

Capital-Market Effects of Securities Regulation: Prior Conditions, Implementation, and Enforcement*

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Abstract

This paper examines the economic 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 two directives, but did so at different points in time. Our research design exploits this staggered introduction of the same regulation to identify capital-market effects. We also examine cross-sectional variation in the strictness of implementation and enforcement as well as in prior regulatory conditions. We find that, on average, market liquidity increases as EU countries tighten market abuse and transparency regulation. The effects are larger in countries that implement and enforce the directives more strictly. They are also stronger in countries with traditionally stricter securities regulation and with a better prior track record of implementing regulation and government policies. The results indicate that the same forces that limited the effectiveness of regulation in the past are still at play when new rules are introduced, leading to hysteresis in regulatory outcomes. The findings further illustrate that harmonizing regulation in countries with different prior conditions can make countries diverge more, rather than move them closer together. This insight has important implications for global regulatory reform.

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1. Introduction

Extensive securities regulation is widespread around the world. Yet, as with regulation in general, the academic debate on the costs and benefits of securities regulation is controversial and the evidence is fairly mixed.¹ Whether or not securities regulation is beneficial to the economy appears to be largely an empirical matter. Regulatory effects likely depend also on how regulation is implemented and enforced (e.g., Djankov et al. 2003a). Moreover, the state of prior regulation and a country's regulatory quality in general could play an important role. Prior studies typically focus on regulatory changes in a single country (e.g., the Sarbanes-Oxley Act in the U.S.; 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 recent changes in EU securities regulation and examine their capital-market effects. The EU setting has several desirable features. First, it allows us to analyze the same regulatory change across EU member states at different points in time. The staggered implementation offers better identification of regulatory effects than a single regulatory event such as SOX. 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 allows us to analyze the role of existing securities regulation for regulatory outcomes, that is, the interaction between a country's prior regulatory conditions and newly introduced regulation.

¹ For the debate and discussions of the evidence see, e.g., Coffee (1984), Easterbrook and Fischel (1984), Shleifer (2005), Mulherin (2007), Leuz and Wysocki (2008), and Zingales (2009).

One hypothesis posits that countries with weaker prior regulation benefit more from the new (stricter) directives because they have to catch up. Alternatively, the same forces and constraints that limited the scope and effectiveness of securities regulation in the past (e.g., lack of institutional fit, insufficient resources, political resistance, inefficient bureaucracies, etc.) are still at play when new rules come into force. This alternative hypothesis suggests that there is hysteresis in regulatory outcomes because prior conditions matter. It also implies that imposing the same regulation on countries with disparate prior conditions could make them diverge more, rather than move together.

We study the capital-market effects of two EU directives that concern central elements of securities regulation: the Market Abuse Directive (MAD) addresses insider trading and market manipulation and the Transparency Directive (TPD) addresses corporate reporting and disclosure. We analyze changes in stock market liquidity around the staggered implementation of these two directives. Market liquidity is a well suited outcome variable for our setting and identification strategy, as it can be measured over relatively short intervals and is less anticipatory in nature than other economic constructs that could be used to evaluate securities regulation (e.g., firm value or cost of capital). Improvements in liquidity are also frequently used to justify securities regulation. We measure liquidity by combining four individual proxies for market liquidity, following Daske et al. (2008) and Lang et al (2012).

We estimate quarterly panel regressions from 2001 to 2011 for EU firms. Member states need to implement the directives in a predetermined and relatively short time period after the directives are passed at the EU level. The local implementation of the directives leads to entry-into-force dates that differ across countries and are spread over two to three years. We exploit this variation by introducing quarter-year fixed effects that flexibly account for liquidity trends in

EU capital markets, along with country fixed effects. In this design, the identification of regulatory effects comes solely from within-EU variation in the effective dates of two directives.

Using this setting and design, we find that market liquidity increases when new market abuse (MAD) and transparency (TPD) regulation 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 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. This magnitude is clearly economically significant, when considering the recurring nature of the savings, but it is not too large to be implausible. In sum, our results suggest that improving key elements of securities regulation leads to substantial economic benefits in capital markets.²

The staggered implementation of the directives mitigates standard concerns in regulatory studies (e.g., about concurrent shocks). To induce spurious results by chance, unrelated economic shocks to liquidity would have to line up with the implementation dates of the two directives in a large set of EU countries, which is implausible. Moreover, the relatively narrow implementation window set by the EU and the inflexible and lengthy legislative processes implementing the directives at the national level minimize concerns that the entry-into-force dates are endogenous (e.g., related to local shocks). It seems unlikely that a large set of countries experiences a series of differentially-timed local shocks, each prompting national lawmakers to start the country's implementation process for the directive, and then leading to entry-into-force dates that coincide with subsequent liquidity improvements (or reversals). It is even more

² 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.

unlikely that lawmakers could explicitly time the entry-into-force dates such that they coincide with (future) increases in liquidity. Moreover, the political decision to enact the regulation is made years earlier at the EU level, mitigating the concern that the results could reflect a market response to the events giving rise to the new directives in the first place. Thus, we argue (and provide evidence) that this setting is well suited for the identification of causal regulatory effects.

We gauge the validity of our identification strategy with four additional tests. First, we analyze the liquidity patterns in event time. We find that liquidity is not significantly elevated in the year leading up to the implementation, improves roughly around the time the directives come into force, and stays at a higher level for the remainder of the sample period. Thus, liquidity changes relatively ‘sharply’ around the directives’ entry-into-force dates suggesting that the directives are indeed responsible for the effects. Second, to address concerns about differential local trends, we control for pre-implementation liquidity changes (or shocks) and also introduce country-specific (linear and quadratic) time trends in market liquidity. The results are robust to both extensions. Third, we exploit the fact 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. These markets allow us to estimate the regulatory effects within-country, and hence to control for concurrent local shocks or liquidity changes that are common to all publicly traded firms in an economy. In this test, the identification comes from differential liquidity effects across firms trading on regulated and unregulated markets in a particular country and following the country-specific entry-into-force date. To further tighten the test, we use propensity score matching to ensure that firms on regulated and unregulated markets have similar characteristics. We find that liquidity improves significantly on regulated markets relative to unregulated markets after the two new directives come into force. This evidence

supports a causal link between securities regulation and liquidity and further mitigates concerns that the directives' entry-into-force dates line up with country-specific shocks, by chance or endogenously. Fourth, we show that pre-implementation liquidity changes as well as institutional characteristics (that explain cross-sectional variation in the treatment effects) are not significantly related to countries' implementation timing. We also conduct a falsification test in the spirit of Altonji et al. (2005). This test confirms that observable time-variant proxies for local market conditions, political and economic forces, which in principle could influence implementation timing (e.g., induce lawmakers to implement the directives more quickly), do not produce significant liquidity effects and the coefficients do not come close to the estimated treatment effects of the two directives.

Overall, we conclude that the average liquidity effects of the two directives are well identified. We then examine whether there is heterogeneity in the treatment effects. First, we exploit differences in prior regulatory conditions across EU countries. We document that the liquidity effects of the two directives are stronger in countries with higher prior regulatory quality.³ One explanation for these findings is that countries with a better track record of applying regulation and government policies in general are more willing and/or better able to implement and enforce the new regulation. Put differently, the same forces that limited securities regulation in the past appear to be at work when new and tighter rules come into effect. We note that these forces could have many reasons, including concerns about institutional fit, resource constraints, inefficient bureaucracies, regulatory capture, and political pressures.

³ We use a proxy from Kaufman et al. (2009) that is not specific to securities regulation, but more generally measures the ability of a government to formulate and implement sound policies and regulations. We obtain similar results using a proxy for the strength of prior securities regulation.

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 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 directives as a measure 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 around the two directives are strongest in countries with high past regulatory quality *and* strong implementation. We find no liquidity increases for countries with low-quality prior regulation and weak implementation. Moreover, stricter implementation of the new directives often has an incremental effect, but this effect is more pronounced in countries with high past regulatory quality. Thus, there is evidence 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 that the two directives had the opposite effect, illustrating that imposing the same (new) regulation on countries with disparate prior conditions can make countries diverge more, not less. This finding sends an important message to regulators attempting to harmonize markets globally through regulatory reforms.

Our paper makes several contributions to the literature. First, we show that the imposition of stricter securities regulation can have significant economic benefits for a broad cross-section of firms. Moreover, we identify market liquidity as a specific channel through which securities regulation confers economic benefits. Prior studies often cast doubt on the existence of benefits

from securities regulation, especially those examining the capital-market effects of U.S. securities regulation in the 1930s (e.g., Stigler, 1964; Benston, 1969 and 1973; Jarrell, 1981; Mahoney and Mei, 2009). Similarly, the evidence on Regulation Fair Disclosure (e.g., Heflin et al., 2003; Gintchel and Markov, 2004; Francis et al., 2006) or SOX (e.g., Chhaochharia and Grinstein, 2007; Zhang, 2007; Li et al., 2008) is decidedly mixed and often emphasizes the costs rather than the benefits of securities regulation. Earlier studies examining securities regulation in the 1930s often lack a convincing control group. As pointed out in Collver (2007) and Leuz (2007) similar concerns apply to the studies on Regulation Fair Disclosure or SOX, as these regulatory changes affected all SEC registrants.⁴ 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 for smaller firms; Iliev (2009) uses a size-based compliance cutoff in SOX Section 404 to employ a regression-discontinuity design. The evidence from these studies is mixed but suggests that regulation can impose net costs on 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 27 countries.⁵ This approach offers not only tight identification but also yields evidence on the capital-market effects of securities regulation for the population of publicly traded firms in many countries.

Second, we shed light on forces that make securities regulation more or less effective. We show that regulatory outcomes depend on countries' prior regulatory conditions as well as countries' ability and willingness to implement and enforce new rules. These findings highlight the role of implementation and enforcement and document substantial hysteresis in regulatory

⁴ To control for contemporaneous shocks, several studies focus on the differences in firms that are more or less affected by the regulatory act. However, this approach needs to assume which firms benefit from the law.

⁵ Agrawal (2009) uses a similar identification strategy to ours. He uses the staggered passage of state investor protection statutes in the U.S. during the early 1900s to identify the effects of investor protection laws on the financing and investment decisions of firms in a particular industry (i.e., mining).

outcomes. They are consistent with the enforcement theory formulated in Djankov et al. (2003a) as well as its application to securities regulation in Shleifer (2005).

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 use the same dataset and 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.⁶ 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 paper proceeds as follows. Section 2 develops our hypotheses and provides more details on the institutional setting. Section 3 delineates our research design and describes the data. Section 4 presents estimates of the average liquidity effects of the two securities law directives. It also assesses the validity of our identification strategy. In Section 5, we report results of cross-sectional differences along the dimensions of prior regulatory conditions, implementation, and enforcement strength. Section 6 concludes.

⁶ We also analyze (changes in) supervisory resources. In this sense, our study is related to Coffee (2007) and Jackson and Roe (2009), both of which point to an association between capital-market outcomes and the level of enforcement staff and budgets.

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 (leading to low market liquidity) 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 mitigate these problems and hence be beneficial for a country's capital market.

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; Leuz and Wysocki, 2008, Zingales, 2009). However, these arguments often set aside problems of how to implement and enforce securities regulation.⁷ 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.⁸ They point out that regulators face serious information problems, are often incompetent or even corrupt, and can be captured in the regulatory process. These arguments, however, do not imply that regulation necessarily has negative effects. Private contracts as an alternative to regulation rely heavily on functioning courts and private litigation. But 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).

⁷ Shleifer (2005) argues that the same can be said for the public interest theory of regulation in general.

⁸ 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).

Against this backdrop, 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 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” problem.⁹ 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 discussed 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 references in Section 1).

Furthermore, much of the evidence stems from U.S. securities regulation.¹⁰ 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; Bhattacharya and Daouk, 2009). In addition, a

⁹ 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).

¹⁰ There is some international evidence (e.g., Glaeser et al., 2001; 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.

country's past 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 also depend on its implementation and enforcement, and not just its design.

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 economic benefits (e.g., improves market liquidity). 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 the 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. Specifically, we hypothesize that countries with stricter implementation and enforcement of the EU directives exhibit larger capital-market effects.

In addition, the setting 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 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 hypotheses, we examine the Market Abuse Directive (MAD) on insider trading and market manipulation, and the Transparency Directive (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 provide a brief description of 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 establishes a common definition of an insider and transparency standards requiring people who recommend investments to disclose their relevant interests. It also requires each member state to establish a supervisory authority that is responsible for monitoring insider trading and market manipulation

and to give this authority the necessary supervisory and investigative powers.¹¹ 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 tighten and harmonize the implementation and enforcement of existing EU regulation (e.g., Lamfalussy, 2000; CRA, 2009).

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.¹² The TPD also requires that the

¹¹ 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.

¹² 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

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 and improves 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 security offerings, the Markets in Financial Instruments Directive on the provision of investment services across the EU, and the Takeover Directive, which provides a common framework for mergers and acquisitions, and takeover bids in the EU. All these directives were implemented over the 2004 to 2009 period, and because they potentially improve, or at least affect market liquidity, they could confound an empirical analysis of the MAD and TPD. We address this issue with our identification strategy.

3. Research Design and Data

3.1. Empirical Model and Identification Strategy

We test the hypotheses developed in Section 2 using a panel dataset with quarterly firm-level observations of stock market liquidity. We focus on liquidity for three reasons. First, economic theory predicts that reducing insider trading or enhancing transparency reduces information asymmetries between investors and hence increases market liquidity (e.g., Glosten and Milgrom, 1985; Diamond and Verrecchia, 1991; Verrecchia, 2001). Thus, a liquidity analysis for the two EU directives is well grounded in economics. Second, a stated goal of the directives was to increase market confidence, which is closely related to market liquidity (e.g.,

violations and corrections to the financial reports going forward. Some EU countries like the U.K. introduced such a review process already with IFRS adoption and hence before the TPD. See Christensen et al. (2013) for a discussion and an analysis of these changes.

Lamfalussy, 2000; Enriques and Gatti, 2008; CRA, 2009). We therefore focus on an outcome that matters to regulators and policy makers. Third, liquidity is well suited for our empirical strategy. Market liquidity can be measured over reasonably short intervals and should reflect the effects of regulatory changes around the time that they occur. That is, liquidity proxies are likely less anticipatory than other economic outcomes (e.g., firm value or the cost of capital).¹³

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 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 liquidity factor. *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

¹³ 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).

result the identification stems from within-EU variation in the entry-into-force dates of the two directives. We draw statistical inferences based on standard errors clustered by country.¹⁴

Our empirical strategy for estimating the effects of securities regulation on market liquidity is based on the following key 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). This staggered introduction allows us to use quarter-year fixed effects, which alleviates two common concerns that typically arise when analyzing a regulatory change.¹⁵ 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 possible that markets respond to these preceding events rather than to the new regulation itself (e.g., Ball, 1980; Mulherin, 2007). In our setting, confounding events would need to be correlated with the staggered imposition of *two* directives across *many* 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

¹⁴ 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, we conclude that, if anything, our inferences using standard errors clustered by country are conservative.

¹⁵ See also Kalemli-Ozcan et al. (2010a, 2010b). The first paper uses the transposition dates of the 27 FSAP directives to estimate the effect of financial reform on EU banking integration. The second paper uses the FSAP transposition dates as instruments to estimate the effects of financial integration on international business cycle synchronization.

limited 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.¹⁶ In Appendix A, we provide more details on the legislative processes in the EU supporting the claim that they are lengthy and inflexible.

The limited implementation window set by the EU combined with lengthy and inflexible legislative processes at the national level substantially reduces concerns about reverse causality as well as 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 concern that the entry-into-force dates are endogenous because lawmakers respond to local conditions or shocks by implementing the directives. In our setting, this concern implies that many countries experiences a series of *differentially*-timed local shocks, each prompting national lawmakers to start the country's implementation process for the directive, and in turn leading to entry-into-force dates that coincide with subsequent liquidity improvements (or reversals). This sequence seems unlikely, considering the narrow

¹⁶ 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 timing of implementation per se, given our fixed-effects structure accounts flexibly for liquidity trends). We explore this issue in Section 5.

implementation window and the fact that our setting uses two different directives that are implemented in a large set of countries.

In sum, our setting is well suited to identify causal effects of securities regulation on market liquidity. Nevertheless, we perform several sensitivity analyses to assess our identification strategy (see Section 4.2). One of these analyses uses within-country estimation exploiting that firms on unregulated markets are not affected by the directives to the same extent. The within-country specification flexibly accounts for country-specific trends or shocks that apply to all firms in an economy and, thus, is even more stringent than the main analysis. This specification further mitigates the concern that our results reflect endogenous responses to local conditions, rather than the directives' regulatory effects.

3.2. Data and Construction of the Variables

Our sample period starts in the first quarter of 2001, i.e., 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.¹⁷ The sample comprises 112,260 firm-quarter observations from 26 countries. We exclude firms with a U.S.

¹⁷ 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.

cross-listing as they are subject to insider trading and transparency rules in the U.S. In addition, we eliminate firms trading on unregulated EU markets that are not necessarily subject to the MAD and TPD (e.g., the Alternative Investment Market in London) as well as very small firms with, on average, market values below US\$ 5 million.¹⁸ 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 given 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 contact 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 United Kingdom) to August 2009 (Czech Republic). This variation in the effective dates of the directives is at the core of our identification strategy.

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 MAD and TPD, i.e., as binary variables that are coded as ‘1’ beginning in the quarter in which the respective directive comes

¹⁸ 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.

into force in a given country (and ‘0’ otherwise). 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).

To address measurement error and concerns about any individual proxy, we aggregate four liquidity proxies into a single measure for market liquidity. 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 *Zero Returns*, defined as the proportion of trading days with zero daily stock returns out of all potential trading days per quarter. The third 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).¹⁹ 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).²⁰ We follow Daske et al. (2008) and Lang et al. (2012) and aggregate the four liquidity proxies into a

¹⁹ 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 medians.

²⁰ 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).

single *Liquidity Factor* employing factor analysis, and use the factor scores from the first (and only) factor with an Eigenvalue greater than one as dependent variable.²¹

As control variables, we include GDP per Capita to capture differences in countries' macro-economic development and, following the prior literature (e.g., Chordia et al., 2000; Leuz and Verrecchia, 2000), the market value of equity, share turnover, and return variability to account for differences in firm characteristics—all lagged by four quarters to account for seasonal effects. The notes to Table 1 provide further details on 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.²² 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 used in the regressions.

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. As is common for liquidity models and given our extensive fixed-effects structure, the explanatory power of the regressions is high with an R^2 of 65 percent. The firm-specific control variables are significant and exhibit the expected signs. Large firms

²¹ In Appendix B, we consider alternative ways to construct the liquidity factor and also use bid-ask spreads and zero returns, individually, to measure liquidity.

²² 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.

and firms with a high share turnover are more liquid, and firms with more volatile returns are less liquid. GDP per Capital 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 Cumming et al. (2011).

For our test variables, we find that the coefficient on both *MAD* and *TPD* are negative and statistically significant. The results imply that tighter securities regulation increases market liquidity. The estimated effects are economically significant. For *MAD*, the coefficient of -0.111 suggests that, on average, liquidity increases by roughly 11 percent.²³ For *TPD*, the coefficient of -0.085 indicates an average increase in liquidity of about 8 percent. *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.

As a way to further gauge the economic magnitude we translate the percentage effects into annual trading cost savings for the average firm. Doing so, we multiply the estimated trading cost reductions with the yearly dollar trading volume per firm and compute the mean. For bid-ask spreads, the average cost savings are approximately US\$ 0.16 and 0.13 million (or 0.05 and 0.04 percent of market value) for the *MAD* and the *TPD*, respectively. For total round-trip trading costs, the annual savings are approximately US\$ 0.43 and 0.35 million (or 0.10 to 0.08 percent of market value) for the *MAD* and the *TPD*, respectively. These numbers are clearly economically significant, in particular when considering the recurring nature of the savings, but at the same time they are not too large to be implausible. Thus, the tightening of securities regulation in the EU provides significant capital-market benefits.

²³ To gauge the economic magnitude we compute the average percentage change in bid-ask spreads as $e^{-0.111} - 1 = -0.105$.

4.2. Assessing Identification and Results from Within-Country Estimation

The EU setting provides a natural experiment to estimate the causal effects of stricter securities regulation on market liquidity. The staggered implementation of the directives during a limited window, across many countries, and through lengthy and inflexible national legislative processes mitigates many of the usual concerns about identification and should provide a reasonable estimate of the causal effects.²⁴ In this section, we further assess our empirical strategy, in particular, the notion that countries' entry-into-force dates provide reasonably sharp identification with respect to changes in market liquidity. If they do, 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 as well as concurrent changes in market liquidity. Such a correlation would arise, for instance, if countries chose to implement the directives in response to local liquidity shocks. We argue in Section 3 that such selection of the dates is unlikely for institutional reasons. To provide additional empirical support that our findings are not driven by selection and local shocks, we show that within-country estimation yields very similar results and that selection on time-variant observables (and time-invariant unobservables) does not even come close to producing the estimated treatment effects.

To first assess the sharpness of the liquidity effects, 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). 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

²⁴ In fact, the introduction of EU-specific quarter-year fixed effects is very demanding and could even 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.

liquidity is significantly higher after the directives become effective compared to the liquidity level in the preceding year. Thus, liquidity appears to increase right around the time the directives enter into force.

That said, it is a priori not obvious that the treatment effect of the directives is indeed “sharp” right at the entry-into-force date. More generally, the effect is not expected to be as sharp as in medical trials or educational experiments for which the timing of 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 for 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 we argue in footnote 13). Moreover, we know that several countries (like the U.K.) implemented some 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 enforcement personnel.²⁵ Thus, particularly the timing of the TPD liquidity effects is less likely to be sharp.

To more finely gauge the sharpness of the entry-into-force dates, we graphically inspect the liquidity patterns. In Figure 2, we estimate and plot separate coefficients for each quarter, starting eight quarters before and ending four quarters after each directive becomes effective. There is a clear and sustained increase in liquidity for both directives. For the MAD, the

²⁵ 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., 2010). Our survey of regulators and auditors also supports the notion that enforcement activities under the TPD were gradually increased over time.

liquidity effect is visible and significant starting in the quarter before the effective date ($t-1$). For the TPD, the effect is visible and significant starting a quarter earlier ($t-2$). For both directives, the coefficient estimates in earlier quarters ($t-3$ and before) are essentially zero, consistent with limited anticipation effects and a well-specified model. The estimates also get larger after the entry-into-force dates, consistent with implementation taking some time.

We also (counter-factually) shift the assignment of the implementation dates quarter-by-quarter for all EU countries, and each time re-estimate our base regression 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. The pattern for *TPD* is less clear and appears delayed, i.e., coefficients continue to increase beyond the true entry-into-force dates before they start reversing (after quarter $t+5$). The pattern suggests that it takes a few quarters until the TPD becomes fully effective, but it again confirms that the entry-into-force dates are critical for the onset of the liquidity effects.

All of the above tests for the time pattern of the liquidity effects confirm that the entry-into-force dates provide reasonably sharp identification. The documented liquidity increases do not reflect a gradual, longer-term trend in the EU; instead they occur within a few quarters of the country-specific entry-in-force dates and are sustained. Given these findings, we next focus on the question of whether the country-specific implementation dates could be endogenous, for example, because countries choose to implement the directives in response to local liquidity

shocks. As discussed in Section 3, selection is unlikely to be a major concern in our setting but we nevertheless conduct five additional tests and present them 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 specification mitigates 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 average liquidity change in a country in the year after the respective directive passed at the EU level explains the entry-into-force dates (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 we would expect if liquidity shocks affected countries' implementation dates. Thus, prior liquidity shocks do not explain timing or our findings.

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 time pattern of the liquidity effects shown in Figure 2 as well as the more gradual implementation of this directive noted earlier. However, the *TPD* coefficient remains significantly negative, confirming that the treatment effect is reasonably sharp and distinct from a quadratic trend. Thus, differential local 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 apply only to a lesser extent to 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 can provide a within-country benchmark. That is, 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. In this specification, the coefficients on *MAD* and *TPD* identify the liquidity effects around the directives for firms on regulated markets relative to firms 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 this specification for a balanced and propensity-matched sample to ensure that regulated and unregulated firms have similar characteristics.²⁶ Again, the coefficients on *MAD* and *TPD* remain significant and are only slightly attenuated relative to the full-sample estimates in Table 2.

In Column (5), we present our final test gauging the concern that the staggered dates are endogenous. Under the selection explanation, local shocks affect liquidity and at the same time 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).

²⁶ We conduct the matching within each country based on the following firm characteristics in the quarter before *MAD* came into force such that the number of regulated and unregulated firms is the same: total assets, return on assets, book-to-market, asset growth, quarterly stock returns, and the 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 and after *MAD* and *TPD* came into force.

We first predict liquidity based on local economic (and political) forces that could reasonably influence the timing of when lawmakers implement the directives and, at the same time have the potential to be correlated with liquidity changes. Specifically, we use election dummies capturing the relative time to local elections as well as several macroeconomic variables, i.e., GDP, inflation, growth, domestic savings, foreign portfolio flows, and the net capital account. These first-stage regressions generate predicted liquidity values conditional on local observables. We then use these predicted values in our base model as dependent variable. Under the alternative explanation that local conditions and shocks induce our results, we should find similar results as before. However, as Column (5) shows, the coefficients on *MAD* and *TPD* are statistically and economically insignificant, and the magnitudes are much smaller than in Table 2. Given that selection on observables does not come close to explaining our findings, selection on (time-variant) unobservables would have to be huge (and at the same time unrelated to the observables) if it were to explain our results. Thus, our last test sends a similar message as the earlier tests (and our arguments based on the institutional setting): selection and endogenous entry-into-force dates are unlikely to be responsible for our findings.

In Appendix B, Table B1, we provide an extensive set of additional sensitivity analyses regarding (i) the clustering of the standard errors, (ii) the choice of the fixed-effects structure, (iii) the composition of the sample, and (iv) the set of control variables. These analyses show that our findings and estimated magnitudes are robust to 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 MAD (CESR 2008).²⁷ (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).²⁸ 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).²⁹ (iv) *Maximum Fine_{TPD}* is the maximum monetary

²⁷ 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.

²⁸ 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?”

²⁹ 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).

penalty that the supervisory authority can impose on security issuers for violations of Articles 4 to 6 of the TPD (CESR 2009a).³⁰ (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. 1.³¹ (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 audit firm, in each EU country.³² 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 2009.³³ Staff numbers are more readily available and easier to compare across countries

³⁰ 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.

³¹ CESR Standard No. 1 comprises 21 principles on how each EU member states should enforce the provision of financial information. In 2009, CESR released a review report on whether or not its principles were implemented. As 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).

³² 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, Appendix A).

³³ 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

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. As before, we 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.

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 *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.³⁴ Consistent with this finding, the directives indeed led to significant changes even in countries with strong prior securities regulation (e.g., the U.K.).³⁵

relative weights of the respective sectors or use information about the allocation of staff in the annual reports. In countries that created separate monitoring bodies to review financial statements (e.g., Germany and the U.K.), we include the staff growth of these bodies as well.

³⁴ We get similar, albeit slightly weaker results if we use the public enforcement index from La Porta et al. (2006) to partition the EU member states. One reason for the slightly weaker results is probably loss of power, as the index is missing for several EU countries.

³⁵ 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).

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.³⁶ 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 suggesting that our standard errors are conservative Footnote 14). 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 or the TPD late, which is inconsistent with the existence of

³⁶ For related insider trading studies, see Bhattacharya and Daouk (2002), and Ackerman et al. (2008).

spillover effects.³⁷ 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 4. In addition, the *lack* of correlation between countries’ implementation timing and the partitioning variables (which we show indeed explain differences in the treatment effects) lends further support to our earlier conclusion that endogenous selection of the entry-into-force dates is unlikely to be responsible for the main results in Table 2.

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 combine the binary *Regulatory Quality 2003* indicator (high vs. low) with each of the eight 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.

³⁷ It is important to note that, without spillover effects, countries’ relative implementation timing should have no implication for the (relative) magnitude liquidity effects 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 magnitude of countries’ liquidity effects.

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 for 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 rank order of the coefficient magnitudes going from the *High RQ/Strong IS* bin to the *Low RQ/Weak IS* bin is generally monotonically decreasing. Moreover, differences in the liquidity effects for countries with strict versus weak implementation are generally larger and primarily significant in countries with high prior regulatory quality. Similarly, holding strict implementation fixed, the directives always have stronger liquidity effects in countries with high prior regulatory quality rather than in countries with low regulatory quality.

The evidence does not support catching-up by 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 have 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 regulation matter a great deal for regulatory outcomes, which clearly illustrates the difficulties of regulatory harmonization.

6. Conclusion

In this paper, we examine 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 are essentially tightening and harmonizing 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. In addition, we examine the role of differences in prior regulation, implementation and enforcement for regulatory outcomes.

Our empirical identification strategy relies on within-EU variation in the dates of when the directives become effective in each country. The staggered imposition of the two directives alleviates common concerns in regulatory studies about concurrent but unrelated economic shocks and endogenous market responses around the introduction of new regulation. In addition, countries' legislative processes that implement the directives into national law are lengthy and inflexible, which mitigates concerns about selection of implementation timing. Thus, our setting affords better identification of the capital-market effects than prior studies, in particular those focusing on a single regulatory act in a single country.

Overall, the results show that stronger securities regulation can have significant economic benefits. Specifically, we find that tighter insider trading and transparency regulation increases the liquidity of firms' share markets. We present extensive sensitivity analyses, including within-country estimation, to gauge our identification strategy and probe whether our results are indeed causal. We also address concerns about the timing of the regulatory effects and the implementation of the directives.

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. Stricter implementation and enforcement of the two directives also result in larger liquidity effects, but these effects exist primarily in countries with strong prior regulatory quality. 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 more willing and better able to implement the new EU directives. Put differently, the same forces that limited the strict regulation in the past appear to be at work when new rules are introduced. It is important to note that these forces could span a wide range, including institutional fit, resource constraints, inefficient bureaucracies, and political pressures, and that our tests cannot distinguish which of these factors drives the results.

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 regulation is implemented and enforced. Thus, policy debates should pay close attention to implementation and enforcement issues 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) effect 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. 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 EU 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.³⁸

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

³⁸ 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.

features in 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.³⁹ 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 EU Parliament passed both directives without substantive amendments and with overwhelming majorities across party lines and nationalities. The EU Parliament adopted the MAD on October 24, 2002 without a roll-call vote.⁴⁰ Thereafter, the

³⁹ See Article 288 of the Treaty on the Functioning of the European Union.

⁴⁰ Only roll-call votes are recorded in the minutes of the meetings of the EU 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 EU Parliament.

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

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. It is therefore 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 a limited window of about two years for implementation. 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).⁴¹ 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.⁴² 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.

⁴¹ 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.

⁴² 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 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.⁴³ 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.⁴⁴ 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

⁴³ IFRS is a set of accounting standards that were adopted by all EU member states (and other countries around the world) as of 2005.

⁴⁴ 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).

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.

Appendix B: Additional Sensitivity Tests

In this appendix, we report a series of robustness checks for the market-liquidity effects presented in Section 4.⁴⁵ We gauge the sensitivity of our findings to (i) the clustering of the standard errors, (ii) the choice of the fixed-effects structure, (iii) the inclusion of non-EU benchmark countries not subject to the directives in the sample, (iv) the use of share turnover as a control variable, 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 (e.g., Western Europe, Eastern Europe, etc.) 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

⁴⁵ We also conduct several robustness checks for the tests in Section 5. We re-run the cross-sectional analyses (1) with standard errors clustered by economic region instead of country, (2) adding separate quarter-year-fixed effects for developed markets, (3) replacing country- and industry-fixed effects with firm-fixed effects, or (4) controlling for macroeconomic factors. The results and inferences are similar to those in Tables 5 and 6.

inferences remain the same as those in our main analysis, although the TPD effect is slightly attenuated with separate quarter-year fixed effects for developed countries. In additional tests (not tabulated), we further include separate volatility coefficients for each quarter, interactions between the country indicators and firm size, or interactions between the country indicators and the industry dummies. Again, the results are similar to those reported in Table 2, and 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 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 when estimating the coefficients on the control variables. To maintain within-EU estimation, we add separate quarter-year fixed effects for the non-EU countries to the model. The results remain virtually the same when including non-EU benchmark countries.

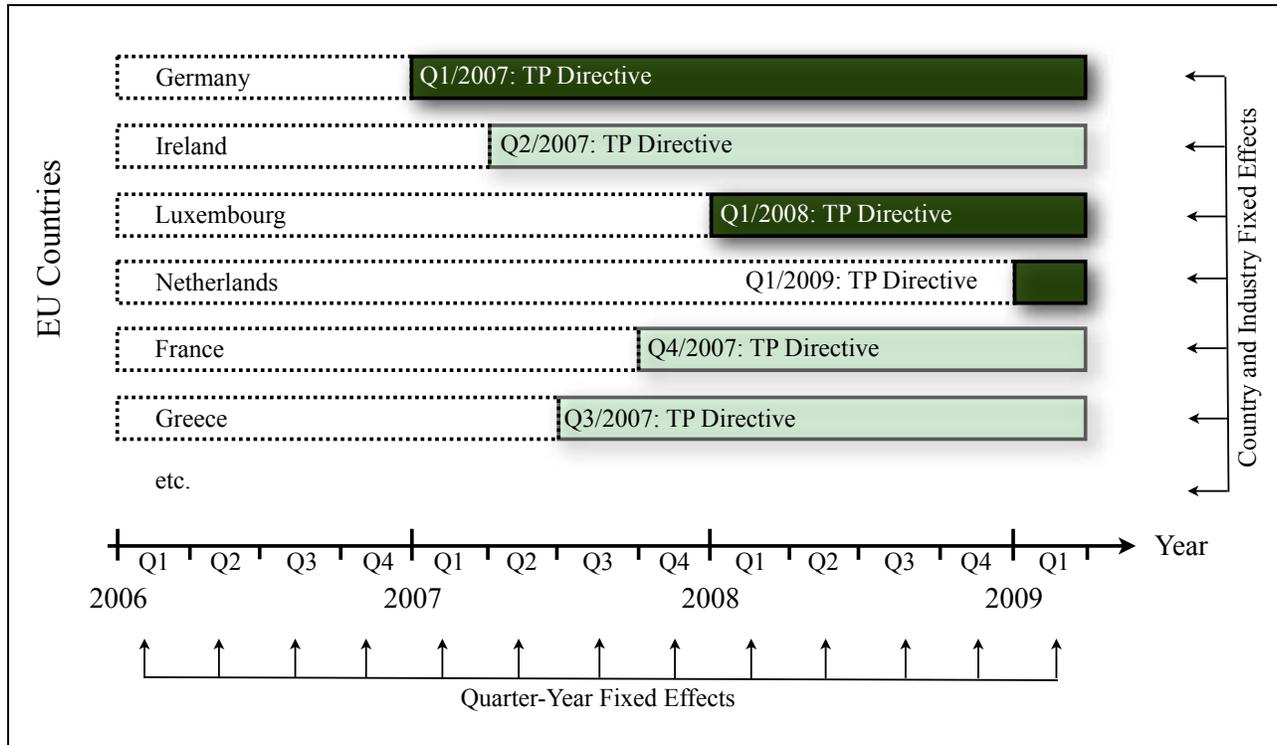
Under the fourth heading in Table B1, we re-estimate our model after excluding *Share Turnover* from the set of controls. The motivation for this robustness check is that turnover is conceptually close to some of the variables that enter the liquidity factor and that it could be viewed as a proxy for liquidity as well. Again, the results are very similar and the inferences remain unaffected.

In untabulated tests, we also consider alternative ways to construct of the liquidity factor. Inferences are unaffected when we re-estimate the liquidity factor using median quarterly spreads and price impact (instead of means), with promax or varimax rotation in the factor analysis, using a winsorized liquidity factor without taking the natural logarithm, or 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 being 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.

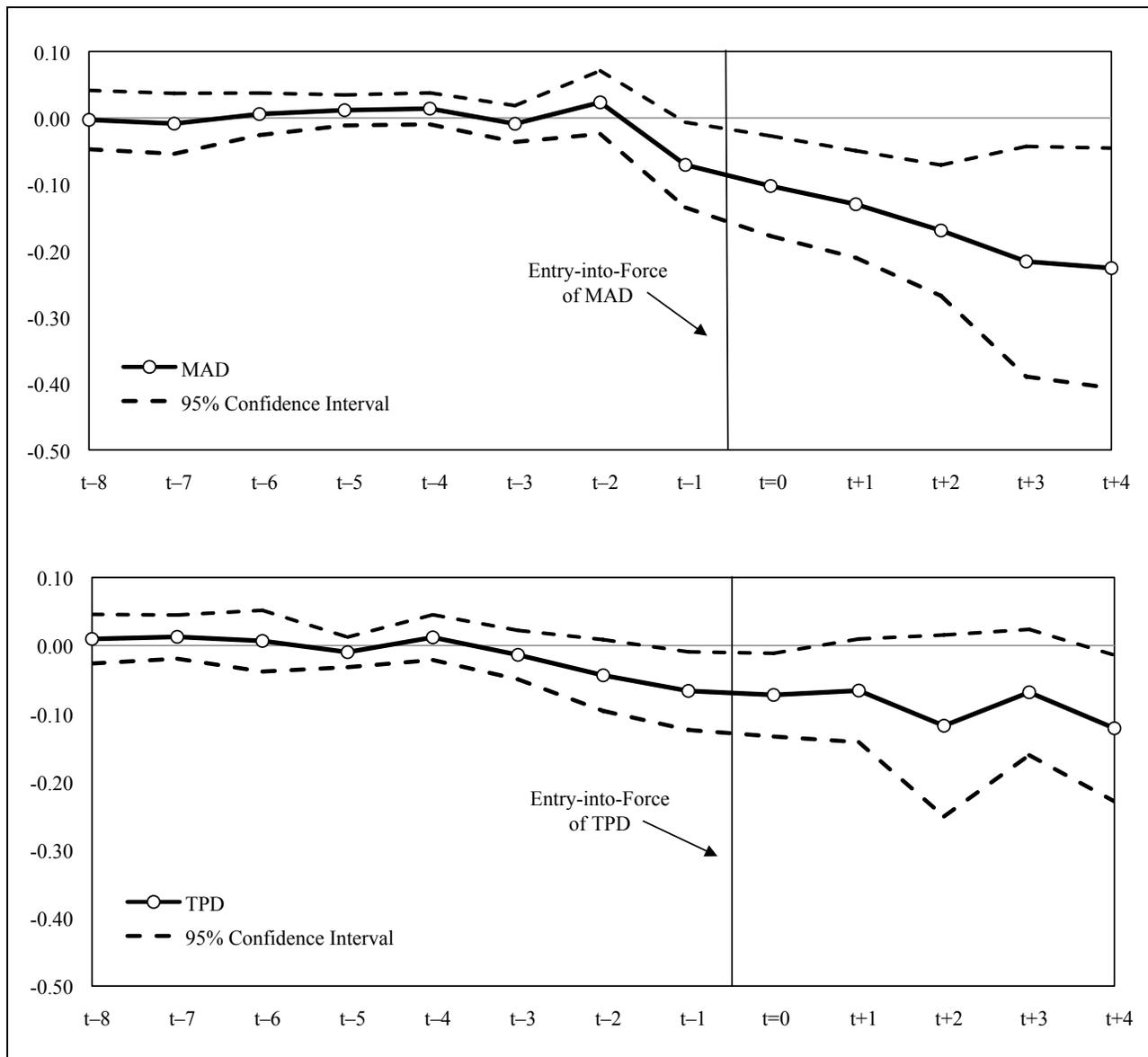
Finally, we examine whether the differential timing of the entry-into-force dates of the MAD and TPD across EU member states creates spillover effects from early countries to late countries. Such spillover effects could in turn induce heterogeneity in the treatment effects. That is, if markets *throughout 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. 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 the directives.

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



The figure illustrates the liquidity patterns around the entry-into-force dates of the Market Abuse Directive (*MAD*) and the Transparency Directive (*TPD*) in event time. To map out the liquidity pattern, we separately estimate a coefficient for each depicted event quarter using Model 4 in Table 2 (i.e., in each specification we include only the treatment observations from the respective quarter as well as all non-treatment observations). We plot the estimated coefficient (together with the 95% confidence intervals) for eight quarters before and four quarters after the entry-into-force quarter ($t=0$) for *MAD* (upper panel) and *TPD* (lower panel). The sample comprises firm-quarter observations from the EU treatment countries between the first quarter 2001 and the second quarter 2011. 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.

Table 1: Sample Composition and Descriptive Statistics*Panel A: Sample Composition and Entry-Into-Force Dates of the MAD and TPD*

<i>Country</i>	<i>Observations (N)</i>	<i>MAD Entry-into- Force Dates</i>	<i>TPD Entry-into- Force Dates</i>
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*

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

<i>(N = 112,260)</i>	<i>Zero Returns</i>	<i>Price Impact</i>	<i>Total Trading Costs</i>	<i>Liquidity Factor</i>	<i>Market Value</i>	<i>Share Turnover</i>	<i>Return Variability</i>	<i>GDP per Capita</i>
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

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

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

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

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

(continued)

Table 3 (continued)*Panel C: Controlling for Various Time Trends and Unobservable Factors*

<i>Ln(Liquidity Factor +1) as Dependent Variable</i>	<i>Controlling for Time Trends</i>		<i>Within-Country Estimation (Germany, Ireland, U.K.)</i>		<i>Falsification Test</i>
	<i>Including Lagged Changes in Liquidity</i>	<i>Including Linear and Quadratic Time Trend Variables</i>	<i>Regulated & Unregulated Markets (All Firms)</i>	<i>Regulated & Unregulated Markets (Matched & Ba- lanced Panel)</i>	<i>Selection on Observables (First Stage Prediction Model)</i>
	(1)	(2)	(3)	(4)	(5)
<i>Test variables:</i>					
MAD	-0.128*** (-4.14)	-0.091*** (-3.75)	–	–	-0.011 (-1.42)
TPD	-0.104** (-2.19)	-0.058* (-1.91)	–	–	-0.011 (-1.04)
MAD _{Regulated vs. Unregulated Firms}	–	–	-0.103*** (-4.00)	-0.093*** (-2.92)	–
TPD _{Regulated vs. Unregulated Firms}	–	–	-0.120*** (-5.07)	-0.072*** (-2.61)	–
<i>Control Variables:</i>					
From Base Specification	Yes	Yes	Yes	Yes	Yes
$\Delta \text{Ln(Liquidity Factor +1)}_{t-8 \text{ to } t-4}$	Yes	–	–	–	–
Country*Time Trend	–	Yes	–	–	–
Country*Time Trend ²	–	Yes	–	–	–
<i>Fixed Effects:</i>					
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

Country	Prior Regulation		MAD Implementation Strength				TPD Implementation Strength				Supervisory Resources		
	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*

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

Panel B: Results for the Transparency Directive

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

<i>Ln(Liquidity Factor +1)</i> <i>as Dependent Variable</i> <i>(N = 112,260)</i>	<i>MAD Implementation Strength (IS)</i>			
	<i>Maximum</i> <i>Fine_{MAD}</i>	<i>Supervisory</i> <i>Powers_{MAD}</i>	<i>Shift in En-</i> <i>forcement_{MAD}</i>	<i>Staff</i> <i>Growth</i>
<i>Regulatory Quality (RQ):</i>				
High RQ/Strong IS	-0.230*** (-3.05)	-0.337*** (-3.39)	-0.239*** (-3.16)	-0.236*** (-2.85)
High RQ/Weak IS	-0.072 (-1.63)	-0.106** (-2.31)	-0.031 (-0.59)	-0.080* (-1.78)
Low RQ/Strong IS	-0.025 (-0.59)	-0.086 (-1.58)	-0.018 (-0.37)	0.076 (1.63)
Low RQ/Weak IS	0.059 (1.23)	0.039 (0.72)	0.034 (0.76)	-0.066 (-1.23)
<i>F-test for Differences across Coefficients (p-value):</i>				
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
<i>Control Variables</i>	Yes	Yes	Yes	Yes
<i>Fixed Effects</i>	Yes	Yes	Yes	Yes

Panel B: Results for the Transparency Directive

<i>Ln(Liquidity Factor +1)</i> <i>as Dependent Variable</i> <i>(N = 112,260)</i>	<i>TPD Implementation Strength (IS)</i>			
	<i>Maximum</i> <i>Fine_{TPD}</i>	<i>Supervisory</i> <i>Powers_{TPD}</i>	<i>Shift in En-</i> <i>forcement_{TPD}</i>	<i>Staff</i> <i>Growth</i>
<i>Regulatory Quality (RQ):</i>				
High RQ/Strong IS	-0.192** (-2.14)	-0.189** (-2.05)	-0.196** (-2.50)	-0.213** (-2.49)
High RQ/Weak IS	-0.064 (-1.21)	-0.076 (-1.60)	0.023 (0.41)	-0.033 (-0.70)
Low RQ/Strong IS	0.022 (0.46)	0.025 (0.51)	0.004 (0.08)	0.118** (1.98)
Low RQ/Weak IS	0.140 (1.48)	0.143 (1.45)	0.035 (0.71)	-0.018 (-0.30)
<i>F-test for Differences across Coefficients (p-value):</i>				
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
<i>Control Variables</i>	Yes	Yes	Yes	Yes
<i>Fixed Effects</i>	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

<i>Country</i>	<i>Date of Initial Draft</i>	<i>Description of Initial Draft Date</i>	<i>Date of Signature or Publication</i>	<i>Entry-into-Force Date</i>	<i>Length of Observable Process (mths)</i>
<i>Market Abuse Directive (Passage at EU Level on 28-Jan-2003):</i>					
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
<i>Transparency Directive (Passage at EU Level on 17-Dec-2004):</i>					
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 of when the initial draft of a law is released by the government 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

<i>Ln(Liquidity Factor +1)</i> <i>as Dependent Variable</i>	<i>N</i>	<i>Market Abuse Directive</i>	<i>Transparency Directive</i>
<i>(1) Alternative Clustering:</i>			
- Two-Way Clustering by Country and Quarter-Year	112,260	-0.115*** (-4.63)	-0.093** (-2.26)
- Clustering by Economic Region	112,260	-0.115*** (-3.92)	-0.093* (-1.94)
<i>(2) Alternative Fixed Effects Structures:</i>			
- Firm-Fixed Effects	112,260	-0.126*** (-5.09)	-0.069** (-2.22)
- Add Separate Quarter-Year-Fixed Effects for Developed Markets	112,260	-0.120*** (-2.62)	-0.056* (-1.84)
- Add Separate Quarter-Year-Fixed Effects Interacted with Firm Size	112,260	-0.114*** (-3.55)	-0.094** (-2.33)
<i>(3) Alternative Sample Specifications:</i>			
- Including Non-EU Benchmark Countries	718,229	-0.117*** (-3.79)	-0.098** (-2.27)
<i>(4) Alternative Control Variables:</i>			
- Not Controlling for Ln(Share Turnover _{<i>t-4</i>})	112,260	-0.121*** (-3.81)	-0.074* (-1.97)

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 add 605,969 firm-quarter observations from 31 non-EU countries (that hence are not subject to the MAD and TPD) as benchmark sample. In this specification we define the quarter-year-fixed effects separately for EU and non-EU countries to allow for different time trends across the two groups. Fourth, we exclude share turnover from the set of control variables (as it is sometimes used as liquidity proxy). 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

<i>Market Abuse Directive</i>					<i>Transparency Directive</i>				
<i>Regulatory Quality 2003</i>					<i>Regulatory Quality 2003</i>				
		Low (a)	High (b)	(a)+(b)			Low (a)	High (b)	(a)+(b)
Late	(i)	9	5	14	Late	(i)	7	7	14
Early	(ii)	6	9	15	Early	(ii)	8	7	15
	(i)+(ii)	15	14	29		(i)+(ii)	15	14	29
		p-value: 0.191					p-value: 0.858		
<i>Maximum Fine_{MAD}</i>					<i>Maximum Fine_{TPD}</i>				
		Weak (a)	Strong (b)	(a)+(b)			Weak (a)	Strong (b)	(a)+(b)
Late	(i)	6	8	14	Late	(i)	7	7	14
Early	(ii)	9	6	15	Early	(ii)	8	7	15
	(i)+(ii)	15	14	29		(i)+(ii)	15	14	29
		p-value: 0.356					p-value: 0.858		
<i>Supervisory Powers_{MAD}</i>					<i>Supervisory Powers_{TPD}</i>				
		Weak (a)	Strong (b)	(a)+(b)			Weak (a)	Strong (b)	(a)+(b)
Late	(i)	9	5	14	Late	(i)	7	7	14
Early	(ii)	9	6	15	Early	(ii)	9	6	15
	(i)+(ii)	18	11	29		(i)+(ii)	16	13	29
		p-value: 0.812					p-value: 0.588		
<i>Shift in Enforcement_{MAD}</i>					<i>Shift in Enforcement_{TPD}</i>				
		Weak (a)	Strong (b)	(a)+(b)			Weak (a)	Strong (b)	(a)+(b)
Late	(i)	8	6	14	Late	(i)	9	5	14
Early	(ii)	7	8	15	Early	(ii)	8	7	15
	(i)+(ii)	15	14	29		(i)+(ii)	17	12	29
		p-value: 0.573					p-value: 0.550		
<i>Staff Growth</i>					<i>Staff Growth</i>				
		Weak (a)	Strong (b)	(a)+(b)			Weak (a)	Strong (b)	(a)+(b)
Late	(i)	7	7	14	Late	(i)	5	9	14
Early	(ii)	7	8	15	Early	(ii)	9	6	15
	(i)+(ii)	14	15	29		(i)+(ii)	14	15	29
		p-value: 0.858					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.