

Herkenhoff, Manuel; Nienhaus, Martin

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## ARTICLE

# Investor overreactions to transnational peer firm earnings: The role of accounting standards

Manuel Herkenhoff<sup>1</sup> | Martin Nienhaus<sup>2</sup>

<sup>1</sup>Chair of International Accounting, University of Münster, Münster, Germany

<sup>2</sup>Department of Financial Accounting, Ruhr University Bochum, Bochum, Germany

## Correspondence

Martin Nienhaus, Department of Financial Accounting, Ruhr University Bochum, Universitätsstraße 150, 44801 Bochum, Germany.

Email: [martin.nienhaus@rub.de](mailto:martin.nienhaus@rub.de)

## Abstract

This study finds that accounting standards play an important role in cross-border investor reactions to peer firm earnings. Specifically, we document that when international peer firms report under the same accounting standards, investors overreact to peer firms' earnings announcements. Using a sample of 35,116 firm-pair-years from 51 countries between 2000 and 2010, we show that heightened information transfers for international same-standard firms are followed by predictable price reversals when investors observe own-firm earnings. However, overreactions are not present for international firm-pairs that follow different accounting standards. While we find that institutional investors learn over time, overreactions do not decline among retail investors. Additional tests suggest that overreactions cause significant excess volatility, which results in economically significant costs. Collectively, our findings document an unintended consequence of financial reporting harmonization in the form of increased investor overreactions.

## KEYWORDS

accounting standards, international information transfers, overreactions

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## Réactions excessives des investisseurs aux résultats des entreprises homologues transnationales : le rôle des normes comptables

### Résumé

Cette étude met en lumière l'influence significative des normes comptables sur les réactions des investisseurs transfrontaliers aux résultats des entreprises homologues. Plus précisément, les auteurs démontrent que lorsque des entreprises homologues internationales publient leurs résultats selon les mêmes normes comptables, les investisseurs réagissent de façon excessive aux annonces de résultats des entreprises homologues. L'analyse d'un échantillon de 35 116 paires d'entreprises-années provenant de 51 pays entre 2000 et 2010 révèle que l'augmentation des transferts d'informations entre les entreprises internationales qui appliquent des normes communes est suivie de renversements prévisibles des cours, en réponse aux résultats observés par les investisseurs concernant leur propre entreprise. Cependant, les réactions excessives ne se produisent pas pour les paires d'entreprises internationales qui suivent des normes comptables différentes. Alors que les investisseurs institutionnels acquièrent de l'expérience au fil du temps, les réactions excessives continuent de se produire chez les investisseurs particuliers. Des tests supplémentaires indiquent que les réactions excessives entraînent une grande volatilité excessive, ce qui engendre des coûts importants sur le plan économique. En somme, les résultats démontrent que l'harmonisation de l'information financière a pour conséquence inattendue de susciter davantage de réactions excessives de la part des investisseurs.

### MOTS-CLÉS

normes comptables, réactions excessives, transferts d'informations au plan international

## 1 | INTRODUCTION

A large stream of prior research beginning with Foster (1981) shows that one firm's earnings announcement is a valuable information signal for other firms' investors from the same industry (i.e., information transfers). However, later research by Thomas and Zhang (2008) indicates that investors may systematically misjudge these signals and overestimate their relevance (i.e., overreactions). While our understanding of the initial information transfer among industry peers is substantial, we know little about overreactions beyond the analysis of US domestic overreactions in Thomas and Zhang (2008) and Bhojraj et al. (2020), and the role of reporting frequency in Arif and De George (2020). Hence, in this study, we leverage a cross-country setting to better understand the role of investor misjudgments and overreactions to peer firm earnings.

Focusing on a cross-country setting and the value of *foreign* earnings signals is important, both because capital markets are global and cross-border capital flows are significant for

developed countries. Moreover, using a cross-country setting allows us to analyze a potentially important factor in the value of foreign earnings signals, that is, accounting standards. Prior research already shows that when two firms use the same accounting standards (i.e., financial reporting harmonization), cross-country information transfers to foreign earnings increase (Brochet et al., 2013; Cascino & Gassen, 2015; Wang, 2014; Yip & Young, 2012). We conjecture that accounting standards play an equally important role in the potential reversal and thus in overreactions to foreign peer earnings signals.

The starting point for our conjecture is similar to the theory on overreactions in a purely domestic setting, as in Thomas and Zhang (2008). Specifically, we use a simple theoretical framework in which investors observe own-firm signals, peer firm signals, and other information signals to assess firm values. Next, we introduce two behavioral theories, the representative heuristic (e.g., Barberis et al., 1998; Tversky & Kahneman, 1974) and overconfidence (e.g., Hirshleifer, 2001; Lichtenstein et al., 1982). According to the representative heuristic, investors consider earnings news from the same industry as representative of the upcoming earnings news of the firms they are invested in, thereby ignoring common laws of probability. Moreover, overconfidence-based asset pricing models suggest that low-precision signals, such as peer firms' earnings, increase investors' overconfidence in their private information (e.g., Daniel et al., 1998, 2002) because people are generally overconfident, particularly concerning the precision of their knowledge (Odean, 1998). This overconfidence causes the overweighting of peer firm signals for own-firm valuation, and investors then start to overreact.

Expanding this framework to a cross-country setting probably lowers the precision of the peer firm signal. Specifically, the underlying covariance of cash flows between foreign peer firms is likely to be lower, and uncertainty about this is higher due to fundamental differences in firms and their operating environments. When introducing accounting standards, which can either be the same for two foreign firms (i.e., same standards) or different, to the cross-border setting with less precise signals, we will most likely see an exacerbation of the two behavioral biases mentioned before. Specifically, with financial reporting harmonization, the group of stocks that can be considered as representative widens—although their signal is less precise, thereby expanding this bias to foreign stocks. Moreover, peer firm signals of formerly foreign-GAAP reporters may become usable, and when investors start to rely on these less precise peer firm signals after harmonization, overconfidence-based overreactions occur.

Furthermore, a potential difference between actual and perceived comparability from same-standards peers may exacerbate both biases. Generally, investors welcome financial harmonization because they expect comparability benefits (e.g., Armstrong et al., 2010; Joos & Leung, 2013) and regulators motivate their harmonization efforts with comparability improvements (e.g., European Commission, 2015; SEC, 2010). However, the harmonization of accounting standards neither creates identical rules nor prevents differences in firm-level adoption. For example, many countries and jurisdictions that have adopted IFRS have carve-outs of certain rules or additional country-specific reporting requirements (Hail et al., 2010a, 2010b; SEC, 2012). Additionally, even if rules were identical, firms may adopt them on paper without serious application or quality improvements (Daske et al., 2013). Prior research also casts doubt on whether widespread comparability or network benefits follow IFRS mandates (Christensen et al., 2013). Moreover, other firm- and country-specific factors and institutional incentives shape financial reporting decisions. Thereby, the change in financial reporting standards may have little impact on actual reporting outcomes. Collectively, investors may have difficulty assessing the true extent of comparability that is achieved, because many of the factors mentioned before are unobservable, and thus investors may overestimate the comparability benefits of same-standards earnings.

To empirically test the hypothesis of overreactions, we generally follow Thomas and Zhang (2008), in which overreactions to peer firm news manifest themselves in observable stock-price patterns. Using cross-country firm-pairs, we regress the 3-day abnormal returns of the same

late-announcing firm  $i$  in response to their own earnings release on the 3-day abnormal return of late-announcing firm  $i$  in response to earlier announcing industry bellwether  $k$ 's earnings release. If investors of firm  $i$  respond appropriately to bellwether peer firms' earnings signals, no predictable price reaction should exist when firm  $i$  subsequently reports its own earnings. However, if investors overreact to bellwether peer firm signals, we expect a negative correlation between the two market responses for firm  $i$ , implying that the overreaction is subsequently corrected (Thomas & Zhang, 2008).

In the empirical implementation of our tests, we exploit the ongoing global convergence toward IFRS. The IFRS adoption setting provides a powerful treatment because the entire financial reporting system is harmonized. Moreover, thousands of firms adopted IFRS during our sample period, resulting in an increase from 17.4% of the sample firms using IFRS in 2000 to 68.9% in 2010. Hence, we have substantial variation in our treatment variable *Same\_standards*, which indicates whether firm-pairs use the same reporting standards. Based on this variable, we test whether the degree of overreaction differs for firm-pairs with harmonized financial reporting, compared with those using different accounting standards.

Our sample includes 3,068 annual earnings announcements with fiscal year-ends on December 31 from bellwether firms and 12,078 matched peer firm-years from 51 countries during 2000–2010. First, we corroborate the findings of Wang (2014) and confirm that the international harmonization of financial reporting is associated with stronger transnational information spillovers. Second, we validate the applicability of our overreaction measurement in the international context and show that overreactions exist *domestically* within countries other than the United States (Thomas & Zhang, 2008). In the main analysis, we do not find overreactions in a *transnational* context for firm-pairs using different accounting standards. However, consistent with our conjecture, we find statistically significant overreactions for transnational firm-pairs that follow the same accounting standards.

The overreactions we document are also economically meaningful. The magnitude of the price reversals due to the harmonization of accounting standards *across* countries is approximately 50% larger than the *domestic* overreaction within countries for firms using the same accounting standards. Moreover, the average peer firm in our subsample of same-standards firm-pairs (i.e., firm-pairs that follow the same accounting standards) experiences “unnecessary” firm-value movements of 0.1%. We also find that same-standards overreactions are associated with periods of elevated volatility. Peer firms with the same accounting standards as bellwether firms show excess volatility during earnings announcement periods that is approximately 80% higher than the excess volatility of peer firms reacting to non-harmonized bellwether earnings during announcements.

We also find that international firm-pairs with similar but not identical accounting standards show increased information spillovers. However, the heightened information spillovers for these firms are not associated with overreactions. Hence, the label of reporting under identical standards seems to be a prerequisite for international overreactions, which is consistent with our theory.

Further tests show that overreactions are stronger for firms with a greater fraction of retail investors compared to institutional investors, which is consistent with the notion that institutional investors are less prone to behavioral bias and less susceptible to misinterpretations of comparability benefits. Moreover, overreactions are significantly stronger when short-selling restrictions are in place. Hence, institutional investors seem to correct retail investor-driven mispricing, but this potential corrective force is limited if trading restrictions exist.

Next, we test whether harmonization-driven overreactions are temporary or form a sustainable equilibrium. In line with the notion that rational investors update their beliefs about the precision of peer firm signals after observing them, we find that learning over time takes place. Stocks with a high fraction of institutional investors show a significant attenuation of the initial overreaction over time. Economically, it takes on average 3.2 years after harmonization for the

overreactions to disappear. The learning effect is, however, statistically insignificant for stocks predominantly owned by retail investors.

Finally, we conduct a variety of robustness tests. For example, our results are robust to using a difference-in-differences (DiD) design similar to Wang (2014), by exploiting the fact that many firms have voluntarily adopted IFRS before country-level mandatory adoption. Moreover, we show that the occurrence of overreactions does *not* depend mechanically on the magnitude of initial spillover effects. Instead of an axiomatic relationship between spillovers and overreactions, our results imply that specific frictions drive the overreactions in a transnational context, such as the perceived similarity in accounting standards. Furthermore, we show that cross-listings do not provide an alternative explanation for our findings. Finally, we provide specification curve analyses in the Supporting Information that document the sensitivity of our results regarding our research design choices.

Our main contribution is to the literature on transnational investor reactions and the transmission of earnings signals worldwide, which is an important topic for a globally intertwined economy. While investor overreactions to peer firm disclosures have hitherto only been documented in the domestic US context (Thomas & Zhang, 2008), or for interim reporting (Arif & De George, 2020), our study is the first to show systematic overreactions to annual earnings announcements in a transnational setting. Such overreactions exist only in the transnational context for peer firm disclosures based on the same accounting standards. Hence, accounting standards seem to play an important role in how investors process international earnings signals. Our results also imply that transnational overreactions are not generally axiomatic to spillovers. Rather, behavioral bias drives overreactions in the international context. We also provide new evidence on the role of institutional investors in the information-transfer literature. Our results suggest that institutional investors are an important corrective force for overreactions, and market restrictions such as short-selling limitations impede their corrective ability.

Our results also contribute to the rich literature on the consequences of financial reporting harmonization. To the best of our knowledge, our study is the first to document an unintended consequence in the form of increased investor overreactions due to harmonization, which ultimately causes excess volatility. Our additional analyses suggest that retail investors ultimately bear the costs of these overreactions. However, while our study highlights a potential non-negligible negative consequence of harmonization, other benefits and costs should also be considered when analyzing the net effects of financial reporting harmonization.

Collectively, our findings shed light on factors that moderate and enable the unique phenomenon of investors' overreactions, which is important to understand, because overreactions potentially impede the efficient allocation of capital—especially in a transnational context.

## 2 | THEORETICAL FRAMEWORK AND HYPOTHESIS

Similar to the theory on overreactions in a purely domestic setting, we rely on a simple theoretical framework in which investors observe own-firm signals, peer firm signals, and other information signals to assess firm values. The usefulness of each signal depends on its precision, where the precision of an information signal increases with the quality of information, reducing uncertainty about the distributional characteristics of an uncertain event (e.g., the variance of a firm's expected future cash flows) (Verrecchia, 1990). We assume that firms' own signals are generally more precise for own-firm valuation than peer firm signals.<sup>1</sup> Moreover, the actual use

<sup>1</sup>We acknowledge that this notion may not apply to all firms. For example, new market entrants or high-tech firms can provide signals that are noisier and less useful for valuation than industry averages. Our results and inferences, however, are insensitive to the exclusion or inclusion of high-tech firms (following the definition of Kile and Phillips (2009)), and those within the first 3 years following their IPO.



of signals for valuation depends on the costs of processing the signal and the level of precision perceived by investors, which may deviate from the underlying objective precision.

Next, we introduce two behavioral theories to the domestic setting, that is, the representative heuristic (e.g., Barberis et al., 1998; Tversky & Kahneman, 1974) and overconfidence (e.g., Daniel et al., 1998, 2002; Hirshleifer, 2001). The representative heuristic builds on investor sentiment, which seeks to explain an observable pattern in which the correlation between two assets goes beyond the correlation in the changes in the assets' fundamental values. A common explanation of investor sentiment is the representative heuristic (Barberis et al., 1998; Tversky & Kahneman, 1974), which describes the tendency of individuals to view certain events as representative of a specific class. In this regard, investors consider earnings news from the same industry as representative of the upcoming earnings news of the firms in which they are invested, thereby ignoring common laws of probability.

Moreover, the second behavioral explanation for overreactions builds on overconfidence-based asset pricing models. These models predict that low-precision signals, such as peer firms' earnings, increase investors' overconfidence in their private<sup>2</sup> information (Daniel et al., 1998, 2002; Hirshleifer, 2001; Odean, 1998), which may cause an undue weighting of peer firm signals for own-firm valuation. This notion is consistent with the findings of the overconfidence literature that overconfident investors trade excessively, which is detrimental to their wealth (e.g., Barber & Odean, 2000, 2001).

To derive our hypothesis, we expand this basic framework to the international setting. Predicting that *information spillovers* between international peer firms increase after financial reporting harmonization is straightforward within this framework. In a classic rational setting such as Grossmann and Stiglitz (1980), harmonization decreases information-processing costs, which in turn increases the likelihood that investors incur these costs in order to process the signal.<sup>3</sup> Specifically, harmonization decreases acquisition costs and integration costs in the sense of Blankespoor et al. (2020), because formats are similar and fewer accounting adjustments are necessary to compare financial statements across firms under the same standards. Moreover, the decreased need for accounting adjustments probably reduces the risk of mispricing. Hence, investors will increasingly process these signals and foster more spillovers.

However, to enable the prediction that investors misjudge signals in the sense of *overreactions* that go beyond what we can observe domestically requires a more nuanced discussion. First, in an international setting, the precision of the peer firm signal is likely to decrease, because the underlying covariance of cash flows between foreign peer firms is probably lower, and uncertainty about this is higher due to fundamental differences in firms and their operating environments. This potential decrease in precision does not per se warrant overreactions. However, we next introduce accounting standards to this setting, which can either be the same for two foreign firms (i.e., harmonization; same standards) or different.

When peer firms start to report using the same accounting standards, the group of securities that can be considered as representative widens. Consequently, financial reporting harmonization may exacerbate the representative heuristic. The habitat model of Barberis et al. (2005) provides a related sentiment-based explanation. In the habitat model, some investors only trade a subset of available assets, that is, assets in their preferred habitat, due to transaction costs, international trading restrictions, or a lack of information. When investors' liquidity needs or risk aversion change, they alter their exposure to the securities only within their habitat. This change induces a common factor in the returns of these securities. With financial reporting harmonization, the group of securities that can potentially be part of an investor's habitat

<sup>2</sup>Note that while these signals stem from public disclosures such as a firm report or communication, the existence of information processing costs renders these signals private, and learning from public disclosures to generate private signals is a choice (Blankespoor et al., 2020).

<sup>3</sup>Rational inattention models such as Sims (2003) and Veldkamp (2011) allow for the same prediction based on alleviated processing capacity constraints.

increases, because information-processing costs decrease, and international barriers are reduced.

Similarly, the same accounting standards may be a prerequisite for investors to use peer signals, because they may not be confident using information based on foreign GAAP, or avoid information-gathering costs for foreign investments (e.g., Cooper & Kaplanis, 1986). This notion is also consistent with the fact that mutual funds significantly increase their foreign equity holdings after harmonization (DeFond et al., 2011), or the general findings of the home bias literature (e.g., Karolyi & Stulz, 2003; Lewis, 1999). The peer firm signals of formerly foreign-GAAP reporters may become usable after harmonization. Hence, when investors start to rely on peer firm signals after harmonization, overconfidence-based overreactions occur.

Moreover, the potential behavioral biases discussed before may be amplified by a difference between the *actual* degree of comparability achieved via harmonization and the *perceived* degree of comparability. We believe that several reasons exist why the perceived comparability may be higher, at least in the initial periods. First, while harmonization efforts are generally motivated by the hope of facilitating comparability (e.g., Armstrong et al., 2010), the harmonization of accounting standards across countries does not necessarily create an identical set of rules. For example, many of the more than 100 countries that have adopted IFRS have carve-outs of certain rules, or additional country-specific reporting requirements (Hail et al., 2010a). Additionally, even if rules were identical, firms may adopt them without serious application or with minimal or no quality improvements (Daske et al., 2013). Prior research also casts doubt on whether widespread comparability or network benefits follow IFRS mandates (Christensen et al., 2013). Moreover, many other firm- and country-specific factors shape financial reporting decisions. For example, prior research underlines the importance of firms' reporting incentives and institutional environments (e.g., Ball et al., 2000, 2003; Burgstahler et al., 2006; Leuz et al., 2003). Additionally, economic agents such as auditors play a role in shaping reporting outcomes (e.g., Francis et al., 2014; Mauritz et al., 2023). Thereby, the change in financial reporting standards may have only a small impact on actual reporting outcomes. Similarly, Zeff (2007) emphasizes that cultural differences impede worldwide financial statement comparability that can be potentially achieved through financial reporting harmonization. Hence, we believe that the perceived comparability from harmonization efforts is higher than the actual comparability.

Based on these frictions and the behavioral explanations above, we hypothesize:

**Hypothesis.** Investors overreact to the intra-industry information from foreign peer firms when firms start to report under the same accounting standards.

### 3 | RESEARCH DESIGN AND DATA

#### 3.1 | Identification strategy and empirical model

Our empirical strategy is twofold. First, we determine the market response of a late-announcing firm (i.e., peer firm  $i$ ) to the earnings release of an earlier-announcing firm in the same industry (i.e., bellwether firm  $k$ ) following a traditional earnings-information transfer research design (e.g., Dong & Young, 2021; Foster, 1981). Based on these information spillovers, we analyze the correlation between the market response of late-announcing peer firm  $i$  to its own earnings release and the initial reaction of the same peer firm  $i$  to the previous earnings announcement of bellwether  $k$ . A negative correlation implies a potential overreaction by investors to bellwether  $k$ 's announcement. This overreaction is subsequently corrected when peer firm  $i$  releases its own earnings.



Following prior research (e.g., Arif & De George, 2020; Kajüter et al., 2019; Wang, 2014), we focus on early earnings announcements made by the largest firms in each industry, as according to the market value of equity at the end of each fiscal year (i.e., industry bellwether firms).<sup>4</sup> Bellwether earnings announcements provide both a common information component that informs investors about industry fundamentals and information about competitive shifts in their industry, as they usually have a large market share (Arif & De George, 2020). Based on the size-related differential information hypothesis (Atiase, 1985; Bamber, 1987; Freeman, 1987), larger firms' earnings news are potentially related to information on the overall economy or industry, meaning they do not provide only firm-specific information. Additionally, larger firms can also serve as bellwethers, as they typically represent industry leaders with high visibility and tend to have shorter reporting lags. To determine bellwethers empirically, we rely on a two-step matching procedure used in prior research. First, we follow Thomas and Zhang (2008), and for each (late-announcing) firm we match all available earnings announcements by earlier-announcing firms in the same industry-year. Second, out of the resulting firm-pair list, we then only retain earlier announcements made by the three largest earlier-announcing firms (i.e., bellwether firms) similar to Wang (2014) and Arif and De George (2020).<sup>5</sup>

To gauge information spillovers, we model the market reaction for a late-announcing peer firm  $i$  with the concurrent abnormal return of an earlier-announcing bellwether firm  $k$  around the date of the bellwether earnings announcement. Specifically, we estimate the following regression at the firm-pair-year level:

$$\begin{aligned} Peer\_early\_car_{ik} = & \alpha + \gamma_1 BW\_early\_car_{ik} \\ & + \gamma_2 Same\_standards + \gamma_3 BW\_early\_car_{ik} \times Same\_standards \\ & + \sum \beta_n Controls_n + \sum \delta_n FE + \varepsilon, \end{aligned} \quad (1)$$

where  $Peer\_early\_car_{ik}$  is the 3-day cumulative abnormal return of the late-announcing peer firm  $i$  around the date of the respective industry bellwether  $k$ 's earnings announcement.  $BW\_early\_car_{ik}$  measures earnings news for early-announcing bellwether firm  $k$  as the cumulative abnormal return surrounding bellwether  $k$ 's earnings announcement. The corresponding coefficient captures the baseline intra-industry spillover effect.  $Same\_standards$  is an indicator variable equal to one when peer firm  $i$  and the respective industry bellwether  $k$  use the same reporting standards, and zero otherwise.<sup>6</sup> The coefficient on the interaction term  $BW\_early\_car_{ik} \times Same\_standards$  captures the incremental information transfer related to financial reporting harmonization. Consistent with the results of Wang (2014), we predict this incremental information-transfer coefficient to be statistically significant and positive.

Next, we examine a potential investor overreaction to the initial information-transfer effects. Specifically, we model the corresponding market reaction for a late-announcing peer firm  $i$  around the release date of its earnings announcement with its previous abnormal return at the time of bellwether firm  $k$ 's earnings announcement. We estimate the following regression equation at the firm-pair-year level:

<sup>4</sup>The inferences are not sensitive to the focus on bellwethers, as the results are similar when we match each peer firm announcement to all other previous announcements in the same industry. Moreover, in Figure S-4 in the Supporting Information, we provide a specification curve analysis showing the results for any number of bellwethers between one and 100.

<sup>5</sup>The main results do not change if we define the three earliest-announcing firms for each industry-year as bellwethers and then match these announcements with all subsequent peer firm announcements in the following 60 days.

<sup>6</sup>We define the accounting standards variable using the Datatype WC07536 in Datastream ("Accounting Standards Followed") and follow Daske et al. (2013) in their coding of International Accounting Standards (IAS), US GAAP, and local GAAP.

$$\begin{aligned}
Peer\_late\_car_{ik} = & \alpha + \gamma_1 Peer\_early\_car_{ik} \\
& + \gamma_2 Same\_standards + \gamma_3 Peer\_early\_car_{ik} \times Same\_standards \\
& + \gamma_4 BW\_early\_car_{ik} + \gamma_5 BW\_late\_car_{ik} \\
& + \sum \beta_n Controls_n + \sum \delta_n FE + \varepsilon,
\end{aligned} \tag{2}$$

where  $Peer\_late\_car_{ik}$  is the 3-day cumulative abnormal return of late-announcing peer firm  $i$  around the date of its (late) earnings announcement. Coefficient  $\gamma_1$  captures the correlation between the market response of late-announcing peer firm  $i$  to the respective early-announcing bellwether firm  $k$ 's earnings and the market response to peer firm's  $i$  own earnings release. The interaction term,  $Peer\_early\_car_{ik} \times Same\_standards$ , captures the incremental abnormal return correlation related to the harmonization of financial reporting. In line with the notion that investor overreactions increase due to financial reporting harmonization, we predict this incremental abnormal return correlation coefficient to be statistically significant and negative.  $BW\_late\_car_{ik}$  measures the 3-day cumulative abnormal return of early-announcing bellwether firm  $k$  around the date of peer firm  $i$ 's (late) earnings announcement.

In both the spillover regression (Equation 1) and the overreaction regression (Equation 2), we control for a variety of factors at the firm-pair  $k-i$  level, bellwether  $k$  level, and firm  $i$  level, which may influence information transfers and the subsequent investor corrections. Our first group of controls includes variables at the firm-pair level. Specifically, we capture the relative timeliness of earnings information using *Reporting\_lag*, which measures the difference in calendar days between the early bellwether firm's earnings announcement and the late peer firm's earnings announcement. While a small timespan between the two announcements might influence the earnings news for both firms, a high reporting lag is expected to dilute a potential negative correlation between the market responses, as an increasing number of other firms in the same industry announce their earnings in the meantime.

Additionally, the earnings announcements of bellwether firms may provide information to investors about the components of their cash flows, which are also relevant for the assessment of peer firms' future cash flows. Following the literature, we rely on the stock return correlation between industry bellwethers and peer firms as a proxy for cash flow similarity (Erwin & Miller, 1998; Firth, 1996; Lang & Stulz, 1992). We measure *Corr* as the correlation between daily returns for each firm-pair over a period of 12 months prior to the corresponding bellwether announcement. We further control for different degrees of economic integration between bellwether and peer firms' home countries. Similar to Wang (2014), we measure *Trade* as the mutual export volume, scaled by the exports of both countries to the rest of the world.

The second group of controls includes variables at the level of both the bellwether and peer firms. *BW\_size* and *Peer\_size* control for the size of the sample firms, measured as the natural logarithm of the market value of equity at the fiscal year-end. *BW\_fsr* and *Peer\_fsr* capture the degree of internationalization by the respective firms' foreign sales ratios in each fiscal year. We also control for differences in ownership structures, measured by the percentages of closely held shares, *BW\_chshr* and *Peer\_chshr*. We further account for differences in growth by the book-to-market ratios (*BW\_bm* and *Peer\_bm*), calculated as the ratio of book value of equity to the market value of equity at the end of each fiscal year. *BW\_lev* and *Peer\_lev* control for different leverage ratios and are defined as total debt to total assets at the end of the fiscal year. Finally, we follow Hayn (1995), who documents that negative earnings have a lower information content and include loss indicator variables *BW\_loss* and *Peer\_loss*.

Additionally, we allow the coefficients on the control variables to vary, depending on whether the firm-pairs follow the same or different accounting standards. Specifically, we include interaction

terms of *Same\_standards* with all control variables.<sup>7</sup> We also use country, industry, year, and month fixed effects. Further, we include announcement-year-month-*k*-to-announcement-year-month-*i* fixed effects, which absorb any systematic differences between returns within all year-month combinations. We cluster standard errors at the country-pair level.

### 3.2 | Sample description

We use a global firm-year data set based on the intersection of I/B/E/S, Worldscope, and Datastream.<sup>8</sup> The initial sample includes all annual earnings announcements disclosed by non-US firms with fiscal year-ends on December 31 from January 1, 2000, until December 31, 2010. We center our sample period on the European Union (EU) mandatory IFRS adoption in 2005 to allow sufficient variation in the degree of financial reporting harmonization.<sup>9</sup> We retrieve announcement dates and analyst forecast data from I/B/E/S, and financial information and daily return data from Worldscope and Datastream. Based on the above bellwether definition, we construct a firm-pair sample in which we match every (late) earnings announcement of a peer firm to each of the respective earlier bellwether earnings announcements in the same industry-year using the Fama–French 30 industry classifications. In our main tests, we focus on *international* spillovers and overreactions, and thus only use transnational firm-pairs.

Following prior research, we make several adjustments so as to increase the power to detect potential spillovers and overreactions (Arif & De George, 2020; Foster, 1981; Wang, 2014). First, we exclude highly regulated industries (#20 Utilities and #29 Financials). Second, to ensure the international visibility and relevance of bellwether firm announcements and the dependence on international markets for peer firms, we only retain firm-year observations with nonzero foreign sales.

To ensure that firms are sufficiently traded by investors, we exclude illiquid stocks with more than 10 zero-return days in each year. For matched announcements, we further exclude firm-pairs whose matched announcements are released within 3 days of each other, to avoid a contamination of information-transfer effects. Additionally, we discard firm-pairs whose timespan between announcements exceeds 60 days, as a potential negative correlation between market responses may be diluted over time. Finally, following Wang (2014) and Arif and De George (2020), we ensure a minimum size of industries by eliminating firm-pairs in industry-years with fewer than 100 observations.<sup>10</sup>

These adjustments lead to a final sample of 3,068 earnings announcements of 986 individual bellwether firms. These early bellwether announcements are matched to 12,078 late earnings announcements of 2,897 individual peer firms. Our matching procedure results in a total of 35,116 international firm-pair-year observations. Panel A of Table 1 reports the development of the same accounting standards variable over time, as well as the sample composition by industry. In the years before the 2005 mandatory adoption by the EU, few firm-pairs had the same accounting standards. Afterwards, more than half of the observations in our matched sample use the same accounting standards. We report the sample composition by country in Panel B of Table 1. The 3,068 announcements from 986 industry leaders are concentrated in

<sup>7</sup>We provide coefficient estimates of the interactions with *Same\_standards* and our vector of controls in the Supporting Information.

<sup>8</sup>We choose a global sample to maximize variation in mandatory country-level adoption (dates). We acknowledge that our sample contains many smaller and nonimportant domestic capital markets that may not play an important role in a global context, and thus introduce noise to our analyses. To assess the sensitivity of this research design choice, we replicate our analyses based on a sample of (1) only countries that have at least one global industry leader in their market (i.e., bellwether firm), (2) only countries with at least 10 peer firms, and (3) firms from countries that eventually adopted IFRS at the country level. Untabulated results show that none of these specifications change our inferences.

<sup>9</sup>Our sample period begins with the fiscal years ending in 2000 and ends in 2010 because we use a symmetric 5-year window for the pre- and post-period surrounding the mandatory adoption in the EU in 2005. However, our results and inferences remain unchanged when we use different windows ranging from 2 to 5 years.

<sup>10</sup>Note that we provide specification curve analyses of these research design choices in the Supporting Information.

**TABLE 1** Sample composition by year, industry, and country.

<b>Panel A: Sample composition by year and industry</b>			
	<b>Firm-pair-years</b> <i>N</i>	<b>Same standards</b> <i>N</i>	<b>%</b>
<b>Year</b>			
2000	1,258	59	4.69
2001	1,840	108	5.87
2002	2,496	131	5.25
2003	2,479	126	5.08
2004	2,458	185	7.53
2005	3,159	1,581	50.05
2006	3,781	1,807	47.79
2007	4,296	2,298	53.49
2008	4,362	2,426	55.62
2009	4,237	2,166	51.12
2010	4,750	2,517	52.99
<b>Total</b>	<b>35,116</b>	<b>13,404</b>	<b>38.17</b>
<b>FF30 industry</b>			
Aircraft, ships, and trains	288	64	22.22
Apparel	548	307	56.02
Automobiles and trucks	1,277	444	34.77
Business equipment	5,736	1,115	19.44
Business supplies	893	473	52.97
Chemicals	1,095	470	42.92
Communication	1,130	552	48.85
Construction	3,631	1,308	36.02
Consumer goods	1,022	304	29.75
Electrical equipment	505	49	9.70
Food products	1,760	697	39.60
Games	567	223	39.33
Healthcare	1,747	840	48.08
Hospitality	571	106	18.56
Machinery	2,294	1,226	53.44
Mining	329	256	77.81
Other	475	338	71.16
Petroleum and gas	1,289	720	55.86
Printing and publishing	142	103	72.54
Retail	1,235	431	34.90
Services	4,832	1,953	40.42
Steel works	1,108	437	39.44
Transportation	1,396	521	37.32
Wholesale	1,246	467	37.48
<b>Total</b>	<b>35,116</b>	<b>13,404</b>	<b>38.17</b>

(Continues)

TABLE 1 (Continued)

Panel B: Sample composition by country					
Country	N	Bellwether announcements		Peer firm announcements	
		Individual firms	Firm-pair-years	Individual firms	Firm-pair-years
Argentina	0	0	0	3	15
Australia	43	15	445	39	424
Austria	43	16	135	32	466
Azerbaijan	0	0	0	1	3
Belgium	55	19	346	54	809
Bermuda	0	0	0	2	9
Brazil	31	11	236	30	212
Chile	11	6	116	11	63
China	119	55	418	148	1,354
Colombia	0	0	0	1	9
Cyprus	1	1	5	4	30
Czech Republic	0	0	0	1	15
Denmark	36	9	622	41	601
Egypt	0	0	0	1	6
Faroe Islands	0	0	0	1	9
Finland	196	48	2,937	95	1,504
France	416	106	6,073	262	4,109
Germany	316	96	3,440	252	3,953
Gibraltar	0	0	0	2	24
Greece	21	8	317	47	365
Hong Kong	167	51	1,015	166	2,077
Hungary	7	4	15	9	81
India	17	5	188	14	118
Indonesia	10	4	19	8	63
Ireland	24	7	457	27	299
Israel	20	4	192	26	261
Italy	137	44	1,020	128	1,736
Japan	99	29	1,027	48	632
Korea (South)	120	50	2,001	65	585
Luxembourg	12	5	203	11	156
Macau	1	1	11	1	3
Malaysia	29	15	183	113	1,110
Mexico	13	7	107	23	127
The Netherlands	111	39	1,654	101	1,195
New Zealand	6	2	66	3	29
Norway	107	27	1,022	104	1,234
Papua New Guinea	0	0	0	3	31
Peru	1	1	2	5	20
Philippines	10	5	24	9	111
Poland	17	8	52	35	281

TABLE 1 (Continued)

Panel B: Sample composition by country					
Country	N	Bellwether announcements		Peer firm announcements	
		Individual firms	Firm-pair-years	Individual firms	Firm-pair-years
Portugal	26	7	126	28	396
Russian Federation	13	9	60	10	45
Singapore	104	34	815	112	1,267
South Africa	23	8	159	14	144
Spain	122	27	1,681	72	1,116
Sweden	167	50	2,885	111	1,252
Switzerland	193	60	3,437	130	2,217
Taiwan	140	56	1,115	248	2,504
Thailand	11	3	61	18	115
Turkey	12	7	24	26	257
United Kingdom	61	27	405	202	1,674
<b>Total</b>	<b>3,068</b>	<b>986</b>	<b>35,116</b>	<b>2,897</b>	<b>35,116</b>

Note: This table illustrates the sample composition by country, year, and industry following the sample selection procedure. We provide an overview of the firm-pair distribution for each country-pair in the Supporting Information.

France (13.56%) and Germany (10.30%). Peer firms are domiciled in over 50 countries, with the largest concentration in France (9.04%), Germany (8.70%), and Taiwan (8.56%).<sup>11</sup>

Table 2 presents descriptive statistics. On average, the lag between the early bellwether firm announcement and the late peer firm announcement is 26 calendar days. Substantial differences between bellwether and peer firms exist, especially regarding size, which is consistent with our approach to identifying bellwether firms. Specifically, bellwether firms have a mean market capitalization of USD 11.0 billion, which, at USD 3.2 billion, is more than three times larger than that of peer firms. In addition, bellwether firms are less likely to announce negative earnings compared to their peer firms.

## 4 | MAIN RESULTS

In the main tests, we first validate the information *spillover* results of Wang (2014) based on our sample. This is important because information spillovers form the basis of any potential overreaction. Moreover, initial spillovers provide a reference point to assess the magnitude of overreactions. Next, we report the results of *overreactions*. In addition to international spillovers and overreactions, we also report *domestic* spillovers and overreactions, which provide a benchmark for the magnitude of any effects.

### 4.1 | Spillovers

Panel A of Table 3 presents the spillover results. Before we measure the interaction effect using our *Same\_standards* variable, which captures the differences between harmonized and non-

<sup>11</sup>To ensure that no specific country-pair drives our results, we repeated our main analyses, sequentially dropping the three largest country-pairs in terms of observations. However, the results and inferences (Table S-8 in the Supporting Information) are similar to our main specification.



TABLE 2 Descriptive statistics.

Variable	Mean	SD	P25	Median	P75
Firm-pair variables					
<i>Reporting_lag</i>	26.374	16.365	13.000	23.000	40.000
<i>Corr</i>	0.211	0.164	0.092	0.183	0.305
<i>Trade</i>	0.031	0.036	0.006	0.015	0.043
<i>Same_standards</i>	0.382	0.486	0.000	0.000	1.000
Bellwether firm variables					
<i>BW_late_car</i>	0.001	0.037	−0.018	0.000	0.019
<i>BW_early_car</i>	0.003	0.054	−0.025	0.001	0.029
<i>BW_size</i> (ln)	8.097	1.679	6.944	8.196	9.259
<i>BW_size</i> (million USD)	11,038	21,566	1,036	3,626	10,500
<i>BW_fsr</i>	55.020	28.390	31.400	56.730	78.590
<i>BW_chshr</i>	40.570	24.680	20.660	40.830	59.300
<i>BW_bm</i>	1.256	1.576	0.556	0.925	1.531
<i>BW_lev</i>	0.240	0.156	0.117	0.232	0.344
<i>BW_loss</i>	0.100	0.300	0.000	0.000	0.000
Peer firm variables					
<i>Peer_late_car</i>	0.0040	0.0600	−0.0270	0.0010	0.0320
<i>Peer_early_car</i>	0.0020	0.0410	−0.0190	0.0000	0.0230
<i>Peer_size</i> (ln)	6.311	1.791	5.037	6.149	7.478
<i>Peer_size</i> (million USD)	3,162	10,879	153	468	1,768
<i>Peer_fsr</i>	51.490	29.950	25.200	52.250	77.320
<i>Peer_chshr</i>	43.850	23.800	25.050	45.830	61.660
<i>Peer_bm</i>	1.513	2.726	0.620	1.044	1.739
<i>Peer_lev</i>	0.227	0.168	0.085	0.218	0.337
<i>Peer_loss</i>	0.179	0.383	0.000	0.000	0.000

Note: This table presents the summary statistics for the variables used in the transnational overreaction analyses. The number of observations for the variables representing peer and bellwether firms (*BW*) is 12,078 and 3,068, respectively. The number of observations for the variables at the firm-pair level is 35,116. Variable definitions are presented in the [Appendix](#). All continuous variables are winsorized at the 1st and 99th percentiles.

harmonized financial reporting standards, we estimate two benchmark models. In Column 1, we first report the results for domestic information spillovers. Our domestic sample includes 17,668 firm-pair observations, applying the sample adjustment procedures outlined above.<sup>12</sup> As predicted, and consistent with the results of prior research, we find a positive and significant coefficient on *BW\_early\_car* of 8.17% (*t*-value: 9.47). This coefficient is similar to the coefficient of 6.59% for the cumulative abnormal return correlation between domestic bellwether and peer firms reported by Wang (2014).

Next, we establish the baseline spillover level in the transnational sample in Column 2. Using 35,116 transnational firm-pair-years, we document a similar, albeit weaker, *international* information-transfer effect compared to our *domestic* benchmark test (*BW\_early\_car*: 3.85%; *t*-value: 5.42). The effect is again similar in magnitude to the results from prior research, which

<sup>12</sup>For the domestic sample, we relax the requirement of a minimum of 100 firm-pair observations per industry-year, because this requirement would overly restrict the domestic sample size.

**TABLE 3** Multivariate analysis of transnational information transfers.

<b>Panel A: Spillover research design</b>			
<b>DV = <i>Peer_early_car</i></b>	<b>Domestic sample</b>	<b>Transnational sample</b>	
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>
<i>BW_early_car</i>	0.0817*** (9.47)	0.0384*** (5.42)	0.0288*** (2.94)
<i>Same_standards</i>			0.0036 (0.65)
<i>BW_early_car</i> × <i>Same_standards</i>			0.0265** (2.04)
<i>Reporting_lag</i>	0.0000 (0.10)	0.0000 (−0.65)	−0.0001** (−2.07)
<i>Corr</i>	−0.0024 (−0.73)	−0.0065** (−2.55)	−0.0034 (−0.89)
<i>Trade</i>		−0.0067 (−0.77)	−0.0070 (−0.64)
<i>BW_size</i>	−0.0004 (−1.33)	0.0004 (1.21)	0.0005 (1.32)
<i>BW_fsr</i>	0.0000 (0.20)	0.0000* (−1.70)	0.0000 (−1.30)
<i>BW_chshr</i>	0.0000 (−0.96)	0.0000 (−1.20)	0.0000 (−1.10)
<i>BW_bm</i>	−0.0008 (−1.63)	0.0001 (0.31)	0.0005 (0.86)
<i>BW_lev</i>	−0.0006 (−0.18)	0.0056** (2.36)	0.0082*** (2.95)
<i>BW_loss</i>	−0.0013 (−0.87)	0.0012 (1.02)	0.0026** (2.09)
<i>Peer_size</i>	0.0002 (0.83)	−0.0002 (−1.14)	−0.0002 (−1.10)
<i>Peer_fsr</i>	0.0000 (−0.08)	0.0000 (−0.40)	0.0000 (−0.82)
<i>Peer_chshr</i>	0.0000 (0.08)	0.0000 (−1.15)	0.0000 (−1.28)
<i>Peer_bm</i>	0.0011*** (3.34)	0.0001 (0.50)	0.0001 (0.48)
<i>Peer_lev</i>	−0.0031 (−1.30)	−0.0053*** (−3.45)	−0.0042** (−2.11)
<i>Peer_loss</i>	−0.0063*** (−6.26)	−0.0043*** (−5.95)	−0.0050*** (−5.56)
Controls× <i>Same_standards</i>	No	No	Yes
FEs: country ( <i>BW</i> ), industry, year, month, and month-year to month-year	Yes	Yes	Yes
Observations	17,668	35,116	35,116
Adj. $R^2$	0.0620	0.0390	0.0400

## Panel B: Overreaction research design

DV = <i>Peer_late_car</i>	Domestic sample (1)	Transnational sample	
		(2)	(3)
<i>Peer_early_car</i>	-0.0302** (-1.99)	-0.0177* (-1.65)	-0.0026 (-0.21)
<i>Same_standards</i>			-0.0010 (-0.14)
<i>Peer_early_car</i> × <i>Same_standards</i>			-0.0429** (-2.19)
<i>Reporting_lag</i>	0.0000 (-0.77)	-0.0001** (-2.46)	-0.0001*** (-2.75)
<i>Corr</i>	-0.0087* (-1.73)	-0.0040 (-1.33)	-0.0008 (-0.17)
<i>Trade</i>		0.0510*** (3.10)	0.0297* (1.73)
<i>BW_early_car</i>	-0.0133 (-1.40)	-0.0101 (-1.36)	-0.0035 (-0.41)
<i>BW_late_car</i>	0.1686*** (11.50)	0.0345*** (2.68)	0.0324** (1.99)
<i>BW_size</i>	-0.0004 (-1.13)	0.0003 (0.67)	0.0002 (0.50)
<i>BW_fsr</i>	0.0000 (-0.41)	0.0000 (0.61)	0.0000 (0.49)
<i>BW_chshr</i>	0.0000 (-1.40)	0.0000 (-0.27)	0.0000 (-0.12)
<i>BW_bm</i>	0.0002 (0.35)	0.0007 (1.41)	0.0000 (-0.06)
<i>BW_lev</i>	0.0064* (1.78)	-0.0021 (-0.65)	-0.0026 (-0.59)
<i>BW_loss</i>	0.0025* (1.71)	-0.0009 (-0.63)	-0.0013 (-0.78)
<i>Peer_size</i>	-0.0006 (-1.01)	-0.0014*** (-4.63)	-0.0011*** (-3.29)
<i>Peer_fsr</i>	0.0000 (1.05)	0.0000*** (2.87)	0.0000 (0.95)
<i>Peer_chshr</i>	0.0000 (0.35)	0.0000 (0.69)	0.0000 (0.07)
<i>Peer_bm</i>	0.0023*** (3.45)	0.0015*** (4.62)	0.0016*** (4.31)
<i>Peer_lev</i>	-0.0115** (-2.18)	-0.0107*** (-4.25)	-0.0098*** (-3.18)
<i>Peer_loss</i>	-0.0109*** (-4.94)	-0.0129*** (-11.13)	-0.0118*** (-8.12)

**TABLE 3** (Continued)

<b>Panel B: Overreaction research design</b>			
<b>DV = <i>Peer_late_car</i></b>	<b>Domestic sample</b>	<b>Transnational sample</b>	
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>
Controls× <i>Same_standards</i>	No	No	Yes
FEs: country ( <i>BW</i> ), industry, year, month, and month-year to month-year	Yes	Yes	Yes
Observations	17,668	35,116	35,116
<i>Adj. R</i> <sup>2</sup>	0.0690	0.0380	0.0390

*Note:* This table presents the ordinary least squares results for domestic and transnational information transfers and overreactions. Panel A presents the baseline information-transfer results, and Panel B presents the results on potential investor overreactions linked to the initial information transfers. In both panels, Column 1 shows the results for the domestic benchmark subsamples, Column 2 the baseline transnational effects, and Column 3 illustrates our primary results capturing the interaction effects for harmonized financial reporting. Variable definitions are presented in the [Appendix](#). We provide coefficient estimates of the control variables and their interactions in the Supporting Information. All continuous variables are winsorized at the 1st and 99th percentiles. For the domestic sample (international sample), we cluster standard errors at the bellwether announcement level (country-pair level).

\*, \*\*, and \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, for two-tailed tests.

documents a positive abnormal return correlation of 1.5% (Wang, 2014) to 2.6% (Arif & De George, 2020) domestically.

In Column 3, we test whether spillovers increase with harmonized financial reporting in the transnational sample. The interaction term *Same\_standards*×*BW\_early\_car* shows the incremental spillover for firm-pairs with harmonized financial reporting. The baseline information transfer for non-harmonized financial reporting standards is weaker, but still significant at the 1% level (*BW\_early\_car*: 2.88%; *t*-value: 2.94). For firm-pairs with harmonized financial reporting, we observe an additional information-transfer effect of 2.65% (*t*-value: 2.04). These results are consistent with the literature documenting stronger transnational information-transfer effects for harmonized financial reporting.

## 4.2 | Overreactions

Building on the validated results for the initial transnational information-transfer effects in our sample, we next test whether these spillovers partly reflect investor overreactions. In Panel B of Table 3, we first test whether overreactions exist, on average, both domestically and internationally. Our domestic benchmark test detects a significant intra-national negative association between the earnings announcement of early bellwether and those of late peer firms of −3.02% (*t*-value: −1.99, Column 1). These domestic overreactions are in line with the results of Thomas and Zhang (2008),<sup>13</sup> who show similar overreactions in the United States. In Column 2, we also find a marginally significant, albeit weaker, overreaction for our international sample, with a coefficient of −1.77% (*t*-value: −1.65).

Column 3 presents our main analysis of whether harmonized financial reporting is associated with increased international overreactions. We find a significant and negative correlation for our international subsample with harmonized financial reporting standards. Specifically, the coefficient on *Peer\_early\_car*×*Same\_standards* is −4.29% and significant (*t*-value: −2.19),

<sup>13</sup>Interestingly, using a US sample from 1995 to 1997, Ramnath (2002) finds that the earnings forecast errors of early announcers are useful for predicting the forecast errors of late announcers in the same industry. However, investors and analysts do not fully incorporate this information from early announcing firms. These results imply that investors and analysts may underreact to peer information. Thomas and Zhang (2008) attribute the apparent difference to their own results by Ramnath's (2002) focus on first announcers and sampling differences.

indicating a predictable price reversal. The coefficient on *Peer\_early\_car* is insignificant and close to zero, which implies that the negative interaction term *Peer\_early\_car* × *Same\_standards* does not represent a correction of a previous underreaction.

This effect is also economically meaningful. The coefficient on the interaction term implies that, on average, 4.55%<sup>14</sup> of the initial return reaction of peer firms to early earnings announcements by same-standards bellwether firms of 2.21%<sup>15</sup> is reversed. Therefore, the correction resembles an average “unnecessary” return of 0.1% ( $2.21\% \times 4.55\%$ ) for each peer firm in our same-standards subsample. Moreover, comparing the magnitude of same-standards overreactions (−4.55%) with the average domestic overreaction (−3.02%) shows that the overreaction from accounting standard harmonization across countries is substantially more pronounced than the overreaction within countries for firms using the same accounting standards (i.e., 50% larger). Collectively, we interpret these results as meaning that harmonized financial reporting is associated with economically meaningful investor overreactions to peer firm earnings news. In this regard, our results offer an alternative interpretation of the findings in Wang (2014), as heightened information spillovers seem not to entirely reflect the extraction of incremental value-relevant information, and instead, to some extent, investor overreaction to the bellwether announcement.

To further explore the role of accounting standards for overreactions, we test whether overreactions extend to situations in which the accounting standards are not identical, but similar. A priori, we expect that overreactions are mitigated in these cases, because reporting standards that do not carry the same label may be less susceptible to over-interpretation bias, as explained before. To test this conjecture, we remove all IFRS reporters from our sample and create an international sample consisting of only local GAAP reporters. We adjust our bellwether identification strategy in response to the restricted non-IFRS sample and impose a size criterion of 1 billion USD market capitalization, as the adoption of IFRS is concentrated among the three largest firms in each industry-year, especially for the fiscal years starting in 2005 and afterward. However, the results and inferences do not change if we retain all observations from our main tests and simultaneously test for firm-pairs with the same standards, similar standards, and different standards. To measure accounting similarity, we use Bae et al.’s (2008) measure of pairwise differences between the local GAAP along 21 important GAAP dimensions. Specifically, we determine the differences between the GAAP of each country-pair based on 21 accounting standards characteristics and their compliance or noncompliance with IAS. We follow Yu and Wahid (2014) and modify the measure of Bae et al. (2008) to define two non-compliant local GAAP as different if the two countries do not share the same legal origin, as outlined in La Porta et al. (2008).

First, we replicate the spillover design in Column 1 of Table 4 and find that international firm-pairs with more similar, but not identical accounting standards, show stronger information spillovers. Second, however, we cannot find any evidence of overreactions for the firm-pairs with more similar local GAAP reporting standards in Column 2. This finding supports the notion that the label of reporting under identical standards is a prerequisite for overreactions.

### 4.3 | Do same-standards overreactions cause excess volatility?

Next, we analyze whether these overreactions manifest themselves in tangible costs. Specifically, increases in information spillovers and subsequent corrections could cause costly excess volatility. Moreover, the misjudgment of comparability benefits may alleviate disagreement among

<sup>14</sup>The total return reversal of 4.55% represents the sum of the base effect of 0.26% and the incremental harmonization effect of 4.29%.

<sup>15</sup>The initial return reaction of 2.21% represents the nondirectional average of peer firms’ abnormal returns to same-standards bellwether announcements.

**TABLE 4** Similar but nonidentical accounting standards.

	(1) Spillover DV = <i>Peer_early_car</i>	(2) Overreaction DV = <i>Peer_late_car</i>
<i>Peer_early_car</i>		−0.0140 (−0.94)
<i>Peer_early_car</i> × <i>GAAP_sim</i>		0.0168 (0.76)
<i>BW_early_car</i>	0.0451*** (4.09)	−0.0050 (−0.49)
<i>BW_early_car</i> × <i>GAAP_sim</i>	0.0521** (2.05)	−0.0072 (−0.39)
<i>GAAP_sim</i>	−0.0163** (−2.09)	0.0175 (1.58)
Controls	Yes	Yes
Controls × <i>GAAP_sim</i>	Yes	Yes
FEs: country ( <i>BW</i> ), industry, year, month, and month-year to month-year	Yes	Yes
Observations	22,244	22,244
Adj. $R^2$	0.0478	0.0796

*Note:* This table presents the parameter coefficients and *t*-statistics for our analyses on transnational overreactions and the role of national GAAP similarity. *GAAP\_sim* is an indicator variable for high and low similarity between the local GAAP of the bellwether firm and the peer firm based on Bae et al.'s (2008) GAAP differences measure. It takes the value of one if a firm-pair is characterized by an above median value on GAAP similarity, and zero otherwise. All other variable definitions are presented in the Appendix.

Coefficient estimates of the control variables and their interactions are available from the authors upon request. All continuous variables are winsorized at the 1st and 99th percentiles. We cluster standard errors at the country-pair level.

\*, \*\*, and \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, for two-tailed tests.

investors, which will also result in heightened price volatility. To test whether same-standards overreactions are associated with periods of elevated price volatility for peer firms, we calculate *Excess\_volatility* as the announcement-related standard deviation of a peer firm *i*'s daily returns in the interval between the peer firm's own earnings announcements and the bellwether earnings announcement, scaled by the mean standard deviation of peer firm *i*'s daily returns outside of the two earnings announcements minus one. The mean return volatility of daily return is measured at the peer firm level, starting from 150 days and continuing until 2 days prior to the bellwether announcement, and 2 days until 150 days after the own-announcement to mitigate concerns regarding potential time trends in volatility during the announcement window. As a result, *Excess\_volatility* indicates the percentage deviation of volatility from normal levels. As a baseline, we find a significant excess volatility of 1.40% for peer firms around earnings announcements from non-harmonized bellwethers (untabulated). For the peer firms that react in the period around harmonized bellwether announcements, the excess volatility increases significantly by 79.32%–2.51%.

To corroborate this descriptive evidence, we also follow the regression approach in Arif and De George (2020), using *Excess\_volatility* as our dependent variable and replacing *BW\_early\_car* with its absolute value (*Abs\_BW\_early\_car*). We rely on absolute daily returns to control for the information content of the initial bellwether announcement, because stock-price volatility has to be positive by construction and is equally driven by positive and negative daily returns. The untabulated results strengthen the descriptive inferences in that same-standards overreactions are significantly related to a period of excess volatility in the interval



between the earnings announcement of the two firms.<sup>16</sup> Economically, we find that a one standard deviation increase in the earnings news coefficient of *Abs\_BW\_early\_car* of 3.92% is associated with additional excess volatility for same-standards peer firms of 19.24% over the period between the two announcements. However, we do not observe any spikes in volatility for non-harmonized information spillovers. Collectively, the results suggest that the misjudgments of comparability benefits of harmonization may impair the ability to value firms.

## 5 | ADDITIONAL ANALYSES

### 5.1 | Investor types and learning over time

To corroborate our main result, we further test whether same-standards overreactions vary among different investor groups. Specifically, most prior research suggests that retail investors, compared to professional investors, are less sophisticated and more susceptible to behavioral bias (G. Chen et al., 2007). Specifically, the tendency to make mental shortcuts in the form of sentiment trading and heuristic representativeness is more pronounced for retail investors (e.g., DeLong et al., 1990; Nofsinger & Sias, 1999; Shiller, 1984). Moreover, ample evidence indicates that overconfidence particularly affects retail investors (e.g., Odean, 1999). To test whether overreactions are more pronounced for peer firms that are traded more heavily among retail investors, we collect ownership data for each of the 12,078 peer-firm-years in our sample using the Refinitiv Eikon Data API. The data provide us with information on individual investors or funds and their percentage of shareholdings, which enables us to distinguish between firms with different ownership structures.

Columns 1 and 2 of Panel A of Table 5 present the results. We (median-)split the transnational firm-pair sample according to the share of peer firm institutional ownership and estimate Equation (2) for each subsample. The differences between subsamples on the effect of *Peer\_early\_car* × *Same\_standards* are based on a joint estimation using a seemingly unrelated regression design. Specifically, we find that the effect on *Peer\_early\_car* × *Same\_standards* (−8.17%; *t*-value: −3.40) is highly significant for our subsample of peer firms with low institutional ownership, but muted, on average, for firms that are held predominantly by institutional investors (−0.27%; *t*-value: −0.11). This subsample difference is significant at the 5% level, a finding that is consistent with the notion that retail investors, exacerbated by taking mental shortcuts and overconfidence, are ultimately the main facilitators of overreactions.

Next, we shed further light on the role of institutional investors and analyze whether they serve as a “corrective force” in the context of same-standards overreactions. If retail investors predominantly drive overreactions, we expect overestimated comparability benefits potentially to give rise to arbitrage profits for better-informed investors, who do not overreact to peer firm signals but try to gain from the resulting under- or overvaluation of firms’ stock prices. However, this potential corrective force will be limited if trading restrictions are in place. For example, when short selling is restricted, institutional investors will not be able to correct positive overreactions that would require short positions. To test this notion, we retrieve country-specific data on short-selling restrictions following Jain et al. (2013), and test whether overreactions are more pronounced when short-selling restrictions are in place.<sup>17</sup>

<sup>16</sup>See Table S-12 in the Supporting Information.

<sup>17</sup>Note that we discard the bellwether-focus from our main tests and use a larger sample of information transfers and reversals between all firm-pairs (see Thomas & Zhang, 2008 for a similar approach). The reason is that we can only use positive overreactions, because short-selling restrictions only impede the correction of overvalued of stock prices. Moreover, the presence of short-selling restrictions further limits the sample. However, also note that our inferences remain unchanged if we use this sample for our main tests.

TABLE 5 Role of investor types.

Panel A: Ownership structure				
DV = <i>Peer_late_car</i>	<i>Institutional ownership</i>		<i>Short-selling restriction</i>	
	(1) Low	(2) High	(3) No	(4) Yes
<i>Peer_early_car</i>	0.0047 (0.33)	0.0016 (0.11)	−0.0184*** (−3.37)	−0.0278* (−1.77)
<i>Same_standards</i>	0.0156 (1.48)	−0.0130 (−1.24)	0.0182*** (8.78)	−0.0380*** (−3.94)
<i>Peer_early_car</i> × <i>Same_standards</i>	−0.0817*** (−3.40)	−0.0027 (−0.11)	−0.0053 (−0.61)	−0.0848** (−2.45)
$\chi^2$ test diff.: <i>Peer_early_car</i> × <i>Same_standards</i>	[0.0497]**		[0.1495]	
Controls	Yes	Yes	Yes	Yes
Controls× <i>Same_standards</i>	Yes	Yes	Yes	Yes
FEs: country ( <i>BW</i> ), industry, year, month, & month-year to month-year	Yes	Yes	Yes	Yes
Observations	16,181	16,149	195,482	15,531
Adj. $R^2$	0.058	0.067	0.045	0.175
Panel B: Learning over time				
DV = <i>Peer_late_car</i>	Full sample (1)	<i>Institutional ownership</i>		
		High (2)	Low (3)	
<i>Peer_early_car</i>	−0.0421*** (−2.96)	−0.0350** (−2.04)	−0.0495** (−2.22)	
<i>Same_standards_years</i>	0.0020* (1.69)	0.0049*** (3.07)	0.0018 (0.97)	
<i>Peer_early_car</i> × <i>Same_standards_years</i>	0.0043 (1.07)	0.0085* (1.83)	0.0006 (0.10)	
Controls	Yes	Yes	Yes	
Controls× <i>Same_standards_years</i>	Yes	Yes	Yes	
FEs: country ( <i>BW</i> ), industry, year, month, and month-year to month-year	Yes	Yes	Yes	
Observations	111,806	58,258	53,528	
Adj. $R^2$	0.0480	0.0700	0.0610	

*Note:* This table presents the parameter coefficients and *t*-statistics for our analyses of transnational overreactions and the role of different investor types. Panel A shows the cross-sectional results for different ownership structure using our *Institutional ownership* variable in Columns 1 and 2 as well as the impact of cross-selling restrictions for positive overreactions using an enlarged global sample of firm-pairs in Columns 3 and 4. Panel B presents the results from our analyses on learning effects over time. Column 1 in Panel B shows the results for an enlarged global sample of firm-pairs with sufficient data on applied accounting standards for at least 5 years. Columns 2 and 3 illustrate the cross-sectional differences for different ownership structures of peer firms, using a median-split on our *Institutional ownership* variable. Variable definitions are presented in the [Appendix](#). Coefficient estimates of the control variables and their interactions are available from the authors upon request. All continuous variables are winsorized at the 1st and 99th percentiles. For the domestic sample (international sample), we cluster standard errors at the bellwether announcement level (country-pair level). \*, \*\*, and \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, for two-tailed tests.

In Columns 3 and 4, we focus on positive overreactions, which require short positions to be corrected. We find that overreactions are concentrated among country-years with short-selling restrictions. However, we fail to find a significant overreaction when better-informed investors had the opportunity to short-sell overvalued stocks.

Next, we examine whether overreactions arise temporarily or form a sustainable equilibrium. For this test, we retain only same-standards observations and introduce a new variable, *Same\_standards\_years<sub>ik</sub>*, which captures the number of years since a firm-pair has reported according to the same accounting standards. We estimate the following regression:

$$\begin{aligned} Peer\_late\_car_{ik} = & \alpha + \gamma_1 Peer\_early\_car_{ik} + \gamma_2 Same\_standards\_years_{ik} \\ & + \gamma_3 Peer\_early\_car_{ik} \times Same\_standards\_years_{ik} \\ & + \gamma_4 BW\_early\_car_{ik} + \gamma_5 BW\_late\_car_{ik} \\ & + \sum \beta_n Controls_n + \sum \delta_n FE + \varepsilon, \end{aligned} \quad (3)$$

where the interaction term *Peer\_early\_car<sub>ik</sub> × Same\_standards\_years<sub>ik</sub>* captures changes in the overreaction over time, depending on each firm-pair's history of reporting under harmonized financial reporting standards. The initial overreaction effect is captured by the base coefficient on *Peer\_early\_car<sub>ik</sub>*.

In Column 1 of Panel B of Table 5, we find a significant baseline overreaction of  $-4.21\%$  ( $t$ -value:  $-2.96$ ), which corresponds to our main results. The interaction term with *Same\_standards\_years<sub>ik</sub>*, is insignificant and close to zero, indicating that investors do not learn over time. However, the capability of updating and learning over time may vary among different investor types. Based on the inferences from prior research that retail investors are more susceptible to behavioral bias, attribution theory predicts that misjudgments may persist for this investor group, but not for more rational institutional investors. Thus, we investigate the cross-sectional variation in our learning tests and re-run Equation (3) for subsamples of above and below median institutional ownership in Columns 2 and 3, respectively. While the results show a significant baseline overreaction for both subsamples, we only detect a significant reduction of overreaction for our subsample of firms with a high share of institutional owners. Specifically, the baseline overreaction for this group of  $-3.50\%$  ( $t$ -value:  $-2.04$ ) is reduced each year by 0.85 percentage points ( $t$ -value: 1.83).

In sum, we find that learning over time takes place at the level of institutional investors. Economically, it takes on average 3.2 years for the overreactions to disappear for this group. However, for stocks predominantly owned by retail investors and without significant block holdings, we find no significant attenuation over time. Accordingly, our results imply that overreactions are strongest in the first periods after two firms start to report under the same standards and then decline to some extent but do not disappear completely.

## 5.2 | Univariate analysis of market reactions to subsequent earnings announcements

In additional univariate tests, we perform a portfolio analysis on the relation between the market reaction of peer firms to earlier bellwether announcements and subsequent own-announcements. If investors only overreact to harmonized bellwether announcements, market responses of peer firms to bellwether announcements should negatively predict subsequent market responses to own earnings, but only for firm-pairs with identical financial reporting standards. Thus, we sort our sample into quintiles, based on the initial market reaction to bellwether earnings announcements (*Peer\_early\_car*). We then split the sample between harmonized and non-harmonized firm-pair observations and calculate the respective quintile average of peer firm market reactions to own earnings announcements (*Peer\_late\_car*).

The results in Panel A of Table 6 indicate a strong and negative correlation between the two sets of market returns, but only for the subsample of firm-pairs restricted to identical reporting

**TABLE 6** Univariate analysis of market reactions to subsequent earnings announcements.

Panel A: Portfolio analysis on same-standards overreactions							
Quintile rank of <i>Peer_early_car</i> in %	Same standards					Different standards <i>Peer_late_car</i> in %	Quintile difference in %
	<i>Peer_late_car</i> in %	Size rank	BM rank	<i>Illiq</i> rank			
Q1	−5.23	0.69	3.07	2.96	3.14	0.26	0.43*** [2.62]
Q2	−1.55	0.57	3.11	2.99	3.06	0.15	0.42*** [3.02]
Q3	0.01	0.55	3.24	2.98	3.03	0.15	0.40*** [3.01]
Q4	1.73	0.50	3.22	2.94	3.06	0.34	0.16*** [1.16]
Q5	5.95	0.31	3.07	2.87	3.10	0.24	0.07*** [0.48]
<b>Q1 – Q5</b>		<b>0.38**</b>				<b>0.02</b>	
		[2.03]				[0.12]	
Panel B: Portfolio analysis on antedated market reactions							
Quintile rank of <i>Abs_Peer_early_car</i> in %	Same standards		Different standards		Quintile		
	<i>Abs_Peer_late_car</i> in %		<i>Abs_Peer_late_car</i> in %		difference in %		
Q1	0.37	3.94	3.80		0.14**		[1.41]
Q2	1.15	4.00	3.85		0.15**		[1.61]
Q3	2.11	4.16	4.00		0.16**		[1.64]
Q4	3.59	4.40	4.31		0.09**		[0.85]
Q5	7.60	5.24	5.03		0.21***		[1.73]
<b>Q1 – Q5</b>		<b>−1.30***</b>	<b>−1.23***</b>				
		[−10.66]	[−13.25]				

Note: This table presents the quintile averages of peer firms' market reaction to its own earnings announcement sorted by the market reaction to respective bellwether announcements. Panel A reports the quintile averages of *Peer\_late\_car* and the quintile ranking on market capitalization, book-to-market ratio, and illiquidity sorted by *Peer\_early\_car*. Panel B reports the quintile averages for absolute values of *Peer\_late\_car* (*Abs\_Peer\_late\_car*) sorted by absolute values of *Peer\_early\_car* (*Abs\_Peer\_early\_car*). Variable definitions are presented in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles.

\*, \*\*, and \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, for two-tailed *t*-tests.

standards. The difference between the average own-announcement return in the smallest and the largest quintile of *Peer\_early\_car* amounts to 0.38% and is statistically significant at the 5% level (*t*-value: 2.03). Hence, this simplified univariate analysis supports our main findings. Moreover, the return wedge between the lowest and highest quintile cannot be explained by market microstructure differences related to size, book-to-market ratios, or illiquidity.

The portfolio analysis also serves as a falsification test for natural learning dynamics, as a potential alternative explanation for the negative return correlation. In this regard, information transfers might antedate own-announcement market reactions, when bellwether earnings news merely subsume for later own-firm's earnings news. This interpretation implies that larger absolute market reactions to bellwether earnings are followed by smaller absolute market reactions to own earnings, and vice versa. However, the results in Panel B of Table 6 using absolute values of *Peer\_early\_car* and *Peer\_late\_car* do not support this notion. Instead, we document a significant positive relation between the magnitudes of peer firm market reactions.<sup>18</sup> Hence, our results are unlikely to be driven by antedated market responses where the reaction to own earnings news is dampened by earlier bellwether earnings announcements.

<sup>18</sup>The results are similar when we restrict the portfolio analysis in Panel B to same-direction pairs of market returns (i.e., *Peer\_early\_car* and *Peer\_late\_car* are both positive or both negative).

### 5.3 | Test for an axiomatic relationship between initial spillovers and subsequent overreactions

Another alternative explanation of our results may be that overreactions are a mechanical by-product that follows *any* information spillover. To test this assertion, we restrict our sample to international non-harmonized peer firm announcements that cause relatively strong spillovers. Specifically, we require spillovers for these non-harmonized announcements to be at least 4% (which approximately equals the average international spillover for harmonized disclosures). Based on these relatively strong spillovers, we still do not find any significant overreactions for non-harmonized peer firm announcements (untabulated).

We also exploit the relationship between the magnitude of spillover effects and the degree of economic integration at the country-to-country level. We find that international firm-pairs using different accounting standards located in countries with stronger economic ties show significantly stronger information spillovers. However, we do not find stronger overreactions for these firm-pairs. These findings show that the occurrence of overreactions does *not* depend mechanically on the magnitude of initial spillovers. Instead of an axiomatic relationship between spillovers and overreactions, our results imply that behavioral frictions drive overreactions in a transnational context.

### 5.4 | Cross-listings

Prior research suggests that mandatory IFRS adoption facilitates firms' cross-listing activities (L. Chen et al., 2015) and that firms tend to increase voluntary disclosures after cross-listing (L. Chen et al., 2019). Hence, to rule out our results possibly being explained by different information transfers of firms starting to cross-list after IFRS adoption, we identify and remove all firms that had a cross-listing at any point during our sample period. The inferences from these untabulated results remain the same, indicating that cross-listings do not provide an alternative explanation.

### 5.5 | DiD analysis

To further strengthen our identification, we exploit the fact that many firms voluntarily adopted IFRS before the country-level mandatory adoption. Using a DiD design similar to that of Wang (2014), we analyze the change in overreactions to voluntary IFRS adopter earnings announcements for a treatment group of mandatory adopters compared to a control group of non-adopters around the mandatory country-level IFRS adoption. We can then identify whether same-standards overreactions start to appear by evaluating the time-series change in market reactions for treatment firms against benchmark firms. Theoretically, the quality of earnings signals of voluntary adopters around the mandatory country-level adoption should not change. Hence, the treatment and control groups both observe the same earnings signals, but only the treatment group experiences (perceived) comparability benefits. Because we require the treatment and control groups to have at least one observation before and after the mandatory country-level adoption, we control for the underlying fundamental comparability between firm-pairs. For both groups, we estimate the following regression:

$$\begin{aligned} Peer\_late\_car_{ik} = & \alpha + \gamma_1 Peer\_early\_car_{ik} + \gamma_2 Post + \gamma_3 Peer\_early\_car_{ik} \times Post \\ & + \gamma_4 BW\_early\_car_{ik} + \gamma_5 BW\_late\_car_{ik} \\ & + \sum \beta_n Controls_n + \sum \delta_n FE + \varepsilon, \end{aligned} \quad (4)$$

where *Post* is an indicator variable that equals one for all observations after 2005 in both samples (i.e., *Post* coincides with *Same\_standards* for our treatment sample). Interaction term *Peer\_early\_car<sub>ik</sub>*  $\times$  *Post* captures the incremental abnormal return correlation related to firm-years after 2005. Only firm-pair observations in our treatment sample switch from different to the same financial reporting standards in 2005, in contrast to the observations in our benchmark samples. Based on the main results, we predict a significantly higher overreaction for firm-pair-years after 2005 in the harmonized treatment sample, compared to the effects from observations in the benchmark sample.

For this test, we slightly modify the sample criteria. Similar to Wang (2014), we include all earnings announcements reported by the firms that voluntarily adopted IFRS before 2005 and impose an additional size criterion of 1 billion USD market capitalization to ensure that bellwether firms have sufficient visibility in a transnational context and are followed by investors and analysts. We then identify the corresponding late earnings announcements by peer firms in the same industry-year and discard all peer firm observations from voluntary adopters. Finally, we create a treatment, as well as a benchmark subsample, based on whether the respective peer firms mandatorily adopted IFRS in 2005 or maintained their previous reporting standards. These revised sample adjustment procedures lead to a benchmark sample consisting of 984 firm-pair-years, compared to the treatment sample of 3,657 firm-pair-years.

Table 7 shows the results of estimating Equation (4) for our benchmark and treatment samples. We include the same control variables and fixed effects as in our main empirical specification and cluster standard errors at the country-pair level. Consistent with our prediction, we

**TABLE 7** Transnational overreactions for the voluntary IFRS adopters sample.

	Benchmark sample (1) Bellwether: Voluntary adopters Peer firms: Non-adopters	Treatment sample (2) Bellwether: Voluntary adopters Peer firms: Mandatory adopters
DV = <i>Peer_late_car</i>		
<i>Peer_early_car</i>	−0.0634 (−0.62)	0.0404 (1.02)
<i>Post</i>	−0.2392** (−2.23)	−0.0549 (−0.47)
<i>Peer_early_car</i> $\times$ <i>Post</i>	0.0917 (0.77)	−0.0982* (−1.85)
$\chi^2$ test diff.: <i>Peer_early_car</i> $\times$ <i>Post</i>	[0.0387]***	
Controls	Yes	Yes
Controls $\times$ <i>Post</i>	Yes	Yes
FEs: country ( <i>BW</i> ), industry, year, month, and month-year to month-year	Yes	Yes
Observations	984	3,657
<i>R</i> <sup>2</sup>	0.1073	0.0690

*Note:* This table presents the parameter coefficients and *t*-statistics for the DiD analysis of the overreactions to transnational information transfers using two samples of voluntary IFRS adopters. Our treatment sample comprises firm-pair observations between voluntary IFRS adopters' earnings announcements and mandatory IFRS adopting peer firms. We match the firm-pair observations between the voluntary IFRS adopters' earnings announcements with a control group of non-adopters around the mandatory country-level IFRS for the treatment sample. *Post* is an indicator variable that equals one for all observations after 2005 in both samples (i.e., it coincides with *Same\_standards* for our treatment sample). Variable definitions are presented in the Appendix. Coefficient estimates of the control variables and their interactions are available from the authors upon request. All continuous variables are winsorized at the 1st and 99th percentiles. We cluster standard errors at the country-pair level.

\*, \*\*, and \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively, for two-tailed tests.



calculate a negative and statistically significant coefficient on  $Peer\_early\_car \times Post$  ( $t$ -value:  $-1.85$ ) for our treatment sample, where peer firms mandatorily switched to IFRS in 2005. However, our results do not indicate a significant effect for the benchmark sample, where financial reporting standards remained heterogeneous, as the coefficient on  $Peer\_early\_car \times Post$  is insignificant ( $t$ -value:  $0.77$ ). Additionally, our results document a significant difference between the coefficients for both subsamples, with a  $p$ -value of  $3.9\%$ .

Collectively, the DiD results support our main finding, in that more harmonized financial reporting is associated with an increase in investor overreactions to peer firm earnings news.

## 6 | CONCLUSION

This study analyzes how accounting standards affect investor use of international peer firm earnings. Our analysis relates to prior research in that using the same accounting standards facilitates transnational information spillovers. We demonstrate that these spillovers create predictable reversals in returns. We thereby contribute to the literature on transnational investor reactions and the transmission of earnings signals worldwide, which is important for a globally intertwined economy. While investor overreactions to peer firm disclosures have hitherto only been documented in the domestic US context (Thomas & Zhang, 2008) or for interim reporting (Arif & De George, 2020), our study is the first to show systematic overreactions in a transnational setting. The results also imply that transnational overreactions are not generally axiomatic to spillovers. Rather, behavioral biases drive overreactions in the international context. We also provide new evidence on the role of institutional investors that extends the information-transfer literature. Our results suggest that institutional investors are an important corrective force for overreactions, and market restrictions such as short-selling limitations impede their corrective ability.

To the best of our knowledge, our study is also the first to document an unintended consequence of financial reporting harmonization in the form of increased investor overreactions, which cause significant excess volatility. Additional analyses suggest that the costs of these overreactions are ultimately borne by retail investors. Nonetheless, our findings should not be interpreted as meaning that financial reporting harmonization is bad per se. We simply highlight a potential non-negligible cost of harmonization, but other consequences should be considered as well when analyzing the net effects of financial reporting harmonization. In sum, we inform the ongoing debate on the usefulness and desirability of financial reporting harmonization.

An important caveat to our findings is the limited sample period focused on the global convergence toward IFRS in the early 2000s. The evolution of international markets, particularly the rise of ETFs over the past decade, may significantly impact the emergence of overreactions. As Bhojraj et al. (2020) demonstrate, broad-based ETFs can exacerbate overreactions, whereas sector-focused ETFs may help mitigate them. In the international context, the results presented in this paper may be shaped by the extent to which international ETFs are predominantly broad-based or sector-specific.

Collectively, our findings shed light on factors that enable and moderate the unique phenomenon of investor overreactions, which is important to understand, because overreactions potentially impede the efficient allocation of capital in global markets.

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## DATA AVAILABILITY STATEMENT

Data are publicly available from the sources identified in the paper.

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## SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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## APPENDIX: VARIABLE DEFINITIONS

Variable	Definition
<b>Firm-pair variables</b>	
<i>Corr</i>	Correlation of daily returns for each firm-pair over a period of 12 months prior to the corresponding bellwether firm's earnings announcement
<i>GAAP_sim</i>	Indicator variable for high and low similarity between the local GAAP of the bellwether firm and the peer firm based on Bae et al.'s (2008) GAAP differences measure. It takes the value of one if a firm-pair is characterized by an above median value on GAAP similarity, and zero otherwise
<i>Post</i>	Indicator variable taking the value of one for firm-pair-year observations after 2005, and zero otherwise.
<i>Reporting_lag</i>	Number of calendar days between the release of the bellwether firm's earnings announcement and the peer firm's earnings announcement
<i>Same_standards</i>	Indicator variable taking the value of one if both the bellwether firm and the peer firm follow the same reporting standards, and zero otherwise
<i>Same_standards_years</i>	Number of years since a pair of companies has reported according to the same accounting standards

(Continues)

## APPENDIX (Continued)

Variable	Definition
<i>Trade</i>	Total mutual export volume between the bellwether firm's home country and the peer firm's home country, scaled by the total exports of both countries to the rest of the world
<b>Peer firm variables</b>	
<i>Abs_Peer_early_car</i>	Absolute value of <i>Peer_early_car</i>
<i>Abs_Peer_late_car</i>	Absolute value of <i>Peer_late_car</i>
<i>Excess_volatility</i>	Announcement-related standard deviation of a peer firm <i>i</i> 's daily returns in the interval between the peer firm's own earnings announcements and the bellwether earnings announcement, scaled by the mean standard deviation of peer firm <i>i</i> 's daily returns outside of the two earnings announcements minus one
<i>Illiq</i>	Illiquidity measure, calculated as the average of the peer firm's absolute value of daily returns scaled by the peer firm's dollar trading volume over the month leading up to 1 week prior to its earnings announcements
<i>Institutional ownership</i>	Percentage of the peer firm's shares held by institutional investors at the end of the fiscal year
<i>Peer_bm</i>	Book-to-market ratio of the peer firm, calculated as the ratio of book value of equity to the market value of equity at the end of the fiscal year
<i>Peer_chshr</i>	Ownership concentration based on the closely held shares ratio for the peer firm at the end of the fiscal year
<i>Peer_early_car</i>	Three-day cumulative abnormal return of the peer firm around the date of the respective industry bellwether firm's earnings announcement
<i>Peer_fsr</i>	Ratio of foreign sales over total sales for the peer firm at the end of the fiscal year
<i>Peer_late_car</i>	Three-day cumulative abnormal return of the peer firm around the date of its own earnings announcement
<i>Peer_lev</i>	Leverage ratio of the peer firm, defined as the total debt to total assets at the end of the fiscal year
<i>Peer_loss</i>	Indicator variable taking the value of one if the peer firm reported negative earnings for the fiscal year, and zero otherwise
<i>Peer_size</i>	Natural logarithm of the market value of equity for the peer firm at the end of the fiscal year
<i>Short-selling restriction</i>	Indicator variable taking the value of one for peer-firm observations linked to country-years with short-selling restrictions in place, and zero otherwise
<b>Bellwether firm variables</b>	
<i>Abs_BW_early_car</i>	Absolute value of <i>BW_early_car</i>
<i>BW_bm</i>	Book-to-market ratio of the bellwether firm, calculated as the ratio of book value of equity to the market value of equity at the end of the fiscal year
<i>BW_chshr</i>	Ownership concentration based on the closely held shares ratio for the bellwether firm at the end of the fiscal year
<i>BW_early_car</i>	Three-day cumulative abnormal return of the bellwether firm around the date of its own earnings announcement
<i>BW_fsr</i>	Ratio of foreign sales over total sales for the bellwether firm at the end of the fiscal year
<i>BW_late_car</i>	Three-day cumulative abnormal return of the bellwether firm around the date of the respective industry peer firm's earnings announcement
<i>BW_lev</i>	Leverage ratio of the bellwether firm, defined as the total debt to total assets at the end of the fiscal year

APPENDIX (Continued)

Variable	Definition
<i>BW_loss</i>	Indicator variable taking the value of one if the bellwether firm reported negative earnings for the fiscal year, and zero otherwise
<i>BW_size</i>	Natural logarithm of the market value of equity for the bellwether firm at the end of the fiscal year

*Note:* This table presents the variables used in the multivariate regression analysis. Data on daily stock prices, financial fundamentals, analyst coverage, and institutional ownership are retrieved from Worldscope, Datastream, I/B/E/S, and Refinitiv Eikon Data API.