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Louis Mangeney

Approved by the dissertation committee:

Prof. Peter FIECHTER, Université de Neuchâtel, thesis director

Prof. Tim A. KROENCKE, Université de Neuchâtel

Prof. Garen MARKARIAN, University of HEC Lausanne

Prof. Luc PAUGAM, HEC Paris, France

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Faculté des sciences économiques
Avenue du 1er-Mars 26
CH-2000 Neuchâtel

www.unine.ch/seco

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Three Essays on Financial Information Intermediaries

Louis Mangeney

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La Faculté des sciences économiques,
sur le rapport des membres du jury

Prof. Peter Fiechter (directeur de thèse, Université de Neuchâtel)
Prof. Tim A. Kroencke (président du jury, Université de Neuchâtel)
Prof. Luc Paugam (HEC Paris, France)
Prof. Garen Markarian (HEC Lausanne)

autorise l'impression de la présente thèse.

Neuchâtel, le 13 septembre 2022

Le doyen

Valéry Bezençon

Faculté des sciences économiques
Avenue du 1er-Mars 26
CH-2000 Neuchâtel
Email: secretariat.seco@unine.ch
Tel: +41 32 718 15 00
www.unine.ch/seco

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“When you arise in the morning think of what a privilege it is to be alive, to think, to enjoy, to love”

- Marcus Aurelius

Executive Summary

This dissertation comprises three distinct chapters. The first chapter contributes to the recent debate on the net benefits of the growing complexity and detailed disclosure requirements of International Financial Reporting Standards (IFRS). I investigate the impact of a voluntary turn away from IFRS on analysts in a country specific setting, Switzerland, where departure from IFRS is made possible for listed firms. The chapter studies analysts' following and accuracy around the switch from IFRS to the Swiss domestic accounting standard (Swiss GAAP). Firms leaving the international standard experience lower analyst following and accuracy. However, further analysis provides evidence that such effects are principally driven by foreign analysts, and analysts without prior experience on Swiss GAAP. Overall, IFRS –despite its demanding requirements and complexity of information– delivers beneficial information to international analysts.

The second chapter is co-written with Peter Fiechter (University of Neuchatel) and Annelies Renders (Maastricht University). While prior literature focuses on individual determinants of analyst herding, we examine the influence of contextual factors (i.e., the working environment) on herding behavior. We circumvent endogeneity issues by exploring shocks to analysts' working environment, i.e., acquisitions of private brokerage houses. Using a difference-in-differences design, we find that acquired analysts issue significantly more herding forecasts after the shock to their working environment, and this effect is strongest right after the acquisition. Further analyses suggest that increased peer pressure and job security concerns due to the change in working environment are plausible explanations for our findings.

In the third chapter I investigate the role of social media users as information intermediaries to mitigate processing costs. Specifically, I explore the potentially different effects of Twitter's user-generated content on information asymmetry around earnings announcements relative to firm-initiated content on social media. I control for market reactions to the announcement and other factors affecting firms' information environments (e.g., analyst following or traditional media activity). To provide plausible variation in processing costs, I exploit two institutional features from Twitter: the platform's identification feature for users of public interest (verified accounts) and the introduction of the clickable cashtag (automatic access). I find that user-generated content is associated with increased information asymmetry around earning announcements, especially due to users without verified accounts on Twitter. On the other hand, I find that only firm-initiated content from verified accounts is associated with a reduction in information asymmetry around earnings announcements. Overall, I provide evidence that social media can potentially lead to investors' misinformation when used as an additional source for disclosure processing. My findings also suggest how credibility on social media affects processing costs.

Keywords: Financial Information Intermediaries, IFRS, Swiss GAAP, Information Overload, Financial Analysts, Security Analyst, Herding Behavior, Mergers, Acquisitions, Institutions, Social media, Information Asymmetry, Earnings Announcements, Processing Costs

Résumé

Cette thèse comprend trois chapitres distincts. Le premier chapitre contribue au récent débat sur les avantages nets de la complexité croissante et des exigences de divulgation détaillées des normes internationales d'information financière (IFRS). J'étudie l'impact d'un abandon volontaire des IFRS sur les analystes dans un pays spécifique, la Suisse, où l'abandon des IFRS est rendu possible pour les sociétés cotées. Le chapitre étudie le suivi et la précision des analystes autour du passage des IFRS à la norme comptable nationale suisse (Swiss GAAP). Les entreprises qui quittent la norme internationale connaissent une baisse du suivi et de la précision des analystes. Cependant, une analyse plus approfondie montre que ces effets sont principalement dus aux analystes étrangers et aux analystes sans expérience préalable des Swiss GAAP. Dans l'ensemble, les IFRS −malgré leurs exigences exigeantes et la complexité de l'information− fournissent des informations utiles aux analystes internationaux.

Le deuxième chapitre est co-écrit avec Peter Fiechter (Université de Neuchâtel) et Annelies Renders (Université de Maastricht). Alors que la littérature antérieure se concentre sur les déterminants individuels du comportement grégaire des analystes, nous examinons l'influence des facteurs contextuels (c'est-à-dire l'environnement de travail) sur le comportement grégaire. Nous contournons les problèmes d'endogénéité en explorant les chocs sur l'environnement de travail des analystes, c'est-à-dire les acquisitions de maisons de courtage privées. Nous constatons que les analystes acquis émettent beaucoup plus de prévisions de troupeaux après le choc sur leur environnement de travail, et cet effet est le plus fort juste après l'acquisition. D'autres analyses suggèrent que l'augmentation de la pression des pairs et les problèmes de sécurité d'emploi dus au changement de l'environnement de travail sont des explications plausibles de nos résultats.

Dans le troisième chapitre, j'étudie le rôle des utilisateurs de médias sociaux en tant qu'intermédiaires d'information pour atténuer les coûts de traitement. Plus précisément, j'explore les effets potentiellement différents du contenu généré par les utilisateurs de Twitter sur l'asymétrie de l'information autour des annonces de résultats par rapport au contenu initié par les entreprises sur les réseaux sociaux. Je contrôle pour les réactions du marché à l'annonce et d'autres facteurs affectant l'environnement d'information des entreprises (par exemple, le suivi des analystes ou l'activité des médias traditionnels). Pour fournir une variation plausible des coûts de traitement, j'exploite deux fonctionnalités institutionnelles de Twitter : la fonctionnalité d'identification de la plateforme pour les utilisateurs d'intérêt public (comptes vérifiés) et l'introduction du cashtag cliquable (accès automatique). Je trouve que le contenu généré par les utilisateurs est associé à une asymétrie accrue des informations autour des annonces de gains, en particulier en raison des utilisateurs sans compte vérifié sur Twitter. D'autre part, je constate que seul le contenu initié par l'entreprise à partir de comptes vérifiés est associé à une réduction de l'asymétrie d'information autour des annonces de résultats. Dans l'ensemble, je fournis des preuves que les médias sociaux peuvent potentiellement conduire à la désinformation des investisseurs lorsqu'ils sont utilisés comme source supplémentaire pour le traitement de la divulgation. Mes conclusions suggèrent également comment la crédibilité sur les médias sociaux affecte les coûts de traitement.

Mots-clés: Intermédiaires d'Informations Financières, IFRS, Swiss GAAP, Surcharge d'Informations, Analystes Financiers, Analyste de Sécurité, Comportement Grégaire, Fusions, Acquisitions, Institutions, Médias Sociaux, Asymétrie de l'Information, Annonces de Résultats, Coûts de Traitement

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Introduction

Well-functioning capital markets rely on the efficient distribution of information between firms receiving capital and the investors who provide it (Bradshaw et al., 2017). Investors have limited attention to allocate for information acquisition and processing (Hirshleifer and Teoh, 2003; Dyck and Zingales, 2004) and can be distracted by simultaneous events such as earnings announcement seasons (Hirshleifer et al., 2009; deHaan et al., 2015). Information processing is costly for investors due to frictions for learning, acquiring, and integrating firms' disclosures. Financial information intermediaries are capital market agents mitigating investors' processing costs by serving three main functions (Blankespoor et al., 2020). First, they can redistribute extracted public information to reduce awareness and acquisition costs for investors. Second, with their expertise, they may interpret the implications of public information on firms' value, decreasing integration costs for investors. Finally, financial information intermediaries could uncover private information.

In the first chapter, I explore the effect of firms' voluntary disclosures on information intermediaries' coverage decisions and ability to mitigate interpretation costs for investors. I investigate the effects of a voluntary turn away from International Financial Reporting Standards (IFRS) to Switzerland's local GAAP (Swiss GAAP) on financial analysts. Despite the inherent selection issue and the limited scope of this study, such setting allows me to (i) contribute to the literature on firms' level of voluntary disclosures and analyst following and (ii) question the cost and benefits of the growing complexity and detailed disclosure requirements of IFRS on firms' information environment. While holding regulatory requirements and enforcement systems constant, which prove to be determinants of analysts' forecast quality[1], I find that firms leaving the international standard experience lower analyst following and forecasts accuracy. However, further analysis provides evidence that such effects are principally driven by foreign analysts, and analysts without prior experience on the Swiss local GAAP. Overall, the results provide evidence that IFRS still provides comparability benefits to foreign analysts or analysts without country-specific experience despite its one-size-fits-all approach for standard setting.

In the second chapter co-written with Peter Fiechter (University of Neuchatel) and Annelies Renders (Maastricht University), we investigate the influence of institutional factors on financial intermediaries' herding in the processing of information for capital markets. While prior literature focuses mainly on individual determinants of analyst herding (Hong, et al., 2000; Clement and Tse, 2005), we examine the influence of contextual factors (i.e., the working environment) on herding behavior. We circumvent endogeneity issues by exploring shocks to analysts' working environments. Specifically, we focus on acquisitions of private brokerage houses, as the working environment for acquired analysts plausibly changes along various dimensions (e.g., changes in peer pressure, career concerns, or tasks assigned), potentially affecting their herding behavior. Using a staggered difference-in-differences design, we find

[1] See Fiechter, Halberkann, and Meyer (2017), Hope (2003a), Hope (2003b), Hope (2004), Barniv, Myring, and Thomas (2005)

that acquired analysts issue significantly more herding forecasts after the shock to their working environment, and this effect is more pronounced for analysts acquired by a publicly listed brokerage house as compared to acquisitions by a privately held brokerage house. Additional tests show that this effect is strongest right after the acquisition and holds also for analysts who cover the same firm before and after the acquisition. Cross-sectional analyses suggest stronger treatment effects for analysts facing higher peer pressure after the shock and for analysts acquired by public brokerage houses with a more pronounced hire-and-fire culture. Taken together, our findings suggest a causal link between the working environment and herding behavior.

In the final chapter, I investigate the role of another type of information intermediary to mitigate disclosure processing costs. Specifically, I explore Twitter's users as information intermediaries to reduce processing costs around earning announcements while controlling for firm-initiated tweets. Prior literature shows that firms strategically use Twitter as an additional channel to communicate with investors, reducing processing costs (Blankespoor et al., 2014; Jung et al., 2018). But it remains an empirical question whether user-generated content is incrementally useful for mitigating disclosure processing costs (Blankespoor et al., 2020). Prior research shows that the ex-ante "wisdom of the crowds" (i.e., user-driven dissemination on social media) influences investor attention and response to earnings (Bartov et al., 2018). However, user-generated content potentially has different effects on processing costs than firm-initiated content due to three defining features of social media. First, social media allows instant access and dissemination of information, potentially aggravating differences in investor processing capabilities (Kim and Verrecchia, 1994). Second, user-generated content can be subject to social transmission biases (Hirshleifer, 2020) leading to investor's misinformation. Finally, social media's user base differs from traditional media due to lower barriers to entry, leading to concerns about users' credibility (Miller and Skinner, 2015; Blankespoor et al., 2020). I use data from Twitter, the most widely used social media platform for investor relations, to investigate the different effect of user-generated content relative to firm-initiated content on information asymmetry around earnings announcements. This result provides evidence that user-generated content potentially leads to misinformation due to investors' limited processing abilities and social transmission bias. Furthermore, I confirm existing concerns about users' credibility due to social media's lower barriers to entry relative to other traditional media.

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Chapter 1: IFRS Information Overload? Evidence from Analyst Following and Accuracy in Switzerland

1.1. Introduction

A recent debate emerged on International Financial Reporting Standards' (IFRS) economic benefits, as both financial statements users and preparers complained about the growing complexity of its disclosure requirements (IASB, 2013a; IASB, 2013b). Such "information overload", referring to investors' confusion by an excessive amount of information provided, has been discussed by several Accounting Standard Boards and academic papers[2]. Using a country specific setting where the departure from IFRS is made possible and a distinct group of financial statements users (i.e., financial analysts), this study investigates how IFRS "information overload" affects firms' information environment.

As regulators are converging towards the adoption of internationally recognized accounting standards, Switzerland's maintains the possibility to report under its local standard Swiss GAAP while recognizing IFRS and US GAAP. In a trade-off between accounting harmonization with IFRS and competitiveness with Swiss GAAP's lower costs and disclosure requirements, a substantial number of Swiss firms voluntarily switched back to the Swiss domestic standard. Past research already investigated Switzerland's case: while Meyer (2009) support the notion that Swiss GAAP compared to IFRS include less complexity, volume, and implementation costs; Fiechter et al. (2017) do not predict adverse capital-market effects after going away from IFRS. I also choose to address this country-specific setting looking at the impact of the departure from IFRS on analysts following and accuracy.

This paper compares in a difference-in-differences (DiD) setting analyst following and forecast accuracy around this event. Financial analysts are primary users of financial accounting information. The literature identifies them as dispensers of information and for example utilizes the number of analysts following a company as a proxy for information environment[3]. Since any transaction relies primarily on information, analysts play a valuable role in improving market efficiency (Healy and Palepu, 2001) bridging information gaps between investors and corporate entities. However, this article cautiously draws conclusions from such a small specific group of market agents.

IFRS adoption research is already a well-documented field in numerous settings either internationally or in country-specific settings, looking at accounting, capital-market, and other economic effects around its mandatory adoption[4]. At first, the purpose of IFRS was to increase financial statements comparability through enhanced disclosure and improvement of recognition practices worldwide (Schipper, 2005; Whittington, 2005; Brown et al., 2014;

[2] see Guay, Samuels and Taylor (2016) and Dyer, Lang and Strice-Lawrence (2016)
[3] See Brennan and Subrahmanyam (1995), Duarte, Han, Harford, and Young (2008) or Merton (1987)
[4] See Brueggemann et al. (2013), De George et al. (2016), and Leuz and Wysocki (2016) for detailed literature reviews.

Beuselinck et al., 2017), however empirical research showed mixed results on the cost and benefits of IFRS. Various papers confirmed this purpose and found increased accounting comparability from its adoption[5], however other studies only found little to no effect on financial reporting disclosure and quality, particularly in countries with poor enforcement regimes[6]. Looking at the literature on financial analysts, they tend to be attracted by IFRS and issue more accurate forecasts (Byard et al., 2011; Cotter et al., 2012; Amhed et al., 2013; Horton et al., 2013). But once again, the evidence is mixed and other papers found that the increase in accuracy is principally driven by foreign analysts (Tan et al., 2011), sector-specialists (Beuselinck et al., 2017) and that such gains on accuracy fade away over time while analysts become accustomed to the new standard (Barniv and Myring, 2014).

On the one hand, IFRS as an internationally recognized standard reduces information acquisition costs and allows analysts to cover wider portfolios of companies. Its comprehensive structure compared to local GAAPs eases analysts' understanding and predictability of financial information (Ball, 2006). However, the growing complexity of IFRS with more detailed requirements and extensive use of fair value accounting leads to timely accounts, difficult to implement and understand. Even if attractive to analysts, such information is tougher to predict due to its complexity and earnings volatility. IFRS' "information overload" can therefore positively impact analysts following, but negatively affect forecast accuracy, hence departure from it should lead to a decrease in analysts following and an increase in accuracy. On the other hand, culture and history play a major role in shaping countries' accounting standards, an argument against IFRS claim as a uniform and worldwide applicable regulation (Tan et al., 2011). Difficulties in complying with IFRS could lower transparency, reliability of accounting information, and enhance earnings smoothing, together canceling the objective of a worldwide uniform accounting regulation. Departure from it should lead analysts following to increase and accuracy to decrease due to prior earnings smoothing and non-serious implementation. Both theories are reinforced by the Swiss setting, in which companies switch to the local standard due to the increasing complexity and costly implementation of IFRS (Fiechter et al., 2017; Deloitte, 2013) questioning the misappropriate "one size fits all" approach.

The uniqueness of the Swiss institutional environment provides valuable information about such empirical question. Swiss GAAP as a country specific standard is not recognized internationally. It is less complex and easier to implement but has limited to no guidance in certain areas (Deloitte, 2013). It should experience a decrease in analysts' coverage and increase in forecasting accuracy due to its lower attractiveness but easier to understand accounting information. As firms face a trade-off between harmonization and competitiveness analysts are balanced between complex information, attractive but difficult to predict and simplified accounts less informative but easier to forecast. I expect a decrease in analyst following when firms switch back to Swiss GAAP since IFRS provide comparable and more detailed accounting information to both foreign and local analysts. Even though expecting a decrease in

[5] See DeFond et al. ,2011, Brochet et al. 2013, Horton et al. 2013, and Wang 2014

[6] Daske and Gebhardt, 2006; Verriest et al., 2013; Morais and Curto, 2008; Iatridis, 2010; Ahmed et al., 2013.

accuracy principally driven by foreign analysts, I predict an increase in accuracy for analysts already familiar with the Swiss regulation. For those analysts experienced with both regulations, the switch to a standard with lower complexity and earnings volatility should enhance their accuracy, hence evidence of the detrimental "information overload" of IFRS in the recent years.

The first empirical test focuses on the impact of the switch from IFRS to Swiss GAAP on analysts following in a univariate DiD method. Pairing switching firms with IFRS reporting firms using Propensity Score Matching (PSM), I find a significant decrease in the number of analysts following the change in accounting standard. This univariate model supports the hypothesis that the change in accounting standard leads to a loss of analysts following.

Second, I focus on the impact of leaving IFRS on analysts' coverage in a multivariate DiD test adapting a traditional ordinary least square (OLS) regression to the staggered adoption (phased-in over time) of Swiss GAAP, following the methodology of Bertrand and Mullainathan (2003). The number of analysts following a firm is regressed on a dummy variable for the switch from IFRS to Swiss GAAP. The method is repeated after differentiating foreign and local analysts. Overall, I find a decrease in analysts' coverage for both foreign and local analysts, providing evidence of IFRS' comparative advantages in terms of detailed disclosure requirements and comparability benefits. Nevertheless, analysts might also stop covering such firms due to their lack of expertise on the domestic standard.

The third empirical test focuses on analyst-level data. I use the same OLS method with the staggered adoption of Swiss GAAP to investigate the impact of the departure from IFRS on analysts' forecasting accuracy and repeat the method after differentiating foreign and local analysts. I find a decrease in analysts' accuracy for switching firms with weak evidence of a stronger impact on foreign analysts. This also reinforces IFRS' comparative advantages in terms of disclosure requirement and predictability of earnings. However, such negative effects might be once again solely explained by analysts' lack of knowledge on the Swiss domestic accounting standard i.e., Swiss GAAP.

The final test allows to understand the impact of IFRS' "information overload" on analysts. Identifying analysts with prior experience on the Swiss domestic standard reveals that the decrease in accuracy is explained by analysts' inexperience with the Swiss regulation. The results reveal that the decrease in accuracy after the switch from IFRS to Swiss GAAP is driven by local and foreign analysts with no prior experience on the Swiss domestic standard over the sample period. Experienced local analysts see their accuracy increasing after the change in accounting regulation. Even though firms leaving IFRS significantly decrease financial statements disclosure, the negative effect on accuracy is explained by analysts' lack of knowledge and preparation for the newly adopted accounting regulation rather than the loss of information. For analysts with prior experience on both standard, the switch to a less demanding accounting regulation with lower complexity and disclosure requirement does not negatively affect them and even facilitate their predicting abilities. This provides evidence on IFRS' "information overload" as accounting data that is attractive due to its detailed disclosure requirements and comparability, but difficult to predict due to its implementation's complexities. For analysts prepared for the switch, there is no negative effect on their predicting abilities hence justifying firms' choices to adopt a more straightforward accounting standard in terms of implementations, recognition rules and costs.

Despite the self-selection bias and relatively small sample of this study, the combined results provide insights on firms' information environment and predictability of their earnings around reporting changes. The ever-changing nature of IFRS leads to a growing complexity in its interpretation and implementation. This study shows that small to mid-cap Swiss companies going back to the Swiss local GAAP see a reduction in analysts following and accuracy driven by non-Swiss analysts. However, analysts familiar with the Swiss domestic standard see their accuracy increasing despite the significant decrease in firms' financials statements disclosure after leaving IFRS.

The paper contributes to the prior literature on reporting changes' effects on analysts' following and accuracy. For example, Tan et al. (2011) find a positive relation between the number of analysts following firms that mandatory and voluntary adopt IFRS. The present results indirectly support those findings as analysts' following decrease when firms leave IFRS for a more specific and less comparable standard.

This study is distinguished from the prior literature: first, the existing research on the impact of reporting changes on financial analysts mainly focus on IFRS adoption. The impact of higher disclosure requirements on analysts is observed before 2005 for voluntary adopters and around 2005 for mandatory adopters but fails to capture the growing intricacy of IFRS since 2005. Second, such papers suffer from identification problems since IFRS benefits depend on enforcement regimes, but its adoption is probably part of wider institutional reforms (Leuz and Wysocki, 2016). This country-specific study allows to control for institutional regimes differences and changes as the choice to go away from IFRS is confined to Switzerland and staggered in time.

This paper complements prior research on the growing concerns against IFRS uniformity. In the same environment, Fiechter et al. (2017) do not predict adverse capital-market effects after a switch from IFRS to Swiss GAAP. Similarly, I do not find adverse effects on analysts familiar with the Swiss regulation when firms go away from IFRS.

1.2. Background and Hypotheses

1.2.1. Institutional background

In 2005, Switzerland recognized IFRS and USGAAP as International Reporting Standards for listed companies on the "Main Segment", but Swiss GAAP remained applicable for firms registered on the "Domestic Segment" classified under the Swiss Reporting Standard[7]. A significant number of Swiss listed firms adopted IFRS in a trend to homogenize reporting with worldwide practices. However, IFRS evolved over time growing the number of standards and disclosure requirements leading to the re-emergence of Swiss GAAP in Switzerland. Until 2012 few small to medium size companies moved from the "Main Segment" to the "Domestic Segment" to report under Swiss GAAP. It was in October 2012, when Swatch Group announced its decision to leave international standard even though the firm was listed on the Swiss Market Index (SMI), that an antecedent was created. From 2008 until 2016, a total of 43 Swiss companies decided to go away from IFRS (see Appendix 1.A for a detailed list).

Following prior studies on analyst accuracy in country-specific institutional backgrounds (Peek, 2005; Ernstberger et al., 2008; Garrido-Miralles and Sanabria-García, 2014), Switzerland provides a unique framework for a comparative analysis. Regulatory requirements and the enforcement system which prove to be determinants of analysts' forecast quality[8] are kept constant isolating the effect of the accounting standard change (Fiechter et al., 2017).

1.2.2. Analysts' information needs and IFRS/Swiss GAAP differences

Based on a survey and content analysis of sell-side analysts' reports, Previts et al. (1994) find that analysts base their recommendations on company income relative to balance sheet evaluation, disaggregate company performance into a greater number of operating units, emphasize core earnings, prefer conservative earnings management, pay attention to earnings momentum, evaluate asset and liabilities at cost and not on a market value basis, develop non-GAAP cash flows and extensively rely on non-financial information (company risk, anticipated changes, competitive positioning, management and strategy). They also document analysts' inability to understand detailed information in all the notes from annual reports, and lack of key information on the company production strategy and profitability drivers.

For the 43 Swiss switching firms, the number of pages and notes respectively decreased by 15% and 26% in the pre- and post-switch periods[9]. Such decrease is explained by two major differences between IFRS and Swiss GAAP concerning financial information presentation. For the financial statements and disclosure notes, IFRS have more detailed guidelines on the

[7] See SIX regulation (2015)

[8] See Hope (2003a), Hope (2003b), Hope (2004), Barniv et al. (2005)

[9] Following Fiechter et al. (2017), the number of pages and notes in the switching firms' annual report before and after the switch were hand collected

balance sheet, requires the distinction between operating, non-operating and exceptional activities, mandates other comprehensive income (OCI) that does not exist under Swiss GAAP and has more detailed notes. Second, segment reporting has higher disclosure requirements whereas the only guidance under the Swiss regulations is the disclosure of segment revenues either by geographic regions or businesses (Deloitte, 2013).

Swiss GAAP also differs from IFRS in three major areas concerning recognition and measurement. First Goodwill under the Swiss standard can be accounted for in two different ways: as an intangible asset and is amortized over its estimated useful life or can be recorded against equity at the date of acquisition. Under IFRS, Goodwill is treated as an intangible asset with an indefinite useful life and must be tested at least annually for impairments. Second, pension accounting is simplified under Swiss GAAP with no distinction by type of retirement benefit plans and the company only evaluates its obligations based on the financial statements of the pension fund. IFRS distinguish between defined contribution plans and defined benefit plans. For defined benefit plans the company must evaluate the pension plan assets at fair value, determine the present value of the plan obligations and recognize in its income statement services costs, contributions for the period and results of the calculation of the capitalization for both plan assets and obligations. Revaluation differences are recognized immediately in OCI. Finally, hedge accounting under Swiss GAAP is streamlined, fair value hedge is authorized with no specific requirement on the hedge risk and type of instruments used whereas IFRS requires extensive documentation and hedge effectiveness assessment (SIX, 2015b; Deloitte, 2013).

Higher disclosure requirements under IFRS might be valuable to analysts concerning the balance sheet, segment reporting information and distinction between operating, non-operating, and exceptional activities. For these reasons, even if IFRS do not fully fulfill analysts' information needs (disclosure of the company strategy, profitability drivers or information by-products) they should nonetheless generate greater attention. Swiss GAAP should not lower analysts' ability to accurately predict earnings due to its lower reliance and guidance on fair value accounting, producing less volatile information thus facilitating analysts' balance sheet valuations and predictions of earnings. The Swiss standard should require less appraisals and adjustments for pension expenses (notably for realized and unrealized gains and losses from applying fair value accounting) in the identification of firms' core operating incomes. Moreover, goodwill can be difficult for analyst to handle in their balance sheet valuation. Firms, when acquiring an entity might have the incentives to allocate more of the purchasing price to goodwill than intangibles to only be subject to annual impairment tests for goodwill and avoid intangibles depreciations in their expenses (Penman, 2013). Offsetting Goodwill against equity should facilitate balance sheet's valuation. Concerning the notes, analysts do not exhaustively understand and use all the information provided by firms (Previts et al., 1994) so the switch to less detailed information might not significantly impact their predicting abilities. Finally, non-financial information can still be found under the Swiss domestic standard. Swiss GAAP provides analysts with simplified and comprehensive accounting information, to the extent that they are familiar with its practices and major differences with IFRS.

1.2.3. Related Literature

Major findings on the drivers of analysts' forecast accuracy can be divided between analyst-specific and firms-specific factors (Ernstberger et al., 2008), and its relation to reporting changes is principally studied through voluntary and/or voluntary IFRS adoptions (see Figure 1). For IFRS voluntary adoption, Ashbaugh and Pincus (2001) find higher forecast accuracy for firms primarily listed in Switzerland, France, and Canada and observe an increase in firms' market capitalizations and analyst's coverage. Developing a measure of IFRS' voluntary compliance, Hodgdon et al. (2008) show that serious implementation of IFRS reduces information asymmetry and enhances the ability of financial analysts to provide accurate forecasts. Similarly, Bae et al. (2008) find that GAAP differences between countries are negatively related to both foreign analysts' following and accuracy, thus are associated with economic costs for analysts. In a country-specific environment, Ernstberger et al. (2008) and Glaum et al. (2011) find higher forecast accuracy for the estimates based on IFRS or US GAAP compared to the ones based on German GAAP accounts. However, several studies show mixed results. Studying European firms Buijink and Cuijpers (2005) find a positive relation between non-local GAAP adoption and analyst following but fail to find evidence of a lower cost of capital for non-local GAAP adopters. Moreover, they find higher uncertainty among analysts and investors following firms using IAS or US GAAP compared to analysts covering local GAAP reporting firms.

[Figure 1.1. about here]

For mandatory IFRS adoption, Byard et al. (2011) use firms that have already voluntarily adopted IFRS as a control group to show that forecast errors and dispersion decrease, especially for adopting firms domiciled in countries with both strong enforcement regimes and domestic accounting standards that differ the most from IFRS. Tan et al. (2011) find the same effect when the adoption eliminates GAAP differences between firms' and analysts' home countries, but local analysts' accuracy is unaffected by IFRS. In country-specific environments, Garrido-Miralles and Sanabria-García (2014) and Cotter et al. (2012) show identical results respectively in Spain and Australia, highlighting the importance of enforcement regimes and magnitude of the differences between IFRS and local GAAP. Petaibanlue et al. (2015) provide evidence that comparability (i.e., the number of peer firms reporting according to IFRS) plays a key role in the improved forecasting accuracy of analysts after the mandatory adoption in Europe. Similarly, Beuselinck et al. (2017) find that IFRS mandatory adoption allows analysts to cover a wider portfolio of companies due to comparability benefits and that sector specialized analysts outperformed generalists in terms of accuracy around the adoption whereas country specialists retained a comparative advantage.

Studying both voluntary and mandatory IFRS adoptions in 17 EU countries Wang et al. (2008) find for both groups decreasing earnings forecast errors and dispersion and conclude that IFRS improve firms' information environment. But the academic consensus is again far from homogeneous. Daske et al. (2008) find that voluntary adopters benefit significantly more from mandating IFRS compared to mandatory adopters, in contradiction with Horton et al. (2013)

results. The latter find after controlling for firm, industry, and country unobservable characteristics that IFRS adoption is associated with a higher quality of the information environment for mandatory adopters relative to non-adopters and voluntary adopters.

Concerning analyst-specific factors as drivers for accuracy, the literature shows that analysts are optimistic, revising upward their forecast for positive information (Easterwood and Nutt, 1999) and update their views rather gradually (Bartov et al., 2002). In addition, analysts exhibit different skills due to their experience, workload, or risk tolerance.

Firm's characteristics (e.g., size, industry, country location or regulatory environment) also drive analysts' forecast accuracy[10]. Ang and Ciccone (2001) study international differences in analysts' forecast properties using 42 countries and suggest that country-specific (e.g., corporate governance structures) and firm-specific components help determine dispersion and error, the most important component being firms' profitability. Companies with losses are associated with significantly higher dispersion, forecast error and higher percentages of forecast optimism. The literature also points out the influence of firms' actions on analysts forecast accuracy as managers tend to smooth earnings towards the consensus forecast [11].

1.2.4. Hypothesis

This study investigates how the growing complexity of the detailed rules and disclosure requirements under IFRS affect financial analysts. Analysts' coverage and accuracy are observed before and after the switch from IFRS to Swiss GAAP. The first hypothesis concerns the change in the number of analysts following firms that leave IFRS. Prior literature demonstrates a positive relationship between IFRS adoption and analysts' following due to higher disclosure requirements and comparability benefits. Therefore, firms adopting Swiss GAAP with lower disclosure requirements, less detailed guidance and the limited degree of comparison should see a decrease in the number of analysts covering them.

H1: Firms switching from IFRS to Swiss GAAP experience a decrease in their number of analysts following due to the lower attractiveness and comparability of accounting information.

The second hypothesis relates to the impact of the switch on analysts' accuracy. The existing research converges towards positive benefits from IFRS adoption on analyst accuracy and provides evidence of a positive relationship between disclosure and analysts forecast accuracy[12]. Nevertheless Tan et al. (2011) found that the increase in analysts predicting skills from IFRS adoption is different depending on analysts' location. I predict a stronger decrease in analysts' accuracy for analysts located in other countries than Switzerland when firms leave

[10] Das and Saudagaran (1998), Higgins (1998)
[11] Bannister and Newman (1996), Degeorge et al. (1999), Matsumoto (2002), Bartov et al. (2002), Abarbanell and Lehavy (2003), Hutton (2005)
[12] Lang and Lundholm (1996), Higgins (1998), Acker et al. (2002), Hope (2003a), Hope (2003b), Hope (2004)

IFRS. Local analysts, due to their proximity with Swiss firms and possible experience relating to Swiss GAAP, should have better information networks and knowledge of the Swiss corporate culture. Foreign analysts should be the most affected by the loss of accounting information disclosed by switching firms.

H2: Analysts following firms switching from IFRS to Swiss GAAP experience a decrease in their accuracy. However, foreign analysts are the ones the most affected by the change in regulation.

Finally, the third hypothesis concerns analysts' accuracy depending on their knowledge of both accounting standards. IFRS continuously changed since 2005, growing more complex and difficult to understand while Swiss GAAP is relatively stable and has lower guidance in certain areas. At the country level, controlling for institutional changes, enforcement regimes and analysts' expertise, going away from IFRS should not decrease analysts' ability to predict earnings. If analysts are familiar with both accounting standard, specifically with Swiss GAAP they should face, after the switch, easier to understand accounting information and experience higher forecasting accuracy.

H3: The switch from IFRS to Swiss GAAP increases the accuracy of analysts already familiar with the Swiss domestic standard due to its lower complexity compared to IFRS.

1.3. Variables and Model

1.3.1. Dependent variables

To study the impact of a change in accounting standard, different measures of analysts' following, and accuracy are used as proxies. *Coverage* corresponds to the number of analysts covering a firm each year. *Foreigners* corresponds to the number of foreign analysts covering a firm each year and *Locals* is the equivalent for analysts based in Switzerland. Following Tan et al. (2011), accuracy is measured as follow:

$$Accuracy_{i,j,t} = -100 \times \frac{\left|(FEPS_{i,j,t} - EPS_{i,t})\right|}{P_{i,t-1}} \quad (1.1)$$

where $FEPS_{i,j,t}$ designates the forecasted earnings per share (EPS) of firm *i* by analyst *j* for the current fiscal year *t* and $EPS_{i,t}$ firm *i* realized earnings per share for the current fiscal year. The absolute forecasting errors are scaled by the latest available stock price $P_{i,t-1}$ of firm *i* from the prior year and multiplied by −100 to facilitate exposition.

1.3.2. Independent and control variables

[Figure 1.2. about here]

The independent variable is a dummy variable for the change in accounting standard. Taking IFRS as a reference group, $POST_{i,t}$ is equal to one if actual EPS for firm *i* at time *t* are based on Swiss GAAP and not IFRS anymore. By construction, $POST_{i,t}$ is a staggered adoption variable since the switch from IFRS occurs at different times *t* for each switching firms. Control variables are used to isolate the impact of the reporting change[13]. *Time* refers to the number of days between the forecasting date and the earnings announcement date (see Figure 2). For example, a firm issuing its annual report on February 15, 2014, for the fiscal year ending on December 31, 2013, then *Time* is the number of days between the predicted earnings forecast issued on October 1st, 2013, and the firm's publication date (i.e., 123 days). *FirmExp* measure analysts' firms experience as the number of years an analyst has been following an individual company. *BrokerSize* corresponds to the number of analysts active in each brokerage house per year. Other controls proven to be significant from the literature are included: dummy variables for loss years, size, market-to-book ratio, return-on-assets, stock return and stock returns volatility. It is supposed that an analyst issuing a forecast on December 1st, 2013, for the 2013 EPS base its valuation on 2012 accounting information since the 2013 annual report is not published yet (see Figure 2). Market capitalization, market to book ratios, monthly stock return volatility and stock return are computed with the latest available stock prices from the prior year (see Appendix 1.B for variables definition).

1.3.3. Models

An example will illustrate the methodology followed in this article. Swatch Group announced its decision on October 3, 2012, to switch from IFRS to Swiss GAAP and released its first annual report under the new accounting regulation on May 5, 2014, for 2013 financial results. The straightforward analysis would subtract the number of analysts and accuracy measures before and after May 2014, however exogenous factors could explain a change in coverage and accuracy in Switzerland. The first model pairs every switching firm with a control firm reporting under IFRS using PSM to investigate a univariate DiD in similar fashion to Fiechter et al. (2017). The differences in analyst following, foreign analysts following, Swiss analysts following and mean accuracy before and after May 2014 for Swatch group is compared to the same difference at the same point in time for its peer IFRS reporting firm. This DiD is the effect of the switch from IFRS to Swiss GAAP for Swatch Group.

The multivariate models implement the same strategy with the benefits of handling the staggered switches from IFRS to Swiss GAAP and allows to compare each switching firm to all other firms as a control group (Bertrand and Mullainathan, 2003). The first model follows

[13] See related literature on the control variables: Bhushan (1989), O'Brien and Bhushan (1990), Lang and Lundholm (1996), Lang et al. (2004), Brown et al. (1987), Das and Saudagaran (1998), Hope (2003a), Hope (2003b), Hope (2004), Clement (1999), Hwang et al. (1996), Das (1998), Peek (2005), Tan et al. (2011), Brown (1997).

$$Coverage_{i,t} = \alpha_t + \alpha_i + \delta\, Switch_{i,t} + \gamma\, X_{i,t} + \epsilon_{i,t} \qquad (1.2)$$

where *i* indexes firms and *t* time. $Coverage_{i,t}$ designates the number of analysts following firm *i* at time *t*, α_t and α_i are time and firm fixed effects. $Switch_{\,i,t}$ is a dummy variable for the change in accounting standard equal to 1 for firm *i* at time *t* if the firm now publishes EPS under Swiss GAAP and not in accordance with IFRS anymore. Finally, $X_{i,t}$ are control variables, and $\epsilon_{i,t}$ is an error term. The same methodology is applied for the accuracy of analysts, with a different sample and set of control variables $Y_{i,j,t}$,

$$Accuracy_{i,j,t} = \alpha_i + \alpha_j + \alpha_t + \beta\, Switch_{i,j,t} + \lambda\, Y_{i,j,t} + \varepsilon_{i,j,t}. \qquad (1.3)$$

The coefficients for the reporting change effects on analysts' following and accuracy are δ and β respectively. The introduction of firm and time fixed effect for regression (2) and firm, analyst, and time fixed effects for regression (3) ensure that δ and β measure within firm (and within analyst) variations while being immune to time trends. By construction, both models take as a control group all Swiss firms not subject to a change in accounting standard at time *t*, independently of the accounting standard followed.

1.3.4. Sample

The study is restricted to Switzerland for three major reasons. First, selecting one country allows to control for institutional factors like regulatory requirements and enforcement regimes, proven to be related to analyst forecast accuracy[14]. Second, under the SIX regulation firms in Switzerland have the choice to disclose their financial information under IFRS, US GAAP or Swiss GAAP. Such setting even if suffering from an endogeneity problem, allows the study of a departure from IFRS. Compared to the prior literature on voluntary and mandatory IFRS adoption effects on financial analysts, the Swiss particularity provides new evidence on the effects of a voluntary IFRS departure. Finally, Swiss GAAP is a relatively stable accounting standard with lower to no guidance on specific accounting policies compared to IFRS. Comparing this study to prior findings will assess whether similar conclusions can be drawn even though IFRS significantly changed.

The sample consists of all listed Swiss firms followed by analysts from 2006 until 2016 hence forecasts issued for 2007 until 2017 fiscal years, omitting US GAAP reporting firms and financial sector companies. The starting date is at the intersection between IFRS mandatory application for EU countries in 2005 (Switzerland included) and the first switch from a Swiss company for the 2008 fiscal year. The final sample contains 120 Swiss firms including 23 out of the 42 switching firms leading to a total of 992 firm-level observations and 5'732 analyst-level observations. Important to notice, 6 of the 42 switching firms were excluded from the

[14] Hope (2003a), Hope (2003b), Hope (2004), Barniv et al. (2005), Fiechter et al. (2017)

sample since they were only followed by analysts before the change in accounting standard but had no analyst coverage once they switched back to Swiss GAAP (see Appendix 1.A).

1.3.5. Data

Analysts' forecasts are obtained from IBES detailed estimates files that also provide the analyst and broker code, forecast date issuance, and forecasted fiscal year end. I match this query with other IBES tables to obtain actual EPS value, earnings publication date, firms' identifiers, analysts' names, and brokerage houses' names. Following Tan et al. (2011), I retain the last annual earnings forecast by foreign analyst *j* before the annual earnings announcement date for firm *i* in year *t* and require all forecasts to be issued at least 30 days before the earnings announcement date from the firm followed to only retain serious analysts. I require analysts to be following a company for at least two consecutive years and make sure that analysts following switching firms are present before and after the change in accounting standard for the switching firms. Finally, market data and accounting information are retrieved from Thomson Reuters (see Appendix 1.B).

Analysts are classified as foreign or local. Using their names and if not available the name of the institution they are active into I assess whether they are based in Switzerland or not. Resources like analysts' reports, phone numbers, LinkedIn profiles and others are used to determine their geographical location. Furthermore, I compute a dummy variable *GAAP* equal to one if the analyst follows or ever followed a firm reporting according to the Swiss domestic standard.

1.3.6. Descriptive Statistics and moments analysis

[Table 1.1. about here]

Table 1.1 panel A presents descriptive statistics for firm-level data. On average Switching firms have the lowest coverage for both local and foreign analysts. For all the sample firms no foreign analyst has prior experience on Swiss GAAP whereas respectively 80% and 70% of local analysts for non-switching and switching firms do have prior knowledge about the domestic standard. Hence local analysts with such knowledge should be better prepared for the change in accounting standard. Looking at companies' characteristics, the average size and market to book ratios confirm that the most likely firms to switch from IFRS to Swiss GAAP are small to mid-capitalization companies on average (see also Fiechter et al., 2017). Looking at return on assets and stock returns, even though switching firms are less profitable the difference appears meaningless. Table 1.1 panel B presents descriptive statistics for switching firms in the pre and post periods. The number of analysts following decreases after the change in accounting standard with an average effect from 1.4 to 0.6 and 3.4 to 2 approximatively for foreign and local analysts respectively. The decrease in market to book ratio and asset to equity ratio can be explained by the offset of Goodwill against equity as possible according to Swiss GAAP.

Profitability of switching firms slightly increases after the switch when looking at return on assets but this is partly linked to the restatement's effects on net income.

[Table 1.2. about here]

Table 1.2 presents correlations between variables for firm-level data. The event dummy Switch is negatively related with *Coverage* (i.e., analyst following) and significant at the 1% level. Looking at the impact on foreign analysts and local analysts, the correlation between local analysts following and *Switch* is stronger, indication that local analysts are more likely to stop following firms leaving IFRS than foreigners. Prior experience with the Swiss accounting standard is positively correlated and significant at the 1% level with the accounting regulation change by firms, indication that analysts with prior knowledge on the Swiss regulation are more likely to keep following the firms adopting such standard when leaving IFRS. Finally, the *Switch* variable is negatively correlated and significant at the 1% level with firms' size, market to book ratio and asset to equity ratio as expected since most of the firms abandoning IFRS are small to mid-capitalization companies. The high correlation coefficients between *Mbook* and *Asset* should be a red flag for controls chosen for later regressions in the paper.

[Table 1.3. about here]

Table 1.3 panel A presents descriptive statistics for analyst-level data. Switching firms have on average over the sample period a −2.91 accuracy with 8.01 standard deviation compared to an average accuracy of −2.60 with a standard deviation of 7.31 for non-switching firms. Hence, there is no significant difference on analysts' ability to predict earnings for the treatment and control group of firms when looking at descriptive statistics. Similarly looking at the averages of *Time*, *FirmExp* and *BrokerSize*, no significant difference can be found between switching and non-switching companies. *Coverage* and the proportion of foreign analysts are lower for switching firms but their proportion of analysts with Swiss GAAP knowledge is higher. Furthermore, no foreign analyst has expertise on Swiss GAAP.

Table 1.3 panel B presents descriptive statistics for switching firms in the pre and post switch periods. Accuracy decreases on average from −3.07 to −2.66 after the change in accounting standard, hence evidence from analysts' difficulties to predict earnings after the switch. Analysts firm experience significantly increases after the change, indication that analysts with lower firms' experience stop their coverage after the change in accounting standard. The number of analysts following decreases, but the proportion of foreign analysts seems unchanged after the switch whereas the opposite can be observed for the proportion of analysts with Swiss GAAP experience, indication that analysts stop following firms adopting accounting standard they are unfamiliar with.

Table 1.4 presents correlations between variables for analyst-level data. Accuracy is negatively correlated with the Switch dummy but not significant. However, the analysts predicting abilities are negatively correlated and significant with *Time*, *BrokerSize*, *Loss*, *Returns* and *Volatility*. First intuitively, analysts' ability to forecast firms' earnings per share is more difficult when the forecast is issued far for the announcement date, analysts closer to announcement date have a wider set of information to use for their predictions. the older the forecast, the large the brokerage house where the analyst work, firms with negative net income, positive stock returns and high volatility negatively impact. The negative correlation between accuracy and analysts working in larger brokerage houses in counterintuitive, we would expect analysts working in larger institutions to have better predicting abilities from larger resources available both internally from other analysts and externally from the institution reputation and network. The negative correlation with firms having negative net income confirms the hypothesis that analysts have more difficulties to predict future earnings when having abnormal negative figures in their historical set of accounting data. Finally, difficulties to predict earnings from companies with active market activity, i.e., higher returns and volatility seems intuitive. Positive and significant correlations between accuracy and *FirmExp*, *Coverage*, *Size*, *Mbook* and *RoA* are in line with the assumptions that analysts a better able to predict future earnings when they have experience on the company, the company is a large firm with higher analysts' coverage and is profitable. The high correlation coefficients for *Coverage* with *Size* and *Mbook* with *Asset* should be red flags for the controls chosen for later regressions in the paper.

[Table 1.4. about here]

1.4. Results

1.4.1. Univariate model

Table 1.5 presents the results of the univariate DiD. This model matches each switching company with IFRS reporting firms using PSM scores and computes cross-differences between pre-switch and post-switch periods for *Coverage*, *Foreigner*, *Local* and *meanACC*.

In Panel A, the difference in *Coverage* between the pre-switch and post-switch period is significant at the 1% level for Switching firms (i.e., Switcher) and is equal to a decrease of 2.253 analysts after the change in accounting standard. Furthermore, the number of analysts for switching firms after the change is significantly different at the 1% confidence level with the control group and is equal to 1.868 analysts. The DiD for *Coverage* in bold characters is significant at the 1% level and correspond to a difference of 2.805 analysts following due to the switch from IFRS to Swiss GAAP.

[Table 1.5. about here]

In Panel B and C, similar results to Coverage can be identified for local and foreign analysts. The DiD in Panel B for Swiss analysts (i.e., Local) is equal to −1.781 and significant at the 1% level whereas the DiD in Panel C for foreign analysts is equal to −1.024 and significant at the 10% level. Those results combined with panel A provide evidence on the first hypothesis: Firms leaving IFRS see a reduction in their information environment as both foreign and Swiss analysts stop covering them after the change in accounting standard. I identify two possible explanations for such phenomenon: first, analysts might abandon their coverage due to Swiss GAAP lower attractiveness and comparability of accounting information compared to IFRS. Second, and possibly simultaneously analysts might put an end to their coverage due to their lack of knowledge on the Swiss regulation.

In Panel D and E, interesting results can be observed for analysts with or without prior experience with the domestic Swiss accounting standard. The DiD in Panel D for analysts with prior experience with Swiss GAAP FER (i.e., GAAP) is equal to 1.172 and significant at the 1% level whereas the DiD in Panel E for analysts without prior experience with Swiss GAAP FER (i.e., NoGAAP) is equal to −3.977 and significant at the 1% level.

1.4.2. Multivariate model

As explained earlier, combining firms and year fixed effects with an event dummy in the regression allows to study the impact of the switch from Swiss GAAP to IFRS in a DiD setting. The first ordinary least squares (OLS) regression in Table 1.6 takes *Coverage* (i.e., the number of analysts following each firm), *Foreigner* (i.e. the number of foreign analysts following each firm) and *Local* (i.e. the number of foreign Swiss analysts following each firm) as the dependent variable in the three successive columns. All regressions have time and firm fixed effects. The sample contains 992 firm-year observations for 120 Swiss firms. Since *Mbook* and *Asset* are highly correlated, adding them successively to the models in each column was tested but the results did not change the inferences on the impact of the Switch on analysts following. The analysis will continue with the full specified model.

[Table 1.6. about here]

In column 1, the estimate for δ is negative and significant at the 1% level, the number of analysts is predicted to decrease by 1.858 when firms abandon IFRS, hence a decrease of 38% from the pre-switch number of analysts. All other coefficients are not significant. In column 2 the estimate for δ is also negative and significant at the 5% level, the number of foreign analysts is predicted to decrease by −0.457 representing a loss of 33% from the pre-switch number of foreign analysts. All other coefficients are not significant. In column 3 the estimate for δ is negative and significant at the 1% level, the number of local analysts is predicted to decrease by −1.400 (40% of the pre-switch number) when firms abandon IFRS. All other coefficients are not significant. Finally, in column 4 (5) the estimate for δ is positive (negative) and significant at the 1% level, the number of analysts with (without) prior GAAP experience is predicted to increase (decrease) by 1.056 (−2.913) when firms leave IFRS. All other coefficients are not significant. Table 1.6 provides support for H1: on average within year and within firm, the number of analysts following a company switching from IFRS to Swiss GAAP decreases. The information loss and change to accounting standard with lower comparability has negative incentives on analysts' coverage decisions. The loss of analysts appears to be stronger for the ones located in Switzerland. Nevertheless, international coverage by analysts in the pre-switch period was already lower for the switching firms, and the adoption of Swiss GAAP appears to reinforce such low attention by non-Swiss analysts.

[Table 1.7. about here]

The second OLS regression results in Table 1.7 correspond to equation (3) where the accuracy is used as the dependent variable and year, firm and analysts fixed effects are added successively. Analysts fixed affects allow to implement the same DiD analysis for the staggered switch from IFRS to Swiss GAAP while accounting for unobserved differences among analysts and allowing our estimates to be immune to time firms and analysts variations. The sample contains 5'732 analyst-year observations for the same 120 Swiss firms.

The estimate for β is significant at the 5% level when adding firm and year fixed effects without controls and is equal to −1.642. Once adding controls, the coefficient is significant at the 10% level ad is equal to −1.192. Since the correlation coefficients for *Coverage* with *Size* and *Mbook* with *Asset* are high and significant as seen in table 1.4 but adding them successively does not significantly change the estimates for β. On column 3 analysts fixed effects are added to the firm and year fixed effects without any controls, I find an estimates for β of −1.640 significant at the 5% level. Considering column 4 with coefficients immune to time, firms, and analysts' variation as well as controls, I predict on average negative impact of −1.266 representing a 41% decrease of the pre-switch accuracy measure for firms leaving IFRS for Swiss GAAP.

[Table 1.8. about here]

In table 1.8, the sample is divided by foreign and local analysts and by analysts with prior knowledge on Swiss GAAP and inexperienced analysts concerning the Swiss regulation. For foreign analysts, the estimate for β in column 1 is equal to −1.276 and significant at the 1% level (compared to -1.266 significant at 5% on the full sample) with a high t-statistics of −3.05, indicating stronger statistical evidence of the negative impact of the switch on accuracy for non-Swiss analysts. In column 2 for the sub-sample with only local analysts, the coefficient for β is −1.249 and not significant. The value for the t-statistics of −1.65 is close to the significance threshold stresses the need to be careful about the interpretation of the difference between the results of column 1 and 2. Such results partly support H2: after the switch analyst's accuracy decreases and panel B provides slight indication on a stronger impact on foreign analysts. For the sub-sample with experienced analysts, the estimate for β in column 3 becomes positive and is equal to 1.436 and significant at the 10% level. Looking at the fourth column for the inexperienced analysts' sample, the estimate for β is equal to −1.635 and significant at the 5% level (compared to −1.266 significant at 5% on the full sample). As mentioned in section 3.6 no foreign analysts in the sample have experience on Swiss GAAP, however both Swiss analysts and foreign analysts do not have prior expertise on the Swiss regulation. Hence, we further divide the group of non-experienced analysts among *Foreigner* and *Local* in columns 5 and 6. The results for the β estimates are similar to the findings in table 1.7, the coefficient for foreign non-experienced analysts in column 5 is equal to −1.276 and significant at the 1% level (with a t-statistic of −3.06) whereas the coefficient for local non-experienced analysts in column

6 is equal to −1.630 and non-significant (with a close to significance t-statistic of −1.58). Table 1.8 provide evidence on the third hypothesis: the decrease in accuracy due to the switch from IFRS to Swiss GAAP is mainly explained by analysts inexperienced with the Swiss regulation hence being unprepared for the change in regulation from the firms they follow. Furthermore, the results provide weak evidence of a higher impact on foreign analysts compared to local analysts when unfamiliar with the newly adopted domestic regulation by the firms.

1.5. Conclusion

This paper studies the departure from IFRS for small to mid-cap companies in Switzerland in order to assess the impact of IFRS information overload in the recent years. In the trade-off between accounting harmonization with IFRS and competitiveness with Swiss GAAP FER's lower costs, the choice for competition impacts firms' information environment quantitatively and qualitatively. Quantitatively, firms lose dispensers of information, as both foreign and local analysts tend to go away from firms that abandon IFRS probably due to Swiss GAAP lower attractiveness and lower comparability benefits. Qualitatively, the predictions released for their earnings are of higher quality when issued by analysts already familiar with the Swiss domestic standard. For analysts prepared for the switch from IFRS to Swiss GAAP, their predicting skills seem to improve despite the decrease in accounting information provided in annual reports after the change. This is evidence of Swiss GAAP lower complexity and the difficulties encountered by analysts to understand financial information disclosed in accordance with IFRS requirements.

Despite the weaknesses of this experiment, some policy implications can be drawn from its findings. IFRS's "information overload" for this specific group seems difficult to comply with and produces difficult accounting information to interpret for a certain group of analysts. Those findings indicate that Swiss GAAP provides analysts already experimented with the regulation the necessary accounting information they need and questions the usefulness of higher disclosure for small to mid-cap companies. Cost savings and easier to understand accounting information seem to compensate the loss of information and international recognition that firms experience when going back to Swiss GAAP. For this specific group, the marginal benefits of complex accounting information are lower than the benefits from simplified accounts.

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Figures and Tables

Figure 1.1. Existing Literature classification on analyst forecast accuracy

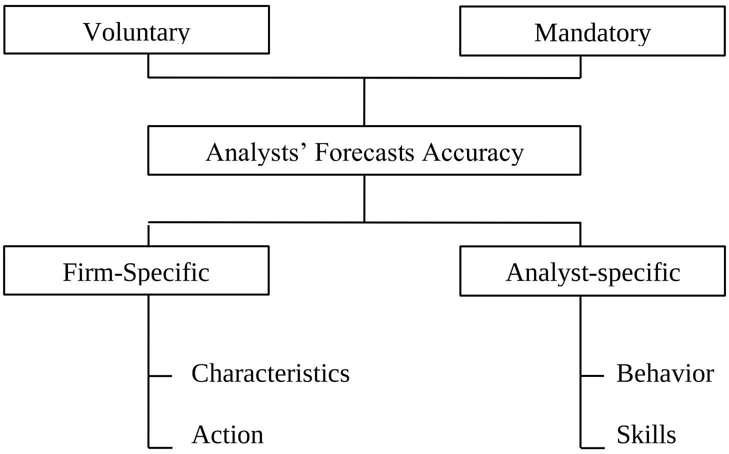

Figure 1.2. Timeline for control variables

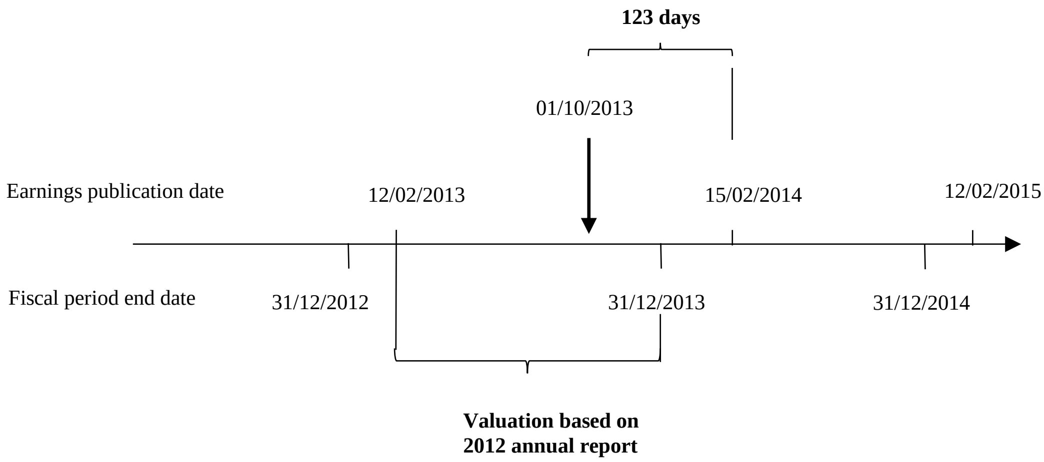

Table 1.1. Descriptive Statistics firm-level data

Panel A: Treatment and control group descriptvie statistics

Variable	*Observations*			*Mean*			*Standard Deviation*		
	Total	*Non-Switcher*	*Switcher*	*Total*	*Non-Switcher*	*Switcher*	*Total*	*Non-Switcher*	*Switcher*
Coverage	992	790	202	6.465	7.097	3.832	6.667	6.947	4.590
Foreigner	992	790	202	2.523	2.900	1.050	4.844	5.146	2.997
Local	992	790	202	3.942	4.197	2.782	2.587	2.598	2.181
GAAP	992	790	202	1.000	0.826	1.678	1.430	1.331	1.608
NoGAAP	992	790	202	5.460	6.304	2.158	7.282	7.465	5.383
GAAPforeign	992	790	202	0.000	0.000	0.000	0.000	0.000	0.000
GAAPlocal	992	790	202	0.780	0.800	0.703	0.932	0.965	0.786
Switch	992	790	202	0.092	0.000	0.450	0.289	0.000	0.499
Size	992	790	202	20.884	21.125	19.940	1.652	1.658	1.243
Mbook	992	790	202	2.502	2.821	1.255	5.272	2.358	10.642
Asset	992	790	202	2.234	2.313	1.928	2.262	1.053	4.555
RoA	992	790	202	4.991	5.088	4.611	10.935	11.403	8.884
Loss	992	790	202	0.096	0.084	0.144	0.294	0.277	0.352
Return	992	790	202	0.334	0.297	0.469	1.750	1.766	1.681
Volatility	992	790	202	3.241	3.093	3.818	4.168	4.002	4.731

Panel B: Pre and post treatmeent descriptive statistics

Variable	*Observations*			*Mean*			*Standard Deviation*		
	Total	*Pre-Switch*	*Post-Switch*	*Total*	*Pre-Switch*	*Post-Switch*	*Total*	*Pre-Switch*	*Post-Switch*
Coverage	202	111	91	3.832	4.847	2.593	4.590	4.989	3.715
Foreigner	202	111	91	1.050	1.378	0.648	2.997	3.289	2.558
Local	202	111	91	2.782	3.468	1.945	2.181	2.366	1.580
GAAP	202	111	91	1.678	1.090	2.396	1.608	1.372	1.591
NoGAAP	202	111	91	2.158	3.766	0.198	5.383	5.587	4.415
GAAPforeign	202	111	91	0.000	0.000	0.000	0.000	0.000	0.000
GAAPlocal	202	111	91	0.703	0.468	0.989	0.786	0.698	0.796
Switch	202	111	91	0.450	0.000	1.000	0.499	0.000	0.000
Size	202	111	91	19.940	19.999	19.867	1.243	1.203	1.292
Mbook	202	111	91	1.255	1.753	0.647	10.642	1.070	15.838
Asset	202	111	91	1.928	2.332	1.435	4.555	1.009	6.682
RoA	202	111	91	4.611	4.045	5.302	8.884	7.059	10.701
Loss	202	111	91	0.144	0.171	0.110	0.352	0.378	0.314
Return	202	111	91	0.469	0.796	0.070	1.681	1.826	1.392
Volatility	202	111	91	3.818	3.666	4.004	4.731	4.230	5.297

The final sample contains 120 firms with 992 firm-year observations from 2006 until 2016. Switcher are firms that change their accounting standard from IFRS to Swiss GAAP FER. Non-Switcher are firms that did not change their accounting standard over the sample period. *Coverage* is the number of analysts following a firm each year. *Foreigner* and *Local* are respectively the number of non-swiss and swiss analysts. *GAAP* is the number of analysts per frim having prior knowledge on Swiss GAAP. *NoGAAP* is the number of analysts per frim having no prior knowledge on Swiss GAAP. *GAAPforeign* and *GAAPlocal* are respectively the number of analysts with prior knowledge on Swiss GAAP among foreign and local analysts. See Appendix 1.B for variables definitions.

Table 1.2. Correlations firm-level data

	Coverage	*Foreigner*	*Local*	*GAAP*	*NoGAAP*	*Switch*	*Size*	*Mbook*	*Asset*	*RoA*	*Loss*	*Return*	*Volatility*
Coverage	1.000												
Foreigner	0.948***	1.000											
Local	0.803***	0.572***	1.000										
GAAP	-0.345***	-0.301***	-0.326***	1.000									
NoGAAP	0.983***	0.927***	0.799***	-0.513***	1.000								
Switch	-0.185***	-0.123***	-0.245***	0.310***	-0.230***	1.000							
Size	0.732***	0.695***	0.585***	-0.319***	0.732***	-0.196***	*1.000*						
Mbook	0.166***	0.139***	0.166***	-0.167***	0.184***	-0.112***	*0.191****	1.000					
Asset	0.085***	0.071**	0.087***	-0.120***	0.102***	-0.112***	*0.045*	0.844***	1.000				
RoA	0.119***	0.076**	0.163***	0.081**	0.093***	0.009	*0.297****	-0.011	-0.080**	1.000			
Loss	-0.127***	-0.086***	-0.166***	-0.012	-0.114***	0.015	*-0.303****	-0.003	0.039	-0.630***	1.000		
Return	-0.104***	-0.106***	-0.071**	-0.018	-0.092***	-0.048	*-0.160****	-0.006	0.041	-0.003	0.040	1.000	
Volatility	-0.142***	-0.119***	-0.143***	0.063**	-0.142***	0.058*	*-0.283****	-0.017	0.037	-0.262***	0.304***	0.154***	1.000

The final sample contains 120 firms with 992 observations from 2006 until 2016. *Coverage* is the number of analysts following a firm each year. *Foreigner* and *Local* are respectively the number of non-swiss and swiss analysts. *GAAP* is the number of analysts per frim having prior knowledge on Swiss GAAP. *NoGAAP* is the number of analysts per frim having no prior knowledge on Swiss GAAP. See Appendix 1.B for variables definitions. *, **, and *** represent significance level at 5%, 1%, and 0.1% respectively.

Table 1.3. Descriptive statistics analyst-level data

Panel A:

Variable	*Observations*			*Mean*			*Standard Deviation*		
	Total	*Non-Switcher*	*Switcher*	*Total*	*Non-Switcher*	*Switcher*	*Total*	*Non-Switcher*	*Switcher*
Accuracy	5'732	5'247	485	-2.626	-2.600	-2.907	7.955	8.012	7.310
Time	5'732	5'247	485	169.755	169.257	175.144	71.489	72.154	63.676
FirmExp	5'732	5'247	485	5.061	5.025	5.447	4.550	4.566	4.355
BrokerSize	5'732	5'247	485	11.312	11.315	11.282	5.037	5.098	4.321
Coverage	5'732	5'247	485	13.309	13.719	8.878	9.422	9.469	7.609
GAAP	5'732	5'247	485	0.135	0.120	0.293	0.342	0.326	0.456
NoGAAP	5'732	5'247	485	0.864	0.879	0.707	0.342	0.325	0.455
GAAPforeign	5'732	5'247	485	0.000	0.000	0.000	0.000	0.000	0.000
GAAPlocal	5'732	5'247	485	0.869	0.850	1.074	1.016	0.993	1.221
Size	5'732	5'247	485	22.187	22.287	21.102	1.942	1.917	1.875
Mbook	5'732	5'247	485	3.467	3.621	1.801	3.416	2.838	6.917
Asset	5'732	5'247	485	2.459	2.516	1.840	1.394	1.125	2.980
RoA	5'732	5'247	485	6.514	6.531	6.325	9.262	9.379	7.896
Loss	5'732	5'247	485	0.050	0.046	0.101	0.218	0.209	0.302
Return	5'732	5'247	485	0.131	0.108	0.374	1.475	1.464	1.575
Volatility	5'732	5'247	485	2.639	2.557	3.525	3.533	3.446	4.272

Panel B

Variable	*Observations*			*Mean*			*Standard Deviation*		
	Total	*Pre-Switch*	*Post-Switch*	*Total*	*Pre-Switch*	*Post-Switch*	*Total*	*Pre-Switch*	*Post-Switch*
Accuracy	485	284	201	-2.907	-3.076	-2.668	7.310	7.289	7.350
Time	485	284	201	175.144	168.447	184.607	63.676	55.838	72.435
FirmExp	485	284	201	5.447	3.873	7.672	4.355	3.404	4.584
BrokerSize	485	284	201	11.282	12.349	9.776	4.321	4.599	3.371
Coverage	485	284	201	8.878	10.035	7.244	7.609	7.669	7.232
GAAP	485	284	201	0.293	0.183	0.448	0.456	0.387	0.499
NoGAAP	485	284	201	0.707	0.817	0.552	0.455	0.387	0.498
GAAPforeign	485	284	201	0.000	0.000	0.000	0.000	0.000	0.000
GAAPlocal	485	284	201	1.074	0.673	1.642	1.221	1.113	1.141
Size	485	284	201	21.102	21.135	21.055	1.875	1.801	1.978
Mbook	485	284	201	1.801	1.931	1.617	6.917	0.964	10.697
Asset	485	284	201	1.840	1.985	1.635	2.980	0.833	4.521
RoA	485	284	201	6.325	6.274	6.397	7.896	7.006	9.024
Loss	485	284	201	0.101	0.116	0.080	0.302	0.321	0.271
Return	485	284	201	0.374	0.688	-0.070	1.575	1.585	1.451
Volatility	485	284	201	3.525	3.455	3.624	4.272	3.880	4.781

The final sample contains 120 firms with 5,732 firm-year observations from 2006 until 2016. Switcher are firms that change their accounting standard from IFRS to Swiss GAAP FER. Non-Switcher are firms that did not change their accounting standard over the sample period. *Switch* is an indicator variable for the change in accounting standard equal to 1 if the firm now publishes EPS under Swiss GAAP and not in accordance with IFRS anymore. *Coverage* is the number of analysts following a firm each year. See Appendix 1.B for variables definitions.

Table 1.4. Correlations analyst-level data

	Accuracy	*Switch*	*Time*	*FirmExp*	*BrokerSize*	*Coverage*	*GAAP*	*NoGAAP*	*Size*	*Mbook*	*Asset*	*RoA*	*Loss*	*Return*	*Volatility*
Accuracy	1														
Switch	-0.001	1													
Time	-0.072***	0.040***	1												
FirmExp	0.048***	0.109***	-0.002	1											
BrokerSize	-0.031**	-0.058***	-0.006	-0.093***	1										
Coverage	0.052***	-0.123***	-0.181***	-0.012	-0.104***	1									
GAAP	-0.017	0.174***	0.027**	0.123***	-0.069***	-0.274***	1								
NoGAAP	0.017	-0.174***	-0.027**	-0.123***	0.069***	0.274***	-1	1							
Size	0.115***	-0.111***	-0.176***	0.107***	-0.174***	0.815***	-0.229***	0.229***	1						
Mbook	0.112***	-0.103***	-0.077***	0.026*	-0.032**	0.272***	-0.130***	0.130***	0.387***	1					
Asset	0.009	-0.113***	-0.070***	-0.046***	-0.021	0.142***	-0.068***	0.068***	0.124***	0.634***	1				
RoA	0.211***	-0.002	-0.066***	0.065***	0.013	0.171***	-0.001	0.001	0.356***	0.229***	-0.059***	1			
Loss	-0.235***	0.026*	0.044***	-0.066***	0.037***	-0.146***	-0.002	0.002	-0.313***	-0.085***	0.004	-0.633***	1		
Return	-0.046***	-0.026*	0.030**	-0.093***	0.075***	-0.177***	0.046***	-0.046***	-0.251***	-0.103***	-0.001	-0.082***	0.094***	1	
Volatility	-0.142***	0.053***	0.006	-0.063***	0.013	-0.236***	0.071***	-0.071***	-0.351***	-0.104***	-0.025*	-0.263***	0.337***	0.233***	1

The final sample contains 120 firms with 5,732 firm-year observations from 2006 until 2016. Switcher are firms that change their accounting standard from IFRS to Swiss GAAP FER. Non-Switcher are firms that did not change their accounting standard over the sample period. See Appendix 1.B for variables definitions. *, **, and *** represent significance level at 5%, 1%, and 0.1% respectively.

Table 1.5. Univariate difference-in-differences

Panel A: Coverage

			(a)	(b)	
	Group	*N*	*Pre-Switch*	*Post-Switch*	*(a)-(b)*
(1)	*Switcher*	202	4.847	2.593	2.253***
					(0.63)
(2)	*Control Group*	202	3.910	4.461	-0.552
					(0.56)
(2)-(1)			-0.937	1.868***	**-2.805*** **
			(0.58)	(0.61)	**(-0.84)**

Panel B: Local

			(a)	(b)	
	Group	*N*	*Pre-Switch*	*Post-Switch*	*(a)-(b)*
(1)	*Switcher*	202	3.468	1.945	1.523***
					(0.29)
(2)	*Control Group*	202	3.072	3.330	-0.258
					(0.30)
(2)-(1)			-0.396	1.385***	**-1.781*** **
			(0.29)	(0.30)	**(-0.42)**

Panel C: Foreigner

			(a)	(b)	
	Group	*N*	*Pre-Switch*	*Post-Switch*	*(a)-(b)*
(1)	*Switcher*	202	1.378	0.648	0.730*
					(0.42)
(2)	*Control Group*	202	0.838	1.132	-0.294
					(0.33)
(2)-(1)			-0.540	0.483	**-1.024***
			(0.37)	(0.38)	**(-0.53)**

Panel D: GAAP

			(a)	(b)	
	Group	*N*	*Pre-Switch*	*Post-Switch*	*(a)-(b)*
(1)	*Switcher*	202	1.000	2.374	-1.374***
					(0.19)
(2)	*Control Group*	202	1.117	1.319	-0.202
					(0.21)
(2)-(1)			0.117	-1.055***	**1.172*** **
			(0.18)	(0.24)	**(0.29)**

Panel E: NoGAAP

			(a)	(b)	
	Group	*N*	*Pre-Switch*	*Post-Switch*	*(a)-(b)*
(1)	*Switcher*	202	3.847	0.220	3.627***
					(0.72)
(2)	*Control Group*	202	2.793	3.143	-0.350
					(0.65)
(2)-(1)			-1.054	2.923***	**-3.977*** **
			(0.65)	(0.71)	**(0.97)**

The sample contains 404 firm-year observations from 2006 until 2016. Switcher are firms that change their accounting standard from IFRS to Swiss GAAP FER. The control group is composed of matched firms based on a PSM method. *Coverage* is the number of analysts following a firm each year. *Foreigner* and *Local* are respectively the number of non-Swiss and Swiss analysts. *GAAP* is the number of analysts with prior Swiss domestic GAAP experience. *NoGAAP* is the number of analysts with no prior Swiss domestic GAAP experience. *Switch* is an indicator variable for the change in accounting standard equal to 1 if the firm now publishes EPS under Swiss GAAP and not in accordance with IFRS anymore. See Appendix 1.B for other variables definitions. The standard errors are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1% respectively.

Table 1.6. Impact of the Switch on analysts following

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	*Coverage*	Foreigner	Local	GAAP	NoGAAP
Switch	-1.858***	-0.457**	-1.400***	1.056***	-2.913***
	(-5.81)	(-2.17)	(-5.74)	(5.84)	(-8.04)
Size	-0.055	-0.087	0.032	0.186	-0.241
	(-0.17)	(-0.46)	(0.18)	(1.31)	(-0.64)
Asset	-0.011	-0.029	0.019	0.010	-0.021
	(-0.15)	(-0.60)	(0.43)	(0.28)	(-0.24)
Mbook	-0.013	0.043	-0.056	-0.028	0.015
	(-0.08)	(0.43)	(-0.53)	(-0.31)	(0.08)
RoA	-0.005	-0.002	-0.002	0.005	-0.009
	(-0.30)	(-0.31)	(-0.23)	(0.80)	(-0.52)
Loss	-0.021	0.033	-0.054	0.139	-0.160
	(-0.04)	(0.13)	(-0.17)	(0.67)	(-0.27)
Return	0.043	0.008	0.035	-0.030	0.073*
	(1.41)	(0.50)	(1.48)	(-1.30)	(1.78)
Volatility	-0.016	-0.007	-0.009	0.009	-0.025
	(-0.80)	(-0.80)	(-0.63)	(0.62)	(-0.97)
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
Firm fixed-effects	Yes	Yes	Yes	Yes	Yes
N	992	992	992	992	992
Adj. R2	0.027	0.001	0.043	0.052	0.057

The final sample contains 120 firms with 992 firm-year observations from 2006 until 2016. Switcher are firms that change their accounting standard from IFRS to Swiss GAAP FER. The control group is composed of matched firms based on a PSM method. *Coverage* is the number of analysts following a firm each year. *Foreigner* and *Local* are respectively the number of non-Swiss and Swiss analysts. *GAAP* is the number of analysts with prior Swiss domestic GAAP experience. *NoGAAP* is the number of analysts with no prior Swiss domestic GAAP experience. *Switch* is an indicator variable for the change in accounting standard equal to 1 if the firm now publishes EPS under Swiss GAAP and not in accordance with IFRS anymore. See Appendix 1.B for other variables definitions. The standard errors are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1% respectively.

Table 1.7. Impact of the Switch on financial analysts' accuracy

	Dependent variable: Accuracy			
	(1)	*(2)*		*(3)*
Sample:	*All*	*All*		*All*
Switch	-1.642**	-1.193*	-1.640**	-1.266**
	(-2.15)	(-1.69)	(-2.44)	(-2.01)
Time		-0.007**		-0.007***
		(-2.56)		(-2.83)
FirmExp		-0.062*		-0.014
		(-1.92)		(-0.45)
BrokerSize		0.051		0.043
		(1.51)		(1.33)
Coverage		0.139		0.152**
		(1.65)		(1.99)
Size		0.630		0.312
		(0.95)		(0.54)
Mbook		0.144		0.203
		(0.53)		(0.76)
Asset		-0.483		-0.566
		(-0.70)		(-0.82)
RoA		0.146**		0.157**
		(2.25)		(2.22)
Loss		-4.623***		-4.720***
		(-2.67)		(-2.92)
Return		0.130		0.132
		(0.83)		(0.87)
Volatility		-0.131*		-0.125*
		(-1.85)		(-1.89)
Year fixed-effects	*Yes*	*Yes*	*Yes*	*Yes*
Firm fixed-effects	*Yes*	*Yes*	*Yes*	*Yes*
Analyst fixed-effects	*No*	*No*	*Yes*	*Yes*
N	5732	5732	5732	5732
Adj. R2	0.320	0.359	0.347	0.386

The final sample contains 120 firms with 5,732 firm-year observations from 2006 until 2016. *Accuracy* is measure as the absolut difference between an analyst forecast and the actual earnings of a firm scaled by the last avaialbel stock price form the prior year, the variable obtained is mutliplied by -100 and winsorized at the 1th and 99th percentiles. See Appendix 1.B for other variables definitions. The standard errors are clustered by firms and the resulting t statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1% respectively.

Table 1.8. Impact of the Switch on forecasts accuracy depending on analysts' characteristics

	Dependent variable: Accuracy					
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Sample:	*Foreign analysts*	*Local analysts*	*GAAP Exp.*	*No GAAP Exp.*	*Foreign / No GAAP Exp.*	*Local / No GAAP Exp.*
Switch	-1.276***	-1.249	1.436*	-1.635**	-1.276***	-1.630
	(-3.05)	(-1.65)	(1.79)	(-2.27)	(-3.05)	(-1.58)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Analyst fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
N	2160	3572	774	4958	2160	2798
Adj. R2	0.408	0.384	0.527	0.369	0.408	0.355

The final sample contains 120 firms with 5,732 firm-year observations from 2006 until 2016. *Accuracy* is measure as the absolut difference between an analyst forecast and the actual earnings of a firm scaled by the last avaialbel stock price form the prior year, the variable obtained is mutliplied by -100 and winsorized at the 1th and 99th percentiles. See Appendix 1.B for other variables definitions. The standard errors are clustered by firms and the resulting t statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1% respectively.

Appendix 1.A. Swiss firms switching from IFRS to Swiss GAAP FER

			Analyst coverage	
Ticker	*Company Name*	*Effective date*	*Pre-Switch*	*Post-Switch*
ADVN	Adval Tech Holding	01/01/2010		
ASCN	Ascom	01/01/2015	x	x
APGN	APG SGA SA	01/01/2013	x	x
BANB	Bachem	01/01/2013	x	x
BOBNN	Bobst Group SA	01/01/2015	x	
BOS	Bossard Holding	01/01/2009	x	x
BRKN	Burkhalter Holding	01/01/2013	x	x
BSAN	Bank J Safran Sarasin	01/01/2013	x	
CPGN	Cham Paper Group Holding	01/01/2009	x	
VCH	Charles Voegele Holding AG	01/01/2014	x	x
CIE	CI Com	01/01/2010		
CLXN	Crealogix	01/07/2009		
DCN	Datacolor	01/10/2008		
DAE	Dätwyler Holding	01/01/2010	x	x
ESUN	Edisun Power Europe	01/01/2013		
ELMN	Elma Electronics	01/01/2010		
EDHN	Energiedienst Holding AG	01/01/2014	x	
GSMN	Genolier Swiss Medical Network	01/01/2010		
FIN	Georg Fischer	01/01/2013	x	x
GBMN	Goldbach Group AG	01/01/2014	x	
GUR	Gurit Holding	01/01/2009	x	x
HUE	Hügli Holding	01/01/2009	x	x
ISN	Intershop	01/01/2013	x	x
IPS	I.P.S	01/01/2010		
INI	Infranor Inter	01/05/2009		
KARN	Kardex	01/01/2011	x	x
LOHN	Looser Holding AG	01/01/2015	x	
MBTN	Meyer Burger Technology AG	01/01/2013	x	x
MIKN	Mikron Holding	01/01/2008	x	x
SPA	Mindset Holding	01/01/2010		
MOZN	Mobilezone Holding	01/01/2012	x	
OFN	Orell Füssli Holding	01/01/2011	x	
PEDP	Perrot Duval Holding	01/05/2009		
PUBN	Publigroup	01/01/2012	x	x
STRN	Schlatter	01/01/2012	x	
SFZN	Siegfried Holding	01/01/2012	x	x
SSTN	Sunstar Holding	26/11/2008		
UHR	Swatch	01/01/2013	x	x
TOHN	Tornos Holding	01/01/2014	x	x
VILN	Villars Holding	01/07/2008		
WMN	Walter Meier AG	01/01/2014	x	x
YPSN	Ypsomed Holding	01/04/2011	x	x
ZWM	Zwahlen & Mayr	01/01/2011		

Effective date corresponds to the first retrospective application of Swiss GAAP in the annual report. Out of the 43 Swiss companies switching from IFRS to Swiss GAAP, 29 are covered by analysts before the switch (Pre-Switch) and 20 are covered after (Post-Switch).

Appendix 1.B. Variables definitions and sources

Name	*Description*	*Source*
Accuracy	Absolut difference between analyst *j* forecast for firm *i* at year *t* and the earnings per share (EPS) for the forecasted year multiplied by -100 and scaled by the last available closing price of the firm's stock from the prior year.	IBES
Switch	dummy variable for the change in accounting standard equal to 1 for firm *i* at time *t* if the firm now publishes EPS under Swiss GAAP and not in accordance with IFRS anymore	Thomson Reuters
Coverage	Number of analysts following firm *i* at year *t*	IBES
Foreigner	Number of foreign analysts following firm *i* at year *t*	IBES
Local	Number of local analysts following firm *i* at year *t*	IBES
POST	Dummy variable equal to one if firm *i* is a switching firm and publish its EPS under Swiss GAAP FER	Thomson Reuters
FirmExp	Number of years analyst *j* has been following firm *i* as of year *t*	IBES
BrokerSize	Number of analysts working at the brokerage house where analyst *j* works as of year *t*	IBES
GAAP	Dummy variable equal to 1 if analyst *j* has prior experience on Swiss GAAP over the sample period	Thomson Reuters
NoGAAP	Dummy variable equal to 1 if analyst *j* has no prior experience on Swiss GAAP over the sample period	
GAAPforeign	Number of foreign analysts following firm *i* at year *t* with prior experience on Swiss GAAP over the sample period	Thomson Reuters
GAAPlocal	Number of local analysts following firm *i* at year *t* with prior experience on Swiss GAAP over the sample period	Thomson Reuters
Time	Number of days between the forecast date and the EPS announcement date for firm *i*	IBES
Loss	Dummy variable equal to one if firm *i* has negative EPS for the current year	IBES
Size	Logarithm of market capitalization for firm *i* calculated as the number of outstanding shares multiplied by the last available closing price from the prior year	Thomson Reuters
Mbook	Market-to-book ratio calculated as the market capitalization of firm *i* divided by its book value of equity from the last published annual report	Thomson Reuters
RoA	Return on asset computed as firm *i* net income divided by total assets from the last published annual report	Thomson Reuters
Volatility	Standard deviation of firm *i* stock return at month *t-1*	Thomson Reuters
Return	Monthly stock return of firm *i* at month *t-1*	Thomson Reuters

Chapter 2: Under pressure: The influence of the working environment on analysts' herding behavior co-written with Peter Fiechter (University of Neuchatel) and Annelies Renders (Maastricht University)

2.1. Introduction

Sell-side equity analysts are among the most important information intermediaries in capital markets. However, prior literature has provided evidence that these analysts are prone to herding (Hong et al., 2000; Clement and Tse, 2005). Analysts' herding behavior can be detrimental to capital markets because herding forecasts limit the information available to investors. That is, herding analysts do not fully incorporate their private information but rather follow the average (consensus) forecast when revising their forecasts, in turn increasing information asymmetry (Scharfstein and Stein, 1990; Trueman, 1994; Bradshaw et al., 2017). For example, Clement and Tse (2005) show that herding forecasts incorporate less private information of analysts and provide less relevant information to investors than bold forecasts. Consequently, it is important to understand why and under which circumstances analysts' herding behavior arises.

Prior literature on herding behavior mainly focused on individual analyst characteristics. For example, Hong et al. (2000) and Clement and Tse (2005) find that individual characteristics, such as job experience, prior accuracy, forecast frequency, and the number of companies and industries analysts follow, influence analysts' herding behavior. More recently, Hirshleifer et al. (2019) shows that decision fatigue affects analysts' herding behavior. However, this prior herding literature is largely silent about the influence of contextual factors (i.e., the working environment) on analysts' herding behavior. Only few studies have investigated factors affecting herding behavior beyond individual analyst characteristics such as the regulatory environment (Mensah and Yang, 2008) or the media (Frijns and Huynh, 2018). As anecdotal evidence suggests that the working environment influences analysts' behavior, we examine whether the working environment affects analysts' herding, too.[15]

A few studies provide evidence on the effect of the working environment on analyst forecast bias. For example, Bradshaw et al. (2021) finds that after an IPO of their brokerage house, analysts are less optimistically biased, consistent with increased scrutiny and monitoring during the IPO. In addition, Cowen et al. (2006) provides evidence that analyst optimism varies with the business activities used to fund research and the types of clients that brokerage firms serve.

[15] For example, the New York Times reported about a famous retail analyst who left her employer, Wells Fargo, to start her own firm, stating that "more analysts would be leaving their firms to avoid the day-to-day pressures of issuing earnings reports, and the increasing scrutiny of relationships between analysts and the companies they work for." (New York Times, 2003). Also, Hong and Kacpercyk (2003) point out that "independent analysts who "blow the whistle" tend to come from lower-tier brokerage houses that have lower investment-banking business revenues."

While these studies focus on analyst forecast bias, we investigate analysts' herding behavior, which is a different type of analyst behavior.[16]

We explore acquisitions of private brokerage houses to investigate the effect of the working environment on analysts' herding behavior. We do this for the following reasons. First, we use acquisitions of private brokerage houses as a plausibly exogenous shock to the working environment of the acquired analysts. Prior research indicates that analysts with higher skills and ability are more likely associated with larger and more prestigious brokerage houses (Hong et al., 2000; Hong and Kubik, 2003). Consequently, the relation between analysts' working environment and herding behavior is likely endogenous, which is why we focus on a shock to the working environment of analysts.

Second, acquisitions result in substantial changes to the working environment, which may affect the behavior and performance of the acquired employees (Conyon et al., 2002; Siegel and Simons, 2010). In addition, acquisitions create substantial uncertainty for the acquired employees regarding their future career prospects (e.g., job security concerns). Herding theory suggests that issuing herding forecasts could be optimal for analysts who are concerned about their career prospects (Banerjee, 1992; Brandenburger and Polak, 1996; Graham, 1999; Prendergast and Stole, 1996; Scharfstein and Stein, 1990; Welch, 1992; Zwiebel, 1995). Consequently, the uncertainty about future career prospects that comes with being acquired by another brokerage house could induce acquired analysts to herd more, especially less experienced analysts who typically exhibit higher career concerns (Clement and Tse, 2005). In addition, acquired analysts could be forced to cover new firms after the acquisition (Clement, 1999; Wu and Zang, 2009), potentially resulting in more herding behavior.

We begin our empirical analyses by investigating whether analysts employed at a privately held brokerage house (hereafter, "private analysts") who were subsequently acquired by another brokerage house herd more after the acquisition compared to before, using private analysts who were never acquired during our sample period as a control group.[17] To measure herding behavior, we construct a refined version of the continuous herding proxy used by Gleason and Lee (2003) and Clement and Tse (2005). We define *Herding* as the yearly sum of analysts' forecast revisions towards the consensus. Our measure has two distinct features: First, we not only consider the distance between the revised and the consensus forecast, but also the pre-revision distance. Our herding measure thus reflects analysts' attempts to move closer to the consensus when revising their forecasts. Second, our herding proxy is an aggregated and dynamic measure of all forecasts issued by an analyst during the fiscal year (instead of a single point in time at year-end). Both features mitigate concerns about "spurious herding" (Bikhchandani and Sharma, 2000), whereby analysts converge mainly because of correlated

[16] For example, analysts' motives to herd are different from the motives to issue biased forecasts. In addition, the implications for capital markets are different for herding forecasts as compared to biased forecasts. Moreover, empirical measures for optimistic bias focus solely on the last forecast issued by an analyst and thus do not shed light on forecasting dynamics (revisions) during the year.

[17] Throughout our empirical analyses, we also present findings using the treatment group only (within-estimation). The advantage of this within-estimation is that it mitigates potential concerns that treatment effects could be driven by unobservable differences across treatment and control group (Christensen et al., 2016), for example factors associated with both the propensity of being acquired and increased herding behavior.

information signals (Kerl and Pauls, 2014). In addition, our measure responds to the call for improvement on the measurement of "herding" by Bradshaw et al. (2017).[18]

To identify broker acquisitions and acquired private analysts, we collect data on analysts from I/B/E/S and data on broker acquisitions from the Securities Data Company (SDC). Appendix 2.A provides an overview of the various broker acquisitions over time. This dataset allows us to track analysts and their (change in) affiliation over time. We then apply a staggered difference-in-differences (DiD) design (e.g., Bertrand and Mullainathan, 2003) with a comprehensive fixed effects structure (i.e., analyst-, covered firm-, and year-fixed effects). Based on a sample of 150,322 analyst-year observations and 122 private broker acquisitions between 1983 and 2016, we find an increase in herding behavior for acquired analysts. This effect is incremental to changes in analysts' characteristics, broker size, and competition—determinants that prior literature finds to be associated with herding. The results are robust to different estimation windows around the acquisition date, clustering methods, and DiD specifications.[19]

In a second step, we examine whether our treatment effect varies with the extent of the shock to analysts' working environment. Specifically, using hand-collected data on the listing status of acquiring brokerage houses, we distinguish between acquisitions by private versus public brokerage houses. We argue that private analysts acquired by a public brokerage house experience a larger shock to their working environment, in turn affecting their herding behavior. Our findings show that the increase in herding is concentrated in analysts acquired by public brokerage houses. We interpret this evidence as indicating that increased peer pressure and career concerns at publicly listed brokerage houses might induce acquired analysts to herd more. For example, Gao et al. (2017) provides evidence that public firms have higher CEO turnover rates and exhibit greater turnover-performance sensitivity than private firms, especially due to shareholders' shorter investment horizons. In contrast, private brokerage houses are usually organized as partnerships (Rajan and Zingales, 2001) and held by majority shareholders who often participate in the management of the firm (Beatty and Harris, 1999; Ke et al., 1999; Beatty et al., 2002).

Next, we shed light on the timing of the herding effects by estimating dynamic DiD models (e.g., Bertrand and Mullainathan, 2003; Giroud, 2013) for acquisitions by public and private brokerage houses separately. The corresponding findings reveal no significant treatment effects in the years before and the year of the acquisition for analysts acquired by public brokerage houses, whereas herding significantly increases in the year after the acquisition and then decreases in the following years. The finding that the treatment effect is nonexistent before the acquisition, strongest right after the acquisition, and decreasing afterwards is consistent with the explanation that the pressure on analysts is particularly pronounced right after the shock to

[18] Our inferences are robust to alternative definitions of herding (see Section 2.2 for a more detailed discussion of our herding measure and online Appendix 2.C for results based on an alternative herding measure).

[19] We use different event windows to address concerns about staggered DiD estimations, especially concerns relating to long estimation windows and serial correlation of outcomes (Bertrand et al., 2004). Appendix 2.D and Appendix 2.G present results based on different event windows and clustering of standard errors, respectively. In addition, online Appendix 2.I presents results based on a "stacked" DiD regression, which addresses concerns about potential bias due to treatment effect heterogeneity (e.g., Goodman-Bacon 2021; Baker et al. 2022).

the working environment. In comparison, we find no such effects for analysts acquired by private brokerage houses, consistent with our previous finding that treatment effects are concentrated in analysts acquired by public brokerage houses. Taken together, the findings from the dynamic DiD models mitigate concerns about pre-existing trends, supporting our inferences that increased herding is attributable to the shock to the working environment.

So far, we have interpreted our findings as indicating that the working environment (e.g., higher job uncertainty and peer pressure) drives herding behavior. A potential alternative explanation for our findings is that acquired analysts are forced to cover new firms due to overlap with portfolios of analysts of the acquiring firm (Clement, 1999; Wu and Zang, 2009). As the acquired analysts may lack experience with the new firms they have to cover, this may (temporarily) result in increased herding behavior. To test this alternative explanation, we rerun the analyses focusing on analysts who cover the same firm before and after the acquisition by a public brokerage house. The results from these analyses still show a significant increase in herding behavior, even though analysts cover the same firm before and after acquisition by a public brokerage house.

As we do not find that changes in coverage drive our findings, we conduct further cross-sectional analyses to shed more light on possible reasons why analysts may herd more after being acquired by a public brokerage house. First, as the (perceived) uncertainty around the acquisition might be more pronounced for less experienced analysts with higher career concerns, we test whether analysts with ex ante lower experience exhibit higher herding behavior after the shock. We find stronger treatment effects for less experienced analysts, but the difference is not statistically significant. Second, herding could be more pronounced for analysts who cover similar firms or sectors as analysts of the acquiring firm, i.e., analysts facing more peer pressure after the acquisition. We find that herding is more pronounced when there are more analysts at the acquiring firm who cover the same sector as the acquired analyst. Third, we find stronger treatment effects for analysts acquired by public brokerage houses with a relatively more pronounced hire-and-fire culture (as measured by the acquirer's historical analyst turnover ratio) and thus lower job security. Finally, exploiting changes in the regulatory environment across time (i.e., Reg FD and SOX 404), we find some (albeit weak) evidence of more pronounced herding behavior for acquired analysts by public brokerage houses in times of stricter regulation and potentially higher day-to-day pressure on analysts. Taken together, the cross-sectional findings are consistent with the explanation that increased peer pressure and job security concerns affect private analysts' herding behavior after the acquisition by a public brokerage house.

We extend prior literature (Hong et al., 2000; Clement and Tse, 2005; Hirshleifer et al., 2019) by documenting that the working environment, beyond individual characteristics, influences analysts' herding behavior. Using a novel and intuitive measure of herding, we show an increase in herding behavior after acquisition by a public brokerage house. As such, our findings inform investors and regulators about factors that potentially hinder genuine and independent forecasting by equity analysts, in turn increasing information asymmetries in financial markets. Our study also relates to the literature investigating the influence of broker mergers, closures, and IPOs on analysts' optimistic forecast bias (Cowen et al., 2006; Hong and Kacperczyk, 2010; Bradshaw et al., 2021). However, our focus on herding behavior distinguishes our study from the forecasting bias literature, as herding is a different phenomenon of analyst behavior, which requires separate investigation. A somewhat related study by Wang et al. (2020) investigates the effect of changes in analyst competition after broker mergers on analyst effort (including herding). However, Wang et al. (2020) focus only on changes in competition, rather than changes in analysts' working environment, and they find no evidence that reduced competition following mergers affects analyst herding. Finally, as our setting and empirical approach allow us to draw causal inferences on the link between working environments and employee (analyst) behavior, our findings add to the literature on organizational behavior (e.g., Rajan and Zingales, 2001; Stein, 2002; Mehran and Stulz, 2007; Wu and Zang, 2009; Pacelli, 2019). In particular, we identify specific circumstances under which analysts are more prone to herding, namely higher peer pressure and a more pronounced hire- and-fire culture.

2.2. Data, variables, and empirical design

2.2.1. Data and sample

To test our research question whether analysts' working environment affects herding behavior, we employ data on earnings forecasts from I/B/E/S from Thomson Reuters. Following the methodology of Wu and Zang (2009), we first match the full list of I/B/E/S' brokerage houses with the data on broker mergers from the SDC Platinum database (see online Appendix 2.A for a more detailed description). This procedure allows us to track analysts in the pre- and post-acquisition period. We use the I/B/E/S broker translation files to obtain the identifier, names, and tracking codes of the brokerage houses, and we match them with the merger events collected from SDC as well as other sources, such as FINRA-BrokerCheck, the Institution Search database of the National Information Center of the Federal Financial Institutions Examination Council (FFIEC), the Investment Adviser Information Reports of the Security Exchange Commission (SEC), Bloomberg Businessweek, LexisNexis, Wikipedia, and company webpages. We then hand-collect information on the listing status of the brokerage houses and identify broker mergers for which the target is a private brokerage house. Over the sample period from 1983 until 2016, we identify a total of 122 private broker acquisitions (see Appendix 2.A).

We obtain the data on analysts' forecasts from I/B/E/S Detailed Earnings History Files from 1983 (i.e., the beginning of the I/B/E/S database) until 2016, spanning 34 years. Consistent with prior studies employing annual earnings estimates, we retain earnings estimates of analyst i for a specific stock j in a given year t, at least 30 days and at most one year prior to the end of stock j's financial period (O'Brien, 1990; Sinha et al., 1997; Clement, 1999; Clement and Tse, 2005). We eliminate all observations for which only one forecast per stock-year exists or one forecast per broker-year exists, as well as observations with no prior year data on the analyst's forecast error. In addition, we exclude observations with no data on the analyst's broker affiliation. Further, we follow Clement and Tse (2005) and eliminate potential outliers. First, we obtain stock prices from CRSP. Second, we facilitate comparisons across stocks by deflating forecast revisions and forecast errors by the firm's stock price two days before the forecast revision date. Third, we omit outliers with these price-deflated forecast revisions above 0.10 or below −0.10 and discard observations with a price-deflated analyst error above 0.40 or below −0.40.

Next, for the analysts working at a broker matched with the merger data from SDC, we retain analysts working at a private broker who are subsequently acquired.[20] We use a control group comprising all other private analysts: These are analysts working for private brokers that are never acquired during the sample period. The sample selection yields an unbalanced panel data set of 150,322 analyst-year observations from 4,672 unique analysts working at 572 brokerage houses from 1983 to 2016.

2.2.2. Dependent variable

We define the dependent variable *Herding* as the yearly sum of analysts' forecast revisions towards the consensus. We focus on forecast revisions because analysts have to decide whether to follow the herd or use their own private information (i.e., issuing a bold forecast) before issuing a new forecast. We start from the continuous boldness measures $YTD\_Dist1_{f\text{-}1ijt}$ and YTD_Dist2_{fijt} from Clement and Tse (2005), where $F^{Pre\text{-}revision}_{f\text{-}1ijt}$ ($F^{Revision}_{fijt}$) denotes analyst *i*'s pre-revision (revised) forecasts *f-1* (*f*) concerning firm *j* in year *t*; and $C^{Pre\text{-}revision}_{fjt}$ is the pre-revision consensus forecast (see Equations 1 and 2 below).[21] All forecasts are scaled by the stock price of firm *j* two days before the forecast announcement date (Clement and Tse, 2005).

$$YTD\_Dist1_{f\text{-}1ijt} = \left| F^{Pre\text{-}revision}_{f\text{-}1ijt} - C^{Pre\text{-}revision}_{fjt} \right| \quad (2.1)$$

$$YTD\_Dist2_{fijt} = \left| F^{Revision}_{fijt} - C^{Pre\text{-}revision}_{fjt} \right| \quad (2.2)$$

We then calculate the difference between (1) the absolute distance of analyst *i*'s pre-revision forecast from the pre-revision consensus forecast ($YTD\_Dist1_{f\text{-}1ijt}$), and (2) the absolute distance of analyst *i*'s revised forecast from the pre-revision consensus forecast (YTD_Dist2_{fijt}). Higher values of this difference ($YTD\_Dist1_{f\text{-}1ijt} - YTD\_Dist2_{fijt}$) indicate that the analyst moves closer to the consensus when revising her forecast compared to the pre-revision

[20] Due to this restriction, our results do not speak to the effect of a broker acquisition on herding behavior of analysts who leave (voluntary or involuntary) the brokerage house right after the acquisition. To mitigate concerns that our findings are driven by analysts leaving the acquirer after the acquisition (e.g., 4 years after the merger), we conduct a dynamic DiD in Section 3.4, and we perform robustness checks with smaller event windows (see online Appendix B).

[21] The consensus forecast consists of all other analysts' forecasts covering firm *j*, issued 90 days prior to analyst *i*'s revision (Clement and Tse, 2005). Our inferences are robust to alternative windows to calculate the consensus forecast (i.e., 60 and 120 days).

distance, which is consistent with herding behavior.[22] Figure 1 outlines the timeline of analyst forecasts, consensus forecasts, and forecast revisions.

[Figure 2.1. about here]

Next, we define *Herding* as the sum of analyst *i'*s forecast revisions for firm *j* during year *t* (see Equation 3 below). By aggregating the forecast revisions over the year—instead of considering only a single point in time at year-end—we measure analyst herding during a year. Therefore, higher values of *Herding* indicate analysts' average tendency to revise their forecasts towards the consensus.

$$Herding_{ijt} = \sum_{f=1}^{n} \left(YTD\_Dist1_{f\text{-}1ijt} - YTD\_Dist2_{fijt} \right) \qquad (2.3)$$

Extant herding measures are often criticized for measuring "spurious herding" (Bikhchandani and Sharma, 2000). That is, analysts converge due to correlated information signals, unexpected common shocks, or systematic optimism or pessimism (Kerl and Pauls, 2014). Our refined herding measure addresses this criticism via two distinct features: First, we do not only consider the distance of the revised forecasts from the consensus, but also the pre-revision distance. Second, our herding proxy is an aggregated measure of all forecasts issued by an analyst during a year. As such, our measure consists of multiple observations over the year, decreasing the likelihood that our measure picks up spurious herding.[23]

2.2.3. Empirical model

Following the empirical methodology of Bertrand and Mullainathan (2003), we run an OLS regression of our herding measure (*Herding*) on *Treated*, controlling for other firm- and analyst-specific differences. We define *Treated* as an indicator variable that is equal to one for private analysts after the acquisition by another brokerage house, and zero otherwise. As the treatment events occur at different points in time, the indicator variable *Treated* is staggered over time. The empirical model is as follows:

$$Herding_{ijt} = \alpha_i + \alpha_j + \alpha_t + \beta Treated_{it} + \gamma' X_{ijt} + \varepsilon_{ijt} \qquad (2.4)$$

α_i, α_j, and α_t are analyst-, covered firm-, and year-fixed effects, respectively. The coefficient of interest measuring the impact of the change in working environment on herding behavior is β, X is a vector of control variables, and ε is the error term. The introduction of analyst-, covered

[22] For illustration of our herding measure, consider the following example: Analyst A's and Analyst B's prior forecasts were both 5 and the pre-revision consensus is 2. If Analyst A (B) revises the forecast to 1.5 (4.5), the Herding measure is calculated as follows: Analyst A's herding score = |5 – 2| – |1.5 – 2| = 2.5; Analyst B's herding score |5 – 2| – |4.5 – 2| = 0.5. Accordingly, Analyst A herds more than Analyst B (see Figure 1 for more examples).

[23] In online Appendix C, we discuss and use an alternative measure of herding behavior, which is based on the categorical *Bold* measure by Clement and Tse (2005) and Hirshleifer et al. (2019). Our inferences do not change when using this alternative herding measure.

firm-, and year-fixed effects ensures that the coefficient on *Treated* measures within analyst variations, while controlling for covered firm-specific factors and time trends. The use of a staggered event indicator combined with our fixed effects structure yields a counterfactual control group of analysts who do not experience changes in their working environment at time *t.* We also perform tests focusing on the treatment group only (within-estimation) to mitigate concerns that treatment effects could be driven by simultaneous, unobservable differences across the treatment and control group (Christensen et al., 2016), such as factors associated with both the propensity of being acquired and increased herding behavior. In addition, to address concerns about long estimation windows (e.g., Bertrand et al., 2004), we conduct tests based on restricted event windows (e.g., 5-year window) around the treatment events.[24] Given the recent critique towards staggered DiD designs and the use of already-treated observations in the control group (e.g., Goodman-Bacon, 2021; Baker et al., 2022), we also perform "stacked" DiD regressions (e.g., Gormely and Matsa, 2011) and discuss respective results in Appendix 2.I.

In addition to our fixed effects structure, we construct a comprehensive set of control variables. First, following Hong and Kubik (2003) and Clement and Tse (2005), we control for the analysts' self-assessed ability (*lagAFE*), defined as analyst *i*'s absolute forecast error of last year (*t-1*) for stock *j*. The absolute forecast error is the absolute difference between the analyst's last earnings estimate and the firm's actual EPS, both scaled by the stock price of firm *j* two days before the last revision. Next, we follow prior research and control for other analyst, firm, and broker characteristics (Lim, 2001; Hong et al., 2000; Hong and Kubik, 2003; Jackson, 2005; Clement and Tse, 2005; Hilary and Hsu, 2013). *DaysElapsed* counts the days elapsed since the last forecast by any other analyst following the same firm. *ForHorizon* measures the number of days between the forecast date and the end of the fiscal period. Both *DaysElapsed* and *ForHorizon* are computed for all forecasts of an analyst during each year. *ForFrequency* is the number of forecasts an analyst issued for a specific firm during the year. *FirmExperience* is the number of years an analyst has been covering a specific firm. *BrokerSize* is the number of analysts working for a specific brokerage house in a given year. As broker acquisitions typically also lead to changes in competition (Hong and Kacperczyk, 2010; Wang et al., 2020), we control for changes in competition. We measure the level of competition (*Competition*) for analyst *i* in year *t* covering firm *j* as the number of other analysts following the same firm *j* in year *t*. Finally, *Companies* is the number of companies covered by an analyst in a given year and *Industries* counts the number of two-digit SIC codes that an analyst covers in a given year. Appendix 2.B provides detailed variable descriptions.

[24] We cluster standard errors by covered firm to account for the potential presence of serial correlation in the data (Bertrand et al., 2004). Our results are robust to alternative clustering, including clustering by analyst, brokerage house, covered firm-by-year, analyst-by-year, and broker-by-year (see online Appendix D).

2.3. Results

2.3.1. Descriptive statistics

Panel A of Table 1.1 presents descriptive statistics for the full sample (treatment and control group). The mean of *Herding* is 2.66, the median is 1.45, and the standard deviation is 23.35. Although mean and median are positive, *Herding* can also be negative (e.g., 25th percentile is −6.06), reflecting relatively bold forecast revisions (anti-herding). Most analysts make rather accurate prior forecasts, with only few analysts issuing very poor forecasts as indicated by the skewed distribution of lagged absolute forecast error (*LagAFE*; mean = 8.52, median = 2.13). Each year analysts issue around four forecasts per covered firm (*ForFrequency*), and they issue their forecasts on average 11 days after another analyst covering the same stock issues a forecast (*DaysElapsed*). They do this on average 190 days before the firm's financial period ends (*ForHorizon*). On average, analysts are working with a brokerage house that employs about 33 analysts, with a median of 21 analysts (*BrokerSize*). The average analyst has an experience per covered firm of more than 4 years (*FirmExperience*), covers around 14 companies (*Companies*), and almost 8 industries (*Industries*). Finally, firms are on average followed by 13 other analysts (*Competition*).

[Table 2.1 about here]

Panel B provides descriptive statistics for the treatment group, while Panel C presents descriptives for the control group. The treatment group represents 36 percent of the overall sample, consistent with the observation in Wu and Zang (2009) that a significant part of the analysts from the I/B/E/S database are affected by broker acquisitions. While all variables are very similar across treatment (Panel B) and control group (Panel C) in terms of means, medians, and standard deviations, *BrokerSize* is higher in the treatment group (mean value of 49 analysts) compared to the control group (mean value of 23.5 analysts), which is likely the result of the acquisition activities.

2.3.2. Broker acquisitions and analyst herding

Table 2.2 presents regression results for different specifications of Equation (4). In column (1), we use the full sample (treatment and control group). In column (2), we focus on the treatment group only (i.e., within-estimation). In columns (3) and (4), we present results for the full sample and within-estimation while restricting the estimation window to five years around the acquisition events.[25]

[25] While we tabulate results based on the full event and a restricted 5-year event window, our inferences do not change when using shorter event windows (see online Appendix B)

In column (1) of Table 2.2, the coefficient on *Treated* of 0.837 is significantly positive (*t*-stat = 2.85), suggesting that acquired private analysts issue more herding forecasts after the acquisition compared to before the acquisition and compared to nonacquired analysts. This finding is also economically significant, indicating a 31% increase in herding relative to the sample mean for *Herding*. The coefficients on the control variables are also significant, except for *Companies* and *Industries*. The coefficients on *ForFrequency* and *BrokerSize* are negative, suggesting that analysts issuing forecasts more frequently and working for larger brokers, respectively, tend to herd less in their forecast revisions. Furthermore, the negative coefficients on *ForHorizon* and *DaysElapsed* indicate that analysts herd less when they are further away from firms' financial period-ends and when more time has elapsed since the last estimate of another analyst following the same firm, respectively. In addition, the negative and significant coefficient on *Competition* indicates that when competition increases, herding behavior decreases, and analysts will place more weight on their private information. Finally, the coefficients on *LagAFE* and *FirmExperience* are positive, indicating more herding behavior by analysts with higher past absolute forecast errors (i.e., lower self-assessed ability) and more firm-specific experience, respectively.

Column (2) presents the results of the within-estimation. We find a significant and positive coefficient on *Treated* (coef = 1.031, *t*-stat = 2.98), consistent with the findings in column (1). In addition, the coefficient estimates for the control variables are very similar to those reported in column (1), except for the now insignificant coefficient on *FirmExperience*. Analysts tend to be bolder in their revisions when they have lower past forecast errors, release more forecasts, work for larger brokerage houses, and are among the first to release forecasts. These associations are plausible, and thus increase confidence in our herding measure.

[Table 2.2 about here]

The results in columns (3) and (4) based on a restricted estimation window of five years around the acquisition date are very similar compared to those reported in columns (1) and (2). As such, these findings mitigate concerns that our treatment effects are driven by serial correlation and long estimation windows. Taken together, these findings suggest that the shock to the working environment of analysts affects their herding behavior.

2.3.3. Acquisitions by public versus private brokerage houses

The results discussed above indicate that private analysts herd more after being acquired by another brokerage house. Next, we examine whether this effect varies with the extent of the shock to analysts' working environment by distinguishing between acquisitions by private and public brokerage houses. We expect that private analysts acquired by a public brokerage house experience a larger shock to their working environment, in turn affecting their herding behavior.

We hand-collected data on the listing status of acquiring brokerage houses to distinguish acquisitions by private and public brokerage houses, and we create two nonoverlapping indicator variables, namely *TreatedPublic* and *TreatedPrivate*. *TreatedPublic* is equal to one for a private analyst after the acquisition by a public brokerage house, and zero otherwise. *TreatedPrivate* is equal to one for a private analyst after the acquisition by a private brokerage house, and zero otherwise. In Table 2.3, Panels A and B, we first present descriptive statistics for private analysts acquired by public and private brokerage houses separately. We observe similar descriptive statistics for the two groups. Note, however, that given our staggered DiD, the differences in descriptive results should be interpreted with caution because, for example, the treatment group serves as its own control before being treated (Bertrand and Mullainathan, 2003).

[Table 2.3 about here]

The regression results are presented in Panel C of Table 2.3, where column (1) presents the findings for the full sample and column (2) the within-estimation. Again, the remaining columns (3) and (4) present the findings using a 5-year window around the acquisition date. Our findings show that the increase in herding is concentrated in analysts acquired by public brokerage houses, as the coefficient on *TreatedPublic* is significantly positive across all four columns. In comparison, the coefficient on *TreatedPrivate* is positive but insignificant in all specifications. An F-test on the difference in the coefficients indicates that the differences are marginally significant for the within-estimations, while the coefficients are not significantly different for the full sample estimations.

We interpret the findings in Table 2.3 as (weak) evidence that the particular working environment at public brokerage houses (e.g., increased peer pressure or greater career concerns) induces acquired analysts to herd more. For example, Gao et al. (2017) show that, compared to private firms, public firms have higher CEO turnover rates due to investors' shorter investment horizons. In comparison, private brokerage houses are usually organized as partnerships (Rajan and Zingales, 2001) with majority shareholders participating in the management of the firm (e.g., Beatty and Harris, 1999). These institutional differences across private and public brokerage houses potentially explain our finding that treatment effects are concentrated in analysts acquired by public brokerage houses.

2.3.4. Dynamic effects

Next, we investigate the dynamic effects of the acquisitions on herding behavior. Similar to Bertrand and Mullainathan (2003) and Giroud (2013), we replace our treatment indicator variable, *Treated*, by a set of indicator variables for different periods around the acquisition: $TreatedPublic^{-2}$ ($TreatedPrivate^{-2}$) is an indicator variable equal to one for private analysts who are acquired by a public (private) brokerage house two years before the acquisition; $TreatedPublic^{-1}$ ($TreatedPrivate^{-1}$) is an indicator variable equal to one for private analysts who are acquired by a public (private) brokerage house one year before the acquisition;

TreatedPublic^{0} (*TreatedPrivate*^{0}) is an indicator variable equal to one for private analysts who are acquired by a public (private) brokerage house in the year of the acquisition; *TreatedPublic*^{1} (*TreatedPrivate*^{1}) is an indicator variable equal to one for private analysts who are acquired by a public (private) brokerage house one year after the acquisition; *TreatedPublic*$^{2+}$ (*TreatedPrivate*$^{2+}$) is an indicator variable equal to one for private analysts who are acquired by a public (private) brokerage house two or more years after the acquisition. In a second specification, we further replace *TreatedPublic*$^{2+}$ (*TreatedPrivate*$^{2+}$) by *TreatedPublic*^{2} (*TreatedPrivate*^{2}) and *TreatedPublic*$^{3+}$ (*TreatedPrivate*$^{3+}$).[26, 27]

[Table 2.4 about here]

Table 2.4, Panel A presents the result of the dynamic effect of the change in working environment on herding behavior for acquisitions by public brokerage houses. The indicator variables *TreatedPublic*$^{-2}$, *TreatedPublic*$^{-1}$, and *TreatedPublic*^{0} allow us to assess whether analysts' herding behavior already changed prior to or at the acquisition. Finding such an effect would be an indication of a predetermined trend. However, in columns (1) and (2), the coefficients on *TreatedPublic*$^{-2}$, *TreatedPublic*$^{-1}$, and *TreatedPublic*^{0} are economically and statistically insignificant, suggesting that herding behavior is not trending ahead of the treatment. In contrast, the coefficients on *Treated*^{1} are larger in magnitude and highly significant, suggesting that the effect of the change in working environment on herding manifests in the year after the acquisition. The lower coefficients (in terms of magnitude and significance) on *Treated*$^{2+}$ in column (1) as well as *Treated*^{2} and *Treated*$^{3+}$ in column (2) suggest a decreasing effect over time.

The within-estimations in columns (3) and (4) of Panel A are largely consistent with those based on the full sample. While the coefficients on *TreatedPublic*$^{-2}$ and *TreatedPublic*$^{-1}$ remain statistically and economically insignificant, the estimates of 0.832 (*t*-stat 1.38) and 0.773 (*t*-stat = 1.28) on *TreatedPublic*^{0} in columns (3) and (4), respectively, indicate a slight increase in herding in the year of the acquisition. However, the treatment effect is (again) strongest in the year right after the acquisition. Similar to the findings in columns (1) and (2), the within-estimations in columns (3) and (4) also reveal a decreasing (but not fully diminishing) effect over time, as evidenced by the lower but still significant coefficients on *TreatedPublic*$^{2+}$, *TreatedPublic*^{2}, and *TreatedPublic*$^{3+}$. We interpret this evidence as indicating that peer pressure and career concerns of analysts are particularly pronounced right after the acquisition. In other words, the effect of the shock to analysts' working environment is strongest right after acquisition and then decreases, e.g., due to analysts adjusting to the new working environment over time or due to herding analysts leaving the acquirer after some time.

[26] *TreatedPublic*^{2} (*TreatedPrivate*^{2}) is an indicator variable equal to one for private analysts who are acquired by a public (private) brokerage house two years after the acquisition. *TreatedPublic*$^{3+}$ (*TreatedPrivate*$^{3+}$) is an indicator variable equal to one for private analysts who are acquired by a public (private) brokerage house three or more years after the acquisition.

[27] For the dynamic DiD analyses, we do not limit the estimation window, because we are interested in whether our treatment effects potentially revert in the longer run, which we capture with our coefficients on *Treated*$^{2+}$ and *Treated*$^{3+}$.

In Panel B of Table 2.4, we rerun the analyses by focusing on acquisitions by private brokerage houses. We do not find any evidence that acquisitions by private brokerage houses lead to more herding of acquired analysts, neither before nor after the acquisition. Taken together, the results from Table 2.4 suggest that private analysts acquired by a public brokerage house exhibit higher herding behavior after, but not before, the shock to the working environment. As such, these findings mitigate concerns about predetermined trends and increase confidence in the interpretation that the working environment affects herding behavior.

2.3.5. Change in firm coverage

A potential explanation for our findings is that private analysts acquired by public brokerage houses might be forced to change the portfolio of firms they are covering, e.g., due to overlap with portfolios of analysts of the acquiring firm (Clement, 1999; Wu and Zang, 2009). As these acquired analysts may lack experience with their new portfolio, they may be more prone to herding. Although we partially address this issue with our fixed effects structure, particularly the use of covered firm-fixed effects, we run the analyses using a sample of private analysts who cover the same firm before and after the acquisition by a public brokerage house.[28] We focus on the sample of private analysts acquired by a public brokerage house, as our prior findings indicate that the increase in herding is concentrated in this sample. These restrictions yield a reduced sample of 89,758 firm-year observations for the full sample and 14,290 firm-year observations for the within-estimation in columns (1) and (2), respectively.

[Table 2.5 about here]

Using this reduced sample, Table 2.5 shows that the coefficients on *TreatedPublic* remain statistically significant at the 1% level in column (1) and (2) with *t*-stats of 2.78 and 2.85, respectively. The results using a 5-year estimation window (column 3 and 4) are very similar. Overall, these results provide evidence that the documented higher herding behavior by private analysts following the acquisition by a public brokerage house is not driven by a change in coverage, as we still observe higher herding behavior by acquired analysts who cover the same firm before and after acquisition.

2.3.6. Cross-sectional analyses

So far, we provided evidence that a shock to the working environment affects analysts' herding behavior—especially for private analysts acquired by a public brokerage house—and this increase in herding behavior cannot be explained by changes in coverage of the acquired analysts. To shed more light on the reasons why analysts herd more after being acquired by a public brokerage house, we conduct additional cross-sectional analyses by using the sample

[28] Specifically, we retain only those forecasts of firms that are followed by the same analyst before and after the treatment. We also restrict our control group to forecasts covering the same firms at the same time as the treated analysts. Our results, however, hold without this restriction of the control group.

from Section 3.5 where acquired analysts cover the same firm before and after the acquisition by a public brokerage house. Our first cross-sectional test reported in columns (1) and (2) of Table 2.6 exploits differences in analysts' characteristics for which the shock to the working environment likely has a stronger effect on their herding behavior.[29] Clement and Tse (2005) for example shows that herding behavior is negatively associated with analysts' experience (i.e., less career concerns). We therefore expect a stronger treatment effect for private analysts with relatively little experience when acquired by a public brokerage house. Accordingly, we split our treatment group (*TreatedPublic*) into two nonoverlapping groups, consisting of relatively low experienced (*HighConcerns*) and more experienced (*LowConcerns*) analysts.[30] In column (1) of Table 2.6, the coefficient on *HighConcerns* is significant at the 10% level (coef. = 0.923, *t*-stat = 1.68) and larger in magnitude than the coefficient on *LowConcerns* (coef. = 0.729, *t*-stat = 1.43). For the within-estimation in column (2), the coefficient on *HighConcerns* is larger in magnitude and significant at the 1% level (coef. = 2.047, *t*-stat = 2.65) than that on *LowConcerns* (coef. = 1.497, *t*-stat = 1.75). These findings suggest a slightly more pronounced effect of the change in working environment for relatively unexperienced analysts, although the F-test on the coefficients is not significantly different from zero (*p*-values of 0.593 and 0.408, respectively).

In columns (3) through (6) of Table 2.6, we investigate characteristics of the working environment of the acquiring company that likely have a stronger effect on acquired analysts' herding behavior. First, we expect the effect on herding behavior to be more pronounced for acquired analysts who face more peer pressure at the acquiring brokerage house. More specifically, herding could be more pronounced for analysts who cover similar firms or sectors as analysts of the acquiring firm. We measure peer pressure as the number of analysts covering the same sector k (two digit SIC) working at the same broker m as analyst i in year t. We split our treatment events into above (*HighPressure*) and below (*LowPressure*) median of our peer pressure variable. To the extent that the treatment effect is stronger for analysts experiencing larger peer pressure after the acquisition, we expect the coefficient on *HighPressure* to be larger than that on *LowPressure*.

[Table 2.6 about here]

In column (3) of Table 2.6, the coefficient on *HighPressure* is significant at the 1% level (coef. = 1.890, *t*-stat = 2.86) and larger in magnitude than the coefficient on *LowPressure* (coef. = 0.353, *t*-stat = 0.81) and the *F*-test on the differences in coefficients is significant (*p*-value = 0.046). Likewise, in column (4) for the within-estimation, the coefficient on *HighPressure* is larger in magnitude and significant at the 1% level (coef. = 3.088, *t*-stat = 3.08) than that on *LowPressure* (coef. = 1.341, *t*-stat = 1.97) and again the difference between coefficients is significant (*p*-value = 0.081). The results provide evidence that higher peer pressure at the

[29] Our cross-sectional inferences hold when using the 5-year estimation window around treatment events.

[30] The median treated analyst's general experience is 10.9 years. Therefore, *HighConcerns* (*LowConcerns*) is equal to one for treated analysts with general experience below (above) 10.9 years, and zero otherwise. We then replace *TreatedPublic* in our main specification by *HighConcerns* and *LowConcerns*.

acquiring brokerage houses increases the herding behavior of acquired analysts, consistent with the explanation that the working environment affects herding behavior.

In columns (5) and (6) we explore another characteristic of the acquiring company that likely moderates the effect of the change in working environment on analysts' herding behavior, namely the acquirer's hire-and-fire culture. Job security concerns might cause analysts to herd more. We thus expect a more pronounced treatment effect for analysts acquired by a public brokerage house with a more pronounced hire-and-fire history (as measured by the historical analyst turnover rate). Specifically, for each acquiring public brokerage house, we count the annual number of analysts who issued forecasts the year before but completely stopped issuing forecasts during the current year, divide it by the number of currently hired analysts, and compute a historical average. We then use the median historical turnover ratio at the date of acquisition among acquiring companies and split our treatment events into above (*HighTurnover*) and below (*LowTurnover*) median turnover companies.[31] To the extent that the effect of the change in working environment is more pronounced for acquirers with a higher historical analyst turnover ratio (i.e., hire-and-fire culture), we expect the coefficient on *HighTurnover* to be larger than that on *LowTurnover.*

In columns (5) and (6) for the full sample and within-estimation, respectively, the coefficients on *HighTurnover* are significant at the 1% level (*t*-stats of 2.66 and 3.31, respectively) and larger in magnitude than the coefficients on *LowTurnover* (*t*-stats of 0.34 and 0.93, respectively). The coefficients are also significantly different from each other (*p*-values of 0.078 and 0.030, respectively). The results suggest a stronger treatment effect for analysts acquired by a public brokerage house with a more pronounced hire-and-fire culture.

In our final cross-sections in columns (7) and (8) of Table 2.6, we investigate changes in the regulatory environment over time that likely impact the working environment (e.g., level of supervision, monitoring, and day-to-day pressure) of treated analysts. Starting in 2000 with Regulation Fair Disclosure (Reg FD) and later with the Sarbanes-Oxley Act (SOX) of 2002 that led to the Global Research Analyst Settlement (Global Settlement), a series of reforms considerably impacted financial analysts' work (Heflin et al. 2003; Kadan et al., 2009; Ramnath et al., 2008). Hence, to the extent that higher regulatory requirements lead to more day-to-day pressure for financial analysts, we expect our treatment effect to be significantly stronger for acquisitions in the post-2000 period. Specifically, we divide our treatment events into acquisitions effective before (*LowRegulation*) and after the year 2000 (*HighRegulation*), yielding 44 mergers with effective dates before December 31, 2000 and 40 mergers with effective dates after December 31, 2000.

In columns (7) and (8) for the full sample and within-estimation, respectively, the coefficients on *HighRegulation* are significant (*t*-stats of 2.63 and 2.50, respectively) and larger in magnitude than the coefficients on *LowRegulation* (*t*-stats of 1.13 and 1.46, respectively).

[31] The median historical turnover for the treated analysts is 2.78%. Therefore, *LowTurnover* (*HighTurnover*) is equal to one for treated analysts acquired by a public company with a historical turnover ratio below (above) 2.78%, and zero otherwise.

However, we fail in both columns to reject the null hypothesis that *HighRegulation* is equal to *LowRegulation.* Therefore, we find some (albeit weak) evidence of more pronounced herding behavior for acquired analysts by public brokerage houses in times of stricter regulation (i.e., in the post-2000 period) and potentially more day-to-day pressure on these analysts.

Overall, our cross-sectional findings from Table 2.6 are consistent with a more pronounced herding behavior for analysts subject to a more substantial shock to their working environment. In addition, these findings also shed light on the circumstances under which analysts herd more, namely higher peer pressure and a more pronounced hire-and-fire culture.

2.4. Conclusion

As herding behavior can be detrimental for information aggregation, hindering efficient resource allocation (Scharfstein and Stein, 1990; Trueman, 1994; Bradshaw et al., 2017), it is important to understand the determinants of herding behavior. In this paper, we shed light on the causal impact of analysts' working environment on herding behavior.

We exploit broker acquisitions as shocks to the working environment of analysts. We do so because acquisitions likely change the working environment of analysts along various dimensions (e.g., changes in peer pressure, career concerns, or tasks assigned), potentially affecting analysts' herding behavior. Using a staggered difference-in-differences design, we find that treated analysts issue significantly more herding forecasts after the acquisition. In addition, our findings show that the increase in herding is concentrated in analysts acquired by public brokerage houses as compared to analysts acquired by private brokerage houses. Next, we find that the increase in herding occurs right after the acquisition but not before, mitigating concerns about predetermined trends. We also rule out that our findings are driven by changes in analyst coverage portfolios after the acquisition. Finally, we find more pronounced herding behavior for acquired analysts with lower ex ante experience (albeit weak evidence), for analysts with higher peer pressure after the acquisition, when the acquirer has a relatively more pronounced hire-and-fire culture, and in periods of stricter regulation of public brokerage houses (again weak evidence). We interpret this evidence as indicating that increased peer pressure and job security concerns at public brokerage houses induce acquired analysts to herd more.

Taken together, our findings suggest a causal link between the working environment and analysts' herding behavior. As such, our findings have implications for investors and regulators by shedding more light on the determinants of herding behavior beyond individual analyst characteristics.

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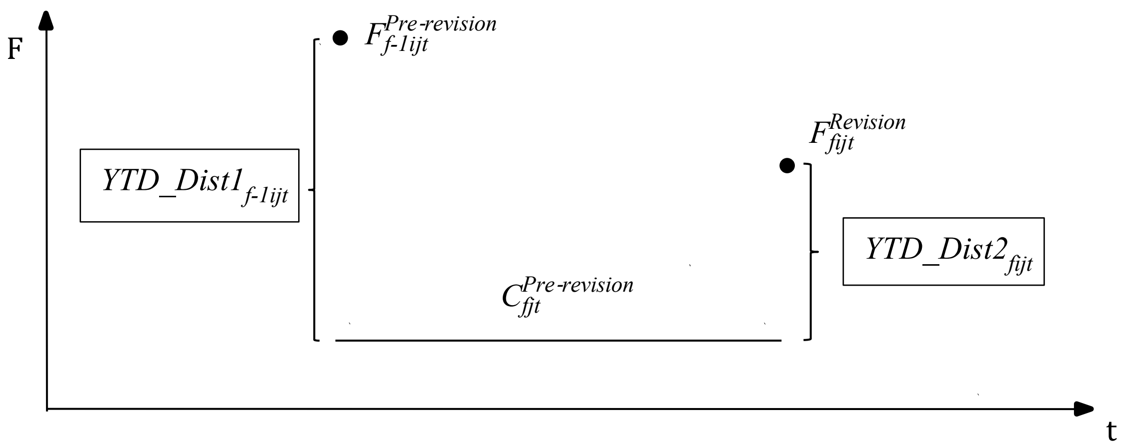
Figures and Tables

Figure 2.1. Timeline for analyst forecasts, consensus forecasts, and forecast revisions



For each forecast revision *f* of analyst *i* covering firm *j* in year *t*, we measure the difference between (1) $YTD\_Dist1_{f\text{-}1ijt}$: the absolute distance of analyst *i*'s prior forecast ($F^{Pre\text{-}revision}_{f\text{-}1ijt}$) from the consensus forecast ($C^{Pre\text{-}revision}_{fjt}$), and (2) YTD_Dist2_{fijt} : the absolute distance of analyst *i*'s revised forecast $\left(F^{Revision}_{fijt}\right)$ from the consensus forecast ($C^{Pre\text{-}revision}_{fjt}$). The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s revision.

The main dependent variable, $Herding_{ijt}$, is defined as the sum of analyst *i*'s forecast revisions for firm *j* during year *t:*

$$Herding_{ijt} = \sum_{f=1}^{n} \left(YTD\_Dist1_{f\text{-}1ijt} - YTD\_Dist2_{fijt} \right)$$

For illustration purposes of our herding measure, consider the following examples:

	Prior forecast	*Revision forecast*	*Pre-revision consensus*	*Herding*
Analyst A	*5*	*2.5*	*2*	*|5 – 2| – |2.5 – 2| = 2.5*
Analyst B	*5*	*4.5*	*2*	*|5 – 2| – |4.5 – 2| = 0.5*
Analyst C	*1*	*0.5*	*2*	*|1 – 2| – |0.5– 2| = –0.5*
Analyst D	*1*	*–0.5*	*2*	*|1 – 2| – |–0.5– 2| = –1.5*

Accordingly, Analyst A herds the most and Analyst D the least.

	Date	*Forecasts*	*Pre-revision consensus*	*Herding*
Analyst E	*March15*	*1.5*		
	June21	*3*	*4*	*|1.5 – 4| – |3 – 4| = 1.5*
	Sept21	*4*	*5*	*|3 – 5| – |4– 5| = 1*
	Nov11	*5*	*6*	*|4 – 6| – |5– 6| = 1*

Accordingly, Analyst E's herding score (yearly sum) is 3.5.

Table 2.1. Descriptive statistics

Panel A: Full sample

Variables	Full sample (N=150,322)						
	Mean	p1	p25	Median	p75	p99	Std. dev.
Dependent variable							
Herding	2.66	-66.18	-6.06	1.45	10.46	80.71	23.35
Control variables							
LagAFE	8.52	0.00	0.70	2.13	6.35	119.73	24.34
ForHorizon	189.62	54.00	155.67	188.30	218.67	336.00	57.56
DaysElapsed	10.85	0.75	2.20	5.71	14.00	68.00	13.59
ForFrequency	3.65	1.00	2.00	3.00	5.00	10.00	2.01
FirmExperience	4.18	1.00	2.00	3.00	6.00	16.00	3.49
BrokerSize	32.76	1.00	12.00	21.00	42.00	168.00	32.57
Competition	12.79	1.00	6.00	11.00	18.00	38.00	8.77
Companies	13.98	2.00	9.00	13.00	17.00	44.00	7.97
Industries	7.59	1.00	4.00	7.00	10.00	26.00	4.74

Panel B: Treatment group (private analysts eventually subject to a broker acquisition)

Variables	Treatment group (N=54,457)						
	Mean	p1	p25	Median	p75	p99	Std. dev.
Dependent variable							
Herding	2.17	-68.04	-6.44	1.15	10.02	81.36	23.75
Control variables							
LagAFE	8.40	0.00	0.67	2.06	6.15	119.68	24.01
ForHorizon	188.10	54.00	155.50	187.63	216.33	336.00	56.72
DaysElapsed	10.08	0.67	2.00	5.33	12.67	63.67	12.73
ForFrequency	3.72	1.00	2.00	4.00	5.00	10.00	2.06
FirmExperience	4.77	1.00	2.00	4.00	6.00	18.00	3.89
BrokerSize	49.07	2.00	17.00	37.00	73.00	186.00	42.68
Competition	13.26	1.00	6.00	12.00	19.00	38.00	8.73
Companies	14.65	2.00	10.00	14.00	18.00	41.00	7.28
Industries	7.31	1.00	4.00	7.00	9.00	20.00	4.19

Panel C: Control group (private analysts never acquired during the sample period)

Variables	Control group (N=95,865)						
	Mean	p1	p25	Median	p75	p99	Std. dev.
Dependent variable							
Herding	2.94	-65.16	-5.83	1.61	10.72	80.29	23.12
Control variables							
LagAFE	8.59	0.00	0.72	2.18	6.45	120.00	24.52
ForHorizon	190.49	54.00	156.00	188.73	220.00	336.00	58.02
DaysElapsed	11.28	0.75	2.25	6.00	14.50	70.00	14.04
ForFrequency	3.61	1.00	2.00	3.00	5.00	10.00	1.98
FirmExperience	3.84	1.00	2.00	3.00	5.00	15.00	3.19
BrokerSize	23.50	1.00	11.00	18.00	29.00	97.00	19.78
Competition	12.53	1.00	5.00	10.00	18.00	38.00	8.78
Companies	13.60	1.00	9.00	12.00	17.00	46.00	8.32
Industries	7.75	1.00	4.00	7.00	10.00	28.00	5.02

The final sample contains 150,322 analyst-year observations from 4,672 unique analysts working at 572 brokerage houses during the years 1983 to 2016. The treatment group comprises analysts working at a privately held brokerage house eventually subject to an acquisition during the sample period. The control group comprises all other analysts working at privately held brokers never acquired during the sample period. *Herding* is the yearly sum of analyst forecast revisions measured as the difference between (1) the absolute distance of the analyst's prior forecast from the consensus forecast, and (2) the absolute distance of analyst's revised forecast from the consensus forecast. The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s forecast revision. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates. See Appendix 2.B for all other variable definitions.

Table 2.2. Broker acquisitions and herding

Dependent variable: ***Herding***	Baseline specification		5-year window	
	Full sample	Within-estimation	Full sample	Within-estimation
Variables	(1)	(2)	(3)	(4)
Treated	**0.837***** **(2.85)**	**1.031***** **(2.98)**	**0.849***** **(2.84)**	**1.302***** **(2.67)**
LagAFE	0.101*** (13.58)	0.105*** (9.75)	0.101*** (13.09)	0.113*** (7.52)
ForHorizon	-0.041*** (-24.79)	-0.045*** (-17.62)	-0.041*** (-24.69)	-0.049*** (-16.13)
DaysElapsed	-0.123*** (-19.05)	-0.127*** (-11.71)	-0.122*** (-18.54)	-0.125*** (-8.74)
ForFrequency	-2.485*** (-42.34)	-2.536*** (-29.41)	-2.475*** (-41.77)	-2.711*** (-24.49)
FirmExperience	0.050** (2.10)	0.015 (0.34)	0.050** (2.00)	0.039 (0.56)
BrokerSize	-0.019*** (-4.78)	-0.015*** (-3.39)	-0.021*** (-4.26)	-0.017** (-2.42)
Competition	-0.157*** (-5.99)	-0.126*** (-3.61)	-0.158*** (-5.91)	-0.084* (-1.84)
Companies	0.000 (0.01)	-0.037 (-0.99)	-0.006 (-0.26)	-0.060 (-1.12)
Industries	-0.019 (-0.43)	-0.006 (-0.08)	0.007 (0.15)	0.057 (0.56)
Analyst fixed effect	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	150,322	54,457	143,244	35,046
Adjusted R^2	0.128	0.128	0.128	0.129

This table reports regression results from estimating Equation (4). The full sample contains both treatment and control group. The within-estimation sample comprises the treatment group only. In columns (3) and (4) the estimation window is restricted to five years around the acquisition events. *Treated* is an indicator variable equal to one for private analysts after acquisition by another brokerage house, and zero otherwise. *Herding* is the yearly sum of analyst forecast revisions measured as the difference between (1) the absolute distance of the analyst's prior forecast from the consensus forecast, and (2) the absolute distance of analyst's revised forecast from the consensus forecast. The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s forecast revision. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates. See Appendix 2.B for all other variable definitions. Standard errors are clustered by covered firm and the resulting *t*-statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Table 2.3. Acquisitions by public versus private brokerage houses

Panel A: Public treatment (private analysts eventually acquired by public brokerage house)

Variables	Public treatment (N=42,865)						
	Mean	p1	p25	Median	p75	p99	Std. dev.
Dependent variable							
Herding	2.12	-69.91	-6.59	1.10	10.08	83.33	24.31
Control variables							
LagAFE	8.92	0.00	0.69	2.12	6.50	127.80	25.18
ForHorizon	188.06	54.00	155.00	187.33	217.33	337.00	57.60
DaysElapsed	10.31	0.67	2.33	5.67	13.00	64.00	12.77
ForFrequency	3.71	1.00	2.00	3.00	5.00	10.00	2.12
FirmExperience	4.78	1.00	2.00	4.00	6.00	18.00	3.97
BrokerSize	50.64	2.00	17.00	36.00	74.00	186.00	44.81
Competition	13.13	1.00	6.00	12.00	19.00	37.00	8.57
Companies	14.67	2.00	10.00	14.00	18.00	44.00	7.63
Industries	7.34	1.00	4.00	7.00	9.00	21.00	4.29

Panel B: Private treatment (private analysts eventually acquired by private brokerage house)

Variables	Private treatment (N=11,592)						
	Mean	p1	p25	Median	p75	p99	Std. dev.
Dependent variable							
Herding	2.38	-60.92	-5.87	1.37	9.77	71.49	21.57
Control variables							
LagAFE	6.46	0.00	0.63	1.84	4.91	84.12	18.96
ForHorizon	188.22	56.00	157.63	188.25	213.00	334.00	53.35
DaysElapsed	9.23	0.75	1.50	4.25	11.50	62.00	12.54
ForFrequency	3.78	1.00	2.50	4.00	5.00	9.00	1.81
FirmExperience	4.74	1.00	2.00	4.00	6.00	16.00	3.59
BrokerSize	43.24	2.00	14.00	39.00	60.00	168.00	33.01
Competition	13.72	1.00	6.00	12.00	19.00	40.00	9.26
Companies	14.56	2.00	11.00	15.00	18.00	31.00	5.81
Industries	7.23	1.00	4.00	6.00	9.00	18.00	3.81

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Table 2.3. (continued)

Panel C: Regression results

Dependent variable: ***Herding***	Baseline specification		5-year window	
	Full sample	Within-estimation	Full sample	Within-estimation
Variables	(1)	(2)	(3)	(4)
TreatedPublic	**0.927*****	**1.261*****	**0.943*****	**1.640*****
	(2.74)	**(3.23)**	**(2.69)**	**(3.10)**
TreatedPrivate	**0.599**	**0.426**	**0.618**	**0.314**
	(1.25)	**(0.80)**	**(1.27)**	**(0.45)**
F-test [p-value]	[0.552]	[0.154]	[0.569]	[0.058]
LagAFE	0.101***	0.105***	0.101***	0.113***
	(13.58)	(9.75)	(13.09)	(7.52)
ForHorizon	-0.041***	-0.045***	-0.041***	-0.049***
	(-24.79)	(-17.62)	(-24.69)	(-16.12)
DaysElapsed	-0.123***	-0.127***	-0.122***	-0.125***
	(-19.05)	(-11.71)	(-18.54)	(-8.72)
ForFrequency	-2.485***	-2.538***	-2.475***	-2.711***
	(-42.36)	(-29.44)	(-41.78)	(-24.50)
FirmExperience	0.050**	0.016	0.050**	0.041
	(2.10)	(0.35)	(2.00)	(0.58)
BrokerSize	-0.019***	-0.015***	-0.021***	-0.018**
	(-4.80)	(-3.42)	(-4.28)	(-2.52)
Competition	-0.157***	-0.127***	-0.158***	-0.085*
	(-5.99)	(-3.63)	(-5.91)	(-1.86)
Companies	0.001	-0.035	-0.006	-0.055
	(0.03)	(-0.94)	(-0.25)	(-1.01)
Industries	-0.019	-0.009	0.006	0.046
	(-0.45)	(-0.12)	(0.13)	(0.45)
Analyst fixed effect	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	150,322	54,457	143,244	35,046
Adjusted R^2	0.128	0.128	0.128	0.129

This table reports descriptive statistics (Panels A and B) and regression results (Panel C) from estimating Equation (4) with two non-overlapping treatment indicators: *TreatedPublic* (*TreatedPrivate*) is an indicator variable equal to one for private analysts after acquisition by a public (private) broker, and zero otherwise. The full sample contains both treatment and control group. The within-estimation sample comprises the treatment group only. In columns (3) and (4) the estimation window is restricted to five years around the acquisition events. *Herding* is the yearly sum of analyst forecast revisions measured as the difference between (1) the absolute distance of the analyst's prior forecast from the consensus forecast, and (2) the absolute distance of analyst's revised forecast from the consensus forecast. The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s forecast revision. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates. See Appendix 2.B for all other variable definitions. Standard errors are clustered by covered firm and the resulting *t*-statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Table 2.4. Dynamic herding effects

Panel A: The dynamic effect of acquisitions by public brokers on herding behavior

Dependent variable: ***Herding***	Full sample		Within-estimation	
Variables	(1)	(2)	(3)	(4)
TreatedPublic^{-2}	**-0.576**	**-0.585**	**-0.307**	**-0.338**
	(-0.93)	**(-0.94)**	**(-0.47)**	**(-0.52)**
TreatedPublic^{-1}	**-0.169**	**-0.183**	**0.260**	**0.210**
	(-0.27)	**(-0.29)**	**(0.39)**	**(0.32)**
TreatedPublic^{0}	**0.347**	**0.333**	**0.832**	**0.773**
	(0.63)	**(0.60)**	**(1.38)**	**(1.28)**
TreatedPublic^{1}	**1.518*****	**1.504*****	**2.124*****	**2.048*****
	(2.83)	**(2.81)**	**(3.44)**	**(3.32)**
TreatedPublic^{2+}	**0.691**		**1.431****	
	(1.59)		**(2.46)**	
TreatedPublic^{2}		**0.955***		**1.714*****
		(1.69)		**(2.59)**
TreatedPublic^{3+}		**0.540**		**1.111***
		(1.17)		**(1.79)**
LagAFE	0.099***	0.099***	0.098***	0.098***
	(13.05)	(13.05)	(8.55)	(8.55)
ForHorizon	-0.041***	-0.041***	-0.046***	-0.046***
	(-24.99)	(-24.98)	(-17.08)	(-17.08)
DaysElapsed	-0.121***	-0.121***	-0.122***	-0.122***
	(-18.33)	(-18.33)	(-10.10)	(-10.10)
ForFrequency	-2.485***	-2.485***	-2.536***	-2.537***
	(-41.14)	(-41.14)	(-26.24)	(-26.25)
FirmExperience	0.050**	0.050**	0.010	0.009
	(1.99)	(1.98)	(0.18)	(0.17)
BrokerSize	-0.018***	-0.018***	-0.013**	-0.012**
	(-4.12)	(-4.08)	(-2.51)	(-2.45)
Competition	-0.163***	-0.163***	-0.129***	-0.129***
	(-6.04)	(-6.04)	(-3.29)	(-3.29)
Companies	-0.002	-0.002	-0.045	-0.047
	(-0.08)	(-0.08)	(-1.07)	(-1.10)
Industries	-0.012	-0.011	-0.023	-0.018
	(-0.27)	(-0.25)	(-0.28)	(-0.23)
Analysts fixed effect	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	138,730	138,730	42,865	42,865
Adjusted R^2	0.128	0.128	0.127	0.127

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Table 2.4. (continued)

Panel B: The dynamic effect of acquisitions by private brokers on herding behavior

Dependent variable: ***Herding***	Full sample		Within-estimation	
Variables	(1)	(2)	(3)	(4)
TreatedPrivate^{-2}	**-0.237**	**-0.241**	**0.328**	**0.163**
	(-0.25)	**(-0.26)**	**(0.28)**	**(0.14)**
TreatedPrivate^{-1}	**-0.297**	**-0.291**	**0.059**	**-0.081**
	(-0.32)	**(-0.31)**	**(0.05)**	**(-0.07)**
TreatedPrivate^{0}	**-1.459**	**-1.456**	**-1.486**	**-1.700**
	(-1.63)	**(-1.63)**	**(-1.23)**	**(-1.36)**
TreatedPrivate^{1}	**0.280**	**0.274**	**-0.969**	**-1.277**
	(0.33)	**(0.32)**	**(-0.73)**	**(-0.91)**
TreatedPrivate^{2+}	**0.131**		**0.037**	
	(0.19)		**(0.02)**	
TreatedPrivate^{2}		**0.692**		**0.197**
		(0.76)		**(0.13)**
TreatedPrivate^{3+}		**-0.036**		**-0.628**
		(-0.05)		**(-0.35)**
LagAFE	0.102***	0.102***	0.148***	0.148***
	(11.84)	(11.84)	(5.78)	(5.78)
ForHorizon	-0.039***	-0.039***	-0.041***	-0.041***
	(-22.07)	(-22.07)	(-6.69)	(-6.70)
DaysElapsed	-0.120***	-0.120***	-0.136***	-0.136***
	(-16.03)	(-16.03)	(-5.64)	(-5.63)
ForFrequency	-2.465***	-2.465***	-2.555***	-2.554***
	(-38.59)	(-38.59)	(-14.80)	(-14.77)
FirmExperience	0.071**	0.071**	0.071	0.069
	(2.40)	(2.41)	(0.52)	(0.51)
BrokerSize	-0.037***	-0.037***	-0.033**	-0.033**
	(-4.59)	(-4.54)	(-2.38)	(-2.38)
Competition	-0.168***	-0.168***	-0.093	-0.092
	(-5.73)	(-5.73)	(-1.43)	(-1.42)
Companies	0.025	0.025	0.020	0.023
	(0.89)	(0.89)	(0.23)	(0.27)
Industries	-0.041	-0.041	-0.139	-0.139
	(-0.80)	(-0.80)	(-0.89)	(-0.90)
Analysts fixed effect	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	107,457	107,457	11,592	11,592
Adjusted R^2	0.131	0.131	0.130	0.130

(Continued on next page)

Table 2.4. (continued)

This table reports dynamic DiD regression results for private analysts subject to an acquisition by either a public broker (Panel A) or a private broker (Panel B) during the sample period. The full sample comprises the control group and the public (private) treatment group. The within-estimation sample comprises the public (private) treatment group only. $TreatedPublic^{-2}$ ($TreatedPrivate^{-2}$) is an indicator variable equal to one for private analysts two years before the acquisition by a private (public) broker. $TreatedPublic^{-1}$ ($TreatedPrivate^{-1}$) is an indicator variable equal to one for private analysts one year before acquisition by a private (public) broker. $TreatedPublic^{0}$ ($TreatedPrivate^{0}$) is an indicator variable equal to one for private analysts in the year of the acquisition by a private (public) broker. $TreatedPublic^{1}$ ($TreatedPrivate^{1}$) is an indicator variable equal to one for private analysts one year after the acquisition by a private (public) broker. $TreatedPublic^{2+}$ ($TreatPrivate^{2+}$) is an indicator variable equal to one for private analysts two or more years after the acquisition by a public (private) broker. $TreatedPublic^{2}$ ($TreatPrivate^{2}$) is an indicator variable equal to one for private analysts two years after the acquisition by a public (private) broker. $TreatedPublic^{3+}$ ($TreatPrivate^{3+}$) is an indicator variable equal to one for private analysts three or more years after the acquisition by a public (private) broker. *Herding* is the yearly sum of analyst forecast revisions measured as the difference between (1) the absolute distance of the analyst's prior forecast from the consensus forecast, and (2) the absolute distance of analyst's revised forecast from the consensus forecast. The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s forecast revision. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates. See Appendix 2.B for all other variable definitions. Standard errors are clustered by covered firm and the resulting *t*-statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Table 2.5. Covering the same firm before and after acquisition

	Baseline specification		5-year window	
Dependent variable: ***Herding***	Full sample	Within-estimation	Full sample	Within-estimation
Variables	(1)	(2)	(3)	(4)
TreatedPublic	**1.126*** **	**1.832*** **	**1.031** **	**1.729** **
	(2.78)	**(2.85)**	**(2.48)**	**(2.22)**
LagAFE	0.112***	0.105***	0.112***	0.109***
	(11.02)	(4.83)	(10.95)	(4.64)
ForHorizon	-0.043***	-0.054***	-0.043***	-0.055***
	(-20.78)	(-9.83)	(-20.87)	(-9.46)
DaysElapsed	-0.108***	-0.139***	-0.108***	-0.129***
	(-12.11)	(-5.82)	(-11.97)	(-5.00)
ForFrequency	-2.403***	-2.360***	-2.396***	-2.379***
	(-32.94)	(-14.06)	(-33.09)	(-12.33)
FirmExperience	0.058**	-0.356*	0.058*	-0.350
	(1.98)	(-1.66)	(1.95)	(-1.28)
BrokerSize	-0.018***	-0.013*	-0.019***	-0.019*
	(-3.32)	(-1.72)	(-2.91)	(-1.84)
Competition	-0.147***	-0.139**	-0.145***	-0.056
	(-4.89)	(-1.98)	(-4.79)	(-0.62)
Companies	-0.005	-0.189**	-0.006	-0.179*
	(-0.17)	(-2.10)	(-0.21)	(-1.73)
Industries	0.019	0.318**	0.026	0.340*
	(0.36)	(2.18)	(0.48)	(1.82)
Analyst fixed effect	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	89,758	14,290	88,478	11,648
Adjusted R^2	0.130	0.100	0.130	0.099

This table reports regression results from estimating Equation (4) for a restricted treatment and control group. The treatment group comprises private analysts covering the same firm before and after the acquisition by a public broker. The control group is also restricted to forecasts covering the same firms at the same time as the treated analysts. The full sample contains both treatment and control group. The within-estimation sample comprises the treatment group only. In columns (3) and (4) the estimation window is restricted to five years around the acquisition events. *TreatedPublic* is an indicator variable equal to one for private analysts after acquisition by a public broker, and zero otherwise. *Herding* is the yearly sum of analyst forecast revisions measured as the difference between (1) the absolute distance of the analyst's prior forecast from the consensus forecast, and (2) the absolute distance of analyst's revised forecast from the consensus forecast. The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s forecast revision. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates. See Appendix 2.B for all other variable definitions. Standard errors are clustered by covered firm and the resulting *t*-statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Table 2.6. Cross-sectional analyses

	Career concerns		Peer pressure		Hire- and-fire culture		Regulatory pressure	
Dependent variable: ***Herding***	Full sample	Within-estimation	Full sample	Within-estimation	Full sample	Within-estimation	Full sample	Within-estimation
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HighConcerns	**0.923***	**2.047*****						
	(1.68)	**(2.65)**						
LowConcerns	**0.729**	**1.497***						
	(1.43)	**(1.75)**						
HighPressure			**1.890*****	**3.088*****				
			(2.86)	**(3.08)**				
LowPressure			**0.353**	**1.341****				
			(0.81)	**(1.97)**				
HighTurnover					**1.458*****	**2.821*****		
					(2.66)	**(3.31)**		
LowTurnover					**0.168**	**0.698**		
					(0.34)	**(0.93)**		
HighRegulation							**1.266*****	**1.931****
							(2.63)	**(2.50)**
LowRegulation							**0.836**	**1.620**
							(1.13)	**(1.46)**
F-tests [p-value]	[0.593]	[0.408]	[0.046]	[0.081]	[0.078]	[0.030]	[0.625]	[0.814]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analysts fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	89,758	14,290	89,758	14,290	89,758	14,290	89,758	14,290
Adjusted R^2	0.130	0.100	0.130	0.100	0.130	0.100	0.130	0.100

The table reports regression results from estimating Equation (4) for different cross-sectional partitions based on treatment group median splits. The treatment group comprises private analysts covering the same firm before and after the acquisition by a public broker (same sample restrictions as in Table 2.5). *HighConcerns* (*LowConcerns*) is equal to one for treated analysts with a general experience below (above) the median of 10.9. *HighPressure* (*LowPressure*) is equal to one for treated analysts subject to a change in the number of other analysts covering the same sector at the same broker above (below) the median of 0. *HighTurnover* (*LowTurnover*) is equal to one for treated analysts acquired by a public institution with a historical turnover above (below) the median of 2.78%. *HighRegulation* (*LowRegulation*) is equal to one for treated analysts subject to treatment effective after (before) December 31, 2000. See Appendix 2.B for all other variable definitions. Standard errors are clustered by covered firm and the resulting *t*-statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Appendix 2.A. Event list of broker mergers and acquisitions

Target company (private broker)	Acquiring company	Effective date	Public acquirer
Becker Paribas	Merrill Lynch & Co Inc	09/10/1984	Yes
James Capel & Co	HSBC Hong Kong	01/01/1986	Yes
Nesbitt Thomson Inc	Bk Of Montreal, Montreal, Canada	10/31/1987	Yes
Howard Weil Financial Corp	Legg Mason Inc	12/28/1987	Yes
McLeod Young Weir Ltd	Bank of Nova Scotia	03/30/1988	Yes
Wood Gundy Corp	Canadian Imperial Bk Commerce	06/13/1988	Yes
Butcher & Co Inc	Wheat First Securities Inc(WF)	10/31/1988	No
Levesque Beaubien & Co	National Bank of Canada	12/13/1988	Yes
C. J. Lawrence	Deutsche Bank AG	11/28/1989	Yes
Craig-Hallum Capital Group LLC	Hamilton Investments Inc	06/05/1992	No
Loewen Ondaatje McCutcheon Inc	Ondaatje Corp	06/30/1992	No
Martin Simpson & Co Inc	Vantage International Corp	09/21/1992	Yes
Burns Fry Ltd.	Nesbitt Thomson Inc	07/19/1994	Yes
Wertheim Schroder & Co	Schroders PLC	07/30/1994	Yes
Kidder Peabody & Co	PaineWebber Group Inc	12/16/1994	Yes
Mabon Securities Corp	Rodman & Renshaw Capital Group	03/30/1995	Yes
Kleinwort Benson Group PLC	Dresdner Bank AG	08/23/1995	Yes
Conning Corp	General American Investment	09/28/1995	Yes
Sanwa McCarthy Securities Ltd	Investor Group	10/02/1995	No
Marleau Securities	Polaris Securities Co Ltd	06/14/1996	No
Richardson Greenshields of CA	RBC Dominion Securities Ltd	11/01/1996	Yes
RBC Dominion Securities Ltd	Royal Bank of Canada	12/31/1996	Yes
ChiCorp Inc	ABN AMRO North America Inc	01/02/1997	Yes
Alex Brown Inc	Bankers Trust New York Corp	09/02/1997	Yes
Dillon Read & Co (UBS AG)	SBC Warburg (Swiss Bank Corp)	09/02/1997	Yes
Montgomery Securities,CA	NationsBank Corp,Charlotte,NC	10/01/1997	Yes
Robertson Stephens & Co	BankAmerica Corp	10/01/1997	Yes
Furman Selz LLC	ING Barings (ING Groep NV)	10/08/1997	Yes
Principal Financial Securities	EVEREN Capital Corp	01/09/1998	Yes
Wheat First Butcher Singer	First Union Corp,Charlotte,NC	02/02/1998	Yes
Jensen Securities Co	DA Davidson & Co	02/17/1998	No
Wessels Arnold & Henderson LLC	Dain Rauscher Corp	04/06/1998	Yes
Cowen & Co	Societe Generale Securities	06/30/1998	Yes
Midland Walwyn Inc	Merrill Lynch & Co Inc	08/27/1998	Yes
SBC Warburg Premier Sec Plc	UBS AG	08/28/1998	Yes
Van Kasper & Co	First Security Corp,Utah	02/16/1999	Yes
Fifth Third-Brkg Offices (6)	Fahnestock & Co	02/19/1999	Yes
Fox-Pitt Kelton Group Ltd	Swiss Reinsurance Co	03/18/1999	Yes
Scott & Stringfellow Financial	BB&T Corp.	03/26/1999	Yes
Interstate/Johnson Lane Inc	Wachovia Corp,Winston-Salem,NC	04/01/1999	Yes
Northern Securities Inc	American Gem Corp	06/11/1999	No
Roney & Co,Detroit,Michigan	Raymond James Financial Inc	06/14/1999	Yes
Vector Securities Intl Inc	Prudential Securities Inc	07/31/1999	Yes
First Marathon Inc	National Bank of Canada	08/13/1999	Yes
Fechtor,Detwiler & Co Inc	JMC Group	08/30/1999	Yes
Volpe Brown Whelan & Co	Prudential Securities Inc	12/13/1999	Yes
Hanifen Imhoff Holding Inc	Stifel Financial Corp	01/12/2000	Yes
Black & Co Inc	First Security Van Kasper & Co	04/28/2000	Yes
R.J. Steichen & Company	Miller Johnson Steichen Kinnard, Inc. R. J	08/30/2000	No
Sanford C Bernstein & Co Inc	Alliance Capital Mgmt Hldg LP	10/02/2000	Yes
Branch Cabell & Co Inc	Tucker Anthony Sutro	10/03/2000	Yes
Goepel McDermid Inc	Raymond James Financial Inc	12/29/2000	Yes
Wasserstein Perella Group Inc	Dresdner Bank AG	01/05/2001	Yes
Dain Rauscher Corp	Royal Bank of Canada	01/10/2001	Yes
Newcrest Capital	TD Securities Inc	01/17/2001	Yes
BlueStone Capital Corp	Healthstar Corp	06/29/2001	No
GBI Capital Management Corp	Ladenburg, Thalmann Group Inc	07/07/2001	Yes
Robinson-Humphrey Co	SunTrust Banks Inc,Atlanta,GA	07/27/2001	Yes
J Michael-Patrick LLC	Wunderlich Securities Inc	11/13/2001	No
Putnam Lovell Group Inc	National Bank Financial Inc	06/19/2002	Yes
Arnhold & S Bleichroeder Inc	Natexis Banques Populaires	12/06/2002	Yes
Pershing	Bank of New York Co Inc,NY	05/01/2003	Yes
CDC IXIS Capital Markets	CNCE	06/30/2004	No

Schwab Soundview Capital	UBS AG	10/29/2004	Yes
Parker/Hunter Inc	Janney Montgomery Scott LLC	03/22/2005	No
Berean Capital Inc	Jackson Securities LLC	06/07/2005	No
Kepler Equities SA	Landsbanki Islands hf	11/14/2005	No
Merrion Capital Group	Landsbanki Islands hf	12/21/2005	No
Robert W Baird Ltd-Equity	Bridgewell	12/31/2005	No
Adams Harkness Financial Group	Canaccord Capital Inc	01/03/2006	Yes
Investec (US) Inc	Bank Hapoalim BM	03/19/2006	Yes
Miller Johnson Steichen Kinnard	Stifel	05/30/2006	Yes
Hoefer & Arnett Inc	Howe Barnes Investments Inc	07/31/2006	No
Fideuram Wargny-wargny.com	Bourse Direct SA	08/31/2006	Yes
BroadWall Capital, LLC	Ladenburg Thalmann Financial Services Inc.	09/11/2006	Yes
Jesup & Lamont Securities Corp	Empire Financial Holding Co	11/13/2006	No
Petrie Parkman & Co Inc	Merrill Lynch & Co Inc	12/08/2006	Yes
Barrington Associates	Wells Fargo Securities LLC	12/31/2006	Yes
Santander Investment SA Corred	Santiago SA Corredores de Bols	01/16/2007	No
ThinkEquity Partners LLC	Panmure Gordon & Co PLC	04/02/2007	Yes
Oddo France Dis	Oddo Generation SICAV	06/15/2007	No
CE Unterberg Towbin	Collins Stewart PLC	07/17/2007	Yes
Cochran Caronia Waller	Fox-Pitt Kelton Inc	09/04/2007	Yes
First Albany Cos Inc	MatlinPatterson FA Acquisition	09/24/2007	Yes
Orion Financial Inc	Macquarie Bank Ltd	12/05/2007	Yes
Westwind Partners Corp	Thomas Weisel Partners Group	01/02/2008	Yes
Hilliard Lyons Inc	Houchens Industries Inc	03/31/2008	No
Punk Ziegel & Co LP	Ladenburg Thalmann Finl Svcs	05/05/2008	Yes
Ferris Baker Watts Inc	RBC Dain Rauscher Corp	06/20/2008	Yes
American Technology Research	Broadpoint Securities Group,	09/03/2008	No
H&R Block Financial Advisors	Ameriprise Financial Inc	11/03/2008	Yes
Nordic Partners, Inc	Pareto Securities ASA	01/01/2009	No
Merriman Curhan-Cash Distribs	Investor Group	01/30/2009	No
Commerce Capital Markets Inc	TD Securities Inc	01/31/2009	No
Pacific Growth Equities LLC	Wedbush Morgan Securities Inc	02/28/2009	No
Renaissance Capital	Gruppa Oneksim	06/05/2009	No
Sanders Morris Harris-Capital	Siwanoy Capital LLC	06/30/2009	No
Petersen Capital Corp	Versant Partners Inc	08/18/2009	No
Tristone Capital Global Inc	Macquarie Grp Ltd	09/01/2009	Yes
Edward Jones Ltd	Towry Law PLC	11/12/2009	No
Blackmont Capital Inc	Macquarie Grp Ltd	12/31/2009	Yes
JPMorgan Cazenove Ltd	JPMorgan Chase & Co	01/04/2010	Yes
JF Mackie & Co	Mackie Research Capital Corp	02/01/2010	No
Next Generation Equity Research, LLC	Hudson Securities, Inc	03/31/2010	No
Genuity Capital Markets	Canaccord Financial Inc.	04/23/2010	Yes
First Securities AS	Swedbank AB	11/16/2010	Yes
Jefferies & Co Inc-Clearing	Pershing LLC	01/01/2011	No
Barnard Jacobs Mellet Hldg Ltd	FirstRand Ltd	01/03/2011	Yes
Tri-Artisan Capital Partners	Morgan Joseph LLC	01/03/2011	No
Howe Barnes Hoefer & Arnett	Raymond James Financial Inc	03/31/2011	Yes
Miller Tabak Roberts	GMP Capital Inc	09/26/2011	Yes
Troyka Dialog ZAO	Sberbank Rossii	01/23/2012	Yes
Kansas City Board of Trade	CME Group Inc	12/03/2012	Yes
Caris & Co	B Riley & Co LLC	12/17/2012	Yes
AS SEB Enskilda-Operations	Investor Group	01/08/2013	No
Dahlman Rose & Co LLC	Cowen Group Inc	03/11/2013	Yes
Credit Agricole Cheuvreux SA	Kepler Capital Markets SA	05/06/2013	No
CLSA Ltd	CITIC Sec Intl Co Ltd	07/31/2013	Yes
McAdams Wright Ragen Inc	Robert W Baird & Co Inc	04/09/2014	No
B Riley & Co LLC	Great American Group Inc	06/18/2014	Yes
Zermatt Wealth Partners LLC	Thompson Davis & Co Inc	08/01/2014	No
Pacific Crest Securities	KeyCorp,Cleveland,Ohio	09/03/2014	Yes
Sterne Agee Group Inc	Stifel Financial Corp	06/05/2015	Yes
Simmons & Co International	Piper Jaffray Cos	02/29/2016	Yes

Appendix 2.B. Variable definitions and data sources

Name	Description	Source
Herding$_{ijt}$	Yearly sum of analysts' forecast revisions measured as the difference between (1) the absolute distance of analyst *i*'s prior forecast from the consensus forecast, and (2) the absolute distance of analyst *i*'s revised forecast from the consensus forecast. The consensus forecasts are based on forecasts issued in the 90 days prior to analyst *i*'s forecast revisions. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates.	I/B/E/S
Treated$_{it}$	Indicator variable equal to one for private analysts after acquisition by another broker, and zero otherwise.	SDC & hand-collection
TreatedPublic$_{it}$	Indicator variable equal to one for private analysts after acquisition by a public broker, and zero otherwise.	SDC & hand-collection
TreatedPrivate$_{it}$	Indicator variable equal to one for private analysts after the acquisition by a private broker, and zero otherwise.	
LagAFE$_{ijt-1}$	Lagged absolute forecast error (*AFE*), defined as analyst *i*'s AFE of last year (*t-1*) for firm *j*. *AFE* is the absolute difference between the analyst's earnings estimate and the firm's actual EPS scaled by the stock price of firm *j* two days before the revision.	I/B/E/S
ForHorizon$_{ijt}$	Average number of days between analyst *i*'s forecasts during year *t* and the end of the fiscal period for firm *j*.	I/B/E/S
DaysElapsed$_{ijt}$	Average number of days between analyst *i*'s forecasts and the last forecasts by any other analyst following the same firm *j* in year *t*.	I/B/E/S
ForFrequency$_{ijt}$	Number of forecasts analyst *i* issued for firm *j* during year *t*.	I/B/E/S
FirmExperience$_{ijt}$	Number of years analyst *i* is covering firm *j* until year *t*.	I/B/E/S
BrokerSize$_{it}$	Number of analysts working with analyst *i* at the same brokerage house in year *t*.	I/B/E/S
Competition$_{ijt}$	Number of other analysts following the same firm *j* in year *t* for analyst *i*.	I/B/E/S
Companies$_{it}$	Number of companies covered by analyst *i* in year *t*.	I/B/E/S
Industries$_{it}$	Number of two-digit SIC codes analyst *i* covers in year *t*.	I/B/E/S

Appendix 2.C. Matching of the SDC database with I/B/E/S brokers

We combine two databases with no direct links or matching files between them by using a similar methodology to Wu and Zang (2009). First, we start our matching procedure by retrieving from the SDC database all available completed transactions within the financial sector firms that lead to full ownership of the target company's shares by the acquirer. We collect the announcement date, effective date, target and acquiring company as well as the synopsis of the transaction and resulting share ownership for each transaction. Second, the full set of brokerage houses is extracted from the I/B/E/S database: I/B/E/S identifier (later used to match the brokers to analysts' earnings forecasts), names, BAID code, and first and last year the broker is present on I/B/E/S. From these two lists, we match the I/B/E/S brokerage name with the target company names from SDC. As the names of the brokerage houses in I/B/E/S can change over time (in part due to mergers), the BAID code tracks the original names and helps us in identifying and matching the transactions.

A simple example is the I/B/E/S brokerage house *C.E. Unterberg, Towbin* (identifier *1583*) which is matched with the SDC's target company *CE Unterberg Towbin* acquired by *Collins Stewart PLC* on July 17, 2007. A more complicated example is the brokerage house with the identifier *112*, named *ING Financial Markets* with the BAID code *FURMAN* that we match with the SDC's target company *Furman Selz LLC* acquired by *ING Barings (ING Groep NV)* on October 8, 1997.

We are also able to track targeted analysts when their I/B/E/S brokerage house identifier (as reported in the database) changes after a merger to that of the acquiring company. If an analyst brokerage house's affiliation on I/B/E/S (matched with a transaction from the SDC database) stops in the same year as the merger effective date, we make sure to track those analysts if their new affiliation on I/B/E/S corresponds to the acquiring company. For example, the I/B/E/S broker *Dahlman Rose & Co.* (identifier *2488*) is identified on SDC as *Dahlman Rose & Co LLC,* which was acquired by the company *Cowen Group Inc* on March 11, 2013. If analysts affiliated to the I/B/E/S broker *Dahlman Rose & Co.* (identifier *2488*) change their I/B/E/S affiliation after the merger date to *Cowen and Company* (identifier *122*), we make sure to track them as treated analysts.

Appendix 2.D. Results based on different event windows

Dependent variable: **Herding**	5-year window (as per **Table 2.2**)		4-year window		3-year window		2-year window		1-year window	
	Full sample	Within-estimation	Full sample	Within-estimation	Full sample	Within-estimation	Full sample	Within-estimation	Full sample	Within-estimation
Variables	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treated	**0.849*** **	**1.302*** **	**0.965*** **	**1.358*** **	**0.937*** **	**1.837*** **	**1.001*** **	**1.681** **	**1.022*** **	**2.115** **
	(2.84)	**(2.67)**	**(3.19)**	**(2.59)**	**(3.04)**	**(3.10)**	**(3.15)**	**(2.43)**	**(3.14)**	**(2.37)**
LagAFE	0.101***	0.113***	0.100***	0.116***	0.100***	0.122***	0.101***	0.124***	0.101***	0.135***
	(13.09)	(7.52)	(13.10)	(7.39)	(13.06)	(6.96)	(13.03)	(6.30)	(12.94)	(5.56)
ForHorizon	-0.041***	-0.049***	-0.041***	-0.050***	-0.041***	-0.050***	-0.040***	-0.049***	-0.040***	-0.053***
	(-24.69)	(-16.13)	(-24.61)	(-15.23)	(-24.39)	(-14.00)	(-24.10)	(-11.05)	(-24.17)	(-8.59)
DaysElapsed	-0.122***	-0.125***	-0.122***	-0.126***	-0.123***	-0.120***	-0.123***	-0.097***	-0.122***	-0.084***
	(-18.54)	(-8.74)	(-18.53)	(-8.13)	(-18.53)	(-6.81)	(-18.48)	(-4.68)	(-18.53)	(-3.18)
ForFrequency	-2.475***	-2.711***	-2.475***	-2.729***	-2.469***	-2.685***	-2.463***	-2.547***	-2.480***	-2.697***
	(-41.77)	(-24.49)	(-41.59)	(-21.88)	(-41.19)	(-19.12)	(-41.15)	(-15.75)	(-41.75)	(-13.02)
FirmExperience	0.050**	0.039	0.055**	0.058	0.060**	0.194**	0.062**	0.203*	0.058**	0.181
	(2.00)	(0.56)	(2.19)	(0.74)	(2.37)	(2.16)	(2.42)	(1.90)	(2.25)	(1.32)
BrokerSize	-0.021***	-0.017**	-0.025***	-0.026***	-0.023***	-0.017*	-0.027***	-0.022*	-0.026***	-0.032
	(-4.26)	(-2.42)	(-4.80)	(-3.08)	(-4.18)	(-1.68)	(-4.69)	(-1.70)	(-4.55)	(-1.60)
Competition	-0.158***	-0.084*	-0.157***	-0.073	-0.158***	-0.109**	-0.160***	-0.071	-0.163***	-0.140
	(-5.91)	(-1.84)	(-5.90)	(-1.46)	(-5.89)	(-2.00)	(-5.96)	(-1.11)	(-6.02)	(-1.55)
Companies	-0.006	-0.060	-0.009	-0.072	-0.005	-0.094	-0.002	-0.064	-0.004	0.179
	(-0.26)	(-1.12)	(-0.39)	(-1.18)	(-0.21)	(-1.26)	(-0.07)	(-0.69)	(-0.16)	(1.31)
Industries	0.007	0.057	0.016	0.083	0.013	0.232*	0.005	0.197	0.007	-0.037
	(0.15)	(0.56)	(0.36)	(0.72)	(0.28)	(1.69)	(0.11)	(1.07)	(0.14)	(-0.13)
Analyst fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	143,244	35,046	141,907	30,449	140,596	25,057	139,206	18,829	137,924	11,717
Adjusted R^2	0.128	0.129	0.128	0.127	0.128	0.126	0.128	0.136	0.129	0.151

This table reports regression results from estimating Equation (4) for different event windows around the acquisition events. The full sample contains both treatment and control group. The within-estimation sample comprises the treatment group only. *Treated* is an indicator variable equal to one for private analysts after acquisition by another brokerage house, and zero otherwise. *Herding* is the yearly sum of analyst forecast revisions measured as the difference between (1) the absolute distance of the analyst's prior forecast from the consensus forecast, and (2) the absolute distance of analyst's revised forecast from the consensus forecast. The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s forecast revision. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates. See Appendix 2.B for all other variable definitions. Standard errors are clustered by covered firm and the resulting *t*-statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively

Appendix 2.E. Alternative definitions of herding

We test the robustness of our main dependent variable. Although our results are robust to different consensus time windows (i.e., 60 and 120 days), we propose an alternative measure of herding behavior. We first replicate the categorical boldness measure (*Bold*) of Clement and Tse (2005) and Hirshleifer et al. (2019) for all forecast revisions issued by an analyst during a year. For each forecast revision f from analyst i for firm j during year t, *Bold* is an indicator variable equal to one if an analyst deviates from both the analyst's own prior forecast and the pre-revision consensus forecast. *Bold* equals zero if the analyst i's forecast revision lies between the analyst's prior forecast and the pre-revision consensus. We then define *Boldness* as the yearly sum of *Bold* divided by the number of forecast revisions (n) issued by analyst i for firm j during year t (see equation 5 below).

A key difference between *Boldness* and *Herding* is that while *Herding* is a continuous measure of herding during the year, *Boldness* is a yearly categorical measure, indicating the percentage of revisions by an analyst in a given year that move away from the consensus (e.g., Analyst A is bold 20% of the time while Analyst B is bold 80% of the time). Consequently, due to its categorical nature, *Boldness* is by definition a coarser measure than the continuous *Herding* measure and may fail to capture some important subtleties of analyst's herding behavior.[32]

$$Boldness_{ijt} = \frac{\sum_{f=1}^{n} Bold_{fijt}}{n} \tag{5}$$

Given that *Boldness* measures the percentage of being bold of analysts' forecast revisions during each year, we expect a decrease in boldness for analysts subject to a change in working environment (in contrast to the predicted sign for the *Herding* measure). Columns (1) and (2) of online Appendix C show the results with *Boldness* as dependent variable. As predicted, the coefficient of interest on *Treated* of −0.834 is significantly negative (t-stat = −2.05) in column (1) where the control group is included. In column (2) for the within-estimation, the coefficient on *Treated* of −0.897 is also significantly negative (t-stat = −1.87). The results based on the 5-year estimation window in columns (3) and (4) are even stronger. Overall, the results support our prior findings, suggesting that treated analysts issue less bold (i.e., more herding) forecasts after the acquisition by a public brokerage house.

[32] According to the definition of *Bold*, all forecast revisions that are not between the analyst's prior forecast and the consensus are classified as *Bold* = 1, including some revisions that move towards the consensus but then go slightly beyond it. Consider the example where Analyst A's and Analyst B's prior forecasts were both 5 and the pre-revision consensus is 2. If Analyst A revises the forecast to 4.5, the revision is classified as *Bold* = 0 (i.e., herding). If Analyst B revises the forecast to 1.5, the revision is classified as *Bold* = 1 (i.e., bold). Yet, analyst B's behavior is more consistent with what we perceive as "herding behavior," as analyst B's revision moves closer to the consensus forecast. Note that our continuous *Herding* measure better captures these subtleties of analyst behavior (Analyst A's herding score: |5 – 2| – |4.5 – 2| = 0.5; Analyst B's herding score: |5 – 2| – |1.5 – 2| = 2.5).

Appendix 2.F. Alternative definitions of herding

Dependent variable: **Boldness**	Baseline specification		5-year window	
	Full sample	Within-estimation	Full sample	Within-estimation
Variables	(1)	(2)	(3)	(4)
Treated	**-0.834****	**-0.897***	**-0.912****	**-1.584****
	(-2.05)	**(-1.87)**	**(-2.19)**	**(-2.48)**
LagAFE	-0.026***	-0.032***	-0.025***	-0.027***
	(-5.50)	(-4.34)	(-5.09)	(-3.04)
ForHorizon	0.033***	0.037***	0.032***	0.038***
	(15.80)	(11.15)	(15.11)	(9.13)
DaysElapsed	0.011	-0.001	0.009	-0.017
	(1.27)	(-0.06)	(1.02)	(-0.84)
ForFrequency	1.032***	1.064***	1.034***	1.096***
	(17.67)	(12.21)	(17.31)	(9.95)
FirmExperience	-0.011	-0.015	0.007	0.063
	(-0.33)	(-0.26)	(0.20)	(0.74)
BrokerSize	0.022***	0.018***	0.024***	0.005
	(4.36)	(3.09)	(3.69)	(0.56)
Competition	0.146***	0.132***	0.152***	0.128**
	(5.29)	(3.31)	(5.42)	(2.54)
Companies	-0.019	0.013	-0.025	0.021
	(-0.57)	(0.27)	(-0.70)	(0.30)
Industries	0.114*	0.131	0.111*	0.255*
	(1.82)	(1.34)	(1.69)	(1.80)
Analyst fixed effect	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	150,322	54,457	143,244	35,046
Adjusted R^2	0.066	0.060	0.066	0.063

This table reports regression results from estimating Equation (4) for an alternative herding measure, *Boldness*. The full sample contains both treatment and control group. The within-estimation sample comprises the treatment group only. In columns (3) and (4) the estimation window is restricted to five years around the acquisition events. *Treated* is an indicator variable equal to one for private analysts after acquisition by another brokerage house, and zero otherwise. *Boldness* is the percentage of the forecast revisions from an analyst during a year for a specific firm that are above both the analyst's prior forecasts and the consensus forecasts, or else below both. The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s forecast revision. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates. See Appendix 2.B for all other variable definitions. Standard errors are clustered by covered firm and the resulting *t*-statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Appendix 2.G. Alternative clustering of standard errors

Dependent variable: **Herding**	Baseline specification		5-year window	
	Full sample	Within-estimation	Full sample	Within-estimation
Cluster: Analysts	(1)	(2)	(3)	(4)
Treated	**0.837****	**1.031****	**0.849****	**1.302****
	(2.46)	**(2.47)**	**(2.42)**	**(2.26)**
N of clusters	4,672	766	4,671	762
Cluster: Brokers	(1)	(2)	(3)	(4)
Treated	**0.837****	**1.031****	**0.849****	**1.302****
	(2.38)	**(2.44)**	**(2.37)**	**(2.44)**
N of clusters	572	331	559	282
Cluster: Years	(1)	(2)	(3)	(4)
Treated	**0.837****	**1.031****	**0.849****	**1.302****
	(2.10)	**(2.30)**	**(2.05)**	**(2.54)**
N of clusters	34	33	34	33
Cluster: Firm#Year	(1)	(2)	(3)	(4)
Treated	**0.837*****	**1.031*****	**0.849*****	**1.302*****
	(2.74)	**(2.85)**	**(2.74)**	**(2.66)**
N of clusters	44,417	28,886	43,921	21,761
Cluster: Analyst#Year	(1)	(2)	(3)	(4)
Treated	**0.837*****	**1.031*****	**0.849****	**1.302****
	(2.59)	**(2.67)**	**(2.55)**	**(2.50)**
N of clusters	25,624	8,086	24,738	5,370
Cluster: Broker#Year	(1)	(2)	(3)	(4)
Treated	**0.837****	**1.031*****	**0.849****	**1.302****
	(2.52)	**(2.61)**	**(2.48)**	**(2.49)**
N of clusters	4,920	2,600	4,638	1,669
Controls	Yes	Yes	Yes	Yes
Analyst fixed effect	Yes	Yes	Yes	Yes
Covered firm fixed effect	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	150,322	54,457	143,244	35,046
Adjusted R^2	0.128	0.128	0.128	0.130

This table reports regression results from estimating Equation (4) for different clustering of standard errors. The full sample contains both treatment and control group. The within-estimation sample comprises the treatment group only. In columns (3) and (4) the estimation window is restricted to five years around the acquisition events. *Treated* is an indicator variable equal to one for private analysts after acquisition by another brokerage house, and zero otherwise. *Herding* is the yearly sum of analyst forecast revisions measured as the difference between (1) the absolute distance of the analyst's prior forecast from the consensus forecast, and (2) the absolute distance of analyst's revised forecast from the consensus forecast. The consensus forecast is based on forecasts issued in the 90 days prior to analyst *i*'s forecast revision. All forecasts are scaled by the stock price of firm *j* two days before the forecasts' announcement dates. See Appendix 2.B for all other variable definitions. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Appendix 2.H. Alternative DiD specification: stacked regression design

Our DiD design in Equation (4) uses never-treated, already-treated, and later-treated analysts as control group. For the within-estimation specification, the control group comprises already-treated and later-treated analysts. As Goodman-Bacon (2021) and Baker et al. (2022) show that the inclusion of already-treated observations in the control group may add bias in the presence of treatment effect heterogeneity, we perform an additional robustness check using "stacked" regressions. Specifically, we use 5-year event windows around the treatment events and identify for each of the 122 treatment events a "clean" set of control analysts that are either never-treated or treated after the 5-year window. We define this combination of treated and control observations in a 5-year window around the acquisition date as cohort. We then stack all cohorts together, yielding a sample of 1,191,492 analyst-year observations. This approach ensures that already-treated analysts are excluded from the control group. We follow Gormley and Matsa (2011) and saturate our fixed effects structure with cohort fixed effects, namely analyst-by-cohort fixed effects, covered firm-by-cohort fixed effects, and year-by-cohort fixed effects.

The results in column (1) of online Appendix E are very similar to the main findings reported in Table 2.2. For example, the coefficient on *Treated* of 0.912 (t-stat = 2.78) is similar in magnitude and statistical significance to the coefficient in Table 2.2 from the 5-year window specification (coef. = 0.849, t-stat = 2.84). Also, the signs and significance levels of the control variables are very similar to those reported in Table 2.2.

In addition, we also run a stacked regression that uses only *never-treated* analysts as control observations for each cohort, i.e., later-treated analysts are excluded from the control group. The corresponding findings in column (2) are consistent with those in column (1) and with our main findings from Table 2.2 (coefficient estimate on *Treated* of 1.085; t-stat = 3.21). Taken together, these additional analyses confirm our main inferences.

Appendix 2.I. Alternative DiD specification: stacked regression design

Dependent variable: ***Herding***	Stacked regression design	
Control group:	later-treated & never-treated	never-treated
Variables	(1)	(2)
Treated	**0.912***** **(2.78)**	**1.085***** **(3.21)**
LagAFE	0.105*** (12.55)	0.106*** (11.67)
ForHorizon	-0.041*** (-23.45)	-0.040*** (-22.74)
DaysElapsed	-0.112*** (-15.50)	-0.112*** (-14.37)
ForFrequency	-2.535*** (-39.85)	-2.509*** (-37.53)
FirmExperience	0.057* (1.89)	0.081** (2.52)
BrokerSize	-0.015** (-2.30)	-0.032*** (-3.83)
Competition	-0.197*** (-6.02)	-0.218*** (-6.42)
Companies	0.007 (0.25)	0.030 (1.02)
Industries	-0.017 (-0.32)	-0.039 (-0.70)
Analyst × Cohort fixed effect	Yes	Yes
Covered firm × Cohort fixed effect	Yes	Yes
Year × Cohort fixed effects	Yes	Yes
N	1,191,492	979,858
Adjusted R^2	0.145	0.128

This table reports regression results from estimating Equation (4) with a stacked regression design. Specifically, we use 5-year event windows around the treatment events (i.e., acquisition of private brokerage house) and identify for each of the 122 treatment events a "clean" set of control analysts that are never-treated or later-treated after the 5-year window. We define this combination of treated and control firms in a 5-year window around the acquisition date as cohort. We then stack all cohorts together, yielding a sample of 1,191,492 analyst-year observations. In column (2), we also run a stacked regression that uses only never-treated firms as controls for each cohort, yielding a sample of 979,858 analyst-year observations. We use analyst-by-cohort fixed effects, covered firm-by-cohort fixed effects, and year-by-cohort fixed effects. See Appendix 2.B for all variable definitions. Standard errors are clustered by covered firm and the resulting t-statistics are in parentheses. *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

Chapter 3: Social media for disclosures processing? The role of Twitter's users as information intermediaries

3.1. Introduction

Social media platforms are internet-based communication technologies that feature user-generated content and lower barriers to entry relative to other traditional media channels. They provide the opportunity to instantaneously share financial information for large audiences, influencing investors' perceptions and firms' information environments despite growing concerns about the quality and credibility of the content disseminated. In fact, after a series of negative news for Tesla in November 2013, automated Twitter accounts came to life and began publishing positive sentiments about Tesla (Mitchell, 2022). Commonly called *Twitter bots,* these fake accounts are programmed through Twitter's Application Programming Interface (API) to influence users' perceptions about certain companies using cashtags, firm-specific markers for tweets written as the symbol *$* followed by a company's ticker (e.g., *$TSLA*). Such user activity questions the beneficial role of social media as a source of financial information for investors. Nonetheless, firms increasingly invest resources to use social media as a dedicated media channel for investor relations and led the Securities and Exchange Commission (SEC) to officially endorse social media as an additional communication channel for companies (SEC, 2013). To the extent that information-seeking investors use such platforms to access firms' disclosures or intermediary coverage, it seems essential to evaluate the usefulness of social media for disclosure processing.

Prior literature shows that firms strategically use Twitter as an additional channel to communicate with investors, reducing information asymmetry (Blankespoor et al., 2014; Jung et al., 2018; Nekrasov et al., 2021). On the other hand, research shows that the "wisdom of the crowds" emanating from Twitter' users provide value-relevant information to capital markets (Curtis et al., 2014; Bartov et al., 2018; Elliott et al., 2018) and thus might also reduce information asymmetry. However, user-generated content potentially has different effects than firm-initiated content due to three defining features of social media. First, social media allows instant access and dissemination of information, potentially leading to increased information asymmetry due to differences in investor processing capabilities when faced with disclosures over short periods of time (Kim and Verrecchia, 1994). Second, user-generated content can be subject to social transmission biases due to the systematic directional modification of public information as they get repeated or republished by users (Hirshleifer, 2020). Finally, social media's user base differs from traditional media due to lower barriers to entry, leading to concerns about users' credibility (Miller and Skinner, 2015; Blankespoor et al., 2020). I investigate in this paper the role of social media users as financial information intermediaries for capital markets. Specifically, I evaluate the effect of Twitter's user-generated content relative to firm-initiated content on information asymmetry around earnings announcements.

Social media content can be demanding to collect due to its proprietary nature and are often coming from third-party data providers leading to unique datasets never analyzed before. In this paper, I use Twitter's API to investigate the different effects of user-generated content relative to firm-initiated content on information asymmetry around earnings announcements. To isolate coverage of earnings announcements' disclosures on Twitter concerning specific firms, I focus on tweets marked with a cashtag to measure user-generated content on social media and use bid-ask spread as a proxy for information asymmetry (e.g., Leuz and Verrecchia, 2000). I attempt to overcome endogeneity by controlling for the market reaction and conditions to the announcement (e.g., earning surprise, turnover, volatility, etc.), as well as other factors associated with firms' information environments like firms' characteristics (e.g., presence on other social media platforms) or coverage by other information intermediaries (i.e., financial analysts and news articles). To further mitigate endogeneity concerns, I exploit two specific features from Twitter to provide variation in processing cost. I first use the verified account feature to provide variation in both users' and firms' credibility since Twitter marks accounts of public interest with a blue verified badge (i.e., verified accounts) to inform users of those accounts' authenticity. Specifically, I explore content from verified accounts before Twitter announced an online application process for all users to receive verified status. I then use the introduction of the clickable cashtag as a second feature. In 2012, Twitter officially endorsed the cashtag to incentivize users to contribute and consume financial information on its platform by providing users with the opportunity to automatically access a Twitter feed concerning a specific company's ticker. I use this introduction as an exogenous variation of investors' access to information on social media. Finally, I address the selection bias from the sample by exploring the different effects of user-generated content on information asymmetry for firms with a higher level of Twitter activity relative to other comparable firms using a Propensity Score Matching (PSM) method.

The results suggest that user-generated content is positively associated with firm-initiated content on Twitter, coverage by other intermediaries and is negatively associated with periods of lower investor attention. The results also suggest that user-generated content has a different effect on information asymmetry than firm-initiated content. I find that users' tweets are positively associated with information asymmetry around earnings announcements controlling for firms' tweets, market reaction, and other factors influencing a firm's information environment. This result provides evidence that user-generated content potentially leads to misinformation due to investors' limited processing abilities and user-generated content's social transmission bias. Investigating verified accounts, I find that the association between user-generated content and information asymmetry is stronger for users without a verified status on Twitter (i.e., users with a lower level of credibility). On the other hand, I find that the negative association between firm activity and information asymmetry is stronger for firms with verified accounts (i.e., more credible firms). Overall, the results confirm existing concerns about users' credibility due to social media's lower barriers to entry relative to other traditional media.

Exogenous variation in access to information on social media mitigates further endogeneity concerns as I observe that the positive association between user-generated content from unverified accounts and information asymmetry is stronger after Twitter introduced the clickable cashtag feature. However, I find that both user and firm-initiated content from verified accounts is only associated with a decrease in information asymmetry before the clickable cashtag introduction by Twitter. The results suggest that the sudden opportunity to automatically access a Twitter feed concerning a specific company's ticker aggravates the positive association between unverified users' activity and investors' misinformation. I also suggest that the introduction of the feature moderates the potential benefits of information disseminated by users and firms with verified accounts on Twitter. Overall, the latter results suggest that the automatic access to information on social media has detrimental effects on information asymmetry, potentially due to social transmission bias and investors' differences in processing abilities. Finally, using a PSM method to address the potential selection bias from the sample, I find that firms with a higher level of user-generated content on Twitter still experience higher information asymmetry relative to matched firms. Overall, I provide evidence that social media affects disclosure processing costs depending on users' credibility.

This paper contributes to the intermediary literature by investigating social media users as financial information intermediaries. I attempt to isolate the capital market effects of intermediary coverage by controlling for firm-initiated content on social media and other factors affecting firms' information environments. Furthermore, this paper focuses on features from social media to identify firm-specific content and introduce exogenous variation to address the empirical challenges of investigating information intermediaries (Blankespoor et al., 2020; Luca, 2015). I also contribute to the social media literature by providing evidence of the role of social media on processing costs around disclosures. I suggest that user-generated content and firm-initiated content on Twitter affect processing costs differently, and that platform features can have detrimental effects for capital markets. Finally, as prior literature raises concerns about the quality and credibility of social media's content (Miller and Skinner, 2015; Blankespoor et al., 2020), I provide evidence of its potential for misinformation and demonstrate the effect of credibility on processing costs.

The remainder of the paper is structured as follows. Section 3.2 discusses prior literature and develops my hypotheses. Section 3.3 describes the sample and variable measurement. Section 3.4 presents results, and section 3.5 concludes.

3.2. Prior literature and hypothesis development

Investors have limited attention to allocate for information acquisition and processing (Hirshleifer and Teoh, 2003; Dyck and Zingales, 2004) and can be distracted by simultaneous events such as earnings announcements seasons (Hirshleifer et al., 2009; deHaan et al., 2015). Financial information intermediaries mitigate investors' processing costs by serving three main capital market functions (Blankespoor et al., 2020). First, through a media channel, they can redistribute extracted public information to reduce awareness and acquisition costs for investors. Second, with their expertise and credibility, they may interpret the implications of public information on firms' value, decreasing integration costs for investors. Finally, financial information intermediaries could uncover private information. This paper focuses on the first two functions of information intermediaries around specific and predictable corporate events (i.e., earnings announcements). I develop in this section the hypothesis related to the potential role of social media users as information intermediaries to mitigate investors' processing costs. A growing body of the literature investigates social media as a proxy for ex-ante investor attention to predict market outcomes or as an additional channel for firms' dissemination choices. However, there is relatively little research on the role of social media users as information intermediaries for capital markets. Indeed, prior research on social media faces empirical challenges to identify firm- or event-specific content and isolate their capital market effects. I begin this section with the literature related to the impact of the internet and social media platforms on processing costs and then discuss the literature on Twitter.

3.2.1. The internet and social media as a source of information

Prior literature finds mixed evidence that the internet can provide value-relevant information. Bagnoli et al. (1999) compare First Call analyst forecasts to whisper forecasts (online forecasts of quarterly earnings per share) and find that whispers contain incrementally useful information for capital markets. However, Dewally (2003) investigates investment advice on the internet and finds no evidence of incremental valuable information for capital markets. Hu et al. (2013) suggest that stocks with low blog exposure earn higher returns than stocks with high blog exposure. Antweiler and Frank (2004) demonstrate the predictability of internet stock message boards for market volatility but find that the disagreement among the posted messages is associated with an increase in trading volume. Drake et al. (2017) find that additional coverage by non-professionals through the internet hinders price information relative to professionals' and semi-professionals' coverage. The internet expands the sources for information acquisition and processing but potentially leads to quality and credibility concerns since it provides the opportunity to a larger audience to disseminate information.

Social media platforms are computer-based technologies that feature user-generated content and personalized user profiles with low barriers to entry. User-generated content originates from several types of social media actors with different incentives (Luca, 2015). In traditional media, *contributors* and *consumers* are usually two different types of users, and the entry costs are high enough for the *contributors* to be principally professionals. Social media users can decide to produce or consume content at a significantly lower cost, leading to credibility concerns about *contributors*. *Bystanders* are the subject of the content, but they also get the opportunity to be active users and potentially influence the information disseminated on social media. Finally, *designers* define platforms' features with the ultimate objective to incite users to be consumers or contributors.[33] Social media's broader user base relative to traditional media potentially leads to different economic effects depending on users' credibility.

[33] Luca (2015) also identifies a fourth type of social media users, namely *advertisers* that try to reach users by either disseminating promotional content or trying to directly influence user content. In the context of this study, I consider *advertisers* as part of the broader group of *contributors*.

The recent popularity of social media provides the opportunity to access individual public opinions about firms through user-generated content. Prior literature finds that social media contain value-relevant information predicting future corporate disclosures. For example, Chen et al. (2014) find that the different types of user-generated content on Seeking Alpha (a provider of crowdsourced opinion and analysis for stocks) predict future stock returns and earnings surprises. On the same platform, Gomez et al. (2018) find that more pre-announcement crowdsourced financial analysis mitigates information asymmetry, decreases opinion divergence for earnings announcements, and is more beneficial to less sophisticated investors. Jame et al. (2016) find that Estimize (an open platform that solicits and reports forecasts) is incrementally useful in measuring the market's expectations and forecasting earnings. The paper concludes that crowdsourced forecasts are a supplementary source of information in capital markets. Finally, the literature focusing on Glassdoor.com (a platform where employees can share their opinions) finds evidence that employee voluntary disclosures about firm performance predict future corporate disclosures (Hales et al., 2018, Huang et al., 2020). Huang et al. (2020) also find that employee disclosures on Glassdoor.com are incrementally informative in predicting future returns of firms that attract less attention from information intermediaries. Recently, Booker et al. (2022) use StockTwits data to generate a measure of investors disagreement and find that it is associated with trading volume especially when disclosure processing costs are lower.

Overall, the literature suggests that social media's user-generated content provides incrementally useful information to capital markets but lacks evidence on the role of user-generated content for information processing around earnings announcements (Blankespoor et al., 2020). Furthermore, social media platforms' lower barrier to entry and lack of traditional oversight might also lead to investors' misinformation (Miller and Skinner, 2015; Drake et al., 2017) depending on the quality and credibility of the disseminated information.

3.2.2. Prior Literature on Twitter

Prior literature on social media focuses on Twitter for two main reasons. First, Twitter allows its users to share and access timely financial information aggregated by handles such as hashtags (use of the symbol # followed by a keyword) and cashtags (use of the symbol $ followed by a company's ticker). Second, Twitter's timeliness and reach for financial information made it the most popular social media platform among firms to create official corporate accounts (symbol @ followed by a company's username) for investor relations. For those two reasons, the literature either focuses on user activity aggregated by handles such as hashtags to proxy investors' attention or focuses on firms and their dissemination choices through official corporate accounts.[34]

[34] I acknowledge an additional strand of the literature focusing on the role of user-driven dissemination on Twitter as information with predictive power for financial market. Bollen et al. (2011) using a large-scale sample of public tweets investigate whether collective mood from Twitter users is correlated to the value of the Dow Jones Industrial Average (DJIA) over time and find that the accuracy of DJIA predictions can be improved by the inclusion of specific public mood dimensions but not others. Bartov et al. (2018) aggregate the opinion of

The first strand of the literature investigates the role of user-driven dissemination on Twitter as a measure of investor attention. Curtis et al. (2014) use a proprietary measurement for user-driven content on Twitter to proxy investor attention around earnings announcements. They find that high levels of investor attention are associated with greater sensitivity of earnings announcements returns to earnings surprises and suggest that social media activity has distinct effects on the pricing and mispricing of earnings news when compared to other information sources. Similarly, Jiang and Shen (2017) separate tweets sent by journalists and individuals about business scandals and find that individuals' opinions on social media complement rather than substitute traditional media in covering business scandals. Recent papers investigate the potential negative market effect of social media activity. Jia et al. (2020) investigate twitter activity around merger rumors and find that user activity can act as a "rumor mill" causing mispricing and potentially spreading false information. Finally, Campbell et al. (2022) investigate cashtag tweets around earnings announcements and suggest that companies subject to extreme levels of user-driven dissemination (a phenomenon they label "Earnings virality") can experience counter-productive market effects.

The second strand of the literature examines firms' dissemination choices on Twitter and their impact on market quality. Blankespoor et al. (2014) focus on firms' use of Twitter and find that firms who indicate on social media their existing disclosures are associated with a reduction in information asymmetry. Interestingly, Lee et al. (2015) is the only paper attempting to measure both firm and user activity on Twitter. The paper investigates tweets from official corporate accounts (the symbol @ followed by the company's username) and tweets referring to those accounts in their text. The authors document that the attenuation benefits of Twitter on the price reaction to consumer product recall depends on the level of control the firm has over its social media content. Looking at earnings announcements, Jung et al. (2018) provide further evidence on firms' adoption and strategic use of social media platforms for investor relations. They suggest that companies with higher visibility are more likely to adopt social media platforms and are less likely to disseminate negative news concerning earnings via social media. Nekrasov et al. (2020) find that firms using visuals for earnings-related tweets from their official corporate handles experience greater Twitter user engagement and higher earnings response coefficient. Such results suggest the importance of ex-ante firms' social media disclosure to attract investor attention and reduce information asymmetry.

Despite such compelling evidence of the capital market benefits of Twitter as a source of valuable information (Curtis et al., 2014; Bartov et al., 2018; Campbell et al., 2022) and as an additional outlet for firms' investor relations (Blankespoor et al., 2014, Jung et al., 2018, Nekrasov et al., 2020) the literature remains silent on the role of Twitter for information processing around firms' earnings announcements and especially the impact of user-generated content on processing costs.

Twitter users using tweets marked by cashtags and find that such measure successfully predicts company's forthcoming quarterly earnings and announcement returns. Finally, Tang (2018) demonstrates the predictive ability of tweeted product opinions for sales growth using third party proprietary information to aggregate Twitter comments about products and brands at the firm level.

3.2.3. Hypothesis development

Social media provides the opportunity for a larger audience of users to be *consumers* and *contributors* to platforms' content. From traditional information intermediaries to non-professional actors, this variety of users can not only disseminate and interpret financial information but also directly interact with and influence firms' information environment voluntarily. Furthermore, firms as bystanders get the opportunity to be active users and thus potentially influence their information environment on social media. My first hypothesis relates to the determinants of user-generated content on Twitter. Bushee et al. (2010) find that news dissemination in the business press from journalists is associated with firm-initiated news articles and coverage by other intermediaries such as analysts. I predict that user-generated content on Twitter about specific firms is positively associated with firm-initiated tweets and coverage by other financial intermediaries during earnings announcements periods. Furthermore, to the extent that prior literature uses social media activity to measure investors' attention (Curtis et al., 2014), I predict that user-generated content is positively associated with investors' attention. My hypothesis is the following:

H1 – User-generated content is positively associated with firms' disclosures on social media, other intermediaries' coverage, and investors' attention around earnings announcements.

My second hypothesis relates to the role of social media on processing costs. Information asymmetry can occur from differences in investors' processing abilities. Prior literature emphasizes that the dissemination of public information reduces information asymmetry (Diamond and Verrecchia 1991) and how uninformed investors can benefit from seeking additional sources for information (Grossman and Stiglitz 1980). The intermediary literature finds evidence that financial intermediaries such as traditional media provide information-seeking investors with valuable information and mitigate processing costs.[35] As the literature finds that firm-initiated content (Blankespoor et al., 2014; Jung et al., 2018; Nekrasov et al., 2020) or user-generated content (Bollen et al., 2011; Bartov et al., 2018; Tang, 2018) on platforms such as Twitter contains value-relevant information, social media also has the potential to be an additional source for information-seeking investors and thus mitigate processing costs.

However, I argue that user-generated content can negatively impact information asymmetry around earnings announcements. Kim and Verrecchia (1994) show that greater firm disclosure in short windows allows investors to differentially exploit their information processing abilities, which increases information asymmetry. As social media platforms provide the opportunity to spread information to large audiences instantaneously, they can potentially exacerbate investors' differences and increases information asymmetry. Second, Hirshleifer (2020) explains how distortive network effects, called social transmission bias, can amplify information biases due to repeating or republishing public information. Cade (2018) suggests that the volume rather than the quality of user-generated content criticizing firms influences

[35] e.g., Huberman and Regev, 2002; Bushee et al., 2010; Engelberg and Parsons, 2011; Drake et al., 2014; Lawrence et al., 2018

the perceptions of nonprofessional investors. Jung et al. (2018) find that negative earnings disclosed by firms on Twitter are intensified by social media users and negatively impact firms' information environments. Jia et al. (2020) find that user-generated content on social media intensifies merger rumors, potentially causing misinformation. Campbell et al. (2022) find that extreme levels of user-generated content (a phenomenon they label "Earnings virality") is associated with lower market quality around earnings announcements.

On the other hand, firms get the opportunity to be active on social media, and prior literature suggests that firms benefit from both disseminating information (Blankespoor et al., 2014; Jung et al., 2018) and monitoring user-generated content (Cade, 2018; Tang, 2018). To the extent that investors use social media platforms to access disclosures from firms and intermediaries, I hypothesize that user-generated content has a different effect on processing costs than firm-initiated content. I predict that user-generated content is positively associated with information asymmetry around earnings announcements due to investors' limited processing abilities and social media's quality concerns. On the other hand, I predict that firm-initiated content is associated with a decrease in information asymmetry around earnings announcements. My second hypothesis is the following:

H2 – The association with information asymmetry is positive for user-generated content and negative for firm-initiated content on social media around earnings announcements.

My third hypothesis relates to the credibility concerns of social media. Social media's lower barriers to entry for users relative to other traditional media channels raises concerns about users' credibility due to the lack of regulatory oversight on social media platforms (Miller and Skinner, 2015; Blankespoor et al., 2020). The internet literature finds mixed evidence for those concerns: Dewally (2003) suggests that credibility concerns for online investment advice are groundless, but Drake et al. (2017) find that the association between market quality and internet coverage is dependent on intermediaries' level of professionalism. As social media platforms provide verification features to increase user experience, I hypothesize the association of social media activity with information asymmetry to be dependent on users' credibility. I predict a stronger association between user-generated content and increased information asymmetry for less credible users. I also expect a stronger negative association between firm-initiated content and information asymmetry for more credible firms. My final hypothesis is as follows:

H3 – The association with information asymmetry for user-generated content and firm-initiated content on social media depends on users' and firms' credibility.

3.3. Sample selection

I obtain quarterly earnings announcement dates for the sample from January 2010 until December 2014 for companies with non-missing values across Compustat, CRSP, and IBES. I manually collect the company's official corporate accounts on Twitter from companies' websites. As firms can have multiple Twitter accounts (accounts dedicated to geographic regions, career opportunities, customer services, etc.), I only retain the accounts explicitly designated by firms for investor relations. Unsurprisingly, Twitter Inc. has multiple accounts on Twitter (e.g., @Twitter, @TwitterAPI, @TwitterSupport), but I only retrieve tweets from its dedicated account for investor relations (i.e., @TwitterIR) to measure the company's initiated financial disclosures on Twitter around earnings announcements. I also collect from companies' websites and other social media corporate accounts such as LinkedIn, Facebook, Instagram, and YouTube.

I obtain tweet data directly from Twitter's API. First, I download all tweets that include a company's cashtag defined as the symbol $ followed by the company's ticker (e.g., $TWTR for Twitter, Inc.). This methodology is used in prior research to identify user-disseminated financial information relating to a specific firm (e.g., Bartov et al., 2018; Jia et al., 2020; Campbell et al., 2022). Second, for each firm associated with a cashtag and active on Twitter, I download all tweets from the firm's corporate account identified by the symbol @ followed by the company's username if existing (e.g., @TwitterIR for Twitter, Inc.). Of the 5'153 unique firms present in the sample, 2'508 have a corporate account on Twitter.

3.3.1. Twitter user-generated content measures

I examine the effect of Twitter user-generated content on firms' information environments, thus following prior literature on the role of information intermediaries for information processing (Bushee et al., 2010; Blankespoor et al., 2020). I use firms as their control and compute abnormal Twitter activity during earnings announcement periods. I measure user-generated Twitter content for a specific firm as the number of tweets marked with the firm's cashtag during earnings announcements. *AbnUserTweets* is the log of the average daily number of tweets issued by users containing a specific firm's cashtag on day t. over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. I also measure firm-initiated disclosures on Twitter as the number of tweets issued by firms' corporate accounts on Twitter during earnings announcements. *AbnFirmTweets* is the log of the average daily number of tweets from a company's corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date.

3.3.2. Information asymmetry measure

I investigate the role of Twitter's users as information intermediaries to mitigate information processing costs relative to firms' disclosures on Twitter. Thus, I examine the impact of user activity on firms' information environments using a measure of abnormal information asymmetry around earnings announcements. Following prior literature, I use the bid-ask spread to proxy information asymmetry (Leuz and Verrecchia, 2000). I define *AbnBidAsk* as the log of the average daily bid-ask spread divided by the midpoint price over the three-day earnings announcements period divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the earnings announcements date.

3.3.3. Control variables

Following deHaan et al. (2015), I control for periods of low investor attention. *After* is an indicator variable equal to one for earnings announced after 4 pm. *Friday* is an indicator variable equal to one for earnings announced on Fridays. *EAFrequency* is the decile rank of the frequency of earnings announcements per day. Deciles are formed within the sample by calendar year. I also control for the market reaction to firms' earnings announcements using several measures. *AbsEarningSurprise* is the absolute value of the consensus earnings forecast error deflated by the price at the end of the prior year's fiscal quarter. *AbnTurnover* is the log of the average daily trading dollar volumes divided by the average market value of shares outstanding over the three-day earnings announcements period divided by the average daily impact over the trailing eight weeks ending two days before the earnings announcements date. *AbnVolatility* is the log of the average daily volatility over the three-day earnings announcements period divided by the average daily impact over the trailing eight weeks ending two days before the earnings announcements date. *AbnReturn* is the absolute value of the difference between a firm's cumulative return and the cumulative equal-weighted market return over days [−1, +1]. *BooktoMarket* is the Book-To-Market assets ratio on the fiscal quarter end date. I control for other factors associated with firms' information environment. *MarketCap* is the log of the market value (in thousands) on the fiscal quarter end date. *Analyst* is the log of one plus the number of analysts following a firm retrieved from IBES. *AbnNews* is the log of the average *News* over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *News* is the daily number of news articles based on Jeon et al. (2021) measure. *SocialMedia* is the log of one plus the number of other social media platforms used by a company.

3.3.4. Descriptive Statistics

[Table 3.1 about here]

Table 3.1 provides preliminary descriptive statistics for the variables described above. The mean and median for *AbnUserTweets,* abnormal user-generated content, are positive (1.426 and 1.403, respectively), indicating a higher use of firms' cashtags during earnings announcements on Twitter. The mean abnormal firm-initiated Twitter activity is positive (0.137) and suggests that firms tweet more on average around earnings announcements. However, the median is zero and indicates a positively skewed distribution of firms' behavior on Twitter around revenues announcements. Interestingly, the mean is higher for both user-generated content and firm-initiated content coming from unverified accounts on Twitter, indicating that a significant part of the content on Twitter is coming from less credible sources on the platform. The mean and median abnormal bid-ask spreads are equal to 0.068 and 0.024, respectively, suggesting that information asymmetry on average does not increase significantly during earnings announcements relative to Twitter activity.

3.4. Results

3.4.1. The determinants of user-generated content on Twitter

User-generated content on Twitter about a specific firm cashtag represents a variety of identification challenges as any observed association leads to potential reflection problems (Manski, 1993), making difficult any causal interpretation. User-generated content is voluntary and therefore exhibits self-selection (Luca, 2015): the decision to tweet about a firm's cashtag depends on users' incentives such as ownership, extreme opinions, willingness to uncover perceived obscure information, or platform incentive design. In this section, I provide descriptive evidence on the determinants of abnormal user-generated content around firms' quarterly earnings announcements using the OLS estimation of the following model:

$$AbnUserTweets = \beta_1 AbnFirmTweets + \beta_i Controls_i + \alpha_{FE} + \varepsilon \qquad (3.1)$$

Where *AbnUserTweets* is the log of the average daily number of tweets issued by users containing a firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmTweets* is the log of the average daily number of tweets issued by a company's corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *Controls* is a vector of *i* controls for the market reaction around earnings announcements and other factors associated with firms' information environments. I use industry-quarter fixed effects that control for industry-specific (contemporaneous) shocks in abnormal user-generated content. I use standard errors clustered by firms to assess statistical significance.

Table 3.2 presents the results of these regressions. In column 1, I examine the association between *AbnUserTweets* and *AbnFirmTweets*. I then introduce in column 2 industry-quarter fixed effects to account for trends in user-generated content within industries. Finally, in columns 3 and 4, I control for different factors to assess the association of *AbnUserTweets* with investor attention and other intermediary coverage.

[Table 3.2 about here]

Across all four specifications, the coefficients on *AbnFirmTweets* are significantly positive. In column 1 (2), the coefficient of 0.037 (0.034) indicates that ceteris paribus, a 1% increase in firms' tweets is associated with a 0.037% (0.034%) increase in user tweets during earnings announcements periods relative to the two months before. In column 3, I find that

AbnUserTweets is negatively associated with all three controls for periods of low investor attention providing evidence for my first hypothesis. The final column suggests that a 1% increase in firms' tweets is associated with a 0.027% increase in user tweets during earnings announcements relative to the two months before. The coefficient of −0.047 on *After* in column 4 indicates 4.7% fewer abnormal user tweets mentioning a firm's cashtag for earnings announced after trading hours. Earnings announcements on Fridays are associated with a decrease of 18.8% in the number of tweets mentioning firms' cashtags. Finally, the negative coefficient of −0.031 on *EAFrequency* indicates that a one decile increase in *EAFrequency* is associated with 3.1% fewer cashtag tweets from users. User-generated Twitter is also negatively associated with the absolute earning surprise and negative earnings surprise. There are 4.6% fewer cashtag tweets for negative earnings surprise. The significant positive coefficients on *AbnTurnover* and *AbnVolatility* indicate that a 1% increase in turnover (volatility) during firms earning announcements is associated with a 0.38% (0.10%) higher number of user tweets mentioning firms' hashtags relative to the two months before. Larger companies and firms with lower growth potential experience lower user-generated content about their cashtag potentially due to higher prior cashtag activity for those firms in the two-month leading to their earnings announcements or users' incentives to tweet about smaller firms and higher growth firms. Finally, the number of cashtag tweets from users is positively associated with the number of news articles concerning a firm and the presence of firms on other social media platforms. Overall, the results provide evidence for my first hypothesis. I suggest that the number of user tweets about firms' cashtags during earnings announcements is positively associated with firm-initiated tweets, investor attention, and intermediary coverage on other media channels. The results also suggest that user-generated content on Twitter is positively associated with higher turnover and volatility trading periods and negatively associated with the earning surprise (when negative), with larger firms or firms with lower growth potential and higher abnormal returns.

3.4.2. The role of Twitter's users as information intermediaries

I examine whether activity on Twitter is associated with liquidity around earning announcements using the OLS estimation of the following model:

$$AbnBidAsk = \gamma_1 AbnUserTweets + \gamma_2 AbnFirmTweets + \gamma_i Controls_i + \alpha_{FE} + \varepsilon \qquad (3.2)$$

Where *AbnBidAsk* is the log of the average daily bid-ask spread divided by the midpoint price over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the earnings announcements date. The main explanatory variables measuring Twitter activity are *AbnUserTweets* and *AbnFirmTweets*. *AbnUserTweets* is the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmTweets* is the log of the average daily number of tweets issued by a company's corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. I use a set of quarter and industry (Fama-French 49) fixed effects. *Controls* is a vector of *i* controls for the market reaction around earnings announcements as well as other factors associated with firms' information environments. I use a set of industry-quarter fixed effects to control for industry-specific (contemporaneous) shocks.[36] Statistical significance is based on robust standard errors clustered by firm.[37] To the extent that the measure for user-generated content *AbnUserTweets* represents overall user activity on Twitter, I predict positive coefficients for γ_1 and negative coefficients for γ_2 as firms' disclosure on social media platforms should decrease information asymmetry.

Table 3.3 presents the regression result of the second model. I use in all four columns industry-quarter fixed effects. I investigate in columns 1 and 2 the association between *AbnUserTweets* and *AbnBidAsk* with and without controls respectively. In columns 3 and 4, I replace *AbnUserTweets* with *AbnFirmTweets* to examine its association with *AbnBidAsk*. Finally, I introduce in columns the last two columns both measures of social media activity (*AbnUserTweets* and *AbnFirmTweets*). Column 6 corresponds to the full specification model as I control for market reactions to earnings announcements, and other factors affecting firms' information environments.

[Table 3.3 about here]

Across all specifications, the coefficients on *AbnUserTweets* are positive and significant, suggesting a negative association between abnormal levels of user-generated content and market information asymmetries. The significantly positive coefficient of 1.188 on *AbnUserTweets* in column 1 indicates that ceteris paribus, a 1% increase in abnormal user cashtag tweets is associated with an increase of 0.011% (as the dependent variable is multiplied by a 100 to facilitate coefficient exposure) in abnormal bid-ask spreads. In column 2, the significantly positive coefficient of 1.890 on *AbnUserTweets* indicates that a 1% increase in abnormal user tweets is associated with an 0.018% increase in abnormal bid-ask spreads. I replace *AbnUserTweets* with *AbnFirmTweets* in columns 3 and 4 but fail to find any association between firm-initiated tweets and bid-ask spreads. In column 6, the results indicate that a 1% increase in user cashtag tweets during their earnings announcements is associated with an increase in bid-ask by 0.018% relative to the two months before. *AbnBidAsk* is significant and positive for periods of lower investor attention, higher volatility, larger firms, and higher analysts' coverage. But the coefficients on *AbnBidAsk* are negative and significant for the magnitude of the earning surprise, turnover, higher abnormal returns, and Book-to-Market ratio. In columns 5 and 6, the insignificant coefficient on *AbnFirmTweet* and the rejection of the null hypothesis that the coefficients on *AbnUserTweets* and *AbnFirmTweet* are equal provide partial evidence for my second hypothesis. Overall, the results suggest partial evidence for the second hypothesis as I find a different association on abnormal bid-ask spread for user-

[36] Luca (2015) also identifies a fourth type of social media users, namely *advertisers* that try to reach users by either disseminating promotional content or trying to directly
influence user content. In the context of this study, I consider *advertisers* as part of the broader group of *contributors*.
[37] I acknowledge an additional strand of the literature focusing

generated content than for firm-initiated content. I suggest that the positive association between information asymmetry and user-generated content on social media around earnings announcements is due to investors' limited processing abilities and social media's quality concerns.

3.4.3. User-level variation in credibility: the verified accounts feature

Following scandals in 2009, Twitter introduced verified accounts to improve user experience by establishing the authenticity of identities on Twitter (Stone, 2009). Twitter reviews on an ongoing basis all persons of public interest within areas such as politics, religion, journalism, media, sport, business, and others and verifies the identity of such accounts to help users identify the source of tweets' information. In 2016, Twitter announced an online application process for all users to receive verified status (Bhatnagar, 2016). I exploit the verified accounts feature from Twitter to provide variation in users' credibility. I examine the different effects of Twitter user activity on information asymmetry depending on the verified status (verified or unverified) of users' accounts using the following model:

$$\begin{aligned} AbnBidAsk = &\lambda_1 AbnUserVerified + \lambda_2 AbnUserUnverified \\ &+ \lambda_3 AbnFirmVerified + \lambda_4 AbnFirmUnverified \\ &+ \lambda_i Controls_i + \alpha_{FE} + \varepsilon \end{aligned} \tag{3.3}$$

Where *AbnBidAsk* is the log of the average daily bid-ask spread over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread divided by the midpoint price over the trailing eight weeks ending two days before the earnings announcements date. *AbnUserVerified* is the log of the average number of tweets from users with a verified account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnUserUnverified* is the log of the average number of tweets from users with an unverified account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmVerified* is the log of the average number of tweets issued by a company with a verified account on Twitter over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmUnverified* is the log of the average number of tweets issued by a company with a verified account on Twitter over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements. I use a set of industry-quarter fixed effects. [38] *Controls* is a vector of *i* controls for firm activity on Twitter, the market reaction around earnings announcements, and other factors associated with firms' information environments. Statistical significance is based on robust standard errors clustered by firm.[39] To the extent that tweets from verified accounts

on the role of user-driven dissemination on Twitter as information with predictive power for financial market. Bollen et al. (2011) using a large-scale sample of public tweets investigate whether collective mood from Twitter users is correlated to the value of the Dow Jones Industrial Average (DJIA) over time and find that the accuracy of DJIA predictions can be improved by the inclusion of specifi should decrease information asymmetry as opposed to tweets from unverified accounts, I predict the coefficients for tweets from verified accounts ($\lambda_1$and λ_3) to be negative and the coefficients for tweets from unverified accounts ($\lambda_2$and λ_4) to be positive.

[Table 3.4 about here]

Table 3.4 presents the results of these regressions. Across all specifications including both verified and unverified user generated content, only coefficients on *AbnUserUnverified* are significant and positive across. In column 4, the coefficient of 1.918 indicates that a 1% increase in the number of tweets issued by users without a verified account on Twitter is associated with a 0.019% higher bid-ask spread around earnings announcements. The coefficients on *AbnUserVerified* are not significantly different from zero and F-tests for the differences in coefficients on *AbnUserVerified* and *AbnUserUnverified* reject the null hypothesis at the 1% level across all specifications. The latter results suggest that the positive association between *AbnUserTweets* and *AbnBidAsk* from table 3.3 is driven by tweets from users without a verified account on Twitter. In column 2 the coefficient for *AbnFirmTweets* is not significantly different than zero providing evidence of a lack of association between firms' activity on Twitter and information asymmetry. In columns 3 and 4, I replace *AbnFirmTweets* with *AbnFirmVerified* and *AbnFirmUnverified* and observe that only the coefficients for *AbnFirmVerified* (i.e., tweets issued by companies with a verified account) are significantly negative. In column 4, a 1% increase in the number of tweets initiated by firms with verified accounts is associated with 0.014% lower bid-ask spreads. The coefficients on *AbnFirmUnverified* (i.e., tweets issued by companies without a verified account on Twitter) are not significantly different from zero and all the F-tests for the difference in coefficients on *AbnFirmVerified* and *AbnFirmUnverified* reject the null hypothesis at the 5% level across all specifications. I suggest that the negative association between user-generated content on Twitter with information asymmetry originates from users without a verified account. The results also imply that only tweets initiated by firms with a verified account are associated with lower information asymmetry. The heterogeneity of the level of verified users' professionalism on Twitter might explain the absence of a significant association between tweets issued by verified users and information asymmetry. Overall, the results provide evidence for the second and third hypotheses. I suggest that unverified users are at the origin of the association between user-generated content and investors' misinformation around earnings announcements. Furthermore, I suggest that only content initiated by firms perceived to be more credible on social media is associated with a reduction in information asymmetry around earnings announcements.

[Figure 3.1 about here]

3.4.4. Exogenous variation in processing costs: the clickable cashtag feature

On the 31st of July 2012 Twitter issued a tweet through its corporate account *@Twitter* stating that "Now you can click on ticker symbols like $GE on twitter.com to see search results about stocks and companies" (Twitter inc., 2012). I use the introduction of the clickable cashtag as a shock to investors processing costs since it suddenly provided Twitter users with the opportunity to automatically access a Twitter feed concerning a company's ticker symbol. Figure 1 provides evidence of the influence of this exogenous shock on users' incentives to use cashtags in their tweets by plotting the quarterly average of the variable *AbnUserTweets* during the sample period.

I examine whether the association between Twitter user activity and information asymmetry is more pronounced for earnings announced in the period after the introduction of the clickable cashtag using the following model:

$$\begin{aligned} AbnBidAsk = \; & \delta_1 AbnUserTweets + \delta_2 AbnUserTweets \times Clickable \\ & + \delta_3 AbnFirmTweets + \delta_4 AbnFirmTweets \times Clickable \\ & + \delta_i Controls_i + \alpha_{FE} + \varepsilon \end{aligned} \quad (3.4)$$

Where *AbnBidAsk* is the log of the average daily bid-ask spread over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread divided by the midpoint price over the trailing eight weeks ending two days before the earnings announcements date. The main explanatory variables measuring Twitter activity are *AbnUserTweets* and *AbnFirmTweets*. *AbnUserTweets* is the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmTweets* is the log of the average daily number of tweets issued by a company's corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *Clickable* is an indicator variable equal to 1 for quarterly earnings announcements in the period after the 31st of July 2012. I use a set of industry-quarter fixed effects. *Controls* is a vector of *i* controls for firm activity on Twitter, the market reaction around earnings announcements, and other factors associated with firms' information environments. Statistical significance is based on robust standard errors clustered by firm. The coefficient of interest δ_2 (δ_4) measures the difference in the effect of *AbnUserTweets* (*AbnFirmTweets*) on *AbnBidAsk* between earnings announcements after the third quarter of 2012 and base EA issued before the introduction of the clickable cashtag.

[Table 3.5 about here]

Table 3.5 presents the results of these regressions. In columns 1 and 2, the coefficients on *AbnUserTweets* are significant and positive, indicating that for base earnings announcements (i.e., earnings announcements before the third quarter of 2012) I find a similar association between users' cashtag tweets and bid-ask spreads to the one observed in table 3.3. For earnings announcements before the clickable cashtag introduction, a 1% increase in users' cashtag tweets leads to 0.015% higher bid-ask spreads after controlling industry trends (compared to an estimate of 0.018% increase in table 3.3). The coefficients on *AbnUserTweets×Clickable* are also significantly positive, and I reject the null hypothesis that the sum of coefficients δ_1 and δ_2 is equal to zero. This result indicates a different association between *AbnUserTweets* and *AbnBidAsk* in the post-period. In column 1, the coefficient on the interaction term is equal to 1.072, indicating a 0.010% increase in bid-ask spreads for a 1% increase in users' tweets during earnings announcements in the post-period. The effect of *AbnUserTweets* on *AbnBidAsk* in the post-period is 2.625 (i.e., $\delta_1+\delta_2$). This result implies that a 1% increase in users' cashtag tweets is associated with a 0.026% increase in bid-ask spreads during earnings announcements after the second quarter of 2012 (relative to a 0.015% increase for earnings announcements the before the second quarter of 2012). Figure 2 represents graphically this result where I replace the indicator variable *Clickable* with quarterly indicators variables interacted with *AbnUserTweets* omitting the interaction term *AbnUserTweets×2012q2* to use the second quarter of 2012 as a benchmark (see Appendix 3.B for the model estimation). The results confirm the differential association between *AbnUserTweets* and *AbnBidAsk* in the post-clickable period observed in table 3.4.

[Figure 3.2 about here]

In column 2, I also interact *Clickable* with *AbnFirmTweets*. I find a negative association (albeit weak) between firms' tweets and bid-ask spreads for earnings announcements in the pre-period as the coefficients on *AbnFirmTweets* are negative. The coefficient on the interaction term *AbnFirmTweets × Clickable* is positive albeit not being significantly different from zero, and I fail to reject the null hypothesis of the sum of coefficients between δ_3 and δ_4. This result indicates that there is no significant different association between *AbnFirmTweets* and *AbnBidAsk* around the introduction of the clickable cashtag.

To provide further evidence of the effect of the clickable cashtag introduction, I replace in column 3 *AbnUserTweets* with *AbnUserVerified and AbnUserUnverified* and interact both coefficients with *Clickable*. I find a similar association between unverified users' cashtag tweets and bid-ask spreads to the one observed in table 3.4. For earnings announcements before the clickable cashtag introduction, a 1% increase in unverified users' cashtag tweets is associated with a 0.016% increase in bid-ask spreads. The coefficient for *AbnUserUnverified ×Clickable* is also significantly positive, and I reject the null hypothesis for the sum of coefficients. The result suggests that ceteris paribus, a 1% increase in unverified users' cashtag tweets is associated with a 0.025% increase in bid-ask spreads during earnings announcements after the second quarter of 2012. Interestingly the coefficient for *AbnUserVerified* is significant and negative, but the coefficient on the interaction term *AbnUserVerified × Clickable* is significant and positive. As I fail to reject the null hypothesis that the coefficient for both *AbnUserVerified* and the interaction term is different than zero, the results suggest that user-generated content from verified accounts is associated with a decrease in information asymmetry only in the pre-period.

In column 4, I also replace *AbnFirmTweets* with *AbnFirmVerified* and *AbnFirmUnverified.* Interestingly the coefficient on *AbnFirmVerified* is significant and negative, but the coefficient on the interaction term *AbnFirmVerified* × *Clickable* is not statistically different from zero. I fail to reject the null hypothesis that the coefficients for both *AbnFirmVerified* and *AbnFirmVerified* × *Clickable* differ from zero. I observe similar results for the interaction of *Clickable* with both *AbnUserVerified and AbnUserUnverified.* Exogenous variation in processing costs leads to a stronger positive association between unverified users' tweets and information asymmetry. However, I only find that both user and firm-initiated content from verified accounts is associated with a decrease in information asymmetry before the introduction of the clickable cashtag by Twitter. Overall, the results provide evidence that the sudden opportunity to automatically access a Twitter feed concerning a specific company's ticker aggravates the positive association between investors' misinformation and social media activity coming from unverified users. I also suggest that the clickable feature moderates the potential benefits of information disseminated by users and firms with verified accounts on Twitter.

3.4.5. Firm-level variation in social media activity: Viral firms on Twitter

Firms' and disclosures' characteristics potentially determine the existence of Twitter activity and lead to a selection bias. I address such concern in this section by investigating the differential association between user-generated content on Twitter and information asymmetry for firms with extreme levels of cashtag tweets on Twitter (viral firms) using the following model:

$$AbnBidAsk = \alpha_1 ViralFirm + \alpha_2 AbnFirmTweets + \alpha_i Controls_i + \alpha_{FE} + \varepsilon \qquad (3.5)$$

AbnBidAsk is the log of the average daily bid-ask spread over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread divided by the midpoint price over the trailing eight weeks ending two days before the earnings announcements. The main explanatory variables measuring Twitter activity are *ViralFirm* and *AbnFirmTweets*. *ViralFirm* is an indicator variable equal to 1 for firms with an average *AbnUserTweets* above the 75% quantile within the sample. *AbnUserTweets* is the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmTweets* is the log of the average daily number of tweets issued by a company's corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. I use a set of industry-quarter fixed effects and assess statistical significance with standard errors clustered by firms. *Controls* is a vector of *i* controls for firm activity on Twitter, the market reaction around earnings announcements, and other factors associated with firms' information environments. The coefficient of interest α_1 measures the effect on *AbnBidAsk* for firms with extreme levels of cashtag tweets on Twitter relative to other similar firms.

Table 3.6 presents the results of these regressions. In column 1 for the full sample, I find a positive coefficient for the association between users' tweets and information asymmetry for firms with extreme levels of Twitter activity. In column 2, I use the Propensity Score Matching (PSM) method to match each viral firm with a similar non-viral firm. As a first step, I estimate the probit model using *ViralFirm* as the dependent variable and using all covariates from the determinant model and industry fixed effects (see Appendix 3.C for an explanation of the PSM estimation). The results in column 2 using the PSM sample imply a positive association between users' tweets and information asymmetry for firms with extreme levels of Twitter activity relative to other similar firms. In the last columns of table 3.6, I estimate equation 5 for the viral firms and non-viral firms separately. Across all specifications, the results suggest a positive association between *AbnUserTweets* on *AbnBidAsk.* In column 3 (4), a 1% increase in the number of cashtag users' tweets for viral (non-viral) firms is associated with 0.017% (0.010%) lower bid-ask spreads around earnings announcements. The results suggest that firms with higher levels of user-generated content on Twitter still experience higher information asymmetry relative to matched firms.

[Table 3.6 about here]

3.5. Conclusion

This paper investigates the role of social media in mitigating processing costs. Social media platforms allow anyone to be an information intermediary for capital markets by redistributing extracted or interpreted public information. Firms as the subject of user-generated content also get the opportunity to contribute or monitor information disseminated on social media. I explore in the paper the potentially different effects of Twitter's user-generated content on information asymmetry around earnings announcements relative to firm-initiated content on social media. The results indicate that information asymmetry around earnings announcements is differently associated with user-generated content than firms-initiated content on Twitter after controlling for market reaction and other factors influencing a firm's information environment. Investigating verified accounts, I find that the positive association between user-generated content and information asymmetry comes from users without a verified status on the platform (i.e., less credible users). However, the negative association between firm activity on Twitter and information asymmetry originates from firms with verified accounts on Twitter (i.e., more credible firms). I confirm prior literature's concerns about the credibility of social media's content and provide evidence of the platforms' potential for social transmission bias. Additionally, I suggest that platforms' features detrimentally affect information asymmetry due to investors' differences and limitations in processing abilities. Overall, the results question the usefulness of social media for disclosure processing.

This study is subject to several caveats. First, I focus on a specific corporate event in a short window. The impact of user-generated content may be different in less predictable corporate events with lower coverage from other information intermediaries. One would expect social media platforms to be advantaged compared to other traditional media as channels to disseminate timely information about unforeseeable corporate events. Second, I focus on a single social media platform to attempt a causal interpretation of the impact of user-generated content. As other platforms are available to investors, a cross-platform comparison could be another approach to investigate user-generated content and exploit differences in platform features.

Prior literature on social media should be moving toward causal claims rather than predicting market outcomes and bring the theoretical literature more directly into empirical studies with the help of better-identified research questions. This paper attempts to move toward causal estimates of the impact of user-generated content from social media platforms. Future research should focus on optimal data and identification challenges to investigate relatively neglected phenomena within social media platforms with a reliance on social economics and finance theory.

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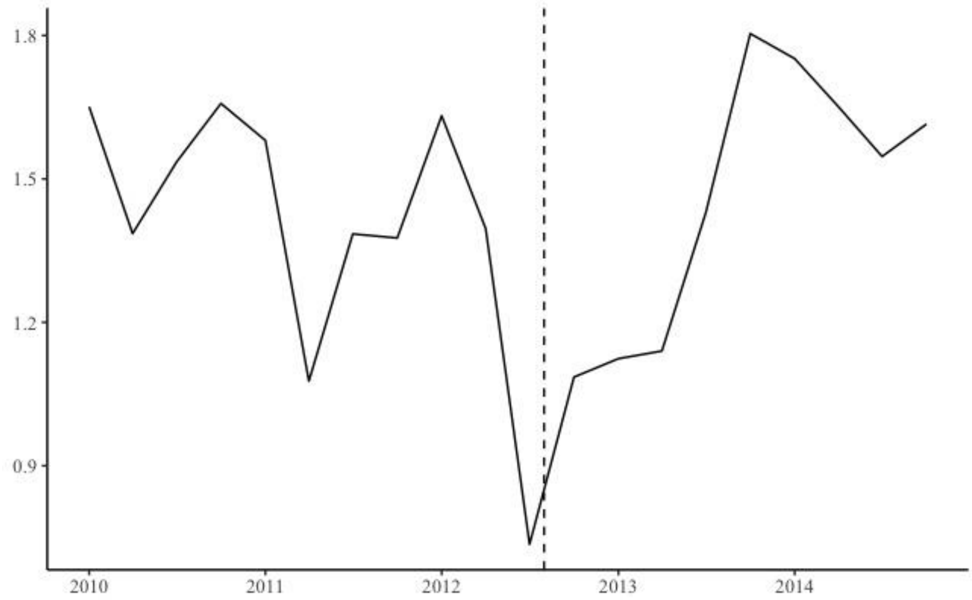
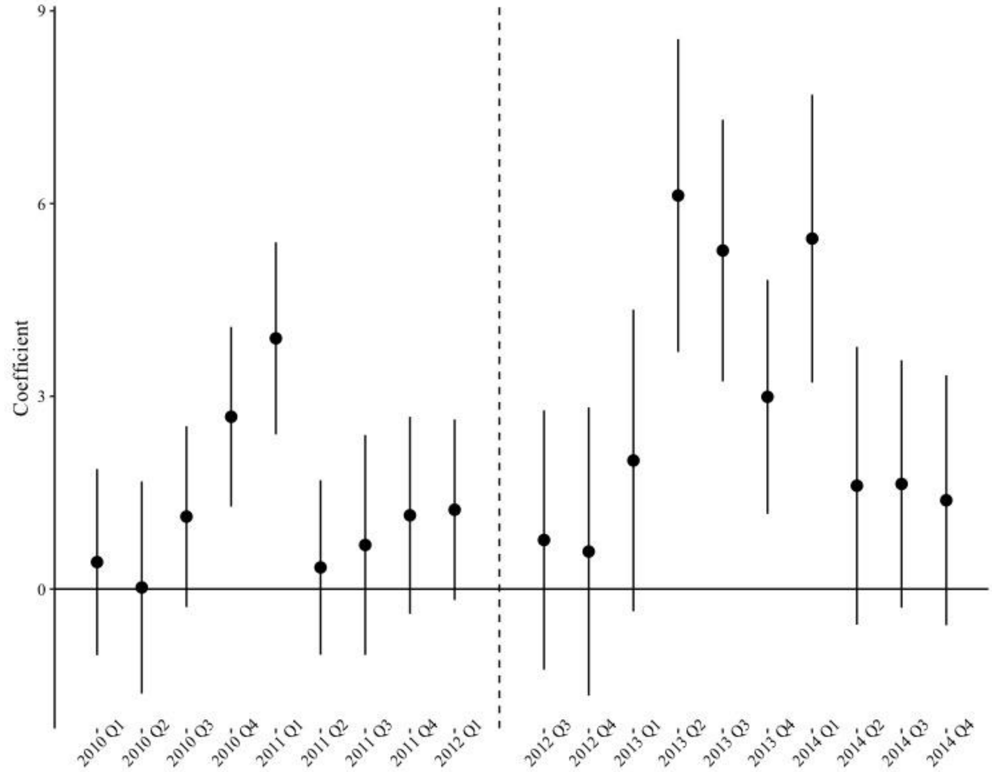
Figures and Tables

Figure 3.1. Abnormal user tweets during earnings announcements



Notes: This figure plots the quarterly average of the variable *AbnUserTweets* during the sample period. *AbnUserTweets* is the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. The dotted line represents the introduction of the clickable cashtag by Twitter Inc. on July 31, 2012

Figure 3.2. Effect of the introduction of the clickable cashtag over time



Notes: This figure plots quarterly point estimates together with 95% confidence intervals (based on the regression model as used in Appendix 3.B, column 3). As the indicator for *AbnUserTweets × 2012q2* is omitted, the second quarter of 2012 serves as benchmark as it corresponds to the last quarter before the introduction of the clickable cashtag by Twitter Inc. on July 31, 2012.

Table 3.1. Descriptive Statistics

Variable	Mean	1th	25th	Median	75th	99th	Std. Dev
Twitter Activity measures							
AbnUserTweets	1.426	-0.859	0.839	1.403	1.983	3.929	0.949
AbnUsersVerified	0.639	-0.625	0.000	0.000	1.099	4.146	1.165
AbnUsersUnverified	1.416	-0.859	0.830	1.403	1.971	3.968	0.949
AbnFirmTweets	0.137	-0.907	0.000	0.000	0.000	2.319	0.520
AbnFirmVerified	0.046	-0.660	0.000	0.000	0.000	1.472	0.315
AbnFirmUnverified	0.090	-0.525	0.000	0.000	0.000	2.239	0.424
Market Measures							
AbnBidAsk	0.068	-0.979	-0.137	0.024	0.283	1.156	0.404
After	0.504	0.000	0.000	1.000	1.000	1.000	0.500
Friday	0.067	0.000	0.000	0.000	0.000	1.000	0.251
EAFrequency	5.499	1.000	3.000	5.000	8.000	10.000	2.872
AbsEarningSurprise	0.173	0.000	0.001	0.003	0.009	10.187	1.128
NegativeES	0.414	0.000	0.000	0.000	1.000	1.000	0.493
AbnTurnover	0.425	-1.273	0.063	0.422	0.790	2.097	0.647
AbnVolatility	0.734	-3.610	-0.270	0.779	1.832	4.265	1.636
AbnReturn	0.053	0.001	0.015	0.034	0.070	0.279	0.059
MarketCap	6.976	3.207	5.686	6.929	8.160	11.406	1.788
BooktoMarket	0.990	-0.313	0.303	0.555	0.911	7.665	7.045
Information envirnonements measures							
Analyst	1.966	0.693	1.386	1.946	2.565	3.466	0.750
AbnNews	0.287	0.000	0.000	0.000	0.000	2.996	0.685
SocialMedia	0.781	0.000	0.000	0.693	1.386	1.792	0.658

The sample consists of 55,650 firm-quarter observations (5,153 distinct firms) with earnings announcements dates between January 1, 2010, and December 31, 2014. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnUserVerified* is calculated as the log of the average daily number of tweets issued by users with verified accounts containing a specific firm's cashtag on day t over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnUserUnverified* is calculated as the log of the average daily number of tweets issued by unverified users accounts containing a specific firm's cashtag on day t over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmTweets* is calculated as the log of the average daily number of tweets issued by a company corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date. *AbnFirmVerified* is calculated as the log of the average daily number of tweets issued by a company with a verified account on Twitter over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date. *AbnFirmUnverified* is calculated as the log of the average daily number of tweets issued by a company with an unverified account on Twitter over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date. *AbnBidAsk* as the log of the average daily bid-ask spread divided by the midpoint price over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcements date. *After* is an indicator variable equal to one for earnings announced after 4pm. *Friday* is an indicator variable equal to one for earnings announced on Fridays. *EAFrequency* is the decile rank of the frequency of earnings announcements per day. Deciles are formed within sample by calendar year. *AbsEarningSurprise* is the absolute value of the consensus earnings forecast error deflated by the price at the end of the prior year's fiscal quarter. *NegativeES* is an indicator variable equal to one for negative earnings surprise. Earning surprise is measured as the consensus earnings forecast error deflated by the price at the end of the prior year's fiscal quarter. *AbnTurnover* is the log of the average daily trading dollar volumes divided by the average market value of shares outstanding over the three days earnings announcements period [−1, +1] divided by the average daily impact over the trailing eight weeks ending two days before the before the earnings announcements date. *AbnVolatility* is the log of the average daily volatility over the three days earnings announcements period [−1, +1] divided by the average daily volatility over the trailing eight weeks ending two days before the before the earnings announcements date. *AbnReturn* is the absolute value of the difference between a firm's cumulative return and the cumulative equal-weighted market return over days [−1, +1]. *MarketCap* is the log of the market value (in thousands) on the fiscal quarter end date. *BooktoMarket* is the Book-To-Market assets ratio on the fiscal quarter end date. *Analyst* is measured as the log of one plus the number analysts following from IBES. News is the daily number of news articles based on Jeon et al. (2021) measure. *AbnNews* is calculated as the log of the average daily number of news articles based on Jeon et al. (2021) measure over days [−1,+1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *SocialMedia* is the log of one plus the number of other social media platforms used by a company.

Table 3.2. Determinants of user-generated content on Twitter

	Dependent variable: AbnUserTweets			
	(1)	(2)	(3)	(4)
AbnFirmTweets	0.037***	0.034***	0.035***	0.027***
	(3.621)	(3.575)	(3.747)	(3.397)
After			-0.058***	-0.047***
			(-4.460)	(-3.933)
Friday			-0.274***	-0.188***
			(-10.844)	(-8.113)
EAFrequency			-0.033***	-0.031***
			(-14.125)	(-15.028)
AbsEarningSurprise				-0.044***
				(-7.245)
NegativeES				-0.046***
				(-5.407)
AbnTurnover				0.388***
				(43.560)
AbnVolatility				0.104***
				(30.755)
AbnReturn				-0.054
				(-0.535)
MarketCap				-0.029***
				(-5.515)
BooktoMarket				-0.004***
				(-2.952)
Analyst				0.016
				(1.339)
AbnNews				0.112***
				(10.478)
SocialMedia				0.027***
				(2.652)
Industry Quarter Fixed effects	No	Yes	Yes	Yes
N	55,650	55,650	55,650	55,650
Adj. R2	0.001	0.144	0.153	0.303

This table provides regressions of user-generated content tweets on a set of determinants variables using Industry×Quarter fixed-effects (Fama-French 49 industry classification) with clustered standard errors at the firm level. The sample consists of 55,192 firm-quarter observations (5,153 distinct firms) with earnings announcements dates between January 1, 2010, and December 31, 2014. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmTweets* is calculated as the log of the average daily number of tweets issued by a company corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date. See appendix 3.A for other variables definition. T-statistics in parenthesis are based on standard errors clustered by firm . *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 3.3. Regression of abnormal bid-ask spreads on Twitter activity

	Dependent variable: AbnBidAsk					
	(1)	(2)	(3)	(4)	(5)	(6)
AbnUserTweets	1.188***	1.890***			1.187***	1.895***
	(5.507)	(8.019)			(5.500)	(8.033)
AbnFirmTweets			0.090	-0.388	0.050	-0.439
			(0.258)	(-1.124)	(0.143)	(-1.273)
F-test for diff. [p-value]					[0.006]	[0.000]
After		1.357***		1.266***		1.354***
		(3.252)		(3.018)		(3.245)
Friday		4.989***		4.606***		4.961***
		(5.473)		(5.037)		(5.440)
EAFrequency		0.613***		0.556***		0.615***
		(7.648)		(6.937)		(7.670)
AbsEarningSurprise		-0.466**		-0.547**		-0.465**
		(-2.212)		(-2.572)		(-2.206)
NegativeES		0.179		0.092		0.179
		(0.484)		(0.249)		(0.485)
AbnTurnover		-9.025***		-8.291***		-9.027***
		(-21.005)		(-19.972)		(-21.007)
AbnVolatility		2.947***		3.142***		2.945***
		(16.829)		(17.989)		(16.818)
AbnReturn		-7.345		-7.419		-7.317
		(-1.315)		(-1.323)		(-1.309)
MarketCap		1.155***		1.109***		1.163***
		(5.823)		(5.560)		(5.861)
BooktoMarket		-0.032		-0.039*		-0.031
		(-1.460)		(-1.675)		(-1.443)
Analyst		0.802**		0.827**		0.797**
		(1.988)		(2.038)		(1.974)
AbnNews		0.626**		0.837***		0.625**
		(2.071)		(2.773)		(2.071)
SocialMedia		0.243		0.349		0.298
		(0.730)		(1.041)		(0.893)
Industry×Quarter fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
N	55,650	55,650	55,650	55,650	55,650	55,650
Adj. R2	0.029	0.050	0.028	0.049	0.029	0.050

This table provides regressions of bid-ask spreads on Twitter activity and a set of determinants variables using Industry×Quarter (Fama-French 49 industry classification) fixed-effects with clustered standard errors at the firm level. The sample consists of 55,650 firm-quarter observations (5,153 distinct firms) with earnings announcements dates between January 1, 2010, and December 31, 2014. *AbnBidAsk* as the log of the average daily bid-ask spread divided by the midpoint price over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcements date, multiplied by 100 to facilitate exposure. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmTweets* is calculated as the log of the average daily number of tweets issued by a company corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date. See appendix 3.A for other variables definition. T-statistics in parenthesis are based on standard errors clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 3.4. User-level variation in authenticity and credibility

	Dependent variable: AbnBidAsk			
	(1)	(2)	(3)	(4)
AbnUserVerified	-0.101	-0.098		-0.077
	(-0.593)	(-0.573)		(-0.451)
AbnUserUnverified	1.922***	1.926***		1.918***
	(8.185)	(8.197)		(8.163)
F-test for diff. [p-value]	[0.000]	[0.000]		[0.000]
AbnFirmTweets		-0.429		
		(-1.245)		
AbnFirmVerified			-1.467**	-1.450**
			(-2.531)	(-2.502)
AbnFirmUnverified			0.190	0.119
			(0.450)	(0.282)
F-test for diff. [p-value]			[0.020]	[0.026]
Controls	Yes	Yes	Yes	Yes
Industry Quarter Fixed effects	Yes	Yes	Yes	Yes
Observations	55,650	55,650	55,650	55,650
Adj. R2	0.050	0.050	0.049	0.050

This table provides regressions of bid-ask spreads on Twitter activity using Industry×Quarter (Fama-French 49 industry classification) fixed-effects with clustered standard errors at the firm level. The sample consists of 55,650 firm-quarter observations (5,153 distinct firms) with earnings announcements dates between January 1, 2010 and December 31, 2014. *AbnBidAsk* as the log of the average daily bid-ask spread over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcements date, multiplied by 100 to facilitate exposure. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnUserVerified* (*AbnUserUnverified*) is calculated as the log of the average daily number of tweets issued by verified (unverified) users accounts containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. *AbnFirmTweets* is calculated as the log of the average daily number of tweets issued by a company corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date. *AbnFirmVerified* (*AbnFirmUnverified*) is calculated as the log of the average daily number of tweets issued by a company verified (Unverified) Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date. Controls are *After*, *Friday*, *EAFrequency*, *AbsEarningSurprise*, *NegativeES*, *AbnTurnover*, *AbnVolatility*, *AbnReturn*, *MarketCap*, *BooktoMarket* , *Analyst*, *AbnNews* and *SocialMedia*. See Appendix 3.A for variables descriptions. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 3.5. Exogenous variation in processing costs

	Dependent variable: AbnBidAsk			
	(1)	(2)	(3)	(4)
AbnUserTweets	1.553***	1.556***		
	(5.885)	(5.898)		
AbnUserTweets × Clickable	1.072**	1.061**		
	(2.376)	(2.350)		
F-test for sum [p-value]	[0.000]	[0.000]		
AbnFirmTweets	-0.444	-0.750	-0.425	
	(-1.289)	(-1.506)	(-1.234)	
AbnFirmTweets × Clickable		0.530		
		(0.876)		
F-test for sum [p-value]		[0.604]		
AbnUserVerified			-0.721***	-0.681***
			(-3.271)	(-3.081)
AbnUserVerified × Clickable			1.092***	1.056***
			(3.702)	(3.546)
F-test for sum [p-value]			[0.104]	[0.102]
AbnUserUnverified			1.668***	1.661***
			(6.359)	(6.327)
AbnUserUnverified × Clickable			0.839*	0.836*
			(1.824)	(1.817)
F-test for sum [p-value]			[0.000]	[0.000]
AbnFirmVerified				-2.059***
				(-2.809)
AbnFirmVerified × Clickable				1.240
				(1.227)
F-test for sum [p-value]				[0.304]
AbnFirmUnverified				0.253
				(0.391)
AbnFirmUnverified × Clickable				-0.217
				(-0.287)
F-test for sum [p-value]				[0.942]
Controls	Yes	Yes	Yes	Yes
Industry×Quarter fixed-effects	Yes	Yes	Yes	Yes
N	55,650	55,650	55,650	55,650
Adj. R2	0.050	0.050	0.050	0.050

This table provides regressions of bid-ask spreads on Twitter activity and a set of determinants variables using Industry×Quarter (Fama-French 49 industry classification) fixed-effects with clustered standard errors at the firm level. The sample consists of 55,650 firm-quarter observations (5,153 distinct firms) with earnings announcements dates between January 1, 2010 and December 31, 2014. *AbnBidAsk* as the log of the average daily bid-ask spread over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcements date, multiplied by 100 to facilitate exposure. *Clickable* is a dummy equal to one after the July 31, 2012, when Twitter Inc. made the cashtag clickable for users and zero otherwise. See Appendix 3.A for variables descriptions. T-statistics in parenthesis are based on standard errors clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 3.6. Viral firms on Twitter

	Dependent variable: AbnBidAsk			
	Full sample	PSM group	ViralFirm=1	ViralFirm=0
	(1)	(2)	(3)	(4)
ViralFirm	3.628***	3.387***		
	(7.194)	(6.132)		
AbnUserTweets			1.795***	1.057**
			(3.471)	(2.237)
AbnFirmTweets	-0.455	0.177	0.140	0.176
	(-1.232)	(0.324)	(0.166)	(0.245)
F-test for diff. [p-value]	[0.000]	[0.000]	[0.103]	[0.315]
Controls	Yes	Yes	Yes	Yes
Industry Quarter Fixed effects	Yes	Yes	Yes	Yes
Observations	55,650	27,826	13,913	13,913
Adjusted R2	0.050	0.048	0.046	0.048

This table provides regressions of bid-ask spreads on Twitter activity and a set of determinants variables using Industry×Quarter (Fama-French 49 industry classification) fixed-effects with clustered standard errors at the firm level. The sample consists of 55,650 firm-quarter observations (5,153 distinct firms) with earnings announcements dates between January 1, 2010, and December 31, 2014. *AbnBidAsk* as the log of the average daily bid-ask spread over the three-day earnings announcements period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcements date, multiplied by 100 to facilitate exposure. *ViralFirm* is an indicator variable equal to 1 for firms with an average AbnUserTweets above the 75% quantile within the sample. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date. See Appendix 3.A for variables descriptions. T-statistics in parenthesis are based on standard errors clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Appendix 3.A. Variables

Name	Description	Source
Twitter Activity measures		
AbnUserTweets	*UserTweets* is the number of tweets and retweets issued by users containing a specific firm's cashtag on day t. *AbnUserTweets* is calculated as the log of the average *UserTweets* over days [−1,+1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date.	Twitter API
AbnUserVerified	*UserVerified* is the number of tweets and retweets issued by verified users accounts containing a specific firm's cashtag on day t. *AbnUserVerified* is calculated as the log of the average *UserVerified* over days [−1,+1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date.	Twitter API
AbnUserUnverified	*UserUnverified* is the number of tweets and retweets issued by unverified users accounts containing a specific firm's cashtag on day t. *AbnUserUnverified* is calculated as the log of the average *UserVerified* over days [−1,+1] divided by the average over the trailing eight weeks ending two days before the earnings announcements date.	Twitter API
AbnFirmTweets	*FirmTweets* is the number of tweets issued by a company corporate Twitter account. *AbnFirmTweets* is calculated as the log of the average *FirmTweets* over days [−1,+1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date.	Twitter API
AbnFirmVerified	*FirmVerified* is the number of tweets issued by a company verified Twitter account. *AbnFirmVerified* is calculated as the log of the average *FirmVerified* over days [−1,+1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcements date.	Twitter API
AbnFirmUnverified	*FirmUnverified* is the number of tweets issued by a company verified Twitter account. *AbnFirmUnverified* is calculated as the log of the average *FirmVerified* over days [−1,+1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcement date.	Twitter API
ViralFirm	*ViralFirm* is an indicator variable equal to 1 for firms with an average *AbnUserTweets* above the 75% quantile within the sample	Twitter API
Clickable	*Clickable* is a dummy equal to one after the July 31, 2012, when Twitter Inc. made the cashtag clickable for users and zero otherwise.	
Market Measures		
AbnBidAsk	*BidAsk* is the daily bid-ask spread divided by the midpoint price. *AbnBidAsk* as the log of the average daily bid-ask spread over the three-day earnings announcement period [−1,+1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcement date.	CRSP
After	Dummy variable equal to one for earnings announced after 4pm until midnight	IBES
Friday	Dummy variable equal to one for earnings announced on Fridays	IBES
EAFrequency	The decile rank of the frequency of earnings announcements per day. Deciles are formed within sample by calendar year	IBES
AbsEarningSurprise	absolute value of the consensus earnings forecast error deflated by the price at the end of the prior year's fiscal quarter.	IBES, CRSP
NegativeES	Dummy variable equal to one for negative earnings surprise. Earning surprise is measured as the consensus earnings forecast error deflated by the price at the end of the prior year's fiscal quarter.	IBES,CRSP
AbnTurnover	*Turnover* is the average of daily trading dollar volumes divided by the average market value of shares outstanding. *AbnTurnover* is the log of the average daily *Turnover* over the three days earnings announcement period [−1,+1] divided by the average daily impact over the trailing eight weeks ending two days before the before the earnings announcement date.	CRSP
AbnVolatility	*AbnVolatility* is the log of the average daily volatility over the three days earnings announcement period [−1,+1] divided by the average daily volatility over the trailing eight weeks ending two days before the before the earnings announcement date.	CRSP
AbnReturn	*AbnReturn* is the absolute value of the difference between a firm's cumulative return and the cumulative equal-weighted market return over days [−1,+1]	CRSP
MarketCap	log of the market value (in thousands) on the fiscal quarter end date	Compustat, CRSP
BooktoMarket	Book-To- Market assets ratio on the fiscal quarter end date.	Compustat, CRSP
Information environments measures		
Analyst	Analyst is measured as the log of one plus the number analysts following from IBES	IBES
AbnNews	*News* is the daily number of news articles based on Jeon et al. (2021) measure. *AbnNews* is calculated as the log of the average *News* over days [−1,+1] divided by the average over the trailing eight weeks ending two days before the earnings announcement date.	Jeon et al. (2021)
SocialMedia	*SocialMedia* is the log of one plus the number of other social media platforms used by a company.	

Appendix 3.B. Effect of the introduction of the clickable cashtag over time

	Dependent variable: AbnBidAsk			
	(1)	(2)	(3)	(4)
AbnUserTweets × 2010q1	-0.351	0.473		
	(-0.456)	(0.619)		
AbnUserTweets × 2010q2	-0.694	0.101		
	(-0.805)	(0.117)		
AbnUserTweets × 2010q3	0.645	1.238*		
	(0.854)	(1.654)		
AbnUserTweets × 2010q4	2.142***	2.675***		
	(2.949)	(3.681)		
AbnUserTweets × 2011q1	3.540***	4.118***		
	(4.362)	(5.117)		
AbnUserTweets × 2011q2	-0.621	0.347	-0.621	0.355
	(-0.869)	(0.487)	(-0.869)	(0.493)
AbnUserTweets × 2011q3	0.434	0.829	0.434	0.956
	(0.471)	(0.893)	(0.471)	(1.025)
AbnUserTweets × 2011q4	1.194	1.907*	1.194	2.009**
	(1.189)	(1.889)	(1.189)	(2.030)
AbnUserTweets × 2012q1	0.606	1.228*	0.606	1.367*
	(0.848)	(1.730)	(0.849)	(1.903)
AbnUserTweets × 2012q3	0.220	0.347	0.220	0.666
	(0.201)	(0.328)	(0.201)	(0.614)
AbnUserTweets × 2012q4	0.349	0.994	0.349	1.277
	(0.286)	(0.821)	(0.286)	(1.037)
AbnUserTweets × 2013q1	1.731	2.037	1.731	2.536**
	(1.370)	(1.612)	(1.370)	(1.965)
AbnUserTweets × 2013q2	6.286***	6.602***	6.286***	7.041***
	(4.754)	(5.136)	(4.756)	(5.384)
AbnUserTweets × 2013q3	4.082***	5.123***		
	(3.721)	(4.676)		
AbnUserTweets × 2013q4	1.425	2.591***		
	(1.490)	(2.718)		
AbnUserTweets × 2014q1	4.755***	5.435***		
	(3.821)	(4.395)		
AbnUserTweets × 2014q2	0.621	1.771		
	(0.543)	(1.567)		
AbnUserTweets × 2014q3	0.311	1.741		
	(0.286)	(1.622)		
AbnUserTweets × 2014q4	0.368	1.778		
	(0.338)	(1.636)		
Controls	No	Yes	No	Yes
Industry×Quarter fixed-effects	Yes	Yes	Yes	Yes
N	55,650	55,650	26,202	26,202
Adj. R2	0.030	0.051	0.030	0.051

This table provides regressions of bid-ask spreads on Twitter activity and a set of determinants variables Industry×Quarter (Fama-French 49 industry classification) fixed-effects with clustered standard errors at the firm level. The sample consists of 55,650 firm-quarter observations (5'153 distinct firms) with earnings announcement dates between January 1, 2010, and December 31, 2014. *AbnBidAsk* as the log of the average daily bid-ask spread divided by the midpoint price over the three-day earnings announcement period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcement date, multiplied by 100 to facilitate exposure. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcement date. *AbnUserTweets* is interacted with dummy variables for each quarter from the sample expect for *2012q2* that serves as a benchmark since Twitter Inc. made the cashtag clickable for users on July 31, 2012. Controls are *AbnFirmTweets*, *After*, *Friday*, *EAFrequency*, *AbsEarningSurprise*, *NegativeES*, *AbnTurnover*, *AbnVolatility*, *AbnReturn*, *MarketCap*, *BooktoMarket*, *Analyst*, *AbnNews* and *SocialMedia*. See Appendix 3.A for variables descriptions.*, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Appendix 3.C. Propensity Score Matching (PSM) estimation

Panel A

Dependent variable: ViralFirm	
AbnFirmTweets	-0.017
	(-0.733)
After	-0.219***
	(-9.656)
Friday	-0.258***
	(-5.261)
EAFrequency	-0.052***
	(-12.939)
AbsEarningSurprise	-0.403***
	(-10.316)
NegativeES	-0.138***
	(-6.211)
AbnTurnover	0.322***
	(14.242)
AbnVolatility	0.180***
	(18.120)
AbnReturn	-3.160***
	(-11.604)
MarketCap	-0.218***
	(-20.371)
BooktoMarket	-0.326***
	(-18.824)
Analyst	0.092***
	(4.076)
AbnNews	0.138***
	(9.442)
SocialMedia	0.185***
	(10.679)
Industry fixed-effects	Yes
N	55,650
Adj. R2	0.142

Panel B

		Mean value		Difference	
Variable		ViralFirm=0	ViralFirm=1	t-stat	p-value
AbnFirmTweets	Unmatched	0.133	0.138	-1.145	0.252
	Matched	0.142	0.138	0.608	0.543
After	Unmatched	0.506	0.501	0.963	0.335
	Matched	0.506	0.501	0.734	0.463
Friday	Unmatched	0.0698	0.0576	5.235	0.000
	Matched	0.0596	0.0576	0.718	0.473
EAFrequency	Unmatched	5.59	5.22	13.360	0.000
	Matched	5.22	5.22	0.029	0.976
AbsEarningSurprise	Unmatched	0.221	0.0301	26.544	0.000
	Matched	0.0432	0.0301	2.829	0.005
NegativeES	Unmatched	0.426	0.375	10.695	0.000
	Matched	0.377	0.375	0.410	0.682
AbnTurnover	Unmatched	0.364	0.612	-42.384	0.000
	Matched	0.591	0.612	-3.029	0.002
AbnVolatility	Unmatched	0.605	1.18	-36.436	0.000
	Matched	1.14	1.18	-2.320	0.020
AbnReturn	Unmatched	0.0490	0.0604	-20.533	0.000
	Matched	0.0595	0.0604	-1.364	0.172
MarketCap	Unmatched	7.08	6.81	16.583	0.000
	Matched	6.78	6.81	-1.926	0.054
BooktoMarket	Unmatched	0.825	0.621	28.349	0.000
	Matched	0.619	0.621	-0.234	0.814
Analyst	Unmatched	1.98	1.98	0.017	0.986
	Matched	1.96	1.98	-2.338	0.019
AbnNews	Unmatched	0.251	0.386	-17.591	0.000
	Matched	0.365	0.386	-2.178	0.029
SocialMedia	Unmatched	0.748	0.896	-22.594	0.000
	Matched	0.886	0.896	-1.171	0.242

This table provides the probit regression of *ViralTweets* on a set of determinants variables using industry (Fama-French 49 industry classification) fixed-effects with clustered standard errors at the firm level. The sample consists of 55, 650 firm-quarter observations (5'153 distinct firms) with earnings announcement dates between January 1, 2010, and December 31, 2014. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcement date. *AbnFirmTweets* is calculated as the log of the average daily number of tweets issued by a company corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcement date. See appendix 3.A for other variables definition. T-statistics in parenthesis are based on standard errors clustered by firm . *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Appendix 3.D. Alternative fixed effects

	Dependent variable: AbnBidAsk			
	(1)	(2)	(3)	(4)
AbnUserTweets	0.877***	0.912***		
	(3.390)	(3.526)		
AbnFirmTweets	-0.040	-0.062		
	(-0.104)	(-0.163)		
F-test for diff. [p-value]	[0.051]	[0.039]		
AbnUserVerified			0.259	0.245
			(1.463)	(1.385)
AbnUserUnverified			0.856***	0.901***
			(3.303)	(3.480)
F-test for diff. [p-value]			[0.069]	[0.045]
AbnFirmVerified			-0.333	-0.457
			(-0.518)	(-0.698)
AbnFirmUnverified			0.184	0.219
			(0.384)	(0.459)
F-test for diff. [p-value]			[0.519]	[0.405]
Controls	Yes	Yes	Yes	Yes
Quarter fixed-effects	Yes	No	Yes	No
Firm fixed-effects	Yes	Yes	Yes	Yes
Industry×Quarter fixed-effects	No	Yes	No	Yes
N	55,650	55,650	55,650	55,650
Adj. R2	0.094	0.096	0.094	0.096

This table provides regressions of bid-ask spreads on Twitter activity and a set of determinants variables using Industry×Quarter fixed-effects (Fama-French 49 industry classification) with clustered standard errors at the firm and quarter level. The sample consists of 55,650 firm-quarter observations (5'153 distinct firms) with earnings announcement dates between January 1, 2010, and December 31, 2014. *AbnBidAsk* as the log of the average daily bid-ask spread over the three-day earnings announcement period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcement date, multiplied by 100 to facilitate exposure. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcement date. *AbnUserVerified* (*AbnUserUnverified*) is calculated as the log of the average daily number of tweets issued by verified (unverified) users accounts containing a specific firm's cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcement date. *AbnFirmTweets* is calculated as the log of the average daily number of tweets issued by a company corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcement date. *AbnFirmVerified* (*AbnFirmUnverified*) is calculated as the log of the average daily number of tweets issued by a company verified (Unverified) Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcement date. Controls are *After, Friday, EAFrequency, AbsEarningSurprise, NegativeES, AbnTurnover, AbnVolatility, AbnReturn, MarketCap, BooktoMarket , Analyst, AbnNews* and *SocialMedia*. See Appendix 3.A for variables descriptions. T-statistics in parenthesis are based on standard errors clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Appendix 3.E. Alternative clustering of standard errors

Panel A: Firm & quarter clustering

	Dependent variable: AbnBidAsk			
	(1)	(2)	(3)	(4)
AbnUserTweets	1.187***	1.895***		
	(3.392)	(5.336)		
AbnFirmTweets	0.050	-0.439	-0.429	
	(0.101)	(-0.982)	(-0.963)	
F-test for diff. [p-value]	[0.041]	[0.000]		
AbnUserVerified			-0.098	-0.077
			(-0.450)	(-0.357)
AbnUserUnverified			1.926***	1.918***
			(5.361)	(5.340)
F-test for diff. [p-value]			[0.000]	[0.000]
AbnFirmVerified				-1.450**
				(-2.205)
AbnFirmUnverified				0.119
				(0.256)
F-test for diff. [p-value]				[0.032]
Controls	Yes	Yes	Yes	Yes
Quarter fixed-effects	Yes	No	Yes	No
Industry fixed-effects	Yes	No	Yes	No
Industry×Quarter fixed-effects	No	Yes	No	Yes
N	55,650	55,650	55,650	55,650
Adj. R2	0.029	0.050	0.050	0.050

Panel B: Industry clustering

	Dependent variable: AbnBidAsk			
	(1)	(2)	(3)	(4)
AbnUserTweets	1.187***	1.895***		
	(4.972)	(5.116)		
AbnFirmTweets	0.050	-0.439	-0.429	
	(0.165)	(-1.412)	(-1.382)	
F-test for diff. [p-value]	[0.003]	[0.000]		
AbnUserVerified			-0.098	-0.077
			(-0.640)	(-0.504)
AbnUserUnverified			1.926***	1.918***
			(5.140)	(5.136)
F-test for diff. [p-value]			[0.000]	[0.000]
AbnFirmVerified				-1.450***
				(-2.748)
AbnFirmUnverified				0.119
				(0.277)
F-test for diff. [p-value]				[0.040]
Controls	Yes	Yes	Yes	Yes
Quarter fixed-effects	Yes	No	Yes	No
Industry fixed-effects	Yes	No	Yes	No
Industry×Quarter fixed-effects	No	Yes	No	Yes
N	55,650	55,650	55,650	55,650
Adj. R2	0.029	0.050	0.050	0.050

(Continued on next page)

Appendix 3.E. Alternative clustering

Panel C: Industry & quarter clustering

	Dependent variable: AbnBidAsk			
	(1)	(2)	(3)	(4)
AbnUserTweets	1.187***	1.895***		
	(3.387)	(4.193)		
AbnFirmTweets	0.050	-0.439	-0.429	
	(0.109)	(-1.053)	(-1.032)	
F-test for diff. [p-value]	[0.021]	[0.000]		
AbnUserVerified			-0.098	-0.077
			(-0.481)	(-0.380)
AbnUserUnverified			1.926***	1.918***
			(4.183)	(4.150)
F-test for diff. [p-value]			[0.001]	[0.001]
AbnFirmVerified				-1.450**
				(-2.357)
AbnFirmUnverified				0.119
				(0.256)
F-test for diff. [p-value]				[0.040]
Controls	Yes	Yes	Yes	Yes
Quarter fixed-effects	Yes	No	Yes	No
Industry fixed-effects	Yes	No	Yes	No
Industry×Quarter fixed-effects	No	Yes	No	Yes
N	55,650	55,650	55,650	55,650
Adj. R2	0.029	0.050	0.050	0.050

This table provides regressions of bid-ask spreads on Twitter activity and a set of determinants variables using Industry×Quarter fixed-effects (Fama-French 49 industry classification) fixed with clustered standard errors at the firm and quarter level. The sample consists of 55,650 firm-quarter observations (5’153 distinct firms) with earnings announcement dates between January 1, 2010, and December 31, 2014. *AbnBidAsk* as the log of the average daily bid-ask spread over the three-day earnings announcement period [−1, +1] divided by the average daily bid-ask spread over the trailing eight weeks ending two days before the before the earnings announcement date, multiplied by 100 to facilitate exposure. *AbnUserTweets* is calculated as the log of the average daily number of tweets issued by users containing a specific firm’s cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcement date. *AbnUserVerified* (*AbnUserUnverified*) is calculated as the log of the average daily number of tweets issued by verified (unverified) users accounts containing a specific firm’s cashtag over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the earnings announcement date. *AbnFirmTweets* is calculated as the log of the average daily number of tweets issued by a company corporate Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcement date. *AbnFirmVerified* (*AbnFirmUnverified*) is calculated as the log of the average daily number of tweets issued by a company verified (Unverified) Twitter account over days [−1, +1] divided by the average over the trailing eight weeks ending two days before the before the earnings announcement date. Controls are *After, Friday, EAFrequency, AbsEarningSurprise, NegativeES, AbnTurnover, AbnVolatility, AbnReturn, MarketCap, BooktoMarket , Analyst, AbnNews* and *SocialMedia*. See Appendix 3.A for variables descriptions. T-statistics in parenthesis are based on standard errors clustered by firm. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.