



Employment restrictions on resource transferability and value appropriation from employees

Natarajan Balasubramanian¹ | Evan Starr² | Shotaro Yamaguchi³

¹Department of Management, Syracuse University, Syracuse, New York, USA

²Department of Management and Organization, University of Maryland, College Park, Maryland, USA

³Department of Management and Human Resources, University of Wisconsin, Madison, Wisconsin, USA

Correspondence

Evan Starr, Department of Management and Organization, University of Maryland, College Park, MD, USA.
Email: estarr@umd.edu

Abstract

Research Summary: We examine the joint adoption of four employment restrictions that limit firm resource outflows—nondisclosure (NDA), non-solicitation, non-recruitment, and noncompete agreements—and their associations with value appropriation from employees. Using novel individual- and firm-level survey data, we find that when firms adopt restrictions, they tend to adopt either all four restrictions or only an NDA. Adoption of all four restrictions is more likely when workers have access to valuable resources, noncompetes are more enforceable, and states adopt the inevitable disclosure doctrine. Employees with all four restrictions earn 5.4% less than employees with only NDAs, and this effect is driven by workers with low bargaining power. Analyses of earnings and a single restriction (e.g., only noncompetes) yield opposite results from those considering joint adoption, likely because of selection.

This is an open access article under the terms of the [Creative Commons Attribution-NonCommercial-NoDerivs](#) License, which permits use and distribution in any medium, provided the original work is properly cited, the use is non-commercial and no modifications or adaptations are made.

© 2024 The Author(s). *Strategic Management Journal* published by John Wiley & Sons Ltd.



Managerial Summary: Valuable firm resources are often embedded in employees. We study whether and when firms adopt four employment restrictions that could protect such resources—agreements not to disclose information, not to solicit clients or coworkers, and not to join or start a competitor—and examine the extent to which they are associated with value capture from employees. Using novel firm and worker-level surveys, we find that firms mostly adopt either all four restrictions together, only an NDA, or use no restrictions. Workers are more likely to have all four restrictions when they have access to valuable resources, when noncompetes are more enforceable, and when states adopt the inevitable disclosure doctrine. Finally, all four restrictions are associated with 5.4% lower earnings on average relative to workers with only an NDA, driven by workers with low-bargaining power.

KEY WORDS

bargaining, bundling, employment restrictions, resource transferability, value appropriation

1 | INTRODUCTION

Many valuable resources of firms—information, client relationships, and capabilities—are accessible to, and often are embedded in, their workers. Because workers can move or otherwise share such resources, firms face the risk of those resources leaking to their competitors (Campbell et al., 2012; Coff, 1997; Mawdsley & Somaya, 2016). A widely discussed way for firms to address this risk is by using noncompete agreements (NCAs), which prohibit departing workers from joining or starting competing firms (Garmaise, 2011; Marx, 2011; Marx et al., 2009; Starr et al., 2018; Young, 2021).¹ However, the literature has largely ignored other closely related contract terms that firms can use to achieve similar goals, including non-disclosure agreements (NDAs), which prohibit workers from disclosing confidential information, and non-solicitation (NSAs) and non-recruitment agreements (NRAs), which prohibit

¹For an overview, see Starr (2019a). Prior empirical research has also studied how NCAs and their enforceability influences entrepreneurship (Marx, 2022; Starr et al., 2018; Stuart & Sorenson, 2003), business dynamism (Kang & Fleming, 2020), innovation (Conti, 2014; Johnson et al., 2021; Samila & Sorenson, 2011), investment (Jeffers, 2022; Starr, 2019b; Starr et al., 2021), acquisitions (Younge et al., 2015), firm value (Hiraiwa et al., 2022; Younge & Marx, 2016), and value appropriation (Balasubramanian et al., 2020; Lavetti et al., 2020; Lipsitz & Starr, 2022; Rothstein & Starr, 2021; Starr et al., 2021). It has also studied predictors of NCA use (Johnson & Lipsitz, 2020; Lavetti et al., 2020; Rothstein & Starr, 2021; Starr et al., 2021) as well as how NCA enforceability affects the adoption of other protection mechanisms (Kang & Lee, 2022; Mukherjee & Vasconcelos, 2012; Sanga, 2018).

departing workers from soliciting/recruiting former clients and co-workers, respectively. Consequently, empirical evidence on these restrictions is limited.

Studying these restrictions is important for at least two reasons. Firms are known to use multiple protection mechanisms such as patenting and trade secrecy (Cohen et al., 2000; Contigiani et al., 2018; Kang & Lee, 2022; Levin et al., 1987) to sustain resource-based advantages (Barney, 1991; Melero et al., 2020). However, in the context of employment restrictions, we have no evidence on (a) whether and under what circumstances firms co-adopt such restrictions or substitute between them, and (b) how (co-)adoption of restrictions relates to important outcomes like value appropriation by firms and employees. Such evidence may become particularly valuable if the Federal Trade Commission's recent NCA ban survives legal challenges (Federal Trade Commission, 2024). Second, the co-adoption of restrictions requires us to reconsider prior observational studies of just one restriction and think carefully about the implicit comparisons being made when workers bound by that restriction are compared with those that are not bound by it (e.g., with four restrictions, there are 16 unique combinations of restrictions; NCAs are present in eight of them and absent from the others).

To address these gaps, we leverage two novel, large-scale surveys—one of workers, and one of firms—derived from a 2017 partnership with Payscale.com, and use an abductive, question-driven approach to provide descriptive answers to three interrelated questions: (1) How common are NDAs, NSAs, NRAs, and NCAs, and do they tend to be jointly or separately adopted? (2) How do resource access and legal protection related factors relate to the set of restrictions firms adopt? (3) How do different sets of restrictions relate to value appropriation from workers?

Answering the first two questions helps us understand what combinations of restrictions firms choose, and whether key factors studied in the prior literature (largely on NCAs), such as workers' access to valuable resources (e.g., trade secrets) or the state's legal regime, are associated with the restrictions firms adopt. The third question focuses on an important outcome for strategic management, value appropriation, and is associated with an important theoretical tension (Coff, 1999; Sevcenko et al., 2021): can firms extract value from employees by using these restrictions (Arnow-Richman, 2006; Balan, 2021; Lobel, 2021), or will workers extract value by refusing to agree to such restrictions without additional compensation, as anticipated by efficient contracting theories (e.g., Rubin & Shedd, 1981)? Investigating the third question also gives us the opportunity to study how incorporating a broader set of restrictions yields different insights relative to studying just one.

Our answers to these questions make at least two important contributions. First, we offer new, descriptive empirical evidence on the individual and joint use of such restrictions that open the door to future questions in strategic management. We find that, although NCAs have attracted the most attention, they are the least common. They cover about 1 in 5 workers and are almost always found with the other three restrictions. Moreover, just three out of 16 possible sets of restrictions—no restrictions, only an NDA, or all four—cover 82% of workers. Such a strong co-occurrence of restrictions suggests the need for a broader theory that can explain such bundling and raises questions about whether NCAs (or other restrictions) can be studied—and their effects estimated—individually. We also find that the use of all four restrictions is positively correlated with the worker's access to valuable firm resources such as client information, and that firms substitute away from all four restrictions toward other combinations when NCAs are less enforceable—but marginally toward all four restrictions when the state has adopted the inevitable disclosure doctrine (IDD).



Our second contribution is to offer new evidence on how these restrictions are related to value appropriation from workers, as measured by their earnings. We document that among the three most common sets of restrictions, workers with only an NDA are the highest earning, while those with all four restrictions earn 3–7% less, and those with no restrictions are the lowest earning. To explain this non-monotonic pattern, we posit that the comparison between workers with all four restrictions versus only an NDA is less affected by selection bias than a comparison between workers with all versus no restrictions—because it nets out selection into *any* restrictions. We provide support for this idea using Monte Carlo simulations, tests of unobservable selection, and related evidence from the firm survey. Consistent with this idea, we show that estimates from studying one restriction alone paint a very different story from that obtained by simultaneously studying multiple restrictions. Finally, we show that the negative earnings results related to value capture are driven by non-top-managers and workers with low bargaining power, while top managers and workers with greater bargaining power are relatively better off. Thus, workers' bargaining power seems to be a boundary condition for whether and when firms can extract value from employees by using these restrictions.

2 | FRAMING THE QUESTIONS

2.1 | Adoption of employment restrictions limiting resource transferability

Prior literature covering employment restrictions that limit resource transferability focuses almost entirely on NCAs, perhaps because they are seemingly the most severe of these restrictions and directly prohibit mobility to competitors. Relative to NCAs, three other restrictions—NDAs, NSAs, and NRAs—apply more broadly beyond direct competitors but protect resources more narrowly. NDAs tend to prohibit workers from using or disclosing firm information and apply in perpetuity, while NSAs and NRAs prohibit workers from soliciting former clients and coworkers, respectively, within a limited period of time post-departure. See Table A1 for a description of each restriction (table and figure numbers starting with “A” are in the Online Appendix). Per employment lawyers, these restrictions also differ in their enforceability, with NCAs being the most difficult to enforce and NDAs being the easiest, conditional on being able to prove a violation.

Except for NDAs, only recently have even legal scholars been writing about these other restrictions, and these writings have been theoretical in nature (Arnow-Richman et al., 2022; Graves, 2022; Lobel, 2021). Sockin et al. (2022), who use the industry NDA data reported herein, is one exception. Indeed, where these restrictions appear to be emphasized most is in policy and practitioner discussions and primarily as substitutes for NCAs if NCAs were banned (Beck, 2022; Federal Trade Commission, 2024; Silverman, 2020).

Given this lack of research, we currently do not know how common these other restrictions are and whether firms do indeed adopt them in lieu of or alongside NCAs. For example, if the firm can prohibit the worker from joining or starting a competitor with an NCA, then would they require the other restrictions? They may choose not to do so if there are costs to adopting the other restrictions and they offer little incremental protection beyond NCAs. Or, if the firm can effectively leverage NDAs, NSAs, and NRAs to limit the outflow of information, clients, and workers, then would they need to adopt NCAs as well? That could be the case if the protective effect of these restrictions works multiplicatively. Without such baseline information on

these restrictions, answering broader questions about their role in value appropriation is difficult. Accordingly, we ask the following research question first:

Question 1. *How common are NDAs, NSAs, and NRAs? Do they tend to be jointly or separately adopted?*

2.2 | Why do firms adopt certain combinations of employment restrictions?

The standard justification for NCAs is that they are required to support investments in the development and sharing of valuable information. For example, Rubin and Shedd (1981) discuss a situation in which the firm is concerned that a worker with access to trade secrets may appropriate the value of such secrets without paying for them. Since the worker likely will not be able to pay for such trade secrets due to liquidity constraints (Johnson & Lipsitz, 2020; Wickelgren, 2018), the NCA serves as a promise by the worker not to appropriate such investments. If courts enforce NCAs and hold workers to their promises, the argument goes, we should observe a greater incidence of NCAs for workers with access to valuable information.

Prior research substantiates these ideas to some extent; NCAs are most likely to be found in jobs that require access to valuable information and resources, such as managerial or professional jobs (Lavetti et al., 2020; Rothstein & Starr, 2021; Starr et al., 2021), with executives of publicly traded firms signing NCAs at a rate between 64 and 80% (Bishara et al., 2015; Garmaise, 2011; Shi, 2023). NCAs are also found, albeit in lower proportion, in low-wage jobs (Johnson & Lipsitz, 2020; Rothstein & Starr, 2021; Starr et al., 2021). Indeed, Colvin and Shierholz (2019) find in the only preexisting broad survey of firms that 31.8% of firms use NCAs with *all* their employees, while 49.4% use them for at least some workers.

While in theory, court enforceability of NCAs serves as a backstop to maintain investment incentives, there is mixed evidence on how NCA adoption relates to NCA enforceability. All available studies document that NCAs are still used in places where they are unenforceable (Colvin & Shierholz, 2019; Rothstein & Starr, 2021; Sanga, 2018; Starr et al., 2021), with some finding that NCAs are somewhat more common where they are enforceable (Lavetti et al., 2020; Sanga, 2018), and others finding little difference (Colvin & Shierholz, 2019; Prescott et al., 2016). To our knowledge, no study has examined how other legal protection mechanisms, such as those related to trade secrets (Contigiani et al., 2018; Png, 2017), relate to NCA enforceability.

These existing arguments regarding NCAs offer useful guidance for when we might expect NDAs, NSAs, and NRAs to be adopted, particularly as theory about the latter is largely undeveloped.² Because these restrictions also protect against the diffusion of valuable information and resources whose leakage may hurt firms, we might expect firms to deploy them in jobs that involve access to such resources. Moreover, because we have more than one restriction, it is important to consider how firms might substitute (or not) between these restrictions based on

²We could develop a broader theory that examines how various factors, including differences in costs of adopting the restrictions, costs and likelihood of enforceability, and so forth might drive adoption of certain restrictions individually and in various combinations. Given the focus of prior literature and data availability, we limit our attention to the legal framework and a worker's access to valuable information, but emphasize the opportunities for this line of thought in the discussion.



state policies. For example, if NCAs are unenforceable, and the other restrictions can (partially) substitute for NCAs, firms may shift away from NCAs toward other restrictions. In contrast, if the protection afforded is complementary, firms may choose to reduce their reliance on other restrictions. Similarly, if a state adopts the Inevitable Disclosure Doctrine (IDD)—which allows a firm to preclude a departing worker from moving to a competitor on the basis that they would inevitably disclose trade secrets, even without an NCA—then perhaps they do not need these agreements since the IDD can be used by firms to protect against the leakage of valuable resources. This leads to our second research question.

Question 2. *How do resource access and protection-related factors, such as state policies on NCA enforceability and IDD, relate to the set of restrictions firms adopt?*

2.3 | Restriction adoption and value appropriation

An important question for strategy is whether and how these restrictions relate to value appropriation. In this regard, prior literature on NCAs suggests that the underlying motivation for using NCAs is to protect firms from the leakage of valuable resources. However, by imposing restrictions on workers, *ex post*, they also confer (temporary) monopsony power to firms, which can be used by firms to appropriate value from workers (like how patents not only protect inventions by granting firms temporary monopoly power, but also allow “trolls” to use them purely to extract value). Indeed, that firms can use NCAs to insulate themselves from labor market competition (Hardaway, 2015) and potentially reduce wages are key reasons why NCAs have been a topic of longstanding debate (Balan, 2021; Balasubramanian et al., 2020).

In classic efficient contracting theories in labor economics, value appropriation in the firm-worker relationship is the outcome of a two-sided bargaining process, where workers and firms negotiate over and voluntarily agree to the terms of the employment contracts, including any restrictions (Rubin & Shedd, 1981). These models incorporate a participation constraint, which implies that workers only agree to NCAs when, in expectation, they are weakly better off than they would be otherwise, either through receiving an *ex ante* compensating differential sufficient to compensate for any *ex post* harm (Shi, 2023), or through investments induced by the NCAs that pass through to worker earnings (Kini et al., 2020; Lavetti et al., 2020). Such models assume that if a worker were to expect harm from agreeing to an NCA, they would negotiate for agreeable terms or not take the job.

However, various behavioral factors or frictions in labor markets mean that workers may not be able to bargain for such efficient outcomes, and firms may still be able to use NCAs to appropriate value from workers. For instance, firms may delay when they offer the noncompete to the first day of the job (Marx, 2011; Starr et al., 2021), making it difficult for workers to bargain. Workers may also be unaware of the law (Prescott & Starr, 2022) or otherwise lack the wherewithal to understand or negotiate their contract terms, even if they are unenforceable (Starr et al., 2020). All these factors reduce the workers' bargaining power relative to firms. In this regard, legal scholars have been concerned that employees do not read the details of their contracts *ex ante* (Arnow-Richman, 2006) and overestimate their protections *ex post* (Kim, 1997). Broadly then, there is a question about not only the effect of NCAs on value appropriation, but also whether NCAs have differential effects for workers with low versus high sophistication and bargaining power (Krueger & Posner, 2018).

Of course, if firms could costlessly impose NCAs (or other restrictions) on workers and appropriate value—and there are benefits to using them—then we should observe all workers being bound by NCAs. Yet this is not true. Thus, there must be either some costs to requiring NCAs that, for at least for some workers, outweigh any firm benefits, or there must be some other behavioral explanation (e.g., perhaps an owner had a bad experience with a noncompete and does not wish to impose one on the workers). Setting aside potential behavioral explanations, requiring NCAs may come with costs for at least some workers, especially those who are willing to bargain, are sophisticated enough to review it, and may turn down the job if the firm insists on the noncompete (Cowgill et al., 2024; Ganco et al., 2023). At the same time, firms may also find it costly to engage in negotiations with every worker, and hence, may adopt boilerplate contracts, so that groups of workers within the firm are covered by the same set of restrictions.

Evidence from the literature on NCAs is conflicted on the overall direction of the NCA-earnings relationship (Starr, 2021). A major disconnect comes from the level of analysis. Well-identified studies of state NCA policies (at the state level) tend to find that where NCAs are more likely to be enforced, firms tend to invest more (Jeffers, 2022; Starr, 2019b), but both low- and high-wage workers (including high-tech workers and executives) are less mobile and have lower earnings (Balasubramanian et al., 2020; Fallick et al., 2006; Garmaise, 2011; Johnson et al., 2021; Lipsitz & Starr, 2022; Marx et al., 2009; Starr, 2019b; Young, 2021). In contrast, every study of NCA use—including studies of the average worker and of physicians and executives—finds that NCAs themselves are associated with higher earnings (Kini et al., 2020; Lavetti et al., 2020; Rothstein & Starr, 2021; Shi, 2023; Starr et al., 2021). It is possible to theoretically resolve these directionally discordant findings (e.g., through negative spillovers to workers without NCAs, Starr, 2019a; Starr, 2019b), but a positive selection effect on earnings may be a more practically plausible explanation.

In light of this large prior literature on NCAs, when we consider the broader suite of employment restrictions, it is natural to think about how they also relate to value appropriation from workers—either on their own or in combination. For example, Graves (2022) makes a theoretical argument that NRAs are tools to capture value, courts have determined that the other restrictions can also be as restrictive as NCAs,³ and Lobel (2021) argues that the co-adoption of multiple restrictions makes for an ironclad combination that multiplicatively locks workers in and allows firms to extract value from them. At the same time, the same efficient contracting theories that apply to NCAs also apply to the broader set of restrictions as well. Thus, any such relationship between earnings and restrictions that put *ex post* limits on the transferability of resources is ambiguous—and may well depend on employee bargaining power. And just as the empirical literature on NCA use has been unable to address whether selection into restriction adoption drives overall wage differentials, we may wonder if selection has a role here as well. This leads to our third question:

Question 3. How do different sets of restrictions relate to value appropriation from workers?

³See, for example, *TLS Management & Marketing Services, LLC v. Rodriguez-Toledo*, No. 19-1104 (1st Cir. 2020) and *Deere Emps. Credit Union v. Smith* (Ill. App. Ct. 2016) regarding rulings that find that broad NDAs are effective NCAs.



3 | DATA

Our primary source of data is an employee-level survey that was the result of a collaboration with an American compensation software and data company, Payscale.com (“Payscale”). Payscale deployed the survey to individuals who visited the website between February 7, 2017, and August 28, 2017, and indicated their interest in knowing their earnings potential.⁴ Overall, 44,523 individuals responded to the survey and completed the demographic questions.⁵ We then limit the sample to private sector, working-age employees, comprising 35,983 individuals.⁶ We further excluded those missing data on any of the four employment restrictions, which leads to our final sample of 33,637 individuals. This sample is three times larger than the largest previous nationally representative survey (Prescott et al., 2016). Table A2 shows that the differences in demographics between our final sample and those who are missing data on the employment restrictions are negligible. The precise wording and question structure for the restrictions is in Figure A1.

Since the sample of individuals visiting Payscale.com and completing the survey is likely not random, we weight the individual data to match the US population by income, age, gender, and for-profit status of the employee.⁷ Table A3 compares the weighted and unweighted individual data to the American Community Survey data for 2017 (Ruggles et al., 2020), which reflects the US population. The table shows that on average our unweighted sample is younger, more female, higher earning, and more likely to be in the nonprofit sector. Weighting virtually eliminates these differences, however (though it does not necessarily remove differences in unobservables). Below, unless stated otherwise, we report weighted results for the individual-level data.

We complement this data with a 2017 firm-level survey, deployed annually by Payscale to HR professionals and leaders within the firm (and independent of the individual-level survey).⁸ We limited our sample to private or publicly traded firms located in the United States with nonmissing answers regarding the use of restrictions and key independent variables ($N = 1855$).

In addition to these Payscale datasets, we draw from two alternative datasets to develop our empirical measures for access to valuable information as well as bargaining power at the job level. The bargaining power question derives from a question on the National Longitudinal Survey of Youth 1997, a long-running survey conducted by the Bureau of Labor Statistics, which asks workers whether they bargained over their wages. With regard to access to valuable resources, we leverage the nationally representative dataset from Prescott et al. (2016), which surveys workers about their access to trade secrets, working with clients, and access to client

⁴The survey was marketed as a “Salary Survey” and came with the tagline “Do you know what people like you are earning? Stop guessing.” Thus, respondents have an incentive to respond accurately to the information, so that they can get accurate information on their earnings potential.

⁵Demographics include age, gender, wage, employer type, employer size, job level, industry, occupation, and state.

⁶We dropped individuals outside of age 18–65, those who were not working, or who were working in public administration, education, and fishing and forestry occupations, as well as independent or government contractors.

⁷We used iterative proportional fitting (“raking”) to create the weights. We matched on age (deciles), gender, income (quartiles), and whether the employee is for-profit or nonprofit. We considered several alternative weighting schemes, but this set did the best in terms of matching overall fit without producing substantial imbalance in the weights.

⁸Table A4 shows the distribution of job characteristics for the individual who filled out the survey on behalf of the firm. Most of the time it was a “Manager” (36.6%) or “Director” (23.1%) whose job functions included “Human Resources” (54.8%). These facts are reassuring since human resource managers or directors are very likely to know the types of employment restrictions and practices used by the firm. Information on the size distribution of firms in the survey is provided in Table A5. The precise wording and question structure is available in Figure A2.

lists. For both surveys, we aggregate the relevant measures to the occupation by industry level (two-digit SOC by two-digit NAICS) and merge with the individual-level Payscale data at that level.

4 | EMPIRICAL FINDINGS

Below, we present the findings from our inquiry into the three aforementioned questions. Because the empirical techniques needed to answer each question vary, we describe them as we go.

4.1 | The (joint) adoption of employment restrictions

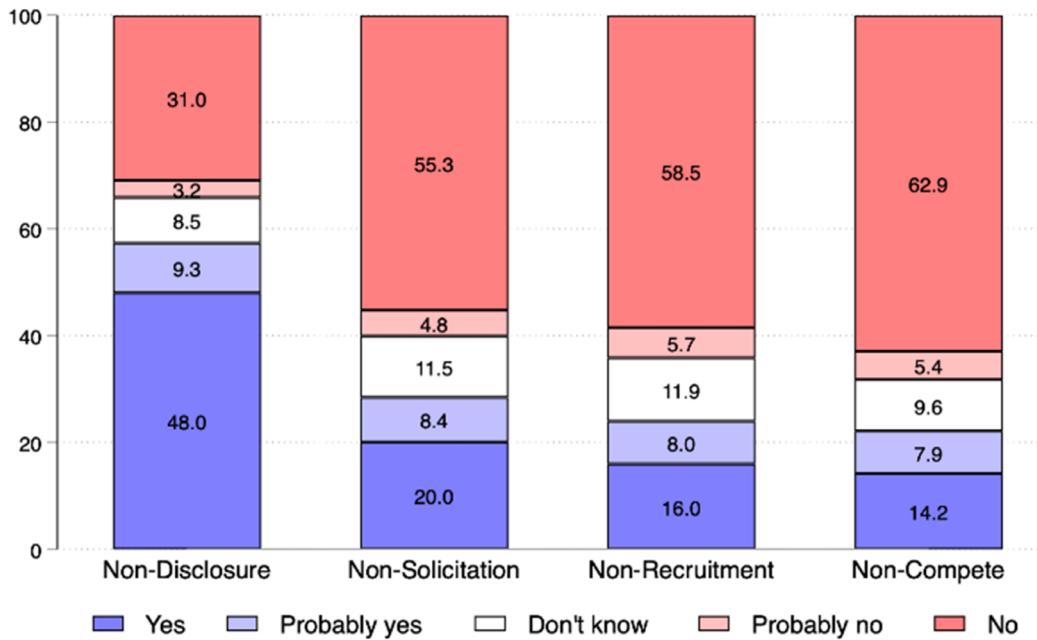
Figure 1, Panel a shows the weighted distribution of each of the four restrictions in the individual-level data (unreported, unweighted results are similar). Approximately 57% of employees in the United States in 2017 were definitely or probably bound by an NDA, with 8.5% not knowing if they were bound.⁹ Following NDAs, NSAs are the next most common restriction—28.4% of employees report agreeing to or probably agreeing to one. On the heels of NSAs are NRAs, which bind 24% of employees. Finally, although they have received the most attention in the literature, NCAs are the least common of these restrictions, and cover 22.1% of employees, similar to prior estimates (Rothstein & Starr, 2021; Starr et al., 2021).

Panel b of Figure 1 shows the distribution of the responses for each of the four restrictions in the firm-level data. The pattern of results is similar to the individual data: 70.9% of firms use NDAs with all of their employees, while another 17.3% use them with some but not all of their employees. Following NDAs, 40.9% of firms use NSAs with all employees and 28.5% report using them with some employees, while NRAs cover all employees at 32.6% of firms and some employees at 24.2% of firms. Consistent with the individual-level data, NCAs are the least common restriction, as 29.5% of firms report using them with all employees and 37% report using them with some but not all employees. These statistics about NCA adoption at the firm level are very similar to those in Colvin and Shierholz (2019), which suggests the estimates of the restrictions are likely credible.

Table 1 presents the joint distribution of all 16 combinations of restrictions. Here, we exclude those reporting that they do not know about the use of a specific restriction because we cannot create bundles for them. Among the 16 combinations, column (1) shows that just three combinations cover 82.3% of employees: 38.4% of employees have no restrictions, 25.9% have only an NDA, and 18% have all four. Columns (2) and (3) show similar results from the firm survey. In the firm-level survey, defining restriction adoption as firms using it for all employees, column (2) shows that 70.6% of firms use one of no restrictions (22.2%), only an NDA (25.7%), or all four restrictions (22.7%). Column (3) shows that if we define restriction adoption as using a restriction for all or some employees, 55.2% of firms use all four restrictions, 10.9% use only an NDA, and only 5.2% of firms use no restrictions for any employees.

⁹Since employees may not know what they have agreed to, we allow for uncertainty by giving them the chance to assess whether they have definitely or probably signed, or whether they have no idea. In general, when we report that an employee agrees, we group the definitely and probably agreed together.

(a) Individual-Level Survey Data



Results are from the weighted sample.

(b) Firm-Level Survey Data

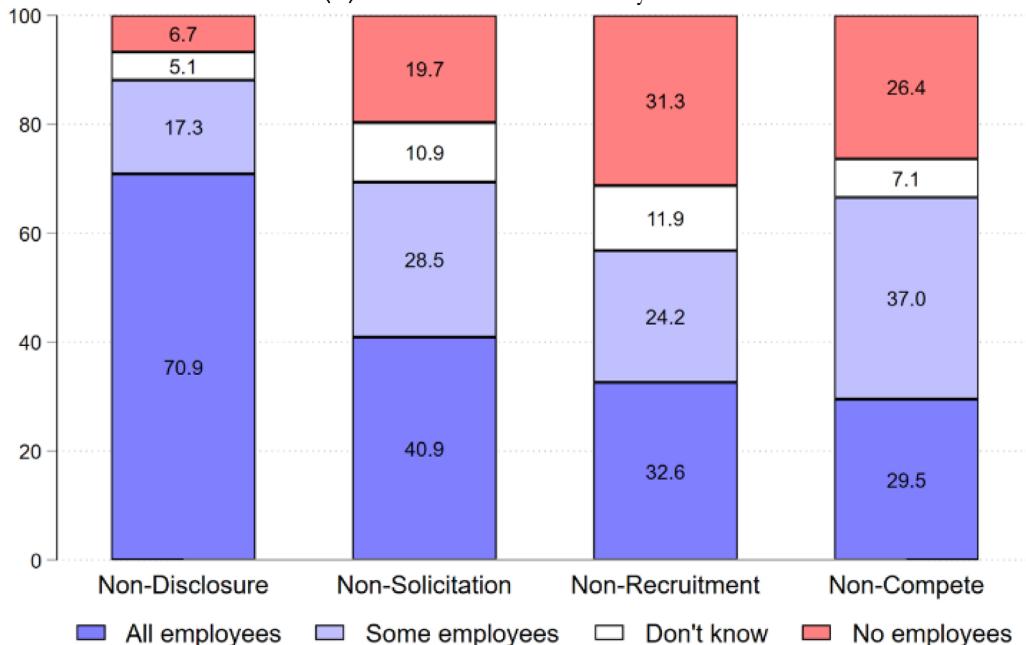


FIGURE 1 Incidence of postemployment restrictions. Panel a. Individual-level survey data. Panel b. Firm-level survey data.



TABLE 1 Joint distribution of restrictions.

Combination of restrictions: (NDA, NSA, NRA, NCA)	Category	Individual-level data (1)	Firm-level data	
		1(adopt) = yes or probably yes	1(adopt) = all employees (2)	1(adopt) = all or some employees (3)
(0,0,0,0)	None	38.4	22.2	5.2
(1,0,0,0)	Only NDA	25.9	25.7	10.9
(0,1,0,0)		0.3	1	0.2
(0,0,1,0)		0.1	0.5	0.1
(0,0,0,1) ^a		0.6	0.7	0.9
(0,1,1,0)		0.3	0.7	0.1
(0,1,0,1) ^a		0.2	0.3	0.7
(0,0,1,1) ^a		0.1	0	0
(0,1,1,1) ^a	Other	0.2	0.1	0.2
(1,1,0,0)		3.1	4.5	3.2
(1,0,1,0)		1.6	1.6	0.5
(1,0,0,1) ^a		2	3	3.8
(1,1,0,1) ^a		2.6	5	10
(1,0,1,1) ^a		0.5	0.5	1.2
(1,1,1,0)		6.2	11.5	7.8
(1,1,1,1) ^a	All Four	18	22.7	55.2
Total		100.0	100.0	100.0

Note: In column (1), adoption of each restriction includes those who indicate they agreed or probably agreed. In column (2), adoption of each restriction is equal to 1 if the firm uses that provision for all employees. Column (3) considers firm-level adoption as 1 if the firm uses that provision for or all or some employees. “Don’t know” responses are omitted.

^aCombinations that include NCAs.

The joint distribution in Table 1 highlights two important relationships among these restrictions. First, if a worker has, or a firm uses, an NCA, then there is a 70–75% chance that all three other restrictions are present (see Table A6 for pairwise adoption).¹⁰ That probability is only 30% for NDAs, and between 58 and 67% for NSAs and NRAs in both datasets. Second, and in contrast, if a worker has an NDA, then there is only a 38–50% chance of having an NCA, NRA, or an NSA (with similar numbers for the firm-level data). In contrast, if a firm adopts an NSA, NRA, or NCA, there is at least a 95% chance that an NDA is also adopted. These results suggest that NDAs are the baseline restriction, and others layer on top of it. In Table A7, we formally test and confirm these patterns—that NDAs are the most likely to be used alone while NCAs are

¹⁰This is calculated as the probability of having all four restrictions divided by the sum of the probability of all combinations with an NCA (combinations highlighted with an asterisk in Table 1), which for the individual level is 18/24.2 = 0.74.



the most likely to be bundled with the other three restrictions—while controlling for several firm and individual characteristics.

Taken together, these findings strongly suggest that firms perceive complementarities among the restrictions. We revisit potential rationales for complementarities, as well as why NDAs might stand alone, in our discussion. These findings may also matter for thinking about potential outcomes of restriction use and for re-interpreting work on just one restriction. For example, these results suggest that a comparison between a worker with versus without an NCA is mostly a comparison between workers with all four restrictions and a weighted average of workers with only an NDA and no restrictions. Accordingly, the actual treatment in studies of NCA use is (likely) all four restrictions. Similarly, the heterogeneity in those without NCAs asks whether those with only an NDA and those with no restrictions should be in one control group, or whether there is heterogeneity across the groups that motivates separating them. We revisit these ideas in our value appropriation analysis in Section 4.3.

4.2 | Where and why do firms adopt particular sets of restrictions?

A natural question after observing these baseline patterns is whether the adoption of restrictions is (uniformly) random. To test this directly, we simulate a distribution in which employees randomly have or in which firms randomly use restrictions for all workers, keeping the sample probability of adopting each restriction constant. We then test whether this simulated distribution is different from our observed distribution. For both the firm-level and individual-level data, a Kolmogorov-Smirnov test rejects the null that these observed and simulated distributions are the same with a p -value $<.01$ in each of 1000 simulations. Thus, it is extremely unlikely that firms are (uniformly) randomly choosing among the 16 combinations.

Since the joint distribution of restrictions is far from uniformly random, it is natural to wonder what factors shape the combination of restrictions firms select. While there are many possible such factors, following the literature on NCAs, as discussed above, we focus on resource access and protection-related factors such as state policies related to NCAs and the IDD.

To examine access to valuable resources, we leverage the employee-level survey and employ a multinomial logit regression with the dependent variable as the four possible combinations of restrictions: “None,” “Only an NDA,” “Others,” and “All.” We proxy for access to valuable resources in two ways. First, we use data from Prescott et al. (2016) to calculate how likely an employee is in their current occupation-industry to access trade secrets, client information, and work directly with clients. We create a measure of overall “access to information and relationships” by multiplying these three likelihoods together and merge in by occupation-industry. Second, we also examine whether the worker is a top manager (defined as being a CEO, vice president, or director), which covers 10.2% of our sample,¹¹ under the assumption that top managers have access to valuable resources. See Table A8 for summary statistics of sets of restrictions, and Table A9 for the incidence of each restriction by occupation and industry.

Panel a of Table 2 shows the results, which includes a full set of control variables—age, gender, class of employee, log firm size, and state fixed effects (industry and occupation fixed effects

¹¹This includes 1988 directors, 465 chief executives, and 378 vice presidents.



TABLE 2 Restriction adoption by resource value and top manager status.

Dependent variable	(1) 1(None)	(2) 1(Only NDA)	(3) 1(Other)	(4) 1(All)
<i>Panel a. Top manager status and access to valuable resources</i>				
P(Work w/clients) × P(Client info) × P(Secrets)	-0.514 (0.054)	0.149 (0.036)	0.162 (0.027)	0.203 (0.029)
Top Manager	-0.045 (0.013)	0.006 (0.011)	0.021 (0.009)	0.018 (0.010)
<i>p</i> -Values from test of relative risk ratio P(All)/P(None) = P(Only NDA)/P(None) for				
P(Work w/clients) × P(Client info) × P(Secrets)				0.013
Top manager				0.308
<i>Panel b. NCA enforceability and inevitable disclosure doctrine</i>				
NCA non-enforceability	-0.013 (0.002)	0.011 (0.003)	0.007 (0.002)	-0.005 (0.002)
Favorable IDD	-0.015 (0.011)	-0.003 (0.010)	0.006 (0.006)	0.011 (0.006)
Against IDD	-0.005 (0.009)	0.000 (0.009)	0.003 (0.006)	0.003 (0.008)
<i>p</i> -Values from test of relative risk ratio P(All)/P(None) = P(Only NDA)/P(None) for				
NCA non-enforceable				0.002
Favorable IDD				0.228
Against IDD				0.836

Note: The results are from a multinomial logit model, where the dependent variable is categories for the four mutually exclusive combinations of restrictions (None, Only NDA, Other, All Four). The estimates have been converted into average marginal effects such that each coefficient can be interpreted as a percentage point increase for a one-unit increase in the independent variable. Since an increase in the likelihood of being in one category must be offset with a decrease in another, each row must add up to 0. The NCA non-enforceability is based on the 2009 NCA enforceability measure from Starr (2019b), calculated as: (NCA enforceability/s.d.) × (-1). Panel a is based on 27,476 observations while Panel b is based on 27,804 observations. Panel a includes controls for age, gender, the class of employee, log firm size, and state FEs, while Panel b includes age, gender, the class of employee, log firm size, industry FEs and occupation FEs. Standard errors are clustered at the firm level in Panel a and at the state level in Panel b.

are not included because information and relational access is at the industry-occupation level), with standard errors clustered by firm. The estimates have been converted into marginal effects, reflecting the percentage point increase or decrease in a given outcome category from a one unit increase in a given independent variable. The results support the thesis that *access to valuable resources is a reason for an employee to be bound by all four restrictions*. A 10 percentage-point increase in information and relational access increases the likelihood that an employee is bound by all four restrictions by 2.03 percentage points (10.7% increase relative to the sample mean; *p*-value <.001), and the likelihood of only an NDA by 1.49 percentage points (5.5% increase; *p*-value <.001). This All vs. only NDA difference rejects a null hypothesis of no difference at *p*-value = 1.3% (based on relative risk ratios). Further, the likelihood of being bound by no restrictions falls by 5.14 percentage points (14.7% decrease; *p*-value <.001). Similarly, relative to other employees, employees in top management are 1.8 percentage-point more likely to have



all the restrictions (9.5% increase; p -value = .066) and 4.5 percentage-point less likely to have no restrictions (12.9% decrease; p -value = .001).¹²

Panel b of Table 2 examines state-level variation in NCA enforceability and IDD. To code NCA enforceability, we use a continuous enforceability measure developed by Bishara (2011) and Starr (2019b) and reverse it to represent non-enforceability. To code whether a state has adopted IDD, we follow Castellaneta et al. (2016) and use three categories: states favorable toward the IDD, against the IDD, or have no policy on the IDD (the base category). Note that because our survey is cross-sectional, we cannot exploit changes to these NCA and IDD laws over time as some prior literature has done; thus we can only exploit cross-state variation. Hence, in panel b, we omit state fixed effects (since otherwise NCA enforceability and IDD would not be identified), while including the other controls and industry and occupation fixed effects.

The results in Panel b of Table 2 show that a one standard-deviation increase in non-enforceability of NCAs is associated with a 1.3 percentage point (3.7% of the sample mean; p -value <.001) reduction in the use of no restrictions, increases in the likelihoods of only an NDA by 1.1 percentage point (4.0% of the mean; p -value <.001) and other combinations by 0.7 percentage point (3.7% of the mean; p -value <.001), and decreases the likelihood of using all four restrictions by 0.5% percentage point (2.6% of the mean; p -value = .046). In contrast, we find that rejecting IDD has little relationship with the combination of restrictions a firm chooses, but that in states favorable to IDD, workers are marginally more likely to have all four restrictions. Table A10 replicates these broad patterns in the firm-level data. Finally, to examine NCA substitution more explicitly, Table A11 replicates the NCA enforceability and IDD analysis in the individual-level survey with an independent category of All but NCA (i.e., bound by NDA, NSA, and NRA but not an NCA), separated from Others. The result shows that the NCA non-enforceability is associated with a 0.7 percentage point (10.8% of the mean; p -value <.001) increase in the likelihood of all restrictions but NCAs, with no such increase in the remaining other category. This provides some evidence that firms substitute away from all four restrictions toward the set of the other three (NSA, NDA, NRA) restrictions when NCAs are less enforceable.

Taken together, these results offer modest evidence consistent with the theoretical arguments in the prior literature related to hold-up. This includes, for example, that workers who have access to more valuable resources are more likely to be bound by these restrictions. However, the explanatory power seems small. For example, including top managers and access to information in the model marginally increases the correct prediction rate from 35.7 to 35.9%. The evidence on NCA enforceability suggests some moderate substitution based on the law—when states are less likely to enforce NCAs, firms are more likely to use only an NDA or some other set of restrictions and less likely to use all four. Finally, even though IDD allows firms to restrict worker movement to competitors without having workers agree to NCAs, we find that where IDD is favored workers are marginally more likely to have all four restrictions, not less. We revisit this puzzle in our discussion.

¹²Industry analyses in the firm-level survey are consistent with these findings. The industries in which firms are most likely to use all four restrictions and least likely to use none are technology (41.8% use all four vs. 10.2% none), marketing and public relations (35.5% use all four vs. 6.5% none), and biotech and science (33.3% use all four vs. 2.8% none).

4.3 | Restriction adoption and value appropriation

In this section, we focus on how the adoption of restrictions relates to value appropriation from workers, as measured by their earnings. We also consider the role of selection into restrictions in driving these results and look for heterogeneity based on the extent of worker bargaining power. One important challenge to estimating such relationships is that with four restrictions there are 15 potential comparison groups for any given set of restrictions. It is not necessarily obvious (to the researcher) which one of those 15 comparisons reflects the comparisons a typical firm was making. To simplify our analysis, make interpretation easier, and preserve statistical power, we focus on the three main combinations of restrictions found earlier—None, Only an NDA, and All four—since they account for the vast majority of firms and workers.

We take a descriptive empirical approach in which we analyze the unconditional relationships between earnings and restriction adoption, and then condition on several related controls and firm fixed effects. In the ensuing section, we consider whether these results may be driven by various forms of selection. Our most saturated specification is:

$$Y_{ijoks} = \beta_0 + \beta_1 All_i + \beta_2 OnlyNDA_i + \beta_3 Other_i + \gamma X_{ij} + \lambda_s + \delta_o + \xi_k + \alpha_j + e_{ijoks} \quad (1)$$

where Y_{ijoks} is log annual earnings for individual i in firm j , in occupation o , in industry k , and state s . State fixed effects are given by λ_s , while δ_o and ξ_k refer to occupation (two-digit SOC) and industry (two-digit NAICS) fixed effects. In contrast to prior studies which did not have firm-level identifiers (e.g., Rothstein & Starr, 2021; Starr et al., 2021), in some specifications, we also include firm fixed effects in α_j .¹³ Included in X_{ij} are controls for age, gender, whether the employee is in a for-profit or nonprofit firm, and the log of firm size. We cluster standard errors at the firm level.

One way to highlight the challenge relating to the multiplicity of comparison groups is to think about the omitted category. In the above specification, the omitted category of restrictions is “None,” such that β_1 refers to the average difference in log earnings between those with all four restrictions and those with none, while β_2 refers to the average difference in log earnings between those with only an NDA and those with none. Alternatively, we can reestimate the model with Only NDA as the baseline omitted category (this is numerically identical to estimating the difference between β_1 and β_2 when “no restrictions” is the reference category, as in Equation 1).

Table 3 presents the coefficients, while Figure 2 provides a visual representation. The left panel of Figure 2 shows the predicted log earnings from estimating an unconditional model, a model with controls, and a model with controls and firm fixed effects, corresponding to the columns of Table 3 (note that in the conditional models, the figure shows the expected effect while holding the covariates at their sample means). *The resulting pattern is a non-linear relationship of earnings across the three main sets of restrictions.* Relative to workers with no restrictions, workers with all four restrictions have 4–17% higher earnings (see the middle panel of Figure 2 and columns 1–3 of Table 3), but relative to workers with only an NDA, workers with all four restrictions have 3–7% lower earnings (see the right panel of Figure 2 and columns 4–6 of Table 3). The p -values for tests of each difference are reported in Table 3.

¹³Note that including firm fixed effects reduces effective sample size because firms with just one observation in the data are dropped. The effective sample of individuals representing firms with at least two or more observations is 7527. Unreported robustness checks confirm that our main results are not sensitive to using this specific subsample.



TABLE 3 Log annual earnings and main combinations of restrictions.

Dependent variable Ln(annual earnings)	(1) Base category: No restrictions	(2) Base category: No restrictions	(3) Base category: No restrictions	(4) Base category: Only NDA	(5) Base category: Only NDA	(6) Base category: Only NDA
All four	0.159 (0.023)	0.064 (0.016)	0.044 (0.032)	-0.076 (0.024)	-0.030 (0.016)	-0.053 (0.030)
Only an NDA	0.235 (0.019)	0.093 (0.013)	0.097 (0.025)			
Others	0.236 (0.023)	0.078 (0.015)	0.052 (0.032)	0.001 (0.024)	-0.016 (0.016)	-0.046 (0.033)
No restrictions				-0.235 (0.019)	-0.093 (0.013)	-0.097 (0.025)
Constant	10.489 (0.013)	9.815 (0.052)	10.159 (0.156)	10.724 (0.015)	9.909 (0.054)	10.256 (0.159)
Observations	27,804	27,804	27,804	27,804	27,804	27,804
R-squared	.021	.510	.827	.021	.510	.827
p-Values (all four vs. base category)	<.001	<.001	.166	.001	.070	.078
δ (all four vs. base category)			0.676			2.047
Controls	No	Yes	Yes	No	Yes	Yes
Firm FE	No	No	Yes	No	No	Yes

Note: Controls include age, gender, class of employee, log(firm size), occupation FEs, industry FEs, and state FEs. Note that in the model with firm fixed effects, all firms with just one observation in the data are implicitly dropped. Standard errors in parentheses, clustered by firm.

4.3.1 | Selection into restriction use and alternative explanations

Given that the results above reflect conditional correlations, it is natural to wonder if selection into restriction adoption drives the results.¹⁴ We assess the potential role of selection in two ways. First, we consider whether selection into the use of any restriction can explain the potentially confusing finding that workers with all four restrictions appear to have higher earnings than those with no restrictions, but lower earnings than those with only an NDA. Second, we consider several selection-related stories and see if the implications of those stories hold in our data.

Our premise is that comparisons between workers with some restrictions and workers with no restrictions might be more highly selected relative to comparisons between workers with some, but different sets of restrictions. For example, our prior analyses showed that workers in jobs with access to valuable information like trade secrets and client lists were more likely to be bound by all four restrictions or only an NDA, and *less likely* to be bound by no

¹⁴Indeed, selection affects every study of NCA use that seeks to test hypotheses. Prior research has not found credible instruments that alter firms' restriction adoption choices but do not affect wages through any other channels. While studies try to avoid this problem by examining changes in state NCA policies, our results above and those of the prior literature make clear that NCA enforceability and NCA use are not the same (Prescott & Starr, 2022; Starr et al., 2020). Relatedly, we do not have panel data to study policy changes.

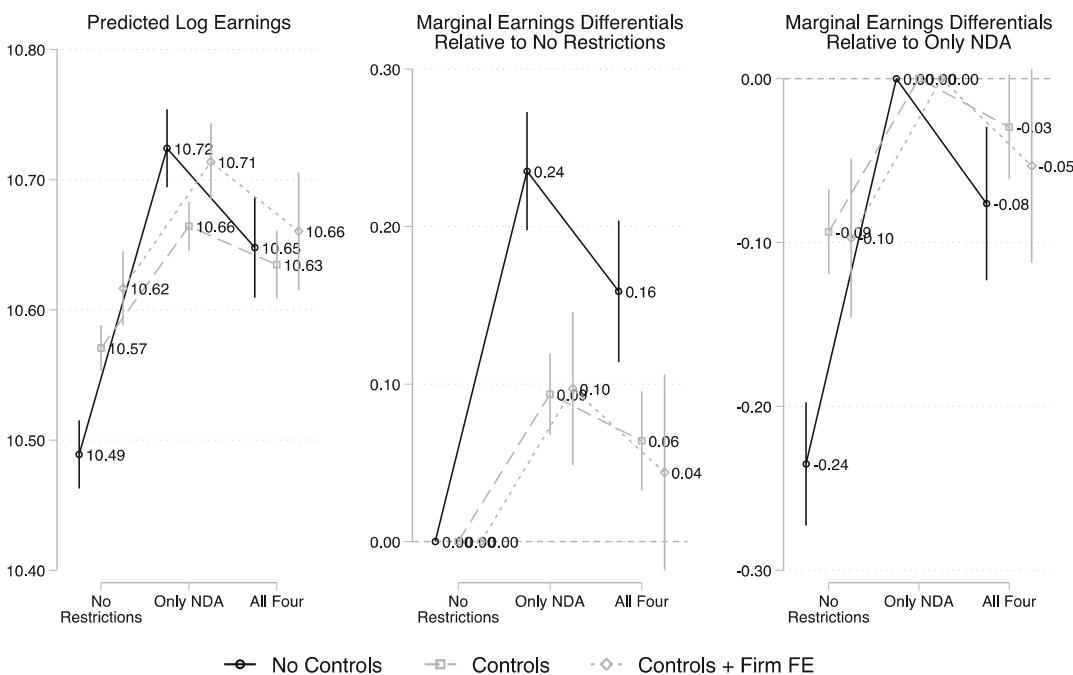


FIGURE 2 Main earnings results. These results reflect the estimates from Table 3. Controls include age, gender, class of employee, log(firm size), as well as occupation, industry, and state fixed effects. Standard errors are clustered by firm. The left panel shows expected log earnings, holding other covariates at their means. The middle and left panels examine marginal effects, changing the reference group from no restrictions to only an NDA. The middle panel shows the log earnings differential between no restrictions and all four, as well as no restrictions and only an NDA, as in columns (1)–(3) of Table 3. The right panel shows the log earnings differential between workers with only an NDA and those with all four restrictions as well as the differentiable between workers with only an NDA and those with no restrictions, as in columns (4)–(6) of Table 3.

restrictions. If workers with access to more valuable information (including potentially unobserved access) are also more highly compensated, then it may not be surprising that workers with only NDAs or all four restrictions earn more than those with no restrictions. So, if there is selection into the use of *any* restrictions, then the comparison between all four restrictions and only an NDA will net out this selection—since some restrictions are used in both cases—while the comparison to no restrictions will be biased by this selection effect. Hence, the former comparison will be less biased than the latter (note though that the former comparison will still not net out any selection unique to the use of all four restrictions). In Appendix B, we formally document these ideas using Monte Carlo simulations.¹⁵

To examine the potential for unobservable selection to bias certain comparisons, we use the method developed by Oster (2019). The idea underlying this test is that while we cannot include

¹⁵To see this, suppose that firms first choose whether they want to use an NDA, and if they do so then they choose randomly between only an NDA and all four restrictions. In this extreme situation, a comparison between all four restrictions and only an NDA will yield an unbiased estimate, while the comparisons to no restrictions will be biased. In the more realistic scenario that there is some selection into restriction use, but it is not random whether firms use only an NDA versus all four restrictions (as our analysis of access to valuable resources suggests), differencing between all four restrictions and only an NDA will still be less biased for the true effect relative to no restrictions, because it is differencing out some selection.



controls we never observe, we can learn about selection on unobservables from models where we do not use controls and models where we include them. The differences in the point estimates and the R^2 between these models determine the key diagnostic statistic, referred to as δ . If, as controls are added, the R^2 rises dramatically, and the point estimate stays the same, the results are less likely to be driven by selection on unobservables, since there will be less residual variation in the dependent variable to overturn the result. Alternatively, if the R^2 does not change much or the point estimate falls dramatically when controls are added, then we should have less confidence that the results are robust to selection on unobservables. Specifically, $\delta=1$ suggests selection on unobservables would have to be equally strong as selection on observables to overturn the results, while in the case $\delta>1$, then selection on unobservables would have to be stronger than selection on observables.

We report δ in Table 3, for both the comparison of workers with all four restrictions to workers with none, and the comparison between workers with all four restrictions and workers with only an NDA. We find that $\delta=0.676$ for the comparison of all four restrictions to no restrictions, while $\delta=2.047$ for the all four versus only an NDA comparison.¹⁶ Thus, as expected, the comparison between all four restrictions and only an NDA seems more robust to selection on unobservables than a comparison between all four restrictions and none. One can see this clearly in the change in the R^2 and the coefficients between models with no controls and models with all the controls. Relative to no restrictions, the coefficient on all four restrictions falls from 0.159 to 0.044—a rather large drop—while the R^2 rises from 0.021 to 0.827. Meanwhile, the coefficient on all four restrictions in the comparison to only an NDA falls from -0.076 to -0.053, a much smaller drop. This result also suggests that selection on unobservables would need to be more than twice as large as selection on observables to overturn the direction of the All versus Only NDA point estimate.

In Appendix C, we consider the plausibility of several other selection mechanisms. These include whether individuals with higher ability or bargaining power are more likely to bargain for fewer restrictions, or whether firms target these restrictions to workers with low-wage growth or low mobility. We find no evidence that these channels drive our results.

4.3.2 | Heterogeneity by bargaining power

Our previous analyses suggest that workers bound by all four restrictions versus workers bound by only an NDA have lower earnings on average, and that these differentials are not explained by several selection stories. We next turn to the question of how these results differ for workers with, versus without bargaining power. We consider two proxies for bargaining power. First, we look at heterogeneity for top managers versus non-top managers (an individual-level variable in our data), given that top managers are more likely to be represented by legal counsel during job negotiations (Rajgopal et al., 2012). Second, we leverage the fact that workers in certain jobs are more likely to bargain over their wages. In particular, we aggregate a question on the National Longitudinal Survey of Youth in 1997 (NLSY97) which asks whether employees negotiated over their wages to the occupation-industry level (two-digit SOC by two-digit NAICS). We then merge this into our data and refer to below-median occupation-industry cells as “low-bargaining.”

¹⁶Per Oster (2019), we set the maximum R^2 to the lesser of 1 or 30% higher than the R^2 in the most saturated model. The δ term is calculated using a comparison between a model with no controls and the most saturated model.



To study heterogeneous earnings effects related to restriction adoption and bargaining power, we add the main effect of our bargaining power proxy and interact it with the combinations of restrictions. Thus, the model looks as follows:

$$Y_{ijoks} = \beta_0 + \beta_1 All_i + \beta_2 OnlyNDA_i + \beta_3 Other_i + \beta_3 BP_i + \beta_4 All_i * BP_i + \beta_5 OnlyNDA_i \\ * BP_i + \beta_6 Other_i * BP_i + \gamma X_{ij} + \lambda_s + \delta_o + \xi_k + \alpha_j + e_{ijoks} \quad (2)$$

where BP_i refers to the individual-level proxy for bargaining power. One must be careful to consider the base category when interpreting these coefficients. For example, in the above specification β_1 refers to the average difference in log earnings between those who are bound by all four restrictions and those who are bound by none, among those with $BP_i=0$. In contrast, β_4 estimates how the All vs. None log earnings differential differs between those with $BP_i=1$ and those with $BP_i=0$.

To help simplify the relevant comparisons, the left panel of Figure 3 takes the results from estimating model (2), where “no restrictions” is the omitted category, and reports the marginal effects of the main combinations of restrictions, separately for $BP_i=0$ and $BP_i=1$. The right panel of Figure 3 replicates this analysis but changes the default comparison to “only an NDA.” In Panel a of Figure 3, we estimate model (2) using top managers as our proxy for bargaining power. In Panel b, we reestimate the model with wage bargaining in a job as our proxy for bargaining power. See Table A12 for the underlying estimates of model (2).

Across these proxies for bargaining power, the results are markedly similar. Relative to non-top-managers with only an NDA, non-top-managers with all four restrictions have 8% lower earnings; for those in low-bargaining power jobs, the same comparison is $-12.2\% (1-e^{(-0.13)})$. In contrast, for those with high bargaining power, workers seem to be weakly better off with all four restrictions relative to none or only an NDA. Across specifications, those with more bargaining power are better off when bound by all four restrictions versus only by an NDA, relative to the same difference for those with less bargaining power.

4.3.3 | Evidence on mechanisms from the firm-level survey

In this section, we consider three potential mechanisms that might underlie the negative earnings differential between workers with all four restrictions and only an NDA. Drawing from the idea that all four restrictions are more likely to isolate the worker from the external market, we might naturally expect that firms using all four restrictions experience less turnover and face less pressure to give workers raises. The combination of reduced wage growth and increased retention suggests that over time workers are likely to experience earnings losses, especially if they are not compensated upfront (due to, e.g., low bargaining power) for such *ex post* losses—which would be consistent with the individual-level results. An alternative mechanism is that workers reduce their effort and productivity under these restrictions (Garmaise, 2011). While the individual-level data does not include any proxies for retention, raises, or productivity, the firm-level data does. We briefly explore these outcomes using the firm-level data, estimating models like the individual-level analyses above. We cannot use firm fixed effects, however since we have only a single observation per firm.

In columns (1) and (2) of Table A13, we examine firms' willingness to raise wages using a firm-level question about whether the firm intends to increase base pay (84% of the sample

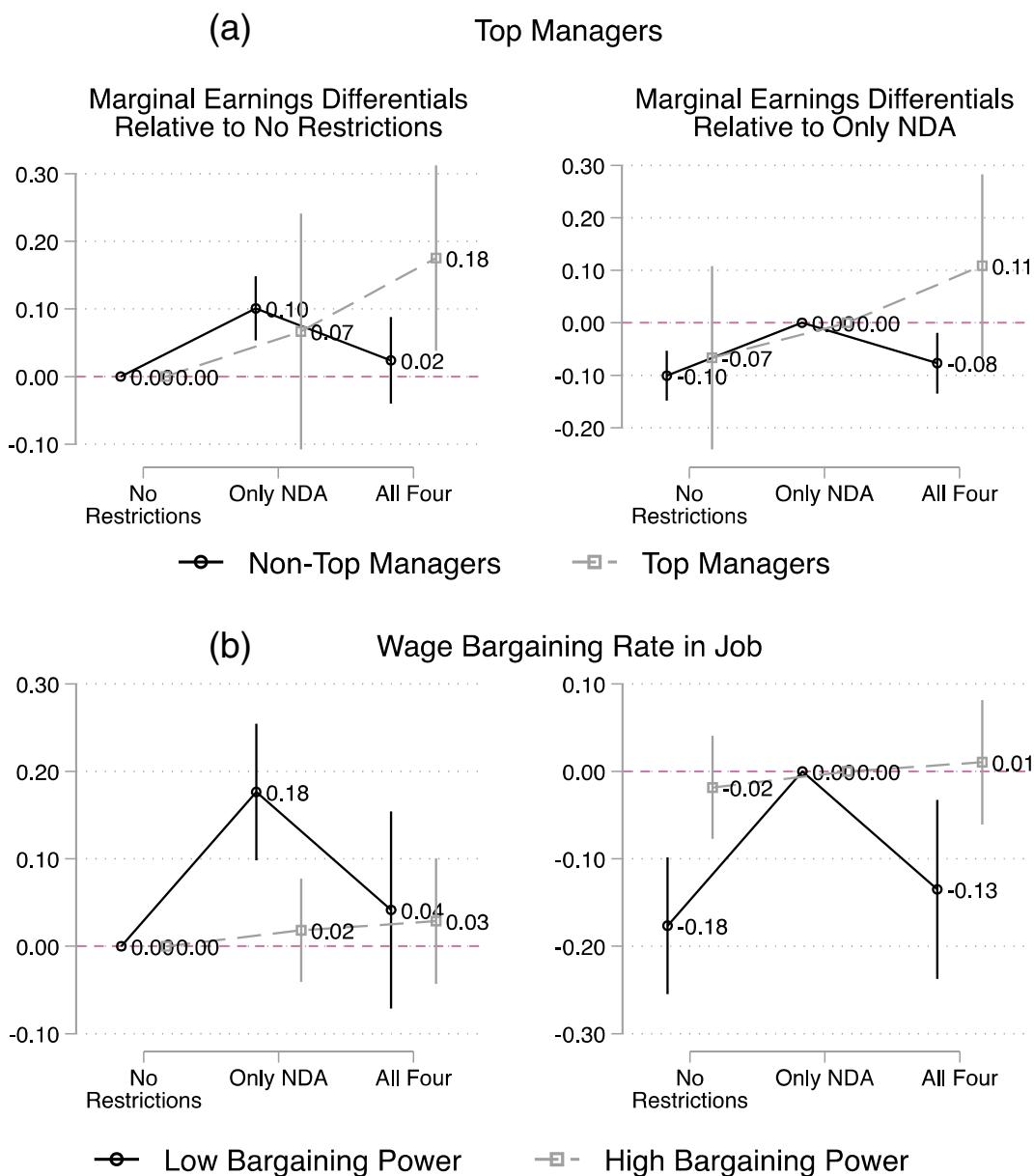


FIGURE 3 Heterogeneity in earnings results by bargaining power. The model underlying this figure includes individual-level controls and firm fixed effects, corresponding to even columns of Table A12. The left panel focuses on comparisons to workers with no restrictions, while the right panel focuses on comparisons to workers with only an NDA.

indicated that they would). It shows that firms that use all four restrictions are 5.5 percentage points (6.5% of the mean) less likely to increase base pay relative to firms that use only an NDA (p -value = .062). Taken together with the results in the individual survey, these results support the idea that the use of all four restrictions allows firms to appropriate more value both by decreasing the earnings *levels* of average employees (Table 3) and suppressing wage growth by not offering raises.

To examine employee retention, we leverage a question asking the extent to which the firm agreed with the statement: “Employee retention is a major concern for our company.” We coded this as a dummy equal to one if the firm agreed or strongly agreed, which 60% did as a baseline. Columns (3)–(4) in Table A13 show the results. When we include control variables, relative to firms that adopt only an NDA, firms that adopt all four restrictions are 7.7 percentage points (13% of the mean) less likely to perceive retention as a major problem (p -value = .082).

Finally, to consider possible productivity mechanisms, we leverage a question in the firm-level survey about training and examine if firms that adopt all restrictions are also more likely to spend 1 month or more on training their new hires (66.6% report that they are). The estimates in columns (5) and (6) of Table A13 suggest that firms that use all four restrictions provide, on average, *more* training than firms that use only an NDA, but the estimates are relatively imprecise (p -value of .18). Thus, there is no strong evidence in our data that the adoption of all four restrictions is associated with a decrease in employee training. Nonetheless, we cannot rule out that employee productivity does not decline through other channels (e.g., reduced motivation).

5 | DISCUSSION

This study takes a question-driven approach toward understanding whether and when firms (co-)adopt four employment restrictions that limit the transferability of valuable firm resources, and how such adoption relates to value appropriation by firms and employees (Coff, 1997; Coff, 1999; Molloy & Barney, 2015; Sevcenko et al., 2021). In this section, we describe the contributions of our results and the many new avenues of inquiry that they open.

5.1 | Baseline restriction adoption

Our baseline analysis of the (joint) adoption of these restrictions suggests that we have focused most of our academic efforts on employment restrictions that are the least common—and which may be eliminated in the US if the Federal Trade Commission’s proposed NCA ban takes effect. This finding, and the fact that firms tend to co-adopt these restrictions with only NDAs standing alone, has several substantive implications for prior and future research. First, if NCAs are banned, we need to expand our inquiries into the other restrictions and the resources they protect (e.g., clients, coworkers, information). Second, we should be careful when interpreting prior observational studies of, for example, only NCAs, since those estimates likely reflect the set of jointly adopted restrictions relative to a weighted average of potentially heterogeneous comparison groups. A related avenue for future research is that there may be other management practices that are also co-adopted, such as IP assignment agreements or arbitration agreements, which may similarly color the interpretation of our results.

Second, our findings strongly suggest that firms perceive complementarities between NCAs and the other restrictions since they are frequently co-adopted. It is an open theoretical question, however, where such complementarities might come from. One natural hypothesis is that lawyers take a “belt and suspenders” approach to resource protection. That is, if one restriction is unable to perfectly protect a certain resource, then the other restrictions may be able to help in the event that a restriction fails. This is obviously not true of all firms, however, since some firms adopt only an NDA, and some adopt none. What explains this discrepancy is an open question for future research.



Future research may also build on our joint adoption analyses to consider how these individual restrictions work in tandem with other firm-level protection mechanisms such as patents, secrecy, or complementary capabilities. Research questions might include how the adoption of employment restrictions differs depending on the source of competitive advantage for the firm, and whether individual restrictions and firm-level protection mechanisms are substitutes or complements. Such analyses may also help us understand why some firms do not use certain restrictions (e.g., perhaps they do not face the risk of resource outflows or have other mechanisms in place to mitigate such risks) and what the typical negotiation process over these restrictions entails. Finally, we may ask how firms deal with the curse of dimensionality associated with protection mechanisms. With one restriction, there are only two choices (adopt or not adopt). With four, there are 16. When combined with other possible protection mechanisms, the number of combinations explodes. Thus, there are likely to be simplifying mechanisms at work. “Boilerplating” is likely to be one of them (Choi et al., 2017; Lobel, 2021), but are there others and how much explanatory power do they have?

5.2 | Factors that shape restriction adoption

Our second contribution moves toward understanding when and why firms deploy certain key sets of restrictions. As expected, we find that firms are more likely to adopt all four restrictions and only an NDA when the worker has access to more valuable information. However, this variable has relatively low explanatory power. This raises an important avenue for future research: Why do firms adopt certain restrictions for the entire workforce versus for only a few workers? Is it that some firms find the other restrictions unfair? That those restrictions make it harder to recruit?

In addition, our analysis of the legal enforceability of NCAs and IDD raises some important new questions. Consistent with some prior work on NCAs (Shi, 2023), we find some evidence of substitution away from NCAs and toward other sets of restrictions when NCAs are less enforceable. This suggests that bans on NCAs may have more muted effects, as firms substitute toward similar protection mechanisms. More surprisingly, even though IDD adoption allows firms to effectively apply NCAs without having workers agree to them, we find that firms are marginally more likely to use all four restrictions when the state has adopted the IDD, not less. One possible explanation for this IDD result is that, perhaps in order to have the IDD applied in court in a given case, the firm actually needs to show that it is using NCAs and related restrictions to convince the court that they are protecting secrets via other means. This was the case in *Hydrofarm vs. Orendorff* (2008), where the Franklin County Court of Appeals suggested that the IDD may only be deployed where the employee has an NCA. If this is right, then the fact that favorable IDD rulings are associated with an increased likelihood of all four restrictions suggests that results from prior studies of IDD adoption may partly be driven by firms adopting all four restrictions. Ultimately, we need future research with longitudinal data on restriction use as well as variation in the adoption of the IDD to sort these out.

In addition to the factors studied above, future research may build a more refined conceptual model and study other industry, state, firm, or individual characteristics which explain the individual and joint adoption of restrictions. One may wonder, for example, how restriction adoption relates to the underlying competitive environment, risk of spillovers, and need for complementary assets. In addition, we have only studied NCA enforceability, but policies that ban or limit NSAs, NRAs, or NDAs may have differential effects. Combining litigation data on

these restrictions with state policy shocks (as in Hiraiwa et al., 2022 and Marx, 2022) may be especially revealing.

5.3 | Restriction adoption and value capture

Our third contribution is to uncover that workers with all four restrictions have lower earnings than those with only an NDA, but more than those with no restrictions. Our selection tests reveal that the comparison to no restrictions is more likely to be driven by selection on unobservables, while the comparison between all four restrictions and only an NDA is less susceptible to unobserved selection. We further show that these effects are driven by non-managers and workers with low-bargaining power and find evidence that part of the mechanism likely runs through firms holding on to workers longer and reducing the likelihood of a raise. If right, then our work suggests these restrictions can help firms appropriate value from non-managers and the average worker (relative to only an NDA). This finding contradicts efficient contracting theories (Friedman, 1991; Grossman & Hart, 1986; Rubin & Shedd, 1981; Williamson, 1975), suggesting that these theories need modification when applied to non-managers or workers with low bargaining power. Taken together, these results are consistent with the thesis that employment restrictions both limit resource outflows and increase firms' value appropriation from nonmanagerial, low bargaining power employees.

These findings also seemingly contradict prior studies of NCA use, which find that NCAs are positively related to wages (Kini et al., 2020; Lavetti et al., 2020; Rothstein & Starr, 2021; Shi, 2023; Starr et al., 2021). However, since those studies do not possess information on the other restrictions (or even firm fixed effects in most cases), we may wonder, if we only had data on a single restriction, as in the prior literature, whether our results might differ from those above. In Table 4, we present “naïve” regressions examining one restriction at a time—as if we did not have data on the others. Just as in the previous studies of NCA use, we find that each individual restriction is unconditionally positively related to earnings, with the NCA and NDA coefficients remaining positive even when including controls and firm fixed effects. In no specification do we observe precise negative estimates, as in our main results.

What explains the discrepancy between these naïve, positive estimates and the negative estimates found earlier between workers with all four restrictions and workers with only an NDA? One explanation for the positive, naïve wage differentials is that there are competing selection and treatment effects which we cannot sort out with just one restriction. For example, based on our joint adoption analyses, the naïve NCA estimate reflects a comparison between employees bound by all four restrictions and (mostly) a weighted average of employees bound by only an NDA or employees not bound by any restrictions. In this case, there may be negative earnings effects from all four restrictions but positive selection effects into the use of any restrictions. With multiple restrictions, we can (partially) disentangle this form of selection by changing the comparison group to those with only an NDA, but we cannot do this with data on just one restriction. If there is positive selection into restriction use, as our analyses suggest, then the positive, naïve NCA-wage relationship estimated here and in prior work is likely driven by selection.

These findings are important because they suggest selection as one possible way to reconcile the directionally discordant findings between studies of NCA use and NCA enforceability (Starr, 2021). If we take as given the more plausibly causal negative effects of NCA enforceability estimated in the prior literature, then it is difficult to reconcile those negative wage estimates



TABLE 4 Ln(annual earnings) and individual restrictions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable: Log(annual earnings)												
NCA	0.082 (0.008)	0.043 (0.013)	0.021 (0.023)									
NDA				0.1140 (0.007)	0.078 (0.011)	0.064 (0.021)						
NSA							0.047 (0.007)	0.020 (0.012)	-0.020 (0.024)			
NRA										0.038 (0.008)	0.006 (0.012)	-0.031 (0.027)
Constant	10.81 (0.005)	9.85 (0.052)	10.20 (0.159)	10.74 (0.006)	9.82 (0.052)	10.17 (0.158)	10.82 (0.005)	9.85 (0.052)	10.21 (0.159)	10.82 (0.005)	9.86 (0.052)	10.21 (0.158)
Obs.	27,804	27,804	27,804	27,804	27,804	27,804	27,804	27,804	27,804	27,804	27,804	27,804
R-squared	.004	.508	.826	.015	.510	.826	.002	.507	.826	.001	.507	.826
p-Values	<.001	.001	.341	<.001	<.001	.003	<.001	.091	.411	<.001	.648	.244
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Firm FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Note: These models analyze as if we have data only on that restriction, and compare those who have signed that restriction to those who have not signed, irrespective of any other restrictions they may have signed. So, for instance, columns (1)–(3) compare those who have signed NCAs with those who have not signed NCAs. ND_A, NCA, NSA, and NRA are dummy variables, which are equal to 1 if the employee has signed that restriction. Controls include age, gender, class of employee, log(firm size), occupation FEs, industry FEs, and state FEs. Standard errors in parentheses, clustered by firm.

with the positive NCA-specific estimates presented here and in the literature. Indeed, if NCAs do increase wages, and if the enforceability of NCAs both increases the use of NCAs and positively moderates the NCA-wage relationship, then the only way that NCA enforceability could be negatively associated with earnings is if there are substantial negative spillovers (Starr, 2019a; Starr, 2019b). A more plausible explanation, however, is that NCAs actually do reduce earnings on average, but that prior studies of NCA use have not been sufficiently able to separate this negative treatment effect from the effect of positive selection into NCAs. Thus, by finding that a less selected comparison—between all four restrictions and only an NDA—results in negative earnings estimates, we suggest a potentially more likely resolution to the sign discrepancy between studies of NCA use and NCA enforceability.¹⁷

5.4 | Future directions

We conclude by sketching out several additional directions for future research. Building from our appropriation results, if it is right that adopting all four restrictions protects valuable resources, enables firms to capture value from the average worker, and increases retention, then why are not all firms adopting such practices? Potential explanations include that firms vary in their benefits from such restrictions or are unaware of these benefits, that perhaps employees are dissuaded from joining firms using all such restrictions, or that unobserved employee effort falls commensurately with the adoption of these restrictions. Another explanation emerging from the legal literature is that such restrictions may be boilerplate, and that their adoption or reconsideration may not respond to market or economic forces as we might otherwise expect, as in Choi et al. (2017).

Another important direction for future research is to consider the impact of the mass use of these restrictions, similar to Starr (2019a) and Starr (2019b). For example, does the use of these restrictions by one or multiple firms make hiring more difficult for other firms? How do the potential external effects of these restrictions affect employee mobility, wages, productivity, investments, profitability, and M&A and alliance activity (Younge et al., 2015)? In a different vein, these restrictions appear to take otherwise fungible resources and tie them to the firm, creating a sort of “contract-induced firm-specificity.” Then, is their effect akin to other forms of specificity (e.g., asset-specificity) studied in transactions costs theory (Cuypers et al., 2021)? For instance, how do they affect the direction and mode of corporate diversification? (Silverman, 1999, 2002, Chap. 6). As noted above, another way to study these patterns is to look at litigation activity, as done in the patent context (Ganco et al., 2015). What litigation strategies do firms use and what are the implications for firms and the market?

Finally, our bundling results are relevant for researchers, policymakers, and practitioners concerned about the efficacy of individual restrictions or sets of restrictions. For example, a common argument for banning NCAs is that other restrictions can protect firms without so bluntly restricting employee mobility (Federal Trade Commission, 2024; Silverman, 2020). Examining this argument with observational data is challenging, however, because it is rare to observe all three restrictions without NCAs. Indeed, analyses of these comparisons in our data are underpowered and yield imprecise results. Accordingly, (quasi-) experimental research

¹⁷Note that to fully reconcile these results would require studying decomposing how changes in state NCA policy affect the use of NCAs, the moderating effect of NCAs on wages, and the spillovers to those without NCAs. We think this is a fruitful avenue for future research as data becomes available.



designs, albeit removed from reality, may offer a path for estimating the causal effects of (sets of) individual restrictions, where treatments and counterfactuals can be properly specified and powered. Such experimental work may also address how effective the restrictions are in protecting the firm's competitive advantage, knowledge, and relational resources, whether certain sets of restrictions are more effective than others, and why. It could also offer a more complete theory of when and why certain restrictions may be adopted either in isolation or in combination. For instance, while NDAs are broadly applicable and courts are more willing to enforce them, observing a violation may be difficult. In contrast, it is easier to observe a violation of an NCA, but courts are less willing to enforce them. Hence, analogous to how patents are costlier to file and enforce compared with trade secrets, which in turns affects firm decisions to use patents, a broader theory of employment restriction use could delve deeper into how such costs of enforcing the various restrictions, individually or in combination, and the corresponding benefits drive firm decisions.

In conclusion, using a descriptive, question-driven approach, we have documented several novel and important facts regarding the use of four employment restrictions and their association with value appropriation from workers. These facts answer some questions but raise many others. We have outlined some of those questions here, but many others certainly remain. Finding and answering them, we believe, will be a productive line of inquiry.

DATA AVAILABILITY STATEMENT

The data used in this manuscript are covered by a nondisclosure agreement. To those able to access the data, we can provide replication code.

ORCID

Natarajan Balasubramanian <https://orcid.org/0000-0002-3513-5090>

Evan Starr <https://orcid.org/0000-0002-4368-1710>

Shotaro Yamaguchi <https://orcid.org/0009-0005-6660-4354>

REFERENCES

- Arnow-Richman, R. (2006). Cubewrap contracts and worker mobility: The dilution of employee bargaining power via standard form noncompetes. *Michigan State Law Review*, 963–992.
- Arnow-Richman, R. S., Carlson, G., Lobel, O., Roginsky, J., Short, J. L., & Starr, E. (2022). Supporting market accountability, workplace equity, and fair competition by reining in non-disclosure agreements. *Day One Project*. <https://fas.org/publication/supporting-market-accountability-workplace-equity-and-fair-competition-by-reining-in-non-disclosure-agreements/>
- Balan, D. J. (2021). Labor noncompete agreements: Tool for economic efficiency or means to extract value from workers? *The Antitrust Bulletin*, 66(4), 593–608.
- Balasubramanian, N., Chang, J. W., Sakakibara, M., Sivadasan, J., & Starr, E. (2020). Locked in? The enforceability of covenants not to compete and the careers of high-tech employees. *Journal of Human Resources*, 57, 1218–9931R1.
- Barney, J. (1991). Firm resources and sustained competitive advantage. *Journal of Management*, 17(1), 99–120.
- Beck, R. (2022). A noncompete ban by any other name—Time to take action.
- Bishara, N. (2011). Fifty ways to leave your employer: Relative enforcement of noncompete agreements, trends, and implications for employee mobility policy. *University of Pennsylvania Journal of Business Law*, 13, 751–795.
- Bishara, N. D., Martin, K. J., & Thomas, R. S. (2015). An empirical analysis of noncompetition clauses and other restrictive postemployment covenants. *Vanderbilt Law Review*, 68, 1.
- Campbell, B. A., Coff, R., & Kryscynski, D. (2012). Rethinking sustained competitive advantage from human capital. *Academy of Management Review*, 37(3), 376–395.

- Castellaneta, F., Conti, R., Veloso, F. M., & Kemeny, C. A. (2016). The effect of trade secret legal protection on venture capital investments: Evidence from the inevitable disclosure doctrine. *Journal of Business Venturing*, 31(5), 524–541.
- Choi, S. J., Gulati, M., & Scott, R. E. (2017). The black hole problem in commercial boilerplate. *Duke LJ*, 67, 1.
- Coff, R. W. (1997). Human assets and management dilemmas: Coping with hazards on the road to resource-based theory. *Academy of Management Review*, 22(2), 374–402.
- Coff, R. W. (1999). When competitive advantage doesn't lead to performance: The resource-based view and stakeholder bargaining power. *Organization Science*, 10(2), 119–133.
- Cohen, W. M., Nelson, R., & Walsh, J. P. (2000). Protecting their intellectual assets: Appropriability conditions and why US manufacturing firms patent (or not).
- Colvin, A., & Shierholz, H. (2019). Noncompete agreements: Ubiquitous, harmful to wages and to competition, and part of a growing trend of employers requiring employees to sign away their rights. *Economic Policy Institute*. 57. <https://www.epi.org/publication/noncompete-agreements/>
- Conti, R. (2014). Do non-competition agreements lead firms to pursue risky R&D projects? *Strategic Management Journal*, 35(8), 1230–1248.
- Contigiani, A., Hsu, D. H., & Barankay, I. (2018). Trade secrets and innovation: Evidence from the “inevitable disclosure” doctrine. *Strategic Management Journal*, 39(11), 2921–2942.
- Cowgill, B., Freiberg, B., & Starr, E. (2024). “Clause and effect: A field experiment on noncompetes, knowledge flows, mobility, and wages.” Working Paper.
- Cuypers, I. R., Hennart, J. F., Silverman, B. S., & Ertug, G. (2021). Transaction cost theory: Past progress, current challenges, and suggestions for the future. *Academy of Management Annals*, 15(1), 111–150.
- Fallick, B., Fleischman, C. A., & Rebitzer, J. B. (2006). Job-hopping in Silicon Valley: Some evidence concerning the microfoundations of a high-technology cluster. *The Review of Economics and Statistics*, 88(3), 472–481.
- Federal Trade Commission. (2024). Non-compete clause rule. Retrieved from <https://www.federalregister.gov/documents/2024/05/07/2024-09171/non-compete-clause-rule>
- Friedman, D. (1991). *Non-competition agreements: Some alternative explanations*. Mimeo.
- Ganco, M., Liu, J., Wang, H., & Yamaguchi, S. (2023). “Strategic restraint: When do human-capital-intensive companies choose (not) to use noncompete agreements?” Working Paper.
- Ganco, M., Ziedonis, R. H., & Agarwal, R. (2015). More stars stay, but the brightest ones still leave: Job hopping in the shadow of patent enforcement. *Strategic Management Journal*, 36(5), 659–685.
- Garmaise, M. J. (2011). Ties that truly bind: Noncompetition agreements, executive compensation, and firm investment. *The Journal of Law, Economics, and Organization*, 27(2), 376–425.
- Graves, C. T. (2022). Questioning the employee non-solicitation covenant. *Loyola of Los Angeles Law Review*, 55 (4), 959.
- Grossman, S. J., & Hart, O. D. (1986). The costs and benefits of ownership: A theory of vertical and lateral integration. *Journal of Political Economy*, 94(4), 691–719.
- Hardaway, A. B. (2015). The paradox of the right to contract: Noncompete agreements as thirteenth amendment violations. *Seattle University Law Review*, 39, 957.
- Hiraiwa, T., Lipsitz, M., & Starr, E. (2022). “Do firms value court enforceability of non-compete agreements? A revealed preference approach.” Working Paper.
- Jeffers, J. (2022). The impact of restricting labor mobility on corporate investment and entrepreneurship. Retrieved from SSRN 3040393.
- Johnson, M. S., Lavetti, K., & Lipsitz, M. (2021). The labor market effects of legal restrictions on employee mobility. Retrieved from SSRN 3455381.
- Johnson, M. S., & Lipsitz, M. (2020). Why are low-wage employees signing noncompete agreements? *Journal of Human Resources*, 57(3), 689–724.
- Kang, H., & Fleming, L. (2020). Non-competes, business dynamism, and concentration: Evidence from a Florida case study. *Journal of Economics & Management Strategy*, 29(3), 663–685.
- Kang, H., & Lee, W. (2022). How innovating firms manage knowledge leakage: A natural experiment on the threat of worker departure. *Strategic Management Journal*, 43(10), 1961–1982.
- Kim, P. T. (1997). Bargaining with imperfect information: A study of worker perceptions of legal protection in an at-will world. *Cornell Law Review*, 83, 105.



- Kini, O., Williams, R., & Yin, D. (2020). CEO noncompete agreements, job risk, and compensation. *Review of Financial Studies*, 34, hhaa103.
- Krueger, A. B., & Posner, E. A. (2018). A proposal for protecting low-income workers from monopsony and collusion. *Hamilton Project Policy Proposal*, 5, 1–18.
- Lavetti, K., Simon, C., & White, W. D. (2020). The impacts of restricting mobility of skilled service employees evidence from physicians. *Journal of Human Resources*, 55(3), 1025–1067.
- Levin, R. C., Klevorick, A. K., Nelson, R. R., Winter, S. G., Gilbert, R., & Griliches, Z. (1987). Appropriating the returns from industrial research and development. *Brookings Papers on Economic Activity*, 1987(3), 783–831.
- Lipsitz, M., & Starr, E. (2022). Low-wage workers and the enforceability of noncompete agreements. *Management Science*, 68(1), 143–170.
- Lobel, O. (2021). Boilerplate collusion: Clause aggregation, antitrust law & contract governance. *Minnesota Law Review*, 106, 877–949.
- Marx, M. (2011). The firm strikes back: Non-compete agreements and the mobility of technical professionals. *American Sociological Review*, 76(5), 695–712.
- Marx, M. (2022). Employee non-compete agreements, gender, and entrepreneurship. *Organization Science*, 33(5), 1756–1772.
- Marx, M., Strumsky, D., & Fleming, L. (2009). Mobility, skills, and the Michigan non-compete experiment. *Management Science*, 55(6), 875–889.
- Mawdsley, J. K., & Somaya, D. (2016). Employee mobility and organizational outcomes: An integrative conceptual framework and research agenda. *Journal of Management*, 42, 85–113.
- Melero, E., Palomeras, N., & Wehrheim, D. (2020). The effect of patent protection on inventor mobility. *Management Science*, 66(12), 5485–5504.
- Molloy, J. C., & Barney, J. B. (2015). Who captures the value created with human capital? A market-based view. *Academy of Management Perspectives*, 29(3), 309–325.
- Mukherjee, A., & Vasconcelos, L. (2012). Star wars: Exclusive talent and collusive outcomes in labor markets. *The Journal of Law, Economics, & Organization*, 28(4), 754–782.
- Oster, E. (2019). Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business and Economic Statistics*, 37(2), 187–204.
- Png, I. P. (2017). Secrecy and patents: Theory and evidence from the uniform trade secrets act. *Strategy Science*, 2 (3), 176–193.
- Prescott, J. J., Bishara, N. D., & Starr, E. (2016). Understanding noncompetition agreements: The 2014 non-compete survey project. *Michigan State Law Review*, 2016(2), 369–464.
- Prescott, J. J., & Starr, E. (2022). Subjective beliefs about contract enforceability. *The Journal of Legal Studies*.
- Rajgopal, S., Taylor, D., & Venkatachalam, M. (2012). Frictions in the CEO labor market: The role of talent agents in CEO compensation. *Contemporary Accounting Research*, 29(1), 119–151.
- Rothstein, D., & Starr, E. (2021). “Mobility restrictions, bargaining, and wages: Evidence from the National Longitudinal Survey of youth 1997.” Working Paper.
- Rubin, P. H., & Shedd, P. (1981). Human capital and covenants not to compete. *The Journal of Legal Studies*, 10 (1), 93–110.
- Ruggles, S., Flood, S., Goeken, R., Grover, J., Meyer, E., Pacas, J., & Sobek, M. (2020). IPUMS USA: Version 10.0 [dataset]. Minneapolis, MN: IPUMS.
- Samila, S., & Sorenson, O. (2011). Noncompete covenants: Incentives to innovate or impediments to growth. *Management Science*, 57(3), 425–438.
- Sanga, S. (2018). Incomplete contracts: An empirical approach. *The Journal of Law, Economics, and Organization*, 34(4), 650–679.
- Sevcenko, V., Lynn, W., Kacperczyk, A'O., & Ethiraj, S. (2021). Surplus division between labor and capital: A review and research agenda. *Academy of Management Annals*, 16, 334–390.
- Shi, L. (2023). Optimal regulation of noncompete contracts. *Econometrica*, 91(2), 425–463.
- Silverman, B. S. (1999). Technological resources and the direction of corporate diversification: Toward an integration of the resource-based view and transaction cost economics. *Management Science*, 45(8), 1109–1124.
- Silverman, B. S. (2002). *Technological resources and the logic of corporate diversification*. Routledge.
- Silverman, E. (2020). “Report on B23-0494, the ban on non-compete agreements amendment act of 2020.” Council of DC Committee on Labor and Workforce Development Committee Report.

- Sockin, J., Sojourner, A. J., & Starr, E. (2022). Non-disclosure agreements and externalities from silence (December 19, 2022). Upjohn Institute Working Paper 22-360.
- Starr, E. (2019a). *The use, abuse, and enforceability of non-compete and no-poach agreements: A brief review of the theory, evidence, and recent reform efforts*. Economic Innovation Group.
- Starr, E. (2019b). Consider this: Training, wages, and the enforceability of covenants not to compete. *ILR Review*, 72(4), 783–817.
- Starr, E. (2021). Are noncompetes holding down wages? In B. Harris & S. Block (Eds.), *Inequality and the labor market: The case for greater competition*. Brookings Institution Press.
- Starr, E., Ganco, M., & Campbell, B. A. (2018). Strategic human capital management in the context of cross-industry and within-industry mobility frictions. *Strategic Management Journal*, 39(8), 2226–2254.
- Starr, E., Prescott, J. J., & Bishara, N. (2020). The behavioral effects of (unenforceable) contracts. *The Journal of Law, Economics, and Organization*, 36(3), 633–687.
- Starr, E., Prescott, J. J., & Bishara, N. (2021). Noncompetes in the US labor force. *Journal of Law and Economics*, 64, 53–84.
- Stuart, T. E., & Sorenson, O. (2003). Liquidity events and the geographic distribution of entrepreneurial activity. *Administrative Science Quarterly*, 48(2), 175–201.
- Wickelgren, A. L. (2018). A novel justification for legal restrictions on non-compete clauses. *International Review of Law and Economics*, 54, 49–57.
- Williamson, O. E. (1975). *Markets and hierarchies: Analysis and antitrust implications*. Free Press.
- Young, S. G. (2021). “Noncompete clauses, job mobility, and job quality: Evidence from a low-earning non-compete ban in Austria.” Working Paper.
- Younge, K. A., & Marx, M. (2016). The value of employee retention: Evidence from a natural experiment. *Journal of Economics & Management Strategy*, 25(3), 652–677.
- Younge, K. A., Tong, T. W., & Fleming, L. (2015). How anticipated employee mobility affects acquisition likelihood: Evidence from a natural experiment. *Strategic Management Journal*, 36(5), 686–708.

SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Balasubramanian, N., Starr, E., & Yamaguchi, S. (2024). Employment restrictions on resource transferability and value appropriation from employees. *Strategic Management Journal*, 45(12), 2519–2547. <https://doi.org/10.1002/smj.3634>