Cost of Capital Free-Riders

Stephen P. Baginski[a]
The University of Georgia

Lisa A. Hinson[b]
The University of Georgia

November 17, 2013

ABSTRACT: We document the interrelationship of disclosure policy decisions by providing evidence that the cessation of quarterly management forecast guidance by 656 firms ("stoppers") during 2004-2009 is associated with a pursuant increase in quarterly and annual forecasts by previously non-forecasting firms in the same industries ("free-riders"). Increased forecasting by free-riders is positively associated with the information loss in the industry (proxied by the number of stoppers in the industry) and the importance of the information loss to the free-rider (proxied by analyst following and the existence of new share issues). Following the cessation event, free-rider cost of capital decreases as a function of the extent to which free-riders immediately initiate both quarterly and annual forecasting, each incremental to the other.

Keywords: *Management forecasts; disclosure policy; cost of capital; information transfer; free-rider*

JEL Classifications: *M41*

Data Availability: *Data are available from the sources indicated in the text.*

(Please do not quote without permission from authors.)

We thank Ben Ayers, Linda Bamber, John Campbell, Jim Kau, Clive Lennox, Santhosh Ramalingegowda, Casey Schwab, Bridget Stomberg, Erin Towery, Steven Utke, Ben Whipple, Julia Yu, and workshop participants at Nanyang Technological University and the University of Georgia for helpful comments.

[a] Corresponding author; J. M. Tull School of Accounting, 255 Brooks Hall, Terry College of Business, University of Georgia, Athens, GA 30602, baginski@terry.uga.edu

[b] J. M. Tull School of Accounting, 255 Brooks Hall, Terry College of Business, University of Georgia, Athens, GA 30602, lisalee@uga.edu

I. INTRODUCTION

A substantial body of empirical research addresses the causes and consequences of voluntary management forecast disclosure.[1] This research tends to focus on a firm's management forecast disclosure choice in isolation, ignoring potential informational interdependencies among firms that could cause potential interdependencies in disclosure choice. Although prior research has established that management earnings forecasts by one firm affect the *share prices of other firms* (i.e., the information transfer relation documented in Baginski 1987; Han, Wild, and Ramesh 1989; and Kim, Lacina, and Park 2008), very little work has considered whether the management forecast disclosure choices of one firm affect the *management forecast disclosure choices of other firms* and the consequences of those reactive choices.

The potential for interdependent choice of disclosure among firms is plausible given that some firms share operational commonalities. Firms in a given industry, for example, acquire the factors of production and make sales in common markets. Industry-wide supply and demand conditions cause their earnings and cash flows to co-vary either positively or negatively, depending on competitive conditions and their strategy differences. Because of the economic interdependence among firms and the fact that management forecast disclosures are associated with information transfers, firms can satisfy investors and financial analyst demand for management forecasts by issuing a forecast or, alternatively, by potentially free-riding on the forecasts of other firms. If regularly disclosing industry co-members change their policy to one of non-disclosure, free riders may have to change their disclosure policy as well to replace the lost information transfer.

Understanding the interdependencies in voluntary disclosure decisions is important for two reasons. First, at the most basic level of describing management/firm behavior, if interdependencies among firms affect the forecasting decision, then models describing a given firm's forecasting decisions are incomplete if they do not consider the forecasting decisions of other firms. Second, from a policy perspective, the existence of interdependencies in disclosure represents a potential externality in financial

[1] See King, Pownall, and Waymire (1990) and Hirst, Koonce, and Venkataraman (2008) for a review.

reporting. In contrast to information transfer interdependency which Dye (1990) argues is an example of a financial externality (i.e., altered investor perceptions of cash flows), interdependencies in *disclosure decisions* are a real externality (i.e., altered cash flows). The act of voluntary disclosure has cash flow consequences in terms of disclosure production cost, the costs of revealing proprietary information (Bamber and Cheon 1998; Verrecchia and Weber 2006), and legal costs (Skinner 1997; Francis, Philbrick, and Schipper 1994). Free-riding firms avoid these disclosure costs if another firm provides the forecast and incur these incremental costs if the other firm ceases disclosure and the free-riding firm must disclose. Further, the benefits of voluntary disclosure can differ depending on whether a firm self-discloses or free rides. For example, if disclosure precision affects cost of capital, financing cash flows might differ depending on whether firms provide more precise self-disclosure or free ride on less precise disclosures of other firms. Dye (1990) argues that real externalities may be more important for disclosure regulation than financial externalities. In his analysis, when only financial externalities are present, "mandated disclosures are superfluous, because the optimal mandated disclosures simply coincide with firms' voluntary disclosure decisions" (p. 3). In contrast, when real externalities are present, "optimal mandatory and equilibrium voluntary disclosures tend to diverge" (p. 3).[2]

Evidence on voluntary management forecast disclosure decision interdependence and its consequences, however, is scarce; although some pieces of the puzzle have been described empirically. Pownall and Waymire (1989) find that firms that do not issue forecasts (a *policy* choice) receive greater information transfers from other firms' disclosures relative to those firms that issue forecasts. This finding suggests that some firms may free ride on the disclosures of other firms. Tse and Tucker (2010) focus on performance-driven strategic disclosure *acts* and document herding in the *timing* of management forecasts. Although their focus is somewhat different than Pownall and Waymire's (1989) disclosure policy focus in that they examine the timing of the disclosure *act*, their results also suggest that firms consider the disclosure choices of other firms when making their own disclosure decisions.

[2] Foster (1980) evaluates arrangements to deal with externalities other than by regulation. Leftwich (1980) evaluates whether these types of externalities are market failures and worthy of regulation when the existence or possibility of alternative markets/arrangements that could achieve optimal social welfare have not been shown.

The lack of understanding of the interdependencies of firms' voluntary disclosure choices, the importance of real externalities in financial reporting, and the plausibility of interdependencies given the information transfer relation jointly motivate our first research question: *Does a change in the management earnings forecast disclosure policy of one firm affect the management forecast policy choices of other firms?* To address this question, we identify two groups of firms that share industry membership but that clearly follow different forecast disclosure policies – a group of firms that regularly provide quarterly management forecasts and a group of firms that do not provide management forecasts of either quarterly or annual results. We designate this latter group "potential free-riders", and we examine whether a forecast policy change by the regular forecasters, cessation of forecasting (as in Houston, Lev, and Tucker 2010 and Chen, Matsumoto, and Rajgopal 2011), is associated with the initiation of forecasting by the potential free-riders.

Capital market consequences of externalities are generally couched in terms of disclosure underproduction by the non-free-riding firm in the presence of free-riders. That is, non-free-riding firms do not factor in the benefits of their disclosures for free-riders when making the disclosure decision, and thus, a firm's decision not to disclose might be suboptimal from a social welfare perspective. This interpretation assumes that the free-rider will not change its disclosure decision if the non-free-rider chooses non-disclosure and thus will not receive any disclosure benefits. We take another step forward in documenting the consequences of disclosure decision interdependency by asking a second research question about the disclosure replacement by previously free-riding firms pursuant to the cessation event: *Is the disclosure replacement by the previously free-riding firm associated with a decrease in its equity cost of capital?* Using the concept that disclosure precision is negatively related to cost of equity capital (Lambert, Leuz, and Verrecchia 2007, 2012), we expect a *decrease* in its cost of equity capital because the free-riders' own disclosure is a more precise signal than the (now absent) information transfer from the cessation firm, resulting in a net reduction in cost of capital after the free-rider initiates forecast disclosure. Therefore, in the presence of disclosure policy dependency and reactive disclosure by the previously free-riding firm, the net benefits of self-disclosure might exceed the net benefits of free-riding.

Using First Call (and subsequently checking against Factiva), we examine the quarterly management forecast disclosure practices of firms between the first quarter of 2004 and the first quarter of 2009 to identify firms that ceased management forecast disclosure (*stoppers*). We identify firms in the stoppers' industries (*co-members*) that are potentially free-riding on other firms' disclosures (potential *free-riders*) by virtue of the fact that they do not issue either a quarterly or annual management forecast during the year prior to the forecast cessation by the stopper. To document the interdependency of forecast policy, we examine whether the potential free-riders start to forecast after the cessation event. Then, to document the consequences of the replacement disclosure, we use a method proposed by O'Hanlon and Steele (2000) and adapted by Easton (2006, 2009) to estimate ex ante cost of equity capital on an aggregate basis for the free-rider group before and after the cessation event, conditional on the extent to which the free-rider increased its management forecast disclosure pursuant to the event. We replicate our cost of capital change tests on a firm-specific basis using an approach in Kothari, Li, and Short (2009) to estimate the cost of equity capital using the Fama and French (1993) three-factor model.

With respect to our first research question, we find evidence that a subset of the non-disclosers are free-riders as evidenced by their increase in annual and quarterly management forecast disclosures pursuant to the cessation of management earnings forecast disclosure by a stopper. We show that the average quarterly and annual management forecast disclosure increase by previously non-disclosing firms is significantly greater than the increase in disclosure by all previously non-disclosing firms in non-event periods and greater than the disclosure change during the event period of a control group of industry co-members who do not fall into the stopper or non-disclosure groups. After controlling for their performance and access to equity markets, increased forecasting by free-riders is positively associated with the information loss in the industry as proxied by the number of stoppers in the industry (and somewhat mitigated by the information transfer potential of the remaining industry co-members in a broader sample of smaller firms) and the importance of the information loss to the free-rider as proxied by analyst following and share issues.

With respect to our second research question, we document that, following the cessation event, free-rider cost of capital decreases as a function of the extent to which free-riders immediately initiate both quarterly and annual forecasting (each incremental to the other).

Our findings contribute to the literatures in several ways. First and foremost, our research extends the literature on the causes and consequences of voluntary management forecast disclosure by showing that firms' disclosure *policy* decisions are interrelated. Interestingly, our finding of a substitutive relationship for forecast policy decisions (e.g., an increase in disclosure pursuant to an industry co-member's decrease in disclosure) implies that the policy decisions and the timing of individual acts of disclosure are likely driven by distinct economic causes. Tse and Tucker (2010) find a "follow the leader" or "herding" in the *timing* of management forecasts, presumably to maximize firm value.

Second, our findings add to recent research on real externalities in two ways. First, the interdependency in forecast policies that we document is a real externality in the sense that forecast disclosure decisions by one firm have real cash flow consequences for other firms. Either the free-riding firms avoid the costs of producing forecasts and enjoy the benefits of free-riding, or they initiate forecasting pursuant to forecast cessation by the industry co-member. Their change in disclosure is associated with an increase in financing cash flows from a reduced cost of equity capital and a decrease in operating cash flows due to the direct costs of information production and indirect proprietary and legal costs (if any).[3] Second, our finding that disclosure policy choices of some firms lead to cash flow-changing choices of other firms adds to an emerging body of research that establishes a link between the accounting quality and disclosure choices of some firms to the investment decisions of other firms. Durnev and Mangen (2009) link accounting restatements to reduced investment by non-restating industry co-members. Beatty, Liao, and Yu (2011) document the spillover effects of high profile frauds on peer

[3] The nature of the real financing cash flow effects depends on the extent to which the free-rider uses debt or equity financing. In this study, we examine the cost of equity capital, and thus, the cash flow effects exist if the free-rider uses new equity financing or engages in share repurchases. To the extent that voluntary earnings disclosures also affect debt pricing, the potential financing cash flow effects extend to debt financing as well.

firms' investments. Badertscher, Shroff, and White (2013) investigate the externalities of public firm disclosures in regard to the investment decisions of private firms.

Finally, we contribute to research on the disclosure quality/cost of capital relation in two ways. First, the quality of a firm's information environment is not completely characterized by the quality of its own disclosures. Because of this condition research design consequences are possible. For example, if voluntary disclosures create information transfers and if other firms free-ride on the voluntary disclosures of their industry competitors by not disclosing, then empirical models attempting to explain a given firm's cost of capital with its own voluntary disclosure quality may be misspecified if they do not control for other firms' disclosure decisions. If cost of capital and disclosure are, in fact, associated, then the misspecification is likely to create a bias in favor of the null hypothesis of no relation because measurement of non-disclosing firms' voluntary disclosure does not capture the disclosing firms' voluntary disclosure on which the non-disclosers free ride. Prior research in this area has ignored the information transfer phenomenon and the potential for free-riding behavior.[4]

Second, as noted by Core (2001), research on the relation between disclosure and cost of capital is plagued by an endogeneity problem. Most often, the source of the endogeneity is described as common factors driving both disclosure choice and cost of capital. In the setting we examine, however, we can identify a firm that discloses and then ceases to disclose (a "stopper") and other industry co-members that are non-disclosers (potential "free-riders"). The cessation of disclosure by the stopper, an endogenous choice for that firm, is a plausibly exogenous event for free-riders because it is unlikely that economic conditions that initially led to free-rider status have changed for the free-riders. We are able to examine whether an exogenous event leads to a cost of capital-reducing disclosure increase by free-riders.[5] Our

[4] The same statement could be made about research into the relation between earnings quality and cost of equity capital, a notable exception being Ma (2013).

[5] It is difficult to envision a shared economic event or common change in economic condition that would cause a set of regular disclosers to suddenly change to nondisclosure and a set of free-riders to suddenly initiate forecasting . We are also not aware of any empirical work documenting such a phenomenon. Our analysis does address potential endogeneity arising from firm and time-period-specific conditions which might be associated with the timing of the shock and with the potential free-riders' cost of capital change (i.e., performance and share issues). An alternative endogeneity-related concern is that the shock (a reduction in industry-wide information) affects the free-riders' cost of capital directly. However, if disclosure and cost of equity capital are inversely related, the shock *increases* free-

use of a "reverse experiment" (i.e., exogenous shock to cost of capital leading to disclosure change and cost of capital effects) is similar in spirit to recent approaches in Leuz and Schrand (2009) and Balakrishnan, Billings, Kelly, and Ljungqvist (2012). Leuz and Schrand (2009) use the Enron scandal, a period-specific, broad, risk-increasing, exogenous shock to detect that managers increase the length and frequency of SEC filings in response and subsequently experience declines in risk. Balakrishnan et al. (2012) use brokerage firm closures, a non-period-specific, exogenous shock to the supply of public information to show that firms respond through increased voluntary disclosure and enjoy liquidity benefits as a result. We add support to these findings by showing that, pursuant to industry-specific decreases in the supply of information, free-riders' disclosure increases, and the disclosure increases are associated with a reduction in cost of capital, as predicted by theory. As pointed out by Balakrishnan et al. (2012), this type of research informs the debate on whether guidance is desirable. While practitioners and influential institutions decry the practice of providing guidance, theoreticians provide analytical support for the value of disclosure for risk-sharing (Diamond 1985), firm value in general (Diamond and Verrecchia 1991; Easley and O'Hara 2004; Lambert, Leuz, and Verrecchia 2007, 2012), and more efficient managerial investment resulting from price efficiency (Fishman and Hagerty 1989). The combination of our findings and the findings in Leuz and Schrand (2009) and Balakrishnan et al. (2012) support a benefit of disclosure. Our findings are particularly relevant to the guidance debate because our exogenous shock is the cessation of quarterly management forecasting, which is a response to practitioner calls for a halt in the focus on short-term guidance because of its damaging long-run consequences (Houston, Lev, and Tucker 2010; Chen, Matsumoto, and Rajgopal 2011).

The remainder of our paper is organized as follows. In section II, we provide background and empirical predictions. In section III, we describe the selection of the stopper sample. In section IV, we provide the research design and results. In section V, we present additional tests, and in section VI, we conclude the paper.

---

rider cost of capital, which biases against our predicted finding that resulting increases in disclosure by the free-rider decrease free-rider cost of capital.

II. BACKGROUND AND EMPIRICAL PREDICTIONS

Our empirical predictions are motivated by two phenomena: empirical evidence of the existence of information transfers associated with earnings-related disclosures and the theoretical relation between disclosure and cost of equity capital. First, we argue that disclosure policy choices are interrelated and predict how a free-rider would respond to a disclosure policy change of an industry co-member. Then, we predict the consequences of the free-rider response.

Interrelated Disclosure Policy

A large body of literature investigates whether disclosures of actual earnings and management earnings forecasts affect the *stock prices* of other firms (an information transfer). Foster (1981) and Han and Wild (1990) document positive information transfers associated with actual earnings releases. Baginski (1987), Han, Wild, and Ramesh (1989), and Pyo and Lustgarten (1990) document positive information transfers associated with management forecasts of earnings. Kim, Lacina, and Park (2008) identify a within-industry differential information transfer. They find that both positive and negative information transfers from management forecasts exist depending on whether the forecast indicates industry commonalities or competitive shifts. Freeman and Tse (1992) identify an across-industry differential intra-industry information transfer. They find that the greatest price reactions by non-announcers exist in industries with higher within-industry earnings correlations.

The existence of information transfer suggests the potential for non-disclosing firms to free ride on other firms' voluntary disclosures such as management forecasts. That is, if firms receive information transfers, they might not incur the costs to produce their own management forecasts. Evidence in Pownall and Waymire (1989) is consistent with this idea. They find that firms that do not issue forecasts receive greater information transfers relative to those firms that do issue forecasts.

Casual observation suggests that there is substantial within-industry variation in voluntary disclosure policy, especially management forecasts, and the forces that lead to the industry disclosure equilibrium are not well understood. Recently, Jorgensen and Kirschenheiter (2012) address multi-firm threshold-type voluntary disclosure strategies (e.g., Verrecchia 1983) in a sequential disclosure model with exogenous disclosure costs. Equilibrium disclosure strategies for leader and follower firms differ depending on their private signal correlations. They show that positively correlated private signals lead to free-riding behavior by one firm, while negatively correlated private signals lead to defensive disclosure by one of the firms. Prior empirical research shows that, on average, management forecasts and earnings releases convey intra-industry *commonalities* in firms' earnings (Baginski 1987; Han et al. 1989; Foster 1981; Han and Wild 1990), suggesting that the private signals regarding earnings of pairs of firms in the same industry are expected to be positively correlated on average. Accordingly, we would expect free-riding behavior to exist.

The non-disclosing firm's disclosure policy decision in the presence of information transfer is driven by a comparison of costs and benefits. Let Benefit_{FR} equal that benefit and let Cost_{FR} represent the cost of disclosure for the free-riding firm. Let Benefit_{SD} and Cost_{SD} represent the cost of capital benefit and cost of self-disclosure, respectively. Self-disclosure occurs if:

$$\text{Benefit}_{SD} - \text{Cost}_{SD} > \text{Benefit}_{FR} - \text{Cost}_{FR} \quad (1)$$

By definition, Cost_{FR} equals zero from the non-discloser's perspective, so substituting zero for Cost_{FR} and rearranging:

$$(\text{Benefit}_{SD} - \text{Benefit}_{FR}) - \text{Cost}_{SD} > 0 \quad (2)$$

Benefit_{SD} is greater than Benefit_{FR} because a firm's own disclosures are likely to be more precise signals about its own prospects than competitors' disclosures. Firms who currently self-disclose satisfy equation (2). Firms who do not satisfy equation (2) do not self-disclose. However, not all non-disclosing firms are free-riders. Inspection reveals that equation (2) can be violated by relatively small Benefit_{SD}, relatively large Benefit_{FR}, relatively large Cost_{SD}, or some combination thereof. If equation (2) is violated because of relatively large Benefit_{FR}, then the non-disclosing firm is free-riding.

In our design, we introduce a plausibly exogenous shock to this equilibrium. We examine a situation in which Benefit_{FR} is substantially reduced or eliminated by the cessation of a policy of regular disclosure by one or more industry co-members. Faced with the reduction in benefits from free-riding (i.e., a reduction in a relatively high Benefit_{FR}), previously free-riding firms are more likely to satisfy equation (2) and increase their voluntary disclosure:

> ***H1**: Free-riding firm j will increase voluntary disclosure pursuant to firm i's voluntary disclosure cessation.*

We can attribute the free-rider's disclosure policy change to the variation in Benefit_{FR} because the benefit and cost of self-disclosure, Benefit_{SD} and Cost_{SD}, have not changed in the short run.

Self-disclosure costs (Cost_{SD}) create significant tension surrounding our conjecture that free-riders will begin to disclose. It is not clear how market participants will interpret the free-riding firms' increase in voluntary disclosure. Market participants may subscribe to arguments that management forecasting (at least, quarterly forecasting) is an example of short-termism and pool a firm that initiates forecasting activity with the set of firms without a long-term focus (e.g., Fuller and Jensen 2002, Krehmeyer and Orsagh 2006; Scherin 2010). This condition would impose a significant cost on increasing voluntary disclosure. Further, changes in disclosure policy are not easily reversible at a low cost (Leuz and Verrecchia 2000; Einhorn and Ziv 2008), suggesting that free-riders might balk at initiating disclosure.

Reactive Disclosure Response and the Cost of Equity Capital

Theoretical work links disclosure with cost of equity capital through the effect of disclosure quality on *information asymmetry* (Amihud and Mendelson 1986; Diamond and Verrecchia 1991; Easley and O'Hara 2004) and on *information risk* (Barry and Brown 1985; Handa and Linn 1993; Coles, Loewenstein, and Suay 1995). Easley and O'Hara's (2004) argument that disclosure quality affects cost of capital through information asymmetry forms the most common theoretical justification for the empirical prediction of disclosure quality's effect on cost of equity capital. However, Hughes, Liu, and Liu (2007) show that Easley and O'Hara's result is driven by under-diversification in a finite economy. Further, Lambert et al. (2007, 2012) argue that, in Easley and O'Hara's pure competition setting, the effects of reducing information asymmetry on cost of equity capital only occur when accompanied by an increase in the average level of information precision. In summary, while analytical research continues to work on specifying the exact path by which disclosure affects cost of equity capital, a theoretical basis exists for the prediction that disclosure quality is negatively associated with cost of equity capital.[6]

Empirical research generally supports the predicted negative relation when annual report disclosure levels proxy for disclosure quality (e.g., Botosan 1997; Botosan and Plumlee 2002; Botosan, Plumlee, and Xie 2004). However, investigations of timely, high profile, voluntary disclosures have been mixed. Botosan and Plumlee (2002) document a positive relation between cost of equity capital and more timely forms of disclosure. Piotroski (2002) links management earnings forecast disclosure with increased return volatility. Francis, Nanda, and Olsson (2008) find a positive relation between a single year index of management earnings forecast disclosure quality and cost of equity capital.

The aforementioned empirical studies focus on single period disclosure indices or ratings of many types of disclosure practices rather than disclosure policy of any one type of disclosure. Recently, Baginski and Rakow (2012) examine the direct link between voluntary management earnings forecasting *policy* and cost of equity capital. They define policy as a *stable* set of disclosure practices. Frequent forecasting allows the market to form beliefs about whether managers possess private information, whether the firm has a policy to disclose private information, what that policy is, whether the policy benefits investors, and whether it will continue. The disclosure policy can be changed, but there is a significant cost of doing so (Leuz and Verrecchia 2000; Healy and Palepu 2001; Einhorn and Ziv 2008). Baginski and Rakow (2012) find that the quality of a firm's management earnings forecasting policy is negatively associated with its cost of equity capital.

Baginski and Rakow (2012) and several other studies in the area use statistical methods (i.e., instrumental variables and two stage least squares estimation) to deal with the endogeneity of disclosure quality. However, statistical approaches to deal with endogeneity are difficult to implement, primarily

[6] Recent related work by Bhattacharya, Ecker, Olsson, and Schipper (2012) examines empirically the path by which earnings quality affects cost of capital.

because of a lack of underlying theory to specify the form of the endogeneity and weak instruments in first-stage model specifications (Larker and Rusticus 2010). Recently, several studies have used experimental design to mitigate the endogeneity problem. Leuz and Schrand (2009) argue that the 2001 Enron scandal is a plausibly exogenous shock to systematic risk and investigate the disclosure changes pursuant to the cost of capital change. They discover that firms increase disclosure in response to the cost of capital-increasing shock and subsequently have a reduced cost of capital. Similarly, Balakrishnan et al. (2012) use the closure of sell-side research operations as a plausibly exogenous shock that decreases the liquidity of firms that lose coverage. They document an increase in liquidity for firms that subsequently increase their management forecasting activity in response to the closures.

The reason firms initially chose a forecast policy to free-ride is unobservable. However, the most likely reason is so that they receive the cost of capital benefits from disclosing firms via information transfer. Other, non-cost of capital-related, strategic, management forecast disclosure benefits such as conveying good performance (Verrecchia 1983, Miller 2000), signaling control over the operating environment (Trueman 1986), manipulating market beliefs around insider and compensation-related transactions (e.g., Noe 1999; Aboody and Kasznik 2000; Cheng, Luo, and Yue 2013), avoiding legal liability (Skinner 1994, 1997), winning proxy contests (Baginski, Clinton, and McGuire 2013), obtaining higher prices in takeovers (Brennan 1999), discouraging potential entrants (Newman and Sansing 1993), and also non-strategic benefits such as following exchange rules (Li, Wasley, and Zimmerman 2012) are not achievable from free-riding on another firms' disclosure policy. For each of these benefits, management must make the disclosure and control the content and timing of the disclosure. Free-riding, on the other hand, relinquishes control of the act, timing, and content of disclosure. The benefit of a policy of free-riding is informational, not strategic.

If firm j ceases to free-ride on firm i, then its cost of equity capital should decrease. Firm j's own forecast is of higher precision than the forecast of an industry co-member. Therefore, a net increase in the quality of information about firm j occurs because a more precise firm-specific signal substitutes for a (now absent) less precise information transfer signal. Accordingly, we predict that free-rider disclosure increases decrease cost of equity capital for the free-rider:

> ***H2***: *Free-riding firm j's increase in voluntary disclosure pursuant to firm i's reduction in voluntary disclosure decreases firm j's cost of equity capital.*

Evidence consistent with H1and H2 implies cost of capital free-riding prior to the cessation event. If the free-riders begin to disclose but the disclosures are not associated with cost of capital decreases (i.e., we fail to reject H2), it is less likely that the free-riding benefits were cost of capital-related or more likely that the increased disclosure is strategic and not intended to reduce information asymmetry or information risk.[7]

We do not predict that cost of capital will increase for pre-cessation non-disclosers who remain non-disclosers. It may be the case that, for this set of firms, pre-cessation Benefits_{FR} do not exist or are relatively small due to a weak or non-existent information transfer relation with the stopper or post-cessation Benefits_{FR} remain relatively unchanged due to a sufficiently strong information transfer relation with remaining industry co-members who continue to disclose.

III. SAMPLE SELECTION

Similar to Houston et al. (2010), we define each quarter in our sample – Q12004 to Q12009 – as an "event quarter." The "pre-event" period consists of the four quarters immediately prior to the event quarter, and the "post-event" period consists of the event quarter and the three subsequent quarters. A *stopper* is a firm that issues management forecasts for at least three of the four quarters in the pre-event

[7] An important potential source of endogeneity in disclosure/cost of capital studies is the possibility that disclosure costs drive both disclosure and the cost of capital. These disclosure costs – legal liability, reputation, proprietary costs, the cost of forecast preparation, etc. – are primarily firm-specific. Our claim that the cessation decision by the stopper is plausibly exogenous for the free-rider is based on the notion that the change in costs or benefits for the stopper that lead to the cessation is uncorrelated with the free-rider's costs and benefits of self-disclosure. It must also be the case that the cost/benefit comparison is different for stoppers and freeriders; if not, they would behave the same in the pre-cessation equilibrium. We verify the assertion that the change in free-rider disclosure is not due to other phenomenon in empirical tests described later by ruling out the contemporaneous effects of economy-wide and industry-wide changes in disclosure (using control groups), the effects of fixed firm-specific disclosure determinants (using a firm-held-constant time series design), and period-specific firm effects (using statistical control for performance and stock issues).

period and does not issue a forecast for any of the four quarters in the post-event period.[8] The stopper's event quarter is the first quarter for which it does not issue a forecast.

Table 1 presents the initial sample and observation loss at each step. Using First Call's Company Issued Guidance (CIG) database, we obtained an initial sample of 820 stoppers. Because the CIG database is incomplete (Chuk, Matsumoto, and Miller 2013), we performed a Factiva news search to verify the changes in disclosure policy. For each potential stopper, we searched for management forecasts in Business Wire, PR Newswire (US), Reuters Significant Developments, and Associated Press Newswires during the time period starting one quarter prior to the pre-event period and going through the post-event period. First, we searched by the key word "guidance" in the headline and lead paragraph. If we did not identify an inconsistency, we proceeded to search by the key words "sees," "expects," "expectation," and "outlook." We removed a CIG-identified stopper from the sample if we found an earnings or revenue forecast for at least one of the post-event quarters. The manual verification resulted in the removal of 164 stoppers, leaving a final sample of 656 stoppers.

Co-members are firms that report net income and belong to the same four-digit SIC code (in the event quarter) as a stopper. Across all event quarters, there were initially 55,159 co-member/stopper pairs. In order to use Easton's (2006) adaptation of the O'Hanlon and Steele (2000) method (described later), we further require that each co-member has the following data (see the timeline in Figure 1 for specific measurement dates):

1. Earnings per share (*Eps*) for each quarter in the pre-event and post-event periods,
2. Price (*P*) at the beginning and end of the post-event period, and
3. Book value per share (*Bps*) at the beginning of the pre-event period and at the beginning and end of the post-event period.

These data requirements eliminate 14,692 co-member/stopper pairs. We also remove 13,648 of the pairs because the co-member is not a potential free-rider. That is, the co-member issued at least one

[8] We define management forecasts as all CIG forecasts of earnings per share, earnings before interest, taxes, depreciation, and amortization, and funds from operations. The forecasts are issued prior to the fiscal period end and therefore, do not include preannouncements.

quarterly management forecast or one annual management forecast in the pre-event period.[9] Finally, 19,358 of the remaining pairs have negative earnings per share, negative book values, book values per share less than $1, or possess values of the continuous dependent or independent variables that fall below the 1st or above the 99th percentile of their respective distributions either in the pre-event or post-event period.[10] Removing these observations yields a final sample of 7,461 potential free-rider/stopper pairs.

We use the sample of 7,461 to test both H1 and H2 for reasons described later. However, tests of H1 do not require observation elimination based on values of earnings per share and book values. Accordingly, we replicate tests of H1 with a much larger sample. In supplemental tests, we also re-test H2 using a cost of capital estimation technique that has substantially different data requirements.

For each event quarter, potential free-rider/stopper pairs are unique; however, an individual potential free-rider can appear in the sample more than one time for two reasons: 1) it appears in the sample for different event quarters and/or 2) it appears more than once in the same event quarter because the SIC code/event quarter combination has multiple stoppers. We control for the potential effects of this clustering on standard errors in subsequent analyses.

Our classification equates larger numbers of quarters for which management provides at least one earnings forecasts with a higher quality disclosure policy. Clearly, our proxy is subject to error as a means of measuring quality. For example, two firms could provide management forecasts for an equal

[9] We use the term "potential free-rider" because non-disclosure does not necessarily imply free-riding. A portion of the discarded non-free-riders that meet all other data requirements (approximately 8,000) are used in one subsequent analysis as a comparison group for free-riders. Also, we rely on First Call's coverage to define the free-riders and do not search the news wires to discover forecasts missed by First Call. We note that Chuk et al. (2013) identify biases in First Call management forecast coverage that are primarily firm-specific which is less of a concern when we examine short-run changes in disclosure. They identify period-specific performance as a potential bias, which we consider in a subsequent test. Finally, given that we expect that our hypothesized relations will hold for free-riders because they do not forecast, mis-identification of a free-rider caused by First Call coverage bias is a bias in favor of the null hypotheses that we reject. That is, we mistakenly sample a firm for which our hypothesized effects are not expected.

[10] Ex ante cost of capital models do not perform well with negative book values. The earnings number used in the O'Hanlon and Steele (2000) model is an estimate of permanent earnings. Negative earnings are not persistent (Hayn 1995). Because it is used as a deflator, a book value per share below $1 creates large values of both the main independent and dependent variables and can cause the observation pair to dominate the regression estimation. A similar concern leads us to discard the bottom and top percentiles. Because each firm has an observation before and after the event, discarding one observation leads to discarding another so that the same firms appear in the pre and post event periods and using the firm as its own control is preserved.

number of quarters, but one firm might provide relatively less ex ante precise or less ex post accurate or unbiased forecasts. We recognize this measurement error as a bias in favor of the null, but do not expect it to be significant. Stocken (2000) presents a cheap talk model that shows the importance of historical verifiability to enhance voluntary disclosure credibility in a repeated disclosure game. Hutton and Stocken (2009) empirically document that investors and analysts equate historical forecast frequency with disclosure quality. Our use of forecast frequency as a quality measure is also consistent with Francis et al. (2008) and Baginski and Rakow (2012).

Table 2 presents several characteristics of the stoppers. Although the sampling method in our study differs from Houston et al. (HLT) (2010) in terms of years, objectives, and data requirements, the samples share many similarities. From Panel A, the stoppers have a median market value of $888.99 million ($809 million in HLT), a median analyst following of 7 (a somewhat lower 5.6 in HLT), and evidence of negative performance provided by negative median percentage price changes and percentage earnings changes (using slightly different performance measures, HLT show the same performance downturn).[11] Panel B reports the top ten industries for stopper activity. SIC 73 (business services) has the largest representation (as is the case in HLT), and eight of the top ten industries in our sample are also in the top ten in HLT's sample. Panel C shows that stoppers are diversified across years. Panel D provides information on the *annual* forecasting behavior of the stoppers. 78.1% of the stoppers maintain a stable annual forecasting pattern, comprised of 57.0% of the stoppers that issue an annual forecast for both the pre-event year and the event year, and 21.1% that do not issue an annual forecast for either year. Of the remaining set of stoppers that changed annual forecasting behavior (21.9%), the large majority (16.3%) stopped providing an annual forecast.[12]

Table 3 presents the potential free-rider characteristics. Panel A reports firm size and analyst following for the main sample used to test H1 and H2 and a supplemental sample to test H1 that also

[11] Note that our stopper descriptive statistics are based on roughly 60% of the sample that has available data. We have very few data requirements for the full sample of 656 stoppers because our hypotheses are about the free-riders in their industries. The stoppers are used to specify the existence and timing of the industry shock.

[12] The fact that very few quarterly stoppers initiate annual forecasting is out-of-sample confirmation of a similar finding in Houston et al. (2010).

includes the firms with negative book values and earnings. Relative to the stoppers (Table 2), the main sample free-riders are smaller (median market value $539.7 versus $888.9 for the stoppers) and have less analyst following (median 1 versus 7 for the stoppers). Further, only 58.2% (4,346 / 7,461) of the free-riders are followed by analysts. The larger sample for the supplemental test of H1 adds very small firms with little to no analyst following. Panel B reports distributional data for the 14,922 observations (one pre- and one post-event) of the primary dependent variable *ROE* and independent variable *Goodwill*. We describe these variables used to test H2 in greater detail in a subsequent section. They cannot be calculated for the supplemental H1 sample due to data requirements. Their means, medians, and standard deviations are very close to those reported in Easton and Sommers (2007), suggesting that the values are representative of the population of values used to estimate the cross-sectional adaptation of O'Hanlon and Steele's (2000) model.

IV. EMPIRICAL DESIGN AND RESULTS

Do Non-Forecasters Begin to Forecast Pursuant to Stopper Forecast Cessation? (H1)

We begin by examining H1's prediction that previously non-forecasting firms begin to forecast pursuant to the stopper's forecast cessation. Table 4 presents our analysis for the main sample of 7,461 and the extended sample of 26,819. In Panel A, we document that, of the 7,461 potential free-riders in the main sample, 94.8% did *not* begin to issue quarterly forecasts in the post-event period (measured as four quarters long, beginning with the quarter immediately following the event quarter; see Figure 1). We delay the start of the "immediate" reaction period by one quarter because of our definition of cessation (i.e., three or four quarters of forecasting followed by no forecasting). Free-riders might not be able to detect cessation until the second quarter because observing no forecast in the first quarter does not rule out forecasts in each of the remaining three quarters. The 5.2% who did respond forecasted at least once per quarter for as many as four post-event quarters. Slightly more free-riders (6.6%) began to issue annual forecasts (measured as the year that includes the event quarter; see Figure 1).[13]

The extended sample results are similar. Of the 26,819 potential free-riders, 95.8% did *not* begin to issue quarterly forecasts in the post-event period. The 4.2% who did respond forecasted at least once per quarter for as many as four post-event quarters. Slightly more free-riders (5.0%) began to issue annual forecasts.

These disclosure increase percentages do not appear to be large. However, because it is difficult to specify *ex ante* the expected reduction in free-riding benefits after an industry co-member's forecast cessation event, we have sampled liberally to include some cessation events and potential free-rider information environment conditions that are far less likely to capture a sufficiently large expected reduction in free-riding benefits. In Panel B, we partition our sample to re-measure the percentage of free-riders for various levels of the expected free-riding benefit reduction. Free-rider benefit reduction is primarily driven by the magnitude of industry information loss implied by the cessation event and the demand for that information. In our sample period, the number of stoppers in an industry and event quarter ranges from one to as many as eight. As the number of stoppers increases, so does the magnitude of information loss in the industry. Further, as the number of stoppers increases, the precision of the information loss is greater. Single stoppers send a disclosure message that contains firm-specific and industry-wide components. Collective consideration of the disclosure message of several stoppers diversifies away the firm-specific component and maximizes the precision of the industry-wide component and thus the information transfer signal. Therefore, as the number of stoppers increases, the reduction in the benefit of free-riding increases. Also, many of our sample firms are not followed by analysts who demand information about the firm, either from the firm or from other sources. If the other

[13] We begin the measurement period for the annual forecast count to include the event quarter. Houston et al. (2010) document a clustering of stoppers in the first fiscal quarter which would include the annual earnings release. Anilowski, Feng, and Skinner (2007) and Rogers and Van Buskirk (2013) detect an increasing tendency to bundle forecasts with earnings releases. We show later that the annual forecast increase persists, so it is unlikely that we have introduced bias into our results by starting with the event quarter to avoid missing the first bundled annual forecast.

source is removed by forecast cessation of an industry co-member, analysts will demand firm self-disclosure.

The results in Table 4, Panel B indicate much higher percentages of free-riders when the number of stoppers in an industry event quarter is large and the free-rider is covered by analysts. We report percentages of pre-cessation non-disclosers who begin to issue either quarterly or annual forecasts. So, for example, 5.2% (6.6%) of firms in the main sample in Panel A began to issue quarterly (annual) forecasts. If we measure whether a given firm issues either a quarterly or annual forecast, 9.9% did so for the main sample (shown as the "Full sample" percentage at the bottom of the first column in Panel B). The percentage is 7.9% for the extended sample.

These percentages increase substantially as the magnitude of the expected loss of free-riding benefits increases. First, note in the "Full sample" row that far more free-riders covered by analysts began to disclose (13.7% and 16.1% for the main and extended samples, respectively). Second, note in the "All Observations" columns that percentages increase substantially as the number of stoppers in the industry increases, the largest percentages reported for industries with seven stoppers (26.4% and 17.4% for the main and extended samples, respectively). Third, when the number of stoppers increases and the firms are followed by analysts, the percentages are all larger and increase to a maximum of 31.9% and 39.5% for the main and extended samples, respectively. In summary, a substantial amount of free-riding can be detected by limiting the sampling to conditions in which one would expect the largest loss of free-riding benefits.

Given that we view the initiation of forecasting as a policy change, another important issue is whether the increases in disclosure are "one-off" or indicative of a change in forecasting policy expected to lead to the decrease in cost of capital predicted in H2. Table 4, Panel C examines the persistence of forecasting once the free-rider begins to forecast. The main and extended samples yield similar results, so we discuss only the main sample in the first two columns. For firms issuing only one quarterly forecast in the post-event period, 41.9% continue to forecast in the next post-event period (post-event period +1). The remainder of these observations could be considered "one-off". However, 79.3% of firms that issue two quarterly forecasts in the post-event period continue to forecast in the next post-event period. 72.4% of firms that issue two quarterly forecasts in the post-event period continue to provide two or more forecasts in the next post-event period (i.e., 72.4% of all firms, not 72.4% of the 79.3%). The percentages continue to increase to the point where 100.0% of firms who issue four quarterly forecasts in the post-event period continue to forecast in the next post-event period, 81.8% of them providing four forecasts in the next post-event period. Firms who initiate annual forecasts continue at a 64.8% rate.

The persistence patterns suggest that the previously non-disclosing free-riders are establishing a policy of regular disclosure, more so for the firms who have issued either two or more quarterly forecasts or started to provide annual forecasts. Accordingly, we expect the cost of capital effects to be stronger for the firms with larger numbers of forecasts in the post-event period.

Is the Free-Rider Disclosure Increase Driven by Other (Macro) Uncontrolled Events?

One potential explanation for the free-riders' disclosure increase is the existence of a general increase in voluntary disclosure for all firms in the industry due to some shock other than the forecast cessation event or simply a general upward movement of forecast frequencies over time. We rule out this possibility in Table 5. Panel A presents three groups of firms for both the main and extended samples – stoppers, potential free-riders, and pre-cessation non-free-riders, defined as a firm that issued *either* a quarterly or annual forecast in the pre-event period (8,296 and 13,648 firms for the main and extended samples, respectively). We present the mean number of voluntarily disclosed quarterly (*VDQ*) and annual (*VDA*) management forecasts for each of these groups in the pre-event, post-event, and a post-event subsequent period (*pre*, *post*, and *post+1*, respectively in Figure 1). Although we expect the relatively large control group of pre-cessation non-freeriding industry co-members to respond similarly to industry and economy-wide shocks to their incentives to provide management forecasts, we do not expect that they will respond to stopper firms' forecast cessation by increasing their management forecast frequencies. These firms are not free-riding on other industry co-members' disclosures to any great extent given that they are providing their own forecasts already. That is, they have a higher threshold of information loss that would prompt them to increase disclosure.

We again discuss the results for the main sample given that the two samples have similar disclosure patterns. Stopper firms have a mean 3.547 disclosures in the pre-event period, 0.038 in the four quarters subsequent to the event quarter, and 0.172 in the four quarters beyond that.[14] In contrast, the free-riders increase their mean quarterly disclosures from 0 to 0.097, and their disclosure persists into the following four quarters. In fact, it slightly increases to a mean of 0.148. In the control sample of industry-matched non-free-riders, mean disclosures monotonically *decrease* across the three periods. The patterns of monotonic decrease in disclosure also occur for annual forecasts for stoppers and non-free-riders. In contrast, free-riders monotonically increase their mean annual forecasting. Thus, it does not appear that the free-rider disclosure increase is driven by another shock or a general trend of forecast increases over time.

To provide evidence that free-riders' increases in quarterly and annual forecasts are statistically significantly greater in periods pursuant to the industry information shock compared to periods with no shock, we created empirical distributions of potential free-riders' disclosures for all quarterly periods during our sample period, eliminating the potential free-riders in the industries and time periods impacted by stopper cessation. For each period, we calculated the non-disclosing potential free-riders' changes in quarterly forecast frequencies in the post-cessation and the following period (VDQ_{post} and VDQ_{post+1}) using the same procedures as we use for our cessation sample. The grand means of VDQ_{post} and VDQ_{post+1} in the non-cessation sample are 0.061 and 0.083, respectively, and we reject the null hypothesis that the treatment (cessation) sample means of 0.097 and 0.148 are equal to the grand means computed

[14] By definition, the mean quarterly forecast disclosure in the post-event period should be zero for stoppers. However, because the free-rider and non-free-rider quarterly voluntary disclosure measurement periods are four quarters long beginning with the quarter *after* the event quarter, we realigned the stoppers measurement period to match. Thus, a small number of stoppers might have issued a management forecast in the quarter immediately following the four quarter period for which they stopped forecasting.

for the non-cessation sample (t = 6.44 and 8.58, respectively; results not tabulated).[15,16] Analyzing empirical distributions of forecast increases in benchmark periods provides evidence that forecast increases are significantly greater in periods impacted by industry information loss.

Does Information Loss Drive Free-Rider Frequency Increases?

The results in Table 5, Panel A suggest that some free-riders increased their disclosure while their industry co-members decreased forecast disclosure. We have ruled out other economy and industry-wide shocks in that we show a consistent industry-wide disclosure *decrease* through time, and our analysis controls for firm-specific determinants of disclosure in that it uses the firm as its own control over a relatively short window. However, our analysis does not control for events and conditions that are both firm-specific *and* period-specific and that have well-known ties to voluntary disclosure, the two primary candidates being firm performance and new share issues.

The primary firm and period-specific driver of disclosure is performance, although it is not clear whether it is good or bad performance that drives disclosure. Miller (2002) documents a positive relation between performance and disclosure using earnings releases. The finding in Rogers and Van Buskirk (2013) that bundled management forecasts are good news also implies a positive relation. In contrast, findings in other studies suggest that legal liability, reputation issues, and fear of the capital market consequences of failing to meet or beat analysts' forecasts incent firms to issue (especially quarterly) forecasts when performance is not as good as expected (e.g., Skinner 1994; Matsumoto 2002; Cotter,

[15] This statistical test assumes normality. Given that *VDQ* is a count variable, we also constructed an empirical distribution of 21 quarterly means of *VDQ*, and we computed the standard deviation across the means. This approach relies on the more reasonable assumption that the distribution of sample means is normal. The t-statistics for this test are 8.50 and 10.67. In fact, the treatment sample VDQ_{post} of 0.097 is almost as large as the maximum value of 0.109 from the non-cessation empirical distribution, and the treatment sample VDQ_{post+1} of 0.148 is larger than the maximum value of 0.136 from the non-cessation empirical distribution. The results for the extended sample are similar.

[16] We analyze annual forecasts using the same empirical distribution approach described for quarterly forecasts. We computed a grand mean in non-cessation periods of 0.034 and 0.045 for VDA_{post} and VDA_{post+1}, respectively. Given that *VDA* is a zero/one count variable, these grand means represent the probability of an annual forecast in each post period. Using a binomial test, we reject the null that the main sample cessation means (observed probabilities during the treatment periods) of 0.065 and 0.093 for VDA_{post} and VDA_{post+1}, respectively, are equal to the expected probabilities derived from the empirical distribution (Z = 15.03 and 20.06, respectively). The results for the extended sample are similar.

Tuna, and Wysocki 2006). In addition, many of our sample non-disclosers are small, and thus, may periodically issue shares to raise equity capital. The information asymmetry surrounding capital market transactions is likely to create a demand for more disclosure. Accordingly, we perform (related) additional tests in this section a) to demonstrate a stronger link between stopper cessation and free-rider disclosure increases, and b) to test the link formally with controls for current period performance and share increases.

In our first test, we replicate the results in Table 4, Panel B on the effect of number of stoppers on free-rider disclosure behavior. Recall that the goal of Table 4, Panel B was to show that more than a minor number of free-riders exist if one isolates the largest expected free-rider benefit reductions. In Table 5, Panel B (and in contrast to Table 4, Panel B), we focus on quarterly and annual disclosure behavior separately, compute the mean increases in the number of forecasts issued rather than simply designate whether a forecast of some kind was issued, and show comparative behavior of a non-free-riding industry matched control group. Our goal is to perform a much stronger test than that performed in Table 5, Panel A, in which we specify an event (i.e., industry co-member stops disclosure) and then calculate whether free-rider disclosure increases. In Table 5, Panel B, we link a characteristic of the event period, the *magnitude* of information loss, to the *magnitude* of free-rider disclosure change.[17] To proxy the amount of information loss in the industry, we again transact on the fact that while many event periods have only one stopper, some event periods have as many as eight stoppers in the industry, a significant loss of information in the industry. Table 5, Panel B shows that, in the main sample, as the *magnitude* of industry stoppers increases from one to eight, free-riders increase the *magnitude* of quarterly and annual forecasts to a greater extent. Non-free-riders also have markedly different disclosure behaviors as the number of stoppers increase. When the number of stoppers is less than the median (categories one through four), non-free-riders, on average, *decrease* quarterly forecasts. Above the median, non-free-riders *increase* their quarterly forecasts. Non-free-rider behavior with respect to annual

[17] In essence, this stronger test imposes a considerable amount of structure on any potential correlated, omitted variable. Not only must the variable cause a *decrease* in stopper disclosure and an *increase* in free-rider disclosure, the magnitude of the omitted variable must correlate with the magnitude of information loss.

forecasting is similar except that the change from decrease to increase occurs at the upper quartile of the number of stoppers distribution (categories seven and eight). Thus, it appears that the *magnitude* of both free-rider and non-free-rider disclosure change is associated with the *magnitude* of information loss in the industry.[18]

Our second test provides formal statistical tests of the extent to which free-rider disclosure is linked to the expected decrease in free-rider benefits pursuant to the cessation shock while statistically controlling for performance and share issues. Table 6 presents results from estimating two forms of the following cross-sectional model estimated using ordered logistic regression:

$$VDQ\ or\ VDA = \beta_0 + \beta_1\ Performance + \beta_2\ Issue + \beta_3\ NumStoppers + \beta_4\ Log(\#Analysts + 1) + \beta_5\ AnalystDecrease + \beta_6\ AbsCorrFR\_NFR + \varepsilon \qquad (3)$$

VDQ and *VDA* measure the post-cessation quarterly and annual forecast disclosure magnitudes, respectively, for pre-cessation non-disclosers. *Performance* is a 1/0 indicator variable for whether current period return on equity (*ROE*) increased or decreased during the post-cessation relative to the pre-cessation period.[19] For reasons discussed earlier, we do not predict a sign for the relation between performance and disclosure. *Issue* is likewise a 1/0 indicator variable for whether shares increased in the post-cessation relative to the pre-cessation period. We expect a positive relation between share issues and disclosure. We capture the amount of industry information loss by setting *NumStoppers* equal to the number of stoppers in the industry during the event quarter. We predict a positive association between *NumStoppers* and both the mean number of quarterly (*VDQ*) and annual (*VDA*) management forecasts issued by the free-rider pursuant to the shock because greater industry information loss should lead to greater free-rider disclosure increases. Analysts tend to follow several firms in the same industry and rely

[18] The non-free-rider tendency to increase disclosure only when the information loss is severe is also consistent with the idea that their current disclosure level creates a higher threshold for any response to co-member forecasting cessation. The patterns of free-rider disclosure in the extended sample are similar. The non-free-rider behaviors are not as clear cut. For seven stoppers, the non-free-riders continue to reduce quarterly forecasts and for six and eight stoppers, they continue to reduce annual forecasts.

[19] The main sample free-rider mean *ROE* decreases from 14.7% in the pre-event period to 13.7% in the post-event period (t = 5.27; results not tabulated).

somewhat on information transfer relationships to predict earnings (King, Pownall, and Waymire 1990). Therefore, we include (the log of) analyst following (*#Analysts* + 1) as a proxy for the importance of the information loss to the free-rider. If the free-rider is followed by analysts who rely on the information transfer relation and stopper forecasting ceases, then the free-rider has greater incentive to replace the stopper's disclosure with its own. Accordingly, we predict a positive relation between analyst following and both *VDQ* and *VDA.*

It is important to remember that *VDQ* and *VDA* are *changes* in management forecasting behavior because the pre-event period values of both are zero by definition. Firm-specific disclosure drivers such as analyst following, firm size, industry competitive structure, legal environment, and so on have very little variance over a few quarters and thus do not explain short-window forecasting *change.* We are not using analyst following as a proxy for unconditional demand for forecast disclosure. We use analyst following as a proxy for the importance of the information transfer signal that, once removed, must be replaced by the free-rider. That being said, it is possible that free-rider analyst following may change in the short run as a result of the cessation event. Houston et al. (2011) document a decrease in analyst following for stoppers, which, from the free-rider's point of view, represents an additional derivative source of industry information loss. Given Roulstone's (2003) finding of a positive relation between analyst following and liquidity, the consequences for the free-rider's cost of capital of the cause (co-member cessation) and the consequence (reduction in analyst following) are consistent. However, Anantharaman and Zhang (2011) document management forecast guidance increases subsequent to analyst forecast coverage decreases. If free-rider analyst forecast coverage also decreased pursuant to cessation, it might result in additional increases in management guidance. Accordingly, we include a 1/0 indicator variable, *AnalystDecrease*, and expect it to be positively related to management forecast increases given the findings in Anantharaman and Zhang (2011).

We estimate equation (3) excluding *AbsCorrFR_NFR* so that we can use the full sample and then re-estimate it on a slightly smaller sample with the variable included. The variable captures the average absolute correlation between the potential free-riding firm and the remaining non-free riding industry co-members that continue to forecast. The motivation for including this variable is the possibility that free-riders obtain free-riding benefits from another set of industry co-members that have disclosed in the past and continue to disclose after the cessation event. *AbsCorrFR_NFR* equals the average absolute Pearson correlation between the earnings changes of the free-rider and non-free-riding industry co-members over the 16 quarters preceding the event quarter (15 change observations). We predict a negative association between *AbsCorrFR_NFR* and both *VDQ* and *VDA.*

Table 6 presents the results of estimating equation (3) for the main and extended samples. Beginning with the main sample reported in the top half of the table, the first two results columns show that the number of stoppers in a given industry/event quarter is positively related to free-rider increases in both quarterly and annual management forecasting ($\beta_3 > 0$; $p < 0.01$). This result links the magnitude of the free-rider change in disclosure to the magnitude of the potential information loss in the industry caused by stopper forecast cessation. Also, analyst following is positively associated with the increase in quarterly and annual disclosure ($\beta_4 > 0$; $p < 0.01$), suggesting that the demand for the now non-existing industry information analysts use to assess free-rider prospects motivates free-riders to begin to disclose in response to the stopper's cessation. These findings suggest that the observed increase in free-rider disclosure is associated with both the cessation of forecasting by industry co-members and a predetermined condition, analyst following, which proxies for the importance of the information loss to the free-rider. As shown in the bottom of the table, the dominant effects on disclosure of number of stoppers and analyst following persist and are stronger in the extended sample that includes smaller, less often followed, and poor performing firms (i.e., negative earnings and book value firms that are added back to this sample).

The main variables to capture the decline in the net benefit of free-riding, number of stoppers and financial analyst coverage, are highly significant after controlling for performance and share issues. In the main sample, coefficient β_1 on *Performance* is insignificant when quarterly forecast change is the dependent variable and significantly positive when annual forecast change is the dependent variable. In the extended sample, *Performance* is significantly positively associated with both quarterly and annual forecasting. We examined this variable further using alternative definitions of *Performance* (results described below but not tabulated). In Table 6, *Performance* captures whether ROE was higher in the post- relative to the pre-cessation period. It ignores magnitude. In the main sample, if we redefine *Performance* as post-cessation ROE minus pre-cessation ROE and thus consider magnitude, coefficient β_1 becomes significantly *negative* ($p < 0.01$) for quarterly forecasts and remains significantly positive for annual forecasts but at a much lower significance level ($p < 0.10$). In the extended sample, coefficient β_1 becomes insignificant in the quarterly sample and the annual sample. Further, firms with increases in ROE may still be relatively poor performers in the cross-section. If we redefine *Performance* as post-cessation ROE, the main sample β_1 is significantly *negative* ($p < 0.01$) for quarterly forecasts and insignificant for annual forecasts. In the extended sample, coefficient β_1 is again insignificant for both quarterly and annual forecasts. Based on the whole of this evidence, we conclude that performance is not related to free-rider quarterly forecast increases or negatively related. Some evidence exists that free-rider increases in annual forecasts are positively associated with performance in our main sample.

Share issues also explain the increase in both quarterly and annual forecasts. Coefficient β_2 is significantly positive in every regression. We also tested the robustness of this finding to consider "significant" share increases, which we define at greater than 5% of outstanding shares. The results are unaffected.[20]

The last two columns repeat the estimation for slightly smaller samples of 6,705 (main sample) and 23,073 (extended sample) firms adding *AbsCorrFR_NFR* to the regression. The results discussed above remain unaffected except that, consistent with the findings in Anantharaman and Zhang (2011), quarterly management forecast guidance increases subsequent to analyst forecast coverage decreases ($\beta_5 > 0$; $p < 0.10$ in the main sample). However, the result does not hold for annual forecasts or for quarterly and annual forecasts in the extended sample. In the extended sample, β_6 is significantly *negative* ($p <$

[20] The tests in this section focus on H1, specifically whether the reduction in net free-riding benefits pursuant to the cessation event drives the free-rider disclosure responses and the elimination of alternative interpretations. We use the main sample in tests of H2 and revisit the implications of the performance and share issue associations for testing H2.

0.01) for quarterly forecasts, suggesting that the cessation-related reduction in net benefits is not as severe for the much larger sample which includes very small, non-covered, poor performing firms when these firms have a higher potential information transfer relationship with remaining disclosing industry co-members.

In summary, our evidence supports the idea that free-riders began to disclose pursuant to the cessation event. Increased disclosure is not present among non-free-riders on average. In the cross-section, free-rider disclosure increases are associated with characteristics of the event (number of stoppers) as well as demand for information that is lost (number of financial analysts).

Does Free-Rider Cost of Capital Change Pursuant to Cessation and Disclosure Change? (H2)

To test H2, we must measure cost of equity capital.[21] The primary proxies for cost of equity capital are *ex post* realized returns, risk factor proxies from specific asset pricing models (e.g., betas from the CAPM or Fama-French models), and *ex ante* estimates derived from some form of the residual income model developed in Preinrech (1938), Edwards and Bell (1961), Ohlson (1989, 1990, 1995) and Feltham and Ohlson (1995, 1996). Beginning with Botosan (1995), the *ex ante* proxy has been used in disclosure research due to its documented usefulness in valuation (Bernard 1995) and the controversy surrounding the more conventional estimates of cost of equity capital derived from *ex post* realized returns, which tend to be volatile and which require a long time series to obtain stable estimates.

The *ex ante* proxy has its own set of problems.[22] Easton (2009) argues that *ex ante* firm-specific cost of capital estimates are likely to contain significant measurement error due to low quality financial analyst forecasts. While measurement error tends to bias regression coefficients toward the null of no association, it can also raise the possibility that an observed relation is spurious. For example, evidence in

[21] Our interest is in cost of equity capital, not information asymmetry, which theoretically may or may not be related to cost of equity capital depending on the level of information precision (Lambert et al. 2012). Accordingly, we do not employ bid-ask spreads in our analysis.

[22] The empirical models based on the residual income valuation concept that are used to estimate *ex ante* cost of equity capital exist in many different forms. The differences in model form are from different assumptions about terminal value, growth rates, and the behavior of residual income over time. See Botosan and Plumlee (2005), Easton and Monahan (2005), and Botosan, Plumlee, and Wen (2011) for explanations of these alternative approaches.

Wang (2012) suggests that measurement error in the *ex ante* firm-specific cost of equity capital estimates is associated with firm-specific risk or growth characteristics. Further (and potentially due to this measurement error), Botosan and Plumlee (2005) detect that only two of the many alternative models to estimate *ex ante* cost of equity capital yield estimates that are correlated with risk factor proxies such as beta, size, book-to-market, and leverage, Easton and Monahan (2005) find that not one of the alternative models yield *ex ante* cost of capital estimates that are associated with *ex post* realized returns, and Botosan, Plumlee, and Wen (2011) find that only two of 12 *ex ante* model estimates are related to realized returns. Moreover, Larocque (2013) finds that correcting predictable analyst forecast errors in the most commonly used models does not appear to improve the associations with realized returns, although the inflation in cost of capital estimates generally associated with analyst forecast errors documented by Easton and Sommers (2007) is reduced.

Faced with these and other issues (described later), we use Easton's (2006) adaptation of a method introduced by O'Hanlon and Steele (2000) to obtain our *ex ante* cost of equity capital estimates.[23] O'Hanlon and Steele rely on Ohlson's (1989) linear information dynamics framework which expresses asset prices as a function of a vector of current information variables – current earnings, book value, dividends, and all other information at the time – to derive the following estimable regression:

$$\frac{X_i}{BPS_{i,t-1}} = \lambda_0 + \lambda_1 \frac{P_{i,t} - BPS_{i,t}}{BPS_{i,t-1}} + \varepsilon_{i,t} \qquad (4)$$

where X is actual earnings per share, BPS is book value per share at the beginning of the estimation period, and P is price. In this formulation, λ_0, the model intercept, is the cost of equity capital estimate.[24]

[23] In supplemental tests described later, we replicate our test of H2 with an alternative cost of equity capital measures based on realized returns.

[24] We refer the reader to O'Hanlon and Steele (2000) for details on how Ohlson (1995) imposes constraints on the linear mapping of the vector of information variables into price so that the model is consistent with the present value relationship, the clean surplus relationship, and dividend policy irrelevance. Ohlson (1995) obtains a formula expressing price to equal book value, earnings discounted by the equity cost of capital, and the effects of other information. O'Hanlon and Steele algebraically manipulate the model and impose additional restrictions on the effects of other information on prices to arrive at equation (4). The O'Hanlon and Steele model has been used to estimate cost of equity capital by Easton (2006), Easton and Sommers (2007), and Baginski and Rakow (2012).

This model is similar to a model used in Easton, Taylor, Shroff, and Sougiannis (2002) to simultaneously estimate cost of equity capital and growth in residual earnings implied by current stock price, current book value of equity, and aggregate forecasted earnings.[25]

Easton (2006) adapts the method in O'Hanlon and Steele (2000) to provide an alternative way to examine cost of capital differences across regimes. Easton (2009) notes that hypotheses such as ours do not require firm-specific estimation of implied cost of equity capital. His approach uses a dummy variable to partition the sample into portfolios of firms on the variable of interest. In our case, we can use a dummy variable, D, that is equal to zero (one) for free-riders pre (post) cessation event, and estimate the following model with 14,922 observations, half pre-cessation and half post-cessation:

$$\frac{X_i}{BPS_{i,t-1}} = \lambda_0 + \lambda_1 \frac{P_{i,t} - BPS_{i,t}}{BPS_{i,t-1}} + \lambda_2 D + \lambda_3 D \frac{P_{i,t} - BPS_{i,t}}{BPS_{i,t-1}} + \varepsilon_{i,t} \tag{5}$$

Similar to the Easton et al. (2002) approach, the O'Hanlon and Steele (2000) approach also obtains simultaneous estimates of cost of equity capital and growth; however, it relies on actual rather than forecasted earnings per share to obtain growth and cost of capital estimates, and thus, is independent of analyst forecast quality. This is important for our study because many of our free-riders are not covered by analysts and those that are covered have low following. It is likely that low coverage is associated with low analyst quality. In fact, Bradshaw, Drake, Myers, and Myers (2012) find that random walk earnings forecasts are more accurate than analyst forecasts for longer horizons and for smaller or younger firms and that long-term earnings estimates obtained from naïve extrapolation of analysts' forecasts are more accurate than the analysts' long-term forecasts.

While the O'Hanlon and Steele (2000) method does not suffer from potential analyst forecast bias, it does require a choice of "actual" earnings to include in the model (presumably some estimate of permanent actual earnings). We use earnings before extraordinary items, which proxies for permanent

[25] The primary difference is that the O'Hanlon and Steel (2000) model uses current earnings and thus growth is estimated on current earnings rather than one-period ahead earnings as in Easton et al. (2002).

earnings with error. Note however that equations (4) and (5) are "reverse" regressions of earnings on prices in which measurement error in the dependent variable earnings is less of an issue to coefficient estimation. Also, firm-specific systematic error, if any, is removed by our comparison of pre- and post-cessation cost of equity capital, and diversified by estimation at the portfolio level (i.e., one pre-cessation and one post-cessation estimate for a portfolio of all free-riders in the sample). Generally, the estimates of cost of capital from the O'Hanlon and Steele (2000) model are lower than estimates from the Easton et al. (2002) model (as found in both Easton and Sommers 2007 and Baginski and Rakow 2012).[26]

In equation (5), λ_0, the model intercept, is the cost of equity capital estimate. An estimate of the change in cost of capital for the firm pursuant to the industry shock is given by coefficient λ_2 on D, the dummy variable equal to zero (one) for the firm before (after) cessation. Given that the dependent variable is a return on equity (*ROE*) and the primary independent variable is the difference between fair and book value per share scaled by beginning book value (a "goodwill" measure, *Goodwill*), we use the terms for simplicity and drop the firm and time subscripts:

$$ROE = \lambda_0 + \lambda_1\, Goodwill + \lambda_2\, D + \lambda_3\, D{*}Goodwill + \varepsilon \qquad (5^*)$$

Figure 1 shows exactly when *ROE* and *Goodwill* are measured in the pre and post-event periods.

Although we tabulate an estimate of equation (5*), it omits hypothesized determinants of the change in the cost of capital. Accordingly, for our primary tests, we estimate the following model:

$$\begin{aligned} &ROE = \\ &\textit{Baseline:} && \lambda_0 + \lambda_1\, Goodwill + \lambda_2\, D + \lambda_3\, D{*}Goodwill \\ &\textit{Quarterly forecasting change:} && + \lambda_4\, ChVDQ{*}D + \lambda_5\, ChVDQ{*}D{*}Goodwill \\ &\textit{Annual forecasting change:} && + \lambda_6\, ChVDA{*}D + \lambda_7\, ChVDA{*}D{*}Goodwill + \varepsilon \qquad (6) \end{aligned}$$

[26] Lower cost of capital estimates are not a problem if the estimates are systematically lower and we compute differences in cost of capital, firm held constant, as we do. Also, lower cost of capital estimates are consistent with the cost of capital estimates obtained by correcting predictable analyst forecast errors (Larocque 2013).

H2 predicts $\lambda_4 < 0$ and $\lambda_6 < 0$. That is, the increase in voluntary disclosure by the free-riding firm pursuant to the industry shock reduces its cost of equity capital. Our focus on λ_4 and λ_6 is a conservative test in that we require the cost of capital decrease to be associated with the *magnitude* of free-rider disclosure increases. In equation (6), when we control for changes in free-rider disclosure, λ_2 represents cost of capital shifts not associated with changes in free-rider disclosure (e.g., year effects or possibly changes in other disclosures by industry co-members which we consider in supplemental tests described later).[27]

Table 7 presents our results of estimating the base model (equation 4) and equations (5*) and (6). The base model establishes an overall estimate of average cost of equity capital for 7,461 free-riding firms across two points in time (the beginning of the event quarter and the end of the post-event period; 7,461 firms x 2 = 14,922 observations), given by coefficient λ_0 which equals 8.55%.[28] The univariate effect of cessation on free-rider cost of capital is given by coefficient λ_2 (-0.67%) in the second reported regression column. Thus, overall, free-rider cost of capital decreased pursuant to the cessation event.[29] However, the estimated univariate effect (equation 5*) is confounded by the fact that the free-riders increased disclosure during the period, which will have a cost of capital reducing effect on the free-riders. As shown in the third results column, cost of equity capital is decreasing in increases in the free-riders' voluntary quarterly forecasts ($\lambda_4 < 0$, t = -4.29). As shown in the fourth results column (the complete estimate of equation 4), cost of equity capital decreases as free-riders' voluntary annual forecasts increase as well ($\lambda_6 < 0$, t = -3.16). Thus, we reject the H2 null hypothesis, and we conclude that voluntary

[27] The market-wide expected return varies inter-temporally due to economy-wide factors that affect risk-free rates and risk premia. For example, risk-free rates varied considerably during our sample period. Treasuries with one year maturities had yields that ranged from a high of 4.94% in 2006 to a low of 0.47% in 2009. The year-to-year change in risk-free rates was a decline of 28 basis points per year on average during our sample period.

[28] Cost of capital estimates vary by method and sample period. An estimate of 8.55% is lower than what is typically obtained from methods using analysts' forecasts. However, Easton (2006) notes that the lower estimate from the O'Hanlon and Steele (2000) method is a pervasive result. Baginski and Rakow (2012) use the O'Hanlon and Steele method on 2004 cross-sectional data and obtain an average cost of capital estimate across high and low disclosure quality portfolios of 8.16%. Thus, our estimate appears to be in line with prior research. In a subsequent section, we gauge the reasonableness of our average cost of capital estimate by comparing it to two additional independent cost of capital estimates.

[29] We cluster by firm and quarter (Peterson 2009) to obtain conservative standard errors in all regressions.

management forecast disclosure quality is negatively related to cost of equity capital, as predicted by theory. This result is consistent with the notion of free-riding to obtain cost of capital benefits.

In an earlier section, we estimated equation (3) to test H1 and discovered that performance was either unrelated or negatively related to quarterly management forecasting. The sign of this association is important for testing our hypothesis that the increase in free-rider disclosure is associated with a decrease in the free-riders' cost of equity capital (H2). Poorer performing free-riders have either no disclosure change or greater increases in their disclosure. Using content analysis, Kothari, Li, and Short (2009) document increases in uncertainty and cost of capital pursuant to disclosures of poor performance. Rogers, Skinner, and Van Buskirk (2009) use implied volatilities from exchange-traded options prices to show that management earnings forecasts, on average, increase short-term volatility, and that the effect is primarily attributable to sporadic forecasters conveying poor performance. So, combining our finding, consistent with Rogers et al. (2009), that poor performance is either unrelated to increased quarterly forecast disclosure or one driver of increased disclosure by free-riding (i.e., sporadic at best) disclosers with both of the aforementioned studies' finding that poor performance is associated with increased uncertainty/volatility suggests that the increased disclosure of poor performance might lead to no change or an increase in free-rider cost of capital. This condition implies a bias against our prediction that increases in free-rider disclosures are associated with decreases in their cost of equity capital. Given that we reject the null, we do not control for performance in our main tests.[30]

In contrast, we also discovered that performance was positively associated with free-rider increases in annual forecasts. Thus, it is possible that performance increased annual forecasts and reduced cost of equity capital. In tests described later, we control for firm-specific performance to document that the decreases in cost of capital associated with annual forecast increases are incremental to any possible change in cost of capital related to an increase in performance.

[30] An additional consideration is that the form of the *ex ante* model we estimate has ROE as the dependent variable (see equation 6). Thus, we would be adding to the regression an independent variable which is a transformation of the dependent variable. In supplemental tests described later, we employ an alternative test of H2 that does not suffer from this problem. In these tests, we control for firm-specific performance to estimate firm-specific cost of capital effects.

Finally, we discovered in our earlier tests that share issues were strongly positively associated with changes in both quarterly and annual forecasts in our sample. In supplemental tests, we added intercept and slope shifts for *Issue* analogous to the intercept and slope shifts for the changes in disclosure in equation (6). Our results (not tabulated) were not affected in a way that would change our inferences.

Economic Significance

The O'Hanlon and Steele (2000) estimation method yields direct estimates of cost of equity capital, and thus, allows a relatively easy way to gauge economic significance for free-riders. In Table 8, we present cost of capital (λ_0) and the difference between cost of capital in the pre-event and post-event periods (λ_2) estimates for the simple "univariate effect of cessation" model (equation 5*) for four different groups of free-riding firms. The groups represent various combinations of whether the free-rider responded to the cessation by issuing some form of forecast (quarterly or annual) and whether the industry/quarter had a single stopper or multiple stoppers.

We gauge the economic significance of increased disclosure on cost of equity capital using the magnitude of the change in λ_2 between groups. In the single stopper industries, λ_2 (column 2) minus λ_2 (column 1) shows that free-riders that begin to forecast have a decline in cost of capital that is 36 basis points more than the decline in cost of capital for free-riders that do not begin to forecast. In multiple stopper industries where information loss and thus cost of capital gains from disclosure are likely greatest, λ_2 (column 4) minus λ_2 (column 3) shows that the free-riders that begin to forecast have a decline in cost of capital that is 73 basis points more relative to the decline in cost of capital for free-riders who do not begin to forecast. The larger difference in declines when free-riders begin to forecast in multiple stopper industries compared to when free-riders begin to forecast in single stopper industries is consistent with the idea that the potential cost of capital decrease from forecasting is greater in instances where the information loss is the greatest.

V. REPLICATION USING ALTERNATIVE COST OF CAPITAL ESTIMATES

The large majority of cost of capital/disclosure relation tests are based on the levels of cost of capital rather than short-window changes. Faced with the need to obtain changes in cost of capital over the short run, Kothari et al. (2009) estimate the cost of equity capital using the Fama and French (1993) three-factor model. In this section, we use cost of capital estimates from this model as a robustness check on our primary tests of H2 using the O'Hanlon and Steele (2000) *ex ante* cost of capital specification.[31]

Firm-specific regressions of firm i's monthly excess return on the monthly factor returns provide factor loadings (b, s, and h):

$$R_i - R_f = \mathrm{a_i} + \mathrm{b_i}\,[R_m - R_f] + \mathrm{s_i}\,SML + \mathrm{h_i}\,HML + \mathrm{e_i} \qquad (7)$$

where the size factor (*SML*) equals small minus large firm returns, the book-to-market factor (*HML*) equals high minus low book-to-market firm returns, and the market factor ($R_m - R_f$) equals the excess return on the CRSP value-weighted portfolio. The monthly time-series returns on these factors are from the website provided by Kenneth French. We estimate b, s, and h coefficients using monthly returns for 60 months ending just prior to the event quarter (D = 0). We then multiplied each firm's estimated factor loadings by the average returns for the factors from 1963-2009 and annualize to obtain our cost of capital estimate based on the Fama-French model before cessation, CC_{FF0}. We repeat the process for the 60 months ending just prior to the end of the post-event period to obtain the cost of capital after the cessation disclosure responses by the free-riders, CC_{FF1}. $ChgCC_{FF}$ equals CC_{FF1} minus CC_{FF0}.

Our final sample has 10,716 cost of equity capital changes.[32] The mean and median cost of capital estimates are 10.08% and 9.04%, respectively, which are not much larger than the 8.55% *ex ante* cost of

[31] Kothari et al. (2009) question the appropriateness of *ex ante* models for tests of short-window cost of capital changes because the analyst forecasts of long-term growth used to estimate the models are sticky. Note that the primary *ex ante* measure we use estimates growth and cost of capital simultaneously, does not employ analyst forecasts, and is easily adaptable to event quarters that do not line up with fiscal year ends. They also point out that use of the Fama and French (1993) model assumes that the cost of capital effect is reflected in the sensitivities to the three Fama-French factors. Our primary *ex ante* measure also allows for a distinct information risk effect.

[32] We discard cost of capital estimates that are in the 1st and 99th percentile of the distribution and negative cost of capital estimates although our results do not depend on these filters.

capital estimate from our main tests. The slightly larger cost of capital estimate from this supplemental sample is not surprising given that it is about 50% larger than our main sample and includes smaller and riskier firms due to less restrictive data requirements relative to the requirements of the *ex ante* model.[33] To retest H2, we regress $ChgCC_{FF}$ on the change in free-rider disclosure, performance, and share issues:[34]

$$ChgCC_{FF} = \gamma_0 + \gamma_1\, ChVDQ + \gamma_2\, ChVDA + \gamma_3\, Performance + \gamma_4\, Issue + \varepsilon \qquad (8)$$

H2 predicts $\gamma_1, \gamma_2 < 0$. The findings in Kothari et al. (2009) suggest $\gamma_3 < 0$. Given that share issues generally are associated with increased information asymmetry, we predict $\gamma_4 > 0$.

Table 9 presents our results. We show the model estimation before and after the performance and share issue controls. The H2 predictions are supported by the change in the Fama-French cost of capital measure. Free-rider increases in quarterly and annual management forecasts are associated with decreases in cost of equity capital both before and after performance and share issue controls that have predicted relations with cost of equity capital.

VI. CONCLUSIONS

We show that a set of firms free-ride on the voluntary disclosure activities of other firms. In response to an information-decreasing industry disclosure shock, these free-riders increase annual and quarterly management earnings forecast disclosures. Our finding of a substitutive relationship for forecast policy choices implies distinct economic causes for policy decisions vis-à-vis herding behavior in the timing of individual acts of disclosure. Models describing a given firm's forecasting decisions are

[33] Our 8.55% *ex ante* cost of capital estimate is also fairly close to an independent estimate which can be calculated from data provided in Graham and Harvey (2010). They provide survey data over our time period on U.S. Chief Financial Officer perceptions of the equity risk premium relative to the 10-year U.S. Treasury bond yield. If the quarterly risk premia are weighted by the average CAPM beta for the 10,716 firms in our supplemental sample (1.24, a reasonable beta estimate given that the industries we are looking at have higher than average risk according to French's data), added to the bond yield, and averaged over the quarters in our sample, the cost of capital estimate is 8.24%.

[34] We cluster the regression on firm and quarter. Given that Table 6 shows that share issues are a firm-specific and period-specific determinant of free-rider disclosure change, we include it in the regression as a control. Although the Table 6 results on performance obtain only when performance is measured as a 1/0 indicator variable and only in the extended sample, we include it as a control variable because of Kothari et al.'s (2009) finding of a negative association between favorable/unfavorable news and cost of equity capital.

incomplete if they do not consider the forecasting decisions of other firms. The interrelationship in disclosure policy choice is a real externality in that it is associated with financing cash outflows via a reduction in cost of equity capital and operating cash flows via forecast disclosure costs. In the presence of disclosure policy dependency and reactive disclosure by previously free-riding firms, the net benefits of self-disclosure exceed the net benefits of free-riding for many firms. This informs the debate on the consequences of guidance policy.

Future research is necessary to completely specify the industry dynamics at play in disclosure decisions and cost of capital effects. We rely on intra-industry information transfer to predict intra-industry responses and effects. Other research can focus on inter-industry relationships (e.g., customer-supplier) as well as less obvious but equally interesting relationships that exist between public and private firms. Also, we focus on financial effects of disclosure responses (which is itself a real effect). Other research can focus on real effects on product or process investment and operating decisions.

Finally, we define the treatment effect as a change in forecast policy (cessation) rather than as a property of a given forecast act, such as its timing or content. We define the reaction to the treatment effect as the initiation of forecasting rather than measuring the properties of the reactive forecasts, such as their timing or content. To complete the understanding of industry disclosure dynamics, future research can examine the interrelationships of forecast properties as well.

REFERENCES

Aboody, D., and R. Kasznik. 2000. CEO stock option awards and the timing of corporate voluntary disclosures. *Journal of Accounting and Economics* 29: 73-100.

Amihud, Y., and H. Mendleson. 1986. Asset pricing and the bid-ask spread. *Journal of Financial Economics* 17: 223-249.

Anantharaman, D., and Y. Zhang. 2011. Cover me: Managers' responses to changes in analyst coverage in the post-Regulation FD period. *The Accounting Review* 86: 1851-1885.

Anilowski, C., M. Feng, and D. Skinner. 2007. Does earnings guidance affect market returns? The nature and information content of aggregate earnings guidance. *Journal of Accounting and Economics* 44: 36-63.

Badertscher, B., N. Shroff, and H. White. 2013. Externalities of public firms' disclosures: Evidence from private firms' investment decisions. *Journal of Financial Economics* 109: 682-706.

Baginski, S. 1987. Intraindustry information transfers associated with management forecasts of earnings. *Journal of Accounting Research* 25: 196-216.

Baginski, S., S. Clinton, and S. McGuire. 2013. Forward-looking voluntary disclosure in proxy contests. Working paper, University of Georgia, University of Tennessee, Texas A&M University, *SSRN eLibrary*.

Baginski, S., and K. C. Rakow. 2012. Management earnings forecast disclosure policy and the cost of equity capital. *Review of Accounting Studies* 17: 279-321.

Balakrishnan, K., M. Billings, B. Kelly, and A. Ljungqvist. 2012. Shaping liquidity: On the causal effects of voluntary disclosure. *Journal of Finance*, forthcoming.

Bamber, L., and S. Cheon. 1998. Discretionary management earnings forecast disclosures: Antecedents and outcomes associated with forecast venue and forecast specificity choices. *Journal of Accounting Research* 36: 167-190.

Barry, C., and S. Brown. 1985. Differential information and security market equilibrium. *Journal of Financial and Quantitative Analysis* 20: 407-422.

Beatty, A., S. Liao, and J. Yu. 2011. The spillover effect of fraudulent financial reporting on peer firms' investments. *Journal of Accounting and Economics* 55: 183-205.

Bernard, V. 1995. The Feltham-Ohlson framework: Implications for empiricists. *Contemporary Accounting Research* 11: 733-747.

Bhattacharya, N., F. Ecker, P. Olsson, and K. Schipper. 2012. Direct and mediated associations among earnings quality, information asymmetry and the cost of equity. *The Accounting Review* 87: 449-482.

Botosan, C. 1997. Disclosure level and the cost of equity capital. *The Accounting Review* 72: 323–49.

Botosan, C., and M. Plumlee. 2002. A Re-examination of disclosure level and expected cost of equity capital. *Journal of Accounting Research* 40: 21–40.

Botosan, C., and M. Plumlee. 2005. Assessing alternative proxies for the expected risk premium. *The Accounting Review* 80: 21-53.

Botosan, C., M. Plumlee, M., and H. Wen. 2011. The relation between expected returns, realized returns, and firm risk characteristics. *Contemporary Accounting Research*, 28: 1085–1122.

Botosan, C., M. Plumlee, and Y. Xie. 2004. The role of information precision in determining cost of equity capital. *Review of Accounting Studies* 9: 233-259.

Bradshaw, M., M. Drake, J. Myers, and L. Myers. 2012. A re-examination of analysts' superiority over time-series forecasts of annual earnings. *Review of Accounting Studies* 17: 944-968.

Brennan, N. 1999. Voluntary disclosure of profit forecasts by target companies in takeover bids. *Journal of Business Finance & Accounting* 26: 883-917.

Chen, S., D. Matsumoto, and S. Rajgopal. 2011. Is silence golden? An empirical analysis of firms that stop giving quarterly earnings guidance. *Journal of Accounting and Economics* 51: 134-150.

Cheng, Q., T. Luo, and H. Yue. 2013. Managerial incentives and management forecast precision. *The Accounting Review*, forthcoming.

Chuk, E., D. Matsumoto, and G. Miller. 2013. Assessing methods of identifying management forecasts: CIG vs. researcher collected. *Journal of Accounting and Economics* 55: 23-42.

Coles, J., U. Loewenstein, and J. Suay. 1995. On equilibrium pricing under parameter uncertainty. *Journal of Financial and Quantitative Analysis* 30: 347-364.

Core, J. 2001. A review of the empirical disclosure literature: Discussion. *Journal of Accounting and Economics* 31: 441-456.

Cotter, J., I. Tuna, and P. Wysocki. 2006. Expectations management and beatable targets: How do analysts react to explicit earnings guidance? *Contemporary Accounting Research* 23(3): 593-624.

Diamond, D. 1985. Optimal release of information by firms. *Journal of Finance* 40: 1071-1094.

Diamond, D., and R. Verrecchia. 1991. Disclosure, liquidity and the cost of equity capital. *The Journal of Finance* 46: 1325-1360.

Durnev, A., and C. Mangen. 2009. Corporate investments: Learning from restatements. *Journal of Accounting Research* 47: 679-720.

Dye, R., 1990. Mandatory versus voluntary disclosures: The cases of financial and real externalities. *The Accounting Review* 65: 1-24.

Easley, D., and M. O'Hara. 2004. Information and the cost of capital. *The Journal of Finance* 59: 1553-1583.

Easton, P. 2006. Use of forecasts of earnings to estimate and compare cost of capital across regimes. *Journal of Business, Finance, and Accounting* 33: 374-394.

Easton, P. 2009. Estimating the cost of capital implied by market prices and accounting data. *Foundations and Trends in Accounting* 2: 241-364.

Easton, P., and S. Monahan. 2005. An evaluation of accounting-based measures of expected returns. *The Accounting Review* 80: 501-538.

Easton, P., and G. Sommers. 2007. Effects of analysts' optimism on estimates of the expected rate of return implied by earnings forecasts. *Journal of Accounting Research* 45: 983-1015.

Easton, P., G. Taylor, P. Shroff, and T. Sougiannis. 2002. Using forecasts of earnings to simultaneously estimate growth and the rate of return on equity investment. *Journal of Accounting Research* 40: 657-676.

Edwards, E., and P. Bell. 1961. *The Theory and Measurement of Business Income*. Berkeley, CA: University of California Press.

Einhorn, E., and A. Ziv. 2008. Intertemporal dynamics of corporate voluntary disclosures. *Journal of Accounting Research* 46: 567-590.

Fama, E., and K. French. 1993. Common risk factors in the returns on stocks and bonds. *Journal of Financial Economics* 33: 3-56.

Feltham, G., and J. Ohlson. 1995. Valuation and clean surplus accounting for operating and financial activities. *Contemporary Accounting Research* 11: 689-731.

Feltham, G., and J. Ohlson. 1996. Uncertainty resolution and the theory of depreciation. *Journal of Accounting Research* 34: 209-34.

Fishman, M., and K. Hagerty. 1989. Disclosure decisions by firms and the competition for price efficiency. *Journal of Finance* 44: 633-646.

Francis, J., D. J. Nanda, and P. Olsson. 2008. Voluntary disclosure, earnings quality, and cost of capital. *Journal of Accounting Research* 46: 53-100.

Francis, J., D. Philbrick, and K. Schipper. 1994. Shareholder litigation and corporate disclosures. *Journal of Accounting Research* 32: 137–164.

Freeman, R., and S. Tse. 1992. An earnings prediction approach to examining intercompany information transfers. *Journal of Accounting and Economics* 15: 509-523.

Foster, G. 1980. Externalities and financial reporting. *Journal of Finance* 35: 521-533.

Foster, G. 1981. Intra-industry information transfers associated with earnings releases. *Journal of Accounting and Economics* 3: 201-232.

Fuller, J., and M. Jensen. 2002. Just say no to Wall Street: Putting a stop to the earnings game. *Journal of Applied Corporate Finance* 14: 41-46.

Graham, J., and C. Harvey. 2010. The equity risk premium in 2010. Working paper, Duke University and the National Bureau of Economic Research.

Han, J., and J. Wild. 1990. Unexpected earnings and interindustry information transfers: Further evidence. *Journal of Accounting Research* 28: 211-219.

Han, J., J. Wild, and K. Ramesh. 1989. Managers' earnings forecasts and intra-industry information transfers. *Journal of Accounting and Economics* 11: 3-33.

Handa, P., and S. Linn. 1993. Arbitrage pricing with estimation risk. *Journal of Financial Economics* 33: 81-100.

Hayn, C. 1995. The information content of losses. *Journal of Accounting and Economics* 20: 125-153.

Healy, P., and K. Palepu. 2001. Information asymmetry, corporate disclosure, and the capital markets: A review of the empirical disclosure literature. *Journal of Accounting and Economics* 31: 405-440.

Hirst, E., L. Koonce, and Venkataraman. 2008. Management earnings forecasts: A review and framework. *Accounting Horizons* 22: 315-338.

Houston, J., B. Lev, and J. Tucker. 2010. To guide or not to guide? Causes and consequences of stopping quarterly earnings guidance. *Contemporary Accounting Research* 27: 143-185.

Hughes, J., Liu, J., and Liu, J. (2007). Information, diversification, and cost of capital. *The Accounting Review* 82, 705-30.

Hutton, A., and P. Stocken. 2009. Prior forecasting accuracy and investor reaction to management earnings forecasts. Unpublished working paper, Boston College and Dartmouth. *SSRN eLibrary*.

Jorgensen, B., and M. Kirschenheiter. 2012. Interactive discretionary disclosures. *Contemporary Accounting Research* 29: 1-15.

Kim, Y., M. Lacina, and M Park. 2008. Positive and negative information transfers from management forecasts. *Journal of Accounting Research* 46: 885–908.

King, R., G. Pownall, and G. Waymire. 1990. Expectations adjustments via timely management forecasts: Review, synthesis, and suggestions for future research. *Journal of Accounting Literature* 9: 113-114.

Kothari, S. P., X. Li, and J. Short. 2009. The effect of disclosures by management, analysts, and business press on cost of capital, return volatility, and analyst forecasts: A study using content analysis. *The Accounting Review* 84 (5): 1639–1670.

Krehmeyer, D., and M. Orsagh. 2006. Breaking the short-term cycle: Discussion and recommendations on how corporate leaders, asset managers, investors, and analysts can refocus on long-term value. Proceedings of the CFA Center on Financial Markets Integrity and Business Roundtable Institute for Corporate Ethics Symposium Series on Short-termism.

Lambert, R., C. Leuz, and R. Verrecchia. 2007. Accounting information, disclosure, and the cost of capital. *Journal of Accounting Research* 45: 385-420

Lambert, R., C. Leuz, and R. Verrecchia. 2012. Information asymmetry, information precision, and the cost of capital. *Review of Finance* 16: 1-29.

Larcker, D., and T. Rusticus. 2010. On the use of instrumental variables in accounting research. *Journal of Accounting and Economics* 49: 186-205.

Larocque, S. 2013. Analysts' earnings forecast errors and cost of equity capital estimates. *Review of Accounting Studies* 18: 135-166.

Leftwich, R. 1980. Market failure fallacies and accounting information. *Journal of Accounting and Economics* 2: 193-211.

Leuz, C., and C. Schrand. 2009. Disclosure and the cost of equity capital: evidence from firms' responses to the Enron shock. Working paper, University of Chicago and University of Pennsylvania.

Leuz, C. and R. Verrecchia. 2000. The economic consequences of increased disclosure. *Journal of Accounting Research* 38: 91-124.

Li, E., C. Wasley, and J. Zimmerman. 2012. Should management guidance be pooled? Classifying management earnings guidance into capital market, opportunistic, and disclose or abstain rationales: Implications for research. Working paper, University of Rochester. *SSRN eLibrary*.

Ma, M. 2013. Industry peer firm earnings quality and the cost of equity capital. Working paper, University of Oklahoma.

Matsumoto, D. 2002. Management's incentives to avoid negative earnings surprises. *The Accounting Review* 77: 483-514.

Miller, G. 2002. Earnings performance and discretionary disclosure. *Journal of Accounting Research* 40: 173-204.

Newman, P., and R. Sansing. 1993. Disclosure policies with multiple users. *Journal of Accounting Research* 31: 92-112.

Noe, C. 1999. Voluntary disclosures and insider transactions. *Journal of Accounting and Economics* 27: 305-326.

O'Hanlon, J., and A. Steele. 2000. Estimating the equity risk premium using accounting fundamentals. *Journal of Business, Finance, and Accounting* 27: 1051-1084.

Ohlson, J. 1989. Accounting earnings, book value and dividends: The theory of the clean surplus equation (Part I). Reproduced in R. Brief and K. Peasnell (eds.) *Clean Surplus: A Link Between Accounting and Finance* (Garland): 167-227.

Ohlson. J. 1990. A Synthesis of Security Valuation Theory' and the Role of Dividends, Cash Flows and Earnings. *Contemporary Accounting Research*:6: 648-76.

Ohlson, J. 1995. Earnings, Book Values and Dividends in Equity Valuation. *Contemporary Accounting Research* 11: 661-87.

Peterson, M. 2009. Estimating standard errors in finance panel data sets: Comparing approaches. *The Review of Financial Studies* 22: 435-480.

Piotroski, J. 2002. The impact of management forecasts on short-term stock price volatility. Working paper, Stanford University.

Pownall, G., and G. Waymire. 1989. Voluntary disclosure choice and earnings information transfer. *Journal of Accounting Research* 27: 85-105.

Preinreich, G. 1938. Annual survey of economic theory: The theory of depreciation. *Econometrica* 6: 219-241.

Pyo, Y., and S. Lustgarten. 1990. Differential intra-industry information transfer associated with management earnings forecasts. *Journal of Accounting and Economics* 13: 365-379.

Rogers, J., D. Skinner, and A. Van Buskirk. 2009. Earnings guidance and market uncertainty. *Journal of Accounting & Economics* 48: 90-109.

Rogers, J., and A. Van Buskirk. 2013. Bundled forecasts in empirical accounting research. *Journal of Accounting and Economics* 55: 43-65.

Roulstone, D. 2003. Analyst following and market liquidity. *Contemporary Accounting Research* 20: 552-578.

Scherin, K. 2010. Financial planning and investor communications at GE (With a look at why we ended earnings guidance). *Journal of Applied Corporate Finance* 22: 8-17.

Skinner, D. 1994. Why firms voluntarily disclose bad news. *Journal of Accounting Research* 32: 38-60.

Skinner, D. 1997. Earnings disclosure and stockholder lawsuits. *Journal of Accounting and Economics* 23: 249–282.

Stocken, P. 2000. Credibility of voluntary disclosure. *The RAND Journal of Economics* 32 (2): 359-374.

Trueman, B. 1986. Why do managers voluntarily release earnings forecasts? *Journal of Accounting and Economics* 8 (1): 53–71.

Tse, S., and J. W. Tucker. 2010. Within-industry timing of earnings warnings: Do managers herd? *Review of Accounting Studies*: 15: 879–914.

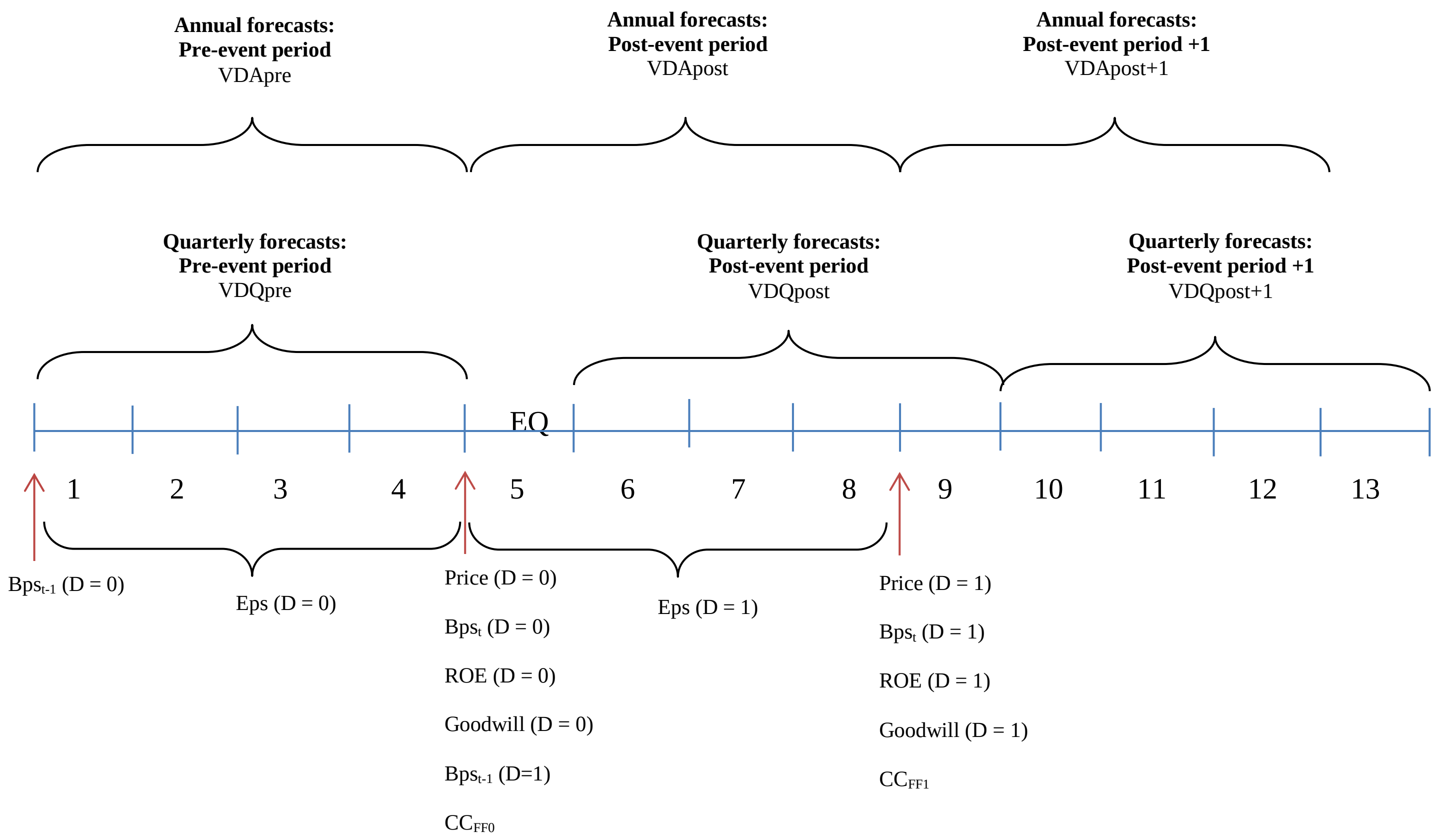
Verrecchia, R. 1983. Discretionary disclosure. *Journal of Accounting and Economics* 5: 179–194.

Verrecchia, R. 2001. Essays on disclosure. *Journal of Accounting and Economics* 32: 97-180.

Verrecchia, R., and J. Weber. 2006. Redacted disclosure. *Journal of Accounting Research* 44: 791-814.

Wang, C. 2012. Measurement errors of expected returns proxies and the implied cost of capital. Working paper, Stanford University.

FIGURE 1
Time Frames for Free-Rider Variable Measurements



APPENDIX A
Variable Definitions

Variable	Definition
Stopper	a firm that issues quarterly management forecasts for at least three of the four quarters in the pre-event period and does not issue a forecast for any of the four quarters in the post-event period; the *event quarter* is the first quarter for which it does not issue a forecast
Co-members	firms that report net income and belong to the same four-digit SIC code (in the event quarter) as a *Stopper*
Free-riders	*Co-members* that do not issue forecasts (either quarterly or annual) in the four quarters preceding the event quarter
VDQpre	the number of quarters for which the firm issued at least one quarterly management forecast out of the four quarters in the "pre-event" period
VDQpost	the number of quarters for which the firm issued at least one quarterly management forecast out of the four quarters subsequent to the event quarter
VDQpost+1	the number of quarters for which the firm issued at least one quarterly management forecast out of the four quarters in quarters five through eight subsequent to the event quarter
VDApre	1 if the firm issued at least one annual management forecast for the year preceding the event year and 0 otherwise
VDApost	1 if the firm issued at least one annual management forecast for the event year and 0 otherwise
VDApost+1	1 if the firm issued at least one annual management forecast for the year following the event year and 0 otherwise
D	0 for the end of the "pre-event" period and 1 for the end of the "post-event" period
P_{jt}	closing stock price for firm j at time t
Bps_{jt}	book value per share for firm j at time t
Eps_{jt}	annual earnings per share for firm j for the year ended at time t
ROE	$\frac{Eps_{jt}}{Bps_{jt-1}}$; primary dependent variable measured for firm j and time t; when D=0, t = the end of the "pre-event" period; when D=1, t = the end of the "post-event" period
Goodwill	$\frac{P_{jt}-Bps_{jt}}{Bps_{jt-1}}$; primary independent variable measured for firm j and time t; when D=0, t = the end of the "pre-event" period; when D=1, t = the end of the "post-event" period
NumStoppers	the number of *Stoppers* in the SIC code and event quarter
Market value of equity	market value of equity at the end of the event quarter
#Analysts	the number of one-quarter ahead analyst forecasts of EPS for the event quarter
%Earnings Change	the change in earnings before extraordinary items from the quarter preceding the event quarter to the event quarter deflated by beginning value
%Price Change	the change in closing price from the beginning of the event quarter to the end of the event quarter deflated by beginning value
Performance	1 if post-event *ROE* is greater than pre-event *ROE* and 0 otherwise
Issue	1 if average shares outstanding in the post-event period is greater than the average shares outstanding in the pre-event period and 0 otherwise
SML, HML, $(R_m – R_f)$	size factor (small minus large firm returns), book-to-market factor (high minus low book-to-market firm returns), and market factor (excess return on the CRSP value-weighted portfolio) obtained from Kenneth French's website
CC_{FF0}, CC_{FF1}	Cost of equity capital estimated from Fama-French three factor model in pre-event and post-event periods
$ChgCC_{FF}$	CC_{FF1} minus CC_{FF0}
AnalystDecrease	1 if analyst coverage decreased between pre- and post-event periods and zero otherwise
AbsCorrFR_NFR	average absolute Pearson correlation between the earnings changes of the free-rider and non-free-riding industry co-members over the 16 quarters preceding the event quarter (15 change observations)

TABLE 1
Sample Selection Process

Quarterly Forecast Stoppers	
Initial sample from First Call	820
Remove observations due to discovery of quarterly forecasts on Business Wire, PR Newswire, Reuters Significant Developments, or Associated Press Newswire	(164)
Final sample of quarterly forecast stoppers	656
Industry Co-members	
Four digit SIC code co-members of quarterly forecast stopper	55,159
Firms with insufficient data to estimate cost of capital	(14,692)
Non-free-riders (i.e., issued annual or quarterly management forecasts in pre-event year; a subset of these firms meeting all other data requirements are used as a control group when testing H1)	(13,648)
Delete negative EPS, book value less than 1, and observations above (below) the 99th percentile (1st percentile) of either the dependent or independent variable (reintroduced into the sample for a supplemental test of H1)	(19,358)
Final sample of free-riders used to test both H1 and H2	7,461

TABLE 2
Stopper Characteristics

Panel A: Size, Market-to-Book, Analyst Following, and Performance

Variable	Observations	Mean	Standard deviation	25th percentile	Median	75th percentile
Market value of equity	409	4,881.91	18,309.56	283.12	888.99	3,230.70
#Analysts	386	8.17	5.89	4.0	7.0	12.0
% Price change	414	-0.05	0.27	-.20	-0.02	0.09
% Earnings change	415	-0.94	20.78	-0.65	-0.04	0.62

Panel B: Top Ten Industries (2-digit SIC) Included in Sample

Industry	SIC	Observations
Business services	73	139
Measurement equipment	38	46
Machinery and computers	35	43
Holding and investment offices	67	38
Chemical products	28	30
Electrical equipment	36	28
Home furniture stores	57	27
Health services	80	20
Eating and drinking services	58	17
Apparel	56	14

Panel C: Event Year

Year	Observations	Percent
2004	91	13.8%
2005	115	17.5%
2006	142	21.7%
2007	129	19.7%
2008	124	18.9%
2009	55	8.4%
Total	656	100.0%

Panel D: Annual Management Forecasting Behavior

Year	Observations	Percent
Annual forecast in pre-event and event period	374	57.0%
Annual forecast initiator	32	5.6%
Annual forecast stopper	107	16.3%
Annual forecast in neither period	128	21.1%
Total	656	100.0%

See Appendix A for all variable definitions.

TABLE 3
Free-Rider Characteristics

Panel A: Size, Analyst Following, and Market-to-Book Ratio

Variable	Firm Observations	Mean	Standard deviation	25th percentile	Median	75th percentile
Main sample for tests of H1 and H2						
Market value of equity	7,461	4,320.77	16,196.88	138.70	539.70	1,714.54
#Analysts	7,461[1]	3.59	5.56	0.0	1.0	5.0
Extended sample for supplemental test of H1						
Market value of equity	26,515	1,467.80	9,104.69	19.59	82.16	394.76
#Analysts	26,819[2]	1.91	4.00	0.0	0.0	2.0

Panel B: Primary Dependent and Independent Variables Used in the Cost of Capital Regression to Test H2

Variable	Observations	Mean	Standard deviation	25th percentile	Median	75th percentile
ROE	14,922	0.14	0.11	0.07	0.11	0.18
Goodwill	14,922	1.78	2.14	0.41	1.08	2.36

[1] 4,346 of the observations have nonzero analyst coverage.

[2] 9,875 of the observations have nonzero analyst coverage.

See Appendix A for all variable definitions.

TABLE 4
Free-Rider Forecasting Behavior

Panel A: Immediate Increases in Management Forecast Disclosures by Free-Riders

	Main Sample		Extended Sample	
Quarterly Forecast Increases	**Firm Observations**	**Percentage**	**Firm Observations**	**Percentage**
No increase	7,072	94.8%	25,702	95.8%
Forecasted for one of the four quarters subsequent to event quarter	210	2.8%	501	1.9%
Forecasted for two of the four quarters subsequent to the event quarter	87	1.2%	209	0.7%
Forecasted for three of the four quarters subsequent to the event quarter	26	0.3%	193	0.8%
Forecasted for four of the four quarters subsequent to the event quarter	66	0.9%	214	0.8%
Total	7,461	100.0%	26,819	100.0%
Annual Forecast Increases				
No increase	6,972	93.4%	25,477	95.0%
Released at least one annual forecast for the event year	489	6.6%	1,342	5.0%
Total	7,461	100.0%	26,819	100.0%

Panel B: Immediate Increase in Either Annual or Quarterly Management Forecast Disclosures Partitioned by the Number of Stoppers in the Industry and Analyst Coverage

	Main Sample (n = 7,461): Percentage of Firms that Initiated Forecasting		Extended Sample (n = 26,819): Percentage of Firms that Initiated Forecasting	
Number of Stoppers	**All Observations**	**Covered by Analysts**	**All Observations**	**Covered by Analysts**
One	7.0%	10.2%	5.4%	9.6%
Two	8.1%	10.6%	5.6%	11.4%
Three	12.3%	15.9%	10.1%	23.0%
Four	14.2%	21.2%	6.9%	18.4%
Five	14.7%	20.0%	12.3%	27.0%
Six	11.3%	15.1%	10.5%	24.2%
Seven	26.4%	31.9%	17.4%	39.5%
Eight	18.6%	24.2%	10.1%	29.6%
Full sample	9.9%	13.7%	7.9%	16.1%

TABLE 4 (continued)

Panel C: Persistence in Quarterly and Annual Forecast Frequencies over Time

	Main Sample		Extended Sample	
Number of Forecasts Issued in Post-Event Period	**Percent of Firms that Continued to Forecast in Post-Event Period +1**	**Percent of Firms that Maintained or Increased Number of Forecasts in Post-Event Period +1**	**Percent of Firms that Continued to Forecast in Post-Event Period +1**	**Percent of Firms that Maintained or Increased Number of Forecasts in Post-Event Period +1**
Quarterly Forecasts				
One	41.9%	41.9%	43.7%	43.7%
Two	79.3%	72.4%	68.9%	58.9%
Three	92.3%	69.2%	85.0%	66.8%
Four	100.0%	81.8%	99.1%	74.3%
Annual Forecasts				
One	64.8%	64.8%	58.9%	58.9%

See Appendix A for all variable definitions.

TABLE 5
Do Macro Factors Drive Free-Rider Forecast Frequency Increases?

Panel A: Intertemporal Forecasting Behavior Relative to Other Industry Co-members

		Main Sample		Extended Sample	
Mean number of forecasts issued by the firms in the following periods:	**Stoppers (656 firms)**	**Potential Free-Rider Industry Co-members (7,461 firms)**	**Non-Free-Rider Industry Co-members (8,296 firms)**	**Potential Free-Rider Industry Co-members (26,819 firms)**	**Non-Free-Rider Industry Co-members (13,648 firms)**
Quarterly:					
VDQpre	3.547	0.000	2.194	0.000	2.065
VDQpost	0.038	0.097	2.111	0.088	1.944
VDQpost+1	0.172	0.148	1.880	0.123	1.732
Annual:					
VDApre	0.733	0.000	0.822	0.000	0.780
VDApost	0.626	0.065	0.793	0.050	0.712
VDApost+1	0.362	0.093	0.767	0.068	0.676

Panel B: Event Period Potential Free-Rider and Non-Free-Rider Responses by Number of Stoppers

Number of Stoppers	**Free-Rider Firm Observations**	**Percentage**	**Mean Increase in *Free-Rider Quarterly* Forecasts**	**Mean Increase in *Free-Rider Annual* Forecasts**	**Mean Increase in *Non-Free-Rider Quarterly* Forecasts**	**Mean Increase in *Non-Free-Rider Annual* Forecasts**
Main Sample						
One	3,472	46.5%	0.065	0.047	-0.103	-0.040
Two	1,195	16.0%	0.062	0.054	-0.188	-0.036
Three	830	11.1%	0.119	0.086	-0.093	-0.003
Four	309	4.1%	0.207	0.052	-0.239	-0.093
Five	757	10.2%	0.113	0.118	0.002	-0.043
Six	573	7.7%	0.160	0.052	0.011	-0.039
Seven	163	2.2%	0.257	0.264	0.045	0.052
Eight	162	2.2%	0.247	0.049	0.142	0.043
Total	7,461	100.0%				
Extended Sample						
One	10,069	37.5%	0.050	0.036	-0.164	-0.084
Two	4,808	17.9%	0.053	0.039	-0.200	-0.071
Three	3,558	13.3%	0.105	0.067	-0.154	-0.044
Four	1,452	5.4%	0.143	0.033	-0.382	-0.104
Five	2,570	9.6%	0.117	0.082	0.043	-0.072
Six	2,124	7.9%	0.127	0.059	0.040	-0.048
Seven	966	3.6%	0.196	0.123	-0.057	0.000
Eight	1,272	4.7%	0.195	0.038	0.038	-0.075
Total	26,819	100.0%				

See Appendix A for all variable definitions.

TABLE 6
Does Information Loss Drive Free-Rider Forecast Frequency Increases?

			Dependent variable: Increase in[1]			
Main Sample			**Quarterly Forecasts**	**Annual Forecasts**	**Quarterly Forecasts**	**Annual Forecasts**
Variable	**Coefficient**	**Predicted Sign**	**Coefficient Estimate (χ^2)**	**Coefficient Estimate (χ^2)**	**Coefficient Estimate (χ^2)**	**Coefficient Estimate (χ^2)**
Performance	β_1	None	-0.0173 (0.0)	0.4212** (6.5)	0.0925 (0.2)	0.3868** (4.8)
Issue	β_2	+	0.5418*** (7.1)	0.4625*** (7.2)	0.5242*** (5.9)	0.5349*** (8.2)
NumStoppers	β_3	+	0.1782*** (14.5)	0.1479*** (11.7)	0.1555*** (8.8)	0.1815*** (15.5)
Log (#Analysts + 1)	β_4	+	0.6646*** (57.6)	0.3900*** (25.0)	0.5873*** (37.9)	0.3360*** (16.2)
AnalystDecrease	β_5	+	0.2550 (1.1)	0.1551 (0.4)	0.3715* (2.3)	0.2684 (1.1)
AbsCorrFR_NFR	β_6	-			-0.7979 (0.9)	0.2985 (0.2)
Observations			7,461	7,461	6,705	6,705
Model χ^2			94.2***	66.6***	78.0***	59.4***
Extended Sample						
Performance	β_1	None	0.2007* (2.8)	0.3763*** (13.2)	0.2709** (4.1)	0.3239*** (8.2)
Issue	β_2	+	0.2563** (3.2)	0.2380** (3.8)	0.3241** (4.0)	0.3358*** (6.0)
NumStoppers	β_3	+	0.2372*** (72.4)	0.1462*** (35.2)	0.2203*** (49.7)	0.1599*** (35.2)
Log (#Analysts + 1)	β_4	+	1.0250*** (324.5)	0.7361*** (227.4)	0.9529*** (220.4)	0.0749*** (152.6)
AnalystDecrease	β_5	+	0.1240 (0.5)	0.1127 (0.5)	0.1812 (1.0)	0.1990 (1.4)
AbsCorrFR_NFR	β_6	-			-2.2554*** (8.0)	-0.0046 (1.0)
Observations			26,819	26,819	23,073	23,073
Model χ^2			392.5***	290.4***	273.8***	211.6***

Model (estimated using ordered logistic regression clustered by firm and quarter; intercepts not tabulated):

$$VDQ \text{ or } VDA = \beta_0 + \beta_1\, Performance + \beta_2\, Issue + \beta_3\, NumStoppers + \beta_4\, Log(\#Analysts + 1) + \beta_5\, AnalystDecrease + \beta_6\, AbsCorrFR\_NFR + \varepsilon$$

*, **, and *** indicate statistical significance at $p < 0.1$, $p < 0.05$ and $p < 0.01$ (one-tailed tests when a sign is predicted).

[1]The increase in quarterly and annual forecasts from the pre to post-event period simply equals *VDQpost* and *VDApost*, respectively, because, by definition, free-rider *VDQpre* and *VDApre* equal zero.

See Appendix A for all variable definitions.

TABLE 7

The Effect of Voluntary Disclosure Cessation by Firm i on the Cost of Capital of Firm j

		Base Model	**Univariate Effect of Cessation**	**Conditioned On Change in Quarterly Forecasting**	**Conditioned On Change in Quarterly and Annual Forecasting**
Variable	**Coefficient**	**Coefficient Estimate (*t*-statistic)**	**Coefficient Estimate (*t*-statistic)**	**Coefficient Estimate (*t*-statistic)**	**Coefficient Estimate (*t*-statistic)**
Intercept	λ_0	0.0855*** (51.80)	0.0889*** (42.15)	0.0889*** (42.14)	0.0889*** (42.14)
Goodwill	λ_1	0.0318*** (33.81)	0.0305*** (27.86)	0.0305*** (27.85)	0.0305*** (27.85)
D	λ_2		-0.0067*** (-2.94)	-0.0065*** (-2.81)	-0.0054** (-2.33)
*D * Goodwill*	λ_3		0.0026* (1.86)	0.0035** (2.38)	0.0035** (2.40)
*ChVDQ * D*	λ_4			-0.0113*** (-4.29)	-0.0092*** (-3.15)
*ChVDQ * D * Goodwill*	λ_5			-0.0023*** (-2.75)	-0.0022* (-1.72)
*ChVDA * D*	λ_6				-0.0221*** (-3.16)
*ChVDA * D * Goodwill*	λ_7				0.0003 (0.08)
R^2		35.7%	35.8%	36.1%	36.2%
Observations		14,922	14,922	14,922	14,922

Models (estimated using ordinary least squares with firm and quarter clustering):

Base model: $ROE = \lambda_0 + \lambda_1\ Goodwill + \varepsilon$

Univariate effect of cessation: $ROE = \lambda_0 + \lambda_1\ Goodwill + \lambda_2\ D + \lambda_3\ D{*}Goodwill + \varepsilon$

Conditioned on disclosure change: $ROE = \lambda_0 + \lambda_1\ Goodwill + \lambda_2\ D + \lambda_3\ D{*}Goodwill$

Quarterly forecasting change: $+ \lambda_4\ ChVDQ{*}D + \lambda_5\ ChVDQ{*}D{*}Goodwill$

Annual forecasting change: $+ \lambda_6\ ChVDA{*}D + \lambda_7\ ChVDA{*}D{*}Goodwill$

*, **, and *** indicate statistical significance at $p < 0.1$, $p < 0.05$ and $p < 0.01$ (two-tailed tests).

See Appendix A for all variable definitions.

TABLE 8
Economic Significance for Various Effects

		Single stopper industry		**Multiple stopper industry**	
		(Column **1**) *No increase* in management earnings forecast disclosure	(Column **2**) *An increase* in management earnings forecast disclosure	(Column **3**) *No increase* in management earnings forecast disclosure	(Column **4**) *An increase* in management earnings forecast disclosure
Variable	**Coefficient**	**Coefficient Estimate (n = 6,458)**	**Coefficient Estimate (n = 486)**	**Coefficient Estimate (n = 6,994)**	**Coefficient Estimate (n = 984)**
Intercept	λ_0	0.0952	0.0878	0.0846	0.0662
D	λ_2	-0.0043	-0.0079	-0.0097	-0.0170
Effect of free-rider disclosure in single stopper industry	λ_2(**2**) - λ_2(**1**)	36 basis points			
Effect of free-rider disclosure in multiple stopper industry	λ_2(**4**) - λ_2(**3**)	73 basis points			

Model (estimated using ordinary least squares with firm and quarter clustering):

$$ROE = \lambda_0 + \lambda_1\ Goodwill + \lambda_2\ D + \lambda_3\ D{*}Goodwill + \varepsilon$$

See Appendix A for all variable definitions.

TABLE 9
The Association of Changes in Firm j Disclosure and Changes in Firm j Cost of Capital (Fama-French Based Estimates of Cost of Equity Capital)

Variable	**Coefficient**	**Predicted Sign**	**Coefficient Estimate (*t*-statistic)**	
Intercept	γ_0	None	0.0022** (2.14)	0.0024 (1.31)
ChVDQ	γ_1	-	-0.0105*** (-4.59)	-0.0103*** (-4.60)
ChVDA	γ_2	-	-0.0090** (-2.15)	-0.0082** (-1.97)
Performance	γ_3	-		-0.0088*** (-4.60)
Issue	γ_4	+		0.0048*** (2.40)
R^2			1.1%	1.8%
Observations			10,736	10,736

Model (estimated using ordinary least squares with firm and quarter clustering):

$ChgCC_{FF} = \gamma_0 + \gamma_1\ ChVDQ + \gamma_2\ ChVDA + \gamma_3\ Performance + \gamma_4\ Issue + \varepsilon$

*, **, and *** indicate statistical significance at p < 0.1, p < 0.05 and p < 0.01 (two-tailed tests; one-tailed for sign predictions).

See Appendix A for all variable definitions.