Worker Heterogeneity and the Effect of Non-Competes on Firm Performance

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April 2026

Abstract

Using staggered state-level changes in non-compete enforceability, we document that reduced enforcement increases profitability, valuation, productivity, and plant-level growth in knowledge-worker-intensive firms relative to other firms. Critically, these gains are concentrated among the most productive knowledge-worker firms, consistent with an assortative matching mechanism in which top firms attract the most productive workers when non-competes no longer bind. Using inventor-level data, we find direct evidence of this sorting: more productive inventors migrate toward more productive firms following reductions in enforceability. Consequently, performance dispersion widens across knowledge-worker-intensive industries, as the most productive firms pull further ahead of their less productive peers.

Keywords: Assortative Matching, Firm Performance, Knowledge Workers, Non-competes, Labor Mobility, Productivity Dispersion

JEL Classifications: D61, G30, J24, J31, J41, J61, K31, O34

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1. Introduction

The economic implications of labor mobility have received renewed attention from academics, the business press, and policymakers (e.g., Treasury, 2016; White House, 2016; Federal Trade Commission, 2023, 2024). A fluid labor market facilitates efficient labor allocation while enhancing the quality of matching between workers and firms, all leading to more productive use of resources. The ability to change jobs also stimulates a worker's incentives to improve her own human capital, which in turn raises her productivity (Amir and Lobel, 2014; Treasury, 2016). Yet such ability creates holdup problems due to the portability of human capital (Becker, 1962), which may discourage firms from investing in innovation and their employees (Hart and Moore, 1990, 1994), reducing the societal benefit of unfettered mobility. This tension is particularly acute in knowledge-intensive companies that rely on employees' incentives for developing groundbreaking ideas essential for sustaining growth and market-leading positions.

The simultaneously positive and negative virtues of restricting mobility raise two important questions that we believe prior work has not fully addressed. First, are the firm-level effects of changing labor mobility concentrated among firms that rely most heavily on knowledge workers, for whom the trade-offs inherent in non-competes are most acute? Second, and more novel, what mechanisms account for any such differential effects, and can we distinguish between them empirically? We address both questions by exploiting state-level changes in the enforceability of non-competes together with cross-sectional variation in firms' reliance on knowledge workers. We predict, and find, that reducing barriers to labor mobility generates disproportionate performance gains in knowledge-worker-intensive firms. Critically, we further predict and find that these gains are most pronounced among the most productive firms within that group, a pattern most consistent with assortative matching, in which top firms attract the best workers when non-competes no longer bind. This reallocation of talent in turn widens the dispersion of performance and productivity across knowledge-worker-intensive industries.

To restrain worker mobility, firms frequently require employees to sign post-employment contracts containing restrictive clauses. Non-compete agreements, arguably the most powerful binding covenants (Marx, 2011), prohibit departing employees from joining competitors or starting a competing firm within specific regions for a specified period. A growing body of research and survey evidence documents that non-competes are ubiquitous in the U.S., with nearly a fifth of U.S. workers subject to a non-compete (Starr, Prescott, and Bishara, 2021), and that these covenants are highly effective in restricting the mobility of highly skilled workers.[1] Due to mounting concerns over anti-competitive practices in labor markets and wage stagnation, over four dozen legislative bills have been introduced in 21 states to reform non-competes in recent years. Furthermore, the Federal Trade Commission proposed a rule to ban non-compete clauses in January 2023 and passed the rule in April 2024.[2]

While prior academic research has largely focused on the average effect of non-competes on *all* firms, we focus squarely on *cross-sectional* variation *across* firms, and specifically on why we expect reducing non-compete enforceability to benefit knowledge-worker-intensive firms. Our prediction follows from two theoretical mechanisms that operate in the same direction but through distinct channels. The first is a human capital incentive effect. Knowledge workers are, generally, those whose work is non-routine, or non-linear, and who often need to apply insights across many

[1] See Garmaise (2011) for the impact on corporate executives, Fallick, Fleischman, and Rebitzer (2006) on technology workers, and Marx, Strumsky, and Fleming (2009) on patent-holding inventors.
[2] The 21 states are Florida, Hawaii, Illinois, Indiana, Iowa, Kentucky, Massachusetts, Missouri, New Hampshire, New Jersey, New York, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, Tennessee, Vermont, Virginia, West Virginia, and Wisconsin. In January 2021, Washington D.C., passed a near ban on non-competes, which became effective on April 1, 2022. In January 2023, President Biden issued an executive order asking the Federal Trade Communication (FTC) to issue new rules to curtail the use of non-competes. The FTC passed final rule to ban non-competes in April 2024. For a recent summary on non-compete reforms and proposals, see https://www.faircompetitionlaw.com/changing-landscape-of-trade-secrets-laws-and-noncompete-laws/.

different disciplines in their work.[3] Developing this ability often requires a combination of both non-firm-specific professional training, self-directed learning and a high level of self-motivation. Because knowledge workers develop highly portable general human capital that is valuable to both their current employer and competitors, mobility restrictions most directly suppress their incentive to invest in that capital (Garmaise, 2011; Amir and Lobel, 2014). Weaker enforcement raises the expected return to self-investment by expanding outside options, improving worker productivity and, in turn, firm performance. This effect is most pronounced in firms whose value creation depends most on knowledge workers, as it is precisely these workers whose human capital development is most sensitive to the availability of outside opportunities.

The second, and to our knowledge less explored, mechanism is assortative matching. If non-competes impede efficient employer-employee matching, their removal should trigger increased sorting, with the most productive workers gravitating toward the most productive firms, particularly in knowledge-worker-intensive industries where general human capital is most portable (Abowd, Kramarz, and Margolis, 1999). This reallocation of talent toward its highest-value use would itself generate performance gains, concentrated among the most productive firms that are best positioned to attract workers when mobility constraints are relaxed. As a result, the gains from weakening non-compete enforceability are likely to exhibit a "Matthew effect" (Merton, 1968). Initial advantages, such as higher productivity or better access to talent, become self-reinforcing, allowing leading firms to accumulate disproportionate gains over time and widening disparities in performance.

[3] While this broad definition of knowledge workers is widely used, and dates at least as far back to Drucker (1959), it may be ambiguous. To avoid any ambiguity, our primary definition of knowledge workers comes from the Occupational Employment Statistics (OES) Survey of the Bureau of Labor Statistics. It consists of employees classified as being in managerial or professional occupations and it includes scientists, engineers, technologists, heath practitioners, accountants, editors, computer programmers, etc.

Both mechanisms predict the same directional outcome, namely improved performance in knowledge-worker-intensive firms following a reduction in enforceability, and both are likely operative to some degree. However, they have meaningfully different empirical implications for the *distribution* of performance gains across firms. The human capital incentive effect should manifest broadly across knowledge-worker-intensive firms as workers throughout the distribution invest more in their general human capital. Assortative matching, by contrast, predicts that gains are concentrated at the top of the productivity distribution, as the most productive firms capture a disproportionate share of the newly mobile talent. Distinguishing between these mechanisms is therefore possible, at least indirectly, by examining whether performance improvements are broadly distributed or concentrated among the most productive firms.

There is also a practical consideration: improvements in human capital are inherently difficult to observe directly, as they accrue gradually and are embedded in individual workers rather than recorded in firm-level data. Assortative matching, by contrast, leaves observable traces in the movement of workers across firms, in the correlation between worker productivity and firm productivity, and in the widening dispersion of firm-level performance outcomes. For these reasons, while we acknowledge that both mechanisms likely contribute to our findings, our empirical analysis focuses on assortative matching as the more novel mechanism and the one most amenable to direct examination.

To test these predictions, we exploit staggered changes in the enforceability of non-competes across U.S. states as an exogenous shock to worker mobility. This setting is appealing for three reasons. First, these legal shifts arose primarily from precedent-setting court decisions or legislative statutes and were therefore not due to any particular firm's choice (Garmaise, 2011; Ewens and Marx, 2018). Second, time-series variation in enforceability allows consistent identification using a difference-in-differences approach. Third, empirical evidence suggests that in states where enforceability is high, even workers who have not explicitly signed non-competes experience reduced mobility, allowing us to treat state-level enforcement as applying to all firms and workers in the state (Starr, Frake, and Agarwal, 2019).

In a large panel of 68,170 firm-year observations from Compustat over 1992–2014, we begin by establishing that reduced enforceability increases performance in knowledge-worker-intensive firms generally. Firms in knowledge-worker-intensive industries exhibit higher ROA of 1.4 percentage points (a 22% increase from the sample mean) and higher Tobin's Q relative to other firms in the same state following a reduction in enforceability. In absolute terms, this translates into a \$2.95 million relative increase in net income, or \$3,688 more in net profits per employee at the median firm. These performance gains are accompanied by significantly greater total factor productivity (TFP) and higher sales, confirming that the effects operate through real productivity improvements rather than purely financial channels.

We then turn to the assortative matching mechanism, which is the central and most novel contribution of the paper. More productive firms experience disproportionately larger increases in ROA, Tobin's Q, and employment growth following a reduction in enforceability, and these effects are concentrated specifically among knowledge-worker-intensive firms. ROA is 1.3 percentage points higher (48% of the median), Tobin's Q is 0.10 higher (7% of the median), and employment grows at a 6.8% higher rate at more productive firms relative to less productive firms in the same state. Using data on inventors, we find direct evidence of sorting: more productive inventors tend to work for more productive firms following a reduction in enforceability. These patterns are difficult to reconcile with a pure human-capital-incentive mechanism and instead point to the reallocation of talent across firms as a distinctive driver of performance gains.

The firm-level sorting dynamics carry a direct implication for aggregate performance dispersion. If the most productive knowledge-worker firms pull ahead by attracting the best talent from less productive rivals, the cross-sectional spread in firm performance should widen following a reduction in enforceability. We find precisely this: after enforceability decreases, dispersion in performance and productivity increases across firms in knowledge-worker-intensive industries relative to other firms within the state. This widening implies that the aggregate welfare effects of non-compete reform depend critically on how one weights gains at the top of the productivity distribution against competitive losses lower in that distribution.

One potential concern is that our firm-level results, based on headquarters state, may not capture employees subject to non-compete regimes in other states where the firm operates. Establishment-level analyses address this directly. We find that reductions in enforceability increase employment growth and sales growth more in knowledge-worker-intensive establishments than in others, and that dispersion in both measures widens more across knowledge-worker-intensive establishments, corroborating our firm-level findings.

Our study contributes to a growing literature studying the economic consequences of non-competes. Prior studies document various costs of non-competes such as suppressing worker wages (Garmaise, 2011; Balasubramanian et al., 2022) and impeding entrepreneurship and innovation (Samila and Sorenson, 2011; He, 2025). On the positive side, non-competes help solve the holdup problems for employers and thus encourage firm-sponsored training (Starr, 2019). We uncover a positive effect of lower non-compete enforceability on firm performance, productivity and growth in knowledge-worker-intensive industries. By exploring the assortative matching dynamics engendered by greater mobility, we find unequal distribution of economic surplus across firms – those who are the most productive obtaining more of the surplus created from efficient labor reallocation, which has further implications for aggregate-level performance dispersion across firms. To the best of our knowledge, we believe we are the first to provide such evidence.

Our work is related to two recent studies on labor mobility and corporate outcomes. Jeffers (2024) examines increases in non-compete enforceability and finds that reduced mobility encourages incumbent firms to increase physical capital investment, consistent with a holdup-mitigation mechanism. In contrast, we examine reductions in enforceability and find improved firm performance concentrated in knowledge-worker-intensive industries, operating through labor-market channels: more outside options, stronger worker incentives, and most distinctively, assortative matching, with higher-quality workers reallocating to more productive firms. These patterns point to productivity gains from relaxing binding mobility constraints, highlighting complementary but distinct mechanisms operating at different policy margins. Shen (2021) focuses on labor mobility of immigrants and finds that increased mobility reduces firm value, but the narrow population of green card holders and their concentration in knowledge-intensive industries limits the ability to identify the cross-sectional variation across firm types that is central to our analysis. We view our work as both orthogonal and complementary to these studies.

2. Institutional Background and Literature Review

2.1 Institutional background on non-competes

Workers who have signed covenants-not-to-compete (CNCs, or non-competes) are forbidden from competing against their employers either by working for or starting a rival firm in the same industry and within a specified region for some period of time (often one to two years) after the termination of the employment (Gilson, 1999; Posner and Triantis, 2001). The agreements typically include a list of competitors or fields where employees cannot work upon separation

(Valiulis, 1985). The geographic scope of the restrictions is often within state, such as within a county, a city, or within a 10- or 50-mile radius around the business location (Malsberger, 2004).

Upon breaching the terms of a non-compete, the employer can file suit in court. The judge will determine whether the agreement is enforceable based on state statute or case law precedents. The necessary condition for an enforceable non-compete is that the worker must possess a "legitimate business interest" that the employer seeks to protect and prevent from being publicly known. The protectable interests include trade secrets, customer lists, advertising strategies and other confidential information. The courts will then stipulate a "reasonableness test" – considering the contract valid if it does not impose broader restrictions to injure the worker and society than necessary to protect the interests of the ex-employer (Gilson, 1999; Lester and Ryan, 2009). The weight of evidence required to determinate the reasonableness (e.g., time, geographic and scope restrictions) varies significantly across states.

Most states have their own statues and court rulings to govern the enforceability of non-competes. At one extreme is California, which has long had a tradition of not enforcing non-competes at all. The California Business and Professional Code Sec. 16600, a statute dating back to 1872, has long been interpreted by California courts as a complete ban on non-competes. In recent judicial ruling such as in the case of *Edwards v. Arthur Andersen LLP, 189 P2d 285*, the California Supreme Court has upheld past precedents affirming general non-enforceability of non-competes. At the other extreme, Florida has one of the highest enforceability of non-competes and can be characterized as having an "employer-friendly" regime (Garmaise, 2011). A 1996 statute allows the employer to obtain an injunction once the non-compete is violated, while prohibiting courts from considering economic or other hardships faced by the employee. Other states have degrees of enforceability that lie somewhere in between these two.

Assessing the degree of non-compete enforceability across states does have a degree of subjectivity but this might be done on at least seven dimensions (Bishara, 2010). These include the following: (1) whether or not a state statute is in place (such as California); (2) the definition of a legitimate protectable interest; (3) who has the burden of proving the non-compete reasonable; (4) time and geographic restrictions; (5) whether the non-compete can apply to employees fired without cause; (6) whether the employer needs to provide some consideration – a benefit received by the worker such as promotions, pay raises, or training – for a non-compete to be valid (e.g., Texas and Wyoming), or continued at-will employment is sufficient consideration; (7) whether judicial modification is allowed (or required) such that the non-compete will be made reasonable to enforce (e.g., Florida), under the "blue pencil" or "equitable reform" doctrines.[4] These differences in the enforcement across states usually have deep historical roots, which mitigates the concerns about the endogeneity of these laws from the perspective of any individual firm.

Although we lack the data on the actual use of non-competes by individual firms and employees, extant literature indicates the prevalence of these agreements among U.S. workers. Recent survey results from Starr, Prescott, and Bishara (2021) suggest that nearly one out of five workers in the U.S. labor force (about 30 million people) is bound by a non-compete, and that 37% of workers have had one at some point during their career. Recent reports from the White House (White House, 2016) and U.S. Treasury Office of Economic Policy (Treasury, 2016) also sound the alarm on the widespread use and misuse of non-competes.

In fact, firms frequently ask employees to sign non-competes even when such contracts are largely unenforceable under the state law. For example, according to Garmaise (2011), 58% of

[4] On the opposite side of the spectrum, some states that implement a "red pencil" doctrine (e.g., Nebraska, Virginia, and Wisconsin) will refuse to enforce an entire non-compete contract that contains any unenforceable/unreasonable provisions. This approach creates incentives for employers to write an enforceable (reasonable) non-compete.

S&P 1500 firms headquartered in California use non-competes even though, as we noted above, non-competes are generally unenforceable in that state. Kaplan and Stromberg (2003) document similar levels of use among California entrepreneurs. Not surprisingly, non-competes are more common among knowledge-intensive workers: 70% of top executives and nearly half of technical professionals have signed non-competes (Garmaise, 2011; Marx, 2011), further justifying our argument that non-competes should be most relevant to firms relying on this type of workers.

2.2 Prior literature on non-competes

Our paper builds on the literature of non-competes and employee mobility. This literature overwhelmingly documents that non-competes restrain mobility of high-skill labor – technology workers (Fallick, Fleischman, and Rebitzer, 2006), inventors (Marx, Strumsky, and Fleming, 2009), scientists and engineers (Marx, 2011), entrepreneurs (Kaplan and Strömberg, 2003), and top executives (Garmaise, 2011), all demonstrating that non-competes are "one of the most important mechanisms binding employees to a firm" (Garmaise 2011).

Theoretically, the use/enforcement of non-competes has ambiguous implications for firm financial performance and productivity. On one hand, enforceable non-competes may encourage firms to make investments because the clauses increase the firms' ability to capture a larger share of returns on the investments by mitigating potential holdup by employees. Property rights theory (Grossman and Hart, 1986; Hart and Moore, 1990, 1994) highlights that bilateral relationships suffer from holdup problems when contracts are incomplete. When workers are free to leave for competitors, firms face constant threats of losing firm-specific organizational capital and proprietary knowledge typically embodied in the employee human capital, which might cause the firms to underinvest in the first place. The loss of human capital would further challenge a firm's ability to generate revenue due to the erosion of its competitive advantages to rivals. By prohibiting knowledge workers from moving to rivals, non-competes effectively protect the firm's proprietary knowledge and enhance its ability to appropriate returns generated by firm-specific investments related to training and developing intellectual property.

Therefore, weaker enforcement of non-competes could dampen firms' incentives to make a wide variety of investments that would improve productivity and firm performance. Some recent studies find empirical evidence that stronger enforcement of non-competes may stimulate firm-specific investments. For example, Starr (2019) suggests that states with a stronger enforcement regime are associated with more firm-sponsored employee training programs. Jeffers (2024) finds that increased enforceability increase capital investment and argues that this may be because of the complementary relationship between physical capital and skilled human capital.[5]

On the other hand, non-competes discourage workers from investing in their own human capital by weakening their outside options, which could ultimately reduce productivity and firm financial performance (Fulghieri and Sevilir, 2011; Garmaise, 2011). Prospective employers might refrain from hiring employees bound by a non-compete at another firm to avoid dealing with potential legal procedures and paying a non-compete based premium. Workers with a non-compete tend to turn down outside offers in order to avoid potential legal suits from their employers. Moreover, current employers frequently remind their workers of the non-competes in place. All these buttress the argument that non-competes deter workers' ability to leave consequently chilling labor market competition (He, 2018). Furthermore, Starr, Frake, and Agarwal (2019) report that in states with a higher incidence and enforceability of non-competes, even workers who have not signed these agreements receive relatively fewer job offers, have reduced mobility, earn lower wages, and are less satisfied with their jobs. The decrease in mobility reduces allocative efficiency

[5] Garmaise (2011), however, finds results that contradict those in Jeffers (2024) with respect to the impact of non-competes on capital investment.

because workers are constrained by the "job lock" and cannot move to firms in which they will be more productive.

This means that weaker enforcement of non-competes should incentivize workers to invest in themselves because they perceive greater expected payoff from investing in their general human capital-a type of human capital that is portable and transferable across firms. With greater bargaining power and more opportunities to capitalize on their skills, a lower non-compete enforceability increases workers' incentives to learn and be more productive, resulting in higher labor productivity and better quality of human capital over time. This argument is in line with the findings from an experimental study by Amir and Lobel (2014).[6]

As such, at the intensive margin, reducing enforceability of non-competes affects firm performance by encouraging workers to invest in their general human capital while discouraging firms from making firm-specific investments.[7] At the extensive margin, weaker enforceability of non-competes can influence firm performance by facilitating assortative matching between firms and employees, allowing better firms to recruit better workers from competitors and improving employer-employee match quality over time.

2.3 Hypothesis development and statements

Our empirical analysis is organized around two theoretical mechanisms, each generating distinct but complementary predictions about the cross-sectional distribution of performance

[6] In theory, workers should bargain for higher wages when signing the restrictive covenants to make up for any loss from restrained mobility further down the road in their careers. Existing evidence, however, suggests that the opposite is happening in practice. Executives and high-tech workers earn lower wages in high-enforcing states than those in low-enforcing states (Garmaise, 2011; Balasubramanian et al., 2022). There is also survey evidence from Marx (2011) finding that in 70% of cases, firms ask for a non-compete after the engineer has just accepted the job offer – a point at which the worker has little leverage over the firm – thereby restricting workers' ability to bargain.

[7] Despite a short-term increase in firm value after the inadvertent repeal of non-compete ban in Michigan's Antitrust Reform (Younge and Marx, 2016), Younge and Marx (2013) document that the positive effect on Tobin's Q for Michigan firms eventually reversed. Michigan firms became increasingly myopic in their R&D activities, as research teams became less diverse, inventors increasingly relied on older technologies, and firms concentrated on fewer technical areas, all of which supports this theoretical prediction.

effects following a change in non-compete enforceability. The first concerns the human capital incentive effect of non-competes on knowledge workers and the firms that employ them. The second, and more novel, concerns assortative matching between workers and firms.

Non-competes suppress workers' incentives to invest in general human capital by limiting their outside options. Because knowledge workers develop highly portable general human capital that is valuable to both their current employer and competitors, this suppression is most acute among knowledge-worker-intensive firms (Garmaise, 2011; Amir and Lobel, 2014). Decreases in enforcement thus increase the expected return to self-investment by expanding outside options, improving worker productivity and, in turn, firm performance. Consistent with this channel, He (2025) finds evidence for firm innovation performance that patents produced after a lower non-compete enforceability are more valuable, potentially reflecting stronger inventor incentives. We therefore predict that knowledge-worker-intensive firms are differentially affected by changes in non-compete enforceability:

H1: *A reduction (increase) in the enforceability of non-competes increases (reduces) the performance and productivity of knowledge-worker firms.*

H1 establishes the baseline cross-sectional prediction. However, the human capital incentive effect alone does not predict where the gains should be concentrated within the knowledge-worker sector. It implies broadly distributed effects as workers throughout the distribution respond to changes in their outside options. A sharper and more novel prediction follows from assortative matching. If non-competes impede efficient employer-employee matching, changes in their enforceability should trigger changes in sorting, with reductions allowing the most productive workers to gravitate toward the most productive firms, and increases reinforcing barriers to such reallocation, particularly in knowledge-worker-intensive industries where general human capital is most portable (Abowd, Kramarz, and Marzolis, 1999). Because high-ability workers and high-quality firms are natural complements (Song et al., 2019), the performance effects of changes in enforceability should be most pronounced at the upper tail of the productivity distribution:

H2: *A reduction (increase) in the enforceability of non-competes increases (reduces) the performance and productivity of the most productive firms, especially among knowledge-worker firms.*

Note that H1 is a necessary condition for H2, and that H2 provides the sharper, more distinctive prediction that separates a sorting-based explanation from a uniform human-capital-incentive effect. While both mechanisms are likely operative, assortative matching is the more novel contribution and the one more amenable to direct empirical examination. Improvements or deteriorations in human capital accrue gradually and are embedded in individual workers, whereas sorting leaves observable traces in the movement of workers across firms and in the distribution of firm-level performance outcomes.

The firm-level sorting dynamics in H1 and H2 carry a direct implication for the distribution of performance across firms within knowledge-worker-intensive industries. If changes in enforceability shift the allocation of talent across firms, the cross-sectional spread in firm performance should widen when enforceability falls, as top firms pull ahead by attracting the best workers, and compress when enforceability rises, as mobility barriers insulate lower-productivity firms from competitive talent reallocation:

H3: *A reduction (increase) in the enforceability of non-competes increases (reduces) the dispersion of performance within knowledge-worker-intensive industries.*

Finally, while our hypotheses are stated symmetrically, the effects of reductions versus increases in enforceability need not be equal in magnitude. Labor market frictions may be easier to remove than to reimpose. When enforcement weakens, workers can act on expanded outside options relatively quickly, whereas reinstating enforcement binds only workers who subsequently sign new agreements and offers no mechanism to reverse matches or human capital investments already made. Additionally, the organizational and reputational costs of enforcing non-competes against departing employees, including litigation expenses, morale effects on remaining workers, and reputational harm in the labor market, may create a practical wedge that attenuates firms' willingness to act on stronger enforcement (Starr, Frake, and Agarwal, 2019). Together, these frictions suggest the possibility that increases in enforceability generate smaller effects than equivalent reductions, though whether such asymmetry exists remains an empirical question that we examine in our tests.

3. Data and Empirical Strategy

3.1 Data and variables

We start with all publicly traded U.S. industrial firms with SIC codes outside the ranges 4900-4949 (utilities) and 6000-6999 (financials) in Compustat North America Fundamentals Annual files over 1992–2014. We require firm-year observations to have positive assets, positive sales, non-negative common equity and non-missing values for the number of employees. As Compustat only reports current headquarters location, we extract information on historical headquarters from Securities and Exchange Commission (SEC) 10-K filings in the EDGAR database. Firms without identifiable location information are excluded. We choose 1992–2014 period because of the availability of a clear window of time-series variation in non-compete enforcement across U.S. states.[8]

Our data on changes in non-compete enforceability comes from Garmaise (2011) and Ewens and Marx (2018). Garmaise (2011) evaluates the strength of non-competition enforcement for each U.S. state from 1992 to 2004 based on twelve questions proposed by Malsberger (2004). He identifies three states that experienced major changes in non-compete enforcement. Ewens and Marx (2018) reviewed Malsberger, Brock, and Pedowitz (2016), which provides the definitive reference regarding both legislative and judicial changes to state-by-state policy regarding non-compete enforcement from 1995-2016. They found 14 material changes during their sample period.[9] We focus on the first occurrence of the law change in each state because it is arguably most exogenous compared with later changes. As we discuss in the following subsection, this process for identifying changes in non-compete enforcement yields twelve "treatment" states in which the enforcement of non-competes has changed over our sample period, thus allowing us to carry out a difference-in-differences analysis of the effect of changes in enforceability.

Our measures of firm performance are return on assets (*ROA*) for profitability, defined as the ratio of net income over book assets, and market-to-book ratio (*MktBk*) as the proxy for firm value. We employ sales per worker (*Sales Per Emp*) and total factor productivity (*TFP*) to measure the productivity of workers and firms. Sales per worker is calculated as the firm's total sales

[8] Several states revised non-compete enforceability with a limited scope in 2015 and 2016, while some other states proposed to reform the laws, which can give rise to somewhat anticipation among corporations. For instance, Hawaii (specific to IT industry), New Mexico (to physicians), and Wisconsin changed non-compete laws in 2015. Alabama, Arkansas, Rhode Island (to medical industry), New Hampshire (to medical industry), Nevada, Connecticut (to medical industry), and Utah changed the enforcement in 2016. Most of these events were not used in Ewens and Marx (2018) either because of the specificity of the law change or because of plausible endogenous nature of the law.

[9] They excluded firms in New York and New Mexico, two states that weakened the enforceability, because the changes were made specific to one industry or one type of workers. We exclude South Carolina's 2010 change due to its ambiguous effect on non-compete enforceability. We note that in our robustness checks dropping these restrictions do not affect our inference.

revenue scaled by the number of employees. *TFP* is the natural logarithm of the residual estimated from a log-linear Cobb-Douglas production function using a rolling regression of the logarithm of value added on the logarithm of capital stock and the logarithm of the labor stock according to Imrohoroglu and Tuzel (2014).

As discussed in the Introduction, firms may have plants or facilities in multiple states with different enforcement regimes, adding noise to our firm-level estimations. To address this issue, we obtain establishment-level data from the National Establishment Time-Series (NETS) Database provided by Walls & Associates, which collaborated with Dun and Bradstreet (D&B) in developing a longitudinal database that tracks every establishment in the U.S. since its birth. We use the NETS Publicly Listed Database to extract establishment-level information including establishment ID (or DUNS number), business name and location, headquarters linkage (or "Family Tree"), annual employment, annual sales and industry classification (SIC codes). We match establishments' parenting headquarters in NETS to our sample firms by their names and address using a fuzzy name matching algorithm.[10] We then calculate one-year forward employment and sales growth for each establishment to examine future establishment growth.

To determine the extent to which a firm or establishment relies on knowledge workers, we obtain data on industry occupation profiles from the Occupational Employment Statistics (OES) survey. The OES of the Bureau of Labor Statistics provides detailed breakdown of the number of workers employed in each industry by occupational code.[11] We define the percentage of knowledge workers that a firm uses as the fraction of managers and professional workers employed in the firm's industry (*Industry-level Knowledge Workers*), based on the industry classification (at

[10] We also manually check all the matches to ensure matching accuracy during this procedure.
[11] The OES surveys 1.2 million non-farm businesses over three-year cycles. Each industry was surveyed every three years from 1988 to 1995 and every year since 1997. More information is available at https://www.bls.gov/oes/.

the 3-digit SIC code level before 2001, and at the 4-digit NAICS code level afterwards) provided in OES. We apply a similar definition for establishments. Alternatively, we use data on industry-level skilled labor from Belo et al. (2017) to construct a proxy for high-skill labor dependency.

Because OES used its own taxonomy with 258 broad occupations before 1998, managerial occupations take codes from 10,000 to 19,999, and professional workers are assigned with occupational codes under the major group of 20,000, which includes scientists, engineers, technologists, heath practitioners, accountants, editors, computer programmers, and so forth. In 1999, the OES changed the occupation definitions to Standard Occupational Classification (SOC) system with 444 broad occupations. Thus, from 1999 onwards, managerial occupations are those in the major group of 11-0000; professional workers are assigned in the major groups with the first two digits of 13, 15, 17, 19, 21, 23, 25, 27, and 29, followed by 0000.

We also collect data from several other sources. We obtain state-level data on GDP growth rates, total population, per capita personal income and labor force from the Bureau of Economic Analysis, state unemployment rates from Bureau of Labor Statistics and state partisan composition from the National Conference of State Legislatures.[12]

Table 1 reports descriptive statistics of our main variables of interest for our sample period. All continuous variables are winsorized at their first and ninety-ninth percentiles to mitigate the effect of outliers on our results. Dollar values are CPI-adjusted in 2016 dollars. Appendix B describes variable constructions in detail. An average firm has a ROA of –0.06 and market-to-book ratio of 2.07. The median firm's ROA is 0.03, and its market-to-book ratio is 1.48. The mean values for sales per worker and TFP are $36,000 and –0.34, respectively.

[12] Data can be retrieved at https://thedataweb.rm.census.gov/ftp/cps_ftp.html

3.2 Empirical strategy

We employ a difference-in-differences-in-differences (DiDiD) framework that exploits major time-series changes in the enforceability of non-competes that took place in twelve states from 1992-2014. States that reduced non-compete enforceability include Texas (1994), Louisiana (2001), Oregon (2008), and New Hampshire (2012). States that increased non-compete enforceability are Florida (1996), Ohio (2004), Vermont (2005), Idaho (2008), Wisconsin (2009), Georgia (2011), Colorado (2011), and Illinois (2011) (Garmaise 2011; Ewens and Marx 2018).

Based on the shifts in the enforceability of non-competes in these states, we create two indicators to capture changes of non-compete enforceability. *CNC Enf. Down* is a binary variable that equals one for firms headquartered in states after experiencing a reduction in non-compete enforceability and is zero otherwise. *CNC Enf. Up* is a binary variable that equals one for firms headquartered in states after experiencing an increase in non-compete enforceability and is zero otherwise. Table 1 shows that 8.2% of firm-years in our sample experienced a reduction in enforceability, while 6.9% of the observations experienced an increase in the enforceability. We then implement the DiDiD test using regressions specified as follows:

$$\begin{aligned} Y_{i,s,t} = \alpha &+ \beta_1\, \mathit{CNC\ Enf.\ Down}_{s,t} \times \mathit{More\ Knowledge\ Workers}_{i,t} + \beta_2\, \mathit{CNC\ Enf.\ Up}_{s,t} \times \\ &\mathit{More\ Knowledge\ Workers}_{i,t} + \beta_3\, \mathit{More\ Knowledge\ Workers}_{i,t} + \beta'\, X_{i,s,t-1} + \mu_i \\ &+ \omega_s \times d_t + \varepsilon_{i,s,t} \end{aligned} \tag{1}$$

where $Y_{i,s,t}$ is one of the aforementioned measures of firm performance or productivity such as *ROA, MktBk, Sales Per Emp* and *TFP*, for firm *i* headquartered in state *s* in year *t*.

Equation (1) tests the differential treatment effects of changes in enforceability of non-competes on knowledge-worker firms relative to non-knowledge-worker firms. The key variables are the interaction terms of the treatment variables ($\mathit{CNC\ Enf.\ Down}_{s,t}$ and $\mathit{CNC\ Enf.\ Up}_{s,t}$) with *More Knowledge Workers*, a binary variable equal to one for firms in industries where the fraction of managers and professional workers above the median level across all industries every year, and is zero otherwise.[13, 14] $X_{i,s,t-1}$ is a set of firm- and state-level controls including *Size, Leverage, log(age), State GDP Growth, log(State Unemployment), State Industry HHI, MktBk, Acquisition Cost, R&D* and *Capex*, all measured in year *t-1*.

We incorporate state of headquarters × year fixed effects ($\omega_s \times d_t$) into our analyses to alleviate the endogeneity concern that unobserved time-varying state heterogeneity correlated with non-compete reforms might be driving the observed results, since this set of fixed effects should account for all time-varying, state-level factors that affect all firms within the state. β_1 and β_2 provide estimates of the differences in the treatment effects on firms with more knowledge workers relative to those with less in the same state. We cluster standard errors at the state level to account for possible correlations of the error terms for firms within the same state (Bertrand, Duflo, and Mullainathan, 2004). μ_i represents firm fixed effects.

4. Empirical Results

4.1 Non-compete enforceability, firm performance, and productivity

We first examine the cross-sectional effects of changes in non-compete enforceability on firm performance and productivity using the DiDiD model specified in Equation (1). Table 2 reports the estimation results. The dependent variable in columns (1)-(2) is a firm's ROA, and in columns (3)-(4) is the firm's market-to-book ratio (*Mktbk*). In column (1), we estimate the equation without controls. The estimated coefficient on the interaction term of *CNC Enf. Down × More*

[13] Knowledge-worker intensive firms account for 74% of our sample. As a result, 6% of the sample are affected by a reduction in the enforceability, and 5% are affected by an increase in the enforceability. To demonstrate that that our sample of treated knowledge-worker firms is representative of the full population of knowledge-worker firms, we note that tests of differences in sample means for the samples of treated and all knowledge-worker firms, respectively, are not significantly different from each other on several major firm observables (Internet Appendix Table IA.1).

[14] Our results are robust to using the continuous variable *Industry-level Knowledge Workers* (which is the percentage of knowledge workers in an industry) in the interaction term.

Knowledge Workers is 0.012 ($t = 2.42$). After including all controls, column (2) shows a coefficient of 0.014 on this interaction term, which is significant at the 1% level. This suggests that firms relying more on knowledge workers experience an increase in ROA of 1.4 percentage points (22% of the mean) following a lower enforceability, *relative* to same-state firms with less such workers.[15] In dollar terms, this translates to firms with more knowledge workers achieving a $2.95 million relative increase in profits compared to non-knowledge-worker firms. As the median firm has 800 employees, this means that an average worker generates $3,688 more in net profits for her employer (after paying out wages) when non-competes become less enforceable.

While there are cross-sectional differences in the effects of reducing the enforceability of non-competes, our results show little evidence for differential effects of increased non-compete enforceability on profitability between the two groups of firms, as the estimated coefficients on *CNC Enf. Up × More Knowledge Workers* are never significant in the two columns. As we conjecture in our hypothesis development, this finding is consistent with non-competes operating as binding mobility constraints with nonlinear impacts. Weakening enforceability strengthens worker outside options and improves matching, leading to better firm performance. In contrast, marginal increases in enforceability have limited effects because firms adjust compensation and governance structures to changes in enforceability (Garmaise, 2011) and rely on alternative retention mechanisms such as trade secret law and non-disclosure agreements. Moreover, non-competes are widespread and often redundant. So further increases in enforceability affect only a narrow set of binding contracts (Starr, Prescott, and Bishara, 2021). This asymmetry is consistent with monopsony and worker-side hold-up models, which predict first-order effects from relaxing

[15] We have also tested the average treatment effects and report these results in the Internet Appendix Table IA.2. We find little evidence for an average effect of reduced enforceability on firm profitability, a result consistent with the one previous study that examines the impact of non-competes on overall profitability (Garmaise, 2011) which finds no association between the two in a sample of S&P 1500 firms over 1992-2004.

binding mobility constraints but muted responses to further tightening (Manning, 2003), and aligns with prior work showing strong responses to reduced enforceability but weak or null effects from increases in enforceability (Marx, Strumsky, and Fleming, 2009; Ewens and Marx, 2018).

As is the case with firm profitability, the treatment effect of decreased enforceability of non-competes on firm value is much larger among firms that employ more highly skilled workers, as shown in columns (3) and (4). After including all controls in column (4), the coefficient estimate on *CNC Enf. Down* × *More Knowledge Workers* is 0.142 (t = 3.53), indicating that a weaker enforceability leads to a 6.8% or 9.6% (relative to sample mean or median) higher firm value for knowledge-worker firms compared with other same-state firms.[16] These results support our argument that the effect of reducing the enforceability on profitability and firm value is much stronger among firms in which knowledge workers are a more important part of value creation.

We next estimate, the cross-sectional variation in the effect of reducing the enforceability of non-competes, using productivity measures – *Sales per Employee* and *TFP* – as dependent variables. We present the results in columns (5) and (6) in Table 2. We find that reduced enforceability increases worker productivity more in firms with more knowledge workers relative to those with fewer such workers in the same state. The estimated coefficient on *CNC Enf. Down* × *More Knowledge Workers* is 0.076 (t = 7.54) and 0.028 (t = 2.44) for *Sales Per Emp* and *TFP*, respectively, suggesting that sales per employee increases 21% more and total factor productivity increases 8% more for firms relying more on knowledge workers after a lower enforceability compared with other in-state firms.[17]

[16] We also split the sample into enforcement up and enforcement down cases and find consistent results. These results are reported in the Internet Appendix Table IA.3.

[17] The average treatment effects of reduced enforceability on productivity are positive (see Internet Appendix Table IA.4).

4.2 Non-compete enforceability and growth: establishment-level evidence

We next examine differential effects of reduced non-compete enforcement on future employment growth and sales growth at the establishment level. As we note in the Introduction, a potential concern is that our cross-sectional firm-level results, which are based on the state in which a firm is headquartered, may not account for the fact that there are firms that have plants or other facilities in different states. Employees may be subject to the non-compete enforcement regime in the states in which their plant or facility is located rather than the regime in the state of the firm's headquarters. We thus replicate our analyses from Equation (1) using establishment level data in which we replace firm fixed effects with plant fixed effects and include parent-firm industry fixed effects. The dependent variable is one-year forward employment growth rate or one-year forward sales growth rate. We present the results in Table 3.

We find that establishments with more knowledge workers grow their workforce at a higher rate than other establishments in the same state following a lower enforceability. Column (1) reports that the coefficient on *CNC Enf. Down × More Knowledge Workers* is 0.077 (t = 2.37) for one-year forward employment growth, implying that weaker enforcement allows firms in knowledge intensive industries to recruit more knowledge workers and expand their workforce.

Turning to establishment sales growth, column (2) shows that the estimated coefficient on *CNC Enf. Down × More Knowledge Workers* is 0.096 (t = 2.54), suggesting that establishments with more knowledge workers also experience higher growth in future sales revenue following a reduction in non-compete enforcement. These cross-sectional effects of weaker non-compete enforcement across establishments provide corroborative evidence for our previous firm-level results and suggest that more productive human capital could lead to higher future firm growth.

4.3 Non-compete enforceability and assortative matching

We next examine whether a reduction in the enforceability of non-competes increases assortative matching between productive workers and productive firms. If there is significant variation in the quality of firms and workers, we expect that increased mobility will increase opportunities for matching better firms with better workers. We thus predict that reduced enforceability should have a more significant effect on performance and employment in more productive firms. To show this, we sort on our productivity measure *Sales Per Emp* and create an indicator, *More Productive Firms*, which equals one if the firm's sales per worker are above the median value among all the firms headquartered in the same state every year, and is zero otherwise. We then regress our performance measures on interactions of the treatment indicators with *More Productive Firms* based on a similar specification of Equation (1), in which we additionally include industry fixed effects. We present the results in Table 4.

We find evidence to support our prediction that reduced enforceability increases profits and value more in productive firms than in other same-state firms. In columns (1) and (2), the estimated coefficient on *CNC Enf. Down* × *More Productive Firms* is 0.013 (t = 2.83) and 0.104 (t = 4.65), for *ROA* and *Mktbk,* respectively. These estimates suggest that ROA increases by 1.3 percentage points more (48% of the median), and Tobin's Q is 0.10 higher (7% of the median) for more productive firms.

Our underlying assumption behind our test of assortative matching is that dynamic matching actually occurs. In other words, the increase in performance at these firms is due, at least in part, to increased hiring of productive workers following weakened enforceability of non-competes. We examine this mechanism in two ways.

First, we examine employment growth in the next year and report results in column (3). We find that productive firms experience higher employment growth than their less productive counterparts when non-compete enforceability goes down. The coefficient of *CNC Enf. Down* × *More Productive Firms* is 0.068 (t = 8.86), translating to a 6.8% higher rate. This also suggests that productive firms are able to recruit more workers than they lose to rivals, leading to a significant *net expansion* in their workforce. Furthermore, we expect these matching dynamics to be especially strong in knowledge-worker-intensive industries. We attempt to show this by creating a triple interaction term of *CNC Enf. Down* × *More Productive Firms* × *More Knowledge Workers* and estimating similar specifications as in columns (1) – (3) of Table 4. Indeed, the results in columns (4) – (6) confirm our prediction. The coefficient on the triple interaction is 0.018 (t = 2.95), 0.082 (t = 2.09), and 0.025 (t = 3.20), for *ROA, Mktbk,* and *Emp. Growth,* respectively.[18]

Second, and to provide more direct evidence, we construct a sample consisting of inventors who switched employers during our sample period based on patent records, following the approach of Marx, Strumsky, and Fleming (2009) to identify moving inventors. Our dependent variable is calculated as new employer's TFP minus old employer's TFP in the inventor's moving year. The key independent variable is an interaction term of *CNC Enf. Down* and an indicator for more productive inventors. Inventor productivity is measured as the number of patents the inventor has applied for in the past five years. *More productive inventor* is a dummy equal to one if the inventor's productivity is greater than the sample median in the state every year.

[18] The sample size of treated productive knowledge firms subject to a lower enforceability is 3.1%. To mitigate a potential concern of outliers driving our results, we also run subsample analysis by splitting firms in knowledge worker industries from those not, and then compare coefficient estimates of *CNC Enf. Down* × *More Productive Firms* across the two subsample tests. Our inference from the results remains similar as that from using the triple interaction term (see Internet Appendix Table IA.5).

Table 5 reports the results. In column (1), we find a significant and positive coefficient on the interaction term, suggesting that more productive inventors tend to work for firms with a higher TFP after a decreased non-compete enforceability. In column (2), we interact *CNC Enf. Down* with the continuous measure of inventor productivity and find a similar result.

Taken together, the results in this subsection provide evidence to support the assortative matching mechanism through which restricting non-competes boosts firm productivity and performance. Productive firms perform better and experience higher growth in employment when enforceability goes down, indicating that greater labor mobility is beneficial to these firms owing to increased sorting and better-quality matching. These gains are mostly concentrated in knowledge-worker-intensive industries.

4.4 Non-compete enforceability and dispersion across firm productivity

Our analysis thus far shows that increased mobility is associated with increases in firm productivity and performance but that these increases are not evenly distributed across firms. The gains are concentrated in more productive firms in the state and in industries with more knowledge workers. This raises the possibility as we predict in our hypothesis section that reduced enforcement of non-competes could be associated with increased dispersion in performance across firms. The benefits of increased mobility, which funnels higher quality workers to more productive firms, may lead to a wider dispersion in performance and productivity across firms in the state, which we expect to observe mainly in knowledge-worker intensive industries.

To examine this possibility, we start by testing the association between weakened non-compete enforcement and performance dispersion across knowledge-worker firms. To do so, we construct a sample at state-industry-year level. Dispersion in firm performance is measured by yearly standard deviations of ROA or MktBk for firms in each industry and state. We calculate standard deviations of Sales Per Emp and TFP in a similar way to measure dispersion in productivity. We also apply the concept of Gini Coefficient to calculate dispersion of profits using firms' ROA at state and industry level. We then regress each dispersion variable on interactions of More Knowledge Workers and the two treatment indicators. We account for industry, and state× year fixed effects, and cluster standard errors by state. We present the results in Table 6.

In the first four columns across all four dispersion measures, we observe a significant and positive association between a reduction in non-compete enforceability and dispersion in performance and productivity across firms in the state, especially in knowledge-worker intensive industries. The coefficient estimates of *CNC Enf. Down × More Knowledge Workers* range from 0.039 to 0.190 and are significant at a 1% level in three out of the four regressions. Applying the concept of Gini Coefficient to calculate dispersion of ROA, we find similar results (column 5).[19]

To address the concern over firm-level data as we have discussed, we also use establishment-level data to construct a sample at state-industry-year level, and carry out state-industry level tests of establishment outcome dispersion. We examine dispersion in employment growth and sales growth across establishments by calculating annual standard deviations of these two variables for all establishments in each industry and state. We present the results in Table 7. We consistently find a positive and significant relation between reduced enforceability and dispersion in employment growth and sales growth across establishments among those in knowledge-intensive industries.

Together, these results provide evidence to support our conjecture that greater mobility of knowledge workers due to reduced enforceability of non-competes results in wider dispersion in productivity and performance across firms or establishments relying more on such workers.

[19] Our results are robust to an alternative specification in which we include time-varying state level controls and industry, state, and year fixed effects. These results are reported in Internet Appendix Tables IA6 and IA7.

4.5 A stacked event-study approach

As our empirical setting relies on staggered changes in state-level policies, this raises well-known challenges for conventional two-way fixed effects (TWFE) estimators when treatment effects are heterogeneous across cohorts or over time (Goodman-Bacon 2021; de Chaisemartin and d'Haultfoeuille 2020). TWFE can implicitly combine treated units with already-treated units, generating biased estimates and misleading event-study dynamics (Sun and Abraham 2021).

To address these concerns, we adopt a stacked event-study design following Cengiz et al. (2019). For each policy change, we create a dataset consisting of the treated state and all clean control states from three years before to seven years after the event. Clean control states are those that never experienced any material enforceability changes during our sample period. Each dataset is assigned with a cohort indicator. We then stack all the datasets and estimate Equation (1) while controlling for Cohort × State × Year fixed effects in addition to firm fixed effects. This set of fixed effects allows us to examine within-state variation in treatment effects across firms using never treated states as control units in each cohort. This design eliminates comparisons between treated and already-treated units and ensures clean comparison, directly addressing the concerns raised by Goodman-Bacon (2021) and de Chaisemartin and d'Haultfoeuille (2020). This stacked design is consistent with the group-time ATT framework of Callaway and Sant'Anna (2021). Finally, we cluster standard errors at the treatment level, which is the state level.

Table 8 reports the results. We find consistent results using the stacked events framework. In panel A, the estimated coefficient on *CNC Enf. Down* × *More Knowledge Workers* is positive and significant for *ROA*, *Mktbk* and *Sales Per Emp*. Applying this approach to our dispersion tests (as reported in Table 6), panel B shows that the estimated coefficient on the interaction term is significant in four out of five specifications. These results suggest that our main results are robust to correcting the biases of TWFE estimator.

5. Robustness Tests and Additional Analysis

5.1 Labor skill measure as an alternative definition of knowledge workers

Thus far, we have employed the occupational code from the OES survey of the Bureau of Labor Statistics to identify knowledge workers. As a robustness test, we employ an alternative definition based on labor skills from Belo et al. (2017) which relies on the Specific Vocational Preparation (SVP) level of the occupation from the OES survey. Accordingly, skill in an industry is defined as the percentage of workers that work on occupations requiring a high level of training and preparation (i.e., occupations with Specific Vocational Preparation >= 7). We create an indicator, *More High-skilled Labor*, equal to one for firms in high-skill industries with skill value above the median level across all industries every year. We then estimate Equation (1) and replace *More Knowledge Workers* with *More High-skilled Labor.* The dependent variable is one of the following measures: the firm's *ROA*, *Mktbk*, *Sales Per Emp* and *TFP*.

Table 9 presents the results. We find that using this alternative measure does not change our inference. Following a decreased enforceability, firms in high-skill industries exhibit significantly better financial performance and achieve greater productivity relative to other in-state firms. The estimated coefficient on *CNC Enf. Down* × *More High-skilled Labor* is 0.021 (t = 5.60), 0.063 (t = 1.90), 0.045 (t = 4.99) and 0.052 (t = 5.27), for *ROA, Mktbk, Sales Per Emp* and *TFP,* respectively. Therefore, our results are not specific to a particular definition of knowledge workers.

5.2 Determinants of the Treatment and Timing of the Treatment Effects

A potential explanation for our findings is that the timing of changes in the enforcement of non-competes was simply a part of a trend of changes that was already altering firm performance in the state. One hypothetical version of this potential alternative explanation could be the following: in states with rising economic growth, politicians (and perhaps judges) stepped in to reduce the enforcement of non-competes as a stimulus such that the post-event increase in firm performance might be simply a continuation of a pre-event trend.

We conduct two tests to address this concern. First, we examine the drivers of the reform of non-compete laws. Table 10 investigates whether a state's macroeconomic, political climate, or other intellectual protection laws predict the changes in non-compete enforceability. The dependent variable is an indicator equal to one if a state has decreased or increased the enforceability in the year (*CNC Enf. Down* in the first two columns and *CNC Enf. Up* in next two). Observations for states that change the level of enforcement are dropped from the sample after the law is passed. All control variables are lagged by one year.

Column (1) shows that the change in the enforceability was unrelated to preexisting state-level economic and political conditions. None of the variables (a state's GDP Growth, unemployment rate, population, income per capita, labor force participation and percent of republicans) load significantly. In column (2), we further include two relevant intellectual protection laws: the Inevitable Disclosure Doctrine and UTSA (Trade Secrecy).[20] Only income per capita marginally lowers the odds of reducing non-compete enforceability. We note similar results for predicting a strengthened enforcement in the next two columns. Therefore, the timing of the shift in non-compete enforceability appears unlikely to have been a function of political, economic, or other legal institutional conditions. This suggests that weakened enforceability was

[20] Inevitable Disclosure Doctrine (IDD) is a legal doctrine that primarily protects a firm's trade secrets by allowing employers to compel courts to block employees from working for another employer if this would result in the disclosure of trade secrets (Klasa et al., 2018). State UTSA (Trade Secrecy) is an index that measures the strength of legal protection of trade secrets based on the effective UTSA and case law precedents based on Png (2017).

not merely an event that coincided with a trend in a change in firm performance in that state but represented a genuine shock that resulted in a shift in performance.

Second, we examine the timing of the differential treatment effects using the following dynamic DiDiD model built upon Equation (1):

$$Y_{i,s,t} = \alpha + \sum_{\tau=-5}^{\tau=5} \beta_{\tau}\, CNC\ Enf.\ Down_{s,\tau} \times More\ Knowledge\ Workers_{i,t} + \beta_{\kappa}\, More\ Knowledge\ Workers_{i,t} + \beta'\, X_{i,s,t-1} + \mu_i + \omega_s \times d_t + \varepsilon_{i,s,t} \quad (2)$$

Where *Y* is one of the four performance measures (*ROA*, *MktBk*, *Sales Per Emp*, or *TFP*). β_{τ} are the yearly coefficient estimates of the differential treatment effects on knowledge-worker firms relative to non-knowledge-worker firms from five years before to five years after a reduction in non-compete enforceability. We use *t-1* as the reference year, which is the last pre-treatment year. We include all controls, and firm and state × year fixed effects. This model allows us to estimate timing of the differential treatment effects following the reduction in the enforceability of non-competes, while also assessing the validity of the "parallel trends assumption" of no difference in performance prior to the policy change.

To alleviate the concern over TWFE estimators as previously discussed, we estimate Equation (2) on a matched sample. Specifically, each treated firm, defined as firm that has experienced a reduction in non-compete enforceability, is matched with five control firms with the closest size, in the same industry, and in a never treated state (i.e., states that have never experienced any material changes in their non-compete policy) in the year prior to the reform.

We plot the estimates of β_{τ} in Figure 1 and report the regression results in the Internet Appendix Table IA.8. Panels A through D show the results for *ROA*, *MktBk*, *Sales Per Emp*, and *TFP*, respectively, along with the 95% confidence intervals for point estimates. For all outcome variables, the pre-treatment differences between the two groups are small and insignificant, suggesting that there is not much difference in performance across firms before the treatment. This provides evidence supporting the parallel trends assumption.

We also include solid horizontal lines to show average pre- and post-treatment differences in the figures, along with the 95% confidence intervals using box-shaped dash lines. These point estimates are from a similar regression specification of Equation (2) where we replace the interaction terms with *Prior* and *Post*, each interacting with *More Knowledge Workers*. *Prior* is an indicator for all pre-treatment years, and *Post* is an indicator for all post-treatment years. Again, we find that average pre-treatment differences between the two groups are small and insignificant, while average post-treatment differences are positive and significant for all outcome variables.

We conduct similar analysis for employment growth and sales growth using a matched sample at the establishment level. Each treated establishment is matched with control establishments with similar size (within a 20% difference in sales revenue), in the same industry, and in a never treated state in the year prior to the policy change. Figure 2 panels A and B plot the estimates of β_τ and the pre- and post-treatment differences between knowledge-intensive and non-knowledge-intensive establishments. The corresponding regression results are reported in the Internet Appendix Table IA.9. We observe no significant differences in employment growth or sales growth across establishments before the treatment, but a significant difference of growth rates across establishments in knowledge-intensive industries than those in other industries after a reduced enforceability.

5.3 Additional tests

In this subsection, we briefly discuss additional robustness tests that we have performed. Our results remain to be robust in all these tests. Detailed description of each test and results are provided in the internet appendix.

First, to address the concern that firms might relocate their headquarters to states based on varying non-compete enforcement regime, we exclude all firms that have changed headquarters during our sample period. Second, we exclude firms affected by a law-based weakening of the enforcement in Oregon and New Hampshire due to its potentially limited effectiveness. Third, we exclude firms in California to address the concern that California's non-compete ban and the sheer size of its population might have a disproportionate impact on our results. Lastly, we double cluster standard errors by state and year, or by state and industry to account for the possible correlations of error terms for firms over time, or within an industry.[21]

6. Conclusion

This paper provides novel evidence on the cross-sectional impact of non-compete enforceability on firm performance and productivity. Exploiting staggered changes in non-compete enforceability across U.S. states over 1992–2014, we establish that reductions in enforceability increase the performance and productivity of knowledge-worker-intensive firms relative to other firms in the same state. Establishment-level evidence corroborates this finding: knowledge-intensive establishments achieve higher workforce and sales revenue growth than other establishments when non-competes are less enforceable, consistent with knowledge workers responding positively to greater mobility and expanded outside options.

A key contribution of the paper, however, lies in the assortative matching mechanism underlying these performance gains. Performance improvements are not uniformly distributed across knowledge-worker-intensive firms but are concentrated among the most productive firms within that group, which attract higher-quality workers following a reduction in enforceability.

[21] On a cautionary note, our specification already includes state by year fixed effects and firm fixed effects, leaving little remaining common residual variation at the year level to be captured by year clustering. As a result, adding year-level clustering dimension is not empirically identified in this setting. Alternatively, we include state and year fixed effects and double cluster standard errors by state and year, results remain robust.

Using inventor-level data, we find direct evidence of this sorting: more productive inventors migrate toward more productive firms after enforceability falls. These patterns are difficult to reconcile with a pure human-capital-incentive mechanism and instead point to the reallocation of talent across firms as a distinctive driver of performance gains.

The sorting dynamics in turn generate an aggregate consequence: reductions in non-compete enforceability are associated with significantly greater dispersion in performance and productivity across firms in knowledge-worker-intensive industries. This widening implies that the welfare effects of non-compete reform are not uniform, and that evaluating such reform requires careful attention to how gains at the top of the productivity distribution are weighed against competitive losses lower down.

Taken together, our findings underscore the importance of non-compete law as a labor market institution whose effects extend well beyond the average firm. We contribute to the ongoing debate on the economic value of non-compete contracts by showing that the net costs and benefits of enforcement are heterogeneously distributed across firms and industries, and that assortative matching is a central mechanism through which changes in enforceability affect both individual firm performance and the aggregate distribution of productivity. As economists, legal scholars, and policymakers continue to evaluate non-compete reform, our study suggests that the most consequential effects may be those operating through the reallocation of talent rather than through any uniform change in worker incentives or firm investment.

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Table 1. Summary Statistics

This table reports descriptive statistics of variables of interest for the sample over 1992 – 2014. This sample is based on all publicly traded non-financial and non-utility U.S. industrial firms with positive assets, positive sales, non-negative common equity in COMPUSTAT and identifiable historical headquarters location information in SEC 10-K filings. Data on changes in non-compete enforceability are obtained from Ewens and Marx (2018). *CNC Enf. Down* is an indicator equal to one for firms headquartered in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for firms headquartered in states following an increase in the enforceability of non-competes, and zero otherwise. We exclude firms in New York and New Mexico due to the limited coverage of the laws, exclude firms in South Carolina due to the controversial effect of the legal case, and exclude firms in Texas after 2012 and Louisiana after 2004 as we focus on the first occurrence of the law change in a given state. Our sample contains 68,170 firm-year observations. Variable construction is described in detail in the Appendix. All continuous variables are winsorized at their 1st and 99th percentiles. Dollar values are CPI-adjusted in 2016 dollars.

Variable	Mean	S.D.	25th Percentile	Median	75th Percentile
ROA	-0.063	0.283	-0.069	0.027	0.072
Mktbk	2.073	1.794	1.089	1.477	2.273
Sales Per Emp ($million)	0.361	0.438	0.147	0.237	0.385
TFP	-0.340	0.436	-0.546	-0.326	-0.107
CNC Enf. Down	0.082	0.275	0.000	0.000	0.000
CNC Enf. Up	0.069	0.253	0.000	0.000	0.000
Industry-level Knowledge Workers	0.316	0.218	0.128	0.263	0.465
Size	5.392	2.117	3.827	5.288	6.843
Leverage	0.204	0.189	0.022	0.171	0.332
log(age)	2.608	0.763	2.079	2.565	3.178
Acquisition Cost	0.022	0.059	0.000	0.000	0.007
R&D	0.055	0.107	0.000	0.000	0.063
Capex	0.061	0.070	0.019	0.039	0.076
Cash Flow	0.006	0.230	-0.003	0.071	0.119
State Industry HHI	0.468	0.297	0.226	0.390	0.673
State GDP Growth	0.053	0.030	0.036	0.052	0.071
log(State Unemployment)	1.728	0.312	1.526	1.697	1.938
Operating Income/assets	0.055	0.298	0.011	0.112	0.199
Total Q	1.313	2.127	0.267	0.682	1.419

Table 2. Differential Effects of CNC on Firm Performance and Productivity

This table presents results examining the cross-sectional effects of changes in CNC enforceability on firm profitability, value, and productivity from 1992 to 2014. The dependent variables are a firm's *ROA*, *Mktbk*, *Sales Per Emp* and *Total Factor Productivity. CNC Enf. Down* is an indicator equal to one for firms headquartered in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for firms headquartered in states following an increase in the enforceability of non-competes, and zero otherwise. *More Knowledge Workers* is an indicator equal to one for firms in industries with the fraction of managers and professional workers above the median level of all industries every year. All control variables are measured in year *t-1* and are as defined in the Appendix. All regressions include firm fixed effects, and state × year fixed effects. The *t*-statistics in parentheses are based on robust standard errors clustered by the firm's headquarter state. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	ROA	ROA	Mktbk	Mktbk	Sales Per Emp	TFP
CNC Enf. Down	**0.012****	**0.014*****	**0.135*****	**0.142*****	**0.076*****	**0.028****
× More Knowledge Workers	**(2.417)**	**(2.725)**	**(2.987)**	**(3.533)**	**(7.542)**	**(2.442)**
CNC Enf. Up	-0.003	-0.003	-0.097	-0.084	0.008	-0.043
× More Knowledge Workers	(-0.599)	(-0.734)	(-0.697)	(-0.904)	(0.642)	(-1.669)
More Knowledge Workers	-0.006	-0.006	-0.124***	-0.092***	-0.009	0.002
	(-1.589)	(-1.422)	(-3.352)	(-2.690)	(-1.262)	(0.231)
Size		-0.032***		-0.338***	0.038***	0.068***
		(-7.286)		(-12.161)	(3.674)	(10.939)
Leverage		0.099***		0.127***	0.060***	0.064***
		(6.850)		(2.786)	(3.397)	(2.794)
log(age)		0.034***		-0.159***	-0.020	-0.125***
		(5.685)		(-5.537)	(-1.519)	(-8.909)
MktBk		0.010***		0.302***	0.006***	0.060***
		(7.934)		(16.310)	(4.496)	(13.695)
Acquisition Cost		0.029***		-0.150*	-0.007	0.168***
		(3.300)		(-1.837)	(-0.488)	(5.215)
R&D		-0.050**		1.092***	0.127**	-0.459***
		(-2.425)		(4.701)	(2.522)	(-4.960)
Capex		0.018		-0.376***	0.057*	-0.217***
		(1.062)		(-3.263)	(1.708)	(-4.256)
Cash Flow		0.398***		-0.036	0.169***	1.060***
		(19.612)		(-0.399)	(10.088)	(19.612)
State Industry HHI		-0.004		0.056*	-0.004	-0.028*
		(-0.490)		(1.773)	(-0.227)	(-2.012)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	68170	68170	68170	68170	67544	45035
Adjusted R^2	0.5603	0.5976	0.5244	0.6044	0.7929	0.6418

Table 3. Differential Effects of CNC on Firm Growth: Establishment-Level Evidence

This table presents results examining the cross-sectional effect of changes in CNC enforceability on firm growth using plant-level data from 1992 to 2014. The dependent variables are an establishment's employment growth and sales growth rates from year *t* to *t + 1. CNC Enf. Down* is an indicator equal to one for establishments located in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for establishments located in states following an increase in the enforceability of non-competes, and zero otherwise. *More Knowledge Workers* is an indicator equal to one for establishments in industries with the fraction of managers and professional workers above the median level of all industries every year. All regressions incorporate control variables, plant fixed effects, state × year fixed effects, and firm industry fixed effects. The *t*-statistics in parentheses are based on robust standard errors clustered by the firm's headquarter state. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Employment Growth	(2) Sales Growth
CNC Enf. Down	**0.077****	**0.096****
× More Knowledge Workers	**(2.365)**	**(2.537)**
CNC Enf. Up	-0.025	-0.114
× More Knowledge Workers	(-0.388)	(-1.476)
More Knowledge Workers	0.050	0.050
	(1.225)	(1.163)
Size	-0.046***	-0.076***
	(-2.814)	(-3.737)
Leverage	0.053	0.090
	(1.098)	(1.657)
Log(age)	0.071*	0.114***
	(1.903)	(2.778)
MktBk	0.004	0.001
	(0.816)	(0.138)
Acquisition Cost	0.134	0.569***
	(1.429)	(4.615)
R&D	-0.053	-1.033
	(-0.178)	(-0.938)
Capex	0.205	-0.044
	(1.626)	(-0.362)
Cash Flow	0.188**	-0.007
	(2.242)	(-0.070)
Plant FEs	Yes	Yes
State × Year FEs	Yes	Yes
Firm Industry FEs	Yes	Yes
N	2,255,678	2,255,678
Adjusted R^2	0.0008	0.0640

Table 4. Differential Effects of CNC on Firms with Different Levels of Productivity

This table presents results examining the differential effect of changes in CNC enforceability on firm performance across two dimensions—reliance on knowledge workers and firm productivity. The dependent variables are a firm's *ROA* (columns 1 and 4), *Mktbk* (columns 2 and 5), and one-year forward employment growth (columns 3 and 6). *More Productive Firms* is a dummy variable equal to one for firms with *Sales Per Emp* above the sample median every year. *CNC Enf. Down* is an indicator equal to one for firms headquartered in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for firms headquartered in states following an increase in the enforceability of non-competes, and zero otherwise. *More Knowledge Workers* is an indicator equal to one for firms in industries with the fraction of managers and professional workers above the median level of all industries every year. All control variables are measured in year *t-1* and are as defined in the Appendix. All regressions incorporate firm fixed effects, state × year fixed effects and industry (at 2-digit SIC code level) fixed effects. The *t*-statistics in parentheses are based on robust standard errors clustered by the firm's headquarter state. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	ROA	Mktbk	Emp. Growth	ROA	Mktbk	Emp. Growth
CNC Enf. Down × More Productive Firms				**0.018*** **	**0.082** **	**0.025*** **
× More Knowledge Workers				**(2.953)**	**(2.094)**	**(3.202)**
CNC Enf. Up × More Productive Firms				-0.015	-0.007	-0.052**
× More Knowledge Workers				(-1.268)	(-0.091)	(-2.340)
CNC Enf. Down	**0.013*** **	**0.104*** **	**0.068*** **	-0.004	0.043	0.042***
× More Productive Firms	**(2.826)**	**(4.645)**	**(8.855)**	(-0.705)	(1.278)	(4.897)
CNC Enf. Up	-0.015	0.064	-0.015	-0.003	0.061	0.019
× More Productive Firms	(-1.526)	(0.894)	(-1.361)	(-0.301)	(0.660)	(1.168)
More Productive Firms	0.021***	-0.001	0.052***	0.021***	0.017	0.052***
	(4.282)	(-0.056)	(7.079)	(3.668)	(0.594)	(5.339)
CNC Enf. Down				-0.019	-0.028	-0.017
× More Knowledge Workers				(-1.480)	(-0.383)	(-0.996)
CNC Enf. Up				0.004	-0.153	0.064
× More Knowledge Workers				(0.227)	(-0.898)	(1.635)
More Knowledge Workers				0.000	0.007	-0.002
				(0.001)	(0.101)	(-0.109)
More Productive Firms				0.000	-0.024	-0.002
× More Knowledge Workers				(0.109)	(-0.690)	(-0.221)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	59879	59664	56936	57079	56886	54333
Adjusted R^2	0.5523	0.5602	0.1449	0.5582	0.5636	0.1446

Table 5. Assortative Matching: Evidence from Inventors

This table presents results examining whether productive inventors tend to work for more productive firms after a reduced non-compete enforceability based on a sample consisting of inventors who switched employers during our sample period. The dependent variable, *TFP Difference*, is calculated as new employer's TFP minus old employer's TFP. *More Productive Inventor* is a dummy variable equal to one if the inventor's productivity is above the sample median across the state every year. *Inventor productivity* is calculated as natural logarithm of one plus the number of patents the inventor has applied for in the past five years. *CNC Enf. Down* is an indicator equal to one for firms headquartered in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for firms headquartered in states following an increase in the enforceability of non-competes, and zero otherwise. *Inventor Patent Experience* is natural logarithm of one plus the number of years since the inventor's first patent. *Asset Ratio* is the new employer's assets over the old employer's assets. All regressions incorporate industry (at 2-digit SIC code level), state, and year fixed effects. The *t*-statistics in parentheses are based on robust standard errors clustered by the firm's headquarter state. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) TFP Difference	(2) TFP Difference
CNC Enf. Down	**0.034*** **	
× More Productive Inventor	**(4.201)**	
CNC Enf. Up	0.028	
× More Productive Inventor	(1.460)	
CNC Enf. Down		**0.019*** **
× Inventor Productivity		**(3.821)**
CNC Enf. Up		0.008
× Inventor Productivity		(0.696)
CNC Enf. Down	0.094**	0.089**
	(2.658)	(2.494)
CNC Enf. Up	0.086	0.088
	(1.106)	(0.728)
More Productive Inventor	-0.020**	
	(-2.504)	
Inventor Patent Experience	-0.020***	-0.016***
	(-4.222)	(-3.453)
Asset Ratio	0.001***	0.001***
	(9.017)	(9.005)
Inventor Productivity		-0.015***
		(-2.805)
Industry FEs	Yes	Yes
State FEs	Yes	Yes
Year FEs	Yes	Yes
N	35836	35836
Adjusted R^2	0.1267	0.1268

Table 6. Dispersion in Performance and Productivity across Firms Within the State

This table presents state-industry level analysis testing the differential effects of changes in CNC enforceability on dispersion of firm performance and productivity across firms in the state. The dependent variables are standard deviations of *ROA, Mktbk, Sales/Emp,* or *TFP* for all firms in an industry and state. In column (5), the dependent variable is Gini coefficient based on ROA in each industry and state. We require each industry-state group to have at least three firms in a given year to enter the sample. *CNC Enf. Down* is an indicator equal to one for firms headquartered in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for firms headquartered in states following an increase in the enforceability of non-competes, and zero otherwise. All regressions incorporate industry (at 2-digit SIC code level) fixed effects and state × year fixed effects. Standard errors are clustered by state.

	(1) Dispersion of ROA	(2) Dispersion of Mktbk	(3) Dispersion of Sales/Emp	(4) Dispersion of TFP	(5) Gini Coeff. of ROA
CNC Enf. Down	**0.039*****	**0.190*****	**0.046****	**0.057*****	**0.014*****
× More Knowledge Workers	**(3.769)**	**(3.738)**	**(2.185)**	**(3.284)**	**(3.150)**
CNC Enf. Up	0.034	0.151	-0.004	-0.017	0.013
× More Knowledge Workers	(1.273)	(1.236)	(-0.180)	(-0.379)	(1.070)
More Knowledge Workers	0.001	0.098**	-0.023	-0.004	-0.001
	(0.111)	(2.072)	(-1.465)	(-0.365)	(-0.299)
Industry FEs	Yes	Yes	Yes	Yes	Yes
State × Year FEs	Yes	Yes	Yes	Yes	Yes
N	6127	6124	6159	5215	6159
Adjusted R^2	0.2915	0.2800	0.4299	0.2739	0.2928

Table 7. Dispersion in Growth across Establishments Within the State

This table presents state-industry level analysis testing the differential effects of changes in CNC enforceability on dispersion of employment growth and sales growth across establishments in the state. The dependent variables are standard deviations of *employment growth,* or *Sales growth* for all plants in each industry and state. We require each industry-state group to have at least three plants in a given year to enter the sample. *CNC Enf. Down* is an indicator equal to one for establishments located in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for establishments located in states following an increase in the enforceability of non-competes, and zero otherwise. All regressions incorporate industry (at 2-digit SIC code level) fixed effects and state × year fixed effects. Standard errors are clustered by state.

	(1)	(2)
	Dispersion of Emp growth	Dispersion of Sales growth
CNC Enf. Down	**0.280*****	**0.341*****
× More Knowledge Workers	**(2.853)**	**(3.488)**
CNC Enf. Up	0.040	0.062
× More Knowledge Workers	(0.956)	(1.171)
More Knowledge Workers	-0.004	-0.003
	(-0.124)	(-0.084)
Industry FEs	Yes	Yes
State × Year FEs	Yes	Yes
N	40504	40504
Adjusted R^2	0.0509	0.0505

Table 8. Stacked Event Study Approach

This table presents stacked event-study regression results. Each cohort consists of one treatment state and clean control states during three years before and seven years after the event. Clean control states are those that did not experience material changes in CNC enforcement during our sample period. In panel A, the dependent variables are a firm's *ROA, Mktbk, Sales Per Emp* and *TFP*. In panel B, the dependent variables are standard deviations of *ROA, Mktbk, Sales/Emp,* or *TFP* for all firms in an industry and state. In column (5), the dependent variable is Gini coefficient based on ROA in each industry and state. *More Knowledge Workers* is an indicator equal to one for firms in industries with the fraction of managers and professional workers above the median level of all industries every year. *CNC Enf. Down* is an indicator equal to one for firms headquartered in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for firms headquartered in states following an increase in the enforceability of non-competes, and zero otherwise. All control variables are measured in year *t-1*. All regressions incorporate control variables, firm fixed effects and cohort × state × year fixed effects. The *t*-statistics in parentheses are based on robust standard errors clustered by the firm's headquarter state. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Performance Tests

	(1)	(2)	(3)	(4)
	Performance		Productivity	
Dependent Variable:	ROA	Mktbk	Sales Per Emp	TFP
CNC Enf. Down	**0.009***	**0.132****	**0.038*****	0.008
× More Knowledge Workers	**(1.898)**	**(1.970)**	**(4.914)**	(0.529)
CNC Enf. Up	-0.012	-0.045	-0.007	-0.013
× More Knowledge Workers	(-1.302)	(-0.791)	(-0.482)	(-0.658)
More Knowledge Workers	-0.003	-0.057***	0.010*	0.027***
	(-1.593)	(-3.187)	(1.936)	(4.370)
Controls	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Cohort × State × Year FEs	Yes	Yes	Yes	Yes
N	252310	252310	252320	163480
Adjusted R^2	0.6953	0.6782	0.8322	0.7285

Panel B. Dispersion Tests

	(1) Dispersion of ROA	(2) Dispersion of Mktbk	(3) Dispersion of Sales/Emp	(4) Dispersion of TFP	(5) Gini Coeff. of ROA
CNC Enf. Down	**0.031*****	**0.203*****	0.012	**0.028*****	**0.009*****
× More Knowledge Workers	**(4.959)**	**(6.118)**	(0.825)	**(3.057)**	**(3.685)**
CNC Enf. Up	-0.002	0.040	0.032	0.026	0.000
× More Knowledge Workers	(-0.093)	(0.253)	(1.264)	(0.810)	(0.014)
More Knowledge Workers	-0.016**	0.027	0.008	-0.013	-0.008***
	(-2.355)	(0.717)	(0.714)	(-1.535)	(-2.839)
Industry FEs	Yes	Yes	Yes	Yes	Yes
Cohort × State × Year FEs	Yes	Yes	Yes	Yes	Yes
N	18941	18935	19050	15951	19050
Adjusted R^2	0.2988	0.2992	0.4296	0.3295	0.3049

Table 9. Alternative Definition of Knowledge Worker: "Highly Skilled Labor"

This table presents results examining the differential effects of changes in CNC enforceability on performance, productivity and employment growth among firms relying more or less on highly skilled workers from 1992 to 2014. The dependent variables are a firm's *ROA, Mktbk, Sales Per Emp,* and *TFP*. *More High-skilled Labor* is a dummy equal to one for firms in high-skill industries, in which the percentage of workers that work in occupations requiring a high level of training and preparation is above the median level of all industries every year. The measure relies on the Specific Vocational Preparation (SVP) level of the occupation from the OES survey where skill in an industry is defined as the percentage of workers that work on occupations requiring a high level of training and preparation (i.e., occupations with Specific Vocational Preparation >= 7). *CNC Enf. Down* is an indicator equal to one for firms headquartered in states following a reduction in the enforceability of non-competes, and zero otherwise. *CNC Enf. Up* is an indicator equal to one for firms headquartered in states following an increase in the enforceability of non-competes, and zero otherwise. All control variables are measured in year *t-1*. All regressions incorporate control variables, firm fixed effects and state × year fixed effects. The *t*-statistics in parentheses are based on robust standard errors clustered by the firm's headquarter state. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
	Performance		Productivity	
Dependent Variable:	ROA	Mktbk	Sales Per Emp	TFP
CNC Enf. Down	**0.021*** **	**0.063***	**0.045*** **	**0.052*** **
× More High-skilled Labor	**(5.597)**	**(1.895)**	**(4.985)**	**(5.266)**
CNC Enf. Up	0.000	-0.034	-0.003	-0.011
× More High-skilled Labor	(0.074)	(-0.425)	(-0.142)	(-0.497)
More High-skilled Labor	-0.007*	-0.043	0.003	-0.000
	(-1.929)	(-1.245)	(0.284)	(-0.011)
Controls	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
State × Year FEs	Yes	Yes	Yes	Yes
N	65289	65289	65289	42171
Adjusted R^2	0.5941	0.6016	0.7922	0.6225

Table 10. Predicting the Timing of Change in Non-compete Enforceability

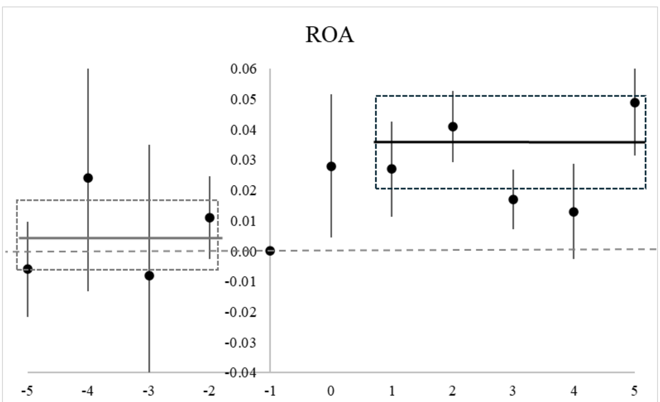
This table presents results examining whether a state's macroeconomic, political, and legal institutional conditions predict the reform of non-compete laws. The dependent variable in columns (1) and (2) is *CNC Enf. Down*, an indicator equal to one if a state has decreased non-compete enforceability in the year, and in columns (3) and (4) is *CNC Enf. Up*, an indicator equal to one if a state has increased non-compete enforceability in the year. All control variables are lagged by one year. *State GDP Growth* is the annual state GDP growth rate. *Ln(State Unemployment)* is the natural logarithm of state's unemployment rate. *ln(State Population)* is the natural logarithm of total population in the state. *ln(Per Capita Personal Income)* is the natural logarithm of per capita personal income in the state. *State Labor Force (Pct.)* is the ratio of labor force over total population in the state. *State Republicans (Pct.)* is the ratio of Republican to Democrat legislators in state legislatures and government to measure state partisan composition. *Inevitable Disclosure Doctrine* is an indicator equal to one for firms headquartered in states once recognized IDD in the year. *State UTSA (Trade Secrecy)* an index that measures the strength of legal protection of trade secrets based on the effective UTSA and case law precedents. Details on variable construction are described in the Appendix. All regressions control for state and year fixed effects. The *t*-statistics in parentheses are based on robust standard errors clustered by state. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
Dependent Variable:	CNC Enf. Down		CNC Enf. Up	
State GDP Growth	-0.000	-0.000	0.000	0.000
	(-0.103)	(-0.105)	(0.377)	(0.403)
ln(State Unemployment)	-0.020	-0.020	0.001	0.000
	(-1.425)	(-1.451)	(0.051)	(0.009)
ln(State Population)	-0.032	-0.033	0.031	0.027
	(-0.986)	(-1.040)	(0.342)	(0.306)
ln(Per Capita Personal Income)	-0.089	-0.093*	-0.184**	-0.189**
	(-1.603)	(-1.685)	(-2.069)	(-2.071)
State Labor Force (Pct.)	0.081	0.082	0.070	0.084
	(0.268)	(0.271)	(0.202)	(0.239)
State Republicans (Pct.)	0.022	0.024	-0.022	-0.030
	(0.539)	(0.557)	(-0.376)	(-0.506)
Inevitable Disclosure Doctrine		0.003		0.012
		(0.353)		(0.834)
State UTSA (Trade Secrecy)		-0.018		0.031
		(-1.520)		(0.554)
State FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
N	957	957	957	957
Adjusted R^2	0.0643	0.0625	0.0310	0.0302

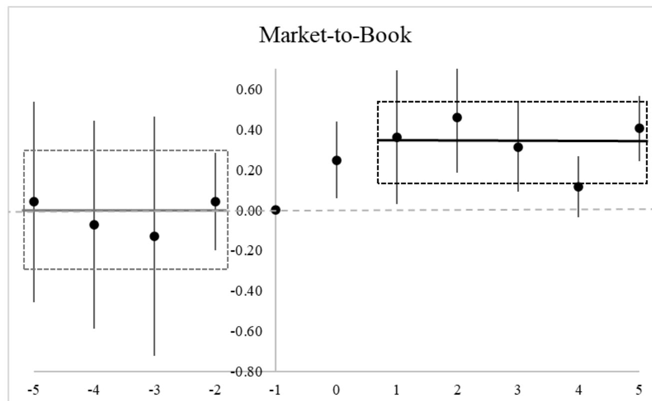
Figure 1. Timing of the Treatment Effects on Firm Performance and Productivity Following a Reduction in Non-competes Enforceability

Panels A – D plot yearly point estimates (with 95% confidence intervals) of pre- and post-treatment differences in the main outcome variables (*ROA*, *Mktbk*, *Sales Per Emp*, and *TFP*) between firms in knowledge-worker intensive industries and those in other industries five years before and after a reduced enforceability of non-competes, based on regression analysis using the specification in Equation (2). The regressions are estimated on a matched sample. Each treated firm, defined as firm that has experienced a reduction in the non-compete enforceability, is matched with five control firms with the closest size, in the same industry at the two-digit SIC code level, and in a never treated state (i.e., states never have had any material changes in their non-compete policy) in the year prior to the event year. The dark lines represent the average pre- and post-treatment differences in the outcome variables between the two groups of firms, based on regression analysis using a similar specification.

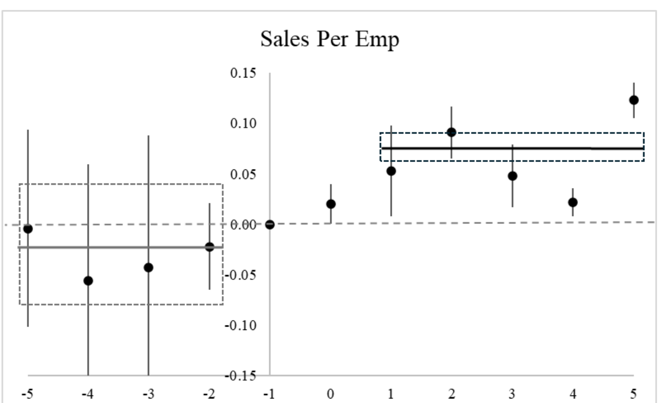
A. ROA



B. Mktbk



C. Sales Per Emp



D. TFP

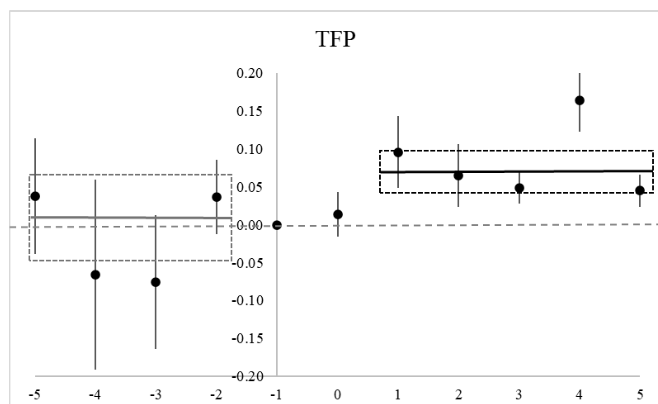
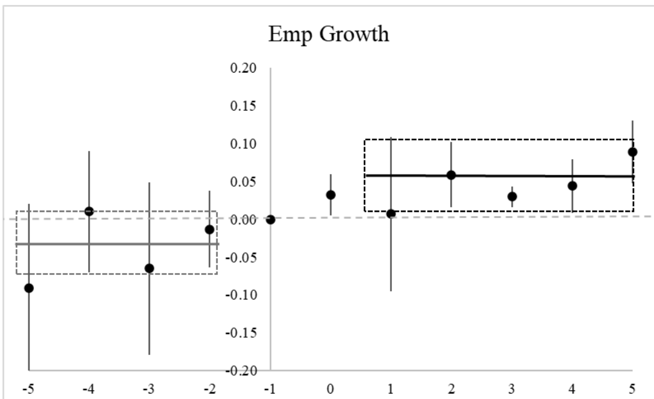


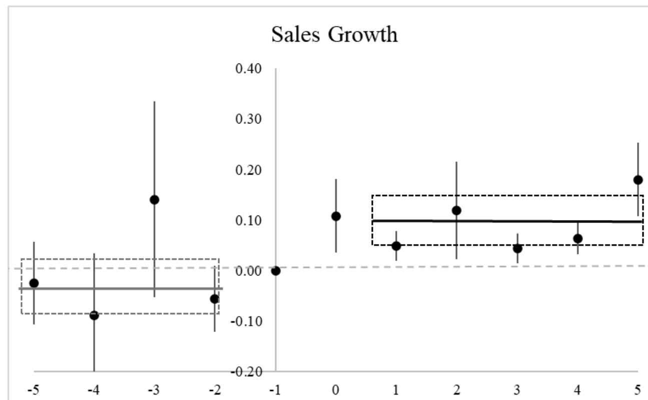
Figure 2. Timing of the Treatment Effects on Employment and Sales Growth at the Establishment Level

Panels A – B plot yearly point estimates (with 95% confidence intervals) of pre- and post-treatment differences in the employment growth and sales growth between establishments in knowledge-worker intensive industries and those in other industries five years before and after a reduced enforceability of non-competes, based on regression analysis using the specification in Equation (2). The regressions are estimated on a matched sample. Each treated plant, defined as plant that has experienced a reduction in the non-compete enforceability, is matched with control plants with a close size (within a 20% difference in sales), in the same industry at the two-digit SIC code level, and in a never treated state (i.e., states never have had any material changes in their non-compete policy) in the year prior to the event year. The dark lines represent the average pre- and post-treatment differences in the outcome variables between the two groups of establishments, based on regression analysis using a similar specification.

A. Employment Growth



B. Sales Growth



Appendix B
Variable Definitions

Variable	Description
ROA	the ratio of net income (NI) over book assets (AT).
MktBk	the ratio of total assets (AT) minus book value of common equity (CEQ) plus the market value of common equity (PRCC_F × CSHO) over total assets (AT).
Sales Per Emp ($millions)	The firm's total sales revenue (SALE) scaled by the number of employees (EMP), converted into 2016 dollars using CPI from the US Bureau of Labor Statistics
Total Factor Productivity (TFP)	the natural logarithm of the residual estimated from a log-linear Cobb-Douglas production function using a rolling regression of the logarithm of value added (deflated by GDP deflator) on the logarithm of capital stock (deflated gross property, plant, and equipment) and the logarithm of the labor stock, while controlling for industry × year fixed effects (Tuzel and Imrohoroglu, 2013).
Noncompetition (CNC) Enf. Down	A binary indicator that equals one for firms headquartered in states following a reduction in non-compete enforceability, and zero otherwise (Garmaise 2011; Ewens and Marx 2018). The events are Texas (1994), Louisiana (2001), Oregon (2008) and New Hampshire (2012). We do not include South Carolina (2010) due to its controversial effect. Information on firm historical headquarters location is extracted from Securities and Exchange Commission (SEC) 10-K filings in the EDGAR database.
Noncompetition (CNC) Enf. Up	A binary indicator that equals one for firms headquartered in states following an increase in non-compete enforceability, and zero otherwise (Garmaise 2011; Ewens and Marx 2018). The events are Florida (1996), Ohio (2004), Vermont (2005), Idaho (2008), Wisconsin (2009), Georgia (2011), Colorado (2011) and Illinois (2011). We exclude firms in Louisiana after 2004 and Texas after 2012 as we focus on the first occurrence of the law change in a given state.
Industry-level Knowledge Workers	the fraction of managers and professional workers employed in an industry at the 3-digit SIC code level before 2001 and at the 4-digit NAICS code level afterwards. Data on employment estimates are obtained from the Occupational Employment Statistics (OES) survey from the Bureau of Labor Statistics. The OES provides detailed breakdown of the total number of people employed in each industry by the occupational code. Because OES used its own taxonomy (with 258 broad occupations) before 1998, managerial occupations take codes from 10,000 to 19,999, and professional workers are assigned with occupational codes under the major group of 20,000, which includes scientists, engineers, technologists, heath practitioners, accountants, editors, computer programmers, and so forth. In 1999, the OES changed the occupation definitions to Standard Occupational Classification (SOC) system (with 444 broad occupations). Thus, from 1999 onward, managerial occupations are in the major group of 11-0000; professional workers are in the major groups with the first two digits of 13, 15, 17, 19, 21, 23, 25, 27, 29, followed by 0000. The OES data is available at https://www.bls.gov/oes/tables.htm.
More Knowledge Workers	an indicator equal to one for firms in knowledge worker intensive industries, defined as industries with the fraction of managers and professional workers above the median level across all industries every year.
Size	the natural Logarithm of book assets (AT), converted into 2016 dollars
Leverage	the ratio of long-term debt (DLTT) plus debt in current liabilities (DLC) to total assets (AT).
Age	number of years the firm is listed with a non-missing stock price on COMPUSTAT.

State GDP Growth	the annual state GDP growth rate from the Bureau of Economic Analysis.
Ln(State Unemployment)	the natural logarithm of state unemployment rate from US Bureau of Labor Statistics.
State Industry HHI	the sales-based Herfindahl-Hirschman Index within firms in the same two-digit SIC industry and headquartered in the same state.
Acquisition Cost	the ratio of Acquisition spending (AQC) to assets (AT).
R&D	the ratio of R&D expenditures (XRD) to assets (AT); missing values are set to zero.
Capex	the ratio of Capital expenditures (CAPX) over assets (AT).
Cash Flow	the operating income before depreciation (OIBDP), less interest (XINT) and taxes (TXT), scaled by total assets (AT).
Emp Growth	One-year forward employment growth rate from t to t+1 at the firm level or establishment level
Sales Growth	One-year forward sales growth rate from t to t+1 at the firm level or establishment level
More Productive Firms	a dummy variable equal to one for firms with Sales Per Emp above the sample median every year.
Dispersion in ROA/Mktbk/Sales Per Emp/TFP	annual standard deviation of ROA/Mktbk/Sales Per Emp/TFP of all firms in an industry and state. We require each industry-state group to have at least three firms in a given year to enter the sample.
Dispersion in Emp. Growth/Sales Growth	annual standard deviation of Emp. Growth/Sales Growth of all establishments in an industry and state. We require each industry-state group to have at least three establishments in a given year to enter the sample.
Inevitable Disclosure Doctrine (IDD)	an indicator equal to one for firms headquartered in states once recognized IDD in the year (Klasa et al 2016).
State UTSA (Trade Secrecy)	an index that measures the strength of legal protection of trade secrets based on the effective UTSA and case law precedents based on Png (2017).
Wrongful Discharge Laws	an indicator equal to one once the firm's state of headquarters adopted any of the three WDLs exceptions (i.e., good faith, implied contract, and public policy) according to Auto, Donohue, and Schwab (2006).
Ln(State Population)	the natural logarithm of total population in the state from the Bureau of Economic Analysis.
Ln(Per Capita Personal Income)	the natural logarithm of per capita personal income (dollars) in the state from the Bureau of Economic Analysis.
State Labor Force (Pct.)	the ratio of labor force over total population in the state from the Bureau of Economic Analysis.
State Republicans	the ratio of Republican to Democrat legislators in state legislatures and government. Nebraska is not included because members are elected on a nonpartisan basis. Data are obtained from the National Conference of State Legislatures.