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Abstract

Disclosure of financial information is essential for the well-functioning of capital markets. However, there exist several market frictions in the supply and demand of disclosure, and the different aspects of the disclosure process, that lead to sub-optimal outcomes. The constantly changing information environment and several recent developments in capital markets highlight the need of understanding the role of financial information and market frictions in the financial information environment in a broader perspective. This dissertation examines three interventions and attempts aimed at resolving market frictions with the goal of improving the financial information environment in different aspects, ultimately benefiting capital markets. Focusing on regulating disclosure to increase transparency, Chapter 2 finds that while introducing disclosure regulation in the nascent crypto token market is perceived to be costly by investors, transparency matters. Next, focusing on enhancing trust in capital markets, Chapter 3 finds, if any, a limited impact of mandating public company audits following the Securities Exchange Act of 1934. Lastly, focusing on the role of new information intermediaries in the information environment and capital markets, Chapter 4 finds that capital markets benefit significantly from the increase in higher quality information available for firms that previously lacked it, after the introduction of

specific financial incentives by a information intermediary that focus on increasing information for these firms. In sum, the three studies that form this dissertation build on the understanding of the constantly changing information environment and the role of regulators, gate-keepers and intermediaries in addressing market frictions.

Contents

1 Introduction

This dissertation examines the role of financial accounting in capital markets. In particular, I examine three interventions and attempts aimed at resolving market frictions with the goal of improving the financial information environment in different aspects, ultimately benefiting capital market efficiency.

Disclosure of financial information is essential for the well-functioning of capital markets. Capital markets are subject to significant agency conflicts between managers and investors due to the separation of ownership and control in public companies, which causes both parties to incur significant agency costs (Jensen & Meckling, 1976). Information asymmetries between managers and investors also cause adverse selection problems. Investors faced with difficulty in discerning companies at the time of investment, are concerned that only bad companies accept their investment terms, causing them to price-insure (Akerlof, 1970). Both problems are costly for companies and investors, and lead to inefficient capital market outcomes or even breakdowns of capital markets. Management can reduce agency and adverse selection costs by disclosing financial information (Glosten & Milgrom, 1985; Milgrom, 1981; Watts & Zimmerman, 1986), which is why disclosure is associated with higher market liquidity (Verrecchia, 2001) and lower cost of capital (Diamond

& Verrecchia, 1991).

Even though the importance of financial information is evident, there exist several market frictions in the supply and demand of disclosure, and the different aspects of the disclosure process, that lead to sub-optimal outcomes. First, companies have incentives to disclose information to reduce the aforementioned agency and adverse selection costs, which suggests that market forces can solve information asymmetry issues (Grossman, 1981; Milgrom, 1981). However, disclosure costs (Verrechia, 1983) and uncertainty about the initial level of information (Dye, 1985) can prevent companies from full disclosure. As a result, regulators and lawmakers, concerned with efficient capital markets, mandate disclosure of financial information to ascertain a minimum level of disclosure (Healy & Palepu, 2001). The economic basis for disclosure mandates, however, is unclear, and documented capital market benefits are inconclusive (Leuz & Wysocki, 2016).

In Chapter 2, which is joint work with Edith Leung, we investigate the role of transparency-enhancing regulation in the nascent crypto token market. It is not clear, ex ante, whether mandated transparency regulation is beneficial for this market. Crypto token characteristics make them in some aspects similar to equity, whereas they are distinctly different from equity in other aspects. Like equity, crypto token value depends on the success of the underlying business, but crypto token holders have no claim on terminating cash flows and have no ownership rights. On the one hand, disclosure regulation is beneficial to investors. Regulators argue that disclosure regulation is necessary to improve the functioning of the crypto token market (SEC, 2018). Crypto token holders have information needs that mandated disclosures could help fulfill, and increased disclosure is associated with positive capital market outcomes (Leuz & Wysocki, 2016). On the other hand, disclosure regulation is not necessary. Crypto token firms potentially voluntarily disclose sufficient information for market participants to assess viability and success (Bourveau, De George, Ellahie, & Macciocchi, 2022; Howell, Niessner, & Yermack, 2020). In addition to the high costs associated with mandated disclosures that can stifle innovation, crypto tokens' distinctness from equity makes the link between increased transparency through mandates and token value less clear.

We examine how market participants perceive regulatory proposals to increase transparency by examining market reactions to regulation news. We find that market participants react negatively to news about transparency regulation, suggesting they perceive it as costly, or burdensome, on average. Cross-sectionally, we find that this reaction is attenuated for crypto token companies that are rated higher on quality and transparency by intermediaries, for those that have higher levels of disclosure, and for those listed on more liquid exchanges. These results provide initial evidence on the current debate on perceived costs and benefits in the cryptocurrency market. Our study shows the value of disclosure regulation in alternative, emerging financial markets.

Second, disclosure is more effective in reducing agency and adverse selection costs when it is credible. Companies have incentives to hire a third-party auditor on behalf of investors, to verify disclosure and strengthen its credibility (Jensen & Meckling, 1976; Watts & Zimmerman, 1983), when credible disclosures do not arise endogenously (Stocken, 2000). These incentives imply that market forces can yield an optimal level of auditing in public companies. However, the externalities associated with the audit product (DeFond & Zhang, 2014) and the difficult value proposition of an audit (DeFond, Lennox, & Zhang, 2016) may lead to sub-optimal levels of auditing. Again, regulators and law-makers impose regulation, but it is unclear whether there is market failure and whether audit regulation is necessary (see e.g., Demsetz, 1969; Kausar, Shroff, & White, 2016; G. J. Stigler, 1971).

In Chapter 3, which is joint work with Thomas Bourveau, Matthias Breuer and Robert Stoumbos, we investigate the public company audit landscape around the introduction of the Securities and Exchange Commission (SEC) in 1934. The new U.S. federal regulator had the authority to set audit standards and oversee the audit profession and mandated the auditing of public companies' financial statements. From prior literature, it is not clear whether an audit mandate improves companies' reliance on audits and investor's trust in companies' reports. Companies have incentives to obtain independent audits themselves, limiting the impact of a mandate (e.g., Ball, 1989; Watts & Zimmerman, 1983). However, due to the public-good characteristics of the product, unregulated markets may insufficiently provide quality audits (e.g., DeFond et al., 2016; Donovan, Frankel, Lee, Martin, & Seo, 2014). It is therefore unclear whether audit market regulation, and especially audit mandates, result in sufficient and high-quality assurance of financial information. Nevertheless, regulators and lawmakers consistently argue for the need for audit mandates.

We examine how the SEC shaped the landscape of public companies by analyzing the annual reports, and the audit statements they contain, of a broad sample of public companies trading on regulated and unregulated stock markets and spanning several decades from the late 1800s till 1940. We document that public companies frequently hired auditors, even absent any mandate, and find that companies favored auditors that exhibit characteristics reflecting independence and competence. We then explore audit services provided to public companies around the SEC's introduction. Focusing on the content of audit statements, we uncover a push for lower expectations regarding the level of assurance provided by auditors, and a trend toward standardization of companies' financial reporting and their auditing services that appears to not be fully attributable to the SEC, but to concurrent events. Finally, we examine public companies' capital market outcomes around the SEC's introduction. We find that companies with audits cater to a different investor clientele (trading-oriented investors) than companies without audits (consumption-oriented investors). In addition, our findings show no real effect of the SEC audit regulations on capital market outcomes for both mandated companies and the market as a whole. Overall, our results do not support the notion that audit market regulation is central to the functioning of public companies' auditing and capital markets, and shows that auditing is not a result of regulation. Even though we do not imply that mandatory public company auditing is meaningless, we show that the SEC mandate had, at best, a limited impact.

Finally, disclosure is costly to process for market participants (Blankespoor, deHaan, & Marinovic, 2020) and the presence of heterogeneously informed investors exacerbates adverse selection problems (Verrecchia, 2001). This fuels the need for information intermediaries to summarize, disseminate, synthesize and analyze, and uncover information (Bradshaw, Ertimur, & O'Brien, 2017), which improves capital market outcomes (see e.g., Barth & Hutton, 2004; Bowen, Chen, & Cheng, 2010). However, their coverage is not equally distributed across companies, or present at those for which the marginal benefit of coverage is greatest, due to firm-specific factors and career incentives. This results in sub-optimal levels of coverage and capital market outcomes (see e.g., Anantharaman & Zhang, 2011; Groysberg, Healy, & Maber, 2011).

In Chapter 4, I investigate the effects of introducing financial incentives to social media analysts. Social media analysts are a valuable alternative to traditional sell-side analysts (see e.g., Bartov, Faurel, & Mohanram, 2018; Chen, Prabuddha, Yu, & Byoung-Hyoun, 2014; Farrell, Clifton Green, Jame, & Markov, 2022; Jame, Johnston, Markov, & Wolfe, 2016) and more explicitly interested in broad coverage due to the business model of the platforms on which they offer their services, namely generating advertising revenue and selling subscriptions. However, even social media analyst coverage appears to be unevenly distributed, which is why the largest online platform offering social media analyses, SeekingAlpha (SA), introduced financial incentives to promote coverage of firms that are undercovered on the platform. In January 2018, SA introduced the 'minimum payment guarantee' (MPG) incentive, which establishes a minimum payment for contributors who cover undercovered firms. It is unclear whether the MPG helps to improve information provision for these firms on SA. Although it is reasonable to expect that financial incentives increase coverage (e.g., Kanfer, 1987), it is less clear whether financial incentives increase high quality, informative coverage. Financial incentives may result in less informative coverage because the incentives can reduce intrinsic motivation – the motivation crowding out effect – that leads to reduced contributions of effective, high quality contributors in favor of those by less effective contributors that are motivated primarily by external, financial incentives (Hui, Li, & Wang, 2021; Khern-am-Nuai, Kannan, & Ghasemkhani, 2018; Toubia & Stephen, 2013). Financial incentives could also increase informative coverage for undercovered firms. Prior literature documents a direct relation between financial incentives and quality output through exerted effort (Gneezy, Meier, & Rey-Biel, 2011; Gneezy & Rustichini, 2000), and financial incentives increase competition for user attention – the competition crowding out effect – that reduces contributions by less effective contributors, in favor of contributions by effective, high quality contributors (Liu & Feng, 2021). In addition, SA subjects all coverage to an editorial process, providing a minimum quality level for contributions.

I examine the impact of the introduced financial incentives using a difference-in-difference analysis, and find that the MPG incentive significantly increased coverage of undercovered firms on SA. I show that effective contributors, i.e., those with more experience and expertise, cover undercovered firms, suggesting no motivation crowding out effect by less effective contributors who are responding more strongly to financial incentives. Instead, my results suggest a competition crowding out effect, as financial incentives increase competition for limited attention of users, which favors effective contributors. Consistent with this reasoning, I document that the incentivized coverage is equally informative to other non-incentivized coverage, increasing the information level available to investors. I then investigate whether this improved information provision by the social media analyst platform improves capital market outcomes. I find that the incentivized coverage on SA is associated with reduced information asymmetries between market participants, and increased price discovery, which indicates improved capital market efficiency.

The financial information environment is constantly evolving, which is why research that aims to broaden our understanding of this environment and helps to inform the debate among practitioners, regulators and academics, is relevant. Several recent developments in capital markets highlight the need of understanding the role of financial information and market frictions in the financial information environment in a broader perspective (Miller & Skinner, 2015). First, there is a surge in non-sophisticated, retail investors with market access, who increasingly rely on alternative information intermediaries for information.[1] Second, there is a significant rise of new alternative asset markets, such as the cryptocurrency market, in which the information needs of investors and the role of disclosure regulation is less understood. Third, there is an exponentially growing amount of alternative information, such as social media and satellite data, available to investors, outside the scope of traditional

[1] See e.g., https://www.ft.com/content/5fb0f315-23b0-4f7f-b909-cbce1954a03f and https://www.ft.com/content/2ca31de6-2a2a-4e8f-8e0c-f5c313ce8fe3.

regulatory frameworks (e.g., Zhu, 2019). Fourth, the scope of disclosure beyond financial information is widening, offering new challenges and opportunities for gatekeepers of financial accounting. Regulators and stakeholders increasingly expect companies to disclose on their operations and the social and environmental challenges they face and how they respond to these challenges. In addition to the recent incidences of corporate scandals, such as the Wirecard scandal that highlighted the shortcomings of auditors, regulatory proposals call for audits on non-financial disclosures as well.[2]

In sum, the three studies that form this dissertation build on the understanding of the constantly changing information environment and the role of regulators, gate-keepers and intermediaries in addressing market frictions that lead to sub-optimal levels of credible and financial information, negatively affecting capital market efficiency. Each chapter focuses on a different aspect of the corporate disclosure of financial information and the market frictions that are associated with this aspect. In Chapter 2, my coauthor and I study the role of disclosure regulation in enhancing transparency in a new asset market. Chapter 3 studies the impact of the first audit market regulations. In Chapter 4, I evaluate the introduction of financial incentives to non-traditional information intermediaries. I use alternative data and settings as a basis for each

[2] See e.g., EU Directive 2014/95/EU (https://eur-lex.europa.eu/legal-content/EN/TXT/PDF/?uri=CELEX:32014L0095&from=EN).

study, to provide new perspectives on long-standing questions, and test existing theories in non-traditional settings. Together, these three studies shed light on addressing market frictions in the information environment, and its implications for stakeholders, firms and capital markets. My results also put into perspective the role of disclosure gatekeepers in light of the recent significant developments in capital markets and financial information.

2 Investor Reactions to Crypto Token Regulation

2.1 Introduction

Cryptocurrency tokens, or (crypto) tokens for short, have become a popular way for start-up companies to raise capital. ‘Utility tokens’, the focus of this study, are a specific type of crypto token that represent a right to use a product or service offered by the issuer. Companies issue these tokens in exchange for capital in a process called an ‘initial coin offering’, or ICO. ICOs overtook early stage venture capital funding for tech companies by threefold in 2017 and 2018 (Olsson, 2018) and by the second quarter of 2018, total ICO volume had risen to 45 percent of total IPO volume (Long, 2018). Although most token issuers usually claim that utility tokens should not be viewed as securities, these tokens are used to raise capital and are subsequently traded post-ICO on various

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crypto exchanges, and their value largely depends on the success of the token issuer. The similarity of utility tokens to traditional securities has therefore sparked debate about how these tokens should be regulated.

Securities regulators also typically regard utility tokens as similar to traditional securities (SEC, 2017), although much ambiguity exists regarding the classification of these assets. As a result, utility tokens are still largely issued and traded outside of regular financial markets and regulatory frameworks, and most regulators have maintained a cautious approach towards regulating crypto tokens. The lack of a clear regulatory framework has led to increasing concern about investor protection in these markets. For instance, in December 2017, SEC Chairman Jay Clayton acknowledged the efficiency-enhancing properties of cryptocurrencies in facilitating capital formation, the primary objective of the SEC (SEC, 2017), and stated it would not actively regulate the ICO market. However, he also issued a warning to investors in this market to be wary of fraudulent crypto token offerings. In April 2019, the SEC's Division of Corporate Finance released guidance for crypto token issuers, although it emphasized that staff-issued guidance should not be seen as legally binding regulation. More recently, the SEC has started to publicize interventions against ICO frauds to raise more awareness of potential scams.[3] The popularity of crypto tokens and the occurrence of

[3] See e.g.: https://www.sec.gov/spotlight/cybersecurity-enforcement-actions.

ICO frauds in this market suggest that some involvement of regulators may be required (Zetsche, Buckley, Warner, & Fohr, 2019).

In this study, we focus on regulatory attempts to enhance the transparency surrounding utility tokens. How investors in crypto tokens weigh the benefits and costs of such efforts is an open question. On the one hand, token holders or investors have several information needs that increased disclosure could help address. For instance, information about the development of the product or service tied to the token is relevant to these holders for assessing the value of the token. In addition, since utility tokens bear a similarity to equity, token investors have a similar demand for information about an issuer's performance and prospects to accurately price tokens. To the extent issuers do not provide these disclosures voluntarily, mandates for increased transparency may help to reduce information asymmetry and adverse selection problems in this emerging market, which investors could perceive as beneficial. For instance, requiring token issuers to register their offerings with securities regulators and comply with mandatory disclosure requirements around and after issuance could increase the amount of price-relevant information available to investors (SEC, 2017).

On the other hand, token investors may not perceive regulation to be necessary. For instance, Bourveau et al. (2022) document that crypto token issuers have incentives to voluntarily disclose information to signal their quality to market participants, reducing the need for disclosure regulation. Several crypto exchanges, such as Coinbase, also voluntarily maintain strict licensing requirements based on a token's intended purpose, the quality of the underlying product technology, team and governance, compliance with applicable law, and market supply and demand for the crypto token (GDAX, 2017). These developments suggest there may be limited incremental benefits to regulation aimed at reducing adverse selection. In addition, it is unclear whether the benefits of (mandatory disclosure) regulation documented for equity markets (Greenstone, Oyer, & Vissing-Jorgensson, 2006; Leuz & Wysocki, 2016; Shleifer & Wolfenzon, 2002) apply in the crypto token setting, as tokens do not represent equity shares. Crypto industry participants have also expressed concerns that the costs associated with increased regulation might impede innovation, e.g., by decreasing incentives for risk-taking or that high compliance costs reduce the availability of funds for innovative developments. Because the need for regulation is unclear and these direct and indirect compliance costs could negatively affect token value, investors could also react negatively to events that increase the likelihood of regulation.

We examine investor reactions to regulatory news in the crypto token market to provide empirical evidence on this issue. To align our analyses with our theoretical arguments above, we focus on news about regulation aimed at increasing transparency by requiring similar disclosures as under traditional securities regulation. We also limit our sampling to news events in countries that have significant crypto token activity, by only including countries with the hundred largest crypto token exchanges in terms of market capitalization. We use an event study approach similar to Zhang (2007), Armstrong, Barth, Jagolinzer, and Riedl (2010) and Joos and Leung (2013) and gather regulation news from Cointelegraph, a leading source of crypto-related news. We identify 15 dates between June 2017 and August 2018 on which regulatory news was announced.

Using token price data from Coinmarketcap, we document that the overall market reaction to news events that increase the likelihood of regulation is negative. This result is robust to a variety of specifications, including different event- and estimation windows, the exclusion of news events that are not picked up in traditional news media, the exclusion of tokens that are not directly affected by a particular regulatory news event at the country-level, or dropping each individual event and country from the sample. The negative reaction suggests that investors may perceive these regulatory proposals aimed at improving transparency through enhanced disclosure as burdensome or costly. If so, we would expect higher quality and transparent token issuers to experience muted negative reactions to these regulatory proposals, as the costs of such regulations are relatively lower. We conduct several cross-sectional tests to verify this intuition. First, we examine whether the market reaction varies with crypto token characteristics and ICO rating from ICOBench to capture token transparency and quality (Bourveau et al., 2022). We indeed find that the negative reactions are attenuated for crypto tokens with higher expert ratings for their information environment, management team, and underlying business proposition. We find a similar muted market reaction for crypto token issuers that engage more with followers on social media: investors react less negatively to increased regulation news for crypto tokens with a higher ICOBench rating for social media activity around the time of the ICO. Because the ICOBench ratings are static (i.e., only represent token quality at the time of the ICO), we conduct two additional tests based on several measures of token disclosure levels, and the characteristics of the exchanges on which tokens are listed. We find that the negative reaction is attenuated for token issuers with more expansive websites and when tokens are traded on exchanges with higher liquidity scores. In sum, our results suggest that token investors perceive regulation to be costly, but less so for higher quality and more transparent tokens, potentially because investors believe these token issuers are better equipped to navigate the burden of regulatory changes.

Our study provides initial evidence on the perceived costs and benefits of regulation in the cryptocurrency market, which is a relevant and current issue in regulatory and cryptocurrency communities. Although most jurisdictions have maintained a largely hands-off approach to the cryptocurrency market, there is increasing pressure on regulators to clarify their position on cryptocurrencies and to meet regulatory demands with an actionable approach. For instance, in September 2018, over a dozen US Congress members asked the SEC to provide more guidance on how it determines whether cryptocurrencies are investment assets (i.e., subject to SEC regulation) or commodities. In response, in April 2019 the SEC's Division of Corporation Finance published a framework to help crypto issuers assess whether their tokens constitute a securities offering, but cautioned that the framework should not be viewed as an official regulation or statement by the SEC (SEC, 2019). At a global level, the G20 also continues to mention that cryptocurrencies do not pose an immediate risk to financial stability but has stated it remains vigilant (Canepa, 2018). We stress that our results do not imply that regulation is unnecessary or enhanced transparency does not matter. In fact, the result that token issuers do engage in voluntary disclosures suggests that issuers expect increased transparency to have some benefits. However, the negative market reactions could suggest that investors perceive current regulatory proposals to be costly. Several jurisdictions such as Hong Kong, Singapore, and the UK have initiated regulatory sandboxes for cryptocurrency trading or for FinTech start-ups to test new products and services in restricted settings without having to comply with strict regulatory frameworks that might stifle innovation, which may be an alternative to applying existing regulation to this type of digital assets. (Kharpal, 2018).[4]

In a related article, Auer and Claessens (2018) assess the market reactions of the largest cryptocurrencies such as bitcoin to regulatory events. Their results are relevant in providing preliminary evidence that crypto markets exhibit a degree of market efficiency by predictably reacting to regulatory news. Our article still differs in several dimensions. First, Auer and Claessens (2018) examine bitcoin and similar cryptocurrencies, whereas we examine crypto utility tokens. The fundamental value of currency-like cryptocurrencies is unclear, or argued to be zero (Cheah & Fry, 2015), whereas the value of utility tokens is arguably linked to the underlying value of the token issuer. Hence, we can make a clearer prediction as to why transparency regulation news affects crypto token prices, while it is unclear whether transparency matters in the crypto*currency* setting. Second, although we document the overall market reaction of crypto tokens to regulatory news, we focus on whether crypto token characteristics affect investor reactions in a predictable manner to provide more detailed evidence on the perceived benefits and costs of transparency in the token setting.

Our study also adds to the literature on the role of information intermediaries and voluntary disclosure in emerging financial markets such

[4] See e.g.: `https://www.fca.org.uk/firms/regulatory-sandbox`.

as the crypto setting (Barton & Waymire, 2004; Bushee & Leuz, 2005; Leuz & Wysocki, 2016). Our cross-sectional analyses suggest that in the absence of a clear regulatory framework aimed at improving transparency, crypto token experts act as intermediaries: their quality ratings of crypto token characteristics can provide information that investors find relevant as evidenced by their mitigated reactions to regulatory news for higher rated crypto tokens. Investors also seem to value the extent of disclosures both during and after an ICO, suggesting an important role for voluntary disclosure in this nascent market. Our results complement those of Bourveau et al. (2022), who find that crypto token issuers with a better disclosure and information environment have a higher likelihood of successfully completing the offering, and have a lower subsequent crash risk, illiquidity, and volatility in secondary markets. Howell et al. (2020) also find that post-ICO success is related to disclosure and other quality signals. We provide additional evidence to support the conclusion that investors indeed value voluntary disclosure by documenting that the negative reactions to regulation are attenuated by transparency in the predicted direction.

2.2 Setting and Predictions

2.2.1 Crypto Token Markets

Crypto tokens are crypto assets that can act as an investment instrument but also also as a medium of exchange. Crypto tokens run on an existing blockchain, while cryptocurrencies, such as Bitcoin or Ethereum, have their own blockchain. There are two types of crypto tokens: 'utility tokens', which represent the right to use a product, service or protocol at the company that issued the tokens, and 'security tokens', which represent ownership rights and a claim on future cash flows. Utility tokens are issued through a process called an 'Initial Coin Offering' (ICO), which is similar to an IPO. A company releases a whitepaper with details of the ICO and investors can transfer other cryptocurrencies or fiat currency to the company to receive issued tokens on the day of token distribution. Security tokens are issued through a process called a 'tokenized IPO'. In most countries, tokenized IPOs are regulated under the traditional securities regulation framework.[5] In this study, we therefore only consider 'utility tokens', which are largely unregulated.

After distribution, crypto tokens are publicly tradable on cryptocur-

[5] E.g., in the EU under Regulation (EU) 2017/1129 (https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=CELEX:32017R1129), and in the U.S. under the Securities Acts of 1933 and 1934 (https://www.investor.gov/introduction-investing/investing-basics/role-sec/laws-govern-securities-industry).

rency exchanges (Chod & Lyandres, 2022) and can be exchanged for other cryptocurrencies, crypto tokens, or fiat currencies. Crypto exchanges exist in many countries and a crypto token does not have to be domestic. Not all crypto tokens are listed on all exchanges, but when a token is listed on multiple exchanges, prices can vary by exchange.[6] Once tokens are distributed after issuance, or a new coin is founded, exchanges decide (sometimes following the request of the company or founders of a cryptocurrency) whether to list the crypto token, a process that varies among exchanges.[7] For instance, Coinbase, one of the biggest cryptocurrency exchanges, decides on the (de-)listing of cryptocurrencies using a strict framework (GDAX, 2017). In contrast, BitForex requires much less information and is less transparent about its specific requirements upfront (BitForex, 2019).[8]

Our empirical tests rely on the assumption that investors rationally weigh the costs and benefits of crypto market regulation and incorporate this assessment into token prices. However, a common concern about the cryptocurrency market is that it is speculative and dominated by retail investors, calling into question the validity of this assumption. Due to the

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[6] For our study, we use the price that is provided by Coinmarketcap, which is the volume weighted average of all market pair prices reported for the cryptoasset on all exchanges: `https://support.coinmarketcap.com/hc/en-us/articles/360015968632-How-are-prices-calculated-on-CoinMarketCap-`.

[7] See: `https://www.bitforex.com/en/tokenListing/introduce` and `https://support.bitforex.com/hc/en-us/articles/360015527192`.

[8] Our data also suggest that Coinbase is indeed more selective in listing cryptocurrencies. Coinmarketcap reports 20 cryptocurrencies listed on Coinbase and 100 listed on BitForex.

anonymous nature of crypto assets and the lack of regulation requiring disclosure of holdings, it is impossible to get a full overview of investors in crypto tokens. However, anecdotal and survey evidence suggests the presence of sophisticated investors in this market (PwC, 2019) and that 22 percent of institutional investors already have exposure to crypto assets (Fidelity, 2019). Although some studies document speculative, bubble-like periods for Bitcoin and other currency-like cryptocurrencies (e.g., Cheah & Fry, 2015; Cheung, Roca, & Su, 2015; Corbet, Lucey, & Yarovaya, 2018), other articles also suggest a degree of efficiency in these markets. Bhambhwani, Delikouras, and Korniotis (2019) document that fundamental characteristics of cryptocurrencies significantly explain variation in their prices and Pieters and Vivanco (2017) find that variation in exchange-level regulations predictably affect the prices of cryptocurrencies. Auer and Claessens (2018) investigate intraday price movements of Bitcoin in response to regulation news and find that the price of Bitcoin quickly and efficiently reacts positively (negatively) to the release of favorable (unfavorable) regulation news. Hence, we assume that token markets exhibit some degree of efficiency and investors are able to rationally react to the implications of potential regulation.

2.2.2 Regulatory Landscape

ICOs and utility token issuers emerged in a largely unregulated landscape, in which they do not have to comply with the strict registration and disclosure requirements for regular securities offerings (Bourveau et al., 2022; Global Legal Research Center, 2018). This exemption is mostly due to the ambiguity surrounding the classification of utility tokens, making it unclear which regulatory framework applies. As described in the previous section, issuers often argue that their tokens represent a service, good, or obligation for their company rather than an ownership claim, and therefore should not be viewed as securities. However, the value of utility tokens often depends on the performance of the token issuer, as demand for a token increases with the success of the issuing company (Conley, 2017). Therefore, securities regulators such as the U.S. SEC argue that despite issuers' claims, many utility tokens should be treated as securities that are subject to the securities regulation and that offerings should be 'accompanied by the important disclosures, processes and other investor protections that our securities laws require' (SEC, 2017). Despite the SEC's view and warnings, the regulator has been reluctant to enforce or mandate registration or increased disclosure. Rather, the SEC has taken a case-by-case approach and initially only acted against egregious cases of misrepresentation.[9] Although more recently, the SEC has increased its scrutiny of ICO and token issuers, it has yet to uniformly require crypto platforms to register or hold all ICOs to a similar disclosure standard as regular securities offerings. This trend is also observed in many other countries (Global Legal Research Center, 2018). ICOs and token issuers remain largely unregulated, leaving investors to rely on voluntary disclosures and information intermediaries to reduce information asymmetries in this market (Bourveau et al., 2022).

2.2.3 Predictions

We focus on regulatory proposals that address concerns about the lack of transparency of utility tokens and issuers, which inhibits investors' ability to adequately assess the fundamental value of a cryptocurrency (Zetsche et al., 2019). Utility token holders or investors likely have two types of information needs. First, these token holders are interested in product- or service-specific information, as a utility token can be viewed as a prepayment for access to an issuer's product or service that is often still under development. Some of this information is provided in the white paper at the time of an ICO, but post-ICO product development updates are useful to these investors for assessing the likelihood of redeeming and

[9] For our sample period of 2017–2018, the SEC undertook 22 ICO-related enforcement actions (see `https://www.sec.gov/spotlight/cybersecurity-enforcement-actions`) while ICOBench reported 415 U.S. ICOs in this period (see `https://icobench.com/icos`).

value of the token. Second, utility token investors are likely generally interested in going-concern-related aspects of the issuer itself, such as their financial prospects and managerial competence. Unlike a regular product or service, utility tokens are traded post-issuance where their value depends largely on the success and potential of the underlying business. Both information needs are likely (partly) met through enhanced disclosures as required by traditional securities regulation, such as periodic disclosure of financial and other material value-relevant information. Hence, even though utility tokens do not represent an ownership stake in the issuer, their information needs overlap with those of regular capital market participants.

This view is echoed in securities regulators' calls for utility token issuers to comply with usual registration and disclosure requirements for securities offerings, which would increase the amount of information available to investors and allow for a more informed investment decision (SEC, 2018). In particular, securities regulators have called for increased transparency of the token issuer during the ICO *and* in subsequent periodic disclosures. More stringent ICO disclosure requirements likely benefit investors by reducing adverse selection between a potential token investor and the firm during the initial offering, while periodic disclosures reduce information asymmetries between investors in subsequent trading. Although much of the public debate surrounds disclosure during an

ICO, news about enhanced securities regulation proposals typically also include increased requirements for transparency post-ICO. For instance, the SEC's 2017 DAO report clarifies that 'DAO tokens are securities under the Securities Act of 1933 ("Securities Act") and the Securities Exchange Act of 1934 ("Exchange Act")'. While the former applies to disclosure requirements around the initial offering, the latter regulates periodic subsequent disclosures.[10] We emphasize this point, since we examine investor reactions to issued tokens. Any observed market reaction to securities regulation news events are therefore more likely related to calls for increased transparency *post*-ICO, which go hand-in-hand with disclosure requirements around the time of the ICO.

Whether token investors react positively or negatively to such transparency-increasing regulations is an open question. Empirical evidence from traditional capital market settings suggests that increased disclosure is associated with positive capital market effects such as higher liquidity and a lower cost of capital (see e.g., Leuz & Wysocki, 2016, for an overview, although they also note many issues with this literature that prevent one from drawing unambiguous conclusions). Following this line of thought, we would expect to observe a positive market reaction to news events that increase the perceived likelihood of transparency regulation, due to reduced risk of trading in such tokens. However, it is not clear ex ante that these potential benefits will materialize. First, token issuers

[10] See: `https://www.sec.gov/litigation/investreport/34-81207.pdf`.

may have incentives to voluntarily take measures to protect investors, reducing the need for regulation (Leuz & Wysocki, 2016). Bourveau et al. (2022) find that in the absence of regulation in the ICO setting, issuers with a better disclosure and information environment have a higher likelihood of successfully completing the offering, and have a lower crash risk, illiquidity, and volatility in secondary markets. Howell et al. (2020) also find that post-ICO success is related to disclosure, credible commitment to the project, and other quality signals. These studies suggest that token issuers have incentives to voluntarily disclose information and credibly signal their quality to market participants. Similarly, Barton and Waymire (2004) find that in the pre-securities regulation era, financial reporting quality was higher for firms whose managers had incentives to supply higher quality disclosures, and that such firms experienced a smaller stock price decline during the 1929 stock market crash.

Second, although studies find benefits to voluntary disclosure, it is unclear whether mandatory disclosure yields similar outcomes. Although prior work in the context of equity markets suggests some benefits to disclosure regulation (Leuz & Wysocki, 2016), these results may not directly translate to the crypto token market. Increased transparency may reduce uncertainty about firm value or the discounted liquidating cash flow on which equity holders have a claim, which in turn lowers the discount rate of the liquidating cash flow and increases firm's stock price (see e.g., Lambert, Leuz, & Verrecchia, 2007; Verrecchia, 2001). In addition to reducing uncertainty, disclosure regulation affects firm value by influencing managers' decisions and the distribution of future cash flows (Greenstone et al., 2006), or by reducing the cash flows that managers can appropriate (Shleifer & Wolfenzon, 2002). However, as explained earlier, since utility tokens do not grant voting or cash flow rights to the holder, the link between increased transparency and token value is less clear. The lack of voting rights also impairs crypto token investors' ability to directly discipline or replace management, or to motivate management to act in the interest of crypto token holders. Hence, securities regulation aimed at enhancing the transparency of tokens may be less beneficial given the lack of redress for utility token issuers beyond selling the token. In sum, due to the differences between traditional equity securities and utility tokens, we cannot assume that the identified benefits of mandated disclosure in equity markets also hold in the crypto token market.

Third, even if mandating disclosure results in the benefits discussed above, it is unclear whether they outweigh the costs of regulation. Although investors do not directly bear these compliance costs, token issuers and other crypto industry participants commonly raise the concern that disclosure regulations divert issuers' resources away from product development and innovation towards regulatory compliance (Rooney, 2018). For example, in New York State, companies operating with cryptocurrency are required to obtain a BitLicense, which also includes providing detailed financial data about their operations.[11] Companies that have attempted to apply for these licenses have reported costs between US$50,000 and US$100,000, and initially, few companies were able to successfully obtain a BitLicense (Perez, 2015; Wieczner, 2018). As innovative activities are likely vital to token issuers' longer-term success and growth, especially since these issuers are often developing or start-up companies, the lack of sufficient funds will negatively affect the value of these issuers. Given the direct and indirect costs that regulation imposes and the lack of clear arguments for the benefits of regulation in the crypto token setting, investors may not react, or react negatively to events that increase the likelihood of regulation.

Note that to observe a market reaction, these news events should affect current demand for crypto tokens. We believe this assumption is plausible. First, our events only include news events that clarify regulators' stance on crypto tokens (e.g., the SEC's statement that DAO tokens should be considered as securities subject to securities laws on July 25, 2017), which could have immediate regulatory implications and therefore may also affect current demand for crypto tokens. Second, with respect to news events relating to future transparency regulation,

[11] See: https://www.dfs.ny.gov/system/files/documents/2021/04/financial_statement.pdf.

prior studies find market reactions to news about regulation that has not yet been implemented (e.g., Armstrong et al., 2010; Joos & Leung, 2013; Zhang, 2007) whereas Daske, Hail, Leuz, and Verdi (2008) find that markets anticipate the potential cost of capital and equity valuation effects of mandatory IFRS adoption. Auer and Claessens (2018) also find that prices of cryptocurrencies such as bitcoin, respond predictably to regulatory news. For tokens, news that relates to future regulation may affect investors' perceptions of token issuers' incentives and their ability to innovate. If investors believe that regulation hampers innovation and therefore the value of the product or service underlying the token, this belief should also be reflected in current demand for tokens and lead to a change in current prices.

Finally, we stress that an overall positive or negative market reaction to regulatory events should not be interpreted as support for or against increased transparency mandates. Rather, our interest is to gain insight into whether token investors' reactions to regulatory proposals are in line with theoretical predictions on the potential costs and benefits of increased transparency in the crypto token setting. Such evidence allows us to better understand the role of transparency in crypto markets and whether token investors incorporate the potential value implications of these regulatory proposals in their trading decisions. It also allows us to gauge the extent of transparency among tokens absent regulation, which sheds some light on the potential effects of mandates. We are therefore mainly interested in the cross-sectional analyses relating variation in token transparency and quality to token investors' reaction to regulatory news events.

2.3 Methodology & Data

To gauge how investors perceive regulation, we conduct an event study around the dates of news that relates to the likelihood of transparency regulation in the cryptocurrency market, following e.g., Zhang (2007), Armstrong et al. (2010) and Joos and Leung (2013). We explain our research design in the following sections.

2.3.1 News Events Coding

We compile our sample of transparency regulation news events by reviewing all regulatory news articles related to cryptocurrencies from Cointelegraph, one of the biggest cryptocurrency news-platforms, between August 8, 2013 and September 1, 2018.[12] Auer and Claessens (2018) use news from Reuters, but our informal discussions with blockchain-practitioners reveal that cryptocurrency market participants primarily use more industry-specific news sources, i.e., cryptocurrency and blockchain-oriented news platforms.

[12] August 8, 2013 is the date of the first article on CoinTelegraph.

Our search of Cointelegraph yields 1,009 potential regulatory news articles, i.e., the articles Cointelegraph tags as regulatory news. First, each author independently coded a test sample of 100 articles to agree on whether a news article represents a change in the likelihood of regulation. We focus on news that relates to a concrete action that leads to an increased likelihood of regulation, or actions/statements by regulators that clarify whether cryptocurrencies are subject to a certain existing rule. After agreeing on a coding scheme, each author again separately coded each news item and compared the coding after completion. Disagreements in coding were then resolved through discussion among the author team. Table 2.1 provides an overview of our event selection procedure. We first exclude articles that are unrelated to regulation ('non-regulation news items') or without news value ('non-news items'), e.g., summary and clarification articles, background stories, and analyses. Next, we exclude news articles about (regulators in) countries without a sizable cryptocurrency market, which we define as countries that do not have a top 100 cryptocurrency exchange in terms of market capitalization and trading volume (see Table 2.A2 in the Appendix for an overview). We then exclude news articles about regulations that are not aimed at increasing transparency for cryptocurrencies. Lastly, given the focus of our arguments on the effects of transparency about a token's prospect, we exclude news articles that are not about regulation in the context of securities regulation, e.g., regulation aimed at increasing transparency of trades on exchanges. We also take care to verify the date of news articles by checking the original sources referenced in Cointelegraph, where available. After cross-checking these news items manually with sources on LexisNexis, we find that 12 of our 15 articles are also mentioned by traditional, international news outlets. Our final sample comprises 15 news items that correspond to 15 unique dates (i.e., news events). We discuss each of these events as well as checks for potential confounding events in more detail in the Appendix.

2.3.2 Crypto Token Return Data

We gather crypto token market data from Coinmarketcap. This website has data on open, close, high and low prices, trading volume, and market capitalization, for a total of $1,886$ coins and tokens starting April 28, 2013. When a cryptocurrency is listed on multiple exchanges, Coinmarketcap presents cryptocurrency prices as the volume-weighted average of all cryptocurrency exchange prices. Coinmarketcap sums the volume across all exchanges as the total trading volume. We exclude crypto tokens that have a close-price lower than 0.001 (one tenth of a cent), because their return series is affected by rounding errors on Coinmarketcap.

Table 2.1: Event and Sample Selection

Panel A: Event Selection	**N**
All items	1,009
− Non-news items	−589
− Non-regulation news items	−204
= All regulation news items	216
− Regulation news items from countries without a top 100 crypto-exchange	−54
− Non-transparency regulation news items	−128
− Non-securities regulation news items	−19
= Transparency regulation news events	15

Panel B: Sample Selection	**Tokens**	**N**
All cryptocurrencies listed on Coinmarketcap, Apr. 28, 2013 – Sep. 1, 2018	1,886	849,152
− No market data on any event date (keep only event days)	−100	−831,827
− No market cap., market cap. $<$ US$10,000, and/or price $<$ US$0.001	−393	−4,028
− Extreme *CAR* due to data error	0	−2
− No ICOBench data, not a utility token	−938	−9,594
= 'Market Reaction' sample (Table 2.2)	455	3,701
− No *Expert Rating* data	−140	−1,283
= 'Role of Rating' sample (Tables 2.5 and 2.6)	315	2,418
'Role of Rating' sample	315	2,418
− No disclosure data (any of the variables missing)	−63	−671
= 'Role of Disclosure' sample (Table 2.7)	252	1,747
'Role of Rating' sample	315	2,418
− No exchange data (any of the variables missing)	−86	−647
= 'Role of Exchanges' sample (Table 2.8)	229	1,771

This Table presents an overview of the events and sample selection procedures. Panel A reconciles the number of all regulatory news items identified on Cointelegraph with those included in our event study. We exclude news items that do not represent news, are unrelated to securities regulation focused on enhancing transparency, or are news items from countries that are minor players in the cryptocurrency market (based on the existence of a large cryptocurrency exchange). Panel B shows how the four different test samples reconcile with all listed cryptocurrencies on Coinmarketcap. We present the sample selection in number of unique crypto tokens ('Tokens') and crypto token trading date observations ('N').

We also exclude tokens that have missing market capitalization data or a market capitalization lower than US$10,000, because data for these tokens are relatively difficult to verify by Coinmarketcap due to their 'exotic' nature (Kakushadze, 2018).

To ensure the accuracy of the price data, we investigate the returns time-series of any tokens for which the return on a given day is in the 1st or 99th percentile of all tokens in the cross-sectional sample. Within these percentiles, we first check the most extreme tails of our return distributions, i.e., returns exceeding $10,000$ (99) percent daily increases (decreases). Although these return thresholds may seem extreme compared to other asset markets, they are more common in the more volatile cryptocurrency market, especially from 2015 to early 2018. We find that most of the extreme positive returns are due to Coinmarketcap providing an incorrect closing price. We resolve these errors by replacing the closing price with the opening price of the subsequent day when the return is in the 99th percentile. Next, to gain more confidence in the data of the remaining extreme negative and positive returns, we cross-check our Coinmarketcap data with price data from CoinGecko, another major provider of cryptocurrency prices. Except for two observations, the remaining extreme returns appear to be correct and we delete these two observations. Finally, we manually adjust the price series of Xaurum to account for an $8,000$-for-1 split on August 22, 2016 for which Coinmarketcap did not account.

Consequently, our final sample for the market reaction tests consists of 455 unique crypto tokens. Panel B of Table 2.1 presents an overview of the crypto token sample selection, as well as the final samples for the other tests.

2.3.3 Event Study Design

Similar to e.g., Zhang (2007), Armstrong et al. (2010) and Joos and Leung (2013), we conduct an event study around the dates of regulation news. We use a two-day event-window, defined as $t \in [0;+1]$, where $t = 0$ is the event date or the date of the regulation news.[13] We define the market reaction to regulation news as CAR_i , the two-day cumulative abnormal return of a crypto token i over the event-window as:

$$\begin{aligned} CAR_i &= \sum_{t\,=\,0}^{+1} = AR_{i,t} \\ &= \sum_{t\,=\,0}^{+1} R_{i,t} - E[R]_{i,t}, \end{aligned} \tag{1}$$

where $AR_{i,t}$ is defined as the abnormal return of the crypto token i at time t, $t \in [0;+1]$, where $t = 0$ is the event date. As such, abnormal return is the difference between the observed daily return $R_{i,t}$ and the expected daily return $E[R]_{i,t}$, defined as the mean daily return in an 80-day non-event estimation window centered on the news event.[14] Because

[13] Our inferences are unchanged if we use the following event-windows: $[0;+2]$, $[0;+3]$, $[-1;+1]$, $[-2;+2]$ and $[-3;+3]$ (results untabulated).
[14] Our inferences are unchanged if we use a 40, 120, 160 or 200-day estimation window, and if the estimation window ends before the event window (results untabulated).

regulation news affects the cryptocurrency market as a whole, we do not use a market model to calculate abnormal returns. Additionally, because the occurrence of other regulatory events in the estimation window likely affects the expected return, we exclude these specific event dates from the estimation window. Dropping these dates means that whereas the estimation period is always 80 trading days, it does not always span 80 consecutive calendar days around the event. However, our results for the mean market reaction tests are statistically similar when these event dates are not removed from the estimation window.

Market reactions to regulation news events are correlated in the cross-section, which violates the independence assumption of the test statistics and may overestimate the significance of abnormal returns if we run our analyses at the crypto token level (Brown & Warner, 1980, 1985). We therefore cluster the standard errors at the event- and crypto token-level.

2.4 Results

We first document the overall market reaction to all transparency regulation news events. We then examine whether these reactions vary across the quality and transparency of crypto tokens.

2.4.1 Mean Market Reactions

Table 2.2 shows the cumulative abnormal return for each transparency regulation news events and across all events. We find a significantly negative average cumulative abnormal return CAR around 12 of the 15 transparency regulation news events for all crypto tokens in our sample. Across all events, the mean abnormal market reaction is also significantly negative, namely -5.20 percent (t-statistic $= -2.56$). Overall, our results suggest that on average investors perceive increased regulation to be costly.

We conduct several tests to ensure that the cumulative abnormal returns capture market reactions to transparency regulation news. First, as our events often concern country-specific regulatory news, we repeat the tests using only crypto tokens traded on an exchange in the specific country of a regulation news item. We use crypto token listing data from Coinmarketcap and manually collect the country of incorporation/registration for all crypto exchanges mentioned on Coinmarketcap. Overall, the results in the 'Exchange-Country Sample' columns in Panel A of Table 2.1 of the Online Appendix are similar to our main tests, and the mean reaction across events is stronger at -10.30 percent (t-statistic $= -4.39$). Second, we limit our news event sample to only include news articles that are also covered by traditional media. We search LexisNexis for mentions of the news articles reported by Cointelegraph and verify that 12 (out of 15) events are also reported by traditional, international news outlets (e.g., Bloomberg, Reuters).[15]The 'Traditional Media Covered Sample' columns in Panel A of Table 2.1 of the Online Appendix reports an average cumulative abnormal return of −5.88 percent (t-statistic = −2.79) for the reduced sample of news items with traditional media mentions, which is similar to our main result. Third, to limit the possibility that the event window returns are driven by factors unrelated to regulation news, we conduct a placebo test in which we conducts 100 draws of 15 non-event dates between January 1st, 2017 and September 1st, 2018.

We find that the mean of the distribution of cumulative abnormal returns for these placebo events is 0.007 with a standard error of 0.0023, which is significantly different and in the opposite direction of the market returns on regulation news dates.[16] Fourth, instead of univariate tests of the mean market reaction on event dates, we also regress daily token returns for the entire sample period on an event dummy equal to one if the date corresponds to a transparency regulation event, and zero otherwise, including token fixed effects and clustering standard errors

[15] The following events (numbers) are not covered by traditional, international news outlets: "Canada Looking To Classify Digital Currencies As Securities" (number 4), "US: Republican, Democrat Officials Calling For Crypto Regulation In Rare Show Of Unity" (number 8), "ICOs Can 'Prove Their Legitimacy' Under New Crowdfunding Rules, Says EU Lawmaker" (number 15).

[16] When we restrict the non-event dates period to match our event period even more closely, i.e., June 1st, 2017 till September 1st 2018, we find an average abnormal market reaction of 0.005 with an standard error of 0.002.

at the token level. We present these results in Panel B of Table 2.1 of the Online Appendix and find a significantly negative coefficient for the event indicator of -0.025 (t-statistic $= -15.02$), and market reactions to the individual events consistent with the results in Table 2.2. Fifth, to ensure our results are not driven by any individual country, token or event, we repeat these analyses and exclude each country, token or event consecutively in each estimation. These results are statistically and economically similar to those documented in Table 2.2 (untabulated).

Table 2.2: Overview of News Events and Market Reactions

Event	Date	Event Headline	Country	Market Reaction: # Tokens	Market Reaction: *CAR*	Market Reaction: *T*-stat.
1	Jun. 13, 2017	*"SEC is Still Eyeing to Regulate the ICO Market"*	US	40	−0.074***	−3.80
2	Jul. 25, 2017	*"SEC Deals Blow To ICOs: DAO Tokens Are Securities, Subject to Securities Laws"*	US	71	−0.154***	−13.00
3	Aug. 1, 2017	*"Singapore Clarifies ICO Token Regulation, Follows US"*	Singapore	73	0.041***	2.52
4	Aug. 24, 2017	*"Canada Looking To Classify Digital Currencies As Securities"*	Canada	88	0.029	1.41
5	Sep. 7, 2017	*"Digital Currencies, ICO-Based Tokens Are Securities, Says Kiwi Finance Regulator"*	New Zealand	93	−0.096***	−7.05
6	Feb. 11, 2018	*"Gibraltar To Introduce 'World's First' ICO Regulations"*	Gibraltar	246	−0.007	−0.58
7	Feb. 14, 2018	*"Canadian Stock Exchange Launches 'Fully-Regulated' Token Platform, 'Unlike' ICOs"*	Canada	249	0.116***	10.90
8	Feb. 19, 2018	*"US: Republican, Democrat Officials Calling For Crypto Regulation In Rare Show Of Unity"*	US	255	−0.057***	−5.45
9	Mar. 7, 2018	*"US: Cryptocurrency Trading Platforms Must Be Registered With SEC"*	US	284	−0.187***	−21.77
10	Mar. 13, 2018	*"Thailand's SEC To Release Crypto Market Regulatory Framework In March"*	Thailand	295	−0.125***	−12.74
11	May 14, 2018	*"Thailand: Legal Framework For Cryptocurrencies Comes Into Force"*	Thailand	361	−0.020***	−2.56
12	May 21, 2018	*"Thai SEC Holds Focus Group to Clarify New Crypto, ICO Regulations"*	Thailand	371	−0.087***	−11.73
13	Jul. 5, 2018	*"Thai Regulator Confirms July Start Date for Regulated ICOs"*	Thailand	409	0.002	0.36
14	Jul. 20, 2018	*"Ukrainian Financial Stability Council Supports Regulatory Concept for Cryptocurrencies"*	Ukraine	421	−0.035***	−5.32
15	Aug. 10, 2018	*"ICOs Can 'Prove Their Legitimacy' Under New Crowdfunding Rules, Says EU Lawmaker"*	Global	445	−0.104***	−10.31
		Mean Market Reaction		3,701	−0.052***	−2.56

This Table presents an overview of the 15 transparency regulation news events between January 1st, 2017 and December 31st, 2018, which are the focus of this study, and the market reaction to each event and on average across all events. For the market tests, we cluster standard errors at the event- and crypto token-level. *, **, and *** denote statistical significance at the two-tailed 10, 5, and 1 percent level, respectively.

2.4.2 Cross-sectional Variation in Market Reactions

Next, we examine whether the perceived costs and benefits of transparency regulation differ across crypto token characteristics. We focus primarily on measures of crypto token quality and transparency, following our theoretical arguments in Section 2. If the previously documented negative market reactions reflect investor concerns about the costs of transparency regulations, we expect a less negative reaction for crypto tokens of higher quality, and with a higher degree of transparency absent regulation. We expect that transparency regulation is less costly for such issuers as they are already more transparent, or more efficient or competent in dealing with new regulatory requirements. These predictions are in line with Bourveau et al. (2022) and Howell et al. (2020), who find that (post-)ICO success is linked to dimensions such as disclosure and management team experience at the time of the ICO. In short, the costs of increased transparency regulation are likely to be lower for more transparent and higher quality crypto tokens, resulting in a less negative reaction to transparency regulation news.

Like Bourveau et al. (2022), we use ratings data from ICOBench to gauge the quality of crypto tokens and several measures of disclosure activity, such as the firm's website and social media activity, to capture their transparency. We first describe the ICOBench ratings data and analyses in more detail in the next section and then describe the analyses using disclosure and exchange data.

2.4.2.1 The Role of ICOBench Ratings

ICOBench is a crowd-based ratings platform that provides independent assessments of a crypto token's quality and transparency at the time of the ICO. The ICOBench page for a token issue provides an overview of the ICO, links to social media, and an overview of ratings. The overall rating or *Total Rating* is based on the weighted average of the algorithmically calculated *Benchy Rating* and on the *Expert Rating*, which is based on cryptocurrency/blockchain experts' assessments. The *Benchy Rating* is available for all crypto tokens that host an ICO and are included on ICOBench. This rating is an algorithmic assessment of management quality, transparency about the ICO, presence on social media, and the underlying product or service. It is based on information publicly provided in the application of the ICO to ICOBench, the ICO whitepaper, and elsewhere online. In practice, the *Benchy Rating* is the weighted average of four subcomponents (*Team Info. Score*, *ICO Info. Score*, *Product Info. Score* and *Social Media Score*). These subcomponent scores are based on a check list including, e.g., whether the ICO has a whitepaper online, whether the hard-cap and soft-cap are mentioned, and whether the teams provide LinkedIn accounts and full names. The weighting in the overall *Benchy Rating* is based on the number of items on the check list for each subcomponent. ICOBench expresses the subcomponent scores as percentages, but transforms the weighted average *Benchy rating* to a score between 0 and 5. We provide more details in Table 2.A1 in the Appendix.

An *Expert Rating* is based on assessments by (ICOBench-designated) cryptocurrency and blockchain experts of a token issuer's management team (*Team Rating*), its strategy and investments (*Vision Rating*), and the product maturity and usefulness (*Product Rating*). These scores range from 1 (lowest) to 5 (highest). The weight placed on an individual expert's assessment in the overall *Expert Rating* depends on the tenure of the expert on ICOBench, their total number of ratings, and the completeness of the expert's profile. The weight of the *Benchy Rating* in the *Total Rating* decreases with the number of *Expert Ratings. Expert Ratings* are only available for half of the ICOs on ICOBench (2,828 of 5,149). If this *Expert Rating* is missing, the *Total Rating* is equal to the *Benchy Rating.* As Panel B of Table 2.1 shows, we restrict the sample of the ICOBench ratings tests to those ICOs for which all ratings are available to facilitate comparison across tests. This yields a restricted sample of 2,418 observations, while the univariate sample comprises 3,701 observations.[17]. To illustrate ICOBench ratings, Figure 2.A1 in the Appendix presents an example of expert ratings for the Javvy ICO.[18]

Table 2.3 provides descriptive statistics at the token-event level for the variables in our cross-sectional analyses. Consistent with our previous analyses, the average and median abnormal reaction to regulatory news is negative. Table 2.4 also suggests that experts incorporate different or additional information into their assessments of a crypto token: the Spearman (Pearson) correlation between *Benchy Rating* and *Expert Rating* is 0.48 (0.03). The relatively low correlation suggests that these two ratings may capture different dimensions of an ICO, and we therefore also analyze the effect of both ratings separately in addition to *Total Rating*.

We also provide descriptive statistics for the underlying scores that make up the *Benchy Rating*. As mentioned earlier, *Benchy Rating* is based on the amount of available information about the management team (*Team Info. Score*), the ICO (*ICO Info. Score*), the underlying product (*Product Info. Score*) and the extent to which a company communicates with its users or investors via social networks (*Social Media Score*). Table 2.3 shows that the median crypto token company

[17] The mean market reaction for this restricted sample is −0.052, with a t-statistic of −2.61, similar to the reaction for the full sample in Table 2.2

[18] More details are available here: `https://icobench.com/ico/javvy`.

provides all relevant details surrounding an ICO (median *ICO Info. Score* is 1.00), which is unsurprising as it only captures whether companies have reported basic details such as the ICO start and end dates, the number of tokens for sale, and the ICO price. There is more variation in the availability of information about the underlying product or business and the management team. There is also significant variation in a company's *Social Media Score*, because this measure not only captures the existence of (social) communication channels, but also accounts for activity on these channels. The correlations between these scores in Table 2.4 are also relatively low, suggesting that they do not seem to capture a single underlying construct. We also observe significant variation in the distributions of the components of Expert Rating, and that they are highly correlated (around 0.8).

Table 2.3: Descriptive Statistics

Variable	N	Mean	S.D.	Min.	Q1	Med.	Q3	Max.
CAR	2,418	−0.052	0.177	−0.790	−0.142	−0.064	0.008	2.284
Size	2,418	16.563	1.877	11.595	15.365	16.587	17.783	21.317
Supply	2,418	18.182	2.138	11.528	16.856	18.383	19.674	23.627
Total Rating	2,418	3.242	0.772	0.800	2.900	3.400	3.800	4.700
Benchy Rating	2,418	3.053	0.743	0.700	2.800	3.200	3.500	4.800
Expert Rating	2,418	3.630	1.054	1.000	3.167	3.933	4.367	5.000
Benchy Rating Components								
Team Score	2,418	0.445	0.240	0.000	0.500	0.500	0.500	1.000
ICO Info. Score	2,418	0.951	0.116	0.170	1.000	1.000	1.000	1.000
Product Info. Score	2,418	0.673	0.213	0.000	0.600	0.800	0.800	1.000
Social Media Score	2,418	0.522	0.206	0.000	0.380	0.530	0.690	0.940
Expert Rating Components								
Team Rating	2,418	3.635	1.154	1.000	3.000	4.000	4.400	5.000
Vision Rating	2,418	3.765	1.090	1.000	3.200	4.000	4.500	5.000
Product Rating	2,418	3.489	1.108	1.000	3.000	3.700	4.200	5.000
Disclosure Variables								
Website Size (unlogged)	1,747	183,136	387,068	1,655	44,302	100,642	203,160	8,098,158
Website Size	1,747	11.376	1.292	7.631	10.764	11.584	12.236	14.340
GitHub	1,747	0.652	0.476	0.000	0.000	1.000	1.000	1.000
Total GitHub Changes	1,747	5.117	5.050	0.000	0.000	4.942	9.650	14.474
Total Tweets	1,747	5.744	1.183	0.693	5.252	5.814	6.540	7.875
Total Replies	1,747	4.177	1.484	0.000	3.466	4.357	5.124	6.917

Variable	**N**	**Mean**	**S.D.**	**Min.**	**Q1**	**Med.**	**Q3**	**Max.**
Exchange Variables								
Exchange Volume	1,771	20.999	2.409	14.041	20.955	21.810	22.375	23.987
Exchange Liquidity	1,771	5.937	0.671	1.099	5.916	6.055	6.236	6.488
Exchange Visits	1,771	13.556	1.611	7.563	12.647	13.722	14.647	16.595
Exchange #Cryptos	1,771	5.528	0.731	2.303	5.388	5.614	5.943	6.721

This Table presents the descriptive statistics for the variables used in the cross-sectional analyses. The variables are defined in Table 2.A1 of the Appendix.

Table 2.4: Correlation Matrix

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
1	*CAR*		−0.04	−0.07*	0.06*	0.08*	−0.01	0.04	0.02	0.05*	0.06*	0.01	−0.02	−0.01	−0.03	−0.01	−0.02	0.01	0.05	0.03	−0.03	0.01	0.00
2	*Size*	−0.06*		0.33*	0.11*	0.03	0.37*	−0.12*	0.01	−0.15*	0.22*	0.36*	0.35*	0.29*	0.47*	0.18*	0.52*	−0.07*	−0.05	0.06*	0.12*	0.15*	0.30*
3	*Supply*	−0.07*	0.42*		0.10*	0.09*	0.06*	0.05	−0.07*	0.08*	0.08*	0.09*	0.00	0.06*	0.19*	0.18*	0.19*	0.09*	0.17*	0.11*	0.11*	0.04	0.11*
4	*Total Rating*	0.01	0.16*	0.27*		0.77*	0.43*	0.49*	0.31*	0.52*	0.61*	0.42*	0.35*	0.39*	0.16*	0.05	0.14*	0.21*	0.19*	0.31*	0.01	0.12*	0.04
5	*Benchy Rating*	0.01	0.11*	0.21*	0.91*		0.03	0.63*	0.35*	0.61*	0.79*	0.07*	0.00	0.01	0.05	0.00	0.02	0.13*	0.17*	0.37*	−0.08*	0.13*	0.00
6	*Expert Rating*	0.00	0.42*	0.33*	0.73*	0.48*		0.05	0.03	−0.05	0.10*	0.91*	0.91*	0.93*	0.35*	0.15*	0.36*	0.18*	0.04	−0.06*	0.14*	0.05	0.13*
7	*Team Score*	−0.02	0.07*	0.13*	0.59*	0.67*	0.32*		0.10*	0.39*	0.26*	0.09*	0.07*	0.00	−0.06*	−0.06*	−0.05*	0.07*	0.11*	0.14*	−0.01	0.01	−0.12*
8	*ICO Info. Score*	0.01	−0.23*	−0.07*	0.32*	0.31*	0.11*	0.06*		0.21*	0.18*	−0.03	0.02	0.05	−0.05	0.02	−0.11*	0.07*	0.10*	0.09*	−0.12*	−0.02	−0.09*
9	*Product Info. Score*	0.00	−0.04*	0.16*	0.64*	0.69*	0.37*	0.35*	0.14*		0.30*	0.02	−0.11*	−0.07*	−0.04	−0.02	−0.08*	0.13*	0.20*	0.18*	−0.08*	−0.02	−0.16*
10	*Social Media Score*	0.02	0.25*	0.21*	0.71*	0.78*	0.40*	0.30*	0.07*	0.33*		0.12*	0.07*	0.10*	0.16*	0.04	0.15*	0.10*	0.11*	0.36*	0.02	0.19*	0.13*
11	*Team Rating*	0.00	0.38*	0.32*	0.73*	0.51*	0.94*	0.34*	0.14*	0.39*	0.41*		0.78*	0.74*	0.42*	0.23*	0.40*	0.21*	0.06*	−0.02	0.13*	0.07*	0.12*
12	*Vision Rating*	−0.01	0.41*	0.32*	0.70*	0.46*	0.95*	0.32*	0.05*	0.35*	0.38*	0.84*		0.79*	0.27*	0.09*	0.31*	0.11*	0.00	−0.05	0.14*	0.05	0.13*
13	*Product Rating*	0.00	0.38*	0.30*	0.65*	0.39*	0.94*	0.24*	0.12*	0.29*	0.34*	0.81*	0.85*		0.28*	0.09*	0.28*	0.14*	0.04	−0.09*	0.13*	0.03	0.12*
14	*Exchange Volume*	−0.07*	0.47*	0.25*	0.20*	0.16*	0.27*	0.03	−0.04*	0.07*	0.23*	0.33*	0.20*	0.23*		0.69*	0.83*	0.39*	0.00	0.10*	0.10*	0.16*	0.22*
15	*Exchange Liquidity*	0.02	0.20*	0.14*	0.07*	0.00	0.17*	−0.06*	−0.08*	−0.02	0.07*	0.18*	0.14*	0.17*	0.46*		0.54*	0.51*	−0.02	−0.02	0.10*	0.14*	0.15*
16	*Exchange Visits*	−0.03	0.44*	0.26*	0.12*	0.05*	0.27*	0.00	−0.12*	−0.04*	0.16*	0.31*	0.22*	0.22*	0.66*	0.46*		0.34*	−0.02	0.08*	0.13*	0.18*	0.23*
17	*Exchange #Cryptos*	−0.03	0.11*	0.14*	0.20*	0.18*	0.17*	0.11*	0.05*	0.16*	0.13*	0.25*	0.07*	0.16*	0.55*	0.45*	0.57*		0.03	0.15*	−0.02	0.06*	0.04
18	*Website Size*	0.04	0.03	0.15*	0.23*	0.20*	0.09*	0.12*	0.08*	0.24*	0.12*	0.10*	0.11*	0.06*	0.00	0.08*	0.06*	0.06*		0.04	0.02	0.09*	0.02
19	*GitHub*	0.01	0.07*	0.09*	0.38*	0.40*	0.11*	0.17*	0.07*	0.21*	0.45*	0.13*	0.14*	0.05*	0.15*	0.01	0.17*	0.12*	0.04		0.00	0.04	0.01
20	*Total GitHub Changes*	−0.02	0.16*	0.13*	0.07*	0.01	0.21*	0.04*	−0.14*	−0.02	0.06*	0.21*	0.21*	0.17*	0.09*	−0.05*	0.13*	−0.05*	0.03	0.09*		0.01	−0.02
21	*Total Tweets*	0.01	0.24*	0.19*	0.21*	0.22*	0.22*	0.15*	0.02	0.02	0.27*	0.23*	0.20*	0.19*	0.09*	0.11*	0.05*	−0.01	0.12*	0.02	0.01		0.75*
22	*Total Replies*	−0.01	0.30*	0.22*	0.10*	0.10*	0.18*	0.09*	0.01	−0.07*	0.16*	0.19*	0.14*	0.18*	0.15*	0.14*	0.15*	0.06*	0.03	−0.02	0.00	0.79*	

This Table presents the correlation matrix for the variables used in the cross-sectional analyses. Pearson (Spearman) correlations are below (above) the diagonal.The variables are defined in Table 2.A1 of the Appendix. * denotes statistical significance at the two-tailed 5 percent level.

To assess how crypto token ratings affect the market reaction to regulation news, we estimate the following model:

$$
\begin{aligned}
CAR_{i,t} = \alpha &+ \beta_1 Rating_i + \beta_2 Size_{i,t} + \beta_3 Supply_{i,t} \\
&+ Platform\ Fixed\ Effects_i + Event\ Fixed\ Effects_t \qquad (2) \\
&+ \varepsilon_{i,t},
\end{aligned}
$$

where *Rating* equals the Total Rating of a crypto token i, the *Benchy Rating*, the *Expert Rating*, or the scores underlying the *Benchy Rating*. As explained above, we expect the coefficient on the ratings variables to be positive, i.e., we expect investors to react less negatively to regulatory news for higher quality crypto tokens. We also control for crypto token size, supply and platform fixed effects, to capture differences in market micro-structure that likely affect returns.[19] We include event fixed effects to control for differences in CAR across events, ensuring that the only variation in CAR is cross-sectional.[20] The dependent variable CAR is the cumulative abnormal return around the regulation news event, i.e., the sum of the mean-adjusted crypto token return over the two-day event-window.

[19] The platforms are: Bitshares, Counterparty, Ethereum, NEM, NEO, Nubits, Qtum, Stellar, Ubiq, Waves, or a proprietary platform. Different platforms are built with different protocols that affect how transactions are settled, which applications can be built on the platform, and how the supply of the token is arranged (Chod & Lyandres, 2022; Johan & Pant, 2019).

[20] Because event fixed effects subsume any controls at the event level, we do not include Bitcoin returns in this estimation. However, results are similar if we drop the fixed effects and control for Bitcoin or Ethereum return.

Table 2.5 presents the ratings regression results. We find that *Total Rating* is significantly associated with *CAR* in the predicted direction. Because *Total Rating* comprises the automated *Benchy Rating* as well as the *Expert Rating* if available, we separately examine the relation between *CAR* and these two subcomponents of the rating. Like the aggregate *Total Rating*, *Benchy Rating*, which is purely based on an algorithmic assessment of a crypto token's whitepaper and other available information, is significantly associated with the market reaction to transparency regulation news in the predicted positive direction. Because the correlations between the subcomponents of the *Benchy Rating* are relatively low, we include them jointly in the regression in Column (3) in order to assess which subcomponent drives this result. We find that only *Social Media Score* is significantly associated with *CAR*: all else equal, a one standard deviation increase in this score results in a 1.3 percentage point less negative market reaction to regulation news.[21] Because this variable captures the extent to which a crypto token company communicates with potential investors during the ICO period, this result suggests that investors expect more transparent crypto tokens, or those with better disclosure policies to be less affected by news concerning increased transparency regulation, because they are potentially less affected by regulatory efforts aimed at increasing the transparency of crypto tokens.

[21] We calculate this effect as follows: 0.206 (standard deviation of *Social Media Score*) $\times$ 0.061 (coefficient) = 0.013.

Table 2.5: The Role of *Total Rating* and *Benchy Rating*

		CAR		
	Prediction	**(1)**	**(2)**	**(3)**
Total Rating	+	0.017**		
		(2.28)		
Benchy Rating	+		0.012**	
			(2.24)	
Team Info. Score	+			0.004
				(0.36)
ICO Info. Score	+			−0.008
				(−0.23)
Product Info. Score	+			−0.008
				(−0.24)
Social Media Score	+			0.061***
				(4.47)
Size	?	−0.012***	−0.011***	−0.011***
		(−4.14)	(−3.32)	(−3.53)
Supply	?	−0.003	−0.002	−0.003
		(−1.74)	(−1.46)	(−1.63)
N		2,418	2,418	2,418
No. of Crypto Tokens		315	315	315
No. of Events		15	15	15
R^2		0.191	0.190	0.191
Platform FE		Yes	Yes	Yes
Event FE		Yes	Yes	Yes

This Table presents the coefficients (t-statistics in parentheses) for the analyses of the effect of *Total Rating* and *Benchy Rating* on market reactions to transparency regulation news events. The variables are defined in Table 2.A1 of the Appendix. We cluster standard errors at the event- and crypto token-level and include crypto token platform- and event fixed effects. *, **, and *** denote statistical significance at the two-tailed 10, 5, and 1 percent level, respectively.

This result is consistent with our prediction that investors expect regulatory costs to be lower for more transparent crypto tokens.

Next, we examine the effect of *Expert Rating* on market reactions to transparency regulation news. Table 2.6 shows that *Expert Rating* and its components are all significantly related to *CAR* in the predicted direction. The results indicate that investors view regulation to be less costly for crypto tokens that have a more competent management team (*Team Rating*), a clearer business strategy (*Vision Rating*), and a more mature product (*Product Rating*). These results also suggest that experts incorporate information into their ratings that investors perceive to be valuable, but which is not captured by the automated assessments provided by the underlying components of the *Benchy Rating*. Furthermore, because these three components do not appear to capture the disclosure activity on social media, we also include *Social Media Score* and *Expert Rating* in the same regression to assess whether they capture separate constructs. The results in Column (5) show that both variables are significant, suggesting that experts' assessment of the crypto token and the extent of communication with investors measure different dimensions of perceived crypto token quality.

Our analyses with the ratings data reveal that cryptocurrency/blockchain experts' confidence in a crypto token's business strategy, management team and core product, as well as the extent of disclosure via social media at the time of the ICO, mitigate the negative reaction to regulation news. In short: both token quality and transparency appear to matter. In the next sections, we perform some additional tests to gauge the transparency effect further.

Table 2.6: The Role of *Expert Rating*

		CAR				
	Prediction	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Expert Rating	+	0.013***				0.010***
		(2.64)				(2.28)
Team Rating	+		0.012***			
			(3.28)			
Vision Rating	+			0.009*		
				(1.92)		
Product Rating	+				0.010***	
					(2.47)	
Social Media Score	+					0.047***
						(3.70)
Size	?	−0.013***	−0.012***	−0.012***	−0.012***	−0.013***
		(−4.32)	(−4.12)	(−4.21)	(−3.78)	(−4.75)
Supply	?	−0.002	−0.002	−0.002	−0.002	−0.003
		(−1.39)	(−1.40)	(−1.27)	(−1.38)	(−1.75)
N		2,418	2,418	2,418	2,418	2,418
No. of Crypto Tokens		315	315	315	315	315
No. of Events		15	15	15	15	15
R^2		0.191	0.192	0.190	0.191	0.193
Platform FE		Yes	Yes	Yes	Yes	Yes
Event FE		Yes	Yes	Yes	Yes	Yes

This Table presents the coefficients (t-statistics in parentheses) for the analyses of the effect of *Expert Rating* on market reactions to transparency regulation news events. The variables are defined in Table 2.A1 of the Appendix. We cluster standard errors at the event- and crypto token-level and include crypto token platform- and event fixed effects. *, **, and *** denote statistical significance at the two-tailed 10, 5, and 1 percent level, respectively.

2.4.2.2 The Role of Disclosure

One drawback of the ICOBench data is that it captures dimensions, such as disclosure, at the time of the ICO, rather than at the time of a news event. To counter this concern, we use various measures of a crypto token issuer's disclosure activity to more precisely capture transparency at the time of a news event.

Because there is no mandated disclosure for crypto tokens in the sample period, we focus on three measurable channels of voluntary disclosure following prior literature: corporate websites, product information, and social media interaction. Boulland, Bourveau, and Breuer (2021) propose a standardized measure of voluntary disclosure based on the quantity of information on firms' websites. We use the WebArchive Wayback Machine to find the website size on the closest date to the event date.[22] A larger website contains more information, which can indicate a more transparent crypto token firm. Next, Bourveau et al. (2022) document that the disclosure of product information and source code on GitHub, a code repository website, is associated with the transparency of a crypto token firm. We measure two aspects of source code transparency: whether there is a GitHub page, and how many code changes have been shared, up until the event date. Lastly, we focus on a crypto token's firm activity

[22] More information about the Wayback Machine is available here: `https://archive.org/about/`.

on Twitter, because it is a relatively visible and interactive medium (Zhou, Lei, Wang, Fan, & Wang, 2015), and most crypto token issuers are on this platform: 4,893 of the 5,149 crypto tokens that hosted an ICO and are listed on ICOBench are on Twitter.[23] We use Twitter data to calculate the following transparency measures: the number of tweets by a crypto token issuer (*Total Tweets*) and how many tweets are replies to followers or previous tweets (*Total Replies*), up until the event date. We distinguish between replies and general tweets, because we observe that crypto token tweets can contain token-specific content as well as general content that is irrelevant to the token, whereas replies are typically more focused on answering questions about the token.

Tables 2.3 and 2.4 provide descriptive statistics for these disclosure measures. There appears to be significant cross-sectional variation in the amount of disclosure across crypto token issuers: for instance, we observe that the interquartile range of website size in bytes is over 4.5 times larger than the value at the 25th percentile. In addition, 35 percent of the issuers provide no information about the product or underlying on GitHub, and while most issuers have Twitter, their activity on this platform varies. We also observe differences over time (untabulated): almost all issuer websites increase in size over time, while Twitter activity does not vary much on a rolling basis. Overall, these observations suggest differences in the amount of information that token issuers disclose, both

[23] In contrast, only 1,500 are active on Reddit.

cross-sectionally and over time.

Interestingly, Table 2.4 reports that *Website Size* and *Social Media Score* do not appear to be highly correlated with Twitter activity (between −0.01 and 0.27), nor is *Product Info. Score* highly correlated with GitHub activity (between −0.02 and 0.21). One explanation could be that these disclosure measures vary over time, whereas *Social Media Score* and *Product Info. Score* is only based on activity around the time of the ICO. Alternatively, the ICOBench scores are based on activity on multiple platforms.

To gauge the effect of disclosure on the reaction to transparency regulation news, we estimate the following regression:

$$\begin{aligned} CAR_{i,t} = \alpha &+ \beta_1 \mathit{Website\ Size}_{i,t} + \beta_2 \mathit{GitHub}_i \\ &+ \beta_3 \mathit{Total\ Github\ Changes}_{i,t} + \beta_4 \mathit{Total\ Tweets}_{i,t} \\ &+ \beta_5 \mathit{Total\ Replies}_{i,t} + \beta_6 \mathit{Expert\ Rating}_i \\ &+ \beta_7 \mathit{Social\ Media\ Score}_i + \beta_8 \mathit{Size}_{i,t} + \beta_9 \mathit{Supply}_{i,t} \\ &+ \mathit{Platform\ Fixed\ Effects}_i + \mathit{Event\ Fixed\ Effects}_t + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where *Website Size* is the natural log of website size (in bytes) of the website HTML code. *GitHub* is an indicator variable that is equal to one if the crypto token company shares a link to a GitHub code repository on the ICO page on ICOBench, and zero otherwise. *Total Github Changes* is the natural log of the total number of publicly shared GitHub code changes (both additions and deletions) from CoinGecko. *Total Tweets* is the natural log of one plus the number of a crypto token's tweets and *Total Replies* is the natural log of one plus the number of its tweets are replies to followers or previous tweets. All countable variables are measured up until the event date, but our results hold when we measure them using a rolling window of 90 days prior to the event. In the full specification, we include *Expert Rating* and *Social Media Score* to control for management capabilities such as responsiveness and disclosure activity at time of the ICO. We again include crypto token size, supply and platform fixed effects to capture difference in market micro-structure, and event fixed effects to ensure that the only variation in CAR is cross-sectional. Starting with the sample of crypto tokens for which we have all ICO rating data (Tables 2.5 and 2.6), we further restrict the sample of the disclosure tests to those crypto tokens for which all disclosure variables are available on the event date. This yields a sample of 1,747 observations.[24].

Table 2.7 presents results from the regressions using these three alternative disclosure measures. We find that *Website Size* is significantly positively associated with *CAR*, consistent with the prediction that investors view regulation as less costly for crypto tokens that are more transparent and disclose more information. This result is robust for other

[24] The mean market reaction for this restricted sample is -0.054, with a t-statistic of -2.56, similar to the reaction for the full sample in Table 2.2

measures of transparency and quality captured by the rating variables, and for other disclosure measures. We do not find a significant association between disclosure on GitHub or Twitter and *CAR*, suggesting not all tweets and code change is informative for token investors. Although *Expert Rating* is no longer significant, *Social Media Score* remains significantly positive throughout.[25] Overall, these analyses are in line with our earlier conclusions about the value of transparency and disclosure to crypto token investors, with the caveat that not all types of disclosure seem to matter equally.

[25] The diminished significance of *Expert Rating* could be due to the restricted sample size.

Table 2.7: The Role of Disclosure

	Prediction	*CAR*										
		(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
Website Size	+		0.009*	0.008*							0.009*	0.008*
			(1.99)	(1.87)							(2.08)	(1.93)
GitHub	+				0.013	0.007					0.014	0.008
					(1.55)	(0.69)					(1.64)	(0.82)
Total GitHub Changes	+				0.000	0.000					0.000	0.000
					(−0.21)	(−0.33)					(−0.20)	(−0.34)
Total Tweets	+						0.000	−0.002			−0.002	−0.005
							(0.06)	(−0.32)			(−0.22)	(−0.57)
Total Replies	+								0.000	0.000	0.001	0.003
									(0.12)	(−0.04)	(0.24)	(0.52)
Expert Rating	+	0.005		0.005		0.006		0.005		0.005		0.006
		(1.07)		(1.01)		(1.13)		(1.11)		(1.07)		(1.27)
Social Media Score	+	0.060***		0.057***		0.053**		0.061***		0.060***		0.052**
		(3.07)		(3.21)		(2.17)		(3.45)		(3.09)		(2.57)
Size	?	−0.009***	−0.005	−0.009***	−0.005	−0.008**	−0.005	−0.009***	−0.005	−0.009***	−0.005	−0.008**
		(−3.16)	(−1.41)	(−3.26)	(−1.12)	(−2.72)	(−1.48)	(−3.24)	(−1.35)	(−3.19)	(−1.26)	(−2.76)
Supply	?	−0.004*	−0.004*	−0.004**	−0.004*	−0.004**	−0.003	−0.004*	−0.003	−0.004*	−0.004**	−0.004**
		(−2.05)	(−2.03)	(−2.33)	(−2.04)	(−2.20)	(−1.72)	(−2.03)	(−1.75)	(−2.07)	(−2.40)	(−2.54)
N		1,747	1,747	1,747	1,747	1,747	1,747	1,747	1,747	1,747	1,747	1,747
No. of Crypto Tokens		252	252	252	252	252	252	252	252	252	252	252
No. of Events		15	15	15	15	15	15	15	15	15	15	15
R^2		0.227	0.226	0.230	0.224	0.227	0.222	0.227	0.222	0.227	0.227	0.230
Platform FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This Table presents the coefficients (*t*-statistics in parentheses) for the analyses to the effect of cross-sectional differences in crypto token firm disclosure on market reactions to transparency regulation news events. TThe variables are defined in Table 2.A1 of the Appendix. We cluster standard errors at the event- and crypto token-level and include crypto token platform- and event fixed effects. *, **, and *** denote statistical significance at the two-tailed 10, 5, and 1 percent level, respectively.

2.4.2.3 The Role of Crypto Exchanges

Besides capturing transparency with the alternative disclosure measures in the previous sub-section, we also assess whether investors' reactions differ with the trading environment of tokens. Crypto exchanges play an important gate-keeping role by facilitating crypto token trading to investors and deciding on the (de-)listing of crypto tokens. As highlighted in Section 2, there are significant differences in listing requirements. Crypto exchanges with stricter listing requirements put more emphasis on crypto token transparency as this is associated with higher investor confidence, while those with looser listing requirements aim to facilitate trading in as many crypto tokens as possible. For example, Coinbase requires disclosure on e.g., governance, compliance and underlying economics (GDAX, 2017), while Bitforex only requires contact information and a minimum market capitalization.[26] As a consequence, we expect to observe differences in the transparency across trading exchanges due to differential listing requirements.

We examine observable market-level trading outcomes to capture transparency. Although anecdotally, we know that the strictness of listing requirements differs across exchanges, we are unable to objectively

[26] See the application form of Coinbase (https://www.coinbase.com/assethub) and Bitforex (https://docs.google.com/forms/d/e/1FAIpQLSfHSFgFn3dHpdMMHwHKzfBTLYL6FMpWJ-pYj8bExKQ1Orzsdg/viewform) for more information on the specific listing process.

measure these differences systematically, since not all exchanges publicly provide a detailed set of listing requirements. First, we assess the liquidity of exchanges on which a token is listed, because prior research documents that transparency should reduce information asymmetry and increase liquidity (see e.g., Chae, 2005; Welker, 1995). Therefore, we expect that the negative market reaction to transparency regulation news for crypto tokens that trade in a more liquid, and therefore more transparent, environment should be attenuated. Second, we also study exchange-level trading volume, although the theoretical relation with information asymmetry is less clear in this case. Although prior work has established that information asymmetry can reduce trading volume (Admati & Pfleiderer, 1988), some studies predict the opposite (Kim & Verrecchia, 1994). Moreover, specifically for crypto exchanges, Cong, Li, Tang, and Yang (2021) find that some exchanges engage in crypto wash trading to artificially inflate trading volume. Hence, we do not predict ex ante how exchange trading volume moderates investors' reactions to transparency regulation news.

To gauge the effect of these characteristics on the reaction to transparency regulation news, we estimate the following regression:

$$
\begin{aligned}
CAR_{i,t} = \alpha &+ \beta_1 Exchange\ Liquidity_i + \beta_2 Exchange\ Volume_i \\
&+ \beta_3 Exchange\ Visits_i + \beta_4 Exchange\ \#Cryptos_i \\
&+ \beta_5 Expert\ Rating_i + \beta_6 Social\ Media\ Score_i \\
&+ \beta_7 Size_{i,t} + \beta_8 Supply_{i,t} + Platform\ Fixed\ Effects_m \\
&+ Event\ Fixed\ Effects_t + \varepsilon_{i,t}.
\end{aligned} \tag{4}
$$

Our main measures of interest are *Exchange Liquidity* and *Exchange Volume*. *Exchange Liquidity* is the natural log of one plus the per crypto token liquidity score. The liquidity score ranges from 0 to 1,000 and is based on the slippage incurred by various order sizes.[27] *Exchange Volume* is the natural log of the per crypto token average total dollar trading volume. We also gather data on two other exchange characteristics to control for other factors that could drive liquidity or volume. *Exchange Visits* is the natural log of the per crypto token average of total number of unique visitors. *Exchange #Cryptos* is the natural log of one plus the per crypto token average of the total number of cryptocurrencies. Since crypto tokens can be listed on multiple exchanges, we take the average across all exchanges on which a token is listed to calculate the

[27] Coinmarketcap calculates 'slippage' as the number of times a hypothetical order is settled for a different from the price that order was originally requested, out of 1,000.

following variables at the token i level, using data from Coinmarketcap.[28] We collect these measures from Coinmarketcap on December 7, 2021.[29] In the full specification, we include *Expert Rating* and *Social Media Score* to control for management capabilities such as responsiveness, and disclosure activity. We again include crypto token size, supply and platform fixed effects to capture difference in market micro-structure, and event fixed effects to ensure that the only variation in CAR is cross-sectional. Starting with the sample of crypto tokens for which we have all ICO rating data (N = 2,419), we further restrict the sample of the exchange-level tests to those crypto tokens for which all these exchange variables are available. This yields a restricted sample of 1,771 observations.[30].

We present the results of our final tests in Table 2.8. Consistent with our predictions, we find that *Exchange Liquidity* is significantly positively associated with the market reaction to transparency regulation news, suggesting that investors perceive the costs of transparency regulation to be less for tokens that are likely already more transparent, based on the environments in which they are traded. Second, we find a negative, but

[28] See `https://coinmarketcap.com/nl/rankings/exchanges/` for more information.

[29] Due to data limitations, these measures are time-invariant and calculated by Coinmarketcap over 24 hours. However, we have tracked these measures at several points over the course of a month and find that both the level and the relative ranking between exchanges do not vary much.

[30] The mean market reaction for the sample in this analysis is -0.055, with a t-statistic of -2.66, similar to the reaction for the full sample in Table 2.2

only marginally significant coefficient for *Exchange Volume*. This seems consistent with the results in Cong et al. (2021), which suggest that higher trading volume does not necessarily indicate a more transparent trading environment, but rather the opposite due to crypto wash trading. These results are similar if we control for *Expert Rating* and ICO transparency in column (2), and that these two variables also load in the same direction as in previous tests. Hence, our results appear consistent with the conclusion that token investors value transparency but may view regulatory efforts to mandate more disclosure as costly.

Table 2.8: The Role of Crypto Exchanges

	Prediction	*CAR* (1)	(2)
Exchange Liquidity	+	0.014**	0.013**
		(2.20)	(2.87)
Exchange Volume	?	−0.005	−0.005*
		(−1.75)	(−1.92)
Exchange Visits	?	0.008*	0.008*
		(2.04)	(2.06)
Exchange #Cryptos	?	−0.012	−0.013
		(−0.84)	(−0.89)
Expert Rating	+		0.007*
			(1.85)
Social Media Score	+		0.038**
			(2.35)
Size	?	−0.012**	−0.014***
		(−2.97)	(−3.82)
Supply	?	−0.003*	−0.003*
		(−1.88)	(−1.96)
N		1,771	1,771
No. of Crypto Tokens		229	229
No. of Events		15	15
R^2		0.232	0.235
Platform FE		Yes	Yes
Event FE		Yes	Yes

This Table presents the coefficients (t-statistics in parentheses) for the analyses to the effect of cross-sectional differences in characteristics of exchanges on which tokens are listed on market reactions to transparency regulation news events. The variables are defined in Table 2.A1 of the Appendix. We cluster standard errors at the event- and crypto token-level and include crypto token platform- and event fixed effects. *, **, and *** denote statistical significance at the two-tailed 10, 5, and 1 percent level, respectively.

2.5 Conclusion

Despite calls for, and ad hoc attempts at regulating the cryptocurrency market, the benefits and costs of regulation in this setting are unclear. We provide empirical evidence on this issue by examining investor reactions to transparency regulation news from investors in a type of cryptocurrency issued through an ICO: crypto utility tokens. We identify 15 dates between June 2017 and August 2018 with transparency regulation news and find that the cumulative abnormal two-day return is negative for news that increases the likelihood of transparency increasing regulation. These results are robust for several sample restrictions, and different specifications and methodologies. Cross-sectionally, we observe variation in the degree of token transparency, not only around the ICO but also post-ICO, highlighting that some issuers choose to be more transparent, even absent regulation. We find that the negative reaction is attenuated for crypto tokens that have higher expert ratings for transparency, management competence, and the underlying product idea at the time of the ICO. Furthermore, investors react less negatively to regulation news if crypto tokens disclose more information to investors on their website, and if crypto tokens are listed on more liquid exchanges. These results are consistent with transparency being important to investors. Although our results do not imply crypto tokens should remain largely unregulated, they suggest that investors perceive current regulatory proposals to be costly, but less so for higher quality and more transparent crypto tokens that are likely to be less affected by regulation.

Appendix

Description of Events

We define a transparency regulation news event as: (announcements of) actions related to concrete and specific regulations and/or laws, or the establishment of working groups, initiated by market authority/regulatory bodies focused on enhancing the transparency of crypto tokens and crypto tokens issuers in the context of securities. Importantly, in order to observe a market reaction, we focus on regulation efforts that are not only aimed at increasing transparency at the time of the ICO, but also after issuance. We provide more background on the events mentioned in Table 2.2 and a discussion of potential confounding events on the event dates.

In 2017, we identify five events. On June 13, 2017, the SEC publicly stated that it is looking to regulate the ICO process and the companies behind the ICO after the issuance. Although the precise details were not mentioned, the SEC's statements about aiming to increase transparency signal an increasing likelihood of transparency-increasing regulation. On July 25, 2017, the SEC ruled that Decentralized Autonomous Organization (DAO) Tokens, issued by ICO in 2016, are officially securities subject to securities regulation for disclosure. This ruling set a precedent for other ICOs for case-by-case reviews by the SEC. As such, we expect this event increases the likelihood of transparency regulation. On August

1, 2017, the financial regulator of Singapore, a country with 10 of the 100 largest cryptocurrency exchanges globally, announced that it will regulate ICOs and crypto token companies on a case-by-case basis. The regulations mentioned a focus on disclosure and transparency, and therefore we expect this to increase the likelihood of transparency regulation. On August 24, 2017, the financial regulator of Canada announced that it perceives crypto tokens to be more like securities, rather than its own asset class. With this statement, the Canadian regulator implied that most crypto tokens are subject to the disclosure regulations of traditional securities, increasing the likelihood of transparency regulation for these tokens. On September 7, 2017, the financial markets regulator of New Zealand announced that all crypto tokens issued through the ICO process are considered to be securities, and have to adhere to security regulations.

In 2018, we identify ten events. On February 11, 2018, Gibraltar became the first country to introduce regulations specifically aimed at crypto tokens and ICOs. The regulation set forth disclosure rules that provide information to anyone buying tokens, at the time of the ICO and thereafter. On February 14, 2018, the Canadian Stock Exchange announced that it would start a regulated platform for trading in security tokens. Registration with the Canadian security regulator is required, which increases the likelihood of disclosure regulation for tokens looking to register on this exchange. On February 19, 2018, a bipartisan group of U.S. lawmakers announced that they were looking to form new legislation to regulate cryptocurrencies. Part of the proposed legislation is a disclosure framework to protect investors against manipulation and fraud. On March 7, 2018, the SEC announced that cryptocurrency exchanges must be registered with the SEC and subject to similar rules as traditional exchanges, meaning that exchanges are to adopt the SEC rules for (de-)listing, disclosure, and financial responsibility. This increases the expected disclosure of crypto tokens listed on U.S. cryptocurrency exchanges. On March 13, 2018, and again with more details on May 14, 2018, Thailand announced a regulatory framework for crypto tokens that brings these assets under the jurisdiction of the securities regulator. The framework is focused on investor protection and involves disclosure requirements, but it is yet unclear to what degree. On May 21, 2018, Thai regulators hosted a focus group meeting to clarify proposed crypto regulations of May 14, 2018. At the center of the proposed regulation sits a new framework, which comes with specific rules on capital and disclosure for digital tokens. On July 5, 2018, Thailand officially announced the proposed set of general regulations for ICOs and crypto tokens, which increased the disclosure requirements for ICOs and crypto tokens. On July 20, 2018, the regulatory body of Ukraine officially supported a regulatory concept to regulate cryptocurrencies, which identifies crypto tokens as financial instruments. This regulation also defines information disclosure conditions and requirements, increasing the likelihood of transparency regulation for crypto tokens. On August 10, 2018, the EU announced a new crowdfunding regulation that is also intended for ICOs. The regulation is focused on increasing investor protection through disclosure requirements, dependent on the type of crypto token. We expect this event to increase the likelihood of crypto token transparency regulation.

Confounding Events

Next, we examine the possibility of confounding events affecting our results. We search LexisNexis for any important confounding events during all fifteen event windows and do not find significant events during any of the windows. The lack of significant confounding news is supported by an average event window return of the S&P500 of 0.22 percent. We then search for other confounding events related to cryptocurrencies in particular.

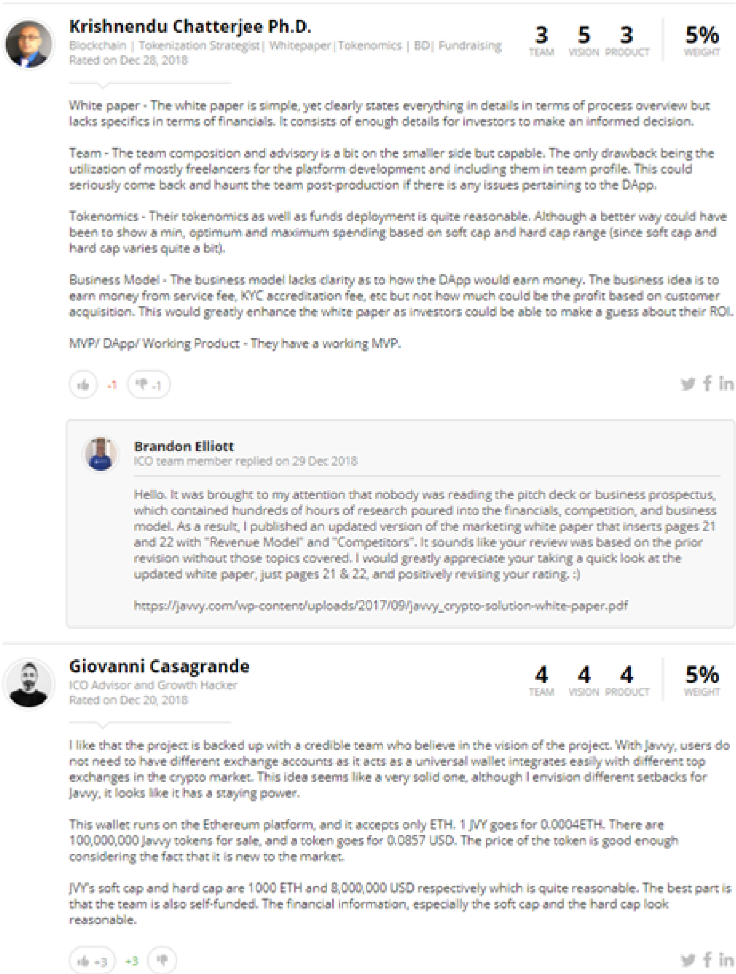
We then search for cryptocurrency-related confounding events. We are able to find one confounding cryptocurrency event during our event windows. On March 13, 2018, IMF head Christine Lagarde wrote in a blogpost that cryptocurrency technology should be used to crackdown and regulate cryptocurrencies.[31] While this post still deals with regulation of cryptocurrencies, it is not related to transparency regulation. However, as mentioned in Section 2.4.1, excluding this (or any other) event from

[31] See: `https://blogs.imf.org/2018/03/13/addressing-the-dark-side-of-the-crypto-world/`.

the analyses yields very similar results. Taken together, we trust that our results cannot be explained by confounding events, both general and cryptocurrency-specific.

Figures

Figure 2.A1: Example of ICO Rating on ICOBench



This Figure provides an example of a typical rating overview of an ICO on ICOBench: the ICO of a project named Javvy. The figure presents two of the 17 *Expert Ratings*, along with information about the specific expert that rated the ICO. The weight of the rating in the total *Expert Rating* is based on the level of the expert. This level is based on the tenure of the expert on ICOBench, the number of ratings of the expert, and the completeness of his/her profile.

Tables

Table 2.A1: Definition of Variables

Variable	**Source**	**Description**
CAR	Coinmarketcap	Sum of mean-adjusted return over [0;+1] where t = 0 is the event date. For mean adjustment we use the average return over the 80 calendar days around t = 0, excluding other event-dates.
Size	Coinmarketcap	Natural logarithm of total market capitalization of a crypto token.
Supply	Coinmarketcap	Natural logarithm of total market capitalization divided by closing price of a crypto token.
Total Rating	ICOBench	Weighted average rating of *Benchy Rating* and *Expert Rating*, provided by ICOBench.
Benchy Rating	ICOBench	Rating of ICO information provided by ICO Analyzer Bot 'Benchy', as the weighted average of the following four subcomponents: (1) *Team Info. Score*, (2) *ICO Info. Score*, (3) *Product Info. Score*, and (4) *Social Media Score*. ICOBench transforms the percentage ratings of individual subcomponents (see below) to a score between 0 and 5 prior to averaging.
Expert Rating	ICOBench	Weighted average of cryptocurrency/blockchain expert ratings, based on the following subcomponents: (1) *Team Rating*, (2) *Vision Rating* and (3) *Product Rating*.
Team Score	ICOBench	Score (in %) for total information available about team behind ICO, provided by ICO Analyzer Bot 'Benchy' on ICOBench.
ICO Info. Score	ICOBench	Score (in %) for total information available about ICO provided by ICO Analyzer Bot 'Benchy' on ICOBench.
Product Info. Score	ICOBench	Score (in %) for the total information available about product, provided by ICO Analyzer Bot 'Benchy' on ICOBench.
Social Media Score	ICOBench	Score (in %) for presence on social media, both in terms of total number of platforms and in activity on those platforms, provided by ICO Analyzer Bot 'Benchy' on ICOBench.
Team Rating	ICOBench	Rating (out of 5) of team behind the ICO provided by experts on ICOBench.
Vision Rating	ICOBench	Rating (out of 5) of the vision and/or plans outlaid in the ICO provided by experts on ICOBench.
Product Rating	ICOBench	Rating (out of 5) of the actual product or service offered by the company doing an ICO provided by experts on ICOBench.
Exchange Volume	Coinmarketcap	Natural log of the per crypto token average total dollar trading volume of all exchanges on which a crypto token is listed. The dollar trading volume is measured from December 7th till December 8th, 2021.

Variable	Source	Description
Exchange Liquidity	Coinmarketcap	Natural log of the per crypto token liquidity score of all exchanges on which a crypto token is listed. The liquidity score ranges from 0 to 1000 and is based on the slippage incurred by various order sizes, and is calculated by Coinmarketcap between December 7th and December 8th, 2021.
Exchange Visits	Coinmarketcap	Natural log of the per crypto token average of total number of unique visitors on the exchanges on which a crypto token is listed. The number of visitors are measured from December 7th till December 8th, 2021.
Exchange #Cryptos	Coinmarketcap	Natural log of the per crypto token average of the total number of cryptocurrencies on the exchanges on which a crypto token is listed. The number of cryptocurrencies is measured from December 7th till December 8th, 2021.
Website Size	WebArchive	Natural log of the size in bytes of the website of the crypto token company, following Boulland et al. (2021). We take the snapshot from the Internet Wayback Machine of the WebArchive at the date closest prior to the event.
GitHub	ICOBench	Indicator variable that is equal to 'one' if the crypto token company shares a link to a GitHub code repository on the ICO page on ICOBench.
Total GitHub Changes	CoinGecko	Natural log of the sum of additions and deletions in the code shared by the crypto token company in the GitHub code repository, up until the date of an event.
Total Tweets	Twitter	Natural log of one plus the number of tweets sent by the crypto token company up until the date of an event.
Total Replies	Twitter	Natural log of one plus the number of reply tweets sent by the crypto token company, i.e., replies to their own or others' tweets up until the date of an event.

This Table gives an overview of the variables used in the analyses, their sources, and their descriptions.

Table 2.A2: Overview of the Top 100 Cryptocurrency Exchanges

Country	**No. of Top 100 cryptocurrency exchanges**	**Exchanges**
Australia	1	TOPBTC
Canada	2	BCEX, Coinsquare
Cayman Islands	1	BitMart
China	9	Binance, ZB.COM, DOBI trade, OEX, IDCM, Fatbtc, C2CX, Allcoin, LakeBTC
Cyprus	2	Coindeal, Cryptology
Dubai	1	RightBTC
Estonia	4	Bibox, CoinsBank, P2PB2B, CryptalDash
Gibraltar	1	GBX Digital Assets
Hong Kong	7	OKEx, HitBTC, LBank, Bitfinex, Bit-Z, Coinsuper, CHAOEX
India	1	UEX
Indonesia	2	Exrates, Indodax
Ireland	1	Bitsane
Japan	2	Bitbank, BTCBOX
Luxembourg	1	Bitstamp
Mongolia	1	IDAX
New Zealand	1	Cryptopia
Panama	1	IDEX
Peru	1	Bitinka
Poland	2	Coinroom, Coinbe
Russia	3	Simex, B2BX, YoBit
Singapore	10	Huobi, DigiFinex, CoinBene, DragonEX, CoinTiger, LATOKEN, Kucoin, MBAex, HADAX, Coinut
South Korea	6	Upbit, Allbit, CPDAX, Coinone, Korbit, GOPAX
Switzerland	1	Rfinex
Taiwan	1	Bitrue

Country	No. of Top 100 cryptocurrency exchanges	Exchanges
Thailand	1	TDAX
Turkey	5	Sistemkoin, Vebitcoin, BtcTurk, Paribu, Ovis
UK	11	Cryptonex, CoinEgg, Bitlish, Exmo, Livecoin, CEX.IO, Mercatox, Bilaxy, BTC-Alpha, DSX, Luno
Ukraine	1	Liqui
Unknown	10	BitBay, Hotbit, InfinityCoin, Trade by Trade, BtcTrade.im, BiteBTC, Coinhub, Ethfinex, Liquid, Waves
US	10	Kraken, Coinbase Pro, Kryptono, Bittrex, Gate.io, Gemini, itBit, bitFlyer, Poloniex, Tidex

This Table presents the total number and the names of top 100 cryptocurrency exchanges per country. If the country of registration could not be determined, the country of registration is ‘Unknown’. Data is per November 2018.

3 Public Company Auditing Around the Securities Exchange Act

3.1 Introduction

Securities market crashes and accounting scandals have startled the public throughout history (e.g., Hail, Tahoun, & Wang, 2018). In response to these events, the public tends to call for more transparency and oversight (e.g., Flesher & Flesher, 1986; Langenbucher, Leuz, Krahnen, & Pelizzon, 2020; Sellhorn, 2020; Stein, Salterio, & Shearer, 2017). Heeding this call, public company auditors position themselves as gatekeepers, ensuring public companies' credible reporting and investors' trust (e.g., Coffee, 2006; Roychowdhury & Srinivasan, 2019). Their effectiveness

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as gatekeepers in securities markets, however, is frequently called into question, as they are often blamed for failing to prevent and detect accounting scandals (see, e.g., the recent Wirecard scandal).[32]

To safeguard investors against market crashes and accounting scandals, securities markets are increasingly regulated (e.g., La Porta, Lopez-de Silanes, & Shleifer, 2006; Leuz & Wysocki, 2016). A centerpiece of securities regulation is the regulation of public company auditing, which nowadays comprises audit mandates, audit standards, and even auditor oversight. Whether such regulatory intervention is necessary for and effective in sustaining audit and securities markets, however, remains the subject of a controversial debate (e.g., DeFond et al., 2016; DeFond & Zhang, 2014; Donovan et al., 2014).[33]

We aim to inform the debate on the need for audit regulation by exploring the landscape of public company auditing around the first major regulatory intervention in the U.S. audit market, the introduction of the Securities and Exchange Commission (SEC) in 1934. This intervention created a federal regulator that mandated the auditing of public companies' financial statements and had the authority to set audit

[32] In the 1920s, for example, Touche Niven (nowadays Deloitte) failed to uncover overstated accounts receivables in the financial reports of its client, Ultramares. In the early 2000s, Arthur Anderson was blamed for failing to detect Enron's accounting fraud. Most recently, Ernst & Young was scrutinized for its failure to detect Wirecard's overstated cash accounts.

[33] For a recent example, see the debate on internal control audits required under Section 404(b) of the Sarbanes-Oxley Act (e.g., Barth, Landsman, Schroeder, & Taylor, 2019; Posner, 2020).

standards and oversee the audit profession. Using a broad sample of historical annual reports, we examine both the state of public company auditing before the SEC's introduction *and* changes in public company auditing and associated capital-market outcomes around the SEC's introduction. The former sheds light on the necessity of regulation for the functioning of audit markets, whereas the latter provides information about the effectiveness of regulation in making improvements over and above unregulated audit and capital markets. Our broad-sample exploration of the historical audit landscape is enabled by recent advances in the digitization and automated textual analysis of historical documents and inspired by the seminal work of Watts and Zimmerman (1981, 1983), which explores the need for audit regulation using qualitative evidence from various historical episodes ranging from the English merchant guilds of the 11^{th} century up to the SEC's introduction.

In theory, the need to regulate the auditing of public companies is not apparent. Fama and Jensen (1983a, 1983b), Watts and Zimmerman (1983), and Ball (1989), for example, suggest that public companies have private incentives to obtain independent audits. They argue that public companies, characterized by the separation between ownership and control, stand to benefit from reduced agency frictions between managers and investors. Moreover, DeAngelo (1981) suggests that auditors have private incentives to provide *independent* audits. She argues that even though auditors are paid by management, they are reluctant to give in to management's demands for bias or partiality, because they fear damage to their reputation and loss of other clients.

To justify regulation, the literature advances two main reasons auditing is different from a normal good and would therefore be insufficiently provided by unregulated markets. Donovan et al. (2014) and Minnis and Shroff (2017), for example, conjecture that audits could have public good features (e.g., externalities on trust in securities markets), which lead companies to undervalue audits and auditors to under-provide effort. DeFond et al. (2016), moreover, conjecture that audits have credence-good features, which make companies reluctant to obtain audits because they (or their investors) cannot judge the value of the audit. Other general reasons for regulation include cost reductions due to increased standardization or reduced duplicative contracting efforts, as well as stricter enforcement and penalties (e.g., Leuz, 2010; Minnis & Shroff, 2017). Notwithstanding these potential reasons for regulation and issues with unregulated markets, it is ultimately an empirical question whether regulation, which comes with its own imperfections, can address these issues more efficiently than market forces (e.g., Demsetz, 1969; G. J. Stigler, 1971).

Public company audits were widely unregulated in the U.S. until the Securities Act of 1933 and the Securities Exchange Act of 1934. The acts, motivated by the 1929 securities market crash, aimed at establishing "truth in securities" markets through legal liability and disclosure. The 1933 act expanded auditors' legal liability to third parties and required the disclosure of audited prospectuses.[34] The 1934 act added requirements to disclose audited annual reports and created a federal securities market regulator, the SEC. This regulator was tasked with enforcing the new disclosure and auditing requirements, which were applicable to companies trading on stock exchanges, but not the over-the-counter (OTC) market (Greenstone et al., 2006). Before these federal mandates, audit requirements were limited to certain industries (e.g., the railroad companies had been subject to inspection by examiners of the Interstate Commerce Commission since 1906) or set by private actors, such as the NYSE, which had been asking listing companies to commit to annual audits since April 1932 (see p. 19 of Forbes, 1934; Abs et al., 1954). In addition to enforcing audit mandates, the SEC was granted the authority to regulate audit standards and audit supervision (Coffee, 2006). As a result of lobbying by prominent auditors and the limited subject-matter expertise of politicians and regulators, the SEC initially abstained from

[34] Before the act, privity (i.e., a contractual relationship) was required to sue auditors for negligence, limiting auditors' liability to clients (e.g., boards of directors). Shareholders of audited companies and other third parties could sue auditors only for fraudulent behavior, which required a high burden of proof (i.e., intent), or for gross negligence, since the *Ultramares Corp. v. Touche* decision in 1932. The 1933 act extended auditors' liability to third parties by discarding the privity requirement (Kothari, Lys, Smith, & Watts, 1988).

using these powers (Wiesen, 1978).[35] This reliance on the self-regulation of public company auditors provides a first indication of the limits of regulation.[36] While unregulated markets may not work perfectly, the potential for capture and the lack of expertise of politicians and regulators may render regulation an equally imperfect solution (Demsetz, 1969; G. J. Stigler, 1971).

To explore the audit landscape around the SEC's introduction, we construct a historical panel tracking a broad sample of public companies over several decades. Our sample consists of U.S. public companies with annual reports available in the archives maintained by *Mergent* and *ProQuest* up until fiscal year 1940. From the companies' reports, we extract audit statements using optical character recognition (OCR) and natural language processing (NLP) techniques. The audit statements provide information on companies' auditors (if any), auditor locations, audit sign-off dates, audit procedures (as reported), and audit opinions. We combine this audit information with information on each public company's location, industry, trading venue, basic financials (size, earnings

[35] In 2002, in response to the bursting of the dot-com bubble and high-profile accounting scandals (e.g., Enron), the Sarbanes-Oxley Act created a dedicated regulator, the Public Company Accounting Oversight Board (PCAOB), to oversee the audit profession, upending the reliance on the profession's self-regulation (e.g., DeFond & Lennox, 2017; Gipper, Leuz, & Maffett, 2020).

[36] Initially, the rule-makers even considered the use of federal auditors or, at least, federal licensing of auditors. Leading auditors convinced them otherwise. The auditors successfully argued that they themselves had the necessary expertise and independence, and that establishing a federal auditor would be costly and inefficient (Wiesen, 1978).

per share (EPS), dividend policy), and equity-market outcomes, which are provided by the historical databases of *Global Financial Data* (*GFD*) and the *Center for Research on Securities Prices* (*CRSP*).

Our combined sample comprises 1,528 unique companies and 124 unique auditors over more than four decades. Of the 1,528 companies, 91% trade on stock exchanges (including 56% on the New York Stock Exchange (NYSE)), while the remaining 9% trade on the OTC market. Most of the companies are located in the Northeastern U.S., though our sample includes companies from all parts of the country. On the auditor side, our sample is composed of both small and large auditors, many of which are predecessors of today's dominant auditors (e.g., Price Waterhouse, Ernst & Ernst, Arthur Young, and Touche & Niven). Similar to today, the bulk of audit engagements in our dataset is executed by just a few auditors, with the ten largest auditors accounting for 68% of the audit engagements.

We begin our exploration of the audit landscape by investigating public companies' propensity to hire auditors. While less than 30% (40%) of public companies hired auditors in 1900, the (value-weighted) audit rate steadily increased to about 80% (80%) in 1933, just before the SEC's introduction. This high audit rate limited the impact of the SEC's 1934 audit mandate. Our estimates suggest an 8-percentage-point increase in market-wide audit rates, at most, as a result of the mandate. Collectively, these results suggest that companies frequently hired auditors, even absent any mandate (consistent with Watts & Zimmerman, 1983). They thus cast doubt on a need for regulation due to auditing being a public good, in which companies underinvest. We caution, though, that the mandate forced some large companies to obtain audits. These large companies may have had market-wide externalities; accordingly, our audit-rate results do not rule out the need for regulation per se. However, they do suggest that the impact was likely limited.

Next, we explore how the public companies with audits chose their auditors. We find that companies tended to hire auditors with greater client-portfolio sizes and lower client-portfolio concentrations. We further find that companies tended to select auditors that were located closer to their headquarters and specialized in their industries. These findings are consistent with companies favoring auditors that exhibit characteristics reflecting independence (DeAngelo, 1981) and competence (Rajgopal, Srinivasan, & Zheng, 2021; Solomon, Shields, & Whittington, 1999). Notably, we find that these characteristics, if anything, mattered more in the period before the SEC than after. These findings cast doubt on the argument that regulation is necessary because auditing is a credence good and companies cannot differentiate between auditors. This is not to say that auditing is not a credence good.[37] Rather, our findings

[37] In fact, Aobdia, Siddiqui, and Vinelli (2021) document evidence consistent with auditing being a credence good.

suggest that private contracting solutions (e.g., reputation) seem to limit the issues arising from the credence-good features of auditing, and that the regulation does not appear to provide clear improvements over and above those provided by private contracting solutions. Even more so, our findings raise the possibility that regulation may weaken the market forces that incentivize companies, on the one hand, to choose independent and competent auditors and incentivize auditors, on the other hand, to ensure independence and invest in competence (Donovan et al., 2014).

After examining companies' audit rates and auditor choices, we explore audit services provided to public companies around the SEC's introduction. As a window to the hard-to-observe practices of auditors, we use the format and content of audit statements. We find that the length of audit statements increased by about 50% around the SEC's introduction, whereas the length of the audit process (sign-off date relative to fiscal year end) did not clearly change. We further find that audit statements shifted, around the SEC's introduction, from testifying on companies' financial positions to opining on companies' compliance with generally accepted accounting principles (GAAP). Lastly, we find that audit statements increasingly featured only a few dominant topics prevalent in all reports rather than various company- or auditor-specific topics. Taken together, these findings uncover a push for lower expectations regarding the level of assurance provided by auditors, and a trend toward standardization of companies' financial reporting and their auditing services. Notably, the push for lower expectations gained momentum in 1932, even before the auditor liability extension of the 1933 act, through a prominent tort law case against an auditor of a fraudulent company (*Ultramares Corp. v. Touche*). Similarly, the trend toward standardization primarily reflects the concurrent efforts of private-sector parties (e.g., the NYSE and the American Institute of Accountants (AIA, now AICPA)), according to historical accounts (e.g., Hatfield, 1936; Hilke, 1986; Wiesen, 1978; Zeff, 1982, 2007). Hence, the SEC may have been a catalyst for the standardization and codification of practices in a developing profession, but not its root cause. Consistent with this view, we find that many of the changes in audit services had already begun before the SEC's introduction and were not limited to companies that were traded on stock exchanges and hence affected by the audit mandate.

Finally, we examine public companies' capital-market outcomes around the SEC's introduction. In our examination, we differentiate between three distinct company types: voluntary adopters, which adopted audits before the SEC mandate; mandatory adopters, which were forced to adopt audits by the SEC mandate; and never adopters, which comprise non-compliant and non-mandated companies that never adopted audits in our sample period. Compared to voluntary adopters, mandatory adopters are of similar size and profitability but have lower market liquidity and exhibit a higher propensity to pay dividends. Never adopters, while substantially larger and more profitable than both voluntary and mandatory adopters, have even lower market liquidity and exhibit an even higher propensity to pay dividends. These univariate differences suggest that companies with audits cater to a different investor clientele (trading-oriented investors) than companies without audits (consumption-oriented investors). They further provide prima facie evidence consistent with voluntary auditing helping the liquidity of a company's stock. However, the univariate differences between the three groups do not change substantially around the introduction of the SEC mandate. Furthermore, difference-in-differences tests show no significant change in capital-market outcomes (market value and liquidity) for mandatory adopters relative to voluntary adopters at that time. Similarly, when we use never adopters as the control group, we find only weak evidence of improved liquidity for the mandatory adopters.

Our capital-market results are consistent with the view that the SEC's audit mandate had no significant effect on the mandatory adopters. Still, they are also consistent with the contrary view that the mandate benefited both the mandatory adopters *and* the other (audited) companies (e.g., due to greater trust in auditing or regulated securities markets). We expect the former view to be more plausible than the latter for a number of reasons. First, the direct effect of an audit mandate on the mandated company's capital-market outcomes should likely dominate any indirect effect on other companies' capital-market outcomes. In this case, we should observe a significant difference-in-differences effect, which we do not. Second, we would expect market-wide externalities to manifest primarily in the regulated markets. We find similar trends, however, in capital-market outcomes around the SEC's introduction for companies trading on the regulated exchanges and those trading on the unregulated OTC market. Lastly, we note that only a small share of the market, even in value-weighted terms, was effectively forced by the mandate to be audited. Collectively, these findings cast doubt on the importance of SEC audit regulations to capital-market outcomes for both mandated companies and the market as a whole.

In sum, our descriptive evidence provides little support for the popular view that audit regulation is central to the functioning of public companies' auditing and capital markets (e.g., DeFond & Zhang, 2014). Instead, it supports the view that public company auditing, though frequently regulated, is not a product of regulation (e.g., Buijink, 2006; Watts & Zimmerman, 1983). It does *not* imply that public company auditing is worthless, though. To the contrary, our evidence suggests that audits were sufficiently valuable to be widely adopted and associated with greater capital-market access even without regulation. Our evidence that the SEC mandate had, at best, a limited impact on capital-market outcomes merely indicates that there appears to be little benefit, for individual companies and capital markets as a whole, to forcing audits on companies that would not choose them voluntarily. More broadly, our evidence and the pertinent historical accounts (e.g., on the rule-making process) suggest that the promise of regulation is limited by regulatory capture and expertise constraints (e.g., Demsetz, 1969; G. J. Stigler, 1971; Wiesen, 1978).

Our paper contributes to the literature on the state of auditing in the pre-SEC era. Existing evidence provides qualitative assessments (e.g., Watts & Zimmerman, 1983) and documents audit rates for a limited number of companies and/or years (e.g., Barton & Waymire, 2004; Benston, 1969; Chow, 1982; Merino, Mayper, & Sriram, 1994).[38] Our paper extends this evidence thanks to our novel data, which allows us to paint a detailed picture of the auditing landscape (not just audit rates) in the early 20^{th} century. Our data cover a broad sample of companies traded on stock exchanges and unregulated OTC markets. This feature permits us to examine the state of auditing for a representative cross-section of companies and across various trading venues. In addition, our data span several decades. This feature is pivotal to learn about long-run

[38] Benston (1969) provides audit rates for 333 (508) companies traded on the NYSE in 1926 (1934). Chow (1982) provides audit rates for 379 (65) companies traded on the NYSE (OTC markets) in 1926. Merino et al. (1994) provide audit rates for 430 (365) companies traded on the NYSE (other New York markets) in 1927. Barton and Waymire (2004) provide audit rates for 540 companies traded on the NYSE in 1929.

trends and developments in the audit market. Finally, our data comprise the texts of companies' audit statements. This feature opens a window to auditors' practices and services of the time, allowing us to shed light on standardization efforts and changes in the level of assurance provided by auditors.

Our paper also contributes to the literature on the regulation of auditing (e.g., DeFond & Zhang, 2014; Minnis & Shroff, 2017; Vanstraelen & Schelleman, 2017).[39] It informs the controversial debate about the need for audit regulation, especially audit *mandates*, (e.g., DeFond et al., 2016; Donovan et al., 2014). Recent evidence, primarily from Europe and Canada, casts doubt on the need for auditing regulation for private companies (e.g., Breuer, 2021; Dedman, Kausar, & Lennox, 2014; Esplin, Jamal, & Sunder, 2018; Kausar et al., 2016; Lennox & Pittman, 2011; Minnis & Shroff, 2017). Evidence on the need for auditing regulation for public companies remains scarce, though. The scarcity is owed to the fact that public companies around the world have almost invariably been subject to auditing regulation for several decades already. To learn about the need for regulating these companies' auditing, we examine the

[39] The Securities Acts extended auditor liability and granted wide-reaching authorities to the SEC, including the regulation of audit standards and oversight. Accordingly, the SEC introduction constitutes a major change in audit regulation, which allows us to learn about the political prospects of various regulatory aspects (e.g., standard setting and oversight) from the historical accounts of the rule-making and regulatory practice. As the SEC initially abstained from actively intervening in standard setting and oversight, our empirical evidence on economic consequences, by contrast, primarily sheds light on the impact of audit mandates absent a concurrent intervention in standard setting or oversight.

firm-level *and* market-wide impact of the first federal audit regulation for public companies in the U.S. Thereby, we extend recent private-company evidence to the realm of large, economically important public companies for which regulators around the world appear to see the need for regulation given their stark separation of ownership and control.[40]

Our paper is related to the literature on unregulated markets. Several recent studies document that unregulated capital markets, including the OTC market (Brüggemann, Aditya, Leuz, & Werner, 2018), the peer-to-peer lending market (Michels, 2012; Verstein, 2011), the market for initial coin offerings (Bourveau et al., 2022), and the equity crowdfunding market (Schwartz, 2018) function even in the presence of information asymmetries. Absent regulation, information asymmetries are addressed by private contracting solutions such as voluntary disclosure (Bourveau, Breuer, & Stoumbos, 2020) and certification (Jamal & Sunder, 2011). In line with these studies, our paper suggests that public company auditing is a prominent *private* contracting solution, which alleviates information frictions in capital markets. It does not appear to be a market which itself is in obvious need of regulation (Watts & Zimmerman, 1983).

Our paper is also closely related to earlier studies on the introduction of the SEC. Several studies document that the SEC had a limited im-

[40] A related reason to separately study public companies is that auditors might play a different role for private companies. In a field study, Esplin et al. (2018) find that for private companies, auditors often are more accounting experts and business-service providers than fraud detectors or monitors of management.

pact on companies' disclosure, corporate fraud, and investors' trust in capital markets (e.g., Benston, 1969, 1973; Daines & Jones, 2012; Ely & Waymire, 1999; G. J. Stigler, 1971).[41] Our paper complements these studies by specifically exploring the SEC's audit regulation and its impact on companies' audit practices and investors' trust.

Our paper's historical evidence does not provide immediate policy implications, but it does invite skepticism about the promise of regulatory interventions in the audit market in response to securities market crashes and accounting scandals, such as the recent Wirecard scandal (e.g., Langenbucher et al., 2020). It documents that the first major regulatory intervention (i.e., the SEC's introduction) and its main regulatory measure with respect to audits (i.e., the audit mandate), which nowadays are both taken for granted, had only a limited impact when they were introduced. Thus, it raises the possibility that less or smarter regulation may be called for, not necessarily more regulation (e.g., Leuz, 2009). Our paper may thereby help counter the human tendency to add rather than subtract features when problem solving (Adams, Converse, Hales, & Klotz, 2021).[42]

[41] Several studies criticize this evidence, though, and argue in favor of the SEC (e.g., Fox, 1999; Fox, Morck, Yeung, & Durnev, 2003; Friend & Herman, 1964; SEC, 1977; J. Seligman, 1983). Most recently, Binz and Graham (2021), improving upon prior literature with better data and a difference-in-differences design, document evidence of increased short-window reactions to earnings announcements after the SEC introduction.

[42] DeFond et al. (2016), for example, state that they would not be comfortable suggesting less regulation (e.g., no mandates) even if less regulation were optimal.

3.2 Conceptual Underpinnings

Public companies are characterized by the separation of ownership and control (Berle & Means, 1932).[43] The separation gives rise to an agency conflict between investors, who own the companies' resources, and managers, who control the resources (Jensen & Meckling, 1976). The agency conflict is costly to management, because investors, anticipating the diversion of their resources, are reluctant to supply them. As a result, management has an incentive to reduce agency costs.

Management can reduce agency costs by reporting the company's financial position and performance to investors (Kothari, Ramanna, & Skinner, 2010; Watts & Zimmerman, 1986). For such reporting to be effective, it needs to be credible. Management can bolster the credibility of its financial reporting by hiring a third-party auditor to check the reporting on behalf of the company's investors (Ball, 1989; Fama & Jensen, 1983a, 1983b; Watts & Zimmerman, 1983).

Third-party auditors need to be independent and competent to provide effective assurance to investors. The independence of auditors is important to prevent them from giving in to management's demands for bias or partiality. Although auditors are paid by management, they have

[43] In the U.S., this separation occurred as early as in the late 19^{th} century. By 1930, the number of individuals owning stock in listed companies had reached 10 million (Coffee, 2010).

incentives to resist a given management's demands, because a tarnished reputation jeopardizes their business with all their other clients. Accordingly, larger auditors with dispersed client portfolios tend to be more independent (DeAngelo, 1981). The competence of auditors is important to ensure that they are in a position to critically and efficiently evaluate management's reporting procedures and assumptions. Following that reasoning, auditors with industry- and location-specific knowledge tend to provide higher-quality audits (Rajgopal et al., 2021; Solomon et al., 1999).

The above arguments suggest that an independent audit is a normal good, demanded by companies with agency costs and supplied by third-party auditors (Donovan et al., 2014). In this case, an unregulated audit market yields the optimal level of auditing. To justify the regulation of the audit market, proponents argue that an independent audit is a special good, not a normal one. They argue, for example, that an independent audit is a public good, because it provides externalities (e.g., trust in capital markets) (DeFond & Zhang, 2014). In this case, an unregulated market underprovides auditing. They also argue that an independent audit is a credence good, because the value of the auditor's service cannot easily be discerned by companies and their investors. In this case, an unregulated market again underprovides auditing (DeFond et al., 2016). Other reasons typically advanced in favor of regulation include cost reductions due to increased standardization or reduced duplicative contracting efforts, as well as stricter enforcement and penalties (e.g., Leuz, 2010; Minnis & Shroff, 2017).

While the audit market left to its own devices may deliver inefficient levels of auditing, it is unclear whether regulation, which comes with its own imperfections, can address these issues more efficiently than market forces (Demsetz, 1969). Regulators grapple with informational constraints, which are often worse than those faced by companies and their investors. Accordingly, they frequently resort to one-size-fits-all regulations. These regulations neglect differences in companies' needs for audits, putting excessive burdens on some companies (Breuer, 2021). Similarly, they mute market forces that incentivize auditors to differentiate their services and allow companies to signal their type (Kausar et al., 2016). Regulators can also be captured by well-organized interested parties, which advocate for regulation to protect their rents rather than to improve the functioning of the audit market (G. J. Stigler, 1971).[44] This concern appears particularly relevant in the case of auditors, which are not only well-organized, but also lobby for a politically convenient good (i.e., trust, assurance, and transparency) (e.g., Wiesen, 1978). Accordingly, the need for and promise of regulation of public company

[44] In response to William L. Douglas's public endorsement of securities regulation, a prominent lawyer, for example, raised the concern that "political objections" may interfere with the application of securities regulation such "that the consequences would be far more harmful than the benefit which would result in protecting the investors" (E. Seligman, 1933).

auditing is ultimately an empirical question.

3.3 Institutional Background

In the early 20^{th} century, the number of public companies rapidly increased with the expansion of public securities markets in the U.S. (Rajan & Zingales, 2003). At the same time, the U.S. audit profession, influenced by its counterpart in the United Kingdom, developed and matured (e.g., May, 1926; Montgomery, 1913; Moss, 1914). Its maturity is exemplified by Montgomery's *Audit Theory and Practice*, the leading textbook on auditing principles and practices in the U.S. at the time, which was first published in 1912 and issued its fourth edition by 1933. In the absence of authoritative accounting and audit standards, textbooks and private initiatives by professional associations created de facto standards for the profession (e.g., Nouri & Lombardi, 2009). Most notably, the AIA (now AICPA) had collaborated with the NYSE since the 1920s to harmonize accounting and auditing practices (e.g., Zeff, 2007). This harmonization project gained momentum in response to a prominent tort case brought against an auditor of a fraudulent public company. The case, *Ultramares Corporation v. Touche* (1932), established that auditors are liable to third parties for gross negligence, not just fraud. It resulted in a reckoning for the profession by revealing the gap between the level of assurance expected by investors and the level actually provided by auditors (Carmichael & Winters, 1982). This reckoning propelled leading auditors' efforts to limit the auditors' service to opining on companies' compliance with accounting rules and practices instead of certifying companies' financial positions (Pandit & Baker, 2021; Wiesen, 1978).

The audit and securities markets were widely unregulated at the federal level until 1933. Existing disclosure and auditing requirements applied only within certain states, industries, or exchanges. A number of states, for example, introduced *Blue Sky Laws*, which created issuer liability and required prospectus disclosures for newly listed companies (e.g., Macey & Miller, 1991; Mahoney, 2003). However, these laws were typically limited in scope, weakly enforced, and easy to circumvent (e.g., by issuing in other states) (Loss, 1951). Besides state laws, there were a number of industry-specific disclosure and auditing requirements (e.g., those targeting the transportation industry). The Interstate Commerce Commission, for example, had required inspections of railroad companies since the Hepburn Act of 1906. In addition, in 1932 the NYSE, the primary stock exchange, started requiring listing companies to provide audited financial reports.

The Securities Acts of 1933 and 1934 marked a notable change in the federal regulation of audit and securities markets (Barton & Waymire, 2004). Motivated by the 1929 stock market crash and corporate scandals, the acts aimed at securing "truth in securities" markets through legal liability and disclosure.[45] The Securities Act of 1933 expanded auditors' legal liability to third parties, allowing them to sue auditors for negligence (e.g., Douglas & Bates, 1933; Jaenicke, 1977; Kothari et al., 1988). It further required newly listed public companies with securities traded on centralized exchanges (not the OTC market) to disclose audited prospectuses. The Securities Exchange Act of 1934 extended the disclosure requirements to public companies' annual reports. It also established a federal regulator, the Securities and Exchange Commission (SEC), which was tasked with enforcing the new requirements. Most relevant to this study, the 1934 Act gave the SEC power to require audits of public company annual reports, a requirement that it implemented within months of the Act's passage.[46] The SEC was further granted the power to regulate acceptable accounting and auditing standards and audit oversight. As a result of limited expertise and resources as well as successful lobbying by the audit profession, however, the SEC relied on independent instead of federal auditors to inspect companies' financial reports and left the definition of acceptable accounting and auditing practices to the profession, at least initially (Wiesen, 1978). Only after a

[45] Flesher and Flesher (1986), for example, argue that the 1932 bankruptcy of the Kreuger & Toll conglomerate, which operated a pyramid scheme and resisted audits, contributed significantly to the passage of the acts.

[46] Section 13(a)(2) of the 1934 Securities Exchange Act, as originally enacted, stated that annual reports would be certified by independent public accountants "if required by the rules and regulations of the [Securities and Exchange] Commission." Securities and Exchange Commission Release No. 66, promulgated on December 21, 1934, makes clear that the SEC had imposed the audit requirement by that time.

prominent fraud case (the *McKesson & Robbins* scandal) in 1938 did the SEC take greater interest in audit practices (Coffee, 2006).

The SEC is regarded as one of the most successful federal regulators (McCraw, 1984). Accordingly, we expect the first major discrete change in federal audit regulation to meaningfully affect the audit landscape if audit regulation is imperative for the functioning of audit and securities markets.

3.4 Historical Data

We construct a historical panel tracking a broad sample of public companies over several decades. Our sample construction proceeds in several steps. We first gather photocopy scans of all U.S. public companies' annual reports available in the archives maintained by *Mergent* and *ProQuest* up until fiscal year 1940.[47] We next convert the scans into machine-encoded text via optical character recognition (OCR). We then search the texts for audit statements and characteristics, using natural language processing techniques (NLP). From these statements, we extract information on companies' auditors (if any), auditor locations, audit sign-off dates, audit procedures (as reported), and audit opinions, again using NLP. Finally, we combine the audit information with information on

[47] Most of the original annual reports in the archives of *Mergent* and *ProQuest* are held by public libraries in the U.S. (e.g., the Cleveland Public Library).

each public company's location, industry, trading venue, basic financial information (size, EPS, dividend policy), and equity-market outcomes obtained from the historical databases of *Global Financial Data* (*GFD*) and the *Center for Research on Securities Prices* (*CRSP*). Appendix 3.A1 defines the variables in our data and Appendix 3.A2 lists the search terms used in our NLP approach.

Our combined sample comprises 1,528 unique public companies over more than four decades. Table 3.1 documents that *Mergent* covers 1,190 of these companies, whereas *ProQuest* covers 590 of them. The overlap of the two databases is limited (234 companies), which makes combining the two archives particularly useful. While *Mergent* covers a broader cross-section of companies than *ProQuest*, it spans a shorter time period (1892–1940) than *ProQuest* (1844–1940). For both archives, most companies are observed in the latter part of our sample period (1910–1940), consistent with the increasing prevalence of public companies during the early 20th century (Rajan & Zingales, 2003). Despite any differences in covered companies and time periods, the distribution of sectors, trading venues, and regions is similar across the two archives. Combined with the overlap with Global Financial Data, this bolsters our confidence that our sample covers a reasonably representative set of public companies.

The majority of our 1,528 unique companies operate in either the industrial (19%), the consumer discretionary (18%), or the materials (17%) sectors. 91% of our sample companies trade on stock exchanges, while the remaining 9% trade on the OTC market. The NYSE is the largest trading venue, with 56% of our sample companies listed on it. Unsurprisingly, the majority of our sample companies are located in the North-East region of the U.S. (47%), closely followed by the Mid-West (40%). The remaining companies are located in the West (7%) and South (6%) of the U.S.

The public companies in our sample are audited by 124 unique auditors. Our sample comprises both large and small auditors. The ten largest auditors in our sample account for 68.2% of the audit statements in our data. They include several familiar names and predecessors of today's auditors. As of 1927, Price Waterhouse (23.2%) was the largest auditor, followed by Ernst & Ernst (14.1%); Peat Marwick Mitchell (10.1%); Arthur Young (8.7%); Haskins & Sells (8.1%); Lybrand, Ross Bros. & Montgomery (6.7%); Touche & Niven (4.4%); Barrow Wade Guthrie (2.7%); FW LaFrentz & Co. (2.7%); and Arthur Andersen (2.4%). This list closely corresponds to the historical account in Zeff and Fossum (1967) and Merino et al. (1994). It comprises auditors of British origin as well as newly founded American auditors. An overview of our sample's 15 largest auditors and their number of engagements is presented in Appendix 3.A3.

Table 3.1: Descriptive Statistics

Panel A: Sample Overview

	Mergent	ProQuest	Overlap	Total	Auditors
N	9,021	9,871	1,174	17,168	10,436
Companies	1,190	590	234	1,528	124
Years	1892–1940	1844–1940	1897–1940	1844–1940	1845–1940
> 100 company-years starting in	1920	1910	1934	1910	1919
Sector *Comp.-years (comps.)*					
Communications	209 (34)	69 (8)	16 (5)	262 (37)	192 (2)
Consumer Discretionary	1,548 (201)	1,326 (77)	341 (41)	2,533 (236)	1,902 (15)
Consumer Staples	1,189 (143)	1291 (81)	328 (40)	2,152 (182)	1,495 (16)
Energy	401 (44)	533 (23)	106 (12)	828 (61)	459 (2)
Finance	264 (43)	923 (60)	13 (4)	1,174 (93)	286 (7)
Health Care	144 (17)	215 (17)	56 (8)	303 (26)	228 (4)
Industrials	1,388 (185)	1,622 (106)	349 (49)	2,661 (242)	1,773 (27)
Information Technology	116 (13)	120 (10)	46 (4)	190 (18)	154 (4)
Materials	1,443 (178)	1,364 (82)	322 (44)	1,485 (215)	1,814 (24)
Real Estate	20 (4)	0 (0)	0 (0)	20 (4)	18 (1)
Transports	607 (72)	860 (31)	17 (6)	1,450 (97)	540 (2)
Utilities and Telecommunications	599 (64)	517 (30)	74 (9)	1042 (83)	674 (2)
Trading Venue *Comp.-years (comps.)*					
ASE	693 (79)	130 (9)	35 (5)	788 (83)	541 (66)
NYSE	4,515 (544)	6,020 (350)	1,167 (161)	9,368 (727)	6,060 (633)
OTC	587 (92)	412 (31)	81 (8)	918 (115)	510 (71)
Other *(33 exchanges)*	2,160 (285)	2,265 (142)	384 (48)	4,041 (372)	2,432 (289)
Region *Comp.-years (comps.)*					
Mid-West	3,570 (512)	2910 (181)	671 (89)	5,809 (597)	3,920 (33)
North-East	4,218 (515)	5,810 (321)	913 (123)	9,106 (703)	5,325 (75)
South	515 (67)	587 (35)	77 (12)	1,025 (90)	524 (5)
West	656 (83)	471 (37)	63 (10)	1,064 (109)	612 (9)

Panel B: Descriptive Statistics, Full Sample								
	N	**Mean**	**S.D.**	**Min.**	**Q1**	**Med.**	**Q3**	**Max.**
Company Variables								
Size (Market Value)	11,260	2.538	1.850	−4.382	1.342	2.587	3.790	8.148
EPS	5,385	2.517	4.539	−21.950	0.350	1.770	3.830	78.880
Dividend Payer	5,385	0.638	0.480	0.000	0.000	1.000	1.000	1.000
Zero Return Days	11,535	0.341	0.412	0.000	0.000	0.083	0.909	1.000
Zero Volume Days	11,535	0.373	0.422	0.000	0.000	0.154	1.000	1.000
Amihud Illiquidity	8,582	0.011	0.114	0.000	0.000	0.001	0.003	9.505
Auditor Variables								
Portfolio Size	10,437	48.012	43.093	1.000	8.000	35.000	78.000	141.000
Portfolio Concentration	10,427	0.311	0.293	0.000	0.090	0.189	0.451	1.000
Audit Variables								
Audit Indicator	17,168	0.698	0.459	0.000	0.000	1.000	1.000	1.000
Audit Report Length	7,932	5.023	0.709	1.386	4.522	5.234	5.493	7.201
Audit Report Lag	16,225	5.021	1.069	0.000	4.060	5.940	5.940	5.940
Client-Auditor Distance	10,274	5.202	1.448	−1.265	4.723	5.132	6.182	8.997
Client-Auditor Specialist	17,168	0.284	0.451	0.000	0.000	0.000	1.000	1.000
Economic Position	7,932	0.345	0.475	0.000	0.000	0.000	1.000	1.000
GAAP	7,932	0.202	0.402	0.000	0.000	0.000	0.000	1.000
HHI Topics	7,914	0.144	0.031	0.111	0.121	0.137	0.157	0.358

Panel C: Descriptive Statistics, Pre-1934								
	N	**Mean**	**S.D.**	**Min.**	**Q1**	**Med.**	**Q3**	**Max.**
Company Variables								
Size (Market Value)	6,934	2.650	1.805	−4.382	1.489	2.723	3.864	8.056
EPS	2,627	3.181	5.722	−21.950	0.100	2.230	5.340	78.880
Dividend Payer	2,627	0.574	0.495	0.000	0.000	1.000	1.000	1.000
Zero Return Days	6,915	0.404	0.432	0.000	0.000	0.167	1.000	1.000
Zero Volume Days	6,915	0.454	0.439	0.000	0.000	0.250	1.000	1.000
Amihud Illiquidity	4,694	0.011	0.146	0.000	0.000	0.000	0.002	9.505
Auditor Variables								
Portfolio Size	5,410	30.207	27.763	1.000	6.000	21.000	47.000	106.000
Portfolio Concentration	5,400	0.355	0.303	0.000	0.110	0.237	0.508	1.000
Audit Variables								
Audit Indicator	10,717	0.594	0.491	0.000	0.000	1.000	1.000	1.000
Audit Report Length	4,009	4.735	0.690	1.386	4.304	4.635	5.273	7.172
Audit Report Lag	10,139	5.212	1.031	0.000	4.234	5.940	5.940	5.940
Client-Auditor Distance	5,167	5.341	1.437	−1.265	4.864	5.237	6.249	8.987
Client-Auditor Specialist	10,717	0.276	0.447	0.000	0.000	0.000	1.000	1.000
Economic Position	4,009	0.431	0.495	0.000	0.000	0.000	1.000	1.000
GAAP	4,009	0.030	0.171	0.000	0.000	0.000	0.000	1.000
HHI Topics	4,001	0.140	0.037	0.111	0.117	0.124	0.146	0.358

Panel D: Descriptive Statistics, Post-1934								
	N	**Mean**	**S.D.**	**Min.**	**Q1**	**Med.**	**Q3**	**Max.**
Company Variables								
Size (Market Value)	4,326	2.357	1.905	−4.358	1.107	2.381	3.636	8.148
EPS	2,758	1.883	2.868	−15.820	0.490	1.600	2.840	32.430
Dividend Payer	2,758	0.700	0.458	0.000	0.000	1.000	1.000	1.000
Zero Return Days	4,620	0.245	0.359	0.000	0.000	0.083	0.250	1.000
Zero Volume Days	4,620	0.252	0.362	0.000	0.000	0.083	0.250	1.000
Amihud Illiquidity	3,888	0.010	0.055	0.000	0.000	0.001	0.004	2.000
Auditor Variables								
Portfolio Size	5,027	67.173	48.145	1.000	18.000	75.000	114.000	141.000
Portfolio Concentration	5,027	0.264	0.275	0.000	0.077	0.144	0.339	1.000
Audit Variables								
Audit Indicator	6,451	0.872	0.334	0.000	0.000	1.000	1.000	1.000
Audit Report Length	3,923	5.316	0.601	1.386	5.088	5.323	5.656	7.201
Audit Report Lag	6,086	4.704	1.056	0.000	3.912	4.331	5.940	5.940
Client-Auditor Distance	5,107	5.061	1.446	−1.265	4.587	4.977	5.950	8.997
Client-Auditor Specialist	6,451	0.297	0.457	0.000	0.000	0.000	1.000	1.000
Economic Position	3,923	0.257	0.437	0.000	0.000	0.000	1.000	1.000
GAAP	3,923	0.378	0.485	0.000	0.000	0.000	1.000	1.000
HHI Topics	3,913	0.149	0.024	0.111	0.134	0.146	0.160	0.326

The table presents the descriptive statistics for the variables used in the analyses. Panel A gives an overview of the sample. We start with annual reports from Mergent and ProQuest, and we use the outer-join of both as our full sample of annual reports. Auditor data are proxied from the audit statements attached to the annual reports. Sector, trading venue, and market data are taken from Global Financial Data (GFD). Panel B presents the descriptive statistics for the full sample period, Panel C presents the descriptive statistics for the pre-1934 period, and Panel D presents the descriptive statistics for the post-1934 period. Variables are grouped on the level on which they are defined: 'company variables' are defined on the company-year level, 'auditor variables' are defined on the auditor-year level, and 'audit variables' are defined on the company-auditor-year level. See Appendix 3.A1 for detailed definitions of the variables.

3.5 Findings

3.5.1 Audit Rates

We start our exploration of the auditing landscape around the SEC's introduction by examining public companies' propensity to hire an auditor. In Figure 3.1, we plot the fraction of public companies with an audit over the period 1900 to 1940. We observe that less than 30% of public companies obtained an audit in 1900. This rate, however, increased gradually over time, reaching 80% in 1933, just before the SEC audit mandate. This high audit rate is consistent with historical accounts in Wiesen (1978) and cross-sectional evidence in Benston (1969) and Barton and Waymire (2004), validating our NLP-based audit rate measure. Notably, we do not observe a stark jump in the audit rate after the SEC imposed its audit mandate in 1934. While the audit rate increased around those years, the increase does not appear abnormal when seen in the context of the long-run trend observed over decades.[48] We observe similar trends for the market-capitalization-weighted fraction of audited companies, with the exception of a more notable increase after 1934. This increase, however, is still only about 10% of the entire market capitalization. We also caution that the value-weighted fraction in general is more variable, because a few large companies have a greater influence on it than on the equally weighted fraction.[49]

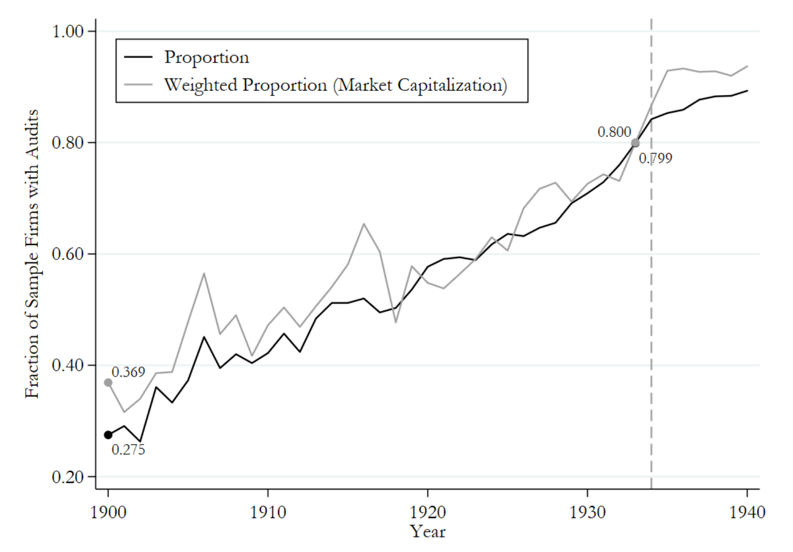
We corroborate the graphical impression with statistical tests for changes in the audit rate around 1934 in Table 3.2. In Panel A, we find that the average audit rate before 1934 is 71.5% (column 1). After 1934, this rate is about 15.6% higher. When we control for the long-run time trend observed in Figure 3.1,[50] the estimated increase shrinks to 4.5% (column 2). Further controlling for firm characteristics does not materially change this estimate (columns 3 and 4).

[48] The long-run trend in audit rates is positively associated with concurrent trends in aggregate market capitalization and the number of auditors (see Table 3.1 of the Online Appendix) (e.g., Rajan & Zingales, 2003). In addition, audit rates appear to increase after corporate scandals (especially accounting scandals) (see Table 3.2 of the Online Appendix), consistent with auditors' role as gatekeepers ensuring investor trust.

[49] Table 3.3 of the Online Appendix provides a breakdown of the audit-rate trend by entering, continuing, and exiting companies. It documents that the market-wide audit-rate increase reflects a secular trend toward auditing among all types of companies (entering, continuing, and exiting). Interestingly, the rate at which continuing companies switch toward obtaining an audit appears to pick up slightly in the later years. Notably, this acceleration in the adoption rate had already started in 1929, consistent with increased demand for auditing in response to securities market crashes.

[50] This time trend control counts from 1934. Thus it is -1 for 1933, 0 for 1934, 1 for 1935, and so on.

Figure 3.1: Audit Rate



The figure shows the fraction of companies in our sample that have been audited, proxied by the attachment of an audit statement to their annual report, over time. The proportion is calculated in two ways: as a proportion in terms of number of sample companies, and as a proportion in terms of total sample market capitalization. The dashed line indicates 1934, the year of the Securities Exchange Act and the audit mandate imposed by the Securities and Exchange Commission.

Taken together, the time-series evidence in Panel A suggests that the impact of the SEC's audit mandate on the market-wide audit rate was limited, ranging from 4.3 to 6.0%, after we control for the long-run time trend in the audit rate.

To sharpen the identification of the SEC impact, we test for differential changes in the audit rates of companies subject to the mandate vis-a-vis companies not subject to the mandate (the OTC market[51] and

[51] Section 13 of the original Securities Exchange Act, which allows the SEC to require audits, applies to "[e]very issuer of a security registered on a national securities exchange."

the transportation sector[52]) around 1934. We also compare companies listed on the NYSE to those listed on other exchanges (excluding OTC companies), since the NYSE had been asking listing companies to commit to annual audits since April 1932 (see p. 19 of Forbes, 1934). Relative to the respective control groups, mandated companies exhibit a small and statistically insignificant increase in audit rates, which amounts to 5.7% in column 2 (sample: full; control: OTC) and 7.9% in column 4 (sample: non-OTC; control: transportation sector). Similarly, column 6 shows that non-NYSE companies, relative to already-mandated NYSE companies, exhibit a small and statistically insignificant 3.4% increase in audit rates. These difference-in-differences results confirm our time-series evidence.

Collectively, our audit-rate results suggest that the SEC's audit mandate had a limited impact on market-wide audit rates. The impact was limited because, even absent a mandate, there was a long-run trend toward public company auditing, which led to pervasive auditing of public companies by the time the SEC was introduced.

[52] The SEC did not require audits for railroads or other entities regulated by the Interstate Commerce Commission (17 C.F.R. §240.13b-1(b) (1938)). For almost thirty years, these companies had already been subject to inspection by examiners from the Interstate Commerce Commission.

Table 3.2: Audit Rate

Panel A: Time-Series Difference				
	Audit Indicator			
	(1)	**(2)**	**(3)**	**(4)**
Post 1934	0.156***	0.045***	0.043***	0.060***
	(14.27)	(3.99)	(4.45)	(4.36)
Size				0.004
				(0.41)
EPS				−0.005***
				(−2.67)
Dividend Payer				0.040
				(1.20)
Constant	0.715***	0.760***	0.764***	0.823***
	(53.21)	(62.95)	(150.60)	(26.26)
N	11,140	11,140	10,989	4,592
R^2	0.038	0.044	0.687	0.615
Time Trend	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes

Panel B: Difference-in-Differences						
	Audit Indicator					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Non-OTC	0.190***					
	(2.89)					
Non-OTC × *Post 1934*	−0.032	0.057				
	(−0.69)	(1.63)				
Non-Transportation			0.304***			
			(4.34)			
Non-Transportation × *Post 1934*			0.062	0.079		
			(1.09)	(1.43)		
Non-NYSE					−0.132***	
					(−4.79)	
Non-NYSE × *Post 1934*					0.057**	0.034
					(2.51)	(1.55)
Constant	0.650***	0.776***	0.858***	0.808***	0.495***	0.774***
	(13.20)	(41.15)	(72.91)	(149.60)	(8.38)	(26.07)
N	11,140	10,989	10,417	10,283	10,417	10,283
R^2	0.057	0.689	0.065	0.673	0.089	0.673
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes

The table presents audit rate changes around the SEC's introduction. Panel A presents the time-series differences in audit rates, pre- and post-1934 for the full sample of companies. Panel B presents the results for difference-in-differences specifications using various control groups: companies trading on the OTC (versus all other companies), transportation companies trading on regular exchanges (versus all non-transportation companies trading on regular exchanges), and companies trading on the NYSE (versus all other companies trading on regular exchanges other than the NYSE). Models (1)–(4) in Panel A add increasingly stringent controls: Model (1) is the base model, Model (2) adds a time-trend (which takes a value of 0 in 1934), Model (3) adds firm-fixed effects, and Model (4) adds time-varying company controls. Models (1)–(6) in Panel B estimate the difference-in-differences specifications with year-fixed effects ((1), (3) and (5)) and additional firm-fixed effects ((2), (4) and (6)). *Audit Indicator* is a dummy variable that is equal to one if a company is audited, proxied by the attachment of an audit statement to the annual report, and zero otherwise. *Post 1934* is a dummy variable that is equal to one if the year is later than 1933, and zero otherwise. *Size* is the natural log of a company's market capitalization. *EPS* is a company's earnings per share. *Dividend Payer* is a dummy variable that is equal to one if a company pays a dividend, and zero otherwise. *Non-OTC* is a dummy variable that is equal to one for companies trading on regular exchanges, and zero for companies trading on the OTC market. *Non-Transportation* is a dummy variable that is equal to one for non-transportation companies trading on regular exchanges, and zero for transportation companies trading on regular exchanges. *Non-NYSE* is a dummy variable that is equal to one for companies trading on regular exchanges other than the NYSE market, and zero for companies trading on the NYSE market. We drop companies trading on the OTC market from the sample for models (3)–(6). See Appendix 3.A1 for detailed definitions of the variables. Standard errors are clustered at the company level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

3.5.2 Auditor Choices

We next explore how the companies chose their auditors.[53] This exploration helps us understand whether companies differentiated between the offerings of various auditors, and whether they purposely selected audi-

[53] The match between companies and auditors is not a one-sided choice by companies. Auditors, however, are less likely to actively choose their clients (as more is typically better) than companies are to choose their auditors, because companies chose only one auditor out of several options. Accordingly, we refer to our match analysis as an analysis of companies' auditor choice for the sake of simplicity.

tors with characteristics like independence and competence that promised greater levels of assurance—and thus greater value to the investors who relied on the companies' financial statements.

We generate a dyadic data structure, which includes one observation for each possible company-auditor pairing in a given year, to study the characteristics determining companies' auditor choices.[54] In Table 3.3, we regress an indicator variable that is equal to one for a given company's actual auditor (and zero for all other possible auditors) on company (e.g., size), auditor (e.g., portfolio size), and company-auditor-specific characteristics (e.g., distance between company and auditor).[55] Importantly, in defining the auditor characteristics, we exclude each company's own impact on its auditor's size, concentration, distance, and industry specialization measures. This adjustment reduces concerns about a mechanical relation between a company's auditor choice and the auditor's characteristics.[56] It, however, does not address the fact that auditors with larger portfolio sizes are more likely to be chosen by the average company in our sample. Accordingly, if there is a notable concentration of audit engagements among a few large auditors (as

[54] Dyadic models have been widely used in the social sciences to understand the relation between pairs of actors. Recent work, for example, uses such models to examine determinants of team formation in venture capital (Gompers, Mukharlyamov, & Xuan, 2016) and audit firms (Downar, Ernstberger, & Koch, 2021).

[55] The sample across the different specifications in Table 3.3 is restricted to companies with audited financial statements in a given year and information on company characteristics (e.g., earnings per share).

[56] Table 3.4 of the Online Appendix documents the results using raw and lagged auditor characteristics separately.

suggested in Section 3.4), we should expect companies' auditor choice to be positively related to auditors' portfolio size. While this relation could be viewed as mechanical, we note that it reflects the audit market structure, which may be endogenously driven by companies' preference for and choice of large auditors.[57]

In Panel A, we first examine the determinants of companies' auditor choices across our entire sample period. We find that company characteristics, such as size, earnings per share, and an indicator for dividend-paying companies, do not explain companies' auditor choices. Accordingly, larger companies, for example, do not systematically choose one auditor (e.g., Price Waterhouse) over another (e.g., Ernst & Ernst). By contrast, auditor and company-auditor-specific characteristics are significantly associated with companies' auditor choices. In particular, we find that public companies are more likely to choose auditors with larger client portfolios and lower client-portfolio concentration. This is consistent with companies preferring to pick auditors with lower dependence on any one of their clients. We further find that public companies are more likely to choose auditors with offices located closer to their headquarters and auditors that specialize in their respective sectors. This

[57] After controlling for auditors' portfolio size, the relation between companies' auditor choice and their auditor's portfolio concentration, by contrast, is less likely to reflect a mechanical relation. Holding portfolio size constant, auditors can service companies of comparable or distinct sizes. Hence, the relation between companies' auditor choice and their auditors' portfolio concentration plausibly captures the extent to which companies (or their auditors) care about portfolio concentration.

is consistent with companies preferring auditors with greater expertise in their local markets and their lines of business.

In Panel B, we next examine whether companies' auditor choices differ before and after the Securities and Exchange Act of 1934. We find some evidence that auditors' portfolio concentration, distance, and specialization all matter more in the period before the Securities and Exchange Act than after. This can be inferred from the fact that the coefficients —though not always significant— tend to take the opposite sign when we interact the company, auditor, and company-auditor characteristics with a post-1934 indicator. For example, *Client-Auditor Distance* has a coefficient of -0.005, but its interaction with *Post 1934* has a coefficient of the opposite sign: 0.001 (column 1).

Taken together, the auditor-choice results are consistent with public companies favoring auditors with characteristics reflecting independence (large, dispersed portfolio; DeAngelo, 1981) and competence (local and industry expertise; Rajgopal et al., 2021; Solomon et al., 1999). Auditors with these characteristics can be expected to provide greater assurance to companies' investors. Companies' attention to these characteristics, in turn, can be expected to incentivize auditors to ensure their independence and invest in their competence. Interestingly, public companies appear to pay special attention to auditors' independence and competence in the period before the SEC. After the SEC's introduction, these characteristics appear, if anything, less relevant for companies' auditor choices.

Table 3.3: Auditor Choice

Panel A: Full Sample				
	Auditor Choice			
	(1)	**(2)**	**(3)**	**(4)**
Size	0.000		−0.001	
	(−0.17)		(−0.73)	
EPS	0.000		0.000	
	(−0.24)		(−0.37)	
Dividend Payer	0.000		−0.001	
	(−0.01)		(−0.56)	
Portfolio Size	0.004**	0.004**		
	(2.36)	(2.00)		
Portfolio Concentration	−0.048***	−0.043***		
	(−3.68)	(−3.52)		
Client-Auditor Distance	−0.004***	−0.007***	−0.003***	−0.005***
	(−6.41)	(−4.73)	(−4.22)	(−3.74)
Client-Auditor Specialist	0.003	0.004	0.005*	0.006**
	(1.18)	(1.32)	(1.92)	(2.08)
N	151,829	151,829	151,796	151,796
R^2	0.044	0.049	0.201	0.207
Year FE	Yes	No	No	No
Firm-Year FE	No	Yes	No	Yes
Auditor-Year FE	No	No	Yes	Yes

Panel B: Pre- and Post-1934				
	Auditor Choice			
	(1)	**(2)**	**(3)**	**(4)**
Size	0.000		−0.001	
	(−0.29)		(−0.98)	
Size × Post 1934	0.000		0.001*	
	(0.63)		(1.81)	
EPS	0.000		0.000	
	(−0.33)		(−0.22)	
EPS × Post 1934	0.000		0.000	
	(0.37)		(−0.33)	
Dividend Payer	0.001		−0.001	
	(0.26)		(−0.23)	
Dividend Payer × Post 1934	−0.001		−0.001	
	(−0.62)		(−0.35)	
Portfolio Size	0.004**	0.003*		
	(2.38)	(1.77)		
Portfolio Size × Post 1934	0.001	0.001		
	(0.70)	(1.00)		
Portfolio Concentration	−0.058***	−0.050***		
	(−3.52)	(−3.38)		
Portfolio Concentration × Post 1934	0.016*	0.012		
	(1.69)	(1.41)		
Client-Auditor Distance	−0.005***	−0.008***	−0.003***	−0.006***
	(−5.91)	(−4.06)	(−4.08)	(−3.25)
Client-Auditor Distance × Post 1934	0.001**	0.003***	0.001	0.002*
	(2.22)	(2.04)	(1.61)	(1.80)
Client-Auditor Specialist	0.005	0.005*	0.006**	0.007**
	(1.60)	(1.68)	(2.01)	(2.35)
Client-Auditor Specialist × Post 1934	−0.004	−0.003	−0.002	−0.002
	(−1.32)	(−1.19)	(−0.50)	(−0.56)
N	151,829	151,829	151,796	151,796
R^2	0.045	0.050	0.201	0.207
Year FE	Yes	No	No	No
Firm-Year FE	No	Yes	No	Yes
Auditor-Year FE	No	No	Yes	Yes

The table presents determinants of companies' auditor choice. The estimates are based on a dyadic regression model. This model includes all possible company-auditor matches in a given year. The dependent variable *Auditor Choice* is equal to zero for all auditors, except for the auditor that is chosen by the company. The explanatory variables contain company-specific variables (*Size*, *EPS*, and *Dividend Payer*), auditor-specific variables (*Portfolio Size* and *Portfolio Concentration*), and company-auditor-specific variables (*Client-Auditor Distance* and *Client-Auditor Specialist*). *Portfolio Size* is the logarithm of the sum of the market capitalization of all companies in an auditor's client portfolio in a given year. *Portfolio Concentration* is the sum of squared client shares (client capitalization over an auditor's total portfolio size) of a given auditor in a given year. *Client-Auditor Distance* is the logarithm of the geodetic distance between the city of the headquarters of the company and the city of the auditor's office that is closest the company, out of all cities in which the auditor has an office. *Client-Auditor Specialist* is an indicator variable that is equal to one if the auditor is a specialist in the sector in which the company is active, and zero otherwise. The auditor is considered to be a specialist in the sector for which the proportion of total portfolio size (in terms of market capitalization) in that sector to the total auditor portfolio is largest. See Appendix 3.A1 for detailed definitions of the variables. All variables are adjusted for the mechanical effect of each company on its actual auditor's characteristics (e.g., portfolio size). Models (1)–(4) control for increasingly stringent fixed effects: Model (1) includes year-fixed effects, Model (2) includes firm-year-fixed effects, Model (3) includes auditor-year-fixed effects, and Model (4) includes firm-year- and auditor-year-fixed effects. Panel A shows the results for the full sample of audited companies. Panel B includes an interaction term for the post-1934 period, to allow for changes in the association between the explanatory variables and the *Auditor Choice* around the Securities and Exchange Act of 1934. Standard errors are two-way clustered at the company and auditor level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

3.5.3 Audit Services

In addition to audit rates and auditor choices, we examine the services provided by public company auditors around the SEC's introduction. This examination allows us to paint a more complete picture of the auditing landscape and the SEC's impact on it. While the SEC appears to have had a limited impact on audit rates and auditor choices, it may have had a substantial impact on the audit services and practices at the time, as conjectured in Benston (1969).

We exploit our textual data to learn about audit services and practices from the characteristics and content of public companies' audit statements. While clearly limited, focusing on the audit statements attached to the annual reports provides us with a window to auditors' notoriously hard-to-observe services and practices, enabling the first large-scale investigation of reported services and practices in the early audit market.

In Table 3.4, we examine changes in the characteristics and content of audit statements around the SEC's introduction in 1934. In Panel A, we document that audit statements significantly increased in length after 1934 (an increase of around 49%, or 98 more words in column 4). Despite an increase in length, we do not find a clear change in the timing of the audit statement. At best, we find a marginal increase in the time between companies' fiscal year ends and auditors' sign-off dates (an increase of 8%, or 15 more days in column 4). These findings suggest that while audit statements became longer after 1934, the underlying work may not have increased significantly. In line with this interpretation, Table 3.5 in the Online Appendix documents that the number of certified public accountants (CPAs) per public company did not significantly increase after the introduction of the SEC, once we control for the time-trend. This finding suggests that auditors do not seem to have contracted more CPAs to increase the supply of labor, in order to do more work in the same time window.

To better understand the drivers of the increased audit statement length, we next investigate specific changes in the content of the audit statements. We use three approaches to dissect the content. Our first supervised approach involves reading a sample of audit statements to identify key terms (e.g., financial position, accounting standards, etc.). Equipped with manually selected terms, we search all statements for these terms. (Appendix 3.A2 summarizes the search terms.) Our second unsupervised approach uses a standard topic modeling approach, Latent Dirichlet Allocation (LDA), to uncover common clusters of terms appearing in the audit statements. Based on the respective terms, we assign each cluster a descriptive topic label. (The caption to Figure 3.2 summarizes the terms per topic and our labels.) Our final approach involves calculating the (cosine) similarity between companies' audit statements and the standard audit statement formats proposed by various private actors (e.g., the AIA) and reported in Carmichael and Winters (1982).[58] The average similarity of companies' audit statements and the standard format provides a measure of standardization of audit formats over time.

In Panel B, we report the results of our content analysis. We find that auditors shifted from attesting companies' financial (or economic)

[58] The various versions of the standard audit format were proposed by the Federal Reserve Board in 1917, the AIA in 1929, and the AIA in collaboration with the NYSE in 1931, 1934, and 1936. See Pandit and Baker (2021) for a history of standard audit reports in the U.S. up to the present.

position to opining on companies' compliance with GAAP around 1934 (see also Figure 3.3). A clear example of this shift can be found in Figure 3.A1 in the Appendix, where the same auditor auditing the same company changed from expressing an opinion on financial condition in 1932 to expressing an opinion on compliance with GAAP in 1935. We further find that audit statements became more concentrated on fewer topics after 1934, as evidenced by both an increased concentration of topics discussed in companies' audit statements and an increased focus on the most dominant topic in the typical audit statement.[59]

[59] For each audit statement, the LDA assigns a value (ranging from 0 to 1) to each of the nine topics corresponding to the relative likelihood with which each of the topics is discussed in a given statement. The *HHI Topics* variable captures the concentration of these likelihoods, while the *Dominant Topic Distribution* captures the likelihood assigned to the most likely topic. Both variables capture important aspects of topic concentration. The *Dominant Topic Distribution* variable considers the concentration at the top (i.e., the maximum probability), whereas the *HHI Topics* variable captures the concentration of the entire distribution over the topics.

Table 3.4: Audit Services

Panel A: Audit Statement Characteristics

	Audit Statement Length				*Audit Statement Lag*			
	(1)	**(2)**	**(3)**	**(4)**	**(1)**	**(2)**	**(3)**	**(4)**
Post 1934	0.575***	0.506***	0.510***	0.492***	−0.234***	−0.044	0.064	0.082**
	(23.20)	(14.38)	(14.67)	(13.87)	(−7.91)	(−1.22)	(1.58)	(2.17)
N	6,145	6,145	6,134	5,979	10,487	10,487	7,305	7,163
R^2	0.157	0.158	0.265	0.562	0.011	0.014	0.058	0.540
Time Trend	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Auditor FE	No	No	Yes	Yes	No	No	Yes	Yes
Firm FE	No	No	No	Yes	No	No	No	Yes

Panel B: Audit Statement Content

	Economic Position				*GAAP*			
	(1)	**(2)**	**(3)**	**(4)**	**(1)**	**(2)**	**(3)**	**(4)**
Post 1934	−0.153***	−0.120***	−0.115***	−0.103***	0.324***	0.248***	0.247***	0.231***
	(−8.34)	(−5.46)	(−5.16)	(−5.17)	(20.90)	(12.27)	(12.08)	(11.93)
N	6,181	6,181	6,135	5,980	6,181	6,181	6,135	5,980
R^2	0.025	0.025	0.091	0.707	0.127	0.129	0.181	0.703
Time Trend	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Auditor FE	No	No	Yes	Yes	No	No	Yes	Yes
Firm FE	No	No	No	Yes	No	No	No	Yes

	HHI Topics				***Dominant Topic Distribution***			
	(1)	**(2)**	**(3)**	**(4)**	**(1)**	**(2)**	**(3)**	**(4)**
Post 1934	0.014***	0.003**	0.003**	0.002	0.040***	0.008**	0.009**	0.007*
	(12.35)	(2.00)	(2.19)	(1.37)	(14.00)	(2.09)	(2.32)	(1.77)
N	6,132	6,132	6,121	5,964	6,132	6,132	6,121	5,964
R^2	0.060	0.073	0.119	0.415	0.075	0.092	0.135	0.412
Time Trend	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Auditor FE	No	No	Yes	Yes	No	No	Yes	Yes
Firm FE	No	No	No	Yes	No	No	No	Yes

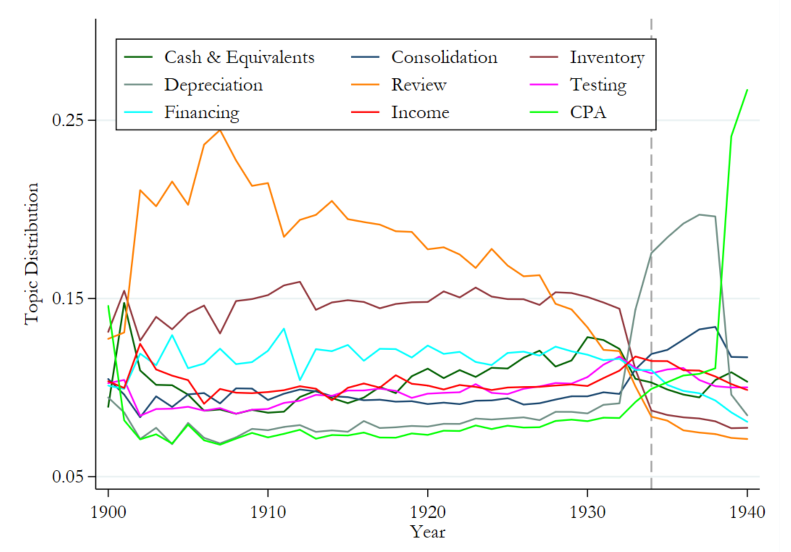
Panel C: Audit Statement Difference-in-Differences (Non-OTC v. OTC)

	Audit St. Length	***Audit St. Lag***	***Economic Position***	***GAAP***	***HHI Topics***
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Non-OTC × Post 1934	−0.094	−0.068	0.032	0.035	−0.005
	(−1.01)	(−0.59)	(0.49)	(0.38)	(−1.05)
N	5,979	7,164	5,978	5,978	5,966
R^2	0.579	0.539	0.710	0.718	0.442
Year FE	Yes	Yes	Yes	Yes	Yes
Auditor FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes

The table presents changes in audit services around the SEC's introduction. Panels A and B present time-series differences in the characteristics and content of audit statements, respectively. Panel C presents difference-in-differences specifications using companies traded on the OTC market as a control. All coefficients are estimated using the sample of audit statements between 1927 and 1940. Models (1)–(4) in Panel A and Panel B add increasingly stringent fixed effects: Model (1) is the base model, Model (2) adds a time-trend, Model (3) adds auditor-fixed effects, and Model (4) adds firm-fixed effects. Models (1)–(6) in Panel C estimate the most stringent specification for all audit statement variables. *Audit Statement Length* is the natural log of the total number of words in the audit statement. *Audit Statement Lag* is the natural log of the number of days between the auditor's sign-off date and the end of the company's fiscal year. *Economic Position* is a dummy variable that is equal to one if the audit statement contains any of the words that are associated with the company's economic position, and zero otherwise. *GAAP* is a dummy variable that is equal to one if the audit statement contains any of the words that are associated with compliance with Generally Accepted Accounting Principles, and zero otherwise. Table 3.A2 in Appendix B gives an overview of the words that are associated with these two categories. *HHI Topics* is the Hirschman-Herfindahl Index of the probability that each of the identified nine topics is contained in the audit statement. *Dominant Topic Distribution* is the probability that the topic with the highest probability is contained in the audit statement. The nine topics are identified with Latent Dirichlet allocation (LDA) using the full sample of audit statements, and named based on the five most common words associated with the topic. *Post 1934* is a dummy variable that is equal to one if the year is later than 1933, and zero otherwise. *Non-OTC* is a dummy variable that is equal to one for companies trading on regular exchanges, and zero for companies trading on the OTC market. See Appendix 3.A1 for detailed definitions of the variables. Standard errors are clustered at the company level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

In Figure 3.2, we plot the various topics, identified by our LDA approach, over time. Consistent with our regression results in Panel B, we observe a greater plurality of topics discussed in earlier years, and an increasing convergence to a few topics over time. The dominating topics emerging in the later years are related to depreciation and generally accepted accounting principles. Notably, these patterns appear to reflect concurrent developments in the profession (e.g., Hatfield, 1936; Hilke, 1986).

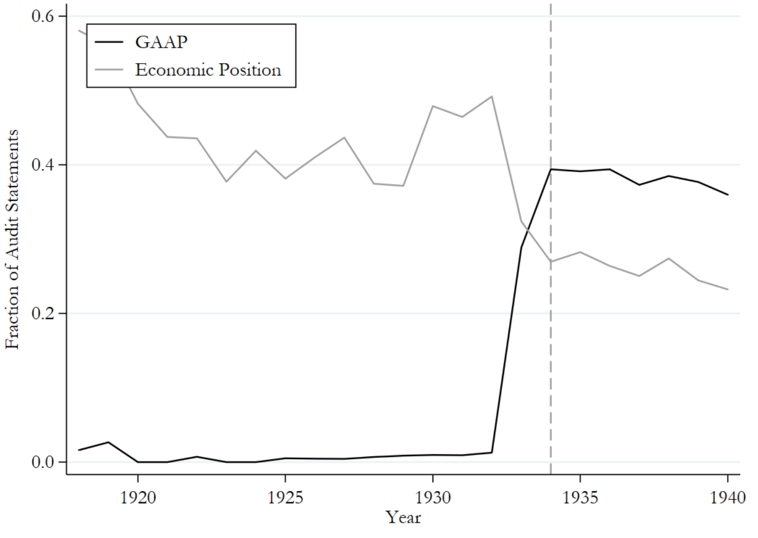
Figure 3.2: Audit Statement Topic Distribution



The figure shows the probability distribution of the nine topics that are discussed in the sample of audit statements over time. The nine topics are identified with Latent Dirichlet allocation (LDA) using the full sample of audit statements, and named based on the five most common words associated with the topic. The topics (associated words) are *Cash & Equivalents* ('provision', 'security', 'cash', 'certificate', 'verify'), *Consolidation* ('report', 'examination', 'consolidate', 'asset', 'foreign'), *Inventory* ('inventory', 'cost', 'price', 'market', 'quantity'), *Depreciation* ('depreciation', 'amount', 'reserve', 'property', 'charge'), *Review* ('examination', 'information', 'accounting', 'review', 'obtain'), *Testing* ('accounting', 'test', 'precede', 'method', 'control'), *Financing* ('stock', 'liability', 'share', 'capital', 'note'), *Income* ('loss', 'profit', 'transaction', 'review', 'support'), *CPA* ('certify', 'book', 'accountant', 'public', 'condition'). The dashed line indicates 1934, the year of the Securities Exchange Act.

The trend toward harmonized practices, the use of depreciation, and the promulgation of GAAP all started before the SEC (see also Figure 3.3).

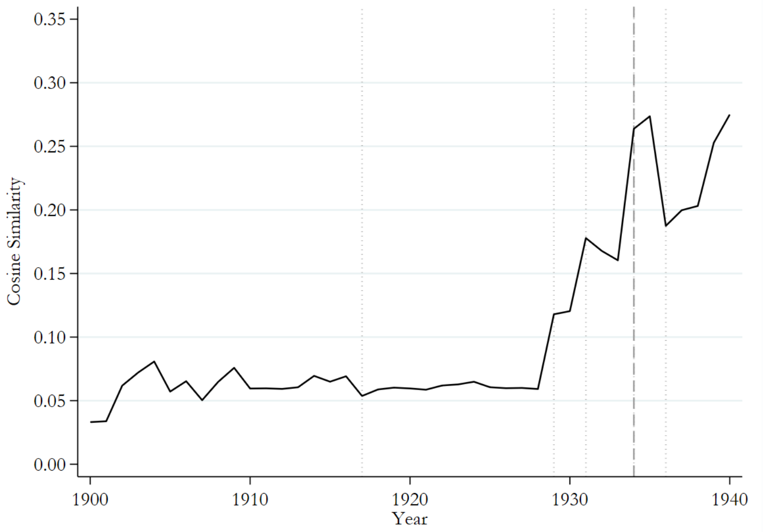
Figure 3.3: Attestation of 'Economic Position' vs. 'GAAP' Compliance



The figure shows the fraction of audit statements in our sample that mention any of the words that are associated with compliance with Generally Accepted Accounting Principles ('GAAP'), or with a company's 'economic position', over time. Appendix 3.A2 gives an overview of the words that are associated with these two categories. The dashed line indicates 1934, the year of the Securities Exchange Act.

In Figure 3.4, we plot the average similarity of companies' audit statements and the standard audit format over time. Consistent with our previous findings, we observe a strong increase in standardization around the SEC's introduction. Importantly, however, the standardization of audit formats appears to have started several years before the SEC's introduction. Indeed, the format suggested by the NYSE and the AIA in January 1934 preceded the passage of the Securities Exchange Act by several months.

Figure 3.4: Cosine Similarity of Audit Statements with Standard Statements from Carmichael and Winters (1982)



The figure shows the average cosine similarity between the audit statements attached to the annual reports in our sample in a particular year and the latest standard audit statement as reported by Carmichael and Winters (1982). The dashed line indicates 1934, the year of the Securities Exchange Act. The dotted lines indicate 1917, 1929, 1931, 1934, and 1936, years in which a new standard audit statement is proposed by the Federal Reserve Board (1917), the American Institute of Accountants (AIA) (1929), and the AIA/NYSE (1931, 1934, 1936).

It was the product of correspondence that had begun even before the passage of the Securities Act of 1933.[60] Consistent with our evidence, this standard audit statement, though optional, came into general use according to Montgomery (1940).

Taken together, our findings reveal notable changes in the characteristics and content of audit statements around 1934. Most notably, we observe a trend toward standardized audit statements and a shift

[60] This correspondence is preserved in a published collection, which shows the standard audit statement (see p. 47 of Forbes, 1934).

from attesting to economic positions toward opining on compliance with accounting standards, possibly lowering the level of assurance provided to investors.[61] These trends, however, were already occurring before 1934, which suggests that they cannot necessarily be attributed to the impact of the SEC. Rather, the changes in audit services and practices appear to reflect concurrent efforts by private-sector actors to standardize accounting and manage audit expectations (e.g., due to litigation concerns; Previts & Merino, 1998). Consistent with this interpretation, we do not find that companies regulated by the SEC experienced different trends than other companies outside of the SEC purview. In a series of difference-in-differences tests in Panel C of Table 3.4, we do not observe any significant differences in the changes of mandated companies' audit statements over time relative to the changes in the audit statements of companies traded on the OTC market that are not subject to the audit mandate.

Our findings align with historical accounts that the SEC focused primarily on enforcing the audit mandate rather than shaping audit practice, at least initially. According to those accounts, the SEC started taking a more active role in audit practices only after the *McKesson & Robbins* scandal in 1938 (e.g., Coffee, 2006). Notably, we observe a stark increase in standardization and the use of *certified* public accountants

[61] Pandit and Baker (2021) relate that the 1934 standard audit report reflected the accounting profession's new belief, in response to litigation, that it was unwise to certify financial statements and thus guarantee their accuracy.

after 1938 (Figure 3.2). In sum, our evidence suggests that the SEC, while possibly a catalyst for contemporaneous standardization efforts of the profession (e.g., due to the threat of litigation and intervention), had a limited *direct* impact on audit services and practices in its early years.

3.5.4 Market Quality

Finally, we investigate the capital-market outcomes (i.e., market value and liquidity) associated with public company auditing around the SEC's introduction. This investigation sheds light on the usefulness of public company auditing for improving companies' capital-market access and the functioning of capital markets as a whole.

Our investigation proceeds in three steps. We first examine differences in companies' characteristics and capital-market outcomes between voluntarily, mandatorily, and never audited companies (including non-compliant and non-mandated companies) around the SEC's introduction (seven years before and seven years after).[62] In Panel A of Table 3.5, we show the distribution of these three types of companies by trading venue. In Panel B, we provide descriptive statistics for and univariate differences between these groups. Focusing on the pre-1934 sample, which predates the SEC, we find that mandatory adopters are similar in size and prof-

[62] For a subset of the companies classified as non-compliant, we corroborate their status by manually checking their annual reports for audit statements, alleviating concerns that our NLP-based approach fails to detect these companies' audit statements.

itability as voluntary adopters, but smaller and less profitable than never adopters. Mandatory adopters are more likely to pay dividends than voluntary adopters, but less likely than never adopters. With respect to capital-market outcomes, we find that mandatory adopters exhibit lower liquidity than voluntary adopters, but higher liquidity than never adopters.

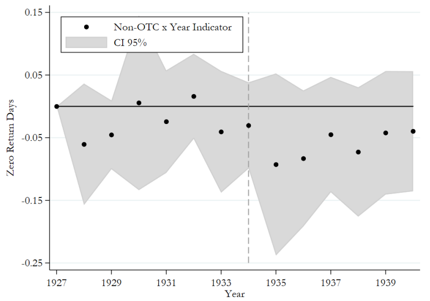
The pre-1934 differences between the voluntary adopters, on the one hand, and the mandatory and never adopters, on the other, suggest that companies with greater financing needs (i.e., smaller companies with lower profitability and dividend frequency) are more likely to rely on auditing. The univariate differences also provide prima facie evidence that auditing is useful in improving companies' access to capital markets, as documented by the fact that the voluntary adopters have the most liquid securities in the pre-SEC period. In this vein, we also find that the securities of mandatory adopters experience a significant improvement in liquidity after the SEC mandate. A similar improvement in liquidity, however, is also observed for voluntary adopters, so we should be cautious in interpreting this time-series change as evidence of the usefulness of *mandatory* auditing.

We next examine the change in capital-market outcomes of mandatory adopters around the SEC's introduction in a difference-in-differences design, controlling for concurrent changes experienced by voluntary adopters (which, by definition, had already voluntarily obtained audits before the SEC mandate). In Panel C, which contains this difference-in-differences, we find no significant evidence that the mandatory adopters experienced improvements in their market values or liquidity (i.e., zero return days, zero volume, Amihud illiquidity). Compared to never-audited companies, mandatorily audited companies show some weak evidence of liquidity improvement. These findings are consistent with the notion that mandatory audits have a limited impact on companies' capital-market outcomes and, hence, capital markets as a whole. The difference-in-differences findings, however, can fail to detect significant improvements if the mandate helps not only the mandated companies, but also other companies (e.g., voluntary adopters). In this case, we may not detect a significant effect, despite the mandate's beneficial impact on the entire regulated capital market.

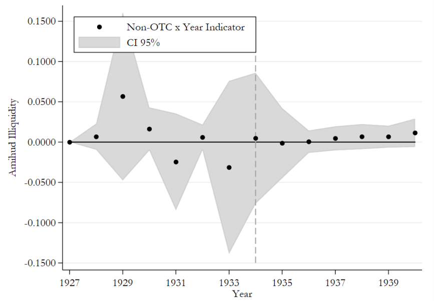
Finally, to explore the possibility of market-wide improvements, we examine the change in capital-market outcomes experienced by all companies trading on regulated markets around the SEC introduction. We compare this change with the concurrent change experienced by companies trading on the unregulated OTC market. In Panel D, we find limited evidence of a significant improvement in regulated markets as compared to the unregulated market. While there is some weak evidence of improved liquidity on average (columns 5 and 6), there is little evidence of aggregate liquidity improvements (i.e., when weighting companies with their relative market capitalization within their respective market).[63] Confirming these regression results, we do not observe notable differential trends for the average company traded on regulated exchanges vis-a-vis the OTC market around the SEC's introduction, nor the aggregate capital-market outcomes on these markets, in Figure 3.5.

[63] The weighting is supposed to achieve a measure of aggregate liquidity within the respective markets (OTC vs. non-OTC). In the tabulated results, we use fixed weights calculated as of 1927. The use of fixed weights allows us to home in on changes in aggregate liquidity, while abstracting from changes in market value due to sample composition changes (e.g., new listings). The fixed-weights approach reduces our sample size, though. In untabulated results, we find very similar results when using changing weights, which does not restrict our sample size. Allowing for changes in the sample composition yields, if anything, a slight deterioration in the aggregate liquidity of the regulated market relative to the OTC market.

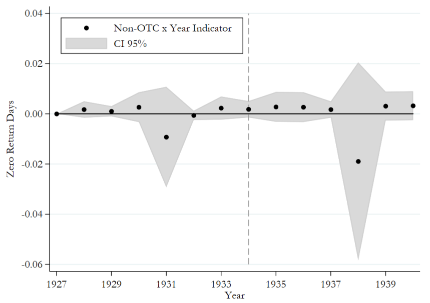
Figure 3.5: Capital-Market Quality



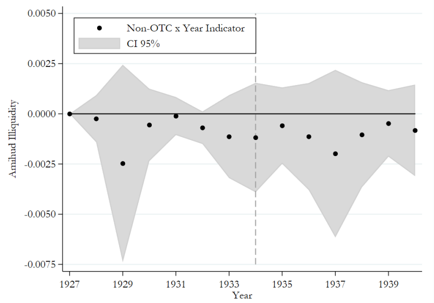
(a) Difference in *Zero Return Days*



(b) Difference in *Amihud Illiquidity*



(c) Weighted Diff. in *Zero Return Days*



(d) Weighted Diff. in *Amihud Illiquidity*

The figures compare the evolution of capital-market liquidity separately for companies trading on an exchange and stocks trading on the OTC market. The figures plot annual difference-in-differences coefficients, capturing the difference between companies traded on exchanges and those traded on the OTC market relative to the difference in the base year 1927. The underlying regressions account for firm and year fixed effects. The gray area provides the point-wise 95/5 confidence interval, based on standard errors clustered at the company level. Figures (a) and (b) are based on equally weighted company-year observations, whereas figures (c) and (d) are based on market-capitalization (within each market (exchanges vs. OTC) as of 1927) weighted company-year observations. The dashed line indicates 1934, the year of the Securities Exchange Act.

Table 3.5: Market Quality

Panel A: Trading Venues of Mandatory, Voluntary, and Never Adopters						
	Voluntary Adopters		Mandatory Adopters		Never Adopters	
Trading Venue	**Name**	**Obs.**	**Name**	**Obs.**	**Name**	**Obs.**
	ASE	485	ASE	122	ASE	31
	NYSE	4,788	NYSE	873	NYSE	241
	OTC	400	OTC	0	OTC	152
	Other	2,081	Other	469	Other	125
	Total	7,754	Total	1,464	Total	549

Panel B: Univariate Comparison (Mandatory Adopters vs. Others)								
		Mandatory Adopters	**Voluntary Adopters**	**Diff.**	**T-stat.**	**Never Adopters**	**Diff.**	**T-stat.**
		(1)	**(2)**	**(2) − (1)**		**(3)**	**(3) − (1)**	
Full Sample								
N	11,141	1,878	8,509			754		
Audited	0.805	0.708	0.898	0.190	22.24***	0.000	−0.708	−42.71***
Size (Market Value)	2.385	2.087	2.397	0.310	4.65***	2.989	0.902	7.18***
EPS	2.089	1.782	2.012	0.229	1.35	5.225	3.443	9.88***
Dividend Payer	0.691	0.652	0.690	0.037	1.77*	0.877	0.225	5.49***
Zero Return Days	0.245	0.353	0.197	−0.156	−15.11***	0.696	0.343	14.02***
Zero Volume Days	0.270	0.373	0.224	−0.149	−13.77***	0.714	0.341	13.91***
Amihud Illiquidity	0.012	0.018	0.010	−0.008	−4.06***	0.078	0.060	2.21**
Pre-1934 Sample								
N	4,713	395	4,014			304		
Audited	0.715	0.000	0.839	0.839	45.41***	0.000	0.000	
Size (Market Value)	2.410	2.460	2.369	−0.092	−0.64	3.146	−0.686	−2.90***
EPS	2.369	2.780	2.189	−0.590	−1.10	6.621	−3.841	−5.02***
Dividend Payer	0.680	0.854	0.664	−0.189	−3.59***	0.887	−0.034	−0.61
Zero Return Days	0.246	0.419	0.210	−0.210	−9.16***	0.686	−0.267	−5.99***
Zero Volume Days	0.297	0.472	0.261	−0.211	−8.44***	0.722	−0.250	−5.69***
Amihud Illiquidity	0.016	0.023	0.011	−0.011	−2.76***	0.172	−0.149	−1.53

		Mandatory Adopters	Voluntary Adopters	Diff.	T-stat.	Never Adopters	Diff.	T-stat.
		(1)	**(2)**	**(2) − (1)**		**(3)**	**(3) − (1)**	
Post-1934 Sample								
N	6,428	1,483	4,495			450		
Audited	0.871	0.896	0.950	0.054	7.44***	0.000	−0.896	−62.29***
Size (Market Value)	2.365	1.983	2.422	0.439	5.79***	2.867	−0.883	−5.76***
EPS	1.883	1.607	1.861	0.254	1.83*	4.030	−2.423	−5.90***
Dividend Payer	0.700	0.617	0.711	0.094	4.03***	0.867	−0.251	−4.50***
Zero Return Days	0.244	0.339	0.187	−0.152	−12.74***	0.703	−0.364	−11.86***
Zero Volume Days	0.251	0.352	0.193	−0.159	−13.22***	0.708	−0.356	−11.56***
Amihud Illiquidity	0.010	0.017	0.009	−0.008	−3.52***	0.004	−0.012	−1.34

Panel C: Market Quality Difference-in-Differences (Mandatory Adopters vs. Others)

	Market Value		*Zero Return Days*		*Zero Volume Days*		*Amihud Illiquidity*	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Mandatory vs. Voluntary Adopters								
Mandatory Adopter	0.017		0.200***		0.195***		0.012*	
	(0.05)		(3.45)		(3.48)		(1.77)	
Mandatory Adopter × Post 1934	−0.425	−0.075	−0.071	−0.018	−0.061	−0.004	−0.003	0.003
	(−1.49)	(−0.57)	(−1.35)	(−0.47)	(−1.20)	(−0.11)	(−0.40)	(0.46)
N	7,046	6,977	7,295	7,208	7,295	7,208	6,430	6,354
R^2	0.053	0.907	0.033	0.738	0.058	0.737	0.020	0.470
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes

	Market Value		*Zero Return Days*		*Zero Volume Days*		*Amihud Illiquidity*	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Mandatory vs. Never Adopters								
Mandatory Adopter	−0.750		−0.277***		−0.262***		−0.148	
	(−1.49)		(−2.85)		(−2.78)		(−0.94)	
Mandatory Adopter × Post 1934	−0.044	0.088	−0.090	−0.086*	−0.098	−0.087**	0.160	0.011
	(−0.12)	(0.48)	(−1.29)	(−1.90)	(−1.50)	(−2.04)	(1.02)	(0.73)
N	1,318	1,266	1,606	1,532	1,606	1,532	1,075	1,028
R^2	0.071	0.938	0.117	0.869	0.124	0.868	0.038	0.514
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes

Panel D: Market Quality Difference-in-Differences (Non-OTC vs. OTC)

	Market Value		*Zero Return Days*		*Zero Volume Days*		*Amihud Illiquidity*	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Unweighted								
Non-OTC	0.266		−0.444***		−0.401***		−0.026	
	(0.73)		(−4.13)		(−3.79)		(−0.85)	
Non-OTC × Post 1934	−0.001	0.157	−0.091	−0.038	−0.142**	−0.075**	0.010	0.006
	(−0.01)	(0.97)	(−1.34)	(−1.26)	(−2.14)	(−2.54)	(0.37)	(0.34)
N	7,611	7,527	7,893	7,780	7,893	7,780	6,654	6,568
R^2	0.048	0.908	0.075	0.790	0.088	0.784	0.004	0.497
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes

	Market Value		*Zero Return Days*		*Zero Volume Days*		*Amihud Illiquidity*	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Weighted								
Non-OTC	−0.687		−0.032**		−0.034**		−0.001*	
	(−1.23)		(−2.30)		(−2.46)		(−1.69)	
Non-OTC × *Post 1934*	0.004	0.004	0.000	0.000	0.001	0.001	0.000	0.000
	(0.31)	(0.31)	(−0.10)	(−0.10)	(0.88)	(0.88)	(−0.62)	(−0.62)
N	2,254	2,254	2,254	2,254	2,254	2,254	2,018	2,017
R^2	0.187	0.991	0.413	0.903	0.421	0.886	0.297	0.715
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	Yes	No	Yes	No	Yes

The table presents changes in capital-market quality around the SEC's introduction. It compares changes across regulated (non-OTC) and unregulated (OTC) markets and across three groups of companies: 'mandatory adopters' (companies trading on regular exchanges who only got an audit after the audit mandate), 'voluntary adopters' (companies trading on regular exchanges or the OTC market who got audits before the audit mandate), and 'never adopters' (non-compliant companies trading on regular exchanges and non-adopters on the OTC market). Panel A presents the sample composition, broken down by trading venue, of the three groups. Panel B presents descriptive statistics by group and univariate comparisons over time (pre vs. post 1934) and between groups ('mandatory adopters' vs. the two other groups). Panel C presents difference-in-differences specifications using 'voluntary adopters' or 'mandatory adopters' as control groups. Panel D presents difference-in-differences specifications comparing regulated (non-OTC) with unregulated (OTC) markets. The weighted specifications in Panel D are based on within-market (non-OTC vs. OTC) market-capitalization weights as of 1927. All estimates are based on the sample of audit statements between 1927 and 1940. The models in Panels C and D include year-fixed effects ((1), (3), (5), and (7)) and additionally firm-fixed effects ((2), (4), (6), and (8)). *Market Value* is the natural log of companies' market capitalization. *Zero Return Days* is the number of days on which companies' returns are zero, scaled by the total number of days for which there is data. *Zero Volume Days* is the number of days on which companies' trading volume is zero, scaled by the total number of days for which there is data. *Amihud Illiquidity* is the Amihud illiquidity measure calculated as in Amihud (2002). See Appendix 3.A1 for detailed definitions of the variables. Standard errors are clustered at the company level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

Collectively, the capital-market results suggest that the SEC's audit mandate had, at best, a limited impact on mandated companies and regulated capital markets.[64] Importantly, though, they do not suggest that auditing does not matter. By contrast, they are consistent with *voluntary* auditing helping companies' capital-market access. Thus, they explain the high fraction of voluntarily audited companies and echo

[64] Consistent with a limited impact on capital markets, we do not observe any improvements in the value relevance of mandatorily audited companies' earnings in value-relevance tests following Barth, Landsman, and Lang (2008) (Table **??** in the Online Appendix). Mandatorily audited companies' value relevance neither increases from the pre- to the post-SEC period, nor relative to that of other companies (e.g., voluntarily audited companies).

the earlier finding that companies appear to choose independent and competent auditors, which provide assurance to their dispersed investors.

3.6 Discussion

Our exploration of the landscape of public company auditing before the SEC's introduction suggests that public company auditing was flourishing, even absent any federal regulation. Public companies frequently obtained audits from presumably competent and independent auditors. While audit practices were quite diverse, sound practices were promoted early on (e.g., Montgomery, 1913) and refined through the collaboration and coordination of private-sector parties, such as the accounting associations and stock exchanges. Consistent with high quality auditing, companies with financing needs frequently purchased audits and boasted higher-liquidity securities than other companies.

Our investigation of changes in public company auditing around the SEC's introduction provides little support for the importance of the SEC's introduction for audit markets. The impact on audit markets appears limited because the vast majority of companies had already obtained an audit even before the SEC's audit mandate. In addition, the SEC did not appear to intervene into audit practice, at least initially. It primarily appeared to codify existing practices. The SEC started to take an interest in shaping audit practice only after a prominent accounting scandal in

1938. It remains unclear, though, whether this reactive intervention was successful (e.g., Hail et al., 2018). It is further unclear what the audit market reaction to such a scandal would have been absent the SEC (e.g., Ball, 1980).

In line with a limited impact on audit markets, our exploration provides little evidence to support the notion that the SEC improved mandatorily audited companies' capital-market access or trust in regulated securities markets as a whole. Our evidence echoes earlier findings suggesting that the SEC had a limited impact on companies' fraud and investors' trust in capital markets (e.g., Benston, 1969, 1973; G. J. Stigler, 1971). It provides an explanation for such limited impact: auditing was already flourishing before the regulatory intervention (just as companies' disclosures were; Benston, 1969; Hilke, 1986).

To be clear, our descriptive evidence does not imply that the SEC mandate had *no* impact on public company auditing, or that no institutional safeguards (e.g., the legal system) were needed (e.g., Merino et al., 1994; Mills, 1990). It rather suggests that the scope for federal regulation to aid capital markets by regulating public company auditing was limited given the development and functioning of the audit market in the pre-SEC era. This development was driven by several forces, including companies' financing needs, investors' information demands, stock exchanges' requirements, and court rulings. It may also have been aided by the threat of regulatory interventions (e.g., Carmichael & Winters, 1982).

We also want to be clear that our evidence does not mean that public company auditing is useless. To the contrary, by revealed preference, our evidence provides strong support for auditors' pivotal role in moderating agency conflicts between companies' management and investors (e.g., Ball, 1989): many companies hired seemingly competent and independent auditors, and these companies exhibit comparably high levels of liquidity in capital markets.[65]

Although our evidence suggests that the regulation of public company auditing may not be imperative for the functioning of capital markets, such regulation is nevertheless pervasive around the globe. This begs the question why regulators frequently resort to regulating auditing. A benign explanation of this puzzle could be that such regulation primarily codifies existing and developing audit practices. For that reason, it does not help, but also does not hurt much.[66] A potentially complementary but less benign explanation could be that the audit (or accounting)

[65] The positive association between voluntary audits and liquidity for U.S. public firms in the pre-regulation period echoes the findings in the private firms literature that voluntary audits are associated with capital-market benefits (e.g., Blackwell, Noland, & Winters, 1998; Kausar et al., 2016; Minnis, 2011).

[66] Regulatory action may help politicians to ensure their voters' support by signaling awareness/decisiveness and offering regulatory solutions (e.g., after prominent scandals) (e.g., Hail et al., 2018). In this vein, Flesher and Flesher (1986) argue that the highly publicized fraud and bankruptcy of the Kreuger & Toll conglomerate, which resisted voluntary auditing, in 1932 rendered an audit mandate a politically convenient response.

profession, a well-organized interest group with a politically convenient product ("trust" or "transparency"), leverages regulation to extract and protect rents.[67]

While our empirical evidence cannot differentiate between these explanations, historical anecdotes provide some support for both the benign and the capture explanation. Wiesen (1978), using transcripts from congressional hearings and various other historical accounts, suggests that an audit mandate was an easy policy prescription for Congress given the already extensive auditing rate, consistent with the benign explanation.[68] He also explains that leading auditors had a substantial influence on the SEC's rule-making, consistent with the less benign capture explanation. The auditors' expert witnesses, for example, were successful in persuading Congress to leave the responsibility of auditing with external rather than federal auditors. They were further successful in lowering Congress'

[67] In this vein, a recent newspaper article discusses the lobbying prowess of auditors in the aftermath of the Wirecard scandal (Bartz, Hesse, & Traufetter, 2021).

[68] In this vein, Coffee (2006) states (p. 127): "*Carter [President of the New York State Society of CPAs] urged Congress to revise the proposed legislation [the 1933 Act] to provide instead that all registration statements be audited, and Carter testified before the Senate Committee that 85 percent of all listed companies were already audited. Yet, the Committee's response to this information was lukewarm at best. Rather, the Committee's chairman, Senator Duncan Fletcher, understandably wondered why it was necessary to require by law what was already the prevailing practice. His views were echoed by Senator Gore, who pointed out that the fact that 85 percent of the NYSE's companies were audited had not prevented the 1929 Crash. Auditing, they were implying, was no panacea. The practical political explanation of Colonel Carter's testimony may have been that he was seeking to confine Congress to symbolic legislation that would do no harm, but only codify current best practices.*"

expectations regarding the level of assurance provided by audits.[69] The latter agenda reflects auditors' rising concerns about litigation, which were likely fueled (1) by investors' attempt to sue an auditor of a fraudulent company (*Ultramares Corp. v. Touche*) in 1932 (Pandit & Baker, 2021), (2) by the liability extended through the 1933 act (Douglas & Bates, 1933), and (3) by auditors' fears of future interventions by the new regulator (e.g., Carmichael & Winters, 1982). While a systematic investigation of the political economy of audit regulation is outside the scope of our study, we view it as a an important next step to further our understanding of public company auditing and its regulation.

3.7 Conclusion

We explore the landscape of public company auditing around the introduction of the Securities and Exchange Commission (SEC) in 1934. The introduction of the SEC, which mandated the auditing of public companies trading on centralized stock exchanges, is commonly viewed as a sea-change in the regulation of auditing. To uncover how the SEC shaped the landscape of public companies, we exploit the rich textual data provided in historical annual reports of a broad sample of pub-

[69] A letter, co-signed by nine of the largest auditors and addressed to the NYSE in 1933, provides an illustrative example of such lobbying efforts. The auditors argue in favor of a reduced level of verification, which they deem more practical than the responsibilities and expectations initially envisioned by the exchange (Arthur Andersen & Co. et al., 1933).

lic companies trading on regulated and unregulated stock markets and spanning several decades.

We find that most public companies obtained audits even before the SEC's audit mandate, which limited the mandate's impact on audit rates. We further document that these companies selected their auditors based on characteristics reflecting independence and competence, especially before the SEC's mandate. Although we see only limited changes in audit rates and auditor choices, we observe significant changes in the content of audit statements around the SEC's introduction. Audit statements became increasingly standardized and shifted from attesting to companies' financial position to opining on their compliance with GAAP. These changes, however, appear to reflect concurrent standardization efforts initiated and driven by private-sector actors rather than the SEC. Finally, we do not find any significant impact of the SEC's audit mandate on either mandatorily audited companies' capital-market outcomes or regulated capital markets as a whole (e.g., compared to the unregulated OTC market).

Collectively, our descriptive evidence suggests that the introduction of the SEC had a limited impact on companies' reliance on audits and investors' trust in companies' reports, at least initially. Notably, its impact was limited because public company auditing appeared to flourish even in the absence of any (federal) regulation.

Our evidence informs the debate about the need for and promise of audit regulation (e.g., DeFond et al., 2016; DeFond & Zhang, 2014; Donovan et al., 2014). It suggests that public company auditing is *not* a product of regulation, consistent with the view expressed in Watts and Zimmerman (1983) and Buijink (2006). This evidence stands in contrast to the popular view that auditing regulation is imperative for the functioning of audit and securities markets. Nevertheless, we acknowledge that our evidence alone clearly does not settle the debate. Our evidence pertains to a specific audit regulation (primarily an audit mandate) at a specific time (several decades ago). Accordingly, it speaks first and foremost to the need for audit mandates. It casts doubt on the need for such mandates, confirming recent evidence on audit mandates in the private company setting (e.g., Baylis, Burnap, Clatworthy, Gad, & Pong, 2017; Breuer, 2021; Dedman et al., 2014; Minnis & Shroff, 2017) and extending it to the realm of large public companies. By contrast, our evidence does not immediately speak to the need to regulate auditing practices and oversight (e.g., DeFond & Lennox, 2017; Gipper et al., 2020; Hanlon & Shroff, 2020; Shroff, 2020; Vetter, 2020). While the SEC had the power to regulate auditing practices, it largely abstained from doing so during its early years. Historical accounts of the rule-making process, however, suggest that any attempts to regulate audit practices may be hampered by regulatory capture and expertise constraints (e.g., Demsetz, 1969; G. J. Stigler, 1971; Wiesen, 1978).

Appendix

Figures

Figure 3.A1: Audit Statements for the American I. G. Chemical Corporation

May 25, 1932.

American I. G. Chemical Corporation,
521 Fifth Avenue,
New York, N. Y.

DEAR SIRS:

We have examined the accounts and records of the American I. G. Chemical Corporation for the twelve months ended March 31, 1932; and

In our opinion, the accompanying Balance Sheet and Statements of Income and Expense, and Surplus, compiled from our General Report, set forth the financial condition of the Corporation as at March 31, 1932, and the results of its operations for the period.

F. W. LAFRENTZ & CO.

Certified Public Accountants.

(a) 1932

American I. G. Chemical Corporation,
521 Fifth Avenue,
New York, N. Y.

DEAR SIRS:

We have made an examination of the balance sheet of the American I. G. Chemical Corporation as at March 31, 1935 and of the statements of income and surplus for the year ended on that date. In connection therewith, we examined or tested accounting records of the Company and other supporting evidence and obtained information and explanations from officers and employees of the Company; we also made a general review of the accounting methods and of the operating and income accounts for the year, but we did not make a detailed audit of the transactions.

In our opinion, based upon such examination the accompanying balance sheet and related statements of income and surplus fairly present, in accordance with accepted principles of accounting consistently maintained by the Company during the year under review, its position at March 31, 1935 and the results of its operations for the year.

F. W. LAFRENTZ & CO.

Certified Public Accountants.

April 5, 1935.

(b) 1935

The figure showcases two audit statements for the American I. G. Chemical Corporation. Panel A shows the audit statement, signed by F. W. LaFrentz & Co. in 1932. Panel B shows the audit statement, signed by the same auditor in 1935. The red underline is added for emphasis.

Tables

Table 3.A1: Definition of Variables

Name	Definition
Firm Variables	
Size (*Market Value*)	Natural log of the market capitalization.
EPS	Earnings per share, basic and net of all distributions excluding the dividend per share.
Dividend Payer	Indicator variable that is equal to one if the company pays dividends, and zero otherwise.
Zero Return Days	Number of days on which the return is zero, scaled by total number of days for which there is data.
Zero Volume Days	Number of days on which the trading volume is zero, scaled by total number of days for which there is data.
Amihud Illiquidity	Amihud illiquidity calculated as in Amihud (2002).
Auditor Variables	
Auditor	Equal to the auditor name among the auditor name keywords in Appendix 3.A2 that is the best match for all audited companies.
Portfolio Size	Natural log of the sum of the market capitalizations of all companies in the client portfolio, per year.
Portfolio Concentration	Within auditor-year Herfindahl–Hirschman index of the proportions of the client size divided by total auditor portfolio size.
Audit Variables	
Audit Indicator	Indicator variable that is equal to one if the annual report contains one of the audit statement keywords in Appendix 3.A2, and zero otherwise.
Audit Statement Length	Natural log of the number of words in the audit statement.
Audit Statement Lag	Natural log of the number of days between the sign-off date of the auditor on the audit statement and the fiscal year end. The sign-off date is the last date that is mentioned on the page of the audit statement and the subsequent two pages, no later than 1 year after the fiscal year end and no earlier than the fiscal year end. The fiscal year end is taken from *Mergent* or, if missing, from *Global Financial Data*.

Client-Auditor Distance	Natural log of the geodetic distance between the city of the headquarters of the company and the city of the auditor's office that is closest to the company, out of all cities in which the auditor has an office. The list of offices per auditor is compiled out of all top 1,000 U.S. cities (in terms of population in 1940) mentioned in the available audit statements per auditor, per year. A city should be mentioned in at least 1% of all occurrences.
Client-Auditor Specialist	Indicator variable that is equal to one if the auditor is a specialist in the sector in which the company is active, and zero otherwise. The auditor is considered to be a specialist in the sector for which the proportion of total portfolio size of the auditor within the year (in terms of market capitalization) in that sector to the total auditor portfolio of the auditor within the year is largest.
Economic Position	Indicator variable that is equal to one if the audit statement contains any of the economic position keywords in Appendix 3.A2, and zero otherwise.
GAAP	Indicator variable that is equal to one if the audit statement contains any of the GAAP keywords in Appendix 3.A2, and zero otherwise.
HHI Topics	Herfindahl–Hirschman index of the distribution of the nine topics within the audit statement. The nine (latent) topics are identified using Latent Dirichlet Allocation over the full sample of audit statements, and are defined as follows: (1) cash & equivalents, (2) consolidation, (3) inventory, (4) depreciation, (5) review, (6) testing, (7) financing, (8) income, (9) CPA.
Dominant Topic Distribution	The extent to which the audit statement focuses on one of the nine (latent) topics, proxied by the highest probability (according to the Latent Dirichlet Allocation procedure) that one of the nine topics appears in the audit statement (i.e., we compare the probabilities for each of the nine topics within a given audit statement, and set this variable equal to the highest of the probabilities).

This Table gives an overview of the variables used in the analyses and their descriptions.

Table 3.A2: Search Words

Variable	**Search Words**
Audit Indicator	have made an examination, have audited, auditors report, certificate of auditors, hereby certify, certify that, auditors certificate, accountants certificate, have examined the accounts, have examined the books, have examined the balance sheets, having audited the, examined or tested accounting, hereby certify that, have audited your, made an examination of, fairly represent in accordance with, tested the accounting records, in our opinion, based upon our examination, conformity with general accepted accounting principles, have audited the books, have examined the financial records
Auditor	price waterhouse, ernst ernst, haskins sells, arthur young, peat marwick mitchell, allen r smart, allen smart, jd cloud, hadfield rothwell soule coates, lybrand ross bros montgomery, barrow wade guthrie, deloitte plenders griffiths, touche niven, patterson teele dennis, west flint, howard kroehl company, cutler hammer, george dallas, scovell wellington company, arthur andersen, konopak hurst dalton, lafrentz, rg rankin, loomis suffern fernald, pauljoseph esquerre, richards ganly, fa hamilton, lawrence e brown, eastern audit company, marwick mitchell company, bieth macnaughton, general timber service, pogson pelloubet, charles f rittenhouse company, herbert f french company, elliott davis company, american audit, jk lasser, seidman seidman, lawrence brown company, wo ligon company, simonton jones company, stockwell wilson linvill, leslie banks company, leslie banks, wolf company, jh greenhalgh company, miller donaldson company, haselmire cordle, oj neff, of taylor, sd leidesdorf, main company, feinberg jacobs, storer bishop, rogers company, hurdsman cranstoun, pace gore mclaren, chandler murray chilton, marwick mitchell, puderpuder, jones caesar dickinson wilmot, patterson corwin, stagg mather, ernsternst, david himmelblau, audit company of new york, collins company, richards company, grey hunter stenn, ward weber, townsend dix pogson, amos albee son, edward steacie, loganlogan, pearce granata, squires company, wright long, ernest bell company, meech harmon lytle blackmore, quail macoubrey, herbert french company, goettsche company, boyden yardley guay, vollumvollum, cerf cooper, rhyne priaulx bearisto, lingley baird dixon, frazer torbet, stewart watts bollong, mattison davey, mcconnell breiden, hopkins company, seamans stetson tuttle, marvin scudder company, stern porter kingston coleman, detroit trust, bagley vega company, wells baxter miller, leach rindfleisch scott, brockelbank brockelbank, leonhard troub company, miller franklin company, clifford collins company, keller kirschner martin clinger, alexander aderer, mclaren goode, swearingen swearingen, robert douglas company, smith davis wills, amen surdam, snyder ellinger davies, amick spicer, lovejoy mather hough stagg, searle nicholson oakey lill, alexander grant company, searle miller company, boyce hughes farrell

Table 3.A2: Search Words

Variable	**Search Words**
Economic Position	consolidated position, economic position, financial position, financial condition, state of the company
GAAP	accordance with accepted accounting principles, gaap, accepted accounting principles, accounting principles, accepted principles of accounting, accepted principles, standard

The table presents the search words that are used to extract information from the annual reports. See Appendix 3.A1 for detailed definitions of the variables.

Table 3.A3: Overview of Auditors in Sample

	Name	Origin	Engagements					
			Total	**1900**	**1920**	**1927**	**1933**	**1940**
1	Price Waterhouse	UK	2,034	3	34	70	106	141
2	Ernst & Ernst	US	1,502		11	44	75	131
3	Haskins & Sells	US	1,178		21	25	60	94
4	Lybrand, Ross Bros. & Montgomery	US	813	1	4	21	55	89
5	Arthur Young	US	718		18	25	38	41
6	Peat Marwick Mitchell & Co.	UK	699		10	28	43	45
7	Arthur Andersen	US	489		2	7	36	60
8	Barrow Wade Guthrie	US	332		5	8	21	25
9	Touche & Niven	US	283		8	13	18	16
10	Audit Company of New York	US	164	3	5	6	1	
11	Deloitte Plender Griffiths	UK	134		3	4	5	10
12	F. W. LaFrentz & Co.	US	111		1	8	8	9
13	Scovell Wellington & Co.	US	110			3	5	10
14	Patterson Teele Dennis	US	106	3	2	1	3	4
15	Pogson, Peloubet & Co.	US	94		2	4	4	4
	Total		**8,767**	**10**	**126**	**267**	**478**	**679**
	% of total engagements in sample		84.6%	83.3%	85.1%	88.1%	85.8%	82.9%

The table presents the names and origins of the 15 auditors with the most engagements in our sample. The table summarizes the number of engagements in total, as well as for several sample years. The bottom row shows the percentage of all engagements, in total or for the year, performed by the largest 15 auditors.

4 Financial Incentives of Social Media Analysts

4.1 Introduction

Coverage by information intermediaries, such as analysts, is beneficial for both investors and firms. Analysts improve capital market outcomes, such as liquidity, information asymmetry and price efficiency, by summarizing, disseminating, aggregating, synthesizing, and creating information (Barth & Hutton, 2004; Bowen et al., 2010; Hong, Lim, & Stein, 2000). However, analyst coverage is not equally distributed across firms due to the many firm-specific factors and career incentives that play a role in a coverage decision (see, e.g., Anantharaman & Zhang, 2011; Bhushan, 1989; Fang, Hope, Huang, & Moldovan, 2020; Groysberg et al., 2011). In 2019, 20 percent of U.S. listed firms had no analyst coverage, while a quarter of all EPS forecasts accrued to the top five percent of firms in terms of coverage.[70] Social media analysts, individuals who contribute

This paper is serves as my job market paper. I thank Thomas Bourveau, Matthias Breuer, Ferdinand Elfers, Michael Erkens, Jochen Pierk, Sander Renes, Robert Stoumbos, Menghan Zhu; seminar participants at BI Norwegian Business School, Bocconi University, Erasmus University Rotterdam, London School of Economics, Rijksuniversiteit Groningen, Universidad Carlos III de Madrid, Universitat Pompeu Fabra, Universiteit van Amsterdam, and Vrije Universiteit Amsterdam; and participants at the 2021 EAA Doctoral Colloquium for helpful comments and suggestions. An Online Appendix to this paper can be downloaded at `https://drive.google.com/drive/folders/1uiuFzQGgxCpfNjlqfUZ9vmzSQZX-8Vbt?usp=sharing`

[70] Calculated using I/B/E/S quarterly-EPS forecasts and the Compustat universe.

their analyses online, are an alternative to traditional sell-side analysts and more directly address this concern. Social media analysts have an explicit interest in broad, high quality coverage, as this is rewarded with higher remuneration through incentive schemes, image-related utility and higher reputation. Platforms that publish their contributions also benefit through higher advertising revenue and more paying subscribers. However, even social media analysis coverage is not distributed evenly across firms. To stimulate broad, high quality coverage more explicitly, the largest platform offering social media analyst contributions, SeekingAlpha (SA), has introduced a minimum payment incentive to improve coverage of firms that receive little to no coverage, i.e., undercovered firms. In this paper, I investigate the effectiveness of this incentive with respect to the amount and quality of the information provision for undercovered firms.

It is unclear whether financial incentives that explicitly incentivize coverage of undercovered firms help to improve information provision for these firms. Although it is reasonable to expect that financial incentives increase coverage (e.g., Kanfer, 1987), it is less clear whether financial incentives increase high quality, informative coverage. On the one hand, financial incentives may result in less informative coverage. Prior literature finds that financial incentives can reduce intrinsic motivation – the motivation crowding out effect – that leads to reduced contributions of effective, high quality contributors in favor of those by less effective contributors that are motivated primarily by external incentives (Hui et al., 2021; Khern-am-Nuai et al., 2018). This concern is particularly relevant in the setting of social media analyst contributions, as effective, high quality contributors often have high intrinsic motivation and contribute for reciprocity and image-related utility reasons (Toubia & Stephen, 2013). Moreover, incentive design matters and the height of the minimum payment incentive may not be sufficient to motivate higher quality contributors. On the other hand, financial incentives could increase informative coverage for undercovered firms. Prior literature documents a direct relation between financial incentives and quality output through exerted effort (Gneezy et al., 2011; Gneezy & Rustichini, 2000), and financial incentives increase competition for limited user attention – the competition crowding out effect – that reduces contributions by less effective contributors, in favor of contributions by effective, high quality contributors (Liu & Feng, 2021). In addition, SA subjects all coverage to an editorial process, providing a minimum quality level for contributions. In sum, the effects of these financial incentives on high quality coverage of undercovered firms is an open empirical question.

SA is a user-generated content (UGC) platform, which publishes stock analyses by contributors. SA has paid contributors US$13 per $1,000$ page views since 2011 to align its contributors interests with that of itself. In January 2018, SA introduced the 'minimum payment guarantee' (MPG) incentive that ensures contributors of a US$65 payment for any article covering an undercovered firm. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days.

I examine the impact of the introduced financial incentives using a difference-in-difference analysis. First, I document that the MPG incentive meets it primary policy objective by significantly increasing coverage of undercovered firms, both statistically and economically. I find that this result cannot be explained by alternative (statistical) explanations. I acknowledge and document that social media analyst coverage is largely endogenous and associated with changes in underlying economics. However, I find that my results are robust for controlling for these aspects.

Next, I examine the informativeness of the incentivized coverage. First, I examine differences in contributor characteristics between contributors who cover undercovered firms and contributors who do not, both before and after the introduction of the MPG incentive. I find that contributors with experience in writing analyses, those with industry expertise, and with higher skill are more likely to cover undercovered firms, both in the period before and after the implementation of the MPG incentive. Next, I find that the market reacts to the publication of SA articles, confirming their informativeness to market participants, and that the difference in market reactions are not statistically or economically different for incentivized versus non-incentivized coverage. This result is robust for a battery of controls including firm characteristics and confounding events. Taken together, I find that experienced and high quality contributors cover undercovered firms and that incentivized coverage is equally informative to non-incentivized coverage. This implies that the information level undercovered firms on SA has increased following the introduction of the MPG incentive, which improved information provision by SA.

Lastly, I examine whether the improvement of information provision on SA, improves the overall information environment for undercovered firms and therefore capital market outcomes. Focusing solely on undercovered firms in the period after the introduction of the MPG incentive, I find that incentivized coverage is associated with reduced information asymmetry between investors and higher liquidity, both at the quarterly level and around earnings announcements. I also find that incentivized coverage is associated with higher levels of price discovery around earnings announcements, suggesting a more efficient market reaction to earnings news for covered firms. Together these results show the value of social media analysts coverage in improving capital market efficiency, especially for firms with little coverage by traditional sell-side analysts.

This paper contributes to several streams of literature. It contributes to the literature on the value of social media analysts (see Blankespoor et al., 2020, for an overview). I find that incentivized coverage by social media analysts is associated with significant improvements in capital market outcomes. As such, I show the value of social media analysts to market participants in a quasi-experimental setup, especially when traditional sell-side or social media analyst coverage is low (Bartov et al., 2018; Chen et al., 2014; Drake, Moon Jr., Twedt, & Warren, 2021; Farrell et al., 2022; Gomez, Heflin, Moon, & Warren, 2020; Jame et al., 2016; Kogan, Moskowitz, & Niessner, 2021). Given that an increasing number of retail investors rely on social media and internet information intermediaries in making investment decisions, and retail trading volume increased to 27 percent of overall U.S. trading volume in 2021[71], it is especially important to better understand what drives social media analyst coverage. My results add to our understanding of social media analyst platforms, and how they use incentives to shape information provision. By introducing incentives, social media analyst platforms directly address the inefficiencies that result from unequal and absent information intermediary coverage (see, e.g., Fang et al., 2020; M. H. Lang, Pinto, & Sul, 2021). More generally, my paper is among the first to show the consequences of introducing financial incentives for social media analysts. Therefore my findings complement those of Chen, Hu, and Huang (2019), who show that the introduction of an option to monetize contributions on SA led to more

[71] Almost as much as mutual funds and hedge funds combined. See https://www.ft.com/content/7a91e3ea-b9ec-4611-9a03-a8dd3b8bddb5.

contributions, while not affecting their quality. In addition, I corroborate the findings of Clausen, Litterscheidt, and Streich (2021), who show the effects of several SA payments on liquidity. My paper differs from this earlier work, because I focus specifically on the channel through which an incentive change improves information provision, and offer an explanation for the documented capital market effects by focusing on the informativeness of coverage.

My paper also contributes to the literature on the use of incentives in user-generated content (UGC) platforms (e.g., Hui et al., 2021; Khern-am-Nuai et al., 2018; Liu & Feng, 2021; Tang, Gu, & Whinston, 2012). It is not clear from prior research that financial incentives increase higher quality content and how they help the platform and users of these platforms. In addition, most of the studied financial incentives are platform-wide, non-discriminatory and introduced for all effort. My results show that it is possible to improve platform quality and therefore usefulness to users by introducing financial incentives that are geared towards a specific policy objective. In addition, I show that a relatively small incentive already increases output, in contrast to earlier studies that stress the need for larger payments (e.g., Gneezy et al., 2011; Gneezy & Rustichini, 2000). More broadly, my paper is related to the literature on minimum wages and output of labor (e.g., Acemoglu, 2001; Card & Krueger, 1994; Dustmann, Lindner, Schönberg, Umkehrer, & vom Berge, 2021; Flinn, 2006; Ippolito, 2003). Prior literature already shows that the introduction of a market-wide minimum wage can increase labor market quality and raise total factor productivity and output. The introduction of the MPG incentive for coverage of undercovered firms can be thought of as a minimum wage for a specific set of tasks. I show that introducing such a 'conditional minimum wage' increases output.

4.2 Theoretical Underpinnings & Predictions

4.2.1 Analysts and Coverage Incentives

Information intermediaries play a key role in financial markets. The existence of information intermediaries, of which analysts are a primary example, is associated with lower information asymmetries and with higher liquidity, price discovery and investor interest, and therefore beneficial to both investors and firms (see, e.g., Amiram, Owens, & Rozenbaum, 2016; Anantharaman & Zhang, 2011; Irvine, 2003; Kothari, So, & Verdi, 2016). Investors faced with disclosure processing costs are not all equally capable of distilling true signals from financial information, which leads to information asymmetries between investors in financial markets (Blankespoor et al., 2020). Disclosure processing costs breed less-sophisticated and uninformed market participants, who incorrectly assume that they are trading on the correct signal (Black, 1986). This noise-trading reduces the overall level of price informativeness and increases the time it takes for the true signal to be reflected in the price. Analysts mitigate disclosure processing costs by summarizing, disseminating, synthesizing, analyzing, and uncovering information, thereby improving capital market outcomes (Bradshaw, 2009; Bradshaw et al., 2017).

Social media analysts are similar to traditional sell-side analysts, but prior research shows a more mixed image of the value of their coverage to capital markets. As a whole, social media analysis relies on the 'wisdom of the crowds', which posits that the large collection of social media analyses, written by a crowd of contributors and published on an online platform, together give more truthful and valuable information than the analyses of a few expert analysts (Surowiecki, 2004). Multiple studies document the informativeness of social media analysis, and link coverage by social media analysts with improvements in capital market outcomes (see, e.g., Bartov et al., 2018; Chen et al., 2014; Farrell et al., 2022; Jame et al., 2016). In particular, social media analysts are capable of making value relevant earnings forecasts, and their presence is associated with faster and more complete responses to earnings announcements, and a more level playing field among investors (e.g., Antweiler & Frank, 2004; Farrell et al., 2022; Gomez et al., 2020). However, recent studies show that quality and credibility concerns surrounding social media analysis can curb the benefits of social media analyst coverage (e.g., Campbell, DeAngelis, & Moon, 2019; Clarke, Chen, Du, & Hu, 2021; Dyer & Kim, 2021; Kogan et al., 2021; Mitts, 2020). Nonprofessional social media analysts can potentially *hinder* capital market efficiency, because they are less informed and less capable of creating and uncovering information (Drake, Thornock, & Twedt, 2017).

Analyst coverage (quality) is related to firm characteristics, and motivated by personal and professional incentives. Prior literature shows that coverage decision and forecast quality is related to, among others, firm size, firm and capital market performance, a firm's information environment, and a firm's investor base (e.g., Irvine, 2003; M. Lang & Lundholm, 1993; McNichols & O'Brien, 1997; Yu, 2008). Analysts are primarily employed by analyst firms, investment banks or brokerage houses and therefore have less agency over their coverage decisions. In addition, coverage changes frequently as a result of external factors, such as analyst firm mergers and closures (e.g., Derrien & Kecskes, 2013) and regulatory changes (e.g., Anantharaman & Zhang, 2011; Fang et al., 2020; M. H. Lang et al., 2021). Prior studies document that analysts are influenced in their coverage (effort) by career concerns and professional incentives (e.g., Groysberg et al., 2011; Harford, Jiang, Wang, & Xie, 2019; Hong et al., 2000), and by personal incentives (e.g., Hunton & McEwen, 1997).

Social media analyst coverage is associated with similar firm characteristics as traditional sell-side analyst coverage (Farrell et al., 2022), however less is known about incentives that play a role in coverage and effort decisions of social media analysts. Because social media analysts are not associated with, or employed by, analyst firms, investment banks or brokerage houses, they are less susceptible to professional and career incentives. A Contributors of crowdsourced platforms, like social media analysts, primarily contribute for reasons related to image, reciprocity, intrinsic motivation, or reputation building, instead of financial rewards (e.g., Toubia & Stephen, 2013). For these reasons, social media analysts are more likely to make demand-driven, or even independent, coverage decisions. However, regulators increasingly find occurrences of fake and misleading statements in coverage with the intend to influence stock prices, suggesting less benign coverage incentives.[72] Focusing specifically on financial rewards for coverage, Chen et al. (2019) find that introduction of a payment for coverage increased the amount and diversity of coverage, but had no effect on the quality of the coverage, and Clausen et al. (2021) find that changes in payment incentives appear to affect the coverage decisions by social media analysts.

[72] see, e.g., https://www.investor.gov/additional-resources/news-alerts/alerts-bulletins/investor-alert-beware-stock-recommendations and https://www.sec.gov/litigation/admin/2017/33-10337.pdf.

4.2.2 Financial Incentives and Output

While it is reasonable to expect that output increases when financial incentives are introduced, it is not clear that this would lead to *higher quality* output. A financial incentive leads to more output when the expected benefits of the financial incentive outweigh the expected costs of doing the incentivized task, ceteris paribus. Financial incentives therefore play a large role in task selection, as agents forced with the choice between two identical tasks choose the one that is incentivized (Kanfer, 1987). However, several papers show that the effect of financial incentives on effort, and as such on output quality, is less clear (see Bonner & Sprinkle, 2002, for an overview). Following agency theory, financial incentives should increase effort, and therefore higher quality output, because performing incentivized tasks improves an agent's utility by increasing wealth through remuneration on basis of the quality of the output. However, this effect decreases with the noise with which the principal observes the output. In addition, agents start 'shirking' on tasks when output quality is not explicitly incentivized, which reduces output quality. Although reputation concerns and strong monitoring by the principal may strongly reduce the necessity of explicit incentives altogether (Fama, 1980). Alternative theories, such as the expectancy theory (see Vroom, 1964) and the goal-setting theory (see Locke & Latham, 1990), do show a relation between incentives and effort, but highlight the moderating role of incentive design and the (assumed) characteristics of the task, the agent, the principal and their interaction (see Bonner & Sprinkle, 2002, for an overview). Regarding incentive design, prior literature shows a trade-off between a direct price effect – the positive relation between exerted effort and financial rewards (Gneezy et al., 2011; Gneezy & Rustichini, 2000) – and a motivation effect – the negative relation between intrinsic motivation and financial reward (Lepper, Greene, & Nisbett, 1973).

In addition to the intricacies of incentivizing effort, specific characteristics of user-generated content platforms make it unclear whether the introduction of financial incentives results in higher quality content on the platform. User-generated content (UGC) platforms are websites that offer content that is generated by its own users, i.e., contributors.[73] Several UGC platforms have introduced financial incentives to increase (the quality of) contributions, but their effectiveness is not clear from prior literature. The studies finding positive effects of financial incentives on content quantity and quality underline the necessity for a sufficiently high financial incentive (Chen et al., 2019; Tang et al., 2012). Liu and Feng (2021) show that financial incentives increase the competition among

[73] Not everyone on a platform contributes. Often only 1 percent of users actively and regularly contribute content to the platform, 9 percent of users infrequently contribute, and 90 percent of users never contribute. This is commonly referred to as the '1-9-90' rule (van Mierlo, 2014). I find this to be more extreme on SeekingAlpha: for 10 million active monthly users, there are 17 thousand contributors, which is probably due to the curation of content by SA and the time investment it takes to provide content.

contributors for the limited attention of users, which ultimately reduces the contributions by less-effective, lower quality contributors, resulting in a higher average level of contribution quality on the platform. This is referred to as 'competition crowding out'. Conversely, (Khern-am-Nuai et al., 2018) show that financial incentives reduce the average level of contribution quality on the platform because contributions of effective, higher quality contributors are crowded out by the contributions of less-effective, lower quality contributors. This is referred to as 'motivation crowding out', which happens because effective, higher quality contributors are usually less motivated by, or sensitive to, financial incentives. Both these crowding out effects complicate assessing the impact of the introduction of a financial incentive on the quality of contributions, and prior literature show that different equilibrium outcomes exist, dependent on the design on the incentives (Liu & Feng, 2021).

4.2.3 Predictions

Whether the information provision on SA has improved after the introduction of the MPG incentive is therefore an empirical question. On the one hand, the introduction of the MPG incentive may decrease the total level of informative coverage for undercovered firms because the motivation crowding out effect results in a lower average level of contribution quality in undercovered firms. This effect is potentially strengthened in the SA setting, because a contributor's payment is tied to the effectiveness of its output in the form of a payment per view. Less-effective, lower quality contributors, who are, on average, remunerated worse due to a lower average number of page views, are more likely to move to undercovered firms because they are assured of a minimum payment after the introduction of the MPG incentive. The competition crowding out effect is potentially lower, because effective, high quality contributors already accrue high numbers of views. The MPG incentive is possibly not high enough, both absolutely and relatively, to incentivize these contributors to cover undercovered firms. On the other hand, the introduction of the MPG incentive can increase the total level of informative coverage for undercovered firms. This is because the competition crowding out effect increases competition for the limited attention of users between contributors in undercovered firms. This results in fewer contributions by less effective, lower quality contributors, and favors contributions by effective, higher quality contributors. The publication of the list of firms eligible for an MPG incentive payment by SA informs effective, higher quality contributors of an information need for these firms to which they respond, strengthening this effect. The MPG incentive, as a fixed payment, reduces the contributor's risk of not being compensated for the exerted effort. In addition, SA analyses are curated by editors, which will likely ensure a minimum quality level of articles.

4.3 Setting

4.3.1 SeekingAlpha

SeekingAlpha (SA) is the largest investment-related website in the U.S., with 17 (210) million unique monthly visitors (visits), 10 million registered users and 17 thousand contributors as of 2021 (Seeking Alpha, 2021). SA offers a platform to publish, discuss, and read investment articles. A submitted article is reviewed by an editor of SA, who decides to reject or accept the article, or to provide the author with some suggestions for improvement on writing and structure. After acceptance, the article is published on SA and publicly viewable.[74] Contributors write articles on one ticker, or in the case of an industry or market commentary, on multiple tickers. Tickers refer different asset classes and articles range from in-depth analyses to valuations, market commentaries or discussions of earnings releases. Other users can comment on the article and discuss their views with the author of the article, or other users.

SA has seen a significant change in business model over the years. At the start, SA's income solely consisted of ad revenue. A large part of the traffic was directed to SA from other websites, such as Yahoo! Finance. In 2011, when SA no longer relied on referrals to draw traffic, SA started

[74] Except for articles that are classified as 'PRO Articles', which are only accessible to subscribers who pay a monthly fee of US$299.99 (price as of April 2021).

a subscription-based format in which premium, exclusive content was hidden behind a paywall, but free content was still publicly available. In 2018, arguing there is more growth in a subscription-based model, all SA articles are hidden behind a paywall 30 days after publication. Some articles were only accessible to the highest-paying tier of subscribers, the 'PRO'-subscribers. Since January 2021, a limited number of all articles are freely accessible to non-paying users each month, but a premium (or PRO) subscription is needed to read more.[75]

4.3.2 Payment by SeekingAlpha

SA started paying contributors by page view in 2011 to increase the amount of high quality content on the platform. In 2017, SA announced it had paid out US$15 million to contributors since 2011, or US$270 annually per contributor, on average. Payment ranges from a couple of dollars per article to much more: the top earner of 2015 earned upwards of US$200,000.[76] However, remuneration does not appear to be the primary reason for contributors to contribute to SA.[77]

SA has changed the payment of contributors several times since its inception. From the start of SA in 2004, contributors were not remunerated

[75] See https://seekingalpha.com/article/4396836-important-update-for-seeking-alpha-users.

[76] See https://seekingalpha.com/article/3085646-that-was-quick-our-first-premium-author-hits-200000-in-annual-revenue.

[77] See https://seekingalpha.com/article/2134803-how-much-does-seeking-alpha-pay-its-contributors.

for writing articles. SA introduced the ‘premium partnership program’ (PPP) in 2011 as part of the introduction of the subscription model to increase the content available to premium subscribers. Contributors earned US$10 per one thousand article views on these premium articles. Contributors were not paid for articles that were not submitted under the PPP.[78] In June 2013, SA announced that it would boost payments to articles that fit in three particular categories to reward high-quality content: (1) ‘Alpha-Rich articles’, or long and short ideas with high risk-return profiles, receive a fixed payment of US$500, (2) ‘Small-Cap Insights’, or high quality analyses of small-cap stocks that otherwise lack strong coverage, receive a fixed payment of US$150, and (3) ‘Outstanding Insights’, high quality analyses selected by SA editors, receive a flexible cash bonus in addition to a minimum guaranteed payment.[79] Contributors could submit their articles in these categories, but SA’s editors decided whether or not these articles will be published as such, capped at 10 articles per day. In July 2014, a fixed fee of US$35 was added on top of the existing payment structure, and every week SA’s editors pick two articles to receive the ‘Outstanding Performance” award of US$2,500.[80]

On January 1, 2018, SA implemented a new contributor payment

[78] See `https://seekingalpha.com/article/246803-an-open-letter-to-seeking-alpha-contributors`.

[79] See `https://seekingalpha.com/article/1475331-why-were-boosting-payments-to-high-value-contributors`.

[80] See `https://seekingalpha.com/article/2343015-an-end-to-our-relationship-with-yahoo-a-new-era-for-equity-research`.

structure, with the stated objective to increase coverage of undercovered firms, to cover the research gap of traditional analysts and SA itself. This new structure introduces a fixed minimum payment for contributors covering undercovered firms. Undercovered firms are defined as firms with less than two articles in the last 90 days. Specifically, article payment was calculated as follows:

$$\text{Article payment} = \text{PVs} \times \text{CPM} \times \text{QS}, \tag{5}$$

where PVs (page views) is the greater of the actual page views, or the minimum page view guarantee (MPG), CPM is the current applicable rate for per page view payments, and QS is the quality score. The MPG is set at five thousand views, this means that even when an article gets fewer than 5,000 page views, contributor payment is calculated on basis of five thousand page views. If the number of page views accrued to more than five thousand page views, contributor payment is calculated using this higher number of page views. The CPM is US$13 per one thousand page views and remained constant from the outset of the new policy. The QS is determined on a contributor basis, and SA unfortunately does not provide guidance on its website or in its correspondence to contributors on how this is calculated. The guaranteed payment for an article on an undercovered firm is US$65 ($US\$0.013 \times 5,000$), while the guaranteed payment for an article on a well-covered firm is US$0. US$65 is 24 percent of the 2011–2017 average annual payout per contributor.

4.4 Research Design

4.5 Sample & Data

I employ a difference-in-differences methodology with a pre- and post period of two years surrounding the introduction of the minimum payment guarantee (MPG) incentive in January 2018. As such, my sample spans January 1, 2016 – December 31, 2019. Because the primary identifier on SA is ticker, I search SA for all tickers and trading symbols available on CRSP, Compustat and IBES between January 2016 and December 2019, and scrape all available articles. Since tickers vary over time, become defunct, or are mapped to different companies on SA, I use a fuzzy merge algorithm to map each company name-ticker combination in SA with the closest company name-ticker combination from CRSP, Compustat or IBES. This results in a final sample of around 150 thousand articles for nearly 3 thousand firms. I merge the firms with lagged quarterly financial data from Compustat, analyst data from I/B/E/S and stock market data from CRSP and TAQ.

4.5.1 Effect on Coverage

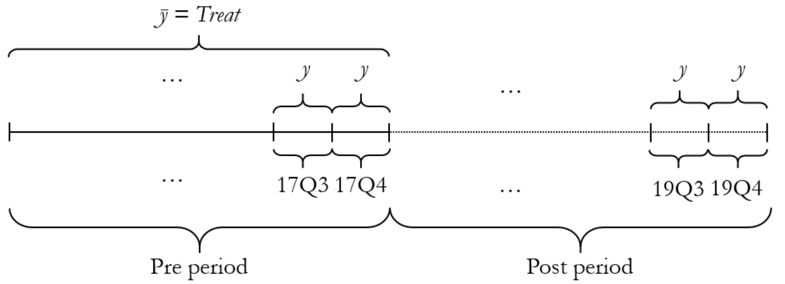
First, I investigate whether the introduction of the MPG in January 2018 improved coverage of undercovered firms. I estimate the following regression model with a continuous difference-in-differences setup:

$$
\begin{aligned}
Coverage_{i,q} = {} & \beta_1 \cdot Treat_i + \beta_2 \cdot Post_q + \beta_3 \cdot Treat \times Post_{i,q} \\
& + Controls + \alpha_q + \alpha_i + \epsilon_{i,q},
\end{aligned} \tag{6}
$$

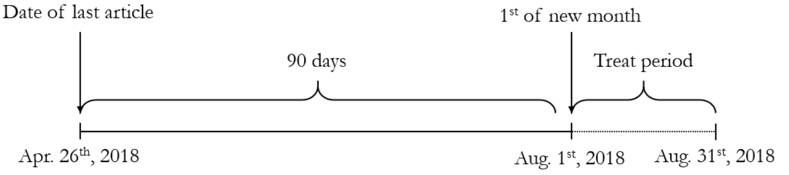
on the firm(i)-quarter(q) level. *Coverage* is an indicator variable that is equal to one if the firm is covered by at least one SA article in a particular year-quarter, and zero otherwise. Ideally, *Treat* would be defined on basis of being undercovered on the year-quarter level. However, this would make *Treat* endogenous. *Treat*, or being undercovered, is dependent on *Coverage* in the current and prior year-quarter as it is calculated every month, and mechanical. I therefore define *Treat* based on a pre-existing condition, i.e., the average of *Coverage* over the year-quarters in the pre period. As such, *Treat* is time-invariant within firm, and *Treat* measures the treatment intensity: a firm with a lower average *Coverage* in the pre period, has a higher likelihood of being undercovered in the post period, ceteris paribus. A graphical representation of the measurement of *Treat* is presented in Subfigure (a) of Figure 4.1. *Post* is equal to one if the year-quarter is equal to, or later than, the first quarter of 2018, and zero otherwise. I also control for firm characteristics and underlying economics that can explain a firm being undercovered (e.g., Bhushan, 1989). I control for characteristics related to size, financial structure, performance, ownership (*Market Cap.*, *#Employees*, *ROA*, *Leverage*, *BTM*, *%Inst. Own.*, and *#Owners*); earnings property variables (*Earn. Pers.*, *Earn. Pred.*, *Earn. Smooth.*, *Earn. Var.*, and *Abn. Disc. Accruals*); stock market variables (*Price*, *Return*, *Share Turnover*, *Dollar Volume*, and *Bid-Ask Spread*); traditional sell-side coverage variables (*#Estimates*, *Surprise*, and *Forecast Error*); and the amount of firm news (*#8Ks*). All these variables are defined in Table 4.A1 of the Appendix. α_q and α_i represent year-quarter and firm fixed effects, respectively. The fixed effects subsume the intercept, and the year-quarter fixed effects subsume the *Post* main effect. For interpretation, I subtract *Treat* from 1, which means that a firm with no coverage in the pre period has a treatment intensity of 1, and vice versa. Coefficient β_3 captures the effect of the MPG incentive treatment on *Coverage*. If the MPG policy had the intended effect, I expect β_3 to be significant and positive. That is, the average coverage in the post period is higher for firms with the highest likelihood of being undercovered in the post period, and vice versa, which I interpret as evidence for an increase of coverage. I cluster standard errors at the firm-level and winsorize all continuous variables by year-quarter at the 1 and 99 percent levels.

Figure 4.1: Definition of *Treat*

(a) Definition of *Treat* in the coverage tests



(b) Definition of *Treat* in the article tests



This figure presents the definition of *Treat* for the coverage tests in Figure (a) and the other tests in Subfigure (b). Subfigure (a) shows that *Treat* is calculated as the average of y in the pre period firm year-quarters (from Q1 2016 till Q4 2017). Subfigure (b) shows that *Treat* is an indicator variable that is equal to one if a firm is eligible for the MPG incentive payment. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days. *Treat* is calculated at the beginning of every month and therefore all articles for a ticker in a given month are either treated or control. This month is denoted in the figure as the 'treat period'.

4.5.2 Effect on Information Provision

Next, I investigate whether the incentivized coverage of undercovered firms increases information provision, i.e., on SA. I examine characteristics of contributors that cover undercovered firms and market reactions to publication of incentivized articles, and assess whether the quality of their coverage have changed after the introduction of the MPG incentive.

4.5.2.1 Contributor Characteristics

First, I investigate whether contributor characteristics differ between contributors that cover undercovered firms versus well-covered firms, and whether these characteristics have changed since the introduction of the MPG incentive. Contributor characteristics allow me to examine a potential channel through which the MPG incentive affects article quality and thus information provision. I measure contributor effectiveness and quality with experience and expertise and expect them to have a positive association with article quality, ceteris paribus (Chen et al., 2014; Dyer & Kim, 2021). An indication of a potential motivation crowding out effect by the MPG incentive is when less-effective, lower quality contributors cover undercovered firms more after the introduction of the MPG incentive. A competition crowding out effect is more likely when effective, higher quality contributors cover undercovered firms, even after the introduction of the MPG incentive. Specifically, I test which contributor characteristics that measure experience and expertise are associated with covering undercoverd firms, and how this association differs between the pre and post implementation periods. I estimate the following regression model:

$$
\begin{aligned}
y_{c,q} = {} & \beta_1 \cdot Post_q + \beta_2 \cdot Tenure_{c,q} + \beta_3 \cdot Experience_{c,q} \\
& + \beta_4 \cdot \%Undercovered_{c,q} + \beta_5 \cdot Expertise\ Industry_{c,q} \\
& + \beta_6 \cdot Expertise\ Firm_{c,q} + \beta_7 \cdot Contributor\ Skill_{c,q} \\
& + \beta_8 \cdot \#Followers_c + \beta_9 \cdot Certification_c \\
& + \beta_{10} \cdot Professional_c + \beta_{11} \cdot Company\ Linked_c \\
& + \beta \cdot \mathbf{x} \times Post_{c,q} + \alpha_q + \alpha_c + \epsilon_{c,q},
\end{aligned} \tag{7}
$$

on the contributor(c)-quarter(q) level. y is a placeholder for two variables that capture whether, and to what extent contributors cover undercovered firms: *Covered Undercovered* is an indicator variable that is equal to one if a contributor covers an undercovered firm with at least one article, and zero otherwise. *%Covered Undercovered* is the total number of contributors' articles covering undercovered firms divided by the total number of contributors' articles covering all firms. I include two sets of variables that together capture contributor effectiveness and quality by measuring contributor experience and contributor expertise. *Tenure* measures experience and is the natural log of one plus the total number of days between the contributor's first article and the start of the year-quarter. *Experience* measures overall writing experience on SA and is natural log of one plus the total number of articles written since the contributor's first article and the start of the year-quarter. *%Undercovered* measures specific writing experience of undercovered firms on SA, and is the total number of articles covering undercovered firms divided by the total number of articles covering all firms, up until the start of the year-quarter. I measure this variable as a percentage, because a ordinal variable would be highly correlated with *Experience*. *Expertise Industry* measures industry expertise and is the Hirschmann-Herfindahl Index of the total number of articles covering all firm by each of the 48 Fama and French (1997) industries divided by the total number of articles covering all firms, up until the start of the year-quarter. *Expertise Firm* measures firm-specific expertise and is the Hirschmann-Herfindahl Index of the total number of articles covering one firm divided by the total number of articles covering all firms, up until the start of the year-quarter. *Contributor Skill* is the average abnormal two day buy-and-hold return to the last five articles of the contributor, up until the start of the year-quarter (Dyer & Kim, 2021). The following variables are time-invariant due to data limitations on SA's end, and captured in April 2021. *#Followers* is the natural log of one plus the total number of followers on SA. *Certification* measures whether a contributor is a certified financial professional and is an indicator variable that is equal to one if the contributor's name or biography contains one of the following words: 'CFA', 'CPA', 'CAIA', 'FRM', 'CISI', 'Series 7', 'IMC'; and zero otherwise. *Professional* indicates whether a contributor is a professional investor and is an indicator variable that is equal to one if the contributor's name or biography contains one of the following words: ‘chief investment manager’, ‘portfolio manager’, ‘hedgefund manager’, ‘chief market’, ‘analyst’; and zero otherwise. *Company Linked* is an indicator variable that is equal to one if the contributor’s personal page on SA contains a link to a company, and zero otherwise. According to Chen et al. (2014), this may signal credibility to readers of the article. Term $\mathbf{x}$ represents a vector of all independent control variables. All variables are also defined in Table 4.A1 of the Appendix. α_q and α_c represent year-quarter and contributor fixed effects, respectively. The fixed effects subsume the intercept, the year-quarter fixed effects subsume the effect of *Post*, and the contributor fixed effects subsume the main effects of the time-invariant variables. I measure time-varying variables up until the start of the year-quarter to reduce the mechanical association between the dependent and independent variables. I cluster standard errors at the firm-level and winsorize all continuous variables by year-quarter at the 1 and 99 percent levels.

4.5.2.2 Differences in Market Reactions

Second, I investigate whether the market reacts differently to the publication of incentivized coverage of undercovered firms, and whether this reaction differs after the introduction of the MPG incentive. Prior literature documents that SA contains information to which investors react (see, e.g., Drake et al., 2021; Farrell et al., 2022), which indicates the usefulness of SA articles in providing information about the firm (Chen et al., 2014), or reducing disclosure processing costs (Blankespoor et al., 2020). I examine these market reactions as a measure of the informativeness of the article to investors. Market reactions are less biased and difficult to interpret compared to textual measures used by other studies (e.g., Dyer & Kim, 2021).[81]. I estimate the following regression model with a standard difference-in-differences setup:

$$\begin{aligned} y_a = {} & \beta_1 \cdot \mathit{Treat}_a + \beta_2 \cdot \mathit{Post}_m + \beta_3 \cdot \mathit{Treat} \times \mathit{Post}_{a,m} + \mathit{Controls} \\ & + \alpha_m + \alpha_i + \alpha_c + \epsilon_i, \end{aligned} \tag{8}$$

on the article(a)-level. y is a placeholder for two variables that capture the market reaction to the publication of an article: *CAAR* is the sum of the absolute difference between the return of the firm that is covered by the article and the value-weighted market portfolio, summed over the event window.[82] *AVOL* is the difference between the average share turnover, trading volume divided by shares outstanding, in the event window and the average share turnover in the estimation window. For the window measures, I use $t \in [0, 1]$ as event window and $t \in [-60, -1]$ as estimation window, where $t = 0$ is the publication date of the article. I examine absolute returns and volume because I am interested in the information

[81] I present an overview of (differences in) textual characteristics in the Online Appendix

[82] Estimating abnormal returns with a CAPM model and an estimation window of $[-100, -1]$ yields similar results in terms of magnitude and significance (untabulated).

content of the article, and not the direction of the information (Beaver, 1968; Landsman, Maydew, & Thornock, 2012). *Treat* is an indicator variable that is equal to one if the article covering a firm is eligible for the MPG incentive payment, and zero otherwise. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days. *Treat* is defined once per month at the beginning of the month, and therefore all articles for a firm in a given month are either treated, or control. Subfigure (b) of Figure 4.1 gives a graphical representation of the definition of *Treat*. *Post* is an indicator variable that is equal to one if the publication date is equal to, or later than, January 1, 2018, and zero otherwise. I control for all characteristics that are included in test on coverage. In addition, I control for momentum in the market variables (following Lee & Swaminathan, 2000), for article characteristics that affect the market reaction to the publication of an article, i.e., *Negative Words* and *Complexity*, (following Chen et al., 2014), and for concurrent events that could explain the market reaction in the event window (*Upgrade*, *Downgrade*, *Positive Surprise* and *Negative Surprise*). All these variables are defined in Table 4.A1 of the Appendix. α_m, α_i and α_c represent year-month and firm-fixed effects, respectively. The fixed effects subsume the intercept, and the year-month fixed effects subsume the *Post* main effect. I cluster standard errors at the firm-level and winsorize all continuous variables by year-quarter at the 1 and 99 percent levels.

4.6 Findings

First, I document the effect of the introduction of the minimum payment guarantee (MPG) on the coverage of undercovered firms on SeekingAlpha (SA). Next, I investigate the effect of the MPG incentive on information provision of undercovered firms on SA, and lastly it's impact on capital markets.

4.6.1 Coverage on SA

I examine whether the introduction of the MPG incentive in January 2018, increased coverage of undercovered firms on SA. Also, I conduct several robustness analyses to rule out alternative explanations for my findings.

4.6.1.1 Effect on Coverage

I present the results in Table 4.1. From Model (1) to (4), I add fixed effects, a pre trend variable and firm characteristics as covariates to control for differences in underlying economics. The $Treat \times Post$ coefficient is positive and statistically and economically significant in all Models of the *Coverage* tests, suggesting an effect of the MPG incentive on coverage on

SA.[83] The introduction of the MPG incentive has increased coverage of undercovered firms on SA significantly: coverage of firms in the lowest 25th percentile of pre period coverage had on average increased with 50 percent after the introduction of the MPG incentive.[84]

I expect the effect to be strongest for the firms with lowest pre period coverage, because they are most likely to be subject to the MPG incentive. Graphically, I show this in Subfigure (a) of Figure 4.2 in which I split the sample on the median of the pre period average coverage, and plot the trends separately for both groups based on the average coverage by year-quarter. As expected, the effect of the MPG incentive can be observed as a significant jump in average coverage at the introduction of the MPG incentive for the 'Below Median' group, while there is no apparent effect for the 'Above Median' group. This effect also persists after the introduction of the MPG incentive, suggesting that the MPG incentive had a permanent effect on the coverage of previously undercovered firms. In addition to the effect being clearly visible, I observe no significant pre trend in the 'Below Median' group versus the 'Above Median' group, suggesting no violation of the parallel trends assumption.

[83] Table 4.2 of the Online Appendix shows a similar result, using *No. of Articles* per firm year-quarter to calculate the outcome and treatment variables.

[84] I calculate this number as follows: the pre period average coverage of the bottom 25th percentile of firms in terms of pre period coverage is 0.125, which yields a treatment intensity of 0.875 ($1 - 0.125$), multiplying with the coefficient (0.210) yields 0.184, which is almost 1.5 times higher than the pre period average of coverage.

Table 4.1: Effect on Coverage

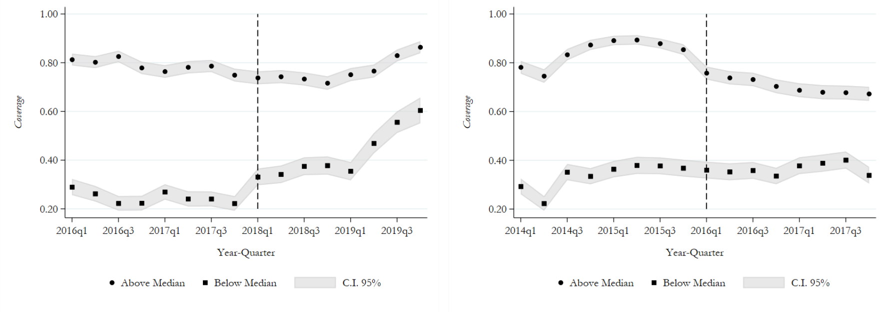
	Coverage			
	(1)	**(2)**	**(3)**	**(4)**
Treat	−0.103***			
	(−13.48)			
Post	−0.999***			
	(−403.08)			
Treat × *Post*	0.344***	0.305***	0.213***	0.210***
	(20.91)	(18.05)	(8.10)	(7.95)
N	26,705	26,705	26,705	26,705
Year-Quarters	16	16	16	16
Firms	2,361	2,361	2,361	2,361
Adj. R^2	0.301	0.320	0.321	0.323
Controls	No	No	No	Yes
Year-Quarter FE	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Treat × YQ	No	No	Yes	Yes

This table presents the results of the difference-in-differences tests evaluating the impact of the introduction of the minimum payment guarantee (MPG) incentive on the coverage of undercovered firms on SeekingAlpha (SA). I present coefficients (standard errors) for several Models. Model (1) is the base-line model, Model (2) includes year-quarter and firm fixed effects, Model (3) adds a control for a potential pre trend, and Model (4), the most stringent model, additionally controls for firm characteristics. *Coverage* is an indicator variable that is equal to one if the firm is covered by at least one SeekingAlpha (SA) article in a particular year-quarter, and zero otherwise. *Treat* is the average of *Coverage* over the firm year-quarters in the pre period. I subtract this number from 1 to simplify the interpretation of the treatment variable. *Post* is an indicator variable that is equal to one if the year-quarter is in the post period, and zero otherwise. I describe the included control variables in Section 4.4 and define them in Table 4.A1 of the Appendix. The MPG incentive was introduced in January 2018. The sample period consists of four years from January 2016 till December 2019, of which the first (last) two are pre (post) period years. All continuous variables are winsorized by year-quarter at the 1 and 99 percent levels. The standard errors are clustered at the firm level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

After January 2019, however, average coverage for both groups appears to increase. Subfigure (b) of Figure 4.2, which repeats the same analysis but then for the 2014–2017 period in which there is no change in contributor payment incentives, shows no significant increase in coverage for undercovered firms.[85]

Figure 4.2: Effect on *Coverage*

(a) Sample Period **(b) 2014–2017 Period**



This figure presents the year-quarter averages and 95 percent confidence intervals of *Coverage*. Subfigure (a) presents the averages for the test sample period 2016–2019, and Subfigure (b) presents the averages for the preceding period 2014–2017. I split the sample on the median of the pre period *Coverage*, i.e., 2016–2017 in Subfigure (a) and 2014–2015 in Subfigure (b). *Coverage* is an indicator variable that is equal to one if the firm is covered by at least one SeekingAlpha (SA) article in a particular year-quarter, and zero otherwise. The striped line in Subfigure (a) represents the introduction of the 'minimum payment guarantee' incentive scheme on SA in January 2018, while it marks the middle of the sample period in Subfigure (b), indicating a figurative introduction of an MPG incentive.

4.6.1.2 Robustness Tests

Overall, the results show that the coverage of undercovered firms on SA has increased after the introduction of the MPG incentive in January 2018. In this section, I examine the robustness of this result. I first examine whether the increase in coverage is a result of a change in underlying economics, and therefore endogenous. Next, I attempt to reduce the

[85] Figure 4.1 of the Online Appendix shows a similar result, using *No. of Articles* per firm year-quarter to calculate the outcome and treatment variables.

concern that my results are primarily driven by a regression to the mean effect, inherent in my research design. Lastly, I investigate whether potential concurrent changes on SA explain the increase in coverage.

Changes in Underlying Economics

I document that changes in firm characteristics that capture the underlying economics cannot fully explain the increase in coverage of undercovered firms following the introduction of the MPG incentive. Like traditional sell-side coverage, coverage on SA is an endogenous outcome, because a contributor consciously and independently determines what firms to cover from the universe of firms. Farrell et al. (2022) document that coverage on SA is strongly associated with, e.g., size, trading volume, and ownership, characteristics that are also associated with traditional analyst coverage (e.g., Bhushan, 1989). In Panel A of Table 4.1 of the Online Appendix, I present the variables that are associated with a firm being undercovered on SA. I find that characteristics related to size, ownership and stock market performance are significantly associated with undercoverage, complementing the result of Farrell et al. (2022). While the association between the firm characteristics and a firm being undercovered on SA does not change much after the introduction of the MPG incentive, Panel B of Table 4.1 of the Online Appendix shows that firm characteristics between undercovered firms in the pre period differ significantly from the post period, highlighting the need to control for these variables.

First, I include these variables in Model (4) of Table 4.1 and still report a significant positive effect of the MPG incentive on coverage of undercovered firms. Second, I repeat the main analysis using a constant sample of firms that are covered on SA at least once in the pre period and once in the post period. While this reduces my sample size by 50 percent, this ensures that the result is not driven by changes in sample composition. Untabulated results show a strong positive statistically and economically significant increase in coverage for undercovered firms, albeit with a slightly lower magnitude. Third, I repeat the main analysis with *Analyst Coverage* as a dependent variable, which is an indicator variable that is equal to one if the firm is covered by at least one I/B/E/S-analyst in a particular year-quarter, and zero otherwise. I present this result in Panel A of Table 4.2. I document no statistically or economically significant effect of the MPG incentive on undercovered firms for traditional analyst coverage. This shows that hypothetical differences in firm characteristics, or underlying economics, of undercovered firms between the pre and post period at least did not lead to more coverage by traditional analysts. Given the similar dynamics between coverage and firm characteristics for traditional analysts and social media analysts, this test strengthens my confidence in the result that the increase in coverage of undercovered firms is a direct result of the introduction of the MPG incentive.

Regression to the Mean

Next, I show that the 'regression to the mean'-effect, while acknowledging its inherent presence in my research design, cannot explain the results of the main analysis. 'Regression to the mean' (RTM) is the statistical phenomenon that subsequent measurement of a variable within a sample selected on the extremeness of that variable will result in a less extreme mean (see, e.g., Barnett, van der Pols, & Dobson, 2005; Samuels, 1991; S. M. Stigler, 1997). RTM is inherently present in my research design. First, the incentive structure is precisely designed to move the extreme bottom of firms in terms of coverage more closely to the mean. The MPG incentive therefore acts to reinforce an RTM-effect. Second, treatment is dependent on the outcome variable in the previous period. Selecting a sample based that is undercovered, which is by definition the bottom extreme of the coverage distribution, and remeasuring coverage for this sample in a subsequent period, will always pick up an RTM-effect to some degree. Although it is impossible to show my results absent any RTM-effect, I attempt to tackle the concern that my result is primarily driven by it in four ways.

First, I establish a baseline expectation of an RTM-effect. I repeat my main analysis over the period 2014–2017, in which there is no known change in contributor payment incentives. I report this result in Panel B of Table 4.2. In line with expectations of an inherently present RTM-effect, I report a positive economically and statistically significant $Treat \times Post$ coefficient, suggesting a significant increase in coverage of undercovered firms after two years. However, this coefficient is lower than the main result in magnitude, but not significantly different for Models (3) and (4).

Second, I directly estimate the RTM-effect as the difference in mean coverage for firms that receive no treatment in my research design, between the pre period and the post period.[86] I present this result in Panel C of Table 4.2. I subtract this directly estimated RTM-effect from the estimated $Treat \times Post$ coefficients and document that there is still a strong positive effect on coverage of undercovered firms in the sample period, but only a marginal effect in the 2014–2017 period.

Third, I divide the treatment variable in quintiles and estimate the average increase in coverage following the introduction of the MPG incentive for each quintile separately. I present these results in Panel D of Table 4.2. I document a monotonically increasing effect of the treatment over the quintiles, which is conform my expectations that the group with the highest treatment intensity has the biggest increase in coverage, and vice versa. For the sample period, four out of five quintiles

[86] Because the treatment variable is defined as $1 -$ mean pre period coverage, firms that are covered in all pre period year-quarters have a treatment variable that is equal to 0 in my research design. I also assume that the RTM-effect is symmetrical, i.e., equal between non-treated firms and firms along the distribution of the treatment variable.

see a significant increase in coverage following the introduction of the MPG incentive, suggesting an effect of the MPG incentive over the whole distribution of the treatment variable. Conversely, I find results that are in line with an RTM-effect for the 2014–2017 period, i.e., a strong significant result in opposite direction only for the extreme quintiles. Notably, the effect in the main sample period is significantly higher for all quintiles, compared to the 2014–2017 period, which I show graphically in Figure 3. These results are conform my expectation that significant effects documented in the 2014–2017 period are mainly attributable to the RTM-effect, while the significant effects in the main sample period are not.[87]

Lastly, I exclude the extreme deciles of the treatment variable. As RTM-effects propagate solely in the extremes of the distribution of a variable, removing these extremes from the sample results in a less-biased result. I present the results of the analyses excluding the extreme deciles for both periods in Panel E of Table 4.2. I still find a positive statistically and economically significant $Treat \times Post$ coefficient for the sample period, but not for the 2014–2017 period, suggesting a stronger influence of the RTM-effect on the results documented in the 2014–2017 period than for the sample period.

[87] A similar conclusion can be drawn from Figure 4.2 of the Online Appendix, using *No. of Articles* to calculate the outcome and treatment variables.

Table 4.2: Robustness Checks of Effect on Coverage

Panel A: Effect on SeekingAlpha vs Traditional Analyst Coverage

	SA Coverage	***Analyst Coverage***
	(1)	**(2)**
Treat × *Post*	0.327***	−0.001
	(27.69)	(−0.16)
N	50,057	50,057
Year-Quarters	16	16
Firms	4,041	4,041
Adj. R^2	0.326	0.885
Year-Quarter FE	Yes	Yes
Firm FE	Yes	Yes

Panel B: RTM-effect in 2014–2017 Period

	Coverage			
	(1)	**(2)**	**(3)**	**(4)**
Treat	−0.182***			
	(−22.04)			
Post	−0.998***			
	(−341.39)			
Treat × *Post*	0.299***	0.232***	0.156***	0.162***
	(17.53)	(13.18)	(5.20)	(5.38)
N	28,253	28,253	28,253	28,253
Year-Quarters	16	16	16	16
Firms	2,438	2,438	2,438	2,438
Adj. R^2	0.266	0.294	0.295	0.296
Controls	No	No	No	Yes
Year-Quarter FE	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Treat × YQ	No	No	Yes	Yes
Treat × *Post* Difference	0.044*	0.073***	0.057	0.048
	(1.87)	(3.00)	(1.42)	(1.20)

Panel C: Effect of *Treat* on *Coverage* minus the Directly Estimated RTM-effect			
	Sample Period	**2014–2017 Period**	**Difference**
Treat × *Post*	0.210***	0.162***	0.048
	(7.95)	(5.38)	(1.20)
RTM-effect	0.066***	0.103***	−0.037***
	(13.65)	(18.27)	(−5.01)
Treat × *Post* -/- RTM-effect	0.144***	0.058*	0.085**
	(5.36)	(1.91)	(2.10)

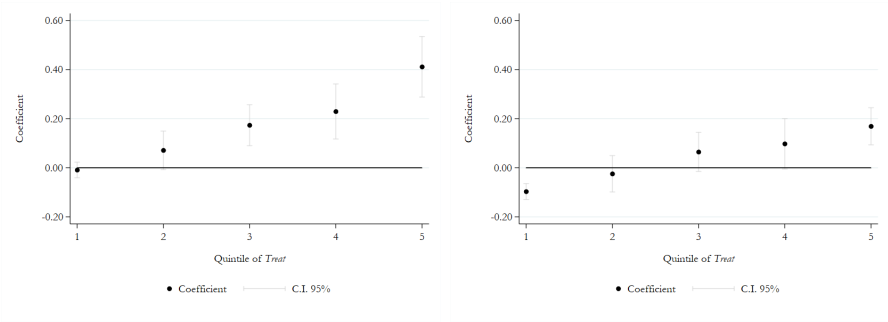
Panel D: Effect of *Treat* on *Coverage* by *Treat* Quintile			
	Sample Period	**2014–2017 Period**	**Difference**
Quintile 1 × *Post*	−0.009	−0.097***	0.088***
	(−0.54)	(−5.83)	(3.78)
Quintile 2 × *Post*	0.071*	−0.024	0.096*
	(1.78)	(−0.65)	(1.74)
Quintile 3 × *Post*	0.174***	0.065	0.109*
	(4.08)	(1.59)	(1.85)
Quintile 4 × *Post*	0.229***	0.098*	0.132*
	(4.01)	(1.87)	(1.70)
Quintile 5 × *Post*	0.411***	0.169***	0.242***
	(6.54)	(4.38)	(3.28)
N	25,745	26,682	
Year-Quarters	16	16	
Firms	2,149	2,098	
Adj. R^2	0.220	0.093	
Year-Quarter FE	Yes	Yes	
Firm FE	Yes	Yes	
Treat × YQ	Yes	Yes	
Controls	Yes	Yes	

Panel E: Effect of *Treat* on *Coverage* Excluding Extreme Deciles			
	Sample Period	**2014–2017 Period**	**Difference**
Treat × *Post*	0.202***	0.054	0.148***
	(7.01)	(1.55)	(3.25)
N	25,741	26,078	
Year-Quarters	16	16	
Firms	2,228	2,190	
Adj. R^2	0.307	0.268	
Controls	Yes	Yes	
Year-Quarter FE	Yes	Yes	
Firm FE	Yes	Yes	
Treat × YQ	Yes	Yes	

This table presents several robustness tests of the evaluation of the impact of the introduction of the minimum payment guarantee (MPG) incentive on the coverage of undercovered firms on SeekingAlpha (SA). In Panel A, I present the coefficients (standard errors) of the difference-in-differences tests evaluating the impact of the introduction of the MPG incentive on the SA analyst coverage and traditional sell-side analyst coverage. In Panel B, I establish a baseline 'regression to the mean'-effect (RTM-effect) and present the coefficients (standard errors) of the difference-in-differences tests using a figurative policy change on the coverage of undercovered firms on SA, using a preceding period from January 2014 till December 2017. I also show the difference between the estimated coefficients using the main sample period and the 2014–2017 period. In Panel C, I present the directly estimated RTM-effect as the difference in mean coverage for the firms that receive no treatment between the pre period and the post period. In Panel D, I present the coefficients (standard errors) of the difference-in-differences tests per quintile of *Treat*. In Panel E, I present the coefficients (standard errors) of the difference-in-differences tests excluding the most extreme deciles of *Treat*. In Panels C, D and E, I present the results for both the main sample period, from 2016 till 2019, of which the results are presented in Table 4.1, and the preceding period, from 2014 till 2017, of which the results are presented in Panel B of this Table. *Coverage* (*SA Coverage* in Panel A) is an indicator variable that is equal to one if the firm is covered by at least one SA article in a particular year-quarter, and zero otherwise. *Analyst Coverage* is defined similarly, but using I/B/E/S earnings forecasts. *Treat* is the average of *Coverage* over the firm year-quarters in the pre period. I subtract this number from 1 to simplify the interpretation of the treatment variable. *Post* is an indicator variable that is equal to one if the year-quarter is in the post period, and zero otherwise. I describe included control variables in Section 4.4 and define them in Table 4.A1 of the Appendix. The MPG incentive was introduced in January 2018. The main sample period consists of four years from January 2016 till December 2019, of which the first (last) two are pre (post) period years. The preceding period consists of four years from January 2014 till December 2017. All continuous variables are winsorized by year-quarter at the 1 and 99 percent levels. The standard errors are clustered at the firm level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

Figure 3: Difference-in-difference Coeficiencts by Quintile of *Treat*

(a) Sample Period **(b) 2014–2017 Period**



This figure presents the difference-in-differences coefficients and 95 percent confidence intervals for the quintiles of *Treat*. Subfigure (a) presents the results for the test sample period 2016–2019, and Subfigure (b) presents the results for the preceding period 2014–2017. The coefficient is β_3 from the following difference-in-differences regression: $Coverage = \beta_1 \cdot Treat + \beta_2 \cdot Post + \beta_3 \cdot Treat \times Post + Controls + Fixed\ Effects + \epsilon$, explained in Section 4.4. *Treat* is the average of *Coverage* over the firm year-quarters in the pre period, i.e., 2016–2017 in Subfigure (a) and 2014–2015 in Subfigure (b). I subtract this number from 1 to simplify the interpretation of the treatment variable. *Coverage* is an indicator variable that is equal to one if the firm is covered by at least one SeekingAlpha (SA) article in a particular year-quarter, and zero otherwise. Quintile 1 (5) comprises of firms with the lowest (highest) treatment intensity, because these are the firms with highest (lowest) average pre period *Coverage*.

Concurrent changes

I do not find support for the idea that concurrent changes on SA, i.e., unrelated to the introduction of the MPG incentive specifically, explain my findings. I thoroughly analyze the institutional details of SA and find that nothing related to coverage of undercovered firms has explicitly changed other than the introduced MPG incentive. Implicitly, if the primary reason for SA to introduce the MPG incentive is to promote coverage on undercovered firms, and SA subsequently promotes these articles, contributors potentially respond to the promotion instead of the financial incentive. This is a potential concern because SA contributors do not contribute primarily for remuneration (Toubia & Stephen, 2013). An analysis of 100 random incentivized articles, i.e., those covering undercovered firms in the post period, reveals that these articles receive no additional advertisement by SA. These specific articles are not linked, or mentioned, more often on the official SA Twitter (@SeekingAlpha) than other articles. Using Wayback Machine WebArchive snapshots of the SA homepage around the publication date of the incentivized articles reveals that these articles are not featured on the homepage of SA. As such, it is unlikely that the increase in coverage is a result of contributors seeking attention and reputation alone.

Overall, while I acknowledge that both underlying economics in the endogenous coverage decision, and 'regression to the mean'-effects play a role in my analyses, neither can fully explain my finding that the coverage of undercovered firms on SA has increased after the introduction of the MPG incentive in January 2018. Moreover, I find no concurrent changes on SA, explicit or implicit, that can explain the increase in coverage.

4.6.2 Information Provision on SA

I have established that the MPG incentive increases coverage of undercovered firms on SA. Next, I investigate the impact of this increased coverage on information provision on SA. I first examine what contributors cover these undercovered firms, and then examine whether the market perceives incentivized content different in terms of informativeness.

4.6.2.1 Contributor Characteristics

I investigate which contributor characteristics are associated with incentivized coverage of undercovered firms on SA, and whether these characteristics change after the introduction of the MPG incentive.

I present these results in Table 4.3. The dependent variable in Models (1) and (3) is an indicator variable that is equal to one if a contributor has covered an undercovered firm in the year-quarter, and zero otherwise. The dependent variable in Models (2) and (4) measures the contributor's intensity of covering undercovered firms, and is the percentage of articles that cover an undercovered firm to the total number of articles in the year-quarter. Columns (1) and (2) use the full sample of contributors, whereas Columns (3) and (4) use a sample of contributors that are active in the pre and post period to assess the impact of new contributors on the results.

I find that undercovered firms are covered by experienced contributors, although the magnitude of this association is reduced after the introduction of the MPG incentive. Contributor experience, measured by the total number of articles written before the year-quarter, is positively associated with covering an undercovered firm. Interestingly, experience in covering undercovered firms is negatively associated with covering an undercovered firm. It suggests that the number of contributors that move from one undercovered firm to the next to capture incentive payments, is very limited, since the magnitude of the effect has increased in the post period.[88]

I find that contributors who cover undercovered firms have more expertise. First, the contributors who cover undercovered firms have more specific industry expertise, measured by the concentration of articles covering one specific industry. Second, specific firm expertise is negatively associated with covering an undercovered firm. This suggests that contributors who cover a single or limited number of firms have invested time and resources into understanding that firm, and are therefore less likely to cover other firms. Together with the result on industry expertise this indicates that contributors with a broader industry expertise and thus for which the costs of covering an (additional) undercovered firm is lower, are more likely to cover these firms. Lastly, I find that skilled contributors, measured by the average abnormal market reaction to their previous articles, are more likely to cover undercovered firms. All these associations are similar in the post period, showing the limited impact of the MPG incentive.

Overall, the results for both samples of contributors are similar. This

[88] This effect is also partly mechanical. When a specific undercovered firm receives coverage, it is no longer undercovered.

suggests that the association between covering an undercovered firm and contributor characteristics have not changed as a result of new contributors offering their service on SA. This reduces the likelihood of a motivation crowding out effect as a result of new, less-effective, lower quality contributors responding to changes in payment incentives.

Taken together, the finding that experienced and expert contributors cover undercovered firms, even after the introduction of the MPG incentive, indicates that undercovered firms are covered by effective, higher quality contributors. The introduction of the MPG incentive has not changed this association significantly, apart from the experience result.

Table 4.3: Contributor Characteristics

	All Contributors		Contributors Active Pre– and Post 2018	
	Covered Under-covered	*%Covered Under-covered*	*Covered Under-covered*	*%Covered Under-covered*
	(1)	**(2)**	**(3)**	**(4)**
Tenure	0.006*	0.004	0.005	−0.002
	(1.92)	(1.03)	(1.12)	(−0.38)
Tenure × *Post*	−0.002	0.005	−0.014	−0.018
	(−0.36)	(0.71)	(−1.58)	(−1.60)
Experience	0.023***	0.053***	0.024***	0.061***
	(5.16)	(8.92)	(4.52)	(8.00)
Experience × *Post*	−0.010**	−0.009	−0.014***	−0.021**
	(−2.08)	(−1.21)	(−2.76)	(−2.54)
%Undercovered	−0.765***	−1.159***	−0.599***	−1.066***
	(−15.16)	(−17.40)	(−9.21)	(−10.99)
%Undercovered × *Post*	−0.055	−0.146**	−0.154***	−0.267***
	(−1.13)	(−2.13)	(−2.95)	(−3.43)
Expertise Industry	0.115***	0.225***	0.096**	0.244***
	(3.25)	(4.34)	(2.18)	(3.47)
Expertise Industry × *Post*	0.000	0.046	0.002	0.011
	(0.00)	(0.59)	(0.05)	(0.12)
Expertise Firm	−0.105*	−0.223***	−0.141*	−0.328***
	(−1.88)	(−2.87)	(−1.96)	(−3.06)
Expertise Firm × *Post*	0.013	−0.036	−0.007	−0.073
	(0.18)	(−0.30)	(−0.08)	(−0.50)
Contributor Skill	0.041***	0.058***	0.017	0.041*
	(2.61)	(3.05)	(0.89)	(1.66)
Contributor Skill × *Post*	−0.020	−0.027	−0.021	−0.035
	(−1.04)	(−0.96)	(−1.00)	(−1.09)
#Followers × *Post*	−0.001	−0.005	−0.002	−0.008
	(−0.08)	(−0.40)	(−0.33)	(−0.70)
Certification × *Post*	0.034	0.084	0.039	0.080
	(0.87)	(1.50)	(1.00)	(1.42)
Professional × *Post*	0.043	−0.009	0.041	−0.015
	(1.14)	(−0.14)	(1.11)	(−0.24)
Company Linked × *Post*	−0.002	0.019	0.007	0.032
	(−0.11)	(0.53)	(0.33)	(0.89)
N	14,818	14,818	9,674	9,674
Year-Quarters	16	16	16	16
Contributors	2,969	2,969	1,316	1,316
Adj. R^2	0.347	0.508	0.342	0.540
Year-Quarter FE	Yes	Yes	Yes	Yes
Contributor FE	Yes	Yes	Yes	Yes

This table presents which contributor characteristics are associated with covering undercovered firms on SeekingAlpha (SA). I present coefficients (standard errors) of the tests using two dependent variables that together capture whether, and to what extent, contributors cover undercovered firms. *Covered Undercovered* is an indicator variable that is equal to one if a contributor covers an undercovered firm with at least one article, and zero otherwise. *%Covered Undercovered* is the total number of contributors' articles covering undercovered firms divided by the total number of contributors' articles covering all firms. I use two samples: Models (1) and (2) use the whole sample of SA contributors, and Models (3) and (4) use a sample of contributors that have covered at least one firm in the pre period and one firm in the post period. A firm is undercovered if the articles covering that firm are eligible for the MPG incentive payment. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days, calculated at the beginning of every month. All other variables are defined in Table 4.A1 of the Appendix. The MPG incentive was introduced in January 2018. The sample period consists of four years from January 2016 till December 2019, of which the first (last) two are pre (post) period years. All continuous variables are winsorized by year-quarter at the 1 and 99 percent levels. I include both year-quarter and contributor fixed effects. The standard errors are clustered at the contributor level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

4.6.2.2 Article Informativeness

Next, I examine whether MPG incentivized articles are less informative to investors by assessing market reactions to their publication. I present the results of this analysis in Table 4.4. For Models (1) – (5), I measure market reactions using cumulative absolute abnormal return, and for Models (6) – (10), I use abnormal volume.

I descriptively confirm findings of prior studies that SA articles are informative to investors. The publication of an SA article is associated with a significant and abnormal absolute return and trading volume. For the sample used in this analysis, I find an average cumulative absolute abnormal return of 3.34 percent (t-stat. $= 22.75$) and an abnormal volume of 0.004 (t-stat. $= 10.23$) (untabulated).

Overall, informativeness of articles that cover undercovered firms has not changed after the introduction of the MPG incentive. In Models (1)–(3) and (6)–(8), the coefficient on $Treat \times Post$ is not economic or statistically significant for both market reaction measures. The results are similar when I include several types of fixed effects and an extensive set of controls. I do find a statistically significnat difference in cumulative absolute abnormal return between articles that cover undercovered firms and well-covered firms.[89] However, this result only captures a level difference in informativeness between undercovered and well-covered firms, and firm-level controls diminish the significance of this association.

When I correct for the sample imbalance between undercovered and well-covered firms, I find stronger results. Because only 12 percent of the total sample of articles cover undercovered firms, the coefficients on *Treat* and $Treat \times Post$ may suffer from lower statistical power. I address this in two ways: I entropy balance the sample, and select a control group using propensity score matching. I entropy balance the sample at the third moment on size (*Market Cap,*), industry (Fama-French 48 industries) and year-month. I find a positive and statistically significant coefficient on $Treat \times Post$ in Model (4). I then match an incentivized

[89] The average cumulative absolute abnormal return is at most 12 percent lower for articles that cover undercovered versus well-covered firms.

article covering an undercovered firm with an non-incentivized article covering a well-covered firm on size, industry and year-month. I again find a significantly higher market reaction to incentivized articles, after the introduction of the MPG incentive. For market reaction in terms of volume, the inferences from the previous paragraph are similar.

The results indicate that SA articles are informative in my sample, and the informativeness of incentivized articles that cover undercovered firms remains similar, or has even increased, after the introduction of the MPG incentive.

Table 4.4: Article Informativeness

	CAAR					***AVOL***				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
Post	0.000					−0.001*				
	(0.13)					(−1.86)				
Treat	0.002	−0.003***	−0.002**	−0.004***	−0.002	0.001	0.000	0.000	0.000	0.000
	(1.04)	(−3.06)	(−2.12)	(−2.80)	(−1.44)	(0.75)	(0.73)	(0.24)	(−0.88)	(−0.28)
Treat × *Post*	0.001	0.002	0.002	0.004**	0.003**	−0.001	−0.001	0.000	0.000	0.000
	(0.48)	(1.49)	(1.63)	(2.45)	(2.08)	(−1.23)	(−1.43)	(−0.56)	(0.37)	(0.50)
N	59,414	59,414	59,414	57,996	13,863	59,414	59,414	59,414	57,996	13,863
N *Treat* = 1	7,283	7,284	7,285	7,211	7,211	7,283	7,284	7,285	7,211	7,211
Year-Months	46	47	48	48	48	46	47	48	48	48
Firms	1,546	1,547	1,548	1,527	1,453	1,546	1,547	1,548	1,527	1,453
Authors	2,376	2,377	2,378	2,355	1,213	2,376	2,377	2,378	2,355	1,213
Adj. R^2	0.000	0.304	0.382	0.434	0.359	0.001	0.123	0.739	0.767	0.736
Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Year-Month FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Contributor FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Entropy Balanced	No	No	No	Yes	No	No	No	No	Yes	No
Matched	No	No	No	No	Yes	No	No	No	No	Yes

This table presents results of the difference-in-differences tests evaluating SeekingAlpha (SA) article informativeness, measured by the market reaction to an article's publication, after the introduction of the minimum payment guarantee (MPG) incentive. I present coefficients (standard errors) for several Models that become increasingly more stringent. Models (1) and (6) are the baseline models, Models (2) and (7) introduce year-month, firm, and contributor fixed effects, Models (3) and (8) additionally controls for firm characteristics. I address the lower statistical power resulting from asymmetry between treated and control firms in my sample in the remaining Models. In Models (4) and (9), I entropy balance the sample at the third moment on size (*Market Cap,*), industry (Fama-French 48 industries) and year-month. In Models (5) and (10), I use propensity score matching to an article covering an treated firm with an article covering a control firm on size, industry and year-month. I use two dependent variable to capture the market reaction to the publication of an SA article. Models (1)–(5) use *CAAR*, which is the sum of the absolute difference between the return of the firm that is covered by the article and the value-weighted market portfolio, summed over the event window $t \in [0, 1]$, where $t = 0$ is the publication date of the article. Models (6)–(10) use *AVOL*, which is the difference between the average share turnover, trading volume divided by shares outstanding, in the event window $t \in [0, 1]$, where $t = 0$ is the publication date of the article, and the average share turnover in the estimation window $[-60, -1]$. *Treat* is an indicator variable that is equal to one if the article covering a firm is eligible for the MPG incentive payment, and zero otherwise. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days. *Treat* is defined once per month at the beginning of the month, and therefore all articles for a firm in a given month are either treated, or control. *Post* is an indicator variable that is equal to one if the publication date is equal to, or later than, January 1, 2018, and zero otherwise. All other variables are defined in Table 4.A1 of the Appendix. The MPG incentive was introduced in January 2018. The sample period consists of four years from January 2016 till December 2019, of which the first (last) two are pre (post) period years. All continuous variables are winsorized by year-quarter at the 1 and 99 percent levels. The standard errors are clustered at the firm level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

I find that, following the increase in coverage of undercovered firms after the introduction of the MPG incentive on SA, information provision on SA has improved for undercovered firms. Undercovered firms are covered by effective, higher quality contributors, which did not change after the introduction of the MPG incentive. In addition, incentivized coverage is equally informative to investors as non-incentivized coverage, as suggested by the market reaction to the publication of incentivized articles.

4.6.3 Capital Markets Effects

Next, I investigate the effect of improved information provision on SA on capital market outcomes. I examine measures commonly associated with information intermediaries: information asymmetry and price discovery.

Whether incentivized social media analyst coverage lowers information asymmetry and increases price discovery is an empirical question. Social media analysts act similar to traditional sell-side analysts, which are shown to reduce information asymmetries and increase price discovery, but the value of the activities of social media analysts to market participants are more debated. On the one hand, informative incentivized coverage can reduce information asymmetries and improve price discovery, as several studies document capital market benefits of social media analyst coverage (see, e.g., Bartov et al., 2018; Chen et al., 2014; Farrell et al., 2022; Jame et al., 2016). On the other hand, incentivized coverage can have no or even an adverse effect on information asymmetries and price discovery, as several studies find drawbacks of social media analyst coverage (see, e.g., Drake et al., 2017; Dyer & Kim, 2021; Kogan et al., 2021). Even though investors value the information dissemination role in addition to the information creation role of social media analysts, the breadth of the additional coverage as a result of the MPG incentive may not be enough to have a strong effect on information asymmetry.

I investigate the effect of incentivized coverage on capital market outcomes by examining whether coverage of undercovered firms in the period after the introduction of the MPG incentive, is associated with decreased information asymmetry and increased price discovery. I am interested in the levels effect of incentivized SA coverage, hence I focus solely on the sample of undercovered firms after the incentives are introduced. Next, I outline the estimated models and present the effect of incentivized coverage on information asymmetry and price discovery for undercovered firms.

4.6.3.1 Information Asymmetry

I estimate the effect of incentivized coverage of undercovered firms on information asymmetry on the quarterly level, and around the earnings announcement. On the quarterly level, I examine the association between incentivized coverage and information asymmetry in general, which does not require a prediction regarding frequency and timing of coverage. Around the earnings announcement, I examine whether incentivized coverage plays a role in reducing information asymmetry during an information event that is important (Ball & Shivakumar, 2008; Basu, Duong, Markov, & Tan, 2013) and strongly associated with SeekingAlpha coverage.[90]

I examine the association between incentivized coverage and information asymmetry using a similar setup to, e.g., Hope and Wang (2017). Specifically, I estimate the following model:

$$y_{i,q} = \beta_1 \cdot \mathit{Covered}_{i,q} + \mathit{Controls} + \alpha_i + \alpha_q + \epsilon_{i,q}, \tag{9}$$

on the firm(i)-quarter(q) level. I include a firm in the sample if it is undercovered in two out of three months of the quarter.[91] A firm is undercovered if the articles covering that firm are eligible for the MPG incentive payment. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days, calculated at the beginning of every month. *Covered* is an indicator variable that is equal to one if the undercovered firm receives coverage in a specific year-quarter, and zero otherwise. y is a placeholder for four variables that capture information asymmetry. I examine multiple measures to increase the robustness of the result. *Bid-Ask Spread* is the quarterly average of daily bid-ask spreads from CRSP, *Amihud Illiquidity* is the quarterly average of the daily Amihud (2002) illiquidity measure,

[90] In Figure 4.3 of the Online Appendix, I show the frequency of SA articles and earnings announcements per day of the year-quarter relative to the previous year-quarter end date. The largest number of articles appear to be published around earnings announcements.

[91] I get similar results if I change this requirement to be one out of three months, or the first two months of the year-quarter (untabulated).

Share Turnover is the quarterly average of daily trading volume divided by the quarterly average of shares outstanding, and *Dollar Volume* is the natural log of one plus quarterly average of daily dollar trading volume. I control for firm characteristics that are associated with information asymmetry. I include *Market Cap* to control for size, *BTM* to control for growth opportunities, and *ROA* and *%ΔROA* to control for performance. I control for differences in (institutional) ownership (*%Inst. Own.* and *#Owners*), the extent of the earnings news (*Surprise* and *Bad News*), prior quarter's stock market performance (*Volatility*, *Price* and *Return*), and prior quarter's stock market liquidity (*Share Turnover*, *Depth*, and *Bid-Ask Spread*).[92] All these variables are defined in Table 4.A1 of the Appendix. α_q and α_i are year-quarter and firm-fixed effects, respectively. The fixed effects subsume the intercept. Coefficient β_1 captures the effect of being covered on SA as an undercovered firm. If coverage on SA for undercovered firms is associated with lower information asymmetry, I expect the coefficient β_1 to be significant and negative for the tests using bid-ask spread and Amihud (2002) illiquidity as dependent variables, and positive for the tests using volume measures as dependent variables. I cluster standard errors at the firm-level and winsorize all continuous variables by year-quarter at the 1 and 99 percent levels.

[92] To reduce the possibility of over-fitting, I do not control for prior quarter's *Bid-Ask Spread* in the tests using bid-ask spread as dependent variable, and not for prior quarter's *Share Turnover* in the tests using volume measures as dependent variables. This is because these measures are sometimes sticky.

I present the results of this analysis in Panel A of Table 4.5. 47 percent of undercovered firms in my test sample receive coverage on SA, up from 38 percent before the introduction of the MPG incentive (untabulated), which is in line with the documented result that the MPG incentive increases coverage. I find that incentivized coverage is associated with lower information asymmetry for undercovered firms. For undercovered firms, incentivized coverage on SA is associated with a statistically significantly lower bid-ask spread and Amihud (2002) illiquidity, and a statistically significant higher share turnover and dollar volume. This difference is economically significant as well: on average, covered firms have a 4.5 percent lower bid-ask spread, 12.4 percent lower Amihud (2002) illiquidity, a 12.2 percent higher share turnover and a 0.57 percent higher dollar volume.[93] Even though my sample is relatively balanced in terms of number of undercovered firms that receive incentivized coverage versus undercovered firms that do not, it is possible that significant differences between these firms drive my results. I entropy balance the sample at the third moment on size (*Market Cap,*), industry (Fama-French 48 industries) and year-quarter and find statistically and economically similar results. In addition, I use propensity score matching to match a covered firm with a non-covered firm on size, industry and year-quarter, and again find statistically and economically similar results. I present

[93] I calculate these numbers as the ratio between the coefficient of interest from Panel A of Table 4.5 and the unconditional mean of the specific information asymmetry measure of non-covered sample firms ($Covered = 0$).

these results in Panel A of Table 4.4 of the Online Appendix.

Next, I examine the association between incentivized coverage and information asymmetry during the earnings announcement using a similar setup to, e.g., Blankespoor, Miller, and White (2014). Specifically, I estimate the following model:

$$y_{i,t} = \beta_1 \cdot Covered_{i,t} + Controls + \alpha_i + \alpha_q + \epsilon_{i,t}, \tag{10}$$

on the firm(i)-earnings announcement window(t) level. I include a firm in the sample if it is undercovered on the date of the earnings announcement. A firm is undercovered if the articles covering that firm are eligible for the MPG incentive payment. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days, calculated at the beginning of every month. *Covered* is an indicator variable that is equal to one if the undercovered firm receives coverage in a specific earnings announcement window, and zero otherwise. y is a placeholder for four variables that capture abnormal information asymmetry around the earnings announcement. *Abnormal Bid-Ask Spread* is average daily bid-ask spread in the earnings announcement window, minus the average daily bid-ask spread in the estimation window from TAQ.[94] I measure the other three measures, *Abnormal Amihud Illiquidity*, *Abnormal Share Turnover* and *Abnormal Dollar Volume*, as the earnings announcement window average of the daily measure, minus the estimation window average of the daily measure, divided by the standard deviation of the measure over the estimation window. Daily measures are calculated similarly to the previous test. For calculating window measures, I use a five-day earnings announcement window, defined as $t \in [-2; +3]$, and a 45-day estimation window, defined as $t \in [-50; -5]$, where $t = 0$ is the date of the earnings announcement. I include the same set of controls as for the quarterly tests. In addition, I control for the abnormal absolute market reaction to the earnings news (*Abnormal Absolute Return*), analyst coverage of the earnings announcement (*#Estimates*), and the delay in the news relative to the quarter-end and the expected announcement date based on last-years announcement date (*#Days after CD* and *#Days after ED*). All these variables are defined in Table 4.A1 of the Appendix. α_q and α_i are year-quarter and firm-fixed effects, respectively, and these fixed effects subsume the intercept. I cluster standard errors at the firm-level and winsorize all continuous variables by year-quarter at the 1 and 99 percent levels. Coefficient β_1 captures the effect of being covered on SA

[94] I use TAQ data for measuring bid-ask spreads in this test because it gives an inter-day average spread, whereas CRSP gives the end-of-day spread. Since my window comprises only a few days, this gives a better representation of information asymmetry around the earnings announcement.

as an undercovered firm. If coverage on SA for undercovered firms is associated with lower information asymmetry, I expect the coefficient β_1 to be significant and negative for the tests using abnormal bid-ask spread and abnormal Amihud (2002) illiquidity as dependent variables, and positive for the tests using abnormal volume measures as dependent variables. Because the number of undercovered firms receiving coverage during the earnings announcement is disproportionate to the undercovered firms receiving no coverage, the coefficient on *Covered* may suffer from lower statistical power. I entropy balance my sample at the third moment on size (*Market Cap,*), industry (Fama-French 48 industries) and year-quarter.[95]

I present the results of this analysis in Panel B of Table 4.5. Earnings announcement coverage of undercovered firms is 10.6 percent, up from 7.1 percent in the period before the MPG incentives (untabulated). This is again in line with the documented result that the MPG incentive increases coverage. Overall, I find that incentivized coverage is associated with lower information asymmetry for undercovered firms during earnings announcements. For undercovered firms, coverage on SA is associated with a statistically significantly lower abnormal bid-ask spread and abnormal

[95] I find similar results when I do not entropy balance my sample, but no statistically significant results when I use propensity score matching on the same criteria (size, industry and year-quarter). This can be explained by the very low number of observations (611) resulting from the matching process in combination with the low degrees of freedom resulting from all controls and fixed effects. I present these results in Panel B of Table 4.4 of the Online Appendix.

Amihud (2002) illiquidity, and a statistically significant higher abnormal share turnover and abnormal dollar volume. Again, these results are economically significant. On average, covered firms have a 10.9 percent higher abnormal share turnover and a 10.8 percent higher abnormal dollar volume. More pronounced are the effects on bid-ask spreads and Amihud (2002) illiquidity. Abnormal bid-ask spread are 43.3 percent lower for undercovered firms that receive coverage during the earnings announcement, and abnormal Amihud (2002) illiquidity is equally large but in the *opposite* direction.[96] This suggests that, following incentivized coverage on SA, undercovered firms' liquidity is higher during the period of the earnings announcement than in the period before, compared to undercovered firms that receive no incentivized coverage.

I argue that the improvement in information asymmetry that is associated with incentivized coverage indicates improved market efficiency. First, I show in Panel A of Table 4.5 of the Online Appendix that the effect of incentivized coverage on the quarterly level of information asymmetry (Panel B), and information asymmetry around earnings announcements (Panel C), is larger when sell-side analyst coverage is below the sample median. This confirms my expectation that the benefits of social media analysts to capital market participants is larger when other information intermediaries are underrepresented or absent. Second, I

[96] I calculate these numbers as the ratio between the coefficient of interest from Panel A of Table 4.5 and the unconditional mean of the specific information asymmetry measure of non-covered sample firms ($Covered = 0$).

show in Panel B of Table 4.5 of the Online Appendix that the effect of incentivized coverage on the quarterly level of information asymmetry is monotonically increasing in the number of articles appearing.[97].

Taken together, I document statistically and economically significant reductions in information asymmetry for undercovered firms that receive incentivized coverage, both on the quarterly level and during a significant information event. This suggests that incentivized social media analyst coverage helps reduce information asymmetries between investors.

[97] This is a similar finding to Stoumbos (2021), who finds that more information events (earnings announcements) result in a lower level and slower growth of information asymmetry.

Table 4.5: Information Asymmetry

Panel A: Quarterly Level of Information Asymmetry				
	Bid-Ask Spread	***Amihud Illiquidity***	***Share Turnover***	***Dollar Volume***
	(1)	**(2)**	**(3)**	**(4)**
Covered	−0.013***	−0.004***	0.001***	0.090***
	(−3.20)	(−4.61)	(9.35)	(9.00)
Market Cap.	−0.237***	−0.028***	−0.001*	0.638***
	(−7.01)	(−3.96)	(−1.86)	(10.22)
ROA	0.012	−0.038*	−0.001	0.286
	(0.14)	(−1.74)	(−0.27)	(1.26)
%Δ*ROA*	0.000	0.000	0.000	−0.001
	(−0.35)	(−0.36)	(−1.16)	(−1.01)
Leverage	−0.017*	0.000	0.000**	0.010
	(−1.68)	(0.20)	(2.14)	(0.62)
BTM	0.041	0.008	0.002**	0.043
	(1.60)	(1.35)	(2.35)	(0.78)
%Inst. Own.	0.005	0.001	0.000	0.004
	(1.39)	(1.07)	(0.23)	(0.26)
#Owners	−0.003	0.001	0.000	−0.010
	(−0.53)	(0.41)	(−1.02)	(−0.92)
$Volatility_{-1}$	0.078***	−0.009	0.003***	0.312***
	(2.76)	(−1.38)	(5.17)	(6.20)
$Price_{-1}$	0.180***	0.012*	0.003***	0.295***
	(5.27)	(1.82)	(3.72)	(4.11)
$Return_{-1}$	0.025	−0.004	0.001*	0.028
	(1.41)	(−0.95)	(1.69)	(0.69)
$Share\ Turnover_{-1}$	−8.642***	−0.761***		
	(−7.48)	(−3.01)		
$Depth_{-1}$	0.184***	−0.009***	0.000	0.028
	(15.47)	(−3.03)	(1.44)	(1.06)
$Bid\text{-}Ask\ Spread_{-1}$		0.077***	0.000	0.051
		(7.01)	(−0.03)	(0.75)
Surprise	0.200**	0.000	0.012***	0.665***
	(2.07)	(0.01)	(4.94)	(3.19)
Bad News	−0.004	−0.002*	0.000	−0.015*
	(−1.05)	(−1.86)	(0.94)	(−1.74)
N	9,800	9,800	9,800	9,800
N *Covered* = 1	4,667	4,667	4,667	4,667
Year-Quarters	8	8	8	8
Firms	1,836	1,836	1,836	1,836
Adj. R^2	0.892	0.837	0.721	0.964
Year-Quarter FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Panel B: Information Asymmetry around Earnings Announcements				
	Abnormal Bid-Ask Spread	*Abnormal Amihud Illiquidity*	*Abnormal Share Turnover*	*Abnormal Dollar Volume*
	(1)	**(2)**	**(3)**	**(4)**
Covered	−0.007*	−0.084**	0.075*	0.074*
	(−1.67)	(−2.26)	(1.89)	(1.89)
Market Cap.	0.066***	0.035	0.012	−0.028
	(2.96)	(0.24)	(0.08)	(−0.198)
ROA	−0.055	−0.432	1.255***	1.364***
	(−0.87)	(−1.10)	(3.08)	(2.67)
%ΔROA	0.000	−0.002	0.003	0.001
	(−0.02)	(−0.688)	(0.76)	(0.38)
Leverage	0.026***	0.051	−0.032	−0.059
	(3.09)	(1.49)	(−0.54)	(−1.03)
BTM	0.031**	0.217**	−0.265**	−0.227**
	(2.30)	(2.57)	(−2.20)	(−2.21)
%Inst. Own.	−0.022***	−0.097*	0.093	0.130**
	(−4.34)	(−1.75)	(1.64)	(2.34)
#Owners	−0.005***	−0.021	0.003	0.007
	(−3.40)	(−1.11)	(0.15)	(0.44)
$Volatility_{-1}$	−0.069***	−0.866***	−0.647***	−0.573***
	(−3.00)	(−7.04)	(−5.30)	(−4.79)
$Price_{-1}$	−0.036*	0.332*	−0.076	−0.258
	(−1.83)	(1.95)	(−0.51)	(−1.63)
$Return_{-1}$	−0.003	0.169*	0.017	−0.021
	(−0.20)	(1.85)	(0.20)	(−0.24)
$Share\ Turnover_{-1}$	3.320***	23.517***		
	(3.69)	(4.67)		
$Depth_{-1}$	−0.021***	0.138***	−0.201***	−0.231***
	(−2.79)	(2.64)	(−3.14)	(−3.41)
$Bid\text{-}Ask\ Spread_{-1}$		−0.252**	0.261*	0.383**
		(−2.08)	(1.86)	(2.46)
#Estimates	0.019	−0.021	0.185*	0.186*
	(1.39)	(−0.24)	(1.89)	(1.78)
Surprise	0.031	0.343	0.262	−0.034
	(0.45)	(0.78)	(0.48)	(−0.06)
Bad News	0.003	0.025	0.053*	0.016
	(0.91)	(0.97)	(1.73)	(0.52)
Abnormal Absolute Return	−0.079***	0.626***	3.828***	3.723***
	(−2.77)	(3.19)	(15.76)	(15.99)
#Days After CD	−0.026**	−0.041	0.243**	0.274**
	(−2.28)	(−0.48)	(2.19)	(2.50)
#Days After ED	−0.004*	0.007	−0.003	−0.001
	(−1.75)	(0.44)	(−0.17)	(−0.06)
N	4,963	4,963	4,963	4,963
N *Covered* = 1	527	527	527	527
Year-Quarters	8	8	8	8
Firms	1,304	1,304	1,304	1,304
Adj. R^2	0.191	0.371	0.583	0.579
Year-Quarter FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Entropy Balanced	Yes	Yes	Yes	Yes

This table presents results for the tests evaluating differences in information asymmetry between undercovered firms that receive incentivized coverage, and undercovered firms that do not. In Panel A, I present coefficients (standard errors) of the tests using quarterly levels of information asymmmetry. For this Panel, I include a firm in the sample if it is undercovered in two out of three months of the year-quarter. I test four measures of information asymmetry. *Bid-Ask Spread* is the quarterly average of daily bid-ask spreads from CRSP, *Amihud Illiquidity* is the quarterly average of the daily Amihud (2002) illiquidity measure. *Share Turnover* is the quarterly average of daily trading volume divided by the quarterly average of shares outstanding, and *Dollar Volume* is the natural log plus one of quarterly average of daily dollar trading volume. In Panel B, I present coefficients (standard errors) of the tests using information asymmetry around the earnings announcement. For this Panel, I include a firm in the sample if it is undercovered on the date of the earnings announcement. I address the lower statistical power resulting from asymmetry between covered and non-covered firms in my sample by entropy balancing the sample at the third moment on size (*Market Cap,*), industry (Fama-French 48 industries) and year-month. *Abnormal Bid-Ask Spread* is average daily bid-ask spread in the earnings announcement window, minus the average daily bid-ask spread in the estimation window from TAQ. I measure the other three measures, *Abnormal Amihud Illiquidity*, *Abnormal Share Turnover* and *Abnormal Dollar Volume*, as the earnings announcement window average of the daily measure, minus the estimation window average of the daily measure, divided by the standard deviation of the measure over the estimation window. Daily measures are calculated similarly to Panel A. For calculating window measures, I use a five-day earnings announcement window, defined as $t \in [-2; +3]$, and a 45-day estimation window, defined as $t \in [-50; -5]$, where $t = 0$ is the date of the earnings announcement. *Covered* is an indicator variable that is equal to one if the undercovered firm receives coverage in the year-quarter (earnings announcement window), and zero if otherwise. A firm is undercovered if the articles covering a firm are eligible for the MPG incentive payment. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days, calculated at the beginning of every month. All other variables are defined in Table 4.A1 of the Appendix. The MPG incentive was introduced in January 2018. The sample period consists of four years from January 2016 till December 2019, of which the first (last) two are pre (post) period years. All continuous variables are winsorized by year-quarter at the 1 and 99 percent levels. The regressions include year-quarter and firm fixed effects. The standard errors are clustered at the firm level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

4.6.3.2 Price Discovery

Finally, I test the effect of coverage following the MPG incentive on price discovery using a similar setup to, e.g., Guest (2021). Specifically, I examine differences in market reactions to earnings surprises, the 'earnings response coefficient', between covered and non-covered firms using the following setup:

$$\begin{aligned} AbnormalReturn_{i,t} = \beta_1 \cdot \textit{Surprise Decile}_{i,t} + \beta_2 \cdot \textit{Covered}_{i,t} \\ + \beta_3 \cdot \textit{Surprise Decile} \times \textit{Covered}_{i,t} \\ + \textit{Controls} + \textit{Surprise Decile} \times \textit{Controls} \\ + \alpha_q + \epsilon_{i,q}, \end{aligned} \tag{11}$$

on the firm(i)-quarter(q) level. I include a firm in the sample if it is undercovered on the date of the earnings announcement. A firm is undercovered if the articles covering that firm are eligible for the MPG incentive payment. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days, calculated at the beginning of every month. *Covered* is an indicator variable that is equal to one if the undercovered firm receives coverage in a specific earnings announcement window, and zero otherwise. *Abnormal Return* is the market-adjusted buy-and-hold return over the earnings announcement window. For calculating window measures, I use a three-day earnings announcement window, defined as $t \in [-1; +1]$, where $t = 0$ is the date of the earnings announcement. *Surprise Decile* is the difference between actual earnings per share in I/B/E/S and the median analyst forecast, scaled by the stock price at the prior year-quarter end. I decile-rank this variable to reduce the effects of outliers. I control for firm characteristics that are associated with the stock price reaction to earnings news. I control for size (*Market Cap* and *#Employees*) and growth opportunities (*BTM*). I control for differences in (intstitutional) ownership (*%Inst. Own.* and *#Owners*), past year-quarter stock market performance (*Volatility* and *Price*). I include an indicator variable for bad news, i.e., a negative *Surprise*. I control for analyst coverage of the earnings announcement (*#Estimates*) to control for the direct effect of coverage on the stock market reaction to earnings surprise, and for differences in coverage bias that is not captured by included firm characteristics. I control for the number of 8Ks per year-quarter (*#8Ks*) for this last reason as well. Lastly, I control for the delay in the news relative to the quarter-end and the expected announcement date based on last-years announcement date (*#Days after CD* and *#Days after ED*). I include the interaction of the earnings surprise decile with all respective control variables, as well as the interaction of the earnings surprise decile with year-quarter fixed effects. All the variables are defined in Table 4.A1 of the Appendix. α_q are year-quarter fixed effects. I do not include firm-fixed effects, because including them and their interaction with *Surprise Decile* lowers the degrees of freedom too much and results in a test with low statistical power. The fixed effects subsume the intercept. Coefficient β_3 captures the difference in earnings response coefficient between undercovered firms that receive coverage as a result of the MPG incentive, and those that do not. If coverage on SA for undercovered firms is associated with lower higher price discovery, I expect the coefficient β_3 to be significant and positive. I cluster standard errors at the firm-level and winsorize all continuous variables by year-quarter at the 1 and 99 percent levels. Again, the coefficient on *Covered* may suffer from lower statistical power because the number of undercovered firms receiving incentivized coverage is disproportionate to the number of undercovered firms receiving no incentivized coverage. I therefore entropy balance my sample at the third moment on size (*Market Cap,*), industry (Fama-French 48 industries) and year-quarter.[98]

I present the result of this test in Table 4.6. I find that incentivized coverage of undercovered firms is associated with statistically and economically significant increases in price discovery. I document that 6.0 percent of my sample of firms are covered in the earnings announcement

[98] I find similar results when I do not entropy balance my sample, but no statistically significant results when I use propensity score matching on the same criteria (size, industry and year-quarter). This can be explained by the very low number of observations (720) resulting from the matching process in combination with the low degrees of freedom resulting from all controls and fixed effects. I present these results in Panel C of Table 4.4 of the Online Appendix.

window, up from 4.3 percent in the period before the introduction of the MPG incentive (untabulated). I document that the coefficient on *Surprise Decile* × *Covered* is positive and statistically significant in all specifications and therefore robust for an extensive set of controls and specifications. The increase in earnings response coefficient is economically significant as well. The earnings response coefficient is between 13.4 percent and 55.9 percent higher for undercovered firms that receive incentivized coverage versus undercovered firms that receive no incentivized coverage.[99] This suggests that, following incentivized coverage on SA, the stock market response to earnings news for undercovered firms' is more efficient, compared to undercovered firms that receive no incentivized coverage.

I find that the increase in price discovery that is associated with incentivized coverage indicates improved market efficiency. First, I show in Panel D of Table 4.5 of the Online Appendix that the price discovery effects are greatest when traditional sell-side analyst coverage is below the sample median. This confirms my expectation that the benefits of social media analysts to capital market participants is larger when other information intermediaries are underrepresented or absent. Second, the increase in price discovery is not a result of investors overreacting

[99] I calculate this as the ratio between the coefficient on *Surprise Decile* and the coefficient on *Surprise Decile* × *Covered*. 13.4 percent is an estimate from the most restrictive Model (4), whereas 55.9 percent is the estimate from the least restrictive Model (2).

to earnings announcements, fueled by uninformed, or overly positive (negative) social media analysts that react to the earnings news in the direction of the market. I show in Table 4.6 of the Online Appendix that there is no significant post-earnings announcement drift in my sample, nor is there a significant reversal for undercovered firms receiving incentivized coverage.

Taken together, I document statistically and economically significant increases in price discovery around the earnings announcement for undercovered firms that receive incentivized coverage. This suggests that incentivized social media analyst coverage for undercovered firms helps incorporate earnings news more quickly and more completely into the price.

Table 4.6: Price Discovery

	Abnormal Return			
	(1)	**(2)**	**(3)**	**(4)**
Surprise Decile	0.076***	0.059***	0.348**	0.314*
	(7.72)	(15.23)	(2.09)	(1.83)
Covered		−0.019	−0.025**	−0.023**
		(−1.61)	(−2.16)	(−2.00)
Surprise Decile × *Covered*		0.033*	0.044**	0.042**
		(1.70)	(2.29)	(2.22)
Market Cap.			0.001	−0.001
			(0.12)	(−0.15)
#Employees			−0.003	−0.003
			(−0.97)	(−1.06)
BTM			0.000	0.005
			(0.03)	(0.41)
%Inst. Own.			0.017	0.015
			(0.78)	(0.67)
#Owners			0.001	0.001
			(0.44)	(0.52)
$Volatility_{-1}$			−0.006	−0.008
			(−0.17)	(−0.24)
$Price_{-1}$			0.000	0.001
			(−0.03)	(0.07)
#Estimates			0.026**	0.027**
			(2.15)	(2.27)
Bad News			0.007	0.009
			(0.34)	(0.44)
#8Ks			−0.025	−0.025
			(−1.23)	(−1.30)
#Days After ED			−0.007	−0.006
			(−1.01)	(−0.91)
#Days After CD			0.027	0.025
			(1.04)	(0.91)
N	6,157	6,157	6,157	6,157
N *Covered* = 1	370	370	370	370
Year-Quarters	8	8	8	8
Firms	1,614	1,614	1,614	1,614
Adj. R^2	0.075	0.078	0.097	0.105
Surprise Decile × Controls	No	No	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Surprise Decile × Year-Quarter FE	No	No	No	Yes
Entropy Balanced	Yes	Yes	Yes	Yes

This table presents results for the tests evaluating differences in price discovery between undercovered firms that receive incentivized coverage, and undercovered firms that do not. I present coefficients (standard errors) of the tests of the market reaction to earnings news, moderated by being covered on SA, for an increasingly stringent set of Models. Model (1) and Model (2) are the baseline models, Model (3) additionally controls for firm characteristics, and Model (4) includes interactions of the year-quarter fixed effects with the earnings suprise. I address the lower statistical power resulting from asymmetry between covered and non-covered firms in my sample by entropy balancing the sample at the third moment on size (*Market Cap,*), industry (Fama-French 48 industries) and year-month. *Abnormal Return* is the market-adjusted buy-and-hold return over the earnings announcement window. For calculating window measures, I use a three-day earnings announcement window, defined as $t \in [-1; +1]$, where $t = 0$ is the date of the earnings announcement. *Surprise Decile* is the difference between actual earnings per share in I/B/E/S and the median analyst forecast, scaled by the stock price at the prior year-quarter end. I decile-rank this variable to reduce the effects of outliers. I include a firm in the sample if it is undercovered on the date of the earnings announcement. A firm is undercovered if the articles covering that firm are eligible for the MPG incentive payment. An article covering a firm is eligible for the MPG incentive payment if that firm has been covered by fewer than two articles in the past 90 days, calculated at the beginning of every month. All variables are defined in Table 4.A1 of the Appendix. The MPG incentive was introduced in January 2018. The sample period consists of four years from January 2016 till December 2019, of which the first (last) two are pre (post) period years. All continuous variables are winsorized by year-quarter at the 1 and 99 percent levels. The regressions include year-quarter fixed effects. The standard errors are clustered at the firm level. *, ** and *** denote statistical significance at the 10%, 5% and 1% level (two-tailed), respectively.

4.7 Discussion

I find that the introduction of the minimum payment guarantee (MPG) incentive on SeekingAlpha (SA) in January 2018 significantly increased coverage of undercovered firms on SA. This result is robust for changes in underlying economics and cannot be explained by alternative (statistical) explanations. Overall, this result is in line with the expectation that output follows incentives, and with findings by Chen et al. (2019) and Clausen et al. (2021), who also document coverage effects following financial incentive changes on SA.

The results suggest that the total information level has increased after the introduction of the MPG incentive, which improved information provision on SA. I offer a credible channel for the this effect, as experienced and expert contributors cover undercover firms, also after the introduction of the incentive. This result is in contrast to several prior studies documenting a motivation crowding out effect of effective, higher quality contributors after the introduction of financial incentives to increase contributions on platforms (e.g., Khern-am-Nuai et al., 2018; Sun, Dong, & McIntyre, 2017). I find no support for such an effect, but instead report findings similar to studies documenting increases in meaningful contributions after the introduction of financial incentives, suggesting a competition crowding out effect (e.g., Tang et al., 2012). Surprisingly, while prior studies highlight that financial incentives need to be sufficiently large to promote high quality contributions (e.g., Gneezy et al., 2011; Gneezy & Rustichini, 2000), I find that, while reputation, enjoyment and reciprocity still play a role in the coverage decision on SA, a comparatively small financial incentive can already incentivize informative contributions. These results echo Liu and Feng (2021) by highlighting the importance of the incentive design and the characteristics of the platform, rather than the size of the incentive payment.

The documented significant capital market effects suggest that market participants benefit from the incentivized coverage. I document significant reductions in information asymmetry and improvements in price discovery similar in magnitude to those documented for traditional analyst coverage (Amiram et al., 2016) and media coverage (Guest, 2021). I argue that the magnitude of the documented results are plausible. First, social media analyses are found to contain similar information as traditional sell-side analyses Drake et al. (2021), therefore I expect similar capital market effects. Second and more specific, SA is a large player in providing financial information and social media analysis for retail and institutional investors. SA is one of the most popular investment-related websites in the U.S. (Seeking Alpha, 2021) with average daily visitors between 50 to 75 percent that of financial news sites such as Yahoo! Finance and MarketWatch.[100] SA is popular among retail and institutional investors: survey evidence shows that 45 percent of retail investors rely on financial blogs, specifically SA, to make investment decisions, and more than half of the ten thousand surveyed financial advisors and analysts use SA.[101] Third, even when only retail investors react to social media analysis,

[100]This is calculated using daily average visitors data from Ahrefs (`http://www.ahrefs.com`) over my sample period 2016–2019.

[101]See the U.S. Federal Reserve Survey on Consumer Finance (`https://www.federalreserve.gov/econres/scfindex.htm`) and the Erdos & Morgan FAMOUS survey (`https://www.erdosmorgan.com/`).

these investors comprise a substantial share of total market volume[102] and help price information around earnings announcements (Friedman & Zeng, 2021).

I argue that the documented capital market effects are a result of social media analysts enhancing market efficiency. While the role of social media analysis in improving capital market outcomes is more debated than that of other information intermediaries, several studies show the informativeness of social media analyst coverage. Social media analysts were initially credited with lower information creation capabilities and lower levels of sophistication (Drake et al., 2017). More recently, Drake et al. (2021) show that social media analyses preempt information contained in traditional sell-side analyses, and Farrell et al. (2022) show that social media analyses facilitate informed retail trading. In addition, my results consistently support the market efficiency hypothesis because I find stronger effects of incentivized coverage on improving capital market outcomes when traditional sell-side analyst coverage is low, and when frequency of coverage increases. Post-earnings announcement drift tests also do not support the overreaction to earnings news following incentivized coverage.

SA, as a business, is primarily self-serving and therefore also focused

[102]Retail trading volume increased to 27 percent of overall U.S. trading volume in 2021, up from 20 percent on average in the years before, almost as much as mutual funds and hedge funds combined. See https://www.ft.com/content/7a91e3ea-b9ec-4611-9a03-a8dd3b8bddb5

on goals other than capital market efficiency when introducing changes to payment structures. From the perspective of SA, any policy change is the result of an ex-ante cost-benefit analysis. Broader coverage, especially of firms that are not covered by other information intermediaries, potentially generate more traffic to SA as a result of market participants' search for information. It is possible that SA reasoned that changes in financial incentives are primarily beneficial to itself and its contributors. My findings, that the introduction of the MPG incentive is beneficial to capital market participants, could therefore be a secondary, or even unintended consequence. I show results that are in line with this reasoning in Table 4.7 of the Online Appendix. I find that web traffic (value), measured by page views (advertising value per page view), to SA as a whole significantly increased after the introduction of the MPG incentive, compared to 39 competitors. This increase in web traffic is a result from more web pages referring to SA, and SA showing up more often in the top-10 results of search engines. I have no exact data on the costs and benefits of the introduction of the MPG incentive, which is why I do not claim that the incentive change is a good business decision ex-post, but the increased web traffic at least shows an increased interest in the service offered by SA and its contributors. This nuances the relation between policy changes by self-serving businesses and the positive externalities they cause.

I acknowledge the caveat that social media analyst coverage is endogenous. I show in Table 4.1 of the Online Appendix that similar factors play a role in social media analyst coverage decisions as those affecting traditional sell-side analyst coverage decisions (Irvine, 2003; McNichols & O'Brien, 1997; Yu, 2008). A similar endogeneity concern is shared by Blankespoor et al. (2020), who note that it is difficult to entangle social media analysis coverage from market reactions and earnings news. It is possible that social media analysts do not inspire a market reaction themselves, but instead follow the market reaction in the direction of the earnings news. I am only able to compare firms that ultimately do receive coverage with firms that do not, given the change in the incentive structure. This makes it impossible to observe the counterfactual, i.e., which firms would have received coverage regardless of the incentive, which likely biases my results. I argue that social media analysts are likely to make more independent coverage decisions compared to traditional sell-side analysts, because they are not affiliated with and employed by analyst firms, investment banks or brokerage houses that play a significant role in analyst coverage and are faced with less career concerns (e.g., Derrien & Kecskes, 2013; Groysberg et al., 2011; Harford et al., 2019; He & Tian, 2013). Because I control for changes in underlying economics, and show that no concurrent change drives my result, I am able to observe a direct response to changes in the incentive structure. I acknowledge that the above-mentioned factors still play a role, but find that the role of these factors in influencing the coverage decision is at least similar before and after the introduction of the MPG incentive.

4.8 Conclusion

I investigate the ability of a user-generated content (UGC) platform to use financial incentives to increase specific output and examine whether this improves the information provision of the platform to users of the platform. I show that the introduction of a 'minimum payment guarantee' (MPG) incentive on SeekingAlpha (SA), which establishes a minimum payment for contributors who cover undercovered firms, increased coverage of undercovered firms significantly. I show that effective contributors, i.e., those with more experience and expertise, cover undercovered firms, suggesting no motivation crowding out effect by less effective contributors who are responding more strongly to financial incentives. Instead, my results suggest a competition crowding out effect, as financial incentives increase competition for limited attention of users, which favors effective contributors. Consistent with this reasoning, I document that the incentivized coverage is equally informative to other non-incentivized coverage, increasing the information level available to investors. I then investigate whether this improved information provision by the social media analyst platform improves capital market outcomes. I find that the incentivized coverage on SA is associated with reduced information asymmetries between market participants, and increased price discovery, which indicates improved capital market efficiency.

My results complement the literature on social media analysts (see Blankespoor et al., 2020, for an overview), by showing the value of social media analyst coverage in a quasi-experimental setup (Bartov et al., 2018; Chen et al., 2014; Drake et al., 2021; Farrell et al., 2022; Gomez et al., 2020; Jame et al., 2016; Kogan et al., 2021). In addition, my results advance our understanding of the role of social media analyst platforms in providing information to investors, and is the first to study the effects of providing financial incentives to social media analysts. My paper adds to the literature on UGC platforms (Khern-am-Nuai et al., 2018; Liu & Feng, 2021; Tang et al., 2012) by showing that these platforms can effectively use financial incentives to steer specific output, increasing the usefulness of the platform to its users, while highlighting that good incentive design does not necessarily equate to higher financial incentives.

Appendix

Tables

Table 4.A1: Definition of Variables

Variable	Definition	Source
Firm variables		
Market Cap.	The natural log of the market value of equity (PRCCQ × CSHOQ).	Compustat
#Employees	The natural log of one plus the total number of employees (EMP).	Compustat
ROA	Income before extraordinary items (IBQ) divided by total assets (ATQ).	Compustat
Leverage	Long-term debt (DLTTQ) divided by market value of equity (PRCCQ × CSHOQ).	Compustat
BTM	Book value of equity (CEQ), divided by market value of equity (PRCCQ × CSHOQ).	Compustat
%Inst. Own.	The percentage of institutional ownership on the most recent date available within three months prior to the earnings announcement.	Thomson Reuters
#Owners	The natural log of one plus the total number of owners (CSHR).	Compustat
Earn. Pers.	Slope coefficient of the regression of current quarter's income before extraordinary items (IBQ) scaled by lagged total assets (ATQ) on that of the prior quarter, over the last eight year-quarters.	Compustat
Earn. Pred.	The R^2 of the regression of current quarter's income before extraordinary items (IBQ) scaled by lagged total assets (ATQ) on that of the prior quarter, over the last eight year-quarters.	Compustat
Earn. Smooth.	The ratio of the standard deviation of income before extraordinary items (IBQ) scaled by lagged total assets (ATQ) to the standard deviation of cash flow from operations (CFOQ) scaled by lagged total assets (ATQ), over the last eight year-quarters.	Compustat
Earn. Var.	The variance of income before extraordinary items (IBQ) scaled by lagged total assets (ATQ) over the last eight year-quarters.	Compustat
Abn. Disc. Accruals	The discretionary accruals from the Modified Jones Model (see Dechow et al., 1995) including quarter- and SIC2-fixed effects.	Compustat
Volatility	The standard deviation of daily stock returns over the year-quarter.	CRSP
Price	The quarterly average of the daily share price	CRSP
Return	The stock market return over the year-quarter	CRSP
Share Turnover	The quarterly total trading volume divided by the quarterly average of shares outstanding.	CRSP

Dollar Volume	The natural log of one plus quarterly average of daily dollar trading volume.	CRSP
Bid-Ask Spread	The quarterly average of daily bid-ask spread.	CRSP
Amihud Illiquidity	The quarterly average of the daily Amihud (2002) illiquidity measure.	CRSP
Depth	The natural log of the quarterly average of daily depths, which is the sum of the total dollar offer size and the total dollar bid size, multiplied by 100.	TAQ
Abnormal Share Turnover	The earnings announcement window average of the daily share turnover, trading volume divided by shares outstanding, minus the estimation window average of the share turnover, divided by the standard deviation of the share turnover over the estimation window. The earnings announcement window is defined as $t \in [-2; +3]$, and the estimation window is defined as $t \in [-50; -5]$, where $t = 0$ is the date of the earnings announcement.	CRSP
Abnormal Dollar Volume	The earnings announcement window average of the daily dollar volume, minus the estimation window average of the dollar volume, divided by the standard deviation of the dollar volume over the estimation window. The earnings announcement window is defined as $t \in [-2; +3]$, and the estimation window is defined as $t \in [-50; -5]$, where $t = 0$ is the date of the earnings announcement.	CRSP
Abnormal varBid-Ask Spread	The average daily bid-ask spread in the earnings announcement window, minus the average daily bid-ask spread in the estimation window. The earnings announcement window is defined as $t \in [-2; +3]$, and the estimation window is defined as $t \in [-50; -5]$, where $t = 0$ is the date of the earnings announcement.	CRSP
Abnormal Amihud Illiquidity	The earnings announcement window average of the daily Amihud (2002) illiquidity measure, minus the estimation window average of the measure, divided by the standard deviation of the measure over the estimation window. The earnings announcement window is defined as $t \in [-2; +3]$, and the estimation window is defined as $t \in [-50; -5]$, where $t = 0$ is the date of the earnings announcement.	TAQ
$AR_{t-1,t+1}$	The market-adjusted buy-and-hold return over the earnings announcement window from $t = -1$ to $t = +1$, where $t = 0$ is the earnings announcement. I adjust the firm's return for the return on the market portfolio.	CRSP
#Estimates	The natural log of one plus the number of unique analysts that have issued an earnings per share forecast.	I/B/E/S
Surprise	The difference between actual earnings per share in I/B/E/S and the median analyst forecast, scaled by the stock price at the prior quarter end.	I/B/E/S
Forecast Error	The standard deviation of the earnings per share earnings forecasts.	I/B/E/S
#8Ks	The natural log of one plus the total number of 8K forms released by the firm in a specific year-quarter.	EDGAR

Article variables

CAAR	The sum of the absolute difference between the return of the firm that is covered by the article and the value-weighted market portfolio, summed over the event window. I use $t \in [0, 1]$ as the event window.	CRSP
AVOL	The difference between the average share turnover, trading volume divided by shares outstanding, in the event window and the average share turnover in the estimation window. I use $t \in [0, 1]$ as the event window and $t \in [-60, -1]$ as the estimation window, where $t = 0$ is the publication date of the article.	CRSP
Complexity	First principal component of three variables that measure the complexity of the article: Readability, Hard Info and Specificity	SeekingAlpha
Negative Words	Total number of words with a negative sentiment in the article text identified by the Valence Aware Dictionary and sEntiment Reasoner (VADER) scaled by the total number of words in the article text	SeekingAlpha
Upgrade	An indicator variable that is equal to one if the article subject ticker experienced an analyst upgrade on the article publication date, and zero otherwise	I/B/E/S
Downgrade	An indicator variable that is equal to one if the article subject ticker experienced an analyst downgrade on the article publication date, and zero otherwise	I/B/E/S
Positive Surprise	An indicator variable that is equal to one if the article subject ticker experienced a positive earnings *Suprise* on the article publication date, and zero otherwise	I/B/E/S
Negative Surprise	An indicator variable that is equal to one if the article subject ticker experienced a negative earnings *Surprise* on the article publication date, and zero otherwise	I/B/E/S

Contributor variables

Covered Undercovered	An indicator variable that is equal to one if a contributor covers an undercovered firm with at least one article in a specific year-quarter, and zero otherwise.	SeekingAlpha
%Covered Undercovered	The total number of contributors' articles covering undercovered firms divided by the total number of contributors' articles covering all firms in a specific year-quarter.	SeekingAlpha
Tenure	The natural log of one plus the total number of days between the contributor's first article and the start of a specific year-quarter.	SeekingAlpha
Experience	The natural log of one plus the total number of articles written since the contributor's first article and the start of a specific year-quarter.	SeekingAlpha
%Undercovered	The total number of articles covering undercovered firms divided by the total number of articles covering all firms, up until the start of a specific year-quarter.	SeekingAlpha
Expertise Industry	The Hirschmann-Herfindahl Index of the total number of articles covering all firms by each of the 48 Fama and French (1997) industries divided by the total number of articles covering all firms, up until the start of a specific year-quarter.	SeekingAlpha

Article variables

Expertise Firm	The Hirschmann-Herfindahl Index of the total number of articles covering one firm divided by the total number of articles covering all firms, up until the start of a specific year-quarter.	SeekingAlpha
Contributor Skill	The average abnormal two day buy-and-hold return to the last five articles of the contributor, up until the start of a specific year-quarter. I adjust the firm's return for the return on the market portfolio.	SeekingAlpha
#Followers	The natural log of one plus the total number of followers on SA.	SeekingAlpha
Certification	An indicator variable that is equal to one if the contributor's name or biography contains one of the following words: 'CFA', 'CPA', 'CAIA', 'FRM', 'CISI', 'Series 7', 'IMC'; and zero otherwise.	SeekingAlpha
Professional	An indicator variable that is equal to one if the contributor's name or biography contains one of the following words: 'chief investment manager', 'portfolio manager', 'hedgefund manager', 'chief market', 'analyst'; and zero otherwise.	SeekingAlpha
Company Linked	An indicator variable that is equal to one if the contributor's personal page on SA contains a link to a company, and zero otherwise.	SeekingAlpha

5 Conclusion

The purpose of this dissertation is to examine the role of financial accounting in capital markets. In particular, I investigate three (proposed) interventions aimed at improving the financial information environment in different aspects, ultimately benefiting capital market efficiency. I examine how regulators, gate-keepers and intermediaries address these market frictions, and whether these attempts are perceived as necessary, or as successful, in improving the information environment and whether they benefit capital markets.

In Chapter 2, Edith Leung and I investigate the role of transparency-enhancing regulation in the nascent crypto token market. By examining how market participants perceive regulatory proposals to increase transparency, we find that market participants react negatively to news about transparency regulation, suggesting they perceive it as costly, or burdensome, on average, but less so for crypto tokens that are more transparent and of higher quality. While our results do not imply that crypto tokens should remain largely unregulated, they do suggest that perceived costs of regulatory proposals are heterogeneous and dependent on the crypto token's information environment absent regulation.

In Chapter 3, Thomas Bourveau, Matthias Breuer, Robert Stoumbos and I examine the landscape of public company auditing around the introduction of the Securities and Exchange Commission (SEC) in 1934, by investigating historical annual reports of a broad sample of public companies trading on regulated and unregulated stock markets. Collectively, our descriptive evidence suggests that the introduction of the SEC had a limited impact on companies' reliance on audits and investors' trust in companies' reports, at least initially. Instead, we argue that *voluntary* public company auditing was flourishing.

In Chapter 4, I investigate the ability of a user-generated content (UGC) platform to use financial incentives to increase specific output and examine whether this improves the information provision of the platform to users of the platform. I show that the introduction of a 'minimum payment guarantee' (MPG) incentive on SeekingAlpha (SA), which establishes a minimum payment for contributors who cover undercovered firms, significantly increased quality coverage of undercovered firms, improving information provision for these firms on SA. Accordingly, I document significant improvements in capital market outcomes for the undercovered firms receiving incentivized coverage.

Taken together, the three studies of this dissertation sheds light on interventions, and attempts at addressing market frictions by regulators, gate-keepers and intermediaries. Market frictions in the financial information environment lead to sub-optimal levels of sufficient, credible and understandable financial information, which negatively affect capital markets. The three studies that comprise this dissertation show that there is a delicate balance between private contracting and market forces, and (proposed) interventions. Because the markets within the financial information environment are characterized by frictions, under-supply of certain goods or services arises, which results in a less than optimal or efficient equilibrium that warrants interventions. However, interventions and attempts at addressing market frictions come with unintended consequences, or are potentially not superior to private contracting or market-based solutions. It remains therefore important to study market frictions in the financial information environment, especially in a constantly changing disclosure landscape with evolving capital markets.

Samenvatting (Summary in Dutch)

Dit proefschrift stelt zich ten doel de rol van financiële verslaglegging op kapitaalmarkten te onderzoeken. In het bijzonder richt mijn onderzoek zich op drie (voorgestelde) interventies die erop gericht zijn verschillende aspecten van de financiële-informatieomgeving te verbeteren, wat uiteindelijk ten bate komt van de marktefficiëntie. Ik onderzoek hoe regelgevende instanties, poortwachters en tussenpersonen met deze wrijvingen op de markt omgaan, alsook of de daarbij gezette stappen ter verbetering van de informatieomgeving als noodzakelijk, of als succesvol, worden gezien en of kapitaalmarkten er baat bij hebben.

In hoofdstuk 2 onderzoeken Edith Leung en ik de rol van regelgeving ter stimulering van transparantie op de snelgroeiende markt voor cryptotokens. In ons onderzoek naar de manier waarop de voorgestelde regelgeving ter vergroting van de transparantie onder deelnemers aan de markt gepercipieerd wordt, stellen we vast dat deelnemers aan de markt negatief reageren op nieuws over transparantiegerelateerde regelgeving. De indruk ontstaat dat men gemiddeld genomen vindt dat dergelijke regelgeving veel kosten met zich meebrengt of eerder een last is, maar dat dat in mindere mate zo is waar het gaat om cryptotokens van hogere kwaliteit die transparanter zijn. Terwijl uit onze resultaten niet blijkt dat cryptotokens beter grotendeels ongereguleerd moeten blijven, wekken ze wel de indruk dat de gepercipieerde kostenlast die met de voorgestelde regelgeving verbonden is, heterogeen is en afhankelijk van de informatieomgeving rondom het desbetreffende cryptotoken bij afwezigheid van regelgeving.

In hoofdstuk 3 wordt door Thomas Bourveau, Matthias Breuer, Robert Stoumbos en mij het controlelandschap op het gebied van verslaglegging bij beursgenoteerde ondernemingen onder de loep genomen, aan de hand van de historische jaarverslagen van een omvangrijke steekproef van beursgenoteerde ondernemingen die op zowel gereguleerde als ongereguleerde markten actief waren omstreeks 1934, het jaar waarin de Amerikaanse Securities and Exchange Commission (SEC) is opgericht. In zijn geheel wijst het door ons verzamelde, descriptieve bewijs erop dat de oprichting van de SEC van beperkte invloed is geweest op de mate waarin, aanvankelijk tenminste, bedrijven zich op verslaglegging hebben verlaten en waarin het vertrouwen van investeerders door de jaarverslagen van bedrijven is beïnvloed. Wij beweren dat in die periode juist de opkomst en bloeitijd van de vrijwillige verslaglegging bij beurgenoteerde bedrijven heeft plaatsgevonden.

In hoofdstuk 4 onderzoek ik de mate waarin een user-generated content (UGC)-platform in staat is om met gebruikmaking van financiële stimuli specifieke bedrijfsresultaten te vergroten, alsook of daardoor de informatieverstrekking door het platform aan gebruikers ervan verbeterd wordt. Ik laat zien dat de invoering van een ‘gegarandeerd vergoedingsminimum' (Minimum Payment Guarantee, MPG) als stimulans op SeekingAlpha (SA) – wat op de vaststelling van een minimale vergoeding neerkomt voor bijdragers die controles uitvoeren voor bedrijven die te weinig worden gecontroleerd – de hoeveelheid kwalitatief hoogstaande controlediensten aanzienlijk vergroot voor bedrijven die te weinig worden gecontroleerd, met voor deze bedrijven een verbeterde informatievoorziening op SA als gevolg. Dienovereenkomstig laat ik significant verbeterde marktresultaten zien voor de bedrijven die te weinig worden gecontroleerd als die gecontroleerd worden door bedrijven die daar een stimulans voor krijgen.

Als geheel werpen deze drie onderzoeken licht op interventies en pogingen van regelgevende instanties, poortwachters en tussenpersonen om voor wrijvingen op de markt een oplossing te vinden. Wrijvingen op de markt in de financiële-informatieomgeving leiden ertoe dat er een suboptimale hoeveelheid geloofwaardige en begrijpelijke financiële informatie beschikbaar is, wat op zijn beurt een negatieve invloed heeft op kapitaalmarkten. Uit de drie onderzoeken waaruit dit proefschrift is opgebouwd blijkt dat er een delicaat evenwicht bestaat tussen private aanbestedingen en marktmechanismen enerzijds en (voorgestelde) interventies anderzijds. Omdat de markten binnen de financiële-informatieomgeving door wrijvingen worden gekenmerkt, ontstaat er een tekort aan aanbod van bepaalde goederen of diensten, met een minder dan optimaal of minder efficiënt evenwicht als gevolg, in het kader waarvan interventies gerechtvaardigd zijn. Interventies en pogingen om een oplossing te vinden voor wrijvingen op de markt hebben echter onbedoelde gevolgen of zijn mogelijk niet boven oplossingen te verkiezen die van privaat aanbestedende partijen of de markt afkomstig zijn. Het blijft daarom van belang om wrijvingen op de markt in de financiële-informatieomgeving te onderzoeken, met name in een situatie waarin het landschap op het gebied van mededelingsplicht voortdurend verandert en kapitaalmarkten zich voortdurend blijven ontwikkelen.

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Portfolio

Publications

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- **'Investor Reactions to Crypto Token Regulation'**, with Edith Leung (Erasmus University Rotterdam. *Forthcoming in European Accounting Review*, doi: `https://doi.org/10.1080/09638180.2022.2090399`

Working Papers

- **'Does Discretion in Defining Segment Earnings Affect Their Informativeness?'**, with Edith Leung (Erasmus University Rotterdam), Harm Schutt (Tilburg University), and Arnt Verriest (KU Leuven)

Work in Progress

- **'Retail Investor Responses to Earnings News'**, with Edith Leung (Erasmus University Rotterdam) and David Veenman (University of Amsterdam)

- **"Same, Same but Different': A Comparison of Financial Analysts and SeekingAlpha Analysts'**, with Tim Martens (Bocconi University) and Christoph Sextroh (Tilburg University)

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- **Columbia Business School (audited):** Valuation and Financial Statement Analysis, Research on Regulation and Accounting Institutions, Methods for Empirical Accounting Research

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Albuquerque de Sousa, J.A., *International stock markets: Essays on the determinants and consequences of financial market development*, Supervisors: Prof. M.A. van Dijk & Prof. P.A.G. van Bergeijk, EPS-2019-465-F&A

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Anantavrasilp, S., *Essays on Ownership Structures, Corporate Finance Policies and Financial Reporting Decisions*, Supervisors: Prof. A. de Jong & Prof. P.G.J. Roosenboom, EPS-2021-516-F&E

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Aydin Gökgöz, Z., *Mobile Consumers and Applications: Essays on Mobile Marketing*, Supervisors: Prof. G.H. van Bruggen & Dr B. Ataman, EPS-2021-519-MKT

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The purpose of this dissertation is to examine the role of financial accounting in capital markets. In particular, I investigate three (proposed) interventions aimed at improving the financial information environment by resolving market frictions in the demand for and supply of financial information, ultimately benefiting capital market efficiency. I examine how regulators, gatekeepers and intermediaries address these market frictions, and whether these attempts are perceived as necessary, or as successful, in improving the information environment and whether they benefit capital markets.

Chapter 2 investigates regulatory proposals in the nascent crypto token market and finds that market participants react negatively to news about transparency regulation, suggesting they perceive it as costly, or burdensome, on average, but less so for crypto tokens that are more transparent and of higher quality. These results provide initial evidence on the current debate on perceived costs and benefits in the cryptocurrency market and show the value of disclosure (regulation) in alternative, emerging financial markets. Chapter 3 examines the landscape of public company auditing around the introduction of the Securities and Exchange Commission (SEC) in 1934. The results suggest that the introduction of the SEC had a limited impact on companies' reliance on audits and investors' trust in companies' reports. Chapter 4 investigates whether the introduction of financial incentives to increase social media analyst coverage of previously undercovered firms on a user-generated content platform improves the information provision of the platform to users of the platform. Results show that the introduction of a minimum payment guarantee incentive, significantly increased the quality coverage of undercovered firms. Accordingly, the improvement in the information environment as a result of the increase in quality coverage, significantly improvements capital market outcomes for these previously undercovered firms. Taken together, this study sheds light on interventions, and attempts at addressing market frictions by regulators, gatekeepers and intermediaries.

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