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The Economics of Disclosure and Financial Reporting Regulation: Evidence and Suggestions for Future Research

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ABSTRACT

This paper discusses the empirical literature on the economic consequences of disclosure and financial reporting regulation, drawing on U.S. and international evidence. Given the policy relevance of research on regulation, we highlight the challenges with (1) quantifying regulatory costs and benefits, (2) measuring disclosure and reporting outcomes, and (3) drawing causal inferences from regulatory studies. Next, we discuss empirical studies that link disclosure and reporting activities to firm-specific and market-wide economic outcomes. Understanding these links is important when evaluating regulation. We then synthesize the empirical evidence on the economic effects of disclosure regulation and reporting standards, including the evidence on International Financial Reporting Standards (IFRS) adoption. Several important conclusions emerge. We generally lack evidence on market-wide effects and externalities from regulation, yet such evidence is central to the

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economic justification of regulation. Moreover, evidence on causal effects of disclosure and reporting regulation is still relatively rare. We also lack evidence on the real effects of such regulation. These limitations provide many research opportunities. We conclude with several specific suggestions for future research.

JEL codes: D78; D82; G14; G18; G30; G38; K22; K42; M41; M42

Keywords: transparency; regulation; accounting standards; capital markets; institutional economics; international accounting; disclosure; IFRS; political economy; cost–benefit analysis; real effects

1. Introduction and Overview

This study reviews the empirical literature on the economic consequences of disclosure and financial reporting *regulation*, summarizing U.S. and international evidence. Our focus on regulation reflects that corporate disclosure and financial reporting are frequently regulated, mandated, or standardized. Therefore, regulation and standardization are core issues for financial accounting. This does not imply that disclosure and financial reporting have to be regulated or could not arise voluntarily. But, undoubtedly, disclosure and reporting regulation is an important and recurring policy issue that deserves significant attention by academic research. Further fueling demand for this research, policy makers, regulators, and standard setters are increasingly asked to conduct cost–benefit (or economic) analyses of intended as well as past regulation and standards.[1]

Three developments that have spurred disclosure and financial reporting regulation around the world make our review timely. First, a series of financial crises and corporate scandals leading to calls for regulatory reform. The Asian financial crisis of 1997, the Enron scandal in the United States, and the financial crisis in 2008 are but a few important examples. In the aftermath of these events, policy makers and regulators enacted significant changes to disclosure and reporting regulation. Second, over the past decade, many countries have adopted International Financial Reporting Standards (IFRS) in an attempt to increase the harmonization and global convergence of accounting rules and reporting standards. Third, recent national debates about the competitiveness of countries' capital markets and the increasing internationalization of capital markets have spurred discussions about reforms to securities and disclosure regulation.[2] These three developments have motivated a large empirical literature, which we review in this paper. As disclosure and reporting regulation has become a global

[1] See, for example, Meeks and Meeks [2001], Schipper [2010], Bartlett [2014], Coates [2014], Cochrane [2014], and Posner and Weyl [2013, 2014].

[2] Examples are the debates in the European Union (e.g., Lamfalussy [2000]) and the United States (e.g., Commission on the Regulation of U.S. Capital Markets in the 21st Century [2007]).

issue, we emphasize international evidence. We also recognize that regulating disclosure and setting accounting standards are intertwined, which is why this review combines evidence on the economic effects of disclosure as well as financial reporting regulation.[3]

While we focus primarily on corporate disclosure and financial reporting, mandated disclosure is used in many other areas, such as product quality, consumer protection, conflicts of interest, environmental policy, health care, etc. In these areas, disclosure mandates are increasingly used in lieu of regulation that explicitly stipulates or prohibits certain behaviors, the idea being that mandated disclosure and transparency incentivize desirable behaviors and discourage undesirable ones. This incentive or governance role of disclosure regulation deserves greater attention—a point that we emphasize in this review. The widespread use of disclosure regulation in many different areas underscores the importance of disclosure and transparency as a research topic that goes beyond corporate reporting. Thus, in our view, understanding the economic effects of disclosure regulation is of first-order importance, not just for accounting and finance.

For the purpose of this review, we deliberately use a broad definition for disclosure and reporting regulation, which includes a central authority formally creating and interpreting disclosure and reporting rules, monitoring compliance with these rules, and enforcing and imposing penalties for deviations from the rules.[4] Disclosure and reporting rules stipulate that firms and hence managers/owners of firms provide certain information to investors, consumers, contracting parties, regulators and government agencies, or the general public.[5] Consistent with this broad definition, we do not review studies on a particular accounting standard or narrow disclosure rule.[6] Instead, we focus on new disclosure mandates (e.g., the Securities Exchange Act), major extensions of the entire disclosure regime

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[3] Other survey papers on the empirical disclosure literature include Healy and Palepu [2001], Core [2001], and Beyer et al. [2010]. Our focus is on regulation and mandated changes in reporting standards.

[4] For example, the Organisation for Economic Co-operation and Development (OECD) defines regulation "as imposition of rules by government, backed by the use of penalties that are intended specifically to modify the economic behavior of individuals and firms in the private sector" (https://stats.oecd.org/glossary/detail.asp?ID=3295). We acknowledge that informal norms and common practices can and do arise without central coordination and mandate. However, for the purposes of this review, the term regulation refers to mandated sets of formal rules and associated penalties that are coordinated and implemented by one or several central authorities, which are not necessarily government authorities, but could also be private standard setters.

[5] The disclosed information is also defined broadly and not confined to reported numbers or footnotes in the financial statements. Our definition of rules encompasses financial reporting and accounting standards. However, we largely exclude work on bank regulation as it is separately discussed in this journal issue by Acharya and Ryan [2016].

[6] Studies on the effects of particular standards have been reviewed in prior surveys (see, e.g., Gonedes and Dopuch [1974], Holthausen and Leftwich [1983], Watts and Zimmerman [1986], Fields, Lys, and Vincent [2001], and Kothari [2001]).

(e.g., Sarbanes–Oxley Act (SOX)), and required changes in the entire set of accounting standards (e.g., mandatory IFRS adoption). However, we acknowledge that the delineation is not always clear-cut. Similarly, we do not review studies on the effects of voluntary disclosure or financial reporting choices in detail. These studies, by their nature, do not speak directly to regulatory consequences or the desirability of regulation. That said, these studies can provide useful evidence on economic links and consequences of disclosure and reporting activities, including externalities. In essence, they contribute an "inventory" of potential economic outcomes, which is useful to consider when evaluating disclosure and reporting regulation. For this reason, we refer to a number of key studies that examine economic outcomes of voluntary disclosures.

Our survey touches on broader issues related to regulation that are also discussed in the economics literature, such as competition, political lobbying, and regulatory capture. However, this literature focuses often on regulation of product-market monopolists and the corresponding impact on consumers (e.g., Kahn [1988], Laffont and Tirole [1993]). Disclosure and reporting settings have their own economic and regulatory issues, and regulated financial reporting has a number of unique features. In fact, one conjecture that we put forward in this review is that disclosure and reporting regulation could even spur competition and innovation, and in that sense, be quite different from product–market regulation.

This review does not provide a summary of the theoretical arguments in favor of and against disclosure regulation (as well as regulation more broadly). They have been summarized elsewhere.[7] Our starting point is that the case in favor or against disclosure and reporting regulation is not ex ante obvious and that the relative magnitudes of various costs and benefits that arise from a mandate are largely an empirical matter. This motivates our focus on empirical studies.

Consistent with the forward-looking theme of this year's *Journal of Accounting Research* Conference, we attempt to identify opportunities for future research throughout this review. In addition, we emphasize issues related to research design and identification for two primary reasons. First, identification and causal inferences are of first-order importance for policy and regulatory debates. Lack of identification generally leads to alternative interpretations, which, in turn, restricts our ability to inform policy makers and regulators. As such, we hope that our research-design focus is useful to regulators and policy makers in evaluating extant research. Moreover, without identification, the magnitude of the estimated effects is of limited relevance, except to gauge the plausibility of findings. This is not to say that

[7] For economic justifications of disclosure and reporting regulation, see Seligman [1983], Coffee [1984], Easterbrook and Fischel [1984], Mahoney [1995], Leuz and Wysocki [2008], Hart [2009], Zingales [2009], Bushman and Landsman [2010], Leuz [2010], and Hermalin and Weisbach [2012].

directional results and associations cannot be informative. But, the magnitude of an effect becomes a critical input into quantitative cost–benefit analyses only if the estimated effect is indeed causal. Second, we hope that our focus on research design provides a guide for *future* empirical research on disclosure and reporting regulation. Many of the points we bring up reflect the collective wisdom of the field and we do not claim that we are the first to identify certain research-design issues or limitations in the prior literature. But, we believe it is nevertheless useful to provide such evaluations of prior studies. We stress that these discussions are not meant as a criticism, but intended to move the field forward.

Having delineated the scope of this review, the remainder of this section presents *five* major conclusions that emerge from it. These conclusions summarize our views on the literature's status quo, as well as broadly identify opportunities for future research. At the end of this review, we additionally discuss numerous specific areas and topics for future research on the economic consequences of financial reporting and disclosure regulation.

Our *first* conclusion is that evidence on the causal effects of disclosure and financial reporting regulation is often difficult to obtain and still relatively rare. Studies often struggle to identify counterfactuals, unaffected control groups, and/or natural experiments that would allow a clean identification of the regulatory effects and their economic consequences. In addition, it is difficult to measure disclosure outcomes (e.g., reporting quality) separately from the underlying economics. This measurement problem limits our ability to produce evidence along the entire "causal path," that is, from regulatory changes to disclosure outcomes to economic consequences. Most studies are reduced form and directly estimate the economic consequences of regulatory changes. Linking disclosure outcomes to the regulatory changes and economic consequences would substantially increase the confidence in the estimates. For most regulatory changes, we are unable to provide causal estimates of their costs and benefits, let alone elasticities for the effects of disclosure and reporting mandates. Thus, while we have a lot of evidence that is *qualitatively* useful, we are still far from being able to perform *quantitative* cost–benefit analyses.

Second, and related to our first conclusion, there is a paucity of evidence on market-wide effects from regulation, especially on externalities. Such evidence is central to the economic justification of regulation in the first place. Regulatory studies document primarily economic consequences for individual firms in a market or an economy. But we lack evidence that reporting standards and disclosure regulation produce information spillovers, externalities, and/or network effects. One reason is that the identification of such indirect effects is arguably even harder than the identification of direct economic consequences on firms or investors. We also have almost no evidence on welfare effects from disclosure and reporting regulation, except perhaps for the corporate sector. Moreover, because

studies typically do not identify or compute counterfactuals, the empirical literature to date has little to say about the efficiency or desirability of existing disclosure and reporting regulation.[8]

Third, the empirical literature exhibits a heavy focus on disclosure regulation in the United States. Each major U.S. regulatory change has been studied extensively. There is much less evidence for major changes in disclosure and reporting regulation in other countries. It is plausible that many basic regulatory tradeoffs are similar across countries and hence many U.S. results may carry over. If so, then there are research-design advantages to studying the effects in one of the largest economies and capital markets with widely available data. But, studying other countries should give us a richer understanding of the many facets of regulatory effects, especially when it comes to interactions among various elements in countries' institutional frameworks. Moreover, novel settings in other countries may permit tighter research designs that are not feasible in a U.S. setting. Thus, we encourage researchers to seek settings outside the United States, be it for better identification or for documenting novel effects. We also encourage researchers to examine nontraditional disclosure and reporting settings, especially to learn about the *real effects* of disclosure mandates.[9] More generally, we conclude that the analysis of real effects deserves more attention.

Fourth, and in contrast to the work on disclosure regulation, there is a huge literature on the effects of reporting standards internationally. The worldwide adoption of IFRS is arguably one of the largest regulatory events in accounting history and not surprisingly has spawned a large literature on the economic consequences of financial reporting standards. Reviewing this literature is a core part of this survey. We conclude that few studies in this literature are able to attribute the documented effects to IFRS adoption, that is, the change in accounting standards. This inability stems from two key problems. IFRS were often adopted amidst a series of other (unrelated) institutional reforms, making it difficult to identify the effects of IFRS adoption separately from other concurrent institutional changes. Moreover, several countries have adopted IFRS together with other changes to the *reporting* infrastructure (e.g., stricter enforcement), often with the intention to support IFRS adoption, which further complicates the identification of IFRS effects. More generally, our review highlights that there are important interactions and complementarities between reporting systems and various institutional factors. These interactions provide major

[8] This holds because outcomes for alternative regulatory (or market-based) regimes that were not implemented cannot be observed and hence we do not know whether these would have provided superior outcomes. To tackle this issue, we need to model and estimate counterfactuals.

[9] We define real effects as situations in which the disclosing person or reporting entity changes its behavior in the real economy (e.g., investment, use of resources, consumption) as a result of the disclosure mandate. Real-effects studies analyze this change in behavior. See also further discussions in sections 3.2 and 4.4. Of course, there can also be real effects from voluntary disclosure, but these are not the focus of this essay.

opportunities for future research, but also pose severe difficulties for identification and economic analysis.

Finally, to make significant progress with respect to the (causal) estimation of regulatory effects and cost–benefit analysis, researchers likely need help from legislators and regulators. For example, one major issue for empirical studies is that most regulation is required as of a particular date, which makes the analysis susceptible to confounding effects, be they other concurrent institutional changes, economic shocks, or market responses to the events that gave rise to the regulation. To mitigate this issue, new regulation could stipulate that rules be implemented in a staggered fashion, which would greatly facilitate ex post economic analysis. A staggered introduction is often viewed as not feasible because it is arbitrary and violates fairness considerations. At the same time, new regulation can have significant costs and unintended consequences. Thus, potentially large societal gains from better economic analysis need to be weighed against the fairness concerns about (by design) arbitrary implementation. The more general point is that, if we want better economic analysis, we need to design regulation with ex post analysis in mind, including provisions for firms and regulators to collect the necessary data. In addition, we need more pilot studies and field experiments, perhaps jointly conducted by regulators and researchers. Such studies would also facilitate ex ante economic analysis and could mitigate the risks of unintended consequences.

The remainder of the survey is organized as follows. Section 2 discusses overarching issues related to cost–benefit analysis, identification, and the measurement of disclosure and reporting outcomes. In section 3, we provide an overview on key economic relations that are particularly relevant and commonly used while evaluating the economic consequences of disclosure and financial reporting regulation. Section 4 reviews empirical studies on major changes in disclosure regulation in the United States and internationally, as well as cross-country comparisons of disclosure regimes. Section 5 discusses evidence on the economic effects of financial reporting standards, primarily from mandatory IFRS adoption. It also reviews evidence on the links between disclosure and reporting regulation and other elements of countries' institutional frameworks. Section 6 concludes the review with an extensive discussion of specific suggestions for future research. Finally, we provide an online appendix with summary tables of studies discussed in the text, as well as relevant studies not explicitly discussed. This appendix is an integral part of this review.

2. Cost–Benefit Analysis, Identification, and Measurement of Disclosure and Reporting

In this section, we discuss three foundational issues that permeate this review. First, we recognize that empirical studies have the potential to inform regulators and policy makers who must consider cost–benefit tradeoffs in evaluating existing and proposed disclosure and reporting regulation. We

therefore discuss a number of challenges in cost–benefit analysis. Second, our understanding of the effects of disclosure and reporting regulation ultimately relies on our ability to draw causal inferences. Causal evidence is central to evidence-based policy making. Thus, we discuss common identification challenges in regulatory studies. Finally, a central issue for the economic analysis of disclosure and reporting regulation is how well we can measure disclosure and reporting activities as well as their properties. We highlight many measurement problems and argue that progress with respect to measurement is critical for tighter identification in regulatory studies.

2.1 COST–BENEFIT ANALYSIS AND EVIDENCE-BASED REGULATION

It has become increasingly common that regulators and standard setters conduct prospective cost–benefit analysis of new regulation as well as postimplementation reviews. While the notion of ex ante and ex post economic analysis of regulation and standards is inherently sensible, it is easier said than done. Our literature review highlights that there is significant risk of an "expectation gap" as to what academic research can actually contribute to cost–benefit analysis. It is unrealistic, at least in the near future, that we will be able to measure and comprehensively evaluate the net benefits of disclosure and reporting regulation to firms and investors, let alone to the economy as a whole (see also Schipper [2010], Coates [2014], Cochrane [2014]). Why is this so difficult?

Naturally, a quantitative cost–benefit analysis requires evidence on causal effects as a critical input. However, such evidence is difficult to generate, especially when it comes to long-run and general equilibrium effects. In many areas of regulation, it is not possible to run long-run, randomized field experiments. It is possible to conduct pilot studies as well as field experiments on particular aspects of a new regulation (see also Floyd and List [2016]). Such evidence would be particularly useful for ex ante economic analysis and for the design of regulation, and it could significantly mitigate the risks of unintended consequences.

In general, however, ex ante and ex post cost–benefit analysis by and large has to rely on empirical studies using archival (or observational) data without the use of randomization. Ideally, such studies would provide quasi-experimental evidence that allows for causal inferences. But, as our review of the literature highlights, we have only a few studies that provide such evidence. While there are opportunities for researchers to improve and tighten their research designs and embrace new econometric methods of estimating treatment effects (e.g., Angrist and Pischke [2009, 2014]), the main limitation is how the underlying data are generated, that is, the institutional settings themselves and the process by which new regulation arises. Our subsequent discussions of the literature on disclosure and reporting regulation (in sections 4 and 5) forcefully illustrate this point, and this is one reason why our review casts the spotlight on research-design issues that come with the institutional settings or regulatory processes.

One might argue that we often have several empirical studies providing consistent results, perhaps even across different settings, and that consistent evidence should make us more confident that certain economic links exist or regulation has certain effects. However, this "piling up" of studies generally does not address the fundamental challenges limiting causal inferences unless the different studies have fairly orthogonal research-design challenges. In our judgment, studies often share fairly similar identification and measurement problems (see section 2.2) and hence different studies do not really "diversify" the research-design problems.[10] Generally speaking, we do not have quasi-random assignment of firms to a treatment group subject to disclosure regulation and an unaffected control group. The identification of externalities and spillover effects, which are critical to the economic justification of regulation, is even harder. Externalities imply that a central assumption of the causal inferences paradigm (Rubin [1978]) no longer holds.[11] Basically, there are direct treatment effects for treated firms as well as potentially indirect effects on treated and untreated firms that arise due to externalities and spillovers. Thus, identification requires an additional control group that is completely unaffected against which the indirect effects of regulation can be benchmarked.

Articulating why a particular setting and design provides proper identification of the economic effects as well as appreciating the potential threats to identification requires a deep understanding of the institutional setting, including the process by which regulation came about and other concurrent institutional changes. The description and study of the institutional setting is, therefore, a critical first step. Given that the features of the institutional settings are often a key limiting factor, there are many opportunities to seek new and nontraditional institutional settings with an eye toward identification. But in this quest for new settings, it is important to recognize that we generally face a tradeoff between internal and external validity. For instance, a setting that provides quasi-random assignment for a particular regulatory change may yield average treatment effects that are fairly local, that is, specific to the setting and hence may not translate to other settings or broader regulatory issues. In fact, one could argue that studying the causal effects for a particular setting often amounts to a case study, at least as far as the magnitude of the estimated treatment effect is concerned. Thus, there is a price that we have to pay for identification.

Despite the aforementioned challenges, academic research can provide many useful inputs into regulatory cost–benefit analysis, especially when the latter takes a more qualitative or less formal approach. Theoretical

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[10] This is also why formal meta-analyses across a set of regulatory studies would likely not be as useful.

[11] The Stable Unit Treatment Value Assumption (SUTVA) maintains that one unit's outcomes are unaffected by another unit's treatment assignment, which is obviously violated in the presence of externalities. In addition, the reflection problem (Manski [1993]) likely arises as noted already in section 3.

research can point to *potential* outcomes of disclosure and reporting regulation, which should underpin any qualitative cost–benefit analysis even if estimating the magnitude of various costs and benefits is difficult. For cost–benefit analysis, it is important to broadly identify potential direct and indirect disclosure costs and benefits to firms, investors, consumers, and the economy as a whole. Especially indirect effects and externalities require substantial attention when justifying the regulatory change, but also to avoid surprises and unintended consequences. For this reason, we briefly take stock of potential effects that have been examined in prior empirical research on disclosure and reporting activities (section 3). Empirical research can play an important role in identifying and quantifying certain economic links as well as regulatory effects, which, in turn, could provide useful inputs when performing cost–benefit analyses. Here, we need both more descriptive studies that highlight potential regulatory effects as well as studies with tight identification that provide causal estimates or even elasticities (e.g., by how much liquidity changes if a mandate improves the quality of information by 1%). In the end, however, cost–benefit analysis still requires the expert judgment of those that have been tasked to make the normative decisions (see also Coates [2014]). Even if we had causal estimates of the costs and benefits, there would still be the questions of how to aggregate them and how to trade off the differential effects on firms, investors, and consumers (e.g., Gonedes and Dopuch [1974]).

However, at this point, we are still far from having such estimates and from performing quantitative cost–benefit analysis. Economic elasticities are very hard to compute (Cochrane [2014]). As noted, the key limitations are (1) the way in which the data are generated institutionally and (2) the lack of data. Thus, to make significant progress with respect to economic analysis, we likely need to design regulation with ex post analysis in mind, which will require the help of regulators and policy makers. Here are several examples illustrating the general idea.[12]

First, new regulation can be implemented in ways that make it more conducive to economic analysis. For instance, having thresholds above or below which the new rules apply as well as a staggered implementation can significantly help the identification of causal estimates, as our discussion in sections 4 and 5 illustrates.[13] Thresholds and, in particular, staggering

[12] See Hahn and Tetlock [2008] for further suggestions on how to improve cost–benefit analysis and a more general discussion of the evidence on the effectiveness of cost–benefit analyses in the United States. For critical reviews of (formal) cost–benefit analysis, see also Schipper [2010], Bartlett [2014], Coates [2014], and Cochrane [2014].

[13] The staggering could be along at least two dimensions. First, components of a "regulatory package" might be implemented in a staggered fashion to facilitate the evaluation of the components. Second, the entire regulatory package could be applied to cohorts of firms in a time-staggered fashion to allow for better identification of regulatory effects using regulated and yet unrelated firms. We note, however, that multiple regulatory events are not a panacea, especially when firms can anticipate regulatory changes, which is more likely if such changes are frequent (Hennessy and Strebulaev [2015]).

often raise fairness concerns. At the same time, much is to be gained from careful ex post analysis of new regulation. Thus, we need to weigh the potential societal gains from better economic analysis against the fairness concerns about (by design) arbitrary implementation. To evaluate this tradeoff, we should study examples in which regulation has been introduced in a staggered fashion or has included thresholds (see discussion in section 4). Abramowicz, Ayers, and Listokin [2011] push even further and argue for randomly assigning individuals, firms, or jurisdictions to different legal rules.

Second, to address the missing data problem, new disclosure and reporting regulation could mandate that firms and/or auditors collect and keep relevant data for ex post analysis. As we discuss in section 2.3, having more granular data about the specific changes in firms' disclosure and reporting activities as a result of the new regulation would allow researchers to perform better economic analyses along the entire causal path, including the mechanism(s). For instance, when implementing a new asset impairment rule, firms could keep data on what the impairment would have been under the old rule, as well as details about the changes made under the new rule. These data would then be made available for economic analysis. As such data could be proprietary, they could be made available to researchers only on a confidential basis, following the model of the U.S. Census Bureau. To reduce the costs from the data collection mandate, it is conceivable that only some firms are ex ante randomly chosen to collect the data (in which case the fairness tradeoff arises again).

Third, we see parallels between the recent push for evidence-based financial regulation and the emergence of evidence-based medicine, which Eddy [1990, p. 1272] describes as "explicitly describing the available evidence that pertains to a policy and tying the policy to evidence." An evidence-based approach to medicine typically involves classifying evidence by its epistemological strength, systematically reviewing and aggregating the evidence (such as the Cochrane Collaboration), and developing practice guidelines and recommendations. Given the parallels, it might be useful for regulators, policy makers, and academics to study the experience in medicine. Importantly, we recognize that the emergence of evidence-based medicine required the creation of an entire "infrastructure" to support the approach (e.g., Eddy [2005]). In accounting, economics, and finance, we are still far from having such an infrastructure and, more generally, from following an evidence-based approach to disclosure and reporting regulation (see also Buijink [2006], Schipper [2010]).

2.2 IDENTIFICATION CHALLENGES FOR DISCLOSURE AND REPORTING REGULATION STUDIES

Regulatory settings have the advantage that disclosure and reporting are *imposed* on firms. Thus, from the perspective of an individual firm, the regulatory regime is exogenously given. We can, therefore, use

differences or changes in disclosure and reporting regulation to estimate economic consequences without the self-selection concern that typically arises in voluntary disclosure settings. Nevertheless, regulatory settings pose several identification challenges for studies estimating the causal effects of disclosure regulation (see also Gow et al. [2016]).

First, new regulation and changes in regulation do not occur in a vacuum. There are typically economic and political reasons for the regulatory changes, which, in turn, lead to selection concerns at the level at which the regulation is imposed (e.g., at the country level). Such reasons do not necessarily imply a selection problem at the firm level. Whatever the reasons, the regulation is still imposed on firms, eliminating or at least mitigating selection at the level of the individual firm. However, the selection concern *at the country level* usually limits the generalizability of the findings to other countries and settings. For instance, a country that decides to impose new disclosure regulation on its firms has likely done so after an explicit or implicit consideration of the costs and benefits. Thus, if a study analyzing this regulatory change documented significant (net) benefits, it is unclear that this result would carry over to other countries that have not imposed such regulation (e.g., the latter may have done so deliberately).[14] Hence, we have to be careful in our interpretation, for example, we cannot advocate for the same change in other countries based on this result.

Second, even though regulation is imposed on firms, selection problems arise when firms can opt out or have ways to avoid the regulation (Heckman [1997]). Examples of such avoidance strategies when it comes to disclosure and reporting regulation are going private, delisting from an exchange, deregistering from the SEC, switching the location of incorporation, etc. The same issue arises when regulation includes a financial threshold and managers have discretion whether or not their firms satisfy the threshold. Thus, it is important for regulatory studies to consider such avoidance strategies as they can systematically bias the estimated regulatory effects.

Third, regulatory changes are often a response to financial or political crises or other major events (e.g., a corporate scandal or bankruptcy). Financial markets also respond to these events. For instance, the Enron scandal reduced trust in financial reporting. It is likely that the ensuing skepticism led to corporate responses attempting to assuage investor concerns (for evidence, see Leuz and Schrand [2009]). The implication is that market reactions to the scandal and the effects of a regulatory change in response to the event are endogenously aligned in time. Thus, it is difficult to empirically disentangle the effects of the market response from the regulatory effects. A study that analyzes the effects around the regulatory change is prone to also pick up the market responses to the scandal (see also Ball

[14] In our minds, there is a subtle distinction between studies that use a regulatory change (or legal shock) to examine the existence of a potential economic link and studies that aim to provide an economic analysis of the regulatory change. For the former, the precise magnitude of the economic link is likely of lesser importance, though not irrelevant.

[1980], Mulherin [2007]). Importantly, this concern is *not* addressed with a difference-in-differences design, unless the control group is subject to the market responses, but *not* subject to the regulation.

Fourth, regulatory changes tend to apply to a larger group of firms at or after a (single) point in time. As a result, the analyses of regulatory studies are susceptible to other institutional changes, general time trends as well as market-wide shocks (e.g., macroeconomic events) that are concurrent with but unrelated to the regulatory change. Difference-in-differences estimation addresses this concern to the extent that the control group is subject to the same concurrent shocks as the treatment group *and* both groups are expected to respond similarly to these concurrent shocks (parallel-trends assumption). If the regulation is implemented in a staggered fashion, that is, phased-in over time, then concurrent events can also be controlled for with appropriate time-fixed effects. Such a staggered implementation arises frequently with disclosure and reporting regulation because of differences in firms' fiscal year ends (e.g., Daske et al. [2008], Christensen, Hail, and Leuz [2013]). However, using the staggered implementation for identification is not a panacea. It is important to ascertain that the effective dates are plausibly exogenous, for example, predetermined or tied to arbitrary characteristics like the ticker symbol. If a firm or country can choose when to adopt or implement the regulation, then economic factors determining this choice are likely to contaminate the estimation of the regulatory effects (and we are back to the selection concern). In addition, spillover effects can be a concern with a staggered implementation. For instance, if investors witness the regulatory effects for firms that adopt first, they might anticipate the effects for later adopters. Depending on the outcome variable of the study, the effects for later adopters can then be muted or no longer present (when measured at the actual implementation date).[15] It would also be a concern if seeing the regulatory effects for early adopters alters the implementation responses of late adopters.

Fifth, capital markets often anticipate regulatory changes, even before the first firms adopt the new rules. In particular, market prices, returns, and the ex ante cost of capital are going to reflect the *expected* regulatory effects, especially after the change has been announced. Thus, the regulation is "priced in" from the time of the announcement, if not earlier, which, in turn, implies that a staggered design will not work with economic outcome variables that are anticipatory in nature (see also Christensen, Hail, and Leuz [2013, 2016]). Similarly, estimated treatment effects are biased when firms can anticipate regulatory changes and hence make adjustments ahead

[15] It is also possible that the regulatory effects depend on the size of the network (e.g., the number of firms that have adopted the regulation). For instance, early adopters could see substantially smaller effects than later adopters. In this case, the observed effects for early adopters likely do not generalize and hence need to be interpreted carefully. The average effect over the entire population of treated firms could still appropriately capture the network effects.

of the mandate. This issue is particularly prevalent when changes occur frequently (Hennessy and Strebulaev [2015]).

Sixth, a regulatory response to a corporate scandal or financial crisis can signal future regulatory actions, including for instance a tougher stance when it comes to enforcement. Similarly, the market response to the announcement of the regulation likely already contains expectations about how the regulation will be implemented and enforced. Moreover, it may include expectations about subsequent regulatory modifications. Such expectations present a major challenge for the ability of return-based event studies to isolate and cleanly measure regulatory effects.[16]

Finally, the effects of regulatory changes likely depend on existing regulation and institutions. For instance, new disclosure regulation or reporting standards need to be enforced and hence are unlikely to be effective without reliable auditing, supervisory agencies, and/or legal remedies. These institutional complementarities can pose major challenges for the estimation of treatment effects, as the observed effects around the regulatory change often reflect joint effects.

The aforementioned challenges are not meant to be comprehensive, but they highlight that it is very difficult to estimate economic effects of disclosure and reporting regulation and to draw causal inferences.[17] We revisit all these challenges in sections 3–5 in the context of specific studies.

2.3 MEASUREMENT OF CORPORATE DISCLOSURE AND REPORTING (QUALITY)

This section discusses the measurement of disclosure and reporting as well as its challenges. Measurement of these activities is obviously critical and, as we explain next, measurement problems are one major reason why we struggle with the (econometric) identification of the economic consequences of disclosure and reporting regulation. First, we briefly review commonly used measures of disclosure and reporting. We start with broader or more comprehensive measures (e.g., a firm's disclosure policy) followed by narrower or more specific measures (e.g., accruals or a specific disclosure item). In principle, more specific (or narrow) measures facilitate consistent measurement across firms and are also more conducive to measuring quality differences. But, with narrower measures, the concern arises that other disclosure activities could serve as a substitute (or a complement). For instance, firms could compensate poor earnings quality with additional disclosures. Thus, without controlling for other disclosure and reporting choices, it is difficult to isolate the effect of the particular measure in question. Broader measures that characterize a disclosure policy or reporting

[16] For the challenges of regulatory *event* studies, see also Gonedes and Dopuch [1974], Foster [1980], Schwert [1981], Holthausen and Leftwich [1983], and Binder [1985].

[17] See also the discussion of research-design challenges for "shock-based research" in Atanasov and Black [2015].

regime are more likely to capture a firm's commitment to a certain level of transparency, that is, a promise to provide certain information, irrespective of its future realizations, and hence are less likely to be influenced by specific realizations (e.g., poor performance in a given year).

Commonly used measures in the broader category are binary indicators of whether a firm publicly provides an annual report; files a 10-K or other disclosure forms with the SEC; and reports financial statements quarterly, reports under IFRS, U.S. GAAP, or a particular reporting regime. Similarly, studies use variables characterizing disclosure policies, for example, whether and how frequently a firm provides management forecasts, hosts conference calls, issues press releases, etc. Generally, these variables are focused on the existence of a certain disclosure (policy). As such, they can be precisely measured, but they primarily capture the quantity, rather than the quality, of the information provided.

Another widely used group of measures in the broader category are disclosure indices. The best known but, at this point, dated measure is the Association for Investment Management and Research (AIMR) rankings, which are based on annual surveys of financial analysts asking them to rank U.S. firms with respect to their disclosure activities (e.g., Lang and Lundholm [1993, 1996], Welker [1995], Healy, Hutton, and Palepu [1999], Nagar, Nanda, and Wysocki [2003]). These rankings arguably reflect the usefulness of firms' disclosures to expert users of this information and hence capture both quantity and quality aspects. The rankings cover a broad range of disclosure activities including annual report information, voluntary disclosures, and investor relations activities, but they are available only for large U.S. firms and for a limited time period. A concern with the rankings is that they not only reflect the usefulness of firms' disclosures, but also reflect sell-side analysts' objectives.[18]

Other studies use (self-constructed) disclosure indices that are generally based on a checklist of corporate disclosure activities (e.g., Botosan [1997], Hail [2002], Francis, Khurana, and Pereira [2005]). Similarly, international studies often rely on the CIFAR index or the S&P Disclosure score, which are constructed from annual report and disclosure checklists. These are available for large firms across a number of countries and often averaged at the country level (e.g., La Porta et al. [1998], Hope [2003], Leuz, Nanda, and Wysocki [2003], Khanna, Palepu, and Srinivasan [2004], Doidge, Karolyi, and Stulz [2007]). These disclosure indices have several limitations: the selection and coding of relevant disclosures are subjective, the construction of an index assigns (equal or subjective) weights to disclosure items that likely differ substantially in their importance and informativeness, and the additive construction does not account for the possibility

[18] There are concerns that sell-side analysts simply assign higher ratings to firms with better prospects and financial performance. Consistent with this concern, Lang and Lundholm [1993] find that AIMR rankings are strongly correlated with past performance. Healy and Palepu [2001] identify additional limitations of the AIMR data.

that some items are complements and others are substitutes. Again, the indices generally capture the existence of particular disclosures, rather than their quality.

By focusing on specific disclosures, it is often possible to construct measures that have a quality (or informativeness) dimension. For instance, for managerial earnings forecasts, we can measure precision and bias (see Hirst, Koonce, and Venkataraman [2008] for a review). For segment disclosures, we could construct measures for the granularity of the disclosures using the number and relative size of the segments that are broken out as well as the number of line items per segment. Many important disclosures such as the Management Discussion & Analysis, 10-K footnote disclosures, and conference call transcripts are qualitative, text-based, and narrative in nature, which previously made it difficult to use them. However, recent advances in text analysis, computational linguistics, and natural language processing allow us to construct new measures for narrative disclosures, some of which have quality dimensions (e.g., Li [2008, 2010], Loughran and McDonald [2011], Lang and Stice-Lawrence [2014]). Text-based proxies can be applied broadly (e.g., to the entire 10-K) or more narrowly (e.g., to an earnings announcement or a particular part of the 10-K), so the earlier discussion of the tradeoffs between narrower and broader measures applies here as well. As text-based measures are fairly new, there are still substantial debates about what the proxies capture and how well they work in empirical studies.[19]

Probably the most frequently used measures are based on firms' reported earnings and hence fall into the narrower category.[20] Earnings management and accrual-based proxies have been used for three decades starting with Healy [1985], Jones [1991], and the modified Jones model introduced by Dechow, Sloan, and Sweeney [1995]. A more recent and also widely used model for accruals quality has been advanced by Dechow and Dichev [2002] and subsequently been extended by McNichols [2002]. In addition, there are numerous other proxies based on the properties of reported earnings, including timely loss recognition and conservatism (e.g., Basu [1997], Ball, Kothari, and Robin [2000]), earnings smoothing (e.g., Ronen and Sadan [1975], Leuz, Nanda, and Wysocki [2003], Francis et al. [2004]), earnings persistence (e.g., Penman [2001], Dechow and Dichev [2002], Francis et al. [2004]), and the value relevance of earnings (Collins, Maydew, and Weiss [1997], Francis and Schipper [1999]). All these proxies capture important positive and negative aspects of firms' reported earnings, but these proxies also suffer from many conceptual and measurement problems (see Dechow, Ge, and Schrand [2010] for an extensive discussion).

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[19] See also Das [2014] and Loughran and McDonald [2014, 2016].

[20] See Healy and Wahlen [1999], Dechow and Skinner [2000], and Dechow, Ge, and Schrand [2010] for extensive reviews of this literature.

International studies often aggregate several of the aforementioned proxies into a combined measure (e.g., Lang, Smith Raedy, and Yetman [2003], Leuz, Nanda, and Wysocki [2003], Burgstahler, Hail, and Leuz [2006], Lang, Smith Raedy, and Wilson [2006]). By aggregating across measures, these studies attempt to obtain a less specific (or summary) measure of reporting quality and also to address concerns about measurement error. However, as with the construction of disclosure indices, there is the question of how to weight the individual earnings properties and the issue that tradeoffs among the various properties (e.g., substitutional relations) are ignored. We also have to be careful that the combined proxies measure the same underlying construct. Moreover, if all proxies suffer from measurement problems for the same or similar reasons, it is not clear that combining measures helps. At this point, we have little evidence that the combined measures are indeed superior and that aggregation reduces measurement error.

Having briefly discussed the array of commonly used measures, we conclude this section by discussing a fundamental problem that essentially all proxies for corporate disclosure and reporting share: They need to separate a firm's economic situation and business model from the *representation* of these fundamentals. By their very nature, proxies for earnings and accruals quality are inherently connected to a firm's economic characteristics and performance because the accounting system measures economic performance (see, e.g., Kothari, Leone, and Wasley [2005], Hribar and Nichols [2007], Cohen [2008]). Therefore, separating a firm's reporting from the underlying economics (or alternatively controlling for the economics) is very difficult. Illustrating that accrual proxies reflect primarily the underlying economics rather than reporting quality, Francis, Khurana, and Pereira [2005] find that economic factors (which determine "innate" accrual properties) dominate the association between accruals quality proxies and the cost of capital. The separation problem has plagued all measures of reporting and accruals quality, earnings management proxies, as well as other reporting properties such as conservatism (see also Dechow, Ge, and Schrand [2010]). Importantly, the same issues arise with narrative or text-based disclosure proxies that describe a firm's economic performance as well as with specific disclosures (e.g., about fair value estimates) that also reflect a particular business model or economic situation.

Making matters worse, managers may endogenously respond to performance shocks by manipulating disclosures and reported numbers, which creates the additional issue of distinguishing between the properties of manipulated and "neutral" or "normal" earnings (e.g., Wysocki [2008], Gerakos and Kovrijnykh [2013]). Furthermore, managers are likely to engage in obfuscation to hide their manipulation and to mimic the properties of normal earnings or financial reports.

Based on our current state of the literature, it is fair to state that, essentially, all commonly used proxies for disclosure and reporting are likely to comingle the firm's underlying economics with the reporting (quality) constructs that they are trying to measure. Accounting research has

not yet found a satisfactory way to empirically *identify* reporting quality.[21] A potentially promising approach is to impose more structure on the problem. Recent attempts in this direction are Gerakos and Kovrijnykh [2013], Zakolyukina [2013], and Nikolaev [2014]. Structural approaches might be particularly suited for this problem because the accounting system naturally provides structure that can be exploited for identification. An example of a study using accounting structure is Nikolaev [2014]. He uses simple relations between accruals and cash flows as well as the fact that accruals reverse to identify accounting quality in a Generalized Method of Moments (GMM) estimation framework. Another approach is to use quasi-experimental methods, such as instrumental-variable estimation and regression-discontinuity analysis. The latter approach requires that researchers find instruments or situations in which reporting (quality) changes, but a firm's underlying economics (e.g., performance) are plausibly unchanged. Such situations (or instruments) are difficult to find, but it is not impossible. Similarly, mandatory disclosure and reporting changes in disclosure could be exploited to serve this purpose.

Partly in response to the described measurement problems, empirical studies often directly estimate the capital-market consequences of regulatory changes in disclosure and reporting, rather than proceeding in two steps by first linking a new reporting mandate to reporting changes and then linking the observed reporting changes to capital-market consequences. The former reduced-form approach sidesteps the measurement of disclosure and reporting changes, but it makes the analysis susceptible to the influence of concurrent but unrelated shocks that directly affect the capital-market variables. In light of the identification challenges discussed in the previous section, providing results along the entire causal path and proceeding in two steps could have its advantages. It would mitigate concerns about unrelated concurrent shocks that directly affect capital-market outcomes.[22] It would provide evidence that a regulatory change indeed operates through disclosure and reporting changes and allow us to explore the mechanism(s). It is for these reasons that we need more research on the measurement of disclosure and reporting activities, which, in turn, would substantially aid empirical studies on disclosure and reporting regulation.

3. Empirical Evidence from Disclosure Studies

In this section, we enumerate an "inventory" of potential economic effects and outcomes associated with firms' disclosure and financial reporting

[21] Conceptually, *identification* implies a clean separation of the accounting properties from the underlying economics.

[22] To be clear, the first stage would in essence provide a falsification test. That is, finding no correlation between the regulatory change and reporting outcomes would make the reduced-form result for capital-market outcomes suspect. Causal estimates at the second stage still require an exclusion assumption (just like instrumental-variable estimation).

activities. These effects and outcomes are relevant when evaluating disclosure and reporting regulation. In online appendix table 3.1, we tabulate a large array of effects that have been investigated empirically. In the main text, we discuss a few key economic links as examples, primarily to give the reader a sense of the progress and challenges in establishing these links, which also point to opportunities for future research.

We view this stock-taking of potential effects as a necessary first step in an economic analysis of disclosure and reporting regulation, especially considering that it is generally not feasible to directly measure and analyze aggregate welfare effects of past or intended regulation. While we are still far from quantitative cost–benefit analyses of disclosure and reporting regulation, as discussed in section 2, the identification and assessment of potential costs and benefits can nevertheless help inform debates about existing and potential future regulation, as well as more qualitative cost–benefit analysis.

With this goal in mind, we highlight several firm-specific (micro-level) and market-wide (macro-level) effects of firms' disclosure and reporting activities in the literature. This overview complements prior surveys of the empirical disclosure literature (e.g., Core [2001], Healy and Palepu [2001], Beyer et al. [2010], Lang and Maffett [2011]). Our focus, however, is on how this evidence can advance our understanding of the economic consequences of *regulated* disclosure and reporting, which we discuss subsequently in sections 4 and 5.

3.1 COMMENTS ON THE ROLE OF VOLUNTARY DISCLOSURE STUDIES FOR REGULATORY ANALYSIS

Many empirical studies in accounting examine the economic consequences of corporate disclosure and reporting based on associations with firms' *voluntary* disclosure and reporting choices. As these studies often suggest that the documented results are of interest to policy makers and regulators, we first want to clarify the role of these studies for regulatory analysis.

In general, studies using firm-level variation in disclosure and reporting that largely reflects firms' choices cannot be used to justify the desirability or need for mandated disclosure, even if they document substantial (net) benefits from disclosure. The reason is that, in this case, firms would already have incentives to provide the information voluntarily (e.g., Ross [1979]). That is, precisely in situations in which firms' benefits from disclosure exceed their costs, we do not need regulation. Therefore, one economic justification for regulation is that the social value of the disclosed information exceeds its private value to firms, in which case there is too little disclosure from a societal perspective due to the existence of positive externalities. Other justifications are that disclosure regulation (1) produces market-wide cost savings, (2) provides strict sanctions that are not available privately, and (3) mitigates dead-weight costs from fraud and agency conflicts.[23]

[23] For a more detailed discussion of these three arguments, see Leuz and Wysocki [2008]. Similar arguments have been used in many other regulatory contexts. Hermalin and Katz

However, voluntary disclosure and reporting studies generally do not provide evidence on the discrepancy between the private and social benefits of information or the existence of externalities. The focus tends to be on the firm-specific benefits or costs. However, such evidence at best represents a treatment effect on the treated, rather than the average treatment effect, which is what would be most relevant to policy makers and regulators.

Therefore, the primary role of studies using variation from firms' choices is to illustrate the existence of certain costs and benefits from corporate disclosure and reporting activities. That is, they can help to establish relevant economic links. Specifically, these studies can inform regulatory analyses and debates in that (1) they pinpoint key costs and benefits of disclosure and financial reporting, which, in turn, should be considered when evaluating mandates of these activities; (2) they inform us about *differential* costs and benefits to firms, which can help us understand how a mandate may differentially affect firms (including potential wealth transfers among firms); (3) they help us predict which firms may take avoidance actions or lobby for or against a proposed regulation given the differential effects on firms and the wealth transfers between them; and (4) they can illustrate the existence of spillovers and externalities because, if they exist, they should arise also from voluntary disclosures and reporting. For these reasons, we discuss several studies on the economic consequences of voluntary disclosure and reporting activities next.

But, even if we use voluntary disclosure and reporting studies merely to establish the existence of an economic link, it is important to recognize that by their very nature these studies face a selection problem, which poses major challenges to identification and hence makes these studies prone to spurious effects.[24] As an illustration, consider the selection issues that arise in a study that examines the association between firms' disclosure choices and their cost of capital. First, firms are likely to raise external capital when they experience shocks to their growth opportunities. But, they likely also increase disclosure when raising capital. New opportunities often change business risk and hence the cost of capital. Thus, one can easily obtain an association between disclosure and the cost of capital, even when the

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[1993] show in a general bargaining context that there are only three reasons for outside interference with private contracting: (1) the parties are asymmetrically informed ex ante, (2) there is an externality on a third party, and (3) the state has access to more remedies than private parties. See also Hart [2009] and the references in footnote 7 for extensive discussions of the potential economic reasons for or against disclosure regulation. We take these arguments as our starting point and focus on the empirical evidence.

[24] A second issue with studies using cross-sectional variation in (voluntary) disclosures, particularly by U.S. firms, is that the economic significance of this variation could be rather small, especially when considering that the U.S. disclosure and reporting system already mandates a rich set of disclosures. Moreover, this variation is less likely to capture major differences in the commitment to disclosure (see also Leuz and Verrecchia [2000]). But, it is the latter for which theory predicts a reduction in information asymmetry and adverse selection in capital markets.

economic link (or causal relation) does not exist. Second, assuming disclosure does indeed reduce the cost of capital as hypothesized, we would expect firms to respond with disclosure when there are cost-of-capital shocks (Leuz and Schrand [2009], Clinch and Verrecchia [2014]). Thus, in a cross-sectional study, one easily obtains a positive, rather than negative relation between disclosure and the cost of capital. Third, firms are likely to change disclosures in response to performance. For example, managers provide more information to explain poor performance. If performance changes also directly affect the cost of capital, then at least part of the estimated relation is again spurious. Adding to this concern is that, as discussed in section 2.3, commonly used measures comingle corporate disclosure and reporting quality and the underlying economics.

Given the above discussion, it is worth noting that, in principle, an analysis of externalities and spillover effects based on voluntary disclosures should be less prone to selection. We expect firms to base their decisions on the private costs and benefits, not the externalities, of their decisions. If that is true, then externalities and spillovers are essentially an unintended (rather than a selected) outcome. Put differently, even if a firm (optimally) chooses to provide a certain disclosure, we can study the effects of this disclosure on other firms. The concern in such a study is not selection or endogeneity of the decision but that the economic conditions determining the firm's disclosure decision also apply to other firms (e.g., industry peers) and that they drive the observed effects for other firms. This concern is essentially a variant of the reflection problem (Manski [1993]).

3.2 FIRM-SPECIFIC BENEFITS FROM DISCLOSURE AND REPORTING

In this subsection, we discuss empirical studies that identify possible benefits of improved corporate disclosure and reporting. We distinguish between capital-market effects and real effects. We define real effects as situations in which the disclosing manager or reporting entity changes its behavior in the real economy (e.g., investment, use of resources, consumption). Thus, the focus is on the nonreporting behavior of the sender of the information. Capital-market effects arise from the behavior of the information receiver, such as investors and financial analysts. The two effects are connected because capital-market effects (or responses of the receiver) are generally the reason the disclosure feeds back to the real actions of the sender (see also Kanodia and Sapra [2016]).

In online appendix table 3.1, we identify major categories of economic benefits of improved disclosure and note the commonly documented direction of the relation or results. The categories are market liquidity, cost of capital, Tobin's *Q,* investors' portfolio allocations, analysts and broader information environment, capital raising and structure, and investment behavior.

3.2.1. Capital-Market Benefits of Corporate Disclosure and Reporting. In its search for potential capital-market benefits from disclosure and

reporting, the empirical literature has concentrated heavily on two economic constructs, namely, market liquidity and the cost of capital. One reason is that, for both constructs, there are relatively clear and direct theoretical links. We therefore focus our discussion on studies related to these two outcomes (or benefits) and summarize key studies in online appendix table 3.2. There are other capital-market benefits that more indirectly relate to firms' capital-market access and the broader information environment, including investor participation, institutional investor activity, and analyst following. We do not discuss them in the main text as they face similar challenges to liquidity or cost-of-capital studies, though the theoretical links are less clear. Key studies in this category are summarized in online appendix table 3.3.

Arguably, the firm-specific benefit of disclosure best supported by theory is the effect on market liquidity. At its core is the insight that information asymmetries among investors introduce adverse selection into share markets (e.g., Copeland and Galai [1983], Glosten and Milgrom [1985], Kyle [1985], Easley and O'Hara [1987], Admati and Pfleiderer [1988]). Uninformed or less-informed investors have to worry about trading with privately or better informed investors.[25] As a result, uninformed investors either price-protect or exit the market to minimize possible losses from trading with informed counterparties. These actions reduce the liquidity of share markets, that is, ability to quickly buy or sell shares at low cost and with little price impact. Corporate disclosure and reporting can mitigate the adverse-selection problem and increase market liquidity by leveling the playing field among investors (Verrecchia [2001]).

Consistent with this prediction, the empirical literature using voluntary disclosures generally documents a positive association between better disclosure and liquidity (e.g., Welker [1995], Healy, Hutton, and Palepu [1999], Leuz and Verrecchia [2000], Heflin, Shaw, and Wild [2005], Brown and Hillegeist [2007]). In addition, survey evidence suggests that managers believe that such liquidity benefits exist (Graham, Harvey, and Rajgopal [2005]).[26] More recent work takes the basic relation as given and focuses (1) on channels or mechanisms for the relation and (2) on other attributes of liquidity in order to gain a deeper understanding of how disclosure affects information asymmetry and liquidity.

The studies in (1) suggest more nuanced relations; for instance, better disclosures reduce information asymmetry, but also induce some liquidity suppliers to exit the market when they have fewer opportunities to transact on private information (Heflin, Shaw, and Wild [2005]). These studies also

[25] In essence, an uninformed investor fears that an informed investor is willing to sell (buy) at the market price only because the price is currently too high (too low) relative to the information possessed by the informed trader.

[26] They find that 44% of the managers strongly agree with the statement that "voluntarily communicating information increases the overall liquidity of our stock" (and only 17% of managers strongly disagree with the statement).

shed light on the channels for the liquidity effect of disclosure, for instance, showing that the liquidity benefit of disclosure arises primarily because disclosure reduces the likelihood that investors discover and trade on private information (Brown and Hillegeist [2007]). In our view, more evidence that disclosure reduces the nonproductive search costs, as predicted in Verrecchia [1982] and Diamond [1985], would be very useful for a welfare analysis of disclosure regulation.

The studies in (2) show that disclosure is also associated with other liquidity attributes. For instance, Lang and Maffett [2011] find that more transparent firms (measured in a number of ways) have lower liquidity volatility, fewer extreme illiquidity events, and a lower association between own firm-level liquidity and market liquidity. These results are more pronounced during crises. Similarly, Ng [2011] suggests that better reporting is negatively associated with liquidity risk, defined by Pástor and Stambaugh [2003] as the sensitivity of stock returns to unexpected changes in market liquidity. Ng [2011] also finds that the negative association between reporting quality and liquidity risk is stronger in times of large market liquidity shocks. In our view, the suggestion that the benefits of transparency are largest in financial crises is worth exploring more as it could play an important role in the economic justification of disclosure regulation (see also section 6).

As discussed in section 3.1, a major problem in voluntary disclosure studies is the selection problem. Many studies recognize this problem and attempt to estimate the relation with some correction for self-selection (e.g., Welker [1995], Leuz and Verrecchia [2000], Brown and Hillegeist [2007]). However, finding valid instruments to implement selection models and instrumental-variable regressions is very difficult and these studies generally provide little discussion explaining why their instruments satisfy the exclusion restriction.[27] Thus, their evidence should probably not be interpreted in a causal way. To strengthen evidence of a causal link, Balakrishnan et al. [2014] exploit an exogenous shock to the information environment, that is, the loss of an analyst, stemming from brokerage house closures and mergers after Regulation Fair Disclosure (Reg FD). They document that firms respond to a loss in analyst following with more management forecasts, and then show in an instrumental-variable regression that, for those firms that increase their disclosures, there is an increase in market liquidity. Such evidence is an important step forward, but we probably need further studies to cement the relation, as Balakrishnan et al. [2014] finds evidence only along the intensive margin, that is, for firms that have provided managerial forecasts in the past, and only for one particular disclosure. Thus, causal evidence from voluntary disclosure studies is still rare. But, the causal link between disclosure and market liquidity is also corroborated by studies

[27] As Gow, Larcker, and Reiss [2016] highlight, there is still too little discussion even in recent accounting studies.

that exploit mandated disclosure and reporting changes and hence do not face selection at the firm level (see section 4).

The other commonly analyzed economic link is how disclosure and reporting relates to firms' cost of capital. Empirical studies on this link have been guided and motivated by several theories. First, the aforementioned link between disclosure and liquidity could manifest in a cost-of-capital effect. Illiquidity and bid–ask spreads essentially impose trading costs on investors, for which they expect to be compensated in equilibrium (e.g., Amihud and Mendelson [1986], Constantinides [1986]). Moreover, adverse-selection problems in secondary markets fold back to the point at which the firm issues shares. Investors anticipate that they will face price protection in the future and hence reduce the price at which they are willing to buy shares in the initial securities offering (Baiman and Verrecchia [1996]). Second, more disclosure and better reporting can improve risk sharing in the economy and hence reduce the market-risk premium. These effects could arise when (some) investors are not aware of all firms in the economy (Merton [1987]) and when large and less risk-averse investors are unwilling to hold shares due to adverse-selection concerns (Diamond and Verrecchia [1991]). Finally, there is a direct link between disclosure and the cost of capital (or expected returns) arising from estimation risk (e.g., Brown [1979], Barry and Brown [1984, 1985], Coles and Loewenstein [1988], Jorgensen and Kirschenheiter [2003], Hughes, Liu, and Liu [2007], Lambert, Leuz, and Verrecchia [2007, 2012]). It is also conceivable that there exists a nondiversifiable information risk factor in returns, though we currently lack theoretical support for such a factor.

Overall, the empirical literature analyzing the cost-of-capital effects of disclosure and financial reporting is more mixed than the evidence for market liquidity (see online appendix table 3.2). Several studies document a negative association between disclosure and the cost-of-equity capital, but the relation is often only present for certain groups of firms (e.g., Botosan [1997]) and certain types of disclosures (e.g., Botosan and Plumlee [2002]). The evidence is also mixed when studies examine the relation outside the United States in weaker disclosure environments (e.g., Hail [2002], Daske [2006]), which likely increase the power of voluntary disclosure studies. In addition, there is evidence showing associations between certain properties of firms' reported numbers (e.g., accruals quality, earnings volatility, or smoothing) and the cost-of-equity capital (e.g., Francis et al. [2004], Verdi [2006]). As discussed in section 2.3, it is difficult to separate attributes of reporting (such as accruals quality) from firms' underlying economics (e.g., operating volatility). Thus, it is possible that these studies do not illustrate the effects of accruals quality or earnings smoothing, but instead reflect differences in firms' operating and economic risks.

We suspect that one potential reason for the mixed cost-of-capital evidence is again the selection problem, which arises in all the aforementioned studies (see also Nikolaev and van Lent [2005]). Several but not all studies attempt to address this problem by estimating selection models or

instrumental-variable regressions. But, as mentioned before, it is difficult to find proper instruments and to satisfy the exclusion restriction in these settings. Larcker and Rusticus [2010] illustrate this problem and the effects of instrument choice on the results in this context. They conclude that, in their setting, there is no evidence that disclosure quality has an association with the cost of capital.

Motivated by the selection problem, Leuz and Schrand [2009] exploit the Enron scandal in 2001 as an exogenous shock to the perceived precision and credibility of U.S. corporate reporting. They show that, in response to this shock, firms increase their disclosures and that these (plausibly exogenous) disclosure changes are, in turn, associated with subsequent declines in the cost of capital, as measured by beta and another proxy for estimation risk. The link between disclosure and the cost of capital is also further corroborated by studies exploiting mandatory changes in disclosure and reporting (e.g., Shroff et al. [2013]). They are discussed in sections 4 and 5.

Similar to the work on liquidity, there are studies that focus on the potential mechanisms linking disclosure and the cost of capital. Using path analysis, Bhattacharya et al. [2012] find associations consistent with a direct path from earnings quality to the cost of equity, and an indirect path that is mediated by information asymmetry. Similarly, Lang, Lins, and Maffett [2012] use mediation analysis to illustrate that liquidity is an important mechanism through which disclosure increases firm valuation (Tobin's *Q*) and also lowers the cost of capital. However, these studies do not establish causality for the mechanisms, which presents an opportunity for future research.

In addition, there are a number of studies examining the relation between disclosure and firms' external capital raising activities, rather than the cost of capital per se. Studies generally find a positive link between capital-raising activities and disclosure quantity and quality (e.g., Frankel, McNichols, and Wilson [1995], Healy, Hutton, and Palepu [1999], Lang and Lundholm [2000], Shroff et al. [2013]). There are also studies that document that more extensive pre-IPO disclosures are associated with lower underpricing, which is a cost of raising capital (e.g., Schrand and Verrecchia [2005], Leone, Rock, and Willenborg [2007]).

Finally, there is a growing literature that investigates to what extent firms' reporting quality represents a nondiversifiable information risk factor that is priced in returns. As noted earlier, there is not much theory supporting such a risk factor. Standard asset pricing models imply a relation with firms' beta factor (e.g., Lambert, Leuz, and Verrecchia [2007]). That said, several studies document a negative relation between reporting quality proxies and firms' cost of equity (e.g., Aboody, Hughes, and Liu [2005], Francis, Khurana, and Pereira [2005], Ecker et al. [2006], Kim and Qi [2010], Ogneva [2012], Barth, Konchitchki, and Landsman [2013]). The evidence has been controversial, although more recent work is generally supportive of a negative relation even using two-stage cross-sectional regressions (Core,

Guay, and Verdi [2008], Kim and Qi [2010], Ogneva [2012]). However, it is important to note that the aforementioned studies do not use exogenous reporting variation. Moreover, commonly used proxies for reporting quality are prone to pick up differences in the underlying economics and could also reflect measurement error in other risk factors, in particular beta. We are not aware of a study that addresses these concerns.

While most studies have focused on the cost of equity, there are also studies on the cost of debt (e.g., Sengupta [1998], Nikolaev and van Lent [2005]). For instance, Miller and Puthenpurackal [2002] find that U.S. debtholders demand economically significant premiums for bonds of foreign firms that have no prior history of ongoing disclosure. Zhang [2008] finds that lenders offer lower interest rates to firms that report conservative earnings numbers.[28] Studies on the cost of debt face the same selection and endogeneity concerns as cost-of-equity studies. A potential advantage of the debt studies is that the cost of debt can be measured more directly. However, a major difficulty with tests involving the cost of debt is to control for default risks as well as the specifics of firms' debt contracts, in particular covenants, and their impact on the cost of debt. Thus, it is a priori not obvious that it is better to focus on the cost of debt, rather than the cost of equity.

In sum, the evidence on the relation between disclosure and reporting and the cost of capital is fairly mixed and still evolving. The empirical results appear to be sensitive to a number of factors, including the cost-of-capital measures (i.e., realized returns vs. ex ante cost of capital), firm size, and the types of disclosures or earnings attributes (see online appendix table 3.2).

3.2.2 Real Effects of Corporate Disclosure and Reporting. A potentially important effect of disclosure and reporting is that they feed back into corporate decision-making and have real effects (as defined earlier). In this subsection, we discuss such real effects and summarize key studies in online appendix table 3.4. Overall, there is relatively sparse empirical evidence on real effects of disclosure and reporting activities, mostly with respect to corporate investment. However, the analytical literature suggests that real effects should be fairly common (see Kanodia and Sapra [2016]). Thus, we need more empirical research on the prevalence and magnitude of real effects with respect to corporate investment and other real economy actions.

One possible channel for real effects of disclosure is that better disclosure and reporting reduces information asymmetries that otherwise give rise to frictions in raising external capital for investment. The idea is that better reporting improves monitoring by outside parties, such as institutional investors and analysts, which, in turn, can reduce inefficiencies in managerial decisions (e.g., Bushman and Smith [2001], Lombardo and Pagano [2002],

[28] This evidence contrasts with evidence in Francis et al. [2004] suggesting that conservative earnings properties are not a primary factor in determining cost of equity.

Lambert, Leuz, and Verrecchia [2007]). The literature on the effects of reporting quality on corporate investment decisions is still in its early stages, but there are a number of studies suggesting that better reporting is associated with higher investment efficiency (e.g., Bens and Monahan [2004], Biddle and Hilary [2006], Bushman, Engel, and Smith [2006], Biddle, Hilary, and Verdi [2009], Cheng, Dhaliwal, and Zhang [2011], Badertscher, Shroff, and White [2013], Chen, Young, and Zhuang [2013], Goodman et al. [2014]). Jung, Lee, and Weber [2014] examine effects on the efficiency of labor investments and document a positive association. There is also related work suggesting that financial misreporting by one firm can lead to inefficient investment decisions for competing firms. We discuss such peer effects in section 3.4.

As with the capital-market studies, most real-effects studies use cross-sectional variation in (voluntary) disclosure and reporting to estimate the links with investment efficiency. Thus, these studies face the selection problem that we discussed earlier. To mitigate this problem, Cho [2015] exploits improvements in mandated segment reporting to analyze real effects on firms' internal capital allocation decisions. He finds that firms with improved segment reporting allocate capital to segments in a way that is more in line with segment opportunities, consistent with the notion that high-quality disclosures improve external monitoring and therefore investment efficiency. A nice feature of this study and setting is that it uses segment reporting under the new standard even for the preadoption period, essentially exploiting restated reports for the prior year. Nevertheless, real-effects studies face a number of challenges, including difficulties in separating capital market and real effects and the measurement of investment efficiency. We discuss these challenges in more detail in section 4.4. Overall, there are still many opportunities for future research on real effects.

3.3 FIRM-SPECIFIC COSTS OF CORPORATE DISCLOSURES AND REPORTING

In this subsection, we highlight various studies that identify possible costs of increased disclosure and better reporting. We identify several categories of possible economic costs and mitigating effects: litigation risk, proprietary costs, competition, and other disclosure costs. In online appendix table 3.4, we summarize studies for each category. Next, we discuss a few examples only.

The direct costs of corporate disclosures, including the preparation, certification, and dissemination of accounting reports, are conceptually straightforward. However, as the debate about the costs of SOX implementation suggests (e.g., Ribstein [2005], Coates and Srinivasan [2014]), these direct costs could be substantial, especially when considering managerial opportunity costs. The latter are particularly hard to quantify. Moreover, due to fixed disclosure costs, certain disclosures can be particularly burdensome for smaller firms. Empirical studies repeatedly find that larger firms provide more and better disclosures (e.g., Lang and Lundholm [1993]), which is consistent with fixed costs from information production and dissemination, resulting in economies of scale. Overall, however, there is a

general paucity of academic evidence that would allow us to quantify the *direct* costs and out-of-pocket expenses of firms' disclosure and reporting practices.

Disclosures can also have indirect costs because information provided to capital-market participants can also be used by other parties (e.g., competitors, labor unions, regulators, and tax authorities). For example, detailed information about line-of-business or product profitability can reveal proprietary information to competitors and hence cause proprietary costs (e.g., Feltham, Gigler, and Hughes [1992], Hayes and Lundholm [1996]). The fact that other parties may use public information to the disclosing firm's disadvantage can dampen its disclosure incentives (Verrecchia [1983], Gal-Or [1985]). However, a competitive threat may not always induce firms to withhold information. For example, incumbent firms may disclose information to deter entry by competitors. Firms might also share information about market demand to prevent overproduction in the industry (Kirby [1988]). Furthermore, competitors can infer information from the fact that a firm does not make certain disclosures. Thus, the relation between disclosures and proprietary costs is complex and depends on the type of competition threat (e.g., Vives [1984], Gal-Or [1986], Verrecchia [1990], Wagenhofer [1990], Feltham, Gigler, and Hughes [1992]).

There are a number of empirical studies that examine the effects of proprietary costs on firms' voluntary disclosure decisions (see online appendix table 3.4). Generally speaking, the evidence is consistent with the notion that firms provide fewer segment disclosures due to proprietary costs or competitive concerns (Harris [1998], Berger and Hann [2003, 2007], Leuz [2004], Botosan and Stanford [2005], Hope and Thomas [2008], Bens, Berger, and Monahan [2011]). However, as several studies point out, a potential alternative explanation for much of the evidence is that managers withhold information for ulterior reasons, for example, to hide poorly performing segments or internal capital allocations that are inefficient and not necessarily in the interest of shareholders. Bens, Berger, and Monahan [2011] provide evidence suggesting that competitive concerns appear to be particularly prevalent for less-diversified firms and for those competing with private firms. In contrast, for many multisegment firms, the firms' disclosure behavior is consistent with an agency cost explanation.

Similarly, disclosures can be costly to managers in that they could face significant personal costs from disclosing bad news to investors. Kothari, Shu, and Wysocki [2009] provide evidence consistent with the idea that career concerns motivate managers to withhold bad news so that they can bury the bad news in subsequent good news disclosures (see also Bertomeu, Marinovic, and Ma [2015]).

Other research posits that the risk of shareholder litigation makes it potentially costly for firms to voluntarily provide disclosures, especially when these are forward-looking. However, as shown in online appendix table 3.4, the evidence regarding the effects of litigation on disclosure is mixed

and also quite subtle or nuanced, especially when it comes to bad news disclosures (e.g., Skinner [1994, 1997], Francis, Philbrick, and Schipper [1994], Kasznik and Lev [1995], Bamber and Cheon [1998], Johnson, Kasznik, and Nelson [2001, 2007], Field, Lowry, and Shu [2005], Rogers and Van Buskirk [2009], Billings and Cedergren [2015]. Several of these papers recognize and provide evidence that the relation between disclosure and litigation is endogenous in that disclosure could also reduce litigation risks and expected litigation costs, which, in turn, would be a benefit from disclosure.

Finally, it is possible that disclosure activities have indirect costs to existing financing or other relationships (e.g., political connections). Evidence in this category comes, for instance, from Bushee and Noe [2000] and Bushee, Matsumoto, and Miller [2004] suggesting that voluntary disclosures can attract transient or retail investors, which, in turn, can increase stock volatility; from Leuz and Oberholzer-Gee [2006] suggesting that transparency and disclosure can be costly to existing political relationships; and from Ball, Li, and Shivakumar [2015] and Chen, Harford, and Lin [2015] suggesting that IFRS adoption can be costly to debt contracting.

3.4 MARKET-WIDE EFFECTS OF FIRMS' DISCLOSURE AND REPORTING ACTIVITIES

As a final category of possible economic outcomes, we discuss potential market-wide effects. The theory argues that the effects of disclosure and reporting activities often extend beyond the firm providing the information, resulting in information spillovers and externalities (e.g., Dye [1990], Admati and Pfleiderer [2000]). Importantly, disclosure of one firm not only provides information to investors when evaluating *other* firms (e.g., industry peers) or to the management of *other* firms when making decisions, but it also has the potential to reduce agency problems in other firms. For example, the disclosure of operating performance and governance arrangements provides useful benchmarks that help outside investors to evaluate other firms' managerial efficiency or potential agency conflicts and, in doing so, can lower the costs of monitoring. Empirically, such information transfers and governance spillovers have not been explored much.

The early literature shows that earnings announcements provide information to investors about other firms in the same industry and hence there are information transfers (e.g., Foster [1981], Olsen and Dietrich [1985], Baginski [1987], Clinch and Sinclair [1987], Han, Wild, and Ramesh [1989], Han and Wild [1990]). These studies suggest the existence of industry-wide and potentially market-wide externalities from firms' disclosure and reporting activities. More recently, studies explore spillover and peer effects from accounting restatements and misreporting. These suggest that restatements and misreporting not only result in equity market penalties for restating firms, but also have information spillover effects on their competitors (Xu, Najand, and Ziegenfuss [2006], Gleason, Jenkins,

and Johnson [2008], Silvers [2016]), as well as negative spillover (or peer) effects on their real investment decisions (e.g., Sidak [2003], Sadka [2006], Durnev and Mangen [2009], Beatty, Liao, and Yu [2013]).[29] We summarize empirical studies examining market-wide effects of disclosure and reporting in online appendix table 3.5.

In sum, we call for more research into the market-wide effects of disclosure and reporting activities, in particular, as they play an important role in the economic justification of disclosure and reporting mandates.[30] As discussed, there are numerous reasons why the disclosures of a single firm extend beyond the firm itself. Even if such spillovers and externalities from any single firm are small, they could be large in the aggregate. However, as individual firms are expected to trade off the private (or firm-specific) costs and benefits, even relatively small disclosure and reporting costs could deter socially optimal disclosure and reporting activities. As with other externalities, the problem is that firms are unlikely to internalize all the market-wide benefits and hence may not provide the socially optimal level of disclosure.

4. Evidence on the Economic Effects of Disclosure Regulation

In this section, we summarize and discuss empirical evidence on the economic effects of disclosure regulation. We focus on the introduction of new disclosure mandates as well as major extensions of the entire disclosure regime.[31] We include studies on major changes in the enforcement as enforcement is an important element of securities or disclosure regulation. We also discuss studies on international differences in disclosure regulation. Studies examining mandated changes in the entire set of reporting standards (e.g., IFRS adoption) are discussed in section 5.

4.1 STUDIES EXAMINING THE INTRODUCTION OF U.S. SECURITIES REGULATION

In this section, we focus on studies examining the imposition of disclosure regulation on U.S. firms via the Securities Act of 1933 and the Exchange Act of 1934. These Acts, which introduced disclosure requirements

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[29] There is also evidence on the spillover effects of mandated changes in disclosure and financial reporting (e.g., Bushee and Leuz [2005], Chen, Young, and Zhuang [2013]). We discuss those changes in sections 4 and 5.

[30] But, we also note that externalities and peer effects are particularly thorny when it comes to identification. See Manski [1993] and Angrist [2014] for the problems of estimating peer effects, which likely also apply to externalities.

[31] As explained in the introduction, we exclude studies that focus exclusively on a particular accounting standard or narrow disclosure rule. While individual rules can have important capital-market effects, these are typically embedded in a set of accounting standards and an existing disclosure regime, which likely makes the results (more) conditional on the existing reporting regime. We admit that the distinction is more a matter of degree than principle.

for all exchange-traded firms, were—over time—extended to cover over-the-counter (OTC) traded stocks (i.e., the 1964 Securities Act Amendments and the 1999 Eligibility Rule on the OTC Bulletin Board (OTCBB)). In addition to the first-time imposition of disclosure requirements, there were also major reforms of securities regulation pertaining to disclosure, such as in 2000 and the Sarbanes–Oxley Act of 2002. Studies examining these major reforms are discussed in the next section.

The early empirical literature on disclosure regulation primarily analyzes the effects around the Acts of 1933 and 1934 and is generally relatively negative or at least skeptical about the benefits of disclosure regulation. Studies find little evidence that market-adjusted returns of unregistered new stock issues are different from the returns of registered issues post SEC regulation (Stigler [1964], Jarrell [1981]). However, the variance of new issues' abnormal returns decreases post regulation (Stigler [1964], Jarrell [1981], Simon [1989]). In addition, Jarrell [1981] provides evidence that default rates of registered bonds have decreased after the Acts. Benston [1969, 1973] finds little evidence of fraud related to financial statements in the period before the new disclosure regulation. He also documents that there was widespread voluntary disclosure prior to the regulation. In addition, he finds little evidence in betas and abnormal returns that the risk of NYSE stocks that did not disclose prior to the mandate has significantly decreased relative to NYSE stocks that voluntarily disclosed in the preperiod.[32] Finally, Chow [1983] analyzes stock reactions to events related to the passage of the Acts and finds negative abnormal stock returns. We list the key studies and summarize their specific findings in online appendix table 4.1.

The early studies and their interpretations have been heavily debated and repeatedly challenged (e.g., Friend and Herman [1964], Seligman [1983], Coffee [1984], Easterbrook and Fischel [1984], Romano [1998], Fox [1999]). Proponents of mandatory disclosures often point to the results indicating that the variance of new issues' abnormal returns decreases after the imposition of SEC disclosure regulation. They interpret this evidence as supporting the notion that mandatory disclosures improve investors' assessment of risky securities (e.g., Seligman [1983]). Opponents of disclosure regulation, in turn, argue that this result likely reflects selection and composition rather than a treatment effect. They point out that there is also a trend from public debt offerings toward private debt placements, which is more pronounced among relatively risky bonds (Benston [1969], Jarrell [1981], Simon [1989]). Thus, the introduction of disclosure regulation may have shifted riskier securities to less-regulated markets.[33]

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[32] Mahoney and Mei [2006] follow a different approach and examine short-window market reactions to the first filings of the mandated disclosures. They find no evidence that these filings contain new information for firms subject to NYSE disclosure rules. This finding is consistent with the evidence in later periods that 10-K filings have modest, if any, market reactions, especially when compared to earnings announcements (e.g., Foster and Vickrey [1978], Asthana and Balsam [2001], Li and Ramesh [2009]).

[33] More recent studies also point to such effects (see, e.g., Bushee and Leuz [2005], Gerakos, Lang, and Maffett [2013]).

This discussion highlights the importance of controlling for firms' responses to the regulation, which can result in sample composition changes in the postperiod. Moreover, the early studies lack a control group of unaffected firms because all exchange-traded firms were affected by the new regulation. The studies typically provide comparisons in the variable of interest before and after the regulation, rather than formal difference-in-differences tests.[34] Several studies attempt to address these issues by exploiting cross-sectional differences such as differential disclosures before the imposition of the regulation (e.g., Benston [1973], Simon [1989], Daines and Jones [2012]). For instance, Benston [1973] uses NYSE firms that voluntarily disclosed prior to the mandate as a benchmark. However, these firms were still affected by the new regulation, though arguably less, in that the new regulation requires certain disclosures and no longer gives firms a choice. Moreover, firms have voluntarily selected into this control group, which raises concerns about the parallel-trends assumption.

A recent study by Daines and Jones [2012] highlights the problems of using cross-sectional differences among affected firms. They examine changes in information asymmetry and market liquidity around the Acts and show that exchange-traded stocks, especially on the Curb Exchange, experience an increase in liquidity relative to OTC firms that were not covered by the Acts. However, when they partition the results by firms' prior disclosure status, they find that liquidity often increases *more* for firms that previously disclosed a required item voluntarily compared to firms that were compelled to provide new disclosures. A potential interpretation is that firms that previously disclosed experience liquidity increases because regulation increases the commitment to disclosure and the enforcement thereof. But, the findings could also be interpreted as casting doubt on the primary result that the new regulation caused the increase in liquidity. That is, the interpretation of cross-sectional differences among treated firms depends heavily on the theory of how firms are affected by the regulation (and its plausibility).[35]

Thus, overall, studies on the capital-market effects around the initial introduction of SEC disclosure regulation are inconclusive and, if anything, present negative evidence on the potential benefits of disclosure regulation. A number of studies revisit the capital-market effects, analyzing extensions of SEC regulation to previously unregulated firms.[36] We list and

[34] This design is also susceptible to changing market conditions around the regulatory changes, in particular, the onset of the Great Depression. Moreover, it is difficult to separate the effects of the regulation from a market response to the excesses in financial markets in the late 1920s and the ensuing crash, which, in turn, gave rise to the regulation.

[35] We discuss this issue also in section 4.2 for SOX studies.

[36] Strictly speaking, firms in the OTC markets are not entirely unregulated as these are also subject to state securities regulation, that is, Blue Sky laws. However, as discussed in Mahoney [2003] and Brüggemann et al. [2014], these laws generally do not require (or result in) publicly available disclosures. The Blue Sky laws are essentially investor protection statutes. See,

summarize key studies in online appendix table 4.2. Both the 1964 Securities Act Amendments and the 1999 Eligibility Rule for the OTC Bulletin Board imposed SEC disclosure regulation on particular firms trading in the OTC equity market that previously did not have to file with the SEC. One advantage of these regulatory events is that firms that trade in the *same* market segment, but already file with the SEC, provide a natural control group. Moreover, the events are more recent, resulting in better data availability and larger samples, and they fall into less-turbulent time periods than the Acts of 1933 and 1934. These factors should facilitate tighter and more powerful research designs.

The Securities Act Amendments of 1964 imposed SEC disclosure regulation on larger OTC securities, many of which later traded on the NASDAQ market. As shown in online appendix table 4.2, two studies document significant capital-market benefits around the disclosure mandate. Ferrell [2007] finds a reduction in volatility among OTC securities relative to NYSE stocks that are already subject to SEC disclosure requirements. He interprets this evidence as consistent with an increase in price efficiency due to information being more quickly reflected in prices. In addition, he documents that OTC securities exhibit positive abnormal returns during the time period over which the passage of the 1964 Amendments became likely. Greenstone, Oyer, and Vissing-Jorgensen [2006] find positive (and large) abnormal stock returns to most affected OTC firms from the time the regulation was proposed to the time it went into force, relative to size and book-to-market matched NYSE/AMEX firms that are not affected by the disclosure mandate. By using this control group, Greenstone, Oyer, and Vissing-Jorgensen [2006] do not rely solely on the asset pricing model to identify the effects of the mandate. To bolster the analysis, they show that there are no longer any abnormal returns for OTC firms once the new mandate is in place, and that OTC firms experience positive abnormal returns in the weeks when their compliance with the new disclosure rules became known to the market.

The authors interpret their evidence through the lens of Shleifer and Wolfenzon [2002], suggesting that mandatory disclosure "causes managers to focus more narrowly on maximizing shareholder value" (p. 399). To support this interpretation, Greenstone, Oyer, and Vissing-Jorgensen [2006] show that OTC firms experience an increase in operating performance relative to unaffected firms. While it is conceivable that disclosure regulation reduces the conflicts between controlling insiders and outside shareholders, the evidence for firm value and operating performance is also

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for example, Agrawal [2013] for an analysis of the effects of these laws on firms' financing and investment decisions.

consistent with other interpretations, such as a decline in the cost of capital due to better disclosure.[37]

Battalio, Hatch, and Loughran [2011] use a different approach to estimate the effect of the 1964 Securities Acts Amendments. They analyze short-term abnormal returns when OTC firms announce their intention to list on the NYSE. They compare these returns before and after the Amendments. They argue that returns should be positive in both cases, but less so in the postperiod if the disclosure mandate increases firm value, as shown in Greenstone, Oyer, and Vissing-Jorgensen [2006] and Ferrell [2007]. Battalio, Hatch, and Loughran [2011] do not find significant announcement return differences between the two periods. They, therefore, question whether the disclosure mandate had an effect on firm value. Their explanation for their results is that most OTC firms were already required by the National Association of Securities Dealers (NASD) to send certified financial statements to shareholders and the NASD on an annual basis. Consistent with this requirement, Battalio, Hatch, and Loughran [2011] were able to find financial information from various sources for most OTC stocks prior to the 1964 Amendments. Given their evidence, Battalio, Hatch, and Loughran [2011] raise the possibility that the results in Greenstone, Oyer, and Vissing-Jorgensen [2006] and Ferrell [2007] reflect the difficulty of measuring abnormal returns over a longer period. However, such difficulties should also affect the postperiod in which Greenstone, Oyer, and Vissing-Jorgensen [2006] and Ferrell [2007] do not find abnormal returns. Moreover, even if firms already provided disclosures through other channels prior to the disclosure mandate, it would still be possible for the Amendments to have significant capital-market effects if the SEC mandate was stricter or more strictly enforced. Thus, we need more research to determine the effects of the 1964 Amendments, particularly, evidence that is not based on returns.

Bushee and Leuz [2005] examine the introduction of SEC disclosure regulation to the OTCBB via the "Eligibility Rule" in 1999. Prior to the Rule, smaller firms that were not subject to 1964 Amendments could be quoted on the OTCBB without filing with the SEC. The Rule eliminates this possibility and forces these firms to comply with the reporting requirements under the Securities Exchange Act of 1934, if they remain quoted on the OTCBB. Bushee and Leuz [2005] point out that OTCBB firms that already file with the SEC could still be affected if the disclosures by newly compliant firms have externalities. They find that OTCBB firms that were already compliant with SEC reporting obligations experience positive abnormal returns around key announcement dates of the rule as well as sustained increases in liquidity, relative to NASDAQ Small Cap firms. This evidence is consistent

[37] Moreover, the documented firm value increases do not necessarily translate into welfare gains as the abnormal returns could be the result of wealth transfers from controlling insiders to outside shareholders.

with the existence of positive externalities from disclosure regulation, possibly due to liquidity spillovers or an enhanced reputation of the OTCBB. However, this interpretation hinges crucially on the extent to which NASDAQ Small Cap firms are an appropriate control group to determine the externalities.

Bushee and Leuz [2005] also show that the imposition of SEC disclosure requirements essentially forced over 2,600 firms (or 76% of the market segment) into the less-regulated and less-liquid Pink Sheets market, at significant costs in terms of market value and liquidity. This evidence suggests that, for the majority of (smaller) OTCBB firms, the (firm-specific) costs of SEC disclosure regulation outweigh the benefits.[38] In sum, the capital-market evidence from the introduction of mandatory disclosure in U.S. securities regulation is fairly mixed. Several studies find a reduction in volatility or liquidity benefits, but the return evidence goes in both directions and differs across studies and regulatory acts (see online appendix table 4.1). In addition, there is some evidence of substitution effects, that is, firms shifted from public offerings to private placements (Benston [1969], Jarrell [1981], Simon [1989]) or moved to a different trading venue (Bushee and Leuz [2005]). These findings illustrate that it is important to consider ways in which firms can respond to or avoid the imposition of regulation. Generally, firms have the option to go private, not to go public, or to move to an unregulated market. Understanding these and other potential responses and avoidance strategies is crucial when empirically evaluating the costs and benefits of disclosure regulation and also when designing the rules in the first place. We have relatively little evidence on such responses and avoidance strategies, although there are SOX studies with this focus (see section 4.2).

We further note that there is relatively scant evidence on the aggregate consequences of these regulatory acts, such as changes in the market equity premium, participation in the stock markets, capital formation, or capital allocation. Bushee and Leuz [2005] is one of the first mandatory disclosure studies that explicitly attempts to provide evidence on positive externalities. Their evidence and the possibility of externalities (or spillovers) caution us more generally to use already-compliant firms in the same market segment as unaffected, untreated, or control firms. More research on this issue and on the market-wide consequences of disclosure regulation is needed. There is also little research examining potential real effects, such as changes in managerial behavior and corporate investment, stemming from the

[38] Even firms that were compelled to adopt SEC disclosures to avoid removal from the OTCBB exhibit negative abnormal returns around the rule change. This finding suggests that the mandate is on balance costly to these firms, consistent with the rule eliminating their preferred disclosure strategy. These firms still experience significant increases in liquidity upon compliance, consistent with the notion that increases in the commitment to disclosure manifest in higher liquidity. But, the return evidence suggests that these benefits do not outweigh the costs.

imposition of disclosure regulation in these U.S. settings. We discuss studies on such effects in section 4.4.

4.2 STUDIES ON MAJOR CHANGES IN U.S. DISCLOSURE REGULATION

In recent years, there were two major changes in U.S. disclosure regulation: Reg FD and SOX, which have already been studied extensively.[39] As there are detailed surveys of these studies (e.g., Koch, Lefanowicz, and Robinson [2013], Coates and Srinivasan [2014]), we review this literature in less detail and focus instead on the interpretation of the findings, as well as on important research-design issues. In addition, online appendix table 4.3 provides an overview of this literature.

4.2.1 Regulation Fair Disclosure. Reg FD prohibits selective disclosure of material, nonpublic information to certain individuals (e.g., analysts, institutional shareholders) without contemporaneous disclosure to the public at large. It was adopted by the SEC in August 2000 and became effective in October 2000. The intention of Reg FD is to increase investor confidence in the integrity and fairness of U.S. capital markets by targeting the distribution of information and hence the degree of information asymmetry between investors. It does not prescribe specific disclosures. However, as has been noted in the legislative process (e.g., Unger [2001]), prescribing how corporate disclosures are distributed can change firms' incentives to provide information in the first place. Moreover, it can affect the behavior of intermediaries engaged in the production and dissemination of information (e.g., financial analysts). Thus, the effects from Reg FD are primarily a tradeoff between leveling the playing field and reducing public information available to markets, sometimes called the "chilling effect" (e.g., Koch, Lefanowicz, and Robinson [2013]).

Consequently, the evidence on Reg FD focuses, broadly speaking, on three outcomes: (1) capital-market effects (e.g., trading volume, stock returns around earnings announcements, information asymmetry, and the cost of capital); (2) firms' disclosure responses, including changes in their communication channels; and (3) effects on and responses by information intermediaries, such as financial analysts (see online appendix table 4.3).

In summary, the evidence suggests that Reg FD has leveled the playing field and reduced information asymmetries among investors, but at the same time had a chilling effect on the amount of available information for some firms, particularly smaller and technology firms. The chilling effect appears to have materialized primarily through changes in information intermediation, such as reduced analyst coverage for some firms (see Koch,

[39] In addition, the Jumpstart Our Business Startups (or JOBS) Act as well as the Dodd–Frank Act are likely to be studied extensively. For both regulatory changes, disclosure studies are starting to emerge, but, as most are still working papers, we do not review this literature. See, for example, Chaplinsky Hanley, and Moon [2015] and Dambra et al. [2015] for early studies examining disclosure provisions of the JOBS Act.

Lefanowicz, and Robinson [2013] for a detailed survey). However, the evidence for specific outcomes is often mixed and the results differ across studies. One finding that consistently comes through is that the documented effects around Reg FD depend on firm size (and trading venue). We discuss several Reg FD studies to illustrate key findings as well as research-design challenges.

For instance, Eleswarapu, Thompson, and Venkataraman [2004] and Chiyachantana et al. [2004] document that bid–ask spreads of NYSE firms decrease after Reg FD, consistent with a decline in information asymmetries without a countervailing reduction in the overall amount of information. In contrast, Sidhu et al. [2008] examine NASDAQ firms and find that the adverse-selection component of spreads increases after Reg FD. Thus, if Reg FD indeed limits selective disclosure, this result implies a decline in the overall amount of information for these firms (as otherwise spreads should not widen). The opposing results could be explained by sample differences: Firms trading on NASDAQ tend to be smaller than NYSE firms and are more frequently in the technology sector.

Similarly, Gomes, Gorton, and Madureira [2007] and Duarte et al. [2008] find no significant changes in the cost of capital for NYSE firms, but an increase in the cost of capital for smaller firms and NASDAQ firms, respectively. In contrast, Chen, Dhaliwal, and Xie [2010] find a decline in the implied cost of capital of large and medium firms after Reg FD, but no significant change for small firms.

The evidence for abnormal returns around earnings announcements and trading volume is also mixed (e.g., Bailey et al. [2003], Heflin, Subramanyam, and Zhang [2003]). Francis, Nanda, and Wang [2006] replicate these studies using foreign firms with U.S. cross-listings (ADRs) as a benchmark to control for concurrent institutional and economic changes unrelated to Reg FD. Their findings suggest that the changes in return volatility and trading volume reflect concurrent events rather than Reg FD. An additional reason why capital-market outcomes are difficult to interpret is that Reg FD is expected to change firms' disclosure strategies as well as the behavior of information intermediaries (e.g., incentives for private information acquisition). The capital-market effects around Reg FD should reflect the confluence of these responses, likely resulting in considerable cross-sectional variation in the observed effects. Thus, it is perhaps not surprising that the results of the aforementioned studies differ considerably by sample composition (or firm size).

Consistent with this conjecture, there is evidence that financial analysts reduced their information production around Reg FD, at least for some firms. For instance, Gomes, Gorton, and Madureira [2007] document a loss in analyst coverage for smaller firms. Gintschel and Markov [2004] find that the informativeness of analyst reports as measured by their price impact upon release declines after Reg FD and this decline is concentrated in growth firms. There is also evidence that forecast accuracy (dispersion) decreases (increases) around Reg FD (e.g., Bailey et al. [2003], Heflin,

Subramanyam, and Zhang [2003]), particularly for forecasts that are made early in the year and for smaller firms (Agrawal, Chadha, and Chen [2006]). Again, it is not clear to what extent these changes in forecast properties are driven by Reg FD versus other concurrent events (Francis, Nanda, and Wang [2006]). But, if the results are attributable to Reg FD, they suggest a chilling effect for some firms as well as a leveling of the playing field, that is, fewer selective private disclosures to analysts.

There are also several studies examining the effects of Reg FD on firms' disclosures. Consistent with its intended effect, Kothari, Shu, and Wysocki [2009] provide evidence that Reg FD constrains managers' tendency to selectively leak good news ahead of public disclosures.[40] In response, firms could make public disclosures in lieu of selective private disclosures. For instance, Bushee, Matsumoto, and Miller [2004] show that firms switch from closed to open conference calls. Similarly, there is evidence that firms increase earnings guidance after Reg FD, for example, to compensate the loss in analyst coverage (Heflin, Subramanyam, and Zhang [2003], Gomes, Gorton, and Madureira [2007], Anantharaman and Zhang [2011]). Firms could also alter the channel of private communication. Credit rating agencies are exempted from Reg FD and hence firms could provide information to them. Consistent with such a response, Jorion, Liu, and Shi [2005] find that credit rating changes have stronger stock market reactions after Reg FD.

When interpreting the Reg FD evidence, it is important to keep a number of research-design challenges in mind. First, and probably foremost, studies on Reg FD have to account for a series of events that took place around the enactment of Reg FD (see also Francis, Nanda, and Wang [2006]). There are the stock market boom and subsequent crash in 2000, the ensuing economic recession, decimalization of quoted stock prices at major U.S. exchanges in early 2001, a series of accounting scandals in 2001 and 2002 (e.g., Enron, WorldCom), the regulatory response with SOX in 2002, and the Global Settlement of major investment firms in 2003. These events and the fact that Reg FD applies to all domestic SEC registrants make it very difficult to isolate the effect of the regulation, even with a difference-in-differences design. However, most Reg FD studies have a pre–post design solely estimating changes in the outcome of interest and hence are even more susceptible to concurrent events. Another design challenge is that prior selective disclosure practices are not observable and hence need to be inferred. As a result, it is difficult to exploit changes in disclosure behavior, for instance, by focusing on firms that were forced to alter their disclosure practices (Koch, Lefanowicz, and Robinson [2013]).

In response to the concern about concurrent events, several studies have used foreign firms with ADRs cross-listed on U.S. exchanges as a

[40] But, there is also evidence that, even after Reg FD, some investors continue to have private access to management, for example, via invite-only conferences or one-on-one meetings (e.g., Bushee, Jung, and Miller [2011], Soltes [2014]).

control group (e.g., Francis, Nanda, and Wang [2006], Gomes, Gorton, and Madureira [2007], Chen, Dhaliwal, and Xie [2010]). ADRs are exempted from Reg FD but likely affected by (many) concurrent events. However, as discussed in Francis, Nanda, and Wang [2006], this control group is appropriate only if (1) ADR firms do not voluntarily comply with Reg FD and (2) foreign jurisdictions do not adopt similar regulations around the same time. Regarding (1), it is not obvious why foreign firms would adopt Reg FD voluntarily, especially if they have engaged in selective disclosures in the past. Besides, committing not to engage in selective disclosure is difficult to promise and, even if feasible, it is something firms could have done much earlier. Thus, it is perhaps not surprising that evidence on voluntary disclosure changes by foreign firms is mixed (Sidhu et al. [2008], Canace et al. [2010]). Regarding (2), it should be noted that, while U.S. regulatory changes often become a role model for other countries, the adoption of similar regulation by a foreign jurisdiction takes time and hence would not likely be concurrent. Thus, the primary concern regarding the use of ADRs as a control group is not (1) or (2) but the extent to which this control group satisfies the parallel-trends assumption.[41]

4.2.2 Sarbanes–Oxley Act. The second major change to U.S. disclosure regulation in recent years was SOX. The Act was passed in 2002 in response to a series of corporate scandals (e.g., Enron, WorldCom). SOX created the Public Company Accounting Oversight Board (PCAOB) to oversee and regulate auditing. It also requires auditors to play a larger role in the enforcement of existing financial reporting and disclosure regulation. Specifically, SOX strengthens the independence and powers of the audit committee (e.g., to hire the auditor) and requires that firms obtain auditor attestation of their internal control systems. This requirement does not prescribe particular internal controls.[42] However, material weaknesses in the internal control system have to be disclosed. In addition, SOX mandates audit partner rotation every five years. As such SOX is more focused on auditing and enforcement of financial reporting, rather than on specific disclosures.

Evidence on the effects of SOX focuses broadly speaking on four aspects: (1) the costs from complying with SOX (e.g., audit fees), (2) the effects on accounting and auditing quality as well as capital-market reactions to 404 disclosures, (3) the net effects on shareholder wealth using stock returns, and (4) corporate responses to SOX and avoidance behavior (e.g.,

[41] It requires that foreign firms with ADRs would have been similarly affected by other economic events that are concurrent, but unrelated to Reg FD. Given that several concurrent events are fairly specific to the U.S. environment, this assumption is tenuous and hence studies with an ADR-based control group should be interpreted with caution.

[42] Section 404 has been the most controversial aspect of SOX. Its implementation has been repeatedly delayed and was eventually limited to companies with a public float over $75 million (see Coates and Srinivasan [2014]).

going private). online appendix table 4.3 provides examples for these outcomes. As with Reg FD, the evidence on many of these aspects is decidedly mixed.

The evidence on the direct costs of SOX compliance is mostly based on surveys (e.g., SEC [2004], Charles River Associates [2005], Alexander et al. [2013]). These surveys show that compliance costs increase in firm size but at a decreasing rate. These also indicate that compliance costs have fallen over time, consistent with the revisions to Section 404 implementation over time. However, one problem in comparing audit fees and other compliance costs before and after SOX is that the accounting scandals and ensuing loss in investor confidence would have increased the demand for assurance and auditing even in the absence of SOX. In response to more skepticism and scrutiny by investors, firms may have also increased their disclosures and improved internal controls (e.g., Leuz and Schrand [2009]). Without a counterfactual that takes these responses into account, it is difficult to estimate incremental SOX compliance costs. Iliev [2010] circumvents this problem using a regression-discontinuity design to estimate the impact of SOX on audit fees. He exploits the exemption of firms with a public float below $75 million. Firms that are just below this cutoff should experience the same market forces (e.g., demanding more disclosure, better controls, and auditing) as firms that are just above the cutoff, but only the latter have to comply with SOX.[43] Based on this design, he estimates that audit fees have increased by 86%, or approximately $530,000, for the average firm in his sample. However, as is often the case with a regression-discontinuity design, these estimates do not generalize to firms that are much larger (or smaller). They also do not capture subsequent changes to SOX implementation that likely have decreased audit fees and compliance costs (e.g., the switch from Auditing Standard 2 to Auditing Standard 5). Thus, to date, we have limited evidence on the direct and long-run costs of SOX compliance.[44]

There are also relatively few studies on the indirect costs of SOX. For instance, SOX compliance could become a major distraction for managers and it could also make them less willing to take risks and to innovate. Such indirect costs could swamp the direct costs, especially for larger firms. Note, however, that an improvement in transparency and investor confidence could also reduce firms' cost of capital and hence increase the net present value of new investments. Thus, it is ex ante unclear how SOX affects corporate investment. Bargeron, Lehn, and Zutter [2010] and Kang, Liu, and Qi [2010] analyze this effect and present evidence consistent with

[43] His study addresses a number of other concerns (e.g., incentives for firms to stay below the $75 million cutoff). See Gao, Wu, and Zimmerman [2009] for evidence that some firms engage in various actions to avoid crossing the public float threshold.

[44] There is little evidence that SOX has led to an increase in litigation costs—a concern that was initially raised when SOX was passed. See Coates and Srinivasan [2014] for a discussion of this concern and the evidence.

a decline in corporate investment by U.S. firms relative to non-U.S. firms in the period after SOX. In contrast, Albuquerque and Zhu [2013] find an increase in R&D (and total) investments using a regression-discontinuity design around the $75 million threshold. But, again, this evidence applies only to firms close to the size threshold. Albuquerque and Zhu [2013] also show that, for a large sample of U.S. firms, corporate investment starts to decline in 1999, not in 2003 when SOX becomes effective. Thus, as discussed in Coates and Srinivasan [2014], while there seems to be a decline in U.S. investment, it is not clear that the trend is driven by SOX, rather than broader changes in the U.S. environment.

With respect to potential SOX benefits, several studies suggest an improvement in reporting and auditing quality. Specifically, there is evidence that, after SOX, accrual-based earnings management, for instance, to meet analysts' earnings targets, has declined (e.g., Cohen, Dey, and Lys [2008], Koh, Matsumoto, and Rajgopal [2008], Bartov and Cohen [2009], Iliev [2010]) and that the recognition of losses has become more timely (e.g., Lobo and Zhou [2006]). However, several of these studies also point to an increase in real earnings management, which partially offsets the changes with respect to accruals. A challenge for these studies is that proxies for earnings management and reporting quality (e.g., timely loss recognition) are noisy. In addition, measuring earnings quality usually requires several consecutive firm-year observations. The relatively long measurement period makes it difficult to attribute the observed changes to SOX (or any other regulatory event). With a few exceptions (notably Iliev [2010]), the aforementioned studies are susceptible to confounding effects by concurrent events. These studies also cannot rule out that the changes are driven by increased market discipline following the accounting scandals (see also Coates and Srinivasan [2014]).

One potential way to mitigate these concerns about concurrent events is to use changes in liquidity to infer changes in reporting quality from SOX. Market liquidity is tightly linked to information asymmetry, which, in turn, is influenced by the quality of reporting and disclosure (see section 3). The advantage of using market liquidity is that it can be reliably measured over relatively short intervals and that liquidity changes should occur if and when reporting improves (and not much earlier). These features allow researchers to design studies that exploit the timing of regulatory changes for identification (e.g., a staggered introduction), that is, to control for unrelated changes in liquidity, including concurrent events and market discipline effects. This approach has been used for other regulatory changes such as IFRS adoption (Daske et al. [2008], Christensen, Hail, and Leuz [2013]). For SOX, only a few studies examine changes in market liquidity. Jain, Kim, and Rezaee [2008] analyze long-run changes in liquidity around the accounting scandals and SOX. They document a decline in liquidity during the time period of the accounting scandals and a subsequent improvement in liquidity that persisted after SOX. To tighten the link to SOX, the study also shows that liquidity changes around SOX

are associated with changes in total accruals (used as a proxy for earnings quality). While Jain, Kim, and Rezaee [2008] trace out changes in liquidity over time, its primary benchmark is the prescandal period. It does not exploit the staggered introduction of SOX for some firms and hence the documented liquidity changes could also reflect general trends, concurrent events, and/or increased market discipline after SOX. To avoid these concerns, Albuquerque and Zhu [2013] exploit the public float cutoff for Section 404 compliance and use a regression-discontinuity design. They find a modest increase in market liquidity for firms subject to Section 404 of SOX relative to firms that are below the cutoff.[45] In addition to the studies on accounting quality, there is evidence suggesting that audit quality has improved. Dyck, Morse, and Zingales [2010] show that auditors play a (relatively) larger role in detecting accounting-related fraud after SOX. DeFond and Lennox [2011] find that a large number of small audit firms exit the market for public company audits following SOX and these auditors are of lower quality than those remaining in the market. Coates and Srinivasan [2014] note that restatements increase dramatically in the first few years after SOX and the spike in restatements is consistent with increased vigilance in the post-SOX era. While these studies corroborate the aforementioned studies on improvements in firms' reporting behavior, it is again difficult to attribute these changes to SOX and to isolate them from other concurrent events, including changes in market discipline.

One way to attribute outcomes more directly to SOX is to study disclosures (or other changes) that have been stipulated by the Act. Along this vein, a number of studies provide evidence that Section 404 disclosures are informative to investors (e.g., Doyle, Ge, and Mcvay [2007], Ashbaugh-Skaife et al. [2008], Feng, Li, and Mcvay [2009], Hammersley, Myers, and Shakespeare [2008]) and that firms' cost of capital increases around these disclosures (e.g., Ashbaugh-Skaife, Collins, and Lafond [2009], Costello and Wittenberg-Moerman [2011], Kim, Song, and Zhang [2011]). While these market reactions are attributable to disclosures that SOX introduced, it is harder to use these results for an evaluation of the Act.

To this end, a series of studies examines the net effect for shareholders using stock returns around the legislative events. The basic idea is that stock returns should among other things reflect the various costs and benefits to shareholders that the aforementioned studies suggest. Akhigbe and Martin [2006], Jain and Rezaee [2006], and Li, Pincus, and Rego [2008] find positive abnormal returns to events that increased the likelihood of the passage of SOX. In contrast, Zhang [2007] finds negative abnormal returns to legislative events leading up to the passage of SOX.

[45] It is again not certain that these results can be extrapolated to larger firms. In particular, the concern is that the effects are more pronounced for smaller firms and hence the liquidity effects could be (economically) insignificant for larger firms.

One reason for the difference in findings is that the studies use different legislative events, illustrating that the choice of event dates is critical. In addition, SOX events are clustered in time and often extend over several days. In fact, Leuz [2007] points out that three of the four key legislative events used in Zhang [2007] fall in the month of July 2002 and the respective event windows of these events cover all, but three trading days in the second half of July. This example illustrates the difficulty of removing unrelated concurrent events and market-wide effects in regulatory event studies. In the case of SOX, the adjustment of event returns is further complicated by the fact that SOX applies to all SEC registrants and hence to the vast majority of U.S. publicly traded firms. Thus, a natural control group of similar, but unaffected *U.S. firms* by which to adjust returns does not exist. For this reason, Zhang [2007] uses event-day returns to non-U.S.-traded foreign firms as a benchmark.[46] Her study illustrates that foreign equity markets experienced large negative abnormal returns around key legislative events as well. Thus, U.S. event returns without some form of market adjustment, as for instance in Jain and Rezaee [2006] and Li, Pincus, and Rego [2008], cannot be attributed solely to SOX and need to be interpreted cautiously. But, even the return adjustment using foreign markets is tricky as many of the confounding events over the legislative period likely affect U.S. and foreign firms differently.[47] A way to mitigate this problem is to rely solely on foreign firms and to estimate the return differential between U.S. cross-listed firms that have to comply with SOX and U.S. cross-listed firms that are exempted from SOX (Litvak [2007], Li [2014]).[48] The idea of this test is that firms with U.S. cross-listings have similar exposure to confounding U.S. events. Consistent with Zhang [2007], Litvak [2007] and Li [2014] find more negative abnormal returns to firms that have to comply with SOX, consistent with net costs to shareholders from SOX. However, this evidence does not necessarily apply to U.S. firms. It is possible, for instance, due to differences in governance structures and other institutional features, that SOX imposes costs on foreign firms, but is beneficial to U.S. firms, after all it was designed for U.S. firms. Another key challenge is that foreign firms that do not have to comply with SOX (e.g., many Level 1 ADRs) tend to be quite different from firms that have to comply with SOX (e.g., Level 2 or Level 3 ADRs). Firms in the latter group tend

[46] Another alternative is to use foreign firms that are cross-listed in the United States, but exempted from SOX. We have already discussed the pros and cons of this approach in the context of Reg FD studies. These are similar here.

[47] For example, as Leuz [2007] points out, these events include the United States moving toward a war with Iraq and Congressional debates over the creation of the Department of Homeland Security.

[48] Zhang [2007] and Li [2014] also provide an analysis comparing the returns of foreign firms with U.S. cross-listings to foreign firms without such cross-listings. However, the exposure of these two groups to confounding U.S. events is probably still substantially different, so this approach is not as tight as an analysis within cross-listed firms.

to be much larger and likely have (endogenously) more exposure to the United States.

An alternative way to identify SOX effects is to exploit cross-sectional differences in the returns to key legislative events (e.g., Jain and Rezaee [2006], Litvak [2007], Zhang [2007], Li, Pincus, and Rego [2008], Li [2014]). The idea of this approach is that, while all firms are affected by unrelated concurrent events, the return differential can be interpreted as a SOX effect. To illustrate, Chhaochharia and Grinstein [2007] find that firms that need to make more changes to become compliant with SOX earn positive abnormal returns around key SOX events compared to firms that are already more compliant. Similarly, Li, Pincus, and Rego [2008] find that abnormal returns around legislative events are positively related to proxies for earnings management prior to SOX. Hochberg, Sapienza, and Vissing-Jørgensen [2009] use lobbying behavior of corporate insiders to identify firms that are likely more affected by SOX. They demonstrate that firms whose insiders lobbied against a strict SOX implementation experience significantly positive abnormal returns over the passage of SOX compared to firms that did not lobby. They interpret this result as evidence that SOX benefits outside shareholders by reducing agency problems. To support this interpretation, they provide evidence that lobbying firms are not as well governed and are more likely to consume more private control benefits at the expense of outsiders. For the cross-sectional identification strategy to work, we need firm characteristics that are directly related to SOX, such as lobbying behavior or elements of the governance structure that make firms already compliant with certain SOX provisions. In addition, we need a theory that links these and other firm characteristics in a way that aids the preferred interpretation and precludes alternative explanations. For instance, without corroborating evidence showing that lobbying firms are poorly governed, the return differential between lobbying and nonlobbying firms alone could occur because *well*-governed firms are better able to cope with costly regulation and also have more time to lobby.

Thus, the interpretation of cross-sectional differences in abnormal SOX returns hinges critically on having convincing a priori predictions on how SOX differentially affects firms. At the same time, the firm characteristics used for the cross-sectional tests need to be uncorrelated with the effects of other unrelated concurrent events. Identifying such characteristics is difficult. Another shortcoming of the cross-sectional approach is that it can provide only relative evidence on the (net) costs and benefits. Despite the evidence of positive abnormal returns for some firms, as in Chhaochharia and Grinstein [2007] and Hochberg, Sapienza, and Vissing-Jørgensen [2009], it is still possible that, for these firms, SOX was overall net costly to shareholders, albeit to a lesser extent.

A final and more general concern about regulatory event studies is that they capture expectations at the time of legislation, rather than the effects after implementation. This concern is particularly relevant in the context

of SOX, given the implementation was substantially adjusted over time (see also Coates and Srinivasan [2014]). One could, therefore, estimate abnormal returns around the regulation's implementation, but these events are likely to be less "sharp" for a number of obvious reasons. In the context of SOX, however, it is possible to estimate the announcement returns to the decision to postpone Section 404 compliance for smaller firms. As the delay applies only to firms below a certain size cutoff, firms above the size cutoff can be used as a control. Using this approach, Zhang [2007] and Iliev [2010] find that firms below the cutoff experience positive returns on days when it is announced that SOX compliance is delayed.[49] These results suggest that SOX was net costly to shareholders of smaller firms, but again are difficult to extrapolate to the bulk of the U.S. market.

An alternative approach to estimating whether a regulatory act has been net costly or beneficial to shareholders is to examine firms' responses to new regulation. For instance, if SOX is net costly to shareholders, we expect firms to engage in avoidance strategies. This prediction assumes that managers act in the interest of shareholders. If not, the approach is more likely to measure the effects of SOX on corporate insiders and managers, rather than outside shareholders.

There are several papers that follow this revealed-preference approach. Engel, Hayes, and Wang [2007] analyze firms' going-private decisions around SOX. The idea is that firms can avoid SOX costs by going private and that they will do so whenever the costs imposed by SOX outweigh its benefits plus the net benefit from being public prior to SOX. Engel, Hayes, and Wang [2007] document an increase in Rule 13e-3 transactions after SOX. These transactions allow firms to deregister their securities from the SEC, a prerequisite to going private. They also show that the announcement returns to these transactions are positive and increase for smaller firms after SOX. These results are consistent with the notion that SOX is net costly for smaller firms. However, Leuz [2007] shows that there are similar going-private trends in other countries around the world, which makes it unlikely that the increase in Rule 13e-3 transactions documented by Engel, Hayes, and Wang [2007] is attributable to SOX.[50] Consistent with this concern, Leuz, Triantis, and Wang [2008] show that the frequency of going-private transactions is not related to SOX-related events. However, Kamar, Karaca-Mandic, and Talley [2009] document evidence using a difference-in-differences design consistent with a compliance cost effect. They examine acquisitions of public targets by private and public firms in the United

[49] Iliev [2010] also provides long-run, return-based evidence that Section 404 compliance was costly to firms that were just above the cutoff for compliance relative to smaller firms just below the cutoff.

[50] In addition, Bartlett [2009] shows that going private does not, per se, exempt firms from SOX as they may still have debt securities that require SEC reporting. Moreover, many of the going-private firms were so small that these never had to comply with SOX Section 404 (Coates and Srinivasan [2014]).

States and elsewhere, and find that the propensity of small public targets to be acquired by private firms (as opposed to public firms) has increased in the United States relative to elsewhere.

Leuz, Triantis, and Wang [2008] show that there is a considerable number of public companies that deregister their securities from the SEC, cease to make periodic SEC filings, but continue to trade publicly in markets that do not require SEC filings. They show that these going-dark activities account for the bulk of the SEC deregistrations after SOX and that going dark (but not going private) is associated with SOX-related events (including Section 404 implementation). Using a cross-sectional approach, Leuz, Triantis, and Wang [2008] provide evidence suggesting that, for many firms, cost savings play a significant role in the decision to go dark, consistent with the notion that SOX imposes substantial costs on firms, particularly smaller ones. However, as for the return-based event studies using a cross-sectional approach, the latter interpretation depends crucially on the reason why firms go dark after the imposition of SOX.[51] Similarly, there is evidence that foreign firms are more likely to delist and deregister their cross-listed securities in the United States (Marosi and Massoud [2008], Doidge, Karolyi, and Stulz [2010], Hostak et al. [2013], Li [2014]). Again, the motive could be compliance costs or, alternatively, a reduction in private control benefits to insiders as a result of SOX. Similar to the going-dark results, there is evidence consistent with both motives playing a role. In addition, there are studies on foreign firms' tendency to enter and raise capital on U.S. debt and equity capital markets (e.g., Piotroski and Srinivasan [2008], Doidge, Karolyi, and Stulz [2009], Gao [2011]). The evidence for new cross-listings on U.S. exchanges is mixed (see online appendix table 4.3).

Further evidence on avoidance strategies comes from Gao, Wu, and Zimmerman [2009]. They document that firms take various actions to keep their market capitalization below the $75 million cutoff for Section 404 compliance. However, as discussed in Coates and Srinivasan [2014], there is little evidence that SOX has had an impact on the frequency with which U.S. firms go public.

Overall, the revealed-preference approach produces relatively consistent evidence for the going dark of U.S. firms, deregistrations of foreign firms, and cross-listings more generally. It suggests that avoidance strategies are more prevalent among smaller firms, consistent with the notion that SOX compliance is particularly costly to smaller firms. However, avoidance strategies are also more common among firms with weaker governance and larger agency problems, consistent with the notion that SOX increased the scrutiny that these firms and their controlling insiders face.

In sum, the empirical findings on the impact of Reg FD and SOX suggest that these regulatory changes had significant costs and benefits, but the

[51] It is also possible that firms go dark in order to avoid the additional scrutiny imposed by SOX. Consistent with this hypothesis, Leuz, Triantis, and Wang [2008] document that, in at least some cases, controlling insiders appear to take firms dark to protect private control benefits and decrease scrutiny.

evidence is often quite mixed and at times even conflicting. A more robust finding is that the effects on firms are generally heterogeneous, especially with respect to firm size, consistent with the notion that new regulation creates winners and losers. However, many of the documented effects around the two regulatory changes are not necessarily causal, consistent with the identification challenges discussed in section 2. Thus, although both regulatory changes have been studied extensively, we are still far from answering the question of whether these regulatory changes were net beneficial and, more generally, whether the market-wide benefits of regulating disclosure exceed the aggregate costs.

4.3 INTERNATIONAL EVIDENCE ON COSTS AND BENEFITS OF DISCLOSURE REGULATION

An alternative approach to studying the economic consequences of disclosure regulation is to exploit cross-sectional variation across countries, rather than regulatory changes in a particular country, as in the previous sections. In this section, we review studies exploiting international variation in disclosure and securities regulation.[52] We begin with cross-sectional studies and then discuss studies that combine cross-sectional and time-series variation, including regulatory changes.

While there are many international differences in disclosure regulation, exploiting this cross-sectional variation poses a number of challenges. Financial reporting standards and disclosure regulation are integral parts of countries' institutional systems. The elements of these systems are often systematically related, leading to institutional clusters—a phenomenon that we discuss in more detail in section 5.5. We note that the existence of these clusters poses major omitted-variable concerns and implies that identifying the effects of a single regulatory element in the institutional system in a cross-sectional study is difficult, if not impossible. Controlling for the other elements of the system is not a satisfactory identification strategy in most settings because the list of potentially omitted variables is long and, on top of that, the various elements are likely *endogenously* related. For instance, it is no coincidence that countries with large public equity markets tend to have extensive disclosure regulation, stronger outside investor protection, and strong legal enforcement.[53] Moreover, some elements of the institutional system are measured with greater precision than others. For this reason, it is generally not appropriate to run a "horse race" among various elements of the institutional system with respect to some outcome variable. Stronger associations of one element compared to another could simply reflect lower measurement error, rather than lower economic relevance. Despite these challenges, cross-country studies have been instrumental in

[52] In addition, there is substantial research on the economic effects of reporting standards using non-U.S. and cross-country settings. We review this research in section 5.

[53] For evidence on these associations, see La Porta, Lopez-De-Silanes, and Shleifer [2006]. See also section 5 for further discussion.

advancing research on the role of institutions. Moreover, these studies have opened new avenues for research by providing a starting point for more in-depth research, posing many new questions, and by providing novel (and much needed descriptive) evidence.

Following the seminal work by La Porta et al. [1997, 1998], there are many cross-country studies on financial reporting and disclosure. Broadly speaking, these studies demonstrate that disclosure and transparency proxies, measured at the country and/or firm level, exhibit significant associations with other institutional factors (e.g., investor protection, judicial efficiency, rule of law) and with various market outcomes (e.g., cost of capital, financial development and market capitalization, analyst forecast accuracy, foreign investment and international portfolio flows, informed trading, market liquidity, ownership structure, and concentration). Examples are Hope [2003], Bushman, Piotroski, and Smith [2004], Francis, Khurana, and Pereira [2005], Gelos and Wei [2005], Eleswarapu and Venkataraman [2006], Guedhami and Pittman [2006], Chen, Chen, and Wei [2009], and Bilinski, Lyssimachou, and Walker [2013]. Many studies in this literature also document that firms' disclosures and countries' institutions have interactive associations with respect to market outcomes (e.g., Aggarwal, Klapper, and Wysocki [2005], Leuz, Lins, and Warnock [2009], Lang, Lins, and Maffett [2012], Maffett [2012]). For example, Aggarwal, Klapper, and Wysocki [2005] find a positive association between firms' voluntary disclosures and foreign mutual fund investment and that this association is more pronounced for firms that reside in jurisdictions with less-mandatory disclosure. In a similar vein, Leuz, Lins, and Warnock [2009] find that foreigners invest less in firms with insider control and opaque earnings when these firms are domiciled in countries with weaker disclosure regulation and outside investor protection. Such interactions suggest that the role and effects of firm-level disclosures depend on countries' institutions.

However, as these cross-country disclosure studies generally demonstrate associations, rather than causal effects, we need to interpret this evidence carefully. Often, it is not clear which of the variables are the primitive ones and in which direction causality runs. Corporate transparency and other institutional factors are likely jointly determined and hence not easily separable (see also Bushman, Piotroski, and Smith [2004]). Moreover, many of the aforementioned studies do not focus specifically on disclosure *regulation*. The proxies used in these studies are often based on a combination of voluntary and mandatory disclosures and hence reflect not only regulatory differences but also firms' practices. As this review focuses on regulation, we provide an overview of these studies and their findings in online appendix table 4.4, but do not discuss them in further detail here.

Instead, we turn to studies providing international evidence specifically on the economic effects of disclosure regulation. Glaeser, Johnson, and Shleifer [2001] compare securities regulation and the associated stock market development in Poland and the Czech Republic in the 1990s. Securities laws were designed from scratch after the two countries emerged from

socialism. The study emphasizes that issuer disclosure and capital-market intermediaries play a key role in securities regulation and for investor protection. It also suggests that enforcement of disclosure regulation by specialized regulators may be more efficient than judicial enforcement, which in our view is an important topic worth further investigation.

La Porta, Lopez-De-Silanes, and Shleifer [2006] examine the links between securities regulation and financial development in 45 countries. They create a data set evaluating the strength of countries' securities regulation and provide evidence that stricter and better enforced securities regulation is associated with higher financial market development, as measured, for example, by the size of the equity market and its IPO activity. Building on this data set, Hail and Leuz [2006] examine international differences in firms' cost-of-equity capital across 40 countries and their association with the quality of countries' legal institutions and securities regulation. They show that firms in countries with more extensive disclosure requirements, stronger securities regulation, and stricter enforcement mechanisms exhibit a significantly lower cost of capital. These effects are smaller when capital markets are more globally integrated, suggesting that market integration reduces the influence of countries' legal and regulatory institutions on firms' cost of capital. Frost, Gordon, and Hayes [2006] examine the link between required corporate disclosure at the exchange level and equity market development. They find that stock exchange disclosure requirements and enforcement are positively associated with market development using proxies such as market capitalization, number of listed firms, and market liquidity (see also Cumming, Johan, and Li [2011]).

Thus, the cross-country evidence is generally consistent with the notion that extensive disclosure regulation fosters capital-market development, improves market liquidity, and reduces the cost of capital. In these studies, strict enforcement tends to have an incremental association (even beyond proxies for disclosure rules or regulation) and not solely a complementary (or interactive) effect. However, it is important to recognize that the identification in these studies is cross-sectional and hence the results should not be interpreted causally. Moreover, it is difficult to estimate the precise nature of the relation between disclosure rules and enforcement as well as to isolate their joint effects from the effects of other complementary institutions.[54]

A different approach is to exploit both cross-sectional and time-series variation in disclosure regulation. Examples for this approach in securities regulation are Bhattacharya and Daouk [2002] and Christensen, Hail, and

[54] For this reason, it would be preferable to study changes in disclosure regulation while the other institutions are being held constant. This design amounts to the U.S. studies that we have discussed in sections 4.1 and 4.2. There are also studies examining changes in disclosure regulation in other countries. The insights and challenges of these studies are similar to those that we have already discussed for the United States. We, therefore, provide only a few examples in online appendix table 4.4.

Leuz [2016]. The former paper analyzes the enforcement of insider trading regulation around the world and provides evidence that the first enforcement action of the new law lowers firms' cost of capital. One concern noted by the authors is that the estimates are quite large (roughly 700 basis points). Aside from measurement problems with the cost of capital, a potential explanation is that the timing of the enforcement action is endogenous. It seems plausible that countries decide to take action when there are major shocks or scandals in their capital markets. These shocks may partially revert even in the absence of regulatory action, in which case the estimates are contaminated (i.e., overstated) by what in labor economics is called the "Ashenfelter dip." That is, even if the enforcement action had no effect on the cost of capital, the endogenous timing of the action in relation to the shock would result in estimates that suggest a decline.

Christensen, Hail, and Leuz [2016] examine changes in insider trading and transparency regulation exploiting two key EU directives on securities regulation. The EU setting allows the authors to analyze the same regulatory change across 27 EU countries implemented at different points in time. This setting and, in particular, the staggered implementation in many countries offer better identification of regulatory effects than a single regulatory event such as SOX. First, by measuring the regulatory effects in multiple countries at various points in time, the analysis is less susceptible to concurrent but unrelated economic shocks. Confounding shocks would have to be correlated with the implementation dates of two directives across several countries to induce the results. Second, the fact that the directives are designed at the EU level, but implemented at the country level in a staggered fashion, makes the study less prone to pick up market responses to preceding events that might have given rise to the directives in the first place.[55] Third, EU countries have limited discretion as to when to implement the new directives, which reduces concerns about the endogeneity of the timing and an "Ashenfelter dip." To even further tighten the identification, Christensen, Hail, and Leuz [2016] exploit the fact that a few EU countries have large *unregulated* markets, which are not or are to a lesser degree affected by the new directives. The existence of these markets allows estimating the regulatory effects within country and hence to control for concurrent *local* (country-specific) shocks.

Given these design features, this study yields plausibly causal evidence on the market liquidity effects of securities regulation, or, more specifically, insider trading and transparency regulation. But, in contrast to the causal estimates of the studies discussed in section 4.1 or 4.2, the estimates in Christensen, Hail, and Leuz [2016] are for the population of publicly traded firms in a large set of countries, rather than a subset of the population (e.g., firms around a size threshold as in Iliev [2010]). The study estimates that

[55] The fact that probably not all EU countries would have adopted these directives at this stage of their development mitigates selection at the country level because there is an element of imposition from the European Union.

market liquidity increases by roughly 10%, relative to predirective levels, illustrating that stricter securities and disclosure regulation can have substantial economic benefits. A drawback of the research design is that it can be used only for outcome variables that are measured over shorter intervals and are not anticipatory in nature. Moreover, the estimated magnitude of liquidity benefits is likely specific to the setting, including prior regulation, the particular directives, and countries' implementation choices. Thus, the generalizability of the estimates beyond the European Union is not obvious.

Consistent with this point, Christensen, Hail, and Leuz [2016] show that, even within the European Union, the liquidity benefits differ considerably from countries' prior regulatory conditions, as well as countries' ability and willingness to implement and enforce new rules. Thus, they document considerable heterogeneity in the treatment effects, even though countries adopt the same regulation and, perhaps more surprisingly, substantial hysteresis in regulatory outcomes. Countries with a stronger past track record in securities regulation exhibit stronger effects as a result of the new directives. Thus, imposing the same regulation on countries with disparate prior conditions can make countries diverge more, rather than harmonize their markets, which is potentially an important general insight. At a minimum, these findings highlight the important role of implementation and enforcement for regulatory outcomes, which is consistent with the enforcement theory formulated in Djankov et al. [2003] and Shleifer [2005].

In summary, international studies on disclosure regulation provide mostly cross-sectional evidence on capital-market benefits but also use a few settings that afford tighter identification. Overall, the evidence is arguably the strongest for market liquidity, which is perhaps not surprising as this proxy is well suited to capture information effects and has desirable features from a research-design perspective compared to other economic constructs that could be used to evaluate securities regulation (such as the cost of capital or Tobin's *Q*). International studies often emphasize the importance of enforcement (e.g., Frost and Pownall [1994], Bhattacharya and Daouk [2002], Hope [2003], La Porta, Lopez-De-Silanes, and Shleifer [2006], Coffee [2007], Jackson and Roe [2009], Cumming, Johan, and Li [2011]) and, more recently, the implementation of regulation (e.g., Christensen, Hail and Leuz [2013, 2016]). To date, we have little evidence on the direct and indirect *costs* associated with disclosure regulation and hence, as noted in earlier sections, we cannot say much about the net effects for firms or the economy as a whole.

One way to obtain evidence that, at least for some firms, the benefits of securities and disclosure regulation outweigh the associated costs is to study instances when firms voluntarily submit themselves to stricter regulation. There is a growing literature suggesting that U.S. cross-listings are a way for firms to overcome regulatory and institutional constraints in their home markets that, among other things, limit their ability to raise capital. The underlying idea is that firms in countries with weak institutions have difficulties in raising external finance because controlling insiders in these

environments cannot sufficiently assure outside investors that they will not expropriate them. Outside investors react to this commitment problem with price protection, which increases firms' cost of capital. This problem matters more to firms with growth opportunities that require outside finance. These firms have an incentive to seek alternative ways to reassure outside investors. Coffee [1999] and Stulz [1999] argue that a U.S. cross-listing makes it harder and more costly for controlling owners and managers to extract private control benefits and to expropriate outside investors. There are several potential reasons. First, U.S. securities laws give stronger rights to outside investors compared to most other countries and these rights are, arguably, more strictly enforced by the SEC and private securities litigation. By cross-listing in the United States, foreign firms subject themselves to these laws and their enforcement.[56] Second, a U.S. exchange listing requires foreign firms to provide certain disclosures (in Form 20-F) that are not necessarily required in firms' home countries. Finally, cross-listing likely increases the attention and monitoring by financial analysts and sophisticated U.S. capital-market participants such as institutional investors.

Consistent with this bonding hypothesis, studies by Reese and Weisbach [2002], Lang, Lins, and Miller [2003], Lang, Smith Raedy, and Yetman [2003], Doidge, Karolyi, and Stulz [2004, 2009], Doidge et al. [2009], Bailey, Karolyi, and Salva [2006], Hail and Leuz [2009], and Ammer et al. [2012] show that foreign firms with U.S. cross-listings raise more external finance and have higher valuations (Tobin's *Q*), a lower cost of capital, stronger earnings announcement reactions, greater analyst following, and larger U.S. investment than their foreign counterparts without U.S. cross-listings.

The relevance of this evidence for our purposes is that it illustrates that at least some firms voluntarily seek bonding opportunities and that, by providing such opportunities, stringent securities regulation and enforcement can confer substantial capital-market benefits on these firms. However, this evidence does not imply that *all* firms would benefit from such regulation. The recent debate about the decline in U.S. cross-listings after the passage of SOX illustrates that regulation can also become too onerous and costly even for firms that seek bonding (e.g., Zingales [2006]). In fact, changes in the number and flow of U.S. cross-listings can provide useful evidence on changes in the costs and benefits of U.S. securities regulation precisely because foreign firms have a choice (see also Piotroski and Srinivasan [2008], Doidge, Karolyi, and Stulz [2009], and our discussion in section 4.2).

For the same reason, and analogous to the voluntary disclosure literature, cross-listing studies face serious research-design challenges. The choice creates a standard selection problem, that is, the concern that

[56] The examples are the Foreign Corrupt Practices Act, Rule 10-b5, SEC enforcement actions, and U.S.-style class action lawsuits.

cross-listing studies select into samples of firms that differ fundamentally from non-cross-listed firms in observable and, in particular, unobservable ways and that these differences, rather than the cross-listing, drive the aforementioned results for Tobin's *Q*, the cost of capital, etc. More recent studies control at least for time-invariant, unobserved heterogeneity using firm-fixed effects and/or estimate selection models to account for time-variant unobservables (e.g., Doidge, Karolyi, and Stulz [2009], Hail and Leuz [2009], Ammer et al. [2012]). However, selection models require a valid instrument, and it is hard to satisfy the exclusion restriction in the cross-listing setting. Firm-fixed effects, in turn, are unlikely to be sufficient as the primary concern is that firms seek cross-listings precisely when they experience an expansion in their growth opportunities and need external finance (for evidence, see Doidge et al. [2009]). Thus, it is easy to confuse these effects with any bonding effects that stem from the cross-listing. Consistent with this concern, Hail and Leuz [2009] provide evidence that a substantial fraction of the documented valuation effects around U.S. cross-listings are attributable to contemporaneous revisions in growth expectations.

Thus, we still need more evidence from settings that allow us to estimate the causal effects of U.S. cross-listings. In addition, the sources of the cross-listing effects are still unclear. Extant studies do not isolate the mechanism. It is not obvious that the effects stem from better outsider protection, additional disclosure requirements, stricter enforcement, market monitoring, or the entire bundle (e.g., Leuz [2003]). For instance, Siegel [2005] raises doubts that the effects are attributable to the protection of outside investors by the U.S. legal system and SEC enforcement, pointing to substantial expropriation by Mexican insiders with impunity (see also Licht [2003]). However, it is important that the bonding hypothesis does not imply that all expropriation is deterred. It maintains that U.S. cross-listings are a way for firms to provide additional reassurance to outside investors. Hence, the relevant question is whether firms with U.S. cross-listings engage in less expropriation than comparable firms without cross-listings. That said, it is plausible that the documented effects stem from a combination of legal and market forces (e.g., Leuz [2006]).

4.4 OTHER STUDIES ON REAL EFFECTS OF DISCLOSURE REGULATION

In this section, we discuss (additional) *real-effects* studies of disclosure regulation as defined in footnote 9 and section 3. We discuss these studies separately because most of them have been conducted in different regulatory settings from those covered in sections 4.1–4.3. In fact, many examine disclosure regulation outside the capital markets (e.g., in medical or consumer settings). It would be impossible and beyond the scope of this paper to review all studies on any kind of disclosure regulation.[57] Thus, we

[57] For a related review on quality disclosure and certification, see Dranove and Jin [2010].

discuss a few examples to encourage researchers to look for ways to study disclosure regulation outside the traditional settings in order to generate new insights. In this regard, real-effects studies are of particular relevance because the notion that mandating disclosure induces desirable and/or discourages undesirable behavior by the disclosing party is an important motivation for transparency regulation in many areas, including corporate governance, consumer protection, health care, and food safety. The underlying idea of such regulation is to control behavior through "prices" (which respond to disclosure), rather than by directly stipulating "quantities" (of the behavior in question). However, there is still relatively little empirical evidence that speaks to this regulatory motivation and, more generally, to the potential real effects of disclosure regulation.

There are studies in accounting and finance that aim to establish the link between disclosure and corporate real effects, for example, changes in investment behavior. But, most of these studies examine firm-level (and hence largely voluntary) variation in disclosure choices, rather than regulatory changes or differences. We have already discussed this literature in section 3.2.1 and noted that voluntary disclosure studies can at best establish the existence of real effects, but their estimates have to be interpreted cautiously when it comes to the potential effects of disclosure regulation. Studies that examine real effects of disclosure *regulation* also tend to focus on firms' investment policies and behavior. We summarize key studies in online appendix table 4.5.

Before we discuss a few examples, it is important to note that, for conceptual reasons, investment effects around changes in disclosure regulation can occur for several reasons. Disclosure regulation also affects the cost of capital and hence an increase in investment could reflect a decline in the cost of capital because the lower hurdle rate essentially expands the set of positive NPV projects. Thus, if the goal is to show that greater transparency following a disclosure mandate facilitates monitoring, improves governance, and ultimately induces managers to make better investment decisions, then showing an *increase* in investment is not sufficient. In addition, it might be important to control for the cost-of-capital channel in order to isolate other real effects.

Consistent with this logic, studies typically analyze the *efficiency* of investment, rather than growth in investment. We distinguish two types of studies. First, there are studies exploiting cross-sectional variation in disclosure and transparency across countries, although the variation is often constructed from firm-level measures and hence does not necessarily reflect regulatory differences, but could also capture voluntary disclosures and reporting choices (e.g., Biddle and Hilary [2006], Francis et al. [2009], Bushman, Piotroski, and Smith [2011], Chen et al. [2011], Shroff, Verdi, and Yu [2014]). These studies find that greater transparency is associated

with higher investment efficiency.[58] Badertscher, Shroff, and White [2013] exploit cross-sectional variation in transparency that comes from greater public-firm presence in an industry. They show that, in industries with greater public-firm presence, investment by private firms is more responsive to industry investment opportunities. The results are consistent with the notion that the disclosure requirements for public firms improve the industry information environment, leading to more efficient investment and hence conferring a positive externality on private firms in the same industry.[59]

Second, there are studies examining real effects around regulatory changes. For instance, several studies analyze investment effects related to the introduction of SOX. We have already reviewed these studies in section 4.2, noting that the documented investment effects are likely not attributable to SOX (see also Coates and Srinivasan [2014]). Cheng, Dhaliwal, and Zhang [2011] do not focus on SOX per se, but exploit the introduction of disclosed internal control weaknesses to study investment effects. They document that firms disclosing internal control weaknesses exhibit investment inefficiencies that disappear in the years after the required disclosure, suggesting that mandated disclosure of control weaknesses remedies the inefficiencies. Cho [2015] exploits improvements in mandated segment reporting to analyze effects on firms' internal capital allocation decisions and finds positive effects in the capital allocation across segments after the mandate.

There are also a few real-effects studies in accounting that do not focus on investment. We provide three recent examples. First, Granja [2014] exploits a change in disclosure regulation for state-regulated banks to analyze the effects on bank behavior and more broadly the stability and development of the banking sector. During the National Banking Era in the United States, several types of banks coexisted, which allows for difference-in-differences estimation using national banks as a local control. Granja [2014] finds that, after the introduction of the disclosure regime, state-regulated banks' failure rates go down, capital ratios decrease (suggesting that they need to hold less capital to reassure depositors), rates on demand deposits decrease to about the same level as the rates of the national banks, and proxies for financial development increase. The research design makes use of the fact that states adopted the new regulation at different points in time, allowing for the introduction of state-time-fixed effects and triple

[58] As noted in section 3, these studies typically do not allow causal interpretations and provide primarily suggestive associations. The evidence is still important as it points to a potential relevance of this relation at the macro level.

[59] The paper includes firm-fixed effects to improve identification. While the latter helps with respect to time-invariant unobservables, it does not address concerns about the reflection problem, that is, the possibility that the results reflect industry-wide shocks, rather than public-firm presence. Nevertheless, the paper deserves credit not only for providing evidence on real effects, but also for being one of the few to explore externalities from disclosure regulation.

differences. As a result, the study takes an important step in the direction of showing a causal link between disclosure regulation, financial stability, and development. The evidence further suggests that, at least in this historical setting, ex post concerns that more transparency could facilitate runs in a crisis do not outweigh the stability effects from better transparency ex ante.

Second, Dyreng et al. [2015] examine how a nonprofit activist group used a subsidiary disclosure requirement to exert public pressure on U.K. firms having subsidiaries in tax haven countries and supposedly engaging in tax avoidance. They provide evidence using a difference-in-differences design that the campaign first pressured noncompliant firms to provide complete subsidiary information, revealing those in tax havens, and then used the information to publicly shame tax avoidance. Targeted firms reported higher effective tax rates following the public scrutiny. This study is interesting not only because it provides an example of how disclosure can be used to change corporate behavior, but also because it sheds light on a particular mechanism.

Third, Christensen et al. [2015] examine the requirement in the Dodd–Frank Act to disseminate (already publicly available) mine safety records through their financial statements and document that the inclusion of this information in financial reports decreases mining-related citations and injuries, as well as mine productivity. The results illustrate that the dissemination of safety records through financial reporting can have real effects with respect to mine safety.

It is important to note that more extensive disclosure regulation could also have *negative* (or nondesirable) real effects. For instance, if the private costs of the mandate outweigh the benefits to firms, we expect firms to engage in avoidance strategies, which may amount to real effects. Consistent with this notion, Gao, Wu, and Zimmerman [2009] provide evidence that the size-based exemptions built into SOX induce some firms to remain below the cutoff, leading them to take real actions that inhibit firm growth, such as investment cuts. In this case, however, the real effects stem from the size-based cutoff, not the required disclosure per se, because, without such a cutoff, firms would likely not have incentives to cut investment. That said, the notion that firms' avoidance strategies can have real effects seems fairly general, but has not been explored much in the literature.

In our view, the relatively small number of studies in accounting and finance examining real effects of disclosure regulation reflects the difficulties in finding a setting in which one can cleanly identify such effects. One important difficulty is the limited (direct) observability of corporate behavior. For this reason, it might be fruitful for researchers to also study disclosure mandates outside traditional corporate settings. Settings that have been studied include restaurant hygiene, health care, environmental disclosures, food labels, and consumer lending (e.g., Dranove et al. [2003], Jin and Leslie [2003], Bennear and Olmstead [2008], Stango and Zinman [2011], Lu [2012], Kolstad [2013], Christensen, Floyd, and

Maffett [2014]).[60] We also summarize these examples in online appendix table 4.5.

An important example for the real effects of disclosure regulation outside a capital-market setting is the study by Jin and Leslie [2003]. They analyze the effects of a mandate to display hygiene quality grade cards in restaurant windows on restaurants' hygiene investments. The underlying economic question is whether mandating disclosures to consumers about the quality of firms' products causes firms to improve the quality of their products. Jin and Leslie [2003] exploit the staggered introduction of a Los Angeles County ordinance in 1997 after a TV report showed unsanitary kitchens. The authors show that the display of hygiene grade cards causes restaurant health inspection scores to increase, consumer demand to become sensitive to changes in restaurants' hygiene quality, and the number of foodborne-illness hospitalizations to decrease. The finding for consumer demand is akin to investor responses in capital-market settings. The real effect is the reduction in foodborne-illness hospitalizations because it is consistent with restaurant investments in hygiene and food quality. However, the latter is not directly observed. It is, therefore, important for this interpretation that the authors provide evidence that the improvement in hospitalizations is *not* fully explained by consumers switching from poor-hygiene restaurants to good-hygiene restaurants, and hence consumer behavior. This issue would not arise if they had data on restaurant investments (or changes in food quality at the restaurant level). More generally, this discussion illustrates that real effects can take place through consumer or investor responses, which makes their identification challenging, in particular if the researcher has to rely on outcomes that could also reflect demand effects by consumers or investors (e.g., restaurant substitution).

Studies of disclosure mandates outside capital-market settings generally illustrate that consumers are sensitive to the information provided, that this response is critical to firms' responses, and that firms adjust product quality. But, the effects can be more complicated. For instance, Lu [2012] shows that, when product quality is multidimensional, firms reallocate their effort and investments toward measured (or disclosed) dimensions and away from unmeasured ones. The problem is akin to the multitasking problem in Holmström and Milgrom [1991]. Thus, the net effect on product quality is not obvious. Similarly, Dranove et al. [2003] provide evidence that health care report cards may give doctors and hospitals incentives to decline treatment of high-risk patients (see also Cutler, Huckman, and Landrum [2004]). Thus, disclosure regulation can also have thorny and unintended consequences, including real effects.[61] However, what constitutes

[60] See also the review of the literature on quality disclosure by Dranove and Jin [2010].

[61] Another example is the "licensed to be biased" effect in Loewenstein, Cain, and Sah [2011]. Dranove and Jin [2010, p. 959]) state that "it is difficult to state with confidence that disclosure in such important sectors as health care, education, or finance has unambiguously helped consumers."

an unintended effect can also differ across settings. In health care, screening or selecting patients based on health status to improve reporting outcomes is likely a major concern. But, an analogous effect in a corporate setting, for instance, high-quality auditors refusing "treatment" of relatively risky clients, could be viewed quite differently. Thus, we have to consider the context when interpreting results.

At this point, the literature highlights that proper measurement of the relevant information is critical for disclosure to have the intended incentive effects and hence that the reporting system matters. For instance, to mitigate the unintended screening effects documented in Dranove et al. [2003], it is important that the reporting system adjusts for differences in incoming patients or, alternatively, that consumers separately obtain information about these differences. Thus, there are many interesting reporting problems that warrant further investigation and to which accounting researchers should be able to contribute. The existing evidence is fairly limited and more studies are needed to better understand the many possible (positive and negative) real effects and, more generally, what the costs and benefits are when using disclosure regulation as a device to induce desirable and discourage undesirable behavior.

5. Evidence on the Economic Effects of Mandated Reporting Standards

In this section, we review empirical studies on the economic effects of mandated reporting standards. We focus on mandates of complete sets of reporting standards, for example, a requirement to report financial statements prepared under IFRS, as opposed to individual rules, such as the adoption of a new standard on pension accounting within U.S. GAAP.[62] Unfortunately, changes in the entire set of standards are relatively rare, which is why most studies focus on IFRS adoption. The global switchover to mandated IFRS reporting represents a profound change in the "rules component" of reporting regulation, possibly the largest in accounting history to date. Studying the economic consequences of this change is clearly of fundamental interest.

In addition, many studies exploit IFRS adoption as an "exogenous" shock to reporting regulation, which is something that is generally hard to find. Therefore, this section not only intends to give an overview of key empirical findings with respect to mandated adoption of IFRS, but also intends to discuss challenges that researchers face when using the "IFRS laboratory" for research-design purposes, as well as when studying the economic effects

[62] Early studies in the international accounting literature often focus on *voluntary* adoptions of entire sets of accounting standards (e.g., IAS or U.S. GAAP). See, for example, Harris and Muller [1999], Leuz and Verrecchia [2000], Leuz [2003], and Daske [2006]. We do not systematically review these studies here. See Soderstrom and Sun [2007] for a review.

of changes in reporting regulation more generally. In this research-design discussion, we come back to themes from section 2 about the identification challenges in regulatory studies, the limitations of many empirical proxies for reporting and disclosure quality, including their slow-moving nature, as well as concerns about the existence of complementarities between various institutions in the economy, which make it difficult to empirically isolate the economic effects of reporting standards.

We begin with a short summary of the "reporting incentives view" on financial reporting standards, which has been quite influential in the international accounting literature and, in our view, is important for the interpretation of many studies on the effects of reporting standards. We then provide a brief overview of key findings. This discussion complements the literature reviews on IFRS adoption by Soderstrom and Sun [2007], Brüggemann, Hitz, and Sellhorn [2013], and the Institute of Chartered Accountants of England and Wales (ICAEW [2014]). Next, we provide our interpretation of the empirical evidence and what we can say about the economic effects of reporting standards and, in particular, the effects of IFRS adoption. We conclude this section with a discussion that highlights that reporting standards are part of a larger institutional system. This "system view" of reporting standards, which characterizes the work on New Institutional Accounting, is very helpful, if not critical, for interpreting the results of studies on mandated reporting standards.

5.1 AN INCENTIVES VIEW ON REPORTING REGULATION

There is an important part of the international accounting literature that focuses on the role of reporting incentives, rather than reporting *standards* (or stated rules), as a fundamental determinant of observed disclosure and reporting *practices* across firms and countries (e.g., Ball, Robin, and Wu [2003], Leuz [2003], Leuz, Nanda, and Wysocki [2003], Ball and Shivakumar [2005], Burgstahler, Hail, and Leuz [2006], Hail, Leuz, and Wysocki [2010a]). The incentives view starts with the notion that reporting standards afford firms (or managers) substantial reporting discretion because the application of the standards involves considerable judgment. As a practical matter, a finite set of standards cannot anticipate all future contingencies that firms may face when applying the standards in the future. New circumstances, events, and transactions may arise, in which case standards need to be interpreted. Furthermore, reporting standards deliberately give discretion to managers because they intend to elicit managers' private information and hence applying the standards involves subjective assessments of the future.[63] The key point is that incentives shape how managers use the discretion allowed within the standards. Note that this is not just a matter of enforcement. Even if enforcement were perfect, standards would provide

[63] A good example is a standard that requires companies to set up a warranty reserve, which, in essence, elicits private information about future warranty risks.

reporting discretion for good reasons, and, as long as there is discretion, reporting outcomes (e.g., the properties of earnings) are heavily influenced by incentives and not solely determined by standards. This view is fundamental to accounting and has many antecedents going back at least to Watts and Zimmerman [1986].

Reporting incentives are shaped by many factors, including a country's legal institutions, the strength of the enforcement regime, capital-market forces, product-market competition, a firm's governance structure, and its operating characteristics. While the extent to which we have supporting evidence differs across these factors, many empirical studies clearly indicate the importance of managerial reporting incentives for observed reporting and disclosure practices (e.g., Ball, Kothari, and Robin [2000], Fan and Wong [2002], Leuz, Nanda, and Wysocki [2003], Haw et al. [2004], Burgstahler, Hail, and Leuz [2006]). Of particular relevance are studies showing that, even when firms are subject to the same accounting standards, reporting practices differ considerably across firms and countries (e.g., Ball, Robin, and Wu [2003], Ball and Shivakumar [2005], Burgstahler, Hail, and Leuz [2006], Lang, Smith Raedy, and Wilson [2006], Daske et al. [2013]).

Thus, the reporting incentives view predicts considerable heterogeneity in firms' reporting practices (even when adopting the same standards) and further implies that changing the standards is likely to have a limited effect on reporting practices even with strict enforcement. These insights are crucial for the interpretation of the IFRS evidence, which we discuss next in more detail.

5.2 EXPECTED ECONOMIC EFFECTS OF IFRS ADOPTION AND IDENTIFICATION CHALLENGES

The case for the IFRS adoption is generally made on the basis of improvements in reporting standards, comparability benefits for firms from different countries, and/or cost savings for investors and firms in using IFRS (see Hail, Leuz, and Wysocki [2010a], for a more extensive discussion). As discussed in section 3, theory suggests that high-quality reporting can have significant capital-market benefits, and extant empirical evidence indicates that capital markets and investors reward higher transparency and high-quality reporting. Similarly, more comparable reporting could make it easier and less costly for investors and other stakeholders to compare firms, which, in turn, has the potential to make reporting more useful, even if reporting quality does not change. As discussed in Hail, Leuz, and Wysocki [2010a], improvements in comparability could also have significant capital-market benefits. In addition, better and more transparent reporting could have significant real effects on managerial behavior (e.g., improve investment decisions). Finally, widespread IFRS adoption in many countries around the world could bring cost savings to firms and investors because firms and investors could use a single set of reporting standards.

However, the key question is whether IFRS adoption delivers these benefits.[64] Thus, most empirical studies focus on capital-market outcomes and to a lesser extent on real effects or direct reporting outcomes. For all studies, the key challenge is to ascertain whether the switch to a new (and harmonized) *set of reporting standards* indeed generates the observed outcomes. In this regard, there are four major challenges. They are not fundamentally different from those discussed in section 2.2, but, given the large literature examining mandatory IFRS adoption, we think that it is important to highlight these challenges and several specific institutional details.

First, the widespread and near simultaneous adoption of mandatory IFRS reporting by many countries in 2005 makes empirical analyses vulnerable to confounding effects from *unrelated*, but concurrent shocks. It is possible, if not likely, that many other regulatory, technological, and market shocks occurred around the same time as the switch to IFRS. The potential myriad of other institutional and market changes makes it difficult to isolate the effects of IFRS adoption. In this regard, it is useful for studies to exploit countries that have switched to IFRS more recently in order to have more variation in the adoption dates.

Second, many jurisdictions consciously adopted IFRS as a package with other regulatory reforms to their reporting and financial systems. These coordinated reforms were likely made recognizing that reporting standards and other institutions are complementary and support each other. For instance, Christensen, Hail, and Leuz [2013] point out that some countries made major institutional changes in the enforcement *of financial reporting* around the time of IFRS adoption. This identification challenge is even more severe than the first one because it implies that the timing of these other reforms lines up endogenously with the timing of the change in reporting standards. As a result, many studies can at best identify the joint effects of the entire bundle of changes to reporting regulation, rather than the effect of the change in reporting standards, that is, IFRS adoption.

Third, the mandate to report under IFRS generally applies to almost all listed firms in the economy. The widespread adoption within a given country makes it difficult to find counterfactuals or unaffected control groups. In some instances, there were exemptions. For example, in Germany firms reporting under U.S. GAAP were given a later transition date. Listed firms that do not provide consolidated financial statements are often exempted as well. These firms can and probably should be used as controls for unrelated economic shocks and as counterfactuals. But, as discussed in section 4, many concerns remain because these control firms are "sufficiently

[64] While our discussion focuses on the benefits consistent with most empirical studies, we note that similar identification challenges arise on the cost side. In fact, there may be additional challenges. For instance, it is plausible that firms postpone unrelated changes to their accounting systems when they know that they have to switch to IFRS. Thus, an estimate of the cost effects around IFRS adoption would likely capture the pent-up demand for other accounting system changes.

special" that it is not clear that unrelated economic shocks affect them in the same way as IFRS-adopting firms, and hence the parallel-trends assumption is a concern. Moreover, the temporary nature of many exemptions makes it difficult to assess the long-term effects of IFRS reporting, which requires comparable control firms over an extended time period.

Finally, the fact that countries adopted the same (or a single) set of reporting standards implies that we cannot exploit cross-sectional differences in the standards that are being adopted, for instance, to learn how certain properties of the reporting standards relate to the observed outcomes. At best, we can use prior differences between local GAAP and IFRS (e.g., Bae, Tan, and Welker [2008], Daske et al. [2008], Tan, Wang, and Welker [2011]). However, as the properties of local GAAP are likely highly correlated with other institutional features of the respective country, it is unlikely that this cross-sectional approach captures only differences in the reporting standards. It likely splits implicitly on many other institutional features.

5.3 OVERVIEW OF KEY FINDINGS IN IFRS STUDIES

In this subsection, we provide an overview of the key findings in the literature on IFRS reporting. We generally discuss only a few examples, but provide a more extensive overview in the online appendix (tables 5.1–5.4). We first discuss evidence on changes to the properties of firms' disclosures and reported numbers as a result of IFRS adoption, then evidence on capital-market effects, followed by evidence on nonmarket and real effects.

5.3.1. Changes in Reporting Properties and Financial Disclosures. Studies examining changes in reporting and disclosure practices around changes in reporting standards draw on a variety of settings, not just IFRS mandates. Some of the evidence stems from voluntary IFRS or earlier IAS adoptions. Other studies exploit the fact that U.S. cross-listed, foreign firms report under local GAAP or IFRS, but also have to provide U.S. GAAP reconciliations, which allows for comparative studies holding the firm constant. More recently, studies examine changes in reporting and disclosure practices around mandated IFRS adoption. We provide an overview of these studies in online appendix table 5.1. Overall, these studies document substantial differences in reporting properties across firms using different reporting standards. However, as we discuss next, we need to exercise caution in attributing or interpreting these differences.

For example, Barth, Landsman, and Lang [2008] find that firms voluntarily applying IAS exhibit less earnings management, more timely loss recognition, and more value relevant accounting amounts than matched firms applying non-U.S. domestic standards. They also show that these reporting differences do not exist prior to IAS adoption and generally arise in the postperiod. However, as the evidence stems from voluntary adoptions, it is difficult to attribute the reporting effects to the change in reporting standards. Given the selection problem, the reporting effects could also stem from the underlying factors that gave rise to the change in

reporting standards in the first place. Illustrating this point, Christensen, Lee, and Walker [2015] compare changes in reporting properties for voluntary and mandatory IFRS adopters in Germany. They examine similar reporting properties to Barth, Landsman, and Lang [2008] and find that improvements are confined to voluntary adopters, suggesting that selection plays an important role in the findings.

Using U.S. cross-listed firms, Gordon, Jorgensen, and Linthicum [2010] and Barth et al. [2012] provide evidence that IFRS and U.S. GAAP accounting amounts have similar properties, except with respect to value relevance. The former study conducts this comparison holding the firm constant. The latter study uses U.S. firms as a benchmark and hence other institutional factors that differentially affect foreign and U.S. firms (such as weaker SEC enforcement for foreign firms) can still drive or play into the results. Barth et al. [2012] also find that comparability with U.S. GAAP numbers increases when firms are mandated to switch from local GAAP to IFRS.

In addition, empirical studies analyze changes in reporting properties after mandatory IFRS adoption. Most of these studies document insignificant or even adverse changes in reporting properties following IFRS adoption (see, e.g., Capkun, Collins, and Jeanjean [2012], Ahmed, Neel, and Wang [2013], Christensen, Lee, and Walker [2015]; but see also Landsman et al. [2012]). In contrast, studies based on analyst forecast dispersion and forecast errors, which can be viewed as indirectly measuring changes in reporting quality, suggest that financial analysts receive better information after IFRS adoption (e.g., Byard, Li, and Yu [2011], Tan, Wang, and Welker [2011], Horton, Serafeim, and Serafeim [2013]). An interesting feature of analyst studies is that IFRS adoption should differentially affect foreign and domestic analysts following the same firm, which can be used to isolate reporting effects from other concurrent shocks that similarly affect foreign and domestic analysts.

In sum, the evidence on changes in reporting properties (or quality) after IFRS adoption is fairly mixed. Results differ substantially across voluntary and mandatory adoptions. Moreover, studies document considerable heterogeneity in firms' reporting practices across firms or countries even after IFRS adoption, which is consistent with on-going differences in reporting incentives. Both findings underscore the difficulty in attributing the differences in observed properties to the accounting standards. To improve attribution to the reporting standards, several studies show that the results are stronger in countries for which prior local GAAP were more distant from IFRS (e.g., Byard, Li, and Yu [2011], Tan, Wang, and Welker [2011]). These cross-sectional splits are useful, but, as discussed in section 5.2, they likely do not isolate the effect of accounting standards due to institutional clusters.

5.3.2. Capital-Market Outcomes. As mentioned earlier, most studies focus on the espoused capital-market effects of a mandatory switch to IFRS. These studies examine a vast array of capital-market outcomes. To

provide a few examples, studies find positive abnormal stock returns during key events leading up to IFRS adoption (Armstrong, Guay, and Weber [2010]), as well as, around IFRS adoption, an increase in market liquidity and a decline in firms' cost of capital (Daske et al. [2008, 2013], Florou and Kosi [2015]), an increase in stock price informativeness (Beuselinck et al. [2009]), larger foreign portfolio investments in firms domiciled in IFRS countries (Brüggemann et al. [2011], DeFond et al. [2011], Beneish, Brian, and Lombardi [2012]), and a reduction in home bias among U.S. investors (Khurana and Michas [2011], Shima and Gordon [2011]). However, a number of recent studies find evidence on potentially costly changes in debt contracting around IFRS adoption (e.g., Ball, Li, and Shivakumar [2015], Chen, Harford, and Lin [2015], Brown [2016]). We summarize key studies in online appendix table 5.2.

Except for recent debt contracting studies, the evidence overwhelmingly suggests that mandatory IFRS adoption is associated with significant capital-market benefits, especially in equity markets. But, how should we interpret this evidence? In light of the strong conceptual links between better reporting and certain capital-market outcomes such as market liquidity, one interpretation is that mandatory IFRS adoption has improved financial reporting, which, in turn, drives the documented capital-market effects, and, more generally, that mandating high-quality reporting standards such as IFRS yields considerable capital-market benefits. However, this interpretation is premature. As we discuss next, it is not clear that the capital-market outcomes are indeed attributable to IFRS adoption, that is, the mandated change in reporting standards. In our view, we have to be cautious in how we label and describe the findings, in particular, when using language that suggests causal effects. The capital-market effects are best described as effects that occur *around* the time of or *after* IFRS adoption, but they are not necessarily effects *of* IFRS adoption.

This interpretation is more appropriate because the vast majority of capital-market studies on IFRS relies on an indicator variable marking the post-IFRS adoption time period, rather than specific outcomes of IFRS reporting. As discussed in sections 2 and 5.2, regime change analyses face a number of serious research-design and identification challenges. Moreover, there are a number of specific findings in the aforementioned studies that make us skeptical that the results are indeed attributable to the switch to IFRS reporting.

For example, Daske et al. [2008] analyze capital-market effects around mandatory IFRS adoption separately for firms that switch to IFRS for the first time and for firms that have already switched to IFRS voluntarily prior to the mandate. As the latter group of firms already reports under IFRS when the mandate becomes effective, it should not exhibit capital-market outcomes that stem from the change in accounting standards. But, Daske et al. [2008] show that voluntary adopters exhibit larger capital-market effects after IFRS becomes mandatory than first-time mandatory IFRS adopters. This result, which holds for several proxies, is difficult to

explain with capital-market effects of the standards per se. For this reason, Daske et al. [2008] caution that their results should not be attributed solely or even primarily to IFRS adoption. Christensen, Hail, and Leuz [2013] confirm this pattern for market liquidity effects of voluntary and mandatory adopters. But there are also studies showing that first-time mandatory adopters exhibit larger effects than voluntary adopters (e.g., Byard, Li, and Yu [2011], Tan, Wang, and Welker [2011]).

It is, of course, possible that IFRS reporting involves significant learning and therefore it may take time for the capital-market effects to materialize. Most firms that adopted IFRS voluntarily switched shortly before the mandate and hence, if there are learning effects, these could show up around or after the mandate (Daske et al. [2008]). Furthermore, it is conceivable that widespread mandatory adoption by all listed firms in an economy confers positive comparability effects on firms that already follow IFRS, that is, the voluntary adopters. To gauge this possibility, Daske et al. [2008] analyze whether *voluntary* adopters exhibit larger capital-market effects around the mandate conditional on how widespread voluntary adoption was prior to the mandate. The idea is that any comparability effects for voluntary adopters should be stronger in industries where fewer firms have previously reported under IFRS and hence the mandate creates more peers. The evidence in Daske et al. [2008] on this matter is inconclusive and more research is warranted.

More generally, we highlight that most IFRS studies focus on the effects on *individual* firms by estimating firm-level regressions. This approach does not capture externalities or market-wide benefits that arise over time, as more and more firms adopt a set of reporting standards.[65] However, information spillovers, comparability (or network) effects, and other market-wide effects that could give rise to positive externalities are crucial for the justification of mandatory reporting regulation in the first place. We recognize that the identification of such market-wide effects and externalities is even more difficult than the identification of direct economic consequences on individual firms. But, at the same time, we need such evidence if we want to assess the desirability of reporting regulation and conduct cost–benefit analyses of changes in the reporting standards.

5.3.3. Going Beyond Capital-Market Effects. While most IFRS studies focus on capital-market effects, there is a growing body of work that goes beyond capital-market effects, for instance, analyzing real effects with respect to corporate behavior. This research can be divided into two categories: (1) studies that are primarily interested in the IFRS mandate and its economic consequences, and (2) studies that exploit the change to IFRS as

[65] The country-month-level analysis in Daske et al. [2008, section 5] is an example of a first step in this direction. They aggregate market liquidity at the country level and then analyze monthly liquidity changes in relation to changes in the fraction of firms reporting under IFRS. This analysis can capture market-wide effects, yet is closely tied to the rollout of IFRS (and other concurrent changes to the reporting system) in a country.

an event that provides exogenous variation, but are primarily interested in real effects of reporting. We tabulate key examples for both types of studies and their results in online appendix table 5.3. A common focus, in these studies, is on corporate investment using proxies such as the investment–cash flow sensitivity and the sensitivity of investment to growth opportunities. Other outcomes are cross-border investment (or foreign direct investment), changes in corporate governance, trade in real goods, and labor mobility. As online appendix table 5.3 illustrates, this strand of the literature is still relatively young and comprises far fewer studies than the strand analyzing capital-market effects.

Examples for studies in the first bin are Biddle et al. [2011], Schleicher, Tahoun, and Walker [2010], and Chen, Young, and Zhuang [2013]. All three studies examine the efficiency of firms' investment decisions, following mandatory IFRS adoption. These studies document improvements in investment efficiency after IFRS adoption, based on different proxies and approaches. Two studies also document substantial heterogeneity in the effects around IFRS, that is, stronger effects for smaller firms, for insider economies (Schleicher, Tahoun, and Walker [2010]) and for countries in which local GAAP and IFRS diverge more (e.g., Biddle et al. [2011]). An entirely different example is Bloomfield et al. [2015], which examines labor market effects after the EU harmonized accounting and auditing standards. They show that regulatory harmonization, including IFRS adoption, increases cross-border migration of accounting professionals relative to other matched professionals.

Examples for studies in the second bin are Hail, Tahoun, and Wang [2014] and Shroff, Verdi, and Yu [2014]. The latter paper investigates whether a richer and more transparent information environment allows multinational corporations to better monitor and evaluate their subsidiaries' investment decisions. To examine this question, Shroff, Verdi, and Yu [2014] use IFRS adoption as a significant shift in the quality of the information environment and find that the sensitivity of investment to growth increases for subsidiaries located in countries that mandate IFRS. An important assumption for this paper, as well as other papers using IFRS as an "exogenous shock," is that IFRS adoption leads to a major change in corporate reporting, the level of information asymmetry or the information environment, and that this change is exogenous with respect to the outcome of interest. The former is not obvious in light of the capital-market evidence on IFRS adoption reviewed in section 5.3.1. However, as studies in the second bin do not necessarily care about IFRS adoption per se, it is not important for them to separate the change in standards from other concurrent changes in the *reporting* system, as long as the bundle jointly provides a major exogenous shock to the information environment.

Nevertheless, real-effects studies face serious identification challenges and often follow techniques from capital-market studies. For instance, Biddle et al. [2011] use voluntary IFRS adopters as a benchmark to mitigate concerns about concurrent, but unrelated, economic shocks and

institutional changes. However, unlike some of the capital-market outcomes, the proxies in real-effects studies (e.g., the investment–cash flow sensitivity) are relatively slow moving and typically have an annual frequency, which makes disentangling unrelated economic shocks and institutional changes (such as a change to the insider trading regulation in the European Union) even more difficult. In addition, the validity of many investment efficiency proxies has been widely debated and criticized (Kaplan and Zingales [1997], Kaplan and Zingales [2000], Whited and Wu [2006], Hadlock and Pierce [2010], Bushman, Smith, and Zhang [2012]). In order to show real effects, studies generally need a benchmark for optimal investment behavior. But, deviations from optimal investment behavior can go in both directions, that is, there can be over- and underinvestment, which makes identification particularly challenging. Perhaps this is an area where structural work could make some headway.

We encourage future research to go beyond the capital-market effects of reporting mandates and to consider other markets and audiences, as well as real effects. Reporting mandates are of particular interest if we want to learn more about alternative measurement regimes and their effects on corporate or managerial behavior, as suggested, for instance, by Kanodia and Sapra [2016]. In addition, we note that we have few academic studies on the costs of IFRS adoption, which is obviously critical for a cost–benefit analysis.[66] Aside from studies on the implementation costs, we need studies on indirect (and less obvious) costs of mandated changes in reporting standards (e.g., with respect to existing contracts, managerial time, and behavior).

5.4 HETEROGENEITY IN THE FINDINGS OF IFRS STUDIES AND THE INTERPRETATION OF THE FINDINGS

A pervasive finding in studies on mandatory IFRS adoption is that the results exhibit considerable cross-sectional heterogeneity. That is, the observed economic outcomes around IFRS adoption vary greatly across countries, institutional regimes, and firms. As discussed in section 5.1, this heterogeneity should not be surprising, given that IFRS (like any other set of reporting standards) affords firms with substantial discretion. Such reporting discretion combined with a myriad of economic incentives faced by firms and managers is expected to lead to a large variation in financial reporting practices, which, in turn, should result in heterogeneous economic outcomes. As such the IFRS literature largely confirms that adopting a single set of reporting *standards* is not sufficient to obtain convergence in reporting *practices*.

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[66] There is some evidence on the costs of IFRS adoption in commissioned reports (e.g., ICAEW [2014]). Kim, Liu, and Zheng [2012] and De George, Ferguson, and Spear [2012] examine changes in audit fees around IFRS adoption. Ball et al. [2015] and Brown [2014] examine changes to debt contracting around IFRS adoption.

But, the cross-sectional heterogeneity in economic outcomes around IFRS adoption also matters for two more reasons. First, it plays an important role for the interpretation of the findings and, in particular, the question of whether the documented effects are indeed attributable to the mandated switch in reporting standards. Second, even if the effects are attributable to IFRS adoption, the differential impact of changes in reporting standards across different institutional regimes serves as a reminder that reporting standards do not operate in isolation and depend upon and interact with other institutions in the economy. For the remainder of section 5, we discuss these two aspects. We also highlight cross-sectional (or conditional) outcomes of various empirical studies in online appendix tables 5.1–5.3.

Beginning with Daske et al. [2008], studies generally find that the observed capital-market outcomes surrounding the mandatory introduction of IFRS are weaker, or even nonexistent, in countries with weaker legal regimes and reporting incentives. In many instances, they also find interaction effects between the strength of countries' legal institutions and the differences between prior local GAAP and IFRS. For instance, Byard, Li, and Yu [2011] find that analyst forecast errors and forecast dispersion decrease around mandatory IFRS adoption, but only in countries with strong legal regimes that also have large differences between local GAAP and IFRS.

One potential and fairly common interpretation of this evidence is that mandatory IFRS reporting brings significant capital-market benefits as long as countries have strong legal systems and other institutions that ensure the new standards are properly applied and enforced.[67] However, there are alternative explanations. The clustered timing of IFRS adoption makes the analysis vulnerable to concurrent, but unrelated economic shocks and institutional changes. If such confounding factors are correlated with the strength of countries' legal regimes and other institutional variables that are used in the cross-sectional splits, then these confounding factors could be responsible for the observed heterogeneity in the capital-market effects and the results would essentially be spurious. Christensen, Hail, and Leuz [2013] highlight this issue and specifically point to the European Union, which had a number of concurrent capital-market reforms (e.g., to insider trading regulation) that are unrelated to IFRS adoption, yet overlap in timing and have the potential to affect capital-market outcomes, such as liquidity.

It is generally difficult to rule out that other institutional changes that are unrelated to financial reporting drive the observed economic effects around IFRS adoption. One strategy is to exploit the differential timing of the various institutional changes and to specifically tie the documented outcomes to the timing of IFRS adoption. Toward this end, Daske et al. [2008]

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[67] Viewed more broadly, the evidence is in line with the notion of institutional complementarities in that the effects of IFRS adoption depend on other elements in countries' institutional infrastructure (see section 5.5).

and Christensen, Hail, and Leuz [2013] exploit variation in firms' fiscal year ends, which determine when a particular firm has to follow the new set of reporting standards. In contrast, other reforms that are unrelated to financial reporting typically apply to all firms from a certain calendar date, and hence the timing of their effects should not be related to firms' fiscal-year ends. However, this strategy requires outcome variables that can be measured at a relatively high frequency *and* are not anticipatory (Christensen, Hail, and Leuz [2013]). Outcome variables that are available only with an annual frequency make it hard to exploit the variation in firms' fiscal-year ends. Anticipatory outcome variables, such as those based on prices, are not suitable for this identification strategy as they are not expected to follow a fiscal-year-end pattern.

Using this approach Christensen, Hail, and Leuz [2013] show that unrelated institutional reforms in the European Union cannot explain the changes in market liquidity around IFRS adoption. The observed liquidity changes are still present when controlling for country-quarter fixed effects, which should absorb the effects of reforms like the Market Abuse Directive and, in turn, suggests that the liquidity changes are related to financial reporting changes. However, this finding does not imply that the changes in market liquidity are necessarily related to or driven by IFRS adoption. As noted earlier, countries have made other changes to the financial reporting system that are meant to support or complement IFRS adoption. For instance, countries could use the introduction of IFRS as an opportunity to improve enforcement of financial reporting. In this case, capital-market outcomes reflect the joint effect of the bundled changes to the financial reporting system. For instance, if the switch to IFRS and the change in enforcement are complements, then the two changes reinforce each other. It is also possible that the effects are simply additive, that is, each element contributes independently. The bundling of *reporting* reforms is very challenging from an identification perspective. Importantly, a strategy using fiscal-year ends alone cannot isolate the effects of IFRS adoption because the other changes to the reporting system also affect firms' reporting practices and hence the timing of their potential effects should also be related to firms' fiscal-year ends.

Consistent with the concern about bundled changes to the financial reporting system, Christensen, Hail, and Leuz [2013] show that the liquidity effects around IFRS introduction are limited to five EU countries that concurrently made substantive changes in reporting enforcement. There is little evidence of liquidity benefits in IFRS countries without substantive enforcement changes even when they have strong legal and regulatory systems. These findings suggest that the observed outcomes in EU countries are not attributable solely to the change in standards. In fact, the weight of evidence suggests that IFRS adoption had little, if any, stand-alone effects on market liquidity.

Similarly, Brown, Preiato, and Tarca [2014] examine changes in analyst forecast properties around mandatory IFRS adoption and find no effect

after the mandate once the effects of enforcement are controlled for. Both studies raise the possibility that capital-market effects around IFRS adoption are entirely driven by enforcement, rather than changes in the reporting standards. However, it is important to note that, in both studies, the enforcement results are identified only by cross-sectional variation. Other institutional changes that are related to financial reporting and also correlated with the enforcement proxies could equally explain the results. Moreover, these studies cannot rule out that there is an interaction effect between the standards and the enforcement changes, as acknowledged in Christensen, Hail, and Leuz [2013] and discussed in Barth and Israeli [2013]. Disentangling the two effects amounts to asking questions about the following counterfactuals: Would the observed capital-market effects have been substantially smaller (or larger) if countries had maintained their local GAAP, yet the other reforms to the reporting system (e.g., enforcement) still occurred? What would the effects have been if countries had adopted a different set of reporting standards (e.g., U.S. GAAP)? At present, the literature cannot answer these important questions.

We conclude that, generally speaking, studies do not present causal evidence of how mandatory IFRS adoption, that is, the change in reporting standards, affects various reporting and economic outcomes. The existing evidence on the effects of reporting standards is still fairly limited and there are many research opportunities, especially if the design allows for causal inferences. The evidence generally is stronger when we broaden the perspective and view the study as examining the combined effects of institutional reforms (or bundles). But, if we go down this path, it is important to be explicit and specific about the institutional changes.

5.5 THE LINKS BETWEEN REPORTING AND OTHER NONREPORTING INSTITUTIONS

While evidence on the impact of IFRS is still evolving, proponents of IFRS often argue that uniform global standards are preferable to disparate, and in many cases competing, standards across markets, for example, because uniform standards reduce transaction costs. However, it is not obvious, nor has it been empirically documented, that one set of mandated global accounting regulations, let alone the specific standards that comprise IFRS are superior to other standards and that uniform standards are preferred to other possible scenarios (see also Dye and Sunder [2001]). In fact, applying insights from institutional economics suggests that it is far from clear whether IFRS are superior, or effective, in countries whose other institutions are not primarily geared toward supporting public equity and bond markets, and it is particularly doubtful in countries that lack complementary institutions to support the effective application and enforcement of uniform global standards (see also Ball [2001, 2006], Hail, Leuz, and Wysocki [2010a, b], Leuz [2010], Walker [2010], Wysocki [2011]). As with other nonaccounting institutions, reporting standards and other elements of the reporting system likely have arisen to facilitate specific

business transactions that commonly arise in a country (e.g., Watts and Zimmerman [1983], Leuz and Wustemann [2004], Leuz [2010]). Thus, there is an inherent interdependency and complementarity between reporting and nonreporting institutions in each country.

Another implication of these institutional interdependencies is that, even if countries adopt uniform reporting standards at a given point in time, it is questionable that this harmonization is stable over time (Hail, Leuz, and Wysocki [2010a, b]). The new, harmonized set of standards will be subject to the same institutional and market pressures that shaped the old set of standards. Thus, unless other institutional factors across countries are also converging, countries that have adopted a common set of reporting standards are likely to drift apart over time, for example, due to local adaptation and interpretation, especially if the new, harmonized standards are not a good fit for the other institutions. Thus, based on extant research, we are much more pessimistic about the convergence in reporting *practices*, despite the global convergence in reporting standards.

Along the lines of New Institutional Economics, empirical studies have explored the determinants, outcomes, and interplay between reporting and other nonreporting institutions, mostly in cross-country comparisons. This literature has been labeled "New Institutional Accounting" (Wysocki [2011]). It examines different aspects of countries' disclosure and financial reporting *systems*, such as the accounting standards (Hung [2000]), securities regulation and disclosure rules (Frost, Gordon, and Hayes [2006], La Porta, Lopez-De-Silanes, and Shleifer [2006]), reporting enforcement (Christensen, Hail, and Leuz [2013]), and audit enforcement (Brown, Preiato, and Tarca [2014]). The literature also examines the association of (nonreporting) institutional variables and international differences in disclosure and reporting *practices*. The range of the institutional variables is large and includes factors that measure aspects of the legal system, the properties of other legal institutions (e.g., investor protection), capital-market features, political institutions, tax systems and enforcement, corruption, culture, and societal trust, to name a few examples (e.g., La Porta et al. [1998, 1999], Ball, Kothari, and Robin [2000], Hung [2000], Leuz, Nanda, and Wysocki [2003], Haw et al. [2004], Riahi-Belkaoui [2005], Bushman and Piotroski [2006], Hail and Leuz 2006], Covrig, Defond, and Hung [2007], Bae, Tan, and Welker [2008], Gupta, Khurana, and Pereira [2008], Hope et al. [2008], Francis and Wang [2008], Brochet, Jagolinzer, and Riedl [2013], Nanda and Wysocki [2013]).

The international accounting literature has amassed at least 70 country-level institutional variables that have explanatory power for disclosure and reporting outcomes around the world (Isidro, Nanda, and Wysocki [2016]).[68] These studies generally document that various institutional vari-

[68] Isidro, Nanda, and Wysocki [2016] explore the associations among 70+ country factors and their explanatory power for reporting practices around the world. The evidence suggests

ables individually affect reporting outcomes or a that given variable interacts with another institutional variable and thereby influences reporting outcomes conditionally. However, as discussed earlier, the *stand-alone* impact of IFRS adoption on reporting or its economic effects are difficult to isolate, in part because the accounting standards are just one of many institutional factors that influence managers' reporting incentives (and hence firms' reporting practices). This issue arises not only for IFRS adoption or the accounting standards, but also for other elements of countries' reporting systems, as well as any other institutional variable. Studies often focus on each newly hypothesized institutional variable and then examine whether this factor has a significant direct or interactive association with certain disclosure and reporting outcomes. But, it is also relevant whether the proposed institutional variable has explanatory power in the presence of a *broader* set of known and documented institutional factors.[69] At this point, it is unclear (1) which institutional factors are incrementally important in determining or mediating firms' reporting practices and (2) which are fundamental primitives that underlie firms' reporting practices. Addressing these questions is an important task for future research in this area.

However, there is a "curse of dimensionality," which arises because most cross-country studies have to rely on between 30 and 50 country observations, yet there are over 70 factors that have been used as country-level explanatory variables in the literature. A potentially promising way to tackle this issue is to use synthetic control groups, which have been used in small sample comparative political studies (e.g., Abadie and Gardeazabal [2003], Abadie, Diamond, and Hainmueller [2015]). Another challenge is that many institutional variables are time-invariant or evolve only very slowly. Thus, the use of time-series observations generally does not help identification much (see also Guiso, Sapienza, and Zingales [2015]). The latter poses a serious issue when studying associations between institutional factors and economic outcomes (see also discussion in Levine and Zervos [1998], Rajan and Zingales [1998]).

Finally, as discussed in Leuz [2010] and Wysocki [2011], the problems go beyond empirical identification. Even with adequate data or research settings, the potential complementarities between a country's institutions make it very difficult to (conceptually or logically) attribute observed international differences in reporting and economic outcomes exclusively to certain factors. The existence of complementarities implies that (1) it

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that very few factors provide incremental explanatory power for reporting outcomes in the presence of even a small set of other factors. But, a combined set of factors appears to have strong predictive power for reporting practices around the world. This evidence is consistent with the notion of institutional complementarities.

[69] That said, and as noted in section 4, empirical researchers must exercise caution in interpreting the significance or insignificance of institutional variables in the presence of others, given that the *proxies* we use differ with respect to their measurement error and closeness to the underlying institutional or economic construct.

might not be possible to see the effect of one factor without changing others and that (2) changing one element, while holding the others constant, may make the system (or economy) worse off even when the changing element improves along a particular quality dimension. We know relatively little about the nature and importance of such institutional complementarities.

Given this paucity of evidence, future research could start by descriptively documenting *broader* institutional patterns and associated outcomes, in essence showing what are commonly observed bundles or combinations of institutions. For instance, researchers could use factor and cluster analyses. While these methods are fairly exploratory in nature and will not provide causal identification of institutions that are core drivers of economic outcomes, it is still useful to know which types of institutional characteristics tend to be observed together. Such regularities can help identify institutional clusters and point toward possible complementarities. For example, analyses in Leuz, Nanda, and Wysocki [2003] and Leuz [2010] suggest that outsider economies with relatively dispersed ownership, strong investor protection, and large stock markets exhibit lower levels of earnings management than insider countries with relatively concentrated ownership, weak investor protection, and less developed stock markets.[70]

Accounting researchers could also look for techniques that have been used for similar problems in other disciplines. An example is Qualitative Comparative Analysis, developed by Ragin [1987] and used in comparative politics and sociology. It is an analytical technique that starts with listing all combinations of variables observed in the data and then uses Boolean algebra to determine which descriptive inferences the data support. This technique allows the identification of multiple pathways and interaction effects that may not be detectable via standard regression analysis. It can be expanded by fuzzy set theory to accommodate more nuanced institutional differences. We encourage researchers to explore this and other techniques.

Summing up this subsection, there has been a proliferation of studies documenting associations between numerous institutional variables and various reporting (and economic) outcomes. However, institutional factors, including those related to countries' reporting systems, are fundamentally intertwined and hence it is very difficult to isolate their effects on reporting outcomes. There are still many unanswered questions about the determinants, effects, interdependencies, and complementarities of reporting and nonreporting institutions. In particular, the literature has not made much headway in identifying and analyzing complementary institutional "bundles" that can be observed around the world. In this regard, the standard regression techniques currently used in cross-country studies have likely reached their limits. However, as discussed above,

[70] See also Isidro, Nanda, and Wysocki [2016]. They undertake factor and cluster analyses for a comprehensive set of institutional variables around the world.

researchers should consider other techniques. In addition, as discussed in Wysocki [2011], structural estimation may hold promise and could move the literature forward. Structural estimation has been used in the macroeconomic literature, which faces similar identification challenges, and hence has the potential to provide additional insights into the interrelations (and causal links) between many possibly complementary institutions. In addition, Athey and Stern [1998] show that a structural model can be used to estimate complementarities in organization design. While the structural approach is not without its own problems and limitations, it would at least lay open the proposed structure and assumptions about the numerous relations between various institutions and observed outcomes (see also Gow et al. [2016]). It may also help identify which factors are more likely to be economic primitives that directly affect outcomes and which factors are associated outcomes or second-order mediating factors. In summary, there are still many research opportunities to better understand the economics and mechanisms underlying the observed associations between reporting and nonreporting institutions and economic outcomes.

6. *Suggestions for Future Research*

In this paper, we review the empirical literature on the economic effects of disclosure and financial reporting regulation, synthesizing U.S. and international evidence. The regulatory focus of our review reflects the central importance of regulation and standardization for financial accounting as well as the policy relevance of work in this area. Despite its focus, however, this review does not advocate in favor of or against regulation, but instead intends to highlight evidence on the tradeoffs in regulating disclosure and reporting and hence to synthesize the lessons from existing empirical research.

After a general discussion of cost–benefit analysis, causal inferences, and the identification challenges of regulatory research, we identify key firm-specific as well as market-wide benefits and costs (or effects) of firms' disclosure and reporting activities that can be, and often are, used to evaluate regulation and standardization of these activities. Next, we review studies on disclosure regulation, both in the United States and internationally. Thereafter, we discuss studies on the economic effects of reporting standards, with a particular emphasis on mandatory IFRS adoption, as well as work on international accounting and the role of reporting standards in countries' institutional frameworks. As we have already summarized the main conclusions and higher level insights of our review in the opening and overview section, we conclude here with *eight* specific suggestions (or themes) for future research on the disclosure and reporting regulation.

First, consistent with our observation that few empirical studies on financial reporting and disclosure regulation provide causal inferences, we call for more research on regulatory effects using experimental settings in which identification is given a priority. Such work could exploit

natural experiments and, in particular, staggered implementations and regulatory thresholds. To this end, it is critically important that researchers understand the institutional features of their setting and can articulate why the regulatory effects are plausibly identified in the setting (internal validity). But it is as important that researchers connect their setting with a larger economic question to provide policy-relevant and more generalizable insights (external validity), rather than merely "cute" identification-driven studies. Given the tradeoff between external and internal validity, field experiments hold particular promise as they use randomization and hence provide identification of the regulatory effects, yet are conducted in the field, ideally without subjects realizing that they are participating in an experiment (see also Floyd and List [2016]). Another approach that has not been used much is structural estimation. This approach allows the computation of counterfactuals, which makes it particularly suited for regulatory questions.[71] For structural studies, it will be particularly important to be clear about the assumptions and the (exogenous) variation that go into the computation of the counterfactuals and the estimation of the parameters. Despite all the emphasis on identification, there is also a role for descriptive empirical studies. Such work could provide important facts, point to novel effects, or provide fundamental ideas and new insights that help advance the literature, for instance, by spawning future theoretical and (better identified) empirical research. In such studies, it will be important to be clear that the evidence is more suggestive and descriptive in nature and that the results cannot be interpreted in a causal way. For this reason, this work would ideally be theory-driven so that at least one mechanism through which the documented association could arise is explicitly spelled out.

Second, we call for more research explaining why disclosure and reporting regulation is so pervasive. Much of the literature in accounting, economics, and finance points out that the need for and the (net) benefits of regulation are not self-evident, highlighting the tradeoff between regulatory and market failures. At the same time, the largest and, arguably, most successful capital markets exhibit strong disclosure and securities regulation. Do these markets thrive because of regulation or in spite of it? As pointed out in this review, we have little evidence that we could bring to bear on this question or the question of why disclosure and reporting regulation is so pervasive around the world. In particular, we lack evidence on externalities, social costs and benefits, and market-wide or network effects.[72] Similarly, we need more evidence on the real and indirect effects of disclosure and reporting regulation, for example, with respect to corporate behavior, competition, and innovation. We conjecture that disclosure regulation could make

[71] Gerakos and Syverson [2015] provide an example for this approach with respect to audit regulation.

[72] An interesting example is the study by Dyck, Morse, and Zingales [2014] attempting to estimate the social costs of fraud.

it easier for young firms to commit to transparency and hence to obtain funding, which, in turn, could have a positive effect on competition and innovation (Leuz and Wysocki [2008]). We also know little about the extent to which disclosure regulation could support (or hinder) the development of innovative transactions in new product or service markets. Examples of transactions and markets for which transparency is likely relevant are (1) crowd-funding of firms outside traditional capital markets, (2) peer-to-peer transactions and information sharing,[73] and (3) alternative payment systems and currencies.

Another conjecture is that disclosure and reporting regulation is so pervasive because contracts and information are fundamentally incomplete and market participants have bounded rationality (see also Hart [2009]). In this situation, regulation could serve as a coordination device for market participants by providing a "coarse" default solution that is widely understood and used in many transactions and contracts.[74] Financial reporting standards and disclosure regulation strike us as one such coarse default solution, facilitating many transactions and financial contracting.

A specific market-wide benefit of disclosure and reporting regulation that deserves more attention by future research is its contribution to the stability of financial markets, specifically by mitigating asset price bubbles and subsequent market crashes. During times of technological and financial innovation, which often precede bubbles and financial crises, mandatory disclosure and reporting could limit asymmetric information among market participants, which can be an important ingredient in the formation of bubbles. Furthermore, we conjecture that forcing firms to disclose verifiable fundamentals based on past transactions and events, such as cash flows, profits, assets, and liabilities essentially "grounds" expectations, making it harder for bubbles to occur. The idea is that the disclosure of fundamentals provides an anchor or "reality check" for market participants who are navigating new market conditions due to technological or financial innovation. Such effects, if they exist, would be an important market-wide (social) benefit considering the harm that asset price bubbles and financial crises can cause in an economy. Disclosure and reporting regulation could also be used specifically to cast the spotlight on certain transactions that are fast growing and fueling asset prices (e.g., securitizations in the early 2000s). Such dynamic disclosure regulation could play an important role in monitoring and potentially even mitigating systemic risk (Leuz [2009]). More broadly, it is still an open question to what extent disclosure and reporting regulation contributes to financial stability by enhancing transparency (see also Acharya and Ryan [2016]).

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[73] Examples for nascent research in this area are Michels [2012], Einav, Farronato, and Levin [2015], and Sutherland [2015].

[74] See, for example, Morris, Shin, and Tong [2006] on the tradeoff between precision and shared understanding in optimal communication. See also Thaler and Sunstein [2008] and Kamenica, Mullainathan, and Thaler [2011] on the importance of defaults.

Third, we suggest more research on the process by which disclosure and reporting regulation arises. As our review highlights, the literature focuses primarily on the effects of disclosure and reporting regulation, rather than the political process by which it comes about.[75] Anecdotally, new regulatory reforms arise often in response to corporate scandals and financial crises. After these events, policy makers and regulators are under pressure to take corrective actions, which could lead to fine-tuning of existing regulation, but also to overreaction and ever increasing regulation. Thus, understanding the rule-making and standard-setting process is important for the evaluation of regulatory outcomes. Furthermore, the implementation of new regulation (e.g., working out the finer details postenactment) is often left to a regulatory agency. We conjecture that these details and, more generally, how regulation is implemented play a crucial role for regulatory outcomes. However, there is little research on the features that make regulation more or less successful. The extent to which political and market forces shape the implementation of regulation as well as regulatory outcomes has received very little attention in existing studies, in part because it is difficult to isolate the effects of implementation.[76] There is burgeoning work on the role of enforcement, suggesting that it plays a critical role for regulatory outcomes. Thus, we need more research that examines the relative roles (and balance) of rules, implementation, and enforcement.

Fourth, and related to our third suggestion, we call for more research on the dynamics or evolution of disclosure and reporting regulation. We have little evidence on how the costs and benefits of this regulation evolve over time, for example, change with different stages of a country's economic development. In principle, regulation should be dynamic and adjusted to changing market and economic conditions. It is a significant concern that existing regulation is inflexible and fails to adapt to fast-changing and dynamic financial and consumer markets. Inflexible regulation and standards could stifle innovation and also make it harder for new technologies and ideas to obtain financing. Most of the literature is static in that studies are primarily concerned with the questions of whether and how to regulate, and not the dynamics of regulation. There are also significant opportunities to study features of regulatory process such as sunset provisions and formal postimplementation reviews. Such features need to be carefully studied as they can affect regulatory outcomes (e.g., firms might be reluctant to make the requisite investments to implement a new accounting standard if they

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[75] Examples of early work on the politics of accounting are Watts [1977], Watts and Zimmerman [1978, 1986], and Francis [1987]. More recent examples are Dechow, Hutton, and Sloan [1996], McLeay, Ordelheide, and Young [2000], McLeay and Merkl-Davies [2004], and Ramanna [2008]. The literature on the economics of regulation also provides significant insights into issues related to special interests and regulatory capture (e.g., Stigler [1971], Posner [1975], Peltzman [1976]).

[76] Christensen, Hail, and Leuz [2016] identify EU directives as a rare setting in which the rules, implementation, and enforcement can be examined separately.

are uncertain about its future). Other aspects of the regulatory process that deserve more attention in research are regulatory path dependencies, international harmonization of regulation as well as the notion that reforms of disclosure regulation and reporting standards are increasingly coordinated among countries.

Fifth, we call for more research on the real effects of disclosure and reporting regulation. We have much less evidence on how such regulation affects corporate behavior than we have evidence on its capital-market effects, which reflects among others the behavior of investors and financial analysts. Moreover, disclosure requirements are increasingly used in many areas outside of accounting and financial reporting as a public policy instrument to encourage or discourage certain behaviors and business practices (see also Graham [2002]). However, we have relatively little evidence of whether mandated disclosure achieves the desired real effects.[77] We also have little knowledge of whether and when disclosure regulation would be preferable to more conventional regulation that directly restricts or mandates certain behaviors or business practices. The premise is often that disclosure regulation is more "benign" than conventional regulation, for which it is known that it can have significant unintended consequences. However, as discussed in section 4.4, there is also evidence of unintended and pernicious consequences of mandated disclosure, primarily in health care settings. Accounting researchers could investigate to what extent these insights generalize and, in particular, examine whether they apply to corporate settings. As societies extend the scope of disclosure and transparency regulation beyond financial reporting, understanding the real effects of such regulation is of first-order importance and a topic to which accounting researchers can contribute significantly.

Sixth, we call for more research on macroeconomic outcomes of disclosure and reporting regulation. Such research would go beyond more traditional capital-market effects and examine real investment, consumption, and possibly social outcomes of disclosure and reporting regulation at the aggregate level. While real-effects studies at the firm level already suggest the possibility of aggregate effects, determining the magnitude of aggregate effects would still be important. Doing so would be a first step toward a welfare analysis for disclosure and reporting regulation. Researchers could explore questions such as: Does disclosure regulation affect aggregate real investment in the corporate sector and if so by how much? Does mandated disclosure of financial information have an effect on households' consumption decisions and how large is this effect in the aggregate? Is there a link between disclosure regulation, transparency of the corporate sector, and economic growth? Of course, answering these questions poses significant

[77] For instance, a concern is that firms satisfy disclosure requirements to consumers with lengthy disclosures that are difficult to process, boilerplate, and legalistic in nature, which, in turn, consumers largely ignore.

identification challenges. But these challenges are not fundamentally different from the challenges of identifying the effects of monetary policy, which we face in macroeconomics. There is already an extensive literature on financial liberalization and economic growth (Levine [2001]). In this literature, however, the role of transparency and disclosure regulation is still largely unexplored.

Seventh, we call for more research that recognizes that disclosure and reporting regulation are part of a larger institutional system in which the elements interact with each other. At present, we have relatively little research into the nature of these interactions. We, therefore, propose to explore institutional complementarities and bundles, starting with descriptive analyses and possibly using new techniques (see section 5.5), but we also highlight a number of challenges. For instance, complementarities imply that the desirability of disclosure and reporting regulation should not be studied in isolation.[78] Explicitly recognizing the interactions and tradeoffs could yield novel and important insights. To provide an example, Glaeser, Johnson, and Shleifer [2001] and Djankov et al. [2003] point to interactions between ex ante regulation and ex post remedies via the legal system and the courts. As regulation cannot specify all future contingencies, parties must often rely on the courts for ex post remedies and damages. However, if there are inequalities in the judicial weapons available to litigants or agency problems with courts and judges, then ex ante regulation (e.g., disclosure rules) can serve to limit the latitude and discretion of courts (Shleifer [2005]). To give another example that is more specific to accounting: There are likely interactions between key properties of the reporting standards (e.g., the amount of discretion, use of estimates, amount of detail and guidance, etc.) and the enforceability of such standards. More generally, the interplay between disclosure and reporting regulation and various enforcement mechanisms deserves more attention. Our review of the IFRS literature in section 5 highlights the central importance of these interactions. In this regard, researchers should also draw on and connect with the large literature on auditing, which studies a particular enforcement mechanism.

In our *eighth* and final suggestion, we call for more research on the effects of global convergence of regulation, as well as the effects of regulatory competition. As discussed in section 5, we still have relatively little evidence on the effects attributable to accounting harmonization, that is, the convergence or similarities of reporting rules and practices (as opposed to improvements or extensions of reporting regulation). Despite the extensive convergence efforts in accounting and elsewhere (e.g., bank regulation), there are still many competing regulatory regimes around the world. Thus,

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[78] Complementarities also imply that institutional systems exhibit path dependencies, bringing us back to our fourth suggestion for future research. In addition, path dependencies suggest a role for historical analyses.

firms still have choices when it comes to regulatory regimes, including fairly unregulated trading venues (such as the U.S. OTC, London's AIM, or various gray markets for equities). Firms' regime choices have been extensively studied with respect to foreign cross-listings (see section 4.4). But, there are many other regulatory choices that firms make and researchers could study to enhance our understanding of which regulatory regimes firms seek or avoid. Moreover, as firms can often circumvent regulation that applies within a given jurisdiction, it is important to study firms' outside options and their potentially confounding effects.

We also emphasize that there is still competition with respect to financial reporting regulation, despite the fact that IFRS have been widely adopted globally (and substantially converged with U.S. GAAP). Reporting regulation goes beyond the accounting standards and also comprises the interpretation of the standards, implementation guidance, monitoring of compliance, and enforcement. Thus, even with the harmonization of the rules, there can still be competition among countries or jurisdictions with respect to other elements, such as (1) implementation guidance, (2) additional disclosure requirements, (3) enforcement and penalties, and even (4) nonregulatory elements such as the monitoring by information intermediaries (e.g., financial analysts, auditors, and the media). An interesting higher level question is whether such competition of regulatory and institutional regimes is beneficial or adds transaction costs. There are arguments in both directions (e.g., Barth, Clinch, and Shibano [1999], Dye and Sunder [2001], Coffee [2002]), but we have little empirical evidence on this matter. Thus, international convergence and competition of disclosure and reporting regimes will likely remain important future research topics.

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