EFiled: May 10 2024 01:28PM EDT
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Case Number 110,2024

IN THE SUPREME COURT OF THE STATE OF DELAWARE

LKQ CORPORATION, Plaintiff/Counter-Defendant, Appellant, v. ROBERT RUTLEDGE, Defendant/Counter-Claimant, Appellee.	No. 110, 2024 Certification of Question of Law from the United States Court of Appeals for the Seventh Circuit. No. 23-2330 D.C. No. 1:21-cv-03022

MOTION FOR LEAVE TO FILE BRIEF OF THE CHAMBER OF COMMERCE OF THE UNITED STATES OF AMERICA AS *AMICUS CURIAE* SUPPORTING APPELLANT AND REVERSAL

Pursuant to Delaware Supreme Court Rule 28, the Chamber of Commerce of the United States of America (the "Chamber") requests leave to file a brief as *amicus curiae* in support of the Appellant and in opposition to the Chancery Court's decision below, and state as follows:

1. The Chamber is the world's largest business federation. It represents approximately 300,000 direct members and indirectly represents the interests of more than three million companies and professional organizations.

2. The Chamber represents a wide range of businesses that share a common interest in the stability, regularity, fairness, and predictability of business practices.

3. Movants seeks leave to file a brief highlighting the significance of the

business interests that forfeiture-for-competition provisions protect in any employer-employee context.

4. The Court should recognize the unique benefits that forfeiture-for-competition agreements provide to employers *and* employees. Forfeiture-for-competition agreements give employees the freedom to choose to join a competitor in exchange for forgoing additional compensation. They also provide a clear understanding of the consequences of that choice, which results in more efficient enforcement and allows employees to negotiate with new employers to backfill the forfeited compensation. Finally, forfeiture-for-competition agreements protect the right of businesses not to pay profits that a former employee is seeking to reduce.

5. Forfeiture-for-competition agreements promote productive innovation by protecting proprietary information, trade secrets, business plans, pricing or bidding strategies, and other confidential and valuable business information. They also encourage employers make investments in employee training and development that they otherwise would not make, while also allowing businesses to grow and preserve their goodwill.

6. The Court should take these factors into account in deciding this case, and the proposed *Amicus Curiae* Brief provides important argument on these issues.

7. Pursuant to Supreme Court Rule 28(b)(3), the parties consent to this Motion.

8. For the foregoing reasons, Movants request leave to file the *Amicus Curiae* Brief attached as **Exhibit A**. For the Court's convenience, a compendium of all secondary sources cited in the proposed brief is attached as **Exhibit B**.

[SIGNATURE PAGE FOLLOWS.]

Dated: May 10, 2024

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EXHIBIT A

IN THE SUPREME COURT OF THE STATE OF DELAWARE

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BRIEF OF THE CHAMBER OF COMMERCE OF THE UNITED STATES OF AMERICA AS *AMICUS CURIAE* SUPPORTING APPELLANT

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IDENTITY AND INTEREST OF AMICUS CURIAE

The Chamber of Commerce of the United States of America (the "Chamber") is the world's largest business federation. It represents approximately 300,000 direct members and indirectly represents the interests of more than three million companies and professional organizations of every size, in every industry sector, and from every region of the country. An important function of the Chamber is to represent the interests of its members before Congress, the Executive Branch, and the courts. To that end, the Chamber regularly files amicus curiae briefs in cases, like this one, that raise issues of concern to the nation's business community.

On behalf of the businesses it represents, the Chamber has an interest in ensuring that Delaware remains a leader of sensible business practices and policies that are predictably upheld by its courts. Businesses regularly rely upon forfeiture-for-competition agreements because of their many pro-competitive benefits. Given that Delaware is home to two-thirds of all Fortune 500 companies,[1] Amicus has a strong interest in ensuring that Delaware courts properly recognize those benefits and consistently enforce forfeiture-for-competition agreements.

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[1] *See* Delaware Division of Corporations, *2021 Annual Report*, *available at* https://corpfiles.delaware.gov/Annual-Reports/Division-of-Corporations-2021-Annual-Report.pdf.

SUMMARY OF ARGUMENT

1. Forfeiture-for-competition agreements are a sensible arrangement between an employer and employee to incentivize an employee to stay with a company or not compete against it for a set period in exchange for valuable consideration. In this case, Robert Rutledge received equity in LKQ Corporation in exchange for a commitment not to compete against his employer for a nine-month period after his resignation.

2. The Chamber submits this brief to highlight the significance of the business interests that forfeiture-for-competition agreements protect. Forfeiture-for-competition agreements promote productive innovation by protecting proprietary information, trade secrets, business plans, pricing or bidding strategies, and other confidential and valuable business information. They also encourage employers to make investments in employee training and development that they otherwise would not make, while allowing businesses to grow and preserve their goodwill.

3. Forfeiture-for-competition agreements provide unique benefits to employers *and* employees. Instead of preventing workers from accepting employment with a competitor as with a traditional noncompete agreement, forfeiture-for-competition agreements give employees an economic incentive that aligns their interests to those of their former employers. Forfeiture-for-competition agreements also provide a clear understanding of the consequences of competition,

which results in more efficient enforcement and allows employees to negotiate with new employers to backfill the forfeited compensation. Finally, forfeiture-for-competition agreements protect the right of businesses not to pay a former employee out of profits that the former employee is actively seeking to reduce.

4. This Court had the occasion to address forfeiture-for-competition agreements in the context of a limited partnership agreement in *Cantor Fitzgerald, LP v. Ainslie*, __ A.3d __, 2024 WL 315193 (Del. Jan. 29, 2024). In *Cantor Fitzgerald*, the Court adopted the view that forfeiture-for-competition agreements are *not* restraints of trade and should not be subject to a reasonableness analysis. That holding should not be cabined to the limited partnership context. The unique features of forfeiture-for-competition agreements highlighted in this brief underscore why they are not restraints of trade in any employer-employee arrangement. And even if the Court were to hold that some forfeiture-for-competition agreements should be reviewed for reasonableness, it should make clear that the unique features of such agreements weigh in favor of finding them enforceable.

ARGUMENT

I. In Deciding This Case, the Court Should Recognize and Give Weight to the Numerous Benefits of Forfeiture-for-Competition Agreements.

Robert Rutledge was afforded the opportunity to participate in a restricted stock program—an opportunity reserved for "key persons," who represent less than 2% of LKQ's workforce. (7th Cir. Op. at 2.) There was no requirement to accept such an opportunity, but when he did, he was required to execute and abide by the terms of the Restricted Stock Unit Agreement (the "RSU Agreement"). (*Id.*) Under the terms of the RSU Agreement, Rutledge received stock distributed to him pursuant to a vesting schedule. (*Id.*) Rutledge sold the vested stock on the open market before he left to work for a competitor. (*Id.*) Under the terms of the RSU Agreement, Rutledge would have been entitled to retain the proceeds from the sale of the vested stock if he waited nine months from the date of his resignation to begin work with a competitor. Instead, Rutledge waited five days. (*Id.* at 3.)

These facts give the Court the opportunity to consider the significant and legitimate business interests that forfeiture-for-competition agreements protect in any employer-employee arrangement. Forfeiture-for-competition agreements' distinct features provide numerous benefits to both businesses and employees. The Court should weigh these factors in deciding this case.

A. Forfeiture-for-Competition Agreements Are Uniquely Beneficial for Both Employers and Employees.

Businesses across the economy—including the Chamber's members—rely on forfeiture-for-competition agreements to protect critical business interests. Those agreements have distinct features that benefit both employers and employees and eliminate concerns that courts have often expressed in assessing noncompete agreements. In deciding this case, the Court should recognize the importance of these interests while valuing the features that distinguish forfeiture-for-competition agreements from noncompete agreements and ensure that businesses can continue to rely on predictable and consistent enforcement of forfeiture-for-competition agreements in Delaware.

Forfeiture-for-competition agreements do not prevent workers from accepting employment elsewhere, even with a competitor. *See Hough Assocs., Inc. v. Hill*, 2007 WL 148751, at *17 (Del. Ch. Jan. 17, 2007) (noting that "[e]mployees, for many legitimate reasons, often desire to move elsewhere" and that traditional noncompete agreements may restrict such movement). Rather, forfeiture-for-competition agreements align the interests of employees with those of their former employers by giving employees an economic incentive to refrain from joining a competitor.

Employees who agree to forfeiture-for-competition agreements also have a clear understanding ahead of time of the additional compensation they will forgo in

the event they elect to join a competitor. By contrast, noncompete agreements typically require employers to seek injunctive relief, which results in court intervention, costly litigation, and the uncertainty associated with a possible injunction that will prevent new employment for an unpredictable period. Forfeiture-for-competition agreements eliminate this costly cloud of uncertainty by setting clear terms relating to an employee's decision to join a competitor. Such clarity works to the benefit of employees, who can often negotiate with their new employers for higher compensation to mitigate the loss of compensation under their forfeiture-for-competition agreements. New employers often agree to backfill the forfeited compensation, thus fostering employee mobility while respecting the terms of the forfeiture-for-competition agreement. In other words, the marketplace handles the issue without the need for judicial intervention.

Forfeiture-for-competition agreements also protect the right of employers not to provide benefits to those actively competing against them. That business interest is especially strong when the deferred competition tied to a forfeiture-for-competition agreement is in the form of equity or stock grants. Businesses have a legitimate interest in not sharing their profits with former employees who are actively competing with them and attempting to *reduce* those profits. Forfeiture-for-competition agreements protect this interest by allowing an employer and employee to sever ties if the employee elects to compete against the employer.

Failure to enforce forfeiture-for-competition agreements will deny employers the benefits of these agreements, including stability, investment in employees, and structures that do not deter proper compliance and disciplinary measures. It will also deny employees significant and entirely voluntary forms of compensation, which, if these contracts are not enforced, would not be offered in the first place. And enforcement of forfeiture-for-competition agreements as written is consistent with Delaware's strong principle of freedom of contract. *See Holifield v. XRI Investment Holdings LLC*, 304 A.3d 896, 931 (Del. 2023) (concluding in context of limited liability company statute that "allow[ing] courts to simply rewrite the contract . . . would negatively impinge on the goal of achieving predictability in contracts and undermine the important principle of freedom of contract legislatively embodied in the alternative entity statutes"); *Abry Partners V, LP v. F&W Acquisition LLC*, 891 A.2d 1032, 1061-62 (Del. Ch. 2006) ("[T]here is also a strong American tradition of freedom of contract, and that tradition is especially strong in our State, which prides itself on having commercial laws that are efficient."); *Libeau v. Fox*, 880 A.2d 1049, 1056-57 (Del. Ch. 2005) ("When parties have ordered their affairs voluntarily through a binding contract, Delaware law is strongly inclined to respect their agreement, and will only interfere upon a strong showing that dishonoring the contract is required to vindicate a public policy interest even stronger than freedom of contract."); *see also Fleming v. U.S. Postal Service AMF O'Hare*, 27 F.3d 259,

261 (7th Cir. 1994) (Posner, J.) ("[A] premise of a free-market system is that both sides of the market, buyers as well as sellers, tend to gain from freedom of contract.").

Given that forfeiture-for-competition agreements preserve the freedom of employer and employee to contract, the Court properly recognized in *Cantor Fitzgerald* that many jurisdictions do not view them as restraints of trade or scrutinize them for reasonableness. *See Cantor Fitzgerald*, 2024 WL 315193, at *12 n.104; *see also Morris v. Schroder Cap. Mgmt. Int'l*, 859 N.E.2d 503 (N.Y. 2006); *Fraser v. Nationwide Mut. Ins. Co.*, 334 F. Supp. 2d 755, 760 (E.D. Pa. 2004); *Courington v. Birmingham Tr. Nat. Bank*, 347 So. 2d 377, 383 (Ala. 1977); *Alco-Columbia Paper Serv., Inc. v. Nash*, 273 So. 2d 630, 634 (La. Ct. App. 1973); *Swift v. Shop Rite Food Stores, Inc*., 489 P.2d 881, 882 (N.M. 1971). "The strong weight of authority holds that forfeitures for engaging in subsequent competitive employment … are valid, even though unrestricted in time or geography." *Rochester Corp. v. Rochester*, 450 F.2d 118, 122-23 (4th Cir. 1971). This Court came to the same conclusion in *Cantor Fitzgerald*, and its rationale is not limited to partnerships; rather it applies with even more force with application to employees.

In light of these unique characteristics, forfeiture-for-competition agreements are not restraints of trade, and should not be subject to a reasonableness analysis. But even if viewed through a reasonableness standard, the fact that a forfeiture-for-

competition agreement does not deprive an employee of the choice to go to a competitor strongly weighs in favor of its enforceability.

B. Forfeiture-for-Competition Agreements Protect Significant Business Interests.

Forfeiture-for-competition agreements not only are uniquely beneficial to employers and employees in aligning their incentives without the prospect of legal compulsion, they also achieve many of the same benefits as reasonable noncompete agreements, including protecting proprietary information, trade secrets, special business relationships (customer, vendors, etc.), business plans, pricing or bidding strategies, and other confidential and valuable business information. *See Tristate Courier and Carriage, Inc. v. Berryman*, 2004 WL 835886, at *10 (Del. Ch. Apr. 15, 2004) (enforcing noncompete agreement when employee "has complete knowledge of . . . proprietary information, including its business strategies, logistics, and costs"); Evan P. Starr et al., *Noncompete Agreements in the U.S. Labor Force*, 64 J.L. & ECON. 53, 64 (2021) (noting that "the incidence of noncompete[] [agreements] is much higher among those who report possessing some type of trade secret or valuable information.").

The protection of confidential business information promotes innovation by "increas[ing] the returns to research and development." John McAdams, *Non-Compete Agreements: A Review of the Literature* at 6 (Fed. Trade Comm., Working Paper, 2019). "[I]nnovation and business developments take large amounts of time,

money and trial and error." *Id.* If the result of that investment is to have an employee with confidential business information poached by a competitor (who was unwilling to invest its own resources), it would reduce the incentive for businesses to make similar investments in the future.

Moreover, absent the ability to rely on forfeiture-for-competition agreements and other contractual commitments, businesses would be forced to keep confidential business information limited to a select group of employees, stifling the flow of valuable information and ideas that support innovation and bring value to customers. When consistently enforced, forfeiture-for-competition agreements, like reasonable noncompete agreements, reduce the incentive of competitors to engage in free-riding behavior and lead "to increases in firm-sponsored training, riskier [research and development] investments, and increases in firm value and the likelihood of acquisition." Norman D. Bishara & Evan Starr, *The Incomplete Noncompete Picture*, 20 Lewis & Clark L. Rev. 497, 535 (2016); *see also Hough Assocs.*, 2007 WL 148751, at *14 (enforcing a noncompete agreement when the agreement "safeguarded" the employer by "prevent[ing] a rival . . . from enlisting" employees.).

Employers are also more likely to spend resources on employee training and development when they do not fear that the employees may immediately take those skills to a competitor. Forfeiture-for-competition agreements, like reasonable noncompete agreements, can solve this "'holdup' problem," which emerges when

employers "forgo making certain investments in their workforce knowing that employees would be able to subsequently quit and appropriate the value of the investment." Camila Ringeling et al., *Noncompete Clauses Used in Employment Contracts, Comment of the Global Antitrust Institute* at 4-5, & n.7, n.9 (George Mason Law & Economics, Research Paper No. 20-04, Feb. 7, 2020). "[B]y discouraging worker attrition before the firm has had the time to recoup the cost of its upfront investment," such agreements encourage "mutually beneficial" investments. McAdams, *Non-Compete Agreements* at 6; *see also Computer Aid, Inc. v. MacDowell*, 2001 WL 877553, at *2 (Del. Ch. July 26, 2001) (enforcing a noncompete agreement to protect an employer's legitimate business interests in the "specialized training" provided to an employee).

While forfeiture-for-competition agreements foster training and development of employees, they also allow businesses to grow and preserve their goodwill, much as noncompete agreements aim to do. *See Sensus USA, Inc. v. Franklin*, 2016 WL 1466488, at *7 (D. Del. Apr. 14, 2016) (enforcing a noncompete agreement when employees' duties involved "cultivating client relationships" including "work[ing] on some of [the employer's] largest accounts"). A business that relies on its employees to obtain customers is at risk of its employees leaving to form their own firm or to join a competitor and taking those customers. Forfeiture-for-competition agreements help "preserv[e] employer goodwill," *id.*, by incentivizing employees

not to compete with their employers by using the same benefits that their employers have bestowed upon them—including training, development, and the use of their employers' brands to develop a customer base.

Any standard applied to forfeiture-for-competition agreements in an employer-employee arrangement must recognize the significance of the business interests that such agreements protect. Just like reasonably crafted noncompete agreements, forfeiture-for-competition agreements are an essential component of how businesses protect their confidential and proprietary information and preserve their goodwill, while also promoting employee development. It is essential that the business community can rely on Delaware's predictable and consistent enforcement of such agreements.

II. Because the Benefits of Forfeiture-for-Competition Agreements Are the Same in the Employment and Limited Partnership Contexts, the Court Should Apply the Reasoning of *Cantor Fitzgerald* to this Case.

In *Cantor Fitzgerald, LP v. Ainslie*, __ A.3d __, 2024 WL 315193 (Del. Jan. 29, 2024), the Court recognized the unique benefits of forfeiture-for-competition agreements that differentiate it from other noncompete agreements. The Court observed, "[t]he distinction between a restrictive non-competition covenant that precludes a former employee from earning a living in his chosen field and an agreement that allows a former partner to compete but at the cost of relinquishing a contingent benefit is, in our observation, significant." *Id.* at 13. It drew upon a forfeiture-for-competition agreement's unique features when it determined that "the strong policy interest that justifies the review of unambiguous contract provisions for reasonableness and a balancing of the equities . . . is diminished—if it does not vanish" when reviewing forfeiture-for-competition agreements. *Id.* "To put it another way, the interest to be vindicated when evaluating a covenant that prohibits competition and that might even preclude gainful employment is significantly weakened when competition—often (as in this case) highly remunerative—is permitted." *Id.*

In this case, LKQ afforded Rutledge an opportunity reserved for "key persons" in the company to participate in a restricted stock program that entitled him to stock paid out on a vested schedule, so long as he abided by the clear terms of a

forfeiture-for-competition provision. (Op. at 2-3.) Rutledge did not comply with those contractual terms and instead elected to work for a competitor. (Op. at 3.) *Cantor Fitzgerald* recognized the importance of freedom of contract in holding that agreement enforceable without a reasonableness review, and although the facts of that case involved a limited partnership agreement, the same freedom-of-contract principles apply to Rutledge, who was *not prohibited* from seeking employment elsewhere and freely entered into the RSU Agreement. *See, e.g.*, *Libeau*, 880 A.2d at 1057 (recognizing outside of limited partnership context that "[w]hen parties have ordered their affairs voluntarily through a binding contract, Delaware law is strongly inclined to respect their agreement, and will only interfere upon a strong showing that dishonoring the contract is required to vindicate a public policy interest even stronger than freedom of contract").

This Court recognized in *Cantor Fitzgerald* that forfeiture-for-competition agreements broadly serve these benefits and interests, and the Court did not suggest that a different conclusion would arise based on the type of agreement involved. To the contrary, in *Cantor Fitzgerald*, the Court expressly rejected the Third Circuit's prediction in *Pollard v. Autotote, Ltd.*, 852 F.2d 67 (3d Cir. 1988), that this Court would apply a reasonableness analysis to forfeiture provisions. 2024 WL 315193 at *11 n.102. Given that *Pollard*, like this case, involved a forfeiture agreement in the employment context, the Court should not newly cabin its analysis in *Cantor*

Fitzgerald to forfeiture-for-competition provisions in limited partnership agreements. It should, instead, answer the certified questions by holding that *Cantor Fitzgerald* applies to forfeiture-for-competition agreements in the employment context, including the RSU Agreement at issue in this case.

CONCLUSION

This case presents an opportunity for Delaware to reaffirm its role as a leader in sensible, business-first policies and practices that are predictably upheld by its courts. Forfeiture-for-competition agreements protect critical business interests, give employees an incentive to refrain from competition, and provide advance clarity that is both beneficial in its own right and because it allows employees to negotiate with new employers to mitigate their lost compensation. The business community has a significant interest in the predictable and consistent enforcement of forfeiture-for-competition agreements in Delaware.

The Chamber respectfully ask the Court to consider these significant business interests and to conclude that forfeiture-for-competition agreements are not restraints of trade and should be enforceable.

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EXHIBIT B

THE INCOMPLETE NONCOMPETE PICTURE

by
Norman D. Bishara[*] *and Evan Starr*[**]

Covenants not to compete ("noncompetes" or "CNCs") are an increasingly controversial element of the U.S. employer–employee relationship. Numerous state legislatures are reconsidering their noncompete policies, however the empirical research remains fractured and ambiguous on several key issues. We begin by discussing the various theoretical perspectives in the relevant legal literature. We then carefully evaluate 24 empirical studies focusing on noncompetes (6 utilizing evidence of workers who signed a noncompete, 3 with data on the intra-firm use of noncompetes, 2 experimentally allocating noncompetes, and 14 focused on ad hoc measures of noncompete enforceability, which examine how policy differences affect workers, firms, and regions). Despite the rapidly expanding empirical literature, we argue that many of the most basic questions regarding the use and consequences of noncompetes remain either entirely unanswered or at least unsettled. We conclude that major gaps remain in the research and then provide recommendations for future research efforts to provide a solid foundation for evaluating the recent calls for banning or reforming longstanding noncompete policies at the state and now the federal level.

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[**] Assistant Professor, University of Maryland, Robert H. Smith School of Business.

INTRODUCTION

At this moment in U.S. legal and business history there is a pronounced increase in the level of discussion surrounding the role of restrictive covenants in employment relationships. The restrictions on employee mobility known as covenants not to compete ("noncompetes" or "CNCs") are the subject of the majority of the discussion and the target of sometimes harsh criticism along with calls to ban or modify their enforcement. For better or worse, policymakers—oftentimes state legislators—are increasingly re-examining the social and business implications of noncompetes and initiating changes to the underlying legal evaluation of these agreements.

The potential problem is that these major policy changes are being made without the adequate portfolio of reliable research needed to ensure that these reforms are both wise and well-tailored to accomplish the stated policy objectives. In many instances legislators' take an uninformed, scattershot approach to noncompete reform. The risk of relying on a partial or unreliable body of research is clear: unsupported and poorly reasoned reforms to a state's current policy on restrictive covenants can have negative as well as the hoped-for positive consequences.

Evidence of the increased interest in noncompetes comes from a variety of quarters of academia[1] and public policy,[2] many of which this Arti-

[1] A prime example is the September 2015 Lewis & Clark Business Law Fall Forum on "Workplace Secrets, Loyalty and Poaching: Protecting Employer Interests

cle will discuss. The policymakers involved in the current CNC debate include the judiciary and, perhaps more crucially when it comes to potential sea changes in policy, reform-minded legislators in various states. Amid this greater attention focused on noncompetes there are numerous interesting questions, many of which are beyond the scope of this Article, about why noncompetes are receiving so much attention from academic, business, and public-policy circles at this point in time. The long-term, macro influences of the changing nature of work and technology, globalization, and the increased pace of innovation are most likely crucial factors driving a greater interest in the law's role in employee mobility and knowledge diffusion.

More immediately there have been, perhaps as a result of these other trends, media reports and lawsuits exposing high-profile instances of certain employers' seemingly abusive use of noncompete agreements for their workforce.[3] Some reports even claim an increase in noncompete use based on reported litigation.[4] For instance, revelations that sandwich fast-food chain Jimmy John's[5] and the preeminent online retailer Amazon, Inc.[6] have widely used noncompetes with low-wage workers have further put the noncompete issue squarely on the agenda of policymakers

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and Employee Liberty," which was the impetus for this Article. We are grateful to the Forum's organizers, especially Professor Henry Drummonds and the leadership and staff of the *Lewis & Clark Law Review*, for the generous invitation to present and discuss our work. The major employee survey described briefly in this Article is part of a long-term research project with our colleague J.J. Prescott of the University of Michigan Law School. We also thank Ki Hoon Kim for his able research assistance.

[2] *See, e.g.*, Press Release, Office of Sen. Chris Murphy, Murphy, Franken Introduce Bill to Ban Non-Compete Agreements for Low-Wage Workers (June 3, 2015), http://www.murphy.senate.gov/newsroom/press-releases/murphy-franken-introduce-bill-to-ban-non-compete-agreements-for-low-wage-workers.

[3] *See, e.g.*, Brunner v. Jimmy John's, LLC, Nos. 14 C 5509, 15 C 1681, 2015 WL 5086388, at *1 (N.D. Ill. Aug. 19, 2015). One current and one former employee of the fast food sandwich chain brought a lawsuit to declare their noncompete agreements void. *Id.*

[4] *See, e.g.*, Steven Greenhouse, *Noncompete Clauses Increasingly Pop Up in Array of Jobs*, N.Y. TIMES (June 8, 2014), http://nyti.ms/1qdOj4y (citing practitioner legal database searches and anecdotal reports as evidence of an increase in use and dispersion across industries).

[5] Dave Jamieson, *Jimmy John's Makes Low-Wage Workers Sign 'Oppressive' Noncompete Agreements*, HUFFINGTON POST (Oct. 15, 2014), http://www.huffingtonpost.com/2014/10/13/jimmy-johns-non-compete_n_5978180.html.

[6] *See* Spencer Woodman, *Exclusive: Amazon Makes Even Temporary Warehouse Workers Sign 18-Month Non-Competes*, VERGE (Mar. 26, 2015), http://www.theverge.com/2015/3/26/8280309/amazon-warehouse-jobs-exclusive-noncompete-contracts; *see also* Todd Bishop, *Former Amazon Employee: Disputed Non-Compete Deal 'Excessive' and 'Overbroad,'* GEEKWIRE (July 2, 2014), http://www.geekwire.com/2014/former-amazon-employee-disputed-non-compete-deal-excessive-overbroad/.

and businesspeople. Beyond the vigorous discussions in various statehouses underway for the last decade, even members of the U.S. Congress have begun to propose federal regulation of the use of noncompetes.[7] Recent U.S. Treasury Department and White House reports examining noncompetes also raise further issues about the impact on low-wage workers as well as the possible benefits to firms.[8]

However, the existing legal and empirical research on the prevalence and impacts of noncompetes in the U.S. labor market remains piecemeal and unsatisfactory. To date the empirical research is scattered across geographic boundaries, academic disciplines, and focused on various outcomes, including innovation,[9] employee mobility,[10] human capital investment and training,[11] new venture creation such as entrepreneurship[12] or spinoffs,[13] or CNC's role in fostering (or harming) agglomeration economies.[14] We discuss 24 major empirical studies involving noncompetes in the United States. Of those, only 6 studies utilize actual evidence

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[7] In mid-2015 several U.S. senators proposed legislation to limit the applicability of noncompete agreements to low-wage workers and to create other related protections. *See* Mobility and Opportunity for Vulnerable Employees Act, S. 1504, 114th Cong. (2015).

[8] OFFICE OF ECONOMIC POLICY, U.S. DEP'T OF THE TREASURY, NON-COMPETE CONTRACTS: ECONOMIC EFFECTS AND POLICY IMPLICATIONS 3 (March 2016) [hereinafter TREASURY REPORT], https://www.treasury.gov/resource-center/economic-policy/Documents/UST%20Non-competes%20Report.pdf ("Employers use these agreements for a variety of reasons: they can protect trade secrets, reduce labor turnover, impose costs on competing firms, and improve employer leverage in future negotiations with workers. However, many of these benefits come at the expense of workers and the broader economy."); THE WHITE HOUSE, NON-COMPETE AGREEMENTS: ANALYSIS OF THE USAGE, POTENTIAL ISSUES, AND STATE RESPONSES (May 2016), https://www.whitehouse.gov/sites/default/files/non-competes_report_final2.pdf.

[9] *See, e.g.*, Sampsa Samila & Olav Sorenson, *Noncompete Covenants: Incentives to Innovate or Impediments to Growth*, 57 MGMT. SCI. 425 (2011); Toby E. Stuart & Olav Sorenson, *Liquidity Events and the Geographic Distribution of Entrepreneurial Activity*, 48 ADMIN. SCI. Q. 175, 184 (2003) (describing methodology).

[10] *See, e.g.*, Matt Marx, Deborah Strumsky & Lee Fleming, *Mobility, Skills, and the Michigan Non-Compete Experiment*, 55 MGMT. SCI. 875 (2009).

[11] *See* Paul H. Rubin & Peter Shedd, *Human Capital and Covenants Not to Compete*, 10 J. LEGAL STUD. 93, 93–94 (1981) (applying economist Gary Becker's general and specific human-capital distinction to noncompete legal analysis).

[12] *See, e.g.*, Stuart & Sorenson, *supra* note 9.

[13] *See, e.g.*, Evan Starr, Natarajan Balasubramanian & Mariko Sakakibara, *Screening Spinouts? How Noncompete Enforceability Affects the Creation, Growth, and Survival of New Firms* (U.S. Census Bureau Cent. for Econ. Studies, Working Paper No. 14-27, 2015), http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2523418##.

[14] *See* Bruce Fallick, Charles A. Fleischman & James B. Rebitzer, *Job-Hopping in Silicon Valley: Some Evidence Concerning the Microfoundations of a High-Technology Cluster*, 88 REV. ECON. & STAT. 472 (2006).

of workers who have demonstrably signed a restrictive covenant, and these studies are limited to important but discrete professional occupations (executives, physicians, and engineers), which together comprise a mere 0.87% of the U.S. labor force.[15] Of the remaining studies, 3 have data on the use of noncompetes within firms, 2 experimentally allocate noncompetes, and 14 studies rely on ad hoc measures of noncompete enforceability to examine how policy differences (i.e., individual states' general legal approaches to enforcement) affect workers, firms, and regions.

We find the empirical work to be unsatisfactory on several dimensions. In particular, the existing research fails to answer even the most basic questions regarding the use and consequences of noncompetes for employees, firms, and regions. For example, despite this large and growing literature, we do not know the likelihood that a typical labor-force participant has a noncompete. Hence, we know very little about how noncompetes are related to employee level outcomes. We also find that the empirical literature has yet to address the use of noncompetes within firms and how the use of noncompetes is associated with firm investments in Research and Development (R&D) and employee human capital.

We then argue that the empirical studies of the impacts of noncompete enforceability, which make up the bulk of the literature, suffer from numerous shortcomings related to the lack of data on who signs noncompetes. The most prominent of these is that comparisons across high- and low-enforceability states may mask significant effects of noncompetes themselves. For example, if noncompetes chill employee mobility even in low-enforceability states, then comparisons across high and low enforceability states will underestimate the impacts of noncompetes themselves. A second important shortcoming is that most empirical studies consider one-dimensional measures of enforceability, which, in addition to being necessarily mismeasured without data on who signs noncompetes, also provide little guidance to legislators about exactly *how* to increase or decrease enforceability to reach state policy goals.

A third shortcoming of these studies is that they cannot discern the effect of enforceability on those who signed noncompetes. Instead, they

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[15] *May 2015 National Occupational Employment and Wage Estimates: United States*, BUREAU LAB. STAT., http://www.bls.gov/oes/current/oes_nat.htm#17-0000. According to the May 2015 data, there are 137,896,660 employed workers. Of those, 238,940 are chief executives (SOC code 111011) representing 0.17% of total employment, 313,970 are electrical and electronics engineers (SOC code 172070) representing 0.22% of total employment, and 642,720 are physicians and surgeons (SOC code 291060) representing 0.46% of the total employment. See also the discussion at Part II, *infra*, concerning the current evidence of which workers have signed noncompete agreements in the United States.

aggregate the effects of enforceability across those who have and have not signed, which has two important implications: First, these studies cannot show that noncompete signers are driving any observed effects of noncompete enforceability. Accordingly, there are concerns which, together with worries about properly disentangling the effect of noncompete enforceability from all the other state-level policies, do little to assuage those skeptical that these studies accurately measure the effect of noncompete enforceability. Second, such aggregation cannot identify any *external* effects of noncompetes on those who have not signed, since such employees cannot be separately identified in the data. We argue that incorporating data on who uses noncompetes could significantly strengthen studies of noncompete enforceability.

This Article provides the required background on the existing research and identifies the missing pieces needed to present the full noncompete picture. As such, we recognize that adequate data is a prerequisite to good policy decision-making. Identifying these gaps is a crucial step to fully understand the role and impact of noncompetes on various types of workers and for various sectors of the economy. Ultimately, a more complete body of reliable data on noncompetes is an essential tool for policymakers interested in legal reforms and for business people interested in understanding the competitive impacts that their choice to use noncompetes will have on their firm.

Part I begins with a discussion of the history and current state of noncompete research and the factors that have, to date, influenced the questions being addressed by researchers from various perspectives, including law, management, and economics. Here, we note that noncompetes have been disfavored since their inception as anti-competitive but generally accepted in most jurisdictions when within the bounds of reasonableness. We introduce our literature review with a discussion of the major justifications for allowing noncompetes, such as encouraging investments in human capital through training and information sharing, and the major points of opposition, including arguments on unfairness, inefficiency, and harm to innovation.

The next Part presents a detailed view of the existing legal literature focused on noncompetes. This Part catalogues the various areas of legal inquiry, which are often descriptive studies focused on individual jurisdictions without a full understanding of cross-state issues. We provide a comprehensive literature review of the growing empirical research on noncompetes encompassing many interesting outcomes, including employee mobility, earnings, innovation, entrepreneurship, and firm value. The Part begins by addressing what we know about the use and impacts of noncompetes on employee mobility and earnings. We then proceed to examine 2 experimental studies of noncompetes, and ultimately examine 14 articles that study the effects of noncompete enforceability, which constitute the bulk of the empirical noncompete literature. Throughout

this Part, we review the content, results, and methodology of the existing empirical studies. Our goal in doing so is to provide a comprehensive review of what the empirical scholarship has found and how it has found it, up to this point.

In Part II, we scrutinize the existing empirical noncompete scholarship to identify the current limitations of this work and to guard against policymakers and others reaching unsupported conclusions based on partial evidence limited to discrete contexts. We focus first on the limitations of studies examining the use and impacts of noncompetes. The two existing studies examining how noncompetes affect worker mobility and earnings find contrasting effects: noncompetes may both be associated with larger wage growth and reduced bargaining power, with both career detours and longer, more productive tenures. We suggest that future research develop more data to identify what drives such differential effects. We next argue that the lack of firm-level data on the use of noncompetes is a gross oversight of the current stream of literature and that the paucity of data prevents analyses examining which types of firms use noncompetes and how such use is related to other investment and innovative activities of the firm.

We end Part II by discussing the value added to the numerous studies of noncompete enforceability by data on who signs noncompetes. In particular, we describe how the lack of data on noncompetes themselves results in seven shortcomings of these studies. These shortcomings include the inability to estimate the chilling effect of noncompetes themselves, the inability to distinguish enforceability from the potentially increased use of noncompetes in higher enforceability states, the inability to measure noncompete enforceability properly, the necessity of assuming what firms and workers know about the enforceability of noncompetes, and the inability to identify external effects on non-signers. We conclude that incorporating data on the use of noncompetes would substantially increase the value of studies of noncompete enforceability.

Part III of the Article is forward-looking and proscriptive in its approach to how to collect more crucial data on who signs noncompetes. This Part presents a research agenda for scholars interested in filling in those gaps. We also discuss how this information is essential to policymakers, such as judges and legislators, when considering reforms to the traditional approaches to allowing noncompetes. This will also allow individual practitioners, businesspeople, and scholars to more accurately assess the crescendo of media and other commentators' criticism of noncompetes. A brief conclusion follows and calls for a well-reasoned and factually-supported debate on noncompetes and, if appropriate, balanced reforms that best match the policy goals of each state.

I. BACKGROUND

The ongoing debate in the legal literature is explored next as a prerequisite to our later discussion on what research is needed to conclusively answer some of the crucial issues about how, when, and why noncompetes are used—and, ultimately, what impact they have, for better or worse. We begin by explaining covenants not to compete in the employment context and their origins.

A. Covenants Not to Compete Explained in Brief

Covenants not to compete are a post-employment restrictive covenant between an employer and an employee that prohibits the employee from going to work for a competitor or otherwise competing with the former employer. Restrictive covenants, including employee covenants not to compete, have a long history in the common law with the first known agreements of this kind dating back to the 1400s in England.[16] From that time on, they have been recognized as anticompetitive by design because of the effect of their enforcement on curtailing what would otherwise be unfettered worker mobility.[17] Employee CNCs are often found with other restrictive covenants, such as nondisclosure and confidentiality agreements, nonsolicitation-of-client clauses, and nonsolicitation-of-former-fellow-employee provisions.[18] The typical noncompete will also restrict a worker from leaving to start a competing business.[19]

Noncompetes impede the flow and use of knowledge by restricting an individual worker's otherwise free choice of leaving one employer to join another competing employer.[20] Essentially, allowing an employer to

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[16] *See* Harlan M. Blake, *Employee Agreements Not to Compete*, 73 HARV. L. REV. 625, 631 (1960) (citing the 1414 *Dyer's Case* and other early cases).

[17] *Id.* at 631–32.

[18] *See* Norman D. Bishara & Michelle Westermann-Behaylo, *The Law and Ethics of Restrictions on an Employee's Post-Employment Mobility*, 49 AM. BUS. L.J. 1, 13, 16 (2012); *see also* Norman D. Bishara, Kenneth J. Martin & Randall S. Thomas, *An Empirical Analysis of Noncompetition Clauses and Other Restrictive Postemployment Covenants*, 68 VAND. L. REV. 1, 3–4 (2015) (finding that executive employment contracts often contain noncompete agreements alongside other restrictive covenants).

[19] Érica Gorga & Michael Halberstam, *Knowledge Inputs, Legal Institutions and Firm Structure: Towards a Knowledge-Based Theory of the Firm*, 101 NW. U. L. REV. 1123, 1183-84 (2007).

[20] The restriction of knowledge transfer has implications for issues such as a resource-based view of the firm. *See, e.g.*, Norman D. Bishara & David Orozco, *Using the Resource-Based Theory to Determine Covenant Not to Compete Legitimacy*, 87 IND. L.J. 979, 982–83 (2012) (discussing disputes between employers and employees over knowledge ownership). Moreover the use and control of knowledge is also essential to firm governance. *See, e.g.*, Gorga & Halberstam, *supra* note 19, at 1127 ("The structure of the firm in a competitive environment can be viewed as a result of three

stop an employee from going to work for a competitor or to start a competing business—even for a limited time or within a limited geographic area—provides an advantage to the former employer. That advantage comes at a cost for the individual employee and harms specific business competitors by denying them access to valuable talent, ideas, and skills. There may also be costs for the economy and harm to the creation of positive spillovers, like innovation and new venture creation.[21]

Despite the potential cost of noncompetes for individuals and regions, the use and enforcement of noncompetes may also provide both private and social benefits. For instance, proponents of private contracting argue that individuals who sign noncompetes will effectively negotiate over the terms of the contract, so that when an employee agrees to a noncompete, her expected future utility is no lower than it would be without the noncompete.[22] Other socially positive spillovers of noncompetes include increases in innovation and employee training, which may be derived from the protection noncompetes offer for trade secrets and employer good will.[23]

Notably, we are not focused here on covenants not to compete that are used during the sale of a business to protect the transferred goodwill associated with the enterprise. Those agreements are also a form of a covenant not to compete that restricts an individual seller's ability to compete with the buyer for a reasonable time and geographic scope, and they are far less controversial than the post-employment restrictions we are discussing.[24] Every state allows CNCs related to preserving the goodwill associated with the sale of a business.[25]

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imperatives: (1) a firm must produce knowledge within the firm; (2) a firm must transfer and diffuse knowledge within the firm; and (3) a firm must bind knowledge to the firm, that is, prevent its transfer outside of the firm." (footnote omitted)).

[21] *See, e.g.*, Stuart & Sorenson, *supra* note 9.

[22] *See* Stewart E. Sterk, *Restraints on Alienation of Human Capital*, 79 VAND. L. REV 383, 385 (1993); Maureen B. Callahan, Comment, *Post-Employment Restraint Agreements: A Reassessment*, 52 U. CHI. L. REV. 703, 705 (1985).

[23] *See* TREASURY REPORT, *supra* note 8, at 9–10 ("[N]on-competes can encourage additional economic activity and broader information sharing when trade secrets are significant. The training and screening explanations for noncompete agreements also suggest social benefits. If worker training is sufficiently enhanced by the availability of noncompetes, or if firms with unusually high separation costs are able to match more appropriately with workers, both worker and firm are better off.").

[24] *See* Bishara & Westermann-Behaylo, *supra* note 18, at 14–15.

[25] Even the State of California, which is well known for its ban on noncompetes in employment situations, allows for sale-of-a-business noncompetes. *See* Edwards v. Arthur Andersen LLP, 189 P.3d 285, 290–91 (Cal. 2008).

Post-employment covenants not to compete are generally disfavored because they are, by definition, anticompetitive agreements.[26] These contracts function by restricting the otherwise free mobility of the worker to join a competitor or start a competing enterprise after employment has ended.[27] As a result, there are often equity concerns related to the impact on the restricted former employee.[28] Because of the anticompetitive impact of these agreements, courts traditionally use a reasonableness test to evaluate whether the benefits of the agreement to protect a legitimate business interest outweigh the harm to the individual and even to the public interest.[29]

While noncompete policy is in transition—and that is reflected in the literature we discuss in this Part—it is nonetheless the case that most states will still enforce noncompete agreements to some extent.[30] In addition it may be that courts are increasingly focused on the issue of employee mobility related to these agreements.[31] The evaluation is also

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[26] *See, e.g.*, BDO Seidman v. Hirshberg, 712 N.E.2d 1220, 1223 (N.Y. 1999) (noting New York's approach to noncompete enforcement as an exception to the general rule against contractual restraints on trade).

[27] *See, e.g.*, Thiesing v. Dentsply Int'l, Inc., 748 F. Supp. 2d 932, 947 (E.D. Wis. 2010) ("Restrictive covenants limit one's right to work and to earn a livelihood and are therefore 'looked upon with disfavor, cautiously considered, and carefully scrutinized.' Though disfavored by Minnesota courts, non-competition agreements are enforceable to the extent they serve a legitimate employer interest and are no broader than necessary to protect this interest." (quoting Bennett v. Storz Broad. Co., 134 N.W.2d 892, 898 (Minn. 1965))).

[28] *See, e.g.*, EarthWeb, Inc. v. Schlack, 71 F. Supp. 2d 299, 311 (S.D.N.Y. 1999) (asserting that "the effect of these provisions is to indenture the employee").

[29] *See* Bishara & Westermann-Behaylo, *supra* note 18, at 18.

[30] *See generally* Norman D. Bishara, *Fifty Ways to Leave Your Employer: Relative Enforcement of Covenants Not to Compete, Trends, and Implications for Employee Mobility Policy*, 13 U. PA. J. BUS. L. 751 (2011) (analyzing the relative enforcement strength of all U.S. jurisdictions); *see also* Reliable Fire Equip. Co. v. Arredondo, 965 N.E.2d 393, 396 (Ill. 2011) ("The modern, prevailing common-law standard of reasonableness for employee agreements not to compete applies a three-pronged test."(citing BDO Seidman v. Hirshberg, 712 N.E.2d 1220, 1223 (N.Y. 1999))).

[31] *See* Michael J. Garrison & John T. Wendt, *The Evolving Law of Employee Noncompete Agreements: Recent Trends and an Alternative Policy Approach*, 45 AM. BUS. L.J. 107, 164 (2008) (asserting that "recent developments signal a shift to a strict approach to [judicial review of] restrictive covenants founded on a dominant policy concern for protection of employee mobility"). Professors Garrison and Wendt conclude that:

> The emerging trend in the law of employee noncompete agreements suggests that courts are generally more inclined to invalidate employee noncompete agreements than under the modern approach and that the law of employee noncompete agreements is becoming more protective of the employee's interest in mobility. This heightened scrutiny of employee

known as the rule of reason, reflecting the fact that it is an exception to a state's general ban on anticompetitive agreements.[32]

For instance in *Reliable Fire Equipment Co. v. Arredondo*, the Illinois Supreme Court took the opportunity to reiterate that it had "long ago explained that a contract in total and general restraint of trade was 'undoubtedly' void because it 'necessarily' injures the public at large and the individual promisor."[33] This is because, as the court explained, "[s]uch a contract deprives the public of the industry of the promisor, and deprives the promisor of the opportunity to pursue an occupation and thereby support his or her family."[34] Nonetheless, "it is equally established that a restrictive covenant will be upheld if it contains a reasonable restraint and the agreement is supported by consideration."[35]

The classic three-part reasonableness test for restrictive covenants, such as noncompetes, is as follows:

> A restrictive covenant, assuming it is ancillary to a valid employment relationship, is reasonable only if the covenant: (1) is no greater than is required for the protection of a legitimate business interest of the employer–promisee; (2) does not impose undue hardship on the employee–promisor; and (3) is not injurious to the public. Further, the extent of the employer's legitimate business interest may be limited by type of activity, geographical area, and time.[36]

The case-by-case nature of evaluating noncompetes and the concern that the reasonableness balancing test is subjective and unevenly applied has generated much criticism over the centuries. Yet restrictive covenants, and post-employment noncompetes specifically, have survived and are still very much in use today in the modern business world. Our next Section discusses some of the factors that continue to make noncompetes controversial.

> noncompete agreements reflects some of the fundamental changes taking place in the economy and in the workplace.

Id. at 112.

[32] *See Reliable Fire Equip.*, 965 N.E.2d at 397 (referring to Illinois standard approach to evaluating restrictive covenants as a "three-dimensional rule of reason").

[33] *Id.* at 396.

[34] *Id.*

[35] *Id.*

[36] *Id.* at 396–97 (citing RESTATEMENT (SECOND) OF CONTRACTS §§ 187 cmt. b, 188(1) & cmts. a–c (AM. LAW INST. 1981)). "This court long ago established the three-dimensional rule of reason in Illinois and has repeatedly acknowledged the requirement of the promisee's legitimate business interest down to the present day." *Id.* at 397.

B. Relevance: The Ongoing Noncompete Debate

In the last decade or so there has been a rising chorus of criticism about the use of noncompete agreements across a range of industries, types of workers, and in relation to socially beneficial outcomes, such as new business creation and innovation. A steady stream of media reports has materialized over what appear to be employers' abuses of noncompete contracts. In one example, it was revealed that the fast food chain Jimmy John's requires virtually all of its employees—from executives to counter workers and sandwich makers—to sign noncompete agreements that restrict the signee from working at an establishment that gets 10% of its revenue from sandwich-like items within 3 miles of any Jimmy Johns location for 2 years.[37] It appears that the employer has never brought a lawsuit to enforce the noncompetes of its low-wage workers, which may indicate a belief that the agreements are not enforceable. One such reason the employer might believe the contract to be unenforceable is that there is not an identifiable legitimate business interest at stake.

Other media reports claim that there is a rise in noncompetes from evidence of more reported court opinions cataloging when the agreement is being challenged.[38] Although more reported disputes likely indicates more of these contracts exist between employers and employees, that is not necessarily the case because these are generally private contracts and the methods of electronically reporting court cases has evolved in the last few decades. These accounts highlight perceptions that noncompetes are being used for employees that fit less clearly into a model of critically important knowledge workers. For instance, reports have cataloged and critiqued instances of noncompete use by employers of low-

[37] *See* Jamieson, *supra* note 5. The employees' lawsuit against Jimmy John's for a declaratory judgment voiding the noncompete agreements was dismissed, in part, because the court concluded that Jimmy John's had never pursued enforcement against these or other low-wage workers and that one of the plaintiffs had moved to a competitor without triggering a lawsuit. Brunner v. Liautaud, No. 14-c-5509, 2015 WL 1598106, at *10 (N.D. Ill. Apr. 8, 2015) ("In submitting the affidavits attesting to their intention not to enforce any breach of the Confidentiality and Non-Competition Agreements, Jimmy John's and the Franchisee Defendants have satisfied their burden of establishing that the challenged conduct will not 'reappear in the future.'" (citation omitted)).

[38] Ruth Simon & Angus Loten, *Litigation Over Noncompete Clauses Is Rising*, WALL ST. J. (Aug. 14, 2013), http://on.wsj.com/15GgvAl (asserting that "[m]ore employers are requiring their new workers to sign 'noncompete' agreements, which they say are needed to prevent insiders from taking trade secrets, business relationships or customer data to competing firms when they leave," without citing evidence for this conclusion).

wage, part-time, or low-skilled workers,[39] as well as younger workers, specifically.[40]

Amid this increased public scrutiny on the potentially abusive use of noncompetes against certain types of workers, policymakers are also taking notice. As a result, the noncompete legal landscape is in flux in several states. On one end of the reform spectrum, there are calls for states to be more like California,[41] which famously has had a strict ban on contracts limiting the freedom of choice and mobility of workers since the 19th century.[42] Some states are still vigorously discussing the need for and potential scope of change, as is the current state of affairs in Massachusetts.[43] In that state, consensus has been hard to achieve with business interests on both sides of the debate pushing for different policies.[44]

Yet several other states have begun experimenting in the last few decades with various models of how to best evaluate or restrict the use of noncompetes for their citizens.[45] For example, Colorado's statute restricts noncompetes to executives and their assistants.[46] Oregon's noncompetition statute, for instance, requires that an employee asked to sign a noncompete must be provided at least two weeks' advance notice of the re-

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[39] *See* Greenhouse, *supra* note 4 (citing examples of noncompete use for employees such as summer camp counselors, hairstylists, and interns).

[40] Aruna Viswanatha, *Noncompete Agreements Hobble Junior Employees*, WALL ST. J. (Feb 2, 2016), http://on.wsj.com/1SWes9p.

[41] *See, e.g.*, ORLY LOBEL, TALENT WANTS TO BE FREE: WHY WE SHOULD LEARN TO LOVE LEAKS, RAIDS, AND FREE RIDING 64 (2013).

[42] For a discussion of the historical roots of California's ban on noncompetes with the adoption of the legislation in the mid-19th century, see Ronald J. Gilson, *The Legal Infrastructure of High Technology Industrial Districts: Silicon Valley, Route 128, and Covenants Not to Compete*, 74 N.Y.U. L. REV. 575, 613–16 (1999).

[43] Despite numerous failed reform efforts in the last few years, legislation to change Massachusetts' noncompete policy has been introduced and, as of March 2016, remains under discussion. *See* Jon Chesto, *Battle over Banning Noncompete Agreements Brews on Beacon Hill*, BOS. GLOBE (Jan. 21, 2015), https://www.bostonglobe.com/business/2015/01/21/battle-over-banning-noncompete-agreements-brews-beacon-hill/JPVAM8TLjWGH56Z2AxEeCO/story.html.

[44] Kyle Gross, *This Is the Year Boston Eradicates Noncompetes*, BOSTINNO (Feb. 5, 2016), http://bostinno.streetwise.co/2016/02/05/massachusetts-could-abolish-noncompete-agreements-in-2016/ (explaining that previous Massachusetts noncompete reform efforts were opposed by the Greater Boston Chamber of Commerce, the Associated Industries of Massachusetts, and EMC, Corp.).

[45] For a list of recent state-level initiatives, including some bills in the mere proposal stage, see Beck Reed Riden LLP, *Changing Trade Secrets | Noncompete Laws*, FAIR COMPETITION L. (2016), http://faircompetitionlaw.com/the-changing-landscape-of-trade-secrets-laws-and-noncompete-laws/.

[46] The Colorado restrictive covenant statute allows covenants not to compete for executive-level employees, but disallows the agreements for other workers. *See* COLO. REV. STAT. § 8-2-113 (Supp. 2013).

quest before the start of employment.[47] Oregon's statute was also recently changed to shorten the allowable temporal scope of noncompetes from 2 years to 18 months.[48] Similarly, in 2015, Alabama updated its noncompete statute to add restrictions on the presumptively reasonable time limit in various restrictive covenants.[49] Notably, Hawaii also recently changed its law in 2015 to restrict the use of employee noncompetes for high-tech workers in an attempt to match California's success in developing the Silicon Valley agglomeration economy.[50] Still, other states have made smaller revisions, sometimes geared toward special categories of workers, such as broadcasters in New York[51], physicians in Massachusetts,[52] or used car salesmen in Louisiana.[53] Utah's recent noncompete law changes in early 2016 came after many months of debate and are a compromise solution that resulted in a tightening of noncompete rules, but stopped short of the originally proposed complete ban.[54]

An interesting change in the landscape of noncompete reform attempts is the entry of federal legislation into the mix through the recently proposed Mobility and Opportunity for Vulnerable Employees (MOVE) Act.[55] The legislation was proposed by several U.S. senators in

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[47] OR. REV. STAT. ANN. § 653.295 (West 2015).

[48] *Id.* The 2015 Oregon revisions became effective on January 1, 2016.

[49] Act of Mar. 31, 2015, No. 2015-465 (codified at ALA. CODE §§ 8-1-190 to -197). The statue provides that employee noncompetes of two years or less in duration are presumptively reasonable on the time dimension and that the party opposing enforcement has the burden of showing undue hardship if enforced.

[50] *See* Claire Zillman, *Hawaii Ban on Noncompetes Leaves out a Huge Chunk of Workers*, FORTUNE (July 8, 2015), http://fortune.com/2015/07/08/hawaii-noncompete-ban/. The Hawaii resolution was H.R. 1090, 28th Leg. (July 1, 2015) and is codified at HAW. REV. STAT. ANN. § 480-4 (West 2015).

[51] N.Y. LAB. LAW § 202-k (McKinney 2015). New York's "Broadcast Employees Freedom Act" is a carve-out from a general policy of allowing reasonable employee covenants not to compete.

[52] MASS. GEN. LAWS ch. 149, § 186 (2014). Massachusetts' law exempts several other categories of workers from noncompete enforcement, including physicians. *Id.* at ch. 112, § 12X, nurses, ch. 112, § 74D, and social workers, ch. 112, § 135C.

[53] LA. STAT. ANN. § 23:921(I)(1)(2015).

[54] *See* Bryan Benard, *Utah Non-Compete Bill Passes in Scaled-Back Form*, NAT'L L. REV. (Mar. 11, 2016), http://www.natlawreview.com/article/utah-non-compete-bill-passes-scaled-back-form; *see also* Dennis Romboy, *Employee Noncompete Bill Stirs Hornet's Nest in Utah Business Community*, DESERET NEWS (Mar. 4, 2016), http://www.deseretnews.com/article/865649318/Employee-noncompete-bill-stirs-hornets-nest-in-Utah-business-community.html?pg=all (describing the business opposition to the proposed changes).

[55] *See* Mobility and Opportunity for Vulnerable Employees Act, S. 1504, 114th Cong. (2015).

June 2015 in an attempt to curtail the use of noncompete agreements.[56] It proposes to require a notice period for when a noncompete is requested and to restrict the use of a noncompete for employees making less than $15 an hour (about $31,000 a year) in an attempt to protect lower-wage workers from being asked to sign noncompetes.[57]

The debate over noncompetes and how to reform the law related to these restrictive covenants continues to produce these new and arguably fractious pieces of legislation across the United States. It is within this ongoing policy debate that the state of the scholarly literature is best evaluated. In the next Section, we examine the development of the United States legal literature related to covenants not to compete before moving on to a discussion of the burgeoning area of empirical research related to aspects of noncompete use and impact.

C. Introduction to the Literature Review

In recent years scholarly interest in restrictive covenants, and particularly employee noncompete agreements, has resulted in various research streams. These streams vary in their relation to the discipline of origin, their methodology, and their degree of focus on macro or micro factors, such as jurisdictional geography or industry. As discussed in detail later in the Article, some states have varied policies related to noncompete enforcement, which has also led to scholarly questions about variance across U.S. jurisdictions and beyond. These agreements are also, by their nature, primarily contractual instruments with commercial implications.

The body of published legal research in this area is immense and includes hundreds of law review articles addressing the topic. Our purpose here is not to catalog and categorize each of these articles, but rather to summarize some of the trends showing how the literature has developed and suggest why those developments have occurred. This Section will focus on some of the major contributions to the legal literature in this field and in doing so provide context for the discussion in Part III. This will then help fill in the gaps in the scholarly literature to best assist the evolution of good human-capital policy related to the issue of the proper use, if any, of covenants not to compete in an employment context. It is first important to lay out the legal theoretical arguments and perspectives that have been posed on noncompetes' relationship to employee welfare, business interests, innovation, and economic prosperity. This is a necessary step in order to identify which theories in the legal literature are empirically testable. This information then allows us to critically evaluate

[56] The bill's sponsors are Democratic Senators Chris Murphy (Connecticut), Al Franken (Minnesota), Elizabeth Warren (Massachusetts), and Richard Blumenthal (Connecticut). *See* Press Release, Office of Sen. Chris Murphy, *supra* note 2.

[57] *Id.*

the emerging, yet still limited articles making up the empirical studies of noncompetes.

Accordingly, in this Section we initially group the research related to these covenants into two broad categories: the legal literature and the empirical literature. In the next Section, we focus on the legal literature covering noncompete use and policy. First, we discuss the historical development of the academic interest in noncompetes, and then look at newer attempts to describe restrictive covenant law in the United States and further scholarship chronicling normative attempts to understand the scope and purpose of noncompete use. Then in the following Section, we further investigate the development of the relatively nascent empirical investigations augmenting the legal understanding of these restrictive mechanisms.

D. Overview of the Legal Literature

Scholarly treatment of noncompete agreements is nothing new, and we are unable to catalog all of the vast literature for this Article. However, our intent is to provide an overview of the historical and modern trends in the research debate over these long-used agreements. The extensive body of early restrictive covenant research led one mid-twentieth century judge to refer to the vast "periodical sea" of writing on restrictive covenants going back to the early days of U.S. law reviews.[58] These articles described the history of judicial review related to covenants not to compete and often cataloged the case law.[59] One often-cited historical review is Harlan Blake's 1960 article *Employee Agreements Not to Compete*.[60] In his assessment of the role and history of noncompetes, Blake discusses the historical roots of covenants against competition and how reasonable partial restrictions on competition by a former apprentice or employee began to gain acceptance as exceptions to the general rule banning such agreements.[61]

Some more recent articles describe the current state of noncompete enforcement and extend the literature in the tradition of the older articles by also focusing on the development of covenants not to compete law and policy. These articles can be as straightforward as describing the

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[58] Arthur Murray Dance Studios of Cleveland, Inc. v. Witter, 105 N.E.2d 685, 687–88 (Ohio Ct. Com. Pl. 1952) (providing an extensive review of the history of noncompete case law and listing dozens of early law review articles covering restrictive covenants).

[59] *See, e.g.*, Charles E. Carpenter, *Validity of Contracts Not to Compete*, 76 U. PA. L. REV. 244 (1928); Winslow Drummond, Note, *Severability of Covenants in Partial Restraint of Trade: A New Rule*, 5 DUKE B.J. 115 (1955).

[60] Blake, *supra* note 16, at 627–28.

[61] *Id.* at 631–34.

implications of specific notable cases in one jurisdiction.[62] Some research covers the status quo of the law of CNCs in a specific state[63] or the effect of the state's policy on specific professions.[64] Still other articles focus on the changes to a state's noncompete policy due to legislative action to reform existing policies for those specific professions.[65]

Certainly many scholarly contributions have been made throughout the long history of commentary related to noncompetes. However, the recent flurry of in-depth treatments of the role and proper use—if any use should be allowed—of covenants not to compete can be in large part traced to Ronald Gilson's influential 1999 article entitled *The Legal Infrastructure of High Technology Industrial Districts: Silicon Valley, Route 128, and Covenants Not to Compete*.[66] Professor Gilson's provocative article built on earlier sociological research from AnnaLee Saxenian comparing the development of two prominent U.S. agglomeration economies: Silicon Valley in northern California and the Route 128 corridor outside Boston, Massachusetts.[67] Saxenian studied the networks and historical origins of these two economies, but Gilson contributed to the understanding of the regions' legal structures by asserting that California's ban on noncom-

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[62] *See, e.g.*, David L. Simson, Note, *Customers, Co-workers and Competition: Employee Covenants in California After* Edwards v. Arthur Andersen, 4 HASTINGS SCI. & TECH. L.J. 239 (2012).

[63] *See, e.g.*, Bradford P. Anderson, *Complete Harmony or Mere Détente? Shielding California Employees from Non-Competition Covenants While Simultaneously Protecting Employer Trade Secrets*, 8 U.C. DAVIS BUS. L.J. 8 (2007); John W. Bowers, Stacey L. Katz & Charles W. Backs, *Covenants Not to Compete: Their Use and Enforcement in Indiana*, 31 VAL. U. L. REV. 65 (1996); Jeffrey T. Rickman, Note, *Noncompete Clauses in Georgia: An Economic Analysis*, 21 GA. ST. U. L. REV. 1107 (2005); Elham Roohani, Note, *Covenants Not to Compete in Nevada: A Proposal*, 10 NEV. L.J. 260 (2009).

[64] *See, e.g.*, Xan Johnson, Comment, *Noncompetition Clauses in Physician Employment Contracts in Oregon*, 76 OR. L. REV. 195, 195, 198–99, 203 (1997) (focusing on doctors' noncompetes under Oregon's statute); Alina Klimkina, Note, *Are Noncompete Contracts Between Physicians Bad Medicine? Advocating in the Affirmative by Drawing a Public Policy Parallel to the Legal Profession*, 98 KY. L.J. 131 (2009).

[65] For a discussion of broadcaster noncompete prohibition in Oregon and other states, see Melissa Ilyse Rassas, Comment, *Explaining the Outlier: Oregon's New Non-Compete Agreement Law & the Broadcasting Industry*, 11 U. PA. J. BUS. L. 447 (2009).

[66] *See* Gilson, *supra* note 42. Professor Gilson theorizes that California's noncompete ban and a related freedom of employee mobility help form a legal framework for business that, in part, made the state's Silicon Valley innovation economy possible. *Id.* at 578.

[67] ANNALEE SAXENIAN, REGIONAL ADVANTAGE: CULTURE AND COMPETITION IN SILICON VALLEY AND ROUTE 128, at 1–4 (1996) (concluding that the networked culture, in addition to connections to certain leading universities and investors, substantially aided Silicon Valley's rise as the preeminent high-tech region in the United States).

petes was an important factor in Silicon Valley's significant growth.[68] This suggestion has been the impetus for much of the empirical testing described in the following Section, testing which continues to this day, as scholars refine the samples and methods to identify and isolate the impact of noncompetes.

However, before we describe the empirical scholarship progeny of Professor Gilson's thesis about the reason for Silicon Valley's fast-moving employment market and related knowledge spillovers, it is useful to briefly categorize the legal scholarship that developed in the fifteen years since. In particular, there has been a body of useful normative research that has followed in the wake of this resurgence in scholarly interest on noncompetes and an alleged "California effect" identified in Gilson's article.

There is also a wealth of legal scholarship focused on the possible negative impact of noncompete enforcement on individual employees. These include important noncompete-related research touching on employee rights[69] and the potential for the employer's abuse of superior bargaining leverage,[70] the negative implications of noncompetes when employees are facing a difficult labor market,[71] or even the ethical implications of noncompetes and other legal doctrines or contractual tools.[72] In addition, researchers have continued to probe the uses and issues related to noncompetes in the new context of greater cross-state mobility,[73]

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[68] Gilson, *supra* note 42, at 578.

[69] *See, e.g.*, Katherine V.W. Stone, *The New Psychological Contract: Implications of the Changing Workplace for Labor and Employment Law*, 48 UCLA L. REV. 519, 581–82 (2001) (noting that employers who use covenants not to compete may be abusing their bargaining position in relation to an employee's right to control his or her own mobility and career advancement).

[70] Rachel S. Arnow-Richman, *Bargaining for Loyalty in the Information Age: A Reconsideration of the Role of Substantive Fairness in Enforcing Employee Noncompetes*, 80 OR. L. REV. 1163, 1214–15 (2001) (addressing both substantive and procedural concerns with employee CNCs).

[71] Kate O'Neill, '*Should I Stay or Should I Go?*'*—Covenants Not to Compete in a Down Economy: A Proposal for Better Advocacy and Better Judicial Opinions*, 6 HASTINGS BUS. L.J. 83, 84 (2010) (arguing that courts should hesitate to enforce noncompetes in situations when the former employee lacks "significant bargaining power").

[72] *See* Bishara & Westermann-Behaylo, *supra* note 18, at 2–3 (listing the potential negative implications of restricting an employee's post-employment mobility).

[73] *See, e.g.*, Timothy P. Glynn, *Interjurisdictional Competition in Enforcing Noncompetition Agreements: Regulatory Risk Management and the Race to the Bottom*, 65 WASH. & LEE. L. REV. 1381, 1385 (2008) (examining cross-state conflict-of-laws, choice-of-law, and forum issues in the noncompete context). *See generally* Gillian Lester & Elizabeth Ryan, *Choice of Law and Employee Restrictive Covenants: An American Perspective*, 31 COMP. LAB. L. & POL'Y J. 389 (2010) (discussing the choice of law implications of modern noncompete agreements).

or international implications of CNCs,[74] and other proposals for reform in curtailing noncompete use.[75]

Other researchers have discussed proposals for reforming or refining the traditional reasonableness test with suggestions to apply the doctrine selectively to certain types of knowledge workers,[76] or to add an understanding of knowledge management with the resource-based view of business strategy,[77] such as when an IP-protection justification for noncompetes is invalid.[78]

Still other scholars have concluded that noncompetes are essentially meritless and should be abandoned on various policy grounds.[79] These include Professor Orly Lobel's focus on the easy transfer of knowledge

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[74] *See* Marisa Anne Pagnattaro, *"The Google Challenge": Enforcement of Noncompete and Trade Secret Agreements for Employees Working in China*, 44 AM. BUS. L.J. 603, 606–13 (2007) (examining the high-profile case of a Microsoft Corporation lawsuit against a former executive who left to become the head of the then new Google China venture).

[75] *See* Garrison & Wendt, *supra* note 31, at 185.

[76] *See, e.g.*, Norman D. Bishara, *Covenants Not to Complete in a Knowledge Economy: Balancing Innovation from Employee Mobility Against Legal Protection for Human Capital Investment*, 27 BERKELEY J. EMP. & LAB. L. 287, 319–22 (2006).

[77] *See generally* Bishara & Orozco, *supra* note 20, at 982–83.

[78] Viva R. Moffat, *The Wrong Tool for the Job: The IP Problem with Noncompetition Agreements*, 52 WM. & MARY L. REV. 873, 878 (2010) ("Even to the extent that trade secret law is *unintentionally* weak, the IP justification for noncompetes is not compelling because noncompetes are not a good tool for achieving the purposes of IP protection.").

[79] Viva R. Moffat, *Making Non-Competes Unenforceable*, 54 ARIZ. L. REV. 939 (2012). Professor Moffat argues that in enforcing a noncompete "a court faces some difficult decisions, such as determining the content and intent of the contract, determining the content of various states' laws, and resolving both the conflict-of-laws issues and the substantive question of the enforceability of the agreement (which can often be a close call)." *Id.* at 942. Moreover, she adds:

> The result is unpredictability on every level—for employees, employers, and courts. This uncertainty has only increased as more entities operate on a nationwide basis and employees are increasingly mobile and willing to move across state boundaries.
>
> This unpredictability, and its accompanying costs, has become enough of a problem that a uniform approach ought to be adopted. Additionally, the benefits of uniformity in the law are much more likely to accrue with a straightforward rule of unenforceability. This rule could be adopted through the Uniform Act process, by reference to a model act, or simply as a result of the dissemination of information about the advantages of uniformity and the benefits of a rule of unenforceability. Regardless of how it is achieved, a rule of unenforceability would virtually eliminate the myriad disadvantages of diversity in state law in this context.

Id. (footnote omitted).

skills between employers and entrepreneurial activity[80] and earlier work by Professor Alan Hyde on the importance of "high-velocity" labor markets.[81] This research—in addition to Gilson's thesis about California's ban on covenants not to compete for employees—has developed alongside empirical research emanating from other academic disciplines, such as management, economics, and strategy. Thus far, the theoretical legal literature has discussed many of the positive and negative spillovers from noncompetes. However greater empirical work is needed to test these theories and determine which ones should form the basis of policy reform.

The next Section discusses the limited, but growing, body of empirical research that has started to create a more complete picture of noncompetes and their impact on employees, firms, and the public interest.

E. Overview of the Empirical Literature

Despite the 600-year history of covenants not to compete,[82] social scientists have only recently begun empirically examining the impacts of noncompetes and noncompete enforcement policies. The interest in studying the uses and impacts of noncompetes and noncompete enforceability was likely spurred by attempts to understand the growth of Silicon Valley and in particular how it outpaced Route 128 to become the much-admired technological hub of the United States.[83] As a result, the earliest studies examined the role of noncompete enforceability on startup behavior and the mobility of executives and engineers.

The empirical literature on noncompetes is burgeoning.[84] In this section we examine 24 empirical studies of noncompetes: 6 use individual level data on the use of noncompetes (4 on CEOs, 1 on physicians, 1 on

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[80] *See generally* LOBEL, *supra* note 41 (advocating for the reduction of legal barriers to knowledge transfer in the pursuit of greater innovation).

[81] *See* ALAN HYDE, WORKING IN SILICON VALLEY: ECONOMIC AND LEGAL ANALYSIS OF A HIGH-VELOCITY LABOR MARKET (2003).

[82] The earliest known challenges to a covenant not to compete date back to the 15th century. *See* Blake, *supra* note 16, at 631.

[83] See generally the discussions of SAXENIAN, *supra* note 67, and Gilson, *supra* note 42.

[84] We are aware of a number of papers at the early stages of work, which we will not comment on due to their still preliminary nature. Included in this work is a paper on the impacts of noncompete enforceability on the career and within-job employment dynamics of technical employees. Natarajan Balasubramanian, Jin Woo Chang, Mariko Sakakibara & Evan Starr, Locked In? Noncompete Enforceability and the Mobility and Earnings of High Tech Employees (manuscript on file with the authors). A second recent working paper is Michael Ewens & Matt Marx, Founder Replacement and Startup Performance (Jan. 17, 2016), http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2717124.

engineers); 3 have data on the use of noncompetes across firms; 2 experimentally allocate noncompetes; and 14 study the effect of noncompete enforceability (without data on who signs).

1. Articles Studying the Use and Consequences of Noncompetes

a. The Use of Noncompetes Among Employees

As far as we are aware, the only systematic evidence on the use of noncompetes among workers comes from three occupations: executives, physicians, and engineers.[85]

Marx's 2011 article describes survey data from 1,029 technological professionals (a 20.6% response rate of the 5,000 people surveyed) within the Institute of Electrical and Electronics Engineers, a nonprofit technical professional association. The article shows that 43.3% of survey respondents sign noncompetes.[86] Bishara, Martin, and Thomas,[87] Garmaise,[88] Schwab and Thomas,[89] and Heen[90] examine executive contracts and show respectively that 80%, 70%, 67%, and 50% of S&P 1500 executives sign noncompetes. Lavetti, Simon, and White's study focuses on a sample of primary-care physicians from five states and finds that 45% of physicians have signed an employee CNC.[91] Thus, among some very high-skill occupations, the incidence of noncompetes appears to be very high.

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[85] For executives, see the study of U.S. public-firm CEOs in Bishara et al., *supra* note 18; and the studies of executives in Mark J. Garmaise, *Ties that Truly Bind: Noncompetition Agreements, Executive Compensation, and Firm Investment*, 27 J.L. ECON. & ORG. 376 (2011); and in Stewart J. Schwab & Randall S. Thomas, *An Empirical Analysis of CEO Employment Contracts: What Do Top Executives Bargain For?*, 63 WASH. & LEE L. REV. 231 (2006); and CEO separation pay and noncompetes in Knut Heen, Working Paper, *Non-Compete Agreements: The Real Cause of Separation Pay?* (Jan. 2008), www.sifr.org/PDFs/KnutHeenJobMarketPaper.pdf. For physicians, see Kurt Lavetti, Carol Simon & William D. White, Buying Loyalty: Theory and Evidence from Physicians (Feb. 16, 2015) (unpublished manuscript), http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2439068. For engineers, see the interview-based study in Matt Marx, *The Firm Strikes Back: Non-Compete Agreements and the Mobility of Technical Professionals*, 76 AM. SOC. REV. 695 (2011).

[86] Marx, *supra* note 85, at 702 tbl.1.

[87] Bishara et al., *supra* note 18, at 3. The authors examined 874 CEO employment contracts initiated between 1996 and 2010 from a random sample of 500 S&P 1500 companies.

[88] Garmaise, *supra* note 85, at 396. Garmaise selected a random sample of 500 firms from the Execucomp database and found evidence from SEC filings that 351 of the firms use noncompetes with their top executives.

[89] Schwab & Thomas, *supra* note 85, at 255 tbl.9. The authors' dataset is similar to that of Bishara, Martin, and Thomas. *See* Bishara et al., *supra* note 18, at 24–27.

[90] Heen, *supra* note 85, at 18. The author's dataset, while also from S&P 500 companies, uses only the first 250 CEOs chosen alphabetically by firm name.

[91] Lavetti et al., *supra* note 85, at 5 (studying the mobility of physicians in relation to noncompete agreements).

We can certainly learn something about how noncompetes are used in these occupations, but these occupations together account for only 0.87% of the U.S. labor force.[92]

Of these six studies, three consider how noncompetes affect worker-level outcomes.[93] Marx's article includes both a survey of electrical and electronic engineers, and data from interviews with 52 inventors with at least 2 patents in the automatic-speech-recognition (ASR) industry.[94] Marx's first main finding is that noncompetes are associated with individuals leaving the industry, which he refers to as career detours.[95] His interview data shows that 87.5% of moves governed by a noncompete were out of the ASR industry (21 out of 24), while only 27.5% of moves not governed by a noncompete were out of the ASR industry (11 of 40).[96] Marx's survey data corroborates the interview results: he finds that of the 276 respondents who signed noncompetes and changed jobs, 32.6% reported taking a job in a different industry.[97]

The other contribution of Marx's paper is to show that many firms manage the process of noncompete signing to reduce employee bargaining power. Among the 455 respondents who signed a noncompete, 47% report that the firm asked them to sign the noncompete on or after the first day.[98] That firms manage the noncompete process strategically is not only an interesting result, but it also brings up important questions about whether noncompetes are more or less likely to affect individuals who did not have the bargaining power to properly negotiate over them. For example, are the individuals who took career detours due to their noncompete the ones who were aware of the noncompete before accepting the job, or were they the ones who were asked on or after their first day of work? Despite having this data, Marx does not examine this question.

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[92] *See May 2015 National Occupational Employment and Wage Estimates: United States*, BUREAU LAB. STAT., http://www.bls.gov/oes/current/oes_nat.htm. Based on the May 2015 data, there are 137,896,660 employed workers. Of those, 238,940 are chief executives (SOC code 111011) representing 0.17% of total employment, 313,970 are electrical and electronics engineers (SOC code 172070) representing 0.22% of total employment, and 642,720 are physicians and surgeons (SOC code 291060) representing 0.46% of the total employment.

[93] *See* Marx, *supra* note 85; Lavetti et al., *supra* note 85.

[94] *See* Marx, *supra* note 85, at 700 (interview and survey and methodology).

[95] *Id.* at 702–03.

[96] *Id.* at 703 (discussing the results of interviews with engineers).

[97] *Id.* at 705. Marx described the conclusions from this data:The fact that similar proportions of in-depth interviewees (one-quarter) and survey respondents (nearly one-third) reported taking career detours in response to a non-compete indicates that the threat of a non-compete lawsuit may have deterred technical professionals from continuing to work in their chosen industry. *Id.*

[98] *Id.* at 706 tbl.4.

While Marx suggests that firms use noncompetes to "strike back" at employees, Lavetti, Simon, and White find that physicians who sign noncompetes are much better off than those who do not sign. Noncompete-signing physicians earn 14% higher incomes, earn 27% more revenue per hour, have 21 percentage point higher within-job wage growth (23% for noncompete signers relative to 2% for nonsigners), see 12% more patients per week (and more privately insured patients), and have 29% longer tenures.[99] The authors note that these differential results for noncompete signers are explained in part by the fact that physicians who sign noncompetes have different incentives in their contracts: the share of total earnings that comes from individual productivity is more than twice as high for physicians who sign noncompetes (27.1% to 13%), while the proportion from a guaranteed fixed salary is significantly lower (59% to 74%).[100]

The authors argue that the combination of contracts that encourage physicians to work with many patients and the noncompete is important: contracts that encourage more interactions with patients create competition risk for the firm if the physician wants to leave.[101] Thus, the noncompete functions to buy the loyalty of the physician, providing the right contractual incentives for the physician to exert effort and the right incentives for the firm to invest in attracting patients.[102] Lavetti, Simon, and White conclude that "share-based compensation contracts can overcome the effects of [noncompetes] on bargaining power, allowing for an incentive-compatible equilibrium with [noncompetes] in which workers with [noncompetes] are more productive, have higher earnings, larger returns to tenure, and longer job spells."[103]

Heen's unpublished article explores the role of noncompetes in determining CEO separation pay. He argues that differences in the timing

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[99] *See* Lavetti et al., *supra* note 85, at 27. The authors' main results show that physicians who sign noncompetes earn 14% more than non-signers, *id.* at 34 tbl.6, show 21 percentage point higher within-job earnings growth, *id.* at 38 tbl.9, see 12% more patients per week and make 27% more revenue, *id.* at 39 tbl.10, and have 29% longer tenures. *Id.* at 40 tbl.12.

[100] *Id.* at 33 tbl.4 (discussing contract-type results).

[101] *Id.* at 5 (noting the risk of losing patients to departing physicians).

[102] *Id.* at 1–3. The authors conclude that to achieve the proper incentives for attracting and retaining doctors, there must be a situation such that:

> when turnover is relatively costly and long-term contracts are not credible, commitments to productivity-based piece-rate linear compensation contracts can overcome the effects of dynamic changes in bargaining power without front-loading compensation. . . . We show that sharing contracts are more strongly tied to output when accompanied by NCAs, and necessarily increase the expected returns to tenure.

Id. at 3.

[103] *Id.* at 27.

of when the noncompete is signed predict the extent of either contractually obligated separation pay or discretionary separation pay. In a sample of 250 CEOs, chosen alphabetically by firm name, he finds that, "firms promise separation pay contractually to executives who sign noncompete agreements years before they leave the firm while firms pay executives discretionarily at separation if they first sign noncompete agreements at this point."[104]

b. The Use of Noncompetes Among Firms

There are three studies of the use of noncompetes within firms. Kaplan and Stromberg show that 70% of 119 portfolio companies receiving venture capital funding were required by their financiers to sign noncompetes, although the 119 companies were funded by a total of just 14 venture capitalists.[105] Galle and Koen surveyed 1,000 human-resources managers and received 123 responses, 55% of which indicated that their firms used noncompetes, though they do not specify in which occupations.[106] A 2007 Society of Human Resource Management survey reports that of the 354 out of 2,886 surveys returned, 56% of firms use noncompetes, which is up from 51% from a similar survey in 2005.[107]

Response rates and sample selection concerns aside, it is unclear whether any of these studies tell us anything new about the use of noncompetes since we already know that the executives within a company are likely to sign them.[108] Furthermore, none of these studies examine which types of employees within the firm are likely to sign, which types of firms are more likely to use noncompetes, and how the use of noncompetes affects firm-level outcomes such as investment in R&D, training, or productivity. Thus we have no empirical understanding of how the use of noncompetes and similar contractual restrictions are related to the firm-level outcomes.

2. Articles Describing Noncompete Experiments

Given that data on the use of noncompetes is sparse and that the nonrandom use of noncompetes may make causal inference difficult, two

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[104] Heen, *supra* note 85, at 24.

[105] *See* Steven N. Kaplan & Per Strömberg, *Financial Contracting Theory Meets the Real World: An Empirical Analysis of Venture Capital Contracts*, 70 REV. ECON. STUD. 281, 289 & tbl.2 (2003).

[106] William P. Galle, Jr. & Clifford M. Koen, *Reducing Post-Termination Disputes: A National Survey of Contract Clauses Used in Employment Contracts*, 9 J. INDIVIDUAL EMP. RTS. 227, 234–35 (2001) (including a summary of related noncompete data).

[107] Interview with Tanya Mulvey, Researcher, Society for Human Resource Management. The 2005 survey had a final sample of 392 respondents.

[108] For a discussion of the evidence of employers' use of noncompetes in executive contracts, see Bishara et al., *supra* note 18, at 3; Garmaise, *supra* note 85, at 396; and Schwab & Thomas, *supra* note 85, at 262.

experimental papers randomly assign noncompetes to examine whether and how noncompetes affect employee effort. The experiment in Amir and Lobel's study has two phases. In phase 1, individuals are randomly assigned to one of two types of pay-for-performance tasks: either (i) a "Matrix Search" task, in which individuals were paid by finding two numbers (with two decimals) in the matrix that sum to 10; or (ii) a "Remote Associates" task, in which individuals were instructed to find a fourth word that is most closely associated with the trios of words presented.[109] The authors classify the former task as "pure effort," and the latter task as "creative."[110] In phase 2, the individuals were invited to do another of these tasks. All individuals were informed that there would be a second phase upfront.[111]

To simulate noncompetes, the authors randomly assigned individuals to two treatments. In the first treatment, the "absolute noncompetition condition," individuals were told that they would be prohibited from performing the same task in the second phase.[112] In the second treatment, "partial noncompete condition," individuals were informed that their earnings in the second phase, regardless of which task they performed, would be reduced by 20%.[113] A third of the respondents were randomly assigned to the control group. The authors consider three outcomes: (1) task completion; (2) performance (number of questions skipped and solved correctly) of the tasks; and (3) enjoyment of the tasks.[114]

The authors find that 57.9% of participants who dropped out of the study were in one of the noncompete conditions, while 51.6% dropped out in the control group. For those in the creative task, the absolute noncompete condition group had a 35% completion rate, the partial noncompete condition group had a 38% completion rate, and the control group had a completion rate of 48%.[115] For those who did not drop out of the study, the authors find that in the Remote Associates task there were no statistically significant differences in the number of skipped questions, the error rate, the time spent, or the reported enjoyment be-

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[109] *See* On Amir & Orly Lobel, *Driving Performance: A Growth Theory of Noncompete Law*, 16 STAN. TECH. L. REV. 833, 852 (2013). The authors' experiment was designed to "examine the effects of postemployment restrictions on motivation and performance, taking into account task characteristics, and in particular, controlling for the level of difficulty and creativity inherent in the tasks." *Id.* at 850–51.

[110] *Id.* at 852.

[111] *Id.* at 852–53.

[112] *Id.*

[113] *Id.*

[114] *Id.*

[115] *Id.* at 854–55.

tween either of the noncompete conditions or the control group.[116] For those in the Matrix Search task, however, the error rate of those in both the absolute noncompete group and the partial noncompete group was more than two times that of those in the control group, though they were no more likely to skip answers, no more likely to spend less time on the questions, and report no difference in enjoyment.[117] The authors conclude that "certain postemployment contractual restrictions may negatively impact motivation and performance, as evidenced by the greater rates at which individuals abandon tasks. . . . [N]oncompetes, under certain conditions, discourage employees to invest in their work performance."[118]

A shortcoming of the Amir and Lobel study is that the use of the noncompete is forcibly applied, resulting in little scope for remuneration or negotiation, which may subsequently affect effort. To assess these agency issues, Bünstorf, Engel, Fischer, and Güth design an experiment in which a principal and an agent are matched together and the principal is free to set the wage of the agent, anticipating how much effort the agent will exert.[119] Agents exert effort to create an innovation, and with a certain probability innovation occurs.[120] The authors find that individuals in the noncompete group exert no differential effort toward the innovation due to the fact that individuals who sign noncompetes receive increased wages.[121] The authors conclude: "Our experiment yields a clear message: introducing a noncompete clause does not affect effort and therefore leaves success unaffected."[122]

The key contribution of these experiments is the development of a better understanding of how noncompetes affect employee effort. Noncompetes may reduce effort by restricting the return at competing firms, but, as shown in Lavetti, Simon, and White's study of physicians, noncompetes may also be associated with fundamentally different contractual incentives.[123] In the real world, many more such determinants are likely correlated with noncompete use, including deferred compensation, con-

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[116] *Id.*

[117] *Id.*

[118] *Id.* at 863.

[119] Guido Bünstorf et al., Win Shift Lose Stay—An Experimental Test of Non-Compete Clauses 3–4 (Sept. 19, 2013) (unpublished manuscript) http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2330262.

[120] *See id.* at 6–7 (describing the design of the experiment).

[121] *Id.* at 18–19. The noncompete manipulation, specific to the German context—in which noncompetes must be limited to two years and are valid only if the principal pays half of the yearly salary—randomly imposes a pre-determined compulsory level of payment in the case where negotiation fails. *Id.* at 2.

[122] *Id.* at 4.

[123] *See supra* notes 99–103 and accompanying text.

tracts or bonuses based on goal achievement, and long internal labor markets. Such mechanisms may induce workers to exert substantial effort in spite of their noncompete.

Additionally, by experimentally manipulating the noncompete condition, these studies do not take into account the fact that firms *choose* to use noncompetes for certain occupations and that employees may or may not agree to them.[124] If individuals who are willing to sign noncompetes feel appropriately motivated by other features of the job, then it is unclear whether they will exert lower effort than those who do not sign.

3. Articles Studying Noncompete Enforceability

a. Entrepreneurship

As discussed by Samila and Sorenson, and by Starr, Balasubramanian, and Sakakibara, the impact of noncompete enforceability on entrepreneurship is theoretically ambiguous. On the one hand, aspiring entrepreneurs might be inclined to start their firm in high-enforceability areas to take advantage of the protection offered by the enforceability regime. On the other hand, if the potential entrepreneur wishes to start a competitor with her current employer, then her own noncompete may be an entry barrier that makes it more costly for him to create a new business. Relatedly, if the potential entrepreneur manages to start her company, it may be risky to hire employees who may be bound by noncompetes. As a result of these competing forces, it is unclear how noncompete enforceability will impact entrepreneurial behavior.[125]

The only published studies of noncompete enforceability and entrepreneurship and innovation are analyses examining how noncompete enforceability moderates the relationship between two other variables. Stuart and Sorenson focus on entrepreneurship rates in the biotech industry following a liquidity event such as an initial public offering (IPO) or an acquisition.[126] They argue that such liquidity events free up employees to follow their latent entrepreneurial preferences.[127] They find that entrepreneurship in the biotech industry spikes following liquidity

[124] Experimentally assigning a noncompete does not give the individual the option of choosing whether or not to sign, and ignores the alternative options that the individual may have such as negotiating to modify the terms, refusing to sign, or taking another job without the restriction.

[125] See the discussion in Starr et al., *supra* note 13, at 8; *see also* Samila & Sorenson, *supra* note 9, at 426–28 (discussing the tension between investment and impediments to growth).

[126] Stuart & Sorenson, *supra* note 9, at 175.

[127] *Id.* at 175–76.

events, but that the effect of such liquidity events on firm founding rates is muted in states that enforce noncompetes.[128]

Aside from the results, a primary contribution of Stuart and Sorenson is the development of the first cross-state measure of noncompete enforceability. Using the Malsberger treatises designed for practitioners, *Covenants Not to Compete: A State by State Survey*, Stuart and Sorenson create a variable equal to one if a state does not enforce noncompetes and equal to zero if the state does enforce noncompetes.[129] Overall, they find that 10 states do not enforce noncompetes.[130] Their categorization of state noncompete policies is used regularly to define control states in subsequent studies that examine the 1985 change in Michigan's noncompete laws identified by Marx, Strumsky, and Fleming.[131] Subsequent studies create more nuanced measures of noncompete enforceability by taking into account various dimensions of enforceability.[132] It bears noting that a recent article by Barnett and Sichelman casts significant doubt on the validity of the initial categorization by Stuart and Sorenson, arguing that at most two states are non-enforcing, while all others enforce to some extent.[133]

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[128] *Id.* at 193 tbl.4 (showing that noncompete enforceability reduces startups post acquisition and post IPO).

[129] *Id.* at 190 tbl.1.

[130] *Id.*

[131] Multiple studies use the non-enforcing states as identified by Professors Stuart and Sorenson. *See* Sharon Belenzon & Mark Schankerman, *Spreading the Word: Geography, Policy, and Knowledge Spillovers*, 95 REV. ECON. & STAT. 884, 885 (2013); Marx et al., *supra* note 10, at 876; Matt Marx, Jasjit Singh & Lee Fleming, *Regional Disadvantage? Employee Non-Compete Agreements and Brain Drain*, 44 RES. POL'Y 394, 394–404 (2015): Kenneth A. Younge, Tony W. Tong & Lee Fleming, *How Anticipated Employee Mobility Affects Acquisition Likelihood: Evidence from a Natural Experiment*, 36 STRATEGIC MGMT. J. 686, 687 (2015); Kenneth A. Younge & Matt Marx, *The Value of Employee Retention: Evidence from a Natural Experiment*, J. ECON. & MGMT. STRATEGY (forthcoming 2016). These empirical studies are described in detail throughout the following Subsection.

[132] *See, e.g.*, Bishara, *supra* note 30; Garmaise, *supra* note 85; Evan Starr, Consider This: Firm-Sponsored Training and the Enforceability of Covenants Not to Compete (Nov. 5, 2015) (unpublished manuscript), http://ssrn.com/abstract=2556669.

[133] Jonathan M. Barnett & Ted Sichelman, *Revisiting Labor Mobility in Innovation Markets* (Univ. of S. Cal. Legal Studies Paper No. 16–15, 2016), http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2758854. The authors conclude that the underlying enforceability assumptions by Stuart and Sorenson are incorrect, for:

> Even if one were to draw an arbitrary line between states, it would result during this time period in at most two "non-enforcing" states. Consistent with both Bishara's (2011) comprehensive state-by-state review and our own independent review, we find that during the relevant time periods, other than California and North Dakota, none of the purported "non-enforcing" states in Stuart and Sorenson (2003)—namely, Alaska, Connecticut, Michi-

In a study similar to Stuart and Sorenson, Samila and Sorenson examine the effect of venture capital on regional outcomes including firm foundings, patent counts, regional employment and payroll.[134] They are interested in whether noncompete enforceability moderates the impact of venture capital on these regional outcomes.[135] They find that while more venture capital is associated with more entrepreneurship and employment, in states that enforce noncompetes the effect of venture capital is mitigated.[136] The authors ambitiously conclude that noncompete enforceability "significantly impedes entrepreneurship and employment growth."[137] Such a conclusion may be too strong, however. As this study examines the moderating effect of noncompete enforceability on the impact of venture capital on entrepreneurship and employment—only "a piece of the puzzle" as Samila and Sorenson themselves say[138]—it could be that the causal effect of noncompete enforceability on entrepreneurship is positive, but that it is diminished in high venture-capital areas.

In a recent working paper, Starr, Balasubramanian, and Sakakibara take a firm-level approach to measure the impact of noncompete enforceability on the formation and subsequent performance of firms.[139] They argue that prior studies suffer from two shortcomings. First, they examine the moderating effect of noncompete enforceability, not the direct effect. Second, studies at the aggregate level necessarily treat formation of all firms the same, regardless of whether noncompetes were relevant for the actual formation of a given firm.[140]

Using employer–employee matched data, they identify firms that were founded as spinouts in the same industry of a parent employer and argue that these firms may face additional noncompete barriers in order to enter the market relative to other new entrants.[141] Using law firms as a control group (since law firms make up the only industry across all the states in which noncompetes are not enforceable) to identify the impact

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gan, Minnesota, Montana, Nevada, Oklahoma, Washington, and West Virginia—can plausibly be classified in this manner.

Id. at 13–14.

[134] *See* Samila & Sorenson, *supra* note 9, at 425.

[135] *Id.* at 426 ("We address this issue by focusing on a piece of the puzzle. Rather than examining the average differences across regions, we estimate how regions respond to shocks in the supply of one form of financial capital—venture capital (VC)—and examine whether the effects of these shocks depend on the enforcement regime.").

[136] *Id.* at 433–435 tbls.4–5.

[137] *Id.* at 425.

[138] *Id.* at 426.

[139] Starr et al., *supra* note 13, at 9–11.

[140] For a full description of this issue, *see generally* Stuart & Sorenson, *supra* note 9.

[141] Starr et al., *supra* note 13, at 12–14 (discussing the phenomenon of within-industry spinouts).

of noncompete enforceability, they find that in higher enforceability states there are fewer within-industry spinouts, but that those that are created tend to start larger, stay larger, and survive longer than other new ventures.[142] They provide evidence that these results are consistent with noncompete enforceability screening low human-capital founders from starting within-industry spinouts, concluding that noncompetes are unlikely to deter the best employees from starting up within-industry spinouts.[143] They further find that in higher enforceability states new firms that are not within-industry spinouts are slightly more likely to enter, but those that do enter start smaller, stay smaller, and are less likely to survive.[144] These results are consistent with a model in which noncompete enforceability induces firms to enter to take advantage of the protection of their future assets, but that unanticipated hiring challenges due to noncompete enforceability deter their success.[145]

To summarize, the literature has found that noncompete enforceability has somewhat negative, though nuanced, effects on entrepreneurial behavior. In particular, the results suggest that enforceability deters firm entry post-IPO and post-acquisition in the bio-tech industry, and reduces firm start-ups relatively more in areas that have high venture capital. However, as Starr, Balasubramanian, and Sakakibara show, the reduced entry is driven by reduced entry of low quality within-industry spinouts.

b. Employee Mobility, Wages, and Training

Fallick, Fleischman, and Rebitzer conducted the first study examining whether or not employee mobility is higher in California, consistent with Gilson's argument that California's ban on noncompetes encouraged high levels of employee mobility.[146] Using employee mobility data from the U.S. Census's Current Population Survey, Fallick and his coauthors find that there is increased mobility in Silicon Valley relative to

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[142] *See id.* at tbls.2 (entry results), 3 (initial size results), 6 (later-life firm-size results), 7 (survival results).

[143] *Id.* at tbl.5 (characteristics of founders). In a recent working paper, Salome Baslandze finds similar results, using patent data to identify spinouts of parent companies. *See* Salome Baslandze, Spinout Entry, Innovation, and Growth (Einaudi Institute for Economics and Finance, Working Paper), https://sites.google.com/site/sabaslandze/research. She shows that noncompete enforceability is negatively correlated with spinout entry. These results are purely cross-sectional however, and no attempt is made at identifying the causal effect of enforceability on spinout formation.

[144] Starr et al., *supra* note 13, at tbls.2 (entry results), 3 (initial size results), 6 (later-life firm-size results), 7 (survival results).

[144] *Id.* at tbl.5 (characteristics of founders).

[145] *Id.* at 28–29 (explaining non-within-industry spinout results).

[146] Fallick et al., *supra* note 14, at 472 ("*Noncompete agreements*, according to Gilson, are the most important legal mechanism for reducing interfirm mobility.").

other metropolitan areas with large IT sectors.[147] Furthermore, they find that there is a California effect, such that employee mobility is actually higher in the computer industry in all metropolitan areas in California relative to elsewhere. Outside of the computer industry, however, these California-specific mobility differentials disappear.[148] While not directly examining noncompete enforceability, this study provides suggestive evidence that a state's noncompete policy may indeed lead to differences in mobility patterns.

Three papers argue that noncompete enforceability affects executive and engineer mobility. First, Marx, Strumsky, and Fleming examine the Michigan Antitrust Reform Act (MARA) of 1985, which unintentionally removed a legislative prohibition on noncompetes, leading Michigan to enforce noncompetes starting in 1987.[149] Using patent data, they identify moves among inventors with at least two patents, comparing the likelihood of movement before and after 1985 in Michigan to a group of 10 control states initially identified in Stuart and Sorenson, which supposedly do not enforce noncompetes.[150] The authors find that after MARA, the mobility of inventors fell by 8% relative to the control states, and fell more for inventors who had developed more firm-specific capital (as measured by citations) and more for inventors whose inventions were concentrated in a specific patent category.[151]

It is important to note that the Marx, Strumsky, and Fleming paper makes a fundamental contribution to the empirical literature on noncompetes, which is to identify a random change in noncompete enforceability in Michigan in 1985.[152] While cross-state differences in noncompete policies are large, in the time since Michigan's reversal no state has had entire shifts in enforceability, either from enforceability to nonenforceability or vice versa.[153] Thus, in the years following the publication of Marx, Strumsky, and Fleming, researchers interested in the effects of noncompete enforceability began to use the Michigan natural experiment to study the causal effect of noncompetes.[154]

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[147] *Id.* at 477 tbl.1.

[148] *Id.* at 480 tbl.3.

[149] Marx et al., *supra* note 10, at 876–79 (discussing the development of MARA).

[150] *Id.* at 879–82 (citing Stuart & Sorenson, *supra* note 9).

[151] *Id.* at 883–86 tbls. 2–4.

[152] *Id.* at 876–79.

[153] For further detail on state laws related to noncompete enforcement, *see generally* BRIAN M. MALSBERGER, COVENANTS NOT TO COMPETE: A STATE-BY-STATE SURVEY (10th ed. 2015); Bishara, *supra* note 30.

[154] *See, e.g.*, Belenzon & Schankerman, *supra* note 131, at 885; Marx et al., *supra* note 131, at 394; Younge et al., *supra* note 131, at 687; Younge & Marx, *supra* note 131.

Given the extensive work that now relies on the Michigan noncompete experiment, it is important to note that recent work by Barnett and Sichelman calls into question the validity of the initial work studying the effect of this change in Michigan law. Among other issues, such as the validity of the comparison group developed in Stuart and Sorenson,[155] Barnett and Sichelman report that the Michigan noncompete reversal was not applied retroactively, such that only new noncompetes signed after the change in the law were enforceable. They conclude,

> [i]f the true regime change (that is, taking into account both nominal and effective changes) took considerable time, a sizable portion of the results in these studies are unlikely to be causally linked to changes in noncompete law. Indeed, Marx et al. (2009) find the exact opposite of the effects one would expect from a gradual noncompete adoption post-MARA, stating that "the effect of the policy reversal remained strong for several years and then weakened, both in terms of the magnitude and statistical significance of the coefficient on the interaction variable."[156]

Marx, Singh, and Fleming examine out-of-state mobility as a result of the Michigan noncompete experiment.[157] Employing the same methodology as Marx, Strumsky, and Fleming, they show that after Michigan's noncompete reversal, the relative risk of post-MARA emigration from Michigan was twice as high among inventors with two patents as in states that continued to not enforce noncompetes (1.35 in Michigan versus 0.68 in non-enforcing states).[158] They further show that those with greater than median citations per patent prior to the policy reversal had a 186.8% higher risk of post-MARA emigration to non-enforcing states relative to the control states.[159] They also show that those inventors with more than the median number of patent co-inventors prior to the policy reversal were 236.3% more at risk for emigration out of state than their counterparts in non-enforcing states.[160] These results highlight that noncompete enforceability may contribute to a brain drain effect in which the most talented inventors leave the state for a lower enforceability state.[161]

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[155] *See* Barnett & Sichelman, *supra* note 133, at 15 (criticizing the Stuart & Sorenson and Garmaise enforcement scales are producing "spurious results").

[156] *Id.* at 22.

[157] Marx et al, *supra* note 131, at 394–95.

[158] *Id.* at 397.

[159] *Id.* at 402.

[160] *Id.*

[161] *Id.* at 403.

Garmaise examines how executive mobility, earnings, and firm investment in capital vary between high- and low-enforceability areas.[162] We focus first on his mobility and wage results and return later to his findings on firm investment in capital. He uses two empirical strategies to identify the impact of noncompete enforceability. First, he identifies changes in state laws in Florida (1996), Texas (1994), and Louisiana (2001) and examines how variation in outcomes changes before and after the laws relative to states without changes.[163] Second, Garmaise develops a new enforceability index to compare mobility and earnings patterns across states. In particular, he improves upon the index developed by Stuart and Sorenson by scoring 12 dimensions of noncompete enforceability for each state on a binary scale using Malsberger's treatises, adding up the 12 scores for each state.[164] Using this new index, he compares how the within-state mobility difference in high- versus low-concentration industries varies with the enforceability of the state. In order to attribute a causal interpretation to the cross-sectional estimates it must be that industries with low levels of in-state competition reflect the mobility that would have occurred in the high concentration industries in the absence of enforcement.

Both approaches yield relatively consistent results. In the longitudinal specification, Garmaise finds that a shift to an increased enforceability regime reduces within-industry transfers by 47% and reduces executive compensation growth by 8.2%.[165] The results from the cross-sectional specification are similar: a one-standard-deviation increase in the enforcement index reduces the arrival of within-industry transfers by 20.8% in higher versus lower concentration industries, and reduces the log of compensation by 1.2% of the mean.[166] Garmaise shows that there are no effects of noncompete enforceability on out-of-industry transfers.[167] These results are consistent, Garmaise argues, with a model in which noncompete enforceability deters executive effort.[168]

Starr focuses on how noncompete enforceability and how consideration-specific policies affect the provision of firm-sponsored training, wag-

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[162] *See* Garmaise, *supra* note 85.

[163] *Id.* at 390–91 (Section 4.2).

[164] *Id.* at 388–89 (Section 4.1).

[165] *Id.* at 397, 402.

[166] *Id.* at 399, 402.

[167] *Id.* at 398.

[168] *Id.* at 415 (examining A.2 Optimal Linear Production Contracts). ("These findings are consistent with a model that has the following three features: noncompetition agreements encourage firm investments in managerial human capital, the agreements discourage managerial investments in their own human capital, and managerial investments have a greater impact than firm investments.") *Id.* at 413–14.

es, and tenure.[169] He argues that the impact of noncompete enforceability on firm-sponsored training and tenure is likely to be positive, but the wage effects are ambiguous. If noncompete enforceability makes employees more likely to stay in the job and less likely to leave for competitors, then the returns to training are larger for the firm.[170] However, he notes that enforceability can constrain individuals from reaching firms in which their training is more productive, which decreases their willingness to seek out training opportunities.[171] To identify the impact of noncompete enforceability on training, wages, and tenure, Starr uses data from the U.S. Census's Survey of Income and Program Participation and categorizes occupations not found in noncompete litigation as a control group for those that are found in litigation.[172] He also develops a novel index of noncompete enforceability by performing factor analysis based on Bishara's quantification of seven dimensions of noncompete enforceability.[173] He finds that a complete transition from non-enforceability to maximal enforceability increases training by at least 13% for high litigation occupations, increases tenure by 10%, and reduces wages by 2.5%.[174]

Starr argues that not all noncompete policies that lead toward a higher likelihood of enforceability also lead to more training or lower wages. He notes that particular state laws that make noncompete enforcement contingent on the provision of additional consideration may increase training and wages.[175] By separately measuring consideration-specific laws apart from other noncompete policies, Starr shows that indeed firms provide more training and pay higher wages to employees in states that enforce noncompetes only when the employee receives additional consideration beyond continued employment.[176] Starr argues that this finding is consistent with a theoretical model in which consideration policies substitute for individual negotiation over noncompetes.[177]

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[169] Starr, *supra* note 132.

[170] *Id.* at 10 (focusing on the unilateral-firm-choice training model and the contractible training model).

[171] *Id.*

[172] *Id.* at 23 tbl. 3 ("Mapping SOC codes to Occupations in Noncompete Litigation"). The categorization of high- and low-litigation occupations comes from Peter J. Whitmore, Empirical Study, *A Statistical Analysis of Noncompetition Clauses in Employment Contracts*, 15 J. CORP. L. 483, 519–23 (1990), and Helen LaVan, *A Logit Model to Predict the Enforceability of Noncompete Agreements*, 12 EMP. RESP. & RTS. J. 219, 226–28 (2000).

[173] Bishara, *supra* note at 30.

[174] Starr, *supra* note 132, at 30 tbl.5 ("Baseline Training Results").

[175] *Id.* at 34 tbl. 7 ("Policy Options").

[176] *Id.* at 35.

[177] *Id.* ("To explain the differential effect of consideration laws, I argue that these laws substitute for the lack of negotiation over training and noncompetes . . . . In

A recent working paper by Starr, Ganco, and Campbell takes an employment lifecycle approach to how noncompete enforceability affects the management of business and technical employees.[178] By recognizing that mobility barriers are also hiring barriers, they theorize that noncompete enforceability changes both who is hired and subsequently how they are managed. In particular, they argue that it is very difficult to hire technical employees because they tend to come with both noncompetes and have skills that are highly specific to an industry. By contrast, it is easier to hire general business employees because they can be hired from noncompetitors without much loss of value. Hence, in higher enforceability states, employers will have to turn to new labor force entrants to hire technical employees, but this need not be the case for those business occupations whose skills are valuable across industries.

Once the employee is hired, however, the same noncompete-related barriers that made it difficult to hire can be used to the firm's advantage. Specifically, noncompete enforceability allows the firm to comfortably train the employee more while paying them less, and ultimately retain the employee for longer. The authors further predict that the effect will be stronger for technical employees relative to business employees because they have fewer opportunities outside their focal industry.

Using the same data and difference-in-difference design as the Starr paper discussed above, the authors find that increases in noncompete enforceability cause firms to hire less experienced technical workers, but more experienced business professionals. Once hired, however, increased enforceability results in lower wages and more training for both technical workers and general business workers (the effects are stronger for technical workers). Technical workers are retained for longer in higher enforceability states, while the effect of enforceability on retention for general business occupations is positive but not statistically significantly different from zero.[179]

c. Firm Capital Investment and Innovation

The impact of noncompete enforceability on innovation is theoretically ambiguous in the existing studies. On the one hand, noncompete enforceability provides incentives for firms to innovate because they can protect their innovation by preventing leakages to competitors. On the other hand, if noncompete enforceability reduces the flow of knowledge across firms by reducing employee mobility, or if noncompete enforcea-

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negotiating on behalf of the worker, these laws result in training outcomes that more closely resemble outcomes under the contractible training model.").

[178] *See* Evan Starr, Martin Ganco & Benjamin Campbell, *Redirect and Retain: Why and How Firms Capitalize on Noncompete Enforceability in Technical and Business Occupations* (Feb. 13, 2016), http://ssrn.com/abstract=2753720.

[179] *Id.* at 38 tbl. 2.

bility discourages individual employee effort, then firms may be less innovative in higher enforceability states.[180]

Using the cross-sectional and longitudinal variation in noncompete enforceability described above, Garmaise shows that increased enforceability reduces the log of capital expenditures per employee.[181] He contends that, combined with the negative effect on executive mobility and earnings, his results are consistent with enforceability reducing executive effort.[182] Samila and Sorenson, using the cross-sectional measure of enforceability from Garmaise and the measure from Stuart and Sorenson, find that high enforceability reduces the effectiveness of venture capital in creating patents.[183] Together these findings suggest that noncompete enforceability reduces firm investment and innovation.

Conti takes a slightly different approach from Garmaise and also Samila and Sorenson. He considers not whether noncompete enforceability affects the overall level of innovation, but the type of innovation.[184] In particular, he argues that the additional protection provided by noncompete enforceability allows firms to better appropriate any successful innovation and thus allows them to take risks that they would not otherwise take in lower enforceability states.[185] Using the same longitudinal variation in noncompete enforceability identified by Garmaise, he finds indeed that increased noncompete enforceability in Florida is associated with both increases in extreme successes (top 1% of forward patent citations) and extreme failures (zero forward citations). He finds the reverse in Texas, where noncompete enforceability declined in 1994.[186]

Belenzon and Schankerman examine how knowledge diffuses geographically from American universities.[187] They argue that knowledge generated by universities is less likely to be cited in states where individu-

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[180] See, for example, the discussion in Samila & Sorenson, *supra* note 9, at 425–28, and Garmaise, *supra* note 85, at 408.

[181] *See* Garmaise, *supra* note 85, at 409 tbl.7.

[182] *Id.* at 412 ("Firm Investment versus Managerial Investment").

[183] *See* Samila & Sorenson, *supra* note 9, at 432 tbl.3, 435 tbl.5.

[184] *See* Raffaele Conti, *Do Non-Competition Agreements Lead Firms to Pursue Risky R&D Projects?*, 35 STRATEGIC MGMT. J. 1230, 1231 (2014).

[185] *Id.* Professor Conti argues that:

> [M]obility-induced knowledge leakages imply that the firm shares its profits, but not its losses, from R&D with rivals. Therefore, a stronger enforcement of non-competes should make high-risk R&D projects relatively more valuable than low-risk ones, such that in regions in which non-competes are enforced more strictly, firms likely undertake R&D paths whose outcomes have a higher probability of being both extremely valuable (i.e., breakthroughs) and extremely poor (i.e., failures).

Id.

[186] *Id.* at 1239–40.

[187] *See* Belenzon & Schankerman, *supra* note 131, at 884.

als are less mobile.[188] Using the Michigan noncompete experiment as a mobility shock, they find that inventors in the same state as a particular university are substantially more likely to cite one of the university's patents than an inventor from outside the state, but that in states that enforce noncompetes this effect is dampened.[189]

Taken together, the literature on noncompete enforceability and innovation suggests that noncompete enforceability tends to reduce innovation, but it increases the riskiness of the innovations that firms pursue. Belenzon and Schankerman show that reduced employee mobility could be a mechanism driving these results, while Garmaise proposed that reduced executive effort is the culprit.

d. Anticipated Employee Mobility, Firm Acquisitions and Value

A recent stream of research examines how noncompete enforceability affects the value of firms and the acquisition of firms. These papers argue that limits on employee mobility, especially limits on employee mobility to competitors, increase the value of the company due to the reduced probability of sensitive information leaking to competitors.[190]

Younge and Marx examine how the Michigan noncompete experiment affected the valuation of firms, which they measured with *Tobin's q*—the physical value of the firm divided by the replacement value.[191] The study uses annual, firm-level data from Compustat for 1997 through 2006 for U.S.-listed manufacturing firms that were headquartered in Michigan or in the set of control states defined in Stuart and Sorenson.[192] The authors find that the ability to block employee mobility to competitors was associated with a 9.75% rise in Tobin's q.[193] This effect is larger in areas with greater competition, but is somewhat attenuated by the use of patent protection.[194]

Younge, Tong, and Fleming use the Michigan noncompete experiment to examine how anticipated employee mobility affects the likelihood that a firm will be acquired.[195] They show that after Michigan started enforcing noncompetes in 1985, firms were more likely to be a target for an acquisition, and even more so if they contained a higher proportion of knowledge workers.[196] Furthermore, firms in areas with high degrees of competition were also more likely to be the target of an acquisi-

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[188] *Id.* at 885.

[189] *Id.* at 900.

[190] *See generally* Younge & Marx, *supra* note 131; Younge et al., *supra* note 131.

[191] *See* Younge & Marx, *supra* note 131, at 13.

[192] *Id.* at 13.

[193] *Id.* at 31.

[194] *Id.* at 24.

[195] *See* Younge et al., *supra* note 131.

[196] *Id.* at 698–700.

tion, while firms protected by stronger IP-toughness regimes were less likely to be targeted.[197]

e. Articles Studying Regional Outcomes

Evidence on the aggregate effects of noncompete enforceability comes from Stuart and Sorenson, as well as Samila and Sorenson. As noted above, these studies find that noncompete enforceability mitigates the effect of venture capital and liquidity events on new firm foundings.[198] Samila and Sorenson further find that high enforceability reduces the effect of venture capital on aggregate employment and payroll.[199] Their results "imply that not only does the enforcement of noncompete agreements limit entrepreneurship, . . . but it also appears to *impede* innovation. We further find that regions as a whole benefit from an employee-friendly legal regime through greater employment."[200] The only other study focusing on regional outcomes is a working paper that examines the impact of noncompete enforceability on employment in the Temporary Help Services (THS) industry.[201]

II. WHAT IS MISSING? DISCUSSION OF THE GAPS IN THE EMPIRICAL RESEARCH FOR GOOD LEGAL POLICY

As the prior review exhaustively shows, there is a robust and growing literature studying the use and impacts of noncompetes. Most papers theorize ambiguous outcomes regarding the impacts of noncompetes or their enforceability, which are at the heart of the tension underlying noncompetes: that these agreements disadvantage employees to protect the firm. This ambiguity is what has contributed to the rich theoretical legal literature, and is why empirical work to disentangle such theories is so important. The literature review makes clear that studies with the actual use of noncompetes are limited by both their sample and the mechanisms that they can identify. Studies of noncompete enforceability, by contrast, are far more numerous and varied. From this work we learn that noncompetes tend to have negative impacts on entrepreneurship, mobil-

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[197] *Id.* at 702.

[198] *See supra* Part I.E.3.a.

[199] *See* Samila & Sorenson, *supra* note 9, at 435–36.

[200] *Id.* at 436 (footnote omitted).

[201] *See* William Cosmo Komiss, *Empirical Analysis of Restrictive Covenants and the Temporary Help Industry* (Oct. 18, 2008) (unpublished manuscript) (on file with authors). Komiss uses the Malsberger treatise to develop a catalogue of noncompete enforceability among the THS industry from the late 1970s until the mid-2000s. *Id.* at 4–6 (cataloging noncompete cases in the temporary help industry cited in Malsberger). Using longitudinal variation to measure state enforcement of noncompetes against THS workers, he finds that enforceability is associated with a 10% decrease in THS employment. *Id.* at 11.

ity, wages, and innovation. Simultaneously, however, noncompete enforceability is also related to increases in firm-sponsored training, riskier R&D investments, and increases in firm value and the likelihood of acquisition. Though it is tempting to think that the rapidly expanding empirical noncompete literature has sufficiently answered the interesting and relevant questions for firms, workers, and policymakers, we argue in this section that there remain severe limitations to our understanding of noncompetes.

We organize this Part in parallel to the prior review of literature. We first discuss what is missing from our understanding of the use and consequences of noncompetes. We argue that the most fundamental questions about noncompetes remain unanswered. Who signs noncompetes? How do they affect the mobility and earnings of workers? How do they affect entrepreneurship? How does the use of noncompetes impact firm investment in R&D and employee skills? We next scrutinize the empirical work examining the impact of noncompete enforceability. We argue that without data on who actually signs noncompetes, it is not only difficult to identify the effect of enforceability but the scope of the studies are also seriously limited. Perhaps most importantly, we argue that relying on studies of noncompete enforceability to identify the impact of noncompetes is risky: even in the absence of enforceability, noncompetes may themselves chill employee mobility or have other consequences. As a result, studies of noncompete enforceability are likely to mask the true impact of noncompetes.

A. What Is Missing in Our Understanding of the Use and Consequences of Noncompetes for Workers and Firms?

Given the lack of empirical work using actual data on noncompete usage, it is safe to say that we know relatively little about the uses and consequences of noncompetes. From the 9 studies that have employee or firm-level data on the use of noncompetes, we learn that 3 out of 4 executives, and almost one in two physicians and engineers sign noncompetes.[202] We also learn that one in two firms use noncompetes. However, since these studies do not describe which employees sign noncompetes, it is unclear whether these numbers provide any additional information, given the proportion of executives that sign. While we might be able to extrapolate that noncompetes are similarly common in similarly high skilled occupations, it is difficult to extrapolate to the other 99.1% of the U.S. labor force. This lack of data on the use of noncompetes itself suggests, at the most basic level, that what is missing from the literature is an

[202] *See supra* Part I.E.1; s*ee also* Garmaise, *supra* note 85, at 396; Lavetti et al., *supra* note 85, at 31; Marx, *supra* note 85, at 702; and Schwab & Thomas, *supra* note 85, at 234.

understanding of what types of firms use noncompetes, what types of workers sign noncompetes, what the conditions of the noncompete are, and why and when such noncompetes are used.

Understanding who signs, however, is only the first rung in the ladder of what is missing in the literature. It is necessary to know who signs noncompetes in order to understand how noncompetes affect the mobility and earnings of workers. Without such data, speculation about the possible effects of noncompetes is endless. The two articles attempting to understand how noncompetes affect earnings and mobility of workers provide some clarity, but contrast in striking ways. The combined evidence suggests that firms sometimes manage the noncompete-signing process to reduce worker bargaining power, but conversely that noncompetes may come with stronger incentives and may increase wages and revenues.[203] Furthermore, noncompetes are associated with both career detours and extended tenures.[204] What is missing from this strand of research is an understanding of exactly why and how noncompetes have such differential effects, and whether these effects are likely to hold in less skilled occupations or less narrowly defined occupations. For example, to what extent are individuals who sign noncompetes staying longer in their jobs because they feel locked in, and to what extent do they choose to stay at the firm voluntarily, perhaps because of higher wages or internal promotion? Do individuals who negotiate or are given a chance to consider the noncompete before signing exhibit different mobility and earnings outcomes?

Aside from questions related to mobility, there is no literature examining the use of noncompetes and entrepreneurial outcomes. Entrepreneurship is far riskier than moves to employers, and often involves collaborations among founding team members with complementary skills. Hence noncompetes may pose an even greater threat to future of entrepreneurship than they do to employee mobility.

Similarly, empirical work on noncompetes within the firm is entirely nonexistent. This omission from the literature is particularly glaring given that the sole reason courts begrudgingly enforce noncompetes is that CNCs protect legitimate business interests. Hence, what is missing from the literature is an understanding of how noncompetes affect the innovative activity, such as R&D investment, investment in employee skill development, and profitability of the firm. Relatedly, for states considering whether they should make the use of noncompetes illegal, it is important to know if firms need the protection of noncompetes to invest in innovative activities or if they simply substitute other protection methods (pa-

[203] *See* Marx, *supra* note 85, at 702–06 (discussing career detours and firms delaying the offering of noncompetes); *see* Lavetti et al., *supra* note 85 at 27.

[204] *See* Marx, *supra* note 85, at 702–05; *see also* Lavetti et al., *supra* note 85 at 27.

tents, nondisclosure agreements)—or are firms adaptable to high-velocity labor markets in the absence of CNCs like in the Silicon Valley Model?

Lastly, without data on who signs noncompetes the literature currently cannot identify the mechanisms linking noncompetes to behavioral outcomes. For example, if an employee receives an outside offer from a competitor, does the employer typically threaten the employee with his noncompete? Does the employer raise the employee's wage, despite the noncompete, or promote the employee? When an employee violates the noncompete, under what circumstances does the employer threaten the employee with a lawsuit? Answers to these questions may help researchers understand some of the mechanisms underlying any effects of noncompetes on worker and firm outcomes.

B. What Is Missing in Our Understanding of the Impacts of Noncompete Enforceability?

The papers examining the impacts of state noncompete policies on workers, firms, and regions make up the bulk of the empirical noncompete literature. Broadly, these studies come in three varieties: (1) Cross-sectional approaches, in which high-enforceability states are compared to low enforceability states; (2) Longitudinal approaches, comparing the within state change (relative to a group of control states) before and after a noncompete policy change; and (3) Moderation approaches, whereby noncompete enforceability is shown to moderate the relationship between two other variables. Each approach has its own unique limitations and assumptions in order to identify the causal effect of noncompete enforceability. Instead of focusing on any particular shortcoming of any individual study, we argue that the primary limitation of all of these studies is that they do not have data on who actually signs noncompetes. As a result, they must take a necessarily aggregated perspective, averaging the effect of noncompete policies across those who sign and those who do not sign noncompetes. This aggregation raises a number of concerns related to the validity and scope of these studies.

First, as the recent Barnett and Sichelman study has pointed out, it is important to note that identifying the causal effects of noncompete enforceability is a challenging task. Cross-sectional studies must somehow disentangle the effect of noncompete policies across states from the myriad of other potential state policies or state differences that are correlated with noncompete policies. Similarly, studies that examine the before and after effects of a noncompete policy change within a state must separately identify the impact of the noncompete laws from other trends or state-level changes that might be occurring simultaneously. These are challenging identification issues to overcome, especially given that very few states have significantly changed their noncompete policies in the last 30 years. If research could show that those who sign noncompetes are driv-

ing any effects found from increases in noncompete enforceability, this would help to allay serious identification concerns.

Second, since not all policy changes equally affect the noncompete-signing population, the measurement of noncompete enforceability is necessarily error-ridden without data on who signs noncompetes. For example, if a state changes the law to allow firms to enforce noncompetes even when workers are fired from their jobs—but no workers who sign noncompetes are actually fired both before and after the change—then such a policy change will be picked up by the enforceability indexes of Garmaise, Bishara, and Starr, but it will not change the *effective* probability of noncompete enforcement. In order to properly calculate noncompete enforceability—that is, the probability that a randomly selected individual's noncompete would be enforced in court if the randomly selected individual were to violate the noncompete and his firm were to sue him—one needs two key pieces of information:

> i) The identification of situations in which a state court will and will not enforce a noncompete; and
>
> ii) The probability that a randomly chosen employee has the characteristics defined in i) for enforcement in that state.

Without any information on ii), the existing measures of noncompete enforceability use only i) when creating their measure of enforceability.[205] As a result, when estimating the effects of noncompete enforceability on worker or firm outcomes, the best possible outcome is that the effects of enforceability are attenuated. At worst, the measurement error is correlated with enforceability (e.g., higher enforceability states are more likely to be mismeasured) and causes us to reach biased estimates.

Third, because enforceability is the key variable, not noncompete-signing status, assumptions about knowledge of noncompete policies among the various actors must be made. While it might be reasonable to believe that firms have a good grasp of noncompete policies within and across states, it may be less reasonable to expect employees to have perfect information. Whether and to what extent these assumptions are reasonable is impossible to test without individual-level data on what workers believe. Furthermore, if workers are uninformed about their state's policy, then noncompetes may chill employee mobility and entrepreneurship, regardless of their actual enforceability. As a result, studies relying on states' variation in noncompete enforceability may seriously underestimate the impact of noncompetes. Also noncompetes' impact could also be benign if workers are unconcerned about their enforceability or even forget that they signed a restriction.

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[205] The measure of noncompete enforceability in Stuart & Sorenson, *supra* note 9, is binary and thus only captures changes from enforceable to non-enforceable.

Fourth, analyses comparing outcomes in high-enforceability versus low-enforceability states cannot disentangle the impact of the potentially increased use of noncompetes in higher-enforceability states from the impact of the noncompete policy on those who do and do not sign noncompetes. For example, if it is found that there is more mobility in California relative to a high-enforceability state like Florida, it could be that individuals who sign noncompetes in Florida are less mobile within their own state at least than those who sign noncompetes in California. But it might well be the case that firms in Florida use noncompetes more frequently, and that noncompetes themselves, regardless of their enforceability, reduce employee mobility. To put it another way, any observed effects of enforceability could be explained by a greater use of noncomnoncompetes in high-enforceability states, not necessarily the impact of enforceability on those who sign.

Relatedly, and perhaps most importantly, noncompetes themselves may deter individuals from leaving their jobs or starting a competing enterprise—known as the in terrorem or chilling effect. The fact that the contract itself may deter employees from moving, regardless of the enforceability level, suggests that the effect of noncompete enforceability likely masks the true impact of noncompetes. For example, if we were to observe that noncompete enforceability was associated with a small difference in employee mobility, this result does not necessarily imply that noncompetes have a small effect on mobility. Noncompetes may reduce mobility similarly in high- and low-enforceability states, which would cause us to observe little difference in mobility across high- and low-enforceability states. Thus, while enforceability studies may show no, small, or large impacts, it could be that noncompetes themselves have enormous effects which are being masked at the aggregate level. This critique is particularly relevant for state courts that may want to discourage or encourage the use of noncompetes, as opposed to tweaking the circumstances under which they are enforced.

Fifth, the aggregate perspective cannot directly identify the potential micro-mechanisms at work, and thus limits the potential policy options. For example, how exactly might noncompete enforceability reduce mobility? Is it that individuals who sign noncompetes search less frequently for jobs in high-enforceability states? Is it that firms are less willing or less able to hire them in high-enforceability states? Is it that the worker's employer is more likely to threaten him or her in high-enforceability states? Is it that employees are uninformed and are thus more susceptible to threats even in states that do not enforce noncompetes? Depending on which mechanisms are operative, policies can be constructed to target these mechanisms to reach the desired result. For example, information-based policies meant to inform individuals when their noncompete is and is not enforceable may reduce the chilling effect of unenforceable noncompetes.

Sixth, without data on who signs noncompetes, it is impossible to determine if there are external effects of noncompete policies on those who do not sign. Such effects could arise through the cycle of hiring generated by an initial move from one employer to another, a phenomenon known as a "vacancy chain."[206] For example, in the papers on employee mobility and training, increased noncompete enforceability may reduce employee mobility for those who sign noncompetes, which may in turn reduce the mobility of those who have not signed since fewer jobs are becoming available. As a result of such decreased mobility among signers and nonsigners, firms in high-enforceability states may have increased incentives to train both employees who sign and those who do not.

Seventh, most studies of noncompete enforceability create aggregate measures of enforceability and do not consider subtle differences in the law. The exception is Starr, who breaks out consideration aspects of noncompete laws apart from other dimensions of enforceability.[207] As a result, in most empirical studies, we learn only that more enforceability is good or bad, but there is no guidance on exactly which enforceability policies might be well-suited to achieve various goals. Using data on who signs noncompetes, and tying behavior of noncompete signers to specific noncompete policies, such as additional consideration requirements and negotiation, has the potential to better help courts and state legislatures identify the ways in which they might consider changing their enforceability policies.

While the analyses studying noncompete enforceability have improved our understanding of the impact of noncompete policies, without data on who signs noncompetes the extent of the analyses are necessarily limited, the policy options cannot target specific mechanisms, and there are a number of reasons to be wary of the validity of the results.

We have identified here how data on the use of noncompetes could be utilized to significantly strengthen and broaden our understanding of the impacts of noncompetes and enforcement policies. Next in Part III we use this understanding to identify what additional research would be useful to help complete the still unclear noncompete picture.

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[206] When an employee leaves a job, she likely creates a vacancy at her former employer. That vacancy can be filled by an employee at another firm or by either an unemployed individual or a new entrant to the labor market. If the job vacancy is filled by an employee from another firm, then it creates another vacancy at the other firm. The vacancy chain continues until eventually an unemployed or new labor-force participant is hired.

[207] *See supra* notes 169–177 and accompanying text for a discussion of Starr.

III. MOVING FORWARD: A RESEARCH PLAN FOR COMPLETING THE NONCOMPETE PICTURE

The growing body of empirical research described in the previous Part is essential to better understanding the role of noncompetes in the United States. Yet without the full picture of noncompete use within and across firms, better measures of enforceability, worker perceptions, and employer motivations, policymakers are still largely in the dark about what reforms, if any, are needed. In this Part we not only call for more empirical research on the subject of noncompetes and other restrictive covenants, we also point out specific examples of studies and methods that may help answer a range of lingering questions about this area of human-capital law and policy.

This suggested research agenda is not merely for academic purposes, but rather it can potentially help business interests, employees, and public advocates make better assessments of the ways to maximize the positive impacts of noncompetes while curtailing some potential negative outcomes that have begun to be discussed in the empirical literature. These include outcomes related to entrepreneurship, employee mobility, wages, training investment, capital investments, innovation, firm value, and the much-discussed topic of regional competitiveness. To begin, we will outline some possible data-collection opportunities for researchers. Then, we end with a section explaining why these new opportunities for research are crucial for the evaluation of many of the proposed policy changes.

A. Opportunities for Gathering More Empirical Evidence

As the discussion in Part II indicates, there is a wealth of information that has been learned about covenants not to compete in recent years. However, there are also many questions left unanswered by the existing empirical literature on CNCs. The most glaring shortcoming is the inability of the empirical literature to answer even the most basic questions about the use and impacts of noncompetes across workers and firms. The evidence from state laws is riddled with measurement error, which make it difficult to be confident in what we learn. Evidence on the use of noncompetes, coupled with careful empirical analysis, is the most promising way to provide more convincing evidence on the use and impacts of noncompetes.

There are numerous outlets to collect such data. The ideal dataset would track workers and firms over time. For workers, a few such datasets are the Survey of Income and Program Participation, the Current Population Survey, the Panel Study of Income Dynamics, and the National Longitudinal Survey of Youth. These studies already involve significant panel dimensions and adding questions about noncompete signing would provide significant value, even if the questions were only on the

surveys for a short time. Alternatively, cross-sectional surveys of workers may be able to provide some answers to these questions, but can only provide retrospective and prospective answers to mobility and entrepreneurship questions. It is far more convincing to observe actual moves between employers.

Given that these outlets are unlikely to add many survey questions due to time constraints of respondents, researchers might consider using other surveying outlets. With the rise and improvement in online surveying technology, such as that used by Qualtrics, ClearVoice, SSI, USamp, Survey Monkey, and many others, the possibility of collecting detailed worker level data is widely and cheaply available.[208] One caution about using online surveys is to be very careful about the sample selection process. This is because individuals who sign up to take online surveys are not a random sample of the U.S. population. Thus, careful cleaning and reweighting should be employed to ensure that the results are as representative as possible.[209]

Another similar possibility is partnering with industry and trade group associations, which may be willing to provide access to their membership. The benefit for those organizations is that they may be able to better assess the use of restrictive covenants among their members or in their industry, and use that information for policy decisions, including how they support or oppose new attempts to reform the existing law of noncompetes.[210] However, their membership may also not be fully representative of individuals in the industry or job categories of interest to researchers.

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[208] For instance, research conducted by the authors has included some of these data-gathering partners with some success, including the over 11,500 respondents to a recent survey. *See* Evan Starr, Norman Bishara & J.J. Prescott, *Noncompetes in the U.S. Labor Force* 11 (June 25, 2015) (unpublished manuscript), http://ssrn.com/abstract=2625714; s*ee also*, Evan Starr, J.J. Prescott & Norman Bishara, *Noncompetes and Employee Mobility* (unpublished manuscript), http://ssrn.com/abstract=2743844.

[209] *Id.* at 12–14 (applying this methodology of sampling and analysis); *see also* J.J. Prescott, Norman D. Bishara & Evan Starr, *Understanding Noncompetition Agreements: The 2004 Noncompete Survey Project*, 2016 MICH. ST. L. REV. (forthcoming, 2016).

[210] Interestingly, to date there is only limited evidence of industry mobilization around the issue of noncompete enforcement. For instance, some medical associations have positions opposing noncompetes. *See, e.g.*, Larry D. Weiss, *AAEM White Paper on Restrictive Covenants: A Policy Paper of the American Academy of Emergency Medicine*, 30 J. EMERGENCY MED. 473 (2006), http://www.aaem.org/em-resources/position-statements/practice-rights/restrictive-covenants. Some states have disallowed noncompetes for doctors; however, there has seemingly not been widespread lobbying against noncompetes by medical associations. One explanation may be that one's perspective on noncompetes changes depending on status and standing in the industry, such that new doctors will resist noncompetes, but established doctors embedded in a practice they intend to stay with or manage are more likely to see some benefit from these agreements.

Beyond conducting research to enhance our understanding of the use and perception of restrictive covenants by individual employees and former employees, it is also important to understand the firm's perspective, as well. However, gathering data on the use of noncompetes among firms is a more challenging endeavor than reaching individual respondents, because it necessarily entails asking questions about which workers at the firm sign noncompetes. This implicates sometimes sensitive issues of identifying workers or raises concerns about proprietary information about the firm's private contracts and policies.

There are also possible options to encourage firms to collect useful data on noncompete use in the United States. One such option is the Occupational Employment Survey at the Bureau of Labor Statistics.[211] This is a survey of non-farm establishments in which significant data on the occupation and industry are already collected. By noting the Employer Identification Number and which occupations within the firm are asked to sign noncompetes, it is possible to link the firm to other data on firm level outcomes (employment, payroll, etc.). Another option is to add questions about the use of noncompetes to the Census of Manufacturers or to the Business R&D and Innovation Survey (BRDIS) run by the NSF and the Census Bureau.[212] Still another option is to outsource the collection of data to Glassdoor.com and other websites committed to providing potential employees with transparent information on the salaries and other job characteristics. This approach in particular would allow researchers to connect the use of noncompetes and other restrictive covenants with the firm's name, industry location, and other publicly available data.

B. Harnessing the Additional Evidence for Better Legal and Policy Outcomes

A better understanding about the prevalence, use, and impact of restrictive covenants in the United States is not an end in itself. The need for better technical data on noncompetes for researchers is clear, but what are some of the implications of this for the public debate over noncompetes?

The lack of research producing data about a wider array of workers than high-tech workers and engineers, doctors, and executives is a signif-

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[211] For an overview of the Occupational Employment Statistics (OES) program, see *Occupational Employment Statistics Overview*, BUREAU LAB. STAT., http://www.bls.gov/oes/oes_emp.htm.

[212] For an overview of the Annual Survey of Manufacturers (ASM), see *Annual Survey of Manufacturers About the Surveys*, U.S. CENSUS BUREAU, https://www.census.gov/manufacturing/asm/about_the_surveys/index.html. For an overview of the Business R&D and Innovation Survey (BRDIS), see *Business R&D and Innovation Survey*, NAT'L SCI. FOUND., http://www.nsf.gov/statistics/srvyindustry/about/brdis/.

icant problem. In particular, more research on a range of employees from a range of important industries—and particularly low-wage workers—is key to addressing concerns that arise related to noncompetes. Much of the public debate centers on two issues: incentivizing innovation and the fairness issues involved with enforcing noncompete agreements against workers, especially younger, low-wage, and low-status workers without many other opportunities if they are unable to compete in the same industry after leaving a job.

On the innovation front, policymakers are interested in whether and how noncompete enforceability affects the development of high-tech clusters like Silicon Valley. The Gilson hypothesis and subsequent empirical work suggest that noncompete enforceability reduces the mobility of inventors, drives them away from the state, and both reduces innovation overall and increases the riskiness of the innovation pursued. As a result of this work, there has been political movement towards banning noncompetes to encourage innovation. As discussed previously, Hawaii, for example, recently passed legislation to ban noncompetes for technical workers.

As we highlight in the prior section, we are concerned that such policy changes are being made without the proper empirical foundation. Indeed, there is not one study examining the relationship between the use of noncompetes and firm-level or regional-innovation outcomes. Furthermore, the recent work by Barnett and Sichelman paper casts significant doubt on the validity of the studies using the Michigan noncompete reversal and studies that use the Stuart and Sorenson index. We propose that collecting data on the use of noncompetes and related restrictive covenants as well as other firm-level innovation outcomes is a first step towards building this foundation. A second step would then be to examine how firms that use noncompetes differ from firms that do not use noncompetes in both high- and low-enforcing states. Future studies could then aggregate this data to the regional level to help policymakers assess the ways in which noncompetes and noncompete enforceability help or hurt the creation of technical clusters.

Regarding the fairness issues, the concern has been that there is a regressive impact of noncompete agreements that harms the mobility, and thus career advancement, of low-wage workers, especially if there is an in terrorem effect that discourages them from leaving because of a fear of facing a lawsuit over their noncompete. While these concerns have some intuitive merit, especially on fairness if not efficiency grounds, there is not any empirical research that addresses this segment of the American workforce.[213] Yet, protecting these types of workers from the oppressive terms of overreaching employers has been the rallying cry for

[213] *See* Prescott, Bishara & Starr, *supra* note 209.

some of the high-profile lawsuits and media reports described above in Part I. For instance, the newly proposed federal legislation, the MOVE Act, is expressly aimed at protecting low-wage workers.[214] In addition, a recent Treasury Department report on noncompetes acknowledged the concerns of low-wage workers, but also discusses the positive role of noncompetes in some instances, especially for firms and investments in workers.[215]

Certainly fairness concerns and worker protections matter greatly. However, if a main justification for the new batch of noncompete reform statutes is low-wage worker protection, then it would be useful to have more data on what impact noncompetes have on these workers. It would also be useful to simply know how many noncompetes are actually signed by this population and how they are enforced, if at all. This gap in the research is particularly pronounced when it comes to the evidence of an in terrorem effect that chills worker mobility and advancement. There is also a lack of data on how restrictions impact younger workers, workers in all jurisdictions, or workers across a range of demographics, including by gender, race, age, education, skills, and experience.

A related policy issue that arises out of the research gaps identified in our Article is that we know surprisingly little about how firms use noncompetes. Many foundational issues remain unaddressed, including questions of how many firms use noncompetes, and if they use them for certain workers or for many types of workers. Also, we have only anecdotal and mostly secondary evidence from judicial opinions and a limited set of employee interviews about what motivates employers to use noncompetes. We also do not know much about who at the firm determines when to deploy noncompetes—and what restrictions are used and why—and importantly how a firm decides to take action to enforce an agreement against a departing employee. Embedded in that decision will likely be clues to issues related to how firms view their protectable interests in employee knowledge, trade secrets, and client relationships that will also provide a deeper understanding of the issues for judges faced with resolving disputes over knowledge assets ownership.

The answers to these questions are important to understanding the business value of noncompetes to firms and to potentially rebalance the public debate from one of fairness to one viewing the overall mix of costs and benefits involved. Like the dearth of research on noncompetes' impact on low-wage or early tenure workers, a lack of data on the firms' perspective means that researchers and policymakers do not have the full noncompete picture before them.

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[214] *See* Press Release, Office of Sen. Chris Murphy, *supra* note 2.

[215] *See* TREASURY REPORT, *supra* note 8, at 8.

Finally, because these missing pieces of information about employees and firms leave many open questions about noncompetes, there are numerous implications for policymakers. State legislators in many jurisdictions are being lobbied to change or preserve the status quo with noncompete enforcement and are on the front lines of the noncompete debate.[216] Moreover, in light of all of the research gaps, we have identified some of the most sweeping policy changes being proposed or actually being enacted at the states are based on somewhat incomplete information. Many intuitions about the regressive nature of noncompetes or the implications for innovation may be correct—for example that their enforcement or even their mere presence in contracts of low-wage workers may chill mobility. However, it is currently unclear if those intuitions are accurate because they have yet to be tested empirically. While it is impossible to have complete information about how noncompetes are used, the data we have currently is woefully inadequate and more research is needed to reach meaningful conclusions about reforms. Unless we have a fuller picture of the impact of firms' use of noncompetes—as well as the impact of new policy solutions such as notice periods, professional carve outs, or wage thresholds—there remains a risk that policy proscriptions may have unintended negative consequences or be aimed at problems that are not significant enough to garner actual policy solutions.

CONCLUSION

Despite the very long history of restrictive covenants and the contentious, long-term debate over the propriety of post-employment covenants not to compete, the controversy over these legal mechanisms has grown recently. This is due, in part, to scholars' research in this area and an increase in media and public attention focused on recent revelations of some high-profile potential abuses of these long-tolerated legal tools. At the same time this new level of academic attention and debate has helped precipitate further discussion, and, in some jurisdictions, action from policymakers. The most obvious outcome in this regard has been that numerous jurisdictions have adopted restrictions on the use and enforcement of noncompetes, or, at a minimum, have modified the reasonableness test and removed discretion from the courts on issues such as the per se reasonableness of the length of the agreements.

However, the recent focus on noncompete agreements has also pressured legislators into making hasty reforms, thus risking poor public-policy decisions even more acutely than in the past. Such reforms may be necessary, but they should not be made lightly or without a full under-

[216] *See, e.g.*, Benard, *supra* note 54 (discussing the Utah compromise between pro-worker and innovation interests and various business interests); Romboy, *supra* note 54 (same).

standing of the costs and benefits of allowing, banning, or modifying a state's noncompete policy. In fact, these decisions should be well supported by both an understanding of the actual legal enforcement picture, as well as nuanced, sophisticated, methodologically sound, and impartial legal and empirical research.

Our Article has discussed the legal background of modern-day enforcement, as well as all of the empirical research that examines noncompetes. In doing so we have provided a complete picture of the questions that have been addressed and answered, although much of the research is subject to criticism. Beyond just pointing out some of the virtues and limitations of the existing body of empirical research touching on noncompetes, we have gone further and proposed a cohesive roadmap for future scholarship that will support reasoned and appropriate policymaking. Seeing this extensive and holistic view of the subject is key to understanding how a more substantial body of research is essential to addressing the debate underlying calls for policy reactions and various reforms to noncompete policies around the United States.

Delaware Division of Corporations: 2021 Annual Report

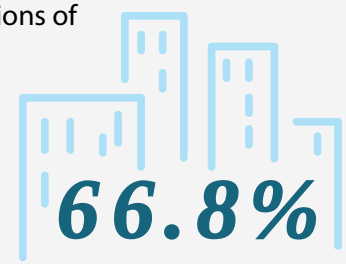
A Message from the Secretary of State Jeffrey W. Bullock

As the First State recovers from the COVID-19 pandemic, Delaware's Corporate Franchise continues to **experience unprecedented growth.**

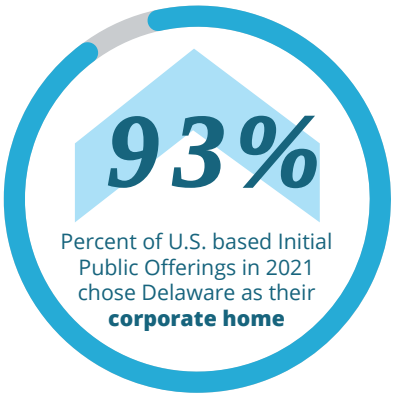
In 2021, we added more than **336,407 business entities** throughout the franchise. Total business entities topped **1.8 million** at year-end with **36.9 percent growth** in new formations of LLCs and **20.8 percent** in the number of new corporations added.

Consistent with past years, the First State continues to be the domicile of choice for members of the **Fortune 500 at nearly 66.8 percent.**

Approximately **93 percent** of all U.S. initial public offerings in the calendar year 2021 were registered in Delaware.



12.9%
Total General Fund Revenue growth for FY 2021



336,407
Total Business Entity Formations in 2021

Business owners and investors have the discretion to select any jurisdiction as their legal home for their business entity. However, they consistently choose Delaware for **four main reasons:**

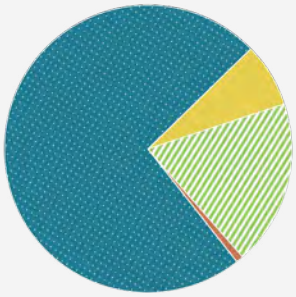
1. **Delaware's General Corporation Law** is widely regarded as the **most advanced and flexible** business formation statute in the nation.

2. **The Court of Chancery is a unique**, **centuries-old business court** that has written most of the modern U.S. corporation case law.

3. Delaware's corporate and legal services community has **unparalleled expertise in the application of Delaware Corporate Law** and receives strong, bipartisan support from the Delaware General Assembly and our Governor for its efforts to continuously improve the state's laws.

4. The Delaware Division of Corporations provides **prompt, friendly, and professional service** and strives to continually improve based on what we hear from our customers.

Total Business Entity Formations in 2021



73.4% LLCs (247,003)

18.6% Corporations (62,510)

7.3% LPs/LLPs (24,588)

0.7% Statutory Trust (2,306)

2020

72.3% LLCs (180,376)
20.7% Corporations (51,747)
6.2% LPs/LLPs (15,348)
0.8% Statutory Trust (1,956)

2019

73.2% LLCs (165,910)
20.0% Corporations (45,405)
6.0% LPs/LLPs (13,513)
0.8% Statutory Trust (1,761)

Business Entity Formations CY 2019-2021 Totals

	2019 CY	2020 CY	2021 CY
LLCs	165,910	180,376	247,003
Corporations	45,405	51,747	62,510
LPs/LLPs	13,513	15,348	24,588
Statutory Trusts	1,761	1,956	2,306
Totals	226,589	249,427	336,407

To continue making Delaware the premiere destination to incorporate, it's important to ensure that **our laws are optimal for engaging in ethical and profitable business.** During my term as Delaware's Secretary of State, I have served on the National Association of Secretaries of States' Executive Board several times.

I have also chaired the organization's Business Services Committee—the committee that spearheads initiatives on best state practices regarding corporate registrations, electronic business filings, and other related services—in an effort to lead in the development of common-sense regulations at the federal and state levels.

Total Number of Entities in Delaware

	2019 CY	2020 CY	2021 CY
LLCs	1,035,872	1,109,736	1,291,911
Corporations	325,174	336,270	370,404
LPs/LLPs	109,176	117,430	137,188
Statutory Trusts	22,763	23,829	25,547
Total Business Entities	1,492,985	1,587,265	1,825,050

Division of Corporations General Fund Revenue

	2019 FY	2020 FY	2021 FY
Net Business Entity Taxes	1,228.2	1,302.8	1,467.8
Business Entity Fees	123.0	127.0	148.3
UCC Fees	25.2	25.6	26.7
Total Revenue	1,376.4	1,455.4	1,642.8

Total Uniform Commercial Code (UCC) Transactions CY 2019 - 2021

	2019 CY	2020 CY	2021 CY
UCC 1	133,187	128,104	151,103
UCC 3	147,419	144,073	168,124
Searches	249,263	247,459	313,459
Total Transactions	529,869	519,636	632,686

It has been my honor and privilege these past **13 years as Secretary of State** to be able to promote the great state of Delaware in many ways, including domestically and abroad. You have my commitment to maintaining this long tradition of ensuring that the Division of Corporations works hard every day to meet the needs of our customers as well as provide them with the best experience possible.

Sincerely,
Jeffrey W. Bullock

Jeffrey W. Bullock serves as the 80th Secretary of State and oversees the nearly two dozen agencies, including the Division of Corporations, at the Delaware Department of State.

Non-Compete Agreements: A Review of the Literature

John M. McAdams[1]
Federal Trade Commission

December 31, 2019

Abstract

Non-compete agreements (NCAs) are employment contracts that limit the post-employment options of workers. On the one hand, they potentially solve an investment hold-up problem, allowing firms to make mutually beneficial investments in workers. On the other hand, the agreements potentially erode workers' future bargaining position by limiting their outside options. In this paper, we review the economic literature on non-compete agreements in the U.S.

Keywords: Non-compete agreements, worker mobility, training, investment holdup

JEL codes: J2, J6, K3, L4, M5

[1] Email: jmcadams@ftc.gov. The views expressed in this article are those of the author and do not necessarily reflect those of the Federal Trade Commission or any individual commissioner. I would like to thank Dan Hosken, Dave Schmidt, and Brett Wendling for helpful comments. All errors are my own.

I. Introduction

Non-compete agreements (NCAs) are employment contracts that limit the ability of an employee to join or start a competing firm after a job separation. The past decade has seen burgeoning interest from academics, policymakers, and the media over non-compete agreements—partly due to concern over whether labor markets have been becoming less competitive, and partly due to several high-profile examples of non-competes involving low-skilled occupations such as sandwich makers, dog walkers, and warehouse workers.[2]

This interest has spurred several state enforcement actions and legislative proposals to limit the perceived harm that non-competes cause.[3] For example, Oregon, Massachusetts, and Washington have passed laws in recent years rendering non-competes unenforceable against low-wage workers. As their very name might suggest, non-compete agreements have also drawn the attention of competition authorities. For instance, the Chairman of the Federal Trade Commission has stated the agency is considering issuing a rule to limit the use of non-compete agreements.[4] This is part of a broader push by the U.S. competition agencies to address competition issues in labor markets.[5] Alongside the increased attention from policymakers and legislators has been a flurry of economic research into non-compete agreements and their effects on labor and product markets. Reviewing this economic literature is the purpose of this paper.

States vary considerably in their legal enforcement of non-compete agreements between employers and workers.[6] Several states do not enforce non-competes at all, or do not enforce them for certain classes of workers.[7] Most states, though, will enforce non-compete agreements to a certain extent. The relative strictness of a state's enforceability regime depends on a number of dimensions. This includes whether the agreements can be enforced for both voluntary and involuntary separations, or only voluntary ones; whether employers must provide additional consideration beyond the job itself to the employee for signing the agreement; whether the firm has a sufficient "protectable interest" to motivate the use of a non-compete; and how the state courts treat agreements that contain provisions which are invalid according to state law.[8] For

[2] On the latter point, see Jamieson, Dave, "Jimmy John's Makes Low-Wage Workers Sign `Oppressive' Noncompete Agreements," *Huffington Post* (Oct. 13, 2014); Jamieson, Dave, "Doggy Day Care Chain Makes Pet Sitters Sign Noncompetes To Protect 'Trade Secrets'," *Huffington Post* (Nov. 24, 2014); and Woodman, Spencer, "Amazon makes even temporary warehouse workers sign 18-month non-competes," *The Verge* (Mar. 26, 2015).
[3] See Johnson and Lipsitz (2018) for a discussion of some recent legislative proposals. President Obama, in 2016, also issued a "State Call to Action on Non-Compete Agreements" making several proposals.
[4] Parts, Spencer, "Simons: Non-Compete Rulemaking May Come Soon," *Global Competition Review* (May 8, 2019).
[5] Remaly, Ben, and Kaela Coote-Stemmermann, "FTC Considers Workers in Deal Reviews," *Global Competition Review* (Oct. 4, 2018).
[6] States themselves do not "enforce" non-compete agreements directly; it is private employers who do. We follow the economic literature in using the terms "enforce" and "enforceability" to reflect whether a state would uphold a non-compete if an employer attempted to enforce one through the courts.
[7] California and North Dakota do not enforce non-competes at all. Other states do not enforce them for specific groups such as technology workers (Hawaii), low-wage workers (Oregon and Washington), and health care workers (various states). Within the legal sector, non-competes are generally not enforceable in any state.
[8] A non-compete agreement which contains an invalid provision can be nullified completely ("red-pencil doctrine"), the invalid provision can be deleted while keeping the rest of the agreement intact ("blue-pencil doctrine"), or the

convenience, researchers often combine the various dimensions of enforceability into a single index. California and North Dakota, two states that do not enforce NCAs, show the lowest levels of enforceability, while Florida and Connecticut display the highest.[9]

Data on non-compete use in the U.S. are sparse. The government surveys that are standard in the study of U.S. labor markets do not ask about non-compete use. Researchers have conducted four surveys of non-compete use in the U.S., one of which is national in scope and covers a broad range of occupations, and three of which cover specific occupations. These surveys are the basis of many studies within the literature. The national survey finds that 18% of workers in the U.S. were bound by an NCA as of 2014, and 38% had signed one at some point during their career (Starr, Prescott, and Bishara ["SPB"] 2019b). Moreover, the incidence of non-competes is generally higher in technical and high-skilled occupations and industries. The other three surveys find a sizeable incidence of non-compete agreements among specific occupations, as discussed below.

Curiously, the existing research consistently finds that non-compete use is common across states regardless of how enforceable the agreements are. In fact, non-competes are only somewhat less common in states where they are completely unenforceable as compared to states with stricter enforceability. The previously mentioned national survey finds that 18% of workers across the U.S. are bound by non-competes, compared to 19% in California and North Dakota—two states where NCAs are unenforceable (SPB 2019b). Two surveys of individual occupations show a similar pattern.[10]

There are several potential explanations for why firms offer non-competes, and why workers accept them. Non-competes potentially solve a "holdup" problem for certain types of investment (e.g., training, information sharing), allowing firms to make mutually beneficial investments in their workforce. Non-competes also allow firms to reduce recruitment and training costs by lowering turnover, and firms may offer a wage premium to compensate signers. Nevertheless, non-competes restrict workers' employment options *ex post*. Thus, workers may experience lower mobility, less competition for their services, and a worse bargaining position vis-à-vis their current employer.

The presence of non-compete agreements also has implications for innovation and entrepreneurship. By limiting the flow of workers to competitors, non-compete agreements simultaneously increase the returns to research and development (R&D) at incumbents while reducing knowledge transfer to new or existing competitors, with the net effect on innovation being ambiguous. The trade-off is analogous to that of patent protection, with stricter protections encouraging investment but temporarily limiting competition. NCAs may also tend to diminish entrepreneurship, as they limit the ability of workers to start competing firms. In theory, this

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invalid provisions can be rewritten so as to render them valid ("equitable reform" or "reformation"). Bishara (2011) is a thorough summary of state statutes and case law on the various dimensions of enforceability.

[9] See, for example, Figure 1 in Balasubramanian et al. (2018).

[10] Johnson and Lipsitz (2018) report that 31% of physicians in California have signed an NCA (vs. 45% nationally). Garmaise (2011) finds that 58% of firms in California have their executives sign NCAs (vs. 70% nationally).

reduction in firm entry could reduce competition in product markets and further reduce competition over wages, though direct evidence does not exist.

There is relatively little research into why non-compete agreements appear in markets for low-skilled workers. Its incidence among low-wage and low-skill workers tends to be lower than among the more affluent or skilled, but still non-trivial: SPB (2019b) report that 12% of individuals earning less than $20,000 per year were covered by a non-compete, compared to 21% of those earning $60-80,000. There are several possible theories. First, turnover tends to be higher in low-wage occupations,[11] and non-competes will tend to limit turnover either by inducing longer tenure or by screening out more mobile individuals. Second, if poorer households tend to be credit constrained, they may have difficulty funding certain types of training themselves that would otherwise be profitable to undertake. Non-competes potentially offer a mechanism through which firms can fund the cost. Third, low-wage workers are more likely than average to be bound by the minimum wage, and firms can extract additional surplus from workers when the minimum wage limits the ability of wages to do so.[12] Further research is necessary to understand why firms offer low-skilled workers non-competes and why those workers sign them.

Although the literature has made important strides in studying non-competes and their effects on workers, firms, and end consumers, further work is needed. Due to the limited availability of data and a paucity of natural experiments (e.g., law changes) to assess the impact of non-competes, much of the literature relies on cross-sectional comparisons of signers and non-signers, or high-enforceability states and low-enforceability ones. The more credible empirical studies tend to be narrow in scope, focusing on a limited number of specific occupations (e.g., executives) or potentially idiosyncratic policy changes with uncertain and hard-to-quantify generalizability (e.g., banning non-competes for technology workers in Hawaii). There is little evidence on the likely effects of broad prohibitions of non-compete agreements. Further research, perhaps exploiting more recent law changes or new sources of data, is necessary to establish the causal impact such agreements have on market participants.

The remainder of the paper is organized as follows. Section II outlines the theory behind non-compete use and Section III reviews the data and evidence. Section IV concludes.

II. Theory

This section discusses several channels through which non-compete agreements affect labor and product markets, many of which are not necessarily mutually exclusive. The focus is on highlighting the potential mechanisms through which the agreements operate rather than offering a detailed exposition or critique of the theories. Section III reviews the empirical evidence and suggests which channels receive more support from the data.

[11] Farber (1999), Choi and Fernández-Blanco (2017).
[12] Johnson and Lipsitz (2018).

A. Effects in the Labor Market

Non-compete agreements potentially offer a solution to a key problem that would otherwise limit investments in the employer-employee relationship, but at the same time may introduce frictions in the labor market, change the bargaining positions of workers and employers, and reduce (ex post) competition over wages. Before discussing the theory specific to non-compete agreements, we briefly overview the theory of worker-firm bargaining in order to frame the discussion.

In the simple, benchmark model of the labor market with perfect competition and no frictions, firms pay workers a wage equal to the full value they contribute to the firm, known as their value of marginal product (Borjas 2013).[13] A worker's value of marginal product incorporates their education, skills, training, and other attributes that contribute to productivity.

Deviating from perfect competition yields the possibility that a given worker-firm pair yields positive rents that the two can bargain over in a Nash-type bargaining game (Cahuc, Postel-Vinay, and Robin 2006). In a Nash bargaining model, equilibrium wages will be determined by the bargaining power and outside options of each party to the negotiation. A worker's outside options could include outside wage offers generated from on-the-job search, expected wage offers from job search during unemployment, or non-market activities. A worker with generous outside wage offers, for example, will have greater negotiating leverage and hence will tend to receive higher wages than a worker with less generous offers.[14] Similarly, a firm's outside options could include recruiting and training a replacement employee, leaving a job opening vacant, or filling a vacancy using an employee from elsewhere in the firm. A firm facing high recruiting and training costs will have less leverage and hence will have to pay higher wages in equilibrium.

1. Lock-in

One potential effect of non-compete agreements is to alter the bargaining positions of workers and firms. Balasubramanian et al. (2018) model how non-competes narrow the outside options and reduce the bargaining power of workers who sign them. The consequence will be lower worker mobility and longer tenure, as well as a flat or declining wage profile over the life of a job, all else equal. Balasubramanian et al. (2018) refer to this effect as "lock in".

The possibility of lock-in raises the question as to why a worker would sign a non-compete to begin with if the firm was expected to use it during future negotiations to extract a higher share of the match surplus. It is possible that workers either heavily discount the future (myopia), do not understand the implications of the clauses to begin with, or are offered sufficient additional compensation so that they are willing to accept the non-compete.

[13] Our discussion throughout generally focuses on wages, but a similar logic applies to non-wage compensation or workplace amenities.

[14] In a structural model estimated using French data, Cahuc, Postel-Vinay, and Robin (2006) find that inter-firm wage competition is a much more important determinant of the worker's share than the worker's bargaining power, especially for lower skilled workers.

2. Mitigating holdup

Employees are free to leave their employer at any time. Cognizant of this mobility, firms may forgo making certain investments in their workforce knowing that employees would be able to subsequently quit and appropriate the value of the investment. This is an example of a "hold-up" problem (Rubin and Shedd 1981; Grossman and Hart 1986). Common examples of investments likely to be subject to hold-up in the present context include non-tangible assets such as training, information (trade secrets or production processes), and client lists.[15]

Non-compete agreements are one arrangement that can mitigate the hold-up problem.[16] They do this by discouraging worker attrition before the firm has had time to recoup the cost of its upfront investment, and thus permit firms to make investments in its workers that are mutually beneficial and that it otherwise may not decide to do (Rubin and Shedd 1981). As the employee-employer relationship becomes more valuable, firms will tend to pass on some portion of the higher profits in the form of higher wages, assuming firms do not possess all the bargaining power in the relationship.[17] Thus, to the extent that non-compete agreements mitigate holdup, we should expect to see wages rise over a worker's tenure, all else equal.

The lock-in and holdup mitigation channels are not mutually exclusive. If the data suggest that wages are flat or fall over a worker's tenure, though, that suggests that the lock-in channel tends to dominate. Similarly, if wages tend to rise, that suggests that holdup mitigation tends to be the dominant mechanism.

While mitigating holdup will tend to increase wages, it generates ambiguous implications for worker tenure and mobility, depending on the relative increase in worker productivity at the incumbent firm as compared to at firms that are outside the scope of the non-compete (Balasubramanian et al. 2018). To the extent that mobility does decline as a result of increases in investment facilitated by non-competes, it is because the worker's current job has become *more* attractive relative to alternatives, unlike with lock-in. Thus, unlike declines in wages, declines in worker mobility are not necessarily informative about whether non-compete clauses are harmful.

Garmaise (2011) argues that non-competes have potentially offsetting effects on investments in training. Reducing holdup tends to increase the incentive for firm-sponsored training. But limiting an employee's outside options of employment will tend to decrease their incentive to

[15] In Becker's (1962) seminal model, firms may find it profitable to make investments in human capital that increases worker productivity at their specific firm ("firm-specific" training), but will generally not sponsor training that raises productivity at other firms. Firm-specific training is unlikely to be subject to a hold-up problem because it is by definition not valuable at other firms.

[16] Alternatively, workers could pay firms *ex ante* a portion of the value of the investment, or could post a bond that would be forfeited if the worker were to leave.

[17] Existing studies are consistent with firms sharing rents to some extent with employees in both union and non-union settings (Blanchflower, Oswald, and Sanfey 1996; Van Reenen 1996). Cahuc, Postel-Vinay, and Robin (2006), however, find that low-wage workers have little to no bargaining power in their study of the French labor market. Evidence on the returns to firm-specific human capital (tenure)—a market with one buyer and one seller—is also consistent with firms and workers splitting rents (Topel 1991; Altonji and Williams 2005). Outside of a bargaining framework, it is common to see compensation schemes designed around splitting rents (e.g., profit sharing, performance bonuses).

invest in portable (general) skills. Thus, the net impact on human capital accumulation is theoretically ambiguous.

3. Labor market frictions

Both mechanisms above (increased returns to tenure and lock-in) are consistent with a decline in worker mobility among individuals who have signed non-compete agreements. A reduction in worker mobility will tend to increase recruitment costs for all firms as the pool of potential applicants for a given posting will shrink. This type of friction can have important implications for wages and productivity. Worker mobility is an important source of wage growth for younger workers, with job changes accounting for approximately a third of early career wage growth (Topel and Ward 1992). In matching models of labor markets, increases in frictions such as recruitment costs will lead to a reduction in average match quality and hence lower aggregate productivity (Jovanovic 1979, 2015).

The presence of non-compete agreements in labor markets may also increase recruitment costs if there is uncertainty regarding whether a potential hire has signed one. Many workers are unsure whether or not they have signed a non-compete. One national survey reports that 30% of respondents did not know whether they had signed one (SPB 2019b). Firms, fearing litigation over hiring a worker bound by a non-compete, may need to expend resources to learn whether potential hires had signed a non-compete with their prior employer.
At the same time, by reducing worker mobility, non-compete agreements reduce turnover costs for the firms that use them. They may also reduce turnover through a screening mechanism: workers who are more likely to leave a job after a short stay will tend to select out of applying for jobs where non-competes are a requirement.

Provided that the firm's benefit from reducing turnover exceeds the cost imposed on the worker, the cost savings will be passed on to workers via higher wages. In perfectly competitive labor markets, workers will capture the entirety of the savings (Johnson and Lipsitz 2017). The premium paid to workers to accept workplace disamenties such as a non-compete agreement is commonly referred to as a compensating differential (Rosen 1974).

Non-compete agreements offer an option for firms to capture a greater portion of the surplus generated from their match with workers in the presence of downward rigidity in wages, such as in the presence of a minimum wage (Johnson and Lipsitz 2017).[18] When a firm cannot adjust total compensation through wages, they may instead adjust along non-wage dimensions such as firm-sponsored training (Schumann 2017), employer-provided health insurance (Marks 2011), or pension coverage (Simon and Kaestner 2004). Johnson and Lipsitz (2017) argue that offering or requiring non-compete agreements is yet another way for firms to adjust compensation (downward, as they impose costs on workers) and capture a larger share of the match surplus.

[18] Minimum wage laws are one example of downward rigidity, but firms may have a number of rationales for not reducing wages below a certain threshold: incentive provision in an efficiency wage model (Shapiro and Stiglitz 1984), concern over fairness (Akerlof and Yellen 1990), or to encourage employee cooperation (Fehr and Falk 1999).

Non-compete agreements can be seen as a non-wage attribute of a job that provide a benefit to firms (in the form of lower turnover costs) while imposing a cost on workers (reduced mobility), with the result being a transfer in the match surplus from workers to firms. In the context of minimum wage laws, firms are able to pay what are effectively sub-minimum wages. While this reduces the utility of inframarginal workers, it also expands the set of workers for which it is profitable for firms to hire. This expansion in employment will attenuate the disemployment effects of minimum wage laws. Johnson and Lipsitz (2017) propose this as one rationale for why non-compete agreements are observed in low-skilled labor markets, where minimum wage laws are more likely to be binding.

4. Reduced firm entry and competition for workers

Not only can non-compete agreements prevent workers from joining competing firms, but they can also prevent workers from founding new firms. If fewer new firms are formed, or if startups are hobbled by a dearth of qualified employees, then demand for workers in industries with a high incidence of non-compete agreements will be lower than otherwise. This mechanism will tend to reduce the wage competition for workers by reducing the frequency and attractiveness of outside offers.

B. Effects in Product Markets

By limiting mobility, non-compete agreements potentially tie up potential entrepreneurs, increase expected litigation costs over non-competes, and raise hiring costs for employed talent. These factors suggest that non-competes have the potential to reduce firm entry. Lower firm entry could dampen competition and product variety in product markets.

The implications of non-competes for innovation are ambiguous. On the one hand, greater worker mobility may lead to knowledge spillovers that spread information to other firms, enhancing their productivity. Gilson (1999) attributes the success of Silicon Valley, with its large concentration of innovative technology firms, to the unenforceability of non-competes in California and concomitant cross-pollination of ideas from a mobile workforce. On the other hand, firms may be reluctant to invest in risky R&D when departing workers can transfer proprietary information to competitors. By restricting the outflow of workers with non-competes, incumbent firms are in a better position to capture the returns to risky R&D investments. When it comes to innovation, the trade-offs involved are analogous to those in patent protection, with stricter protections encouraging investment but temporarily limiting competition.

III. Evidence

We first outline the data used in the literature, as well as some general features and limitations of the empirical models used to assess the effects of non-competes. Then, we turn to the empirical findings of the literature.

A. Data

The standard surveys used in studying U.S. labor markets (e.g., Current Population Survey, American Community Survey, and National Longitudinal Surveys) do not ask about non-compete agreements. Thus, the literature on non-competes relies on four surveys administered by academics to quantify their incidence, as well as to study their impact. One survey is national in scope and covers multiple industries and occupations, and the other three focus on individual industries or occupations. Separately, several papers combine state-level measures of non-compete enforceability with data on various worker and firm outcomes from more traditional government surveys.

The 2014 National Noncompete Survey Project surveyed 11,505 individuals on the use of non-compete agreements and other information using an online survey administered by the survey firm Qualtrics (Prescott et al. 2016; SPB 2019b). The survey collected data from individuals employed in the private sector or for a public healthcare organization, and covered all states, occupations, and (private) industries. Of those in the target sample who began taking the survey, 29% completed it and survived a number of quality checks implemented by the authors. The authors discuss several potential concerns over the validity of their survey instrument—to be included, an individual must participate in online surveys, have responded to the offer to take the survey, and have completed it. If the decision to respond to the survey is somehow correlated with non-compete use, then that could introduce bias into empirical work based on the survey.

The National Noncompete Survey finds that 18% of workers in the U.S. were bound by an NCA as of 2014, and 38% had signed one at some point during their career (SPB 2019b). Moreover, NCAs are prevalent across a number of industries, occupations, and skill levels, though they are more common among technical and high-skill occupations and industries. For example, non-competes are most prevalent in architecture and engineering (36%), computer and math-related jobs (35%), and management (30%). Nevertheless, they also appear with some frequency in grounds maintenance (11%), food preparation and service (11%), and construction and extraction (12%).[19] Non-compete incidence tends to be increasing with educational attainment as well, with holders of professional (39%) and master's degrees (29%) having the highest incidence, while high school graduates (13%) and those with some college (12-14%) have the lowest.[20]

Other surveys focus on specific occupations or industries. Garmaise (2011) and Kini, Williams, and Yin (2019) collect information on non-compete use among executives at public companies from public filings with the Securities and Exchange Commission (SEC) (e.g., 10-Ks and 10-

[19] SPB (2019b), Figure 5.
[20] SPB (2019b), Figure 3.

Qs). Many firms disclose whether their top executives have signed a non-compete in their SEC filings. This information is then combined with data on executive compensation from Standard & Poor's ExecuComp database. ExecuComp is a frequently studied database that tracks details on the compensation for the five highest paid executives of large public companies. Garmaise (2011) finds that about 70% of large, publically traded firms have their top executives sign non-compete agreements over the 1992 to 2004 period. Since some firms may require a non-compete but not disclose that fact publically, this figure is likely a lower bound. Kini, Williams, and Yin (2019) find that 26% of CEOs in their data covering 1992 to 2014 have executed non-compete clauses.[21]

Johnson and Lipsitz (2017) survey non-compete use among hair salons using an e-mail survey conducted in 2015 through a national hair stylist professional trade group, the Professional Beauty Association. A total of 218 salon owners responded with information on non-compete use, training, hiring practices, compensation, and other characteristics of the business. The authors estimate that the response rate to the survey was 31%, conditional on an individual having opened the e-mail survey. Among respondents, 30% of salon owners said they had their most recent hire sign a non-compete, and 39% said they had at least one hire in the past sign one.

Lavetti, Simon, and White (2018) implemented a survey on non-compete use among primary care physicians using web-based and mailed surveys. A total of 1,976 physicians across five states (California, Texas, Illinois, Georgia, and Pennsylvania) responded to the 2007 survey, which had a response rate of 70%. Beyond non-compete use, the survey elicited information on compensation and physician and firm characteristics. They estimate that about 45% of primary care physicians in group practices are bound by a non-compete agreement.

A number of other papers combine a state-level measure of enforceability with worker and firm outcomes from government surveys or data sources in order to compare high vs. low enforceability regimes. For example, Balasubramanian et al. (2018) derive data on worker mobility and wages from the Longitudinal Employer-Household Dynamics survey and the Current Population Survey. Several studies (e.g., Marx, Strumsky, and Fleming 2009; Conti 2014) use public data on patent filings in order to measure R&D and the mobility of inventors. These papers do not observe whether or not a given worker has signed an NCA, or whether a given firm offers NCAs to its workers. As such, they do not offer estimates of the incidence of non-compete use.

B. Empirical Approaches Used in the Literature

There are three general approaches in the literature to assess the effects of non-compete agreements. Some papers follow multiple approaches.

The first is to use state policy changes in enforceability, such as changes in state statutes or changes in judicial interpretations of state statutes. Papers following this approach include Marx,

[21] 42% of CEOs in their sample have reported signing an employment contract, of which 62% have an NCA. This latter figure grew from 46% in 1992-93 to 63% in 2013-14, which demonstrates the growing use of NCAs among executives.

Strumsky, and Fleming (2009), Garmaise (2011), Carlino (2017), Balasubramanian et al. (2018), and Johnson, Lavetti, and Lipsitz (2019), among others. Exploiting policy changes can be a credible way of assessing the impact of state laws and regulations.

In the literature on non-competes, though, there is a paucity of changes in enforceability, with papers often relying on one or a handful of policy changes, such as Hawaii's ban on non-competes for tech workers or Michigan's reversal of its prohibition.[22] The dearth of policy changes raises two problems: assessing external validity and quantifying the uncertainty regarding estimated effects. While the studies exploiting state policy changes are well executed, it is far from clear whether the estimated effects are likely to extend to other states (with, e.g., a different composition of firms for workers to switch to), industries (with, e.g., different opportunities for training), or occupations. Non-compete incidence varies markedly across industry and occupation, which suggests that the underlying determinants of use do as well. Although research directly examining heterogeneity in effects across different groups is sparse, Fallick, Fleischman, and Rebitzer (2006) do find that non-competes matter only for tech workers and not other occupations. Regarding quantifying the uncertainty of any estimated effects, under certain conditions, estimating standard errors in the presence of a small number of treated units can lead to important biases when using clustered standard errors, as is common in this literature.[23] Thus, extra care should be taken in interpreting the precision and statistical significance of estimates.

Even when such policy changes are available to the researcher, the possibility that non-competes have external effects on non-signers complicates evaluating the effects of changes in non-compete enforcement. Several papers provide evidence of such spillover effects (Starr, Frake, and Agarwal *forthcoming*; Johnson, Lavetti, and Lipsitz 2019). For example, Johnson, Lavetti, and Lipsitz (2019) show that changes in NCA enforceability can affect workers in areas across the border from states changing their non-compete policy. In such a setting, estimating the impact of changes in enforceability using a difference-in-differences model is complicated by the fact that outcomes in control states may be affected by the changes in policy of contiguous (treated) states, and treated states may be affected by changes in policy of other adjacent (treated) states. It is not obvious exactly what parameter is identified by such a model.

The second approach evaluates the impact of having a high incidence of non-compete agreements in a state with high enforceability in a difference-in-differences (or triple differences) framework. These studies do not exploit policy changes over time (as above), but rather use within-state groups as controls, such as industries with a low-incidence of non-compete agreements. Thus, differences across states in worker outcomes between high and low enforceability are compared for high incidence industries and low incidence industries. Practically, the use of within-state control groups allows the inclusion of state fixed effects to

[22] Johnson, Lavetti, and Lipsitz (2019) is an exception, which exploits 70 changes in an enforceability index over the 1991 to 2014 period.

[23] See, e.g., Imbens and Kolesár (2016) and MacKinnon and Webb (2018). Lipsitz and Starr (2019) is the only paper using a small number of policy changes (one, in its case) that addresses this issue. They find that p-values are—in some specifications and samples—substantially higher when correcting standard errors to account for the small number of treated units.

control for any unobserved factors that are common to both low and high incidence industries within a state (e.g., cost of living, broad labor market conditions). Examples include Starr (2019), Balasubramanian et al. (2018), Starr, Balasubramanian, and Sakakibara (2017), and Starr, Frake, and Agarwal (2018).

There are several limitations to this second, difference-in-differences approach. First, the types of industries that have low and high incidences of non-competes are markedly different. Non-compete agreements tend to be more prevalent in higher skilled and technical industries such as information technology (IT) and engineering.[24] Any state-level laws or economic factors that affect low-skill workers differently than high-skill workers could potentially bias the models' estimates, to the extent that such laws or factors are correlated with enforceability. For example, state minimum wage laws tend to raise the wages of low-skilled workers more than high-skilled workers.[25] If states that set higher minimum wages tend to have weaker or stronger non-compete enforceability, state fixed effects would be of no use and the estimated impact of non-compete use would be biased.

Another limitation in this second approach is that the underlying variation in non-compete use is poorly understood. It is not clear why—within low- or non-enforcing states—NCAs are common in some industries but not others.[26] Moreover, it is not clear why the *same* industry has a low incidence in some states but high incidence in other states.[27] Without a firm understanding of what drives non-compete use, it is impossible to ascertain whether the necessary exclusion restriction holds and hence whether a difference-in-differences model produces unbiased estimates of the impact of non-compete incidence and enforceability.

The third approach compares labor market outcomes of signers with non-signers after conditioning out the observable characteristics of each group in a regression framework. Some examples include Johnson and Lipsitz (2017), Lavetti, Simon, and White (2018), and Starr et al. (2019). By comparing signers to non-signers, this approach is able to estimate the effect of treatment on the treated. The other two only estimate an intent to treat effect, which does not isolate the effect on signers themselves without information on the change in incidence due to treatment (which none of the studies attempt to estimate).[28]

An important limitation of this approach is the possibility of selection on unobservable worker and firm characteristics that is correlated with NCA use. A general concern with evaluating worker compensation, including arrangements that include non-compete clauses, is that workers are likely to select into jobs that offer a compensation scheme that best meets the preferences and abilities of that worker (Lazear and Shaw 2007). For example, if workers who are most likely to benefit from on-the-job training tend to select into jobs which offer more training, then

[24] SPB (2019b), Marx, Strumsky, and Fleming (2009).
[25] In Johnson and Lipsitz's (2017) model, non-compete use is predicted to be higher in areas where the minimum wage is more likely to be binding, implying that NCAs and minimum wage laws would be correlated.
[26] This fact is not lost on the authors themselves. Starr, Frake, and Agarwal (2018) write that "we have little understanding why the incidence varies in non-enforcing states, given that such provisions are unenforceable".
[27] See Figure 1 in Starr, Frake, and Agarwal (2018), which shows incidence by state and industry. This means that a given industry acts as a treated unit in some states but a control in others.
[28] Angrist and Pischke (2009), pp. 158-164. This assumes there are no externalities to the presence of NCAs.

comparing workers who have signed non-competes to those who have not will tend to overstate their impact on training. Similarly, if workers who select into jobs with strong training opportunities tend to be more productive in general (positive selection), then comparing signers with non-signers would tend to overstate the effect of non-competes on worker outcomes. Firms may also select into states based on state characteristics, such as state taxes, unionization levels, worker productivity, or environmental regulations, which could potentially be correlated with non-compete enforceability.

Beyond selection, it is possible that unobservable features of compensation are correlated with non-compete use. For instance, technology-based startups may tend to offer a higher portion of compensation in stock options (due to cash flow constraints) and also tend to rely more heavily on proprietary information and production processes (and hence require NCAs of their employees). Evaluating the effect of NCAs on wages alone could potentially under- or over-state the impact on total compensation.

To address these two limitations, the literature incorporates controls for worker and firm characteristics in order to reduce any confounding influence of selection. For example, SPB (2019b) control for worker characteristics (gender, education, age, hours and weeks worked, number of past employers), firm characteristics (size, multi-state status), characteristics of employment (other post-employment covenants such as non-disclosure agreements, compensation features such as the presence of health insurance, a retirement plan, etc.), and state-level factors (unemployment, size of labor force). A number of papers also incorporate a test due to Oster (2017) which quantifies how important selection on unobservables would have to be in order to reverse the sign of the coefficient on the policy variable of interest.[29] They generally find that selection on unobservables would have to be "implausibly" strong to reverse their findings.

C. Effects in the Labor Market

Studies of the labor market effects of non-compete agreements have examined a number of outcomes, with particular focus on investments in non-tangible assets (e.g., worker training), worker mobility, and wages.

1. Investments in non-tangible assets (training, information, and client lists)

Non-compete agreements offer an opportunity for firms to invest in various non-tangible assets that might otherwise be subject to holdup. The most common investments analyzed in the literature are training (investments in human capital), sharing information with workers, and sharing client lists with workers. The bulk of the empirical literature finds that workers signing non-compete agreements, or workers who reside in areas with a higher incidence of NCAs, receive more training, more access to information, and more access to client lists. Nevertheless, there is some variation in this finding depending on the type of non-compete and occupation. Garmaise (2011) argues that non-competes have potentially offsetting effects on investments in training: they increase the incentive for firm-sponsored training but decrease that of self-

[29] Starr, Prescott, and Bishara (2019a), SPB (2019b), Starr (2019), Starr, Frake, and Agarwal (*forthcoming*).

sponsored training. The overall impact on human capital accumulation, then, is theoretically ambiguous. Using a credible source of variation—changes in state policy in Florida, Louisiana, and Texas—he finds wage effects among top executives of public companies that are consistent with workers in higher enforceability states tending to receive more firm-sponsored training. Notably, though, he finds that the decline in (self-sponsored) general training is even greater, leading to lower levels of overall human capital investment (and hence wages). Note, though, that he does not directly analyze data on worker training, but rather infers the effects of NCAs on training from its effects on compensation.

The remaining studies rely on comparing non-compete signers with non-signers, or comparing outcomes in high enforceability states to low enforceability states, while attempting to control for selection using observable characteristics of individuals. Starr (2019) estimates that moving a state from non-enforcement to average enforcement would increase the incidence of worker training by 18%. NCAs also allow firms to train employees sooner in the employment relationship. Uncertainty regarding an employee's tenure will tend to lead firms to delay investing in costly training as they screen employees for those who will quit soon, but the presence of enforceable non-competes allows firms to reduce this uncertainty and move up training opportunities (Starr 2019). Among hair stylists, Johnson and Lipsitz (2017) find that NCA use is associated with a 14% higher likelihood of firms providing on-the-job training. Starr et al. (2019) find that the timing of when a worker receives an NCA matters: although they find no overall effect of NCA use on training, workers receiving early notice (prior to accepting a job) are 11% more likely to have received training.

Like investments in human capital, client lists and information are "mobile" in the sense that they are attached to the worker rather than the firm, and workers may exploit such investments once they quit. Surveying primary care physicians within group practices, Lavetti, Simon, and White (2018) find that physicians receive more patient referrals when they have signed a non-compete agreement. Starr et al. (2019) find, however, that timing once again matters: workers receiving early notice of an NCA are more likely to have firms share information with them, while those receiving late notification are substantially less likely.

Gurun, Stoffman, and Yonker (2019) study non-compete clauses in the financial advisory industry. The relationships that financial advisers form with clients may allow financial advisers to take clients with them when moving firms or founding a new firm. Such behavior may attenuate firms' incentives to, for instance, engage in marketing activities that would build its employed advisers' portfolio of clients. To address this issue, many firms in the industry require non-compete agreements. Gurun, Stoffman, and Yonker (2019) find that relaxing the enforceability of non-compete agreements leads to important shifts in the assets under management at financial advisory firms, consistent with financial advisers bringing clients with them when switching firms.

2. Worker mobility and labor market frictions

By limiting the post-employment options of workers who sign them while also potentially increasing the returns to sticking with a given employer, non-compete agreements are predicted to increase worker tenure and decrease job switching.

The empirical evidence consistently bears this out, including the studies using state policy changes to identify the effects of interest. For American workers generally, Johnson, Lavetti, and Lipsitz (2019) find that moving from a policy of NCA unenforceability to the highest enforceability observed across U.S. states in their sample is predicted to reduce the month-to-month probability of workers changing employers by 26.1%.[30] Similarly, for low wage (hourly) workers, Lipsitz and Starr (2019) show that Oregon's ban on enforcing non-competes led to an increase in transitions across employers of 12.2 to 18.3%.

Studies of individual industries and occupations also find that higher NCA enforceability is associated with lower worker mobility. Inventors in Michigan were 8.1% less likely to switch jobs after Michigan strengthened its enforcement of non-compete agreements in the mid-1980s, with even lower switching rates among those with firm-specific and technological expertise (Marx, Strumsky, and Fleming 2009). Hawaii's ban on NCAs for technology workers led to an 11% increase in mobility, relative to comparable workers in other states, in years subsequent to the ban (Balasubramanian et al. 2018). Top executives were substantially (47%) less likely to change jobs within industries as non-competes became more strictly enforced, and their tenure increased by 16% (Garmaise 2011).

CEO turnover is more responsive to a firm stock performance when the firm's CEO has a signed non-compete agreement (Kini, Williams, and Yin 2019). This is consistent with firms being reluctant to fire executives for lackluster performance if their CEO is able to join a competitor. Financial advisers are substantially more likely to switch firms when non-competes are not enforced against them (Gurun, Stoffman, and Yonker 2019). However, Gurun, Stoffman, and Yonker (2019) find that a reduction in the enforcement of non-competes leads to an increase in misconduct among financial advisers, which is consistent with firms being reluctant to discipline employees who can take assets (clients) with them when they switch jobs.

The more correlational studies in the literature also conclude that non-competes tend to lengthen employee tenure. Nationwide, workers in average-enforcing states have had 8% fewer jobs than similar workers in non-enforcing states (Balasubramanian et al. 2018). Workers in states with a higher incidence of non-competes tend to have longer tenure, and that the effect of incidence is even higher in states with stronger enforceability. Starr, Frake, and Agarwal (*forthcoming*) find that a 10 percentage point increase in the incidence of NCA use is associated with an 0.8 year increase in tenure in average- vs. non-enforcing states (a 12% increase over the mean). IT workers in Silicon Valley and elsewhere in California exhibit higher rates of mobility compared to comparable workers in other states, though this pattern appears to be unique to IT and does not extend to other industries within California (Fallick, Fleischman, and Rebitzer 2006).

Not only do non-compete agreements affect the mobility of workers who sign them, but some evidence suggests they also affect the mobility of those who have *not* signed one by increasing uncertainty in the hiring process. Starr, Frake, and Agarwal (*forthcoming*) show that, among workers who have not signed a non-compete agreement, higher incidences of non-competes tend to reduce job offers in high enforceability states more than low enforceability states (i.e., the

[30] This estimate is only marginally statistically significant, however. Their sample covers uses CPS data over the 1991 to 2014 period.

interaction between incidence and enforceability is negative in the regression model).[31] Their model predicts that a 10 percentage point increase in the incidence of non-competes is associated with a 21% lower rate of job offers over the previous year, in average enforceability states relative to non-enforcing states. This finding suggests that the prevalence of non-competes in certain industries could potentially increase frictions in the labor market, generally, not just among those who have signed the agreements. The importance of the externality will depend on how costly it is for firms to discover whether potential hires are bound by a non-compete.[32] Since this paper relies on cross-sectional comparisons of states at different levels of incidence and enforceability, though, rather than (say) an exogenous policy shock, the results should be interpreted with some caution.

Although much of the focus in the literature is on how non-competes introduce frictions in the labor market, one study suggests they may reduce one friction of particular importance to low-wage workers. Johnson and Lipsitz (2017) find that non-competes mitigate the disemployment effects of the minimum wage by allowing firms to pay what is essentially a sub-minimum wage (reducing the wedge between reservation wages and a binding minimum wage). They replicate Dube, Lester, and Reich's (2016) study and find that minimum wage laws have no effect on employment in states with relatively strong enforcement of non-competes, but have negative effects on employment in states which do not enforce non-competes. This finding suggests that non-competes may serve to reduce an important friction in the labor market for low-wage workers. Nevertheless, the fact that non-compete use does not appear to vary considerably across states with different levels of enforceability, as several surveys find, suggests that it may not be the presence of non-competes themselves that are tempering the impact of the minimum wage, but rather other unobservables that are simply correlated with enforceability. If this is true, then it is not clear how important a role that non-competes are playing.

3. Firm entry

The evidence on non-compete enforceability and firm entry is mixed. Using Michigan's (lone) law change, Carlino (2017) finds that an increase in enforceability had no impact on the number of firm startups, and had a small (but statistically insignificant) increase in the rate of job creation by startups.

The remainder of the literature, relying more heavily on cross-sectional comparisons, finds that non-compete enforceability is associated with less entry.

Stuart and Sorenson (2003) study "liquidity events" (initial public offerings and acquisitions), which provide an influx of liquid assets to senior employees. They show that these events generally increase the rate of new firm foundings in the biotech industry, but that non-compete enforceability attenuates this effect, likely because potential entrepreneurs are prevented from starting competitor firms by non-compete agreements.

[31] Curiously, though, within states of average or below average enforceability, workers in high incidence industries are more likely to generate job offers than those in low incidence ones.
[32] In the case of executives, the information is likely to be relatively easy to come by. For instance, Garmaise (2009) gleans it from public 10-K filings.

Samila and Sorenson (2011) study the differential response of states with high and low enforceability regimes to shocks to venture capital availability. They find that states with less strict NCA enforceability respond to such shocks with higher levels of firm startups and employment. These responses are consistent with non-competes inhibiting new firm creation more, on net, than they encourage investments in human capital or knowledge.

Starr, Balasubramanian, and Sakakibara (2017) provide evidence that higher enforceability is associated with fewer spin-off firms within the same industry as their predecessor.[33] Nevertheless, those spin-offs that do appear are (on average) larger, faster growing, and have a higher likelihood of surviving the initial years. They argue that this is because non-compete agreements introduce expected litigation costs for spin-offs, and these costs dissuade less profitable and smaller firms from ever forming. As with Carlino (2017), this is consistent with greater enforceability leading to startups that are more durable.

4. Wages

There are several channels through which NCAs can affect wages, including increasing investments in human and other non-tangible forms of capital, and reducing wage competition by improving the bargaining position of employers and reducing entry of competitors. The empirical evidence on which channel tends to dominate is mixed.

Using state policy changes, Johnson, Lavetti, and Lipsitz (2019) and Lipsitz and Starr (2019) find that increasing enforceability leads to lower wages. For U.S. workers generally, Johnson, Lavetti, and Lipsitz (2019) estimate that moving from NCAs being unenforceable to the highest level of enforceability observed in their sample would lead to an 8.9% drop in average wages. Since only a fraction of workers actually sign non-competes, the effect of strengthening enforceability will be quite a bit higher on those bound by one. Using the 18% incidence estimate from SPB (2019b) and assuming away spillovers on non-signers, a back-of-the-envelope calculation suggests average wage effects on non-compete signers of nearly 50% (0.89/0.18)! These wage effects only appear among (relatively) more educated workers, though: they find no effect of increasing enforceability on workers with less than a college education.

Lipsitz and Starr (2019) estimate that Oregon's ban on non-competes in 2008 led to a 2.2 to 3.1% increase in average wages for low wage (hourly) workers relative to several control groups. Moreover, they find no wage effects for workers with less than a high school degree. However, the timing of Oregon's law banning non-competes is unfortunate from an inferential point-of-view as it coincides with the onset of the Great Recession, the most severe recession since the Great Depression and one which had significant consequences for labor markets. This raises the possibility that the paper's estimated effects are confounded by macroeconomic factors that—similar to NCAs—also influence wage growth and worker mobility, as well as by the differential policy responses by states.[34] Indeed, in Lipsitz and Starr (2019), the mobility of workers in

[33] They define industry according to the four-digit NAICS code.

[34] Research on regional recessions finds that the timing of recessions (both the onset and recovery) differs across states (Hamilton and Owyang 2012). This includes states in the same Census region or division, which are used as

Oregon increased (relative to control states) soon after the ban took force in 2008, but average wages did not increase until a full three years post-ban (in 2011). Actual (or threatened) worker mobility is an important channel through which we expect workers to achieve wage growth in Oregon after its ban on non-competes. The fact that Oregon saw an increase in mobility without an increase in average wages raises the possibility that there are confounding factors at play.

Three studies that also exploit state policy changes but concentrate on individual occupations yield mixed findings. Garmaise (2011) provides evidence that increases in non-compete enforceability from state policy changes led to 8.2% lower growth in the compensation of top executives (25% of the mean growth rate). Kini, Williams, and Yin (2019), on the other hand, show that higher enforceability is associated with higher initial compensation among CEOs who have signed non-competes, consistent with the existence of compensating differentials. They find that a one-standard-deviation increase in their enforceability index is associated with an 11.7% increase in the total initial compensation of CEOs bound by NCAs in their sample. Balasubramanian et al. (2018) show that wages rose among new tech hires by 4.2% after Hawaii eliminated the enforceability of non-compete agreements for technology workers.

Several other, more correlational studies find that NCA signers earn higher wages, consistent with non-competes mitigating holdup. Starr et al. (2019) show that workers bound by non-competes earn 7% higher wages compared with comparable unbound workers. Lavetti, Simon, and White (2018) find that wage growth among primary care physicians in group practices is sharply higher among those having signed a non-compete compared with those who have not, which they attribute to greater within-group patient referrals. They estimate that physicians who sign non-competes experience earnings growth that is eight percentage points higher in each year of the first four years as compared to non-signers, and that their earnings are cumulatively 35 percentage points higher after 10 years.

The particulars of the negotiation process appear to matter. Although Starr et al. (2019) find that NCA signers tend to earn more, the wage premium appears among those who received early notification of the non-compete. Those receiving early notice (about two thirds of the sample) receive 10% higher wages than comparable individuals do, while those receiving late notice (about one third of the sample) receive no wage premium.

Other studies find evidence that workers who sign non-competes tend to earn less and experience lower wage growth over their tenure. Starr (2019) finds that wages are lower among workers, generally, in high enforcement states; in particular, moving from non-enforcement to average enforcement is predicted to lower wages by 4%. Balasubramanian et al. (2018), in a similar setup and using the same data, show that tech workers are predicted to receive average wages that are 2.0-2.8% lower in average vs. non-enforcing states. They also show that wages in

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control groups in some of the difference-in-differences specifications, and (plausibly) states with a pre-2008 trend in wages or mobility similar to Oregon's, which are used in the synthetic control approach. States also varied in their policy responses to the Great Recession, including changing the maximum duration and generosity of unemployment insurance as well as state minimum wage policy. Lipsitz and Starr (2019) do control for changes in state minimum wages.

average enforcing states tend to be lower even early in the employment relationship (at quarter four of the current job spell).

D. Effects in Product Markets

Less firm entry as a result of a higher incidence of non-compete agreements, as discussed above, is suggestive of the fact that competition in product markets may also be attenuated, though no paper has directly studied the link. Given the importance of non-competes in more technical occupations and industries (Marx, Strumsky, and Fleming 2009; SPB 2019b), the impact may tend to be more acute in technical and scientific industries.

A number of papers, though, do consider the implications of non-competes for innovation. Innovation is often measured, somewhat crudely, using data on patent applications. Although patents do not capture every type of innovation in the economy, they have the advantage of being readily measurable as well as available across a number of different industries. Patents are typically assigned a particular geography based on the address of the inventor or inventors, which appears on the application. Patent activity is common enough that it can be analyzed at the state- or even Metropolitan Statistical Area-level.

Samila and Sorenson (2011), in addition to entrepreneurship and employment, also study the impact of venture capital shocks on innovation. They find that states with less enforceability tend to have more new patents. Together, these responses are consistent with non-competes inhibiting new firm creation and innovation more, on net, than they encourage investments in human capital or knowledge.

Several papers find that stricter non-compete enforceability leads to more innovation, consistent with their reducing information spillovers to competitors. Carlino's (2017) evaluation of Michigan's accidental increase in enforceability finds an increase in the number of mechanical patents in Michigan (the most important patent class in the state), though declines in several smaller patent types. The lower mobility among inventors documented by Marx, Strumsky, and Fleming (2009) was likely an important factor in limiting information transfer among Michigan firms. Conti (2014) finds that firms in states with stronger non-compete enforceability tend to pursue riskier R&D projects than firms in states with weaker enforcement.

Little work has been done on whether any cost changes due to the presence or absence of non-competes are ultimately passed on to end consumers. There are two exceptions. Hausman and Lavetti (2019) argue that the use of non-competes can increase the cost structure of physician practices, and that these costs are ultimately passed on to consumers. They document that a 10% increase in their enforceability index is associated with a 4.3% increase in average commercial prices for physician services. Gurun, Stoffman, and Yonker (2019) find that eliminating the enforcement of non-competes among a group of financial advisory firms led to higher fees for end consumers. They argue that a lack of enforceable non-competes increases the cost of worker attrition (as advisers are able to bring clients with them), which is then passed on to consumers.

IV. Conclusion

Although suggestive, the existing empirical literature on non-compete agreements suffers from several important limitations that raise questions as to whether it has successfully estimated the causal effect of such agreements on mobility, wages, entrepreneurship, and innovation. Due to the limited availability of data and a shortage of natural experiments to assess the impact of non-competes, much of the literature relies on cross-sectional comparisons of signers and non-signers, or high-enforceability states and low-enforceability ones.

Nevertheless, the literature offers some tentative findings. Across the board, the literature finds that non-compete agreements are associated with longer worker tenure and less mobility. The findings for other outcomes, however, are mixed. The papers relying on state policy changes for identification find that non-competes lead to more firm-sponsored training among top public executives (Garmaise 2011) but lower wages generally (Johnson, Lavetti, and Lipsitz (2019) and for technology workers specifically (Balasubramanian et al. 2018). Estimates for executives at public companies are mixed (Garmaise 2011; Kini, Williams, and Yin 2019). Studies relying on cross-sectional comparisons tend to find that non-competes are associated with more training and information sharing, as well as higher wages in some instances.[35]. Regarding firm entry and innovation, the only paper using state law changes (Carlino 2017) finds no discernable effect of a state law that changed non-compete enforceability.

Further research is needed in several areas. First, the determinants of why workers sign non-competes and why firms offer them is not well understood. Second, it is puzzling why non-compete incidence is only weakly correlated with state enforceability. Third, there are only a handful of studies of specific industries and occupations (physicians, tech workers, and hair stylists). Given the wide variation across jobs in the potential for investments and the possibility of lock-in, further work would help shed light on where non-competes are likely to increase or decrease efficiency and welfare. Fourth, exploiting further changes in policy or enforcement would be useful in sharpening the empirics used in this literature, which relies somewhat more heavily on cross-sectional comparisons of non-compete signers with non-signers and high-incidence states with low-incidence ones. These changes could consist of state law changes, increases in enforcement action (as has occurred recently in Washington and Illinois), or changes in firm or franchise use of non-compete agreements. Fifth, little work has been done to study how non-compete agreements affect end consumers.

[35] The sign and magnitude of the effect on wages does vary in the studies based on occupation and characteristics of the negotiation (e.g., early vs. late notice).

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NONCOMPETE CLAUSES USED IN EMPLOYMENT CONTRACTS
COMMENT OF THE GLOBAL ANTITRUST INSTITUTE, ANTONIN SCALIA LAW SCHOOL, GEORGE MASON UNIVERSITY

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Noncompete Clauses Used in Employment Contracts
Comment of the Global Antitrust Institute,
Antonin Scalia Law School, George Mason University

February 7, 2020

This Comment is submitted to the Federal Trade Commission (FTC) in relation to its proposed examination of whether there is "a sufficient legal basis and empirical economic support" to promulgate a Commission rule that would restrict the use of noncompete clauses in employer-employee contracts.[1] We submit this Comment based upon our extensive experience and expertise in antitrust law and economics.[2] As an organization committed to promoting sound economic analysis as the foundation of antitrust enforcement and competition policy, the Global Antitrust Institute (GAI)

[1] *See* Press Release, Fed. Trade Comm'n, FTC to Hold Workshop on Non-Compete Clauses Used in Employment Contracts (Dec. 5, 2019), https://www.ftc.gov/news-events/press-releases/2019/12/ftc-hold-workshop-non-compete-clauses-used-employment-contracts.

[2] The Global Antitrust Institute (GAI), a division of the Antonin Scalia Law School at George Mason University (Scalia Law), is a leading international platform for economic education and research that focuses upon the legal and economic analysis of key antitrust issues confronting competition agencies and courts around the world. University Professor Joshua D. Wright, Ph.D. (economics), is the Executive Director of the GAI and a former U.S. Federal Trade Commissioner. Associate Professor John M. Yun, Ph.D. (economics) is the Director of Economic Education at the GAI and a former Acting Deputy Assistant Director in the Bureau of Economics, U.S. Federal Trade Commission. Professor of Law Douglas H. Ginsburg is a Senior Judge, United States Court of Appeals for the District of Columbia Circuit, Chairman of the GAI's International Board of Advisors, and a former Assistant Attorney General in charge of the Antitrust Division of the U.S. Department of Justice. Tad Lipsky is the Director of GAI's Competition Advocacy Program, Adjunct Professor at Scalia Law, a former Deputy Assistant Attorney General for Antitrust and a former Acting Director, Bureau of Competition, U.S. Federal Trade Commission. Camila Ringeling is a former consultant for the World Bank and the Office of International Affairs of the U.S. Federal Trade Commission, and former case handler for the Chilean competition authority, Fiscalía Nacional Económica. The GAI gratefully acknowledges the assistance of Scalia Law student Nathan Detweiler in the preparation of this comment.

commends the FTC for inviting discussion in regard to regulation of this critical business practice.

Introduction

There has been an increased focus by academics, the U.S. competition authorities, and the Treasury Department on the effect of noncompete clauses on employee mobility, wages, and the efficiency of labor markets.[3] These are important issues worthy of serious consideration by the competition authorities. We are concerned, however, that many proposals to address them through *ex ante* antitrust regulatory interventions, such as an FTC rule,[4] are ill-suited and will likely do more harm than good.

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[3] *See, e.g.*, Office of Econ. Policy, U.S. Dep't of the Treasury, NON-COMPETE CONTRACTS: ECONOMIC EFFECTS AND POLICY IMPLICATIONS (2016), (hereinafter "Treasury Report"), https://www.treasury.gov/resource-center/economic-policy/Documents/UST%20Non-competes%20Report.pdf; Evan Starr, J.J. Prescott & Norman D. Bishara, *Noncompetes in the U.S. Labor Force* (Univ. of Michigan Law & Econ Research Paper No. 18-013, 2019), http://dx.doi.org/10.2139/ssrn.2625714; Ioanna Marinescu & Herbert Hovenkamp, *Anticompetitive Mergers in Labor Markets,* PENN LAW: LEGAL SCHOLARSHIP REPOSITORY (Faculty Scholarship 2018), https://scholarship.law.upenn.edu/cgi/viewcontent.cgi?article=2967&context=faculty_scholarship (noting that vertical noncompete agreements between employers and employees can have horizontal effects if multiple employers in a labor market use them, and that such effects could be relevant to merger analysis as an exacerbating factor in assessing potential competitive harm); Alan B. Krueger & Eric A. Posner, *A Proposal for Protecting Low-Income Workers from Monopsony and Collusion* (The Hamilton Project, Feb. 2018), https://www.hamiltonproject.org/assets/files/protecting_low_income_workers_from_monopsony_collusion_krueger_posner_pp.pdf (recommending noncompetes to be uniformly unenforceable and banned if they govern an employee who earns less than the median wage in her state); Marshall Steinbaum, *A Missing Link: The Role of Antitrust Law in Rectifying Employer Power in Our High Profit, Low-Wage Economy* (Roosevelt Inst. Issue Brief, Apr. 2018), https://rooseveltinstitute.org/wp-content/uploads/2018/04/Monopsony-issue-brief.pdf (recommending banning noncompete agreements, no-poaching agreements, mandatory arbitration in employment contracts, and other similar competitive restraints in the labor market).

[4] *See, e.g.*, OPEN MARKETS INST. ET AL., PETITION FOR RULEMAKING TO PROHIBIT WORKER NON-COMPETE CLAUSES (Fed. Trade Comm'n, 2019), https://openmarketsinstitute.org/wp-content/uploads/2019/03/Petition-for-Rulemaking-to-Prohibit-Worker-Non-Compete-Clauses.pdf (hereinafter "OMI Petition"); Letter from Sen. Richard Blumenthal et al., to Joseph Simons, Chairman,

Specifically, some proposed *ex ante* interventions—including an outright ban on noncompete clauses—run the risk of condemning a long-standing employment practice that state laws already address. A ban might make economic sense were the evidence sufficient to show that noncompete clauses systematically reduce consumer or total welfare wherever used or when applied to an identifiable subset of employees, such as, lower wage employees. As discussed below, we do not believe the evidence currently supports such a blunt approach. Absent that evidence, a ban would risk falsely condemning procompetitive uses of noncompetes and thereby reducing productivity and dampening the incentives to invest in trade secrets and to disseminate firm-specific knowledge widely among a firm's workforce.

In this comment, we survey the existing literature on the economic effects of employee noncompete clauses and discuss their current legal treatment—both at the state level and under federal antitrust law. We find that a blanket rule prohibiting the use of noncompetes—or even a more targeted ban—is not justified at this time. We also highlight the research finding that employees are often not fully informed regarding the terms of noncompete clauses and, when they are informed before employment, their

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Fed. Trade Comm'n (Mar. 20, 2019), https://www.blumenthal.senate.gov/imo/media/doc/Sen%20Blumenthal%20et%20al%20re%20non%20competes_vF.pdf; Letter from the Attorneys General of Minnesota, California, Delaware, District of Columbia, Illinois, Iowa, Maine, Massachusetts, Maryland, Michigan, New Mexico, North Carolina, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, and Wisconsin, to Joseph Simons, Chairman, Fed. Trade Comm'n (Fed. Trade Comm'n, Nov. 15, 2019), http://www.illinoisattorneygeneral.gov/pressroom/2019_11/111519_Multistate_FTCNon-CompeteLetter.pdf

welfare improves. The idea is that, when employees are aware of noncompete clauses before employment, they are in a position to bargain for greater compensation. This result suggests a disclosure-based consumer protection type remedy might be more appropriate—and entail a lower risk of chilling procompetitive uses of noncompete clauses—than either a general or more limited ban.

Economics of Noncompete Clauses

Noncompete clauses are contractual provisions that prohibit an employee, after leaving a job, from working for a competing employer within a certain period of time and often within a specified geographic area. From the available data, noncompetes affect 18 to 20 percent of the U.S. workforce and nearly 40 percent have signed at least one noncompete agreement in the past.[5] Not surprisingly, as with virtually all other forms of vertical restraints, economic research has identified both welfare-enhancing as well as welfare-reducing effects.[6]

Noncompete agreements may be justified for a number of reasons. Most important, noncompetes can encourage innovation by preventing employees who have

[5] *See* Treasury Report, *supra* note 3; Starr et al., *supra* note 3 at 6.; *see also* John M. McAdams, *Non-Compete Agreements: A Review of the Literature* at 3 (December 31, 2019), https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3513639 ("Data on non-compete use in the U.S. are sparse…Researchers have conducted four surveys of non-compete use in the U.S., one of which is national in scope and covers a broad range of occupations, and three of which cover specific occupations. These surveys are the basis of many studies within the literature.").

[6] Noncompetes are considered "vertical" rather than "horizontal" restraints since they are intended to specify terms to an "upstream" input, *i.e.*, employees.

acquired trade secrets and other firm know-how from transferring that intellectual property to a rival.[7] While it has been argued that trade secrecy laws perform that function,[8] noncompetes may represent a more efficient mechanism to prevent proprietary knowledge transfers in certain circumstances, particularly when monitoring and the enforcement of trade secrets law is costly.[9] With a greater assurance that valuable, firm-specific information will not be transferred to a rival (at least for a period of time), noncompete agreements can encourage greater employer investments in employee training and human capital.[10] In essence, the ability to use noncompete clauses can solve the "hold up" problem where a firm is reluctant to invest *ex ante* in employees unless the firm is protected *ex post,* that is, after the employees have acquired their knowledge. Again, while trade secrecy laws or nondisclosure clauses could, in theory, be used instead of noncompetes, they would not be as effective or efficient as noncompetes in certain circumstances.

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[7] *See* Treasury Report, *supra* note 3, at 9–10 ("[N]on-competes can encourage additional economic activity and broader information sharing when trade secrets are significant. The training and screening explanations for noncompete agreements also suggest social benefits. If employee training is sufficiently enhanced by the availability of noncompetes, or if firms with unusually high separation costs are able to match more appropriately with employees, both employee and firm are better off.").

[8] *See, e.g.*, Alan Hyde, *Should Noncompetes Be Enforced?*, REGULATION, Winter 2010-11, at 6-11; OMI Petition, *supra* note 4.

[9] Arguably, the practice of *not* using noncompetes could be construed as not engaging in a sufficient effort to maintain secrecy and hence the protection of trade secrecy laws; perhaps this concern might be mitigated through the use of nondisclosure clauses.

[10] *See, e.g.*, Paul H. Rubin & Peter Shedd, *Human Capital and Covenants Not to Compete*, 10 J. LEGAL STUD. 93, 93 (1981) ("restrictive covenants were and are necessary in some circumstances to lead to efficient amounts of investment in human capital").

The above discussion is reminiscent of the vertical integration versus vertical control decision. While vertical contracts can be useful in mitigating hold up or other potential inefficiencies along a vertical supply chain, in some circumstances, vertical integration is the more efficient solution.[11] Even within vertical contracts, some mechanisms to ensure performance may be more effective than others, depending upon the context. For instance, one justification for exclusive dealing arrangements is to induce sufficient levels of retailer investments in various forms of promotion (display, product range and inventory maintenance, sales effort, *et cetera*). Even if there are alternative mechanisms available, they may not be as feasible or cost effective in certain situations. Thus, merely identifying alternative mechanisms to solve a potential employee investment-problem does not provide policymakers useful guidance as to which mechanism achieves the objective at the lowest social cost. Indeed, it may not be possible to determine *ex ante* the circumstances, in a generalizable way, under which noncompetes are the more efficient mechanism. In these circumstances, it is better to leave their use to the discretion of individual market participants—assuming there is full information on both sides of the bargain. Of course, if one side has excessive market

[11] *See, e.g.*, Benjamin Klein, Robert G. Crawford, & Armen A. Alchian, *Vertical Integration, Appropriable Rents, and the Competitive Contracting Process*, 21 J. OF LAW & ECON. 297, 298 (1978) ("The crucial assumption underlying the analysis of this paper is that, as assets become more specific and more appropriable quasi rents are created (and therefore the opportunistic behavior increases), the costs of contracting will generally increase more than the costs of vertical integration."); Oliver E. Williamson, *The Vertical Integration of Production: Market Failure Considerations*, 61 AM. ECON. REVIEW 112, 113 (1971) ("In circumstances, therefore, where protracted bargaining between independent parties to a transaction can reasonably be anticipated, internalization becomes attractive.").

power in the labor market, then that is a relevant consideration in determining the role that the noncompetes should play in that market.

Another potential benefit to noncompetes is that they can effectively sort employees into specific "types," *i.e.*, those who have a low versus a high probability of leaving a firm within a given time.[12] Only those with low probabilities of leaving will be willing to agree to a noncompete clause. In these circumstances, noncompetes can serve as a fairly low-cost approach to solve an information problem.

These potential benefits to the use of noncompetes are achieved, however, by limiting an employee's ability to switch jobs and to negotiate for better conditions and higher wages (assuming the best alternative employment opportunity is affected by the clause). It can even lead to prolonged unemployment if an employee is unable to find a new job that is not precluded by the noncompete. In addition, these restrictions on employees can increase the cost of entry or expansion by rivals in the downstream product market. Ultimately, these restrictions on labor touch upon a fundamental concern that employee freedom and mobility will be negatively affected without a sufficient offsetting benefit.

While these concerns are understandable, the relationship between mobility and efficiency in labor markets is not necessarily straightforward. It is often claimed that labor mobility is good for economic efficiency, but it is the *option*—not necessarily the

[12] *See* Treasury Report, *supra* note 3, at 8-9.

choice—to move that improves efficiency. Specifically, employee welfare is a function of various things—the most important typically being wages but also training and other developments of human capital; hence, the willingness of college and graduate students to intern for low or even no wages. Employees may be willing to trade off greater mobility for higher wages and/or greater opportunities to develop their human capital. For instance, a job that incentivizes a two-year commitment (*e.g.*, with a bonus payment at the end of the term) involves asking the employee to give up some mobility (or, more accurately, raises the cost of being mobile) in exchange for something the employee values more. Consequently, observations regarding changes in an employee's mobility do not, in of themselves, inform us about a particular employee's welfare or about the welfare of employees in the aggregate.

To be certain, a reduction in labor mobility can decrease economic efficiency. But when are employee freedom and mobility, and reductions in either or both, an antitrust problem? Are lower or higher wages an antitrust issue? It is important to emphasize that employee welfare is not the same as consumer or total welfare. In other words, a change in an input market does not necessarily translate into a harm in the output market. For instance, noncompetes may be associated with reduced costs to the firm and with lower prices and greater quantity in the output market. Indeed, Gurun et al. (2019) find that noncompetes reduced employee mobility but lowered costs and prices

to consumers.[13] This study represents the first large-scale effort to examine the effect of noncompetes based upon firm-level variation. They find that when firms in the financial advisory industry agree not to enforce noncompetes (through participation in an industry "protocol" that prohibits firms from taking legal action against employees), the following effects ensue: (1) net employee turnover does not increase but the cost of turnover increases because advisors can now take clients with them; (2) firms are more reluctant to discipline advisor misconduct for fear of advisors taking clients with them—which leads to more incidents of misconduct (more than a 40 percent increase); and (3) firms increased client fees by 14 percent. These results confirm the unavoidable fact that a change in welfare in an input market does not directly map onto a similar change in consumer welfare in the output market.[14] Those tradeoffs break the link between harm to employees and a reduction in total or consumer welfare.

Putting aside the larger question of how noncompetes affect either total or consumer welfare, the available empirical evidence is mixed even in terms of assessing how noncompete agreements affect employees. Among those who find welfare

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[13] Umit G. Gurun et al., *Unlocking Clients: Non-Compete Agreements in the Financial Advisory Industry*, (Kelley School of Bus. Research Paper No. 18-29, 2019), https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3132127.

[14] This has implications for any FTC rulemaking efforts in the area of noncompetes given that the bipartisan UMC Policy Statement tethers the definition of an "unfair method of competition" to "consumer welfare" and a rule of reason analysis. Therefore, evidence that a practice reduced wages, without more, would fall short of satisfying the Commission's own definition of an unfair method of competition. *See* Fed. Trade Comm'n, Statement of Enforcement Principles Regarding "Unfair Methods of Competition" Under Section 5 of the FTC Act (Aug. 13, 2015), (hereinafter UMC Statement), https://www.ftc.gov/system/files/documents/public_statements/735201/150813section5enforcement.pdf.

enhancing effects for employees, Lavetti et al. (2019) find that physicians who sign noncompetes tend to earn more because they are allocated more clients.[15] This leads them to conclude: "Whereas one concern about the use of NCAs [noncompete agreements] is that they could harm employees, these patterns suggest that bundling NCAs with incentive-based compensation contracts can overcome the impacts of reducing workers' bargaining power."[16] Garmaise (2011) finds that noncompetes reduce holdup, which increases the incentive for firm-sponsored training, but tends to decrease employees' incentives to invest in other general skills.[17] Conti (2014) combines a state-level measure of enforceability with data on employee and firm outcomes in order to compare high versus low enforceability regimes and finds the enforceability of noncompetes allows firms to engage in riskier R&D investments since concerns regarding leaks are mitigated.[18] Younge and Marx (2016) find that the ratio between a firm's market value and the value of its assets, *i.e.*, its Tobin's q, increased by 9.75 percent after noncompetes became enforceable in Michigan.[19]

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[15] Kurt Lavetti et al., *The Impacts of Restricting Mobility of Skilled Service Workers: Evidence from Physicians*, J. OF HUMAN RES. at 3 (Feb. 7, 2019), http://kurtlavetti.com/UIPNC_vf.pdf ("Using three years of longitudinal earnings data per physician, we estimate that NCAs increase the annual rate of earnings growth by an average of 8 percentage points in each of the first 4 years of a job, with a cumulative effect of 35 percentage points after 10 years on the job.").

[16] *Id.* at 42.

[17] Mark J. Garmaise, *Ties that Truly Bind: Non-competition Agreements, Executive Compensation and Firm Investment*, 27 J. OF LAW, ECON., AND ORG. 2, at 376-425 (August 2011).

[18] Raffaele Conti, *Do Non-Competition Agreements Lead Firms to Pursue Risky R&D Projects?*, STRATEGIC MGMT. J., at 1230-48 (July 7, 2014).

[19] Kenneth A. Younge & Matt Marx, *The Value of Employee Retention: Evidence from a Natural Experiment,* 25 J. OF ECON. & MGMT. STRATEGY 652 (2016).

On the other hand, there are a number of studies that find that noncompetes have a negative effect on employee wages. Balasubramanian et al. (2018) find that, after a ban on noncompetes for technology employees in Hawaii, mobility increased by 11 percent and new-hire wages increased 4 percent.[20] Similarly, Johnson et al. (2019), using a state-level panel data, find that moving from the 10th to 90th percentile in state enforcement of noncompetes is associated with at three to four percent decrease in wages and a nine percent decline in the probability of changing jobs.[21] Lipsitz and Starr (2019) examine the effect of Oregon's 2008 ban on noncompetes for low-wage workers and find that hourly wages increased 2.2 to 3.1 percent, on average, although, they find no effect for those with less than a high school degree.[22] Starr et al. (2018) find that, in industries with greater noncompete use and enforceability, wages and mobility are lower and there are fewer job offers.[23] Fallick et al. (2006) find evidence that employees in California's computer industry, but not in other industries, are more mobile compared to employees in other states—which suggests the effect of California's non

[20] Natarajan Balasubramanian, Jin Woo Chang, Mariko Sakakibara, Jagadeesh Sivadasan & Evan P. Starr, *Locked In? The Enforceability of Covenants Not to Compete and the Careers of High-Tech Workers* (U.S. Census Bureau Center For Econ. Studies Paper No. CES-WP-17-09, 2019), https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2905782.

[21] Matthew S. Johnson et al., 2019, *The Labor Market Effects of Legal Restrictions on Worker Mobility* (Sept. 22, 2019).

[22] Michael Lipsitz & Evan Starr, *Low-Wage Workers and the Enforceability of Non-Compete Agreements* (Dec. 2019), https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3452240.

[23] Evan Starr, Justin Frake & Rajshree Agarwal, *Mobility Constraint Externalities*, ORG. SCI. (forthcoming 2018), https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3027715.

enforcement of noncompetes may be limited to the tech sector.[24] Again, however, it is the option to be mobile—not actual labor mobility—that improves efficiency.

Importantly, Starr et al. (2019) find the effect of noncompetes on employee outcomes depends upon timing.[25] Specifically, when employees are aware of noncompetes before accepting an offer, wages are 9.7 percent higher, relative to employees without noncompetes, and the likelihood of receiving training is higher. In contrast, employees who agree to noncompetes after employment see no change in wages or training levels, relative to employees without noncompetes.[26] One potential implication is that, when employees are aware of noncompetes they may demand compensation to offset their loss of mobility to rivals within the scope of the noncompete. Thus, the conditions under which noncompetes are presented matters for employee outcomes.

In sum, the empirical evidence on the effect of noncompetes on employee welfare is still largely not settled—at least not to the degree that would allow one to draw broad policy conclusions. The evidence certainly suggests the potential for both harms and benefits to employees. It also suggests mixed results for the effect on total

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[24] Bruce Fallick et al., *Job-Hopping in Silicon Valley: Some Evidence Concerning the Microfoundations of a High-Technology Cluster* (NBER Working Paper 11710, 2005), https://www.nber.org/papers/w11710.pdf.
[25] *See* Starr et al., *supra* note 3, at 35 ("these results imply that policies that encourage the disclosure of all job-relevant information to employees before job acceptance may help employees receive appropriate compensation for giving up their right to compete."). *See also* Evan Starr, *Consider This: Training, Wages, and the Enforceability of Covenants Not to Compete*, 72 ILR REVIEW 783 (2019).
[26] *Id.* at 3.

welfare. During the recent FTC hearings on noncompetes, Lavetti (2020) similarly concluded we are "[s]till far from reaching a scientific standard for concluding NCAs are bad for overall welfare… Also [we] don't yet fully understand the distribution of effects on workers…Welfare tradeoffs are likely context-specific, and may be heterogeneous."[27]

We agree with the FTC's own staff economist, McAdams (2019) who describes the empirical literature as follows:

> Although the literature has made important strides in studying non-competes and their effects on workers, firms, and end consumers, further work is needed. Due to the limited availability of data and a paucity of natural experiments (e.g., law changes) to assess the impact of non-competes, much of the literature relies on cross-sectional comparisons of signers and non-signers, or high-enforceability states and low-enforceability ones. The more credible empirical studies tend to be narrow in scope, focusing on a limited number of specific occupations (e.g., executives) or potentially idiosyncratic policy changes with uncertain and hard-to-quantify generalizability (e.g., banning non-competes for technology workers in Hawaii). There is little evidence on the likely effects of broad prohibitions of non-compete agreements. Further research, perhaps exploiting more recent law changes or new sources of data, is necessary to establish the causal impact such agreements have on market participants.[28]

In sum, we conclude the overall state of the evidence is not adequate to support even a narrowly tailored rule aimed at prohibiting noncompetes for employees with low wages, let alone a broad ban on noncompetes. In time, however, as more empirical

[27] Kurt Lavetti, Economic Welfare Aspects of Non-Compete Agreements, Remarks at the Fed. Trade Comm'n Workshop on Non-Compete Clauses in the Workplace (Jan. 9, 2020), https://www.ftc.gov/system/files/documents/public_events/1556256/non-compete-workshop-slides.pdf.
[28] *See* McAdams, *supra* note 5, at 4.

evidence develops, there will be an opportunity to reevaluate this conclusion. Indeed, even over the past few years, there has been a significant number of new working papers looking at state-specific and industry-specific effects, which clearly demonstrate an opportunity to learn and rely on the laboratory of the states. Given the likelihood of further changes in state laws, these opportunities will surely grow over time.

Legal Treatment of Noncompetes

The legality of noncompetes is primarily governed by state law. According to Hausman and Lavetti (2019), "The permissibility of NCAs dates back to at least 1621 under English common law, and 39 US states still follow common law in determining the enforceability of NCAs."[29] Most states enforce noncompetes with a "reasonableness test," which balances protection of the employer's information and know-how against the injury to the employee.[30] In practice, the enforceability of these standards also depends upon whether employees are allowed to litigate and have not agreed to arbitration.

States vary in their enforceability regimes for noncompetes, depending upon a number of different factors: Whether noncompetes are enforceable for voluntary and non-voluntary work separations; whether consideration is required beyond the job

[29] Naomi Hausman & Kurt Lavetti, Physician Practice Organization and Negotiated Prices: Evidence from State Law Changes (Aug. 31, 2019) (unpublished manuscript), http://kurtlavetti.com/NCA_price_vc.pdf.
[30] *See* Treasury Report, *supra* note 3.

itself; whether the employer has a protectable or legitimate interest to justify a noncompete; and the treatment of these clauses by state courts.[31] Noncompetes are permitted in most states with the exception of California,[32] North Dakota,[33] and, Oklahoma.[34] Additionally, in 2019, the Vermont Legislature considered a bill that would prohibit all noncompete agreements.[35] Among states that allow noncompetes, thirty-two have adopted the equitable reform doctrine.[36] Nine states have adopted the blue pencil doctrine.[37] And three states have adopted the more restrictive red pencil doctrine.[38] Finally, there is a degree of uncertainty about the treatment of noncompetes in a few states and the District of Columbia.[39]

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[31] Broadly speaking, the different states have adopted three main regulatory systems or "doctrines" for dealing with employee noncompetes: (i) red-pencil doctrine, requiring that the court declare an entire noncompete contract void if one or more of its provisions are found to be defective under state law or precedent; (ii) blue-pencil doctrine, requiring that courts delete provisions of a noncompete contract that render it overbroad or otherwise defective, while retaining the enforceable subset of the contract; and (iii) equitable reform doctrine, providing that courts may rewrite a noncompete contract removing defective provisions. *See* Treasury Report, *supra* note 3; *see also* Beck Reed Riden LLP, *Employee Noncompetes, A State by State Survey* (Apr. 27, 2019), https://www.beckreedriden.com/wp-content/uploads/2019/04/Noncompetes-50-State-Survey-Chart-20190427.pdf.

[32] *See* CAL. BUSINESS & PROFESSIONS CODE § 16600 (1941).

[33] *See* N.D. CENT. CODE § 9-08-06 (effective Aug. 1, 2019).

[34] *See* OK STAT. § 15-219A (2014).

[35] *See* Vermont Bill H.1, H.R. 1, 2019 Leg. (Vt. 2019), https://legislature.vermont.gov/bill/status/2020/H.1.

[36] These states are Alabama, Alaska, Arkansas, Colorado, Delaware, Florida, Hawaii, Idaho, Illinois, Iowa, Kansas, Kentucky, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nevada, New Hampshire, New Jersey, New York, Ohio, Oregon, Pennsylvania, Rhode Island, South Dakota, Tennessee, Texas, Washington, West Virginia, Wyoming,. *See* Beck Reed Riden, *supra* note 31.

[37] These states are Arizona, Connecticut, Georgia, Indiana, Louisiana (if allowed by the noncompete), Maryland, Montana (blue pencil likely), North Carolina, and South Carolina (blue pencil likely). *See* Beck Reed Riden, *supra* note 31.

[38] These states are Nebraska, Virginia (although, portions can be enforced if the remaining restrictions are otherwise enforceable), and Wisconsin. *See* Beck Reed Riden, *supra* note 31.

[39] The District of Columbia had adopted reformation or blue pencil, in Vermont the treatment depends on the contract, and in New Mexico and Utah the issue is undecided. *See* Beck Reed Riden, *supra* note 31.

All states that have regimes to enforce employee noncompetes condition them upon the protection of legitimate business interests, such as trade secrets. Several states also include the protection of commercially sensitive information, such as contracts, client, or vendor lists. Finally, some states also specifically mention specialized and unique training as an interest that is protectable via a noncompete agreement.[40]

Given the potential for employees to find themselves unknowingly bound by a noncompete, some states have passed "consideration" laws,[41] which tie the enforceability of noncompetes to some sort of consideration, such as the employer providing higher wages, promotions, or training. Additionally, many states have adopted standards requiring that noncompetes be narrowly tailored in terms of time and geographic reach, and/or be applied only to "key" or professional staff.[42] Several states have passed legislation prohibiting noncompetes for certain low wage employees or for employees within specific industries.[43] In 2008, for example, Oregon banned the use of noncompetes for hourly employees.[44] Further, the Illinois Freedom to Work Act,

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[40] For instance, in Alabama, protectable or legitimate interests include: "unique training involving substantial business expenditure specifically directed to a particular agent, servant, or employee (if identified in writing as consideration for the restriction);" in DC "expert training;" in Florida "extraordinary or specialized training," etc. *See* Beck Reed Riden *supra* note 31, at 1, 3.

[41] Examples include Alabama, Massachusetts, Montana, Nevada, Texas. *See* Beck Reed Riden *supra* note 31, at 1, 7, 9-10, 16.

[42] *See* Beck Reed Riden *supra* note 31, at 3.

[43] *See e.g.*, MD. CODE LAB. & EMPL. § 3-716 328 (2019) (voiding noncompetes for employees earning less than $31,200 annually); WASH. REV. CODE § 49.62.020 (2019) (voiding noncompetes signed by employees making less than $100,000 annually and independent contractors earning less than $250,000 annually); COLO. REV. STAT. § 8-2-113 (2018) (physicians and executives are exempted).

[44] *See* OR. REV. STAT. § 653.295(1)(b); OR. REV. STAT. § 653.020(3). For an assessment of the impact of the Oregon law change on wages, *see* Michael Lipsitz & Evan Starr, *supra* note 22.

prohibits the use of non-compete agreements for employees who earn $13 an hour or less.[45] Similarly, Hawaii enacted legislation prohibiting the enforcement of noncompetes for technology employees,[46] and several states prohibit the enforcement of noncompetes for specific types of healthcare employees.[47]

This survey indicates that, with few exceptions, the enforcement of noncompete clauses depends upon the specific circumstance and context. There is broad recognition that noncompetes can have both welfare-enhancing and welfare-reducing effects, which is precisely the main finding of the economics literature. Courts generally balance considerations of legitimate business interests with the effect on employees and even the public good. In general, these agreements are treated more favorably when they are applied to skilled employees in order to ensure the protection of legitimate business interests, are disclosed *ex ante,* and are entered into for consideration.

What, then, is the proper course for the FTC with regard to restricting the use of noncompetes—even for a subset of the labor force, such as low wage employees. As the preceding overview clearly indicates, states have chosen to deal with noncompetes in

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[45] Illinois Freedom to Work Act, 820 ILL. COMP. STAT. 90/1. Similarly, in February 2019, the New Hampshire Senate approved a bill that would prohibit employers from requiring low-wage employees to enter into non-compete agreements, making such agreements void and unenforceable. *See* N.H. S.B. 197, Reg. Sess. (N.H. 2019). In Washington State, legislation banning noncompete agreements for employees that earn less than $100,000 per year. *See* H.R. 1450, 66th Leg., Reg. Sess. (Wash. 2019). In Maryland, legislation was enacted voiding noncompetes for employees earning less than $15 per hour or $ 31,200 annually. *See* Md. H.B. 38, Reg. Sess. (Md. 2019). Finally, the New York Attorney General has proposed legislation that would prohibit non-competes for employees earning below $75,000 per year, *inter alia*. *See* N.Y. Assembly B. A07864 (N.Y. 2017).

[46] *See* HAW. REV. STAT. § 480-4 (2011).

[47] *See* N.H. REV. STAT. § 329:31-a (2016).

very different ways. State laws and court decisions have developed over a substantial period of time and are continuing to develop. Before adopting any rule that would override those laws, the FTC should have a sound basis for believing it will be improving outcomes for the economy in general and for affected employees in particular.

Noncompete Clauses and Antitrust Law

Antitrust enforcement has proven to be a good tool for addressing problems in labor markets that involve collusive or coordinated practices, such as horizontal wage fixing or non-poaching agreements.[48] Thus far, however, we are aware of no individual case involving employee noncompetes having market effects that would call for antitrust intervention.

Because noncompetes are vertical restraints, they are analyzed under the rule of reason.[49] The competitive analysis typically involves a review of the reasonableness of the duration, and geographic coverage of the noncompete, and whether the restraint is reasonably related to a legitimate business purpose. Courts have considered as legitimate purposes, *inter alia*, protecting a purchaser's ability to realize the full value of

[48] *See California v. eBay, Inc*., 2014 WL 4273888 (N.D. Cal. Aug. 29, 2014); *see also* Dep't of Justice, Antitrust Div. & Fed. Trade Comm'n, *Antitrust Guidance for Human Resource Professionals* 4 (Oct. 2016), https://www.justice.gov/atr/file/903511/download; OECD, *Competition Concerns in Labour Markets – Background Note By the Secretariat*, at ¶ 10 (June 5, 2019), https://one.oecd.org/document/DAF/COMP(2019)2/en/pdf.

[49] *See* Cont'l T.V. v. GTE Sylvania, 433 U.S. 36, 59 (1977).

a purchased business and protecting an employer's valuable personal contacts or trade secrets.[50] Most of the cases deal with breaches contract rather than antitrust injury.

In the European Union, employee noncompetes have not been subject to antitrust investigations or sanctions by the Directorate General for Competition. However, if an employee noncompete were of a magnitude to merit EU scrutiny,[51] and assuming that employees could be considered "undertakings,"[52] the analysis would be similar to that in the U.S., *i.e.*, the conduct would be analyzed according to its effects in the market. The agreements would be analyzed under Article 101 of the Treaty on the Functioning of the European Union (TFEU) or, if the employer is considered dominant, under Article 102 of the TFEU.

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[50] For example, in *Golden Rd. Motor Inn, Inc. v. Islam*, the Supreme Court of Nevada acknowledged the free-rider justification for employee non-compete agreements but stuck down the non-compete because the covenant was overly broad, as it extended beyond what was necessary to protect the employer's legitimate interests. 376 P.3d 151, 155 (Nev. 2016). Similarly, in *Delaware Elevator, Inc. v. Williams*, the court rewrote a non-compete covenant in line with Maryland's law, limiting its scope from a 100-mile radius to 30 miles, and from three years to two years. 2011 WL 1005181, *9-12 (Del. Ch. March 16, 2011).

[51] *See* Eur. Comm'n, *Notice on Agreements of Minor Importance Which Do Not Appreciably Restrict Competition Under Article 101(1) of the Treaty on the Functioning of the European Union (De Minimis Notice)*, Official Journal of the European Union 57 (Aug. 30, 2014), https://eur-lex.europa.eu/legal-content/EN/TXT/PDF/?uri=OJ:C:2014:291:FULL&from=EN.

[52] *See* Case C-41/90, *Höfner and Elser v. Macrotron GmbH* [1991] ECR I-1979; *see also* Ioannis Lianos, Nicola Countouris & Valerio de Stefano, *Rethinking the Competition Law/Labour Law Interaction Promoting a Fairer Labour Market* (Centre for Law, Econ. and Society, UCL Research Paper Series, 2019), https://www.ucl.ac.uk/cles/sites/cles/files/cles_3-2019.pdf (explaining that the concept of "undertaking is widely interpreted as 'an entity engaged in economic activity.' It includes individual persons offering goods or services on a market where they bear financial risk attached to the performance of those services. However, an employee cannot be an undertaking as it does not exercise an autonomous economic activity, in the sense of offering goods or services on a market and bearing the financial risk attached to the performance of such activity." (quoting *Höfner and Elser)*).

There is likewise a complete absence of antitrust cases involving noncompetes among the Member States of the EU. For example, the Spanish Competition Authority recently reported to the OECD that: "no cases have been submitted which involve a concentration of employment demand power (or of purchasing employment) or in a certain entity in relation to a reference market made up of providers of work (workers)."[53] Similarly, the Portuguese authority reported to the OECD that is has not adopted any decision condemning undertakings in their role as employers for prohibited practices (agreements, concerted practices, and decisions by associations of undertakings) involving no-poach or wage-fixing agreements.[54] Nor is there any decisional precedent with regard to the potential applicability of Article 9 of the Portuguese Competition Act and Article 101 TFEU in Portugal. Croatia, on the other hand, reported two cases related to labor markets, both dealing with employee no-poach agreements, one vertical (where the company was found dominant) and the other horizontal, that ended with commitments by the investigated parties.[55]

In brief, the impact of employee noncompetes on competitive outcomes is uncertain and likely depends upon each particular industry and circumstance. There is a fundamental difference between employee welfare and consumer welfare. Given that

[53] OECD, *Competition Issues in Labour Markets –Note by Spain* (June 5, 2019), https://one.oecd.org/document/DAF/COMP/WD(2019)48/en/pdf.
[54] OECD, *Competition Issues in Labour Markets- Note by Portugal* (June 5, 2019), https://one.oecd.org/document/DAF/COMP/WD(2019)47/en/pdf.
[55] OECD, *Competition Issues in Labour Markets- Note by Croatia* (June 5, 2019), https://one.oecd.org/document/DAF/COMP/WD(2019)41/en/pdf.

the lodestar of antitrust remains the consumer welfare standard,[56] noncompetes should be considered of antitrust concern only when they raise consumer welfare issues.

Rulemaking Petitions

The Open Markets Institute, 19 labor and public interest organizations, and 46 individual advocates and scholars (OMI) petitioned the FTC pursuant to the Administrative Procedure Act, 5 U.S.C. §553 , and the Federal Trade Commission Act, 15 U.S.C. § 45, to issue a regulation prohibiting noncompetes (OMI Petition).[57] The OMI Petition proposes that "[t]hrough a rulemaking, the FTC should declare worker non-compete clauses to be an unfair method competition and classify them as per se illegal under the FTC Act."[58] This petition was further supported by a letter from of seven

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[56] Tad Lipsky, Joshua D. Wright, Douglas H. Ginsburg & John M. Yun, *The Federal Trade Commission's Hearings on Competition and Consumer Protection in the 21st Century, the Consumer Welfare Standard in Antitrust Law,* Comment of the Global Antitrust Institute, Antonin Scalia Law School, George Mason University (George Mason Law & Economics Research Paper No. 18-26, Sept. 7, 2018), http://dx.doi.org/10.2139/ssrn.3245912.

[57] *See* OMI Petition, *supra* note 4; *see also* Letter from Sen. Richard Blumenthal et al., *supra* note 4.

[58] *See* OMI Petition, *supra* note 4, at 49. This type of rulemaking initiative has received some support from FTC Commissioner Rohit Chopra:

> "Given the prevalence of forced arbitration provisions in many contractual agreements, private enforcement is almost non-existent with respect to these clauses and other restraints that may harm competition. That's why government action is so essential. The FTC has the authority to define "unfair methods of competition" by rule and is uniquely positioned to take action. Earlier this year, the Commission received a petition for such a rulemaking on non-compete clauses, a petition worthy of public consideration."

See Fed. Trade Comm'n, Opening Remarks of FTC Commissioner Rohit Chopra Future of Work Roundtable U.S. House of Representatives Committee on Education & Labor (Oct. 16, 2019), https://www.ftc.gov/system/files/documents/public_statements/1552143/chopra_-_opening_remarks_before_committee_on_education_labor_future_of_work_roundtable_10-16-19.pdf;

Democratic senators ("Senators' Petition") urging the FTC to use its rulemaking authority, "along with other tools" to protect employees against the proliferation of noncompete clauses.[59] The Senators' Petition reflects a widespread concern regarding the perceived harmful effects of noncompetes for employees; refers to the OMI Petition; and echoes similar arguments, including that employees lack the bargaining power to resist these clauses. Eighteen state Attorneys General issued a narrower recommendation that the FTC prohibit noncompetes for low-wage employees through rulemaking under Section 5 of the FTC Act.[60] Finally, senators of both parties have

See also Fed. Trade Comm'n, Comment Submission of Commissioner Chopra to Department of Justice Initiative on Labor Market Competition (Sept. 18, 2019), https://www.ftc.gov/system/files/documents/public_statements/1544564/chopra_-_letter_to_doj_on_labor_market_competition.pdf.

> "A rulemaking proceeding that defines when a non-compete clause is unlawful is far superior than case-by-case adjudication. The proceeding would allow a broad array of stakeholders, not just a plaintiff and a defendant, to contribute to the development of the law. Earlier this year, the Commission received a petition for rulemaking on non-compete clauses. I strongly support opening up a docket for public comment on this petition to aid the Commission in crafting any potential rulemaking proposals."

[59] *See* Letter from Sen. Richard Blumenthal et al., *supra* note 4.

[60] The 18 State Attorneys General recommend:

> "The FTC should consider using its Section 5 enforcement authority to stop the use of non-compete, non-solicitation, and no-poach agreements in many situations. At a minimum, we recommend that the FTC use its authority to ban intra-franchise no-poach agreements and non-compete agreements for low-wage workers. We understand that the FTC is studying such action right now. We further propose the FTC consider a ban on non-competes involving multi-sided platforms."

See Fed. Trade Comm'n Hearings on Competition and Consumer Protection in the 21st Century, Public Comments of 18 State Attorneys General on Labor Issues in Antitrust at 13 (July 15, 2019), https://oag.dc.gov/sites/default/files/2019-07/State_AGs_Comments_to_FTC_on_Labor_Issues_in_Antitrust.pdf

submitted federal legislative proposals that would either completely ban noncompetes or ban their application to low-wage employees.[61]

The OMI Petition proposes a dramatic shift of antitrust enforcement under Section 5 of the FTC Act.[62] In our view, the proposal is not consistent with the body of empirical evidence concerning the ambiguous effect of noncompetes not only on employees, but on competition and consumer welfare generally. The evidence, as discussed above, supports a rule of reason approach already embodied in the bipartisan FTC Unfair Methods of Competition Policy Statement.[63] The UMC Policy Statement also commits the FTC to use its Section 5 UMC authority to target practices that harm competition and consumer welfare, and not to pursue broader public policy goals.[64] Departing from the consumer welfare standard in a rule restricting or prohibiting

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[61] Connecticut Senator Chris Murphy proposed the "Workforce Mobility Act of 2018" that would prohibit noncompete agreements for most employees. *See* Workforce Mobility Act of 2018, S. 2782, 115th Cong., (2018). Florida Senator Marco Rubio proposed the "Freedom to Compete Act," that would ban the use of noncompete agreements for certain low-wage employees. *See* Freedom to Compete Act, S. 124, 116th Cong., (2019).

[62] The OMI Petition further advocates *per se* treatment of noncompetes under Section 1 of the Sherman Act. or at least treating these agreements as "inherently suspect" under Section 1 of the Shearman Act by applying the "truncated" rule or reason analysis in *PolyGram*. *See* OMI Petition, *supra* note 4, at 4, 49-53 ("Considering the documented harms and unconvincing business justifications for non-competes, the FTC should hold worker non-compete clauses to be an unfair method of competition and categorize them as *per se* illegal;" "Relying on Polygram, non-competes conduct should be considered presumptively illegal under the Sherman Act because they are 'inherently suspect owing to its tendency to suppress competition'."). These propositions must be rejected because "inherently suspect" practices must be reliably known to harm competition. *See Polygram Holding, Inc. v. F.T.C.*, 416 F.3d 29, 36–37 (D.C. Cir. 2005) (holding that "the rebuttable presumption of illegality arises not necessarily from anything 'inherent' in a business practice but from the close family resemblance between the suspect practice and another practice that already stands convicted in the court of consumer welfare.").

[63] *See* UMC Statement, *supra* note 14.

[64] *Id.*

noncompetes would depart from the UMC Policy Statement without a strong evidentiary basis for doing so, increasing uncertainty and strengthening charges that competition agencies are prone to political influence.[65]

Noncompetes, Disclosure, and the Role of Consumer Protection

Antitrust and consumer policy are complementary and reinforce each other in their overarching goal of enhancing consumer welfare.[66] The two policies, however, address consumer welfare from different perspectives.[67] Antitrust approaches the market from the supply-side, seeking to ensure that markets are efficient and competitive by prohibiting anticompetitive agreements, harmful conduct by monopolists, and anticompetitive mergers. Consumer protection, on the other hand, approaches markets from the demand-side, addressing, *inter alia*, information asymmetries between sellers and buyers, false or misleading advertising, and abuses that may derive from contracts with unclear or disproportionate terms. Consumer protection is coextensive with the FTC's "unfair and deceptive acts and practices"

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[65] *See* Seth B. Sacher & John M. Yun, *Twelve Fallacies of the 'Neo-Antitrust' Movement*, GEO. MASON L. REV. (forthcoming 2020), https://ssrn.com/abstract=3369013.

[66] *See* OECD, *supra* note 48; *see also* Joshua D. Wright, *The Antitrust/Consumer Protection Paradox: Two Policies at War with Each Other*, 121 YALE L.J. 2216 (2012); Mark Armstrong, *Interactions Between Competition and Consumer Policy*, COMPETITION POLICY INT'L, at 97, 100-12 (Spring 2008).

[67] *See* Julie Brill, *Competition and Consumer Protection: Strange Bedfellows or Best Friends?*, THE ANTITRUST SOURCE (Dec. 2010).

jurisdiction, which can be thought of as policing the market against conduct that distorts the manner in which consumers make decisions.[68]

Coordination of these policies can lead to greater consumer welfare and, applied consistently, each policy strengthens the other.[69] For example, in competitive markets, producers have incentives to provide better goods and services in order to attract customers away from their rivals. At the same time, when consumers are able to exercise their choices effectively, they can better discipline producers.

Antitrust and consumer protection policies are different, however, in their scope and in the types of conduct addressed.[70] Conduct prohibited by consumer protection law usually involves individual businesses acting in a way that has a direct effect on consumers, for example, by misleading them through false or deceptive advertising.[71] On the other hand, antitrust involves conduct, such as price-fixing and mergers, that affects an entire market.

Moreover, antitrust may raises challenges for consumer protection and *vice versa*.[72] For instance, consumer protection rules, such as prohibitions on comparative

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[68] *See* Timothy J. Muris, Former Chairman, U.S. Fed. Trade Comm'n, Remarks at Fordham Corporate Law Institute's Twenty-Ninth Annual Conference on International Antitrust Law and Policy: The Interface of Competition and Consumer Protection (Oct 31, 2002), https://www.ftc.gov/sites/default/files/documents/public_statements/interface-competition-and-consumer-protection/021031fordham.pdf.
[69] *See* OECD, *supra* note 48.
[70] *See* Brill, *supra* note 67 at 1.
[71] *See* J. HOWARD BEALES & TIMOTHY J. MURIS, STATE AND FEDERAL REGULATION OF NATIONAL ADVERTISING, at Ch. 2 (AEI Press, 1993).
[72] *See* Wright, *supra* note 66.

advertising, mandatory product standards, and price transparency requirements may affect competition by facilitating collusion or limiting competition between firms.

In brief, antitrust and consumer protection policies should not be comingled in such a way as to confuse either or both disciplines. Importing consumer protection goals into antitrust analysis risks weakening enforcement by introducing public policy considerations and tradeoffs unrelated to competition. That said, to the extent that employee harm is based upon information asymmetry, there is a potential role for consumer protection where the "consumer" in question is the employee faced with a noncompete agreement. While the literature in this area is fairly sparse, it stands to reason that employees are better able to assess tradeoffs if they are clearly presented with the terms of a noncompete clause before making an employment decision.

Conclusions and Policy Recommendations

We strongly believe that proposals urging the FTC to prohibit or restrict the use of noncompetes (even for a subset of the labor force) are deficient for several reasons. First, despite the recent increase in empirical studies, the evidence does not suggest a reliable and predictable link between the use of noncompetes and the effect on employee welfare. Second, changes in employee welfare do not map in a reliable way to changes in consumer welfare. The goals of antitrust policy, and of Section 5 of the FTC Act, are firmly grounded in the consumer welfare standard; deviating from that

standard would contradict the FTC's UMC policy, lead to inefficient enforcement, and increase legal uncertainty. Third, state laws already and extensively control the use of noncompetes; therefore, creating a new FTC rule would likely add complexity and uncertainty about how the rule would interface with existing state laws.

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Noncompete Agreements in the U.S. Labor Force

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Noncompete Agreements in the U.S. Labor Force*†

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Abstract

Using nationally representative survey data on 11,505 labor force participants, we examine the use and implementation of noncompete agreements as well as the employee outcomes associated with these provisions. Approximately 18% of labor force participants are bound by noncompetes, with 38% agreeing to at least one in the past. Noncompetes are more likely to be found in high-skill, high-paying jobs, but they are also common in low-skill, low-paying jobs and in states where noncompetes are unenforceable. Only 10% of employees negotiate over their noncompete, and about one-third of employees are presented with their noncompete after having already accepted their job offer. Early-notice noncompetes are associated with better employee outcomes, while employees who agree to late-notice noncompetes are comparatively worse off. Regardless of noncompete timing, however, wages are relatively lower where noncompetes are easier to enforce. We discuss these findings in light of competing theories of the economic value of noncompetes.

Keywords: noncompetes, employment law, transparency
JEL Codes: J4, J6, K31, L41, M5

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†Results from early versions of this paper are discussed in the U.S. Treasury report on noncompetes (U.S. Treasury, 2016) as well as the subsequent White House report (The White House, 2016).

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1 Introduction

Noncompete agreements ("noncompetes") are postemployment restrictions that prohibit departing employees from joining or starting a competing enterprise, typically within time and geographic boundaries.[1] Noncompetes have long faced significant legal hostility because of their often blunt prohibition on employee mobility (Blake, 1960), but they are nevertheless regularly enforced in the United States.[2] Spurred by anecdotes of unpaid interns and minimum wage sandwich makers signing noncompetes, policymakers in recent years have proposed dozens of legal reforms, including banning noncompetes for some or all employees and regulating the noncompete contracting process.[3] Yet relatively little is known about the actual *use* of noncompete agreements by employers because employee-level noncompete data are scarce.[4] In this study, we use nationally representative data from a survey of 11,505 labor force participants to answer three empirical questions: (1) What fraction and which types of employees enter into noncompetes? (2) What is the nature of the noncompete contracting process? And (3) how are noncompetes related to labor market outcomes, like training, wages, and job satisfaction?

Our empirical analysis is motivated by theoretical work in law and economics that considers the costs and benefits of employment contracts that limit an employee's future mobility. The traditional economics perspective has two key tenets. First, due to the inalienability of human capital (Hart and Moore, 1994), employers will be reluctant to invest in developing valuable information or specialized training—given that employees may be unable to compensate employers in advance for access to such information and training (Barron et al., 1999; Acemoglu and Pischke, 1999)—if employees can easily convey the value of any such investments to a competitor simply by taking a new job. Enforceable noncompetes solve this holdup problem by prohibiting departures

[1] Several examples of actual noncompetes are provided in Figures OE1, OE2, and OE3.

[2] All but three U.S. states enforce noncompetes (though to varying degrees) as long as they are protecting legitimate firm interests—such as trade secrets, client lists, or specialized training (Malsberger et al., 2012)—without unduly harming the employee or the public. See Online Appendix C for more on the enforceability of noncompetes.

[3] For a recent summary of noncompete proposals, see `www.https://www.faircompetitionlaw.com/changing-landscape-of-trade-secrets-laws-and-noncompete-laws/`.

[4] See generally Bishara and Starr (2016). Available noncompete data cover executives (Bishara et al., 2012) and engineers (Marx, 2011). There are also two recent papers about the use of noncompetes among physicians (Lavetti et al., 2019) and hair salon employees (Johnson and Lipsitz, 2019). A large literature studies the enforceability of noncompetes, but this work does not use data on actual noncompete use. See, for example, Balasubramanian et al. (2020); Marx et al. (2009); Stuart and Sorenson (2003); Samila and Sorenson (2011); Starr (2019); Starr et al. (2018); Conti (2014); Marx et al. (2015); Younge et al. (2014).

to competitors, which encourages employers to make these fragile but important productivity-enhancing investments (Rubin and Shedd, 1981; Posner et al., 2004; Meccheri, 2009). The second tenet is that employees will not agree to a noncompete unless an employer adequately compensates them (Callahan, 1985; Friedman, 1991), either upfront or through higher future wage growth representing part of the return on the employer's investment in the employee.[5]

In contrast, a more critical perspective recognizes that while noncompetes might solve incentive problems, they can also serve anticompetitive ends, including limiting wage growth by restraining labor-market competition from product-market competitors, retarding product-market competition by reducing information flows to competitors, and preempting future competition from departing employees (Krueger and Posner, 2018; Marx, 2018). Employers might even deploy noncompetes when they are entirely unenforceable (because they are not relying on actual enforceability to align incentives), hoping instead that the *in terorrem* effects of the contract will hold employees to their (unenforceable) promises (Sullivan, 2009; Starr et al., forthcoming; Blake, 1960). This view also challenges the notion that employees will be adequately compensated for entering into a noncompete: employers may impose a noncompete requirement only *after* an applicant has accepted an employment offer, often on the first day of the job, when the employee's bargaining power is much diminished (Arnow-Richman, 2006).

These contrasting views deliver different predictions about the incidence of noncompetes, the noncompete contracting process, and how noncompetes relate to labor market outcomes (Table 1 summarizes the predictions and findings). The more benign view tells us that noncompetes should be confined to occupations and industries that require specialized training or access to valuable information, should exist only in states that enforce noncompetes (because enforceability addresses the holdup problem), should involve negotiation, and should correlate with better employee outcomes (e.g., more training, higher wages), especially in enforcing states. The critical view contends that noncompetes should be common even among employees without access to trade secrets and in nonenforcing states, should follow a contracting process that involves little negotiation or transparency, and should be associated with worse labor market outcomes. In what follows, we describe our data and examine these competing predictions.

[5]There is also the view that noncompetes—a species of within-industry mobility friction—will not matter as long as skills and information are fungible across industries and moving costs are low (Sykuta, 2014).

2 Data

Our data come from a large-scale survey that we developed and administered in 2014 to a panel of verified respondents.[6] The sample population are labor force participants aged 18 to 75 who are employed in the private sector or in a public healthcare system or who are unemployed. The final sample contains 11,505 respondents drawn from all states, industries, occupations, and other demographic categories. We use an online survey instrument to collect these data, which offers several significant research-related benefits, such as the ability to ask technical questions in intuitive ways, easy access to millions of Americans who are comfortable responding to internet surveys, and significantly lower costs (and thus larger sample sizes). Yet surveying people online also comes with several important challenges, such as ensuring respondent reliability and representativeness, addressing item nonresponse, and even calculating the response rate.[7]

With regard to respondent representativeness, we built quotas into the surveying procedure to ensure our unweighted sample would be representative on key demographics. We also created ex post weights using iterative proportional fitting ("raking") to match the marginal distributions of many important variables in the 2014 American Community Survey (ACS).[8] Table 2 presents an unweighted and weighted comparison of our sample and data from the ACS. Our unweighted sample is higher earning, better educated, and more female than the population, but weighting appropriately virtually eliminates these differences. Unfortunately, weighting does not account for any nonrandom selection into our sample on the basis of unobservables.[9] With respect to data quality, we verify the reliability of respondent answers in several ways. In addition to examining long-answer and free-form survey responses directly,[10] we also carefully cleaned our raw data,

[6] We provide a focused discussion of our survey data here, with more details in our Online Data Appendix F. An even more extensive account of our data can be found in Prescott et al. (2016), which describes our investigation into sample-selection issues, hand-coding of occupations and industries, weighting methods, and imputation procedures.

[7] We vetted online panel providers by personally signing up as survey takers with many of these survey firms ourselves. Typically, after we completed an intake questionnaire, a representative called us a few days later at the phone number we listed and asked us questions to confirm the information we had submitted. In later discussions with various online panel providers, we learned that these companies drop applicants who give invalid phone numbers or who are not able to confirm their intake information.

[8] We considered a number of weighting schemes. See Tables 16 and 17 in Prescott et al. (2016) for more details.

[9] As for item nonresponse, note that if only respondents with an axe to grind about noncompetes finish the survey, we may find that noncompetes are associated with negative outcomes. To address this concern, we asked respondents at the end of the survey to indicate why they participated in the exercise, with an option that read: "I wanted to share my experiences with noncompetes." In our robustness checks, we drop these individuals and confirm that our results are robust to their exclusion.

[10] In Table OF1, we reproduce the self-reported job titles, occupational duties, and industries from 15 randomly selected respondents. The entries illustrate how seriously respondents took the survey. The respondent-provided job

identifying and removing repeat survey takers and excluding observations with intentionally non-compliant answers, among many other exhaustive measures that we took to address inconsistent and low-quality survey answers (see Data Online Appendix F).[11]

Respondent willingness to take and complete our survey is comparable to other surveys in the noncompete literature, although response rates are difficult to define and calculate in this setting because panel providers continuously send invitations to a superset of potential respondents—not all of whom are in our population of interest—until they receive a pre-specified number of "complete surveys." We can drop those who are not in our population of interest if they begin the survey (about 40%, see Table 2 of Prescott et al. (2016)), but we do not know and cannot determine whether those who receive an invitation but never start the survey are actually in our population of interest.[12] Given this limitation, the true response rate lies between two extremes: the final sample size over the number who started the survey within our population of interest (23%) and the final sample size over the total number of survey invitations (2%).[13]

3 The Use of Noncompetes

To identify employees bound by noncompetes, our survey instrument first defines a noncompete agreement (explicitly distinguishing a nondisclosure agreement, a common confusion) and asks respondents whether they have ever heard of such provisions (75.2% report yes). Our survey then

descriptions are quite detailed, as are the industry descriptions. We examined all of the survey data comprehensively by reviewing every one of the 11,505 free-form job titles, job duties, and industries by hand in the process of creating occupation and industry codes. It is clear that the vast majority of these respondents took care to write thoughtful responses to these questions.

[11]The final step of our cleaning process was the design and use of a flagging algorithm, which analyzes within-survey responses for internal inconsistencies. The flagging algorithm flags up to 21 different possible inconsistencies, including, for example, whether the respondent reports that the particular establishment or office at which they work is larger in terms of employee numbers than the employer's entire organization, whether there were missing responses, and others (see Table 7 of Prescott et al. (2016) for the full list). Only 1.8% of the final sample was flagged two or more times, with 82.2% receiving zero flags.

[12]The quotas we used to ensure representativeness exacerbate this problem because as the survey stays in the field and quotas begin to bind, respondents who would otherwise qualify for the survey become newly ineligible. Toward the end of the surveying period, when most quotas are full, the online survey company might send out thousands of e-mail invitations when only a handful of respondents satisfy the remaining criteria. In addition, our survey was marketed as a "work experiences survey," and online survey respondents skew toward being out of the labor force (see Table 12 of Prescott et al. (2016)), so it is likely that many who did not respond to the survey invitation were not in our population of interest.

[13]These numbers, while seemingly on the low side, are actually in line with and likely better than response rates to random-digit-dialing surveys, which were around 6% in 2018 (Kennedy and Hartig, 2019). To compare the rates we calculate to response rates for other surveys in this literature, see Table OB1. Moreover, in light of our arguably low response rates, it is important to recall that a low response rate is not problematic per se. Rather, bias results only when the reasons for nonparticipation are correlated with unobservables and outcomes of interest.

asks those who indicate some familiarity with noncompetes whether they have ever agreed to one (25% overall, 42% of those who are aware of them), and, if they answer yes, whether they are currently bound by one. For our 11,505 respondents, the unweighted distribution of those with a noncompetes currently is 15.2% "yes," 55.1% "no," and 29.7% "maybe," where the "maybe" category includes those who have never heard of a noncompete (24.8%), do not know if they have one (2.2%), do not want to say (0.23%), and cannot remember (2.5%).[14]

A key challenge in calculating noncompete incidence is that many in the "maybe" category may actually be bound by a noncompete. In fact, of those in our data who report having ever entered into a noncompete agreement, 8.8% also acknowledge having unknowingly signed at least one such provision that they discovered only at some later date. We address this uncertainty in two ways. First, we treat the "maybes" as their own category, which allows us to interpret the proportion of respondents answering "yes" as a lower bound on the incidence of noncompetes and the proportion of respondents answering either "yes" or "maybe" as an upper bound. Second, because the overall effect of a noncompete is averaged across those who are and who are not aware of their noncompete status, we use multiple imputation methods (King et al., 2001) to predict which respondents in the "maybe" category have a noncompete.[15]

Overall, our weighted estimates indicate that 38.1% of U.S. labor force participants have agreed to a noncompete at some point in their lives, and that 18.1%, or roughly 28 millions individuals,[16] currently work under one.[17] Table 3 shows the distribution of temporal and geographic restrictions of noncompetes in the U.S.: most noncompetes have durations of 2 years or less, while the geographic scope is frequently the state or the entire country (or there is no geographic limitation),

[14]The unweighted distribution for whether an individual has entered into a noncompete at some point in the past in our full sample is 31.5% "yes," 41.5% "no," and 27% "maybe." Among individuals who answer "yes" or "no" (to the question whether they have ever entered into a noncompete), almost all report being confident in their answer—i.e., either completely (74.2%) or fairly (23%) sure.

[15]We provide a more in-depth discussion in Section II.F of Prescott et al. (2016). To calculate our standard errors properly, we impute noncompete status among the "maybe" category 25 separate times. We then estimate our statistical models on each of the 25 different but complete datasets and follow by using Rubin's Rules to combine the resulting point estimates and correct the standard errors to reflect the variation in the imputed values (see Online Appendix F.5 for details). The benefits of multiple imputation methods are that they allow us to create an overall estimate of the use of noncompetes that accounts for the uncertainty surrounding the "maybe" group.

[16]The Bureau of Labor Statistics (BLS) puts the U.S. labor force at 156 million in July of 2014.

[17]The unweighted multiple imputation estimates signal that relatively few "maybes" are likely to have noncompetes in fact. We calculate that 19.9% of individuals (including 16% of the "maybe" respondents) are bound by noncompetes in 2014. These numbers are similar to two other estimates from smaller but more recent surveys: Krueger and Posner (2018), using a similar online survey methodology of 795 respondents in 2017, find a 15.5% incidence rate, while a 2017 survey in Utah of 2,000 employees reports an 18% incidence rate (Cicero, 2017).

though about 20% of individuals with noncompetes are uncertain as to the precise terms. Table 4 provides means—overall and by noncompete status—of important variables in our sample. Table 5 and Figures 1 to 8 document variation in noncompete use by a range of employee and employer characteristics, with additional calculations presented in Online Appendix Figures OA1 to OA5. The figures report the results of both our bounding approach and our multiple imputation strategy.[18] In Table 6, we also examine multinomial logit (Panel A) and linear probability models (Panel B) of employee noncompete status. We briefly describe variation in noncompete use by demographic characteristics before focusing our discussion on the empirical findings that are most relevant to the theoretical and policy debates over noncompetes.

Noncompete incidence differs widely across types of employees and employers. Table 5 shows that noncompetes are more than twice as common among employees of for-profit employers (19%) than they are among those working for private non-profits (9.8%). Men are slightly more likely than women to have entered into a noncompete at some point (39.7% vs. 36.3%) and to be currently bound by one (18.8% vs. 17.3%). Noncompetes are also a bit more frequent among the young (see also Figure 1) and in areas with greater product market competition (Figure 2). Lastly, while noncompetes are more routine among those with higher levels of education (Figure 3) and among those with greater annual earnings (Figure 4) or receiving a salary (Table 5), they are still prevalent among less-educated and lower-earning employees. For example, among those *without* a bachelor's degree, 34.7% of our respondents report having entered into a noncompete at some point in their lives, while 14.3% report currently working under one. Similarly, of those earning less than $40,000 per year, 13.3% are currently subject to a noncompete, with 33% reporting that they have acquiesced to one at some point. Table 6 confirms that these patterns hold in a multivariate framework. Importantly, these figures and Table 4 also demonstrate that a disproportionate share of the "maybe" category are low-earning with lower levels of education.[19]

Consistent with the traditional case for noncompetes, the provisions are more frequent in certain high-skilled occupations and industries, though they are still common in most other occupa-

[18]The size of the bars in the figures shows the size of the "maybe" category. The lower end of the bar represents the lower bound on the incidence of noncompetes, the upper end represents the upper bound on incidence, and the dark dot marks the multiple imputation estimate.

[19]For example, among those who report having less than a bachelor's degree, nearly 45% indicate that they do not know whether they have agreed to noncompete in the past, compared to approximately 20% of respondents with at least a bachelor's degree.

tions (Figure 5) and industries (Figure 6).[20] Per Figure 5, the occupations in which noncompetes are found most frequently are architecture and engineering (36%) and computer and mathematical vocations (35%). Farm, fishing, and forestry positions have the lowest incidence (6%).[21] With respect to industries, Figure 6 shows that noncompetes are most common in information (32%), mining and extraction (31%), and professional and scientific services (31%). Noncompetes are found least frequently in agriculture and hunting (9%) and the accommodation and food services industries (10%).[22] Relatedly, noncompete incidence is much higher among those who report possessing some type of trade secret or valuable information. Figure 7 breaks down noncompete incidence by type of "legitimate business interest."[23] Those who work with trade secrets are most likely to be bound by a noncompete (33–36%), while those who only work with clients or who have client-specific information are roughly half as likely to have a noncompete (15–16%).

Finally, we find very little difference in (unconditional) noncompete incidence between states that will and will not enforce these provisions (Figure 8). This is true even among single-location employers, where we find that the unconditional use of noncompetes in nonenforcing states is only slightly lower than in states that enforce noncompete agreements most zealously (14% vs. 16.5%). By comparison, multivariate results in Table 6 indicate that, comparing two observationally equivalent employees, noncompetes appear to be somewhat more common (4 to 5 percentage points) in the most vigorous enforcing states relative to nonenforcing states. The difference between the unconditional and conditional models suggests some role for geographic selection into the use of noncompetes based on employee and employer observables.

[20] We use two methods to identify the use of noncompetes across occupations and industries: First, we calculate the proportion of respondents who agree to a noncompete within a given occupation or industry. Second, we ask individuals to project how common noncompetes are within their occupations and industries, and then we aggregate those estimates into a single occupation- or industry-specific number. The idea behind using "projected estimates" as a way of estimating noncompete incidence is that an employee's knowledge of their occupation and industry as a whole captures more information than the employee's personal situation alone. See Rothschild and Wolfers (2013) for an example of this method in a voting context.

[21] Two indicia of the quality of our survey data are that legal occupations have the second lowest incidence level (10%) and that employees in these occupations are most likely to know whether they are bound by a noncompete. These facts are reassuring because one would expect that lawyers and legal support staff would be among the most careful readers of contracts and because the practice of law is the only occupation in which noncompetes are unenforceable in all states (Starr et al., 2018).

[22] With respect to the joint occupation-industry incidence distribution, Figure OA5 shows that the use of noncompetes is highest for technical occupations (computer, mathematical, engineering, architecture) in the manufacturing and information industries. Note that in the figure we only analyze occupation-industry cells for which there are at least 20 individuals in the sample in order to ensure that the results are representative.

[23] We define legitimate business interests as trade secrets, relationships with clients, and client information, such as contacts or marketing databases.

To provide some aggregate understanding across all of these characteristics, our simple multivariate model predicts that a salaried employee with a college degree, earning $100,000 per year, with access to the employer's trade secrets, and in a private for-profit firm, has a 44% likelihood of being a party to a noncompete. As a point of comparison, an employee paid by the hour without a bachelor's degree, in a private for-profit firm, earning $50,000 per year, and without access to the employer's trade secrets, has a 13% chance of being bound by a noncompete.

4 Negotiation and the Contracting Process

Table 7 presents descriptive statistics regarding the noncompete contracting process, including the extent of negotiation over noncompetes, when employers initially present noncompetes to applicants or employees, and whether employees consult with others before assenting to such a provision. Panel A shows that 61% of individuals with a noncompete first learn they will be asked to agree not to compete before accepting their job offer while more than 30% first learn they will be asked to agree only *after* they have already accepted their offer (but not with a promotion or change in responsibilities). This late notice appears to matter to employees. In a follow-up question to those who received late notice, 26% report that if they had known about their employer's noncompete plans earlier, they would have reconsidered accepting their offer.

Table 7 also shows that only 10% of employees report attempting to negotiate over the terms of their noncompete or asking for additional compensation or benefits in exchange for agreeing to such an employment condition. However, we find that the timing of noncompete notice is correlated with whether an individual makes an effort to bargain: 11.6% report negotiating when given early notice by their potential employer compared to just 6% of those given notice only after they have accepted their offer.[24] When presented with a noncompete, most respondents report just reading and signing it (88%), with a nontrivial fraction not even reading it (6.7%). Consultation with friends, family, or a lawyer is relatively uncommon (17%), but obtaining advice is strongly associated with attempting to negotiate.[25]

[24] By contrast, 31% of those asked to agree to a noncompete before a promotion or raise report negotiating over their noncompete, suggesting such circumstances allow employees a more favorable bargaining position.

[25] In unreported results, we also find that negotiation is twice as likely for those with a bachelor's degree relative to those without (13% vs. 6.2%) and that men are more likely to report negotiating than women (13% vs. 4.5%). Also, negotiation appears to be uncorrelated with noncompete enforceability—even after controlling for a host of characteristics such as employer size and employee age, gender, industry, occupation, and education.

In Table OB2, we report the reasons individuals cite for not attempting to negotiate over the terms of their noncompete (separately by the timing of notice). The top reasons for forgoing the opportunity to negotiate include that the terms were reasonable (52%) and the assumption that noncompetes were not negotiable (41%). Roughly 20% of employees fear creating tension with their employer or simply being fired if they try to negotiate.[26] In terms of heterogeneity by timing, those asked to agree not to compete after they have already accepted their offer are 9 percentage points less likely to report that they felt the terms were reasonable (55% vs. 46%) and are also 10 percentage points more likely to assume they could not negotiate (48% vs. 38%). In unreported tabulations, we also explore respondent beliefs about the consequences of refusing to agree to a noncompete. We asked respondents with noncompetes, "Would you still have been hired if you refused to sign the noncompete?" Only 11.4% answered affirmatively; 61.6% believed not, and 27% did not know. Taken together, the evidence in this section indicates that employers present (or employees receive) noncompete proposals as take-it-or-leave-it propositions.

5 Labor Market Outcomes

The traditional and critical perspectives on noncompetes offer different predictions about the extent to which employees with noncompetes should receive training and valuable information in their employment as well as whether employees who agree not to compete will be better off on the whole. In this section, we examine the conditional relationships between noncompetes and labor market outcomes. Given that contrasting views on noncompetes also highlight the role of late notice (as eroding employee bargaining power),[27] the enforceability of noncompetes (key to resolving the holdup problem), and effects over tenure (perhaps reflecting an upfront compensating differential), we also explore heterogeneity along these dimensions.

5.1 Empirical Approach

We begin by acknowledging that our analysis of the relationships between noncompete use and labor market outcomes (and the heterogeneity of these relationships across various contracting and

[26] For example, in an open text answer to a survey question, one respondent wrote "i needed the job [expletive], i wasn't trying to make any waves on the first day."

[27] We provide summary statistics by early and late notice in Table OB3.

legal dimensions) are best taken as descriptive and should not be interpreted causally. Noncompete use and the moderator variables we examine are endogenous.[28] Accordingly, any associations we observe may be at least partially due to reverse causation or selection on unobservables. To ease some concerns about this important limitation, we use several approaches to assess the sensitivity of our empirical results, including inspecting the robustness of our findings to the inclusion of a rich set of controls in our regression analysis, testing for selection on unobservables,[29] and asking respondents directly about their experiences with noncompetes.

Our investigation focuses on four critical employee outcomes: wages, training, access to information, and job satisfaction. Our main empirical specification takes the form:

$$Y_{iojs} = \beta_0 + \beta_1 Noncompete_i + \gamma X_{ij} + \omega_{o,j} + \alpha_s + \epsilon_{iojs}. \quad (1)$$

$Noncompete_i$ indicates whether the individual is bound by a noncompete. We study those who affirmatively report a current noncompete ("yes"), grouping "maybe" respondents with "no" respondents (and revisiting the robustness of our findings to this choice in our sensitivity analysis). Y_{iojs} refers variously to employment-related outcomes as reported by employee i in occupation o, industry j, and state s. We represent industry (NAICS 2-digit)-by-occupation (SOC 2-digit) fixed effects and state fixed effects with $\omega_{o,j}$ and α_s, respectively. In later models, we disaggregate our noncompete indicator to account for when the employee first learns about the employer's noncompete requirement (early- and late-notice), with individuals who do not have noncompetes

[28] We considered two possibilities for suitable instruments for noncompete status: differences in the enforcement regime and the projected incidence of noncompetes by *others in the same occupation and industry*. Both approaches yield implausible estimates (see Online Appendix D).

[29] Oster (2017) describes the key aspects of the test: If the R-squared statistic rises substantially as additional control variables are added and the estimate of the coefficient of interest remains stable, then there is less residual variation available to explain away a statistically significant estimate. If, however, the R-squared changes very little or the coefficient falls dramatically as controls are added to the model, then we should be less confident in the magnitude and direction of the estimate under review. Oster's (2017) test for selection bias delivers one parameter, δ, which indicates how powerful selection on unobservables would have to be, relative to the selection that occurs with respect to observables, to push the point estimate in question to zero. A value of $\delta = 1$ implies that selection on unobservables would have to be as important as selection on observables to fully account for an estimated nonzero coefficient while a value of $\delta > 1$ indicates that selection on unobservables would need to be even greater than selection on observables. To carry out the selection-bias test, we set the maximum R^2 at 30% higher than the R^2 in our fully saturated model, as Oster recommends. We also examine the reported δ terms by making comparisons (1) between a model with no controls and one with advanced controls and (2) between a model with basic controls (including state and occupation-by-industry fixed effects) and the advanced-controls model. We set the test's δ statistic equal to 1 as a natural cutoff to assess the stability of our results.

serving as the comparison group; we also examine models in which we interact $Noncompete_i$ with state-level noncompete enforceability and with length of tenure.[30]

Controls are given by X_{ij}, which we divide into "basic" and "advanced" groups in our analysis to gauge the sensitivity of our results to potentially confounding variables. Basic controls include demographic characteristics,[31] while the advanced controls address more noncompete-specific concerns.[32] These advanced controls in truth likely include some that are endogenous, potentially obscuring any causal mechanisms linking noncompete use and employee outcomes. Nevertheless, because we do not have reliably exogenous variation in the use of noncompetes to examine, it is informative to explore whether any noncompete-related patterns we observe survive when we condition on these potentially associated employment terms and conditions.[33]

5.2 Results

Table 8 reports the relationships we find between noncompete status and our four employment outcome: logged hourly wages and separate indicators for whether the respondent agrees or strongly agrees that their employer shares all job-related information with them, whether they received training in the last year, and whether they agree or strongly agree that they are satisfied with their

[30] We cluster our standard errors by state, tracking the level at which noncompetes are enforced (Moulton, 1990).

[31] Specifically, indicators for employee type (hourly, salaried, commission), gender, education, employer size, employer's multi-unit status, linear measures of an employee's hours worked per week, weeks worked per year, and their interaction, a third-degree polynomial in employee age, the logged number of employers in the county-industry cell, and the logged unemployment rate and labor force size in the state and year in which the employer hired the respondent (Beaudry and DiNardo, 1991). When necessary, logged variables take a log of the value plus one.

[32] Because noncompetes and other postemployment restrictive covenants (nondisclosure agreements, nonsolicitation provisions, and similar devices) frequently occur together (see Table 4), we disentangle and isolate any relationship between noncompetes and outcomes by controlling for these related provisions. If the use of postemployment provisions generally correlates with employer or employee quality or sophistication, controlling for them also accounts for any residual quality not addressed by our other controls. In addition, we include controls for poaching rates to and from the employer and within the industry generally to address employer heterogeneity in quality and employee-mobility patterns (for example, some employers are more likely to have their employees poached by competitors and so may be more likely to use noncompetes and may also pay different wages). We also control for other HR benefits, such as whether the employer offers a retirement plan, health insurance, paid vacation, sick leave, and life insurance. The inclusion of the HR-type benefits—retirement plan, paid vacation, sick leave, and life insurance—reduces the sample size from 11,462 to 11,010. Excluding these variables produces results that are nearly identical to our reported coefficients in terms of statistical significance and magnitude. See Tables OB4–OB7. Individuals with special access to sensitive information or who are predictable "flight risks" may also be more likely to have both noncompetes and higher earnings, so we control for the number of employers the employee has had in the last 5 years (a baseline measure of employee mobility) and the types of confidential information the employee possesses (for example, access to trade secrets or client information).

[33] In Online Appendix Tables OB4–OB7, we add our advanced controls sequentially so we can more precisely understand which, if any, shift our estimated noncompete coefficients.

employment.[34] Our baseline results in Panel A with our basic controls show that noncompete agreements are associated with positive differentials in wages and training. However, including our advanced controls reduces the training differential to near zero and causes the wages differential to fall from nearly 11% to 6.6%.[35] These results imply that certain advanced controls are strongly correlated with the use of noncompetes and these outcomes.

Panel B of Table 8 demonstrates that our mainly insignificant baseline results in Panel A are driven by heterogeneous associations that run in opposite directions, depending on when an employee receives notice of their noncompete. Focusing first on those who learn of their noncompete before they accept their job offer, our most saturated model indicates that these employees have 9.7% ($e^{0.093}$) higher earnings, are 4.3 percentage points more likely to have information shared with them (a 7.8% increase relative to the sample average), are 5.5 percentage points more likely to have received training in the last year (an 11% increase), and are 4.5 percentage points more likely to be satisfied in their job (a 6.6% increase) relative to those employees without a noncompete. In contrast, those presented with a noncompete after they accept their offer (excluding those furnished with a noncompete following a promotion or a change in responsibilities) appear to receive no observable boost in wages or training, are 13.4 percentage points less likely to have had information shared with them (a 24% reduction), and are 8.5 percentage points less likely to be satisfied in their employment (a 12.5% reduction). In all specifications but one,[36] within-model tests confirm that those who learn of their noncompete from their prospective employer before they accept that employer's offer do statistically significantly better (in terms of compensation, training, access to information, and satisfaction) relative to those who learn of and acquiesce to their noncompete only after they accept their employment offer.[37]

[34] For each dependent variable, we report results with basic controls and fixed effects as well as results with advanced controls. The results of the selection test (δ) for the comparison to a model with no controls is given in square brackets ('[ ]'), while the comparison between the models with basic and advanced controls is given in curly brackets ('{ }').

[35] As expected, given the large coefficient swings across these two models, the selection tests for the model with advanced controls confirm that we ought to be worried about selection on unobservables explaining our results. For example, when our model of logged hourly wages with advanced controls is compared to an otherwise equivalent model with no controls, the δ term is 0.497, implying that selection on unobservables would only need to be half as important as selection on observables to explain away our estimated coefficient on noncompete status.

[36] As we show in Table OB4, the lack of statistical significance on the before-after difference in the association between noncompete status and logged hourly wages only occurs when we control for HR benefits.

[37] The selection tests show that the statistically significant results for the late-notice category all have $\delta > 1$, while the results in the early-notice category are somewhat more sensitive (except in the satisfaction specification). For example, our results regarding hourly wages for the early notification group do appear rather sensitive to our advanced controls ($\delta = 0.275$), signifying that unobservables may more plausibly account for these estimates.

Given the limitations implicit in the cross-sectional nature of our data, we also study employee beliefs about what they were promised by and what they received from their employer for agreeing to their noncompete, as a way to independently—although only tentatively—corroborate the notice-timing differentials that we report in Table 8. The results, which we record in Table OB8, document that employees are rarely promised anything by their employers for agreeing to a noncompete, and, in fact, most of our survey respondents report having received nothing in exchange for their willingness to be bound by one. Moreover, as in Table 8, our findings indicate that employees who enter into late-notice noncompetes are relatively less likely to be promised and less likely to receive anything in exchange for their commitment not to compete.[38]

5.3 Sensitivity Analyses

To probe the robustness of the relationships we observe between noncompete status and employee outcomes, we investigate the consequences for our findings of treating the "maybe" scenarios as a separate contracting category as well as using multiple imputation to reassign members of the "maybe" group. Both approaches yield very similar results with respect to our notice-timing analysis, though the generally positive association that we estimate in Panel A of Table 8 between noncompetes and wages largely disappears when we use multiple imputation (see Online Appendix Tables OB9 and OB10). We also rerun our analysis without incorporating sample weights and find that none of our results is sensitive to weighting (see Table OB11). In Online Appendix Table OB12, we drop the respondents who indicated they took the survey to discuss their noncompete. Our timing results remain robust to this exclusion, though again the average main effect of a noncompete on wages mostly evaporates (as in the multiple imputation analysis). Finally, in Online Appendix Table OB13, we examine a related set of subjective employee outcomes, including perceived job security, the employer's commitment to upgrading the employee's skills, and whether the employee would consider returning to their employer if they were ever to leave. The results are broadly consistent with our earlier findings.[39]

[38]The precise question in Panel A is: "*Which of the following benefits did your employer promise you [beyond employment alone], either explicitly or implicitly, in exchange for signing the noncompete?*" The precise question in Panel B is "*Regardless of what your employer did or did not promise, which of the following tangible benefits do you believe you have received because you signed a noncompete?*" The survey instrument captures objective outcome measures before it asks these more subjective questions so as not to contaminate the objective measures.

[39]Individuals who become aware of their noncompete upfront are more likely to report that their employer is committed to upgrading their skills relative to those who receive late notice. We also find that those who receive

5.4 Heterogeneity by Tenure and Noncompete Enforceability

In Figure 9, we study whether notice-timing differentials vary by tenure, cognisant that interpreting results later in tenure is troublesome given that tenure itself is endogenous to noncompete status (Starr et al., forthcoming). Within each tenure bin, we rerun our timing specification and report the coefficient and the 90% confidence interval on our early- and late-notice coefficients relative to the baseline outcome for individuals without a noncompete. Early notice is associated with positive compensating earnings differentials early in tenure (Panel A), with higher (but imprecisely estimated) probabilities of receiving training. We also observe negative job-satisfaction and information-access differentials within the first five years for those who agree to their noncompete after accepting their offer of employment (relative to those without noncompetes).

Given the importance of noncompete enforceability for theories justifying noncompetes as a solution to the employer's investment holdup problem, and given that previous empirical work on noncompetes has relied heavily on state-level enforceability,[40] we also estimate models examining the differential relationship of noncompetes in states where such provisions are relatively more or less enforceable.[41] In Table 9, we report estimates with and without state fixed effects (which, when included, subsume the main effect of enforceability). Consistent with prior research examining noncompete enforceability and wages but inconsistent with our main effect of noncompetes, we find that noncompetes in higher enforceability regimes are associated with relatively lower earnings (Balasubramanian et al., 2020; Garmaise, 2009). We also discover that noncompetes in states that are more likely to enforce them are associated with more training, as in Starr (2019). Panel B shows that the negative effects on wages appear invariant to the timing of noncompete notice. By contrast, the relative training benefits we observe in column (6) of Panel B accrue primarily to those who receive early notice of their noncompete.

late notice are less likely (than someone without a noncompete) to consider returning to their employer. Late notice is always associated with statistically significantly worse outcomes relative to early notice.

[40] We discuss noncompete enforceability and measures from a recent study in Online Appendix C.

[41] We use the enforceability measure developed in Starr (2019), which is denominated in standard deviations from a mean enforcement score of zero, and we modify our main timing specification by adding enforceability and its interaction with noncompete status as regressors.

6 Discussion and Conclusion

Motivated by renewed and widespread legislative interest in noncompetes as well as the longstanding debate over their value, our study brings new data and several new findings to the academic and policy conversations about noncompetes and related provisions that regulate employee behavior post-termination: How common is such contracting? What does it look like in practice and what types of employees are bound and to what kinds of employers? How does it relate to employee outcomes? In this section, we consider how the evidence we uncover with respect to noncompete incidence, contracting, and associated labor market success comports with predictions from the traditional and more critical perspectives on noncompetes.

Several of the facts we document are consistent with the traditional economic perspective, which views the noncompete as an efficient contracting device. For instance, our findings that noncompetes are more common in relatively technical jobs and among employees with access to trade secrets aligns with the hypothesis that noncompetes can be effective at protecting valuable information and training, thereby encouraging efficient employer investments. Moreover, our evidence that employees with early notice of a noncompete are compensated—with higher wages, more training, information, and job satisfaction—is compatible with theories that identify noncompetes as a solution to a holdup problem (Rubin and Shedd, 1981; Acemoglu and Pischke, 1999).[42] Our result that employees with early-notice noncompetes have higher wages earlier in tenure is also consistent with an upfront compensating differential (Callahan, 1985).

But the frequency of noncompetes among low-wage employees without access to trade secrets and the lack of negotiation in the contracting process hint at more anticompetitive rationales for the use of noncompetes by employers. We observe, for instance, that late-notice noncompetes are not associated with any additional compensation or training but instead appear to be linked to lower job satisfaction. Heterogeneous associations by noncompete enforceability further challenge the traditional economic perspective. The ability to enforce noncompetes should encourage greater noncompete use, more investment, and higher wages, but employers use noncompetes virtually as often in states where they are clearly unenforceable. Furthermore, while greater enforceability is associated with more training for individuals with early-notice noncompetes, the wage premium

[42]The fact that this noncompete-associated boost in training appears to come earlier in tenure imply that employers may use noncompetes to differentiate "stayers" from "leavers" (Loewenstein and Spletzer, 1997).

for agreeing to a noncompete also diminishes with enforceability, regardless of noncompete timing. This pattern is consistent with the idea that enforceability creates incentives for employers to invest in their bound employees, but it is at odds with the supposition that employees should likewise benefit from agreeing to such a provision.[43] Importantly, these enforceability-specific findings with respect to wages and training also align with prior work studying the effects of noncompete enforceability (Starr, 2019; Balasubramanian et al., 2020).[44]

Our empirical work answers several questions about the use of noncompetes, the contracting process, and labor market associations, but unresolved endogeneity concerns related to noncompete status and timing raise significant questions about how best to interpret our results. For example, we are unable to rule out the possibility that some unobservable association explains our outcome results—such as unobservably "good" employers using early-notice noncompetes and unobservably "bad" employers using late-notice noncompetes. Some of our findings also beg important questions. For instance, if indeed employers can use late-notice noncompetes to avoid compensating employees for giving up their right to compete (and somehow employees do not anticipate this tactic), then why are all employers not springing noncompetes on new employees? Potential explanations include the possibilities that late notice may produce low morale and lower productivity in some contexts and that, if suing to enforce a noncompete is a realistic possibility, judges may look down on any employer giving late notice. We search for determinants of noncompete timing in Table OB14 but find few predictive relationships.

There are several additional limitations to our work that we hope future research will address. First, given the lack of information on the actual use of noncompetes (and related provisions) across the labor force, and the possibility that our online survey approach may not generate data truly representative of the population, future survey efforts to collect longitudinal data on noncompete contracting, which could allow for the study of employee and employer outcomes over time, are sorely needed.[45] Relatedly, our finding that lower-earning employees are less likely

[43] This training finding is also consistent with the idea that employers may use early-notice noncompetes in cases where they may need to convince a judge of an agreement's reasonableness.

[44] While we designed our research to assess the discrepancies between the two main perspectives on noncompetes, we can also rule out the possibility that employers are using noncompetes as a way to sort between committed and uncommitted employees. Figure OA1 shows that employees are no more likely to accept a noncompete if they plan to stay indefinitely versus just a few years, and Figure OA2 similarly finds that noncompetes are only slightly more common when an individual has had many employers in the last 5 years.

[45] Data sets that already collect longitudinal data on employee mobility and entrepreneurship, such as the NLSY or the PSID, would be well suited to undertake this task. Companies such as Glassdoor.com or Indeed.com could

to know whether they are bound by a noncompete raises some uncertainty about our incidence results, and employer-level survey data or actual contracts could help resolve this ambiguity.

These remaining questions notwithstanding, we make several important contributions to our collective understanding of postemployment contractual restrictions and to the related body of work on transparency (Card et al., 2012; Harris, 2018) and labor market frictions (Naidu, 2010). Most concretely, we build on several occupation-specific studies (Marx, 2011; Schwab and Thomas, 2006) to document that noncompetes extend to every corner of the labor market. We also empirically characterize the typically take-it-or-leave-it contracting process surrounding noncompetes, and provide correlational evidence that noncompetes are not uniformly associated with better (or worse) employee outcomes—depending on the timing of notice in the contracting process and a noncompete's enforceability. Overall, the story about noncompetes that emerges from our data is complex and nuanced, drawing on both of the literature's dominant perspectives.

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also add a question on noncompetes in their intake survey and report it to interested job seekers to reduce the information asymmetry regarding the use of noncompetes.

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Figures

Figure 1: Noncompete Incidence by Age

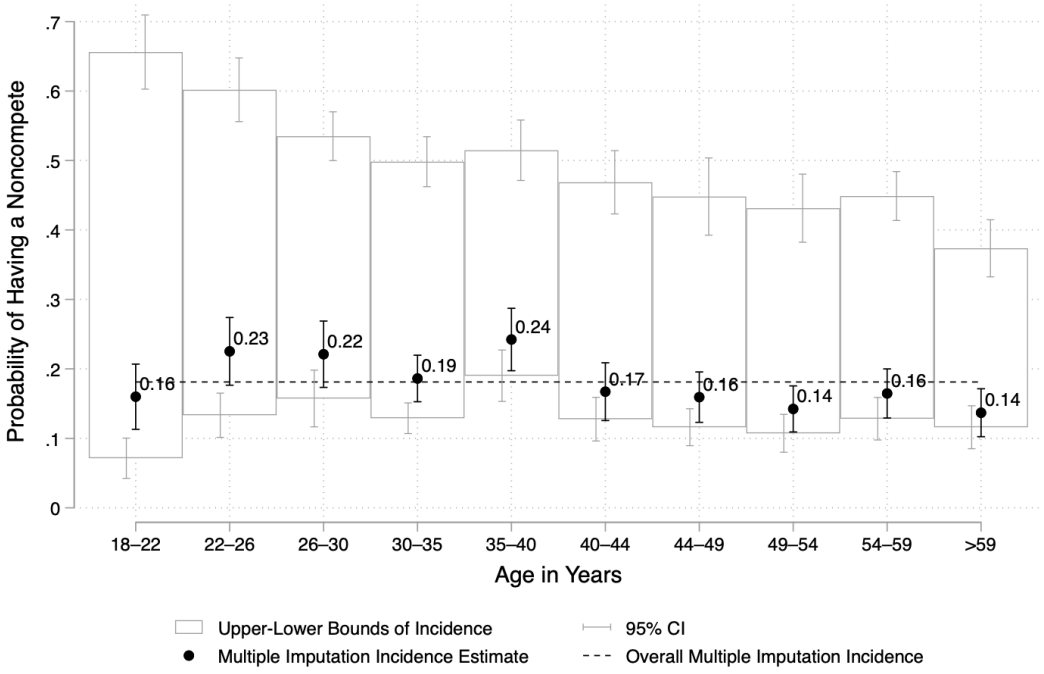


Figure 2: Noncompete Incidence by Number of Employers in County-Industry

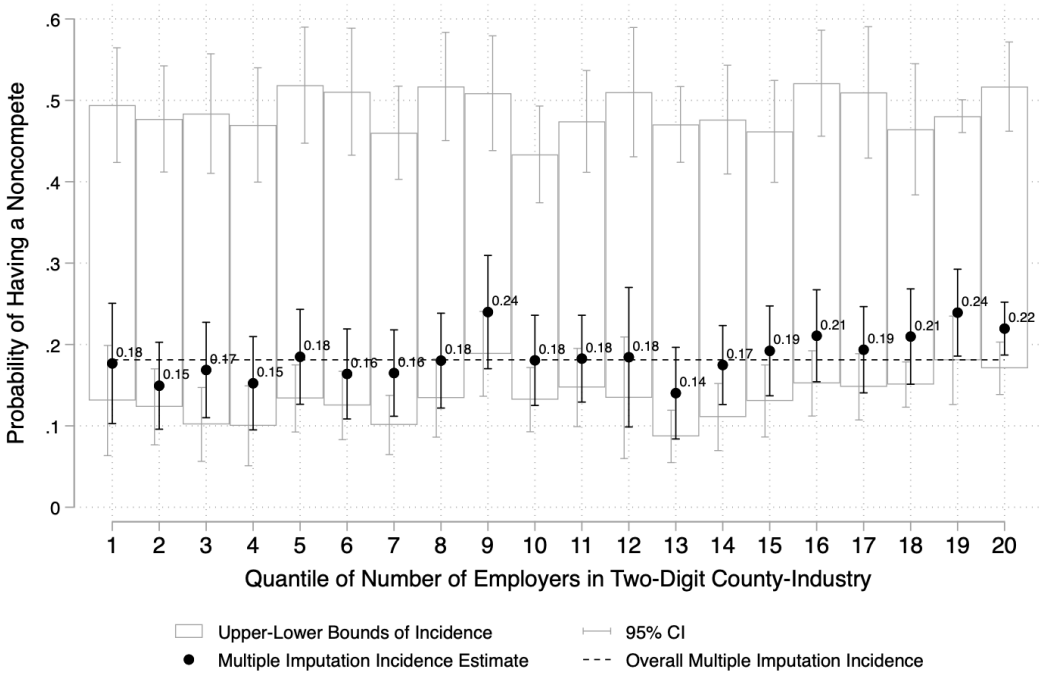


Figure 3: Noncompete Incidence by Education Level

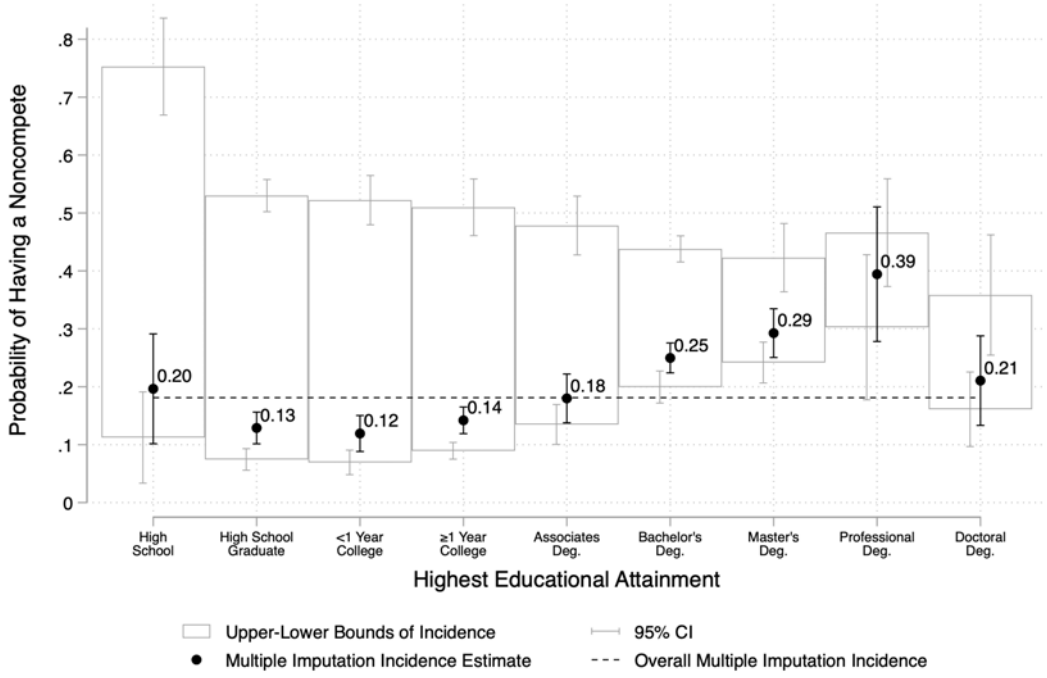


Figure 4: Noncompete Incidence by Employee Annual Earnings

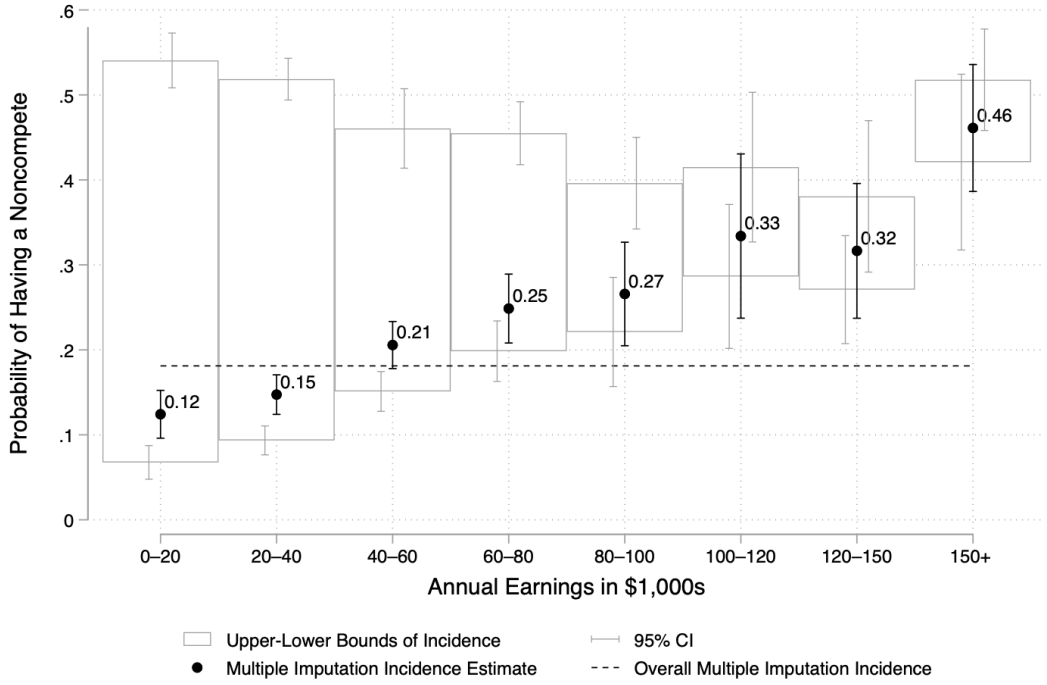


Figure 5: Noncompete Incidence by Occupation

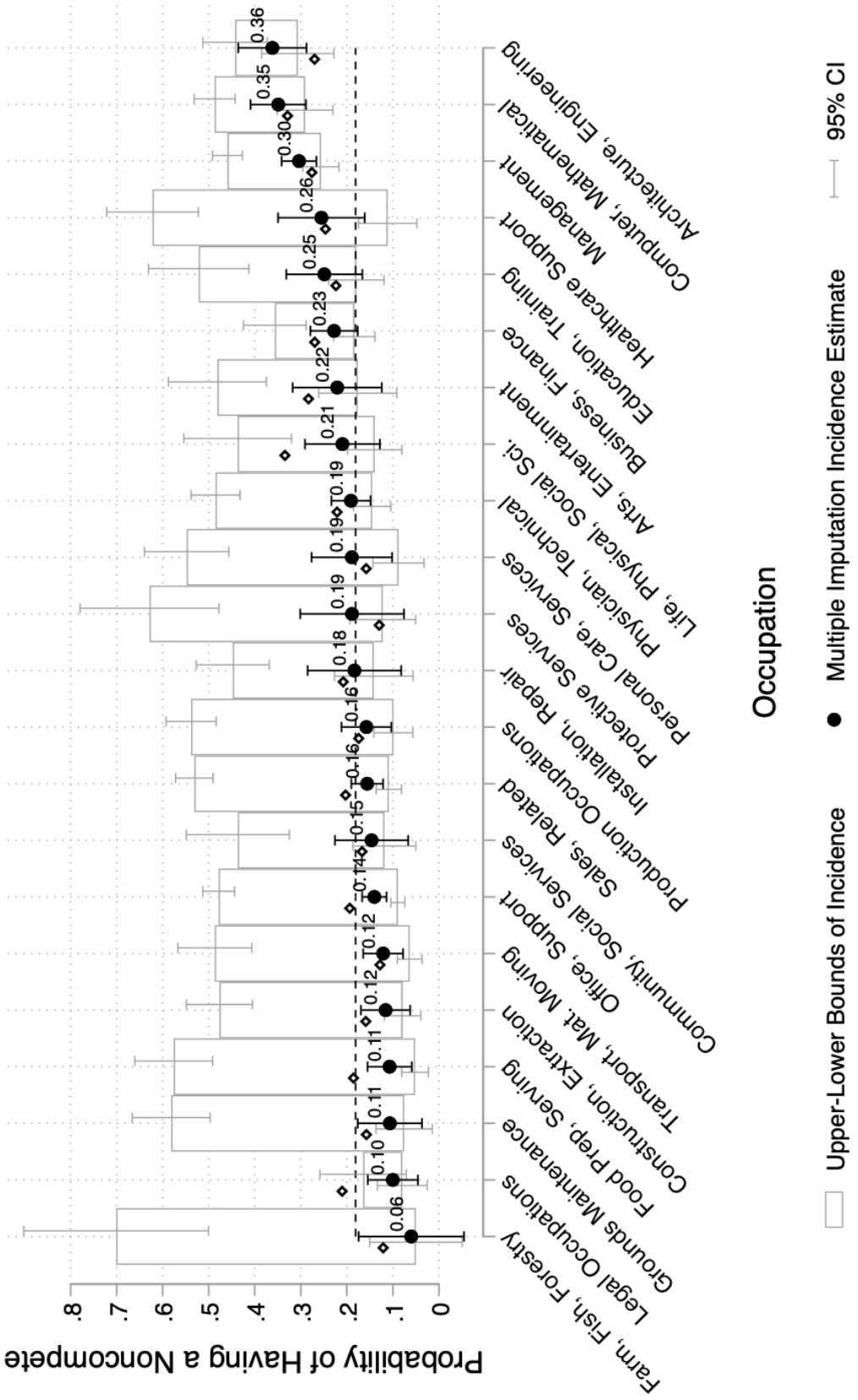


Figure 6: Noncompete Incidence by Industry

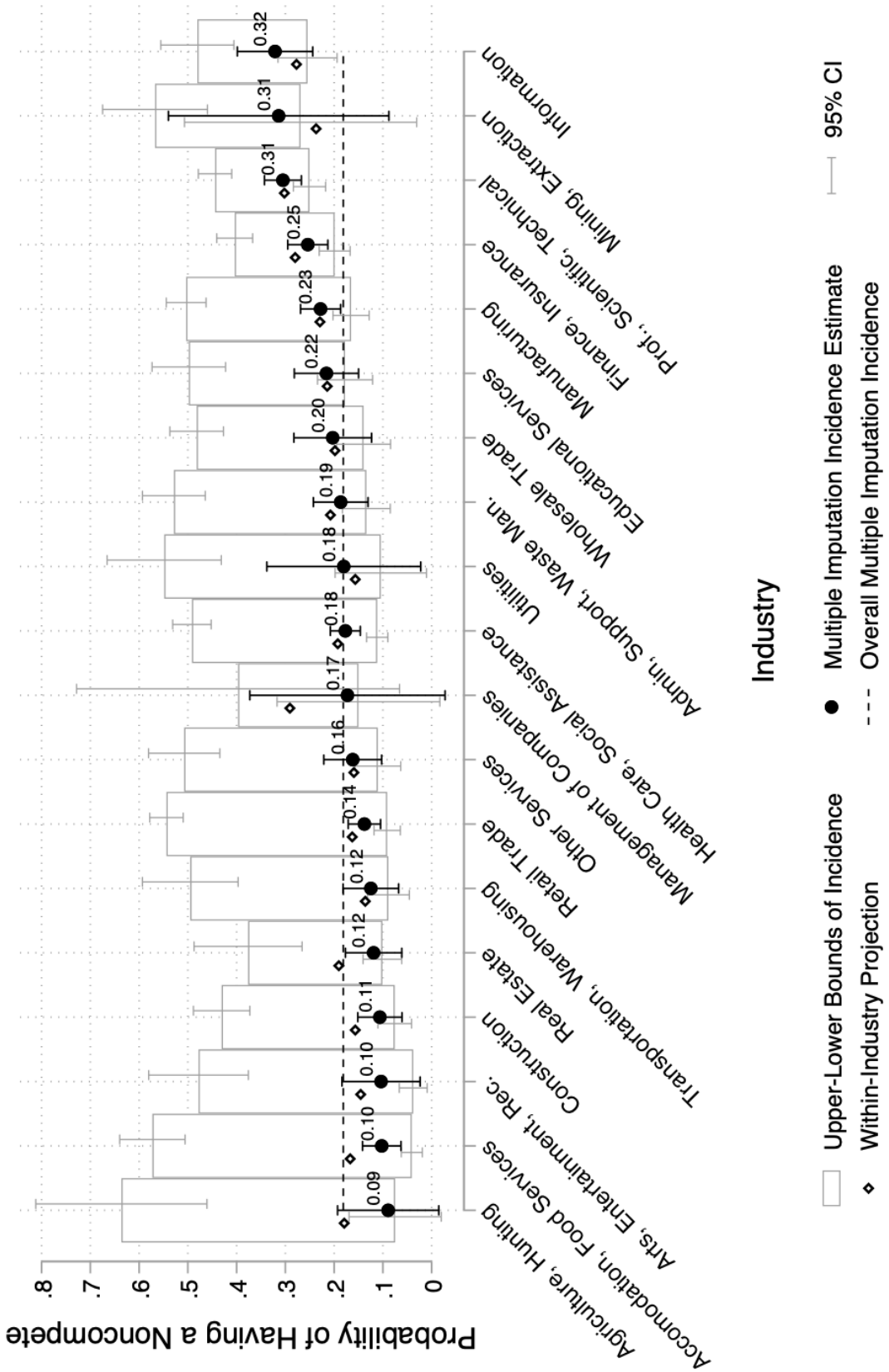


Figure 7: Noncompete Incidence by Legitimate Business Interest

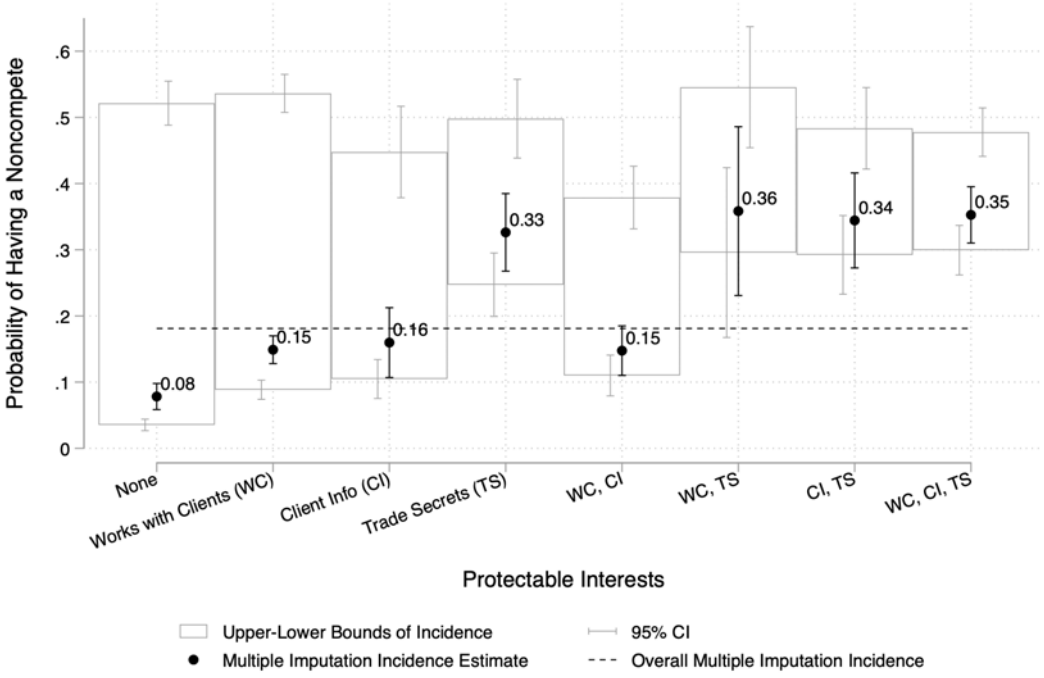


Figure 8: Noncompete Incidence by Noncompete Enforceability

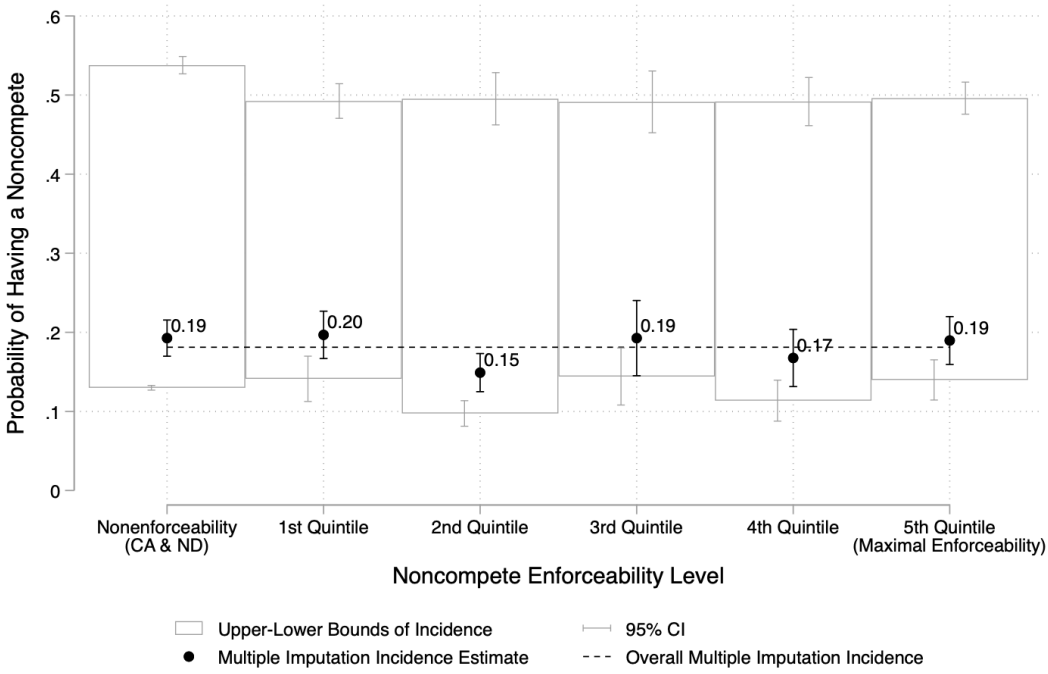
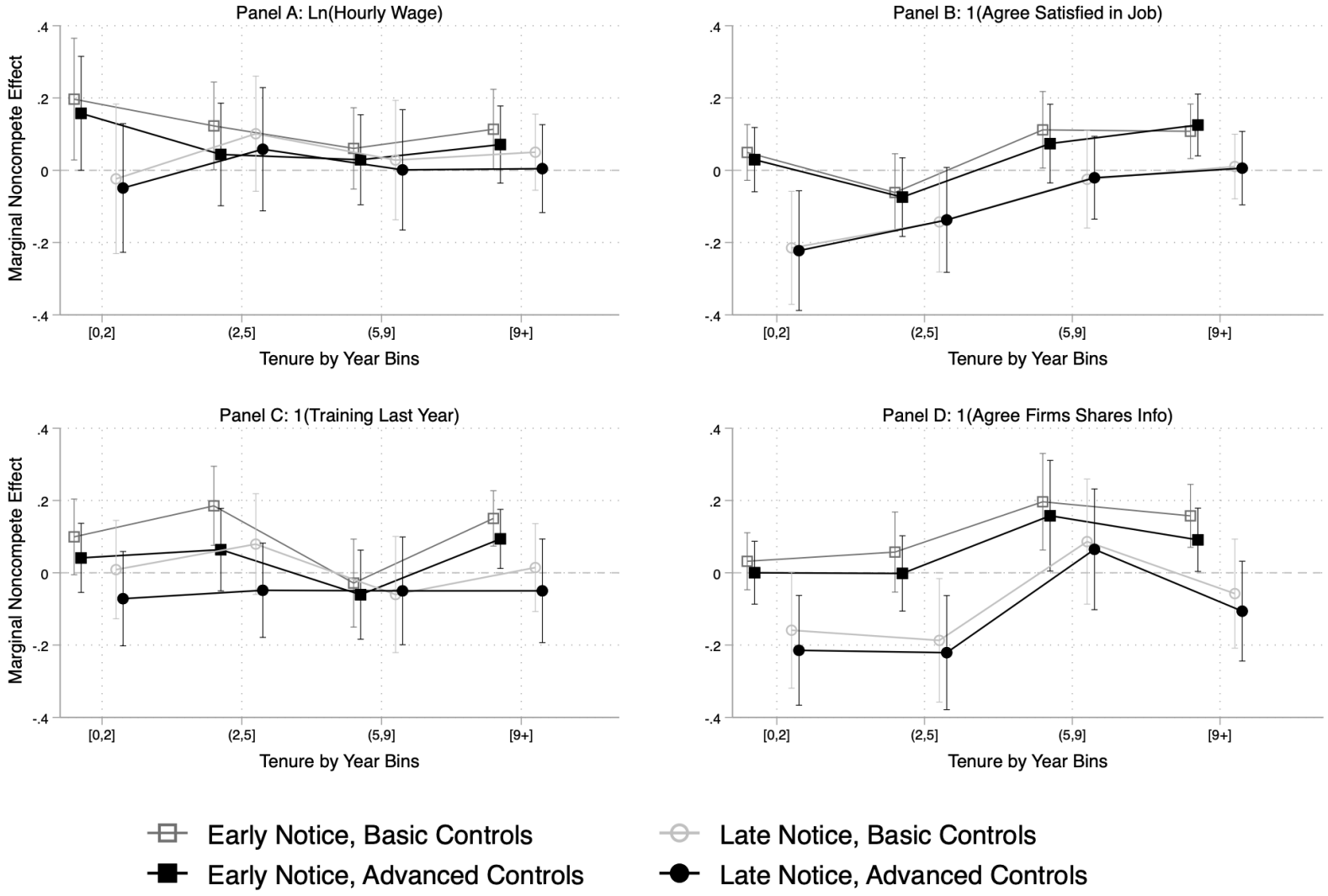


Figure 9: Marginal Effect of Noncompetes over Tenure



Tables

Table 1: Summary of Competing Perspectives on Noncompetes (and Findings)

	Main Arguments	Predictions		
		Use	Contracting Process	Labor Market Outcomes
Traditional Economic Perspective	Employers use noncompetes to solve a holdup problem, protecting "legitimate" employer interests, including trade secrets and client information, which creates incentives for efficient investment. Employees agree not to compete only when doing so makes them better off, either through an upfront compensating differential or through increased growth in wages over time.	Noncompetes should be confined to employees in occupations and industries that involve trade secrets, access to client lists, or other valuable information, or that require specialized industry-specific training. Noncompetes should also be confined to states that enforce noncompetees.	Noncompetes should be negotiated agreements that maximize joint employee-employer surplus and make both parties better off. Alternatively, a compensating differential should be a part of the initial job offer, rendering costly negotiation unnecessary.	Conditional on an employee's job duties, employees who agree to noncompetes should receive more training or valuable information relevant to their employment. Employees should earn higher wages as they capture some of the joint surplus created by the noncompete. Because they capture some of the surplus on average, employees with noncompetes should be more satisfied on job.
Critical Perspective	Noncompetes reduce labor-market competition from product-market competitors and future product market competition from departing employees. Employers can also deploy noncompetes at inopportune times to reduce applicant or employee bargaining power.	Noncompetes should be common for all sorts of employees, regardless of access to confidential information, whenever employers can use noncompetes to limit competition in labor or product markets. Noncompetes should be found where they are unenforceable if employee behavior is affected by noncompetes without regard to the prospect of actual enforcement.	If employers are in a position to impose noncompetes on applicants and employees, noncompetes should rarely be negotiated and employees should rarely seek outside advice since their actual options are few. Employers may opt for "late" noncompetes that reduce employee bargaining power.	Employees may not necessarily see any increase in training or information access since noncompetes may not be deployed to resolve incentive or holdup issues. Employees may also suffer in terms of lower wages and reduced job satisfaction if noncompetes are able to limit competition in labor markets.
		Findings		
		Noncompetes are more common for employees in technical jobs and industries and for employees who have access to valuable, confidential information. However, noncompetes are relatively common in all occupations and industries and bind many employees without access to trade secrets or client information. Noncompetes are also common in states that do not enforce them.	Employees and employers rarely negotiate over noncompetes, and a rarely promised anything in exchange for signing. About one-third of noncompetes are requested by employers after an employee has accepted their job offer (without a change in job title or responsibilities). Few individuals receive outside advice during the contracting process.	Noncompetes are associated with more training, greater access to information, and higher wages and job satisfaction when the noncompete is presented along with the job offer. If a noncompete is presented after the acceptance of a job offer, employees experience no wage or training benefits on average, and they report being less satisfied in their jobs. Higher wages appear to be largest in early tenure for employees receiving early notice, while lower job satisfaction appears early in tenure for late-notice noncompetes. Training associated with noncompetes increases with the enforceability of the noncompete, but wages fall.

Table 2: Comparison of Weighted and Unweighted Sample and 2014 American Community Survey

Variable	Sample Data		ACS	NSP-ACS Difference	
	Unweighted	Weighted		Unweighted	Weighted
Age (in years)	41.98	40.33	40.55	1.43**	-0.22
	(13.23)	(13.63)	(13.64)	(0.16)	(0.27)
Annual Income ($)	49,062	44,001	46,680	2,382**	-2,680
	(42033)	(47378)	(55622)	(769)	(1,748)
1(Work > 40 Hours/Week)	0.70	0.71	0.72	-0.02**	-0.01
	(0.46)	(0.45)	(0.45)	(0.00)	(0.01)
1(Highest Degree < BA)	0.48	0.69	0.70	-0.22**	-0.01
	(0.50)	(0.46)	(0.46)	(0.01)	(0.02)
1(Highest Degree = BA)	0.37	0.21	0.20	0.16**	0.01
	(0.48)	(0.41)	(0.40)	(0.01)	(0.01)
1(Highest Degree > BA)	0.16	0.10	0.097	0.06**	0.00
	(0.36)	(0.30)	(0.30)	(0.00)	(0.01)
1(Male)	0.47	0.53	0.53	-0.07**	-0.00
	(0.50)	(0.50)	(0.50)	(0.01)	(0.01)

Notes: This table shows the distributions of demographic characteristics in our sample data, both weighted and unweighted, and in data from the 2014 American Community Survey. The weighted data use raking weights, as described in the text and in Prescott et al. (2016). Note: ** p<0.01, * p<0.05, $^{+}$ p<0.1. We report standard deviations (first three columns) and robust standard errors (last two columns), clustered at the state level, in parentheses.

Table 3: Temporal and Geographic Scope of Noncompetes

(1) Term Duration	(2) Percent	(3) Geographic Limit	(4) Percent
Duration < 1 Year	30.9	Radius in Miles	7.3
1 < Duration ≤ 2 Years	15.0	City	5.9
Duration > 2 Years	33.8	County	6.1
Don't Know	20.3	MSA	6.0
		Within the State	13.9
		Entire U.S.	15.4
		No limit	23.1
		Other	3.30
		Don't Know	19.0

Notes: Column (2) shows the distribution of noncompete provision duration periods in the sample, while Column (4) shows the distribution of geographic boundaries of the competition prohibitions. The sample includes the 1,747 individuals bound by noncompetes and uses sample weights.

Table 4: Sample Means by Noncompete Use

		Bound by Noncompete?			Δ Relative to "No" Group	
Variable	Overall	No	Maybe	Yes	Maybe	Yes
Labor Market Outcomes						
Ln(Hourly Wage)	2.88	2.92	2.70	3.24	-0.23**	0.31**
1(Employer Shares Info)	0.55	0.57	0.50	0.59	-0.06**	0.02
1(Training Last Year)	0.50	0.52	0.44	0.64	-0.08**	0.12**
1(Satisfied in Job)	0.68	0.69	0.65	0.70	-0.04^{+}	0.01
Demographics						
1(Paid Hourly)	0.68	0.65	0.81	0.45	0.16**	-0.12**
1(Paid by Salary)	0.28	0.31	0.16	0.49	-0.15**	0.18**
1(Paid by Commission)	0.03	0.03	0.02	0.04	-0.01*	0.02
1(Paid by Other Means)	0.01	0.01	0.01	0.01	-0.00	-0.00
Age (in years)	40.28	42.33	37.54	40.22	-4.79**	-2.11**
Hours Worked per Week	37.59	37.92	35.87	41.27	-2.05**	3.34**
Weeks Worked per Year	47.81	48.31	46.96	48.33	-1.35**	0.02
1(Male)	0.53	0.56	0.47	0.58	-0.08**	0.03
1(Private For-Profit Employer)	0.90	0.90	0.87	0.96	-0.03**	0.06**
1(Private Nonprofit Employer)	0.06	0.07	0.07	0.02	0.01	-0.05**
1(Public Health System Employer)	0.04	0.03	0.05	0.02	0.02**	-0.01*
1(Highest Degree $<$ BA)	0.69	0.65	0.81	0.48	0.17**	-0.17**
1(Highest Degree $=$ BA)	0.21	0.24	0.14	0.33	-0.10**	0.09**
1(Highest Degree $>$ BA)	0.1	0.12	0.05	0.19	-0.07**	0.08**
Ln(State Unemployment Rate at Hire)	1.9	1.88	1.92	1.89	0.04**	0.01
Ln(Labor Force Size in State at Hire)	15.35	15.33	15.35	15.41	0.02	0.07*
Ln(Establishments in County-Industry)	6.47	6.47	6.4	6.68	-0.07	0.21*
1(Employer Size $<$ 25)	0.23	0.25	0.23	0.15	-0.02^{+}	-0.10**
1(Employer Size 25–100)	0.16	0.16	0.16	0.15	-0.00	-0.00
1(Employer Size 101–250)	0.09	0.1	0.09	0.1	-0.01	0.00
1(Employer Size 251–500)	0.07	0.08	0.06	0.09	-0.01	0.02^{+}
1(Employer Size 501–1,000)	0.07	0.07	0.07	0.07	0.01	0.00
1(Employer Size 1,001–2,500)	0.07	0.06	0.07	0.07	0.01	0.01
1(Employer Size 2,501–5,000)	0.07	0.06	0.08	0.08	0.02*	0.02*
1(Employer Size $>$ 5,000)	0.24	0.23	0.24	0.29	0.01	0.06**
1(Multi-Unit Employer)	0.63	0.61	0.62	0.73	0.00	0.12**
Other Post-Employment Restrictive Covenants						
1(Nondisclosure)	0.36	0.3	0.3	0.75	-0.00	0.44**
1(Nonpoaching)	0.04	0.02	0.02	0.18	-0.00	0.15**
1(Nonsolicit)	0.12	0.08	0.09	0.35	0.01	0.27**
1(Arbitration)	0.08	0.06	0.05	0.19	-0.01	0.13**
1(IP Assignment)	0.09	0.08	0.05	0.28	-0.03**	0.20**
Observations	11,505	6,344	3,414	1,747		

Notes: This table reports the weighted sample means for the full sample as well as for respondents who report working under a noncompete (15.1% of the unweighted sample), respondents who indicate that they were not bound by a noncompete (55.1% of the unweighted sample), and the "maybe" group of respondents (29.7% of the unweighted sample). Recall that 83.5% of the "maybe" category are in that category because they indicate that they have never heard of a noncompete. ** p<0.01, * p<0.05, $^{+}$p<0.1. We use robust standard errors, clustered at the state level, when testing differences between categories.

Table 5: Noncompete Use By Employee Characteristics

Characteristic	% Currently Bound by Noncompete	% Ever Bound by Noncompete
Employer Class		
Private For-Profit	19.0	38.8
Private Nonprofit	9.8	28.6
Public Healthcare	12.4	37.8
Gender		
Female	17.3	36.3
Male	18.8	39.7
Age in Years		
Under Age 40	20.6	38.7
Age 40 or Older	15.6	37.5
Highest Level of Education		
$<$ Bachelor's Degree	14.3	34.7
Bachelor's Degree	25.0	43.8
$>$ Bachelor's Degree	30.0	49.0
Compensation Type		
Hourly	14.0	33.7
Salary	27.5	47.7
Other	23.6	45.9
Annual Earnings		
$<$ \$40,000	13.3	33.0
$\geq$ \$40,000	25.2	45.6
Confidential Information		
Works with Clients (WC)	14.9	35.6
Access to Client Information (CI)	16.0	36.2
Access to Trade Secrets (TS)	32.6	54.9
WC, CI	14.8	31.3
WC, TS	35.8	53.4
CI, TS	34.4	58.3
WC, CI, TS	35.3	56.2
None	7.8	26.9
Employer Size		
$<$ 25 Employees	11.6	33.6
25–100 Employees	17.7	36.5
101–250 Employees	19.1	40.6
251–500 Employees	22.3	40.9
501–1,000 Employees	16.8	39.1
1,001–2,500 Employees	21.2	42.3
2,501–5,000 Employees	21.0	44.2
$>$ 5,000 Employees	21.5	38.3
Overall	18.1	38.1

Notes: This table shows descriptive statistics related to whether an employee was bound by a noncompete in 2014 ("currently bound') or had ever been bound by a noncompete. The reported incidence statistics we show are from the multiple imputation approach we describe in the text. We weight all estimates.

Table 6: Determinants of Noncompete Status

	Panel A: Compare Yes, No, and Maybe			*Panel B: Yes vs. Maybe or No*
	Multinomial Logit			OLS
	(1)	(2)	(3)	(4)
	Maybe	No	Yes	**1**(Noncompete)
Ln(Hourly Wage)	-0.037*	0.006	0.031**	0.029**
	(0.017)	(0.018)	(0.011)	(0.010)
1(Private Nonprofit Employer)	0.042	0.039	-0.081**	-0.071**
	(0.033)	(0.033)	(0.014)	(0.015)
1(Public Health System Employer)	0.087*	-0.034	-0.053*	-0.054*
	(0.042)	(0.036)	(0.022)	(0.025)
1(Works with Clients (WC))	-0.058**	0.004	0.053**	0.044**
	(0.020)	(0.023)	(0.008)	(0.009)
1(Access to Client Information (CI))	-0.121**	0.055	0.066**	0.055**
	(0.038)	(0.041)	(0.018)	(0.018)
1(Access to Trade Secrets (TS))	-0.178**	0.021	0.157**	0.161**
	(0.024)	(0.028)	(0.018)	(0.021)
1(WC, CI)	-0.193**	0.132**	0.061**	0.051**
	(0.027)	(0.031)	(0.013)	(0.014)
1(WC, TS)	-0.217**	-0.013	0.230**	0.227**
	(0.042)	(0.046)	(0.049)	(0.056)
1(CI, TS)	-0.194**	0.016	0.178**	0.191**
	(0.031)	(0.029)	(0.022)	(0.026)
1(WC, CI, TS)	-0.240**	0.039	0.201**	0.209**
	(0.027)	(0.026)	(0.015)	(0.017)
1(1st Enforceabilility Quintile)	-0.109**	0.068**	0.041**	0.046**
	(0.026)	(0.018)	(0.013)	(0.015)
1(2nd Enforceability Quintile)	-0.100**	0.073**	0.027*	0.033*
	(0.021)	(0.019)	(0.012)	(0.013)
1(3rd Enforceability Quintile)	-0.136**	0.074**	0.062**	0.066**
	(0.026)	(0.025)	(0.019)	(0.022)
1(4th Enforceability Quintile)	-0.118**	0.084**	0.035*	0.039*
	(0.021)	(0.024)	(0.017)	(0.018)
1(5th Enforceability Quintile)	-0.111**	0.064**	0.047**	0.052**
	(0.026)	(0.017)	(0.015)	(0.017)
1(2 Employers in Last 5 Years)	-0.026	0.027	-0.002	-0.000
	(0.021)	(0.022)	(0.011)	(0.011)
1(3–4 Employers in Last 5 Years)	0.010	-0.014	0.004	0.004
	(0.020)	(0.029)	(0.018)	(0.017)
1($>$ 4 Employers in Last 5 Years)	0.048*	-0.056*	0.008	0.007
	(0.022)	(0.023)	(0.018)	(0.019)
1(**E**[Duration 1–2 Years)	0.064^{+}	-0.014	-0.050	-0.047
	(0.039)	(0.042)	(0.032)	(0.029)
1(**E**[Duration 2–4 Years)	0.038	0.015	-0.053*	-0.048^{+}
	(0.040)	(0.036)	(0.027)	(0.026)
1(**E**[Duration 4–10 Years)	0.056	-0.027	-0.029	-0.024
	(0.035)	(0.035)	(0.028)	(0.026)
1(**E**[Duration $>$ 10 Years)	0.122*	-0.095^{+}	-0.028	-0.009
	(0.062)	(0.051)	(0.037)	(0.038)
1(**E**[Duration Indefinite)	0.059^{+}	-0.015	-0.044^{+}	-0.038^{+}
	(0.032)	(0.035)	(0.025)	(0.022)
1(Paid by Salary)	-0.049**	0.015	0.034*	0.040*
	(0.018)	(0.017)	(0.015)	(0.016)

Continued on next page

Table 6 – continued from previous page

1(Paid by Commision)	-0.116**	-0.006	0.121**	0.111*
	(0.042)	(0.042)	(0.041)	(0.044)
1(Paid by Other Means)	-0.012	-0.018	0.030	0.018
	(0.062)	(0.065)	(0.044)	(0.036)
Age (in years)	-0.005**	0.005**	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Hours Worked per Week	0.000	-0.001	0.001	0.001
	(0.001)	(0.001)	(0.000)	(0.000)
Weeks Worked per Year	-0.003*	0.002^{+}	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
1(Male)	-0.024	0.047**	-0.023*	-0.021*
	(0.015)	(0.016)	(0.011)	(0.010)
1(Highest Degree = BA)	-0.108**	0.076**	0.032**	0.041**
	(0.017)	(0.019)	(0.010)	(0.012)
1(Highest Degree > BA)	-0.119**	0.085**	0.033*	0.051**
	(0.029)	(0.029)	(0.014)	(0.018)
Ln(State Unemployment Rate at Hire)	0.001	-0.023	0.022	0.020
	(0.027)	(0.024)	(0.016)	(0.016)
Ln(Labor Force Size in State at Hire)	-0.008	0.005	0.003	0.002
	(0.010)	(0.009)	(0.007)	(0.007)
1(Multi-Unit Employer)	-0.033	0.000	0.032**	0.034**
	(0.022)	(0.027)	(0.011)	(0.012)
1(Employer Size 25–100)	0.016	-0.046*	0.031^{+}	0.022
	(0.018)	(0.024)	(0.016)	(0.016)
1(Employer Size 101–250)	0.017	-0.038	0.022	0.016
	(0.027)	(0.025)	(0.017)	(0.018)
1(Employer Size 251–500)	-0.003	-0.035	0.038*	0.033
	(0.032)	(0.031)	(0.019)	(0.020)
1(Employer Size 501–1,000)	0.059^{+}	-0.076*	0.017	0.010
	(0.033)	(0.038)	(0.025)	(0.027)
1(Employer Size 1,001–2,500)	0.054	-0.078^{+}	0.024^{+}	0.018
	(0.047)	(0.046)	(0.013)	(0.015)
1(Employer Size 2,501–5,000)	0.088**	-0.106**	0.019	0.013
	(0.028)	(0.027)	(0.018)	(0.019)
1(Employer Size > 5,000)	0.046^{+}	-0.078**	0.032^{+}	0.025
	(0.026)	(0.027)	(0.017)	(0.017)
Ln(Establishments in County-Industry)	0.006	-0.007	0.001	0.002
	(0.005)	(0.005)	(0.003)	(0.003)
Observations	11,462	11,462	11,462	11,462
Mean R-Squared				0.139
Occupation and Industry FE	Yes	Yes	Yes	Yes

Notes: Panel A shows the marginal increase in the probabilty of falling into the "maybe," "yes," or "no" noncompete categories from a unit increase in the variable in the left-hand column. Each row adds to zero in Panel A because increases in the probability of being in one category are offset by lower chances of being in another. Panel B is a linear probability model in which the dependent variable is an indicator for agreeing to a noncompete, where those in the "maybe" category are grouped with those in the "no" category. The omitted enforceability group is the set of nonenforcing states (North Dakota and California) and the measure of noncompete enforceability is taken from Starr (2019). ** p<0.01, * p<0.05, $^{+}$ p<0.1. We report robust standard errors in parentheses, clustered at the state level.

Table 7: The Noncompete Contracting Process

	(1) Distribution (%)	(2) % Negotiate
Panel A: When did you first learn you would be asked to sign a noncompete?		
Before Accepting Job Offer	60.8	11.6
After Accepting Job Offer	29.3	6.3
Before Promotion or Raise	2.2	30.8
Other or Cannot Remember	7.7	6.5
Panel B: What did you do when asked to sign?		
Signed without Reading	6.7	7.9
Read Quickly and Signed	31.2	7.1
Read Slowly and Signed	56.4	11.6
Consulted with Friends/Family	10.4	30.8
Consulted a Lawyer	7.9	48.6
Overall		10.1

Notes: The "Distribution (%)" column (1) shows the percentage of individuals in each category for each question (panel). The "% Negotiate" column (2) records the percentage of individuals in the row who report negotiating over the terms of their noncompete or for other benefits in exchange for agreeing not to compete. The first two rows in Panel A ("before" and "after") refer to noncompetes agreed to without a change in job title or duties, whereas the third row addresses noncompetes signed as part of a promotion.

Table 8: Labor Market Outcomes

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
				Panel A: Baseline				
Noncompete	0.109**	0.066**	0.031	-0.020	0.077**	0.006	0.015	0.006
	(0.026)	(0.023)	(0.030)	(0.025)	(0.019)	(0.019)	(0.019)	(0.017)
	[1.033]	[0.497]	[1.361]	[0.715]	[1.180]	[0.104]	[1.463]	[1.399]
		{0.216}		{0.302}		{0.048}		{0.829}
R-Squared	0.503	0.541	0.100	0.146	0.160	0.199	0.099	0.149
				Panel B: Heterogeneity by Timing of Notice				
First Learned of Noncompete								
Before Accepting Job	0.143**	0.093**	0.101**	0.043^{+}	0.131**	0.055*	0.060**	0.045*
	(0.033)	(0.031)	(0.026)	(0.024)	(0.024)	(0.025)	(0.020)	(0.020)
	[1.220]	[0.638]	[4.067]	[1.254]	[1.954]	[0.920]	[4.120]	[3.846]
		{0.275}		{0.518}		{0.406}		{1.972}
After Accepting Job	0.057	0.024	-0.093^{+}	-0.134**	0.017	-0.058	-0.090*	-0.085*
	(0.042)	(0.037)	(0.050)	(0.039)	(0.035)	(0.039)	(0.036)	(0.035)
	[0.759]	[0.316]	[11.830]	[8.474]	[0.089]	[1.112]	[7.862]	[9.004]
		{0.151}		{3.097}		{0.480}		{6.978}
With Promotion	0.202*	0.136	0.039	0.011	-0.060	-0.125	0.070	0.051
	(0.090)	(0.086)	(0.089)	(0.104)	(0.097)	(0.113)	(0.067)	(0.071)
	[1.226]	[0.741]	[0.653]	[0.307]	[0.637]	[2.221]	[1.375]	[2.385]
		{0.269}		{0.186}		{0.850}		{9.855}
Doesn't Remember	0.020	0.010	-0.049	-0.073	-0.076	-0.093	0.043	0.042
	(0.056)	(0.064)	(0.076)	(0.064)	(0.069)	(0.064)	(0.044)	(0.047)
	[1.226]	[0.146]	[0.653]	[4.343]	[0.637]	[4.668]	[1.375]	[4.866]
		{0.506}		{2.164}		{4.559}		{40.34}
P-value: $\beta_{Before} = \beta_{After}$	0.062	0.127	0.000	0.000	0.014	0.021	0.000	0.000
R-Squared	0.503	0.541	0.104	0.150	0.162	0.201	0.102	0.151
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Tables OB9 and OB10 in the Online Appendix show the results of treating the "maybe" group as a separate category and imputing "yes" or "no" status for each respondent in the "maybe" group, respectively. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table 9: Labor Market Outcomes: Heterogeneity by Noncompete Enforceability

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
			Panel A: Baseline					
Enforceability	-0.014^{+}		0.004		-0.002		0.006	
	(0.007)		(0.006)		(0.006)		(0.005)	
Noncompete	0.106**	0.063*	0.030	-0.020	0.081**	0.010	0.008	0.004
	(0.027)	(0.024)	(0.028)	(0.025)	(0.020)	(0.020)	(0.017)	(0.018)
Enforceability×Noncompete	-0.025*	-0.017^{+}	0.004	0.001	0.021*	0.020**	-0.014	-0.011
	(0.010)	(0.009)	(0.024)	(0.021)	(0.008)	(0.008)	(0.012)	(0.009)
R-Squared	0.491	0.541	0.089	0.146	0.151	0.199	0.0908	0.149
			Panel B: Heterogeneity by Timing of Notice					
Enforceability	-0.013^{+}		0.004		-0.001		0.006	
	(0.007)		(0.006)		(0.006)		(0.005)	
First Learned of Noncompete								
Before Accepting Job	0.139**	0.085**	0.100**	0.044^{+}	0.137**	0.061*	0.055**	0.042*
	(0.032)	(0.032)	(0.026)	(0.024)	(0.026)	(0.026)	(0.020)	(0.021)
After Accepting Job	0.051	0.021	-0.093*	-0.132**	0.020	-0.057	-0.098**	-0.087*
	(0.041)	(0.038)	(0.046)	(0.039)	(0.036)	(0.039)	(0.033)	(0.033)
Enforceability×Before Accepting Job	-0.032*	-0.027*	0.006	0.003	0.025*	0.025*	-0.009	-0.006
	(0.012)	(0.011)	(0.016)	(0.015)	(0.010)	(0.009)	(0.009)	(0.008)
Enforceability×After Accepting Job	-0.039*	-0.030*	0.024	0.015	0.008	-0.000	-0.033	-0.027
	(0.016)	(0.014)	(0.046)	(0.037)	(0.017)	(0.016)	(0.025)	(0.019)
R-Squared	0.492	0.542	0.093	0.150	0.154	0.201	0.093	0.152
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
State Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A examines the main association of having a noncompete with the column outcomes and the moderating role of noncompete enforceability, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the association of having a noncompete and noncompete enforceability to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. The Enforceability measure is drawn from Starr (2019), who modifies the initial measure of Bishara (2011). ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Online Appendix

A The Incidence of Noncompetes by Other Characteristics

Our data allow us to describe the incidence of noncompetes by a variety of employee, employer, and geographic characteristics. In Figures OA1 through OA4, we present additional statistics using the format of Figures 1 through 8: the top and bottom of each bar bookend the possible range of noncompete incidence for the group in question; we calculate the top by assuming that those in the "maybe" group did agree to a noncompete and the bottom by assuming that they did not. The dark dot within the bar is the multiple imputation estimate, which is our best guess at the overall incidence of noncompetes for the category. The dashed horizontal line is the population average, 18.1%. In Figure OA5, we show the joint distribution of noncompete use in our data by industry and occupation. Note that in the separate occupation and industry figures in the text (Figures 5 and 6), we also report the "projected" use of noncompetes in each occupation and industry, which are calculated by averaging respondent responses to the question "*What proportion of individuals in your [occupation or industry] have agreed to noncompetes*" within the respondent's occupation and industry, respectively.

Figure OA1: Noncompetes Incidence by Expected Length of Stay

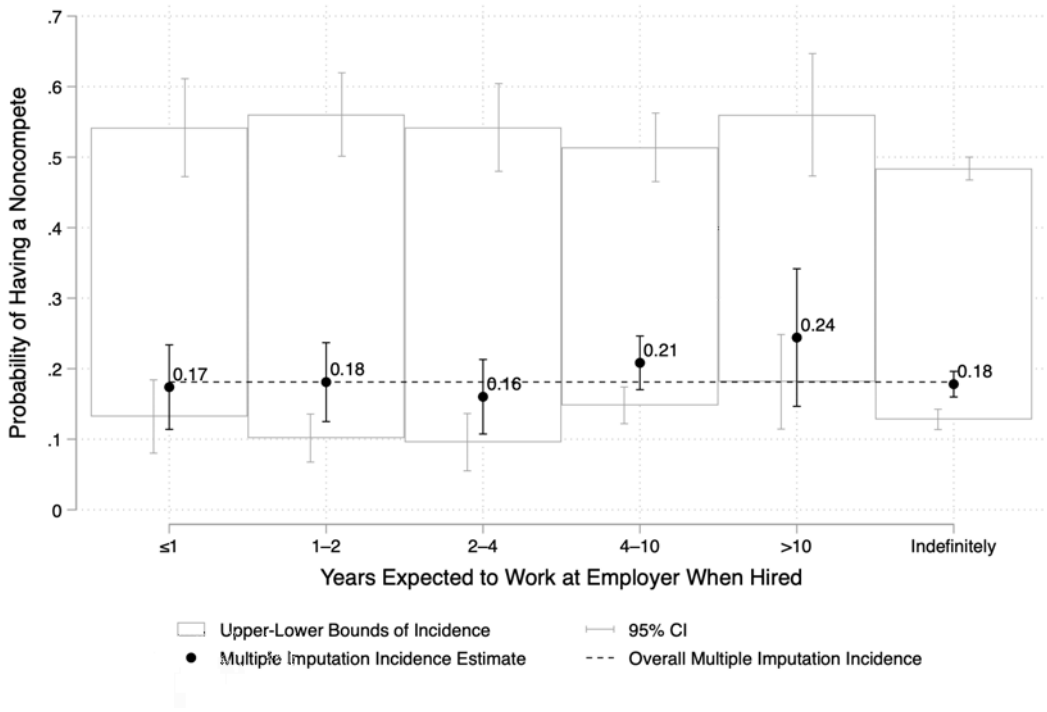


Figure OA2: Noncompete Incidence by Number of Employers in Past 5 Years

Figure OA3: Noncompete Incidence by Year of Hire
(conditional on staying until 2014)

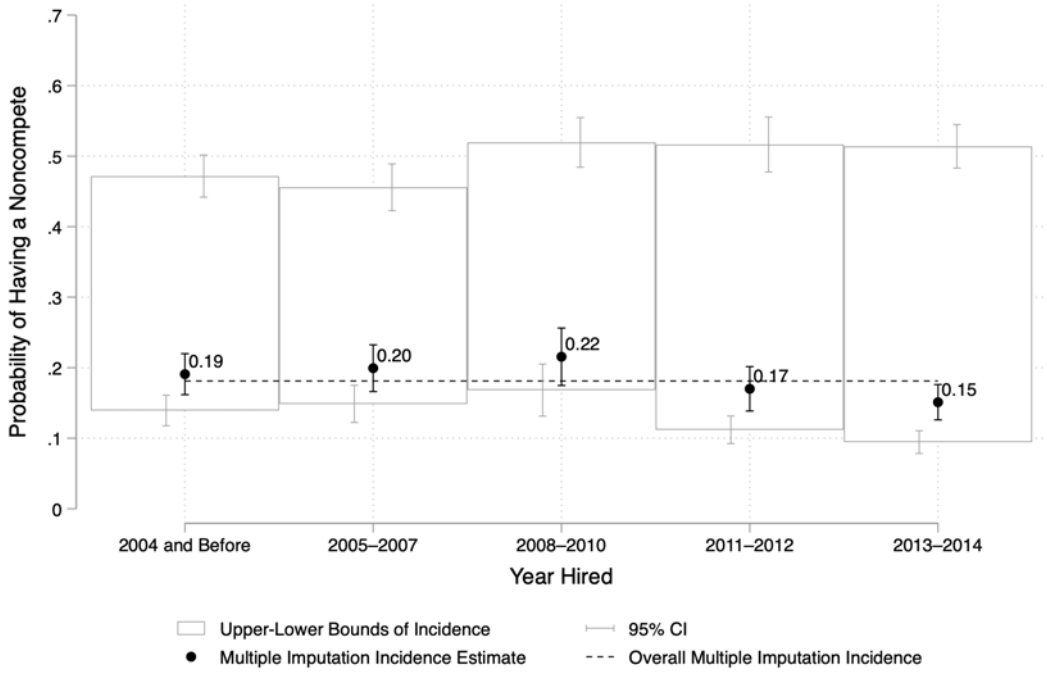


Figure OA4: Noncompete Incidence by Employer Size

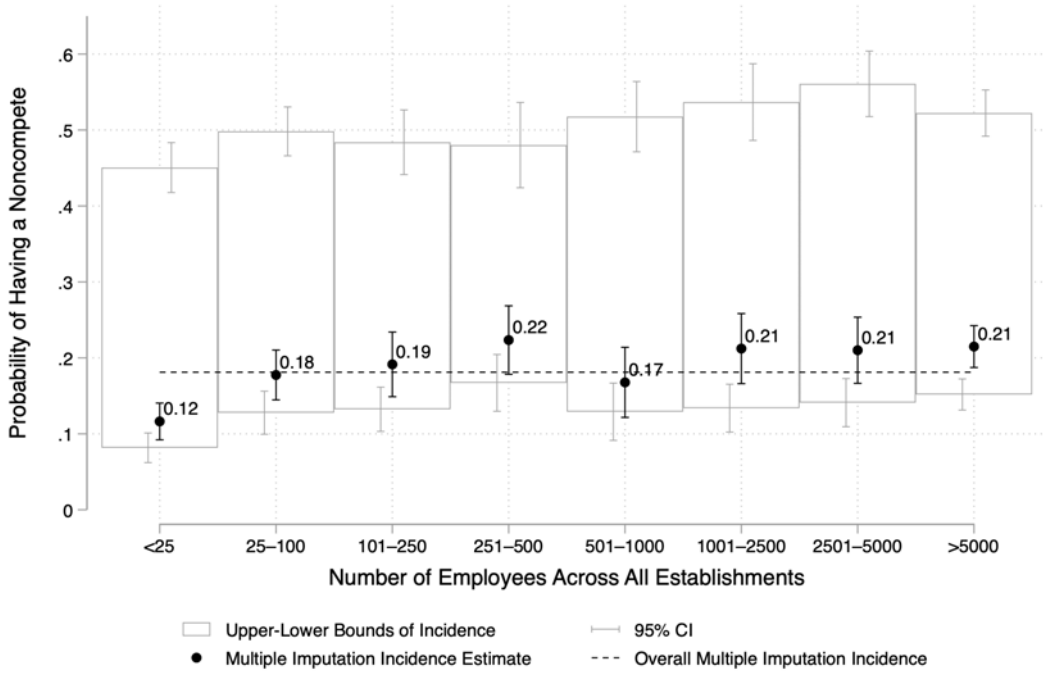
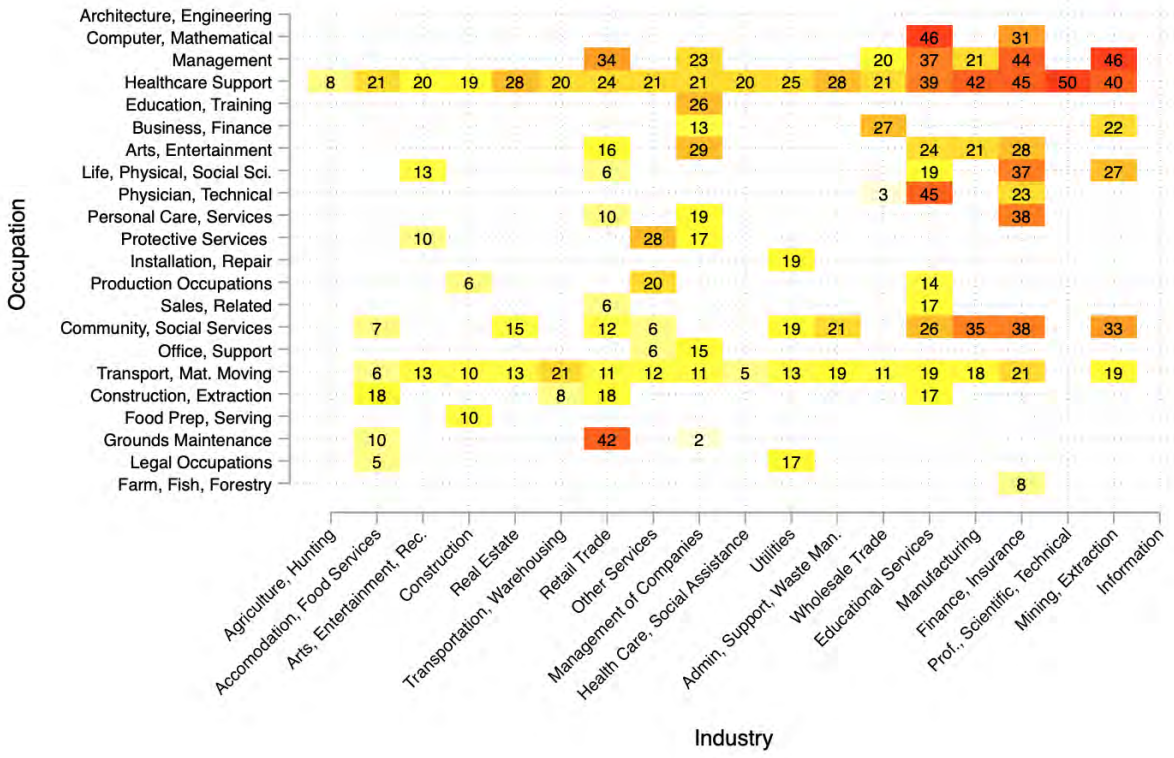


Figure OA5: Incidence of Noncompetes by Industry and Occupation



B Additional Tables

Table OB1: Noncompete Incidence Across Studies and Samples

Study	Population	% U.S. Labor Force	Data Source	Sample Size	Response Rate	Noncompete Incidence
Schwab and Thomas (2006)	Executives	0.18%	The Corporate Library, SEC EDGAR Filings	375 Executives	N/A	67.5%
Garmaise (2009)	Executives	0.18%	Execucomp Firms with 10-K, 10-Q SEC Filings	500 Firms	N/A	70.2%
Bishara et al. (2012)	Executives	0.18%	SEC EDGAR Filings	500 Firms	N/A	78.7%
Marx (2011)	Electrical and Electronics Engineers	0.23%	Survey of Institute of Electrical and Electronics Engineers (professional association)	1,029 Individuals	20.6%	43.3%
Lavetti et al. (2019)	Physicians	0.47%	Survey of American Medical Association Primary Care Physicians in 5 states	1,967 Individuals	69.8%	45.1%
Johnson and Lipsitz (2019)	Hair Stylists	0.25%	Survey of Professional Beauty Association	218 Hair Salons	4%–31%	30.0%
Present Study	U.S. Labor Force	100%	Qualtrics (in conjunction with 7 online survey panel providers)	11,505 Individuals	2%–23%	18.1%

Note: Proportion of U.S. Labor force based on the 2014 BLS Occupational Employment Survey: https://www.bls.gov/news.release/archives/ocwage_03252015.pdf

Table OB2: Reasons for Not Negotiating a Noncompete

	(1)	(2)	(3)
	Presented with Noncompete Before Accepted Job Offer?		*Overall*
	Yes	No	
I Found the Terms Reasonable	0.55	0.46	0.52
I Assumed I Could Not Negotiate	0.38	0.48	0.41
I Wanted to Avoid Creating Tension	0.18	0.19	0.19
I Worried I Would be Fired	0.20	0.22	0.20
I Didn't Think my Employer Would Sue	0.07	0.11	0.08
I Didn't Think a Court Would Enforce	0.08	0.05	0.07
Other	0.04	0.07	0.05

Notes: The table shows the reasons individuals report for not negotiating over their noncompete in response to the question: *"If you did not negotiate over the noncompete, why not?"* Respondents were free to select more than one response. Those who agreed to a noncompete as part of a promotion or who were unable to recall whether they negotiated or why they chose not to negotiate are omitted from the table. Column (3) reports the overall average, and the rows are sorted based on these proportions.

Table OB3: Sample Means by Noncompete Timing

	Noncompete Timing		
	Before Accepted Job Offer	After Accepted Job Offer	Difference
Dependent Variables			
Ln(Hourly Wage)	3.31	3.10	0.214**
1(Firm Shares Info)	0.66	0.46	0.201**
1(Training Last Year)	0.69	0.58	0.116**
1(Satisfied)	0.75	0.57	0.171**
Demographics			
1(Paid Hourly)	0.40	0.55	-0.156**
1(Paid by Salary)	0.53	0.41	0.123*
1(Paid by Commission)	0.05	0.02	0.030^{+}
1(Paid by Other Means)	0.01	0.01	0.002
Age (in years)	40.71	38.35	2.233^{+}
Hours Worked per Week	41.67	39.28	2.405
Weeks Worked per Year	48.27	47.85	0.422
1(Male)	0.63	0.48	0.155**
1(Private For-Profit Employer)	0.95	0.96	-0.010
1(Private Nonprofit Employer)	0.03	0.02	0.009
1(Public Health System Employer)	0.02	0.02	0.002
1(Highest Degree $<$ BA)	0.45	0.50	-0.053
1(Highest Degree $=$ BA)	0.34	0.33	0.007
1(Highest Degree $>$ BA)	0.21	0.17	0.047^{+}
Ln(State Unemployment Rate at Hire)	1.90	1.90	0.000
Ln(Labor Force Size in State at Hire)	15.40	15.41	-0.014
Ln(Establishments in County-Industry)	6.71	6.65	0.056
1(Employer Size $<$ 25)	0.14	0.14	-0.005
1(Employer Size 25–100)	0.14	0.17	-0.032
1(Employer Size 101–250)	0.11	0.08	0.025
1(Employer Size 251–5,000)	0.34	0.27	0.059
1(Employer Size $>$ 5,000)	0.28	0.32	-0.047
1(Multi-Unit Employer)	0.74	0.75	-0.015
Other Post-Employment Restrictive Covenants			
1(Nondisclosure)	0.75	0.77	-0.014
1(Nonpoaching)	0.20	0.14	0.057
1(Nonsolicit)	0.36	0.32	0.039
1(Arbitration)	0.20	0.18	0.025
1(IP Assignment)	0.30	0.24	0.060

Notes: This table reports the weighted sample means for respondents who report working under a noncompete (not including those who are imputed as agreeing to a noncompete). The "After Accepted Job Offer" category does not include those who were asked to sign a noncompete following a promotion or other changes in employment responsibilities. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We use robust standard errors, clustered at the state level, when testing differences between categories.

Table OB4: Ln(Hourly Wage) Results, Sequential Addition of Covariates

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline			
Noncompete	0.407**	0.109**	0.107**	0.084**	0.068*	0.066**
	(0.038)	(0.026)	(0.026)	(0.026)	(0.027)	(0.023)
R-Squared	0.035	0.503	0.507	0.508	0.511	0.541
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.483**	0.143**	0.142**	0.118**	0.102**	0.093**
	(0.047)	(0.033)	(0.032)	(0.033)	(0.033)	(0.031)
After Accepting Job	0.269**	0.057	0.053	0.034	0.017	0.024
	(0.064)	(0.042)	(0.041)	(0.039)	(0.041)	(0.037)
With Promotion	0.650**	0.202*	0.186*	0.154^{+}	0.130	0.136
	(0.154)	(0.090)	(0.086)	(0.090)	(0.087)	(0.086)
Doesn't Remember	0.262**	0.020	0.018	0.007	-0.005	0.010
	(0.078)	(0.056)	(0.057)	(0.058)	(0.060)	(0.064)
P-value: $\beta_{Before} = \beta_{After}$	0.007	0.062	0.053	0.065	0.063	0.127
R-Squared	0.038	0.503	0.507	0.509	0.511	0.541
Observations	11,505	11,462	11,462	11,462	11,462	11,010
Demographic Controls		✓	✓	✓	✓	✓
Poaching Flows/Prior Mobility			✓	✓	✓	✓
Other Restrictive Covenants				✓	✓	✓
Access to Confidential Info					✓	✓
Other HR Benefits						✓

Notes: Panel A examines the aggregate association of having a noncompete with Ln(Hourly Wages), where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. Controls are as defined on page 11. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB5: **1**(Employer Shares Information) Results, Sequential Addition of Covariates

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline			
Noncompete	0.050^{+}	0.031	0.031	0.004	-0.017	-0.020
	(0.030)	(0.030)	(0.031)	(0.027)	(0.026)	(0.025)
R-Squared	0.0011	0.100	0.116	0.120	0.127	0.146
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.120**	0.101**	0.097**	0.068**	0.048^{+}	0.043^{+}
	(0.025)	(0.026)	(0.027)	(0.025)	(0.024)	(0.024)
After Accepting Job	-0.081	-0.093^{+}	-0.086^{+}	-0.109*	-0.131**	-0.134**
	(0.051)	(0.050)	(0.049)	(0.044)	(0.042)	(0.039)
With Promotion	0.109	0.039	0.059	0.023	-0.002	0.011
	(0.102)	(0.089)	(0.089)	(0.092)	(0.094)	(0.104)
Doesn't Remember	-0.019	-0.049	-0.044	-0.059	-0.081	-0.073
	(0.073)	(0.076)	(0.078)	(0.072)	(0.067)	(0.064)
P-value: $\beta_{Before} = \beta_{After}$	0.000	0.000	0.000	0.000	0.000	0.000
R-Squared	0.00550	0.104	0.119	0.123	0.131	0.150
Observations	11,505	11,462	11,462	11,462	11,462	11,010
Demographic Controls		✓	✓	✓	✓	✓
Poaching Flows/Prior Mobility			✓	✓	✓	✓
Other Restrictive Covenants				✓	✓	✓
Access to Confidential Info					✓	✓
Other HR Benefits						✓

Notes: Panel A examines the aggregate association of having a noncompete with an indicator for whether the employer shares all job-related information with the respondent, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. Controls are as defined on page 11. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB6: **1**(Training Last Year) Results, Sequential Addition of Covariates

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline			
Noncompete	0.152**	0.077**	0.073**	0.030	0.011	0.006
	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
R-Squared	0.010	0.160	0.174	0.181	0.190	0.199
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.209**	0.131**	0.126**	0.081**	0.063*	0.055*
	(0.022)	(0.024)	(0.024)	(0.023)	(0.025)	(0.025)
After Accepting Job	0.093**	0.017	0.007	-0.033	-0.056	-0.058
	(0.034)	(0.035)	(0.037)	(0.040)	(0.039)	(0.039)
With Promotion	0.016	-0.060	-0.039	-0.107	-0.135	-0.125
	(0.119)	(0.097)	(0.093)	(0.100)	(0.103)	(0.113)
Doesn't Remember	-0.033	-0.076	-0.055	-0.078	-0.091	-0.093
	(0.069)	(0.069)	(0.068)	(0.066)	(0.065)	(0.064)
P-value: $\beta_{Before} = \beta_{After}$	0.006	0.014	0.013	0.017	0.015	0.021
R-Squared	0.0135	0.162	0.176	0.183	0.192	0.201
Observations	11,505	11,462	11,462	11,462	11,462	11,010
Demographic Controls		✓	✓	✓	✓	✓
Poaching Flows/Prior Mobility			✓	✓	✓	✓
Other Restrictive Covenants				✓	✓	✓
Access to Confidential Info					✓	✓
Other HR Benefits						✓

Notes: Panel A examines the aggregate association of having a noncompete with an indicator for whether the respondent received training in the last year, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. Controls are as defined on page 11. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB7: **1**(Satisfied in Job) Results, Sequential Addition of Covariates

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline			
Noncompete	0.024	0.015	0.017	0.012	0.007	0.006
	(0.021)	(0.019)	(0.020)	(0.018)	(0.017)	(0.017)
R-Squared	0.000	0.0991	0.129	0.132	0.134	0.149
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.072**	0.060**	0.058*	0.052*	0.047*	0.045*
	(0.019)	(0.020)	(0.022)	(0.021)	(0.021)	(0.020)
After Accepting Job	-0.099*	-0.090*	-0.079*	-0.080*	-0.085*	-0.085*
	(0.045)	(0.036)	(0.035)	(0.035)	(0.034)	(0.035)
With Promotion	0.137*	0.070	0.080	0.070	0.065	0.051
	(0.064)	(0.067)	(0.062)	(0.068)	(0.069)	(0.071)
Doesn't Remember	0.077	0.043	0.040	0.043	0.037	0.042
	(0.050)	(0.044)	(0.044)	(0.045)	(0.046)	(0.047)
P-value: $\beta_{Before} = \beta_{After}$	0.001	0.000	0.001	0.002	0.002	0.003
R-Squared	0.004	0.102	0.131	0.134	0.136	0.151
Observations	11,505	11,462	11,462	11,462	11,462	11,010
Demographic Controls		✓	✓	✓	✓	✓
Poaching Flows/Prior Mobility			✓	✓	✓	✓
Other Restrictive Covenants				✓	✓	✓
Access to Confidential Info					✓	✓
Other HR Benefits						✓

Notes: Panel A examines the aggregate association of having a noncompete with an indicator for whether the respondent reports being satisfied in their job, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. Controls are as defined on page 11. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB8: Direct Evidence on the Price of a Noncompete

	(1)	(2)	(3)
	When did you first learn you would be asked to sign?		
	Before Accepting	After Accepting	*Overall*
Panel A: "What did your employer promise, either explicitly or implicitly, in exchange for asking you to sign a noncompete?"			
Nothing	0.84	0.91	0.86
More Compensation	0.09	0.04	0.07
Job Security	0.08	0.04	0.07
More Training	0.07	0.04	0.06
More Trust by Employer	0.07	0.04	0.06
Better Working Conditions	0.05	0.03	0.04
More Responsibility	0.05	0.02	0.04
Promotion	0.03	0.03	0.03
More Access to Confidential Information	0.04	0.03	0.03
More Access to Clients/Lists	0.03	0.02	0.02
More Client Referrals	0.02	0.02	0.02
Other Benefits	0.01	0.01	0.01
Panel B: "What do you believe you received in exchange for signing a noncompete?"			
Nothing	0.45	0.58	0.50
Job Security	0.33	0.25	0.30
More Trust by Employer	0.32	0.24	0.29
More Compensation	0.23	0.11	0.19
More Responsibility	0.17	0.14	0.16
More Access to Confidential Information	0.16	0.12	0.14
More Training	0.17	0.10	0.14
More Access to Clients/Lists	0.13	0.08	0.11
Better Working Conditions	0.13	0.08	0.11
Promotion	0.11	0.05	0.09
More Client Referrals	0.07	0.03	0.05
Other Benefits	0.01	0.02	0.01

Notes: The table shows the proportion of individuals who report receiving or being promised various benefits in exchange for agreeing to a noncompete conditional on when they were asked to agree. Those who signed a noncompete before a promotion or who can't recall are omitted from the columns (1) and (2) for brevity. Column (3) reports the overall average, and the rows are sorted based on these proportions.

Table OB9: Labor Market Outcomes: Treating "Maybes" as Own Category

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
				Panel A: Baseline				
Noncompete: Yes	0.096**	0.056*	0.013	-0.034	0.054**	-0.010	0.009	-0.000
	(0.027)	(0.024)	(0.031)	(0.027)	(0.020)	(0.022)	(0.017)	(0.017)
	[1.196]	[0.528]	[1.057]	[1.484]	[1]	[0.190]	[5.288]	[12.11]
		{0.202}		{0.547}		{0.0831}		{0.500}
Noncompete: Maybe	-0.041	-0.035	-0.052**	-0.047**	-0.069**	-0.050*	-0.019	-0.022
	(0.025)	(0.025)	(0.016)	(0.017)	(0.021)	(0.023)	(0.019)	(0.018)
R-Squared	0.503	0.542	0.102	0.148	0.163	0.200	0.0994	0.150
				Panel B: Heterogeneity by Timing of Notice				
First Learned of Noncompete								
Before Accepting Job	0.129**	0.083*	0.084**	0.029	0.108**	0.040	0.054**	0.038^{+}
	(0.034)	(0.031)	(0.027)	(0.026)	(0.025)	(0.026)	(0.019)	(0.020)
	[1.396]	[0.689]	[6.205]	[0.979]	[1.987]	[0.752]	[17.01]	[4.865]
		{0.265}		{0.359}		{0.310}		{1.716}
After Accepting Job	0.042	0.013	-0.112*	-0.150**	-0.008	-0.074^{+}	-0.097**	-0.092*
	(0.042)	(0.037)	(0.051)	(0.040)	(0.038)	(0.042)	(0.034)	(0.035)
	[0.900]	[0.355]	[63.12]	[11.65]	[0.436]	[1.547]	[4.847]	[7.744]
		{0.138}		{3.676}		{0.657}		{8.069}
With Promotion	0.188*	0.127	0.021	-0.002	-0.084	-0.139	0.064	0.045
	(0.090)	(0.087)	(0.089)	(0.103)	(0.098)	(0.115)	(0.069)	(0.072)
	[1.327]	[0.779]	[0.388]	[0.194]	[1.379]	[2.676]	[1.481]	[2.394]
		{0.263}		{0.118}		{0.995}		{9.510}
Doesn't Remember	0.007	-0.001	-0.067	-0.089	-0.100	-0.109^{+}	0.036	0.035
	(0.057)	(0.065)	(0.076)	(0.065)	(0.071)	(0.065)	(0.045)	(0.047)
	[0.286]	[0.168]	[3.612]	[6.218]	[3.527]	[7.089]	[1.358]	[5.530]
		{1.500}		{2.705}		{10.03}		{52.92}
Noncompete: Maybe	-0.041	-0.036	-0.053**	-0.048**	-0.070**	-0.050*	-0.019	-0.022
	(0.025)	(0.025)	(0.016)	(0.017)	(0.021)	(0.023)	(0.019)	(0.018)
P-value: $\beta_{Before} = \beta_{After}$	0.059	0.123	0.000	0.000	0.013	0.020	0.000	0.003
R-Squared	0.504	0.542	0.106	0.151	0.166	0.202	0.102	0.152
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are treated as their own category. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, where again the "maybe" group is treated as its own category and those not bound by a noncompete are the omitted category. We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB10: Labor Market Outcomes: Multiple Imputation

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
				Panel A: Baseline				
Noncompete	0.074**	0.038	0.015	-0.036	0.070**	0.008	0.022	0.017
	(0.027)	(0.029)	(0.027)	(0.025)	(0.020)	(0.024)	(0.020)	(0.019)
	[0.920]	[0.357]	[1.042]	[1.361]	[1.271]	[0.238]	[30.83]	[6.969]
		{0.137}		{0.544}		{0.113}		{1.551}
R-Squared	0.502	0.541	0.0998	0.147	0.160	0.199	0.0994	0.149
				Panel B: Heterogeneity by Timing of Notice				
First Learned of Noncompete								
Before Accepting Job	0.143**	0.091**	0.100**	0.036	0.134**	0.056*	0.063**	0.048*
	(0.033)	(0.031)	(0.026)	(0.025)	(0.025)	(0.026)	(0.021)	(0.021)
	[1.204]	[0.607]	[3.962]	[0.993]	[1.948]	[0.893]	[4.704]	[4.295]
		{0.253}		{0.408}		{0.395}		{2.084}
After Accepting Job	0.057	0.022	-0.094^{+}	-0.140**	0.020	-0.056	-0.088*	-0.081*
	(0.043)	(0.038)	(0.050)	(0.039)	(0.036)	(0.040)	(0.036)	(0.035)
	[0.739]	[0.298]	[12.01]	[7.916]	[0.132]	[1.044]	[6.788]	[7.845]
		{0.136}		{2.842}		{0.452}		{6.257}
With Promotion	0.202*	0.134	0.038	0.005	-0.057	-0.124	0.072	0.054
	(0.089)	(0.086)	(0.089)	(0.104)	(0.096)	(0.112)	(0.068)	(0.071)
	[1.217]	[0.725]	[0.633]	[0.228]	[0.549]	[2.123]	[1.433]	[2.620]
		{0.259}		{0.134}		{0.820}		{45.26}
Doesn't Remember	0.020	0.008	-0.050	-0.079	-0.073	-0.092	0.045	0.045
	(0.056)	(0.064)	(0.076)	(0.064)	(0.070)	(0.064)	(0.044)	(0.047)
	[0.0619]	[0.138]	[1.968]	[4.262]	[1.747]	[4.252]	[1.291]	[5.574]
		{0.393}		{2.033}		{3.855}		{46.74}
Imputed Signer	-0.003	-0.020	-0.019	-0.055	0.046	0.010	0.033	0.032
	(0.054)	(0.059)	(0.041)	(0.042)	(0.043)	(0.049)	(0.039)	(0.039)
P-value: $\beta_{Before} = \beta_{After}$	0.062	0.127	0.000	0.000	0.014	0.021	0.000	0.003
R-Squared	0.503	0.541	0.104	0.150	0.163	0.201	0.102	0.152
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are assigned to the "yes" group or the "no" group using multiple imputation. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category, and where those imputed to have noncompetes are treated separately (because they were unaware of their noncompete status and so were not asked the timing question). We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB11: Labor Market Outcomes: Unweighted

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer)		**1**(Training Last Year)		**1**(Satisfied in Job)	
Panel A: Baseline								
Noncompete	0.078**	0.052**	0.009	-0.031^{+}	0.065**	0.003	0.014	0.008
	(0.017)	(0.016)	(0.016)	(0.015)	(0.013)	(0.014)	(0.016)	(0.014)
	[0.993]	[0.530]	[0.290]	[1.392]	[1.198]	[0.0831]	[0.848]	[1.235]
		{0.209}		{0.776}		{0.0353}		{1.426}
R-Squared	0.469	0.495	0.0647	0.0992	0.129	0.161	0.0646	0.101
Panel B: Heterogeneity by Timing of Notice								
First Learned of Noncompete								
Before Accepting Job	0.087**	0.057**	0.075**	0.030^{+}	0.096**	0.032^{+}	0.065**	0.054**
	(0.020)	(0.018)	(0.017)	(0.016)	(0.015)	(0.017)	(0.015)	(0.014)
	[0.959]	[0.542]	[2.599]	[1.040]	[1.738]	[0.674]	[3.185]	[4.054]
		{0.221}		{0.559}		{0.282}		{3.212}
After Accepting Job	0.051^{+}	0.031	-0.104**	-0.138**	0.024	-0.039^{+}	-0.077**	-0.071**
	(0.026)	(0.027)	(0.026)	(0.026)	(0.020)	(0.021)	(0.025)	(0.024)
	[1.090]	[0.544]	[7.038]	[10.39]	[0.171]	[0.985]	[14.89]	[10.33]
		{0.215}		{5.096}		{0.389}		{7.317}
With Promotion	0.283**	0.249**	0.008	-0.013	-0.032	-0.091	-0.010	-0.017
	(0.086)	(0.085)	(0.070)	(0.079)	(0.066)	(0.072)	(0.076)	(0.075)
	[2.587]	[1.857]	[0.221]	[0.451]	[0.112]	[1.979]	[0.235]	[1.080]
		{0.801}		{0.431}		{0.805}		{75.62}
Doesn't Remember	0.029	0.022	-0.096*	-0.102*	0.007	-0.019	-0.046	-0.040
	(0.039)	(0.041)	(0.047)	(0.044)	(0.058)	(0.059)	(0.042)	(0.042)
	[0.383]	[0.393]	[6.928]	[15.11]	[1.062]	[1.245]	[1.922]	[9.682]
		{0.442}		{9.503}		{0.439}		{4.960}
P-value: $\beta_{Before} = \beta_{After}$	0.183	0.339	0.000	0.000	0.005	0.006	0.000	0.000
R-Squared	0.470	0.495	0.069	0.103	0.130	0.162	0.067	0.104
Observations	11,462	11,010	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table replicates the main labor market outcome results in Table 8, except the analysis is not weighted. Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Tables OB9 and OB10 show the results of treating the "maybe" group as a separate category and imputing "yes" or "no" status for each respondent in the "maybe" group, respectively. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB12: Labor Market Outcomes: Dropping Noncompete-Focused Respondents

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Ln(Hourly Wage)		**1**(Employer Shares Info)		**1**(Training Last Year)		**1**(Satisfied in Job)	
				Panel A: Baseline				
Noncompete	0.079*	0.045	0.036	-0.009	0.078**	0.011	0.010	0.009
	(0.032)	(0.028)	(0.035)	(0.031)	(0.026)	(0.028)	(0.021)	(0.021)
				Panel B: Heterogeneity by Timing of Notice				
First Learned of Noncompete								
Before Accepting Job	0.122**	0.083*	0.092*	0.040	0.126**	0.055	0.055*	0.048
	(0.043)	(0.040)	(0.037)	(0.035)	(0.035)	(0.035)	(0.026)	(0.030)
After Accepting Job	-0.008	-0.037	-0.087+	-0.123**	0.026	-0.047	-0.107*	-0.092*
	(0.055)	(0.048)	(0.051)	(0.042)	(0.046)	(0.047)	(0.044)	(0.041)
With Promotion	0.249**	0.196*	0.066	0.045	-0.082	-0.153	0.127*	0.113+
	(0.090)	(0.085)	(0.109)	(0.123)	(0.114)	(0.135)	(0.062)	(0.059)
Doesn't Remember	0.031	0.038	0.071	0.044	-0.023	-0.041	0.066	0.068
	(0.070)	(0.080)	(0.096)	(0.075)	(0.067)	(0.064)	(0.059)	(0.059)
P-value: $\beta_{Before} = \beta_{After}$	0.054	0.058	0.001	0.000	0.081	0.067	0.005	0.012
Observations	8,982	8,623	8,982	8,623	8,982	8,623	8,982	8,623
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This table replicates the main labor market outcome results in Table 8, except that it excludes respondents who answered the survey question (which appears at the end of the survey) "Why did you decide to take this survey?" with "I wanted to share my experiences with noncompetes." Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB13: Related Dependent Variables

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	**1**(Job is Secure)		**1**(Employer Committed to Upgrading Skills)		**1**(Boomerang Employee)	
			Panel A: Baseline			
Noncompete	0.008	-0.005	0.049*	0.004	-0.011	-0.041*
	(0.021)	(0.023)	(0.019)	(0.017)	(0.023)	(0.020)
	[1.709]	[0.748]	[1.271]	[0.176]	[1.132]	[3.260]
		{0.272}		{0.109}		{1.436}
R-Squared	0.103	0.135	0.116	0.171	0.080	0.120
			Panel B: Heterogeneity by Timing of Notice			
First Learned of Noncompete						
Before Accepting Job	0.038^{+}	0.022	0.111**	0.059**	0.058*	0.027
	(0.020)	(0.020)	(0.020)	(0.020)	(0.022)	(0.021)
	[96.02]	[2.938]	[2.746]	[1.603]	[3.792]	[1.388]
		{0.736}		{0.865}		{0.617}
After Accepting Job	-0.055	-0.063	-0.059^{+}	-0.100**	-0.142**	-0.173**
	(0.035)	(0.041)	(0.033)	(0.032)	(0.042)	(0.038)
	[6.924]	[15.77]	[2.300]	[4.884]	[171.5]	[53.05]
		{18.66}		{3.443}		{13.39}
With Promotion	0.068	0.041	-0.043	-0.067	0.097	0.109^{+}
	(0.050)	(0.058)	(0.122)	(0.139)	(0.062)	(0.059)
	[1.988]	[1.606]	[0.500]	[1.391]	[8.684]	[7.501]
		{1.886}		{1.670}		{3.915}
Doesn't Remember	-0.004	-0.003	0.004	-0.006	-0.089	-0.098
	(0.036)	(0.037)	(0.067)	(0.055)	(0.077)	(0.071)
	[0.335]	[0.437]	[0.113]	[0.377]	[4.398]	[10.06]
		{1.293}		{0.692}		{6.871}
P-Value: $\beta_{Before} = \beta_{After}$	0.005	0.019	0.000	0.000	0.000	0.000
R-Squared	0.104	0.136	0.119	0.174	0.0852	0.125
Observations	11,462	11,010	11,462	11,010	11,462	11,010
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Advanced Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the relationship between noncompete status and timing and other dependent variables of interest. The dependent variable in columns (1) and (2) is an indicator for whether the employee agrees or strongly agrees that their job is secure. The dependent variable in columns (3) and (4) is an indicator for whether the employee agrees or strongly agrees that their employer is committed to upgrading their skills. The dependent variable in columns (5) and (6) is an indicator for whether the employee would consider returning to their employer if they were ever to leave (i.e., become a "boomerang" employee). Panel A examines the aggregate association of having a noncompete with the outcome of the column, where those who have never heard of a noncompete or are otherwise unaware if they have signed one are grouped with the "no" category of respondents. Panel B allows the direction and magnitude of any association to vary conditional on when the employer first requested the noncompete, with those not bound by a noncompete as the omitted category. We define the variables that make up our basic and advanced controls on page 11. We report the selection test relative to a model with no controls in square brackets ('[ ]'), and we report the selection test between the models with basic and advanced controls in curly brackets ('{ }'). In both cases, the selection test statistic is calculated with the Stata command `psacalc`, using as the maximum R-Squared Oster's suggested 30% more than the R-Squared from the model that includes both the basic and advanced controls. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

Table OB14: Predicting Timing of Noncompete Notice

Model: OLS	(1a)	(2a)		(1b)	(2b)
Dependent Variable:	*1(Before Accepted Job)*				
Ln(State Unemployment Rate at Hire)	0.019	0.002	**1**(Highest Degree = BA)	-0.011	-0.051
	(0.050)	(0.047)		(0.042)	(0.050)
Ln(Labor Force Size in State at Hire)	-0.032	-0.030	**1**(Highest Degree > BA)	-0.002	-0.041
	(0.027)	(0.025)		(0.053)	(0.056)
1(Paid by Salary)	0.093*	0.068	**1**(Multi-Unit Employer)	-0.030	-0.061
	(0.045)	(0.053)		(0.061)	(0.072)
1(Paid by Commission)	0.202*	0.125	**1**(Employer Size 25–100)	-0.002	0.004
	(0.081)	(0.106)		(0.080)	(0.082)
1(Paid by Other Means)	0.066	-0.104	**1**(Employer Size 101–250)	0.074	0.019
	(0.164)	(0.196)		(0.080)	(0.094)
Age (in years)	-0.044	-0.010	**1**(Employer Size 251–500)	0.089	0.097
	(0.054)	(0.047)		(0.088)	(0.095)
Age^2	0.001	0.000	**1**(Employer Size 501–1,000)	0.059	0.022
	(0.001)	(0.001)		(0.078)	(0.082)
Age^3	-0.000	-0.000	**1**(Employer Size 1,001–2,500)	0.077	0.046
	(0.000)	(0.000)		(0.082)	(0.087)
Hours Worked per Week	-0.006	0.002	**1**(Employer Size 2,500–5,000)	0.012	0.001
	(0.012)	(0.012)		(0.098)	(0.104)
Weeks Worked per Year	-0.004	-0.001	**1**(Employer Size > 5,000)	0.006	-0.002
	(0.007)	(0.007)		(0.089)	(0.093)
Hours∗Weeks	0.000	0.000	Ln(Establishments in County-Industry)	-0.005	-0.005
	(0.000)	(0.000)		(0.010)	(0.014)
1(Male)	0.092*	0.089*	Noncompete Enforceability	-0.025	-0.024
	(0.040)	(0.043)		(0.015)	(0.015)
1(Private Nonprofit Employer)	0.081	0.061			
	(0.107)	(0.129)			
1(Public Health System Employer)	0.106	0.065			
	(0.094)	(0.117)			
Observations	1,568	1,568		1,568	1,568
Occ-Ind FE	No	Yes		No	Yes

Notes: The sample only includes individuals who report signing a noncompete (no imputed individuals). Those who were asked to sign with a promotion or cannot remember are excluded. Column (1a) and (1b) are the same regression, without Occupation by Industry FE, while Column (2a) and (2b) are the same regression, with Occupation by Industry FE. ** p<0.01, * p<0.05, $^{+}$ p<0.1. Standard errors in parentheses, clustered at the state level.

C The Enforceability of Noncompetes

Most noncompete scholarship revolves around whether and to what extent noncompetes should be enforced in court (Blake, 1960; Garrison and Wendt, 2008; Marx et al., 2009). In the U.S., noncompetes are governed by state statutes and state case law, with states often coming to markedly different conclusions (Bishara, 2011). For example, California adopted a policy of nonenforceability in 1872 (Gilson, 1999), which remains the policy of the state today, while Florida adopted a statute in 1996 (Florida Statutes §542.335 (g)) that instructed courts to "... not consider any individualized economic or other hardship that might be caused to the person against whom enforcement is sought." Most states employ a three-pronged test, commonly referred to as the "reasonableness criterion," in which the court balances the protection needed by the employer and the harm done to the employee and society (Bishara, 2011). The state-by-state series by Malsberger et al. (2012) provides information regarding when a given state will enforce noncompetes, and many have used this information to quantify the enforceability of noncompetes. In this paper, we use the 2009 measure developed in Starr (2019), which was built off the initial coding of Malsberger et al. (2012) conducted by Bishara (2011). We report the table from Starr (2019) below.

Table OC1: Noncompete Enforceability Index (Starr, 2019)

State	1991	2009	State	1991	2009
AK	-1.33	-0.98	MS	-0.20	0.04
AL	0.36	0.36	MT	-0.63	-0.65
AR	-0.62	-0.58	NC	0.18	0.18
AZ	-0.16	0.15	ND	-4.23	-4.23
CA	-3.76	-3.79	NE	-0.13	-0.13
CO	0.38	0.38	NH	0.26	0.26
CT	0.62	1.26	NJ	0.47	0.90
DC	0.12	0.12	NM	0.74	0.74
DE	0.18	0.52	NV	-0.62	0.03
FL	1.15	1.60	NY	-0.73	-1.15
GA	0.45	0.02	OH	-0.18	0.08
HI	-0.83	-0.17	OK	-0.80	-0.94
IA	0.19	1.01	OR	0.14	0.14
ID	-0.01	0.77	PA	-0.14	0.14
IL	0.55	0.95	RI	-0.67	-0.33
IN	0.70	0.70	SC	-0.20	-0.27
KS	0.69	1.21	SD	0.37	1.02
KY	0.61	0.85	TN	0.22	0.45
LA	-0.70	0.50	TX	-0.04	-0.28
MA	0.87	0.48	UT	1.00	1.00
MD	0.15	0.60	VA	0.09	-0.29
ME	0.06	0.41	VT	0.30	0.60
MI	0.07	0.46	WA	0.64	0.34
MN	-0.07	-0.07	WI	0.16	-0.09
MO	0.93	1.08	WV	-0.80	-0.80
			WY	-0.65	0.23

D Potential Instruments for Noncompetes

Exogenous variation in the use of noncompetes is a traditional prerequisite for rigorously identifying the causal effects of such provisions. Unfortunately, at a minimum, noncompete use may be endogenous to outcomes driven by (potentially unobservable) employer and employee characteristics. The most natural instrument for noncompete use is the set of laws that govern their enforceability or changes in those laws over time. Employers that reside in states where noncompetes are exogenously easier to enforce have greater incentives to use noncompetes, but these laws may not affect employee outcomes except through the higher incidence of noncompetes. In this section, we explore what scrutiny of these instruments reveals, both in terms of predicting the use of noncompetes and in terms of second-stage effects.

We consider four potential instruments related to the enforceability of noncompetes. The first two are simply cross-sectional measures of noncompete enforceability. The second two exploit recent policy changes in noncompete enforceability at the state level. The first of these latter two variables, "Changes in Enforceability," is set to 1 if the respondent was hired after an increase in noncompete enforceability and to –1 if the respondent was hired after a reduction in enforceability. The value is set to 0 if there were no changes in the state in the last 20 years. These variables are subsequently decomposed into states that increased enforceability in the last five years and states that reduced enforceability in the last five years. All of these policy changes are gathered from Ewens and Marx (2017), who examined the state-by-state treatises of noncompete case law and statutes in Malsberger et al. (2012). The logic of these latter two instruments is to compare an employee who was hired before the regulatory change to an employee in the same state who was hired just after the regulatory change. The first-stage and second-stage results for these instruments are shown in Table OD1.

The first two instruments show that enforceability is positively associated with the use of noncompetes, but the second-stage estimates are implausibly large. Further analysis suggests that the exclusion restriction is violated as the enforceability measure appears to have a negative main effect on wages, as we show in column (1) in Table 9. The second set of instruments does not produce any statistically significant first-stage results, and indeed they point in opposite directions.

Table OD1: Potential Instruments for Noncompete Use

	(1)	(2)	(3)	(4)
First Stage				
Dependent Variable: Indicator for Noncompete Provision				
Noncompete Enforceability	0.008^{+}			
	(0.004)			
1st Quartile of Enforceability		0.046*		
		(0.017)		
2nd Quartile of Enforceability		0.028		
		(0.019)		
3rd Quartile of Enforceability		0.050^{+}		
		(0.026)		
4th Quartile of Enforceability		0.040^{+}		
		(0.022)		
5th Quartile of Enforceability		0.035^{+}		
		(0.020)		
Changes in Enforceability			-0.027	
			(0.021)	
Increased Enforceability Last 5 Years				-0.017
				(0.024)
Decreased Enforceability Last 5 Years				0.040
				(0.047)
Second Stage				
Dependent Variable: Ln(Hourly Wage)				
Noncompete	-2.690	-0.887	1.949	0.631
	(2.576)	(1.112)	(8.198)	(15.751)
Basic Controls	Yes	Yes	Yes	Yes
Occupation-Industry FE	Yes	Yes	Yes	Yes
Flow & Info Controls	Yes	Yes	Yes	Yes
Benefits & Contract FE	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes

Notes: The table shows our analysis of potential noncompete enforceability instruments. The instrument in column (1) is a linear measure of enforceability at the state level, while the instruments in second column are indicators for nonenforcing states and quartiles of enforcing states (the omitted category in column (2) is the set of nonenforcing states). Increased enforceability (column 3) is a variable that equals 1 if the respondent was hired after the state increased noncompete enforceability, 0 if the respondent was hired with no change in enforceability over the previous 20 years, and –1 if the respondent was hired after the state reduced noncompete enforceability. Column (4) repeats this analysis but separates out increases and decreases and focuses on changes only in the last 5 years. Columns (3) and (4) condition on tenure and have state fixed effects to compare the likelihood of having a noncompete to others who were hired in the state before the policy change. ** p<0.01, * p<0.05, $^{+}$ p<0.1. We show standard errors in parentheses, clustered at the state level.

E Examples of Noncompetes

Below are examples of actual covenants not to compete that we believe were recently deployed by four organizations: Amazon.com, Inc. (e-commerce company); Jimmy John's Franchise, LLC (fast food company); Blackbaud, Inc. (software company), and Girls on the Run of Silicon Valley (nonprofit). Note: with the exception of the Girls on the Run Noncompete, which we received when we applied for a job, we received the examples we reproduce here from third parties or obtained them online and so we do not vouch for their legal authenticity.

Figure OE1: Example of Amazon Noncompete

4. RESTRICTIVE COVENANTS.

4.1 Non-Competition. During employment and for 18 months after the Separation Date, Employee will not, directly or indirectly, whether on Employee's own behalf or on behalf of any other entity (for example, as an employee, agent, partner, or consultant), engage in or support the development, manufacture, marketing, or sale of any product or service that competes or is intended to compete with any product or service sold, offered, or otherwise provided by Amazon (or intended to be sold, offered, or otherwise provided by Amazon in the future) that Employee worked on or supported, or about which Employee obtained or received Confidential Information.

Figure OE2: Example of Jimmy John's Noncompete

3. **Non-Competition Covenant.** Employee covenants and agrees that, during his or her employment with Employer and for a period of two (2) years after either the effective date of termination of his or her employment for any reason, whether voluntary or involuntary and whether by Employer or Employee, or the date on which Employee begins to comply with this paragraph, whichever is later, he or she will not have any direct or indirect interest in or perform services for (whether as an owner, partner, investor, director, officer, representative, manager, employee, principal, agent, advisor, or consultant) any business which derives more than ten percent (10%) of its revenue from selling submarine, hero-type, deli-style, pita and/or wrapped or rolled sandwiches and which is located within three (3) miles of either (1) 9641 N Milwaukee Ave , Niles IL 60714 [insert address of employment], or (2) any such other JIMMY JOHN'S® Sandwich Shop operated by JJF, one of its authorized franchisees, or any of JJF's affiliates.

Employee also acknowledges and agrees that, for at least twelve (12) months after the effective date of termination of his or her employment for any reason, whether voluntary or involuntary and whether by Employer or Employee, Employee may not become a partner of or investor/owner with, or work for, another JIMMY JOHN'S® Sandwich Shop franchisee. Employee acknowledges that other JIMMY JOHN'S® Sandwich Shop franchisees are contractually prohibited by JJF from recruiting Employee as a partner or investor/owner, or from hiring Employee, for at least twelve (12) months after Employee leaves his or her employment with Employer (regardless of the reason for his or her departure).

Figure OE3: Example of Blackbaud Executive Noncompete

7.1 Noncompetition Provisions. Executive recognizes and agrees that the Company has many substantial, legitimate business interests that can be protected only by Executive agreeing not to compete with the Company or its subsidiaries under certain circumstances. These interests include, without limitation, the Company's contacts and relationships with its customers, the Company's reputation and goodwill in the industry, the financial and other support afforded by the Company, and the Company's rights in its confidential information. Executive therefore agrees that during his employment with the Company and for the twelve (12) month period of time following the termination of such employment by either party for any reason, he will not, without the prior written consent of the Company, engage in any of the following activities in the United States (the "Protected Zones"), relating to the Protected Businesses (as defined below):

a. engage in, manage, operate, control or supervise, or participate in the management, operation, control or supervision of, any business or entity which provides products or services directly competitive with those being actively developed, manufactured, marketed, sold or otherwise provided by the Company or its subsidiaries as of the date hereof (the "Protected Businesses") in the Protected Zones;

b. have any ownership or financial interest, directly or indirectly, in any entity in the Protected Zones engaged in the Protected Businesses, including, without limitation, as an individual, partner, shareholder (other than as an owner of an entity in which Executive owns less than 5% of the economic interests), officer, directly, executive, principal, agent or consultant;

c. solicit, acquire or conduct any Protected Business from or with any customers of the Company or its subsidiaries (as defined below) in the Protected Zones;

d. solicit any of the employees or independent contractors of the Company or its subsidiaries or induce any such persons to terminate their employment or contractual relationships with any such entities; and/or

e. serve as an officer or director of any entity engaged in any of the Protected Businesses in the Protected Zones.

Figure OE4: Example of Girls on the Run of Silicon Valley Noncompete

NON-COMPETE AGREEMENT:

As a coach and volunteer for Girls on the Run of Silicon Valley, I agree to the following:

1.) I will not deliver the Girls on the Run program or any similar program unless I am working as an employee or volunteer of Girls on the Run.

2.) I may not create or help develop a program that has similar goals and structure to that of Girls on the Run International within a two-year period of my involvement with Girls on the Run.

F Data Online Appendix

This article's data derive from a labor force (i.e., employee) survey that we designed and implemented between April and July 2014. Our goal in conducting the survey was to understand the use and effects of covenants not to compete ("noncompetes"), both in a respondent's current job and over the course of a respondent's career. In this appendix, we describe the survey's origin, design, and sampling frame as well as our cleaning and processing of the data to clarify important aspects of this article's analysis. We draw heavily on an earlier technical article that describes these issues in meticulous detail (Prescott et al., 2016), and virtually identical content can be found in the appendix of Starr et al. (forthcoming).

F.1 Sampling Frame and Data Collection Methodology

The sampling frame for this study are U.S. labor force participants aged 18–75 years who are working in the private sector (for profit or nonprofit), working for a public health system,[46] or unemployed and looking for work. We excluded individuals who reported being self-employed, government employees, non-U.S. citizens, or out of the labor force. To collect the data, we considered a few possible survey platforms and collection methods, including using RAND's American Life Panel (ALP), conducting a random-digit-dial survey, and adding questions to ongoing established surveys like the NLSY or the PSID. Ultimately, we concluded that our work required a nationally representative sample that was larger than the ALP could provide. We also determined that, to obtain a complete picture of an employee's noncompete experiences, we needed to collect too many different pieces of new information to build on existing surveys. Instead, it made more sense to design and draft a noncompete-specific survey ourselves so that we would be able to ask all of the potentially relevant questions. In the end, we settled on using Qualtrics, a reputable online survey company with access to more than 10 million *verified* panel respondents.[47]

The target size for this data-collection project was 10,000 completed surveys. We were able to control the characteristics of the final sample through the use of quotas, which are simply constraints on the numbers of respondents with particular characteristics or sets of characteristics. In particular, we sought a final sample in which respondents were 50% male; 60% with at least a bachelor's degree; 50% with

[46] We initially considered focusing only on the private sector, but we recognized that public health systems (e.g., those associated with public universities) also use noncompetes extensively.

[47] The difference between verified and unverified survey respondents is important. The use of unverified survey respondents means that there is no external validation of any information the respondent provides (e.g., a Google or Facebook survey), while verified survey respondents have had some information verified by the survey company. We signed up with a number of these companies to see how they vetted individuals who agreed to respond to online surveys. A typical experience involves filling out an intake form and providing fairly detailed demographic information, including a contact number. A day or so after completing the intake form, the applicant receives a phone call from the survey company at the number the applicant provided. On the call, the applicant is asked a series of questions related to the information previously provided on the intake form. Verified respondents are those who are reachable at the phone number supplied and who corroborate the information initially supplied.

earnings of at least $50,000 annually from their current, highest paying job; and 30% over the age of 55 years. We chose these particular thresholds either to align the sample with the corresponding sample moments for labor force participants in the 2012 American Community Survey (ACS) or to oversample certain populations of interest.

Respondents who completed the survey were compensated differently depending on the panel provider: some were paid $1.50 and entered into prize sweepstakes, others were given tokens or points in online games that they were playing. Respondents took a median time of approximately 28 minutes to complete the survey. Due to the length of the survey, we used three "attention filters" spaced evenly throughout the survey to ensure that respondents were paying attention to the questions. Before we describe the cleaning process for our survey data, we briefly outline the costs and benefits of using online surveys.[48]

F.2 Costs and Benefits of Online Surveys

Online surveys come with a variety of benefits. Relative to random-digit-dial or in-person surveys, the cost per respondent is orders of magnitude lower and the data-collection time is orders of magnitude faster. The interactive survey interface also allows the survey designer to write complicated, nested questions that are easy for respondents to answer through an online platform. Online surveys also allow individuals to respond at their leisure via their preferred method (e.g., computer, phone, tablet, etc.) from wherever they wish (e.g., work, home, or coffee shop). For these reasons, Reuters, the well-known national polling company, has conducted all of its polling since 2012 online, including its 2016 Presidential election polling.[49]

However, these benefits come at a potentially high cost: a sample of online survey takers may not be representative of the population of interest to researchers or policymakers. There are four sample selection concerns in particular. First, not all people in the U.S. labor force are online. Second, not all of those online register to take surveys. Third, not all of those who register to take surveys receive any particular survey. Fourth, not all of those who are invited to take a survey finish it. Among these sample selection concerns, only the second one is unique to online surveys.[50] With respect to the fourth, alternatives seem unlikely to be better. Kennedy and Hartig (2019) find that survey response to random-digit dialing fell to 6% in 2018, raising the very important question whether a sample resulting from a random-digit-dial survey is still a random sample of the population. We address each of these selection concerns in Prescott et al. (2016) and discuss the second concern in particular in Section F.4.

[48] The information contained in the following sections can be found in Tables 1–18 in Prescott et al. (2016).

[49] See the "About" tab at `http://polling.reuters.com/`.

[50] For example, random-digit-dial surveys miss those without a phone, those who have a phone but do not receive the survey call, and those who receive the call but decline to take the survey.

F.3 Survey Cleaning

Qualtrics fielded the survey and obtained 14,668 completed surveys. When we began to review this initial set of responses, we recognized that individuals with the same IP address may have taken the survey multiple times given there were incentives. To address this, we retained only the first attempt to take the survey from a given IP address and only if that attempt resulted in a completed survey, which produced a sample of 12,369 respondents. We next detected, by inspecting the raw data by hand, that some individuals appeared to have the exact same responses, even for write-in questions, despite the fact that the IP addresses recorded in the survey data were different. To weed these out, we compared individual responses for those with the same gender, age, and race, living in the same state and zip code, and working in the same county. We found 665 possible repeat survey takers; the majority of these respondents took the survey with two different panel partners. We reviewed these potential repeat survey takers by hand, and, among those identified as repeat takers from different IP addresses, we kept the first observation and dropped all others, leaving us with a sample of 12,090 respondents.[51]

In the next round of cleaning, we examined individual answers to identify any that were internally inconsistent or unreasonable in substance. In doing so, we developed a "flagging" algorithm that flagged individuals for making mistakes within or across questions, in addition to manually reading through text entry answers. In analyzing these answers, we discovered that some individuals were intentionally non-compliant (e.g., writing curse words or gibberish instead of their job title), while others simply made idiosyncratic errors (e.g., noting that their entire employer was smaller than their establishment—that is, their particular office or factory). We dropped respondents entirely if we deemed them to be intentionally noncompliant because their singular responses indicated that they did not take the survey seriously. This step left us with 11,529 survey responses. [52]

In the last round of cleaning, we began with those who had clean surveys and those who had made some sort of idiosyncratic error. From our flagging algorithm, we determined that 82.2% had no flags and that 16.05% had just one flag (see Table 6 in Prescott et al. (2016)). The most common flag was reporting earnings below the minimum wage (often 0), which was true for 1,007 of the 11,529 respondents. The challenge we faced was how to handle these flagged variables. We adopted four approaches: the first was to do nothing—simply, retain all of offending values as they were. The second was to drop all observations with any flag. The third was to replace offending values as missing. The fourth was to impute or otherwise correct offending values. Our preferred method, and the one we use in this article (although our findings are not very sensitive to this choice), is to impute or correct these offending values. Specifically, we "repaired" entries that were marred by idiosyncratic inconsistency by replacing the less reliable, offending value with

[51] See Tables 3–5 in Prescott et al. (2016) for more details.
[52] See pp.412–14 in Prescott et al. (2016) for more details.

the value closest to the originally submitted value that would not be inconsistent with the respondent's other answers. When an answer was clearly unreasonable or missing, and there was no workable single imputation procedure, we applied multiple imputation methods to calculate substitute values for the original missing or unreasonable survey entries.

We also reviewed by hand the values of reported earnings, occupations, and industries, due to their importance in our work. With regard to compensation, we manually reviewed all reported earnings greater than $200,000 per year and cross-checked them with the individual's job title and duties to ensure the amount seemed appropriate. We also examined potential typos in the number of zeros (e.g., the sizable real-world difference between $20,000 and $200,000 may be missed on a screen by survey respondents) by comparing reported annual earnings to expected annual earnings in subsequent years. If a typo was made by omitting a zero or by including an extra zero, we would expect to see a ratio of 0.1 or 10. We imputed earnings that were unreasonable if we were unable to correct the entry in a reliable way. With regard to occupation and industry, we had respondents self-select two-digit NAICS and SOC codes within the survey and also report their job title, occupational duties, and employer's line of business. To verify the two-digit NAICS and SOC codes—which are crucial for both weighting and fixed effects in our empirical work—we had four sets of RAs independently code the 11,529 responses by taking job titles, occupational duties, and employer descriptions and matching them with the appropriate two-digit NAICS and SOC codes.[53] As part of this process, we found that 24 individuals in the sample were self-employed, worked for the government, or were retired, thus reducing our total number of respondents to 11,505.

F.4 Sample Selection

As we observe above, there are four primary sample selection concerns with an online survey like ours: (1) not everybody is online; (2) not everybody online signs up for online surveys; (3) not everybody who signs up for online surveys receives a particular survey; and (4) not everybody who receives a survey manages to complete it. We describe these issues in greater detail in Section II.E in Prescott et al. (2016). All survey research must confront issues (1), (3) and (4)—the only unique selection concern for online surveys is (2). The key question is why individuals sign up to take online surveys and whether that reason is associated with their noncompete status or experiences.[54] To understand why the individuals who responded to our survey agreed to take online surveys, we asked them directly, and their responses were tabulated in Table 13 in Prescott et al. (2016). The two most common reasons individuals report to explain their interest in taking online surveys are that they enjoy the rewards (59%) and sharing their opinions (58%). Only

[53] See p.422 of Prescott et al. (2016) for details.

[54] A look at the population of online survey takers (see Table 12 of Prescott et al. (2016)) shows that relative to the average labor force participant they tend to be female and less likely to be in full-time employment.

40% indicated that they wanted money, and only 23% claimed that they needed money. Taking these responses seriously, the crucial selection question is, conditional on observables, whether individuals who like the available rewards or sharing their opinions are less likely to be in jobs that require noncompetes. We believe it is certainly plausible that there is no such relationship.

A related sample selection concern is that individuals who participate in a survey may for some reason lie or otherwise provide inaccurate information in a systematic way. We designed our cleaning strategy with the explicit goal of weeding out such individuals. But of course in any surveying effort legitimate concerns remain about the validity of the responses of the individuals who remain in the sample. To assuage these concerns, we present in Table OF1 the self-described job title, self-described occupational duties, and self-described industries for 15 randomly selected observations. These randomly selected respondents include a sales rep, a nurse, an analyst, a pizza delivery driver, an optometrist, and a programmer analyst. Reading their job-duty descriptions reveals a striking amount of detail, suggesting not only that these respondents answered the survey's questions carefully but also that they were responding truthfully.

Table OF1: Randomly Selected Self-Described Job Titles, Job Duties, and Industries)

	Self-Described Job Title	Self-Described Job Duties	Self-Described Industry
1	Associate Analyst	My current job duties are to review and evaluate telephone recordings between our customers and customer contact representatives.	My current employer is a regional utilitiy company which provides/sells electricity and natural gas to residential and commercial customers.
2	project manager	Design and staff community health clinics, write proposals, seek funding, evaluate and educate	Ensure children of low income families get preventive health and treatment if necessary
3	Quality Assurance Director	Review reports before going to our clients	Insurance Inspection Services
4	optometrist	Care for patient's ocular health	Optometry
5	purchasing clerk	I have receptionist duties including purchasing office supplies and filing the shipping department's paperwork.	retail art gallery
6	sales rep	account manager for a sales base	sells office supplies and equipment
7	Sales Associate	Sell phones and other communication devices, assist customers and resolve issues.	Retail sales company for cell phone business
8	Programmer analyst	Software developer	IT Consulting
9	Customer Service	I take phone calls from Customers.	My employer provides Health Insurance.
10	Certified Medical Assistant	Assist the doctor in the office and minor office procedures while making sure the office runs efficiently.	Healthcare provider
11	Analyst	researching our site's traffic	Publishing
12	Registered Nurse	I am responsible for providing dialysis services to current inpatients	It is a rehabilitation hospital
13	Title Coordinator	Process recorded deed of trust	Issue title policies
14	LEGAL ASSISTANT	INTERACT W/STATE BOARD OF WORKERS'COMP, PROVIDE PERSONAL INJURY REPRESENTATION, INVOLVES HIPAA LAWS	PERSONAL INJURY/WORKERS' COMP ATTORNEY
15	delivery driver	deliver food to people	pizza

F.5 Weighting and Imputation

In this section, we describe our approach to 1) weighting our survey data and 2) imputing values that are missing in our data or that we identified as problematic and marked as missing during the data cleaning process. The fact that weights need to be incorporated into the imputation step to impute unbiased population values complicates these two tasks. In line with current survey methods, we generated our analysis data by weighting our nonmissing data elements, imputing the missing variables (including the weights in the imputation step), and then reweighting the data given the imputed values so that the resulting analysis data are nationally representative. Below, after discussing our weighting approach, we explain how we combined weighting and multiple imputation methods to assemble our data.

With respect to weighting, we considered and compared several candidate approaches,[55] including post-stratification, iterative proportional fitting (also called raking), and propensity score weighting. Details on these methods can be found in Kalton and Flores-Cervantes (2003). For each method, we evaluated a variety of potential weighting variables, and then we examined the ability of each weighting scheme to match the distributions of variables within the 2014 American Community Survey (ACS) (see Table 17 in Prescott et al. (2016)). Iterative proportional fitting, or raking, clearly performed better than alternatives in matching our data to the distributions of key variables in the ACS.

To assemble our analysis data, we began by using raking to calculate weights for our original nonmissing survey data. Next, we imputed our missing data. Our goal was to impute values for many different variables (see Table 18 in Prescott et al. (2016) for details), some of which were missing because of the cleaning process we describe above in Section F.4 and others because we added the relevant question to the survey while the survey was in the field. In addition, as we explain in the article, we also aimed to impute whether the "maybe" individuals are currently or have ever been bound by a noncompete. Because we sought to impute missing values across multiple variables, we employed Stata's chained multiple imputation command, which imputes missing values for all variables in one step. As suggested in Sterne et al. (2009), we incorporated all of the variables that we planned to use in our empirical analyses into our imputation model. Doing otherwise would have produced attenuated estimates.[56]

While imputing missing values just one time will allow for unbiased coefficient estimates, the associated standard error estimates will be too small because the predicted values will not convey the uncertainty implicit in those estimates (King et al., 2001). To generate unbiased standard error estimates, Graham et

[55] See pp.436–46 in Prescott et al. (2016) for more details.

[56] Dependent variables should be included as controls in the imputation of an independent variable to avoid attenuation in the imputed estimates (Sterne et al., 2009). See also `http://thestatsgeek.com/2015/05/07/including-the-outcome-in-imputation-models-of-covariates/`. Indeed, a general rule of thumb is that all variables involved in the analysis should be included in the imputation model.

al. (2007) recommend conducting at least 20 imputations when the proportion missing is 30% (relevant for our "maybe" group). We added another 5 to increase power.

The exact mechanics for a given imputation step are as follows: First, we fit a regression model with our initial nonmissing data. Second, we simulated new coefficients based on the posterior distribution of the estimated coefficients and standard errors—this step is what gives us variation across the 25 datasets. Third, we combined these coefficients with the observed values of the covariates for the missing observations to generate a predicted value. For continuous variables, we used predictive mean matching in the third step. Specifically, we took the average of the 15 nearest neighbors to the predicted value. For binary variables, we employed a logit model to create the predicted value. We repeated this process 25 times for all missing values, creating 25 separate datasets.

Once we had 25 imputed datasets in hand, we reweighted within each dataset using the raking procedure we discuss above, so that each individual dataset is nationally representative. In Table 2 in the article, we present a comparison of the distribution of demographics between the 2014 ACS and our weighted and unweighted data. The table shows that the weighted data quite accurately match the distribution of contemporaneous ACS data and that the unweighted data indicate a much more skilled workforce, one that does not align closely with the U.S. labor force. This occurs because we employed quotas to ensure that more than 50% of our sample was composed of respondents with a bachelor's degree.

Estimation of our main analysis via multiple imputation involves running the regression model in question on each individual dataset and then aggregating the 25 different estimates using Rubin's rules, combining the within-imputation variance and the between-imputation variance into our standard error calculations. Specifically, for $i = 1, \ldots, M$ imputations, for a given estimate in a given imputation $\hat{\beta}_i$ and within-imputation standard error $\hat{se}_i$, the formula for combining the within and between variance is:

$$Var_{Total} = Var_{Within} + Var_{Between} + \frac{Var_{Between}}{M},$$

where

$$Var_{Within} = \frac{\sum_{i=1}^{M} \hat{se}_i}{M}$$

and

$$Var_{Between} = \frac{\sum_{i=1}^{M} (\hat{\beta}_i - \bar{\beta})^2}{M - 1}.$$

We note that standard regression statistics, like R-Squared, are not typically reported for regressions conducted with multiple-imputation data because there are 25 distinct estimates of each statistic. To give a rough approximation of fit, we report the mean of our R-Squared estimates.

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IN THE SUPREME COURT OF THE STATE OF DELAWARE

LKQ CORPORATION, Plaintiff/Counter-Defendant, Appellant, v. ROBERT RUTLEDGE, Defendant/Counter-Claimant, Appellee.	No. 110, 2024 Certification of Question of Law from the United States Court of Appeals for the Seventh Circuit. No. 23-2330 D.C. No. 1:21-cv-03022

[PROPOSED] ORDER GRANTING MOTION FOR LEAVE TO FILE *AMICUS CURIAE* BRIEF SUPPORTING APPELLANTS AND REVERSAL

The Court, having read and considered the Motion for Leave to File Amicus Curiae Brief (the "Application") filed by the Chamber of Commerce of the United States of America and all other pertinent filings submitted, and finding good cause therefore, hereby **ORDERS** that Movants' Motion is **GRANTED**. The *Amicus Curiae* Brief, submitted concurrently with the Motion, is hereby deemed filed.

SO ORDERED this ____ day of _____, 2024.

_____
Justice, Delaware Supreme Court

EFiled: May 10 2024 01:28PM EDT
Filing ID 73022185
Case Number 110,2024

CERTIFICATE OF SERVICE

I, Richard L. Renck, Esquire, do hereby certify that on the 10th day of May 2024, I caused a true and correct copy of the foregoing *Motion for Leave to File Brief of the Chamber of Commerce of the United States of America as Amicus Curiae Supporting Appellant and Reversal* to be served via File&ServeXpress, upon the following counsel of record:

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