Noncompete Agreements, Product-Market Competition and Company Disclosure

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Abstract

This study explores the impact on companies' disclosure policies of U.S. states' different propensities to enforce noncompete agreements. I find that, when facing rivals headquartered in the same state, companies headquartered in states where noncompete agreements are not enforced have more-forthcoming disclosure policies than companies headquartered in states where noncompete agreements are enforced. This is evidenced by more management forecasts, more discretionary disclosures in general, a larger analyst following and higher AIMR rankings. These findings are consistent with an increase in the proprietary costs of disclosure resulting from enforcement of noncompete agreements, because rivals know less about each other to begin with due to reduced information leakage from employee transfers across competitors. The results suggest that the overall environment for information spillover surrounding the firm impacts the degree of disclosure to the capital markets and that state-specific enforcement of noncompete agreements can be used as a novel measure of the proprietary costs of disclosure.

1. Introduction

Theoretical research in accounting has argued that the presence of proprietary costs can act as a tension that limits voluntary disclosure of information by firms (see, for example, Verrecchia (2001) and Dye (2001) for survey papers covering the topic). This tension has been confirmed in a survey by Graham et al. (2005) who report that most companies are reluctant to disclose too much information to the capital markets to prevent giving away "company secrets." However, prior empirical literature has mostly focused on relatively unclear proxies for proprietary costs, based on the level of product-market competition, with mixed results. As noted by Beyer et al. (2010), "a major challenge still remains in measuring and quantifying proprietary costs."

I introduce in this paper a novel proxy for proprietary costs by assessing the firm's and its rivals' information environment beyond the information directly available to the capital markets. This proxy is based on the potential for information transfers from one rival firm to another originating from employee mobility between the two firms. Specifically, suppose that two rival firms are already very knowledgeable about each other's current operations and future plans. Then these rivals should have fewer concerns disclosing information to the markets because they already know the information disclosed. A major mechanism that drives how much rivals know about each other is employee mobility (e.g., Almeida and Kogut, 1999; Breschi and Lissoni, 2009; Corredoira and Rosenkopf, 2010). Indeed, a firm that hires its rival's employees can have increased knowledge about its rival's current operations, financial position and future plans.

A major legal tool firms use to prevent their employees from switching sides is to restrict their mobility through the use of noncompete agreements (e.g. Gilson, 1999), a mechanism widely used in practice for wide categories of employees (e.g. Marx, 2011; Kaplan and Stromberg, 2003; Bishara, Martin and Thomas, 2013). For example, in a survey conducted in

2007, the Society for Human Resource Management found that 56% of responding organizations require employees to sign noncompete.[1] Prior research has shown that noncompete agreements are highly effective to prevent employees from switching to a rival firm (e.g, Marx, 2011), as long as the employees are located in a state that enforces them. Indeed, enforcement of non-compete agreements is state specific and mainly depends on the state where the employee works (e.g. Marx, Strumsky and Fleming, 2012). A higher state-specific enforcement level of noncompete agreements has been shown to have a significant negative impact on employee mobility across rivals and information transfer in general (Marx, Strumsky and Fleming, 2009, Garmaise, 2011, Belenzon and Schankerman, Forthcoming). A well-known example involves the success of the Silicon Valley in California compared to the Route 128 corridor in Boston, attributed to the lack of enforcement of noncompete in California (Gilson, 1999). According to Hyde (2011), "California had developed a culture of start-ups, with rapid mobility among firms and thus substantial flows of information across firm boundaries […] Employees talked to their friends and former (and future) colleagues at competing firms." Given this environment, a Silicon Valley firm should have fewer concerns disclosing more information to the markets than a Route 128 firm, because this information is often already known by the firm's rivals. Consequently, I argue that two rival firms headquartered in a state not enforcing noncompete are disclosing more than two rivals headquartered in a state enforcing noncompete, because they know more about each other.

To test this hypothesis, I use a variable that measures the variation in states' levels of enforcement of noncompete. This variable, initially developed by Garmaise (2011), is based on a detailed analysis of individual states' regulations on noncompete agreements, and has been

[1] Survey results are available at http://www.shrm.org/Pages/default.aspx.

gaining traction in both industrial organization and finance literature (e.g. Samila and Sorenson, 2011; Acharya, Baghai and Subramanian, 2013; Belenzon and Schankerman, Forthcoming). Given the absence of a widely accepted measure of disclosure that also makes a distinction between mandatory and voluntary disclosure (Beyer et al., 2010), I use four distinct measures of disclosure: The propensity to issue a management forecast, a new and more comprehensive measure of voluntary disclosure activities based on firms' Key Developments headlines downloaded from Capital IQ, the number of analysts covering the firm, and the AIMR rankings.[2]

In my empirical tests, I specifically focus on firms that have rivals headquartered in the same state, because the potential for employee transfers is higher for firms that are closer to each other, and a within-state focus limits the risk of overlapping or conflicting state jurisdictions.[3] Controlling for other state-related factors that could impact disclosure, I find that these firms, when headquartered in non-enforcing states, have more-forthcoming disclosure policies than firms headquartered in enforcing states. This result is consistent with the joint prediction that proprietary costs are reduced when companies are headquartered in states where noncompete are not enforced, and that companies increase disclosure when proprietary costs are reduced. My results are consistent using all measures of disclosure in the cross-section of states enforcing noncompete agreements. When identification is feasible, I also find consistent results in times-series specifications when states changed their enforcement policies.[4]

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[2] One of the main advantages of the variable built from the Key Developments in Capital IQ is that it alleviates many of the concerns detailed in Beyer et al. (2010) related to existing measures of voluntary disclosure. In particular, the variable only captures voluntary disclosure decisions of firms for a wide range of disclosures and is reasonably easy to replicate.

[3] It is unclear which jurisdiction applies when employees transfer from one state to another. In particular, some states have voided noncompete agreements issued in other states in the past (Kahn, 1999). The scope of noncompete agreements is also sometimes restricted to the state within which they are issued.

[4] One may wonder whether firms choose their headquarters location based on noncompete agreements. As mentioned by Garmaise (2011), the business-location literature has mostly emphasized other factors unrelated to noncompete, making it unlikely that enforcement of noncompete agreements is a first-order effect in headquarter-

In further tests, I find, for firms headquartered in states not enforcing noncompete and facing rivals in the state, that the level of disclosure increases when employee mobility is higher. This result confirms that employee mobility has an impact on firms' disclosure decisions. I also find that the level of disclosure increases when local rivals represent a larger fraction of the industry. This result confirms that the presence of other rivals not headquartered in the same state exercises a negative pressure on disclosure, because nonlocal rivals are not as informed as local ones and may as a result learn from disclosure to the capital markets. I also find that the level of disclosure is higher when rivals are headquartered closer to each other within the state. This result is consistent with a reduction in employee mobility when firms are more distant from each other. In general, the results on distance and presence of rivals outside of the state are consistent with Jaffe et al. (1993) and Marx and Singh (Forthcoming), who find that the amount of information spillover decreases with geographic distance, mostly because of more-limited employee transfers (Breschi and Lissoni, 2009). A final result suggests that the presence of rivals headquartered in the same state increases the level of product-market competition. I find that firms headquartered in states enforcing noncompete consider the level of competition to be higher and disclose less when rivals are headquartered in the same state, which is consistent with the presence of local rivals representing an additional proxy for the level of product-market competition beyond what has been used in prior literature.

Overall, this study contributes to the literature by introducing a novel proxy for the proprietary costs of disclosure, the degree of enforcement of noncompete agreements, which alleviates several endogeneity and measurability issues associated with product-market competition proxies used in prior literature (see Beyer et al., 2010). My results are consistent

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location choice. In addition, I specifically control for this potential self-selection concern by including state-year fixed effects in my cross-sectional specifications.

with proprietary costs acting as a tension going against companies' disclosure of information to the capital markets and confirm the survey evidence from Graham et al. (2005). They also suggest that the overall information environment beyond the one directly available to capital markets matters for firms' disclosure decisions. This study also suggests that cross-sectional variations in state laws and the geography of firms' locations in general matter for firms' disclosure decisions. Finally, this study suggests that fluidity in the labor market has an impact on firms' disclosure decisions.

The remainder of this paper is structured as follows. Section 2 introduces the main hypotheses. Section 3 introduces the research design as well as the data. Section 4 provides the main empirical results and Section 5 additional cross-sectional tests. Section 6 concludes.

2. Hypothesis development

The theoretical voluntary disclosure literature argues that proprietary costs act as a tension that goes against disclosure of information in the context of competition from existing rivals.[5] Verrecchia (1983), using a constant cost of disclosure, shows that firms do not disclose below a certain threshold. This threshold increases when the cost increases, indicating that disclosure is reduced when proprietary costs increase.[6] Einhorn (2007) confirms Verrecchia's findings on average when the market is uncertain about the manager's reporting objectives, and Einhorn and Ziv (2008) show that the effect is enhanced within a multiperiod game setting. These theoretical predictions have been confirmed in a survey by Graham et al. (2005), who report that 58% of

[5] As noted by Li (2010), these results are usually in contrast with another theoretical literature that focuses on disclosure in the context of an entrant and an incumbent. Several papers show that proprietary costs can lead to an outcome equivalent to full disclosure (Darrough and Stoughton, 1990; Wagenhofer, 1990). In this paper, my focus is on within-industry competition and not on the threat of entry. My empirical tests also control for the potential threat of entry at the state level.

[6] Clinch and Verrecchia (1997), in the context of a Cournot duopoly, endogenize the proprietary costs of disclosure and also find that the disclosure interval is reduced when the intensity of competition increases. However, in contrast to Verrecchia (1983), they find that the nondisclosure region applies for very high and very low demand.

corporate executives agree that "limiting voluntary communication of financial information helps avoid giving away company secrets or otherwise harming [their] competitive position."

In contrast, the empirical literature on proprietary costs of disclosure has had mixed results, as noted in a recent review by Beyer et al. (2010). In particular, prior literature has commonly used the level of product-market competition as a proxy for proprietary costs of disclosure (e.g., Li, 2010). However, this approach can be problematic as there is no clearly established measure to proxy for the level of product-market competition (e.g., Li, Lundholm and Minnis, 2012), and competitive intensity is also endogenously determined (Sutton, 1991).

To alleviate these concerns, I use enforcement of noncompete agreements at the state level to proxy for the proprietary costs of disclosure. The level of enforcement of noncompete agreements can have an impact on the level of proprietary costs of disclosure, because enforced noncompete agreements significantly restrict employee mobility from one rival to another. Companies make extensive use of noncompete agreements for their executives (Garmaise, 2011; Kaplan and Stromberg, 2003) and other categories of employees (Leonard, 2001; LaVan, 2000; Marx, 2011). For example, Garmaise (2011) reports that, for S&P 500 firms, 70% of firms have noncompete agreements with their top executives, a result confirmed by Bichara et al. (2013). Marx (2011) finds in a survey of 1,029 engineers across a variety of industries that 47% were asked to sign a noncompete agreement. However, the degree of enforcement of noncompete agreements is regulated by the law of the state where the employee works, with wide fluctuations in enforcement levels. For example, California does not enforce noncompete agreements, while other states, such as Massachusetts, have stronger enforcement policies. Prior literature has found that enforcement of noncompete does indeed largely reduce employee mobility across rivals. In particular, enforcement of noncompete agreements reduces within-industry transfers of

executives by almost 50% (Garmaise 2011). Using patent data, Marx et al. (2009) find that increased enforcement of noncompete agreements for the state of Michigan reduced inventor mobility, especially for inventors specializing in narrow fields. Marx (2011) also finds that technical workers subject to noncompete are 88% likely to switch industries when changing jobs, compared with 28% of workers not subject to noncompete, indicating that noncompetes are highly effective in practice to prevent employees from switching sides.

The evidence above implies that the level of information spillovers across rivals is negatively impacted by the enforcement of noncompete agreements, as confirmed in a recent paper by Belenzon and Schankerman (Forthcoming) and anecdotal evidence about the successful development of the Silicon Valley compared to the Route 128. In addition, several studies provide empirical evidence consistent with employee mobility being a conduit for knowledge spillover (Almeida and Kogut, 1999; Song, Almeida and Wu, 2003), an observation widely confirmed in the daily press. Some notable examples include Mattel's litigation with MGA regarding ownership of the Bratz dolls (*WSJ*, Jan 12, 2011) - the inventor of the dolls developed the concept while working at Mattel before moving to MGA - and Intel corp - Fairchild semiconductor management left the firm with proprietary information about the microprocessor in order to found Intel corp (Rajan and Zingales, 2001). The information transmitted goes beyond pure technical information and can include operational information, future strategic plans, as well as a detailed understanding of a rival's profit areas. For example, prior research documents that firms hiring managers from their rivals have subsequently entered some of the product markets of their rivals (Boeker, 1997; Rao and Drazin, 2002). Note that the transfer of information can also occur in the direction of new employer to former employer and is persistent over time in general because of social ties between the moving employee and her former

colleagues (Agrawal et al., 2006; Corredoira and Rosenkopf 2010). This is also evidenced by extensive interactions between former colleagues at competing firms in the Silicon Valley (Saxenian, 1994; Gilson, 1999; Hyde, 2011). Consequently, firms headquartered in states where noncompete agreements are enforced are less likely to be knowledgeable about their rivals through employee turnover than firms headquartered in states where noncompete agreements are not enforced.[7] This makes the case for increased proprietary costs of disclosure, keeping the level of disclosure constant, because competitors know less about each other.

One caveat, though, is that the probability of employee turnover is higher when two firms are headquartered in close proximity to each other. In such a case, it would be easier for an employee to leave her employer on Friday and start working at the rival on Monday. In addition, it is unclear whether the scope and enforceability of noncompete agreements extend beyond the state where they are issued, given that the jurisdiction is state specific.[8] Consequently, noncompete agreements are most likely to have an impact when rivals are headquartered in the same state. The discussion above also assumes that some of the knowledge transmitted by employees is a substitute for the information disclosed to the capital markets. In particular, typical information disclosed to the capital markets could, in some instances, act as a complement to the information transmitted by former employees. In which case firms located in states not enforcing noncompetes would be disclosing less. I test the following hypothesis:

[7] Firms could have additional locations in states where the level of enforcement of noncompete agreements is different compared with the headquarters. This is unlikely to be an issue in my study as at worst this would bias the results towards the null. In addition, it can be argued that the type of information that would be disclosed by the firm to the capital markets is more likely to be available at the headquarters than at other locations. In general, use of the headquarters to proxy for the location of the firm has been extensively used in the literature (e.g. Hilary and Hui, 2009; Ivkovic and Weisbenner, 2005; Agrawal and Matsa, 2013).

[8] Firms losing employees to their rivals are also more likely to initiate defensive responses when rivals are located far away from each other (see Gardner, 2005).

H1: Rival firms headquartered in a state not enforcing noncompete agreements have more-forthcoming disclosure policies than rival firms headquartered in a state enforcing noncompete agreements.

Conditional on rivals being headquartered in a state where noncompete are not enforced, the proprietary costs of disclosure should decrease when employee mobility increases in general, leading to increased disclosure to the capital markets. I test the following hypothesis:

H2a: Disclosure by rivals headquartered in a state where noncompete agreements are not enforced is more forthcoming when employee mobility is higher.

Conditional on rivals being headquartered in a state where noncompete are not enforced, employee mobility is also likely to be higher when two rivals are located close to each other, as argued in the economic geography literature (e.g., Boschma, Eriksson and Lindgren, 2008; Gardner, 2005). This in turns leads to reduced information spillover when distance between rivals increase (Breschi and Lissoni, 2009), implying that the proprietary costs of disclosure should increase with the distance from one rival to another. I test the following hypothesis:

H2b: Disclosure by rivals headquartered in a state where noncompete are not enforced is more forthcoming when those rivals are located closer to each other.

Last, conditional on a firm and some of its rivals being headquartered in a state where noncompete are not enforced, these rivals may know each other's proprietary information. However, out-of-state rivals are unlikely to benefit from the same level of employee transfers and consequently may learn some information from disclosure to the capital markets. This implies that the proprietary costs of disclosure should increase when a firm has more rivals headquartered outside of the state. I test the following hypothesis:

H2c: Disclosure by rivals headquartered in a state where noncompete agreements are not enforced is less forthcoming when the proportion of rivals outside of the state increases.

3. Research design and data

3.1 Research design

I use a research design comparable to Garmaise (2011). The analysis assesses whether rival firms headquartered in a state not enforcing noncompete agreements disclose more than rival firms headquartered in a state enforcing noncompete agreements (H1).

Several challenges occur from an identification standpoint. First, it is unclear which state jurisdiction will be applied when two rivals are headquartered in different states. For example, the state of California has voided prior noncompete agreements signed in other states in the past (Kahn, 1999). The extent of potential employee transfers from one firm to another is also likely to be diminished when firms are headquartered in different states. This implies that enforcement of noncompete agreements can only be properly identified in the context of two rivals headquartered in the same state, as the laws of the same state apply and the potential for employee transfers from one firm to another is relatively high.[9] Second, other state-specific conditions, not related to the level of enforcement of noncompete agreements, could impact the level of disclosure by firms. For example, Armstrong et al. (2012) find that state antitakeover laws matter for corporate governance, suggesting that antitakeover laws, different across states, could also be influencing firms' disclosure decisions. This implies that it would be difficult to interpret results from a regression of a measure of disclosure on an indicator variable equal to one when noncompete agreements are not enforced (*Nonenforcing state*), because results could

[9] Another advantage of this research design is that it alleviates potential concerns related to the choice of law. The usual choice of law is the state that has the materially greater interest (McDonald 2003). In my research design, this state is the state where the two competitors are located and where the employee works.

be driven by the absence of rivals within the state, the location of rivals in other jurisdictions, or other state-specific conditions not related to the enforcement of noncompete agreements.

In my specifications, I focus on firms that have rivals headquartered in the same state (*In-State Competition* indicator variable equal to one) and assess the difference in disclosure decisions when firms face rivals headquartered in the same state and are headquartered in states enforcing or not enforcing noncompete agreements, controlling for the normal level of disclosure within the state. I estimate the following regression:

$$\begin{aligned} &Disclosure_{i,t} = \alpha + \beta_1.In\text{-}State\ Competition_{i,t} + \\ &\beta_2.In\text{-}State\ Competition_{i,t} \times Nonenforcing\ State_{i,t} \\ &+\beta.X_{i,t} + State\ Year\ Fixed\ Effects \end{aligned} \qquad (1)$$

Disclosure is a measure of disclosure, defined in more detail in section 3.2.

In-State Competition is an indicator variable equal to one when there is at least one rival headquartered in the same state. The level of product-market competition is chosen at the six-digit NAICS level, which is the most granular level available.[10]

Nonenforcing State is based on the analysis of Garmaise (2011), who classifies the level of enforcement in each state on a scale from zero (California, North Dakota), no enforcement, to nine (Florida), extremely strong enforcement. The variable was explicitly built with corporate finance applications in mind and is based on an extensive analysis of each state's policies towards enforcement of noncompete agreements, as detailed in Malsberger (2004), the central reference describing individual states' noncompete regulations. In particular, Garmaise considers

[10] The competition variable is assessed using the primary historical NAICS code of the firm taken from Compustat. The NAICS classification has several advantages over other classifications. First, it is more updated compared with the SIC classification. In addition, the classification spans a longer period of time in Compustat compared with other classifications including GIC codes or the Hoberg and Phillips (2010) classification. Last, the classification is more granular than the other ones, which allows to determine more directly true product-market rivals.

12 questions that are important to gauge the level of enforcement of noncompete agreements and creates a score that goes from 0 to 12, based on the answers to these questions (see the Appendix for additional details on the variable). In my analysis, I set *Nonenforcing State* to zero for any state with an enforcement level of four or more using the Garmaise scale, and *Nonenforcing State* to one for any state with an enforcement level of three or less using the Garmaise scale.[11] Doing so splits the sample of firm years into two comparable subsamples, with 44% of the firm years being headquartered in states where noncompete agreements are not enforced.[12]

$X_{i,t}$ is a vector of control variables, and includes two broad categories. [13] The first one is comprised of company specific variables. Prior studies (see Lang and Lundholm, 1996) have found that these variables influence some measures of disclosure:

Logmkvalt, the natural logarithm of market capitalization of the firm at the beginning of the fiscal year. I take the logarithm in order to reduce the skew in the variable.

Stdroe, the standard deviation of the return on equity, estimated over a ten year period.

Correlret, the correlation between annual returns and company earnings per share, estimated over a ten year period.

[11] The tenor of the results is unchanged when replacing the *Nonenforcing State* dummy with the actual value of the variable taken from Garmaise (2011).

[12] Note that the company headquarters variable comes from Compustat. A potential issue with this variable is that it represents a snapshot of the current headquarters location. Firms that change headquarters during the period could have their headquarters location misrepresented. This is the case, for example, with Boeing, which moved its headquarters from Seattle to Chicago. However, as noted by Garmaise (2011) and Hilary and Hui (2009), the issue is relatively small and at worst, the inclusion of all firms just introduces additional noise in my specifications that go against me finding the results. I still obtain historical headquarter information from S&P on a somewhat smaller sample than the one presented and find qualitatively similar results to the ones presented in the tables when the sample reduction is not too large.

[13] All continuous disclosure and control variables are Winsorized at the 1st and 99th percentiles in order to reduce the impact of outliers in the specifications.

I also include several measures of product-market competition defined in Karuna (2007) and Li (2010) in order to control for the within-industry level of product-market competition in some of the specifications. These variables are calculated using the primary NAICS code of the firm:

CCR3 is the industry concentration index, calculated at the six-digit NAICS code. *CCR3* is the market share of the three largest competitors within a given industry.

Industry Sale is the logarithm of the six-digit NAICS code industry sales. I take the logarithm in order to reduce the skew in the variable.

Logentrycost is equal to the logarithm of the weighted average of the net property, plant and equipment (PP&E) in the industry, weighted by sales, and is a measure of barriers to entry in the industry. I take the logarithm in order to reduce the skew in the variable.

Indpricecostmargin is equal to industry aggregate sales divided by industry aggregate operating costs and is a measure of industry differentiation.

Last, similarly to Garmaise (2011), I include state-year fixed effects. Inclusion of these fixed effects provides a baseline for disclosure for each state, based on the level for firms that do not face rivals headquartered in the same state. The aim is to control for state-specific variables that could have an impact on companies' disclosure decisions but that would not necessarily be related to enforcement levels of noncompete agreements, thus reducing the concern of omitted variables. This control also takes care of potential self-selection concerns of firms into jurisdictions enforcing or not enforcing noncompete agreements as well as of disclosure due to potential entry threats, if these threats are different across states. Consequently, the coefficient on

Nonenforcing State is not identified as it is subsumed by the state year variables.[14] The coefficient on *In-State Competition*, β_1, represents the difference in disclosure for firms facing rivals headquartered in the same state when states enforce noncompete agreements. The direction of this coefficient is unclear overall, as it is uncertain whether the presence of local rivals increases the level of product-market competition. However, if this tends to be the case, then the coefficient is predicted to be negative. The coefficient on the interaction *Nonenforcing State* × *In-State Competition*, β_2, is predicted to be positive if rivals headquartered in the same state know more about each other when headquartered in a state not enforcing noncompete agreements, in contrast to rivals headquartered in a state enforcing noncompete agreements.

3.2 Measures of disclosure

Management Forecasts

First, I use a direct measure of voluntary disclosure, the propensity of firms to disclose a management forecast. An advantage of this measure is that it fits with multiperiod models of voluntary disclosure that incorporate a cost of disclosure (e.g. Einhorn and Ziv, 2008). *Mgmt Forecast* is an indicator variable equal to one when a firm issued at least one management forecast during the year. The data is taken from the IBES management guidance database, which is widely populated beginning in 2003.[15] Consequently, my analysis using this variable covers fiscal years 2003-2011. Given that my focus is on disclosure and not on pre-announcement of earnings, I only keep the guidance that is released before or during the corresponding fiscal

[14] Results are qualitatively unchanged for all specifications but the AIMR rankings when directly regressing the disclosure variable on *Nonenforcing State* and restricting the sample to the firms of interest where rivals are headquartered in the same state. However, results become insignificant when using the AIMR rankings.

[15] Because Thomson discontinued First Call in 2012, I was unable to obtain management forecast data based on this database. Thomson replaced First Call with a separate management forecast feed from IBES. However, the data is only widely populated from 2003 and may suffer from the same issues described in Chuk et al. (2012).

period and exclude the observations released after the close of the fiscal period and prior to the earnings announcement date.

I also use three additional proxies of disclosure of management forecasts as validity checks. The first one, *Mgmt Capex*, is an indicator variable equal to one when a firm issued at least one forecast of capital expenditure during the year. The second, *Lognd*, is equal to the natural logarithm of one plus the number of disclosure events occurring during the year. For example, if the firm released forecasts on two different dates during the year, the variable would equal log(1+2). The third, *Logdisclosures*, is equal to the natural logarithm of one plus the total number of items disclosed during the year, as measured by the number of forecasting lines present in the IBES management forecast database. This measure takes into account the fact that some companies may provide guidance for a wide range of items besides earnings per share, thus providing more granular disclosures as a result.

Capital IQ Key Developments

I also build another direct measure of voluntary disclosures based on the Capital IQ Key Developments. Capital IQ maintains a team of analysts that monitor more than 100 key development types for a wide range of corporations. Some of these developments represent pure voluntary disclosures of the firm, such as management guidance or company conferences and calls, while other ones represent mandatory disclosures, such as announcement of executive and auditor changes. I download from the web interface of Capital IQ the Key Developments from 2007 to 2011 for all public corporations in the U.S. I then classify disclosure types between mandatory and voluntary disclosures and focus specifically on voluntary disclosures. I then create a variable, *Voluntary*, equal to the total number of voluntary disclosures during the fiscal year. I also consider the components of *Voluntary*. *Guidance* is the total number of guidance

events during the fiscal year, *Conf* is the total number of company conferences or conference calls during the year, *Prodcust* is the total number of product or client related announcements, and *OtherVol* represents the remaining voluntary disclosures.

Analyst Coverage

I also use the number of analysts as a proxy for disclosure. In particular, the results in Lang and Lundholm (1996) suggest that analyst coverage can be used as a proxy for the level of disclosure by firms, as this number is strongly correlated with the AIMR rankings, another measure of disclosure I use in this study. I use *Numest*, the number of analysts providing a yearly forecast, as a proxy for disclosure. This variable is calculated as in Lang and Lunhdholm (1996), as the average of analysts following the firm over the twelve periods where a median estimate of the forecast is provided by IBES. The purpose is to measure overall company disclosure over time and not at a specific point during the year.

The advantage of using this proxy is that the dataset spans a longer time period, 1985 to 2011, in comparison with the management forecast data, which becomes available only from 2003. This allows testing for additional time-series specifications that incorporate changes at the state level in enforcement of noncompete agreements.

AIMR Disclosure Data

My last measure of disclosure is from the AIMR reports, a measure widely used in prior studies (e.g., Lang and Lundholm, 1996). Leading financial analysts evaluated the informativeness of companies' disclosure policies along several axes, including annual published information, other published information, and investor relations. The results are summarized in a single score, which essentially represents the financial analysts' assessment of the overall company disclosure policies. Depending on the firm, individual subscores at the level of annual

information, other published information and investor relations are also available. Consistently with prior literature (Brown and Hillegeist, 2007; Bushee and Noe, 2000; Botosan and Plumlee, 2002), I take the percentile rank of the AIMR scores within each covered industry, as the scale and criteria used by the analyst panels for each industry might have been different. A higher rank, with a range from 0 to 100%, indicates that disclosures by the firm were more informative within the same AIMR panel industry. I also take the rank of the continuous control variables in the specifications where the dependent variables are the AIMR rankings. The rank is calculated at each AIMR panel industry level for the sake of consistency with the AIMR rankings.

The advantage of the AIMR rankings is that they represent a comprehensive measure of disclosure activities of the firms. However, the AIMR rankings were discontinued in 1996, making any result potentially dated.

3.3 Data construction

The primary data comes from Compustat. Given that I focus on U.S. states' laws, I eliminate firms not headquartered in the US. I also eliminate subsidiaries, as their inclusion might create a bias in the calculation of the *In-State Competition* variable and in the final results. I keep firms where the historical NAICS code is properly defined at the six-digit level. I then match the Compustat sample with IBES and the IBES management forecasts database in order to compute proxies for company-disclosure policies. Last, I restrict the sample to firm years for which the data to calculate control variables is available. These restrictions generate a primary sample of 126,727 firm years, spanning between 1985 and 2011, for the analysts' sample, and a subsample of 37,297 firm years, spanning between 2003 and 2011, for the management forecasts' sample.

I also match the primary sample with my sample of disclosures obtained from Capital IQ, available from 2007, using the CIK code. To reduce any coverage bias from Capital IQ, I only

keep firms where Capital IQ measures more than 10 mandatory disclosures during the fiscal year. Incorporating this threshold ensures that Capital IQ is actively monitoring the key developments of the firm. These restrictions yield a final sample of 10,446 firm-years.

Finally, I match the 1985-2011 sample with the AIMR data, available between 1985 and 1995. I only keep industries, defined at the AIMR analysts panel level, where at least 15 firms were rated within a specific year. The purpose is to prevent jumps in the AIMR disclosure rankings due to the inclusion or removal of firms assessed from year to year.[16] The final sample, including all control variables and the AIMR data, is reduced to 1,465 firm years.

3.4 Descriptive statistics

Descriptive statistics are presented in Table 1, Panel A. The sample size varies across the different measures of disclosure because not all measures are available for the entire sample.

Thirty-nine percent of firms disclose management forecasts in the sample spanning fiscal years 2003-2011. This result is in line with the results reported in Chuk et al. (2012) for their hand-collected sample, perhaps because the IBES management forecasts database might suffer from fewer biases than the First Call database used in Chuk et al. (2012).[17]

The mean of the *In-State Competition* variable is 0.67, indicating that many firms in the sample are headquartered in the same state as rivals. The mean of the *Nonenforcing State* variable is 0.44. This confirms that the sample is roughly split in half between enforcing and

[16] For example, in an extreme case, a firm could see its score go down from 100% to 0% year over year if in the first year it is the only firm ranked and there are two firms in the second year with the newly rated firm having a slightly higher AIMR score. Untabulated tests confirm that the adjusted R-squares of the regressions increase significantly when adding size restrictions on the sample. Main results on the overall AIMR score remain mostly unchanged when using different size restrictions between 10 and 20 firms within each industry.

[17] Untabulated analyses that split the sample across quartiles based on firm market capitalization, similar to Chuk et al. (2012), still suggest that the management forecasts coverage is biased downwards for firms with lower market capitalization. Consequently, I control for market capitalization in the main specifications and also include the number of analysts covering the firm as a control variable in untabulated tests. The results are qualitatively similar.

non-enforcing states. The values of the control variables are in line with Lang and Lundholm (1996) and Karuna (2007).

(Insert Table 1 About Here)

3.5 Does the presence of a rival in the same state imply more product-market competition?

I test whether the presence of rivals headquartered in the same state (*In-State Competition* variable equal to one) change firms' perceived level of product-market competition in Table 1, Panel B. Following Li et al. (2012), I use as the dependent variable the number of times the word "competition" is included in firms' 10-Ks, deflated by the total number of words in the 10-K.[18] Li et al. (2012) find that this measure is related to the perceived competitive environment of the firm and provides additional information compared with traditional product-market competition proxies. Another advantage of this measure, important for this specific test, is that the measure is available at the firm level. I find in Column (1) that firms that have rivals headquartered in the same state mention the word competition more often, as evidenced by a positive coefficient on the *In-State Competition* variable, consistent with the presence of a rival increasing perceived product-market competition. The results are not significantly different depending on whether the rivals are headquartered in states where noncompete agreements are not enforced, as evidenced by an insignificant coefficient on *Nonenforcing State* × *In-State Competition* in Column (2). This insignificant result provides some confidence that *In-State Competition* and its interaction with *Nonenforcing State* can be interpreted similarly for both enforcing and nonenforcing states in further analyses. Overall, the results suggest that disclosure should be lower in states where noncompete agreements are enforced for firms that have rivals headquartered in the same state.

[18] The data is available at http://webuser.bus.umich.edu/feng/.

4. Main empirical results

4.1 Management forecasts

Table 2 presents correlations for the sample restricted to fiscal years 2003-2011, for which the data on management forecasts is available. The correlation between the issuance of a management forecast and the number of analysts is high, above 0.5, significant at 1%. This result partially confirms that the use of the number of analysts following a firm represents a reasonable proxy for disclosure, but also needs to be interpreted with some caution. In particular, it is possible that, similarly to the First Call guidance database (Chuk et al., 2012), the IBES Management Forecasts database could have a biased coverage towards firms being covered by analysts. This could potentially explain the relatively high correlation between management forecasts and number of analysts.

(Insert Table 2 About Here)

Table 3 uses the measures of disclosure of management forecasts as the dependent variables.[19] I find in the first three columns that the coefficients on *In-State Competition* are significantly negative, indicating that companies disclose less when a rival is headquartered in the same state. I also find in all columns that the interaction *Nonenforcing State* × *In-State Competition* loads positively, which is consistent with firms headquartered in states where noncompete are not enforced disclosing more than firms where noncompete are enforced (H1). The coefficients on the industry control variables are usually consistent with Li (2010), but the signs of *Industry Sale* and *LogentryCost* change in the *MgmtCapex* specification.

(Insert Table 3 About Here)

[19] Due to the large number of fixed effects included in the specifications, the specifications are estimated using OLS. I confirm that the results are qualitatively unchanged when using a logistic specification instead.

In terms of economic significance, based on the results in Column (1), the presence of rivals in a state enforcing noncompete reduces the propensity of the issuance of a management forecast by 2%, while that propensity increases by 5% in states where noncompete are not enforced. This needs to be compared with an average propensity of 39% within the sample.

4.2 Capital IQ Disclosures

Descriptive statistics on the Capital IQ voluntary disclosures are shown in Table 4 Panel A. The average firm has 23.57 voluntary disclosure events during the year, comprised of 2.46 guidance events, 9.37 conference and calls, 5.73 product or customer related announcements, and 5.57 other voluntary disclosure events. These numbers reflect a wide disparity across the firms. For example, more than 25% of the firm-years in the sample issue no guidance and have no announcement related to products or customers. To control for the small skew in the variable, I replace the raw count of disclosure with the natural logarithm of one plus the count.

(Insert Table 4 About Here)

Panel B shows the evolution of the voluntary disclosures over time. Both mean and median numbers have increased over time since 2007, either because firms are disclosing more or because Capital IQ monitoring of disclosure events increased. My empirical specifications control for any potential trend effect.

Main empirical results are presented in Table 5. I find a positive coefficient on the interaction *Non enforcing state* × *In State Competition* when using the total number of disclosures as the dependent variable. This result is consistent with the results in Table 3.

(Insert Table 5 About Here)

These initial results are confirmed when using the individual components of the total number of disclosures as the dependent variable. I find that firms headquartered in states that do not enforce noncompete, when facing rivalry in the state, have more guidance events, more conference and calls, more announcements related to products or customers, and have more other voluntary disclosures. This result is consistent with H1. The coefficient on *In State Competition* is also negative in some of the specifications, consistent with the results in Table 3.

4.3 Number of analysts

Next, I use the number of analysts covering the firm, *Numest*, as a proxy for the companies' disclosure of information. As indicated by the results in Table 2, the correlation between this variable and the Management Forecast is high and above 0.5, providing some assurance that analyst following represents a reasonable proxy for disclosure. Untabulated results also indicate that the correlations between *Numest* and the Capital IQ disclosures and AIMR rankings are high, between 0.3 and 0.5, and significant at the 1% level.

In Table 6, I conduct similar regressions to those in Tables 3 and 5, restricting first the analysis to the subsample where management forecasts are available, in Column (1). The purpose is to validate the use of *Numest* by finding results consistent with Tables 3 and 5 using the same sample. I find that the interaction *Nonenforcing State* × *In-State Competition* is positive, which is consistent with H1. The coefficient on *In-State Competition* loads negatively but insignificantly. Overall, these results confirm that using *Numest* instead of the other variables does not materially change the tenor of the results.

(Insert Table 6 About Here)

In Column (2), I extend the sample to the 1985-2011 period. I again find similar results with a negative coefficient on *In-State Competition* and a positive coefficient on *Nonenforcing State* × *In-State Competition*. Note that *Logentrycost* loads positively. This result is inconsistent with Li (2010) and suggests caution in the use of "traditional" product-market competition proxies to proxy for proprietary costs, consistent with Beyer et al. (2010).

4.4 AIMR disclosure results

Table 7 presents the main results on the AIMR Disclosure variable. All continuous variables in this specification are ranked within each industry defined by the AIMR, which is consistent with prior research (see, for example, Bushee and Noe, 2000). My results mirror the results with management-forecast data in Table 3. I find in the first column, when using the total AIMR score, that the coefficient on *In-State Competition* is significantly negative, indicating that companies disclose less when a rival is headquartered in the same state. I also find that the interaction *Nonenforcing State* × *In-State Competition* loads positively, consistently with firms headquartered in states where noncompete are not enforced disclosing more than those in states where noncompete are enforced (H1). I also assess the impact of noncompete enforcement on the components of the AIMR variable in the other columns. Results are weaker, due to a large reduction in sample size due to unavailability of the AIMR component variables for all firms, yet remain in line with the results in the first column.

(Insert Table 7 About Here)

In terms of economic significance, based on the results in Column (1), the presence of rivals in states where noncompete are enforced reduces the rank of disclosure by 11%, while the rank is increased by 14% in states where noncompete are not enforced.

5. Additional results

5.1 Employee mobility, out-of-state competition and distance between rivals

I conduct several additional cross-sectional analyses in this section. The primary idea driving the first analysis is that the presence of rivals headquartered outside of a state where a firm is headquartered should have an influence on the level of disclosure of this firm. In particular, if a firm facing within-state competition and headquartered in a state not enforcing noncompete agreements faces many rivals headquartered in different states, then the proprietary costs of disclosure should be increased for this firm in contrast to another firm that faces most of its rivals within the same state, because the non-local rivals may not know as much about the firm's activities as do local rivals. Similarly, when two rival firms are headquartered in a state not enforcing noncompete agreements, the level of employee transfer is likely to be dependent on the geographic distance between the two firms. In particular, it would be relatively easy for employees to transfer from one firm to another if they were headquartered within a few miles from each other and more difficult if a change of employment would require the geographic relocation of employees, even if they could remain within the same state. Finally, the level of disclosure should increase with employee mobility overall. Consequently, I predict that a larger presence of rivals headquartered outside the state, a greater distance between rivals headquartered within the state, and lower employee mobility within the industry in general should decrease the level of disclosure (H2c, H2b and H2a, respectively).

To test these hypotheses, I restrict the sample to firms headquartered in states not enforcing noncompete and facing within-state competition. I estimate the following regression:

$$Disclosure_{i,t} = \alpha + \beta_1.Exec\ Mobility_{i,t} + \beta_2.Logempperest_{i,t} + \beta_3.Logmeddistance_{i,t}$$
$$+\beta_4.Stateshareall_{i,t} + \beta_5.Statesharepublic_{i,t} + \beta_6.Flag\ Execucomp_{i,t} \quad (2)$$
$$+\beta_7.Flag\ Bus\ Pattern_{i,t} + \beta_8.Logestab_{i,t} + \beta.X_{i,t} + State\ Year\ Fixed\ Effects$$

Disclosure, for firm *i* at time *t*, is comprised of the same proxies used before. I add several groups of explanatory variables.

The first group of variables proxies for employee mobility within the industry. *Exec Mobility* is an indicator variable equal to one when an executive from the company has moved to a rival or an executive from a rival has moved to the company in the past. The data is taken from Execucomp and represents a direct measure of interfirm mobility. The variable is set to zero when the firm is not covered by Execucomp. To control for potential differences in terms of disclosure for the sample of firms covered by Execucomp, I also add an indicator variable, *Flag Execucomp*, equal to one when the firm is covered by Execucomp in that particular year. This indicator variable also controls for the artificial zeros introduced in the *Exec Mobility* variable due to missing data. *Logempperest* is the natural logarithm of one plus the average number of employees working per establishment in the state in a given six-digit NAICS industry within the year. The data, which includes both public and private firms, is taken from the county business patterns survey of the census bureau and represents a proxy for employee mobility if a larger employee base within each establishment in the state yields increased mobility across firms.[20] The variable is set to zero when the data is missing. I also add *Flag Bus Pattern*, an indicator variable equal to one when the county business patterns data is available, to control for potential differences in disclosure by firms when data is available. *Logmeddistance* is equal to the natural logarithm of one plus the median of the distance between firm *i* and each of its rivals

[20] The total number of employees switching from one firm to the other increases with the number of employees per establishment if the probability that an employee switches from one firm to another remains constant. The data is available at http://www.census.gov/econ/cbp/download/index.htm and is available from 1998 at the NAICS level.

headquartered within the same state. The variable is computed using the zip code of the headquarters in Compustat. Following Ivkovic and Weisbenner (2005), I obtain the latitude and longitude for each of the zip codes from the U.S. Census Bureau Gazetteer Place and compute the distance between both geographic locations.[21] I predict a positive coefficient on *Exec Mobility* and *Logempperest*, and a negative coefficient on *Logmeddistance*, if increased employee mobility has an impact on firms' disclosure decisions.

The second group of variables includes *Stateshareall*, equal to the number of establishments in a given six-digit NAICS industry present in the state minus one divided by the total number of establishments in the U.S. minus one. The data is also taken from the county business patterns data and includes both private and publicly traded firms. *Statesharepublic* is equal to the proportion of industry sales (excluding firm *i*) coming from publicly traded firms headquartered in the same state as firm *i* (excluding firm *i*). The industry is defined at the six-digit NAICS code and the variable is taken from Compustat. I predict a positive coefficient on each variable if a lower presence of rivals outside the state leads to increased disclosure.

Finally, I add the same vector of control variables as in Section 4, $X_{i,t}$, as well as state-year fixed effects in the specifications. I also add as a control *Logestab*, the natural logarithm of one plus the number of establishments in the state at the six-digit NAICS industry level.

(Insert Table 8 About Here)

Results are presented in Table 8 Panel A using the four main measures of disclosure, *Mgmt Forecast*, the propensity to disclose a management forecast, in Column (1), the number of

[21] The data is available at http://www.census.gov/geo/maps-data/data/gazetteer.html. See Ivkovic and Weisbenner (2005), p. 271, for the distance formula. Because the zip code is not available for all firms in Compustat, this slightly reduces the sample of firms facing within-state competition and headquartered in states where noncompete agreements are enforced. The sample reduction is less than 10% in most specifications.

voluntary disclosures from Capital IQ in Column (2), the number of analysts, in Column (3), and the AIMR rankings in Column (4). The analysis in Column (4) uses the rank of the continuous variables. Note that several coefficients on explanatory variables are missing for the *AIMR Ranked Score* disclosure measure, because the data is not available for these variables for the sample period where the AIMR rankings are available.

Consistently with the predictions, *Exec Mobility* and *Logempperest* load positively in two specifications out of four. These results suggest that the higher the level of employee mobility, the more forthcoming the disclosure (H2a). Note that *Flag Execucomp* loads positively in three specifications out of four, confirming the need to control for this variable in this specification. Otherwise, the coefficient on *Exec Mobility* would only capture differences in disclosure between firms in Execucomp and the other ones.

Consistently with H2b, *Logmeddistance* also loads negatively in three out of the four specifications, suggesting that distance between rivals exercises a negative pressure on disclosure due to less mobility across rivals. The lack of statistical significance when using the *AIMR Ranked Score* variable may be due to the limited sample available for this analysis.

Finally, I find that *Stateshareall* loads positively at 1% in the three specifications where the data is available, while *Statesharepublic* loads positively and significantly in two out of four specifications. These results are consistent with the presence of rivals outside of the state exercising negative pressure on disclosure (H2c).

Additional results using other proxies for management forecasts and the split of Capital IQ disclosures between guidance and non-guidance related items are presented in Table 8, Panel

B. The results are consistent with those presented in Table 8, Panel A and confirm the influence of employee mobility and presence of rivals outside of the state on firms' disclosure policies.

5.2 Time series analysis

I finally use time series changes in enforcement of noncompete agreements at the state level, using each firm as its own control in the specifications. Garmaise (2011) mentions that the Supreme Court of Texas lowered the level of enforcement of noncompete agreements in the state in 1994, while the Louisiana Supreme Court lowered the level of enforcement of noncompete agreements in 2001, with a reversal of this decision by the state legislature in 2003. In contrast, Florida increased enforcement levels in 1996, through the actions of the state legislature.[22] These changes of enforcement levels constitute quasi-exogenous shocks that could have an impact on companies' disclosure decisions. I test the following model:

$$Disclosure_{i,t} = \alpha + \beta_1.Nonenforcing\ State + \beta.X_{i,t} \quad (3)$$

$X_{i,t}$ is a vector of control variables and includes the product-market competition variables defined earlier in (1), firm fixed effects and year fixed effects. Thus, *Nonenforcing State* is identified only for firms headquartered in states that switched from enforcing to not enforcing or vice-versa. This happened in Texas and Louisiana.[23] I also control for the overall level of disclosure for each given year and for changes in product-market competition. I predict that β_1 is positive if a decrease in the level of enforcement of noncompete agreements leads to a decrease in the proprietary costs of disclosure and therefore to an increase in disclosures.

[22] Note that I cannot use the change in the enforcement of noncompete agreements in Michigan because the change took place in 1985, the first year the data is available in my sample. Consequently, I do not have any variation in the enforcement level in this state for my sample.

[23] Florida switched from a non-enforcement level of 7, already the highest in the sample at the time, to 9. Consequently, this switch is not identified in the specifications. This does not represent a major issue given that the enforcement level was already very high to begin with.

(Insert Table 9 About Here)

Results are presented in Table 9. I use the number of analysts, *Numest*, as the dependent variable in Columns (1) and (2). I find in Column (1) that the coefficient on *Nonenforcing State* loads positively and significantly at 10%, indicating that analyst coverage increased when enforcement of noncompete agreements was reduced in Texas and Louisiana. I also find weak evidence that the results are stronger for firms facing rivals headquartered in the same state in Column (2). In particular, I interact *Nonenforcing State* with *In-State Competition* and find that the interaction coefficient loads positively but insignificantly. However, the sum of *Nonenforcing State* and the interaction is significant at 5% in an F-test, whereas the coefficient on *Nonenforcing State* becomes insignificant at standard conventional levels. These results suggest that firms that face rivals within the state drive the results in Column (1). Columns (3) and (4) present results for issuance of management forecasts. The results on *Nonenforcing State* are positive but insignificant. However, there is hardly any overlap between the management forecast sample, available from 2003, and the quasi-exogenous shocks used in this analysis. In particular, untabulated analyses indicate that the coefficient on *Nonenforcing State* is identified for only 29 firms in this analysis.[24]

6. Conclusion

In this paper I present evidence consistent with the enforcement of noncompete agreements at the state level having an impact on company disclosure. In particular, firms facing in-state competition have more-forthcoming disclosures when they are headquartered in states where noncompete agreements are not enforced. Results are valid across several measures of disclosure

[24] Results are also insignificant when using other proxies for management forecasts. In addition, the overlap between the sample of firms where AIMR rankings are available and the quasi-exogenous shocks is even smaller, at 9 firms, and consequently I do not present the results of this analysis.

and are confirmed by quasi-exogenous shocks in the level of enforcement of noncompete agreements. I also find that for firms headquartered in states not enforcing noncompete agreements and facing within-state competition, a higher level of employee mobility and an increase in local rival presence positively impact disclosure, whereas an increased distance between rival firms negatively impacts disclosure.

This paper contributes to both the disclosure literature and the growing literature that takes advantage of cross-section in state laws. In particular, this paper adds another measure of variation in proprietary costs that could suffer from fewer endogeneity concerns than measures based on the level of product-market competition. This paper also confirms several concerns in Beyer et al. (2010) about the validity of disclosure measures based on product-market competition by providing conflicting evidence on the role of "traditional" product-market competition proxies in disclosure of information.

Appendix: Evolution of the Nonenforce Variable by State over Time (Garmaise, 2011)

State	Name	Nonenforce Score	Nonenforcing State	Period
AL	Alabama	5	0	All
AK	Alaska	3	1	All
AZ	Arizona	3	1	All
AR	Arkansas	5	0	All
CA	California	0	1	All
CO	Colorado	2	1	All
CT	Connecticut	3	1	All
DE	Delaware	6	0	All
DC	District of Columbia	7	0	All
FL	Florida	7	0	Until 1996
FL	Florida	9	0	From 1997
GA	Georgia	5	0	All
HI	Hawaii	3	1	All
ID	Idaho	6	0	All
IL	Illinois	5	0	All
IN	Indiana	5	0	All
IA	Iowa	6	0	All
KS	Kansas	6	0	All
KY	Kentucky	6	0	All
LA	Louisiana	4	0	Until 2001
LA	Louisiana	0	1	2002-2003
LA	Louisiana	4	0	From 2004
ME	Maine	4	0	All
MD	Maryland	5	0	All
MA	Massachusetts	6	0	All
MI	Michigan	5	0	All
MN	Minnesota	5	0	All
MS	Mississippi	4	0	All
MO	Missouri	7	0	All
MT	Montana	2	1	All
NE	Nebraska	4	0	All
NV	Nevada	5	0	All
NH	New Hampshire	2	1	All
NJ	New Jersey	4	0	All
NM	New Mexico	2	1	All
NY	New York	3	1	All
NC	North Carolina	4	0	All
ND	North Dakota	0	1	All
OH	Ohio	5	0	All
OK	Oklahoma	1	1	All
OR	Oregon	6	0	All
PA	Pennsylvania	6	0	All
RI	Rhode Island	3	1	All
SC	South Carolina	5	0	All
SD	South Dakota	5	0	All
TN	Tennessee	7	0	All
TX	Texas	5	0	Until 1994
TX	Texas	3	1	From 1995
UT	Utah	6	0	All
VT	Vermont	5	0	All
VA	Virginia	3	1	All
WA	Washington	5	0	All
WV	West Virginia	2	1	All
WI	Wisconsin	3	1	All
WY	Wyoming	4	0	All

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Table 1: Descriptive Statistics

Panel A: Descriptive Statistics						
Variable	**N**	**Mean**	**Stdev**	**25th perc.**	**50th perc.**	**75th perc.**
Disclosure Variables						
Mgmt Forecast	37,297	0.39	0.49	0.00	0.00	1.00
Mgmt Capex	37,297	0.11	0.31	0.00	0.00	0.00
Lognd	37,297	0.59	0.79	0.00	0.00	1.39
Logdisclosures	37,297	0.79	1.09	0.00	0.00	1.79
Numest	126,727	3.68	5.50	0.00	1.10	5.00
AIMR Ranked Score	1,465	0.50	0.30	0.24	0.50	0.76
Annual Report	612	0.44	0.28	0.20	0.43	0.67
Other Publications	594	0.44	0.28	0.20	0.43	0.67
Investor Relations	547	0.43	0.28	0.20	0.42	0.65
Competition Variables						
Nonenforcing State	126,727	0.44	0.50	0.00	0.00	1.00
In-State Competition	126,727	0.64	0.48	0.00	1.00	1.00
PctComp	29,420	0.59	0.49	0.24	0.45	0.79
Control Variables						
CCR3	126,727	0.66	0.25	0.46	0.67	0.89
Industry Sale	126,727	9.27	2.15	7.86	9.47	10.84
Logentrycost	126,727	6.02	2.16	4.68	6.25	7.55
Indpricecostmargin	126,727	1.17	0.21	1.06	1.11	1.22
Logmkvalt	126,727	4.64	2.20	3.09	4.57	6.17
Stdroe	126,727	0.60	1.68	0.07	0.16	0.39
Correlret	126,727	0.20	0.49	(0.07)	0.26	0.53

Panel B: Does In-State Competition Reflect Increased Competition?				
Dependent Variable: PctComp	**(1)**		**(2)**	
In-State Competition	0.04	***	0.05	***
	(3.98)		(3.46)	
Nonenforcing State × In-State Competition			-0.01	
			(-0.35)	
Logmkvalt	-0.02	***	-0.02	***
	(-8.94)		(-8.91)	
Stdroe	0.00		0.00	
	(-0.51)		(-0.50)	
Correlret	-0.02	**	-0.02	**
	(-2.13)		(-2.13)	
CCR3	0.03		0.02	
	(0.92)		(0.90)	
Industry Sale	0.04	***	0.04	***
	(8.89)		(8.89)	
Logentrycost	-0.03	***	-0.03	***
	(-5.94)		(-5.92)	
Indpricecostmargin	-0.15	***	-0.14	***
	(-4.22)		(-4.20)	
Constant	0.62	***	0.62	***
	(10.74)		(10.74)	
Number of Observations	29,420		29,420	
State Year Fixed Effects	Yes		Yes	
Clustering	Firm		Firm	
Adjusted R-Square	0.24		0.24	
F-Statistic	31.97	***	28.42	***

Table 1, Panel A presents descriptive statistics. *Mgmt Forecast* and *Mgmt Capex* are indicator variables equal to one when a management forecast or a capital expenditures forecast were issued during the year. *Lognd* is equal to the logarithm of one plus the number of disclosure events during the year, while *Logdisclosures* is equal to the logarithm of one plus the number of disclosure items during the year. *Numest* is the number of analysts following a given stock. *AIMR Ranked Score* is the AIMR rankings disclosure variable, while *Annual Report*, *Other Publications*, and *Investor Relations* are the individual components of the rankings. *Nonenforcing State* is equal to one when the state does not enforce noncompete agreements. *In-State Competition* equals one when the firm has a rival headquartered in the same state. *PctComp* is the proportion of competition-related words as in Li et al. (2012). *CCR3* is the market share of the largest three firms in the industry. *Industry Sale* is the logarithm of total industry sales. *Logentrycost* equals the logarithm of the weighted average of the net PP&E, weighted by sales. *Indpricecostmargin* equals industry sales divided by industry operating costs. *Logmkvalt* is the natural logarithm of the firm market capitalization. *Stdroe* is the standard deviation of the return on equity. *Correlret* is the correlation between annual returns and EPS. Table 1, Panel B validates the *In-State Competition* variable as a measure of competition by regressing *PctComp* on this variable. Coefficient values are presented above and the corresponding t-statistic below. Standard errors are clustered at the firm level in Panel B. Significance levels are * 10%, ** 5% and *** 1%.

Table 2: Correlations Management Forecast Sample

Variable	Mgmt Forecast	Mgmt Capex	Log nd	Log discl	Numest	Nonenf. State	In-State Comp.	CCR3	Ind. Sale	Log Entry cost	Indpr cost mgn	Log mkvalt	Stdroe	Correlret
Mgmt Forecast		**0.44**	**0.96**	**0.96**	**0.63**	0.00	**-0.08**	**0.09**	**-0.04**	**-0.01**	**-0.12**	**0.47**	**-0.08**	**-0.10**
Mgmt Capex	**0.44**		**0.46**	**0.46**	**0.31**	**-0.02**	**-0.12**	**0.14**	**-0.06**	**0.06**	**-0.12**	**0.25**	**-0.06**	**-0.06**
Lognd	**0.94**	**0.46**		**0.98**	**0.63**	0.00	**-0.08**	**0.10**	**-0.04**	0.00	**-0.12**	**0.49**	**-0.09**	**-0.11**
Logdisclosures	**0.91**	**0.45**	**0.97**		**0.63**	0.00	**-0.08**	**0.10**	**-0.04**	**-0.01**	**-0.11**	**0.48**	**-0.08**	**-0.10**
Numest	**0.51**	**0.28**	**0.54**	**0.53**		**0.01**	0.00	**-0.01**	**0.11**	**0.15**	-0.01	**0.72**	**-0.14**	**-0.15**
Nonenf. State	0.00	**-0.02**	0.00	0.00	**0.03**		**0.17**	**-0.03**	0.00	**0.02**	**0.02**	0.00	**0.13**	**0.02**
In-State Comp.	**-0.08**	**-0.12**	**-0.08**	**-0.07**	0.00	**0.17**		**-0.56**	**0.47**	**0.27**	**0.35**	**-0.06**	**0.03**	**0.02**
CCR3	**0.09**	**0.14**	**0.10**	**0.10**	0.00	**-0.03**	**-0.55**		**-0.62**	**-0.24**	**-0.43**	0.01	**0.08**	**-0.02**
Industry Sale	**-0.01**	**-0.04**	-0.01	-0.01	**0.15**	**-0.01**	**0.47**	**-0.60**		**0.73**	**0.46**	**0.11**	**-0.10**	0.00
Logentry cost	**0.02**	**0.08**	**0.03**	**0.02**	**0.19**	**0.01**	**0.26**	**-0.19**	**0.71**		**0.32**	**0.14**	**-0.02**	**-0.03**
Indprice costmargin	**-0.14**	**-0.12**	**-0.14**	**-0.14**	**-0.04**	**0.02**	**0.30**	**-0.40**	**0.38**	**0.11**		**0.07**	**-0.14**	**-0.04**
Logmkvalt	**0.46**	**0.24**	**0.47**	**0.46**	**0.68**	0.00	**-0.05**	0.00	**0.17**	**0.16**	**0.07**		**-0.30**	**-0.23**
Stdroe	**-0.07**	**-0.03**	**-0.07**	**-0.07**	**-0.09**	**0.05**	**0.03**	0.01	**-0.03**	-0.01	**-0.03**	**-0.17**		**0.15**
Correlret	**-0.08**	**-0.05**	**-0.09**	**-0.08**	**-0.14**	**0.01**	**0.02**	**-0.01**	**-0.01**	**-0.03**	**-0.01**	**-0.18**	**0.02**	

Table 2 presents correlations across several disclosure variables for the sample restricted where *Mgmt Forecast* is available. The Spearman correlation is above the diagonal and the Pearson correlation below. Correlations significant at 5% or better are in bold.

Table 3: Analysis of Management Forecasts

Dependent Variable:	**Mgmt Forecast**		**Mgmt Capex**		**Lognd**		**Log disclosures**	
In State Competition	-0.02	*	-0.04	***	-0.05	**	-0.03	
	(-1.81)		(-5.38)		(-2.16)		(-1.06)	
Non enforcing state × In State Competition	0.05	***	0.03	***	0.08	**	0.09	**
	(2.65)		(2.77)		(2.47)		(2.04)	
Logmkvalt	0.11	***	0.03	***	0.19	***	0.25	***
	(62.46)		(33.59)		(56.35)		(52.41)	
Stdroe	0.00		0.00		0.00		0.00	
	(-0.16)		(1.35)		(0.22)		(0.25)	
Correlret	0.00		0.00		-0.01		-0.01	
	(0.48)		(0.06)		(-0.42)		(-0.70)	
CCR3	0.11	***	0.04	***	0.19	***	0.33	***
	(4.17)		(3.01)		(4.38)		(5.36)	
Industry Sale	0.01	**	-0.02	***	0.02	***	0.03	***
	(1.99)		(-8.67)		(2.61)		(3.32)	
Logentrycost	-0.02	***	0.02	***	-0.03	***	-0.05	***
	(-4.89)		(11.88)		(-4.66)		(-5.79)	
Indpricecostmargin	-0.33	***	-0.06	***	-0.53	***	-0.70	***
	(-16.28)		(-6.12)		(-15.91)		(-15.13)	
Constant	0.11	**	0.04		0.07		0.01	
	(2.48)		(1.61)		(0.90)		(0.11)	
Number of Observations	37,297		37,297		37,297		37,297	
State Year Fixed Effects	Yes		Yes		Yes		Yes	
Clustering	Firm		Firm		Firm		Firm	
Adjusted R-Square	0.29		0.25		0.31		0.29	
F-Statistic	532.87	***	200.71	***	417.66	***	359.73	***

Table 3 presents an OLS regression of the propensity of firms to disclose management forecasts. The explanatory variables of interest are *In-State Competition* and *Nonenforcing State × In-State Competition*, predicted to be negative and positive, respectively. Main control variables have been defined in Table 1. State year fixed effects are also added to remove the impact of state-specific effects that are not related to the enforcement of noncompete agreements. Coefficient values are presented above and the corresponding t-statistic below. Standard errors are clustered at the firm level. Significance levels are * 10%, ** 5% and *** 1%.

Table 4: Capital IQ Voluntary Disclosures – Descriptive Statistics

Panel A: Descriptive Statistics

Variable	N	Mean	Stdev	p25	p50	p75
Voluntary	10,446	23.57	20.51	11.00	19.00	29.00
Guidance	10,446	2.46	2.63	0.00	2.00	4.00
Conf	10,446	9.37	8.19	4.00	8.00	12.00
Prodcust	10,446	5.73	10.81	0.00	1.00	6.00
OtherVol	10,446	5.57	3.91	4.00	5.00	7.00

Panel B: Evolution Over Time

		2007	2008	2009	2010	2011
Voluntary	Mean	19.13	20.90	21.61	27.40	32.94
	Median	16	18	18	22	27
Guidance	Mean	2.09	2.14	2.61	2.48	3.37
	Median	1	1	2	2	4
Conf	Mean	6.71	7.35	7.46	12.77	15.13
	Median	6	7	6	10	13
Prodcust	Mean	5.25	5.94	5.65	5.58	6.57
	Median	1	1	1	1	1
OtherVol	Mean	4.65	5.12	5.54	6.04	7.25
	Median	4	5	5	5	7

Table 4 presents descriptive statistics on the sample of voluntary disclosures. The data is taken from Capital IQ. *Voluntary* is equal to the total number of voluntary disclosures during the fiscal year. This number is comprised of *Guidance*, equal to the total number of guidance events, *Conf*, equal to the total number of conferences and /or calls, *Prodcust*, equal to the total number of product related or customer related disclosures, and *OtherVol*, the other voluntary disclosures. Panel A presents descriptive statistics, while Panel B presents evolution of the average and median numbers over time.

Table 5: Capital IQ Voluntary Disclosures

Dependent Variable:	**Log (1+Voluntary)**		**Log (1+Guidance)**		**Log (1+Conf)**		**Log (1+Prodcust)**		**Log (1+OtherVol)**	
In State Competition	-0.05		-0.09	**	-0.02		0.08		-0.10	***
	(-1.33)		(-2.38)		(-0.43)		(1.40)		(-3.47)	
Non enforcing state × In State Competition	0.16	***	0.12	**	0.13	**	0.14	*	0.07	*
	(2.94)		(2.28)		(2.47)		(1.78)		(1.78)	
Logmkvalt	0.30	***	0.39	***	0.11		0.39	***	0.24	***
	(3.88)		(5.49)		(1.41)		(3.36)		(4.58)	
Stdroe	-0.01		-0.02	*	-0.03	***	0.04	**	0.02	**
	(-0.56)		(-1.68)		(-2.80)		(2.32)		(2.04)	
Correlret	0.01		0.02		0.03	***	0.01		-0.03	***
	(0.84)		(1.63)		(3.00)		(0.88)		(-4.44)	
CCR3	-0.42	***	-0.64	***	-0.15	**	-0.53	***	-0.26	***
	(-5.90)		(-9.45)		(-2.15)		(-5.16)		(-4.84)	
Industry Sale	0.28	***	0.18	***	0.25	***	0.14	***	0.20	***
	(40.14)		(32.90)		(41.73)		(13.81)		(42.32)	
Logentrycost	0.03	***	0.00		0.02	***	0.06	***	-0.01	***
	(3.99)		(-0.50)		(3.27)		(5.48)		(-2.69)	
Indpricecostmargin	-0.03		-0.04	*	-0.02		-0.03		0.02	
	(-1.07)		(-1.77)		(-0.88)		(-0.78)		(1.06)	
Constant	1.39	***	0.40	***	0.63	***	-0.09		0.68	***
	(9.21)		(2.91)		(4.37)		(-0.44)		(6.55)	
Number of Observations	10,446		10,446		10,446		10,446		10,446	
State Year Fixed Effects	Yes		Yes		Yes		Yes		Yes	
Clustering	Firm		Firm		Firm		Firm		Firm	
Adjusted R-Square	0.42		0.30		0.41		0.18		0.38	
F-Statistic	217.57	***	199.24	***	230.33	***	33.82	***	249.34	***

Table 5 presents an analysis similar to Table 3, with the dependent variable replaced with the voluntary disclosure data obtained from Capital IQ. Coefficient values are presented above and the corresponding t-statistic below. Standard errors are clustered at the firm level. Significance levels are * 10%, ** 5% and *** 1%.

Table 6: Number of Analysts

Dependent Variable: Number of Analysts				
	(1)		**(2)**	
In State Competition	-0.13		-0.09	
	(-0.93)		(-0.98)	
Non enforcing state × In State Competition	0.52	***	0.43	***
	(2.89)		(3.66)	
Logmkvalt	1.84	***	1.83	***
	(63.84)		(71.78)	
Stdroe	0.04	***	0.01	
	(2.60)		(1.05)	
Correlret	-0.31	***	-0.25	***
	(-3.92)		(-6.76)	
CCR3	0.23		0.46	**
	(0.81)		(2.34)	
Industry Sale	0.08	*	0.14	***
	(1.78)		(4.19)	
Logentrycost	0.18	***	0.11	***
	(5.41)		(4.45)	
Indpricecostmargin	-2.38	***	-2.25	***
	(-11.05)		(-16.64)	
Constant	-5.07	***	-4.46	***
	(-10.60)		(-14.18)	
Number of Observations	37,297		126,727	
State Year Fixed Effects	Yes		Yes	
Clustering	Firm		Firm	
Adjusted R-Square	0.53		0.54	
F-Statistic	485.11	***	599.82	***

Table 6 presents an analysis similar to Table 3, with the dependent variable replaced with *Numest*, the average number of analysts covering the firm during the year. The sample is restricted to the subsample where the management-forecast data is available in Column (1). Coefficient values are presented above and the corresponding t-statistic below. Standard errors are clustered at the firm level. Significance levels are * 10%, ** 5% and *** 1%.

Table 7: AIMR Rankings

Dependent Variable:	**AIMR Ranked Score**		**Annual Report**		**Other Publications**		**Investor Relations**	
In State Competition	-0.11	***	-0.11	**	-0.08		-0.10	*
	(-2.95)		(-2.27)		(-1.54)		(-1.89)	
Non enforcing state ×	0.14	**	0.19	**	0.14		0.10	
In State Competition	(2.06)		(2.25)		(1.44)		(0.99)	
R- Logmkvalt	0.30	***	0.27	***	0.23	**	0.26	***
	(5.47)		(2.67)		(2.33)		(2.84)	
R- Stdroe	-0.06		-0.20	**	-0.18	**	-0.10	
	(-1.15)		(-2.59)		(-2.27)		(-1.25)	
R- correlret	-0.02		0.04		0.05		0.02	
	(-0.48)		(0.65)		(0.77)		(0.24)	
R-CCR3	-0.09	*	-0.10		0.06		-0.05	
	(-1.89)		(-1.56)		(0.76)		(-0.66)	
R-Industry Sale	0.10		0.02		0.04		0.00	
	(1.12)		(0.22)		(0.27)		(-0.01)	
R- Logentrycost	0.08		-0.02		-0.02		-0.02	
	(0.94)		(-0.15)		(-0.20)		(-0.15)	
R- Indpricecostmargin	0.02		0.02		0.03		0.03	
	(0.54)		(0.28)		(0.41)		(0.38)	
Constant	0.36	***	0.44	***	0.35	***	0.39	***
	(6.13)		(5.19)		(3.87)		(4.44)	
Number of Observations	1,465		612		594		547	
State Year Fixed Effects	Yes		Yes		Yes		Yes	
Clustering	Firm		Firm		Firm		Firm	
Adjusted R-Square	0.15		0.22		0.20		0.16	
F-Statistic	10.29	***	4.18	***	3.01	***	1.71	*

Table 7 presents an analysis similar to Table 3, with the dependent variable replaced with *AIMR Ranked Score* in the first column and the individual components of the AIMR Rankings in subsequent columns. Continuous variables have been replaced with their rank, consistent with prior research. Coefficient values are presented above and the corresponding t-statistic below. Standard errors are clustered at the firm level. Significance levels are * 10%, ** 5% and *** 1%.

Table 8: Impact of Employee Mobility, Distance and Out-of-State Competition

Panel A: Main Disclosure Variables								
	Mgmt Forecast		**Log(1+Voluntary)**		**Numest**		**AIMR Ranked Score**	
Exec Mobility	0.04		0.14	*	3.05	***	0.01	
	(1.34)		(1.93)		(6.81)		(0.05)	
Logempperest	0.02	***	0.00		0.07	*		
	(3.30)		(0.13)		(1.69)			
Logmeddistance	-0.01	**	-0.05	***	-0.09	**	0.17	
	(-2.08)		(-3.48)		(-2.57)		(0.75)	
Stateshareall	0.27	***	0.98	***	3.57	***		
	(3.00)		(4.23)		(4.35)			
Statesharepublic	0.03		0.03		0.62	**	0.30	*
	(0.69)		(0.29)		(2.58)		(1.70)	
Logmkvalt	0.08	***	0.25	***	1.54	***	0.27	***
	(21.74)		(18.99)		(34.26)		(2.96)	
Stdroe	0.00	*	0.02		0.02		-0.13	
	(-1.67)		(1.35)		(0.90)		(-1.17)	
Correlret	0.01		0.01		-0.11	*	0.02	
	(0.95)		(0.19)		(-1.77)		(0.18)	
CCR3	0.09	*	0.18		0.50		-0.07	
	(1.91)		(1.47)		(1.63)		(-0.62)	
Industry Sale	0.01	**	0.04	*	0.11	**	0.25	
	(2.01)		(1.71)		(1.98)		(1.40)	
Logentrycost	-0.02	***	-0.01		0.12	***	-0.17	
	(-4.26)		(-0.73)		(3.13)		(-0.99)	
Indpricecostmargin	-0.31	***	-0.43	***	-1.68	***	-0.13	
	(-9.64)		(-3.59)		(-8.06)		(-1.38)	
Logestab	-0.01	***	-0.11	***	0.06	*		
	(-2.61)		(-8.74)		(1.69)			
Flag Execucomp	0.16	***	0.12	**	2.48	***	0.09	
	(7.77)		(2.40)		(16.12)		(0.53)	
Flag Bus Pattern	0.08	*	0.68	***	-0.94	***		
	(1.82)		(4.53)		(-2.82)			
Constant	0.15	*	1.39	***	-4.00	***	0.08	
	(1.88)		(5.09)		(-6.89)		(0.29)	
Number of Observations	12,356		3,393		38,146		276	
State Year Fixed Effects	Yes		Yes		Yes		Yes	
Adjusted R-Square	0.30		0.48		0.58		0.09	
F-Statistic	111.92	***	50.32	***	151.96	***	2.11	**

Panel B: Other Disclosure Variables

	MgmtCapex		Lognd		Log disclosures		Log (1+Guidance)		Log(1+ NonGuidance)	
Exec Mobility	0.02		0.16	**	0.23	**	0.12		0.15	**
	(0.71)		(2.49)		(2.47)		(1.52)		(1.99)	
Logempperest	0.00		0.03	***	0.04	***	0.04	***	0.00	
	(1.48)		(3.58)		(3.83)		(2.69)		(-0.05)	
Logmeddistance	0.00		-0.01		-0.02		-0.02		-0.05	***
	(-0.56)		(-1.26)		(-1.42)		(-1.46)		(-3.48)	
Stateshareall	0.11	**	0.41	***	0.46	**	0.68	***	0.95	***
	(2.35)		(2.75)		(2.18)		(2.94)		(4.15)	
Statesharepublic	0.04	*	0.02		0.03		-0.05		0.04	
	(1.83)		(0.39)		(0.35)		(-0.51)		(0.43)	
Logmkvalt	0.02	***	0.13	***	0.17	***	0.12	***	0.25	***
	(9.42)		(20.61)		(18.91)		(10.04)		(19.04)	
Stdroe	0.00		-0.01		-0.01		-0.01		0.02	
	(1.01)		(-1.33)		(-1.05)		(-1.13)		(1.48)	
Correlret	0.00		0.02		0.03		0.00		0.00	
	(0.18)		(0.75)		(0.91)		(-0.12)		(0.11)	
CCR3	-0.01		0.13	*	0.23	**	0.43	***	0.13	
	(-0.62)		(1.79)		(2.24)		(3.58)		(1.05)	
Industry Sale	-0.02	***	0.02	*	0.04	**	-0.01		0.05	**
	(-5.69)		(1.77)		(2.18)		(-0.41)		(2.13)	
Logentrycost	0.02	***	-0.04	***	-0.06	***	-0.01		-0.01	
	(6.55)		(-4.08)		(-4.86)		(-0.47)		(-0.82)	
Indpricecostmargin	-0.08	***	-0.48	***	-0.65	***	-0.47	***	-0.38	***
	(-5.34)		(-9.19)		(-8.97)		(-4.44)		(-3.25)	
Logestab	0.01	**	-0.01		-0.01		-0.02		-0.12	***
	(2.49)		(-1.58)		(-1.16)		(-1.56)		(-9.22)	
Flag Execucomp	0.07	***	0.29	***	0.41	***	0.25	***	0.08	*
	(6.56)		(8.87)		(8.47)		(4.96)		(1.73)	
Flag Bus Pattern	-0.02		0.11		0.10		-0.15		0.72	***
	(-0.73)		(1.55)		(1.07)		(-1.20)		(4.90)	
Constant	0.13	***	0.18		0.26		0.66	***	1.20	***
	(2.82)		(1.35)		(1.37)		(2.59)		(4.54)	
Number of Observations	12,356		12,356		12,356		3,393		3,393	
State Year Fixed Effects	Yes		Yes		Yes		Yes		Yes	
Adjusted R-Square	0.23		0.33		0.31		0.30		0.48	
F-Statistic	31.81	***	91.87	***	77.72	***	33.60	***	49.39	***

The analysis in Table 8 is restricted to firms headquartered in states not enforcing noncompete agreements and where rivals are also present in the state. The variables of interest in the analysis are *Exec Mobility*, an indicator variable equal to one if the firm hired executives from its rivals or its rivals hired executives from the firm, *Logempperest*, the logarithm of one plus the total number of employees in the industry in the state divided by the total number of establishments in the state, *Logmeddistance*, the logarithm of one plus the median of the distance between the firm and its rivals headquartered in the same state, *Stateshareall*, equal to the share of firms within the industry of the rival firms headquartered in the same state, and *Statesharepublic*, equal to the share of sales within the industry of public rival firms headquartered in the same state. Some of the data are not available for the period corresponding to the AIMR rankings and cannot be included in this regression. Panel A shows the results for the main disclosure variables, while Panel B shows additional results for the other disclosure variables. Coefficient values are presented above and the corresponding t-statistic below. Standard errors are clustered at the firm level. Significance levels are * 10%, ** 5% and *** 1%.

Table 9: Time-Series Analyses

	Number of Analysts				Management Forecasts			
	(1)		(2)		(3)		(4)	
In-State Competition			0.04				-0.01	
			(0.48)				(-0.69)	
Nonenforcing State × In-State Competition			0.10				0.00	
			(0.85)				(0.17)	
Nonenforcing State	0.33	*	0.26		0.04		0.04	
	(1.86)		(1.27)		(0.47)		(0.44)	
Logmkvalt	1.16	***	1.16	***	0.06	***	0.06	***
	(49.53)		(49.54)		(16.97)		(16.96)	
Stdroe	-0.02	*	-0.02	*	0.00		0.00	
	(-1.75)		(-1.74)		(-0.98)		(-0.97)	
Correlret	-0.34	***	-0.34	***	-0.01		-0.01	
	(-10.98)		(-10.99)		(-1.06)		(-1.08)	
CCR3	-0.21		-0.17		-0.07	**	-0.07	**
	(-1.21)		(-0.99)		(-2.01)		(-2.12)	
Industry Sale	0.05	**	0.05	**	-0.02	***	-0.02	***
	(2.18)		(1.98)		(-3.14)		(-3.05)	
Logentrycost	0.05	***	0.05	**	0.01	***	0.01	***
	(2.60)		(2.57)		(2.83)		(2.86)	
Indpricecostmargin	0.42	***	0.41	***	0.11	***	0.11	***
	(3.34)		(3.30)		(4.49)		(4.46)	
Constant	-1.90	***	-1.83	***	0.03		0.04	
	(-6.57)		(-5.91)		(0.41)		(0.49)	
Number of Observations	126,727		126,727		37,297		37,297	
State Year Fixed Effects	No		No		No		No	
Firm Fixed Effects	Yes		Yes		Yes		Yes	
Year Fixed Effects	Yes		Yes		Yes		Yes	
Clustering	Firm		Firm		Firm		Firm	
Adjusted R-Square	0.82		0.82		0.64		0.64	
F-Statistic	108.28	***	105.30	***	92.73	***	82.92	***
F-test Nonenforcing State + Nonenforcing State × In-State Competition = 0								
F-test			3.97	**			0.24	

Table 9 presents time-series specifications with the number of analysts (*numest*) as the dependent variable in Columns (1) and (2) and Management Forecasts in Columns (3) and (4). The coefficient on *Nonenforcing State* is identified only in the context of changes in noncompete enforcement levels in Texas in 1994 and Louisiana in 2000 and 2004. Coefficient values are presented above and the corresponding t-statistic below. Standard errors are clustered at the firm level. Significance levels are * 10%, ** 5% and *** 1%.