ESSAYS ON THE INFORMATIONAL BENEFITS OF ACCOUNTING STANDARDS FOR LISTED FIRMS

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1 Introduction

1.1 Motivational background of the thesis

Financial markets are an indispensable part of a functioning economy that benefits its participants. An efficiently running financial market requires transparent financial information (Akerlof 1970). Throughout the world, governmental bodies have implemented regulatory systems that require firms to periodically prepare and disclose information. As one aspect of such mandatory disclosure frameworks, firms have to disclose financial information prepared in accordance with certain regulations, including comprehensive sets of accounting standards. After a worldwide adoption of International Financial Accounting Standards (IFRS) in the 2000s in most major economies but the United States, two accounting systems are of international importance: IFRS and the United States Generally Accepted Accounting Principles (US GAAP) (Pacter 2015). The developers and issuers of the two accounting systems, the International Accounting Standards Board (IASB) for IFRS and the Financial Accounting Standards Board (FASB) for US GAAP have increasingly extended their cooperation through a bilateral convergence program in recent years. While IFRS and US GAAP are already conceptually similar, the goal of the convergence program is to erase remaining differences between the two regulations (Van der Meulen et al. 2007). Governmental bodies have expressly supported this initiative in the past (G20 2013).

Potential benefits of globally uniform accounting standards are subject of a large body of prior literature and not tackled by this thesis (Rezaee et al. 2010). However, competition has been helping develop several efficient frameworks, for example, for corporate charters, banking, maritime shipping, university accreditation, and stock exchanges (Sunder 2002). The absence of such competition in accounting standards regulation presents a strong case for regulation that is based on comprehensive evidence, not least in an era of “post-truth” (Drezner 2016). Academic research can provide such evidence (see e.g. Schipper 2010; Schipper 1994) and accounting standard setters particularly demand researchers’ support (Leuz and Wysocki 2016; Ewert and Wagenhofer 2012). Specifically, research on accounting and financial markets can help evaluate whether standard setters’ objectives are served by the standards they have issued (Kothari 2001).

The objective of financial reporting for both standard setters, IASB and FASB is the so-called "decision usefulness". It implies that the primary goal of financial accounting information as prepared under IFRS and US GAAP is to "provide information to help present and potential investors and creditors and other users in assessing the amounts, timing, and uncertainty of prospective cash receipts" (FASB 1978). Therefore, the straightforward approach to evaluating accounting regulation is to test whether the regulation is able to provide information that is decision useful for market participants. Financial accounting research can provide such evidence on two levels. On an aggregate (market) level, financial markets research can ascertain whether the adoption of an accounting regime in an economy provides informational benefits relative to the old regime. On a detailed (accounting standard) level, financial markets research can provide evidence on the informational benefits of introductions or alterations of individual standards in existing accounting regimes (Kothari 2001).

This thesis seeks to contribute to the critical evaluation of international accounting standard setting on both levels. Specifically, it takes issue with the informational benefits of recent changes in accounting regulation concerning IFRS and US GAAP.

1.2 Objective of the thesis

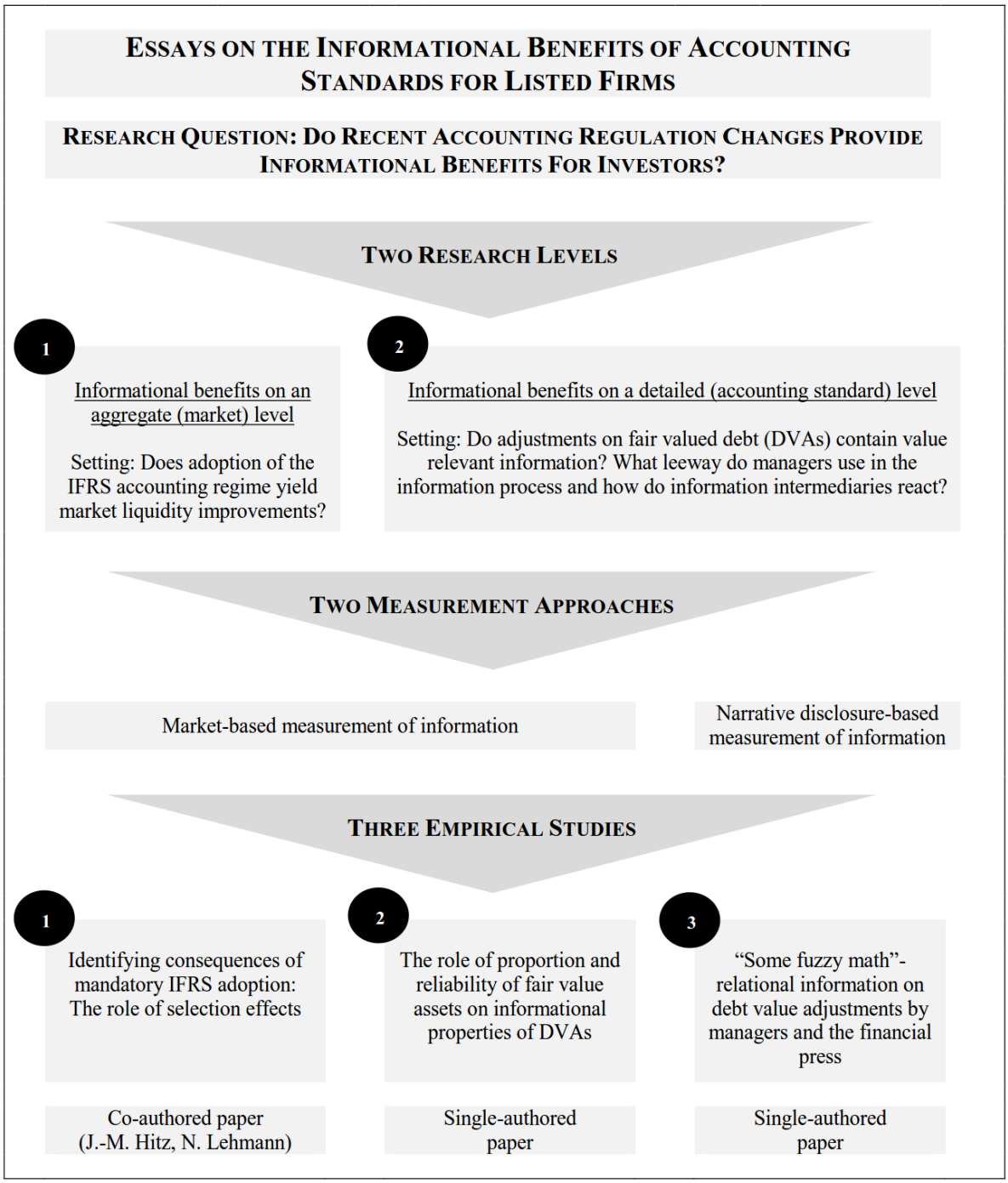
This thesis comprises three studies. The first study investigates informational benefits of introducing a worldwide uniform set of accounting standards geared at providing decision useful information. Consequently, the study examines informational benefits on an aggregate (market) level. As a research setting, the study tests the effects of the worldwide mandatory IFRS adoption on financial market liquidity. For this, the study replicates a seminal study (Christensen et al. 2013) that finds liquidity benefits through mandatory IFRS adoption and other regulation changes. Challenging the findings of Christensen et al. 2013, the study introduces a measure for changes in the underlying sample composition as a potential correlated omitted variable in the test setting. The study thereby aims to increase our knowledge about and possibly adjust our expectations of the informational benefits that mandatory adoption of the IFRS accounting regime provides.

The second study examines informational benefits of accounting regulation, too, this time on a detailed, accounting standard level. Specifically, it tests informational benefits from

a fair value accounting item that was recently introduced in IFRS and US GAAP accounting regulation (IASB 2014a; FASB 2007). Fair values are in a sense the epitome of international standard setters' claim to produce high quality, informative standards (Hitz 2007). Accordingly, both IASB and FASB express their intention to expand the use of fair values in financial accounting (IASB 2014a; FASB 2007). Therefore, testing the decision usefulness of recent fair value accounting items should make a meaningful and lasting contribution to the literature on accounting regulations' informational benefits. Specifically, I analyze the informational properties of debt value adjustments due to a change in credit risk (DVAs). DVAs have stirred up a large public debate in which critics claim that DVAs are "junk income" (Dash 2009) and just "paper profits" (Eavis 2008). Against this background, my second study investigates the value relevance of DVAs with a focus on the moderating role of reliably measured related fair value assets. The study seeks to improve our understanding of the required informational environment in which seemingly "counterintuitive" (IASB 2009) DVAs contain value relevant information for investors.

As the second study, the third study researches informational benefits of recent accounting changes on a detailed, accounting standard level. Again, the focus of the study lies on informational properties of controversial debt value adjustments at fair value. Unlike the first two studies, this study does not employ market-based measures of informational benefits, but narrative disclosure-based measures. Specifically, the third study investigates the DVA relational information that managers and the financial press provide. For this, I collect and analyze the contents of quarterly earnings press releases and financial press articles by hand. I particularly test the claim that managers emphasize negative DVAs, in line with an attempt "to trick the media and investors" (Milstead 2012). The study aims to enhance our understanding of leeway that managers use when providing information to financial markets. Regarding the press, it also seeks to improve our knowledge of the role of the financial press as an intermediary of decision useful information. Following, I describe the embedding of the studies in prior literature in more detail. Figure 1.1 summarizes the objective and research settings of my thesis.

Figure 1.1 Objectives of the thesis



Study 1: Identifying consequences of mandatory IFRS adoption: The role of selection effects

As "the worldwide adoption of IFRS is arguably one of the largest regulatory events in accounting history" (Leuz and Wysocki 2016) it has spawned a large literature of its intended and unintended consequences (Brueggemann et al. 2013). While few studies focus on real effects such as increased cross-border labor migration (Bloomfield et al. 2017), the majority of research is focused on the informational properties of IFRS accounting and their impact on financial markets. A central research question is whether financial statements prepared under IFRS have informational benefits relative to financial statements prepared under the respective preceding local accounting systems. In this regard, an increased market liquidity is a common proxy for such informational benefits as researchers and standard-setters broadly agree that it is "intrinsically desirable" (Schipper 2010).

Prior literature indeed finds evidence for such positive financial market effects surrounding IFRS adoption (e.g. Daske et al. 2008). However, research that directly investigates the properties of IFRS financial statements fails to show conclusive evidence that IFRS financial statements provide superior earnings quality or improve cross-country comparability (Brueggemann et al. 2013). Against this background, a recent study of Christensen et al. 2013 finds that positive effects of mandatory IFRS adoption are limited to countries from the European Union, specifically, to six countries that simultaneously adopted stricter accounting enforcement regulation (EC 2002). The authors argue that it is impossible to disentangle the effects from IFRS adoption from the effects of enforcement regulation changes and therefore to attribute the found positive financial market effects solely to IFRS. In conclusion, the authors state that financial market effects surrounding IFRS adoption "have been extensively studied, but their sources are not yet well understood" (Christensen et al. 2013, p. 147).

The first study of the thesis seeks to contribute to our understanding of this matter. It uses an international setting to study firms' decisions to stay in or opt out of the financial market following mandatory IFRS adoption in different countries. Afterwards, it tests whether differences between countries in this regard are able to explain post-IFRS adoption capital market effects.

Study 2: The role of proportion and reliability of fair value assets on informational properties of DVAs

Shifting the focus from the aggregate level to the detailed level, financial market research can increase our knowledge regarding informational properties of specific accounting items. A feasible, market-based approach to test informational benefits of newly introduced or altered accounting standards is to test the value relevance of the additional information from the accounting regulation change (Schipper 2010). While value relevance is not a criterion explicitly mentioned by accounting standard setters, it is a common way in academic literature to operationalize the two central criteria for information to be decision useful, as stated by IASB and FASB (IASB 2010; FASB 1978): relevance and reliability (Barth et al. 2001). To quantify the value relevance of accounting information, researchers measure its statistical association with share prices or share returns (Kothari 2001).

Within the value relevance literature, the value relevance of fair values is of special interest. The reason for this is a long-standing debate about the trade-offs between fair values' relevance and reliability. Proponents argue that fair values have high relevance and better reflect real volatility. Opponents argue that fair values are less verifiable and more prone to estimation errors and managerial discretion and that this threatens fair values' reliability (Song et al. 2010). A theoretical analysis of Hitz 2007 finds support for the decision usefulness of fair values, but only for those derived from liquid markets. Accordingly, empirical research finds mixed evidence on the value relevance of fair values (see e.g. Barth 1994; Eccher et al. 1996; Carroll et al. 2003).

Debt value adjustments due to a change in credit risk, or DVAs, are a special kind of fair value income. They arise from derivative liabilities or when firms choose to apply a fair value option to their own liabilities. Such fair value options had more than rocky introductions in both, IFRS accounting (the European Union effectively made the IASB change its original fair value option regulation, see Brackney and Witmer 2005) and US GAAP accounting (two of the seven FASB members dissented with the fair value option's issuance, see FASB 2007). Later, the net income effects of DVAs came under heavy criticism because critics perceive them as "counterintuitive" (Chasteen and Ransom 2007) "accounting voodoo" (Carver 2012b) to which investors "rightly don't ascribe much value" (Eavis 2008).

Still, the informational properties of DVAs are open empirical questions. A recent study of Cedergren et al. 2015 finds evidence that DVAs can, in fact, convey value relevant information if certain criteria regarding the respective firms' informational environment are met. My second study seeks to broaden our knowledge in this regard by investigating the role of proportion and reliability of fair value assets on informational properties of DVAs.

Study 3: "Some fuzzy math"- relational information on debt value adjustments by managers and the financial press

Related to the second study, my third and final study on the informational benefits of accounting standards investigates the informational effects of the introduction of DVAs in accounting regulation from a narrative perspective. I draw motivation for the study from the ongoing DVA debate. Here, critics regularly accuse managers of reporting DVAs' effect on net income asymmetrically, potentially to exploit DVAs' unique "counterintuitive" (Chasteen and Ransom 2007) properties. Specifically, critics argue that "banks have been more than happy to highlight these losses in their earnings releases, while being a lot more circumspect when valuation gains boost earnings" which "makes it looks like they are trying to trick the media and investors" (Milstead 2012).

The investigation of the DVA relational information that managers and the financial press provide to financial market participants requires a different approach of measuring informational properties than the first two studies. Instead of measuring information properties with aggregated market measures, I hand-collect managers' quarterly earnings press releases and financial press articles and perform a manual content analysis. As Leuz and Wysocki (2016) point out, the "qualitative, text-based, and narrative" nature of such disclosures makes it difficult to work with them. They argue that in consequence, this measurement approach is "fairly new" but has the benefits of enabling researchers to construct measures with an otherwise difficult to obtain informativeness dimension.

By providing comprehensive descriptive evidence on narrative DVA disclosures by managers and the financial press, my third paper seeks to contribute to the young but growing literature on the informational properties of a recent and rather "quirky" (Eavis 2009b) accounting item.

1.3 Content of the thesis

The thesis is structured as follows: Section 1 outlines the motivational background and the objective of the thesis. Section 2, 3, and 4 present the three empirical studies. The last section concludes.

Section 2: Identifying consequences of mandatory IFRS adoption: The role of selection effects

The study in this section investigates the role of selection effects inherent in the research designs commonly used in studies examining economic or accounting effects of mandatory IFRS adoption. The worldwide adoption of IFRS in several countries at different points in time offers an interesting setting because it diminishes concerns of confounding events of economic significance. In particular, it enables researchers to more clearly identify informational benefits of the mandatory IFRS adoption in the respective economies. A typical way to measure informational benefits in this regard is to test for an increased market liquidity. Exploiting this setting, prior literature finds evidence for informational benefits through mandatory IFRS adoption (e.g. Daske et al. 2008). However, Christensen et al. (2013) challenge the notion that found improvements in market liquidity are attributable to informational benefits from IFRS adoption only. Specifically, they include changes in enforcement regulation as a potential correlated omitted variable in their tests and demonstrate that it is able to partly explain increased liquidity after IFRS adoption. In our study, we introduce another potential correlated omitted variable – systematic opt outs of firms out of the IFRS mandate. To test its economic effect, we fully replicate the study of Christensen et al. 2013. Using a sample of 727,293 firm-quarters from 56 countries, we construct a "Selection Exposure Index" to measure country's exposure to systematic opt-outs and not-materialized opt-ins. Adding the Selection Exposure Index to the tests of Christensen et al. (2013), we find that it, too, is able to explain liquidity benefits beyond the explanatory factors from prior literature.

The findings from this study provide evidence on a potential channel through which concurrent IFRS accounting and enforcement regulation possibly translate into higher market liquidity. Specifically, the findings challenge the notion that liquidity benefits found by prior literature are fully attributable to informational benefits from greater accounting transparency through the adoption of IFRS or a stricter accounting enforcement. Instead, the results imply

that the increased regulation from these events lead to higher regulatory costs for firms in the respective countries. This, in turn, systematically changed the underlying sample composition in these countries because smaller, less liquid firms systematically opted-out of or never opted-in the regulated market, resulting in overall market liquidity improvements. The study thereby further clarifies the informational benefits of the worldwide mandatory adoption of the IFRS accounting regime. Or conversely, it sheds light on a factor whose omission by prior literature potentially induced overestimation of IFRS accounting standards' decision usefulness in the past.

Section 3: The role of proportion and reliability of fair value assets on informational properties of DVAs

This study examines the informational properties of debt value adjustments due to a change in own credit risk, short: DVAs. It does so from a financial market and an accounting perspective. More precisely, it investigates the role of proportion and reliability of related fair value assets for the value relevance, the market pricing, and the persistence of DVAs. The informational properties of DVAs are an interesting research topic because DVAs have rather unique characteristics that critics perceive as "counterintuitive" and "dangerous" (Crooch and Upton 2001). Specifically, DVAs produce net income gains when a firm's own credit risk deteriorates and produce net income losses when a firm's own credit risk improves. This particular feature has stirred up a public debate after DVAs' introduction in IFRS and US GAAP accounting in 2006 and 2007 respectively. Within the debate, the potential of DVAs to blur investors' view on firm performance is a key concern of critics including the IASB and FASB members themselves (IASB 2009; FASB 2007). Experimental literature backs this concern (Gaynor et al. 2011; Lachmann et al. 2015). However, recent empirical literature finds that DVAs contain decision useful information when firms are transparent in the sense that they only have few unrecognized intangible assets (Cedergren et al. 2015). Directly adding to the study of Cedergren et al. (2015), I consider a different factor that potentially influences investors' understanding of DVAs: the proportion and reliability of related fair value assets. Asset valuation that transparently reflects the sources of credit risk changes which underlie DVAs should enhance markets' perception of DVAs.

For a sample of 617 firm-quarters of US banks that adopted the fair value option for liabilities between 2007 and 2014, I hand-collect information on the amount of reliably and less reliably measured fair value assets on the banks' balance sheets. I find that higher

proportions of fair value assets are associated with more value relevant DVAs, but only if the fair value assets are measured reliably, i.e. if they reflect quoted market prices. Conversely, a high proportion of less reliably measured fair value assets do not improve related DVAs' value relevance. Furthermore, the presence of a high proportion of less reliably measured fair value assets is associated with a too conservative pricing of DVAs. For high proportions of reliably measured fair value assets, I do not find this relation. Finally, DVAs' persistence is moderated by the amount of potentially less reliably measured fair value assets. Taken together, the findings are consistent with DVAs providing decision useful information for capital markets when the related fair value assets reflect the sources of the underlying changes in credit risk transparently.

The findings enhance our understanding of the necessary preconditions for DVAs to provide decision useful information. Against the background of recent DVA disclosure regulation changes in both accounting regimes, IFRS and US GAAP, that shift DVAs' recognition from net income to other comprehensive income, they should be of special interest (IASB 2014b; FASB 2016). They also add to the ongoing DVA debate held by researchers ("clearly confusing and counterintuitive", see Chasteen and Ransom 2007), applicants of the standard ("one of the more ridiculous concepts that's ever been invented in accounting", see Rapoport 2012), rating agencies ("an accounting standard that we find particularly unhelpful", see Rapoport and Lucchetti 2011), analysts ("abomination", see Keoun and Henry 2010), financial blogs ("Dumb and Dumber", see Tchir 2012), and the financial press ("some fuzzy math", see Dash 2009).

Section 4: "Some fuzzy math"- relational information on debt value adjustments by managers and the financial press

As the second study, this study also takes issue with the informational properties of DVAs, but from a disclosure perspective. In the ongoing DVA debate, critics argue that DVAs could blur investors' view on firm performance, for example, when "artificial" (Keoun 2008) DVA gains increase net income. Furthermore, critics raise concerns that managers could exploit the fact that DVAs are potentially prone to misinterpretation by emphasizing only the losses from DVAs in their financial reporting relative to DVA gains. If DVA relational information provided by managers followed such a scheme, this could potentially compromise the informational benefits of this accounting standard as strived for by its standard setters.

Using a sample of 353 firm-quarters of 15 US financial firms that report DVAs between 2007 and 2014, I find that managers, indeed, provide more DVA relational information in quarterly earnings press releases in firm-quarters with large negative DVAs relative to large positive DVAs. Furthermore, managers provide relatively more DVA information in firm-quarters in which they have opportunistic incentives to do so, for example, when a negative DVA turned a net profit into a loss. This documented reporting pattern is consistent with concerns voiced in the DVA debate. Analyzing DVA relational information provided by the financial press on 202 firm-quarters, I find that the press picks up managers' reporting spin. Specifically, a more comprehensive DVA reporting in a firm's quarterly press release is associated with a higher probability of financial press reporting of the DVA. Still, the press enhances DVA information by providing new DVA information and by assuming a critical tone towards DVAs, especially in firm-quarters in which managers provide less DVA information and when DVAs improve firms' income. These findings are consistent with the financial press disseminating and enhancing DVA information, thereby potentially increasing the informational benefits from this accounting regulation for investors.

2 Identifying consequences of mandatory IFRS adoption: The role of selection effects

Joerg-Markus Hitz, Sebastian Kaumanns, and Nico Lehmann[1]

Abstract: This paper documents potential selection effects inherent in the research designs that are typically used in studies investigating economic or accounting effects of mandatory IFRS adoption. Replicating prior work by Christensen, Hail, and Leuz (2013), we show that one particular selection effect, the IFRS treatment selection effect, which owes to systematic opt outs of firms out of the IFRS mandate, explains prior findings on positive liquidity effects of IFRS adoption. The paper's implications are twofold. First, we offer a novel explanation how mandatory IFRS adoption and enforcement regulation translate into capital market benefits. Second, we point out limitations of the EU mandatory IFRS adoption setting for testing empirically the effects of *mandatory* IFRS adoption. We outline research strategies and methodological issues to address those research design and identification challenges.

JEL codes: C52, M41, M48

Keywords: Mandatory IFRS adoption, delistings, downlistings, oupt-outs, sample selection, correlated omitted variable, Worldscope

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2.1 Introduction

This paper contributes to the ever growing literature on the effects of mandatory adoption of International Financial Reporting Standards (IFRS). We identify potential selection effects inherent in the research designs typically used in this literature and show how one particular selection effect, driven by systematic opt outs of firms from IFRS and enforcement regulated markets, might explain prior findings on positive liquidity effects around mandatory IFRS adoption. Hence, we provide conceptual reasoning and empirical evidence that prior evidence on positive capital market effects, that coincided with mandatory IFRS adoption and with concurrent enforcement regulation, can be explained by selection effects, that is, systematic changes in the underlying sample and market structure.[2] We also discuss research implications.

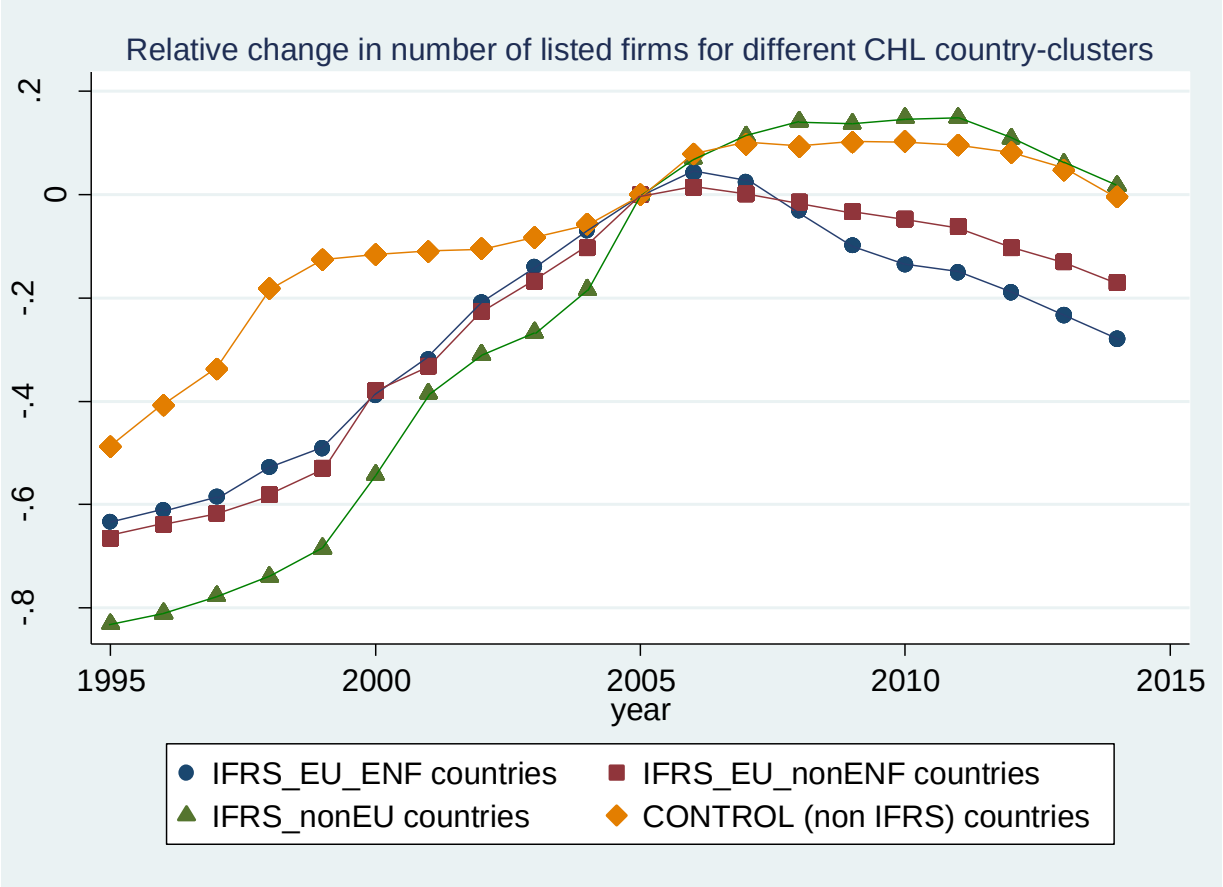
Characteristically, empirical studies on the effects of mandatory IFRS adoption employ a differences-in-differences (DiD) design to identify the causal impact of the IFRS adoption treatment on market, accounting, or other economic outcome variables, incremental to a control group of non-IFRS firms (for overviews, see, e.g., Brueggemann et al 2013; Pope and McLeay 2011). Broadly, this literature provides three main findings. First, there is rich evidence of positive capital market effects (e.g. higher liquidity, lower cost of capital) coinciding with the IFRS mandate (e.g., Daske et al. 2008). Second, and somewhat in contrast, the evidence on accounting outcomes, such as earnings quality, or comparability, is fairly inconclusive. Hence, there is still no thorough understanding about the potential channels through which the IFRS mandate affects said positive capital market benefits (e.g., Brueggemann et al. 2013; Florou and Pope 2012). Third, the recent paper by Christensen, Hail and Leuz (2013) (CHL 2013) identifies one such potential channel by documenting a clustering of liquidity benefits in EU countries which concurrent with the IFRS mandate adopted EU-mandated changes in their enforcement mechanisms, setting up institutions that conduct random-selection based reviews of financial statements, and penalize non-compliance. CHL (2013) conclude that the positive market effects of IFRS reporting, as documented by prior literature (e.g., Armstrong et al. 2010; Byard et al.

[2] A more recent paper by Gutierrez et al. (2017) provides cross-country evidence on the determinants of voluntary and forced delistings of IFRS firms in the post-IFRS period. Revisiting our findings on potential selection effects in the setting of mandatory IFRS adoption, the authors show a higher probability of delistings in strong IFRS enforcement jurisdictions.

2011) might actually be driven by solitary or bundled effects of enforcement regulation. At the same time, CHL (2013) caution that there may be other, and enforcement related, correlated variables that may explain their findings. In addition, Barth and Israeli (2013) point out that the findings presented in CHL (2013) are incomplete to the extent that they cannot fully distinguish between effects of changes in enforcement from effects of mandatory IFRS adoption. Overall, this discussion illustrates that the channels and drivers of positive liquidity effects observed around the IFRS mandate are still somewhat of an open question.

The motivation of our analyses, which provide a new angle on the said IFRS adoption literature, is an empirical phenomenon illustrated in Figure 2.1, which document the coverage of listed firms by the Worldscope database for the period between 1995 and 2014. We categorize firms according to the country clusters used by CHL (2013). Figure 2.1 illustrates that the number of firms is relatively stable in IFRS countries outside the EU and in countries without IFRS adoption (CONTROL cluster) after 2005–the year of mandatory IFRS adoption in the EU and in most non-EU treatment countries covered by CHL (2013). In contrast, there is a rather steady decline in the number of IFRS firms domiciled in the EU, and this decline is particularly pronounced for firms from EU countries that adopted or improved enforcement mechanisms concurrent with IFRS adoption in 2005. For the latter firms (the IFRS_EU_ENF cluster), we observe a significant decline in the number of firms of 27.95% in the period between 2005 and 2014, compared to a quite moderate decline in the number of control firms of only 0.43%.

Figure 2.1 Worldscope Coverage and CHL (2013) Country-Clusters



We propose three non-mutually exclusive explanations for the patterns documented in Figure 2.1: a *control group selection effect*, and a two-fold *treatment group selection effect*. We expect a control group selection effect due to database coverage choices. Prior literature and the analyses of Worldscope's coverage documentation indicate that vendors of financial data choose different levels of coverage, with firm size as a major determinant (Garcia Lara et al. 2006). This coverage pattern is particularly relevant for less developed markets and jurisdictions, where database coverage is incomplete. In contrast, developed markets usually receive full coverage of all listed firms. In a common IFRS DiD design (e.g., CHL 2013), we observe that most IFRS treatment countries, in particular IFRS countries from the EU, have developed markets with full and stable Worldscope coverage, whereas most non-IFRS control countries are less developed with only partial and increasing Worldscope coverage. We thus predict that the time trend in the control group is biased by increasing database coverage of (presumably small and less liquid) second- and third-tier firms over time, which biases the design towards finding liquidity benefits in the

treatment group relative to control firms in the post-IFRS period. Put differently, the database coverage effect potentially introduces a time trend into the liquidity of control firms which violates the common trends assumption that underlies DiD techniques (e.g., Angrist and Pischke 2015).

Second, we argue that the patterns in Figure 2.1 might also reflect potential selection effects in the treatment group. We know from prior economic and accounting literature that in the course of regulatory interventions firms may engage in strategies to avoid the regulation (for an overview, see Leuz and Wysocki 2016, pp. 536, 555-556). Possible avoidance strategies include, among other things, switching to less regulated exchange segments ("going dark", or downlistings), delistings, as well as threshold management in cases the regulation and its adoption require a financial threshold that has to be met. Overall, this literature extensively highlights the role of firm-level selection effects in regulatory settings, that is, systematic changes in the composition of the treatment and control groups as a result of the treatment (e.g., Angrist and Pischke 2008; Leuz and Wysocki 2016). The IFRS literature, however, remains surprisingly silent on whether firm-level selection effects and avoidance strategies might occur in the aftermath of mandatory IFRS adoption. Yet, in a recent study, Hitz and Mueller-Bloch (2016) document that since 2005 a substantial number of listed firms in Germany dispensed with their listing in the regulated market, effectively opting out of the IFRS and enforcement mandates. The authors show that firms that opt out, on average, are relatively small, have relatively little profitability, low liquidity, and have more likely been censured by the enforcement institutions for preparing erroneous financial statements. Hence, we propose that the relative decline of listed IFRS firms compared to listed non-IFRS firms as documented in Figure 2.1 might reflect economic externalities of increased accounting and enforcement regulation. In other words, assuming that IFRS and enforcement regulation systematically affect the probabilities of delistings or downlistings, we would expect to observe systematic differences in the number of listed firms across IFRS and non-IFRS markets following the regulatory event. Moreover, as these systematic changes are induced by specific firms with specific firm characteristics (presumably small and less liquid firms), cross-country IFRS research might become prone to a correlated omitted variable (i.e., treatment selection effect). In essence, treatment selection means that the IFRS treatment assignments become less random as remaining IFRS firms effectively select themselves (ex-post) into the treatment group by choosing not to dispense with IFRS

through opt-outs. We thus concur that this *treatment selection effect* renders it more likely to find liquidity benefits in the post-treatment period compared to a randomized treatment group. Besides this *core* treatment selection effect, we further expect that the mere changes in the sample composition of the treatment group (i.e., the decrease in sample size in post-IFRS period due to size and liquidity related opt-outs and exits) might additionally foster the documented treatment effect in prior IFRS research. As both treatment selection effects differ in terms of research design remedies and the self-selection component, we label the former as "treatment *self* selection effect" and the latter as "treatment *sample* selection effect".

In terms of research design remedies, we expect that two standard econometric techniques— separate time fixed effects for the treatment and control group or a balanced sample approach—should mitigate the control group and part of the treatment group selection effect (i.e., treatment sample selection effect) in a DiD design. However, both techniques do not seem to play a prominent role in prior research on mandatory IFRS adoption. As to our knowledge, only CHL (2013) employ a variant of combined treatment and time fixed effects. In addition, our reading of the 25 recent IFRS studies discussed by Brueggemann et al. (2013) reveals that only six studies appear to employ a balanced sample approach. Regarding our treatment self selection effect, the ex-post firm-level *self selection* into the treatment group by choosing not to dispense with the IFRS mandate, we expect that the aforementioned econometric techniques will be of limited use. The reason is that this self selection effect constitutes a classical correlated omitted variable inherent in the setting of mandatory IFRS adoption that standard econometric techniques are unable to address.

To empirically examine our proposed three-fold selection effect, we conduct a series of analyses to assess the presence as well as the magnitude of these selection effects. In essence, these analyses involve three main steps. Our first set of analyses revisits Figure 2.1 and examines the relative decline in the number of listed firms as documented in Worldscope database within a multivariate (country-level) DiD design comprising country and year fixed effects as well as variation in the sample period. The corresponding findings are fully in line with the casual inferences from Figure 2.1 suggesting a significant and systematic decline of IFRS firms from EU countries with concurrent enforcement changes compared to non-IFRS firms.

Our second set of analyses addresses firm-level characteristics of firms that are potentially affected by our documented selection effects. Assuming that database providers are biased towards large firms (Garcia Lara et al. 2006) and that especially small and poor performing firms tend to trade off costs and benefits of regulatory interventions (Leuz and Wysocki 2016; Hitz and Mueller-Bloch 2016), we expect the sample of firms leaving and entering the market (or Worldscope coverage) to be biased towards certain characteristics such as firm size or profitability. We examine this rationale by estimating different determinants models for firms being affected by our documented sample changes (e.g., firms leaving IFRS or firms becoming covered by Worldscope during our sample period). In essence, we find evidence consistent with prior literature, suggesting that especially small and poor performing firms are entering or leaving the market.

Our third and final set of analyses addresses whether the documented selection effects are sufficiently large to explain the liquidity findings documented in prior IFRS research. This final set involves three steps, and it is based on the benchmark study CHL (2013). To establish meaningful outcome differences, we first replicate the original analyses, that is, CHL's main liquidity regressions, and discuss potential selection effects in the light of the CHL (2013) research design. Consistent with our replication approach, our univariate and multivariate results are fully in line with CHL (2013). In addition, we outline that the CHL (2013) research design with separate time fixed effects for non-IFRS and IFRS countries should effectively control for any control group selection effects due to systematic changes in the database coverage. However, we further argue that their research design fails to address our treatment *sample* selection effect as well as our treatment *self* selection effect.

Therefore, we rerun—in a second step—the main liquidity analyses in CHL (2013), this time explicitly addressing our treatment selection effects. We do so by augmenting the CHL (2013) models by non-overlapping indicator variables that reflect the exposure to our selection effects. To that end, we introduce and validate a country-level *selection exposure index* that comprise systematic sample size changes in the treatment countries during the post-IFRS period. Overall, our findings document that treatment countries with a *high* selection exposure index (and thus with a high exposure to our selection effects) experience significantly *higher* liquidity benefits in the course of mandatory IFRS adoption than their counterparts (i.e., treatment

countries with a low exposure to our selection effects). We further observe that our selection exposure index is able to explain liquidity effects above and beyond the documented IFRS and EU as well as IFRS, EU and enforcement variation (e.g., IFRS_EU_ENF countries with a high selection exposure index benefit stronger from mandatory IFRS adoption than their counterparts with a low exposure index). Thus, our findings suggest that systematic sample and market changes as reflected in our selection exposure index, and with that, the *self and sample selection* of treatment firms in the post-IFRS period, might explain the liquidity findings as documented in CHL (2013).

Third, we differentiate between our two treatment group selection effects—the treatment *self* selection effect and the treatment *sample* selection effect—by re-estimating our augmented model for a balanced sample. In particular, we document that our treatment *self* selection effect (i.e., the quasi self selection of IFRS firms into the treatment group by choosing not to dispense with IFRS through opt-outs) appears to be the driving force behind our findings.

Overall, our paper contributes to the extant literature in several ways. Our findings directly extend the CHL (2013) findings and the Barth and Israeli (2013) discussion on the market liquidity effects around bundled IFRS and enforcement regulation. Specifically, we provide evidence on one potential channel through which concurrent IFRS accounting and enforcement regulation potentially translate into higher market liquidity, namely due to potentially regulation (treatment) induced systematic changes and selections in the underlying sample composition. With that, our findings might further dissolve the perceived inconsistency in the literature on mandatory IFRS adoption, which so far has not been able to fully reconcile evidence on positive capital market benefits (e.g., market liquidity) with inconclusive findings on accounting quality improvements (e.g., Brueggemann et al., 2013). In contrast to the conceptual level of most prior studies on the economic consequences of mandatory IFRS adoption, including CHL (2013), our capital market findings do not inevitably require improvements in accounting or reporting quality as an implicit assumption or precondition for their internal validity. Also, our findings provide a methodological, research design related perspective on the extant IFRS literature, and points out potential research design remedies and avenues to take this literature further. In addition to these research (design) implications, our findings are also of potential interest to regulators, in particular, supra-national regulators such as the EU or the IASB, as we

demonstrate that prior research potentially overstates the benefits of *mandatory* IFRS adoption. More importantly, our findings highlight potential unintended economic consequences of accounting and enforcement regulation.

The remainder of this paper is organized as follows. In Section 2.2, we discuss potential selection effects inherent in mandatory IFRS adoption settings. In Section 2.3, we empirically document the presence of said effects in this setting. In Section 2.4, we test the economic magnitude of these selection effects by replicating and extending the CHL study and discuss research design implications. Section 2.5 concludes.

2.2 Research on mandatory IFRS adoption and potential selection effects

2.2.1 Studies on economic consequences of mandatory IFRS adoption

IFRS research is plentiful, providing insights into various accounting, capital-market and other economic effects around the mandatory adoption of IFRS. Brueggemann et al. (2013), De George et al. (2016), and Leuz and Wysocki (2016) provide comprehensive surveys of this literature. All three surveys jointly note that there is abundant evidence of positive capital market effects upon mandatory IFRS adoption, for example, increases in liquidity, or decreases in companies' cost of equity and debt capital. On the other hand, evidence on the effects of IFRS adoption on desirable accounting properties such as earnings quality or comparability is rather mixed and inconclusive. In addition and especially for market-based outcome variables, the literature on IFRS adoption demonstrates that country-level measures of "enforcement", such as the quality of the judicial system or the level of corruption, are associated with the effects of IFRS adoption, meaning that enforcement quality appears to facilitate positive IFRS adoption outcomes.

More recently, evidence in particular by CHL (2013) provides a different angle on prior findings from the mandatory IFRS adoption literature. The authors detail the role that the installment of enforcement institutions played in shaping IFRS adoption outcomes. The pertinent regulation, that is, the EU's IAS / IFRS directive and ensuing regulation, mandated EU member states to create such enforcement institutions to oversee IFRS compliance by conducting reviews of financial statements on a random basis, and imposing penalties upon non-compliance. CHL (2013) find that these enforcement mechanisms may have indeed played an important role in

establishing positive market effects (i.e., liquidity increases) upon IFRS adoption. Specifically, the authors conclude that this enforcement effect may represent one (of potentially various) correlated omitted variable in the prior IFRS literature. Yet, CHL (2013) do not claim that changes in enforcement alone explain the observed liquidity effects. Rather, they caution that other reasons (economic or research design related) might (also) explain part of their documented findings. Specifically, CHL (2013, p. 172) note that

"[W]hile our research design rules out many concerns about omitted variables and alternative explanations, we acknowledge that other changes to financial reporting (e.g., audit reforms) that are closely aligned with the concurrent enforcement changes could play into our findings. If such other factors exist, they do not alter the main message of this study – there still is a correlated omitted variable problem around IFRS adoption. But it implies that we cannot simply attribute the documented liquidity effects to enforcement changes either."

In addition, Barth and Israeli (2013, p. 186) point out that the findings presented in CHL (2013) are incomplete to the extent that they cannot fully distinguish between effects of changes in enforcement from effects of mandatory IFRS adoption. Given this ongoing debate, our study directly follows up on CHL (2013) and the discussion by Barth and Israeli (2013), by investigating the role of potential correlated omitted variables in the course of mandatory IFRS adoption and enforcement regulation. It is important to note that the purpose of our paper and research design is not to disentangle liquidity effects from enforcement changes to effects from mandatory IFRS adoption. Rather, we are more generally interested whether and how regulatory intensity (e.g., IFRS adoption in the European Union with concurrent enforcement regulation) and other features of the common mandatory IFRS adoption setting might induce potential correlated omitted variables that are able to explain part of the positive capital market benefits as documented in this literature.

2.2.2 Research design challenges in studies on mandatory IFRS adoption

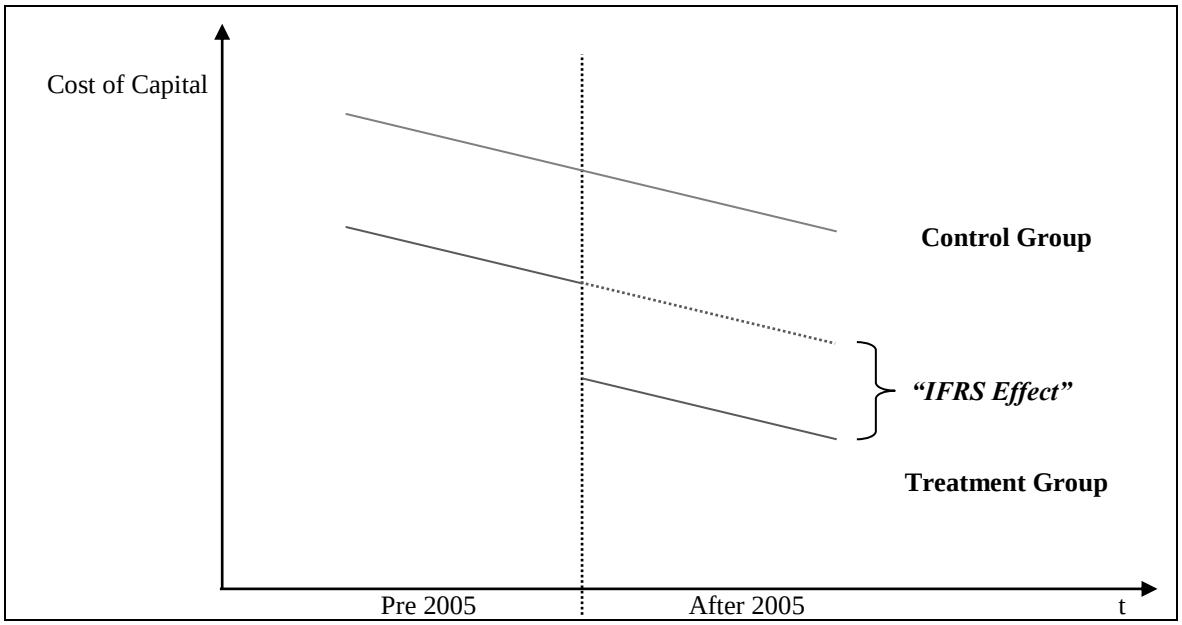
While the IFRS literature is still growing and maturing, so far only little emphasis has been given to methodological aspects. Yet, the three aforementioned surveys on IFRS literature summarize and discuss some of the central research design issues and implications (Brueggemann et al. 2013; De George et al. 2016; Leuz and Wysocki 2016). In addition, recent empirical studies tend

to improve in terms of research design and identification power (e.g., Daske et al. 2008; CHL 2013). In essence, four research design topics are outlined in this literature. First and more generally, prior research highlights research design challenges with respect to the identification of "causal" effects of IFRS adoption, comprising topics such as correlated omitted variables and misspecified regression models (e.g., Barth and Israeli 2013; Brueggemann et al. 2013; Soderstrom and Sun 2007; CHL 2013; Leuz and Wysocki 2016). Second and in a related vein, prior research discusses the suitability of benchmark samples (e.g., Brueggemann et al. 2013, Daske et al. 2008; De George et al. 2016). Research on mandatory IFRS adoption, in particular, cross-country studies, typically employ a DiD design to investigate the causal impact of the IFRS adoption treatment on market, accounting, or other economic outcome variables, incremental to a control group of non-IFRS firms. In the standard DiD design, the outcome variable (e.g., cost of capital) is regressed on an indicator variable for treatment firms (IFRS), on an indicator variable for firm years after IFRS adoption across treatment and control firms (POST), on the interaction of these two variables, and on a set of control variables (including different fixed effect structures):

$$\text{Outcome}_{it} = \gamma_1 + \gamma_2 \text{IFRS}_{it} + \gamma_3 \text{POST}_{it} + \gamma_4 \text{IFRS} \times \text{POST}_{it} + \gamma_5 \text{Controls} + \varepsilon \qquad (1)$$

In this specification, the coefficient estimate on the interaction term (γ_4) captures the presumably causal impact of the IFRS treatment. Figure 2.2 gives a graphical illustration of this identification strategy, using cost of capital as the outcome variable. In essence, in such a DiD design, benchmark firms serve an important role as counterfactuals to mitigate concerns that concurrent and confounding events affect the inferences for treatment firms. So far, prior literature documents that the inferences on IFRS adoption effects potentially vary with the composition of the benchmark sample (e.g., Daske et al. 2008).

Figure 2.2 Differences in Differences Identification Strategy in Research on Mandatory IFRS Adoption



Third, prior research also points out that even the identification of the respective reporting standard, and especially the identification of IFRS reporting, deserves some attention. Specifically, prior research primarily used the Worldscope item "wc07536" (accounting standard followed) to identify the respective accounting standard. However, as outlined by Daske et al. (2013, pp. 535-539), there is some (firm-level) discretion involved when coding this variable (e.g., Worldscope item "wc07536" indicates various different GAAP standards, e.g., local GAAP, IFRS, US GAAP, but also various types of "International Standards" with consistency with IFRS). Fourth, Brueggemann et al. (2013) is one of the few papers that hint at potential selection biases because professional financial databases, which provide the basis for these studies, tend to focus on covering larger firms, which are the very firms which are more likely to benefit from the IFRS mandate.

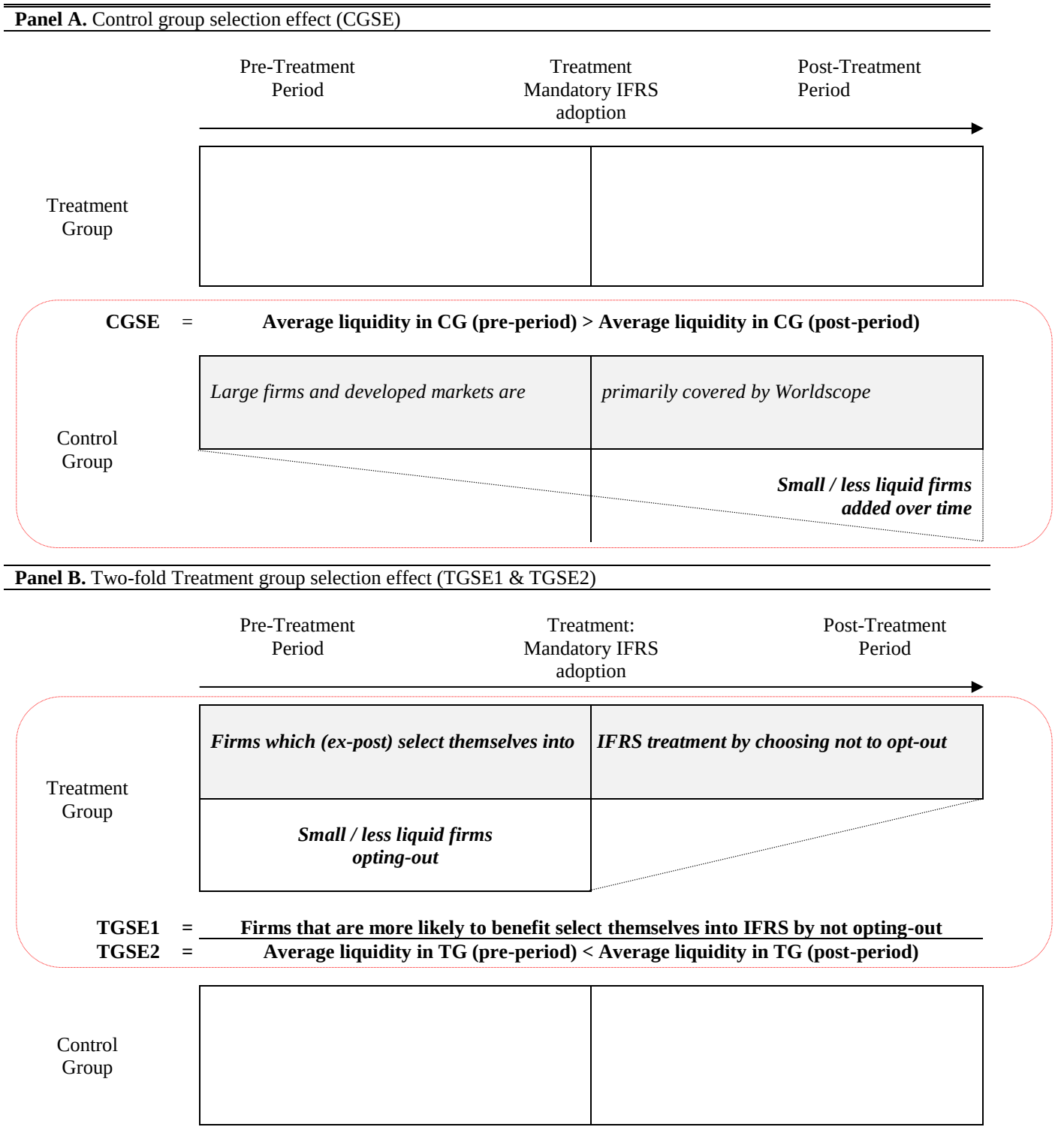
Apart from IFRS research, prior studies, especially in the fields of economics and public finance, provide a rich discussion on merits and research design challenges of DiD estimation in the context of regulatory events (e.g., Abadie 2005; Bertrand et al. 2004; Clair and Cook 2015). In essence, this research suggests that the validity of a DiD design to estimate economic

consequences of regulatory events hinges, among other things, on the exogeneity of the regulatory interventions themselves (e.g., randomized *country*-level treatment selection vs. *country*-level self selection) as well as the suitability of counterfactuals (e.g., parallel trends between treatment and control group). With respect to the former, Angrist and Pischke (2008, p. 241) also highlight the role of *firm*-level selection effects, that is, "when the composition of the treatment and control groups changes as a result of the treatment", as a potential pitfall in this context.

2.2.3 Three-fold selection effect and mandatory IFRS adoption

In this paper, we refine and expand two points of the methodological discussion as outlined in Section 2.2.2, that is, (1) the suitability of counterfactuals (control group selection effect) and (2) firm-level selection effects (two-fold treatment group selection effect), to research on the economic consequences of mandatory IFRS adoption. Figure 2.3 provides a graphical summary of our expected selection effects.

Figure 2.3 Three-fold Selection Effects in Research on Mandatory IFRS Adoption



Notes: CGSE = Control Group Selection Effect, TGSE1 = Treatment Group Self Selection Effect, TGSE2 = Treatment Group Sample Selection Effect.

2.2.3.1 Control group selection effect

Prior research on database coverage conducted by Garcia Lara et al. (2006) documents that professional vendors of financial data choose quite different levels of coverage and that two major determinants of coverage decisions are firm size and the perceived relevance of the respective market for the database vendor's clients. Thomson Reuters, the provider of the Worldscope database, reports, for example, that their coverage decision varies with specific firm and country characteristics such as market capitalization, the coverage by global indices from FTSE, MSCI, S&P, and Dow Jones, or with the country's general state of development. Accordingly, most developed countries received full market coverage since 1999/2003, whereas most of the developing countries obtained partial coverage in the years afterwards and in some cases full coverage only recently. Hence, the observed relative decline of IFRS firms in Figure 2.1 (Section 2.1) may reflect coverage changes over time in response to a growing or declining demand by the customers of Thomson Reuters.[3] Such a coverage effect which is systematically linked to certain firm or country characteristics—most importantly firm size and the country's state of development—potentially biases findings of prior IFRS research in different ways. As Brueggemann et al. (2013) already note, the likely bias of database providers towards covering large firms potentially overstates the positive effects of mandatory IFRS adoption, as these effects are expected to increase with firm size.

However, the potential bias of database providers towards large firms and especially developed markets might additionally affect cross-country IFRS research, if the treatment assignments (i.e., the selection of IFRS treatment countries versus non-IFRS control countries) in the DiD design are systematically correlated with firm and country characteristics. In such cases, coverage changes by database providers can induce *control group selection effects* in research on mandatory IFRS adoption. In fact, based on the treatment and control country selection in CHL (2013), we observe that most IFRS treatment countries have developed markets with full and stable Thomson Reuters' Worldscope coverage, whereas most non-IFRS control countries are less developed with only partial and increasing coverage (see Appendix 2.1 on Worldscope

[3] As documented in Appendix 2.1, the demand for Thomson Reuters' Worldscope database services is stimulated by varies with client groups. Among others, money management firms, investment banks, corporations, consulting firms, and academic institutions are clients of Worldscope. Thus, the coverage by Worldscope is not randomized but rather follows the information demand by their clients.

coverage pattern over time). Consequently, it is plausible to assume that a potentially biased time trend in the control group, biased by the increasing database coverage of (presumably small and less liquid) second- and third-tier firms over time, mechanically decreases the average liquidity within the control group in the post-treatment period. This in turn renders it more likely to find (relative) liquidity benefits for treatment firms in the DiD design. Panel A of Figure 2.3 illustrates this control group selection effect.

2.2.3.2 Two-fold treatment group selection effect

Prior research on accounting and economics commonly acknowledges that in the course of regulatory interventions firms may engage in strategies to avoid the regulation (for an overview, see Leuz and Wysocki, 2016, pp. 536, 555-556). In particular, small firms in the market tend to trade-off cost and benefits of the capital market regulation. Possible avoidance strategies include, among other things, going dark behavior (e.g., switch to a less regulated exchange segment), delistings, as well as threshold management in cases the regulation and its adoption require a financial threshold that has to be met.

Going back at least to empirical studies on the economic effects of the US Securities Act of 1933 and the US Exchange Act of 1934, the literature already outlines the role of potential selection and composition effects in mitigating the validity of the documented treatment effects. Specifically, Benston (1969, p. 527) and Simon (1989, p. 313), among others, argue that the US regulations in the 1930s might have pushed riskier securities to less regulated markets and that this in turn might have biased the average treatment effect as documented in this literature. In other words, these studies imply that findings on the economic consequences of the respective regulations might be affected by firm-level selection effects in the treatment group. In particular, Simon (1989, p. 313) notes that

"In fact, the 1933 Act and subsequent regulation contributed to the growth of the Over-the-Counter market as issuers sought lower costs, unregulated markets. Excluding the OTC from this study imparts a selection bias on the findings. The extent to which SEC regulation shifted riskier securities to unregulated markets is an important issue to be addressed in future research."

In a similar vein, more recent studies on the economic consequences of the Sarbanes-Oxley Act (SOX) provide evidence consistent with the notion that firms tend to trade off cost and benefits of the SOX regulation. Leuz et al. (2008), for example, document a significant increase in firms, especially small and poor performing firms, which deregister with the Security and Exchange Commission after the introduction of the SOX in the US in 2002. Overall, prior research in economics and accounting provides rich evidence consistent with firm-level incentives to avoid regulatory consequences. More importantly, as noted in Leuz and Wysocki (2016, p. 556), "[t]his discussion [also] highlights the importance of controlling for firms' responses to the regulation, which can result in sample composition changes in the postperiod."

The IFRS literature, however, remains surprisingly silent on whether firm-level avoidance strategies might occur in the aftermath of mandatory IFRS adoption and concurrent enforcement regulation. Yet, in a recent study, Hitz and Mueller-Bloch (2016) document that since 2005 a substantial number of listed firms in Germany dispensed with their listing in the regulated market, effectively opting out of the IFRS and enforcement mandates. The authors show that firms that opt out, on average, are relatively small, have relatively little profitability, low liquidity, and have more likely been censured by the enforcement institutions for preparing erroneous financial statements.

In a similar vein, a recent study conducted by Fiechter et al. (2016) documents that IFRS firms may choose to change their reporting standards without altering listing status. Although such an explicit choice of reporting standard is at odds with the notion of mandatory IFRS adoption, the authors document that in Switzerland, such an explicit option has recently been introduced. The authors show that on the Swiss Stock Exchange, a recent rule change which allows firms to opt-out of IFRS in favor of local Swiss GAAP was embraced in particular by small firms. Official press releases by these firms make it evident that they did not see sufficient benefits of the increased transparency that presumably comes with IFRS reporting. Hence, similar to the Hitz and Mueller-Bloch (2016) findings, specific firms that benefited little from the IFRS mandate decided to opt-out.

A more recent study by Gutierrez et al. (2017) provides evidence on the determinants of voluntary and forced delistings of IFRS firms in the post-IFRS period. Supporting the findings of

Hitz and Mueller-Bloch (2016) at a cross-country level, the authors show a higher probability of delistings in strong IFRS enforcement jurisdictions.[4]

Given this evidence, we thus propose that the relative decline of listed IFRS firms compared to listed non-IFRS firms as documented in Figure 2.1 (Section 2.1) might reflect economic externalities of increased accounting and enforcement regulation. In other words, assuming that IFRS and enforcement regulation systematically affects the probabilities of delistings or downlistings, we would expect to observe systematic differences in the number of listed firms across IFRS and non-IFRS markets following the regulatory event. Moreover, as these systematic changes are induced by specific firms with specific firm characteristics (e.g., as outlined by prior research, small and poorly performing firms with low liquidity are more likely to choose to delist or downlist following capital market regulation), cross-country IFRS research might become prone to a correlated omitted variable (i.e., treatment selection effect). If a study investigates, for example, transparency effects of mandatory IFRS adoption, it is likely that over time, IFRS firms will do better in terms of measures of transparency benefits than non-IFRS firms, as they select not to dispense with IFRS by not choosing to delist or downlist for that very reason. From an econometrician's perspective, treatment selection means that the IFRS treatment assignments become less random as remaining IFRS firms effectively select themselves (ex-post) into the treatment group by choosing not to dispense with IFRS through opt-outs. They do so based on their own net benefit expectation of the IFRS treatment (e.g., expected net benefits due to improved liquidity upon IFRS adoption).

We concur that this *treatment selection effect* renders it more likely to find liquidity benefits in the post-treatment period compared to a randomized treatment group. Moreover, it is important to note that we expect this treatment selection effect to be generally contingent on regulatory intensity and costs. At face value, the treatment selection effect should be stronger in IFRS countries with concurrent EU or enforcement regulation compared to those countries that "only" witnessed IFRS regulation. Besides this *core* treatment selection effect, we further expect—in line with the underlying mechanics of the control group selection effect—that the

[4] In a related study, Pownall and Wieczynska (2017) provide evidence on the non-adoption of IFRS in the EU by documenting that not all firms from IFRS mandating EU countries eventually adopt IFRS in the post IFRS period. The authors show that especially small firms with less analyst following and less new debt and equity issuances are more likely to not adopt IFRS in the post IFRS adoption period.

mere changes in the sample composition of the treatment group (i.e., the decrease in sample size in post-IFRS period due to opt-outs and exits of potentially small and less liquid firms) might additionally foster, rather mechanically, the documented treatment effect in prior research on mandatory IFRS adoption. Similar to the control group selection effect, this second treatment selection effect does not inevitably require the underlying firm-level *self selection* component as described above (e.g., that treatment firms *choose* to dispense or not to dispense with IFRS reporting). It rather relies on aggregated (forced or voluntary) sample changes in the post-treatment period and requires these sample changes to be correlated with the treatment event. As both treatment selection effects differ in terms of research design remedies and the self-selection component as outlined above, we label the former and core treatment selection effect as "treatment *self* selection effect" and the latter, rather mechanical second treatment effect as "treatment *sample* selection effect". Panel B of Figure 2.3 illustrates this two-fold treatment group selection effect.

2.2.3.3 Research Design Remedies

Table 2.1 summarizes our three-fold selection problem, that is, the control group selection effect and the two treatment group selection effects, and outlines potential research design remedies. With respect to the former, we expect that two standard econometric techniques should mitigate this selection effect. First, separate time fixed effects for the treatment and control group should control for any liquidity effects induced by changes in the control group composition over time. Second, a balanced sample approach should likewise control for this control group selection effect.

However, both standard econometric techniques do not seem to play a prominent role in prior research on mandatory IFRS adoption. In fact, as to our knowledge, only CHL (2013) employ combined treatment and time fixed effects (e.g., two quarter-year trends for IFRS treatment countries and non-IFRS control countries). In addition, based on the 25 recent IFRS studies, which are discussed by Brueggemann et al. (2013), only six studies appear to consider a balance sample approach.[5] Nevertheless, both research design remedies come with potential costs

[5] Ahmed et al. (2013) explicitly introduce a balanced sample approach. Five additional studies seem to rely on a balanced sample approach as well without noting it explicitly (i.e., Callao and Jarne 2010; Aharony et al. 2010; Byard et al. 2011; Beuselinck et al. 2010; DeFond et al. 2011).

or more severe setting requirements. In the former case, the combined fixed effect structure requires the regulatory intervention to have within treatment group variation of the treatment timing (e.g., calendar year variation across countries and/or firm-level fiscal year end variation of IFRS adoption as exploited in CHL 2013). In the latter case, the balanced sample approach induces the classical survivorship bias (Brueggemann et al. 2013).

An alternative approach to address our control group selection effect might employ a matching strategy to select control countries with a treatment-like database coverage pattern and country-level state of development. However, this approach naturally requires sufficient variation in the control group regarding the matching parameters. Given the non-randomized and ever growing geographical extension of IFRS accounting around the world, this variation might be limited (e.g., De George et al. 2016, p. 82). In fact, 114 out of 138 jurisdictions worldwide require IFRS accounting standards in 2015 for most of their public firms (IFRS Foundation, 2015).

Regarding our two treatment selection effects, we expect that the aforementioned econometric techniques might be of limited use to constrain both selection problems. Although combined treatment-time fixed effects and a balanced sample structure should—in principle—control for the treatment sample selection effect, our core treatment selection effect, that is, the ex-post firm-level *self selection* into the treatment group by choosing not to dispense with the IFRS mandate, will remain part of the DiD design.

For a regulatory setting—apart from the mandatory IFRS adoption setting—to be robust to *self selection* concerns in the course of post regulatory avoidance behavior, one of the two following and more general setting requirements has to be met. The expected outcome effects (i.e., liquidity effects due to the regulation) materialize faster than potential self selection effects (i.e., liquidity effects due to treatment self selection). In that case, the researcher's sample period choice might consider a shorter post-treatment period that primarily includes the expected outcome effects and excludes potential self selection effects. Alternatively, the regulatory setting itself does not allow for any avoidance behavior (e.g., by legally prohibiting any ex-post avoidance of the regulatory action). However, in most *firm-level* capital market or accounting regulation settings, both requirements are rather unlikely to expect or find. For example,

transition periods, learning effects, and short- versus long-term effects might constrain the former requirement, whereas managerial discretion, global capital market and firm (MNE) structures, and more generally the freedom of contract—and thus the freedom to change the legal status of a company—might constrain the latter. In Section 2.4, we will extend this methodological discussion to the CHL (2013) research design.

Table 2.1 Three-fold Selection Effect and Research Design Remedies

Panel A. Summary of the three-fold selection effect	
TGSE1	Treatment firms that are more likely to benefit from the IFRS treatment *select themselves* (ex-post) into the treatment by choosing not to dispense with the IFRS mandate. This in turn inflates the average treatment effect. In other words, it increases the likelihood of observing positive treatment effects for the remaining, self selected treatment firms. ❖ Firm-level self-selection effect within the treatment group.
TGSE2	*Systematic changes* in the *sample* composition of the *treatment group*, that is, the *decrease* in sample size in post-IFRS period due to opt-outs and exits of presumably small and less liquid treatment firms, inflates the average treatment effect in a DiD design. In other words, it increases the likelihood of observing positive treatment effects within a DiD design. ❖ Systematic sample composition effect within the treatment group.
CGSE	*Systematic changes* in the *sample* composition of the *control* group, that is, the *increase* in sample size due to an increasing database coverage of small and less liquid (second- and third-tier) control firms over time, inflates the average treatment effect in a DiD design. In other words, it increases the likelihood of observing positive treatment effects within a DiD design. ❖ Systematic sample composition effect within the control group.

Panel B. Research design remedies: Fixed-effect (FE) and/or balanced sample approach

	DiD research design as commonly used in research on mandatory IFRS adoption with … (e.g., CHL 2013 with 35 IFRS treatment countries and 21 non-IFRS control countries)				
	… unbalanced sample (Models 1 to 4)				**… balanced sample**
	Plain Vanilla (w/o FE structure)	Control Group matching	Time and / or country FE	Separate time FE for treatment & control	From Plain Vanilla to separate time FE
	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*
TGSE1	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
TGSE2	**Yes**	**Yes**	**Yes**	No	No
CGSE	**Yes**	No	**Yes**	No	No

Notes: CGSE = Control Group Selection Effect, TGSE1 = Treatment Group Self Selection Effect, TGSE2 = Treatment Group Sample Selection Effect.

2.3 Presence of selection effects around mandatory IFRS adoption

To empirically examine our proposed three-fold selection effect, we develop a series of analyses in Sections 2.3 and 2.4 comprising tests to assess the presence as well as the magnitude of these selection effects. In essence, these analyses involve three main steps (two in Section 2.3 and one in Section 2.4).

2.3.1 Worldscope coverage of IFRS and non-IFRS firms

Our first set of analyses revisits Figure 2.1 and examines the relative decline in the number of listed firms as documented in Worldscope database within univariate as well as multivariate (country-level) DiD design comprising country and year fixed effects as well as variation in the sample period. Table 2.2 presents the respective results. In response to Figure 2.1, Panel A of Table 2.2 details the number of firms listed across different IFRS and non-IFRS country-clusters between the years 1996 and 2014. Specifically, we follow CHL (2013) and employ the following country-clusters: IFRS countries, EU IFRS countries, EU IFRS countries with concurrent enforcement changes, EU IFRS countries without concurrent enforcement changes, non-EU IFRS countries, and non-IFRS (control) countries (Appendix 2.2 details the respective country-cluster composition).[6] In line with Figure 2.1, we observe a substantial decline in listed IFRS firms in EU countries, which concurrently changed their enforcement regulation in 2005, by 28% (3,277 listed firms in 2005 compared to 2,361 in 2014). In comparison, Panel A of Table 2.2 reveals more moderate or even positive changes in the number of listed firms over the same time period (1) for EU countries that introduced the IFRS mandate *without* concurrent enforcement regulation (-17%, with 3,106 listed firms in 2005 compared to 2,578 in 2014), (2) for *non*-EU countries that mandate IFRS (+1.7%, with 3,534 listed firms in 2005 compared to 3,594 in 2014), as well as (3) for countries which did *not* adopt the IFRS mandate (-0.4%, with 18,536 listed firms in 2005 compared to 18,457 in 2014).

[6] For all analyses considering a sample period between 2001 and 2009, we follow the control group composition as suggested by Christensen et al. (2013). For all analyses considering the complete sample period (1995-2014), we use an adjusted control group comprising only countries without mandatory IFRS adoption until 2014 (for details, see Appendix 2.2).

Table 2.2 Worldscope Coverage and CHL (2013) Country-Clusters

Panel A. Distribution of number of listed firms across CHL Country-Clusters

Year	*CONTROL*		*IFRS*		*IFRS_EU*		*IFRS_EU_ENF*		*IFRS_EU_nonENF*		*IFRS_nonEU*	
	#Firms	*Δ in # F*	*#Firms*	*Δ in # F*	*#Firms*	*Δ in # F*	*#Firms*	*Δ in # F*	*#Firms*	*Δ in # F*	*#Firms*	*Δ in # F*
1996	10971	15.4%	3069	8.1%	2400	7.0%	1276	6.2%	1124	8.0%	669	12.4%
1997	12280	11.9%	3339	8.8%	2552	6.3%	1359	6.5%	1193	6.1%	787	17.6%
1998	15172	23.6%	3766	12.8%	2847	11.6%	1546	13.8%	1301	9.1%	919	16.8%
1999	16193	6.7%	4245	12.7%	3130	9.9%	1670	8.0%	1460	12.2%	1115	21.3%
2000	16389	1.2%	5550	30.7%	3933	25.7%	2005	20.1%	1928	32.1%	1617	45.0%
2001	16508	0.7%	6485	16.8%	4310	9.6%	2233	11.4%	2077	7.7%	2175	34.5%
2002	16595	0.5%	7437	14.7%	4997	15.9%	2594	16.2%	2403	15.7%	2440	12.2%
2003	17014	2.5%	7996	7.5%	5404	8.1%	2816	8.6%	2588	7.7%	2592	6.2%
2004	17484	2.8%	8717	9.0%	5833	7.9%	3046	8.2%	2787	7.7%	2884	11.3%
2005	18536	6.0%	9917	13.8%	6383	9.4%	3277	7.6%	3106	11.4%	3534	22.5%
2006	20000	7.9%	10357	4.4%	6572	3.0%	3419	4.3%	3153	1.5%	3785	7.1%
2007	20334	1.7%	10402	0.4%	6468	-1.6%	3357	-1.8%	3111	-1.3%	3934	3.9%
2008	20268	-0.3%	10271	-1.3%	6237	-3.6%	3173	-5.5%	3064	-1.5%	4034	2.5%
2009	20421	0.8%	9978	-2.9%	5959	-4.5%	2954	-6.9%	3005	-1.9%	4019	-0.4%
2010	20449	0.1%	9854	-1.2%	5795	-2.8%	2833	-4.1%	2962	-1.4%	4059	1.0%
2011	20317	-0.6%	9760	-1.0%	5700	-1.6%	2785	-1.7%	2915	-1.6%	4060	0.0%
2012	20049	-1.3%	9367	-4.0%	5447	-4.4%	2660	-4.5%	2787	-4.4%	3920	-3.4%
2013	19425	-3.1%	8959	-4.4%	5214	-4.3%	2513	-5.5%	2701	-3.1%	3745	-4.5%
2014	18457	-5.0%	8533	-4.8%	4939	-5.3%	2361	-6.0%	2578	-4.6%	3594	-4.0%
Δ since 2005	**-0.43%**		**-13.96%**		**-22.62%**		**-27.95%**		**-17.00%**		**1.70%**	

Panel B. Median change in number of firms across pre- & post IFRS sample period

	CONTROL	*IFRS*	*IFRS_EU*	*IFRS_EU_ENF*	*IFRS_EU_nonENF*	*IFRS_nonEU*
Δ 1996-2004	.0727	.0888	.08	.0714	.0836	.1270
Δ 2001-2004	.0835	.0888	.0836	.0718	.0853	.1036
Δ 2005-2009	.0247	.0182	0	-.0211	0	.0385
Δ 2005-2014	**.0009**	**-.0114**	**-.0183**	**-.0322**	**-.0160**	**.0083**

Panel C. Significance tests on median change Diff in # firms across pre- & post-IFRS period

P-Value	Sample period I: **1996-2004** (***above*** the diagonal) and sample period II: **2005 and 2014** (***below*** the diagonal)					
	CONTROL	IFRS	IFRS_EU	IFRS_EU_ENF	IFRS_EU_nonENF	IFRS_nonEU
CONTROL	1	**0.0391**	0.1399	0.2968	0.1655	0.0135
IFRS	**0.0001**	1	**0.0595**	0.3948	0.2626	**0.0595**
IFRS_EU	**0.0000**	**0.0001**	1	0.8124	0.8124	**0.0595**
IFRS_EU_ENF	**0.0000**	**0.0049**	**0.0873**	1	0.8124	0.1097
IFRS_EU_nonENF	**0.0000**	0.1406	**0.0873**	**0.0873**	1	**0.0934**
IFRS_nonEU	0.8244	**0.0001**	**0.0001**	**0.0001**	**0.0010**	1

Panel D. Country-year DiD and all CHL country clusters

	Dependent Variable: zCOUNT_DATA (*#firms* per year and country, standardized over 2005 value)					
	Sample Period: **1995-2014**			Sample Period: **2001-2009**		
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*
(1) IFRS×POST	-0.2547			-0.1141		
	(-1.42)			(-0.96)		
(2) IFRS_EU ×POST		-0.3337*			-0.1410	
		(-1.84)			(-1.09)	
(3) IFRS_nonEU ×POST		-0.0812	-0.0812		-0.0507	-0.0507
		(-0.42)	(-0.42)		(-0.40)	(-0.39)
(4) IFRS_EU-Enf ×POST			-0.4507**			-0.2822**
			(-2.51)			(-2.42)
(5) IFRS_EU-nonEnf ×POST			-0.2949			-0.0943
			(-1.57)			(-0.65)
F-test for Diff. (p-value)		(2) vs. (3)	(3) vs. (4)		(2) vs. (3)	(3) vs. (4)
		0.0270	0.0013		0.4119	0.0158
			(4) vs. (5)			(4) vs. (5)
			0.1125			0.1050
Year & Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE (country)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	898	898	898	491	491	491
Adj. R-squared	0.6313	0.6474	0.6507	0.4672	0.4690	0.4760

Notes: For the underlying sample / country composition, see Appendix 2. For Panel D, *t* statistics in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel B of Table 2.2 provides complimentary information on median changes in the number of firms across the pre- and post-IFRS sample period. In particular, we observe that the average country-level change in the number of listed firms after 2005 is most pronounced for the IFRS_EU_ENF country-cluster (-2.11% for the period between 2005 and 2009, and -3.22% for the period between 2005 and 2014). In comparison, the corresponding changes across our CONTROL and IFRS_nonEU country-clusters are positive with 0.09% for the period between 2005 and 2014 (2.47% for the period between 2005 and 2009) and 0.83% for the period between 2005 and 2014 (3.85% for the period between 2005 and 2009), respectively. In addition, Panel C of Table 2.2 reveals that most of the country-cluster changes are significantly different from each other in the *post*-IFRS period, whereas the development across countries and country-clusters in the *pre*-IFRS period appears to be to some extent similar. Specifically, the differences between the changes in the CONTROL country-cluster and the different IFRS country-clusters are in almost all cases statistically significant for the *post*-IFRS period (with the only exception being the CONTROL and IFRS_nonEU pair in the post-IFRS period with a p-value of 0.8244). In contrast, the corresponding differences in the *pre*-IFRS period are in most cases insignificant (especially the pre-2005 changes in the average number of listed firms across the CONTROL and the different European IFRS country-clusters are statistically insignificant, with p-values ranging from 0.14 to 0.30).

Panel D of Table 2.2 reports results from using multivariate analyses to explore the reported cluster trends. Specifically, we estimate the following *country-level* DiD regression with year and country fixed effects:

$$\begin{aligned} \text{zCOUNT\_DATA}_{it} = {} & \gamma_1 + \gamma_2 \text{IFRS\_nonEU} \times \text{POST}_{it} + \gamma_3 \text{IFRS\_EU\_Enf} \times \text{POST}_{it} \\ & + \gamma_4 \text{IFRS\_EU\_nonEnf} \times \text{POST}_{it} + \gamma_5 \text{COUNTRY FE} + \gamma_6 \text{YEAR FE} + \varepsilon \end{aligned} \tag{2}$$

For a given variable of interest (i.e., zCOUNT_DATA), regression model (2) compares the changes in the treatment group (IFRS-adopting countries) around the "exogenous" event (mandatory adoption of IFRS in 2005) to the corresponding changes in the "non-treated" control group (non-IFRS adopting countries). In line with our descriptive analyses, we use as our dependent variable zCOUNT_DATA, which measures the relative change in number of firms per

year and country (i.e., zCOUNT_DATA is scaled by the respective absolute values on total number of listed firms per country in 2005). IFRS_nonEU, IFRS_EU_Enf, IFRS_EU_nonEnf reflect the different country-clusters as defined in CHL (2013) (see Appendix 2.2 for further information). COUNTRY FE and YEAR FE represent country and year fixed effects. This fixed-effect structure controls for the underlying main effects of the DiD regression as well (i.e., POST and IFRS Treatment group).[7] In all reported regression models, the standard errors are heteroskedasticity robust (White, 1980) and one-way clustered at the country-level (Gow et al., 2010; Petersen 2009).

Corroborating our descriptive findings, we observe that especially IFRS_EU-ENF countries experience a significant decline in the number of firms listed in the period after 2005 (see column 3, Panel D, Table 2.2). These findings hold for different sample windows (columns 1 to 3 refer to the sample period 1995-2014, and columns 4 to 6 consider a sample period between 2001 and 2009).

Taken together, our descriptive analyses underscore that there is a significant relative decline of IFRS_EU_ENF firms, in particular, compared to those firms which are typically used as control firms in contemporary DiD designs. As outlined in Section 2.2, this relative decline might represent selection effect issues if it is non-random. Moreover, it might provide novel insights into the economic effects of mandatory IFRS adoption (e.g., unintended consequences of mandatory IFRS regulation). Thus, we are interested in whether firms with specific characteristics, for example, firms with potential firm-level benefits (costs) of IFRS adoption, are systematically selected into or (out of) respective treatment and control groups.

2.3.2 Firm-level characteristics and changes in Worldscope coverage

In Section 2.2, we argue that the documented changes in the number of listed firms across the different IFRS and non-IFRS country clusters—as reflected in Figure 2.1 (Section 2.1)—are driven by revised coverage decisions on behalf of Worldscope (e.g., increasing coverage for countries with *partial* coverage status) and / or by real effects such as firm-level changes in the delisting, downlisting or IPO behavior (i.e., post regulation avoidance behavior). Following this rationale, we expect the sample of firms leaving IFRS (getting covered by Worldscope) to be

[7] POST is a dummy variable indicating with 1 the sample period after 2005 (IFRS period).

biased towards certain characteristics such as firm size or profitability. We examine this rationale by estimating determinants on the firms being affected by our documented sample changes (e.g., firms leaving the IFRS coverage or firms becoming covered by Worldscope during our sample period). In essence, we estimate the following two determinant models:

$$\text{SURVIVORSHIP}_{it} = \gamma_1 + \gamma_2 \text{Log\_TA}_{it} + \gamma_3 \text{ROE}_{it} + \gamma_4 \text{LOSS}_{it} + \gamma_5 \text{COUNTRY FE} + \gamma_6 \text{YEAR FE} + \varepsilon \quad (3)$$

$$\text{GAAP\_SWITCH}_{it} = \gamma_1 + \gamma_2 \text{Log\_TA}_{it} + \gamma_3 \text{ROE}_{it} + \gamma_4 \text{LOSS}_{it} + \gamma_5 \text{COUNTRY FE} + \gamma_6 \text{YEAR FE} + \varepsilon \quad (4)$$

Where SURVIVORSHIP represents a dummy variable indicating with one if a firm is constantly part of the respective sample period under investigation. In contrast, GAAP_SWITCH indicates whether an IFRS-adopting firm turns back to local GAAP during the respective sample period (i.e., GAAP_SWITCH is a dummy variable assigning the value of one to all firm-year observations of firms switching back to local GAAP from IFRS based on Worldscope item "wc07536", and zero otherwise). In addition, Log_TA stands for log of firm's total assets (measured in US Dollar), ROE is return-on-equity, and LOSS is a dummy variable indicating firm-periods with negative net income. COUNTRY FE and YEAR FE represent country and year fixed effects. In all reported regression models, the standard errors are heteroskedasticity robust (White, 1980) and one-way clustered at the firm-level (Gow et al., 2010; Petersen 2009). It is important to note that both models (Models 3 and 4) do not claim to examine these market and coverage changes "causally". Instead, they intend to provide descriptive evidence on the line of argument put forward in Section 2.2—with respect to potential Worldscope coverage changes and post regulation avoidance behavior in the setting of mandatory IFRS adoption—and complement our country-level analysis as discussed in Section 2.3.1.

With respect to our first determinant model (Model 3), we estimate a *classical* survivorship bias by comparing firm characteristics across firms staying in the market (coverage) to firms leaving or entering the market (coverage). Assuming that database providers are biased towards large firms (e.g., Garcia Lara et al., 2006) and that especially small and poor performing firms are leaving the market—either forced by mergers or bankruptcies or voluntarily by choosing to delist or downlist (e.g., Leuz et al., 2008)—we expect to find positive (negative) and significant coefficient estimates on Log_TA and ROE (LOSS). Note, however, that this first

determinant model does not distinguish between different sources of coverage changes and thus rather provides an aggregated perspective on firm-level determinants in our setting.

With respect to our second determinant model (Model 4), we assess firm-level differences between firms adopting IFRS and staying with this reporting standard compared to firms adopting IFRS in the first place but turning back to local GAAP afterwards (and thus leaving IFRS coverage as well). In contrast to our first and more general determinant model, this analysis explicitly considers coverage changes induced by the *supply side* of professional database providers (e.g., corporate events, such as delistings or downlisting). As noted earlier, Hitz and Mueller-Bloch (2016) already provide single-country evidence on this firm-level (going-gray) behavior. Recall that the authors examine firms that choose to downlist from the German EU-regulated market to an exchange-regulated market, and thereby dispense with various compliance requirements, among them the mandate to prepare IFRS financial statements, but also supervision by the external enforcement mechanism, as stipulated by the EU IFRS regulations. Hitz and Mueller-Bloch (2016) find evidence that a substantial number of firms embraced this opportunity, in particular small, less profitable firms, and firms that had previously been censured by the enforcement mechanism for materially erroneous accounting. In line with these findings, we predict negative (positive) and significant coefficient estimates for Log_TA and ROE (LOSS).

Table 2.3 Changes in Worldscope Coverage and Firm-Level Characteristics

Panel A. Survivorship Bias (constantly covered firms vs. firms that entry / exit the market)

	Dependent Variable: SURVIVORSHIP (dummy variable indicating with 1 if the firm is constantly part of the sample during the respective sample period)							
	IFRS FIRMS				NON-IFRS FIRMS			
	ALL	2001-2009	2005-2014	2005-2009	ALL	2001-2009	2005-2014	2005-2009
	Column 1	*Column 1*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*	*Column 8*
Log_TA	0.2333***	0.1283***	0.1057***	0.0835***	0.2316***	0.1474***	0.1254***	0.1126***
	(38.78)	(29.02)	(26.15)	(18.76)	(45.97)	(44.13)	(42.57)	(39.36)
ROE	-0.0001	-0.0001	-0.0001	-0.0001*	-0.0000***	-0.0002	-0.0000	-0.0001
	(-1.00)	(-0.72)	(-1.17)	(-1.86)	(-3.37)	(-1.20)	(-0.10)	(-1.63)
LOSS	-0.2094***	-0.0356**	-0.1397***	-0.1132***	-0.1446***	-0.0834***	-0.0947***	-0.0393***
	(-11.39)	(-2.32)	(-9.38)	(-6.30)	(-10.03)	(-6.67)	(-7.81)	(-2.72)
Year and Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE (firm)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	222897	115935	134032	69574	418767	210411	256431	128362
Pseudo r2	0.2001	0.1037	0.0699	0.0597	0.2285	0.1586	0.1302	0.1301
Additional Information								
# Survivorship firms (p.a.)	2236	6806	8365	10957	4288	12250	14967	19072
# Survivorship firm-years	44720	61254	83650	54785	85760	110250	149670	95360
% Survivorship firm-years	20.06%	52.83%	62.41%	78.74%	20.48%	52.40%	58.37%	74.29%

Panel B. IFRS-Local GAAP Switch (IFRS firms vs. firms that switch back to local GAAP)

	Dependent Variable: GAAP_SWITCH (dummy variable indicating with 1 if the firm switches back from IFRS to local GAAP)							
	Sample Period: **1996-2014**				Sample Period: **2001-2009**			
	IFRS	IFRS_ EU_ENF	IFRS_ EU_nonENF	IFRS_ nonEU	IFRS	IFRS_ EU_ENF	IFRS_ EU_nonENF	IFRS_ nonEU
	Column 1	*Column 1*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*	*Column 8*
Log_TA	-0.2664***	-0.2895***	-0.3545***	-0.1956***	-0.1076***	-0.1449***	-0.2043***	-0.0411**
	(-12.09)	(-6.81)	(-6.59)	(-6.19)	(-8.75)	(-5.71)	(-6.73)	(-2.33)
ROE	0.0001	-0.0014	0.0002***	0.0063	0.0001	0.0031	0.0005	0.0000
	(1.19)	(-0.71)	(2.71)	(0.91)	(0.28)	(1.40)	(0.47)	(0.11)
LOSS	0.2417***	0.4058***	0.1200	0.2315***	0.0591	0.1651**	0.0283	0.0293
	(4.98)	(4.46)	(1.31)	(2.90)	(1.49)	(2.20)	(0.39)	(0.48)
Year and Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE (firm)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	46670	16224	14543	15803	60949	16546	15607	28616
Pseudo r2	0.3505	0.4336	0.3360	0.3333	0.3631	0.2426	0.1626	0.4913
Additional Information								
# GAAP-switch firm-year	8274	1497	1545	5232	4428	851	829	2748
% GAAP-switch f-y	17.73%	9.23%	10.62%	33.11%	7.27%	5.14%	5.31%	9.60%

Notes: This table displays changes in the Worldscope database's coverage (Panel A) and switches from IFRS to local GAAP by firms (Panel B).

Table 2.3 reports the corresponding results. Specifically, we observe in Panel A of Table 2.3 that firm size as measured by log of total assets and profitability as measured by loss reporting obtain the expected signs and become highly significant in the respective models (e.g., t-values for log of total assets range between 18.76 and 45.97). This survivorship bias prevails in both the treatment group and the control group and does not vary with the underlying sample period. Corroborating these findings, Panel B of Table 2.3 documents—in line with Hitz and Mueller-Bloch (2016)—that switching firms are significantly smaller and less profitable than their

counterparts. These findings are consistent across different time periods and different treatment groups.

In sum, our first two analyses indicate a systematic and size-related selection effect across IFRS and non-IFRS countries in the course of mandatory IFRS adoption in 2005. From a statistical perspective, this in turn might suggest that—in line with Section 2.2—empirical research on mandatory IFRS adoption is prone to selection effects as the so far documented IFRS / enforcement market benefits might be driven by the systematic variation in the underlying sample composition.

2.4 Economic magnitude of selection effects around mandatory IFRS adoption

Our third set of analyses addresses whether the predicted and documented selection effects are meaningful and sufficiently large to explain the outcomes of extant IFRS studies. For this purpose, we move on to fully replicate our benchmark study on mandatory IFRS adoption, namely, the study by CHL (2013).

2.4.1 CHL (2013) replication

We first start with all firm-quarter observations from countries included in the CHL (2013) sample for the period between 2001 and 2009. Following the selection steps as reported by CHL (2013), we yield a final (bid-ask spread) sample comprising 727,293 firm-quarter observations (in comparison, CHL (2013) report a final bid-ask spread sample of 613,752 firm-quarter observations).[8] Although our sample size is slightly higher compared to CHL (2013), our descriptive analysis documents a similar sample pattern for our main dependent and independent variables. Specifically, we observe a median bid-ask spread of 0.011 (vs. 0.010), median lagged market value of 117.47 (vs. 126), median lagged share turnover of 0.001 (vs. 0.001), and median lagged return variability of 0.026 (vs. 0.025). Table 2.4 summarizes our main sample selection steps, provides a descriptive analysis of our main variables, and presents correlation coefficients between these variables.

[8] Appendix 2.3 details country-level differences between our sample and the CHL (2013) sample. One reason for the sample difference might be the time lag in data download. As noted by Thomson Reuters, the database provider of Worldscope and Datastream, back-filing occasionally takes place over time in the process of increasing coverage.

Table 2.4 Sample Selection and Descriptive Statistics for the CHL (2013) replication (Section 2.4)

Panel A. Sample Selection

Selection Criteria		**Observations**
Start (All firm-quarters from Worldscope country lists of countries included in CHL 2013 from 2001-2009)		2,696,760
Bid-ask data, liquidity data, or accounting standards data unavailable	-1,815,796	880,964
Firms following U.S.GAAP outside the United States	-14,068	866,896
Firms trading on unregulated markets	-13,955	852,941
Firms with average market value below US$ 5 million	-47,938	805,003
Firms without fiscal year end data during mandatory IFRS adoption period	-16,816	788,187
Firm-quarters of IFRS adopters without IFRS adoption in the mandatory adoption period	-235	787,952
Truncation of all continuous variables (1% level)	-60,390	727,562
Obs. within country-quarters with less than five observations (Bid-Ask Spread Sample)	-269	**727,293**

Panel B. Descriptive Statistics

	N	**Mean**	**Std. Dev**	**P1**	**P25**	**Median**	**P75**	**P99**
Bid-Ask Spread$_t$	727,293	0.029	0.049	0.001	0.004	0.011	0.030	0.264
Zero Returns$_t$	713,571	0.253	0.227	0.030	0.077	0.167	0.364	0.908
Price Impact$_t$	720,139	4.462	18.413	0.000	0.012	0.128	1.276	91.827
Liquidity Factor$_t$	660,793	0.072	1.481	-0.991	-0.797	-0.481	0.353	6.428
Market Value$_{t-4}$	727,293	1050.988	5879.694	2.374	32.066	117.472	494.008	17113.236
Share Turnover$_{t-4}$	727,293	0.006	0.228	0.000	0.000	0.001	0.004	0.053
Return Variability$_{t-4}$	727,293	0.034	0.466	0.006	0.018	0.026	0.040	0.126

Panel C. Pearson's Correlation Coefficients

	Zero Returns	**Price Impact**	**Liquidity Factor**	**Market Value**	**Share Turnover**	**Return Variability**
Bid-Ask Spread$_t$	0.6668	0.5987	0.9082	-0.0681	-0.0110	0.0200
Zero Returns$_t$		0.4112	0.8260	-0.1021	-0.0137	0.0063
Price Impact$_t$			0.7857	-0.0361	-0.0057	0.0134
Liquidity Factor$_t$				-0.0803	-0.0122	0.0160
Market Value$_{t-4}$					-0.0014	-0.0000
Share Turnover$_{t-4}$						0.0004

Notes: Bid-Ask Spread is the quarterly median quoted spread (i.e., the difference between bid and ask price scaled by the mid-point calculated at the end of each trading day). Zero Returns is the fraction of trading days without daily stock returns out of all potential trading days in a given quarter. Price Impact is the quarterly median of the Amihud (2002) illiquidity measure (i.e., daily absolute stock return divided by the stock's trading volume in US$).The Liquidity Factor represents an aggregation of the above three liquidity measures. It equals the scores of a single factor extracted from a factor analysis of Bid-Ask Spread, Zero Returns, and Price Impact. Market Value is market capitalization measured at the end of the quarter. Share Turnover represents the quarterly median of the daily turnover (i.e., trading volume in US$ scaled by market value at the end of each trading day). Return Variability is the standard deviation of daily stock returns in a given quarter. We report Market Value, Share Turnover, and Return Variability for the Bid-Ask Spread sample. All correlation coefficients are significant at the 1% level except for the coefficients between Market Value and Share Turnover, Market Value and Return Variability, and Share Turnover and Return Variability. All variables are truncated at the 1st and 99th percentile. The subscripts t and t-4 indicate the calendar quarters of variable measurement.

To establish meaningful outcome differences, we first replicate the main DiD liquidity analysis by CHL (2013, pp. 162-163, Table 3). In line with the original DiD models, we estimate the following firm-quarter level regression models for all available observations from 54 IFRS and non-IFRS countries between 2001 and 2009, whereas Model (7) constitutes the main model:

$$\mathrm{LnBA_{it}} = \gamma_1 + \gamma_2 \mathrm{IFRS_{it}} + \sum \gamma_3 \mathrm{CONTROLS_{it}} + \sum \gamma_4 \mathrm{FE} + \varepsilon \tag{5}$$

$$\mathrm{LnBA_{it}} = \gamma_1 + \gamma_2 \mathrm{IFRS\_EU_{it}} + \gamma_3 \mathrm{IFRS\_nonEU_{it}} + \sum \gamma_4 \mathrm{CONTROLS_{it}} + \sum \gamma_5 \mathrm{FE} + \varepsilon \tag{6}$$

$$\mathrm{LnBA_{it}} = \gamma_1 + \gamma_2 \mathrm{IFRS\_EU\_ENF_{it}} + \gamma_3 \mathrm{IFRS\_EU\_nonENF_{it}} + \gamma_4 \mathrm{IFRS\_nonEU_{it}} + \sum \gamma_5 \mathrm{CONTROLS_{it}} + \sum \gamma_6 \mathrm{FE} + \varepsilon \tag{7}$$

Where LnBA represents log of bid-ask spreads (as quarterly median quoted bid-ask spreads), and IFRS, IFRS_EU, IFRS_nonEU, IFRS_EU_ENF, IFRS_EU_nonENF stand for different interaction terms as defined in CHL (2013) (see CHL, 2013, pp. 155 ff). CONTROLS comprises log of market value, log of return variability, and log of share turnover (all three variables lagged by a four quarter period). FIXED_EFFECTS stands for different fixed effects. In line with CHL (2013), we consider country, industry, and different quarter-year fixed effect specifications. These fixed effects also control for the underlying main effects (treatment group and post effects). Likewise, we employ robust standard errors which are two-way clustered at country and quarter level. Following CHL (2013), we expect that liquidity effects upon mandatory IFRS adoption are most pronounced for firms from EU countries with concurrent enforcement regulation in 2005 (i.e., a significant and negative coefficient estimate on IFRS_EU_ENF in Model (7)).

Table 2.5 summarizes the respective findings. In particular, we observe that our different DiD model specifications replicate and reproduce the liquidity findings of CHL (2013). Specifically, IFRS_EU and, more importantly, IFRS_EU_ENF turn out to be significant across the different model specifications (at a one percent level). It is worth to note that most of the

coefficient estimates and significance levels across the different IFRS treatment as well as control variables are fairly comparable with the findings documented in CHL (2013, p. 162, Table 3).

Table 2.5 Liquidity Effects around mandatory IFRS Adoption and Selection Effects: Replication of CHL (2013)

	Dependent Variable: Ln(Bid-Ask Spread)						
	Global IFRS	EU vs. non-EU IFRS				IFRS in EU with vs. without bundled Δ enforcement	
	Two quarter-year trends	*Three quarter-year trends*	*Other FSAP directives*	*Within country estimation*	*Treatment countries only*	*Three quarter-year trends*	*Within country estimation*
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*
(1) IFRS	-0.0052						
	(-0.05)						
(2) $IFRS_{EU}$		-0.2242***	-0.2326***	-0.2655***	-0.2530***		
		(-3.33)	(-3.43)	(-3.89)	(-5.49)		
(3) $IFRS_{EU\_ENF}$						-0.3988***	-0.3226***
						(-4.00)	(-4.14)
(4) $IFRS_{EU\_nonENF}$						-0.0272	-0.1755**
						(-0.26)	(-2.41)
(5) $IFRS_{nonEU}$		0.2464**	0.2464**	0.0902**	0.2432**	0.2464**	0.0902**
		(2.39)	(2.39)	(1.97)	(2.31)	(2.39)	(1.97)
Ln(Market $Value_{t-4}$)	-0.3525***	-0.3519***	-0.3519***	-0.3522***	-0.3473***	-0.3520***	-0.3521***
	(-25.98)	(-25.68)	(-25.70)	(-26.60)	(-22.85)	(-25.68)	(-26.59)
Ln(Return $Variability_{t-4}$)	-0.3422***	-0.3427***	-0.3428***	-0.3468***	-0.3125***	-0.3429***	-0.3468***
	(-13.35)	(-13.38)	(-13.39)	(-13.33)	(-22.94)	(-13.40)	(-13.33)
Ln(Share $Turnover_{t-4}$)	0.4051***	0.4033***	0.4032***	0.3899***	0.3447***	0.4044***	0.3903***
	(13.40)	(13.21)	(13.19)	(13.09)	(11.41)	(13.47)	(13.17)
MAD			-0.1958***				
			(-4.12)				
TPD			-0.2512**				
			(-2.03)				
Country & industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Global & IFRS countries	Global, IFRS & EU countries	Global, IFRS & EU countries	For each country separately	IFRS & EU countries	Global, IFRS & EU countries	For each country separately
Adj. R-squared	0.7536	0.7558	0.7561	0.7823	0.7244	0.7565	0.7823
Observations	727293	727293	727293	727293	245044	727293	727293
F-test for differences:		(2) vs. (5)	(2) vs. (5)	(2) vs. (5)	(2) vs. (5)	(3) vs. (4)	(3) vs. (4)
[p-value]		[0.0002]	[0.0001]	[0.0000]	[0.0001]	[0.0148]	[0.1216]
TG (self) Selection 1 (TGSE1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TG (sample) Selection 2 (TGSE2)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CG Selection (CGSE)	No	No	No	No	No	No	No

Notes: This table displays regression results from the replication of Christensen et al. (2013, Table 3, Panel A). All variables and fixed effects are specified in accordance with CHL (2013). Specifically, IFRS is an indicator variable for firm-quarters with IFRS reporting that equals '1' in each calendar quarter following the first fiscal-year end after IFRS became mandatory in the respective country. We identify firms without IFRS adoption after the mandate by Worldscope item "accounting standards followed" (wc07536). Following CHL (2013), we partition the IFRS observations using non-overlapping indicator variables: For Column 2 to Column 5, we distinguish between IFRS firms from EU countries (IFRS_EU) and IFRS firms from outside the EU (IFRS_nonEU). For Column 6 and 7, we further distinguish between firms from EU countries with concurrent IFRS adoption and substantive enforcement changes (IFRS_EU_ENF), and firms from EU countries without concurrent enforcement changes (IFRS_EU_nonENF). MAD is an indicator variable indicating adoption of the Market Abuse Directive (MAD) in a given firm-quarter. TPD is an indicator variable indicating adoption of the Transparency Directive (TPD) in a given firm-quarter. For a description of the remaining variables see Table 2.4. As in CHL (2013), the regression models include country-, Campbell (1996) industry-, and quarter-year-fixed effects (globally, for IFRS countries, EU countries, or each country separately, as indicated). We use standard errors clustered by country and calendar quarter. We further report p-values from Wald tests measuring the statistical significance of the differences across coefficients on the IFRS indicator variables. We indicate which of the selection effects discussed in this paper are present in which model specification. ***, *, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

In addition to the replication body of the table, we indicate at the bottom of the table whether we expect our selection effects to be present in the respective CHL (2013) model specifications. Given the specific CHL fixed effect structure and the unbalanced sample approach, we expect the CHL (2013) research design—across all seven model specifications—to be prone to our two-fold *treatment group* selection effect. In essence, their separate time fixed effect structure for non-IFRS and IFRS countries effectively controls for any control group selection effects due to systematic changes in the database coverage. However, CHL's research design comprising different fixed effect specifications fails to address our treatment *sample* selection effect as well as our treatment *self* selection effect.

With respect to the former, CHL's separate time fixed effect specifications for IFRS and non-IFRS countries (i.e., two-quarter-year trends, three-quarter-year trends, and within country estimation) do not (fully) control for the average liquidity variation in the underlying treatment group (or underlying country-level treatment groups). The reason is that CHL (2013) employ non-IFRS adopters in IFRS countries as control firms. This research design specification generates treatment and control group variation within IFRS countries. Consequently, any correlated omitted treatment-level variation within a specific quarter-year—for example, a treatment-country-level increase in liquidity due to systematic sample changes in the course of opt-outs of small and less liquid IFRS treatment firms in a specific post-IFRS quarter-year (i.e., treatment *sample* selection effect)—is not absorbed by CHL's fixed effect structure.

With respect to the latter, we do not expect the CHL's research design to be useful to address our treatment *self* selection effect. In fact, we expect—as already detailed in Section 2.2—this self selection effect to be a classical correlated omitted variable in the setting of mandatory IFRS adoption which standard econometric techniques are unable to address.

2.4.2 CHL (2013) setting, selection effects, and Selection Exposure Index

To examine the magnitude of our treatment group selection effects in the CHL (2013) setting, we start off by developing an empirical construct that is likely to reflect the exposure to these selection effects. In essence, we argue that systematic country-level changes in the number of firms around mandatory IFRS adoption—as documented in Figure 2.1 (Section 2.1)—should in

principle reflect country-level exposure to systematic sample changes and self selection behavior. In this subsection, we thus delineate a country-level variable (GROWTH RANK) capturing changes in the number of firms around mandatory IFRS adoption. Based on that variable, we further estimate different country-level indices—the *Selection Exposure Index* (SEI) and four *Abnormal Selection Exposure Indices* (ASEI_1 to ASEI_4)—which we deem to be suitable to extend the CHL (2013) research design. Table 2.6 summarizes this empirical approach and details each of our selection exposure indices.

Table 2.6 CHL (2013) Sample, Selection Effects, and the (Abnormal) Selection Exposure Index

Panel A. Country-quarter-level DiD and CHL country clusters

Sample Period: 2001-2009	*Dependent Variable*: zCOUNT_DATA (*#firms* per year and quarter, standardized over Q42005 value)		
	Column 1	*Column 2*	*Column 3*
(1) IFRS×POST	-0.1864**		
	(-2.51)		
(2) IFRS_EU×POST		-0.2242***	
		(-2.71)	
(3) IFRS_nonEU×POST		-0.1040	-0.1044
		(-1.38)	(-1.38)
(4) IFRS_EU-ENF×POST			-0.2921***
			(-3.23)
(5) IFRS_EU-nonENF×POST			-0.1974**
			(-2.08)
F-test for Differences		(2) vs. (3)	(3) vs. (4)
[p-value]		[0.0918]	[0.0170]
			(4) vs. (5)
			[0.3391]
Year-Quarter & Country FE	Yes	Yes	Yes
Clustered SE (country)	Yes	Yes	Yes
Adj. R-squared	0.4612	0.4715	0.4754
Observations	1380	1380	1380

Panel B. Descriptive Analysis: Change in number of firms (GROWTH RANK) and Country-level Indicator Variables

N=52	Mean	SD	No.	1	2	3	4	5
GROWTH RANK	25.83	15.59	1	1				
(p-value)				-				
GDP	844.56	1939.28	2	-0.2289	1			
(p-value)				(0.1027)	-			
RQI	0.86	0.77	3	-0.3176	0.0505	1		
(p-value)				(0.0218)	(0.7224)	-		
DEVC	0.54	0.50	4	-0.4138	0.3316	0.7699	1	
(p-value)				(0.0023)	(0.0163)	(0.000)	-	
COMMLAW	0.27	0.45	5	-0.1286	0.0376	0.2095	0.2141	1
(p-value)				(0.3637)	(0.7915)	(0.1361)	(0.1275)	-
VOIFRS	391.88	842.58	6	-0.354	0.3726	0.2574	0.2945	-0.006
(p-value)				(0.01)	(0.0065)	(0.0654)	(0.0341)	(0.9661)

Panel C. Determinant Analysis: Change in number of firms (GROWTH RANK) and Country-level Indicator Variables

Cross-sectional & country-level	Dependent Variable: GROWTH RANK (Country-level variable based on changes in number of firms between IFRS adoption—or 2005 for non-IFRS control countries—and the year 2009 which presents the sample end of CHL 2013)					
	IFRS and non-IFRS countries (CHL 2013)				*IFRS*	*IFRS EU*
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
GDP	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(-0.70)	(-0.36)	(-0.28)	(-0.17)	(-1.33)	(-1.21)

Table 2.6 (*continued*)

RQI	-5.9557*	-0.1824	-0.1911	-0.4076	1.4192	-27.6317*
	(-1.90)	(-0.04)	(-0.04)	(-0.08)	(0.15)	(-1.95)
DEVC		-11.7811	-11.5350	-9.6716	-4.6084	6.3226
		(-1.59)	(-1.59)	(-1.31)	(-0.40)	(0.48)
COMMLAW			-1.4513	-2.9379	-3.5965	-2.4139
			(-0.27)	(-0.52)	(-0.56)	(-0.28)
VOIFRS				-0.0034*	-0.0012	-0.0014
				(-1.96)	(-0.54)	(-0.39)
Pseudo r2	0.0116	0.0189	0.0191	0.0231	0.0108	0.0471
Observations	52	52	52	52	33	22
Est. residuals	ASEI_1	ASEI_2	ASEI_3	ASEI_4	-	-

Panel D. Country-level Overview of Selection Exposure Indices

		Different specifications of the Selection Exposure Index					Common country-level Determinants				
	Growth rank	SEI	ASEI_1	ASEI_2	ASEI_3	ASEI_4	GDP '05	RQI '05	DEV	COMML	VOIFRS
IFRS EU ENF COUNTRIES											
Finland	8	-8	12.7	12.3	12.7	14.4	204	1.76	1	0	205
Germany	14	-14	7.1	5.7	6.2	-2.7	2861	1.42	1	0	3439
Iceland	42	-42	-20.6	-21.7	-21.2	-18.9	17	1.66	1	0	0
Netherl.	3	-3	17.8	17.2	17.7	19.4	679	1.70	1	0	194
Norway	32	-32	-9.6	-11.7	-11.2	-9.1	309	1.47	1	0	94
UK	12	-12	8.4	7.7	6.9	7.2	2419	1.58	1	1	217
Median	*13*	*-13*	*7.7*	*6.7*	*6.6*	*2.3*	*494*	*1.62*	*1.00*	*0.00*	*199.50*
IFRS EU NON-ENF COUNTRIES											
Austria	21	-21	0.8	-0.7	-0.2	-0.6	315	1.56	1	0	816
Belgium	19	-19	4.4	1.3	1.8	1.9	387	1.29	1	0	707
Denmark	6	-6	15.0	14.3	14.7	14.9	265	1.71	1	0	657
Estonia	39	-39	-16.1	-6.8	-6.6	-6.1	14	1.41	0	0	0
France	5	-5	18.4	14.9	15.4	14.4	2204	1.10	1	0	1101
Greece	52	-52	-26.1	-31.6	-31.1	-28.6	248	0.88	1	0	0
Hungary	1	-1	23.6	31.2	31.4	30.9	113	1.12	0	0	342
Ireland	7	-7	14.7	13.3	12.3	13.2	211	1.59	1	1	0
Italy	22	-22	2.9	-2.0	-1.4	-9.8	1853	0.89	1	0	3264
Lithuania	27	-27	-2.3	5.2	5.4	6.0	26	1.11	0	0	0
Luxemb.	2	-2	18.6	18.3	18.8	21.1	37	1.79	1	0	0
Poland	34	-34	-7.6	-1.8	-1.6	-1.6	304	0.79	0	0	205
Portugal	17	-17	7.1	3.4	3.9	5.9	197	1.20	1	0	121
Slovenia	40	-40	-13.5	-7.7	-7.5	-6.8	36	0.81	0	0	0
Spain	13	-13	10.3	7.1	7.6	10.2	1157	1.23	1	0	0
Sweden	23	-23	-1.0	-2.7	-2.2	-0.5	389	1.53	1	0	181
Median	*20*	*-20*	*3.6*	*2.4*	*2.8*	*3.9*	*256*	*1.22*	*1.00*	*0.00*	*151.00*
IFRS NON-EU COUNTRIES											
Abu Dhabi	49	-49	-20.3	-16.7	-16.5	-15.7	181	0.42	0	0	0
Australia	26	-26	-4.8	-5.8	-6.8	-7.6	693	1.62	1	1	523
Hong K.	36	-36	-15.7	-15.7	-16.7	-17.4	182	1.84	1	1	460
Israel	16	-16	10.2	4.5	3.5	4.5	143	0.85	1	1	8
New Zeal.	9	-9	12.3	11.3	10.3	11.2	115	1.68	1	1	0
Pakistan	1	-1	33.8	31.5	30.3	29.6	110	-0.59	0	1	61
Philippines	29	-29	2.6	3.4	3.6	4.4	103	-0.05	0	0	36
Singapore	38	-38	-17.5	-17.7	-18.7	-20.5	127	1.80	1	1	789
South Afr.	28	-28	0.3	4.3	3.1	1.4	258	0.48	0	1	277
Switzerl.	11	-11	11.3	9.3	9.8	0.1	408	1.47	1	0	3578
Turkey	15	-15	15.0	17.3	17.5	11.6	483	0.18	0	0	2019
Median	*26*	*-26*	*2.6*	*4.3*	*3.5*	*1.4*	*181*	*0.85*	*1.00*	*1.00*	*277.00*
NON-IFRS COUNTRIES											
Argentina	48	-48	-13.0	-15.5	-15.3	-14.3	221	-0.64	0	0	0
Brazil	35	-35	-4.5	-2.8	-2.5	-1.6	892	0.05	0	0	0
Canada	53	-53	-31.6	-32.9	-33.9	-33.0	1169	1.54	1	1	22
Channel Is.	45	-45	-	-	-	-	9	-	1	1	0
Chile	51	-51	-28.3	-18.9	-18.6	-18.1	124	1.43	0	0	0
China	33	-33	-1.5	-1.1	-0.8	-1.9	2269	-0.26	0	0	649
Egypt	37	-37	-3.0	-4.5	-4.3	-3.7	90	-0.46	0	0	108
India	30	-30	2.1	2.3	1.1	0.5	834	-0.21	0	1	0
Indonesia	18	-18	16.0	14.5	14.7	15.6	286	-0.48	0	0	0
Japan	4	-4	17.5	15.3	15.9	18.7	4572	1.17	1	0	35
Malaysia	10	-10	18.1	10.5	9.5	10.6	144	0.52	1	1	0

Table 2.6 (*continued*)

Mexico	20	-20	8.9	12.2	12.4	13.3	866	0.32	0	0	0
Morocco	47	-47	-13.7	-14.5	-14.3	-13.4	62	-0.33	0	0	0
Qatar	46	-46	-16.7	-13.6	-13.4	-12.7	45	0.33	0	0	0
Russia	41	-41	-8.2	-8.7	-8.4	-8.3	764	-0.33	0	0	240
Saudi Ar.	43	-43	-12.0	-10.7	-10.4	-9.6	328	0.02	0	0	0
S. Korea	31	-31	-4.9	-10.7	-10.2	-7.6	898	0.79	1	0	0
Sri Lanka	50	-50	-17.4	-17.5	-17.3	-16.4	24	-0.21	0	0	0
Taiwan	44	-44	-	-	-	-	-	1.08	0	0	0
Thailand	24	-24	4.8	8.3	7.1	6.3	189	0.41	0	1	30
US	25	-25	-11.1	-8.0	-8.4	-6.3	13100	1.54	1	1	0
Median	*37*	*-37*	*-4.9*	*-8.0*	*-8.4*	*-6.3*	*307*	*0.19*	*0.00*	*0.00*	*0.00*

Notes: This table displays statistics on sample selection effects, and the (Abnormal) Selection Exposure Index. Panel A displays results from a regression of the number of firms on different country-clusters. zCOUNT_DATA is the number of firms per year and quarter, standardized over the respective value in the 4th quarter of 2005. IFRS, IFRS_EU, IFRS_nonEU, IFRS_EU-ENF, and IFRS_EU-nonENF are indicator variables indicating that a country falls in the respective CHL (2013) country-cluster. Panel B displays descriptive statistics on the change in number of firms and country-level variables. GROWTH_RANK represents the rank of a country in a ranking of the percentage change in listed firms for 52 sample countries. GDP is the gross domestic product in billion US dollar of a given country in 2005 (Source: Worldbank). RQI equals the regulatory quality index in 2003 of a given country as measured by Kaufmann et al. (2009). DEVC indicates a country with a developed capital market. COMMLAW indicates a country with common law as opposed to code law (both measures: Brown et al. (2014). VOLIFRS equals the number of voluntary IFRS adopters in a given country (see CHL 2013). Panel C displays the results of a determinant analysis of 52 sample countries' 'GROWTH_RANK'. The residuals from Model 1 to 4 represent different specifications of a measure for abnormal exposure to sample selection (ASE_I 1 to ASE_I 4). Panel D displays the values of different specifications of the Selection Exposure Index (SEI) for the sample countries. SEI equals GROWTH_RANK multiplied with (-1). ASEI_1 to ASEI_4 equal the inverse residuals from the regression models 1 to 4 in Panel C.

2.4.2.1 CHL (2013) setting and changes in the number of firms.

To validate this proposed empirical approach, we first revisit the sample patterns as documented in Figure 2.1 (Section 2.1) in the light of the CHL (2013) sample selection. Note that Figure 2.1 and the corresponding analyses in Section 2.3 are based on a sample selection process comprising firm-year level observations without any substantial sample reduction due to specific data requirements.[9] In contrast and as already outlined in Table 4, CHL (2013) sample selection includes, among other things, firm-quarter-year level observations, data requirements with respect to their main outcome variable, liquidity, and their control variables (e.g., missing values and truncation), as well as country-level requirements (e.g., country-quarter with at least five observations). Consequently and similar to Table 2.2 (Section 2.3), we re-estimate the following country-level DiD analysis with quarter-year and country fixed effects within the CHL (2013) liquidity setting:

$$\begin{aligned} \text{zCOUNT\_DATA}_{it} = {} & \gamma_1 + \gamma_2 \text{IFRS\_nonEU} \times \text{POST}_{it} + \gamma_3 \text{IFRS\_EU\_ENF} \times \text{POST}_{it} \\ & + \gamma_4 \text{IFRS\_EU\_nonENF} \times \text{POST}_{it} + \gamma_5 \text{COUNTRY FE} + \gamma_6 \text{QUARTER} \times \text{YEAR FE} + \varepsilon \end{aligned} \quad (8)$$

[9] Minimum data requirements in Worldscope: ISIN (wc06008), fiscal year (wc05350), country of firm domicile (wc06026), accounting standard followed (wc07536), total assets (wc02999), book value of equity (wc03501), and net income (wc01751).

In particular, we use zCOUNT_DATA as our dependent variable, which measures the relative change in number of firms per quarter-year and country (i.e., zCOUNT_DATA is scaled by the respective absolute values on total number of listed firms per country in Q4 2005). In addition, IFRS_nonEU, IFRS_EU_ENF, IFRS_EU_nonENF reflect the different country-clusters as defined in CHL (2013) (see Appendix 2.2 for further information). COUNTRY FE and QUARTER×YEAR FE represent country and quarter-year fixed effects. This fixed-effect structure controls for the underlying main effects of the DiD regression as well (i.e., POST and IFRS Treatment group). In all reported regression models, the standard errors are heteroskedasticity robust (White 1980) and one-way clustered at the country-level (Gow et al. 2010; Petersen 2009).

Panel A of Table 2.6 documents the main findings. Corroborating Figure 2.1 and Table 2.2, we observe that the IFRS, IFRS_EU, and IFRS_EU_ENF treatment groups in the different model specifications exhibit a significant and relative decline in the sample size in the period after 2005. More importantly, we further observe that these systematic sample changes are most pronounced for the IFRS_EU_ENF countries (Column 3). Overall, these findings highlight (again) a systematic decline in the number of listed firms that adopt IFRS (especially in EU countries with bundled enforcement regulation) in the CHL (2013) liquidity setting.[10]

2.4.2.2 CHL (2013) setting, growth rank and selection exposure indices.

As we observe similar systematic sample changes around mandatory IFRS adoption in the CHL (2013) setting, compared to our previous firm-year analyses in Sections 2.1 and 2.3 (e.g., Figure 2.1), we proceed with estimating a country-level variable capturing the severeness of these sample changes. In doing so, we rank all 54 CHL (2013) countries along their sample changes in the CHL (2013) post-IFRS period (i.e., the period between IFRS adoption, or the end of 2005 in the case of a control country, and the end of the sample period in 2009). We label the resulting variable as GROWTH RANK (with smaller ranks for higher sample declines). Panel D of Table 2.6 outlines the distribution of this rank variable across the different countries. In addition, Panels B and C of Table 2.6 document univariate and multivariate correlations between this country-level rank variable and other country-level variables which are commonly used in the

[10] Appendix 2.4 outlines the distribution of number of listed firms across CHL country clusters within the CHL setting.

(accounting) literature to assess the market structure / quality of the respective countries. For example, Panel B of Table 2.6 suggests that our rank variable GROWTH RANK negatively correlates with Kaufman's regulatory quality index (RQI), with the country's state of development (DEVC), as well as with the presumably firm-level incentives to voluntarily adopt IFRS (VOIFRS). At face value, this implies that IFRS countries with high RQI, high DEVC, and high VOIFRS are thus more likely to observe severe sample declines (and with that a higher selection exposure) in the post-IFRS adoption period.

To empirically exploit our GROWTH RANK variable in the CHL (2013) setting, we delineate different *selection exposure indices* from this rank variable. First, we take the inverse of GROWTH RANK as our primary measure of country-level selection exposure, SELECTION EXPOSURE INDEX (SEI). Thus, higher values of SEI indicate higher exposure to our expected selection effects. In addition, we estimate four different *abnormal* selection indices to control for sample changes which are rather driven by other market characteristics such as high regulatory quality. To that end, we explicitly attempt to disentangle our selection exposure index from pre-existing differences in the legal system, the regulatory quality of the respective countries, and other related market characteristics. This is important as empirical studies, which aim to test benefits of IFRS adoption contingent on country-level partitioning variables, have to ensure that the documented variation in the treatment effect is incremental to previously documented effects (Barth and Israeli, 2013). Thus, we take the residuals of the regression models in Columns 1 to 4 in Panel C of Table 2.6—with GROWTH RANK as the dependent variable and different country-level variables such as Kaufman's regulatory quality index (RQI) as independent variables—as our *abnormal* proxies for the selection exposure (ASEI_1 to ASEI_4). Again, for the sake of readability, we take the inverse of the residuals to derive exposure indices that indicate higher selection exposure risk with higher values.

Panel D of Table 2.6 summarizes all country-level variables, including our different selection indices, across all 54 CHL (2013) countries. In line with the empirical motivation of our estimation approach (Figure 2.1) and the line of argument put forward in Section 2.2, we observe that each of our *selection exposure indices* is increasing with the regulatory intensity within the mandatory IFRS adoption setting. In other words, IFRS_EU_ENF countries appear to have on average the highest selection exposure indices, and thus the highest exposure risk to our selection

effects, followed by countries from the IFRS_EU_nonENF, and IFRS_nonEU clusters. It is also interesting to note—on a rather anecdotal level—that the only non-IFRS control country with a constantly *high selection exposure* index is Japan (e.g., $SEI_{Japan} = -4$ vs. $SEI_{Non\text{-}IFRS\_COUNTRIES} = -37$) which was affected by comprehensive enforcement changes in 2005 and for which CHL (2013, p. 170, Table 6) document significant liquidity effects in the post 2005 period as well.

2.4.3 CHL (2013) extension and (Abnormal) Selection Exposure Index

To finally assess the magnitude of our two-fold treatment selection effect within the CHL (2013) liquidity setting, we extend the original models proposed by CHL (2013) and introduce additional interaction terms based on our different *selection exposure indices* (starting with our baseline SELECTION EXPOSURE INDEX – SEI). Specifically, we estimate the following firm-quarter level regression models for all available observations from 54 IFRS and non-IFRS countries between 2001 and 2009:

$$\mathrm{LnBA_{it}} = \gamma_1 + \gamma_2 IFRS\_SEI\_High_{it} + \gamma_3 IFRS\_SEI\_Low_{it} + \sum \gamma_4 \mathrm{CONTROLS_{it}} + \sum \gamma_5 \mathrm{FE} + \varepsilon \quad (9)$$

$$\mathrm{LnBA_{it}} = \gamma_1 + \gamma_2 IFRS\_EU\_SEI\_High_{it} + \gamma_3 IFRS\_EU\_SEI\_Low_{it} + \gamma_4 \mathrm{IFRS\_nonEU_{it}} + \sum \gamma_5 \mathrm{CONTROLS_{it}} + \sum \gamma_6 \mathrm{FE} + \varepsilon \quad (10)$$

$$\mathrm{LnBA_{it}} = \gamma_1 + \gamma_2 IFRS\_EU\_Enf\_SEI\_High_{it} + \gamma_3 IFRS\_EU\_Enf\_SEI\_Low_{it} + \gamma_4 \mathrm{IFRS\_EU\_nonEnf_{it}} + \gamma_5 \mathrm{IFRS\_nonEU_{it}} + \sum \gamma_6 \mathrm{CONTROLS_{it}} + \sum \gamma_7 \mathrm{FE} + \varepsilon \quad (11)$$

Where LnBA, IFRS_nonEU, IFRS_EU_nonENF, CONTROLS, and FIXED EFFECTS are defined as for the original CHL models (Models 5 to 7). IFRS_SEI_High and IFRS_SEI_Low are two non-overlapping variables indicating a high versus low SELECTION EXPOSURE INDEX within the IFRS treatment group (based on median sample splits). In the same logic, IFRS_EU_SEI_High and IFRS_EU_SEI_Low (IFRS_EU_Enf_SEI_High and IFRS_EU_Enf_-SEI_Low) represent non-overlapping variables indicating a high versus low SELECTION EXPOSURE INDEX in the IFRS_EU treatment group (IFRS_EU_Enf treatment group). In line with the treatment selection effects as discussed in Section 2.2, we expect to observe higher

liquidity benefits for treatment groups with a high SELECTION EXPOSURE INDEX, and thus with a high exposure risk to our selection effects. Table 2.7 summarizes the respective findings. Panel A of Table 2.7 provides the extended CHL (2013) DiD analysis based on our baseline selection proxy, SEI, whereas Panels B to E re-estimate these extended CHL analyses for each of our abnormal selection exposure index (i.e., ASEI_1 to ASEI_4).

Across all panels and specifications, we observe that our additional interaction terms based on the different selection exposure indices explain the CHL (2013) liquidity results above and beyond the IFRS_EU and IFRS_EU_ENF variation. Specifically, we observe that treatment groups with a high (ABNORMAL) SELECTION EXPOSURE INDEX (e.g., "_SEI_{High}" or "$ASEI_{High}$") experience significantly higher liquidity benefits in the course of mandatory IFRS adoption than their counterparts (e.g., treatment groups with "_SEI_{Low}" or "$ASEI_{Low}$"). These results are highly consistent with t-values on the different "_(A)SEI_{High}" coefficient estimates ranging between –1.74 (for the mere IFRS disaggregation) and –10.51 (for the IFRS_EU_ENF disaggregation). Overall, these findings suggest that our two-fold treatment selection effect is indeed sufficiently large to moderate the liquidity findings as provided by CHL (2013).

Table 2.7 Liquidity Effects around mandatory IFRS Adoption and the (Abnormal) Selection Exposure Index: Extension of CHL (2013)

Panel A. CHL (2013) Liquidity Findings and Selection Exposure Index (**SEI**)

	Dependent Variable: Ln(Bid-Ask Spread)						
	High vs. low Selection Exposure Index (SEI)						
	Countries with high selection exposure (high sample decline) vs. countries with low selection exposure (low sample decline)						
	Global IFRS	EU vs. non-EU IFRS				IFRS in EU with vs. without bundled Δ enforcement	
	Two quarter-year trends	*Three quarter-year trends*	*Other FSAP directives*	*Within country estimation*	*Treatment countries only*	*Three quarter-year trends*	*Within country estimation*
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*
(1) $IFRS\_SEI_{High}$	-0.1918*						
	(-1.74)						
(2) $IFRS\_SEI_{Low}$	0.2419***						
	(2.81)						
(3) $IFRS_{EU\_}$ SEI_{High}		-0.3061***	-0.3041***	-0.2877***	-0.3355***		
		(-4.01)	(-4.00)	(-4.12)	(-5.88)		
(4) $IFRS_{EU\_}$ SEI_{Low}		0.0235	-0.0058	-0.1354*	-0.0058		
		(0.22)	(-0.07)	(-1.68)	(-0.05)		
(5) $IFRS_{EU\_ENF\_}$ SEI_{High}						-0.5234***	-0.3874***
						(-7.85)	(-10.51)
(6) $IFRS_{EU\_ENF\_}$ SEI_{Low}						-0.1281*	-0.1655***
						(-1.73)	(-3.08)
(7) $IFRS_{nonEU}$		0.2464**	0.2464**	0.0902**	0.2431**	0.2464**	0.0902**
		(2.39)	(2.39)	(1.97)	(2.31)	(2.39)	(1.97)
(8) $IFRS_{EU\_nonENF}$						-0.0275	-0.1754**
						(-0.26)	(-2.41)
Ln(Market $Value_{t-4}$)	-0.3521***	-0.3519***	-0.3519***	-0.3521***	-0.3471***	-0.3520***	-0.3522***
	(-25.81)	(-25.66)	(-25.67)	(-26.59)	(-22.83)	(-25.66)	(-26.58)
Ln(Return $Variability_{t-4}$)	-0.3423***	-0.3427***	-0.3428***	-0.3468***	-0.3122***	-0.3426***	-0.3467***
	(-13.36)	(-13.38)	(-13.39)	(-13.33)	(-23.27)	(-13.37)	(-13.32)
Ln(Share $Turnover_{t-4}$)	0.4046***	0.4039***	0.4038***	0.3900***	0.3463***	0.4053***	0.3907***
	(13.37)	(13.35)	(13.33)	(13.12)	(11.77)	(13.65)	(13.23)
MAD			-0.2025***				
			(-3.60)				
TPD			-0.1770**				
			(-2.30)				
Country & industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Global & IFRS countries	Global, IFRS & EU countries	Global, IFRS & EU countries	For each country separately	IFRS & EU countries	Global, IFRS & EU countries	For each country separately
Adj. R-squared	0.7551	0.7563	0.7564	0.7823	0.7261	0.7569	0.7823
Observations	727293	727293	727293	727293	245044	727293	727293
F-test for differences:	(1) vs. (2)	(3) vs. (4)	(3) vs. (4)	(3) vs. (4)	(3) vs. (4)	(5) vs. (6)	(5) vs. (6)
[p-value]	[0.0029]	[0.0338]	[0.0233]	[0.1221]	[0.0323]	[0.0000]	[0.0000]
TG (self) Selection 1 (TGSE1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TG (sample) Selection 2 (TGSE2)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CG Selection (CGSE)	No	No	No	No	No	No	No

Table 2.7 (continued)

Panel B. CHL (2013) Liquidity Findings and High vs. Low *Abnormal* Selection Exposure Index (**ASEI_1**)

	Dependent Variable: Ln(Bid-Ask Spread)						
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*
(1) IFRS_ASEI_1$_{High}$	-0.1918*						
	(-1.74)						
(2) IFRS_ ASEI_1$_{Low}$	0.2419***						
	(2.81)						
(3) IFRS$_{EU}$_ ASEI_1$_{High}$		-0.3061***	-0.3041***	-0.2877***	-0.3355***		
		(-4.01)	(-4.00)	(-4.12)	(-5.88)		
(4) IFRS$_{EU}$_ ASEI_1$_{Low}$		0.0235	-0.0058	-0.1354*	-0.0058		
		(0.22)	(-0.07)	(-1.68)	(-0.05)		
(5) IFRS$_{EU_ENF}$_ ASEI_1$_{High}$						-0.5234***	-0.3874***
						(-7.85)	(-10.51)
(6) IFRS$_{EU_ENF}$_ ASEI_1$_{Low}$						-0.1281*	-0.1655***
						(-1.73)	(-3.08)
(7) IFRS$_{nonEU}$		0.2464**	0.2464**	0.0902**	0.2431**	0.2464**	0.0902**
		(2.39)	(2.39)	(1.97)	(2.31)	(2.39)	(1.97)
(8) IFRS$_{EU_nonENF}$						-0.0275	-0.1754**
						(-0.26)	(-2.41)
Control variables, country & industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Global & IFRS	Global, IFRS & EU	Global, IFRS & EU	Country-level	IFRS & EU	Global, IFRS & EU	Country-level
Adj. R-squared	0.7551	0.7563	0.7564	0.7823	0.7261	0.7569	0.7823
Observations	727293	727293	727293	727293	245044	727293	727293

Panel C. CHL (2013) Liquidity Findings and High vs. Low *Abnormal* Selection Exposure Index (**ASEI_2**)

	Dependent Variable: Ln(Bid-Ask Spread)						
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*
(1) IFRS_ASEI_2$_{High}$	-0.2122*						
	(-1.89)						
(2) IFRS_ ASEI_2$_{Low}$	0.2134**						
	(2.26)						
(3) IFRS$_{EU}$_ ASEI_2$_{High}$		-0.3101***	-0.3082***	-0.2891***	-0.3393***		
		(-4.04)	(-4.04)	(-4.18)	(-5.85)		
(4) IFRS$_{EU}$_ ASEI_2$_{Low}$		0.0277	0.0001	-0.1282	-0.0020		
		(0.26)	(0.00)	(-1.60)	(-0.02)		
(5) IFRS$_{EU_ENF}$_ ASEI_2$_{High}$						-0.5234***	-0.3874***
						(-7.85)	(-10.51)
(6) IFRS$_{EU_ENF}$_ ASEI_2$_{Low}$						-0.1281*	-0.1655***
						(-1.73)	(-3.08)
(7) IFRS$_{nonEU}$		0.2464**	0.2464**	0.0902**	0.2431**	0.2464**	0.0902**
		(2.39)	(2.39)	(1.97)	(2.31)	(2.39)	(1.97)
(8) IFRS$_{EU_nonENF}$						-0.0275	-0.1754**
						(-0.26)	(-2.41)
Control variables, country & industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Global & IFRS	Global, IFRS & EU	Global, IFRS & EU	Country-level	IFRS & EU	Global, IFRS & EU	Country-level
Adj. R-squared	0.7550	0.7563	0.7564	0.7823	0.7262	0.7569	0.7823
Observations	727293	727293	727293	727293	245044	727293	727293

Table 2.7 (continued)

Panel D. CHL (2013) Liquidity Findings and High vs. Low *Abnormal* Selection Exposure Index (**ASEI_3**)

	Dependent Variable*:* Ln(Bid-Ask Spread)						
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*
(1) $IFRS\_ASEI\_3_{High}$	-0.2099*						
	(-1.80)						
(2) $IFRS\_ ASEI\_3_{Low}$	0.2120**						
	(2.52)						
(3) $IFRS_{EU\_} ASEI\_3_{High}$		-0.3101***	-0.3082***	-0.2891***	-0.3393***		
		(-4.04)	(-4.04)	(-4.18)	(-5.85)		
(4) $IFRS_{EU\_} ASEI\_3_{Low}$		0.0277	0.0001	-0.1282	-0.0020		
		(0.26)	(0.00)	(-1.60)	(-0.02)		
(5) $IFRS_{EU\_ENF\_} ASEI\_3_{High}$						-0.5234***	-0.3874***
						(-7.85)	(-10.51)
(6) $IFRS_{EU\_ENF\_} ASEI\_3_{Low}$						-0.1281*	-0.1655***
						(-1.73)	(-3.08)
(7) $IFRS_{nonEU}$		0.2464**	0.2464**	0.0902**	0.2431**	0.2464**	0.0902**
		(2.39)	(2.39)	(1.97)	(2.31)	(2.39)	(1.97)
(8) $IFRS_{EU\_nonENF}$						-0.0275	-0.1754**
						(-0.26)	(-2.41)
Control variables, country & industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Global & IFRS	Global, IFRS & EU	Global, IFRS & EU	Country-level	IFRS & EU	Global, IFRS & EU	Country-level
Adj. R-squared	0.7551	0.7563	0.7564	0.7823	0.7262	0.7569	0.7823
Observations	727293	727293	727293	727293	245044	727293	727293

Panel E. CHL (2013) Liquidity Findings and High vs. Low *Abnormal* Selection Exposure Index (**ASEI_4**)

	Dependent Variable*:* Ln(Bid-Ask Spread)						
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*
(1) $IFRS\_ASEI\_4_{High}$	-0.2208*						
	(-1.86)						
(2) $IFRS\_ ASEI\_4_{Low}$	0.1898**						
	(2.01)						
(3) $IFRS_{EU\_} ASEI\_4_{High}$		-0.3471***	-0.3438***	-0.3268***	-0.3784***		
		(-4.38)	(-4.42)	(-5.54)	(-6.51)		
(4) $IFRS_{EU\_} ASEI\_4_{Low}$		-0.0261	-0.0494	-0.1288***	-0.0538		
		(-0.27)	(-0.63)	(-2.88)	(-0.52)		
(5) $IFRS_{EU\_ENF\_} ASEI\_4_{High}$						-0.5234***	-0.3874***
						(-7.85)	(-10.51)
(6) $IFRS_{EU\_ENF\_} ASEI\_4_{Low}$						-0.1281*	-0.1655***
						(-1.73)	(-3.08)
(7) $IFRS_{nonEU}$		0.2464**	0.2464**	0.0902**	0.2431**	0.2464**	0.0902**
		(2.39)	(2.39)	(1.97)	(2.31)	(2.39)	(1.97)
(8) $IFRS_{EU\_nonENF}$						-0.0275	-0.1754**
						(-0.26)	(-2.41)
Control variables, country & industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Global & IFRS	Global, IFRS & EU	Global, IFRS & EU	Country-level	IFRS & EU	Global, IFRS & EU	Country-level

Table 2.7 (continued)

Adj. R-squared	0.7550	0.7563	0.7565	0.7823	0.7264	0.7569	0.7823
Observations	727293	727293	727293	727293	245044	727293	727293

Notes: This table displays regression results for five extensions of CHL (2013). Each extension employs one of five different specifications of the Selection Exposure Index: SEI (Panel A), ASEI_1 (Panel B), ASEI_2 (Panel C), ASEI_3 (Panel D), and ASEI_4 (Panel E). For further information on the (remaining) variables and the regression model specifications, see Table 2.5.

2.4.4 CHL (2013) selection effects and balanced sample approach

As outlined in Section 2.2, a balanced sample approach across the treatment and the control group mitigates the control group selection as well as part of our treatment selection effect (i.e., the treatment *sample* selection effect). Yet, most of prior research on mandatory IFRS adoption (including CHL, 2013) does not consider a balanced panel approach. Based on the 25 recent IFRS studies, which are discussed by Brueggemann et al. (2013), only six studies seem to consider a balance sample approach.[11] However, as likewise outlined in Section 2.2, a balanced sample approach does not mitigate our core treatment selection effect (i.e., treatment *self* selection effect). To disentangle our two treatment group selection effects as documented in Section 2.3, we thus repeat our extended CHL (2013) DiD analyses for a balanced sample.

Results are reported in Table 2.8. First, Panel A of Table 2.8 shows that the initial findings of CHL (2013) prevail when using a balanced sample, yet on a comparably lower lever with respect to the economic as well as statistical significance of the respective main coefficient estimates (e.g., IFRS_EU_ENF). Among other things, this might indicate the magnitude of our treatment *sample* selection effect. Second, as expected, findings in Panel B of Table 2.8 show that our treatment *self* selection effect prevails, as the positive liquidity findings are still clustered in countries with a high SELECTION EXPOSURE INDEX (SEI). We take this as evidence that the degree of *ex-post* self selection into the treatment group, inherent in the different treatment group specifications, still explains liquidity effects above and beyond EU and enforcement variation.

[11] Ahmed et al. (2013) explicitly introduce a balanced sample approach. Five additional studies seem to rely on a balanced sample approach as well without outlining it explicitly (i.e., Callao and Jarne 2010; Aharony et al. 2010; Byard et al. 2011; Beuselinck et al, 2010; DeFond et al. 2011).

Table 2.8 Balanced Sample Approach and Treatment Self Selection Effect

Panel A. Replication of CHL (2013) - *Balanced Sample Approach*

	Dependent Variable: Ln(Bid-Ask Spread)						
	Global IFRS	EU vs. non-EU IFRS				IFRS in EU with vs. without bundled Δ enforcement	
	Two quarter-year trends	*Three quarter-year trends*	*Other FSAP directives*	*Within country estimation*	*Treatment countries only*	*Three quarter-year trends*	*Within country estimation*
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*
(1) IFRS	-0.1107						
	(-0.90)						
(2) $IFRS_{EU}$		-0.1326**	-0.1470**	-0.2893***	-0.1454***		
		(-2.33)	(-2.47)	(-3.53)	(-2.73)		
(3) $IFRS_{EU\_ENF}$						-0.3104***	-0.3325***
						(-2.81)	(-4.40)
(4) $IFRS_{EU\_nonENF}$						0.1491	-0.1750***
						(0.80)	(-2.65)
(5) $IFRS_{nonEU}$		0.0900	0.0900	0.0250	0.0905	0.0901	0.0250
		(0.81)	(0.81)	(0.37)	(0.81)	(0.81)	(0.37)
Ln(Market $Value_{t-4}$)	-0.3287***	-0.3291***	-0.3291***	-0.3307***	-0.3376***	-0.3292***	-0.3307***
	(-45.05)	(-44.72)	(-44.79)	(-50.30)	(-30.33)	(-44.65)	(-50.30)
Ln(Return $Variability_{t-4}$)	-0.3238***	-0.3246***	-0.3248***	-0.3244***	-0.3094***	-0.3252***	-0.3244***
	(-12.28)	(-12.36)	(-12.38)	(-11.53)	(-14.20)	(-12.41)	(-11.53)
Ln(Share $Turnover_{t-4}$)	0.4109***	0.4092***	0.4096***	0.4084***	0.3862***	0.4110***	0.4087***
	(22.89)	(22.44)	(22.65)	(23.72)	(13.66)	(22.79)	(23.72)
MAD			-0.2145***				
			(-2.66)				
TPD			-0.3070*				
			(-1.65)				
Country & industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Global & IFRS countries	Global, IFRS & EU countries	Global, IFRS & EU countries	For each country separately	IFRS & EU countries	Global, IFRS & EU countries	For each country separately
Adj. R-squared	0.7555	0.7570	0.7575	0.7909	0.7452	0.7584	0.7910
Observations	243432	243432	243432	243432	66384	243432	243432
F-test for differences:		(2) vs. (5)	(2) vs. (5)	(2) vs. (5)	(2) vs. (5)	(3) vs. (4)	(3) vs. (4)
[p-value]		[0.0692]	[0.0543]	[0.0021]	[0.0578]	[0.0526]	[0.0547]
TG (self) Selection 1 (TGSE1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TG (sample) Selection 2 (TGSE2)	No	No	No	No	No	No	No
CG Selection (CGSE)	No	No	No	No	No	No	No

Table 2.8 (continued)

Panel B. Treatment Self Selection Effect - *Balanced Sample Approach*

	Dependent Variable: Ln(Bid-Ask Spread)						
	High vs. low Selection Exposure Index (SEI)						
	Global IFRS	EU vs. non-EU IFRS				IFRS in EU with vs. without bundled Δ enforcement	
	Two quarter-year trends	*Three quarter-year trends*	*Other FSAP directives*	*Within country estimation*	*Treatment countries only*	*Three quarter-year trends*	*Within country estimation*
	Column 1	*Column 2*	*Column 3*	*Column 4*	*Column 5*	*Column 6*	*Column 7*
(1) $IFRS\_SEI_{High}$	-0.2343						
	(-1.63)						
(2) $IFRS\_SEI_{Low}$	0.2224**						
	(2.54)						
(3) $IFRS_{EU\_}SEI_{High}$		-0.1924***	-0.1933***	-0.2902***	-0.2057***		
		(-3.07)	(-3.09)	(-3.48)	(-3.30)		
(4) $IFRS_{EU\_}SEI_{Low}$		0.2692	0.2073	-0.2729*	0.2537		
		(1.39)	(1.29)	(-1.77)	(1.31)		
(5) $IFRS_{EU\_ENF\_}SEI_{High}$						-0.4525***	-0.3710***
						(-4.50)	(-9.62)
(6) $IFRS_{EU\_ENF\_}SEI_{Low}$						0.1161	-0.0433
						(0.72)	(-0.26)
(7) $IFRS_{nonEU}$		0.0900	0.0900	0.0250	0.0904	0.0903	0.0250
		(0.81)	(0.81)	(0.37)	(0.81)	(0.81)	(0.37)
(8) $IFRS_{EU\_nonENF}$						0.1781	-0.1751***
						(0.91)	(-2.65)
$Ln(Market\ Value_{t-4})$	-0.3288***	-0.3291***	-0.3290***	-0.3307***	-0.3374***	-0.3295***	-0.3307***
	(-44.76)	(-44.68)	(-44.72)	(-50.30)	(-30.02)	(-44.14)	(-50.28)
$Ln(Return\ Variability_{t-4})$	-0.3243***	-0.3249***	-0.3249***	-0.3244***	-0.3101***	-0.3241***	-0.3244***
	(-12.34)	(-12.39)	(-12.40)	(-11.53)	(-14.33)	(-12.33)	(-11.53)
$Ln(Share\ Turnover_{t-4})$	0.4111***	0.4097***	0.4099***	0.4084***	0.3879***	0.4119***	0.4087***
	(22.85)	(22.66)	(22.74)	(23.72)	(13.77)	(22.98)	(23.73)
MAD			-0.2008**				
			(-2.31)				
TPD			-0.1752				
			(-1.54)				
Country & industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-year FE	Global & IFRS countries	Global, IFRS & EU countries	Global, IFRS & EU countries	For each country separately	IFRS & EU countries	Global, IFRS & EU countries	For each country separately
Adj. R-squared	0.7568	0.7578	0.7580	0.7909	0.7486	0.7593	0.7910
Observations	243432	243432	243432	243432	66384	243432	243432
F-test for differences:	(1) vs. (2)	(3) vs. (4)	(3) vs. (4)	(3) vs. (4)	(3) vs. (4)	(5) vs. (6)	(5) vs. (6)
[p-value]	[0.0114]	[0.0361]	[0.0263]	[0.9106]	[0.0401]	[0.0036]	[0.0357]
TG (self) Selection 1 (TGSE1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TG (sample) Selection 2 (TGSE2)	No	No	No	No	No	No	No
CG Selection (CGSE)	No	No	No	No	No	No	No

Notes: This table displays the CHL (2013) replication based on a balanced panel structure. Specifically, the analysis only includes firms for which the data required for the regression was available for all 36 quarters of the sample period (2001-2009). For further information on the model and variable definitions, see Table 2.5.

2.5 Conclusion

This paper documents a systematic decline in the number of listed firms that adopt IFRS according to Thomson Reuters' Worldscope, and links this trend to three underlying selection issues, a non-IFRS firms (*control* group) and a two-fold IFRS firms (*treatment* group) selection effect. We provide empirical evidence on the presence and the magnitude of these selection effects. Most importantly, we document that our treatment group selection effect is able to explain the liquidity findings documented in one prominent IFRS paper, CHL (2013). In essence, these findings introduce a so far unobserved variable—ex-post self selection into mandatory IFRS by not choosing to dispense with the mandate—that might drive liquidity findings documented in prior research.

Our paper contributes to the extant literature in several ways. First, we contribute to the literature on mandatory IFRS adoption in a general way by highlighting an important research design issue that might explain the respective documented IFRS effects as a correlated omitted variable. Second, our paper specifically extends recent research on one important economic outcome, the liquidity effects that are observed concurrently with bundled IFRS and enforcement regulation (i.e., CHL 2013). In essence, our paper provides evidence on a direct channel in which concurrent IFRS accounting and enforcement regulation translate into higher market liquidity. With that, our findings might be relevant for dissolving the perceived inconsistency in the literature on mandatory IFRS adoption: While this literature shows that IFRS and enforcement regulation may lead to capital market benefits (e.g., market liquidity), it fails so far to provide conclusive evidence on potential channels for this relationship. In contrast to the conceptual level of most prior studies on the economic consequences of mandatory IFRS adoption, including CHL (2013), our liquidity findings do not inevitably require improvements in accounting or reporting quality as an implicit assumption or precondition for their internal validity. Third, our paper sheds light on firm-level characteristics and, more importantly, country-level institutional factors that shape cost-benefit tradeoffs pertaining to delisting and downlisting decisions. To that end, we augment another large stream in the accounting and finance literature by providing institutional country-level insight into the strictness of the IFRS mandate.

Finally, we note that our paper not only potentially contributes to some of the main research streams in the fields of accounting and finance during the recent years, but also potentially provides policy-relevant information. Specifically, from a European perspective, the mandatory adoption of IFRS for the consolidated accounts of firms listed on EU regulated stock markets is arguably the largest accounting related regulatory event in European history. Thus, research on potential unintended economic consequences and potential market externalities of this regulatory event, which we attempt to document in this paper, is of potential interest and value to national as well as supranational regulators.

2.6 Appendix

Appendix 2.1 Worldscope Coverage over Time (2003-2013)

Overview (WDG, 2013, p. 28)	• **Origins and Development**: "The Worldscope Database originated in the international investment management activities of Wright Investors' Service, a U.S. based global money management firm. In 2000, Primark Corporation was acquired by Thomson Corporation. In 2008, Thomson Corporation acquired Reuters. Today, the content operations group, which supports the Worldscope database, employs over 500 people in seven global data centers." • **Database applications include**: "Construction and maintenance of global investment portfolios, Global Industry and Sector analysis, Comparison of capital structures and financing strategies of corporations worldwide, Evaluation and monitoring of the efficiency of capital markets, Style Analysis, Generalized screening, reporting and presentation of corporate data" • **Existing Worldscope clients include**: "Money Management firms: Portfolio managers, research analysts, Investment banks: Corporate finance, mergers & acquisitions departments, Corporations: Strategic planners, librarians, treasurers, controllers, auditors, Academic and Public Sector: Research libraries, Consulting firms: Management consultancies, accounting firms, pension consultants"
Coverage (WDG, 2007, p. 19)	• Worldscope database targets coverage of publicly quoted companies and provides either full, partial, or no coverage of the respective markets • The base year for the Worldscope Database is 1980, although statistically significant company and data item representation is best represented from January 1985 forward. • **Coverage criteria for partial coverage** (as of 2007/2010): – Firms need to meet one or more of the following criteria to be included in our coverage: (1) broker estimates equal to or greater than 5, (2) market capitalization equal or greater than 100 million US dollars, (3) firm belongs to the FTSE ALL Worlds, Dow Jones Global, MSCI World, MSCI EMF, S&P Global, S&P Citigroup, (4) firms are included in EASDAQ or EURO.NM, (5) non-US firm which has a listing on the NYSE, ASE or NASDAQ – Some exceptions to the above criteria are highlighted in the respective Worldscope Definition Guides (see, for example, WDG 2010, p. 36)
2003 (WDG, 2013, pp. 18-19)	• **Full coverage for 30 countries (mainly developed countries)** – Full coverage of almost all Western European countries since 1999 (i.e., 15 countries in the EU together with Norway and Switzerland) – Full coverage for the US (all US firms filing with the SEC) • **Partial coverage for 23 countries**
2007 (WDG, 2013, pp. 18-19)	• **Full coverage for 33 countries (mainly developed countries)** – *Developed markets*: Australia, Austria, Belgium, Canada (excls Canadian Venture Market), Denmark, Finland, France, Germany, Greece, Hong Kong, Ireland, Italy, Japan, Luxembourg, Netherlands, Norway, Portugal, Singapore, Spain, Sweden, Switzerland, UK, US – *Advanced emerging / emerging*: Brazil, Mexico, Korea (excls KOSDAQ) , South Africa, Indonesia, Malaysia, Philippines, Thailand, China, Taiwan • **Partial coverage for 28 countries** – *Developed, emerging, advanced emerging markets*: Argentina, Bermuda, Canadian Venture Market, Cayman Islands, Chile, Colombia, Czech Republic, Egypt, Hungary, Iceland, India, Israel, Jordan, Korean KOSDAQ market, Morocco, New Zealand, Pakistan, Peru, Poland, Russia, Saudi Arabia, Slovakia, Slovenia, Sri Lanka, Turkey, Venezuela, Virgin Islands, Zimbabwe
2010 (WDG, 2013, pp. 35-36)	• **Full coverage for 33 countries (mainly developed countries)** – *Developed markets*: Australia, Austria, Belgium, Canada (excls Canadian Venture Market), Denmark, Finland, France, Germany, Greece, Hong Kong, Ireland, Italy, Japan, Luxembourg, Netherlands, Norway, Portugal, Singapore, Spain, Sweden, Switzerland, UK, US – *Advanced emerging / emerging*: Brazil, Mexico, Korea (excls KOSDAQ) , South Africa, Indonesia, Malaysia, Philippines, Thailand, China, Taiwan • **Partial coverage for 47 countries** – *Developed, emerging, advanced emerging markets*: Argentina, Bahrain, Bermuda, Bulgaria, Canadian Venture Market, Cayman Islands, Chile, Colombia, Croatia, Czech Republic, Egypt, Estonia, Hungary, Iceland, India, Israel, Jordan, Kazakhstan, Kenya, Korean KOSDAQ market, Kuwait, Lebanon, Lithuania, Mauritius, Morocco, New Zealand, Nigeria, Oman, Pakistan, Peru, Poland, Qatar, Romania, Russia, Saudi Arabia, Serbia, Slovakia, Slovenia, Sri Lanka, Tunisia, Turkey, Ukraine, United Arab Emirates (Dubai and Abu Dhabi markets), Venezuela, Vietnam, Virgin Islands, Zimbabwe

Appendix 2.1 (continued)

2013 (WDG, 2013, pp. 38-39)	• **Full coverage for 55 countries (both developed and developing countries)** – *Developed markets*: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Hong Kong, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Singapore, Spain, Sweden, Switzerland, UK, US – *Advanced emerging / emerging*: Argentina, Bahrain, Brazil, Bulgaria, Chile, China, Colombia, Cyprus, Czech Republic, Estonia, Iceland, Indonesia, Jordan, Kuwait, Latvia, Lebanon, Lithuania, Malaysia, Mexico, Oman, Philippines, Poland, Qatar, Russia, South Africa, South Korea, Taiwan, Thailand, Turkey, UAE, Vietnam • **Partial coverage for 56 countries** – *Developed, emerging, advanced emerging markets*: India, Bolivia, Bosnia & Herzegovina, Botswana, Cote d'Ivoire, Croatia, Ecuador, Egypt, Fiji, Ghana, Guyana, Israel, Kazakhstan, Kenya, Macedonia, Malawi, Mauritius, Montenegro, Morocco, Namibia, Nigeria, Pakistan, Palestine, Peru, Romania, Serbia, Swaziland, Tanzania, Tunisia, Uganda, Ukraine, Venezuela, Zambia, Zimbabwe, Costa Rica, El Salvador, Guatemala, Honduras, Panama, Paraguay, Syria, Canada - NEX Board of TSX Venture Exchange listings, Bahamas, Bangladesh, Barbados, Trinidad & Tobago, Jamaica, Sri Lanka, Faroe Islands, Georgia, Guernsey, Isle of Man, Jersey, Mongolia, Niger, Senegal Sudan

Appendix 2.2 Detailed Data and Sample Description (Section 2.3)

Complete Sample (countries and firm-year observations for sample period between 1995-2014)				**FULL Worldscope Coverage** (since 2003/2007)	**Partial Worldscope Coverage** (since 2003/2007)
wc06026	***# firm-year***	***wc06026***	***# firm-year***	***wc06026***	***wc06026***
ARGENTINA	1863	MOROCCO	876	AUSTRALIA	ARGENTINA
AUSTRALIA	26324	NETHERLANDS	3243	AUSTRIA	CHANNEL ISLANDS
AUSTRIA	1771	NEW ZEALAND	2000	BELGIUM	CHILE
BELGIUM	2296	NORWAY	3361	BRAZIL	CZECH REPUBLIC
BRAZIL	7592	PAKISTAN	351	CANADA	EGYPT
CANADA	40022	PHILIPPINES	3735	CHINA	ESTONIA
CHANNEL ISLANDS	11	POLAND	4587	DENMARK	HUNGARY
CHILE	3909	PORTUGAL	1007	FINLAND	ICELAND
CHINA	39921	QATAR	461	FRANCE	INDIA
CZECH REPUBLIC	382	RUSSIA	22	GERMANY	ISRAEL
DENMARK	3135	RUSSIAN FED	6599	GREECE	LITHUANIA
EGYPT	2087	SAUDI ARABIA	1560	HONG KONG	MOROCCO
ESTONIA	179	SINGAPORE	727	INDONESIA	NEW ZEALAND
FINLAND	2427	SLOVAKIA	228	IRELAND	PAKISTAN
FRANCE	10856	SLOVENIA	468	ITALY	POLAND
GERMANY	11475	SOUTH AFRICA	6118	JAPAN	QATAR
GREECE	4675	SOUTH KOREA	19	LUXEMBOURG	RUSSIA
HONG KONG	1861	SPAIN	2821	MALAYSIA	RUSSIAN FED
HUNGARY	628	SRI LANKA	2431	MEXICO	SAUDI ARABIA
ICELAND	202	SWEDEN	6264	NETHERLANDS	SLOVAKIA
INDIA	27589	SWITZERLAND	3824	NORWAY	SLOVENIA
INDONESIA	7181	TAIWAN	24833	PHILIPPINES	SRI LANKA
IRELAND	1437	THAILAND	9948	PORTUGAL	TURKEY
ISRAEL	5154	TURKEY	4383	SINGAPORE	
ITALY	5220	UNITED KINGDOM	28371	SOUTH AFRICA	
JAPAN	73809	UNITED STATES	159710	SOUTH KOREA	
LITHUANIA	343			SPAIN	
LUXEMBOURG	987			SWEDEN	
MALAYSIA	16594			SWITZERLAND	
MEXICO	3466			TAIWAN	
				THAILAND	
				UNITED KINGDOM	
				UNITED STATES	

Appendix 2.2 (continued)

IFRS Sample	**IFRS_EU Sample**	**IFRS_EU_ENF Sample**	**IFRS_EU_nonENF Sample**	**IFRS_nonEU Sample**	**CONTROL Sample** (no IFRS until 2009)	**CONTROLadj Sample** (no IFRS until 2014)
wc06026	***wc06026***	***wc06026***	***wc06026***	***wc06026***	***wc06026***	***wc06026***
AUSTRALIA	AUSTRIA	FINLAND	AUSTRIA	AUSTRALIA	ARGENTINA	CHANNEL ISLANDS
AUSTRIA	BELGIUM	GERMANY	BELGIUM	HONG KONG	BRAZIL	CHINA
BELGIUM	CZECH REPUBLIC	ICELAND	CZECH REPUBLIC	ISRAEL	CANADA	EGYPT
CZECH REPUBLIC	DENMARK	NETHERLANDS	DENMARK	NEW ZEALAND	CHANNEL ISLANDS	INDIA
DENMARK	ESTONIA	NORWAY	ESTONIA	PAKISTAN	CHILE	INDONESIA
ESTONIA	FINLAND	UNITED KINGDOM	FRANCE	PHILIPPINES	CHINA	JAPAN
FINLAND	FRANCE		GREECE	SINGAPORE	EGYPT	MALAYSIA
FRANCE	GERMANY		HUNGARY	SOUTH AFRICA	INDIA	MOROCCO
GERMANY	GREECE		IRELAND	SWITZERLAND	INDONESIA	QATAR
GREECE	HUNGARY		ITALY	TURKEY	JAPAN	RUSSIA
HONG KONG	ICELAND		LITHUANIA		MALAYSIA	RUSSIAN FED
HUNGARY	IRELAND		LUXEMBOURG		MEXICO	SAUDI ARABIA
ICELAND	ITALY		POLAND		MOROCCO	THAILAND
IRELAND	LITHUANIA		PORTUGAL		QATAR	UNITED STATES
ISRAEL	LUXEMBOURG		SLOVAKIA		RUSSIA	
ITALY	NETHERLANDS		SLOVENIA		RUSSIAN FED	
LITHUANIA	NORWAY		SPAIN		SAUDI ARABIA	
LUXEMBOURG	POLAND		SWEDEN		SOUTH KOREA	
NETHERLANDS	PORTUGAL				SRI LANKA	
NEW ZEALAND	SLOVAKIA				TAIWAN	
NORWAY	SLOVENIA				THAILAND	
PAKISTAN	SPAIN				UNITED STATES	
PHILIPPINES	SWEDEN					
POLAND	UNITED KINGDOM					
PORTUGAL						
SINGAPORE						
SLOVAKIA						
SLOVENIA						
SOUTH AFRICA						
SPAIN						
SWEDEN						
SWITZERLAND						
TURKEY						
UNITED KINGDOM						

Notes: This table displays the number of firms and corresponding countries available in Worldscope according to the following minimum data requirements: ISIN (wc06008), fiscal year (wc05350), country of firm domicile (wc06026), accounting standard followed (wc07536), total assets (wc02999), book value of equity (wc03501), and net income (wc01751). We acknowledge that Switzerland has not mandated the use of only IFRS (Zeff, 2016). Still, we include it to ensure consistency between the analyses of CHL (2013) and this study.

Appendix 2.3 Sample Details and CHL (2013) Treatment Groups (Section 2.4)

						Disaggregation along SEI					
Country	**# obs.**	**# obs. CHL (2013)**	**# adj. obs. in mand. IFRS adoption quarter/in 4th quart. 05**	**# (IFRS) obs. at the end of sample period**	**Relative change in # obs. between those periods**	**$IFRS_{High}$**	**$IFRS_{Low}$**	**$IFRS\_EU_{High}$**	**$IFRS\_EU_{Low}$**	**$IFRS\_EU\_Enf_{High}$**	**$IFRS\_EU\_Enf_{Low}$**
European Union Countries											
Austria	1703	1201	46	54	0.17	0	1	0	1	0	0
Belgium	3553	3250	91	104	0.14	0	0	0	1	0	0
Czech Rep.	0	156	0	0	0.00	0	0	0	0	0	0
Denmark	4891	4802	120	119	-0.01	1	0	1	0	0	0
Estonia	209	146	6	15	1.50	0	1	0	1	0	0
Finland	4428	4056	121	124	0.02	1	0	1	0	1	0
France	18176	15817	441	432	-0.02	1	0	1	0	0	0
Germany	14227	8296	332	356	0.07	1	0	1	0	0	1
Greece	3956	0	1	254	253.00	0	1	0	1	0	0
Hungary	870	674	24	0	-1.00	1	0	1	0	0	0
Iceland	82	89	1	6	5.00	0	1	0	1	0	1
Ireland	1273	629	30	30	0.00	1	0	1	0	0	0
Italy	7963	7569	208	248	0.19	0	1	0	1	0	0
Lithuania	415	71	18	22	0.22	0	1	0	1	0	0
Luxembourg	645	11	20	16	-0.20	1	0	1	0	0	0
Netherlands	4732	3443	117	111	-0.05	1	0	1	0	1	0
Norway	4738	4897	118	159	0.35	0	1	0	1	0	1
Poland	4169	5454	144	208	0.44	0	1	0	1	0	0
Portugal	1462	1361	38	42	0.11	1	0	1	0	0	0
Slovakia	0	63	0	0	0.00	0	0	0	0	0	0
Slovenia	373	208	8	22	1.75	0	1	0	1	0	0
Spain	3902	3195	103	110	0.07	1	0	1	0	0	0
Sweden	9888	8071	264	318	0.20	0	1	0	1	0	0
UK	36309	18809	703	746	0.06	1	0	1	0	1	0
IFRS adoption countries outside the European Union											
Abu Dhabi	686	308	1	71	70.00	0	1	0	0	0	0
Australia	39374	31543	1140	1391	0.22	0	1	0	0	0	0
Hong Kong	26976	23222	26	46	0.77	0	1	0	0	0	0
Israel	4611	385	328	360	0.10	1	0	0	0	0	0
New Zealand	3656	3060	114	117	0.03	1	0	0	0	0	0

Appendix 2.3 (continued)

						Disaggregation along SEI					
Country	**# obs.**	**# obs. CHL (2013)**	**# adj. obs. in mand. IFRS adoption quarter/in 4th quart. 05**	**# (IFRS) obs. at the end of sample period**	**Relative change in # obs. between those periods**	**$IFRS_{High}$**	**$IFRS_{Low}$**	**$IFRS\_EU_{High}$**	**$IFRS\_EU_{Low}$**	**$IFRS\_EU\_Enf_{High}$**	**$IFRS\_EU\_Enf_{Low}$**
IFRS adoption countries outside the European Union (continued)											
Pakistan	1208	722	1	0	-1.00	0	0	0	0	0	0
Philippines	4527	4495	123	153	0.24	0	1	0	0	0	0
Singapore	15650	14841	11	26	1.36	0	1	0	0	0	0
South Africa	8909	6635	232	284	0.22	0	1	0	0	0	0
Switzerland	6453	5927	150	155	0.03	1	0	0	0	0	0
Turkey	5030	5842	58	63	0.09	1	0	0	0	0	0
Non-IFRS countries											
Argentina	731	128	1	57	56.00	0	0	0	0	0	0
Brazil	5791	4585	151	249	0.65	0	0	0	0	0	0
Canada	24582	13226	1	1938	1937.00	0	0	0	0	0	0
Channel Isl.	224	436	1	27	26.00	0	0	0	0	0	0
Chile	1395	280	1	106	105.00	0	0	0	0	0	0
China	40195	39562	1079	1555	0.44	0	0	0	0	0	0
Egypt	2146	1635	60	107	0.78	0	0	0	0	0	0
India	14904	156	338	436	0.29	0	0	0	0	0	0
Indonesia	6616	6864	182	202	0.11	0	0	0	0	0	0
Japan	111879	108877	3183	3080	-0.03	0	0	0	0	0	0
Malaysia	27142	26509	816	838	0.03	0	0	0	0	0	0
Mexico	2789	912	80	92	0.15	0	0	0	0	0	0
Morocco	660	397	1	48	47.00	0	0	0	0	0	0
Qatar	228	179	1	40	39.00	0	0	0	0	0	0
Russia	1749	1114	36	140	2.89	0	0	0	0	0	0
Saudi Arabia	1233	918	13	121	8.31	0	0	0	0	0	0
South Korea	37308	28630	1134	1502	0.32	0	0	0	0	0	0
Sri Lanka	637	249	1	75	74.00	0	0	0	0	0	0
Taiwan	18205	15305	50	1339	25.78	0	0	0	0	0	0
Thailand	10727	11526	333	403	0.21	0	0	0	0	0	0
United States	173108	163016	4289	5198	0.21	0	0	0	0	0	0

Appendix 2.4 Distribution of number of listed firms across CHL Country-Clusters (CHL replication sample)

Year	*CONTROL*		*IFRS*		*IFRS_EU*		*IFRS_EU_ENF*		*IFRS_EU_nonENF*		*IFRS_nonEU*	
	#Firms	*Δ in # F*	*#Firms*	*Δ in # F*	*#Firms*	*Δ in # F*	*#Firms*	*Δ in # F*	*#Firms*	*Δ in # F*	*#Firms*	*Δ in # F*
1Q2001	9376		4011		2573		1382		1191		1438	
2Q2001	9839	4.94%	5332	32.93%	3031	17.80%	1509	9.19%	1522	27.79%	2301	60.01%
3Q2001	10572	7.45%	5540	3.90%	3181	4.95%	1577	4.51%	1604	5.39%	2359	2.52%
4Q2001	10473	-0.94%	5742	3.65%	3306	3.93%	1641	4.06%	1665	3.80%	2436	3.26%
1Q2002	10595	1.16%	5946	3.55%	3460	4.66%	1736	5.79%	1724	3.54%	2486	2.05%
2Q2002	10712	1.10%	6224	4.68%	3697	6.85%	1948	12.21%	1749	1.45%	2527	1.65%
3Q2002	10768	0.52%	6353	2.07%	3746	1.33%	2017	3.54%	1729	-1.14%	2607	3.17%
4Q2002	10796	0.26%	6341	-0.19%	3731	-0.40%	1976	-2.03%	1755	1.50%	2610	0.12%
1Q2003	10569	-2.10%	6111	-3.63%	3519	-5.68%	1844	-6.68%	1675	-4.56%	2592	-0.69%
2Q2003	10696	1.20%	6252	2.31%	3617	2.78%	1898	2.93%	1719	2.63%	2635	1.66%
3Q2003	10808	1.05%	6412	2.56%	3639	0.61%	1932	1.79%	1707	-0.70%	2773	5.24%
4Q2003	10876	0.63%	6506	1.47%	3682	1.18%	1931	-0.05%	1751	2.58%	2824	1.84%
1Q2004	10713	-1.50%	6273	-3.58%	3495	-5.08%	1789	-7.35%	1706	-2.57%	2778	-1.63%
2Q2004	10915	1.89%	6339	1.05%	3511	0.46%	1795	0.34%	1716	0.59%	2828	1.80%
3Q2004	11039	1.14%	6367	0.44%	3485	-0.74%	1761	-1.89%	1724	0.47%	2882	1.91%
4Q2004	11098	0.53%	6555	2.95%	3587	2.93%	1813	2.95%	1774	2.90%	2968	2.98%
1Q2005	11336	2.14%	6494	-0.93%	3465	-3.40%	1714	-5.46%	1751	-1.30%	3029	2.06%
2Q2005	11486	1.32%	6378	-1.79%	3305	-4.62%	1746	1.87%	1559	-10.97%	3073	1.45%
3Q2005	11566	0.70%	6538	2.51%	3391	2.60%	1756	0.57%	1635	4.87%	3147	2.41%
4Q2005	11744	1.54%	6743	3.14%	3494	3.04%	1817	3.47%	1677	2.57%	3249	3.24%
1Q2006	12315	4.86%	6783	0.59%	3479	-0.43%	1772	-2.48%	1707	1.79%	3304	1.69%
2Q2006	13914	12.98%	6947	2.42%	3564	2.44%	1825	2.99%	1739	1.87%	3383	2.39%
3Q2006	14911	7.17%	6932	-0.22%	3512	-1.46%	1793	-1.75%	1719	-1.15%	3420	1.09%
4Q2006	17973	20.54%	7436	7.27%	3641	3.67%	1858	3.63%	1783	3.72%	3795	10.96%
1Q2007	16916	-5.88%	7288	-1.99%	3489	-4.17%	1739	-6.40%	1750	-1.85%	3799	0.11%
2Q2007	17007	0.54%	7441	2.10%	3546	1.63%	1733	-0.35%	1813	3.60%	3895	2.53%
3Q2007	17724	4.22%	7780	4.56%	3639	2.62%	1837	6.00%	1802	-0.61%	4141	6.32%
4Q2007	17638	-0.49%	8054	3.52%	3823	5.06%	1929	5.01%	1894	5.11%	4231	2.17%
1Q2008	17221	-2.36%	7776	-3.45%	3617	-5.39%	1772	-8.14%	1845	-2.59%	4159	-1.70%
2Q2008	17512	1.69%	7980	2.62%	3723	2.93%	1823	2.88%	1900	2.98%	4257	2.36%
3Q2008	17552	0.23%	7985	0.06%	3753	0.81%	1857	1.87%	1896	-0.21%	4232	-0.59%
4Q2008	16720	-4.74%	7862	-1.54%	3891	3.68%	1901	2.37%	1990	4.96%	3971	-6.17%
1Q2009	16572	-0.89%	7684	-2.26%	3667	-5.76%	1743	-8.31%	1924	-3.32%	4017	1.16%
2Q2009	17327	4.56%	8233	7.14%	3971	8.29%	1777	1.95%	2194	14.03%	4262	6.10%
3Q2009	17417	0.52%	8076	-1.91%	3731	-6.04%	1787	0.56%	1944	-11.39%	4345	1.95%
4Q2009	17553	0.78%	8330	3.15%	4003	7.29%	1788	0.06%	2215	13.94%	4327	-0.41%
Δ since 4Q2005		49.46%		23.54%		14.57%		-1.60%		32.08%		33.18%

3 The role of proportion and reliability of fair value assets on informational properties of DVAs

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Abstract: A large public debate denounces debt value adjustments due to a change in own credit risk (DVAs) as "junk income" without relation to value creation. In contrast, recent literature finds that DVAs are value relevant if a firm has few unrecognized intangible assets (Cedergren et al. 2015). I add to this by testing the informational properties of DVAs conditional on the proportion and reliability of related fair value assets. Using a sample of 617 firm-quarters of US banks in the period of 2007-2014, I find firms' DVAs to be value relevant when a large proportion of related assets are measured at fair value reliably, specifically, at level 1. In contrast, when a large proportion of fair value assets are measured less reliably, i.e., at level 2 and 3, DVAs are not value relevant. I further find that financial markets do not price DVAs efficiently when large proportions of related assets are measured unreliably. Finally, I find that DVAs' persistence is moderated by the amount of related level 2 fair value assets. Taken together, the results imply that reliable information on related fair value assets is a necessary condition for controversial DVAs to reflect useful information for investors.

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3.1 Introduction

In this study, I test the informational properties of debt valuation adjustments due to a change in credit risk (DVAs), conditional on the proportion and reliability of related financial assets carried at fair value. In 2007, the FASB introduced DVAs into US GAAP accounting with FAS 159 "The Fair Value Option for Financial Assets and Financial Liabilities" (FASB 2007). If a firm chooses to apply this said fair value option for liabilities (FVOL) to (a part of) its debt, this implies that this debt is irrevocably carried at fair value in future periods. If the market value of said debt changes significantly due to a change in the credit risk of the issuing firm, the firm adjusts the debt's fair value accordingly and recognizes these changes as DVAs in its net income.

The main objective of DVAs is to mitigate volatility that arises if assets are measured at fair value but liabilities are not (FASB 2007). An unexpected change in a firm's assets' value simultaneously triggers a change in the firm's liabilities' value (e.g. a decrease in assets' value leads to a decrease in liabilities' value). This is because of the declined assets' value, which lowers the firm's ability to pay back its debt, i.e., increases the firm's credit risk. If the firm measures both, assets and liabilities, at fair value, it will recognize concurrent write-downs on assets and on liabilities in this case. The asset write-downs result in a negative net income effect, while the liability write-downs result in a positive net income effect that mitigates the first. The second effect is the intended mitigating effect of DVAs. In the opposite case of the example, an increase in asset value, the DVAs' mitigating effect correspondingly runs in the other direction.

Still, under certain circumstances, DVAs' income effect does not only mitigate the income effects from unexpected asset value changes but exceeds them. If the change in credit risk stems from unexpected changes in the value of assets that are not or not reliably measured at fair value, it is possible that the balance sheet only reflects the liabilities' value change to a full extend but not the assets' value change. Accordingly, in such a case, only DVAs' income effect is fully recognized but not the income effect that it is supposed to mitigate. If so, an increase in credit risk leads to a positive net income effect and a decrease in credit risk leads to a negative net income effect.

Critics perceive this characteristic of DVAs as "counterintuitive" (Keoun 2008) and started an intensive public debate. DVAs' opponents state that investors do not perceive

DVAs as value relevant but rather as "junk income" (Dash 2009) or "paper profits" to which they "rightly don't ascribe much value" (Eavis 2008). In line with this, recent experimental literature finds that investors have difficulties interpreting firms' performances and risk if DVAs influence net income (Gaynor et al. 2011; Lachmann et al. 2015). Representatives from both large international standard setters, FASB and IASB, acknowledged that DVAs are potentially "misleading" investors (FASB 2007; IASB 2009). Accordingly, both standard setters recently changed DVA accounting regulation in the respective standards so that, in the future, firms will recognize DVAs in other comprehensive income instead of net income. (FASB 2016; IASB 2014b)

On the other hand, advocates of DVAs' value relevance argue that DVAs reflect wealth transfers between shareholders and debtholders as a paper by Merton (1974) details. In this context, they stress the importance of an accurate display of credit risk changes on the balance's asset side for investors to correctly assess DVAs. If a firm's assets are either unrecognized or not recognized at fair value, "it is difficult to see how credit impairment can be deemed to have taken place" (Peasnell 2006). Findings from prior literature support this notion. Cedergren et al. 2015 find that DVAs are value relevant when a firm has a low level of unrecognized intangible assets but are not value relevant when this level is high. Fontes et al. 2014 find that DVAs decrease information asymmetry when a large proportion of firm assets are measured at fair value but potentially increase information asymmetry otherwise.

No research yet has addressed the role of the reliability of firms' fair value assets' valuation for investors' perception of DVAs. This is somewhat surprising, given that prior research on fair value assets' reliability has shown its impact on financial markets in related areas, for example, on share prices (Song et al. 2010) and on credit risk (Kadous et al. 2012). Building on prior literature, I expect that a large proportion of reliably measured fair values on a firm's balance sheet improve the informational properties of DVAs.

My sample consists of 617 firm-quarters of US bank holding companies that applied the FVOL between 2007 and 2014 and were therefore required to recognize and disclose significant DVAs. As tests, I use established regression models that measure the association of current share returns and net income with quantifiable information, in my case: DVAs. As a proxy for fair values' reliability, I use the hierarchical FASB system that divides fair values into level 1, level 2, and level 3 where an increasing level means decreasing reliability (FASB 2006).

In line with Cedergren et al. 2015 but in contrast to Chung et al. 2012, I do not find DVAs to be value relevant for investors *per se*. Testing the role of the proportion of firms' fair value assets, I do not find that a higher proportion of fair value assets increases DVAs' value relevance. For my main test, I split firms' fair value assets into reliably measured fair value assets and less reliably measured fair value assets. As expected, I find that a higher proportion of reliably measured fair value assets increase DVAs' value relevance for investors. Also as predicted, a higher proportion of less reliably measured fair value assets does not increase DVAs' value relevance. Taken together, the findings suggest that the reliability of related assets' fair values have implications for investors' perception of DVAs' value relevance.

Testing DVAs' market pricing, I find that investors' reaction to DVAs' announcements are moderated by the proportion of unreliably measured related fair value assets. Specifically, investors tend to overreact to DVAs when firms carry lower proportions of level 2 and 3 fair value assets on the balance sheet. In contrast, when firms carry larger proportions of level 2 and 3 fair value assets on the balance sheet, financial markets tend to underreact to DVAs' announcements, consistent with a conservative pricing of DVAs with low value relevance. Finally, testing DVAs' persistence, I find that DVAs are in themselves not a persistent part of net income. However, firms with large proportions of level 2 fair value assets have persistent DVAs. This finding is in line with firms exerting discretion over assets' valuation to smooth earnings among other explanations.

I conduct several sensitivity tests to challenge my findings from the main analysis. First, I substitute my explaining variables by ratios of reliably measured fair values to less reliably measured fair values. Next, as an alternative proxy for fair value assets' reliability, I use the composite governance index GOV_{41} that was established by Aggarwal et al. (2011). I proceed by rerunning my analyses with additional fixed effects. Finally, as a different return measure, I consider quarterly compounded returns. The findings from the sensitivity tests largely support the results from the main tests.

The findings directly add to the findings of Cedergren et al. (2015), thereby contributing to the literature on informational properties of DVAs. Specifically, they improve our understanding of the complementary financial information that investors require to perceive DVAs as a value relevant part of net income and to price them efficiently. The findings also contribute to the literature on the role of fair value assets' reliability centered

around Song et al. (2010) by showing asset reliability's importance for DVAs' correct financial market perception. Against the background of the ongoing DVA debate and the recent changes in DVAs' accounting regulation, the findings should also be of interest for regulatory bodies.

The paper proceeds as follows. The next section outlines the theoretical background on the fair value option for liabilities and on DVAs. Section 3.3 summarizes prior literature. Section 3.4 describes the research methodology. Section 3.5 details the sample selection and data collection. Section 3.6 presents the main tests' results and Section 3.7 provides sensitivity tests' results. Section 3.8 concludes.

3.2 Background: The fair value option for liabilities and debt valuation adjustments

In February 2007, the Financial Accounting Standards Board (FASB) issued the Statement of Financial Accounting Standards No. 159 (FAS 159), "The Fair Value Option for Financial Assets and Financial Liabilities" (new codification since 2009: ASC Topic 825-10). The therein codified fair value option for financial liabilities (FVOL) permits firms to measure financial liabilities at fair value. With some exemptions, the option is applicable to all financial liabilities. Firms can elect the FVOL for an instrument only at specified "election dates", for example, the day of an item's first recognition. The option can only be applied to entire instruments (not to portions) and cannot be revoked (FASB 2007). Besides the election dates, as a one-time measure, FAS 159 allowed firms to apply the FVOL to eligible items at the standard's effective date, which was either the fiscal year beginning after November 15, 2007, or for the early adoption option, the fiscal year that began before the effective date.

The measurement of liabilities for which firms elected the FVOL follows FAS 157 "Fair Value Measurements" (nowadays: ASC 820). This standard defines the fair value as "the price that would be (…) paid to transfer a liability in an orderly transaction between market participants" (FASB 2006, p. 2). Consequently, firms have to treat liabilities' fair values as (hypothetical) market values. At initial recognition, a liability's fair value equals its "transaction price". For the subsequent measurement, FAS 157 offers different valuation techniques to ensure that the liabilities' fair values continuously reflect current market prices (FASB 2006).

In each interim or annual financial statement, FAS 159 requires firms to disclose their reasons for electing the fair value option for each item. Firms also need to disclose which balance sheet items contain liabilities for which the FVOL has been elected but they are not required to disclose the exact amount of FVOL liabilities. Specifically for long-term debt instruments, firms further have to disclose differences between aggregate fair values and unpaid principal balances. Regarding firms' income statements, FAS 159 requires firms to disclose the gains and losses from fair value changes for each income statement line item included in earnings. Generally, gains and losses from changes in fair values for which the FVOL has been elected do not need to be disclosed separately from other fair value changes in the same line item. As an exception, firms need to separately disclose gains and losses from fair value changes that are attributable to changes in firms' own credit risk.

There are several causes that can change the fair value of a liability. For example, liabilities' market values typically increase when the central bank responsible for the currency that the liabilities are denoted in decreases the respective key interest rate. Also, when financial markets' general pricing of certain risks inherent in liabilities (e.g. credit risk) changes, the liabilities' fair values alter. Most importantly for my study, however, are changes of the value of a firm's liabilities due to changes in its individual credit risk. For example, if a firm's ability to meet its outstanding debt decreases, the market value of its issued debt decreases simultaneously. On the other hand, if markets assume that a firm's solvency improved, its liabilities' market values increase. Such value changes are called DVAs – debt valuation adjustments due to a change in own credit risk.

As Merton (1974) explains, DVAs represent wealth redistributions between the shareholders and the debtholders of the firm that issued the debt. This is because a firm's debt implies an option of the shareholders to put the firm's assets to the debtholders for an amount equal to the debt's face value. This option constitutes an economic asset to the firm and the asset's value depends on the value of the firm's debt (Barth and Landsman 1995). For example, a decrease in the value of the firm's debt means a wealth transfer from debtholders to shareholders. Or, to quote Barth and Landsman (1995): "Effectively, the debtholder contractually has committed to accept an interest rate that subsequently proves to be economically too low."

If recognized financial assets and financial liabilities are measured at reliable fair values, DVAs help mitigate earnings volatility. For example, an unexpected decrease in an

asset's value leads to an increase in credit risk. The increased credit risk in turn leads to a decrease in the corresponding liability's fair value which results in positive DVAs. Here, the positive DVAs' income effect mitigates the negative income affect from the asset's depreciation. In reality, the described match can be imperfect. Reasons for this can be that the affected financial assets are not recognized on the balance sheet, that the assets are not measured at fair value, or that the assets' fair values are not measured reliably. Under these circumstances, the DVAs' income effect does not mitigate but dominate the initial income effect. In the described example, the asset's value decreases and the consequential increase in credit risk would thus result in a net profit.

Experimental evidence suggests that investors potentially misinterpret the income effects of DVAs, i.e., they perceive gains from DVAs as a sign for lowered credit risk (Gaynor et al. 2011) and improved firm performance (Lachmann et al. 2015) when actually the opposite is true. Concerns about this have stirred up a large public debate (see Section 4.2.2 and Appendix 4.4). Critics argue that DVAs are counterintuitive "accounting tricks" (Carver 2012a) that cloud investors' view on actual firm performance. DVAs can be large and even alter the sign of net income. For example, in the third quarter of 2011, a positive $3.41 billion DVA helped Morgan Stanley to an overall $2.2 billion profit.[13] Acknowledging the controversy around DVAs (Rapoport and Lucchetti 2011; IASB 2009), international standard setters changed the still relatively young regulation. After an amendment to ASC 825, DVAs will be recognized in other comprehensive income instead of net income in fiscal years beginning after December 15, 2017 (FASB 2016). With IAS 39, IFRS accounting also introduced a fair value option for financial assets and liabilities that became effective in 2005. As in US GAAP accounting, IFRS firms initially have been recognizing DVAs in net income. This changed with the introduction of IFRS 9 in 2014 that succeeded IAS 39 and that requires recognition of DVAs in other comprehensive income instead. IFRS 9 will come into effect on 1 January 2018 with early application permitted (IASB 2014b).

3.3 Prior literature and empirical predictions

In this study, I test the implications of related fair value assets' reliability for informational properties of DVAs. By doing so, I contribute to the young but growing literature of DVAs'

[13] As an example for the opposite direction of action, a negative $3.22 billion DVA turned Bank of America's net profit into a $1.24 billion loss in the fourth quarter of 2010.

financial markets' perception. Barth et al. (2008) find that the decrease (increase) in a firm's equity value associated with an increase (decrease) in the credit risk of the firm is mitigated by a higher debt-to-assets ratio. Confirming theoretical predictions of Merton (1974), the finding suggests that investors correctly price the wealth transfer from debtholders to shareholders that DVAs represent when firms do not recognize DVAs (the sample covers a period before DVAs' introduction in accounting). Chung et al. (2012) are the first to directly test the value relevance of DVAs. They find that investors perceive DVAs as economic income, i.e., as value-relevant. They also find that gains and losses from FAS 159 (which include DVAs) are relevant to investors' perception of firms' risk. My study is most closely related to a recent study of Cedergren et al. (2015). The authors argue that a firm's net income becomes biased if a change in credit risk is only reflected on the liabilities side (through DVAs) but not on the asset side. They develop a model that demonstrates a negative association between unrecognized asset value changes and the value relevance of corresponding DVAs. They test the model using the amount of unrecognized intangible assets (UIA) as a mediating variable. As predicted, they find that a large amount of unrecognized assets decreases the value relevance of corresponding DVAs.

Motivating further research, Cedergren et al. (2015) state that "[i]t is possible that other assets not measured at fair value could also affect the valuation of DVA in the same way as UIA does". In an according study, Fontes et al. (2014) find that accounting completeness for the asset side of the balance sheet influences how investors interpret reported DVAs. In their sample of European banks, DVA recognition *per se* potentially increases information asymmetry among investors, by misleading them about firm's income and performance. However, the relation is different for firms in their sample who measure a large proportion of assets at fair value. For those firms, the DVAs' recognition decreases information asymmetries. The authors conclude that investors are "more confident in their ability to ascertain the reliability of reported [DVA] gains and losses when the sources of these gains and losses are recognized in the financial statements." I expand this stream of literature by testing whether information properties of DVAs, namely their value relevance, their market pricing, and their persistence, additionally depend on the reliability of the related assets' fair value measurement.

I draw further motivation for this analysis from a second stream of literature to which this study also contributes: the literature on financial market consequences of fair value

assets' measurement reliability. Prior research in this field shows that investors' perception of fair values' reliability has significant influence on their decisions. As a proxy for fair values' reliability, these studies typically use the hierarchic system introduced by FAS 157 (FASB 2016). It measures fair values' reliability according to the lowest level of inputs that were used to determine them (FASB 2006). Level 1 inputs are quoted prices in active markets for identical assets or liabilities. Therefore, level 1 fair values reflect the highest reliability. Level 2 inputs are inputs other than quoted prices that are observable. Finally, unobservable inputs are level 3.

A central study of this literature, Song et al. (2010), compares the value relevance of fair value instruments for 1,260 US bank firm-quarters depending on the instruments' level of reliability. Generally, they find fair values of assets and liabilities of all levels to be value relevant. Still, in their sample, the value relevance of level 1 and level 2 instruments is significantly higher than the value relevance of level 3 instruments. Kolev (2008) confirms this finding for fair value assets for a sample of 349 US bank-firm-quarters. Goh et al. (2015) also find fair value assets and liabilities of all levels to be value relevant. However, as Song et al. (2010), they find differences between the value relevance of level 1, level 2, and level 3 fair value assets. Finally, a recent study by Arora et al. 2014 sheds light on the consequences of fair value assets' measurement reliability for credit markets. The authors find that firms with lower asset reliability have higher short-horizon credit risk.

Building on the findings of these two streams of literature, I expect that investors' perception of DVAs is mediated by the proportion of reliably measured related fair value assets. Specifically, I expect that a large proportion of reliably measured fair value assets on the balance sheet provides information on the sources, the quantity, and the quality of the credit risk change that underlies DVAs. Accordingly, investors' perception of DVAs, as expressed by its value relevance, will be in line with management's. However, I do not expect that a large proportion of less reliably measured fair value assets enhance investors' perception of DVAs in a similar way.

3.4 Research methodology

To examine the associations between informational properties of DVAs and the proportion and reliability of fair value assets, I use OLS regression models as employed by prior literature. The value relevance literature studies the associations between accounting measures

and equity market values. This suggests testing whether these accounting measures explain cross-sectional variation in share prices (Barth et al. 2001). The objective is to examine whether and how quickly the accounting measures reflect changes in the information set that is incorporated in share returns over a given period (Kothari 2001). To test this association between share returns and individual net income components, prior literature uses return regressions with the components of a disaggregated net income as independent variables (Ohlson and Penman 1992; Lipe 1986). In accordance with this prior research, I use the following model:

$$R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA1_{it} + \beta_4 DVA_{it} * FVA23_{it} + \beta_5 FVA1_{it} + \beta_6 FVA23_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it} \quad (1)$$

where *R* is the quarterly share return of firm *i* in period *t*, *E_excl_DVA* is quarterly net income minus DVAs, *DVA* are quarterly debt value adjustments due to a change in own credit risk on liabilities for which the fair value option has been elected, *FVA1* is the proportion of level 1 fair value assets to total assets, *FVA23* is the proportion of level 2 and 3 fair value assets to total assets, *UIA* is the amount of unrecognized intangible assets, and *OCI* is the quarterly other comprehensive income. I scale *E*, *E_excl_DVA*, *DVA*, *OCI*, and *UIA* by the market value of equity in t-1 (see Cedergren et al. 2015; Barth et al. 2008). I further include leverage (*LEV*) and the natural logarithm of firm's market value (*SIZE*) as control variables (Chen et al. 2011). I use robust standard errors that are clustered along the dimensions quarter-years and firms (White 1980).

My categorization of reliable fair values (level 1 fair values) versus less reliable fair values (level 2 and level 3 fair values) follows Arora et al. (2014) who use the proportion of level 2 and 3 fair values to total assets as proxy for unreliable assets. Further, it is in line with Penman 2007 who discusses level 2 and level 3 fair value measurements jointly, as both "admit estimates of hypothetical market prices". However, theoretical considerations (Hitz 2007) and recent empirical literature (Song et al. 2010; Goh et al. 2015) are rather consistent with differences in the value relevance of level 2 and level 3 fair values. To be able to explore such potential differences, I also consider the following alternative model:

$$R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA12_{it} + \beta_4 DVA_{it} * FVA3_{it} + \beta_5 FVA12_{it} + \beta_6 FVA3_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it} \quad (2)$$

where *FVA12* is the proportion of level 1 and 2 fair value assets to total assets and *FVA3* is the proportion of level 3 fair value assets to total assets.

The literature on market pricing examines associations between current accounting measures and future share returns (Sloan 1996). The goal is to test how efficiently current accounting information is reflected in share returns. Assuming efficient, quick pricing, current accounting information should have no significant association with future share returns. To gain insights in how financial markets react to published DVA information under different circumstances, I test the association between DVAs and future stock prices moderated by proportion and level of fair value assets. Following prior literature (Chen et al. 2011; Kraft et al. 2007), the model for my tests is:

$$R_{it+4} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA1_{it} + \beta_4 DVA_{it} * FVA23_{it} + \beta_5 FVA1_{it} + \beta_6 FVA23_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it} \quad (3)$$

where R_{it+4} is the share return of firm i measured between quaters t+1 and t+4. Again, I scale *E*, *E_excl_DVA*, *DVA*, *OCI*, and *UIA* by the market value of equity in t-1 and use robust standard errors that are clustered along the dimensions quarter-year and firms.

As for the test of value relevance, I additionally employ a model with a different distinction between reliably and unreliably measured fair value assets:

$$R_{it+4} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA12_{it} + \beta_4 DVA_{it} * FVA3_{it} + \beta_5 FVA12_{it} + \beta_6 FVA3_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it} \quad (4)$$

The earnings persistence literature examines the predictive value of current earnings and its components for future earnings. In this context, high persistence is considered a desirable informational property (Hanlon 2005). However, critics argue that persistence can also be achieved through earnings management (Dechow et al. 2010). To measure persistence, prior

literature directly tests the association between earnings' components and future earnings. In accordance with this literature, I use the following model (see Chen et al. 2011):

$$E_{it+4} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA1_{it} + \beta_4 DVA_{it} * FVA23_{it} + \beta_5 FVA1_{it} + \beta_6 FVA23_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it} \quad (5)$$

Again, I scale *E*, *E_excl_DVA*, *DVA*, *OCI*, and *UIA* by total assets in t-1 (Chen et al. 2011). As for the other models, I use robust standard errors clustered along quarter-years and firms. The model with the alternative distinction of fair values' reliability looks as follows:

$$E_{it+4} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA12_{it} + \beta_4 DVA_{it} * FVA3_{it} + \beta_5 FVA12_{it} + \beta_6 FVA3_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it} \quad (6)$$

All variables used in the analyses are summarized in Table 3.1.

Table 3.1 Variable Definitions and Measurement

Panel A: Variables of the main tests	
Variable Name	**Definition**
R	Quarterly share returns, calculated as raw returns: $\frac{P_t - P_{t-1}}{P_{t-1}}$ where *P* is Thomson Reuters Datastream item P and *t* indicates quarters
E	Net income before preferred dividends (Thomson Reuters Worldscope item: WC01651A or, if not available from this source, from 10-Q/10-K filings)
E_excl_DVA	*E – DVA*
DVA	Debt value adjustments due to a change in own credit risk on liabilities for which the fair value option has been elected (DVAs), source: 10-Q/10-K filings
FVA	Fair value assets (source: 10-Q/10-K filings) divided by total assets (WC02999A or, if not available from this source, from 10-Q/10-K filings)
FVA1	Fair value assets at level 1 divided by total assets
FVA23	Fair value assets at level 2 and 3 divided by total assets
FVA12	Fair value assets at level 1 and 2 divided by total assets
FVA3	Fair value assets at level 3 divided by total assets
UIA	Unrecognized intangible assets, measured as market value of equity (Datastream item: MV) minus book value of equity (WC03501A or, if not available from this source, from 10-Q/10-K filings)
OCI	Quarterly other comprehensive income, source: 10-Q/10-K filings
LEV	Leverage, calculated as total liabilities (WC03351A or, if not available from this source, from 10-Q/10-K filings) divided by total assets
SIZE	Natural logarithm of market value of equity (Datastream item: MV)
Panel B: Variables of the sensitivity tests	
Variable Name	**Definition**
HighFVA	Binary variable indicating an above median value of *FVA* within the full sample
HighFVA1	Binary variable indicating an above median value of *FVA1/FVA23* within the full sample
HighFVA12	Binary variable indicating an above median value of *FVA12/FVA3* within the full sample
HighFVA1_75	Binary variable indicating a value above the 75%-quantile of *FVA1/FVA23* within the full sample
GOV41	Composite governance index of 41 individual attributes, established by Aggarwal et al. 2011. As the index is not available as time-series data beyond the year 2008, I assume stickiness of the index on the firm-level and use the youngest available index for each firm as a firm-level variable, source: http://faculty.msb.edu/aggarwal/Gov.xls
CR	Quarterly share returns, calculated as daily compounded returns

This table summarizes the definitions and the measurements of the variables used in this paper. All 10-Q/10-K filings are collected from the SEC Edgar database.

3.5 Sample selection and data collection

Following prior literature on the fair value option for liabilities, I constrain my sample to firms from the banking industry (e.g. Cedergren et al. 2015; Schneider and Tran 2015). I consider 36,096 firm-quarters between 2007 and 2014 of all 1128 bank holding companies

that filed a report on *Consolidated Financial Statements for Holding Companies* (FRY9C) in the fourth quarter of 2014. FRY9C reports contain the item "F553: Net gains (losses) on liabilities [elected to account for under a fair value option]". It follows that all firms that filed a non-zero entry for this item are necessarily adopters of the fair value option for liabilities (FVOL) while the others are most likely not (see very similar Cedergren et al. 2015). The drawbacks of this approach are: It falsely labels adopters as non-adopters of the FVOL if they never had a change in the value of fair value liabilities. Because such firms are not in the focus of my investigation, I find the approach still suitable. It also neglects firms that applied the FVOL only in 2007, because the item "F553: Net gains (losses) on liabilities [elected to account for under a fair value option]" has only been part of FRY9C reports since the first quarter of 2008. Lastly, it ignores all bank holding companies that did not file FRY9C reports in the fourth quarter of 2014. Therefore, the sample contains a size and a survivorship bias. The size bias should be a minor concern, as it is mostly large firms that elect the FVOL (see Henry 2009; Guthrie et al. 2011).

Because the regulatory data from FRY9C filings is possibly not fully compliant with US GAAP regulation, I only use it to identify 64 "FVOL adopters" (i.e., firms with a minimum of one non-zero entry for item F553 in the sample period). This excludes 34,048 firm-quarters. Afterwards, I hand-collect all available 10-Q and 10-K filings of the 64 FVOL adopters between 2007 and 2014. I exclude 800 firm-quarters of 25 adopters for which there are no SEC filings available for the sample period.[14] I further exclude 192 firm-quarters of six adopters that did not disclose any information on FVOL adoption in their 10-Q or 10-K filings. Finally, I exclude 32 firm-quarters of one adopter whose price data is unavailable in the Thomson Reuters Datastream database. My final sample consists of the 617 firm-quarters in which the remaining 32 bank holding companies adopted the FVOL. Table 3.2 displays the sample selection process.

[14] These firms are not listed in the United States or not required to file reports for other legal reasons.

Table 3.2 Sample Selection

Sample: Firms-quarters with FVOL adoption between 2007 and 2014		
Firm-quarters between 2007 and 2014 of all bank holding companies filing FRY9C reports in 4Q2014		36,096
- Firms without FVOL adoption in the sample period	-34,048	2,048
- Firms without SEC filings throughout sample period (10-Q/10-K)	-800	1,248
- Firms without information on FVOL adoption in SEC filings	-192	1,056
- Firms without price data on Thomson Reuters Datastream	-32	1.024
- Firm-quarters without FVOL adoption	-407	**617**

This table displays the sample selection process. "Firms without FVOL adoption" are firms that never filed a non-zero entry for item "F553: Net gains (losses) on liabilities [elected to account for under a fair value option]" in their FRY9C reports between 2008 and 2014 (the item in question was not part of the FRY9C report in 2007). I determine "Firm-quarters without FVOL adoption" by analyzing hand-collected SEC filings (10-Q/10-K).

3.6 Results

3.6.1 Descriptive Statistics

Table 3.3 shows descriptive statistics for the regression inputs. Panel A displays descriptive statistics for the unscaled inputs. The mean quarterly share return in the sample is 0.68% (median: 0.97%). Quarterly earnings are positive on average with a mean of $536 million (median: $22.8 million). DVAs are negative on average with a mean of -$10.4 million (median: $0.00 million). The lowest DVA in the sample is -$3.60 billion and was recognized by Bank of America Corp. in the second quarter of 2009. The largest DVA in the sample is $4.51 billion and was also recognized by Bank of America Corp. in the third quarter of 2011. The other comprehensive income in the sample has a mean of -$20.2 million (median: $0.3 million). The proportion of assets that firms measured at fair value is 25.63% on average (median: 22.51%). The mean proportion of level 1 fair value assets is 4.48% (median: 1.09%). The minimum is 0.00% indicating that in some quarters, some firms held no level 1 fair valued assets. The maximum is 27.30%. For level 2 fair valued assets, the mean proportion throughout the sample period is 19.53% (median: 18.94%). The average proportion of assets that sample firms carry at fair value level 3 is 1.62% (median: 0.83%). Panel B, Panel C, and Panel D show descriptive statistics for variables scaled by lagged market value of equity, for variables scaled by lagged total assets, and of regression inputs of the sensitivity tests respectively.

Table 3.3 Descriptive Statistics

Panel A: Descriptive statistics on regression inputs (unscaled)						
	Mean	Standard Dev	Min	Median	Max	N
R	0.0068	0.2161	-0.7801	0.0097	0.9923	617
E	0.5364	1.6249	-9.8330	0.0228	6.3130	617
E_excl_DVA	0.5468	1.6927	-10.2760	0.0232	6.8240	617
DVA	-0.0104	0.4919	-3.6000	0.0000	4.5060	617
FVA	0.2563	0.1461	0.0253	0.2251	0.8046	617
FVA1	0.0448	0.0632	0.0000	0.0109	0.2730	617
FVA2	0.1953	0.1037	0.0002	0.1894	0.5571	617
FVA3	0.0162	0.0195	0.0000	0.0083	0.1702	617
UIA	-3.9100	25.4036	-155.0156	0.0186	130.6114	617
OCI	-0.0202	0.9207	-11.1940	0.0003	5.3700	617
LEV	0.8982	0.0240	0.8255	0.9009	0.9703	617
SIZE	1.2636	2.5005	-3.9065	0.7709	5.5362	617
Panel B: Descriptive statistics on regression inputs scaled by lagged market value of equity						
E	0.0017	0.2300	-2.5736	0.0173	4.4116	617
E_excl_DVA	0.0025	0.2278	-2.5480	0.0176	4.3446	617
DVA	-0.0008	0.0121	-0.1153	0.0000	0.0768	617
UIA	-0.1747	1.0719	-16.5628	0.0207	0.9898	617
OCI	-0.0001	0.0397	-0.6731	0.0003	0.3850	617
Panel C: Descriptive statistics on regression inputs scaled by lagged total assets						
E	0.0010	0.0039	-0.0416	0.0019	0.0195	617
E_excl_DVA	0.0010	0.0039	-0.0416	0.0019	0.0192	617
DVA	-0.0000	0.0004	-0.0037	0.0000	0.0041	617
UIA	0.0096	0.0491	-0.2878	0.0017	0.2214	617
OCI	-0.0000	0.0017	-0.0086	0.0000	0.0180	617
Panel D: Descriptive statistics on regression inputs of the sensitivity tests						
GOV41	0.6932	0.0942	0.4390	0.7317	0.8293	611
QCReturn	-0.0003	0.0037	-0.0230	0.0001	0.0105	617

Panel A of this table provides descriptive statistics on regression inputs if unscaled. Panel B and C provide descriptive statistics on regression inputs scaled by lagged market value of equity and scaled by lagged total assets respectively. Panel D provides descriptive statistics on regression inputs of the sensitivity tests. For variable definitions, see Table 3.1.

Table 3.4 displays the correlation coefficients of the regression inputs. Panel A displays the correlation coefficients for unscaled inputs. Quarterly share returns are positively associated with quarterly net income and with quarterly other comprehensive income but negatively associated with DVAs. Apart from that, DVAs are not significantly associated with other variables except for a negative association with net income excluding DVAs and with OCI. Panel B, Panel C, and Panel D show regression coefficients for variables scaled by lagged market value of equity, for variables scaled by lagged total assets, and for regression inputs of the sensitivity tests respectively.

Table 3.4 Correlation Coefficients

Panel A: Correlation coefficients of regression inputs (unscaled)

N=617	R	E	E_excl_DVA	DVA	FVA	FVA1	FVA23	FVOL	OCI	LEV	SIZE
R	1										
E	**0.109**	1									
E_excl_DVA	**0.204**	**0.957**	1								
DVA	**-0.342**	0.011	**-0.280**	1							
FVA	0.035	**0.362**	**0.350**	-0.009	1						
FVA1	0.020	**0.368**	**0.359**	-0.020	**0.701**	1					
FVA23	0.035	**0.266**	**0.255**	-0.001	**0.915**	**0.352**	1				
FVOL	0.029	**0.308**	**0.312**	-0.056	**0.745**	**0.574**	**0.652**	1			
OCI	**0.148**	**0.106**	**0.177**	**-0.258**	-0.012	-0.005	-0.013	-0.047	1		
LEV	-0.050	**0.140**	**0.117**	0.057	**0.372**	**0.409**	**0.256**	**0.353**	-0.039	1	
SIZE	**0.094**	**0.452**	**0.445**	-0.036	**0.619**	**0.600**	**0.472**	**0.616**	-0.014	**0.195**	1

Panel B: Correlation coefficients of regression inputs scaled by lagged market value of equity

N=617	R	E	E_excl_DVA	DVA	FVA	FVA1	FVA23	FVA12	FVA3	UIA	OCI	LEV	SIZE
R	1												
E	0.044	1											
E_excl_DVA	0.061	**0.999**	1										
DVA	**-0.320**	**0.207**	**0.155**	1									
FVA	0.036	0.044	0.048	-0.064	1								
FVA1	0.020	0.042	0.047	**-0.083**	**0.701**	1							
FVA23	0.036	0.034	0.037	-0.036	**0.914**	**0.352**	1						
FVA12	0.036	0.045	0.048	-0.057	**0.993**	**0.684**	**0.914**	1					
FVA3	0.017	0.019	0.024	**-0.079**	**0.563**	**0.475**	**0.469**	**0.461**	1				
UIA	0.065	-0.010	-0.015	**0.092**	0.072	-0.007	**0.099**	**0.081**	-0.022	1			
OCI	**0.121**	**0.293**	**0.303**	**-0.143**	0.016	-0.001	0.022	0.015	0.020	**0.322**	1		
LEV	-0.050	-0.073	-0.073	-0.008	**0.371**	**0.409**	**0.255**	**0.350**	**0.340**	0.020	-0.036	1	
SIZE	**0.094**	**0.100**	**0.105**	**-0.080**	**0.620**	**0.600**	**0.472**	**0.600**	**0.457**	**0.113**	0.026	**0.195**	1

Table 3.4 (continued)

Panel C: Correlation coefficients of regression inputs scaled by lagged total assets

N=617	R	E	E_excl_DVA	DVA	FVA	FVA1	FVA23	FVA12	FVA3	UIA	OCI	LEV	SIZE
R	1												
E	**0.107**	1											
E_excl_DVA	**0.140**	**0.994**	1										
DVA	**-0.315**	0.023	**-0.083**	1									
FVA	0.036	**0.118**	**0.120**	-0.030	1								
FVA1	0.020	**0.083**	**0.089**	-0.056	**0.701**	1							
FVA23	0.036	**0.107**	**0.108**	-0.007	**0.914**	**0.352**	1						
FVA12	0.036	**0.131**	**0.134**	-0.032	**0.993**	**0.684**	**0.914**	1					
FVA3	0.017	-0.030	-0.030	0.002	**0.563**	**0.475**	**0.469**	**0.461**	1				
UIA	**0.092**	**0.228**	**0.225**	0.030	-0.070	-0.028	-0.076	-0.050	**-0.178**	1			
OCI	0.065	**-0.164**	**-0.162**	-0.022	-0.005	-0.008	-0.002	-0.001	-0.029	0.008	1		
LEV	-0.050	-0.068	-0.072	0.043	**0.371**	**0.409**	**0.255**	**0.350**	**0.340**	**0.201**	-0.033	1	
SIZE	**0.094**	**0.159**	**0.162**	-0.036	**0.620**	**0.600**	**0.472**	**0.600**	**0.457**	**-0.081**	-0.039	**0.195**	1

Panel D: Correlation coefficients of regression inputs of sensitivity tests

N=617	R	CR	E_excl_DVA	DVA	High FVA1	High FVA12	High FVA1_75	High FVA	GOV41 (N=611)	UIA	OCI	LEV	SIZE
R	1												
CR	**0.966**	1											
E_excl_DVA	0.061	**0.093**	1										
DVA	**-0.320**	**-0.293**	**0.155**	1									
High FVA1	0.017	-0.004	0.028	-0.069	1								
High FVA12	0.000	0.023	-0.020	0.076	**-0.280**	1							
High FVA1_75	0.025	0.014	0.029	-0.071	**0.580**	**-0.199**	1						
High FVA	0.029	0.045	0.047	-0.075	**0.238**	-0.037	**0.347**	1					
GOV41 (N=611)	0.027	0.020	0.043	-0.065	**0.322**	**-0.309**	**0.323**	**0.343**	1				
UIA	0.065	**0.127**	-0.015	**0.092**	-0.074	0.044	**-0.114**	0.073	-0.077	1			
OCI	**0.121**	**0.109**	**0.303**	**-0.143**	-0.020	-0.025	-0.033	0.048	0.011	**0.322**	1		
LEV	-0.050	-0.078	-0.073	-0.008	**0.277**	**-0.190**	**0.317**	**0.261**	**-0.154**	0.020	-0.036	1	
SIZE	**0.094**	**0.111**	**0.105**	**-0.080**	**0.514**	**-0.399**	**0.428**	**0.453**	**0.582**	**0.113**	0.026	**0.195**	1

Panel A of this table provides correlation coefficients on regression inputs if unscaled. Panel B and C provide correlation coefficients on regression inputs scaled by lagged market value of equity and scaled by lagged total assets respectively. Panel D provides correlation coefficients on regression inputs of the sensitivity tests. Bold letters indicate significance at the 10%-level. For variable definitions, see Table 3.1.

3.6.2 Value relevance tests

Table 3.5 shows the results of the multivariate return regression analysis that tests for DVAs' value relevance. In Model 1, earnings (including DVAs) are not significantly associated with returns. Splitting earnings in DVAs and the remaining components of earnings (Model 2), I find that the coefficient on *DVA* is significantly negative. This is consistent with DVAs not being value relevant for investors in the sample, in line with findings of Cedergren et al. (2015). While this stands in contrast to the findings of Chung et al. (2012), their sample differs from mine in several aspects, for example, their sample period goes only until 2010. In Model 3, I test whether the proportion of corresponding assets measured at fair value mediates the value relevance of DVAs. As the coefficient on the variable that tests for this association (*DVA*FVA*) is insignificant, I conclude that this is not the case. Additionally, an F-Test for the coefficients on *DVA* and *DVA*FVA* shows that the coefficients are not jointly significant (p-value: 0.8200). In conclusion, while Fontes et al. (2014) demonstrate that a higher proportion of fair value assets is generally able to mediate the information asymmetry from DVAs among investors, I do not find that a higher proportion of fair value assets similarly improves investors' perception of DVAs concerning DVAs' value relevance. Among other reasons, one explanation could be the different samples between their and my study. Especially, Fontes et al. 2014 conduct their tests for a sample of European banks, not US banks.

Concerning my main tests, I find that the coefficient on *DVA*FVA1* in Model 4 is significantly positive. Additionally, an F-test shows joint significance for *DVA* and *DVA*FVA1* (p-value: 0.0000). This finding suggests an increasing DVA value relevance in the presence of a higher proportion of corresponding level 1 fair value assets. The finding is consistent with investors being able to better assess the wealth transfers between shareholders and debtholders due to changes in credit risk (and therewith: DVAs), when the corresponding assets provide *reliable* information on the change in credit risk. In contrast, the coefficient on *DVA*FVA23* is negative. An F-test shows joint significance of the coefficients on *DVA* and *DVA*FVA23* (p-value: 0.0885). This finding suggests that a higher proportion of level 2 and 3 fair values do not increase DVAs' value relevance. The finding is consistent with the notion that investors do not price the wealth transfer between shareholders and debtholders due to a change in credit risk (as reflected by DVAs), when the corresponding assets related to the change in credit risk are recognized on the balance sheet but not measured reliably. In Model

5, the coefficients on *FVA12* and *FVA3* are insignificant and not jointly significant with *DVA*. This implies that the found association between DVAs' value relevance and fair values' reliability from Model 4 does not hold when classifying level 2 assets as reliable. In the light of findings from Hitz (2007) and Song et al. (2010), this is somewhat surprising. A potential explanation could lie in the relatively low proportions of level 3 fair value assets that firms hold in my sample. The descriptive statistics from Table 3.3 show that the average proportion of held level 3 fair value assets (1.62%) is notably smaller than the proportions of level 1 (4.48%) and level 2 fair value assets (19.53%). This finding could be indicative of a mutual understanding of investors and managers that level 3 fair value measures are not reliable and thus managers waive from using level 3 measures due to expected negative effects. As the amounts of firms' level 3 fair value assets are consequently low, they are relatively less likely to reflect the changes in credit risk that underlie DVAs.

Throughout the tests, the coefficient on *DVA*UIA* is significantly negative. This is consistent with the finding of Cedergren et al. (2015) that DVAs are more value relevant when the level of related unrecognized intangible assets is low. However, in contrast to their findings, I do not find value relevance for earnings throughout the models. Also, unlike them, I do not find DVAs to be value relevant in Model 2, the model which most closely follows Cedergren et al. 2015. The differences are potentially due to the fact that both studies' samples are relatively small as adoption of the fair value option for liabilities is not widespread (Guthrie et al. 2011). Therefore, the small differences in variable measures and in the sample selection between their study and my study might lead to the differing results. To highlight one difference in variable measures: Cedergren et al. (2015) include DVAs in their sample that are only recognized in regulatory reports (FRY9C reports) but not in annual and quarterly reports (10-Q and 10-K filings). In annual and quarterly reports, firms need to disclose only "significant" DVAs (FASB 2007). Therefore, a number of firms disclose DVAs only in regulatory reports as anecdotal evidence in the course of my data collection suggests. Contacting investor relations services of such firms, I receive confirmation that this is indeed a matter of materiality. I do not include such DVAs in my sample as they are possibly not compliant with US GAAP regulation and "not audited or reviewed by external auditors" (Cedergren et al. 2015). However, the higher relevance of DVAs that the findings of Cedergren et al. (2015) reflect in comparison to my findings could potentially be driven by their inclusion of such DVAs. This would imply that investors, on average, price DVAs even

when they are not recognized in annual and quarterly reports, consistent with the findings of Barth et al. 2008.

In sum, my findings add to the findings of Cedergren et al. 2015. Their study shows that investors do not price DVAs when the sources of the change in credit risk are clouded by a large amount of unrecognized intangible assets. My findings additionally show that even when the corresponding assets are recognized at fair value on the balance sheet, investors demand high reliability of these assets' fair values to perceive them as useful for their assessment of the value relevance of DVAs.

Table 3.5 Value Relevance Test

Dependent variable: Quarterly Share Return R_t					
	Model 1	Model 2	Model 3	Model 4	Model 5
E	-0.0059 (-0.29)				
E_excl_DVA		-0.0057 (-0.09)	-0.0689 (-0.65)	-0.0857 (-1.25)	-0.0030 (-0.05)
DVA		-7.6002*** (-3.38)	-13.5498 (-1.47)	-5.5998 (-0.70)	-10.5727 (-1.05)
DVA*FVA			16.8740 (0.72)		
DVA*FVA1				51.4527*** (6.93)	
DVA*FVA23				-32.3260 (-1.08)	
DVA*FVA12					1.6034 (0.05)
DVA*FVA3					43.5183 (0.94)
FVA			-0.0714 (-0.72)		
FVA1				-0.0822 (-0.49)	
FVA23				0.0030 (0.06)	
FVA12					-0.0216 (-0.21)
FVA3					-0.2095 (-0.22)

Table 3.5 (continued)

DVA*UIA		-0.9430*	-1.9121*	-1.4215***	-1.1703**
		(-1.72)	(-1.75)	(-4.17)	(-2.21)
UIA		0.0177*	0.0290*	0.0235**	0.0176
		(1.76)	(1.94)	(2.34)	(1.60)
OCI	0.6417*	0.1216	-0.2351	-0.0262	0.1176
	(1.89)	(0.40)	(-0.41)	(-0.09)	(0.38)
LEV	-0.6033*	-0.5476	-0.6792*	-0.4596	-0.4574
	(-1.68)	(-1.47)	(-1.78)	(-0.94)	(-1.43)
SIZE	0.0090**	0.0059	0.0062*	0.0073**	0.0069*
	(2.08)	(1.59)	(1.89)	(2.06)	(1.90)
Constant	0.5373*	0.4891	0.6176*	0.4128	0.4151
	(1.73)	(1.53)	(1.95)	(1.00)	(1.61)
F-Tests (two-sided):					
DVA+DVA*UIA		0.0016	0.1179	0.3879	0.2565
DVA+DVA*FVA			0.8200		
DVA+DVA*FVA1				0.0000	
DVA+DVA*FVA23				0.0885	
DVA+DVA*FVA12					0.7081
DVA+DVA*FVA3					0.4987
Adj. R^2	0.0208	0.1295	0.1322	0.1518	0.1274
N	617	617	617	617	617

This table displays coefficient estimates from an OLS model. The underlying regression model is (Model 5):

$$R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA12_{it} + \beta_4 DVA_{it} * FVA3_{it} + \beta_5 FVA12_{it} + \beta_6 FVA3_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it}$$

For all variable definitions, see Table 1. *E, E_excl_DVA, DVA, OCI,* and *UIA* are scaled by lagged market value of equity. The regression models have standard errors that are heteroscedasticity robust and clustered at firm and quarter-year level. t-values are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

3.6.3 Market pricing tests

Table 3.6 displays the results of the tests for DVAs' market pricing. In Model 1, I find the coefficient on *E* to be insignificant. Accordingly, earnings in period t are not associated with returns in subsequent periods, suggesting that markets price earnings (including DVAs) efficiently at their announcement. In Model 2, I disaggregate earnings in DVAs and its remaining components. Here, the coefficient on DVAs is also insignificant suggesting that financial markets price DVAs efficiently in general at their announcement. This potentially stands in contrast to concerns from the public DVA debate that DVAs blur investors' view on firm performance. In Model 3, I additionally control for firms' proportion of fair value assets. Here, the coefficient on *DVA* is significantly negative. This suggests that DVA information is not fully incorporated into share prices after its announcement. Instead, the DVAs also

negatively affect share returns in the following year. This is consistent with financial markets overreacting to the announcements of DVAs in financial reports under certain circumstances.

To shed more light on this finding, I further disaggregate the variable *FVA* in Model 4 into the proportion of assets carried at level 1 fair values (*FVA1*) and carried at level 2 and 3 fair values (*FVA23*). The significantly positive coefficient on *FVA23* implies that financial markets underreact to DVAs when large proportions of assets are measured at level 2 and 3 fair values. This is consistent with a too conservative pricing of DVAs by investors when the sources of the underlying change in credit risk are not reliably measured and displayed. At the same time, the coefficient on *DVA*FVA1* is insignificant. This suggests that the proportion of reliably measured fair value assets has no association with investors' market pricing of DVAs. The coefficients on the test variable in Model 5 confirm the findings of Model 4, but with a lower level of significance. This is consistent with level 1 and level 2 fair values having less similar properties in the context of this test than level 2 and level 3 fair values. As in Section 3.6.2, I caution the reader to consider the overall low proportion of level 3 fair value assets in the sample as an additional explanation for the lower significance of the coefficient on *DVA*FVA3* compared to *DVA*FVA23*.

In conclusion, the tests of DVAs' market pricing suggest that financial markets generally price DVAs efficiently at their announcement in financial statements. However, when sources of the underlying change in credit risk are not transparent on the balance sheet, specifically, when the proportion of fair value assets on the balance sheet is small or largely consists of level 2 and 3 fair value assets, financial markets tend to overreact respectively underreact to DVAs initially and later correct their pricing. In the summary of FAS 159, the FASB states that the standard's objective is to improve financial reporting by enabling firms to mitigate volatility caused by measuring related assets and liabilities differently. My findings stress the importance of an application of the FVOL in accordance with FAS 159 for investors to be able to efficiently price DVAs. Specifically, the desired positive financial markets effects from the application of the option seem to be conditional on a thorough and reliable measurement of related fair value assets.

Table 3.6 Market Pricing Test

Dependent variable: Future Returns R_{t+4}					
	Model 1	Model 2	Model 3	Model 4	Model 5
E	0.0483				
	(1.07)				
E_excl_DVA		0.0490	0.2416***	0.1574	0.0622
		(0.44)	(3.01)	(1.55)	(0.53)
DVA		1.5790	-10.8050*	-20.8923***	-11.4509**
		(1.31)	(-1.96)	(-3.63)	(-2.27)
DVA*FVA			24.0005		
			(1.36)		
DVA*FVA1				-6.1090	
				(-0.97)	
DVA*FVA23				84.4136***	
				(3.70)	
DVA*FVA12					24.1461
					(1.12)
DVA*FVA3					81.7636*
					(1.81)
FVA			0.2273		
			(1.22)		
FVA1				0.1877	
				(0.48)	
FVA23				0.2188	
				(1.47)	
FVA12					0.2218
					(1.52)
FVA3					0.2759
					(0.21)
DVA*UIA		0.2719	1.2579	-0.2874	-0.5129
		(0.35)	(1.33)	(-0.99)	(-1.26)
UIA		-0.0207	-0.0428	-0.0279	-0.0205
		(-0.92)	(-1.54)	(-1.25)	(-0.94)
OCI	0.6332*	0.8881*	1.6022**	0.9934**	0.8471*
	(1.71)	(1.87)	(2.05)	(2.02)	(1.72)
LEV	-1.7583*	-1.7652*	-1.8159*	-2.0051*	-2.0599**
	(-1.96)	(-1.88)	(-1.71)	(-1.75)	(-1.97)
SIZE	0.0055	0.0068	-0.0007	-0.0007	-0.0013
	(0.77)	(0.91)	(-0.09)	(-0.10)	(-0.16)
Constant	1.5813**	1.5831*	1.5811*	1.7523*	1.8001**
	(1.99)	(1.90)	(1.70)	(1.75)	(1.98)

Table 3.6 (continued)

F-Tests (two-sided):					
DVA+DVA*UIA		0.2516	0.1221	0.0001	0.0059
DVA+DVA*FVA			0.2773		
DVA+DVA*FVA1				0.0001	
DVA+DVA*FVA23				0.0002	
DVA+DVA*FVA12					0.4611
DVA+DVA*FVA3					0.1237
Adj. R^2	0.0224	0.0230	0.0423	0.0443	0.0319
N	617	617	617	617	617

This table displays coefficient estimates from an OLS model. The underlying regression model is (Model 5):

$$R_{it+4} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA12_{it} + \beta_4 DVA_{it} * FVA3_{it} + \beta_5 FVA12_{it} + \beta_6 FVA3_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it}$$

For all variable definitions, see Table 3.1. *E*, *E_excl_DVA*, *DVA*, *OCI*, and *UIA* are scaled by lagged market value of equity. The regression models have standard errors that are heteroscedasticity robust and clustered at firm and quarter-year level. t-values are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

3.6.4 Persistence tests

Table 3.7 displays the results of the tests of DVAs' persistence. In Model 1, the coefficient on earnings is positive and significant. This is consistent with earnings (including DVAs) being persistent, i.e., being associated with future earnings. The coefficient is significantly different from 1 (p-value: 0.0000, untabulated), which indicates that earnings are not fully persistent in the sample but instead revert to the mean over time (Chen et al. 2011). Separating DVAs from earnings in Model 2 results in an insignificant negative coefficient on *DVA*. This is consistent with DVAs not being persistent. In Model 3, the coefficient on *DVA* is significantly negative stressing DVAs' lack of persistence. Instead, positive (negative) DVAs seem to indicate smaller (higher) earnings one year later. This is likely due to the fact that changes in the value of a firm's credit risk are not only subject to firm's actions, for example, its management and its credit risk politics, but are also influenced by factors beyond their control, for example, the economy. I test whether DVAs' lack of persistence is moderated by the proportion of assets that firms measure at fair value. The significantly positive coefficient on *DVA*FVA* and the F-test for joint significance of the coefficients on *DVA*FVA* and *DVA* (p-value: 0.0014) indicate that this is the case.

In Model 4, I additionally test whether the level of fair value assets moderates the association found in Model 3. The significantly positive coefficient on *DVA*FVA23* implies that the results from Model 3 are driven by level 2 and level 3 fair values, i.e., DVAs' persistence is higher when a larger proportion of assets is carried at level 2 and 3 fair values. In Model 5, the significant positive coefficient on *DVA*FVA12* and the insignificant

coefficient on *DVA*FVA3* suggest that the findings from Model 4 are mainly attributable to level 2 fair value assets. Among other potential explanations, DVAs' increased persistence in the presence of large proportions of level 2 fair value assets could be a result of higher managerial discretion regarding earnings (Dechow et al. 2010) and is a potential reason for the demonstrated lower value relevance of DVAs under these circumstances. However, if managerial discretion was a main driver for DVAs' persistence, one would also expect to find a significantly positive association between the persistence and large proportions of level 3 fair values, as level 3 fair values' measurement allows for the largest managerial discretion (e.g., Song et al. 2010). However, I do not find such an association. A possible explanation for this could be that proportions of level 3 fair values on average are low within in the sample. This could limit managers' scope for exercising discretion concerning level 3 fair values and might shift their focus on level 2 fair values in this regard instead.

Table 3.7 Persistence Test

Dependent variable: Future Earnings E_{t+4}					
	Model 1	Model 2	Model 3	Model 4	Model 5
E	0.1975***				
	(4.12)				
E_excl_DVA		0.1576***	0.1565***	0.1552***	0.1521***
		(2.89)	(3.20)	(3.03)	(3.18)
DVA		-0.6334	-3.5883***	-3.8701***	-3.7637***
		(-1.32)	(-2.82)	(-3.22)	(-2.85)
DVA*FVA			6.8498***		
			(2.97)		
DVA*FVA1				5.6486	
				(1.47)	
DVA*FVA23				8.8878***	
				(3.37)	
DVA*FVA12					7.7474***
					(2.88)
DVA*FVA3					6.6312
					(1.03)
FVA			0.0036*		
			(1.78)		
FVA1				0.0020	
				(0.67)	
FVA23				0.0023	
				(1.43)	

Table 3.7 (continued)

FVA12					0.0027 (1.53)
FVA3					-0.0057 (-0.41)
DVA*UIA		-14.9985 (-1.12)	-21.2597** (-2.18)	-22.3225** (-2.26)	-21.7197* (-1.80)
UIA		0.0112** (2.23)	0.0113** (2.21)	0.0120** (2.28)	0.0114** (2.12)
OCI	0.2353 (1.54)	0.2164 (1.43)	0.2170 (1.42)	0.2157 (1.40)	0.2127 (1.41)
LEV	-0.0095 (-1.23)	-0.0148** (-1.99)	-0.0158** (-2.02)	-0.0183** (-2.16)	-0.0171* (-1.87)
SIZE	0.0002* (1.80)	0.0002** (2.13)	0.0001 (1.29)	0.0001 (1.37)	0.0002 (1.38)
Constant	0.0092 (1.35)	0.0138** (2.11)	0.0141** (2.08)	0.0165** (2.26)	0.0155* (1.94)
F-Tests (two-sided):					
DVA+DVA*UIA		0.2607	0.0176	0.0144	0.0456
DVA+DVA*FVA			0.0014		
DVA+DVA*FVA1				0.5719	
DVA+DVA*FVA23				0.0029	
DVA+DVA*FVA12					0.0105
DVA+DVA*FVA3					0.6076
Adj. R^2	0.0578	0.0733	0.0807	0.0762	0.0771
N	617	617	617	617	617

This table displays coefficient estimates from an OLS model. The underlying regression model is (Model 5):

$$E_{it+4} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA12_{it} + \beta_4 DVA_{it} * FVA3_{it} + \beta_5 FVA12_{it} + \beta_6 FVA3_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it}$$

For all variable definitions, see Table 3.1. *E*, *E_excl_DVA*, *DVA*, *OCI*, and *UIA* are scaled by lagged total assets. The regression models have standard errors that are heteroscedasticity robust and clustered at firm and quarter-year level. t-values are reported in parentheses. ***, **, and * indicate significance at the 10%, 5%, and 1% level, respectively.

3.7 Sensitivity tests

I conduct several sensitivity tests to validate the robustness of my findings. I hereby focus on the tests of DVAs' value relevance as my main contribution to prior literature. For a first sensitivity test, I use several binary variables and their interactions as an alternative proxy for the proportion and reliability of fair value assets. Specifically, I use the following model:

$$R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA * HighFVA * HighFVA1 + \beta_4 DVA_{it} * HighFVA1_{it} + \beta_5 HighFVA_{it} * HighFVA1_{it} + \beta_6 HighFVA1_{it} + \beta_7 HighDVA * DVA_{it} + \beta_8 HighDVA_{it} + \beta_9 DVA_{it} * UIA_{it} + \beta_{10} UIA_{it} + \beta_{11} OCI_{it} + \beta_{12} LEV_{it} + \beta_{13} SIZE_{it} + \varepsilon_{it} \quad (7)$$

where *HighFVA* is a binary variable indicating a ratio of fair value assets to total assets above the sample's median value and *HighFVA1* is a binary variable indicating a ratio of level 1 fair value assets to level 2 and 3 fair value assets above the sample's median value. Table 3.8 displays the results. The findings from Model 1 do not support my findings from the main specification as the main test variable *DVA*HighFVA*HighFVA1* is not significantly different from zero. This implies that in this specification, a high proportion of fair value assets in combination with a high proportion of such fair value assets being measured reliably do not improve the value relevance of DVAs. In Model 2, I employ the same model, but instead use a dummy that reflects an above median ratio of level 1 and 2 fair values relative to level 3 fair values (*HighFVA12*). Again, I do not find confirmation for my main tests. In Model 3, I further explore the reasons for the differences between these sensitivity tests and my main tests. For this, I rerun Model 1 with a dummy that indicates a ratio of level 1 fair value assets to level 2 and 3 fair value assets that lies above the 75%-quantile of the sample. I find that the coefficient on *DVA*HighFVA*HighFVA1_75* is significantly positive. Furthermore the joint coefficient on *DVA+DVA*HighFVA+DVA*HighFVA1_75 +DVA*HighFVA*HighFVA1_75* is significantly positive (p-value: 0.0000) while the coefficient on *DVA* is significantly negative. This adds to my findings from the main test. Specifically, this finding is consistent with the notion that investors perceive DVAs as especially value relevant if the proportion of a firms' related level 1 fair value assets is in the highest 25%-quantile.[15]

In my next sensitivity test, I analyze an alternative measure of fair values assets' reliability. Song et al. 2010 find that investors perceive a firm's fair value assets' valuation as more reliable when the firm has strong means of corporate governance. In conclusion, corporate governance could proxy for fair value assets' reliability and therefore could mediate DVAs' value relevance. To test for this, I include an interaction term for firms' level of

[15] I do not employ an according test with a binary variable indicating a value above the 75%-quantile of FVA12/FVA3. This is because such a split would result in an interaction term *DVA*HighFVA*HighFVA12_75* that assumes only values of zero.

corporate governance with DVAs and with the proportion of fair value assets. Specifically, I use the following model:

$$\begin{aligned} R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA_{it} * GOV41_{it} + \beta_4 DVA_{it} * FVA_{it} \\ + \beta_5 DVA_{it} * GOV41_{it} + \beta_6 FVA_{it} * GOV41_{it} + \beta_7 GOV41_{it} + \beta_8 FVA_{it} \\ + \beta_9 DVA_{it} * UIA_{it} + \beta_{10} UIA_{it} + \beta_{11} OCI_{it} + \beta_{12} LEV_{it} + \beta_{13} SIZE_{it} + \varepsilon_{it} \end{aligned} \quad (8)$$

where *GOV41* is the GOV_{41}-Index from Aggarwal et al. 2011[16] and *FVA* is the proportion of assets measured at fair value. Table 3.8, Model 4 displays the test results. The coefficient on *DVA*FVA*GOV41* is positive. This is consistent with DVAs' being more value relevant for investors if a high proportion of assets is carried at fair value and the firm's strong corporate governance implies that these assets' fair values are reliable. The coefficient on *DVA*FVA* is negative which is in line with a high proportion of fair values assets possibly being detrimental to investors' perception of DVAs, potentially because the overall average reliability of such fair values is low. The coefficient on *DVA*GOV41* is not positive, suggesting that in my sample, DVAs' value relevance is on average not higher for firms with higher corporate governance. However, none of the three coefficients is significantly different from zero. Therefore, this sensitivity test does not provide profound evidence to support the findings from my main tests.

[16] Because this measure is unavailable for two banks in my sample, the sample size for this test is reduced by six firm-quarters.

Table 3.8 Sensitivity Tests with Ratio-Dummies and with Alternative Reliability Measure

Dependent variable:	R_t	R_t	R_t	R_t
	Model 1	Model 2	Model 3	Model 4
Model specification:	FVA1	FVA12	FVA1_75	Alt. Rel.Measure
E_excl_DVA	-0.0037	0.0155	-0.0122	-0.1213***
	(-0.06)	(0.25)	(-0.20)	(-2.59)
DVA	-12.6906**	-10.1497***	-10.9822***	240.4261
	(-2.27)	(-10.88)	(-6.19)	(1.45)
DVA*HighFVA*HighFVA1	-8.0345			
	(-0.69)			
DVA*HighFVA*HighFVA12		1.8247		
		(0.36)		
DVA*HighFVA*HighFVA1_75			18.2593***	
			(3.60)	
DVA*FVA*GOV41				986.9653
				(1.31)
DVA*HighFVA1	4.6853			
	(0.47)			
HighFVA*HighFVA1	-0.0479**			
	(-2.13)			
HighFVA1	0.0049			
	(0.51)			
DVA*HighFVA12		0.1091		
		(0.04)		
HighFVA*HighFVA12		0.0179		
		(0.50)		
HighFVA12		0.0146		
		(0.70)		
DVA*HighFVA1_75			-11.2783**	
			(-2.00)	
HighFVA*HighFVA1_75			-0.0134	
			(-0.61)	
HighFVA1_75			0.0083	
			(0.24)	
HighFVA*DVA	8.4601	2.5754	-3.2415***	
	(1.08)	(1.15)	(-3.82)	
HighFVA	0.0093	-0.0243	-0.0061	
	(0.47)	(-0.71)	(-0.34)	
DVA*FVA				-693.3175
				(-1.26)

Table 3.8 (continued)

DVA*GOV41				-354.7850
				(-1.57)
FVA*GOV41				-0.6110*
				(-1.95)
FVA				0.3788***
				(4.08)
GOV41				-0.0073
				(-0.04)
DVA*UIA	-0.9577*	-0.9982	-2.3682***	-1.2114
	(-1.70)	(-1.44)	(-4.84)	(-1.17)
UIA	0.0155	0.0152**	0.0177	0.0259**
	(1.37)	(2.45)	(1.32)	(2.09)
OCI	0.1243	0.1591	0.1019	-0.1395
	(0.35)	(0.60)	(0.25)	(-0.34)
LEV	-0.4024	-0.3635	-0.4495	-0.6439
	(-1.12)	(-1.49)	(-1.04)	(-1.13)
SIZE	0.0107**	0.0091*	0.0074*	0.0124*
	(2.14)	(1.83)	(1.80)	(1.69)
Constant	0.3601	0.3200	0.4039	0.5853
	(1.20)	(1.46)	(1.11)	(1.01)
F-Tests (two-sided):				
DVA+DVA*UIA	0.0098	0.0000	0.0000	0.1526
DVA*HighFVA*HighFVA1	0.0009			
DVA*HighFVA*HighFVA12		0.0000		
DVA*HighFVA*HighFVA1_75			0.0000	
DVA+DVA*FVA+DVA* GOV41+DVA*FVA*GOV41				0.2025
Adj. R^2	0.1261	0.1246	0.1397	0.1463
N	617	617	617	611

This table displays coefficient estimates from OLS models. The underlying regression models are
Model 1:

$$R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * HighFVA_{it} * HighFVA1_{it} + \beta_4 DVA_{it} * HighFVA1_{it} + \beta_5 HighFVA_{it} * HighFVA1_{it} + \beta_6 HighFVA1_{it} + \beta_7 HighFVA * DVA_{it} + \beta_8 HighFVA + \beta_9 DVA_{it} * UIA_{it} + \beta_9 UIA_{it} + \beta_{10} OCI_{it} + \beta_{11} LEV_{it} + \beta_{12} SIZE_{it} + \varepsilon_{it}$$

Model 2 and Model 3 are constructed accordingly with $HighFVA12$ respectively $HighFVA1\_75$

Model 4:

$$R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA_{it} * GOV41_{it} + \beta_4 DVA_{it} * FVA_{it} + \beta_5 DVA_{it} * GOV41_{it} + \beta_6 FVA_{it} * GOV41_{it} + \beta_7 FVA_{it} + \beta_8 GOV41_{it} + \beta_9 DVA_{it} * UIA_{it} + \beta_{10} UIA_{it} + \beta_{11} OCI_{it} + \beta_{12} LEV_{it} + \beta_{13} SIZE_{it} + \varepsilon_{it}$$

For all variable definitions, see Table 3.1. *E*, *E_excl_DVA*, *DVA*, *OCI*, and *UIA* are scaled by lagged market value of equity. The regression models have standard errors that are heteroscedasticity robust and clustered at firm and quarter-year level. t-values are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Next, I address the potential issue of possible correlated omitted variables. Specifically, my results could be driven by undetected firm characteristics that are associated with both, the proportion of reliable fair value assets and DVAs' value relevance. To remove this concern, I add firm fixed effects to my main tests (i.e., Equation (1) and (2)). I simultaneously control for potential correlated time trends that could affect my results by adding quarter-year fixed effects. Table 3.9, Model 1 and 2 display the results of the regression estimation. Sign and significance of all main test variables remain unaltered by the inclusion of the fixed effects.

In a final robustness test, I use quarterly share returns that are calculated as daily compounded returns instead of raw returns as dependent variable for the estimation of my main tests (Equation (1) and (2)). Table 3.9, Model 3 and 4 display the results. Here, too, all coefficients on my test variables remain unchanged. Taken together, the results from the sensitivity tests provide some additional support for the findings of my main tests.

Table 3.9 Sensitivity Tests with Fixed Effects and Daily Compounded Returns

Dependent variable:	R_t	R_t	CR_t	CR_t
	Model 1	Model 2	Model 3	Model 4
Model specification:	Incl. Fixed Effects	Incl. Fixed Effects	Comp. Returns	Comp. Returns
E_excl_DVA	-0.1855***	-0.1203	-0.0002	0.0010
	(-2.97)	(-1.62)	(-0.18)	(1.06)
DVA	-4.0794	-11.9062	-0.0923	-0.1711
	(-0.77)	(-1.27)	(-0.66)	(-1.12)
DVA*FVA1	46.0338***		0.7831***	
	(7.14)		(12.31)	
DVA*FVA23	-28.1997		-0.4848	
	(-1.62)		(-0.96)	
DVA*FVA12		22.3731		0.0375
		(0.73)		(0.08)
DVA*FVA3		-30.1589		0.6619
		(-1.04)		(0.89)
FVA1	0.0041		-0.0004	
	(0.02)		(-0.15)	
FVA23	-0.2453		-0.0001	
	(-1.13)		(-0.11)	
FVA12		-0.1910		0.0003
		(-1.04)		(0.18)
FVA3		-0.3197		-0.0134
		(-0.18)		(-0.74)

Table 3.9 (continued)

DVA*UIA	-1.2134***	-1.0509***	-0.0197***	-0.0161
	(-5.54)	(-2.87)	(-2.76)	(-1.60)
UIA	-0.0192	-0.0307**	0.0007***	0.0005**
	(-1.61)	(-2.58)	(3.29)	(2.50)
OCI	0.5592***	0.7409***	-0.0052	-0.0028
	(3.08)	(3.58)	(-0.69)	(-0.35)
LEV	0.1941	0.2735	-0.0130	-0.0111**
	(0.28)	(0.33)	(-1.57)	(-1.97)
SIZE	0.1533***	0.1682***	0.0001**	0.0001*
	(3.79)	(4.16)	(2.19)	(1.93)
Constant	-0.9317	-1.0864	0.0113	0.0096**
	(-1.35)	(-1.48)	(1.62)	(2.11)
F-Tests (two-sided):				
DVA+DVA*UIA	0.3285	0.1800	0.4367	0.2387
DVA+DVA*FVA1	0.0000		0.0000	
DVA+DVA*FVA23	0.0079		0.1176	
DVA+DVA*FVA12		0.6262		0.6942
DVA+DVA*FVA3		0.2243		0.5043
Adj. R^2	0.5159	0.4976	0.1546	0.1391
N	617	617	617	617

This table displays coefficient estimates of OLS models. The underlying regression models are:
Model 1:

$$R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA1_{it} + \beta_4 DVA_{it} * FVA23_{it} + \beta_5 FVA1_{it} + \beta_6 FVA23_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it}$$

Model 2:

$$R_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA12_{it} + \beta_4 DVA_{it} * FVA3_{it} + \beta_5 FVA12_{it} + \beta_6 FVA3_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it}$$

Model 3:

$$CR_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA1_{it} + \beta_4 DVA_{it} * FVA23_{it} + \beta_5 FVA1_{it} + \beta_6 FVA23_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it}$$

Model 4:

$$CR_{it} = \beta_0 + \beta_1 E\_excl\_DVA_{it} + \beta_2 DVA_{it} + \beta_3 DVA_{it} * FVA12_{it} + \beta_4 DVA_{it} * FVA3_{it} + \beta_5 FVA12_{it} + \beta_6 FVA3_{it} + \beta_7 DVA_{it} * UIA_{it} + \beta_8 UIA_{it} + \beta_9 OCI_{it} + \beta_{10} LEV_{it} + \beta_{11} SIZE_{it} + \varepsilon_{it}$$

For all variable definitions, see Table 3.1. E, *E_excl_DVA*, *DVA*, *OCI*, and *UIA* are scaled by lagged market value of equity. The regression models have standard errors that are heteroscedasticity robust and clustered at firm and quarter-year level. t-values are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

3.8 Conclusion

This study investigates the role of proportion and reliability of fair value assets on informational properties of DVAs. Based on an established return regression model, this study's findings suggest that investors perceive the controversial DVAs as a value relevant part of firms' net income if the sources of the underlying change in credit risk are made

transparent through a large proportion of reliably measured fair value assets. If, in contrast, a firm measures a large proportion of its assets at less reliable fair values, investors do not perceive DVAs as value relevant and tend to price them too conservatively. The findings further suggest that DVAs' persistence is moderated by the proportion and reliability of related fair valued assets.

The findings contribute to two streams of literature. First, they extend the relatively young research on the financial market effects of DVAs. Building on prior literature, my findings suggest that even when assets are recognized and carried at fair value, they might not provide sufficient information about credit risk changes for investors to perceive the consequential DVAs as value relevant. Instead, a second necessary condition is that these fair value assets are measured reliably. This adds to our understanding of investors' information demand regarding a part of income that critics describe as opaque and value irrelevant.

Second, the findings extend the literature on the role of fair value assets' reliability. Prior literature shows that fair value assets' reliability increases the value relevance of these assets (Song et al. 2010; Goh et al. 2015). Based on this, my findings suggest that the reliability of fair value assets' measurement also increases investors' perception of DVAs. Thereby, the finding improve our knowledge of the relevance of fair values assets' reliability in financial accounting. They further enhance our understanding of potential differences between investors' perception of level 1, level 2, and level 3 fair values.

However, my employed research setting does not allow for the identification of causal relations between the examined items. I therefore caution the reader to interpret my results as merely descriptive. For further limitations of this study, see Section 5.2. My findings create opportunities for future research. While the results show that investors benefit from a large proportion of reliably measured fair value assets for their assessment of DVAs, they do not allow for conclusions how exactly this information is processed by investors and what amount of transparency is required for an assessment of DVAs that benefits investors the most. Against the background of the ongoing DVA debate and the current changes in DVA accounting regulation, such research would be of interest.

4 "Some fuzzy math"- relational information on debt value adjustments by managers and the financial press

Sebastian Kaumanns[17]

Abstract: This study provides comprehensive descriptive evidence on managers' and the financial press' reporting of debt value adjustments due to a change in own credit risk (DVAs). The study is motivated by a public debate about DVAs in which critics name them "counterintuitive" and claim that managers disclose DVA information in a way that makes their firms "look good". Analyzing a sample of 353 firm-quarters of 15 US financial firms that report DVAs between 2007 and 2014, I find that managers provide more DVA relational information in firm-quarters with large negative DVAs compared to positive DVAs. Analyzing newspaper articles on 202 firm-quarters, I find that the financial press is more likely to cover DVAs on which managers provide more information. Examining the contents of the articles, I find that the press is more likely to provide new DVA information if managers' press releases contain little information. Finally, I find that the financial press often assumes a critical tone towards DVAs, especially when DVAs increase net income. The findings are in line with popular claims of asymmetric DVA reporting by managers. They also offer insights on the role of the financial press concerning the information of financial markets about controversial DVAs.

JEL codes: G21, M41

Keywords: Fair value option, Debt value adjustments, Own credit risk, ASC 825, Financial instruments

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4.1 Introduction

This study provides comprehensive descriptive evidence on relational information on debt value adjustments due to a change in credit risk (DVAs) as provided by managers and by the financial press. Under US GAAP and IFRS accounting regulation as applied to this day, DVAs cause net income gains when a firm's credit risk deteriorates and net income losses when a firm's credit risk improves. This characteristic has stirred an ongoing public debate. Critics perceive it as "counterintuitive" and call DVAs "some fuzzy math" (Dash 2009), "one of the more ridiculous concepts that's ever been invented in accounting" (Rapoport 2012), a "mess" (Tchir 2012) or an "abomination" (Keoun and Henry 2010). They state that DVAs' introduction in accounting was a result of lobbying efforts by big banks (Keoun 2008) that "were looking for ways to find profits" (Rice 2012). Moreover, critics accuse managers of asymmetric DVA reporting, i.e., highlight DVA losses but downplay DVA gains, in an attempt "to trick the media and investors" (Milstead 2012). Potentially incentivized by the debate, the IASB shifted DVAs' recognition from net income to other comprehensive income for future periods (IASB 2014a). The FASB, acknowledging that DVAs are controversial (Rapoport and Lucchetti 2011) followed in January 2016 (FASB 2016).

The assessment of prior literature of DVAs is potentially not fully consistent. Theoretical literature warns that DVAs could be "counterintuitive" (Chasteen and Ransom 2007) and even "dangerous" (Lipe 2002). Recent experimental literature finds that investors have difficulties interpreting firms' performance and risk if DVAs influence net income (Gaynor et al. 2011; Lachmann et al. 2015). Somewhat in contrast to such concerns, recent empirical studies do not find that DVAs' perceived counterintuitivity manifests in adverse capital market effects. Instead, the evidence suggests that investors perceive DVAs as value- and risk-relevant (Chung et al. 2012), that investors understand the relation between DVAs and incomplete fair value accounting (Cedergren et al. 2015), and that DVAs do not increase information asymmetry (Schneider and Tran 2015).

So far, there is relatively little evidence on the occurrence, magnitude and reporting of DVAs. The young stream of DVA literature provides only few descriptive DVA statistics, mostly in the form of scaled measures (see e.g. Schneider and Tran 2015; Fontes et al. 2014) or for samples that potentially do not include all DVA reporting firms (Cedergren et al. 2015; Chung et al. 2012). Prior evidence on DVAs' reporting stems from only two studies. Bischof

et al. (2014) find for a European sample that the majority of observed analysts explicitly exclude DVAs. Eichner and Mettler (2014) find low DVA disclosure quality in annual reports of European firms in the year 2012 in a preliminary study. Motivated by the public DVA debate, the recent changes in DVA accounting regulation, and the potentially varying assessment of DVAs in prior literature, I seek to add to these findings by providing comprehensive descriptive evidence on DVAs' occurrence, magnitude and reporting by managers and the financial press.

Analyzing 353 firm-quarters of DVA-reporting US firms between 2007 and 2014, I find that positive and negative DVAs occur equally often and on average with similar magnitude, contrasting concerns that managers use DVAs to systematically inflate their profits. Analyzing managers' DVA reporting in corresponding quarterly earnings press releases, I find that managers present certain pieces of DVA information more regularly for negative than for positive DVAs. I also find weak evidence that managers provide more DVA relational information when they have opportunistic incentives to do so, for example, when negative DVAs turn a net profit into a loss. These findings are consistent with claims from the DVA debate that managers report negative, income-decreasing DVAs more transparently than positive DVAs which improve firms' performance figures.

Analyzing 202 article-firm-quarters, I find that several factors determine the likelihood of financial press' DVA reporting. Specifically, the probability for press coverage is higher for large positive DVAs, when managers provide more DVA information, and when managers place DVA information on the first page of their press release. The first finding is consistent with the press providing some counterweight to the emphasis on negative DVAs in firms' press releases. The latter two findings are consistent with the financial press picking up spin from managers' DVA reporting.

Analyzing the content of finacial press articles, I find that the financial press provides new information on DVAs beyond the information from the press release in 20.8% of the articles. The probability for new DVA information is higher when the corresponding press releases contain little information. Finally, I find that the financial press is more likely to critically comment on positive DVAs relative to negative DVAs. The findings are in line with the financial press assuming a "watchdog" role in two regards. First, in that the press adds new DVA information when DVA information by managers is scarce. Second, in that it takes

a critical stance especially against positive, income-increasing DVAs that could mislead investors towards a too positive evaluation of managers' and firms' performances.

Taken together, the findings contribute to the young DVA literature by providing thorough descriptive evidence on DVAs' occurrence, magnitude and reporting by managers and the financial press in the US, thereby enhancing our knowledge of an unusually controversial accounting item. The results also contribute to the public DVA debate by providing clarification on some commonly heard arguments. Finally, the findings add to the literature on the financial press by expanding our knowledge of the press' role as a provider of financial accounting information and critical watchdog of accounting and reporting practice.

The results should be of interest to international accounting standard setters. For example, the Financial Accounting Standards Board (FASB) and the International Accounting Standards Board (IASB) take an active role in the public DVA debate acknowledging that DVAs are "controversial" (Rapoport and Lucchetti 2011) and "potentially misleading" (IASB 2009). The regulation of DVA accounting is a central topic for both standard setters and has seen recent changes in US GAAP and IFRS (FASB 2016; IASB 2014a). The results should also be of interest to public policy makers who are involved in the DVA debate such as the European Commission that initially "carved-out" DVA accounting from their endorsement of accounting standard IAS 39 (EC 2005). Finally, the results should yield valuable insights to all remaining participants of the public DVA debate which include investors, researchers, managers, financial analysts, and rating agencies (Fitch Ratings 2012), amongst others.

The paper proceeds as follows. The next section outlines the theoretical background on DVA accounting regulation, the public DVA debate, and prior literature. Section 4.3 describes the sample selection and data collection. Section 4.4 presents results on DVAs' occurrence, magnitude and reporting by managers. Section 4.5 presents results on DVAs' reporting by the financial press and Section 4.6 concludes.

4.2 Debt value adjustments: theoretical background, the public debate, and prior literature

4.2.1 Debt value adjustments due to a change in own credit risk (DVAs)

In February 2007, the Financial Accounting Standards Board (FASB) issued the Statement of Financial Accounting Standards No. 159 (FAS 159), "The Fair Value Option for Financial Assets and Financial Liabilities" (new codification since 2009: ASC Topic 825-10). The fair value option for financial liabilities (FVOL) permits the recording of liabilities at fair value in US GAAP financial statements. The standard defines the fair value as a (hypothetical) market value. Firms may decide whether to elect the FVOL for each eligible item separately on each of its "election dates" which typically is the day of its first recognition. The option can only be applied to entire instruments (not just to portions) and cannot be revoked (FASB 2007).

After application of the FVOL, firms need to update the fair values of the respective liabilities periodically. One reason for a change in a liability's fair value is a change in firm's own credit risk (FASB 2006). Naturally, if market participants assume that the probability for a settlement of an outstanding liability has changed, its market price and therewith its fair value will change. As Merton (1974) explains, a change in firm's own credit risk causes a wealth redistribution between shareholders and debtholders of the firm that issued the debt. This is because a firm's debt implies an option of the shareholders to put the firm's assets to the debtholders for an amount equal to the debt's face value. This option is an economic asset of the firm and its value depends on the value of the firm's debt (Barth and Landsman 1995). For example, a decrease in the value of the firm's debt means a wealth transfer from debtholders to shareholders. Or, to quote Barth and Landsman (1995): "Effectively, the debtholder contractually has committed to accept an interest rate that subsequently proves to be economically too low."

In this paper, I refer to such fair value changes due to a change in own credit risk as debt value adjustments or DVAs. To this day, DVAs are recognized in firms' income statements and thereby increase or decrease firms' net income. FAS 159/ASC 825 requires separate disclosure of DVAs in the notes of the financial statements if DVAs affected a liability's fair value significantly. Firms further have to disclose reasons for the value changes and their determination (FASB 2007). After an amendment to ASC 825, DVAs will be recognized in other comprehensive income instead of net income in fiscal years beginning

after December 15, 2017 (FASB 2016). With IAS 39, IFRS accounting also introduced a fair value option for financial assets and liabilities that became effective in 2005. As in US GAAP accounting, IFRS firms initially recognized DVAs in net income. This changed with the introduction of IFRS 9 in 2014 that succeeded IAS 39 and requires recognition of DVAs in other comprehensive income instead (IASB 2014b).

4.2.2 The public DVA debate

Since DVAs' introduction in US GAAP accounting, a large public debate surrounds them. Focal point of the debate is a unique characteristic of DVAs that critics perceive as counterintuitive: the fact that economically unfavorable increases in firms' own credit risk lead to DVA gains which increase net income. If the firm's credit risk decreases, on the other hand, it recognizes a loss from DVAs in net income. A first concern by critics is that investors are unable to differentiate between earnings from DVAs and core earnings. They suspect that, as a consequence, investors' view on the actual firm performance could be blurred, e.g. in cases in which large DVA gains turn a net loss into a net gain. Some of the associations with DVAs from financial media that express this concern are "counterintuitive", "artificial", "phantom revenue" (Keoun 2008), "ludicrous" (Goff 2011), "accounting fiction", "paper profits" (Burne 2011), "unnatural" (Pollack 2011), "weird" (Hofman 2011), an "abomination" (Keoun and Henry 2010), "accounting voodoo" (Carver 2012b), "bizarre", "Alice in Wonderland-ish" (Rice 2012), "schmee-VA", an "accountancy spider's web (…) as dusty and all-enswathing as Miss Havisham's boudoir" (DVA, CVA, schmee-VA! 2013), or just "some fuzzy math" (Dash 2009).

A second concern voiced in the course of the debate is that managers use DVAs to improve their earnings. Bob Rice, general managing partner with Tangent Capital Partners LLC, argues in an interview that the FASB introduced DVAs "because frankly the banks and their accountants were looking for ways to find profits (Rice 2012). Analyst Meredith A. Whitney said in 2009 that banks use DVAs as a tool for a "great whitewash" to create the impression that banks are stabilizing after the financial crisis (Dash 2009). Other critics that share this point of view see DVAs as "accounting tactics – gimmicks" (Dash 2009), "accounting tricks", or even a "shameful scam" that banks deploy "to boost their profits" (Carver 2012a). Still other voices in the DVA debate contradict this view. For example, Joyce Frost, co-founder of Riverside Risk Advisors LLC, states that DVAs are "not something banks decided to use to boost their earnings" (Burne 2011). Another expert argues that

"shareholders can't push for more of a mark-to-market world, but then cherry-pick when they want to include the mark-to-market" (Burne 2011).

Related to the first and second concern, a third concern by critics is that managers do not provide DVA relational information transparently but only in certain cases, potentially to shape investors' perceptions of the firms' performance. David Milstead from the large Canadian newspaper *The Globe and Mails* says "the banks have been more than happy to highlight these [DVA] losses in their earnings releases, while being a lot more circumspect when valuation gains boost earnings". Quoting another expert, he adds that this "makes it look like they are trying to trick the media and investors and make the story better than it is" (Milstead 2012). An article in the news publication *Euroweek* asserts that "when the DVA strip-out makes the bank look good, it's more likely to end up in the press release headlines than when it doesn't" (DVA, CVA, schmee-VA! 2013). Similarly, Rolf Benders states in the German newspaper *Handelsblatt* that managers only complain about DVAs' artificiality in quarters in which DVAs reduce banks' profits but less so when DVAs result in gains (Benders 2012). Laurie Carver, Senior Staff Writer at Risk magazine more generally argues that "[b]anks downplay [DVAs] in their earnings report" (Carver 2012b). Other experts disagree and describe managers' DVA reporting as transparent. Financial executive Bob Pozen says: "Thankfully, most large banks realize that DVA should not count as profit. When reporting earnings, financial firms have been clearly laying out what part of their earnings come from DVA" (Pozen 2011). Others observe that managers ensure reporting transparency by excluding DVAs from reported figures (Goff 2011) although still others claim that managers exclude DVAs just "to avoid the public reputation risk" (Castagna 2012). Another concern about managers' DVA reporting is that they are "doing it all a bit differently" (Wroblewska 2014) so that performance comparisons between firms allegedly are as difficult as "doing a crossword in Sanskrit" (DVA, CVA, schmee-VA! 2013). Another, less common concern relates to the role of the financial press. Bob Rice said in 2012 that "[m]ost mainstream media is only now picking up on the basic idea that these [DVAs] are really value irrelevant" (Rice 2012). Contradicting this view, Bob Pozen stated in 2011 that "the media has reported earnings explaining that profits from DVA are an 'accounting gain' rather than true earnings" (Pozen 2011). For further evidence on the public DVA debate, see Appendix 4.4.

4.2.3 Prior literature

The relatively young research on DVA accounting consists of theoretical, experimental, and empirical studies. Two theoretical studies deal with DVAs prior to DVAs' introduction in US GAAP accounting in 2007. Lipe (2002) is the first to document a counterintuitive DVA result in a "what if"-scenario and concludes that DVAs are potentially "dangerous". Chasteen and Ransom (2007) state that DVAs are "counterintuitive" and propose an alternative approach for liability measurement that avoids including DVAs. Two experimental studies find that DVAs mislead even accounting professionals. Gaynor et al. (2011) find that among 184 Certified Public Accountants that participated in their experiment, over 70% wrongly interpreted DVA gains as a signal for decreased credit risk and DVA losses as a signal for increased credit risk. Lachmann et al. (2015) conduct an experiment with 93 auditors. They find that participants are more likely to misinterpret a firm's performance if net income included DVAs relative to a firm's performance where equivalent DVAs are disclosed in other comprehensive income instead.

Findings from empirical DVA literature do not show evidence for DVAs blurring investors' view. Barth et al. (2008) find that the decrease (increase) in a firm's equity value associated with an increase (decrease) in the credit risk of the firm is mitigated by a higher debt-to-assets ratio. The finding suggests that investors price the wealth transfer from debtholders to shareholders that DVAs represent correctly when firms do not recognize DVAs (the sample covers a period before DVAs' introduction in accounting). Chung et al. (2012) find that investors perceive DVAs as economic income, i.e. as value-relevant. They also find that gains and losses from FAS 159 (which include DVAs) are relevant to investors' perception of firms' risk. Cedergren et al. (2015) add to their findings by showing that investors additionally understand the offsetting relation between DVAs and changes in unrecognized intangible assets' fair values that result from changes in firms' own credit risk. Finally, Schneider and Tran (2015) find that information asymmetry among adopters of the fair value option for liabilities (FVOL) is not higher for those firms that recognize DVAs compared to other FVOL adopting firms that do not recognize DVAs.

Regarding the reporting of DVAs, Bischof et al. (2014) find that a majority of financial analysts discuss DVAs' impact on performance figures in analyst reports and often exclude them from performance figures. In a preliminary study, Eichner and Mettler (2014) find low DVA disclosure quality in annual reports of European firms in the year 2012.

Overall, the findings from theoretical, experimental, and empirical DVA literature are not necessarily inconclusive. Still, they seem to indicate some variation concerning researchers' attitude towards DVAs' usefulness. In any case, prior DVA literature offers informative insights in DVAs' properties from different points of view. Still, no research yet provides comprehensive descriptive evidence on comparably basic characteristics of DVAs such as their occurrence, magnitude, and their reporting. In light of DVAs' topicality and the still rather fragmented DVA literature such evidence should make a valuable contribution.

Additionally, thorough descriptive evidence on DVA reporting by the financial press should contribute to the literature of the financial press as an information intermediary (Bushee et al. 2010). As my findings are indicative of the financial press providing counterweight to potentially imbalanced reporting by managers, the evidence might especially improve our understanding of the financial press' information enhancement function (Bushee et al. 2010). Thereby, it should contribute to the literature on the role of the financial press as a watchdog for accounting and corporate governance related topics such as good accounting practice (Foster 1987; Foster 1979), reporting of non GAAP figures (Koning et al. 2010); accounting fraud (Miller 2006), corporate fraud (Dyck et al. 2010), and executive compensation (Core et al. 2008).

4.3 Sample selection and data collection

I provide descriptive evidence on DVAs' occurrence, magnitude and reporting by managers for all firm-quarters of US financial firms that reported DVAs and published quarterly earnings press releases between 2007 and 2014. To ensure a broad identification of DVA-reporters, I use two identification strategies from recent literature on FVOL adoption and combine the results (Cedergren et al. 2015; Wu et al. 2016). For this, I collect initial information on FVOL adoption from regulatory reports and from the Compustat annual database. Afterwards, I thoroughly search annual and quarterly financial reports of such potential FVOL adopters for DVA reporting. I thereby identify fifteen banks that adopt the FVOL in a total of 353 firm-quarters and report DVAs. I am confident that my approach allows for the most complete identification of DVAs in recent research. For example, while the sample of Cedergren et al. (2015) contains 193 firm-quarter observations with non-zero DVAs between 2007 and 2013, my sample contains 299 such observations in the same period (untabulated).

I further provide evidence on DVA reporting by the financial press for all articles from US newspapers containing DVA information that I can link to firms in my sample. Again, I run a twofold approach to make sure that the sample is as complete as possible. First, I consider all articles from four major nationwide daily newspapers that cover firm-quarters of DVA-reporting firms. Specifically, I collect articles from the *Wall Street Journal*, *The New York Times*, *The Washington Post*, and *USA Today* via ProQuest and Nexis. Second, I perform a search for DVA-related keywords in all 202 US newspapers indexed in Nexis and the Wall Street Journal. In total, I collect 173 articles. Because some articles contain DVA information on several banks, I am able to link such articles with more than one firm-quarter. In total, my final sample analyzes 202 article-firm-quarters, i.e. non-distinct firm-quarters that are covered by a financial press article that contains DVA information. Appendix 4.1 of this paper provides a thorough description of the sample selection process. Table 4.1 summarizes the sample selection process and the linkage between financial press articles and article-firm-quarters.

Table 4.1 Sample Selection Process

Panel A: Sample selection process for the base sample			
Step 1:	Identification of DVA-reporters through Bank Regulatory Reports		
	All bank holding companies filing FRY9C reports between 2007 and 2014		8,558
-	Firms without FVOL adoption in the sample period	-8,464	94
-	Firms without link between RSSD ID and PERMCO	-39	55
-	Firms without link between PERMCO and CIK	-2	53
-	Firms without information on DVAs in SEC filings	-43	**10**
Step 2:	Identification of DVA-reporters through Compustat accounting data		
	All financial firms with coverage in "Compustat North America Annual Database between 2007 and 2014		5,783
-	Firms without fair value option adoption	5,576	207
-	Firms without fair value liabilities	-47	160
-	Firms without fair value changes recognized in earnings	-65	95
-	Firms without CIK identifier	-10	85
-	Firms whose filings I searched in Step 1 of the selection process	-31	54
-	Firms without mentioning of DVA-related keywords in 10-K filings	-13	41
-	Firms without information on DVAs in SEC filings	-35	**6**
Step 3:	Identification of FVOL firm-quarters of DVA-reporting firms with earnings press releases		
	Firm-quarters of 16 DVA-reporting firms with SEC filings (10-Q and 10-K) between 2007 and 2014		483
-	Firm-quarters without 8-K filings (quarterly earnings press releases)	-52	431
-	Firm-quarters without FVOL adoption	-78	**353**

Table 4.1 (continued)

Panel B: DVA articles' distribution over time and across newspapers									
Newspaper	2007	2008	2009	2010	2011	2012	2013	2014	Total
AdvisorOne					3	9	2		14
American Banker			2	1	1	3			7
Business Insurance			1						1
Crain's New York Business					1				1
National Mortgage News		1							1
Research & Research Breaking News		1				1	3		5
St. Louis Post-Dispatch							1		1
St. Paul Pioneer Press			1						1
The New York Post						2			2
The New York Times			10	9	8	11	7	7	52
The Washington Post			3	2	2	2	5		14
ThinkAdvisor							1	1	2
USA Today			1		1	2	2		6
The Wall Street Journal	1	3	14	2	10	20	6	10	66
Total	1	5	32	14	26	50	27	18	173

Panel C: Linkage between individual financial press articles and article-firm-quarters observations						
154	article(s) provide(s) DVA information for	1	sample firm-quarter(s)	=	154	article-firm-quarters
13	"	2	"	=	26	"
3	"	3	"	=	9	"
2	"	4	"	=	8	"
1	"	5	"	=	5	"
173	articles provide DVA information for a total of				**202**	article-firm-quarter observations

Panel A of this table summarizes the sample selection process. Appendix 4.1 provides a description of the sample selection process. Panel B provides an overview over the distribution of DVA articles over time and across newspapers. A description of the DVA article collection process is provided in Appendix 4.3. Panel C provides information on the linkage between individual financial press articles and article-firm-quarters. Further information on the linkage is provided in Appendix 4.5.

4.4 DVAs' occurrence, magnitude, and reporting by managers

4.4.1 Characteristics of DVA-reporting firms

Prior literature remains silent on the characteristics of DVA-reporting firms. To gain insights, I compare certain financial characteristics of DVA reporters with those of FVOL adopters that do not report DVAs and with non-FVOL adopters. I use data of the fourth quarter of 2006 as it is the effective date for firms' decisions whether to early adopt the FVOL and the last

quarter with accounting data that is unaffected by this choice.[18] Consequently, my research setting follows Guthrie et al. (2011). Table 4.2 presents definitions of used variables and Table 4.3 presents descriptive statistics. Comparing FVOL adopters that do not report DVAs with non-FVOL adopters, I find FVOL adopters to be significantly larger in terms of total assets and total liabilities. For example, the average total assets for a FVOL adopter without DVAs are $66.39 billion whereas the average total assets for a non-FVOL adopter are $26.07 billion. This is consistent with larger US financial institutions that are more engaged in complex hedging activities, having higher demand for an adoption of the FVOL to facilitate such activities (Guthrie et al. 2011). FASB's reason for the issuance of the FVOL was to relieve firms from the burden to comply with complex hedge accounting provisions for derivatives (FASB 2007). Accordingly, I find that FVOL adopters are significantly more likely to use derivatives prior to the introduction of the FVOL than non-adopters (48% compared to 20%).

Comparing DVA-reporting FVOL adopters with FVOL adopters that do not report DVAs, I find that the DVA-reporters are significantly larger than the latter. For example, the median total assets of DVA-reporters are $838.20 billion compared to $8.05 billion of other FVOL adopters. DVA-reporters also carry significantly larger amounts of fair value liabilities in both terms, absolute and relative to total liabilities (according to a *Wilcoxon* rank-sum test). The finding is consistent with DVA-reporters applying the FVOL to larger portions of liabilities. This is plausible as FAS 159 only requires reporting of material DVAs (FASB 2007) and the magnitude of DVAs depends on the amount of debt for which the FVOL has been elected. Finally, the large majority of DVA-reporters use derivatives (87%), significantly more than other FVOL adopters (48%).

In conclusion, DVA-reporters seem to be systematically different from other FVOL adopters and from non-FVOL adopters. DVAs appear to concern only few but therefore very large and complex US financial institutions that apply the FVOL extensively. For example, five of the largest six US bank holding companies are among the fifteen DVA-reporters.[19]

[18] As data for one firm from the sample of DVA reporters is missing for the fourth quarter of 2006, I use the earliest data available for this firm instead, which is data from the fourth quarter of 2008. The results are not sensitive to the exclusion of this firm.

[19] Specifically: JPMorgan Chase & Co., Bank of America Corp., Citigroup Inc., the Goldman Sachs Group Inc., and Morgan Stanley. See https://www.ffiec.gov/nicpubweb/nicweb/HCSGreaterThan10B.aspx. Accessed 13 March 2017.

Table 4.2 Variable Definitions and Measurement

Panel A: Variables on firm characteristics	
Total assets	Total assets (Compustat item *at*)
Total liabilities	Total liabilities (Compustat item *lt*)
Total fair value liabilities	Total liabilities at fair value (Compustat item *tfvl*)
Total fair value liabilities (%)	*Total fair value liabilities / Total liabilities*
Eligible instruments (%)	Instruments eligible for fair value measurement [Compustat items (*rect+ivst+ivaeq+ivao+ap+dlc+dltt*)/(*at+lt*)]
Derivative user (dummy)	Binary variable indicating use of derivatives, i.e. one or both of Compustat items *cidergl* and *aocidergl* are different from zero
Return on equity (%)	Return on equity (Compustat items *ib*/*seq*)
Panel B: DVA, DVA information, and further variables	
Variable Name	**Definition**
DVAs	Debt value adjustments due to a change in own credit risk on liabilities for which the fair value option has been elected (DVAs). Source: 10-Q/10-K filings.
AbsDVA	Absolute DVAs scaled by natural log of lagged total assets
NegDVA	Binary variable indicating negative DVAs in a given firm-quarter
DVA (%)	Ratio of absolute quarterly DVAs to absolute quarterly net income
Incentive	Binary variable indicating the presence of an opportunistic incentive for managers to report/emphasize DVAs. Specifically, *Incentive* indicates the presence of one (or more) of the following situations in a given firm-quarter: 1. missed last year's earnings due to DVAs, 2. missed profitability due to DVAs, 3. missed mean consensus analyst forecast while DVAs are negative
Pages	Natural log of the number of pages in the quarterly earnings press of a given firm-quarter
Quarter	Time trend variable increasing with equal steps from 1 (first quarter of 2007) to 32
TA	Natural log of lagged total assets
Ment	Binary variable indicating that a quarterly earnings press release (qepr) mentions DVAs.
Sign	Binary variable indicating that a qepr provides the sign of DVAs
Amount	Binary variable indicating that a qepr provides the amount of DVAs
Due	Binary variable indicating that a qepr provides the information that DVAs result from a change in the value of the firm' own debt or from a change in firm's own credit risk. Example: "net revenues included negative revenue of $189 million related to debt-related credit spreads".
Dir	Binary variable indicating that a qepr provides information on the direction of the change in debt's value or the direction of the change in credit risk. Example: "net revenues included the negative impact of $2.0 billion from the tightening of debt-related credit spreads".
Comment	Binary variable indicating that a qepr provides an evaluative comment of DVAs, for example, by the CEO
Excl	Binary variable indicating that a qepr provides a non-GAAP figure that excludes DVAs or a description of a situation that excludes DVAs. Example: "excluding debt-related mark-to-market losses, trading income increased"
AggInfo	*Ment + Sign + Amount + Due + Dir + Comment + Excl*
FirstPageMent	Binary variable indicating that a qepr's first page provides DVA information or a non-GAAP figure excluding DVAs
Coverage	Binary variable indicating that a firm-quarter is covered by one or more financial press articles that provide DVA information

Table 4.2 (continued)

Net income	Quarterly net income (Compustat item *niq)*
AbsNI	Absolute value of *Net income* (Compustat item *niq)*
NegNI	Binary variable indicating negative *Net income* in a given firm-quarter
Panel C: Information enhancement variables	
NewInfoDum	Binary variable indicating that a financial press article provides a piece of information as measured by *Ment, Sign, Amount, Due, Dir, Comment*, or *Excl* that the corresponding firm qepr does not provide
Critique	Binary variable indicating that a financial press articles' tone is critical towards DVAs

This table summarizes the definitions and the measurements of the variables used in this paper.

Table 4.3 Characteristics of DVA Reporters

	Mean	Std. Dev	Min	Median	Max	N
Panel A: DVA reporters						
Total assets (in $bn)	665.97***	617.06	1.90	838.20***	1884.32	15
Total liabilities (in $bn)	620.30***	573.27	1.49	797.66***	1764.54	15
Total fair value liabilities (in $bn)	136.08***	180.92	0.17	28.84***	479.9	15
Total fair value liabilities (%)	0.17	0.14	0.00	0.18***	0.45	15
Eligible instruments (%)	0.80	0.19	0.23	0.85	0.97	15
Derivative user (dummy)	0.87***	0.35				15
Return on equity (%)	0.14	0.06	0.01	0.13	0.27	15
Panel B: FVOL adopters that do not report DVAs						
Total assets (in $bn)	66.39**	180.44	0.41	8.05***	1030.51	82
Total liabilities (in $bn)	60.68**	170.88	0.13	6.37***	982.18	82
Total fair value liabilities (in $bn)	11.81	63.23	0.00	0.10	467.20	82
Total fair value liabilities (%)	0.10	0.21	0.00	0.01	1.00	82
Eligible instruments (%)	0.80	0.21	0.10	0.91	0.99	82
Derivative user (dummy)	0.48***	0.50				82
Return on equity (%)	0.13	0.12	-0.07	0.12**	0.83	82
Panel C: Non-FVOL adopters						
Total assets (in $bn)	26.07	152.89	0.00	0.81	1965.16	1716
Total liabilities (in $bn)	24.23	146.12	0.00	0.63	1919.42	1716
Total fair value liabilities (in $bn)	0.00	0.00	0.00	0.00	0.00	1716
Total fair value liabilities (%)	0.00	0.00	0.00	0.00	0.00	1716
Eligible instruments (%)	0.76	0.26	0.00	0.92	1	1716
Derivative user (dummy)	0.20	0.40				1716
Return on equity (%)	0.69	20.37	-18.57	0.10	840.53	1716

Panel A, B, and C of this table provide descriptive statistics on financial characteristics of DVA reporters, FVOL adopters that do not report DVAs, and non-FVOL adopters, respectively. The identification of DVA reporters follows Step 1 and 2 of the sample selection process, the identification of FVOL adopters follows Step 2 of the sample selection process, see Table 4.1. ***, **, and * indicate significance at the 1%, 5%, and 10% level of two-tailed t-tests (means) and *Wilcoxon* rank-sum tests (medians) of equality between statistics of DVA reporters and statistics of FVOL Adopters that do not report DVAs in Panel A and between statistics of FVOL Adopters that do not report DVAs and statistics of non-FVOL adopters in Panel B. Definitions of variables are reported in Table 4.3, Panel A.

4.4.2 Descriptive statistics on DVAs' occurrence and magnitude

Table 4.4, Panel A depicts descriptive statistics regarding DVAs' magnitude and reporting by managers. The average of quarterly DVA amounts in the sample is $9.857 Mio (median: $0.000 Mio.). Negative DVAs appear in 49.9% of the firm quarters. The lowest DVA amount in the sample is -$3,600.000 Mio, the largest is $4,506.000 Mio. The mean of absolute quarterly DVA amounts is $314.910 Mio (median: $76.000 Mio). The average ratio of absolute quarterly DVA amounts relative to absolute quarterly net income is 73.1% (median: 6.9%). Of the 353 firm-quarters, only eleven firm-quarters have DVA amounts of zero (untabulated). Taken together, the descriptive evidence is consistent with DVAs causing losses equally often as gains in the sample. Further, the low mean of DVAs is consistent with positive and negative DVAs assuming comparable amounts on average. The findings contrast concerns from the public DVA debate that DVAs are an "accounting trick" (Carver 2012a) used "to find profits" (DVA, CVA, schmee-VA! 2013). Still, they are consistent with a regular and notable impact of DVAs on firms' results and therewith further motivate my following research on DVAs' reporting by managers and the financial press.

4.4.3 DVA reporting by managers

4.4.3.1 Descriptive statistics

To explore managers' reporting of DVAs, I examine firms' issued quarterly earnings press releases. Quarterly earnings press releases are a direct way of firms to communicate with financial markets and among the most common and important instruments of voluntary disclosure (Davis et al. 2012; Davis and Tama-Sweet 2012). Compared to financial reports, they are unaudited and less regulated and therefore allow for a higher level of discretion concerning form and content (Henry 2008). In addition, as seen in Section 4.2.2 common claims in the DVA debate regarding managers' DVA reporting usually refer to DVAs' reporting in earnings press releases.

I construct several disclosure variables that indicate the presence of specific information in a quarterly earnings press release (see similar: Baumker et al. 2014). The arguably broadest measure, *Ment*, indicates that the quarterly earnings press release mentions DVAs. *Sign* indicates that the press release contains information on the DVAs' sign (i.e. whether DVAs were positive or negative). *Amount* indicates that the press release contains the

amount of DVAs as an absolute or per share figure.[20] Regarding variables that indicate information provided on DVAs' characteristics, *Due* indicates that the press release contains information explaining that DVAs stem from a change in debt value or from a change in credit risk. *Dir* indicates that the press release contains directional information on DVAs. For example, in firm-quarters with negative DVAs, *Dir* indicates that the press release contains the information that the negative DVAs stem from an *increase* in own debt's value and/or that they stem from a *decrease* in the firm's own credit risk. According to experimental evidence, this information can help investors unravel the criticized "counterintuitivity" of DVAs (Gaynor et al. 2011).

The variable *Comment* indicates that the press release provides an evaluative comment on DVAs. For example, in Morgan Stanley's press releases of the first quarter of 2009, its CEO John J. Mack says „In fact, Morgan Stanley would have been profitable this quarter if not for the dramatic improvement in our credit spreads – which is a significant positive development, but had a near-term negative impact on our revenues." *Excl* indicates that the press release provides a non-GAAP performance figure that excludes DVAs or a description of a scenario that excludes DVAs, for example, "excluding debt-related mark-to-market losses, trading income increased". Prior literature considers such non-GAAP figures as information on the excluded items' transitoriness (Curtis et al. 2014; Baumker et al. 2014). As an overall measure of disclosure quality and of reporting emphasis, *AggInfo* is an aggregated measure equal to the sum of *Ment, Sign, Amount, Due, Dir, Comment*, and *Excl*. Such "checklist" measures are founded in prior literature on disclosure quality (see e.g. Hail 2002; Botosan 1997). Finally, *FirstPageMent* indicates that the press release provides DVA information or a non-GAAP figure excluding DVAs on its first page. Prior literature finds that managers use prominent placement of items within press releases as means of emphasis (see e.g. Bowen et al. 2005; Guillamon-Saorin et al. 2012). I provide an example for the measures' codification in Appendix 4.2 and a discussion of the measures in the conclusion of the paper.

Table 4.4 depicts descriptive statistics and correlation coefficients on the disclosure measures and regression inputs. It shows that there is considerable variation in the frequency

[20] In some quarterly press releases, I find that managers report DVA amounts aggregated with other amounts such as credit value adjustments (from counterparty risk changes) or certain fair value adjustments on derivatives. If the inclusion of DVAs in such aggregated amounts is explicitly stated in the press release, I consider it reporting of an *Amount*. Apart from that, I also say that a quarterly earnings press release contains a DVA amount if, within one paragraph, a figure is reported including and excluding DVAs, for example, "net income was $1.8 billion and $1.6 billion excluding DVAs". The results from all following tests are not sensitive to the exclusion of the *Amount* variable.

of the different disclosures. In particular, 55.2% of quarterly earnings press releases in the sample mention DVAs, a narrow majority. Information on the sign of DVAs, on their amount, and on the fact that they arise from changes in debt and credit risk are provided similarly often (means of 51.6%, 47.6%, and 53.5%). In contrast, only 24.9% of the press releases provide directional DVA information that explains DVAs' "counterintuitivity". Evaluative comments on DVAs appear in 2.5% of the press releases. A non-GAAP figure excluding DVAs is provided in 39.1% of the press releases. An average press release contains 2.75 of the seven investigated pieces of information (median: 3). Finally, 23.8% of the press releases provide DVA information on their first page.

In sum, the descriptive evidence on managers' DVA disclosures is not consistent with managers providing DVA disclosures very continuously but it is also not consistent with DVA disclosures by managers being very scarce. Rather, Managers' DVA reporting in quarterly earnings press releases shows a certain degree of variation which further motivates my following analysis on its determinants.

Table 4.4 Descriptive Statistics and Correlation Coefficients for the Base Sample

Panel A: Descriptive Statistics

	Mean	Std. Dev	Min	Median	Max	N
DVA measures						
DVAs	9.857	698.514	-3600.000	0.000	4506.000	353
NegDVA	0.499					353
Absolute DVAs	314.910	623.353	0.000	76.000	4506.000	353
DVA (%)	0.731	3.839	0.000	0.069	61.419	353
DVA information measures						
Ment	0.552					353
Sign	0.516					353
Amount	0.476					353
Due	0.535					353
Dir	0.249					353
Comment	0.025					353
Excl	0.391					353
AggInfo	2.745	2.280	0	3	7	353
FirstPageMent	0.238					353
Coverage	0.212					353
Further regression inputs						
AbsDVA	22.773	43.933	0.000	6.001	307.844	353
Incentive	0.198					353
Quarter	17.079	8.802	1	17	32	353
Pages	2.617	0.422	1.099	2.639	3.611	353
TA	12.747	2.051	7.528	13.632	15.016	353
AbsNI	199.219	411.260	0.051	89.076	4447.359	353
NegNI	0.317					353

Table 4.4 (continued)

Panel B: Correlation Coefficients

N = 353	DVA	AbsDVA	NegDVA	Ment	Sign	Amount	Due	Dir	Comment	Excl	AggInfo	FirstPage Ment	Coverage	NegIncent	Quarter	Pages	TA	AbsNI	NegNI
DVA	1																		
AbsDVA	0.080	1																	
NegDVA	**-0.451**	-0.019	1																
Ment	-0.032	**0.319**	-0.003	1															
Sign	-0.032	**0.345**	0.014	**0.929**	1														
Amount	-0.008	**0.340**	0.014	**0.858**	**0.924**	1													
Due	-0.031	**0.329**	-0.003	**0.966**	**0.927**	**0.876**	1												
Dir	-0.067	**0.440**	0.054	**0.519**	**0.519**	**0.500**	**0.524**	1											
Comment	**-0.200**	**0.234**	0.054	**0.146**	**0.121**	**0.134**	**0.151**	**0.281**	1										
Excl	-0.067	-0.053	0.049	-0.038	-0.013	0.050	-0.010	-0.032	0.091	1									
AggInfo	-0.063	**0.381**	0.030	**0.923**	**0.932**	**0.917**	**0.934**	**0.654**	**0.263**	**0.212**	1								
FirstPage Ment	-0.095	**0.243**	0.002	**0.302**	**0.329**	**0.373**	**0.320**	**0.324**	**0.205**	**0.398**	**0.451**	1							
Coverage	-0.016	**0.357**	0.008	**0.426**	**0.448**	**0.476**	**0.442**	**0.373**	**0.224**	**0.194**	**0.521**	**0.474**	1						
NegIncent	**-0.296**	0.096	**0.456**	0.076	0.070	0.052	0.079	**0.140**	**0.145**	**0.140**	**0.127**	0.089	0.089	1					
Quarter	**-0.116**	**-0.202**	**0.257**	-0.017	0.001	-0.020	-0.038	-0.094	-0.040	**0.281**	0.024	**0.233**	**0.174**	**0.116**	1				
Pages	-0.035	0.094	-0.052	**0.484**	**0.436**	**0.425**	**0.487**	**0.202**	0.080	-0.006	**0.444**	**0.137**	0.064	-0.064	0.031	1			
TA	-0.014	**0.274**	-0.068	**0.292**	**0.282**	**0.249**	**0.268**	**0.306**	0.086	**-0.252**	**0.249**	-0.026	**0.350**	**-0.238**	-0.017	**0.280**	1		
AbsNI	0.044	0.066	-0.019	-0.009	-0.045	-0.058	-0.022	0.009	-0.032	0.018	-0.026	**-0.134**	-0.017	-0.054	**-0.118**	0.038	0.291	1	
NegNI	0.057	0.102	-0.010	-0.035	-0.033	-0.040	-0.024	**-0.111**	-0.033	**0.265**	0.004	-0.038	**-0.131**	**0.226**	**-0.180**	-0.006	-0.286	**0.139**	1

Panel A of this table provides descriptive statistics on DVA measures and the inputs of the regression tests for the base sample. Panel B provides correlation coefficients for the inputs of the regression tests for the base sample. Bold letters indicate significance at the 10%-level in Panel B. Definitions of variables are reported in Table 4.2, Panel B.

4.4.3.2 Regression analysis

In the prior section, I find a certain degree of variation in the quantity and quality of DVA relational information that managers provide in quarterly earnings press releases. In this section, I examine potential determinants of such differences using multivariate regression analyses. This method allows for a better identification of the potential variation in reporting that is associated with individual determinants because it simultaneously controls for reporting variation that is associated with other factors in the model. Motivated by the DVA debate and prior literature, I investigate whether DVA relational information in quarterly earnings press releases is associated with DVAs' magnitude and sign (Schrand and Walther 2000) and with opportunistic incentives to provide more information (Marques 2010; Baumker et al. 2014). I employ a probit model for regressions with binary dependent variables and an OLS model for regressions with *AggInfo* as dependent variable. The regression model is:

$$Disclosure_{it} = \beta_0 + \beta_1 AbsDVA_{it} + \beta_2 AbsDVA_{it} * NegDVA_{it} + \beta_3 NegDVA_{it} + \beta_4 Incentive_{it} + \beta_5 Pages_{it} + \beta_6 Quarter_{it} + \beta_7 TA_{it} + \varepsilon_{it} \quad (1)$$

where *Disclosure* represents the different measures of DVA information: *Ment*, *Sign*, *Due*, *Dir*, *Comment*, *Excl*, *AggInfo*, and *FirstPageMent* (see Section 4.4.3 and Table 4.2, Panel B for details). *AbsDVA* is the absolute dollar-amount of DVAs deflated by the natural log of lagged total assets. *NegDVA* is a binary variable indicating that DVAs are negative in the firm-quarter. *Incentive* is a binary variable indicating the presence of an opportunistic incentive for managers to report/emphasize DVAs. In line with prior literature, I consider three situations (Marques 2010): 1. missed last year's earnings due to DVAs, 2. missed profitability due to DVAs, and 3. missed mean consensus analyst forecast while DVAs are negative.[21] *Pages* is the natural log of the number of pages in the quarterly earnings press releases. *Quarter* is a time trend variable increasing with equal steps from 1 (first quarter of 2007) to 32 (fourth

[21] Bischof et al. 2014 find for a European sample that most of the observed analysts exclude DVAs from reported earnings. Checking Thomson Reuters TRAA database, I find that there is also variation in the sense that for some of my sample firms, DVAs are excluded from analysts' earnings forecasts, but for others they are not. Therefore, I am unable to limit this third component of my *Incentive* variable to cases in which negative DVAs are the cause for missed consensus analysts' forecast. This inaccuracy potentially weakens the measure's association with the test variables in my regression analyses.

quarter of 2014). *TA* is the natural log of lagged total assets. I use robust standard-errors that are clustered by firms (White 1980).

Table 4.5 provides the results of the regression estimations. The coefficient on *AbsDVA* is significantly positive in all models except for Model 6 and 7 as the results of F-tests indicate. Similarly, the sum of coefficients *AbsDVA* and *AbsDVA*NegDVA* is positive and significant in all models but Model 3 and 7. These findings are consistent with managers providing more DVA relational information in quarterly earnings press releases for larger positive DVAs and for larger negative DVAs. The coefficient on *AbsDVA*NegDVA* is significantly positively associated with four disclosure measures in the regression models (*Ment, Sign, Due,* and *Comment*). This finding is in line with managers being more likely to provide these pieces of DVA information for large negative DVAs relative to large positive DVAs. The coefficient on *NegDVA* is negatively significant in Model 1, 2, and 4. This indicates that managers are less likely to provide this information when small DVAs are negative. The coefficient on *Incentive* is positive and statistically significant in five models, including Model 8 (*AggInfo*). This is consistent with managers providing more DVA information, regarding specific information and overall, when they have a strategic incentive to do so, for example, when a negative DVA turned a quarterly net profit into a loss. The coefficient on *Pages* is significantly positive in several models indicating a higher likelihood for DVA information in more comprehensive earnings press releases. The coefficient on *Quarter* is only significant in Model 7 (*Excl*). This is consistent with the reporting of non-GAAP figures excluding DVAs becoming more common over time. For Model 9, the coefficients on *AbsDVA*, *NegDVA*, and the joint coefficient of *AbsDVA+AbsDVA*NegDVA* indicate that DVA reporting on a press release's first page is more likely for larger DVAs, positive and negative, but more common for positive DVAs when DVAs are small. The significantly positive coefficient on *Quarter* is in line with more prominent DVA reporting by managers over time.

I rerun all analyses including firm indicators. Thereby, I control for time-invariant firm characteristics that are associated with the propensity to provide DVA information. The procedure leads to varying sample sizes between the models as some firms never provide specific pieces of DVA information. Table 4.5, Panel B displays the results. I find that the implications of the results largely persist. In particular, the coefficients on *AbsDVA*NegDVA*

in Models 1, 2, 4, and 6 remain positive and statistically significant. The coefficients on *NegDVA* and *Incentive* are mostly insignificant in this specification.

In conclusion, the findings from my regression analysis on the determinants of managers' DVA reporting are consistent with claims from the public DVA debate that managers provide more relational information on large negative DVAs than on large positive DVAs. For smaller DVAs, I find weak evidence that managers provide more information on positive DVAs relative to negative DVAs. I also find some evidence that managers provide more information on DVAs when they have strategic incentives to do so. However, as these findings largely do not hold after inclusion of firm indicators in the model, I caution to interpret these results carefully. Finally, I do not find evidence that managers report large negative DVAs significantly more often on the first page of quarterly earnings press releases than positive DVAs.

Table 4.5 Regression Analysis on Managers' DVA Reporting

Panel A: Regression results									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	*Ment*	*Sign*	*Amount*	*Due*	*Dir*	*Comment*	*Excl*	*AggInfo*	*FirstPageMent*
AbsDVA	0.020***	0.023***	0.021***	0.021***	0.010***	0.001	0.001	0.015***	0.008**
	(2.9263)	(2.7429)	(3.0724)	(2.8192)	(2.8720)	(0.7988)	(0.2239)	(3.4304)	(2.2097)
AbsDVA*NegDVA	0.044***	0.039***	-0.009	0.042***	0.002	0.006**	0.003	0.004	0.009
	(2.8269)	(3.1002)	(-0.9043)	(2.8736)	(0.4276)	(2.2092)	(1.0519)	(0.8402)	(1.4607)
NegDVA	-0.502**	-0.379*	0.158	-0.471**	0.011	-0.324	-0.257	-0.176	-0.460***
	(-2.1044)	(-1.9108)	(0.6781)	(-2.0114)	(0.0545)	(-1.0266)	(-1.5555)	(-0.6719)	(-2.7662)
Incentive	0.668***	0.412*	0.175	0.584**	0.553**	0.659	0.221	0.782**	0.041
	(2.8120)	(1.7588)	(0.7549)	(2.0646)	(2.0383)	(1.5499)	(1.2011)	(2.9440)	(0.1546)
Pages	2.039***	1.776***	1.542***	2.085***	0.723	0.571	0.181	2.131***	0.510
	(4.3170)	(4.3580)	(3.5956)	(4.5674)	(1.2836)	(1.5587)	(0.3110)	(4.3605)	(1.1586)
Quarter	0.014	0.019	0.009	0.011	-0.012	-0.014	0.050***	0.017	0.061**
	(0.6270)	(0.8809)	(0.4159)	(0.4718)	(-0.9645)	(-0.6684)	(2.9781)	(0.5606)	(2.4988)
TA	0.088	0.067	0.074	0.058	0.263**	0.112	-0.189	0.089	-0.136
	(0.7018)	(0.5684)	(0.6851)	(0.4882)	(2.3087)	(0.9911)	(-1.2830)	(0.6653)	(-1.0996)
Constant	-6.931***	-6.245***	-5.647***	-6.688***	-6.282***	-5.021***	0.798	-4.714**	-1.557
	(-3.8883)	(-4.0928)	(-3.7077)	(-4.1377)	(-3.0334)	(-2.7858)	(0.3562)	(-2.6953)	(-0.8640)
Firm Indicators	No	No	No	No	No	No	No	No	No
F-Test: AbsDVA+ AbsDVA*NegDVA	0.002	0.001	0.209	0.001	0.054	0.001	0.342	0.039	0.004
Pseudo R^2/Adj. R^2	0.370	0.343	0.259	0.372	0.261	0.227	0.125	0.326	0.187
N	353	353	353	353	353	353	353	353	353

Table 4.5 (continued)

Panel B: Regression results with firm indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	*Ment*	*Sign*	*Amount*	*Due*	*Dir*	*Comment*	*Excl*	*AggInfo*	*FirstPageMent*
AbsDVA	0.016**	0.017**	0.019**	0.018**	0.009***	0.005**	0.001	0.009***	0.005
	(2.0570)	(1.9972)	(2.4807)	(2.0709)	(3.1286)	(2.0698)	(0.3814)	(3.0617)	(1.4987)
AbsDVA*NegDVA	0.026***	0.018*	-0.014	0.027***	0.001	0.009**	0.005	0.002	0.006
	(3.7348)	(1.6933)	(-1.5074)	(3.7893)	(0.3362)	(2.3407)	(1.3235)	(0.8296)	(1.4131)
NegDVA	-0.241	0.084	0.702*	-0.262	0.233	-0.381	0.224	0.212	-0.167
	(-1.0323)	(0.3761)	(1.8971)	(-1.0243)	(0.8710)	(-1.2055)	(0.5751)	(1.1778)	(-0.7331)
Incentive	0.972*	0.488	0.132	0.813	0.506	0.781	-0.371	0.311	-0.078
	(1.9218)	(1.2378)	(0.3250)	(1.6009)	(1.4219)	(1.2060)	(-1.2552)	(1.1844)	(-0.2269)
Pages	2.090**	1.250	1.597	2.023**	0.322	-0.550	1.948*	1.722**	-0.394
	(2.1519)	(1.6130)	(1.5204)	(2.1192)	(0.5393)	(-0.3172)	(1.7707)	(2.9411)	(-0.8371)
Quarter	0.036	0.049	0.031	0.028	-0.004	-0.018	0.139***	0.035	0.081*
	(1.0045)	(1.3568)	(0.7936)	(0.8445)	(-0.2448)	(-1.3442)	(3.2064)	(0.9678)	(1.6707)
TA	-0.201	1.150	0.535	-0.394	0.422	2.129***	-1.774	-0.038	-2.935**
	(-0.2317)	(0.9799)	(0.5163)	(-0.5014)	(1.0079)	(6.6282)	(-1.0794)	(-0.0773)	(-1.9741)
Constant	-3.326	-15.115	-9.636	-1.201	-6.430	-25.320***	12.852	-2.730	29.573*
	(-0.3819)	(-1.3993)	(-0.9999)	(-0.1557)	(-1.1810)	(-2.7604)	(0.7791)	(-0.6661)	(1.9142)
Firm Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test: AbsDVA+ AbsDVA*NegDVA	0.000	0.000	0.432	0.000	0.006	0.001	0.012	0.004	0.000
Pseudo R^2/Adj. R^2	0.506	0.428	0.393	0.491	0.183	0.241	0.516	0.571	0.360
N	301	273	273	301	188	120	185	353	213

Panel A of this table displays coefficient estimates from regression models. Panel B displays coefficient estimates of the same model including firm indicators. The underlying regression model is:

$$Disclosure_{it} = \beta_0 + \beta_1 AbsDVA_{it} + \beta_2 AbsDVA_{it} * NegDVA_{it} + \beta_3 NegDVA_{it} + \beta_4 Incentive_{it} + \beta_5 Pages_{it} + \beta_6 Quarter_{it} + \beta_7 TA_{it} + \varepsilon_{it}$$

The dependent variables (1)-(7) are binary variables indicating whether managers provide specific DVA information in the respective quarterly earnings press release (qepr). *AggInfo* is a continuous variable measuring aggregated DVA relational information provided by managers in the respective qepr. *FirstPageMent* is a binary variable indicating whether managers provided DVA information on the first page of the respective qepr. The regression models have standard errors that are heteroskedasticity robust and clustered by firms. z-statistics/t-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. All F-tests are two-tailed. Definitions of variables are reported in Table 4.2, Panel B.

4.5 DVAs' reporting by the financial press

4.5.1 Introduction to the financial press analyses

As noted before, prior evidence on DVA information in financial markets is scarce. The only evidence, to my knowledge, stems from Bischof et al. (2014) who find that the majority of financial analysts in their sample provide information on DVAs. An analysis of DVAs' reporting by the financial press could still be of interest. If "counterintuitive" DVAs indeed complicated investors' assessment of firms' performance and risk, as experimental literature finds (Lachmann et al. 2015; Gaynor et al. 2011), retail investors are likely more affected by this, as they have higher information processing costs (Hirshleifer and Teoh 2003). However, prior literature finds that retail investors' assessment of financial analysts' information is rather naïve (Malmendier and Shanthikumar 2007). Indeed, retail investors perceive financial media as the most useful source of information (Pellens and Schmidt 2014). Also, my summary of the public DVA debate suggests that the debate is reflected in the financial press which spurs additional interest in the press' DVA reporting behavior (see Section 4.2.2 and Appendix 4.4). My analysis of DVA reporting by the financial press consists of two analyses. First, I analyze the determinants of DVA reporting by the financial press. Second, I analyze the determinants of new DVA information by the press and the determinants for a critical tone of the press reporting towards DVAs.

4.5.2 Determinants of DVA coverage

Table 4.1, Panel B provides an overview over the distribution over time and across newspapers of 173 financial press articles containing DVA information that I find in US financial newspapers. Appendix 4.3 provides information on the collection of the articles. The 173 articles cover 75 of my 353 sample firm-quarters (21.2%). To analyze the determinants for financial press' DVA reporting, I employ the following probit regression model:

$$\begin{aligned} Coverage_{it} = {} & \beta_0 + \beta_1 AbsDVA_{it} + \beta_2 AbsDVA_{it} * NegDVA_{it} + \beta_3 NegDVA_{it} + \beta_4 AggInfo_{it} \\ & + \beta_5 FirstPageMent_{it} + \beta_6 AbsNI_{it} + \beta_7 NegNI_{it} + \beta_8 AbsNI * NegNI_{it} \\ & + \beta_9 Pages_{it} + \beta_{10} Quarter_{it} + \beta_{11} TA_{it} + \varepsilon_{it} \end{aligned} \quad (2)$$

where *Coverage* is a binary variable indicating that a firm-quarter is covered by at least one financial press article that provides DVA information. *AbsNI* is the absolute value of net

income and *NegNI* is a binary variable indicating negative net income. See Table 4.2, Panel B for all further variable definitions. I use robust standard-errors that are clustered by firms (White 1980).

Table 4.6 provides results of the regression estimations. In Model 1, the coefficient on *AbsDVA* is significantly positive. This is consistent with press coverage being more likely for larger positive DVAs. In contrast, the joint coefficient on *AbsDVA+AbsDVA*NegDVA* is not different from zero (p-value: 0.691). This is inconsistent with press coverage being more likely for large negative DVAs. The coefficient on *AggInfo* is significantly positive. This is in line with a higher chance for press' DVA reporting when managers provide more DVA relational information in quarterly earnings press releases. The coefficient on *FirstPageMent* is significantly positive, too. This implies a higher probability of DVA reporting by the press when managers emphasize DVAs with a prominent placement in earnings press releases. The significantly negative coefficient on *AbsNI* and the significantly positive coefficient on *AbsNI*NegNI* indicate higher chances for press' DVA reporting for firm-quarters with low net profits and high net losses. Finally, the significantly positive coefficients on *Quarter* and *TA* imply an increased DVA coverage by the press over time and a higher DVA coverage for larger firms. I rerun the analysis including firm indicators (Model 2). This decreases the sample size as some firms never receive DVA press coverage. The coefficients on my main variables remain unchanged except for the now significantly negative coefficient on *AbsDVA*NegDVA.* The finding is consistent with the financial press being less likely to cover large positive DVAs. I repeat both analyses, Model 1 and 2, including *Ment* as control variable. The results remain largely unchanged (see Model 3 and 4).

In conclusion, findings from my analyses on the determinants of DVA coverage by the financial press are in line with the financial press being more likely to cover large positive DVAs relative to large negative DVAs. This is consistent with the financial press providing some counterweight to asymmetric DVA reporting by managers who rather emphasize negative DVAs. It is also consistent with prior literature that finds that "bad news" are more likely to be covered by the financial press, because large positive DVAs reflect high increases in credit risk on average (Gaa 2008). The findings are also consistent with the financial press potentially following managers' reporting spin in the sense that DVA coverage is associated with managers' reporting emphasis on DVAs in press releases (Ahern and Sosyura 2014; Dyck and Zingales 2003).

Table 4.6 Determinants of Financial Press' DVA Reporting

	(1)	(2)	(3)	(4)
Dependent Variable:	*Coverage*	*Coverage*	*Coverage*	*Coverage*
AbsDVA	0.006***	0.004**	0.006***	0.004**
	(5.3277)	(2.4547)	(5.3706)	(2.1960)
AbsDVA*NegDVA	-0.004	-0.008***	-0.004	-0.009***
	(-1.1455)	(-3.1789)	(-1.1492)	(-4.7110)
NegDVA	-0.005	0.198	-0.002	0.223*
	(-0.0674)	(1.3756)	(-0.0205)	(1.7470)
AggInfo	0.277**	0.292***	0.242*	0.540***
	(2.3339)	(2.7138)	(1.9005)	(3.1564)
FirstPageMent	1.049***	1.042**	1.078***	0.810*
	(2.8758)	(2.2653)	(3.1402)	(1.7429)
Ment			0.178	-1.423
			(0.2403)	(-1.5690)
AbsNI	-0.002**	-0.000	-0.002**	-0.000
	(-2.2127)	(-0.4199)	(-2.1843)	(-0.7134)
NegNI	-0.412	0.200	-0.411	0.231
	(-1.5097)	(0.4910)	(-1.5413)	(0.4807)
AbsNI*NegNI	0.002**	0.000	0.002**	0.001
	(2.2295)	(0.8034)	(2.2215)	(1.2913)
Pages	-0.992*	0.109	-0.994*	-0.180
	(-1.7769)	(0.1612)	(-1.8335)	(-0.2082)
Quarter	0.035***	0.016	0.034***	0.025**
	(2.7105)	(1.6398)	(2.7375)	(1.9659)
TA	0.669***	-0.174	0.680***	-0.131
	(3.0864)	(-0.1204)	(2.9930)	(-0.0875)
Constant	-8.932***	0.588	-9.087**	0.892
	(-2.6479)	(0.0293)	(-2.4889)	(0.0430)
Firm Indicators	No	Yes	No	Yes
F-test: AbsDVA+ AbsDVA*NegDVA	0.691	0.106	0.698	0.031
Pseudo R^2	0.530	0.434	0.530	0.445
N	353	188	353	188

This table displays coefficient estimates from probit regression models. The underlying regression model is:

$$Coverage_{it} = \beta_0 + \beta_1 AbsDVA_{it} + \beta_2 AbsDVA_{it} * NegDVA_{it} + \beta_3 NegDVA_{it} + \beta_4 AggInfo_{it} + \beta_5 FirstPageMent_{it} + \beta_6 AbsNI_{it} + \beta_7 NegNI_{it} + \beta_8 AbsNI * NegNI_{it} + \beta_9 Pages_{it} + \beta_{10} Quarter_{it} + \beta_{11} TA_{it} + \varepsilon_{it}$$

The dependent variable is a binary variable indicating that a firm-quarter of the base sample is covered by a minimum of one financial press article. The regression models have standard errors that are heteroskedasticity robust and clustered by firms. z-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. All F-tests are two-tailed. Definitions of variables are reported in Table 4.2, Panel B.

4.5.3 Determinants of financial press' DVA information enhancement

To analyze the financial press' information enhancement function regarding DVA information (Bushee et al. 2010), I analyze 202 article-firm-quarters (see Table 4.1, Panel C and Appendix 4.3 for details). I construct two measures of information enhancement. *NewInfoDum* is a binary variable indicating that a financial press article provides a piece of DVA information that the quarterly earnings press release in the firm-quarter covered by the article does not provide. I find that this is the case for 42 article-firm-quarters (20.8%, untabulated). Further untabulated results show that the most common form of DVA information enhancement are the provision of non-GAAP figures excluding DVAs and the provision of evaluative comments on DVAs by journalists. In contrast, for example, I find only three instances in which the press mentions DVAs that the corresponding firm press release does not mention and only one article that provides a DVA amount that is not disclosed in the respective press release. My second measure of information enhancement *Critique* is a binary variable indicating a critical tone in the press release towards DVAs. Narrative DVA disclosures in firms' quarterly earnings press releases are very rare, as the low mean of *Comments* indicates. Especially, I only find two instances of very mild DVA criticism in firms' press releases. I therefore argue that a critical tone by the press can raise investors' awareness on DVAs' unusual and potentially "counterintuitive" characteristics. For examples of critical tone in press articles in the sample, see Appendix 4.4. I find a critical tone towards DVAs in 53 article-firm-quarters (26.2%). Appendix 4.5 provides information on the coding of *NewInfoDum* and *Critique*. For my test, I employ the following probit regression:

$$Enhance_{it} = \beta_0 + \beta_1 AbsDVA_{it} + \beta_2 AbsDVA_{it} * NegDVA_{it} + \beta_3 NegDVA_{it} + \beta_4 AggInfo_{it} + \beta_5 FirstPageMent_{it} + \beta_6 Pages_{it} + \beta_7 Quarter_{it} + \beta_8 TA_{it} + \varepsilon_{it} \quad (3)$$

where *Enhance* represents *NewInfoDum* or *Critique.* I use robust standard-errors that are clustered by firms (White 1980).

Panel A of Table 4.7 provides descriptive statistics on regression inputs, Panel B provides correlation coefficients, and Panel C provides results of the regression estimations. In Model 1, the coefficient on *AbsDVA* is significantly positive. This is consistent with the financial press being more likely to provide information beyond the information already released in firms' press releases for large positive DVAs. The insignificant joint coefficient of

*AbsDVA+AbsDVA*NegDVA* (p-value: 0.304) implies that this association, in contrast, does not hold for large negative DVAs. The significantly negative coefficient on *AggInfo* is consistent with the financial press being more likely to enhance DVA information when managers provide fewer DVA disclosures. The coefficient on *FirstPageMent* is insignificant which suggests that a prominent placement of DVAs in earnings press releases does not increase or decrease press' propensity to provide new information. The negative coefficient on *Pages* provides weak evidence that the press enhances information more frequently for less transparent firms. I rerun the estimation including firm indicators (Model 2). The sample size hereby decreases slightly. In this specification, the coefficient on *AbsDVA* is not significant. However, the coefficient on *AggInfo* retains its sign and significance. To ensure that the coefficient is not mainly driven by observations where managers already provided thorough information and the financial press therefore has little opportunities to add information, I re-estimate Model 1 and 2 excluding all observations where *AggInfo* is equal or above 5 (Model 3 and 4). For this reduced sample, I find the coefficients on my test variables unchanged. Model 5 presents results on the determinants of a critical tone by the financial press towards DVAs. As in Model 1, I find that the association between large DVAs and information enhancement only holds for positive DVAs, but not for negative DVAs. The significantly negative coefficient on *NegDVA* is in line with smaller positive DVAs being more likely to be critically covered by the press than smaller negative DVAs. The results hold when including firm indicators (Model 6).

Taken together, the findings on the determinants of DVA information enhancement by the financial press are consistent with the financial press providing additional DVA information when managers provide little information, in line with an information enhancement function of the press. As the press' tone seems to be more critical towards positive, income-improving DVAs relative to negative DVAs, this could potentially indicate that the press assumes a watchdog role regarding controversial DVA accounting and reporting, consistent with prior literature (Miller 2006, Koning et al. 2010).

Table 4.7 DVA Information Enhancement by the Financial Press

Panel A: Descriptive statistics for article-firm-quarter observations

	Mean	Std. Dev.	Min	Median	Max	N
NewInfoDum	0.208	0.407	0.000	0.000	1.000	202
Critique	0.262	0.441	0.000	0.000	1.000	202
AbsDVA	75.342	79.063	0.343	38.673	307.844	202
NegDVA	0.530	0.500	0.000	1.000	1.000	202
AggInfo	5.391	1.124	0.000	5.000	7.000	202
FirstPageMent	0.713	0.454	0.000	1.000	1.000	202
Pages	2.702	0.338	2.079	2.639	3.332	202
TA	14.159	0.466	13.347	14.444	14.717	202
Quarter	19.738	6.917	3.000	21.000	32.000	202

Panel B: Correlation coefficients for article-firm-quarter observations

N = 202	NewInfoDum	Critique	AbsDVA	NegDVA	AggInfo	FirstPage Ment	Pages	TA	Quarter
NewInfoDum	1								
Critique	**0.221**	1							
AbsDVA	-0.042	**0.139**	1						
NegDVA	-0.079	**-0.272**	-0.009	1					
Emp	**-0.461**	0.043	**0.219**	**0.187**	1				
FirstPageMent	**-0.376**	**-0.144**	**0.229**	**0.191**	**0.524**	1			
Pages	-0.087	0.130	0.111	0.070	0.117	-0.127	1		
TA	-0.051	**0.226**	-0.028	**-0.149**	**0.177**	**-0.316**	**0.713**	1	
Quarter	**-0.230**	**-0.188**	**-0.291**	0.112	**0.144**	**0.395**	**0.169**	-0.055	1

Panel C: Determinants of DVA information enhancement by the financial press

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	*NewInfoDum*	*NewInfoDum*	*NewInfoDum*	*NewInfoDum*	*Critique*	*Critique*
AbsDVA	0.002**	0.001	0.002*	0.002	0.002**	0.002***
	(1.9934)	(1.1125)	(1.7695)	(1.3868)	(2.2488)	(3.0366)
AbsDVA*NegDVA	0.001	-0.001	0.002	0.001	-0.002	-0.002
	(0.4157)	(-0.2933)	(0.8017)	(0.5334)	(-0.8233)	(-0.7695)
NegDVA	0.103	0.191	0.386	0.461	-0.602**	-0.616***
	(0.3402)	(0.5110)	(1.0985)	(0.9901)	(-2.3445)	(-2.9571)
AggInfo	-0.605***	-0.568**	-1.048***	-1.081***	0.164	0.133
	(-3.1699)	(-2.2866)	(-3.2112)	(-2.9642)	(0.8722)	(0.6279)
FirstPageMent	-0.594	-0.572	-0.515	-1.129	-0.267	-0.195
	(-1.4277)	(-1.0661)	(-0.7390)	(-1.2726)	(-1.0439)	(-0.9979)
Pages	-0.429*	-0.427	-0.300	-1.112	0.345	-0.079
	(-1.7171)	(-0.7782)	(-0.9706)	(-1.3871)	(0.8685)	(-0.1134)
Quarter	-0.019	-0.035	0.007	0.009	-0.027	-0.035**
	(-0.7849)	(-1.1855)	(0.1490)	(0.1715)	(-1.4721)	(-2.3263)
TA	-0.101	0.204	-0.810	-2.733	0.262	-1.978***
	(-0.4356)	(0.2318)	(-1.6220)	(-1.5449)	(0.7578)	(-8.0554)
Constant	5.441**	1.442	16.141**	44.968*	-5.300	26.465***
	(2.1957)	(0.1180)	(2.3054)	(1.7508)	(-1.3940)	(12.8178)
Firm Indicators	No	Yes	No	Yes	No	Yes

Table 4.7 (continued)

F-test: AbsDVA+ AbsDVA*NegDVA	0.304	0.817	0.100	0.092	0.890	0.889
Pseudo R^2	0.258	0.288	0.380	0.424	0.137	0.189
N	202	198	146	144	202	198

This table displays coefficient estimates from probit regression models. The underlying regression model is:

$$Enhance_{it} = \beta_0 + \beta_1 AbsDVA_{it} + \beta_2 AbsDVA_{it} * NegDVA_{it} + \beta_3 NegDVA_{it} + \beta_4 AggInfo_{it} + \beta_5 FirstPageMent_{it} + \beta_6 Pages_{it} + \beta_7 Quarter_{it} + \beta_8 TA_{it} + \varepsilon_{it}$$

NewInfoDum is a binary variable indicating that an article-firm-quarter provided DVA information beyond the information provided in the respective firm quarterly earnings press release. Critique is a binary variable indicating that an article-firm-quarter assumed a critical tone towards DVAs. The regression models have standard errors that are heteroskedasticity robust and clustered by firms. z-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. All F-tests are two-tailed. Definitions of variables are reported in Table 4.2, Panel B and C.

4.6 Conclusion

Based on an extensive sample of 353 firm-quarters and 202 article-firm-quarters of US financial firms that report DVAs between 2007 and 2014, I provide comprehensive descriptive evidence on the occurrence, magnitude, and reporting by managers and the financial press of controversial debt value adjustments due to a change in own credit risk (DVAs). I find that DVAs occur only for few but therefore the largest US banks who apply the fair value option for liabilities thoroughly. For these banks, I find that DVAs occur regularly and that DVA gains and DVA losses occur equally often and with comparable magnitude on average. I further find that managers provide DVA information in slightly more than half of their quarterly earnings press releases and scarcely provide directional DVA information that could help unravel DVAs' perceived "counterintuitivity". Consistent with claims from the public DVA debate, I find that managers provide specific DVA information more often for large positive DVAs relative to large negative DVAs. Also, I find weak evidence that managers provide more DVA relational information when they have strategic incentives to do so. Concerning financial press' DVA reporting, I find that the press potentially forms a counterweight to this behavior by covering large positive DVAs more often than negative DVAs. However, I also find evidence that press' decision to cover DVAs follows the emphasis that managers put on DVAs. Finally, I find that the financial press possibly enhances investors' understanding of DVAs by providing new DVA information when managers' DVA reporting is scarce and by assuming a critical tone especially towards positive, income-increasing DVAs. The evidence expands our knowledge of the properties and the informational environment of a novel accounting item that is target of a public debate.

My findings are subject to several limitations. First, the findings offer purely descriptive evidence and can therefore not be interpreted as causal relations. Second, there is no evidence of the "optimal" level of DVA information by managers or the financial press. Therefore, while the found reporting behavior is consistent with behavior from prior literature, it could still follow different incentives. Third, my text-based measures of DVA information underlie inherent limitations. Foremost, they are subjective by nature. Also, the aggregating measures (*AggInfo* and *NewInfoDum*) weigh pieces of information equally that are likely of different importance and do not account for the possibility that some pieces of information might be complements and others might be substitutes (Leuz and Wysocki 2016). Finally, DVAs are innately a small-sample story and while the firms in the sample arguably cover a large share of the financial market, the evidence in this paper only represents fifteen firms and their press coverage.

The found variation in DVA information provided by managers and the financial press provides avenues for future research. Specifically, a test of the role of DVA information from the two sources on financial markets' perception of DVAs should make a valuable contribution. The recent change in IFRS and US GAAP accounting regulation that shifts DVAs' recognition from net income to other comprehensive income could provide an interesting setting for such research.

4.7 Appendix

Appendix 4.1: Sample selection process for the base sample

To ensure a broad identification of DVA-reporting financial firms in the US in the sample period between 2007 and 2014, I combine two identification approaches from prior literature. First, following Cedergren et al. (2015), I use data from regulatory FRY9C reports as provided by the "Bank Regulatory Database - Bank Holding Companies". The inherent restriction of my sample to firms from the financial industry is consistent with prior literature on DVAs (Schneider and Tran 2015; Cedergren et al. 2015). I consider all 8,558 bank holding companies with available data for the sample period between 2007 and 2014. I require firms to file a non-zero amount for one or both of the following items at least once in the sample period: net gains (losses) on fair value liabilities (BHCKF553) and estimated net gains (losses) on fair value liabilities attributable to changes in instrument-specific credit risk (BHCKF554). I find that this is true for 94 firms. Because the regulatory data from FRY9C filings is possibly not fully compliant with US-GAAP, I require accounting data from 10-K filings. Therefore, I use the New York Fed link data to match the firms' RSSD IDs from the regulatory database with PERMCO identifiers.[22] This excludes 39 firms. I then match the retrieved PERMCOs with CIK identifiers through the "CRSP/Compustat Merged - Fundamentals Annual" database which excludes another two firms. For the remaining 53 firms, I collect all available 10-Q and 10-K filings from the SEC Edgar company filings database. Performing a thorough manual search of these filings, I find that ten of the firms report DVAs in the sample period.

Second, following Wu et al. (2016), I consider all 5,783 financial firms, i.e., firms with a SIC code starting with "6", that are covered by the "Compustat North America annual database" for fiscal years between 2007 and 2014. I identify 207 financial firms as adopters of the fair value option because they have "Adoption of Accounting Changes" (*accthcg*) equal to "FS159" in any year of the sample period. To exclude firms that did not elect the fair value option for liabilities, I eliminate 47 firms without any fair value liabilities (*tfvl*) in periods subsequent to the fair value adoption. I further eliminate 65 firms without fair value changes reported in earnings subsequent to their FVO adoption (*tfvce*) because potential DVAs are

[22] The data is available from http://www.newyorkfed.org/research/banking_research/datasets.html (Accessed 10 March 2017).

reflected in this item, among other fair value changes. Next, I eliminate 10 firms without CIK identifier (*cik*) and 31 firms whose 10-K and 10-Q filings I already searched in the first part of the identification process. For the remaining 54 firms, I collect all available 10-K filings in the sample period from the SEC Edgar company filings database. I search all collected filings for the terms "instrument-specific credit risk", "own credit", "own debt", "own liabilities", and "own spread". I find these terms in 10-K filings of 41 firms. I perform a thorough manual search of all available 10-Q and 10-K filings of these firms in the sample period. I hereby identify an additional six firms that report DVAs in the sample period.

To analyze managers' DVA reporting, I require the sixteen DVA-reporting firms to file 8-K reports with the SEC because quarterly earnings press releases are attached to these reports. This requirement eliminates one private firm from the sample that never filed 8-K reports. In total, I collect such quarterly press releases for 431 firm-quarters in the sample period. Finally, I exclude 78 firm-quarters without FVOL adoption. In conclusion, my sample consists of 353 firm-quarters in which DVA-reporting US financial firms adopted the FVOL and released a quarterly earnings press release. Table 4.1, Panel A summarizes the sample selection process.

Appendix 4.2: Example for the coding of DVA reporting measures in quarterly earnings press releases

The following example describes the coding of my DVA reporting measures *Ment, Sign, Amount, Due, Dir, Excl, Comment, AggInfo,* and *FirstPageMent* for the sample that relates to the third quarter of 2011 of Morgan Stanley. The displayed excerpt is taken from the respective quarterly earnings press release.

Morgan Stanley Reports Third Quarter 2011:

- **Net Revenues of $9.9 Billion; Income from Continuing Operations of $1.14 per Diluted Share**
- **Results Included Revenues of $3.4 Billion, or $1.12 per Diluted Share, from the Widening of Morgan Stanley's Debt-Related Credit Spreads**
- **Strong Performance in Equity Sales & Trading, Interest Rates and Commodities; Ranked #1 in Global Completed M&A; Net New Assets of $15.5 Billion in Global Wealth Management**

I find that this press release mentions DVAs (*Ment*), provides DVAs' sign (*Sign*), provides DVAs' amount (*Amount*), provides the information that DVAs stemmed from a change in debt value/credit risk (*Due*), and provides directional information, here: explains that credit spread widened, i.e. that the positive DVAs stemmed from an increase in own credit risk

(*Dir*). Accordingly, for this firm-quarter observation, I code the respective five binary variables as 1. I do not find non-GAAP figures excluding DVAs (*Excl*) or evaluative comments on DVAs (*Comment*) in this press release. Accordingly, I code these two binary variables as 0. *AggInfo* is equal to the sum of the seven binary variables. Therefore, I code it as 5 in this firm-quarter. The displayed excerpt is taken from the first page of the press release. Ergo, the first press release page provides DVA information and I consequently code *FirstPageMent* as 1 in this firm-quarter.

Appendix 4.3: Collection of press articles providing DVA information

To analyze the information content of financial press articles concerning DVAs, I link all DVA information from newspaper articles (excluding online contents) in the sample period between 2007 and 2014 to the 353 firm-quarters from the base sample.

To collect respective press articles, I pursue a twofold approach. First, I collect all articles by four influential daily newspapers with nationwide circulation that cover the 353 base sample firm-quarters (Fang and Peress 2009): the *Wall Street Journal*, *The New York Times*, *The Washington Post*, and *USA Today*. I collect *Wall Street Journal* articles from ProQuest and the remaining articles from Nexis. Following Engelberg and Parsons (2011), I define that an article "covers" a firm's quarterly earnings press release if the respective database indexes the article on day 0, 1, or 2 after the issuance of the firm's earnings press release and mentions the firm in the article's index. Performing an extensive manual search of the articles, I find 125 articles that cover the firm-quarters and contain DVA information.

Second, I conduct a search in all US newspapers included in the Nexis database. The Nexis database provides news and business information from a number of source. Specifically, I search the 205 US newspapers included in Nexis for all combinations of the keywords "debt/debit/credit", "value/valuation", and "adjustment/adjustments", as well as for "DVA", "CVA", and "own credit risk" in the sample period between 2007 and 2014. I additionally perform the same searches for the Wall Street Journal via ProQuest. Analyzing all found articles by hand, I identify an additional 56 articles containing information on DVAs. Consequently, in sum, I find a total of 181 articles with DVA information in the sample period. I exclude eight articles that contain general DVA information or cover non-US firms and which I therefore cannot link to one of the firm-quarters from the overall sample. The fact that I do not find articles about DVAs concerning US firms outside my overall

sample provides some reassurance on the completeness of the sample. In conclusion, I use 173 articles with DVA information for my tests, covering 75 distinct firm-quarters of the base sample. Table 4.1, Panel B provides an overview over the distribution of the articles over time and across newspapers.

Appendix 4.4: Evidence on the DVA debate

To provide insights in the DVA debate, I structure and present some of the narrative disclosures from the 173 articles containing DVA information that I found (see Appendix 4.3). I find that a very common way of journalists to deal out mild criticism of DVAs is to label DVAs mere "accounting items" with little relation to fundamental value creation (e.g. Gogoi 2009; Murakami Tse 2010; Craig 2011; New York Times). In addition or alternatively, journalists often refer to DVAs as "one-time" gains or losses that "boosted" respectively "hit" net income (e.g. Murakami Tse 2009; Healy and Story 2009; Schwartz 2011; Schwartz 2012). Increasing the degree of criticism, journalists often use nicknames for DVAs. For example, I find the expression "accounting quirk" in ten articles (e.g., Eavis 2009c; Landy 2011). Another frequent name for DVAs in press articles is "paper gains" (e.g., Eavis 2008; DeCambre 2012).

Many journalists explicitly state that DVAs can be "counterintuitive" (e.g. Eavis 2008; Landy 2009; Rapoport and Lucchetti 2011; Browdie 2012). Other journalists imply that DVAs' effects on net income can be perceived as confusing by refering to them as "weird results" (Phillips 2009), "ugly results" (Eavis 2009a), "obscuring" (Appelbaum 2009), "erratic" (Davis 2010), "noise" (Protess 2012), "a mirage" (Elstein 2012) "arcane" (Eavis 2015), or "fuzzy math" (Beck 2009; Dash 2009). Still other journalists rather target the accounting rule itsself by calling it "nonsensical" (Currie and Cox 2010) or "twisted" (Currie and Campbell 2010).

Some journalists or experts cited in press articles argue almost polemically against DVAs. They say that DVAs are "fundamentally unconscionable" (Landy 2009) and have a "perverse practical impact" (Reilly 2011). One journalist states after explaining DVAs "I am not making this up" (Weinreich 2012). Finally, one journalist of the Washington Post goes on a lenghty rant towards managers DVAs' reporting in quarterly earnings press releases (see Sloan 2013):

"To say that the Citi earnings release and supplementary data are complicated is as understated as calling Yankee closer Mariano Rivera a reasonably competent pitcher.

The headline on Citi's news release - the part of the package presumably aimed at a general audience - reads as follows: "Citigroup Reports Second Quarter 2013 Earnings per Share of $1.34; $1.25 Excluding CVA/DVA." There's a footnote after "CVA/DVA," and an explanation later on, but I still can't translate either term into language approaching English.

I'm not blaming Citi's public relations or investor relations people for distributing gibberish and sowing confusion. They do what they're supposed to do, and what their superiors (and legal departments) are willing to sign off on. And presumably what Wall Street is interested in seeing."

Finally, I find sixteen articles that detail the logic behind DVAs and thereby provide constructive criticism. The vast majority does so by explaining that as a consequence of, for example, an increase in own credit risk, banks book DVA gains because they could "theoretically buy the debt back at a lower cost" (Associated Press 2011). The journalists behind these articles argue that DVAs make "some sense" (Eavis 2008) "as they seem to offer a clearer picture of to the actual value of a company's liabilities" (Phillips 2009) and that the regulation "was well intended" as it was "designed to let banks show investors changes in the fair value" (Elstein 2012). Three journalists explicitly refer to the respective accounting standard, FAS 159 (Landy 2009; Enrich 2009; Browdie 2012). Five of the articles focus on DVAs as a main topic and provide a thorough DVA discussion. Still, most of these articles also contain negative DVA critique.

Appendix 4.5: Preparation of the article-firm-quarter observations sample and coding of financial press' information enhancement variables

To avoid a loss of information on DVAs from the articles, I link the 173 found articles to all firm-quarters about which they provide information. An example: The following excerpt is from an article in the newspaper "American Banker" from April 19, 2012.

"[Bank of America's] bottom-line results were skewed by a $4.8 billion accounting charge involving the performance of its debt, as well as from gains from equity investments and securities sales. Citigroup (NYSE:C) also reported profits that were similarly skewed by a negative debt-valuation adjustment."

For my sample, I code this article as two article-firm-quarter observations. One article-firm-quarter observation on Bank of America's first quarter of 2012 providing the DVA

information pieces *Ment*, *Sign*, and *Amount*, and a second article-firm-quarter observation on Citigroup's first quarter of 2012 providing the information *Ment* and *Sign*.

For each article-firm-quarter observation, I check whether all pieces of information provided by the press article are also provided in the respective firm-quarter's earnings press release or not. In the latter case, I code the variable *NewInfoDum* as 1 and otherwise as 0.

For the coding of *Critique*, I check whether an article's tone towards DVAs is critical. If so, I code *Critique* for the article-firm-quarter(s) that the article refers to as 1, otherwise as 0. For narrative examples of critical tone in financial press articles in the sample, see Appendix 4.4. Table 4.1, Panel C displays statistics on the linkage between the 173 used individual financial press articles and the 202 article-firm-quarters that constitute the sample of my tests of financial press' DVA information enhancement.

5 Conclusion of the thesis

5.1 Summary of major findings

Based on three empirical studies, this thesis aims to improve our understanding of the informational benefits of recent accounting regulation changes. It takes issue with the decision usefulness of recent regulation on an aggregate level by examining the market-wide consequences of the introduction of a whole accounting regime. It further takes issue with the decision usefulness of a specific regulation change on a detailed level, examining the consequences for the adopting firms and their shareholders. To measure informational benefits, I apply market-based measures (liquidity, value relevance, market pricing) and disclosure based measures (hand-collected disclosure content measures). In all three studies, I use archival data from financial information databases. In the second study, I additionally use hand-collected data from financial reports. In the third study, I further use hand-collected data from earnings press releases and financial press articles. In sum, the thesis contributes firstly to the literature of the intended and unintended consequences of mandatory IFRS adoption (Brueggemann et al. 2013; Christensen et al. 2013) and secondly to the literature on the informational properties of recently introduced controversial debt value adjustments due to a change in own credit risk (DVAs) (Cedergren et al. 2015; Schneider and Tran 2015). Thirdly, the thesis adds to the literature on the role of the financial press as a financial information intermediary (Bushee et al. 2010; Miller 2006).

Concerning the first stream of literature, the first study of the thesis finds systematic changes in the underlying sample composition in economies after mandatory adoption of IFRS and concurrent enforcement regulation. Specifically, it finds evidence that especially smaller, less liquid firms opt out of the market or refrain from opting in, potentially due to costs that are associated with the regulation mandate. The study further introduces a Selection Exposure Index that measures a country's proneness to changes in the underlying sample composition. Finally, the study finds that the Selection Exposure Index is able to explain post-regulation liquidity effects beyond the explanatory variables from prior literature of this stream. The study's findings possibly reconcile so far inconclusive evidence on the informational benefits of mandatory IFRS adoption (Brueggemann et al. 2013). Specifically, the findings suggest that a part of the liquidity benefits after mandatory IFRS adoption found by prior literature (Daske et al. 2008) is not attributable to higher decision

usefulness of IFRS financial reporting but to less-liquid firms experiencing stronger pressure to leave regulated markets and therewith, the sample of such studies.

Concerning the second stream of literature, the second study of the thesis finds that the proportion and reliability of related fair value assets moderates the value relevance, the market pricing, and the persistence of controversial debt valuation adjustments. Specifically, the study finds that large proportions of related fair value assets are only associated with value relevant DVAs if the assets are measured reliably, i.e. reflect quoted market prices. Furthermore, large proportions of unreliably measured related fair value assets are associated with a too conservative market pricing of DVAs by investors. Finally, I find that DVAs' persistence is associated with large proportions of fair values whose measurement allows for managerial discretion. The findings are consistent with controversial DVAs having desirable informational properties mainly when investors are able to transparently assess the sources of the underlying changes in credit risk.

Contributing to the second and third stream of literature, the third study of the thesis finds that managers provide more DVA relational information in quarterly earnings press releases when DVAs are negative and when opportunistic incentives motivate them to do so. The findings are consistent with a strategy of managers to emphasize and explain DVAs that decrease net income but to be relatively more silent about DVAs when they improve net income. The study additionally finds that more DVA relational information by managers is associated with a higher chance of the DVAs being covered by the financial press. This is consistent with the press picking up some of managers' reporting spin. The study further finds that the financial press is more likely to provide new DVA information the less DVA information managers provide. Finally, a critical tone towards DVAs in financial press articles is more likely for positive DVAs than for negative DVAs. This finding is consistent with the financial press assuming a critical stance against DVAs. In conclusion, the findings imply that concerns from the public DVA debate about managers' imbalanced DVA reporting could be warranted. At the same time, the findings are in line with the financial press fulfilling an information dissemination role and an information enhancement role regarding DVA information.

Taken together, the findings of the thesis stress the importance of empirical research evidence for the assessment of accounting regulation quality and evidence based accounting

regulation (Leuz and Wysocki 2016). Therefore, they might be insightful for accounting standard setters. For example, the regulatory costs of IFRS implied by our findings and their impact on financial markets might be of relevance to the IASB in their further development of the standards. Also, IASB and FASB might take interest in the informational properties of DVAs whose accounting regulation both recently changed (FASB 2016; IASB 2014b). Also, the findings could be of interest for governmental bodies that are in charge of a potential adoption of IFRS, or of adoption of stricter accounting enforcement regulation, or of disclosure regulation in quarterly earnings press releases. The findings might also be of interest to a broader general public as both, IFRS and DVA accounting, are currently discussed with some controversy (Christensen et al. 2013; IASB 2009). Finally, the findings could be relevant for academics. For example, the inclusion of the Selection Exposure Index from the first study should be able to refine results on informational benefits of mandatory IFRS adoption in future studies. I point out some further avenues for future research in Section 5.3. While an impact of this thesis' findings on the opinions or actions of individuals or interest groups is far from certain, their preparation, writing down, and publication introduces them in the global market for information and might prove decision useful.

5.2 Limitations of the thesis

As pointed out in the respective sections of the studies, there are several limitations to this thesis' findings. First and foremost, the associations found in this study are mostly descriptive and therefore stand for themselves instead of establishing causal links. In the first study, for example, we observe that liquidity improvements in IFRS countries are moderated by changes in the underlying sample composition. However, we are unable to ascertain that the underlying changes in the sample composition reflect different exposure between countries to IFRS and enforcement cost. Furthermore, we are only able to show that, for example, a high decrease in the underlying country sample and improvements in liquidity are correlated in the sample, but so far do not causally connect the sample changes and the liquidity changes.

Similarly, in the second study, omitted variables could drive both, a firm's tendency to increase the proportion of reliably measured fair value assets and the value relevance of DVAs. For the third study, while my findings show an asymmetric reporting pattern by managers between positive and negative DVAs, I am unable to explain the reasons for the pattern. Therefore, whether

this is an attempt by managers "to trick the media and investors" (Milstead 2012) or rather an attempt to convey private information is beyond the scope of the paper. The same is true for the potential consequences of this reporting behavior apart from the associations with financial press reporting.

Additionally, the validity of my findings relies on the validity of the used measures and proxies. While the measures from the first two studies are relatively well-established in the literature, my disclosure and information measures in the third study are grounded in theory but novel. Therefore, several concerns apply to them. For example, my measure for DVA relational information in press releases and the financial press aggregates a set of DVA disclosures whose relevance I cannot objectively verify. Also, some DVA disclosures could be complements or substitutes (Leuz and Wysocki 2016). For example, if investors perceived a non-GAAP earnings figure excluding DVAs as a substitute for all additional DVA information, then a measure of DVA information should not increase with any information that is provided beyond such a non-GAAP figure. My measure does not account for this possibility.

5.3 Avenues for future research

The findings of this thesis point to several opportunities for future research. The first study argues that the increased regulatory costs, for example, through mandatory IFRS adoption and stricter accounting enforcement systematically influenced firms' decision to opt-out of the regulated market respectively to abandon IFRS. However, the literature that provides evidence on such effects on a country level is still in its infancy (e.g. Hitz and Mueller-Bloch 2016; Fiechter et al. 2016). Considering the worldwide IFRS adoption and therewith the diversity of economies that adopted IFRS and are potentially prone to such selection effects, this stream of literature could provide many new insights.

Furthermore, the findings of the first study indicate that research on the potential channels of liquidity benefits through IFRS adoption could provide further interesting results. For example, findings from prior literature do not rule out that positive IFRS adoption effects could partly be driven by a higher demand for professionally trained accountants and auditors or a better allocation

of such professionals' service through the reduction of economic mobility barriers (Bloomfield et al. 2017)

Future research could also consider combining the research settings of the second and the third study to enhance our understanding of DVAs. As mentioned in Section 5.2, the reasons for managers' asymmetric reporting of positive and negative DVAs are unclear. An analysis of DVAs' perception by financial markets depending on managers' DVA reporting might be able to verify whether managers potentially emphasize DVAs that provide more informational benefits. Also, such research could test whether variations in the reporting of managers and the financial press influence investors' perception of DVAs.

While the third study examines short-term consequences of managers' DVA reporting on the financial press DVA reporting, an examination of the reverse direction could yield interesting insights, too. Specifically, testing if critical DVA reporting by the financial press has a medium- or long-term impact on managers' DVA disclosures, as prior literature finds for the reporting of pro forma earnings (Koning et al. 2010), would make a potentially interesting contribution to the so far thin literature of the financial press as a "watchdog" for good accounting and corporate governance practice (Miller 2006; Dyck et al. 2008).

6 References

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7 Eidesstattliche Versicherung

Ich versichere an Eides Statt, dass ich die eingereichte Dissertation “Essays on the informational benefits of accounting standards for listed firms” selbstständig verfasst habe. Anderer als der von mir angegebenen Hilfsmittel und Schriften habe ich mich nicht bedient. Alle wörtlich oder sinngemäß den Schriften anderer Autorinnen und/oder Autoren entnommenen Stellen habe ich kenntlich gemacht.

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Ort, Datum Unterschrift