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How firm value reacts to exogenous shocks: evidence from the announcements of takeover defenses and the propagation of Covid-19

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DISSERTATION

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Preface

This dissertation strongly benefitted from the contribution and support of my supervisors, co-authors, family, and friends. I would like to express my sincere gratitude to all who accompanied and supported me during my time as a doctoral student.

First and foremost, I would like to thank my supervisor Prof. Dr. Jörn Block. I am extremely grateful that he gave me the opportunity to write my dissertation. I am also very grateful for his enduring support, helpful advice, quick feedback, and invaluable discussions that improved this dissertation. Second, I would like to thank my second supervisor, Prof. Dr. Jan-Christoph Rülke for agreeing to become my second supervisor and for his support during my time as a doctoral student. Third, I would like to thank my co-authors Prof. Dr. Christian Andres and Prof. Dr. Martin Jacob for contributing to my research project on anti-takeover legislation.

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Table of Abbreviations

AIM	Alternative Investment Market
AR	Abnormal return
BC	Business Combination
CAR	Cumulative abnormal return
CDAX	Composite DAX (German stock index)
CEO	Chief Executive Officer
et al.	Et alii (and others)
e.g.	Exempli gratia (for example)
EBITDA	Earnings before Interest, Taxes, Depreciation and Amortization
FE	Fixed Effects
FTSE	Financial Times Stock Exchange
i.e.	id est (that is)
IPO	Initial Public Offering
IRRC	Investor Responsibility Research Center
ISIN	International Security Identification Number
LSE	London Stock Exchange
M&A	Mergers & Acquisitions
MSCI	Morgan Stanley Capital International
OLS	Ordinary least squares
PPE	Property, plant, and equipment
PUSU	Put up or shut up rule
R&D	Research & Development
ROA	Return on Assets
ROCE	Return on Capital Employed
ROE	Return on Equity
S&P	Standard & Poor’s
SDC	Securities Data Company
SEC	Securities and Exchange Commission
SIC	Standard Industrial Classification
U.K.	United Kingdom
U.S.	United States

Zusammenfassung

Seit jeher ist es für die Forschung und Praxis von großer Bedeutung zu verstehen, wie sich (ökonomische) Ereignisse auf den Unternehmenswert auswirken. Optimalerweise treten diese Ereignisse exogen auf, das heißt plötzlich und unerwartet, so dass eine genaue Messung der Effekte auf den Unternehmenswert erfolgen kann. Neuere Untersuchungen belegen jedoch, dass selbst die Bewertung von exogenen Ereignissen oftmals mit einigen Herausforderungen verbunden ist, die zu unterschiedlichen Interpretationen und insofern mitunter auch zu hitzigen Debatten führen können. In den letzten Jahren werden intensive Debatten insbesondere über die Auswirkungen von Übernahmeschutzmechanismen und von Covid-19 auf den Unternehmenswert geführt. Die Einführung von Schutzmechanismen gegen feindliche Übernahmen und die Ausbreitung von Covid-19 sind exogene Ereignisse, die weltweit auftreten und ökonomisch eine große Wichtigkeit aufweisen, jedoch bisher noch unzureichend untersucht sind. Durch die Beantwortung noch offener Forschungsfragen verfolgt diese Dissertation das Ziel ein größeres Verständnis über die heterogenen Wirkungsweisen von exogenen Ereignissen wie zum Beispiel von Übernahmeschutzmechanismen und Covid-19 auf den Unternehmenswert zu vermitteln. Zusätzlich analysiert diese Dissertation den Einfluss gewisser Unternehmenscharakteristika auf die Effekte dieser beiden exogenen Ereignisse und identifiziert Einflussfaktoren, die widersprüchliche Ergebnisse in der bereits bestehenden Literatur erklären und somit unterschiedliche Sichtweisen wieder in Einklang zu bringen vermögen.

Im Kontext der Einführung von Übernahmeschutzmechanismen weisen kürzlich erzielte Fortschritte in der Forschung darauf hin, dass sich die negative Sichtweise auf Schutzmechanismen gegen feindliche Übernahmen substantiell zum Positiven hin verändert hat. Der Literaturüberblick in Kapitel zwei dieser Dissertation verdeutlicht dieses Ergebnis für die sechs am häufigsten verwendeten Schutzmechanismen gegen feindliche Übernahmen in

den USA und analysiert die Gründe für die nun deutlich positivere Sicht der Forschung auf diese Übernahmeschutzmechanismen. Durch eine steigende Berücksichtigung der Heterogenität von Unternehmen konnten neue Untersuchungen von innovativen und langfristig orientierten Unternehmen zeigen, dass diese von einem erhöhten Schutz vor feindlichen Übernahmen profitieren, da dieser Schutz sowohl ihrer Innovationskraft als auch im Falle eines feindlichen Gebots ihrer Verhandlungsmacht zugutekommt. Des Weiteren führt die Adressierung von statistischen Herausforderungen (Endogenität, Unklarheiten über die Gültigkeit von Schutzmechanismen) zu einer veränderten Erkenntnis über den Wert von Schutzmechanismen insgesamt, denn die Einführung dieser trägt im Durchschnitt zur Steigerung des Unternehmenswerts bei. Diese neuen Erkenntnisse weisen ebenfalls einen hohen Praxisbezug auf und sollten dort Berücksichtigung finden.

Kapitel drei untersucht die Effekte der Ankündigung und Implementierung einer Übernahmeschutzregulierung in Großbritannien. Wie Kapitel drei zeigt, verleiht diese Regulierung Unternehmen in Großbritannien einen größeren Schutz vor feindlichen Übernahmen und wird von den Aktionären im Durchschnitt als positiv bewertet. Querschnittsanalysen offenbaren jedoch verschiedene Gewinner und Verlierer dieser Regulierung. Es liegt insofern ein heterogener Effekt dieser Regulierung auf Unternehmen vor. Zu den Verlierern zählen zukünftige Bieter, also Unternehmen, die anorganisches Wachstum anstreben und die deshalb unter einem erhöhten Übernahmeschutz der potentiellen Zielunternehmen leiden. Ebenso verlieren diejenigen Unternehmen an Marktkapitalisierung, die eine höhere Wahrscheinlichkeit aufweisen, gegen die Interessen ihrer Aktionäre zu verstoßen und durch die Regulierung besser vor Disziplinierungseffekten des M&A Markts geschützt sind. Gewinner dieser Regulierung sind Unternehmen, die in Zukunft potentielle Ziele für Übernahmen darstellen und insofern von mehr Verhandlungsmacht bei Übernahmen profitieren ebenso wie innovative und langfristig orientierte Unternehmen, die ebenfalls von

einer größeren Verhandlungsmacht und Sicherheit langfristiger Innovationen profitieren. Neben den Effekten auf potentielle Bieter und Zielunternehmen untersucht dieses Kapital auch zum ersten Mal den Effekt einer Übernahmeschutzregulierung auf Unternehmen mit unterschiedlicher Produktivität und leistet insofern einen Beitrag zur Forschung. Wie dieses Kapitel zeigt, bilden sich als Folge der Übernahmeschutzregulierung Verzerrungen bei der Kapitalallokation. Diese Verzerrungen haben zur Folge, dass Unternehmen mit niedriger Produktivität von einer größeren Verhandlungsmacht profitieren, die es Unternehmen mit höherer Produktivität erschwert, diese zu erwerben. Der Erhalt von Unternehmen mit niedriger Produktivität durch diese Regulierung erscheint vor dem Hintergrund von volkswirtschaftlichen Wohlfahrtsverlusten fragwürdig. Dieses Kapitel zeigt also, dass eine im Durchschnitt positive Bewertung der Regulierung durch Aktionäre volkswirtschaftlich zumindest zu hinterfragen ist.

Kapitel vier dieser Dissertation untersucht die Auswirkungen der ersten Welle der Covid-19 Pandemie auf die kurzfristigen Aktienrenditen deutscher Unternehmen und unterscheidet dabei zwischen verschiedenen Arten der Mitwirkung von Gründerfamilien. Während der negativen Covid-19 Ereignisse weisen Unternehmen mit Managern aus der Gründerfamilie weniger negative Aktienrenditen auf als Unternehmen mit Eigentümern aus der Gründerfamilie oder Nicht-Familienunternehmen. Gemäß diesen Ergebnissen vermag nur aktives Management durch Familienmitglieder die negativen Auswirkungen der Pandemie abzumildern, da Familienmanager über großes, unternehmensspezifisches Wissen verfügen, entscheidungsschnell handeln, und eine starke Position im Unternehmen innehaben, die ihnen eine aktive Rolle als Krisenmanager ermöglicht. Eigentümer aus der Gründerfamilie hingegen können die negativen Auswirkungen der Pandemie nicht abmildern, da ihre Schwerpunkte mehr im Monitoring des Unternehmens liegen. Diese Schwerpunkte scheinen während einer Krise von geringerer Relevanz als das aktive Krisenmanagement. Eine Unterteilung der

Pandemieereignisse in Ereignisse mit Gesundheits-, Wirtschafts- und Politikbezug liefert ähnliche Ergebnisse, zeigt jedoch die Wichtigkeit von Ereignissen mit Wirtschafts- oder Politikbezug, die mit statistisch und ökonomisch signifikant größeren Aktienrenditen verbunden sind und deshalb in der Literatur Beachtung finden sollten. Die Ergebnisse für Familienmanager stehen in starkem Kontrast zu bereits existierender Literatur, die Familienmanager als inkompetent erachtet, da diese nur aus einem kleinen Familienpool ausgewählt werden und zusätzlich davon ausgeht, dass Familienmanager durch Verfolgung ihrer eigenen, nicht-finanziellen Interessen Minderheitsaktionäre enteignen.

1. Introduction

1.1. Motivation

For decades, academics and practitioners (e.g., investors and regulators) aim to understand whether and how (economic) events affect firm value (Atanasov & Black, 2016). Starting with correlational analyses between news events and firm value for the average firm, the literature has evolved to consider heterogeneous reactions of firms and to establish best practices for causal links as well as for dealing with unobserved heterogeneity, e.g. noise (Atanasov & Black, 2016; Gormley & Matsa, 2014; Karpoff & Wittry, 2018). These academic advances set standards for the analyses of economic, mostly exogenous, events and their effects on firm value.

However, the evaluation of the consequences of economic events on firm value is often prone to a plethora of challenges which can result in different interpretations and thus in heated debates. For example, different employed firm categorizations, different firm performance proxies, and diverse empirical approaches as well as settings lead to mixed and inconclusive empirical evidence (Atanasov & Black, 2016; Karpoff & Wittry, 2018). This ambiguity in empirical results shows that our understanding of the effects of economic events on firm value is still limited. More importantly, evidence on more recent and important economic events is still scarce, since recent literature often revisits events employed by older literature many years ago (Atanasov & Black, 2016; Karpoff & Wittry, 2018). Although recent streams of literature clearly illustrate how academic advances alter the findings of prior literature, these streams of literature only offer limited contributions to the evaluation of recent economic events that are exogenously induced by new types of regulations or crises.

This dissertation aims to contribute to the limited evidence and ambiguous findings of prior literature with regard to the firm value effects of economic events in the context of new announcements of takeover defenses and the propagation of Covid-19. Recently, these streams

of literature have experienced intensive debates about the effects of takeover defenses and Covid-19 on firm value. In this dissertation, I identify influencing factors that may contribute to the mixed and inconclusive results of prior literature. My employed research designs and empirical approaches either mitigate the impact of these influencing factors when it is difficult to control for them or explicitly study these influencing factors for a better understanding of their effects on firm value. In addition, this dissertation examines heterogeneous reactions of firms to economic events that need to be understood to assess the overall impact of regulatory takeover interventions or crisis events, such as Covid-19. Therefore, I provide evidence on characteristics that drive the reactions of firms to these exogenous events. This approach allows me to reconcile opposing findings of prior literature and to provide novel insights on so far unexplored research questions and recent economic events.

I focus on the announcements of takeover defenses and the Covid-19 propagation, since these economic events affect firms all over the world and cause heterogenous stock market reactions among these firms. These heterogeneous reactions of firms can be explained with different costs and benefits across firms, which in the case of takeover defenses have resulted in different views on takeover defenses and hence varying levels of acceptance across jurisdictions (Lel & Miller, 2015; Nenova, 2012). Despite the prevalence of takeover defenses in some jurisdictions, other jurisdictions do not grant any takeover protection to firms at all. For example, in the world's largest economy, the U.S., firms have access to manifold – on average more than nine – takeover defenses (Bebchuk, Cohen, & Ferrell, 2009; Karpoff, Schonlau, & Wehrly, 2021). Despite the availability of takeover defenses in the U.S., shareholder rights activists and corporate governance rating agencies heavily criticize and penalize firms with takeover defenses. For example, the Harvard Law School's Shareholder Rights Project successfully operated a program to abolish one certain type of takeover defense among S&P 500 and Fortune 500 firms (Cremers & Sepe, 2017). Recent literature, however,

suggests that this program destroyed about $149 billion in shareholder wealth (Cremers & Sepe, 2017), which questions the costs of takeover defenses and contributes to the debate about the value of takeover defenses. Another contributing factor to the debate about the value and effectiveness of takeover defenses are the economic and empirical challenges for research, resulting from the large number of available takeover defenses in the U.S. When firms on average have eight other takeover defenses in place, the level of protection against hostile takeovers is as unclear as are the resulting performance consequences. Due to the economic importance of takeover defenses, their widespread and heterogeneous nature, and their heavily debated costs and benefits, I study them in this dissertation.

Another focus of this dissertation is to examine the heterogenous reactions of firms to the propagation of Covid-19. This pandemic is different from prior crises with regard to severity, scope, and cause, because a health emergency has induced worldwide policy responses amounting to a unique global economic crisis. This global economic crisis has overall resulted in decreases in firm value, yet with pronounced differences among firms. To explore these heterogeneous reactions of firms to Covid-19, I study firms with family involvement which are the most prevalent form of company across the world (Faccio & Lang, 2002; Filser, Brem, Gast, Kraus, & Calabrò, 2016). Specifically, I compare the stock market reactions of firms with different forms of family involvement to each other and to non-family firms. The heavy debates on the costs and benefits of family involvement additionally motivate my research. Opponents of family involvement point out that family members are costly, since they expropriate minority shareholders, or are incompetent, because they are chosen from a smaller talent pool (Bertrand & Schoar, 2006; Lemmon & Lins, 2003; Perez-Gonzalez, 2006). However, proponents document that family involvement can be beneficial, since family members have a strong identification with the firm (Sirmon & Hitt, 2003), are highly committed, and well connected to the stakeholders of the firm (Miller & Le Breton-Miller,

2006; Miller, Le Breton-Miller, & Scholnick, 2008). Evidence on how exogenous crisis events affect firm value of firms with different forms of family involvement, however, is still limited, although prior literature detects different behaviors between different forms of family involvement (Block, 2010, 2012). I motivate my research on the propagation of the Covid-19 pandemic and its effects on firms with different forms of family involvement with the limited evidence in prior literature as well as with the economic importance and widespread existence of Covid-19 and family firms. In addition, the intensive debates about the heterogeneous effects of Covid-19 and about the costs and benefits of family involvement motivate my research.

1.2. Research goals

With the chapters of this dissertation I pursue different research goals. The literature review in chapter two has the goal to provide a deeper understanding about the different designs of takeover defenses and their relations to firm value. As a one-size-fits-all view on takeover defenses hampers our understanding of the costs and benefits of takeover defenses (Straska & Waller, 2014), chapter two examines the most important takeover defenses in detail, explores their heterogeneous relations to firm value and thereby shows which specific debates on the costs and benefits of takeover defenses exist. In addition, chapter two pursues the goal to identify influencing factors that foster debates among academics and practitioners. By elaborating on the causes for the disagreement about the costs and benefits of takeover defenses, chapter two aims to provide a better understanding for the opposing results of prior literature. Moreover, it is the goal of chapter two to also reconcile the opposing results of prior literature by elaborating on why over time the views on certain takeover defenses have changed.

One of the research goals of chapter three is to overcome the challenges of prior literature examining takeover defenses in the U.S. As the average U.S. firm is protected by nine

takeover defenses, the interplay between these takeover defenses needs to be understood well to draw correct conclusions. Furthermore, endogeneity issues are common in the U.S. setting. Due to these challenges, chapter three aims to look for changes in the level of takeover protection in the U.K., a jurisdiction that until recently has not granted takeover protection to its firms. Apart from overcoming empirical challenges, chapter three also aims to deepen our understanding on how a market unprotected from hostile takeovers assesses more takeover protection that is introduced through an exogenous shock. In contrast to prior evidence on the U.S. with abundant takeover protection, chapter three pursues the goal to offer novel evidence on the evaluation of takeover protection in a market with no prior protection against hostile bids. In addition, chapter three aims at a better understanding of the beneficiaries and victims of additional takeover protection. The analysis of heterogeneous reactions to more takeover protection provides a better understanding on the effects takeover protection has on firms with different characteristics. Additionally, chapter three has the goal to examine potential distortions on capital allocation, induced by more takeover protection. This analysis examines stock-market reactions for firms with high and low productivity and interprets the potential welfare consequences of more takeover protection.

The research goal of chapter four is to delve into the heterogeneous reactions of firms to exogenous crises events and to provide a better understanding about which firm characteristics increase firm resilience. This chapter specifically aims to examine the costs and benefits of family involvement during crisis situations, i.e. during the Covid-19 pandemic. Crises pose enormous threats to the survival and performance of firms, particularly to the legacy of families whose firms are the most widespread form of company in the world. Chapter four aims to extend our understanding on which form of family involvement matters in a situation of a global crisis and on the resulting firm value consequences. Furthermore, chapter four pursues the goal to understand whether and which forms of family involvement behave as

financial stewards in a crisis. Surprisingly, these two research questions are still largely unexplored. In this chapter I bring both open questions together, to study the stewardship role of family owners and managers in the context of a global crisis. By distinguishing between family ownership and management, chapter four deepens our understanding of which form of family involvement is better able to mitigate the negative firm value consequences induced by a crisis. Another research goal of this chapter is to examine the effects of health-related, economic and political events on the stock-market reactions of firms with family involvement during the first wave of the Covid-19 outbreak. Since evidence on the effects of economic and political events of the Covid pandemic is extremely scarce, this chapter expands our view on the most recent pandemic.

1.3. Structure of the dissertation

The thesis comprises five chapters: an introduction, one literature review, two empirical research papers, and an overall summary and conclusion.

Chapter one provides an introduction to this dissertation. Chapter two reviews the literature on the costs and benefits of takeover defenses. First, this chapter describes the efficacy of the six most prominent takeover defenses in the U.S. Then, this chapter provides theoretical arguments for the beneficial and detrimental effects of takeover defenses in general. Afterwards, chapter two provides empirical evidence on the costs and benefits for each of the examined takeover defenses. For each takeover defense it shows how research has evolved as well as which influencing factors contribute to heavy debates, and identifies potentially fruitful avenues for future research. This chapter is intended to provide the reader with a better understanding of the effects of takeover defenses and of the debates about the costs and benefits of takeover defenses that are empirically studied in the following chapter.

Chapter three examines the announcement and implementation of a takeover regulation that increases the takeover protection in the U.K. After providing information on the institutional background and details on the efficacy of this takeover protection, chapter three provides information on the empirical strategy that exploits exogenous variation in takeover protection and describes sample and data. Then, chapter three depicts the event study results and further explores the costs and benefits of takeover protection for acquirers and targets in cross-sectional regressions. For a better understanding of the wealth transfers between acquirers and targets, this chapter elaborates on the real effects of takeover protection on the functioning of the takeover market. Then, chapter three illustrates the effects of the takeover regulation on long-term oriented firms, firms with different levels of productivity, and firms that suffer from managerial entrenchment. This examination evolves our understanding of the heterogeneous effects of the takeover regulation and for the first time provides insights on the effects takeover protection has on firms with different levels of productivity, for which evidence is extremely scarce. Afterwards, the findings are discussed, additional robustness tests and limitations presented, and then this chapter concludes.

Chapter four deals with the effects of the Covid-19 pandemic on firms with family owners and managers and aims to understand whether family owners and managers behave as stewards in a global crisis. After the development of the hypothesis for family ownership and management, the data and method section provides insights on the context of the study, the sample, the variables and summary statistics, and the empirical strategy that exploits stock-market reactions of sample firms to Covid-related events. In the following results section, event study and cross-sectional regression results present evidence on the reactions of firms with family owners and managers to the Covid-19 pandemic. To also shed light on the stock-market reactions to more detailed events, this chapter then splits the Covid-19 events into categories such as health, economic, and political Covid-related events. Robustness tests and further

analyses confirm the presented results. This chapter concludes with a discussion and conclusion section, and a section on limitations and avenues for future research. Chapter five concludes this dissertation.

2. Does recent evidence alter the views on takeover protection? A survey of the literature

2.1. Introduction

There is abundant literature covering takeover protection, spanning at least five academic disciplines: finance, accounting, economics, law, and management. This amount of research is not surprising, as regulators, academics, and practitioners controversially discuss the benefits and costs of takeover protection. Identifying the aggregate effects of takeover protection on firm value can be challenging due to a pronounced heterogeneity in takeover protection measures across jurisdictions and even across U. S. states. In this survey, I review the empirical research that examines the relation between takeover protection and firm value. To date, evidence on this relation is mixed. My survey overlaps with and draws from an excellent survey about takeover protection in general by Straska and Waller (2014). My emphasis, however, is different.

First, I divide prior studies based on the actual type of takeover defense studied to overcome challenges with regards to a one size fits all approach of prior takeover protection research and reviews. For brevity my survey focuses on six takeover defenses which are among the most important and most prominently studied takeover defenses (Bebchuk et al., 2009; Karpoff et al., 2021). My survey comprises fair price, directors' duties, poison pill, and business combination laws as well as golden parachutes and staggered boards. The coverage of these defenses across U. S. firms is impressive. On average, each U. S. firm has 3.3 of these six takeover defenses implemented. For example, almost nine out of ten firms are protected by business combination laws. My survey does not only comprise the most important anti-takeover laws (e.g., business combination laws), but also comprises the most prominent firm-level takeover defenses such as staggered boards, poison pills, and golden parachutes, which exhibit a steady increase in scope and importance (Karpoff et al., 2021).

Second, for each of the surveyed takeover defenses I divide prior research based on the publication date. I show that the picture emerging from more recent studies is that takeover protection is more often good than bad. This positive interpretation of takeover protection is in stark contrast to the conclusion of Straska and Waller's (2014) survey and to the findings of literature from prior to 2014. Also, this new and much more positive view on takeover protection is surprising in its breadth. This survey cautiously concludes that the relation between takeover defenses and firm value has changed substantially for six of the most important takeover defenses in more recent literature. This conclusion may suggest that large parts of prior literature on takeover protection deserve a re-interpretation. This re-interpretation does not only affect papers studying one of the six takeover defenses surveyed, but also the vast amount of papers relying on proxies for takeover protection, such as the E- and G-index. For example, the views on three of the six takeover defenses contained in the E-index have changed substantially.

Third, my survey provides explanations for why results have changed substantially. Surprisingly, influencing factors for the contrasting results vary with the specific takeover defenses surveyed, suggesting a pronounced heterogeneity in these takeover defenses. In my survey I identify the following influencing factors: More robust research designs, increases in the availability of additional and important controls, more recent sample periods, the consideration of papers providing insignificant results, as well as papers whose results for certain takeover defenses are ignored despite their importance. For example, prior literature refers extensively to the E- and G-index results of the seminal papers by Bebchuk et al. (2009) and Cremers and Ferrell (2014). However, their analyses about single takeover defenses are largely neglected. These analyses are important, as they show that golden parachutes for example are negatively related to firm value. A finding that contrasts with findings of more recent literature. I discuss the influencing factors in more detail in the main part of this survey.

My survey indicates that these influencing factors can reconcile the mixed results documented in the literature.

Fourth, my survey illuminates on which takeover defenses are related to which specific costs and benefits. Interestingly, different defenses – although all of them increase the takeover protection – come with different costs and benefits. While some defenses are insignificantly related to firm value on average because they do not exhibit negative firm value effects for entrenched firms (e.g., fair price laws), other defenses generate positive effects for innovative (e.g., golden parachutes and staggered boards) and stakeholder-oriented firms (e.g., poison pills or business combination laws).

Fifth, in this literature review I identify potentially fruitful avenues for further research. For each of the six takeover defenses I document in which areas evidence is extremely scarce and thus additional research may help to better understand the effects of these heterogenous takeover defenses.

I organize this survey as follows. In section two, I provide an overview of the takeover defenses that I cover in this survey. Section three reviews theoretical arguments that discuss how takeover defenses can decrease or increase firm value. In section four, I review the literature's existing conclusions on the relation between takeover defenses and firm value. In this section, I contrast evidence from prior to 2014 with more recent evidence. Further, I identify potentially fruitful avenues for future research. Section 5 concludes.

2.2. Overview of takeover defenses

2.2.1. Staggered Boards

A staggered board is a board which comprises different classes of directors with overlapping terms. As only parts of a board can be replaced each year, a corporate raider before gaining full control of the target and its board must wait for several years. Control of the targets'

board of directors is compulsory to cancel other takeover defenses, e.g., poison pills. As a staggered board is the prerequisite for other takeover defenses to be effective, it is the most powerful takeover defense (Catan & Kahan, 2016). In fact, this defense is so enduring, difficult, and costly to overcome by acquirers that not a single corporate raider ever successfully acquired a target with a staggered board through a hostile takeover.

2.2.2. Business Combination Laws

Targets protected by business combination laws can delay certain transactions such as asset sales or mergers for between two to five years after the corporate raider has passed a certain threshold (Gompers, Ishii, & Metrick, 2003). Business combination laws can make acquisitions extremely costly for acquirers, since acquirers cannot combine their firms with the targets to benefit from synergies or cost reductions for a certain time. Therefore, business combination laws can increase the costs for a hostile bidder and aggravate acquisitions. According to Bertrand and Mullainathan (2003) business combination laws are more stringent than other only "marginally effective" takeover defenses. Bertrand and Mullainathan's (2003) reasoning has since then served as the basis for increased research activities exploiting the introduction of business combination laws.

2.2.3. Directors' Duties Laws

Directors' duties laws allow corporate directors to not only consider the impact of corporate decisions, e.g., acquisitions, on shareholder value, but to also take into account stakeholder interests (Cremers, Guernsey, & Sepe, 2019). Stakeholder interests comprise among others the interests of customers, suppliers, creditors, or employees. To ensure a successful acquisition of firms incorporated in states with directors' duties laws, acquirers may have to submit an offer that is attractive to both shareholders and stakeholders. The fulfillment

of stakeholder rights can be an extra cost for acquirers, since for example layoffs of targets' employees may be limited after the acquisition. Therefore, the additional consideration of stakeholder interests can exacerbate acquisitions and protect potential targets from hostile takeovers.

2.2.4. Fair Price Laws

Fair price laws guarantee all target shareholders the highest price a bidder has paid to any target shareholder during the acquisition process (Gompers et al., 2003). Fair price laws prevent bidders from exerting pressure on targets' shareholders to sell their shares at too low a price (Gompers et al., 2003). Therefore, fair price laws make acquisitions more expensive for bidders and hamper acquisitions due to increased costs for bidders.

2.2.5. Golden Parachutes

Golden parachutes are agreements between firms and their executives that specify the executives' benefits upon termination of their contracts due to a change in control (Gompers et al., 2003). These agreements comprise severance pay, cash bonuses, stock options, or other benefits. Gompers et al. (2003) view golden parachutes as agreements that decrease shareholder rights due to the additional costs for executives' compensation that are passed on to acquiring firms' shareholders. Since acquirers usually replace target management teams with their own management, they must bear the costs induced by golden parachutes (Lambert & Larcker, 1985). As acquisition costs increase for bidders, golden parachutes have the power to deter takeovers.

2.2.6. Poison Pills

During a hostile takeover approach firms with poison pills can provide their shareholders with the right to purchase additional shares of the firm at a steep discount. Firms use poison pills to deter corporate raiders from bidding for them. As soon as a hostile bidder acquires more than a certain fraction of shares of the target against the will of the target management, the target can trigger a poison pill. The poison pill grants every shareholder except for the hostile bidder the right to buy shares of the target at a steep discount (Catan & Kahan, 2016). Since poison pills dilute the hostile bidder's voting power, they make hostile takeover approaches extremely costly for the bidder (Gompers et al., 2003). Due to the extreme costs for mounting a hostile takeover, poison pills are very effective in deterring hostile acquisitions (Catan & Kahan, 2016).

2.3. Theoretical arguments on firm value effects of takeover defenses

Prior literature puts forward three theoretical arguments how takeover defenses can affect firm value.

2.3.1. Managerial Entrenchment Hypothesis

Takeover defenses are important to shareholders as these defenses limit the shareholders' rights and can affect shareholder wealth. The critics of takeover defenses argue that they provide managers with opportunities to block takeovers, which hinders the disciplining force of the market for corporate control (DeAngelo & Rice, 1983). Once protected against takeovers, managers might become more entrenched and agency costs might be exacerbated. These costs of managerial entrenchment are ultimately borne by shareholders. Decreases in firm value after the adoption of takeover defenses are therefore consistent with the managerial entrenchment hypothesis. The literature observes different reactions of managers to increased protection against hostile takeovers.

According to Jensen (1986), managers after being insulated from the market for corporate control may have incentives for excessive spending. Literature refers to this act of increasing the size and scope of managers' power and influence as empire building (Jensen, 1986). Besides, entrenched managers may enjoy the quiet life (Bertrand & Mullainathan, 2003). According to the quiet life hypothesis, managers avoid hard decisions and difficult tasks. Additionally, takeover defenses may provide managers with the incentive to "play if safe" (Gormley & Matsa, 2016). The playing it safe hypothesis describes value-destroying activities of managers to reduce their firms' risks below optimal levels.

2.3.2. Long-term Benefit Hypothesis

Proponents of takeover defenses argue that a reduction in the takeover threat due to higher protection against takeovers can help managers to avoid myopic decisions. According to the long-term benefit hypothesis, takeover defenses incentivize managers to conduct more long-term investments and mitigate the risk that managers sacrifice investments with long-term payoffs for short-term investments (Stein, 1988). According to Stein (1988) the value of long-term payoffs cannot credibly be communicated to market participants due to asymmetries of information. This can lead to an undervaluation of firms with long-term investment horizons, making them attractive takeover targets. Takeover defenses that aggravate bidders to benefit from this undervaluation, therefore, support longer-term investments. Increases in firm value for innovative firms after the adoption of takeover defenses are therefore consistent with the long-term benefit hypothesis.

2.3.3. Bonding Hypothesis

The bonding hypothesis argues that takeover defenses support firms' commitments to their counterparties and decreases incentives to act opportunistically (Johnson, Karpoff, & Yi,

2015). According to Johnson et al. (2015), takeover defenses mitigate the risk for hostile takeovers and therefore also decrease the likelihood for changes in firms' operating strategy that are usually triggered by takeovers. Since changes in the operating strategy after a takeover may decrease the value of relationship-specific investments for counterparties and firms themselves, takeover defenses ensure the value of these investments. Therefore, takeover defenses bond the firms' guarantees given to their counterparties and protect relationship investments that are beneficial to both, firms and counterparties. These positive effects of higher protection against takeovers can increase firms' operating performance and create positive spillovers for firms and their business partners. Increases in firm value for stakeholder-oriented firms after the adoption of takeover defenses are therefore consistent with the bonding hypothesis.

2.4. Empirical evidence

2.4.1. Staggered Boards

2.4.1.1. Earlier correlational evidence

Prior literature provides mostly correlational evidence that staggered boards are detrimental for firm value (see Table 2-1 at the end of this subchapter). A seminal paper by Bebchuk and Cohen (2005) reveals that staggered boards are associated with a 16% lower industry-adjusted Tobin's Q during the sample period 1995 to 2002. This relation is statistically and economically significant and consistent with the entrenchment hypothesis. Charter-based staggered boards are the driver for this negative association, since shareholders cannot amend the corporate charter. Bylaw-based staggered boards, however, are not associated with decreases in firm value. In a follow-up paper Bebchuk et al. (2009) by more specifically controlling for other takeover defenses confirm the negative association between staggered boards and industry-adjusted Tobin's Q for a sample ranging from 1990 to 2002. However,

they document a lower economic magnitude across specifications (-2.6% to -5.1%), which is still statistically significant. Similarly, other correlational studies also corroborate the negative association between staggered boards and firm value (Cremers & Ferrell, 2014; Faleye, 2007; Jiraporn, Chintrakarn, & Kim, 2012). Cremers and Ferrell's (2014) study, for example, comprises a longer time horizon ranging from 1978 to 2006 and, thereby, incorporates the rise of the staggered board takeover defense in the 1980s. Faleye (2007) finds that staggered boards are associated with a statistically significant reduction in firm value and further explores which firms suffer most. The reduction in firm value is more pronounced for R&D intensive firm, for firms with high intangible assets, high sales growth and large firms, a result that is at odds with the long-term benefit hypothesis (e.g., Stein, 1988). This hypothesis argues that innovative firms benefit from more takeover protection, as they can shift their investment horizon to more beneficial long-term investments.

Faleye (2007) also conducts an event study to analyze the change in firm value after adoptions and eliminations of staggered boards during the 1995 to 2002 period in which staggered boards were already firmly in place (Bebchuk & Cohen, 2005). Faleye (2007) shows that adoptions of staggered boards are harmful to shareholder wealth, while eliminations of staggered boards are either not priced or positive for firm value. This event study evidence contrasts with prior event study evidence documenting insignificant results for adoptions of staggered boards (Agrawal & Mandelker, 1990; Bojanic & Officer, 1994; Mahoney, Sundaramurthy, & Mahoney, 1996). These three studies, however, comprise sample periods before staggered boards became popular and only partly cover periods after the Moran decision of 1985 that increased the validity of takeover defenses, at least in Delaware (Cremers & Ferrell, 2014). The lack of clarity about the validity of takeover defenses can explain the insignificant results. Moreover, these three event studies rely on announcement dates that are endogenously set by management (Cohen & Wang, 2013). Above that, the announcement dates

of staggered board adoptions may not represent clean events, since they coincide with other information from proxy filings or annual meeting dates that are incorporated into firms' stock prices at the same time (Faleye, 2007).

2.4.1.2. Earlier experimental evidence

The challenges of prior literature examining the relation between staggered boards and firm value are that, on the one hand, staggered boards may lead to lower value or, on the other hand, low value firms might have a higher propensity to adopt a staggered board. Cohen and Wang (2013) overcome these endogeneity challenges with their experiment that relies on two Delaware court rulings influencing the power of staggered boards. Both Delaware court rulings in 2010 are only separated by weeks and go in opposite directions. Cohen and Wang (2013) reveal that a weakening of staggered boards increases firm value significantly, while the subsequent reversal of the court ruling – strengthening staggered boards – destroyed firm value significantly. This effect is also economically meaningful. Cohen and Wang's (2013) results suggest that staggered boards reduce firm value by 0.76% to 0.96%.

2.4.1.3. Replications of earlier results

Amihud and Stoyanov (2017) examine the conclusion of Cohen and Wang (2013) and find insignificant results when they remove one stock's delisting return from Cohen and Wang's (2013) sample. Also, an exclusion of only few penny stocks, low market capitalization stocks, or over-the-counter stocks renders Cohen and Wang's (2013) results insignificant. The results are also insignificant when Amihud and Stoyanov (2017) use less granular industry fixed effects (a four digit instead of a six digit industry classification).

Furthermore, more recent literature starts to replicate prior studies reporting negative associations between staggered boards and Tobin's Q (e.g, Bebchuk & Cohen, 2005) and

concludes that the significantly negative correlations between staggered boards and Tobin's Q are sensitive to the inclusion of important control variables. Cremers and Sepe (2016), Cremers, Masconale, and Sepe (2016) as well as Cremers, Litov, and Sepe (2017) illustrate that the relation between staggered boards and Tobin's Q is significantly negative for industry and year fixed effects, but significantly positive for firm and year fixed effects. Cremers et al. (2017) by employing industry fixed effects also show that adoptions of staggered boards are significantly positively related to Tobin's Q, while removals are significantly negatively related to Tobin's Q. These associations are driven by the 1996 to 2015 subsample period and insignificant for the 1978 to 1995 subsample. Cremers and Sepe (2016) confirm this result by using firm fixed effects. Furthermore, Cremers and Sepe (2016) as well as Cremers et al. (2017) document that the significantly positive associations between staggered boards and firm value are driven by innovative firms, firms with high operational complexity, and stakeholder commitment consistent with the long-term benefit and bonding hypotheses. Importantly, non-innovative firms with a staggered board are also associated with significant increases in Tobin's Q, while staggered boards in firms without stakeholder commitment or operational complexity are not significantly related to firm value (Cremers et al., 2017; Cremers & Sepe, 2016).

In three papers Amihud, Schmid, and Solomon (2018a, 2018b, 2019) challenge the robustness of Bebchuk and Cohen's (2005) as well as Cremers et al.'s (2017) findings. The authors report insignificant results when including more firm-level control variables or other takeover defenses to Bebchuk and Cohen's (2005) analyses. When adding firm and year fixed effects to the subsample analysis of Cremers et al. (2017) their results also become insignificant. However, Amihud et al. (2018a, 2018b, 2019) use different subsample periods than Cremers et al. (2017) and, overall, confirm the significantly positive relation between staggered boards and Tobin's Q for the entire sample period. Interestingly, the interpretation of Amihud et al. (2018a, 2018b, 2019) results suggest heterogenous relations between

staggered boards and firm value. On average, staggered boards are associated with significant increases in Tobin's Q, while other takeover defenses (the modified E-index) are significantly negatively related to Tobin's Q, indicating differences between takeover defenses' relation to firm value.

2.4.1.4. New identification strategies

With the evolvement of the literature, researchers employ quasi-natural experiments studying the effects of staggered boards on Tobin's Q and Return on Assets more often. Overwhelmingly, they use three quasi-natural experiments: First, Massachusetts in 1990 required incorporated firms to adopt a staggered board and made an opt out of this requirement difficult (Cremers et al., 2017). Second, the Delaware takeover regime in 1995 made it easier for target firms to defend themselves in a hostile takeover. Targets were no longer required to demonstrate that the takeover approach presented a threat and that the defense used by the target was proportional to that threat (Bhojraj, Sengupta, & Zhang, 2017). This change, however, affected both staggered boards and poison pills at the same time, thus exacerbating clear interpretations. Further, Heron and Lie (2015) conclude that the change in the Delaware takeover regime does not affect the use and effectiveness of these takeover defenses, mitigating the strength of this shock. Third, the Harvard Law School's Shareholder Rights Project between 2011 and 2014 led a substantial majority of the S&P 500 and Fortune 500 firms to de-stagger their corporate boards (Cremers & Sepe, 2017). This exogenous pressure on firms is used by

Cremers and Sepe (2017) to evaluate firm value consequences of de-staggering corporate boards.

2.4.1.5. Recent experimental evidence

Using the Massachusetts requirement to adopt a staggered board, Cremers et al. (2017) show that these mandatory adoptions of staggered boards increase the affected firms Tobin's Q positively and significantly. Daines, Li, and Wang (2021) confirm these results and illustrate the robustness of this effect to different subsample periods, firm fixed effects, and industry-year fixed effects. Innovative firms, e.g., young firms or R&D intensive firms, drive this positive firm value effect of staggered boards. Consistent with the findings of Cremers and Sepe (2016) and Cremers et al. (2017) the mandatory adoption of staggered boards in Massachusetts does not negatively affect non-innovative firms.

Bhojraj et al. (2017) report similar but weaker results for the change of the Delaware takeover regime facilitating the use of takeover defenses. Due to this change, the Tobin's Q of innovative firms with a staggered board increases significantly more than that of non-innovative firms with a staggered board. This result is in line with the long-term benefit hypothesis. However, there is no significant increase for innovative and protected firms compared to innovative and unprotected firms. This result casts doubt on the strength of this shock and can be consistent with Heron and Lie's (2015) finding that the effectiveness of takeover defenses does not change for Delaware in 1995.

In another experiment, Cremers and Sepe's (2017) findings corroborate the positive effects of a staggered board. After firms de-stagger their corporate boards due to pressure of the Harvard Law School's Shareholder Rights Project their firm value decreases significantly. Especially innovative firms suffer from de-staggering their boards, consistent with the long-term benefit hypothesis. Daines et al. (2021) confirm the above findings for Tobin's Q also for

return on assets. After the legislator strengthens the effectiveness of staggered boards ROA for affected firms increases significantly. Again, this effect is significantly more pronounced for innovative firms.

Event study evidence employing the three quasi-natural experiments draws similar conclusions. Cremers et al. (2017) conduct a long-term stock return event study by buying stocks of firms before they stagger up their boards and short-selling stocks of firms before they de-stagger their boards. The long-short portfolio generates a monthly abnormal return of about 1%. This abnormal return is statistically and economically significant. The authors interpret this result as evidence for the long-term value of staggered boards. Short-term event studies corroborate this conclusion. Cremers and Sepe (2017) show that firms that de-stagger their board after being targeted by the Shareholder Rights Program have a significantly lower stock market performance than firms that de-stagger without being targeted by that program, amounting to a five to seven percent lower abnormal return per year. The magnitude of this abnormal return is also economically significant. Bhojraj et al. (2017) show positive and significant results for firms protected by staggered boards in Monte Carlo simulations (bootstrapped non-event dates) after an increase in the effectiveness of takeover defenses. However, the authors document insignificant results for firms protected by staggered boards for a non-bootstrapped control group based on the Market Model or a four-factor model.

2.4.1.6. Summary and avenues for future research

All in all, prior literature from before 2014 overwhelmingly suggests that staggered boards are costly for firms. This finding is consistent with the entrenchment hypothesis. More recent evidence, in contrast, documents the positive value effects of staggered boards, in aggregate and especially for innovative firms. These findings are consistent with the long-term benefit and bonding hypothesis. As this survey describes, the use of more granular fixed effects

and quasi-natural experiments changed the findings of the staggered board literature substantially. Despite these recent advances in the literature, there are still potentially fruitful avenues for future research. First, there is surprisingly little evidence on the different types of staggered boards. Although Bebchuk and Cohen (2005) demonstrate different results for charter- vs. bylaw-based staggered boards, follow-up literature does not examine this finding in more detail, e.g., in natural experiments. By separating both types of staggered boards in a more detailed analysis clearer and potentially stronger effects might occur that may even change some of the interpretations of more recent literature with regards to staggered boards. Second, prior literature strongly focuses on Tobin's Q and cumulative abnormal returns as dependent variables, but largely ignores other performance indicators, such as the value of cash or sales growth. An analysis of the latter performance indicators may provide a more thorough understanding of the effects of staggered boards. Third, it is unclear if the presented evidence for staggered boards is robust to a more detailed set of controls for takeover protection. Some studies employ aggregate measures for shareholder rights such as the E-index or G-index as additional controls. However, evidence controlling for the single provisions of the latter indices supplemented by other controls for court decisions and state anti-takeover laws (e.g., Cain, McKeon, & Solomon, 2017) is scarce. It may be worthwhile to explore the effects of staggered boards in the context of other takeover defenses more deeply.

Table 2-1: Staggered Boards

This table shows studies on staggered boards and their relation to firm value.

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel A: Event study results					
Agrawal and Mandelker (1990)	18 firms	1979-1985	CAR(-40,1) CAR(-20,1) CAR(-1,0)	Insignificant Insignificant Insignificant	Staggered boards are not significantly related to firm value.
Bojanic and Officer (1994)	50 firms	1967-1986	CAR(-1,0)	Insignificant	Staggered boards are not significantly related to firm value.
Mahoney, Sundaramurthy, and Mahoney (1996)	106 firms	1985-1988	CAR(-50,5)	Insignificant	Staggered boards are not significantly related to firm value.
Faleye (2007)	159 firms	1995-2002	Adoptions: CAR(-1,1) CAR(-5,1) CAR(-5,5) Eliminations: CAR(-1,1) CAR(-5,1) CAR(-5,5)	 Statistically negative Insignificant Statistically positive Insignificant	 -0.34%* -0.70%* -1.78%** 0.78% 1.28%* 0.85% Adoptions of staggered boards are harmful, while eliminations are not priced or are positive for firm value.
Cohen and Wang (2013)	278 firms	2010	CAR(0,1)	Statistically negative	Staggered boards decrease firm value by between 0.76% to 0.96%. Both effects are statistically significant at the 10% level.
Amihud and Stoyanov (2017)	276 firms	2010	CAR(0,1)	Insignificant	Staggered boards are neither beneficial nor detrimental to firm value.

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Table 2-1 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Bhojraj, Sengupta, and Zhang (2017)	304 firms	1995	CAR(0,1)	Statistically positive	0.026%* (January event) 0.127%*** (December event)
			CAR(0,3)	Statistically positive	0.214%*** (January event) 0.040%** (December event)
					Staggered boards increase firm value. Firms with staggered boards exhibit positive and statistically significant cumulative abnormal returns for a 4-factor model with simulated nonevent period.
			CAR(0,1) CAR(0,3)	Insignificant Insignificant	0.15% 0.25%
					Staggered boards do not significantly affect firm value in a 4-factor model.
Cremers, Litov, and Sepe (2017)	34,476 firm-year observations	1978-2015	Stock returns	Statistically positive	The strategy of buying firms that staggered up and short-selling firms that de-staggered earns 1.09% monthly alpha (from 4-factor model). The strategy of buying firms that staggered up earns 0.58% monthly alpha (from 4-factor model). These monthly alphas are statistically significant at the 5% and 10% level, respectively.
Cremers and Sepe (2017)	14,106 firm-year observations	2011-2014	Stock returns	Statistically negative	The strategy of buying firms that de-stagger after being targeted by the Shareholder Rights Program and short-selling firms that de-stagger without being targeted earns a yearly alpha of -5.53% to -6.74% (from 4-factor model). These monthly alphas are statistically significant at the 5% and 10% level, respectively.
Panel B: Tobin's Q					
Bebchuk and Cohen (2005)	IRRC sample	1995-2002	Industry-adjusted Tobin's Q	Statistically negative	Staggered Boards are negatively related to Tobin's Q. The decrease in Tobin's Q is 16.6% to 17.4% and significant at the 1% level. Driver for this decrease are charter-based staggered boards.

(continued on next page)

Table 2-1 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Faleye (2007)	2,021 firms that filed proxy statements with the US SEC.	1995-2002	Tobin's Q	Statistically negative	Staggered Boards are negatively associated with Tobin's Q. The decrease in Tobin's Q is 16.2% to 19.0%. This decrease is significant at the 1% level. R&D intensive firms, firms with high intangible assets, high sales growth, and large firms suffer most.
Bebchuk, Cohen, and Ferrell (2009)	IRRC sample	1990-2002	Log(industry-adjusted Tobin's Q)	Statistically negative	Negative association between staggered boards and Log(industry-adjusted Tobin's Q). Tobin's Q decreases by 2.6% to 5.1% for firms with a staggered board. This association is statistically significant at the 5% and 1% level, respectively.
Jiraporn, Chintrakarn, and Kim (2012)	12,525 firm-year observations	1990-2006	Tobin's Q	Statistically negative	The relation between staggered boards and Tobin's Q is negative. Tobin's Q for firms with staggered boards decreases by 44.3% to 59.2%.
			Industry-adjusted Tobin's Q	Statistically negative	The relation between staggered boards and industry-adjusted Tobin's Q is negative. Tobin's Q for firms with staggered boards decreases by 55.0%. These associations are statistically significant at the 1% level.
Cremers and Ferrell (2014)	24,358 firm-year observations	1978-2006	Industry-adjusted Tobin's Q	Statistically negative	Staggered boards are negatively related to industry-adjusted Tobin's Q. Tobin's Q decreases by 8.2% for firms with staggered boards. This association is statistically significant at the 5% level.
Cremers, Masconale, and Sepe (2016)	28,281 firm-year observations	1978-2008	Tobin's Q	Statistically positive	The relation between staggered boards and Tobin's Q is positive when using firm and year fixed effects. Firms with staggered boards exhibit 7.1% to 12.0% higher Tobin's Q. These associations are statistically significant at the 1% level.

(continued on next page)

Table 2-1 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Cremers and Sepe (2016)	30,797 firm-year observations	1978-2011	Tobin's Q	Statistically positive	The relation between staggered boards and Tobin's Q is positive when using firm and year fixed effects. Tobin's Q increases by 5.9% for firms with staggered boards. This association is statistically significant at the 5% level.
				Statistically positive	Adoptions of staggered boards are positively related to Tobin's Q, while removals are negatively related to Tobin's Q. For the 1995-2011 period, staggering up is associated with increases in Tobin's Q (13%), while staggering down is associated with decreases in Tobin's Q (-13.8%). These associations are statistically significant at the 5% and 1% level, respectively. Drivers for the positive effects of staggered boards are innovative firms, firms with high operational complexity, and stakeholder-oriented firms.
Bhojraj, Sengupta, and Zhang (2017)	4,028 firm-year observations	1990-2000	Tobin's Q	Statistically positive	Strengthening staggered boards increases Tobin's Q by 38.6% for innovative firms and by 24.8% for innovative firms vis-à-vis other unaffected innovative firms. These effects are statistically significant at the 1% level.
Cremers, Litov, and Sepe (2017)	34,476 firm-year observations	1978-2015	Tobin's Q	Statistically positive	The relation between staggered boards and Tobin's Q is positive when using firm and year fixed effects. Staggered boards are associated with a 5.1% to 9.8% higher Tobin's Q. These associations are statistically significant at the 5% and 1% level, respectively.
				Statistically positive	Adoptions of staggered boards positively related to Tobin's Q, while removals are negatively related to Q. Drivers for positive effects: innovative firms, firms with high operational complexity, and stakeholder-oriented firms.

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Table 2-1 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Cremers and Sepe (2017)	14,106 firm-year observations	2011-2015	Tobin's Q	Statistically positive	De-staggering decreases Tobin's Q by between 17.6% to 27.8%. These effects are statistically significant at the 5% and 10% level, respectively. Drivers for these effects are innovative firms.
Amihud, Schmid, and Solomon (2018a)	23,962 firm-year observations	1990-2013	Log(Tobin's Q)	Insignificant	By including additional firm-level controls and controls for takeover defenses results of prior literature are insignificant.
				Statistically positive	The relation between staggered boards and Tobin's Q is positive when using firm fixed effects. Staggered boards are associated with a 3.1% to 4.0% higher Tobin's Q. These associations are statistically significant at the 5% level.
Amihud, Schmid, and Solomon (2018b)	23,962 firm-year observations	1990-2013	Log(Tobin's Q)	Insignificant	By including additional firm-level controls and controls for takeover defenses results of prior literature are insignificant
				Statistically positive	The relation between staggered boards and Tobin's Q is positive when using firm fixed effects. Staggered boards are associated with a 3.1% to 4.0% higher Tobin's Q. These associations are statistically significant at the 5% level.
Daines, Li, and Wang (2021)	1,862 firm-year observations	1984-1997	Tobin's Q	Statistically positive	Staggered boards increase Tobin's Q by 14.1% to 14.7%. These effects are statistically significant at the 1% level. Drivers for this effect are innovative firms, whose Tobin's Q increases by 17.7% to 25.3%. These effects are significant at the 5% and 1% level, respectively.

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Table 2-1 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Amihud, Schmid, and Solomon (2019)	23,962 firm-year observations	1990-2013	Log(Tobin's Q)	Insignificant	By including additional firm-level controls and controls for takeover defenses results of prior literature are insignificant.
				Statistically positive	The relation between staggered boards and Tobin's Q is positive when using an instrumental variables approach. Staggered boards are associated with a 4.2% to 6.5% higher Tobin's Q. These associations are statistically significant at the 5% level.
Panel C: Other performance measures					
Daines, Li, and Wang (2021)	1,836 firm-year observations	1984-1997	Return on assets	Statistically positive	Staggered boards increase return on assets by 1.6%. Drivers for this effect are innovative firms, whose return on assets increases by 2.1% to 3.4%. These effects are significant at the 10% and 1% level, respectively.

2.4.2. Business Combination Laws

2.4.2.1. Earlier evidence

Prior literature provides mostly exogenous evidence that business combination laws are detrimental for firm value (see Table 2-2 at the end of this subchapter). A seminal paper by Bertrand and Mullainathan (2003) exploits the implementation of state anti-takeover laws as a quasi-natural experiment. For the sample period 1976-1995 Bertrand and Mullainathan (2003) reveal that business combination laws statistically significantly decrease total factor productivity and return on capital (total value of shipments net of labor and material costs divided by capital stock). After the implementation of business combination laws the relative productivity of firms drops by more than one percentile. Further, business combination laws lead to a drop in the return on capital by roughly one percent. Both effects are also economically significant. Follow-up literature exploiting the same research design and using the same sample period confirms these negative effects of business combination laws. Giroud and Mueller (2010) show that the announcements of business combination laws decrease affected firms cumulative abnormal returns by 0.32% over a two-day event window. This effect is statistically and economically significant. This decrease in firm value is driven by firms in non-competitive industries, whose cumulative abnormal returns decrease by 0.54% to 0.67% contingent on the level of industry competitiveness. Hedge portfolio partitions based on industry competitiveness illustrate that non-competitive firms affected by the announcements of business combination laws perform by between 0.75% to 0.97% worse than competitive firms affected by the same announcements. These magnitudes are statistically and economically significant. Also, Giroud and Mueller (2010) report decreases in return on assets (ROA) after the implementation of business combination laws. On average, ROA of affected firms drops by statistically and economically significant 0.6%. Again, this drop is driven by firms in non-competitive industries, whose ROA drops by statistically and economically significant 0.5% to 3.3%.

Giroud and Mueller's (2010) findings are consistent with the entrenchment hypothesis and, more specifically, with the quiet life hypothesis. The quiet life hypothesis proposes that managers after being insulated from threats of hostile takeovers try to avoid cognitively difficult tasks so that costs increase. After the adoption of business combination laws Giroud and Mueller (2010) demonstrate an increase in costs, e.g., in selling, general, and administrative expenses, costs of goods sold, and wages. Atanassov (2013) confirms the negative effects of business combination laws. In his analysis covering a slightly larger sample period (1976-2000) he finds that business combination laws decrease Tobin's Q by about 7%. This effect is statistically and economically significant.

2.4.2.2. More recent evidence

More recent literature, however, documents largely insignificant or even positive effects for business combination laws, attributing the findings of prior literature to omitted variables concerns. In a seminal paper Karpoff and Wittry (2018) replicate the findings of Giroud and Mueller (2010) and Atanassov (2013). Karpoff and Wittry (2018) show that, among others, Giroud and Mueller's (2010) as well as Atanassov's (2013) findings change substantially when controlling for other state anti-takeover laws, for preexisting takeover defenses of firms, and for court decisions affecting the takeover protection of firms. According to Karpoff and Wittry (2018) the negative effects of business combination laws vanish and turn out to be insignificant. Follow-up literature incorporates the proposed controls by Karpoff and Wittry (2018) and provides novel insights on the firm value effects of business combination laws. For example, Cen, Dasgupta, and Sen (2015) suggest a beneficial effect of business combination laws when stakeholder relationships are important for firms. Firms with key customers benefit from the announcements of business combination laws and experience a statistically and economically significant increase in firm value by between 2.1% to 3.9%. Further, ROA

increases statistically and economically significantly after the implementation of business combination laws for firms with key customers, firms in durable goods industries, and for high R&D firms. Moreover, business combination laws increase affected firms' sales to principal customers. This effect, again, is statistically and economically significant. Cen et al.'s (2015) findings are consistent with the long-term benefit and bonding hypotheses which suggest that a reduction in the threat of a hostile takeover increases firms' ability to conduct long-term investments and strengthens the relationships with existing customers. Fich, Harford, and Yore (2021) outline the positive effects of business combination laws on the value of cash. They document that on average the value of cash increases statistically and economically significantly in firms with business combination laws. This increase is more pronounced in firms in which takeover protection helps to bond important commitments, e.g., with major customers. This finding, again, is consistent with the bonding hypothesis. According to Fich et al. (2021) the value of cash in firms susceptible to managerial entrenchment problems does not increase. This finding suggests that the beneficial and detrimental effects of more takeover protection outweigh each other in firms with a higher propensity for entrenchment. Moreover, this finding is at odds with the results of prior literature documenting decreases in firm value after the implementation of business combination laws.

2.4.2.3. Summary and avenues for future research

All in all, prior literature from before 2014 overwhelmingly suggests that business combination laws are costly for firms. This finding is consistent with the entrenchment hypothesis. More recent evidence, in contrast, documents the positive value effects of business combination laws, in aggregate and especially for long-term oriented firms and for firms with major customers. These findings are consistent with the long-term benefit and bonding hypotheses. Additional controls for the institutional background of business combination laws

and a more detailed exploration of the cross-section of firms (e.g., long-term orientation and bonding) have substantially changed the findings of the literature on business combination laws. Despite these recent advances in the literature, there are still potentially fruitful avenues for future research. First, there is surprisingly little evidence on the robustness of the findings by Bertrand and Mullainathan (2003) among others. To the best of my knowledge, it is unclear if their findings with regards to total factor productivity and return on capital are robust to a set of additional controls. These additional controls can comprise proxies for whether firms are subject to managerial entrenchment problems, to long-term orientation, to important customer and business relationships, to certain court decisions affecting the validity of business combination laws, or to prior firm-level takeover defenses. These prior defenses may determine the direction of the effects to the staggered adoption of business combination laws across states. Further, Catan and Kahan (2016) hint at pronounced categorization errors of Bertrand and Mullainathan (2003). These categorization errors may influence the results of Bertrand and Mullainathan (2003). One potential obstacle to a replication of Bertrand and Mullainathan's (2003) study, however, is their use of plant-level data, which are not widely available. Second, literature so far largely ignores the identification of the drivers behind the value effect of business combination laws. Instead literature attributes on average positive value effects of business combination laws to the long-term benefit or bonding hypotheses, while negative value effects are in line with the entrenchment hypothesis[1]. Specific tests of different drivers against one another are still extremely scarce. These tests can be fruitful to help explore the cross-section of firms further and to identify firms for which business combination laws are beneficial or detrimental. Third, since the first tests of the bonding hypothesis by Johnson et al. (2015), literature testing the bonding hypothesis has evolved, largely neglecting the long-

[1] An exception is Giroud and Mueller (2010), whose findings have been re-interpreted by Karpoff and Wittry (2018).

term benefit hypothesis. The motivation for this sudden shift to the bonding hypothesis is unclear, especially since both hypotheses exhibit a certain intersection. Surprisingly, evidence on the size of the overlap between both hypotheses is still extremely scarce. Also, no paper to the best of my knowledge tests both hypotheses against each other to elaborate on which hypothesis can better explain the effects of business combination laws.

Table 2-2: Business Combination Laws

This table shows studies on business combination laws and their relation to firm value.

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel A: Event study results					
Giroud and Mueller (2010)	77,460 firm-year observations	1976-1995	CAR(-1,0)	Statistically negative	The announcements of business combination laws decrease the cumulative abnormal returns of affected firms by 0.32%. This effect is statistically significant at the 1% level. Firms in non-competitive industries drive this effect. They perform by 0.75% to 0.97% worse than competitive firms.
Cen, Dasgupta, and Sen (2015)	62,041 firm-year observations	1979-1995	CAR(announced, adoption of BC law)	Statistically positive	3.9%*
			CAR(announced, 3 months)	Statistically positive	2.1%***
			CAR(announced, 6 months)	Statistically positive	2.4%** Announcements of business combination laws increase firm value of stakeholder-oriented firms.
Panel B: Tobin's Q					
Atanassov (2013)	47,410 firm-year observations	1976-2000	Log(Tobin's Q)	Statistically negative	Business combination laws decrease firm value by 6.3% to 7.4%. These effects are statistically significant at the 1% level.
Panel C: Other performance measures					
Bertrand and Mullainathan (2003)	224,188 plant-year observations	1976-1995	Total factor productivity percentile	Statistically negative	Business combination laws decrease total factor productivity by 1.1 to 1.3 percentiles.
			Return on capital	Statistically negative	Business combination laws decrease return on capital by 0.7 to 0.8 percent. These effects are statistically significant at the 1% and 5% level, respectively. After being protected against takeovers, managers enjoy the quiet life.

(continued on next page)

Table 2-2 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Giroud and Mueller (2010)	77,460 firm-year observations	1976-1995	Return on assets	Statistically negative	Business combination laws decrease return on assets by 0.6%. This effect is statistically significant at the 5% level. Driver for this effect are firms in non-competitive industries, whose return on assets drops by 0.5% to 3.3%.
Cen, Dasgupta, and Sen (2015)	96,064 firm-year observations	1979-1995	Return on assets	Statistically positive	Business combination laws increase return on assets of stakeholder-oriented and innovative firms by 1.1% to 1.2%. These effects are statistically significant at the 5% level.
			Sales Growth	Statistically positive	Business combination laws increase sales growth of affected suppliers by 25% vis-à-vis control firms. This effect is statistically significant at the 1% level.
Fich, Harford, and Yore (2021)	89,958 firm-year observations	1972-2010	Excess stock return	Statistically positive	The value of cash increases by 20.8% to 34.9% for firms affected by the implementation of business combination laws. These effects are statistically significant at the 1% level. Driver for this effect are stakeholder-oriented firms.
				Insignificant	There is no increase in the value of cash for firms subject to entrenchment.
Karpoff and Wittry (2018)	86,920 firm-year observations	1976-1995	Return on assets	Insignificant	Business combination laws do not significantly affect return on assets.

2.4.3. Directors' Duties Laws

2.4.3.1. Earlier evidence

Prior literature devotes surprisingly little attention to directors' duties laws with few exceptions (see Table 2-3 at the end of this subchapter). One such exception is the study by Alexander, Spivey, and Marr (1997) studying three states that pass directors' duties laws in isolation in 1984 and 1989. The authors show that the announcements and adoptions of directors' duties laws reduce affected firms' cumulative abnormal returns by a statistically and economically significant 0.3% to 0.4%. This effect is driven by poorly managed firms (as proxied for by a low market-to-book ratio) and by firms with no other takeover defenses. In line with this finding Gompers et al. (2003) categorize directors' duties laws as statutes that weaken shareholder rights. Two follow-up correlational studies which among others control for directors' duties laws document an insignificant association between directors' duties laws and Tobin's Q (Bebchuk et al., 2009; Faleye, 2007).

2.4.3.2. More recent evidence

Recently, a study by Cremers et al. (2019) investigates the value effect of directors' duties laws in a large sample from 1983 to 2015 covering all states that have passed these laws. Further, this study controls for the state-level institutional, political, and economic context that may explain states' propensity to pass directors' duties laws. Cremers et al. (2019) document a statistically significant and positive effect of the passage of directors' duties laws on Tobin's Q. This positive effect – a 3.8% increase in Tobin's Q – is also economically meaningful. This positive effect is more pronounced for complex firms (measured by firm size, sales, and employees), for innovative firms (measured by patents and R&D expenses), as well as for stakeholder-oriented firms (measured by large customers, supplier dependency, labor intensity, and strategic alliances). Moreover, Cremers et al.'s (2019) analysis reveals statistically

significant and positive effects of directors' duties laws on operational efficiency proxies, e.g., return on assets (ROA), return on equity (ROE) and return on capital employed (ROCE). These positive effects are also economically significant. For example, the implementation of directors' duties laws increases ROA by 7% relative to the sample median. The long-term stock return event study evidence in Cremers et al. (2019) also documents the positive effects of the implementation of directors' duties laws. By buying stocks of firms in states before a directors' duties law is implemented and by holding these stocks for one or two years, this long portfolio can generate positive and statistically significant abnormal returns (alphas). These monthly alphas exhibit magnitudes of between 0.48% and 0.62% and are also economically significant. The long-short portfolio consisting of the long portfolio minus the short portfolio of stocks of matched control firms generates a monthly abnormal return (alpha) of between 0.43% to 0.74%. These abnormal returns are statistically and economically significant. The findings of Cremers et al. (2019) are consistent with both the long-term benefit and bonding hypotheses.

2.4.3.3. Summary and avenues for future research

All in all, prior literature from before 2014 – although limited in breadth – suggests that directors' duties laws are either costly or of no importance for firms. Therefore, Gompers et al. (2003) classify directors' duties laws as statutes that reduce shareholder rights. Alexander et al.'s (1997) results consider these laws to exacerbate agency problems due to managerial entrenchment. More recent evidence, in contrast, documents the positive value effects of directors' duties laws, in aggregate and especially for innovative and stakeholder-oriented firms. These findings are consistent with the long-term benefit and bonding hypotheses. Additional controls for the institutional background of directors' duties laws and controls for states' propensity to pass directors' duties laws have substantially changed the findings of prior literature. Despite these very recent advances in the literature, the literature on directors' duties

laws is still limited, providing potentially fruitful avenues for future research. First, a potentially fruitful avenue for future research is to understand the relation between the strength of directors' duties laws and firm value. Barzuza (2009) describes the differences in directors' duties laws across different states. Barzuza's (2009) analysis implies that stronger directors' duties laws are more effective. The findings of Alexander et al. (1997), however, cast doubt on Barzuza's (2009) implications and question whether the strength of directors' duties laws affects firm value. For example, Barzuza (2009) classifies the directors' duties law of the state of New York as weak, since this law does not explicitly state that directors can consider stakeholder interests at the expense of shareholder interests. Interestingly, this weak law decreases firm value significantly according to Alexander et al. (1997). Firms in Indiana, a state with a strong directors' duties law, however, do not significantly react to the announcement of the directors' duties law. These inconsistent results may deserve more attention. Second, Cremers et al. (2019) document on average positive effects of directors' duties laws but do not test whether these laws are detrimental for other firms and for which firm characteristics these laws may be costly. Additional tests for the entrenchment hypothesis may help to explain why the adoption of directors' duties laws engendered a heated debate (Cremers et al., 2019), invoking a weakening of shareholder rights (Gompers et al., 2003). Third, Alexander et al.'s (1997) findings are consistent with the managerial entrenchment hypothesis, but the specific drivers behind this effect remain unclear. Therefore, tests distinguishing between different characteristics of managerial entrenchment such as the empire building, the quiet life, and the playing it safe hypothesis might be fruitful in providing a more complete picture on the effects of directors' duties laws.

Table 2-3: Directors' Duties Laws

This table shows studies on directors' duties laws and their relation to firm value.

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel A: Event study results					
Alexander, Spivey, and Marr (1997)	318 firms	1984, 1989	CAR(0,1)	Statistically negative	-0.33%*
			CAR(-1,1)	Statistically negative	-0.37%*
					Directors' duties laws decrease firm value. Poorly-managed firms and firms with no other takeover defenses drive these negative effects.
Cremers, Guernsey, and Sepe (2019)	992 observations	1983-2015	Stock returns	Statistically positive	The long portfolio buying firms before the implementation of directors' duties laws exhibits monthly abnormal returns of between 0.48% to 0.62%. The long-short portfolio generates returns of between 0.43% to 0.74%. Contingent on the factor model used, these abnormal returns are statistically significant at the 10%, 5%, and 1% level, respectively.
Panel B: Tobin's Q					
Faleye (2007)	2021 firms that filed proxy statements with the US SEC.	1995-2002	Tobin's Q	Insignificant	Directors' duties laws are not significantly related to Tobin's Q.
Bebchuk, Cohen, and Ferrell (2009)	IRRC sample	1990-2002	Log(industry-adjusted Tobin's Q)	Insignificant	Directors' duties laws are not significantly related to Tobin's Q.
Cremers, Guernsey, and Sepe (2019)	101,989 firm-year observations	1983-2015	Tobin's Q	Statistically positive	Directors' duties laws increase firm value by 6.1% to 8.0%. These effects are statistically significant at the 5% and 1% level, respectively. Complex firms, innovative firms, and stakeholder-oriented firms benefit from directors' duties laws.

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Table 2-3 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel C: Other performance measures					
Cremers, Guernsey, and Sepe (2019)	90,844 firm-year observations	1983-2015	Return on assets	Statistically positive	Directors' duties laws increase the return on assets by 0.8%**.
	84,705 firm-year observations		Return on equity	Statistically positive	Directors' duties laws increase the return on equity by 1.5%**.
	86,775 firm-year observations		Return on capital employed	Statistically positive	Directors' duties laws increase the return on capital employed by 1.0%**.

2.4.4. Fair Price Laws

2.4.4.1. Earlier evidence

Prior literature provides evidence that fair price laws are costly for potential targets (see Table 2-4 at the end of this subchapter). Karpoff and Malatesta (1989) outline that the announcements of fair price laws decrease shareholder wealth of affected firms by 0.59% for a two-day event window. This effect is statistically and economically significant. Faleye (2007) confirms this negative effect of fair price laws. In his sample, the implementation of a fair price law decreases firm value as measured by Tobin's Q by roughly 8%. This effect is statistically significant and economically meaningful.

2.4.4.2. More recent evidence

More recent literature, however, cannot find evidence for negative effects of fair price laws. John, Li, and Pang (2017) and Karpoff and Wittry (2018) document that fair price laws do not affect firms' operational efficiency as measured by return on assets. Both papers control for the institutional and legal background of takeover defenses, such as the implementation of other anti-takeover laws and lobbying activities of certain firms to mitigate endogeneity concerns. John et al. (2017) further explore the cross section of firms and test whether fair price laws affect firms that have a higher propensity to suffer from managerial entrenchment. Their proxy for managerial entrenchment is excess cash, which is the difference between the predicted amount of cash needed for future liquidity and the actual cash holdings. Interestingly, profitability of firms being more subject to agency problems is not significantly changed by

the implementation of fair price laws, a finding that is inconsistent with the managerial entrenchment hypothesis.

2.4.4.3. Summary and avenues for future research

All in all, prior literature from before 2014 – although limited in breadth – unanimously suggests that fair price laws are costly for firms. Therefore, Gompers et al. (2003) classify fair price laws as statutes reducing shareholder rights. More recent evidence, in contrast, reveals substantial changes in the interpretation of fair price laws and reveals that fair price laws do not reduce firm value. Moreover, recent literature points out that even firm value of firms being subject to agency problems is not statistically significantly changed through the implementation of fair price laws. Improved identification strategies controlling for the institutional background of takeover protection as well as cross-sectional analyses of firms are responsible for these new interpretations.

Despite these very recent advances in the literature, the literature on fair price laws is still limited, providing potentially fruitful avenues for future research. First, a deeper analysis of the cross-section of firms is largely unexplored. For example, evidence on whether long-term oriented firms or firms with major customers can benefit from fair price laws is extremely scarce. To the best of my knowledge, neither the long-term benefit nor the bonding hypothesis have been tested in the context of fair price laws. Second, also evidence on the strength of fair price laws across states is still scarce. Karpoff and Malatesta (1989) provide anecdotal evidence for differences in the power of fair price laws across states but, to date, research has not exploited these differences to study the effects of stronger vis-à-vis weaker fair price laws on firm value. Third, the influence of fair price laws on firm value in the context of other takeover defenses remains uncertain. Although Karpoff and Malatesta (1989) document that the direction and magnitude of the effects of takeover defenses on firm value are contingent on

preexisting takeover defenses it is still unclear whether this also is the case for fair price laws. This analysis may broaden the understanding of when fair price laws affect firm value. It may also be fruitful to supplement this analysis with further information on other additional takeover defenses largely ignored by prior literature to show how these largely ignored defenses affect the effects of fair price laws on firm value. Fourth, evidence exploiting differences in the takeover likelihood of firms in the context of fair price laws is scarce. As Eckbo and Langohr (1989) document, regulation governing the acquisition process to mitigate the advantage for bidders can be associated with increases in the firm value of targets. Since fair price laws guarantee target shareholders to obtain the highest price paid by bidders, firms with a higher takeover likelihood might react stronger to fair price laws than other firms. Studies exploiting these cross-sectional differences are very scarce.

Table 2-4: Fair Price Laws

This table shows studies on fair price laws and their relation to firm value.

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel A: Event study results					
Karpoff and Malatesta (1989)	95 firms	1982-1987	CAR(-1,0)	Statistically negative	-0.59%** Announcements of fair price laws decrease firm value.
Panel B: Tobin's Q					
Faleye (2007)	2021 firms that filed proxy statements with the US SEC.	1995-2002	Tobin's Q	Statistically negative	Fair price laws are negatively related to Tobin's Q. Tobin's Q decreases by 8.3% to 8.7%. The relation to Tobin's Q is statistically significant at the 1% level.
Panel C: Other performance measures					
John, Li, and Pang (2017)	43,319 firm-year observations	1976-1995	Return on assets	Insignificant	Fair price laws do not significantly affect firm profitability. Profitability of firms more subject to entrenchment is not significantly changed by the implementation of fair price laws.
Karpoff and Wittry (2018)	86,920 firm-year observations	1976-1995	Return on assets	Insignificant	Fair price laws do not significantly affect return on assets.

2.4.5. Golden Parachutes

2.4.5.1. Earlier evidence

Lambert and Larcker (1985) are the first to study the relation between the adoption of golden parachutes and firm value. Based on their event study evidence they conclude that golden parachutes are associated with significant increases in shareholder wealth. Their findings, however, are subject to specific event windows, to information regarding golden parachutes provided in proxy statements which may be contaminated with other firm-specific information, and to the market updating the takeover likelihood of the firm after the adoption of a golden parachute that is hard to control for. They only find significant results when using wider event windows, e.g., from 0 to 4 days or from -5 to 10 days after issuing the proxy statement. Interestingly, for the window -1 to +1 the relation between golden parachutes and firm value is negative with negative abnormal returns in each day around the publication of the proxy statement. These negative daily abnormal returns cast doubt on Lambert and Larcker's (1985) interpretation of golden parachutes. Follow-up literature studying the relation between golden parachutes and firm value reveals the lower performance of firms with golden parachutes consistent with the managerial entrenchment hypothesis (see Table 2-5 at the end of this subchapter). Long-term event study evidence suggests that golden parachute adopters have a significantly lower performance than a control group (Bebchuk, Cohen, & Wang, 2014). This relation is also economically meaningful. Golden parachute adopters exhibit a lower monthly performance of between 20 bps to 59 bps. Besides, Bebchuk et al. (2014) document that firms with golden parachutes experience negative abnormal stock returns during the adoption of golden parachutes and in the period subsequent to the adoption. Correlational evidence with regards to Tobin's Q confirms the above results. Bebchuk et al. (2009) as well as Cremers and Ferrell (2014) show that golden parachutes are negatively related to Tobin's Q. This association is statistically and economically significant. For example, Bebchuk et al.'s

(2009) results suggest that golden parachutes are associated with a 2.4% to 3.7% decrease in Tobin's Q.

2.4.5.2. More recent evidence

In contrast, more recently published literature suggests that golden parachutes either have no significant or a significantly positive relation to firm value and attribute these contrasting findings to weaknesses of prior literature. For example, Sepe and Whitehead (2015) demonstrate that golden parachutes are insignificantly related to Tobin's Q, but that R&D intensive firms benefit from golden parachutes. The adoption of a golden parachute is positively and statistically significantly related to firm value in innovative firms. Economically, this relation is meaningful and suggests a 13% higher Tobin's Q in innovative firms after these firms equip their executives with golden parachutes. Sepe and Whitehead (2015) conclude that their analysis points to the importance of long-term value creation due to golden parachutes. Their result is consistent with the long-term benefit hypothesis. Lund and Schonlau (2017) express that prior literature documents negative relations between golden parachutes and firm value due to an omitted variables bias triggered by data unavailability. Until 2006 there was no requirement for firms to report severance packages of their senior executives not tied to a change in control. Voluntary disclosures indicate that only six percent of all CEOs have these type of severance packages. After the disclosure requirement severance packages upon termination but regardless of a change in control have to be reported. Data indicate that half of the CEOs have these severance packages suggesting an eightfold increase vis-à-vis prior figures. Lund and Schonlau (2017) show that controlling for only voluntarily disclosed severance packages in the years prior to 2006 or ignoring severance packages as a control variable drives the negative relation between golden parachutes and firm value. Lund and Schonlau (2017) incorporate these additional controls and document that golden parachutes are

no longer negatively related to firm value. Therefore, more granular data on the severance packages of executives change the results of prior literature substantially. Lund and Schonlau (2017) also elaborate on the golden parachute proxies. A dummy variable approach proxying for the existence of a golden parachute suggests that golden parachutes are not significantly related to firm value. However, the dollar amount of a golden parachute is positively associated with Tobin's Q. This association is statistically and economically significant.

2.4.5.3. Summary and avenues for future research

All in all, prior literature from 2014 and before overwhelmingly suggests that golden parachutes are costly for firms. This finding is consistent with the entrenchment hypothesis. More recent evidence, in contrast, documents increases in firm value after the adoption of golden parachutes, in aggregate (for the dollar amount of golden parachutes) and especially for innovative firms. These findings are consistent with the long-term benefit hypothesis. More in-depth analyses that exploit the cross-section of firms and make use of increased data availability have changed the findings of prior literature on golden parachutes substantially. Despite these recent advances in the literature, there are still potentially fruitful avenues for future research. First, heterogeneity in the power and design of golden parachutes is largely unexplored. Golden parachutes may be of differing importance to executives. These differences may affect the relation to firm value. Moreover, golden parachutes exhibit different designs and can comprise several components such as severance pay, cash bonuses, stock options, and/or other benefits. These different designs of golden parachutes can lead to agreements that may or may not align shareholders' and executives' interests. The design of golden parachutes may therefore account for differing relations between golden parachutes and firm value. Evidence incorporating the details of the golden parachute design is extremely scarce. Above that, it is still unclear how individual components of golden parachutes relate to firm value. Second, literature usually

controls for golden parachutes through dummy variables. These dummy variable approaches separate executives with golden parachutes from executives without golden parachutes. However, these approaches neglect the value of golden parachutes in general and notably for the executives, e.g., the value of the golden parachute related to the executives' total compensation. As Lund and Schonlau's (2017) findings demonstrate different proxies for golden parachutes lead to different conclusions with regards to golden parachutes. However, none of the golden parachute proxies considers the salary loss of an executive triggered by a successful takeover. Analyzing the relation between firm value and a golden parachute measure scaled by the salary of the executive may provide a fruitful avenue for further research into the incentives of executives. Future research in this area can broaden our understanding of golden parachutes in the context of other compensation available to managers. Third, evidence on the relation between golden parachutes and firm value in the context of other takeover defenses is extremely scarce. Future research may benefit from assessing the robustness of the results for golden parachutes by incorporating a more detailed set of controls for other takeover defenses. This may also enable research to study the interactions between golden parachutes and other takeover protection. Fourth, evidence that exploits exogenous variation in the adoption and validity of golden parachutes is extremely scarce. Literature on takeover protection in general suggests that research designs that attenuate endogeneity concerns have the power to change prior results substantially. Literature on golden parachutes may benefit from these designs greatly. Fifth, apart from results being consistent with the long-term benefit hypothesis, evidence on other drivers in the context of golden parachutes is extremely scarce. Notably, it is unclear to what extent long-term partnerships with suppliers or customers – the bonding hypothesis – are related to firm value after the adoption of a golden parachute.

Table 2-5: Golden Parachutes

This table shows studies on golden parachutes and their relation to firm value.

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel A: Event study results					
Lambert and Larcker (1985)	90 firms and a subsample of 61 firms with non-confounding events.	1975-1982	CAR(-5,-1)	Insignificant	-0.1%
			CAR(0,4)	Statistically positive	0.5%**
			CAR(-5,10)	Statistically positive	0.6%**
			CAR(-1,1)	Negative	-0.2% (significance not tested) Lambert and Larcker (1985) conclude that golden parachutes increase firm value. Cumulative abnormal returns around the announcements of golden parachutes cast doubt on this conclusion.
Bebchuk, Cohen, and Wang (2014)	160 observations	1990-2003	Stock returns	Statistically negative	-0.59%*** The strategy of buying future-adopters and short-selling non-future-adopters of golden parachutes earns monthly abnormal returns of -0.59%.
	184 observations	1990-2005		Statistically negative	-0.20%* The strategy of buying adopters and short-selling non-adopters of golden parachutes earns monthly abnormal returns of -0.20%.
	150 observations	1993-2005		Statistically negative	-0.28%*** The strategy of buying long-term adopters and short-selling long-term non-adopters earns monthly abnormal returns of -0.28%.
	150 observations	1993-2005		Statistically negative	-0.24%** The strategy of buying long-term adopters and short-selling long-term non-adopters earns monthly abnormal returns of -0.24%.

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Table 2-5 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel B: Tobin's Q					
Bebchuk, Cohen, and Ferrell (2009)	IRRC sample	1990-2002	Log(industry-adjusted Tobin's Q)	Statistically negative	Golden parachutes are negatively related to industry-adjusted Tobin's Q (2.4% to 3.7%). This association is statistically significant at the 5% and 1% level, respectively.
Cremers and Ferrell (2014)	24,756 firm-year observations	1978-2006	Industry-adjusted Tobin's Q	Statistically negative	Golden parachutes are negatively related to industry-adjusted Tobin's Q. Tobin's Q decreases by 8.8% for firms with golden parachutes. This association is statistically significant at the 1% level.
Sepe and Whitehead (2015)	3,771 firm-year observations	2009-2011	Tobin's Q	Insignificant	Golden parachutes are not significantly related to Tobin's Q.
				Statistically positive	Golden parachutes in innovative firms are positively associated with Tobin's Q. A one standard deviation increase in R&D is associated with a 13% higher Tobin's Q. This association is statistically significant at the 1% level.
Lund and Schonlau (2017)	37,009 firm-year observations	1990-2013	Tobin's Q	Insignificant	Not controlling for executives' severance packages is one explanation for the negative views on golden parachutes. By controlling for these packages golden parachutes are not significantly related to Tobin's Q.
				Insignificant	By using a dummy variable proxy for the existence of a golden parachute, golden parachutes are not significantly related to Tobin's Q.
				Statistically positive	The dollar amount of a golden parachute is positively related to Tobin's Q. This relation is statistically significant at the 10% level.

2.4.6. Poison Pills

2.4.6.1. Earlier correlational evidence

Prior literature overwhelmingly considers poison pills to destroy firm value (see Table 2-6 at the end of this subchapter). Early event studies analyzing the announcements of poison pills document their costs. Malatesta and Walkling (1988) reveal that poison pills reduce firm value by between 0.9% to 2.3%. This reduction is statistically and economically significant. Ryngaert (1988), Karpoff and Malatesta (1989), Bojanic and Officer (1994), Comment and Schwert (1995), and Mahoney et al. (1996) confirm this result. Ryngaert (1988), for example, shows that the strongest type of poison pill, one that imposes financial penalties on the bidder when the bidder's ownership in a target passes a certain threshold, exhibits statistically significant and negative cumulative abnormal returns of 2.1%. This magnitude is economically meaningful. Ryngaert (1988) also shows that firms with a high takeover likelihood react negatively to the announcements of poison pills. Again, this reaction is statistically and economically significant. Johnson and Meade's (1996) results, however, do not suggest any significant relation between poison pill announcements and cumulative abnormal returns. Instead, the authors show that prior takeover defenses can influence the reaction to poison pill announcements. Firms with other takeover defenses react statistically and economically significantly more positive (1.4%) to poison pill announcements than firms without other takeover defenses. However, the reactions of both sub-samples are not significantly different from zero.

Prior literature studying the association between poison pills and Tobin's Q draws similar conclusions as prior event studies and overwhelmingly suggests that poison pills are costly and negatively related to Tobin's Q. For example, Bebchuk et al. (2009) show that poison pills are negatively related to industry-adjusted Tobin's Q. This relation is statistically significant and the decreases of between 4.2% to 6.1% in Tobin's Q after the implementation

of poison pills are economically meaningful. Cremers and Ferrell (2014) confirm the negative relation between poison pills and Tobin's Q and even report larger magnitudes. Cremers and Ferrell (2014) show that Tobin's Q decreases by up to 12% after the adoption of poison pills. This result is statistically and economically significant. Also, these negative views on poison pills are consistent with the managerial entrenchment hypothesis. One exception to this negative view on poison pills, however, is the paper by Faleye (2007) documenting insignificant results.

2.4.6.2. Earlier experimental evidence

Karpoff and Malatesta's (1989) study exploits the announcement of poison pills across states. Karpoff and Malatesta (1989) show that firms react negatively to these announcements. This effect is statistically and economically significant and decreases firm value by between 1.1% and 1.8%, a finding that is consistent with the entrenchment hypothesis. By exploiting poison pill adoptions across states Karpoff and Malatesta's (1989) research design mitigates endogeneity concerns such as the endogenous decision of a firm to implement a poison pill. Adoptions of poison pills across states are largely exogenous to firms. Further, Karpoff and Malatesta's (1989) research design addresses challenges with regards to which firms have access to poison pills. Prior literature suggests that firms that have not explicitly adopted a poison pill but are incorporated in a state with a poison pill statute do not have access to a poison pill. Cain et al. (2017) among others clarify that all firms incorporated in states with poison pill laws indeed have access to the poison pill defense. The corporate boards of these firms can almost immediately and unilaterally decide to implement a poison pill to deter an attack of a hostile bidder.

2.4.6.3. More recent (experimental) evidence

Recently, literature studying the adoption of poison pills across states and the influence of court decisions on poison pill effectiveness documents positive effects on firm value. Cremers, Jackson, and Morley (2016) show that firm value increases by 0.3% over a two-day event window for court rulings upholding the poison pill. This increase is statistically and economically significant. Bhojraj et al. (2017) exploit a change in the legal environment in Delaware in 1995, making it easier for firms to use poison pills. Firms protected by poison pills exhibit cumulative abnormal returns of between 0.7% to 1.1%. These abnormal returns are statistically and economically significant. Cremers, Litov, Sepe, and Zator (2022) conduct a long-term stock return event study by buying stocks of firms before the states in which these firms are incorporated adopt poison pill statutes and by short-selling stocks of firms incorporated in states that do not adopt poison pills. The long-short portfolio generates a monthly abnormal return of about 0.7%. The long portfolio alone generates a monthly abnormal return of between 0.7% to 0.9%. Both results are statistically and economically significant. The authors interpret their results as evidence for the long-term value of poison pills. Furthermore, Cremers et al. (2022) reveal that the positive effects of poison pill adoptions across states are more pronounced in the period covering 1995 to 2009, while being largely insignificant in the period from 1986 to 1990. They attribute this finding to discussions about the validity of poison pills during 1986 to 1988. In the following years the transparency about the validity of poison pills in states has increased, clearly documenting which states approve poison pills and which do not.

The evolvement of the literature studying the relation between poison pills and Tobin's Q is similar as to the event study evidence already surveyed. While prior literature overwhelmingly demonstrates that poison pills are costly, more recent literature that largely attenuates the endogeneity concerns of prior literature shows substantially different results.

Bhojraj et al. (2017) examine a legal change in Delaware that increases the effectiveness of poison pills. This change statistically significantly and positively affects Tobin's Q. The Tobin's Q of innovative firms affected by the legal change increases statistically significantly more than the Tobin's Q of non-innovative firms. The increase in Tobin's Q by 22% for innovative vis-à-vis non-innovative firms is also economically significant. Cremers et al. (2022) confirm the positive effects of poison pills on firm value. On average, poison pills increase Tobin's Q by 10%, a statistically significant and economically meaningful magnitude. This effect is driven by poison pills being adopted between 1995 to 2009 and is largely insignificant for poison pills adopted earlier. Moreover, innovative firms and firms with stakeholder relationships drive this effect. Innovative firms are defined as firms with high R&D expenses, high intangible assets, or high knowledge capital. Firms with stakeholder relationships are characterized by having a large customer, a strategic alliance, or a high fraction of labor capital to total assets. According to Cremers et al. (2022) a one standard deviation increase in intangible capital increases Tobin's Q by 7.1% for firms incorporated in states with poison pill statutes relative to control firms with average intangible assets. Firms with a large customer, for example, experience an increase in Tobin's Q of 6.9% relative to the sample mean. These results are statistically and economically significant. Further, these results are consistent with the long-term benefit and bonding hypotheses.

Moreover, Cremers et al. (2022) document statistically significant and positive effects of poison pills on sales growth and the net profit margin. For example, the adoption of poison pills increases sales growth by 2%, a magnitude that is economically meaningful. Fich et al. (2021) complement the findings of Cremers et al. (2022). Fich et al. (2021) document positive effects of the adoption of poison pill statutes on the marginal value of cash, on average and notably for firms with stakeholder relationships. For example, the implementation of poison pill statutes increases the marginal value of cash by almost 2%, a statistically and economically

significant effect. Fich et al.'s (2021) results also reveal that poison pills do not increase the marginal value of cash for firms more prone to entrenchment problems, such as for firms in concentrated industries or mature firms. However, the marginal value of cash increases statistically and economically significantly in firms with stakeholder relationships, consistent with the bonding hypothesis.

2.4.6.4. Summary and avenues for future research

All in all, prior literature overwhelmingly suggests that poison pills are costly for firms. This finding is consistent with the entrenchment hypothesis. More recent evidence, in contrast, points to the positive value effects of poison pills, in aggregate and especially for innovative and stakeholder-oriented firms. These findings are consistent with the long-term benefit and bonding hypotheses. As this survey describes, three advances in the recent literature are responsible for the substantial changes in the interpretation of poison pills. Recent literature has a higher propensity to comprise sample periods in which the validity of the poison pill defense is clear and unchallenged. Prior literature often employs sample periods in which the validity of poison pills was unclear (e.g., Ryngaert, 1988 and Karpoff & Malatesta, 1989). Moreover, recent literature explores the cross-section of firms to identify for which firms poison pills are costly or beneficial. For example, Fich et al.'s (2021) findings document that firms subject to entrenchment do not benefit from poison pills, but that firms with customer or supplier relationships benefit. Finally, more recent literature mitigates the endogeneity challenges that prior literature suffers from. Notably, the use of exogenous variation in the adoption of poison pills as well as the availability of additional controls for the institutional background of other takeover defenses substantially altered the interpretations about poison pills. Despite these recent advances in the literature, there are still potentially fruitful avenues for future research. First, there is surprisingly little evidence on the different types of poison

pills and to what extent these types vary across states. Malatesta and Walkling (1988) describe the different types of poison pills that come with different costs for firms (see Ryngaert, 1988). An experiment like the one of Cremers, Jackson, and Morley (2016) may reveal differences in the firm value effect of certain types of poison pills to court decisions strengthening or weakening these defenses. Second, the effects of poison pill adoptions deserve additional robustness checks. Although papers such as Cremers et al. (2022) test the robustness of their results, they do not put emphasis on controlling for among others court decisions influencing the validity of poison pills. Elaborations on these contexts can be fruitful avenues for future research. Third, evidence on the firm characteristics affecting the direction of the relation between poison pills and firm value is scarce. Ryngaert (1988) shows that the takeover likelihood can explain the relation between poison pills and firm value. Johnson and Meade (1996) document that prior takeover defenses can affect this relation. However, in more recent literature exploiting exogenous variation in poison pill adoption across states evidence on influencing factors such as the takeover likelihood or prior takeover defenses is extremely scarce. For example, these influencing factors may determine whether and when long-term oriented firms with stakeholder relationships benefit from poison pills. Fourth, evidence on the drivers of the event study results with regards to poison pill adoption is extremely limited. This is surprising, since researchers can easily exploit the staggered announcements of poison pills across states in an event study setting that interacts the poison pill announcements with firm-level controls proxying for entrenchment, long-term orientation or a stakeholder focus.

Table 2-6: Poison Pills

This table shows studies on poison pills and their relation to firm value.

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel A: Event study results					
Malatesta and Walkling (1988)	113 firms	1982-1986	CAR(-1,0)	Statistically negative	-0.92%*** Negative market reaction to poison pill adoptions.
	12 firms		CAR(-1,0)	Statistically negative	-2.30%*** Negative market reaction to poison pill adoptions of recent takeover targets.
Ryngaert (1988)	380 firms	1982-1986	CAR(-1,0)	Insignificant	-0.03%
	27 firm observations for takeover targets and strongest form of poison pill (PP)		CAR(-1,0)	Statistically negative	-2.12%***
	90 firm observations for non-takeover targets and strongest form of PP		CAR(-1,0)	Statistically negative	-0.61%** The strongest form of poison pill reduces firm value by 0.6% to 2.1%. Especially, firms with a high takeover likelihood react more negatively to the announcements of poison pills.
Karpoff and Malatesta (1989)	83 firms	1982-1987	CAR(-1,0)	Statistically negative	-1.125%** to -1.846%*** Poison pill adoptions across states decrease firm value by 1.1% to 1.8%.
Bojanic and Officer (1994)	210 firms	1967-1986	CAR(-1,0)	Statistically negative	-0.47%** Poison pill adoptions of firms decrease firm value.
Comment and Schwert (1995)	1459 firms	1983-1991	CAR(-1,1)	Statistically negative	-1.6%*** Poison pill adoptions of firms subject to takeover speculation decrease firm value.
Mahoney, Sundaramurthy, and Mahoney (1996)	196 firms	1985-1988	CAR(-50,5)	Statistically negative	-2.86%** Poison pill adoptions of firms decrease firm value.

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Table 2-6 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Johnson and Meade (1996)	31 firms with no existing takeover defenses at pill adoption	1983-1987	CAR(-1,0)	Insignificant	-0.94%
	160 firms with existing takeover defenses at pill adoption		CAR(-1,0)	Insignificant	0.43% Firms with other takeover defenses react statistically more positive than firms without takeover defenses (1.37%*). However, the reactions of both groups of firms are not significantly different from zero.
Cremers, Jackson, and Morley (2016)	604 firms	2004	CAR(0,1)	Statistically positive	0.319%***
			CAR(0,2)	Statistically positive	0.492%***
			CAR(0,3)	Statistically positive	0.459%*** Court rulings strengthening the power of poison pills increase firm value.

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Table 2-6 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Bhojraj, Sengupta, and Zhang (2017)	304 firms	1995	CAR(0,1)	Statistically positive	0.339%*** (January event) 0.380%*** (December event)
			CAR(0,3)	Statistically positive	0.660%*** (January event) 0.407%*** (December event) Poison pills increase firm value. Firms with poison pills exhibit positive and statistically significant cumulative abnormal returns for a 4-factor model with simulated nonevent period.
			CAR(0,1)	Statistically positive	0.70%**
			CAR(0,3)	Statistically positive	1.02%** Poison pills increase firm value. Firms with poison pills exhibit positive and statistically significant cumulative abnormal returns for a 4-factor model.
Cremers, Litov, Sepe, and Zator (2022)	491 observations	1986-2009	Stock returns	Statistically positive	The strategy of buying firms incorporated in states that adopt poison pill laws earns 0.7% to 0.9% monthly alpha (from 4-factor model). The long-short portfolio generates monthly abnormal returns of 0.7%. These monthly alphas are statistically significant at the 5% and 10% level, respectively. The driver of these positive abnormal returns is the 1995 to 2009 period, in which the validity of poison pills was clear across states.
Panel B: Tobin's Q					
Bebchuk, Cohen, and Ferrell (2009)	IRRC sample	1990-2002	Log(industry-adjusted Tobin's Q)	Statistically negative	Poison pills are negatively related to firm value. Tobin's Q decreases by 4.2% to 6.1%. These associations are statistically significant at the 1% level.

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Table 2-6 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Cremers and Ferrell (2014)	24,358 firm-year observations	1978-2006	Industry-adjusted Tobin's Q	Statistically negative	Poison pills are negatively related to industry-adjusted Tobin's Q. Tobin's Q decreases by 12% for firms with poison pills. This association is statistically significant at the 1% level.
Faleye (2007)	2021 firms that filed proxy statements with the US SEC.	1995-2002	Tobin's Q	Insignificant	Poison pills are not significantly related to Tobin's Q when controlling for staggered boards.
				Statistically negative	After the adoption of poison pills Tobin's Q decreases by 6%. This association is statistically significant at the 1% level.
Bhojraj, Sengupta, and Zhang (2017)	4,028 firm-year observations	1990-2000	Tobin's Q	Statistically positive	A legal change strengthening poison pills increases Tobin's Q by 43.5%. Tobin's Q of innovative firms increases by 22% vis-à-vis non-innovative firms. These effects are statistically significant at the 1% and 5% level, respectively.
Cremers, Litov, Sepe, and Zator (2022)	33,826 firm-year observations	1983-2012	Tobin's Q	Statistically positive	Poison pills increase Tobin's Q by 10%. This effect is statistically significant at the 5% level. This effect is driven by the 1995 to 2009 period. In this period the validity of poison pills across states was clarified. Also, innovative firms and stakeholder-oriented firms drive this effect. A one standard deviation increase in intangible capital increases Tobin's Q by 7.1% relative to control firms with average intangible assets. Firms with a large customer experience an increase in Tobin's Q by 6.9% relative to the sample mean.

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Table 2-6 (continued)

Paper	Sample size	Period	Performance measure	Effect on performance	Results
Panel C: Other performance measures					
Fich, Harford, and Yore (2021)	89,958 firm-year observations	1972-2010	Excess stock return	Statistically positive	The implementation of poison pills increases the marginal value of cash by 2%. These effects are statistically significant at the 5% and 1% level, respectively. These effects are driven by stakeholder-oriented firms.
				Insignificant	The value of cash does not increase for firms more prone to entrenchment problems, such as for firms in concentrated industries or mature firms.
Cremers, Litov, Sepe, and Zator (2022)	5,897 firm-year observations	1983-2012	Sales Growth	Statistically positive	The adoption of poison pills increases sales growth by 2%(*).
			Net Profit Margin	Statistically positive	The adoption of poison pills increases the net profit margin by 1.6%(**).

2.5. Conclusion

This survey summarizes the literature and categorizes the empirical studies on the topic into subgroups based on the specific takeover defense being studied and based on the publication date of the studies. Through this approach I have identified takeover defenses for which results change substantially so that the findings of prior literature may deserve a re-interpretation.

For the six takeover defenses surveyed, older studies that have been published prior to 2014 share a negative view on takeover defenses and focus on tests of the entrenchment hypothesis. These studies have found a receptive audience among shareholder rights activists and corporate governance rating agencies that have used these findings to penalize firms with takeover defenses. For example, the Harvard Law School's Shareholder Rights Project operated a program on board de-staggering proposals from 2011 to 2014, which led a substantial majority of the S&P 500 and Fortune 500 firms de-stagger their corporate boards (Cremers & Sepe, 2017). More recent literature, in contrast, has a positive view on takeover defenses and shows that it can be extremely costly to rely on older findings of the literature which motivated the Harvard Law School to implement the Shareholder Rights Project in 2011. Cremers and Sepe (2017) show that the de-staggering wave amounted to large decreases of about $149 billion in shareholder wealth. More recent literature suggests that the views on some takeover defenses have changed substantially.

Despite researchers have devoted considerable amount of time and attention to examining the relation between takeover defenses and firm value, older and newer literature seems to be divided on the net effect of more protection against takeovers. Given these ambiguous findings, this survey compiles the literature with the aim to identify influencing factors that can reconcile the mixed results for takeover defenses documented in the literature. Notably, this survey shows a pronounced heterogeneity in takeover defenses and cautiously

concludes that the net effect on firm value depends on the studied takeover defense. Further influencing factors comprise among others improved identification strategies (providing causal evidence), increased awareness for additional controls unidentified by prior literature, and changed views of shareholders on takeover defenses over time.

To date, it remains uncertain to what extent these influential factors can reconcile the evidence for other takeover defenses not covered in this survey. For brevity I only touch upon fruitful avenues for further research beyond the six surveyed takeover defenses. One potentially fruitful avenue aims at an improvement of the categorization of takeover defenses. For example, literature categorizing takeover defenses into increasing or decreasing shareholder rights assumes that decreases in shareholder rights are negatively related to firm value (e.g., Gompers et al., 2003). It is unclear to what extent the altered interpretations of six of the most important takeover defenses change the results of studies that rely on shareholder rights indices such as the G- or E-index. Moreover, the lack of recent evidence on many other (anti-)takeover provisions (e.g., cumulative voting, secret ballot, or blank check provisions) provides substantial scope for further research. Prior literature suggests that these provisions are costly for shareholder wealth (Bebchuk et al., 2009; Bhagat & Jefferis, 1991; Mahoney et al., 1996). Whether the recent advances in the literature might substantially change the views on these and other provisions is an open question. Furthermore, interactions among takeover defenses are still largely unexplored and provide an interesting avenue for further research. The same applies to the optimal number of takeover defenses. As this survey indicates not all firms have all of the surveyed takeover defenses. This may hint at an optimal, value-maximizing number of takeover defenses that future studies may examine in greater detail.

3. Takeover Protection and Firm Value[2]

3.1. Introduction

Mergers and acquisitions (M&As) are among the largest and most important investments firms make, amounting to a global value of $3.6 trillion in 2016 (data from SDC Platinum). Despite their importance for firms and markets, the regulation of M&As through anti-takeover laws differs across jurisdictions. While the U.S., Canada, Finland, and France have implemented anti-takeover laws, most countries have not. Instead, countries such as Germany, China, Japan, Italy, Sweden, Norway, Netherlands, Belgium, and Switzerland allow firms to implement firm-specific anti-takeover provisions. Other countries, such as India, Australia, Russia, Singapore, and South Africa, do not grant any takeover protection to firms. Regulators, academics, and practitioners have not reached a consensus on whether anti-takeover laws decrease firm value (e.g., because they could foster entrenchment) or increase firm value (e.g., because they could increase long-term orientation) and heatedly debate anti-takeover provisions or laws.[3] We contribute to this debate (1) by examining the effect of anti-takeover regulation on firm value and (2) by analyzing the potential distortions on capital allocation that such regulation can create.

To examine these two issues, we exploit the staggered announcement and implementation of the "put up or shut up" (PUSU) rule, which is a U.K. anti-takeover regulation that is binding and enforced by the regulator. An earlier version of the PUSU rule was implemented in 2004, but remained underused and largely ineffective (Financial Times, 2010a, 2010b; The Panel on Takeovers and Mergers, 2011) as its applicability could be circumvented. The new PUSU rule requires bidders to present a fully financed offer within 28

[2] This chapter is closely based on Andres, Jacob, and Ulrich (2019).

[3] Karpoff and Malatesta (1989), Comment and Schwert (1995), Bertrand and Mullainathan (2003), Giroud and Mueller (2010, 2011), Atanassov (2013), Cohen and Wang (2013), Bebchuk et al. (2014), and Cremers and Ferrell (2014) document a negative relation between anti-takeover provisions/laws and firm value. Caton and Goh (2008), Cremers, Masconale, and Sepe (2016), and Cremers and Sepe (2016) find a positive relation between anti-takeover provisions and firm value. Cain et al. (2017) report a decrease in firm value but higher takeover premiums. Karpoff and Wittry (2018) and Amihud and Stoyanov (2017) find no significant effect of anti-takeover provisions/laws on firm value.

days, beginning with leakage of interest in the target firm (put up) or to abstain from bidding for the specific target for six months (shut up). The PUSU rule, therefore, provides targets with the incentive and the permission to leak confidential, but non-friendly approaches. Deliberate leaks can benefit the target for several reasons. First, after a leak, public knowledge of the increased takeover probability is reflected in higher target stock prices. This increase in market value increases takeover premiums and represents a cost for the bidder (Schwert, 1996). Second, leaks can induce the entry of rival bidders, which again increases takeover premiums (Eckbo, 2009). Third, a leak increases the pressure for bidders and hampers their due diligence process and financing. This suggests that leaks by potential targets impose additional risks and financial burdens on bidders, which may lead to either less successful bids or higher takeover premiums.

This anti-takeover regulation represents a substantial shift in takeover regulation as the U.K. had a long tradition of banning any defensive tactics of managers (Armour & Skeel, 2007). Prior to the new PUSU rule, managers in the U.K. were not protected by anti-takeover provisions, anti-takeover laws, or dual-class share structures (e.g., Becht, Franks, Mayer, & Rossi, 2009, Franks, Mayer, & Rossi, 2009). For the first time, this anti-takeover regulation provides U.K. firms with takeover protection that has a "significant impact on market practice" (Grant Thornton UK LLP, 2011). Further, both stages of the staggered announcement of the anti-takeover regulation in 2010 and 2012 were unexpected. Using this quasi-natural experiment allows us to contribute to the literature with limited exogenous variation in anti-takeover laws, which yields mixed results (e.g., Cain et al., 2017; Catan & Kahan, 2016; Karpoff & Wittry, 2018).[4] One key advantage of the U.K. anti-takeover regulation is its staggered announcement and the fact that the regulation does not apply to all firms. This

[4] One potential reason for the mixed results is the interplay of several takeover protection measures in the U.S. These measures were adopted by multiple states and firms and influenced by court decisions, which could raise an omitted variables problem (Catan and Kahan, 2016; Karpoff and Wittry, 2018).

provides us with a set of control firms that are listed in the U.K., but unaffected by the U.K. anti-takeover regulation.

Using this event in a difference-in-differences design, we first compare the abnormal returns on the announcement days (first difference) between treated and control firms (second difference). We find strong evidence that, on average, the U.K. anti-takeover regulation increases firm value. Upon announcement of the anti-takeover regulation firms affected by the regulation have a 0.7 percentage point higher abnormal return than unaffected firms do. This result is robust to using alternative benchmarks or to using a matched sample of firms to account for observable differences between the treated and control groups.

Next, we examine the heterogeneity in the effect of takeover protection across potential bidders and targets. In theory, potential acquirers could lose firm value because anti-takeover regulation makes takeovers more costly. In contrast, we expect potential targets to increase in value because their position is strengthened. Targets benefit from an increase in bargaining power and better protection against so-called "bear hugs" or "virtual bids". These tactics may be used by bidders who consider making an offer for the target. However, instead of making a formal (and potentially hostile) takeover bid, interest is expressed through an unsolicited offer that is often made public. The intention of such an approach is to persuade target shareholders to exert pressure on the target board and to keep the target company "in play" for a longer time. This could eventually turn a potentially hostile into a (less costly) friendly takeover. To test for such cross-sectional differences, we use a triple-difference setting that compares the abnormal returns on the announcement days (first difference) between treated and control groups (second difference) and between potential acquirers/targets and the remaining firms (third difference). As proxies for potential acquirers, we use acquisition activity within the past three years, as well as firm size (e.g., Andrade, Mitchell, & Stafford, 2001; Andrade & Stafford, 2004). To proxy for potential targets, we use the target takeover probability before the U.K. anti-takeover regulation was implemented (Cremers, Nair, & John, 2009), as well as sales growth (Danbolt,

Siganos, & Tunyi, 2016). Consistent with our prediction, we find that potential bidders lose market value while potential targets gain market value. We also show that these findings are due to the increased protection of targets: there are fewer announced deals, fewer hostile takeovers, and the average takeover premium increases significantly if firms are subject to the anti-takeover regulation. This suggests that anti-takeover regulation can create binding frictions in the takeover market.

We next examine our second research question, the potential distortions on capital allocation and investment decisions caused by the regulation. In general, there are potential costs and benefits of anti-takeover regulation. On the one hand, anti-takeover regulation can mute the (detrimental) effect of information asymmetries for innovative, long-term–oriented projects and could incentivize managers to conduct more long-term projects (Stein, 1988, 1989). We find evidence consistent with this explanation: the value of long-term–oriented firms increases relative to short-term–oriented firms. Further, we show that long-term–oriented firms change their investment behavior after the reform by investing more in research and development (R&D) and intangible assets. Hence, the increase in market value of long-term–oriented firms indicates that the market expects a shift of these firms towards more long-term–oriented projects.

On the other hand, anti-takeover regulation could result in the potential misallocation of funds across firms. Higher takeover premiums can prevent high-productivity firms from taking over low-productivity firms that make less efficient investment decisions (Bloom et al., 2019; Bloom, Mahajan, McKenzie, & Roberts, 2010). In other words, the friction in the takeover market created by anti-takeover regulation could prevent market share from moving from less efficient to more efficient firms. Consistent with this argument, we find that the value of low-productivity firms increases significantly around the announcement of the anti-takeover regulation, since the owners of these firms might be able to negotiate higher takeover premiums. In contrast, high-productivity firms experience significantly lower abnormal returns

than low-productivity firms do. We also provide evidence for the mechanism behind this effect. We show that the takeover premiums of low-productivity firms increase by 25% after the enactment of the anti-takeover regulation.

In the final step, we examine the influence of managerial entrenchment, notably overinvestment, on announcement returns. We show that firms prone to overinvestment respond negatively to the announcement of the anti-takeover regulation. Under the new regulation, entrenched managers are no longer subject to disciplining market forces (Atanassov, 2013; Bertrand & Mullainathan, 2003; Cain et al., 2017; Giroud & Mueller, 2010; Karpoff & Malatesta, 1989). When we control for overinvestment, we still find an increase in the value of low-productivity firms. In contrast, the resulting increase in the value of R&D-intense firms becomes substantially smaller. Overall, our results indicate that an average positive market response does not necessarily indicate that anti-takeover regulation has a net benefit. While there are potential benefits, such as increased innovative activities, our findings imply that anti-takeover regulation induces potential distortions in the takeover market by preventing market share from moving from less efficient to more efficient firms.

Altogether, our paper complements studies on the costs (Atanassov, 2013; Bertrand & Mullainathan, 2003; Cain et al., 2017; Giroud & Mueller, 2010; Karpoff & Malatesta, 1989) and benefits of anti-takeover regulation (e.g., Fich et al., 2021; Stein, 1988, 1989): we show that anti-takeover regulation can help alleviate the negative consequences resulting from information asymmetries between the shareholders and managers, for example, of firms with innovative (and long-term) projects. However, at the same time, anti-takeover regulation not only results in more severe agency issues but, as we show, also prevents market share from moving from less efficient to more efficient firms.

3.2. Institutional background

3.2.1. General information

Our study focuses on the U.K., which is an open economy with well-developed institutions and capital markets. The U.K. corporate governance system is generally comparable to that of the U.S. due to their historical, cultural, and financial ties (Acharya, Sundaram, & John, 2011; Aggarwal, Erel, Ferreira, & Matos, 2011; Atanassov & Kim, 2009; Bruner, 2011; Kadyrzhanova & Rhodes-Kropf, 2011; Michaely & Roberts, 2012; Nenova, 2012) and similar legal systems, securities market regulations (La Porta, Lopez-de-Silanes, & Shleifer, 2006), law enforcement (La Porta et al., 2006; La Porta, Lopez-de-Silanes, Shleifer, & Vishny, 1998), and shareholder rights (Djankov, La Porta, Lopez-de-Silanes, & Shleifer, 2008; Kim & Lu, 2013). They are both characterized by dispersed share ownership (Allen, Carletti, & Marquez, 2015; Armour & Skeel, 2007; Bebchuk & Weisbach, 2010; Dahlquist, Pinkowitz, Stulz, & Williamson, 2003), similar shareholder proposal features (Christoffersen, Geczy, Musto, & Reed, 2007), mostly passive institutional investors as large corporate owners (Buchanan, Netter, & Yang, 2015; Cziraki, Renneboog, & Szilagyi, 2010), and shareholder value orientation (Martynova & Renneboog, 2011). In terms of M&As, both the U.K. and the U.S. are among the most active and most competitive takeover markets (Alexandridis, Petmezas, & Travlos, 2010; Defond & Hung, 2004; Martynova & Renneboog, 2008). Takeover premiums and the volume of transactions are higher in the U.S. and U.K. than in other countries (Alexandridis et al., 2010). Cumulative abnormal returns (CARs) around M&As and M&A performance are also very similar in both countries (Martynova & Renneboog, 2008).

3.2.2. Takeover regulation in the U.K.

Despite these institutional similarities, a key difference between the U.K. and U.S. corporate governance system is that U.K. takeover regulation leaves very little power to the board of potential target firms (Becht et al., 2009). Managers in the U.K. are not protected by

anti-takeover provisions (e.g., staggered boards, poison pills), dual-class shares, anti-takeover laws, or other defensive tactics common in the U.S. (Armour & Skeel, 2007; Becht et al., 2009; Danbolt & Maciver, 2012; Financial Times, 2013; Franks et al., 2009; Martynova & Renneboog, 2011). In the U.K., managers must refrain from taking actions that could frustrate an offer. In case of a bid, the target board must act in the target's interests and is not allowed to deny shareholders the opportunity to decide on the merits of a bid. One can only fight an offer by publishing a defense document that states the reasons for the rejection, lobbying for a reference to the competition authorities or seeking a white knight.

Takeovers in the U.K. are regulated by the Panel on Takeovers and Mergers (henceforth the Takeover Panel). Firms are subject to this regulation if they are incorporated in the U.K. and their securities trade on the London Stock Exchange (LSE). The LSE is the main and by far the largest U.K. exchange, with more than 2,200 listed companies with a total market value of more than £4,500 billion (as of 31 January 2017). It comprises two markets: the main market (official list) and the Alternative Investment Market (AIM).

The last major change in U.K. takeover regulation for decades was implemented in the wake of the Kraft–Cadbury takeover of 2009/2010. On September 7, 2009, Kraft made an indicative takeover bid for Cadbury, a U.K. company regulated by the Takeover Panel. Cadbury's management rejected the offer, which motivated Kraft to initiate a hostile bid for Cadbury. After a five-month takeover fight, Kraft succeeded in taking over Cadbury on February 2, 2010. Shortly before the deal was finalized, Kraft had given assurances not to close certain factories in the U.K. Only one week after deal completion, however, Kraft announced it was closing one of Cadbury's U.K. factories, leading to an outcry in the U.K. Despite this outcry, no changes to the takeover regulation were expected, due to the long tradition of deregulated markets in the U.K. (Financial Times, 2011).

Surprisingly, on October 21, 2010, the Takeover Panel announced suggestions for a new version of the "put up or shut up" (PUSU) rule, representing "one of the biggest shake-

ups of U.K. deal making rules in decades" (Financial Times, 2011). The new PUSU rule effectively protects firms against (hostile) acquisitions by requiring bidders to make an offer that is fully financed (i.e., they need a cash confirmation from their financial advisers) within 28 days, beginning with leakage of interest in the target firm (put up). If no bid is announced by the end of the 28-day PUSU period, the bidder must abstain from bidding for the specific target for the next six months (shut up). Without the target's consent, it is impossible for bidders to circumvent the PUSU rule. To address concerns that this rule may be circumvented or that exemptions have been granted, we contacted the Takeover Panel. According to the Takeover Panel, there have neither been exemptions from the PUSU rule nor extensions of the PUSU deadline without consent of the target. This implies that the PUSU rule as well as the related deadline are applied in practice as laid out in (the Notes on) Rule 2.6 of the Takeover Code.

As outlined in the Takeover Code Rule 2.3(d) and as described in the Practice Statements No 20, Point 3.5 and No 29, Point 2.3, targets are allowed to leak an approach of a potential bidder and to publicly identify the potential bidder at any time. Potential targets may have an incentive to leak the interest of a (potentially hostile) bidder to trigger the 28-day PUSU period for several reasons. First, after a leak, stock prices reflect the increased takeover probability, which implies a higher market value of the target. This increase in market value is an additional cost to the bidder, as the takeover premium is unrelated to the pre-bid stock price run-up of the target (Schwert, 1996). In other words, target shareholders expect the same takeover premium, irrespective of a target stock price run-up. The run-up, therefore, is an additional compensation for target shareholders and an extra cost to the bidder. Second, leaks can induce the entry of rival bidders, e.g., white knights, which are friendly bidders. Eckbo (2009) documents that bidding competition increases takeover premiums so that investors receive a higher compensation for their shares. Third, the binding 28-day PUSU deadline may hamper the bidder's financing and due diligence process, thereby increasing the risk of a failed bid. These arguments suggest that leaks impose additional risks and costs on bidders, which

may result in less successful bids or higher takeover premiums. This strengthens the bargaining position of potential targets.

In other instances—when the target does not leak information—the U.K. Takeover Panel requires a public announcement if there is rumor or speculation about a target firm or if the target experiences an untoward share price movement to start the PUSU period.[5] Failure to comply is sanctioned with cold shouldering, that is, a ban from any merger-related activity in the U.K. and punishment of the financial advisers.

The new PUSU rule replaces an earlier version, which had been implemented in 2004 and was underused and largely ineffective for several reasons (Financial Times, 2010a, 2010b; The Panel on Takeovers and Mergers, 2011). First, the old version of the PUSU rule allowed bidders to make an approach without having sufficient financing in place. Second, the PUSU period was not triggered automatically, but had to be requested by the target. Third, targets could only request a PUSU period if the bidder had revealed its identity. Finally, after these criteria had been met, the Takeover Panel could set a much longer PUSU period of usually 56 days (or more) for the bidder. Accordingly, the new PUSU rule improves the protection of potential targets across several dimensions.

A few days before the announcement of the new PUSU rule in 2010, Shearman & Sterling LLP (2010) considered changes to the U.K. takeover regulation unlikely. After the announcement, Gibson, Dunn & Crutcher LLP; Gibson, Dunn & Crutcher LLP (2010, 2011) analyzed "a number of ugly surprises" of this anti-takeover regulation for bidders and termed this anti-takeover regulation a "new approach." The announced regulation was implemented largely unaltered on September 19, 2011. It affects firms incorporated in the U.K. and listed on the LSE's main market, as well as AIM-listed firms if these are incorporated and managed in the U.K. Firms are considered to be managed in the U.K. if more than 50% of the board members have their residence in the U.K. On July 5, 2012, the Takeover Panel announced an

[5] An untoward share price movement is characterized by a 5% jump on a single day or 10% after an offer is first actively considered or an approach has been made.

extension of the anti-takeover regulation to all firms incorporated in the U.K. and listed on the LSE. Hence, firms incorporated in the U.K., listed on the AIM, but not managed in the U.K. were affected by this second announcement. This announcement of the expansion of the anti-takeover regulation became effective as of September 30, 2013. Table 3-1 summarizes the events in a timeline.

Both announcements of the U.K. anti-takeover regulation can be regarded as exogenous. The content and timing of the announcements came as a surprise to firms, the media, academics, financial advisors, lawyers, and the stock market. Prior to the announcements, there had been no news about the anti-takeover regulation.[6] Upon announcement, coverage of the anti-takeover regulation in the financial press was extensive, with cover pages articles in all *Financial Times* editions. It became clear that the U.K. anti-takeover regulation is "very far-reaching" (The Wall Street Journal, 2014) and has "significant impact on market practice" (Grant Thornton UK LLP, 2011). Obviously, the expansion of the anti-takeover regulation to all U.K. incorporated and listed firms on the AIM (second announcement) could have been anticipated. However, we could not find any evidence that the second wave of the U.K. anti-takeover regulation was anticipated. Moreover, none of our sample firms opted out of the anti-takeover regulation through reincorporation in a different jurisdiction. Finally, to mitigate concerns related to lobbying, we checked newspapers for potential lobbying efforts but did not find any such evidence.

Altogether, the U.K. anti-takeover regulation is expected to raise substantial friction in the takeover process in a country with no prior takeover defenses and is enforced like an anti-takeover law. According to M&A experts (Gibson, Dunn & Crutcher LLP, 2010, 2011; Grant Thornton UK LLP, 2011; Jefferies LLC, 2018) and the media (Financial Times, 2011, 2013; The Wall Street Journal, 2014), the anti-takeover regulation deters bidders from proceeding with a non-friendly approach and, therefore, increases the protection for targets.

[6] We carefully check that no events affected the treated cohorts differently than their control peers.

Table 3-1: Timeline of the events that led to the U.K. anti-takeover regulation

09 February 2010 — Kraft announces it is closing the Somerdale factory.
Speech by Roger Carr (former chairperson of Cadbury) urging more protection for U.K. firms.

01 June 2010 — The U.K. Takeover Panel announces the start of a consultation period with respect to a potential anti-takeover regulation and explains that it is open minded on the issues. At this time, an anti-takeover regulation is unlikely to be announced or implemented (Shearman & Sterling LLP, 2010).

21 October 2010 — Announcement of the anti-takeover regulation by the Takeover Panel. This is the first time a U.K. anti-takeover regulation is likely to be implemented (Gibson, Dunn & Crutcher LLP, 2010, 2011; Shearman & Sterling LLP, 2010).

	Incorporated in the U.K.		Not incorporated in the U.K.
	Managed in the U.K.	Not managed in the U.K.	
Main Market	Treated	Treated	Control
AIM	Treated	Control	Control

Total number of firms in cohort 1: 670
Number of treated firms in cohort 1: 485
Number of control firms in cohort 1: 185

21 July 2011 — Final form of the anti-takeover regulation is published, which is close to the initial announcement of the anti-takeover regulation on October 21, 2010.

19 September 2011 — U.K. anti-takeover regulation is implemented.

05 July 2012 — Announcement of the expansion to all AIM-listed and U.K.-incorporated firms (cohort 2).

	Incorporated in U.K.		Not incorporated in U.K.
	Managed in U.K.	Not managed in U.K.	
Main Market	-	-	Control
AIM	-	Treated	Control

Total number of firms in cohort 2: 225
Number of treated firms in cohort 2: 70
Number of control firms in cohort 2: 155

15 May 2013 — Final form of the expansion is published

30 September 2013 — The expansion to all AIM-listed and U.K.-incorporated firms is implemented

3.3. Empirical strategy and data

3.3.1. Event study

Our baseline empirical strategy relies on the staggered announcement and implementation of the U.K. anti-takeover regulation. We use these two events as quasi-natural experiments to analyze the effect of takeover protection on firm value. All of our sample firms are listed on the LSE and must comply with the same governance regulation established by the LSE. At the time of the first announcement of the anti-takeover regulation in 2010, firms incorporated in the U.K. and listed on the main market of the LSE as well as AIM-listed firms incorporated and managed in the U.K. were affected by the anti-takeover regulation. These firms comprise our treatment group. Our control group comprises all firms listed on the LSE but not incorporated in the U.K., as well as firms incorporated in the U.K. and listed on the AIM market of the LSE but not managed in the U.K.[7] For the second event in 2012, firms incorporated in the U.K., listed on the AIM, and not managed in the U.K. comprise the treatment group. Altogether, our treatment group comprises 555 observations. All other firms listed on the LSE and unaffected by the anti-takeover regulation (340 observations) comprise the control group. Table 3-1 illustrates the definitions of the treatment and control groups and their composition.

To capture the short-term effect of a change in takeover regulation on firm value, we apply an event study methodology and measure abnormal returns around the two announcements of the anti-takeover regulation for our treatment group relative to the control group. This results in a difference-in-differences approach. We compare returns around the announcement dates relative to firms' expected returns (first difference) to obtain abnormal returns. We then compare these abnormal returns between treated and untreated firms (second difference). We follow Karpoff and Malatesta (1989) and form portfolios of treated and control

[7] We do not differentiate between a primary or secondary listing on the LSE within our control group, since we do not expect these firms to react differently upon announcement of the U.K. anti-takeover regulation.

firms for the two announcements of the anti-takeover regulation. This approach mitigates the concern of cross-sectional correlation, since the event dates of the two portfolios are different. Firms can be in our sample twice, for example, when a firm is part of our control group during the first announcement and when it is treated during the second announcement. We thus use portfolios of treated and control firms for two events: the first and second announcements of the anti-takeover regulation in 2010 and 2012, respectively. We then estimate performance relative to the Fama-French three factor model and the Carhart momentum factor:

$$R_t = \alpha_t + \beta_t RMRF_t + \gamma_t SMB_t + \delta_t HML_t + \theta_t MOM_t + \varepsilon_t, \quad (1)$$

R_t is the daily return of the equally weighted portfolio of all firms that are treated (control) firms in the respective event years. $RMRF_t$ is the daily excess return on an European market proxy. SMB_t, $HML_t, and\ MOM_t$ are returns on zero-investment, factor-mimicking portfolios for size, book-to-market equity, and one-year momentum in stock returns. We estimate equation (1) from 241 to 41 trading days prior to each of the two announcements. We use the Fama-French European factors to account for different firm characteristics of LSE-listed firms that operate internationally. We estimate the expected portfolio return $\hat{R}_t$ by including the estimates of $\hat{\alpha}_t, \hat{\beta}_t, \hat{\gamma}_t, \hat{\delta}_t, and\ \hat{\theta}_t$ in equation (1). We obtain the abnormal return for each portfolio of treated and control firms in each event year as

$$AR_t := R_t - \hat{R}_t. \quad (2)$$

In other words, we subtract the estimated portfolio return $\hat{R}_t$ from the actual return R_t. In addition, we form a hedge portfolio of treated firms relative to control firms for each event. This portfolio is formed by subtracting the abnormal returns of the portfolio of control firms from the abnormal returns of the portfolio of treated firms. To obtain the CARs, we sum up the abnormal returns over the time interval around the announcement (in our case -1 to +1). We report the average CARs for the two events weighted by the number of observations in each portfolio to document an average effect of takeover protection on firm value.

3.3.2. Data and summary statistics

We use stock price and accounting data from Thomson Reuters Datastream/Worldscope. We drop firms for which we cannot calculate CARs[8] and firms with less than $10 million of total assets, missing information for total assets and other variables, or missing firm age, and financial firms (Standard Industrial Classification, or SIC, codes 6000–6999). Continuous variables are in U.S. dollars. All variables are defined in Table 3-2. We winsorize all variables at the top and bottom 1%. The above selection criteria leave us with a sample of 895 observations. We provide a detailed description of the sample selection in Table 3-3.

[8] We drop firms missing stock price information and that are thinly traded (more than 50% missing returns or more than 75% returns of zero within the estimation window).

Table 3-2: Variable definitions

This table defines our main variables. All continuous variables are winsorized at 1% and 99%.

Independent variables	Intangibles	Intangible Assets / lagged Total Assets, such as goodwill, patents, copyrights, trademarks, licenses.
	R&D	R&D expenses/lagged total assets.
	Innovative Firm	Dummy variable that equals 1 if the firm is smaller than the median firm and has positive R&D expenses and 0 otherwise.
	Target Takeover Probability	Likelihood of the takeover of a target firm, following Cremers et al. (2009). We model the probability of a firm becoming a target the next year via a logit specification. This specification is estimated by using several independent variables at the end of the previous year, such as Tobin's Q, PPE, cash, block ownership, market capitalization, leverage, and return on assets.
	Sales Growth	One-year growth in sales.
	Acquirer in past 3 years	Dummy variable that equals 1 if the firm was an acquirer within the last three years and 0 otherwise.
	Ln(Assets)	Natural logarithm of Total Assets.
	Overinvestment	Dummy variable that equals 1 if a firm is prone to overinvestment and 0 otherwise. We follow Biddle, Hilary, and Verdi (2009) as well as Cheng, Dhaliwal, and Zhang (2013) and use the magnitude of the residuals obtained from a firm-specific regression of investment on sales growth as a proxy for overinvestment. Firms are classified as overinvesting if their residual is larger than twice the standard deviation of their industry peers.
	High Productivity	We follow prior literature (Cappellari, Dell'Aringa, & Leonardi, 2012; Chemmanur, He, & Nandy, 2010; Kim & Ouimet, 2014; Krishnan, Nandy, & Puri, 2015) and define firm-level total factor productivity as the residual of a log-linear Cobb–Douglas production function. For each two-digit SIC code industry–year group, we regress value added (EBITDA + labor) on capital and labor proxied by fixed assets and wage expenses, respectively. Firms whose average residuals over 2 years are above the median of each 2-digit SIC code industry–year group are categorized as high-productivity firms.
Outcome variables	Announced Bid	Dummy variable that equals 1 if there is a bid for a firm in the respective year and 0 otherwise.
	Hostile Bid	Dummy variable that equals 1 if the bid is classified as hostile and 0 otherwise.
	All Cash Bid	Dummy variable that equals 1 if the method of payment is solely cash and 0 otherwise.
	Takeover Premium	Final offer for the target stock price four weeks prior to the M&A announcement, as reported by SDC Platinum, following Field and Karpoff (2002) and Officer (2003).
	Transaction Value / Total Assets	Transaction value to total assets of the fiscal year before the M&A announcement.
	CAR(-1, 1)	CAR for the event window (-1, 1), calculated from the Fama-French three factor model plus Carhart momentum factor

Table 3-3: Sample selection

This table reports our sample selection process. Column (1) reports the number of observations after each query step. Column (2) describes the query step.

No. of Obs. after query		Query Description
Cohort 1:	2,239	All LSE-listed firms with ordinary shares
Cohort 2:	2,115	
Cohort 1:	1,817	Match to the Thomson Datastream/Worldscope sample based on ISIN. We drop closed-ended funds, equity exchange-traded funds, and global depository receipts.
Cohort 2:	1,770	
Cohort 1:	1,606	We drop investment trusts and duplicate observations based on the Worldscope Permanent ID
Cohort 2:	1,558	
Cohort 1:	1,205	We drop missing fiscal years, missing total assets, and missing firm age
Cohort 2:	1,158	
Cohort 1:	1,005	We drop financial firms (SIC codes 6000–6999)
Cohort 2:	971	
Cohort 1:	878	We drop firms with less than $10 million in total assets
Cohort 2:	859	
Cohort 1:	670	We drop firms with missing stock price information or that are thinly traded (more than 50% missing returns or more than 75% returns of zero within the estimation window)
Cohort 2:	696	
Cohort 1:	670	We drop observations of cohort 2 if they were treated in cohort 1
Cohort 2:	225	

Table 3-4 presents summary statistics for our sample firms. We split these statistics by firms that are affected and unaffected by the U.K. anti-takeover regulation. We present the mean as well as the standard deviation (in parentheses, below the mean). The variable *Treat* is equal to one if the firm is affected by the announcement of the U.K. anti-takeover regulation and zero otherwise. About 62% (= 555/895 observations) of our sample observations are from treated firms. Of the 555 treated firms, 485 are affected by the first announcement, while 70 firms are affected by the second announcement of the U.K. anti-takeover regulation. Altogether, we have 340 firms in the control group (185 for the first announcement and 155 for the second).[9]

[9] One potential concern is that there are confounding market events affecting our control group as some of them are cross-listed in other countries. We carefully searched for such events but could not find confounding macro-events around our two event dates that can affect our results.

The treatment and control groups are similar in some dimensions but different in others. For example, treated firms tend to be more innovative and more mature than control firms are. The differences between the treatment and control groups are not a concern for our analysis if 1) the common trends assumption is not violated, 2) the assignment to the treatment and control groups is exogenous to the announcement of the anti-takeover regulation, and 3) there is no omitted variable correlated with the difference between the treatment and control groups that is also correlated with abnormal returns around the announcement days.

To test for parallel trends (see Table A1 of Appendix A), we study all possible pre-event windows, starting from 40 trading days to two trading days prior to the announcements of the anti-takeover regulation. For 99.1% of these event windows, we cannot find significant differences in abnormal returns between treated and control firms. Further, the mostly insignificant coefficients are very evenly distributed around zero, supporting the parallel trends assumption.

We argue that the other potential issues do not affect our results for three reasons. First, since the announcement of both changes in the regulation came as a surprise, it is unlikely that the selection into the main and AIM markets is endogenous with respect to the anti-takeover regulation. In fact, we observe that no firm changes from the main to the AIM market to avoid the anti-takeover regulation. Second, we use several fixed effects to mitigate the concern that industry–year or country of incorporation–year characteristics drive our results.[10] Third and most importantly, we show that our results are robust to using a matching approach that eliminates differences in observable characteristics (see Table A8 of Appendix A). Since the matching results are close to our baseline results in the paper, we cautiously conclude that differences between our treatment and control groups do not affect our estimates.

[10] More than 78% of our sample firms are incorporated in the U.K., more than 5% in Ireland, and more than 5% in Australia, New Zealand, and Bermuda. All in all, our sample firms are from 26 different jurisdictions.

Table 3-4: Summary statistics

This table presents descriptive statistics for our pooled sample, the sample firms that were affected by the U.K. anti-takeover regulation, and those that were not affected. Panels A and B present summary statistics for our control and outcome variables. Panel C presents statistics for our treatment variables. The mean and standard deviation (in parentheses) for each variable are reported separately. Column (1) reports the estimates for our whole sample of LSE-listed firms. Column (2) reports estimates for the sample firms affected by the anti-takeover regulation. Column (3) reports estimates for sample firms not affected by the anti-takeover regulation. Column (4) reports the p-value from a t-test of the difference between affected and unaffected firms, with standard errors adjusted for clustering at the firm level. The control variable *Intangibles* is defined as intangible assets over lagged total assets; *R&D* is defined as R&D expenses over lagged total assets; *Innovative Firm* is a dummy variable that equals one if the firm is smaller than the median firm and has positive R&D expenses and zero otherwise; *Target Takeover Probability* is a target firm's likelihood of takeover (Cremers et al. (2009). We model the probability of a firm becoming a target the next year via a logit specification. This specification is estimated by using several independent variables at the end of the previous year, such as Tobin's Q, property, plant, and equipment (PPE), cash, block ownership, market capitalization, leverage, and return on assets; *Sales Growth* is the one-year growth in sales; *Acquirer in past 3 years* is a dummy variable that equals one if the firm was an acquirer within the last three years and zero otherwise; *Ln(Assets)* is the natural logarithm of total assets; *Overinvestment* is a dummy variable that equals one if the firm is prone to overinvestment and zero otherwise (Biddle et al. (2009) Cheng et al. (2013); *Ln(Firm Age)* is the logarithm of years since the firm's incorporation; and *High Productivity* is defined as the residual of a log-linear Cobb–Douglas production function. For each two-digit SIC code industry–year group, we regress value added on capital and labor. Firms whose average residuals over two years are above the median of each two-digit SIC code industry–year group are categorized as high-productivity firms. The variable *Announced Bid* is a dummy that equals one if there was a bid for the firm in the respective year and zero otherwise; *Hostile Bid* is a dummy variable that equals one if the bid for the firm is classified as hostile and zero otherwise; *All Cash Bid* is a dummy variable that equals one if the method of payment is solely cash and zero otherwise; *Takeover Premium* is the ratio of the final offer price to the target stock price four weeks prior to the M&A announcement; *Transaction value / total assets* is the ratio of the M&A transaction value to total assets of the fiscal year before the M&A announcement.

Variables	(1) Full Sample	(2) Affected by U.K. Anti-Takeover Regulation	(3) Unaffected by U.K. Anti-Takeover Regulation	(4) p-Value of Difference (2) - (3)
Panel A: Control variables				
Intangibles	0.261 (0.395)	0.262 (0.270)	0.259 (0.540)	0.908
R&D	0.017 (0.047)	0.016 (0.046)	0.018 (0.047)	0.582
Innovative Firm	0.152 (0.359)	0.175 (0.380)	0.115 (0.319)	0.022
Target Takeover Probability	0.010 (0.008)	0.010 (0.006)	0.010 (0.010)	0.267
Sales Growth	0.092 (0.846)	-0.029 (0.588)	0.291 (1.123)	0.000
Acquirer in past 3 years	0.532 (0.499)	0.550 (0.498)	0.503 (0.501)	0.207
Ln(Assets)	12.934 (2.290)	12.761 (2.015)	13.216 (2.658)	0.024
Overinvestment	0.040 (0.197)	0.036 (0.187)	0.047 (0.212)	0.444
Ln(Firm Age)	2.763 (1.093)	2.863 (1.043)	2.599 (1.152)	0.002
High Productivity	0.513 (0.500)	0.522 (0.500)	0.493 (0.501)	0.502
Panel B: Outcome variables				
Announced Bid	0.037 (0.188)	0.041 (0.199)	0.029 (0.169)	0.030

Hostile Bid	0.100 (0.301)	0.117 (0.322)	0.089 (0.285)	0.260
All Cash Bid	0.385 (0.487)	0.307 (0.462)	0.436 (0.496)	0.002
Takeover Premium	14.657 (26.645)	11.467 (24.622)	16.886 (27.784)	0.013
Transaction Value / Total Assets	0.469 (0.161)	0.393 (0.058)	0.862 (0.151)	0.034
Panel C: Treatment overview				
Number of LSE sample firms	895	555	340	
Number of M&A deals	798	316	482	

3.4. Takeover protection and firm value

3.4.1. Event study results

Figure 3-1 presents daily abnormal returns for a pooled sample of both announcements of the U.K. anti-takeover regulation. The graph shows that the treated and control firms have similar daily abnormal return patterns before the announcements of the U.K. anti-takeover regulation. This supports the parallel trends test in Table A1 of Appendix A. Upon announcement of the anti-takeover regulation, treated firms experience higher abnormal returns than control firms for about two days. After two days, treated and control firms again experience similar abnormal returns. Due to the wide media attention starting one day after the actual announcements, it is reasonable to assume that, after a few days, the announcements of the regulatory changes are fully priced.

Figure 3-1: Daily abnormal returns around the announcements of the U.K. anti-takeover regulation

This figure plots the daily abnormal returns for a pooled sample of treated and control firms with respect to the Fama-French three factor model plus Carhart momentum factor over a window of nine trading days. Day 0 is the event date.

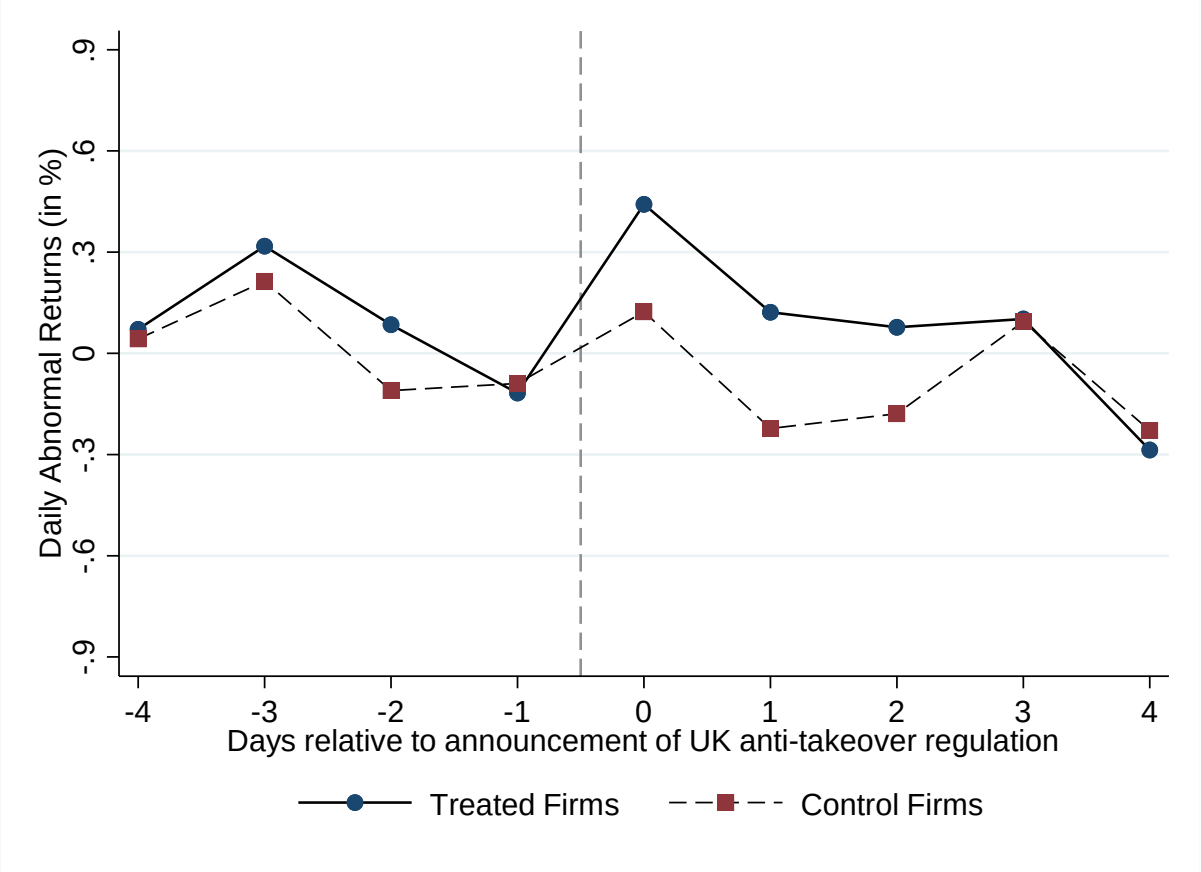

Table 3-5, Panel A, reports event study results for the announcements of the U.K. anti-takeover regulation supporting the graphical evidence. For the event window (-1, 1), we find a significant and positive abnormal return of 0.64% for the hedge portfolio of treated minus control firms. This effect goes in the opposite direction of that documented by prior U.S. literature (Giroud & Mueller, 2010; Jahera & Pugh, 1991; Karpoff & Malatesta, 1989; Linn & McConnell, 1983). Our results indicate that firms affected by the U.K. anti-takeover regulation, on average, benefit from takeover protection. The control firms experience statistically insignificant abnormal returns.

Table 3-5: Event study results for the announcements of the U.K. anti-takeover regulation

This table reports the event study results for the staggered announcement of the U.K. anti-takeover regulation. We identify the relevant announcement dates and form portfolios of the treated and control firms following Karpoff and Malatesta (1989) to avoid cross-sectional correlation problems. The numbers reported in the table are the average portfolio CARs weighted by the number of firms. Announcement returns are presented for the treated and control firms, as well as for the hedge portfolios of treated firms minus control portfolios. Panel A shows our main results using a four factor model including the Fama French 3 factors and the Carhart momentum factor. Panel B uses the market model with several market indices. Panel C matches treated to control firms using entropy balancing. Patell Z-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: Main results

Event Window	Treatment	Control	Treatment – Control
(-1, 0)	**0.32%***	0.03%	0.29%
	(1.79)	(0.13)	(1.21)
(-1, 1)	**0.45%***	-0.19%	**0.64%***
	(1.75)	(0.80)	(1.85)
(-1, 2)	0.52%	-0.37%	**0.89%****
	(1.46)	(0.12)	(2.07)

Panel B: Alternative market indices, results for the event window (-1, 1)

Market index	Treatment	Control	Treatment – Control
FTSE 100	**0.78%***	0.11%	**0.67%****
	(1.76)	(0.20)	(2.14)
FTSE All-Share	**0.78%***	0.11%	**0.67%****
	(1.84)	(0.24)	(2.13)
MSCI UK	**0.78%***	0.11%	**0.67%****
	(1.76)	(0.20)	(2.14)
Eurostoxx 50	**0.75%***	0.13%	**0.62%***
	(1.81)	(0.74)	(1.84)
MSCI Europe	**0.68%***	0.04%	**0.64%****
	(1.77)	(0.49)	(2.01)
MSCI World	0.61%	-0.09%	**0.70%****
	(1.61)	(0.51)	(2.11)

Panel C: Matching

Event Window	Treatment	Control	Treatment – Control
(-1, 0)	**0.32%***	-0.37%	0.70%
	(1.79)	(0.29)	(1.60)
(-1, 1)	**0.45%***	-1.01%	**1.46%*****
	(1.75)	(-1.01)	(3.16)
(-1, 2)	0.52%	-1.03%	**1.56%*****
	(1.46)	(-0.93)	(2.75)
Observations	555	340	895

Panels B and C, Table 3-5, present the results of robustness tests of average market reactions. In Panel B, we show that our results are robust to using the market model with different market indices as the benchmark. The CARs in Panel B for treated firms are slightly higher, while those of the hedge portfolios are of the same magnitude as in Panel A. Most three-day event window CARs for the treatment group and the hedge portfolios are statistically significant. The CARs for the control group are insignificant. Second, to address concerns that our results are driven by differences between the treatment and control groups, we use a matched sample using entropy balancing (Hainmueller, 2012; Hainmueller & Xu, 2013). We match control firms to treated firms based on size, sales growth, leverage, cash, a main market or AIM market dummy, and pre-announcement abnormal returns. All matching variables are determined before the announcement of the U.K. anti-takeover regulation. The resulting hedge portfolio of treated minus control firms yields a statistically significant CAR of 1.46% over the three-day event window (see Panel C).

3.4.2. Wealth transfer from potential acquirers to potential targets

Before turning to the analysis of the potential costs and benefits of anti-takeover regulation (Section 5), we first examine one potential mechanism behind the average market response. If the U.K. anti-takeover regulation indeed represents friction in the capital market, constraining potential acquirers from taking over potential targets (as argued in Section 2), we should observe a wealth transfer from potential acquirers to potential targets. We thus run OLS regressions using the CAR(-1, 1) of each firm as the dependent variable. We then use a difference-in-difference-in-differences approach, where the first difference is the abnormal return comparing stock returns at the announcement of the anti-takeover regulation with predicted returns. The second difference is the difference between the abnormal returns for the treated and control firms. The third difference compares the market response of potential

targets and potential acquirers, respectively, to firms not classified as potential targets and potential acquirers. In our regressions, we pool our sample to have sets of control firms for firms treated by the announcements of the regulation in 2010 and 2012. Sample pooling ensures that we measure the average effect of the staggered announcement of the anti-takeover regulation, since the treatment of each group (cohort *c*) of firms occurs at two different points in time: firms of the first (second) cohort are affected by the announcements of the anti-takeover regulation in 2010 (2012) and the control firms are not. We thus estimate

$$\begin{aligned} \mathrm{CAR}_{i,c} = {} & \alpha_{j,c} + \alpha_{k,c} + \beta_1 \mathrm{Treat}_{i,c} + \beta_2 \mathrm{Target}_{i,c} \\ & + \beta_3 (\mathrm{Treat}_{i,c} \times \mathrm{Target}_{i,c}) + \gamma' \mathrm{X}_{i,c} + \varepsilon_{i,c}, \end{aligned} \tag{3}$$

$$\begin{aligned} \mathrm{CAR}_{i,c} = {} & \alpha_{j,c} + \alpha_{k,c} + \beta_1 \mathrm{Treat}_{i,c} + \beta_2 \mathrm{Acquirer}_{i,c} \\ & + \beta_3 (\mathrm{Treat}_{i,c} \times \mathrm{Acquirer}_{i,c}) + \gamma' \mathrm{X}_{i,c} + \varepsilon_{i,c}, \end{aligned} \tag{4}$$

where $CAR_{i,c}$ is the dependent variable CAR(-1, 1) for firm *i* of cohort *c*, following Faleye (2007), industry *j* and country of incorporation *k*. The variable $Treat_{i,c}$ is a dummy equal to one if firm *i* of cohort *c* is affected by the announcement of the U.K. anti-takeover regulation. This coefficient is a difference-in-differences coefficient capturing the effect of treated firms in the baseline group. The interaction terms *Treat* × *Target* and *Treat* × *Acquirer*, respectively, estimate the third difference, namely, whether the value implications of the anti-takeover regulation announcements are different for potential targets and potential acquirers (relative to the baseline group, i.e., the group not captured with either *Target* or *Acquirer*). Our proxies for potential targets ($Target_{i,c}$) are the target takeover probability according to Cremers et al. (2009) before the U.K. anti-takeover regulation,[11] as well as one-year sales growth (Danbolt et al., 2016). We expect that potential targets can benefit from the anti-takeover regulation, which increases their bargaining power and may allow them to negotiate higher takeover premiums.

[11] We follow Cremers et al. (2009), who model the probability of a firm becoming a target in the next year in a logit model. This specification is estimated using several independent variables, namely, Tobin's Q, PPE, cash, block ownership, market capitalization, leverage, and return on assets.

Further, potential targets are better protected against "bear hugs" or "virtual bids", which may distract target management from focusing on daily business activities. On the other hand, increased takeover protection may also further entrench poorly performing managers of potential targets and therefore destroy shareholder value. In our analyses we study the net effect of all explanations. If potential targets on average benefit from the U.K. anti-takeover regulation, β_3 in equation (3) should be positive.

We proxy for potential acquirers ($Acquirer_{i,c}$) in equation (4) by acquisition activity within the past three years and by firm size (e.g., Andrade et al., 2001; Andrade & Stafford, 2004). Our motivation for using firm size as a proxy for a firm's acquisitiveness is based on the work of Andrade et al. (2001), who find acquirers to be larger than targets. If potential acquirers lose firm value because the anti-takeover regulation introduces friction in the takeover market, β_3 should be negative.

We include a vector of firm control variables $X_{i,c}$, including firm size, size squared, and firm age (see also Giroud and Mueller (2010). We include industry–cohort ($\alpha_{j,c}$) and country of incorporation–cohort ($\alpha_{k,c}$) fixed effects to control for industry-, country-, and year-specific effects. Since the two cohorts that we analyze are from two distinct years, cohort fixed effects are equal to year fixed effects. Industry–cohort fixed effects ensure that the control and treatment firms are from the same industry and subject to the same economic conditions. We cluster standard errors at the firm level.[12] We further standardize continuous variables to have a mean of zero and a standard deviation of one to simplify the interpretation of the results.

The results from estimating equation (3) are presented in Panel A of Table 3-6. We find that potential targets benefit from takeover protection upon announcement of the anti-takeover regulation, as indicated by the positive and significant interaction terms. A one standard

[12] We alternatively cluster standard errors by the country of incorporation and find the results to be unaffected by this different clustering (see also Lel and Miller, 2015).

deviation increase in takeover probability is associated with a 0.9 percentage point higher CAR for potential target firms relative to firms of average takeover probability. Overall, firms with a higher takeover probability experience a significant absolute change of 2.1 percentage points in their market value, as indicated by the positive sum of coefficients *Treat* + *Treat* × *Target Takeover Probability*.

In the following, we illustrate these interpretations. Since the dependent variable CAR already accounts for the difference in actual vs. expected (i.e., without announcement of the new regulation) stock returns, the coefficient *Treat* is the difference-in-differences estimate. It measures the difference in CAR for treated firms with average takeover probability vis-à-vis control firms with average takeover probability. The coefficient suggests that treated firms have a 1.2 percentage point higher CAR than control firms (both groups with average takeover probability). This difference is, however, not statistically significant. The interaction effect of *Treat* × *Target Takeover Probability* then measures the incremental effect of takeover probability for treated firms. This triple difference estimate amounts to 0.9 percentage points and suggests that treated firms with a higher takeover probability experience a higher announcement return, relative to treated firms with lower takeover probability. The overall 'net' effect for firms with a higher takeover probability is then the sum of the coefficients *Treat* + *Treat* × *Target Takeover Probability* (i.e., 0.9 percentage points + 1.2 percentage points). This sum of 2.1 percentage points (statistically significant) measures the difference in CAR for treated firms with a one standard deviation higher takeover probability vis-à-vis control firms.

We obtain very similar results when we use sales growth to proxy for potential targets. A one standard deviation increase in sales growth leads to a 0.9 percentage point higher CAR relative to the average treated firm. Further, a one standard deviation increase in sales growth leads to a 2.3 percentage point higher CAR for treated firms as compared to control firms.

Table 3-6: Winners and losers of the announcements of the U.K. anti-takeover regulation

This table reports the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the announcements of the U.K. anti-takeover regulation. Panel A interacts target firm characteristics that are associated with a higher takeover probability with the treatment dummy and therefore reports the value effects for potential targets affected by the announcements of the U.K. anti-takeover regulation. According to Cremers et al. (2009) and Danbolt et al. (2016), public target firms are characterized by high sales growth, low profitability as measured by return on assets, low leverage, and young age. Panel B reports the value effects for past acquirers and firms that likely will be acquired in the future due to their size and which are affected by the announcements of the U.K. anti-takeover regulation. All the specifications report only the treatment and interaction terms for brevity, but control for the respective interacted variable. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: Targets

	(1)	(2)
Variables	Target Takeover Probability	Sales Growth
Treat	0.012	0.014
	(1.46)	(1.63)
Treat × Target Takeover Probability	0.009*	
	(1.74)	
Treat × Sales Growth		0.009**
		(2.01)
Joint Significance		
Treat + Treat × Target Takeover Probability	0.021**	
	(2.04)	
Treat + Treat × Sales Growth		0.023**
		(2.25)
Controls	YES	YES
Industry × Year FE	YES	YES
Country × Year FE	YES	YES
Observations	895	895
R-Squared	0.183	0.184

Panel B: Acquirers

	(1)	(2)
Variables	Acquirer in past 3 years	Ln(Assets)
Treat	0.024***	0.008
	(2.57)	(0.95)
Treat × Acquirer in past 3 years	-0.024**	
	(-2.33)	
Treat × Ln(Assets)		-0.012*
		(-1.91)
Joint Significance		
Treat + Treat × Acquirer in past 3 years	0.000	
	(0.00)	
Treat + Treat × Ln(Assets)		-0.003
		(-0.29)
Controls	YES	YES
Industry × Year FE	YES	YES
Country × Year FE	YES	YES
Observations	895	895
R-Squared	0.186	0.182

In Panel B of Table 3-6, we present the results from estimating equation (4). We document that firms affected by the anti-takeover regulation that have not been acquirers within the past three years (*Treat*) experience a 2.4 percentage point higher CAR relative to similar control firms. This effect is statistically significant. However, treated firms that acted as acquirers in the past three years (interaction of *Treat* × *Acquirer in past 3 years*) experience a negative CAR of 2.4 percentage firms, relative to treated firms that did not acquire. The sum of these coefficients *(Treat + Treat* × *Acquirer in past 3 years)* is not statistically significant, suggesting that firms that have been acquirers within the past three years do not experience an effect on their value following the announcements of the PUSU rule.

For large firms, the results are similar. Treated firms that are one standard deviation larger than the average firm experience a 1.2 percentage point drop in market value, relative to treated firms of average size. This effect is statistically significant and suggests that firms that are more likely to acquire experience lower CARs than other treated firms. Overall, past acquirers and large firms do not experience a significant change in their market value upon announcement of the PUSU rule, as indicated by the insignificant sum of coefficients. This suggests that for acquirers and large firms the negative effects offset the positive effects of the anti-takeover regulation.

The results in Table 3-6 show that potential targets benefit from the anti-takeover regulation and experience a share price increase. In contrast, potential acquirers experience negative returns upon announcement, plausibly, because the takeover premiums increase. For these firms, the benefits of the anti-takeover regulation are offset by the negative effects. The average (positive) market response suggests that the positive response from potential targets outweighs the negative market response from potential acquirers.

3.4.3. Establishing the mechanism: Changes in the takeover market

One potential mechanism for the wealth transfer from potential acquirers to targets is the friction the anti-takeover regulation creates in takeovers, which increases the protection of target firms (DeAngelo & Rice, 1983; Harris, 1990; Stulz, 1988). To test this empirically, we examine different M&A outcome variables. We therefore complement our data with M&A data from Thomson SDC Platinum. We restrict this sample (the M&A sample) to target firms that receive bids that, if completed, would result in the bidder gaining control of the target (i.e., equity stakes greater than 50%). We exclude M&A deals prior to 2005 to enhance the similarity of pre- and post-regulation deals. This leaves us with 798 M&A deals from 2005 to 2015.

We use the following M&A variables: *Announced Deal* is an indicator variable that is set to one if an M&A deal is announced. For the sample of targets that received a bid, we use three outcome variables: First, we use *Hostile Bid*, a dummy variable that is one if a bid is classified as hostile. Second, *All Cash Bid is* a dummy variable that is one if the method of payment is solely cash. Third, we use the *Takeover Premium*, defined as the ratio of the final offer price to the target stock price four weeks prior to the M&A announcement, following Field and Karpoff (2002) and Officer (2003). Fourth, we use transaction value to total assets. Summary statistics of these dependent variables (presented in Table 3-4, Panel B) are comparable to those in the literature (Comment & Schwert, 1995; Cunat, Gine, & Guadalupe, 2020; Field & Karpoff, 2002; Kadyrzhanova & Rhodes-Kropf, 2011; Sokolyk, 2011). The average takeover premium for our sample firms is 15%; 10% of our M&A bids are hostile and 39% are solely in cash.

When examining the likelihood of an M&A deal, we conduct an unconditional analysis, as Comment and Schwert (1995). Hence, firms that are not targeted in year *t* are also in the sample. We then estimate a linear probability model to be able to include several fixed effects:

$$y_{i,t} = \alpha_{c,t} + \alpha_{c,j} + \alpha_{c,k} + \alpha_{c,i} + \beta_1 \mathrm{Treat}_{c,i} + \beta_2 \mathrm{Post}_{c,i} + \beta_3 (\mathrm{Treat}_{c,i} \times \mathrm{Post}_{c,i}) + \gamma' X_{i,t} + \varepsilon_{i,t}, \quad (5)$$

for firm *i* in time (year) *t*, where $\alpha_{c,t}$, $\alpha_{c,j}$, $\alpha_{c,k}$, and $\alpha_{c,i}$ are year–cohort, industry–cohort, country–cohort, and firm–cohort fixed effects, respectively. The dependent variable $y_{i,t}$ is the dummy variable *Announced Deal* if firm *i* is targeted in year *t*; $Treat_{c,i}$ is a dummy that equals one if firm *i* in cohort *c* is affected by the U.K. anti-takeover regulation; $Post_{c,i}$ is a dummy that equals one if firm *i* of cohort *c* is affected by the implementation of the U.K. anti-takeover regulation; and $X_{i,t}$ is a vector of controls including firm size, size squared, and firm age, following Giroud and Mueller (2010). For the other three dependent variables, *Hostile Bid*, *All Cash Bid*, and *Takeover Premium*, we only use announced M&A deals and do not include firm fixed effects, since firms are typically observed only once in this sample.[13]

Column (1) of Table 3-7 shows that firms protected by the U.K. anti-takeover regulation are less likely to be targeted. Conditional on having received an offer, there are fewer hostile (Column (2)) and significantly more all-cash bids (Column (3)). Finally, the average takeover premium for treated firms increases significantly after the implementation of the anti-takeover regulation. In economic terms, we find that, after the adoption of the U.K. anti-takeover regulation, 1.9% fewer bids were announced. This effect is driven by 10.8% fewer hostile bids after the implementation of the anti-takeover regulation (Column 3). Target firms affected by the anti-takeover regulation can negotiate a 15.1% higher takeover premium, leading to significantly higher valuations (Columns 4 and 5). These findings are robust to alternative specifications.[14] Altogether, these results provide evidence that the anti-takeover regulation

[13] We also note that our results are robust to M&A controls (see Table A2 of Appendix A). We do not add M&A controls since they could be affected by the anti-takeover regulation and would bias our results.

[14] In untabulated regressions, we follow Schwert (1996) and use the CARs from 42 trading days prior to the date of the bid announcement through delisting or 126 trading days after the bid (whichever comes first) as an alternative proxy for the takeover premium. In these regressions, we confirm our above-mentioned results. Target firms can negotiate statistically and economically significantly higher takeover premiums (18.7% compared to 15.1% in our main specification). Following Comment and Schwert (1995), we analyze unconditional takeover premiums, that is, we set the premiums to zero in non-takeover firm–years. Our results remain statistically significant.

indeed created significant frictions in the M&A market by reducing the number of M&A deals and by increasing the takeover premium acquirers have to offer.

Table 3-7: U.K. anti-takeover regulation and changes in the M&A market

This table reports the effects of the staggered implementation of the U.K. anti-takeover regulation on several M&A outcome variables associated with the target firm's bargaining power. Column (1) reports the results of the OLS regressions with an announced deal dummy as the dependent variable that equals one if the firm is a target in an M&A deal. Column (2) reports the results of OLS regressions with a completed deal dummy as the dependent variable that equals one for targets successfully acquired in an M&A deal. Column (1) reports the results for a sample that is not conditional on the firm receiving an M&A bid. Column (2) reports the results of OLS regressions with a hostile bid dummy as the dependent variable. Column (3) reports the results of OLS regressions with an all-cash bid dummy as the dependent variable. Columns (4) and (5) report the results of OLS regressions with the takeover premium (transaction value over total assets of the target) as the dependent variable. We measure the effect of a difference-in-differences specification on the different outcome variables. All the specifications control for all constituents of the difference-in-differences interaction term, which are not tabulated here for brevity. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Dependent variables	(1) Announced Deal	(2) Hostile Bid	(3) All-Cash Bid	(4) Takeover Premium	(5) Transaction Value / Total Assets
Treat × Post	-0.019*	-0.108*	0.180*	0.151***	1.114**
	(-1.78)	(-1.68)	(1.72)	(2.98)	(2.57)
Controls	YES	YES	YES	YES	YES
Year × Cohort FE	YES	YES	YES	YES	YES
Industry × Cohort FE	YES	YES	YES	YES	YES
Country × Cohort FE	YES	YES	YES	YES	YES
Firm × Cohort FE	YES	NO	NO	NO	NO
Observations	9,172	798	798	705	704
R-Squared	0.148	0.267	0.319	0.302	0.438

3.5. Potential costs and benefits of anti-takeover regulation

The average positive market reaction shown in the previous section does not necessarily imply that the U.K. anti-takeover regulation is beneficial for the economy. An average market response is an insufficient measure of the overall consequences of anti-takeover regulation. On the one hand, higher firm value could reflect improved overall welfare, because anti-takeover regulation can reduce information asymmetries related to the valuation of innovative, long-term–oriented projects (Stein, 1988, 1989). In the absence of takeover protection, managers could be tempted to invest more in easily understandable projects and forgo innovative projects

(Manso, 2011). Anti-takeover regulation can reduce myopia and incentivize managers to invest in long-term projects (Stein, 1988, 1989).

On the other hand, the positive market reaction could reflect the misallocation of funds across firms: there is a risk that the friction in the takeover environment created by the increased protection of targets can prevent takeovers of less efficient firms by more efficient competitors. The more efficient firm can either employ its superior technology in the production process or use its superior sales, pricing, or marketing processes in the less efficient firm they intend to take over. Synergies from such takeovers may not be realized when less efficient, low-productivity firms gain more takeover protection. Low-productivity firms can then only be acquired at higher cost by high-productivity firms. This prevents some of the market share of less efficient firms from being moved to more efficient firms. Hence, aggregate sales and productivity could decrease (see, e.g., Bethmann, Jacob, and Müller, (2018)). An anti-takeover regulation could thus result in the potential misallocation of funds across firms, because low-productivity firms make less efficient investment decisions (Bloom et al., 2010; Bloom et al., 2019) and because the takeover process of these less efficient firms is constrained by the regulator. However, since the value of low-productivity firms can increase due to more takeover protection, the overall market reaction can still be positive. In the following, we examine the potential costs—discouraging the takeover of less efficient firms—and benefits—encouraging a long-term orientation—of anti-takeover regulation and compare their economic magnitudes.

Finally and directly related to the costs of anti-takeover regulation, takeover protection can insulate managers from the disciplining forces of the market for corporate control (Volpin, 2002) and could encourage them to act against shareholders' interests (Bertrand & Mullainathan, 1999; Gormley & Matsa, 2016), thereby destroying firm value. While this

explanation does not explain the positive average market reaction reported in the U.K., an evaluation of anti-takeover regulation needs to consider these potential consequences.

3.5.1. Benefits of anti-takeover regulation: Encouraging long-term projects

We start by examining whether anti-takeover regulation can mitigate the negative consequences arising from information asymmetries that could induce managers to invest in short-term–oriented projects to prevent takeovers. To test whether takeover protection reduces myopia, we examine the cross-sectional variation in CARs with respect to long-term orientation. Our proxies for long-term value creation are *R&D* (R&D expenses divided by lagged total assets), *Intangibles* (intangible assets divided by lagged total assets), and *Innovative Firm* (a dummy equal to one if the firm's size is below the median and if the firm has positive R&D expenses).[15] Intangibles such as patents or goodwill proxy for innovation but, at the same time, measure information asymmetries, since the value of intangibles cannot be verified by outside shareholders (Cremers & Sepe, 2016; Edmans, Heinle, & Huang, 2016; Tirole, 2006). On average, intangible assets (R&D expenses) amount to 26% (2%) of total assets. A total of 15% of our sample firms are characterized as innovative.

We build on equation (3) and use the three variables *R&D*, *Intangibles*, and *Innovative Firm* as alternative proxies for long-term orientation. Panel A of Table 3-8 presents the regression results. We again standardize continuous variables to have a mean of zero and a standard deviation of one to simplify the interpretation. For all three variables, we find support of a positive market reaction for firms with a more long-term orientation. For example, a one standard deviation increase in R&D expenses leads to a 0.7 percentage point higher CAR for firms affected by the announcements of the U.K. anti-takeover regulation compared to treated firms with average R&D. A one standard deviation increase in intangible assets induces a 1.4

[15] We obtain similar results when defining *Innovative Firm* based on age and R&D (Daines et al., 2021).

percentage point higher CAR relative to treated firms with average intangibles. Innovative firms experience a 2.5 percentage point higher CAR after being affected by the announcements of the U.K. anti-takeover regulation relative to firms that are less innovative. Our joint significance tests suggest that long-term oriented firms, in general, benefit from the announcements. For example, treated innovative firms outperform firms unaffected by the announcements by 3.5 percentage points. These results are consistent with takeover protection mitigating short-termism and providing managers with a more long-term view that increases firm value (Cremers et al., 2017; Cremers & Sepe, 2016; Daines et al., 2021).

One explanation for this finding is that the market seemed to expect firms susceptible to short-termism to change their behavior following the implementation of the anti-takeover regulation. However, whether a shift toward more innovation really occurred is an empirical question. To investigate this interpretation further, we test whether firms with the greatest incentives for short-termism, that is, firms with high R&D prior to the staggered enactment of the anti-takeover regulation, in fact changed their innovation activity under the anti-takeover regime. As in our prior analyses, we apply a triple-difference approach.[16] This time, the first difference measures differences in R&D activity between the treatment and control firms. The second difference encompasses the difference between before and after the implementation of the U.K. anti-takeover regulation. The third difference is the difference between high- and low-R&D firms (defined prior to the enactment of the anti-takeover regulation). Accordingly, the interaction term *Treat* × *Post* × *High R&D* captures the change in innovation activity of long-term–oriented firms after the implementation of the anti-takeover regulation. We thus estimate

[16] We use the same sample as for the analysis of announced M&A deals. However, we lose a few observations as information on some additional controls is missing for some observations.

$$\begin{aligned} \mathrm{R\&D_{i,t}} = {} & \alpha_{c,t} + \alpha_{c,j} + \alpha_{c,k} + \alpha_{c,i} + \beta_1 \mathrm{Treat_{i,c}} + \beta_2 \mathrm{Post_{i,t}} \\ & + \beta_3(\mathrm{Treat_{i,c}} \times \mathrm{Post_{i,t}}) + \beta_4 \mathrm{High\ R\&D_i} \\ & + \beta_5(\mathrm{Treat_{i,c}} \times \mathrm{High\ R\&D_i}) + \beta_6(\mathrm{Post_{i,t}} \times \mathrm{High\ R\&D_i}) \\ & + \beta_7(\mathrm{Treat_{i,c}} \times \mathrm{Post_{i,t}} \times \mathrm{High\ R\&D_i}) + \gamma' \mathrm{X_{i,t}} + \varepsilon_{i,t}, \end{aligned} \tag{6}$$

where $R\&D_{i,t}$ is R&D expenses over lagged total assets for firm *i* in year *t*; $Treat_{i,t}$ is a dummy that equals one if firm *i* is affected by the U.K. anti-takeover regulation; $Post_{i,t}$ is a dummy that equals one after the implementation of the U.K. anti-takeover regulation; and *High* $R\&D_i$ is our proxy for the long-term value creation of firm *i*, a dummy that equals one if firm *i*'s R&D expenses are in the top quartile before the implementation of the U.K. anti-takeover regulation. We expect β_7 to be positive, as long-term–oriented firms—which had the greatest incentives for short-termism before the anti-takeover regulation—are expected to increase their innovation activity when takeover protection increases. We again include the vector of control variables $X_{i,t}$, including firm size, size squared, firm age, cash holdings, leverage, and return on assets, to proxy for the availability of funds, as well as Tobin's Q to proxy for investment opportunities. The fixed effects structure follows equation (5). Standard errors are clustered by firm.

The results are reported in Panel B of Table 3-8. We find a statistically significant and positive effect of the U.K. anti-takeover regulation on innovation activity for firms with a long-term orientation. Firms in the highest R&D quartile before the implementation of the regulation increase R&D by 0.9% of total assets relative to firms in other R&D quartiles after being more protected from takeovers. When using intangible assets as the dependent variable (Column (2)), we continue to find that firms with a large fraction of intangible assets relative to total assets prior to the anti-takeover regulation increased their intangible assets after the reform compared to treated firms in other quartiles. Further, firms that were already among the most innovative group before the announcements seem to experience further increases in innovation activity, as indicated by the positive and significant sum of the coefficients, i.e., *Treat* × *Post* + *Treat* × *Post* × *High R&D pre-reform*.

In sum, Table 3-8 documents that firms with the greatest incentives for short-termism were positively revalued at the announcement of the anti-takeover regulation. This likely occurred because the market plausibly expected an increase in innovation activity after the anti-takeover regulation was enacted.

Table 3-8: U.K. anti-takeover regulation and long-term orientation

Panel A reports the OLS regression results with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the announcements of the U.K. anti-takeover regulation. In this analysis, firm characteristics that can be associated with long-term value creation are interacted with the treatment dummy to test the long-term benefit hypothesis. Panel A reports the results for *R&D*, *Intangibles*, and the dummy *Innovative Firm*. Panel B reports changes in real innovation activity after firms are affected by the U.K. anti-takeover regulation. In this analysis, the difference-in-differences dummy variable is interacted with a long-term orientation dummy variable. This dummy equals one if the firm is in the top quartile of R&D or intangible assets, respectively, before the implementation of the U.K. anti-takeover regulation. All the specifications in Panel A report only the treatment and interaction terms for brevity, but control for the respective interacted variable. Panel B controls for all possible interaction terms of a triple-difference approach and for the additional controls for cash, Tobin's Q, leverage, and return on assets. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: CAR analysis

	(1)	(2)	(3)
Variables	R&D	Intangibles	Innovative Firm
Treat	0.014	0.011	0.010
	(1.62)	(1.37)	(1.10)
Treat × R&D	0.007*		
	(1.87)		
Treat × Intangibles		0.014**	
		(2.47)	
Treat × Innovative Firm			0.025*
			(1.72)
Joint Significance			
Treat + Treat × R&D	0.020**		
	(2.43)		
Treat +		0.025**	
Treat × Intangibles		(2.36)	
Treat +			0.035**
Treat × Innovative Firm			(2.46)
Controls	YES	YES	YES
Industry × Year FE	YES	YES	YES
Country × Year FE	YES	YES	YES
Observations	895	895	895
R-Squared	0.180	0.187	0.182

Panel B: Change in real innovation activity

	(1)	(2)
Dependent variables	R&D	Intangibles
Treat × Post × High R&D pre-reform	0.009*	
	(1.66)	
Treat × Post × High intangibles pre-reform		0.192***
		(3.00)
Treat × Post	-0.000	-0.064***
	(-0.41)	(-2.83)
Joint Significance		
Treat × Post + Treat × Post × High R&D pre-reform	0.009*	
	(1.68)	
Treat × Post + Treat × Post × High intangibles pre-reform		0.127**
		(2.25)
Controls	YES	YES
Additional Controls	YES	YES
Year × Cohort FE	YES	YES
Industry × Cohort FE	YES	YES
Country × Cohort FE	YES	YES
Firm × Cohort FE	YES	YES
Observations	8,860	8,860
R-Squared	0.860	0.794

3.5.2. Cost of anti-takeover regulation: Preventing the takeover of low-productivity firms

In addition to these potential benefits, we next examine whether the frictions in the takeover environment induced by the U.K. anti-takeover regulation can lead to a potential misallocation of funds across firms. The anti-takeover regulation could make it more costly and thus less likely for low-productivity firms to be taken over by more efficient firms. Hence, the overall positive market response could coincide with distortion of the takeover market, since the competitive selection of firms in the market is distorted.

To test this prediction empirically, we follow prior literature (Cappellari et al., 2012; Chemmanur et al., 2010; Kim & Ouimet, 2014; Krishnan et al., 2015) and define firm-level total factor productivity (*Productivity*) as the residual of a log-linear Cobb–Douglas production function. For each industry–year group of a European sample comprising firms from the U.K., France, Germany, Ireland, Italy, the Netherlands, Spain, and Sweden, we regress value added—

defined as earnings before interest, taxes, depreciation, and amortization (EBITDA) plus wage expenses—on capital input and labor input proxied by fixed assets and wage expenses, respectively. All variables are in natural logarithms. Firms whose average residuals over two years are above the median of each industry–year group are categorized as high-productivity firms (*High Productivity* = 1). Firms below the median are categorized as low-productivity firms. Therefore, our productivity measure can be interpreted as a firm's productivity relative to its industry peers each year. Since we define productivity as the gap between a firm's observed output and its predicted output, our productivity measure contains only the idiosyncratic part of each firm's productivity. This reduces concerns that we are measuring intangible asset investment within the industry (see also Bethmann et al. 2018).

Using this productivity proxy, we re-estimate equation (3) but use the dummy variable *High Productivity* instead of *Target*. We expect the value of low-productivity firms to increase upon the announcement of the anti-takeover regulation. The overall effect for low-productivity firms is measured by the coefficient of *Treat*. We further expect high-productivity firms to experience a much lower increase in value as taking over low-productivity firms becomes more costly for them. This effect is captured by the interaction term *Treat* × *High Productivity*. The sum of both coefficients reflects the overall response for high-productivity firms.

Column (1) of Table 3-9 presents regression results consistent with the potential distortion created by anti-takeover regulation. We find that low-productivity firms benefit from greater takeover protection, as indicated by the positive coefficient of *Treat*. In economic terms, we find that the value of treated low-productivity firms increases by 2.6% relative to the control group. Treated high-productivity firms experience a 3.7 percentage point lower CAR than treated low-productivity firms do. Overall, treated high-productivity firms do not experience a significant absolute change in their market value relative to the control group, as indicated by the insignificant sum of coefficients *Treat* + *Treat* × *High Productivity*.

In Column (2) of Table 3-9, we provide evidence of a potential mechanism for this finding. We rerun the test on the takeover premium from Table 3-7, Column (4), and estimate the effect on the takeover premium for low- as well as high-productivity firms. Because of the improved takeover protection for targets, we expect that low-productivity firms are able to negotiate higher takeover premiums. This is exactly what we find: the coefficient of *Treat* × *Post* indicates that low-productivity firms' takeover premium increases by 25% after the implementation of the anti-takeover regulation. Further, high-productivity firms do not experience a change in their takeover premiums from before to after the implementation of the anti-takeover regulation, as indicated by the insignificant sum of the coefficients. A potential explanation is that these firms are (already) quasi-protected by their high productivity and the premium that bidders pay for this level of productivity. However, we do not find a significantly lower change (relative to low-productivity firms) in the takeover premium for high-productivity firms, which could be a power issue in this test.

Taken together, our results suggest that the anti-takeover regulation increases the takeover protection for low-productivity firms, thereby preventing market share from moving from less efficient to more efficient firms. Therefore, even though the average firm value in our sample increases, there could be more distortions in the takeover market, as anti-takeover regulation can constrain the allocation of capital and assets across firms.

Table 3-9: Announcement returns and takeover premiums: Breakdown by productivity level

Column (1) of this table reports the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the announcements of the U.K. anti-takeover regulation. In this analysis, firm characteristics that can be associated with high productivity are interacted with the treatment dummy to report the value effects for high-productivity firms that are affected by the announcements of the U.K. anti-takeover regulation. We define firm-level total factor productivity as the residual of a Cobb–Douglas production function where we regress value added (EBITDA + labor) on capital and labor proxied by fixed assets and wage expenses, respectively. We estimate this firm-level regression separately for each two-digit SIC code industry–year group. Firms with residuals above the median in each industry–year group are categorized as high-productivity firms. Columns (2) and (3) report the effects of the staggered implementation of the U.K. anti-takeover regulation on the takeover premium and the ratio of transaction value to total assets. These M&A outcome variables can be associated with the bargaining power of target firms. All the specifications control for all the constituents of the difference-in-difference-in-differences interaction terms, which are not tabulated here for brevity. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

	(1)	(2)	(3)
Dependent variables	CAR(-1, 1)	Takeover Premium	Transaction Value / Total Assets
Treat	0.026**	-0.189***	-0.231
	(1.97)	(-2.94)	(-0.85)
Treat × High Productivity	-0.037**	0.061	-0.768
	(-2.46)	(0.77)	(-1.61)
Treat × Post		0.253***	0.916**
		(2.92)	(2.33)
Treat × Post × High Productivity		-0.161	0.235
		(-1.38)	(0.37)
Joint Significance			
Treat + Treat × High Productivity	-0.011		
	(-0.94)		
Treat × Post +		0.092	1.151**
Treat × Post × High Productivity		(1.15)	(1.97)
Controls	YES	YES	YES
Industry × Year FE	YES	NO	NO
Country × Year FE	YES	NO	NO
Year × Cohort FE	NO	YES	YES
Industry × Cohort FE	NO	YES	YES
Country × Cohort FE	NO	YES	YES
Observations	715	573	573
R-Squared	0.250	0.349	0.355

3.5.3. Role of overinvestment

In the final step, we examine other potential negative effects of anti-takeover regulation. Takeover protection could insulate managers from the disciplining force of the market for corporate control. The more managers are insulated, the more they are encouraged to extract private benefits (Volpin, 2002) and to act against the interests of shareholders through

overinvestment (Jensen, 1986). Hence, we would expect the value of firms prone to overinvestment to decrease around the announcement of the U.K. anti-takeover regulation, because an anti-takeover regulation mitigates disciplining market forces. Increased takeover protection hampers successful takeovers and could thus foster the entrenchment of managers subject to agency conflicts.

To identify firms with potential overinvestment, we follow Biddle et al. (2009) and Cheng et al. (2013). We regress investment on sales growth in each two-digit SIC code industry–year to predict the expected or normal investment. We use the residuals for each firm as a proxy for overinvestment. Since we run these regressions at the industry level, the overinvestment proxy compares a firm's unexplained investment to that of its industry peers. Firms are classified as overinvesting if their residual is larger than twice the standard deviation in the respective industry–year. About 4% of our sample firms meet this criterion. In univariate tests (not tabulated), we find that overinvestment does not differ across the treatment and control groups.

We then examine how the announcement returns of increased takeover protection are related to our overinvestment proxy, using equation (3). The results in Table 3-10 suggest that firms prone to overinvestment have lower values once firms are provided with stronger takeover protection. Across all specifications, treated firms prone to overinvesting experience a statistically significant drop in value of about 9 percentage points relative to treated firms not subject to overinvestment. Overall, firms that overinvest and are affected by the announcements incur a decline in market value of 7 to 11 percentage points. These findings are consistent with the value-decreasing effect of entrenchment when firms are protected from takeovers (Atanassov, 2013; Bertrand & Mullainathan, 2003; Cain et al., 2017; Giroud & Mueller, 2010; Karpoff & Malatesta, 1989).

Table 3-10: Comparison of alternative explanations

This table reports the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the announcements of the U.K. anti-takeover regulation. In this analysis, firm characteristics that can be associated with long-term orientation and overinvestment are interacted with the treatment dummy to measure short-term value effects around the announcements of the U.K. anti-takeover regulation. Panel A uses *R&D* to measure long-term orientation. Panel B uses *Intangibles* and Panel C uses the *Innovative Firm* dummy. All the specifications report only the treatment and interaction terms here for brevity, but control for the respective interacted variable. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: Using R&D investments as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Treat	0.014	0.029**	0.026**	0.014	0.030**
	(1.41)	(2.29)	(1.98)	(1.43)	(2.34)
Treat × High Productivity		-0.031**	-0.037**		-0.032**
		(-2.09)	(-2.48)		(-2.15)
Treat × R&D			-0.002	-0.001	-0.003
			(-0.30)	(-0.17)	(-0.42)
Treat × Overinvestment	-0.106***	-0.095**		-0.108***	-0.097**
	(-2.96)	(-2.35)		(-3.13)	(-2.47)
Joint Significance					
Treat + Treat × High Productivity		-0.002	-0.011		-0.003
		(-0.20)	(-0.96)		(-0.22)
Treat + Treat × R&D			0.024*	0.013	0.026*
			(1.76)	(1.09)	(1.88)
Treat + Treat × Over-investment	-0.092***	-0.066		-0.094***	-0.067*
	(-2.68)	(-1.57)		(-2.84)	(-1.65)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.264	0.276	0.251	0.266	0.278

Panel B: Using intangibles as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Treat	0.014	0.029**	0.022*	0.010	0.025**
	(1.41)	(2.29)	(1.73)	(1.13)	(2.06)
Treat × High Productivity		-0.031**	-0.035**		-0.030**
		(-2.09)	(-2.31)		(-1.98)
Treat × Intangibles			0.014**	0.015**	0.015**
			(2.06)	(2.10)	(2.07)
Treat × Overinvestment	-0.106***	-0.095**		-0.102***	-0.091**
	(-2.96)	(-2.35)		(-3.08)	(-2.46)
Joint Significance					
Treat + Treat × High Productivity		-0.002	-0.013		-0.005
		(-0.20)	(-1.18)		(-0.47)
Treat + Treat × Intangibles			0.037**	0.025**	0.040***
			(2.53)	(2.03)	(2.82)
Treat + Treat × Over-investment	-0.092***	-0.066		-0.092***	-0.066*
	(-2.68)	(-1.57)		(-2.90)	(-1.71)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.264	0.276	0.261	0.276	0.287

Panel C: Using innovative firms as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Treat	0.014	0.029**	0.025*	0.013	0.028**
	(1.41)	(2.29)	(1.77)	(1.24)	(2.11)
Treat × High Productivity		-0.031**	-0.037**		-0.031**
		(-2.09)	(-2.46)		(-2.08)
Treat × Innovative Firm			0.011	0.008	0.009
			(0.53)	(0.42)	(0.47)
Treat × Overinvestment	-0.106***	-0.095**		-0.107***	-0.095***
	(-2.96)	(-2.35)		(-3.03)	(-2.40)
Joint Significance					
Treat + Treat × High Productivity		-0.002	-0.012		-0.003
		(-0.20)	(-1.05)		(-0.28)
Treat + Treat × Innovative Firm			0.035*	0.021	0.037*
			(1.71)	(1.13)	(1.86)
Treat + Treat × Over-investment	-0.092***	-0.066		-0.094***	-0.067
	(-2.68)	(-1.57)		(-2.71)	(-1.60)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.264	0.276	0.251	0.266	0.277

3.5.4. Discussion of the findings

Our findings indicate that while, on average, there appears to be an increase in firm value when the anti-takeover regulation was announced, the overall implications of this increase in firm value are ambiguous. On the one hand, innovative firms benefit from anti-takeover protection, since it incentivizes innovative and long-term–oriented firms to invest more in R&D (Table 3-8). On the other hand, low-productivity firms are one driver of the average positive firm value effect (Table 3-9, Column (1)), since taking over low-productivity firms becomes more costly after implementation of the U.K. anti-takeover regulation (Table 3-9, Column (2)). This latter finding suggests that the anti-takeover regulation results in a potential misallocation of funds across firms.

While our approach does not allow us to draw overall welfare conclusions, we can at least test whether the two effects—encouraging a long-term orientation versus discouraging the takeover of less efficient firms—remain significant when we control for both at the same time, as well as for overinvestment. The results in Table 3-10 suggest that the positive market response of R&D-intense firms in our triple difference approach becomes much weaker once we control for productivity and overinvestment. In fact, we continue to find significant effects only if we use intangible assets as a measure of long-term orientation (see Table 3-10, Panel B). The positive market response of low-productivity firms (indicated by the *Treat* dummy in Columns (2), (3), and (5)) is robust across the three proxies for long-term orientation and robust to the inclusion of the overinvestment proxy. Our results thus indicate that, while there are some benefits to anti-takeover regulation—encouraging long-term innovative projects (Table 3-8, Panel B)—they can create frictions in the capital market that prevent market share from being moved from less to more efficient firms.

3.6. Robustness tests

We test the overall robustness of our results in two ways. First, we address concerns that our results are driven by general market trends. We therefore document that our results are robust to using a random event date in placebo tests (Tables A3 to A7 of Appendix A). Placebo tests mitigate the concern that omitted variables drive our results and enable us to demonstrate that there is no effect when we do not expect one. In our placebo tests, we assume that the respective announcements of the U.K. anti-takeover regulation occurred either 50 trading days prior or 50 trading days after the actual announcements. When rerunning the analyses of the M&A outcome variables and innovation activity, we restrict the placebo tests to a time span before the U.K. anti-takeover regulation became effective and assume that firms were affected by the U.K. anti-takeover regulation two years prior to the respective implementation. We find that most of the more than 50 coefficients of these placebo results are insignificant, five cases are significant in the opposite direction, and one case is significant at the 10% level in the predicted direction. Hence, our results do not appear to be driven by general market trends.

Second, to address concerns that our results are driven by differences between the treatment and control groups, we use entropy balancing (Hainmueller, 2012; Hainmueller & Xu, 2013) to obtain a sample of treated and control firms that are similar in observable characteristics. We follow the matching approach from Table 3-5, Panel C. The results of the matched sample analysis (reported in Tables A8 to A13 of Appendix A) are similar to our main specifications. This finding indicates that our results are not driven by differences in observable characteristics across the treatment and control firms.

3.7. Limitations

Despite our robustness tests, there are, as with most empirical studies, limitations that should be considered when interpreting our results. We would like to acknowledge that we do

not find support for our results in Table 3-5 when using the Kolari-Pynnönen test statistic (Kolari & Pynnönen, 2010). This test statistic takes into account cross-correlation among abnormal returns on single event days but does not allow for a portfolio approach and the aggregated analysis of both event days. The portfolio approach employed in this paper also adjusts for cross-correlation. We use the portfolio approach for two reasons: First, we are interested in the average effect of two uncorrelated events that provide U.K. firms with more takeover protection. The Kolari-Pynnönen test statistic is not suitable and overly conservative for uncorrelated events, as it assumes correlation across events that occur at different times, as in our study. Second, the portfolio approach is a conservative approach with low power according to Kolari and Pynnönen (2010, p. 3997) suggesting that the test statistic of the portfolio approach is downward biased. Hence, this test statistic suffers from underrejection of the null hypothesis.

Another potential limitation of our paper is the choice of our control group. We use a setting in which the control group is also listed in the U.K. to avoid that cross-country differences affect our results. In additional, untabulated analyses, we test whether our matched sample results can be generalized beyond the U.K. setting by matching U.S. and/or European firms to treated U.K. firms via entropy balancing matching. Although these additional tests point in the same direction as the results for matched U.K. firms presented in this paper, they are sometimes not statistically significant at conventional levels. This lack of significance can suggest a limited robustness of our results to using a non-U.K. control group. On the other hand, unobservable characteristics may confound our matching strategy of non-U.K. control firms to treated U.K. firms. First, non-U.K. control firms may have listing and governance requirements that differ from treated U.K. firms. Second, we can no longer match on the LSE listing status (main market or AIM indicator) so that firms from other European "main markets"

can be matched to treated U.K. and AIM listed firms. We carefully note these two limitations when interpreting our results.

3.8. Conclusion

There is an ongoing policy debate about the effects of takeover protection on the takeover environment and on the market values of affected firms. To overcome the identification challenges of prior literature (Cain et al., 2017; Catan & Kahan, 2016; Karpoff & Wittry, 2018), we use the announcement of an anti-takeover regulation in the U.K. as a quasi-natural experiment. We use shareholders' valuation of firms to assess whether and how takeover protection affects firm value. We find that, on average, the market value of firms increases upon announcement of the U.K. anti-takeover regulation. Potential targets' firm values increase while the firm value of potential acquirers decreases because the anti-takeover regulation provides potential targets with more protection. We find fewer deals announced, fewer hostile deals, and higher takeover premiums.

We further find evidence that the positive market response can reflect increased innovative activity, but could also reflect potential distortions in the market for corporate control. On the one hand, we find evidence that long-term–oriented firms benefit. On the other hand, we find that wealth is transferred from high- to low-productivity firms. While these two effects—encouraging long-term orientation and innovation versus discouraging the takeover of less efficient firms—both result in a positive average market response, they have different implications for the evaluation of anti-takeover regulation. While reducing the adverse effects of information asymmetries in innovative firms could be perceived as a desirable outcome of anti-takeover regulation, there are costs to anti-takeover regulation. In addition to potentially fostering managerial entrenchment (Atanassov, 2013; Bertrand & Mullainathan, 2003; Cain et al., 2017; Giroud & Mueller, 2010; Karpoff & Malatesta, 1989), we find that anti-takeover

provisions limit the ability of highly productive firms to acquire less productive firms, thereby distorting the allocation of funds and assets across less efficient and more efficient firms.

4. Are family owners and managers stewards in global crises? Evidence from stock market reactions to Covid-19[17]

4.1. Introduction

Global crises, such as the Covid-19 pandemic (hereafter Covid pandemic), can threaten firm survival and performance (Dowell, Shackell, & Stuart, 2011; Flammer & Ioannou, 2021). Understanding the role of firm ownership and management in dealing with such crises is therefore of crucial importance and has been a central question in prior research (Chrisman, Chua, & Steier, 2011; Madsen & Rodgers, 2015; Miller, Minichilli, & Corbetta, 2013; Minichilli, Brogi, & Calabrò, 2016; Salvato, Sargiacomo, Amore, & Minichilli, 2020). In this regard, existing research emphasizes the (financial) stewardship role attributed to families as firm owners and managers (Le Breton-Miller, Miller, & Lester, 2011; Neckebrouck, Schulze, & Zellweger, 2018). The ability of firms to mitigate the negative consequences during crises grows stronger with families as firm owners or managers because of their strong identification with the firm, their long-term orientation and their alignment with stakeholder goals (Amore, Quarato, & Pelucco, 2021; Miller et al., 2013; Neckebrouck et al., 2018).

However, this stewardship role of families as firm owners and managers has rarely been analyzed explicitly, particularly in the context of a global crisis. In fact, we know little about which form of family involvement really matters and how it affects firm value. From a theoretical perspective, all forms of family involvement can be associated with stewardship behavior and thus an increase of firm resilience. Irrespective of how the family is involved, they may use their networks and strong links to stakeholders as well as their high motivation to attenuate negative financial consequences for the firm. Nevertheless, different ways and roles of family involvement exist and it might indeed make a difference whether a family is

[17] This chapter is closely based on Block and Ulrich (2021).

involved as an owner and/or a manager. Families in their role as owners may support the firm through the provision of additional financial capital or forgoing dividends while families who act as managers may use their experience, social capital and strong goal alignment with shareholders to help the firm through the crisis. Ultimately, it is an open and empirical question which form of family involvement matters in a situation of a global crisis.

To study the stewardship behavior of the different forms of family involvement, we make use of the Covid pandemic as an example of a global crisis. We focus on financial stewardship and analyze the daily stock market reactions of a sample of German-listed firms to negative Covid-related events. In our event study, we measure how these stock returns are influenced by different forms of family involvement. The negative Covid-related events comprise virus propagation, economy disturbance, and the political restriction of events during the first wave of the Covid outbreak. Our results show that only family management has an effect and reduces the negative returns; family ownership was found to have no effect.

With these results, our study contributes to research on the stewardship role of families during crises (Dyer, Nenque, & Hill, 2014; Lange, Boivie, & Westphal, 2015; Miller et al., 2013) as well as to research on the valuation consequences of financial stewardship (Miller et al., 2013; Neckebrouck et al., 2018). It also contributes to corporate governance research about (global) crises (Bae, Baek, Kang, & Liu, 2012; Baek, Kang, & Suh Park, 2004; Conyon, Judge, & Useem, 2011; Crespí & Martín-Oliver, 2015; Dowell et al., 2011; Johnson, Boone, Breach, & Friedman, 2000; Lemmon & Lins, 2003) by illustrating that it is the management and not the ownership dimension that is crucial in the context of crisis management. Finally, we contribute to recent research on the Covid pandemic and its impact on financial markets (Amore et al., 2021; Ding, Levine, Lin, & Xie, 2021; Fahlenbrach, Rageth, & Stulz, 2021; Ramelli & Wagner, 2020) by showing that family-managed firms exhibit less negative returns than other firms. In contrast to these studies, we do not limit our analyses to virus propagation

events, but also study economic and political events, for which our sample firms exhibit even stronger stock market reactions.

4.2. Theory and hypotheses

In this section, we draw from the literature on (financial) stewardship (Davis, Schoorman, & Donaldson, 1997; Neckebrouck et al., 2018) and the literature on family involvement (Miller et al., 2013), to derive hypotheses about the effects of family management and ownership on financial stewardship and firm value in the context of a global crisis. We focus on the financial consequences of stewardship, hence the term 'financial stewardship'.

Stewardship theory assumes that stewards are intrinsically motivated to behave according to their principals' interests (Davis et al., 1997; Donaldson & Davis, 1991). In an organizational context, stewards act pro-organizational and put more emphasis on higher level needs, altruistic motives, social contributions, loyalty, and generosity than on individualistic and opportunistic, self-serving behavior (Davis et al., 1997; Eddleston & Kellermanns, 2007; Zahra, 2003). Even with non-aligned interests, stewards focus on the cooperation with principals instead of deviating behavior (Corbetta & Salvato, 2004; Davis et al., 1997; Kellermanns & Eddleston, 2004). Stewards are collectivistic and make decisions that benefit the organization as a whole (Le Breton-Miller & Miller, 2009; Miller et al., 2008). They aim to satisfy the needs of the stakeholders of the organization and their own utility in line with the needs of the organization as a whole. Due to their strong identification with the organization, stewards pursue organizational goals even when they are costly for them personally (Davis et al., 1997). Stewardship behavior reduces the principal's monitoring costs creating value for organizations (Combs, Penney, Crook, & Short, 2010; Davis et al., 1997).

4.2.1. Family ownership and financial stewardship in the Covid pandemic

Family owners view crises as a severe threat to their wealth and their altruistic commitments to society (Madison, Holt, Kellermanns, & Ranft, 2016; Pearson & Marler, 2010). The Covid-19 pandemic poses such a crisis for family owners' wealth as not only the survival of their firms is at risk but also their reputation as owners. To deal with this threat, stewardship theory proposes that family owners become more risk-seeking during a crisis to seize opportunities for recovery and to improve short-term financial results (Le Breton-Miller et al., 2011). Moreover, stewardship theory argues that the interests and goals of family owners and other stakeholders are closely aligned and mutually dependent (Davis et al., 1997; Eddleston, Kellermanns, & Sarathy, 2008; Zahra, 2003; Zahra, Hayton, Neubaum, Dibrell, & Craig, 2008). It is the strong long-term and dynastic orientation of families as owners that distinguishes them from other types of firm owners. This long-term and dynastic orientation manifests itself not only in the provision of additional financial capital or forgoing dividends (Neckebrouck, Manigart, & Meuleman, 2017) but also in social capital towards important stakeholder groups such as banks and suppliers. Shared goals with stakeholders allow steward owners to credibly build and maintain relationships to other stakeholders based on mutual trust and loyalty for the long run (Carr, Cole, Ring, & Blettner, 2011; Chrisman, Chua, & Kellermanns, 2009). During a crisis, these strong relationships and the trustworthiness of family owners can be a competitive advantage for the family-owned firm (Barney & Hansen, 1994; Chrisman et al., 2009; Faccio & Parsley, 2009; Salvato et al., 2020; Sirmon & Hitt, 2003) and result in better borrower-lender relationships (D'Aurizio, Oliviero, & Romano, 2015) or help to overcome supplier shortages. The pursuit of long-term orientation and business continuity decreases the likelihood of opportunistic actions. Stakeholders anticipate this stewardship behavior and give family-owned firms access to badly needed resources in a situation of a global crisis. The following hypothesis should apply:

Hypothesis 1: Family ownership reduces the negative stock market reactions to the Covid pandemic.

4.2.2. Family management and financial stewardship in the Covid pandemic

Family managers and family owners typically share the same goals because family managers are a part of the family themselves. The principal-agent conflict between firm owners and managers is reduced as compared to a non-family manager (Ang, Cole, & Lin, 2000). Apart from this agency argument for goal alignment, a stewardship argument exists. Family managers tend to identify strongly with the firm as it is part of their own family and personal identity (Corbetta & Salvato, 2004; Zellweger, Eddleston, & Kellermanns, 2010). Hence, they are deeply committed to navigating the firm successfully through the crisis. Their personal identity is at stake. This stewardship position has allowed them to build strong social capital in particular with the employees of the firm. Prior research shows that family managers provide employees with opportunities for personal growth, self-actualization, and job security (Bassanini, Breda, Caroli, & Rebérioux, 2013; Block, 2010; Sraer & Thesmar, 2007). In a crisis, family managers can then rely on their employees' commitment (Corbetta & Salvato, 2004; Davis et al., 1997), proactivity (Miller et al., 2008) and productivity (Barth, Gulbrandsen, & Schønea, 2005; Sraer & Thesmar, 2007), resulting in a competitive advantage (Knott, Bryce, & Posen, 2003; Nahapiet & Ghoshal, 1998). Recent research by Amore et al. (2021) suggests that family managers can indeed benefit from their strong stakeholder ties during a crisis because these ties allow them to make the necessary organizational adjustments to overcome challenges.

Another argument concerns the job security and long tenure of the family managers themselves (Davis et al., 1997; Le Breton-Miller et al., 2011), providing them with a high

tolerance towards temporary uncertainties (Davis et al., 1997; Le Breton-Miller et al., 2011). Especially during a crisis, risk and uncertainty tolerance are beneficial for crisis management and can result in a competitive advantage vis-à-vis other firms (Le Breton-Miller et al., 2011; Miller et al., 2013). Moreover, their long tenure provides them with high experience and deep knowledge about the firm which contributes to their decision-making competence, again representing a competitive advantage vis-à-vis other managers (Henderson, Miller, & Hambrick, 2006; Miller et al., 2013; Miller & Shamsie, 2001; Minichilli, Corbetta, & MacMillan, 2010). Finally, as family managers are embedded in an organizational structure that empowers them with a great decision-making latitude and power (Kets de Vries, 1993; Miller et al., 2013), they are not only willing to take the necessary decisions to survive the crisis but are also able to do so. This can make the difference in a situation such as the Covid pandemic, where decisions have to be taken quickly and under high outcome uncertainty. The following hypothesis should hold:

Hypothesis 2: Family management reduces the negative stock market reactions to the Covid pandemic.

4.3. Data and method

4.3.1. Context of the study

We examine our hypotheses in the empirical context of the Covid pandemic, which hit Germany relatively early (Robert Koch-Institut, 2021). The first case was detected as early as 27 January (Spiegel, 2020), only 15 days after the World Health Organization confirmed that a novel coronavirus had caused respiratory illnesses in China (World Health Organization, 2020). In the following weeks, several clusters of Covid infections were detected across several German regions leading to curfews, border closures to neighboring countries, and the shutdown

of whole industries (Tagesschau, 2020a). Economically, the severe responses to the Covid pandemic induced a collapse in exports, supply chain disruptions, and let to an economic crisis (Federal Ministry of Finance, 2020; Handelsblatt, 2020; Tagesschau, 2020a). By the end of the first quarter of 2020, almost half a million firms in Germany used "Kurzarbeit", a government-subsidized short-time working program (Hamburger Abendblatt, 2020), for roughly 10 million employees (Tagesschau, 2020b). Despite this program, during the following weeks the unemployment rate in Germany increased by 25% on a year-to-year basis (Deutsche Welle, 2020) and exports from German firms dropped by 31% compared to the previous year, an unprecedented decrease since 1950 (Süddeutsche Zeitung, 2020). Germany's gross domestic product decreased by 9.7% during the second quarter as compared to the first quarter of 2020 and by 11.3% on a year-to-year basis (Federal Statistical Office of Germany, 2020). Compared to the 2019 levels, the gross domestic product of Germany in 2020 decreased by 5.0 percentage points (Federal Statistical Office of Germany, 2021).

4.3.2. Sample and data sources

We gather a sample of Covid-related announcements pertaining to virus expansion, economy disturbance, and political restrictions during the first wave of the Covid outbreak in Germany. We obtain this information from tagesschau.de, the main German national and international news service, and from the Handelsblatt, a German-language business newspaper. We extend this dataset with data on the Covid propagation from the Robert Koch Institute, which monitors the virus propagation in Germany, and with data from the Johns Hopkins Coronavirus Resource Center, which monitors the worldwide Covid-19 propagation.

Our sample of firms comprise firms from the German composite stock market index (CDAX) as of December 31, 2019. For these firms, we collect accounting, subsidiary, ownership, and management data from Bureau van Dijk's Amadeus database as well as stock

price data from Bloomberg. We supplement these data with information on family involvement as owners, managers, or chairmen.

These data and news sources enable us to ascertain when a Covid-related event occurs and which firms are affected by that event. We use the data from the Robert Koch Institute to identify which firms in which German districts and federal states are affected by news pertaining to the virus propagation. We assume that firms with headquarters, legal incorporation, or subsidiaries in a district or state in which a Covid event occurs are more affected than firms in other German districts or states. The same rationale applies to the use of the Johns Hopkins University data in the international context. For example, the more exposure a firm has to a country, e.g., due to subsidiaries in the country that is affected by a Covid-outbreak, the more likely this firm is also affected. With this approach, we exploit the heterogeneity of our sample firms in terms of geographic diversification.

We exclude financial firms (Standard Industrial Classification, or SIC, codes 6000-6999) and missing information for total assets or other firm-level control variables. Moreover, we do not consider firms for which we cannot calculate abnormal returns because these firms are thinly traded and exhibit more than 40% missing returns or more than 60% returns of zero within 2019, our estimation window. This leaves us with a sample of 300 firms and altogether 32,110 firm-day observations for our sample period ranging from January to end of August 2020.

4.3.3. Variables and summary statistics

Table 4-1 provides a detailed description of our dependent and independent variables.

Dependent variable. The purpose of our research is to assess the stock market reactions to negative Covid-related events for firms with different forms of family involvement and accordingly financial stewardship. We therefore rely on daily abnormal returns as our

dependent variable. We calculate abnormal returns for all event days and sample firms. With this approach, we follow a large body of literature that uses daily stock market information, e.g., abnormal returns, as the dependent variable to study short-term reactions (Hail, Muhn, & Oesch, 2021; Larcker, Ormazabal, & Taylor, 2011).

Independent variables. We control for three Covid-related announcement types. The virus expansion dummy applies to news about the evolvement of infections. For example, we classify the first virus case in a country as a negative event. On a district level, we classify the passage of a 7-day virus incidence of 35 or 50 (dependent on the state threshold) as a negative event. The 7-day incidence is defined as the number of confirmed Covid-19 cases within the last seven days per 100,000 population. In addition, a negative virus event occurs, when the daily growth rate of cases exceeds 200%. Our economy disturbance dummy applies to negative news about the economy, e.g., supply chain disruptions or worse order situations. The political restriction dummy is used for instances when political restrictions impede businesses, e.g., due to border closures or production stops.

We include a number of standard firm-level control variables that, according to the literature, can have an effect on stock market reactions (Andres et al., 2019; Ding et al., 2021; Giroud & Mueller, 2010). The controls are: R&D (R&D expenses/lagged total assets), capital expenditures (capital expenditures/lagged total assets), cash holdings (cash & equivalents/lagged total assets), one-year sales growth, leverage (total debt/lagged total assets), firm size (natural logarithm of total assets), return on assets (earnings before interest, taxes and depreciation, amortization/lagged total assets), and firm age (natural logarithm of years since initial public offering).

Furthermore, we include variables capturing different forms of family involvement. The family ownership dummy characterizes firms in which a founding-family blockholder owns more than 25% of the voting shares of the firm. The family management dummy refers

to firms where the founding-family is on the executive board and the family chairman dummy refers to firms where the founding-family provides the chairman of the supervisory board.

Table 4-2 presents summary statistics for our sample firms. In this table, we present the mean and the standard deviation for all of our employed variables. For our continuous variables, we additionally depict the 25th, 50th, and 75th percentiles. Since our Covid-related event dummy variables in Panel B may overlap, the sum of the three variables does not add up to one. Table 4-3 illustrates the correlations for all of our employed variables.

Table 4-1: Description of Variables

This table defines the main variables. All continuous variables are winsorized at 1% and 99%.

Outcome variable	Daily abnormal returns	The daily abnormal return for each firm in the sample period (January to August 2020) is calculated from the European Fama-French-Carhart six-factor model This model is directed at capturing the market, size, value, profitability, investment, and momentum patterns in average stock returns. The six-factor model is estimated over all trading days in the year 2019.
Covid-19 related event variables (negative exogenous shocks)	Virus expansion	Dummy variable that equals 1 for news related to the expansion of the virus and 0 otherwise. The virus expansion dummy comprises among others negative news about the evolvement of infections around the world.
	Economy disturbance	Dummy variable that equals 1 for news related to the global economy and 0 otherwise. The economy disturbance dummy comprises among others negative news about consumer confidence, order situation in firms/industries, production stops, or job markets.
	Political restrictions	Dummy variable that equals 1 for news related to political restrictions and 0 otherwise. The political restriction dummy comprises among others negative news about political restrictions so that businesses have to stop/cut production or restrictions with regards to inter-country trade such as border closures.
Firm-level control variables	Family chairman dummy	Dummy variable that equals 1 if the founding-family provides the chairman of the supervisory board, and 0 otherwise.
	R&D	R&D expenses/lagged total assets.
	Capex	Capital Expenditures/lagged total assets
	Cash & equivalents	Cash & Equivalents/lagged total assets.
	Sales growth	One-year growth in sales.
	Leverage	Total debt/lagged total assets
	Ln(Assets)	Natural logarithm of total assets.
	Return on assets	EBITDA/lagged total assets.
	Ln(Firm age)	Natural logarithm of years since Initial Public Offering (IPO).
Family involvement variables	Family ownership dummy	Dummy variable that equals 1 if a firm has a founding-family blockholder that owns more than 25% of the voting shares of the firm, and 0 otherwise. Family blockholders comprise the founder(s) and their family members.
	Family management dummy	Dummy variable that equals 1 if the founding-family is represented on the executive board, and 0 otherwise.

Table 4-2: Summary statistics

This table presents descriptive statistics for the sample firms. Panel A provides summary statistics for the outcome variable, Panel B presents summary statistics for the Covid-19 related event variables as described in Table 4-1, Panel C provides summary statistics for the firm-level control variables, Panel D for the family involvement variables, and Panel E presents an overview about the sample. Column (1) reports the mean and Column (2) the standard deviation for each variable. Column (3) reports the 25th percentile, column (4) the 50th percentile (median), and column (5) the 75th percentile for each continuous variable. For variable definitions please refer to Table 4-1.

	(1)	(2)	(3)	(4)	(5)
Variables	Mean	Standard deviation	P25	P50 (median)	P75
Panel A: Outcome variable					
Daily abnormal returns	-0.001	0.041	-0.018	-0.001	0.015
Panel B: Covid-19 related event variables (negative exogenous shocks)					
Virus expansion	0.771	0.420			
Economy disturbance	0.274	0.446			
Political restrictions	0.150	0.357			
Panel C: Firm-level control variables					
Family chairman dummy	0.131	0.338			
R&D	0.038	0.071	0	0.006	0.049
Capex	0.043	0.040	0.017	0.033	0.057
Cash & Equivalents	0.221	0.290	0.069	0.134	0.243
Sales Growth	0.073	0.274	-0.012	0.044	0.136
Leverage	0.231	0.304	0.041	0.176	0.318
Ln(Assets)	13.28	2.52	11.69	13.06	14.91
Return on Assets	0.091	0.171	0.057	0.110	0.158
Ln(Firm Age)	2.71	0.81	2.48	3.00	3.09
Panel D: Family involvement variables					
Family ownership dummy	0.282	0.450			
Family management dummy	0.188	0.391			
Panel E: Sample overview					
Number of observations	32,110				
Number of firms	300				

Table 4-3: Correlation matrix

This table reports the correlations for the dependent and independent variables. Italicized correlations are statistically significant at $p < .05$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
(1) Daily abnormal returns	1														
(2) Family ownership dummy	0.01	1													
(3) Family management dummy	0.02	*0.36*	1												
(4) Family chairman dummy	0.00	*0.32*	*0.02*	1											
(5) Political restrictions	*-0.02*	*0.06*	*0.05*	*0.04*	1										
(6) Economy disturbance	-0.01	*0.09*	*0.07*	*0.05*	*-0.04*	1									
(7) Virus expansion	-0.01	*-0.05*	*-0.04*	-0.01	*-0.23*	*-0.58*	1								
(8) Ln(Assets)	0.01	*0.07*	*-0.15*	*0.04*	0.00	0.00	0.00	1							
(9) Ln(Firm age)	0.00	*0.10*	*-0.09*	0.01	-0.01	0.00	0.00	*0.11*	1						
(10) R&D	0.00	0.01	*-0.06*	*0.04*	0.01	0.01	-0.01	*-0.21*	*0.10*	1					
(11) Capex	0.00	*0.11*	*0.05*	*0.10*	0.01	0.01	0.00	*0.08*	*-0.04*	*-0.07*	1				
(12) Cash & equivalents	0.00	*-0.05*	*0.11*	*-0.04*	0.01	0.00	-0.01	*-0.25*	*-0.28*	*0.15*	*0.07*	1			
(13) Leverage	0.00	*0.11*	*0.04*	-0.02	0.01	0.01	-0.01	0.00	*-0.09*	0.02	*0.13*	*0.11*	1		
(14) Sales growth	0.00	-0.01	*0.03*	*0.11*	0.01	0.01	0.00	*0.02*	*-0.07*	*0.05*	*-0.06*	*0.20*	*0.10*	1	
(15) Return on assets	0.01	*0.16*	*0.09*	*0.12*	0.01	0.01	0.01	*0.25*	*0.10*	*-0.30*	*0.17*	*-0.18*	*0.04*	*0.09*	1

4.3.4. Empirical strategy

In our empirical tests, we study the reactions of German CDAX firms to Covid-related events. Our identification strategy relies on the variation in treatment to Covid-related events across our sample firms and across time. This variation in treatment is caused by German regions being affected by Covid to varying degrees and at different times (Robert Koch-Institut, 2021), providing important variation for our identification strategy. Additionally, Germany's federal system and its different and varying responses to Covid has contributed to many Covid-related events that affected different firms at different times. Furthermore, varying degrees of the engagement of German firms in global regions ensures valuable variation in treatment status to Covid-related events across firms and time.

To capture the short-term effects of negative Covid-related events on firms with different forms of family involvement, we apply event study methodology and measure daily abnormal returns to all of our sample firms on each trading day from January to August 2020. We estimate daily abnormal stock returns using the Fama-French-Carhart six-factor model for Europe. The model includes five factors proposed by Fama and French (2016) as well as the momentum factor as suggested by Carhart (1997). The factors correct the daily returns for market, size, value, profitability, investment, and momentum patterns so that none of these patterns can be a driver for abnormal returns. We estimate the parameters of the six-factor model for each firm in our sample over all trading days in 2019. We choose all trading days of 2019 (and none of 2020) as estimation window to avoid influences from Covid-related events on the estimation of the parameters. Daily abnormal returns during the sample period are calculated by subtracting the expected return implied by the six-factor model from the realized return. Our event study method is consistent with standard event study methodology (Brown & Warner, 1980; Kolari & Pynnönen, 2010).

Since Covid-related events simultaneously have an impact on several of our sample firms, these firms share the same event period and hence exhibit some degree of cross-sectional correlation in abnormal returns which biases conventional test statistics. Thus, we test for statistical significance using the t-statistic of Boehmer, Masumeci, and Poulsen (1991) and adjust it with the Kolari and Pynnönen (2010) adjustment factor so that the test statistics account for cross-sectional correlation in abnormal returns across sample firms. This is the state-of-the-art technique to account for cross-sectional correlation (Kolari & Pynnönen, 2010).

To investigate the cross-sectional determinants of the market reaction to Covid-related events, we regress the daily abnormal returns on the forms of family involvement proxies and on additional firm characteristics. The cross-sectional regressions allow us to condition out all time-varying and time-invariant trading day characteristics as well as industry and firm characteristics that go beyond the patterns identified by Fama and French (2016) and may influence daily stock price reactions to Covid-related events. Due to cross-sectional correlation of the daily abnormal returns, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992) to adjust our cross-sectional tests for cross-sectional correlation. We follow Fernando, May, and Megginson (2012) in the application of this approach and thereby ensure that cross-sectional correlation does not bias our regression results.

4.4. Event study and regression results

4.4.1. Family involvement and stock market reactions to negative Covid-related events

Table 4-4 explores how firms with different forms of family involvement react to negative Covid-related events. Firms with family ownership exhibit a negative but insignificant average abnormal return of -0.09 percentage points during negative Covid-related events. In contrast, firms with no family involvement exhibit a statistically significantly negative average abnormal return of -0.23 percentage points (p=.003). The difference in means for family-owned

vis-à-vis firms with no family involvement is 0.14 percentage points and statistically significant (p=.012). In line with hypothesis 1, this result suggests that family-owned firms exhibit less negative stock market reactions to negative Covid-related events. We obtain similar results for family management. Firms with family management do not statistically significantly react to negative Covid-related events, while firms with no family involvement experience negative and statistically significant average abnormal returns. Consistent with hypothesis 2, family-managed firms exhibit statistically and economically significantly less negative stock market reactions to negative shocks during the Covid pandemic as compared to firms with no family involvement (0.20 percentage points, p=.003).

Table 4-4: The stock price reactions of firms with family involvement to Covid-related events

This table reports the mean abnormal returns to Covid-related announcements for different forms of family involvement, e.g., family ownership and management, vis-à-vis firms with no family involvement. For mean abnormal returns, t-statistics are computed with Boehmer, Masumeci, and Poulsen's (1991) standardized cross-sectional method and adjusted for cross-sectional correlation (Kolari and Pynnönen 2010). The t-statistics for the differences in means assume unequal variances across the respective two samples and are computed with the cross-sectional variances of abnormal returns in two-tailed tests.

	Family involvement		No family involvement		Difference in Means	
Family involvement variables	(1) Mean AR	t-stat (p value)	(2) Mean AR	t-stat (p value)	(1)-(2) Mean AR	t-stat (p value)
Family ownership	-0.09%	-1.43 (p = .154)	-0.23%	-3.00 (p = .003)	0.14%	2.51 (p = .012)
Family management	-0.03%	-0.42 (p = .673)	-0.23%	-3.00 (p = .003)	0.20%	3.00 (p = .003)

Since the reactions of firms to negative Covid-related events are likely to be influenced by firm and industry characteristics, we explore the stock market reactions to different forms of family involvement in a multivariate setting. In Table 4-5, Models 1 and 2, we confirm our event study results in cross-sectional analyses, in which we regress our dependent variable daily abnormal returns on event-specific and firm-level controls (as defined in Table 4-1) as well as on day and industry fixed effects. Throughout the different models of Table 4-5, the constant is statistically and economically significantly negative, indicating that the Covid

pandemic negatively affects firms with no family involvement. Consistent with hypothesis 1, Model 1 documents that family-owned firms exhibit by 0.11 percentage points less negative abnormal returns than firms with no family involvement. This difference is statistically significant (p =.081) and economically meaningful. In line with hypothesis 2, in Model 2, family-managed firms experience by 0.36 percentage points less negative abnormal returns vis-à-vis firms with no family involvement. This result is statistically and economically significant (p=.000).

Due to the coexistence of family ownership and management in some of our sample firms, as documented in the correlation matrix of Table 4-3, our event study results could be spurious. This could mean that the less negative stock market reaction attributed to family ownership might be driven by family management. As our univariate event study analysis is unable to detect spurious correlations, we investigate this issue in our multivariate analyses in Table 4-5, Models 3 to 8. We include all of our form of family involvement variables simultaneously. In our baseline model (Model 3), we do not control for other firm- or industry-specific characteristics and find that family-owned and -managed firms experience statistically significantly less negative abnormal returns. In further analyses (Models 4 to 8), we show that family-owned firms do not exhibit abnormal returns that are statistically significantly different from the returns of non-family firms after simultaneously controlling for the forms of family involvement. Family-managed firms, however, experience statistically significantly less negative abnormal returns (p<=.027), which is consistent with hypothesis 2. The effect of family management is also economically meaningful and indicates higher stock market returns vis-à-vis non-family firms of between 0.17 to 0.35 percentage points. In addition, family-managed firms exhibit statistically significantly less negative abnormal returns than family-owned firms. In model 7, for example, the abnormal returns of family-managed firms are by 0.30 percentage points higher than for family-owned firms. This return difference is

statistically significant (p=.038) and suggests that family management provides firms with relatively better short-term stock market returns during a crisis. In line with stewardship theory, our results suggest that different forms of family involvement during crises focus on different duties and hence lead to variations in financial stewardship behavior. These variations in financial stewardship result in divergent stock market reactions to family owners and managers. The results of Table 4-5 alleviate our positive view of family ownership and suggest that (1) the family ownership effect in Tables 4-4 and 4-5, Model 1 is spurious and actually driven by family management and that (2) literature solely studying family ownership may suffer from misleading interpretations due to spurious correlations between family ownership and management.

Table 4-5: Cross-sectional analysis of family involvement and firms' stock price reactions to Covid-related events

This table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement pertaining to family ownership and management. Event-specific control variables comprise dummy variables indicating virus expansion, economy disturbance, and political restriction events. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	-0.51	-0.69	-0.25	-0.23	-0.24	-0.60	-0.66	-0.65
	(-1.76)	(-2.26)	(-6.76)	(-6.39)	(-6.05)	(-2.48)	(-2.29)	(-2.24)
	(p = .079)	(p = .025)	(p = .000)	(p = .000)	(p = .000)	(p = .014)	(p = .023)	(p = .026)
Family ownership dummy	0.11		0.11	0.08	0.07	0.04	0.04	0.06
	(1.75)		(1.67)	(1.14)	(1.15)	(0.57)	(0.61)	(0.86)
	(p = .081)		(p = .095)	(p = .256)	(p = .253)	(p = .566)	(p = .540)	(p = .389)
Family management dummy		0.36	0.20	0.17	0.17	0.24	0.34	0.35
		(3.89)	(2.76)	(2.23)	(2.24)	(2.90)	(3.40)	(3.20)
		(p = .000)	(p = .006)	(p = .027)	(p = .026)	(p = .004)	(p = .001)	(p = .002)
Event-specific controls	YES	YES	NO	NO	YES	YES	YES	YES
Firm-level controls	YES	YES	NO	NO	NO	YES	YES	YES
Day FE	YES	YES	NO	YES	YES	YES	YES	NO
Industry FE	YES	YES	NO	NO	NO	NO	YES	NO
Industry-Day FE	NO	NO	NO	NO	NO	NO	NO	YES
Observations	32,110	32,110	32,110	32,110	32,110	32,110	32,110	32,110
R-Squared	0.176	0.177	0.001	0.173	0.174	0.174	0.177	0.343

4.4.2. Distinguishing between health, economic, and political Covid-related events

In Table 4-6, we investigate the reactions of firms with different forms of family involvement to specific negative Covid-related events, e.g., to virus expansion announcements, economy disturbance events, and announcements about political restrictions during the Covid pandemic. In Panel A of Table 4-6, our event study results reveal less negative abnormal returns of family-owned vis-à-vis non-family firms to economy disturbances and political restrictions. The difference in abnormal returns is statistically and economically significant. On average, family-owned firms experience by 0.56 percentage point less negative abnormal returns to economy disturbance announcements than non-family firms (p=.000) and by 0.85 percentage point less negative abnormal returns to announcements of political restrictions (p=.000). These statistically significant results are in line with hypothesis 1. In Panel B, family-managed firms exhibit less negative abnormal returns to all three Covid-related events, indicating by between 0.19 to 1.12 percentage points (p<=0.015) higher stock market returns vis-à-vis non-family firms. These results are consistent with hypothesis 2. The results of Table 4-6 provide evidence that the mere focus of prior literature on news about the actual virus propagation neglects other economically more important events that more positively contribute to the relative stock market returns of firms with different forms of family involvement.

Table 4-6: The stock price reactions of firms with family involvement to specific Covid-related events

This table reports the mean abnormal returns to Covid-related announcements for firms with family ownership or management vis-à-vis firms with no family involvement. Panel A compares abnormal returns for family owned firms (with at least 25% family ownership) vis-à-vis firms with no family involvement. Panel B compares abnormal returns for family management vis-à-vis firms with no family involvement. For mean abnormal returns, t-statistics are computed with Boehmer, Masumeci, and Poulsen's (1991) standardized cross-sectional method and adjusted for cross-sectional correlation (Kolari and Pynnönen 2010). The t-statistics for the differences in means assume unequal variances across the respective two samples and are computed with the cross-sectional variances of abnormal returns in two-tailed tests.

Panel A: Family ownership vs. no family involvement

	Family ownership		No family involvement		Difference in Means	
Events	(1) Mean AR	t-stat (p value)	(2) Mean AR	t-stat (p value)	(1)-(2) Mean AR	t-stat (p value)
Virus expansion	-0.14%	-1.97 (p = .049)	-0.23%	-3.01 (p = .003)	0.09%	1.44 (p = .150)
Economy disturbance	0.08%	0.31 (p = .759)	-0.48%	-2.46 (p = .014)	0.56%	5.35 (p = .000)
Political restrictions	0.05%	0.39 (p = .697)	-0.80%	-2.22 (p = .026)	0.85%	4.33 (p = .000)

Panel B: Family management vs. no family involvement

	Family management		No family involvement		Difference in Means	
Events	(1) Mean AR	t-stat (p value)	(2) Mean AR	t-stat (p value)	(1)-(2) Mean AR	t-stat (p value)
Virus expansion	-0.04%	-0.68 (p = .499)	-0.23%	-3.01 (p = .003)	0.19%	2.47 (p = .014)
Economy disturbance	0.14%	0.77 (p = .442)	-0.48%	-2.46 (p = .014)	0.62%	4.87 (p = .000)
Political restrictions	0.32%	1.40 (p = .161)	-0.80%	-2.22 (p = .026)	1.12%	4.81 (p = .000)

To further explore the effects of forms of family involvement on financial stewardship behavior and stock market reactions to Covid-related events, we study all of our family involvement measures in multivariate regressions. In Table 4-7, we document statistically significant and less negative reactions of family-managed firms to virus expansion, economy disturbance, and political restriction events. These reactions are also economically meaningful and, on average, suggest by 0.22 percentage points less negative abnormal returns to virus expansion announcements (Model 3, p=.031), by 0.50 percentage points less negative abnormal returns to economy disturbance events (Model 6, p =.000), and by 0.67 percentage

points less negative abnormal returns to political restrictions (Model 9, p=.002). These results are consistent with hypothesis 2 and thus suggest that family-managed firms are less negatively affected by negative Covid-related events than non-family firms, which are subsumed in the constant. In contrast to the results of Table 4-6, family-owned firms do not exhibit different returns than non-family firms. We attribute this no-result for family-owned firms to spurious correlations with the family management variable that is the driver of the significant event study results for our family involvement proxies in Table 4-6. When we conduct a t-test for the individual differences of our forms of family involvement coefficients, we find that family-managed firms consistently achieve statistically and economically significantly higher abnormal returns than family-owned firms ($p<.05$). This result is consistent with family managers acting as financial stewards. However, we fail to find evidence that family owners also are financial stewards. Importantly, our results also show that family ownership is not a disadvantage relative to firms with no family involvement during a crisis, as the stock market reactions do not statistically significantly differ from non-family firms. Overall, the findings of Table 4-7 corroborate our conjecture that not only virus expansion events need to be analyzed, but that non-health-related events exhibit stronger stock market reactions during the Covid pandemic. For example, we find that firms with family managers exhibit by 0.28 percentage point higher abnormal returns during economic disturbances (Model 6-3) and by 0.45 percentage point higher abnormal returns during political restriction announcements vis-à-vis virus expansion announcements (Model 9-3). The t-test for the differences of coefficients is statistically significant ($p <.02$) and economically meaningful for both differences. We therefore conclude that these non-health-related events are important and thus deserve more attention in the literature.

Table 4-7: Cross-sectional analysis of family involvement and firms' stock price reactions to specific Covid-related events

This table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement pertaining to family ownership and management. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Event	Virus expansion			Economy disturbance			Political restrictions		
Constant	-0.20	-0.47	-0.56	-0.22	-0.60	-0.71	-0.41	-1.24	-1.10
	(-5.62)	(-1.83)	(-1.76)	(-4.61)	(-2.20)	(-2.52)	(-3.81)	(-1.85)	(-1.47)
	(p = .000)	(p = .069)	(p = .079)	(p = .000)	(p = .029)	(p = .012)	(p = .000)	(p = .065)	(p = .141)
Family ownership dummy	0.02	-0.02	0.00	0.17	0.11	0.11	0.19	0.15	0.09
	(0.22)	(-0.22)	(0.03)	(1.99)	(1.33)	(1.31)	(1.42)	(1.07)	(0.57)
	(p = .830)	(p = .829)	(p = .977)	(p = .047)	(p = .184)	(p = .190)	(p = .156)	(p = .286)	(p = .567)
Family management dummy	0.11	0.18	0.22	0.19	0.25	0.50	0.42	0.45	0.67
	(1.47)	(2.27)	(2.17)	(1.58)	(1.99)	(3.73)	(2.86)	(2.71)	(3.20)
	(p = .143)	(p = .024)	(p = .031)	(p = .115)	(p = .047)	(p = .000)	(p = .005)	(p = .007)	(p = .002)
Firm-level controls	NO	YES	YES	NO	YES	YES	NO	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	20,720	20,720	20,720	7,363	7,363	7,363	4,027	4,027	4,027
R-Squared	0.158	0.159	0.161	0.181	0.183	0.190	0.216	0.217	0.231

4.4.3. Robustness tests and further analyses

To mitigate concerns of alternative explanations for our results we test the robustness of our results in several ways. First, to address concerns that general market trends (e.g., omitted variables) may drive our results, we conduct placebo tests (Tables B1 and B2 of Appendix B). In our placebo tests, we randomly assign treatment to our Covid-related announcements among our sample observations. Our placebo tests demonstrate that there is no significant effect when we do not expect any effect. Hence, the results of the placebo tests mitigate the propensity that omitted variables drive our results. Second, we address concerns that differences between family firms and non-family firms may drive our results. We employ a 1:1 matching approach and match each family firm to one non-family firm based on firm size, leverage, sales growth, profitability, and cash holdings. This approach ensures that both groups of firms are similar with regard to these observable control variables: Our matched sample results are similar to our main specifications, indicating that differences across family and non-family firms do not drive our results (Tables B3 and B4 of Appendix B). Third, we address concerns that our results might be subject to the choice of the Fama-French-Carhart six-factor model. Therefore, we rerun our event study analyses with the following return models: Fama-French three factor (Fama & French, 1993), Fama-French-Carhart four factor (with momentum), Fama-French five factor (Fama & French, 2016), as well as the market model with the Stoxx Europe 600 and the MSCI World market indices as benchmarks. Further, we rerun our analyses without any market model and estimation period, but solely with raw returns (log returns). Our results are robust to all of these adjustments and reported in Tables B5 and B6 of Appendix B. Fourth, our robustness tests show that our results are robust to weighting or not weighting our sample observations (Tables B7 and B8 of Appendix B). In our main specifications, observations are weighted to adjust for cross-sectional correlation. Fifth, in Table B9 of Appendix B, we conduct further sensitivity analyses for the main results of

Table 4-5. We obtain quantitatively and qualitatively similar results when we employ a random effects model, cluster the standard errors by day or by firm and day (two-way clustering), and use different thresholds of percentage ownership for the family ownership dummy. Finally, we address concerns that due to many firm-level controls and fixed effects, collinearity might bias our results. Even in regression models with many independent variables, collinearity does not affect the interpretation of our results, since variance inflation factors for all independent variables employed are far from critical thresholds.

4.5. Discussion and conclusion

Our findings show that family managers are financial stewards in crises, while we fail to find evidence for family owners to act like financial stewards. By relying on stock market reactions to negative Covid-related events as our measurement for financial stewardship behavior in crises, we find that stock price reactions to family-managed firms are less negative than to family-owned and non-family firms. These results persist when we study different types of Covid-related events, e.g., virus propagation, economic disturbance, and political restriction events. Surprisingly, the reactions of family-managed firms to economic disturbance and political restriction events are statistically and economically less negative than to health-related events, suggesting that non-health related Covid events matter and should deserve more attention in the literature. Importantly, in our analyses, we uncover a spurious correlation between family ownership and management, resulting in less negative stock market reactions to Covid-related events for family-owned firms when we do not control for family management. These spurious results emphasize the importance of simultaneously controlling for different forms of family involvement.

We interpret our results as follows: In contrast to family owners that have a larger focus on monitoring the firm (Zahra & Pearce, 1989), family managers act as financial stewards by

shaping actively the crisis strategy of the firm. For example, family managers as corporate leaders possess deep knowledge of the firm, are active in weighing complex issues and trigger quick actions. These skills and their embedded position allow them to take a very active role in crisis management. In their role as crisis managers, family managers also can rely on information flows and support from their stakeholders that share the same objectives as they do. Therefore, family managers can contribute to better daily decisions, resulting in less negative stock market reactions in crises. Our findings strongly contrast with the view that family managers who are chosen from a smaller talent pool are incompetent (Bertrand & Schoar, 2006; Perez-Gonzalez, 2006) and expropriate minority shareholders in crises, e.g., through the pursue of non-financial benefits (Chrisman, Chua, Steier, Wright, & McKee, 2012; Lemmon & Lins, 2003). Consistent with Hansen and Block (2020), this non-finding might be due to the particular nature of our sample comprising only publicly listed firms in a highly-developed country with a well-developed system of corporate governance (Germany).

But why do we not find a positive effect for family ownership? Our insignificant results for family ownership suggest that during a crisis that requires quick, complex, and active decision-making, family owners may be less involved in the daily decision-making processes and more likely to act as investors that monitor the management (Daily, Dalton, & Rajagopalan, 2003; Miller, Le Breton-Miller, Minichilli, Corbetta, & Pittino, 2014). This could be one reason why the stock market reactions of family-owned firms do not differ from those of widely held firms.

4.6. Limitations and future research

As most empirical studies, our study has limitations that offer opportunities for future research. One potentially fruitful avenue for further research is to study long-term effects of different forms of family involvement in the context of Covid. To date, it is unclear, whether

firms with family involvement may suffer less than other firms in the long run. Short-term event studies provide a short-dated assessment of the reactions of the market to specific events, but cannot provide inferences about the durability and persistence of short-term effects during the Covid pandemic. We also see a research opportunity in identifying which specific duties of family managers contribute to the competitive advantage of family-managed firms. It is beyond the scope of our paper to analyze, whether family managers' networks, their competences and decision-making skills, or their presence and leadership in a crisis are responsible for better stock market reactions. Furthermore, future research on the stewardship behavior of firms with family involvement could benefit from a broader scope to study cross-country differences. Our focus on German firms ensures a high similarity among firms and thereby provides a high internal validity to uncover the consequences of forms of family involvement on financial stewardship and stock market reactions. However, our findings cannot be generalized to other countries in which firms with family involvement might react differently (Hansen & Block, 2020, 2021). Moreover, we only examine the first Covid wave. This restriction ensures that Covid-related events – due to Covid's sudden and abrupt occurrence – are exogenous to our sample firms. Stock market reactions to other Covid waves depend much more on the degree of preparation of the firms to mitigate severe Covid-induced business disruptions. In any case, it would be worthwhile to investigate, whether firms led by financial stewards prepare better for difficult market environments than other firms do.

Notwithstanding these limitations, we hope that future research can use our study as a starting point for the analysis of stock market reactions to crises situations and can delve into potential solutions for how firms can mitigate the negative consequences of crises beyond solely relying on family managers.

5. Conclusion

5.1. Summary of the main findings

This dissertation provides novel insights on the firm value effects of exogenous shocks related to the announcements of takeover defenses and the propagation of Covid-19. The results of this dissertation point to heterogeneous reactions of firms to the examined exogenous events. The detailed analyses of this dissertation help to identify firm characteristics that influence the reactions of firms to these exogenous events and to reconcile mixed and inconclusive empirical evidence of prior literature.

Chapter two of this dissertation surveys the literature on the most prominent takeover defenses in the U.S. and documents that recently the views on takeover defenses have changed. Surprisingly, up until the year 2014 the literature considers takeover defenses as costly for firms. Therefore, Straska and Waller (2014) conclude that takeover protection is more often bad than good. However, chapter two of this dissertation documents opposite results for more recently published papers and thus suggests that the most prominent takeover defenses are beneficial for the firms. In this chapter, I also identify drivers for the changed views on takeover defenses. I find that more detailed, cross-sectional analyses reveal that especially long-term oriented, innovative, and stakeholder-oriented firms benefit from more protection against hostile takeovers. This result is consistent with Stein's (1988) conjecture that innovative firms suffer from more severe asymmetries of information and thus have difficulties to avoid an undervaluation on the stock market, from which hostile bidders can benefit. Takeover defenses shift the negotiating power to target firms so that target firms can negotiate harder with bidders or even block bids that undervalue the target firm. Therefore, innovative (target) firms benefit from more takeover protection. Another influencing factor for the altered views on takeover defenses recently are the advances in research on how to address statistical challenges. Specifically, the manifold takeover defenses in the U.S., the endogenous implementation of

takeover defenses by some firms, and the ambiguity about the judicial validity of some defenses pose researchers with challenges. Recently, literature tries to overcome these challenges through extensive controls for other defenses as well as for the institutional context, and through the use of natural experiments (Cain et al., 2017; Karpoff & Wittry, 2018). These advances in the literature have substantially changed the negative views of prior studies on takeover defenses, as replications in the literature document and this chapter summarizes. Therefore, this chapter helps to reconcile opposing empirical results of prior literature. Moreover, this chapter unveils that takeover defenses are heterogeneous in their efficacy and with regard to their economic consequences so that a one-size-fits-all approach in dealing with takeover defenses is not recommended. This applies especially to the common practice of constructing governance indices that proxy for weak corporate governance and merely count the number of takeover defenses a firm has (Gompers et al., 2003).

Chapter three of this dissertation studies the implementation of an anti-takeover regulation in the U.K., which protects U.K. firms against hostile bids. The results of chapter three suggest that, on average, the stock market views more takeover protection as firm value increasing. Apart from stock market reactions to this regulation, this takeover regulation has real effects on the takeover market: hostile bids are less likely to succeed and takeover premiums for targets increase. As the results of chapter three illustrate, these real effects imply a wealth transfer from bidders, for whom it becomes more difficult to expand in the U.K. through M&A, to targets, which benefit from more negotiating power in case of a hostile bid. Moreover, I document that more takeover protection is positive for innovative firms, as it encourages them to conduct more worthwhile long-term projects. In contrast, firms that are more likely to be insulated from the disciplining forces of the market for corporate control, exploit the increase in takeover protection to act against the interests of shareholders. Hence, for these firms shareholder wealth decreases. This chapter also extends our understanding on

the frictions that this takeover regulation induces. The findings suggest that the anti-takeover regulation makes it more costly for high-productivity (efficient) firms to acquire low-productivity (inefficient) firms. Hence, the overall positive stock market response to the anti-takeover regulation coincides with distortions of the takeover market that constrain the allocation of capital across firms. This constrained capital allocation could reflect a decline in overall welfare. This chapter therefore concludes that positive stock market reactions result in welfare effects that may not be in the interest of the regulator.

Chapter four of this dissertation elaborates on the heterogeneous reactions of firms to exogenous events during the Covid-19 pandemic and documents that the form of family involvement influences the behavior of family members and stock-market reactions to Covid-19. Based on stewardship theory, this chapter provides evidence consistent with pronounced differences between the duties and stewardship behavior of family owners and managers during a crisis. While family owners may rather monitor their firm, family managers actively manage their firm through crises and thus act as financial stewards. These differences in behavior result in differential stock-market performance: family-managed firms mitigate the negative effects of negative Covid-related events so that their stock-market performance is less negative than the stock market reactions of family-owned and non-family firms. When I separate the Covid-19 events into health- and non-health-related events, I find similar results. However, in terms of economic significance, the non-health-related events lead to higher stock market reactions vis-à-vis health-related events. This result shows that non-health-related events deserve more attention in the Covid-19 literature that so far does not consider them. Interestingly, this chapter unveils a spurious correlation between family ownership and management. When I do not control for family management, the results suggest that family owners attenuate the negative performance consequences of Covid-related events. This finding implies that it is always necessary to simultaneously control for the forms of family involvement.

5.2. Implications for theory and practice

This dissertation offers several implications for theory and practice.

Chapters two and three complement the literature stream on corporate governance and firm value consequences (e.g., Gompers et al., 2003) by analyzing the costs and benefits of takeover defenses. By pointing out that the view on takeover defenses deserves a re-interpretation, chapter two questions the construction of governance or entrenchment indices (Bebchuk et al., 2009; Gompers et al., 2003) that proxy for bad corporate governance by merely counting the available takeover defenses of firms. The findings of chapter two suggest that research relying on these indices may need a re-interpretation, since the performance consequences of the most prominent takeover defenses – that are constituents of these governance indices – have substantially changed. Chapter two also complements studies on the diversity of takeover defenses (Cain et al., 2017; Karpoff et al., 2021; Karpoff & Wittry, 2018). It suggests that a one-size-fits-all evaluation of takeover defenses is not recommended, because takeover defenses exhibit substantial differences in their efficacy and their performance consequences.

Chapter three also complements literature on the effectiveness as well as the benefits and costs of takeover defenses (Cain et al., 2017; Catan & Kahan, 2016) by illustrating that targets benefit from an effective anti-takeover regulation, while potential acquirers lose. In addition, chapter three complements literature on the aggregate output and productivity of firms (Bethmann et al., 2018; Bloom et al., 2010; Bloom et al., 2019). It documents that, despite on average positive shareholder wealth effects, takeover regulation can distort the capital allocation. These distortions result in unintended consequences that disadvantage firms with high productivity, which – from a welfare perspective – does not seem desirable.

Since the views on takeover defenses have changed substantially and more takeover protection on average increases shareholder wealth, chapters two and three suggest that

shareholder rights activists, corporate governance rating agencies, and shareholders should rethink their approaches to penalize firms for relying on takeover defenses. Chapters two and three suggest that especially long-term oriented, innovative, and potential target firms benefit from more takeover protection. Therefore, practitioners can learn from both chapters of this dissertation to first consider the characteristics of a firm, before penalizing it for takeover defenses. Policymakers and regulators may learn from both chapters that takeover defenses have heterogeneous effects so that an anti-takeover regulation usually results in benefits and costs, if it affects all firms of a jurisdiction. It should be the goal of regulators to implement anti-takeover regulations for the benefit of firms, but to avoid side effects on firms, for which more protection is detrimental.

Chapter four complements research on the Covid-19 pandemic (Amore et al., 2021; Ding et al., 2021) and reveals that firms with different forms of family involvement exhibit diverse stock-market reactions during the Covid-19 pandemic. Of these firms, only family-managed firms can mitigate the negative effects of exogenous crisis events on firm value. This chapter also complements research on family owners' and managers' stewardship role (Dyer et al., 2014; Miller et al., 2013) and the respective firm value consequences of financial stewards (Miller et al., 2013; Neckebrouck et al., 2018) by documenting that family managers behave like financial stewards and attenuate the negative consequences induced by crises. Family owners, however, are not effective in mitigating negative crisis-induced consequences. Moreover, this chapter complements research on corporate governance during crises (Bae et al., 2012; Baek et al., 2004; Lemmon & Lins, 2003) by showing that active management by a family member is one way to overcome a crisis. Furthermore, this chapter adds to the event study literature focusing on stock market returns of firms with family involvement. This literature explores broad event windows, e.g., weekly (Ding et al., 2021), at least two-months (Amore et al., 2021), or yearly event windows (Amann & Jaussaud, 2012; Minichilli et al.,

2016; van Essen, Strike, Carney, & Sapp, 2015) that do not allow inferences about the impact of specific, daily events. In this chapter, I overcome the limitations of prior literature and examine short-term stock market reactions to negative Covid-related events.

Chapter four also has implications for practitioners and illustrates that it is important to study the characteristics of family firms, such as the forms of family involvement, to understand the differential effects of exogenous, economic events on firm value. This chapter also suggests that stock markets attribute a special skillset to family managers that allows them to better manage crisis situations than family owners or non-family firms. This result has implications for the hiring process of family managers, as it recommends families to hire family members as managers to better overcome crisis situations. Despite being chosen from a smaller talent pool, family managers have firm-specific knowledge, a powerful position, and stakeholder support, resulting in a relatively better performance of family-managed firms in crises.

5.3. Limitations and avenues for future research

As most analyses, this dissertation has limitations that offer potentially fruitful avenues for future research.

Chapter two of this dissertation serves as a starting point to question the negative views on takeover defenses. As it is only the starting point, this chapter provides several research opportunities. An examination of the effect of takeover defenses beyond the most prominent takeover defenses reviewed in chapter two can be worthwhile to evaluate whether views on these takeover defenses have also changed due to more detailed analyses and strategic advances in the literature. Obviously, extensions of the literature should rely on state of the art event study methodology in the spirit of Kolari and Pynnönen (2010) and additional controls for the institutional context in the spirit of Karpoff and Wittry (2018). Furthermore, future research

could investigate the quality of popular governance indices proxying for the number of takeover defenses. According to the literature surveyed in chapter two, many takeover defenses increase shareholder wealth and thus do not affect corporate governance negatively. These results ultimately question the reliability of corporate governance indices and determined firm-level characteristics associated with good corporate governance. Therefore, the changed views on takeover defenses open interesting paths for future research to reexamine the construction of governance indices and the correlations between governance indices and firm performance. Moreover, chapter two suggests that interactions among takeover defenses so far are largely unexplored and unconsidered. As firms in the U.S. often are protected by several takeover defenses, an understanding of the interactions among these takeover defenses is desirable. The interplay of several takeover defenses also affects multinational companies headquartered and incorporated in different countries and listed on stock exchanges in several countries, since they therefore are affected by several (anti-)takeover regulations. Empirical evidence on the takeover protection of multinational corporations however is extremely scarce. Another interesting path for further research is to study the value-maximizing number of takeover defenses for different subgroups of firms. To date, evidence on this topic is still extremely scarce. Chapter two serves as a starting point and reviews the effects of takeover defenses in a jurisdiction that grants their firms access to a diverse set of takeover defenses. Chapter three contributes evidence on the effects of takeover protection in a jurisdiction with no prior takeover protection. Yet, the value-maximizing number of takeover defenses is still unclear.

Chapter three also offers several potentially fruitful avenues for future research. For example, to explore the generalizability of the findings in the U.K. to other countries that have implemented takeover protection. More external validity can contribute to our understanding of the effectiveness and effects of takeover defenses. Additionally, more detailed cross-sectional analyses would improve our understanding about the interplay between takeover

protection, firm-level characteristics, and firm performance. Prior literature so far – if at all – focuses on innovative firms or firms with governance problems, e.g., firms suffering from managerial entrenchment. Chapter three, in this sense, provides a starting point for future analyses to investigate the costs and benefits of more takeover protection for different subsets of firms. These analyses should go beyond classifying firms into acquirers or targets and into high or low productivity firms.

Chapter four of this dissertation documents the value of family managers during crises such as the Covid-19 pandemic. Yet, it remains an open question, whether different forms of family involvement lead to relatively better firm performance in prosperity periods. Since chapter four studies short-term market reactions to Covid-related events, the persistence of these short-term effects in the long-run remains unexplored. A long-term event study would be a fruitful avenue for future research and could show how the forms of family involvement perform during the different waves of the Covid-19 pandemic. Since chapter four, for internal validity reasons, is limited to the first Covid wave that affected the sample firms unexpectedly, extensions of the research scope to consider further Covid waves would provide more insights on the long-run reactions to the pandemic. Also, researchers could examine which firms – after the first wave – prepare better for difficult market environments that they experience in the then upcoming Covid waves. Moreover, the findings of chapter four solely focus on German firms. Future studies studying the interplay between the forms of family involvement, financial stewardship, and stock market reactions of firms should also consider firms from other countries. This consideration could provide valuable insights into cross-country differences during the Covid pandemic, which is an interesting path for further research. Furthermore, future research could analyze which special duties of family managers exactly contribute to the competitive advantage of family-managed firms during crises. Although chapter four provides evidence consistent with stewardship behavior of family-managed firms, it remains an open

question, which duties of family managers drive this result. This represents another potentially interesting avenue for future research.

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Appendix

Appendix A: Appendix for chapter 3

Table A1: Similarity between treated and control firms before the announcements of the U.K. anti-takeover regulation

This table tests treatment and control groups for differences in abnormal returns before the announcements of the PUSU rule. Specifically, we study each combination of abnormal returns for treated minus control firms from 40 trading days to two trading days prior to the announcements of the anti-takeover regulation. This yields 780 combinations of abnormal returns. This table reports the number of these 780 event windows that are negative and significant, negative and insignificant, positive and insignificant, and positive and significant. We report these results for an unmatched sample and for a matched sample. For the matched sample, we match treated to control firms using entropy balancing. Columns (1) and (4) denote statistical significance at the 5% level.

	(1)	(2)	(3)	(4)
Sample	Negative significant	Negative insignificant	Positive insignificant	Positive significant
Not matched	4	326	447	3
Matched	0	121	641	18

Table A2: U.K. anti-takeover regulation and changes in the M&A market: Robustness to additional M&A controls

This table reports the effects of the staggered implementation of the U.K. anti-takeover regulation on several M&A outcome variables associated with the bargaining power of the target firm. In this table, we include M&A controls for hostile bids, all-cash bids, competitive bids, and diversifying bids where applicable. The competitive bid dummy equals one if there is a competing bidder. The diversifying bid dummy equals one if the target two-digit SIC code differs from the acquirer's SIC code. We also control for deal value, which is the natural logarithm of the deal value as reported by SDC Platinum. Panel A reports the results of OLS regressions with a hostile bid dummy and an all-cash bid dummy as the dependent variables. Panel B reports the results of OLS regressions with the takeover premium and transaction value to total assets as the dependent variables. We measure the effect of a difference-in-differences specification on the different outcome variables when firms become takeover targets. All the specifications control for the constituents of the difference-in-differences interaction term, which are not tabulated here for brevity. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A

	(1)	(2)	(3)	(4)
Dependent variables	Hostile Bid		All Cash Bid	
Treat	0.098***	0.171*	-0.256***	-0.300**
	(2.73)	(1.92)	(-4.10)	(-2.52)
Treat × Post	-0.141**	-0.234**	0.221**	0.399**
	(-2.34)	(-2.02)	(2.14)	(2.42)
Controls	YES	YES	YES	YES
M&A controls	YES	YES	YES	YES
Deal value control	NO	YES	NO	YES
Year × Cohort FE	YES	YES	YES	YES
Industry × Cohort FE	YES	YES	YES	YES
Country × Cohort FE	YES	YES	YES	YES
Observations	798	484	798	484
R-Squared	0.323	0.442	0.376	0.445

Panel B

	(1)	(2)	(3)	(4)
Dependent variables	Takeover Premium		Transaction Value / Total Assets	
Treat	-0.056*	-0.174**	-0.478	-0.527
	(-1.85)	(-2.47)	(-1.64)	(-1.25)
Treat × Post	0.109**	0.267***	0.876**	1.357*
	(2.31)	(2.85)	(2.28)	(1.84)
Controls	YES	YES	YES	YES
M&A controls	YES	YES	YES	YES
Deal Value control	NO	YES	NO	YES
Year × Cohort FE	YES	YES	YES	YES
Industry × Cohort FE	YES	YES	YES	YES
Country × Cohort FE	YES	YES	YES	YES
Observations	705	400	704	400
R-Squared	0.414	0.382	0.479	0.656

Table A3: Winners and losers of the announcements of the U.K. anti-takeover regulation, placebo tests

This table reports the results of placebo tests to rule out systematic factors driving our results in Table 3-6. Columns (1) and (2) report the results for event dates that are 50 trading days prior to the announcements of the U.K. anti-takeover regulation. Columns (3) and (4) report the results for event dates that are 50 trading days after the announcements. As Table 3-6, this table reports the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the placebo announcements of the U.K. anti-takeover regulation. Panel A interacts target firms' characteristics that are associated with a higher takeover probability (Cremers et al., 2009; Danbolt et al., 2016) with the treatment dummy and therefore reports value effects for potential targets affected by the placebo announcements of the U.K. anti-takeover regulation. According to Cremers et al. (2009) and Danbolt et al. (2016), public target firms are characterized by high sales growth, low profitability as measured by return on assets, low leverage, and young age. Panel B reports the value effects for past acquirers and firms that will likely be acquired in the future due to their size and that are affected by the placebo announcements of the U.K. anti-takeover regulation. All the specifications report only the treatment and interaction terms for brevity, but control for the respective interacted variable. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A

Placebo tests	(1)	(2)	(3)	(4)
	Treatment 50 Trading Days prior to Announcements		Treatment 50 Trading Days after Announcements	
Variables	Target Takeover Probability	Sales Growth	Target Takeover Probability	Sales Growth
Treat	-0.006	-0.007	-0.003	-0.004
	(-0.88)	(-0.98)	(-0.36)	(-0.51)
Treat × Target Takeover Probability	-0.001		-0.006	
	(-0.24)		(-0.99)	
Treat × Sales Growth		0.003		-0.005
		(0.62)		(-0.99)
Joint Significance				
Treat + Treat × Target Takeover Probability	-0.008 (-0.90)		-0.009 (-0.94)	
Treat + Treat × Sales Growth		-0.004 (-0.49)		-0.010 (-1.09)
Controls	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES
Observations	895	895	895	895
R-Squared	0.149	0.145	0.270	0.271

Panel B

Placebo tests	(1) Treatment 50 Trading Days prior to Announcements		(3) Treatment 50 Trading Days after Announcements	
Variables	Acquirer in past 3 years	Ln(Assets)	Acquirer in past 3 years	Ln(Assets)
Treat	-0.007	-0.005	-0.008	-0.002
	(-0.89)	(-0.74)	(-0.78)	(-0.31)
Treat × Acquirer in past 3 years	0.001		0.008	
	(0.09)		(0.70)	
Treat × Ln(Assets)		0.003		0.004
		(0.42)		(0.62)
Joint Significance				
Treat + Treat ×	-0.006		0.000	
Acquirer in past 3 years	(-0.62)		(0.04)	
Treat +		-0.003		0.001
Treat × Ln(Assets)		(-0.26)		(0.12)
Controls	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES
Observations	895	895	895	895
R-Squared	0.145	0.145	0.269	0.269

Table A4: U.K. anti-takeover regulation and changes in the M&A market, placebo tests

This table reports the results for a placebo test to rule out systematic factors driving our results in Table 3-7. Columns (1) to (5) report the results for an implementation date that is shifted two years backward. Hence, in this table, we assume that firms are affected by the staggered implementation of the U.K. anti-takeover regulation two years earlier than they actually are. As Table 3-7, this table reports the effects of the staggered (placebo) implementation of the U.K. anti-takeover regulation on several M&A outcome variables associated with the bargaining power of the target firm. Column (1) reports the results for a sample that is unconditional on a firm receiving an M&A bid. Column (2) reports the results of OLS regressions with a hostile bid dummy as the dependent variable. Column (3) reports the results of OLS regressions with an all-cash bid dummy as the dependent variable. Column (4) reports the results of OLS regressions with the takeover premium as the dependent variable. Column (5) reports the results of OLS regressions with the transaction value to total assets as the dependent variable. We measure the effect of a difference-in-differences specification on the different outcome variables. All the specifications control for all constituents of the difference-in-differences interaction term, which are not tabulated here for brevity. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

	(1)	(2)	(3)	(4)	(5)
Placebo test	Treatment 2 Years prior to Implementation of the U.K. Anti-Takeover Regulation				
Dependent variables	Announced Deal	Hostile Bid	All Cash Bid	Takeover Premium	Transaction Value / Total Assets
Treat × Post	-0.009	-0.047	-0.018	-0.010	0.361
	(-1.06)	(-0.69)	(-0.20)	(-0.23)	(0.85)
Controls	YES	YES	YES	YES	YES
Year × Cohort FE	YES	YES	YES	YES	YES
Industry × Cohort FE	YES	YES	YES	YES	YES
Country × Cohort FE	YES	YES	YES	YES	YES
Firm × Cohort FE	YES	NO	NO	NO	NO
Observations	9,172	798	798	705	704
R-Squared	0.148	0.262	0.320	0.289	0.434

Table A5: U.K. anti-takeover regulation and long-term orientation, placebo tests

This table reports the results of placebo tests to rule out systematic factors driving our results in Table 3-8. In Panel A, Columns (1) to (3) report the results for event dates that are 50 trading days prior to the announcements of the U.K. anti-takeover regulation. Columns (4) to (6) report the results for event dates that are 50 trading days after the announcements. As in Table 3-8, Panel A of this table reports the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the placebo announcements of the U.K. anti-takeover regulation. In this analysis, firm characteristics that can be associated with long-term value creation are interacted with the treatment dummy to test the long-term benefit hypothesis, that is, whether innovative firms benefit from additional (placebo) takeover protection when affected by the placebo announcements of the U.K. anti-takeover regulation. Panel A reports the results for *R&D*, *Intangibles*, and an *Innovative Firm* dummy. All the specifications report only the treatment and interaction terms for brevity, but control for the respective interacted variable. Panel B reports the results for an implementation date that is shifted two years backward. Hence, we assume that firms are affected by the staggered implementation of the U.K. anti-takeover regulation two years earlier than they actually are. Panel B reports changes in the real innovation activity after firms are affected by the placebo implementation of the U.K. anti-takeover regulation. In this analysis, the difference-in-differences dummy variable is interacted with a long-term orientation dummy variable. This dummy equals one if the firm is in the top quartile of R&D or intangible assets, respectively, before the placebo implementation of the U.K. anti-takeover regulation. Panel B controls for all possible interaction terms of the triple-difference approaches. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: CAR analysis

Placebo tests	(1)	(2)	(3)	(4)	(5)	(6)
	Treatment 50 Trading Days prior to Announcements			Treatment 50 Trading Days after Announcements		
Variables	R&D	Intangibles	Innovative Firm	R&D	Intangibles	Innovative Firm
Treat	-0.006	-0.006	-0.006	-0.004	-0.004	-0.004
	(-0.82)	(-0.94)	(-0.78)	(-0.47)	(-0.44)	(-0.50)
Treat × R&D	-0.010*			0.004		
	(-1.73)			(0.71)		
Treat × Intangibles		-0.000			-0.003	
		(-0.06)			(-0.44)	
Treat × Innovative Firm			-0.002			0.000
			(-0.14)			(0.03)
Joint Significance						
Treat + Treat × R&D	-0.016*			-0.000		
	(-1.76)			(-0.04)		
Treat + Treat × Intangibles		-0.007			-0.007	
		(-0.70)			(-0.73)	
Treat + Treat × Innovative Firm			-0.008			-0.004
			(-0.55)			(-0.22)
Controls	YES	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES	YES
Observations	895	895	895	895	895	895
R-Squared	0.156	0.146	0.146	0.270	0.269	0.268

Panel B: Change in real innovation activity

Placebo test	(1) Treatment 2 Years before Implementation of U.K. Anti-Takeover Regulation	(2)
Dependent variables	R&D	Intangibles
Treat × Post × High R&D pre-reform	0.004	
	(1.05)	
Treat × Post × High intangibles pre-reform		0.077
		(0.92)
Treat × Post	-0.001	-0.061**
	(-1.23)	(-2.09)
Joint Significance		
Treat × Post + Treat × Post × High R&D pre-reform	0.003	
	(0.76)	
Treat × Post + Treat × Post × High intangibles pre-reform		0.016
		(0.21)
Controls + Cash, Q, Leverage, ROA	YES	YES
Year × Cohort FE	YES	YES
Industry × Cohort FE	YES	YES
Country × Cohort FE	YES	YES
Firm × Cohort FE	YES	YES
Observations	8,860	8,860
R-Squared	0.864	0.794

Table A6: Announcement returns and takeover premiums, breakdown by productivity, placebo tests

This table reports the results of placebo tests to rule out systematic factors driving our results of Table 3-9. Column (1) reports the results for event dates that are 50 trading days prior to the announcements of the U.K. anti-takeover regulation. Column (2) reports the results for event dates that are 50 trading days after the announcements. Columns (3) and (4) report the results for an implementation date that is shifted two years backward. Hence, we assume that firms are affected by the staggered implementation of the U.K. anti-takeover regulation two years earlier than they actually are. Columns (1) and (2) of this table report the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the (placebo) announcements of the U.K. anti-takeover regulation. In this analysis, firm characteristics that can be associated with high productivity are interacted with the treatment dummy to report value effects for high-productivity firms affected by the (placebo) announcements of the U.K. anti-takeover regulation. We define firm-level total factor productivity as the residual of a Cobb–Douglas production function where we regress value added (EBITDA + labor) on capital and labor proxied by fixed assets and wage expenses, respectively. We estimate this firm-level regression separately for each two-digit SIC code industry–year group. Firms with residuals above the median in each industry–year group are categorized as high-productivity firms. Columns (3) and (4) report the effects of the staggered (placebo) implementation of the U.K. anti-takeover regulation on takeover premium and transaction value to total assets. Both M&A outcome variables can be associated with the bargaining power of the target firm. All the specifications control for all constituents of the difference-in-difference-in-differences interaction terms, which are not tabulated here for brevity. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

	(1)	(2)	(3)	(4)
Placebo tests	Treatment 50 Trading Days prior to Announcements	Treatment 50 Trading Days after Announcements	Treatment 2 Years prior to Implementation of the U.K. Anti-Takeover Regulation	
Dependent variables	CAR(-1, 1)		Takeover Premium	Transaction Value / Total Assets
Treat	-0.000	-0.006	-0.126*	0.144
	(-0.03)	(-0.49)	(-1.67)	(0.45)
Treat × High Productivity	0.004	0.006	0.113	-0.729
	(0.31)	(0.38)	(1.17)	(-1.37)
Treat × Post			0.050	-0.070
			(0.69)	(-0.24)
Treat × Post × High Productivity			-0.159	0.208
			(-1.57)	(0.40)
Joint Significance				
Treat + Treat × High Productivity	0.004	0.000		
	(0.34)	(0.01)		
Treat × Post + Treat × Post × High Productivity			-0.110*	0.138
			(-1.66)	(0.26)
Controls	YES	YES	YES	YES
Industry × Year FE	YES	YES	NO	NO
Country × Year FE	YES	YES	NO	NO
Year × Cohort FE	NO	NO	YES	YES
Industry × Cohort FE	NO	NO	YES	YES
Country × Cohort FE	NO	NO	YES	YES
Observations	715	715	573	573
R-Squared	0.176	0.274	0.337	0.341

Table A7: Comparing alternative explanations, placebo tests

This table reports the results of placebo tests to rule out systematic factors driving our results of Table 3-10. In Panels A, C, and E, Columns (1) to (5) report the results for event dates that are 50 trading days prior to the announcements of the U.K. anti-takeover regulation. In Panels B, D, and F, Columns (1) to (5) report the results for event dates that are 50 trading days after the announcements. Panels A and B use *R&D* to measure long-term orientation. Panels C and D use *Intangibles* and Panels E and F use the *Innovative Firm* dummy. As in Table 3-10, this table reports the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the placebo announcements of the U.K. anti-takeover regulation. In this analysis, firm characteristics that can be associated with high-productivity, firm innovativeness, and entrenchment are interacted with the treatment dummy to report value effects for these firms after being affected by the placebo announcements of the U.K. anti-takeover regulation. We define firm-level total factor productivity as the residual of a Cobb–Douglas production function where we regress value added (EBITDA + labor) on capital and labor proxied by fixed assets and wage expenses, respectively. We estimate this firm-level regression separately for each two-digit SIC code industry–year group. Firms with residuals above the median in each industry–year group are categorized as high-productivity firms. All the specifications report only the treatment and interaction terms for brevity, as well as control for the respective interacted variable. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: Using *R&D investments* as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Placebo test	Treatment 50 Trading Days prior to Announcements				
Treat	0.003	0.000	0.000	0.003	0.001
	(0.39)	(0.04)	(0.05)	(0.36)	(0.13)
Treat × High Productivity		0.006	0.002		0.004
		(0.42)	(0.13)		(0.26)
Treat × R&D			-0.011	-0.012	-0.011
			(-1.36)	(-1.38)	(-1.36)
Treat × Overinvestment	-0.027	-0.029		-0.030	-0.031
	(-1.17)	(-1.27)		(-1.29)	(-1.33)
Joint Significance					
Treat + Treat × High Productivity		0.007	0.002		0.005
		(0.52)	(0.19)		(0.40)
Treat + Treat × R&D			-0.011	-0.009	-0.010
			(-0.83)	(-0.69)	(-0.76)
Treat + Treat × Over-investment	-0.024	-0.029		-0.027	-0.030
	(-1.09)	(-1.26)		(-1.21)	(-1.25)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.178	0.179	0.183	0.186	0.186

Panel B: Using *R&D investments* as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Placebo test	Treatment 50 Trading Days after Announcements				
Treat	-0.005	-0.006	-0.004	-0.004	-0.005
	(-0.53)	(-0.57)	(-0.37)	(-0.50)	(-0.44)
Treat × High Productivity		0.004	0.003		0.001
		(0.26)	(0.18)		(0.08)
Treat × R&D			-0.011	-0.011	-0.011
			(-1.37)	(-1.38)	(-1.36)
Treat × Overinvestment	0.033	0.031		0.027*	0.027
	(1.52)	(1.45)		(1.65)	(1.57)
Joint Significance					
Treat + Treat × High Productivity		-0.003	-0.002		-0.004
		(-0.23)	(-0.14)		(-0.33)
Treat + Treat × R&D			-0.016	-0.016	-0.016
			(-1.10)	(-1.25)	(-1.14)
Treat + Treat × Over-investment	0.028	0.025		0.023	0.022
	(1.34)	(1.06)		(1.43)	(1.11)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.276	0.277	0.290	0.292	0.292

Panel C: Using *Intangibles* as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Placebo test	Treatment 50 Trading Days prior to Announcements				
Treat	0.003	0.000	0.003	0.007	0.004
	(0.39)	(0.04)	(0.35)	(0.86)	(0.44)
Treat × High Productivity		0.006	0.004		0.006
		(0.42)	(0.25)		(0.39)
Treat × Intangibles			-0.015***	-0.016***	-0.016***
			(-2.66)	(-2.79)	(-2.78)
Treat × Overinvestment	-0.027	-0.029		-0.031	-0.033
	(-1.17)	(-1.27)		(-1.44)	(-1.53)
Joint Significance					
Treat + Treat × High Productivity		0.007	0.007		0.010
		(0.52)	(0.61)		(0.82)
Treat + Treat × Intangibles			-0.012	-0.009	-0.012
			(-1.06)	(-0.83)	(-1.02)
Treat + Treat × Over-investment	-0.024	-0.029		-0.024	-0.028
	(-1.09)	(-1.26)		(-1.19)	(-1.33)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.178	0.179	0.193	0.196	0.197

Panel D: Using *Intangibles* as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Placebo test	Treatment 50 Trading Days after Announcements				
Treat	-0.005	-0.006	-0.005	-0.005	-0.006
	(-0.53)	(-0.57)	(-0.46)	(-0.52)	(-0.54)
Treat × High Productivity		0.004	0.005		0.003
		(0.26)	(0.35)		(0.22)
Treat × Intangibles			0.000	0.000	0.000
			(0.05)	(0.03)	(0.08)
Treat × Overinvestment	0.033	0.031		0.032	0.031
	(1.52)	(1.45)		(1.49)	(1.44)
Joint Significance					
Treat + Treat × High Productivity		-0.003	-0.000		-0.003
		(-0.23)	(-0.01)		(-0.25)
Treat + Treat × Intangibles			-0.005	-0.004	-0.006
			(-0.39)	(-0.43)	(-0.45)
Treat + Treat × Over-investment	0.028	0.025		0.028	0.025
	(1.34)	(1.06)		(1.31)	(1.07)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.276	0.277	0.275	0.277	0.277

Panel E: Using *Innovative Firm* as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Placebo test	Treatment 50 trading days prior announcements				
Treat	0.003	0.000	-0.000	0.004	0.001
	(0.39)	(0.04)	(-0.02)	(0.39)	(0.06)
Treat × High Productivity		0.006	0.005		0.006
		(0.42)	(0.32)		(0.44)
Treat × Innovative Firm			0.000	0.000	0.000
			(0.02)	(0.02)	(0.01)
Treat × Overinvestment	-0.027	-0.029		-0.027	-0.030
	(-1.17)	(-1.27)		(-1.19)	(-1.29)
Joint Significance					
Treat + Treat × High Productivity		0.007	0.004		0.007
		(0.52)	(0.34)		(0.52)
Treat + Treat × Innovative Firm			0.000	0.004	0.001
			(0.01)	(0.25)	(0.04)
Treat + Treat × Over-investment	-0.024	-0.029		-0.024	-0.029
	(-1.09)	(-1.26)		(-1.07)	(-1.26)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.178	0.179	0.177	0.179	0.179

Panel F: Using *Innovative Firm* as Long-Term Orientation Proxy

	(1)	(2)	(3)	(4)	(5)
Placebo test	Treatment 50 Trading Days after Announcements				
Treat	-0.005	-0.006	0.002	0.003	0.001
	(-0.53)	(-0.57)	(0.14)	(0.36)	(0.07)
Treat × High Productivity		0.004	0.007		0.005
		(0.26)	(0.47)		(0.35)
Treat × Innovative Firm			-0.043	-0.042	-0.043
			(-1.63)	(-1.61)	(-1.61)
Treat × Overinvestment	0.033	0.031		0.031*	0.029*
	(1.52)	(1.45)		(1.86)	(1.74)
Joint Significance					
Treat + Treat × High Productivity		-0.003	0.008		0.006
		(-0.23)	(0.86)		(0.58)
Treat + Treat × Innovative Firm			-0.042	-0.039	-0.042
			(-1.64)	(-1.54)	(-1.64)
Treat + Treat × Over-investment	0.028	0.025		0.037**	0.030
	(1.34)	(1.06)		(2.09)	(1.49)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.276	0.277	0.293	0.295	0.295

Table A8: Summary statistics, matched sample analysis

This table presents descriptive statistics for our pooled sample, the sample firms that were affected by the U.K. anti-takeover regulation, and those that were not affected. Firms are matched based on firm characteristics in the year before the announcements and on abnormal stock returns before the announcements via entropy balancing. The mean and standard deviation (in parentheses) for each variable are reported separately. Column (1) reports the estimates for our whole sample of LSE-listed firms. Column (2) reports estimates for the sample firms affected by the anti-takeover regulation. Column (3) reports estimates for sample firms not affected by the anti-takeover regulation. Column (4) reports the p-value from a t-test of the difference between affected and unaffected firms, with standard errors adjusted for clustering at the firm level. The control variable *Intangibles* is defined as intangible assets over lagged total assets; *R&D* is defined as R&D expenses over lagged total assets; *Innovative Firm* is a dummy variable that equals one if the firm is smaller than the median firm and has positive R&D expenses and zero otherwise; *Target Takeover Probability* is a target firm's likelihood of takeover (Cremers et al. (2009). We model the probability of a firm becoming a target the next year via a logit specification. This specification is estimated by using several independent variables at the end of the previous year, such as Tobin's Q, property, plant, and equipment (PPE), cash, block ownership, market capitalization, leverage, and return on assets; *Sales Growth* is the one-year growth in sales; *Acquirer in past 3 years* is a dummy variable that equals one if the firm was an acquirer within the last three years and zero otherwise; *Ln(Assets)* is the natural logarithm of total assets; *Overinvestment* is a dummy variable that equals one if the firm is prone to overinvestment and zero otherwise (Biddle et al. (2009) Cheng et al. (2013); *Ln(Firm Age)* is the logarithm of years since the firm's incorporation; and *High Productivity* is defined as the residual of a log-linear Cobb–Douglas production function. For each two-digit SIC code industry–year group, we regress value added on capital and labor. Firms whose average residuals over two years are above the median of each two-digit SIC code industry–year group are categorized as high-productivity firms. The variable *Announced Bid* is a dummy that equals one if there was a bid for the firm in the respective year and zero otherwise; *Hostile Bid* is a dummy variable that equals one if the bid for the firm is classified as hostile and zero otherwise; *All Cash Bid* is a dummy variable that equals one if the method of payment is solely cash and zero otherwise; *Takeover Premium* is the ratio of the final offer price to the target stock price four weeks prior to the M&A announcement; *Transaction value / total assets* is the ratio of the M&A transaction value to total assets of the fiscal year before the M&A announcement.

Variables	(1) Full Sample	(2) Affected by U.K. Anti-Takeover Regulation	(3) Unaffected by U.K. Anti-Takeover Regulation	(4) p-Value of Difference (2) - (3)
Panel A: Control variables				
Intangibles	0.244 (0.367)	0.264 (0.267)	0.224 (0.445)	0.284
R&D	0.015 (0.045)	0.017 (0.048)	0.014 (0.041)	0.376
Innovative Firm	0.152 (0.359)	0.181 (0.385)	0.122 (0.328)	0.166
Target Takeover Probability	0.010 (0.008)	0.010 (0.006)	0.010 (0.011)	0.809
Sales Growth	-0.042 (0.618)	-0.043 (0.560)	-0.042 (0.673)	0.986
Acquirer in past 3 years	0.498 (0.500)	0.543 (0.499)	0.453 (0.499)	0.179
Ln(Assets)	12.776 (2.176)	12.779 (2.051)	12.774 (2.297)	0.982
Overinvestment	0.031 (0.173)	0.036 (0.187)	0.026 (0.159)	0.407
Ln(Firm Age)	2.673 (1.032)	2.852 (1.040)	2.493 (0.994)	0.011
High Productivity	0.532 (0.499)	0.526 (0.500)	0.541 (0.500)	0.849

Panel B: Outcome variables				
Announced Bid	0.035 (0.183)	0.041 (0.199)	0.028 (0.166)	0.025
Hostile Bid	0.109 (0.312)	0.118 (0.323)	0.100 (0.301)	0.522
All Cash Bid	0.364 (0.482)	0.301 (0.459)	0.428 (0.495)	0.007
Takeover Premium	14.343 (27.572)	11.630 (25.004)	17.056 (29.714)	0.041
Transaction Value / Total Assets	0.516 (1.359)	0.406 (1.015)	0.626 (1.626)	0.047
Panel C: Treatment overview				
Number of LSE sample firms	895	555	340	
Number of M&A deals	798	316	482	

Table A9: Winners and losers of the announcements of the U.K. anti-takeover regulation, matched sample analysis

Similar to Table 3-6, this table reports OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the announcements of the U.K. anti-takeover regulation. Panel A interacts target firm characteristics that are associated with a higher takeover probability (Cremers et al., 2009; Danbolt et al., 2016) with the treatment dummy and therefore reports value effects for potential targets affected by the announcements of the U.K. anti-takeover regulation. According to Cremers et al. (2009) and Danbolt et al. (2016), public target firms are characterized by high sales growth, low profitability as measured by return on assets, low leverage, and young age. Panel B reports the value effects for past acquirers and firms that will likely be acquired in the future due to their size and which are affected by the announcements of the U.K. anti-takeover regulation. Firms are matched based on firm characteristics in the year before the announcements and on abnormal stock returns before the announcements via entropy balancing. All the specifications report only the treatment and interaction terms for brevity, but control for the respective interacted variable. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: Targets

	(1)	(2)
Variables	Target Takeover Probability	Sales Growth
Treat	0.001	0.005
	(0.07)	(0.33)
Treat × Target Takeover Probability	0.011*	
	(1.72)	
Treat × Sales Growth		0.006
		(0.52)
Joint Significance		
Treat + Treat × Target Takeover Probability	0.012	
	(0.68)	
Treat + Treat × Sales Growth		0.010
		(0.45)
Controls	YES	YES
Industry × Year FE	YES	YES
Country × Year FE	YES	YES
Observations	895	895
R-Squared	0.285	0.281

Panel B: Acquirers

	(1)	(2)
Variables	Acquirer in past 3 years	Ln(Assets)
Treat	0.025**	-0.004
	(2.08)	(-0.26)
Treat × Acquirer in past 3 years	-0.047***	
	(-3.10)	
Treat × Ln(Assets)		-0.016**
		(-2.29)
Joint Significance		
Treat + Treat × Acquirer in past 3 years	-0.023	
	(-1.14)	
Treat + Treat × Ln(Assets)		-0.020
		(-1.17)
Controls	YES	YES
Industry × Year FE	YES	YES
Country × Year FE	YES	YES
Observations	895	895
R-Squared	0.307	0.280

Table A10: U.K. anti-takeover regulation and changes in the M&A market, matched sample analysis

Similar to Table 3-7, this table reports the effects of the staggered implementation of the U.K. anti-takeover regulation on several M&A outcome variables associated with the bargaining power of the target firm. Column (1) reports the results of OLS regressions with an announced deal dummy as the dependent variable that equals one if the firm is a target in an M&A deal. The sample for Column (1) is not conditional on a firm receiving an M&A bid. This sample is matched based on firm characteristics before and after the implementation of the U.K. anti-takeover regulation. Column (2) reports the results of OLS regressions with a hostile bid dummy as the dependent variable. Column (3) reports the results of OLS regressions with an all-cash bid dummy as the dependent variable. Column (4) reports the results of OLS regressions with the takeover premium as the dependent variable. Column (5) reports the results of OLS regressions with transaction value to total assets as the dependent variable. We measure the effect of a difference-in-differences specification on the different outcome variables. The conditional sample is matched based on firm characteristics before the deal announcements via entropy balancing. All the specifications control for all constituents of the difference-in-differences interaction term, which are not tabulated for brevity. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Dependent variables	(1) Announced Deal	(2) Hostile Bid	(3) All Cash Bid	(4) Takeover Premium	(5) Transaction Value / Total Assets
Treat × Post	-0.023*	-0.109*	0.184	0.173***	0.995***
	(-1.65)	(-1.69)	(1.61)	(3.11)	(3.14)
Controls	YES	YES	YES	YES	YES
Year × Cohort FE	YES	YES	YES	YES	YES
Industry × Cohort FE	YES	YES	YES	YES	YES
Country × Cohort FE	YES	YES	YES	YES	YES
Firm × Cohort FE	YES	NO	NO	NO	NO
Observations	9,172	798	798	705	704
R-Squared	0.153	0.283	0.342	0.317	0.260

Table A11: U.K. anti-takeover regulation and long-term orientation, matched sample analysis

Similar to Table 3-8, Panel A of this table reports the results of OLS regression results with CAR(-1, 1) as the dependent variable. In this analysis, firm characteristics that can be associated with long-term value creation are interacted with the treatment dummy to test the long-term benefit hypothesis, that is, whether innovative firms benefit from additional takeover protection when affected by the announcements of the U.K. anti-takeover regulation. Panel A reports the results for *R&D*, *Intangibles*, and an *Innovative Firm* dummy. Panel B reports changes in the real innovation activity after firms are affected by the U.K. anti-takeover regulation. In this analysis the difference-in-differences dummy variable is interacted with a long-term orientation dummy variable. This dummy equals one if the firm is in the top quartile of R&D or intangible assets, respectively, before the implementation of the U.K. anti-takeover regulation. Firms are matched based on firm characteristics in the year before the announcements and on abnormal stock returns before the announcements of the anti-takeover regulation (Panel A) and matched based on firm characteristics before and after the implementation (Panel B) via entropy balancing. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: CAR analysis

	(1)	(2)	(3)
Variables	R&D	Intangibles	Innovative Firm
Treat	0.004	0.002	-0.003
	(0.30)	(0.16)	(-0.18)
Treat × R&D	0.009*		
	(1.77)		
Treat × Intangibles		0.013	
		(1.59)	
Treat × Innovative Firm			0.036*
			(2.00)
Joint Significance			
Treat + Treat × R&D	0.013		
	(0.93)		
Treat +		0.015	
Treat × Intangibles		(0.78)	
Treat +			0.033*
Treat × Innovative Firm			(1.92)
Controls	YES	YES	YES
Industry × Year FE	YES	YES	YES
Country × Year FE	YES	YES	YES
Observations	895	895	895
R-Squared	0.275	0.279	0.281

Panel B: Change in real innovation activity

	(1)	(2)
Dependent variables	R&D	Intangibles
Treat × Post × High R&D	0.007	
	(1.50)	
Treat × Post × High intangibles		0.170**
		(2.12)
Treat × Post	0.000	-0.037*
	(0.40)	(-1.69)
Joint Significance		
Treat × Post +	0.007*	
Treat × Post × High R&D pre-reform	(1.67)	
Treat × Post +		0.133*
Treat × Post × High intangibles pre-reform		(1.69)
Controls	YES	YES
Year × Cohort FE	YES	YES
Industry × Cohort FE	YES	YES
Country × Cohort FE	YES	YES
Firm × Cohort FE	YES	YES
Observations	8,860	8,860
R-Squared	0.872	0.814

Table A12: Announcement returns and takeover premiums, breakdown by productivity, matched sample analysis

Similar to Table 3-9, Column (1) of this table reports the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the announcements of the U.K. anti-takeover regulation. In this analysis, firm characteristics that can be associated with high productivity are interacted with the treatment dummy to report value effects for high-productivity firms affected by the announcements of the U.K. anti-takeover regulation. Firms are matched based on firm characteristics in the year before the announcements and on abnormal stock returns before the announcements via entropy balancing. All the specifications report only the treatment and interaction terms for brevity, but control for the respective interacted variable. Columns (2) and (3) report the effects of the staggered implementation of the U.K. anti-takeover regulation on the takeover premium and transaction value to total assets. Both M&A outcome variables can be associated with the bargaining power of the target firm. Firms are matched based on firm characteristics before the deal announcements via entropy balancing. All the specifications control for all constituents of the difference-in-difference-in-differences interaction terms, which are not tabulated for brevity. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

	(1)	(2)	(3)
Dependent variables	CAR(-1, 1)	Takeover Premium	Transaction Value / Total Assets
Treat	0.039**	-0.209***	-0.286
	(2.44)	(-3.00)	(-1.27)
Treat × High Productivity	-0.058***	0.087	-0.393
	(-3.29)	(0.98)	(-1.08)
Treat × Post		0.273***	0.923***
		(3.01)	(2.78)
Treat × Post × High Productivity		-0.159	0.251
		(-1.29)	(0.42)
Joint Significance			
Treat + Treat × High Productivity	-0.018		
	(-1.13)		
Treat × Post +		0.114	1.174**
Treat × Post × High Productivity		(1.29)	(2.16)
Controls	YES	YES	YES
Industry × Year FE	YES	NO	NO
Country × Year FE	YES	NO	NO
Year × Cohort FE	NO	YES	YES
Industry × Cohort FE	NO	YES	YES
Country × Cohort FE	NO	YES	YES
Observations	715	573	573
R-Squared	0.452	0.369	0.322

Table A13: Comparison of alternative explanations, matched sample analysis

Similar to Table 3-10, this table reports the results of OLS regressions with CAR(-1, 1) as the dependent variable, where CAR(-1, 1) measures the short-term value effects around the announcements of the U.K. anti-takeover regulation. In this analysis, firm characteristics that can be associated with high productivity, firm innovativeness, and entrenchment are interacted with the treatment dummy to measure short-term value effects around the announcements of the U.K. anti-takeover regulation. Panel A uses *R&D* to measure long-term orientation. Panel B uses *Intangibles* and Panel C uses the *Innovative Firm* dummy. Firms are matched based on firm characteristics in the year before the announcements and on abnormal stock returns before the announcements via entropy balancing. All the specifications report only the treatment and interaction terms for brevity, but control for the respective interacted variable. As Giroud and Mueller (2010), we use firm size, size squared, and firm age as control variables. Standard errors are clustered by firm and t-statistics are in parentheses. Asterisks denote statistical significance at the 10% level (*), 5% level (**), and 1% level (***), respectively.

Panel A: Using R&D investments as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Treat	0.008	0.041**	0.039**	0.008	0.041**
	(0.55)	(2.52)	(2.40)	(0.55)	(2.49)
Treat × High Productivity		-0.053***	-0.057***		-0.052***
		(-2.87)	(-3.28)		(-2.83)
Treat × R&D			-0.007	-0.002	-0.004
			(-0.72)	(-0.24)	(-0.44)
Treat × Overinvestment	-0.096***	-0.067**		-0.096***	-0.066**
	(-3.26)	(-2.10)		(-3.47)	(-2.12)
Joint Significance					
Treat + Treat × High Productivity		-0.012	-0.018		-0.012
		(-0.69)	(-1.12)		(-0.68)
Treat + Treat × R&D			0.032*	0.006	0.036*
			(1.68)	(0.34)	(1.90)
Treat + Treat × Over-investment	-0.088***	-0.026		-0.088***	-0.026
	(-3.41)	(-0.79)		(-3.67)	(-0.80)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.427	0.462	0.455	0.430	0.465

Panel B: Using *Intangibles* as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Treat	0.008	0.041**	0.038**	0.006	0.040**
	(0.55)	(2.52)	(2.43)	(0.47)	(2.51)
Treat × High Productivity		-0.053***	-0.058***		-0.054***
		(-2.87)	(-3.15)		(-2.76)
Treat × Intangibles			0.007	0.010	0.008
			(0.57)	(0.77)	(0.65)
Treat × Overinvestment	-0.096***	-0.067**		-0.094***	-0.065**
	(-3.26)	(-2.10)		(-3.48)	(-2.20)
Joint Significance					
Treat + Treat × High Productivity		-0.012	-0.020		-0.014
		(-0.69)	(-1.27)		(-0.84)
Treat + Treat × Intangibles			0.045**	0.016	0.048**
			(2.27)	(0.73)	(2.36)
Treat + Treat × Over-investment	-0.088***	-0.026		-0.088***	-0.026
	(-3.41)	(-0.79)		(-3.68)	(-0.80)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.427	0.462	0.456	0.432	0.467

Panel C: Using *Innovative Firm* as a long-term orientation proxy

	(1)	(2)	(3)	(4)	(5)
Treat	0.008	0.041**	0.036**	0.007	0.038**
	(0.55)	(2.52)	(2.11)	(0.42)	(2.15)
Treat × High Productivity		-0.053***	-0.057***		-0.052***
		(-2.87)	(-3.27)		(-2.83)
Treat × Innovative Firm			0.014	0.010	0.015
			(0.62)	(0.36)	(0.67)
Treat × Overinvestment	-0.096***	-0.067**		-0.096***	-0.068**
	(-3.26)	(-2.10)		(-3.37)	(-2.18)
Joint Significance					
Treat + Treat × High Productivity		-0.012	-0.021		-0.014
		(-0.69)	(-1.24)		(-0.79)
Treat + Treat × Innovative Firm			0.051**	0.017	0.053**
			(2.15)	(0.68)	(2.34)
Treat + Treat × Over-investment	-0.088***	-0.026		-0.090***	-0.031
	(-3.41)	(-0.79)		(-3.34)	(-0.91)
Controls	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Country × Year FE	YES	YES	YES	YES	YES
Observations	715	715	715	715	715
R-Squared	0.427	0.462	0.456	0.433	0.466

Appendix B: Appendix for chapter 4

Table B1: Cross-sectional analysis of family involvement and firms' stock price reactions to Covid-related events, placebo tests

This table reports the results of placebo tests that randomly assign treatment to our Covid-related announcements among our sample observations to rule out that systematic factors are driving our results in Table 4-5. As Table 4-5, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Event-specific control variables comprise dummy variables indicating virus expansion, economy disturbance, and political restriction events. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Family ownership dummy	0.07			0.08	0.05	0.05	0.06	0.08	0.07
	(1.22)			(1.40)	(1.07)	(1.07)	(1.29)	(1.21)	(1.11)
	(p = .224)			(p = .162)	(p = .285)	(p = .287)	(p = .198)	(p = .227)	(p = .269)
Family management dummy		0.03		0.02	0.03	0.03	0.03	-0.00	0.00
		(0.40)		(0.35)	(0.47)	(0.47)	(0.41)	(-0.04)	(0.04)
		(p = .692)		(p = .727)	(p = .640)	(p = .641)	(p = .683)	(p = .971)	(p = .969)
Family chairman dummy			-0.01	0.01	0.04	0.04	0.03	-0.03	-0.02
			(-0.12)	(0.21)	(0.79)	(0.79)	(0.68)	(-0.50)	(-0.25)
			(p = .904)	(p = .831)	(p = .431)	(p = .428)	(p = .500)	(p = .615)	(p = .805)
Event-specific controls	YES	YES	YES	NO	NO	YES	YES	YES	YES
Firm-level controls	YES	YES	YES	NO	NO	NO	YES	YES	YES
Day FE	YES	YES	YES	NO	YES	YES	YES	YES	NO
Industry FE	YES	YES	YES	NO	NO	NO	NO	YES	NO
Industry-Day FE	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	32,110	32,110	32,110	32,110	32,110	32,110	32,110	32,110	32,110
R-Squared	0.103	0.103	0.103	0.000	0.100	0.100	0.101	0.103	0.307

Table B2: Cross-sectional analysis of family involvement and firms' stock price reactions to specific Covid-related events, placebo tests

This table reports the results of placebo tests that randomly assign treatment to our Covid-related announcements among our sample observations to rule out that systematic factors are driving our results in Table 4-7. As Table 4-7, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Event	Virus expansion			Economy disturbance			Political restrictions		
Family ownership dummy	0.02	0.03	0.07	0.09	0.05	0.08	0.19	0.22	0.14
	(0.32)	(0.59)	(1.02)	(0.96)	(0.59)	(0.75)	(1.42)	(1.66)	(1.03)
	(p = .746)	(p = .554)	(p = .307)	(p = .337)	(p = .555)	(p = .456)	(p = .156)	(p = .097)	(p = .303)
Family management dummy	0.05	0.03	-0.04	0.02	0.12	0.13	-0.05	-0.15	-0.09
	(0.74)	(0.49)	(-0.41)	(0.20)	(1.02)	(1.08)	(-0.33)	(-0.86)	(-0.50)
	(p = .458)	(p = .623)	(p = .682)	(p = .839)	(p = .307)	(p = .282)	(p = .741)	(p = .391)	(p = .621)
Family chairman dummy	0.05	0.06	0.00	-0.02	-0.03	-0.11	0.01	-0.02	-0.06
	(1.06)	(1.18)	(0.05)	(-0.24)	(-0.42)	(-1.16)	(0.05)	(-0.12)	(-0.42)
	(p = .290)	(p = .238)	(p = .960)	(p = .810)	(p = .677)	(p = .249)	(p = .958)	(p = .901)	(p = .673)
Firm-level controls	NO	YES	YES	NO	YES	YES	NO	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	20,720	20,720	20,720	7,363	7,363	7,363	4,027	4,027	4,027
R-Squared	0.112	0.112	0.115	0.109	0.111	0.117	0.107	0.110	0.126

Table B3: Cross-sectional analysis of family involvement and firms' stock price reactions to Covid-related events, matched sample analysis

This table reports the results of a matched sample analysis that matches family to non-family firms to rule out that differences in observable control variables are driving our results in Table 4-5. As Table 4-5, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Event-specific control variables comprise dummy variables indicating virus expansion, economy disturbance, and political restriction events. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Family ownership dummy	0.15			0.16	0.11	0.11	0.08	0.06	0.07
	(2.49)			(2.62)	(1.76)	(1.81)	(1.18)	(0.80)	(0.93)
	(p = .013)			(p = .009)	(p = .079)	(p = .072)	(p = .240)	(p = .425)	(p = .355)
Family management dummy		0.40		0.31	0.25	0.25	0.32	0.39	0.38
		(5.19)		(4.52)	(3.60)	(3.63)	(4.13)	(4.42)	(4.02)
		(p = .000)		(p = .000)	(p = .000)	(p = .000)	(p = .000)	(p = .000)	(p = .000)
Family chairman dummy			0.01	0.05	0.03	0.03	0.04	0.01	-0.00
			(0.09)	(0.70)	(0.38)	(0.41)	(0.50)	(0.15)	(-0.06)
			(p = .931)	(p = .483)	(p = .704)	(p = .684)	(p = .617)	(p = .879)	(p = .954)
Event-specific controls	YES	YES	YES	NO	NO	YES	YES	YES	YES
Firm-level controls	YES	YES	YES	NO	NO	NO	YES	YES	YES
Day FE	YES	YES	YES	NO	YES	YES	YES	YES	NO
Industry FE	YES	YES	YES	NO	NO	NO	NO	YES	NO
Industry-Day FE	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	26,576	26,576	26,576	26,576	26,576	26,576	26,576	26,576	26,576
R-Squared	0.126	0.127	0.126	0.002	0.123	0.124	0.124	0.127	0.292

Table B4: Cross-sectional analysis of family involvement and firms' stock price reactions to specific Covid-related events, matched sample analysis

This table reports the results of a matched sample analysis that matches family to non-family firms to rule out that differences in observable control variables are driving our results in Table 4-7. As Table 4-7, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Event	Virus expansion			Economy disturbance			Political restrictions		
Family ownership dummy	0.03	-0.01	-0.05	0.30	0.27	0.27	0.22	0.19	0.15
	(0.39)	(-0.19)	(-0.66)	(3.22)	(2.75)	(2.66)	(1.38)	(1.18)	(0.79)
	(p = .697)	(p = .851)	(p = .511)	(p = .001)	(p = .006)	(p = .008)	(p = .170)	(p = .237)	(p = .427)
Family management dummy	0.19	0.26	0.26	0.30	0.37	0.52	0.51	0.58	0.78
	(2.58)	(3.23)	(3.01)	(2.68)	(2.84)	(3.95)	(3.04)	(3.10)	(3.67)
	(p = .010)	(p = .001)	(p = .003)	(p = .008)	(p = .005)	(p = .000)	(p = .003)	(p = .002)	(p = .000)
Family chairman dummy	0.04	0.06	0.04	0.07	0.10	0.09	0.01	-0.05	-0.10
	(0.55)	(0.77)	(0.46)	(0.66)	(0.88)	(0.86)	(0.05)	(-0.29)	(-0.53)
	(p = .581)	(p = .443)	(p = .646)	(p = .509)	(p = .380)	(p = .392)	(p = .960)	(p = .772)	(p = .593)
Firm-level controls	NO	YES	YES	NO	YES	YES	NO	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	15,624	15,624	15,624	7,074	7,074	7,074	3,878	3,878	3,878
R-Squared	0.119	0.120	0.123	0.124	0.125	0.132	0.142	0.145	0.155

Table B5: Cross-sectional analysis of family involvement and firms' stock price reactions to Covid-related events, alternative stock return models

This table reports the results for stock returns that are generated from alternative stock return models to mitigate the risk that out results in Table 4-5 are subject to the choice of the Fama-French-Carhart six factor model. As Table 4-5, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Event-specific control variables comprise dummy variables indicating virus expansion, economy disturbance, and political restriction events. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	FF3	FF3	FF4	FF5	Stoxx Europe 600	MSCI World	MSCI World	Raw Returns	Raw Returns
Family ownership dummy	0.03	0.05	0.04	0.05	0.03	0.01	0.03	0.02	0.04
	(0.42)	(0.68)	(0.56)	(0.63)	(0.45)	(0.22)	(0.44)	(0.29)	(0.52)
	(p = .675)	(p = .495)	(p = .574)	(p = .532)	(p = .654)	(p = .824)	(p = .660)	(p = .770)	(p = .604)
Family management dummy	0.35	0.36	0.34	0.33	0.32	0.29	0.30	0.30	0.31
	(3.34)	(3.21)	(3.33)	(3.15)	(3.26)	(2.95)	(2.77)	(3.47)	(3.23)
	(p = .001)	(p = .001)	(p = .001)	(p = .002)	(p = .001)	(p = .003)	(p = .006)	(p = .001)	(p = .001)
Family chairman dummy	-0.03	-0.04	-0.01	-0.03	-0.04	-0.01	-0.02	0.00	-0.02
	(-0.43)	(-0.60)	(-0.13)	(-0.46)	(-0.63)	(-0.08)	(-0.25)	(0.01)	(-0.22)
	(p = .670)	(p = .550)	(p = .896)	(p = .646)	(p = .531)	(p = .935)	(p = .803)	(p = .995)	(p = .826)
Event-specific controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm-level controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day FE	YES	NO	YES	YES	YES	YES	NO	YES	NO
Industry FE	YES	NO	YES	YES	YES	YES	NO	YES	NO
Industry-Day FE	NO	YES	NO	NO	NO	NO	YES	NO	YES
Observations	32,110	32,110	32,110	32,110	32,110	32,110	32,110	32,110	32,110
R-Squared	0.067	0.245	0.160	0.067	0.090	0.081	0.258	0.170	0.309

Table B6: Cross-sectional analysis of family involvement and firms' stock price reactions to specific Covid-related events, alternative stock return models

This table reports the results for stock returns that are generated from alternative stock return models to mitigate the risk that out results in Table 4-7 are subject to the choice of the Fama-French-Carhart six factor model. As Table 4-7, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Event	Virus expansion			Economy disturbance			Political restrictions		
	FF3	MSCI World	Raw Returns	FF3	MSCI World	Raw Returns	FF3	MSCI World	Raw Returns
Family ownership dummy	-0.01	-0.02	-0.08	0.11	0.08	0.23	0.10	-0.01	0.06
	(-0.11)	(-0.21)	(-1.21)	(1.25)	(0.96)	(1.92)	(0.53)	(-0.04)	(0.29)
	(p = .912)	(p = .832)	(p = .227)	(p = .212)	(p = .337)	(p = .055)	(p = .596)	(p = .971)	(p = .769)
Family management dummy	0.24	0.18	0.22	0.51	0.48	0.54	0.69	0.57	0.49
	(2.32)	(1.87)	(2.52)	(3.80)	(3.59)	(3.72)	(2.96)	(2.50)	(2.08)
	(p = .021)	(p = .062)	(p = .012)	(p = .000)	(p = .000)	(p = .000)	(p = .003)	(p = .013)	(p = .039)
Family chairman dummy	-0.00	0.01	0.02	0.07	0.09	0.11	-0.20	-0.17	-0.12
	(-0.05)	(0.23)	(0.24)	(0.82)	(1.00)	(0.80)	(-1.14)	(-1.02)	(-0.65)
	(p = .962)	(p = .818)	(p = .810)	(p = .412)	(p = .319)	(p = .425)	(p = .257)	(p = .309)	(p = .516)
Firm-level controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	20,720	20,720	20,720	7,363	7,363	7,363	4,027	4,027	4,027
R-Squared	0.064	0.084	0.161	0.080	0.092	0.188	0.098	0.099	0.207

Table B7: Cross-sectional analysis of family involvement and firms' stock price reactions to Covid-related events, unweighted regression analysis

This table reports the results of an unweighted regression analysis to document that our results in Table 4-5 which are weighted to adjust for cross-sectional correlation are quantitatively and qualitatively similar to not-weighting our sample observations. As Table 4-5, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Event-specific control variables comprise dummy variables indicating virus expansion, economy disturbance, and political restriction events. Firm-level controls are defined in Table 4-1. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Family ownership dummy	0.14			0.15	0.11	0.12	0.07	0.04	0.06
	(2.41)			(2.43)	(1.78)	(1.88)	(1.17)	(0.56)	(0.77)
	(p = .017)			(p = .016)	(p = .077)	(p = .061)	(p = .243)	(p = .576)	(p = .444)
Family management dummy		0.39		0.30	0.25	0.25	0.31	0.38	0.38
		(5.26)		(4.43)	(3.65)	(3.72)	(4.06)	(4.42)	(4.01)
		(p = .000)		(p = .000)	(p = .000)	(p = .000)	(p = .000)	(p = .000)	(p = .000)
Family chairman dummy			0.00	0.03	0.02	0.02	0.02	0.01	-0.01
			(0.04)	(0.48)	(0.26)	(0.32)	(0.22)	(0.07)	(-0.09)
			(p = .965)	(p = .631)	(p = .792)	(p = .750)	(p = .827)	(p = .942)	(p = .931)
Event-specific controls	YES	YES	YES	NO	NO	YES	YES	YES	YES
Firm-level controls	YES	YES	YES	NO	NO	NO	YES	YES	YES
Day FE	YES	YES	YES	NO	YES	YES	YES	YES	NO
Industry FE	YES	YES	YES	NO	NO	NO	NO	YES	NO
Industry-Day FE	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	32,110	32,110	32,110	32,110	32,110	32,110	32,110	32,110	32,110
R-Squared	0.115	0.115	0.114	0.001	0.112	0.112	0.113	0.115	0.266

Table B8: Cross-sectional analysis of family involvement and firms' stock price reactions to specific Covid-related events, unweighted regression analysis

This table reports the results of an unweighted regression analysis to document that our results in Table 4-7 which are weighted to adjust for cross-sectional correlation are quantitatively and qualitatively similar to not-weighting our sample observations. As Table 4-7, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Firm-level controls are defined in Table 4-1. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Event	Virus expansion			Economy disturbance			Political restrictions		
Family ownership dummy	0.00	-0.01	-0.05	0.36	0.27	0.24	0.28	0.19	0.14
	(0.06)	(-0.18)	(-0.70)	(3.73)	(2.60)	(2.20)	(1.72)	(1.20)	(0.74)
	(p = .948)	(p = .860)	(p = .486)	(p = .000)	(p = .010)	(p = .029)	(p = .086)	(p = .231)	(p = .462)
Family management dummy	0.17	0.23	0.24	0.35	0.40	0.57	0.54	0.60	0.90
	(2.41)	(3.08)	(2.84)	(3.14)	(3.13)	(4.27)	(3.27)	(3.25)	(4.09)
	(p = .017)	(p = .002)	(p = .005)	(p = .002)	(p = .002)	(p = .000)	(p = .001)	(p = .001)	(p = .000)
Family chairman dummy	0.02	0.03	0.02	0.10	0.11	0.13	0.04	-0.04	-0.07
	(0.34)	(0.35)	(0.23)	(0.89)	(0.94)	(1.10)	(0.24)	(-0.21)	(-0.35)
	(p = .735)	(p = .724)	(p = .816)	(p = .374)	(p = .349)	(p = .273)	(p = .810)	(p = .836)	(p = .724)
Firm-level controls	NO	YES	YES	NO	YES	YES	NO	YES	YES
Day FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	20,720	20,720	20,720	7,363	7,363	7,363	4,027	4,027	4,027
R-Squared	0.105	0.105	0.108	0.124	0.126	0.133	0.132	0.136	0.145

Table B9: Cross-sectional analysis of family involvement and firms' stock price reactions to Covid-related events, further sensitivity analyses

This table reports the results of further sensitivity analyses to document the robustness of our results in Table 4-5 to employing random effects models, different clustering of standard errors, and to using different thresholds of percentage ownership for our family ownership dummy. As Table 4-5, this table reports the results of OLS regressions with daily abnormal returns as dependent variable to investigate the cross-sectional determinants of the market reaction to Covid-related announcements for different forms of family involvement. Event-specific control variables comprise dummy variables indicating virus expansion, economy disturbance, and political restriction events. Firm-level controls are defined in Table 4-1. Because sample firms share the same event periods in calendar time, we use the portfolio weighted least squares approach of Chandra and Balachandran (1992), which produces unbiased estimates of the regression coefficient standard errors when abnormal returns are heteroskedastic and correlated across firms. Standard errors are clustered by firm and t-statistics as well as p values are in parentheses.

Further analyses for Table 4-5, model 7, that controls for event- and firm-specific controls as well as day and industry fixed effects	OLS regression coefficients	
	Family ownership dummy	Family management dummy
Random effects	0.04	0.38
	(0.51)	(4.40)
	(p = .607)	(p = .000)
Fixed effects with standard errors clustered by day	0.04	0.34
	(0.58)	(2.26)
	(p = .562)	(p = .025)
Fixed effects with standard errors clustered by firm and day	0.05	0.34
	(0.56)	(2.21)
	(p = .578)	(p = .029)
Family ownership dummy is one, when the family has more than 5% ownership	0.02	0.35
	(0.24)	(3.38)
	(p = .814)	(p = .001)
Family ownership dummy is one, when the family has more than 50% ownership	0.06	0.34
	(0.71)	(3.67)
	(p = .477)	(p = .000)

Curriculum Vitae
Lennart Ulrich

Education

01.2021 - 07.2022	**Doctoral candidate in Business Administration, University of Trier, Trier** • Thesis title: How firm value reacts to exogenous shocks: evidence from the announcements of takeover defenses and the propagation of Covid-19
07.2014 - 02.2019	**Research Assistant, WHU - Otto Beisheim School of Management, Vallendar** • Publication of the research article "Takeover Protection and Firm Value" in the Review of Law & Economics
04.2011 - 05.2014	**MSc in Business Engineering, Karlsruhe Institute of Technology, Karlsruhe** • Thesis title: Rating- and Bond-Performance of Make Whole Call and Equity Clawback Provisions
10.2007 - 04.2011	**BSc in Business Engineering, Karlsruhe Institute of Technology, Karlsruhe** • Thesis title: Value Adding in Venture Capital

Professional Experience

03.2019 - present	**AXA Konzern AG, Cologne, Germany** • Product Owner Smart Data • Data Consultant
03.2018 - 03.2020	**Koblenz University of Applied Sciences, Koblenz, Germany** • Lectureship in Corporate Finance and Corporate Governance
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