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

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Abstract

This paper examines capital market effects of changes in securities regulation. We analyze two key capital market directives in the European Union (EU) that tightened market abuse and transparency regulation and, in particular, their 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 for identification and uses cross-sectional variation in the capital-market effects to highlight the role of implementation and enforcement for regulatory outcomes. We find that, on average, market liquidity increases and firms' cost of capital decreases 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. Overall, 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 securities 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 and depends crucially on how regulation is designed, implemented and enforced. While the design of the rules generally receives much attention in the literature and in regulatory debates, we have far less evidence on how the implementation and enforcement of securities regulation affect regulatory outcomes.

To contribute to this debate, we examine the capital-market effects of recent changes in EU securities regulation. We choose the EU setting because it has several desirable features. First, it allows us to analyze the same regulatory change across EU member states at different points in time. Thus, we can better isolate regulatory effects from general time trends and other major events than studies examining a single regulatory event. 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 post-enactment variation across countries allows us to focus on implementation and enforcement separately from the rule change, which is difficult to do when studying a single regulatory event. Third, the EU directives we study pertain to 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 disclosure. However, as there were prior EU directives and national laws banning insider trading and stipulating extensive reporting requirements, the new directives are largely

[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).

geared towards tightening securities regulation, in particular, by improving supervisory and enforcement 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. Fourth, the setting allows us to analyze the extent to which regulatory outcomes differ depending on prior regulation and hence to study the interaction between initial conditions and new regulation. One hypothesis is that countries with weaker prior regulation exhibit larger capital-market benefits from the new directives. 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.

In this setting, we analyze changes in stock market liquidity around the staggered implementation of the two EU directives. We measure liquidity using the bid-ask spread and the percentage of zero-return days. Both proxies have been used extensively in the literature. In addition, we examine changes in the cost of capital using the implied cost of capital and dividend yields as proxies. Improvements in market liquidity and the cost of capital commonly serve as justifications for securities regulation and they are also closely linked to the stated goals of the EU capital market directives (e.g., Lamfalussy, 2000; CRA, 2009). That said, proxies for market liquidity are better suited for the setting and our identification strategy (see Section 3) and hence, we conduct the cost of capital analysis merely 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 member states, we also introduce separate

quarter-year fixed effects for the EU countries to account for common shocks and trends in EU capital markets. Thus, the identification of the regulatory effects comes entirely from the fact that the effective dates of the directives fall into different quarters across EU countries, which addresses many of the common concerns about measuring regulatory effects (see Mulherin, 2007, for discussion of such concerns).

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 percent for the MAD and between 5 and 21 percent for the TPD. We also find that the cost of capital decreases when the directives come into force. Depending on the proxy, our estimates for the cost of capital reduction range between 9 and 58 basis points for the MAD and between 7 and 20 basis points for the TPD. However, the cost-of-capital effects are less precisely estimated than the market liquidity effects. 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 are robust to the introduction of firm-fixed effects, within-EU estimation, additional quarter-year fixed effects for more developed countries, and controls for macroeconomic changes. To gauge our timing-based identification strategy, we conduct three 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 the year leading up to the directives does not exhibit statistically significant liquidity changes, that liquidity starts to improve around the time 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 become smaller in magnitude and significance as we move away from the true implementation dates, which is what we expect if the directives drive the effects. In fact, the liquidity effect peaks around the true implementation date for MAD and shortly afterwards for TPD. Finally, 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.

Next, we turn to the cross-sectional analyses exploiting differences in prior regulation and in the implementation and enforcement of the directives across EU countries. We document that the liquidity effects of the two directives are stronger in countries with higher regulatory quality.[2] 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 (e.g., resource constraints, inefficient bureaucracies, political pressures) appear to be at work when implementing new rules.

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

[2] 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.

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. There is no significant change in liquidity for countries with weak prior regulation and weak implementation. Moreover, stricter implementation of the new directives has an incremental effect, but only in countries with high past regulatory quality or in countries where securities regulators have high prior staff levels. Thus, there is strong evidence of hysteresis. Countries with weaker securities regulation do not catch up with stronger countries following the new EU directives. This conclusion and the documented role of prior conditions have important implications for the expected outcomes of regulatory reforms as well as efforts to harmonize regulation across countries.

Our paper makes several contributions to the literature. First, we examine a novel setting to provide evidence that stronger securities regulation can have significant economic benefits in terms of market liquidity and cost of capital. Prior studies often cast doubts 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, 2006).[3] 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 the Sarbanes-Oxley Act (e.g., Chhaochharia and Grinstein, 2007; Zhang, 2007; Li et al. 2008; Iliev, 2009) is decidedly mixed, with several studies emphasizing that securities regulation can have significant costs, especially for smaller firms.

Second, a key challenge for all studies is isolating the effects of regulation from other concurrent events. The early studies examining securities regulation in the 1930s often lack a (convincing) control group (Mahoney and Mei, 2006). Similar concerns apply to many studies on the Sarbanes-Oxley Act (Leuz, 2007; Hochberg et al., 2009) as well as on Regulation Fair Disclosure (Collver, 2007). In contrast, our study exploits the staggered implementation of the same regulatory act in 29 countries to identify the regulatory effects, which offers several advantages compared to prior studies that generally rely on a regulatory act in a single country.[4]

Third, 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). Our findings also add to the budding literature on securities law enforcement. As Bhattacharya (2006) points out, there is relatively little evidence on the role of enforcement. In an important paper, Bhattacharya and Daouk (2002) provide evidence that the first enforcement of insider

[3] Greenstone et al. (2006) also provide evidence on significant benefits from an extension of SEC regulation in the 1960s. However, their study provides little evidence on the source of the benefits. Bushee and Leuz (2005) study a similar extension in 1999 and find more mixed evidence.

[4] A related 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 effects of investor protection laws on the financing and investment decisions of firms in the mining industry.

trading regulation lowers firms' cost of capital. Subsequent papers use the same dataset and provide evidence on other capital-market effects associated with insider trading regulation and enforcement (e.g., Bushman et al., 2005; Ackerman et al., 2008). Our analysis goes beyond insider trading regulation. Moreover, prior evidence on securities law enforcement is 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]

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,

[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.

2009). However, these arguments often set aside problems of how to implement and enforce 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 is necessarily undesirable. Arguments in favor of 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 are an efficient solution in securities markets. He suggests that, 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

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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).

securities regulation. Obviously, this observation is not sufficient to settle the case. As mentioned earlier, there are several reasons to be skeptical about the benefits of regulation. Consistent with these concerns, much of the empirical evidence on the effects of securities regulation is mixed and often negative (see Mulherin, 2007, and also Section 1 of this paper).

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 track record with respect to implementing regulation in the past is likely revealing about the ability and political will of its government to put in place and enforce regulation that induces (curbs) behavior that is deemed socially desirable (undesirable). In sum, the benefits of securities regulation (or any other 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.

[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.

Towards this end, our analysis exploits regulatory changes in EU capital markets for which implementation and enforcement issues are pertinent. The EU setting also has the desirable feature that a directive applies to all member states, so the regulatory act is held constant across countries. However, the transposition of the directive into national law and its supervision, including 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, which allows us to examine the role of prior regulation. There are two competing hypotheses. One prediction is that the effects are larger in countries where prior securities regulation has been weak, which essentially implies that, as a result of the EU directives, the weaker countries are "catching up" with the stronger ones (*catching-up hypothesis*). An alternative prediction, which follows from our earlier discussion in this section, is that regulatory outcomes exhibit "hysteresis," i.e., the capital-market effects from the new directives are larger in countries with stronger past regulation (*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 still 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 information regulation. These two 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 stipulate appropriate supervisory and enforcement regimes.

To be more specific, the MAD was passed by the EU legislature in January 2003 followed by several implementing directives in December 2003.[10] 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 and dealing with insider trading and market manipulation and to give this authority the necessary supervisory and investigative powers. The MAD further requires cooperation among national supervisory authorities and some, although not complete, harmonization of penalties. It replaces Directive 89/592/EEC, which in 1989 already required EU member states to ban insider trading. Thus, while the MAD expands market abuse regulation in some areas, it primarily tightens and

[10] 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.

harmonizes the implementation and enforcement of existing EU market abuse regulation (e.g., Lamfalussy, 2000; CRA, 2009).[11]

The TPD was passed by the EU legislature in December 2004 and its implementing directive was enacted in March 2007. The directive requires issuers of traded securities to ensure appropriate transparency for investors by disclosing and disseminating periodic and ongoing regulated information. Regulated information comprises 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 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.[12] The TPD also requires that the authority is given appropriate enforcement tools, including the power to carry out on-site inspections.[13] Thus, the TPD primarily clarifies and harmonizes existing disclosure regulation and improves enforcement.

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[11] For instance, the Financial Services Authority (FSA) in the U.K. received additional powers that allow it to obligate persons to comply with market abuse provisions and 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 CESR (2010) for further examples.

[12] 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.

[13] 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).

To complete our description of the institutional setting, we note that, aside from the MAD and the TPD, there are two other so-called Lamfalussy directives, namely, the Prospectus Directive (PD) and the Markets in Financial Instruments Directive (MiFID). Together, the four directives represent the key securities market directives under the FSAP. 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.[14]

3. Research Design and Data

3.1. Identification Strategy and Empirical Model

In studying the economic consequences of the two directives, we focus on market liquidity and the cost of capital. We do so for two reasons. First, there are well-established economic links for these construct that guide the analysis. Reducing insider trading and market abuse as well as enhancing transparency should reduce information asymmetries between investors and hence increase market liquidity (e.g., Diamond and Verrecchia, 1991; Verrecchia, 2001). Increased transparency should reduce non-diversifiable estimation risk, which in turn lowers the cost of capital (e.g., Coles et al., 1995; Lambert et al., 2007). Second, improvements in market liquidity and the cost of capital commonly serve as justifications for securities regulation. In fact, the stated goals of the two capital-market directives are to increase market confidence and,

[14] In our sensitivity analyses, 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. Along the same lines, we also check that our results are not affected by the introduction of International Financial Reporting Standards in the EU and elsewhere in the world.

more specifically, to lower trading costs and firms' cost of capital (e.g., Lamfalussy, 2000; Enriques and Gatti, 2008; CRA, 2009).

We test the hypotheses developed in Section 2 using a panel dataset of quarterly observations. The choice of quarterly data reflects a tradeoff between reliably measuring capital-market effects over some interval and capturing changes in these variables in a timely fashion, i.e., when the MAD and TPD come into force. A primary concern about our setting is that market-wide changes (e.g., macroeconomic shocks), general time trends in capital-market variables as well as other regulatory and economic events (e.g., the financial crisis) confound our analysis. To address this concern, our identification strategy exploits the staggered implementation of the two directives across EU member states to isolate the effect of the two EU directives on the capital-market variables (see Figure 1 for an illustration).

There is considerable variation in the implementation timing across EU countries. After the enactment of an EU directive, each member state must transpose the directive into national law within a specified timeframe, which typically requires legislative changes. In the case of MAD and TPD, transposition required amending national law(s) in all member states. This process depends on a country's constitution and legislative system and varies considerably across member states. Moreover, the process is often lengthy and generally inflexible.[15] These features and the fact that the regulation is at the EU level (and hence not specific to a particular country) ameliorate many concerns that often arise about the endogeneity of regulation and of its timing in a single-country setting (e.g., Ball, 1980; Mulherin, 2007).

[15] 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 same dates as instruments to estimate the effects of financial integration on international business cycle synchronization.

Given our identification strategy, which relies heavily on the timing of the regulatory effects, market liquidity proxies seem better suited than cost of capital proxies because they can be measured over relatively short intervals. Moreover, a firm's cost of capital is more likely to anticipate regulatory changes given its forward-looking nature.[16] Nevertheless, we estimate quarterly regressions for market liquidity and the cost of capital, given improvements in both are the stated goals of the MAD and the TPD. Specifically, we use the following model:

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

The dependent variable, *CapEff*, stands for the capital-market effects (i.e., liquidity and cost of capital). *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. As the key variables of interest vary only at the country level, we draw statistical inferences based on standard errors that are clustered by country.

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 cost of capital (and to better estimate the coefficients for the control variables). Our extensive fixed-effects structure captures any unobserved (time-invariant) heterogeneity across countries and industries (using the Campbell, 1996, industry classification). It also includes separate quarter-year fixed effects for EU and non-EU countries, which let us estimate a flexible quarterly time trend and at the same time

[16] Market liquidity proxies are less likely to anticipate the 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 effect is likely to be small.

control for common economic shocks. This specification essentially amounts to within-EU estimation and eliminates variation or shocks to the capital-market variables that are common to all EU member states in a given quarter.[17]

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 (1).[18] The bid-ask spread sample, which we use in our main analyses, comprises 655,588 firm-quarter observations from 27 EU countries and 27 non-EU countries. To minimize the issue that firms are subject to a "mix" of regulations, the sample excludes firms that follow U.S. GAAP in their financial reporting and firms with U.S. cross-listings. In addition, we eliminate firms with market values below US$ 1 million, and we require at least four valid quarterly observations per firm and, for the benchmark countries, more than 20 firms per country. Table 1 provides information on the sample composition by EU country (together with descriptive information on the country-level partitioning variables described in Section 4.3).

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[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.

[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.

Next, we construct binary indicators for the Market Abuse Directive (MAD) and the Transparency Directive (TPD). They are the primary variables of interest. We select the date when the national law(s) that implemented the respective directive came into force in each member state (*Entry-Into-Force* date). We take these entry-into-force dates from publications by the European Commission for the MAD and by Linklaters LLP, an international law firm, for the TPD. We validate the dates from these sources with the dates on which each EU member state informed the European Commission of their compliance with MAD and TPD. In case of discrepancies, we contact the national securities regulator to resolve the issue.

Table 1 provides the entry-into-force dates for the MAD and TPD that we use in the analyses. The *MAD Dates* vary from April 2004 (Lithuania) to January 2007 (Bulgaria and Romania), the *TPD Dates* from January 2007 (Bulgaria, Germany, United Kingdom) to August 2009 (Czech Republic and Italy). This variation in the timing when the two directives became effective across member states 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 with the quarter of the country-specific MAD and TPD entry-into-force dates, respectively.

We use several proxies for market liquidity and the cost of capital. Our primary measure of market liquidity is the *Bid-Ask Spread*, which is commonly used in empirical studies to capture information asymmetry (e.g., Welker, 1995; Healy et al., 1999; Leuz and Verrecchia, 2000; Lang et al., 2009). 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.[19] To obtain firm-quarter observations, we compute the median daily spread over the quarter for a given firm. As a second proxy, we use *Zero Returns* measured as the proportion of trading days with zero daily

[19] Intra-day data to further decompose the spread into various components is not widely available. To address this shortcoming, we use an alternative liquidity proxy.

stock returns out of all potential trading days in a given quarter (e.g., Lesmond et al., 1999; Bekaert et al., 2007).

We use the implied cost of capital and dividend yields as proxies for the cost of equity capital. The basic idea of implied cost of capital models is to substitute price and earnings forecasts into an accounting-based valuation equation and to back out the cost of capital as the internal rate of return that equates current stock price with the expected sequence of future (abnormal) earnings (e.g., Botosan, 1997; Claus and Thomas, 2001; Gebhardt et al., 2001; Hail and Leuz, 2006). Conceptually, the models are consistent with discounted dividend valuation. Since the estimation of the implied cost of capital neither requires a long time series of data nor a stance on the level of market integration, it is particularly suited for cross-country settings (e.g., Hail and Leuz, 2006; Pástor et al., 2008; Lee et al., 2009). To avoid problems with these models due to analyst forecast bias and forecast staleness (e.g., Easton and Sommers, 2007), and to include firms (and countries) without analyst coverage in our sample, we do not use analyst earnings forecasts. Instead, 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. We plug these earnings 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.[20] We assign this cost of capital estimate to the quarter of the pricing date.

[20] 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

Our second cost of capital proxy, a firm's 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 the *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.

In the liquidity regressions, we control for firm size using the market value of equity, share turnover and return variability (Chordia et al., 2000; Leuz and Verrecchia, 2000). We follow the prior literature and estimate the liquidity models in a log-linear form using the natural logarithm of the continuous variables. We lag the above control variables by four quarters. For the cost-of-capital specifications, we follow Hail and Leuz (2006, 2009) and control for contemporaneous inflation, GDP per capita, firm size measured by total assets, financial leverage, and return variability. In the dividend yield regressions, we control for asset growth because dividend yields also reflect differences in growth expectations (unlike the implied cost of capital, which explicitly accounts for growth forecasts in its construction). We measure total assets and leverage as of the last fiscal year-end, and return variability over the last calendar year before the pricing date used in the cost of capital imputation. We obtain financial data from Worldscope, price and volume data from Datastream, inflation from the International Monetary Fund, and GDP per capita from the World Bank.[21] Except for variables with natural lower and upper

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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).

[21] 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 change our results.

bounds, we truncate all variables at the first and 99th percentile.[22] Table 2 provides descriptive statistics of the dependent and independent variables as well as further details on the variable measurement.

4. Results

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

We begin our analysis by examining the effects on firms' stock market liquidity and cost of capital following the implementation of MAD and TPD in the EU member states. As described in Section 2, MAD and TPD are an attempt to harmonize and tighten EU securities regulation, particularly, with respect to the implementation and enforcement of key existing regulations. We use cross-sectional, time-series panel regressions that benchmark EU firms after the MAD and the 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 3 presents results from OLS regressions for the average capital-market effects of the two directives. Panel A reports the coefficient estimates and t-statistics using the natural log of bid-ask spreads as the dependent variable. 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 these regressions is high with an R^2 of 78 percent. The firm-specific control variables are highly significant and exhibit the expected signs. That is, large firms and firms with a high trading volume have lower bid-ask spreads while firms with more volatile returns have higher spreads. In terms of our test variables, the coefficient on MAD is negative and statistically significant.

[22] Because the Hou et al. (2009) implied cost of capital estimates as well as dividend yields are fairly noisy, we truncate r_{GLS} and *Dividend Yield* at the fifth and 95th percentile. We also delete all cost of capital estimates that fall below the local yearly inflation rate, because such estimates seem implausible.

The estimated effects are also economically significant. For instance, a coefficient of -0.147 suggests that, on average, bid-ask spreads decrease by 13 percent, which equals a 31-basis point reduction for the median pre-directive spread of 230 basis points. The spread effects around TPD are also negative. A coefficient estimate of -0.229 suggests a bid-ask spread reduction by 20 percent.[23] The MAD and TPD coefficients are also significant and of similar magnitude when we introduce the variables jointly into the model.

Panel B of Table 3 reports results for the combined model using alternative liquidity and cost of capital measures as the dependent variable. First, we address the concern that spreads are scaled by contemporaneous stock price and hence our spread results could be driven by changes in the deflator over time. We therefore re-compute the spread variable and scale the daily spread between the bid and the ask price 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. As shown in the first column of Panel B, the results are very similar and, if anything, stronger using the earliest available price to scale spreads (which would be expected if liquidity effects are reflected in stock prices). We also confirm that our results are similar and the inferences the same if we estimate linear, rather than log-linear, spread regressions. Next, we use the proportion of zero returns as dependent variable. One advantage of this metric is its exclusive reliance on price data, which are more frequently available in an international setting than spread or trading volume data (e.g., Lesmond et al., 1999). Using zero returns, the coefficients on MAD and TPD are negative and highly significant, suggesting an increase in market liquidity after the directives, consistent with the spread results. The estimated coefficients on MAD and TPD suggest an

[23] We compute the percentage effect on the spread as follows: $e^{-0.147} - 1 = -0.137$ for MAD and $e^{-0.229} - 1 = -0.205$.

increase in market liquidity around 14% and 5% relative to the respective pre-directive levels of market liquidity, which is very similar to the magnitude of estimated spread effects for MAD and smaller but still economically significant for TPD.

In the last two columns of Panel B, we report results for the cost of capital measures. Consistent with prior studies and the notion that cost of capital estimates are relatively noisy, the explanatory power of the implied cost of capital (r_{GLS}) and dividend yield models is substantially smaller (R^2 equals 34% and 25%, respectively) than of the liquidity models. In the r_{GLS} model, the control variables behave as expected and are significant, except for return variability. Cost of equity capital is higher for firms in less developed countries with higher inflation, for firms with higher financial leverage, and for smaller firms. In the dividend yield model, several of the controls are insignificant and return variability has the wrong sign, pointing to substantial measurement error in this variable. In both cost of capital models, the coefficients on MAD and TPD are negative and statistically significant, except for the MAD using dividend yields for which the coefficient has a two-sided p-value of 0.20, suggesting that the two directives reduce firms' costs of capital. Using the implied cost of capital model, the estimated reduction in cost of capital amounts to 58 basis points for the MAD and 20 basis points for the TPD. The effects are considerably smaller (9 and 7 basis points, respectively) using dividend yields as a proxy. Overall, the cost-of-capital results are consistent with our findings for market liquidity as well as the cost of capital effects in Bhattacharya and Daouk (2002) after the first enforcement of insider trading rules. Nevertheless, the estimated cost of capital effects should be interpreted cautiously given the difficulties in measuring the underlying construct, particularly on a quarterly basis.

An additional concern about cost of capital effects in our setting is that both proxies are derived from contemporaneous stock prices and hence are forward-looking by construction. As

a result, it is possible that the cost of capital reflects the regulatory effects from the directives prior to the implementation by the EU member states. That is, if investors lower the discount rate they apply to future cash flows in expectation of either of the two directives, this effect should increase prices before the directives become effective, and hence should already be present in the pre-directives cost of capital estimates.[24] Conceptually, such an anticipation effect poses a challenge to our identification strategy, which is based on the differential timing of the regulatory changes. Moreover, the amount of anticipation likely differs across countries, which poses an even bigger problem to our subsequent cross-sectional analyses. Hence, for the remainder, we focus on market liquidity effects. We use bid-ask spreads as the primary dependent variable but note that the findings are very similar, and in some cases stronger, using zero returns. As discussed earlier (see footnote 16), market liquidity proxies are conceptually less likely to reflect future regulatory changes than the cost of capital. That said, it is possible that the passage of the MAD and the TPD at the EU level raises market liquidity in some countries or that some countries make changes prior to their formal implementation of the directives. Such effects would make the entry-into-force dates less relevant for our analysis. We gauge this issue in the next section.

4.2. Assessing Identification and Sensitivity Analyses

In this section, we gauge the robustness of our findings and address several concerns about our identification strategy. One important question is whether the implementation dates provide reasonably sharp identification and whether they are indeed important for the estimated effects. A related concern is that the indicators for the directives might reflect unspecified trends or

[24] Consistent with this conjecture, we find that, in the cost of capital regressions, the coefficient on TPD is significantly negative if we shift the implementation dates by as much as six quarters back in time. As we show in Table 4, Panel B, this is not true for spreads (and also not true for the zero returns).

changes in EU market liquidity that are unrelated to the directives. The use of EU-specific quarter-fixed effects mitigates but does not fully eliminate this concern. We therefore conduct three additional tests to assess this concern and to gauge our identification strategy.

First, we introduce three separate indicator variables into the model: one for the year prior to the directives, one for the year of the implementation, and one for the years afterwards. This analysis essentially maps out the market liquidity patterns around the directives. Table 4, Panel A provides the results from these regressions. In both cases, we find that the year leading up to the directives does not exhibit significant liquidity changes. Liquidity starts to improve around the time MAD and TPD come into force, i.e., the coefficient in the year before the respective directive is significantly different from the coefficient for the year the directive became effective. As introducing finer time-period partitions around the directives quickly results in collinearity and variance inflation problems, we conduct a second test. 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 treatment effects of the directives should be attenuated as we move away from the true implementation dates. Panel B in Table 4 shows that the coefficient for each directive becomes much smaller 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 and that liquidity changes close to the entry-into-force dates. The attenuation of the coefficients on MAD and TPD as we move back in time also confirms that the liquidity regressions do not suffer from significant anticipation problems. Note that the decline (and insignificance) of the MAD coefficient as we move our assignment beyond the true implementation date does *not* indicate

that the increase in liquidity is temporary. By shifting the date forward in time, we essentially assign treatment quarters to the pre-directive period, which should reduce the treatment coefficient even when the treatment effect is permanent. For the TPD, the coefficients continue to increase beyond the true entry-into-force dates, peaking in quarter t+4 before reversing. This pattern suggests a delay of about a year until the TPD became fully effective, which is plausible considering that setting up a review and monitoring process for financial information requires hiring and training additional enforcement personnel.[25]

Finally, we 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.[26] Using 300 replications, the estimated average placebo effect is close to zero (-0.0017) and there are only 3 (0) cases for which the regressions produce a coefficient that is larger (i.e., more negative) than the MAD (TPD) coefficient reported in Panel A.[27] Thus, the placebo tests indicate that our results are unlikely to be spurious or the result of chance.

We conduct a number of additional robustness checks and report the results in Panel C of Table 4. First, we confirm that our inferences are unchanged if we use alternative ways to cluster our standard errors, i.e., when we use two-way clustering (by country and quarter) or regional clustering. The latter combines several EU countries (e.g., Western Europe, Eastern Europe, etc.) and hence implies fewer clusters than using the number of countries.

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[25] Our surveys of regulators and auditors confirm the conjecture that the enforcement activities under TPD were gradually implemented after the entry-into-force dates. For example, evidence on enforcement changes in Germany shows that it may take a year or two before an enforcement institution operates at full capacity (see Ernstberger et al., 2010).

[26] We extend the randomization to the second quarter of 2004 to maximize the sample period for the placebo analysis but stop one full quarter before the first large EU country (i.e., Germany) adopted a directive.

[27] The placebo analysis also provides a sensitivity check on our inferences as this procedure amounts to bootstrapping the significance levels of the coefficients.

Second, we examine alternative fixed-effect structures. The panel shows that the results are similar using firm-fixed effects, though the TPD coefficient has only a two-sided p-value of 0.128. Then, we augment the base model adding separate quarter-year fixed effects for developed countries. This specification accounts for the possibility that developed markets exhibit different liquidity trends or are differentially affected by economic shocks during the sample period. The results for MAD and TPD become stronger with this specification. Along the same lines, we introduce separate size or, alternatively, volatility coefficients for *each* quarter. This extension of the fixed-effects structure should absorb economic shocks that affect larger (less volatile) firms differently from smaller (more volatile) firms. We find that the results and inferences do not change, and report the regression using size interactions in Panel C. Similarly, we introduce interactions between the quarter-year-fixed effects and the industry dummies to allow for different trends across industries or shocks in certain industries (e.g., banks during the crisis). Again, we find that the coefficients for MAD and TPD are very similar to those reported and the inferences remain the same. As a final variation on our fixed-effects structure, we add interactions between the country indicators and firm size or, alternatively, between the country indicators and the industry dummies to control for differences in the composition of firms across treatment countries. Again, the results are similar to those reported in Table 3 and the inferences remain unchanged. Based on all these tests, we conclude that it is unlikely that our liquidity results are simply an artifact of economic shocks to particular groups of firms or differences in the composition of firms.

Third, we gauge the sensitivity of our findings to the benchmark sample and the fact that a few countries comprise the bulk of our sample. The panel shows that we obtain very similar results to those reported in Panel A estimating our regressions within the EU sample only.

Dropping the two largest benchmark countries (i.e., Japan and the U.S.) or, alternatively, limiting the number of observations per benchmark country to 150 randomly selected firms does not alter the results. We also confirm that our results are not solely driven by the U.K., which contributes by far the largest number of observations to the treatment sample. We find similar results when we drop U.K. observations from the analysis. As a final check, we restrict the estimation to firms that enter our sample before 2004, essentially holding the sample constant over time. The results are very similar and, if anything, stronger than those reported in Panel A.

Finally, we address the concern that the results might reflect macroeconomic changes or other regulatory changes in the EU during the sample period. First, we find that adding several macroeconomic controls, i.e., inflation, the level of and growth in per-capita GDP, leaves our results virtually unchanged (Panel C). Second, we augment our empirical model by indicators for the two other major capital-market directives that came into force during our sample period, namely, the Prospective Directive (*PROSP*) and the Markets in Financial Instruments Directive (*MiFID*). Along with the MAD and the TPD, they form the four Lamfalussy Directives. As shown in Panel C, the two other directives do not exhibit significant liquidity effects.[28] Moreover, the coefficients on MAD and TPD remain negative and significant when we introduce all four Lamfalussy Directives in the model, and the four directives together are jointly significant.

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

The previous analyses estimate the average capital-market effects from the two EU directives. However, as discussed in Section 2, it is unlikely that the two directives are

[28] This finding is in contrast to a recent paper by Cumming et al. (2010). They find that market liquidity on EU stock exchanges increases after November 2007 and attribute this effect to the introduction of MiFID.

uniformly and consistently implemented in all member states and hence that they yield the same capital-market 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 the two new directives complement existing securities regulation, mainly by improving the enforcement regime. Alternatively, one could argue that countries with weaker securities regulation should benefit the most from harmonized, EU-wide efforts to improve extant regulation and enforcement. To explore these arguments, we examine cross-sectional differences in the liquidity effects of the directives, exploiting differences in prior regulation and in the implementation and enforcement of the directives.

We begin with an analysis of the cross-sectional differences in the liquidity effects of MAD and TPD conditional on *prior* regulation by estimating the following variation of our model:

$$CapEff = \beta_0 + \beta_1\, EU\ Directive \times Quality\ of\ Prior\ Regulation_{High} + \beta_2\, EU\ Directive \times Quality\ of\ Prior\ Regulation_{Low} + \sum \beta_j\, Controls_j + \sum \beta_i\, Fixed\ Effects_i + \varepsilon. \quad (2)$$

The two *Quality of Prior Regulation* variables stand for binary indicators splitting the post-MAD or TPD observations into two distinct groups, one for EU countries with higher regulatory quality (or stronger prior regulation) and one for the remaining EU member states. We use two institutional variables to partition the data, one that is more general in nature but measures a country's past track record implementing regulation and one that is more specific to prior securities regulation enforcement: (i) *Regulatory Quality* is 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. Higher index values indicate better regulatory quality; (ii) *Supervisory Staff* measures the number of full-time employees

working for the supervisory authority in charge of securities regulation.[29] 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 assume that, in general, a higher ratio of supervisory staff to the number of supervised firms indicates better enforcement quality. Both variables are measured as of 2003 and reflect features of countries' regulatory environments *prior* to the directives. For the analyses, we split the treatment sample countries by the EU median for these variables (see also Table 1).[30]

We report the liquidity effects of tighter EU securities laws conditional on the quality of prior regulation in Table 5 underneath the heading for each directive. Throughout the cross-sectional analyses we use bid-ask spreads as proxy for liquidity, but the results are very similar for zero returns (i.e., slightly weaker for MAD but stronger for TPD). As in Table 3, the models include the full set of firm-level control variables and fixed effects, and the t-statistics are based on robust standard errors that are clustered at the country level.[31] However, we report only the coefficients (and t-statistics) for the main variables of interest. The results are fairly consistent across the two partitions. In countries with better prior regulation, measured with either regulatory quality or supervisory staff, the coefficients on MAD and TPD are always negative and significant. In the countries with relatively weak prior regulation, the coefficients are still

[29] Our principal source for full-time supervisory staff numbers 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.

[30] 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 these results is probably loss in power, as the index is missing for several EU countries.

[31] When we conduct the robustness checks from Section 4.2 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.

negative but not significant (with one exception). Thus, the results show that the liquidity effects for both directives are concentrated in countries with a strong track record of implementing regulation as well as stronger prior securities law enforcement. Consistent with this finding, our survey of local regulators indicates that even in countries with strong prior securities regulation (such as the U.K.) the two directives led to significant changes.[32] More generally, the result suggests that countries with stronger prior regulation are likely better able and more willing to implement the new EU directives. Thus, countries' initial conditions matter for regulatory outcomes.

To more directly examine the role of differential implementation and enforcement across countries for the liquidity results, we apply a similar partitioning approach as in (2) using directive-specific proxies for the implementation and enforcement of MAD and TPD. Specifically, we estimate the following extension of our model:

$$CapEff = \beta_0 + \beta_1\, EU\ Directive \times Implementation_{Strong} + \beta_2\, EU\ Directive \times Implementation_{Weak} + \sum \beta_j\, Controls_j + \sum \beta_i\, Fixed\ Effects_i + \varepsilon. \quad (3)$$

The two *Implementation* variables are binary indicators that split the post-MAD or TPD observations into two distinct groups, one for countries with substantial changes around the implementation of the directives and with strict enforcement, and one for the remaining EU countries. We use five directive-specific variables, two for MAD and three for TPD (see also Table 1): (i) *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

[32] 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).

supervisory powers regarding the translation of MAD into local law (CESR 2007).[33] Higher values imply more supervisory powers. (ii) *Action Taken by 2009* indicates EU countries that have taken at least a single enforcement action regarding violations of the MAD (e.g., imposed a fine).[34] (iii) *Maximum 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).[35] (iv) *Shift in Enforcement* indicates a substantial change in the enforcement activity of the local supervisory authority around the time of the TPD implementation. We construct this variable based on a survey that we sent to the supervisory authority as well as the technical department of PricewaterhouseCoopers in each EU country.[36] (v) *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.[37] For the analyses, we transform each continuous implementation proxy into binary indicators by utilizing the treatment sample median.

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[33] 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?"

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

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

[36] We code *Shift in Enforcement* as 1 if the local enforcement authority indicated that it implemented a comment and review process for the first time, and the audit firm replied that, according to their own assessment, a significant shift in the intensity of enforcement did occur during the sample period. In cases of disagreement between the two and if we could not resolve the issue either by other answers to our survey questions or by going back to the respondents, we coded *Shift in Enforcement* as zero.

[37] CESR Standard No. 1 comprises 21 principles on the enforcement of financial information. In 2009, CESR released a review report on whether or not its principles were implemented by the EU member states. As many of the principles in Standard No. 1 essentially became law with the TPD, we use this report to construct a

We report the liquidity results conditioning on the implementation and enforcement of the two directives in Table 5. The tenor of the results is very similar across the five split variables. The MAD is associated with a significant improvement in liquidity in countries that confer relatively strong supervisory powers to the local regulator and that subsequently enforce the insider trading rules.[38] Similarly, the TPD is associated with a liquidity improvement in countries that set relatively high monetary fines for violations, when regulators and auditor indicate that there were substantial changes in the enforcement regime and in countries that fully comply with CESR's enforcement principles. In each partition, the coefficient on MAD or TPD is significant only for the sub-set of EU countries with strong implementation. Moreover, the liquidity effects differ across strong and weak implementation countries at the 10 percent level or better in all cases. Taken together, the findings in Table 5 suggest that prior regulation as well as ex-post implementation and enforcement play a role for the capital-market effects of the directives.

Next, we combine the binary *Regulatory Quality 2003* indicator variable (high vs. low quality) with each of the five implementation variables (strong vs. weak implementation). 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 (2) and (3). 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.

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variable that measures the extent to which a country enforces financial information as set out in Standard No. 1 and the TPD (see CESR, 2009b).

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

Table 6 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 that combine a strong track record for past regulation with strict implementation and enforcement of the new directives had 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 11 percent level or better. Moreover, the rank order of the coefficient magnitudes going from the *High RQ/Strong IS* to the *Low RQ/Weak IS* bin is monotonically decreasing (except for one case), underscoring the importance of prior regulation. Strong implementation also improves liquidity but (with one exception) matters only in countries that have strong prior regulation.

Our liquidity results and the five directive-specific measures indicate that there is considerable variation in the implementation and enforcement across EU countries. Not all EU countries endow the supervisory authority with the same powers, some countries set relatively low penalties and not all countries have taken enforcement actions against violations. Another possibility is that countries fully adopt the directives but do not commit the necessary resources to enforce the new regulation (see also Enriques and Gatti, 2008). This channel is perhaps more subtle (or less visible). To explore this channel, we measure changes in supervisory resources using *Supervisory Staff Growth*, defined as the percentage change in full-time employees working for the local securities regulator from 2004 to 2008.[39] As before, we create a binary indicator by splitting the staff growth in the EU countries by the median (high vs. low growth)

[39] 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.

and then interact this variable with the respective EU directive to separately estimate the liquidity consequences of MAD or TPD for the two partitions.

Table 7 reports the coefficient estimates and t-statistics for the main variables of interest of this analysis. The coefficients on MAD and TPD are negative and significant for the high growth partition. In countries with moderate or no changes in supervisory staff, the magnitude of the liquidity effect is smaller, though the difference is statistically significant only in the case of the TPD. Next, we combine the changes in supervisory resources with the level of *Supervisory Staff* in 2003 (high vs. low level) to again form four bins. The MAD and TPD coefficients are negative and highly significant for the *High Level/High Growth* group. The liquidity effects in the other bins are smaller and often insignificant, underscoring the important role of the prior conditions.[40] Thus, similar to Table 6, the countries that exhibit the largest liquidity improvements are those that already had relatively well-staffed supervisors but further increased the staff resources with the adoption of the new directives.

Overall, the results presented in Tables 6 and 7 provide little evidence supporting the catching-up hypothesis. In contrast, there appears to be considerable hysteresis in regulatory outcomes, either because of significant complementarities between existing and new regulation or because the same forces that have limited the effectiveness of securities regulation in the past are still at play when new rules are introduced.

[40] The coefficients for MAD across the four bins have the expected rank order but are generally not statistically different from each other. The differences are significant for the TPD. This latter finding makes sense considering that supervisory staff increases are often related to the reviews of financial statements, which many countries introduced following the TPD.

5. 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 can be viewed as tightening and harmonizing existing EU securities regulation, particularly its enforcement. We use this setting to highlight that implementation and enforcement of regulation play an important role for regulatory outcomes. Our empirical identification strategy exploits the staggered implementation of the two directives across EU countries. This feature allows us to control for general market movements and other potentially confounding events that occurred within and outside the EU over the sample period.

Overall, the results show that stronger securities regulation can have significant economic benefits. Specifically, we find that market liquidity increases and firms' cost of capital decreases as the two EU directives become effective in the member states. The results are economically significant for both proxies but on balance stronger for market liquidity than for the cost of capital. The latter is perhaps not surprising considering that the cost of capital is a more anticipatory measure, for which it is harder to pinpoint changes.

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, it is important to recognize that the same forces that limited the strength of past regulation are likely still at work when new rules are introduced. In addition, there may be complementarities between existing and new regulation, especially considering that the two new directives primarily improve the enforcement regime of existing market abuse and transparency regulation.

In sum, our findings support the notion that the success of regulation depends critically on how regulation is implemented and enforced and not just on how it is designed. Thus, policy debates should pay particular attention to implementation and enforcement issues. Our finding that the two EU directives did not make member states with weaker securities regulation "catch up" with EU peers with stronger securities regulation raises concerns about the likely success of regulatory harmonization in the EU and around the world. More generally, the existence of hysteresis has important implications for the expected outcomes of regulatory reforms.

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. For the same reason, our results 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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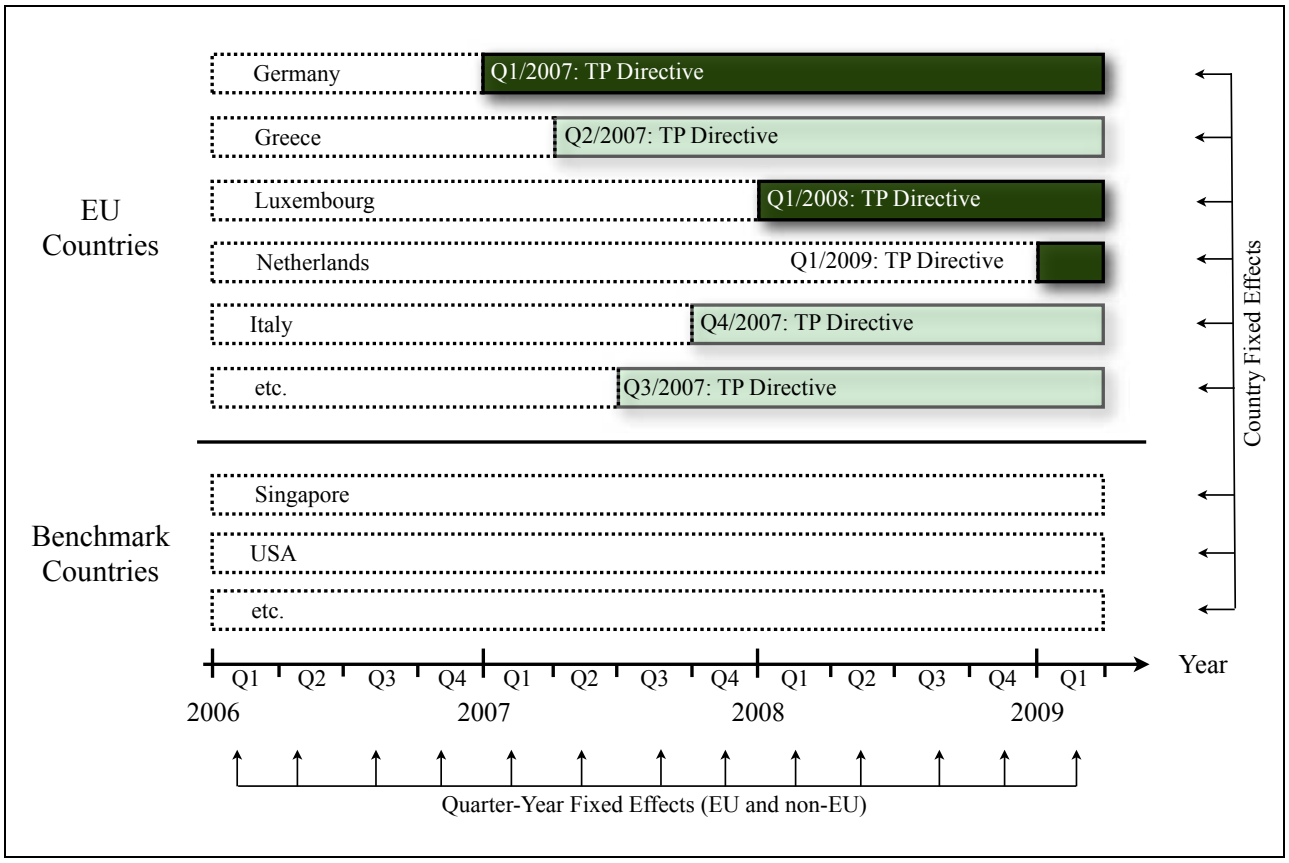
Figure 1: Illustrating the Identification Strategy for the Capital-Market Effects of the Transparency Directive



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, Entry-into-Force Dates of MAD and TPD, Prior Regulation and Implementation Variables by EU Country

				Prior Regulation				*MAD Implementation*			*TPD Implementation*				*Δ Resources*	
Country	*Bid-Ask Spreads (N)*	*MAD Entry-into-Force Dates*	*TPD Entry-into-Force Dates*	*Regulatory Quality 2003*		*Supervisory Staff 2003*		*Supervisory Powers*		*Action Taken by 2009*	*Maxium Fine (EUR 000)*		*Shift in Enforcement*	*Compliance CESR Std. 1*	*Supervisory Staff Growth*	
Austria	1,469	Jan-05	Apr-07	1.52	(1)	0.23	(0)	70	(0)	1	30	(0)	0	0	1.32	(1)
Belgium	3,787	Sep-05	Sep-08	1.36	(1)	0.24	(0)	69	(0)	0	2,500	(1)	0	1	-0.27	(0)
Bulgaria	310	Jan-07	Jan-07	0.59	(0)	0.16	(0)	69	(0)	0	5,112	(1)	0	0	0.13	(0)
Cyprus	1,336	Sep-05	Mar-08	1.20	(0)	0.22	(0)	68	(0)	0	341	(0)	0	0	0.07	(0)
Czech Republic	155	Feb-06	Aug-09	1.12	(0)	0.49	(1)	64	(0)	0	400	(0)	0	0	0.89	(1)
Denmark	5,016	Apr-05	Jun-07	1.79	(1)	0.19	(0)	60	(0)	0	No limit	(1)	1	1	0.10	(0)
Estonia	232	Mar-05	Dec-07	1.40	(1)	1.86	(1)	60	(0)	1	16,000	(1)	1	0	0.10	(0)
Finland	4,081	Jul-05	Feb-07	1.90	(1)	0.33	(0)	63	(0)	0	200	(0)	1	1	-0.02	(0)
France	19,903	Jul-05	Dec-07	1.18	(0)	0.35	(1)	75	(1)	1	10,000	(1)	1	1	0.04	(0)
Germany	18,038	Oct-04	Jan-07	1.51	(1)	0.38	(1)	64	(0)	1	200	(0)	1	0	0.18	(1)
Greece	n.a.	Jul-05	Jul-07	1.01	(0)	0.39	(1)	60	(0)	0	1,000	(1)	0	0	0.13	(0)
Hungary	861	Jul-05	Dec-07	1.08	(0)	2.69	(1)	73	(1)	0	24	(0)	1	0	-0.18	(0)
Iceland	81	Jul-05	Nov-07	1.67	(1)	0.25	(0)	60	(0)	1	300	(0)	0	0	0.94	(1)
Ireland	1,272	Jul-05	Jun-07	1.66	(1)	1.35	(1)	73	(1)	0	2,500	(1)	1	1	0.27	(1)
Italy	7,980	May-05	Aug-09	1.02	(0)	1.51	(1)	70	(0)	1	620	(1)	0	1	0.38	(1)
Latvia	378	Jul-05	Apr-07	1.03	(0)	0.25	(0)	80	(1)	1	14	(0)	1	1	0.16	(1)
Lithuania	607	Apr-04	Feb-07	1.10	(0)	0.92	(1)	70	(0)	0	29	(0)	1	0	0.11	(0)
Luxembourg	194	May-06	Jan-08	1.94	(1)	2.40	(1)	80	(1)	0	125	(0)	0	0	0.44	(1)
Malta	n.a.	Apr-05	Oct-07	1.27	(0)	1.54	(1)	75	(1)	1	466	(0)	0	0	0.09	(0)
Netherlands	3,823	Oct-05	Jan-09	1.76	(1)	1.14	(1)	67	(0)	1	120	(0)	1	0	0.18	(1)
Norway	5,040	Sep-05	Jan-08	1.39	(1)	0.34	(0)	59	(0)	1	No limit	(1)	0	1	0.26	(1)
Poland	5,494	Oct-05	Mar-09	0.61	(0)	0.78	(1)	70	(0)	0	1,389	(1)	0	1	1.41	(1)
Portugal	1,484	Apr-06	Nov-07	1.21	(0)	2.76	(1)	73	(1)	0	2,500	(1)	0	1	0.03	(0)
Romania	442	Jan-07	Jan-07	-0.12	(0)	0.04	(0)	73	(1)	1	13	(0)	0	0	0.14	(0)
Slovakia	79	Jan-05	May-07	0.95	(0)	0.02	(0)	74	(1)	0	664	(1)	0	0	-0.06	(0)
Slovenia	422	Aug-04	Sep-07	0.88	(0)	0.26	(0)	51	(0)	0	125	(0)	0	0	0.18	(1)
Spain	3,620	Nov-05	Dec-07	1.29	(1)	0.10	(0)	60	(0)	0	600	(0)	0	1	0.16	(1)
Sweden	10,160	Jul-05	Jul-07	1.69	(1)	0.29	(0)	73	(1)	1	1,000	(1)	1	0	0.17	(1)
United Kingdom	42,528	Jul-05	Jan-07	1.68	(1)	0.43	(1)	76	(1)	1	No limit	(1)	1	1	0.15	(1)

(continued)

Table 1 (continued)

The treatment sample consists of all countries in the European Union (EU). We also include Iceland and Norway from the European Economic Area (EEA), as they agreed to adopt the EU capital market directives (for brevity, we refer to the EU countries as the treatment sample). For each EU country, the table presents the number of firm-quarter observations used in our main analysis (i.e., the bid-ask spread regressions in Table 3), the dates when the Market Abuse Directive (*MAD)* and the Transparency Directive (*TPD*) came into force, as well as several proxies for prior regulation, implementation strength of the two directives, and changes in supervisory resources. We measure prior regulation with the following two variables: *Regulatory Quality* is an index capturing the ability of the government to formulate and implement sound policies and regulations, taken from Kaufman et al. (2009) and measured in the year 2003. Higher values indicate higher regulatory quality. *Supervisory Staff* equals the number of full-time employees working for the local regulatory authority supervising securities laws and scaled by the number of public firms in a given country, both measured in the year 2003. We collect staff numbers from the annual reports of the local regulators and the survey in Central Banking Publications (2009). If the sources only provide data for a joint regulator (i.e., including the banking and insurance sectors), we use the relative weight of the three sectors to allocate staff to securities regulation. In case of missing staff data, we interpolate staff numbers. The number of public firms per country is from the World Bank. We use two variables to measure the strength of *MAD Implementation:* (i) *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 MAD into local law (CESR 2007). Higher values indicate more supervisory powers. (ii) *Action Taken by 2009* equals 1 if the local regulator has taken at least a single enforcement action under MAD by the end of 2009. We construct this variable based on a CESR Review Panel report on the implementation of MAD (CESR 2010). We use the following three variables to measure the strength of *TPD Implementation:* (i) *Maximum Fine* is the maximum fine (in EUR thousands) that can be imposed on security issuers for violations of Articles 4 to 6 of TPD (CESR 2009a). (ii) *Shift in Enforcement* equals 1 if local auditors and regulators indicate that the enforcement activity 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 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 the year 2008 (CESR 2009b). We measure changes (Δ) in supervisory resources as the percentage change in full-time employees working for the local securities regulator over the 2004 to 2008 period (*Supervisory Staff Growth*). 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. For our analyses, we transform the continuous variables into binary variables (indicated in parentheses) splitting the treatment sample by the median.

Table 2: Descriptive Statistics for Variables Used in the Liquidity and Cost of Capital Regressions

Panel A: Liquidity Regressions

	N	*Mean*	*Std. Dev.*	*P1*	*P25*	*Median*	*P75*	*P99*
European Union Countries (Treatment Sample):								
Bid-Ask Spread$_t$	138,792	0.044	0.056	0.001	0.009	0.023	0.055	0.286
Zero Returns$_t$	161,387	0.343	0.279	0.000	0.108	0.246	0.563	0.954
Market Value$_{t-4}$	138,792	788	3,357	2	22	82	367	13,103
Share Turnover$_{t-4}$	138,792	0.001	0.002	0.000	0.000	0.001	0.001	0.010
Return Variability$_{t-4}$	138,792	0.024	0.012	0.006	0.015	0.021	0.030	0.060
Non-European Union Countries (Benchmark Sample):								
Bid-Ask Spread$_t$	516,796	0.027	0.046	0.001	0.003	0.009	0.027	0.244
Zero Returns$_t$	674,048	0.255	0.239	0.000	0.077	0.156	0.369	0.937
Market Value$_{t-4}$	516,796	1,159	8,061	3	33	116	438	17,676
Share Turnover$_{t-4}$	516,796	0.003	0.005	0.000	0.000	0.001	0.004	0.023
Return Variability$_{t-4}$	516,796	0.028	0.013	0.007	0.018	0.025	0.035	0.066

Panel B: Cost of Capital Regressions

	N	*Mean*	*Std. Dev.*	*P1*	*P25*	*Median*	*P75*	*P99*
European Union Countries (Treatment Sample):								
Cost of Capital$_t$ (r_{GLS})	64,385	8.724	4.082	2.154	5.603	8.013	11.254	19.561
Dividend Yield$_t$	67,682	3.247	1.795	0.606	1.850	2.931	4.323	8.140
Inflation$_t$	64,385	2.432	1.136	0.280	1.619	2.259	3.097	5.000
GDP per Capita	64,385	24,266	6,862	5,224	22,271	25,094	27,903	41,901
Total Assets$_{FYE-1}$	64,385	1,831	6,298	10	88	260	941	30,735
Financial Leverage$_{FYE-1}$	64,385	0.553	0.202	0.056	0.421	0.568	0.694	0.948
Return Variability	64,385	0.021	0.008	0.008	0.015	0.019	0.026	0.046
Asset Growth$_{FYE-1}$	67,682	0.180	0.453	-0.311	0.003	0.084	0.211	2.077
Non-European Union Countries (Benchmark Sample):								
Cost of Capital$_t$ (r_{GLS})	274,876	8.000	4.220	1.681	4.729	7.200	10.463	19.462
Dividend Yield$_t$	262,006	2.783	1.829	0.559	1.375	2.281	3.704	8.125
Inflation$_t$	274,876	1.927	2.141	-1.458	0.091	1.879	3.036	8.736
GDP per Capita	274,876	27,072	14,300	546	14,650	35,304	37,888	40,718
Total Assets$_{FYE-1}$	274,876	1,805	5,910	12	110	294	936	28,851
Financial Leverage$_{FYE-1}$	274,876	0.515	0.237	0.045	0.334	0.510	0.681	0.950
Return Variability	274,876	0.024	0.009	0.009	0.017	0.022	0.029	0.052
Asset Growth$_{FYE-1}$	262,006	0.138	0.354	-0.243	-0.002	0.063	0.169	1.495

(continued)

Table 2 (continued)

The table presents descriptive statistics for the dependent variables and the continuous independent variables used in the liquidity analyses (Panel A) and the cost of capital analyses (Panel B). We present the statistics separately for the treatment (EU) and the benchmark (non-EU) samples, which comprise up to 29 and 35 countries, respectively. The numbers in Panel A (Panel B) are generally based on the bid-ask spread (cost of capital) sample. 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 turnover (i.e., daily 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 (in a given calendar year for the cost of capital regressions). *Cost of Capital (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 only exclude zero-dividend observations in computing this variable. *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). If indicated, we lag market-based data 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 99^{th} (95^{th}) percentile (r_{GLS} and *Dividend Yield*). All means (medians) are significantly different at the 1%-level across EU and Non-EU countries using t-tests (Wilcoxon rank sum tests).

Table 3: Capital Market Effects of Tightening EU Securities Regulation

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

	Market Abuse Directive	*Transparency Directive*	*Both Directives Combined*
Test Variables:			
MAD	-0.147**	–	-0.160***
	[-2.59]		[-2.73]
TPD	–	-0.229*	-0.232*
		[-1.89]	[-1.94]
Control Variables:			
Ln(Market Value$_{t-4}$)	-0.391***	-0.392***	-0.392***
	[-30.15]	[-30.21]	[-30.21]
Ln(Share Turnover$_{t-4}$)	-0.295***	-0.295***	-0.295***
	[-9.78]	[-9.80]	[-9.80]
Ln(Return Variability$_{t-4}$)	0.395***	0.395***	0.395***
	[7.93]	[7.92]	[7.92]
Fixed Effects:			
Country	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Quarter-Year	Yes	Yes	Yes
Quarter-Year (EU specific)	Yes	Yes	Yes
R-squared	0.777	0.777	0.777
N Treatment/Benchmark Countries	27/27	27/27	27/27
N Firm-Quarter Observations	655,588	655,588	655,588

(continued)

Table 3 (continued)

Panel B: Alternative Dependent Variables

	Bid-Ask Spread$_{P0}$	*Zero Returns*	*Cost of Capital (r_{GLS})*	*Dividend Yield*
Test Variables:				
MAD	-0.237**	-0.051***	-0.577**	-0.094
	[-2.12]	[-5.44]	[-2.14]	[-1.29]
TPD	-0.332**	-0.017**	-0.203*	-0.072*
	[-2.48]	[-2.31]	[-1.70]	[-1.69]
Control Variables:				
Ln(Market Value$_{t-4}$)	-0.208***	-0.066***	–	–
	[-7.21]	[-13.18]		
Ln(Share Turnover$_{t-4}$)	-0.318***	-0.049***	–	–
	[-10.30]	[-18.22]		
Ln(Return Variability$_{t-4}$)	-0.217**	-0.046***	–	–
	[-2.38]	[-4.57]		
Inflation	–	–	0.466***	0.003
			[9.49]	[0.23]
Ln(GDP per Capita)	–	–	-3.860*	-0.621
			[-1.84]	[-0.43]
Ln(Total Assets$_{FYE-1}$)	–	–	-0.792***	-0.118***
			[-11.90]	[-4.14]
Financial Leverage$_{FYE-1}$	–	–	2.815***	-0.008
			[8.36]	[-0.04]
Return Variability	–	–	-19.350	-27.38***
			[-1.28]	[-7.46]
Asset Growth$_{FYE-1}$	–	–	–	-0.309***
				[-5.57]
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
R-squared	0.444	0.559	0.344	0.248
N Treatment/Benchmark Countries	27/27	29/35	22/29	22/29
N Firm-Quarter Observations	649,797	835,435	339,261	329,688

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. In Panel A, we use the *Bid-Ask Spread* measured as the quarterly median quoted spread as the dependent variable. In Panel B, we use the following alternative dependent variables: (1) the median quoted spread scaled by the earliest available price for each firm (*Bid-Ask Spread$_{P0}$*). (2) *Zero Returns* is the proportion of trading days with zero daily stock returns per quarter. (3) *Cost of Capital (r_{GLS})* is the quarterly implied cost of capital estimate following Hou et al. (2009). (4) *Dividend Yield* equals actual dividends divided by the stock price at the end of the quarter. *MAD* and *TPD* are binary indicator variables that take on the value of one beginning in the quarter when the Market Abuse Directive or the Transparency Directive came into force (see Table 1). For a description of the remaining control variables see Table 2. We use the natural log of the raw values and lag the variables by four quarters where indicated. We include country-, Campbell (1996) industry-, and quarter-year-fixed effects (for the entire sample and separately for EU countries) 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 4: Sensitivity Analyses for the Liquidity Effects of Tightening EU Securities Regulation

Panel A: Analysis of Non-Overlapping Time Periods around the Entry-into-Force Dates of MAD and TPD

Ln(Bid-Ask Spread) as Dependent Variable (N=655,588)	*Market Abuse Directive*		*Transparency Directive*	
	Coefficient	*t-stat*	*Coefficient*	*t-stat*
Period Relative to Entry-into-Force Date:				
Period t-4 to t-1	-0.098	[-1.24]	-0.033	[-0.86]
Period t to t+3	-0.269*	[-1.88]	-0.189*	[-2.23]
Period t≥+4	-0.360*	[-1.66]	-0.566*	[-3.22]
F-test for Differences across Coefficients (p-value):				
Period t-4 to t-1 = Period t to t+3	0.016		0.003	
Control for Other Directive	Yes		Yes	
Control Variables	Yes		Yes	
Fixed Effects:				
Country	Yes		Yes	
Industry	Yes		Yes	
Quarter-Year	Yes		Yes	
Quarter-Year (EU specific)	Yes		Yes	

Panel B: Analysis of Varying Entry-into-Force Dates of MAD and TPD

Ln(Bid-Ask Spread) as Dependent Variable (N=655,588)	*Market Abuse Directive*		*Transparency Directive*	
	Coefficient	*t-stat*	*Coefficient*	*t-stat*
Shifting of Entry-into-Force Dates Relative to t=0:				
t–6	-0.057	[-1.21]	0.001	[0.01]
t–5	-0.045	[-0.59]	-0.033	[-0.29]
t–4	-0.079	[-1.19]	-0.068	[-0.66]
t–3	-0.097*	[-1.71]	-0.118	[-1.22]
t–2	-0.120**	[-2.26]	-0.149	[-1.42]
t–1	-0.165***	[-3.32]	-0.174	[-1.49]
t = 0 ('True' Entry-into-Force Dates)	-0.160***	[-2.73]	-0.232*	[-1.94]
t+1	-0.152**	[-2.16]	-0.299**	[-2.37]
t+2	-0.134	[-1.64]	-0.371***	[-2.79]
t+3	-0.119	[-1.47]	-0.401***	[-2.86]
t+4	-0.074	[-1.15]	-0.410***	[-2.79]
Control for Other Directive	Yes		Yes	
Control Variables	Yes		Yes	
Fixed Effects:				
Country	Yes		Yes	
Industry	Yes		Yes	
Quarter-Year	Yes		Yes	
Quarter-Year (EU specific)	Yes		Yes	

(continued)

Table 4 (continued)

Panel C: Alternative Model Specifications

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	655,588	-0.160***	[-2.99]	-0.232*	[-1.70]
- Clustering by Economic Region	655,588	-0.160**	[-2.81]	-0.232*	[-2.00]
(2) Alternative Fixed Effects Structures:					
- Firm-Fixed Effects	655,588	-0.134***	[-2.98]	-0.190	[-1.55]
- Add Separate Quarter-Year-Fixed Effects for Developed Markets	655,588	-0.123*	[-1.81]	-0.250**	[-2.05]
- Add Separate Quarter-Year-Fixed Effects Interacted with Firm Size	653,511	-0.150**	[-2.25]	-0.273**	[-2.01]
(3) Alternative Sample Specifications:					
- European Union only	138,792	-0.158**	[-2.73]	-0.230*	[-1.91]
- Random Benchmark Sample	203,397	-0.156***	[-2.76]	-0.225*	[-1.90]
(4) Controlling for Macroeconomic Factors:					
- Inflation, GDP per Capita, Δ GDP per Capita	642,791	-0.163***	[-2.80]	-0.235*	[-1.91]
(5) Controlling for Other Lamfalussy Directives in the EU:		*MAD*	*TPD*	*MiFID*	*Prospectus*
- Markets in Financial Instruments Directive (MiFID)	655,588	–	–	-0.020	–
				[-0.48]	
- Prospectus Directive	655,588	–	–	–	0.092
					[1.23]
- All Four Lamfalussy Directives Combined	655,588	-0.174***	-0.243*	0.051	0.081*
		[-2.90]	[-1.86]	[0.61]	[1.69]

(continued)

Table 4 (continued)

The sample comprises firm-quarter observations from 27 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 one beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force (see Table 1). For a description of the remaining variables see Table 2. In Panel A, we estimate the liquidity pattern around the directives by replacing *MAD* and *TPD* with the following three binary indicator variables: (1) *Period t-4 to t-1* represents the four quarters immediately leading up to the entry-into-force of MAD and TPD, (2) *Period t to t+3* marks the initial four quarters under the two directives, and (3) *Period t*≥*+4* stands for the time period after the initial year under MAD and TPD. In Panel B, we report the *MAD* and *TPD* coefficients from 11 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-6 to t+4 relative to the 'true' entry-into-force dates. In Panel C, 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), or (ii) limit the benchmark sample to 150 randomly selected firms from each country. Fourth, we control for macroeconomic factors by adding inflation, annual GDP per capita, and the percentage change (Δ) in annual GDP per capita as control variables to the model (see Table 2 for variable details). We lag the macroeconomic controls by one year. Fifth, we assess the influence of the remaining Lamfalussy Directives on our 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 (*Prospectus*), and (iii) all four of the Lamfalussy Directives (i.e., *MAD*, *TPD*, *MiFID*, and *Prospectus*). Unless indicated otherwise, we include the full set of control variables and fixed effects in the model (see Table 3, Panel A), but only report OLS coefficient estimates (t-statistics) for the main variables in the table. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

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

Ln(Bid-Ask Spread) as Dependent Variable (N=655,588)	*Market Abuse Directive*				*Transparency Directive*				
	Prior Regulation		*MAD Implementation*		*Prior Regulation*		*TPD Implementation*		
	Regulatory Quality 2003	*Supervisory Staff 2003*	*Super-visory Powers*	*Action Taken by 2009*	*Regulatory Quality 2003*	*Supervisory Staff 2003*	*Maximum Fine*	*Shift in Enforcement*	*Compliance CESR Std. 1*
Prior Regulation Quality:									
High	-0.203***	-0.160***	–	–	-0.264**	-0.286**	–	–	–
	[-3.45]	[-2.56]			[-2.09]	[-2.17]			
Low	-0.021	-0.109*	–	–	-0.075	-0.070	–	–	–
	[-0.35]	[-1.86]			[-0.52]	[-0.52]			
Implementation Strength:									
Strong	–	–	-0.226***	-0.190***	–	–	-0.276**	-0.267**	-0.285**
			[-3.05]	[-3.33]			[-2.20]	[-2.10]	[-2.28]
Weak	–	–	-0.093	-0.020	–	–	-0.104	-0.004	-0.097
			[-1.66]	[-0.31]			[-0.85]	[-0.03]	[-0.78]
F-test for Differences across Coefficients (p-value):									
High/Strong = Low/Weak	0.021	0.481	0.058	0.040	0.142	0.093	0.105	0.034	0.092
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The sample comprises firm-quarter observations from 27 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 one beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force (see Table 1). 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) or the strength of MAD/TPD implementation (strong vs. weak). We measure *Prior Regulation* with the following two variables: *Regulatory Quality* is an index capturing the ability of the government to formulate and implement sound policies and regulations (Kaufman et al., 2009). *Supervisory Staff* equals the ratio of full-time employees working for the local securities regulator to the number of public firms in a given country. Both variables are measured in the year 2003. We use two variables to measure the strength of *MAD Implementation:* (i) *Supervisory Powers* equals the number of positive replies by the local regulator to a questionnaire on the existence of specific supervisory powers regarding the translation of MAD into local law. (ii) *Action Taken by 2009* equals 1 if the local regulator has taken at least a single enforcement action under MAD by the end of 2009. We use the following three variables to measure the strength of *TPD Implementation:* (i) *Maximum Fine* is the maximum fine that can be imposed on security issuers for violations of Articles 4 to 6 of TPD. (ii) *Shift in Enforcement* equals 1 if local auditors and regulators indicate that the enforcement activity has substantially increased over the sample period. (iii) *Compliance CESR Std. 1* takes on the value of 1 if a country complies with all the enforcement principles outlined in CESR Standard No. 1. For our analyses, we transform the continuous partitioning variables into binary indicators splitting the treatment sample by the median (see Table 1 for more details on the partitioning variables). We include the full set of control variables and fixed effects in the model (see Table 3, Panel A), but only report OLS coefficient estimates (t-statistics) for the main variables in the table. We also report p-values from Wald tests assessing the statistical significance of the difference in coefficients across groups where indicated. ***, **, 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 (N=655,588)	*Market Abuse Directive*		*Transparency Directive*		
	MAD Implementation Strength (IS)		*TPD Implementation Strength (IS)*		
	Supervisory Powers	*Action Taken by 2009*	*Maximum Fine*	*Shift in Enforcement*	*Compliance CESR Std. 1*
Regulatory Quality (RQ):					
High RQ / Strong IS	-0.279***	-0.241***	-0.327**	-0.299**	-0.341***
	[-4.74]	[-4.44]	[-2.62]	[-2.32]	[-2.81]
High RQ / Weak IS	-0.163***	-0.076	-0.113	0.005	-0.109
	[-3.29]	[-1.20]	[-0.93]	[0.03]	[-0.89]
Low RQ / Strong IS	-0.129***	-0.062	-0.082	-0.094	-0.091
	[-2.72]	[-1.08]	[-0.56]	[-0.68]	[-0.62]
Low RQ / Weak IS	0.088	0.083	-0.030	0.003	0.024
	[1.59]	[1.13]	[-0.18]	[0.02]	[0.14]
F-test for Differences across Coefficients (p-value):					
High RQ / Strong IS = High RQ / Weak IS	0.003	0.009	0.033	0.020	0.025
High RQ / Strong IS = Low RQ / Strong IS	0.000	0.010	0.057	0.110	0.049
Low RQ / Strong IS = Low RQ / Weak IS	0.000	0.118	0.564	0.259	0.224
Control Variables	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes

The sample comprises firm-quarter observations from 27 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 one beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force (see Table 1). 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/TPD implementation (strong vs. weak). We measure prior regulation with *Regulatory Quality*, an index capturing the ability of the government to formulate and implement sound policies and regulations as measured in 2003 (Kaufman et al., 2009). We use two variables to measure the strength of *MAD Implementation:* (i) *Supervisory Powers* equals the number of positive replies by the local regulator to a questionnaire on the existence of specific supervisory powers regarding the translation of MAD into local law. (ii) *Action Taken by 2009* equals 1 if the local regulator has taken at least a single enforcement action under MAD by the end of 2009. We use the following three variables to measure the strength of *TPD Implementation:* (i) *Maximum Fine* is the maximum fine that can be imposed on security issuers for violations of Articles 4 to 6 of TPD. (ii) *Shift in Enforcement* equals 1 if local auditors and regulators indicate that the enforcement activity has substantially increased over the sample period. (iii) *Compliance CESR Std. 1* takes on the value of 1 if a country complies with all the enforcement principles outlined in CESR Standard No. 1. For our analyses, we transform the continuous partitioning variables into binary indicators splitting the treatment sample by the median (see Table 1). We include the full set of control variables and fixed effects in the model (see Table 3, Panel A), but only report OLS coefficient estimates (t-statistics) for the main variables in the table. We also report p-values from Wald tests assessing the statistical significance of the difference in coefficients across groups where indicated. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 7: Effects of Prior Supervisory Resources and Changes in Supervisory Resources on the Liquidity Benefits of Tighter Securities Regulation

Ln(Bid-Ask Spread) as Dependent Variable (N=655,588)	*Market Abuse Directive*		*Transparency Directive*	
	Changes in Resources	*Prior Level of & Changes in Resources*	*Changes in Resources*	*Prior Level of & Changes in Resources*
Supervisory Staff Growth:				
High Growth	-0.164***	–	-0.282**	–
	[-2.73]		[-2.21]	
Low Growth	-0.098*	–	-0.067	–
	[-1.77]		[-0.49]	
Supervisory Staff Level & Supervisory Staff Growth:				
High Level / High Growth	–	-0.175***	–	-0.339**
		[-2.75]		[-2.56]
High Level / Low Growth	–	-0.111*	–	-0.069
		[-1.67]		[-0.49]
Low Level / High Growth	–	-0.094*	–	-0.067
		[-1.70]		[-0.48]
Low Level / Low Growth	–	-0.098	–	-0.052
		[-1.37]		[-0.36]
F-test for Differences across Coefficients (p-value):				
High Growth = Low Growth	0.289	–	0.086	–
High Level / High Growth = High Level / Low Growth	–	0.247	–	0.045
High Level / High Growth = Low Level / High Growth	–	0.454	–	0.048
Low Level / High Growth = Low Level / Low Growth	–	0.840	–	0.830
Control Variables	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

The sample comprises firm-quarter observations from 27 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 one beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force (see Table 1). We first 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 *Supervisory Staff Growth* over the 2004 to 2008 period (high growth vs. low growth). In a second model, we partition the treatment sample into four distinct groups by interacting the *MAD* and *TPD* variables with the *Supervisory Staff Growth* indicator and a binary indicator for the level of *Supervisory Staff* in the year 2003 (high level vs. low level). See Table 1 for variable details. We include the full set of control variables and fixed effects in the model (see Table 3, Panel A), but only report OLS coefficient estimates (t-statistics) for the main variables in the table. We also report p-values from Wald tests assessing the statistical significance of the difference in coefficients across groups where indicated. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).