How does restricting labor mobility affect financial statement comparability?

An examination of U.S. covenants not-to-compete

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ABSTRACT

This study explores how labor mobility restrictions in the form of covenants not-to-compete (CNCs) affect financial statement comparability. Evidence shows that when a state increases the legal enforceability of these covenants, the financial statements of industry peers in this state become less comparable. The impact is stronger for industry followers and firms surrounded by more local peers, suggesting that labor-based knowledge spillover is a mechanism of this impact. Moreover, increases in CNC enforceability lead firms to face greater difficulties in raising capital and sustaining investment due to lower comparability. These findings have important implications for policymakers, accounting standard setters, and firm owners.

Keywords: covenants not-to-compete, labor mobility, financial statement comparability, learning through hiring

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1. Introduction

When employees leave their employers, they may take away valuable human capital from the firms and pose a risk of revealing trade secrets to competitors. These concerns may prompt firms to bind their talents and limit their mobility in the labor market. In the U.S., one way for firms to do so is through using covenants not-to-compete (CNCs). These are covenants in employment contracts that prevent workers from competing with their former employers, either by joining or establishing a competing firm, for a specified period after leaving. CNCs are prevalent: Boesch et al. (2023) find that 11% of U.S. workers in all sectors are subject to CNCs, whereas earlier estimates by Starr et al. (2021) suggest this ratio to be 18%.

In this study, I examine how labor mobility restrictions such as CNCs affect financial statement comparability. The comparability of financial statements allows users to identify similarities and differences between the economic phenomena that these statements depict (FASB, 2010). Two accounting systems are comparable if they report similar financial statements when given the same economic events (De Franco et al., 2011). In other words, comparable financial statements use similar treatments for similar transactions, and different treatments for different transactions. Comparability enhances the usefulness of accounting information and leads to better investment decisions (e.g., Young & Zeng, 2015). Thus, it is a characteristic chased after by prominent accounting standard setters such as the International Accounting Standards Board (IASB) and the Financial Accounting Standards Board (FASB).

I propose two possible reasons why restricting labor mobility could reduce financial statement comparability. First, accounting choices can be transferred through labor flows. When executives and accounting staff move between firms, they could bring their personal preferences and familiar practices from their old firms to new ones. Restricting mobility hinders this transfer of accounting choices between firms, potentially reducing comparability. Second, prior research has shown that accounting choices depend on, among other factors, a firm's investment opportunity set and product market conditions (Cazavan-Jeny et al., 2011; Dhaliwal et al., 2014; Hui et al., 2012; Skinner, 1993). By hiring peers' former employees, firms learn about and mimic their investments and innovations. This process, known as learning through hiring, may increase the similarity in firms' investment opportunity sets and product market conditions (Bloom et al., 2013; Byun et al., 2023; Ganguli et al., 2020), which in turn leads to increased similarity in accounting choices and comparability of financial statements. Conversely, financial statements could become less comparable when learning through hiring is mitigated by mobility restrictions.

In my research design, I exploit state-level changes in the enforcement strength, or enforceability, of CNCs as shocks to the degree to which these covenants restrict labor mobility. I follow the literature (Cengiz et al., 2019; Jeffers, 2023) and adopt a stacked difference-in-differences (DiD) approach to estimate the effect of increased mobility restrictions on financial statement comparability. My baseline result indicates that when a state strengthens CNC enforceability, financial statements of industry peers within that state become less comparable. This result survives through robustness tests that aim to mitigate potential endogeneity issues. It also persists when I use an alternative measure of comparability or test the hypothesis on a firm pair-year panel (instead of a firm-year panel).

To better understand the mechanisms behind the impact of mobility restrictions on financial statement comparability, I conduct additional cross-sectional tests. Firstly, I anticipate that the effect of mobility restrictions on comparability is more pronounced for industry followers than for industry leaders. The reason is that industry followers are more intensive learners (Blazsek & Escribano, 2016; Eeckhout & Jovanovic, 2002) and can presumably adjust their accounting choices more flexibly. I also expect a stronger comparability response when more local peers surround a firm. Within a denser firm network, knowledge spillovers occur more frequently and may have more impact (Meagher & Rogers, 2004). Hence, the amount of comparability attributed to knowledge spillovers may be more sensitive to labor mobility in such networks. In line with these predictions, I find that the impact of strengthened CNC enforceability on comparability is more pronounced for industry followers and firms within denser local peer networks.

Finally, I find that firms experience a decline in capital growth and investment intensity following an increase in CNC enforceability. More importantly, decreased financial statement comparability is a driver of this outcome. These results indicate that the negative impact of labor mobility restrictions on comparability has significant implications for firms and capital market investors.

The paper makes several contributions to the literature. First, it extends the nascent literature on the determinants of comparability. Previous studies have shown that comparability is higher when two firms share a joint auditor (J. Z. Chen et al., 2020; Francis et al., 2014), when accounting standards restrict managers' discretion (Young, 2023), or when managers' demand for legitimacy is higher (De Franco et al., 2023). To the best of my knowledge, this study is the first to relate financial statement comparability to legal restrictions on labor mobility, highlighting how labor market frictions can influence the usefulness of accounting information. Second, the paper adds to our understanding of the real effects of labor mobility restrictions. Studies under this theme have examined how such restrictions affect firm value (Belo et al., 2014; Shen, 2021), investment (Gu et al., 2022; Jeffers, 2023), innovation, and entrepreneurship (Kaiser et al., 2015; Samila & Sorenson, 2011), financial policies (Klasa et al., 2018; Qiu & Wang, 2021), financial disclosure (Callen et al., 2020; Oh & Park, 2023), and corporate governance (Garmaise, 2011; Kini et al., 2021). This study indicates that restricting labor mobility reduces financial statement comparability. More notably, while prior studies emphasize career concerns in explaining why financial disclosure is sensitive to labor mobility, this study suggests that knowledge spillovers and peer learning are also important mechanisms.

The findings of this study carry practical implications for policymakers, standard setters, and firm owners. They indicate that policymakers should consider the side effects of mobility restrictions on financial statement comparability and the resultant economic consequences when formulating related policies. They also underscore the necessity of improving accounting rules and disclosure requirements to promote comparability of financial statements. Moreover, they suggest that when mobility restrictions cause their financial statements to be less comparable, firms face the risk of weakened capital acquisition and investment intensity. This risk is more prominent for firms with less market power and denser local peer networks. To alleviate this issue, firms should enhance transparency and internal audit quality to increase the informativeness of their disclosure to investors.

The rest of the thesis is structured as follows. Section 2 reviews related literature and establishes the research hypothesis. Section 3 introduces the data and empirical methods. Section 4 reports the results of estimating the effect of increased mobility restrictions on comparability. Section 5 provides the results of cross-sectional tests. Section 6 explores the economic consequences of decreased comparability as mobility restrictions strengthen. Section 7 concludes.

2. Related literature

2.1. Labor mobility restrictions and CNC enforceability

Labor mobility and its restrictions, such as the use of CNCs, significantly impact economic outcomes at the firm level. First, labor movement is an important medium for knowledge spillovers between proximate companies (Matray, 2021; Parrotta & Pozzoli, 2012; Rosenkopf & Almeida, 2003). Prior research finds that when labor mobility is restricted, innovation and entrepreneurship activities diminish (Gu et al., 2022; Jeffers, 2023; Kaiser et al., 2015; Samila & Sorenson, 2011). Second, higher labor mobility, by decreasing the cost of job dismissal and job finding, provides workers with more outside opportunities. This safety net prompts workers to modify their behaviors, such as their commitment levels and willingness to take risks (Agarwal et al., 2023; Cici et al., 2021; Çolak & Korkeamäki, 2021).

Mobility restrictions also influence financial disclosure. For example, Callen et al. (2020) find that when mobility is restricted, disclosure is more opaque as firms face higher proprietary costs of disclosure. T. Y. Chen et al. (2018) indicate that when labor mobility declines, managers boost short-term performance by reducing discretionary expenditures, suggesting that managers feel more pressure to keep their jobs. Oh and Park (2023) show that stricter mobility restrictions incentivize earnings management and impair reporting quality, which they also attribute to managers facing heightened career concerns.

In the U.S., CNCs are commonly used to limit labor mobility. These covenants prevent departing employees from competing with their previous employers. For example, a CNC may require that upon job separation, an employee cannot join or establish a competing firm within a 60-mile radius of their former employer for the subsequent two years. Survey evidence suggests that technology and engineering firms, professional services providers, and financial institutions are the most intensive users of such covenants (Boesch et al., 2023; Starr et al., 2021). Violators of CNCs face the risk of costly litigation and suffer significant financial and reputational damage if unsuccessful in court[1]. Employers also hesitate to hire CNC-bound candidates to avoid disrupting their operations in the event of legal disputes. As a result, CNCs discourage workers from moving to another firm and decrease overall labor mobility.

In the U.S., CNC enforcement is mainly regulated at the state level[2]. As of December 2023, CNCs are prohibited in five states while enforced at varying degrees in the rest of the country[3]. The legal enforceability of a CNC determines the likelihood that a court will rule it to be binding (i.e., the terms in the covenant must be followed through by the parties involved). Changes in CNC enforceability, either stronger or weaker, are brought about by new state case laws and statutes (Ewens & Marx, 2018; Kini et al., 2021). Stronger CNC enforceability may depress labor mobility in more than one way. First, when enforceability increases, employees who already sign a CNC are further discouraged from changing jobs, knowing they would likely face a greater disadvantage in court. Second, when CNCs are more enforceable, more employers may start using CNCs, and those who already use CNCs may feel incentivized to write more restrictive ones. Empirically, Balasubramanian et al. (2020), Jeffers (2023), Johnson, Lavetti, et al. (2023), Johnson, Lipsitz, et

[1] For example, in 2012, Capital One filed a lawsuit against John Kanas, former President of Banking Segment, and John Bohlsen, former Executive Vice President of Commercial Banking, for CNC breaching. After leaving Capital One, Kanas and Bohlsen founded their own bank, which later acquired business interests within the geographic region outlined in their CNCs (for more details, see Capital One Financial Corp. v. Kanas). Resolution is reached when Kanas and Bohlsen agreed to compensate Capital One $20 million.

[2] CNC enforcement is receiving higher regulation at the federal level. For example, in January 2023, the Federal Trade Commission proposed a rule change that would render CNC unenforceable across the country. For more details, see https://www.ftc.gov/legal-library/browse/federal-register-notices/non-compete-clause-rulemaking

[3] Fives states that outright ban CNCs are California, Colorado, Oklahoma, North Dakota, and Minnesota.

al. (2023), Marx et al. (2009), and Starr et al. (2021) provide evidence that stronger (weaker) CNC enforceability depresses (promotes) workers' mobility.

2.2. Financial statement comparability

The practical importance of comparability has spurred increasing interest in this topic within the accounting literature. The introduction of empirical comparability measures by De Franco et al. (2011) and Barth et al. (2012) has also contributed to this momentum. Studies on comparability have identified significant economic implications of this characteristic for capital market participants. For example, Fiechter et al. (2024) find that following an improvement in comparability, firms enjoy an increase in equity capital growth not explained by industry trends or economic conditions. C. W. Chen et al. (2018) find that acquisitions are more successful when target firms' financial statements are more comparable to those of industry peers. In addition, evidence from the secondary equity market suggests that higher comparability is associated with a lower cost of equity (Imhof et al., 2017), higher valuation and liquidity (Neel, 2017), lower perceived crash risk (J. B. Kim et al., 2016), and higher foreign mutual fund ownership (DeFond et al., 2011). Similar benefits arise in the credit market, where higher comparability leads to lower credit spreads for bonds and credit default swaps (S. Kim et al., 2013) and more favorable syndicated loan contract terms (Fang et al., 2016). The main argument underlying these results is that comparability reduces information asymmetry and the costs of acquiring and processing firm-specific information. Consequently, firms are less susceptible to information risk and enjoy a lower cost of capital.

The role of comparability in reducing information uncertainty extends beyond capital market transactions. For instance, when a firm's accounting practices mirror those of its peers, it can better estimate the financial value of peer knowledge and predict its gains from acquiring such knowledge. Consequently, comparability improves innovation efficiency and knowledge accumulation (Chircop et al., 2020; Tseng & Zhong, 2024). In addition, comparability improves auditing outcomes (Zhang, 2018) and analyst forecast accuracy (De Franco et al., 2011).

Comparability is often attributed to the adoption of, either mandatory or voluntary, a universal accounting standard (e.g., DeFond et al., 2011; Fiechter et al., 2024). However, even under a standardized reporting regime, firms exercise significant discretion in their reporting choices. An implication is that different reporting entities can present the same economic transactions differently (Bowen et al., 2008; Stubben, 2010). While a common accounting standard would enhance overall comparability, the extent and specific peers to which a firm's accounting is comparable depend greatly on its accounting choices. Indeed, Wu & Xue (2023) propose that financial statement preparers prefer a lower level of comparability than financial statement readers. In the literature, fewer studies delve into the determinants of this character than its value for investment decisions. A few exceptions include J. Z. Chen et al. (2020) and Francis et al. (2014), who found that two firms sharing the same auditor tend to have more comparable financial statements; Young (2023), who finds that comparability decreases following GAAP changes that restrict managers' discretion; and De Franco et al. (2023), who found that managers at industry followers tend to make their accounting comparable to that of industry leaders to gain legitimacy.

2.3. Hypothesis development

Labor movements may serve as a medium for the transfer of accounting knowledge between firms. As executives and accounting workers enter new firms, they could introduce practices and procedures they are familiar with from their previous employment, making the accounting system of their new firm similar to that of old ones. Furthermore, prior research has pointed out that top managers such as CFOs and CEOs could inject their personal preferences into financial disclosures. These preferences may stem from their dispositions, living experiences, or career track records and manifest in varying levels of disclosure optimism (Bochkay et al., 2019), earnings management (Ge et al., 2011), or voluntary disclosure (Bamber et al., 2010). If these preferences persist within their former firms and travel with workers to new firms, one could expect comparability between firms to increase. Conversely, restricting labor mobility impedes the transfer of accounting practices and preferences through worker movements, which may diminish comparability.

Through hiring peers' former workers, firms also learn about and mimic peers' investment opportunities and innovations – a phenomenon called learning through hiring (Matray, 2021; Palomeras & Melero, 2010; Parrotta & Pozzoli, 2012; Rosenkopf & Almeida, 2003). I propose that the extent of learning through hiring, which is subject to labor mobility, can also influence the comparability of financial statements.

Prior research has highlighted that a firm's accounting choices are influenced by the investment opportunity set that it faces. For instance, Skinner (1993) takes a principal-agency perspective and argues that the investment opportunity set dictates the degree of flexibility granted to managers in financial reporting, which determines the final accounting choices of the managers. Empirically, the author finds that different measures of growth opportunities explain managers' choices of inventory cost flow assumption, depreciation method, and goodwill amortization period. Wyatt (2005) finds that firms' decision to record intangible assets depends on the potential profitability and cycle time (i.e., the time it takes to turn a new idea into commercial viability) of the technology they invest in. Cazavan-Jeny et al. (2011) provide evidence that a firm's decision to capitalize or expense R&D expenditures is affected by its relative R&D intensity. Meanwhile, a firm's product market conditions may also influence its accounting choices. For example, Dhaliwal et al. (2014)

suggest that when the product market is competitive, firms recognize losses more quickly, a phenomenon known as accounting conservatism. On the same note, Hui et al. (2012) find that firms display more accounting conservatism when their suppliers and customers have greater bargaining power.

As local peers learn about and mimic each other through hiring, the overlap in their investment opportunity sets and product market conditions would expand (Bloom et al., 2013; Byun et al., 2023; Ganguli et al., 2020). Under the influence of more similar investment opportunity sets and product market conditions, firms adopt more similar accounting choices. Therefore, one can expect financial statements to be more comparable when learning through hiring intensifies. In contrast, restricting labor mobility could reduce labor-based knowledge spillovers, thus depressing financial statement comparability.

However, an opposite effect may take place. Previous studies suggest that firms with less market power tend to mimic the accounting choices of industry leaders (De Franco et al., 2023; Kubick et al., 2015). Restricting labor mobility may lessen the ability of these firms to catch up with leaders through labor-based knowledge spillovers, and the gap between leaders and followers may widen (Blazsek & Escribano, 2016; Eeckhout & Jovanovic, 2002; Fung, 2005). This increased inequality may lead to an even more pronounced accounting mimicry effect, rendering higher comparability of financial statements.

Formally, the central hypothesis of this study is as follows:

> *H1: Restricting labor mobility reduces financial statement comparability among local peers*

3. Empirical methods and data

3.1. Measuring financial statement comparability with local peers

I define a firm's local peers as firms in the same 2-digit SIC industry and having headquarters in the same state as the firm's headquarters. I use the method in De Franco et al. (2011) to measure the comparability of financial statements. As in De Franco et al. (2011), an accounting system translates economic events into accounting performance; two accounting systems are comparable if they report similar financial statements under the same economic events.

Specifically, for each firm-year, I estimate the following equation using data from the previous 16 quarters (including four quarters from the current year and twelve quarters from the preceding three years):

$$Earnings_{i,t} = \alpha_i + \beta_i * Return_{i,t} + \varepsilon_{i,t} \quad (1)$$

where $Earnings$ is the quarterly net income before extraordinary items scaled by the lagged total market value of equity, and $Return$ is the buy-and-hold stock return during the same quarter. The coefficient estimates $\widehat{\alpha_i}$ and $\widehat{\beta_i}$ represent the accounting system of firm i, which maps economic events ($Return$) to accounting performance ($Earnings$). Then, for each pair of focal firm i and local peer j, I compare their accounting systems using the following relations:

$$\mathbb{E}(Earnings)_{i,i,t} = \widehat{\alpha_i} + \widehat{\beta_i} * Return_{i,t} \quad (2)$$

$$\mathbb{E}(Earnings)_{i,j,t} = \widehat{\alpha_j} + \widehat{\beta_j} * Return_{i,t} \quad (3)$$

where $\mathbb{E}(Earnings)_{i,i,t}$ and $\mathbb{E}(Earnings)_{i,j,t}$ are the expected earnings from the accounting systems of i and j when stock returns of i serve as underlying economic events in both. If firm i and peer j have more similar accounting systems, $(\widehat{\alpha_i}, \widehat{\beta_i})$ should be more similar to $(\widehat{\alpha_j}, \widehat{\beta_j})$, and so are $\mathbb{E}(Earnings)_{i,i,t}$ and $\mathbb{E}(Earnings)_{i,j,t}$, given that the underlying economic events are held constant. Accordingly, the financial statement comparability between firm i and peer j, denoted $GDFComp_{i,j}$, is defined as the negative value of the average absolute difference between the predicted earnings from equations (2) and (3) over the last 16 quarters:

$$GDFComp_{i,j} = -1 * 1/16 \sum_{t-15}^{t} |\mathbb{E}(Earnings)_{i,i,t} - \mathbb{E}(Earnings)_{i,j,t}| \quad (4)$$

Since we take the negative of the average distance, the higher $GDFComp_{i,j}$ means the accounting systems and financial statements of i and j are more comparable. The firm-level measure of comparability of firm i with local peers, denoted $GDFComp_i$, is the average of all pairwise $GDFComp_{i,j}$ across all local peers j of i.

3.2. Changes in CNC enforceability

Data on changes in CNC enforceability are adopted from Ewens & Marx (2018). As discussed in section 2.1, changes to CNC enforceability are brought about by precedent-setting court rulings or state statutes. Ewens & Marx (2018) compile a comprehensive pool of CNC-related law changes and enlist legal experts to identify changes that significantly impact enforceability. Due to data availability, I can only examine changes between 2002 and 2022. Figure 1 visualizes the geographic distribution and shows the timing of the state-level changes in CNC enforceability used in my research design. Out of the twelve changes, enforceability weakened in four (Oregon, South Carolina, New Hampshire, Kentucky) and strengthened in eight (Ohio, Vermont, Idaho, Wisconsin, Georgia, Colorado, Illinois, and Texas). Additionally, four changes were made by legislators (Idaho, Georgia, Oregon, and New Hampshire), while the others are outcomes of new case law precedents.

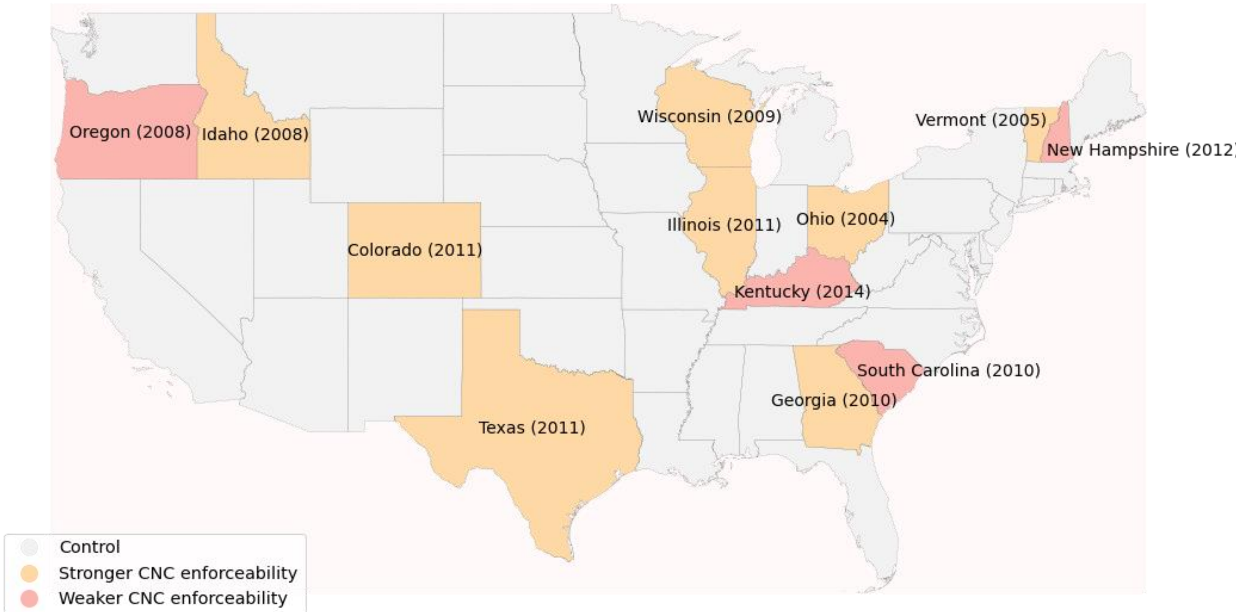

Figure 1. Map of CNC enforceability changes. This figure visualizes the geographic distribution and timing of the state-level changes to CNC enforceability used in this study. Data are adopted from Ewens & Marx (2018)

3.3. Empirical methods

To examine the impact of CNC enforceability changes on comparability, I follow the literature (Cengiz et al., 2019; Chang et al., 2022; Jeffers, 2023) and use a stacked difference-in-differences (DiD) approach. In the usual DiD strategy, a researcher would regress an outcome variable on the treatment variable (i.e., the interaction of a post-treatment indicator with the treatment status), controls, and firm and calendar time fixed effects. The coefficient on the treatment variable is an estimate of the treatment effect (i.e., the difference between the change in the outcome of treated firms and the change in the outcome of control firms over the same period). However, Baker et al. (2022) demonstrate that estimates produced by this strategy may be biased, especially when treatment is staggered and the treatment effect is heterogeneous across treatment cohorts. The stacked DiD estimator, meanwhile, is robust to these issues (Baker et al., 2022; Roth et al., 2023).

Specifically, for each of the twelve treatment events, I create a separate dataset, called a cohort, that captures the eleven-year window around the event (i.e., from five years before to five years after the treatment year). Each cohort includes treated firms (i.e., firms in the state that adopt the corresponding change) and control firms (i.e., firms that are never treated or treated after the cohort's window). The first cohort is for Vermont (2005), and the last is for Kentucky (2014). These cohorts are then "stacked" vertically, aligning the data based on treatment timing rather than actual calendar time.

On the stacked sample, I estimate the following baseline specification for firm *i* in state *s*, year *t*, and cohort *c*:

$$GDFComp_{i,s,t,c} = \beta_0 + \beta_1 * EnforceCNC_{s,t,c} + Controls_{i,s,t,c} + \gamma_{i,c} + \theta_{t,c} + \varepsilon_{i,s,t,c} \quad (5)$$

where $GDFComp$ is financial statement comparability from the procedure described in section 3.1; $EnforceCNC$ is an ordinal variable that equals 1 following an increase in CNC enforceability, -1 following a decrease in CNC enforceability, and 0 otherwise; $\gamma_{i,c}$ and $\theta_{t,c}$ denote cohort-firm fixed effects and cohort-year fixed effects, respectively. Following previous studies (De Franco et al., 2023; Francis et al., 2014; Young, 2023), I include the following control variables: *Size, Tobin's Q, Leverage*, and *Sales growth vol*. Additionally, I include *Sales growth*, *CAPEX*, and *Tangibility*. *Sales growth* and *CAPEX* may be indicators of growth firms, whose higher demand for capital raises may prompt them to report more comparable financial statements. Meanwhile, accounting treatments of intangible assets are often indefinite and subject to arbitrary judgments (Barker et al., 2022; Barth et al., 2001), so firms with low *Tangibility* may display lower comparability. Appendix A provides all variable definitions. Heteroskedasticity-robust standard errors are clustered at the firm level.

3.4. Sample

Fundamental data of listed firms between 2002 and 2022 are collected from the Compustat COMPUSTAT database. I restrict my sample to firms headquartered in the U.S. only. Following the accounting literature, I exclude firms from the financial (SIC code 6000-6999) and utility (SIC code 4900-4999) sectors. In addition, I follow De Franco et al. (2011) and exclude firms whose names contain “holding,” “adr,” “partnership,” “lp,” and “llp” to avoid comparing the accounting of parent companies to subsidiaries.

Table 1 provides summary statistics of the variables used in my empirical tests. Panel A shows statistics for unique firm-year observations included in the twelve cohorts before stacking. Panel B provides summary statistics for firm-year observations in the final stacked sample. Before stacking, all continuous variables are winsorized at the top and bottom 1% of their sample distributions. A row-by-row comparison of Panel A and Panel B reveals that the variables’ central tendencies, specifically the mean and median, show little difference before and after the staking process. Panel A shows that the mean GDFComp is -0.1860, while the median is -0.1317. These values are comparable to the values estimated by De Franco et al. (2011). Panel C of Table 1 reports the Pearson correlation coefficient matrix of the independent variables for observations in the stacked sample. The treatment assignment $EnforceCNC$ does not display significant correlations with control variables. Also, the variance inflation factors are much less than the threshold of 10. Thus, multicollinearity is not a prominent problem for statistical inferences.

Table 1. Summary Statistics

Panel A reports summary statistics for unique firm-year observations before stacking. Panel B reports summary statistics for the stacked sample. Panel C reports the Pearson correlation matrix of the independent variables in the stacked sample. The unit of observation is a firm-year. For each state-level change in CNC enforceability between 2002 and 2012, I create a cohort encompassing the eleven-year event window centered around the change. Each cohort includes treated firms (i.e., firms in the state that adopt the corresponding change) and control firms (i.e., firms that are never treated or treated after the cohort's window). Cohorts are then stacked vertically to form the stacked sample. All continuous variables are winsorized at the 1st and 99th percentiles. Appendix A provides all variable definitions. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Summary statistics for unique firm-year observations before stacking

	N	Mean	Std. Dev.	p25	Median	p75
GDFComp	43,684	-0.1860	0.2055	-0.2113	-0.1317	-0.0745
BarthComp	42,344	-1.2883	2.9876	-0.9748	-0.4032	-0.2114
EnforceCNC	43,684	0.0113	0.1103	0	0	0
Size	43,684	5.4226	2.5963	3.7170	5.5924	7.2681
Sales growth	43,684	1.2029	0.9776	0.9513	1.0652	1.2090
Tangibility	43,684	0.2215	0.2278	0.0548	0.1364	0.3086
Leverage	43,684	0.9381	3.0787	0.2984	0.4940	0.6966
CAPEX	43,684	0.0536	0.0877	0.0123	0.0277	0.0575
Tobin's Q	43,684	3.3537	13.3833	0.9018	1.3892	2.4330
Sales growth vol	43,684	0.7125	2.5063	0.0809	0.1695	0.3632
Capital growth	42,248	0.0669	0.4917	-0.0813	0.0465	0.1895

Panel B: Summary statistics for the stacked sample

	N	Mean	Std. Dev.	p25	Median	p75
GDFComp	298,150	-0.1724	0.1738	-0.1987	-0.1267	-0.0716
BarthComp	287,719	-1.2214	2.8771	-0.9251	-0.3953	-0.2111
EnforceCNC	298,150	0.0121	0.1169	0	0	0
Size	298,150	5.4129	2.5146	3.7290	5.5891	7.2435
Sales growth	298,150	1.1366	0.5260	0.9440	1.0619	1.2035
Tangibility	298,150	0.2204	0.2251	0.0549	0.1361	0.3072
Leverage	298,150	0.7243	1.2039	0.2955	0.4874	0.6886
CAPEX	298,150	0.0513	0.0690	0.0125	0.0280	0.0587
Tobin's Q	298,150	2.5986	5.1722	0.8843	1.3463	2.3319
Sales growth vol	298,150	0.4951	1.0935	0.0847	0.1720	0.3612
Capital growth	288,545	0.0590	0.4846	-0.0849	0.0442	0.1831

Panel C. Pearson correlation coefficients of independent variables in the baseline model									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	VIF
(1) *EnforceCNC*	1.000								1.00
(2) *Size*	0.038***	1.000							1.36
(3) *Sales growth*	-0.008***	-0.041***	1.000						1.19
(4) *Tangibility*	0.053***	0.221***	-0.009***	1.000					1.74
(5) *Leverage*	-0.006***	-0.391***	-0.010***	-0.004*	1.000				1.92
(6) *CAPEX*	0.032***	0.132***	0.165***	0.616***	-0.044***	1.000			1.71
(7) *Tobin's Q*	-0.017***	-0.409***	0.066***	-0.091***	0.669***	-0.021***	1.000		1.95
(8) *Sales growth vol*	-0.009***	-0.268***	0.347***	-0.022***	0.173***	0.049***	0.210***	1.000	1.24

4. The impact of labor mobility restrictions on comparability

4.1. Baseline results

Table 2 reports the stacked DiD estimates of the effect of increased state-level CNC enforceability on comparability with local peers, following specification (5). In column (1), I only include cohort-firm and cohort-year fixed effects to alleviate concerns about bad controls. In column (2), I include the firm-level control variables mentioned in section 3.3. In both columns, the coefficients on the variable of interest, $EnforceCNC$, are negative and statistically significant, consistent with my hypothesis that increased CNC enforceability is associated with lower financial statement comparability with local peers. In terms of economic significance, the coefficient of -0.0135 in column (2) means that when CNC enforceability strengthens, $GDFComp$ declines by 1.35% of total market value of equity. Panel A of Table 1 shows that the sample has a median $GDFComp$ value of -13.17% of total market value of equity. This means that strengthening CNC enforceability leads to a 10.25% decrease from the median comparability in our sample (10.25% = 0.0135/0.1317).

4.2. Robustness

I conduct additional tests to mitigate potential endogeneity issues, which may bias the estimated effect of increased mobility restrictions on comparability. Table 3 reports the results of these tests.

First, enforceability changes by new state statutes may not be exogenous. In our (stacked) DiD approach, an important assumption is that state-level changes to CNC enforceability are orthogonal to unobserved firm-level attributes. In Ewens and Marx (2018), the authors provide the background behind each law change and argue that they are all exogenous shocks to local labor mobility. However, compared to court rulings, legislative changes are more susceptible to lobbying and thus more likely to be endogenous to firm activities and outcomes. Therefore, I re-estimate the baseline model, this time dropping the four cohorts where the changes to CNC enforceability are not judicial decisions. These cohorts include Idaho (2008), Georgia (2010), Oregon (2008), and New Hampshire (2012). Column (1) of Table 3 reports the results of estimating equation (5) on the remaining cohorts. The coefficient on $EnforceCNC$ remains statistically significant and consistent with the hypothesis that restricting labor mobility lowers comparability.

Table 2. The effect of increased CNC enforceability on comparability with local peers

This table reports the stacked DiD estimates of the effect of increased CNC enforceability on financial statement comparability with local peers. Two firms are local peers if they share the same 2-digit SIC industry and headquarters states. The dependent variable is the measure of comparability with local peers, $GDFComp$. $EnforceCNC$ is an ordinal variable that equals 1 following an increase in CNC enforceability, -1 following a decrease in CNC enforceability, and 0 otherwise. All specifications include cohort-year fixed effects and cohort-firm fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. Appendix A provides all variable definitions. Heteroskedasticity-robust standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	GDFComp	
	(1)	(2)
EnforceCNC	-0.0121**	-0.0135***
	(0.0049)	(0.0049)
Size		0.0353***
		(0.0030)
Sales growth		-0.0033***
		(0.0010)
Tangibility		0.0167
		(0.0194)
Leverage		-0.0027**
		(0.0012)
CAPEX		0.0390*
		(0.0205)
Tobin's Q		0.0006**
		(0.0002)
Sales growth vol		-0.0005
		(0.0008)
Observations	298,150	298,150
R-squared	0.6633	0.6714
Cohort - Year FE	YES	YES
Cohort - Firm FE	YES	YES

Secondly, although judicial decisions are thought of as more impartial, we may not rule out the chance that judges' decisions could be influenced by political pressure. To alleviate this concern, I exclude from my sample all states with partisan judicial elections (i.e., states where judges are selected via election and the candidates' political affiliations are listed on the ballot). These states are Texas, Louisiana, Alabama, Illinois, Pennsylvania, and North Carolina (Bannon, 2018). I then re-run equation (5) and report the results in column (2) of Table 3. Again, the coefficient on $EnforceCNC$ remains negative and statistically significant.

Another source of endogeneity could be that the observed effect is not due to shocks to labor mobility but to omitted regional economic, legal, or political trends that correlate with these shocks. To mitigate this concern, I only keep states that border the treated states to qualify as control since adjacent states presumably face similar regional conditions. Column (3) of Table 3 reports the results and shows that the baseline finding is also robust to this adjustment.

There is also a concern that variations in firm characteristics between the treated and control groups might influence the observed effect. Hence, I re-estimate the baseline model on a propensity score-matched sample, balancing pre-treatment covariates between the treated and control groups. To do this, within each cohort, I retain all treated and control observations in the year before the treatment year. I then match each treated firm with a control firm using the nearest-neighbor propensity score method with a 0.1 caliper, matching on covariates used in the baseline model (i.e., *Size, Sales growth, Tangibility, Leverage, CAPEX, Tobin's Q,* and *Sales growth vol*). Results of re-estimating equation (5) on this matched sample are reported in column (4) of Table 3. The coefficient on $EnforceCNC$ is negative and significant, consistent with the hypothesis. Moreover, the estimate of -0.0241 on the matched sample is nearly double in magnitude compared to the estimate on the unmatched sample, i.e., -0.0135 in column (2) of Table 2.

Finally, to make sure that the observed effect is not exclusive to $GDFComp$, I re-estimate equation (5) using another measure of comparability, $BarthComp$. The estimation of $BarthComp$ follows the same procedure as $GDFComp$. However, instead of using equation (1) to operationalize a firm's accounting system, I use the following equation, introduced by Barth et al. (2012):

$$Return_{i,t} = \alpha_i + \beta_{1,i} * Earnings_{i,t} + \beta_{2,i} * \Delta Earnings_{i,t} + \beta_{3,i} * LOSS_{i,t} \\ + \beta_{4,i} * \left(LOSS_{i,t} * Earnings_{i,t}\right) + \beta_{5,i} * \left(LOSS_{i,t} * \Delta Earnings_{i,t}\right) + \varepsilon_{i,t} \quad (6)$$

where $Return$ is the buy-and-hold stock return during the quarter, $Earnings$ is the quarterly net income before extraordinary items scaled by the total market value of equity and, $\Delta Earnings$ is the quarterly change in $Earnings$, and $LOSS$ is an indicator variable that equals 1 if $Earnings$ is negative, and 0 otherwise.

I regress $BarthComp$ against CNC enforceability using the same specification as in equation (5). Column (5) of Table 3 shows the result of this test. The coefficient on $EnforceCNC$ is -0.2456 and is statistically significant, in line with the baseline finding that increased CNC enforceability reduces comparability. Moreover, Panel A of Table 1 shows that the median $BarthComp$ of the sample is -40.32 percentage point. Thus, a 24.56 percentage point decrease in $BarthComp$ is a 60.91% decrease from the median observation (60.91% = 0.2456/0.4032), suggesting that the impact is economically significant.

Table 3. Robustness tests

This table reports the stacked DiD estimates of the effect of increased CNC enforceability on financial statement comparability with local peers. Two firms are local peers if they share the same 2-digit SIC industry and headquarters states. In column (1), I drop the four treatment cohorts where changes to CNC enforceability are not judicial decisions. In column (2), I drop all firms in states with partisan judicial elections. In column (3), I only include states that border the treated states in the control group. In column (4), treated and control firms are nearest-neighbor propensity score-matched with a 0.1 caliper, matching on pre-treatment *Size*, *Sales growth*, *Tangibility*, *Leverage*, *CAPEX*, *Tobin's Q*, and *Sales growth vol*). The dependent variable in columns (1) to (4) is $GDFComp$. In column (5), the dependent variable is $BarthComp$. $EnforceCNC$ is an ordinal variable that equals 1 following an increase in CNC enforceability, -1 following a decrease in CNC enforceability, and 0 otherwise. All specifications include cohort-year fixed effects and cohort-firm fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. Appendix A provides all variable definitions. Heteroskedasticity-robust standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	GDFComp				BarthComp
	(1)	(2)	(3)	(4)	(5)
	Drop treatment cohorts with nonjudicial CNC changes	Drop states with partisan judicial elections	Neighboring states as control states	Propensity score matched sample	Stacked sample
EnforceCNC	-0.0158***	-0.0162**	-0.0102**	-0.0241***	-0.2456***
	(0.0054)	(0.0075)	(0.0048)	(0.0081)	(0.0612)
Size	0.0345***	0.0343***	0.0344***	0.0472***	0.4261***
	(0.0030)	(0.0034)	(0.0033)	(0.0069)	(0.0502)
Sales growth	-0.0034***	-0.0030***	-0.0042***	0.0010	-0.0142
	(0.0010)	(0.0011)	(0.0010)	(0.0020)	(0.0214)
Tangibility	0.0153	0.0293	0.0161	-0.0022	0.3364
	(0.0193)	(0.0218)	(0.0201)	(0.0324)	(0.3006)
Leverage	-0.0027**	-0.0035***	-0.0026*	-0.0014	-0.0181
	(0.0012)	(0.0014)	(0.0014)	(0.0025)	(0.0192)
CAPEX	0.0501**	0.0009	0.0563***	0.0934***	0.1097
	(0.0201)	(0.0239)	(0.0207)	(0.0332)	(0.3609)
Tobin's Q	0.0005**	0.0006**	0.0004*	0.0015*	0.0053
	(0.0002)	(0.0003)	(0.0002)	(0.0008)	(0.0036)
Sales growth vol	-0.0004	-0.0011	-0.0003	0.0004	-0.0109
	(0.0008)	(0.0008)	(0.0008)	(0.0010)	(0.0151)
Observations	183,600	228,855	248,222	12,318	287,719
R-squared	0.6740	0.6754	0.6808	0.5896	0.5529
Cohort - Year FE	YES	YES	YES	YES	YES
Cohort - Firm FE	YES	YES	YES	YES	YES

4.3. Dynamic effects

Another important assumption for the unbiasedness of the (stacked) DiD estimator is that without the treatment, the trends in comparability between treated and control groups would have been the same through time, i.e., the parallel-trend assumption. To find indirect evidence supporting this assumption, I estimate the following equation for company *i* in state *s*, year *t*, and cohort *c*:

$$GDFComp_{i,s,t,c} = \beta_0 + \sum_{k=-5}^{5} \beta_1^k * EnforceCNC_{s,t,c}^k + Controls_{i,s,t,c} + \gamma_{i,c} + \theta_{t,c} + \varepsilon_{i,s,t,c} \quad (7)$$

where $EnforceCNC^k$ equals 1 if CNC enforceability increases k years from now, -1 if CNC enforceability decreases k years from now, and 0 otherwise. This specification is the dynamic version of equation (5). It allows us to observe whether there are significant differences between the comparability trends of the treated and control groups before the treatment is effective, as well as the trajectory of any treatment effect once the treatment is applied. I estimate equation (7) on the stacked sample and the propensity score-matched stacked samples, as described in section 4.2.

Figure 2 plots the coefficient estimates on $EnforceCNC^k$ against k (i.e., the number of years to treatment), for k from -5 to 5. Estimates on the unmatched sample (red) show that the treated and control groups exhibit different comparability trends in years -5 and -4. However, this difference is no longer significant in the years nearer the treatment. Meanwhile, on the propensity score-matched sample (blue), no pre-treatment coefficient estimates are significantly different from zero. Hence, assuming that the parallel-trend assumption holds conditionally on the covariates is reasonable. Figure 2 also shows the dynamics of the treatment effect. The post-treatment coefficient estimates in both samples display an increase in magnitude up to five years following the treatment, suggesting that the effect is significant and not transient.

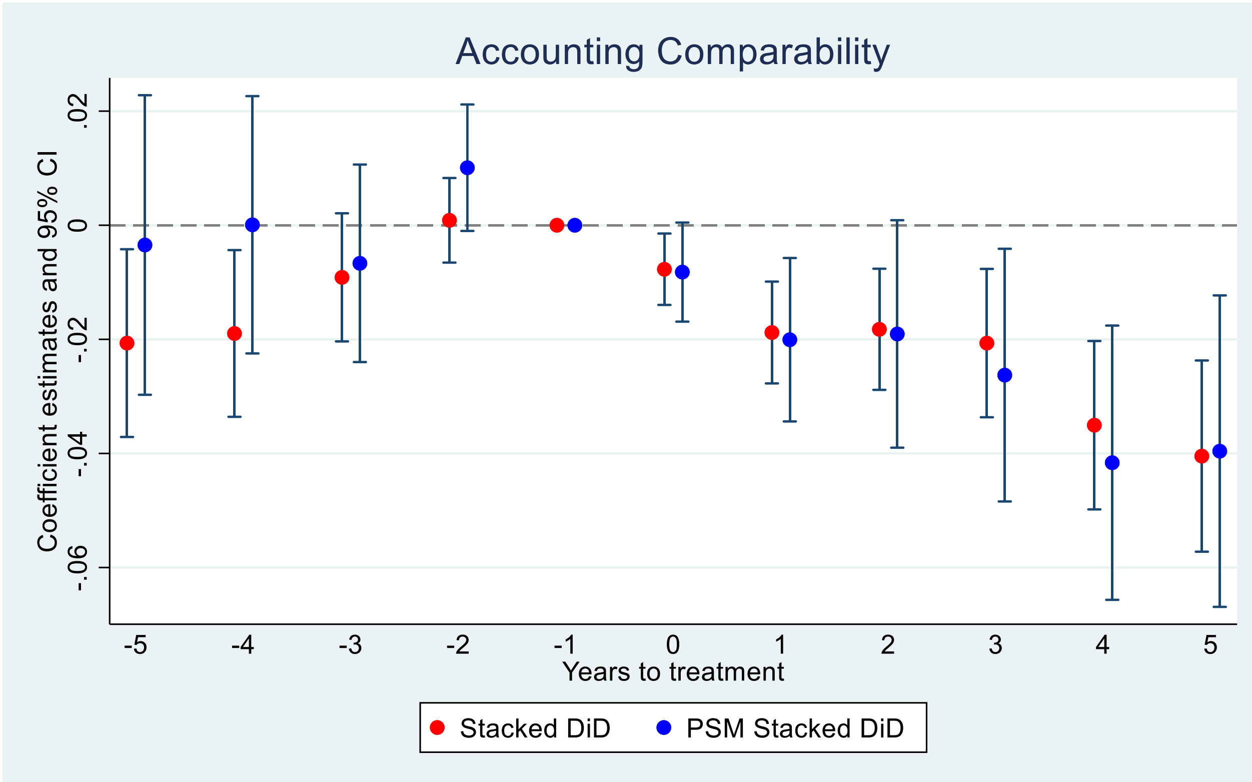

Figure 2. Dynamic treatment effects. This figure plots the dynamic stacked DiD estimates of the impact of increased CNC enforceability on financial statement comparability with local peers. Estimates on the stacked sample are plotted in red, and estimates on the propensity score-matched sample are plotted in blue. The year before the effective change in CNC enforceability serves as the base and is omitted. The dependent variable is the measure of comparability with local peers, $GDFComp$. All continuous variables are winsorized at the 1st and 99th percentiles. Intervals are 95% confidence intervals. Heteroskedasticity-robust standard errors are clustered at the firm level.

4.4. Firm-pair level evidence

In this test, instead of aggregating the pairwise $GDFComp$ to arrive at a firm-year panel, I retain the pairwise $GDFComp$ measures and estimate the effect of CNC enforceability on comparability in a firm pair-year panel. I apply the stacked DiD approach and estimate the following equation:

$$GDFComp_{ij,s,t,c} = \beta_0 + \beta_1 * EnforceCNC_{s,t,c} + Controls_{ij,s,t,c} + \gamma_{ij,c} + \theta_{t,c} + \varepsilon_{i,s,t,c} \quad (8)$$

where $GDFComp_{ij}$ is pairwise financial statement comparability of firm i and local peer j. The treatment variable, $EnforceCNC$, is an ordinal variable that equals 1 following an increase in

CNC enforceability, -1 following a decrease in CNC enforceability, and 0 otherwise. $\gamma_{ij,c}$ and $\theta_{t,c}$ denote cohort-firm-pair fixed effects and cohort-year fixed effects, respectively. Following previous studies on the comparability of firm pairs (De Franco et al., 2023; Francis et al., 2014), the set of control variables for this specification includes the absolute differences between i and j in *Size, Sales growth, Tangibility, Leverage, CAPEX, Tobin's Q,* and *Sales growth vol,* as well as the minimum values of these variables in the i-j pair. Heteroskedasticity-robust standard errors are clustered at the firm pair level.

Table 4 reports the results of estimating equation (8). In columns (1) and (2), where the comparability measure is $GDFComp$, the coefficient estimates on *EnforceCNC* are -0.0333 without controls and -0.0378 with controls, respectively. The estimates are statistically and economically significant – an effect of -3.78% of lagged total market value of equity in column (2) suggests that stronger CNC enforceability leads to a 44.11% decrease in comparability from the median $GDFComp$ value of -8.57% of lagged total market value of equity in the firm pair sample, as shown in Appendix B (44.11% = 0.0378/0.0857).

5. Cross-sectional tests

Labor-based knowledge spillover is intuitively sensitive to labor mobility. However, some factors may affect how strong this sensitivity is. If labor-based knowledge spillover is indeed a causal channel of the tie between mobility and financial statement comparability, these factors could moderate the extent to which comparability responds to stronger mobility restrictions. In this section, I conduct additional tests to see if this is the case.

Table 4. Firm pair-level evidence

This table reports the stacked DiD estimates of the effect of increased CNC enforceability on pairwise financial statement comparability of local peers. Two firms are local peers if they share the same 2-digit SIC industry and headquarters state. The dependent variable is *GDFComp*. *EnforceCNC* is an ordinal variable that equals 1 following an increase in CNC enforceability, -1 following a decrease in CNC enforceability, and 0 otherwise. All specifications include cohort-year and cohort-pair fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. Appendix A provides all variable definitions. Heteroskedasticity-robust standard errors are clustered at the firm pair level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Pairwise GDFComp	
	(1)	(2)
EnforceCNC	-0.0333***	-0.0378***
	(0.0016)	(0.0015)
d(Size)		0.0214***
		(0.0009)
d(Sales growth)		-0.0011***
		(0.0001)
d(Tangibility)		0.1028***
		(0.0043)
d(Leverage)		-0.0013***
		(0.0001)
d(CAPEX)		0.0077***
		(0.0020)
d(Tobin's Q)		0.0005***
		(0.0000)
d(Sales growth vol)		-0.0001***
		(0.0000)
Min(Size)		0.0620***
		(0.0010)
Min(Sales growth)		0.0007
		(0.0006)
Min(Tangibility)		0.1195***
		(0.0059)
Min(Leverage)		-0.0227***
		(0.0023)
Min(CAPEX)		0.1662***
		(0.0064)
Min(Tobin's Q)		0.0013***
		(0.0003)
Min(Sales growth vol)		0.0024***
		(0.0006)
Observations	6,967,432	6,967,432
R-squared	0.7315	0.7430
Cohort - Year FE	YES	YES
Cohort - Pair FE	YES	YES

First, I project that the impact of increased CNC enforceability on financial statement comparability is stronger for industry followers (as opposed to industry leaders). Followers presumably face less scrutiny from the capital market, making them more willing to adjust their accounting systems when incentives are introduced. More importantly, knowledge spillovers typically flow from leaders to followers, making the latter more intensive learners (Blazsek & Escribano, 2016; Eeckhout & Jovanovic, 2002). Thus, the amount of comparability explained by learning through hiring could be more sensitive to labor mobility among industry followers.

I test this prediction by adding to equation (4) the indicator variable $Follower$ and its interaction with *EnforceCNC*. $Follower$ equals 1 if a firm's price-cost margin falls within the bottom quartile (bottom 25%) of all firms in its 2-digit SIC industry and headquarters state, and 0 otherwise. By this construct, $Follower$ indicates that a firm has less product market power than its local peers and thus has more incentives to learn from them. The use of price-cost margin to identify industry followers follows De Franco et al. (2023) and Kubick et al. (2015). The results of estimating this specification are reported in column (1) of Table 5. The coefficient on the interaction term $EnforceCNC * Follower$ is negative and significant, in line with my prediction that CNC enforceability's impact on financial statement comparability is more substantial for industry followers.

I also explore how the density of the local peer networks that firms belong to modulates how comparability reacts to CNC enforceability. I define a local peer network as a set of all firms sharing the same 2-digit SIC industry and headquarters state, and its density is the number of firms located in it. Within a denser local peer network, learning is more frequent because firms are exposed to knowledge from more peers. Moreover, Meagher & Rogers (2004) formalize the idea

Table 5. Cross-sectional effects

This table reports the stacked triple-difference estimates of the effect of increased CNC enforceability on financial statement comparability with local peers. Two firms are local peers if they share the same 2-digit SIC industry and headquarters states. The dependent variable is the measure of comparability with local peers, $GDFComp$. $EnforceCNC$ is an ordinal variable that equals 1 following an increase in CNC enforceability, -1 following a decrease in CNC enforceability, and 0 otherwise. In column (1), I interact $EnforceCNC$ with $Follower$. $Follower$ equals 1 if a firm's price-cost margin falls within the bottom quartile (bottom 25%) of all firms in its 2-digit SIC industry and headquarters state, and 0 otherwise. In column (2), I interact $Treat * Post$ with $Dense$. $Dense$ equals 1 if the density of a firm's local peer network falls within the top quartile (top 25%) of all local peer networks in its 2-digit SIC industry, and 0 otherwise. All specifications include cohort-year fixed effects and cohort-firm fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. Appendix A provides all variable definitions. Heteroskedasticity-robust standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	GDFComp	
	(1)	(2)
EnforceCNC	-0.0108**	-0.0014
	(0.0048)	(0.0087)
Follower	-0.0012	
	(0.0033)	
*EnforceCNC*Follower*	-0.0195**	
	(0.0092)	
Dense		-0.0013
		(0.0068)
*EnforceCNC*Dense*		-0.0269**
		(0.0124)
Size	0.0353***	0.0349***
	(0.0030)	(0.0035)
Sales growth	-0.0034***	-0.0031***
	(0.0010)	(0.0010)
Tangibility	0.0168	0.0258
	(0.0194)	(0.0190)
Leverage	-0.0027**	-0.0027**
	(0.0012)	(0.0011)
CAPEX	0.0388*	0.0408
	(0.0205)	(0.0263)
Tobin's Q	0.0006**	0.0005**
	(0.0002)	(0.0002)
Sales growth vol	-0.0005	-0.0006
	(0.0008)	(0.0007)
Observations	298,150	292,406
R-squared	0.6714	0.6754
Year FE	YES	YES
Firm FE	YES	YES

that denser firm networks have a higher level of aggregate innovativeness (i.e., the ratio of firms in a network that innovate). This finding suggests that learning in a denser local peer network is also more impactful since firms are more likely to encounter diverse perspectives and novel ideas. Therefore, I predict that in denser local peer networks, mobility restrictions would have a greater effect on knowledge spillover intensity and, thus, the amount of comparability explained by knowledge spillovers.

To test this prediction, I add to equation (4) the indicator variable $Dense$ and its interaction with $EnforceCNC$. $Dense$ equals 1 if the density of a firm's local peer network falls within the top quartile (top 25%) of all local peer networks in its 2-digit SIC industry, and 0 otherwise. The results of estimating this specification are reported in column (1) of Table 5. The coefficient on the interaction term $EnforceCNC * Dense$ is negative and significant. This result indicates that the impact of labor mobility on comparability is stronger for firms surrounded by more local peers.

6. The economic consequences of lower financial statement comparability as restrictions on mobility strengthen

Having observed that stricter CNC enforcement depresses financial statement comparability among local peers, I explore the economic consequences of this decline in comparability. As discussed in section 2.2, comparability benefits investors by resolving information uncertainty around their investments. Previous studies show that a higher level of comparability is associated with a lower cost of capital (Imhof et al., 2017) and higher equity capital growth (Fiechter et al., 2024). This leads us to question whether the fall in comparability due to increased CNC enforceability is significant enough to affect capital acquisition and subsequent investment. To answer this question, I estimate the following model:

$$y_{i,s,t,c} = \beta_0 + \beta_1 * EnforceCNC_{s,t,c} + Controls_{i,s,t,c} + \gamma_{i,c} + \theta_{t,c} + \varepsilon_{i,s,t,c} \quad (9)$$

where *y* is *Capital Growth* (natural logarithm of annual growth of invested capital, where invested capital includes equity capital and long-term debt) or *CAPEX* (total capital expenditures scaled by total assets). A lower *Capital Growth* means it is more challenging for firms to acquire new capital, while a decline in *CAPEX* suggests that firms reduce their capital expenditure intensity. Panel A of Table 6 reports the results of these tests. The coefficients on $EnforceCNC$ are negative and statistically significant in both columns (1) and (2), where the dependent variables are *Capital Growth* and *CAPEX*, respectively. These results show that stronger CNC enforceability is associated with weaker capital acquisition, which leads to weaker capital expenditure intensity.

To provide more robust evidence that declining comparability explains, at least partially, the effect above, I follow the procedure in Neel (2017). Specifically, I first rank treated firms based on their changes in comparability following the treatment and sort them into quartiles. I calculate a treated firm's change in comparability following the treatment as its mean *GDFComp* during the post-treatment period minus its mean *GDFComp* during the pre-treatment period. I then estimate the following equations:

$$y_{i,s,t,c} = \beta_0 + \sum_{k=1}^{4} \beta_1^k * \left(EnforceCNC_{s,t,c} * \Delta CompQ_{i,c}^k\right) + \sum_{k=1}^{4} \beta_2^k * \Delta CompQ_{i,c}^k + Controls_{i,s,t,c} + \theta_{t,c} + \varepsilon_{i,s,t,c} \quad (10)$$

where $\Delta CompQ^1$ is an indicator variable that equals 1 if a treated firm belongs to the first (bottom) quartile of change in comparability among all treated firms and 0 otherwise. $\Delta CompQ^2$, $\Delta CompQ^3$, *and* $\Delta CompQ^4$ are defined similarly. Following treatment, treated firms in $\Delta CompQ^1$ experience the largest decreases in comparability, whereas treated firms in $\Delta CompQ^4$ experience the largest increases in comparability. The variable $EnforceCNC$ in equation (9) is then replaced by $EnforceCNC * \Delta CompQ^1$, $EnforceCNC * \Delta CompQ^2$, $EnforceCNC * \Delta CompQ^3$, *and* $EnforceCNC * \Delta CompQ^4$. This approach enables us to assess how restricting labor mobility affects capital growth and capital expenditure intensity at varying levels of comparability change following the treatment.

Panel B of Table 6 reports the results of this test. In both specifications for *Capital Growth* and *CAPEX*, the coefficients of the interaction terms decrease in magnitude as we go from the 1st to the 4th quartile range. Moreover, the coefficients on $EnforceCNC * \Delta CompQ^1$ are statistically significant, while the coefficients on $EnforceCNC * \Delta CompQ^4$ are not. These results imply that the adverse effects of stronger CNC enforceability on capital acquisition and investment intensity are much more pronounced for firms that experience the greatest declines in comparability. I also test for the differences between the coefficients on $EnforceCNC * \Delta CompQ^1$ and $EnforceCNC * \Delta CompQ^4$. The results, also shown in Panel B, confirm that the coefficients on $EnforceCNC * \Delta CompQ^1$ are significantly more negative (the p-value is 0.0318 on *Capital Growth* and 0.0013 on *CAPEX)*. Overall, these tests provide evidence that increased CNC enforceability adversely impacts capital acquisition and investment intensity and that decreased financial statement comparability is an important driver of this effect.

7. Conclusion

The literature widely documents how labor mobility restrictions, such as the enforcement of CNCs, can significantly impact economic outcomes at the firm level. While useful for firms in protecting their proprietary intangible assets, these restrictions can hinder knowledge spillovers between firms. This study enhances our understanding of the real effects of labor mobility restrictions by showing how they affect financial statement comparability, an essential characteristic that contributes to accounting information's usefulness. I hypothesize that financial statement comparability diminishes when labor mobility is more restricted. I suspect this is because labor mobility restrictions mitigate the transfer of accounting practices along labor flows and the adoption of similar accounting choices explained by labor-based knowledge spillovers.

Table 6. The economic consequences of lower financial statement comparability as restrictions on labor mobility strengthen

Panel A reports the stacked DiD estimates of the effect of increased CNC enforceability on capital growth and capital expenditure intensity. The dependent variable is *Capital Growth* in column (1) and *CAPEX* in column (2). *EnforceCNC* is an ordinal variable that equals 1 following an increase in CNC enforceability, -1 following a decrease in CNC enforceability, and 0 otherwise. Coefficients on controls are omitted for brevity. Specifications include cohort-year fixed effects and cohort-firm fixed effects.

Panel B reports the stacked DiD estimates of the effect of increased CNC enforceability on capital growth and capital expenditure intensity, conditional on the change in comparability. The dependent variable is *Capital Growth* in column (1) and *CAPEX* in column (2). To examine the role of comparability, I rank and sort treated firms into quartiles of their post-pretreatment changes in comparability. $\Delta CompQ^1$, $\Delta CompQ^2$, $\Delta CompQ^3$, $\Delta CompQ^4$ are indicator variables that equal 1 if a treated firm belongs to the 1st, 2nd, 3rd, and 4th quartile of post-pretreatment changes in comparability of all treated firms, and 0 otherwise. I then interact *EnforceCNC* with these indicator variables. Specifications include cohort-year fixed effects. Panel B also reports p-values for tests on the difference between the coefficients on $EnforceCNC{*}\Delta CompQ^1$ and $EnforceCNC{*}\Delta CompQ^4$.

All continuous variables are winsorized at the 1st and 99th percentiles. Appendix A provides all variable definitions. Heteroskedasticity-robust standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Effect of increased CNC enforceability on Capital growth and CAPEX		
	(1) Capital Growth	(2) CAPEX
EnforceCNC	-0.0281***	-0.0056***
	(0.0095)	(0.0020)
Observations	286,420	296,816
R-squared	0.2839	0.6521
Control	YES	YES
Cohort - Year FE	YES	YES
Cohort - Firm FE	YES	YES

Panel B: The role of comparability		
	(1) Capital Growth	(2) CAPEX
$\Delta CompQ^1$	-0.0150	0.0302***
	(0.0149)	(0.0059)
$\Delta CompQ^2$	0.0239***	0.0024
	(0.0092)	(0.0042)
$\Delta CompQ^3$	0.0119	-0.0083**
	(0.0123)	(0.0033)
$\Delta CompQ^4$	0.0391**	-0.0059*
	(0.0155)	(0.0032)
$EnforceCNC*\Delta CompQ^1$	-0.0716***	-0.0229***
	(0.0203)	(0.0066)
$EnforceCNC*\Delta CompQ^2$	-0.0469***	-0.0034
	(0.0140)	(0.0036)
$EnforceCNC*\Delta CompQ^3$	-0.0266*	0.0010
	(0.0147)	(0.0030)
$EnforceCNC*\Delta CompQ^4$	-0.0186	0.0007
	(0.0202)	(0.0039)
p-value for test of difference in coefficients $H_0 = \beta_{EnforceCNC*\Delta CompQ1} \geq \beta_{EnforceCNC*\Delta CompQ4}$	0.0318	0.0013
Observations	287,287	296,816
R-squared	0.0876	0.3466
Control	YES	YES
Cohort - Year FE	YES	YES

I find robust evidence that after a state increases CNC enforceability, the financial statements of industry peers within that state become less comparable. I also find empirical support for my prediction that knowledge spillovers, especially learning through hiring, is a mechanism of this impact. Specifically, comparability is more sensitive to labor mobility for firms with less product market power than local peers and for firms in denser local peer networks. Finally, due to less comparable financial statements, firms subject to stronger mobility restrictions witness declining capital acquisition and investment intensity.

This study has significant practical implications. First, it calls for regulators' attention to the possible side effects of restricting labor mobility on financial disclosure and firm growth. Second, by re-emphasizing the value of comparability for firms and investors, this study stresses the need for further improvements in accounting standards and disclosure requirements to foster this qualitative characteristic. Third, this study shows that when mobility is more restricted, firms may face more challenges in accessing capital and maintaining investment, partly because their financial statements become less comparable to those of peers. This risk is more grave for industry followers and firms with a denser local peer network. To mitigate this effect, these firms should take steps to enhance transparency and internal audit quality to ensure their disclosure is informative to investors.

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APPENDIX

Appendix A. Variable definitions

Panel A: Variables in the firm-year panel	
GDFComp	Firm-level financial statement comparability measure, calculated as the mean of all pairwise *GDFComp* between firm *i* and its local peers *j*. Parwise *GDFComp* is measured using the method of De Franco et al. (2011) and the accounting system proxy of De Franco et al. (2011).
BarthComp	Firm-level financial statement comparability measure, calculated as the mean of all pairwise *BarthComp* between firm *i* and its local peers *j*. Parwise *BarthComp* is measured using the method of De Franco et al. (2011) and the accounting system proxy of Barth et al. (2012).
EnforceCNC	Ordinal variable that equals 1 following an increase in CNC enforceability, -1 following a decrease in CNC enforceability, and 0 otherwise
Size	Natural logarithm of total assets
Sales growth	Total sales divided by lagged total sales
Tangibility	Total net property plant and equipment scaled by total assets
Leverage	The sum of total long-term debts and total short-term debts, divided by total assets
CAPEX	Total capital expenditures scaled by total assets
Tobin's Q	The sum of total debts and total market value of equity divided by total assets
Sales growth vol	The standard deviation of *Sales growth* over the last three years
Panel B: Control variables in the firm pair-year panel	
d(Size)	The absolute value of the difference in *Size* between focal firm *i* and local peer *j*
d(Sales growth)	The absolute value of the difference in *Sales growth* between focal firm *i* and local peer *j*
d(Tangibility)	The absolute value of the difference in *Tangibility* between focal firm *i* and local peer *j*
d(Leverage)	The absolute value of the difference in *Leverage* between focal firm *i* and local peer *j*
d(CAPEX)	The absolute value of the difference in *CAPEX* between focal firm *i* and local peer *j*
d(Tobin's Q)	The absolute value of the difference in *Tobin's Q* between focal firm *i* and local peer *j*
d(Sales growth vol)	The absolute value of the difference in *Sales growth vol* between focal firm *i* and local peer *j*
Min(Size)	The minimum value of *Size* between focal firm *i* and local peer *j*
Min(Sales growth)	The minimum value of *Sales growth* between focal firm *i* and local peer *j*

Min(Tangibility)	The minimum value of *Tangibility* between focal firm *i* and local peer *j*
Min(Leverage)	The minimum value of *Leverage* between focal firm *i* and local peer *j*
Min(CAPEX)	The minimum value of *CAPEX* between focal firm *i* and local peer *j*
Min(Tobin's Q)	The minimum value of *Tobin's Q* between focal firm *i* and local peer *j*
Min(Sales growth vol)	The minimum value of *Sales growth vol* between focal firm *i* and local peer *j*
Panel C: Variables used in additional tests	
Follower	Indicator variable that equals 1 if a firm's price-cost margin falls within the bottom quartile (bottom 25%) of all firms in its 2-digit SIC industry and headquarters state, and 0 otherwise. Price-cost margin is total sales minus cost of goods sold and selling, general, and administrative expenses, divided by total sales.
Dense	Indicator variable that equals 1 if the density of a firm's local peer network falls within the top quartile (top 25%) of all local peer networks in its 2-digit SIC industry, and 0 otherwise. A local peer network is a set of all firms in the same headquarters state and 2-digit SIC industry, and its density is the number of firms located in it.
Capital growth	Natural logarithm of total invested capital divided by one-year-lagged total invested capital
$\Delta CompQ^{1}$, Δ*CompQ2, *Δ*CompQ3, *Δ*CompQ4*	Indicator variable that equals 1 for a treated firm if its change in *GDFComp* is in the first, second, third, and fourth quarter across all treated firms. Change in *GDFComp* is the average *GDFComp* during the post-treatment period minus the average *GDFComp* during the pre-treatment period

Appendix B. Summary statistics for the stacked firm pair sample

Panel B reports summary statistics for the stacked firm pair sample. The unit of observation is a pair-year. For each state-level change in CNC enforceability between 2002 and 2012, I create a cohort encompassing the eleven-year event window centered around the change. Each cohort includes treated pairs (i.e., pairs of industry peers in the state that adopt the corresponding change) and control pairs (i.e., pairs in states that are never treated or treated after the cohort's window). Cohorts are then stacked vertically to form the stacked sample. All continuous variables are winsorized at the 1st and 99th percentiles. Appendix A provides all variable definitions.

	N	Mean	Std. Dev.	p25	Median	p75
GDFComp	6,967,432	-0.1669	0.2359	-0.1917	-0.0857	-0.0353
BarthComp	6,439,488	-0.9510	2.5444	-0.6271	-0.288	-0.1542
EnforceCNC	6,967,432	0.0114	0.1071	0	0	0
d(Size)	6,967,432	2.6448	2.0862	1.0059	2.1629	3.8081
d(Sales growth)	6,967,432	0.9396	3.4778	0.1167	0.2774	0.604
d(Tangibility)	6,967,432	0.1386	0.1619	0.0319	0.0806	0.1776
d(Leverage)	6,967,432	1.4525	7.0616	0.114	0.2591	0.5102
d(CAPEX)	6,967,432	0.0718	0.1612	0.0097	0.0248	0.0625
d(Tobin's Q)	6,967,432	4.3906	23.4176	0.3913	0.9771	2.317
d(Sales growth vol)	6,967,432	2.5314	11.3471	0.0773	0.2056	0.6388
Min(Size)	6,967,432	3.9846	2.2598	2.5804	4.1227	5.5611
Min(Sales growth)	6,967,432	0.9225	0.3631	0.7856	0.9719	1.0924
Min(Tangibility)	6,967,432	0.1452	0.2185	0.0236	0.0534	0.13
Min(Leverage)	6,967,432	0.3340	0.2332	0.1681	0.2885	0.4499
Min(CAPEX)	6,967,432	0.0346	0.0614	0.0059	0.0144	0.0306
Min(Tobin's Q)	6,967,432	1.4127	1.2331	0.7775	1.1037	1.6584
Min(Sales growth vol)	6,967,432	0.2526	0.5947	0.0779	0.1463	0.2563