Noncompete Clauses, Job Mobility, and Job Quality: Evidence from a Low-Earning Noncompete Ban in Austria[*]

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Abstract

I study the effect of noncompete agreements on low-earning workers using a noncompete ban in Austria. The ban increased treated workers' annual job-to-job transition rate by 0.3 percentage points (a two percent increase). This effect was driven by within-industry job transitions. The reform also disproportionately increased transitions to higher-quality firms and transitions accompanied by earnings gains. However, I do not find that the ban increased treated workers' overall earnings growth rates. This evidence shows that noncompetes in Austria restricted low-earning workers' job mobility but that this impact was not large enough to affect aggregate mobility or earnings trends.

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1 Introduction

Noncompete clauses in employment contracts are used around the globe to restrict workers from moving to competitor firms. Although commonly associated with high-earning workers, noncompetes are also prevalent for lower-earning workers. Examples of low-earning workers with noncompetes include taxi drivers in Austria, dog groomers in Hong Kong, and sandwich makers in the U.S.[1] Moreover, in the U.S. in 2014, Starr et al. (2020) estimated that 14% of hourly employees had a noncompete in their employment contract. This high prevalence has motivated discussions that the rise in noncompete clauses could explain macro trends in labor market dynamism and wage growth (Council of Economic Advisers, 2016; Krueger, 2018). In response, multiple U.S. states and European countries recently banned noncompetes for low-earning workers, and President Biden's Federal Trade Commission (FTC) released a proposed rule to ban noncompetes almost entirely.[2]

The economic question concerning noncompetes is how much their job mobility restrictions benefit firms versus hurt workers. Noncompetes could increase firms' productivity by protecting trade secrets or increasing investment in worker training. For workers, noncompetes could reduce their earnings by restricting their ability to move to higher-paying firms or to bargain for pay raises. While prior empirical research has explored these effects, it has primarily focused on high-earning workers. Yet, the effects of noncompetes may be more muted for low-earning workers. For example, noncompetes only restrict moves to competitor firms. Consequently, if low-earning workers can easily switch jobs across industries, noncompetes may not affect them. This example shows how the effects of noncompetes on workers depend on the wage-setting and job-mobility processes. Thus, studying how noncompetes affect these workers helps us to learn how their labor markets function.

In this paper, I analyze a 2006 ban on noncompetes for low-earning workers in Austria. Using administrative data, I provide quasi-experimental evidence on how the reform affected workers' job mobility, firm quality, and earnings. I find that the ban increased the types of job transitions restricted by noncompetes (e.g., within-industry transitions). These increased transitions were disproportionately to higher-quality firms and higher-paying jobs. I do not, however, detect effects of the ban on workers' overall earnings growth rates. Together, this evidence shows that noncompetes in Austria restricted low-earning workers' job mobility and prevented them from moving to better jobs. However, the magnitude of the job mobility effect was small, so the ban did not substantially

[1] See Meickl (2005) for Austria, Campbell (2014) for Hong Kong, Quinton (2017) for the U.S.

[2] Belgium and Luxembourg restrict noncompetes below certain earnings thresholds, the U.K. banned them in zero-hour contracts in 2015, and several U.S. states recently proposed or implemented bans for low-earning workers (OECD, 2019). See Ferguson et al. (2023) for a summary of the FTC's rulemaking.

affect macro trends in job mobility or earnings.

Before 2006, noncompetes in Austria were enforceable for all adult employees and prevalent for high- and low-earning workers. According to a 2005 survey, over 30% of low-earning workers had a noncompete in their employment contract. In 2006, the parliament changed the legislation so that noncompetes were not enforceable for employees who signed their contract after March 2006 and whose earnings were below around € 2,100 per month (52nd percentile of the earnings distribution).

To exploit this sharp change in noncompete enforceability, I use a difference-in-differences empirical strategy based on when employees signed their contracts and their monthly earnings. Intuitively, the strategy tests whether the differences in outcomes between high- versus lower-earning employees changed for employment spells started after the ban. I implement this strategy using the near-universe of employment records from the Austrian Social Security Database. These data allow me to define outcomes motivated by different theories of the effects of noncompetes. For example, I can define multiple wage and non-wage measures of firm quality and separate earnings changes for job switchers versus job stayers.

I find that the noncompete ban increased job mobility, which was driven by the types of transitions restricted by noncompetes. The ban increased treated workers' annual job-to-job mobility rate by 0.27 percentage points (a 1.7% increase relative to the base rate of 16.0%).[3] This increase was driven by a 6.4% increase in transitions between firms in the same narrowly defined industries. This is consistent with noncompetes only restricting transitions between firms in the same "line of business." Additionally, most of the increase in transitions came from direct employment-to-employment transitions without intervening unemployment spells. Since firms often enforced noncompetes via waiting periods, transitions with periods of unemployment between jobs should not have increased.

To understand what types of job transitions noncompetes restricted, I separately analyze the ban's effect on transitions to higher-quality firms and transitions accompanied by earnings increases. The ban disproportionately increased transitions to higher-quality firms based on wage and non-wage measures (e.g., higher job security and average earnings). Next, I analyze the ban's effect on job transitions where the individual earned more at the new job than the previous job. I find that the ban increased transitions with earnings increases substantially more than transitions with decreases. These two results provide evidence that noncompetes can hurt low-earning workers by preventing them from moving up the job ladder to better firms and higher-paying jobs.

[3]This *reduced-form* estimate does not take into account that not all treated workers had noncompetes before the ban. Scaling this estimate by the pre-reform noncompete prevalence of .33 yields a *treatment-on-treated* estimate of .82 percentage points.

To expand the analysis beyond earnings changes just from moving jobs, I analyze the ban's impact on workers' overall earnings growth rates. Although collective bargaining is common in Austria, firms have the flexibility to pay workers *above* the negotiated wage floors, which implies the reform could have affected worker-level wages.[4] I estimate that the ban's effect on worker-level earnings growth was very close to zero and can rule out increases in the annual earnings growth rate of more than 0.2 percentage points. Similarly, the ban's effect on earnings growth for job stayers (workers who remain with their previous employer) was statistically indistinguishable from zero.[5]

Finally, I benchmark my job transition effect to aggregate mobility trends in Austria. My estimated effect is small relative to annual fluctuations in the job mobility rate for low-earning workers. Thus, the ban did not affect overall mobility trends. The small magnitude of the mobility effect also reconciles how the ban increased transitions with earnings gains but had no effect on overall earnings growth rates. I show that even if all job transitions caused by the ban had earnings increases of more than 25%, this would not have caused a detectable effect on overall earnings growth.

A potential concern with my empirical strategy is that spillovers or earnings manipulation may bias my comparison of workers just above and below the earnings threshold. To address this, I estimate several "donut-hole" specifications that leave out progressively larger groups of workers immediately around the threshold. My estimates are qualitatively the same across these specifications, which alleviates these concerns. I also find no evidence of bunching at the threshold.

The institutional details of noncompete enforcement in Austria provide some clues about why the ban's effects were relatively small despite more than 30% of workers having a noncompete. First, noncompetes in Austria could legally only restrict transitions between close competitors. Second, less than a quarter of lower-earning workers' job transitions were between firms in the same narrow industry (i.e., transitions that noncompetes restricted). Additionally, free employment law advice for all employees through the Chamber of Labor (*Arbeiterkammer*) made it difficult for employers to "abuse" noncompetes by restricting broader transitions. Consequently, even though I estimate that a noncompete decreased a worker's within-industry job transition rate by 20% (a *treatment-on-treated* estimate), this does not translate into large effects on overall job mobility. In other words, the ban had a sizable impact on job transitions restricted by noncompetes for workers with noncompetes.

[4] Consistent with this pay flexibility, Nekoei and Weber (2017) find that UI extensions increased individual-level earnings.

[5] My analysis does not directly address whether the ban increased earnings at the firm rather than the individual level or whether the ban affected workers' starting earnings rather than earnings growth. However, in Section 7, I present evidence showing that workers' starting earnings did not increase after 2006 for workers treated by the noncompete ban relative to ineligible workers. This suggests that the ban also did not impact workers' starting earnings.

Yet, scaling this estimate by the fact that low-earning workers often switch jobs across industries and that only one-third of workers had noncompetes yields a much smaller overall effect.

These empirical results help evaluate predictions of how noncompetes affect workers from different models of wage setting and job mobility. First, my job mobility results reject perfectly competitive labor markets. In this benchmark, restricting job transitions to a few competitor firms should not reduce overall job mobility because workers could costlessly switch to other firms. Second, my finding that the increased job mobility was directed towards higher-quality firms and higher-paying jobs supports job mobility theories with firm differentiation and search frictions (Moscarini and Postel-Vinay, 2018). In these theories, when switching jobs becomes easier, workers move up the job ladder to better jobs. My results support this by providing evidence that workers systematically move to better jobs in response to exogenous reductions in mobility costs. Third, the lack of earnings effects for job stayers is inconsistent with theories where workers use outside job offers to get pay raises from their current employer (Postel-Vinay and Robin, 2002).

This paper also contributes to the literature on the effects of noncompetes on workers. This literature has focused on the U.S. and mostly studied higher-earning workers, despite the policy focus on lower-earners and interest outside the U.S. (Pugh and Davis, 2021; OECD, 2022).[6] Additionally, my analysis complements the literature about the effect of noncompetes on firm outcomes and the effects of *non-poaching* agreements.[7] The closest related paper is Lipsitz and Starr (2020), which analyzes a ban in Oregon on noncompetes for hourly workers. My analysis differs in three dimensions. First, the rich employer-employee data allow me to test specific theoretically motivated channels through which noncompetes could affect workers. Second, my research design allows me to compare treated versus control workers in the *same* labor market (e.g., industry by region), allowing me to control for several shocks that might otherwise bias my results. Finally, although the bans were similar, Lipsitz and Starr (2020)'s mobility and earnings estimates are an order of magnitude larger than my estimates. These differences suggest that labor market institutions may mediate the effects of noncompetes. Thus, the effects of noncompete bans may be different in the U.S. than in other countries with different labor market institutions.

My analysis provides lessons for the noncompete policy debates across different countries. First, in other settings where noncompetes only restrict job transitions between close competitors (e.g., due

[6]See Garmaise (2011); Thomas et al. (2015) for CEOs, Marx (2011); Balasubramanian et al. (2020) for technical workers, and Lavetti et al. (2017) for doctors. Johnson et al. (2023) study all state-level changes in noncompete enforceability since 1991.

[7]For other firm outcomes, see Jeffers (2024), Belenzon and Schankerman (2013), and Hausman and Lavetti (2020). Non-poaching agreements prevent *employers* from hiring workers from similar firms (Krueger and Ashenfelter, 2018).

to their legal interpretation) and where workers often transition across industries, we would expect smaller effects on overall job mobility and earnings. Alternatively, in settings where noncompetes restrict a larger share of job transitions, noncompetes could have large effects on mobility. Second, understanding workers' wage-setting process helps inform if and where we would expect earnings effects. For example, in settings where employers counteroffer outside job offers, we would expect noncompetes to decrease earnings growth at the individual level for job stayers. Alternatively, when most wage changes are due to switching jobs, noncompetes would only reduce earnings growth through fewer transitions to higher-paying jobs.

This paper is structured as follows. Section 2 discusses theoretical predictions of the effects of noncompetes. Section 3 describes the ban and the Austrian labor market. Section 4 discusses the data and outcomes. Section 5 describes the empirical strategy. Sections 6 and 7 present the results on job mobility, firm quality, and earnings. Section 8 discusses the implications of my estimates.

2 Noncompetes Theoretical Framework

In this section, I discuss the predictions from various models about the effect of noncompetes on workers' job mobility and wages. Ultimately, the theoretical predictions are ambiguous, so the effects are an empirical question. These predictions depend on the underlying labor market structure (e.g., how easily workers can switch jobs across industries). Consequently, my results help inform us about the nature of labor markets in Austria.

2.1 Noncompetes in Perfectly Competitive Labor Markets

In a competitive labor market, noncompetes should not affect workers' wages or job mobility. In the competitive benchmark, there are many firms that would all pay a worker the same wage, and workers can costlessly move between these firms. In this benchmark, consider a noncompete that prevents a worker from moving to firms in the same industry. Since the worker could still costlessly move to many other firms, this noncompete would not affect her wage or overall job mobility.

2.2 Productivity Effects and Wage Increases

Noncompetes could increase workers' wages by increasing their productivity. Whether these productivity changes increase wages depends on the degree of "rent-sharing" (i.e., the passthrough of productivity to wages). First, noncompetes could increase firms' capital investment by lowering employee turnover. If it is difficult to train new workers to use capital, increased turnover would lead to lower capital utilization. By reducing turnover, noncompetes could increase capital investment.[8] Second, noncompetes could prevent the spread of company trade secrets or intellectual property. This would increase workers' revenue productivity by reducing product market competition.[9]

Finally, noncompetes could increase firms' investment in worker training. Firms may not invest in workers' *general* training (i.e., skills that are beneficial across firms) because workers can take these skills to other firms.[10] However, firms are incentivized to invest in *firm-specific* training because workers are not compensated for these skills at other firms. Noncompetes can turn industry-specific skills into firm-specific skills by restricting within-industry job mobility. Consequently, noncompetes could incentivize firms to increase industry-specific training because workers no longer benefit from this training at other firms (Meccheri, 2009; Starr, 2019).[11]

2.3 Noncompetes in Imperfectly Competitive Labor Markets

When labor markets are not perfectly competitive, noncompetes can decrease wages and lower overall job mobility. The relevant difference from the competitive benchmarks is that workers care about which firm they are employed at (e.g., some firms pay higher wages). By restricting workers' job mobility, noncompetes can decrease their wages by making it harder for them to move to better firms. Different models with imperfect competition have different predictions for the channels through which wages could decrease. However, the models all predict that noncompetes will have larger effects when they restrict more job transitions. Since noncompetes restrict within-industry job transitions, their effects will be smaller when workers can easily switch jobs across industries.

If noncompetes restrict workers from moving to better jobs, the workers may receive a "compensating differential" for signing noncompetes. For example, consider a worker deciding between job offers with and without a noncompete clause. To take the offer with the noncompete, the worker would need to be paid more to compensate for the job mobility restrictions (Coase, 1960). However, there are many reasons why this Coasian argument may fail. For example, employees might be unaware of the noncompete in their contract or underestimate the costs of signing a noncompete

[8] If noncompetes decrease workers' bargaining power, they could alleviate the capital "hold-up problem," which would increase investment (Grout, 1984). However, a more natural interpretation of noncompetes in bargaining frameworks is that they decrease workers' outside options rather than bargaining power. In this case, they would not alleviate the hold-up problem.

[9] In equilibrium, however, stopping the flow of company trade secrets might decrease overall productivity by preventing knowledge spillovers or deterring startup activity (Stoyanov and Zubanov, 2012).

[10] In competitive labor markets, however, *workers* are incentivized to invest in their own general training, but credit constraints or wage floors may cause suboptimal investment (Becker, 1964; Rubin and Shedd, 1981).

[11] Alternatively, noncompetes could increase *general* training investment by giving firms monopsony power over workers. Firms would then receive some of the surplus from general training and be incentivized to invest in it (Acemoglu and Pischke, 1998).

due to myopia or misunderstanding (Prescott and Starr, 2020).

Wage Effects from Transitions to Better Jobs Noncompetes could reduce workers' wages by preventing them from moving to higher-paying jobs. Most models of imperfectly competitive labor markets imply that some jobs are more desirable than others, but search frictions prevent workers from finding them immediately. Thus, noncompetes restrict workers from being able to move up the "job ladder" to better jobs. It is theoretically ambiguous, however, what types of firms workers would move to if they were not restricted by noncompetes. For example, models with *vertically differentiated* firms predict that all workers would move to the same type of firm (e.g., higher-paying or more productive firms) (Moscarini and Postel-Vinay, 2018). Alternatively, models with *horizontally differentiated* firms assume that the best match for one worker might not be the best for other workers (Jovanovic, 1979). In this case, noncompetes would prevent workers from finding their highest productivity firm match. Finally, firms may differ in *non-wage amenities*. In this case, workers may move to jobs with more job security or safer workplaces.

Individual-Level Wage Bargaining Noncompetes could also reduce wages by preventing a worker from threatening to switch jobs while negotiating with their employer. Interpreted through *wage bargaining* models, noncompetes reduce workers' "outside option" of employment at other firms. Specifically, models with individual-level wage bargaining and on-the-job search imply that one source of wage growth is employees using job offers from other firms to bargain for higher wages at their current firm (Cahuc et al., 2006) (see Shi (2023) for a model that incorporates noncompetes into an on-the-job search model). Noncompetes dampen this channel by shrinking the set of credible outside offers employees receive.[12] Consequently, these models predict that noncompetes decrease wage growth even for workers who remain at the same firm (i.e., "job stayers"). The magnitude of the wage decrease would depend on what share of potential outside job offers noncompetes restrict.

Wage Posting and Monopsony Monopsony and wage-posting models also predict that noncompetes can lower workers' wages (Potter et al., 2022; Gottfries and Jarosch, 2023). In these models, a firm sets a single wage for all workers by trading off the costs and benefits of lower wages. The benefit of a lower wage is increased profits per worker. The cost comes from higher turnover since workers are more likely to be poached by higher-paying firms. When firms can offer noncompetes, the cost

[12] Relatedly, Jarosch et al. (2023) develop a model where market concentration depresses wages by lowering workers' outside options while bargaining.

of lowering wages is muted because workers are restricted from leaving to some higher-paying firms. Consequently, firms will lower wages. The magnitude of the wage effects depends on the share of job transitions the noncompetes restrict and the share of workers with noncompetes. Additionally, in these models, imposing noncompetes on some workers could have spillover effects on other workers. First, since firms set one wage for all workers, imposing a noncompete on one worker could affect the firm's other workers, even if they do not have a noncompete. Second, one firm introducing noncompetes could affect market-level job mobility and wages at other firms.[13]

Theoretical Motivation of my Analysis The differing theoretical predictions from these models motivate the different parts of my empirical analysis. First, estimating the ban's effect on overall job mobility tests the competitive benchmark where we would not expect increased mobility. Second, the analysis of whether the ban increased transitions to different types of "higher-quality" jobs tests which types of "job ladders" workers move up after an exogenous reduction in mobility costs. Finally, the analysis of whether the ban increased individual-level wage growth for job stayers tests bargaining models of wage setting.

3 Noncompetes, Wage Setting, and Job Mobility in Austria

In this section, I first discuss the noncompete ban for low-earning workers in 2006. Then, I describe wage setting and job mobility in Austria and how they inform the expected effects of the ban.

Noncompetes in Austria Noncompete clauses have long been used in Austria. Before 2006, post-contractual noncompete clauses (*Konkurrenzklausel*) were enforceable for all adult employees.[14] Legally, noncompete clauses in employment contracts can prevent employees from moving to firms in the same "line of business" as their current employer. The legislation also stipulates that noncompetes cannot impose an "unreasonable impediment on workers' career advancement." Consequently, courts are less likely to uphold noncompetes that restrict mobility across an entire industry than a narrower set of competitors (Crisolli and Deur, 2017). Additionally, noncompetes are not enforceable if the *employer* terminates the relationship *without* good cause (e.g., layoffs) or the *employee* terminates the relationship *with* good cause (e.g., unsafe work conditions).

[13] Gottfries and Jarosch (2023) show that one firm offering noncompetes reduces competition for other firms' workers, allowing these firms to pay lower workers. Johnson et al. (2023) provide evidence of such spillovers. We estimate several "donut-hole" specifications that leave out progressively larger groups of workers immediately around the threshold, which suggests that our results are not biased by such spillovers.

[14] Post-contractual means restrictions after *leaving* a firm and does not include restrictions on multiple job holding.

Noncompete clauses can be enforced in two ways. First, the clause could include a "contractual penalty" that the employee must pay the original employer to move to a competitor. Second, the clause could include a waiting period of up to one year, during which the employee cannot work at the competitor firm. Finally, survey evidence indicates that workers had some limited ability to negotiate over their noncompete clause before signing their contract.[15] This shows that noncompetes were not completely determined at the firm level or through collective bargaining agreements.

The enforceability of noncompetes in Austria is similar to their enforceability in other countries. For example, enforceability in the U.S. also requires weighing how much a noncompete protects "a legitimate business interest" versus how much it harms employees. However, many U.S. states allow noncompetes to be enforced after all employer-initiated terminations, making noncompetes easier to enforce in these states than in Austria (Bishara, 2011). Alternatively, the enforcement of noncompetes in Austria was more employer-friendly along some dimensions than in other European countries where noncompetes have recently received policy interest (OECD, 2022). For example, noncompetes in Italy and other European countries require payments from the employer to the employee during the mandated waiting period (Boeri et al., 2022).

2006 Noncompete Ban In 2006, Austria banned noncompetes for low-earning workers. Specifically, on March 16th, 2006, the Parliament changed the legislation so that noncompetes were not enforceable for employees whose previous month's earnings were below an earnings threshold and *whose employment contract was signed after March 2006.*[16] Consequently, noncompete clauses are still enforceable for employees with earnings above the threshold or employment contracts from before March 2006.[17] For contracts starting between 2006–2015, the monthly earnings threshold was indexed to 17/30ths of the maximum monthly social security contribution limit. In 2006, this cutoff was at the 52$^{\text{nd}}$ percentile of the earnings distribution at €2,125 per month.

The reform was motivated by an increasing prevalence of noncompete clauses for low-earning workers (Walch and Silhavy, 2005) (see Section F for details of the political process behind the

[15] A 2012 survey reported that four percent of workers could negotiate the terms of their noncompete before signing their contract (the denominator includes workers without noncompetes). Additionally, the share who could negotiate was relatively similar at firms with and without works councils (Vevera, 2013).

[16] Specifically, the cutoff date is March 17th for white-collar workers and March 18th for blue-collar workers. However, some employers may have interpreted the ban to apply to all contracts signed in 2006. I conservatively include contracts signed in the first three months of 2006 in the control group (i.e., recode the start year as 2005). But, for some outcomes, I find evidence of an anticipation effect in 2005, which could reflect this alternative interpretation of the cutoff dates by employers.

[17] The ban considers the date the employee signed their employment contract but contemporaneous earnings. Thus, if they signed a contract with earnings below the limit but their earnings then increased above the limit, a noncompete clause could still be enforced. Anecdotally, some employment contracts in Austria today include such "kick-in" clauses that specify a noncompete clause only if an employee's earnings increase above the limit.

reform). Figure 1 presents results from a 2005-2006 survey of employees about clauses in their employment contracts.[18] It shows the share of employees who had a noncompete in their employment contract by their monthly gross earnings. At least 33% of employees in the two lowest earnings groups (between € 324-1,000 and € 1,001-3,630 per month) reported a noncompete clause in their contract. Consequently, the estimated pre-reform prevalence of noncompetes for low-earning workers in Austria is higher than current estimates in the U.S. of 10-20% (Starr et al., 2020).[19]

Based on the estimates in Figure 1, full compliance with the ban would have reduced the prevalence of noncompetes for low-earning workers by approximately 33 percentage points (the "first stage"). However, there are two reasons why the true first stage might be lower. First, employers may have tried to enforce illegal noncompetes after the ban. Although there is evidence of such "noncompete abuse" in the U.S. (Prescott et al., 2016), it may be less common in Austria because all employees have access to free employment law advice from the Chamber of Labor. Second, some noncompetes before the ban may have been from "boilerplate" employment contract templates and never actually enforced.[20] However, the estimates in Figure 1 are from an *employee* survey. So, even if employers did not enforce the clauses, employees' awareness may have still changed their labor market behavior. Unfortunately, no comparable data are available on the prevalence of noncompetes before and after the ban. Going forward, I will present *reduced-form* estimates of the ban. In Section 8, I discuss how the magnitude of the first stage changes the interpretation.

Wage Setting and Job Mobility In Austria, wage setting is centralized at the sector level yet still allows for substantial wage flexibility. During this time period, over 95% of employees were covered by collective bargaining agreements (CBAs), which are negotiated at roughly the industry by occupation level (Glassner and Hofmann, 2019). These agreements set wage floors for specific job title by experience levels. Yet, firms are free to pay workers above these wage floors ("overpayments") and provide other firm-wide bonuses or fringe benefits. As evidence that overpayments were common, Leoni and Pollan (2011) find that actual wages were 20-30% higher on average than CBA wage floors in manufacturing during this time period. Additionally, Dickens et al. (2007) find that Austria

[18] The survey was conducted by the Chamber of Labor, the Trade Union Federation (*ÖGB*), and FH Wiener Neustadt. I received tabulations from this survey via private communication from *Arbeiterkammer Wien*, which are available upon request. The survey results were also discussed in numerous Arbeiterkammer press releases (e.g., Klein and Leutner (2006)).

[19] However, the Austrian survey has a smaller sample size ($N = 823$) and is less rigorous than the U.S. evidence. A subsequent survey in 2012 with a larger sample size ($N = 2,641$) also found that 34% of individuals reported a noncompete in their employment contract (Vevera, 2013). However, this survey does not separately report the prevalence for individuals who signed employment contracts before and after 2006, so I cannot use it to assess the ban's effects.

[20] For example, sample employment contracts provided by the Austrian Chamber of Commerce (*Wirtschaftskammer*) for sales representatives and shop assistants in 2005 both included noncompetes (WKO, 2005b,a).

has an above-average degree of downward wage flexibility compared to the U.S. and other European countries, and Borovickova and Shimer (2020) find substantial wage variation across Austrian firms, even within the same industry. This evidence shows that, despite wage-setting centralization in Austria, the noncompete reform could have plausibly affected worker-level wages (consistent with other Austrian policy changes that affected individual-level wages (Nekoei and Weber, 2017)).

In Austria, there are few restrictions on employee-initiated job transitions, but job dismissal is more regulated. Job dismissal regulations include consultations with work councils and mandatory pre-layoff notice periods of up to five months (Nagl and Jandl-Gartner, 2016). However, since the noncompete ban affected *employee-initiated* terminations, these frictions on *employer-initiated* terminations should not diminish the reform's potential effect. Most private-sector workers were entitled to between 0-12 months of earnings in severance pay (Koman et al., 2005).[21] The unemployment insurance (UI) replacement rate was 55% of net earnings, and quitters in Austria are eligible for UI after a one-month waiting period, which reduces some financial barriers to voluntary job mobility (Jäger et al., 2020). Bachmann et al. (2020) find that Austria's average job transition rate from 2011-2014 was slightly higher than the EU average but below rates in the U.K. and U.S.

4 Data, Treatment Definition, and Outcomes of Interest

Data Sources My main data source is the near-universe of linked employee-employer data from 1972-2018 from the Austrian Social Security Database (ASSD).[22] The data cover all Austrian employees except tenured public-sector and self-employed employees (Zweimüller et al., 2009). The data include information on individuals' *daily* labor force status and the *annual* earnings received from each employer. It also includes individuals' gender, age, nationality, and white versus blue-collar status and firms' industry and location.

To convert the daily labor market status data to an annual panel, I first restrict the sample to private-sector employees aged 24-54 who are not multiple job holders.[23] I construct employment spells as continuous months where an individual is employed by the same firm.[24] I aggregate the

[21]There was a reform to the severance pay system designed to increase job mobility in 2002. My analysis separates the effects of this reform from the effect of the noncompete reform because the severance pay reform should affect all spells starting in 2003, and it did not differentially affect workers above and below the noncompete earnings threshold.

[22]There is no guidance about whether "employers" are firms or establishments. Fink et al. (2010) compare the ASSD employer size distribution to external *firm* size data and find similar distributions, which suggests that the IDs correspond to firms.

[23]I define multiple job holders as employees employed by multiple employers *on the same day.* They are dropped because defining job transitions for them is conceptually difficult. They make up 3.5% of workers in the ASSD from 1990-2018.

[24]I count parental leave, sickness spells, and nonemployment spells of up to six months, where the employee returns to the same employer as continuous employment. The six-month allowance accounts for seasonal workers.

data to the annual level by assigning each worker the firm where they spend the most time each year. All individuals employed at least one month each year are classified as employed.

Defining Treated Employment Spells The earnings and labor force status concepts in the ASSD align closely with the concepts used to determine noncompete enforceability. As described in Section 3, noncompetes are not enforceable for employees who signed their employment contracts after March 2006 and whose earnings in the previous month were below the earnings threshold. I assume the first date an individual receives earnings from a firm in the ASSD is the employment contract signing date.[25] To approximate individuals' monthly earnings relevant for the noncompete ban, I convert the annualized earnings in the ASSD to a monthly earnings measure.[26] Since I only observe annual earnings, my treatment definition will be measured with some error.

I further restrict the sample based on employees' earnings and job tenure to create comparable treatment and control groups. First, I restrict the sample to employees whose monthly earnings are within 25% of the noncompete earnings threshold each year (i.e., between 75–125% of the threshold).[27] Second, I restrict to employees with less than six years of job tenure. The motivation for this restriction is that my empirical strategy compares different cohorts of employment spells (i.e., groups of workers who started jobs in the same year). Due to the right-censoring of the data in 2018, different cohorts will mechanically have different job tenure distributions. The job tenure restriction balances the tenure distribution across cohorts. Finally, I restrict the sample to employment spells that started between 1995–2013, which ensures that I observe five years of job tenure for all cohorts. Overall, these specific sample selection choices do not meaningfully affect the results. Figure A4 shows robustness to varying the bandwidth around the ban threshold, and Figures A5 and A6 show that the main results are robust to not imposing any tenure restrictions.

Job Mobility and Earnings Outcomes of Interest The main effects of the noncompete ban that I analyze are its impact on job mobility, firm quality, and individual-level earnings changes.

I first define overall job-to-job transitions and then divide these into types of transitions more or less likely to have been affected by the ban. I define a *job-to-job transition* in year t as being employed by a firm in year t and primarily employed by a different firm in year $t+1$. Since the

[25]See Appendix Section H.2 for a discussion about cases where this would over or underestimate the true signing date.

[26]For the main analysis, I use only workers' monthly salaries, adjusting for 13th and 14th monthly payments, to define noncompete eligibility (i.e., I exclude bonuses and variable payments). See Appendix Section H.2 for details about the earnings concepts and Appendix Figures A7 and A8 for robustness to defining treatment based on total earnings at each firm.

[27]Since the earnings threshold is indexed to the time-varying maximum social security contribution limit, I use this limit to define the threshold for years before 2006.

ban should have affected voluntary transitions, I exclude transitions from firm deaths.[28,29] Next, I separate job transitions into *within-narrow-industry*, *within-broad-industry*, and *across-industry* transitions.[30] Finally, I separate within-industry job transitions into direct *employer-to-employer (EE)* transitions and *employer-to-unemployment-to-employer (EUE)* transitions with varying lengths of unemployment between jobs.[31]

To test whether the noncompete ban increased job transitions to higher-quality firms, I define different measures of firm quality that capture wage and non-wage characteristics.[32] For non-wage measures of firm quality, I use firm size, a firm's share of white-collar workers, the average tenure of employees at a firm, a firm's "poaching index" (the share of a firm's hires from other firms rather than nonemployment), a firm's "churn rate" (the number of separations plus hires divided by total employees), and a firm's separation rate to unemployment. Average tenure, the poaching rate, and the churn rate are "revealed preference" measures of firm quality because desirable firms can retain their current employees and can poach employees from other firms (Bagger and Lentz, 2018). The separation to unemployment rate measures firm-specific job security. For wage characteristics, I calculate each firm's average earnings and its AKM fixed effect (Abowd et al., 1999).

My main earnings outcome is the annual change in an individual's "daily wage" from their primary employer. Using the daily labor force status and annual earnings from each employer, I construct the daily wage as annual earnings divided by days worked.[33] Consequently, the earnings measure accounts for differences in annual days worked but not hours per day. For each person-year observation, I use the earnings from the primary (longest-duration) employer.

Summary Statistics Table 1 provides summary statistics for the final sample of employees in 2005 (one year before the reform). The statistics are presented separately for treated employees (earnings below the threshold) and control employees (earnings above the threshold). Treated

[28] See Appendix Section H for a description of the "worker flows" method I use to account for firm ID relabeling. This definition of transitions does not include recalls, which were common in Austria (Nekoei and Weber, 2020).

[29] This definition of job-to-job transitions and my sample selection result in higher transition rates than other statistics in Austria. First, how I define annual employment spells would classify the following as a job-to-job transition: employed at firm A in January of year 1, unemployed for 22 months, employed at firm B in December of year 2. Surveys that measure point-in-time labor force status (e.g., employment in March) would not count this as a job-to-job transition. Second, restricting the sample to lower-earning workers and workers in their first five years of job tenure results in higher job transition rates.

[30] Narrow industries are four-digit NACE 2008 codes. Broad-industry transitions are within the same coarse industry but not the same four-digit industry. Across-industry transitions are across coarse industries. See Appendix Section I for details.

[31] Given how monthly employment is defined, EE transitions can include up to one month of unemployment between jobs.

[32] All firm quality measures are ten-year moving averages lagged by one year (e.g., quality for year t is average quality from $t-10$ to $t-1$). This definition avoids a job separation at time t mechanically affecting firm quality at time t. For a firm's AKM coefficient in year t, I use the AKM coefficient estimated using all transitions from years $t-10$ to $t-1$.

[33] The right censoring of earnings at the social security maximum does not affect my analysis since I exclude high-earners.

workers are less likely to be male or white-collar workers. They are also more likely to be employed at lower-quality firms (e.g., firms with lower average tenure and higher separation risk). Finally, low-earning workers have a larger share of job transitions across- versus within-industry.

5 Difference-in-Differences Empirical Strategy

My difference-in-differences (DiD) strategy compares the outcomes of employees affected by the noncompete ban to those unaffected by the ban due to either the earnings or contract signing date thresholds. Specifically, this strategy compares two groups: (1) employees who signed their contract before and after March 2006 and (2) employees whose monthly earnings were above and below the earnings threshold. Thus, the strategy tests whether the difference in outcomes for high- versus lower-earners changed for employment spells that started after March 2006.

The main estimating equation for worker i at time t is

$$Y_{it} = \sum_{n} \gamma_n \mathbb{1}[S_{it} = n] + \sum_{n} \delta_n \mathbb{1}[S_{it} = n] \times D_{it} + D_{it} + X_{it}'\beta + \varepsilon_{it}, \tag{1}$$

where S_{it} is the employment spell starting year, D_{it} is an indicator for being in the treated earnings range (e.g., monthly earnings below the threshold), and X_{it} includes individual- and firm-level covariates.[34] All specifications include year-fixed effects (FEs) and two-digit industry FEs. They also include earnings percentile FEs, but Appendix Figures A9 and A10 show that the paper's main results are qualitatively the same dropping these controls.[35] I refer to this set of controls as the `baseline controls`. I also present results adding progressively richer controls to address potential identification threats. The outcomes of interest, Y_{it}, include indicators for job transitions, changes in firm quality, and individual-level earnings changes.

The coefficients of interest are the δ_n estimates of the interaction between starting an employment spell in year n and earning below the ban threshold. These coefficients capture the average differences in the outcome variable for individuals in the treated versus control earnings range for each employment spell starting cohort. I normalize δ_{2005} to zero, so all estimates are relative to

[34] In our setting, "event-time" is based on the year each worker started their job. Since all workers are treated at the same time (e.g., before and after 2006), the recent concerns about negative weights in DiD designs do not apply (Sun and Abraham, 2021; Goodman-Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2020).

[35] The earnings percentile FEs account for mechanical changes in the composition of the treated and control groups due to changes in the earnings threshold over time. This threshold is indexed to the social security contribution limit, which increases each year due to inflation. In the 1990s, this limit shifted slightly in real terms, which changed the composition of workers just around the threshold (e.g., the threshold increased from the 48th to the 50th percentile of the earnings distribution). The earnings percentile FEs adjust for these shifts. Since all mobility and earnings outcomes are changes from time t to $t + 1$ and these FEs are at time t, they are not "bad controls."

2005.

To increase precision and report a single treatment estimate, I also estimate a "pooled specification"

$$Y_{it} = \sum_n \gamma_n \mathbb{1}[S_{it} = n] + \sum_m \delta_m \mathbb{1}\left[S_{it} \in [L^m, U^m]\right] \times D_{it} + D_{it} + X'_{it}\beta + \varepsilon_{it}. \quad (2)$$

This specification averages the effects across groups of cohorts. Here, δ_m captures the average outcome difference for workers in the treated versus control earnings ranges across all employment spells starting between L^m and U^m. The $[L^m, U^m]$ groups are [1995–1999], [2000–2004], [2006–2010], and [2011–2013]. Since 2005 is omitted from all groups, the estimates are relative to 2005.

In both specifications, I cluster standard errors at the earnings percentile level. This accounts for treatment being correlated within an earnings percentile and an employment spell starting year (e.g., all workers who start their jobs in 2007 at the 45th earnings percentile are treated). Additionally, there could be correlated shocks to outcomes within earnings percentiles.[36]

Identifying Assumption The identifying assumption for this empirical strategy is a conditional parallel trends assumption. The assumption is that absent the policy change, conditional on the included covariates, the difference in average outcomes between employees in the treated and control earnings ranges would not have varied for different employment spell cohorts. The first testable implication of this assumption is that there should be parallel trends for pre-2006 employment spells. I test for this by reporting estimates of δ_n for spells that started between 1995-2005. A second implication is that for outcomes less likely to be affected by the noncompete ban, we should not detect changes after 2006. I test for this by estimating the ban's effect on across-industry job transitions and job transitions with intervening unemployment.

I address endogeneity concerns related to changes in worker composition in two ways. The specific concern is that changes in the composition of new hires after the ban might bias my estimates. For example, the noncompete ban may have caused firms to hire employees less likely to switch jobs or to increase employees' earnings to just above the threshold to avoid the ban. I address this concern in two ways. First, I add detailed individual- and firm-level controls to the baseline specification (`individual controls`).[37] Including these controls does not meaningfully change my

[36]This level of clustering allows for an unrestricted covariance structure within earnings percentiles for different spell starting years. This addresses the serially correlated outcomes in difference-in-difference designs (Bertrand et al., 2004).

[37]These include experience by white/blue-collar FEs, age by gender FEs, firm location FEs, tenure controls interacted with treatment, and spell starting month by treatment FEs. The tenure controls are indicators for one or two years of job tenure (I

results, which suggests that compositional changes are not biasing my estimates. Second, Appendix Figures A1 and A2, show "manipulation tests" around the ban's threshold. Figure A1 plots the density of employment spells and shows no evidence of bunching at the threshold after the ban.[38] Figure A2 plots workers' *predicted* job transition and earnings growth based on workers' baseline characteristics and also shows no change in predicted outcomes after the ban. The null results from both tests are inconsistent with changes in worker composition biasing my estimates.

Another potential identification threat is that the treated and control groups could be differentially affected by the business cycle. I address this concern in three ways. First, I show that the results are robust to adding time-varying controls that would capture differing sensitivities to the business cycle (the `time-varying controls`).[39] Second, the differing cyclicality could explain a transitory change for a few spell-starting year cohorts but could not explain the persistent change in outcomes starting in 2006 that we find. Finally, if the treatment and control groups were differentially affected by the business cycle, we would not expect parallel trends during the pre-period.

6 Results: The Effect of Noncompetes on Job Mobility

This section presents results showing that the noncompete ban increased job mobility. First, I transparently show this result by plotting raw job transition rates for treated and control employees. Second, I present coefficients from the formal DiD specification. Across both strategies, I find that the noncompete ban increased the annual job-to-job mobility rate by about 0.3 *percentage points* (a two *percent* increase). This was driven by an increase in the types of job transitions we would expect to be restricted by noncompetes (e.g., transitions between firms in the same narrow industries and job transitions without unemployment gaps between jobs). In this section, all outcome variables are indicator variables for job transitions *multiplied by 100.*

Nonparametric Graphical Analysis I first show the paper's main results nonparametrically and transparently by plotting the raw job transition rates for the treatment and control groups. Figures 2 and 3 plot these rates for within four-digit industry and overall job-to-job transitions, respectively. Panel A of each figure plots the average annual job transition rate (y-axis) for employees

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cannot nonparametrically control for tenure because it is collinear with the year and cohort). The spell starting month indicators account for the fact that I shift employment spells that started in the first three months of 2006 into 2005 to align with the timing of the policy change.

[38]A McCrary (2008) test around the threshold yields a t-stat of -1.17 for 2006-2007 employment spells.

[39] These include year by two-digit-industry by white/blue-collar FEs, Bundesland by year FEs, and contemporaneous and forward GDP growth rates interacted with an indicator for being in the treated earnings range. The annual GDP growth rate data are from Statistik Austria.

with different contemporaneous monthly earnings (x-axis). It plots these transition rates separately for employment spells that started in 2004-2005 and 2006-2007. Employees whose earnings are less than the ban's threshold (left of the dashed line) and who started their employment spell in 2006 or 2007 are eligible for the noncompete ban (i.e., "treated"). Thus, our DiD strategy will compare how the difference in job mobility between the '04-05 and the '06-07 cohorts changed across the earnings threshold. Panel B plots this difference for each earnings bin (solid red circles) and the average difference for the treatment and control groups (dashed black lines).

For within-industry and overall job mobility, these figures show increases in mobility for treated workers. Figure 2 shows that within-industry job mobility for the 2004-2005 cohorts was higher than the 2006-2007 cohorts in the control earnings range. However, job mobility was higher for the 2006-2007 cohorts in the treated earnings range. Subtracting these two differences yields a treatment effect of 0.3-0.4 percentage points. Similarly, for overall job-to-job mobility in Figure 3, the treatment effect is around 0.5 percentage points. For both figures, it is reassuring that the treatment effect starts close to the earnings threshold and is relatively constant across the earnings distribution.[40] Finally, given the reform's sharp earnings cutoff, we might expect a discontinuity in job transitions at the threshold. However, measurement error between the reform-relevant earnings concept and the earnings concept in the ASSD make it unlikely that I could detect a discontinuity.[41]

Job Mobility DiD Regression Results I next present the DiD results, which confirm the previous graphical estimates. Figures 4 and 5 plot the estimated δ_n coefficients from equation 1 (i.e., the interactions between spell starting years and earnings in the treated range).

Figure 4 presents estimates for within-four-digit-industry job transitions. The hollow blue coefficients plot the δ_n coefficients with just the baseline controls. The estimates for spells starting between 1995-2004 are relatively stable, which shows a lack of pre-trends before the ban.[42] Starting in 2006, the treatment effects increase and remain elevated between 0.2 and 0.4 (i.e., an increase in the annual job transition rate of 0.2 and 0.4 percentage points). Relative to the baseline within-industry transition rate of 3.6 percent, this represents a 6-11 percent increase.

[40]Additionally, following the argument in Wang and Young (2024), the lack of differential trends in the outcome variable for earnings percentiles above the ban's threshold is supportive of our empirical strategy's identifying assumption.

[41]There are two potential sources of measurement error. First, I observe annual earnings, while the ban is based on earnings the month before separation. Second, as described in Section H.2, the earnings concepts are conceptually slightly different.

[42]There is slight evidence of an anticipation effect in 2005. This is plausible if many employees signed their employment contracts after first receiving earnings in the ASSD (e.g., provisional employment periods) or if some employers interpreted the ban as applying to all spells starting in 2006 (I include spells that started in the first three months of 2006 in the 2005 cohort). As discussed in Appendix Section F, there was limited public discussion of the reform earlier than ten months before its implementation, so anticipation effects beyond ten months are unlikely.

These estimates are robust to including richer worker and firm controls. The solid red triangles in Figure 4 present estimates that add baseline worker-level controls (see footnote 37), and the results are almost identical to the baseline specification. The hollow black circles are the estimates, including the time-varying and macroeconomic condition controls (see footnote 39). With these controls, the estimates after 2005 are unaffected, and some of the pre-period estimates are no longer significant. This estimate stability after adding richer controls suggests that the results are not driven by changing worker composition or differential business-cycle sensitivities.

Figure 5 presents the same estimates for overall job-to-job transitions and reveals a similarly sized treatment effect. Across all three specifications, the noncompete ban increased the annual rate of overall job-to-job mobility by about 0.3 percentage points. Relative to the higher baseline annual job-to-job transition rate of 16 percent, this represents around a 2 percent increase. The treatment effect trend in Figure 5 is somewhat less sharp than in Figure 4. However, this is expected since noncompetes are restricted only within-industry transitions.

To summarize the annual event-study coefficients as a single estimate, Table 2 presents results from the pooled specification in equation 2. The treatment effects are very similar to the average event-study coefficients in Figures 4 and 5. The estimate for within four-digit industry transitions for 2006-2010 employment spells with the most detailed controls, column (3), is 0.23 percentage points (std. err. = 0.07). Additionally, pooled pre-trend estimates are indistinguishable from zero. The same estimate for overall job-to-job transitions, column (6), is 0.27 (std. err. = 0.10).

Heterogeneity by Transition Type To provide further support that the mobility increases are from the noncompete ban, I split job transitions into types more and less likely to have been affected. For the below figures, the estimates are from the pooled specification with baseline controls. Appendix Tables A1 and A2 present estimates from the other control specifications.

First, I present estimates separately for within- versus across-industry job transitions. Since noncompetes could only restrict mobility between firms in the same "line of business," we would expect larger effects on the within-industry transitions. Panel A of Figure 6 includes pre-trend and treatment effect estimates for within four-digit industry (already presented), other within-industry (i.e., job transitions within the same coarse NACE industry but not the same four-digit industry), and across-industry job transitions. For the two new outcome variables, the pre-trend estimates are insignificant. For the estimated treatment effects, the impact on within four-digit industry transitions is more than five times larger than other estimates and the only significant estimate.

Next, I further separate within-industry job transitions into transitions with and without periods of unemployment between jobs. Since noncompetes were sometimes enforced via waiting periods, the ban should not have increased job transitions with unemployment spells between jobs. Panel B of Figure 6 presents estimates of the effect on within four-digit industry job transitions split into direct employer-to-employer (EE) transitions and transitions with 1-3, 4-6, and 7+ months of unemployment between the jobs (EUE transitions). The effect on EE transitions is more than double the other transition types. The effect on EUE transitions with seven-plus months of unemployment is significant but a smaller 0.05 percentage point increase. Both pieces of heterogeneity analysis in Figure 6 provide strong evidence that the noncompete ban drove the increased job mobility.

Industry Heterogeneity Finally, I estimate which industries experienced increased job mobility due to the ban. I then compare this heterogeneity to anecdotal accounts of which industries used noncompetes for low-earnings workers before the ban. Figure 7 presents separate treatment effects on within-industry job transitions for fourteen industries. I find positive and significant effects for wholesale and retail trade, transportation, and administration and support. Additionally, I find large positive but insignificant effects in information and communication and finance and insurance. This industry heterogeneity lines up with anecdotal accounts of where noncompetes were prevalent before the ban from analyzing newspaper articles and the legislative debate around the reform.[43]

Addressing Spillover Concerns with Donut Hole Specifications A potential concern with my empirical strategy is that there may be biases from comparing workers just above and below an earnings threshold. First, since the ban affected a non-trivial share of the labor market, there may have been general equilibrium effects that spilled over to the control group. For example, noncompetes could affect overall labor market dynamism or workers' outside options (Starr et al., 2019; Johnson et al., 2023). Second, dynamic considerations could change the treatment intensity for workers closer versus farther from the threshold. For example, a worker with earnings just below the threshold might not accept a job paying just above the threshold if it came with a noncompete.

To address these spillover concerns, I estimate a series of "donut-hole" specifications that leave out progressively larger groups of workers immediately around the threshold. Assuming that any

[43]These accounts reveal examples of noncompetes for salespeople and furniture retailers (*wholesale and retail trade*) (Dulnerits, 2005; Tageszeitung, 2005), taxi drivers (*transportation*) (Meickl, 2005), mobile phone companies (*information and communication*) (Dulnerits, 2005), and temporary help workers (*administration and support*) (Bartosch, 2006). The only two industries where I found examples of noncompete use but I do not estimate mobility effects are tourism (*accommodation and food services*) (Initiativantrag, 2003) and hairdressers (*other services*) (Tageszeitung, 2005). For these two industries, however, the confidence intervals are large enough that I cannot rule out meaningful increases in job mobility.

general equilibrium spillovers were larger between employees with similar earnings, dropping workers around the threshold should eliminate some of the bias and change the coefficient estimates.[44] Appendix Figure A3 shows how the overall and within-industry job mobility estimates change when I drop up to 10 percentage points around the threshold (i.e., including workers between 75-90% and 110-125% of the threshold). When dropping the donut holes of varying sizes, both job transition estimates remain significant and are statistically indistinguishable. This coefficient stability shows that the general equilibrium and dynamics concerns are unlikely to bias my estimates substantially.

Another potential spillover is that changes in noncompete use norms after the ban decreased their use in the control group. Two institutional details provide evidence against such spillovers. First, survey evidence after the ban shows no decrease in noncompete prevalence for higher-earners.[45] Second, the ban's earnings threshold was increased in 2015, which suggests that firms did not stop offering noncompetes to high-earning workers (Crisolli and Deur, 2017).

Robustness The job mobility estimates are robust to different sample selection and specification choices. First, I vary the bandwidth around the earnings threshold that determines the size of the treated and control earnings ranges. Appendix Figure A4 presents estimates for within-industry and overall job-to-job mobility where the included bandwidth varies from 5% to 50% around the threshold. For very narrow bandwidths, the estimates are noisy due to the smaller sample size, but for bandwidths between 20-50%, the estimates are significant and qualitatively the same.

Next, I relax the restriction limiting the sample to employees with less than six years of job tenure. Appendix Figure A5 plots results analogous to Figures 4 and 5 for the full sample. Appendix Figure A6 is a summary figure that compares the headline estimate from the paper to the same estimates for the full sample (including subsequent firm quality and earnings results). Across both figures, the results are qualitatively the same without the tenure restriction.

Finally, I show robustness to defining treatment based on workers' total *earnings* rather than *salaries* (see Appendix Section H.2 for details). Appendix Figures A7 and A8 summarize the paper's estimates with the *earnings*-based treatment definition. The results are qualitatively the same as the main results. However, the overall mobility estimates are slightly less precise, consistent with more measurement error in the *earnings*-based treatment definition.

[44] Supporting this assumption, Nimczik (2018) shows that, in Austria, employees at firms with similar pay premia are more likely to be in the same endogenous labor market.

[45] In 2005-2006, 44% of employees earning above € 3,630 per month had a noncompete in their contract compared to 45% of employees earning above € 4,230 in 2011-2012 (See Figures 1 for 2005-2006 and Vevera (2013) for 2011-2012).

7 Results: Firm Quality and Earnings Effects

Next, I analyze the ban's impact on transitions to higher- versus lower-quality firms and workers' earnings. I find that the ban disproportionately increased transitions to higher-quality firms (e.g., firms with higher job security). Additionally, the ban had a larger effect on transitions with earnings increases than decreases. Finally, even though the ban disproportionately increased transitions with earnings increases, I cannot detect an effect on overall earnings growth rates for treated workers. I reconcile these findings by showing that the number of additional job transitions is small enough that I do not have enough power to detect their impact on overall earnings growth.

Firm Quality Estimates I first estimate whether the noncompete ban systematically increased job mobility to higher-quality firms. This analysis allows me to test whether relaxing barriers to mobility allows workers to move up the firm job ladder. Consequently, it also tests a novel channel through which noncompetes could harm workers by preventing them from finding better jobs.

I estimate the specification in equation 2 but split up the indicator for a job transition into transitions to higher- or lower-quality firms. The outcome variable for transitions to higher-quality firms is defined as follows for individual i employed at firm $j(i,t)$ at time t

$$Y_{it} = 100 \times \mathbb{1}\left[j(i,t+1) \neq j(i,t) \text{ AND Firm Quality}_{j(i,t+1),t} > \text{Firm Quality}_{j(i,t),t}\right], \qquad (3)$$

and defined analogously for transitions to lower-quality firms.[46] The measures of firm quality are firm size, white-collar share, the average employee tenure, the share of new hires from unemployment versus employment, the churn rate, the annual separation to unemployment rate, average earnings of workers at the firm, and AKM firm-fixed effects (see Section 4 for details). I rescale these measures so better firms have more positive firm quality (e.g., I use $-1 \times$ Churn Rate in Equation 3).

Figure 8 presents the estimated treatment effects for transitions to higher- versus lower-quality firms. It shows that the ban disproportionately increased transitions to higher-quality firms. Panel A plots the coefficients for transitions to higher-quality firms, and Panel B shows transitions to lower-quality firms. For firm quality based on tenure, churn rate, separations to UE, and average firm earnings, the estimates for transitioning to higher-quality firms are all significant and above 0.25.

[46] Although I compare employees' firms at time t and $t+1$, I use the firm quality of both firms at time t. This ensures that leaving a firm will not mechanically affect its quality. As described in Section 4, I also use 10-year *lagged* measures of firm quality to ensure no mechanical relationship between separations and changes in firm quality. Using firm quality in year t also ensures that secular trends in overall firm quality will not affect whether job transitions are to higher-quality firms.

The corresponding estimates for transitions to lower-quality firms are below 0.15 and insignificant. When I formally test the equality of the high- and low-quality coefficients for these outcomes, I find significant differences at the 10% level for average tenure, churn rate, and average earnings.[47] These estimates suggest that the noncompete ban disproportionately increases transitions to higher-quality firms based on the firm's job security and "revealed-preference" quality measures. This finding is consistent with Nekoei and Weber (2017)'s finding in Austria that extended unemployment benefit duration also led to workers finding higher-quality reemployment firms. For firms' white-collar share and hire from UE rate, the reform increased transitions to higher- and lower-quality firms by roughly the same amount. For firm size, however, the increased job transitions are largely transitions to smaller firms. One explanation for this result is that larger firms may have disproportionately used noncompetes, so workers could more easily leave large firms after the ban.

Transitions with Earnings Increases versus Decreases To assess whether the reform-induced transitions were also to higher-paying jobs, I separately estimate the effect on transitions accompanied by earnings increases versus earnings decreases. This analysis shows whether the ban benefited workers by allowing them to move to higher-paying jobs. It also tests whether relaxing mobility restrictions allows workers to move up an earnings-based job ladder.

Following the firm-quality analysis, transitions with earnings increases are defined as

$$Y_{it} = 100 \times \mathbb{1}\left[j(i,t+1) \neq j(i,t) \text{ AND } \ln \text{Earnings}_{i,t+1} > \ln \text{Earnings}_{i,t}\right]. \tag{4}$$

The outcome for earnings decreases is defined analogously. The earnings measure is the average nominal "daily wage" from each employee's primary employer that year. Thus, for job movers, the measure only includes earnings from their post-separation employer in year $t+1$.

I first present the annual event-study figures separately for job transitions with earnings increases and decreases. Specifically, Figures 9 and 10 present the δ_n coefficient estimates from equation 1 with the outcome defined by equation 4. Figure 9 includes all job-to-job transitions, while Figure 10 only includes within four-digit industry transitions. Panels A and B present the estimates for earnings increases and decreases, respectively. For overall transitions, the results show a

[47] Since the coefficients in Panel A and B come from separate regressions, I implement a "stacked data" approach to estimate the coefficients jointly and implement the tests of their equality. See Appendix Section C for details. For the baseline control estimates presented in Figure 8, the p values from a t-test of equality are 0.00, 0.98, 0.23, 0.73, 0.06, 0.13, 0.14, and 0.50 from left to right, respectively. Instead, when I include the most detailed time-varying controls, the p values are 0.003, 0.91, 0.10, 0.98, 0.06, 0.30, 0.06, and 0.54, respectively.

persistent increase in job transitions accompanied by earnings increases starting in 2006 (up to a 0.5 percentage point increase) and a smaller and more delayed increase in job transitions accompanied by earnings decreases (less than a 0.25 percentage point increase). For the transitions with earnings increases, there is some evidence of an anticipation effect in 2005, which masks potentially larger effects after 2006 (see footnote 42 for why such anticipation is plausible). The within-industry results are similar where the coefficients after 2005 are the same size for earnings increases and decreases, but the transitions with earnings increases again show evidence of an anticipation effect in 2005.

To summarize these estimates into a single treatment effect and account for the anticipation effect, I present estimates from a modified version of the "pooled specification." Specifically, I estimate the specification in equation 2 but change the omitted year to 2004, so all estimates are relative to *two* years before the reform.[48] Table 3 presents estimates from this specification for job transitions with earnings increases versus earnings decreases. With the most detailed controls, overall job transitions with positive earnings changes increased by 0.52 percentage points (std. err. = 0.16) compared to an insignificant impact on overall job transitions with earnings decreases (point estimate = -0.06 and std. err. = 0.13). These estimates are significantly different, with p values of 0.019 and 0.009 for the individual and time-varying specifications, respectively (these p values are calculated as in Appendix Section C). Similarly, for within-industry job transitions, the effect on transitions with earnings increases was larger than on transitions with decreases (.25 with std. err. = 0.07 versus 0.12 with std. err. = 0.06). Here, the p values are 0.15 and 0.12, respectively. Overall, these estimates show that the reform disproportionately increased job transitions accompanied by earnings growth, consistent with the previous firm-quality results.

Earnings Transition Robustness One potential concern with this analysis is that it attributes earnings increases that individuals would have experienced without a job transition to the transition. For example, earnings might increase each year due to inflation. To account for this, I redefine the outcome as job transitions with increases in *earnings percentiles*. This definition accounts for the fact that different parts of the earnings distribution may experience different earnings growth each year. Table A3 presents the same results as Table 3 but with this earnings percentile outcome. Across all specifications, the results are almost identical. Finally, Figures A6, A8, and A10 show that the earnings transition results are robust to relaxing the tenure restriction, defining treatment based on total earnings, and dropping the earnings percentile controls.

[48] The pooled groups in this specification are [1995 – 1999], [2000 – 2003], [2005], [2006 – 2010], and [2011 – 2013].

Overall Earnings Growth Rate Estimates Although the reform disproportionately increased transitions accompanied by earnings increases, this might not have led to a noticeable increase in overall earnings growth. Accordingly, I next estimate the impact of the reform on earnings growth for all workers and specifically for job stayers. This analysis helps us understand the channels through which noncompetes could affect workers' earnings growth. To analyze the reform's effect on overall wage growth, I estimate Equation 1 with the following outcome

$$Y_{i,t} = 100 \times \left[\ln \left(w_{i,t+1} \right) - \ln \left(w_{i,t} \right) \right]. \tag{5}$$

Since the outcome variable is earnings log *differences* and the independent variable is the treatment *level*, this specification estimates how the noncompete ban affected workers' earnings *growth rates*. Specifically, an estimate of 1 is approximately a 1 percentage point increase in the annual earnings growth rate. Consequently, this specification does not pick up the ban's effects on workers' starting earnings (see Appendix Section D for details).

Panel A of Figure 11 plots the earnings growth treatment effects for the entire sample. Panel B presents the results for job stayers (i.e., employees with the same primary employer in subsequent years). In both samples, there is no evidence of excess earnings growth for cohorts treated by the noncompete ban. After 2005, the estimated effects on earnings growth are generally insignificant and below 0.2 (i.e., approximately a 0.2 *percentage point* increase in the earnings growth rate). The coefficient estimates before 2005 are also relatively stable and generally insignificant, consistent with parallel trends in earnings growth. Table 4 presents estimates for the same outcomes and samples but from the pooled specification in equation 2. For the specifications corresponding to the previous figures (columns (1)-(4)), the earnings growth point estimates are below 0.07, and the confidence intervals can rule out increases of more than 0.16 percentage points. Overall, these estimates do not provide evidence that noncompetes lead to slower year-to-year earnings growth (see Section 8 for a power calculation that reconciles this result with the earnings job transition results). This finding is consistent with Caldwell and Harmon (2019), who do not find evidence that better employment outside options increases incumbent workers' earnings for workers in the bottom half of the skill distribution (although they do increase wages for higher-skilled workers).

Earnings Growth Robustness These earnings growth estimates are robust to different sample selection and specification choices. First, to address the possibility of spillover wage effects on non-treated workers immediately above the threshold, I estimate the "donut-hole" specifications discussed in Section 6 for one-year earnings changes. Figure A11 shows that the insignificant effect on overall earnings growth and job stayer earnings growth is robust to leaving out progressively larger groups of workers right next to the earnings threshold.

Second, to address the concern that our earnings concept does not incorporate hours, columns (5)-(8) in Table 4 present the same results but restrict the sample to males. Since prime-age men are more likely to be employed full-time, the hours concern is less applicable. Across these estimates, the point estimates are below 0.1, and the treatment and 2000-2004 pre-trend estimates are insignificant.

Third, the *one-year* estimates may miss larger *long-term* effects. Table A4 addresses this by presenting longer time-horizon estimates. Specifically, it presents the same results as columns (1) and (2) in Table 4 but for log-earnings differences up to five years in the future. The estimates are less than 0.15 and insignificant from one to four years. For five years, the effect with individual controls is positive and significant but becomes insignificant when I add the time-varying controls.

Fourth, the estimates may be sensitive to the earnings concept definition. To address this, Appendix Table A5 presents estimates using alternative earnings measures. Columns (3) and (4) use changes only in employees' salaries that do not include bonus payments or other sources of earnings. Columns (5) and (6) winsorize earnings growth at the 1^{st} and 99^{th} percentiles. The estimated impact on log earnings growth remains insignificant, and I can still rule out increases above 0.2.

Finally, Appendix Figures A6, A8, and A10 show that the lack of an overall earnings effect is also robust to relaxing the job tenure restriction, defining treatment based on workers' total earnings, and dropping the earnings percentile fixed effects.

Effects on Earnings Levels The previous results analyzed the ban's effect on individual-level earnings *growth* but not the effect on workers' starting earnings *levels*. The motivation for focusing on wage growth is that many theoretical channels of how noncompetes could *depress* workers' earnings operate through changes in wage growth (e.g., preventing workers from moving to better jobs or from using outside offers to receive pay raises).

Next, I present two pieces of evidence suggesting that the ban did not increase workers' starting wage levels. First, the lack of bunching of starting wages around the ban's threshold and the smoothness of workers' average characteristics across the threshold both suggest small effects on starting wages (Figures A1 and A2). If firms adjusted workers' starting wages due to the ban, we would expect to see a changing distribution of starting wages before and after the ban. Second, Appendix Figure A12 plots the raw wage profiles over time for workers who started an employment spell before and after 2006 and whose initial earnings were above and below the earnings threshold. The figure shows that after 2006, average starting wages did not increase more for workers who could no longer have noncompetes in their contract than for ban ineligible workers. Thus, this analysis also does not provide any support for the ban affecting workers' starting earnings.[49]

8 Discussion: Magnitudes and Policy Implications

I have shown three core findings about the effect of banning noncompetes for low-earning workers. First, the ban increased job mobility. Second, the increased transitions were disproportionately to higher-quality firms and were accompanied by earnings increases. Third, the ban did not noticeably impact overall earnings growth. In this section, I interpret the magnitudes of these effects. Additionally, I discuss some institutional details that help rationalize the core findings and provide lessons for the ongoing debate about noncompetes for low-earning workers.

Benchmarking the Job Mobility Estimates A helpful way to interpret the magnitude of my job mobility estimates is to calculate what they imply for how an individual worker's job mobility would change with and without a noncompete. This interpretation requires calculating the ban's *treatment-on-treated* (ToT) effect by scaling up the previous reduced-form estimates by the pre-reform, noncompete prevalence of 0.33.[50] Figure 12, Panel A plots these ToT estimates of 0.70 percentage points for within-industry job transitions and 0.82 for overall job transitions. Relative to the base rates, these represent 20% and 5% increases in job transitions, respectively. Thus, at the individual level, a noncompete greatly reduced the probability that a worker would switch jobs within the same narrow industry.

An alternative way to interpret the magnitude of my job mobility estimates is to compare the overall impact of the ban to aggregate job mobility trends in Austria. This interpretation is motivated by policy discussions that the rise of noncompetes can explain declining labor market dynamism. To evaluate this claim, Figure 12, Panel B plots the annual job mobility rate for employees in the treated earnings range in red. To construct the black line, I use my estimates to calculate a counterfactual mobility rate for workers in the treated earnings range if noncompetes had

[49]This analysis should be interpreted cautiously because treatment assignment is based on a worker's starting earnings. Thus, treatment effects on starting earnings could affect who is classified in the treated and control groups.

[50]This scaling assumes full compliance with the ban (e.g., no illegal noncompetes after the ban). Additionally, if the effects of the noncompete ban spilled over to other workers in the labor market, this would bias this ToT scaling. Yet, the "donut-hole" specification results do not provide strong evidence in support of these spillovers, which mitigates this concern.

not been banned.[51] Thus, the difference between the two lines is the ban's effect on aggregate job mobility. Overall, the ban had a small effect on aggregate mobility (less than 0.3 percentage points).

Two facts can reconcile the reform's relatively large treatment-on-treated effect on within-industry transitions with its small effect on overall job mobility. First, only around 33% of low-earning employees had a noncompete before the ban. Second, noncompetes could only legally restrict a small share of workers' job transitions (e.g., within four-digit industry transitions only represented 22% of total transitions).[52] To see how these rates determine the overall effect on job mobility, consider the following decomposition (see Appendix Section E for details)

$$\begin{array}{c}\%\,\Delta\,\text{Job Transition}\\ \text{Probability}\end{array} = \underbrace{\begin{array}{c}\text{Share Workers}\\ \text{w/ Noncompete}\end{array}}_{33\%} \times \underbrace{\begin{array}{c}\%\text{ Transitions}\\ \text{Restricted}\end{array}}_{22\%} \times \underbrace{\begin{array}{c}\%\text{ ToT on}\\ \text{Restricted Transitions}\end{array}}_{20\%} = 1.5\%. \tag{6}$$

The left-hand side is the *percent* effect of the ban on treated workers' overall job-to-job transition rate. It is equal to the share of workers with noncompetes times the share of employees' job transitions that were restricted by noncompetes times the *percent* treatment-on-treated effect of the ban on restricted job transitions. This decomposition shows that the relatively large ToT effect on within-industry job transitions (a 20% increase) is scaled by 0.33×0.22, which results in a small overall impact on job mobility of 1.5%. In other words, the ban had a large effect on the types of job transitions restricted by noncompetes for workers with noncompetes. However, since low-earning workers frequently switch jobs across industries and only one-third of workers had noncompetes, it had a much smaller effect on overall job mobility.

My estimates of the effect of noncompetes on job mobility for low-earning workers are also much smaller than comparable estimates in the U.S. For example, Lipsitz and Starr (2020) find that a ban on noncompetes for low-earning workers in Oregon increased treated workers' annual job-to-job mobility rate by 3.6 percentage points (i.e., the intention-to-treat effect).[53] This is an order of magnitude higher than my estimate of around 0.3. Moreover, the estimated prevalence of noncompetes for low-earning workers in the U.S. is actually lower than the survey evidence from Austria (Starr et al. (2020) estimate a 14% prevalence for hourly workers). Consequently, the decomposition in equation 6 implies that noncompetes in the U.S. must have either restricted

[51] If there are spillover mobility effects onto the control group, this counterfactual mobility rate would underestimate the impact of the noncompete reform.

[52] This 22% estimate may overestimate the share of restricted transitions because there may be within-industry transitions that were not restricted by noncompetes. For example, across-region job transitions are generally harder to legally restrict.

[53] To make their estimates comparable to mine, I annualized the monthly estimate in Column (1) of Table 5 of Lipsitz and Starr (2020). Specifically, I report the annualized (base rate + treatment effect) minus the annualized base rate.

a larger share of job transitions (e.g., through a broader legal interpretation) or their effect on restricted transitions was much larger than my estimates for Austria.

Benchmarking the Earnings Estimates I previously showed that the ban disproportionately increased job transitions with earnings increases but had no detectable effect on overall earnings growth. These results can be reconciled by the fact that the total increase in job transitions was relatively small, and the ban did not impact earnings growth for job stayers. To illustrate this, consider the following decomposition of the effect of the ban on overall earnings growth

$$\underbrace{\mathrm{E}[\Delta \ln(w_{it})|\boldsymbol{T}] - \mathrm{E}[\Delta \ln(w_{it})|\boldsymbol{C}]}_{\text{Earnings Growth Treatment Effect}} = \underbrace{\Pr[\text{Complier Mover}_{it}]}_{\substack{\text{Increased}\\ \text{Job Transitions}}} \times \underbrace{\mathrm{E}[\Delta \ln(w_{it})|\boldsymbol{T}, \text{Complier Mover}]}_{\text{Earnings Change for Marginal Movers}}. \tag{7}$$

To derive the expression, I assume the ban only affected earnings growth by increasing the number of job transitions (see Appendix Section G for details). Under this assumption, the difference in earnings growth between the treated, $\boldsymbol{T}$, and control, $\boldsymbol{C}$, groups is equal to the reform-induced increase in the job transition rate times the average earnings growth rate for these marginal movers. Since I have estimated the ban's effect on job transitions, the earnings effect only depends on the average earnings change for the marginal transitions. Appendix Figure A13 plots the implied earnings effect from equation 7 for the job mobility estimate and different values of earnings changes for the marginal movers. The figure shows we need implausibly large earnings change for marginal movers (e.g., almost 50% increases in earnings) to get a detectable effect on overall earnings growth. In other words, although the ban increased transitions to higher-paying jobs, the total increase in job transitions was small, so the ban did not impact overall earnings growth for all workers.

Why Did Noncompetes have Small Effects in Austria? Several institutional details about Austria can help explain why I find much smaller effects of noncompetes than comparable U.S. studies. Specifically, my estimates are much lower than Lipsitz and Starr (2020) and Johnson et al. (2023), who both find substantial mobility and earnings effects from relaxing noncompete enforceability in the U.S. Given the recent policy concerns about noncompetes across a broad range of countries (Pugh and Davis, 2021; Boeri et al., 2022; OECD, 2022), it is important to understand how countries' labor market institutions and noncompete legal frameworks could mediate the effect of noncompetes. Consequently, the institutional details about Austria that I describe below can help inform our understanding of noncompetes in other countries.

First, noncompetes could legally only be enforced for narrowly defined competitors. For example, case law suggests that a noncompete restricting a refrigerator salesperson from moving to a competitor selling dishwashers would not be upheld (Crisolli and Deur, 2017). However, low-earning workers in Austria frequently transitioned jobs across industries. Thus, noncompetes only legally restricted a relatively narrow set of job opportunities. Additionally, the presence of free and accessible legal advice for all employees through the Chamber of Labor made it easier for employees to understand the scope of their noncompetes and harder for firms to enforce illegal noncompetes.[54] For example, as of February 2024, the first link when you Google *"Konkurrenzklausel Österreich"* is an accessible guide from the Chamber of Labor explaining when noncompetes are enforceable (Arbeiterkammer, 2024). Consequently, in other countries or settings where the legal scope of noncompetes is narrow and they are unlikely to be abused, we would also expect relatively small effects. Alternatively, we would expect larger effects when within-industry transitions represent a larger share of workers' overall job transitions.

Second, although survey evidence indicates that over 30% of low-earning workers had a noncompete before the ban, this may overestimate the true prevalence. For example, one explanation for the rise of noncompetes was the use of boilerplate employment contracts that included noncompetes. If some employers had no intention of enforcing these noncompetes or were unaware of them, the true prevalence could have been lower. This lower prevalence would suggest larger individual-level ToT effects. Consequently, estimates of what share of noncompetes in employment contracts ever stand a chance of being enforced would help understand their true prevalence.

9 Conclusion

I analyze the effects of a ban on noncompetes for low-earning workers in Austria on workers' job mobility, job quality, and earnings. I find that the ban increased workers' job mobility by increasing the types of job transitions previously restricted by noncompetes. Additionally, it disproportionately increased transitions to higher-quality firms and job transitions accompanied by earnings increases. These results reject the argument that noncompetes will not harm low-earning workers because their labor markets are perfectly competitive. The results also suggest that one channel through which noncompetes hurt workers is by restricting them from improving job quality through "job hopping." I do not, however, find that the ban noticeably increased workers' overall earnings growth

[54] In interviews, several labor lawyers in Austria suggested that the Chamber of Labor would have made it very difficult for employers to threaten to use unenforceable noncompetes. This suggests that a promising area of future research is understanding how workers' information about labor market regulations may mediate their impact.

rates. Additionally, the job mobility effects that I find are small relative to similar estimates from noncompete bans in the U.S. Several institutional details about the labor market in Austria can help explain why I find much smaller effects. They also help inform our understanding of when to expect noncompetes to have larger effects.

In addition to improving our understanding of noncompetes, this paper highlights how exogenous variation in job mobility costs paired with employer-employee data is useful for testing different theories of job mobility and wage setting. For example, my finding that noncompetes prevented workers from moving to higher-quality firms based on wage- and non-wage measures supports the presence of job ladders in these characteristics for low-earning workers in Austria. Similar work in other labor markets (e.g., other countries or for higher-earning workers) or using other types of variation in job mobility costs (e.g., changes at the firm- or labor-market level) could further our understanding of which theories best describe different labor markets.

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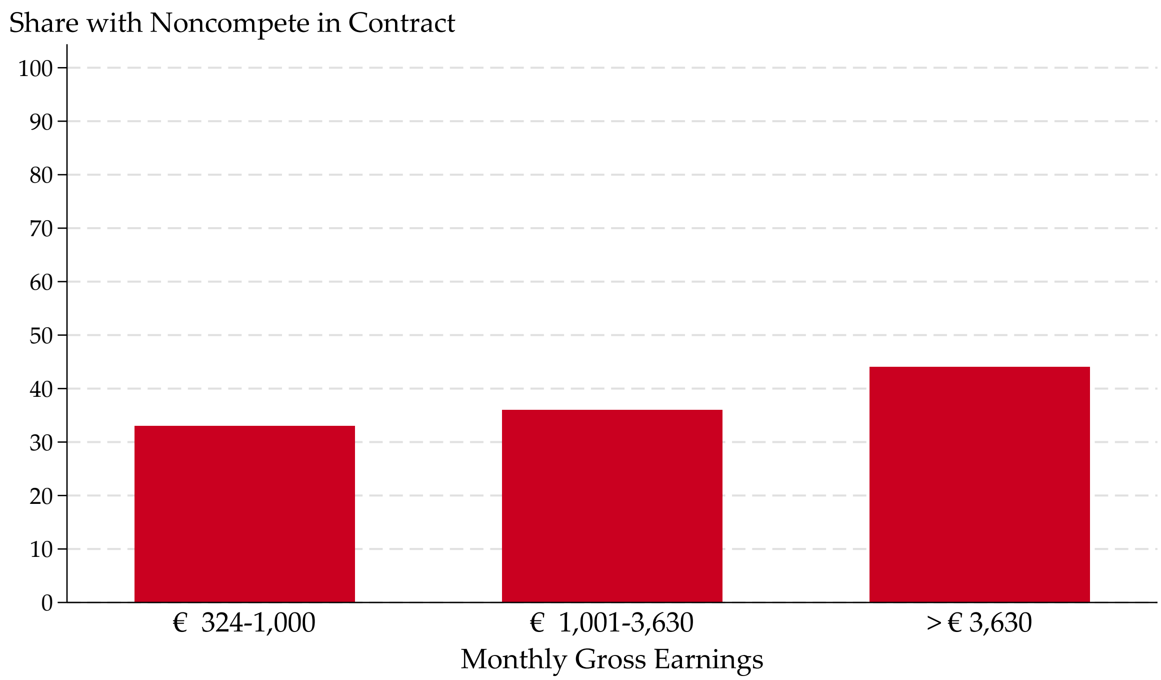
10 Figures

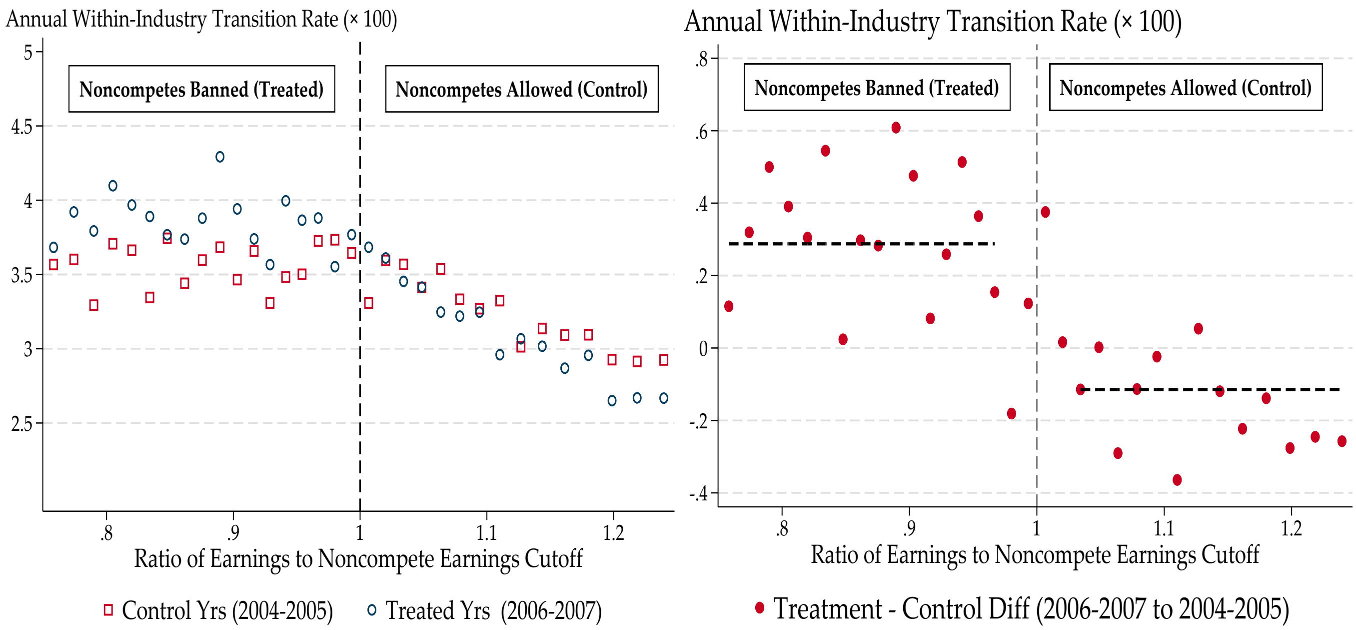
Figure 1: Prevalence of Noncompete Clauses in Employment Contracts, 2005-2006



Note: The figure presents the share of employees who report a noncompete in their employment contract from a 2005-2006 survey conducted jointly by the Arbeiterkammer, ÖGB, and FH Wiener Neustadt on clauses in employment contracts. The sample size of individuals who responded to this question was 807 employees. 5% of individuals in the lowest-earning category, 2% of individuals in the middle category, and no individuals in the highest category responded "do not know." The survey also found that the prevalence of noncompetes was higher for younger workers, more educated workers, and workers who did not have a works council at their company.

Figure 2: Raw Transition Rates for *Within Four-Digit Industry Job Transitions*

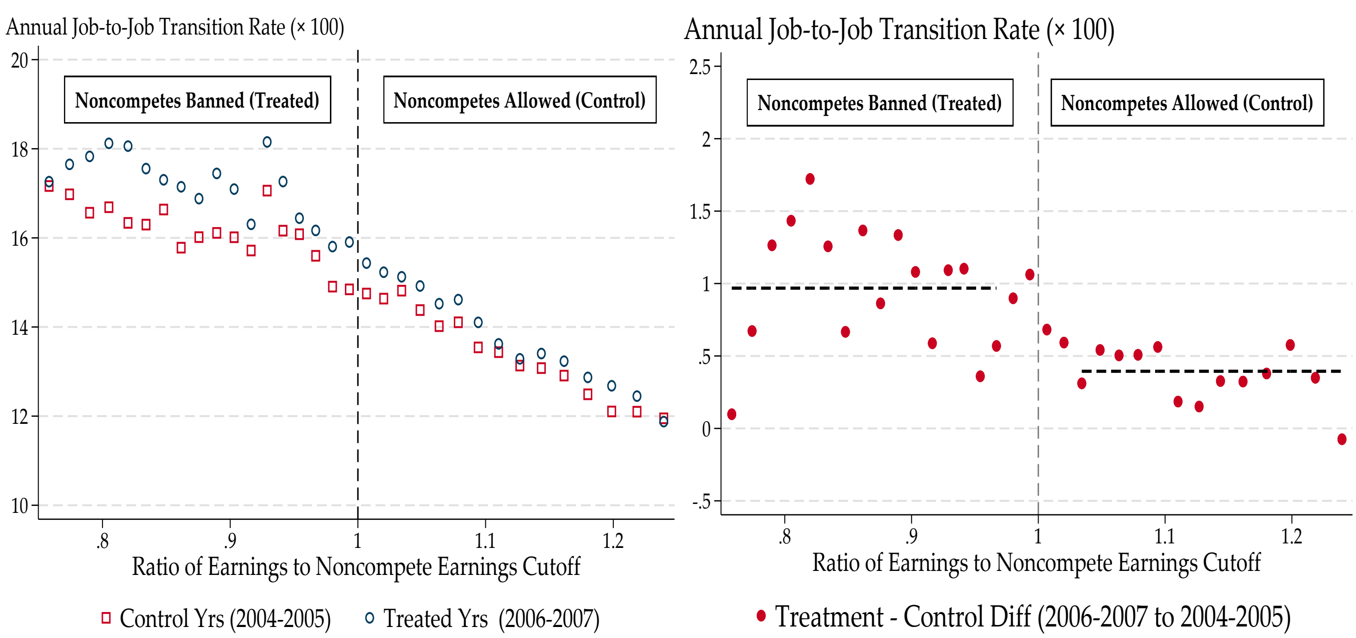
Panel A: Transition Rate Levels | Panel B: Transition Rate Differences



Note: See the Figure 3 note.

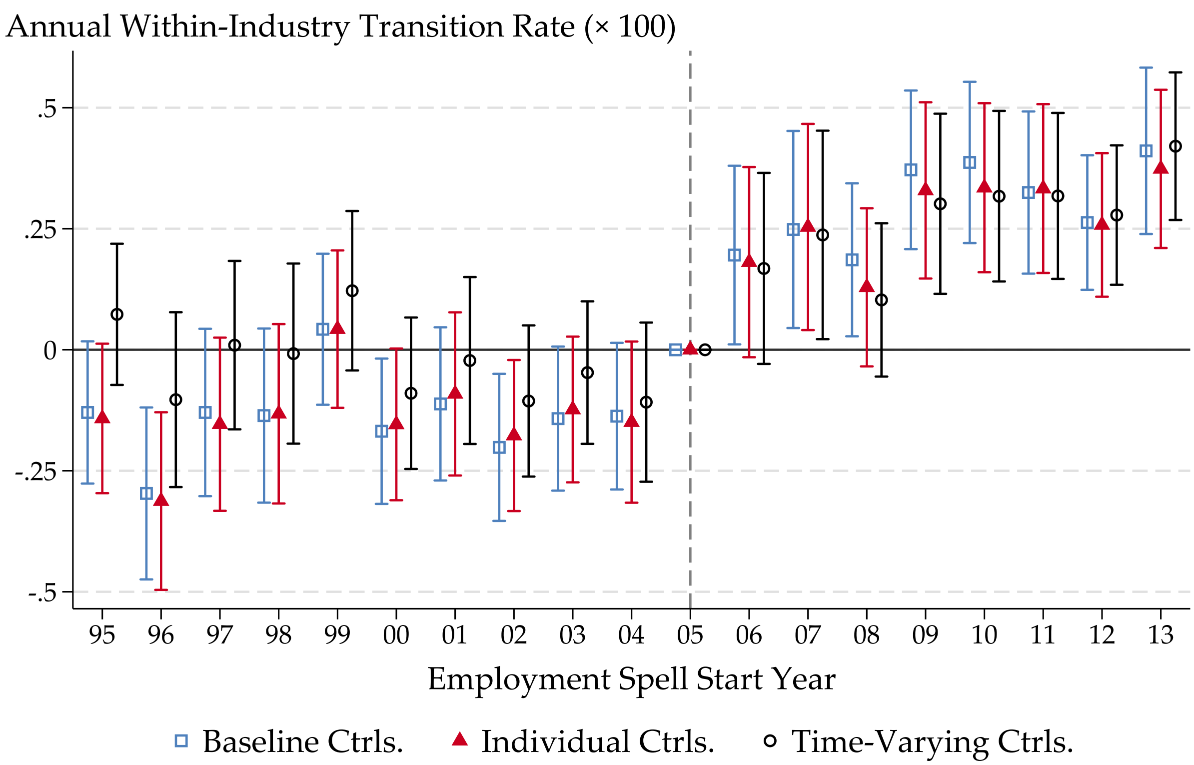
Figure 3: Raw Transition Rates for *Overall Job-to-Job Transitions*

Panel A: Transition Rate Levels | Panel B: Transition Rate Differences



Note: Figures 2 and 3 plot the raw job transition rates for the first two "treated years" (i.e., employment spells that started in 2006 and 2007) and the previous two "control years" by employees' contemporaneous annual earnings. Panel A in each figure plots the average transition rate for employees binned by the ratio of their earnings to the ban's earnings threshold. Panel B in each figure plots the difference for each earnings bin between the treated and control years. The outcome variable for Figure 2 is an indicator variable for an annual job transition between two firms in the same four-digit NACE industry. The outcome variable for Figure 3 is an indicator variable for any annual job-to-job transition. The outcome variables are scaled by 100, so a transition rate of 2 represents a 2 percent annual probability of transitioning jobs.

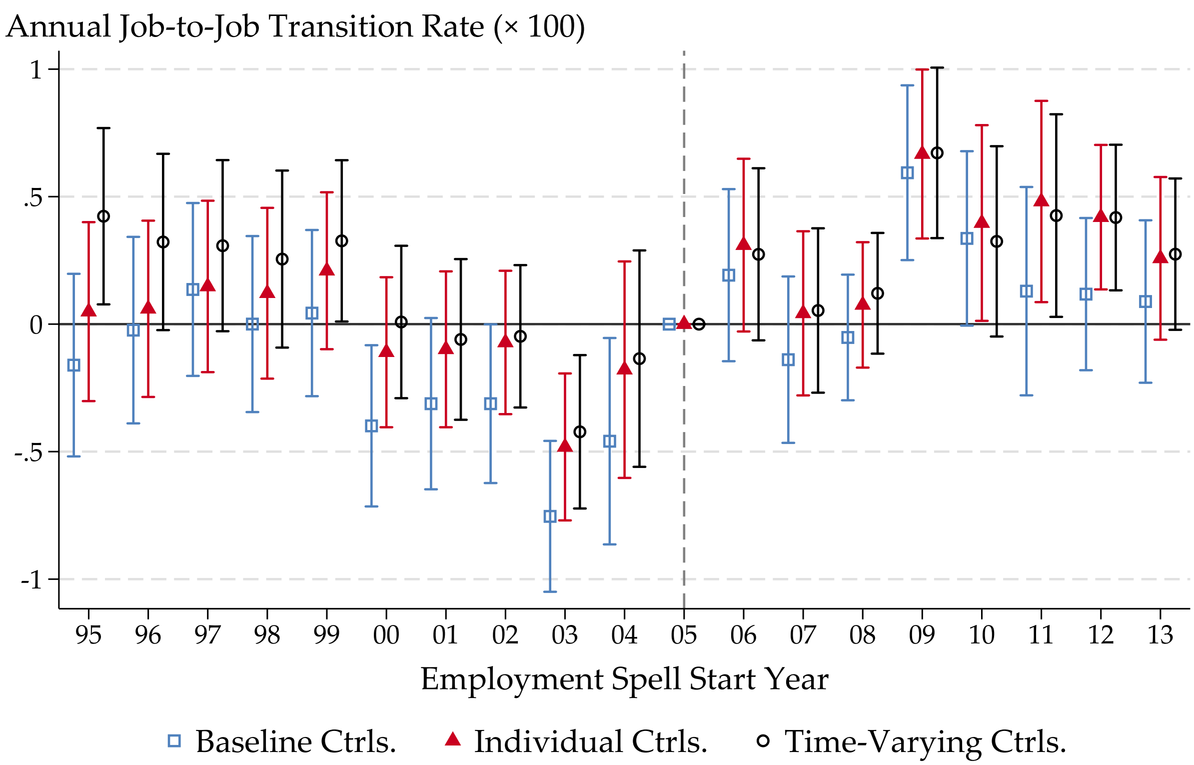
Figure 4: DiD Estimates for *Within Four-Digit Industry Job Transitions*



Base Rate: The annual four-digit industry transition rate for the treated earnings range in 2005 was 3.6%.

Note: Figure 4 plots the estimated coefficients on the interaction between starting an employment spell in year n and having contemporaneous earnings in the treated range (the δ_n coefficients in equation 1). The outcome variable is an indicator variable for an annual job transition between two firms in the same four-digit NACE industry. The outcome variable is scaled by 100, so a coefficient estimate of 0.2 represents a 0.2 percentage point increase in the annual probability of transitioning. Standard errors are clustered at the earnings percentile level. The baseline controls include year fixed effects, two-digit industry by white/blue-collar fixed effects, and earnings percentile fixed effects. The individual controls add experience by white/blue-collar fixed effects, age by gender fixed effects, Bundesland fixed effects, tenure controls that vary by treatment, and spell starting month fixed effects that vary by treatment. The time-varying controls add two-digit industry by white/blue-collar by year fixed effects, Bundesland by year fixed effects, and annual GDP growth rates interacted with treatment.

Figure 5: DiD Estimates for *Overall Job-to-Job Transitions*

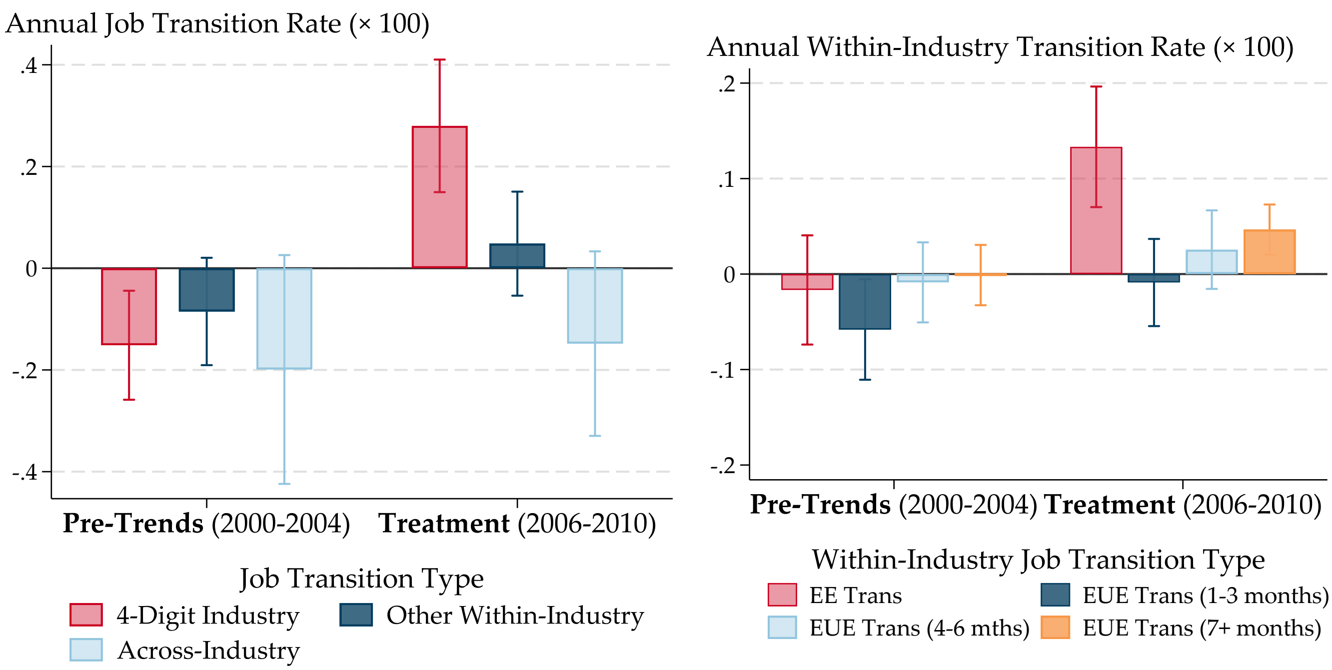


Base Rate: The annual job-to-job transition rate for the treated earnings range in 2005 was 16.0%.

Note: Figure 5 plots the estimated coefficients on the interaction between starting an employment spell in year n and having contemporaneous earnings in the treated range (the δ_n coefficients in equation 1). The outcome variable is an indicator variable for any annual job-to-job transition. The outcome variable is scaled by 100, so a coefficient estimate of 0.2 represents a 0.2 percentage point increase in the annual probability of transitioning. Standard errors are clustered at the earnings percentile level. The baseline controls include year fixed effects, two-digit industry by white/blue-collar fixed effects, and earnings percentile fixed effects. The individual controls add experience by white/blue-collar fixed effects, age by gender fixed effects, Bundesland fixed effects, tenure controls that vary by treatment, and spell starting month fixed effects that vary by treatment. The time-varying controls add two-digit industry by white/blue-collar by year fixed effects, Bundesland by year fixed effects, and annual GDP growth rates interacted with treatment.

Figure 6: Job Transition Heterogeneity: Transition Type and Unemployment Durations

Panel A: Within- versus Across-Industry Transitions Panel B: EE versus EUE Job Transitions



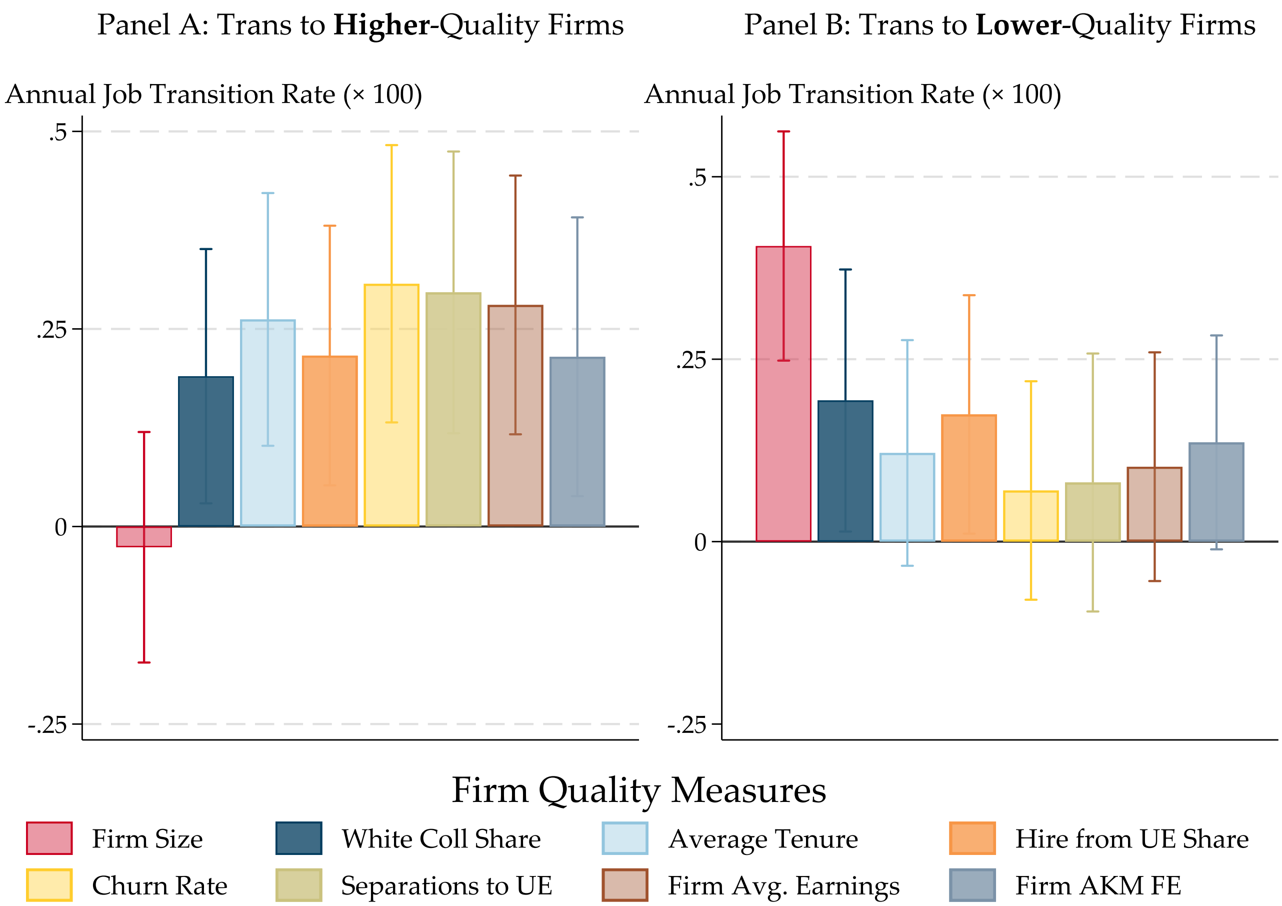
Base Rates: The base rates for the treated earnings range in 2005 were: four-digit industry transitions = 3.6%, other within-industry transitions = 2.9%, across-industry transitions = 9.53%, within-industry EE transitions = 1.3%, within-industry EUE (1-3 Months) transitions = 0.7%, within-industry EUE (4-6 Months) transitions = 0.4%, and within-industry EUE (7+ months) transitions = 0.4%.

Note: This figure plots the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the δ_m coefficients from the pooled specification in equation 2). The different colored estimates are from separate specifications with different outcome variables. The **Pre-Trends** estimates are for spell starting years from 2000-2004, and the **Treatment** estimates for spell starting years from 2006-2010. The four-digit industry transition is an indicator variable for an annual job transition between two firms in the same four-digit NACE industry. The other within-industry transitions are transitions within the same coarse industry but not the same four-digit industry. The across-industry transitions are transitions between two firms in different coarse industries. The outcomes in Panel B split up within four-digit industry transitions based on the duration of unemployment between jobs. Since job transitions with only non-unemployment between jobs are excluded, the base rates will not add up to the total within-industry job transition rate. Standard errors are clustered at the earnings percentile level. These estimates are from the baseline control specification. See Tables A1 and A2 for the estimates from the other specifications.

Figure 7: Within-Industry Job Transition Heterogeneity by Industry

Note: This figure plots the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the 2006-2010 δ_m coefficients from the pooled specification in equation 2) estimated separately for different coarse industry groups. I do not present results for smaller industry groups that yield noisy results. These include agriculture, mining, electricity and gas, water and sewage, activities of households as employers, and extraterritorial organizations. The within-industry transition outcome is an indicator variable for an annual job transition between two firms in the same four-digit NACE industry. Standard errors clustered at the earnings percentile level. These estimates are from the baseline control specification.

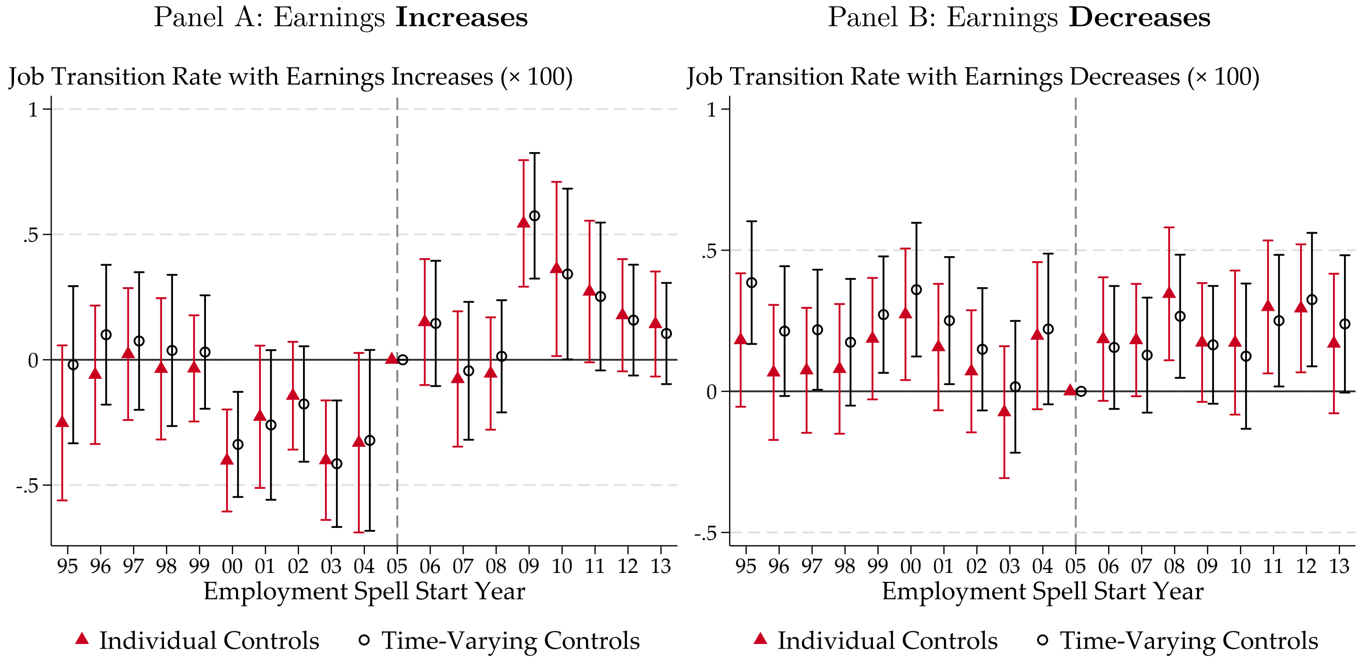
Figure 8: Job Transitions to Higher- versus Lower-Quality Firms



Note: This figure plots the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the 2006-2010 δ_m coefficients from the pooled specification in equation 2) estimated separately for job transitions to higher- versus lower-quality firm outcome variables. Firm quality measures are normalized so that positive changes are "better" (for the firm's hire rate from unemployment, churn rate, and separation to unemployment rate, a transition to a firm with a *lower rate* indicates a transition to a higher-quality firm). See Section 4 for the quality measure definitions and Section 7 for the outcome variable definition. Although overall job-to-job transitions should be the sum of transitions to higher- and lower-quality firms, this equality does not hold in the data because firm quality measures are missing for some firms. These estimates are from the baseline control specification. Standard errors are clustered at the earnings percentile level.

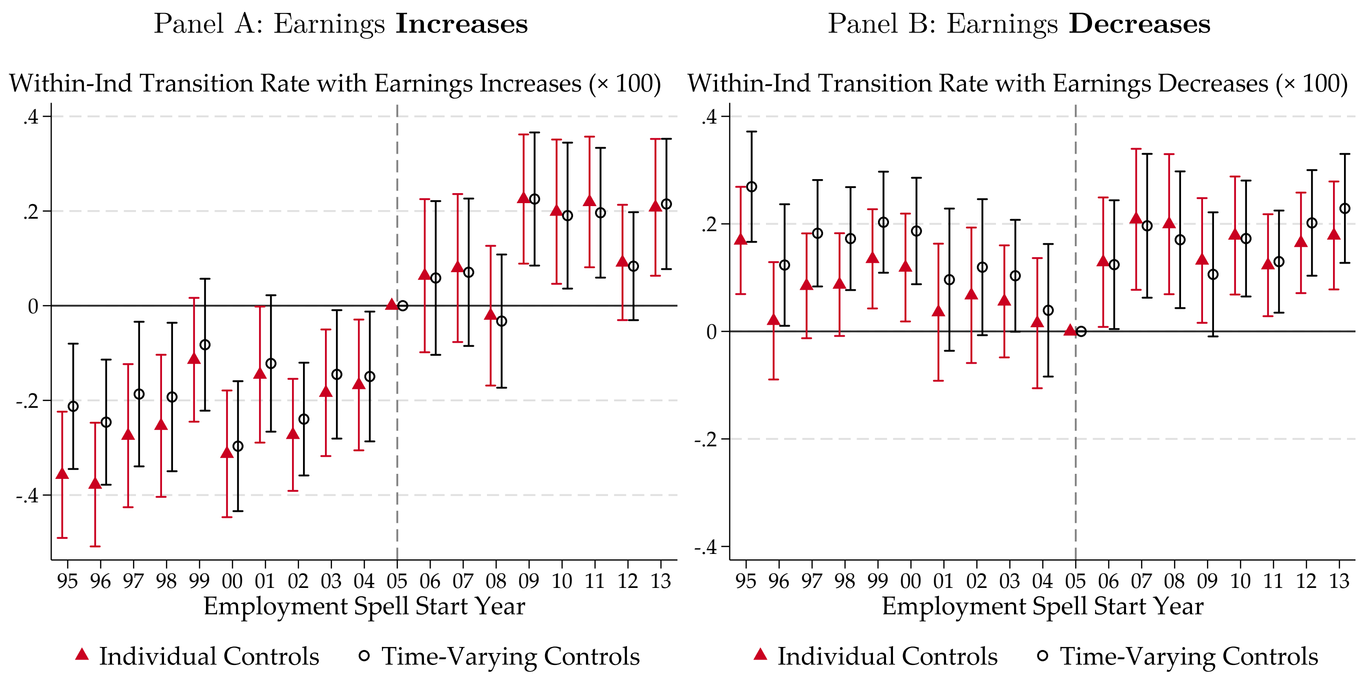
Statistical Tests of Coefficient Equality: Since the coefficients in Panel A and B come from separate regressions, I implement a "stacked data" approach to estimate the coefficients jointly and test the equality of the higher- and lower-quality firm estimates. See Appendix Section C for details. For the baseline control estimates presented in Figure 8, the p values from a t-test of equality are 0.00, 0.98, 0.23, 0.73, 0.06, 0.13, 0.14, and 0.50 from left to right, respectively. Instead including the most detailed time-varying controls, the p values are 0.003, 0.91, 0.10, 0.98, 0.06, 0.30, 0.06, and 0.54, respectively.

Figure 9: Job Transitions by Earnings Changes: *Overall Job-to-Job Transitions*



Note: See the Figure 10 note.

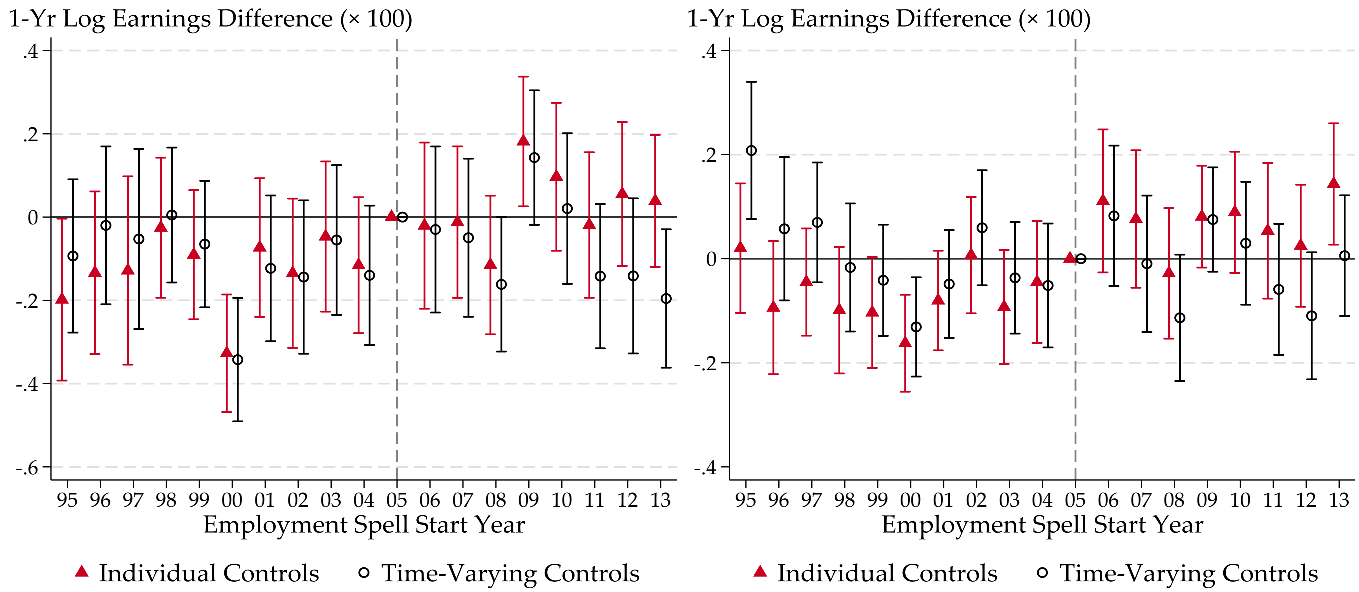
Figure 10: Job Transitions by Earnings Changes: *Within Four-Digit Industry Job Transitions*



Note: Figures 9 and 10 plot the estimated coefficients on the interaction between starting an employment spell in year n and having contemporaneous earnings in the treated range (the δ_n coefficients in equation 1). The outcome variables in the left panels (right panels) are indicators for job transitions with earnings increases (decreases) defined by equation 4. Figure 9 considers overall annual job-to-job transitions, and Figure 10 considers annual job transitions between two firms in the same four-digit NACE industry. The outcome variables are scaled by 100, so a coefficient estimate of 0.2 represents a 0.2 percentage point increase in the annual probability of transitioning with an earnings increase or decrease. Standard errors are clustered at the earnings percentile level. The individual controls include year fixed effects, two-digit industry by white/blue-collar fixed effects, earnings percentile fixed effects, experience by white/blue-collar fixed effects, age by gender fixed effects, Bundesland fixed effects, tenure controls that vary by treatment, and spell starting month fixed effects that vary by treatment. The time-varying controls add two-digit industry by white/blue-collar by year fixed effects, Bundesland by year fixed effects, and annual GDP growth rates interacted with the treatment indicator.

Figure 11: DiD Estimates for *Individual Log Earnings Differences*

Panel A: Overall Earnings Differences Panel B: Earnings Differences for Stayers

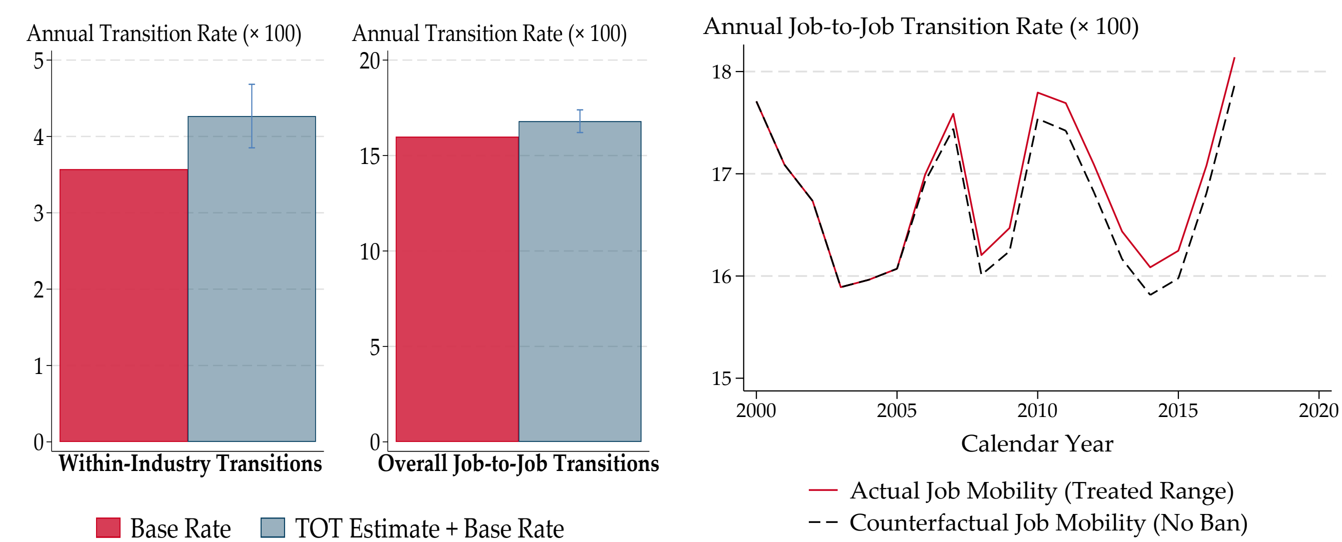


Base Rate: The `ln` earnings diff base rate for the treated earnings range in 2005 was 3.2 overall and 4.3 for job stayers.

Note: Figure 11 plots the estimated coefficients on the interaction between starting an employment spell in year n and having contemporaneous earnings in the treated range (the δ_n coefficients in equation 1). The outcome variable is the annual change in employees' log earnings from their primary employer, defined in Section 4. The left panel includes all employees with a non-missing earnings change, and the right panel restricts the sample to job stayers. The outcome variables are scaled by 100, so a coefficient estimate of 0.5 is approximately a half-percentage point increase in the annual earnings growth rate. Standard errors clustered at the earnings percentile level. See the Figure 10 note for a description of the controls.

Figure 12: Benchmarking the Magnitude of the Job Mobility Effects

Panel A: Individual **Treatment-on-Treated** Ests. Panel B: **Intention-to-Treat** Ests. Relative Trends



Note: The blue bars in Panel A plot the annual job transition base rates for the treated earnings range in 2005. The red bars in Panel A add the treatment-on-treated estimates implied by the reduced-form point estimates (Columns (3) and (6) in Table 2) and the pre-reform prevalence of 0.33 (i.e., the reduced-form estimate divided by the first stage) to the base rates. I assume that the first stage is known with certainty, so the standard errors scale linearly. Panel B plots the annual job-to-job transition rate over time with the main analysis's sample restrictions for the treated earnings range (red). The dashed black line plots the implied effects of my reduced-form estimates of the impact of the noncompete ban on overall job transitions on the annual rate. The effect size increases over time because a larger share of employees is eligible each year following 2005.

11 Tables

Table 1: Treatment and Control Summary Statistics
Year 2005 with ≤ 5 Year Tenure Restriction

	Treated Earnings Range		Control Earnings Range	
	Mean	SD	Mean	SD
Individual Characteristics				
Age	36.6	8	36	7.9
Female	0.43	0.49	0.32	0.47
White Collar	0.38	0.49	0.48	0.5
Tenure	2.26	1.34	2.49	1.4
Experience	11.8	6.8	13	6.8
Firm Characteristics				
Firm Size	345	1,224	432	1,352
White Collar Share	0.46	0.37	0.52	0.37
Average Tenure	4.24	3.43	4.7	3.61
Separation to UE Rate	0.08	0.09	0.06	0.08
Churn Rate	0.62	0.4	0.55	0.37
Hire from UE Share	0.31	0.2	0.28	0.18
Average Log Monthly Earnings	7.45	0.26	7.59	0.25
Outcome Base Rates (Scaled by 100)				
Job-to-Job Transition Rate	15.98	36.65	13.59	34.26
Within Four-Digit Industry	3.57	18.54	3.25	17.73
Other Within Industry	2.88	16.74	2.97	16.97
Across Industry	9.53	29.37	7.37	26.13
Log Earnings Change	3.2	19.98	2.1	18.13
Sample Size	274,615		222,882	

Note: This table presents the average individual and firm characteristics for the main analysis sample in 2005 (one year before the reform was enacted). All of the sample restrictions in section 4 are used, including the restriction that all individuals have less than or equal to five years of job tenure. The summary statistics are for observations with non-missing within four-digit industry job transitions. Experience is defined over the past 25 years. The within-four-digit industry job transitions are annual job transitions between two firms in the same four-digit NACE industry. The other within-industry transitions are transitions within the same coarse industry but not the same four-digit industry. The across-industry transitions are transitions between two firms in different coarse industries. See footnote 29 for explanations of why these transition rates are higher than other estimates of job transition rates in Austria. The firm characteristics are all lagged ten-year moving averages that do not include the current year. The `Separation to UE Rate` is the annual probability of separating from the firm to unemployment. The `Churn Rate` is defined as the annual number of separations plus hires divided by total employees (by definition, it is between 0 and 2). The `Hire from UE Share` is the share of all new hires from unemployment rather than other firms. Consistent with a right-skewed firm size distribution, the median firm size is 37 employees for the treated earnings range and 50 employees for the control earnings range.

Table 2: Pooled DiD Estimates – Within-Industry and Overall Job-to-Job Transitions

Job Transition Type:	Within Four-Digit Industry			Overall Job-to-Job		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × '95-'99	-0.12*	-0.13*	0.02	-0.00	0.12	0.32**
	(0.07)	(0.07)	(0.07)	(0.13)	(0.12)	(0.12)
Treatment × '00-'04	-0.15***	-0.14**	-0.08	-0.45***	-0.19	-0.13
	(0.05)	(0.06)	(0.06)	(0.13)	(0.12)	(0.12)
Treatment × '06-'10	0.28***	0.25***	0.23***	0.17*	0.29***	0.27***
	(0.06)	(0.07)	(0.07)	(0.10)	(0.10)	(0.10)
Treatment × '11-'13	0.33***	0.32***	0.34***	0.11	0.39***	0.37***
	(0.07)	(0.07)	(0.06)	(0.14)	(0.14)	(0.14)
Baseline	X	X	X	X	X	X
Individual		X	X		X	X
Time-Varying			X			X
Observations	8,625,616	8,353,564	8,353,525	8,633,655	8,361,446	8,361,407
R^2	0.028	0.031	0.033	0.040	0.051	0.053

Base Rates: The base rate for the treated earnings range in 2005 was 16.0% for overall and 3.6% for within-industry transitions.

Note: This table presents the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the δ_m coefficients from the pooled specification in equation 2). The outcome variable for the left three columns is an indicator for an annual job transition between two firms in the same four-digit NACE industry, and for the right three columns, it is an indicator for any job-to-job transition. The outcome variables are scaled by 100, so a coefficient estimate of 0.2 represents a 0.2 percentage point increase in the annual probability of transitioning. Standard errors clustered at the earnings percentile level. The baseline controls include year fixed effects, two-digit industry by white/blue-collar fixed effects, and earnings percentile fixed effects. The individual controls add experience by white/blue-collar fixed effects, age by gender fixed effects, Bundesland fixed effects, tenure controls that vary by treatment, and spell starting month fixed effects that vary by treatment. The time-varying controls add two-digit industry by white/blue-collar by year fixed effects, Bundesland by year fixed effects, and annual GDP growth rates interacted with treatment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Pooled DiD Estimates – Job Transitions with Earnings Increases versus Decreases

Job Transition Type:	Overall Job-to-Job				Within Four-Digit Industry			
Transition Condition:	*Earnings Increase*		*Earnings Decrease*		*Earnings Increase*		*Earnings Decrease*	
Treatment × '95-'99	0.26*	0.36**	-0.08	0.03	-0.10	-0.03	0.08*	0.15***
	(0.15)	(0.15)	(0.12)	(0.11)	(0.07)	(0.07)	(0.05)	(0.05)
Treatment × '00-'03	0.04	0.02	-0.09	-0.03	-0.06	-0.05	0.05	0.09*
	(0.15)	(0.15)	(0.10)	(0.10)	(0.07)	(0.07)	(0.05)	(0.05)
Treatment × '06-'10	0.51***	0.52***	0.02	-0.06	0.28***	0.25***	0.16***	0.12**
	(0.16)	(0.16)	(0.13)	(0.13)	(0.07)	(0.07)	(0.05)	(0.06)
Treatment × '11-'13	0.53***	0.49***	0.06	0.04	0.34***	0.31***	0.14**	0.14**
	(0.16)	(0.16)	(0.15)	(0.15)	(0.07)	(0.07)	(0.06)	(0.06)
Baseline	X	X	X	X	X	X	X	X
Individual	X	X	X	X	X	X	X	X
Time-Varying		X		X		X		X
Observations	7,732,960	7,732,920	7,732,960	7,732,920	7,725,089	7,725,049	7,725,089	7,725,049
R^2	0.042	0.044	0.025	0.026	0.019	0.021	0.020	0.021

Note: The specification, controls, and clustering are described in the Table 2 note. The spell starting year groups are different than the other pooled specifications and are [1995 – 1999], [2000 – 2003], [2005], [2006 – 2010], and [2011 – 2013]. I do not report the coefficient for 2005, but the annual estimates are presented in Figures 9 and 10. The outcome variables for the *Earnings Increase* (*Decrease*) columns are indicators for job transitions with earnings increases (decreases) defined by equation 4. The first four columns include overall annual job-to-job transitions, and the last four columns include within-industry transitions. The outcome variables are scaled by 100, so a coefficient estimate of 0.2 represents a 0.2 percentage point increase in the annual probability of transitioning with an earnings increase. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Pooled DiD Estimates – One-Year Log Earnings Differences

Sample Restriction:	Full Sample		Job Stayers		All Males		Male Stayers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × '95-'99	-0.11	-0.05	-0.06	0.04	0.02	0.01	0.09*	0.14**
	(0.07)	(0.06)	(0.04)	(0.04)	(0.09)	(0.09)	(0.05)	(0.06)
Treatment × '00-'04	-0.14**	-0.16**	-0.08**	-0.05	-0.04	-0.07	-0.05	-0.05
	(0.06)	(0.06)	(0.04)	(0.04)	(0.09)	(0.09)	(0.05)	(0.05)
Treatment × '06-'10	0.02	-0.01	0.06	0.01	0.06	0.05	0.09	0.04
	(0.06)	(0.06)	(0.05)	(0.05)	(0.08)	(0.08)	(0.05)	(0.05)
Treatment × '11-'13	0.02	-0.15**	0.07	-0.05	-0.02	-0.19**	0.11**	-0.05
	(0.07)	(0.08)	(0.05)	(0.05)	(0.08)	(0.09)	(0.06)	(0.05)
Baseline	X	X	X	X	X	X	X	X
Individual	X	X	X	X	X	X	X	X
Time-Varying		X		X		X		X
Observations	7,732,960	7,732,920	5,676,424	5,676,363	5,006,864	5,006,785	3,539,315	3,539,230
R^2	0.019	0.021	0.021	0.025	0.019	0.022	0.026	0.031

Base Rates: The one-year log earnings difference base rate for the treated earnings range in 2005 was 3.2 for the full sample, 4.3 for job stayers, 4.2 for all males, and 4.5 for male job stayers.

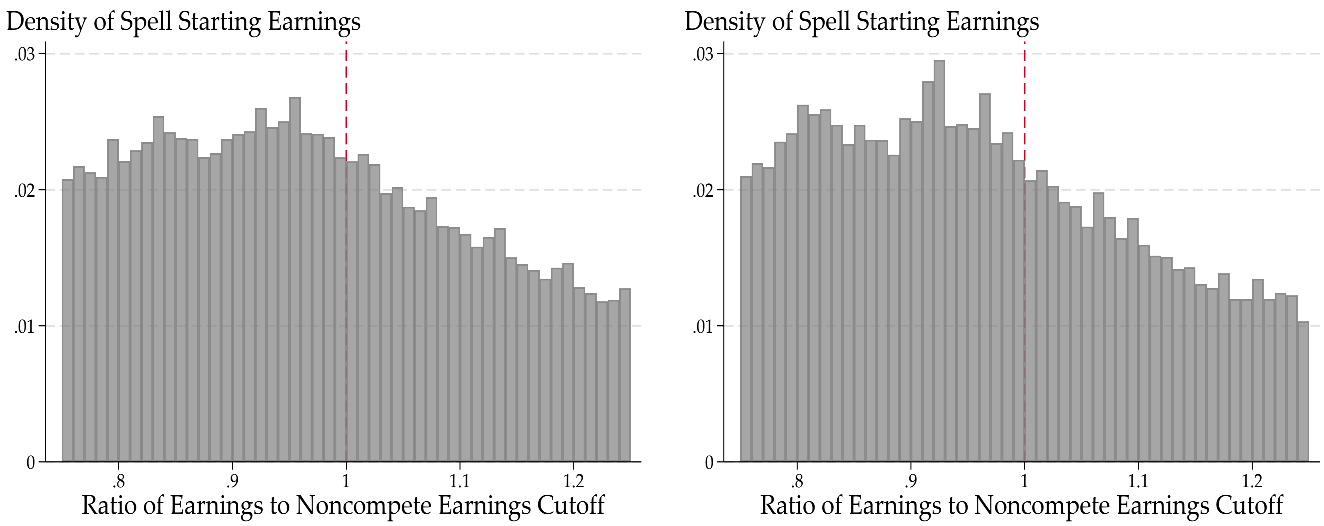
Note: This table presents the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the 2006-2010 δ_m coefficients from the pooled specification in equation 2). The outcome variable is individual-level, one-year log earnings changes. The first two columns present results for the full sample. The third and fourth columns restrict the sample to job stayers. The last four columns restrict the sample to only males. The outcome variables are scaled by 100, so a coefficient estimate of 0.5 is approximately a half percentage point increase in the annual earnings growth rate. The baseline controls include year fixed effects, two-digit industry by white/blue-collar fixed effects, and earnings percentile fixed effects. The individual controls add experience by white/blue-collar fixed effects, age by gender fixed effects, Bundesland fixed effects, tenure controls that vary by treatment, and spell starting month fixed effects that vary by treatment. The time-varying controls add two-digit industry by white/blue-collar by year fixed effects, Bundesland by year fixed effects, and annual GDP growth rates interacted with treatment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix for Noncompete Clauses, Job Mobility, and Job Quality: Evidence from a Low-Earning Noncompete Ban in Austria

A Appendix Figures

Figure A1: Manipulation Test – Density of Employment Spells by Starting Monthly Earnings

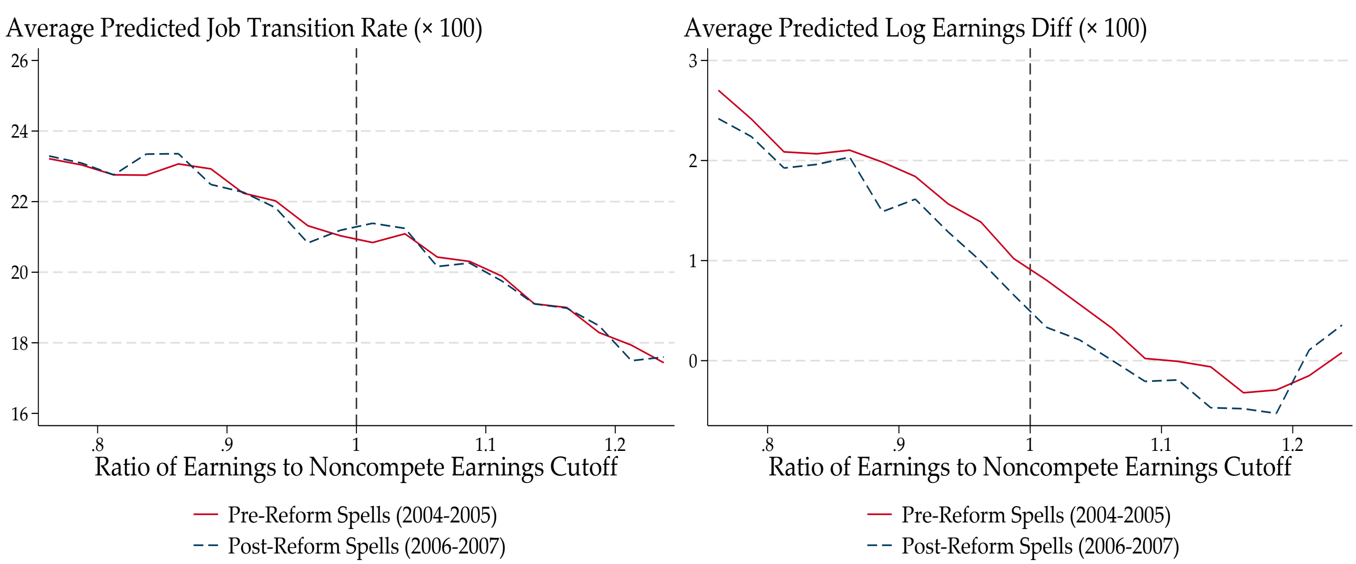
Panel A: Spells Starting in 2004-2005 – **Control** Panel B: Spells Starting in 2006-2007 – **Treated**



Note: This figure plots the density of employment spells by employees' earnings at the start of the spell. To capture starting earnings, the sample is restricted to individuals with one year of job tenure (i.e., the first observation of each individual by firm combination). The left panel plots the density for spells starting in 2004-2005 (pre-reform spells), while the right panel plots the density for spells starting in 2006-2007 (post-reform spells). A McCrary (2008) test around the threshold yields a t stat of -1.17 for treated spells starting in 2006-2007 (i.e., the t-test from testing whether the density of spells is discontinuous from just above versus just below the earnings cutoff). If firms manipulated workers' earnings to avoid the ban, we would expect bunching just above the threshold (e.g., a positive and significant t stat).

Figure A2: Manipulation Test – Predicted Outcomes by Starting Monthly Earnings

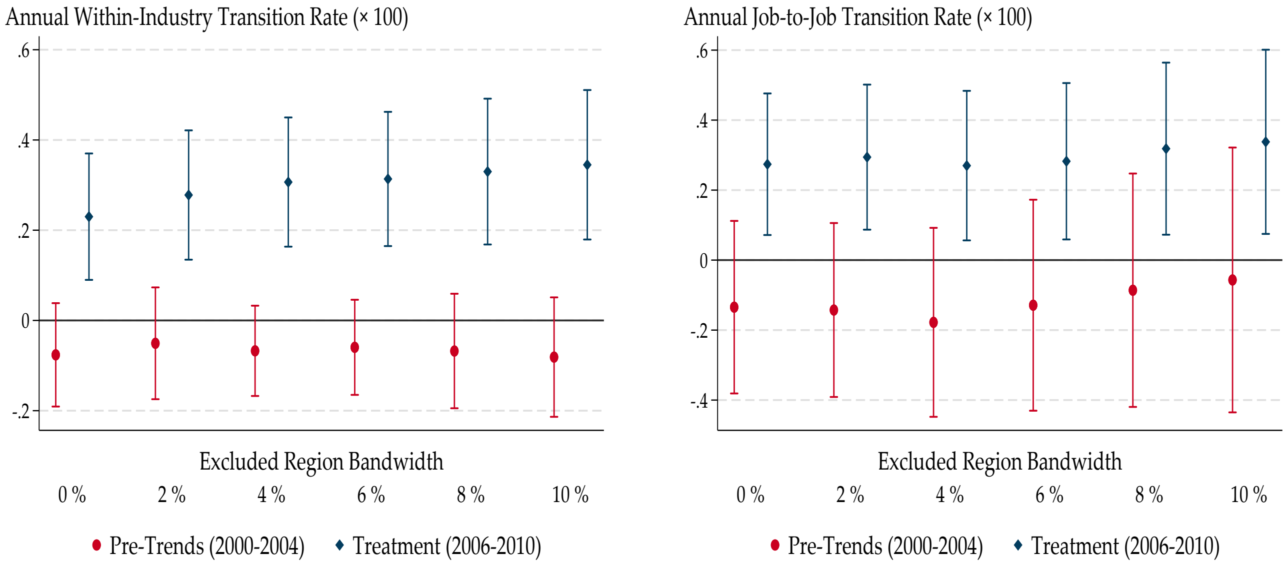
Panel A: Predicted Job-to-Job Transition Rate Panel B: Predicted One-Year Log Earnings Difference



Note: This figure plots the average predicted job transition rate and log earnings difference for employment spells unaffected by the ban (2004-2005) and employment spells that started right after the ban (2006-2007). The sample is restricted to employees with one year of job tenure. The variables used in the prediction model include experience by female FEs, age by female FEs, earnings percentile by white/blue-collar by female FEs, and region by white/blue-collar by female FEs. The prediction model was estimated on pre-reform employment spells that started from 2002-2005. The predicted means are different than those presented in Table 1 because the sample for this analysis is restricted to individuals with one year of job tenure.

Figure A3: Job Mobility DiD Estimates – *“Donut-Hole” Robustness*

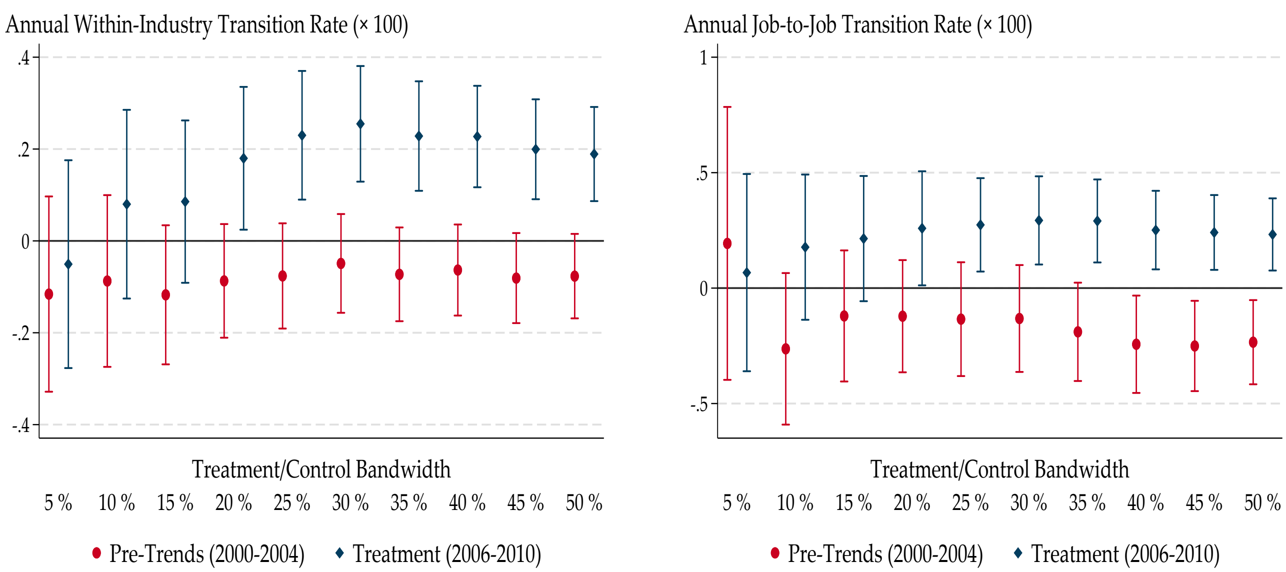
Panel A: Within Four-Digit Industry Job Transitions   Panel B: Overall Job-to-Job Transitions



Note: Figures A3 and A4 plot the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the δ_m coefficients from the pooled specification in equation 2). The Pre-Trends estimates are the coefficient on spell starting years from 2000-2004, and the Treatment estimates are the coefficient on spell starting years from 2006-2010. The outcome variable for Panel A is an indicator variable for an annual job transition between two firms in the same four-digit NACE industry. The outcome variable for Panel B is an indicator variable for an annual job-to-job transition. These estimates are from the time-varying control specification. Standard errors are clustered at the earnings percentile level. Figure A3 uses an overall bandwidth of 25% but varies the bandwidth around the earnings threshold that is *excluded* from the sample. For example, the 10% donut-hole specification includes employees earning between 75 and 90% of the threshold in the treated group and employees earning between 110% and 125% of the threshold in the control group. Figure A4 varies the bandwidth around the noncompete earnings threshold from 5% to 50%. A bandwidth of 25% indicates that all observations with earnings between 75% and 125% of the threshold are included in the sample.

Figure A4: Job Mobility DiD Estimates – *Bandwidth Robustness*

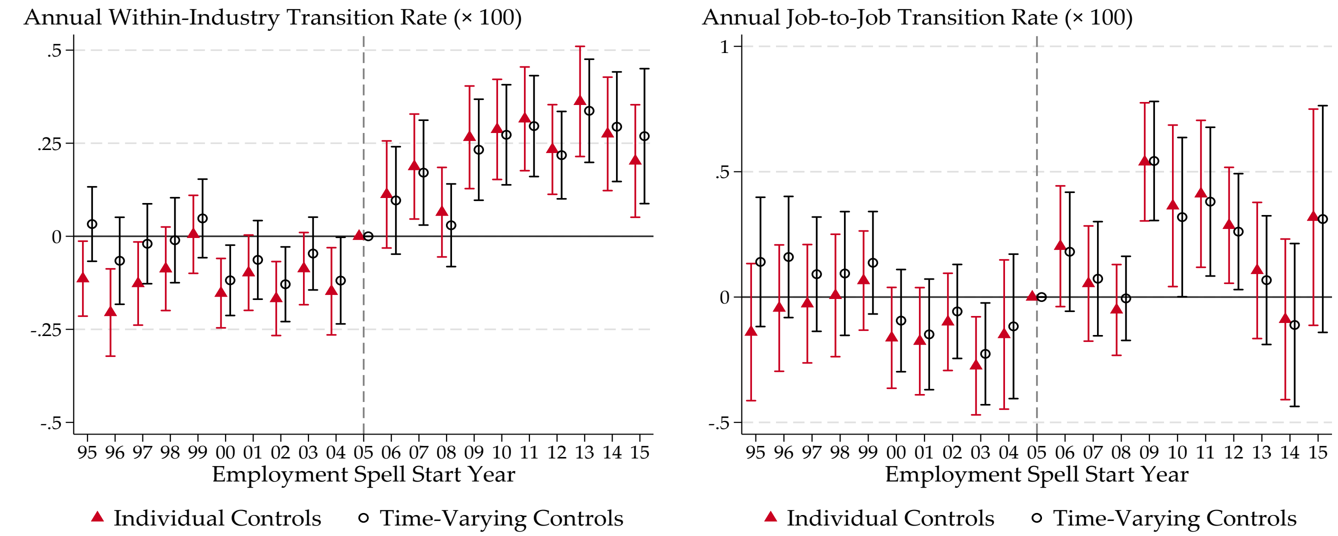
Panel A: Within Four-Digit Industry Job Transitions   Panel B: Overall Job-to-Job Transitions



Note: See Figure A3 note.

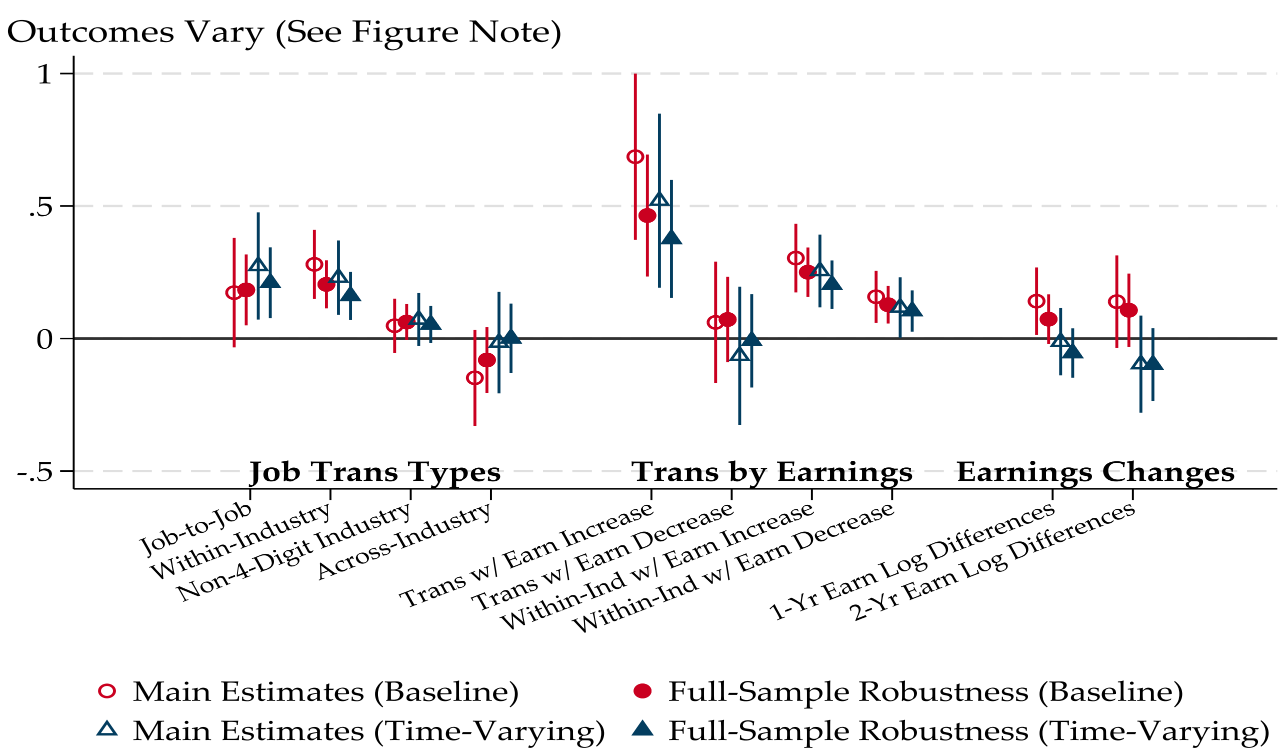
Figure A5: Job Mobility DiD Estimates – *"Full Sample" Robustness*

Panel A: Within Four-Digit Industry Job Transitions Panel B: Overall Job-to-Job Transitions



Note: This figure presents the same estimates as Figures 4 and 5, with two differences. First, the sample is no longer restricted to individuals with less than or equal to five years of job tenure. Consequently, I now also include employment spells starting in 2014 and 2015. Second, to account for the fact that job tenure is now mechanically different across spell starting year cohorts, all specifications include parametric controls for job tenure (linear, cubic, quadratic, and indicators for one and two years of tenure) interacted with being in the treated earnings range. To identify the collinear tenure, year, and spell starting year effects, I restrict the job tenure profile to be flat after 15 years. This is also a common solution to canonical age, time, and cohort effects collinearity problem (see e.g., Card et al. (2013)).

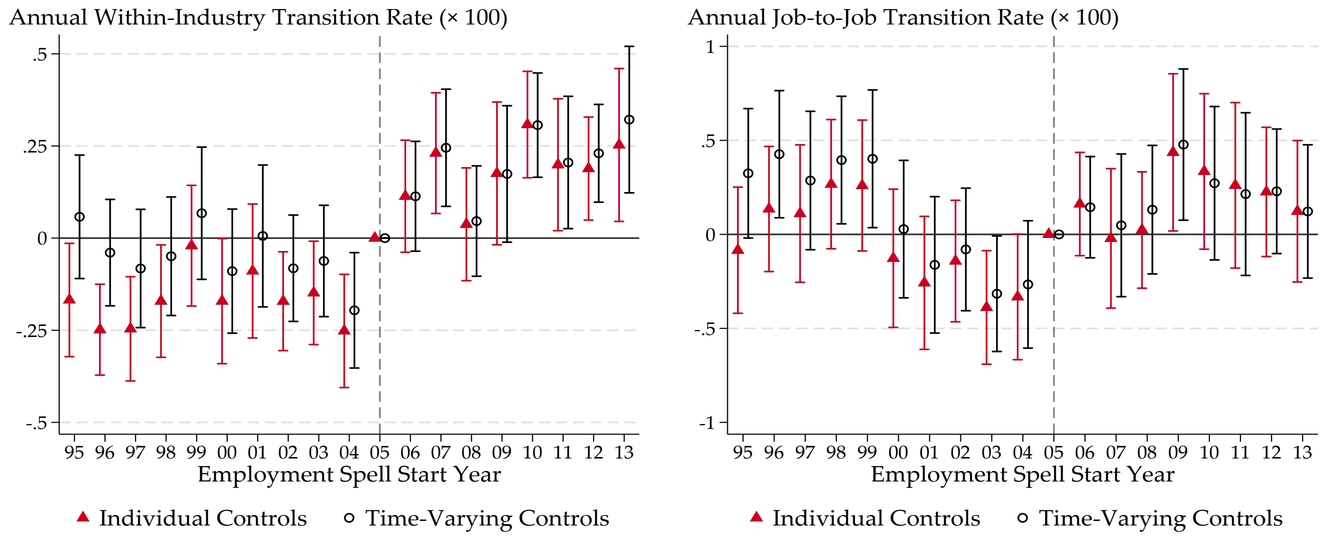
Figure A6: Summary Robustness Figure – *"Full Sample" Robustness*



Note: This figure presents the papers' main results (*Main Estimates*) and the same results including the "full-sample" of job tenure observations (*Full-Sample Robustness*). The full-sample robustness results include the same changes as described in the Figure A5 note. The main *Job Trans Types* estimates match the results in Tables 2 and A1. The main *Trans by Earnings* estimates match the results in Table 3. The main *Earnings Changes* estimates match the results in Tables 4 and A4. The *Baseline* results include the baseline controls, and the *Time-Varying* results include the time-varying controls.

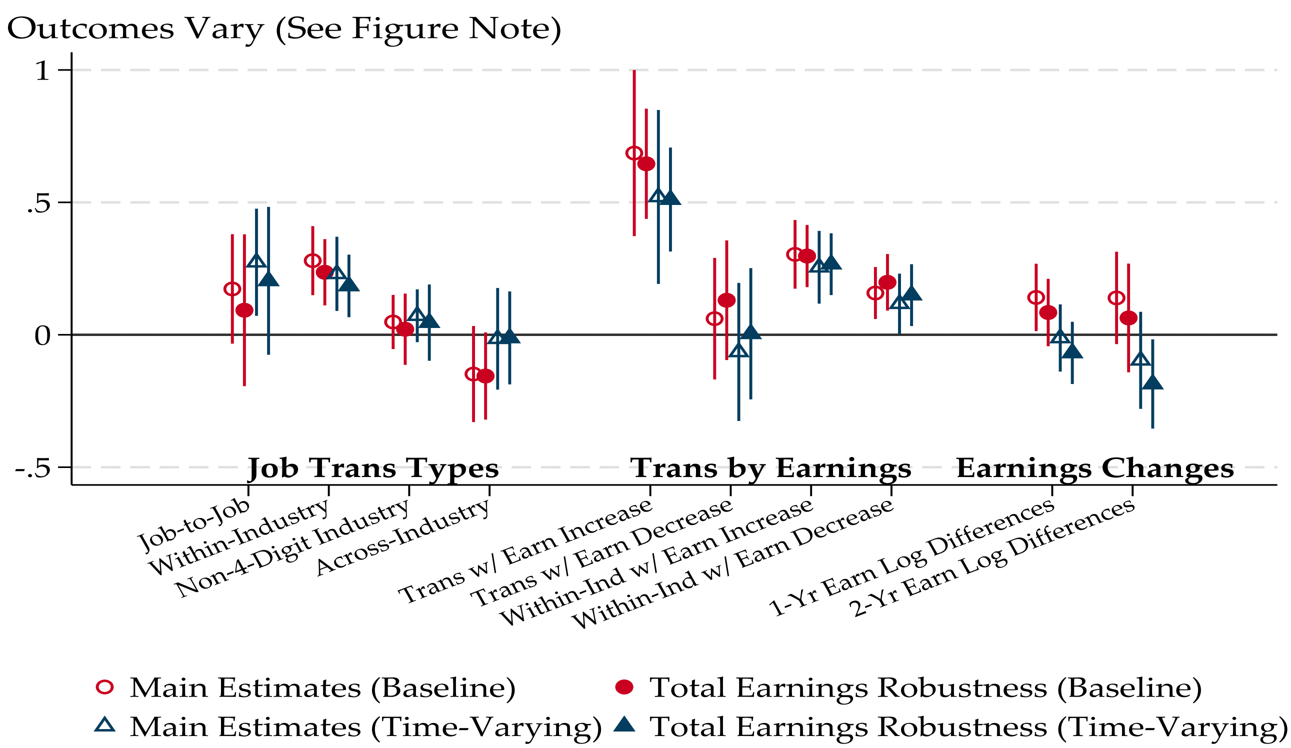
Figure A7: Job Mobility DiD Estimates – *"Total Earnings" Robustness*

Panel A: Within Four-Digit Industry Job Transitions   Panel B: Overall Job-to-Job Transitions



Note: This figure presents the same estimates as Figures 4 and 5 except that individuals' total annual earnings are used to define their treatment status instead of just their salaries. See Section H.2 for more details on the differences in these earnings concepts and how they relate to the legal earnings threshold. The change in the earnings concept affects who is included in the treated and control earnings groups and affects the earnings percentiles used as controls.

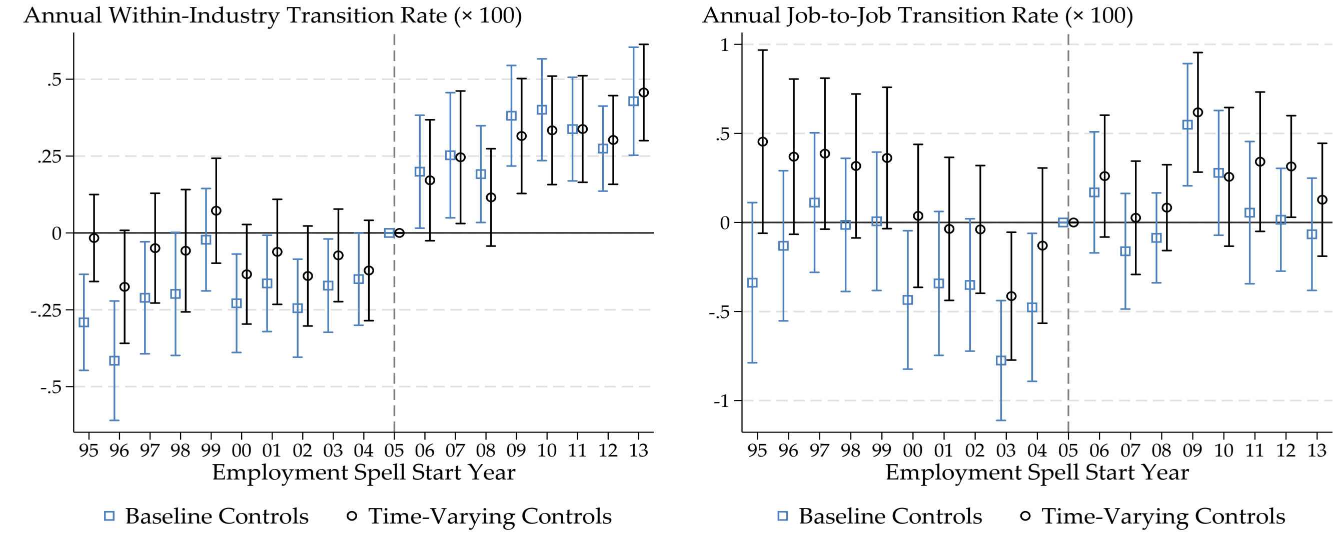
Figure A8: Summary Robustness Figure – *"Total Earnings" Robustness*



Note: This figure presents the papers' main results (*Main Estimates*) and the same results defining treatment based on individuals' total earnings (*Total Earnings Robustness*). The total earnings robustness results include the same changes described in the Figure A7 note. The main *Job Trans Types* estimates match the results in Tables 2 and A1. The main *Trans by Earnings* estimates match the results in Table 3. The main *Earnings Changes* estimates match the results in Tables 4 and A4. The *Baseline* results include the baseline controls, and the *Time-Varying* results include the time-varying controls.

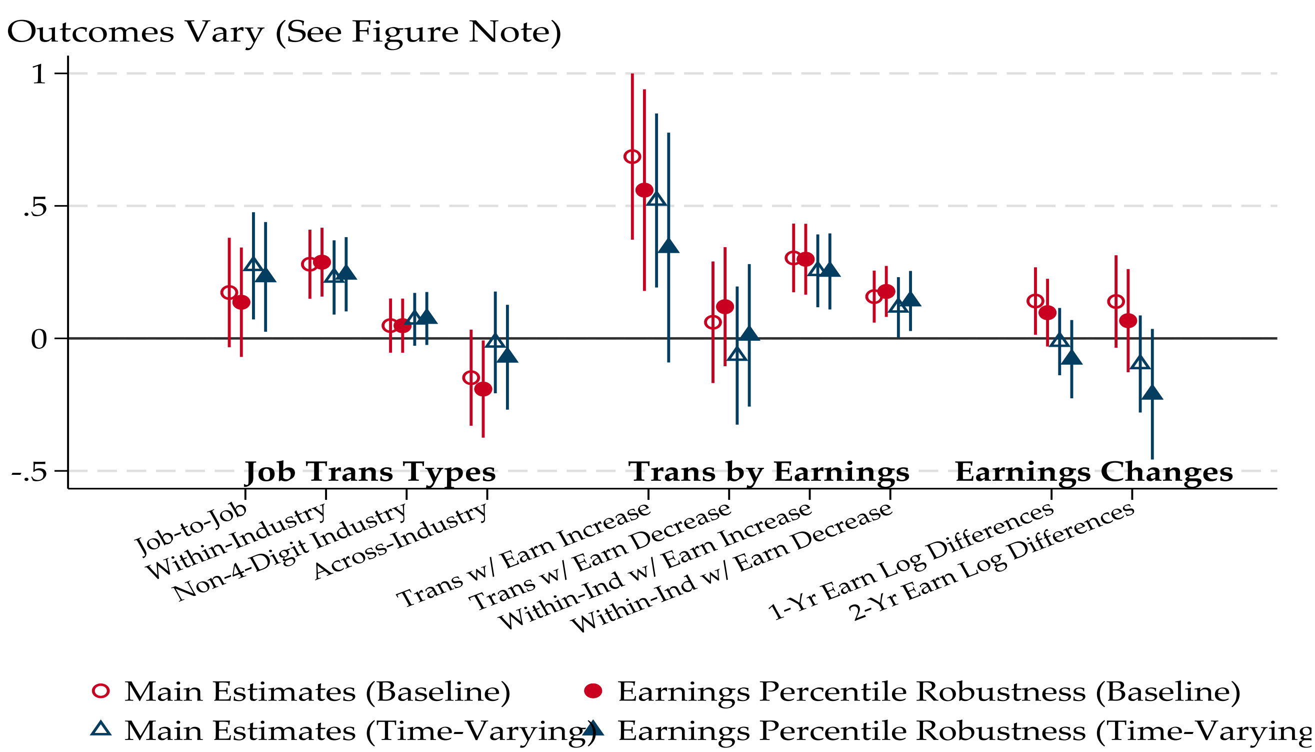
Figure A9: Job Mobility DiD Estimates – *"Earnings Percentile Controls" Robustness*

Panel A: Within Four-Digit Industry Job Transitions Panel B: Overall Job-to-Job Transitions



Note: This figure presents the same estimates as in Figures 4 and 5, except that the individual earnings percentile fixed effects are excluded from both specifications. I present the baseline and time-varying specifications and not the individual-control specification because the baseline specification is the one that is affected by omitting these controls.

Figure A10: Summary Robustness Figure – *"Earnings Percentile Controls" Robustness*

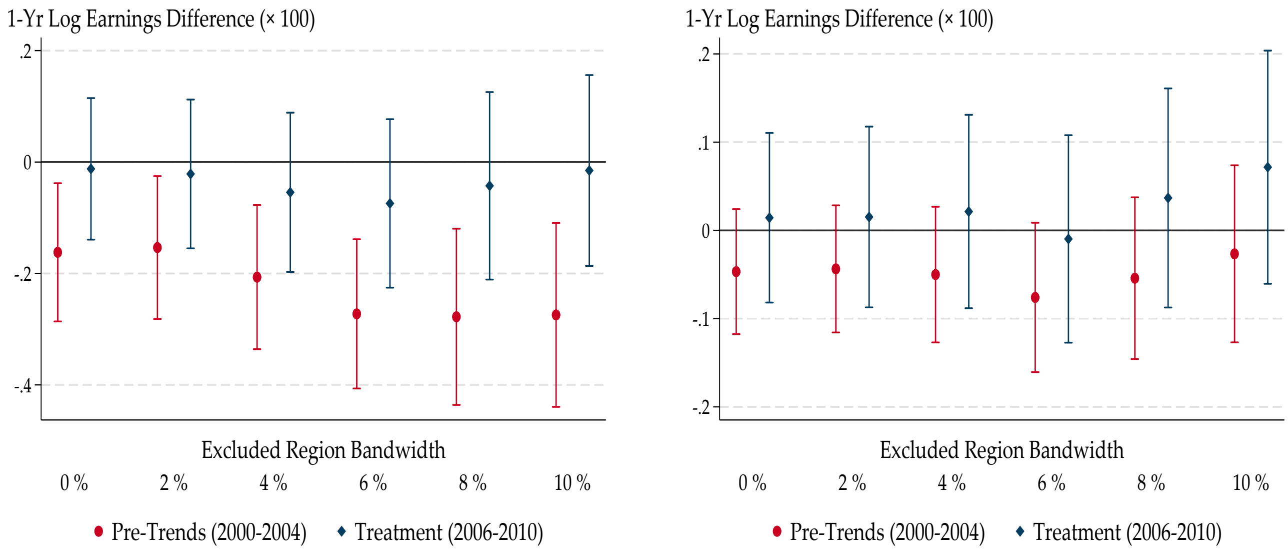


Note: This figure presents the papers' main results (*Main Estimates*) and the same results excluding individual earnings percentile fixed effects from the controls (*'Earnings Percentile Controls'*). The main *Job Trans Types* estimates match the results in Tables 2 and A1. The main *Trans by Earnings* estimates match the results in Table 3. The main *Earnings Changes* estimates match the results in Tables 4 and A4. The *Baseline* results include the baseline controls, and the *Time-Varying* results include the time-varying controls.

Figure A11: Earnings Change DiD Estimates – *"Donut-Hole" Robustness*

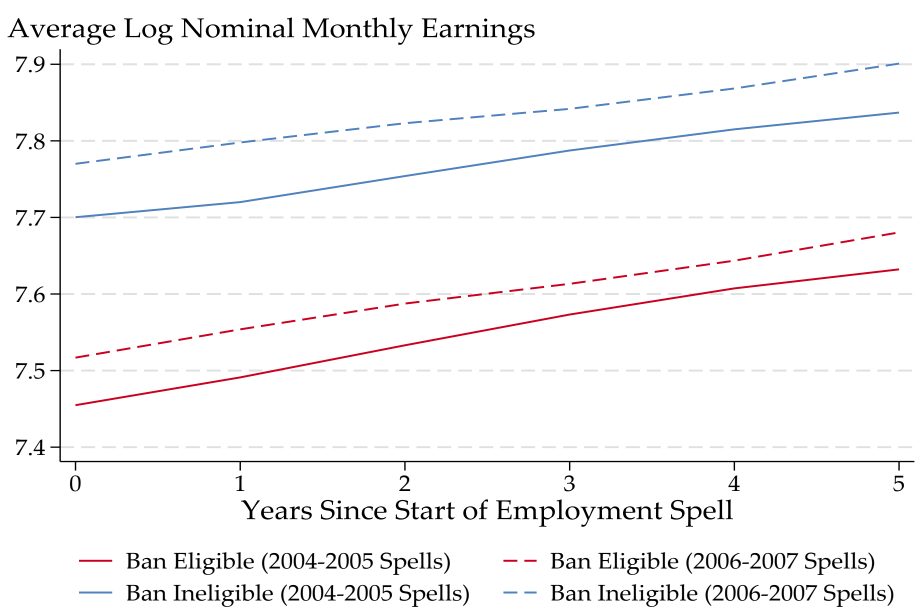
Panel A: Overall Earnings Differences

Panel B: Earnings Differences for Stayers



Note: Figure A11 plots the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the δ_m coefficients from the pooled specification in equation 2). The Pre-Trends estimates are the coefficient on spell starting years from 2000-2004, and the Treatment estimates are the coefficient on spell starting years from 2006-2010. The outcome variable in the annual change in employees' log earnings from their primary employer defined in Section 4. The left panel includes all employees with a non-missing earnings change, and the right panel restricts the sample to job stayers. The outcome variables are scaled by 100, so a coefficient estimate of 0.5 is approximately a half-percentage point increase in the annual earnings growth rate. These estimates are from the time-varying control specification. Standard errors are clustered at the earnings percentile level. The figure uses an overall bandwidth of 25% but varies the bandwidth around the earnings threshold that is *excluded* from the sample. For example, the 10% donut-hole specification includes employees earning between 75 and 90% of the threshold in the treated group and employees earning between 110% and 125% of the threshold in the control group.

Figure A12: Wage Profiles for Treated and Untreated Employment Spells



Note: This figure plots the average log nominal monthly earnings for individuals starting a ban eligible (earnings under the threshold) and ban ineligible (earnings above the threshold) employment spell from 2004-2005 and from 2005-2006. It only includes employees whose earnings in their first year of employment were between 75% and 125% of the earnings threshold. It follows these employees over the next five years regardless of whether or not they move employers (e.g., an individual starting a job in 2006 under the threshold will be in the `Ban Eligible (2006-2007 Spells)` category even if she later earns above the earnings threshold).

B Appendix Tables

Table A1: Job-to-Job Transition Heterogeneity: Within- versus Across-Industry Transitions

Job Transition Type:	Within Four-Digit Industry			Other Within-Industry			Across-Industry		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment × '95-'99	-0.12*	-0.13*	0.02	0.03	0.04	0.10*	0.10	0.22**	0.20*
	(0.07)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)	(0.11)	(0.10)	(0.10)
Treatment × '00-'04	-0.15***	-0.14**	-0.08	-0.09	-0.04	-0.02	-0.20*	0.00	-0.04
	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.11)	(0.11)	(0.11)
Treatment × '06-'10	0.28***	0.25***	0.23***	0.05	0.09	0.07	-0.15	-0.04	-0.02
	(0.06)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)	(0.09)	(0.09)	(0.10)
Treatment × '11-'13	0.33***	0.32***	0.34***	-0.02	0.03	0.03	-0.17	0.06	0.03
	(0.07)	(0.07)	(0.06)	(0.06)	(0.07)	(0.06)	(0.12)	(0.11)	(0.12)
Baseline	X	X	X	X	X	X	X	X	X
Individual		X	X		X	X		X	X
Time-Varying			X			X			X
Observations (1,000s)	8,626	8,354	8,354	8,626	8,354	8,354	8,626	8,354	8,354
R^2	0.028	0.031	0.033	0.011	0.012	0.013	0.031	0.039	0.041

Note: This table presents the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the δ_m coefficients from the pooled specification in equation 2). The outcome variable for the left three columns is an indicator variable for an annual job transition between two firms in the same four-digit NACE industry. The outcome variable for the next three columns is an indicator variable for transitions within the same coarse industry but not the same four-digit industry. The outcome variable for the last three columns is an indicator variable for transitions between two firms in different coarse industries. The outcome variables are scaled by 100, so a coefficient estimate of 0.2 represents a 0.2 percentage point increase in the annual probability of transitioning. Standard errors clustered at the earnings percentile level. The baseline controls include year fixed effects, two-digit industry by white/blue-collar fixed effects, and earnings percentile fixed effects. The individual controls add experience by white/blue-collar fixed effects, age by gender fixed effects, Bundesland fixed effects, tenure controls that vary by treatment, and spell starting month fixed effects that vary by treatment. The time-varying controls add two-digit industry by white/blue-collar by year fixed effects, Bundesland by year fixed effects, and annual GDP growth rates interacted with treatment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Job-to-Job Transition Heterogeneity: Within-Industry Job Transitions by UE Duration

Job Transition Type:	EE Trans			EUE Trans (1-3 months)			EUE Trans (4-6 months)			EUE Trans (7+ Months)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment × '95-'99	-0.04	-0.06*	0.01	-0.04	-0.06*	-0.04	0.00	-0.01	0.01	-0.01	-0.02	-0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)
Treatment × '00-'04	-0.02	-0.03	-0.01	-0.06**	-0.06**	-0.05*	-0.01	-0.01	-0.00	-0.00	-0.00	-0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Treatment × '06-'10	0.13***	0.10***	0.09**	-0.01	-0.01	0.01	0.03	0.03	0.03	0.05***	0.05***	0.06***
	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Treatment × '11-'13	0.22***	0.20***	0.18***	-0.08***	-0.04	0.00	0.00	0.02	0.03	0.03**	0.05***	0.07***
	(0.04)	(0.04)	(0.04)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Baseline	X	X	X	X	X	X	X	X	X	X	X	X
Individual		X	X		X	X		X	X		X	X
Time-Varying			X			X			X			X
Observations	8,625,616	8,353,564	8,353,525	8,625,616	8,353,564	8,353,525	8,625,616	8,353,564	8353525	8,625,616	8,353,564	8,353,525
R^2	0.006	0.008	0.010	0.009	0.011	0.011	0.005	0.006	0.006	0.005	0.006	0.006

Note: This table presents the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the δ_m coefficients from the pooled specification in equation 2). The outcome variables split up within four-digit industry transitions based on the duration of *unemployment* between jobs. For example, an *EUE Transition (4-6 months)* means that an employee transitioned between two jobs in the same industry but spent 4-6 months unemployed between the jobs. For EUE transitions with 7+ months of unemployment, the individual must be employed at each of the two firms for at least one month in two consecutive years. Transitions with intermediate spells of nonemployment that do not include any unemployment are excluded because they may include labor market states that are more similar to employment (e.g., self-employment or minor employment). A spell that includes both unemployment and other types of nonemployment between jobs, however, is included. For example, a job-to-job transition with three months of unemployment and then three months of self-employment between jobs would be classified as an EUE 4-6 month transition. The outcome variables are scaled by 100, so a coefficient estimate of 0.2 represents a 0.2 percentage point increase in the annual probability of transitioning. Standard errors clustered at the earnings percentile level. The baseline controls include year fixed effects, two-digit industry by white/blue-collar fixed effects, and earnings percentile fixed effects. The individual controls add experience by white/blue-collar fixed effects, age by gender fixed effects, Bundesland fixed effects, tenure controls that vary by treatment, and spell starting month fixed effects that vary by treatment. The time-varying controls add two-digit industry by white/blue-collar by year fixed effects, Bundesland by year fixed effects, and annual GDP growth rates interacted with treatment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Pooled DiD Estimates – Job Transitions by Earnings Quantile Changes

Job Transition Type:	Overall Job-to-Job				Within Four-Digit Industry			
Transition Condition:	*Earnings Increase*		*Earnings Decrease*		*Earnings Increase*		*Earnings Decrease*	
Treatment × ’95-’99	0.26*	0.34**	-0.08	0.05	-0.08	-0.02	0.07	0.14***
	(0.14)	(0.14)	(0.12)	(0.12)	(0.06)	(0.06)	(0.05)	(0.05)
Treatment × ’00-’03	0.03	0.00	-0.08	-0.01	-0.05	-0.04	0.04	0.07
	(0.13)	(0.13)	(0.11)	(0.10)	(0.07)	(0.07)	(0.05)	(0.05)
Treatment × ’06-’10	0.48***	0.50***	0.05	-0.05	0.27***	0.25***	0.16***	0.12*
	(0.16)	(0.16)	(0.13)	(0.13)	(0.07)	(0.07)	(0.06)	(0.06)
Treatment × ’11-’13	0.46***	0.44***	0.13	0.09	0.33***	0.31***	0.15**	0.15**
	(0.16)	(0.16)	(0.15)	(0.15)	(0.07)	(0.07)	(0.06)	(0.06)
Baseline	X	X	X	X	X	X	X	X
Individual	X	X	X	X	X	X	X	X
Time-Varying		X		X		X		X
Observations	7,732,960	7,732,920	7,732,960	7,732,920	7,725,089	7,725,049	7,725,089	7,725,049
R^2	0.041	0.043	0.027	0.028	0.017	0.019	0.021	0.022

Note: This figure presents the same estimates as in Table 3, except that the earnings transitions are classified based on individuals' earnings quartiles rather than nominal earnings changes. Specifically, I define the outcome variable based on 2000 year-specific earnings quantiles:

$$Y_{it} = 100 \times \mathbb{1}\left[j(i,t+1) \neq j(i,t) \text{ AND earn qnt}_{i,t+1} > \text{earn qnt}_{i,t}\right].$$

Defining the transitions based on earnings quantile changes accounts for the fact that individuals may have experienced earnings growth regardless of whether or not they switched jobs. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Pooled DiD Estimates – Longer Time Horizon Earnings Log Differences

	Panel A: Individual Control Specification				
Earnings Log Diff Horizon:	1-Yr	2-Yr	3-Yr	4-Yr	5-Yr
Treatment × ’06-’10	0.02	-0.05	0.12	0.09	0.27**
	(0.06)	(0.09)	(0.11)	(0.11)	(0.11)
Observations	7,732,960	7,337,307	6,990,233	6,606,076	6,149,139
R^2	0.019	0.031	0.042	0.054	0.064
	Panel B: Time-Varying Control Specification				
Earnings Log Diff Horizon:	1-Yr	2-Yr	3-Yr	4-Yr	5-Yr
Treatment × ’06-’10	-0.01	-0.10	0.03	-0.02	0.16
	(0.06)	(0.09)	(0.11)	(0.12)	(0.12)
Observations	7,732,920	7,337,266	6,990,191	6,606,038	6,149,100
R^2	0.021	0.033	0.045	0.056	0.066

Note: This table presents the estimated coefficients on the interaction between the spell starting year groups and having contemporaneous earnings in the treated range (the δ_m coefficients from the pooled specification in equation 2). The outcome variables are individual-level log earnings changes over different time horizons. For example, the five-year earnings change is $Y_{i,t} = 100 \times [\ln(w_{i,t+5}) - \ln(w_{i,t})]$. The outcome variables are scaled by 100, so a coefficient estimate of 0.5 is approximately a half-percentage point increase in the annual earnings growth rate. Standard errors clustered at the earnings percentile level. The individual controls include year fixed effects, two-digit industry by white/blue-collar fixed effects, earnings percentile fixed effects, experience by white/blue-collar fixed effects, age by gender fixed effects, Bundesland fixed effects, tenure controls that vary by treatment, and spell starting month fixed effects that vary by treatment. The time-varying controls add two-digit industry by white/blue-collar by year fixed effects, Bundesland by year fixed effects, and annual GDP growth rates interacted with the treatment indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Pooled DiD Estimates – One-Year Log Earnings Difference Robustness

Earnings Concept:	Total Earnings		Salaries		Winsorized Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × '95-'99	-0.11	-0.05	-0.14*	-0.07	-0.13**	-0.07
	(0.07)	(0.06)	(0.07)	(0.07)	(0.06)	(0.05)
Treatment × '00-'04	-0.14**	-0.16**	-0.13*	-0.15**	-0.14**	-0.15***
	(0.06)	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)
Treatment × '06-'10	0.02	-0.01	0.02	-0.01	0.00	-0.03
	(0.06)	(0.06)	(0.07)	(0.07)	(0.05)	(0.06)
Treatment × '11-'13	0.02	-0.15**	0.07	-0.09	-0.03	-0.19***
	(0.07)	(0.08)	(0.08)	(0.08)	(0.06)	(0.06)
Baseline	X	X	X	X	X	X
Individual	X	X	X	X	X	X
Time-Varying		X		X		X
Observations	7,732,960	7,732,920	7,732,960	7,732,920	7,732,960	7,732,920
R^2	0.019	0.021	0.021	0.023	0.025	0.027

Note: This figure presents the same estimates as in Table 4 with two different earnings concepts as the outcome variables. The outcome variables are different measures of individual-level, one-year log earnings differences. The first two columns present results for total earnings. The third and fourth columns use only individuals' salaries (i.e., excluding special payments and bonuses). The fifth and sixth columns winsorize total earnings differences at the 1^{st} and 99^{th} percentiles. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Testing Coefficient Equality across Different Models

In this section, I describe how I conduct hypothesis tests of whether coefficients are equal across different models. I use this method for the analysis of firm-quality changes and job transitions with earnings increases versus decreases. For these analyses, I initially estimate two regressions with different outcome variables but the same independent treatment variable. At a high-level (e.g., ignoring controls), I estimate the following separate regressions

$$Y^1_{it} = \beta_1 X_{it} + \epsilon_{it} \tag{A1}$$

$$Y^2_{it} = \beta_2 X_{it} + \gamma_{it}, \tag{A2}$$

and want to test whether $\beta_1 = \beta_2$. This hypothesis test depends on the covariance of $\hat{\beta}_1$ and $\hat{\beta}_2$, which is not recovered when estimating equations A1 and A2 separately.

To conduct this hypothesis test, I instead "stack" the two datasets used to estimate equations A1 and A2 separately and estimate β_1 and β_2 jointly as follows:

$$\begin{pmatrix} Y^1_{it} \\ Y^2_{it} \end{pmatrix} = \begin{pmatrix} X_{it} & 0 \\ 0 & X_{it} \end{pmatrix} \begin{pmatrix} \beta_1 \\ \beta_2 \end{pmatrix} + \begin{pmatrix} \epsilon_{it} \\ \gamma_{it} \end{pmatrix}. \tag{A3}$$

I still cluster the standard errors at the earnings percentile level. However, the earnings percentiles are defined to be the same across both stacked datasets (i.e., I cluster at the overall earnings percentile level rather than the earnings percentile by dataset level). This allows for a within-observation correlation between ϵ_{it} and γ_{it}, which allows for a non-zero covariance between $\hat{\beta}_1$ and $\hat{\beta}_2$. I then conduct a t test of whether $\hat{\beta}_1 - \hat{\beta}_2 = 0$ and report the p value.

D Interpretation of the First Differenced Earnings Regressions

Conceptually, the results estimating the effect of noncompetes on workers' overall wage growth (e.g., Figure 11 and Tables 4 and A4) are similar to results from estimating the following regression

$$100 \times \Big(\ln (w_{i,t+1}) - \ln (w_{i,t})\Big) = \beta \cdot \boldsymbol{T}_{i,t} + \varepsilon_{i,t}. \tag{A4}$$

Here $\boldsymbol{T}_{i,t}$ is an indicator for whether an individual i at time t had a noncompete in her employment contract. In my analysis, treatment is instead an indicator for *noncompete eligibility* but the interpretation remains the same if the estimate is properly scaled by the effect of eligibility on the actual use of noncompetes (i.e., the first stage). The estimated treatment effect β is consequently the effect of having a noncompete at time t on workers' *earnings growth* from time t to $t+1$. To quantitatively interpret β, note that log-differenced earnings approximate earnings percent changes

$$100 \times \Big(\ln (w_{i,t+1}) - \ln (w_{i,t})\Big) \approx 100 \times \frac{w_{i,t+1} - w_{i,t}}{w_{i,t}} \tag{A5}$$

Consequently, a one-unit increase in β represents a **one percentage point increase in the annual earnings growth rate**. For example, a coefficient of 0.5 in Table 4 would represent a 0.5 percentage point increase in annual earnings growth.

To see why this specification captures the effect of noncompetes on workers' earnings growth rather than starting earnings, imagine that noncompetes also linearly affect workers' starting level of earnings. Assuming that within each employment spell individuals' noncompete treatment does not change, this effect would be differenced out because it would affect both $\ln (w_{i,t+1})$ and $\ln (w_{i,t})$

the same. There are, however, two reasons why an individuals' noncompete status might change between periods t and $t+1$. First, the employee might change jobs. But by focusing on job-stayers in Figure 11, the original interpretation is preserved. Second, in my analysis, $\boldsymbol{T}_{i,t}$ represents noncompete eligibility. While working for the same employer, an employees' noncompete eligibility could change (e.g., if at the start of their employment they earned below the earnings threshold but their earnings then increased to above the threshold). In this case, β would also pick up some of the effect of noncompetes on workers' earnings levels.[55] However, these cases seem sufficiently rare that they likely do not affect the interpretation of β as capturing the effect of noncompetes on workers' earnings growth rather than any effect of noncompetes on the level of earnings.

E Overall Job Transition Effect Decomposition

The purpose of this decomposition is to decompose how large of an impact a noncompete ban would have on overall job-to-job mobility depending on (1) the share of workers who had a noncompete before the ban, (2) the share of workers' job transitions that were restricted by noncompetes before the ban, and (3) the effect of a noncompete on the transitions that were restricted by the ban. Let the overall average job transition rate for a group of workers be $\mathrm{E}[Y_i|\boldsymbol{X}_i]$ where $\boldsymbol{X} \in \{\boldsymbol{T}, \boldsymbol{C}\}$ is an indicator for whether the workers were treated by a noncompete ban, $\boldsymbol{T}$, or not treated $\boldsymbol{C}$.[56]

Some workers have noncompetes when not treated and other workers do not so average job mobility is equal to the following where `NC = 1` indicates having a noncompete when not treated

$$\mathrm{E}[Y_i|\boldsymbol{X}_i] = \mathrm{E}[Y_i|\boldsymbol{X}_i, \mathrm{NC}_i = 1] \times \Pr(\mathrm{NC}_i = 1) + \mathrm{E}[Y_i|\boldsymbol{X}_i, \mathrm{NC}_i = 0] \times \Pr(\mathrm{NC}_i = 0) \tag{A6}$$

I assume that the noncompete ban only affects workers who would have had noncompetes absent the ban (i.e., $\mathrm{E}[Y_i|\boldsymbol{X}_i, \mathrm{NC}_i = 0]$ does not depend on $\boldsymbol{X}_i$). This assumption rules out GE effects that might spill over from the workers with noncompetes to workers without noncompetes.

The quantity of interest is the percent change in the job transition rate between the treated and control groups which we can rearrange as follows

$$\frac{\mathrm{E}[Y_i|\boldsymbol{T}_i] - \mathrm{E}[Y_i|\boldsymbol{C}_i]}{\mathrm{E}[Y_i|\boldsymbol{C}_i]} = \Pr(\mathrm{NC}_i = 1) \times \frac{\mathrm{E}[Y_i|\boldsymbol{T}_i, \mathrm{NC}_i = 1] - \mathrm{E}[Y_i|\boldsymbol{C}_i, \mathrm{NC}_i = 1]}{\mathrm{E}[Y_i|\boldsymbol{C}_i]} \tag{A7}$$

$$+ \underbrace{\Pr(\mathrm{NC}_i = 0) \times \frac{\mathrm{E}[Y_i|\boldsymbol{T}_i, \mathrm{NC}_i = 0] - \mathrm{E}[Y_i|\boldsymbol{C}_i, \mathrm{NC}_i = 0]}{\mathrm{E}[Y_i|\boldsymbol{C}_i]}}_{=0 \text{ by assumption}} \tag{A8}$$

Additionally, not all job transitions might be affected by noncompetes. Let $Y_i = Y_i^A + Y_i^U$ where Y_i^A are affected job transitions (e.g., within-industry transitions) and Y_i^U are unaffected transitions (e.g., across-industry transitions). By assumption, $\mathrm{E}[Y_i^U|\boldsymbol{T}_i, \mathrm{NC}_i = 1] = \mathrm{E}[Y_i^U|\boldsymbol{C}_i, \mathrm{NC}_i = 1]$ since the noncompete ban doesn't have an effect on unaffected transitions. Plugging $Y_i = Y_i^A + Y_i^U$ into

[55] Note that if noncompetes increased workers' starting earnings, this would actually increase the earnings growth for workers with $\boldsymbol{T}_{i,t} = 0$ because I measure earnings changes from time t to $t+1$ and only workers with $\boldsymbol{T}_{i,t} = 0$ can go from not having a noncompete to having a noncompete.

[56] Note, that here treatment and control are different than the treated and control earnings ranges used in the paper's main analysis. Instead, it is clearer to think of control workers as the workers below the earnings limit *before* the ban was enacted and treated workers as workers below the earnings limit *after* the ban was enacted.

equation A7 yields

$$\frac{\mathrm{E}[Y_i|\boldsymbol{T}_i] - \mathrm{E}[Y_i|\boldsymbol{C}_i]}{\mathrm{E}[Y_i|\boldsymbol{C}_i]} = \Pr(\mathrm{NC}_i = 1) \times \frac{\mathrm{E}[Y_i^A|\boldsymbol{T}_i, \mathrm{NC}_i = 1] - \mathrm{E}[Y_i^A|\boldsymbol{C}_i, \mathrm{NC}_i = 1]}{\mathrm{E}[Y_i|\boldsymbol{C}_i]} \tag{A9}$$

$$= \Pr(\mathrm{NC}_i = 1) \times \frac{\mathrm{E}[Y_i^A|\boldsymbol{C}_i]}{\mathrm{E}[Y_i|\boldsymbol{C}_i]} \times \frac{\mathrm{E}[Y_i^A|\boldsymbol{T}_i, \mathrm{NC}_i = 1] - \mathrm{E}[Y_i^A|\boldsymbol{C}_i, \mathrm{NC}_i = 1]}{\mathrm{E}[Y_i^A|\boldsymbol{C}_i]} \tag{A10}$$

$$= \underbrace{\begin{array}{c}\text{Share Workers} \\ \text{w/ Noncompetes}\end{array}}_{33\%} \times \underbrace{\begin{array}{c}\text{\% Transitions} \\ \text{Restricted}\end{array}}_{22\%} \times \underbrace{\begin{array}{c}\text{\% ToT on} \\ \text{Restricted Transitions}\end{array}}_{20\%} = 1.5\% \tag{A11}$$

The terms in equation A10 correspond to the terms in equation 6 where

- $\Pr(\mathrm{NC}_i = 1)$ = the share of workers with a noncompete when they are not banned.
- $\frac{\mathrm{E}[Y_i^A|\boldsymbol{C}_i]}{\mathrm{E}[Y_i|\boldsymbol{C}_i]}$ = the share of workers' job transitions that are restricted by a noncompete. In this context, it is the share of all job transitions that are within the same four-digit industries because it was difficult for noncompetes to legally prevent broader transitions.
- $\frac{\mathrm{E}[Y_i^A|\boldsymbol{T}_i, \mathrm{NC}_i=1] - \mathrm{E}[Y_i^A|\boldsymbol{C}_i, \mathrm{NC}_i=1]}{\mathrm{E}[Y_i^A|\boldsymbol{C}_i]}$ = the percentage treatment effect on affected job transitions. In this context, it is the effect of the reform on within-industry job transitions scaled by the estimated pre-reform prevalence of noncompetes.

Note, the 1.5 percent increase is slightly smaller than the headline estimate of two percent because it extrapolates from the within-industry job transition point estimate rather than the overall job transition point estimate.

F Institutional Details of the 2006 Reform

In this section, I describe the political process leading up to the 2006 noncompete ban. The analysis is based on searching for "Konkurrenzklausel(n)" in the legislative records provided by the Austrian Parliament, the Archive of Austrian Press Releases by the Austrian Press Agency (APA-OTS), and Wiso-Net from 2000-2020. There are two main takeaways from this analysis relevant for the interpretation of my empirical results.

1. Before May 2005 (10 months before the bill was enacted), there was no public discussion of the noncompete ban. Between May 2005 and March 2006, however, the legislative debate about noncompetes was covered by newspapers. Additionally, the eventual earnings limit was not proposed until Nov 2005. Consequently, while some anticipatory behavior by employers between May 2005 and March 2006 is possible (e.g., employers stopping putting noncompetes in employees' contracts), any anticipatory behavior before that window is unlikely.

2. The reform's exact earnings cutoff appears to have been agreed upon through a fairly idiosyncratic negation process (e.g., the proposed limits ranged from €324 to €3,600 per month) and the limit was unrelated to other policy discussions. This provides reassurance that the limit was chosen somewhat randomly and not based on (future) mobility changes for workers above and below the limit.

The first legislation concerning noncompetes in Austria was passed in 1921 and banned noncompetes for workers earning less than 120,000 Kronen and for restrictions beyond an employees' current "line of business."[57] The nominal limit of 120,000 Kronen (soon converted into a Shillings limit), however, was quickly eaten away by inflation rendering it effectively non-existent. With the introduction of the Euro in 2002, the earnings limit was removed from the legislation.[58]

In 2003, the opposition party, the Social Democrats Austria (SPÖ), introduced a bill to parliament that would completely ban noncompetes in employment contracts. The justification in the bill was that noncompetes impede workers' job mobility and that they were increasingly used for low-wage workers including workers in tourism, hospitality, and seasonal workers.[59]

In May 2005, the governing Austrian People's Party (ÖVP) introduced a bill countering the SPÖ's proposal that would only ban noncompetes for "marginal employment" (earning less than €324/month). In response, the SPÖ and the Chamber of Labor launched a public campaign arguing for the complete abolition of noncompetes (Zeitung, 2005). Over the next few months, major newspapers start covering the prevalence of noncompetes and the ongoing political debate (Vavra, 2005; Meickl, 2005; Presse, 2005).[60] During this debate Christoph Klein of the Chamber of Labor suggested that the earnings limit should be €3,000/month (Nachrichten, 2005).

In November of 2005, a coalition of parties amended the ÖVP's bill to set the threshold for banning noncompetes to €2,048 per month (the final $\frac{17}{30}$ths of the social security maximum that eventually passed).[61] At the time, however, there was still a push from other parties and the Chamber of Labor to include a higher limit (e.g., Karl Öllinger argued that the limit should instead be €3,600 per month (Marschall, 2005)). On March 1st 2006, the bill passed and went into effect on March 17th for white-collar workers and on March 18th for blue-collar workers.

[57] See Angestelltengesetz Art. 1 §36 in effect from 1921-2001.

[58] See Angestelltengesetz Art. 1 §36 in effect from 2002-2006.

[59] Relatedly, a report from the Parliamentary Committee on Labor and Social Affairs observed that "noncompete clauses are agreed not only among highly skilled and well-educated workers, but to an increasing extent also among poorly educated and low-income workers" and that these agreements "impose disproportionate impediments on workers' mobility" (Walch and Silhavy, 2005).

[60] Prior to 2005, I did not find any major newspaper articles concerning noncompetes or covering the legislative debate.

[61] See page 302 of the parliamentary records from December 2005 for a discussion of this negotiation.

G Earnings Effects from Increased Transitions Power Calc.

For this power calculation, I am interested in understanding how large of an impact on overall wage growth can result from just increasing job-to-job transitions (potentially with earnings increases). Let individual i at time t have a wage $w_{i,t}$. The overall treatment effect on wage growth that I estimate is the difference between average wage growth for treated workers and control workers

$$\text{Earnings Treatment Effect} = \mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{T}] - \mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{C}]. \tag{A12}$$

To help decompose changes in earnings growth from job movers versus job stayers, we can classify individuals as `always stayers`, `always movers`, and `complier movers`. `Always stayers` do not move jobs regardless of the noncompete policy. `Always movers` move jobs regardless of the noncompete policy. `Complier movers` only move jobs when noncompetes are banned.[62] With these classifications, overall wage growth for group $\boldsymbol{X} \in \{\boldsymbol{T}, \boldsymbol{C}\}$ is equal to

$$\begin{aligned}
\mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{X}] =& \mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{X}, \text{Always Stayer}] \times \Pr(\text{Always Stayer}) + && \text{(A13)}\\
& \mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{X}, \text{Always Mover}] \times \Pr(\text{Always Mover}) + && \text{(A14)}\\
& \mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{X}, \text{Complier Mover}] \times \Pr(\text{Complier Mover}) && \text{(A15)}
\end{aligned}$$

For this power calculation, I assume that the noncompete ban only affects workers by increasing their propensity to switch jobs. This consequently affects the earnings growth of the `complier movers` because they get the earnings growth from moving to a new firm rather than staying at their original firm. This assumption implies that the earnings growth for stayers and for always movers is not affected by the policy change (i.e., $E[\Delta \ln(w_{i,t})|\boldsymbol{X}, \text{Always Stayer}]$ and $E[\Delta \ln(w_{i,t})|\boldsymbol{X}, \text{Always Mover}]$ do not depend on $\boldsymbol{X}$).[63] Under this assumption, the overall effect on earnings growth is

$$\mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{T}] - \mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{C}] = \Bigg(\mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{T}, \text{Complier}] - \mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{C}, \text{Complier}]\Bigg) \times \Pr(\text{Complier}) \tag{A16}$$

Consequently, to benchmark the effect on overall wage growth, we need estimates of the difference in wage growth for compliers under treatment and control and of the share of individuals who are compliers.

1. The share of individuals who are compliers is the estimated treatment effect of the policy on overall job-to-job transitions. I pick the estimate in Column (6) of Table 2 of 0.0027 (note the estimates in that table are scaled by 100). I also present estimates that assume this share is equal to the lower and upper confidence intervals of the same estimate (0.07 and 0.47 respectively).

2. I conservatively assume that $\mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{C}, \text{Complier}]$, the wage growth for the compliers if they stay in their same job, is equal to zero. This is empirically false but will *overestimate* how large of an effect job transitions will have on overall earnings growth. Instead, I could

[62] I am assuming away the existence of `defier movers` who move jobs when noncompetes are allowed but do not move jobs when noncompetes are banned. Some general equilibrium effects (e.g., the crowding out of available spots at some firms) could result in defier movers.

[63] Using counteroffers to increase your wages through on-the-job search would imply that $E[\Delta \ln(w_{i,t})|\boldsymbol{X}, \text{Always Stayer}]$ could be affected by treatment. Yet, Figure 11 does not find any detectable impact on stayers' wages. It is also possible that treatment would affect the entire distribution of wage changes for movers (e.g., if workers would have transitioned across industries before the ban but now can transition to a better job in their same industry). Yet, Figure 6 and Table A1 do not provide strong evidence of a decrease in across-industry job transitions which we might expect if this were the case.

assume that $\mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{C}, \text{Complier}]$ is equal to the average earnings growth for job stayers after the reform.

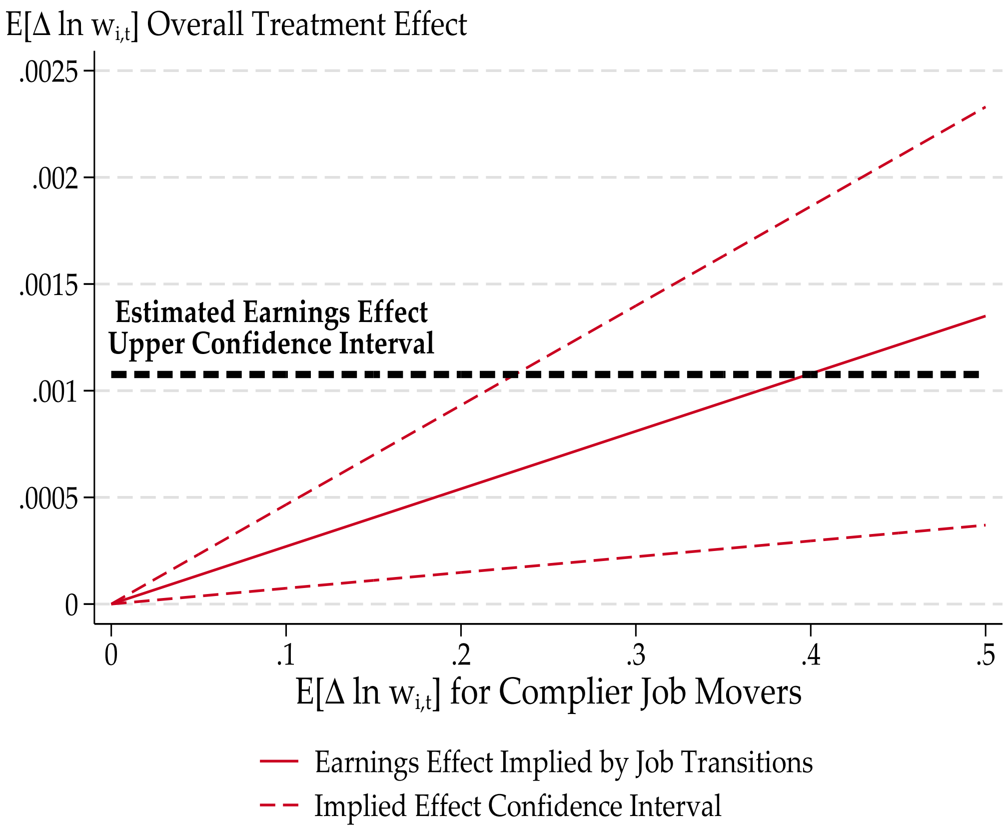
3. I vary $\mathrm{E}[\Delta \ln(w_{i,t})|\boldsymbol{T}, \text{Complier}]$ between 0 and 0.5 where 0.5 implies that all marginal movers increase their log earnings by 0.5 when they move jobs which is roughly a 65% increase in earnings. This is more than an order of magnitude larger than the average earnings increases experienced by job switchers.

For the compliers' earnings changes under control and treatment, I assume there is no uncertainty so that the only uncertainly in the overall earnings effect is from the estimate on the effect on job-to-job transitions.

Figure A13 presents the overall earnings effects implied by Equation A16 for different values of $\mathrm{E}[\Delta \ln(w_{i,t})|\, \boldsymbol{T}, \text{Complier}]$ and the point estimate and confidence intervals for Pr(Complier). The solid red line plots the implied overall earnings effect using the Pr(Complier) point estimate in Table 2, Column (6). The two dashed red lines use the confidence intervals for that point estimate. The horizontal dashed black line is the upper bound of the confidence interval of the estimated overall earnings effect in Table 4. Consequently, comparing the red and black lines tells us for what values of earnings growth for the marginal job movers, would the implied effect from the estimated increase in job transitions be above the confidence interval for the estimated overall effect on earnings.

Figure A13 shows that the earnings changes for job switchers would need implausibly large such that the effect just from increased job transitions would show up in the effect on overall earnings growth. Using the point estimate for the effect on job transitions (the solid red line) the average earnings log difference for these induced job transitions would need to be almost 0.4 or almost a 50% increase in earnings for the effect to be outside the estimated confidence interval for overall earnings growth. Even using the upper bound of the estimated effect on job transitions (the upper dashed red line) the average earnings log difference for the induced job transitions would need to be above 0.2 or a 22% increase in annual earnings. Overall, the figure helps reconcile why I find that the reform increased job transitions with earnings increases but that I find no detectable impact on overall average wage growth for the treated workers. The effect on job transitions is too small for it to have a significant impact on overall wage growth for plausible values of earnings changes for job switchers.

Figure A13: Effect of Job Transitions on Earnings Growth – *Power Calculation*



Note: This figure presents a power calculation of how large of an impact on overall wage growth can result from just increasing job-to-job transitions estimated in Table 2. The y-axis plots the overall change in earnings for the treated versus control groups. The x-axis varies the change in earnings for *complier job movers* (e.g., the job movers who are induced to move jobs because of the noncompete ban). The solid red line plots the effect on overall earnings taking the point estimate from Table 2 of the effect on overall job transitions. The dashed red lines plot the effect instead using the upper and lower confidence interval values of the estimated job transition effect. The thicker dashed black line is the upper bound of the estimated effect on overall earnings growth from Table 4.

H Sample Construction Details

H.1 Creating Consistent Firm IDs Across Time

Firm IDs in the ASSD may change for administrative reasons without any economic rationale (Fink et al., 2010). In my context, this would create spurious job transitions and spurious new employment spells (e.g., if firm A changed its ID to B in 2006, it would look like there are many new employment contracts starting in 2006 when all workers would actually have the same employment contract as beforehand). To account for these ID changes, I use a "worker-flows approach" that follows similar approaches in Austria and Germany (see Fink et al. (2010) and Hethey-Maier and Schmieder (2013) respectively). Note that while I use this classification for all firms, they will be less precise for smaller firms (e.g., 3 out of 4 workers going to the same other firm is less strong evidence of an ID change than 300 out of 400). Specifically, I create a set of consistent firm IDs as follows:

1. For each year, I take each firms' employees in December of that year.

2. For firm IDs that have employees one year, have no employees for at least one following year, and then appear again, I make a new firm ID for the second set of firm observations. The rationale here is that for classifying firm characteristics, the firm is likely very different when it reappears after a few years without employees (e.g., new management, new ownership, etc.).

3. Define the year of firm entry and exit as follows:

   (a) A firm (enters) exits in a given year if that year is the (first) last year the firm is observed with employment.

   (b) If a firms' size increases by more than 20-fold (e.g., a firm that increases employment from 10 to 201 employees) and the firm entered in the previous five years, I also count the large increase as the year the firm entered. The reason is that there are observations of firm spin-offs or firm ID changes where first a few workers move to the new firm and then the ID changes. For example, in year t, 3 employees move and in year $t+1$, the remaining 2,000 workers move. If I classified the entry year as year t, I would not then classify this transition as a firm ID change.

   (c) If a firms' size decreases to less than 5% of previous employment and exits in the next five years, I count the year of the large decrease as the year of firm exit. The reason is similar to above (e.g., often a firm will reduce all of its employment in year t except for a few employees that leave the following year).

4. For each firm A in year t, I classify all of the worker flows to the firm from year $t-1$ to year t and the flows out of the firm from year t to year $t+1$. For example, I classify what share of workers stayed at the firm, what share of workers exited the firm to other firms, and what share of the workers exited to nonemployment. I then further classify the largest share of workers who all exited to the same other firm each year. I calculate similar quantities for where worker flows came from each period.

5. Using the worker flows to and from each firm, I classify firm exits as follows:

   (a) **Continuing Firm:** Firm A does not exit.

   (b) **Firm Exit – Death:** Firm A exits. *Less than* 70% of its employees moved to the same firm.

(c) **Firm Exit – ID Change:** Firm A exits. *More than* 70% of its workers moved to firm B. Firm B entered in year $t+1$ and *more than* 70% of its workers came from Firm A.

(d) **Firm Exit – Acquisition by New Firm:** Firm A exits. *More than* 70% of its workers moved to firm B. Firm B entered in year $t+1$ but *less than* 70% of its workers came from Firm A.

(e) **Firm Exit – Acquisition by Existing Firm:** Firm A exits. *More than* 70% of its workers moved to firm B. Firm B did not enter in year $t+1$.

6. Using the worker flows to and from each firm, I classify firm entry as follows:

   (a) **Incumbent Firm:** Firm A did not enter.

   (b) **Firm Entry – Natural:** Firm A enters. *Less than* 70% of its employees came from the same firm.

   (c) **Firm Entry – ID Change:** Firm A enters. *More than* 70% of its workers came from the same firm B. Firm B exited in year $t-1$ and *more than* 70% of its workers went to Firm A.

   (d) **Firm Entry – Spinoff from Exiting Firm:** Firm A enters. *More than* 70% of its workers came from firm B. Firm B exited in year $t-1$ but *less than* 70% of its workers went to Firm A.

   (e) **Firm Entry – Spinoff from Continuing Firm:** Firm A enters. *More than* 70% of its workers came from firm B. Firm B did not exit in year $t-1$.

7. If firm A has a **Firm Exit – ID Change** to firm B, the firm ID for B is replaced with A. These replacements are done chronologically so if $A \to B$ and then $B \to C$, then both firms B and C will have their firm IDs replaced with A. Note, I do not replace any firm IDs for acquisitions by new firms or existing firms. The rationale here is that the biggest motivation for changing the firm IDs is to figure out when switching from firm A to firm B would have actually resulted in a new employment contract (which determines eligibility for the noncompete ban). For an acquisition, it is plausible that the employees would need to sign a new employment contract. For the job transition outcomes, I exclude job transitions due to any of the above types of firm exits. So acquisitions will not create spurious job transitions as part of the outcome variables.

H.2 Mapping the ASSD Variables to the Treatment Definition

In this section, I describe how to map the earnings variables and spell-starting dates in the ASSD to the earnings concepts and contracts signing dates specified in the noncompete legislation.

Mapping the Earnings Concepts In the ASSD, I observe two earnings concepts: normal payments, *bezug*, and special payments, *sonderzahlung*. The earnings concepts correspond to different legal earnings concepts that are treated differently by the Social Security Act (*Allgemeines Sozialversicherungsgesetz*). The general distinction between the two types of payments is that *bezug* includes payments that are granted at the frequency of the contribution periods (e.g., at a monthly frequency). *Sonderzahlung* includes payments that are granted at a frequency that is longer than the contribution periods. The primary component of *sonderzahlung* is 13th and 14th monthly payments referred to as Christmas and holiday payments. These components are negotiated by the trade unions but are included in almost all collective bargaining agreements. *Sonderzahlung* also includes profit-sharing payments, anniversary bonuses (e.g., bonuses for working at a company for 25 years), and similar payments. The normal payments then include normal monthly salary payments, overtime payments, hardship allowances, and other payments that are granted at the frequency of the contribution periods.

For the noncompete ban, a different definition of earnings applied. The literal translation of the legislation describing the earnings relevant for the ban is "remuneration due for the last month of employment" (*für den letzten Monat des Dienstverhältnisses gebührende Entgelt*). While this translation suggests only looking at workers' salary in the month before the termination, the legal interpretation is different. First, between 2006-2015, employers interpreted the definition of earnings to include the 13th and 14th monthly payments. In 2015, the legislation was changed such that only 12 months of salary were considered for the ban. Second, some Supreme Court cases after 2006 clarified whether other parts of earnings should be included the determine eligibility. These cases clarified that non-salary payments that were paid on a "regular basis" should be included for determining eligibility. These could include regular commission payments or bonus payments. However, "one-time" payments were not to be included (e.g., bonuses for working at a company for 25 years). Given that Supreme Court cases were required to clarify the interpretation of whether these payments were included, it is unclear whether or not most companies included these payments for determining noncompete eligibility right after the ban. See Theuer (2010) for more details and references about the debate around which payments should be considered when determining noncompete eligibility.

Based on the above descriptions of the ASSD earnings concepts and the earnings concept relevant for the ban, it is not clear whether or not to include *sonderzahlung* when defining individuals' noncompete eligibility. Some parts of *sonderzahlung* were commonly understood to be included when determining eligibility (e.g., the 13th and 14th monthly payments), other parts were legally included but it is unclear how widely understood this was (e.g., regular commission or bonus payments), and other parts were not legally included (e.g., anniversary bonuses payments). Consequently, for the main analysis, I define treatment based only on workers' normal payments but scale these payments up to take into account the 13th and 14th monthly payments that most workers receive.[64] Formally, I define treatment as

$$\text{Annual } \textbf{Normal Payments}_{i,t} \times \frac{1}{12} \times \frac{14}{12} < \frac{17}{30} \times \text{Maximum Monthly Social Security Contribution}_t. \tag{A17}$$

Note, that the average normal payments are only for the firm that the employee is currently employed at. Since I only observe annual payments for each firm, however, there will be some measurement error if workers experience within-year salary changes. For example, if a worker went from earning €1,000/month to €1,200/month within the year this could affect her eligibility for the noncompete ban but I would only observe her *average* monthly earnings over the whole year.

To assess the robustness of this way of defining the earnings concepts, Appendix Figures A7 and A8 present the main results in the paper instead defining treatment based on total earnings (including both normal payments and special payments). Formally, the treatment definition for

[64]Even if a collective bargaining agreement does not specify additional payments, firms may still make payments over 14 months rather than 12 months for tax-related reasons. Some collective bargaining agreements include 15 months of payment (e.g., workers in the banking sector). Consequently, this treatment definition will classify some workers with 15 monthly payments who were legally in the control group as treated. The *total earnings* robustness results presented in Appendix Figures A7 and A8 account for this and would properly classify these workers with 15 months of salary payments.

these robustness checks is

$$\text{Annual } \textbf{Total Payments}_{i,t} \times \frac{1}{12} < \frac{17}{30} \times \text{Maximum Monthly Social Security Contribution}_t. \tag{A18}$$

Mapping Spell Starting Dates The second component of defining treatment is mapping the labor market status data in the ASSD to "contract signing dates" relevant for the noncompete ban. For each employment spell in the ASSD, I define the spell's starting month (i.e., when the worker signed their employment contract) as the first month I see that worker employed at that firm. There are a few reasons why this classification might lead to some measurement error

- I classify employees' monthly employment based on which firm they are employed at on the 15th day of the month. Consequently, the actual starting date may have actually been in the previous month. However, the actual ban cutoff dates were the 16th and 17th of March so this classification works well for separating treated and untreated workers in 2006.
- Workers might sign their employment contracts before they start working at a firm. This would lead to the spell starting date in the ASSD being **later** than the contract signing date.
- Workers may be initially on temporary contracts or prohibitional periods before signing their formal employment contracts. This would lead to the spell starting date in the ASSD being **before** the contract signing date.

The one other difficulty with assigning spells to treatment versus control is that the reform affected spells that started in the middle of 2006. To account for this, I assign spells that started in the first three months of 2006 as having started in 2005 (i.e., control employment spells). To account for the fact that this mechanically changes the composition of employment spells in 2005 and 2006 (i.e., the 2005 cohort will have extra spells that started in the first three months of the year and the 2006 cohort will have no spells that start in the first three months of the year), the specifications with *individual controls* include fixed effects for which month each employment spell started in. These fixed effects are interacted with the treated earnings indicator.

I NACE 08 Industry Codes

The industry codes available in the ASSD correspond to four-digit NACE 08 industry codes. See Austria (2016) for more details about these industry codes. All references to four-digit industry codes in the paper correspond to the four-digit NACE codes. Examples of four-digit industries are *retail sale of telecommunications* (4422), *retail sale of computers* (4421), *book publishing* (5811), and *newspaper publishing* (5813). References to *coarse* industry codes are NACE industry letter groupings (i.e., the NACE sections). The reason for using this grouping rather than one-digit NACE codes is that one-digit NACE codes do not always correspond to the same broad industries (e.g., software publishing is in the one-digit NACE industry 5 while computer programming is in one-digit NACE industry 6 but they are both in `NACE section J - Information and Communications`).

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