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CAPITAL-MARKET EFFECTS OF SECURITIES REGULATION: PRIOR CONDITIONS, IMPLEMENTATION, AND ENFORCEMENT

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ABSTRACT

This paper examines the capital-market effects of changes in securities regulation. We analyze two key directives in the European Union (EU) that tightened market abuse and transparency regulation. All EU member states were required to adopt these directives, but for plausibly exogenous reasons did so at different times. We exploit this staggered introduction to estimate causal effects of tighter securities regulation for the population of European firms, and find significant increases in market liquidity. Examining cross-sectional variation, we find larger treatment effects in countries that implement and enforce the directives more strictly. They are also stronger in countries with traditionally stricter securities regulation and a better track record of implementing regulation. The cross-sectional results indicate that the same forces that limited the effectiveness of regulation in the past are at play when new rules are introduced, leading to hysteresis in regulatory outcomes. The findings suggest that harmonizing regulation in countries with different prior conditions can make countries diverge more rather than less.

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1. Introduction

Securities regulation is widespread around the world. However, as with regulation in general, the academic debate on the costs and benefits of securities regulation is controversial. Whether or not securities regulation is beneficial to the economy is largely an empirical matter. The evidence, so far, is fairly mixed and we have only few studies that provide causal estimates for the effects of securities regulation.[1] These latter studies focus on specific market segments (e.g., smaller over-the-counter firms) or samples around a narrow size threshold to obtain causal estimates (e.g., Greenstone et al., 2006; Iliev, 2009). Thus, there exists little evidence on the causal effects of securities regulation for the population of publicly listed firms.

Regulatory effects likely not only depend on the written rules, but also on how regulation is implemented and enforced as well as on the state of prior regulation (e.g., Djankov et al. 2003a). Extant studies typically focus on regulatory changes in a single country (e.g., the Sarbanes-Oxley Act; henceforth SOX). In these settings, it is difficult to separate the role of prior regulation, new rules, implementation, and enforcement, as these factors are essentially a bundle, and observable regulatory outcomes reflect the entire bundle.

In this paper, we exploit major regulatory changes in the EU to provide causal estimates for the capital-market effects of securities regulation in a large population of EU firms as well as evidence on how these effects differ by prior regulatory conditions, implementation and enforcement. The EU setting offers several unique advantages. First, changes in securities regulation are enacted in the form of EU-level directives. Member states need to implement directives within a predetermined and relatively short time period, resulting in country-specific entry-into-force dates that are spread over two to three years. The variation in effective dates is

[1] For the debate and surveys of the evidence see, e.g., Coffee (1984), Easterbrook and Fischel (1984), Shleifer (2005), Mulherin (2007), Zingales (2009), Coates and Srinivasan (2014), and Leuz and Wysocki (2015).

plausibly exogenous within the narrow implementation window, considering countries' inflexible legislative processes. Second, as EU directives apply to all member states, the regulatory act itself is held constant across countries, but the transposition of a directive into national law, the design of supervision, the penalties for violations, and the actual supervision are left to the EU member states. This variation across countries allows us to analyze the role of implementation and enforcement separately from the rule change. Third, the EU setting comprising 26 countries allows us to analyze the interaction between a country's prior regulatory conditions and newly-introduced regulation. This interaction has not been analyzed in prior literature.

We study the capital-market effects of two EU directives that contain central elements of securities regulation: the Market Abuse Directive (MAD) concerns insider trading and market manipulation, and the Transparency Directive (TPD) addresses corporate reporting and disclosure. Both types of regulation are intended to reduce adverse selection in capital markets arising from information asymmetries. We therefore focus our analysis on stock market liquidity. An extensive body of theory links market liquidity, measured for instance by bid-ask spreads or price impact of trades, to the magnitude of information asymmetry and adverse selection in markets (e.g., Copeland and Galai, 1983; Glosten and Milgrom, 1985; Kyle, 1985; Easley and O'Hara, 1987; Admati and Pfleiderer, 1988). Furthermore, a primary purpose of securities markets is to provide liquidity to market participants, and hence securities regulation is frequently justified with expected improvements in market liquidity (see, e.g., the EU's Financial Services Action Plan in 1999). Liquidity also has several desirable features from a research design perspective: it can be measured over relatively short intervals and is less anticipatory in nature than other economic constructs (e.g., firm value or cost of capital). We need both features to exploit the staggered implementation of the directives. At the same time, improvements in liquidity, due to a reduction

in adverse selection, should ultimately manifest in a lower cost of capital, higher market valuations, and improved market efficiency (e.g., Amihud et al., 2005).

We measure market liquidity by combining four individual proxies for information asymmetry and illiquidity (i.e., bid-ask spreads, price impact, zero returns, and total trading costs), following Daske et al. (2008) and Lang et al (2012). We estimate quarterly panel regressions with year-quarter fixed effects for a large sample of EU firms over a 10-year window around the two directives. In these regressions, the identification of the regulatory effects comes solely from within-EU variation in the effective dates of the two directives. To further tighten identification, we also estimate regressions adding *country-specific* linear and quadratic time trends to the model. In addition, we estimate the regulatory effects of the directives *within-country*, exploiting that some EU countries have large unregulated markets (Germany, Ireland, and the U.K.), which are not or to a lesser degree affected by the new directives. In these regressions, the identification comes from differential liquidity effects across propensity-matched firms trading on regulated and unregulated markets in a particular country following the country-specific entry-into-force date.

We find that market liquidity increases when new market abuse (MAD) and transparency (TPD) regulations come into force. The estimated magnitudes for MAD and TPD are similar and suggest an increase in liquidity around 10 percent relative to pre-directive liquidity levels. We can translate this improvement into (volume-weighted) average trading cost savings between US$ 130 and 430 thousand per year, directive, and sample firm, which implies a combined *yearly* benefit of both directives between 0.1 and 0.2% of market capitalization.[2] This magnitude is economically significant, especially when considering the recurring nature of the savings and the number of sample firms. At the same time, the estimated per-stock magnitude is not so large as to be

[2] This translation provides a lower bound for the economic effects of two directives because it measures only the transaction cost savings. As discussed above, liquidity improvements indicate less adverse selection and, among other things, map into firms' cost of capital (e.g., Amihud and Mendelson, 1986).

implausible. In sum, our results suggest that improving key elements of securities regulation leads to substantial economic benefits in capital markets.[3]

The staggered implementation of the two directives in a large number of EU countries mitigates many concerns that typically arise in studies of a single regulatory event. For instance, it is unlikely that unrelated shocks to liquidity line up in time with the implementation pattern across countries. Thus, in our study, the primary concern about identification pertains to the variation in the implementation timing. We argue that there are good reasons to believe that in our setting this timing is plausibly exogenous. First, the political decision to enact the regulation was made years earlier, at the EU level, mitigating the concern that our analysis picks up a market response to events giving rise to the new directives in the first place. Second, the transposition window is relatively narrow and predetermined by the EU, yet the national legislative processes implementing the directives are fairly lengthy and inflexible. This combination makes it unlikely that the effective dates reflect a response to local shocks or liquidity changes. For the latter to explain our findings, a large set of countries would have to experience a series of differentially-timed local shocks that each prompt national lawmakers to start the country's implementation process for the directives *and* then choose entry-into-force dates that coincide with subsequent liquidity improvements (or reversals). It is even more unlikely that lawmakers could explicitly time the entry-into-force dates such that they coincide with (future) increases in liquidity. That said, we provide several tests that gauge the validity of these arguments and our identification.

First, we analyze the liquidity patterns in event time. We find that market liquidity is not significantly elevated in the year leading up to the implementation, improves around the time the

[3] We conduct extensive sensitivity analyses and show that our results, among other things, are robust to the introduction of firm-fixed effects, separate quarter-year fixed effects for more and less developed EU countries, controls for differences in the composition of the country samples (e.g., with respect to size or industry), and the inclusion of non-EU benchmark countries. See Appendix B for details.

directives come into force, and stays at a higher level for the remainder of the sample period. Thus, liquidity responds relatively "sharply" around the entry-into-force dates suggesting that the directives are indeed responsible for the effects. Second, to address the concern that differential local shocks prompt the directives' implementation, we control for pre-implementation changes (or shocks) in market liquidity. Note further that our within-country analysis for three EU countries with large unregulated markets controls for local trends or liquidity changes that are common to all publicly traded firms in that particular country. Third, we analyze countries' implementation timing and show that it is *not* significantly correlated with pre-implementation liquidity shocks or institutional characteristics that explain cross-sectional variation in the treatment effects. If the dates were indeed endogenous, we would expect these correlations to be significant. Instead, countries' relative timing is fairly persistent across EU directives and can be predicted by the relative timing of other directives (e.g., the timing of MAD explains the timing of TPD).[4] Fourth, we conduct a falsification test in the spirit of Altonji et al. (2005). We show that observable, *time-variant* proxies for local market conditions, political, and economic forces, which, in principle, could influence lawmakers and legislative processes, do not produce liquidity effects that come close to the estimated treatment effects of the two directives.

In sum, we conclude that the average liquidity effects of the two directives are well identified. Next, we examine whether there is heterogeneity in the treatment effects. We consider two alternative explanations. One hypothesis posits that countries with weaker prior regulation benefit more from the new directives because they essentially catch up with the other countries. But it is also possible that the same forces and constraints that limited the scope and effectiveness of

[4] Other variables that predict the relative timing are the number of previous EU notices a country received about implementation delays or whether the country's legislative body is bicameral. While we do not believe these variables are proper instruments for our analysis, we note that using the predicted timing based on them (instead of the actual dates) still produces significant effects. See Section 4.2.

securities regulation in the past are at play when new rules come into force. This alternative hypothesis suggests hysteresis in regulatory outcomes, because prior conditions and their underlying factors matter. It implies that imposing the same regulation on countries with disparate prior conditions could make markets diverge more, rather than converge.

We test these competing hypotheses by analyzing whether the treatment effects differ depending on countries' prior regulatory conditions, and find that the liquidity benefits of the two directives are stronger in countries with higher prior regulatory quality.[5] While this finding may be surprising to the proponents of harmonization, it is consistent with the argument that countries with a better track record and stronger prior conditions are also more willing and/or better able to implement and enforce the new rules. We highlight that this hysteresis in regulatory outcomes could occur for several reasons, including lack of institutional fit, resource constraints, inefficient bureaucracies, regulatory capture, and political pressures (e.g., Demsetz, 1969; Stigler, 1971).

Next, we examine the role of differences in implementation and enforcement for regulatory outcomes. We create specific measures of how strictly EU countries implement and enforce the two new directives using data on supervisory powers, penalties, and enforcement actions. We also use staff growth at the securities regulator around the entry-into-force of the two directives as a proxy for the extent to which countries commit resources to the implementation and enforcement of the new regulation. Our results show that countries with stricter implementation and enforcement experience significantly larger capital-market effects.

Finally, we condition on both prior regulation and the proxies for implementation strength. We document that the liquidity effects are strongest in countries with high past regulatory quality

[5] We use the regulatory quality index from Kaufman et al. (2009) to measure the ability of a government to formulate and *implement* sound policies and regulations. While this proxy seems suited for our hypothesis, it is not specific to securities regulation. We find similar results using proxies with a closer link to the strength of prior securities regulation (La Porta et al., 2006; Cumming et al., 2011).

and strong implementation. There are no liquidity increases for countries with low-quality prior regulation and weak implementation. Moreover, while stricter implementation of the new directives often has an incremental effect, this effect is more pronounced in countries with high past regulatory quality. This evidence supports the notion of hysteresis in regulatory outcomes. Countries with weaker securities regulation at the outset do not catch up with stronger countries upon harmonizing regulation. In fact, our results imply the opposite; liquidity differences across countries increase after the directives. Thus, imposing the same regulation on countries with disparate prior conditions can make countries diverge more.

Our paper makes several contributions to the literature. First, we provide causal evidence that the imposition of stricter securities regulation can have significant economic benefits for a broad cross-section of firms. Much of prior evidence showing benefits of securities regulation and strong legal enforcement stems from cross-country studies (e.g., La Porta et al., 1998 and 2006; Doidge et al., 2004; Hail and Leuz, 2006; Cumming et al., 2011). One concern with these studies is that the evidence relies solely on cross-sectional identification. Another stream of studies examines major regulatory *changes* in a single country. Evidence from these studies often casts doubt on the existence of benefits from securities regulation, which is especially true for studies examining the effects of U.S. securities regulation in the 1930s (e.g., Stigler, 1964; Benston, 1969 and 1973; Jarrell, 1981; Mahoney and Mei, 2009). But it also holds for many studies on Regulation Fair Disclosure (henceforth Reg FD) or SOX.[6] However, even these studies face identification challenges because they often lack a convincing control group due to the fact that the new regulation concurrently applies to essentially all SEC registrants and hence they have to rely

[6] For Reg FD see, e.g., Heflin et al. (2003), Gintschel and Markov (2004), Francis et al. (2006). For SOX see, e.g., Chhaochharia and Grinstein (2007), Zhang (2007), Li et al. (2008), Doidge et al. (2010), and Coates and Srinivasan (2014). Cumming et al. (2011) show evidence of liquidity benefits from the MiFID directive in the EU. As this study is related to ours, we discuss its findings in more detail in Section 4.1 and Footnote 25.

on cross-sectional differences among the treated firms (see discussions in Collver, 2007; Leuz, 2007; Coates and Srinivasan, 2014). To get around this issue, Bushee and Leuz (2005) and Greenstone et al. (2006) exploit extensions of U.S. securities regulation that apply only to specific market segments of smaller firms; Iliev (2009) uses a size-based compliance cutoff in SOX Section 404 to employ a regression-discontinuity design. While these studies provide causal estimates, the results are directionally mixed, and they apply primarily to smaller firms. In contrast, our study is not limited to smaller firms. The liquidity relation is identified using the staggered imposition of two EU directives across 26 countries.[7] This approach offers tight identification and yields evidence on the capital-market effects of securities regulation for almost the entire population of publicly traded firms in a broad set of countries.

An important second contribution of our paper is the finding that regulatory outcomes depend on countries' prior regulatory conditions as well as countries' ability and willingness to implement and enforce new rules. This evidence highlights the role of implementation and enforcement and, perhaps more surprisingly, documents substantial hysteresis in regulatory outcomes. The findings are consistent with the enforcement theory formulated in Djankov et al. (2003a) as well as its application to securities regulation in Shleifer (2005). They also provide important insights for attempts to harmonize markets globally through regulatory reforms.

Third, our findings add to the budding literature on securities law enforcement. As Bhattacharya (2006) points out, there is still relatively little work on the role of enforcement in securities markets. In an important paper, Bhattacharya and Daouk (2002) provide evidence that the first enforcement of insider trading regulation lowers firms' cost of capital. Subsequent papers

[7] Agrawal (2013) uses a similar identification strategy, i.e., the staggered passage of investor protection statutes by U.S. states in the 1900s to identify the effects of investor protection on firms' financing and investment decisions in the mining industry. Similarly, Kalemli-Ozcan et al. (2010, 2013) use the staggered entry-into-force dates of the 27 FSAP directives to estimate the effect of financial reform on EU banking integration and as instruments for the effects of financial integration on international business cycle synchronization.

using the same dataset demonstrate other capital-market effects associated with insider trading regulation and enforcement (e.g., Bushman et al., 2005; Ackerman et al., 2008). Our analysis goes beyond insider trading regulation. Moreover, prior evidence on securities law enforcement is based on ex-post measures, i.e., complaints, lawsuits, enforcement actions. The timing of these measures raises endogeneity concerns. The EU setting allows us to provide evidence on capital-market effects associated with regulatory changes in the legal design of enforcement regimes.[8] We show that improvements of countries' supervisory and enforcement regimes have immediate capital-market effects (even before the first enforcement action).

The remainder of the study proceeds as follows. Section 2 develops the hypotheses and provides details on the institutional setting. Section 3 delineates our research design and describes the data. Section 4 presents evidence on the average liquidity effects of the two securities law directives. It also contains validity checks of our identification strategy. In Section 5, we report cross-sectional differences in the treatment effects. Section 6 concludes.

2. Conceptual Underpinnings, Hypotheses, and Institutional Setting

In raising external capital, firms need to reassure outside investors. If outside investors have doubts whether firms will return their money, they are unlikely to provide funds in the first place or, if they provide capital to firms, they are likely to demand a higher return. As providing such reassurance can be difficult and is costly, there is a long-standing debate as to whether securities regulation can be beneficial for a country's capital market, for instance, by mitigating information asymmetry and adverse selection problems.

[8] We also analyze (changes in) supervisory resources. In this sense, our study is related to Coffee (2007), Jackson and Roe (2009), and Cumming et al. (2014), which point to an association between capital-market outcomes and the level of enforcement (e.g., using staff and budgets numbers).

The arguments in favor of securities regulation refer, among other things, to the existence of externalities, economy-wide cost savings, commitment problems, and insufficient private penalties (e.g., Coffee, 1984; Easterbrook and Fischel, 1984; Zingales, 2009; Leuz and Wysocki, 2015). However, these arguments often set aside problems of how to implement and enforce securities regulation.[9] In contrast, Stigler (1971), Posner (1974), Peltzman (1976), and Becker (1983) highlight the difficulties of implementing and enforcing regulation in a way that is socially beneficial.[10] They point out that regulators face serious information problems, are often incompetent or even corrupt, and can be captured in the regulatory process. Thus, much depends on the implementation and enforcement of regulation. That said, private contracts, which are the alternative to regulation, also rely heavily on functioning courts and private litigation. In practice, courts and private litigation can be quite imperfect as well (e.g., Easterbrook and Fischel, 1984; Johnson et al., 2002; Djankov et al., 2003b).

Given these tradeoffs, Djankov et al. (2003a) propose an "enforcement theory of regulation." Their premise is that all strategies for implementing socially desirable policies (e.g., creating deep and functioning capital markets) are likely imperfect, and that the optimal institutional design involves a tradeoff between necessarily imperfect alternatives. Shleifer (2005) applies this theory to securities regulation and argues that the "inequality of weapons" between corporate insiders and promoters on the one side and (often unsophisticated) outside investors on the other side makes it unlikely that private contracts combined with litigation offer an efficient solution in securities markets. In this situation, regulation that prescribes what firms have to disclose to investors could be beneficial because it limits the discretion of courts and mitigates the "inequality of weapons"

[9] Shleifer (2005) argues that the same can be said for the public interest theory of regulation in general.

[10] Illustrating that these concerns also apply to securities regulation, Carvajal and Elliott (2007) point to shortcomings in the ability of securities regulators to effectively enforce compliance with existing rules as a recurring theme in the assessment reports by the International Organization of Securities Commissions (IOSCO).

problem.[11] Thus, securities markets could be an instance in which regulation is beneficial to the economy. Consistent with this conjecture, almost all economies have extensive securities regulation. Obviously, this observation is not sufficient to settle the matter. As mentioned earlier, there are several reasons to be skeptical about the benefits of securities regulation. In line with these concerns, much of the empirical evidence on the effects of securities regulation is mixed and often negative (see survey by Leuz and Wysocki, 2015).

Furthermore, much of the evidence stems from U.S. securities regulation.[12] However, as Djankov et al. (2003a) point out, the tradeoffs can differ greatly across countries. For instance, securities regulation is likely to be more effective in richer economies with better institutions, more efficient bureaucracies, and a greater ability to implement and enforce such regulation. In countries with weak institutions and inefficient bureaucracies, the risk that regulation is abused and hence harmful is larger (Shleifer, 2005; Bhattarcharya and Daouk, 2009). In addition, a country's track record with respect to implementing regulation is likely revealing about its ability and political will to put in place and enforce regulation that induces (curbs) behavior that is deemed socially desirable (undesirable). In sum, the benefits of securities regulation do not just depend on its regulatory design, but also on its implementation and enforcement as well as on the institutional framework that is already in place.

The above discussion provides the conceptual underpinnings for our empirical analysis. We recognize that this study cannot settle the issue of whether securities regulation or a particular regime of securities regulation is socially desirable. But by analyzing capital-market effects around changes in securities regulation, we can provide evidence on whether such regulation has

[11] Based on prior work (e.g., Hay and Shleifer, 1998; La Porta et al., 2006), Shleifer also argues that it can make sense to combine public rules with private enforcement through litigation. See also Jackson and Roe (2009).

[12] There is some international evidence (e.g., Glaeser et al., 2001; Doidge et al., 2004; Hail and Leuz, 2006; La Porta et al., 2006), but it is largely from cross-sectional settings in which it is difficult to isolate the effects of securities regulation.

economic benefits. We can also shed light on the aforementioned tradeoffs and the forces that make securities regulation more or less effective.

Towards this end, our analysis exploits regulatory changes in EU capital markets for which implementation and enforcement issues are pertinent. While the regulatory act itself is held constant across countries, the transposition of EU-wide directives into national law and their supervision, including the specific changes to the supervisory structure, the resources given to the supervisor and the penalties for violations, are left to EU member states. Thus, the setting provides within-EU variation with respect to implementation and enforcement. Based on the earlier discussion, we hypothesize that countries with stricter implementation and enforcement of the EU directives exhibit larger capital-market effects.

The setting also provides cross-sectional variation in countries' prior regulatory conditions, which gives rise to two competing hypotheses. One prediction is that the effects of the new directives are larger in countries where prior securities regulation has been weak, effectively reducing differences across countries (*catching-up hypothesis*). An alternative prediction is that the capital-market effects of the new directives are larger in countries in which regulation has been stronger and more effective in the past (*hysteresis hypothesis*). The reason is that prior regulatory conditions likely reflect various institutional, market, and political forces (or constraints) that determine a country's ability and willingness to implement and enforce policies that induce or curb certain behavior (e.g., insider trading). If such forces are pertinent when new regulation is introduced, the likely outcome is hysteresis, rather than catching up.

To explore the above arguments, we examine the MAD on insider trading and market manipulation, and the TPD, which addresses reporting and disclosure requirements. Both directives pertain to key elements of securities regulation and are at the core of the EU's Financial

Services Action Plan (FSAP), which was established in 1999 with the goal to improve and integrate EU financial markets. As there already was prior EU and national regulation in both areas, the two directives essentially tighten existing regulation, harmonize remaining differences across EU countries and, importantly, stipulate appropriate supervisory and enforcement regimes. The transposition of the MAD and TPD required amending national law(s) in all member states. In the remainder of this section, we briefly describe the two directives. Additional details about the directives as well as the EU's legislative and political process are given in Appendix A.

The MAD was passed by the EU legislature in January 2003 followed by several implementing directives in December 2003. Its purpose is to ensure market integrity and equal treatment of market participants in EU securities markets by defining and prohibiting insider trading and market manipulation. Among other things, the MAD provides a common definition of an insider and establishes transparency standards requiring people who recommend investments to disclose their relevant interests. It also requires each member state to set up an authority that is responsible for monitoring insider trading and market manipulation and to give this authority the necessary supervisory and investigative powers.[13] The MAD further requires cooperation among national supervisory authorities and some, although not complete, harmonization of penalties. It replaces Directive 89/592/EEC, which required EU countries to ban insider trading. Thus, while the MAD expands market abuse regulation in some areas, it is primarily intended to strengthen and harmonize the implementation and enforcement of existing EU regulation (e.g., Lamfalussy, 2000; CRA, 2009).

[13] For instance, the Financial Services Authority (FSA) in the U.K. received additional powers that allow it to obligate persons to comply with market abuse provisions and to gather evidence in the course of an investigation by requesting a search warrant. Similarly, the Portuguese regulator (Comissão do Mercado de Valores Mobiliários) received additional powers to seize, freeze, seal, or inspect any documents related to the suspected offences from persons and entities subject to its supervision.

The TPD was passed by the EU legislature in December 2004 and its implementing directive was enacted in March 2007. The directive requires issuers of traded securities to ensure appropriate transparency for investors by disclosing and disseminating periodic and ongoing regulated information. Regulated information comprises, among other things, periodic financial reports and information on major holdings of voting rights. However, prior EU directives, member state laws, and exchange requirements already stipulated annual and interim financial reports as well as the disclosure of other ongoing information. As such, the TPD does not substantially expand existing disclosure requirements, but rather focuses on (better) supervisory regimes and enforcement. For instance, the TPD stipulates that, in each member state, a supervisory authority assumes responsibility for monitoring compliance with the provisions of the directive and that this authority examines firms' regulated disclosures (e.g., financial statements). Such an authority and review process did not exist in several EU countries at the time and was introduced (or expanded) following the TPD.[14] The TPD also requires that the authority is given appropriate enforcement tools, including the power to carry out on-site inspections. Thus, the TPD primarily clarifies and harmonizes existing disclosure regulation, but substantially improves its enforcement.

Apart from the MAD and TPD, the FSAP brought numerous other legislative initiatives in the area of financial market regulation. Among the ones geared towards securities markets were the IAS Regulation that mandated International Financial Reporting Standards (IFRS), the Prospectus Directive regulating disclosures during public securities offerings, the Markets in Financial

[14] For instance, following the TPD, the Swedish Financial Supervisory Authority (SFSA) was charged to enforce financial reporting requirements starting in July 2007. Before that date, the stock exchanges (and not a national supervisory body) performed reviews of the annual reports. The new regime is stricter, more extensive and proactive. The first sanctions under the new regime were imposed in 2008 and included public disclosure of violations and corrections to the financial reports going forward. Some EU countries like the U.K. introduced such a review process already with the adoption of IFRS and hence before the TPD. See Christensen at al. (2013) for a discussion and analysis of these changes.

Instruments Directive (MiFID) on the provision of investment services across the EU, and the Takeover Directive, which provides a framework for mergers and acquisitions and takeover bids in the EU. All these directives were implemented over the 2004 to 2009 period, and hence could potentially confound an empirical analysis of the MAD and TPD.[15] We address the issue of concurrent other regulations with our identification strategy.

3. Research Design and Data

3.1. Empirical Model and Identification Strategy

Based on the institutional discussion in the previous section, the MAD and TPD are predicted (intended) to reduce information asymmetries and adverse selection in capital markets. We test this hypothesis using a panel dataset with quarterly firm-level observations of stock market liquidity. We focus on liquidity for three reasons. First, an extensive body of theory links market liquidity to the magnitude of information asymmetry and adverse selection in markets (e.g., Copeland and Galai, 1983; Glosten and Milgrom, 1985; Kyle, 1985; Easley and O'Hara, 1987; Admati and Pfleiderer, 1988). Furthermore, economic theory predicts that reducing insider trading or enhancing transparency reduces information asymmetries between investors and hence, adverse selection in capital markets (e.g., Diamond and Verrecchia, 1991; Verrecchia, 2001). Second, a frequently stated policy goal of the directives and of securities regulation in general is to increase market confidence and liquidity (e.g., Lamfalussy, 2000; Enriques and Gatti, 2008; CRA, 2009). Thus, the analysis focuses on an outcome that matters to regulators and policy makers. Third, liquidity is well suited for our empirical strategy. We can measure market liquidity over

[15] Conceptually, MiFID is the biggest potential confound for a liquidity analysis given its focus on investment services (e.g., order handling and trade transparency). However, MiFID was passed in part by regulation, which implies that the effective date is set by the EU (November 2007). If its effect occurs around that date, it is absorbed by our quarter-year fixed effects. In addition, we also use the dates by which EU countries completed the implementation of all the MiFID provisions to control for its effects.

reasonably short intervals, and it should reflect the effects of regulatory changes at the time they occur. That is, liquidity is likely less anticipatory than other economic outcomes like firms' market valuations or cost of capital.[16] At the same time, liquidity conceptually and empirically maps into these important other capital market outcomes (see survey by Amihud et al., 2005).

We estimate the following model (without firm and time subscripts) using quarterly observations (also illustrated in Figure 1). The choice of quarterly data reflects a tradeoff between reliably measuring liquidity over some interval and capturing changes in liquidity in a timely fashion around the time the MAD and the TPD come into force:

$$Liq = \beta_0 + \beta_1\, MAD + \beta_2\, TPD + \sum \beta_j\, Controls_j + \sum \beta_i\, Fixed\ Effects_i + \varepsilon. \quad (1)$$

The dependent variable, *Liq*, stands for market liquidity, described in more detail below. *MAD* and *TPD*, our main variables of interest, are binary indicators coded as '1' beginning in the quarter in which the corresponding directive comes into force in a given EU member state and '0' otherwise. $Controls_j$ denotes a set of firm- and country-level control variables. Among other things, we explicitly control for several other FSAP directives introduced over the sample period that are less pertinent for our purposes but could affect liquidity (see Section 2). $Fixed\ Effects_i$ represents country, industry, and quarter-year fixed effects. The purpose of the quarter-year fixed effects is to eliminate liquidity trends and shocks that are common to all EU member states in a given quarter. As a result, the identification stems from within-EU variation in the entry-into-

[16] While investors likely adjust their estimates of firm value or the cost of capital as soon as regulatory changes are announced, liquidity is less anticipatory because investors primarily worry about adverse selection at the time they trade. It is of course possible that investors anticipate when buying shares that future regulatory changes will reduce adverse selection at the time they sell the position. But this anticipatory effect is likely small (consistent with the evidence in Figure 2 and, in particular, the results in Table 3, Panel A).

force dates of the two directives. We draw statistical inferences based on standard errors clustered by country.[17]

Our empirical strategy for estimating the effects of securities regulation on market liquidity is based on the following institutional features. First, after the enactment of a new EU directive, each member state must transpose the directive into national law. As a result of this legislative process, new directives come into force at different points in time across member states. For the MAD, the dates range from April 2004 to January 2007, and for the TPD they spread from January 2007 to August 2009 (see Table 1). The staggered introduction allows us to use quarter-year fixed effects, which alleviates two common concerns that typically arise when analyzing a regulatory change (see also Kalemli-Ozcan et al., 2010, 2013). One concern is that the results could reflect general time trends or market-wide changes (e.g., macroeconomic shocks) that are concurrent with but unrelated to the regulatory change. Another concern is that new regulation is often put in place after major economic events, such as a crisis or corporate scandal. It is likely that markets respond to these preceding events and, hence, the results could reflect this market response rather than the effects of the new regulation (e.g., Ball, 1980; Mulherin, 2007). For these two concerns to arise in our setting, confounding events would have to be correlated with the effective dates of *two* directives across 26 countries, which seems unlikely.

Second, the regulatory act takes place at the EU level and, hence, the new regulation is not specific to any particular country. Member states have to transpose the directives within a limited

[17] We also conduct placebo analyses in which we randomly assign implementation dates between the first quarter of 2001 and the second quarter of 2004. This period precedes the entry-into-forces dates for the MAD in all countries, except for Lithuania. Using 500 replications, the average placebo effect is close to zero, as expected. We use the distribution of placebo coefficients to bootstrap the significance levels in our main analyses. None of the 500 placebo regressions produced coefficients that exceed the MAD or TPD coefficients reported in Table 2. Based on this evidence, the estimated effects are clearly statistically significant and, if anything, our inferences using standard errors clustered by country are conservative. We also consider two-way clustering and clustering by economic region. See Appendix B.

amount of time (typically about two years). This window is not particularly generous. To illustrate, the law that implemented the MAD in the U.K. was laid before the parliament in February 2005 and entered into force in July 2005. The consultation period started in June 2004 with an initial draft, so the implementation process must have started even earlier. In total, the observable part of the implementation process exceeded one year. We illustrate in Table A1 in the appendix that such a lengthy process is not uncommon among EU countries. Moreover, standard due process for new EU laws implies that the transposition of the directives into national law is fairly inflexible.[18] In Appendix A, we provide more details on the legislative processes in the EU supporting the claim that they are lengthy and inflexible.

The narrow transposition window set by the EU combined with lengthy and inflexible legislative processes at the national level substantially reduce concerns about reverse causality and endogenous implementation timing. In our setting, reverse causality would imply that lawmakers are able to predict future liquidity changes (by a year or more) and then time legislative processes and effective dates accordingly. This seems implausible. The institutional setting also mitigates the more relevant concern that the entry-into-force dates are endogenous because lawmakers respond to local conditions (e.g., liquidity shocks) when deciding to implement the directives. For this concern to arise in our setting, countries would have to experience a series of *differentially*-timed local shocks, which in turn prompt national lawmakers to start the country's implementation process for a directive *and* this legislative process has to result in effective dates that coincide with subsequent liquidity improvements (or reversals). As this sequence seems unlikely, we argue that the directives' entry-into-force dates across countries can be viewed as plausibly exogenous.

[18] Despite the inflexibility, it is possible that timing is related to a country's willingness to implement the new directives. Differential willingness across countries could lead to heterogeneous treatment effects. But such heterogeneity likely stems from the way countries implement the directives (and not the implementation timing per se, given our fixed-effects structure accounts flexibly for liquidity trends). See Section 5.

In sum, our setting is well suited to identify causal effects of securities regulation on market liquidity for a broad cross-section of firms and countries. However, as the exogeneity of the implementation dates is key to our identification strategy, we provide extensive analyses gauging this assumption in Section 4.2. In particular, we perform several sensitivity analyses that more tightly control for local shocks. One of these analyses uses within-country estimation exploiting that firms on unregulated markets are not (or less) affected by the directives. The within-country specification flexibly accounts for local shocks that apply to all firms in an economy.

3.2. Data and Construction of the Variables

Our sample period starts in the first quarter of 2001, before the EU adopted the MAD and the TPD, and hence well in advance of the first country-specific entry-into-force dates for the two directives. The sample period ends in the second quarter of 2011, which gives us a minimum of two years of post-implementation data for all EU countries. We include all the firm-quarter observations from EU countries for which we have the necessary data to compute the capital-market and control variables to estimate our basic regression model stated in Eq. (1). Table 1, Panel A, provides an overview of the sample composition by country.[19] The sample comprises 112,260 firm-quarter observations from 26 countries. We exclude firms with U.S. cross-listings as they are subject to U.S. insider trading and transparency rules. In addition, we eliminate firms trading on unregulated EU markets that are not necessarily subject to the MAD and TPD (e.g., the

[19] Our treatment sample also includes Iceland and Norway, which are not in the EU but belong to the European Economic Area (EEA). We include them because they have agreed, among other things, to adopt the EU capital market directives (such as the MAD and the TPD) in exchange for access to the EU's single market. For simplicity, we refer to them as EU countries throughout this paper. Furthermore, we exclude Bulgaria and Romania in the empirical analysis even though they are EU member states because they adopted *all* EU regulations (including the MAD and TPD) upon joining the EU in January 2007. The results are not sensitive to either of those sample choices. We exclude Malta as we do not have sufficient data.

Alternative Investment Market in London) as well as very small firms with, on average, market values below US$ 5 million.[20] We further require at least four quarterly observations per firm.

Panel A of Table 1 also lists the dates when the national law(s) that implemented the MAD and TPD came into force in a country. We collect the *Entry-into-Force Dates* from publications by the European Commission for the MAD and by Linklaters LLP, an international law firm, for the TPD, and validate them with the dates on which each EU member state informed the European Commission of its compliance with the directives. In case of discrepancies, we contacted the national securities regulator to resolve the issue. As the table shows, the MAD dates vary from April 2004 (Lithuania) to January 2007 (Bulgaria and Romania), the TPD dates from January 2007 (Bulgaria, Germany, Romania, and the U.K.) to August 2009 (Czech Republic).

As mentioned in the previous section, we also include controls for other FSAP directives that are related to securities markets, namely the Takeover Directive (*TAKEOVER*), the Market in Financial Instruments Directive (*MiFID*), the Prospective Directive (*PROSP*), and the IAS Regulation that mandated IFRS adoption (*IFRS*). Except for *IFRS*, we define the control variables for the other FSAP directives in the same way as for the MAD and TPD, i.e., as binary variables that are coded as '1' beginning in the quarter in which the respective directive comes into force in a given country (and '0' otherwise). We collect the entry-into-force dates from publications by the European Commission. For *IFRS*, we use a firm-specific indicator to account for the fact that a firm's fiscal-year end determines the quarter it starts reporting under IFRS. We code a binary variable that switches to '1' in the quarter after the first fiscal-year end for which IFRS reporting is mandatory (see also Christensen et al., 2013).

[20] We impose the latter restriction because tracking listings on unregulated markets over time is difficult. For instance, Datastream provides only static exchange information. As firms trading on unregulated markets tend to be smaller, the size criterion is another way to identify such firms and to make sure that they do not enter the main analysis. We exploit firms trading on unregulated markets as a benchmark in Section 4.2.

We use four different liquidity proxies. The first proxy is *Bid-Ask Spread*, which is conceptually close to the desired construct and commonly used in empirical research to capture information asymmetry (e.g., Stoll, 1978; Venkatesh and Chiang, 1986; Glosten and Harris, 1988). We obtain the closing bid and ask prices for each day and compute the daily quoted percentage spread as the difference between the two prices divided by the mid-point. We then take the mean daily spread over the quarter for a given firm. The second proxy is *Price Impact* computed as the quarterly mean of the Amihud (2002) illiquidity measure (i.e., daily absolute stock return divided by US$ trading volume).[21] The third proxy is *Zero Returns*, defined as the proportion of trading days with zero daily stock returns out of all potential trading days per quarter. The fourth proxy is *Total Trading Costs*—an estimate of the total round trip transaction costs (including bid-ask spreads, commissions, and implicit costs from short-sale constraints or taxes). The *Total Trading Costs* are calculated based on a quarterly time-series regression of daily stock returns on the aggregate market returns (Lesmond et al. 1999).[22] To mitigate measurement errors and concerns about any individual proxy, we follow Daske et al. (2008) and Lang et al. (2012) and aggregate the four liquidity proxies into a single *Liquidity Factor* using factor analysis. We use the factor scores from the first (and only) factor with an Eigenvalue greater than one as the dependent variable.

As control variables, we include gross domestic product (GDP) per capita to capture differences in countries' macro-economic development and, following prior literature (e.g., Chordia et al., 2000; Leuz and Verrecchia, 2000), the market value of equity, share turnover, and

[21] To avoid the misclassification of days with no or low trading activity (i.e., days potentially yielding a price impact of zero), we omit zero-return days from the computation of the quarterly means.

[22] This measure is based on the logic that informed investors do not trade when the cost of trading exceeds the value of new information. Since private information is not observable, we use log-likelihood estimation to extract a proxy of total trading costs from a system of equations employing a panel of firms' daily stock returns and equal-weighted local market index returns. Following Lesmond (2005), we require at least 24 daily returns and 20 percent of the daily returns to be different from zero per firm-quarter observation. To reduce measurement error, we eliminate estimates below a lower bound of one basis point (see the appendix in Daske et al., 2008, for details on the estimation).

return variability to account for differences in firm characteristics—all lagged by four quarters to allow for seasonal effects. The notes to Table 1 provide further details on the variable measurement. We estimate the regressions in a log-linear form taking the natural logarithm of the liquidity factor and the continuous control variables. Price and volume data are from Datastream.[23] Except for variables with natural upper and lower bounds, we truncate variables at the first and 99th percentile. Panel B (Panel C) of Table 1 reports descriptive statistics for (correlation coefficients between) the firm-level variables.

4. Capital-Market Effects of Tighter Securities Regulation in the EU

4.1. Average Liquidity Effects

We present the results of four specifications in Table 2. In Columns (1) and (2) we estimate the effects for each directive separately; in Column (3) we combine the two directives in one model; and in Column (4) we include additional controls for other EU directives in the FSAP that are related to securities markets. The explanatory power of the regressions is high with an R^2 of 66 percent, suggesting that the model accounts for the main drivers of liquidity. The firm-specific control variables are significant and exhibit the expected signs. Large firms and firms with a high share turnover are more liquid, and firms with more volatile returns are less liquid. GDP per capita is insignificant and among the other FSAP directives only *IFRS* is negative and significant. The negative coefficient on *IFRS* in the EU is consistent with Daske et al. (2008) and Christensen et al. (2013). The insignificant coefficient on *MiFID* is inconsistent with the results in Cumming et al. (2011). Cumming et al. (2011) examines the effect of exchange trading rules on market liquidity and also uses the introduction of MiFID in some of the tests. Our insignificant *MiFID*

[23] Our primary source of bid-ask spread data is Datastream. To increase sample size in some of the smaller EU countries (i.e., Czech Republic, Latvia, Luxembourg, Romania, Slovakia, and Slovenia) we complement this data with spreads from Bloomberg. Doing so does not materially affect the results.

result could have two reasons. First, it could indicate that the concerns raised about MiFID (e.g., increased market fragmentation due to a higher number of trading platforms) are indeed warranted. Second, it could reflect the fact that MiFID was passed in part by regulation, which means that not all provisions and implementing directives required approval from each EU member state's national parliament, and that a common effective date was set by the EU (November 1, 2007).[24] Thus, MiFID offers, at best, little variation in the entry-into-force dates. Given this issue, the quarter-year fixed effects in our model could absorb the effects of MiFID, if there is one.[25]

For our test variables, we find that the coefficient on both *MAD* and *TPD* are negative and statistically significant. *MAD* and *TPD* remain significant and have similar magnitudes when we jointly include them in the model and when we control for the other FSAP directives. The results imply that tighter securities regulation increases market liquidity. The estimated effects are economically significant. Using Column (4), the MAD coefficient of -0.115 suggests that, on average, liquidity increases by 11 percent.[26] For TPD, the coefficient of -0.093 indicates an average increase in liquidity of about 9 percent.

As a way to further gauge the economic magnitude, we translate the percentage effects into value-weighted average annual trading cost savings per firm (although we note that the economic effects of liquidity improvements go beyond trading costs savings, so our computation provides a lower bound on the capital-market effects. We multiply the estimated trading cost reductions with

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[24] In our analysis, we use instead the date by which *all* MiFID provisions were adopted in a given country. Nonetheless, the variation in dates is limited: one country adopted in the third quarter of 2007, 24 in the fourth quarter of 2007, one in the first quarter of 2008, two in the second quarter of 2008, and one in the fourth quarter of 2008 (see summary transposition tables provided by the European Commission).

[25] To test whether limited variation in the entry-into-force dates is responsible for the insignificant MiFID result, we alternatively include non-EU benchmark countries in the sample and define the year-quarter fixed effects across all countries. In this specification, we identify the effect of MiFID off differential liquidity trends between EU firms and non-EU firms. We find that the coefficient on MiFID is insignificantly negative when we do not control for the other directives. However, once we add the other directives to the model, the coefficient on MiFID becomes insignificantly positive.

[26] We compute the average percentage change in liquidity as $e^{-0.115} - 1 = -0.108$ (or roughly 11%).

the total yearly dollar trading volume per firm and then divide by the mean market capitalization. Using bid-ask spreads, the annual cost savings are approximately US$ 0.16 and 0.13 million (or 0.053 and 0.043 percent of market value) for the MAD and the TPD, respectively. Using total round-trip trading costs, the annual savings are approximately US$ 0.43 and 0.35 million (or 0.103 to 0.084 percent of market value) for the MAD and the TPD. These numbers are clearly economically significant, in particular when considering the recurring nature of the savings. In sum, we provide evidence that the tightening of securities regulation in the EU has resulted in significant capital-market benefits.

4.2. Assessing Identification and Results from Within-Country Estimation

As discussed earlier, the way the EU passes and implements directives provides a natural experiment to estimate the causal effects of stricter securities regulation on market liquidity. The process mitigates many of the usual concerns when analyzing regulatory changes and provides variation in the implementation dates that can be exploited in the estimation.[27] In this section, we further assess our empirical strategy, in particular, the notion that countries' entry-into-force dates are plausibly exogenous. If the dates provide reasonably sharp identification with respect to changes in market liquidity, then any bias or threat to identification would have to come from omitted factors that are correlated with both the distribution of entry-into-force dates within the EU *and* concurrent changes in market liquidity. Such a correlation would arise if countries chose to implement the directives in response to local liquidity shocks. In Section 3, we argue that such selection of the dates is unlikely for institutional reasons. Here, we provide empirical support for this claim as well as additional analyses that mitigate concerns about local shocks.

[27] Note that within-EU estimation and the introduction of quarter-year fixed effects are very demanding and could capture some fraction of the treatment effect, particularly (i) if there is clustering of the implementation dates across countries, (ii) if the dates are measured with noise, or (iii) if the directives have a more gradual rather than a sharp effect. The latter concern primarily applies to the TPD for which many countries had to create new supervisory and enforcement processes including hiring additional staff. See Appendix A.

We begin by assessing the sharpness of the liquidity effects. It is a priori not obvious that the treatment effect of the directives is indeed "sharp" right at the entry-into-force date. We do not expect a pattern as sharp as in medical trials or educational experiments for which the timing of the treatment can be precisely controlled. For instance, investors may already respond with less insider trading *shortly* before the directives' effective dates because they expect countries' enforcement capabilities to increase in preparation of the new directives. Similarly, investors may expect a higher willingness to pursue insider trading violations if they occurred shortly before the new regime. Thus, some anticipation or response prior to the entry-into-force date is conceivable, but it should be limited (as explained in Footnote 16). Moreover, several countries (like the U.K.) implemented several TPD measures (e.g., proactive reviews of financial statements) before the official entry-into-force date and in conjunction with IFRS adoption (Christensen et al., 2013). Conversely, it may take some time for the new enforcement bodies that were created due to the TPD to become fully effective. For instance, setting up a review process for financial information requires hiring and training additional staff.[28] Thus, the TPD effect likely is less sharp.

To empirically gauge whether the implementation dates provide reasonable identification, we introduce a separate indicator variable into the model for the year leading up to the two directives (i.e., quarter t-4 to quarter t-1). The purpose of this analysis is to see whether liquidity is already elevated or trending ahead of the directives (in event time). Contrary to this concern, we find that the coefficient marking the liquidity effects in the year prior to the directives is small and insignificant (Table 3, Panel A). An F-test confirms that liquidity is significantly higher after the directives become effective compared to the preceding year. Thus, liquidity increases right around the time the directives enter into force.

[28] Descriptive evidence for Germany on the introduction of such reviews shows that it may take a year or two before the enforcement agency gets to a steady state (Ernstberger et al., 2012). Our survey of regulators and auditors also supports the notion that enforcement activities under the TPD were gradually increased over time.

Next, we (counter-factually) shift the assignment of the implementation dates by one quarter for all EU countries, and each time re-estimate our base model from Table 2 noting the coefficient on *MAD* or *TPD*. If the liquidity effects are indeed caused by the directives, the estimated coefficients should be attenuated as we move away from the true implementation dates (in *both* directions). As Panel B of Table 3 shows, the coefficients on *MAD* peak at the true entry-into-force dates and become smaller in magnitude (and significance) as we counterfactually move away from the true dates. This pattern shows that the implementation dates are indeed critical, consistent with a causal interpretation. For the TPD, the pattern is not centered at t=0; coefficients continue to increase beyond the true entry-into-force dates before they start reversing (after quarter t+5). This delayed pattern suggests that it takes a few quarters until the TPD becomes fully effective, but it again shows that the onset of the liquidity effects is closely related to the entry-into-force dates.

To graphically illustrate the sharpness of the entry-into-force dates, we plot the liquidity effects in event time. We estimate a version of the model in Eq. (1), in which we replace the single *MAD* or *TPD* indicator with a series of 16 separate indicator variables, each marking one quarter over the period t–8 to t+8. We omit the indicator for the quarter immediately before the entry-into-force of the respective directive (t–1) so that this quarter serves as benchmark period for the other quarters (i.e., by definition, the coefficient value for quarter t–1 is zero). For this analysis, we limit the sample to quarters t–8 to t+8 and keep only countries that have observations in all 17 periods, so that the country composition is held constant and coefficients are comparable over time. Figure 2 plots the coefficient estimates for each relative quarter together with the 95% confidence interval. For the MAD, the liquidity effect is fairly immediate and becomes significant starting with the entry-into-force quarter (t=0). The effect slightly increases in magnitude over time and remains consistently significant through quarter t+8. For the TPD, the time-series pattern

is less sharp, as expected, and it takes some time for the quarterly indicators to fall significantly below the benchmark period. Nonetheless, the graph suggests a clear shift in market liquidity after the TPD becomes effective. For both directives, the coefficients in the pre-period (*t*–2 to *t*–8) are indistinguishable from the benchmark quarter, showing again that there is no pre-trend.

All the tests above confirm that the entry-into-force dates provide reasonably sharp identification. For both directives, the documented increase in liquidity occurs within a few quarters of the country-specific entry-in-force dates and is sustained. Thus, for the remainder of the section, we focus on the question of whether the country-specific implementation dates are indeed plausibly exogenous. In particular, we gauge the concern that countries choose to implement the directives in response to local liquidity shocks, which would render the dates endogenous. We conduct five additional tests and present their results in Panel C of Table 3.

In Column (1), we include an additional control for the firm-specific change in liquidity from quarter *t*-8 to *t*-4, which is the period over which the national bureaucracies typically prepared the regulations implementing the directives (see Appendix A). This model gets at the possibility that mean reversion in liquidity or a response to local liquidity shocks for particular groups of firms play into our findings. Column (1) shows that adding lagged changes does not materially affect the estimated coefficients for *MAD* or *TPD*. In addition, we test whether the relative timing of the entry-into-force dates is associated with countries' average liquidity change in the year after the respective directive passed at the EU level (not tabulated). For both directives, the association is insignificant (p=0.24 and p=0.39, respectively) and, if anything, the association is positive, which is the opposite of what one would expect if liquidity shocks motivated countries to begin or accelerate the directives' implementation. Thus, prior liquidity shocks do not explain our findings or the relative implementation timing.

In Column (2), we add linear and quadratic time trends for each country to the model. This specification controls for time trends in individual EU countries on top of the quarter-fixed effects, which already control for EU-wide trends. The estimated coefficient on *MAD* is hardly affected by the additional trends. The *TPD* coefficient is somewhat attenuated, which is not surprising given the pattern of the liquidity effects shown in Figure 2 (and consistent with the more gradual implementation of this directive). However, the *TPD* coefficient remains significantly negative, confirming that the treatment effect is reasonably sharp and distinct from a quadratic trend. Thus, differential country-specific trends are unlikely to explain our results.

In Column (3), we provide an even stricter test, but for a limited set of countries. We restrict the sample to three countries with large unregulated markets: Germany (Open Market), Ireland (Enterprise Securities Market), and the U.K. (Alternative Investment Market AIM). The idea for this test is that the two directives are geared primarily towards regulated markets and do not apply (or only to a lesser extent) to firms trading on unregulated markets. Thus, unless there are spillover effects (e.g., due to externalities or competition with the regulated markets), firms on unregulated markets should be less affected by the two directives and, hence, provide a within-country benchmark. That is, by adding unregulated market firms to the sample, we can use within-country estimation and include country-specific year-quarter fixed effects. These fixed effects absorb local shocks and country-specific trends in an even more flexible fashion than do the linear and quadratic trends in Column (2). The coefficients on *MAD* and *TPD* identify the liquidity effects around the directives for firms on regulated markets relative to firms in the same country on unregulated markets. As shown in Column (3), both the coefficients on *MAD* and *TPD* are significant and similar in magnitudes to the estimates in Table 2. In Column (4), we go one step further and estimate the within-country specification for a balanced and propensity-matched sample to ensure that we have the same number of regulated and unregulated firms and that these

firms have similar characteristics.[29] Again, the coefficients on *MAD* and *TPD* remain significant and are only slightly attenuated relative to Table 2.

In Column (5), we present a test directly gauging the concern that the staggered dates are endogenous. Under the selection explanation, local shocks affect both liquidity and the timing of when lawmakers implement the directives, leading to an endogenous correlation between liquidity and countries' entry-into-forces dates (and hence the treatment variables). We test this alternative explanation with a falsification exercise in the spirit of Altonji et al. (2005). We first re-estimate Model 3 of Table 2, but exclude the variables of interest (*MAD* and *TPD*) and instead add several proxies for local economic conditions and political forces. These forces have the potential to influence the timing of when lawmakers implement the directives and, at the same time, could be correlated with liquidity changes. Specifically, we regress the liquidity factor on a series of election dummies capturing the relative timing to local elections (i.e., years t–5 to $t=0$) as well as five macroeconomic variables (i.e., annual inflation, GDP growth, gross domestic savings, net portfolio equity inflows, and the net capital account, all scaled by GDP). This first-stage regression generates *predicted* liquidity values conditional on observable candidates for selection. We then use these predicted values as dependent variable in our main specification. Under the alternative explanation that local conditions and shocks induce our results, we should find coefficients that are similar to those in Table 2. However, as Column (5) in Panel C shows, the coefficients on *MAD* and *TPD* are statistically insignificant, and their magnitudes are small in an absolute and relative sense. Given that a model with selection on observables does not come close

[29] We conduct the propensity matching within each country based on the following firm characteristics (measured in the quarter before the MAD came into force): total assets, return on assets, book-to-market, asset growth, quarterly stock returns, and annual standard deviation of daily stock returns. For each matching variable, we ensure that there are no statistically significant differences between regulated and unregulated firms. We further require that firms exist prior to and after the MAD and TPD became effective.

to explaining our findings, selection on (time-variant) unobservables would have to be huge (and also unrelated to the observables in the model) if it were to explain our results.

Finally, we analyze whether we can explain the relative transposition timing of the two directives with variables that indicate how early or late a country is in implementing EU regulation in general. Specifically, we attempt to predict countries' relative timing of the MAD (TPD) in a regression model with the following variables: (i) the time (measured in months) it took a country to implement the TPD (MAD), (ii) whether the country has a bicameral legislature, and (iii) the median annual number of letters of formal notice that a country received between 2005 and 2011.[30] These explanatory variables are significantly associated with countries' implementation timing (the *F*-statistics and R^2 values for the MAD and TPD models are 4.60 and 4.75, and 39.6% and 40.4%, respectively).[31] This evidence suggests that countries' *relative* timing is persistent across different directives, consistent with the notion that the implementation dates are plausibly exogenous and driven by features of the national legislative processes such a bicameral legislature. While we do not believe that the three variables are proper instruments in an IV sense, we still find about half of the treatment effects for each directive when we use the *predicted* dates based on the model with the three variables (instead of the actual dates). These effects are economically and also statistically significant using the distribution of placebo coefficients described in Footnote 17.

In sum, it is unlikely that selection and endogenous entry-into-force dates are responsible for our findings. The relative timing is not associated with prior liquidity shocks, implementation strength, prior regulatory quality, or macro variables at the time of implementation. Instead

[30] A letter of formal notice is a request by the European Commission for comments by national governments on issues of non-compliance with EU law and represents the first step in a formal procedure designed to resolve suspected violations, including tardiness or delays in the adoption of EU directives.

[31] In contrast, the five macro-economic variables used in the falsification exercise above do not explain the relative timing of the MAD and TPD (F-stats are 1.35 and 0.17, respectively). Similarly, Table B2 presents contingency tables that show that implementation timing is *not* significantly related to regulatory quality and implementation strength. If the dates were endogenous, one might expect to see such associations.

countries are consistently early or late in implementing directives. In Appendix B, Table B1, we provide an extensive set of sensitivity analyses regarding (i) the clustering of the standard errors, (ii) the choice of the fixed-effects structure as well as shocks that differentially affect firms and countries, (iii) the sample composition, (iv) the use of share turnover and market value as control variables; and (v) the construction and use of the liquidity factor. These analyses show that our findings and estimated magnitudes are robust to a wide variety of alternative design choices.

5. Role of Prior Regulation and Differential Implementation of Securities Regulation

5.1. Partitioning Variables to Test for Heterogeneity in the Treatment Effects

Our analyses up to this point suggest that the imposition of tighter securities regulation has a causal effect on market liquidity. In this section, we examine heterogeneity in the treatment effects, in particular, due to differences in prior regulatory conditions as well as in implementation and enforcement. For the reasons discussed in Section 2, it is unlikely that the directives have uniform effects throughout the EU. For instance, countries with a proven track record of implementing regulation and government policies are expected to implement new regulation in an effective manner; others might do so to a lesser extent. Furthermore, it is plausible that, by improving the enforcement regime, the new directives complement existing securities regulation and, hence, benefit mostly countries with extensive regulation. Alternatively, one could argue that countries with weaker securities regulation should benefit the most from EU-wide efforts to harmonize and improve extant regulation.

To explore these arguments and test for cross-sectional differences in the liquidity effects, we introduce two (non-overlapping) partitioning variables, one for high levels and one for low levels, into the base specification, leading to the following extended model:

$$Liq = \beta_0 + \beta_1\, MAD\ (TPD) \times Partitioning\ Variable_{High} + \beta_2\, MAD\ (TPD) \times$$
$$Partitioning\ Variable_{Low} + \sum \beta_j\, Controls_j + \sum \beta_i\, Fixed\ Effects_i + \varepsilon. \quad (2)$$

The *Partitioning Variables* are binary indicators set to '1' for the group of EU countries with high/low realizations of several institutional characteristics, respectively, and to '0' otherwise. We then test for significant differences between the coefficients β_1 and β_2 to assess whether the liquidity effects of the directives differ within the EU. Everything else is defined as in our base specification (see Model 4 in Table 2). We partition EU countries with regard to the quality of prior regulation and the strength with which the MAD and TPD are implemented and enforced. Thus, the analyses examining the heterogeneity in the treatment effects rely solely on cross-sectional differences across countries. Such variation is subject to fairly standard correlated omitted variable concerns and, hence, we cannot draw causal inferences about the precise reasons (or sources) for the heterogeneity in the treatment effects, despite the fact that the average treatment effect is well identified as shown in Section 4.

Table 4 provides a by-country overview of the partitioning variables (and the resulting binary indicators). The first variable is the quality of prior regulation. We use an index taken from Kaufmann et al. (2009) that measures "the government's ability to formulate and implement sound policies and regulations that permit and promote private sector development" (*Regulatory Quality*). Higher index values indicate better regulatory quality. We split the sample countries by the EU median in 2003, that is, before the two directives came into force.

Next, we develop directive-specific measures for the implementation and enforcement of the MAD and TPD across EU member states: (i) $Maximum\ Fine_{MAD}$ is the maximum monetary penalty that the supervisory authority can impose on security issuers for violations of Article 2 of

the MAD (CESR 2008).[32] (ii) *Supervisory Powers*$_{MAD}$ equals the number of positive answers (out of 86 possible) by the supervisory authority in each EU member state to a questionnaire on the existence of specific supervisory powers regarding the translation of the MAD into local law (CESR 2007).[33] Higher values imply more supervisory powers. (iii) *Shift in Enforcement*$_{MAD}$ indicates EU countries that have taken at least a single enforcement action regarding violations of the MAD by 2009 (e.g., imposed a fine).[34] (iv) *Maximum Fine*$_{TPD}$ is the maximum monetary penalty that the supervisory authority can impose on security issuers for violations of Articles 4 to 6 of the TPD (CESR 2009a).[35] (v) *Supervisory Powers*$_{TPD}$ represents the sub-set of EU countries that by the end of 2008 fully comply with all the enforcement principles proposed in CESR Standard No. 1.[36] (vi) *Shift in Enforcement*$_{TPD}$ indicates a substantial change in the enforcement of financial reporting rules around the entry-into-force of the TPD. We construct this variable based on a survey that we sent out to the authority responsible for supervising compliance with accounting standards and the technical departments of PricewaterhouseCoopers, an international

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[32] Article 2 of the MAD deals with insider trading. More specifically, Article 2 prohibits any person who possesses inside information from using that information in trading securities, for his own account or the account of a third party.

[33] The CESR (2007) survey covers Articles 1.5 through 16.4 of the MAD. For instance, the question for Article 2, which bans the use of insider information, is: "Does your authority have the power to establish whether or not an individual has access to insider information?" For Article 3, banning the tipping of third parties, they ask: "Does your authority have the power to evaluate the application of the provisions of MAD related to the disclosure of inside information to third parties?"

[34] We establish whether enforcement actions were taken based on CESR (2010), a review report that summarizes the enforcement actions in the EU since the introduction of MAD. For instance, the U.K. supervisory authority fined Woolworths Group plc £350,000 with respect to a breach of the rule related to Article 6.1 of the MAD. These provisions impose the obligation on security issuers to release inside information as soon as possible, and to avoid the creation or continuation of a false market in listed securities (CESR, 2010, p. 72).

[35] Articles 4 to 6 of the TPD deal with periodic reporting requirements. More specifically, Article 4 requires the release of an annual report within four months of the end of the fiscal year including audited financial statements, a management report, and a statement of compliance by the persons responsible within the issuer. Article 5 regulates the publication of semi-annual financial reports. Article 6 requires that issuers make a public announcement during both the first and the second half of the fiscal year about the financial position and performance of the firm.

[36] CESR Standard No. 1 comprises 21 principles on how each EU member state should enforce the provision of financial information. In 2009, CESR released a review report on whether or not its principles were implemented. Because many of the principles in Standard No. 1 essentially became law with the TPD, we use this report to construct a variable that measures the extent to which a country enforces the provisions of the TPD (CESR 2009b).

audit firm, in each EU country.[37] We transform the continuous implementation proxies into binary partitioning variables splitting by the sample median.

The last partitioning variable, which applies to both the MAD and TPD, focuses on the resources that countries commit to enforcing the directives (see also Enriques and Gatti, 2008). We use *Staff Growth* as a proxy, and measure it as the percentage change in the number of full-time employees working for the supervisory authority in charge of securities regulation from 2004 to 2009.[38] Staff numbers are more readily available and easier to compare across countries than budgets. To make staff numbers comparable, we scale them by the number of publicly listed firms per country. In line with Jackson and Roe (2009), we assume that a higher rate of staff growth indicates stronger implementation and enforcement. We again create a binary partitioning variable splitting by the sample median.

5.2. Differential Liquidity Effects within the EU

In this section, we report findings on the heterogeneity in the treatment effects. For brevity, we tabulate only the coefficients (and *t*-statistics) for the main variables of interest from estimating Eq. (2) in Table 5. We begin each panel with splits for prior regulatory conditions for the MAD (Panel A) and the TPD (Panel B). Thereafter, we present splits using the directive-specific proxies for the way in which countries implement and enforce the directives. The table also reports *p*-values for the tests of differences in the coefficients across groups.

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[37] We code *Shift in Enforcement*$_{TPD}$ as '1' if the local enforcement authority indicated that it implemented a proactive comment and review process for compliance with accounting standards for the first time, and the audit firm replied that, according to their own assessment, a significant shift in the intensity of enforcing compliance with accounting standards occurred over the 2004 to 2009 period. We crosscheck the survey information with public sources (see also Christensen et al., 2013, Table 1 and Appendix A).

[38] Our principal source for full-time supervisory staff is the annual report of the local securities regulators or else the staff numbers reported in Central Banking Publications (2009). If neither of these sources provides staff numbers in a given year, we interpolate from adjacent years with available data. If possible, we use the growth of staff specifically assigned to the oversight of securities regulation. If the sources provide data only for a joint regulator (that also oversees banking and/or insurance), we allocate staff to securities regulation based on the relative (market capitalization) weights of the respective sectors. In countries with separate monitoring bodies to review financial statements (e.g., Germany, the U.K.), we include the staff growth of these bodies as well.

Using *Regulatory Quality* in 2003 to partition the EU countries, we find that the coefficients on *MAD* and *TPD* are negative and significant when prior regulatory quality is relatively strong. In countries with weaker prior regulation, the coefficients are insignificant and close to zero. For both the MAD and TPD, the liquidity effects are statistically different across the two groups. The results indicate that the liquidity effects are concentrated in countries with a stronger track record of implementing regulation in the past.[39] Consistent with this finding, the directives indeed led to significant changes even in countries with strong prior securities regulation (e.g., the U.K.).[40]

Next, we report results using the directive-specific implementation and enforcement variables. The tenor of the results is very similar across the various partitions. The MAD and the TPD coefficients are always negative and significant for the subset of EU countries that implement and enforce the directives relatively strongly. The coefficients are much smaller and, with one exception, not significant for countries with weak implementation. More specifically, the liquidity improvements are concentrated in countries that impose higher monetary fines for violations, confer more supervisory powers to local regulators, shifted to a stronger enforcement regime when the directives came into force, and allocated more resources to the supervisory authority.[41] The (two-sided) *p*-values for the differences in the coefficients between strong and weak implementation countries are often not significant at conventional levels, but the reason is likely power (see also our bootstrapping exercise described in Footnote 17, which suggests that the

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[39] We get similar, albeit sometimes slightly weaker results for several alternative proxies of prior regulatory quality: (i) the public enforcement index from La Porta et al. (2006), (ii) the rule of law index from Kaufmann et al. (2009), (iii) the anti-self-dealing index from Djankov et al. (2008), and (iv) an index measuring the quality of a country's auditing and reporting standards based on survey results from the Global Competitiveness Report (source: World Economic Forum).

[40] Post-directive reviews conducted by CESR confirm multiple changes to the oversight and enforcement procedures in the U.K. following the implementation of the MAD and the TPD (CESR 2009a, 2010).

[41] The results for MAD are also very similar when we partition countries based on the price manipulation index or the market manipulation index from Cumming et al. (2011), or the number of detected offenses in 2008 from Cumming et al. (2014). However, except for detected offences in 2008, these indices are available only for a subset of our sample countries.

tabulated standard errors are rather conservative). The relative coefficient magnitudes across all eight specifications clearly support the interpretation that the liquidity effects of the two EU directives are concentrated in countries with stronger implementation and enforcement.

An alternative explanation for the observed heterogeneity in the treatment effects is that the implementation of the directives in some countries creates spillover effects in other countries that have not yet implemented the directives, which in turn implies weaker effects for countries with later entry-into-force dates. The tests presented in Section 4.2 already mitigate this concern as they show that the liquidity effects occur relatively sharply around the entry-into-force dates. In untabulated tests, we find no evidence of liquidity effects prior to the entry-into-force dates for countries that adopt the MAD (TPD) late, which is inconsistent with the existence of spillover effects.[42] Moreover, Table B2 in the appendix shows that there is essentially no correlation between a variable splitting countries into 'early' and 'late' depending on their relative implementation timing and our partitioning variables for regulatory quality and implementation strength. Thus, implementation timing and spillover effects are unlikely to explain the heterogeneity in the treatment effects presented in Table 5. In addition, the *lack* of correlation between countries' relative implementation timing and the partitioning variables (which, as we show, indeed explain differences in the treatment effects) lends further support to our earlier conclusion that the entry-into-force dates are not selected with liquidity in mind.

5.3. Conditioning on Prior Regulation

To further examine the hysteresis and catching-up hypotheses, we next condition on prior regulatory conditions as well as implementation and enforcement of the two directives. We

[42] Without spillover effects, countries' relative implementation timing should have no implication for the (relative) magnitude of the liquidity effects across countries upon implementation. Our empirical specification accounts for general liquidity trends in a flexible manner. Thus, implementing the directives a quarter earlier or later should have no systematic effect on the liquidity effects.

combine the binary *Regulatory Quality 2003* indicator (high vs. low) with each of the seven implementation variables (strong vs. weak), and report results in Table 6. These two-way partitions sort the post-MAD and post-TPD observations into four distinct bins, for which we estimate the liquidity effects using essentially the same model as in Eq. (2). For instance, the coefficient estimate labeled *High RQ/Strong IS* represents the liquidity effect of the directives in countries with high quality prior regulation and strong implementation. *Low RQ/Weak IS* stands for the opposite end of the spectrum, i.e., countries with low prior regulatory quality and weak implementation of the new directives.

The table presents only the coefficient estimates and *t*-statistics of the four distinct groups of EU countries, but the model includes all controls and fixed effects from our base specification. The analyses indicate that countries with a strong track record of past regulation and a strict implementation and enforcement of the new directives have the largest liquidity improvements. The coefficients in the *High RQ/Strong IS* bins are always negative and significant. The effects for this sub-set of EU countries are statistically different from any other sub-set at the 10 percent level or better in all but two cases. The coefficient magnitudes going from the *High RQ/Strong IS* bin to the *Low RQ/Weak IS* bin are generally monotonically ranked. Moreover, differences in the liquidity effects for countries with strong versus weak implementation are generally larger and primarily significant in countries with high prior regulatory quality. Similarly, holding strong implementation fixed, the directives always have larger liquidity effects in countries with high prior regulatory quality than in countries with low regulatory quality.

The evidence does not support the catching-up hypothesis for countries with weaker prior conditions. Instead, liquidity differences widen across EU countries as a result of the directives. Thus, imposing the same regulation in countries with different prior regulatory conditions can result in countries drifting further apart, rather than moving them closer together. There appears to

be considerable hysteresis in regulatory outcomes. One explanation is that the same forces that limited the extent and effectiveness of securities regulation in the past are again at play when new rules are introduced. The findings suggest that history and countries' prior institutions matter greatly for regulatory outcomes and pose a major obstacle for regulatory harmonization.

6. Conclusion

In this paper, we examine the economic effects of securities regulation in capital markets. We focus on two key EU capital-market directives that tighten market abuse and transparency regulation. As there were prior EU directives and national laws banning insider trading and requiring financial reporting, the two directives essentially strengthen and harmonize existing EU securities regulation, particularly with respect to supervision and enforcement. We use this setting to estimate the causal effects of securities regulation on market liquidity for almost the entire population of publicly traded firms in a large number of European countries. In addition, we examine the role of cross-country differences in prior regulation, implementation, and enforcement for regulatory outcomes, holding the design of regulation fixed.

Our empirical identification strategy relies on variation in the effective dates across member states within a relatively narrow transposition window set by the EU. The staggered imposition alleviates common concerns in regulatory studies about concurrent but unrelated economic shocks and endogenous market responses around the introduction of new regulation. In addition, the variation in implementation dates is plausibly exogenous and unlikely to be a response to local conditions, considering that countries' legislative processes that transpose the directives into national law are time-consuming and inflexible. Thus, our setting affords better identification of the capital-market effects than prior studies, in particular, compared to those focusing on a single regulatory act in a single country.

We document that tightening insider trading and transparency regulation significantly increases the liquidity of firms' share markets. We conduct extensive sensitivity analyses, including within-country estimation, and show that the results are robust. We also perform several tests gauging the validity of our identification strategy and, in particular, the assumption that the effective dates are plausibly exogenous. Overall, our analysis shows that stronger securities regulation can have significant economic benefits by reducing information asymmetries and adverse selection in capital markets.

The second part of the paper presents evidence of substantial hysteresis in regulatory outcomes. The liquidity effects of the two directives are stronger in countries with a history of higher regulatory quality. The effects are also stronger in countries with stricter implementation and enforcement, but primarily when they also have exhibited strong regulatory quality in the past. One explanation for these findings is that countries that have put more resources into securities regulation and have a better track record of implementing and enforcing regulation are also more willing and better able to implement the new EU directives. Put differently, the same forces that limited the effectiveness of securities regulation in the past appear to be at work when new rules are introduced. These forces could span a wide range, including lack of institutional fit, resource constraints, inefficient bureaucracies, and political pressures, and our tests cannot distinguish which of these factors drives the results. However, the cross-sectional variation in the treatment effects is consistent with early work on the economics of regulation, highlighting the difficulties of implementing and enforcing regulation (e.g., Stigler, 1971; Posner, 1974; Peltzman, 1976; Becker, 1983), as well as the enforcement theory by Djankov et al. (2012).

In sum, our findings support a causal link between stricter securities regulation and market liquidity. They also support the notion that the success of regulation depends critically on how it

is implemented and enforced. Thus, implementation and enforcement issues warrant significant attention if regulation is to have the intended effects. Our finding that countries with weaker securities regulation do not catch up with stronger countries illustrates the difficulty of harmonizing capital markets through regulatory reforms. It highlights that prior regulatory conditions matter and that imposing the *same* regulation on countries with disparate initial conditions can have the unintended consequence of making countries diverge more, not less.

In closing, we highlight an important caveat about our study. While the results suggest substantial economic benefits from securities regulation, the analysis does not consider the costs of regulation. Thus, we cannot show that the directives are beneficial net of costs or that they are socially beneficial. For the same reason, our results also do not imply that countries with weaker implementation and enforcement of securities regulation "leave money on the table." We need more research to assess these issues and establish the welfare consequences.

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Appendix A: Overview of Market Abuse and Transparency Directives in the EU

In this appendix, we describe the institutional background on what motivated the enactment of the Market Abuse Directive (MAD) and the Transparency Directive (TPD) in the European Union (EU), discuss the legislative and administrative procedures associated with the implementation of EU directives, both in the European Parliament and the individual member states, and provide more details on the regulatory changes due to the two directives.

A.1. Financial Services Action Plan

In 1999, the EU initiated the Financial Services Action Plan (FSAP) because EU regulation was perceived to be insufficient and lagging behind the United States (Lamfalussy, 2000). In addition, the FSAP is a crucial part of the EU's attempt to create a single financial market. The FSAP's stated aims are to improve market confidence and eliminate capital-market fragmentation, and thereby to reduce the cost of raising capital on EU markets (FSAP 1999, p. 3).

The FSAP introduced 42 different measures, each with its own specific objective (CRA 2009). The four so-called Lamfalussy Directives form the core of the FSAP in the area of securities regulation: the MAD, the TPD, the Prospectus Directive (PROSP), and the Markets in Financial Instruments Directive (MiFID). Our empirical analysis focuses on the MAD and the TPD because they are conceptually most clearly related to secondary-market outcomes like liquidity. The PROSP pertains to securities offerings in the primary market and hence seems less relevant for our analysis of secondary-market liquidity. The MiFID is the last of the four Lamfalussy directives. It became effective in November 2007. Its main objective is to increase competition and consumer protection in investment services. Thus, like the PROSP, it seems less relevant to our analysis than the two directives that we have chosen.[43]

A.2. Legislative and Administrative Process in the EU and the Member States

Several features of the legislative and administrative process for EU directives make our setting well suited for our analysis and the identification of causal effects. We describe the main institutional features in

[43] Nevertheless, we control for the extent to which the other two Lamfalussy Directives influence our results for the MAD and TPD. See Model 4 in Table 2.

Section 3.1. Below, we provide additional details that describe the process and illustrate why the entry-into-force dates are likely exogenous for the purpose of our analysis.

EU directives are unusual in the sense that they require member states to implement certain rules but do not prescribe the means by which to implement the rules.[44] Moreover, as EU directives have to go through the EU institutions as well as the national parliaments of each member state, the legislative process from the initial proposal to the final effective date in the member states is lengthy. Directives do not always require new laws in the EU member states, but in the case of the MAD and TPD the changes were substantial enough to require at least one new law in each country. We first explain the administrative process at the EU level and then describe the enactment process in the member states.

A.2.1. Enactment at the Supranational Level of the EU

The EU has a bicameral legislature consisting of the European Parliament (elected directly by the citizens of the EU) and the Council of Ministers, comprising one minister from each member state. The specific minister depends on the topic under consideration, but the Council members always represent the national governments that they are part of. National governments also exert influence through the appointment of the European Commission, the EU's executive body, which proposes legislation, implements decisions, and is responsible for the day-to-day running of the EU. The heads of the EU member states initiated the FSAP-project in June 1998 at the Cardiff European Council.

Some FSAP directives, e.g., the Takeover Directive, were controversial and required extensive political negotiation ultimately delaying their adoption (see Hix et al. 2007). This was not the case for the MAD and the TPD. The MAD was passed as originally scheduled in the FSAP. The TPD was delayed by two years but for reasons unrelated to the directive itself. The European Parliament passed both directives without substantive amendments and with overwhelming majorities across party lines and nationalities. The European Parliament adopted the MAD on October 24, 2002 without a roll-call vote.[45] There was

[44] See Article 288 of the Treaty on the Functioning of the European Union.

[45] Only roll-call votes are recorded in the minutes of the meetings of the European Parliament. A roll-call vote requires a request from approximately a fifth of the parliament members. A lack of a roll-call vote indicates that there is little disagreement regarding a directive. See Hix et al. (2007) for details on votes in the European Parliament.

(only) one roll-call vote when the TPD was debated, and it was adopted by the European Parliament on March 30, 2004 with 380 members of the parliament voting yes, 8 voting no, and 102 abstaining. After the European Parliament has adopted the directives, they are sent to the European Council of Ministers for final approval and adoption.

For the purpose of identification, it is beneficial that the political decision to adopt the FSAP was made at the EU level and precedes our sample period by several years because that makes it unlikely that economic or political events that played into the adoption of the FSAP affect our analysis. Furthermore, the fact that the two directives were fairly uncontroversial makes it more likely that procedural features of countries' legislative systems rather than political considerations determined the timing of the directives' transposition into national law.

A.2.2. Transposition into National Law in EU Member States

When adopted by the EU, directives give member states a common deadline for transposition into national law. The transposition deadlines for the MAD and the TPD were October 12, 2004 and January 20, 2007, respectively, giving member states about two years for implementation. But as the EU's implementing (Level 2) directives came relatively late, especially for the TPD (December 2003 and March 2007, respectively), the implementation window was closer to three years.

The implementation process in the member states follows the national rules of enacting laws, generally laid out in countries' constitutions, and hence is fairly inflexible. It typically involves drafting the national law(s) required to accomplish the result mandated by the directive, a consultation period, multiple readings in the various chambers of parliament, signing by the head of state, and public notification. As a result, the process is also lengthy. To illustrate this point, Table A1 describes the time span for the enactment of the MAD and the TPD in five large EU countries. For the two directives, it takes on average about 13 months from the public release of the initial draft of the new law to its entry-into-force date. This time span understates the true length of the legislative process because we observe only when governments publish the initial draft but not when they initiate the process and start drafting the law. Thus, the national legislative processes typically start more than a year before the eventual entry-into-force dates. Given the

length of this entire process, the implementation window is narrow. Consistent with this notion, the standard deviation of the time span between countries' entry-into-force dates and the transposition deadlines is only 2.5 (3) quarters for the MAD (TPD).[46] Considering the length and inflexibility of the legislative process, it is unlikely that legislators could respond (quickly) to local conditions (e.g., adverse changes to market liquidity) by implementing the directives. These features of the setting considerably mitigate concerns about reverse causality and endogenous implementation timing.

Furthermore, as directives have to be implemented by virtue of being member of the EU, the transposition of directives into national law is rarely controversial. At this stage, the main political decisions have already been made. Political battles over a directive have to occur earlier, at the EU level. For this reason, national parliaments monitor the legislative initiatives and processes of the EU and authorize the national governments to exert their influence through the Council of Ministers.[47] That said, member states have some flexibility in how they implement the directives, e.g., with respect to the penalties they impose or the resources they devote. We analyze this variation in Section 5.

A.3. Changes in Securities Regulation due the MAD and the TPD

A.3.1. Market Abuse Directive (MAD)

The MAD aims to prevent insider trading and market manipulation. It contains three key elements: (i) disclosure rules designed to reduce the scope of inside information, (ii) ex-post sanctions for insider trading or market manipulation, and (iii) tightened enforcement of compliance with insider trading and market manipulation rules. The core disclosure rule in the MAD requires issuers of financial instruments to inform the public as soon as possible of inside information (Article 6). Moreover, executives must disclose the transactions in the securities of the firm they manage in a quick and transparent manner.

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[46] Moreover, as shown in Panel A of Table 1, only a few member states transposed the MAD and the TPD before the respective deadline. A small delay is common practice in the EU, owing to lengthy national processes. On average, FSAP directives became effective 1.7 quarters after their respective transposition deadlines. In this sense, the transposition deadline is somewhat flexible but delay is limited. If a directive is not implemented, the Commission (or other member states) can eventually bring a case to the European Court of Justice, which can impose financial penalties. For the MAD and the TPD, no such penalties were applied.

[47] For instance, in the U.K., the House of Lords and the House of Commons both have European Scrutiny Committees. The two committees approved an explanatory memorandum on the MAD in December 2001 and March 2002, respectively, which is well ahead of the directive's passage at the EU level.

The MAD also aims to harmonize sanctions for the violation of insider trading rules across EU member states. However, the requirement is generic, and it is the member states, not the EU, that set penalties. For instance, Article 14 states that "member states shall ensure, in conformity with their national law, that the appropriate administrative measures can be taken or administrative sanctions be imposed against the persons responsible where the provisions adopted in the implementation of this Directive have not been complied with."

With respect to enforcement, the MAD requires member states to designate a single authority with the competence of ensuring the application of the insider trading provisions (Article 11). The MAD further prescribes a number of specific powers for the authority, for instance, the right to carry out on-site inspections and to demand information from any person. However, apart from a generic statement in Article 12, that "the competent authority shall be given all supervisory and investigatory powers necessary for the exercise of its functions," the directive is silent on the resources necessary for the authority to fulfill its task, again giving significant discretion to the member states.

The MAD replaced an older directive from 1989 banning insider trading. Thus, generally speaking, the MAD should be viewed as improving and tightening existing insider trading regulation in the EU, particularly with respect to enforcement. According to the British Institute of International and Comparative Law (2005), EU member states followed the directive's exact language closely when implementing it into national regulation. It led to many substantial changes in all countries' market abuse regimes (see CESR, 2007, 2008, and 2010 for detailed descriptions of the changes in each country). For example, many countries increased the powers of the national supervisory authority to seize and retain documents and data that may further an investigation into insider trading.

A.3.2. Transparency Directive (TPD)

The TPD aims to ensure transparency for investors through a regular flow of information. It uses two regulatory tools to improve transparency: (i) a set of disclosure requirements, and (ii) tightened enforcement of compliance with the disclosure provisions. The TPD includes provisions for ongoing disclosures (e.g., the filing of annual and semi-annual reports in accordance with International Financial

Reporting Standards, IFRS) and requirements that ensure the disclosure of significant events (e.g., significant holdings by shareholders). However, IFRS reporting was already mandated by older EU regulation (Regulation No. 1606/2002) and most exchanges already required the filing of semi-annual reports and the disclosure of significant events.[48] Hence, the TPD did not significantly expand existing disclosure requirements. Yet, it stipulated major changes to the supervisory regime and the enforcement of corporate reporting and disclosure rules. Thus, it should be viewed as improving and tightening existing transparency regulation. The TPD required legislative changes in all EU member states.

To mention a few specific changes, the TPD requires each member state to designate a competent supervisory authority.[49] This authority is in charge of monitoring compliance with the reporting and disclosure requirements set out in the directive and it must be given appropriate powers to enforce these requirements. Similar to the MAD, the TPD stipulates that the authority is given certain supervisory powers (Article 24). It must examine and monitor required disclosures and, if infringements are discovered, take appropriate action (e.g., issue a fine). Moreover, when investigating compliance, the authority must be able to request information from auditors and shareholders, and to carry out on-site inspections. The requirement to monitor and enforce compliance with existing disclosure rules represents a significant change because most member states had no, or very limited, monitoring and enforcement of corporate disclosures by a securities regulator prior to the TPD.

In addition, the TPD increases access to regulated information. The directive requires member states to set up an Officially Appointed Mechanism (OAM) in which regulated information is centrally stored and through which investors can access the information fast and free of charge (Article 21). In practice, the member states have produced online databases that allow the public to search for all required information, similar to the EDGAR database set up by the Securities and Exchange Committee in the U.S. Such central repositories are expected to aid access to and the dissemination of financial information.

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48 IFRS is a set of accounting standards that were adopted by all EU member states (and other countries around the world) as of 2005.

49 For instance, in Sweden, the supervision and enforcement of periodic financial reporting requirements was transferred from the Swedish stock exchange to the national supervisory authority (Finansinspektionen), which also received better means of imposing sanctions (CESR, 2009a). For further details and examples, see CESR (2009a) and Mazars (2010).

Appendix B: Additional Sensitivity Tests

In this appendix, we report a series of robustness checks for the market-liquidity effects presented in Section 4.[50] We gauge the sensitivity of our findings to (i) the clustering of the standard errors; (ii) the choice of the fixed-effects structure as well as shocks that differentially affect firms and countries; (iii) the sample composition; (iv) the use of share turnover and market value as control variables; and (v) the construction and use of the liquidity factor. Furthermore, we test for the existence of spillover effects from countries that implement the directives earlier.

First, we consider alternative ways to cluster the standard errors. In our main specification, we use clusters by country. As the first two rows under heading (1) in Table B1 illustrate, the inferences remain the same when we use two-way clustering by country and quarter, or when we cluster the standard errors by 18 economic regions. The latter approach combines several EU countries and in this sense is more conservative than country-level clustering.

Second, we expand the fixed-effects structure of our empirical specification (three rows under the second heading in Table B1). When we replace the country- and industry-fixed effects with firm-fixed effects, the results are very similar, although the magnitude (but not significance) of the TPD effect is slightly attenuated. Next, we augment the current base model by adding separate quarter-year fixed effects for developed countries (as identified in the Morgan Stanley Capital International database). This specification accounts for the possibility that developed markets exhibit different liquidity trends or are differentially affected by economic shocks during the sample period. Along the same lines, we introduce separate size coefficients for *each* quarter. This expansion of the fixed-effects structure should help absorb economic shocks that affect larger firms differently from smaller firms, and hence accounts for differences in the firm size distribution across countries. In both cases, the results are similar and the inferences remain the same as those in our main analysis, although the TPD effect is slightly attenuated with separate quarter-

[50] We also conduct several robustness checks for the tests in Section 5. We re-run the cross-sectional analyses (i) with standard errors clustered by economic region instead of country, (ii) adding separate quarter-year-fixed effects for developed markets, (iii) replacing country- and industry-fixed effects with firm-fixed effects, or (iv) controlling for macroeconomic factors. The results and inferences are similar to those in Tables 5 and 6.

year fixed effects for developed countries. In additional tests (not tabulated), we include separate volatility coefficients for each quarter, interactions between the country indicators and firm size, or interactions between the country indicators and industry. Again, the results are similar to those reported in Table 2. Hence we conclude that it is unlikely that the findings are driven by differences in the composition of firms across countries or by economic shocks to particular groups of firms.

Under the third heading in Table B1, we assess the importance of sample composition. First, we exclude the countries that experienced the most serious economic downturn during the financial crisis. Specifically, we exclude firms from the so-called GIIPS countries (Greece, Ireland, Italy, Portugal, and Spain) as well as Cyprus from the sample. As shown, the results are not materially affected. Second, we report results from analyses including a benchmark sample of observations from non-EU countries. The idea is to also use firms that are unaffected by the introduction of the MAD and TPD as a benchmark instead of relying on the variation in entry-into-force dates. This identification strategy assumes that EU and non-EU countries would have the same liquidity trends absent MAD and TPD, which is a strong assumption and the reason why we do not use this identification strategy in our main analyses. Despite this weakness, the results are quite similar.

Under the fourth heading in Table B1, we consider the importance of the control variables. First, we re-estimate our model after excluding *Share Turnover* from the set of controls. The motivation for this robustness check is that turnover is conceptually close to some of the variables that enter the liquidity factor and hence it could be viewed as a proxy for liquidity as well. Again, the results are very similar and the inferences remain unaffected. Second, we replace the *Market Value* measured rolling in quarter t–4 with a fixed control variable using the market value as of 2001 (i.e., the beginning of our sample period) or whenever a firm enters the sample for the first time. The motivation for this robustness test is that liquidity is likely being priced in firms' market values and that the directives could have increased firm valuation ahead of the entry-into-force dates. For this reason, our main analyses use lagged market value. As a robustness check, we fix the market value prior to the directives, so that we still control for size but make

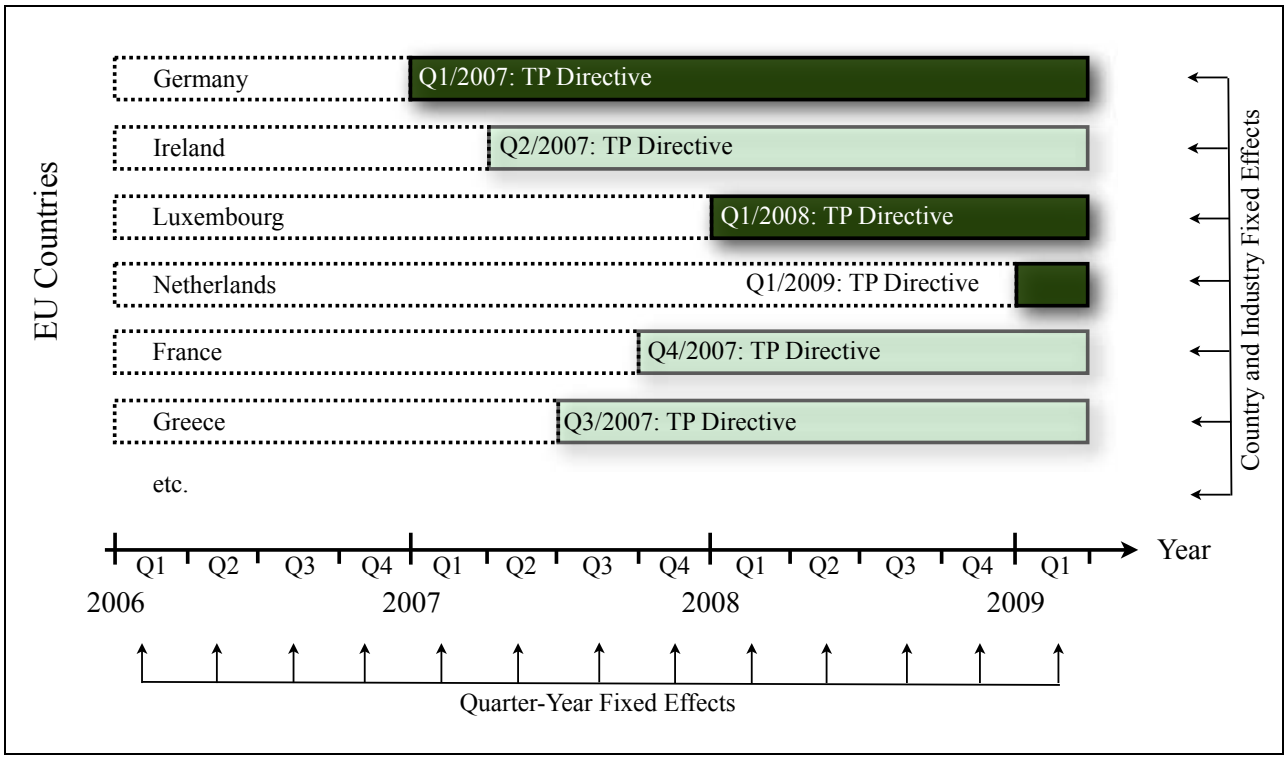
sure that the effects of the directives are not yet reflected. As shown in Table B1, the results remain largely unchanged.

In untabulated tests, we also consider alternative ways to construct of the liquidity factor. Inferences are unaffected when (i) we re-estimate the liquidity factor using median quarterly spreads and price impact (instead of means), (ii) with promax or varimax rotation in the factor analysis, (iii) using a winsorized liquidity factor without taking the natural logarithm, or (iv) when we construct the liquidity factor based on three liquidity proxies whenever one is missing to enlarge the sample. Furthermore, as tabulated in an earlier version of the paper, we obtain similar results and the inferences remain the same when using either bid-ask spreads or zero returns as outcome variables, rather than the liquidity factor. Spreads and zero returns can be viewed as proxies on opposite ends of a spectrum: Bid-ask spreads are conceptually appealing but more sensitive to market-micro structure differences and not as widely available. Zero returns are conceptually less appealing but less sensitive to micro-structure differences and more widely available. Thus, it is comforting that both proxies yield similar results.

Finally, we examine whether the differential timing of the entry-into-force dates of the MAD and TPD across EU member states creates spillover effects from early countries to late countries. Such spillover effects could in turn induce heterogeneity in the treatment effects. That is, if markets *throughout the EU* reacted at the time the first set of countries implements the directive, then the liquidity effects would be weaker or non-existent by the time the directive becomes effective in countries with later dates. In this case, the heterogeneity in the liquidity effects is an artifact of differential timing, rather than of differences in implementation or enforcement. We perform two analyses to address this concern. First, we examine the relation between implementation timing and our prior regulation and implementation strength proxies. To do so, we partition EU countries into early and late MAD/TPD adopters using the median entry-into-force date as cut-off value and provide simple contingency tables with Chi Square tests. Table B2 shows that the early countries are not necessarily the countries with a stricter implementation (or vice versa). In fact, the observations are quite evenly distributed and the Chi Square tests are never significant. As discussed earlier in the paper, this evidence also supports the idea that the entry-into-force dates and hence

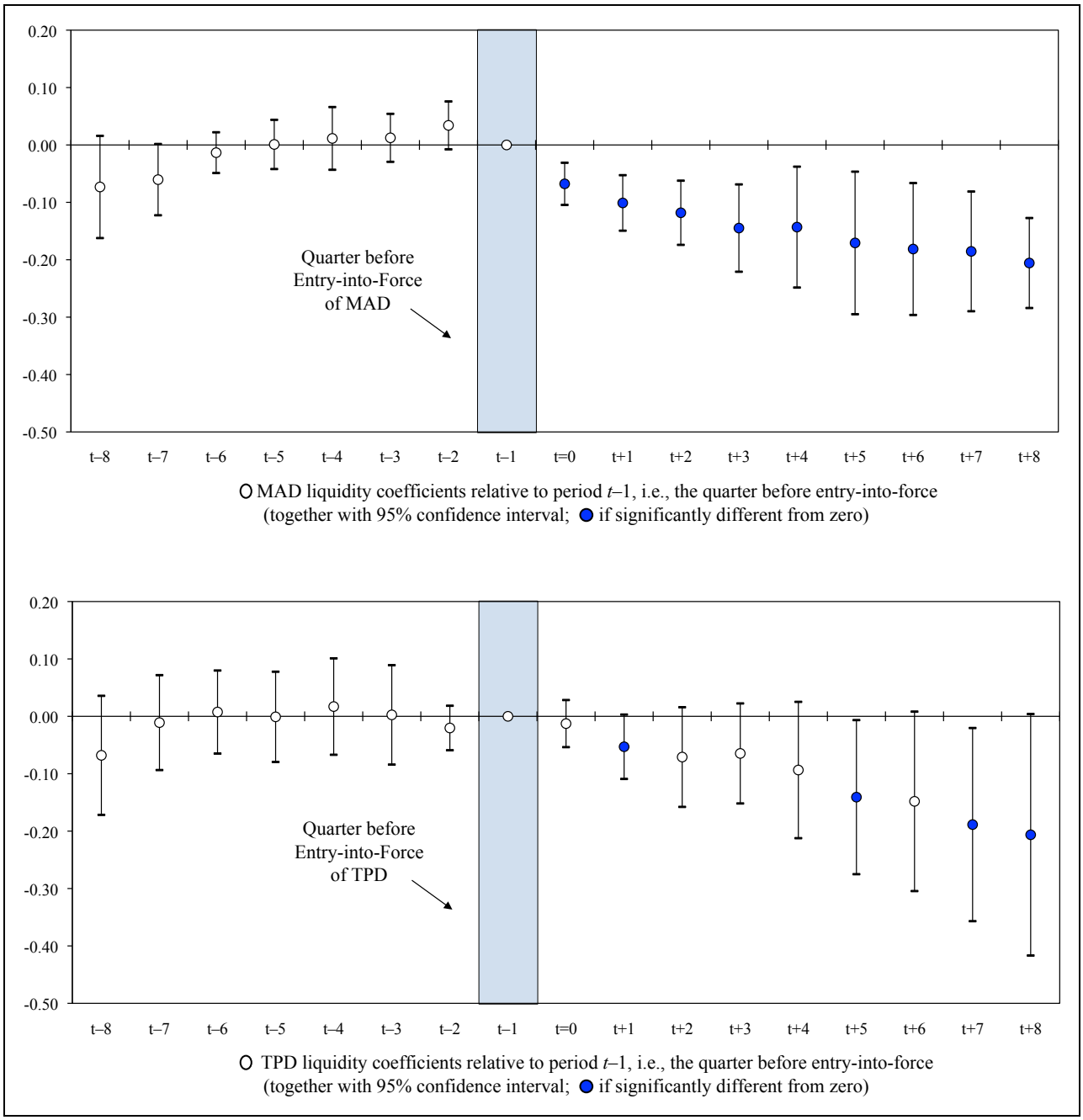
the relative timing across countries is plausibly exogenous. Second, we examine the liquidity patterns around the entry-into-force dates, separately for early and late countries (not tabulated). We find no evidence that late countries experience liquidity changes prior to the entry-into-force dates, which is inconsistent with spillover effects from earlier countries to later countries. Thus, it is unlikely that our cross-sectional results showing substantial heterogeneity in the treatment effects are driven merely by differential timing.

Figure 1: Illustrating the Identification Strategy for the Capital-Market Effects of the Transparency Directive



The figure illustrates our identification strategy using the Transparency Directive as an example. The sample comprises the EU member states. For each country, we switch the TPD indicator variable from '0' to '1' in the quarter when the directive comes into force. Thereafter, the indicator remains at '1'. The entry-into-force dates vary across EU member states. This variation allows us to introduce fixed effects for each country (and industry) as well as for every calendar quarter over the sample period. The latter implies that the model includes a flexible quarterly time trend. The different shadings of the countries after the directive becomes effective illustrates that we also exploit cross-sectional differences in the way countries implement and enforce the directives.

Figure 2: Liquidity Patterns around the Entry-Into-Force Dates of the MAD and TPD (in Event Time)



The figure maps out the liquidity patterns around the entry-into-force dates of the Market Abuse Directive (*MAD)* and the Transparency Directive (*TPD*) in event time. We estimate the model in Eq. (1) but replace the *MAD* or *TPD* coefficient with 16 separate indicator variables, each marking one quarter over the *t*–8 to *t*+8 period relative to the quarter *before* the entry-into-force of the respective directive (*t*–1). We omit the indicator for period *t*–1. It therefore serves as benchmark for all the other periods, and has a coefficient value of zero (and no confidence interval). The figure plots the coefficient estimates of the 16 quarters together with their 95% confidence intervals for *MAD* (upper panel) and *TPD* (lower panel). To balance the sample through time and keep the sample composition constant for each estimated coefficient, we use all firm-quarter observations from the EU treatment countries over the *t*–8 to *t*+8 period, and require at least one observation per country and quarter. The dependent variable, *Liquidity Factor*, is an aggregate measure of liquidity and represents the scores of a single factor extracted from four individual liquidity proxies (bid-ask spreads, zero returns, price impact, and total trading costs) using factor analysis. We include the control variables and fixed effects in the estimation, but not the other EU directives (see Models 1 and 2 in Table 2).

Table 1: Sample Composition and Descriptive Statistics

Panel A: Sample Composition and Entry-Into-Force Dates of the MAD and TPD

Country	*Observations (N)*	*MAD Entry-into-Force Dates*	*TPD Entry-into-Force Dates*
Austria	1,343	Jan-05	Apr-07
Belgium	4,145	Sep-05	Sep-08
Bulgaria	305	Jan-07	Jan-07
Cyprus	1,329	Sep-05	Mar-08
Czech Republic	162	Feb-06	Aug-09
Denmark	5,437	Apr-05	Jun-07
Estonia	286	Mar-05	Dec-07
Finland	4,455	Jul-05	Feb-07
France	19,461	Jul-05	Dec-07
Germany	9,018	Oct-04	Jan-07
Greece	1,243	Jul-05	Jul-07
Hungary	789	Jul-05	Dec-07
Iceland	91	Jul-05	Nov-07
Ireland	772	Jul-05	Jun-07
Italy	9,038	May-05	Apr-09
Latvia	285	Jul-05	Apr-07
Lithuania	530	Apr-04	Feb-07
Luxembourg	5	May-06	Jan-08
Malta	n.a.	Apr-05	Oct-07
The Netherlands	4,039	Oct-05	Jan-09
Norway	5,422	Sep-05	Jan-08
Poland	7,455	Oct-05	Mar-09
Portugal	1,528	Apr-06	Nov-07
Romania	346	Jan-07	Jan-07
Slovakia	59	Jan-05	May-07
Slovenia	401	Aug-04	Sep-07
Spain	3,849	Nov-05	Dec-07
Sweden	9,658	Jul-05	Jul-07
United Kingdom	21,460	Jul-05	Jan-07

(continued)

Table 1 (continued)

Panel B: Descriptive Statistics for Variables Used in the Liquidity Regressions

(N = 112,260)	*Mean*	*Std. Dev.*	*P1*	*P25*	*Median*	*P75*	*P99*
Dependent Variables:							
Bid-Ask Spread$_t$	0.027	0.031	0.001	0.008	0.017	0.033	0.156
Zero Returns$_t$	0.240	0.226	0.000	0.062	0.156	0.379	0.800
Price Impact$_t$	4.976	14.942	0.000	0.040	0.387	2.600	82.720
Total Trading Costs$_t$	0.039	0.039	0.010	0.013	0.025	0.049	0.191
Liquidity Factor$_t$	-0.054	0.799	-0.748	-0.594	-0.336	0.180	2.999
Independent Variables:							
Market Value$_{t-4}$	828	2,733	3	41	144	556	11,324
Share Turnover$_{t-4}$	0.001	0.002	0.000	0.000	0.001	0.002	0.011
Return Variability$_{t-4}$	0.024	0.012	0.007	0.016	0.022	0.031	0.061
GDP per Capita$_{t-4}$	23.647	7.780	4.600	21.775	24.323	28.146	41.400

Panel C: Pearson's Correlation Coefficients Between Variables Used in the Liquidity Regressions

(N = 112,260)	*Zero Returns*	*Price Impact*	*Total Trad-ing Costs*	*Liquidity Factor*	*Market Value*	*Share Turnover*	*Return Variability*	*GDP per Capita*
Bid-Ask Spread	0.589	0.584	0.792	0.893	-0.189	-0.199	0.291	0.060
Zero Returns		0.371	0.704	0.774	-0.231	-0.306	0.029	0.069
Price Impact			0.586	0.683	-0.095	-0.138	0.264	-0.102
Total Trading Costs				0.964	-0.186	-0.167	0.307	0.083
Liquidity Factor					-0.208	-0.220	0.287	0.059
Market Value						0.176	-0.107	0.002
Share Turnover							0.157	0.084
Return Variability								0.006

The sample in the empirical tests consists of all countries in the European Union (EU) except for Bulgaria and Romania, which did not join the EU until 2007, and Malta, for which we do not have the necessary liquidity data. We also include Iceland and Norway from the European Economic Area (EEA), as they agreed to adopt the EU capital market directives in their entirety. We have 112,260 firm-quarter observations beginning in the first quarter of 2001 through the second quarter of 2011 with financial data in Worldscope and price/volume data in Datastream. The table presents the number of observations per country and the calendar months when the Market Abuse Directive (*MAD)* and the Transparency Directive (*TPD*) came into force (Panel A), as well as distributional characteristics (Panel B) and Pearson's correlation coefficients (Panel C) for the variables used in the regression analyses. The five dependent variables are: (1) The *Bid-Ask Spread* is the quarterly mean quoted spread (i.e., difference between the bid and ask price divided by the mid-point and measured at the end of each trading day). (2) *Zero Returns* is the proportion of trading days with zero daily stock returns out of all potential trading days in a given quarter. (3) *Price Impact* is the quarterly mean of the Amihud (2002) illiquidity measure (i.e., daily absolute stock return divided by US$ trading volume). (4) *Total Trading Costs* is a quarterly estimate of total round-trip transaction costs (i.e., bid-ask spreads, commissions as well as implicit costs such as short-sale constraints or taxes) inferred from the time-series of daily security and aggregate market returns, as developed by Lesmond, Ogden, and Trzcinka (1999). (5) The *Liquidity Factor* is an aggregate liquidity measure and represents the scores of the single factor obtained employing factor analysis with the four liquidity variables. The continuous independent variables consist of the following measures: *Market Value* is stock price times the number of shares outstanding (in US$ million) measured at the end of the quarter. *Share Turnover* is the quarterly mean of the daily turnover (i.e., US$ trading volume divided by the market value at the end of each trading day). We compute *Return Variability* as the standard deviation of daily stock returns in a given quarter. Annual *GDP per Capita* is from the World Bank (in constant US$ as of 2000). All correlation coefficients are significant at the 1% level (except for *GDP per Capita* and the correlation between *Market Value* and *Return Variability*). All variables (except *GDP per Capita*) are truncated at the 1st and 99th percentile. The subscript *t* indicates the calendar quarter of variable measurement.

Table 2: Liquidity Effects from Tighter EU Securities Regulation

	Ln(Liquidity Factor +1) as Dependent Variable			
(N = 112,260)	*Market Abuse Directive* (1)	*Transparency Directive* (2)	*Both Directives Combined* (3)	*Plus Other Directives* (4)
Test Variables:				
MAD	-0.111***	–	-0.116***	-0.115***
	(-3.73)		(-3.70)	(-3.61)
TPD	–	-0.085*	-0.087*	-0.093**
		(-1.84)	(-1.87)	(-2.20)
Control Variables:				
Ln(Market Value$_{t-4}$)	-0.250***	-0.250***	-0.250***	-0.248***
	(-13.47)	(-13.52)	(-13.52)	(-13.48)
Ln(Share Turnover$_{t-4}$)	-0.155***	-0.155***	-0.155***	-0.155***
	(-18.92)	(-19.18)	(-19.11)	(-18.63)
Ln(Return Variability$_{t-4}$)	0.172***	0.172***	0.172***	0.172***
	(4.60)	(4.61)	(4.63)	(4.67)
Ln(GDP per Capita$_{t-4}$)	-0.053	-0.019	-0.013	-0.046
	(-0.09)	(-0.03)	(-0.02)	(-0.08)
MiFID	–	–	–	0.033
				(0.51)
PROSP	–	–	–	-0.013
				(-0.56)
TAKEOVER	–	–	–	0.003
				(0.20)
IFRS	–	–	–	-0.101**
				(-2.33)
Fixed Effects:				
Country	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
Quarter-Year	Yes	Yes	Yes	Yes
R-squared	0.658	0.658	0.658	0.659

The sample comprises firm-quarter observations from 26 EU countries over the 2001 to 2011 period. We report results for the *Liquidity Factor* equal to the factor scores extracted from the four individual measures bid-ask spreads, zero returns, price impact, and total trading costs using factor analysis. *MAD* and *TPD* are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive or the Transparency Directive came into force. For further details on the sample and a description of the control variables see Table 1. In Model 4 we also include binary indicator variables for other regulatory changes in the EU, i.e., the Markets in Financial Instruments Directive (*MiFID*), the Prospectus Directive (*PROSP*), the Takeover Directive (*TAKEOVER*), and the mandatory adoption of International Financial Reporting Standards (*IFRS*). For *MiFID*, *PROSP*, and *TAKEOVER* we set the indicator variables to '1' beginning in the respective entry-into-force quarter. *IFRS* accounts for variation in firms' reporting periods and takes on the value of '1' beginning in the calendar quarter immediately following a firm's first fiscal-year end with mandatory IFRS reporting. We identify firms' accounting policy based on the "accounting standards followed" field in Worldscope (field 07536). If indicated, we use the natural log of the raw values (plus one), and lag the variables by four quarters. We include country-, Campbell (1996) industry-, and quarter-year-fixed effects in the regressions, but do not report the coefficients. The table reports OLS coefficient estimates and (in parentheses) t-statistics based on robust standard errors that are clustered by country. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 3: Assessing Identification of the Liquidity Effects from Tighter EU Securities Regulation

Panel A: Analysis of the Liquidity Effects in the Year Leading Up to the MAD and TPD

(N = 112,260)	*Ln(Liquidity Factor +1) as Dependent Variable*			
	Market Abuse Directive		*Transparency Directive*	
	Coefficient	*t-stat*	*Coefficient*	*t-stat*
Period Relative to Entry-into-Force Date (t=0):				
Year Leading Up to MAD (*t*–4 to *t*–1)	0.010	(0.39)	–	–
MAD (*t*=0 and onwards)	-0.104*	(-1.90)	-0.115***	(-3.63)
Year Leading Up to TPD (*t*–4 to *t*–1)	–	–	-0.010	(-0.59)
TPD (*t*=0 and onwards)	-0.093**	(-2.18)	-0.103**	(-2.51)
F-test for Differences across Coefficients (p-value):				
(*t*–4 to *t*–1) = MAD or TPD	0.002		0.037	
Control Variables	Yes		Yes	
Fixed Effects	Yes		Yes	

Panel B: (Counterfactually) Varying the Entry-into-Force Dates of the MAD and TPD

(N = 112,260)	*Ln(Liquidity Factor +1) as Dependent Variable*			
	Market Abuse Directive		*Transparency Directive*	
	Coefficient	*t-stat*	*Coefficient*	*t-stat*
Shifting of Entry-into-Force Dates Relative to t=0:				
t–6	0.039	(1.21)	0.013	(0.46)
t–5	0.032	(1.04)	-0.003	(-0.16)
t–4	0.016	(0.65)	-0.012	(-0.69)
t–3	-0.005	(-0.27)	-0.034**	(-2.11)
t–2	-0.020	(-1.01)	-0.054**	(-2.04)
t–1	-0.080***	(-3.64)	-0.060*	(-1.75)
t=0 ('True' Entry-into-Force Date)	-0.115***	(-3.61)	-0.093**	(-2.20)
t+1	-0.103**	(-2.73)	-0.121**	(-2.32)
t+2	-0.083**	(-2.13)	-0.130**	(-2.12)
t+3	-0.064*	(-1.69)	-0.137**	(-2.03)
t+4	-0.050	(-1.38)	-0.154**	(-2.11)
t+5	-0.056	(-1.94)	-0.165**	(-2.07)
t+6	-0.040	(-1.47)	-0.165*	(-1.93)
Control Variables	Yes		Yes	
Fixed Effects	Yes		Yes	

(continued)

Table 3 (continued)

Panel C: Controlling for Various Time Trends and Unobservable Factors

Ln(Liquidity Factor +1) as Dependent Variable	*Controlling for Time Trends*		*Within-Country Estimation (Germany, Ireland, U.K.)*		*Falsification Test*
	Including Lagged Changes in Liquidity	*Including Linear and Quadratic Time Trend Variables*	*Regulated & Unregulated Markets (All Firms)*	*Regulated & Unregulated Markets (Matched & Balanced Panel)*	*Selection on Observables (First Stage Prediction Model)*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Test variables:					
MAD	-0.128***	-0.091***	–	–	-0.011
	(-4.14)	(-3.75)			(-1.42)
TPD	-0.104**	-0.058*	–	–	-0.011
	(-2.19)	(-1.91)			(-1.04)
$MAD_{\text{Regulated vs. Unregulated Firms}}$	–	–	-0.127***	-0.096***	–
			(-5.54)	(-3.21)	
$TPD_{\text{Regulated vs. Unregulated Firms}}$	–	–	-0.121***	-0.071***	–
			(-5.03)	(-2.58)	
Control Variables:					
From Base Specification	Yes	Yes	Yes	Yes	Yes
Δ Ln(Liquidity Factor +1)$_{t-8 \text{ to } t-4}$	Yes	–	–	–	–
Country*Time Trend	–	Yes	–	–	–
Country*Time Trend^2	–	Yes	–	–	–
Fixed Effects:					
Country	Yes	Yes	–	–	Yes
Industry	Yes	Yes	Yes	Yes	Yes
Quarter-Year	Yes	Yes	–	–	Yes
Country-Quarter	–	–	Yes	Yes	–
Unregulated Market Indicators	–	–	Yes	Yes	–
N	67,735	112,260	53,736	12,684	112,260

(continued)

Table 3 (continued)

The sample comprises firm-quarter observations from 26 EU countries over the 2001 to 2011 period. We report results for the *Liquidity Factor* as dependent variable. *MAD* and *TPD* are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force. For a description of the control variables see Table 1. If not stated differently, we use Model 4 in Table 2 as our base specification. In Panel A, we include a separate indicator variable marking the four quarters leading up to the entry-into-force quarter of the MAD or TPD (i.e., the period t–4 to t–1 relative to the entry-into-force date in t=0). We also report p-values from Wald tests assessing the statistical significance of the differences in the coefficients across periods. In Panel B, we report the *MAD* or *TPD* coefficients from 13 separate regressions. For each regression we counterfactually shift the 'true' MAD or TPD entry-into-force dates (t=0) to a different quarter. That is, we set the binary MAD or TPD indicator variables equal to '1' beginning in each quarter from t–6 to t+6 relative to the 'true' entry-into-force date. In Panel C, we estimate the following specifications: (1) we include lagged seasonal changes (Δ) in the liquidity factor as control. (2) We include a linear and quadratic time trend variable, and interact each trend variable with the country fixed effects to allow for country-specific variation. (3) We estimate within-country regressions. Doing so, we add firms trading on unregulated EU markets to the sample as the two directives do not (or to a lesser extent) apply to these firms and hence they can serve as a within-country benchmark. We perform this analysis for the three sample countries with the largest unregulated markets, i.e., Germany (Open Market), Ireland (Enterprise Securities Market), and the U.K. (Alternative Investment Market AIM). We include a separate *Unregulated Market Indicator* for each market (equal to '1' for unregulated market firms). We further replace the country and calendar quarter fixed effects with separate quarter-year fixed effects per country. This structure also absorbs the indicators for the other EU directives included in the base model (e.g., *MiFID*). The *MAD* and *TPD* coefficients represent the incremental effects of regulated firms relative to unregulated firms (as indicated by the subscripts). In (4), we repeat (3) but use a propensity-matched and balanced sample. We conduct the matching in the quarter before MAD came into force and use the following firm characteristics as matching criteria: total assets, return on assets, book-to-market, asset growth, quarterly stock returns, and the annual standard deviation of daily stock returns. We further ensure that, in each country, the number of regulated and unregulated firms is the same, and require that firms are in the sample before and after MAD and TPD came into force. (5) We provide a falsification test in the spirit of Altonji et al. (2005) to gauge the effect of selection on observables and endogenous timing on our results. We first estimate Model 3 in Table 2, but exclude the variables of interest (*MAD* and *TPD*) and instead add several variables potentially capturing forces and local conditions to which lawmakers might respond when implementing the two directives. Specifically, we include gross domestic savings, the net capital account, portfolio equity inflows, annual growth in GDP per capita, yearly inflation, and five binary indicator variables for the closeness to national elections in a country (for quarters t–4 to t=0 relative to the election quarter). We scale the raw variables by GDP and lag them by four quarters. Macroeconomic data are from the World Bank and the IMF, and we collect election dates from national archives. We then use the predicted values from this first-stage regression as the dependent variable in our base specification and report the *MAD* and *TPD* coefficients from this falsification test in the table. Insignificant coefficient estimates indicate that local conditions and forces to which lawmakers might respond cannot explain the estimated effects obtained in Table 2. Throughout the table, we include the full set of control variables and fixed effects in the models (see Model 4 in Table 2), but only report OLS coefficient estimates (t-statistics) for the main variables. We compute t-statistics based on robust standard errors clustered by country, except in Models 3 and 4 in which we apply two-way clustering by firm and quarter-year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 4: Prior Regulation and Implementation Variables by EU Country

	Prior Regulation		*MAD Implementation Strength*				*TPD Implementation Strength*				*Supervisory Resources*	
Country	*Regulatory Quality 2003*		*Maximum Fine$_{MAD}$ (EUR 000)*		*Supervisory Powers$_{MAD}$*	*Shift in Enforcement$_{MAD}$*	*Maximum Fine$_{TPD}$ (EUR 000)*		*Supervisory Powers$_{TPD}$*	*Shift in Enforcement$_{TPD}$*	*Staff Growth (2004 to 2009)*	
Austria	1.52	(1)	No fine	(0)	70 (0)	1	30	(0)	0	0	2.08	(1)
Belgium	1.36	(1)	Profit-based	(1)	69 (0)	0	2,500	(1)	1	0	-0.27	(0)
Bulgaria	0.59	(0)	50	(0)	69 (0)	0	5,112	(1)	0	0	0.13	(0)
Cyprus	1.20	(0)	1,710	(0)	68 (0)	0	341	(0)	0	0	0.07	(0)
Czech Republic	1.12	(0)	350	(0)	64 (0)	0	400	(0)	0	0	0.93	(1)
Denmark	1.79	(1)	No fine	(0)	60 (0)	0	No limit	(1)	1	0	0.23	(0)
Estonia	1.40	(1)	No fine	(0)	60 (0)	1	16,000	(1)	0	0	-0.01	(0)
Finland	1.90	(1)	200	(0)	63 (0)	0	200	(0)	1	1	-0.20	(0)
France	1.18	(0)	Profit-based	(1)	75 (1)	1	10,000	(1)	1	0	0.06	(0)
Germany	1.51	(1)	Profit-based	(1)	64 (0)	1	200	(0)	0	1	0.25	(1)
Greece	1.01	(0)	6,000	(0)	60 (0)	0	1,000	(1)	0	0	0.16	(0)
Hungary	1.08	(0)	Profit-based	(1)	73 (1)	0	24	(0)	0	1	0.81	(1)
Iceland	1.67	(1)	10,000	(0)	60 (0)	1	300	(0)	0	1	1.15	(1)
Ireland	1.66	(1)	588	(0)	73 (1)	0	2,500	(1)	1	1	0.30	(1)
Italy	1.02	(0)	Profit-based	(1)	70 (0)	1	620	(1)	1	0	0.58	(1)
Latvia	1.03	(0)	Profit-based	(1)	80 (1)	1	14	(0)	1	1	0.25	(1)
Lithuania	1.10	(0)	Profit-based	(1)	70 (0)	0	29	(0)	0	1	-0.04	(0)
Luxembourg	1.94	(1)	Profit-based	(1)	80 (1)	0	125	(0)	0	1	2.36	(1)
Malta	1.27	(0)	Profit-based	(1)	75 (1)	1	466	(0)	0	0	0.18	(0)
The Netherlands	1.76	(1)	Profit-based	(1)	67 (0)	1	120	(0)	0	1	0.25	(1)
Norway	1.39	(1)	Profit-based	(1)	59 (0)	1	No limit	(1)	1	1	0.32	(1)
Poland	0.61	(0)	1,250	(0)	70 (0)	0	1,389	(1)	1	0	1.59	(1)
Portugal	1.21	(0)	2,500	(0)	73 (1)	0	2,500	(1)	1	0	0.04	(0)
Romania	-0.12	(0)	Profit based	(1)	73 (1)	1	13	(0)	0	0	0.13	(0)
Slovakia	0.95	(0)	600	(0)	74 (1)	0	664	(1)	0	0	-0.06	(0)
Slovenia	0.88	(0)	125	(0)	51 (0)	0	125	(0)	0	0	0.32	(1)
Spain	1.29	(1)	Profit-based	(1)	60 (0)	0	600	(0)	1	0	0.28	(1)
Sweden	1.69	(1)	No fine	(0)	73 (1)	1	1,000	(1)	0	1	0.17	(0)
United Kingdom	1.68	(1)	No limit	(1)	76 (1)	1	No limit	(1)	1	1	0.26	(1)

(continued)

Table 4 (continued)

The table presents proxies for the quality of prior regulation, the implementation strength of the two directives, and changes in supervisory resources around the introduction of the two directives. For the analyses, we use the proxies to partition the sample into two groups and hence, we transform all the continuous variables into binary indicators (shown in parentheses) splitting by the sample median. We measure the quality of prior regulation with the *Regulatory Quality* index as of 2003, capturing the "ability of the government to formulate and implement sound policies and regulations," and taken from Kaufman et al. (2009). Higher index values indicate higher regulatory quality. The three variables to measure the strength of MAD implementation are: (i) the *Maximum Fine*$_{MAD}$ (in EUR thousands) that can be imposed on security issuers for violations of Article 2 of the MAD (CESR 2008). If the fine is unlimited or indicated as a percentage of profits from violations, we set the binary indicator variable equal to '1'. (ii) *Supervisory Powers*$_{MAD}$ equals the number of positive replies (out of 86 possible) by the local regulator to a questionnaire on the existence of specific supervisory powers regarding the translation of the MAD into local law (CESR 2007). Higher values indicate more supervisory powers. (iii) *Shift in Enforcement*$_{MAD}$ equals '1' if the local regulator has taken at least a single enforcement action under the MAD by the end of 2009. We construct this variable based on a CESR Review Panel report on the implementation of the MAD (CESR 2010). The three variables to measure the strength of TPD implementation are: (i) the *Maximum Fine*$_{TPD}$ (in EUR thousands) that can be imposed on security issuers for violations of Articles 4 to 6 of the TPD (CESR 2009a). If the fine is unlimited, we set the binary indicator variable equal to '1'. (ii) *Supervisory Powers*$_{TPD}$ takes on the value of '1' if a country complies with all the enforcement principles outlined in CESR Standard No. 1 as assessed by the CESR Peer Review in 2008 (CESR 2009b). (iii) *Shift in Enforcement*$_{TPD}$ equals '1' if local auditors and regulators indicate that the enforcement activity for the provision of financial information has substantially increased over the 2004 to 2009 period. We code this variable based on the answers to a self-constructed survey that we sent to the technical departments of PricewaterhouseCoopers and the supervisory authority in each EU member state (see also Christensen et al., 2013, Table 1). We measure the change in supervisory resources, which applies to both the MAD and TPD, with *Staff Growth* equal to the percentage change in full-time employees working for the national securities regulator over the 2004 to 2009 period. If available, we use the growth of supervisory staff specifically assigned to the oversight of securities regulation. Otherwise, we use the staff growth for the joint regulator (i.e., including the banking and insurance sectors). We collect staff numbers from the annual reports of the local regulators and the survey in Central Banking Publications (2009).

Table 5: Liquidity Effects of Tighter EU Securities Laws When Prior Regulation or Implementation Differs

Panel A: Results for the Market Abuse Directive

Ln(Liquidity Factor +1) as Dependent Variable (N = 112,260)	*Prior Regulation*	*MAD Implementation Strength*			
	Regulatory Quality 2003	*Maximum Fine$_{MAD}$*	*Supervisory Powers$_{MAD}$*	*Shift in Enforcement$_{MAD}$*	*Staff Growth*
Prior Regulation Quality:					
High	-0.180***	–	–	–	–
	(-2.84)				
Low	-0.003	–	–	–	–
	(-0.07)				
Implementation Strength:					
Strong	–	-0.151***	-0.222**	-0.164***	-0.139**
		(-2.73)	(-2.28)	(-2.99)	(-2.52)
Weak	–	-0.025	-0.053*	-0.000	-0.051
		(-0.57)	(-1.94)	(-0.00)	(-0.96)
F-test for Differences across Coefficients (p-value):					
High/Strong = Low/Weak	0.047	0.175	0.135	0.055	0.392
Control Variables	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes

Panel B: Results for the Transparency Directive

Ln(Liquidity Factor +1) as Dependent Variable (N = 112,260)	*Prior Regulation*	*TPD Implementation Strength*			
	Regulatory Quality 2003	*Maximum Fine$_{TPD}$*	*Supervisory Powers$_{TPD}$*	*Shift in Enforcement$_{TPD}$*	*Staff Growth*
Prior Regulation Quality:					
High	-0.149**	–	–	–	–
	(-2.19)				
Low	0.034	–	–	–	–
	(0.68)				
Implementation Strength:					
Strong	–	-0.108*	-0.103*	-0.191**	-0.135**
		(-1.95)	(-1.79)	(-2.48)	(-1.96)
Weak	–	-0.042	-0.058	0.032	-0.022
		(-0.73)	(-1.12)	(0.75)	(-0.39)
F-test for Differences across Coefficients (p-value):					
High/Strong = Low/Weak	0.064	0.486	0.619	0.032	0.334
Control Variables	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes

The sample comprises firm-quarter observations from 26 EU countries over the 2001 to 2011 period. We report results for the *Liquidity Factor* as dependent variable. *MAD* (in Panel A) and *TPD* (in Panel B) are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force. For each model we partition the sample into two distinct groups by interacting the *MAD* and *TPD* variables with a binary indicator variable for the quality of prior regulation (high vs. low) or the strength of MAD or TPD implementation (strong vs. weak). For a description of the country-level partitioning variables see Table 4. Throughout the table, we include the full set of control variables and fixed effects (see Model 4 in Table 2), but only report OLS coefficient estimates (t-statistics) for the main variables. We compute t-statistics based on robust standard errors clustered by country. We also report p-values from Wald tests assessing the statistical significance of the differences in coefficients across groups. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 6: Liquidity Effects When Combining Prior Regulation and Implementation Differences

Panel A: Results for the Market Abuse Directive

Ln(Liquidity Factor +1) as Dependent Variable (N = 112,260)	*MAD Implementation Strength (IS)*			
	Maximum Fine$_{MAD}$	*Supervisory Powers$_{MAD}$*	*Shift in En-forcement$_{MAD}$*	*Staff Growth*
Regulatory Quality (RQ):				
High RQ/Strong IS	-0.230***	-0.337***	-0.239***	-0.236***
	(-3.05)	(-3.39)	(-3.16)	(-2.85)
High RQ/Weak IS	-0.072	-0.106**	-0.031	-0.080*
	(-1.63)	(-2.31)	(-0.59)	(-1.78)
Low RQ/Strong IS	-0.025	-0.086	-0.018	0.076
	(-0.59)	(-1.58)	(-0.37)	(1.63)
Low RQ/Weak IS	0.059	0.039	0.034	-0.066
	(1.23)	(0.72)	(0.76)	(-1.23)
F-test for Differences across Coefficients (p-value):				
High RQ/Strong IS = High RQ/Weak IS	0.074	0.030	0.024	0.085
High RQ/Strong IS = Low RQ/Strong IS	0.038	0.012	0.026	0.003
Low RQ/Strong IS = Low RQ/Weak IS	0.093	0.075	0.359	0.004
Control Variables	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

Panel B: Results for the Transparency Directive

Ln(Liquidity Factor +1) as Dependent Variable (N = 112,260)	*TPD Implementation Strength (IS)*			
	Maximum Fine$_{TPD}$	*Supervisory Powers$_{TPD}$*	*Shift in En-forcement$_{TPD}$*	*Staff Growth*
Regulatory Quality (RQ):				
High RQ/Strong IS	-0.192**	-0.189**	-0.196**	-0.213**
	(-2.14)	(-2.05)	(-2.50)	(-2.49)
High RQ/Weak IS	-0.064	-0.076	0.023	-0.033
	(-1.21)	(-1.60)	(0.41)	(-0.70)
Low RQ/Strong IS	0.022	0.025	0.004	0.118**
	(0.46)	(0.51)	(0.08)	(1.98)
Low RQ/Weak IS	0.140	0.143	0.035	-0.018
	(1.48)	(1.45)	(0.71)	(-0.30)
F-test for Differences across Coefficients (p-value):				
High RQ/Strong IS = High RQ/Weak IS	0.262	0.326	0.041	0.099
High RQ/Strong IS = Low RQ/Strong IS	0.080	0.091	0.043	0.006
Low RQ/Strong IS = Low RQ/Weak IS	0.216	0.235	0.582	0.063
Control Variables	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

The sample comprises firm-quarter observations from 26 EU countries over the 2001 to 2011 period. We report results for the *Liquidity Factor* as dependent variable. *MAD* (in Panel A) and *TPD* (in Panel B) are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force. For each model we partition the sample into four distinct groups by interacting the *MAD* and *TPD* variables with a binary indicator for the quality of prior regulation (high vs. low) and another binary indicator for the strength of MAD or TPD implementation (strong vs. weak). For a description of the country-level partitioning variables see Table 4. Throughout the table, we include the full set of control variables and fixed effects (see Model 4 in Table 2), but only report OLS coefficient estimates (t-statistics) for the main variables. We compute t-statistics based on robust standard errors clustered by country. We also report p-values from Wald tests assessing the statistical significance of the differences in coefficients across groups. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table A1: Examples of Implementation Timeline of EU Securities Regulation

Country	*Date of Initial Draft*	*Description of Initial Draft Date*	*Date of Signature or Publication*	*Entry-into-Force Date*	*Length of Observable Process (mths)*
Market Abuse Directive (Passage at EU Level on 28-Jan-2003):					
France	27-Oct-2004	Consultation period begins	21-Jul-2005	27-Jul-2005	9
Germany	10-Mar-2004	Preliminary draft published	29-Oct-2004	30-Oct-2004	8
The Netherlands	27-Oct-2004	First Royal Message (Kamerstuk 29827)	12-Jul-2005	1-Oct-2005	11
Sweden	7-Jul-2004	Consultation period begins	13-Jun-2005	1-Jul-2005	12
United Kingdom	1-Jun-2004	Consultation period begins	23-Feb-2005	1-Jul-2005	13
Transparency Directive (Passage at EU Level on 17-Dec-2004):					
France	27-Jul-2006	Consultation period begins	18-Dec-2007	19-Dec-2007	17
Germany	3-May-2006	Preliminary draft published	5-Jan-2007	20-Jan-2007	9
The Netherlands	3-Jul-2007	First Royal Message (Kamerstuk 31093)	20-Nov-2008	1-Jan-2009	18
Sweden	26-Apr-2006	Consultation period begins	13-Jun-2007	1-Jul-2007	14
United Kingdom	1-Mar-2005	White Paper published by the Department for Trade and Industry	8-Nov-2006	1-Jan-2007	22

The table lists various observable dates of the transposition of the Market Abuse Directive *(MAD)* and Transparency Directive *(TPD)* into national law for a select group of five EU member states. The *Date of Initial Draft* is the first date for which a draft of the national law became publically available via official communication channels. Because the stage when the government releases the initial draft of a law varies across countries, we add a short description. The *Date of Signature or Publication* represents the date when the finalized national law was officially published or signed by the head of state. The *Entry-into-Force Date* is the point in time when the MAD or TPD came into force in each EU member state (see also Table 1, Panel A). The *Length of Observable Process* gives the number of months between when the initial draft of the law was made publicly available and when the law came into force. All dates are from government websites or archives and other public sources.

Table B1: Sensitivity Analyses of the Liquidity Effects from Tighter EU Securities Regulation

Ln(Liquidity Factor +1) as Dependent Variable	*N*	*Market Abuse Directive*	*Transparency Directive*
(1) Alternative Clustering:			
- Two-Way Clustering by Country and Quarter-Year	112,260	-0.115***	-0.093**
		(-4.63)	(-2.26)
- Clustering by Economic Region	112,260	-0.115***	-0.093*
		(-3.92)	(-1.94)
(2) Alternative Fixed Effects Structures:			
- Firm-Fixed Effects	112,260	-0.126***	-0.069**
		(-5.09)	(-2.22)
- Add Separate Quarter-Year-Fixed Effects for Developed Markets	112,260	-0.120***	-0.056*
		(-2.62)	(-1.84)
- Add Separate Quarter-Year-Fixed Effects Interacted with Firm Size	112,260	-0.114***	-0.094**
		(-3.55)	(-2.33)
(3) Alternative Sample Specifications:			
- Excluding GIIPS Countries plus Cyprus	94,501	-0.116**	-0.087**
		(-2.52)	(-2.06)
- Including Non-EU Benchmark Countries	695,156	-0.099*	-0.090**
		(-1.81)	(-2.48)
(4) Alternative Control Variables:			
- Not Controlling for Ln(Share Turnover$_{t-4}$)	112,260	-0.121***	-0.074*
		(-3.81)	(-1.97)
- Controlling for Ln(Market Value$_{2001}$)	112,260	-0.096***	-0.073*
		(-3.55)	(-1.83)

The sample comprises firm-quarter observations from 26 EU countries over the 2001 to 2011 period. We report results for the *Liquidity Factor* as dependent variable. *MAD* and *TPD* are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive or the Transparency Directive came into force. We report results for the following specifications: First, we use alternative clustering criteria when computing standard errors. That is, we apply (i) two-way clustering by country and quarter-year, or (ii) clustering by 18 economic regions (e.g., Southern Europe, Central Europe, etc.). Second, we use alternative fixed-effect structures. That is, we (i) replace the country- and industry-fixed effects with firm-fixed effects, (ii) add separate quarter-year fixed effects for developed markets, or (iii) add quarter-year fixed effects that are interacted with the *Market Value* of the firm. We identify developed markets based on the Morgan Stanley Capital International database. Third, we change the composition of the sample. That is, we (i) exclude the so-called GIIPS countries (Greece, Ireland, Italy, Portugal, and Spain) as well as Cyprus from the sample as they all experienced a serious economic downturn over the sample period, and (ii) add 582,896 firm-quarter observations from 31 non-EU countries (that are not subject to the MAD and TPD) as benchmark sample. In this latter specification we define the quarter-year-fixed effects over all sample countries. Fourth, we use alternative sets of control variables. That is, we (i) exclude *Share Turnover* from the model (as it is sometimes used as liquidity proxy), and (ii) replace *Market Value* measured in quarter *t*–4 with *Market Value* as of 2001 (i.e., the beginning of our sample period) or whenever a firm for the first time enters the sample. Unless indicated otherwise, we include the full set of control variables and fixed effects (see Model 4 in Table 2), but only report OLS coefficient estimates (t-statistics with country clustering) for the main variables. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table B2: Contingency Tables of Implementation Timing, Regulation Quality, and Implementation Strength

Market Abuse Directive

Regulatory Quality 2003

		Low (a)	High (b)	(a)+(b)
Late	(i)	9	5	14
Early	(ii)	6	9	15
	(i)+(ii)	15	14	29

p-value: 0.191

Maximum Fine$_{MAD}$

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	6	8	14
Early	(ii)	9	6	15
	(i)+(ii)	15	14	29

p-value: 0.356

Supervisory Powers$_{MAD}$

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	9	5	14
Early	(ii)	9	6	15
	(i)+(ii)	18	11	29

p-value: 0.812

Shift in Enforcement$_{MAD}$

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	8	6	14
Early	(ii)	7	8	15
	(i)+(ii)	15	14	29

p-value: 0.573

Staff Growth

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	7	7	14
Early	(ii)	7	8	15
	(i)+(ii)	14	15	29

p-value: 0.858

Transparency Directive

Regulatory Quality 2003

		Low (a)	High (b)	(a)+(b)
Late	(i)	7	7	14
Early	(ii)	8	7	15
	(i)+(ii)	15	14	29

p-value: 0.858

Maximum Fine$_{TPD}$

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	7	7	14
Early	(ii)	8	7	15
	(i)+(ii)	15	14	29

p-value: 0.858

Supervisory Powers$_{TPD}$

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	7	7	14
Early	(ii)	9	6	15
	(i)+(ii)	16	13	29

p-value: 0.588

Shift in Enforcement$_{TPD}$

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	9	5	14
Early	(ii)	8	7	15
	(i)+(ii)	17	12	29

p-value: 0.550

Staff Growth

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	5	9	14
Early	(ii)	9	6	15
	(i)+(ii)	14	15	29

p-value: 0.191

The table presents cross-tabulations of the MAD or TPD adoption timing (late vs. early) with either the quality of prior regulation (low vs. high) or the implementation strength of the MAD and TPD (weak vs. strong). The analysis comprises country-level observations for the 29 EU member states listed in Table 1. We classify a country as late (early) if the adoption of the MAD or TPD occurred after (before) the median entry-into-force date of the respective directive. For a description of the country-level partitioning variables see Table 4. We also report p-values from chi-squared tests assessing the statistical significance of the frequencies across cells for each contingency table.