CEO SUCCESSION AND FIRM PERFORMANCE

THE INTERCONNECTEDNESS BETWEEN THE NATURE OF DEPARTURE AND SUCCESSOR ORIGIN

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CEO Succession and Firm Performance: The Interconnectedness Between the Nature of Departure and Successor Origin

Abstract:

We investigate whether firm performance following CEO succession is influenced by the nature of the departure in combination with the origin of the successor CEO. We hypothesize that an interconnectedness of those two processes plays a fundamental role in explaining the link between executive turnover and subsequent firm performance. Using a quasi-experimental difference-in-difference model on 514 succession events for Swedish listed firms in the period 2000-2017, we examine this interaction and demonstrate its significance for the understanding of post-succession performance changes. Notably, we find strong empirical evidence that forced CEO departures followed by outsider CEO successors yield significant positive change in operating firm performance. Also, we verify that this effect can be explained neither by the nature of the departure nor by the successor origin alone. These are important findings that help shift the analysis from the separately treated dismissal and successor events to the full turnover event.

Keywords:

CEO Succession, Forced Turnover, Outside Successor, Operating Performance

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1. Introduction

A chief executive officer (CEO) is arguably the most important individual in a company, with the ultimate decision-making power and a strong connection to external stakeholders' perception of the firm. Changing the CEO is one of the most critical events in the lifespan of a firm and may cause major impact on various stakeholders. It may be costly and bring internal turmoil, while finding the right replacement can bring great upside. Therefore, much attention has been devoted to understanding the underlying reasons behind, and financial implications of, various types of CEO successions.

There is broad consensus that the probability of executive succession is inversely related to firm performance in terms of both stock returns and accounting performance measures (e.g. Weisbach, 1988; Fee & Hadlock, 2004; Huson et al., 2004). In other words, bad performance increases the likelihood that a CEO is replaced. How the market reacts to news of CEO succession has received considerable attention in the literature (e.g. Reinganum, 1985; Bonnier & Bruner, 1989; Khanna & Poulsen 1995; Adams & Mansi, 2009). Although findings vary, such studies have in common that they focus predominantly on the expectations of investors, as opposed to the actual operating performance of the company, and one does not necessarily reflect the other. The amount of research on operating performance implications from CEO successions is less extensive and exhibits contradicting results. While some studies focus on post-succession firm performance for CEO changes in general (see e.g. Fee & Hadlock, 2004), others aim to go one step deeper and distinguish effects connected to the characteristics of the change. Whether or not there is a distinction in subsequent firm performance arising from the nature of departure, i.e. if a CEO is forced from the position or leaves voluntarily, has not been unanimously concluded (see e.g. Schepker et al., 2017). Similarly, there are discrepancies in the empirics regarding how firm performance is affected by the appointment of a new CEO from inside or outside the firm (see e.g. Hutzschenreuter et al., 2012). Hence, based on the literature to date, it is difficult to draw any clear conclusions concerning how the nature of the CEO departure and the origin of the CEO successor affects operating performance.

Furthermore, prevailing theory typically treat these factors separately, instead of aiming at understanding how outsider versus insider origin of CEO successive recruitment *combined* with the nature of CEO departure impact firm performance. A view of the interconnected effect of the CEO background with the type of departure would be much needed to build deeper insight into when CEO successions create, or destroy, value.

We suggest that if a CEO has been forced to leave, the desire or need for change in the organisation ought to be greater than if the CEO left on a voluntary basis. We hypothesize that when a CEO has been forced to leave the position, an outside recruited CEO will have greater potential to drive the required organisational change. Ultimately, the following broader research question is the focus of our study:

> *Can post-succession operating performance of a firm be influenced by the nature of the CEO departure in combination with the origin of the CEO successor?*

In this study, we provide new empirical evidence contributing to a deeper understanding of the dynamics in place when firms experience a CEO succession. The new evidence reveals the significance of capturing the interconnectedness between the nature of CEO departures and the origin of the CEO successor for the understanding of post-succession performance changes. Our findings and approach illustrate the benefits of trying to capture interactions of the broader system dynamics rather than investigating observed phenomena in isolation. All in all, we hope our study helps shift the analysis from the separately treated dismissal and successor events to the full turnover event.

1.1. Contribution

Our intention is to contribute to existing literature in four ways. First, understanding the impact of CEO succession on a firm's subsequent operational performance yields significant implications for different stakeholders, including a firm's board of directors and potential investors. The discrepancies in the current research on the area bring purpose for further investigation. By examining forced versus voluntary CEO departures and inside versus outside CEO appointments in a combined manner, we aim to provide some additional explanatory components to the field. Second, while a large extent of the prior research has focused on US data (e.g. Huson et al., 2004; Barron et al., 2011; Jalal & Prezas., 2012), we explicitly aim to increase the understanding of the subject through examining the Swedish setting. To our knowledge, post-succession firm performance in this setting has received little attention as of now. Third, we use more recent data and investigate CEO succession and subsequent firm performance throughout a longer time span than most prevailing research. This allows us to contribute to the subject by providing further insight on whether or not there is a distinction in the effect on firm performance following CEO succession in a longer and more up-to-date period. Finally, in contrast to several studies (e.g. Denis & Denis, 1995; Huson et al., 2004), we

examine firm performance changes using five separate time specifications, potentially allowing for stronger inferences.

1.2. Delimitation

We limit our study to CEO successions, in the period 2000-2017, for Swedish firms that were listed on the Nasdaq OMX Nordic Stockholm during the succession event. A reason for the decision to investigate public companies is that they are obliged to comply with stricter rules and regulations than private firms and firms listed on OTC markets, increasing the comparability between them. Moreover, public firms tend to exhibit more transparency in regard to CEO succession events than private firms, e.g. through press releases and media coverage. This enables us to more easily identify the reasons behind a succession event and the origin of the replacement CEO.

1.3. Definitions

The term CEO succession is used in the literature to describe both voluntary and forced turnover. The terms turnover and succession are used interchangeably in this paper. An insider appointment refers to a CEO successor who has been with the firm for more than 6 months prior to CEO appointment, while an outsider appointment refers to all other successors. Moreover, we refer to the event of a voluntary CEO departure followed by an insider appointment as a *voluntary inside succession*, the event of a voluntary CEO departure followed by an outsider appointment as a *voluntary outside succession*, the event of a forced CEO departure followed by an insider appointment as a *forced inside succession*, and the event of a forced CEO departure followed by an outsider appointment as a *forced outside succession*.

1.4. Disposition

The study is divided into nine sections. Section 2 provides a review of previous literature and the theoretical framework that connects to the study. Section 3 connects the theories in use and the findings of prevailing research in order to develop and present the hypotheses. Section 4 describes the methodology by exhibiting how the data is processed and how the study is designed. Section 5 presents the descriptive statistics and the results from the regression models. Section 6 contains the analysis and discussion of the empirical results. Section 7 provides the conclusion. Section 8 discusses potential limitations of the study. Lastly, section 9 gives suggestions for future research.

2. Literature Review and Theoretical Framework

This section presents the previous literature and theories on which we base our study. First, several studies regarding executive succession and subsequent firm performance are presented in order to provide an understanding of the consistencies and discrepancies that prevail in the literature. Second, an assessment of related theories is exhibited and connected with empirical findings, in order to establish the foundation on which our hypotheses are developed.

2.1. Description of Previous Findings

Numerous studies seek to investigate the performance effects arising from CEO successions. However, little consensus on the subject prevails in the current state of the literature.

There are discrepancies in the empirical findings on how a change in CEO affects subsequent firm performance. Denis and Denis (1995) examine unadjusted and industry-adjusted changes in operating return on assets (OROA) following top management departures for 908 non-takeover related successions over the period 1985 to 1988 in the US. They find that top management changes in general are followed by significant increases in operating performance. Huson et al. (2004) contribute to the literature by investigating unadjusted, industry-adjusted and control group-adjusted changes in OROA following 1,344 CEO turnover events at public firms from 1971-1994 in the US setting. They conclude that CEO succession positively affects subsequent firm performance. Based on a meta-analysis of 13,578 events from 1972-2013, Schepker et al. (2017) present conflicting findings. Specifically, they argue that CEO succession has a negative effect on firm performance in the short-run and has no significant impact on firm performance in the long-run.

From distinguishing forced from voluntary successions, Denis and Denis (1995) conclude that both types of succession enhance operating performance, but that the positive effect is substantially larger for forced than voluntary successions. In contrast, Huson et al. (2004) and Schepker et al. (2017) find no significant difference in post-succession firm performance when comparing forced versus voluntary CEO successions. There is also a lack of consensus on how the origin of a successor affects post-succession firm performance. Huson et al (2004) exhibit empirical evidence that the employment of outside CEOs, i.e. successors recruited from outside the firm, is more related to firm performance than the employment of inside successors. However, Shen and Cannella (2002) and Schepker et al. (2017) provide the opposite

conclusions. Zhang and Rajagopalan (2010) contribute to the discussion by presenting evidence that outsider succession has a complex non-linear relationship with firm performance.

Importantly, a succession event encompasses both the CEO departure and the appointment of a successor. While these processes are often handled independently, we argue that they are in fact intertwined and therefore should be analysed as such. To our knowledge, limited research on the interaction between these processes exists in the current state of the literature. Therefore, the following section discusses several underlying arguments, theories and empirical findings that may facilitate the understanding of the dynamics of CEO departures and appointments. These considerations provide the foundation on which we form our hypotheses.

2.2. Theoretical Framework

2.2.1. Changing the CEO

The role of a CEO is associated with a significant level of influence over a firm's corporate decisions and policies. This influence is found to have increased in substance and perception in recent years (Hambrick & Quigley, 2014; Quigley, Crossland & Campbell, 2017). The nature of the CEO position and the accompanying impact on key decisions and policies within an organization raise questions regarding the effect on firm performance originating from changing the CEO.

The Disruptive Perspective

The disruptive perspective suggests that changing the CEO is coupled with substantial disturbances for firms, inducing negative effects on subsequent performance (Boeker, 1992; Schepker et al., 2017). Specifically, disruption theorists argue that turnover events are associated with the disruption of routines and relationships, resulting in organizational instability and impaired connections with external parties (Boeker, 1992; Ballinger & Marcel, 2010). The firm-specific human capital and comprehension of the firm's activities that the departing CEO has acquired over time is lost in connection with the succession event (Greiner et al., 2003). Consequently, the new CEO is obliged to engage in acquiring such firm-specific knowledge, including an understanding of the dynamics of procedures and resources within the organization and the role played by external stakeholders (Karaevli, 2007).

The Adaptation Perspective

The adaptation perspective suggests that a CEO turnover event is associated with opportunities of readapting strategies and organizational resources to match the external environment (Shen & Cho, 2005). It is argued that a new CEO brings different experiences and perspectives to a firm, facilitating the realignment of strategic endeavours (Haveman et al., 2001; Shen & Cho, 2005). The potential benefit in firm performance from a CEO succession and the subsequent adaptation to the external environment is projected to grow over time, as a CEO becomes accustomed to the new position and organizational context (Hambrick & Fukutomi, 1991).

2.2.2. Forced versus Voluntary Successions

The underlying motives that initiate CEO succession can be of different character. Therefore, it can be argued that identifying factors contributing to the forced or voluntary departure of a CEO facilitates the understanding of how different types of departures may relate to subsequent firm performance.

Forced Successions

The Scapegoat Perspective

The scapegoat perspective, which is connected to the agency theories of Holmström (1979), Shavell (1979), and Mirrlees (1976), refers to the action of attributing the failures of one party to another party. The theory suggests that all managers are equal in terms of managerial quality. Therefore, the theory argues that poor firm performance is a consequence of misfortune, which is not attributable to the manager (Huson et al., 2004). Hence, managers who are forced to leave their positions are used as scapegoats. The concept implies that candidates for replacement are of equal quality to the departing manager and have the same impact on firm performance. Since forced managerial succession is assumed to arise from bad luck, the theory argues that the expected change in performance succeeding a turnover is positive due to mean-reversion of the component of chance (Huson et al., 2004).

The Improved Management Perspective

Contrarily to the scapegoat perspective, the concept of improved management following forced succession suggests that managers do differ in quality and that such disparities affect firm performance. In Kim's (1996) article on long-term firm performance and chief executive turnover, firm performance is assumed to be determined by the aggregate of a chief executive's

ability to manage the firm and a random factor originating from chance. Huson et al. (2004) correspondingly incorporate this view. An underlying idea is that the prevailing differences in managerial quality are not directly observable, but boards tend to use realized firm performance as a proxy for determining the quality of a manager. If a firm experiences poor performance, it is assumed that the current CEO is of low quality and that the estimated advantages of replacing that person exceeds the associated costs. As previously mentioned, firm performance is assumed to also be affected by the component of chance. Consequently, firm performance is expected to improve post-succession as a result of enhanced managerial quality and mean-reversion due the component of chance. That firms have a tendency to fire executive managers following poor performance has considerable empirical support (e.g. Warner et al., 1988; Fee & Hadlock, 2004; Huson et al., 2004). Hutzschenreuter et al. (2012) contribute to the discussion, arguing that forced CEO succession is a sign that the board desires strategic change. Evidence regarding the occurrence of abnormal policy changes following forced managerial succession has been documented (e.g. Murphy & Zimmerman, 1993; Denis & Denis, 1995; Weisbach, 1995; Wiersema, 1995).

The Prospect of Different Managerial Styles

Bearing the arguments from the scapegoat perspective and the improved management perspective in mind, questions are raised regarding whether or not the phenomenon of managerial quality exists and affects post-succession firm performance. Fee, Hadlock and Pierce (2013) shed further light on the area. They argue that executive turnovers can be highly endogenous events. Therefore, determining whether or not managerial style affects the policies and the performance of a firm is a complex task. The authors first investigate the possibility that there are causal managerial-style effects that are idiosyncratic and not fully anticipated, observed and controlled by the board. From examining exogenous CEO successions, i.e. resulting from health events, death and natural retirement, the authors find no evidence of abnormal policy changes. Thereby, they argue that it is improbable that a firm's investment decisions or policies originate from random or unanticipated differences in managerial traits or styles. This can be interpreted as an argument against the phenomenon of managerial quality differences. However, they also dig deeper into the subject of forced CEO departures and whether managerial traits and styles affect the outcome, by aiming to address the endogeneity. Specifically, they find that corporations with a large supply of replacement candidates for a CEO position have a tendency to exhibit greater shifts in corporate policies following forced succession. Consequently, the authors argue for the presence of causal managerial style effects

that are anticipated and intentionally selected by the directors in order to induce the firm to move in a certain direction. This supports the concept that managers actually do differ in quality, and that these differences are observable and taken into account in the context of CEO succession.

Voluntary Successions

In contrast to forced departures, voluntary executive succession in itself does not suggest that the board is dissatisfied with the current CEO or desires strategic change (Hutzschenreuter et al., 2012; Schepker et al., 2017). Furthermore, the turnover event is not necessarily connected to preceding performance, possibly arising from managerial quality and/or chance. Instead, a voluntary CEO succession may originate from normal retirement, health reasons or the acceptance of another position (Huson et al., 2004).

As voluntary turnover often is associated with persistence in corporate strategy (Wiersema, 1995), questions are raised regarding the aftermath of voluntary succession in terms of firm performance. Huson et al. (2004) argue that a firm's directors will aim to appoint the available replacement candidate with the highest estimated quality, but the managerial quality of the best replacement candidate may not exceed that of the departing CEO, making the firm worse off. Fee, Hadlock and Pierce (2013) contribute to the discussion by casting doubt on the possibility that random, unanticipated variation in the distribution of managerial style amongst replacement candidates plays a causal role in a firm's policy choices and subsequent performance. Moreover, from a disruptive perspective, the nature of voluntary succession is arguably less challenging for firms than forced succession. Voluntary succession events are often accompanied by a transition period, in which knowledge sharing can take place between the departing and arriving CEO, decreasing the disruption component associated with turnovers (Vancil, 1987).

2.2.3. Outside versus Inside Successions

When appointing a successor to a departing CEO, the abilities of the available candidates, and how well they are suited for the firm's competitive situation, are taken into consideration (Parrino, 1997). Whether the successor is an insider or an outsider can play an important role in the context of managerial actions (Hutzschenreuter et al., 2012).

Outside Successions

Outside CEO candidates, by nature of their employment at other corporations, have been exposed to a different range of business and strategy related issues than inside candidates. Consequently, an outside CEO candidate may possess more experience and deeper comprehension concerning alternative ways of managing a firm than an inside candidate (Parrino, 1997). Virany et al. (1992) and Zhang and Rajagopalan (2004) contribute to the discussion by arguing that an outside CEO often brings new strategic perceptions and ties to the environment to a new firm. Quigley and Hambrick (2014) further suggest that outside CEOs have more discretion, allowing greater autonomy in inducting and implementing strategic change. Therefore, the prospect of appointing an outside CEO can be connected to a desire to incorporate novel managerial perspectives, i.e. changing firm strategies or policies. Poor firm performance often leads to a desire for change, a context in which outside CEOs become more attractive candidates (Parrino, 1997). Accordingly, outside CEO succession has been empirically connected to an increased level of organizational change (Helmich & Brown, 1972; Hambrick & Mason, 1984; Schepker et al., 2017).

However, the appointment of an outside CEO can be connected with higher costs than an inside option. Helfat and Bailey (2005) argue that hiring a CEO from another corporation implies greater disruption, more uncertainty and higher transition costs than promoting an inside CEO. Moreover, the absence of firm-specific knowledge and the potential resistance of remaining members of the top management team can put outside CEOs at a disadvantage (Barron et al., 2011). Since outside succession is associated with higher costs than inside succession, outside CEO candidates will not be selected if they are merely marginally superior in quality to inside candidates, hence outsiders are regularly handicapped in the process of succession (Agrawal et al., 2006).

Inside Successions

In contrast to the appointment of an outside CEO, inside succession is found to be connected with strategic continuity (Friedman & Olk, 1995; Lauterbach et al., 1999; Schepker et al., 2017). The decision to promote an inside candidate to a CEO position may reflect the preference for a CEO who is more proficient in implementing a firm's prevailing policies than another who may be better suited to change the strategic direction of the firm. Parrino (1997) argues that candidates for inside CEO succession, in particular those with extensive experience from working at the firm, are more qualified for a CEO position if the mandate is to maintain the

status quo. An underlying basis for that argument is that inside CEO candidates have a deeper comprehension of the firm's existing policies as a result from having helped develop or implement these polices in their current positions or as lower-level managers. Insiders are also likely to possess firm-specific human capital in terms of knowledge of internal processes and understanding of the firm's position in the competitive environment (Parrino, 1997). Schepker et al. (2017) contribute to the discussion by arguing that inside CEO candidates have a superior cognitive perception in regard to the firm's strategies and policies than outside candidates, enabling them to leverage resources without engaging in strategic change. Strong preceding firm performance promotes continuity in strategic endeavours, a context in which inside CEO candidates are arguably more attractive than outside candidates.

As previously brought to light, the disruptive effects from managerial succession tends to be less substantial for inside candidates, compared to outside ones. Specifically, Helfat and Bailey (2005) hold that inside succession implies less uncertainty and lower transition costs than outside options. Moreover, maintaining the assumption that inside CEOs tend to engage in less strategic change, Schepker et al. (2017) discuss that they are less likely to reconfigure resources and disrupt well-functioning relationships than outside candidates.

3. Hypotheses

This section combines the previously discussed theoretical arguments with the related empirics in order to develop and articulate our hypotheses.

A CEO has a significant influence over a firm's corporate decisions and policies (Hambrick & Quigley, 2014; Quigley et al., 2016). Disruption theorists claim that changing the CEO is coupled with substantial disturbances for firms, including the loss of firm-specific human capital and the disruption of routines and relationships, resulting in organizational instability and impaired connections with external parties (Greiner et al., 2003; Ballinger & Marcel, 2010; Schepker et al., 2017). Adaptation theorists argue that a new CEO brings different experiences and perspectives to a firm, which serves as an opportunity to readapt strategies and resources to match the external environment (Haveman et al., 2001; Shen & Cho, 2005). Empirical evidence exhibits varying results. Denis and Denis (1995) find that top management succession positively affects firm performance. Huson et al. (2004) come to the same conclusion regarding CEOs. However, through performing a meta-analysis, Schepker et al. (2017) find that CEO successions have a negative effect in the short-run and has no effect in the long-run. From

grasping the discrepancies of previous literature, questions are raised to whether post-succession firm performance instead is influenced by the nature of the CEO departure and the origin of the CEO successor.

A CEO departure is either forced or voluntary and the successor is either appointed from inside or outside the firm. Forced succession is found to be preceded by poor firm performance (e.g. Weisbach, 1988; Fee & Hadlock, 2004; Huson et al., 2004) and connected to the initiation of policy changes (e.g. Murphy & Zimmerman, 1993; Weisbach, 1995; Wiersema, 1995). Contrarily, voluntary succession is not necessarily coupled with poor performance or a desire for change (Hutzschenreuter et al., 2012; Schepker et al., 2017). Given the CEO's influence over a firm's decisions and policies (Hambrick & Quigley, 2014; Quigley et al., 2016), it can be argued that the increased likelihood for policy changes following forced succession can be linked to the CEO, thereby opposing the scapegoat perspective. However, the view of the improved management perspective, namely, that firm performance is determined by the aggregate of managerial quality and a component of chance (Kim, 1996; Huson et al., 2004) is not necessarily accurate either. In line with Fee, Hadlock and Pierce (2013), we argue that it is improbable that a firm's investment decisions or policies originate from random or unpredictable differences in managerial quality. Instead, a firm's investment decisions or policies may be affected by causal managerial style effects that can be predicted and intentionally selected by the board of directors in order to induce the firm to move in a certain direction (Fee et al., 2013).

Therefore, the capabilities and managerial style of a CEO successor should reflect the desired direction of the firm. By virtue of their employment at other corporations, outside CEOs may possess a greater understanding of alternative ways of managing a firm than inside candidates (Parrino, 1997). Since outside candidates often bring new strategic perceptions, the prospect of appointing an outside CEO can be connected to a desire for changing firm strategies or policies (Virany et al., 1992; Zhang & Rajagopalan, 2004; Quigley & Hambrick, 2014). As previously discussed, such a desire for change tends to arise from poor previous performance (Parrino, 1997) and outside succession has been shown to increase the level of organizational change (Helmich & Brown, 1972; Hambrick & Mason, 1984; Schepker et al., 2017). In contrast, inside succession may reflect the preference of a CEO who is more competent at implementing a firm's current policies, i.e. maintaining the status quo when things are going well (Lauterbach et al., 1999; Barron et al., 2011; Schepker et al., 2017).

Much of the prevailing research treat forced versus voluntary succession and inside versus outside CEO appointments separately. Denis and Denis (1995) find that forced succession improves firm performance, whereas Huson et al. (2004) and Schepker et al. (2017) present no significant differences. Moreover, Huson et al. (2004) find that outside CEO succession leads to increased firm performance, whereas Shen and Cannella (2002) and Schepker et al. (2017) present the opposite conclusions. Zhang and Rajagopalan (2010) contribute to the discussion by presenting evidence that outsider succession has a complex non-linear relationship with firm performance.

We believe that combining these factors, thereby forming four different succession scenarios, provides additional insight to the subject, possibly adding some explanatory components to the discrepancies in the prevailing literature. Voluntary successions are associated with a desire for strategic continuity, whereas forced successions are associated with a desire for strategic change. Also, potential disruption effects are less extensive for voluntary departures than forced departures (Vancil, 1987). Conversely, inside CEO appointments are linked to proficient implementation of a firm's current policies, whereas outside CEO appointments are linked to the implementation of new policies. Also, potential disruption effects are less extensive for inside CEO appointments than outsides ones (Helfat & Bailey, 2005; Schepker et al., 2017). Intuitively, these scenarios yield different effects on operating performance. Arguably, voluntary CEO departures followed by inside successors imply little effect on firm performance due to strategic continuity and merely slight disruption. However, forced CEO departures followed by inside successors may yield a slight negative impact on performance, arising from an enlarged disruptive effect. Moreover, it can be argued that the appointment of an outside CEO successor affects firm performance due to strategic change and the component of disruption. For voluntary successions, this may imply a negative effect on firm performance. However, positive effects on subsequent operating performance may be distinguished in the context of forced CEO departures. Bearing these scenarios in mind, we formulate our hypotheses as follows:

H1: Voluntary CEO departures followed by the appointments of inside successors are not related to subsequent changes in operating performance.

H2: Voluntary CEO departures followed by the appointments of outside successors are negatively related to subsequent changes in operating performance.

H3: Forced CEO departures followed by the appointments of inside successors are negatively related to subsequent changes in operating performance.

H4: Forced CEO departures followed by the appointments of outside successors are positively related to subsequent changes in operating performance.

N.B. H1 constitutes a null hypothesis while H2, H3, and H4 are stated as alternative hypotheses and tested against their corresponding non-correlation null hypothesis.

4. Methodology

This section introduces the collection of data and the following turnover sample construction. Moreover, we detail the procedure for succession classification and introduce the difference-in-difference model design.

4.1. Turnover Sample

To construct the CEO turnover sample, we obtain a dataset of press releases from publicly listed Swedish firms from Capital IQ. The dataset consists of 709 CEO succession events from 2000 through 2017. 250 events from firms listed at the time of succession but delisted during the period, hence not included in the Capital IQ database, are manually added to the dataset. The effect of this is twofold. First, it increases the sample size considerably, second, it reduces the likelihood of a significant survivorship bias on the firm level. Financial firms are excluded as they have a fundamentally different business structure, reporting requirements, and regulatory environment, and hence could distort comparisons. This excludes 53 successions from the sample. Thereafter, we process the press releases in the ways outlined below, to allow for a correct classification.

First, press releases describing a chain of events pertaining to the same succession are identified. In order to isolate unique successions, the departing CEO, the successor CEO and the potential temporary CEO are identified. In accordance with Huson et al. (2004), the CEO is defined as temporary if his/her time in office does not exceed 6 months. This excludes 223 events from the sample.

Second, the announcement date is distinguished from the date the departing CEO left office, and the date the new CEO took over (i.e. the effective date) in order to facilitate the categorization process.

This processing ultimately yields a turnover sample of 683. Next, each succession is categorized as being either forced or voluntary and followed by either an insider or an outsider.

4.2. Categorization

Using the constructed sample of Capital IQ press releases, complemented with a review of business and trade press, each CEO succession is classified in regard to whether it was forced or voluntary, and whether the successor was an insider or outsider.

4.2.1. Forced or Voluntary Departures

First, each turnover event is classified as either forced or voluntary. As acknowledged in previous literature (Warner et al., 1988; Weisbach, 1988; Denis & Denis, 1995), identification of forced departures is difficult in nature since press releases often do not describe them as such. For instance, an announced retirement may in reality be a forced succession. However, a press release stating a turnover as forced or due to poor performance is strong evidence that the departure is indeed forced.

In order to systematically classify the different turnover events in the dataset, an approach based on the methodology of several previous studies is followed. The overarching framework developed by Parrino (1997), and later refined by Huson et al. (2004), is used together with a more conservative approach to forced turnovers, in line with Gao et al. (2012) and Warner et al. (1988). Specifically, we do not classify turnovers as forced solely on the basis of CEO age. We outline our classification scheme below.

If the press release reports that the CEO was fired, forced from the position, or departed due to policy differences, the turnover event is classified as forced. Additionally, CEO turnover events are classified as forced if any of the following conditions are met: (1) the departure was not announced at least six months in advance; (2) the announcement did not report the reason for the departure as death, poor health, or the acceptance of another position (elsewhere or within the firm); (3) the reason for the departure was not retirement; (4) the CEO did not serve on the firm's board of directors after leaving the CEO position.

Press releases with limited information in regard to the above criteria, are tentatively classified as forced. These tentatively classified forced turnovers are investigated further through searching the business and trade press for relevant articles to reduce the incidence of classification errors. These turnover events are reclassified as voluntary if the CEO took a

comparable position elsewhere or departed for personal or business reasons that were unrelated to the firm's activities. Ultimately, 241 events (47% of the sample) are classified as due to a forced turnover, while the remaining 273 events (53%) are classified as due to voluntary turnover.

As a consequence of the inherently difficult task of clearly distinguishing internal disciplining efforts from voluntary successions, we recognize that the above classification scheme may in some cases label normal retirement as forced or vice versa. Also, we acknowledge that no distinct line can be drawn in all situations. To address this issue, 62 events (12%) with a high degree of uncertainty are labelled as such to allow for a sensitivity analysis as a robustness test.

4.2.2. Insider or Outsider Successors

The incoming CEOs are classified as either insider or outsider successors. In an insider turnover event, the new CEO previously had either an operative role within the firm or was serving on the board of directors prior to the new position (e.g., Graffin et al., 2011; Schepker et al., 2017). On the contrary, turnover events where the new CEO had no previous engagement with the firm are classified as outside (e.g., Friedman & Singh, 1989; Graffin et al., 2011; Schepker et al., 2017). The 14 successions lacking sufficient information are excluded. Ultimately, 263 (51%) events are classified as followed by an outside successor, while the remaining 251 (49%) events are classified as followed by an inside successor.

4.3. Final Sample

Data on firm characteristics (company ID, company name, industry code, fiscal year, currency, and number of employees) and accounting measures (total assets, total equity, total debt, EBIT, net income, and sales) are obtained from Standard & Poor's Compustat database and matched to the turnover sample on firm-year level. These data are used to compute ratios of earnings before interest and taxes to book assets (OROA) and net income to book assets (ROA). The constructed sample is subsequently subject to the screening criteria outlined below, in order to have relevant and comparable data.

First, successions directly related to a merger, an acquisition, or other event significantly altering the company structure, are excluded in line with Huson et al. (2004). This reduces the sample by 59, leaving 610 events.

Second, successions lacking required accounting and financial data in the Compustat database are excluded. This reduces the sample by 96 events.

The final sample consists of unbalanced panel data with 514 CEO succession events from 307 firms, and a total of 4,399 firm-year observations. We use firm-years with no CEO turnover during this period as our control group, 3,885 firm-year observations, and refer to this as the no turnover sample. Full sample selection adjustments are presented in Table 1.

Table 1. Sample selection

	Adjustment	# of observations
Capital IQ press releases		709
Plus: Manually added press releases	250	959
Less: Financial firms	53	906
Less: Duplicates	223	683
Less: No data on successor	14	669
Less: Related to M&A	59	610
Less: No financial data	96	514
Final turnover sample		**514**
% forced turnovers		*46.9%*
% voluntary turnovers		*53.1%*
% outside successors		*51.2%*
% inside successors		*48.8%*

Note: 62 turnovers are labelled as uncertain in regard to the forced/voluntary classification

4.4. Difference-in-Difference Technique

To empirically investigate the performance effect associated with different succession types, we conduct performance matched difference-in-difference tests. Although not a perfect substitute for randomized experiments, the DID design represents a feasible way to learn about casual relationships. As a consequence of conducting an observational study, biases due to non-randomisation will by definition affect our results.

First, to mitigate concern that the turnover firms' performance changes as a result of mean reversion (or other confounding changes) rather than as an effect of the succession, we construct a sample of index firms. To ensure that the treatment and control firms have a similar basis for performance, in line with the important DID assumption of parallel trends, we match these firms on the most important predictors of future performance according to prior research (Barber and Lyon, 1996), detailed below. Second, performance change outliers are excluded, in accordance with Huson et al. (2004).

4.4.1. Pre-succession Performance Matching

To mitigate the risk of violating the important assumption of parallel paths underlying the difference-in-difference model, we benchmark the performance of our treatment group against a hypothetical non-treatment group. Through establishing a counterfactual, this quasi-experimental research design allows us to better study potential causal relationships, despite the fundamental challenge of selection bias in a non-experimental setting. The identifying assumption in our regression is that the treatment sample and the control sample have parallel trends in performance. To satisfy this assumption, the control group is generated using the performance-based control group matching method, as described by Barber and Lyon (1996), and takes both industry and pre-turnover performance into account. Hence, the issue of mean reversion, thoroughly documented in previous studies (e.g. Fama and French, 1988; Penman, 1991), is addressed.

Mean reversion in accounting data reflects a transitory component of operating income. Underlying economic forces, such as nonrecurring revenue and costs, temporary shifts in demand or accounting manipulation could all possibly affect the transitory component. As the temporary component dissipates, OROA will revert toward a population mean. To illustrate, a poorly performing firm firing their CEO can experience a rebound in performance as a mere consequence of the accounting measure reverting back to its mean in a predictable fashion. Not controlling for this might lead a researcher to conclude that the firm is in fact experiencing a positive effect from the managerial turnover. Furthermore, some of the cross-sectional variation in performance are assumed to be explained by an industry benchmark.

To control for both the performance-based selection bias and the industry effects, sample firms are matched to firms of similar pre-event performance in the same, or similar, industry. Specifically, the adjustment is performed using the method first proposed by Barber and Lyon (1996) and recognised by several papers since (e.g. Huson et al., 2004).

Each sample firm is matched to comparison firms with the same two-digit Compustat SIC code whose performance measures in the year prior to the turnover are within 90%-110% of the sample firm's performance in the corresponding year. For instance, if firm *i* has a pre-succession OROA of 7%, the firm is matched to firms with the same two-digit industry code and an OROA between 6.3% and 7.7% in the year prior to their hypothetical non-turnover event.

If there are no such firms, we use an alternative rule in two steps. First, we match performance within the ±10% filter using all firms with the same *one-digit SIC code*. For firms without matches after this procedure, we use all firms with performance within the filter bounds *regardless of SIC code*. The four succession events without matches at this stage are excluded. Each sample firm's performance is then adjusted by subtracting the median performance of its control group.

Although the alternative rules are not theoretically based, they are performed for two purposes. First, not using alternative rules would force us to discard events without matches strictly adhering to the criteria. As a consequence, the sample would be biased; excluded firms tend to be unusually well, or poorly, performing the year prior to the event. Second, we would be limited in comparing results between different adjustment methods, since the samples would be drawn from different populations.

The obvious trade-off in choosing either two-digit or full four-digit matching is that a control group would need to be significantly larger to allow for four-digit matching. Our choice is based on the several studies (e.g. Clarke, 1989) that have shown that the two-digit definition captures similarities among firms as efficiently as three- or four-digit classifications.

4.4.2. Outlier Adjustment

In line with Huson et al (2004), extreme performance observations are excluded as a measure of eliminating substantial firm events not related to managerial successions. If the performance measure differs in absolute value from the mean by more than three times the standard deviation of the respective distribution, the observation is excluded from the sample. This reduces the number of observations by 36, of which 21 are positive extremes, and 15 are negative extremes. We acknowledge that this filter can exclude relevant data points and thus impact the results and significance of the study. However, the excluded observations are equally distributed across the subsamples; 7 voluntary inside successions, 9 voluntary outside successions, 11 forced inside successions, and 9 forced outside successions. Without this screen, the empirical findings are qualitatively similar to those reported below.

4.5. Model Design

In order to assess the effect of different types of CEO successions on operational performance, we establish a difference-in-difference model according to the following specification:

$$\Delta PERFORMANCE_{i,t} = \alpha + \beta_1 FORCED_i + \beta_2 OUTSIDE_i + \beta_3 FORCED_i * OUTSIDE_i + \beta_4 SIZE_i + Year\ Fixed\ Effects + \varepsilon \quad (1)$$

Where $\Delta PERFORMANCE_{i,t}$ refers to total change in firm performance from the year prior to the CEO turnover to t years after the turnover, t = 1, 2, 3, 4, and 5, and i denotes firm-year observation. $FORCED_i$ refers to a dummy variable equalling one for forced successions and zero for voluntary successions, $OUTSIDE_i$ refers to a dummy variable equalling one for outside successors and zero for inside successors, $FORCED_i$*$OUTSIDE_i$ refers to an interaction dummy variable equalling one for forced successions followed by outside successors and zero if not, $SIZE_i$ refers to the logarithm of total sales, and *Year Fixed Effects* refers to the adjustment for time-invariant characteristics. α denotes the intercept, β_i denotes the coefficients and ε denotes the error term. In accordance with Petersen (2009), all standard errors are clustered on a firm level to account for over-time correlation within the same firm.

4.5.1. Dependent Variable

Change in performance ($\Delta PERFORMANCE_{i,t}$): Change in performance is estimated using operating return on assets (OROA). OROA is measured as the ratio of accounting earnings before interest and taxes (EBIT) to one-year lagged book assets and adjusted according to the performance and industry matching procedure outlined in section 4.4.1.

The change is calculated by subtracting performance in the year prior to the succession (t = -1) from performance t years after the event. We exclude the first year the CEO is in office (t = 0) since the overlap between the departing and successor CEO in that year is likely to make measured performance an especially noisy signal of effect attributed to the new CEO (Huson et al., 2001).

Performance change is measured separately one, two, three, four, and five years post turnover, to be able to capture and analyse differences in effect over different time intervals. Since data needs to be available for the entire period being measured, only surviving firms and CEOs are

included. Hence, facing the same issue as previous literature, our sample is inevitably biased, and we will remain cautious as to draw conclusions on single indications. Nevertheless, we question the rationale in previous studies, where comparisons are only made between $t = -1$ and $t = 3$ and consider our approach more robust in terms of accounting for this selection bias.

Previous research suggests that a three-year time frame between CEO succession and performance is needed to fully capture the effects (e.g., Datta & Rajagopalan, 1998; Karaevli, 2007). The studies done by Denis and Denis (1995) and Huson et al. (2004) both compare performance in the year prior to the succession with performance three years after the event, including only firms and CEOs that survived the entire period. Hence, we find it highly interesting to investigate the effect on performance for different time specifications, since it clearly has an impact on the sample characteristics. Also, it will serve as a robustness test addressing the survivorship bias.

We acknowledge that the profitability measure used has its drawbacks, namely, (1) that assets as a balance sheet item are recorded at historic cost, while operating income is recorded in current value, (2) that assets do not only reflect operating assets, likely understating true productivity on operating assets, and (3) that managers can manipulate earnings using discretionary accruals (Barber and Lyon, 1996). However, our specific research question motivates the use of accrual-based, rather than cash-based, performance measures.

The importance of the change in performance definition validates the use of different specifications as robustness checks. This will be further examined in section 5.4.2.

4.5.2. Main Independent Variables

Forced dummy ($FORCED_i$): A dummy assigned the value of one (zero) if the succession is categorized as forced (voluntary) is used to capture the effect of the turnover nature on subsequent operating performance.

Outside dummy ($OUTSIDE_i$): A dummy assigned the value of one (zero) if the successor CEO is recruited from outside (inside) the firm is used to capture the effect of successor origin on subsequent operating performance.

*Forced-outside interaction dummy ($FORCED_i$ * $OUTSIDE_i$):* An interaction variable taking the value of one if the succession is categorized as forced and the replacement CEO is from outside the firm is used to capture interaction effect of the variables. The interaction term is a prerequisite to obtain the difference-in-difference estimates needed to answer our research

question. We expect forced resignations to be associated with outside appointments and therefore have a non-random distribution (Denis & Denis, 1995). As we have, a priori, reason to suspect that there is an interaction, the model interpretation would be unclear without it.

4.5.3. Control Variables

Control variables are chosen with three considerations in mind. First, we limit ourselves to control variables used in previous management turnover studies. Second, we leave out control variables addressed in other parts of the model. For instance, part of the sample selection problem (Heckman, 1979), addressed by Huson et al. (2004) through including Inverse Mill's Ratio (IML) as a control variable, is addressed by our control group-adjustment, albeit in a different manner. Third, we ignore variables where we lack sufficient data.

Firm size ($SIZE_i$): Firm size is defined as the natural logarithm of sales and is used to control for the firm size effect on operating performance, in line with previous literature. Also, we acknowledge the risk of performance measures for smaller firms mean-reverting more quickly than similar measures for larger firms (Fama and French, 1988). Firm size is included as a control to adjust for this cross-sectional variation. Since the distribution of sales for different firms vary considerably, the natural logarithm of sales is included instead of sales, in line with Huson et al (2004). Previous research suggests that there is a positive relation between firm size and operating performance. Hence, we expect this coefficient to be positive.

Year fixed effects: Year fixed effects are included to remove the effects of the time-invariant characteristics and address the concern that general time trends drive our results. For instance, a turnover prior to the 2008 crisis may exhibit different performance changes than a post-crisis turnover, regardless of managerial quality.

5. Empirical Results

This section first gives a presentation of the descriptive statistics for the performance variables over our different subsamples. Second, we present Pearson correlations between our variables and subsequently, the results of the regression models are exhibited. Finally, we present our robustness tests results, including tests for multicollinearity and heteroscedasticity, different performance definitions, and a sensitivity analysis for the turnover type classification.

5.1. Descriptive Statistics

Table 2 reports summary statistics for the full turnover sample, the voluntary turnover sample, the forced turnover sample, the insider succession sample, the outsider succession sample, and all its combinations. The summary statistics include the operating performance change over each of the different time specifications and provide preliminary insights on the performance implications of different types of turnovers.

Initially, we observe a significant difference between forced and voluntary successions for the t = 2, 3, 4, and 5 sample. However, we argue that this difference in mean change obscure important variation. If our assumption of interaction between departure type and successor origin is correctly placed, the observed effect is entirely conditional upon the specific proportions of outsider/insider turnovers in our sample. Hence, a different sample with different proportions of insider and outsider turnovers will yield a different average effect. Consequently, there are no scalable implications of this finding.

Further, the descriptive statistics give us an indication that there is a special effect arising from the dynamic between a forced turnover and an outsider successor. This signal would be completely overlooked by studying departure type and successor origin as separate processes. Specifically, the descriptive statistics suggest that forced outside successions are indeed positively associated with subsequent change in profitability for time specifications two, three, four and five years post succession. We recognize that the limited sample size makes this specific two-tailed t-test sensitive to a few outliers. Although confirmed using non-parametric Wilcoxon Sign Rank test on median values, the differences need to be tested in a multivariate regression with controls to find the actual implications.

Table 2. Descriptive statistics

	All turnovers					Voluntary turnovers					Forced turnovers				
	n	Mean	Median	SD	*t*-Statistic	n	Mean	Median	SD	*t*-Statistic	n	Mean	Median	SD	*t*-Statistic
Panel A: All turnovers															
$\Delta PERFORMANCE_{i,1}$	422	-0.0013	-0.0485	0.096	-0.27	234	-0.0064	-0.0041	0.091	-1.08	188	0.0051	0.0075	0.103	0.69
$\Delta PERFORMANCE_{i,2}$	331	0.0106	-0.0545	0.114	1.69*	183	-0.0016	-0.0591	0.106	-0.20	148	0.0256	0.0174	0.122	2.56**
$\Delta PERFORMANCE_{i,3}$	242	0.0010	-0.0581	0.123	0.13	143	-0.0198	-0.0625	0.113	-2.08**	99	0.0310	0.0272	0.130	2.38**
$\Delta PERFORMANCE_{i,4}$	189	0.0074	-0.0560	0.119	0.85	117	-0.0060	-0.0612	0.119	-0.54	72	0.0291	0.0324	0.117	2.11**
$\Delta PERFORMANCE_{i,5}$	133	0.0088	-0.0411	0.124	0.82	88	-0.0080	-0.0581	0.101	-0.75	45	0.0417	0.0231	0.155	1.80*
Panel B: Inside turnovers															
$\Delta PERFORMANCE_{i,1}$	206	-0.0012	0.0010	0.094	-0.18	119	0.0034	0.0002	0.085	0.44	87	-0.0075	0.0031	0.105	-0.66
$\Delta PERFORMANCE_{i,2}$	158	0.0046	-0.0038	0.111	0.53	92	0.0047	-0.0045	0.102	0.44	66	0.0046	0.0123	0.123	0.30
$\Delta PERFORMANCE_{i,3}$	127	-0.0026	-0.0050	0.131	-0.22	73	-0.0183	-0.0136	0.121	-1.29	54	0.0186	0.0203	0.142	0.96
$\Delta PERFORMANCE_{i,4}$	95	0.0054	-0.0031	0.102	0.52	55	0.0080	-0.0070	0.081	0.73	40	0.0019	0.0163	0.126	0.10
$\Delta PERFORMANCE_{i,5}$	67	0.0079	0.0217	0.107	0.61	43	0.0144	0.0269	0.081	1.17	24	-0.0037	0.0026	0.144	-0.13
Panel C: Outside turnovers															
$\Delta PERFORMANCE_{i,1}$	216	-0.0014	0.0027	0.099	-0.20	115	-0.0166	-0.0127	0.096	-1.85*	101	0.0160	0.0143	0.099	1.62
$\Delta PERFORMANCE_{i,2}$	173	0.0160	0.0116	0.116	1.80*	91	-0.0079	0.0004	0.110	-0.69	82	0.0425	0.0235	0.118	3.25***
$\Delta PERFORMANCE_{i,3}$	115	0.0050	0.0003	0.113	0.47	70	-0.0213	-0.0292	0.105	-1.69*	45	0.0459	0.0411	0.113	2.72***
$\Delta PERFORMANCE_{i,4}$	94	0.0093	0.0082	0.135	0.67	62	-0.0184	-0.0241	0.144	-1.00	32	0.0630	0.0493	0.096	3.71***
$\Delta PERFORMANCE_{i,5}$	66	0.0096	0.0010	0.140	0.56	45	-0.0295	-0.0339	0.114	-1.74*	21	0.0935	0.0504	0.155	2.76***

*Note: Although our hypotheses are directional, these t-statistics are obtained from two-tailed t-tests. This will decrease the possibility of finding significance and hence work against us. *** p<0.01, ** p<0.05, * p<0.1*

5.2. Pearson Correlations

To provide further insights of the data and recognize significant correlations between variables, Pearson correlations are presented in Appendix 3. First, we expect all our dependent variables to be highly correlated, e.g., firms experiencing high performance change from the pre-succession year to 4 years post succession are likely to continue exhibiting above average total performance change when comparing the pre-succession year to 5 years after the succession. Second, we expect our dependents and independents to be correlated, indicating they add value in the regression. Third, we naturally expect a strong correlation between $FORCED_i$, $OUTSIDE_i$ and their interaction term. Last, we do not expect strong correlations between $SIZE_i$ and any of the independent variables, as this would indicate multicollinearity. Overall, the results are in line with our expectations. Noticeably, $SIZE_i$ is significantly negatively correlated with $FORCED_i$.

In order to explore the possibility of multicollinearity and its impact on the results, we further conduct robustness tests after estimating the regression models.

5.3. Regression Analysis

In the test of our hypotheses we aim to shed light on our research question; *if post-succession operating performance of a firm can be influenced by the nature of the CEO departure in combination with the origin of the CEO successor.* Convincingly, our regression results show clear evidence that the interaction term is vital in determining post-succession performance changes. Also, we observe a statistically significant positive expected outcome for forced outsider successions in all but the first time specification. Hence, we find strong empirical evidence for hypothesis 4: forced CEO departures followed by the appointments of outside successors are positively related to subsequent changes in operating performance. All in all, we provide significant evidence that the dynamic between the nature of departure and origin of the successor is indeed a strong driver of post-succession performance.

5.3.1. Results from the Regression Model

The regression results using model (1) with t = 1, 2, 3, 4, and 5 are presented in Table 3. The interaction term coefficient is positive and statistically significant at the 5% level for t = 1, 2, and 3 and statistically significant at the 1% level for t = 4, and 5. This empirical evidence confirm our expectation that the dynamic between the nature of departure and origin of the successor is indeed a strong driver of post-succession performance. $FORCED_i$, $OUTSIDE_i$ and

the control variable $SIZE_i$ are either slightly positive or slightly negative in all of the time specifications but no meaningful interpretation can be made given their, in absolute terms, low *t*-statistics.

The coefficient of $FORCED_i$ represents the expected difference in outcome between a forced and an unforced turnover *if the turnover is to an insider.* The coefficient of $OUTSIDE_i$ represents the expected difference in outcome between a turnover to an outsider and one to an insider *if the turnover is unforced.* The coefficient of the interaction term represents either of the following (they are the same): (1) the difference between the effects of forced and unforced turnovers on outcomes *if the turnover is to an outsider*, or (2) the difference between the effects of turnover to an outsider or an insider on outcomes *if the turnover is forced.* N.B. $FORCED_i$ and $OUTSIDE_i$ are effects, whereas the interaction is a *difference between effects.*

Worth reiterating is the sample differences between the time specifications, i.e. the number of observations. The potential effect of the survivor bias increases with time and it can be reasonably assumed that the $t = 5$ sample group, with 132 observations, have notably different characteristics than the $t = 1$ sample, with 413 observations. Hence, comparisons between the different specifications bear little value.

Furthermore, we observe that the model explains between 10% and 23% of the variance in operating performance. However, since the aim of the regression model does not involve making predictions or fully explain firm performance changes, the R^2 value is of low importance in our model.

Table 3. Regression results

	$\Delta PERFORMANCE_{i,t}$				
	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$
$FORCED_i \times OUTSIDE_i$	0.046**	0.061**	0.067**	0.104***	0.133***
	(0.020)	(0.024)	(0.030)	(0.033)	(0.049)
$FORCED_i$	-0.014	-0.005	0.028	-0.017	-0.020
	(0.015)	(0.019)	(0.023)	(0.023)	(0.033)
$OUTSIDE_i$	-0.024*	-0.025	-0.038*	-0.039*	-0.035
	(0.011)	(0.015)	(0.019)	(0.020)	(0.021)
$SIZE_i$	0.001	0.002	0.004	-0.001	0.004
	(0.002)	(0.003)	(0.004)	(0.005)	(0.004)
CONSTANT	-0.042	-0.121	-0.120	-0.010	0.002
	(0.058)	(0.103)	(0.117)	(0.098)	(0.067)
Year Fixed Effects	Y	Y	Y	Y	Y
N	413	327	241	188	132
R^2	0.102	0.095	0.154	0.124	0.227

*Note: *** p<0.01, ** p<0.05, * p<0.1*

5.3.2. Subsample Analysis

A subsample analysis based on the regression results allows us to distinguish between the expected outcome, i.e. the difference-in-difference, of each subsample and the differences in those outcomes between the subsamples, more clearly illustrating the implications for our hypotheses. The subsample analysis also builds on separate subsample *t*-tests performed in a multivariate setting with controls.

The expected outcomes are calculated from the above regression results (see Table 3) according to the below scheme (Williams, 2012), and presented in Table 4.

$$E\left(\Delta PERFORMANCE_{i,t} - Forced\ outside\right) = \alpha + \beta_1 + \beta_2 + \beta_3 + \beta_4 * avg(SIZE)$$

$$E\left(\Delta PERFORMANCE_{i,t} - Forced\ inside\right) = \alpha + \beta_1 + \beta_4 * avg(SIZE)$$

$$E\left(\Delta PERFORMANCE_{i,t} - Voluntary\ outside\right) = \alpha + \beta_2 + \beta_4 * avg(SIZE)$$

$$E\left(\Delta PERFORMANCE_{i,t} - Voluntary\ inside\right) = \alpha + \beta_4 * avg(SIZE)$$

Where E denotes expected value of change in operating performance for the subsample, and $avg(SIZE)$ refers to average firm size in the subsample.

For $t = 1$ we observe a slight negative expected outcome for voluntary outside successions, indicating a minor disruption as a consequence of the succession event. However, the effect is only statistically significant at the 10% level and no meaningful conclusions can be drawn only on that basis. For $t = 2$ we observe a positive expected outcome for forced outside successions, statistically significant at the 1% level. No other expected outcomes are meaningfully different from zero for $t = 2$. For the $t = 3$ sample, voluntary outside successions are again experiencing negative expected outcomes on the 10% significance level and we observe a clear and statistically significant (1% level) expected outcome for forced outside successions. Interestingly, voluntary successions exhibit negative expected outcome regardless of whether the successor is an outsider or insider. Equivalently, forced successions exhibit positive expected outcome regardless of successor type. However, the *t*-statistics for voluntary inside and forced inside remain low in absolute terms and we remain cautious as to draw conclusions on this indication. Regarding $t = 4$, and 5, a clear positive expected outcome can be observed, significant at the 1% level. Undoubtedly, the predicted outcome in the forced outsider condition is the largest of all the predicted outcomes. The other three conditions are noticeably smaller in terms of expected outcome, and about equal to each other in magnitude, though voluntary

insider is slightly positive, and the other two are slightly negative. However, none of these combinations yield strong statistically significant results.

Table 4. Expected outcome split by subsample

	Expected outcome t years after succession event				
	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$
Voluntary inside sample	0.005	0.005	-0.021	0.011	0.011
	(0.009)	(0.011)	(0.015)	(0.013)	(0.014)
Voluntary outside sample	-0.014*	-0.008	-0.026*	-0.022	-0.024
	(0.008)	(0.012)	(0.013)	(0.017)	(0.016)
Forced inside sample	-0.007	0.006	0.026	-0.002	-0.009
	(0.011)	(0.015)	(0.018)	(0.020)	(0.029)
Forced outside sample	0.015	0.041***	0.049***	0.062***	0.089***
	(0.011)	(0.014)	(0.016)	(0.018)	(0.034)

*Note: *** p<0.01, ** p<0.05, * p<0.1*

5.4. Robustness Tests

To verify the reliability of our results and inferences, a number of robustness tests will be presented in the following subsections.

5.4.1. Multicollinearity and Heteroscedasticity

To be prudent and analyse the data for the possible impact of multicollinearity before drawing conclusions, we perform additional tests following the Pearson correlation results. Referring to when two or more independent variables in a regression are highly correlated, multicollinearity poses challenges to the interpretation of the regression. Ultimately, it does not render the model invalid, but makes the contribution of an independent variable harder to distinguish (Farrar & Glauber, 1967).

In order to account for this potential issue, the variance inflation factors (VIF) are calculated for the regression variables. The results are shown inAppendices

Appendix 1. While some disagreements on acceptable VIF values prevail in the literature (O'Brien, 2007; Wooldridge, 2012), none of our values are deemed high enough to have a meaningful effect on our results.

To mitigate the issue of heteroscedasticity, we use robust standard errors clustered on firm level, in accordance with White (1980) and Peterson (2009). If variables in the model exhibit different variances, the model would violate an assumption in the OLS regression and suffer from

heteroscedasticity. The OLS coefficients would still be unbiased and consistent, but their standard errors would be biased and inconsistent, making hypothesis testing and confidence intervals rendered not valid (Cohen et al., 2002). Since our data is structured as an unbalanced panel, the independent and identically distributed random variables (i.i.d.) is not likely to be correct. As a consequence of autocorrelation, our estimated standard errors could understate the standard deviation of the difference-in-difference estimate. As expected, White's test for heteroscedasticity (White, 1980), reject the null hypothesis of homoscedasticity at the 1% significance level. Consequently, standard errors are corrected using robust estimation methods, clustering on firm-level (e.g. White, 1980; Davidson et al.,1985).

5.4.2. Alternative Performance Specifications

To test the sensitivity of our model in terms of our specific change in performance definition, our regression model is run with an additional five performance definitions, over the same five time specifications, for a total of 25 additional regressions. Performance is defined in the five following ways; (1) unadjusted OROA, (2) industry-adjusted OROA, (3) unadjusted ROA, (4) industry-adjusted ROA, and (5) control group-adjusted ROA, as described in Table 5. In the following subsections we will outline these in detail and present noteworthy differences in the results.

Table 5. Alternative performance specifications

	Unadjusted	Industry-adjusted	Control group-adjusted
ROA	*Robustness*	*Robustness*	*Robustness*
OROA	*Robustness*	*Robustness*	**Original model**

Note: OROA is measured as the ratio of accounting earnings before interest and taxes (EBIT) to one-year lagged book assets. Return on assets is measured as the ratio of net income to one-year lagged book assets.

Unadjusted OROA

Regression results from the tests using change in unadjusted OROA are presented in Table 6. The results are to some extent quantitatively similar to the original regressions. However, we can observe a few interesting differences. Generally, the results show even stronger and more significant expected outcomes. Specifically, we observe a strong positive effect from forced outside succession, significant at the 1% level in all time specifications. A noticeable difference compared to the control group-adjusted results is the significant positive expected outcomes for

the forced inside successions in the $t = 3$, 4, and 5 samples. Interestingly, only analysing the unadjusted performance measure would lead one to conclude that forced successions, regardless of successor type, have a positive expected outcome for the $t = 3$, 4, and 5 samples.

Table 6. Regression results – unadjusted OROA

	$\Delta PERFORMANCE_{i,t}$				
	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$
$FORCED_i \times OUTSIDE_i$	0.034**	0.060**	0.035	0.054*	0.087**
	(0.017)	(0.023)	(0.028)	(0.029)	(0.038)
$FORCED_i$	0.000	0.002	0.051**	0.033	0.036
	(0.013)	(0.018)	(0.020)	(0.021)	(0.028)
$OUTSIDE_i$	-0.016	-0.034**	-0.031*	-0.036*	-0.039*
	(0.011)	(0.016)	(0.018)	(0.019)	(0.023)
$SIZE_i$	-0.003*	-0.004	-0.008***	-0.008***	-0.009**
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
CONSTANT	-0.035	-0.118	-0.068	0.015	0.160
	(0.063)	(0.077)	(0.081)	(0.078)	(0.117)
Year Fixed Effects	Y	Y	Y	Y	Y
N	413	327	241	188	132
R^2	0.184	0.143	0.262	0.246	0.301

*Note: *** p<0.01, ** p<0.05, * p<0.1*

Industry-adjusted OROA

As previously stated, some of the cross-sectional variation in performance are assumed to be explained by an industry benchmark. Denis and Denis (1995) argue that an industry-adjustment is sufficient in isolating the effect of the succession event. To conduct the adjustment, the performance measures are subtracted by the median value of the corresponding measure for all firms in the same two-digit Standard Industrial Classification (SIC) industry. Several studies (e.g. Clarke, 1989) has shown that the two-digit definition captures similarities among firms as efficiently as three- or four-digit classifications. The results are presented in Table 7.

Similar to the unadjusted OROA results, we observe a strong positive expected outcome, statistically significant at the 1% level, for forced outside successions over all time specifications. Also, a similar effect can be distinguished following forced inside successions for $t = 3$, 4, and 5. Remarkably, we also observe a negative expected outcome for voluntary outside turnovers at $t = 1$, and 2, statistically significant at the 1% level.

Table 7. Regression results – industry-adjusted OROA

	$\Delta PERFORMANCE_{i,t}$				
	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$
$FORCED_i \times OUTSIDE_i$	0.042**	0.068***	0.036	0.052	0.115***
	(0.018)	(0.023)	(0.042)	(0.033)	(0.024)
$FORCED_i$	-0.001	0.002	0.061***	0.046**	0.034
	(0.012)	(0.017)	(0.023)	(0.020)	(0.026)
$OUTSIDE_i$	-0.018*	-0.038***	-0.023	-0.026*	-0.038**
	(0.008)	(0.013)	(0.030)	(0.017)	(0.016)
$SIZE_i$	-0.004*	-0.005	-0.007	-0.006	-0.006
	(0.002)	(0.004)	(0.005)	(0.005)	(0.006)
CONSTANT	-0.017	-0.110	-0.076	-0.004	-0.222***
	(0.047)	(0.137)	(0.117)	(0.103)	(0.059)
Year Fixed Effects	Y	Y	Y	Y	Y
N	413	327	241	188	132
R^2	0.194	0.155	0.267	0.252	0.377

*Note: *** p<0.01, ** p<0.05, * p<0.1*

Return on Assets

Since return on assets (ROA) also captures effect from e.g. financial choices, we consider it a noisy measure of actual operating performance. However, as several well-cited studies in the area estimate models using ROA (Parrino, 1997; Vafeas, 1999), and as Barber and Lyon (1996) recommend examining several measures, ROA is included as a robustness check. Also, if similar results can be observed using ROA, it serves as a good indicator that the observed effect is indeed a consequence of change in operating performance. Return on assets is measured as the ratio of net income to one-year lagged book assets. As for OROA, ROA is separately adjusted for industry performance and to the matched control group and therefore ultimately measured in three different forms; as unadjusted, industry-adjusted and control group-adjusted.

Generally, similar outcomes can be distinguished, although with some deviations. In particular, the magnitude of effect is generally smaller using ROA as the performance measure. For the control group-adjusted ROA regression we also report slightly less significant results, e.g., the positive expected outcome for forced outside successions is only significant at the 5% level.

5.4.3. Categorization Sensitivity

As discussed in the method section, a significant concern is the influence of uncertainty in the categorization. We recognize that the applied classification scheme may in some cases label normal retirement as forced or vice versa. To address this issue, events with a high degree of uncertainty are labelled as such to allow for a sensitivity analysis. In total, 62 events are labelled

as uncertain, of which 52% are categorized as forced and 48% as voluntary. We run three separate regressions, each treating the uncertain events in a different way. First, all uncertain successions are classified as forced. Second, all uncertain successions are classified as voluntary. Last, all uncertain successions are excluded.

Whether we classify uncertain events as all forced or all voluntary has surprisingly low impact on the magnitude of effect, as well as the *t*-statistics. Classifying all uncertain as forced makes the forced outside effect significant at only 5% for $t = 3$. The equivalent regression when all uncertain are classified as voluntary decreases forced outside *t*-statistics for both $t = 3$, and 4, making them significant at only 5%.

When excluding all uncertain successions, we observe a slight decrease in the forced outside *t*-statistics for $t = 2$, and 3, making the expected outcome positive at only the 5% significance level. No other meaningful changes can be observed.

Overall, our results are considered robust in terms of categorization sensitivity. For brevity, these additional robustness tests are not reported in detail.

6. Discussion

In this section we discuss our empirical results in connection to our hypotheses. First, the discussion focuses on the results from our main regression model and the corresponding expected outcomes for each subsample. Second, an analysis of the numerous robustness tests is presented.

6.1. Analysis of Results

Our research question posits whether post-succession operating performance of a firm can be influenced by the nature of the CEO departure in combination with the origin of the CEO successor. Specifically, our investigation yields four different CEO succession scenarios: (1) voluntary inside CEO succession, (2) voluntary outside CEO succession, (3) forced inside CEO succession and (4) forced outside CEO succession. Our findings emphasize that an interconnectedness of the nature of departure and the origin of the successor plays a fundamental role in explaining the link between executive turnover and subsequent firm performance.

The regression results indicate no post-succession performance effects arising from voluntary CEO departures followed by insiders. While we remain cautious as to interpret these results, Frick (1995) reports that the null hypothesis can in fact be accepted under certain conditions. Specifically, the null result needs to be possible and coherent with the appropriate theoretical framework. Hutzschenreuter et al. (2012) and Schepker et al. (2017) argue that voluntary successions, in contrast to forced successions, do not indicate that the board is dissatisfied with the current CEO or desires strategic change. Instead, Wiersema (1995) hold that voluntary turnover is connected to a desire for persistence in strategic endeavours. Also, disruption effects are less extensive for voluntary departures than forced departures (Vancil, 1987). Moreover, Parrino (1997) claim that inside successors often possess firm-specific human capital in terms of knowledge of internal processes and an understanding of the firm's position in the competitive environment. Therefore, we argue that the decision to promote an inside successor may reflect the preference of a CEO who is more proficient in implementing a firm's current policies, i.e. maintaining the status quo. Accordingly, inside succession is found to be connected with strategic continuity (Friedman & Olk, 1995; Lauterbach et al., 1999; Schepker et al., 2017). Also, disruption effects are deemed low for inside successions (Helfat & Bailey, 2005). Hence, our expectations and the theoretical framework are in line with the expected outcome from voluntary inside successions (see Table 4). Despite strong alignment with theory, we do not consider empirical evidence strong enough to accept the null hypothesis, and hence fail to reject our first hypothesis.

Moreover, as exhibited in Table 3 and Table 4 our findings provide some indication that post-succession firm performance is negatively affected when appointing an outside CEO following voluntary succession. As previously discussed, the voluntary succession type indicates little desire for strategic change. However, outside CEO successors often bring new strategic perceptions to a firm (Virany et al., 1992; Zhang & Rajagopalan, 2004). In line with the findings of e.g. Schepker et al. (2017), we believe that outside successions may yield more policy changes than an inside succession. In the context of voluntary turnover, these policy changes may be undesired and affect firm performance negatively. Moreover, we believe that several negative effects may derive from disruption, due to an outside successor's absence of firm-specific knowledge, uncertainty and transition costs (Helfat & Bailey, 2005). However, due to low t-statistics, in absolute terms, we do not deem the results convincing enough to validate a rejection of the null hypothesis. Therefore, we cannot confidently confirm our second

hypothesis. Perhaps, the potential negative impact from disruption and undesired strategic change, brought on by outside successors, is less fundamental than anticipated.

In regard to our third hypothesis, that forced CEO departures followed by the appointments of inside successors are negatively related to subsequent changes in operating performance, we cannot reject the corresponding null hypothesis (see Table 3 and Table 4). Hence, we find no evidence for our alternative directional hypothesis. Forced CEO turnovers is found to be connected to poor prior firm performance and a desire for strategic change (e.g. Denis & Denis, 1995). Also, forced departures may imply several components of disruption, negatively affecting firm performance (Vancil, 1987). As previously brought to light, inside CEO successors may be more proficient than outsiders in implementing a firm's current policies. Therefore, our expectations were such that forced inside successions would bring on the negative effects on operating performance from disruption but yield limited performance improvements due to the lack of competence in engaging in the desired strategic change. Possible ex-post explanations for this result are that the disruptive effect from forced succession is smaller than anticipated or that inside successors may be capable to engage in value adding policy changes that mitigates potential negative effects from disruption. Clearly, this combination of CEO succession warrants more investigation to provide further understanding.

Interestingly, we find strong empirical support for our fourth hypothesis, i.e., that forced CEO departures followed by the appointments of outside successors are positively related to subsequent changes in operating performance. As previously discussed, the forced departure can be connected to a desire for change, and the outside CEO brings new perspectives and may engage in proficient implementation of policy changes. Even though forced CEO departures followed by outside appointments could be coupled with performance-deteriorating effects from disruption, we observe a positive effect on firm performance for the t = 2, 3, 4 and 5 samples with a significance level of 1% (see Table 4). These findings support a key proposition of the adaptation perspective, i.e. that CEO succession may serve as an opportunity to realign strategies and organizational resources with the external environment, to the eventual benefit in firm performance. Also, our results argue for the presence of causal managerial style effects that can be anticipated and intentionally selected by directors in order to induce the firm to move in a certain direction, as outlined by Fee et al. (2013). Furthermore, since these four regressions include different samples, they cannot be directly compared but rather serve as a robustness check. Noticeably, the magnitude of the performance change is greater with each time specification. However, this gradual increase can be an immediate effect of the

survivorship bias. That is, firms and CEOs experiencing lower, or negative, change in performance post-succession are possibly more likely to disappear from the sample with increasing time.

Conceptually, it can be argued that our findings are in line with previous research to some extent, since the underlying and empirically backed arguments are mostly supported by our findings. However, by treating the nature of the CEO departure and the origin of the successor as intertwined events we apply the theoretical framework in a new context.

6.2. Analysis of Robustness Tests

Taken together, the robustness tests strengthen the findings of our difference-in-difference estimates and support the previously discussed arguments concerning how the findings relate to our research question. However, some discrepancies can be identified, which provide room for further analysis.

First, we conclude that our results show no sign of being significantly affected by a multicollinearity issue. However, heteroscedasticity seems to be prevalent, warranting the use of robust estimation methods and clustering on firm-level to correct the standard errors.

In broad terms, the results from our unadjusted OROA regressions, shown in Table 6, support the findings of the control group-adjusted regressions. However, while the control group-adjusted results present positive changes in firm performance for forced CEO departures followed by an outsider for the t = 2, 3, 4, and 5 samples, the unadjusted regressions exhibit performance improvements in all time intervals, statistically significant at the 1% level. Also, the magnitude of the mean improvements is larger for the unadjusted sample. Second, the unadjusted regressions present improved firm performance for forced CEO departures followed by an insider in the t = 3, 4, and 5 samples, significant at the 5% level. Consequently, the unadjusted performance measures suggest that forced successions, regardless of successor type, positively affects firm performance for the t = 3, 4, and 5 samples. The control group-adjusted model adjusts for effects related to industry-affiliation and mean-reversion issues, while the unadjusted model does not. Therefore, the differences in the results may be connected to these components. The two-fold technical distinction between the unadjusted and the control group-adjusted regression model leaves little room for thorough analysis of the findings. However, incorporating the results of the industry-adjusted OROA regressions may yield some valuable insights.

The industry-adjusted regressions also support the findings of the control group-adjusted regressions in broad terms. Interestingly, the results are in several ways similar to those of the unadjusted model. Concerning forced CEO departures followed by outside CEO appointments, we observe a positive expected effect in firm performance for all time specifications, statistically significant at the 1% level. Moreover, the industry-adjusted regressions also exhibit an improvement in firm performance for forced CEO departures followed by the appointment of an inside successor in the t = 3, 4, and 5 samples, statistically significant at the 5% level. In line with the findings of the unadjusted regressions, the industry-adjusted results on a stand-alone basis suggest that forced successions, regardless of successor type, positively affects firm performance in the t = 3, 4, and 5 samples. Interestingly, the industry-adjusted performance measures exhibit a highly significant negative effect on firm performance for voluntary CEO departures followed by outside CEO appointments, for the t = 1, and 2 samples. Bearing in mind that the industry-adjustment aims to eliminate potential effects in performance arising from industry-related factors (Clarke, 1989; Denis & Denis, 1995), the discrepancies between the industry-adjusted and control group-adjusted results enable some interpretation of the factor of mean-reversion. The scapegoat perspective and the improved management perspective both reason for the existence of a component originating from chance that affects firm performance (Huson et al., 2004). Poor firm performance is therefore argued to be partly related to misfortune. Since poor firm performance tends to initiate CEO dismissal (Warner et al., 1988; Fee & Hadlock, 2004; Huson et al., 2004), it can be inferred that some forced CEO successions could arise from misfortune and should be followed by increased firm performance. This implies that potential post-succession improvements in firm performance could originate from simple mean-reversion in accounting measures, and not the succession itself or factors related to managerial quality. Therefore, we can infer that the results from the industry-adjusted regressions suggest that forced turnovers, regardless of successor type, leads to improved firm performance in the t = 3, 4, and 5 samples partly due to the mean-reversion issue. Moreover, while a slight decrease in firm performance, significant at the 10% level, can be identified for the control group-adjusted OROA regression in the t = 1 test, the industry-adjusted regressions exhibit a highly significant decrease in firm performance for both the t = 1 and the t = 2 samples. Potentially, these findings indicate the presence of a disruptive effect from outside CEO appointments, deteriorating post-succession firm performance in the short-run (Helfat & Bailey, 2005; Schepker et al., 2017). However, the reasons to why the effect is more distinct for the industry-adjusted models than the control group-adjusted models are not obvious. Possibly, it can indicate that some firms that are performing unusually well tend to appoint an

outside CEO following voluntary departures, resulting in mean-reversion of the accounting measures. However, these interpretations are not clear-cut and may give cause for future investigation.

Moreover, some distinctions in firm performance can be distinguished from comparing the regressions that use ROA and those that use OROA as the dependent variable. Specifically, the results from the ROA regressions exhibit an overall lower magnitude of the differences in performance. Moreover, we report slightly less significant results. Since ROA also captures e.g. financing and tax related factors, we argue that examining ROA makes it more difficult to distinguish potential causal effects in operating performance originating from managerial succession. Therefore, we find the results in line with our expectations.

As previously discussed, forced CEO departures have been associated with a desire for change, conceivably resulting from poor previous performance (Weisbach, 1988; Fee & Hadlock, 2004; Huson et al., 2004). Contrarily, voluntary successions are not necessarily coupled with poor prior performance or a desire for change (Hutzschenreuter et al., 2012; Schepker et al, 2017). From performing categorization sensitivity tests on 62 CEO departures, labelled as uncertain in terms of whether forced or voluntary, we observe no fundamental changes in post-succession firm performance. Arguably, CEO departures are not necessarily black-and-white. Likely, there is a spectrum of accuracy and intensity in the categorization components, based on the strength of the underlying reasons for CEO departure. Intuitively, the 62 succession events labelled as uncertain may appertain to the grey zone of this spectrum. For example, a forced departure labelled as uncertain may be less related to a board of director's desire for strategic change than an apparent forced CEO departure, possibly arising from poor prior performance. Arguably, these findings further support our line of reasoning, that in order to acquire a deeper understanding of potential post-succession implications in firm performance, the context of a succession event should be taken into consideration.

7. Conclusion

This study investigates whether operating performance following CEO succession is influenced by the nature of the CEO departure in combination with the origin of the CEO successor. To our knowledge, limited evidence have previously been provided to support such a relationship. We hypothesize that an interconnectedness of the nature of departure and the origin of the successor plays a fundamental role in explaining the link between executive turnover and

subsequent firm performance. Using a quasi-experimental difference-in-difference model on 514 succession events for Swedish listed firms in the period 2000-2017, we examine such a potential interrelation. Our results demonstrate its significance for the understanding of post-succession performance changes.

Our findings are based on a two-step quasi-experiment, pre-succession performance matching and difference-in difference tests, aiming to eliminate any back-door paths that connect CEO succession with operating performance. In order to assess actual performance deviations following CEO succession, we use control group-adjusted changes in operating performance.

Notably, we find strong empirical evidence that forced CEO departures followed by outside successors yield significant positive change in operating performance. The results are significant at the 1% level for four out of five time specifications and hold for several robustness tests. Also, we verify that this effect can be explained neither by the nature of the departure nor by the successor origin alone. Based on our findings, we conclude that in order to further comprehend what drives a firm's post-succession operating performance, the nature of departures and the origins of successors need to be investigated in a combined manner.

Understanding the impact of CEO successions on a firm's subsequent operating performance yields significant implications for different stakeholders, including a firm's board of directors and investors. Many current studies seem to overlook the potential influence originating from the combined dynamics of the nature of CEO departure and the origin of the CEO successor. Arguably, our results contribute with explanatory components to the many contradicting findings in the field of CEO successions and operating performance. Ultimately, we hope that our study can contribute to shifting the analysis from the separately treated dismissal and successor events to the full turnover event.

8. Limitations

Now, a few caveats are in order. First, we challenge the fundamental difference-in-difference assumption of parallel trends. Would the post-turnover performance have continued to be parallel, had it not been for the succession? It is possible that the turnover firms systematically experience a deterioration in operating performance pre-turnover that is not shared by the non-turnover firms, and subsequently observe a positive change as an effect of simple mean reversion? Our research design takes several steps to mitigate the concern that the turnover firm trend would have changed even in the absence of the turnover. First, we match on pre-

succession operating performance, according to Barber & Lyon (1996) the most predictive indicator of future performance. Second, we match on industry affiliation to difference away unobserved time varying or dynamic post-turnover trends at the industry level. Third, we include year-fixed effects in our regressions to difference away unobserved year-specific trends in operating performance. Fourth, we control for firm size to mitigate any effect size can have on divergence from the parallel trend post-succession unrelated to CEO turnover. However, we acknowledge the fundamental source of bias arising from difference-in-difference tests in a non-experimental setting.

Second, another source of bias important to consider is the survivorship bias. In our study, this issue is twofold. First, we only consider firms with financial data from year *t,* hence excluding firms that did not survive. Second, we only measure change in performance as long as the particular CEO is still in office, hence excluding CEOs that did not survive. If not addressed properly, it would cause the results of our study to skew higher. We take several measures to mitigate this issue. First, we manually extend our sample to include 250 turnovers from firms that were not successful enough to survive until 2017. Second, as we question the rationale in prevalent literature to compare the year prior to turnover with only three years post, we include change in performance specifications for t = 1, 2, 3, 4, and 5 years post-succession. We argue that this serves as an important robustness check, confirming whether the observed effect is indeed present in most, or all, of the specifications. An effect only observed for e.g. t = 4, and 5 should be questioned in light of the risk of sample selection bias. Nevertheless, we urge future research to confirm our findings, possibly in other settings, using a different year span, and including other variables, such as control for hazard rate and/or earnings management effect.

Third, a potential source of bias is introduced when excluding outliers and missing data points. Possibly, these adjustments are non-random and will therefore cause a selection bias in the data. However, we recognize that the adjustments are indeed equally distributed to a high degree, mitigating most of our concern.

Fourth, we acknowledge the higher likelihood of obtaining a false-positive result when conducting multiple comparisons (Gelman & Loken, 2013). As a consequence of not registering our research method pre-study, our findings are subject to undisclosed flexibility in data collection and analysis, possibly lowering the reliability (Simmons et al., 2011).

Lastly, while our evidence points to a potential upside of choosing an outsider successor following a forced turnover, this implication could be specific to the context and does not

constitute a recommendation in itself. Nevertheless, we believe that the board of directors should carefully consider the full circumstances surrounding the turnover before making a decision on the suitable replacement CEO.

9. Directions for Future Research

This paper focuses on the influence that the nature of CEO departure and CEO successor origin have on post-succession operating performance of a firm. Conducting this study, we noted numerous interesting adjacent topics, unfortunately out of the scope of this study. Also, we believe some of our findings validate further research and would allow for stronger inferences if confirmed by other researchers.

Specifically, questions on when the full effect of the new CEO can be realized remains unanswered. Studies addressing this materializing effect are needed to paint a clearer picture of the succession dynamics and their long-term implications. Clearly, it is a fundamentally difficult task to assign causality in a setting with non-experimental data, but quasi-experimental methods could possibly be combined with other econometric tools to allow for better understanding. Also, we encourage future research to confirm our findings in a non-Swedish setting and investigate other factors that could likely have an effect on change in performance, namely, board composition and CEO tenure. An interesting addition would be to code the outsider variable as continuous, rather than dichotomous, and account for different degrees of outsiderness.

We find compelling evidence that forced outside successions impact performance in the medium- and long-term horizon. However, an interesting addition for future research would be to include the earnings management component when investigating the short-term effects. Finally, we hope that we can provide direction for future research on performance changes following CEO successions by shifting attention from the separate drivers to the overarching context and dynamics in play. Our findings and approach illustrate the benefits of trying to capture interactions of the broader system dynamics rather than investigating observed phenomena in isolation.

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Appendices

Appendix 1. VIF analysis

Variable	VIF	Tolerance
$FORCED_i \times OUTSIDE_i$	2.67	0.37
$FORCED_i$	2.31	0.43
$OUTSIDE_i$	1.72	0.58
$SIZE_i$	1.14	0.88
Mean	1.96	0.57

Note: Tolerance is defined as 1/VIF

Appendix 2. Distribution of observations by industry and year

SIC	*Sector*	*2000*	*2001*	*2002*	*2003*	*2004*	*2005*	*2006*	*2007*	*2008*	*2009*	*2010*	*2011*	*2012*	*2013*	*2014*	*2015*	*2016*	*2017*	***Total***
1	Construction	0	2	4	2	0	1	1	2	1	2	2	2	3	2	0	1	1	3	**29**
2	Manufact. A	0	6	9	5	8	3	4	6	8	7	5	7	6	4	5	7	6	9	**105**
3	Manufact. B	1	9	9	12	9	12	12	8	9	11	10	13	8	7	4	13	11	15	**173**
4	Transportation	0	3	2	2	1	1	0	4	2	1	1	2	4	4	1	2	1	1	**32**
5	Retail	1	4	2	1	2	2	2	3	1	5	1	7	2	4	1	3	3	1	**45**
7	Services A	0	6	7	6	5	6	9	9	8	4	4	6	5	5	4	2	7	4	**97**
8	Services B	0	2	2	2	0	2	3	1	2	2	0	1	4	2	3	0	1	2	**29**
Total		**2**	**32**	**35**	**30**	**25**	**27**	**31**	**33**	**31**	**32**	**23**	**38**	**32**	**28**	**18**	**28**	**30**	**35**	**510**

Note: SIC grouping 6 – Financial firms are excluded

Appendix 3. Pearson's Correlation

	$\Delta PERFORMANCE_{i,t}$								
	$t = 1$	$t = 2$	$t = 3$	$t = 4$	$t = 5$	$FORCED_i$	$OUTSIDE_i$	$FORCED_i \times OUTSIDE_i$	$SIZE_i$
$\Delta PERFORMANCE_{i,1}$	1.000								
$\Delta PERFORMANCE_{i,2}$	0.498*	1.000							
$\Delta PERFORMANCE_{i,3}$	0.353*	0.652*	1.000						
$\Delta PERFORMANCE_{i,4}$	0.242*	0.422*	0.625*	1.000					
$\Delta PERFORMANCE_{i,5}$	0.207*	0.398*	0.510*	0.588*	1.000				
$FORCED_i$	0.060	0.119*	0.204*	0.143*	0.191*	1.000			
$OUTSIDE_i$	-0.001	0.050	0.031	0.016	0.007	0.036	1.000		
$FORCED_i \times OUTSIDE_i$	0.101*	0.161*	0.176*	0.211*	0.297*	0.610*	0.564*	1.000	
$SIZE_i$	0.034	0.018	0.082	0.013	0.141	-0.144*	-0.003	-0.031	1.000

*Note: * $p<0.05$*