The Capital Market Effects of Centralizing Regulated Financial Information*

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Abstract: We study the capital market effects of information centralization by exploiting the staggered implementation of digital storage and access platforms for regulated financial information (Officially Appointed Mechanisms, or OAMs, in the European Union). Consistent with OAMs lowering investors' processing costs associated with regulated financial information, we find that the implementation of OAMs results in significant improvements in capital market liquidity, especially for small firms with low business press coverage and firms with high levels of retail ownership. We then identify a key mechanism through which centralization facilitates capital market effects: information spillovers. First, liquidity improvements are larger when OAMs have features that easily allow investors to co-search for peer firm information. Second, liquidity improvements are larger when OAMs facilitate greater changes in the ease of acquisition and use of peer firm information. Third, firms exhibit improved liquidity and higher stock return synchronicity with peer firms during peer firm information events.

Keywords: Information centralization, disclosure, processing costs, liquidity, securities regulation

JEL Classifications: G14, G15, G18, M41, M48

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1 Introduction

Although firms disclose a large amount of value-relevant information to investors, processing costs inhibit investors' ability to become aware of, acquire, and use that information. Material processing costs can lead to negative capital market consequences, especially when investors seek to incorporate peer firm information into their trading decisions (Blankespoor et al. [2020]). *Centralization* of information—generally referring to a system that provides market participants with single-point access to various disclosures from multiple firms—is a key facet of both existing and planned regulations that target a reduction in these costs.[1]

Beyond making a firm's disclosures more accessible to its investors and potentially reducing information asymmetry regarding that firm (and improving that firm's liquidity as a result), information centralization could also facilitate *information spillovers* by making peer firms' disclosures more accessible. That is, due to the collection of *multiple* firms' disclosures in one location, centralization could lower investors' costs of becoming aware of, acquiring, and using *peer* firm information in *focal* firm trading decisions and thereby further improve focal firm liquidity. However, because disclosures and alternative information technologies are widely available in modern interconnected capital markets, the effects of centralization and related information spillovers are unclear, warranting an empirical evaluation of centralization's implications for capital markets.

In this paper, we explore a rich cross-country setting to study the following questions: What are the capital market effects of centralizing regulated financial information in the modern information age, and what are the mechanisms through which capital market effects materialize? Specifically,

[1]For example, the European Securities and Markets Authority (ESMA) plans to centralize regulatory disclosures across the European Union through a European Electronic Access Point (EEAP), and calls for this type of system have been reestablished (Financial Times [2021-11-28]).

we exploit the staggered implementation of Officially Appointed Mechanisms (OAMs) for the storage of regulated financial information in the European Union (EU).[2] Similar to the Securities and Exchange Commission's EDGAR database in the US, OAMs are country-specific repositories that store multiple firms' regulated financial disclosures in one location. OAMs (i) store information of all publicly traded firms in a searchable manner, (ii) provide public access to this information at no financial cost to the user, and (iii) provide this information in a timely fashion.

Unlike the EDGAR setting, the OAM setting has multiple favorable factors that allow for a rich assessment of the capital market effects of centralization. First, OAMs are implemented in a cross-country setting, allowing for an investigation of the heterogeneous effects of centralization driven by differences in (i) centralization features such as the ability to effectively co-search for information and (ii) the counterfactual visibility of firm disclosures absent an OAM. Second, OAMs were implemented as an EU directive throughout the 2000s; therefore, studying their implementation represents an analysis of information centralization in the modern information age, an era in which regulated financial information is widely available through other means. Third, the setting fosters an identification strategy that allows us to separate the capital market effects of information centralization from the effects of other information changes. We are able to separate material regulatory changes in *disclosure and (decentralized) dissemination*—which could have significant effects on firm liquidity, disclosure content, and real behavior—from the subsequent addition of a *centralized* database for those disclosures.[3]

[2]Technically, our setting covers the European Economic Area (EEA), of which the EU is a subset. Along with EU member countries, we also include Iceland and Norway in our sample, which are not in the EU but belong to the EEA. We include these countries because they are legally subject to EU directives regarding internal capital markets. For simplicity, we refer to all countries in our sample as "EU" countries throughout.

[3]Also, the timing of OAM implementation is plausibly exogenous to firm capital market outcomes. EU member states have to transpose EU directives into national legislation over a limited amount of time, and the timing mainly differs due to cross-country legislative process differences. For a broader discussion of directive transposition, see Hix and Hoyland [2011] and Christensen et al. [2016].

In the first part of the paper, we estimate the overall capital market effects of centralization. To do so, we implement a generalized difference-in-differences design using a sample of firms in 19 EU countries between 2001 and 2015. The effect of information centralization is thus identified from within-EU variation in the implementation dates of OAMs, after accounting for variation explained by country-industry, time, firm, and regulatory characteristics.[4] We focus primarily on capital market liquidity due to its relation to information asymmetry and processing costs; to measure liquidity, we combine two liquidity proxies (bid-ask spread and percentage of zero return days) into a liquidity factor (e.g., Daske et al. [2008], Lang et al. [2012], Christensen et al. [2016]).

Our main estimates imply that, after the implementation of an OAM, firm liquidity improves by eight percentage points relative to the percent change in liquidity outcomes of control observations, on average.[5] Within the subset of countries that implement centralization after other transparency initiatives, the short-term capital market effects of transparency regulation *absent* centralization appear to be negligible. We also find that the documented liquidity improvement is strongest for (i) small firms with low ex-ante business press coverage (i.e., firms with low information visibility) and (ii) firms with high retail ownership. These results are consistent with the benefits of centralization being strongest when investors' processing costs are high (due to poor information visibility or limited investor sophistication). Relatedly, we document increases in coverage by the business press for previously uncovered firms and increases in retail ownership after OAM implementation. Overall, these results support our hypothesis that centralization reduces the processing costs associated with regulated financial information.

[4]Among the countries in our sample, there is a subset that implements centralization apart from other directives. In our main tests, it is this variation that allows us to isolate the effect of information centralization on capital markets. Therefore, the countries outside that subset primarily serve as a benchmark group.

[5]We conduct several sensitivity tests—including a within-country test that exploits variation in exchange listing requirements in the United Kingdom—and find that this result is robust to various specification changes.

In the second part of the paper, we investigate whether information spillovers from peer firms to focal firms are a mechanism through which liquidity improvements materialize. We first assess whether co-search features on the OAM moderate liquidity improvements, and we find that the presence of an industry co-search feature produces stronger liquidity improvements after OAM implementation. We find some similar evidence for having a high number of search criteria in general. These combined findings suggest that a moderating component of centralization is the presence of centralization features that enhance investors' ability to become aware of and co-search for peer firm information. Next, we identify peer groups in which OAMs should facilitate a stronger change in the visibility of peer firm information. We find some evidence that firms in country-industries in which peer firms are more opaque absent an OAM and in which a higher proportion of peer firms are covered on the same OAM experience the largest liquidity improvements.

We then zoom in on firm information events to directly test whether centralization facilitates investors' use of *peer firm information* in trading decisions. Specifically, for each focal firm, we identify annual report releases of industry-peer firms (ensuring that the focal firm does not release an annual report simultaneously), and we assess whether the focal firm's capital market outcomes differ around those releases before and after OAM implementation. If centralization facilitates information spillovers, then one could expect (i) greater liquidity for the focal firm around the peer information event and (ii) the reflection of relevant peer-level information in focal firm returns. We find evidence consistent with both hypotheses. First, focal firm liquidity improves around peer firm annual report releases after OAM implementation. Second, stock return synchronicity between the focal firm and the peer firm increases during the peer firm information event after OAM implementation, suggesting that OAMs facilitate the reflection of peer-disclosed information (already impounded in peer firm returns) in focal firm returns. To generalize this result beyond peer firm

annual report releases, we return to the aggregate quarterly-level design and assess the synchronicity between focal firm returns and the portfolio returns of industry peers. We find some evidence that stock return synchronicity increases after OAM implementation, especially synchronicity with the smaller firms in the peer portfolio. In collection, these results are consistent with centralization facilitating both (i) the broader awareness of relevant peer information and (ii) the reflection of that information in focal firm returns.[6]

We contribute to the capital markets literature in multiple ways. First, in comparison to financial databases (e.g., Compustat (D'Souza et al. [2010]); IBES (Akbas et al. [2018]); First Call (Schaub [2018]), public credit registries (Balakrishnan and Ertan [2021]), analysts (Kelly and Ljungqvist [2012]), and newswires (Li et al. [2011], Blankespoor et al. [2018]), OAMs have a combination of centralization features that allow for a more complete assessment of the effects of centralization, as most financial databases, registries, or newswires (i) are available only for a subset of information, (ii) are only used by a subset of investors and other stakeholders, due to subscription costs, and/or (iii) do not provide intuitive storage, search, and access functions. Instead, OAMs generally provide, in a timely fashion, the public storage and public access of a wide array of searchable regulated disclosures covering multiple firms at no cost to the information user.

A stream of literature focuses on the capital market effects of EDGAR, a comparable reporting technology, introduced over the course of 1993 to 1996 in the United States (e.g., Asthana and Balsam [2001], Asthana et al. [2004], Gomez [2020]), while other studies focus on information production and managerial learning after similar reporting technology changes (e.g., Christensen

[6]Note that these results do not come without theoretical tension (Veldkamp [2006]). Specifically, the directional change in synchronicity hinges on the relative importance of (i) centralization increasing the fraction of investors informed about a fixed set of available information (increasing synchronicity) and (ii) centralization "revealing" a broader class of assets that nearly all investors were not aware of (decreasing synchronicity). We find evidence suggesting the first force is dominant in our setting.

et al. [2013], Gao and Huang [2020], Goldstein et al. [2020], McClure et al. [2021]). Yet, in providing a more specific and relevant assessment of centralization in the modern information age, the EDGAR setting is not ideal. Before the introduction of EDGAR, the public information environment regarding regulated financial information was quite sparse; its implementation represents a drastic change not only in the centralization of regulated financial information, but also in the broad availability and quality thereof (e.g., Gao and Huang [2020], Gomez [2020], Goldstein et al. [2020]). Thus, EDGAR implementation resembles a multichannel shock to the information environment, whereas OAM implementation more specifically resembles a shock to centralization because it occurs in a market environment that already deploys decentralized dissemination, newswire dissemination, and financial database availability. This allows us to better understand the added effects of centralization beyond a robust transparency regime.[7] Understanding these added effects is also practically relevant given upcoming regulatory centralization initiatives.[8]

Second, the setting naturally allows not only for an opportunity to document baseline overall capital market effects for an important economy, but also for a unique opportunity to understand the *information spillovers* of centralization, an important contribution to the literature on processing costs.[9] Specifically, because OAMs store and make accessible/searchable the near universe of regulated financial information provided by all domestic and publicly traded firms at no cost, focal firm investors can better collect domestic peer firm information and transfer relevant information to

[7] In this regard, our setting more closely relates to Christensen et al. [2017], in which, as a result of regulation, publicly-traded firms were required to disclose the *same* information that was available to investors ex-ante (mine safety reports) through a *new* mechanism (financial reports). Similarly, OAM implementation involves taking the *same* regulated financial disclosures that were publicly available to investors ex-ante and adding a layer of centralization through a *new* officially appointed database. In this way, we study the role of archiving regulated financial information—already available and disseminated otherwise—in a central location.

[8] For example, plans to create the European single access point, a cross-EU amalgamation of OAM information, have been reestablished and are likely to launch in 2024. For details, see Financial Times [2021-11-28].

[9] See Blankespoor et al. [2020] for a review of other processing costs literature.

trading decisions for focal firms. Importantly, in the cross-country setting, *heterogeneity in OAM co-search features and peer information visibility* allows for a unique opportunity to understand which characteristics moderate the effectiveness of OAMs in facilitating information spillovers. Our tests on co-search features, peer firm information visibility, and peer firm information events buttress the main takeaway of our paper: centralization provides unique capital market benefits, especially in facilitating information spillovers.

Finally, our contribution to the literature on EU securities regulation is twofold (e.g., Christensen et al. [2016], Meier [2019], Christensen et al. [2019]). First, we provide novel evidence on the effects and the importance of information centralization in EU capital markets. Second, while previous literature often assesses securities regulations as a bundle, we highlight the importance of conducting a more granular assessment of individual components of a regulation.

2 Institutional Background

As part of the EU's Financial Services Action Plan (FSAP), the European Parliament passed the Transparency Directive (TPD) in 2004. It was the last directive in a series of multiple EU directives aimed at improving and integrating EU financial markets.[10] In general, TPD implemented and harmonized disclosure and dissemination requirements and the enforcement thereof in the EU. As part of TPD, member countries were also required to establish Officially Appointed Mechanisms (OAMs) to facilitate centralized information availability.[11] Specifically, OAMs are

[10] Apart from TPD, other relevant directives are (i) the Market Abuse Directive (MAD), which focuses on insider trading and market manipulation, (ii) the Prospectus Directive (PROSP), which regulates disclosures during public securities offerings, and (iii) the Markets in Financial Instruments Directive (MiFID), which standardizes investment services across the European Union. Because these directives are often transposed prior to TPD in our sample period, we do not include them as controls in our main analyses. In a robustness exercise, we control for these other directives in Internet Appendix Table IA.1. Our results are similar.

[11] Although OAM implementation was technically required under TPD, for the sake of brevity, we will refer to "OAM" as just the centralization aspect and "TPD" as all other elements of the Transparency Directive.

country-level electronic repositories that store regulatory disclosures and financial reports for regulated firms. The disclosures on the OAM match those that firms post on their individual websites or disseminate otherwise (and firms continue to disseminate on their individual websites and through dissemination services along with archiving in the OAM). In some member countries, the implementation of TPD and the OAM was relatively simultaneous and complete; however, in a subset of member countries, the implementation of the OAM came after TPD. Ultimately, we identify six EU countries in which there was a timing difference between the implementation of TPD and the implementation of the OAM (which we call "non-bundling" countries). In these countries, we can specifically assess the effects of centralization.

An example of one of these "non-bundling" countries is Belgium. With the transposition of various EU directives such as TPD through 2008, Belgium increased its transparency requirements. These changes in part required firms to disclose all regulated information on a dedicated page on their respective firm websites (see Panel A of Figure 1 for an example). However, it was not until 2011 that Belgium implemented an OAM to centralize past and continuing disclosures in an online location as a supplement to these preexisting disclosure and decentralized dissemination requirements. According to the regulator, the reason for the delay was quite exogenous to market liquidity and quality; an initial attempt to contract a third party to manage the OAM did not materialize, leading to the regulator implementing the OAM itself. Once the OAM went live in 2011, investors could search for all regulated financial disclosures by firm and by disclosure type, as shown in Panel B of Figure 1.

[Figure 1]

The case of Belgium highlights the favorable identification factors of the EU setting in assessing the effects of information centralization. First, the timing of OAM implementation is plausibly exogenous to market outcomes. The mandate to build an OAM was part of an EU-wide directive, which results in EU member states implementing OAMs over multiple years due to legislative country differences. This staggered implementation alleviates concerns that other economic changes are conflated with OAM implementation.[12] Second, we specifically exploit the difference in timing of OAM implementation and other regulatory changes that were part of TPD, allowing us to separate (or "unbundle") centralization changes from prior material changes in disclosure and decentralized dissemination requirements. Thus, unlike EDGAR implementation in the United States, we can specifically assess the capital market effects of centralization, as any material changes in disclosure behavior or real firm behavior are likely to occur with prior identified changes in disclosure and decentralized dissemination requirements.

Beyond favorable identification factors, the OAM setting also provides important heterogeneity in centralization features and ex-ante information visibility, allowing for a rich assessment of the moderating elements of centralization's effects on capital markets. For example, some OAMs have only a few search features such as firm name, date, and document type, whereas others also allow users to co-search by more complex filters such as industry. Moreover, although all firms have a decentralized dissemination requirement as part of TPD, some firms generally had lower visibility absent an OAM. This factor was highlighted in an article from *Les Echos*, a French business publication, centered around a statement from the then-deputy secretary general of the Autorité des marchés financiers (AMF), Benoît de Juvigny:[13]

[12] For a broader discussion of directive transposition, see Hix and Hoyland [2011] and Christensen et al. [2016].
[13] Source: Les Echos [2009-01-28].

It was the 'missing link' to the transposition of the transparency directive, explains Benoît de Juvigny. ... This information, easily accessible on the sites of the main stocks of the quotation, suffers from a lack of visibility for those several companies with smaller market capitalization.

In our empirical tests, we leverage cross-country and firm-level heterogeneity in the factors above, allowing for a complete, specific, and relevant assessment of the capital market effects of centralization and the mechanisms through which capital market effects materialize.

3 Conceptual Underpinnings

Centralization refers to a system that provides market participants central access to firm disclosures from multiple firms. We posit that information centralization decreases processing costs in two ways. The first effect is direct: a firm's disclosures become more accessible and/or visible to investors with information centralization, thus decreasing investors' processing costs associated with that firm. The second effect comes from *information spillovers*: because of the storage of disclosures from *multiple* firms in one location, investors can better acquire and use *peer* firm information when making trading decisions about a focal firm. Importantly, prior evidence suggests that investors value peer firm information, especially at the industry level (e.g., Shroff et al. [2017], Roychowdhury et al. [2019], Breuer [2021]). Thus, for an investor interested in a focal firm, centralization also reduces the costs associated with processing relevant peer firm disclosures.

Consequently, we expect that information centralization—both through its direct effect and information spillovers—affects market liquidity by reducing processing costs. If these reduced processing costs are material, information asymmetry between investors and firms is likely to decrease, and information centralization will lead to more liquid capital markets (e.g., Stiglitz and

Grossman [1980], Diamond and Verrecchia [1981]). This relation is predicted in models of rational imperfect competition (Kyle [1989]) and rational inattention (Duffie [2010]).[14] If lowering processing costs is a channel through which the effects of centralization on market liquidity materialize, we expect that (i) when firms have poor information visibility absent centralization (i.e., small firms with low to no press coverage) or (ii) when firms have an investor base that is primarily composed of less-sophisticated investors (i.e., firms with high retail ownership), firms will experience greater liquidity improvements upon information centralization. In fact, due to the high processing costs that retail investors face relative to sophisticated investors, OAMs could facilitate greater retail investor participation. Additionally, liquidity improvements could be partially moderated by information intermediaries improving their coverage and services due to lower processing costs after the onset of centralization (Gao and Huang [2020], Gibbons et al. [2021]).

Although an overall liquidity improvement after the onset of centralization is intuitive, understanding the capital market effects of information spillovers specifically is more nuanced. For example, gathering peer firm information and applying that information to trading decisions could be facilitated by centralization features such as the ability to co-search for peer firm information. In addition, the change in visibility of peer firm information due to centralization could also be an important factor in defining highly affected peer groups where centralization has the most impact.

To better capture and characterize the nature of information spillovers, we also consider another capital market outcome that provides a unique theoretical tension as it relates to centralization: stock return synchronicity. Specifically, stock return synchronicity captures the explanatory power

[14]However, there are other considerations that make the liquidity effect plausibly ambiguous. Greater processing of information (due to lower costs) implies that price changes are more likely to reflect information rather than noise, which may induce adverse selection for less informed investors and result in lower liquidity (Glosten and Milgrom [1985], Avdis and Banerjee [2018], Blankespoor et al. [2020]). Furthermore, heterogeneity in investor sophistication may lead to decreases in processing costs for only a subset of investors, possibly leading to lower liquidity (Blankespoor et al. [2014]).

of peer/market returns on focal firm returns. Extant literature points out the various camps on the relation between synchronicity, transparency, and price informativeness (Chan and Chan [2014], Hameed et al. [2015], Gassen et al. [2020]). Veldkamp [2006] provides a summary of the two main ways a market for information can affect synchronicity:

1. An increase in the number of investors who are informed about a fixed subset of asset information leads to more informative prices, allowing investors to make stronger inferences about the appropriate prices of all assets based on the fixed subset of asset information. This increases synchronicity.

2. An increase in the asset information subset (i.e., a broader class of assets is observed) reduces the extent to which investors use information about one asset to make inferences about other assets. This decreases synchronicity.

Considering the two forces above, the effect of OAM implementation on synchronicity is ex-ante unclear and calls for empirical analysis. Specifically, the directional change in synchronicity hinges on the relative importance of (i) centralization increasing the fraction of investors informed about a (fixed) set of available information and (ii) centralization "revealing" a broader class of assets that nearly all investors were not aware of. In general, finding evidence for the dominance of the first force would highlight a unique feature of centralization, whereas finding evidence for the dominance of the second would likely convey that centralization resembles most other types of transparency shocks in previous studies (Gassen et al. [2020]).

4 Data

4.1 Dates of OAM Implementation

OAMs were not always simultaneously implemented with other aspects of TPD. We determined the date that each respective OAM went live through a stepwise process. We first emailed a representative from each OAM operating body and asked for the date of implementation of the OAM. If a response to this email was not available, we searched for press releases and news articles that mention the start date of the OAM. If these were not available, we then found legislation within the country that is specific to the implementation of the OAM. Christensen et al. [2019] provide dates for the entry-into-force of TPD disclosure and enforcement requirements in each country. As shown in Table 1, we confirm the OAM implementation date for 19 countries.[15]

4.2 Empirical Proxies and Sample Selection

Our main outcome variable of interest is a log liquidity factor, which is constructed by a principal component analysis of the average quarterly bid-ask spread and the percentage of zero return days at the firm-year-quarter level. We use daily capital market data from Datastream to calculate the average bid-ask spread, percentage of zero return days, and other firm-year-quarter capital market characteristics. We use Factset data to calculate institutional ownership variables at the firm-year-quarter level.[16] We use Ravenpack data (Dow Jones edition) to capture business press coverage. Variable definitions are provided in Appendix A.

[15] We are also able to confirm the OAM implementation date in the United Kingdom (UK). However, we exclude UK firms from our cross-country analyses due to a contested national election being roughly concurrent with OAM implementation, as this may have implications for market liquidity (Cox and Griffith [2019]). Because it is a major economy that exhibits "non-bundling," we do provide supplemental within-country tests for the UK, exploiting listing requirements across UK exchanges (Internet Appendix Table IA.2).

[16] Note that institutional ownership data is not available for all firm-year-quarters. We only impute zero ownership when a firm has no year-quarters with ownership data. Inferences in our processing costs tests are qualitatively similar without this imputation.

We impose some restrictions to compose our final sample. First, we require firms to have their main security listed in a country for which we have identified an OAM date. We only include the primary quotation of the largest issuance of a firm (as determined by Datastream and QA Direct) to avoid over-weighting firms with many different stock issuances. Second, we require firms to be in regulated markets as identified by Christensen et al. [2016].[17] Third, we require a firm to have multiple year-quarters of data available for all outcome and control variables in our sample period of 2001 to 2015. This results in a final sample of 129,357 firm-year-quarters. Table 1 summarizes our TPD and OAM implementation dates and sample composition by country.

[Table 1]

5 Research Design

5.1 Overall Capital Market Effects of Centralization

In our main tests, we measure the effect of OAM implementation on liquidity over the course of an entire quarter. This allows us to capture the combination of the direct effects of information centralization (that is, a decrease in the costs of processing a focal firm's disclosures) and any information spillovers (that is, the facilitation of investors' acquisition and use of peer firm information). As implementation is staggered across time, we employ a difference-in-differences approach and cluster standard errors two ways at the country-industry and year-quarter levels:

$$\begin{aligned} Y_{ijct} =& \beta_1 PostOAM_{ct} + \beta_2 PostTPD_{ct} + \beta_3 PostTPDOAMBundled_{ct} \\ &+ X_{ijct}^{T}\gamma + \lambda_{cj} + \delta_t + \epsilon_{ijct} \end{aligned} \tag{1}$$

[17] We thank the authors for details and data regarding regulated firms. At first glance, Germany seems underrepresented in our sample. This is the case because we exclude firms that operate in the less-regulated Open Market with lower transparency obligations (Freiverkehr firms, as in Christensen et al. [2016]) and firms with limited data availability/quality. Note that Germany is not a "non-bundling" country in our sample; therefore, in our main tests, German firms primarily serve as a benchmark group rather than a group directly identifying our coefficient of interest.

Above, Y_{ijct} is the natural log of one plus the liquidity factor, i denotes a firm, j denotes a firm's industry, c denotes a firm's country, and t denotes a year-quarter.[18] λ and δ denote country-industry and time fixed effects, respectively.[19] $PostOAM_{ct}$ is an indicator variable that takes a value of one in and after the quarter of OAM implementation. X_{ijct} represents a set of linear covariates to account for time-varying firm and country characteristics: lagged market value, lagged share turnover, lagged return variability, and lagged GDP per capita.

Importantly, we include $PostTPD_{ct}$, which is an indicator variable that takes a value of one in and after the quarter of TPD implementation, and $PostTPDOAMBundled_{ct}$, which is an indicator variable that takes a value of one in and after the quarter in which some countries introduce TPD and the OAM at the same time. The inclusion of these dummies facilitates the separation between "bundling" countries that introduce TPD and the OAM simultaneously and "non-bundling" countries in which a delay in the introduction of the OAM allows us to specifically assess the information centralization effect. Consequently, β_1—the coefficient of interest on $PostOAM$—represents the change in a firm's capital market liquidity subsequent to OAM implementation *only when the OAM is implemented separately from TPD*. We hypothesize that β_1 is negative, as information centralization lowers the costs associated with processing disclosures (Blankespoor et al. [2019]). Note that, with this design, the effect of OAM implementation is estimated when implemented in isolation, i.e., not in concert with changes in disclosure and decentralized dissemination

[18] Throughout the paper, industry is defined based on the Campbell industry classification (Campbell [1996]). Compared to alternate industry classifications (e.g., the Fama-French 12-industry classification), the Campbell industry classification provides both reasonable granularity and conditional support in terms of our fixed effects use throughout the paper. Nonetheless, as shown in Internet Appendix Table IA.1, our results are robust to the replacement of the Campbell industry classification with the Fama-French 12-industry classification.

[19] Due to changes in the panel composition of firms over time, firm assignment to our partitioning variables in Table 4 is time-varying, therefore making country-industry baseline fixed effects more intuitive than firm fixed effects. However, as shown in Table 3, our main result is generally robust to the inclusion of firm fixed effects, along with the results in Table 4.

(TPD). This is the case among the six "non-bundling" countries in our sample.[20]

We then test whether the overall effects of information centralization are heterogeneous in processing costs. Specifically, we expect overall liquidity effects to be largest for small firms with ex-ante low press coverage and firms with high retail investor ownership, These constructs proxy for variation in processing costs coming from firms having poor information visibility absent an OAM and from firms having a less sophisticated investor base, respectively (e.g., Drake et al. [2015], Blankespoor et al. [2018]). For the first partition, based on the median firm size by country-year, we categorize firms that are both small and have low to no pre-OAM press coverage in the lower partition. We place small firms with more coverage and large firms in the upper partition. We refer to this partition variable as firm information visibility, as firms in the lower partition (i) likely have less visible and sophisticated firm websites and (ii) are less covered by the business press (p_a).[21] For the second partition, we categorize firms based on the median of firm institutional ownership by country-year (p_o). Then, for both partitioning variables, we conduct tests of the following form:

$$\begin{aligned} Y_{ijct} =& \beta_{1H} PostOAM_{ict}^{p=H} + \beta_{1L} PostOAM_{ict}^{p=L} + \beta_{2H} PostTPD_{ict}^{p=H} + \beta_{2L} PostTPD_{ict}^{p=L} \\ &+ \beta_{3H} PostTPDOAMBundled_{ict}^{p=H} + \beta_{3L} PostTPDOAMBundled_{ict}^{p=L} \\ &+ X_{ijct}^{T}\gamma + \zeta_{cjp} + \iota_{pt} + \epsilon_{ijct} \end{aligned} \tag{2}$$

The coefficients of interest are β_{1H} and β_{1L}, which are estimates of the effect of OAM im-

[20] In untabulated analysis for our main tests, we consistently document a liquidity improvement in a "leave-one-out" exercise in which we drop all observations for each of the "non-bundling" countries.

[21] Based on the median value of the number of press articles for non-zero Ravenpack observations, we define "low to no coverage" as having fewer than 10 articles in all year-quarters prior to OAM implementation. Results of this analysis are not sensitive to this cutoff, and in Internet Appendix Table IA.3, we provide alternative specifications of firm information visibility. We also fill in zeroes for firm-year-quarters without Ravenpack data. We retain large firms with low to no coverage in the upper partition due to the lack of coverage more likely reflecting the limited scope with which Ravenpack-Dow Jones captures all business press coverage (rather than truly low coverage).

plementation on firms within partition p (firms with high processing costs due to low information visibility or high retail investor ownership, $p = H$, or firms with low processing costs, $p = L$).[22] We hypothesize that $\beta_{1H} < \beta_{1L}$ and that β_{1H} is negative because the liquidity effects from information centralization are greater when investors' processing costs are high.

5.2 Information Spillovers and Capital Market Effects

We then conduct three distinct tests to assess whether information spillovers are a mechanism through which OAM implementation affects capital markets. First, we exploit country-level variation in the depth of OAM co-search features. We implement empirical tests of the following form:

$$Y_{ijct} = \beta_{1H} PostOAM_{ct}^{r=H} + \beta_{1L} PostOAM_{ct}^{r=L} + X_{ijct}^{T}\gamma + \lambda_{cj} + \iota_t + \epsilon_{ijct} \tag{3}$$

Because this coarse country-level variation applies to both "bundling" and "non-bundling" countries in our sample, we take a simpler (bundled) specification that ensures conditional support in the estimation of the effect. We focus on three partitions (r) of OAM co-search features. First, we partition countries based on the number of search functions available on the OAM. Second, we partition countries on whether the OAM has an industry co-search function, highly related to our intuition and research design. Third, we create a three-tier measure of OAM co-search features based on a combination of the first two proxies. We hypothesize that $\beta_{1H} < \beta_{1L}$ and that β_{1H} is negative because centralization features that allow for peer firm information co-searches are likely to facilitate larger information spillovers and greater liquidity improvements.

Second, we exploit country-industry-level variation in changes in peer firm information visibility induced by OAM implementation. We first estimate the share of local peer firms with poor

[22] Importantly, along with time-invariant country-industry-partition fixed effects (ζ_{cjp}) we include a series of partition-specific time fixed effects (ι_{pt}); therefore, β_{1H} and β_{1L} are estimated *within* partition, acquiescing concerns that any results are driven by general time trends within each group.

information visibility absent an OAM (i.e., share of low-visibility peer firms), and we partition country-industries based on the median of this measure by country-year. We then estimate the share of industry peers that appear on the OAM; because OAMs are country-specific, we do so by measuring the percentage of industry peers that are in the same country, and then we split each industry at the median (i.e., share of domestic peer firms). We conduct tests of the following form, returning to a tighter research design due to the finer level of variation:

$$\begin{aligned} Y_{ijct} =& \beta_{1H} PostOAM_{jct}^{q=H} + \beta_{1L} PostOAM_{jct}^{q=L} + \beta_{2H} PostTPD_{jct}^{q=H} + \beta_{2L} PostTPD_{jct}^{q=L} \\ &+ \beta_{3H} PostTPDOAMBundled_{jct}^{q=H} + \beta_{3L} PostTPDOAMBundled_{jct}^{q=L} \\ &+ X_{ijct}^{T}\gamma + \zeta_{cjqp_ap_o} + \iota_{qp_ap_ot} + \epsilon_{ijct} \end{aligned} \tag{4}$$

The coefficients of interest are β_{1H} and β_{1L}, which are estimates of the effect of OAM implementation on firms within partition q (i.e., firms in a country-industry with high spillover opportunities due to a larger change in peer firm information visibility, $q = H$, or firms in a country-industry with low spillover opportunities, $q = L$).[23] We hypothesize that $\beta_{1H} < \beta_{1L}$ and that β_{1H} is negative; the liquidity effects from centralization should be larger for firms in country-industries with greater changes in peer firm information visibility (and a higher potential for information spillovers).

Third, we exploit variation in the timing of firm information events to test more directly whether centralization facilitates investors' increased use of *peer firm information*. Within each country-industry, we examine *focal firm* capital market outcomes in the 12 days [-1,10] around a peer firm's annual report release.[24] We run the following difference-in-differences regressions:

[23] Importantly, along with time-invariant country-industry-partition-visibility-ownership fixed effects ($\zeta_{cjqp_ap_o}$), we include a series of partition-visibility-ownership-time fixed effects ($\iota_{qp_ap_ot}$); therefore, β_{1H} and β_{1L} are estimated *within* each partition, after flexibly controlling for whether a firm has high/low ex-ante information visibility (p_a) and whether a firm has many/few institutional investors (p_o). These flexible controls help alleviate the "reflection problem" by attempting to compare firms with similar ex-ante firm information visibility (p_a) and similar institutional ownership (p_o) within each partition, but with a difference in OAM implementation timing due to staggering.

[24] We focus on annual reports for two reasons. First, these dates are most widely available for most firms in our

$$Y_{ijct} = \beta_1 PostOAM_{ct} + \beta_2 PostTPD_{ct} + \beta_3 PostTPDOAMBundled_{ct} + \lambda_{cjp_a^i p_a^h} + \iota_{jp_a^i p_a^h t} + \epsilon_{ijct} \quad (5)$$

Our fixed-effects structure "stacks" information events in the same year-quarter before and after OAM implementation, accounting for industry-level shocks and the ex-ante information visibility of both the focal firm (i) and the peer firm (h). For example, within the French manufacturing sector, we assess the change around OAM implementation in a high-visibility focal firm's capital market outcomes during each low-visibility peer firms' information event, relative to the change for same-industry and similar-visibility pairs in the control group. First, we estimate changes around OAM implementation in focal firm liquidity during peer information events.[25] Second, we estimate changes around OAM implementation in stock return synchronicity between focal firm and peer firm returns during peer information events. If centralization facilitates information spillovers, then one would expect (i) greater focal firm liquidity ($\beta_1 < 0$) and (ii) the reflection of relevant peer information in focal firm returns (i.e., greater stock return synchronicity and $\beta_1 > 0$) during peer information events. We then generalize this latter test beyond annual report windows and return to the aggregate quarterly level design. For each firm, we assess OAM-induced changes in the synchronicity between that firms' weekly returns and the weekly returns of a portfolio of its country-industry peers in each quarter.

sample. Second, they represent high-profile events that are unlikely to benefit from a simple dissemination effect. Third, given we know each firms' annual report date, we can exclude instances where both firms release reports simultaneously.

[25] Given the short window calculations, we use percent of zero return days as our liquidity measure outcome because bid-ask spread data is more sparse in these shorter intervals (and zero returns more closely relates to our synchronicity measure).

6 Results

6.1 Descriptive Statistics

Summary statistics for our main variables are in Table 2. In Panel A, we summarize key variables for the full sample, which is comprised of firms in "non-bundling" countries that implement OAMs in isolation and "bundling" countries that implement OAMs concurrently with TPD. However, as shown in Panel B, the mean differences in key characteristics between firms in the two groups are not statistically significant in the pre-period, and our difference-in-differences design acquiesces concerns that relate to these level differences.

[Table 2]

6.2 Overall Liquidity Effect of Information Centralization

We estimate the effect of OAM implementation on firm liquidity in Table 3. Prior to running our main specification, we run a difference-in-differences regression in column 1 of Table 3, where the coefficient on $PostOAM$ represents the bundled liquidity effect after OAM implementation without linear controls. The coefficient on the $PostOAM$ indicator implies that, after OAM implementation, firms in countries that implement an OAM experience a liquidity improvement relative to control observations, and in column 2, inferences remain similar with the inclusion of linear controls. However, this baseline specification does not separate the effects of disclosure and decentralized dissemination (TPD) from the effects of information centralization (OAM). In column 3 of Table 3, we introduce our main specification, as outlined in Section 5, Equation (1).

β_1—the coefficient of interest on $PostOAM$—represents the change in a firm's capital market liquidity subsequent to OAM implementation *only when the OAM is implemented separately from*

TPD. Therefore, it is this specification that estimates the effect of centralization in isolation. The coefficient on $PostOAM$ implies that, after OAM implementation, firms in countries that implement an OAM experience an increase in liquidity that is 7.7 percentage points greater than the percent change for control observations, as approximated by $exp(-0.0802) - 1$.[26] Although we include country-industry fixed effects in our main specification as discussed in Section 5, note that this specification is also robust to the inclusion of firm fixed effects in column 4 of Table 3. In Figure 2, we plot the coefficient from column 3 of Table 3 over time by splitting the OAM indicator variable by year relative to implementation. In the periods preceding information centralization, we do not see any significant difference in trends in liquidity between treated and control observations. However, in the year of OAM implementation, we see an increase in liquidity. Furthermore, this increase is persistent and present in each period after centralization.[27]

Overall, the evidence is consistent with the hypothesis that information centralization is important in facilitating capital market liquidity. Furthermore, note that the coefficients on $PostTPD$ in Table 3 are statistically insignificant. This estimate implies that implementing transparency initiatives *without* centralization does not lead to material liquidity improvements in the short run for

[26] In terms of within-design variation, the implementation of the OAM is associated with a decrease in the log liquidity factor (i.e., an improvement in liquidity) from the 50th to the 40th percentile. Note that we explore heterogeneity in the size of this effect in Tables 4, 5, and 6. Also, the sum of the first three coefficients ($PostTPD$, $PostOAM$, and the bundled dummy) of columns 3 and 4 of Table 3 give the full bundled effect for firms in countries that bundle the two regulations. In column 3 of Table 3, these sum to -0.0672, and an F-test finds the sum of these coefficients to be different from zero at a 1% significance level.

[27] We discuss various robustness tests for our main result in the Internet Appendix (Table IA.1). Note that many aspects of our main design and robustness tests alleviate concerns relating to control group assignment, linear covariate inclusion, and sample period selection in our generalized staggered difference-in-differences design [Baker et al., 2021]. In terms of control group assignment, note that our within-country tests in Table IA.2 and Internet Appendix Table IA.4 (columns 1, 3, and 5) do not depend on the cross-country staggering of OAM implementation in identifying the centralization effect. In these tests, we exploit within-country benchmark groups that are not directly affected (or are less affected) by information centralization. Furthermore, Figure 2 provides an alternate specification less subject to estimation issues involved in staggered research designs. In untabulated analysis, we consistently document a liquidity increase in a "leave-one-out" exercise in which we drop all observations for each of the "non-bundling" countries. As for linear covariate inclusion, column 1 of Table 3 omits linear covariates, and inferences are similar. We also present various adjustments to our specification in Internet Appendix Table IA.1 to address sample period selection concerns.

firms in non-bundling countries.[28]

[Table 3]

[Figure 2]

6.2.1 Overall Liquidity Effect and Processing Costs

In Table 3, we document an average improvement in firm liquidity after the implementation of the OAMs. If centralization affects liquidity through a reduction in processing costs, we expect the liquidity effect to be heterogeneous in investors' information processing costs; when processing costs are high, centralization could be relatively more important. Note that variation in processing costs can be due to the ex-ante visibility of a firm's information or the sophistication of a firm's investor base. To proxy for information visibility absent an OAM, we partition firms based on a combination of firm size and business press coverage. To proxy for the sophistication of the investor base, we partition firms based on the level of retail ownership. We expect that small firms with low ex-ante press coverage and firms with high levels of retail ownership have the highest processing costs. We describe our empirical approach in more detail in Section 5.[29]

[28] Specifically, the coefficients on OAM in columns 3 and 4 of Table 3 represent the liquidity effects of OAMs being passed in isolation in the non-bundling countries but is estimated conditional on a robust reporting and enforcement regime introduced by TPD ($PostTPD$). With the inclusion of $PostOAM$, the null coefficient on $PostTPD$ captures the liquidity effects for the six non-bundling countries in the window after TPD implementation but before OAM implementation. Therefore, the null coefficient estimate does not imply that TPD has "no effect," as ultimately, the OAM is an integral component of the Transparency Directive (for the sake of narrative expediency, we split the Transparency Directive into "TPD" and "OAM"). Instead, it implies that many of the liquidity benefits are realized when both TPD and OAM exist in concert; therefore, in increasing liquidity, the OAM is a key component of transparency regulation. Consistently, for bundling countries, we add the three coefficients together, and find an improvement in liquidity of comparable magnitude after joint TPD/OAM implementation. In sum, our claims apply to the implementation of the OAM conditional on a robust transparency regime. To address the concern that our estimates merely represent the general delayed effects of TPD implementation (i.e., the delayed effects of disclosure and decentralized dissemination enhancements), we conduct randomization inference by repeatedly and randomly assigning implementation dates across time within the countries in our sample. Results from these randomization inference tests are discussed in the Internet Appendix.

[29] In our main analyses, we partition firms into high and low groups on a rolling basis, ensuring reasonable sorting over these time-varying variables and allowing for firm entry. In the Internet Appendix, we provide an alternative sorting based only on the pre-OAM implementation means of these proxies (and, for the information visibility proxy, we

In Panel A of Table 2, we provide summary statistics for the variables underlying our partitioning variables.[30] In Panel A of Table 4, we show the correlations between our partitioning variables. We see that, although the partitions are positively correlated across proxies, they are not identical. For example, the information visibility partition and the ownership partitions have correlation coefficients below 0.4.

We run a partitioned regression that modifies the specification in column 3 of Table 3 by including both country-industry-partition and year-quarter-partition fixed effects, thus producing within-partition estimates of the liquidity effect of centralization (see Section 5, Equation (2) for details). Results are in Panel B of Table 4. For all three processing cost proxies, we find that firms in the high processing costs partition—-that is, small firms with low ex-ante coverage and firms with high levels of retail ownership—exhibit an economically stronger increase in liquidity after OAM implementation. Furthermore, these differences are statistically significant at conventional levels.[31]

To further explore the channels through which OAMs affect processing costs, we test whether centralization facilitates changes in retail ownership and changes in business press coverage in Panel C of Table 4.[32] In column 1, Panel C of Table 4, the outcome variable is the natural log

separate out partitions based on press coverage and firm size). Although magnitudes and statistical significance vary, inferences remain generally unchanged. See Internet Appendix Table IA.3.

[30] Note that, as institutional ownership data comes from Factset and are not always available quarterly or for all firms, the number of observations differs from our full sample. However, we can still calculate a partition based on these underlying continuous variables (even if sometimes missing).

[31] We can further modify these tests to include within-country fully flexible time trends. Specifically, rather than assessing the within-partition estimates of liquidity effects for high-processing costs and low-processing costs firms separately (as we do in Table 4), we use firms with low processing costs as the benchmark group. Doing so allows us to include country-year-quarter fixed effects, acquiescing concerns that results are driven by cross-country differences. Also, it follows that doing so subsumes the coefficient on low processing costs firms. Results of these within-country tests are in Internet Appendix Table IA.4.

[32] Note that, if there are changes in coverage, we believe that we would still capture effects of centralization in our tests, albeit through a specific mechanism: centralization could change the information acquisition strategies of the business press, and this coverage change could moderate the liquidity improvements we observe. Given the timely and no-cost availability, OAMs can also be directly used by investors to inform trading decisions, regardless of changes in coverage. In Internet Appendix Table IA.1, we explicitly control for changes in coverage and find similar results as our main tests.

of one plus the number of articles that appear for a firm in that year-quarter in Ravenpack data. We use the same design as in our main test in Table 3. We find that overall coverage increases, but the change is not statistically significant at conventional levels. In column 2, Panel C of Table 4, we then "unpack" this change further. We limit the sample to those firms that receive any coverage at any point during our sample period, and we partition firms on whether they receive their initial coverage before OAM implementation or after OAM implementation. We find that the increase in coverage is quite large and concentrated in those firms that did *not* have coverage prior to the OAM. This analysis suggests that OAMs facilitate the coverage of firms not covered prior to OAM implementation. Lastly, in column 3, Panel C of Table 4, we assess changes in overall retail ownership around OAM implementation. If processing costs are generally very high for retail investors, then the introduction of an OAM could facilitate retail investor participation. We find that retail ownership increases after OAM implementation, consistent with retail investors generally facing higher processing costs alleviated by OAM implementation.

[Table 4]

6.3 The Role of Information Spillovers

Liquidity improvements of centralization could originate not only from making it easier for investors to process focal firm disclosures, but also from facilitating the acquisition and use of peer firm disclosures (i.e., information spillovers). We investigate the role of information spillovers in three ways. First, we test whether firms in countries with OAMs that have better co-search features experience stronger liquidity effects (Table 5). Second, we examine whether firms in country-industries with greater changes in peer firm information visibility experience stronger liquidity effects (Table 6). Lastly, we investigate whether investors incorporate more peer firm information

into their trading decisions after OAM implementation (Table 7).

6.3.1 Heterogeneity in OAM Co-search Features

We first leverage country-level variation in OAM co-search features to test whether centralization features materially moderate liquidity effects. OAMs host a combination of centralization features such as the ability to search for stored information, and these features are likely important in producing information spillovers. Given the spillover mechanisms we have in mind—that OAMs facilitate investors' ability to find information on peer firms—we focus on three constructs. First, we consider the number of search functions in each OAM and split each country into having a high or low number of search functions, because more search functions should facilitate investors' ability to co-search information. Second, we focus on a particular co-search function that highly relates to our intuition and research design: the presence of an industry search function. Third, we create a three-tier holistic measure of co-search based on a combination of the first two proxies. Tier 1 includes firms in countries with many search criteria and an industry search feature; Tier 2 includes firms in countries with many search criteria but no industry search feature; and Tier 3 includes firms in countries with very few search criteria and no industry search feature.

In Panel A of Table 5, we list these partitioning variables. In Panel B of Table 5, we test whether liquidity effects are heterogeneous in co-search features. As this coarse country-level variation applies to both "bundling" and "non-bundling" countries in our sample, we adapt the specification from column 2 of Table 3, as described in Section 5. We then assess cross-sectional heterogeneity in liquidity effects for all OAMs. For all three proxies, we find evidence consistent with co-search features facilitating liquidity improvements after OAM implementation. This evidence is most statistically significant for industry co-search features, implying an especially important role for

this particular centralization feature in facilitating information spillovers. However, due to the limited country-level variation and cross-sectional nature of the research design, these results are primarily descriptive.

[Table 5]

6.3.2 Heterogeneity in Peer Firm Information Visibility

The goal of our next information spillovers test is to identify country-industries that are likely to experience a significant change in peer firm visibility as a result of the OAM. One source of variation is directly measuring the share of peer firms that have poor information visibility that could be alleviated by the OAM. Another source of variation is directly measuring how many industry peers appear on the OAM. As OAMs are country-specific, we measure the percentage of industry peers that are in the same country. We then partition country-industries into a group with high spillover opportunities (i.e., a high share of the two proxies) and a group with low spillover opportunities (i.e., a low share of the two proxies). Important to this design are our attempts to address endogeneity concerns by comparing firms that have similar firm information visibility absent an OAM, a similar firm ownership base, and similar *peer* firm information visibility absent an OAM, but only differ in OAM timing due to staggered implementation. We describe our empirical approach in detail in Section 5.

In column 1 of Table 6, we show that firms in country-industries with larger changes in peer firm information visibility (and thus high spillover opportunities) experience greater increases in liquidity than firms in country-industries with smaller changes in peer firm information visibility (and thus low spillover opportunities), and we show that the difference in liquidity improvements between both groups is statistically significant. In column 2 of Table 6, although we document

similar economic differences in liquidity effects when using the share of domestic firms as a proxy for spillovers, such differences are not statistically significant at conventional levels.[33]

[Table 6]

6.3.3 Peer Firm Information Events and Stock Return Synchronicity

Although the analyses in the previous two sections suggest the existence of information spillovers around OAM implementation, they do not directly test the mechanism of interest: centralization facilitates the use of peer firm information in investors' trading decisions about focal firms. Therefore, in our next analysis, we explicitly test for changes in focal firm capital market outcomes around peer firm information events. We focus on annual report releases by peer firms and implement a stacked event study within a generalized difference-in-differences design. In this analysis, we also introduce another capital market outcome that provides a unique theoretical tension as it relates to centralization: stock return synchronicity. We discuss this tension in Section 3 and our empirical design in detail in Section 5. If centralization facilitates information spillovers, then one would expect (i) greater liquidity for the focal firm around the peer information event and (ii) the reflection of relevant peer information in focal firm returns (i.e., greater stock return synchronicity).

In column 1, Panel A of Table 7, we first assess whether firm liquidity during their own annual report releases changes around OAM implementation. Our identification strategy is as follows: we

[33] In Internet Appendix Table IA.7, we restrict the estimation sample to high institutional ownership firms. The goal is to create a "credible null" and reduce the reflection problem; if a firm with many institutional investors has no peers to learn about, then liquidity effects should be negligible, as institutional investors have low processing costs regarding the focal firm they are invested in especially. We find this to be the case, and any information spillovers in high spillover opportunity country-industries are relatively modest in magnitude. Furthermore, these results are not particularly sensitive to our Campbell industry classification (untabulated); in fact, using a Fama-French 12-industry classification strengthens the statistical significance of the domestic peers split. Nonetheless, by the construction of our share of low-visibility peer firms proxy, the firms in high spillover opportunity country-industries are mechanically smaller and have lower press coverage, on average. In untabulated results for this proxy, if we further disaggregate the estimation by the information visibility partition, the direction of coefficient differences holds within visibility group as well.

assess changes in liquidity pre/post-OAM within a country, industry, and firm information visibility partition (e.g, French manufacturing firms with high information visibility absent an OAM), relative to the changes for firms with similar information visibility in the same industry, but in a control country (e.g., German manufacturing firms with high information visibility absent an OAM). This design ensures a like-to-like comparison in terms of any industry-level shocks that would affect firms with similar information visibility in the same way (and generally helps hold constant the nature of the firm's information environment). We find that, after OAM implementation, there is no significant difference in the change of a firm's liquidity during its own annual report window relative to control events.

To test for information spillovers, we examine changes in focal firm capital market outcomes around peer firm information events in column 2, Panel A of Table 7. The identification strategy is similar, except that we control for both the focal firm's and the peer firm's information visibility. We find that focal firm liquidity increases during peer firm annual report releases after OAM implementation relative to control pairs, consistent with an increase in information spillovers. However, such a liquidity result is only suggestive of information spillovers. To better test whether the peer information signal is reflected in focal firm returns, we calculate the stock return synchronicity $(ln(R^2) - ln(1 - R^2))$ between the focal firm's and peer firm's daily returns in each 12-day window. In column 3, Panel A of Table 7, we find an increase in stock return synchronicity after OAM implementation, and in column 4 of the same panel, we find that this result is robust to the inclusion of peer firm $\times$ focal firm fixed effects.

These results are consistent with the following: the introduction of the OAM centralizes the information of a fixed subset of assets—allowing for an increase in the fraction of informed investors about that fixed subset of assets—and results in increased synchronicity. In other words, a

greater fraction of focal firm investors become informed of peer information due to the information spillover opportunities afforded by OAMs.

In Table 7, Panel B, we generalize this result beyond annual report windows (as many other information events occur throughout the quarter). We return to our main quarterly design from Table 3. For each firm-quarter, we compute the synchronicity between the firms' weekly returns and the weekly returns of different industry-peer portfolios. First, we compute the return synchronicity with a portfolio of all country-industry peer firms and find a statistically insignificant increase in synchronicity after OAM implementation. Second, we compute synchronicity with a portfolio of small country-industry peer firms (likely to be the most opaque ex-ante) and find a statistically significant increase in synchronicity after OAM implementation.[34] Third, we find no increase in synchronicity with a portfolio of large country-industry peer firms after OAM implementation. Again, the evidence suggests a similar story: OAM implementation facilitates the broader awareness of relevant peer information (especially information from smaller peer firms) and facilitates the reflection of that information in focal firm returns.[35] We believe that these results add to the literature on understanding the nuanced ways in which centralization can impact information spillovers.

[Table 7]

[34]The synchronicity calculation requires minimum firm populations to calculate portfolio returns, leading to a drop in the number of observations. To facilitate conditional support, we use within-estimation-sample small and large portfolios. See Variable Definitions for details.

[35]Gassen et al. [2020] mention the downward measurement biases in synchronicity introduced by illiquidity. Although controlling for liquidity in our synchronicity tests could represent a "bad control" problem, doing so would alleviate these measurement concerns. In Internet Appendix Table IA.8, we find the results of Table 7 (both Panels A and B) hold if we flexibly control for liquidity.

7 Conclusion

Our findings highlight the importance of centralized and easily accessible regulated financial information in capital markets. Specifically, we show that the implementation of information centralization mechanisms facilitates increases in liquidity, especially when processing costs are high. Importantly, we show evidence consistent with information spillovers being a key mechanism through which these effects materialize. The evidence in this paper has important policy implications for upcoming efforts to implement cross-country centralization. First, we establish centralization's importance for firms with low visibility, for investors facing high processing costs, and in facilitating information spillovers. Second, our cross-sectional results on co-search features suggest that regulators should carefully consider how to design such a platform.

Our inferences and conclusions are subject to three caveats. First, given the institutional features of the setting, we can only speak to the effects of centralization in the presence of an already-robust transparency regime. Importantly, we *cannot* speak to the long-term effects of implementing a robust transparency regime absent centralization nor the effects of centralization absent a robust transparency regime. Second, inferences from our main tests depend on the "unbundling" of centralization from other directives in six countries. Although these countries provide an advantageous opportunity for identification, they are not necessarily representative of capital markets globally. Third, although we show increases in market liquidity and stock return synchronicity after centralization, we cannot fully speak to the overall social welfare implications of centralization. For example, although increases in stock market liquidity are generally associated with capital market improvements as they are a result of lowering information asymmetries, an increase in liquidity can also be induced by an uptick in noise traders (e.g., Black [1986], Bloomfield et al. [2009]).

Appendix A

Variable Definitions

Variable Name	Definition	Data Source
PostOAM	Indicator variable that is 1 in and after the quarter of OAM implementation. Officially Appointed Mechanisms (OAMs) are country-specific electronic repositories that store firms' regulated financial disclosures.	Regulators, press release & legislation
PostTPD	Indicator variable that is 1 in and after the quarter of TPD implementation	Christensen et al. [2019]
PostTPDOAM (Bundled)	Indicator variable that is 1 in and after the quarter when OAM and TPD are simultaneously implemented	Regulators, press release, legislation, Christensen et al. [2019]
Bid-Ask Spread	Quarterly average of daily bid-ask spread (when between 0 and 1)	Datastream
Zero Return Days	Percentage of trading days in a quarter without a change in the return index and price	Datastream
Liquidity Factor	The first principal component of the average quarterly bid-ask spread and the percentage of zero return days	Datastream & own calculation
Market value	Quarterly average of (number of shares * absolute price)	Datastream
Share turnover	Quarterly average of (trading Volume / number of shares) / 1000	Datastream
Return volatility	Quarterly standard deviation of stock returns	Datastream
GDP per capita	Annual GDP per capita (2010 USD)	World Bank
# of press articles (business press coverage)	Quarterly aggregate number of firm-relevant business press articles in Ravenpack-Dow Jones Edition that (i) are assigned by Ravenpack to the topic of "business" and (ii) are not firm-initiated press releases.	Datastream, Ravenpack
Firm (information) visibility	A partitioning indicator representing how easy it is to find and/or access firm information absent an OAM, combining information on market value and business press coverage. The lower partition (High Processing Costs) is comprised of small-firm observations (i.e., below-median market value by country-year) for firms with less than ten Ravenpack-Dow Jones Edition business articles in each year-quarter prior to OAM implementation. The upper partition (Low Processing Costs) is comprised of large-firm observations (i.e., above-median market value by country-year) and small-firm observations for firms that have ten or more business articles in at least one year-quarter prior to OAM implementation.	Datastream, Ravenpack
Coverage starts before/after	A partitioning indicator representing when intermediary coverage starts for a firm, only defined for firms that receive intermediary coverage at any point during our sample period. "Coverage starts before" indicates that a firm received its first intermediary coverage prior to OAM implementation. "Coverage starts after" indicates that a firm received its first intermediary coverage after OAM implementation.	Ravenpack
% of market value owned by funds	Percent of outstanding market value held by institutions, mutual fund portfolios, and non-institutional insider/stake holders	Factset
% of shares owned by funds	Percent of outstanding shares held by institutions, mutual fund portfolios, and non-institutional insider/stake holders.	Factset
Firm ownership (firm own.)	A partitioning indicator representing the level of institutional ownership. The upper partition (Low Processing Costs) is comprised of firm observations with high institutional ownership (i.e., above-median % shares or % market value owned by funds by country-year). The lower partition (High Processing Costs) is comprised of firm observations with low institutional ownership (i.e., below-median % shares or % market value owned by funds by country-year).	Factset
Retail ownership	(1 - % of market value owned by funds) or (1 - % of shares owned by funds)	Factset

(Continued)

Variable Name	Definition	Data Source
Search criteria (SC)	A partitioning indicator representing whether an OAM has more than five (High) or less than or equal to five (Low) search filters	Own calculation
Industry Search (IC)	A partitioning indicator representing whether an OAM has an industry search filter	Own collection
Combined co-search ranking	A three-tier holistic measure of co-search based on a combination of the first two proxies. Tier 1 includes firms in countries with many search criteria and an industry search feature; Tier 2 includes firms in countries with many search criteria but no industry search feature; and Tier 3 includes firms in countries with very few search criteria and no industry search feature.	Own calculation
Share of low-visibility peer firms	A partitioning indicator representing the share of observations in a country-industry that belong to firms having low information visibility absent an OAM (i.e., small and low to no ex-ante press coverage). The partition is formed by the median on a country-year basis, with the upper partition (High spillover opportunities) being comprised of country-industries above the median, and the lower partition (Low spillover opportunities) being below the median.	Datastream, Ravenpack, own calculation
Share of domestic peer firms	A partitioning indicator representing the share of observations in an industry that are within a country-industry (i.e., share of domestic peers). The partition is formed by the median on an industry-year basis, with the upper partition (High spillover opportunities) being comprised of country-industries above the median, and the lower partition (Low spillover opportunities) being below the median.	Datastream, own calculation
Firm zero return days, own event	The percentage of trading days in the [-1,10] event window around an own-firm annual report release in which the firm exhibits no change in the return index and price.	Datastream, own calculation
Firm zero return days, peer event	The percentage of trading days in the [-1,10] event window around a peer-firm annual report release in which the focal firm exhibits no change in the return index and price.	Datastream, own calculation
Focal-Peer Synchronicity	For each [-1,10] event window around a peer-firm annual report release, $ln(R^2) - ln(1 - R^2)$ of a regression of the focal firm's daily returns on the daily returns of the peer firm.	Datastream, own calculation
Peer-focal Firm visibility	The 2x2 interaction (i.e., 4 partitions) of the firm information visibility partitioning indicators for the peer firm and the focal firm.	Datastream, Ravenpack, own calculation
Stock return synchronicity^{industry}	For each year-quarter, $ln(R^2) - ln(1 - R^2)$ of a regression of a firm's weekly returns on the weekly returns of a portfolio of all country-industry peer firms. We restrict our estimation sample to country-industry-year-quarters with at least 4 firms having adequate returns data.	Datastream, own calcuation
Stock return synchronicity^{small}	For each year-quarter, $ln(R^2) - ln(1 - R^2)$ of a regression of a firm's weekly returns on the weekly returns of a portfolio of all country-industry peer firms that are small as per a median partition on firm market value. We determine small firms within each portfolio and rebalance portfolios annually.	Datastream, own calculation
Stock return synchronicity^{large}	For each year-quarter, $ln(R^2) - ln(1 - R^2)$ of a regression of a firm's weekly returns on the weekly returns of a portfolio of all country-industry peer firms that are large as per a median partition on firm market value. We determine large firms within each portfolio and rebalance portfolios annually.	Datastream, own calculation

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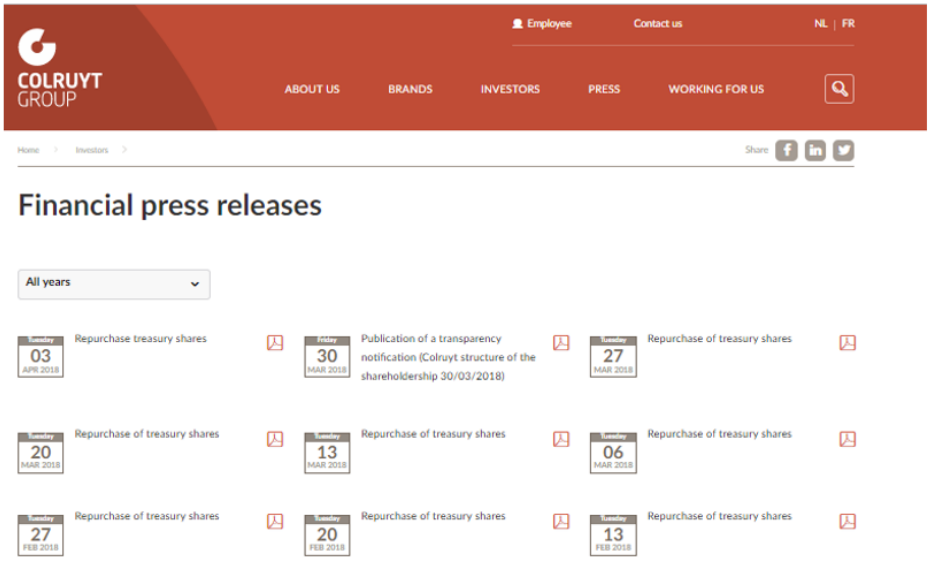
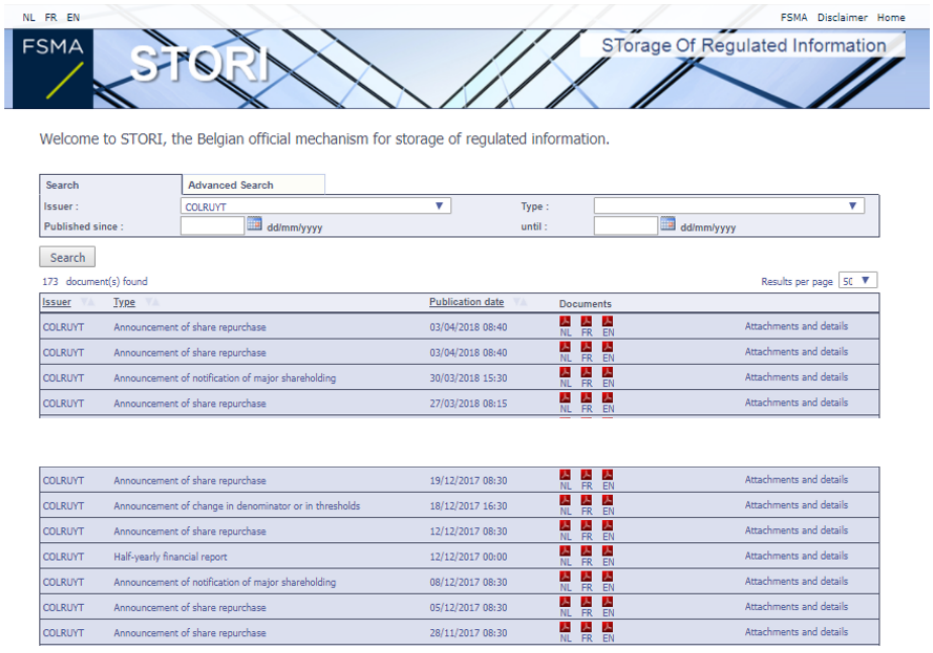
Figures and Tables

Figure 1: Regulated Financial Disclosures and OAMs

Panel A: Firm Disclosures on Website



Panel B: Firm Disclosures on Stori (Belgian OAM)



Panel A of Figure 1 shows how Colruyt, a publicly-listed Belgian firm, disseminates regulated financial information through its individual website. Before the implementation of Stori, the Belgian OAM, this was the primary way regulated financial information was disclosed by firms. Panel B shows the same disclosures by Colruyt on Stori (any information on Stori is still also available on firm websites or through other dissemination means).

Figure 2: Coefficient over Time

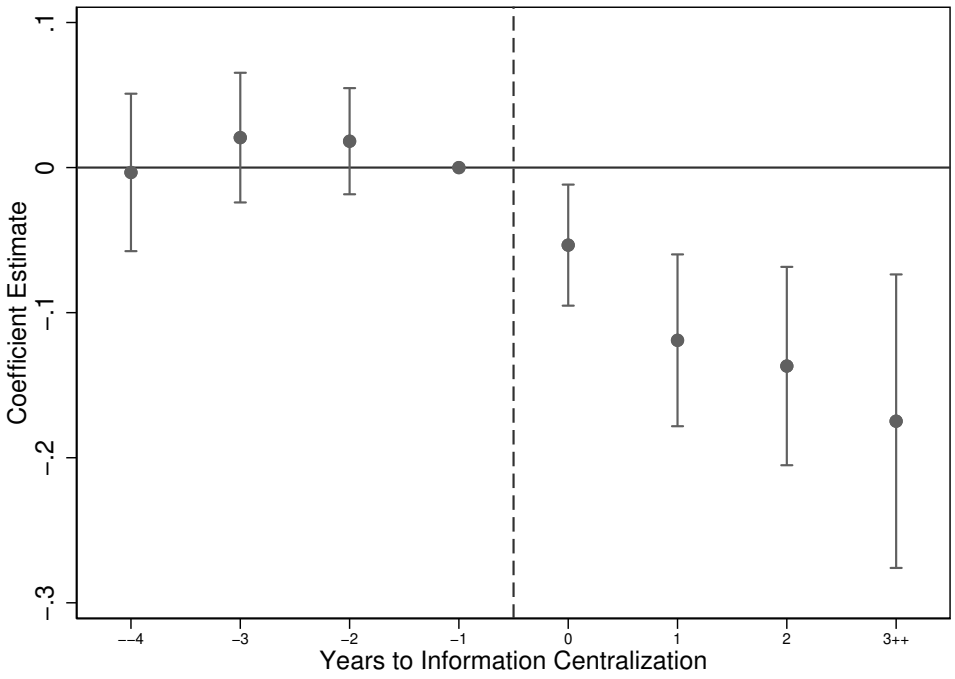


Figure 2 plots coefficient estimates over time, matching the specification from column 3 in Table 3, with the addition of a separate dummy for various time periods aligned in OAM event time for the PostOAM indicator. On the horizontal axis, "$--4$" is a dummy representing all time periods including and prior to 4 years before the implementation of information centralization. "-3, -2, -1" are dummies representing years before implementation. "0, 1, 2" are dummies representing the corresponding year of or after implementation. "$3++$" is a dummy representing all time periods including and post 3 years after implementation. "-1" serves as the relative comparison point and therefore has a coefficient estimate of zero and no confidence interval (omitted from regression). Standard error bars, representing a 95% confidence interval, are clustered two ways at the country-industry level and the year-quarter level.

Table 1: Sample Composition and Entry-into-force Dates

Country	Unique Firms	Observations	TPD Date	OAM Date	"Bundling" Country
Austria	69	2,462	2007q2	2007q2	Y
Belgium	137	5,871	2008q3	2011q1	N
Cyprus	77	2,522	2009q3	2012q4	N
Denmark	198	8,687	2007q2	2007q2	Y
Finland	131	6,407	2007q1	2007q4	N
France	705	29,026	2007q4	2009q2	N
Germany	97	3,192	2007q1	2007q1	Y
Greece	191	6,862	2007q2	2007q2	Y
Iceland	12	163	2007q4	2008q1	N
Ireland	23	701	2007q2	2007q2	Y
Italy	338	13,332	2009q2	2009q2	Y
Latvia	10	498	2007q2	2007q2	Y
Lithuania	31	1,178	2007q1	2008q1	N
Netherlands	139	5,628	2009q1	2009q1	Y
Norway	256	8,904	2008q1	2008q1	Y
Poland	336	12,048	2009q1	2009q1	Y
Portugal	52	2,225	2007q4	2007q4	Y
Spain	146	5,646	2007q3	2007q3	Y
Sweden	341	14,005	2007q3	2007q3	Y

Table 1 displays the number of unique firms (column 2) and firm-year-quarter observations (column 3) for each EU country in our sample (between 2001 and 2015). Columns 4 and 5 provide for each EU country the implementation dates of TPD (which put in place EU transparency and disclosure regulations) and the OAM (which carried out the centralization of information), respectively. Column 6 indicates for each EU country whether TPD regulation and OAM implementation were at the same time, i.e., a "bundling" country (Y), or at different times, i.e., a "non-bundling" country (N).

Table 2: Descriptive Statistics

Panel A: Full Sample

	N	Mean	SD	P1	P25	P50	P75	P99
Main variables:								
Bid-Ask Spread$_t$	129,357	0.035	0.061	0.001	0.007	0.016	0.035	0.320
Zero Return Days$_t$	129,357	0.215	0.227	0.000	0.049	0.129	0.292	0.934
Liquidity Factor$_t$	129,357	-0.007	0.736	-0.630	-0.463	-0.265	0.155	2.882
# of press articles$_t$	129,357	2.477	6.749	0.000	0.000	0.000	0.000	35.000
% Market value owned by funds$_t$	112,318	0.076	0.104	0.000	0.005	0.034	0.109	0.462
% Shares owned by funds$_t$	110,566	0.090	0.139	0.000	0.004	0.032	0.120	0.698
Stock return synchronicity$_t^{\text{industry}}$	121,238	-2.442	2.238	-9.771	-3.578	-2.035	-0.860	1.146
Stock return synchronicity$_t^{\text{small industry}}$	121,238	-2.822	2.250	-10.09	-3.969	-2.415	-1.237	0.809
Stock return synchronicity$_t^{\text{large industry}}$	121,236	-2.357	2.246	-9.747	-3.506	-1.962	-0.768	1.267
Firm zero return, own event$_{[-1,10]}$	23,879	0.189	0.213	0.000	0.000	0.091	0.250	0.917
Firm zero return, peer event$_{[-1,10]}$	334,981	0.200	0.213	0.000	0.000	0.167	0.273	0.917
Focal-peer synchronicity$_{[-1,10]}$	334,981	-3.320	2.297	-10.92	-4.490	-2.901	-1.702	0.433
Linear covariates:								
Market value$_{t-4}$	129,357	934.2	3,209	1.965	30.45	111.0	483.1	15,677
Share turnover$_{t-4}$	129,357	0.002	0.003	0.000	0.000	0.001	0.002	0.016
Return volatility$_{t-4}$	129,357	0.029	0.023	0.006	0.016	0.023	0.034	0.121
GDP per capita$_{t-4}$	129,357	41.61	17.47	9.610	33.89	40.85	48.44	90.92

Panel B: Differences between "Bundling" and "Non-bundling" Countries, 2001-2004

	Liquidity Factor	Market value	Share turnover	Return volatility	GDP per capita
Difference	0.0582	124.03	-0.0003	0.0022	-3.2438
	(0.061)	(163.876)	(0.001)	(0.003)	(2.010)

Panel A of Table 2 reports descriptive statistics for our main variables and linear covariates. Observations with a t subscript are measured at the firm-year-quarter level, and observations with a $[-1, 10]$ subscript are measured in the window around a firm's (or a peer firm's) annual report release. For linear covariates and our main outcome variable, Panel B tests for simple differences in means between firm-year-quarter observations in the "bundling" and "non-bundling" countries from 2001-2004, with standard errors (clustered at the country-industry level) in parentheses. The sample period is from 2001 to 2015. All variables are defined in Appendix A.

Table 3: Overall Liquidity Effect of Information Centralization

	Bundled effect		Unbundled effect	
Dep. variable: Ln(1+liquidity factor$_t$)	(1)	With linear controls (2)	Main specification (3)	Firm fixed effects (4)
Test variables:				
PostOAM	-0.0649***	-0.0776***	-0.0802***	-0.0807***
	(0.024)	(0.022)	(0.025)	(0.024)
PostTPD			0.0310	0.0339
			(0.029)	(0.031)
PostTPDOAM (Bundled)			-0.0180	-0.0118
			(0.027)	(0.030)
Control variables:				
Ln(Market value$_{t-4}$)		-0.1634***	-0.1634***	-0.1695***
		(0.007)	(0.007)	(0.008)
Ln(Share turnover$_{t-4}$)		-0.1286***	-0.1288***	-0.0767***
		(0.006)	(0.006)	(0.005)
Ln(Return volatility$_{t-4}$)		0.0844***	0.0855***	0.0069
		(0.020)	(0.020)	(0.012)
Ln(GDP per capita$_{t-4}$)		0.1969*	0.2011*	0.1307
		(0.101)	(0.105)	(0.103)
Fixed effects:				
Country × Industry	Y	Y	Y	N
Firm	N	N	N	Y
Year-quarter	Y	Y	Y	Y
Aggregate liquidity effect for bundling countries			-0.0672***	-0.0587**
p-value (bundling countries)			*0.0079*	*0.0144*
Adjusted R-Squared	0.230	0.621	0.621	0.753
Observations	129,357	129,357	129,357	129,357

Table 3 reports results from our analysis of the effect of information centralization (PostOAM) on liquidity (Ln(1+liquidity factor$_t$)), using OLS regressions. Our main specification in column 3 is detailed in Section 5, Equation (1). The sample period is from 2001 to 2015. "Non-bundling" countries and "bundling" countries are defined in Table 1. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. "Aggregate liquidity effect for bundling countries" is the sum of the PostOAM, PostTPD, and PostTPDOAM (Bundled) coefficients, and ***, **, and * indicate statistical significance based on Wald tests of the sum of the three coefficients at the 1%, 5%, and 10% levels (two-tailed), respectively. "p-value (bundling countries)" gives the associated p-value.

Table 4: Overall Liquidity Effect and Processing Costs

Panel A: Correlations between High/Low Processing Cost Partitions

	Processing costs partition based on	
	Firm information visibility	% market value owned by funds
% market value owned by funds	0.371	1
% shares owned by funds	0.361	0.821

Panel B: Liquidity Effects, Partitioned on Proxies of Processing Costs

	Processing costs partition based on		
	Firm information visibility	Firm ownership	
Dep. variable: Ln(1+liquidity factor$_t$)	(1)	% market value owned by funds (2)	% shares owned by funds (3)
Test variables:			
High processing costs × PostOAM	-0.1223***	-0.1155***	-0.1182***
	(0.0318)	(0.0331)	(0.0330)
Low processing costs × PostOAM	-0.0513**	-0.0237	-0.0236
	(0.0241)	(0.0164)	(0.0171)
F-test for difference (p-value)			
High = Low	*0.0153*	*0.0054*	*0.0048*
PostTPD/PostTPDOAM × High/Low	Y	Y	Y
Control variables	Y	Y	Y
Fixed effects:			
Country × Industry × High/Low	Y	Y	Y
Year-quarter × High/Low	Y	Y	Y
Adjusted R-Squared	0.642	0.632	0.632
Observations	129,356	114,068	114,068

(Continued)

Table 4: Overall Liquidity Effect and Processing Costs (Continued)

Panel C: Effects of Centralization on Business Press Coverage and Retail Ownership

	Dependent variable:			
	Business Press Coverage		Retail ownership	
	Ln(1+# of press articles)		1 - % market value owned by funds	1 - % shares owned by funds
	(1)	(2)	(3)	(4)
Test variables:				
PostOAM	0.1242		0.0145***	0.0179***
	(0.077)		(0.005)	(0.005)
Coverage starts before × PostOAM		0.1203		
		(0.1190)		
Coverage starts after × PostOAM		0.5218***		
		(0.1367)		
F-test for difference (p-value)				
High = Low		*0.0058*		
PostTPD/PostTPDOAM	Y	N	Y	Y
PostTPD/PostTPDOAM × Before/After	N	Y	N	N
Control variables	Y	Y	Y	Y
Fixed effects:				
Country × Industry	Y	N	Y	Y
Year-quarter	Y	N	Y	Y
Country × Industry × Before/After	N	Y	N	N
Year-quarter × Before/After	N	Y	N	N
Adjusted R-Squared	0.424	0.507	0.356	0.267
Observations	129,357	75,052	112,318	110,566

Panel A of Table 4 shows the correlation matrix between our firm-level partitioning proxies for processing costs. Panel B reports results from our analysis on the effect of the centralization of information (PostOAM) on liquidity (Ln(1+liquidity factor$_t$)) split across two distinct groups based on firm-level processing costs. We use the specification from column 3 in Table 3 as our base specification and partition the OLS regression as specified in Section 5, Equation (2). Processing costs are proxied by firm information visibility and firm ownership. *High/Low* is a partitioning indicator representing whether a firm has high or low processing costs. Panel C reports results from our analysis of the effect of information centralization (PostOAM) on business press coverage and retail ownership using OLS regressions. We use the specification from column 3 in Table 3 as our base specification. *Before/After* is partitioning indicator representing whether a firm received its first business press coverage before or after OAM implementation. The sample period is from 2001 to 2015. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. When relevant, we also report p-values from a Wald test assessing whether the differences between the coefficients for both groups are statistically significant.

Table 5: Heterogeneity in OAM Co-Search Features

Panel A: OAM Features

Country	High # Search Criteria	Low # Search Criteria	Industry-search Criteria	Combined Co-Search Ranking
Austria	1	0	0	2
Belgium	1	0	0	2
Cyprus	1	0	0	2
Denmark	1	0	0	2
Finland	1	0	1	1
France	1	0	0	2
Germany	1	0	0	2
Greece	0	1	0	3
Iceland	1	0	1	1
Ireland	0	1	0	3
Italy	1	0	0	2
Latvia	1	0	0	2
Lithuania	1	0	1	1
Netherlands	0	1	0	3
Norway	0	1	0	3
Poland	0	1	0	3
Portugal	0	1	0	3
Spain	0	1	0	3
Sweden	1	0	1	1

(Continued)

Table 5: Heterogeneity in OAM Co-Search Features (Continued)

Panel B: Regression Results

Ln(1+liquidity factor$_t$)	(1)	(2)	(3)
Search Criteria (SC):			
High SC × PostOAM	-0.0894***		
	(0.0227)		
Low SC × PostOAM	-0.0465		
	(0.0357)		
Industry Search (IC):			
IC × PostOAM		-0.1473***	
		(0.0309)	
No IC × PostOAM		-0.0634***	
		(0.0217)	
Combined Co-Search Ranking:			
Tier 1 × PostOAM			-0.1453***
			(0.0314)
Tier 2 × PostOAM			-0.0705***
			(0.0231)
Tier 3 × PostOAM			-0.0502
			(0.0361)
F-test for difference (p-value)			
$\beta_1 = \beta_2$	*0.2219*	*0.0005*	
Tier 1 = Tier 2			*0.0080*
Tier 2 = Tier 3			*0.5980*
Tier 1 = Tier 3			*0.0001*
Control variables	Y	Y	Y
Fixed effects:			
Country × Industry	Y	Y	Y
Year-quarter	Y	Y	Y
Adjusted R-Squared	0.621	0.621	0.621
Observations	129,357	129,357	129,357

Panel A of Table 5 shows which countries have an OAM with more than (less than or equal to) five online search criteria in column 1 (2) (see Internet Appendix for more details on the OAM search criteria by country). Column 3 of Panel A indicates whether a country's OAM has a search criteria based on industry. Lastly, column 4 categorizes the OAMs into 3 tiers based on the combination of the variables in column 1 and 3. Panel B reports results from our analysis on the effect of the centralization of information (PostOAM) on liquidity (Ln(1+liquidity factor$_t$)) split across distinct groups based on the variables in Panel A. We use an OLS regression (based on column 2 of Table 3) as specified in Section 5, Equation (3). The sample period is from 2001 to 2015. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. We also report p-values from a Wald test assessing whether the differences between the coefficients for the different groups are statistically significant. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table 6: Heterogeneity in Peer Firm Information Visibility

	Share of low-visibility peer firms	Share of domestic peer firms
Dep. variable: Ln(1+liquidity factor$_t$)	(1)	(2)
Spillover opportunities due to change in peer firm info. visibility:		
High spillover opportunities × PostOAM	-0.0926***	-0.0918***
	(0.0268)	(0.0283)
Low spillover opportunities × PostOAM	-0.0392*	-0.0270
	(0.0209)	(0.0371)
F-test for difference (p-value)		
High = Low	*0.0576*	*0.2019*
PostTPD/PostTPDOAM × High/Low	Y	Y
Control variables	Y	Y
Fixed effects:		
Country × Industry × High/Low × Firm visibility × Firm own.	Y	Y
Year-quarter × High/Low × Firm visibility × Firm own.	Y	Y
Adjusted R-Squared	0.660	0.657
Observations	114,064	114,055

Table 6 reports results from our analysis on the effect of the centralization of information (PostOAM) on liquidity (Ln(1+liquidity factor$_t$)) split across two distinct groups (high/low) based on spillover opportunities within the country-industry, as measured by the share of low-visibility peer firms (column 1) and the share of domestic peer firms (column 2) in the focal firm's peer group. We use the specification from column 3 in Table 3 as our base specification and partition the OLS regression as specified in Section 5, Equation (4). *High/Low* is a partitioning indicator representing whether a firm has above- or below-median spillover opportunities. At the firm-year-quarter observation level, the correlation coefficient between the low-visibility share partition and the domestic share partition is 0.13. *Firm visibility* is a partitioning indicator representing whether a firm is in the upper or lower partition of firm information visibility. *Firm own.* is a partitioning indicator representing whether a firm has above- or below-median institutional ownership based on the percent of market value owned by funds. The sample period is from 2001 to 2015. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. We also report p-values from a Wald test assessing whether the differences between the coefficients for both groups are statistically significant. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table 7: Peer Firm Information Events and Stock Return Synchronicity

Panel A: Liquidity and Focal-Peer Synchronicity around Disclosure Events

Dep. variable:	Firm zero return days, own event	Focal firm zero return days, peer event	Focal-Peer Synchronicity	
	(1)	(2)	(3)	(4)
Test variables:				
PostOAM	-0.0179	-0.0417***	0.1099***	0.1005**
	(0.0128)	(0.0129)	(0.0409)	(0.0490)
PostTPD/PostTPDOAM	Y	Y	Y	Y
Fixed effects:				
Country × Industry × Firm visibility	Y	N	N	N
Year-quarter × Industry × Firm visibility	Y	N	N	N
Country × Industry × Peer-focal Firm visibility	N	Y	Y	N
Year-quarter × Industry × Peer-focal Firm visibility	N	Y	Y	Y
Peer firm × Focal firm	N	N	N	Y
Adjusted R-Squared	0.283	0.255	0.018	0.028
Observations	23,879	334,981	334,981	334,981

Panel B: Quarterly Firm-level Stock Return Synchronicity

Reference Group =	General Industry Portfolio	Small Firm Industry Portfolio	Large Firm Industry Portfolio
Dep. variable: Stock Return Synchronicity	(1)	(2)	(3)
Test variables:			
PostOAM	0.1500	0.2479**	0.0203
	(0.122)	(0.117)	(0.108)
PostTPD/PostTPDOAM	Y	Y	Y
Control variables	Y	Y	Y
Fixed effects:			
Country × Industry	Y	Y	Y
Year-quarter	Y	Y	Y
Adjusted R-Squared	0.148	0.096	0.167
Observations	121,238	121,238	121,236

Panel A of Table 7 shows the results of our analysis regarding firm liquidity and stock return synchronicity changes during peer firm information events around OAM implementation. *Peer-focal Firm visibility* is the interaction between the underlying firm information visibility partitioning indicators for both the peer firm and the focal firm (to stabilize pair characteristics, we use a fixed version of firm information visibility, using only pre-OAM observations to characterize the split). *Peer firm × Focal firm* represents peer firm - focal firm pair fixed effects. Panel B shows the results of our analysis of whether quarterly stock return synchronicity between firm returns and portfolio returns of industry peers changes around OAM implementation. The sample period is from 2001 to 2015. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Internet Appendix

The Capital Market Effects of Centralizing Regulated Financial Information

Gurpal Sran, Marcel Tuijn, Lauren Vollon

Robustness Tests

We conduct various robustness tests on our main liquidity result; the relevant coefficients are in Internet Appendix Table IA.1.

Separate time trends for "non-bundling" and "bundling" countries. Note that, in Table 3, a single fully flexible time trend (year-quarter fixed effects) is included; this means that, even with the inclusion of the $PostTPDOAM$ dummy in columns 3 and 4, firms in the "bundling" countries serve as a relative group for firms in the "non-bundling" countries that identify our effect of interest. However, there may be a concern that firms in these "non-bundling" countries are very different over time than the firms in "bundling" countries in our sample, and thus, those firms in "bundling" countries are not an adequate benchmark. As a robustness test, we run two separate regressions for firms in "non-bundling" and "bundling" countries, as shown in Table IA.1 in the Internet Appendix. In another alternative specification, we interact a "bundling" indicator with the year-quarter fixed effects. These specifications allow the year-quarter fixed effects to load uniquely for each group. Inferences are similar.

Alternate sample specifications and control variables. We then alleviate concerns relating to control group assignment, linear covariate inclusion, and sample period selection in our generalized difference-in-differences design [Baker et al., 2021]. In terms of control group assignment, note that our within-country tests in Table IA.2 and Internet Appendix Table IA.4 (columns 1, 3, and 5) do not depend on the cross-country staggering of OAM implementation in identifying the centralization effect. In these tests, we exploit within-country benchmark groups that are not directly affected (or are less affected) by information centralization, and we document a similar liquidity improvement in each case. As for linear covariate inclusion, our first column of Table

3 omits linear covariates, and inferences are similar. We present various adjustments to our specification in Internet Appendix Table IA.1 to address sample period selection concerns. First, we limit our sample period to a ten-year sample of 2004 to 2013 (rather than our standard time period of 2001 to 2015, which ensures reasonable pre- and post-TPD and OAM periods for all countries). Inferences are generally unchanged. Then, we limit "non-bundling" observations to the event window 6 years prior to and 3 years subsequent to centralization; inferences are similar.

In our main specification, we define Italy as a "bundling" country. The rationale for this assignment is the presence of an OAM-like system already in place upon TPD implementation that immediately served many of the functions of the OAM.[36] However, Italy's first "official" OAM (run by private entity 1Info) went online in 2014. Therefore, in an alternate specification, we define Italy as a "non-bundling" country and rerun our main specification, yielding similar inferences.[37]

As mentioned in Section 5, we use the Campbell industry classification throughout the paper due to its reasonable granularity and conditional support for our fixed effects (and standard error clustering) usage. Nonetheless, our result is robust to changing the Campbell industry classification to the Fama-French 12 industry classification.

In addition, given that OAMs are implemented at the country level, there may be concern that our results are driven by general country-level time trends. We re-estimate our main specification and include linear country time trends; our coefficient remains similar.

In another test, we specifically control for the difference in coverage by the business press. This allows us to test (1) whether our main results are robust to variations in coverage by the press and (2) whether centralization has a direct channel to impact capital markets besides potentially

[36] See Latham and Watkins article, September 22, 2009, *Client Alert Number 935* (last accessed March 3, 2021).
[37] As an alternative and untabulated treatment, dropping all Italian firms does not change our main inferences.

changing coverage by information intermediaries. Controlling for business press coverage as a flexible linear control by year-quarter yields modest attenuations in our coefficient estimates. This implies that, although an intermediary mechanism could hold some role, it does not seem to fully explain our liquidity effects. Once again, it is important to note that we do not include coverage in our main analysis, as a change in coverage is one of the potential channels through which centralization yields capital market effects. Generally, including coverage in our analyses would thus represent a "bad controls" problem, where the research design will not capture the overall effect of interest.

Also, although TPD is the primary concurrent directive that may confound inference on the effects of centralization, other directives such as MAD, MiFID, and PROSP are also implemented primarily during the pre-TPD period of our sample. Our inferences are similar when including these other directives in our main specification.

Alternate dependent variables. In our main specification, we use the natural log of one plus a liquidity factor as the main outcome variable. To ensure that the addition of a constant does not heavily impact our results, we instead redefine our outcome variable as the inverse hyperbolic sine of the liquidity factor and produce similar coefficients.[38] Furthermore, to better understand the role of each underlying liquidity proxy, we modify the main specification to include bid-ask spread and zero return days separately as the outcome variables. Both proxies show an improvement after the implementation of OAMs.

Standard error clustering. In our main tests, we cluster standard errors two ways at the country-industry level and the year-quarter level. It is plausible that such clustering understates standard

[38]The inverse hyperbolic sine transformation ($ln(x + \sqrt{x^2 + 1})$) is well-defined at both zero and negative values. Although the transformation does introduce undesired convexity over negative values, this convexity is slight over the range of negative values for the liquidity factor in our data.

errors.[39] Although our randomization inference in Internet Appendix Figure IA.1 provides a non-parametric alternative, we conduct statistical inference with alternative clustering options as well. In all cases, we retain statistical significance at conventional levels.

[Table IA.1]

Randomization Inference Placebo Test

Although our main specification in column 3 of Table 3 ensures that we identify the effect of information centralization only when OAMs are implemented in isolation, there may be a concern that our estimates merely represent the general delayed effects of TPD implementation (i.e., the delayed effects of disclosure and decentralized dissemination enhancements). To address this concern (and as an alternative form of statistical inference), we conduct randomization inference by repeatedly and randomly assigning implementation dates across time within the countries in our sample. The proportion of simulations that produce a greater liquidity improvement (i.e., a more negative coefficient on $PostOAM$) represents a non-parametric (one-tailed) p-value. Results from these randomization inference tests are in Internet Appendix Figure IA.1. In each case, we conduct 3,500 simulations, utilizing the specification from column 3 of Table 3.

In the first test, we provide a benchmark non-parametric alternative by allowing full randomization of OAM and TPD implementation dates across our sample period. Around 2% of simulations produce a coefficient that is more negative than that which is documented in our main specification (i.e., a non-parametric p-value).

In the second test, we more carefully assess how consequential the actual OAM implementation dates are. Specifically, the passage of TPD and OAM, even in the six countries that do

[39] Although assignment is determined at the country level, with only 19 countries in the sample and with uneven cluster size, standard errors based on country clusters are unlikely to satisfy homogeneity assumptions.

so separately, are sometimes quite close in time. Furthermore, the increase in transparency provided by TPD always precedes (or is concurrent with) the information centralization provided by OAM implementation. Therefore, it may be the case that the improvements in liquidity we document around OAM implementation dates are merely a delayed effect of liquidity improvements provided by TPD implementation. If so, the exact placement of our OAM dates would not be very important in documenting the liquidity improvement; rather, under this alternative explanation, many implementation dates after TPD would garner a similarly strong negative coefficient on $PostOAM$. To assess whether the OAM implementation dates are consequential, we conduct a similar randomization as in the first test with a key change; we *fix* the TPD date at its actual date and randomize the OAM date only *on or after* this true TPD date. In this case, around 5% of the simulations produce a coefficient that is more negative than that which we document in our main specification.

We conduct a third test to further corroborate our results and to assess the uncertainty over which units within our sample are assigned to "non-bundling" or "bundling" treatment status. We randomize the OAM date after the actual TPD date for "non-bundling" countries only and keep "bundling" countries at their true bundled date. Even in this stringent randomization exercise, only around 10% of simulated coefficients are more negative than our main coefficient. Overall, these randomization inference placebo tests imply that the true information centralization dates themselves represent important liquidity events.

[Figure IA.1]

Within-country Estimation, United Kingdom

Ultimately, our cross-country research design exploits timing differences in OAM implementation and differences in "bundling" status as sources of identification. A within-country benchmark group—firms unaffected by the implementation of OAMs—would allow us to study the effect of OAM implementation after controlling for fully flexible time trends by country.

To construct such a group, we exploit differences in listing requirements in a major economy: the United Kingdom (UK). In the UK, there is a significant time gap between TPD implementation (2007) and OAM implementation (2010). Furthermore, firms that trade on the London Stock Exchange (LSE) are required to submit disclosures to the OAM, whereas firms on smaller stock exchanges such as the Aquis Stock Exchange (AQSE) are not bound by these requirements. In sum, OAM implementation in the UK was a centralization event for LSE firms but not for AQSE firms, thereby creating within-country treatment and control groups.[40]

Given that the AQSE is a relatively new exchange, we conduct our analysis in a shorter time period, between 2009 and 2013. Also, we only use the lower tercile of LSE firms by market value in our treated sample. Finally, we require that firms survive until the fourth quarter of 2013. Taking these steps helps ensure that we have a comparable sample of firms over time, as AQSE firms are often small growth firms, whereas LSE firms are generally much larger. Summary statistics regarding our UK within-country sample are provided in Panels A and B of Table IA.2. Note that, although the set of LSE firms and AQSE firms are different in terms of liquidity and market size, the short-window difference-in-differences design should mitigate these level differences. Furthermore, their distributions of share turnover and return volatility are more similar.

[40]We do not include the United Kingdom in our cross-country tests, as significant election uncertainty exists around the time of OAM implementation. However, this is less of a concern when comparing firms within the same country.

In Panel C of Table IA.2, we test whether LSE firms—the firms that experience information centralization—show improved liquidity after OAM implementation, relative to AQSE firms. Column 1 includes firm and year-quarter fixed effects. We find that OAM implementation is associated with a significant liquidity improvement for LSE firms relative to AQSE firms. In column 2, we further sharpen our comparison through a radius propensity score match based on firm size, share turnover, and return variability just prior to our sample period. In this matched sample, we similarly find a liquidity improvement upon centralization. This within-country analysis supports our primary cross-country findings that information centralization improves capital market liquidity.

[Table IA.2]

Figure IA.1: Randomization Inference Placebo Tests

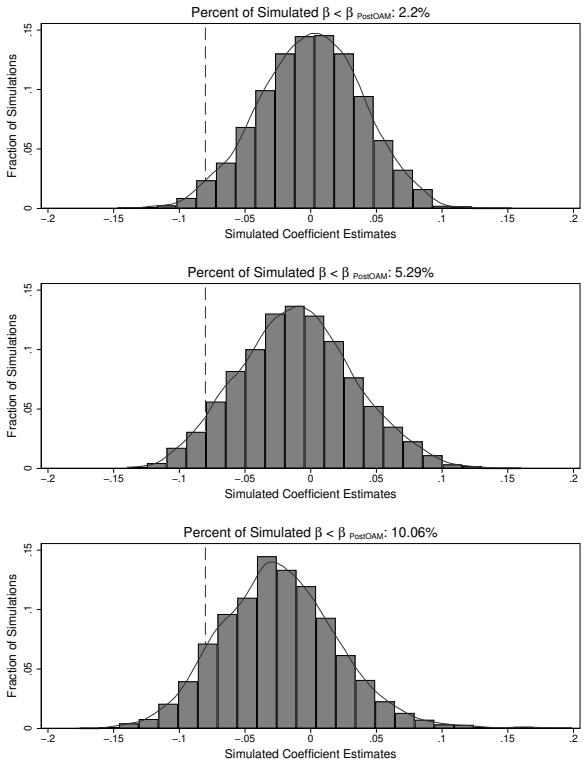


Figure IA.1 plots (both as a histogram and a kernel density) the results of randomization inference placebo tests regarding the implementation dates. In each case we conduct 3,500 simulations. In the first test, we fully randomize OAM and TPD implementation dates for all countries in our sample. In the second test, we fix the TPD date at its true date and randomize OAM dates only after the TPD date for all countries in our sample. In the third test, we only randomize the OAM dates for the "non-bundling" countries after the true TPD date (and keep the date fixed for bundling countries). Above each test, we report the percentage of simulations that produce a coefficient that is more negative than that reported in Table 3, column 3.

Table IA.1: Sensitivity Analyses of the Average Effects of Information Centralization

	N	PostOAM	PostTPD	PostOAMTPD (Bundled)
(1) Baseline model:				
- Column 3, Table 3	129,357	-0.0802***	0.0310	-0.0180
		(0.025)	(0.029)	(0.027)
(2) Separate time trends for "bundling" and "non-bundling":				
- Only non-bundling countries	45,167	-0.1282***	-0.0066	
		(0.046)	(0.036)	
- Only bundling countries	84,190			-0.0503**
				(0.025)
- Year-quarter × non-bundling indicator fixed effects	129,357	-0.1301***	-0.0001	0.0789
		(0.046)	(0.036)	(0.067)
(3) Alternate sample specifications and control variables:				
- Alternate sample period	88,497	-0.0744***	0.0428	-0.0276
		(0.027)	(0.029)	(0.026)
- Event time sample period	112,537	-0.0739***	0.0258	0.0022
		(0.021)	(0.027)	(0.024)
- Alternate OAM implementation date, Italy	129,357	-0.0901***	0.0333	-0.0397
		(0.023)	(0.028)	(0.030)
- Fama-French 12 industry classification	129,357	-0.0812***	0.0321	-0.0203
		(0.030)	(0.032)	(0.029)
- Linear time trends	129,357	-0.0879***	-0.0073	0.0306
		(0.022)	(0.024)	(0.028)
- ln(1 + # of press articles) × year-quarter controls	129,357	-0.0757***	0.0323	-0.0207
		(0.024)	(0.029)	(0.027)
- Other directives	129,357	-0.0787***	0.0232	-0.0175
		(0.025)	(0.030)	(0.027)
(4) Alternate dependent variables:				
- Inverse hyperbolic sine transformation	129,357	-0.0938***	0.0534*	-0.0373
		(0.028)	(0.031)	(0.029)
- Bid-ask spread	129,357	-0.0126***	0.0072*	-0.0022
		(0.004)	(0.004)	(0.002)
- Zero return days	129,357	-0.0315***	0.0334**	-0.0358**
		(0.011)	(0.013)	(0.014)
(5) Alternate clustering:				
- Country-industry level	129,357	-0.0802***	0.0310	-0.0180
		(0.021)	(0.024)	(0.025)
- Country-industry level and country-year level	129,357	-0.0802**	0.0310	-0.0180
		(0.036)	(0.034)	(0.034)
- Country-year level	129,357	-0.0802**	0.0310	-0.0180
		(0.033)	(0.029)	(0.025)
- Firm level	129,357	-0.0802***	0.0310***	-0.0180
		(0.011)	(0.012)	(0.012)

Table IA.1 reports results from our main analysis in Table 3 with various changes to the specification. All variables are defined in Appendix A. If not stated differently, standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table IA.2: Within-Country Liquidity Effects, United Kingdom

Panel A: LSE Summary Statistics

(N=2,241)	Mean	SD	P1	P25	Median	P75	P99
Liquidity Factor$_t$	0.199	0.539	-0.534	-0.282	0.137	0.588	1.815
Market value$_{t-4}$	53.87	63.48	1.290	16.15	39.04	71.99	280.1
Share turnover$_{t-4}$	0.002	0.002	0.000	0.001	0.001	0.002	0.014
Return volatility$_{t-4}$	0.028	0.019	0.005	0.016	0.024	0.035	0.102

Panel B: AQSE Summary Statistics

(N = 859)	Mean	SD	P1	P25	Median	P75	P99
Liquidity Factor$_t$	1.578	0.735	0.118	0.996	1.472	2.122	3.057
Market value$_{t-4}$	26.17	112.1	0.389	0.541	1.723	6.143	665.5
Share turnover$_{t-4}$	0.004	0.006	0.000	0.000	0.001	0.004	0.023
Return volatility$_{t-4}$	0.043	0.037	0.002	0.015	0.031	0.058	0.129

Panel C: Regression Results

Dep. variable: Ln(1+liquidity factor$_t$)	(1)	(2)
LSE × PostOAM	-0.0934***	-0.1291**
	(0.033)	(0.052)
Control variables	Y	Y
Fixed effects:		
Firm	Y	Y
Year-quarter	Y	Y
Propensity Score Match	N	Y
Adjusted R-Squared	0.881	0.861
Observations	3,100	2,390

Table IA.2, Panel A reports summary statistics for firms in the lowest size tercile of the London Stock Exchange (LSE), and Panel B for the Aquis Stock Exchange (AQSE). Panel C reports results from our analysis on the effect of the centralization of information (PostOAM) on liquidity (ln(1+liquidity factor)) using OLS regression in column 1 and adding propensity score matching in column 2. The sample period is from 2009 to 2013. Standard errors, reported in parentheses, are clustered two ways at the firm level and the year-quarter level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table IA.3: Sensitivity Analyses, Overall Liquidity Effect and Processing Costs

	Firm information visibility		Firm ownership	
Dep. variable: Ln(1+liquidity factor$_t$)	Business press coverage (1)	Market value (2)	% market value owned by funds (3)	% shares owned by funds (4)
Processing costs:				
High processing costs×PostOAM	-0.1340***	-0.1226***	-0.1159***	-0.1163***
	(0.0347)	(0.0346)	(0.0362)	(0.0334)
Low processing costs×PostOAM	-0.0081	-0.0509**	-0.0516**	-0.0501**
	(0.0180)	(0.0250)	(0.0212)	(0.0219)
F-test for difference (p-value)				
High = Low	*0.0010*	*0.0313*	*0.0565*	*0.0274*
PostTPD/PostTPDOAM × High/Low	Y	Y	Y	Y
Control variables	Y	Y	Y	Y
Fixed effects:				
Country-industry × High/Low	Y	Y	Y	Y
Year-quarter × High/Low	Y	Y	Y	Y
Adjusted R-Squared	0.637	0.643	0.641	0.640
Observations	124,014	124,014	119,590	119,475

Table IA.3 reports an alternate analysis of Table 4, Panel B. Specifications in columns 1 and 2 proxy for processing costs using coverage and market value, respectively. Specifications in columns 3 and 4 use % market value and % shares owned by funds, respectively. Importantly, all groups are split by a median partition within each country, but only using the mean of pre-OAM observations to characterize the split. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table IA.4: Within-Country Liquidity Effects using Variation in Processing Costs

	Firm information visibility		Firm ownership			
			% market value owned by funds		% shares owned by funds	
Dep. variable: Ln(1+liquidity factor$_t$)	(1)	(2)	(3)	(4)	(5)	(6)
Processing costs:						
High processing costs × PostOAM	-0.0771***	-0.0643**	-0.0892***	-0.0504**	-0.0771***	-0.0533**
	(0.0273)	(0.0280)	(0.0221)	(0.0220)	(0.0212)	(0.0219)
PostTPD/PostTPDOAM × High/Low	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y
Fixed effects:						
Country × Industry × High/Low	Y	Y	Y	Y	Y	Y
Year-quarter × Country	Y	Y	Y	Y	Y	Y
Year-quarter × High/Low	N	Y	N	Y	N	Y
Adjusted R-Squared	0.670	0.672	0.659	0.660	0.659	0.660
Observations	129,353	129,353	114,065	114,065	114,065	114,065

Table IA.4 reports results from our analysis on the effect of the centralization of information (OAM) on liquidity (ln(1+liquidity factor)) by processing costs (high vs. low), using OLS regressions and controlling for within-country variation. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table IA.5: OAM Co-Search Features - Further Details

Country	Search Criteria	Search Criteria (#)
Austria	submission date, validity date, organization name, ISIN reference, doc type, language, status, keywords	8
Belgium	published date, received date, firm name, firm ISIN, doc type, keywords	6
Cyprus	date, firm name, doc type, keywords, security, security type	6
Denmark	firm name, firm ISIN, national business ID, position holder name, national business ID, home country, reporting party name, business ID, announcement language, announcement ID, headline, publication date, doc type	13
Finland	date, firm name, firm size, doc type, industry, keywords, exchange	7
France	date, organization name, ISIN reference, doc type, language, keywords	6
Germany	firm name, registered office, register court, register number, type of register, legal status, federal states, language, date, select area	10
Greece	doc type	1
Iceland	date, firm name, firm size, doc type, industry, keywords, exchange	7
Ireland	date, firm name	2
Italy	dissemination Date, storage Date, firm name, doc type, keywords, market	6
Latvia	date, firm name, firm ID, keywords, document type, language	6
Lithuania	date, firm name, firm size, doc type, industry, keywords, exchange	7
Netherlands	date, doc type, keywords	3
Norway	date, firm name, keywords, market, doc type	5
Poland	date, firm name, keywords	3
Portugal	name of entity, date	2
Spain	doc type, firm name, date	3
Sweden	date, firm name, primary market place, secondary market place, GICS code, firm size	6

Table IA.5 displays for each country in our sample the search categories available to the OAM user (column 2) and the number of search criteria (column 3).

Table IA.6: Sensitivity Analyses, Heterogeneity in OAM Co-Search Features

Ln(1+liquidity factor$_t$)	(1)	(2)	(3)
Search Criteria (SC):			
High SC × PostOAM	-0.0942***		
	(0.0217)		
Low SC × PostOAM	-0.0121		
	(0.0357)		
Industry Search (IC):			
IC × PostOAM		-0.1745***	
		(0.0327)	
No IC × PostOAM		-0.0531**	
		(0.0205)	
Combined Co-Search Ranking:			
Tier 1 × PostOAM			-0.1691***
			(0.0330)
Tier 2 × PostOAM			-0.0709***
			(0.0219)
Tier 3 × PostOAM			-0.0174
			(0.0362)
F-test for difference (p-value)			
	0.0251	*0.0000*	
Tier 1 = Tier 2			*0.0016*
Tier 2 = Tier 3			*0.1726*
Tier 1 = Tier 3			*0.0001*
Control variables	Y	Y	Y
Fixed effects:			
Firm	Y	Y	Y
Year-quarter	Y	Y	Y
Adjusted R-Squared	0.754	0.754	0.755
Observations	129,357	129,357	129,357

Table IA.6 reports results from our analysis on the effect of the centralization of information (PostOAM) on liquidity (Ln(1+liquidity factor$_t$)) split across distinct groups based on the variables in Table 5, Panel A. We replicate the analysis in Table 5, Panel B and include firm instead of country-industry fixed effects. The sample period is from 2001 to 2015. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. We also report p-values from a Wald test assessing whether the differences between the coefficients for the different groups are statistically significant. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table IA.7: Heterogeneity in Peer Firm Information Visibility, High Institutional Ownership Subsample

	Share of low-visibility peer firms	Share of domestic peer firms
Dep. variable: Ln(1+liquidity factor$_t$)	(1)	(2)
Spillover opportunities due to change in peer firm info. visibility:		
High spillover opportunities × PostOAM	-0.0607***	-0.0574***
	(0.0190)	(0.0198)
Low spillover opportunities × PostOAM	-0.0067	0.0036
	(0.0163)	(0.0367)
F-test for difference (p-value)		
High = Low	*0.0108*	*0.1733*
PostTPD/PostTPDOAM × High/Low	Y	Y
Control variables	Y	Y
Fixed effects:		
Country × Industry × High/Low × Firm visibility	Y	Y
Year-quarter × High/Low × Firm visibility	Y	Y
Adjusted R-Squared	0.676	0.674
Observations	55,532	55,528

Table IA.7 reports results from our analysis on the effect of the centralization of information (PostOAM) on liquidity (Ln(1+liquidity factor$_t$)) split across two distinct groups (high/low) based on spillover opportunities within the country-industry, as measured by the share of low-visibility peer firms (column 1) and the share of domestic peer firms (column 2) in the focal firm's peer group. We limit the estimation sample to firms with high institutional ownership based on the % market values held by funds. The sample period is from 2001 to 2015. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. We also report p-values from a Wald test assessing whether the differences between the coefficients for both groups are statistically significant. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.

Table IA.8: Peer Firm Information Events and Stock Return Synchronicity, Controlling for Liquidity

Panel A: Liquidity and Focal-Peer Synchronicity around Disclosure Events

Dep. variable:	Firm zero return days, own event	Focal firm zero return days, peer event	Focal-Peer Synchronicity	
	(1)	(2)	(3)	(4)
Test variables:				
PostOAM	-0.0179	-0.0417***	0.0770**	0.0933**
	(0.0128)	(0.0129)	(0.0327)	(0.0453)
PostTPD/PostTPDOAM	Y	Y	Y	Y
Fixed effects:				
Country × Industry × Firm visibility	Y	N	N	N
Year-quarter × Industry × Firm visibility	Y	N	N	N
Country × Industry × Peer-focal Firm visibility	N	Y	Y	N
Year-quarter × Industry × Peer-focal Firm visibility	N	Y	Y	Y
Peer firm × Focal firm	N	N	N	Y
Liquidity controls	Y	Y	Y	Y
Adjusted R-Squared	0.283	0.255	0.025	0.032
Observations	23,879	334,981	334,981	334,981

Panel B: Quarterly Firm-level Stock Return Synchronicity

Reference Group =	General Industry Portfolio	Small Firm Industry Portfolio	Large Firm Industry Portfolio
Dep. variable: Stock Return Synchronicity	(1)	(2)	(3)
Test variables:			
PostOAM	0.1113	0.2220*	-0.0177
	(0.116)	(0.115)	(0.103)
PostTPD/PostTPDOAM	Y	Y	Y
Control variables	Y	Y	Y
Fixed effects:			
Country × Industry	Y	Y	Y
Year-quarter	Y	Y	Y
Liquidity controls	Y	Y	Y
Adjusted R-Squared	0.158	0.100	0.178
Observations	121,238	121,238	121,236

Panels A and B of Table IA.8 repeat the analysis in Table 7 with liquidity controls. Panel A (columns 3 and 4) controls for liquidity with indicators for the number of zero return days for the focal firm and for the peer firm separately, interacted with year-quarter. Panel B's controls categorize observations into deciles based on the liquidity factor and include indicators for each decile. The sample period is from 2001 to 2015. All variables are defined in Appendix A. Standard errors, reported in parentheses, are clustered two ways at the country-industry level and the year-quarter level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively.