suitable counterfactual for SOEs, and it would be likely to violate the main assumption of parallel trends in the difference-in-differences model. In order to handle the issue of a non-comparable control group we have applied inverse probability of treatment weighting (IPTW) using propensity scores in order to construct a synthetic control group.

The IPTW algorithm is based on the propensity scores which are calculated in the year prior to each election. The propensity score is the estimated probability of being in the treatment group given a number of firm characteristics. The propensity scores are derived from the following logit model:

$$\ln\left(\frac{p_i}{1-p_i}\right) = b_0 + b_1 HHI_i + \ b_2 age_i + b_3 ROS_i + \sum_{l=1}^{3} \omega_l \, \Delta_l ROA_i + \sum_k \delta_k \, industry_{ki}$$

Where p_i is the probability of company i being an SOE, and $\Delta_l$is the change in the l^{th} year prior to the election.

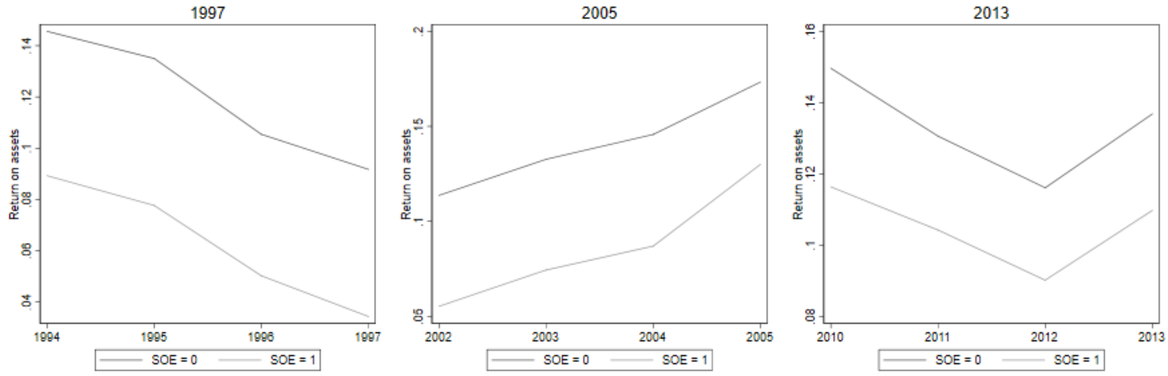
Next, the propensity scores are used to calculate the observation weights using the IPTW formula:

$$w_{ATT} = Z + \frac{e(1-Z)}{(1-e)}$$

Where Z is a dummy variable equal to 1 for SOEs and 0 for POEs, and e is the propensity score.

As apparent from the formula, all treated observations are attributed a weight of 1 and untreated observations are weighted according to their propensity scores, where high propensity scores yield a large weight and vice versa. By assigning weights to all initial observations according to their similarity to SOEs we construct a synthetic control group that share similar traits as the SOEs. The unweighted control group and SOEs exhibit very different trends[3], but from the figure IV it is evident that the weighted POE sample and SOEs have highly parallel trends. Since all treated observations are attributed a weight of 1, this formula gives the average treatment effects on the treated (ATT) estimate, as the treatment group is used as the reference population to which the controls are compared (Austin & Stuart, 2015).

Figure IV. Trends in ROA Prior to Elections for the Weighted Samples



Regarding the selection of variables for the propensity model, it is suggested to include variables that affect the outcome variable (ROA), rather than variables that affect the treatment selection, i.e. whether the company is an SOE (Brookhart et al., 2006; Stuart, 2015). The rationale behind this is that the object of the weighting is to balance covariates that are prognostically important for ROA. For instance, company size has very high explanatory power on $p(SOE = i)$ compared to the other variables. Resultantly, including size will practically result in matching on size alone, with very little improvements in variables that are more important determinants of ROA. We argue that other variables, such as return on sales, HHI and industry, are more important determinants of trends in ROA, and size has therefore not been included.

For the choice of algorithm and implementation of the weighting process we have followed the guidance presented by Caliendo and Kopeinig (2008). From this process, we found that

[3] See figure VIII in appendix for ROA trends when weights are not applied

IPTW is the most suitable algorithm for our sample due to its ability to reduce bias without sacrificing too much efficiency. Further, a comparison of pre-election trends using the different matching algorithms further substantiates the use of IPTW. Please see section 10.2 in appendix for further details related to this process.

According to Austin and Stuart (2015), causal inference using the propensity score requires four assumptions: Consistency, exchangeability, positivity, and no misspecification of the propensity score model. Some of these assumptions can be tested using a number of balancing diagnostics where we have used standardized bias (SB) and visual inspection of covariate distributions.

The SB, as first introduced by Rosenbaum and Rubin (1985), compares the mean of selected covariates between treatment and control groups. An issue with the SB method is that there is no rule regarding how much imbalance that is accepted, but maximum allowed SB tends to range from 10 to 25 percent in most recent research. The formula for the SB is shown below, as presented by Austin (2011).

$$d = \frac{(\overline{x}_{treatment} - \overline{x}_{control})}{\sqrt{\frac{s^2_{treatment} - s^2_{control}}{2}}}$$

"Where $\overline{x}_{treatment}$ and $\overline{x}_{control}$ denote the sample mean of the covariate in treated and untreated subjects, respectively, whereas $s^2_{treatment}$ and $s^2_{control}$ denote the sample variance of the covariate in treated and untreated subjects, respectively" (Austin, 2011, p. 412).

For dichotomous variables, one can define the standardized difference as presented below. This is used to compute the standardized difference for the industry variable.

$$d = \frac{(\hat{p}_{treatment} - \hat{p}_{control})}{\sqrt{\frac{\hat{p}_{treatment}(1 - \hat{p}_{treatment}) + \hat{p}_{control}(1 - \hat{p}_{control})}{2}}}$$

"Where $\hat{p}_{treatment}$ and $\hat{p}_{control}$ denote the prevalence or mean of the dichotomous variable in treated and untreated subjects, respectively. The standardized difference compares the difference in means in unites of the pooled standard deviation" (Austin, 2011, p. 412).

Table III presents the improvements in the SB. The full sample is highly biased with five to six out of the six selected variables having an SB above 25 % in the three periods. We observe that the use of IPTW using propensity scores drastically reduces the bias in most of the relevant variables. However, the inability to remove the bias in terms of size and market size is strikingly clear. According to Austin (2011) such biases might be due to misspecifications of the propensity score model, but after a thorough analysis we conclude that the reason for this issue is that there simply is a lack of POEs with the same size as the SOEs within their respective industries. Please see section 10.2 in appendix for details on the visual inspection.

Table III. Standardized Biases Prior to and After Weighting

		1997			2005			2013		
		Mean			Mean			Mean		
		Treated	Control	SB	Treated	Control	SB	Treated	Control	SB
Age	U	20.21	13.07	37.5	23.18	16.78	29.0	24.14	17.56	31.7
	M	20.21	19.03	**6.2**	23.18	18.03	**23.4**	24.14	22.95	**5.8**
Industry	U	51.21	56.71	-24.7	43.55	56.24	-61.2	41.21	54.58	-75.1
	M	51.21	47.19	**18.1**	43.55	45.62	**-10.0**	41.21	41.08	**0.8**
HHI	U	0.48	0.08	119.2	0.58	0.09	155.2	0.60	0.09	197.1
	M	0.48	0.44	**11.1**	0.58	0.53	**13.9**	0.60	0.60	**2.8**
Assets	U	19,119	30	66.1	59,354	39	81.7	98,931	53	60.1
	M	19,119	132	65.8	59,354	394	81.2	98,931	757	59.6
Market size	U	133,764	169,671	-0.193	88,604	43,213	45.0	91,674	71,747	14.1
	M	133,764	76,097	31.0	88,604	60,553	27.8	91,674	51,355	28.5
Market share	U	0.50	0.02	138.7	0.63	0.01	205.4	0.62	0.01	231.0
	M	0.50	0.28	62.9	0.63	0.07	185.0	0.62	0.14	181.2

The letter U marks the unweighted samples, and M marks the weighted samples. Assets, Market size and market share are stated in million NOK. SB shows the standardized bias and is reported in percent. Industry is calculated using a 2-digit NACE code, whilst market share is based in the 5-digit NACE code. Treated shows the SOE group whilst control shows the POE group.

The remaining differences between the SOEs and POEs could suggest that the groups might react differently to an endogenous shock, meaning that the assumption of parallel trends in the absence of treatment might not hold. However, we argue that the differences in size primarily has an impact on the level of the outcome variable due to economies of scale, but that the trends of companies with different size should not differ by much. Unfortunately, we have not found any previous research that can confirm or refute this assumption, and resultantly our research might be prone to a bias. However, as illustrated in figure IV, the fact that the treatment and control group are parallel in the period prior to treatment is a strong indication that the weighting has been successful and the main assumption of the difference-in-differences model is satisfied. For a comparison of summary statistics between SOEs and POEs after weighting, please see table IV.

Table IV. Summary Statistics of Treatment and Control Group After Weighting

	1997				2005				2013			
	Mean	Std. dev.	Min	Max	Mean	Std. dev.	Min	Max	Mean	Std. dev.	Min	Max
Full sample												
ROA	0.064	0.145	-0.982	0.999	0.151	0.162	-0.982	0.996	0.123	0.160	-0.977	0.994
Assets	9,293	28,927	0	121,538	30,943	76,397	0	284,828	49,844	166,093	0	885,600
Sales	9,612	30,940	3	142,910	30,822	89,478	3	393,718	34,838	119,062	3	637,400
HHI	0.461	0.417	0.005	1.000	0.554	0.391	0.003	1.000	0.600	0.357	0.004	1.000
Market size	103,920	165,011	3	398,397	75,086	117,532	3	411,170	71,515	145,964	3	644,891
Market share	0.387	0.444	0.000	1.000	0.359	0.431	0.000	1.000	0.381	0.419	0.000	1.000
Age	19.60	22.37	3.00	133.00	20.70	21.85	3.00	152.00	23.54	22.06	3.00	160.00
N	7,545				10,845				18,593			
State-Owned Enterprises												
ROA	0.034	0.078	-0.150	0.139	0.130	0.101	0.001	0.365	0.110	0.100	-0.091	0.284
Assets	19,120	40,813	13	121,538	59,355	102,677	69	284,828	98,931	232,835	150	885,600
Sales	19,792	43,816	18	142,910	59,344	123,032	172	393,718	69,185	167,284	324	637,400
HHI	0.480	0.459	0.021	1.000	0.575	0.407	0.009	1.000	0.604	0.340	0.067	1.000
Market size	133,764	178,657	58	398,397	88,604	129,424	322	411,170	91,674	173,192	589	644,891
Market share	0.498	0.477	0.000	1.000	0.626	0.419	0.007	1.000	0.621	0.370	0.006	1.000
Age	20.21	24.27	3.00	92.00	23.18	27.15	5.00	100.00	24.14	25.86	5.00	108.00
N	14				11				14			
Privately Owned Enterprises												
ROA	0.092	0.184	-0.982	0.999	0.173	0.209	-0.982	0.996	0.137	0.204	-0.977	0.994
Assets	132	849	0	58,258	394	2,182	0	74,609	757	3,402	0	52,115
Sales	122	867	3	73,554	155	1,450	3	55,304	490	2,232	3	67,442
HHI	0.443	0.390	0.005	1.000	0.532	0.392	0.003	1.000	0.597	0.383	0.004	1.000
Market size	76,097	152,884	3	398,397	60,553	109,013	3	411,170	51,355	118,084	3	644,891
Market share	0.282	0.402	0.000	1.000	0.073	0.236	0.000	1.000	0.141	0.330	0.000	1.000
Age	19.03	21.36	3.00	133.00	18.03	16.02	3.00	152.00	22.95	18.74	3.00	160.00
N	7,531				10,834				18,579			

Assets, Sales and Market Size are given in million NOK. Age is given in years. The maximum and minimum values are not weighted, and are hence the same as for the unweighted sample. The mean values are weighted according the IPTW using propensity scores.

6. Experimental Design

Throughout this section we describe the experimental design utilized to compare the performance of different governments. Our empirical strategy is based on comparing consecutive governments pairwise, by applying a difference-in-differences model. We argue that this design creates a good foundation for uncovering the governments effects on the performance differential. The temporal proximity of the compared governments is not strictly necessary, but it increases the likelihood that the treatment and control group remain comparable throughout the relevant period. Please see figure I in section 1 for a timeline that highlights the elections that separate every pair of governments compared.

6.1 Rationale Behind Choice of Methodology

The main challenge of estimating the government effect on SOE performance is controlling for the practically unlimited number of other factors that may affect their performance. A common strategy for overcoming this issue is to implement the difference-in-differences (DiD) method. As a result of the government change, the SOEs get new owners whilst POEs do not. Hence, the POEs can be used as a control group that capture all variables affecting firm performance except the change of government. Followingly, this is a natural experiment where the SOEs get treated by switching owners, while the POEs remain untreated. As we have argued, the differences between SOEs and the weighted sample of POEs are assumed to be time-invariant, meaning that the data are well suited for a DiD research design. For a more thorough review of the DiD model, see section 6.2 and section 10.1 in appendix.

The average treatment effects (ATE) measure the effect of the treatment if SOEs were randomly selected (Caliendo and Kopeinig, 2008). We argue that this is unlikely to be the case, and therefore not of relevance to this study as it would measure the government effect on companies that would never be state-owned. For instance, we are not interested in measuring how venture companies would be affected by the different governments if they hypothetically had been SOEs. Thus, the most relevant evaluation parameter for the purpose of this study is the average treatment effects on the treated (ATT), which focus explicitly on the companies that are directly affected by changes in government, namely the SOEs. The standard DiD specification yields the ATT (Athey and Imbens, 2006). This means that our estimate will measure the effect of different governments on state-owned companies that exhibit the typical traits of SOEs, e.g. high number of employees, high market share et cetera.

6.2 Specification of the Difference-in-Differences Model

As the intention of this study is to investigate how different governments fulfill their role as a company owner, we want to employ performance measures that reflect the benefits to the owners. There might be systematic differences in leverage between SOEs and POEs, and we therefore consider ROA to be a more suitable measure of performance than return on equity. Other measures, such as profit margin, might create a substantial bias as this varies considerably across industries with the same level of competition as well as between different strategies within the same industry. ROA resolves this issue to a large extent, as industries with low profit margins tend to have higher turnover and vice versa.

Assuming that POE performance does not get affected by government changes, we can estimate changes in SOE performance by studying the differences between SOE and POE performance over time. An important decision is the determination of which years that should be attributed to which government. The implementation of new policies is likely to take some time, and it is also likely that the effect on ROA of these implementations will not be immediate. As a response to this, we choose to lag the effect of the government change by two years in the main model. Practically, the lag will only be a bit longer than one year, as most government changes take place at the end of a year. This means that for the government change that took place in 2005, the performance of SOEs in both 2005 and 2006 is attributed to the outgoing government, whilst 2007 will be the first year attributed to the incoming government. Finally, we restrict any outgoing or incoming period from including more than four years in the main model. The allocation of years to each government is presented below in table V.

Table V. Researched Government Changes, Showing Government Periods and Years Attributed to Each Government in the Main Model

Month of	Outgoing government		Incoming government		Years attributed to	
gov. change	Name	Period	Name	Period	Outg. gov.	Inc. gov.
Oct. 1997	Brundtland III / Jagland	Nov. 1990 - Oct. 1997	Bondevik I	Oct. 1997 - Mar. 2000	1995-1998	1999-2001
Oct. 2005	Bondevik II	Oct. 2001 - Oct. 2005	Stoltenberg II	Oct. 2005 - Oct. 2013	2003-2006	2007-2010
Oct. 2013	Stoltenberg II	Oct. 2005 - Oct. 2013	Solberg	Oct. 2013 -	2011-2014	2015

The effect of a government change on the ROA of SOEs for each election is examined separately for each election using the model specified below. As shown in table V, we examine three government changes. In the main model, we use observations from 1995 to 2001 for the government change in 1997. For the government change in 2005, we use observations from 2003 to 2010. Observations from 2011 to 2015 are used for the government change in 2013. Also, we test supplementary models with different estimation windows and lags to increase robustness.

The synthetic control group is created based on the weights obtained through the IPTW using propensity scores as described in section 5.4. These weights are utilized through the use of a weighted ordinary least squares (WOLS) regression, where the residual sum squares, contrary to the OLS method, are weighted (DeMaris, 2004). Hence, the POEs that share similar traits with the SOEs get emphasized more. According to Winship and Radbill (1994), the use of WOLS is preferred over OLS when weights are a function of the dependent variable. We use ROA as the dependent variable in the regression model, and lagged ROA variables in the PSM model used to obtain the weights. Therefore, we argue that WOLS should provide consistent estimates of true regression slopes.

The model specification for the main model is presented below.

$$\sqrt{w_i} * ROA_{it} = \sqrt{w_i} * [\beta_0 + \beta_1 SOE_{it} + \sum_t \theta_t \, year_t + \beta_2 incominggovernment_{t-2} * SOE_{it}$$
$$+ \sum_k \delta_k \, industry_{ki} + \sum_l \theta_l \, location_{li} + \beta_3 totincome_{it} + \beta_4 totincome\hat{\ }2_{it}$$
$$+ \beta_5 totassets_{it} + \beta_6 employees_{it} + \beta_7 ROS_{it} + \beta_8 publiclisting_{it} + \beta_9 ifrs_{it}$$
$$+ \beta_{10} marketshare_{it} + \beta_{11} HHI_{it} + u_{it}]$$

Where i is an index over firms, t is an index over time, k is an index over industries and l is and index over locations. For a general explanation of the variables, please see table I in section 5.1.

The interaction term $incominggovernment_{t-2} * SOE_{it}$ takes the value of 1 for companies that are state-owned in year t and the incoming government was governing at least in parts of year $t - 2$. Hence, the coefficient β_2 is the coeffecient of interest in this model, showing the effect of the incoming government on ROA for SOEs.

Although we have limited the differences between the treatment and control group using IPTW, it is still strongly recommended to adjust for covariates in the regression equation to capture any remaining differences (Stuart et al., 2014). We have therefore included a set of control variables. The industry dummy $industry_{ki}$ is included because ROA historically has varied substantially across industries. To account for any regional differences, the dummy variable $location_{li}$ is included. $totincome_{it}$ and $totassets_{it}$ are included to capture any differences in ROA with regard to size. Because the relationship between size and ROA is likely to be non-linear we include the squared terms of these variables, denoted $totincome\hat{}2_{it}$ and $totassets\hat{}2_{it}$ respectively.

We include return on sales to capture the effect of the strategy choice between high volume and high margin on ROA. There may also be systematical differences in the performance between listed and non-listed companies, and we therefore include the dummy variable $publiclisting_{it}$. To account for differences in accounting standards, we include the dummy variable $ifrs_{it}$. Market share and HHI are included because they indicate the company's relative competitive positioning and the competitive environment.

The error term is denoted u_{it}. As we use panel data, the error term can be assumed to consist of three parts, as presented below.

$$u_{it} = \epsilon_i + \epsilon_t + \epsilon_{it}$$

The first part of the error term, ϵ_i, is time-invariant but varies across the companies. An example of this would be that company i for some reason experiences unexplainable high ROA, but that it does so continuously. The second error term, ϵ_t, varies over time but is the same for all companies. This would be any factor affecting ROA that varies over time, but affects all the companies equally. The third error term, ϵ_{it}, varies both over time and companies. In other words, it affects only company i at time t.

As we have panel data, we want to use either fixed effects (FE) or random effects (RE). RE models are shown to be more efficient than FE models. However, the RE model cannot be used if there is correlation between the explanatory variables and the unobserved individual specific effect. FE models do not rely on this condition. We argue there is likely to be some correlation between the explanatory variables and the unobserved individual specific effect. There is almost an endless series of factors that might affect ROA and are part of the individual specific effects. To assume that none of these are correlated with any explanatory variables would be unacceptable. Intuitively, we therefore argue a FE model to be most appropriate.

To evaluate the consistency of the RE estimator compared to the less efficient FE estimator, we also conduct a Hausman test as specified below.

$$H = (\widehat{\beta_{FE}} - \widehat{\beta_{RE}})'[Var(\widehat{\beta_{FE}}) - Var(\widehat{\beta_{RE}})]^{-1}(\widehat{\beta_{FE}} - \widehat{\beta_{RE}})$$

The estimator H is asymptotically chi-squared with a number of degrees of freedom equal to the rank of the matrix $Var(\widehat{\beta_{FE}}) - Var(\widehat{\beta_{RE}})$. The null hypothesis is that the RE and FE estimators are equal. The alternative hypothesis is that the two estimators are unequal, and in this case one would conclude that the RE estimator is inconsistent and choose the FE model. For all the government changes, the test clearly indicates that the two estimators are unequal, supporting our view of using a FE model.

The use of FE will result in the dummy variable SOE being omitted from the regression, as it will be colinear with the firm fixed effects. Due to the interaction term, the SOE variable does only represent the difference between SOEs and POEs in the period prior to treatment. In other terms, it symbolizes the baseline difference between the groups. In a DiD model, such baseline differences can be considered as noise, as we are primarily interested in the changes in the performance differential. Hence, we argue that the inclusion of fixed effects, and resultant omitting of the SOE variable, is not problematic for our research question because the interpretation of the interaction term will remain unchanged.

Further, we apply clustered standard errors as proposed by White (1984), which allow for intragroup correlation, relaxing the usual requirement that observations must be independent. This is recommended in much of the recognized literature on the topic, such as Thompson (2011), and Cameron and Miller (2015). Hence, we account for correlation and heteroscedasticity on the company level.

To increase the robustness of the analysis, we test additional models with different length in the lag of the government change effect and estimation window. In the first supplementary model, the length of the lag is set to one year instead of two years as in the main model. In the second supplementary model, we restrict any outgoing or incoming period from including more than 6 years, contrary to 4 years as in the main model.

7. Results

The estimates based on the main model are presented in table VI. As we are mainly interested in the government effect, coefficients of dummy variables related to industry, location and year are not presented in the table. The model named 1997 reports regression estimates for ROA based on observations from year 1995-2001. The model named 2005 reports regression estimates for ROA based on observations from year 2003-2010. The last model, the model named 2013 reports regression estimates for ROA based on observations from year 2011-2015.

The interaction term, e.g. Bondevik1*SOE, Stoltenberg1*SOE and Solberg*SOE, is not significantly different from zero in any of the three regressions. Hence, we have no evidence that these government changes caused any significant change in the profitability of SOEs compared to POEs. The insignificant coefficients of the interaction terms are 1.9, 4.9 and 4.3 percentage points for the government changes, respectively. These coefficients would unarguably be high if they showed the true causal effect. If the Stoltenberg I government actually caused SOEs to increase their ROA with 4.9 percentage points relative to POEs compared to the Bondevik II government, it would be sensational. When coefficients of this size are not significant, it indicates that the model is not very efficient. In other words, due to the small treatment group it is unlikely that a government change effect would be statistically significant even if it did exist.

The dummy for being publicly listed is significant at the 5 percent level in both the 1997 and 2005 model, but with different signs. Total income and the squared term of total income are not both significant at the 5 percent level in neither of the models, and the same is the case for total assets. The small treatment group and use of weights in the control group may be a reason for the inconsistencies across the models.

Higher ROS is significantly associated with a higher ROA in all of the models. However, the size of the coefficient varies from 0.036 in the 2005 model to 0.375 in the 2013 model. For the 2013 model this indicates that an increase in ROS of 1 percentage point is associated with an increase in ROA of 0.375 percentage points. Higher market share is only significantly associated with higher ROA in the 2013 model, while the Herfindahl index is not significant at the 5 percent level in neither of the models. Due to the IPTW's high ability to match on many of the control variables, a large amount of variation in these variables are removed. Hence, it is not very surprising that they are not always significant.

The results of the supplementary models are mostly consistent with the main model, and are presented in table IX and X in the appendix. All coefficients of the treatment effect are close to the ones from the main model and have the same signs. As in the main model, none of these coefficients are significant at the 5 percent level. However, the Stoltenberg1*SOE coefficient is significant at the 10 percent level in the supplementary model presented in table X, which has an extended estimation window. Hence, one can argue that there is some weak evidence of the Stoltenberg 1 government causing higher performance than the Bondevik II government. Nevertheless, we believe that the extended estimation window adds potential sources of error to the model, and also argue that a 10 percent significance level is not sufficient.

Table VI. Main Model. Weighted Ordinary Least Squares (WOLS): Difference-in-Differences Estimation of the Effect of Government Change. Dependent Variable: Return on Assets

	Year of government change		
	1997	2005	2013
Constant	0.004 (0.050)	0.063 (0.067)	0.001 (0.037)
Publicly listed	-0.082*** (0.031)	0.097** (0.039)	0.012 (0.033)
IFRS		-0.091 (0.062)	0.042 (0.044)
No. of employees (thousand)	0.0022 (0.0016)	-0.0007 (0.0183)	-0.013 (0.0090)
Total income (NOKm)	-0.0024* (0.0014)	0.0007 (0.0009)	0.0025** (0.0011)
Total income (NOKm) squared	1.54e-11*** (4.46e-12)	7.27e-14 (8.53e-13)	-1.79e-12* (1.02e-12)
Total assets (NOKm)	0.0033* (0.0018)	0.0005 (0.0010)	-0.0017* (0.0010)
Total assets (NOKm) squared	-1.54e-11** (6.12e-12)	-1.09e-12 (6.75e-13)	7.17e-13 (5.30e-13)
Return on sales	0.091*** (0.033)	0.036** (0.017)	0.375*** (0.081)
Market share	0.024 (0.042)	-0.045 (0.062)	0.100** (0.044)
Herfindahl index	0.034 (0.043)	0.048* (0.028)	0.023 (0.039)
Bondevik1*SOE	**0.019** **(0.021)**		
Stoltenberg1*SOE		**0.049** **(0.034)**	
Solberg*SOE			**0.043** **(0.042)**
Firm FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Estimation window	1995-2001	2003-2010	2011-2015
Lag of gov. change effect (years)	2	2	2
R^2	0.16	0.13	0.31
N	45,917	70,174	88,073

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors are in parentheses. Dummy for company being an SOE is omitted due to collinearity with firm FEs. Dummies for industries, locations and years are not included in the table.

8. Concluding Remarks

8.1 Summary

Even if the state's role as an owner in many of Norway's biggest enterprises is highly debated, we find no solid evidence that different governments in the past 20 years have affected the performance differential between SOEs and POEs differently. Due to the relatively small number of SOEs with solely commercial objectives, the standard errors in our analysis are quite high. Hence, it is unlikely that the analysis would reveal a statistically significant relationship even if it existed. The fact that the supplementary models give treatment coefficients very similar to those of the main model indicates that the results are not caused by a model misspecification.

As the analysis finds no evidence of a government effect on SOE performance, a natural conclusion is that it is whether the company is state-owned that affects performance, and not which government that represents the state.

8.2 Limitations of Results and Methodology

8.2.1 Inverse Probability of Treatment Weighting

The experimental design relies heavily on the assumption that the performance differential between POEs and SOEs would stay constant in absence of a government change. We have applied IPTW using propensity scores in order to create a synthetic control group with similar traits as the SOEs. This exercise yielded a control group that moves in parallel to the SOEs prior to treatment. However, we wish to highlight the inherent lack of comparable POEs in the population. For instance, it is impossible to find Norwegian companies that are directly comparable to Statoil and Telenor. Followingly, our attempts to create a perfectly balanced sample has been futile and the results cannot with certainty be considered unbiased, as indicated by table III in section 5.4. However, we argue that the achieved bias reduction is sufficient.

IPTW and other methods using propensity scores rely on a number of assumptions. We have tested the balancing of observed covariates, but not all of the assumptions are testable. Propensity score methods are criticized for the impossibility of testing the assumption of balancing of unobserved covariates (Thoemmes and Ong, 2015). Finally, any model using propensity scores might suffer from a misspecified propensity score model.

8.2.2 Small Treatment Group

As this is a natural experiment, the study gets limited by the amount of data that exists. A treatment group ranging from 11 to 14 companies implies that the change in performance differential must be large for making statistical inference. In the main model the standard errors of the treatment effect are as high as 2.1-4.2 percent. A higher number of relevant SOEs would of course have made the uncertainty of the treatment effects decrease.

8.2.3 Implementation Time and Results of New Policies

In the main model, we have applied a two-year lag. For a government change in 2005, the new government is held accountable for the performance first in 2007. We have also examined an alternative one-year lag model. However, it is reasonable to assume that the implementation time of different policies will vary. Hence, the policies of a government may not affect SOEs' ROA before the government has changed again. This could lead to a bias and make the models inaccurate. One example is that the Stoltenberg II government restricted SOEs from having option based bonus programs for the management. However, they had to respect contracts that already existed. Hence, in some companies the existing option programs lasted for several years after the new policy was decided.

8.3 Suggestions for further research

In this study we have restricted the empirical analysis to differentials in performance in terms of profitability. Examining other performance measures, such as productivity would clearly be of interest. One could also suspect SOEs of having several objectives in addition to the pure commercial ones, despite claiming otherwise. This may lead to externalities, potentially both positive and negative ones, and examining these would be a highly relevant topic for further research. Not only are such externalities of relevance in a direct comparison between POEs and SOEs, but it is also possible that different governments affect these externalities differently.

Further research should examine the relationship between governments and SOE performance in other countries than Norway. Especially countries where the political wings differ much in their view on corporate governance of SOEs would be of high relevance. Even though we argue that Norway, for reasons discussed in section 1, is highly suitable for the analysis, we acknowledge that that other countries also may be suitable for such an analysis.

Most empirical research has indicated that POEs are superior to SOEs in terms of performance. However, an interesting question to research would be how the performance differential between SOEs and POEs has developed over time. The answer to this question seems quite open.

9. References

Alchian, A. A. (1965). Some Economics of Property Rights. *II Politico, 30*(4), 816-829.

Al-Matari, E. M., Al-Swidi, A. K., & Fadzil, F. H. (2014). The Measurements of Firm Performance's Dimensions. *Asian Journal of Finance & Accounting, 6*(1), 24-49. doi:10.5296/ajfa.v6i1.4761

Andrews, W. A., & Dowling, M. J. (1998). Explaining Performance Changes in Newly Privatized Firms. *Journal of Management Studies, 35*(15), 601-617. doi:10.1111/1467-6486.00111

Angrist, J. D., & Pischke, J.-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton, NJ: Princeton University Press.

Athey, S., & Imbens, G. W. (2006). Identification and Inference in Nonlinear Difference-in-Differences Models. *Econometrica, 74*(2), 431-497. doi:10.1111/j.1468-0262.2006.00668.x

Austin, P. C. (2011). An Introduction to Propensity Score Methods for Reducing the Effects of Confounding in Observational Studies. *Multivariate Behavioral Research, 46*(3), 399-424. doi:10.1080/00273171.2011.568786

Austin, P. C., & Stuart, E. A. (2015). Moving towards best practice when using inverse probability of treatment weighting (IPTW) using the propensity score to estimate causal treatment effects in observational studies. *Statistics in Medicine, 34*(28), 3661-3679. doi:10.1002/sim.6607

Bai, C.-E., & Xu, L. C. (2005). Incentives for CEOs with multitasks: Evidence from Chinese state-owned enterprises. *Journal of Comparative Economics, 33*(3), 517-539. doi:10.1016/j.jce.2005.03.013

Baker, R. E., Lembke, V. C., & King, T. E. (2004). *Advanced Financial Accounting.* New York, NY: McGraw-Hill Companies.

Baysinger, B. D., & Butler, H. N. (1985). Corporate Governance and the Board of Directors: Performance Effects of Changes in Board Composition. *Journal of Law, Economics, & Organization, 1*(1), 101-124. doi:10.1093/oxfordjournals.jleo.a036883

Berner, E., Mjøs, A., & Olving, M. (2016). *SNF Publications.* Retrieved from SNF: http://www.snf.no/Default.aspx?ID=148&cat=1&mode=detail&RowId=PubID%3d21714&ViewPID=136&PubID=21714

Beuselinck, C., Cao, L., Deloof, M., & Xia, X. (2017). The value of government ownership during the global financial crisis. *Journal of Corporate Finance, 42*, 481-493. doi:doi.org/10.1016/j.jcorpfin.2015.05.002

Bhagat, S., & Black, B. S. (1999). The Uncertain Relationship Between Board Composition and Firm Performance. *Business Lawyer, 54*(3), 921-963. doi:10.2139/ssrn.11417

Boardman, A. E., & Vining, A. R. (1989). Ownership and Performance in Competitive Environments: A Comparison of the Performance of Private, Mixed, and State-Owned Enterprises. *The Journal of Law and Economics, 32*(1), 1-33. doi:10.1086/467167

Borisova, G., Brockman, P., Salas, J. M., & Zagorchev, A. (2012). Government Ownership and Corporate Governance: Evidence from the EU. *Journal of Banking & Finance, 36*(11), 2917-2934. doi:10.2139/ssrn.1533854

Bortolotti, B., & Faccio, M. (2009). Government Control of Privatized Firms. *The Review of Financial Studies, 22*(8), 2907-2939. doi:doi.org/10.1093/rfs/hhn077

Boycko, M., Shleifer, A., & Vishny, R. W. (1996). A Theory of Privatisation. *The Economic Journal, 106*(435), 309-319. doi:10.2307/2235248

Brief, R. P., & Lawson, R. A. (1992). The Role of the Accounting Rate of Return in Financial Statement Analysis. *The Accounting Review, 67*(2), 411-426.

Brookhart, M. A., Schneeweiss, S., Rothman, K. J., Glynn, R. J., Avorn, J., & Stürmer, T. (2006). Variable Selection for Propensity Score Models. *American Journal of Epidemiology, 163*(12), 1149-1156. doi:10.1093/aje/kwj149

Caliendo, M., & Kopeinig, S. (2008). Some Practical Guidance for the Implementation of Propensity Score Matching. *Journal of Economic Surveys, 22*(1), 31-72. doi:10.1111/j.1467-6419.2007.00527.x

Cameron, A. C., & Miller, D. L. (2015). A Practitioner's Guide to Cluster-Robust Inference. *Journal of Human Resources, 50*(2), 317-372. doi:10.3368/jhr.50.2.317

Capobianco, A., & Christiansen, H. (2011). Competitive Neutrality and State-Owned Enterprises: Challenges and Policy Options. *OECD Corporate Governance Working Papers*(1). doi:10.1787/5kg9xfgjdhg6-en

Carvalho, D. (2014). The Real Effects of Government-Owned Banks: Evidence from an Emerging Market. *The Journal of Finance, 69*(2), 577-609. doi:10.1111/jofi.12130

DeMaris, A. (2004). Advanced Issues in Multiple Regression. In A. DeMaris, *Regression with Social Data: Modeling Continuous and Limited Response Variables* (pp. 196-246). Hoboken, NJ: John Wiley & Sons. doi:10.1002/0471677566.ch6

Dewenter, K. L., & Malatesta, P. H. (2001). State-Owned and Privately Owned Firms: An Empirical Analysis of Profitability, Leverage, and Labor Intensity. *American Economic Review, 91*(1), 320-334. doi:10.1257/aer.91.1.320

Dharwadkar, R., George, G., & Brandes, P. (2000). Privatization in Emerging Economies: An Agency Theory Perspective. *The Academy of Management Review*, *25*(3), 650-669. doi:10.5465/amr.2000.3363533

DiNardo, J., & Tobias, J. L. (2001). Nonparametric Density and Regression Estimation. *Journal of Economic Perspectives*, *15*(4), 11-28. doi:10.1257/jep.15.4.11

Dinc, S. (2005). Politicians and banks: Political influences on government-owned banks in emerging markets. *Journal of Financial Economics, 77*(2), 453-479. doi:10.1016/j.jfineco.2004.06.011

Djankov, S., & Murrell, P. (2002). Enterprise Restructuring in Transition: A Quantitative Survey. *Journal of Economic Literature, 40*(3), 739-792. doi:10.1257/002205102760273788

Firth, M., Fung, P. M., & Rui, O. M. (2006). Corporate performance and CEO compensation in China. *Journal of Corporate Finance, 12*(4), 693-714. doi:10.1016/j.jcorpfin.2005.03.002

Furubotn, E. G., & Pejovich, S. (1972). Property Rights and Economic Theory: A Survey of Recent Literature. *Journal of Economic Literature*, *10*(4), 1137-1162.

Garrido, M. M., Kelley, A. S., Paris, J., Roza, K., Meier, D. E., Morrison, R. S., & Aldridge, M. D. (2014). Methods for Constructing and Assessing Propensity Scores. *Health Services Research, 49*(5), 1701-1720. doi:10.1111/1475-6773.12182

Goldeng, E., Grünfeld, L. A., & Benito, G. R. (2008). The Performance Differential between Private and State Owned Enterprises: The Roles of Ownership, Management and Market Structure. *Journal of Management Studies, 45*(7), 1244-1273. doi:10.1111/j.1467-6486.2008.00790.x

Heckman, J. J., Ichimura, H., & Todd, P. E. (1997). Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme. *The Review of Economic Studies, 64*(4), 605-654. doi:10.2307/2971733

Hutchinson, M., & Gul, F. (2004). Investment Opportunity Set, Corporate Governance Practices and Firm Performance. *Journal of Corporate Finance, 10*(4), 595-614. doi:10.1016/S0929-1199(03)00022-1

Inoue, C. F., Lazzarini, S. G., & Musacchio, A. (2013). Leviathan as a Minority Shareholder: Firm-Level Implications of State Equity Purchases. *Academy of Management Journal, 56*(6), 1775-1801. doi:10.5465/amj.2012.0406

Kole, S. R., & Mulherin, J. H. (1997). The Government as a Shareholder: A Case from the United States. *The Journal of Law and Economics*, *40*(1), 1-22. doi:10.1086/467364

La Porta, R., & Lopez-De-Silanes, F. (1999). The Benefits of Privatization: Evidence from Mexico. *The Quarterly Journal of Economics, 114*(4), 1193-1242. doi:10.1162/003355399556250

Lazzarini, S. G., & Musacchio, A. (2015). State Ownership and Firm-Level Performance. *Academy of Management Proceedings, 2015*(1), 237-253. doi:10.1016/j.worlddev.2014.08.016

Lechner, M. (2000). A Note on the Common Support Problem in Applied Evaluation Studies. *Univ. of St. Gallen Economics, Disc. Paper 2001-01*. doi:10.2139/ssrn.259239

May, D., Yozzo, J., & Regan, K. (2001). Return on Assets So Useful...and So Misused. *American Bankruptcy Institute Journal, Nov 2001*.

Megginson, W. L. (2005). *The Financial Economics of Privatization.* Oxford, United Kingdom: Oxford University Press. doi:10.1093/0195150627.001.0001

Megginson, W. L., & Netter, J. M. (2001). From State to Market: A Survey of Empirical Studies on Privatization. *Journal of Economic Literature, 39*(2), 321-389. doi:10.2139/ssrn.262311

Millward, R. (2005). *Private and Public Enterprise in Europe: Energy, Telecommunications and Transport, 1830-1990.* Cambridge, United Kingdom: Cambridge University Press. doi:10.1017/CBO9780511497124

Moita, R. M., & Paiva, C. (2013). Political Price Cycles in Regulated Industries: Theory and Evidence. *American Economic Journal: Economic Policy, 5*(1), 94-121. doi:10.1257/pol.5.1.94

Norwegian Ministry of Trade and Industry. (2002). *White Paper no. 22 (2001-2002). A Smaller and Better State Ownership.* Oslo.

Norwegian Ministry of Trade and Industry. (2006). *White Paper no. 13 (2006-2007). An Active and Long-Term Ownership.* Oslo.

Norwegian Ministry of Trade and Industry. (2011). *White Paper no. 13 (2010-2011). Active Ownership - Norwegian State Ownership in a Global Economy.* Oslo.

Norwegian Ministry of Trade, Industry and Fisheries. (2014). *White Paper no. 27 (2013-2014), Diverse and Value-creating Ownership.* Oslo.

Norwegian Ministry of Trade, Industry and Fisheries. (2017a). *The Ownership Department*. Retrieved from The Norwegian Government: https://www.regjeringen.no/no/dep/nfd/organisation/avdelinger/es/id1522/

Norwegian Ministry of Trade, Industry and Fisheries. (2017b). *The State Ownership Report 2016.* Oslo.

OECD. (2005). *Corporate Governance of State-Owned Enterprises: A Survey of OECD Countries.* Paris, France: OECD Publishing. doi:10.1787/9789264009431-en

Perry, T., & Shivdasani, A. (2005). Do Boards Affect Performance? Evidence from Corporate Restructuring. *The Journal of Business, 78*(4), 1403-1432. doi:10.1086/430864

Rajan, R. G., & Zingales, L. (2004). Making Capitalism Work for Everyone. *Journal of Applied Corporate Finance, 16*(4), 101-108. doi:10.1111/j.1745-6622.2004.00011.x

Rosenbaum, P. R., & Rubin, D. B. (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effects. *Biometrika, 70*(1), 41-55. doi:10.1093/biomet/70.1.41

Rosenbaum, P. R., & Rubin, D. B. (1985). Constructing a Control Group Using Multivariate Matched Sampling Methods That Incorporate the Propensity Score. *The American Statistician, 39*(1), 33-38. doi:10.2307/2683903

Sheshinski, E., & López-Calva, L. F. (2003). Privatization and Its Benefits: Theory and Evidence. *CESifo Economic Studies, 49*(3), 429-459. doi:10.1093/cesifo/49.3.429

Shirley, M., & Nellis, J. (1991). *Public Enterprise Reform: The Lessons of Experience.* Washington, DC: The World Bank.

Shirley, M., & Walsh, P. (2000). Public versus Private Ownership: The Current State of the Debate. *Policy Research Working Paper*. doi:10.1596/1813-9450-2420

Shleifer, A., & Vishny, R. W. (1994). Politicians and Firms. *The Quarterly Journal of Economics, 109*(4), 995-1025. doi:10.2307/2118354

Stuart, E. A., Huskamp, H. A., Duckworth, K., Simmons, J., Song, Z., Chernew, M., & Barry, C. L. (2014). Using Propensity Scores in Difference-in-Differences Models to Estimate the Effects of a Policy Change. *Health Services and Outcomes Research Methodology, 14*(4), 166-182. doi:10.1007/s10742-014-0123-z

The Conservative Party . (2017). *Competition*. Retrieved from The Conservative Party Web site: https://hoyre.no/politikk/temaer/naeringsliv/konkurranse/

The Labour Party. (2017). *Party Platform 2017-2021*. Retrieved from The Labour Party Web page: https://res.cloudinary.com/arbeiderpartiet/image/upload/v1/ievv_filestore/712bd5b874e24958b84c14fbef5b1a98afbfc8ae1e464cac9bcad06f24f2a85f

The Norwegian Government. (2013). *Norway's Governments Since 1945*. Retrieved from The Norwegian Government Web Site: https://www.regjeringen.no/en/the-government/previous-governments/governments/modern-times/governments-since-1945/id438715/

Thoemmes, F., & Ong, A. D. (2015). A Primer on Inverse Probability of Treatment Weighting and Marginal Structural Models. *Emerging Adulthood, 4*(1), 40-59. doi:10.1177/2167696815621645

Thompson, S. B. (2011). Simple Formulas for Standard Errors that Cluster by Both Firm and Time. *Journal of Financial Economics, 99*(1), 1-10. doi:10.1016/j.jfineco.2010.08.016

Toninelli, P. A. (2000). *The Rise and Fall of State-Owned Enterprise in the Western World.* Cambridge, United Kingdom: Cambridge University Press. doi:10.1017/cbo9780511896798

Vaaler, P. M., & Schrage, B. N. (2009). Residual State Ownership, Policy Stability and Financial Performance Following Strategic Decisions by Privatizing Telecoms. *Journal of International Business Studies, 40*(4), 621-641. doi:10.1057/jibs.2008.104

Villalonga, B. (2000). Privatization and efficiency: differentiating ownership effects from political, organizational, and dynamic effects. *Journal of Economic Behavior & Organization, 42*(1), 43-74. doi:10.1016/S0167-2681(00)00074-3

Vining, A. R., & Boardman, A. E. (1992). Ownership versus Competition: Efficiency in Public Enterprise. *Public Choice*, *72*(2), 205-239. doi:10.1007/bf00145092

White, H. (1984). *Asymptotic Theory for Econometricians.* San Diego: Academic Press.

Winship, C., & Radbill, L. (1994). Sampling Weights and Regression Analysis. *Sociological Methods & Research*, *23*(2), 230-257. doi:10.1177/0049124194023002004

10. Appendix

10.1 Difference-in-Differences

The standard design of a DiD model consists of two periods and two groups. The two periods are separated by a treatment event, where the treatment group receives treatment. By subtracting the pre-treatment difference from the post treatment difference, the method takes into account that the two groups were not initially equal (Angrist & Pischke, 2009). Hence, by comparing the difference between the treatment and control group before and after the application of the treatment, one can interpret the effect of the treatment as the difference in differences. An important benefit of the DiD methodology is that it allows for omitted variables that affect the treatment and control group differently as long as they are time invariant. Formally, the two-period DiD model can be defined as:

$$y_{it} = \beta_0 + \beta_1 * dT + \delta_0 * d_t + \delta_1 * dT * d_t + \varepsilon_{it}$$

where y_{it} is the outcome variable for individual i at time t. dT is a dummy variable equal to unity if the observation is from the treatment group, and d_t is a dummy variable equal to unity if the observation is from the period post treatment. $dT * d_t$ is an interaction term equal to unity if the observation is from the treatment group post treatment. Finally, the error term u_{it} is included. The setup is illustrated in the table below.

Table VII. Simple Difference-in-Differences Coefficient Interpretation

	Pre Treatment	Post treatment	Post – Pre
Control	β_0	$\beta_0 + \delta_0$	δ_0
Treatment	$\beta_0 + \beta_1$	$\beta_0 + \beta_1 + \delta_0 + \delta_1$	$\delta_0 + \delta_1$
Treatment – Control	β_1	$\beta_1 + \delta_1$	δ_1

Thus, in a two-period DiD model, the difference in differences estimate is δ_1. The parameter can be estimated in two ways. The first is to compute the changes in averages over time for each group, and then compute the difference in these changes. The second way is to compute the difference between the groups in each period, and then compute the change in this difference.

All assumptions of the ordinary least squares model also apply to the DiD model. Additionally, the DiD model relies on the assumption that the treatment and control groups exhibit parallel trends, but not necessarily at the same level. This implies that the path of the outcome variable for the control and treatment group should not be systematically different in absence of the treatment. Compositional differences in the treatment and control group that can cause a violation of the parallel trends assumption can be controlled for by including additional covariates in the regression specification. However, there is no guarantee that the observed variables can control for all differences in the two groups.

We have several time periods before and after the treatment. Because of this, we adjust the model specification as shown below:

$$y_{it} = \beta_0 + \beta_1 * dT + \delta_0 * year\ dummy_t + \delta_1 * dT * d_t + u_{it}$$

where the notation is similar to the two-period model, except for including year dummies.

10.2 Propensity Score Matching

We have utilized the practical guidance for the implementation of propensity score matching/weighting by Caliendo and Kopeinig (2008). This section presents complementary information to what has already been presented in section 5.4. Caliendo and Kopeinig (2008) suggest using a process that begins with (1) the estimation of the propensity score followed by (2) the selection of matching algorithm. The process continues with (3) checking for overlap/common support, before finally (4) investigating the matching quality.

10.2.1 The Estimation of the Propensity Score

The estimation of the propensity score has already been touched upon in section 5.4. This section will, in more detail, consider the choice of model specification in estimating the propensity score, or more specifically, the choice of variables.

The logit regression applied to estimate the propensity score includes age, return on sales, HHI, industry, and three variables capturing changes in ROA in the years prior to the election. Below is a brief explanation to our choice of variables.

Age

As a result of accounting standards, the age of the company might impact the recognized value of the assets and thereby impacting ROA.

Return on sales

The return on sales and turnover together constitute ROA. These variables can be combined in an infinite number of ways that result in the same ROA. Each of these combinations represent a unique strategy in terms of the choice between low margin and high volume or opposite. By including this variable, we wish to capture the effect of this strategy choice on ROA and make sure that this is taken into consideration when creating the control group.

Herfindahl Index (HHI)

HHI measures the size of firms in relation to the industry in which they operate and serves as an important indicator to the competition among them. HHI is calculated based on the 5-digit NACE code.

Industry

The industry variable is the 2-digit NACE industry code and is used to capture differences between industries.

Change in ROA

Finally, we have included variables that capture the change in ROA in the three years prior to each election. This variable should capture additional unobserved differences between companies that result in a certain ROA trend.

10.2.2 Matching Algorithms

Asymptotically, all propensity score matching estimators would give the same results as the sample size grows, as they become closer to comparing exact matches (Caliendo & Kopeinig, 2008). However, our dataset contains only between 11 and 14 treated observations in each election period, and in such cases the choice of matching algorithm can be of the utmost importance (Heckman, Ichimura, and Todd, 1997).

Generally, when faced with the decision between different matching techniques one face a trade-off in terms of bias and efficiency (Caliendo & Kopeinig, 2008). According to Caliendo and Kopeinig (2008), in large samples, nearest single neighbor matching has the best properties in terms of reducing bias, but increases variance. Increasing the number of neighbors or usage of Kernel matching or IPTW will increase bias and efficiency.

The performance of the different algorithms varies from situation to situation, and as suggested by Caliendo and Kopeinig (2008) we have tried a number of approaches, namely nearest neighbor with 1, 2 and 5 neighbors, Kernel matching, and IPTW using propensity scores. These models differ in terms of the definition of control group neighborhood and the assigned weight to these neighbors. Below follows a brief description of each matching technique.

Nearest Neighbor Matching

We have conducted nearest neighbor matching with one neighbor as well as oversampling using 2 and 5 neighbors with exact matching on industry (2-digit NACE code). The nearest neighbor approach selects the observations from the control group that are closest to each treated individual in terms of the propensity score in order to construct the counterfactual. Further, we have allowed for replacement due to the somewhat limited access to comparable companies. In the cases where we oversample, we implicitly reduce the variance at the expense of increased bias due to more, but poorer matches (Caliendo & Kopeinig, 2005).

Kernel Matching

Nearest neighbor matching selects only a limited number of companies from the control group to construct the counterfactual outcome for an SOE (Caliendo & Kopeinig, 2008). Kernel matching utilizes the weighted average of the entire control group to create the counterfactual, where the weights are derived from the differences in the propensity scores between the treated and the control group. As discussed previously, this has the advantage of reducing variance, but at the expense of an increased bias. When using Kernel matching, one must select a Kernel function and a bandwidth parameter, where the first is considered somewhat unimportant (DiNardo & Tobias, 2001). The choice of bandwidth parameter decides the smoothness of the density function, with a high value resulting in a smooth function with the benefit of relatively low variance. On the other hand, this might result in true features of the underlying data to be smoothed away, leading to a biased estimate (Caliendo & Kopeinig, 2008).

Inverse Probability of Treatment Weighting Using Propensity Scores

As we have argued, IPTW is the most suitable method of creating a counterfactual in this case. Hence, this method has been discussed in the section 5.4. The method has been chosen because, in our small sample, it is more efficient, without increasing the bias compared to the other matching methods measured in standardized bias and visual inspection of covariate distribution.

10.2.3 Overlap/Common Support

For the matching to be successful, one must ensure that the ranges of propensity scores overlap between the SOEs and POEs as ATT is only defined in this region of common support (Caliendo & Kopeinig, 2008; Garrido et al., 2014). For ATT it is sufficient to ensure the existence of potential matches in the control group, whilst additional requirements apply for the ATE.

We have investigated common support by conducting visual inspection of the density function of the propensity scores. It has been argued that this approach is sufficient and that the use of complicated formal tests is superfluous (Lechner, 2000). In nearest neighbor matching where exact matching on industry is conducted, this has been conducted on a per industry basis showing that potential matches do not always exist within each industry. For IPTW and Kernel, where exact matching has not been conducted, the common support region covers all SOEs, and only a very small number of POEs are excluded.

We have found the extent of overlap in propensity scores between SOE and POE to be satisfactory although this might not be obvious from looking at the distribution in the full sample due to the very large control group and many low propensity scores.

10.2.4 Matching Quality

In this section, we investigate if the matching algorithm has been able to balance the variables for SOEs and POEs. In order to investigate the post matching balance, we have conducted both a visual inspection and assessed the bias improvements using the standardized bias as suggested by Rosenbaum and Rubin (1985). The standardized biases are presented in table III in section 5.4. Supplements for visual inspection are presented below.

Figure V. Distribution of ROA by Election Year

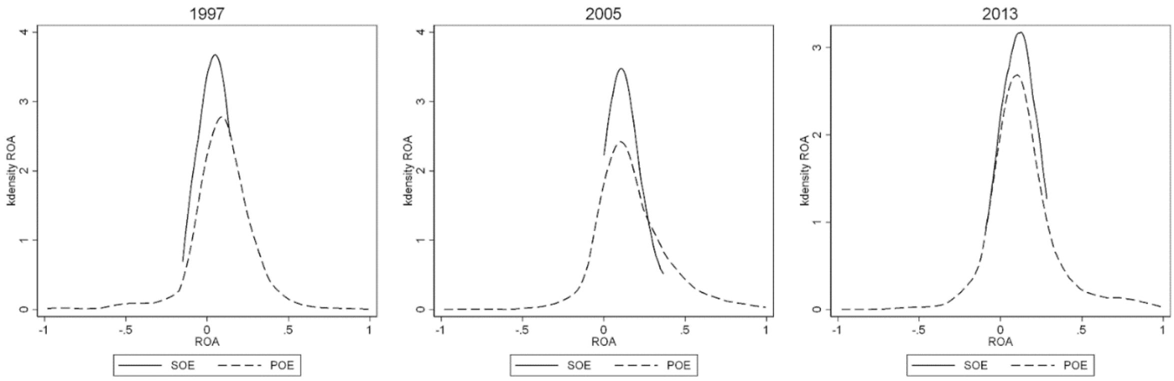


Figure VI. Distribution of Market Share by Election Year

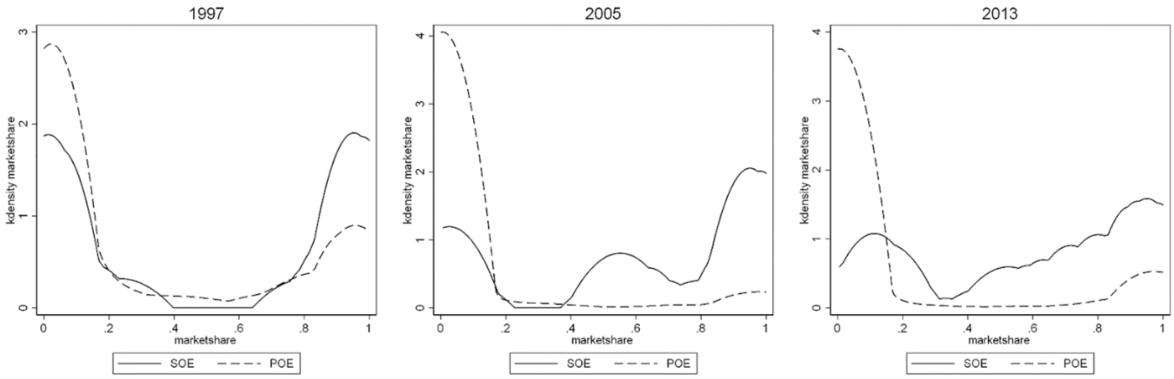
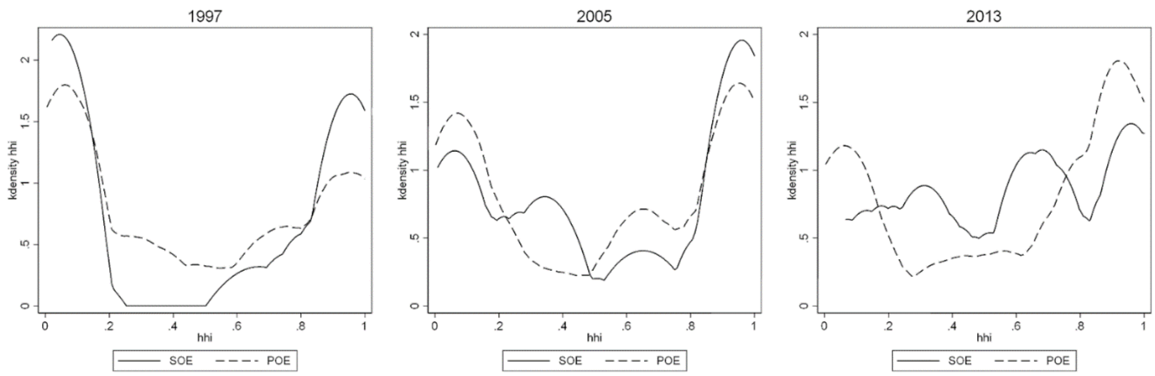


Figure VII. Distribution of Herfindahl Index by Election Year



From the visual inspection we observe substantial improvements in most variables with two important exceptions. These are market share and size, measured in either assets or sales. Furthermore, by including a size variable in the propensity score model, one can reduce the bias in size to a small extent, but this has the trade-off of drastically increasing the bias in other variables.

10.3 Tables and Figures

Figure VIII. Trends in ROA for the Unweighted Sample

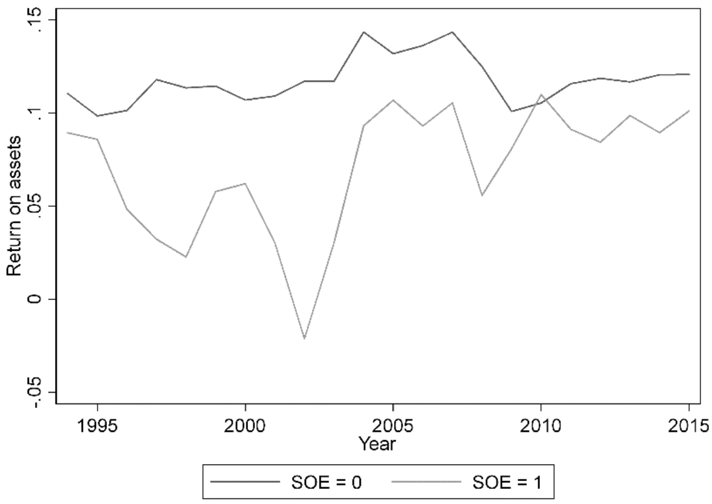


Table VIII. SOEs Used in the Study by Years and Industry

Company	**Years as SOE included in data**	**Industry**
A/S Olivin	1994-2002	Mining of chemical and fertiliser minerals
Aker Solutions	2007-2015	Support activities for petroleum and natural gas extraction
Ambita AS	1994-2015	Web portals
Baneservice AS	2005-2015	Construction of railways and underground railways
Cermaq ASA	1995-2013	Operation of marine ship farms
Drevsjø Trelast AS	1994-1998	Sawing and planing of wood
Entra Eiendom AS	2000-2015	Other letting of real estate
Flytoget AS	1998-2015	Passenger rail transport, interurban
Kongsberg Gruppen ASA	1994-2015	Manufacture of weapons and ammunition
Mesta AS	2008-2015	Construction of roads and motorways
Nammo AS	2000-2015	Manufacture of weapons and ammunition
NOAH AS	1994-2001	Recovering of sorted materials
Norsas AS	1994-1999	Other technical consultancy
Norsk Hydro ASA	1994-2015	Production of primary aluminum
Norsk Medisinaldepot AS	1994-2000	Wholesale of pharmaceutical goods
Raufoss ASA	1994-2003	Installation of machinery and equipment
Statens Skogplanteskoler	1994-1998	Silviculture and other forestry activities
Statoil ASA	1994-2015	Extraction of crude petroleum
Stor-Oslo Lokaltrafikk AS	1994-2007	Other services incidental to land transport
Telenor ASA	1994-2015	Wireless telecommunications activities
VESO AS	1994-2015	Wholesale of pharmaceutical goods
Yara International ASA	2004-2014	Manufacture of fertilisers and nitrogen compounds

Table IX. Supplementary Model I. Weighted Ordinary Least Squares (WOLS): Difference-in-Differences Estimation of the Effect of Government Change. Dependent Variable: Return on Assets

	Year of government change		
	1997	2005	2013
Constant	-0.012 (0.039)	0.168*** (0.065)	0.016 (0.038)
Publicly listed	-0.032 (0.032)	0.079** (0.040)	0.004 (0.030)
IFRS		-0.105* (0.057)	0.018 (0.033)
No. of employees (thousand)	0.0008 (0.0017)	-0.0041 (0.0186)	0.053 (0.0067)
Total income (NOKm)	0.0010 (0.0014)	-0.0004 (0.0010)	0.0025** (0.0011)
Total income (NOKm) squared	4.29e-12 (4.74e-12)	1.12e-12 (9.05e-13)	-6.59e-13 (7.48e-13)
Total assets (NOKm)	0.0010 (0.0009)	0.0016 (0.0012)	-0.0004 (0.0009)
Total assets (NOKm) squared	-7.84e-12** (3.32e-12)	-2.13e-12*** (8.14e-13)	4.28e-14 (5.43e-13)
Return on sales	0.173** (0.082)	0.042** (0.018)	0.434*** (0.095)
Market share	0.029 (0.042)	-0.059 (0.064)	0.042 (0.043)
Herfindahl index	0.037 (0.037)	0.025 (0.048)	-0.002 (0.031)
Bondevik1*SOE	**0.015** **(0.022)**		
Stoltenberg1*SOE		**0.047*** **(0.029)**	
Solberg*SOE			**0.024** **(0.026)**
Firm FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Estimation window	1994-2000	2002-2009	2010-2015
Lag of gov. change effect (years)	**1**	**1**	**1**
R^2	0.20	0.14	0.31
N	48,430	74,049	106,401

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors are in parentheses. Dummy for company being an SOE is omitted due to collinearity with firm FEs. Dummies for industries, locations and years are not included in the table. This model applies a one-year lag of the government change effect, contrary to the two-year lag applied in the main model.

Table X. Supplementary Model II. Weighted Ordinary Least Squares (WOLS): Difference-in-Differences Estimation of the Effect of Government Change. Dependent Variable: Return on Assets

	Year of government change		
	1997	2005	2013
Constant	-0.009	0.041	0.022
	(0.039)	(0.068)	(0.040)
Publicly listed	-0.058**	0.090**	-0.028
	(0.029)	(0.040)	(0.046)
IFRS		-0.093*	0.048
		(0.056)	(0.043)
No. of employees (thousand)	0.0022	-0.0023	0.0064
	(0.0015)	(0.0142)	(0.0102)
Total income (NOKm)	-0.0015	0.0009	0.0019**
	(0.0015)	(0.0006)	(0.0008)
Total income (NOKm) squared	1.22e-11***	-1.41e-13	-1.19e-12*
	(4.53e-12)	(5.69e-13)	(6.52e-13)
Total assets (NOKm)	0.0037*	0.0002	-0.0003
	(0.0020)	(0.0007)	(0.0007)
Total assets (NOKm) squared	-1.67e-11**	-6.39e-13	-7.57e-14
	(6.78e-12)	(4.42e-13)	(4.28e-13)
Return on sales	0.099***	0.035*	0.343***
	(0.037)	(0.019)	(0.094)
Market share	0.001	0.015	0.006
	(0.034)	(0.057)	(0.043)
Herfindahl index	0.057	0.031*	0.009
	(0.036)	(0.018)	(0.031)
Bondevik1*SOE	**0.017**		
	(0.020)		
Stoltenberg1*SOE		**0.049***	
		(0.039)	
Solberg*SOE			**0.049**
			(0.041)
Firm FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Estimation window	**1993-2001**	**2003-2012**	**2009-2015**
Lag of gov. change effect (years)	2	2	2
R^2	0.17	0.11	0.26
N	53,462	83,205	123,495

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors are in parentheses. Dummy for company being an SOE is omitted due to collinearity with firm FEs. Dummies for industries, locations and years are not included in the table. This model applies longer estimation windows than the main model.