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

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Capital-Market Effects of Securities Regulation: Hysteresis, Implementation, and Enforcement
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ABSTRACT

This paper examines capital market effects of changes in securities regulation. We analyze two key directives in the European Union (EU) that tightened market abuse and transparency regulation and its enforcement. All EU member states were required to adopt these two directives, but did so at different points in time. Our research design exploits this differential timing of the same regulatory change to identify the capital-market effects. We also use cross-sectional variation in the strictness of implementation and enforcement as well as in prior regulation to analyze the role of these factors for regulatory outcomes. We find that, on average, market liquidity increases as EU countries tighten market abuse and transparency regulation. The effects are larger in countries that implement and enforce the directives more strictly. They are also stronger in countries with traditionally stricter securities regulation and with a better track record of implementing regulation and government policies in general. The results indicate that the same forces that have limited the effectiveness of regulation in the past are still at play when new rules are introduced, leading to hysteresis in regulatory outcomes. The findings also illustrate that imposing the same regulation in countries with different initial conditions can make countries diverge more, rather than move them closer together, which has important implications for global regulatory reform.

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1. Introduction

Extensive securities regulation is widespread around the world. Yet, as with regulation in general, the academic debate on the costs and benefits of securities regulation is controversial and the evidence is fairly mixed.[1] Whether or not securities regulation is beneficial to the economy appears to be largely an empirical matter. Regulatory effects likely depend also on how regulation is implemented and enforced (e.g., Djankov et al. 2003a). Moreover, the state of prior regulation and a country's regulatory quality in general could play an important role for the effects of regulatory changes. Prior studies typically focus on regulatory changes in a single country (e.g., the Sarbanes-Oxley Act in the U.S.; henceforth SOX), for which it is difficult to separate these factors: prior regulation, new rules, implementation, and enforcement are essentially a bundle, and regulatory outcomes reflect the entire bundle.

In this paper, we exploit recent changes in EU securities regulation and examine their capital-market effects. The EU setting has several desirable features. First, it allows us to analyze the same regulatory change across EU member states at different points in time. The staggered implementation offers better identification of the regulatory effects than a single regulatory event such as SOX. Second, as EU directives apply to all member states, the regulatory act is held constant across countries, but the transposition of a directive into national law, the design of supervision, the penalties for violations and the actual supervision are left to the EU member states. This variation across countries allows us to analyze implementation and enforcement separately from the rule change. Third, while the new directives introduce material regulatory changes, they are largely geared towards tightening existing securities regulation, in particular, by improving supervisory and enforcement regimes in the EU (e.g., CRA 2009). This

[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).

feature reinforces the enforcement focus of our study. Fourth, the EU setting allows us to analyze the role of prior securities regulation, that is, the interaction between a country's initial conditions and new regulation. One hypothesis is that countries with weaker prior regulation benefit more from the new directives as they still have to catch up. An alternative hypothesis is that the same forces and constraints that limited the scope and effectiveness of securities regulation in the past (e.g., insufficient resources, political resistance, inefficient bureaucracies) are still at play when new rules come into force. This hypothesis implies that history matters in the sense that there is hysteresis in regulatory outcomes. It also implies that imposing the same regulation on disparate countries could make them diverge more, rather than move closer together.

We study the capital-market effects of two EU directives that concern central elements of securities regulation: the Market Abuse Directive (MAD) addresses insider trading and market manipulation and the Transparency Directive (TPD) addresses corporate reporting and disclosure. We analyze changes in stock market liquidity around the staggered implementation of these two directives. Market liquidity is a well suited outcome variable for our setting and identification strategy, as it can be measured over relatively short intervals and is less anticipatory in nature than other economic constructs that could be used to evaluate securities regulation (e.g., the cost of capital). We measure liquidity using the bid-ask spread and the percentage of trading days with zero returns. Both proxies have been used extensively in the literature. We also examine changes in the cost of capital to corroborate the liquidity analysis.

We estimate quarterly panel regressions from 2001 to 2009 for EU (treatment) and non-EU (benchmark) firms, introducing quarter-year, industry and country fixed effects. We exploit that each EU country needs to implement the directives within a certain time frame specified in the

directives. Countries' legislative processes lead to staggered implementation dates that are spread over two to three years. Hence, we can introduce separate quarter-year fixed effects for the EU countries to account for common shocks to and trends in EU capital markets. Therefore, the identification of the regulatory effects comes entirely from within-EU variation in the dates of when the two directives become effective. This design exploits that regulatory changes are exogenous at the firm level and also addresses two common concerns about regulatory studies, i.e., that the results reflect a contemporaneous economic shock unrelated to the regulation and/or that the results reflect a market response to the events giving rise to the regulatory act (e.g., a scandal) instead of the regulation itself. In our setting, such shocks or events would have to line up with the implementation dates for 27 treatment countries and two directives to influence the results. Moreover, the staggered implementation dates are generated by countries' legislative processes and constitutions, and therefore fairly inflexible.

Using this setting and design, we find that market liquidity increases when new market abuse (MAD) and transparency (TPD) regulation come into force, using both bid-ask spreads and the percentage of zero-return days. The estimated liquidity improvements are economically significant. Both proxies indicate liquidity improvements around 15 percent. The bid-ask spread reduction implies average annual savings in trading costs of about $660,000 per firm (roughly 0.2% of market capitalization). In our sensitivity analyses, we also find evidence that the cost of capital decreases when the directives come into force. In sum, our results suggest that improving key elements of securities regulation leads to substantial capital-market benefits.

We conduct extensive sensitivity analyses and show that our results, among other things, are not driven by a few large countries and are robust to the introduction of firm-fixed effects, separate quarter-year fixed effects for developed (versus developing) countries, controls for

differences in the composition of the country samples (e.g., with respect to size or industry), controls for other EU directives as well as controls for macroeconomic changes.

To further gauge our identification strategy, we conduct three analyses. First, we analyze the liquidity patterns in event time. We find that liquidity improves right around the time MAD and TPD come into force and remains at a higher level for the remainder of the sample period, yet it is not significantly elevated in the year leading up to the directives. Thus, liquidity changes closely track the introduction of the directives. Second, we counterfactually shift the 'true' implementation dates for the directives quarter-by-quarter and each time re-estimate the liquidity regressions. We find that the coefficients for the directives quickly become smaller in magnitude and significance as we move away from the true implementation dates, suggesting that the dates provide reasonably sharp identification.[2] Third, we benchmark the liquidity effects for our sample against firms trading on "unregulated" EU markets that are not subject to the two EU directives. Such within-country estimation controls for concurrent liquidity changes that are correlated with countries' entry-into-force dates, yet unrelated to the directives and its regulatory effects. We find that the liquidity effects around the two directives occur only for regulated firms and not for unregulated firms, consistent with a causal interpretation of the link between regulation and market liquidity.

Based on these tests, we conclude that the average effects of the directives are well identified. We then examine whether there is heterogeneity in the treatment effects. First, we exploit differences in prior regulation across EU countries. We document that the liquidity

[2] We also conduct analyses in which we randomly assign implementation dates during the pre-treatment period. The average placebo effect is close to zero, as expected. We use the distribution of coefficients generated by these regressions to bootstrap the significance levels and to gauge our inferences. Based on this comparison, we conclude that, if anything, our inferences using standard errors clustered by country are conservative.

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

Second, we examine the role of differences in implementation and enforcement for regulatory outcomes. We create specific measures of how well EU countries implement and enforce the new directives using data on supervisory powers, penalties, enforcement actions as well as a self-constructed survey of securities regulators and auditors in each EU member state. 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. Our results 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 strength. We document that the liquidity effects around the MAD and TPD are strongest in countries with high past regulatory quality *and* strong implementation. We do not find that liquidity increases

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

for countries with low-quality prior regulation and weak implementation. Moreover, stricter implementation of the new directives often has an incremental effect, but primarily so in countries with high past regulatory quality or in countries that dedicated substantial resources to the regulators even prior to the new directives. Thus, there is strong evidence of hysteresis. Countries with weaker securities regulation do not catch up with stronger countries. In fact, our results imply that the two EU directives had the opposite effect. They illustrate that imposing the same regulation on countries with disparate prior conditions can make countries diverge more, not less, and point to the difficulty of harmonizing markets globally through regulatory reforms.

Our paper makes several contributions to the literature. First, we show that the imposition of stronger securities regulation on firms can indeed have significant economic benefits in terms of market liquidity (and cost of capital) for a broad cross-section of firms. Prior studies often cast doubt on the existence of benefits from securities regulation, especially those examining the capital-market effects of U.S. securities regulation in the 1930s (e.g., Stigler, 1964; Benston, 1969 and 1973; Jarrell, 1981; Mahoney and Mei, 2009). Similarly, the evidence on Regulation Fair Disclosure (e.g., Heflin et al. 2003; Gintschel and Markov, 2004; Francis et al., 2006; Gomes et al., 2007) or SOX (e.g., Chhaochharia and Grinstein, 2007; Zhang, 2007; Li et al. 2008) is decidedly mixed and often emphasizes the costs rather than the benefits of securities regulation. As pointed out by Mahoney and Mei (2009), the early studies examining securities regulation in the 1930s often lack a (convincing) control group. Similar concerns apply to the studies on Regulation Fair Disclosure (Collver, 2007) and on SOX (Leuz, 2007; Hochberg et al., 2009), as they affected all SEC registrants.[4] To get around this issue, Bushee and Leuz (2005) and Greenstone et al. (2006) exploit extensions of U.S. securities regulation that apply only to

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

specific market segments of smaller firms using unaffected, larger firms as a benchmark. Iliev (2009) employs a regression discontinuity design around the SOX Section 404 compliance cutoff, which is based on size. The return-based evidence in Bushee and Leuz (2005) and Iliev (2009) suggests that regulation imposed net costs on smaller firms while the abnormal return findings in Greenstone et al. (2006) suggests the opposite. Our study, in turn, is not limited to a set of smaller firms, but examines a broad cross-section of firms. It provides evidence on a specific channel (market liquidity) through which securities regulation confers economic benefits. The liquidity relation is identified using the staggered imposition of two EU directives in 27 countries. This design not only exploits a regulatory act that is exogenous at the firm level, but also alleviates many concerns that typically arise from studying a single regulatory event, particularly about endogenous market responses and unrelated concurrent shocks.[5]

Second, we show that regulatory outcomes depend on the strength of prior regulation and on countries' ability and willingness to implement and enforce new securities regulation. These findings highlight the role of implementation and enforcement and document substantial hysteresis in regulatory outcomes. They are consistent with the enforcement theory formulated in Djankov et al. (2003a) as well as its application to securities regulation in Shleifer (2005).

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

[5] A study that uses a similar identification strategy to ours 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 a particular industry (i.e., mining).

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.[6] We show that tighter securities regulation has immediate capital-market effects (even before the first enforcement action) when countries improve their supervisory and enforcement regimes.

The remainder of the paper proceeds as follows. Section 2 develops our hypotheses and provides more details on the institutional setting. Section 3 delineates our research design and describes the data. Section 4 presents the average liquidity effects of tightening securities regulation. In Section 5, we report results of cross-sectional differences along the dimensions of prior regulation, implementation and enforcement strength. Section 6 concludes.

2. Conceptual Underpinnings, Hypotheses and Institutional Setting

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

The arguments in favor of securities regulation refer, among other things, to the existence of externalities, economy-wide cost savings, commitment problems, and insufficient private

[6] In this sense, our study is 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.

penalties (e.g., Coffee, 1984; Easterbrook and Fischel, 1984; Leuz and Wysocki, 2008, Zingales, 2009). However, these arguments often set aside problems of how to implement and enforce securities regulation.[7] In contrast, Stigler (1971), Posner (1974), Peltzman (1976), and Becker (1983) highlight the difficulties of implementing and enforcing regulation in a way that is socially beneficial.[8] They point out that regulators face serious information problems, are often incompetent or even corrupt, and can be captured in the regulatory process. These arguments, however, do not imply that regulation necessarily has negative effects. Private contracts as an alternative to regulation rely heavily on functioning courts and private litigation. But in practice, courts and private litigation can be quite imperfect as well (e.g., Easterbrook and Fischel, 1984; Johnson et al., 2002; Djankov et al., 2003b).

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

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

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

"inequality of weapons" problem.[9] Thus, securities markets could be an instance in which regulation is beneficial to the economy. Consistent with this conjecture, almost all economies have extensive securities regulation. Obviously, this observation is not sufficient to settle the matter. As discussed earlier, there are several reasons to be skeptical about the benefits of securities regulation. Consistent with these concerns, much of the empirical evidence on the effects of securities regulation is mixed and often negative (see references in Section 1).

Furthermore, much of the evidence stems from U.S. securities regulation.[10] However, as Djankov et al. (2003a) point out, the tradeoffs can differ greatly across countries. For instance, securities regulation is likely to be more effective in richer economies with better institutions, more efficient bureaucracies, and a greater ability to implement and enforce such regulation. In countries with weak institutions and inefficient bureaucracies, the risk that regulation is abused and hence harmful is larger (Shleifer, 2005; Bhattarcharya and Daouk, 2009). In addition, a country's past track record with respect to implementing regulation is likely revealing about its ability and political will to put in place and enforce regulation that induces (curbs) behavior that is deemed socially desirable (undesirable). In sum, the benefits of securities regulation ultimately depend on its implementation and enforcement, and not just its design.

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

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

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

economic benefits (e.g., improves market liquidity). We can also shed light on the aforementioned tradeoffs and the forces that make securities regulation more or less effective.

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

In addition, the setting provides cross-sectional variation in countries' prior regulation that we can exploit in the analysis. There are two competing hypotheses. One prediction is that the effects are larger in countries for which prior securities regulation has been weak and that have been lagging behind (*catching-up hypothesis*). An alternative prediction is that the capital-market effects of the new directives are larger in countries with stronger past regulation, leading to path dependence in regulatory outcomes (*hysteresis hypothesis*). This hypothesis recognizes that prior regulation likely reflects various institutional, market and political forces that determine a country's ability and willingness to implement and enforce policies that induce or curb certain behavior (e.g., insider trading). If such forces are 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) on insider trading and market manipulation, and the Transparency Directive (TPD), which addresses reporting and disclosure requirements. Both directives pertain to key elements of securities

regulation and are at the core of the EU's Financial Services Action Plan (FSAP), which was established in 1999 with the goal to improve and integrate EU financial markets. As there already was prior EU and national regulation in both areas, the two directives essentially tighten existing regulation, harmonize remaining differences across EU countries and, importantly, stipulate appropriate supervisory and enforcement regimes. The transposition of the MAD and TPD required amending national law(s) in all member states. Below we provide a brief description of the two directives (for more details, see Appendix A).

The MAD was passed by the EU legislature in January 2003 followed by several implementing directives in December 2003. Its purpose is to ensure market integrity and equal treatment of market participants in EU securities markets by defining and prohibiting insider trading and market manipulation. Among other things, it establishes transparency standards requiring people who recommend investments to disclose their relevant interests. It also requires that each member state has a supervisory authority that is responsible for monitoring insider trading and market manipulation and gives this authority the necessary supervisory and investigative powers.[11] The MAD further requires cooperation among national supervisory authorities and some, although not complete, harmonization of penalties. It replaces Directive 89/592/EEC, which required EU countries to ban insider trading. Thus, while the MAD expands market abuse regulation in some areas, it is primarily intended to tighten and harmonize the implementation and enforcement of existing EU regulation (e.g., Lamfalussy, 2000; CRA, 2009).

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

The TPD was passed by the EU legislature in December 2004 and its implementing directive was enacted in March 2007. The directive requires issuers of traded securities to ensure appropriate transparency for investors by disclosing and disseminating periodic and ongoing regulated information. Regulated information comprises, among other things, periodic financial reports and information on major holdings of voting rights. However, prior EU directives, member state laws, and exchange requirements already stipulated annual and interim financial reports as well as the disclosure of other ongoing information. As such, the TPD does not substantially expand existing disclosure requirements, but rather focuses on (better) enforcement. For instance, the TPD stipulates that, in each member state, a supervisory authority assumes responsibility for monitoring compliance with the provisions of the directive and that this authority examines firms' regulated disclosures (e.g., financial statements). Such 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. Thus, the TPD primarily clarifies and harmonizes existing disclosure regulation and improves enforcement.

3. Research Design and Data

3.1. Identification Strategy and Empirical Model

We test the hypotheses developed in Section 2 using a panel dataset with quarterly firm-level observations of stock market liquidity. We focus on market liquidity for two reasons. First, economic theory predicts that reducing insider trading or enhancing transparency reduces information asymmetries between investors and hence increases market liquidity (e.g., Glosten

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

and Milgrom, 1985; Diamond and Verrecchia, 1991; Verrecchia, 2001). Second, a stated goal of both EU directives was to increase market confidence, which is closely related to market liquidity (e.g., Lamfalussy, 2000; Enriques and Gatti, 2008; CRA, 2009).[13] The choice of quarterly data reflects a tradeoff between reliably measuring liquidity over some interval and capturing changes in liquidity in a timely fashion, i.e., when the MAD and TPD come into force.

Our empirical strategy for estimating the effects of securities regulation on market liquidity relies on three features. First, we examine EU-wide regulatory changes, which should be exogenous to firms in the member states. However, a common concern when analyzing regulatory change is that the results could reflect general time trends or market-wide changes (e.g., macroeconomic shocks) that are concurrent with but unrelated to the regulatory change. Moreover, new regulation is often put in place after major economic events such as a crisis or scandal. It is possible that markets respond to these preceding economic events rather than the new regulation itself (e.g., Ball, 1980; Mulherin, 2007). The staggered imposition of the two directives, our second design feature, alleviates these concerns. After the enactment of an EU directive, each member state must transpose the directive into national law within a limited timeframe (typically two to three years). The process depends, among other things, on a country's constitution and its legislative system, and varies across member states. As a result, new EU directives come into force at different points in time across member states (see Figure 1 and Table 1). This variation across our 27 treatment countries and two directives reduces the influence of preceding events or concurrent economic shocks and allows us to isolate the regulatory effects on market liquidity. It also helps that the regulatory act takes place at the EU

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

level, and hence is not specific to a particular country. Third, the EU gives countries only a limited amount of time to transpose the directives. Moreover, national legislative procedures that implement the directives are lengthy and inflexible.[14] The standard deviation of the time span between the EU's transposition deadline and countries' entry-into-force dates for the MAD (TPD) is about 2.5 (3) quarters and the average time span for the MAD and TPD differs only by 1.6 quarters from the average time span of the other FSAP directives.[15] Thus, it seems unlikely that the entry-into-force dates are chosen in response to local conditions (e.g., certain liquidity trends), which alleviates concerns about reverse causality. In fact, as the entry-into-force dates are the endpoint of an often lengthy legislative process, reverse causality would imply that politicians or bureaucrats are able to accurately predict future changes in liquidity and then time legislative processes and effective dates accordingly. To illustrate, the law that implemented the MAD in the U.K. was laid before the U.K. parliament in February 2005 and entered into force in July 2005. The parliament vote was preceded by a consultation period for the regulation from June to September 2004. Thus, the process from first draft to effective date exceeded one year.

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

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

The dependent variable, *Liq*, stands for the liquidity proxies. *MAD* and *TPD* are binary variables coded as '1' beginning in the quarter in which the corresponding directive comes into force in a given EU member state and '0' otherwise. $Controls_j$ denotes a set of firm-level control

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

[15] It is of course possible that the timing also reflects a country's willingness to implement the new directives. But this alone does not lead to reverse causality or pose a problem, especially given that discretion is limited. We come back to this issue in Section 5 when we examine the heterogeneity of the treatment effects.

variables. *Fixed Effects*$_i$ represents country, industry, and *separate* quarter-year fixed effects for EU and non-EU countries.[16] As the variables of interest vary only at the country level, we draw statistical inferences based on standard errors clustered by country.

Given the EU-quarter fixed effects, our identification stems from within-EU variation in the dates when the directives become effective. This specification eliminates shocks to the capital-market variables that are common to all EU member states in a given quarter.[17] Thus, for unrelated economic shocks to create spurious results, they would have to be correlated with the two directives' entry-into-force dates across EU countries. We are not aware of a specific concern along those lines. Nevertheless, we perform several sensitivity analyses to assess our identification strategy (see Section 4.2). In one of these analyses, we use within-country estimation using EU firms that are not subject to the directives because they are trading on unregulated EU markets as a benchmark. This design rules out country-specific factors or shocks that apply to all firms in an economy. It also makes it unlikely that our results reflect endogenous responses to local conditions, rather than the directives' regulatory effects.

While the average treatment effect is well identified due to staggered implementation dates, our later analyses examining the heterogeneity in the treatment effects rely solely on cross-sectional differences across countries. Such variation is subject to fairly standard correlated omitted variable concerns and, hence, we cannot draw causal inferences about the precise factors that determine the heterogeneity in the treatment effects.

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

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

3.2. Data and Construction of the Variables

Our sample period starts in the first quarter of 2001, i.e., before the EU adopted the MAD and the TPD, and hence well in advance of the first country-specific entry-into-force dates for the MAD (April 2004) and TPD (January 2007). The sample period ends in the second quarter of 2009, which is the most recent quarter for which we have the necessary data. We include all the firm-quarter observations from EU and non-EU countries for which we have the necessary data to compute the capital-market and control variables to estimate our basic regression model stated in Eq. (1). Table 1, Panel A, provides an overview of the sample composition by EU country.[18] The bid-ask spread (zero returns) sample comprises 611,969 (780,434) firm-quarter observations from 25 (27) EU countries and 27 (35) non-EU countries. We exclude firms that follow U.S. GAAP in their financial reporting and firms with a U.S. cross-listing as they are subject to insider trading and transparency rules in the U.S. In addition, we eliminate very small firms with, on average, market values below US$ 5 million as well as firms trading on EU markets not subject to the MAD and TPD (e.g., the Alternative Investment Market in London). We further require at least four valid quarterly observations per firm, and only include benchmark countries with more than 20 firms.

Panel A of Table 1 also lists the dates when the national law(s) that implemented the MAD and TPD came into force in a given country. We collect the *Entry-into-Force Dates* from publications by the European Commission for the MAD and by Linklaters LLP, an international law firm, for the TPD, and validate them with the dates on which each EU member state

[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 the EU capital market directives (such as the MAD and the TPD) in exchange for access to the EU's single market. For simplicity, we refer to them as EU countries throughout this paper. Furthermore, we exclude Bulgaria and Romania in the empirical analysis even though they are EU member states because they adopted *all* EU regulations (including the MAD and TPD) upon joining the EU in January 2007. The results are not sensitive to either of those sample choices (see also the sensitivity analyses in Appendix B).

informed the European Commission of its compliance with the directives. In case of discrepancies, we contact the national securities regulator to resolve the issue. As the table shows, the MAD dates vary from April 2004 (Lithuania) to January 2007 (Bulgaria and Romania), the TPD dates from January 2007 (Bulgaria, Germany, Romania, and United Kingdom) to August 2009 (Czech Republic). This variation in the effective dates of the directives is at the core of our identification strategy.

We use two proxies for market liquidity. The *Bid-Ask Spread* is conceptually close to the desired construct and commonly used in empirical research to capture information asymmetry (e.g., Stoll, 1978; Venkatesh and Chiang, 1986; Glosten and Harris, 1988). We obtain the closing bid and ask prices for each day and compute the daily quoted spread as the difference between the two prices divided by the mid-point. We then take the median daily spread over the quarter for a given firm. Our second proxy, *Zero Returns*, is the proportion of trading days with zero daily stock returns out of all potential trading days per quarter. It is also commonly used and more widely available than spreads because it relies just on returns data (e.g., Lesmond et al., 1999; Bekaert et al., 2007). In the liquidity regressions, we follow prior literature and control for firm size using the market value of equity, share turnover, and return variability (Chordia et al., 2000; Leuz and Verrecchia, 2000). We estimate the bid-ask spread regressions in a log-linear form using the natural logarithm of the bid-ask spreads and the control variables, and lag the control variables by four quarters. Price and volume data are from Datastream.[19] Except for variables with natural lower and upper bounds, we truncate all variables at the first and 99th

[19] Our primary source of bid-ask spread data is Datastream. To increase sample size in some of the smaller EU countries (i.e., Czech Republic, Latvia, Luxembourg, Romania, Slovakia, and Slovenia) we complement this data with spreads from Bloomberg. For U.S. firms, we add spread data from CRSP because Datastream does not have this data in the early years of our sample period. Doing so does not materially affect the results.

percentile. Panel B of Table 1 reports descriptive statistics of the variables used in the regression analyses, and in the table notes we provide further details on the variable measurement.

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

4.1. Average Liquidity Effects

As described in Section 2, the MAD and TPD are an attempt to tighten and harmonize EU securities regulation, particularly with respect to the enforcement of existing regulations. We estimate cross-sectional panel regressions benchmarking the firm-level liquidity effects after the MAD and TPD against EU firms' own history and against a global sample of non-EU firms that are not subject to the new directives. We present the results in Table 2. We estimate the effects for each directive separately and then combine the two directives in one model. As is common for liquidity models and given our extensive fixed-effects structure, the explanatory power of the regressions is high with an R^2 of 76 percent for bid-ask spreads and 53 percent for zero returns. The firm-specific control variables are significant and exhibit the expected signs. Large firms and firms with a high share turnover have lower bid-ask spreads and fewer zero-return days. Firms with more volatile returns have larger spreads but a lower proportion of zero returns. The negative association between return volatility and zero returns likely stems from the fact that low volatility often means a higher frequency of days with zero returns. However, excluding return volatility (or any other control variable) from the model does not affect the results.

For our test variables, we find that the coefficient on *MAD* is negative and statistically significant for both spreads and zero returns. The estimated effects are also economically significant. In the spread regression, a coefficient of -0.177 suggests that, on average, bid-ask

spreads decrease by 16 percent.[20] In the return regressions, a coefficient of -0.043 suggests a decrease in the proportion of days with zero returns by 15 percent relative to the pre-directive median. The coefficients on *TPD* are also negative. An estimate of -0.305 indicates a reduction in bid-ask spreads by 26 percent, while an estimate of -0.040 translates into a reduction in the proportion of zero returns of 14 percent. Thus, for both directives and proxies, our estimates are quite consistent and, in three cases, around 15 percent. *MAD* and *TPD* remain significant and have a similar magnitude when we introduce both indicators into the model.

A way to further gauge the magnitude of the effects is to compute the round-trip savings in trading costs from the estimated bid-ask spread reduction. The average spread before the introduction of the directives is about $0.35 (or 3.45% of share price). Thus, our estimates imply a decline of the average spread by 5-6 cents. Multiplying the estimated trading costs savings with trading volume for each firm per year, and then aggregating across firms and directives, we obtain annual trading cost savings of roughly 0.2% of market capitalization. This magnitude is clearly economically significant, especially when considering the recurring nature of the savings, but the effect is not too large to be plausible.

4.2. Assessing Identification and Within-Country Estimation

Our goal is to estimate the causal effect of securities regulation on market liquidity. Towards this end, we exploit within-EU variation in the entry-into-force dates for two EU securities regulation directives. Given this empirical strategy, it is important to assess whether these dates provide reasonably sharp identification with respect to changes in market liquidity as well as to address concerns about the anticipation of regulatory effects. Moreover, while our empirical model controls for common shocks to EU market liquidity over the sample period, bias

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

could come from omitted variables that are correlated with both within-EU variation in the entry-into-force dates as well as changes in market liquidity. To gauge these issues, we conduct four additional tests.[21]

First, we introduce a separate indicator variable into the model for the year prior to the directives (i.e., quarter t-4 to quarter t-1). The purpose of this analysis is to see whether liquidity is already elevated or trending ahead of the directives (in event time). We find for both liquidity proxies that the indicator variable marking the year leading up to the directives is not significantly negative (Table 3, Panel A). An F-test for the differences across coefficients confirms that market liquidity is significantly higher after the MAD (TPD) becomes effective compared to its level in the year leading up to the directive. That is, liquidity appears to increase right around the time the directives enter into force. Moreover, the liquidity regressions do not appear to suffer from significant anticipation problems.

Next, to more finely gauge the sharpness of the entry-into-force dates and their relevance for our results, we (counter-factually) shift the assignment of the implementation dates quarter-by-quarter for all EU countries, and each time re-estimate the regression model in Table 2 noting the coefficient on the respective directive. If the liquidity effects are indeed related to the implementation of the directives, the estimated coefficient should be attenuated as we move away from the true implementation dates. The attenuation of the treatment effect should occur in both directions, as we move our assignment ahead of or beyond the true implementation date.

[21] In addition, we conduct analyses for which we randomly assign implementation dates to the EU countries between the first quarter of 2001 and the second quarter of 2004. This period precedes the entry-into-forces dates for the MAD in all countries, except for Lithuania. Using 300 replications, the average placebo effect is close to zero, as expected. The distribution of coefficients generated by these regressions also provides a way to gauge our inferences by bootstrapping the significance levels. For the spreads, we find only two cases (0.67%) for which the placebo regressions produce a coefficient that is more negative than the *MAD* coefficient reported in Table 2. They never exceed the TPD coefficient. For zero returns, the coefficients are always smaller than the MAD or TPD coefficients in Table 2. Based on this evidence, we conclude that our inferences are conservative.

By shifting the date back in time, we essentially assign non-treatment quarters to the treatment period, while moving the date forward assigns treatment quarters to the pre-directive period. As Table 3, Panel B, shows, the coefficients on *MAD* "peak" close to the true entry-into-force dates and become smaller in magnitude (and insignificant) as we shift the assignment of the implementation dates away from the true dates. This pattern is comforting because it suggests that the implementation dates are indeed critical for our findings. The pattern for *TPD* looks similar using zero returns. For the spreads, the TPD effect appears delayed, i.e., coefficients continue to decline beyond the true entry-into-force dates before they start reversing (after quarter t+4). Possibly, it takes time until the TPD becomes fully effective.[22] The more likely explanation is that, because the TPD occurs late in our sample period, countries with late implementation dates drop out of the sample as we shift dates further and further out. As these countries have smaller treatment effects (Section 5), the average effect seemingly increases when shifting the dates.

Third, we graphically inspect the liquidity patterns and plot the effects of both directives in event time. We estimate a separate coefficient for each quarter, starting six quarters before and ending four quarters after each directive becomes effective (see Figure 2). There is a clear and relatively sharp decline in the spreads and zero returns for the MAD and TPD, which typically begins in the quarter before the effective date (t–1). Thus, the liquidity effects appear to be clearly attributable to the directives and do not reflect a gradual trend in the EU countries.

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

Fourth, we construct a separate sample of EU firms trading on one of the alternative (unregulated) markets in the EU, which are not subject to the MAD and TPD.[23] Unless there are some spillover effects (e.g., due to externalities or competition with regulated markets), unregulated firms should not be affected by the two directives. Thus, we can benchmark the liquidity effects for regulated firms around the introduction of the MAD and TPD against this control group of firms, which operate in the same countries and economic environments. This within-country estimation absorbs local shocks that are common to all firms in a country and hence likely unrelated to the directives. We augment the model in Eq. (1) by including a binary variable that indicates unregulated firms in the post-MAD (post-TPD) period and report results in Panel C of Table 3 (for both the worldwide and the EU-only sample).[24] For both liquidity variables the coefficients on *MAD* and *TPD* for firms trading in regulated markets are negative and significant. In contrast, the coefficients on *MAD* and *TPD* for unregulated firms are never significantly negative and, with one exception, close to zero in magnitude. F-tests indicate that the liquidity effects differ significantly across regulated and unregulated firms. This analysis suggests that liquidity improves only for firms that are subject to the new EU directives, which provides further support for a causal effect of securities regulation on market liquidity.

Finally, we address concerns about the influence of other EU regulations. As explained in Section 2, the MAD and TPD are part of the EU's Financial Services Action Plan which contains a series of directives. To assess whether other major directives affect our analysis, we augment

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[23] We identify firms trading on unregulated EU markets by searching the websites of all European exchanges. If we cannot find data on the websites, we contact the exchanges directly to obtain a list of constituent firms. This procedure identifies 32,124 (35,058) firm-quarter observations from unregulated markets with spread (zero returns) data. However, these lists often identify constituents only at a certain point in time, and hence are imperfect. To further purge potentially unregulated firms from the analysis, we delete firms with an average market capitalization under US$ 20 million from the treatment sample of *regulated* EU markets. This cut-off is below the median market value of firms trading on *unregulated* EU markets.

[24] An alternative way to implement within-country estimation is to introduce *country-quarter* fixed effects and to estimate liquidity changes around the MAD and TPD using an indicator for regulated (unregulated) firms. Results using this design are similar to those in Panel C, and the inferences remain the same.

the empirical model by indicators for the Prospectus Directive (*PROSP*) and the Markets in Financial Instruments Directive (*MiFID*), which came into force during the sample period, and together with the MAD and TPD form the four Lamfalussy Directives. The results are reported in Panel D of Table 3. Consistent with Cumming et al. (2011), we find a negative coefficient on the *MiFID* variable indicating an increase in liquidity around the directive. But the effect is only close to conventional significance levels and disappears when we jointly introduce all four Lamfalussy Directives in the model. The results for the Prospectus Directive differ in sign and magnitude across the two liquidity proxies and are likely spurious. More importantly, when we control for *MiFID* and *PROSP*, the coefficients on *MAD* and *TPD* remain significantly negative and have a similar magnitude as before.[25]

We conduct an extensive set of additional sensitivity analyses regarding (i) the clustering of the standard errors, (ii) the choice of the fixed-effects structure, (iii) the composition of the sample, (iv) the inclusion of additional control variables, and (v) the use of alternative dependent variables such as the cost of capital. These analyses show that our findings are robust. We report and discuss them in Appendix B.

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

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

Our analyses up to this point suggest that the imposition of tighter securities regulation has a (causal) effect on market liquidity. In this section, we examine heterogeneity in the treatment effects, in particular, due to differences in prior regulation as well as in implementation and enforcement. For the reasons discussed in Section 2, it is unlikely that the two directives have

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

uniform effects throughout the EU. For instance, countries with a proven track record of implementing regulation and government policies are expected to implement new regulation in an effective manner. Furthermore, it is plausible that, by improving the enforcement regime, the new directives complement existing securities regulation and hence benefit mostly countries with extensive regulation. Alternatively, one could argue that countries with weaker securities regulation benefit the most from harmonized, EU-wide efforts to improve extant regulation. To explore these arguments and to test for cross-sectional differences in the liquidity effects, we introduce two partitioning variables, one for high levels and one for low levels, into the base specification. This extension of Eq. (1) leads to the following empirical model:

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

The *Partitioning Variables* are binary indicators set equal to '1' for either the group of EU countries with a high or a low level of a specific implementation or enforcement characteristic, and to '0' otherwise. We then test for significant differences between the coefficients β_1 and β_2 to assess whether the liquidity effects differ within the EU. As before, we use *Bid-Ask Spreads* and the proportion of trading days with *Zero Returns* as the dependent variables. We use the same set of firm-level control variables and fixed effects as in Table 2, and continue to compute the t-statistics based on standard errors clustered by country.

We employ various country-level characteristics to partition EU countries with regard to (i) the quality of prior regulation, (ii) the strength with which the MAD and TPD are implemented and enforced, (iii) countries' implementation timing for the two directives, and (iv) the level of and change in supervisory resources committed to securities regulation. Table 4 provides a by-country overview of the partitioning variables (and the resulting binary indicators).

The first variable is the quality of prior regulation. We partition the data based on an index taken from Kaufmann et al. (2009) that measures the government's ability to formulate and implement sound policies and regulations that permit and promote private sector development (*Regulatory Quality*). Higher index values indicate better regulatory quality. For the analyses, we split the treatment sample countries by the EU median in 2003, that is, before the two directives came into force.

Next, we develop measures for the implementation and enforcement of the MAD and TPD across EU member states. We create six directive-specific variables, three for the MAD and three for the TPD: (i) *Maximum MAD Fine* is the maximum monetary penalty that the supervisory authority can impose on security issuers for violations of Article 2 of MAD (CESR 2008).[26] (ii) *Supervisory Powers* equals the number of positive answers (out of 86 possible) by the supervisory authority in each EU member state to a questionnaire on the existence of specific supervisory powers regarding the translation of the MAD into local law (CESR 2007).[27] Higher values imply more supervisory powers. (iii) *Action Taken by 2009* indicates EU countries that have taken at least a single enforcement action regarding violations of the MAD by 2009 (e.g., imposed a fine).[28] (iv) *Maximum TPD Fine* is the maximum monetary penalty that the supervisory authority can impose on security issuers for violations of Articles 4 to 6 of the TPD

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

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

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

(CESR 2009a).[29] (v) *Shift in Enforcement* indicates a substantial change in the enforcement of financial reporting rules around the entry-into-force of the TPD. We construct this variable based on a survey that we sent out to the authority responsible for supervising compliance with accounting standards as well as the technical departments of PricewaterhouseCoopers, an international audit firm, in each EU country.[30] (vi) *Compliance with CESR Std. 1* represents the sub-set of EU countries that by the end of 2008 fully comply with all the enforcement principles proposed in CESR Standard No. 1.[31] We transform each continuous implementation proxy into binary partitioning variables splitting by the treatment sample median.

The last set of partitioning variables focuses on the resources that countries commit to enforcing the directives (see also Enriques and Gatti, 2008). We use the *Staff Level* measured as the number of full-time employees working for the supervisory authority in charge of securities regulation in 2003 as a proxy for the level of resources committed to enforcement prior to the MAD and TPD.[32] Staff numbers are more readily available and easier to compare across countries than budgets. To make staff numbers comparable, we scale them by the number of

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

[30] We code *Shift in Enforcement* as '1' if the local enforcement authority indicated that it implemented a comment and review process for compliance with accounting standards for the first time, and the audit firm replied that, according to their own assessment, a significant shift in the intensity of enforcing compliance with accounting standards occurred during the sample period. In cases of disagreement between the two sources and if we could not resolve the issue by going back to the respondents, we coded *Shift in Enforcement* as zero.

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

[32] Our principal source for full-time supervisory staff is the annual report of the local securities regulators. If we are unable to find an annual report, we use the staff numbers reported in Central Banking Publications (2009). If neither of these sources provides staff numbers in a given year, we interpolate the number from the other years with available data. If the sources provide data only for a joint regulator (that also oversees 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.

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

5.2. Differential Liquidity Effects within the EU

In this section, we report findings on the heterogeneity in the treatment effects using bid-ask spreads as liquidity proxy from estimating Eq. (2). The results for zero returns are very similar and available from the authors upon request. For brevity, we tabulate only the coefficients (and t-statistics) for the main variables of interest.[34] In Table 5, we begin with splits for prior conditions and regulatory quality for the MAD (Panel A) and the TPD (Panel B). Thereafter, we present splits using the directive-specific variables for implementation and enforcement. The table also reports p-values for the tests of differences in the coefficients across groups.

Using *Regulatory Quality* in 2003 to partition EU member states, we find that the coefficients on *MAD* and *TPD* are negative and significant when prior regulation is relatively strong. In countries with relatively weak prior regulation, the coefficients are still negative but much smaller and not significant. For the MAD, the liquidity effects are statistically different across the two groups. For the TPD, the difference in the coefficients is not significant at conventional levels, but judging by the coefficient magnitudes the issue is primarily power. It is

[33] If available, we use the growth of supervisory staff specifically assigned to the oversight of securities regulation. If these data are not available, we use the staff growth of the entire regulator. In countries that created separate monitoring bodies to review financial statements (e.g., Germany and the U.K.), we include the staff growth of these bodies as well.

[34] As in Appendix B, we conduct several robustness checks and re-run the cross-sectional analyses (i) with standard errors clustered by economic region, (ii) adding separate quarter-year-fixed effects for developed markets, (iii) replacing country- and industry-fixed effects with firm-fixed effects, and (iv) controlling for macroeconomic factors. The results and inferences are similar to those reported in Tables 5 to 7.

also important to note that we report two-sided tests and that our bootstrapping exercise suggests our standard errors are conservative (see footnote 21). Thus, the results as indicate that the liquidity effects for both directives are concentrated in countries with a stronger track record of implementing regulation in the past.[35] In line with this finding and the hysteresis hypothesis, our survey of auditors and regulators confirms that the directives led to significant changes even in countries with strong prior securities regulation (e.g., the U.K.).[36]

Next, we report bid-ask spread results using the directive-specific implementation and enforcement variables. The tenor of the results is very similar across the six split variables. The MAD and the TPD coefficients are always negative and significant for the subset of EU countries that implement and enforce the directives relatively strongly. The coefficients are much smaller and, with one exception, not significant for EU countries with relatively weak implementation. More specifically, there are significant improvements in liquidity around the MAD in countries that have higher monetary fines for violations, that confer more supervisory powers to the local regulator, and that subsequently enforce the insider trading rules.[37] Similarly, the TPD is associated with an increase in liquidity when countries set relatively high monetary fines for violations, when regulators and auditors indicate that there were substantial improvements in how the supervisory authorities enforce financial reporting rules, and when countries fully comply with CESR's enforcement principles. We note that the (two-sided) p-values for the differences in the coefficients between strong and weak implementation countries

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

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

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

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

Before we combine our proxies for prior regulation and implementation in one model, we turn to the issue of implementation timing. Given that our empirical specification accounts for EU liquidity trends in a flexible manner, a country being a quarter earlier or later should have no systematic impact on the results. Moreover, as discussed in Section 3, countries' legislative processes that implement the directives are lengthy and quite inflexible which makes it unlikely that countries can time the entry-into-force dates *in response to* country-specific liquidity trends or local changes in market liquidity. Moreover, the analyses in Section 4.2 show that the effects occur relatively sharply around the entry-into-force dates. For all these reasons, the exact timing of the implementation dates itself should have little impact on the estimate of the average treatment effect. However, differential timing could create spillover effects from early countries to late countries, which in turn could induce heterogeneity in the treatment effects. That is, if markets *throughout the EU* reacted at the time the first set of countries implements the directive, then the liquidity effects would be weaker or non-existent by the time the directive becomes effective in countries with later dates. In this case, the heterogeneity in the liquidity effects is an artifact of differential timing, rather than of differences in implementation or enforcement.

To address this concern, we perform three analyses. First, we examine the relation between implementation timing and our prior regulation and implementation strength proxies. To do so, we partition EU countries into early and late MAD (or TPD) adopters using the median entry-into-force date as cut-off value and provide simple contingency tables with Chi Square tests.

Table 6 shows that the early countries are not necessarily the countries with a stricter implementation (or vice versa). In fact, observations are quite evenly distributed and the Chi Square tests are never significant. Second, we re-run the analyses examining the sharpness of the entry-into-force dates in Table 3, Panels A and B, separately for the strong and weak countries, and find that the timing patterns look very similar across strong and weak groups, which is inconsistent with spillover effects from earlier to later countries. Third, we randomly assign countries that implement a directive after the median implementation quarter in the EU an 'as if' implementation date from the early countries. The idea behind this counterfactual assignment is to examine whether late countries would have had a stronger liquidity effect if we had measured it earlier or countries had adopted the directives as early as the others. Such effects would be predicted by the spillover explanation. As shown in the last column of Table 5, the MAD and TPD coefficients using the 'as if' early dates are insignificant and positive. Thus, it is unlikely that our cross-sectional results reflect merely differential timing.

5.3. Conditioning on Prior Regulation

To further examine the hysteresis and catching-up hypotheses, we condition on prior regulation and combine the binary *Regulatory Quality 2003* indicator variable (high vs. low quality) with each of the six implementation variables (strong vs. weak implementation) in Table 7. These two-way partitions sort the post-MAD or TPD observations into four distinct bins, for which we estimate the liquidity effects using essentially the same model as in Eq. (2). For instance, the coefficient estimate labeled *High RQ/Strong IS* represents the liquidity effect of the 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 and a weak implementation of the new directives.

The table again presents only the coefficient estimates and t-statistics of the four distinct groups of EU countries, but the model includes all controls and fixed effects. The analyses indicate that countries with a strong track record for past regulation and a strict implementation and enforcement of the new directives have the largest liquidity improvements. The coefficients in the *High RQ/Strong IS* bins are always negative and highly significant. The effects for this sub-set of EU countries are statistically different from any other sub-set at the 10 percent level or better in all but two cases. The rank order of the coefficient magnitudes going from the *High RQ/Strong IS* to the *Low RQ/Weak IS* bin is generally monotonically decreasing. Moreover, the differences in the liquidity effects for countries with strict versus weak implementation are generally larger and they are significant only in countries with high prior regulatory quality. Thus, there is little evidence that countries with weaker securities regulation catch up with stronger countries as a result of the new EU directives. In contrast, the liquidity differences widen. This finding shows that imposing the same regulation in countries with different initial conditions can result in countries drifting further apart, rather than moving closer together, illustrating the difficulties of regulatory harmonization.

To gauge the robustness of our cross-sectional findings, we use an alternative set of variables based on the resources that countries commit to the supervision and enforcement of securities regulation. We distinguish between resources committed in the past and changes in the resources committed around the implementation of the directives, and present the results in Table 8. In the *Staff Level* partition, we find that that the effects of the MAD and TPD are significant only for the group with a high level, and that the differences between the two groups are significant. This pattern is consistent with the results for prior regulatory quality in Table 5. Similarly, in the *Staff Growth* split we find that the effects of the MAD and TPD are significant

only in those countries with substantial changes in the resources they commit to supervision and enforcement. The effects are statistically insignificant in countries with moderate or no changes in supervisory staff, but we note that the differences between the high and low growth countries are never statistically significant. When we combine the *Staff Level* and the *Staff Growth* variables to form four bins (as we did in Table 7), the *MAD* and *TPD* coefficients are significantly negative only for the *High Level/High Growth* group. The liquidity effects in the other bins are smaller and insignificant. Thus, similar to Table 7, the countries that exhibit the largest liquidity improvements are those that already had relatively well-staffed supervisory authorities to begin with, but further increase the supervisory staff around the introduction of the new directives.

Overall, the cross-sectional analyses provide little evidence supporting the catching-up hypothesis. Instead, there appears to be considerable hysteresis in regulatory outcomes. The same forces that have limited the effectiveness of securities regulation in the past appear to be at play when new rules are introduced, suggesting that history and countries' initial conditions matter a great deal for regulatory outcomes.

6. Conclusion

This paper examines capital-market effects of changes in securities regulation. We focus on two key EU capital-market directives pertaining to market abuse and transparency regulation. As there were prior EU directives and national laws banning insider trading and requiring financial reporting, the two directives are essentially tightening and harmonizing existing EU securities regulation, particularly with respect to supervision and enforcement. We use this setting to examine the role of differences in implementation and enforcement for regulatory outcomes. We also analyze the effects of prior regulation, i.e., the extent to which the EU

directives allow weaker countries to catch up or, alternatively, provide a springboard for stronger countries to advance even further.

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

Overall, the results show that stronger securities regulation can have significant economic benefits. Specifically, we find that tighter insider trading and transparency regulation increases the liquidity of firms' share markets. We present extensive sensitivity analyses, including within-country estimation, to gauge our identification strategy, to probe whether our results are indeed causal, and to address concerns about the timing of the regulatory effects and the implementation of the directives. We also provide corroborating evidence that the directives lower firms' cost of capital but hasten to add that our setting and identification strategy are less well suited for anticipatory proxies such as the cost of capital.

The second part of the paper presents 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. Stricter implementation and enforcement of the two directives also result in larger liquidity effects, but these effects stem largely from countries with strong prior regulatory quality and with already relatively well-staffed securities regulators. One explanation for these findings is that countries that have put more resources into securities regulation and that have a better track record of implementing and

enforcing regulation are more willing and better able to implement the new EU directives. Put differently, the same forces that limited the strength of past regulation appear to be at work when new rules are introduced. It is important to note that these forces could span a wide range, including institutional fit, resource constraints, inefficient bureaucracies, and political pressures, and that our tests cannot distinguish between these forces.

In sum, our findings support a causal link between stricter securities regulation and market liquidity. They also support the notion that the success of regulation depends critically on how regulation is implemented and enforced. Thus, policy debates should pay close attention to implementation and enforcement issues if regulation is to have the intended effects. Our finding that countries with weaker securities regulation do not catch up with stronger countries illustrates the difficulty of harmonizing capital markets through regulatory reforms. It highlights that prior regulatory conditions matter and that imposing the *same* regulation on countries with disparate initial conditions can have the effect of making countries diverge more, not less.

In closing, we highlight 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 beneficial net of costs or that they are socially beneficial. Our results also do not imply that countries with weaker implementation and enforcement of securities regulation "leave money on the table." We need more research to assess these issues and establish the welfare consequences.

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Appendix A: EU Securities Regulation, Market Abuse and Transparency Directives

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

The FSAP introduced 42 measures, each with their own specific objective (CRA 2009). There are four so-called Lamfalussy Directives that form the core of the FSAP in the area of securities regulation: the Market Abuse Directive (MAD), the Transparency Directive (TPD), the Prospectus Directive (PROSP), and the Markets in Financial Instruments Directive (MiFID). Our analysis focuses on both the MAD and TPD. The PROSP pertains to securities offerings and hence seems less relevant for our analysis of secondary-market liquidity. The MiFID is the last of the four Lamfalussy directives. It became effective in November 2007. Its main objective is to increase competition and consumer protection in investment services. Thus, like the PROSP, it seems less relevant to our analysis than the MAD or the TPD.[38]

A.1 Market Abuse Directive (MAD)

The MAD aims to prevent insider trading and market manipulation. It contains three key elements: (i) disclosure rules designed to reduce the scope of inside information, (ii) ex-post sanctions for insider trading or market manipulation, and (iii) tightened enforcement of compliance with insider trading and market manipulation rules. The core disclosure rule in the

[38] Nevertheless, we examine the impact of all four directives and the extent to which the other two Lamfalussy Directives influence our results for the MAD and TPD. See Table 3, Panel D.

MAD requires issuers of financial instruments to inform the public as soon as possible of inside information (Article 6). Moreover, executives must disclose the transactions in the securities of the firm they manage in a quick and transparent manner.

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

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

The MAD replaced an older directive from 1989 banning insider trading and its implementation required legislative changes in all EU member states. According to the British Institute of International and Comparative Law (2005), EU member states followed the directive's language closely when implementing it into national regulation. In general, the MAD should be viewed as improving and tightening existing insider trading regulation in the EU, particularly with respect to enforcement. It led to many substantial changes in the market abuse

regimes in EU member states (see CESR, 2007, 2008 and 2010 for detailed by country descriptions). For example, many countries increased the powers of the national supervisory authority to seize and retain documents and data that may further an investigation into insider trading.

A.2 Transparency Directive (TPD)

The TPD aims to ensure transparency for investors through a regular flow of information. It uses two regulatory tools to improve transparency: (i) a set of disclosure requirements, and (ii) tightened enforcement of compliance with the disclosure requirements. The TPD includes provisions for ongoing disclosures (e.g., the filing of annual and semi-annual reports in accordance with International Financial Reporting Standards, IFRS) and requirements that ensure the disclosure of significant events (e.g., significant holdings by shareholders). However, IFRS were already mandated by older EU regulation (Regulation No. 1606/2002) and most exchanges already required the filing of semi-annual reports and the disclosure of significant events.[39] Hence, the TPD did not significantly expand existing disclosure requirements. It stipulated major changes to the supervisory regime and the enforcement of corporate reporting and disclosure rules in the EU. Thus, it should be viewed as improving and tightening existing transparency regulation. The TPD required legislative changes in all EU member states.

To mention a few specific changes to the supervisory and enforcement regime, the TPD requires each member state to designate a competent supervisory authority.[40] This authority is in charge of monitoring compliance with the reporting and disclosure requirements set out in the directive and it must be given appropriate powers to enforce these requirements. Similar to the

[39] IFRS is a set of accounting standards that were adopted by all EU member states in 2005. In untabulated sensitivity tests, we control for IFRS adoption and find that doing so does not affect our results.

[40] For further details and examples, see CESR (2009a) and Mazars (2010).

MAD, the TPD stipulates that the authority is given certain supervisory powers (Article 24). It must examine and monitor required disclosures and, if infringements are discovered, take appropriate action (e.g., issue a fine). Moreover, when investigating compliance, the authority must be able to request information from auditors and shareholders, and to carry out on-site inspections.[41] The requirement to monitor and enforce compliance with existing disclosure rules represents a significant change because most member states had no, or very limited, monitoring and enforcement by a securities regulator prior to the TPD. Even in the U.K., which arguably has had a relatively strong regime, compliance with the reporting and disclosure requirements was examined only in response to specific complaints received by the U.K. regulator. With the TPD, the U.K. moved to a proactive and systematic review process of financial reports. Our survey of regulators and auditors indicates that this move was a significant change to enforcement regime in the U.K.

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

[41] 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).

Appendix B: Additional Sensitivity Tests

In this appendix, we report a series of robustness checks for the market-liquidity effects presented in Section 4.[42] In particular, we gauge the sensitivity of our findings to (i) the clustering of the standard errors, (ii) the choice of the fixed-effects structure, (iii) the composition of the sample, (iv) the inclusion of additional control variables, and (v) the use alternative dependent variables. Unless stated otherwise, we estimate the same specification as in Table 2, and report in Table B1 the coefficients on *MAD* and *TPD* together with the t-statistics (in parenthesis) for bid-ask spreads (Panel A) and the proportion of zero-return days (Panel B).

First, we consider alternative ways to cluster the standard errors. In our main specification, we use clusters by country. As the first two rows in the respective panels in Table B1 illustrate, the inferences remain the same when we use two-way clustering by country and quarter, and when we cluster the standard errors by economic region. The latter approach combines several EU countries (e.g., Western Europe, Eastern Europe, etc.) and in this sense is more conservative than country-level clustering.

Second, we alter as well as expand the fixed-effects structure of our empirical specification. When we replace the country- and industry-fixed effects with firm-fixed effects, the results are very similar, although we note that the magnitude and significance of the TPD effect is slightly attenuated. Next, we augment the current base model by adding separate quarter-year fixed effects for developed countries (as identified in the Morgan Stanley Capital International database). This specification accounts for the possibility that developed markets exhibit different

[42] We also conduct several robustness checks for the analyses in Section 5. We re-run the cross-sectional analyses (1) with standard errors clustered by economic region instead of country, (2) adding separate quarter-year-fixed effects for developed markets, (3) replacing country- and industry-fixed effects with firm-fixed effects, or (4) controlling for macroeconomic controls. The results and inferences are similar to those in Tables 5-7.

liquidity trends or are differentially affected by economic shocks during the sample period. Along the same lines, we introduce separate size coefficients for *each* quarter. This expansion of the fixed-effects structure should absorb economic shocks that affect larger firms differently from smaller firms. In both cases, the results are similar and the inferences remain the same as those reported in Table 2. In additional tests (not tabulated), we further include separate volatility coefficients for each quarter, interactions between the country indicators and firm size, or interactions between the country indicators and the industry dummies. Again, the results are similar to those reported in Table 2. Based on these results, we conclude that it is unlikely that the findings reflect merely economic shocks to particular groups of firms or that they are driven by differences in the composition of firms across countries.

Third, we gauge the impact of the sample composition and the choice of benchmark sample on the results. As Table B1 shows, the results remain virtually the same when we estimate the regressions within the EU. Thus, the use of benchmark firms outside the EU has little impact on our results (as it should with the fixed-effects structure). Next, we restrict and hold the treatment sample constant around the introduction of the directives to make sure that our results are not an artifact of changes in the sample composition over time (e.g., due to avoidance behavior). That is, we limit the treatment sample to firms with data available in both the year immediately before and after the entry-into-force date of the respective directive. As shown in the table, the results are virtually the same. For our next robustness check, we drop the observations from the four largest treatment countries (U.K., Germany, France, and Sweden) either one-by-one (not tabulated) or altogether to check that our results are not driven by a single country or just a few large countries. The results for *MAD* are very similar. For *TPD*, the spread results do not significantly change, but using zero returns the effect is attenuated and loses statistical

significance. Overall, however, it does not appear that a few large countries drive our results, especially when considering that we would expect to see some attenuation in light of the cross-sectional results in Section 5 and the fact that these four countries generally have high quality prior regulation and/or strong implementation. Finally, we check that our results also hold when we include Bulgaria and Romania, which did not join the EU until 2007 and adopted many EU regulations by that date.[43]

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

Fifth, we use alternative dependent variables to examine the effects of the MAD and TPD. In our main analyses we scale the bid-ask spreads by contemporaneous stock prices. Price could also could reflect (and in fact anticipate) the impact of the regulatory changes. To address this concern, we re-compute the spread variable and scale it by the earliest available stock price for each firm. For 50% of the sample, this price stems from the first or second quarter in 2001. The results are reported in Panel C of Table B1. We also report in Panel C that our results hold using the percentage spreads, rather than its natural logarithm, as the dependent variable. The liquidity

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

effect is about 11% for both directives and hence similar in magnitude to our estimates in Table 2. In light of the different spread levels across countries, we also check (but do not tabulate) that the cross-sectional results in Section 5 are not induced by using the logarithm.

Finally, we analyze changes in firms' costs of capital. Regulators often refer to improvements in firms' costs of raising capital as a justification for securities regulation (e.g., Lamfalussy, 2000; Enriques and Gatti, 2008; CRA, 2009). As discussed before, market liquidity seems better suited than proxies like the cost of capital or share prices that potentially anticipate the regulatory effects long before the directives become effective. Such anticipatory effects, in turn, make it difficult to exploit the staggered imposition of the directives for identification. In addition, it is harder to reliably measure the cost of capital on a quarterly basis. Thus, we conduct the cost of capital analysis merely to corroborate the liquidity analysis.

We use two commonly used proxies for the cost of capital: firms' implied costs of capital and dividend yields. The basic idea of the implied cost-of-capital models is to back out the internal rate of return that equates current stock price with the expected sequence of future (abnormal) earnings (e.g., Gebhardt et al., 2001; Hail and Leuz, 2006; Pástor et al., 2008). Conceptually, these models are consistent with discounted dividend valuation. We follow Hou et al. (2009) and use the predicted values from a pooled, cross-sectional regression of future (realized) earnings on a set of contemporaneous firm characteristics to derive earnings forecasts. We then plug these forecasts into the 12-year version of the Gebhardt et al. (2001) valuation model and solve for the internal rate of return (r_{GLS}) that equates a firm's estimated value with its market value of outstanding equity at the end of each calendar quarter.[44] We assign the cost-of-

[44] 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

capital estimate to the quarter of the pricing date. For the regression specification, we follow Hail and Leuz (2006, 2009) and control for inflation, GDP per capita, firm size measured by total assets, financial leverage, and return variability.[45] We report the *MAD* and *TPD* coefficients from the implied cost of capital regression (using similar sampling criteria as for liquidity) in Panel C of Table B1. Both coefficients are negative and significant, suggesting a reduction in the cost of capital of about 57 (20) basis points following the implementation the MAD (TPD). The control variables in the cost-of-capital model behave as predicted and are significant, except for return variability. As expected, the cost-of-capital regressions are noisier and so the explanatory power is only 34 percent compared with up to 76 percent in the liquidity models.

Our second cost-of-capital proxy, dividend yield, has been used extensively in prior finance studies (e.g., Bekaert and Harvey, 2000, Errunza and Miller, 2000; Bhattacharya and Daouk, 2002). We measure *Dividend Yield* as the actual dividends paid during the fiscal year divided by the stock price at the end of each quarter. For firms that do not pay dividends, we set the measure to missing. We use the same set of control variables as before plus asset growth, which is supposed to capture differences in firms' growth expectations.[46] As Panel C of Table B1 shows, the coefficients on *MAD* and *TPD* in the dividend yield regression are negative, but

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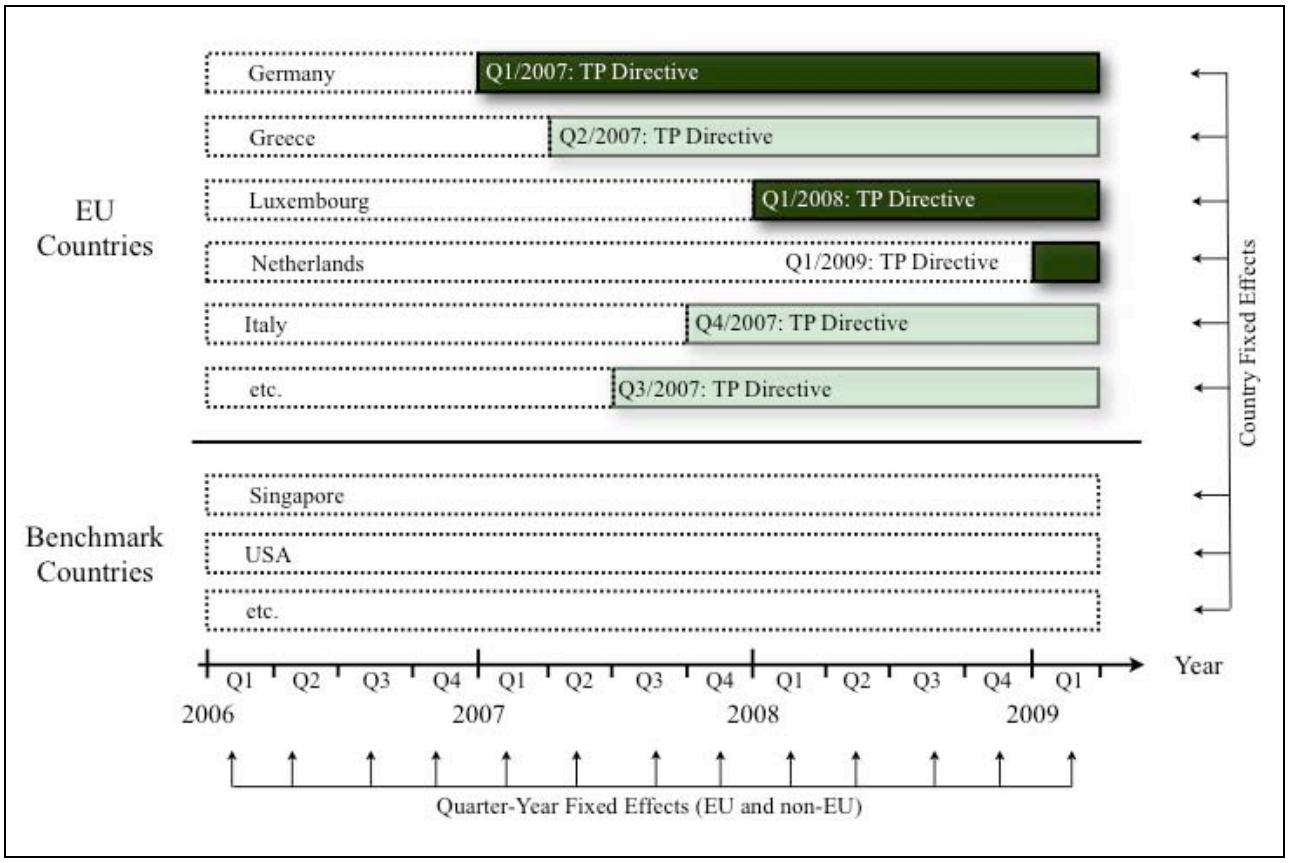
allow for differences in accounting practices across countries, we include country-fixed effects in the model. We estimate this regression for each forecast horizon (i.e., *t*+1, *t*+2, and *t*+3) and year using up to ten years of previous data. The predicted values of annual earnings are strictly out-of-sample. That is, we multiply the coefficient estimates of the pooled cross-sectional regressions with the yearly realizations of the independent variables that occur after the estimation period, but before the pricing date (i.e., the end of the calendar quarter). For details on the additional input parameters and the estimation procedure of the Gebhardt et al. (2001) approach, see also the appendices of Hail and Leuz (2006, 2009).

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

[46] We did not include this variable in the implied cost of capital models because these estimates explicitly account for growth differences by using firm-specific earnings forecasts. We measure asset growth as the year-to-year percentage change in total assets.

insignificant at conventional levels. However, the two directives are jointly significant and the coefficients, while imprecisely estimated, would be economically significant. Taking both proxies together, the evidence for the cost of capital is broadly consistent with our findings for market liquidity as well as the cost-of-capital effects shown in Bhattacharya and Daouk (2002) around the first enforcement of insider trading rules.

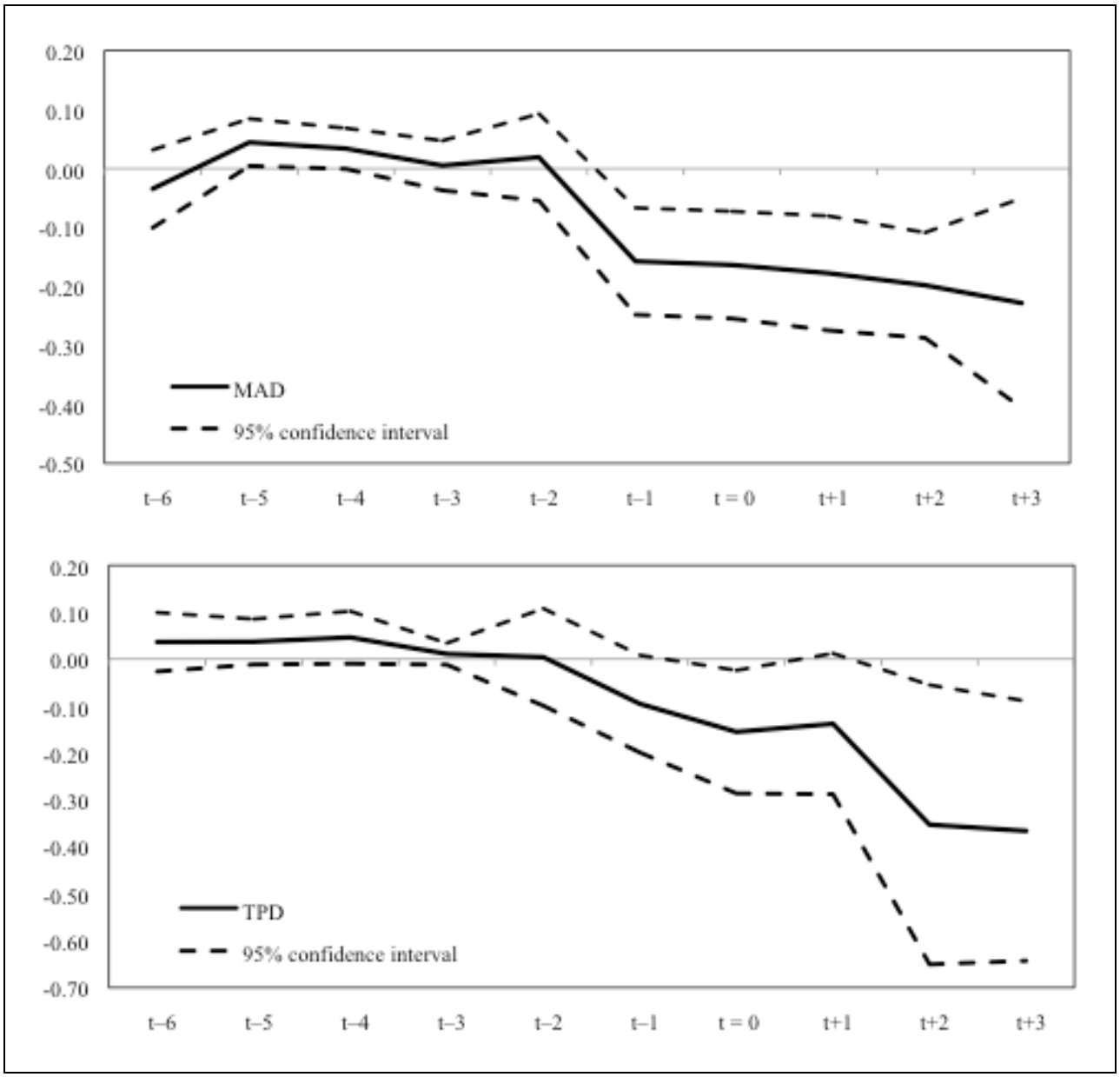
Figure 1: Illustrating the Identification Strategy for the Capital-Market Effects of the Transparency Directive



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.

Figure 2: Liquidity Patterns around the Entry-Into-Force Dates of the MAD and TPD

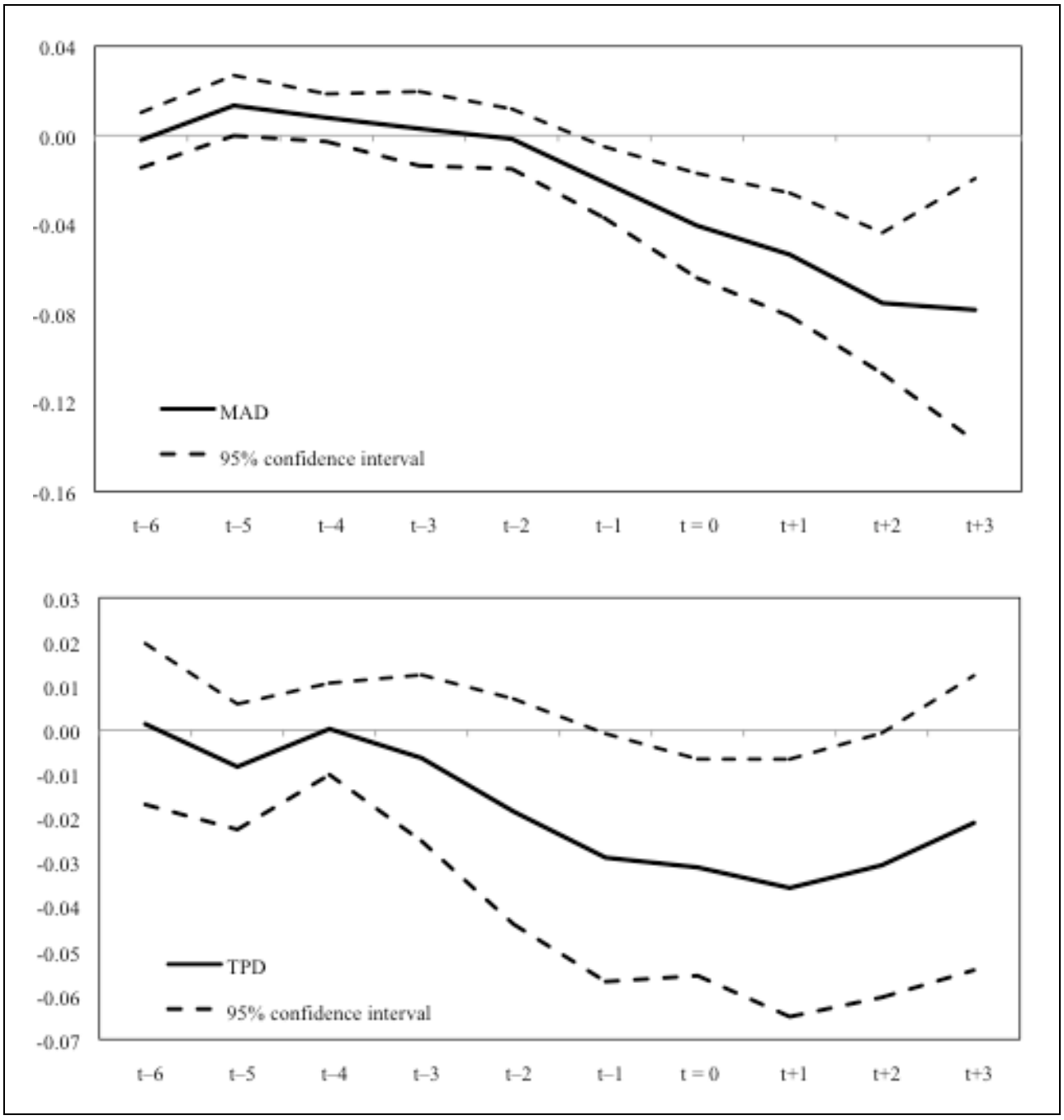
Panel A: Ln(Bid-Ask Spread) as Dependent Variable



(continued)

Figure 2 (continued)

Panel B: Zero Returns as Dependent Variable



The figure illustrates the liquidity patterns around the entry-into-force dates of the Market Abuse Directive (***MAD)*** and the Transparency Directive (***TPD***) in event time. It depicts the estimated liquidity effects (together with the 95% confidence intervals) for the six quarters before and the three quarters after the entry-into-force quarter (t=0) of the MAD or TPD. We estimate the liquidity effects separately for each quarter and directive using the regressions from Table 2, excluding treatment observations from the other quarters. The sample comprises firm-quarter observations from up to 27 (35) EU (non-EU) countries between the first quarter 2001 and the second quarter 2009. We use two dependent variables: (1) the ***Bid-Ask Spread*** measured as the quarterly median quoted spread (Panel A), and (2) ***Zero Returns*** measured as the proportion of trading days with zero daily stock returns in a quarter (Panel B).

Table 1: Sample Composition and Descriptive Statistics

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

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

(continued)

Table 1 (continued)

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

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

The treatment sample consists of all countries in the European Union (EU) except for Bulgaria and Romania, which did not join the EU until 2007. We also include Iceland and Norway from the European Economic Area (EEA), as they agreed to adopt the EU capital market directives in their entirety. For brevity, we refer to the EU countries as the treatment sample. In Panel A, we present the number of firm-quarter observations for the two liquidity measures used in the main analysis as well as the dates when the Market Abuse Directive (*MAD)* and the Transparency Directive (*TPD*) came into force in each EU country. In Panel B, we present descriptive statistics for the dependent variables and the firm-level independent variables used in the analyses. We present the statistics separately for the treatment (EU) and the benchmark (non-EU) samples, which comprise up to 27 and 35 countries, respectively. The sample comprises all firm-quarter observations beginning in the first quarter of 2001 through the second quarter of 2009 with financial data in Worldscope and price/volume data in Datastream. The *Bid-Ask Spread* is the quarterly median quoted spread (i.e., difference between the bid and ask price divided by the mid-point and measured at the end of each trading day). *Zero Returns* is the proportion of trading days with zero daily stock returns out of all potential trading days in a given quarter. *Market Value* is stock price times the number of shares outstanding (in US$ million) measured at the end of the quarter. *Share Turnover* is the quarterly median of the daily turnover (i.e., US$ trading volume divided by the market value at the end of each trading day). We compute *Return Variability* as the standard deviation of daily stock returns in a given quarter. All means (medians) are significantly different at the 1%-level across EU and Non-EU countries using t-tests (Wilcoxon rank sum tests).

Table 2: Capital-Market Effects from Tighter EU Securities Regulation

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

The sample comprises firm-quarter observations from up to 27 (35) EU (non-EU) countries between the first quarter 2001 and the second quarter 2009. We use two dependent variables: (1) the ***Bid-Ask Spread*** measured as the quarterly median quoted spread, and (2) ***Zero Returns*** measured as the proportion of trading days with zero daily stock returns in a quarter. ***MAD*** and ***TPD*** are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive or the Transparency Directive came into force. For a description of the control variables see Table 1. If indicated, we use the natural log of the raw values, and lag the variables by four quarters. We include country-, Campbell (1996) industry-, and quarter-year-fixed effects (for EU and non-EU countries separately) in the regressions, but do not report the coefficients. The table reports OLS coefficient estimates and (in parentheses) t-statistics based on robust standard errors that are clustered by country. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 3: Assessing Identification of the Capital-Market Effects from Tighter EU Securities Regulation

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

	Ln(Bid-Ask Spread) as Dependent Variable (N=611,969)				*Zero Returns as Dependent Variable (N=780,434)*			
	Market Abuse Directive		*Transparency Directive*		*Market Abuse Directive*		*Transparency Directive*	
	Coefficient	*t-stat*	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*
Period Relative to Entry-into-Force Dates (t=0):								
Year Leading up to Directive (*t*–4 to *t*–1)	-0.072	[-0.99]	0.083*	[1.80]	0.003	[0.30]	-0.025	[-1.56]
MAD	-0.276**	[-2.30]	–		-0.043**	[-2.09]	–	
TPD	–		-0.236*	[-1.80]	–		-0.063**	[-2.14]
F-test for Differences across Coefficients (p-value):								
(*t*–4 to *t*–1) = MAD or TPD	0.000		0.034		0.001		0.025	
Control for Other Directive	Yes		Yes		Yes		Yes	
Control Variables	Yes		Yes		Yes		Yes	
Fixed Effects	Yes		Yes		Yes		Yes	

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

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

(continued)

Table 3 (continued)

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

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

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

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

(continued)

Table 3 (continued)

The sample comprises firm-quarter observations from up to 27 EU and 35 non-EU countries between the first quarter 2001 and the second quarter 2009. We use two dependent variables: (1) the *Bid-Ask Spread* measured as the quarterly median quoted spread, and (2) *Zero Returns* measured as the proportion of trading days with zero daily stock returns in a quarter. *MAD* and *TPD* are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force. For a description of the control variables see Table 1. In Panel A, we include a separate indicator variable marking the four quarters leading up to the entry-into-force of the MAD or TPD (i.e., the period t–4 to t–1 relative to the entry-into-force date in t=0). In Panel B, we report the *MAD* and *TPD* coefficients from nine separate regressions. For each regression we counterfactually shift the 'true' MAD or TPD entry-into-force dates (t=0) to a different quarter. That is, we set the binary MAD or TPD indicator variables equal to one beginning in each quarter from t–4 to t+4 relative to the 'true' entry-into-force date. In Panel C, we use firms trading on (unregulated) EU markets that are not subject to the MAD and TPD as an additional benchmark sample. That is, we add a separate binary MAD or TPD indicator variable for unregulated EU firms to the model, and include those firms in the sample. To reduce measurement error, we further exclude treatment sample firms with, on average, market values below US$ 20 million. We report results for the full sample (including the non-EU benchmark firms) and the EU countries only. The unregulated markets (together with their country of domicile) are: Alternative Investment Market AIM (U.K.), AIM Italia and Mercato Alternativo del Capitale (Italy), Dritter Markt (Austria), EN.A (Greece), Enterprise Securities Market (Ireland), Euro MTF (Luxembourg), First North (several European countries), Marché Libre (France and Belgium), Mnohostranný obchodný systém (Slovakia), NewConnect (Poland), NYSE Alternext (several European countries), and Open Market (Germany). In Panel D, we assess the influence of the remaining Lamfalussy Directives on the results. We construct binary indicator variables similar to *MAD* or *TPD* and estimate regressions including (i) only the Markets in Financial Instruments Directive (*MiFID*), (ii) only the Prospectus Directive (*PROSP*), and (iii) all four of the Lamfalussy Directives (i.e., *MAD*, *TPD*, *MiFID*, and *PROSP*). Throughout the table, we include the full set of control variables and fixed effects in the models, but only report OLS coefficient estimates (t-statistics) for the main variables. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 4: Prior Regulation and Implementation Variables by EU Country

	Prior Regulation		*MAD Implementation Strength*					*TPD Implementation Strength*				*Supervisory Resources*			
Country	*Regulatory Quality 2003*		*Maxium Fine (EUR 000)*		*Super-visory Powers*		*Action Taken by 2009*	*Maxium Fine (EUR 000)*		*Shift in Enforce-ment*	*Compliance with CESR Std. 1*	*Staff Level 2003*		*Staff Growth 2004 to 2009*	
Austria	1.52	(1)	No fine	(0)	70	(0)	1	30	(0)	0	0	0.23	(0)	2.08	(1)
Belgium	1.36	(1)	Profit-based	(1)	69	(0)	0	2,500	(1)	0	1	0.24	(0)	-0.27	(0)
Bulgaria	0.59	(0)	50	(0)	69	(0)	0	5,112	(1)	0	0	0.16	(0)	0.13	(0)
Cyprus	1.20	(0)	1,710	(0)	68	(0)	0	341	(0)	0	0	0.22	(0)	0.07	(0)
Czech Republic	1.12	(0)	350	(0)	64	(0)	0	400	(0)	0	0	0.49	(1)	0.93	(1)
Denmark	1.79	(1)	No fine	(0)	60	(0)	0	No limit	(1)	1	1	0.19	(0)	0.23	(0)
Estonia	1.40	(1)	No fine	(0)	60	(0)	1	16,000	(1)	1	0	1.86	(1)	-0.01	(0)
Finland	1.90	(1)	200	(0)	63	(0)	0	200	(0)	1	1	0.33	(0)	-0.20	(0)
France	1.18	(0)	Profit-based	(1)	75	(1)	1	10,000	(1)	1	1	0.35	(1)	0.06	(0)
Germany	1.51	(1)	Profit-based	(1)	64	(0)	1	200	(0)	1	0	0.38	(1)	0.25	(1)
Greece	1.01	(0)	6,000	(0)	60	(0)	0	1,000	(1)	0	0	0.39	(1)	0.16	(0)
Hungary	1.08	(0)	Profit-based	(1)	73	(1)	0	24	(0)	1	0	0.63	(1)	0.81	(1)
Iceland	1.67	(1)	10,000	(0)	60	(0)	1	300	(0)	0	0	0.25	(0)	1.15	(1)
Ireland	1.66	(1)	588	(0)	73	(1)	0	2,500	(1)	1	1	1.35	(1)	0.30	(1)
Italy	1.02	(0)	Profit-based	(1)	70	(0)	1	620	(1)	0	1	0.27	(1)	0.58	(1)
Latvia	1.03	(0)	Profit-based	(1)	80	(1)	1	14	(0)	1	1	0.25	(0)	0.25	(1)
Lithuania	1.10	(0)	Profit-based	(1)	70	(0)	0	29	(0)	1	0	0.92	(1)	-0.04	(0)
Luxembourg	1.94	(1)	Profit-based	(1)	80	(1)	0	125	(0)	0	0	0.26	(1)	2.36	(1)
Malta	1.27	(0)	Profit-based	(1)	75	(1)	1	466	(0)	0	0	1.54	(1)	0.18	(0)
The Netherlands	1.76	(1)	Profit-based	(1)	67	(0)	1	120	(0)	1	0	0.34	(1)	0.25	(1)
Norway	1.39	(1)	Profit-based	(1)	59	(0)	1	No limit	(1)	0	1	0.34	(0)	0.32	(1)
Poland	0.61	(0)	1,250	(0)	70	(0)	0	1,389	(1)	0	1	0.78	(1)	1.59	(1)
Portugal	1.21	(0)	2,500	(0)	73	(1)	0	2,500	(1)	0	1	2.76	(1)	0.04	(0)
Romania	-0.12	(0)	Profit based	(1)	73	(1)	1	13	(0)	0	0	0.04	(0)	0.13	(0)
Slovakia	0.95	(0)	600	(0)	74	(1)	0	664	(1)	0	0	0.02	(0)	-0.06	(0)
Slovenia	0.88	(0)	125	(0)	51	(0)	0	125	(0)	0	0	0.26	(0)	0.32	(1)
Spain	1.29	(1)	Profit-based	(1)	60	(0)	0	600	(0)	0	1	0.10	(0)	0.28	(1)
Sweden	1.69	(1)	No fine	(0)	73	(1)	1	1,000	(1)	1	0	0.29	(0)	0.17	(0)
United Kingdom	1.68	(1)	No limit	(1)	76	(1)	1	No limit	(1)	1	1	0.43	(1)	0.26	(1)

(continued)

Table 4 (continued)

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

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

Panel A: Results for the Market Abuse Directive

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

(continued)

Table 5 (continued)

Panel B: Results for the Transparency Directive

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

The sample comprises 611,969 firm-quarter observations from 25 EU and 27 non-EU countries between the first quarter 2001 and the second quarter 2009. We use the *Bid-Ask Spread* measured as the quarterly median quoted spread as the dependent variable. *MAD* (in Panel A) and *TPD* (in Panel B) are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force. For each model we partition the treatment sample into two distinct groups by interacting the *MAD* and *TPD* variables with a binary indicator variable for the quality of prior regulation (high vs. low) and the strength of MAD or TPD implementation (strong vs. weak). For a description of the country-level partitioning variables see Table 4. In addition, we partition the treatment sample based on the implementation timing of the MAD or TPD. We first distinguish between early and late adoption countries using the median entry-into-force quarter of the respective directive as the cut-off value. In the analyses, we then randomly assign an entry-into-force date from the early implementation countries to the late implementation countries, and compare the liquidity effects across the two groups (early vs. 'as if' early). Throughout the table, we include the full set of control variables and fixed effects in the models, but only report OLS coefficient estimates (t-statistics) for the main variables. We also report p-values from Wald tests assessing the statistical significance of the differences in coefficients across groups. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 6: Contingency Tables of Implementation Timing, Regulation Quality, and Implementation Strength

Market Abuse Directive

Regulatory Quality 2003

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

p-value: 0.191

Maximum Fine

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

p-value: 0.356

Supervisory Powers

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

p-value: 0.812

Action Taken by 2009

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

p-value: 0.573

Transparency Directive

Regulatory Quality 2003

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

p-value: 0.858

Maximum Fine

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

p-value: 0.858

Compliance CESR Std. 1

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

p-value: 0.588

Shift in Enforcement

		Weak (a)	Strong (b)	(a)+(b)
Late	(i)	10	4	14
Early	(ii)	7	8	15
	(i)+(ii)	17	12	29

p-value: 0.176

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

Table 7: Combined Effects of Prior Regulation and Implementation Differences on the Liquidity Benefits of Tighter Securities Regulation

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

The sample comprises 611,969 firm-quarter observations from 25 EU and 27 non-EU countries between the first quarter 2001 and the second quarter 2009. We use the *Bid-Ask Spread* measured as the quarterly median quoted spread as the dependent variable. *MAD* and *TPD* are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force. For each model we partition the treatment sample into four distinct groups by interacting the *MAD* and *TPD* variables with a binary indicator for the quality of prior regulation (high vs. low) and another binary indicator for the strength of MAD or TPD implementation (strong vs. weak). For a description of the country-level partitioning variables see Table 4. Throughout the table, we include the full set of control variables and fixed effects in the models, but only report OLS coefficient estimates (t-statistics) for the main variables. We also report p-values from Wald tests assessing the statistical significance of the difference in coefficients across groups. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 8: Effects of Supervisory Resources on the Liquidity Benefits of Tighter Securities Regulation

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

The sample comprises 611,969 firm-quarter observations from 25 EU and 27 non-EU countries between the first quarter 2001 and the second quarter 2009. We use the *Bid-Ask Spread* measured as the quarterly median quoted spread as the dependent variable. *MAD* and *TPD* are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive and the Transparency Directive came into force. We first partition the treatment sample into two distinct groups by interacting the *MAD* and *TPD* variables with a binary indicator variable for the level of supervisory resources in 2003. Second, we partition the treatment sample into two distinct groups by interacting the *MAD* and *TPD* variables with a binary indicator variable for the change in supervisory resources measured by the percentage *Staff Growth* over the 2004 to 2009 period. In a third model, we partition the treatment sample into four distinct groups by interacting the *MAD* and *TPD* variables with the *Staff Growth* indicator and the binary indicator for the *Staff Level* in the year 2003. For a description of the country-level partitioning variables see Table 4. Throughout the table, we include the full set of control variables and fixed effects in the models, but only report OLS coefficient estimates (t-statistics) for the main variables. We also report p-values from Wald tests assessing the statistical significance of the differences in coefficients across groups. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table B1: Sensitivity Analyses for the Capital-Market Effects of Tighter EU Securities Regulation

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

		Market Abuse Directive		*Transparency Directive*	
	N	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*
(1) Alternative Clustering:					
- Two-Way Clustering by Country and Quarter-Year	611,969	-0.199***	[-5.26]	-0.310**	[-2.20]
- Clustering by Economic Region	611,969	-0.199***	[-3.91]	-0.310**	[-2.06]
(2) Alternative Fixed Effects Structures:					
- Firm-Fixed Effects	611,969	-0.171***	[-4.84]	-0.239*	[-1.89]
- Add Separate Quarter-Year-Fixed Effects for Developed Markets	611,969	-0.161***	[-2.83]	-0.330**	[-2.29]
- Add Separate Quarter-Year-Fixed Effects Interacted with Firm Size	611,969	-0.194***	[-4.09]	-0.318**	[-2.28]
(3) Alternative Sample Specifications:					
- European Union observations only	101,669	-0.196***	[-4.01]	-0.306**	[-2.16]
- Firms Existing before and after the Effective Date of the Directive	594,729	-0.190***	[-4.01]	-0.311**	[-2.19]
- Exclude U.K., Germany, France, and Sweden	555,383	-0.154***	[-2.66]	-0.205**	[-2.40]
- Include Bulgaria and Romania (EU members from 2007 on)	612,647	-0.162***	[-2.65]	-0.304**	[-2.17]
(4) Alternative Control Variables:					
- Controlling for Macroeconomic Factors (Inflation, GDP per Capita, Δ GDP per Capita)	611,818	-0.201***	[-4.03]	-0.311**	[-2.21]
- Not Controlling for Ln(Share Turnover$_{t-4}$)	611,969	-0.185***	[-4.94]	-0.263**	[-2.08]

(continued)

Table B1 (continued)

Panel B: Zero Returns as Dependent Variable

		Market Abuse Directive		*Transparency Directive*	
	N	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*
(1) Alternative Clustering:					
- Two-Way Clustering by Country and Quarter-Year	780,434	-0.046***	[-4.31]	-0.041**	[-2.10]
- Clustering by Economic Region	780,434	-0.046***	[-3.26]	-0.041*	[-1.92]
(2) Alternative Fixed Effects Structures:					
- Firm-Fixed Effects	780,434	-0.047***	[-4.20]	-0.027*	[-1.67]
- Add Separate Quarter-Year-Fixed Effects for Developed Markets	780,434	-0.047***	[-3.70]	-0.038**	[-2.09]
- Add Separate Quarter-Year-Fixed Effects Interacted with Firm Size	780,434	-0.046***	[-3.69]	-0.044**	[-2.24]
(3) Alternative Sample Specifications:					
- European Union observations only	118,907	-0.046***	[-3.51]	-0.046**	[-2.27]
- Firms Existing before and after the Effective Date of the Directive	761,676	-0.044***	[-3.46]	-0.039**	[-2.10]
- Exclude U.K., Germany, France, and Sweden	722,038	-0.042***	[-3.02]	-0.005	[-0.33]
- Include Bulgaria and Romania (EU members from 2007 on)	782,069	-0.052***	[-3.99]	-0.041**	[-2.22]
(4) Alternative Control Variables:					
- Controlling for Macroeconomic Factors (Inflation, GDP per Capita, Δ GDP per Capita)	779,439	-0.046***	[-3.70]	-0.041**	[-2.31]
- Not Controlling for Ln(Share Turnover$_{t-4}$)	780,434	-0.042***	[-3.54]	-0.033*	[-1.81]

Panel C: Alternative Dependent Variables

		Market Abuse Directive		*Transparency Directive*	
Dependent Variables	*N*	*Coefficient*	*t-stat*	*Coefficient*	*t-stat*
- Ln(Bid-Ask Spread$_{PO}$)	607,015	-0.184*	[-1.83]	-0.350**	[-2.24]
- Bid-Ask Spread (without log Transformation)	611,969	-0.004***	[-2.82]	-0.004*	[-1.79]
- Cost of Capital (r_{GLS})	337,466	-0.570**	[-2.22]	-0.198*	[-1.68]
- Dividend Yield	327,387	-0.089	[-1.21]	-0.071	[-1.60]

(continued)

Table B1 (continued)

The sample comprises firm-quarter observations from up to 29 (35) EU (non-EU) countries between the first quarter 2001 and the second quarter 2009. In Panel A, we use the *Bid-Ask Spread* measured as the natural logarithm of the quarterly median quoted spread as the dependent variable. In Panel B, we use the proportion of trading days with *Zero Returns* in a quarter as the dependent variable. Panel C reports results for various alternative dependent variables. *MAD* and *TPD* are binary indicator variables that take on the value of '1' beginning in the quarter when the Market Abuse Directive or the Transparency Directive came into force. We report results for the following specifications: First, we use alternative clustering criteria when computing standard errors. That is, we apply (i) two-way clustering by country and quarter-year, or (ii) clustering by 18 economic regions (e.g., Southern Europe, Central Europe, etc.). Second, we use alternative fixed effects structures. That is, we (i) replace the country- and industry-fixed effects with firm-fixed effects, (ii) add separate quarter-year fixed effects for developed markets, or (iii) add quarter-year fixed effects that are interacted with the *Market Value* of the firm. We identify developed markets based on the Morgan Stanley Capital International database. Third, we (i) estimate the regressions for the EU countries only (treatment sample), (ii) limit the treatment sample to firms with data available in both the year immediately before and after the entry-into-force date of the respective directive, (iii) exclude the four treatment countries with the most observations (U.K., Germany, France, and Sweden), or (iv) include Bulgaria and Romania that did not join the EU until 2007. Fourth, we expand and alter the set of control variables. That is, we (i) control for macroeconomic factors (lagged by one year) by adding inflation, annual GDP per capita, and the percentage change (Δ) in annual GDP per capita, or (ii) exclude share turnover from the set of control variables. In Panel C, we use the following alternative dependent variables: (i) the median quoted spread scaled by the earliest available price for each firm (*Bid-Ask Spread*$_{P0}$), (ii) the percentage *Bid-Ask Spread* without log transformation, (iii) Cost of Capital (r_{GLS}), or (iv) *Dividend Yield*. r_{GLS} is the implied cost of capital estimate based on time-series forecasts of earnings following Hou et al. (2009) and the 12-year version of the Gebhardt et al. (2001) valuation model. We impute r_{GLS} using the market value at the end of each quarter. We measure *Dividend Yield* as the actual dividends paid during the fiscal year divided by the stock price at the end of each quarter. Firms that did not pay dividends are excluded from the analysis. The cost of capital (dividend yield) models include the following control variables: *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. *Return Variability* is the standard deviation of daily stock returns in a calendar year. The dividend yield model also includes *Asset Growth* as the year-to-year percentage change in total assets. We measure accounting data as of the most recent fiscal year before each quarter, and lag all market-based control variables by four quarters (*t*-4). Unless indicated otherwise, we include the full set of control variables and fixed effects from the models in Table 2, but only report OLS coefficient estimates (t-statistics) for the main variables. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).