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Does Longer Duration of Executive Compensation Foster Investment Efficiency?

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ABSTRACT In this paper, I examine whether longer duration of executive compensation influences investment decisions. I exploit a regulation designed to foster long-term orientation in executive compensation as an exogenous trigger to lengthen executives' incentive duration. I find that treated firms reduce their abnormal investment relative to control firms, implying an increase in investment efficiency. These results are robust to different measures of investment, several models of expected investment, and different plausible control groups. The treatment effect is economically significant, as the reduction in abnormal investment amounts to about 10% of mean investment. It appears that a mandated longer duration has the greatest effect on investment efficiency in firms that had a low degree of compensation committee independence before the shock. Further, it seems that the lower abnormal investment stems to a greater extent from reductions in over-investment.

Keywords: Executive compensation; Incentive duration; Investment; Investment efficiency; Regulation

JEL classifications: J33; M12; M48; M52; G31

1. Introduction

In this paper, I examine whether and how mandating companies to implement longer durations of executive compensation affects investment efficiency.[1] Executive compensation is one of the corporate governance tools that draws major attention and creates major controversy about corporations among the general public (Edmans, Gabaix, et al., 2017). With regard to compensation, the duration of the incentive is a crucial aspect (e.g., Bebchuk & Fried, 2010; Gopalan et al., 2014) as it has a direct effect on executives' investment decisions. While research finds that firms reduce investment when there are strong short-term incentives (Edmans, Fang, et al., 2017; Ladika & Sautner, 2020) and increase investment when executive compensation is more long-term (Flammer & Bansal, 2017; Larcker, 1983), it is indeed the case that neither less investment need be less efficient nor more be more efficient. I therefore set out to empirically investigate the link between longer duration of executive compensation and the efficiency of firms' investment.

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[1]For expositional convenience, I use the term *longer (incentive) duration* throughout the paper to refer to changes in compensation systems that relate to the adoption of a multi-year performance assessment, deferral of an awarded variable compensation, gearing of performance-related components to long-term determinants, increasing the vesting period of stocks or stock options, adopting a bonus-malus system, or requiring investment in the firm's own stocks or stock options. This use of the term extends (Gopalan et al., 2014)'s understanding of duration to include the prolonged incentive effect inherent in adopting multi-year assessments, deferrals, and bonus-malus schemes.

Evaluating this link is difficult because firms typically design their compensation scheme independently, whenever shareholders or their representatives on the supervisory board negotiate with executives.[2] I exploit a unique setting, in which an exogenous regulation enables treatment and control firms to be identified. The introduction of the German 'Appropriate Director Compensation Act' (Vorstandsvergütungs-Angemessenheitsgesetz (VorstAG)) in July 2009 is regarded as 'unprecedented interference with private compensation schemes' (Hitz & Müller-Bloch, 2015, p. 659). This act demands that remuneration contracts are reviewed and oriented towards the long term. Variable compensation components should be based on a multi-year assessment period (i.e., multi-year evaluation and deferment of awarded variable compensation), conditional payback provisions are recommended, and most specifically, the minimum vesting period for stock options (if awarded) is increased from two to four years.

A mandated increase in the duration of executive compensation can affect a firm's investment efficiency either positively, negatively, or not at all. By increasing the duration, the board increases the length of the window during which the executive performance can be evaluated. As a consequence, the future pay-off to current-period investment will be realized to a larger extent in observable performance. This may lessen the incentive to empire build (Jensen, 1986), because if executives engage in unprofitable projects to increase their span of control, this unprofitability will materialize within the lengthened evaluation window. Likewise, the longer evaluation window mitigates myopic disincentives for current period investments with future payoffs (Graham et al., 2005), as more of the payoffs will be considered within the lengthened evaluation period.

However, compensation regulation may also interfere with efficient contracting and lead to a decrease in investment efficiency. Strong short-term objectives may be optimal when the executive's short-term stock price manipulation propensity is high (Schroth, 2018), when the executives have valuable alternatives, and when firms' operations are highly uncertain (Evans et al., 2017). Finally, if the regulation is too vague, firms might simply window dress without implementing any substantial changes to the compensation systems, leading to no effect. Which of these opposing forces dominates remains to be empirically explored.

To do this, I compare German firms with firms based in other European countries in a difference-in-differences design. In addition to comparing German firms with all other European firms, I obtain a matched subsample of the other European firms. I use different proxies for investment and operationalize firms' abnormal investment with the residuals of commonly used investment model regressions that follow the accelerator theory (e.g., Biddle et al., 2009; C. Chen et al., 2013; F. Chen et al., 2011) as well as Tobin's q theory (e.g., L. Li et al., 2018; McNichols & Stubben, 2008).[3] I find strong support for the notion that mandated longer duration increases investment efficiency across measures of investment and models of expected investment. My results reveal statistically and economically significant reductions in abnormal investment following the adoption of the VorstAG act. Treated firms invest about 10% more efficiently, when viewed relatively to the sample mean of the respective investment variable.

Next, I analyze cross-sectionally whether the treatment effect is related to firms' pre-treatment corporate governance characteristics. In line with research finding positive effects of supervisory board independence on compensation incentives (e.g., Knyazeva et al., 2013; P. Kumar & Sivaramakrishnan, 2008), I investigate whether the reduced abnormal investment depends on the degree of independence that the firm's compensation committee had in the pre-period. The mandate for longer duration could impose a stronger change on the incentive system for executives in firms in which fewer independent supervisory board members were involved in

[2] Recent survey responses (Edmans et al., 2021) indicate that shareholders and supervisory board members disagree in several regards on how to set executive compensation.

[3] Smaller residuals imply lower abnormal investment, which implies higher investment efficiency.

the compensation contracting before the treatment, leading to stronger effects on investment efficiency. I further analyze a broader measure of corporate governance using a firm's overall corporate governance pillar score. I find evidence to support the notion of mandated longer duration having the largest effect on investment efficiency in firms that have low compensation committee independence or a weak overall corporate governance.

In further analyzes, I mitigate concerns that different contemporaneous forces at a country level might drive the results, by comparing the treated German firms with a control group of unaffected private German firms. I again find that the treated German firms show greater improvements in investment efficiency than the untreated German firms following the VorstAG. I also attempt to differentiate between reductions in over- and under-investment following the mandated longer duration. Across specifications, it appears that the treatment effect is driven by reductions in over-investment. Longer duration therefore seems to hamper over-investment but may not be effective in providing incentives for under-investing executives to invest more. Finally, I employ a different approach where investment aggregated at the industry level proxies for the likelihood of over- or under-investing (e.g., Biddle et al., 2009; Lara et al., 2016) and find that the treatment also leads to lower abnormal investment in situations where whole industries diverge from optimal investment levels.

This study makes three contributions. First, I contribute to the debate on whether long-term incentive duration can improve firm outcomes. On the one hand, many theorists and empiricists argue for and show benefits of longer duration (e.g., Bebchuk & Fried, 2010; Flammer & Bansal, 2017; Jochem et al., 2018; Larcker, 1983). On the other hand, there are arguments for keeping incentives short-term under certain conditions (e.g., Evans et al., 2017; Schroth, 2018). Recent survey responses even indicate some disagreement between shareholders and supervisory board members, as most shareholders believe that more long-term incentives would lead to better investment decisions, while supervisory board members view incentives to be sufficiently long-term and raise concerns about further lengthening them (Edmans et al., 2021). My study allows an estimate of the effect of an exogenously triggered lengthening in incentive duration and shows that it reduces firms' abnormal investment. I generally interpret this as a positive effect on real investment behavior. However, there seems to be little evidence for reductions in under-investment, which demands caution in claiming universal benefits of longer duration.

Second, I contribute insights into the interplay of corporate governance characteristics and executive pay duration. Pay duration may generally be a substitute or a complement to alternate forms of corporate governance (Gopalan et al., 2014). More specifically, the independence of the supervisory board may play a major role in determining the right incentive pay (e.g., Knyazeva et al., 2013; Z. Li & Wang, 2016) and providing the right monitoring (e.g., P. Kumar & Sivaramakrishnan, 2008) for optimal investment decisions. My cross-sectional tests indicate that longer duration matters in firms with low compensation committee independence as well as weak overall corporate governance. This rather suggests a complementary relationship between governance and incentive duration.

Third, I provide information for regulators and political decision makers by adding to the growing literature on the consequences of governance regulation in general and on the effects of compensation regulation in particular. There is a contemporaneous debate on the EU level regarding whether and how to reform executives' and supervisory boards' duties towards sustainable corporate governance.[4] My paper adds to this debate the notion that mandated longer

[4]I refer to the European Commission's 'Study on directors' duties and sustainable corporate governance' published in July 2020. This study recommends far-reaching regulatory action and sparks heavy critique from accounting scholars that urges a more thorough review and analysis of existing governance research before implementing regulatory action (e.g., Roe et al., 2020).

duration leads on average to more efficient investment decisions, which is in line with shareholders' long-term interests. Analyses suggest that generalization of this notion to other jurisdictions may depend on prevalent governance characteristics, and in particular, on whether there are any independent board members who are responsible for compensation contracting. Moreover, the expected effects in other jurisdictions may depend on whether over-investment is a major issue.

2. Research Question

Investment efficiency is a key determinant of a firm's economic productivity (Biddle & Hilary, 2006).[5] A mandated increase in executive compensation duration can have either a positive, negative, or no effect on a firm's investment efficiency. Corporate finance theory provides that increasing duration can lead to more efficient investments by mitigating incentives for both empire building and myopic decision making. When the board increases the length of the window during which the executive is evaluated, the future pay-off to current period investment will to a larger extent be realized in observable performance. When an executive enjoys some discretion (Berle & Means, 1932; Jensen & Meckling, 1976), she may have an incentive to grow her empire beyond the optimum size (Jensen, 1986). If she takes on unprofitable projects to increase her span of control, this unprofitability can be evaluated to a greater extent within the lengthened window. At the same time, lower incentives for over-investment require less ex-ante capital rationing by outside suppliers of funds (Stiglitz & Weiss, 1981), which may have led to ex-post under-investment.

Longer duration also counters incentives for inefficient investment that can stem from myopic decision making (Keynes, 1936). Influential survey results show that the majority of US financial executives would pass up a positive NPV investment if initiating the project meant missing current earnings estimates (Graham et al., 2005). The longer evaluation period reduces the disincentive for current period investments with future payoffs, as these payoffs can be realized within the extended evaluation period (Fudenberg et al., 1990). Likewise, in situations where myopia provides incentives for over-investment to signal a positive outlook for the firm, longer evaluation windows should discipline the executive, as she will have to bear the negative impact of later write-offs of poorly performing projects.[6]

However, mandating changes in compensation design, as in the VorstAG setting, may interfere with efficient contracting. Assuming that firms (i.e., supervisory board members) know best how to compensate their executives, exogenously imposed longer duration may lead to a decrease in efficient investment. It may be optimal for shareholders to implement strong short-term objectives when the executive's short-term stock price manipulation propensity is high (Schroth, 2018). Shareholders may prefer shorter performance periods when executives have valuable alternatives and when firms' operations are highly uncertain (Evans et al., 2017). Moreover, biases towards inefficient investment choices may not be solely eliminated through complete long-term contracting, if investors fail to actively acquire information about executives' activities (von Thadden, 1995). Such monitoring may also be costly, diminishing the returns to

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[5] Investment efficiency means that firms undertake only projects and all projects with a positive net present value (NPV) (Modigliani & Miller, 1958). Inefficient investment may relate to over-investment, which corresponds to investing in negative NPV projects, and under-investment, which corresponds to passing up positive NPV opportunities (Biddle et al., 2009).

[6] Bebchuk and Stole (1993) show that when the *level* of investment in long-term projects is observable by investors, and only its *productivity* is private knowledge of the executive, she may have incentives to signal a positive long-term outlook of the firm and boost her reputation (Stein, 2003) by heavily investing in long-term projects, leading to over-investment.

increasing duration. As a result, regulation may even distort incentives and reduce investment efficiency.

Finally, if the mandate for longer duration is vague or it is not (anticipated to be) rigorously enforced, there may be no effect on investment efficiency. Firms might simply window dress descriptions of their compensation systems without substantially changing the actual contract design and the resulting incentives. Therefore, whether and in which direction mandated longer incentive duration significantly alters the efficiency of firms' investments ultimately remains an empirical question.

3. Regulatory Background

The Appropriate Director Compensation Act (VorstAG) came into force in July 2009 as an amendment to the German stock corporation law and prompted listed German firms to review and adapt their remuneration contracts. Prior to the VorstAG, no law demanding any particular incentive scheme was in place in Germany. The previous version of Paragraph 87 of the German stock corporation law merely mandated the supervisory board to implement 'appropriate' executive compensation.[7] The VorstAG is a hard-law regulation that directly affects the structure of executive compensation, while the disclosure of the compensation components had been mandatory since 2006.

The VorstAG act was a targeted response by the German government to the general perception that the global financial crisis had been fueled by executive contracts that emphasized short-term performance (Deutscher-Bundestag, 2009). Apparently, the regulators presumed that executive compensation contracting had been inefficient in terms of the duration of incentives, demanding regulatory action to prolong them. To this end, the VorstAG act demands that remuneration contracts are oriented towards the long-term. Executives' variable compensation components should have a multi-year assessment period, which may imply that the evaluation spans a multi-year period and/or that a portion of the variable compensation already awarded is deferred for a certain period. Further, the act recommends conditional payback provisions. The most specific requirement demands that the minimum vesting period for stock options is increased from two to four years if firms have such compensation in place.[8] German accounting and law scholars consider the law to have a far-reaching impact on compensation systems (Hitz & Müller-Bloch, 2015).

I rely on two sources of data regarding firms' implementation of the act as stated in contemporaneous annual reports (AR). First, I check a published series on the act's implementation in the 80 firms listed in the two German blue chip indices.[9] Analyzing firms' AR 2009 and AR 2010 (or AR 2008/2009 and AR 2009/2010 if the fiscal year does not end on December 31), the series finds that among other changes, 49 firms had geared performance-related components towards long-term measures, 38 had lifted the ratio of long-term components to short-term components, 14 had adopted a bonus-malus system, 12 had increased the vesting period for options to four years, and 11 had newly mandated their executives to invest in their own shares (Götz & Friese, 2010, 2011).

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[7]Note that the German two-tier system imposes the duty of steering the firm and managing its operations on a public firm's executive board, while the supervisory board appoints, oversees and controls the former, which includes negotiating compensation contracts.

[8]As part of the general goal of orienting executive compensation more in the long term, the act also mandates some corporate governance policies. In particular, it mandates a personal deductible in directors' and officers' liability insurance contracts. Lin et al. (2019) show positive announcement returns regarding the introduction of this aspect. This paper focuses on the aspects of the law that are concerned with the structure of executive compensation.

[9]My final sample of treated German firms is largely a subset of those firms.

Second, I hand-collect AR descriptions of how the German firms included in the final sample implemented the act. My investigation begins with AR 2009 and extends to AR 2010 and AR 2011, until I find a substantial change regarding the remuneration structure. At times, I compare the descriptions of compensation systems with descriptions in earlier documents (i.e., AR 2008) to find apparent differences. Appendix B lists the quoted descriptions exemplary for three firms, the full list is provided in the online Supplement.[10] To group the main changes and any potential concurrent or additional changes, I classify them into six broad categories. As set out in Appendix C, it appears that the adoption of a multi-year performance assessment and the deferral of bonus payouts were the major design choices, with a three-year observation window being used most frequently.[11] It further appears that both the later vesting of stocks and options and the requirement to invest in stocks and options were frequent, yet mostly secondary, changes.[12] As it can therefore be reasonably assumed that German firms were exogenously prompted to implement longer duration, I regard all German firms as treated.[13],[14] In a later section, I show that the exclusion of the few firms with apparently no – or likely very minor – changes leads to stronger results.

At the very latest, firms had to consider the VorstAG when extending existing executive compensation contracts or signing new ones. Still, many firms immediately switched to new contracts.[15] The published series covering the 80 blue chip firms shows that when analyzing firms' AR 2009, despite substantial changes made by many of them, 35 still stated that their assessment of the compensation system was ongoing or that it was postponed until the contract was due to be rewritten (Götz & Friese, 2010). However, an analysis of the firms' AR 2010 showed that the number of firms had dropped to 11 (Götz & Friese, 2011). I therefore exclude the transition year 2009 from the analysis and expect an effect to materialize for the year 2010, potentially becoming stronger in years 2011 and 2012. I make no distinction between arguably earlier or

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[10] For instance, Deutsche Post AG states in AR 2009: '(…) the annual performance-related remuneration will in future no longer be paid in full for the year on the basis of having reached the agreed targets. Instead, 50% of the annual performance-related remuneration will flow into a new medium-term component with a three-year calculation period (…).' Similarly, the much smaller company Wincor Nixdorf AG states in AR 2009/2010: 'In accordance with the requirements of Germany's VorstAG Act, the vesting period for share options granted under the 2010 share option program has been extended from two to four years.'

[11] One third of the treatment group (15 firms) adopted multi-year performance assessment as the main change. One fourth (11 firms) deferred a portion of its awarded variable compensation. The most frequent term for both designs is three years. 7 firms switched to more long-term determinants in their bonus systems or increased the long-term to short-term ratio. While the adoption of a bonus-malus system is the explicit main change for only 4 firms, it is likely that other multi-year assessment or deferral schemes also entail such a system.

[12] While only 4 firms increased the vesting of stocks or options as a main change, many firms implemented this change along with a further main change (total frequency of 12). It further appears that a quarter of firms (11) newly required their executives to invest in their own stocks or options, in addition to other changes.

[13] It is likely that my treatment group includes weakly treated firms, which may impair the detection of significant effects. Trying to differentiate between strongly and weakly treated firms among German firms may endanger the validity of the assigned treatment. Strongly treated firms have an incentive to downplay the impact of the act on compensation contracting so as not to convey that the supervisory board was unable to implement longer duration. Weakly treated firms may have an incentive to describe the changes in the compensation system as more substantial than they were, to meet the expectations of the general public in implementing major revisions.

[14] Flammer and Bansal (2017) exploit close call election outcomes around shareholder proposals on long-term executive compensation to obtain an exogenous variation in compensation contracts. Götz and Friese (2010) sample the degree of approval for a vote on the compensation system during the annual general meeting in the year after the VorstAG act took effect. 70% of the firms covered put their compensation system up for vote. The vote failed in just one firm. There was generally great approval for the systems with most firms' shareholders casting more than 90% of votes in favor. This indicates large support for the compensation systems (many of which already contained substantial changes regarding the incentive duration) in the German firms, making a similar design to Flammer and Bansal (2017) impossible.

[15] For instance, BASF SE states in AR 2009: 'Contracts with all Board members, regardless of existing contractual terms, were consensually and uniformly converted to this system effective as of January 1, 2010.'

later effective dates of changes in firms' compensation systems, as the resulting incentives can have an impact on executives long before that date.

I sample several years of archival data for European firms around the act, making the research design potentially sensitive to concurrent and confounding regulatory action taken in any country that is part of the control sample. I check an EU document for such action in the major subset of firms in my control sample that are domiciled in an EU country. The EU had released a (nonbinding) recommendation concerning the regulation of executive board remuneration (2009/385/EC in May 2009) only months before the VorstAG. A later European Commission staff working document ('Report on the application by Member States of the EU of the Commission 2009/385/EC Recommendation') samples the recommendation's implementation among EU member states. The document reports that by June 2010, only Germany had implemented legally binding rules regarding the structure of the remuneration policy. Belgium is the only country that also enacted regulations in this regard, yet these regulations are weaker.[16] UK firms make up the largest portion of the control sample. However, there were no reforms in the UK in the period before October 2013, which is outside my sample period.[17] The EU itself did not undertake any further binding compensation regulation before July 2015 (Edmans, Gabaix, et al., 2017), when the Shareholder Rights Directive 2007/36/EC was amended.[18]

4. Research Design and Variable Measurement

4.1. Sample Selection

I compare German and non-German firms with regard to the introduction of the VorstAG act in a difference-in-differences (DID) design. I use all firms that were listed in the STOXX Europe 600 index in July 2009, when the regulation came into force. The German firms in the index are the treated firms and all others are control firms. Many firms in my sample had switched to IFRS in 2005. There had also been changes in executive compensation disclosure requirements in that year. I therefore begin my sample selection with the 2006 financial year. I exclude fiscal year 2009 in the DID regressions, as the regulation became effective during 2009 and may have already affected some firms' investment decisions in that year but also allowed firms to only consider it for new contracting. Accordingly, the pre-treatment and post-treatment periods span the three years before and after 2009, respectively.

I obtain accounting and stock-related data from Thomson Reuters (now Refinitiv) Datastream for the financial years 2006 to 2012. Panel A of Table 1 shows the sample selection procedure. First, I eliminate firms from finance and insurance industries as identified by the Fama and French 12 industry (FF12) classification (French, 2018). Financial firms have very different investment measures compared with other industries; moreover, in many jurisdictions, financial firms were subject to special remuneration regulations in the immediate aftermath of the global financial

[16] Belgian firms may deviate from the requirement of basing a part of the variable compensation on multi-year performance assessment as well as minimum vesting periods with shareholders' approval (Royal Decree of June 6, 2010). Nevertheless, I exclude Belgian firms from the control group in untabulated robustness tests, which does not alter the results.

[17] In October 2013, the UK mandated the implementation of binding shareholder votes on forward-looking remuneration policies every three years (Petrin, 2015).

[18] The amended directive, which must be transposed into national law by EU member states, mandates that shareholders shall have binding votes on remuneration policies and prescribes the disclosure of several aspects of the remuneration policy.

Table 1. Sample selection and distribution.

Panel A: Sample selection procedure Selection step	German	Other	Total
Thomson Reuters Datastream annual data from 2006 to 2012 for firms listed in the STOXX Europe 600 as of July 2009	392	3808	4200
– firms from finance and insurance industries	(49)	(868)	(917)
– firms with an 80% decline in equity market value during GFC	(21)	(98)	(119)
– firm-year observations with missing model or control data	(8)	(235)	(243)
Final sample of firm-years for estimating the investment models	314	2607	2921
– firm-years for the 2009 transition year	(46)	(385)	(431)
Final sample of firm-years for DID regressions	268	2222	2490
Final sample of unique firms	46	394	440
– other country firms that are not matched to a German firm	0	(1961)	(1961)
Final PSM sample of firm-years for DID regressions	268	255	523
Final PSM sample of unique firms	46	46	92

Panel B: Firm-year distribution among countries and FF12 industries

Country	Firm-years	FF12 industry	German	Other
Austria	40	(1) Consumer NonDurables	14	221
Belgium	61	(2) Consumer Durables	45	37
Denmark	88	(3) Manufacturing	49	441
Finland	111	(4) Oil, Gas, & Coal Extract. & Products	0	170
France	410	(5) Chemicals & Allied Products	46	127
Germany	314	(6) Business Equipment	27	190
Greece	30	(7) Telephone & Television Transmission	14	185
Ireland	35	(8) Utilities	14	177
Italy	121	(9) Wholesale, Retail, & Some Services	21	239
Luxembourg	26	(10) Healthcare, Medical Equipm., & Drugs	42	208
Norway	51	(11) Finance (excluded)	0	0
Portugal	48	(12) Other	42	612
Spain	155	Total	314	2607
Sweden	195		2921	
Switzerland	183			
The Netherlands	169			
United Kingdom	837			
Others	47			
Total	2921			

crisis (GFC).[19] Second, following earlier research based on a sample period covering the GFC (e.g., Ernstberger et al., 2017), I exclude firms with a decline in equity market value greater than 80% from October 2007 (the month with the highest STOXX Europe 600 closing price before the GFC) to March 2009 (the month with the lowest STOXX Europe 600 closing price during the GFC). My inferences remain unchanged when I do not apply this restriction. Third, I eliminate observations (firm-years) in which one or more model or control variables are missing. I arrive at a sample of 2921 firm-years for the investment model regressions. The sample for the DID regressions contains 2490 firm-years, of which 268 relate to German firms and 2222 to others. Both samples correspond to 46 German firms and 394 non-German firms. Table 1 further shows the firm-years for a propensity score matched (PSM) control sample.

[19]The UK and Switzerland, for instance, implemented reforms applying to financial services firms (Bebchuk & Fried, 2010). Also, Germany implemented the Remuneration Ordinance for Institutions (Institutsvergütungsverordnung) in 2010. Moreover, financial firms receiving funds from bailout or relief programs were required to implement particular caps on compensation.

Panel B of Table 1 shows the distribution of firm-years among countries and FF12 industries for the full sample from 2006 to 2012 used in the investment model regressions. Firms in the United Kingdom account for the biggest portion of firm-years, followed by French and German firms. In terms of industry classification, the biggest portion of firm-years is attributable to 'Residual category 12 – Other,' followed by 'Category 3 – Manufacturing.'

4.2. Measuring Investment and Abnormal Investment

I rely on archival accounting and financial market data to assess a firm's investment in a given year. I use two common measures of a firm's investment level. The first investment measure (*C/PPE*) is the firm's capital expenditures (CAPEX), scaled by the average property plant and equipment balance (PPE) (e.g., Biddle et al., 2009, 2016; McNichols & Stubben, 2008). The second measure defines investment more broadly, as it also accounts for investments in R&D endeavors. *I/TA* adds R&D expenses to CAPEX, scaled by average total assets (e.g., Balakrishnan et al., 2016; C. Chen et al., 2013; Lara et al., 2016). To do this, I assume missing values for R&D to be zero when CAPEX are available (e.g., Edmans, Fang, et al., 2017; Ernstberger et al., 2017). Throughout the paper, I use investment (*Invest*) as an umbrella term to cover the different measures. Abnormal investment (*AbnInvest*) refers to the residuals of different investment model regressions with different investment measures, |*AbnInvest*| refers to the absolute residuals.

In frictionless markets, the expected benefits of an investment should be the sole factor determining the investment decisions of the firm. Expectations regarding future growth and product demand in turn determine the expected benefits. Growth expectations are based on executives' information sets. The core task of investment models is to proxy those growth expectations. I rely on two well-established models to estimate expected investment: the accelerator model and Tobin's q model.[20]

The accelerator model (e.g., Biddle et al., 2009; F. Chen et al., 2011; Hsu et al., 2015) builds on the assumption that investment is related to the level of output. When all available capital is fully utilized, changes in demand require adjustments in investment. Investment is modeled as a function of revenue growth, whereby the revenue growth proxies the (future) output growth, i.e., growth in demand. Expected investment is thereby commonly estimated either over the entire cross-section of the economy or on an industry level, where the regression residuals constitute abnormal investment. The implicit assumption is either that investment is efficient on average over the whole economy or in every industry over time. I report the results from estimating the regression models for each FF12 industry; the cross-sectional results are very similar. Equations (1) and (2) show the baseline specification along with a piecewise linear specification of that approach:

$$Invest_{i,t} = \alpha + \beta RevCh_{i,t-1} + \varepsilon_{i,t}, \tag{1}$$

$$Invest_{i,t} = \alpha + \beta_1 RevCh_{i,t-1} + \beta_2 RevChN_{i,t-1} + \beta_3 RevCh * RevChN_{i,t-1} + \varepsilon_{i,t}. \tag{2}$$

Equation (1) regresses firm *i*'s investment in year *t* on the change in revenue (*RevCh*) from year *t-1* to year *t*. F. Chen et al. (2011) suggest a piecewise linear model that allows the relation between revenue growth and investment to differ between increasing and declining revenues. The authors introduce a variable indicating whether the revenue change is negative (*RevChN* in Equation (2)). As both specifications produce qualitatively and quantitatively similar results, I only report results for the piecewise linear model.

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[20]Gao and Yu (2018) present a recent review of different models and their implicit assumptions offered by the investment literature.

The second approach is Tobin's q model (e.g., Lai et al., 2014; L. Li et al., 2018; McNichols & Stubben, 2008; Verdi, 2006), which also approximates expected investment by the fitted values of a linear model. This model builds on the assumption that marginal q summarizes a firm's growth opportunities.[21] Thus, I regress investment on the firm's ratio of the market valuation of asset stock to its replacement costs, as in the following model:

$$Invest_{i,t} = \alpha + \beta_1 Q_{i,t-1} + \varepsilon_{i,t}. \tag{3}$$

I measure Q in Equation (3)) as the beginning-of-year enterprise value divided by beginning-of-year total assets. My inferences are not affected when I combine the two proxies for growth opportunities of the two models as suggested by C. Chen et al. (2013).

Alongside expected growth opportunities, differences in internal financing capabilities may play a major role in explaining firms' investment decisions (e.g., Biddle et al., 2009; McNichols & Stubben, 2008). One way to address this is by adding the firm's operating cash flow to the investment model (e.g., McNichols & Stubben, 2008). I add firm i's cash flow from operating activities in year t scaled by average total assets (CFO) to both models. Equations (4) and (5) show the specifications for the adjusted accelerator and the Tobin's q model as I use them for presenting my empirical results:

$$\begin{aligned} Invest_{i,t} = \alpha + \beta_1 RevCh_{i,t-1} + \beta_2 RevChN_{i,t-1} \\ + \beta_3 RevCh * RevChN_{i,t-1} + \beta_4 CFO_{i,t} + \varepsilon_{i,t}, \end{aligned} \tag{4}$$

$$Invest_{i,t} = \alpha + \beta_1 Q_{i,t-1} + \beta_2 CFO_{i,t} + \varepsilon_{i,t}. \tag{5}$$

The core investment model variables to proxy the opportunity sets are clearly impacted by the GFC. For instance, the average change in revenue plunges in 2009 and recovers in 2010.[22] One might suspect this to impact and potentially bias the empirical tests. The main tests regarding the evolution of investment efficiency rely on a comparison of German firms with other European firms in relative terms. Therefore, there would only be a bias if the extent to which the change in revenue proxies investment opportunities is systematically differently affected by the crisis for German firms relative to other firms. For example, it would be required that the extent to which firms had an incentive to decrease the change in revenue even further in 2009 and to show an even larger increase in the following years, accordingly, systematically differs between treatment and control firms. This seems very unlikely. To further mitigate this concern, I estimate the investment models not only on the basis of the full sample but also on a matched subsample, the full sample adding untreated German firms, and a sample of treated and untreated German firms only in later sections.

4.3. Empirical Strategy

The absolute value of the residuals of the investment models in (4) and (5) proxy for abnormal investment. This approach is subject to the concerns raised by W. Chen et al. (2018) when residuals from one regression are the dependent variable of another. Following their advice, I add the first-step regressors to the second-step regression.[23] Equation (6) formalizes the second-step

[21] Average q is used to proxy for marginal q, which is unobservable.
[22] This pattern is, however, very similar for treated and control firms (not shown).
[23] The economic interpretation of the residuals requires that they be transformed into their absolute values, which may still bias the estimators. To mitigate this issue, I include a binary variable (*ResidN* in Equation (6)) that is coded 1 for positive and 0 for negative residuals of the first model.

regression for both investment models:

$$|AbnInvest|_{i,t} = \alpha + \beta_1 Treat_i * Post_t + \beta_2 Treat_i + \beta_3 Post_t + \beta_4 ResidN_{i,t} + 1^{st} StepVars + Controls + FEs + \varepsilon_{i,t}. \quad (6)$$

$|AbnInvest|$ is the absolute residual of the respective first-step regression and measures firm i's yearly absolute deviation from the expected investment. *Treat* indicates whether a firm is German. *Post* indicates whether an observation refers to a fiscal year after the regulation took effect. I code firm-years 2006 to 2008 as 0 and firm-years 2010 to 2012 as 1. The interaction of *Treat* and *Post* is the variable of interest. A negative coefficient indicates a reduction in abnormal investment for treated firms, which means an increase in investment efficiency. The coefficient captures reductions in both over-investment and under-investment. In later tests, I attempt to differentiate between the two.

In a later analysis, I employ a third approach that uses investment aggregated at the industry level as a proxy for the likelihood of over- and under-investing (Biddle et al., 2009; Lara et al., 2016). Untabulated results for a fourth investment model, which was used previously in the context of linking compensation to investment distortion (Eisdorfer et al., 2013), do not affect my inferences.[24]

4.4. Control Variables

Control variables (*Controls*) fall into three categories: First, I add the model variables from the accelerator approach to the second step of the Tobin's q approach and vice versa, since these variables should explain abnormal investment behavior. Second and third, I add the control variables from Eisdorfer et al. (2013), who also investigate investment efficiency in a compensation context, and those from F. Chen et al. (2011), who also analyze investment efficiency in a multinational context. A firm's *Size* should be indicative of the level of attention and scrutiny it receives from financial market participants, which should be negatively associated with abnormal investment.[25] I employ Altman's Z score (Z) to proxy for financial distress, which I expect to be positively associated with incentives to make abnormal investment decisions. *Lever* is the firm's leverage. A higher leverage could financially constrain the firm. Thus, I would expect leverage to be negatively associated with the investment level. It is thus unclear whether the association between leverage and abnormal investment is positive or negative. *Regul* is a binary indicator of whether a firm operates in a regulated industry. Abnormal investment should be lower in such firms. *Homog* estimates homogeneity in different industries, whereby I also expect a negative association with abnormal investment decisions. *Tangb* measures the tangibility of the firm's assets. This control variable captures differences in the importance of tangible assets to the firm in general. Finally, *Slack* is added to control for a firm's cash abundance. As with low-leverage firms, cash-rich firms should be less constrained in financing their investments. Therefore this variable may be positively or negatively associated with abnormal investment.

I provide a description of the calculation of all variables in Appendix A. All continuous variables are winsorized yearly on the 1% level. My inferences also hold without winsorization. In line with prior literature in the field that uses a similar DID or pre-post design (e.g., C. Chen

[24] These authors suggest analyzing the difference in a firm's investment from the same year's median industry investment after controlling for certain variables driving investment levels. They regard the median industry year investment as the expected investment. The absolute difference from the expected investment is the abnormal investment after controlling for leverage, firm size, the investment opportunity set, distress, regulation and industry homogeneity.

[25] I measure *Size* by the number of a firm's employees in a given year instead of other commonly used proxies for firm size, such as total sales or assets, because the firms in my sample report in different currencies.

et al., 2013; L. Li et al., 2018), I control for time-invariant fixed effects between treated and untreated firms via firm fixed effects in my main specification. Thereby, the parameter of interest is akin to a weighted combination of all firms' two-period DID estimators.[26] I cluster standard errors on the country-year level as firms are assigned to the treatment group on the basis of their home country. As an alternative, I present results for industry fixed effects as a higher-level identification of units that requires fewer dummies (e.g., F. Chen et al., 2011). In this specification, I add year fixed effects to absorb potential effects of a time-varying shock on the industry level that may coincide with the regulatory treatment, and cluster standard errors on the level of the firm.[27]

5. Empirical Results

5.1. Descriptive Statistics

Table 2 presents descriptive statistics of the variables included in the regression analyzes. Panel A shows investment model data for all firm-years between the financial years 2006 and 2012. The mean investment in terms of CAPEX is 22.80% of average PPE. The mean investment in terms of CAPEX and R&D is 7.12% of average total assets. Panel A shows that the investment models produce more and smaller negative residuals than positive ones, which is in line with previous research (F. Chen et al., 2011).[28] I distinguish between the treatment and control sample in Panel B of Table 2, where I focus on the sample of firm-years used in the DID regressions. Panel B shows that the distribution of the investment variables *C/PPE* and *I/TA* is similar across treatment and control firms. While the absolute residuals of the investment models for *C/PPE* (i.e., $|C/PPE_{Rev}|$ and $|C/PPE_{Q}|$) also do not differ, the absolute residuals for *I/TA* do. It appears that German firms have slightly lower *RevCh* and *CFO* and substantially lower *Q* figures throughout the sample period. Panel B in Table 2 further shows that treatment firms are substantially larger (*Size*) and have a less tangible asset base (*Tangb*) than control firms.

Untabulated Bravais Pearson correlations show that the absolute residuals of the two investment models are highly correlated for each investment measure, for instance, the correlation between $|C/PPE_{Rev}|$ and $|C/PPE_{Q}|$ is 0.93. However, the investment model residuals are only weakly correlated across *C/PPE* and *I/TA* (between 0.30 and 0.33).

5.2. Main Model Results

5.2.1. Regression results

Table 3 shows the results of the accelerator model. $|C/PPE_{Rev}|$ is the dependent variable in the first three columns and $|I/TA_{Rev}|$ is the dependent variable in columns (4) to (6). Columns (1) and (4) ((2) and (5)) show results with firm (industry and year) fixed effects for the full control sample. All estimators for the interaction of *Treat* and *Post* are negative and significant. German firms have on average about 2.3 to 2.6% (about 0.5 to 0.9%) lower abnormal investment in

[26]This DID with firm dummies is less sensitive to few firms driving the effect, particularly in unbalanced designs where firms drop in and out of the sample.

[27]The inclusion of year dummies makes the DID less sensitive to few years (within the pre- or post-period) driving the effect. The results are unaffected when also adding the year fixed effects to the main specification.

[28]For instance, 1705 of C/PPE_{Rev} residuals are negative, with a mean of -7.57, while 1216 residuals are positive, averaging 10.61.

Table 2. Descriptive statistics.

Panel A: Final sample of firm-years for estimating the investment models

	N	Mean	STD	Min	25%	75%	Max
C/PPE	2921	22.80	14.32	2.40	13.65	28.17	98.70
C/PPE_{Rev}	2921	0	12.66	− 32.20	− 7.68	4.60	80.35
$C/PPE_{Rev} < 0$	1705	− 7.57					
$C/PPE_{Rev} > 0$	1216	10.61					
C/PPE_{Q}	2921	0	12.43	− 32.92	− 7.35	4.48	74.86
$C/PPE_{Q} < 0$	1706	− 7.41					
$C/PPE_{Q} > 0$	1215	10.41					
I/TA	2921	7.12	5.30	0.25	3.58	9.14	38.79
I/TA_{Rev}	2921	0	4.55	− 11.65	− 2.71	1.86	31.93
$I/TA_{Rev} < 0$	1711	− 2.75					
$I/TA_{Rev} > 0$	1210	3.89					
I/TA_{Q}	2921	0	4.59	− 18.23	− 2.72	1.83	33.71
$I/TA_{Q} < 0$	1720	− 2.75					
$I/TA_{Q} > 0$	1201	3.94					
RevCh	2921	0.09	0.20	− 0.51	− 0.001	0.15	1.53
RevChN	2921	[0.25]					
Q	2921	1.37	1.05	0.18	0.73	1.65	8.25
CFO	2921	0.11	0.07	− 0.08	0.07	0.14	0.39

Panel B: Final sample of German vs. other firm-years for DID regressions

	German firm-years (N = 268)						Other firm-years (N = 2222)						Diff
	Mean	STD	Min	25%	75%	Max	Mean	STD	Min	25%	75%	Max	p
C/PPE	23.65	13.91	6.38	15.16	27.82	85.43	23.07	14.37	2.40	13.69	28.92	97.16	0.52
$\lvert C/PPE_{Rev}\rvert$	8.99	9.35	0.01	3.16	11.25	59.84	8.80	8.98	0.003	2.99	11.58	80.35	0.75
$\lvert C/PPE_{Q}\rvert$	8.50	9.63	0.01	2.25	10.38	60.54	8.67	8.74	0.01	2.95	11.54	74.86	0.75
I/TA	7.37	3.97	0.67	4.78	9.11	24.82	7.15	5.50	0.25	3.46	9.15	38.79	0.43
$\lvert I/TA_{Rev}\rvert$	2.48	2.51	0.001	0.76	3.21	16.42	3.30	3.33	0.003	1.10	4.36	31.93	0.00
$\lvert I/TA_{Q}\rvert$	2.50	2.43	0.01	0.77	3.30	15.61	3.31	3.38	0.001	1.15	4.26	33.71	0.00
RevCh	0.10	0.14	− 0.51	0.03	0.14	0.90	0.11	0.20	− 0.51	0.01	0.16	1.53	0.22
RevChN	[0.14]						[0.21]						0.00
Q	1.06	0.74	0.19	0.59	1.29	4.90	1.48	1.12	0.18	0.81	1.78	8.25	0.00
CFO	0.09	0.06	− 0.08	0.06	0.11	0.37	0.11	0.07	− 0.08	0.06	0.14	0.39	0.00
Lever	0.23	0.13	0.003	0.14	0.32	0.60	0.25	0.16	0.00	0.14	0.36	0.74	0.02
Size	82,667	111,418	759	11,994	95,193	469,680	41,347	64,154	175	6134	50,817	469,680	0.00
Z	2.93	1.87	0.07	1.72	3.48	12.49	3.60	3.16	− 0.02	1.85	4.10	22.28	0.00
Regul	[0.11]						[0.13]						0.42
Homog	27.14	7.83	18.94	20.23	27.98	46.01	29.88	8.78	15.22	24.46	34.40	52.67	0.00
Tangb	0.25	0.14	0.04	0.12	0.33	0.73	0.29	0.22	0.01	0.11	0.44	0.89	0.00
Slack	0.08	0.07	0.001	0.03	0.11	0.37	0.07	0.07	0.0000	0.02	0.09	0.41	0.01

Notes: This table shows descriptive statistics. Panel A shows descriptives for the investment measures and the investment model variables for the final sample of firm-years. The sample period is from 2006 to 2012. Panel B differentiates between German and other firms included in the final sample of firm-years for the DID regressions which exclude the transition year 2009. Panel B shows descriptives for the variables included in the multivariate regression analyzes. All variables are defined in Appendix A. $\lvert C/PPE_{Rev}\rvert$, for instance, refers to the absolute value of the residuals of investment model regressions based on the accelerator model using CAPEX scaled by average PPE as the investment variable.

terms of *C/PPE* (*I/TA*) after treatment. This corresponds to about 10% of the average level of the respective investment variable.[29] I consider this effect economically significant.

The coefficients for *Treat* and *Post* are mostly insignificant. The coefficients for *ResidN* are negative and significant, as the negative residuals of the first-step models are smaller than the positive residuals. The first-step regressors are not tabulated, as they cannot be meaningfully

[29] For instance, the interaction coefficient of column (1) in Table 3 (− 2.305) divided by the average *C/PPE* reported in Table 2 (22.80) gives 10.1%.

Table 3. Regression analyzes for the accelerator approach.

	Dependent variable: $\lvert C/PPE_{Rev}\rvert$			*Dependent variable:* $\lvert I/TA_{Rev}\rvert$		
	Main specification (1)	Alternative FEs (2)	PSM subsample (3)	Main specification (4)	Alternative FEs (5)	PSM subsample (6)
Treat ∗ Post	**– 2.305*** (0.559)**	**– 2.517** (1.045)**	**– 2.620** (0.865)**	**– 0.493** (0.216)**	**– 0.733* (0.403)**	**– 0.882*** (0.248)**
Treat		2.665* (1.361)			0.013 (0.381)	
Post	0.454 (0.316)		1.000 (0.648)	– 0.146 (0.113)		0.279 (0.209)
ResidN	– 1.995*** (0.448)	– 2.867*** (0.540)	– 1.553** (0.704)	– 0.656*** (0.177)	– 1.252*** (0.230)	– 0.498** (0.244)
Q	0.512 (0.443)	– 0.203 (0.847)	0.598 (1.194)	0.251 (0.209)	0.388** (0.184)	0.649* (0.355)
Lever	– 0.383 (2.382)	4.729 (3.040)	6.394* (3.669)	– 3.527*** (1.126)	– 0.913 (0.865)	– 0.989 (1.129)
log(Size)	– 0.005 (0.580)	– 0.972*** (0.227)	– 1.342 (1.891)	0.181 (0.242)	– 0.482*** (0.096)	0.547 (0.410)
Z	0.125 (0.224)	0.341 (0.365)	0.379 (0.628)	– 0.025 (0.116)	– 0.143** (0.070)	– 0.004 (0.133)
Regul	– 9.836 (10.575)	1.815 (1.340)	– 44.934 (116.121)	2.561 (5.146)	– 0.487 (0.453)	– 19.458 (23.755)
Homog	1.225* (0.665)	0.131** (0.067)	1.798 (5.861)	– 0.086 (0.319)	– 0.050** (0.024)	0.982 (1.221)
Tangb	– 12.499** (5.366)	– 3.904* (2.106)	– 0.761 (5.754)	1.251 (1.695)	– 0.044 (0.734)	2.708 (2.301)
Slack	2.151 (5.111)	2.516 (4.064)	– 0.584 (6.176)	– 1.681 (1.278)	– 0.273 (1.475)	– 2.886* (1.699)
Intercept	– 18.985 (15.352)	12.663*** (3.030)	– 8.765 (119.371)	5.224 (7.247)	7.954*** (1.555)	– 27.047 (27.682)
Firm FE	Yes		Yes	Yes		Yes
FF12 + Year FE		Yes			Yes	
Observations	2490	2490	523	2490	2490	523
Adjusted R^2	0.455	0.153	0.609	0.566	0.233	0.476

Notes: This table presents the results of the treatment effect of longer duration on investment efficiency, in a two-step approach: The first step (not tabulated) estimates the accelerator model regression (4) per FF12 industry. The absolute values of the residuals of this regression (i.e., $\lvert C/PPE_{Rev}\rvert$ for the investment variable CAPEX sclaed by average PPE and $\lvert I/TA_{Rev}\rvert$ for the sum of CAPEX and R&D scaled by average total assets) are the dependent variables in the second-step regression (6). The table shows pooled panel OLS regression coefficients from the second step. The first-step regressors are included in the second step, but not tabulated. The dummies for the respective fixed effects are also included but not tabulated. All variables are defined in Appendix A. Standard errors are shown in parentheses. *, **, *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

interpreted. The control variable *Q* is positive but mostly insignificant. There is no clear pattern for *Lever*, as it has insignificant, significantly positive and significantly negative coefficients. *Size* is mostly negatively correlated with abnormal investment. The distress variable *Z* is positive for $\lvert C/PPE_{Rev}\rvert$ and negative for $\lvert I/TA_{Rev}\rvert$. There are no clear patterns for industry regulation and homogeneity. *Tangb* is negatively correlated with abnormal investment for $\lvert C/PPE_{Rev}\rvert$ in the full sample. The coefficient for *Slack* is mostly insignificant. Overall, the different fixed effects structures produce quite different coefficients for many of the control variables.

In addition to comparing treated German firms with all other firms, I apply a propensity score matching. Panel B of Table 2 shows that German and other firms display significant differences for the majority of the control variables. PSM would address potentially non-linear effects of these controls. I therefore match on these variables' means over the years of the pre-period

(2006 to 2008). More specifically, I match on *log*(*Size*), *Tangb*, and *Slack*, and include dummies for FF12 industry factors into the matching.[30] I match one of the control firms to each treated firm without replacement and exclude those control firms that do not have adequate counterparts among the treated firms. The mean propensity score distance between the 46 treated firms and the 46 matched control firms is 0.006.

Columns (3) and (6) of Table 3 show the results of the accelerator approach for the PSM sample. The interaction of *Treat* and *Post* remains significantly negative. The investment models implicitly assume that investment is efficient on average for all firms in the respective industry in the economy. Therefore, the reported figures in columns (3) and (6) show the results of the respective second step, in which the sample is reduced in line with the matching procedure after the first-step investment model estimations, such that the investment models employ the full sample of the STOXX Europe 600 universe. Nevertheless, in untabulated tests, the results also hold when I estimate both the first step and the second-step regressions after the matching based sample reduction.

Table 4 shows the results of the second regression approach, which follows Tobin's q theory. Again all estimators for the DID interaction are negative, with five of the six being statistically significant. The economic effects have a similar magnitude as before. The negative residuals of the first-step investment model are again smaller than the positive residuals ($ResidN < 0$). The other control variables also appear to have similar effects as in the accelerator model.

Tables 3 and 4 show that the results for *C*/*PPE* are generally stronger than those for *I*/*TA*. This is not surprising, given that the latter relies on assuming R&D to be zero when the variable is missing. I add a dummy variable for firms where missing R&D is set to zero (Koh & Reeb, 2015) in untabulated tests, which produces quantitatively similar results. In the investment efficiency literature, the *I*/*TA* measure is often used net of proceeds from disposals of PPE (e.g., Biddle et al., 2009; F. Chen et al., 2011; Cheng et al., 2013). In untabulated tests, I obtain similar but weaker results when I net both measures of investment by deducting PPE disposals, which are set to zero when the variable is missing. In all, the results for the two main approaches provide strong support for the notion that mandated longer duration increases investment efficiency.

5.2.2. Parallel trends

A key assumption underlying my identification strategy is that German and other firms would have had parallel trends in abnormal investment had it not been for the VorstAG act. I conduct a parallel trends test and visualize the treatment effect by year over the sample period. I run Equation (6) by replacing the single *Treat***Post* interaction with separate interactions between *Treat* and binary indicators for each year. I exclude the indicator for year 2008 immediately prior to the VorstAG, such that 2008 is the benchmark period (e.g., Christensen et al., 2017). I run this test with the full control sample and present results with the industry and year fixed effect specification that generally produces larger standard errors.

Figure 1 displays the regression coefficient estimates and their two-tailed 90% confidence intervals. Panels A and B map the coefficients for *C*/*PPE* and *I*/*TA* following the accelerator model. Panels C and D show results for both investment measures and the Tobin's q approach. The counter-factual treatment effects in the pre-treatment periods are small and statistically indistinguishable from the benchmark period in all four panels. The plots indicate that treated and control firms experienced common trends in abnormal investment levels in the period before the introduction of the VorstAG. This supports the assumption that, in the absence of the VorstAG,

[30] A three-year-average of *Lever* and *Z* makes little sense, so I do not include those variables in the matching. *Regul* and *Homog* vary on the level of the industry.

Table 4. Regression analyzes for the Tobin's q approach.

	Dependent variable: $\|C/PPE_Q\|$			*Dependent variable:* $\|I/TA_Q\|$		
	Main specification (1)	Alternative FEs (2)	PSM subsample (3)	Main specification (4)	Alternative FEs (5)	PSM subsample (6)
Treat ∗ Post	**– 2.563*** (0.508)**	**– 2.839*** (1.065)**	**– 3.080*** (0.706)**	**– 0.465* (0.254)**	**– 0.655 (0.407)**	**– 0.925*** (0.240)**
Treat		2.528* (1.421)			0.003 (0.381)	
Post	0.349 (0.284)		0.661 (0.643)	– 0.051 (0.113)		0.346* (0.194)
ResidN	– 2.003*** (0.387)	– 2.838*** (0.540)	– 1.202** (0.596)	– 0.753*** (0.175)	– 1.291*** (0.239)	– 0.389* (0.220)
RevCh	– 0.449 (1.134)	– 1.296 (1.176)	0.459 (1.733)	– 0.202 (0.403)	0.403 (0.424)	0.363 (0.572)
Lever	2.325 (2.553)	5.252* (2.848)	13.026** (5.629)	– 2.534** (1.000)	– 0.762 (0.885)	– 0.747 (1.152)
log(Size)	– 0.530 (0.624)	– 1.038*** (0.228)	– 2.732 (1.941)	0.200 (0.224)	– 0.512*** (0.101)	1.051*** (0.295)
Z	0.349 (0.251)	0.242 (0.322)	1.516* (0.814)	0.038 (0.090)	– 0.118 (0.073)	– 0.001 (0.152)
Regul	3.503 (6.905)	1.514 (1.374)	– 4.053 (86.591)	5.437 (4.879)	– 0.482 (0.512)	– 14.608 (23.735)
Homog	0.376 (0.392)	0.129* (0.069)	– 0.353 (4.280)	– 0.207 (0.309)	– 0.056** (0.025)	0.729 (1.210)
Tangb	– 11.722** (5.339)	– 3.687* (2.124)	– 0.731 (5.786)	– 0.046 (1.739)	– 0.186 (0.739)	1.682 (2.651)
Slack	0.584 (5.171)	2.814 (3.905)	– 0.759 (6.416)	– 0.727 (1.429)	0.323 (1.466)	0.223 (1.518)
Intercept	1.506 (11.450)	12.979*** (3.118)	52.665 (90.759)	6.927 (6.872)	8.300*** (1.632)	– 27.200 (26.535)
Firm FE	Yes		Yes	Yes		Yes
FF12 + Year FE		Yes			Yes	
Observations	2490	2490	523	2490	2490	523
Adjusted R^2	0.481	0.161	0.640	0.594	0.241	0.495

Notes: This table presents the results of the treatment effect of longer duration on investment efficiency, in a two-step approach: The first step (not tabulated) estimates the Tobin's q model regression (5) per FF12 industry. The absolute values of the residuals of this regression (i.e., $|C/PPE_Q|$ for the investment variable CAPEX sclaed by average PPE and $|I/TA_Q|$ for the sum of CAPEX and R&D scaled by average total assets) are the dependent variables in the second step-regression (6). The table shows pooled panel OLS regression coefficients from the second step. The first-step regressors are included in the second step, but not tabulated. The dummies for the respective fixed effects are also included but not tabulated. All variables are defined in Appendix A. Standard errors are shown in parentheses. *, **, *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

there would have been parallel trends in the post-period. Figure 1 also indicates that the treatment effects begin in year 2010 and increase until year 2012, lending additional support to the claim that longer duration reduces abnormal investment and increases investment efficiency.[31]

[31] Mapping the average dependent variable per treatment and control group for the years 2006 to 2012 (Appendix E) presents a similar picture of common trends in the pre-period, which would have continued in the post-period without the VorstAG intervention. It appears that the average treatment effect is already slightly apparent in 2009 and materializes fully in years 2010, 2011 and 2012. This is in line with the notion that some firms' investment decisions were already affected in 2009, when the VorstAG came into force, and that it took some time for all firms to implement the changes to their compensation systems and for the systems to affect executive decisions.

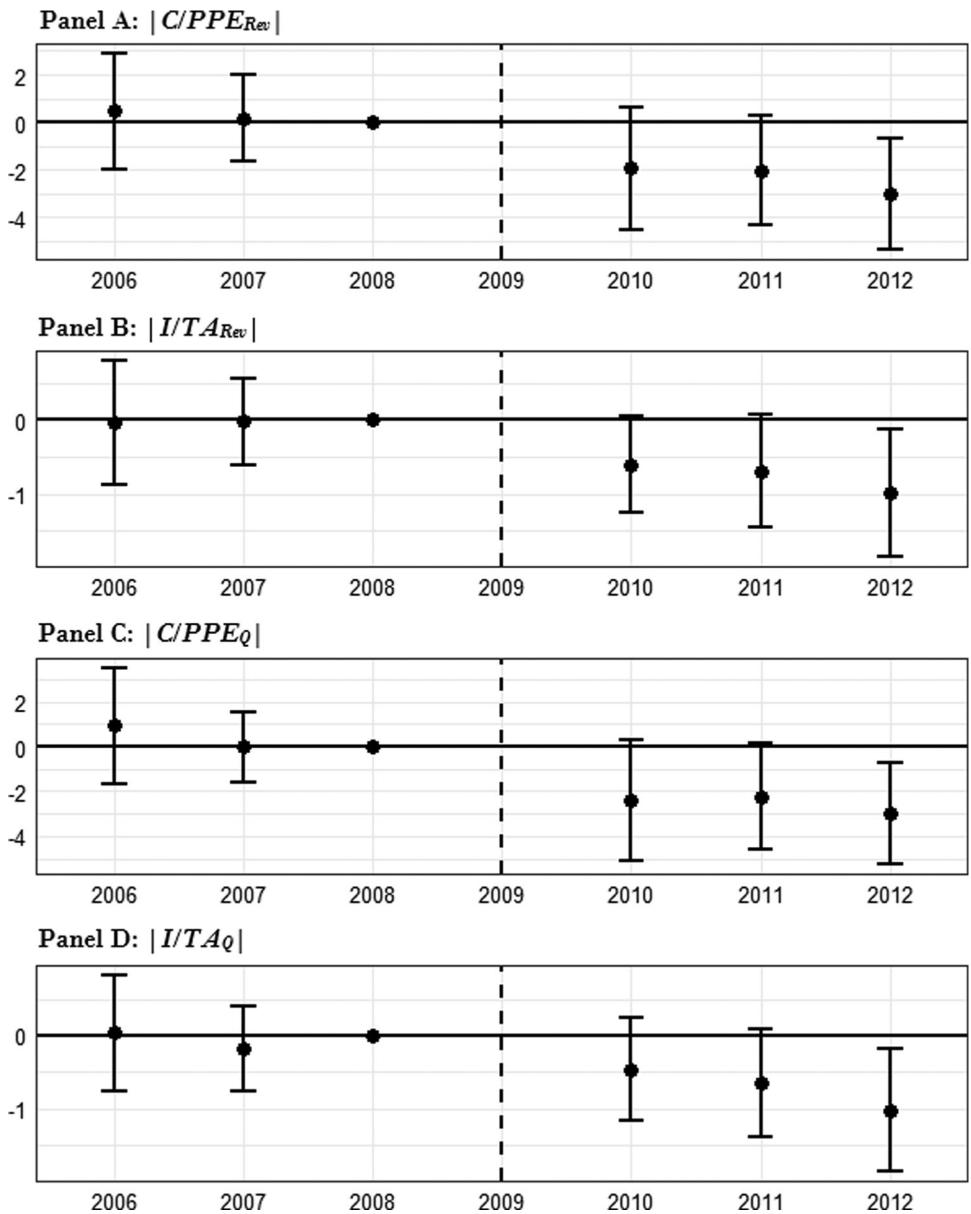

Figure 1. Pattern of the counter-factual treatment effects.

Notes: This figure plots regression coefficient estimates and two-tailed 90% confidence intervals for a parallel trends test to visualize the treatment effect by year over the sample period. The DID interaction (*Treat*Post*) of the main tests is replaced with separate interactions between *Treat* and binary indicators for each year. The indicator for year 2008 immediately before the VorstAG is excluded, such that 2008 is the benchmark period. Panels A and B map the coefficients for $|C/PPE_{Rev}|$ and $|I/TA_{Rev}|$, which are the absolute values of the regression residuals based on the accelerator model using the two alternative investment measures CAPEX scaled by average PPE and the sum of CAPEX and R&D scaled by average total assets. Panels C and D show results for both investment measures and the alternative Tobin's q approach (i.e., $|C/PPE_Q|$ and $|I/TA_Q|$).

5.2.3. Removing firms with likely very minor changes

The hand-collected descriptions of changes to firms' compensation systems given in the annual reports (provided in the online Supplement) indicate that almost all firms implemented substantially longer duration following the VorstAG act. Five firms apparently made no[32] or likely very minor changes[33] to their compensation systems. In untabulated tests, I obtain larger effect sizes when I remove these five firms from the treatment sample. The effect sizes of the main analyzes reported in Table 3 and Table 4 are about 0.5 larger for *C/PPE* and about 0.1 larger for *I/TA* across all models and for both the full and the PSM control sample.

5.3. Cross-Sectional Analyses

Next I analyze cross-sectionally whether the effect of increased incentive duration on investment efficiency is related to corporate governance characteristics. In particular, I test whether the reduced abnormal investment depends on how independent the firm's compensation committee had been in the pre-period. A substantial body of research documents positive effects of supervisory board independence on executive compensation contracting (e.g., Knyazeva et al., 2013; P. Kumar & Sivaramakrishnan, 2008; Z. Li & Wang, 2016). One could expect that firms with fewer independent supervisory board members involved in the design of the executive compensation system likely leads to weaker incentives for efficient investment. In such firms, the VorstAG act should impose a stronger change to the incentive system for executives, which would then have a stronger effect on investment efficiency. There is tension, however, as monitoring by more independent boards may decrease even if they install better compensation incentives (P. Kumar & Sivaramakrishnan, 2008), which may allow more abnormal investment.

I obtain data on the degree of independence of the compensation committee from the executive board from the asset-4 data in Thomson Reuters Datastream and obtain it manually for German firms where it is missing.[34] I first test the cross-sectional effects within the sample of German treated firms. The effect of low compensation committee independence might be particularly strong when there are no or very few independent members and the effect among higher degrees of independence is likely not linear. I therefore define low compensation committee

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[32]Software AG states in AR 2009 that the system 'is geared in an exemplary fashion toward long-term company development' and shows no changes in later years. United Internet AG neither mentions the VorstAG in any of its reports, nor shows any changes throughout AR 2008 to AR 2012).

[33]Fresenius SE subjects the new provision 'that the share of long-term variable compensation components is at least equal in its amount to half of the total variable compensation components' to the discretion of the supervisory board (AR 2009). Symrise AG stated that 'against the background of the (. . .) [VorstAG,] (. . .) the remuneration is justified in both the internal vertical and external horizontal comparison' in AR 2009 and describes in AR 2011 that only the benchmark of the long-term incentive plan had been changed. Tognum AG states in AR 2009 that the 'remuneration already meets the objectives of the new legislation to a large degree' and presents a 'new market-based target remuneration structure' that contains the same components with likely similar weighting as before in AR 2010.

[34]The variable calculates the percentage of independent members in the compensation committee (i.e., 'personnel committee,' 'human resources committee,' 'executive committee,' 'presiding committee') as stipulated in firms' annual reports. I obtain members' independence (binary variable) on the basis of the Thomson Reuters Datastream methodology, pretests for German firms where the variable is available, and in line with bright-line criteria as they are used today and were used at the time in listing requirements of major stock indices (i.e., NYSE and Nasdaq). A member qualifies as independent if she was not employed by the company in the two years prior to joining the supervisory board, does not have a shareholding of more than 5%, does not depend on substantial executive compensation (i.e., the compensation beyond membership and committee fees paid as a variable compensation or in return for advisory contracts between the member and the firm should not exceed 100 thousand Euros (i.e., NYSE and Nasdaq use a threshold of 120 thousand U.S. dollars)), and has not been a member of the board for more than 10 years. I check firms' annual reports and compensation committee members' BoardEx profiles to assess independence.

Table 5. Cross-sectional analyzes of low compensation committee independence.

Dependent variable:	$\|C/PPE_{Rev}\|$ (1)	$\|C/PPE_{Q}\|$ (2)	$\|I/TA_{Rev}\|$ (3)	$\|I/TA_{Q}\|$ (4)
Panel A: Treated German firms				
CCI*Post	**– 2.572*****	**– 1.415**	**– 0.857*****	**– 0.922*****
	(0.701)	**(1.034)**	**(0.319)**	**(0.282)**
Post	– 0.127	– 1.003	– 0.103	– 0.092
	(0.778)	(0.744)	(0.177)	(0.143)
ResidN	– 2.458**	– 1.786**	– 0.736	– 0.466
	(1.035)	(0.702)	(0.517)	(0.397)
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	268	268	268	268
Adjusted R^2	0.667	0.678	0.470	0.463
Panel B: Full sample				
Treat*Post*CCI	**– 4.791*****	**– 4.082*****	**– 0.623**	**– 0.823*****
	(1.071)	**(1.125)**	**(0.409)**	**(0.369)**
Treat*Post	**– 1.475*****	**– 1.998*****	**– 0.239**	**– 0.194**
	(0.540)	**(0.661)**	**(0.165)**	**(0.209)**
Post ∗ CCI	2.343***	2.525***	– 0.399	– 0.219
	(0.869)	(0.865)	(0.288)	(0.247)
Post	0.352	0.256	– 0.089	– 0.027
	(0.325)	(0.277)	(0.119)	(0.114)
ResidN	– 1.733***	– 1.591***	– 0.582***	– 0.704***
	(0.424)	(0.345)	(0.175)	(0.169)
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	2199	2199	2199	2199
Adjusted R^2	0.450	0.482	0.598	0.624

Notes: This table presents the results of low compensation committee independence in the pre-period regarding the effect of longer duration on investment efficiency. The table shows pooled panel OLS regression coefficients from the second step of the two-step approach used before. Panel A focuses on the treated German firms. Panel B extends the analyzes to the full sample and shows the incremental treatment effect for a low level of compensation committee independence in the pre-period. In both panels, the four columns correspond to the combinations of the two alternative investment measures with the two alternative investment models. First-step investment model regressors, other control variables, intercept, and fixed effect dummies are not tabulated. All variables are defined in Appendix A. Standard errors are shown in parentheses. *, **, *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

independence (*CCI*) to be 1 for firms that are in the lowest quartile for the score in 2007 of the pre-period and 0 otherwise. Panel A in Table 5 shows the results for the four different specifications regarding investment measures and models. All four interactions between *CCI* and the *Post* dummy are negative, with three of them being significant, indicating that the reduction in abnormal investment is driven by those firms that had a low degree of compensation committee independence prior to the mandate to lengthen the duration.

Second, I extend the analysis to the full sample, including a three-way interaction between *Treat*, *Post*, and *CCI*, and present the results in Panel B of Table 5.[35] The three-way interaction is negative in all four specifications and significant in three of them, indicating that German firms with a low degree of independence reduce their abnormal investment more than European firms with low independence after the treatment. The DID effect (*Treat*Post*) continues to be

[35]The results in Panel B of Table 5 are based on a reduced sample of 2199 firm-years for which data on compensation committee independence is available. As before, I cluster standard errors on the country-year level in Panel B, which reduces to year-level clusters in Panel A, which focuses on German firms.

significantly negative in two specifications. This indicates that there is a treatment effect in firms with low independence, which is incremental to the general treatment effect.

I further analyze a broad measure of firms' corporate governance using the overall corporate governance pillar score from the same database.[36] In line with the discussion by Gopalan et al. (2014) that pay duration can be a substitute or a complement to alternate forms of corporate governance such as monitoring, there is tension as to whether or not the treatment effect depends on aggregated corporate governance characteristics. I also define *Gov* to be 1 for firms that are in the lowest quartile of German firms' 2007 score and 0 otherwise. Table 6, Panel A shows the results for German firms for which the variable is available and Panel B extends the analysis to the full sample. The significantly negative coefficient in three of the four specifications regarding the interaction of *Gov* and *Post* indicates that the reduction in abnormal investment is driven by firms with generally weak corporate governance in the pre-period. In Panel B, the three-way interaction is significant for the two *C/PPE* specifications, while the *Treat*Post* interaction is significant in all four. This again provides some support for an incremental treatment effect on investment efficiency for firms with particularly low overall corporate governance.

5.4. Further Analyses

5.4.1. German control group of private firms

My main tests cannot fully control for contemporaneous country-level forces that may have affected firms' investment efficiency. It could be that German firms increased their investment efficiency (following the GFC) compared to their European peers, for reasons other than or in addition to the VorstAG treatment. For instance, in 2009, the German government offered several forms of financial aid to stabilize the economy. This may have forced firms to directly address inefficient investment to make them eligible for governmental support. Firms may have been forced to direct funds from former over-investment to support the current workforce and other stakeholders. Also, in the aftermath of the GFC, the public outcry and subsequent scrutiny regarding inefficient executive behavior may have been stronger in Germany than in other countries, which may have disciplined German executives regardless of the change in compensation incentives.

To rule out this concern, I obtain an alternative control group of untreated private German firms. I sample firms from Bureau van Dijk's Dafne database (e.g., Engel & Middendorf, 2009) with a minimum of 5000 employees. I ascertain that the firms in this sample have a legal form other than a stock corporation, as the VorstAG was implemented as an amendment to the German stock corporation law and there exist some privately owned stock corporations that are not publicly listed. I retain only those firms for which there is at least some data in the pre- and post-periods. I further remove firms owned by a group, the government (e.g., major municipal utilities companies), or the church (e.g., church run healthcare companies). Appendix D lists the names of the 32 firms included in this control group. The control group comprises 160 firm-years, after excluding the transition year, which results in 428 firm-years, after adding the 268 treated German firm-years.

I focus on the investment variable CAPEX scaled by average PPE, as R&D expenses are only available for two firms. I employ the accelerator model as before, because the Tobin's q model requires stock market data. Table 7 shows the results of the second step of comparing

[36]The corporate governance pillar score aggregates 35 metrics related to the management category (i.e., executive board structure and compensation characteristics), 12 shareholder category metrics (i.e., shareholder rights and the use of takeover defense mechanisms), and 9 corporate social responsibility metrics into a single score.

Table 6. Cross-sectional analyzes of low corporate governance pillar score.

Dependent variable:	$\lvert C/PPE_{Rev}\rvert$ (1)	$\lvert C/PPE_{Q}\rvert$ (2)	$\lvert I/TA_{Rev}\rvert$ (3)	$\lvert I/TA_{Q}\rvert$ (4)
Panel A: Treated German firms				
Gov*Post	**– 2.857***	**– 2.361**	**– 0.810****	**– 0.661*****
	(1.495)	**(1.512)**	**(0.362)**	**(0.233)**
Post	– 0.333	– 0.922***	– 0.201	– 0.232
	(0.627)	(0.280)	(0.191)	(0.222)
ResidN	– 2.273**	– 1.727***	– 0.854	– 0.640
	(1.078)	(0.659)	(0.545)	(0.465)
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	259	259	259	259
Adjusted R^2	0.671	0.678	0.481	0.466
Panel B: Full sample				
Treat*Post*Gov	**– 3.846*****	**– 3.777*****	**– 0.281**	**– 0.027**
	(1.308)	**(1.328)**	**(0.421)**	**(0.366)**
Treat*Post	**– 1.370****	**– 1.579*****	**– 0.475****	**– 0.477***
	(0.642)	**(0.591)**	**(0.217)**	**(0.276)**
Post ∗ Gov	1.084*	1.200*	– 0.484**	– 0.594***
	(0.658)	(0.650)	(0.229)	(0.201)
Post	0.203	0.114	– 0.001	0.104
	(0.393)	(0.348)	(0.133)	(0.125)
ResidN	– 1.826***	– 1.798***	– 0.608***	– 0.727***
	(0.450)	(0.374)	(0.183)	(0.188)
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	2316	2316	2316	2316
Adjusted R^2	0.443	0.476	0.585	0.611

Notes: This table presents the results of low overall corporate governance in the pre-period regarding the effect of longer duration on investment efficiency. The table shows pooled panel OLS regression coefficients from the second step of the two-step approach used before. Panel A focuses on the treated German firms. Panel B extends the analyzes to the full sample and shows the incremental treatment effect for low corporate governance in the pre-period. In both panels, the four columns correspond to the combinations of the two alternative investment measures with the two alternative investment models. First-step investment model regressors, other control variables, intercept, and fixed effect dummies are not tabulated. All variables are defined in Appendix A. Standard errors are shown in parentheses. *, **, *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

treated German firms with the private German control group across the same sample period as before. I use two different strategies to estimate the investment model for this test. First, I add the private German firms to the full sample of European firms (columns (1) and (2)). Second, I estimate the investment model for German firms only (columns (3) and (4)). I include all control variables as before with the exception of Altman's Z score, which requires market data.[37] The DID interaction is negative in all four specifications and significant in three. The effect sizes are comparable to the figures in the main sample. The distribution of the coefficients of the control variables shows some more diverse and extreme values, as this sample exhibits more variation

[37]I redefine *Lever* as total liabilities over total assets, because total debt is not available from Dafne. I winsorize the continuous variables for the private German firms on the 2.5% level. The specifications employing firm fixed effects (columns (1) and (3)) show yearly clustered standard errors, as the country-year clusters used before do not apply to this single-country sample.

Table 7. Regression analyzes for the German control group.

	Dependent variable: $\lvert C/PPE_{Rev} \rvert$			
	Investment model: Full sample		*Investment model: German sample*	
	Main specification (1)	Alternative FEs (2)	Main specification (3)	Alternative FEs (4)
Treat ∗ Post	**− 2.110****	**− 2.088**	**− 3.731*****	**− 3.321***
	(0.871)	**(2.009)**	**(1.352)**	**(1.725)**
Treat		− 0.448		1.386
		(2.061)		(2.031)
Post	1.221		3.208**	
	(1.155)		(1.612)	
ResidN	− 2.430***	− 5.329***	− 1.614**	− 4.031***
	(0.761)	(1.533)	(0.679)	(1.270)
Lever	− 9.591	6.003	4.552	4.427
	(6.232)	(5.688)	(5.956)	(4.833)
log(*Size*)	− 3.503**	0.086	− 1.226	0.188
	(1.411)	(0.429)	(2.545)	(0.450)
Regul	29.763	− 5.128*	13.662	− 5.428**
	(23.303)	(2.942)	(20.803)	(2.636)
Homog	− 1.804	0.937**	− 1.006	0.862**
	(1.321)	(0.426)	(1.212)	(0.350)
Tangb	− 8.635	− 14.583***	− 2.443	− 12.543***
	(7.507)	(5.077)	(5.144)	(4.653)
Slack	− 1.207	19.241*	− 7.032	10.528
	(10.087)	(9.858)	(9.042)	(7.464)
Intercept	100.460**	− 11.702	41.018	− 14.238
	(44.701)	(12.976)	(53.734)	(10.680)
Firm FE	Yes		Yes	
FF12 + Year FE		Yes		Yes
Observations	428	428	428	428
Adjusted R^2	0.542	0.354	0.520	0.325

Notes: This table presents the results of the effect of longer duration on investment efficiency for an alternative control sample of untreated German firms. The table shows pooled panel OLS regression coefficients from the second step of the two-step approach used before. The dependent variable in all columns is $\lvert C/PPE_{Rev} \rvert$, which is the absolute residual of regressions with *C/PPE* as investment measure based on the accelerator model. In columns (1) and (2), the first step investment regression is estimated on the main sample (including other European firms) supplemented with the untreated German firms. Alternatively, in columns (3) and (4), the first step investment regression is estimated on the German firms only. The respective first-step regressors are included in the second step, but not tabulated. The dummies for the respective fixed effects are also included but not tabulated. All variables are defined in Appendix A. Standard errors are shown in parentheses. *, **, *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

in these firm characteristics. I also obtain significant results when I do not include the control variables.

It could still be, however, that the potential mechanisms in Germany described above simply affected the listed firms more strongly than the private firms, as the public outcry and scrutiny may have focused on the more high-profile listed firms. I would interpret private German firms exhibiting lower levels of abnormal investment when compared with the other European firms as indicative of this conjecture. Untabulated tests show that when comparing private German firms with other European firms, the DID coefficient is insignificantly positive (close to zero), therefore providing no support for this concern. I view the results for this alternative control group as providing strong additional support for the claim that longer duration of executive compensation fosters investment efficiency.

5.4.2. Over-investment vs. under-investment

I further attempt to assess which reductions are larger: those in over-investment or those in under-investment. On the one hand, it may be that mandated longer duration more easily prevents empire builders from wasting resources (Jensen, 1986) than it can encourage executives with a tendency to under-invest to invest at the optimal level. It may be very effective to force an over-investing executive to internalize later negative effects of his investment decisions via long-term compensation contracts. At the same time, long-term incentives may be rather ineffective in incentivizing a rather conservative executive to spend more capital on long-term projects, especially when career concerns (Holmstrom & Costa, 1986) dominate. Under this line of argument one could expect greater reductions in over-investment compared to reductions in under-investment.

On the other hand, the tendency to spend too many resources appears to be a stronger signal of some executive power (Bebchuk & Fried, 2003; Jensen & Meckling, 1976). Powerful executives may dominate the contract-writing process regardless of regulatory requirements for long-term duration, rendering the effect of the VorstAG weak. This would result in a smaller reduction in over-investment than in under-investment. It is possible that reductions in over- or under-investment are also a function of which behavior predominates, on average, among the sample firms.

The empirical literature on investment efficiency does not yet provide a framework for investigating the relative likelihood or magnitude of over- and under-investment. I run several tests concerning this question. First, I repeat the regressions of the main models and include the three-way interaction between the DID variables and the dummy that is coded 0 for positive and 1 for negative residuals of the respective investment model (*Treat*Post*ResidN*). This approach measures whether the treatment effect is different for over- or under-investing firm-years.

Table 8 shows the results for the four different combinations of investment measures and models, $|C/PPE_{Rev}|$, $|C/PPE_{Q}|$, $|I/TA_{Rev}|$, and $|I/TA_{Q}|$. Panel A reports the regressions for the full control sample and Panel B shows results for the PSM sample. I report results for firm fixed effects, the combination of industry and year fixed effects produces very similar results. The three-way interaction coefficients are positive and generally significant. All DID coefficients are negative and significant. The latter measure the reduction in average over-investment magnitude following treatment. For the reduction in average under-investment magnitude, it is necessary to total the two coefficients of each column. For instance, in the Tobin's q model with the full control sample (column (2) of Panel A), the coefficient for the reduction in the magnitude of over-investment is 4.469, whereas the coefficient for the reduction in the magnitude of under-investment is $0.657(-4.469 + 3.812 = -0.657)$. Scaling these coefficients with the signed means of the first-step residuals results in 43% (4.469/10.41) for the reduction in over-investment and 9% (0.657/7.41) for the reduction in under-investment. For columns (1) and (4) of Panel A and columns (1) and (2) of Panel B, the coefficients even indicate an increase in the magnitude of under-investment.[38] This analysis shows that the VorstAG treatment has a larger effect on the average magnitude of over-investment.

The results in Table 8 would also be in line with slight over-investment becoming more frequent after treatment, depressing the average magnitude. Therefore, in an alternative approach, I redefine *ResidN-Pre* as 1 if the average investment residual of a firm in the pre-period is negative and 0 otherwise, to assess the relative treatment effect on former over-investors relative to former under-investors. The results tabulated in Appendix F indicate that the treatment effect on the size

[38]As Eisdorfer et al. (2013) find that executives under-invest when debt-like compensation is predominant, it may be inferred that the VorstAG act led to some emphasis on debt-like compensation components.

Table 8. Differentiating between over-investment and under-investment.

Dependent variable:	$\lvert C/PPE_{Rev}\rvert$ (1)	$\lvert C/PPE_{Q}\rvert$ (2)	$\lvert I/TA_{Rev}\rvert$ (3)	$\lvert I/TA_{Q}\rvert$ (4)
Panel A: Full sample				
Treat ∗ Post ∗ ResidN	**6.308*****	**3.812****	**0.658**	**0.930****
	(1.341)	**(1.684)**	**(0.522)**	**(0.374)**
Treat*Post	**– 5.940*****	**– 4.469*****	**– 0.720****	**– 0.829****
	(1.155)	**(1.218)**	**(0.349)**	**(0.344)**
Treat ∗ ResidN	– 2.719**	– 1.781	– 0.195	– 0.067
	(0.895)	(1.097)	(0.348)	(0.346)
Post ∗ ResidN	0.370	0.526	0.832***	0.749***
	(0.754)	(0.678)	(0.229)	(0.177)
Post	0.227	0.035	– 0.631***	– 0.487***
	(0.641)	(0.590)	(0.199)	(0.177)
ResidN	– 2.222***	– 2.282***	– 1.068***	– 1.143***
	(0.628)	(0.583)	(0.228)	(0.193)
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	2490	2490	2490	2490
Adjusted R^2	0.458	0.482	0.570	0.598
Panel B: PSM subsample				
Treat ∗ Post ∗ ResidN	**5.414*****	**3.431***	**0.883***	**0.963***
	(1.857)	**(2.052)**	**(0.528)**	**(0.502)**
Treat*Post	**– 5.719*****	**– 4.687*****	**– 1.176*****	**– 1.188*****
	(1.600)	**(1.460)**	**(0.349)**	**(0.422)**
Treat ∗ ResidN	– 3.286*	– 3.284**	– 0.777**	– 0.927**
	(1.716)	(1.523)	(0.360)	(0.391)
Post ∗ ResidN	1.657	0.979	0.516	0.649*
	(1.574)	(1.527)	(0.316)	(0.382)
Post	– 0.083	0.090	– 0.080	– 0.123
	(1.301)	(1.278)	(0.314)	(0.364)
ResidN	– 2.128	– 0.954	– 0.602**	– 0.492*
	(1.564)	(1.308)	(0.279)	(0.280)
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	523	523	523	523
Adjusted R^2	0.630	0.648	0.488	0.514

Notes: This table presents the results of the treatment effect of longer duration on over-investment and under-investment. *ResidN* is coded 0 for positive and 1 for negative residuals of the respective investment model. The coefficient of the DID interaction (i.e., the second line) measures the reduction in average over-investment magnitude following treatment. The total of the coefficient of the three-way interaction (i.e., the first line) and the DID interaction measures the reduction in average under-investment magnitude following treatment. In both panels, the four columns correspond to the combinations of the two alternative investment measures with the two alternative investment models. First-step investment model regressors, other control variables, intercept, and fixed effect dummies are not tabulated. All variables are defined in Appendix A. Standard errors are shown in parentheses. *, **, *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

of the residuals for former over-investors is larger than for former under-investors.[39] I draw the same inference when using signed residuals as the dependent variable. Finally, I draw the same inferences when looking at extreme values of former over- and under-investment (i.e., above the third quarter and below the first quarter of the average investment residual in the pre-period) and when looking at the last year before the treatment (i.e., 2008) only. I am therefore inclined

[39]For instance, column (2) in Panel A of Appendix F shows that the coefficient for the reduction in abnormal investment of former over-investors is 4.310, while it is 0.432 for former under-investors ($-4.310 + 3.878 = -0.432$).

to conclude that the mandated longer duration hampers over-investment more than it reduces under-investment.

5.4.3. Investment aggregated at industry level

My main tests assume that investment is efficient on the industry level over time. However, at times, whole industries may be over- or under-investing. In such times, it is more efficient to invest counter cyclically to one's industry peers. Therefore, in a further robustness analysis, I use investment aggregated at the industry level to proxy for the likelihood of over- and under-investing. I measure the industry-year average of the two investment measures and the independent variables of the investment model variables from before. I follow Lara et al. (2016) and Biddle et al. (2009) and use the accelerator model in this approach. I estimate Equation (7) cross-industrially for the financial years 2006 to 2012.

$$Invest_{Ind,t} = \alpha + \beta_1 RevCh_{Ind,t-1} + \beta_2 RevChN_{Ind,t-1} + \beta_3 RevCh * RevChN_{Ind,t-1} + \varepsilon_{Ind,t} \quad (7)$$

I rank the residuals from this regression into deciles, which I rescale from 0 to 1 to form estimates of aggregate over-investment for the industry-years (*OverInd*).[40] *OverInd* indicates situations in which firms are likely to over-invest because of general over-investment throughout the industry. I then estimate Equation (8), including the interaction of *OverInd* with the DID dummies, all investment model and control variables from before, and fixed effects on the firm or industry and year level. I again exclude the transition year 2009 from this regression. A negative coefficient on the three-way interaction between *Treat*, *Post* and *OverInd* would imply a lower level of investment in situations prone to over-investment for treated firms in the post-period.

$$\begin{aligned} Invest_{i,t} = \alpha &+ \beta_1 Treat_i * Post_t * OverInd_{Ind,t} + \beta_2 Treat_i * Post_t \\ &+ \beta_3 Treat_i * OverInd_{Ind,t} + \beta_4 Post_t * OverInd_{Ind,t} + \beta_5 Treat_i \\ &+ \beta_6 Post_t + \beta_7 OverInd_{Ind,t} + Controls + FEs + \varepsilon_{i,t} \end{aligned} \quad (8)$$

Table 9 presents the results of this approach. Columns (1) to (3) ((4) to (6)) correspond to investment measure *C/PPE* (*I/TA*). Columns (1), (2), (4) and (5) show the results for the full control sample with different combinations of fixed effects. Columns (3) and (6) show the results for the PSM control sample. The coefficients of the three-way interactions are negative and significant for five of the six specifications. The coefficients for the DID interaction are positive but mostly insignificant.[41] Firms with longer duration display lower over-investment when this is likely due to the preponderance of over-investment throughout the industry. However, there is less support for higher investment in treated firms in the absence of industry-level over-investment. The coefficients for the variable *OverInd* are mostly significantly positive, which shows that firms generally invest more when there is over-investment in the industry. These results provide further support for a reduction in abnormal investment following the VorstAG treatment. They also provide support for the notion that mandated longer duration more likely leads to less over-investment than under-investment.

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[40] I obtain similar results when ranking the residuals into quartiles or yearly quartiles.

[41] For *C/PPE*, the regression at the industry level has very low explanatory power with an adjusted R^2 of -2% (for *I/TA* the adjusted R^2 is 9%). I include the average *CFO* in this regression, as I also do in my main analyzes (not tabulated). This increases the adjusted R^2 to 26% for the industry mean regression with *C/PPE* and leads to similar results in the second step.

Table 9. Regression analyzes for the mean industry investment approach.

	Dependent variable:C/PPE			*Dependent variable:I/TA*		
	Main specification (1)	Alternative FEs (2)	PSM subsample (3)	Main specification (4)	Alternative FEs (5)	PSM subsample (6)
Treat ∗ Post ∗ OverInd	**– 5.897***** **(2.195)**	**– 9.380*** **(5.157)**	**– 3.307** **(3.727)**	**– 1.781***** **(0.634)**	**– 3.140*** **(1.611)**	**– 2.396*** **(0.791)**
Treat ∗ Post	3.103 (1.961)	2.273 (2.550)	1.000 (2.400)	0.871** (0.388)	1.233* (0.722)	0.701 (0.449)
Treat ∗ OverInd	11.905*** (3.011)	4.561 (6.781)	12.459*** (3.961)	3.014*** (0.770)	– 0.395 (1.821)	4.078*** (0.805)
Post ∗ OverInd	2.353* (1.410)	0.358 (2.030)	0.486 (2.873)	0.366 (0.448)	0.243 (0.654)	1.505** (0.592)
Treat		0.711 (3.589)			0.984 (0.857)	
Post	– 2.448** (1.094)		– 0.104 (1.633)	– 0.940*** (0.277)		– 0.942** (0.387)
OverInd	6.604** (2.069)	10.780*** (2.565)	7.826** (3.328)	1.470*** (0.523)	2.922*** (0.804)	0.658 (0.622)
Firm FE	Yes		Yes	Yes		Yes
FF12 + Year FE		Yes			Yes	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2490	2490	523	2490	2490	523
Adjusted R^2	0.691	0.370	0.732	0.796	0.394	0.827

Notes: This table presents the results of the treatment effect of longer duration on investment efficiency for an alternative approach. In this approach, an industry average regression (Equation (7)) is used to obtain a proxy for the likelihood of over- and under-investing. *OverInd* measures the likelihood of over-investing as the ranked deciles (scaled from 0 to 1) of the industry aggregate regression. The tabulated results are obtained from estimating Equation (8). Control variables, intercept, and fixed effect dummies are included but not tabulated. All variables are defined in Appendix A. Standard errors are shown in parentheses. *, **, *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

5.5. *Reconciliation with Announcement Returns*

I find consistent results showing that mandated longer duration reduces abnormal investment. Early evidence on announcement returns for voluntary adopters of a particular longer duration compensation component are positive (R. Kumar & Sopariwala, 1992). However, previous research indicates that the mean stock market reaction to the VorstAG regulation was negative (Hitz & Müller-Bloch, 2015), suggesting that this regulation was perceived to involve costs to shareholders. Linked to increased incentive duration, a substantial body of literature states that having more debt-like compensation features better aligns executives with debtholders and, in particular, decreases the riskiness of firm investment (e.g., Cassell et al., 2012; He, 2015) as debt-like compensation features reduce risk-shifting over-investments (Edmans & Liu, 2011).[42] As different capital providers may have different investment preferences that follow from their different payoff structures (Eisdorfer et al., 2013; Roychowdhury et al., 2019), shareholders may have anticipated the mandate to inefficiently affect firms' risk-taking. The results that indicate a weak or potentially negative effect on firms' under-investment could be further in line with this notion.

Similarly, while the likely intention of the German (stakeholder-oriented) government was to force executive compensation incentive contracting closer to optimality, the act may have pushed at least some firms out of optimality. Shareholders may have anticipated that, by introducing

[42]This literature mainly focuses on pensions and deferred compensation, viewing such pay as an unsecured liability to the firm. While pension benefits were not subject to the VorstAG act, many firms increased the deferred portion of pay.

contracting constraints, the VorstAG act would only be beneficial to a few firms and detrimental to others. The government may even have regulated all firms while actually targeting only a subset with particularly suboptimal incentive structures. My cross-sectional results indicate that much of the effect on investment efficiency is driven by a small number of firms that had particularly weak compensation-related governance in the pre-period. This seems in line with further announcement return analyzes, indicating that firms with higher abnormal compensation had lower returns, and firms with higher pay-performance sensitivity had higher returns (Hitz & Müller-Bloch, 2015).

6. Conclusion

I exploit an exogenous trigger of a regulation designed to prolong the duration of executive compensation to investigate whether this improves investment efficiency. Mandated longer duration increases the length of executives' evaluation window, which could force them to internalize the unprofitability of today's empire building that materializes in the future, reducing over-investment. At the same time, the longer evaluation window may mitigate myopic disincentives for current-period investments with future payoffs or incentives for over-signaling, reducing under- or over-investment. However, such a regulation may also interfere with efficient contracting or it may be immaterial if it is too vague.

Subjecting this to a series of tests, I find statistically and economically significant improvements in investment efficiency following the regulation. The results are robust to a wide range of (abnormal) investment specifications and different plausible control groups. Cross-sectional analyzes suggest that the effects are driven by firms with weak corporate governance characteristics, in particular, firms whose compensation committee had a low degree of independence before the regulation. It further appears that these improvements are mainly driven by reductions in over-investment.

Overall, my findings support the notion that long-term orientation in executive compensation serves a role in mitigating frictions that hamper efficient investment decisions. Governmental and regulatory intervention into corporate governance in general and executive compensation in particular is frequently discussed and demanded. This paper underscores how regulation potentially affects real firm behavior and emphasizes the need to report and investigate the economic impacts of such intervention.

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Appendix A. Variable Definitions

Variable	Definition and Thomson Reuters Datastream mnemonic
Investment variables:	
C/PPE_t	CAPEX (WC04601) scaled by average PPE (WC02501)
C/PPE_{Rev}	Residual from regressing *C/PPE* based on the accelerator model, Equation (4)
C/PPE_Q	Residual from regressing *C/PPE* based on Tobin's q model, Equation (5)
I/TA_t	CAPEX (WC04601) plus R&D (WC01201) over average total assets (WC02999), whereby missing values for R&D are set to 0 when CAPEX are available
I/TA_{Rev}	Residual from regressing *I/TA* based on the accelerator model, Equation (4)
I/TA_Q	Residual from regressing *I/TA* based on Tobin's q model, Equation (5)
Investment model variables:	
$RevCh_{t-1}$	Change in revenues (WC01001) scaled by previous year's revenues
$RevChN_{t-1}$	Indicator variable set to 1 when the change in revenues is negative, 0 otherwise
Q_{t-1}	Beginning-of-year enterprise value (WC18100) divided by beginning-of-year total assets (WC02999)
CFO_t	Cash flow from operating activities (WC04860) over average total assets (WC02999)
Control variables:	
$Lever_{t-1}$	Leverage is calculated as beginning-of-year total debt (WC03255) over beginning of year total assets (WC02999)
$Size_{t-1}$	Natural logarithm of a firm's number of employees (WC07011) as of the beginning of the year
Z_t	Altman's Z score is calculated as (1.2 times beginning-of-year working capital (WC03151) + 1.4 times retained earnings (WC03495) + 3.3 times EBIT (WC18191) + 0.999 times revenues (WC01001)/beginning-of-year total assets (WC02999) + 0.6 times (beginning-of-year market capitalization (WC08001)/beginning-of-year total liabilities (WC03351))
Regul	Indicator variable set to 1 for regulated industries such as utilities (SIC 49) and airlines & railroads (SIC 40-47)
Homog	Variable measuring industry homogeneity based on the data by Parrino (1997), for industries with missing data I calculate the average of the next higher SIC level
$Tangb_{t-1}$	Asset tangibility is calculated as beginning-of-year PPE (WC02501) over beginning-of-year total assets (WC02999)
$Slack_{t-1}$	Financial slack is calculated as beginning-of-year Cash (WC02003), if missing, Cash generic (WC02005), over beginning-of-year total assets (WC02999)
Cross-sectional variables:	
Gov	Dummy variable coded to 1 for firms that are in the lowest quartile of German firms' 2007 corporate governance pillar score (CGSCORE)
CCI	Dummy variable coded to 1 for firms that are in the lowest quartile of German firms' 2007 compensation committee independence (CGBFO04V)

Appendix B. Changes in Remuneration Systems – Exemplary Extract From Online Supplement

This appendix lists hand-collected indications of how German firms implemented the VorstAG as stipulated in their annual reports, exemplary for three firms. The online Supplement at the journal's Taylor and Francis website provides the full list of descriptions. The change categories numbered from 1 to 6 refer to the summary of frequencies tabulated in Appendix C.

BASF SE: In the second half of 2009, the compensation system for Board members was aligned with the German Act on the Appropriateness of Management Board Remuneration (. . .) Contracts with all Board members, regardless of existing contractual terms, were consensually and uniformly converted to this system effective as of January 1, 2010. (. . .) The main objectives of this were to give greater relevance to the component based on long-term success and to make compensation for the Board of Executive Directors even more performance-oriented. (. . .) The information presented below relates to the compensation system valid until December 31, 2009 and the respective compensation of Board members for 2009. (. . .) The return on assets (ROA) is used to determine the variable compensation of all employee groups and also determines – 2009 for the last time as sole basis – the Board members' annual variable compensation (variable bonus). (AR 2009) The annual variable compensation (variable bonus) of the Board of Executive Directors is based on the performance of the entire Board and the return on assets. (. . .) The Supervisory Board assesses the achievement of goals in relation to the last three years. (AR 2010) *Change categories: 3,1*

Deutsche Post AG: The remuneration component linked to the company's annual profits now also includes a sustainability component in line with the provisions of the (. . .) (VorstAG) (. . .) the annual performance related remuneration will in future no longer be paid in full for the year on the basis of having reached the agreed targets. Instead, 50% of the annual performance-related remuneration will flow into a new medium-term component with a three-year calculation period (performance phase of one year, sustainability phase of two years). (AR 2009) *Change category: 2*

Wincor Nixdorf AG: We plan to submit a new share option program for members of the Board of Directors and others with a subscription entitlement for approval by the AGM in 2010. The changes in the new program, which is based on the existing one, are intended, in part, to ensure compliance with (. . .) (AR 2008/2009) In accordance with the requirements of Germany's VorstAG Act, the vesting period for share options granted under the 2010 share option program has been extended from two to four years. (AR 2009/2010) *Change category: 4*

Appendix C. Summary of Changes in Remuneration Systems

Categories of change in remuneration system	Frequency of change	
	Main change	All changes
1 Multi-year performance assessment[a]	15	18
2 Deferral of a portion of variable components[a]	11	17
3 Gear performance-related components to long-term determinants[b]	7	8
4 Increase in vesting period of stocks or stock options[c]	4	12
5 Adoption of a bonus-malus-system[d]	4	6
6 Requirement of investment in own stocks or stock options[e]	0	11
() Apparently no or likely very minor change	5	5
Total	46	77

Notes: This appendix lists the categories of the changes in the remuneration systems observed in the 46 treated German firms. *Main change* refers to the frequency of a particular category being regarded as the main change in the executive compensation system (i.e., the first change category listed per firm in the online Supplement) and *All changes* refers to the total frequency of the particular category.

[a]Mostly refers to the adoption of such a multi-year assessment system or a deferral of awarded variable compensation, and sometimes implies that an existing system was used for a larger portion of the components.

[b]Refers to changing the determinants or lifting the ratio of long-term to short-term components.

[c]Mostly refers to an increase in the vesting period to four years. This is often implemented in addition to a main change in a component of the system.

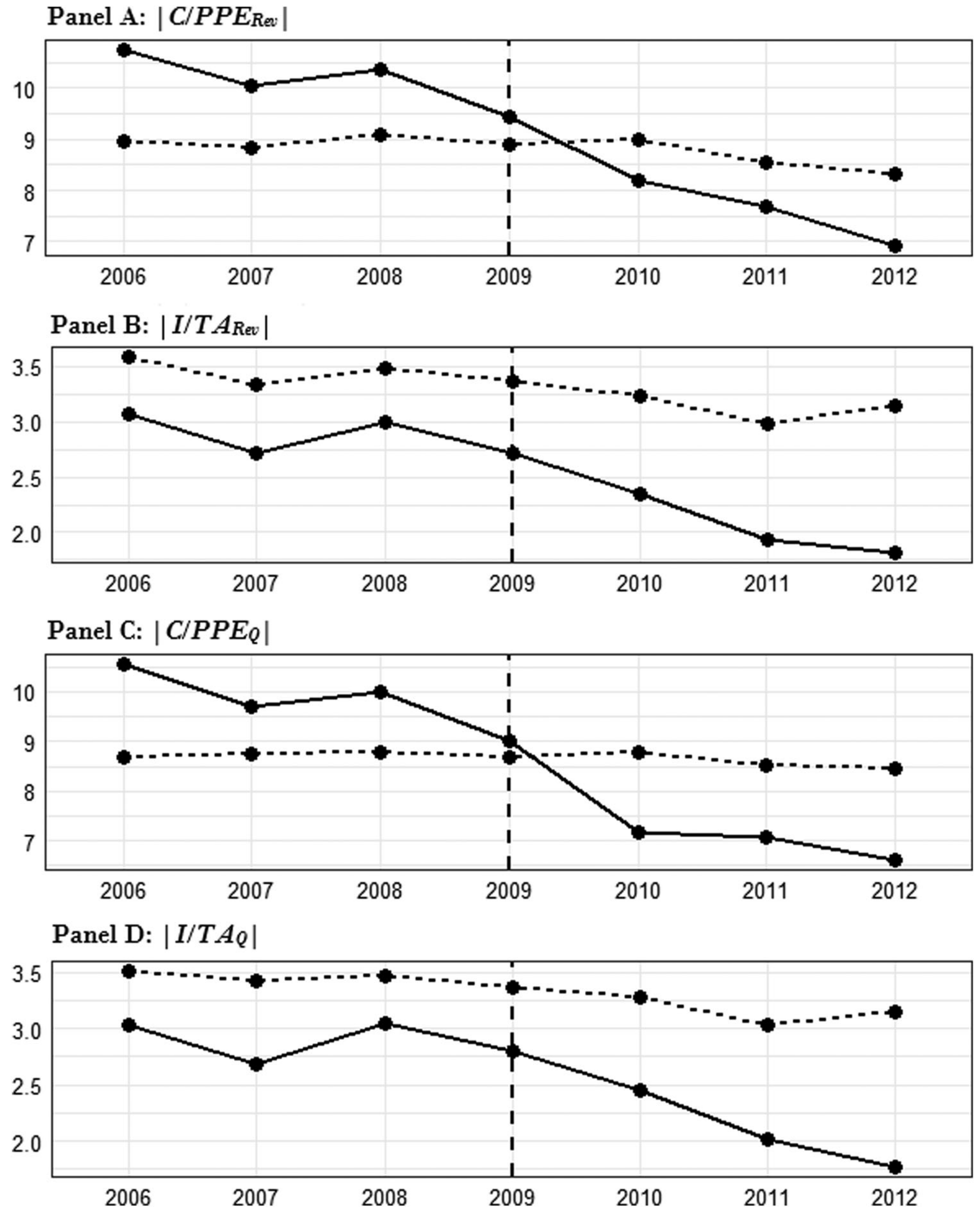
[d]Only refers to instances where this is precisely stipulated; however, it is likely that some more multi-year assessment or deferral schemes contain a bonus-malus or conditional payback provision.

[e]Is never implemented as the main change and is mostly implemented in addition to deferral provisions.

Appendix D. List of Private German Firms

Adolf Würth GmbH & Co. KG, DATEV eG, Dehner Gartencenter GmbH & Co. KG, Diehl Stiftung & Co. KG, Dr. Johannes Heidenhain GmbH, E.G.O. Blanc und Fischer & Co. GmbH, Eberspächer Gruppe GmbH & Co. KG, EHG Service GmbH, Fiege Logistik Holding Stiftung & Co. KG, Friedhelm Loh Stiftung & Co. KG, Fritz Dräxlmaier GmbH & Co. KG, Gebr. Knauf KG, Georgsmarienhütte Holding GmbH, Häfele GmbH & Co. KG, Hettich Holding GmbH & Co. oHG, Hoyer GmbH Internationale Fachspedition, I.K. Hofmann GmbH, K+K Klaas & Kock B.V. & Co. KG, KAEFER Isoliertechnik GmbH & Co. KG, Katharina Kasper Holding GmbH, L. Possehl & Co. mbH, Lohmann GmbH & Co. KG, MAHLE GmbH, Mann + Hummel Holding GmbH, MESSER Group GmbH, Piepenbrock Unternehmensgruppe GmbH + Co. KG, Rohde & Schwarz GmbH & Co. KG, SRH-Holding SdbR, TRUMPF GmbH + Co. KG, Vorwerk & Co. KG, W-E-G GmbH & Co. KG, Wilh. Werhahn KG

Appendix E. Pattern of the Average Dependent Variable Per Treatment and Control Group



Notes: This appendix plots the average abnormal investment dependent variable per treatment (solid line) and control group (dashed line) to visualize the treatment effect by year over the sample period. Panels A and B map the coefficients for $|C/PPE_{Rev}|$ and $|I/TA_{Rev}|$, which are the absolute values of the regression residuals based on the accelerator model using the two alternative investment measures CAPEX scaled by average PPE and the sum of CAPEX and R&D scaled by average total assets. Panels C and D show results for both investment measures and the alternative Tobin's q approach (i.e., $|C/PPE_Q|$ and $|I/TA_Q|$).

Appendix F. Alternative Differentiation Between Over-Investment and Under-Investment

Dependent variable:	$\lvert C/PPE_{Rev}\rvert$ (1)	$\lvert C/PPE_{Q}\rvert$ (2)	$\lvert I/TA_{Rev}\rvert$ (3)	$\lvert I/TA_{Q}\rvert$ (4)
Panel A: Full sample				
Treat ∗ Post ∗ ResidN − Pre	**4.342*** **	**3.878*** **	**0.582**	**0.878** **
	(1.492)	**(1.197)**	**(0.487)**	**(0.431)**
Treat*Post	**− 4.272*** **	**− 4.310*** **	**− 0.632* **	**− 0.758** **
	(0.960)	**(0.833)**	**(0.353)**	**(0.384)**
Post ∗ ResidN − Pre	1.720**	1.157*	0.981***	1.038***
	(0.828)	(0.644)	(0.217)	(0.180)
Post	− 0.721	− 0.460	− 0.728***	− 0.685***
	(0.687)	(0.579)	(0.194)	(0.185)
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	2477	2477	2477	2477
Adjusted R^2	0.441	0.469	0.570	0.598
Panel B: PSM subsample				
Treat ∗ Post ∗ ResidN − Pre	**5.728*** **	**5.131*** **	**0.637**	**0.920** **
	(1.703)	**(1.462)**	**(0.490)**	**(0.455)**
Treat*Post	**− 5.529*** **	**− 5.571*** **	**− 0.945*** **	**− 1.144*** **
	(1.393)	**(1.172)**	**(0.359)**	**(0.369)**
Post ∗ ResidN − Pre	0.795	− 0.170	0.900***	0.960***
	(1.304)	(1.031)	(0.295)	(0.250)
Post	0.453	0.707	− 0.401	− 0.350
	(1.264)	(1.035)	(0.294)	(0.252)
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	523	523	523	523
Adjusted R^2	0.626	0.650	0.493	0.527

Notes: This appendix presents the results of the treatment effect of longer duration on former over-investors and former under-investors. *ResidN-Pre* is coded 1 if the average investment residual of a firm in the pre-period is negative and 0 otherwise. The coefficient of the DID interaction (i.e., the second line) measures the reduction in abnormal investment of former over-investors following treatment. The total of the coefficient of the three-way interaction (i.e., the first line) and the DID interaction measures the reduction in abnormal investment of former under-investors following treatment. In both panels, the four columns correspond to the combinations of the two alternative investment measures with the two alternative investment models. First-step investment model regressors, other control variables, intercept, and fixed effect dummies are not tabulated. All variables are defined in Appendix A. Standard errors are shown in parentheses. *, **, ***denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.