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

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Abstract

This paper reviews the empirical literature on the economics of financial reporting and disclosure regulation, drawing on U.S. and international evidence. Given the policy relevance of research on regulation, we highlight the challenges with: (i) quantifying regulatory costs and benefits, (ii) measuring disclosure and reporting outcomes, and (iii) drawing causal inferences from regulatory studies. We first identify potentially important firm-specific as well as market-wide costs and benefits of corporate reporting and disclosure activities, which could be (and often are) used to evaluate regulation. We then synthesize the empirical evidence on the economic effects of disclosure regulation and reporting standards, such as IFRS. An important conclusion is that we generally lack evidence on market-wide effects and externalities from regulation, yet such evidence is obviously central to the economic justification of regulation. While empirical research on reporting and disclosure regulation has grown in recent years, there are important limitations and unanswered questions. Our survey concludes with numerous suggestions for future research.

Keywords: Accounting standards; Capital markets; Institutional economics; International accounting; Mandatory disclosure; IFRS; Political economy; Cost-benefit analysis

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

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1. Introduction

This paper reviews the empirical literature on the economic consequences of financial reporting and disclosure 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 analyses of intended and past regulation.¹

Three recent factors have played a major role in spurring financial reporting and disclosure regulation around the world and they help motivate aspects of our review. First, financial crises and corporate scandals often lead to calls for reforms. The Asian financial crisis of 1997, the Enron scandal in the U.S., and the recent worldwide financial crisis are but a few important examples. In the aftermath of these events, there were calls for regulators and policy makers to improve corporate transparency and to enact significant changes to reporting and disclosure 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, 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 factors highlight that disclosure and reporting regulation is a global issue, which is why we give special emphasis to international evidence. They

² Examples are the debates in the EU (e.g., Lamfalussy, 2000) and in the U.S. (e.g., Commission on the Regulation of U.S. Capital Markets in the 21st Century, 2007).
also highlight that regulating disclosure and accounting standard setting are intertwined, which is why we combine empirical evidence on the economic effects of disclosure and financial reporting regulation in this review.³

While this review primarily focuses on corporate disclosure and financial reporting, we recognize that disclosure regulation is used in many other areas, such as consumer protection, conflicts of interest, environmental policy, health care, etc. In these areas, mandated disclosure is increasingly used in lieu of regulation that explicitly stipulates or prohibits certain behaviors, the idea being that mandated disclosure and the resulting transparency incentivize desirable behaviors and discourages 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 areas further underscores the importance of the topic beyond corporate reporting. Understanding the economic effects of disclosure regulation is therefore of first-order importance, not just for accounting and finance. As such, the topic provides an opportunity to bring together work from different fields.

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,

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³ Other survey papers on the empirical disclosure literature include Healy and Palepu (2001), Core (2001), Beyer et al. (2010). Our focus is more specifically on regulation and mandates changes in reporting standards.

⁴ For example, the 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 practices can and do arise in an economy where many firms adopt a set of practices without central coordination and mandate. However, for the purposes of this review, our use of the term regulation refers to mandated sets of formal rules and associated penalties that are coordinated and implemented by one or several central authorities.
regulators and government agencies, or the general public.\(^5\) Consistent with this broad definition, we generally do not review studies that exclusively focus on a particular accounting standard or narrow disclosure rule. We do so for two reasons. First, the vast literature on the capital-market effects of mandated changes in certain accounting standards has been reviewed before.\(^6\) Second, while individual rules can have important capital-market effects, they are typically embedded in a set of accounting standards and an existing disclosure regime. As a result, such studies are less able to speak to the economic effects of mandatory disclosure in general. We therefore focus on new disclosure mandates (e.g., Securities Exchange Act), major extensions of the entire disclosure regime (e.g., Sarbanes-Oxley Act), and required changes in the entire set of accounting standards (e.g., mandatory IFRS adoption). But we acknowledge that the delineation is not always clear cut. Similarly, studies on the effects of voluntary disclosure or financial reporting choices generally do not directly speak 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. They in essence contribute an “inventory” of potential economic outcomes, which is useful to consider when evaluating disclosure and reporting regulation. For this reason, we review a number of key studies that examine economic outcomes of voluntary disclosures.

Our survey touches on general issues discussed in the economics literature on regulation, such as political lobbying and regulatory capture. However, this literature often focuses on the regulation of product-market monopolists and the corresponding impact on consumers (see, e.g., Kahn, 1988; Laffont and Tirole, 1993). Disclosure and reporting settings have their own economic

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\(^5\) 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 Acharya and Ryan (2015).

\(^6\) See, e.g., Holthausen and Leftwich (1983), Watts and Zimmerman (1986), Fields et al. (2001) and Kothari (2001). Much of this literature uses the association between reported accounting numbers and stock returns (or prices) as a way to evaluate rules or rule changes. As Holthausen and Watts (2001) and Kanodia and Sapra (2015) point out, there are major limitations to using the association criterion for standard setting (see also Barth et al., 2001).
and regulatory issues and regulated financial reporting has a number of unique features. We also take it as given that there are arguments in favor and against disclosure regulation (as well as regulation more broadly). The relative importance and magnitude of various costs and benefits of disclosure and reporting regulation is largely an empirical matter. This motivates our focus on empirical studies.

Our survey emphasizes issues related to research design and identification for two main 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 directional results and associations cannot be informative. But the magnitude of the estimated effects becomes a critical input into quantitative cost-benefit analyses only if the effects are 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 (e.g., for doctoral education). We stress, however, that these discussions are not meant as a criticism but intended to be constructive in order to move the field forward.

Having delineated the scope of this survey, we foreshadow five major conclusions that emerge from it. These conclusions primarily identify opportunities for future research.

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First, findings on the causal effects of disclosure and financial reporting regulation are often difficult to obtain and are 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, we generally struggle to measure disclosure outcomes (e.g., changes in reporting quality) separately from the underlying economic reality. This measurement problem limits our ability to produce evidence along the entire causal path from regulatory changes to reporting outcomes and then from reporting outcomes to economic consequences. For most regulatory changes, it is therefore difficult to provide causal estimates of the costs and benefits. This paucity of evidence poses major challenges for cost-benefit analyses.

Second, our review highlights that we lack evidence on market- or economy-wide effects from regulation, such as externalities, information spillovers and/or network effects. Yet the existence of such effects is critical for the economic justification of regulation in the first place. Again, research design and identification are limiting factors but not the only ones. We also have little evidence on aggregate outcomes and potential welfare effects from reporting and disclosure regulation. Thus, empirically, we can say relatively little about the desirability and efficiency of reporting and disclosure regulation. Overall, our assessment of the literature is that we have a lot of useful evidence for qualitative cost-benefit analyses, but we are still far from being able to perform quantitative cost-benefit analyses.

Third, the literature exhibits a heavy emphasis on regulatory changes in the U.S. Many major regulatory change in the U.S. has been studied extensively. There is much less evidence for other countries, despite the fact that there have been major changes in reporting and disclosure regulation in other jurisdictions. While there are research-design advantages to studying the effects

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8 This is especially true given that outcomes for alternative regulatory (or market-based) regimes that were not implemented cannot be observed and hence we do not know whether they would have provided superior outcomes. To tackle this issue we need to model and estimate counterfactuals.
in one of the largest economies with widely-available data, studying other countries should give us a richer understanding of the many facets of regulatory effects. Moreover, from an identification perspective, settings in other countries may offer research-design opportunities that may not be available in a U.S. setting.

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. Our review of this literature highlights that studies are generally not able to attribute the documented effects to IFRS adoption, i.e., the change in accounting standards. This inability stems from two key problems. IFRS were often adopted in the midst of other (unrelated) institutional reforms, which makes it difficult to isolate these concurrent institutional changes from IFRS adoption. 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, there are important interactions and complementarities between various institutional factors. These interactions pose major difficulties for identification and economic analysis.

Finally, to make significant progress with respect to the estimation of regulatory effects and hence cost-benefit analysis, researchers likely need help from legislators and regulators. For instance, one of the major issues for empirical studies is that regulation is generally required as of a particular date. A single effective date makes regulatory analysis susceptible to confounding effects, be it concurrent economic shocks or endogenous market responses to the events that gave rise to the regulation. Instead, implementing new regulation in a staggered fashion would greatly facilitate an (ex-post) economic analysis of regulation. More generally, our point is to design regulation with
ex-post analysis in mind, including provisions to collect the necessary data. Similarly, researchers and regulators could team up to devise pilot studies and field experiments, which would shed light on potential regulatory effects and greatly facilitate ex-ante analyses. Given the aforementioned push for cost-benefit analysis of regulation, such initiatives could offer significant benefits.

In addition to these five general conclusions, our review identifies numerous specific areas and topics for future research on the economic consequences of financial reporting and disclosure regulation. They include the question of why disclosure regulation is so pervasive; the dynamics and process of disclosure and reporting regulation; so-called real effects on the behavior of those that are mandated to report or disclose information; the interactions of disclosure and reporting regulation with other elements of the institutional system (e.g., enforcement mechanisms); and regulatory convergence as well as competition.

Due to the vast literature that this survey covers, we also provide an online appendix with tables summarizing both studies we discuss in the text as well as relevant studies not explicitly discussed. This online appendix is an integral part of this review. The remainder of the survey is organized as follows. Section 2 discusses overarching issues related to cost-benefit analysis, identification and measurement of disclosure and reporting outcomes. In Section 3, we provide an overview on key economic relations that are particularly relevant and commonly used when evaluating the economic consequences of disclosure and financial reporting regulation. Towards this end, we discuss potential firm-specific costs and benefits of disclosure as well as market-wide consequences. Section 4 reviews empirical studies on major changes in disclosure regulation in the U.S. 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 IFRS adoption. It also emphasizes that disclosure and reporting regulations cannot be viewed in isolation from other elements of countries’ institutional frameworks. Section 6 concludes the review with an
extensive discussion of suggestions for future research. Finally, the online appendix with summary tables can be viewed at the *Journal of Accounting Research* website.

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

   This section discusses three foundational issues that permeate the rest of our survey. First, empirical studies on regulated reporting have the potential to inform regulators and policymakers who must consider cost-benefit tradeoffs in evaluating existing and proposed disclosure and reporting regulation. Second, our understanding of the potential outcomes of disclosure and reporting regulation ultimately relies on our ability to draw causal inferences from regulatory studies. Finally, a central issue for the economic analysis of disclosure and reporting regulation is the measurement of these activities. We highlight that there are many issues with regard to the measurement of disclosure and reporting and argue that progress with respect to measurement could be a critical factor for tighter regulatory studies. We use these three concepts from this section to understand the contributions and also limitations of the existing literature on disclosure and reporting regulation.

2.1. **Cost-benefit tradeoffs and analyses**

   It has become increasingly common that regulators are asked to conduct both prospective cost-benefit analysis as well as post-implementation 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 review highlights that there is significant risk of an “expectation gap” with respect to what academic research can actually contribute to cost-benefit analysis (see also Schipper, 2010; Coates, 2014). It is unrealistic, at least in the near future, that we will be able to measure and comprehensively evaluate the net benefits of new disclosure and reporting regulation to firms and investors, let alone to consumers or the economy as a whole. 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. That said, it would be possible to conduct pilot studies as well as field (or lab) experiments on particular aspects of a new regulation. Such evidence would be particularly useful for ex-ante economic analysis and for the design of regulation.

In general, however, cost-benefit analysis has to rely to a significant extent on empirical studies using observational data. Ideally, such studies would provide quasi-experimental evidence and causal inferences. But as our review of the literature highlights we have only few studies that provide such evidence. While there are opportunities for researchers to improve and tighten their research designs and to embrace new econometric methods of estimating treatment effects (e.g., Angrist and Pischke, 2009, 2014), the main limitation is how the observational data is generated, i.e., the institutional settings themselves and the process by which new regulation arises. Our subsequent discussions of the relevant literature (Sections 4 and 5) illustrate this point and this is one reason why our review casts the spotlight on research-design issues.

One might argue that we often have multiple empirical studies providing consistent results, perhaps even across different settings, and that such evidence should make us more confident in the findings. 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 (discussed next) and hence different studies do not really “diversify” the research-design problems.\(^9\) Generally speaking, we do not have quasi-random assignment of firms to a treatment group subject to disclosure regulation and a control group that is unaffected. The

\(^9\)This is also why formal meta-analyses across a set of regulatory studies would likely not be useful.
identification of externalities and spillover effects is even harder as it requires identifying an additional completely unaffected group against which the indirect effects of regulation can be measured. Moreover, externalities imply that for affected firms the Stable Treatment Unit Value Assumption,10 which is central to the causal inferences paradigm (Rubin, 1978), no longer holds. Thus, the estimation of externalities and indirect effects is not a trivial problem.

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 non-traditional institutional settings with an eye towards identification. But in our quest for new settings we also have to recognize that there generally is a tradeoff between internal and external validity. For instance, a setting that provides quasi-random assignment for particular regulatory change may yield estimates that are fairly specific to the setting and 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 generally is a price that we have to pay for identification.

Despite the challenges to causal inferences, academic research can provide many useful inputs into cost-benefit analysis, especially when it takes a more qualitative approach, which in our view is still a tremendously useful exercise. Theoretical research can point to potential outcomes of disclosure and reporting regulation, which should underpin any qualitative cost-benefit analysis.

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10 The Stable Unit Treatment Value Assumption (SUTVA) maintains that one unit's outcomes are unaffected by another unit's treatment assignment.
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 attempt to enumerate and briefly take stock of potential effects that prior research has identified with respect to 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. It is with this perspective that we review the literature below. In the end, however, cost-benefit analyses still require the expert judgment of those that have been tasked to make the normative decisions (see also Coates, 2014).

2.2. Identification challenges for disclosure and reporting regulation studies

Regulatory settings have the advantage that disclosure and reporting rules are imposed on firms. Thus, from the perspective of an individual firm, the regulation is exogenously given. We can therefore use the variation in firms’ disclosures resulting from differences or changes in 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.

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 and hence there is no 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 extensive disclosure regulation on its firms has likely done so after an explicit or implicit cost-benefit analysis. Thus, when a study that analyzes this regulatory change documents significant benefits, we have to be careful with our interpretation of the results. In particular, it is unclear that such benefits would carry over to other countries that have not yet imposed the same regulation (e.g., the latter may have done so deliberately).

Second, 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 (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 response and subsequent reversals (see also Ball, 1980; Mulherin, 2007). Importantly, this concern is not addressed with a difference-in-differences design unless the control group is also affected by the trigger event and hence subject to market response (but not subject to the regulation, which often is an unlikely constellation).

Third, regulatory changes tend to apply to a larger group of firms at or after a (single) given point in time. As a result, an empirical analysis of regulatory effects is 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. If the regulation is implemented in a staggered fashion, i.e., phased-in over time, then concurrent events can also be controlled for with appropriate time-fixed effects. Such a staggered implementation can for instance arise because of differences in firms’ fiscal year ends (e.g., Daske et al., 2008; Christensen et al., 2013). However, using the staggered implementation for identification is not a panacea. It is important to ascertain that the dates are plausibly exogenous, e.g., pre-determined or tied to more 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. Similarly, spillover effects over time can be a concern, for instance, if witnessing the regulatory effects for firms that have to adopt the regulation first leads to anticipation of the effects for later adopters (and hence the effects are muted or no longer present around the actual implementation date). It would also be a concern if seeing the regulatory effects for early adopters alters the implementation responses of late adopters.

Fourth, and related to the previous point, it is important to recognize that capital markets often anticipate regulatory changes. In particular, market prices, returns and the ex-ante cost of capital are going to reflect expected changes in regulation, 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 implementation design will not work with economic outcome variables that are anticipatory in nature (see also Christensen et al., 2013, 2014a).

Fifth, 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

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11 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 needs to be interpreted carefully. However, the average effect over the entire population of treated firms appropriately captures the network effects.
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.\(^{12}\)

The aforementioned five challenges are not meant to be comprehensive but highlight that it is very difficult to estimate economic effects of disclosure and reporting regulation and to draw causal inferences.

2.3. **Measurement of corporate disclosure and reporting**

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 below, measurement problems are one major reason why we struggle with the identification of the economic consequences of disclosure and reporting regulation. We first 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, the latter facilitates consistent measurement across firms as well as measurement of quality differences. But with narrower measures, the concern arises that other disclosure activities could serve as a substitute (or complement). For instance, firms could compensate poor earnings quality with additional disclosures. Broader measures that characterize a disclosure policy or reporting regime are more likely to capture a firm’s commitment to a certain level of transparency, i.e., a promise to provide certain information irrespective of its future realizations. In contrast, narrower measures are generally based on specific realizations (e.g., earnings or a particular disclosure in a given year).

\(^{12}\) For the challenges of regulatory event studies more generally, see also Foster (1980), Schwert (1981), Holthausen and Leftwich (1983) and Binder (1985).
Commonly used measures in the broader category are binary indicators whether a firm publicly provides an annual report; files a 10-K or other disclosure forms with the SEC; reports financial statements quarterly, reports under IFRS, U.S. GAAP or a particular reporting regime. Similarly, studies use variables characterizing disclosure policies, e.g., 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 capture primarily 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 AIMR rankings, which are based on annual surveys of financial analysts asking them to rank U.S. firms with respect to their disclosures activities (e.g., Lang and Lundholm, 1993, 1996; Welker, 1995; Healy et al., 1999; Nagar et al., 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 in quarterly reports, and disclosures arising from investor relations activities. AIMR rankings 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 sell-side analysts’ objectives.

Other studies use (self-constructed) disclosure indices that are generally based on a checklist of corporate disclosures activities (e.g., Botosan, 1997; Hail, 2002; Francis et al., 2005). Similarly, international studies often rely on the CIFAR index or the S&P Disclosure score, which are

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13 The rankings are named after the Association for Investment Management and Research that conducted these surveys. The rankings were discontinued in the 1990s.
14 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.
constructed from annual report and disclosure checklists. They 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 et al., 2003; Khanna et al., 2004; Doidge et al., 2007a). 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 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 et al., 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. However, 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 used to be difficult to use in a quantitative way. 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 MacDonald, 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). As these measures are fairly new, there are still substantial debates about what the proxies capture and how well they work in empirical studies.15

15 See, e.g., the survey by Loughran and MacDonald (2014).
Probably the most frequently used measures are based on firms’ reported earnings and hence fall into the narrower category.\textsuperscript{16} Earnings management and accruals-based proxies have been used for two decades starting with Jones (1991) and the modified Jones model (Dechow et al., 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 et al., 2000), earnings smoothing, (e.g., Ronen and Sadan, 1975; Leuz et al., 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 et al., 1997; Francis and Schipper, 1999). All these proxies capture important positive and negative aspects of firms’ reported earnings but they also suffer from many conceptual and measurement problems (see Dechow et al., 2010, for an extensive discussion).

Especially international studies often aggregate several of the aforementioned proxies into a combined measure (e.g., Leuz et al., 2003; Lang et al., 2003b; Lang et al., 2006; Burgstahler et al., 2006). By aggregating across measures, these studies attempt to obtain a less specific measure of reporting quality and 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 relations and tradeoffs among the various properties are ignored. At this point, we have little evidence that the combined measures are indeed superior and the extent to which the aggregation reduces measurement error.

A fundamental problem of essentially all proxies for corporate disclosure and reporting is that they need to separate a firm’s economic situation and business model from the representation

\textsuperscript{16} See Healy and Whalen (1999), Dechow and Skinner (2000) and Dechow et al. (2010) for extensive reviews of this literature.
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 et al., 2005; Hribar and Nichols, 2007; Cohen, 2008). To illustrating this problem with an example, Francis et al. (2005) document that economic factors driving a firm’s accruals properties (labelled “innate” properties) dominate the association between proxies for accruals quality and various measures of a firm’s cost of capital. Broadly speaking, separating a firm’s reporting from its economics (or alternatively controlling for the economics) is very difficult. It is the central problem that has plagued proxies of reporting and accruals quality, earnings management and other reporting properties such as conservatism (see also Dechow et al., 2010). Importantly, the same issues arise with narrative disclosures (and hence the new text-based proxies) that describe economic performance as well as with specific disclosures (e.g., about fair value estimates) that also reflect a particular business model or economic situation.

In addition, it is important to note that the issue is not just a pure technical or statistical measurement problem. Managers endogenously respond to performance shocks and they can manipulate disclosures and reported numbers, which creates the issue of distinguishing between the properties of manipulated earnings and from those of normal earnings (see, 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” financial reports.

Based on our current state of knowledge, it is fair to conclude that essentially all commonly used proxies for disclosure and reporting activities are likely to comingle the firm’s underlying economics with the constructs that they are trying to measure. Accounting research has not yet found a satisfying way to empirically identify reporting quality. ¹⁷ A potentially promising approach is to put more structure onto the problem. Recent attempts in this direction are Gerakos and

¹⁷ Note that identification implies separation from the underlying economics.
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. An example is Nikolaev (2014) who uses simple relations between accruals and cash flows as well as the fact that accruals reverse to identify accounting quality in a GMM estimation framework. Another approach is to use quasi-experimental methods, such as instrumental-variable estimation and regression-discontinuity analysis. This approach requires that one identifies instruments or situations in which reporting (quality) changes, but a firm’s underlying economics (e.g., performance) is plausibly unchanged. Such situations (or instruments) are difficult to find, but it is not impossible. Mandatory changes in disclosure can in some instances serve this purpose. That is, we can use these settings not only to evaluate regulatory changes but also to use them “in reverse” to infer changes in reporting quality.

Along these lines, and in part because of the described measurement problems, empirical studies often directly estimate the capital-market consequences of regulatory changes in disclosure and reporting (e.g., with respect to market liquidity). That is, rather than proceeding in two steps by first linking a regulatory change to reporting outcomes and then linking the reporting outcomes to capital-market consequences, we estimate in essence reduced-form regressions. This approach sidesteps the measurement of reporting and disclosure changes. But it opens up the analysis to concurrent but unrelated shocks to the capital-market variables. In light of the identification challenges discussed in the previous section, providing results along the causal path and hence proceeding in two steps could have advantages in regulatory studies. It would mitigate concerns that concurrent shocks that affect capital-market outcomes but are unrelated to the reporting changes do not drive the results. Similarly, it would allow us to provide some evidence that a regulatory change indeed operates through disclosure and reporting changes. It is precisely for this

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18 The study by Iliev (2010) is an example. However, it is important to keep the earlier discussion on the difficulties of causal inferences from regulatory studies in mind. See Section 2.2.
reason that we need more research on the measurement of disclosure and reporting activities and why we think that such research would substantially aid empirical studies on reporting and disclosure regulation.

3. **Empirical Evidence on the Economics of Disclosure**

In this section, we identify and discuss possible economic outcomes associated with firms’ disclosure and financial reporting activities. The idea is to enumerate an “inventory” of potential effects that could be considered when evaluating possible economic outcomes of disclosure and reporting regulation. To provide an overview, we primarily tabulate the array of effects that have been empirically investigated (Tables 3.1-3.5). In the text, we highlight key economic links that have been examined extensively and attempt to give the reader a sense how strongly these links have been established. We thereby also point to potential 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. But as noted in Section 2, we are also still far from full-blown, quantitative cost-benefit analyses of disclosure and reporting regulation (see also Schipper, 2010). However, the identification and enumeration 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 discuss possible firm-specific (micro-level) and market-wide (macro-level) effects of firms’ disclosure and reporting activities. Our overview complements prior surveys of the empirical disclosure literature (e.g., Healy and Palepu, 2001; Core, 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 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 policymakers and regulators, we begin by clarifying the role of studies using firm-level variation that largely reflects firms’ choices. In general, studies using such variation 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 the firm-specific benefits from disclosure exceed their costs, we do not need regulation. A classic argument for disclosure regulation is therefore that the social value of information exceeds its private value to firms, resulting in too little disclosure from a societal perspective.19 However, voluntary disclosure and reporting studies generally do not provide evidence on the discrepancy between the private and social benefits of information. Due to the voluntary nature of the variation, they provide at best estimates for the treatment effect on the treated, rather than for the average treatment effect, which is what would be most relevant to policymakers and regulators.

The primary role of studies using variation from firms’ choices is therefore to illustrate potential costs and benefits from corporate disclosure and reporting activities, i.e., these studies can help to establish relevant economic links. Specifically, these studies can inform regulatory debates in that (i) they pinpoint key costs and benefits of financial reporting and disclosure, which in turn

19 The arguments for or against disclosure regulation have been heavily debated (e.g., Seligman, 1983; Coffee, 1984; Easterbrook and Fischel, 1984; Zingales, 2009; Bushman and Lang, 2010; Leuz, 2010). We take these arguments as given. In our view, the question of whether or not disclosure regulation is beneficial is largely an empirical matter.
should be considered when evaluating mandating these activities, (ii) 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), and (iii) they help us predict which firms may take avoidance actions or lobby for or against a proposed regulation given the potential differential effects on firms and the wealth transfers between them. For these reasons, we include in our review studies on the economic consequences of voluntary disclosure and reporting activities.

But even with this caveat in mind, it is important to recognize that by their very nature voluntary disclosure and reporting studies face a selection problem. This problem poses a major challenge to identification and implies that one can easily obtain spurious effects. As an illustration, consider the following challenges for 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 change business risk and hence cost of capital. Thus, one can easily obtain a association even if no economic link exists. Second, even if the economic link exists and disclosure reduces the cost of capital, firms likely respond with disclosure to cost of capital shocks (Leuz and Schrand, 2009; Clinch and Verrecchia, 2014). Thus, in the cross-section, one might estimate 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 observed relation is again spurious. Finally, as discussed

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20 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.
in Section 2.3, commonly used measures for corporate disclosure and reporting comingle the underlying economics that are conveyed in the reporting with the disclosure and reporting. Therefore, it is easy to overestimate the effects of disclosure and reporting as they often reflect differences in the underlying economics. We highlight several of these challenges as we review the literature below.

3.2 Firm-specific benefits from reporting and disclosure

In this sub-section, we discuss empirical studies that identify possible benefits of improved reporting and disclosure. We distinguish between capital-market effects and real effects. The latter are defined as effects on the behavior of the disclosing entity and their managers, such as corporate investment. Capital-market effects concern primarily the receiver of the information, such as investors and financial analysts. As summarized in Table 3.1, we identify seven major categories of possible economic benefits of improved reporting and disclosure (with the consensus of empirical results with respect to the direction of the relation reported in parentheses): liquidity (positive), cost of capital (negative), Tobin’s Q (positive), investors’ portfolio allocations (conditionally positive), analysts’ information environment (conditionally positive), investment efficiency (positive), and capital raising (ambiguous relation). In the following subsections, we discuss important examples of these studies.

3.2.1 Capital-market benefits

In its search for potential capital market benefits, the empirical literature has been largely guided by theory when it comes to direct capital-market outcomes of firms’ disclosure and reporting activities such as liquidity, cost of capital and firm valuation. Table 3.2 summarizes key studies in this category. Other capital-market benefits that more indirectly relate to firms’ capital-market
access and funding costs include investor participation, institutional investor activity, and analyst following. Key studies in this category are summarized in Table 3.3.

Arguably, the firm-specific benefit of disclosure best supported by theory is the effect on market liquidity (see also Verrecchia, 2001). At its core is the insight that information asymmetries among investors introduce adverse selection into share markets. Uninformed or less informed investors have to worry about trading with privately or better informed investors. 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, i.e., 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).

The empirical literature has largely confirmed this prediction and generally documents a positive association between better disclosure and liquidity (e.g., Welker, 1995; Healy et al., 1999; Leuz and Verrecchia, 2000; Heflin et al., 2005; Brown and Hillegeist, 2007). In addition, survey evidence suggests that managers believe that such liquidity benefits exist (Graham et al., 2005). More recent work takes the basic relation as given and focuses on possible mechanisms for the relation as well as on other attributes of liquidity in order to gain a deeper understanding of how disclosure affects information asymmetry and liquidity.

For example, Heflin et al. (2005) illustrate that firms with more disclosure have lower effective bid-ask spreads but also lower quoted market depth, suggesting a tradeoff. Better disclosures reduces information asymmetry but also induces some liquidity suppliers to exit the

21 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 (e.g., Glosten and Milgrom, 1985).

22 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).
market when they have fewer opportunities to transact on private information. Combining the effects, they still find that firms with better disclosures have lower depth-adjusted effective spreads. Brown and Hillegeist (2007) investigate possible channels for the liquidity effect of disclosure. Their findings indicate that the liquidity benefit of disclosure arises primarily because disclosure reduces the likelihood that investors discover and trade on private information, rather than because it reduces the trading incentives of uninformed investors. In essence, the results suggest that disclosure reduces the non-productive search costs, as predicted in Verrecchia (1982) and Diamond (1985), which is useful evidence from a welfare perspective.

With respect to other liquidity attributes, 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) examines the connection between reporting quality and priced liquidity risk. His evidence suggests that better reporting is negatively associated with liquidity risk, i.e., the sensitivity of stock returns to unexpected changes in market liquidity (e.g., Pastor and Stambaugh, 2003). He also finds that the negative association between reporting quality and liquidity risk is stronger in times of large shocks to market liquidity. Thus, both studies suggest that the benefits of transparency are largest in financial crises, which is interesting from a macro-perspective.

As noted earlier, a major problem in voluntary disclosure studies is the selection problem. Even the early 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. Thus, the evidence should probably not be
interpreted in a causal way. A recent study taking a step in this direction is Balakrishnan et al. (2014). They exploit an exogenous shock to the information environment, i.e., the loss of an analyst, stemming from brokerage house closures and mergers after Reg FD. Balakrishnan et al. (2014) 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. However, this study provides evidence only along the intensive margin, i.e., for firms that have provided managerial forecasts in the past, and only for one particular disclosure. Thus, while it appears that we have relatively little causal evidence based on voluntary disclosures, there are numerous studies exploiting mandated changes in disclosure and reporting that corroborate a link between disclosure and market liquidity (see Section 4).

Another commonly analyzed economic link is the relation between better disclosure and reporting and 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 also manifest in a cost of capital effect. Illiquidity and bid-ask spreads essentially impose trading costs on investors, for which investors need to be compensated in equilibrium (e.g., Constantinides, 1986; Amihud and Mendelson, 1986). Moreover, adverse selection problems in secondary markets fold back to the point at which the firm issues shares. Investors anticipate that they face price protection in the future and hence reduce the price at which they are willing to buy shares in the initial securities offering (e.g., 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 arising from estimation risk (e.g., Brown, 1979; Barry and Brown,

Overall, the empirical evidence on the cost of capital effects of disclosure and financial reporting is more mixed than the evidence on liquidity (see Table 3.2). To illustrate, Botosan (1997) finds a negative association between disclosure and the cost of equity capital but only for firms with low analyst following. Botosan and Plumlee (2002) document a negative relation for annual report disclosures but not for investor-relations activities and when it comes to more timely disclosure. In contrast, Hail (2002) finds relatively strong and large effects in a sample of Swiss firms. One possibility is that the results are stronger because he examines the effects in a relatively weak disclosure environment, consistent with the idea expressed in Leuz and Verrecchia (2000) that the magnitude of the relation depends on the strength of the mandatory disclosure system. However, Daske (2006) analyzes a similar information environment as Leuz and Verrecchia (2000) and does not find a lower implied cost of equity capital for firms committed to international reporting strategies. 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 firms’ reporting from their underlying economics. Thus, a concern is that these studies do not illustrate the effects of accruals quality or earnings smoothing (or more generally reporting quality) but instead reflect differences in firms’ operating volatility.

There is also survey evidence documenting that managers perceive a cost of capital benefit from expanded voluntary disclosures (Graham et al., 2005). But again, the evidence is not as strong as the evidence for liquidity. Surprisingly, Graham et al. (2005) find that the perceived reduction in the cost of capital is greater for firms with high analyst following, which is in contrast to the
archival evidence in Botosan (1997) but also inconsistent with predictions that could be plausibly derived from most of the theoretical models.²³

We suspect that one potential reason for the mixed evidence is again the selection problem, which arises in all of 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, particularly with respect to the exclusion restriction, in these settings. Larcker and Rusticus (2010) illustrate this problem and the effects of instrument choice on the results in this very context. They ultimately conclude that, in their setting, there is no statistical evidence that disclosure quality has any 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. Again, there are also studies exploiting mandatory changes in disclosure and reporting to illustrate a link between disclosure and the cost of capital (e.g., Shroff et al., 2013).

Similar to the disclosure-liquidity studies, more recent work focuses on the potential mechanisms for a link between disclosure and the cost of capital. Using path analysis, Bhattacharya et al. (2012) investigate the direct and indirect links between different measures of earnings quality and the cost of equity. They find evidence of both a direct path from earnings quality to the cost of equity, and an indirect path that is mediated by information asymmetry. Similarly, Lang et al.

²³ For instance, the Merton (1987) model suggests that firms can make investors aware of their existence and enlarge their investor base, which in turn lowers their cost of capital. This effect would seem most pronounced for small firms (e.g., in the OTC markets). It seems less relevant and plausible for large firms with high analyst following.
(2012) provide evidence for cost of capital effects via a liquidity channel. After establishing a positive empirical relation between disclosure and liquidity, the authors 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.

In addition, there is a number of studies that examine the relation between corporate disclosure and firms’ external capital raising activities. Studies generally find a positive link between capital raising activities and disclosure quantity and quality (e.g., Frankel et al., 1995; Healy et al., 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 (e.g., Schrand and Verrecchia, 2005; Leone et al., 2007).

Finally, there is a growing literature that investigates to what extent firms’ reporting quality represents a non-diversifiable information risk factor that is priced in returns. At present, most theoretical models do not predict such a risk factor but instead imply a relation with firms’ beta factor (e.g., Lambert et al., 2007). That said, several studies document a negative relation between reporting quality proxies and firms’ cost of equity (e.g., Aboody et al., 2005; Francis et al., 2005; Ecker et al., 2006; Kim and Qi, 2010; Ogneva, 2012; Barth et al., 2013). However, Core et al. (2008) argue that the asset pricing tests in Francis et al. (2005) and Ecker et al. (2006) are not appropriate to establish that accruals quality is a priced risk factor. Using the two-stage Fama and Macbeth regressions, Core et al. (2008) find little evidence that accruals quality is priced as a separate risk factor. However, more recent studies by Kim and Qi (2010) and Ogneva (2012) provide evidence that accruals quality is robustly priced as a risk factor, for example, once controlling for shocks to firms’ expected cash flows. Despite the fact that most studies document a negative relation between reporting quality and expected returns, it is important to note that these
studies typically do not use exogenous reporting variation and that the commonly used proxies for reporting quality are prone to pick up differences in the underlying economics.

While most of the research has focused on the cost of equity, there are also studies on the cost of debt (e.g., Sengupta, 1998; Nikolaev and van Lent, 2005). Miller and Puthenpurackal (2002) also find that U.S. debtholders demand economically significant premiums for bonds of foreign firms that have no prior history of ongoing disclosure. Moreover, Zhang (2008) finds that lenders offer lower interest rates to firms that report conservative earnings numbers. Studies on the cost of debt face the same selection and endogeneity concerns as cost of equity studies. One potential advantage of the former is that the cost of debt is arguably more easily measured. However, a major difficulty of tests involving the cost of debt is to control for the specifics of firms’ debt contracts, in particular covenants, and their impact on the cost of debt.

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 and can vary across different measures of cost of capital (i.e., realized returns versus ex ante cost of capital proxies), types of firms (i.e., different sizes), in the presence of intermediaries (i.e., financial analysts), across types of disclosures or earnings attributes (i.e., annual reports versus timely disclosures versus conservative earnings), across types of capital (debt versus equity), and across different institutional environments (i.e., U.S. versus other markets).

3.2.2 Real effects on corporate investment

It is also conceivable that better disclosure and reporting improve corporate decision-making, for example, the efficiency of firms’ investment decisions. Overall, there is relatively sparse empirical evidence on the potential real effects of disclosure and reporting. We list and summarize

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24 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.
key studies in Table 3.4. However, the analytical literature suggests that real effects of corporate should be fairly common (see survey by Kanodia and Sapra, 2015). Thus, more research is necessary to determine how prevalent and large potential real effects are.

One possible channel for real effects of disclosure is based on the idea that higher quality reporting reduces information asymmetries that otherwise give rise to frictions in raising external capital for investment. For instance, high-quality reporting facilitates 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; Lambert et al., 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 et al., 2006; Biddle et al., 2009, Badertscher et al., 2013, Goodman et al., 2014). Similarly, Jung et al. (2014) examine possible effects on the efficiency of labor investments and document a positive relation. Finally, there is growing empirical evidence that financial misreporting by a given firm can lead to inefficient investment decisions for competing firms (see, e.g., Sadka, 2006; Beatty et al., 2013). We come back to these peer effects in the next section.

As for the capital-market studies, most of these studies use cross-sectional variation in disclosure and reporting to estimate the links with investment efficiency. Thus, we have to be careful in our interpretation of their findings. But the documented associations support the notion that there are important real effects from corporate disclosure and reporting.

3.3 Firm-specific costs of corporate disclosures

In this sub-section, we discuss 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 Table 3.5, we list and summarize studies for each category. Below we discuss only several important examples.

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. Moreover, due fixed disclosure costs, certain disclosures can be particularly burdensome for smaller firms. Empirical studies consistently find that larger firms provide more and better disclosures (e.g., Lang and Lundholm, 1993), which is consistent with the notion that there are fixed costs to information production and dissemination that induce economies of scale. Overall, however, there is a general paucity of academic evidence on the direct costs and out of pocket expenses of firms’ disclosure and reporting practices. It is especially difficult to quantify costs that come in the form of opportunity costs, such as managerial time.

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, tax authorities, etc.). For example, detailed information about line-of-business profitability can reveal proprietary information to competitors (e.g., Feltham et al., 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 et al., 1992).

As shown in Table 3.4, there is more evidence on the indirect costs of disclosures. For example, there are a number of empirical studies that examine the effects of proprietary costs on firms’ voluntary disclosure decisions. Harris (1998) is among the first to explore the association between product market competition and detailed industry segment disclosures. She finds that profitable operations in less competitive industries are less likely to be reported as industry segments. Berger and Hann (2003) also provide insights into the issue of proprietary costs by examining a change to U.S. reporting requirements for segment disclosures (i.e., the transition from SFAS 14 to the less flexible segment reporting rules under SFAS 131). Berger and Hann (2003) compare segment disclosures under both standards and find that firms that previously aggregated information under SFAS 14 had higher abnormal profitability and operations with more divergent performance. Subsequent studies by Botosan and Stanford (2005), Berger and Hann (2007), Hope and Thomas (2008), and Bens et al. (2011) find consistent results for firms’ segment reporting decisions in the face of competition. However, the studies also point out that managerial agency costs could also explain or at least play into firms’ segment reporting behavior.

Other research posits that shareholder litigation provides a disincentive for firms to voluntarily provide forward-looking disclosures. Many studies find mixed evidence on the effect of litigation on disclosure, especially for bad news disclosures (see, e.g., Kasznik and Lev, 1995; Skinner, 1994, 1997; Bamber and Cheon, 1998, Johnson et al., 2001, Rogers and Van Buskirk, 2009). Field et al. (2005) attempt to reconcile this mixed empirical evidence on the relation between bad news disclosures and litigation, highlighting that there is possibly an endogenous

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25 Proprietary costs are the costs faced by a firm if it reveals information to outside parties. These costs include the revelation of trade secrets, the disclosure of profitable customers and markets, or the exposure of operating weakness to competing firms, unions, regulators, investors, customers or suppliers.
relation between observed litigation outcomes and management’s bad-news disclosure. Their results suggest that disclosure potentially deters litigation, which in turn suggests another benefit from disclosure in the sense that it reduces expected litigation costs.

As noted in the context of segment reporting, disclosures can be costly to managers in that they may face significant personal costs from disclosing bad news to investors. Kothari et al. (2009) argue that career concerns (broadly-defined) motivate managers to withhold bad news hoping for a turn-around so that they can bury the bad news in future good news. Consistent with this idea, Kothari et al. (2009) present evidence suggesting widespread withholding of bad news by managers. Finally, it is possible that disclosure activities have indirect costs for existing financing or other relationships (e.g., political connections). An example for this effect is the study by Leuz and Oberholzer-Gee (2006).

3.4. Market-wide impact of firms’ disclosure and reporting activities

As a final category of possible economic outcomes, we discuss potential market-wide effects of disclosure. Theory argues that the effects of reporting and disclosure often extend beyond the firm providing the information (e.g., Dye, 1990; Admati and Pfleiderer, 2000). The disclosure of one firm not only provides information to other firms for decision-making purposes, but it can also help 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, lower the costs of monitoring. Empirically, such information transfers and governance spillovers have been less explored, but this does not imply that they are less relevant or unimportant. We summarize empirical studies examining market-wide effects of disclosure and reporting in Table 3.5.
Starting with Foster (1981) who shows that a firm’s earnings announcement provides information to investors about other firms in the same industry, there is a sizeable literature on information transfers (e.g., Olsen and Dietrich, 1985; Clinch et al., 1987; Baginski, 1987; Han et al., 1989; Han and Wild, 1990). These studies suggest the existence of industry- and potentially market-wide externalities from firms’ disclosure and reporting activities.

More recently, studies explore spillover and peer effects from accounting restatements. They suggest that restatement not only result in equity market penalties for restating firms (e.g., due to an increased cost of capital), but also spillover to their competitors (Xu et al., 2006; Gleason et al., 2008). Durnev and Mangen (2007) further argue and present evidence that one firm’s restatement announcement reveals information about the efficiency of its competitors past investments. The idea is that misreporting and the erroneous information lead to sub-optimal investment decisions by the competitors. Sidak (2003) presents similar arguments in his case study of the WorldCom accounting fraud. He argues that WorldCom's fraudulent financial reports had negative real effects, i.e., that they led to: (i) the widespread overinvestment in network capacity by other firms, (ii) the formulation of flawed government telecommunication policies, and (ii) the sustained retrenchment of financing sources away from future telecom investment projects. Sadka (2006) and Beatty et al. (2013) also presents evidence on spillover effects on real investment from firms’ misreporting.26

In summary, there are numerous reasons why an individual firm’s disclosures extend beyond the firm itself. Moreover, the market-wide effects could be large in the aggregate while imposing only relatively small costs on the disclosing firm. But as individual firms generally do not internalize the market-wide benefits of their disclosure activities, even relatively small disclosure costs could deter socially optimal disclosure activities. As with other externalities, the problem is

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26 Chen et al. (2013) provide evidence on the spillover effects of mandated changes in financial reporting on firms’ investment decisions in other countries. We review this study in Section 5.
that firms trade off only the private (or firm-specific) costs and benefits and hence may not provide the socially optimal level of disclosure. As such, studies on market-wide effects play an important role for the economic analysis of disclosure and reporting regulation.

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. We include studies on major changes in the enforcement as the latter is an important element of securities or disclosure regulation. We also discuss studies on international differences in disclosure regulation. Studies that examine mandated changes in the entire set of reporting standards (e.g., mandatory 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 for all exchange-traded firms, were extended over time to cover over-the-counter (OTC) traded stocks (i.e., the 1964 Securities Act Amendments, the 1999 Eligibility Rule on the OTC Bulletin Board). In addition to the first-time imposition of disclosure requirements, there were also major reforms of securities regulation pertaining to disclosure, such as Regulation Fair Disclosure in 2000 and the Sarbanes-Oxley Act of 2002. Studies examining these major reforms are discussed in the next section.

27 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, they are typically embedded in a set of accounting standards and an existing disclosure regime. As a result, such studies are less suited to inform us about the economic effects of mandatory disclosure in general.
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 abnormal returns of new issues 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 pre-period.28 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 Table 4.1.

The early studies and their interpretations have been heavily debated and repeatedly been 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 abnormal returns of new issues 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

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28 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 evidence in later periods that 10K 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).
than a treatment effect. They point out that there is also a trend from public debt offerings towards 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.\(^{29}\)

This discussion highlights the importance to control for firms’ responses to the regulation, which can result in sample composition changes in the post-period. 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.\(^{30}\) 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 are 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

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\(^{29}\) More recent studies also point to such effects. See, e.g., Bushee and Leuz (2005) and Gerakos et al. (2013).

\(^{30}\) 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 gave rise to the regulation.
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 doubts 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 for how firms are affected by the regulation (and its plausibility).31

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.32 We list and summarize key studies in 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 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

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31 We discuss this issue also in Section 4.2 for SOX studies.
32 Strictly speaking, firms in the OTC markets are not entirely unregulated as they are also subject to state securities regulation, i.e., Blue Sky laws. However, as discussed in Mahoney (2003) and Brueggemann 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, e.g., Agrawal (2103) for an analysis of the effects of these laws on firms’ financing and investment decisions.
SEC disclosure requirements. He interprets this evidence as consistent with an increase in price efficiency due to information being more quickly impounded in prices. In addition, he documents that OTC securities exhibit positive abnormal returns during the time period over which passage of the 1964 Amendments became likely. Greenstone et al. (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 et al. (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.” To support this interpretation, Greenstone et al. (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 consistent with other interpretations, such as a decline in the cost of capital due to better disclosure.33

Battalio et al. (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 post period if the disclosure

33 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.
mandate increases firm value as shown in Greenstone et al. (2006) and Ferrell (2007). Battalio et al. (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 et al. (2011) were able to find financial information from various sources for most OTC stocks prior to the 1964 Amendments. Given their evidence, Battalio et al. (2011) raise the possibility that the results in Greenstone et al. (2006) and Ferrell (2007) reflect the difficulty of measuring abnormal returns over a longer period. However, such difficulties should also affect the post period in which Greenstone et al. (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 OTC Bulletin Board (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 announcements dates of the rule as well as sustained increases in liquidity,
relative to NASDAQ Small Cap firms. This evidence is consistent 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.\(^{34}\) 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 Table 4.1). In addition, there is some evidence of substitution effects, i.e., 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).

\(^{34}\) 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 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.
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 about the use of already-compliant firms in the same market segment as unaffected or untreated. More research on this issue and, more generally, on the market-wide consequences of disclosure regulation is needed. There is also little research examining potential real effects from imposition of disclosure regulation in these U.S. settings, such as changes in managerial behavior, corporate investment, etc. We come back to this issue in Section 4.4 (in the context of different settings).

4.2. Studies on major changes in U.S. disclosure regulation

In recent years, there were two major changes in U.S. disclosure regulation, Regulation Fair Disclosure (Reg FD) and the Sarbanes-Oxley Act (SOX). Both changes have led to a large number of regulatory studies. As there are detailed surveys of these studies (e.g., Koch et al., 2013; Coates and Srinivsan, 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 related to these papers. Table 4.3 in the Appendix provides an overview and key examples (rather than a comprehensive list).

4.2.1 Regulation Fair Disclosure

Reg FD prohibits selective disclosure of material non-public 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 et al., 2013).

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

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 et al., 2013, for 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 et al. (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 et al. (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 et al. (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., Heflin et al., 2003; Bailey et al., 2003). Francis et al. (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 et al. (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 that this decline is concentrated in growth firms. There is also evidence that forecast accuracy (dispersion) decreases (increases) around Reg FD (e.g., Heflin et al., 2003; Bailey et al., 2003), particularly for forecasts that are made early in the year and for smaller firms (Agrawal et al., 2006). Again, it is not clear to what extent these changes in forecast properties are driven by Reg FD versus other concurrent events (Francis et al., 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, i.e., 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 et al. (2009) provide evidence that Reg FD constrains managers’ tendency to selectively leak good news ahead of public disclosures.36 In response, firms could make public disclosures in lieu of selective private disclosures. For instance, Bushee et al. (2004) show that firms switch from closed to open conference. Similarly, there is evidence that firms increase earnings guidance after Reg FD, for example, to compensate the loss in analyst coverage (Heflin et al., 2003; Gomes et al., 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 et al. (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 et al., 2006). There are the stock market boom and subsequent crash in 2000, the ensuing economic recession, decimalization

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36 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 et al., 2011; Soltes, 2014).
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 estimating solely 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 et al., 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 control group (e.g., Francis et al., 2006; Gomes et al., 2007; Chen et al., 2010). ADRs are exempted from Reg FD but likely affected by (many) concurrent events. However, as discussed in Francis et al. (2006), this control group is appropriate only if (i) ADR firms do not voluntarily comply with Reg FD and (ii) foreign jurisdictions do not adopt similar regulations around the same time. Regarding (i), 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 (ii), 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 (i) or (ii) but the extent to which this control group satisfies the parallel-trends assumption.\(^{37}\)

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.\(^{38}\) 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 specific disclosures.

Evidence on the effects of SOX focuses broadly speaking on four aspects: (i) the costs from complying with SOX (e.g., audit fees); (ii) the effects on accounting and auditing quality as well as capital-market reactions to 404 disclosures; (iii) the net effects on shareholder wealth using stock returns; (iv) corporate responses to SOX and avoidance behavior (e.g., going private). Table 4.3 provides examples for these outcomes. As with Regulation 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

\(^{37}\) 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 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.

\(^{38}\) 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).
compliance costs increase in firm size but at a decreasing rate. They 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. Illiev (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. Based on this design, he estimates that audit fees have increased by 86 percent 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.

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

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39 His study addresses a number of other concerns (e.g., incentives for firms to stay below the $75 million cutoff). See Gao et al. (2009) for evidence that some firms various actions to avoid crossing the public float threshold.
40 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.
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 et al. (2010) and Kang et al. (2010) analyze this effect and present evidence consistent with 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), there seems to be a decline in U.S. investment but this trend started in the early 2000s and hence 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 et al., 2008; Koh et al., 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 few exceptions (notably Iliev, 2010), the aforementioned studies are susceptible to confounding effects by concurrent events. They 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), i.e., 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 et al., 2013). For SOX, only few studies examine changes in market liquidity. Jain et al. (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 et al. (2008) traces out changes in liquidity over time, its primary benchmark is the pre-scandal 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.41 In addition to the studies on accounting quality, there is evidence suggesting that audit quality has improved. Dyck et al. (2010) show that auditors play a (relatively)

41 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.
larger role in detecting accounting-related fraud after SOX. DeFond and Lennox (2011) find that a large number of small audit firms exits the market for public company audits following SOX and that 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 note that 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 et al., 2007; Hammersley, 2008; Ashbaugh-Skaife et al., 2008; Feng et al., 2009) and that firms’ cost of capital increases around these disclosures (e.g., Ashbaugh-Skaife et al., 2009; Kim et al., 2011; Costello and Wittenberg-Moerman, 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 (see also Coates and Srinivasan, 2014).

Towards this end, a series of studies examines the net effect for shareholders using stock returns to 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 et al., (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. 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 into the month of July in 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 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.\footnote{Another alternative is to use foreign firms that are cross-listed in the U.S. but exempt from SOX. We have already discussed the pros and cons of this approach in the context of Reg FD studies. They are similar here.} 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 et al. (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.\footnote{For example, as Leuz (2007) points out, these events include the U.S. moving towards a war with Iraq and Congressional debates over the creation of the Department of Homeland Security.} 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 exempt from SOX (Litvak, 2007; Li, 2014).\footnote{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 and so this approach is not as tight as an analysis within cross-listed firms.} 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 to be much larger and likely have (endogenously) more exposure to the U.S.

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 et al., 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 et al. (2008) find that abnormal returns around legislative events are positively related to proxies for earnings management prior to SOX. Hochberg et al. (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 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 non-
lobbying 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 \textit{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 et al. (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.\textsuperscript{45} 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.

\textsuperscript{45} 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.
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 et al. (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 et al. (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 et al. (2007) is attributable to SOX.46 Consistent with this concern, Leuz et al. (2008) show that the frequency of going-private transactions is not related to SOX-related events. However, Kamar et al. (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 U.S. and elsewhere, and find that the propensity of small public target to be acquired by private firms (as opposed to public firms) has increased in the U.S. relative to elsewhere.

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46 In addition, Bartlett (2009) shows that going private does not per se exempt from SOX as firms may still have debt securities that require SEC reporting. Moreover, many of the going-private firms were so small that they never had to comply with SOX Section 404 (Coates and Srinivasan, 2014).
Leuz et al. (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 de-registrations 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 et al. (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.47 Similarly, there is evidence that foreign firms are more likely to delist and deregister their cross-listed securities in the U.S. (Marosi and Massoud, 2008; Doidge et al., 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 et al., 2009; Gao, 2011). The evidence for new cross-listings on U.S. exchanges is mixed (see Table 4.3).

Further evidence on avoidance strategies comes from Gao et al. (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.

47 It is also possible that firms go dark in order to avoid the additional scrutiny imposed by SOX. Consistent with this hypothesis, Leuz et al. (2008) document that, in at least some cases, controlling insiders appear to take firms dark to protect private control benefits and decrease scrutiny.
Overall, the revealed-preference approach produces relatively consistent evidence for 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 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. Despite the fact that both regulatory changes have been studied extensively, we are still far from answering the question of whether these regulatory changes were net beneficial to the economy and, more generally, whether the market-wide benefits of regulating disclosure exceed the aggregate costs. In fact, many of the documented effects around the regulatory changes are not necessarily causal, consistent with the identification challenges discussed in Section 2 of this review.

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.\textsuperscript{48} We begin with cross-sectional studies and then discuss studies that combine cross-sectional and time-series variation.

\textsuperscript{48} 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.
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 6. We note here 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, 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.\textsuperscript{49} 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 advancing research on the role of institutions. Moreover, they 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 of 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 country- and/or firm-level, exhibit significant associations with countries’ institutional factors (e.g., investor protection, judicial efficiency, rule of

\textsuperscript{49} For evidence on these associations, see La Porta et al. (2006). See also Section 6 for further discussion.
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 et al. (2004), Francis et al. (2005), Gelos and Wei (2005), Guedhami et al. (2006), Eleswarapu and Venkataraman (2006), Chen et al. (2009), Bilinski et al. (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 et al., 2005; Leuz et al., 2008; Lang et al., 2012; Maffett, 2012). For example, Aggarwal et al. (2005) find that 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 et al. (2008) 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 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. At the country level, corporate transparency and other institutional factors are likely jointly determined and hence not easily separable (e.g., Bushman et al., 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 also reflect firms’ practices, rather than regulatory differences. As the focus of the survey is regulation, we provide an overview of these studies and their findings in Table 4.4. but do not discuss them in further detail.
Instead, we turn to studies providing international evidence specifically on the economic effects of disclosure regulation. Glaeser et al. (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 intermediaries plays a key role in securities regulation and is an important element of investor protection. It also suggests that enforcement of disclosure regulation by specialized regulators may be more efficient than judicial enforcement.

La Porta et al. (2006) examine the links between securities regulation and financial development to 45 countries. They create a dataset 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 dataset, 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 have a significantly lower cost of capital. These effects are smaller when capital markets are more globally integrated, suggesting that capital market integration reduces the influence of countries’ legal and regulatory institutions on firms’ cost of capital. Frost et al. (2006) examine the link between required corporate disclosure at the exchange level and equity market development. They find that the 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.

Thus, the cross-country evidence generally supports the notion that more extensive disclosure regulation and stricter enforcement foster capital market development, improve market
liquidity and reduce the corporate cost of capital. Interestingly, enforcement typically has an incremental effect in these studies, rather than primarily a complementary (or interactive) effect. But again, it is important to note that the evidence is based on a relatively small number of observations and the identification in these studies is cross-sectional. It is therefore difficult, if not impossible, to isolate the effects of disclosure regulation and enforcement from the effects of other complementary institutions.50

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; Christensen et al., 2014a). 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 (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 by what in labor economics is called the “Ashenfelter dip.” Thus, 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 et al. (2014a) 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 offer better identification of regulatory

50 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 U.S. We therefore provide only a few examples in Table 4.4.
effects than a single regulatory event such as SOX. First, by measuring the regulatory effects in multiple countries at various points in time, the study 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 a market response to preceding events that gave rise to the directives, if there is one.51 Importantly, as discussed in Christensen et al. (2014a), 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 et al. (2014a) exploit that a few EU countries have large unregulated markets, which are not or 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.

Thus, this study yields causal evidence on the capital-market effects of securities regulation. But in contrast to the causal estimates of the studies discussed in Section 4.1 or 4.2, the estimates in Christensen et al. (2014a) 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 estimate is that market liquidity increases by roughly 10%, relative to pre-directive levels, illustrating that stricter securities and transparency regulation can have substantial economic benefits. A drawback of the research design is that it can be used only for outcome variables that can be measured over shorter intervals and are not anticipatory in nature. Moreover, the estimated magnitude of liquidity benefits is likely specific to the setting, meaning it depends on prior

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51 The fact that probably not all EU countries would have adopted these directives at this stage of their development implies that the estimates are more informative and generalizable because there is an element of imposition from the EU.
regulation, the particular directives, countries’ implementation choices, etc. Put differently, the generalizability of the estimate is not obvious.

Consistent with this point, Christensen et al. (2014a) show that, even within the EU, the liquidity benefits differ considerably with 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. This finding implies that with disparate prior conditions imposing the same regulation can make countries diverge more, rather than harmonize their markets. More generally, 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 cross-sectional evidence on capital-market benefits but also 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 et al., 2006; Coffee, 2007) and, more recently, the implementation of regulation (e.g., Christensen et al., 2014a). 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 U.S., foreign firms subject themselves to these laws and their enforcement. 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 et al. (2003a and 2003b), Doidge et al. (2004 and 2009), Bailey et al. (2006), Hail and Leuz (2009) and Ammer et al. (2012) show that foreign firms with U.S. cross-listings raise more external finance, have higher valuations (Tobin’s q), a lower cost of capital, stronger earnings announcement

52 Examples are the Foreign Corrupt Practices Act, Rule 10-b5, SEC enforcement actions, and U.S.-style class action law suits.
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 et al., 2009; and 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, i.e., the concern that 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 et al., 2009; Hail and Leuz, 2009; Ammer et al., 2012). However, selection models require a valid instrument, but it is hard to satisfy the exclusion restriction for a valid instrument in this 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. 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 documented cross-listing effects are still unclear. Extant studies do not isolate the mechanism. Thus, 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.53 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 disclosure regulation

In this section, we discuss studies on the economic consequences of disclosure regulation that did not neatly fit in the earlier sections. In this category are, for instance studies, on 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 literature on disclosure regulation. Thus, the purpose of this section is to discuss illustrative examples to encourage researchers to look for ways to study disclosure regulation outside the traditional settings in order to generate new insights. Of particular relevance in this regard are studies analyzing real effects (as defined in Section 3), i.e., effects on the behavior of firms. The expectation that mandating disclosure induces desirable and

53 See also Licht (2003).
discourages undesirable behavior is an important motivation for transparency regulation in many areas, such as corporate governance, consumer protection, health care, and food safety. However, there is still relatively little work on the potential real (or behavioral) effects of disclosure regulation.

There are numerous studies in accounting and finance that aim to establish the link between disclosure and corporate real effects, e.g., changes in investment behavior. But most of these studies focus on firm-level, and hence largely voluntary, disclosure choices, rather than the effects from regulatory changes or differences in disclosure regulation. We have discussed these studies in Section 3. As noted there, they can primarily speak to the existence and magnitude of the economic link but can say little about regulatory consequences.

To the extent there are studies on the real effects of disclosure regulation, they tend to focus on firms’ investment policies and behavior. We summarize key studies in Table 4.5. It is important to note that, for conceptual reasons, investment effects around changes in disclosure regulation are difficult to interpret. Disclosure regulation also affects the cost of capital and hence an increase in investment could simply reflect a decline in the cost of capital, which in turn expands the set of positive NPV projects due to a lower hurdle rate. Thus, if the goal is to show that greater transparency facilitates monitoring, improves governance and ultimately induces managers to make better investment decisions, then showing an increase in investment is not sufficient. For this reason, studies typically analyze the efficiency of investment, rather than growth in investment.

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 et al., 2011; Chen et
These studies find that greater transparency is associated with higher investment efficiency.\textsuperscript{54}

Second, there are studies examining real effects around regulatory changes. For instance, there are several studies analyzing investment effects related to 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). To provide more direct evidence of regulatory effects on firm investment, Cheng et al. (2013) exploit disclosures of internal control weaknesses under SOX. They document that firms with material internal control weaknesses exhibit investment inefficiencies compared to propensity-matched firms and that these inefficiencies disappear in the years after the required disclosure, suggesting that mandated disclosure of control weaknesses remedies the inefficiencies.

It is important to note that more extensive disclosure regulation could also have negative real effects to the extent that the costs of the mandate outweigh its benefits and firms engage in avoidance strategies. Consistent with this notion, Gao et al. (2009) provide evidence that the size-based exemptions in SOX induce firms to remain small, leading them to take real actions that inhibit firm growth, such as investment cuts. However, it is worth noting that the size-based cutoff is critical for the real effects and not the disclosure per se.

Badertscher et al. (2013) examine whether greater public-firm presence in an industry leads to a greater responsiveness of private firms’ investment to investment opportunities, i.e., more efficient investment. The basic idea is that U.S. public firms are subject to strict disclosure requirements and hence their presence improves the industry information environment, conferring a

\textsuperscript{54} As noted in Section 3, these studies typically do not allow causal interpretations and hence provide primarily suggestive associations. This evidence is nevertheless important as it points to the potential importance of this relation at a macro level. In some cases, studies exploit firm-level variation to introduce more extensive fixed-effect structures. See, e.g., Shroff et al. (2014).
positive externality on private firms in the same industry. The results support this idea, even after controlling for firm-fixed effects.\textsuperscript{55}

More evidence on system-wide benefits and real effects from disclosure regulation comes from Granja (2014). He analyzes the effects of disclosure regulation on bank behavior and more broadly the stability and development of the banking sector. The study exploits a natural experiment from the National banking era in the U.S. during which several types of banks coexisted and several state regulators introduced new reporting regulations and conducted periodic on-site inspections of banks. The coexistence of state and national banks allows for difference-in-differences estimation using national banks as a local control. Granja (2014) finds that, after the introduction of disclosure regulation, state banks’ failure rates go down, their capital ratios decrease (suggesting they need to hold less capital to reassure depositors), and their rates on demand deposits decrease to about the same level as the rates of the national banks. Similarly, proxies for financial development increase. The research design of this study is tight because state (but not national) banks within the same state adopted the new regulation at different points in time allowing for the introduction of state-time fixed effects and triple differences. The study makes a first step in the direction of a causal link between disclosure regulation and financial development. It further shows that, at least in this historical setting, ex-post concerns about transparency in a crisis (e.g., that more transparency could facilitate runs) do not outweigh ex-ante benefits from better transparency.

The small number of studies on the real effects of disclosure regulation illustrates that it is rare to find a setting in which one can cleanly identify the effects of a disclosure regime on corporate behavior. However, there are studies outside accounting and finance that exploit disclosure mandates, such as restaurant hygiene scores, report cards in health care settings,\textsuperscript{55} The latter does not address concerns about the reflection problem, i.e., the possibility that the results reflect industry shocks, rather than public-firm presence. But 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.
environmental disclosures and food labels (e.g., Bennear and Olmstead, 2008; Lu, 2012; Kolstad, 2013; Christensen et al., 2014b). We summarize a few key studies in Table 4.5.

An important example for the effects of disclosure regulation on behavior 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 basic economic question is whether an increase in the provision of information 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 three-part TV report showed viewer unsanitary kitchens. The authors show that hygiene grade cards cause 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 reduction in foodborne illness hospitalizations is the real effect as it is consistent with restaurant investments in hygiene and food quality (although the latter is not directly observed). It is important for this interpretation that the authors provide evidence that the improvement in hospitalizations is not fully explained by consumers substituting from poor hygiene restaurants to good hygiene restaurants. This issue would not arise if we could observe and analyze restaurant investments (or changes in food quality at the restaurant level). More generally, it illustrates that real effects take place through consumer (or investor) responses and that it is important yet challenging to account for demand effects to appropriately estimate the real effects on firm behavior.

Other studies on the effects of disclosure mandates in non-capital market settings also illustrate that consumers are sensitive to the information provided, that this response is critical to the firm response, and that firms adjust their product quality. However, Lu (2012) shows that when product quality is multi-dimensional, firms reallocate their effort and investments to dimensions that
are being measured and reported and away from unmeasured ones. Thus, the net effect on product quality is not obvious. Similarly, Dranove et al. (2003) show that health care report cards may give doctors and hospitals incentives to decline treatment to more difficult, severely ill patients. These studies illustrate that disclosure regulation can also have thorny and unintended consequences.\textsuperscript{56} They also highlight that proper measurement of the disclosed information is critical for disclosure to have the intended incentive effects. Moreover, it is important that consumers (or more generally the receiver of the information) can separately evaluate the inputs into firm behavior (e.g., differences in the illness of incoming patients) to mitigate the screening effects documented in Dranove et al. (2003). However, what constitutes an unintended effect can differ across settings. In health care, screening of patients and doctors selecting to treat healthier patients for better report outcomes is likely a major concern. But an analogous effect in a corporate setting could be intended or at least acceptable. For instance, if disclosure requirements in auditing make high-quality auditors refuse to audit relatively risky clients, then it is conceivable that such effects are indeed intended, provided that investors can assess auditor quality (independently from the clients, which underscores the earlier point about the evaluation of inputs).

In sum, there is a growing literature on the effects of disclosure regulation on firm behavior. However, the existing evidence is fairly limited and more studies are needed to better understand the extent to which disclosure regulation can be used as an incentive device to induce desirable and discourage undesirable behavior. At present, the literature suggests that the effects are ambiguous.

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, e.g., a requirement to

\textsuperscript{56} Another example is the “licensed to be biased” effect in Loewenstein et al. (2011).
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.\footnote{Early studies in the international accounting literature largely focus on voluntary adoptions of entire sets of accounting standards (e.g., IAS or U.S. GAAP). See, e.g., Harris and Muller (1999), Leuz and Verrecchia (2000), Leuz (2003), Daske (2006). We do not review these studies here. See Soderstrom and Sun (2007) for a review.} Not surprisingly, there is a large and growing literature examining the large number of firms and countries that have adopted IFRS over the past 15 years. The global switchover to mandated IFRS reporting represents a profound change in the “rules component” of reporting regulation, possibly the largest in accounting history. 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, and hence used extensively.

Therefore, this section not only intends to give an overview of key empirical findings with respect to mandated adoption of IFRS but also to discuss challenges that researchers face when using the “IFRS laboratory” for research-design purposes as well as when studying the economic effects 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 et al. (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 in our view 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 et al., 2000, 2003; Leuz et al., 2003; Ball and Shivakumar, 2005; Burgstahler et al., 2006, Hail et al., 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 (e.g., about future warranty risks) and hence applying the standards involves subjective assessments of the future. The key point is then 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 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 factors, many empirical studies clearly indicate the importance of managerial reporting incentives for observed reporting and disclosure practices (e.g., Ball et al., 2000; Fan and Wong, 2002; Leuz et al., 2003; Haw et al., 2004; Burgstahler et al., 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 et al., 2003; Ball and Shivakumar, 2005; Burgstahler et al., 2006; Lang et al., 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, as we discuss in more detail below.

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 et al., 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 et al. (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 brings these benefits.\(^{58}\) 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 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 concurrent shocks. It is possible, if not likely, that many other regulatory, technological and market shocks occurred around the same time as the switchover to IFRS. The potential myriad of other institutional and market changes makes it difficult to isolate the effects of IFRS adoption.

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 et al. (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.

\(^{58}\) While our discussion focuses on the benefits consistent with most empirical studies, we note that similar identification challenges arise equally 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 they have to switch to IFRS. An estimate of the cost effects around IFRS adoption would likely capture the pent-up demand for other accounting system changes.
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 special” that it is not clear that unrelated economic shocks affect them in the same way as IFRS-adopting firms. 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 et al., 2008; Daske et al., 2008; Tan et al., 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 approach captures only differences in the properties of the reporting standards. Unfortunately, from a research-design perspective, changes in the entire set of standards are relatively rare.

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 and provide a more extensive overview in the Appendix (Tables 5.1-5.4). We first discuss possible changes to the properties of firms’ disclosures and reported numbers, then capital-market outcomes, followed by non-market 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. Some of the evidence stems from voluntary IFRS or earlier IAS adoptions. Other studies exploit the fact that U.S. cross-listed, foreign firms provide local GAAP or IFRS financial statements 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 Table 5.1 in the Appendix. Overall, the empirical evidence from these studies is quite mixed.

For example, Barth et al. (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 post period. However, as the evidence stems from voluntary adoptions, it is difficult to attribute the reporting effects to the change in reporting standards. Without a correction for 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 et al. (2015) compare changes in reporting properties for voluntary and mandatory IFRS adopters in Germany. They examine similar reporting properties as Barth et al. (2008) and find that improvements are confined to voluntary adopters.

Using U.S. cross-listed firms, Gordon et al. (2009) 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 compares properties holding the firm constant. The latter study uses U.S. firms as a benchmark. Hence, other institutional factors that differentially affect foreign and U.S. firms, e.g., weaker SEC enforcement for foreign firms, can still 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 et al., 2012; Ahmed et al., 2013; Christensen et al., 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 analysts receive better information after IFRS adoption (e.g., Byard et al., 2011; Tan et al., 2011; Horton et al., 2013).

In sum, the evidence on changes in reporting properties (or quality) after IFRS adoption is mixed. Moreover, studies often 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. But several studies also find that, after conditioning on other institutional variables, the results are stronger in countries for which local GAAP was more distant from IFRS (e.g., Byard et al., 2011; Tan et al., 2011), suggesting that accounting differences conditionally play into the results.

5.3.2 Capital-market outcomes

As mentioned earlier, most studies focus on the espoused capital-market consequences 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 et al., 2010), an increase in market liquidity and a decline in firms’ cost of capital (Daske et al., 2008, 2013; Florou and Kosi, 2009; Li, 2010), an increase in stock price informativeness (Beuselinck et al., 2009), larger foreign investments in firms domiciled in
IFRS countries (Brüggemann et al., 2011; DeFond et al., 2011; Beneish et al., 2012), and a reduction in home bias among U.S. investors (Khurana and Michas, 2011; Shima and Gordon, 2011). We list and summarize key studies in Table 5.2. in the Appendix.

Overall, the evidence overwhelmingly suggests that mandatory IFRS adoption is associated with significant capital-market benefits. However, a key question is how we should interpret this evidence. In light of the strong conceptual links between better disclosure and reporting and certain capital market outcomes such as market liquidity, which we reviewed in Section 3, one potential interpretation of the capital-market evidence is that mandatory IFRS adoption has improved financial reporting and, more generally, that mandating high-quality reporting standards such as IFRS yields considerable capital-market benefits. However, these conclusions would be premature. In our view, it is not clear to what extent the capital-market outcomes are indeed attributable to IFRS adoption, i.e., a mandated change in reporting standards. We have to be cautious about how we label and describe the findings, in particular, with respect to language that suggests causal effects. The capital-market effects are best described as effects that occur around the time 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, such regime change analyses face a number of serious research-design challenges and are susceptible to confounding effects due to unrelated concurrent events. However, even setting these challenges aside, there are a number of 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, they should not exhibit capital-market outcomes that stem from the change in accounting standards. But Daske et al. (2008) show based on several proxies that voluntary adopters exhibit larger capital-market effects after IFRS become mandatory than first-time mandatory IFRS adopters. This result 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 solely or primarily be attributed to IFRS adoption. Christensen et al. (2013) find similar patterns in capital-market outcomes for voluntary and mandatory adopters, although there are also studies showing that first-time mandatory adopters exhibit larger effects than voluntary adopters (e.g., Byard et al., 2011; Tan et al., 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, they 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. 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 network or comparability effects for voluntary adopters should be stronger in countries 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 note that few studies around IFRS adoption examine such market-wide effects. Most IFRS studies understandably focus on the effects on individual firms 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.\textsuperscript{59} However, information spillovers, comparability (or network) effects and other market-wide effects that could give rise to positive externalities are typically 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 we need such evidence to assess the desirability of reporting regulation. The lack of evidence on \textit{market-wide} effects of disclosure and reporting regulation therefore implies that cost-benefit analyses of changes in the reporting standards, such as IFRS adoption, are currently incomplete at best.

\textbf{5.3.3 Non-market and real effects}

While most IFRS studies focus on capital-market effects, there is growing evidence on non-market and real effects with respect to corporate behavior. This research can be divided into two categories: (i) studies that are primarily interested in the switchover to IFRS and its consequences; (ii) studies that exploit the change to IFRS as an exogenous shock but are primarily interested in the link between corporate reporting and real effects. We tabulate key examples for both type of studies and their results in Appendix Table 5.3. Commonly used proxies are the investment-cash flow sensitivity and the sensitivity of investment to growth opportunities.\textsuperscript{60} As the table illustrates, this literature is still relatively young and comprises far fewer studies than the literature analyzing capital-market effects.

Examples for studies in the first bin are Biddle et al. (2011), Schleicher et al. (2012), and Chen et al. (2013). All three studies examine the efficiency of firms’ investment decisions

\textsuperscript{59} The country-month-level analysis in Daske et al. (2008, Section 5) is an example for 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 tight closely to the rollout of IFRS (and other concurrent changes to the reporting system) in a country.

\textsuperscript{60} Other observables investigated in the “real effects” of IFRS adoption literature include cross-border (FDI) investment, improved corporate governance, and trade in real goods. See Table 5.3 in the Appendix.
following mandatory IFRS adoption. They 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, i.e., stronger effects for smaller firms and insider economies (Schleicher et al., 2012) and for countries in which local GAAP and IFRS diverge more (e.g., Biddle et al., 2011).

Examples for studies in the second bin are Hail et al. (2014) and Shroff et al. (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 et al. (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.

Real-effects studies also face serious identification challenges and they 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 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; Withed and Wu, 2006; Hadlock and Pierce, 2010; Bushman et al., 2012). In order to show real effects, studies need a benchmark for optimal investment behavior. Another aspect that could make identification much harder is that deviations from optimal investment behavior can go in both...

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61 For papers in this bin, it is critical that IFRS adoption was indeed a major change in the information environment and that this change is exogenous with respect to the outcome of interest. While it is questionable that IFRS adoption alone was a major change, given the capital-market research reviewed in the previous section, these papers technically do not need to separate IFRS and other concurrent changes in the reporting system.
directions, i.e., there can be over- and under-investment. Perhaps this is an area where structural work could make some headway.

We conclude our review of IFRS studies noting that we have very few academic studies on the costs of IFRS adoption, be it to firms, managers or investors.\textsuperscript{62} In particular, we need studies on indirect (and less obvious) costs of IFRS adoption or mandated changes in reporting standards. These studies would be an important input into a cost-benefit analysis.

5.4 Heterogeneity in the findings of IFRS studies and the interpretation of the findings

A pervasive finding in the literature on mandatory IFRS adoption is that the results exhibit considerable cross-sectional heterogeneity, i.e., 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) offer firms substantial discretion. This reporting discretion combined with a myriad of economic incentives faced by both firms and managers is expected to lead to 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.

But the cross-sectional heterogeneity in economic outcomes around IFRS adoption also matters for two more reasons. First, it plays an important role with respect to 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

\textsuperscript{62} There is some evidence on the costs of IFRS adoption in commissioned reports. See, e.g., ICAEW (2014). Kim et al. (2012) and De George et al. (2014) examine changes in audit fees around IFRS adoption. Ball et al. (2014) and Brown (2014) examine changes to debt contracting around IFRS adoption.
isolation and depend upon and interact with other institutions in the economy. For the remainder of Section 5, we discuss these two aspects. In addition, we highlight conditional outcomes of various empirical studies in the Appendix Table 5.4.

Beginning with Daske et al. (2008), studies generally find that the observed capital-market outcomes surrounding the mandatory introduction of IFRS are weaker, or in some cases, non-existent in countries with weak legal regimes and limited reporting incentives. In many instances, they also find interaction effects between the strength of countries’ legal institutions and the differences between their local GAAP and IFRS. For instance, Byard et al. (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 ensuring that the new standards are properly applied and enforced. 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 et al. (2013) make this argument and specifically point to the EU, 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.

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63 Viewed more broadly, the evidence is also in line with the notion of complementarities in that the effects of IFRS adoption depend on other elements in countries’ institutional infrastructure. See Section 5.5.
It is generally difficult to rule out that other institutional changes that are unrelated to financial reporting drive the observed capital-market benefits. 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. Towards this end, Daske et al. (2008) and Christensen et al. (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 at the same time, which makes the events separable. However, this strategy requires outcome variables that can be measured at relatively high frequency and that are not anticipatory (Christensen et al., 2013). Outcomes that are available only with an annual frequency cannot separate the effects as they are unable to exploit the variation in firms’ fiscal year ends. Anticipation also destroys the identification strategy as the outcome variables no longer expected to follow the fiscal-year end pattern.

Using this approach Christensen et al. (2013) show that unrelated institutional reforms in the EU cannot explain the changes in market liquidity around IFRS adoption. The observed liquidity changes are still present when controlling for country-quarter fixed effects. This finding suggests that the liquidity changes are related to financial reporting changes. However, it does not imply that the results are necessarily related to IFRS adoption. Countries could also make 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, i.e., each element contributes independently. The bundling of reforms provides some very challenging identification issues. 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 the effects should be related to firms’ fiscal-year ends.

Consistent with the concern about bundled changes to the financial reporting system, Christensen et al. (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 IFRS adoption. Overall, the evidence suggests that IFRS adoption had little, if any, stand-alone effects on market liquidity.

Similarly, Brown et al. (2014) examine changes in analyst forecast properties around mandatory IFRS adoption and find no effect after IFRS adoption once the effects of enforcement are controlled for. Both studies raise the possibility that capital-market effects are entirely driven by enforcement, rather than changes in the reporting standards. However, it is important to note that identification of the enforcement results is cross-sectional only. 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 (Barth and Israeli, 2013). Disentangling the two effects amounts to asking the question 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.
Generally speaking, studies do not present causal evidence on the effects of mandatory IFRS adoption, i.e., the change in reporting standards, with respect to various reporting and economic outcomes. Hence, the evidence on the role of reporting standards is actually still fairly limited. It is generally more informative when we broaden the perspective and view it as examining the combined effects of various institutional reforms (rather than particular elements).

5.5. The links between reporting standards and other 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. 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 will be superior, or effective, in countries that have different institutions and may lack potentially complementary institutions to support the effective application and enforcement of the uniform global standards (see also Ball, 2001, 2006; Hail et al., 2010a, 2010b; Leuz, 2010, Walker, 2010, and Wysocki, 2011). Like any institutional mechanism, reporting standards (as well as other elements of the reporting system) likely have arisen to facilitate complex and changing transactions in a country, which has a given set of existing endowments and institutions (see, e.g., Watts and Zimmerman, 1983). Thus, there is an inherent interdependency and complementarity between accounting and non-accounting 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 et al., 2010a, 2010b). The new set of standards will be subject to the same
institutional and market pressures that shaped the old set of standards in the first place. Thus, unless other institutional factors across countries are also converging, countries starting with a common set of reporting standards are likely to drift apart over time, e.g., due to local adaptation and interpretation (e.g., because the new set of standards are not a good fit). 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, recent empirical has explored the determinants, outcomes and interplay between reporting and other non-accounting institutions, mostly in cross-country comparisons. This literature has been labelled “New Institutional Accounting” (Wysocki, 2011). In Table 5.5, we summarize key studies in this literature and the institutional variables that have been proposed and shown to be individually related to international differences in disclosure and reporting. Collectively, the range of proposed institutional variables is large and includes various variables related to the reporting system, such as accounting standards quality (Hung, 2000), securities regulation and stock exchange rules (La Porta et al., 2006; Frost et al., 2006), reporting enforcement (Christensen et al., 2013), and audit enforcement (Brown et al., 2014). In addition, many other institutional factors have been investigated, including legal origin (Ball et al., 2000), religiosity (Boone et al., 2013), stock market wealth (Riahi-Belkaoui, 2005), tax enforcement (Haw et al., 2004), institutional investment (Covrig et al., 2007), investor protection (Leuz et al., 2003), culture (Hope et al.2008), societal trust (Nanda and Wysocki, 2013), legal institutions (Hail and Leuz, 2006), government quality (La Porta et al., 1999), corruption (Bhattacharya et al., 2003), book-tax alignment (Hung, 2000), creditor rights (Gupta et al., 2008), auditor quality (Francis and Wang, 2008), ownership structure (Core et al., 2014), linguistic properties (Brochet et al., 2012), financial analyst infrastructure (Bae et al., 2008), corporate social responsibility (Dhaliwal et al., 2012), political institutions (Bushman and Piostroski, 2006).
The expansive international accounting literature has amassed an empirical “factor zoo” that has grown to at least 70 country-level institutional factors that individually have explanatory power for disclosure and reporting outcomes around the world.64 These studies generally document that various institutional factors individually affect reporting outcomes or a given factor individually interacts with another institution and thereby influences conditional outcomes. However, as many of the empirical studies summarized in Appendix Tables 5.1-5.4 indicate, the stand-alone reporting impact and economic effects of IFRS are difficult to isolate. Accounting standards are just one of many correlated institutional features in a country that influence firms’ and managers’ reporting incentives and thus it is difficult to unambiguously isolate IFRS effects from numerous other institutional factors.

The same issue arises with respect to the other institutional variables from countries’ reporting systems. However, this issue is in our view insufficiently recognized in this literature. Studies often focus on each newly hypothesized institutional factor and then examine whether this factor has a significant direct or interactive relation with disclosure and reporting outcomes. However, the question conceptually is whether the proposed institutional factor has explanatory power in the presence of a broader set of known and documented institutional factors.65 At this point, it is unclear which institutional factors are: (i) incrementally important in determining or mediating firms’ reporting and disclosure practices, and (ii) fundamental primitives that underlie firms’ reporting practices.66

64 Finance researchers have also identified a similar issue in the empirical asset pricing literature and Cochrane’s 2010 AFA Presidential Address (2010) called for future research to address issues “multidimensional challenge”. For example Zhang, et al. (2014) test the incremental explanatory power of over 100 “return predicting signals” (RPS) from the prior literature in a broader multiple regression setting. Similarly, Harvey, et al. (2014) suggest a framework for multiple hypothesis testing in the presence of a myriad of seemingly-separate empirical factors.

65 However, as noted in section 4, empirical researchers must exercise caution in undertaking and interpreting institutional factor “horse races” given that various institutional proxies also differ in measurement error.

66 In recent work, Isidro et al. (2015) explore the associations among these 70+ institutional factors and their incremental explanatory power for observed reporting practices around the world. The evidence suggests that very few institutional factors provide meaningful additional explanatory power for reporting outcomes in the presence of a
In addition, there is a “curse of dimensionality” problem, which arises because most cross-country studies have to rely on 30-40 country observations, yet there are plausibly over 70 known candidate factors that have been used as country-level explanatory variables in the literature. Furthermore, many of these 70+ institutional variables are time-invariant or evolve very slowly and thus the use of time-series observations generally does help solve the problem (see also Guiso et al., 2015). The latter also poses major problems when studying associations between various 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 setting, the 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 changing one element may make the system (or economy) worse off even when the element itself improves along a particular quality dimension. It therefore might make sense to start more descriptively by simply documenting a broader structure of patterns in institutions and associated outcomes using factor and cluster analysis techniques. These regularities can help point to more plausible theories or mechanisms for explaining reporting outcomes. While the limited set of countries in the world prevents unequivocal (and causal) identification of which institutions are the core drivers of economic outcomes, it is still useful descriptively to know which types of institutional characteristics tend to be observed together. Such evidence could reveal (or be a starting point for) possible complementarities. For example, analyses in Leuz et al. (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.\textsuperscript{67}

In summary, there has been a recent proliferation of studies documenting various determinants and outcomes of disclosure and reporting choices around the world. However, the determinants and outcomes of both accounting institutions (including standards such IFRS) and non-accounting institutions are fundamentally intertwined and very difficult to separate. Overall, there are still many unanswered questions about the determinants, effects, interdependency and efficiency of observed reporting and non-reporting institutions. The literature has not found a way to adequately analyze and understand the multiple endogenous and complementary institutional ‘bundles’ observed around the world. Without major innovations, the standard regression techniques currently used in empirical studies have likely reached their limits for cross-country studies. However, as discussed in Wysocki (2011), other techniques such as structural estimation may hold promise to move this literature forward. Structural estimation, which has also been used in the macro literature, has the potential to provide additional insights on the interrelations (and causal links) between many endogenous and possibly complementary institutions. While this 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. It may also help identify which factors are more likely to be economic primitives that affect directly affect outcomes and which factors are associated outcomes or second-order mediating factors. In summary, many research opportunities still exist to better understand the economics and mechanisms underlying the observed associations between reporting, non-reporting institutions and economic outcomes.

\textsuperscript{67} See also Isidro et al. (2015) who undertake factor and cluster analyses for a comprehensive set of institutional variables around the world.
6. Conclusions and Suggestions for Future Research

In this article, we review the empirical literature on the economic effects of financial reporting and disclosure regulation, synthesizing U.S. and international evidence. The focus of our review on regulation and related empirical studies reflects the central importance of standardization and regulation for financial accounting as well as the policy relevance of these studies. However, this review is not meant to advocate in favor 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.

We begin by identifying and discussing key firm-specific as well as market-wide costs and benefits of firms’ reporting and disclosure activities, which can and are regularly used to evaluate financial reporting and disclosure regulation. Thereafter, we review studies on disclosure regulation, both in the U.S. and internationally. Next, we discuss studies on the economic effects of reporting standards, with a particular emphasis on IFRS adoption, and highlight that standards cannot be viewed in isolation from other elements of countries’ institutional frameworks. In this section, we summarize the main conclusions from our literature review and provide numerous suggestions for future research.

6.1 Main conclusions

Overall, five major conclusions emerge from our review. First, evidence on the causal effects of disclosure and financial reporting regulation is 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. For most regulatory changes, we are not able to provide causal estimates of the costs and benefits. Thus, while we have a lot of evidence that can be used in qualitative cost-benefit
analyses, we are still far from being able to perform *quantitative* cost-benefit analyses. Thus, there are still major opportunities for future research.

Second, and related to the first point, our review highlights a general paucity of evidence on market-wide effects from regulation and/or externalities. Such evidence is obviously critical for the economic justification of regulation in the first place. As discussed in Section 2, it is also a critical input for regulators and policymakers who weigh the cost-benefit tradeoffs when evaluating current and proposed regulation. The evidence from regulatory studies primarily documents economic consequences for individual firms in a market or an economy. However, we generally lack evidence that financial reporting standards and disclosure regulation produce information spillovers, positive externalities for other firms and/or network effects. One potential reason is that the identification of such indirect effects is arguably even harder than the identification of direct economic consequences on firms or investors (see also Section 2). We also have almost no evidence on potential welfare effects from reporting and disclosure regulation, except perhaps for return and valuation effects for the corporate sector. Moreover, because studies typically do not identify or compute counterfactuals, the current empirical literature has little to say about the efficiency or desirability of reporting and disclosure regulations. This paucity of evidence poses major challenges for cost-benefit analyses.

Third, the literature on disclosure regulation exhibits a heavy emphasis on changes in the U.S. Each major regulatory change in the U.S. has been studied extensively. There is much less evidence for other countries, despite the fact that there have been major changes in reporting and disclosure regulation in other jurisdictions. It is plausible that many basic tradeoffs are similar

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68 There is a nascent literature on peer effects in accounting, e.g., from accounting frauds (e.g., Gleason et al., 2008; Durme and Mangen, 2009; Beatty et al., 2013). But see also Manski, (1993) and Angrist (2014) on the problems of estimating peer effects (which likely also apply to the estimation of externalities). There is also earlier work on information transfers (e.g., Foster, 1981; Olsen and Dietrich, 1985; Clinch et al., 1987; Baginski, 1987; Han et al., 1989; Han and Wild, 1990). However, these studies are not focused on regulatory effects.
across countries and hence there are research design advantages to studying the effects in one of the largest economies with widely-available data. But studying other countries should give us a richer understanding of the many facets of regulatory effects. Moreover, from an identification perspective, settings in other countries could offer research-design opportunities that may not be available in a U.S. setting (e.g., a staggered implementation). Thus, we encourage researchers to seek such settings, including non-traditional disclosure and reporting settings.

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. Section 5 of our survey highlights that few studies in this literature are likely to identify economic effects that are attributable to IFRS adoption, i.e., the change in accounting standards. In the EU, where most of the studies document the strongest economic effects, IFRS have adopted amidst a series of other (unrelated) institutional reforms, which makes it difficult to isolate these concurrent institutional changes from IFRS adoption. Moreover, several countries have adopted IFRS together with other changes to the reporting infrastructure, which are most likely intended to support IFRS adoption (e.g., reporting enforcement). As shown in Christensen et al. (2013), this bundling further complicates the identification of IFRS effects. It also provides an example for a more general issue highlighted in our review: there are important interactions and complementarities between reporting regulation and other institutional factors. These interactions provide major opportunities for future research but also pose major difficulties for identification and economic analysis.

Finally, to make further progress on the identification and estimation of regulatory effects and hence cost-benefit analysis, researchers likely need help from policymakers and regulators. For example, a major issue for empirical studies is that regulation is often required as of a particular
date, which makes the analysis susceptible to confounding effects, be it concurrent but unrelated economic shocks or market responses to the events that gave rise to the regulation. To mitigate this issue, policymakers and other parties involved in drafting regulation could stipulate that new regulation be implemented in a staggered fashion, which would greatly facilitate the economic analysis of regulation and post-implementation reviews. Such an introduction is often viewed as not feasible because it is (by design) “arbitrary” and hence it violates fairness considerations. At the same time, new regulation can have significant costs and unintended consequences and hence it makes sense to study the effects carefully. In our view, we need to balance “regulatory risks” with the fairness concerns about “arbitrary” implementation. More generally, our point is that in designing regulation some consideration should be given to ex-post analysis, including provisions to collect the necessary data, which in turn could be made available for research and cost-benefit analysis. In addition, researchers and regulators could jointly devise pilot studies and field experiments, which would shed light on potential regulatory effects and greatly facilitate ex-ante analyses. Given the recent push for economic analysis and the policy debate over cost-benefit analysis (e.g., Posner and Weyl, 2013, 2014; Bartlett, 2014; Coates, 2014; Cochrane, 2014), such initiatives could have significant benefits.

6.2 Suggestions for Future Research

Based on our literature review, we identify the following seven specific areas and topics for future research on the economic consequences of financial reporting and disclosure regulation.

First, consistent with the observation that few empirical studies on financial reporting and disclosure regulation provide causal inferences, we call for more research on regulatory effects

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69 The staggering of regulations could be along at least two potential dimensions. First, components of a “regulatory package” (including individual rules, monitoring of rules, and enforcement of rules) might be staggered in their implementation to facilitate separate evaluation of the effects of each component in the regulatory package. Second, the new regulation could be applied to cohorts of firms in time-staggered fashion to allow for better identification of regulatory effects using regulated and yet unrelated firms.
using experimental settings, in which identification is given a priority. Such work could exploit natural experiments and, in particular, staggered implementations and regulatory thresholds. Towards this end, it is critically important that researchers understand the institutional features of their setting and can articulate why it affords a plausible identification of the regulatory effects (internal validity). But it is also important that researchers connect their setting with a larger economic question to provide policy-relevant and more generalizable insights (external validity), rather than “cute” identification-driven studies. Conversely, external validity is a primary issue for lab and, perhaps to a lesser extent, for field experiments. For both of them internal validity should be achieved by design. Another approach that has not been used much is structural estimation.70 This approach allows the computation of counterfactuals, which makes it particularly suited for regulatory questions. For this approach, 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.

Given the tradeoff between external and internal validity, there should also be a role for theory-driven, but more “descriptive” empirical studies that do not offer clean identification. Such work could provide fundamental ideas and new insights that help advance the literature and spawn future theoretical and (better-identified) empirical research. In such studies, it will be important to be clear about the fact that the paper is more descriptive in nature and that there are potentially several explanations for the results. For this reason, this work should 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 benefits of regulation are not self-evident, highlighting that we need to tradeoff regulatory

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70 Gerakos and Syverson (2014) provide an example for this approach with respect to audit regulation.
and market failures. At the same time, the largest and arguably most successful capital markets generally exhibit strong disclosure and securities regulation. Do these markets thrive because of regulation or in spite of it? As noted above, we have little (causal) evidence that we could bring to bear on this question or could use in quantitative cost-benefit analyses that try to answer this question for a specific setting. In particular, we lack evidence on externalities, social costs and benefits, market-wide or network effects. For instance, we need more work along the lines of Dyck et al. (2014) that estimates the social costs of fraud. Similarly, we need more evidence on the indirect costs of regulation (e.g., in stifling competition and innovation). In contrast, disclosure regulation could make 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).

A different argument that could be explored is that reporting regulation is pervasive because contracts and information are fundamentally incomplete and market participants have only bounded rationality. In this situation, reporting 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. Financial reporting standards and disclosure regulation strike us as such a “coarse” default solution.

Another market-wide benefit of disclosure and reporting regulation could be its contribution to the stability of financial markets, e.g., by mitigating asset bubbles and financial crises. During times of technological and financial innovation, which often precede asset 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, and also “ground

71 See, e.g., the tradeoff between precision and shared understanding in the work on optimal communication by Morris et al. (2006). See also Thaler and Sunstein (2008) and Kamenica et al. (2011) on the importance of defaults.
“expectations” by forcing firms to disclose verifiable fundamentals based on past transactions and events, such as cash flows, profits, assets and liabilities. These fundamentals provide a “reality check” for market participants who are navigating “new” market conditions due to technological or financial innovation. Reporting and disclosure regulation could be adjusted over time to capture the cumulative experience from previous episodes of exuberance or be used to cast the spotlight on certain transactions that are growing fast (e.g., securitizations). As such, it could play an important role in the detection and monitoring of systemic risk. At present, it is still unclear to what extent disclosure and reporting regulation contributes to financial stability (see also Acharya and Ryan, 2015). But if it did, this contribution would be an important market-wide benefit (or public good) in light of the harm that asset bubbles and financial crises can cause in an economy.

Third, we call for more research on the process and dynamics of regulation. Yet another and less flattering explanation for why regulation is pervasive could be that policy makers and regulators must be viewed as “acting” after a financial crisis, leading to ever increasing regulation. As our review indicates, the literature focuses primarily on the effects of disclosure and reporting regulation, rather than the political process by which it comes about. Early work by Watts (1977), Watts and Zimmerman (1978, 1986) and more recent work on the standard setting process provide important insights, but more research on the process is needed, at the market or national level as well as internationally, considering that the creation of disclosure regulation and reporting standards is increasingly coordinated among countries.

A particular aspect that has received very little attention is the extent to which political and market forces shape the implementation of regulation once it has been enacted. Very often the finer details of regulation are left to a regulatory body. It is possible that these details and the

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72 Examples for work on standard setting are Francis (1987), Dechow et al. (1996), McLeay et al. (2000), McLeay and Merkl-Davies (2004), and Ramanna (2008). In addition, the traditional literature on the economics of regulation provides significant insights into issues related to special interests and regulatory capture (see, e.g., Stigler, 1971; Posner, 1975; Peltzman, 1976).
implementation are critically important for regulatory outcomes. In addition, studies suggest that enforcement plays a critical role for regulatory outcomes. We need more research that examines the relative roles (and balance) of rules, implementation and enforcement. We will come back to the issue of enforcement and interactions of various institutional elements below.

Still related to the regulatory process, we have little evidence on how the costs and benefits of disclosure regulation evolve over time, e.g., with different stages of 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 markets. Inflexible regulation and accounting standards can stifle innovation in financial markets, which in turn can affect whether new technologies and ideas are financed. Most of the literature is static in that studies are concerned with the questions of whether and how to regulate, and not the dynamics of regulation. There are significant opportunities for researchers to study sunset provisions, post-implementation reviews, and reforms of the regulatory process. Such mechanisms could have effects on the initial adoption (e.g., firms might be reluctant to make the requisite investments to implement a new accounting standard if they are uncertain about its future). Such tradeoffs need to be carefully studied. There is also nascent evidence that the effects of new regulation depend on a country’s prior regulation (e.g., Christensen et al., 2014a), suggesting significant path dependencies.

Fourth, we call for more research on the real effects of disclosure and reporting regulation. We have much less work on how regulation affects the behavior of firms than we have work on capital-market effects, generally reflecting the behavior of investors and analysts, etc. Moreover, disclosure requirements are increasingly used in many areas as a public policy instrument to encourage or discourage certain behaviors and business practices (see also Graham, 2002). However, we have relatively little evidence whether mandated disclosure induces the desired
behavioral (or real) effects. We also have little knowledge whether disclosure regulation would be preferable to more conventional regulation that directly restricts or mandates certain behaviors or business practices. For the latter, we have significant concerns about and evidence of unintended consequences. The premise is often that disclosure regulation is more “benign” than conventional regulation. However, as discussed in Section 4.4, there is evidence of unintended and pernicious consequences of mandated disclosure, in particular, in health care applications. Accounting researchers could investigate the extent to which these insights generalize and apply to corporate settings. More generally, as societies extend disclosure and transparency regulation to many areas, understanding the behavioral consequences of such regulation is of first-order importance and a topic to which accounting researchers can contribute significantly.

Fifth, 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 first step towards a welfare analysis for disclosure 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 aggregate consumption decisions and how large is this effect? Is there a link between disclosure regulation, transparency of the corporate sector and economic growth? Of course, answering these questions poses significant identification challenges. But they are not fundamentally different from the challenges of identifying the effects of monetary policy. There is also an extensive literature on financial liberalization and economic growth (Levine, 2001).

For instance, there is the concern that firms satisfy the requirements with lengthy disclosures that are often difficult to process, boilerplate and legalistic in nature and that consumers largely ignore them.
However, in this literature, the role of transparency and disclosure regulation is still largely unexplored.

Sixth, 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 institutional interactions. In this review, we highlight complementarities among various elements of the system. Such complementarities imply that the desirability of disclosure and reporting regulation should not be studied in isolation. Explicitly recognizing the interactions and tradeoffs can yield novel and important insights. For instance, Glaeser et al. (2001) and Djankov et al. (2003) point to interactions and tradeoffs 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). More specifically, the interplay between disclosure and reporting regulation and various enforcement mechanisms deserves more attention. Our review of the IFRS literature highlights its central importance.\textsuperscript{74} To give another example that is even more specific: we call for more work that analyzes the 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. Towards this end, researchers could draw more extensively on the audit literature and bring experimental studies (in the lab or field) to bear.

Seventh, we call for more research on the effects of the global harmonization or convergence of regulation as well as the effects of regulatory competition. Despite the extensive

\textsuperscript{74} Complementarities also imply that institutional systems exhibit path dependencies, which brings us back to our third suggestion for future research. In addition, path dependencies suggest a role for historical analyses.
convergence efforts in accounting and elsewhere (e.g., bank regulation), there are still many competing regulatory regimes around the world and firms have choices when it comes to these regulatory regimes, including fairly unregulated markets (such as the U.S. OTC or grey market for equities). The cross-listing literature, which we reviewed in Section 4.4., is one area in which such choices have been explicitly studied. But this is an exception. Moreover, due to advances in information and other technology, firms can often circumvent regulation that applies within a given jurisdiction. Understanding these outside options and their potentially confounding effects is important. A related issue is the role of disclosure regulation in innovative markets. Examples that come to mind are (i) crowd-funding of firms outside traditional capital markets, (ii) peer-to-peer transactions and information sharing (e.g., Michels, 2012; Sutherland, 2015) outside traditional regulated transactions, (iii) alternate payment systems and currencies (such as bitcoin). We know little about the extent to which disclosure regulation supports (or hinders) the development of such innovative markets and innovation more generally.

There is still competition in the area of financial reporting regulation, despite the fact that IFRS have been widely adopted globally (and substantially converged with U.S. GAAP). As we note at the beginning of our survey, reporting regulation is not just the stated rules, but it also comprises the interpretation of the rules, monitoring of compliance with the rules, and enforcing and imposing penalties for deviations from the rules. Moreover, reporting regulation co-exists and interacts with other institutions. Therefore, despite the harmonization of the rules, competition has moved to other aspects of financial reporting and the broader institutional infrastructure. For instance, there can be competition with respect to (i) implementation guidance and interpretation of the standards, (ii) additional disclosure requirements, (iii) the enforcement of financial reporting standards, and (iv) other non-regulatory institutions including effective monitoring by the

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75 This behavior features prominently in the international tax literature and a good example are the attempts of U.S firms to escape world-wide versus territorial taxation.
community of intermediaries including analysts, auditors, and the media (see, e.g., Bushee et al., 2010; Miller and Skinner, 2015; Chen et al., 2015). Is such competition in regulatory and institutional regimes beneficial or a wasteful duplication of resources? There are arguments in both directions (e.g., Barth et al., 1999; Dye and Sunder, 2001; Coffee, 2002), but very little empirical evidence on these matters. Thus, the issue of competition of markets and regimes is likely to remain an important topic in the area of reporting and disclosure regulation.
Listing of Tables Included in Online Appendix

Tables Referenced in Section 3

Table 3.1: List of Studies Examining Links between Disclosure & Economic Outcomes
Table 3.2: Studies on Direct Capital Market Outcomes of Disclosure and Reporting
Table 3.3: Studies on Indirect Capital Market Outcomes of Disclosure and Reporting
Table 3.4: Studies on Real Effects of Disclosure and Reporting
Table 3.5: Studies on Disclosure and Reporting Costs

Tables Referenced in Section 4

Table 4.1: Studies on Introduction of U.S. Securities Regulation
Table 4.2: Studies on Extensions of U.S. Regulations in the OTC Markets
Table 4.3: Studies on Major Changes in U.S. Disclosure Regulation
Table 4.4: Studies on International Effects of Disclosure Regulation
Table 4.5: Studies on Real Effects of Disclosure Regulation

Tables Referenced in Section 5

Table 5.1: Studies on IFRS and Reporting & Disclosure Outcomes
Table 5.2: Studies on Capital Market Outcomes of IFRS Adoption
Table 5.3: Studies on Non-Market and Real Effects of IFRS Adoption
Table 5.4: Studies on Conditional Outcomes of IFRS Adoption
Table 5.5: Studies on Links between Institutions & International Accounting Outcomes
References


