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

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Abstract

This paper examines capital market effects of changes in securities regulation. We analyze two key directives in the European Union (EU) that tightened market abuse and transparency regulation and its enforcement. All EU member states were required to adopt these two directives but did so at different points in time. Our research design exploits this differential timing of the same regulatory change to identify the capital-market effects. We then use cross-sectional variation in the strictness of implementation and enforcement as well as prior regulation to analyze the role of these factors for regulatory outcomes. We find that, on average, market liquidity increases as EU member states 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 track record of implementing regulation and government policies in general. These findings show that the effects of regulation depend crucially on implementation and enforcement. Moreover, the results indicate that the same forces that have limited the effectiveness of regulation in the past are still at play when new rules are introduced, which has important implications for the expected outcomes of regulatory reforms as well as efforts to harmonize regulation across countries.

JEL classification: F30, G15, G18, G30, K22, M41

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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 could play an important role for the effects of regulatory changes. Prior studies typically focus on regulatory changes in a single country (e.g., the Sarbanes-Oxley Act in the U.S.; henceforth SOX) for which it is difficult to separate these factors: prior regulation, new rules, implementation, and enforcement are essentially a bundle and 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 much better identification of the regulatory effects than a single regulatory event such as SOX. Second, as EU directives apply to all member states, the regulatory act 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 implementation and enforcement separately from the rule change. Third, we study key elements of securities regulation: the Market Abuse Directive (MAD) addresses insider trading and market manipulation and the Transparency Directive (TPD) addresses corporate reporting and

[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).

disclosure. However, as there were prior EU directives and national laws banning insider trading and stipulating extensive reporting requirements, the new directives are largely geared towards tightening securities regulation, in particular, by improving supervisory regimes in the EU. To illustrate, the TPD requires member states to have a supervisory authority that, among other things, reviews firms' financial statements on a regular basis and takes actions against discovered infringements, but it makes few changes to reporting requirements for EU companies. This feature reinforces the enforcement focus of our study. Fourth, the EU setting allows us to analyze the role of prior regulation, that is, the interaction between initial conditions and new regulation. One hypothesis is that countries with weaker prior regulation benefit more from the new directives as they still have to catch up. An alternative hypothesis is that the same forces and constraints that limited the effectiveness of securities regulation in the past (e.g., political resistance, inefficient bureaucracies) are still at play when new rules come into force. This hypothesis implies that history matters in the sense that there is hysteresis in regulation. It also implies that the same regulation can yield very different outcomes across countries and, as result, imposing the same regulation on disparate countries can make them drift further apart, rather than move together.

We analyze changes in stock market liquidity around the staggered implementation of the two EU directives. As market liquidity can be measured over relatively short intervals and is also less anticipatory in nature than other constructs that could be used to evaluate securities regulation (e.g., the cost of capital), it is a well suited outcome variable for our setting and identification strategy. We measure liquidity using the bid-ask spread and the percentage of zero-return days. Both proxies have been used extensively in the literature. We also examine changes in the cost of capital to corroborate the liquidity analysis.

We estimate quarterly panel regressions from 2001 to 2009 using EU and non-EU benchmark firms as well as introducing quarter-year, country and industry fixed effects. Given the staggered implementation of the directives across 29 countries, we introduce separate quarter-year fixed effects for the EU countries to account for common shocks to and trends in EU capital markets. Thus, our identification of the regulatory effects comes entirely from within-EU variation in the dates of when the directives become effective. This design addresses two common concerns about regulatory studies (e.g., Mulherin, 2007), i.e., that the results reflect a contemporaneous economic shock that is unrelated to the regulation or that the results reflect a market response to events (such as a scandal) that gave rise to the regulatory act instead of the regulation itself. In our setting, such shocks or events would have to line up with the implementation dates of the 29 treatment countries to bias our estimates.

Using this setting and design, we find that market liquidity increases when new market abuse (MAD) and transparency (TPD) regulation come into force in EU member states, using either bid-ask spreads or the percentage of zero-return days. The liquidity improvements are economically significant. Relative to the pre-directive median liquidity level, our estimates suggest liquidity improvements around 14 to 16 percent for both directives. In our sensitivity analyses, we also find evidence that the cost of capital decreases when the directives come into force. In sum, our results suggest that improving key elements of securities regulation leads to substantial capital-market benefits.

We conduct extensive sensitivity analyses and show that our results, among other things, are not driven by a few large countries and are robust to the introduction of firm-fixed effects, separate quarter-year fixed effects for developed (versus developing) countries, controls for differences in the composition of firms across countries (e.g., by size or industry), controls for

other EU directives as well as controls for macroeconomic changes. To further gauge our identification strategy, we conduct four analyses. First, we analyze the liquidity patterns around the directives and separately estimate the effects for the year prior to MAD and TPD, the year of their implementation, and the period afterwards. We find that liquidity is not significantly higher in the year leading up to the directives, that liquidity improves after MAD and TPD come into force and that it remains at a higher level for the remainder of the sample period. Second, we counterfactually shift the 'true' implementation dates for the directives quarter-by-quarter and each time re-estimate the liquidity regressions. We find that the coefficients for the directives quickly become smaller in magnitude and significance as we move away from the true implementation dates, which is what we expect to see if the directives drive the effects. Third, we conduct placebo analyses by randomly assigning implementation dates during the pre-treatment period from 2001 to 2004. The average effect in the placebo regressions is close to zero and the regressions rarely produce coefficients that are of the same magnitude as our estimated treatment effects.[2] Finally, we benchmark the liquidity effects for our sample against firms trading on "unregulated" EU markets that are not subject to the two EU directives, which amounts to within-country estimation. That is, unregulated firms control for concurrent changes in liquidity that are specific to individual EU countries, correlated with the entry-into-force dates, yet unrelated to the directives and its regulatory effects. We find that the liquidity effects around the two directives occur primarily for regulated firms, and are stronger than for unregulated firms, consistent with a causal interpretation of the link between regulation and market liquidity.

Next, we turn to the cross-sectional analyses exploiting differences in prior regulation and in implementation and enforcement of the directives across EU countries. We document that the

[2] We can also use the distribution of coefficients generated by the placebo regressions to bootstrap our standard errors. Based on this, our inferences using standard errors clustered by country appear to be conservative.

liquidity effects of the two directives are stronger in countries with higher prior regulatory quality.[3] Similarly, we find that countries with higher prior staff levels at the securities regulators exhibit larger liquidity effects. One explanation for these findings is that countries that put more resources into the implementation and enforcement of securities regulation and that have a better track record of implementing and enforcing regulation and government policies in general are more willing and better able to implement the new EU directives. Put differently, the same forces that limited the strength of securities regulation in the past appear to be at work when implementing new rules. It is important to note that these forces could span a wide range, including institutional fit, resource constraints, inefficient bureaucracies, and political pressures.

To examine differences in implementation and associated regulatory outcomes, we create specific measures of how well EU countries implement and enforce the new directives using data on supervisory powers, penalties, enforcement actions and a self-constructed survey of securities regulators and auditors. We also use staff growth at the securities regulator around the implementation of the directives as a measure for the extent to which countries commit resources to support the new regulation. We then analyze whether these implementation and resource-based measures explain differences in the liquidity effects around MAD and TPD. Our results are consistent across measures and show that countries with stricter implementation and enforcement experience significantly larger capital-market effects. Next, we condition on both prior regulation and the measures for implementation and enforcement strength. We document that the liquidity effects around MAD and TPD are strongest in countries with high past regulatory quality *and* strong implementation. We do not find that liquidity increases for countries with weak prior regulation and weak implementation. Moreover, stricter

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

implementation of the new directives often has an incremental effect, but primarily in countries with high past regulatory quality or in countries with high prior staff levels at the regulators. Thus, there is strong evidence of hysteresis. Countries with weaker securities regulation do not catch up with stronger countries following the new EU directives. In fact, our results imply that the two EU directives had the opposite effect. The more general conclusion from this paper is that imposing the same regulation on countries with disparate prior conditions can have the effect of making countries diverge more, not less, illustrating the difficulty of harmonizing countries through regulatory reforms.

Our paper makes several contributions to the literature. First, we show that the imposition of stronger securities regulation on firms can indeed have significant economic benefits in terms of market liquidity (and also cost of capital) for a broad cross-section of firms. Prior studies have 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; Gintschel and Markov, 2004; Francis et al., 2006; Gomes et al., 2007) and on SOX (e.g., Chhaochharia and Grinstein, 2007; Zhang, 2007; Li et al. 2008) is decidedly mixed and often emphasizes the costs of securities regulation. As pointed out by Mahoney and Mei (2009), the early studies examining securities regulation in the 1930s often lack a (convincing) control group. Similar concerns apply to the studies on Regulation Fair Disclosure (Collver, 2007) and on SOX (Leuz, 2007; Hochberg et al., 2009) as they affected all SEC registrants. To get around this issue, Bushee and Leuz (2005) and Greenstone et al. (2006) exploit extensions of U.S. securities regulation to particular market segments with smaller firms using larger unaffected firms as a benchmark. Iliev (2009) employs

a regression discontinuity design around the SOX Section 404 compliance cutoff, which is based on size. The return-based evidence in Bushee and Leuz (2005) and Iliev (2009) suggests that, for their samples of small firms, the costs of securities regulation exceed the benefits, while the abnormal returns documented in Greenstone et al. (2006) suggest significant benefits. In contrast, our study is not limited to a segment of smaller firms. Moreover, we identify the effect between stricter securities regulation and increased liquidity of share markets using the staggered imposition of two EU directives in 29 countries. The staggered design not only exploits a regulatory act that is exogenous at the firm level but also alleviates many concerns that typically arise in studies relying on a regulatory act in a single country, particularly, about endogenous market responses and unrelated concurrent shocks.[4]

Second, we show that regulatory outcomes depend on the strength of prior regulation and on countries' ability and willingness to implement and enforce new securities regulation. These findings highlight the role of implementation and enforcement of regulation and document substantial hysteresis in regulation. 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 also 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

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

analysis goes beyond insider trading regulation. Moreover, prior evidence on securities law enforcement is typically based on ex-post measures, i.e., complaints, lawsuits, enforcement actions. The EU setting allows us to provide evidence on the capital-market effects associated with regulatory changes in the design of enforcement regimes and in supervisory resources.[5] We show that tighter securities regulation has immediate capital-market effects (even before the first enforcement action) when countries improve their supervisory and enforcement regimes.

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 our analyses and results. Section 5 concludes.

2. Conceptual Underpinnings, Hypotheses and Institutional Setting

In raising external capital, firms need to reassure outside investors. If outside investors have doubts whether firms will return their money, they are unlikely to provide funds in the first place (leading to low market liquidity) or, if they provide capital to firms, they are likely to demand a higher return (leading to a higher cost of capital for firms' investments). 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, for instance, by improving market liquidity or reducing firms' cost of capital.

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

[5] In this sense, our study is also related to Coffee (2007) and Jackson and Roe (2009). Both studies point to the association between capital-market outcomes and the level of enforcement staff and budgets.

securities regulation.[6] 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.[7] 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).

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 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 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.[8] 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,

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

7 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 IOSCO assessments.

8 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).

there are several reasons to be skeptical about the benefits of securities regulation. Consistent 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.[9] 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 countries 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 likely larger (Shleifer, 2005; Bhattarcharya and Daouk, 2009). In addition, a 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 ultimately depend on its implementation and enforcement, and not just its design.

This discussion provides the conceptual underpinnings for our empirical analysis. We recognize that our study cannot provide evidence that securities regulation or even a particular regime of securities regulation is socially desirable. But by analyzing capital-market effects around changes in securities regulation, we can provide evidence whether securities regulation has economic benefits (e.g., improves market liquidity). We can also shed light on the tradeoffs that we discussed as well as 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 directive into national law and its supervision,

[9] There is some evidence from different countries (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 securities regulation.

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 cross-sectional variation with respect to implementation and enforcement. If securities regulation has beneficial capital-market effects, these implementation and enforcement differences across countries are expected to produce cross-sectional variation in regulatory outcomes. 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 regulation that we can exploit in the analysis. There are two competing hypotheses. One prediction is that the effects are larger in countries where prior securities regulation has been weak and hence countries have more room to catch up (*catching-up hypothesis*). An alternative prediction, which follows from our earlier discussion in this section, is that the capital-market effects of the new directives are larger in countries with stronger past regulation, leading to path dependence (*hysteresis hypothesis*). This hypothesis recognizes that prior regulation likely reflects various market, institutional 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 these forces are at play when new regulation is introduced, the likely outcome is hysteresis, rather than catching up.

To explore these hypotheses, we examine the Market Abuse Directive (MAD), which covers insider trading and market manipulation, and the Transparency Directive (TPD), which addresses reporting requirements and disclosure regulation. Both directives 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, and they address what are generally considered to be key

elements of securities regulation. As there is 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 MAD and TPD required amending national law(s) in all member states. Below we provide a brief description of each directive. More details are provided 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, it establishes transparency standards requiring people who recommend investments to disclose their relevant interests. It also requires each member state to have a supervisory authority that is responsible for monitoring insider trading and market manipulation and to give this authority the necessary supervisory and investigative powers.[10] 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

[10] 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 also 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. See Appendix A for further examples.

regulated information. Regulated information comprises periodic financial reports, information on major holdings of voting rights and other required disclosures. However, prior EU directives, member state laws and exchange requirements already required annual and interim financial reports as well as other ongoing information. As such, the TPD does not expand existing disclosure requirements in most areas but rather focuses on (better) 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 regulated and disclosed information (e.g., firms' financial statements). Such a review process did not exist in many countries and was introduced (or expanded) following the TPD.[11] The TPD also requires that the authority is given appropriate enforcement tools, including the power to carry out on-site inspections.[12] Thus, the TPD primarily clarifies and harmonizes existing disclosure regulation and improves enforcement.

3. Research Design and Data

3.1. Identification Strategy and Empirical Model

We test the hypotheses developed in Section 2 using a panel dataset with firm-level, quarterly observations of stock market liquidity. We focus on market liquidity for two 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). Second, a stated goal of both EU directives was to increase market confidence, which is closely related to market

[11] For instance, in the U.K., the supervisory authority charged with enforcing financial reporting requirements (Financial Reporting Review Panel) began reviewing financial statements proactively on a sample basis rather than only on the basis of a referral.

[12] 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).

liquidity (e.g., Lamfalussy, 2000; Enriques and Gatti, 2008; CRA, 2009).[13] The choice of quarterly data reflects a tradeoff between reliably measuring liquidity over some interval and capturing changes in liquidity in a timely fashion, i.e., when MAD and TPD come into force.

Our strategy to estimate the effects of securities regulation on market liquidity relies on two features. First, we examine regulatory changes in the EU. This feature controls for time-invariant differences across countries by using them as their own control. However, a common concern about studies exploiting a regulatory change 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. Moreover, new regulation is often put in place after major economic events such as a crisis or scandal. It is possible that markets respond to these economic events that precede the regulatory change rather than the new regulation itself (e.g., Ball, 1980; Mulherin, 2007). To alleviate these concerns, our second design feature exploits the staggered imposition of the two directives across EU member states. After the enactment of an EU directive, each member state must transpose the directive into national law within a specified timeframe. This process depends on a country's constitution and its legislative system, and varies considerably across member states. As a result, the new EU directives come into force at different times across EU countries. We exploit this variation to isolate the effect of the two EU directives on the capital-market variables (see Figure 1 for an illustration). In essence, we can (repeatedly) analyze the regulatory effect of each directive for each of the EU member states at different points in time, which makes our analysis much less prone to the typical concerns described above. It also helps that the regulatory act takes place at the EU level and hence is not

[13] In fact, there is a third reason relative to other capital-market proxies (e.g., the cost of capital). Liquidity proxies are less likely to anticipate regulatory changes because the regulatory regime matters primarily if and when investors trade. It is of course possible that investors anticipate at the time they buy shares that future regulatory changes improve adverse selection in the future and hence liquidity at the time they sell, but this anticipatory effect is likely to be small. We provide evidence in Section 4 that is consistent with this claim.

specific to a particular country. Moreover, the legislative process is often lengthy and generally inflexible.[14] The latter makes it implausible that politicians or bureaucrats can deliberately time the entry-into-force dates to occur when liquidity in the country is high, low or exhibits certain trends, which in turn alleviates concerns about reverse causality.[15]

We estimate the following model (without firm and time subscripts):

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

The dependent variable, *Liq*, stands for the liquidity proxies. *MAD* and *TPD* are binary variables 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-level and country-level control variables. $Fixed\ Effects_i$ represents country, industry, and *separate* quarter-year fixed effects for EU and non-EU countries.[16] As the variables of interest vary only at the country level, we draw statistical inferences based on standard errors clustered by country.

Given the EU-quarter fixed effects, our identification stems from within-EU variation in the dates when the directives become effective. This specification eliminates variation or shocks to the capital-market variables that are common to all EU member states in a given quarter.[17] Thus, for unrelated economic shocks to create spurious results in our setting, they would have to be correlated with the directives' entry-into-force dates in the respective countries. We are not

[14] For similar arguments, 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 banking integration and the second paper uses the FSAP transposition dates as instruments to estimate the effects of financial integration on international business cycle synchronization.

[15] However, the timing may also reflect a country's willingness to implement the new directives. We will come back to this issue and exploit countries' relative implementation timing in our analysis. See, e.g., Table 4.

[16] We include a benchmark sample comprising observations from non-EU countries, which are unaffected by the introduction of MAD and TPD. Their inclusion helps us control for worldwide changes and general trends in market liquidity (and to better estimate the coefficients for the control variables).

[17] The introduction of EU-specific quarter-year fixed effects is very demanding and could capture some fraction of the treatment effect, particularly if there is clustering of the implementation dates across countries, if the dates are measured with noise or if the directives have a more gradual rather than a sharp effect. We therefore assess our identification strategy and the choice of implementation dates in Section 4.2.

unaware of a specific concern along these lines. Nevertheless, to address this concern, we perform sensitivity analyses benchmarking against firms trading on unregulated markets to which the EU directives do not apply, which essentially amounts to within-country estimation.

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 MAD (April 2004) and TPD (January 2007). The sample period ends in the second quarter of 2009, which is the most recent quarter for which we have the necessary data. We include all the firm-quarter observations from EU and non-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 EU country.[18] The bid-ask spread (zero returns) sample comprises 611,969 (780,434) firm-quarter observations from 25 (27) EU countries and 27 (35) non-EU countries. We exclude firms that follow U.S. GAAP in their financial reporting and firms with a U.S. cross-listing as they are also subject to insider trading and transparency rules in the U.S. In addition, we eliminate very small firms with market values below US$ 5 million as well as firms trading on EU markets not subject to the MAD and TPD (e.g., the Alternative Investment Market in London). Our final sample selection criteria are to require at least four valid quarterly observations per firm, and to only include benchmark countries with more than 20 firms.

[18] 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 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 MAD and TPD) upon joining the EU in January 2007. However, the results are not sensitive to either of those sample choices (see also the sensitivity analyses in Appendix B).

Panel A of Table 1 also lists the date when the national law(s) that implemented MAD and TPD came into force and hence the respective directive is applicable 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 respective directive. 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 timing of the implementation is at the core of our identification strategy. To estimate Eq. (1), we create two binary indicators, *MAD* and *TPD*, that are set equal to '1' beginning in the quarter of the country-specific MAD or TPD entry-into-force dates.

We use two proxies for market liquidity. Our first proxy, the *Bid-Ask Spread*, is conceptually close to the desired construct and commonly used in empirical studies 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 spread as the difference between the two prices divided by the mid-point. We then take the median daily spread over the quarter for a given firm. Our second proxy, *Zero Returns*, is the proportion of trading days with zero daily stock returns out of all potential trading days per quarter. It is also commonly used but more widely available than spreads because it relies just on return data (e.g., Lesmond et al., 1999; Bekaert et al., 2007). In the liquidity regressions, we follow prior literature and control for firm size using the market value of equity, share turnover, and return variability (Chordia et al., 2000; Leuz and Verrecchia, 2000). We estimate the bid-

ask spread regressions in a log-linear form using the natural logarithm of the bid-ask spreads and the control variables, and lag the control variables by four quarters. Price and volume data are from Datastream.[19] Except for variables with natural lower and upper bounds, we truncate all variables at the first and 99th percentile. Panel B of Table 1 provides descriptive statistics of the dependent and independent variables as well as further details on the variable measurement.

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

4.1. Average Liquidity Effects

As described in Section 2, the MAD and TPD are an attempt to tighten and harmonize EU securities regulation, particularly, with respect to the implementation and enforcement of existing key regulations. We use cross-sectional, time-series panel regressions at the firm level that benchmark EU firms after the MAD and TPD came into force against their own history before the introduction of the two directives and against a global sample of non-EU firms that are not subject to the new directives. Table 2 presents results from OLS regressions of Eq. (1) estimating the average liquidity effects of the two directives.

We estimate the effects for each of the two EU directives separately and then combine MAD and TPD in one model. As is common for these models and given our extensive fixed-effects structure, the explanatory power of the regressions is high with an R^2 of 76 percent for bid-ask spreads and 53 percent for zero returns. The firm-specific control variables are highly significant and exhibit the expected signs. Large firms and firms with a high share turnover have lower bid-ask spreads and a lower proportion of zero returns. Firms with more volatile returns have higher

[19] 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. For U.S. firms, we add spread data from CRSP because Datastream does not have this data in the early years of our sample period. Doing so does not materially affect our results.

spreads and a lower proportion of zero returns. The negative association between return volatility and zero returns follows from the fact that low return volatility often means a high frequency of zero return days. We note, however, that excluding return volatility (or any other control variable) does not affect the results.

For our test variables, we find that the coefficient on *MAD* is negative and statistically significant for both liquidity proxies. The estimated effects are also economically significant and similar for both proxies. In the bid-ask spread regressions, a coefficient of -0.177 suggests that, on average, bid-ask spreads decrease by 16 percent, which equals a reduction of 31 basis points relative to the pre-directive median of 193 basis points.[20] In the zero return regressions, a coefficient of -0.043 suggests a 15 percent decrease in the proportion of zero returns compared with the pre-directive median. The coefficients on *TPD* are also negative. An estimate of -0.305 indicates a reduction in bid-ask spreads by 26 percent, while an estimate of -0.040 translates into a reduction in the proportion of zero returns of 14 percent. *MAD* and *TPD* remain significant and of similar magnitude when we introduce the variables jointly into the model.

4.2. Assessing Identification and Within-Country Estimation

Our goal is to estimate the causal effect of securities regulation on market liquidity. Towards this end, we exploit within-EU variation in the entry-into-force dates for two EU securities regulation directives. Given this strategy, it is important to assess whether these dates provide reasonably sharp identification. Moreover, while our empirical model eliminates common shocks to EU market liquidity over the sample period, bias could come from omitted variables that are correlated with the within-EU variation in the entry-into-force dates and increases in market liquidity. We are unaware of a specific concern of this nature but

[20] To gauge the economic magnitude we compute the average percentage change in bid-ask spreads as $e^{-0.177} - 1 = -0.162$.

acknowledge the possibility of unspecified trends or changes in EU market liquidity that are unrelated to the directives yet correlated with the entry-into-force dates. To gauge these two issues, we conduct four additional tests.[21]

First, we introduce three separate (non-overlapping) indicator variables into the model, one for the year prior to the directives (t-4 to t-1), one for the year of the implementation (t to t+3), and one for the years afterwards (t≥+4). The purpose of this analysis is to see whether liquidity is already elevated ahead of the directives (in event time) and whether the effect is sustained. For both liquidity measures, we find that the indicators variables marking the year leading up to the directives are not significant, but the indicator for the year MAD and TPD come into force is significant. An F-test for the differences across the two coefficients confirms that they are different from each other. We also find that liquidity remains at significantly higher levels in the years after the implementation of the directives, consistent with the effects being persistent. Thus, the increase in liquidity appears to occur right around the implementation and does not seem to reflect gradual trends in the EU countries.

Due to collinearity and ensuing variance inflation, we cannot use a finer time-period partitioning around the directives. Instead, we use a second test to gauge the relevance and sharpness of the entry-into-force dates and report it in Panel A of Table 3. We shift the assignment of the implementation dates quarter-by-quarter for all EU countries, and each time re-estimate the regression model noting the coefficient on the respective directive. If the liquidity effects are indeed related to the implementation of the directives, the estimated

[21] In untabulated tests, we also conduct a placebo analysis by randomly assigning implementation dates to the EU countries 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 300 replications, the estimated average placebo effect for *Bid-Ask Spreads* is close to zero (-0.0017) and there are only 3 (0) cases for which the regressions produce a coefficient that is more negative than the *MAD* (*TPD*) coefficient reported in Table 2. The results for *Zero Returns* are similar. The placebo analysis also provides a sensitivity check on our inferences as this procedure amounts to bootstrapping the significance levels of the coefficients.

coefficient should be attenuated as we move away from the true implementation dates. The decline of the treatment effect should go both ways, as we move our assignment ahead of or beyond the true implementation date. By shifting the date back in time, we essentially assign non-treatment quarters to the treatment period, while moving the date forward assigns treatment quarters to the pre-directive period. As Panel A shows, the coefficients on *MAD* "peak" close to the true entry-into-force dates and become smaller in magnitude (and insignificant) as we shift the assignment of the implementation dates away from the true dates. This pattern is comforting because it suggests that the implementation dates are indeed critical for our findings. It also confirms that the liquidity regressions do not suffer from significant anticipation problems. The pattern for *TPD* looks similar using zero returns. For the spreads, the TPD effect appears delayed, i.e., coefficients continue to decline beyond the true entry-into-force dates before reversing (after quarter t+4). Possibly, it takes time until the TPD becomes fully effective.[22] The more likely explanation is that, as TPD occurs late in our sample period, countries with late implementation dates drop out of the sample as we shift dates further and further out. As we show later in Section 5, these countries have smaller effects around the directives, which makes the average response *appear* to be increasing as we shift the dates.

Third, we construct a separate sample of EU firms trading on one of the alternative (unregulated) markets in the EU, which are not subject to the MAD and TPD.[23] Unless there are

[22] Our surveys of regulators and auditors support the notion that enforcement activities under the TPD were gradually implemented after the entry-into-force dates. For instance, setting up a review and monitoring process for financial information requires hiring and training additional enforcement personnel. Descriptive evidence on reviews in Germany shows that it may take a year or two before the enforcement agency gets to steady state (Ernstberger et al., 2010).

[23] We identify the firms trading on unregulated EU markets by searching the websites of all European exchanges. If we cannot find data on the websites, we contact the exchanges directly to obtain a list of constituent firms. This procedure identifies 32,124 (35,058) firm-quarter observations from unregulated markets with spread (zero returns) data. However, these lists often identify constituents only at certain points in time and hence are imperfect. To eliminate potentially unregulated firms, we delete firms with a market capitalization under US$

some spillover effects (e.g., through competition or voluntary adoption of best practices), unregulated firms should not be affected by the two EU directives. Thus, we can benchmark the liquidity effects for regulated firms around the introduction of MAD and TPD against this control group that operates in the same countries and economic environments, which amounts to within-country estimation.[24] We augment the model in Eq. (1) and include a binary variable indicating unregulated firms in the post-MAD or post-TPD period, respectively. In Panel B of Table 3, we report results from this augmented model (using the worldwide and the EU sample only). For both liquidity variables, the coefficients on *MAD* and *TPD* for firms trading in regulated markets are negative and significant. In contrast, the coefficients on *MAD* and *TPD* for firms on unregulated markets are never significantly negative and, with one exception, close to zero in magnitude. F-tests for the differences in the coefficients indicate that the liquidity effects differ significantly across regulated and unregulated firms. Thus, liquidity improves only for firms that are subject to the new EU directives, which provides further support for a causal effect of securities regulation on market liquidity.

Finally, we address concerns about the influence of other EU regulations. As explained in Section 2, the MAD and the TPD are part of the EU's Financial Services Action Plan which contains sixteen detailed pieces of regulation. To assess whether other major directives affect our analysis, we augment the empirical model by indicators for the Prospectus Directive (*PROSP*) and the Markets in Financial Instruments Directive (*MiFID*), which came into force during the sample period and together with the MAD and TPD form the four Lamfalussy Directives. The results are reported in Panel C of Table 3. Consistent with Cumming et al.

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20 million from our sample of *regulated* EU markets. This cut-off is below the median market value of firms trading on *unregulated* EU markets.

[24] An alternative way to implement within-country estimation is to introduce country-quarter fixed effects and to estimate the differential liquidity changes around MAD and TPD for regulated and unregulated firms. Results from such regressions are similar to those reported in Table 3, Panel B and the inferences are the same.

(2011), we find negative coefficients indicating an increase in liquidity around MiFID. But the effect is only close to conventional significance levels and disappears when we jointly introduce all four Lamfalussy Directives in the model. The results for the Prospectus Directive differ in sign and magnitude across the two liquidity proxies and are likely spurious. More importantly, the coefficients on *MAD* and *TPD* remain significantly negative and have a similar magnitude as before when we control for these other EU directives.[25]

We conduct a series 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 benchmark and the treatment sample, (iv) the inclusion of additional control variables, and (v) alternative dependent variables such as the cost of capital. Since these analyses do not alter the conclusions drawn from our main tests, we report and discuss them in Appendix B.

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

5.1. Using Cross-Sectional Differences and Partitioning Variables

Up to this point, we analyzed whether the imposition of tighter securities regulation has an effect on market liquidity. In this section, we examine the role of differences in prior regulation as well as in implementation and enforcement for the magnitude of this effect. For the reasons discussed in Section 2, it is unlikely that the two directives have uniform effects throughout the EU. For instance, one could argue that countries with a proven track record of implementing regulation and government policies are better able to implement new regulation in an effective manner. Furthermore, it is plausible that, by improving the enforcement regime, the new directives complement existing securities regulation and hence benefit mostly countries with

[25] In untabulated tests, we also control for two other major regulatory changes during the sample period, namely the Takeover Directive and the adoption of International Financial Reporting Standards IFRS by listed companies in the EU. Both changes have no material effect on the reported *MAD* and *TPD* coefficients.

extensive regulation. Alternatively, one could argue that countries with weaker securities regulation benefit the most from harmonized, EU-wide efforts to improve extant regulation.

To explore these arguments and to test for cross-sectional differences in the liquidity effects, we introduce two partitioning variables, one for high levels and one for low levels, into the base specification. This extension of Eq. (1) leads to the following empirical 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 equal to '1' for either the group of EU countries with a high or a low level of a specific implementation or enforcement characteristic, and to '0' otherwise. We then test for significant differences between the coefficients β_1 and β_2 using a Wald test to assess whether the effects of the two directives on liquidity differ within the EU. As before, we use *Bid-Ask Spreads* and the proportion of trading days with *Zero Returns* as the dependent variables. We use the same set of firm-level control variables and fixed effects as in Table 2, and compute the t-statistics based on standard errors clustered by country.

We employ various country-level characteristics to partition EU countries with regard to (i) the quality of prior regulation, (ii) the strength of implementing the MAD and TPD, (iii) the timing of implementing the MAD and TPD, and (iv) the existence of and change in supervisory resources for securities regulation. Table 4 provides an overview of the partitioning variables used in the analyses in this section (together with the binary indicator variables) by EU country.

The first dimension we examine is the quality of prior regulation. We partition the data based on 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. For

the analyses, we split the treatment sample countries by the EU median in 2003, that is, before the two directives came into force.

Next, we examine the differential strength of implementing and enforcing the MAD and TPD across the EU member states. We create six directive-specific variables, three for the MAD and three for the TPD: (i) *Maximum MAD Fine* is the maximum monetary penalty that the supervisory authority can impose on security issuers for violations of Article 2 of MAD (CESR 2008).[26] (ii) *Supervisory Powers* 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 MAD into local law (CESR 2007).[27] Higher values imply more supervisory powers. (iii) *Action Taken by 2009* indicates EU countries that have taken at least a single enforcement action regarding violations of the MAD by 2009 (e.g., imposed a fine).[28] (iv) *Maximum TPD Fine* is the maximum monetary penalty that the supervisory authority can impose on security issuers for violations of Articles 4 to 6 of TPD (CESR 2009a).[29] (v) *Shift in Enforcement* indicates a substantial change in the enforcement of financial reporting rules around the entry-into-force of the TPD. We construct this variable

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[26] Articles 2 of 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.

[27] The CESR (2007) survey asks questions regarding the powers available to the local authority and covers Articles 1.5 through 16.4 of 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?"

[28] 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 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).

[29] Articles 4 to 6 of 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.

based on a survey sent out to the authority responsible for supervising compliance with accounting standards and the technical department of PricewaterhouseCoopers, an international audit firm, in each EU country.[30] (vi) *Compliance CESR Std. 1* 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] We transform each continuous implementation proxy into binary partitioning variables splitting by the treatment sample median.

We also create partitioning variables based on the timing of the implementation. They serve two purposes. First, we use them to ascertain that our cross-sectional results are not driven by differential timing per se. One concern might be within-EU time trends, though our analysis in Table 4 already mitigates this concern. Another issue could be that, if markets throughout the EU reacted at the time the first set of countries implemented the directives, then one would find cross-sectional differences across countries that are an artifact of differential timing and not driven by differences in the implementation or enforcement. Second, it is important to recognize that the speed and timing of implementing new regulation is likely not only driven by differences in the legislative processes across EU countries, which are plausibly exogenous, but also by factors such as the importance of the regulation to the country, lobbying for or against the implementation of the directives, and the amount of resistance by a country's government. Thus, the implementation timing can be exploited to create a partitioning variable that measures implementation strength and hence the same construct as our other partitioning variables.

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[30] We code *Shift in Enforcement* as 1 if the local enforcement authority indicated that it implemented a 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 occured during the sample period. In cases of disagreement between the two sources and if we could not resolve the issue by going back to the respondents, we coded *Shift in Enforcement* as zero.

[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 provision of financial information as set out in Standard No. 1 and the TPD (see CESR, 2009b).

We provide analyses for each purpose. First, we partition the EU countries into early and late MAD (or TPD) adopters using the median entry-into-force date as cut-off value. We then randomly assign late countries an 'as if' implementation date from the early countries to check whether they would have had a different liquidity effect if they had been as early as the others. Such an effect would be predicted if markets in all EU countries reacted to the new regulation when the first set of countries implemented it. Second, we attempt to parse out the extent to which a country was unusually early or late in implementing MAD or TPD, relative to its own track record of implementing EU directives as well as the average time it takes to implement various directives. That is, we compute for each country the average number of days between the transposition deadlines set by the EU and the actual entry-into-force dates for all 16 FSAP directives (except for the MAD and TPD), relative to the mean for each directive to account for differences in the complexity of the directive. We then compare the time difference between the transposition dates and entry-into-force dates for the MAD and TPD to a country's average implementation timing to see whether a country was unusually late (=1) for the MAD or TPD.

Our last set of partitioning variables focuses on the resources that countries commit to enforcing the directives (see also Enriques and Gatti, 2008). We use the *Staff Level* measured as the number of full-time employees working for the supervisory authority in charge of securities regulation in 2003 as a proxy for the level of resources committed to enforcement prior to the MAD and TPD.[32] To make staff numbers comparable across countries, we scale them by the number of publicly listed firms in a given country. In line with Jackson and Roe (2009), we

[32] Our principal source for full-time supervisory staff is the annual report of the local securities regulators. If we are unable to find an annual report, we use the staff numbers reported in Central Banking Publications (2009). If neither of these sources provides staff numbers in a given year, we interpolate the number from the other years with available data. If the sources provide data only for a joint regulator (that oversees also banking and/or insurance), we allocate staff to securities regulation based on the relative weights of the respective sectors or use information about the allocation of staff in the annual reports.

assume that a higher ratio of supervisory staff to the number of supervised firms indicates better enforcement quality. We define *Staff Growth* as the percentage change in full-time employees working for the local securities regulator from 2004 to 2009.[33] As before, we create binary partitioning variables splitting by the treatment sample median of staff level and staff growth.

5.2. Differential Liquidity Effects within the EU

In this section, we report the cross-sectional results using bid-ask spreads as liquidity proxy. The results for zero returns are very similar and available from authors upon request. As before, the models include the full set of firm-level control variables and fixed effects, and the t-statistics are based on robust standard errors clustered at the country level. For brevity, we tabulate only the coefficients (and t-statistics) for the main variables of interest.[34] We start the empirical tests applying a simple cut through the data along the lines of prior regulatory quality, implementation strength, and implementation timing using the model in Eq. (2). Table 5 shows the results for the MAD (Panel A) and the TPD (Panel B), and reports p-values for statistical differences across groups.

Using *Regulatory Quality* in 2003 to partition the EU member states, we find that in countries with better prior regulation the coefficients on MAD and TPD are negative and significant. In the countries with relatively weak prior regulation, the coefficients are still negative but not significant. The liquidity effects are statistically different from each other for

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[33] If available, we use the growth of supervisory staff specifically assigned to the oversight of securities regulation. If this data are not available, we use the staff growth of the entire regulator. 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] When we conduct the robustness checks from Appendix B and 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 controls (i.e., level of and growth in GDP per capita, and inflation), the results are qualitatively similar to those reported and none of the inferences change.

the MAD. For the TPD, the difference in the coefficients is not quite significant but it is important to note that we report two-sided tests and that our bootstrapping exercise indicated that our standard errors are conservative. We interpret the results as indicating that the liquidity effects for both directives are concentrated in countries with a stronger track record of implementing regulation in the past.[35] In line with this finding and the hysteresis hypothesis, our survey of local regulators indicates that the two directives led to significant changes even in countries with strong prior securities regulation (such as the U.K.).[36]

Next, we report the bid-ask spread results conditioning on the strength of implementation and enforcement of the two directives. The tenor of the results is very similar across the six split variables. The MAD and the TPD coefficients are always negative and significant for the subset of EU countries with strong implementation and, with one exception, not significant for EU countries with relatively weak implementation. In particular, there are significant improvement in liquidity around the MAD in countries with relatively higher monetary fines for violations, that confer more supervisory powers to the local regulator, and that subsequently actually enforce the insider trading rules.[37] Similarly, the TPD is associated with an increase in liquidity in countries that set relatively high monetary fines for violations, when regulators and auditors indicate that there were substantial improvements in how the supervisory authorities enforce financial reporting rules, and in countries that fully comply with CESR's enforcement principles. We note that test statistics for the difference in the coefficients between strong and weak

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35 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 in power, as the index is missing for several EU countries.

36 For instance, the U.K. regulator changed its review process from a referral basis to a proactive (risk-based) sampling approach. Consistent with these survey-based perceptions, reviews conducted by CESR confirm that there were multiple changes to the oversight and enforcement procedures in the U.K. following the implementation of MAD and TPD (see CESR 2009a, 2010).

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

implementation countries are often only close to conventional significance levels, but the issue appears to be primarily power. The coefficient magnitudes across all six directive-specific variables clearly support an interpretation that the liquidity effects of the EU directives are concentrated in countries with stronger implementation and enforcement.

Next, we turn to the issue of countries' implementation timing. As we discussed earlier, it is unlikely that countries can deliberately time the entry-into-force dates with respect to country-specific liquidity trends, especially considering how sharply the effects occur after the entry-into-force dates. But as discussed in Section 5.1, it could be that the cross-sectional differences across EU countries are driven by timing per se if there are spillover effects from the countries that implement the directives early to the rest of the EU countries. That is, if markets already reflected the regulatory changes by the time the directives become effective in the late countries, the results for the actual entry-into-force dates would be weak or not existent, but it would not be because of differences in implementation or enforcement. To address this concern, we counterfactually assign entry-into-force dates from the early countries to the late countries to see whether there is a liquidity effect around that time. As shown in column 5 of Table 5, the MAD and TPD coefficients using 'as if' early dates are insignificant and not even negative. Thus, we do not think that our cross-sectional results reflect merely differential timing.

A second aspect of implementation timing is that we can use it as a proxy for the extent to which the directives face resistance in a country or are viewed as a priority. When we use the abnormal delay constructed in Section 5.1 as partitioning variable, we find that the liquidity benefits exist only in countries that implemented the directives faster than normal and that the their liquidity effects are statistically different from countries that are abnormally late. These findings are consistent with our results for the implementation strength variables.

Next, to further explore our hysteresis and catching-up hypotheses, we combine the binary *Regulatory Quality 2003* indicator variable (high vs. low quality) with each of the six implementation variables (strong vs. weak implementation) in Table 6. This two-way partitioning sorts the post-MAD or TPD observations into four distinct bins for which we separately 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 EU 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 as well as a weak implementation of the new directives.

The table again 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. The analyses indicate that countries with a strong track record for past regulation and also 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 highly 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. Moreover, the rank order of the coefficient magnitudes going from the *High RQ/Strong IS* to the *Low RQ/Weak IS* bin is generally monotonically decreasing. Moreover, the differences in the liquidity effects for countries with strict versus weak implementation are generally larger and significant only in countries with high regulatory quality. Thus, there is little evidence that countries with weaker securities regulation catch up with stronger countries as a result of the new EU directives. In contrast, the liquidity differences become larger, consistent with the hysteresis hypothesis. This finding shows that

imposing the same regulation in different countries can result in countries drifting further apart, illustrating the difficulties of regulatory harmonization.

In Table 7, we present the results partitioning based on the resources that countries commit to the supervision and enforcement of securities regulation. We again distinguish between resources committed in the past and changes in the resources committed around the implementation of the directives. In the *Staff Level* partition, we find that that the effects of MAD and TPD are significant only for the group with a high level, and that the differences between the two groups are significant. This finding is consistent with the results for prior regulatory quality in Table 5. Similarly, in the *Staff Growth* split we find that the effects of the MAD and TPD are significant only in those countries with substantial changes in the resources they commit to supervision and enforcement. The effects are statistically insignificant in countries with moderate or no changes in supervisory staff, but we note that the difference between the high and low growth countries is never statistically significant. When we combine the *Staff Level* and the *Staff Growth* variables to form four bins (as we also did in Table 6), the MAD and TPD coefficients are significantly negative only for the *High Level/High Growth* group. The liquidity effects in the other bins are smaller and insignificant. Thus, similar to Table 6, the countries that exhibit the largest liquidity improvements are those that already had relatively well-staffed supervisors to begin with, but further increased the supervisory staff around the implementation of the new directives.

Overall, the cross-sectional analyses provide little evidence supporting the catching-up hypothesis. In contrast, there appears to be considerable hysteresis in regulatory outcomes. The same forces that have limited the effectiveness of securities regulation in the past appear to be

still at play when new rules are introduced, suggesting that history and countries' initial conditions matter a great deal for regulatory outcomes.

6. Conclusion

This paper examines capital-market effects of changes in securities regulation. We focus on two key EU capital-market directives pertaining to 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 examine the role of differences in implementation and enforcement for regulatory outcomes. We also analyze the effects of prior regulation and the extent to which the EU directives allow weaker countries to catch up or even provide a springboard for stronger countries to advance even further.

Our empirical identification strategy relies on within-EU variation in the dates of when the directives become effective. The staggered imposition of the two directives across EU countries not only exploits that regulatory acts are exogenous at the firm level but it also alleviates common concerns about regulatory studies, particularly, about concurrent but unrelated economic shocks and endogenous market responses around the introduction of new regulation. Thus, our setting also provides for better identification.

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 also provide corroborating evidence that the directives

lower firms' cost of capital but hasten to add that our setting and identification strategy are less well suited for anticipatory proxies such as the cost of capital.

Furthermore, we provide strong evidence of hysteresis in regulatory outcomes. The liquidity effects of the two directives are stronger in countries with a history of higher regulatory quality and with traditionally stronger securities regulators. We also provide evidence that stricter implementation and enforcement of the two directives result in larger liquidity effects, but these effects stem largely from countries with strong prior regulatory quality and that already have relatively well-staffed securities regulators. One explanation for these findings is that countries that have put more resources into securities regulation and that 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 strength of past regulation 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 the tests cannot distinguish between these forces. Thus, our cross-sectional results need to be interpreted cautiously.

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 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 following the new EU directives illustrates the difficulty of harmonizing countries through regulatory reforms. It highlights that prior regulatory conditions matter and that imposing the same

regulation on countries with disparate prior conditions can have the effect of making countries diverge more, not less.

In closing, we note an important caveat about our study. While our 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 beneficially 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: EU Securities Regulation, Market Abuse and Transparency Directives

In 1999 the European Union (EU) initiated the Financial Services Action Plan (FSAP). The FSAP is a key component of the EU's attempt to create a single market for financial services and one of its stated aims is that "capital market fragmentation should be eliminated, thereby reducing the cost of capital raised on EU markets" (FSAP 1999, p. 3). The FSAP introduced 42 measures, each with their own specific objective (CRA 2009). Four of these measures represent key securities regulation and together constitute the Lamfalussy Directives: the Market Abuse Directive (MAD), the Transparency Directive (TPD), the Prospective Directive (PD), and Markets in Financial Instruments Directive (MiFID).[38] The PD pertains to the primary market (securities offerings) and hence seems less relevant for our analysis, which focuses on secondary market outcomes. The MiFID is the final of the four Lamfalussy directives, which as of its effective date in November 2007 replaces the Investment Services Directive. Its main objective is to increase competition and consumer protection in investment services. Thus, like the PD, it seems less relevant to our research question and the purpose of our study than the MAD or the TPD, which is why we focus on the latter two directives.[39]

Market Abuse Directive

The Market Abuse Directive (MAD) is aimed at preventing insider trading and market manipulation. It replaces an older directive of 1989, which already banned insider trading, so the focus of MAD is mainly to improve the implementation and enforcement of rules already in

[38] Under the new Lamfalussy process, which was adopted in 2001 to make EU regulation on securities markets more flexible, the European Council and the EU parliament adopt a piece of legislation (directive), which at the first level establishes the key principles and guidelines on its implementation. The law then progresses to the second level, at which the European Securities Committee (ESC) and the Committee of European Securities Regulators (CESR) advise on technical details, leading to an implementing directive.

[39] In Table 3 Panel D, we consider all four Lamfalussy Directives as one package and, more generally, discuss whether the observed capital-market effects reflect broader regulatory changes in the EU.

place. The MAD utilizes three regulatory tools: (i) disclosure rules designed to reduce the scope for insider information, (ii) ex post sanctions for conducting 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 that issuers of financial instruments inform the public as soon as possible of inside information (Article 6). Moreover, executives must, as soon as possible, disclose their transactions in the securities of the firm they manage.

The MAD also aims to harmonize sanctions across member states but, the requirement is generic, and therefore leaves significant discretion to the member states. 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. Member states shall ensure that these measures are effective, proportionate and dissuasive".

Finally, the most important change to enforcement specified by MAD is the requirement to designate a single authority with the competence of ensuring that the provisions of MAD are applied (Article 11). The MAD further requires that a number of specific powers be given to 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 that should be available to the authority.

Transparency Directive

The Transparency Directive (TPD) is aimed at facilitating appropriate transparency for investors through a regular flow of information. The TPD uses two regulatory tools to improve transparency: (i) a set of disclosure requirements and (ii) tightened enforcement of compliance with disclosure rules.

The disclosure rules in TPD include both ongoing disclosure requirements (annual and half annual reports according to IFRS) and requirements that ensure significant events are disclosed (e.g., significant holdings by shareholders). However, IFRS was already mandated by older EU regulation (Regulation No. 1606/2002) and most exchanges already required half annual reports and the disclosure of significant events. Hence, the TPD did relatively little in terms of changing specific disclosure requirements.

The material changes that the TPD did bring about were related to the enforcement of compliance with what were largely existing disclosure requirements. Most notably the TPD require that each member states designate a competent authority with the responsibility for ensuring that issuers comply with the transparency requirements in the TPD.

As were the case with the MAD, the TPD does require a minimum number of specific powers that the authority must have (Article 24). For instance, the authority must examine that information required by the TPD is disclosed accurately and, if infringements are discovered, take appropriate measures (e.g., issue a fine). Moreover, when investigating compliance, the authority must be able to request information from auditors and shareholders and carry out onsite inspections. The requirement to enforce compliance with disclosure rules is a significant change in the European setting where many member states had no, or limited, enforcement of disclosure rules prior to the TPD. Even in the United Kingdom, where securities laws have historically been

rigorously enforcement, compliance with disclosure rules where only examined in reaction to complaints received by the securities regulator prior to the TPD. Subsequent to the TPD, the UK implemented a proactive comments and review process, similar to the process applied by the Securities and Exchange Commission in the US. Consistent with a significant change to enforcement in the United Kingdom, the response to the survey of auditors, we carried out, indicates that enforcement activity significantly increased over our sample period.

Appendix B: Additional Sensitivity Tests

In this appendix, we discuss a series of sensitivity analyses for the market liquidity effects documented in Section 4. In particular, we gauge (i) the clustering of the standard errors, (ii) the choice of the fixed effects structure, (iii) the composition of the benchmark and the treatment sample, (iv) the inclusion of additional control variables, and (v) consider alternative dependent variables. Unless stated otherwise, we estimate the same specification as in Table 2 and report the coefficients on *MAD* and *TPD* together with the t-statistics (in parenthesis) for bid-ask spreads and the proportion of zero return days in Table A1.

First, we assess the clustering of the standard errors. In our main specification, we use clusters by country. As the first two rows in Table A1 illustrate, when we either use two-way clustering by country and quarter or cluster the standard errors by economic region, the inferences remain the same. The latter combines several EU countries (e.g., Western Europe, Eastern Europe, etc.) and hence is more conservative than country-level clustering.

Second, we examine our choice of the fixed effects structure on the results. When we replace the country- and industry-fixed effects with firm-fixed effects, the results are very similar, though we note that the magnitude and significance of the TPD effect is slightly attenuated. Alternatively, 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 extension of the fixed-effects structure should absorb economic shocks that affect larger firms differently from smaller firms. In both cases, the results are similar and the inferences the same as those

reported in Table 2.[40] Based on these results, we conclude that it is unlikely that our liquidity findings are simply an artifact of economic shocks to particular groups of firms or differences in the composition of firms across countries.

Third, we gauge the impact of the sample composition and the choice of benchmark sample on the results. As the table shows, the results remain virtually the same when we estimate the regressions within the EU or when we limit the number of observations per benchmark country to 150 randomly selected firms. Thus, the use of benchmark firms outside the EU has little impact on our results, as it should with our fixed-effects structure. Next, we drop the observations from the four largest treatment countries (U.K., Germany, France, and Sweden) either one-by-one (not tabulated) or altogether to check that our results not driven by a single or just a few large countries. The results for MAD and TPD (spreads) do not significantly change, while the TPD effect is attenuated and loses statistical significance for the zero returns proxy. Overall, however, it does not appear to be the case that our results are solely driven by a few large countries. Note further that some attenuation would not be surprising in light of our cross-sectional results in Section 5 given that these four countries generally fall into the group of EU countries with strong prior regulation and/or implementation. Finally, we check that our results also hold when we include Bulgaria and Romania, which did not join the EU until 2007 and adopted many EU regulations by that date.[41]

Fourth, we examine our choice of control variables. One frequently voiced concern is that our liquidity findings might reflect general changes in the macroeconomic environment. Thus,

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[40] In additional tests (not tabulated), we further include separate volatility coefficients for each quarter, interactions between the country indicators and firm size, and interactions between the country indicators and the industry dummies, respectively. Again, the results are similar to those reported in Table 2.

[41] The results (not tabulated) also hold and inferences remain the same when we restrict the analysis to firms that already existed and entered our sample in 2004, which essentially attempts to hold the sample constant over time.

we add several variables capturing such macroeconomic changes, namely annual inflation and the level of and growth in per-capita gross domestic product (GDP). We compute inflation as the country-specific yearly percentage change in the consumer price index, measured at the end of each quarter. Annual GDP per capita data is from the World Bank. The results remain largely unchanged after including the additional control variables. We also re-estimate our base model after excluding *Share Turnover* from the set of controls given it is conceptually close to the zero returns proxy. Again, the results are very similar and inferences are unaffected.

Fifth, we use alternative dependent variables to examine the effects of the MAD and TPD. In our main analyses we scale the bid-ask spreads by contemporaneous stock prices, which also could reflect the impact of the regulatory changes. To address this concern, we re-compute the spread variable and scale it by the earliest available stock price for each firm. For 50% of the sample, this price stems from the first or second quarter in 2001, and for about 70% of the EU firms, this price is measured prior to the implementation of the directives. We also confirm that our results hold in the linear (instead of the log-linear) specification. That is, we re-estimate the model using untransformed (raw) spreads as the dependent variable. In both cases, the results are very similar to those reported in the main tests.

Finally, we analyze changes in firms' cost of capital. Regulators often refer to improvements in firms' costs of raising capital as a justification for securities regulation (e.g., Lamfalussy, 2000; Enriques and Gatti, 2008; CRA, 2009). As discussed before, market liquidity seems better suited than proxies like the cost of capital or share price that potentially anticipate the regulatory effects long before the directives become effective in the EU countries, which in turn makes it difficult to exploit the staggered imposition for identification. In addition, it is

harder to reliably measure the cost of capital on a quarterly basis. Thus, we conduct the cost of capital analysis merely to corroborate the liquidity analysis.

We use two commonly used proxies for the cost of capital: firms' implied costs of capital and dividend yields. The basic idea of the implied cost-of-capital models is to back out the internal rate of return that equates current stock price with the expected sequence of future (abnormal) earnings (e.g., Gebhardt et al., 2001; Hail and Leuz, 2006; Pástor et al., 2008). Conceptually, these models are consistent with discounted dividend valuation. We follow Hou et al. (2009) and use the predicted values from a pooled, cross-sectional regression of future (realized) earnings on a set of contemporaneous firm characteristics to derive earnings forecasts. We then plug these forecasts into the 12-year version of the Gebhardt et al. (2001) valuation model and solve for the internal rate of return (r_{GLS}) that equates a firm's estimated value with its market value of outstanding equity at the end of each calendar quarter.[42] We assign the cost-of-capital estimate to the quarter of the pricing date. For the regression specification, we follow Hail and Leuz (2006, 2009) and control for inflation, GDP per capita, firm size measured by total assets, financial leverage, and return variability.[43] We report the *MAD* and *TPD* coefficients from the implied cost of capital regression (using similar sampling criteria as for liquidity) at the

[42] More specifically, we require each firm-quarter observation to have positive one-, two-, and three-year-ahead earnings forecasts. These forecasts are the predicted values of a pooled cross-sectional regression of future realized earnings on the market value of the firm, total assets, dividend payments, current earnings, operating accruals, and a dividend payment as well as a loss indicator variable (see Hou et al., 2009, for details). To allow for differences in accounting practices across countries, we include country-fixed effects in the model. We estimate this regression for each forecast horizon (i.e., t+1, t+2, and t+3) and year using up to ten years of previous data. The predicted values of annual earnings are strictly out-of-sample. That is, we multiply the coefficient estimates of the pooled cross-sectional regressions with the yearly realizations of the independent variables that occur after the estimation period, but before the pricing date (i.e., the end of the calendar quarter). For details on the additional input parameters and the estimation procedure of the Gebhardt et al. (2001) approach, see also the appendices of Hail and Leuz (2006, 2009).

[43] Total assets are denominated in US$ million. We compute financial leverage as the ratio of total liabilities to total assets. We compute return variability as the standard deviation of daily stock returns in a given calendar year. Financial data are from Worldscope, price and volume data from Datastream. We measure inflation and per-capita GDP as outlined above. Because the Hou et al. (2009) implied cost of capital estimates (as well as dividend yields) are fairly noisy, we truncate them at the fifth and 95th percentile, and delete all cost of capital estimates that fall below the local yearly inflation rate, because such estimates seem implausible.

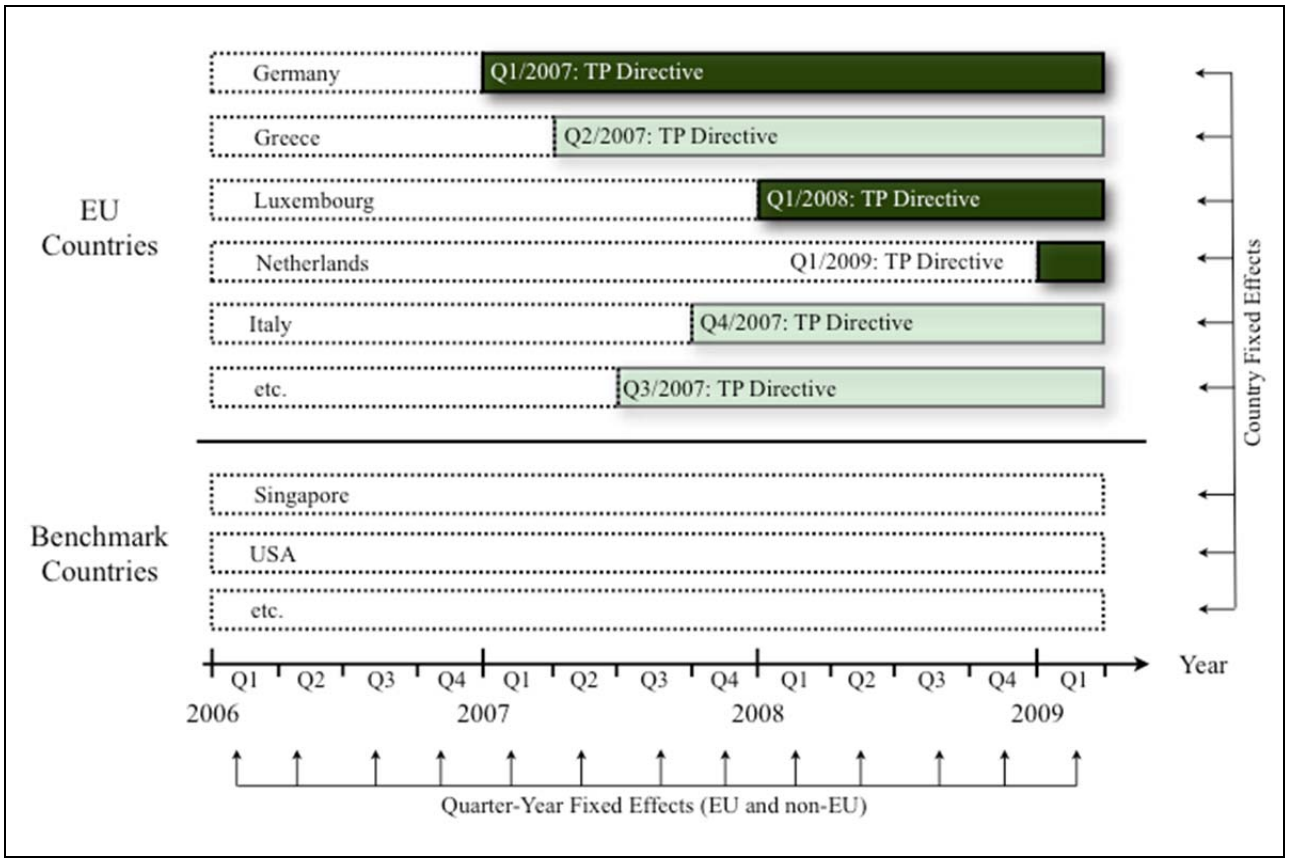
bottom of Table A1. Both coefficients are negative and significant, suggesting a reduction in the cost of capital of about 57 (20) basis points following the implementation the MAD (TPD). The control variables in the cost-of-capital model behave as expected and are significant, except for return variability. As expected, the cost of capital regressions are noisier and so the explanatory power is only 34 percent compared with up to 76 percent in the liquidity regressions.

Our second cost-of-capital proxy, dividend yield has been used extensively in prior finance studies (e.g., Bekaert and Harvey, 2000, Errunza and Miller, 2000; Bhattacharya and Daouk, 2002). We measure *Dividend Yield* as the actual dividends paid during the last fiscal year divided by the stock price at the end of each quarter. For firms that do not pay dividends, we set the dividend yield equal to missing. We use the same set of controls as in the implied cost of capital model plus asset growth, which is supposed to capture differences in firms' growth expectations.[44] As Table A1 shows, the coefficients on *MAD* and *TPD* in the dividend yield regression are negative, but insignificant at conventional levels. However, the two directives are jointly significant and the coefficients, while imprecisely estimated, would be economically significant.

Taking both proxies together, the evidence for the cost of capital is broadly consistent with our findings for market liquidity as well as the cost-of-capital effects documented in Bhattacharya and Daouk (2002) around the first enforcement of insider trading rules.

[44] The implied cost of capital estimates explicitly account for growth differences by using firm-specific earnings forecasts. We measure asset growth as the year-to-year percentage change in total assets.

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 as well as non-EU benchmark countries. For each EU 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 quarter of our sample period for EU and non-EU benchmark countries, separately. The latter implies that the model includes two separate quarterly time trends for EU and non-EU countries. The different shadings of the countries after the directive becomes effective illustrates that we exploit cross-sectional differences in the way countries implement the directives.

Table 1: Sample Composition and Descriptive Statistics

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

	Liquidity Measures		*EU Directives*	
Country	*Bid-Ask Spreads (N)*	*Zero Returns (N)*	*MAD Entry-into-Force Dates*	*TPD Entry-into-Force Dates*
Austria	1,336	1,562	Jan-05	Apr-07
Belgium	3,492	3,534	Sep-05	Sep-08
Bulgaria	310	312	Jan-07	Jan-07
Cyprus	1,305	3,018	Sep-05	Mar-08
Czech Republic	146	231	Feb-06	Aug-09
Denmark	4,993	5,109	Apr-05	Jun-07
Estonia	225	435	Mar-05	Dec-07
Finland	4,034	4,074	Jul-05	Feb-07
France	17,163	17,678	Jul-05	Dec-07
Germany	9,352	9,681	Oct-04	Jan-07
Greece	n.a.	9,582	Jul-05	Jul-07
Hungary	793	837	Jul-05	Dec-07
Iceland	81	86	Jul-05	Nov-07
Ireland	710	741	Jul-05	Jun-07
Italy	7,590	7,964	May-05	Apr-09
Latvia	342	355	Jul-05	Apr-07
Lithuania	560	1,122	Apr-04	Feb-07
Luxembourg	29	73	May-06	Jan-08
Malta	n.a.	360	Apr-05	Oct-07
The Netherlands	3,764	3,793	Oct-05	Jan-09
Norway	5,006	5,162	Sep-05	Jan-08
Poland	5,433	6,291	Oct-05	Mar-09
Portugal	1,416	1,458	Apr-06	Nov-07
Romania	368	1,323	Jan-07	Jan-07
Slovakia	79	98	Jan-05	May-07
Slovenia	383	977	Aug-04	Sep-07
Spain	3,366	3,649	Nov-05	Dec-07
Sweden	8,321	8,763	Jul-05	Jul-07
United Kingdom	21,750	22,274	Jul-05	Jan-07

(continued)

Table 1 (continued)

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

	N	*Mean*	*Std. Dev.*	*P1*	*P25*	*Median*	*P75*	*P99*
European Union Countries (Treatment Sample):								
Bid-Ask Spread$_t$	101,669	0.030	0.042	0.001	0.007	0.016	0.036	0.217
Zero Returns$_t$	118,907	0.283	0.250	0.000	0.092	0.188	0.424	0.939
Market Value$_{t-4}$	118,907	906	3,664	3	32	117	487	13,838
Share Turnover$_{t-4}$	118,907	0.001	0.002	0.000	0.000	0.001	0.002	0.011
Return Variability$_{t-4}$	118,907	0.024	0.012	0.006	0.015	0.021	0.030	0.059
Non-European Union Countries (Benchmark Sample):								
Bid-Ask Spread$_t$	510,300	0.025	0.044	0.001	0.003	0.009	0.026	0.233
Zero Returns$_t$	661,527	0.248	0.234	0.000	0.077	0.154	0.354	0.924
Market Value$_{t-4}$	661,527	1,075	7,877	2	27	95	374	16,589
Share Turnover$_{t-4}$	661,527	0.003	0.005	0.000	0.000	0.001	0.004	0.023
Return Variability$_{t-4}$	661,527	0.029	0.014	0.007	0.018	0.026	0.037	0.069

The treatment sample consists of all countries in the European Union (EU) except for Bulgaria and Romania, which did not join the EU until 2007. 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. For brevity, we refer to the EU countries as the treatment sample. In Panel A, we present the number of firm-quarter observations for the two liquidity measures used in the main analysis as well as the dates when the Market Abuse Directive (*MAD)* and the Transparency Directive (*TPD*) came into force in each EU country. In Panel B, we present descriptive statistics for the dependent variables and the firm-level independent variables used in the analyses. We present the statistics separately for the treatment (EU) and the benchmark (non-EU) samples, which comprise up to 27 and 35 countries, respectively. The sample comprises all firm-quarter observations beginning in the first quarter of 2001 through the second quarter of 2009 with financial data in Worldscope and price/volume data in Datastream. The *Bid-Ask Spread* is the quarterly median 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). *Zero Returns* is the proportion of trading days with zero daily stock returns out of all potential trading days in a given quarter. *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 median 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. All means (medians) are significantly different at the 1%-level across EU and Non-EU countries using t-tests (Wilcoxon rank sum tests).

Table 2: Capital Market Effects of Tightening EU Securities Regulation

	Ln(Bid-Ask Spread) as Dependent Variable			*Zero Returns as Dependent Variable*		
	Market Abuse Directive	*Transparency Directive*	*Both Directives Combined*	*Market Abuse Directive*	*Transparency Directive*	*Both Directives Combined*
Test Variables:						
MAD	-0.177***	–	-0.199***	-0.043***	–	-0.046***
	[-3.81]		[-3.98]	[-3.73]		[-3.66]
TPD	–	-0.305**	-0.310**	–	-0.040**	-0.041**
		[-2.15]	[-2.21]		[-2.14]	[-2.18]
Control Variables:						
Ln(Market Value$_{t-4}$)	-0.382***	-0.382***	-0.382***	-0.060***	-0.060***	-0.060***
	[-28.13]	[-28.17]	[-28.18]	[-14.29]	[-14.30]	[-14.30]
Ln(Share Turnover$_{t-4}$)	-0.300***	-0.300***	-0.300***	-0.049***	-0.049***	-0.049***
	[-9.54]	[-9.56]	[-9.56]	[-19.73]	[-19.70]	[-19.69]
Ln(Return Variability$_{t-4}$)	0.404***	0.404***	0.404***	-0.038***	-0.038***	-0.038***
	[7.73]	[7.73]	[7.73]	[-4.02]	[-4.03]	[-4.03]
Fixed Effects:						
Country	Yes	Yes	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-Year	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-Year (EU specific)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.762	0.762	0.762	0.525	0.526	0.526
N Treatment/Benchmark Countries	25/27	25/27	25/27	27/35	27/35	27/35
N Firm-Quarter Observations	611,969	611,969	611,969	780,434	780,434	780,434

The sample comprises firm-quarter observations from up to 27 (35) EU (non-EU) countries between the first quarter 2001 and the second quarter 2009. We use two dependent variables: (1) the *Bid-Ask Spread* measured as the quarterly median quoted spread, and (2) *Zero Returns* measured as the proportion of trading days with zero daily stock returns in a quarter. *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 a description of the control variables see Table 1. If indicated, we use the natural log of the raw values, and lag the variables by four quarters. We include country-, Campbell (1996) industry-, and quarter-year-fixed effects (for EU and non-EU countries separately) 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 for the Capital Market Effects of Tightening EU Securities Regulation

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

	Ln(Bid-Ask Spread) as Dependent Variable (N=611,969)				*Zero Returns as Dependent Variable (N=780,434)*			
	Market Abuse Directive		*Transparency Directive*		*Market Abuse Directive*		*Transparency Directive*	
	Coefficient	*t-stat*	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*
Shifting of Entry-into-Force Dates Relative to t=0:								
t–4	-0.059	[-0.85]	0.048	[1.39]	0.006	[0.58]	-0.029	[-1.64]
t–3	-0.104*	[-1.95]	-0.029	[-0.55]	-0.004	[-0.46]	-0.034*	[-1.91]
t–2	-0.144***	[-3.05]	-0.095	[-1.14]	-0.014	[-1.46]	-0.039**	[-2.16]
t–1	-0.204***	[-4.52]	-0.183*	[-1.69]	-0.028***	[-3.25]	-0.042**	[-2.35]
t = 0 ('True' Entry-into-Force Dates)	-0.199***	[-3.98]	-0.310**	[-2.21]	-0.046***	[-3.66]	-0.041**	[-2.18]
t+1	-0.153**	[-2.42]	-0.430**	[-2.35]	-0.043***	[-2.73]	-0.036*	[-1.76]
t+2	-0.103	[-1.53]	-0.485**	[-2.33]	-0.033*	[-1.92]	-0.032	[-1.43]
t+3	-0.063	[-0.89]	-0.497**	[-2.34]	-0.020	[-1.04]	-0.030	[-1.19]
t+4	-0.037	[-0.62]	-0.502**	[-2.28]	-0.008	[-0.48]	-0.030	[-1.11]
Control for Other Directive	Yes		Yes		Yes		Yes	
Control Variables	Yes		Yes		Yes		Yes	
Fixed Effects:								
Country	Yes		Yes		Yes		Yes	
Industry	Yes		Yes		Yes		Yes	
Quarter-Year	Yes		Yes		Yes		Yes	
Quarter-Year (EU specific)	Yes		Yes		Yes		Yes	

(continued)

Table 3 (continued)

Panel B: Analysis of the Liquidity Effects of the MAD and TPD in Unregulated Markets

	Ln(Bid-Ask Spread) as Dependent Variable				*Zero Returns as Dependent Variable*			
	Full Sample (N=572,521)		*EU Only (N=122,001)*		*Full Sample (N=704,124)*		*EU Only (N=138,021)*	
	MAD	*TPD*	*MAD*	*TPD*	*MAD*	*TPD*	*MAD*	*TPD*
Test variables:								
Regulated EU Markets	-0.269***	-0.312***	-0.257***	-0.295**	-0.081***	-0.069***	-0.070***	-0.061***
	[-3.00]	[-2.07]	[-3.12]	[-2.00]	[-3.65]	[-2.98]	[-4.23]	[-3.55]
Unregulated EU Markets	0.018	-0.014	-0.009	-0.041	0.043	0.059***	0.004	0.010
	[0.22]	[-0.14]	[-0.11]	[-0.49]	[1.38]	[3.22]	[0.15]	[0.49]
F-test for Differences across Coefficients (p-value):								
Regulated = Unregulated	0.018	0.053	0.017	0.056	0.003	0.000	0.004	0.000
Control for Other Directive	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Analysis of the Liquidity Effects of Other Regulatory Changes

	Ln(Bid-Ask Spread) as Dependent Variable (N=611,969)			*Zero Returns as Dependent Variable (N=780,434)*		
	MiFID Directive	*Prospectus Directive*	*All Lamfalussy Directives*	*MiFID Directive*	*Prospectus Directive*	*All Lamfalussy Directives*
Test Variables:						
MAD	–	–	-0.213***	–	–	-0.042***
			[-3.78]			[-3.88]
TPD	–	–	-0.298**	–	–	-0.042**
			[-2.16]			[-2.32]
MiFID	-0.264	–	-0.053	-0.026	–	-0.001
	[-1.65]		[-0.35]	[-1.47]		[-0.08]
PROSP	–	0.090	0.093*	–	-0.034***	-0.033**
		[1.41]	[1.81]		[-2.73]	[-2.47]
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

(continued)

Table 3 (continued)

The sample comprises firm-quarter observations from up to 27 EU and 35 non-EU countries between the first quarter 2001 and the second quarter 2009. We use two dependent variables: (1) the *Bid-Ask Spread* measured as the quarterly median quoted spread, and (2) *Zero Returns* measured as the proportion of trading days with zero daily stock returns in a quarter. *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. In Panel A, we report the *MAD* and *TPD* coefficients from nine 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 one beginning in each quarter from t-4 to t+4 relative to the 'true' entry-into-force date. In Panel B, we use firms trading on (unregulated) EU markets that are not subject to the MAD and TPD as an additional benchmark sample. That is, we add a separate binary MAD or TPD indicator variable for unregulated EU firms to the model, and include those firms in the sample. To reduce measurement error, we further exclude treatment sample firms with, on average, market values below US$ 20 million. We report results for the full sample (including the non-EU benchmark firms) and the EU countries only. The unregulated markets (together with their country of domicile) are: Alternative Investment Market AIM (U.K.), AIM Italia and Mercato Alternativo del Capitale (Italy), Dritter Markt (Austria), EN.A (Greece), Enterprise Securities Market (Ireland), Euro MTF (Luxembourg), First North (several European countries), Marché Libre (France and Belgium), Mnohostranný obchodný systém (Slovakia), NewConnect (Poland), NYSE Alternext (several European countries), and Open Market (Germany). In Panel C, we assess the influence of the remaining Lamfalussy Directives on the results. We construct binary indicator variables similar to *MAD* or *TPD* and estimate regressions including (i) only the Markets in Financial Instruments Directive (*MiFID*), (ii) only the Prospectus Directive (*PROSP*), and (iii) all four of the Lamfalussy Directives (i.e., *MAD*, *TPD*, *MiFID*, and *PROSP*). Throughout the table, we include the full set of control variables and fixed effects in the models, but only report OLS coefficient estimates (t-statistics) for the main variables. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 4: Prior Regulation and Implementation Variables by EU Country

	Prior Regulation		*MAD Implementation Strength*					*TPD Implementation Strength*			
Country	*Regulatory Quality 2003*		*Maxium Fine (EUR 000)*		*Supervisory Powers*		*Action Taken by 2009*	*Maxium Fine (EUR 000)*		*Shift in Enforcement*	*Compliance with CESR Std. 1*
Austria	1.52	(1)	No fine	(0)	70	(0)	1	30	(0)	0	0
Belgium	1.36	(1)	Profit-based	(1)	69	(0)	0	2,500	(1)	0	1
Bulgaria	0.59	(0)	50	(0)	69	(0)	0	5,112	(1)	0	0
Cyprus	1.20	(0)	1,710	(0)	68	(0)	0	341	(0)	0	0
Czech Republic	1.12	(0)	350	(0)	64	(0)	0	400	(0)	0	0
Denmark	1.79	(1)	No fine	(0)	60	(0)	0	No limit	(1)	1	1
Estonia	1.40	(1)	No fine	(0)	60	(0)	1	16,000	(1)	1	0
Finland	1.90	(1)	200	(0)	63	(0)	0	200	(0)	1	1
France	1.18	(0)	Profit-based	(1)	75	(1)	1	10,000	(1)	1	1
Germany	1.51	(1)	Profit-based	(1)	64	(0)	1	200	(0)	1	0
Greece	1.01	(0)	6,000	(0)	60	(0)	0	1,000	(1)	0	0
Hungary	1.08	(0)	Profit-based	(1)	73	(1)	0	24	(0)	1	0
Iceland	1.67	(1)	10,000	(0)	60	(0)	1	300	(0)	0	0
Ireland	1.66	(1)	588	(0)	73	(1)	0	2,500	(1)	1	1
Italy	1.02	(0)	Profit-based	(1)	70	(0)	1	620	(1)	0	1
Latvia	1.03	(0)	Profit-based	(1)	80	(1)	1	14	(0)	1	1
Lithuania	1.10	(0)	Profit-based	(1)	70	(0)	0	29	(0)	1	0
Luxembourg	1.94	(1)	Profit-based	(1)	80	(1)	0	125	(0)	0	0
Malta	1.27	(0)	Profit-based	(1)	75	(1)	1	466	(0)	0	0
The Netherlands	1.76	(1)	Profit-based	(1)	67	(0)	1	120	(0)	1	0
Norway	1.39	(1)	Profit-based	(1)	59	(0)	1	No limit	(1)	0	1
Poland	0.61	(0)	1,250	(0)	70	(0)	0	1,389	(1)	0	1
Portugal	1.21	(0)	2,500	(0)	73	(1)	0	2,500	(1)	0	1
Romania	-0.12	(0)	Profit based	(1)	73	(1)	1	13	(0)	0	0
Slovakia	0.95	(0)	600	(0)	74	(1)	0	664	(1)	0	0
Slovenia	0.88	(0)	125	(0)	51	(0)	0	125	(0)	0	0
Spain	1.29	(1)	Profit-based	(1)	60	(0)	0	600	(0)	0	1
Sweden	1.69	(1)	No fine	(0)	73	(1)	1	1,000	(1)	1	0
United Kingdom	1.68	(1)	No limit	(1)	76	(1)	1	No limit	(1)	1	1

(continued)

Table 4 (continued)

Country	*MAD Timing*			*TPD Timing*			*Supervisory Resources*			
	'As if' Early Implemen-tation	*Abnormal Delay (Days)*		*'As if' Early Implemen-tation*	*Abnormal Delay (Days)*		*Staff Level 2003*		*Staff Growth 2004 to 2009*	
Austria	0	-73	(0)	0	-30	(0)	0.23	(0)	2.08	(1)
Belgium	0	-120	(0)	1	155	(1)	0.24	(0)	-0.27	(0)
Bulgaria	1	n.a.	–	0	n.a.	–	0.16	(0)	0.13	(0)
Cyprus	0	7	(1)	1	111	(1)	0.22	(0)	0.07	(0)
Czech Republic	1	108	(1)	1	582	(1)	0.49	(1)	0.93	(1)
Denmark	0	-107	(0)	0	-119	(0)	0.19	(0)	0.23	(0)
Estonia	0	-197	(0)	0	-11	(0)	1.86	(1)	-0.01	(0)
Finland	0	-67	(0)	0	-275	(0)	0.33	(0)	-0.20	(0)
France	0	-185	(0)	0	-112	(0)	0.35	(1)	0.06	(0)
Germany	0	-241	(0)	0	-231	(0)	0.38	(1)	0.25	(1)
Greece	0	-366	(0)	0	-448	(0)	0.39	(1)	0.16	(0)
Hungary	0	30	(1)	0	110	(1)	0.63	(1)	0.81	(1)
Iceland	0	n.a.	–	0	n.a.	–	0.25	(0)	1.15	(1)
Ireland	0	41	(1)	0	-142	(0)	1.35	(1)	0.30	(1)
Italy	0	-164	(0)	1	471	(1)	0.27	(1)	0.58	(1)
Latvia	0	475	(1)	0	315	(1)	0.25	(0)	0.25	(1)
Lithuania	0	-557	(0)	0	-341	(0)	0.92	(1)	-0.04	(0)
Luxembourg	1	99	(1)	1	-94	(0)	0.26	(1)	2.36	(1)
Malta	0	-30	(0)	0	95	(1)	1.54	(1)	0.18	(0)
The Netherlands	1	-59	(0)	1	327	(1)	0.34	(1)	0.25	(1)
Norway	0	n.a.	–	1	n.a.	–	0.34	(0)	0.32	(1)
Poland	1	700	(1)	1	1,136	(1)	0.78	(1)	1.59	(1)
Portugal	1	168	(1)	0	-70	(0)	2.76	(1)	0.04	(0)
Romania	1	n.a.	–	0	n.a.	–	0.04	(0)	0.13	(0)
Slovakia	0	-45	(0)	0	-363	(0)	0.02	(0)	-0.06	(0)
Slovenia	0	-392	(0)	0	-77	(0)	0.26	(0)	0.32	(1)
Spain	1	130	(1)	0	84	(1)	0.10	(0)	0.28	(1)
Sweden	0	-165	(0)	0	-238	(0)	0.29	(0)	0.17	(0)
United Kingdom	0	-51	(0)	0	-285	(0)	0.43	(1)	0.26	(1)

(continued)

Table 4 (continued)

The table presents proxies for the quality of prior regulation, implementation strength and implementation timing of the two directives, as well as changes in supervisory resources. For the analyses, we use the proxies to partition the treatment 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* (in EUR thousands) that can be imposed on security issuers for violations of Article 2 of the MAD (CESR 2008). (ii) *Supervisory Powers* 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) *Action Taken* 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* (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 or indicated as a percentage of profits from violations, we set the binary indicator variable equal to '1'. (ii) *Shift in Enforcement* equals '1' if local auditors and regulators indicate that the enforcement activity for the provision of financial information has substantially increased over the sample period. We code this variable based on the answers to a survey that we sent to the technical departments of PricewaterhouseCoopers and the supervisory authority in each EU member state. (iii) *Compliance with CESR Std. 1* 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). We measure the implementation timing of the two directives as follows: (i) *'As if' Early Implementation* equals '1' if the country adopted the MAD (TPD) after the median entry-into-force date of the respective directive. We then randomly assign an entry-into-force date from the early implementation countries to the late implementation countries, and use it as an 'as if' implementation date in the analyses. (ii) We measure the *Abnormal Delay* (in days) of implementing the MAD or TPD by comparing the average delay between the transposition deadlines and the entry-into-force dates for all the other FSAP directives with the actual delay between the transposition and entry-into-force dates for the MAD and TPD. If it took longer than normal (i.e., *Abnormal Delay* is positive), we set the binary indicator variable equal to '1'. The two variables measuring supervisory resources are: (i) *Staff Level* equals the number of full-time employees working for the local authority supervising securities laws, scaled by the number of public firms in a country. We measure this variable as of 2003. We collect staff numbers from the annual reports of the local regulators and the survey in Central Banking Publications (2009). If only data for a joint regulator (i.e., including the banking and insurance sectors) is provided, we use the relative weight of the three sectors to allocate staff to securities regulation. In case of missing years in the data, we interpolate staff numbers. The number of public firms per country is from the World Bank. (ii) *Staff Growth* equals the changes in supervisory resources and is measured as the percentage change in full-time employees working for the local 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.

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

Panel A: Results for the Market Abuse Directive

Ln(Bid-Ask Spread) as Dependent Variable	*Prior Regulation*	*MAD Implementation Strength*			*MAD Timing*	
	Regulatory Quality 2003	*Maximum Fine*	*Supervisory Powers*	*Action Taken by 2009*	*'As if' Early*	*Abnormal Delay*
Prior Regulation Quality:						
High	-0.262***	–	–	–	–	–
	[-3.37]					
Low	-0.019	–	–	–	–	–
	[-0.24]					
Implementation Strength:						
Strong	–	-0.226***	-0.338***	-0.239***	–	–
		[-3.22]	[-2.76]	[-3.53]		
Weak	–	-0.071	-0.091*	-0.034	–	–
		[-0.86]	[-1.80]	[-0.41]		
Implementation Timing:						
Early	–	–	–	–	-0.177*	-0.242***
					[-1.68]	[-3.96]
'As if' Early/Abnormally Late	–	–	–	–	0.062	0.049
					[0.57]	[0.53]
F-test for Differences across Coefficients (p-value):						
High/Strong/Early = Low/Weak/Late	0.058	0.253	0.084	0.117	0.104	0.028
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

(continued)

Table 5 (continued)

Panel B: Results for the Transparency Directive

Ln(Bid-Ask Spread) as Dependent Variable	*Prior Regulation*	*TPD Implementation Strength*			*TPD Timing*	
	Regulatory Quality 2003	*Maximum Fine*	*Compliance CESR Std. 1*	*Shift in Enforcement*	*'As if' Early*	*Abnormal Delay*
Prior Regulation Quality:						
High	-0.371**	–	–	–	–	–
	[-2.11]					
Low	-0.080	–	–	–	–	–
	[-0.72]					
Implementation Strength:						
Strong	–	-0.379**	-0.384**	-0.375**	–	–
		[-2.29]	[-2.31]	[-2.21]		
Weak	–	-0.131	-0.127	-0.001	–	–
		[-1.22]	[-1.15]	[-0.01]		
Implementation Timing:						
Early	–	–	–	–	-0.190**	-0.375**
					[-2.26]	[-2.25]
'As if' Early/Abnormally Late	–	–	–	–	0.209	0.012
					[1.51]	[0.11]
F-test for Differences across Coefficients (p-value):						
High/Strong/Early = Low/Weak/Late	0.160	0.193	0.195	0.053	0.042	0.051
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

The sample comprises up to 611,969 firm-quarter observations from 25 EU and 27 non-EU countries between the first quarter 2001 and the second quarter 2009. We use the *Bid-Ask Spread* measured as the quarterly median quoted spread as the 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 treatment 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), the strength of MAD or TPD implementation (strong vs. weak), and the timing of implementing the MAD or TPD (early vs. 'as if' early/abnormally late). 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 in the models, but only report OLS coefficient estimates (t-statistics) for the main variables. 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: Combined Effects of Prior Regulation and Implementation Differences on the Liquidity Benefits of Tighter Securities Laws

Ln(Bid-Ask Spread) as Dependent Variable	*MAD Implementation Strength (IS)*			*TPD Implementation Strength (IS)*		
	Maximum Fine	*Supervisory Powers*	*Action Taken by 2009*	*Maximum Fine*	*Compliance CESR Std. 1*	*Shift in Enforcement*
Regulatory Quality (RQ):						
High RQ / Strong IS	-0.323***	-0.472***	-0.329***	-0.487**	-0.501**	-0.434**
	[-3.32]	[-3.88]	[-3.51]	[-2.44]	[-2.56]	[-2.23]
High RQ / Weak IS	-0.139*	-0.164***	-0.083	-0.131	-0.122	-0.050
	[-1.87]	[-2.98]	[-1.04]	[-1.20]	[-1.09]	[-0.40]
Low RQ / Strong IS	-0.061	-0.154**	-0.056	-0.083	-0.078	-0.146
	[-0.81]	[-2.52]	[-0.73]	[-0.72]	[-0.67]	[-1.16]
Low RQ / Weak IS	0.101	0.065	0.076	-0.066	-0.059	0.123
	[1.12]	[1.06]	[0.83]	[-0.47]	[-0.40]	[0.68]
F-test for Differences across Coefficients (p-value):						
High RQ / Strong IS = High RQ / Weak IS	0.156	0.016	0.054	0.103	0.092	0.086
High RQ / Strong IS = Low RQ / Strong IS	0.061	0.011	0.041	0.100	0.088	0.198
Low RQ / Strong IS = Low RQ / Weak IS	0.074	0.000	0.188	0.882	0.866	0.136
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

The sample comprises 611,969 firm-quarter observations from 25 EU and 27 non-EU countries between the first quarter 2001 and the second quarter 2009. We use the *Bid-Ask Spread* measured as the quarterly median quoted spread as the 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 each model we partition the treatment 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 in the models, but only report OLS coefficient estimates (t-statistics) for the main variables. We also report p-values from Wald tests assessing the statistical significance of the difference in coefficients across groups. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 7: Effects of Supervisory Resources on the Liquidity Benefits of Tighter Securities Laws

Ln(Bid-Ask Spread) as Dependent Variable	*Market Abuse Directive*			*Transparency Directive*		
	Staff Level 2003	*Staff Growth 2004 to 2009*	*Staff Level & Growth*	*Staff Level 2003*	*Staff Growth 2004 to 2009*	*Staff Level & Growth*
Supervisory Staff Level or Growth Separately:						
High	-0.243***	-0.210***	–	-0.403**	-0.430**	–
	[-3.30]	[3.00]		[-2.29]	[-2.21]	
Low	-0.030	-0.106	–	-0.029	-0.125	–
	[-0.45]	[-1.24]		[-0.30]	[-0.98]	
Prior Level and Growth Combined:						
High Level / High Growth	–	–	-0.293***	–	–	-0.513**
			[-3.10]			[-2.44]
High Level / Low Growth	–	–	-0.121	–	–	-0.146
			[-1.64]			[-1.17]
Low Level / High Growth	–	–	0.074	–	–	0.166
			[1.15]			[1.18]
Low Level / Low Growth	–	–	-0.095	–	–	-0.081
			[-1.26]			[-0.63]
F-test for Differences across Coefficients (p-value):						
High = Low	0.078	0.445	–	0.072	0.204	–
High Level / High Growth = High Level / Low Growth	–	–	0.208	–	–	0.125
High Level / High Growth = Low Level / High Growth	–	–	0.009	–	–	0.020
Low Level / High Growth = Low Level / Low Growth	–	–	0.000	–	–	0.165
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

The sample comprises 611,969 firm-quarter observations from 25 EU and 27 non-EU countries between the first quarter 2001 and the second quarter 2009. We use the *Bid-Ask Spread* measured as the quarterly median quoted spread as the 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. We first partition the treatment sample into two distinct groups by interacting the *MAD* and *TPD* variables with a binary indicator variable for the level of supervisory resources in 2003. Second, we partition the treatment sample into two distinct groups by interacting the *MAD* and *TPD* variables with a binary indicator variable for the change in supervisory resources measured by the percentage *Staff Growth* over the 2004 to 2009 period. In a third model, we partition the treatment sample into four distinct groups by interacting the *MAD* and *TPD* variables with the *Staff Growth* indicator and the binary indicator for the *Staff Level* in the year 2003. 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 in the models, but only report OLS coefficient estimates (t-statistics) for the main variables. 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: Sensitivity Analyses for the Capital Market Effects of Tightening EU Securities Regulation

Panel A: Ln(Bid-Ask Spread) as Dependent Variable

		Market Abuse Directive		*Transparency Directive*	
	N	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*
(1) Alternative Clustering:					
- Two-Way Clustering by Country and Quarter-Year	611,969	-0.199***	[-5.26]	-0.310**	[-2.20]
- Clustering by Economic Region	611,969	-0.199***	[-3.91]	-0.310**	[-2.06]
(2) Alternative Fixed Effects Structures:					
- Firm-Fixed Effects	611,969	-0.171***	[-4.84]	-0.239*	[-1.89]
- Add Separate Quarter-Year-Fixed Effects for Developed Markets	611,969	-0.161***	[-2.83]	-0.330**	[-2.29]
- Add Separate Quarter-Year-Fixed Effects Interacted with Firm Size	611,969	-0.194***	[-4.09]	-0.318**	[-2.28]
(3) Alternative Sample Specifications:					
- European Union only	101,669	-0.196***	[-4.01]	-0.306**	[-2.16]
- Random Benchmark Sample	166,204	-0.195***	[-4.08]	-0.304**	[-2.18]
- Exclude UK, Germany, France, and Sweden	555,383	-0.154***	[-2.66]	-0.205**	[-2.40]
- Include Bulgaria and Romania (EU members from 2007 on)	612,647	-0.162***	[-2.65]	-0.304**	[-2.17]
(4) Alternative Control Variables:					
- Controlling for macroeconomic factors (Inflation, GDP per Capita, Δ GDP per Capita)	611,818	-0.201***	[-4.03]	-0.311**	[-2.21]
- Not Controlling for Ln(Share Turnover$_{t-4}$)	611,969	-0.185***	[-4.94]	-0.263**	[-2.08]

(continued)

Table A1 (continued)

Panel B: Zero Returns as Dependent Variable

		Market Abuse Directive		*Transparency Directive*	
	N	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*
(1) Alternative Clustering:					
- Two-Way Clustering by Country and Quarter-Year	780,434	-0.046***	[-4.31]	-0.041**	[-2.10]
- Clustering by Economic Region	780,434	-0.046***	[-3.26]	-0.041*	[-1.92]
(2) Alternative Fixed Effects Structures:					
- Firm-Fixed Effects	780,434	-0.047***	[-4.20]	-0.027*	[-1.67]
- Add Separate Quarter-Year-Fixed Effects for Developed Markets	780,434	-0.047***	[-3.70]	-0.038**	[-2.09]
- Add Separate Quarter-Year-Fixed Effects Interacted with Firm Size	780,434	-0.046***	[-3.69]	-0.044**	[-2.24]
(3) Alternative Sample Specifications:					
- European Union only	118,907	-0.046***	[-3.51]	-0.046**	[-2.27]
- Random Benchmark Sample	210,481	-0.046***	[-3.57]	-0.044**	[-2.26]
- Exclude UK, Germany, France, and Sweden	722,038	-0.042***	[-3.02]	-0.005	[-0.33]
- Include Bulgaria and Romania (EU members from 2007 on)	782,069	-0.052***	[-3.99]	-0.041**	[-2.22]
(4) Alternative Control Variables:					
- Controlling for macroeconomic factors (Inflation, GDP per Capita, Δ GDP per Capita)	779,439	-0.046***	[-3.70]	-0.041**	[-2.31]
- Not Controlling for Ln(Share Turnover$_{t-4}$)	780,434	-0.042***	[-3.54]	-0.033*	[-1.81]

Panel C: Alternative Dependent Variables

		Market Abuse Directive		*Transparency Directive*	
	N	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*
- Ln(Bid-Ask Spread$_{PO}$)	607,015	-0.184*	[-1.83]	-0.350**	[-2.24]
- Bid-Ask Spread	611,969	-0.004***	[-2.82]	-0.004*	[-1.79]
- Cost of Capital (r_{GLS})	337,466	-0.570**	[-2.22]	-0.198*	[-1.68]
- Dividend Yield	327,387	-0.089	[-1.21]	-0.071	[-1.60]

(continued)

Table A1 (continued)

The sample comprises firm-quarter observations from up to 29 (35) EU (non-EU) countries between the first quarter 2001 and the second quarter 2009. We use two dependent variables: (1) in Panel A we use the *Bid-Ask Spread* measured as the natural logarithm of quarterly median quoted spread, and (2) in Panel B we use *Zero Returns* measured as the proportion of trading days with zero daily stock returns in a quarter. *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 various model 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 effects structures. That is, we (i) replace country- and industry-fixed effects with firm-fixed effects, (ii) add separate quarter-year fixed effects for developed markets, or (iii) add separate quarter-year fixed effects that are interacted with *Market Value*. We identify developed markets based on the Morgan Stanley Capital International database. Third, we estimate the regressions for (i) the EU countries only (treatment sample), (ii) limit the benchmark sample to 150 randomly selected firms from each country, (iii) exclude the four treatment countries with most observations (UK, Germany, France, and Sweden), or include Bulgaria and Romania that became EU members in 2007. Fourth, we use alternative control variables. That is, we (i) control for macroeconomic factors (lagged by one year) by adding inflation, annual GDP per capita, and the percentage change (Δ) in annual GDP per capita as control variables to the model, or refrain from controlling for share turnover. Fifth, we use the following alternative dependent variables: (i) the median quoted spread scaled by the earliest available price for each firm (*Bid-Ask Spread*$_{P0}$), (ii) raw *Bid-Ask Spread*, (iii) Cost of Capital (r_{GLS}), or (vi) *Dividend Yield*. r_{GLS} is the implied cost of capital estimate based on time-series forecasts of earnings following Hou et al. (2009) and the 12-year version of the Gebhardt et al. (2001) valuation model. We estimate r_{GLS} using the market value at the end of each quarter. We measure *Dividend Yield* as the actual dividends paid during the last fiscal year divided by the stock price at the end of each quarter. We exclude zero-dividend observations in computing the Dividend Yield. Unless otherwise indicated, for the regressions with *Bid-Ask Spread* and *Zero Returns* as dependent variables, we include the same set of control variables as in Table 2 (see Table 1 for description). In the regression with r_{GLS} as the dependent variable, we include the following control variables: *inflation*, *GDP per capital*, *total assets*, *financial leverage*, and *return variability*. In the regression with *Dividend Yield* as a dependent variable, we include *asset growth* in addition to the controls we use in the r_{GLS} regression. *Inflation* is the country-specific yearly percentage change in consumer price indices, computed at the end of each quarter (source: International Monetary Fund). Annual *GDP per capita* is from the World Bank (in constant US$ as of 2000). *Total Assets* are denominated in US$ million. We compute *Financial Leverage* as the ratio of total liabilities to total assets. *Asset Growth* is the year-to-year percentage change in total assets. We measure accounting data as of the most recent fiscal year before each quarter (*FYE*-1). We lag market-based data used as control variables by four quarters (*t*-4). We require more than 10 daily observations to compute quarterly variables. Except for variables with natural lower or upper bounds, we truncate all variables at the first (fifth) and 99th (95th) percentile (r_{GLS} and *Dividend Yield*). For all regressions, we include the same fixed effects as in Table 2, but only report OLS coefficient estimates (t-statistics) for the main variables. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).