The Effect of No-poaching Restrictions on Worker Earnings in Franchised Industries *

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Abstract

We investigate the effect of the Washington State Attorney General’s enforcement campaign against employee no-poach clauses in franchising contracts, which unfolded from 2018 until early 2020. We employ a dataset linking chain-level franchising contract provisions with employer-identified job ads from Burning Glass Technologies (BGT). We use several staggered Difference-in-Difference methodologies to measure the effect of removing franchise no-poach clauses on earnings posted in job ads following settlements reached with the Attorney General. Our preferred specification estimates a 3.3% increase in chain-specific annual earnings following no-poach removal.

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¶Burning Glass Technologies
1 Introduction

Franchise no-poach clauses are provisions of franchising contracts (between franchisors, generally national chains with recognizable consumer brands, and franchisees, local retailers or operators that conduct the business associated with the national brand) prohibiting the franchisee party to the contract from hiring workers currently or recently employed by other franchisees (or the franchisor itself) in the national network. In July 2017, Alan Krueger and Orley Ashenfelter released a working paper (Krueger and Ashenfelter, 2017) that reported that 58% of franchising contracts for 156 of the largest franchise chains contained no-poach provisions. That working paper was covered by the New York Times in September 2017 (Abrams, 2017).

Following that high-profile publicity of franchise no-poaches, the Attorney General of Washington State began an investigation into the prevalence of franchise no-poaches among chains with significant presence in the state and their legality under state and federal antitrust law. That investigation quickly yielded results: starting in July 2018, a total of 235 chains entered into an “Assurance of Discontinuance” (“AOD”) with the AG committing to remove no-poach provisions from future franchising contracts and not to enforce those contained in existing contracts. Through the settlements, chains were required to refrain from using franchise no-poaches going forward in a legally-enforceable manner (i.e., they do not have unilateral discretion to resume their use). Those settlements did not impose retrospective penalties, and the chains did not admit their conduct was illegal. Notably, the AODs bind chains throughout the United States, not only in the state of Washington. The final AOD was signed in February of 2020, and the AG announced the end of the enforcement campaign in June of that year.

Krueger and Ashenfelter’s paper was eventually published in the Journal of Human Resources in 2022 (Krueger and Ashenfelter, 2022), including a final postscript recounting the Washington AG’s enforcement campaign stemming from the earlier draft. That postscript notes that “In principle, because this information provides the information needed for a pre-/post-comparison, it could be used to form the basis for the design of a study intended to determine
what effect, if any, these agreements may have had on worker wage rates or conditions of employment.”

This paper conducts that study. Specifically, we construct a dataset that links the provisions of 530 chain-level standard franchise contracts to employer-identified job ads from Burning Glass Technologies (BGT). The job ad data includes job characteristics like occupation, employer names and variables related to geographic location, and posted salaries for those ads that include salary information. Callaci et al. (2022) describe this “matched dataset” in much greater detail and report summary statistics, as well as comparisons with Occupational Employment and Wages Statistics from the Bureau of Labor Statistics that validate its representativeness of the franchising workforce. This impact evaluation uses a slightly different version of that matched dataset, confined to the period three years before the settlements were reached, and a post-period that extends to the end of the dataset in December 2021. We also use only those franchising chains that either entered into a settlement or can be assigned to one of the two “clean” control groups, as described further below. Finally, we have digitized 530 Franchise Disclosure Documents (FDDs), but there are a further 46 chains that entered into an AOD with the AG that were not among the chains whose FDDs we had digitized in prior work. We include them in the impact evaluation (and thus expand the dataset of job ads to include those chains, drawing on the master BGT job ads file).

We employ a staggered Difference-in-Difference methodology to estimate the effect of removing franchise no-poach provisions on posted salaries. That is a natural methodology in several respects: each chain entered into an AOD with the AG at different times during the enforcement effort, and not all franchising chains among the 530 whose provisions we have digitized either entered into a settlement or had a no-poach provision to begin with, giving rise to two separate control groups: chains that had a no-poach provision in their standard contract and did not enter into an AOD (likely because the chain had no presence in the state of Washington), and chains that did not have a no-poach provision ex ante and therefore did not enter into an AOD with the AG. Both control groups are “clean” in that they never entered
into a settlement. Using two-way fixed effects estimation when treatment timing is staggered across cohorts may produce biased estimates due to treatment effect heterogeneity across treatment cohorts (Goodman-Bacon, 2021; Baker, Larcker and Wang, 2021). Our empirical approach accounts for that.

The only direct precedent for this paper arises from outside the franchising context: Gibson (2021) uses data from Glassdoor to estimate the effect of the Department of Justice’s enforcement campaign against no-poaching agreements among Silicon Valley employers, affecting a very different segment of the workforce but operating through similar mechanisms as franchise no-poach provisions. His findings are notably similar to ours: a 2.4% reduction in earnings for workers subject to a no-poach provision for a single year.

This paper proceeds as follows: Section 2 reports some background about the franchising business model, its use of no-poach restraints, and the history of the Washington AG’s campaign against them. Section 3 explains our methodology for estimating its effect. Section 4 reports our results. And Section 5 discusses their implications for franchising labor markets more broadly.

2 Background

The essence of the franchising business model is that national chains with brands recognizable to consumers either distribute their products or perform the service associated with the brand through a network of affiliated franchisees that are separately incorporated.\footnote{Franchisees can be natural persons, but the point is that they are legally distinct from franchisors.} The contractual relationship between franchisors and franchisees has historically been subject to regulation, albeit of decreasing onerousness in the United States since the 1970s (Callaci, 2021). One regulation that is still in force is that franchisors are obligated to disclose the provisions of the contract to franchisees in advance of their agreeing to it, in the form of a “Franchise Disclosure Document” (FDD), by the Federal Trade Commission’s Franchise Rule. Some states further
require FDDs to be filed and recorded by a state regulatory agency. That forms the source of the data on franchising contracts used in this paper: 530 digitized FDDs filed in Wisconsin pertaining to the year 2015, i.e. prior to the Washington AG’s enforcement campaign.²

Substantive regulation of the franchising relationship has generally focused on the allocation of decision-making power between franchisors and franchisees in the contract and its implications for competition in the output market, as well as the recourse available to franchisors to enforce franchisee compliance (Blair and Lafontaine, 2005). For example, the franchisee may have local product market power to increase retail price above wholesale price, but the franchisor has an interest in maximizing sales and so may impose maximum resale price maintenance, to the benefit of both itself and consumers (Spengler, 1950).

That those contracts could affect the balance of power in the labor market is a relatively novel source of academic and policy interest. As mentioned earlier, Krueger and Ashenfelter (2017) found that 58% of franchising contracts contain no-poach clauses restraining franchisees from hiring workers currently or recently employed by franchisees (or the franchisor) in the same chain. We find similar prevalence: 59.2% of the chains in our data (530 chains versus the 158 in Krueger and Ashenfelter (2017)), corresponding to 60.1% of the job ads posted by those chains (Callaci et al., 2022).

The legality of these provisions has been contested since they came to light. The Washington AG, and several private plaintiffs, took the position that multiple employers agreeing not to hire workers employed by one another, or other franchisees in the same network, constituted naked market division and was hence per se illegal, that is to say, the mere fact of the agreement was sufficient to adjudicate its illegality. In weighing in on a private antitrust action, the Department of Justice took the view that it was a vertical restraint like all the others in a franchising relationship and hence subject to antitrust’s Rule of Reason, meaning first of all that antitrust liability would require that the parties to the agreement possess market

²The criteria for inclusion is that the chains had to have at least 80 locations nationally, and had to have filed their FDD in Wisconsin, indicating at least some presence in that state. See Callaci (2021b) and Callaci et al. (2022) for further details about the construction of the dataset.
power in a relevant antitrust market, and that anti-competitive harm may be traded off against pro-competitive efficiencies (e.g., in a better-trained workforce), or alternatively, that the anti-competitive effect of the restraint is ancillary to a legitimate business purpose, in this case, providing consumers with the standard commercial experience associated with the franchisor’s brand.\textsuperscript{3}

Blair and Lafontaine (2005) emphasize that franchisors generally do not have market power over franchisees, even if the franchisee has made relationship- or chain-specific investments that subordinate him to the franchisor, because alternative franchising relationships are on offer from competing chains.\textsuperscript{4} But those authors ignore the labor market, where both franchisors and franchisees may possess market power. We show in Callaci et al. (2022) that computed at the chain level, franchising labor markets defined by either occupation or industry and commuting zone are in general highly concentrated. Thus, whether any no-poach provision is construed as a horizontal or vertical agreement, those entering into it likely possess a high degree of labor market power.\textsuperscript{5}

In embarking on the no-poach enforcement campaign, the Washington AG took the view that franchise no-poaches are horizontal agreements not to compete in the labor market. The settlements were reached before that matter was fully adjudicated by the litigation. Private litigation did proceed on the basis that the agreements were vertical (hence, establishing that the franchisor-defendants had market power in the labor market was part of the plaintiff’s burden). In both of the cases known to the authors, certification of the plaintiff class failed. Recently, in an individual action, Deslandes \textit{v.} McDonalds, the judge ruled for the defendant on the grounds that it did not possess market power and therefore the franchise no-poach

\textsuperscript{3}Delrahim et al. (2019).

\textsuperscript{4}This ignores the fact that many chains bind franchisees with explicit non-compete clauses, as well as what are the equivalent of noncompete clauses, namely the franchisor’s right to purchase the franchisee’s assets at the conclusion of a contractual term (Callaci et al., 2022).

\textsuperscript{5}Azar, Marinescu and Steinbaum (2019) show that labor market concentration covaries negatively with residual labor supply elasticity at the market level, validating the interpretation of concentration in local labor markets as indicative of the absence of competition.
This study makes no assumption about the labor market power of franchisors or franchisees, nor do we attempt to define labor markets and measure employer concentration therein. We do not view that as necessary to establish their competitive effect. Instead we directly evaluate the impact of removing the restraints in question on salaries posted by franchising chains subject to the settlements. However, if there is an effect on earnings in franchising labor markets of removing such a provision, that could be taken as direct evidence that the chains possess labor market power, as they evidently did not take market wages as given, as is assumed in competitive models of the labor market, while the provisions were in effect.

The timing details of the enforcement campaign are as follows: the AG’s investigation began shortly following the release of Krueger and Ashenfelter (2017) and its coverage in the New York Times in the autumn of 2017. The first settlements to the AG’s lawsuits were reached in July 2018 with seven fast-food chains. Over the months after July 2018, the AG secured AODs from many chains in the fast food industry, and thereafter the investigation proceeded to franchising chains in other industries. The final settlements were reached in February 2020. Only one chain, Jersey Mike’s, defended its conduct in state court in Washington and filed a Motion to Dismiss the AG’s lawsuit. In rejecting the Motion to Dismiss, that court left intact the AG’s theory that the no-poach provision amounted to a horizontal agreement and hence merited per se treatment. Jersey Mike’s settled its suit with an AOD shortly thereafter.

The AODs impose an enforceable commitment on each chain not to enforce its franchise no-poach provision going forward, to remove it from future franchising contracts as they are renewed (or originated), and to notify franchisees that the no-poach is no longer binding on them. No particular notice to workers was required, and the signatories did not admit liability or pay retrospective damages. Importantly, in many cases the signatory attests that it had al-

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6 Alonso (2022).
7 Section 4 of Callaci et al. (2022) reports indirect evidence of the competitive effect of franchise no-poach provisions.
8 This narrative, and the paper as a whole, relies on Rao (2020) for details of the AG’s enforcement campaign.
ready removed the no-poach provision prior to entering into the AOD. In the majority of cases where the AOD includes such a stipulation, the no-poach removal seemingly occurred after the AG’s larger investigation into franchise no-poaches commenced and may have been an effort to evade retrospective liability.⁹ Given that, there may be some ambiguity about when exactly those chains were really treated. In the following section, we perform the impact evaluation using the date of the AOD. Appendix A reports alternative specifications with different treatment dates, either the date the AOD says the investigation into the signatory chain began, or a combination of the two in which the chains which attest to having voluntarily removed their no-poach prior to the AOD date are treated on the investigation start date and the remaining chains are treated on the date of the AOD. In three cases, the AOD says the no-poach provision was voluntarily removed prior to the investigation start date, in 2016 or 2017. Those chains are left out of the evaluation entirely.

3 Empirical Approach

This evaluation makes use of two different but related treatment-effect-heterogeneity-robust estimators for staggered Difference-in-Difference research designs: the ones proposed by Borusyak, Jaravel and Spiess (2022) and Sun and Abraham (2021). Throughout, the treatment is a legally-enforceable settlement not to make use of franchise no-poach provisions anywhere in the United States, which operates at the level of the national franchising chain. The Washington AG reached such settlements with 230 chains, the vast majority of which appear in our dataset. Some chains that appear in our dataset are excluded from the evaluation for reasons given below, leaving a total treatment group of 185 chains. The outcome of interest is the natural log of the real annual salary posted in the job ads linked to a franchising chain. BGT converts job ads that report an hourly wage into an annual salary, assuming year-round, full-time work. Hence, we do not observe wages and annual earnings separately, nor do we adjust annual salaries for

⁹The legal significance of the AOD in those cases is that the chains that had already unilaterally removed the no-poach lose the option to unilaterally re-impose it.
hours worked or part-time status (which we do not observe).

As mentioned above, the matched dataset enables the construction of two different clean control groups: chains that had a no-poach provision in place and never entered into a settlement, likely those with no presence in the state of Washington since the AG investigated all franchise chains with a presence in the state, and chains that did not have a no-poach provision in place and never entered into a settlement with the Washington AG.\(^{10}\)

Table 1 reports summary statistics for the treatment group and each clean control group. The mean and median posted salaries are similar across the three. The control group consisting of chains that did not ever utilize a no-poach is probably disproportionately drawn from the Traveler Accommodation industry, since that industry is both large in the matched dataset and few chains in it were found to have a no-poach provision in their FDD (Callaci et al., 2022). But notwithstanding that, large, service-sector chains covering a range of industries and labor markets appear in all three groups. Figure 1 plots the total number of job ads in the evaluation sample, by month. There is a large increase starting in early 2018, due to the increasing share of BGT job ads that report salary information starting then. That is due to the introduction of new job boards with a higher prevalence of including such information than other scraped job posting sources that were already sources for the BGT job ads data. This fact means that the standard errors for the treatment effect in our pre-period are larger than in the post-period, but it does not appear to affect the treatment coefficient estimates.

Throughout, our specifications include fixed effects for calendar year-quarter, the 6-digit SOC occupation in which the job ad is posted, the commuting zone in which the job ad is posted, and the franchising chain with which the job ad is associated. We also include as a covariate the state-level minimum wage, making use of Vaghul and Zipperer (2021)’s publicly-

\(^{10}\)Nearly all of the chains that did enter into a settlement with the AG and which appear in our matched dataset had some form of no-poach provision in place in the FDD/contracts data covering 2015, although the specifics of each provision, e.g. what type of employees it covers, varies across chains. We exclude seven chains that entered into an AOD for which we code them as not having had a no-poach. Other than the possibility that they imposed one between 2015 and the AG’s investigation, that is likely because our criteria exclude chains whose no-poach only bars franchisees from hiring employees of the franchisor, as opposed to other franchisees.
available daily-frequency dataset of state and local minimum wages. Our treatment effects are therefore net of chain-level (and job-ad-level) heterogeneity along all of these dimensions. Notably, the inclusion of franchise-chain-level fixed effects means our results are net of chain-level pay policies that are invariant over time.

We use three years before the treatment date as the pre-period for each cohort, with cohorts being determined by the year-quarter in each the treatment happens. The quarterly frequency means that there are a total of seven treatment cohorts in our specifications (corresponding to each quarter between the third quarter of 2018 and the first quarter of 2020), because chains that entered into an AOD at any time during a quarter are coded as having been treated in that quarter. The post-period extends through the end of 2021, thus corresponding to three-and-a-half years for the earliest-treated cohorts, with no cohort having a post-period of less than two years. We separately report results for each clean control group and for the pooled controls from both.

4 Results

We first report results using the Borusyak, Jaravel and Spiess (2022) estimator, which produces a single treatment effect coefficient estimate as well as a set of coefficients that test for the presence or absence of pre-trends. The premise behind the Borusyak, Jaravel and Spiess (2022) estimator is that the clean controls can be used to estimate counter-factual potential outcomes for the treatment group, without having those counter-factual outcomes be influenced by the realized outcomes of earlier- and later-treated units. The estimated treatment effect is then the difference between those potential outcomes solely estimated from the clean control groups and the actual path of the treated units.

Results from that procedure are reported in Table 2. The table has three columns, first using both sets of clean controls to estimate potential outcomes, then for each control group separately. The coefficient estimate in the first column signifies that entering into an AOD
causes an increase of approximately 3.3% in annual salaries in posted job ads. The estimate is 2.7% when the control group is confined to chains that had a no-poach provision and never removed it, and 3.7% when the control group is only those chains that never had a no-poach provision and thus did not enter into an AOD. The coefficient estimate on the local minimum wage is much larger in magnitude throughout, signifying that minimum wages are binding in this low-wage workforce. All of the pre-trend test coefficients for the 6 quarters preceding the treatment period are statistically indistinguishable from zero.

We then report results from the Sun and Abraham (2021) estimator, which produces a time-varying treatment effect estimate by separately estimating the period-relative-to-treatment coefficients across cohorts, where treatment cohorts are determined by the quarterly date each chain in the treatment group entered into an AOD. Each cohort-by-period coefficient is estimated using variation between the treatment cohort and the clean control group in calendar time. The period-relative-to-treatment coefficients estimated for the full sample are then averaged across cohort-by-period coefficient estimates, where the weights are determined by the relative number of observations in each cohort-by-period. The omitted period is always the quarter just prior to treatment; thus, coefficient estimates are the difference between log real annual salaries in the given quarter relative to the quarter just before treatment.

Those time-varying coefficient estimates are plotted in figures 2-4. Individually, most of the quarter-relative-to-treatment coefficients for the post-period display no temporal pattern suggestive of either a lagged or a transitory effect post-treatment; though they are either only marginally significant at the 5% level or statistically indistinguishable from zero, the point estimates themselves are consistent with the overall treatment effect estimated in Table 2. The coefficients estimated for the control group consisting of chains that never had a no-poach provision are slightly larger in magnitude than the coefficients for the control group consisting of chains that never removed their no-poach provision, as was suggested by the larger pooled treatment effect estimate from the Borusyak, Jaravel and Spiess (2022) estimator.

Appendix A reports on two additional specifications in which the treatment date is var-
ied, given that some AODs report the chain had removed its no-poach provision prior to the date of the agreement. Those coefficient estimates indicate that if the treatment is taken to be the start of the AG’s investigation, as opposed to the AOD date, then the treatment effect is negligible. The reason why we prefer the specifications using the AOD date as the treatment date is that there’s nothing binding about the prior voluntary removal of no-poach provisions. In the context of the AG’s investigation, it appears to be an attempt to avoid liability (since they post-date the overall franchise no-poach investigation), rather than reflecting any policy change or underlying chain-level heterogeneity. More concretely, there is no assurance that franchisors who voluntarily removed their no-poaches informed franchisees of that fact, hence it’s not surprising that it apparently had little effect on salaries in posted job vacancies. Nor is there any verification that no-poaches were, in fact, removed, solely the stipulation contained in the AOD.

5 Discussion and Conclusion

Following the suggestion of Krueger and Ashenfelter (2022), we perform an impact evaluation of the Washington State Attorney General’s franchise no-poach enforcement campaign, which secured nationally-binding and legally-enforceable agreements from most national franchise chains that had previously made use of no-poach provisions in their standard franchising contract not to make use of those provisions going forward. Using a dataset constructed from digitized Franchise Disclosure Documents linked to employer-identified job ads from Burning Glass Technologies, we estimate the effect of entering into an AOD on posted annual salaries. Our preferred specification indicates that the enforcement campaign increased annual earnings by 3.3%. For a worker with median earnings of $26,133 in the treatment group, that corresponds to an increase of $862.39.

It should be noted that the methodology we employ, comparing chains that entered into an AOD to chains that did not, would underestimate the impact of the enforcement campaign on
pay in franchising labor markets broadly if the effect was to increase competition throughout
the sector, not only at chains that entered into AODs. We document a pay increase at treated
chains relative to untreated ones, but the chains that are clean controls for the purposes of a
Difference-in-Difference estimation may nonetheless have been affected by a broader change
in labor market competition brought about by the large-scale disappearance of franchise no-
poaches.

The settlements the AG reached did not obtain retrospective damages for the victims, and
subsequent class action litigation against at least two chains in which plaintiffs sought such
damages did not move forward after classes failed to be certified. In June 2022, one case was
dismissed on the grounds that McDonalds does not possess labor market power and hence
its no-poaching provision could not have been anti-competitive. Nonetheless, our findings
suggest that franchise no-poach provisions are costly for workers in franchised industries, and
that the antitrust enforcement against such restraints that has happened to date has therefore
benefited them. Moreover, only chains that had ever used no-poaches, and that had a presence
in the state of Washington, were investigated and subsequently entered into an AOD. This
means that franchise no-poach provisions remain legal for other franchising chains as of this
writing, and no chain has faced penalties for using them in the past.

Scholarship about the franchising sector generally presumes that its labor markets are com-
petitive, given the large number of low-wage, service-sector employers who operate within
it. Nonetheless, research that finds that minimum wage increases increases do not negatively
impact employment points in the direction of monopsony, in which employers trade off wages
against labor turnover (Card, 2022). The findings in this paper could be interpreted as fur-
ther evidence against perfect competition in low-wage service sector labor markets, given that
no-poach provisions reduce the number of outside options available to workers, and limit the
internal job ladders that would otherwise operate in large national chains, since workers have
to switch chains in order to switch jobs. Research has shown that the large-firm pay premium
is in decline, especially in sectors that employ low-wage workers (Even and MacPherson, 2012;
Bloom et al., 2018). The prevalence of franchise no-poaches, and their earnings effect, may form part of the reason why: chains are getting better at segmenting workers away from profits, the interpretation advanced by Weil (2014).
References


Table 1. Summary statistics for the no-poach impact evaluation. This table reports summary statistics for the treatment group (chains that entered into AODs with the Washington State AG) and both sets of clean controls (chains that had a no-poach provision and did not entered into an AOD, and chains that did not have a no-poach provision) described in Section 3.

<table>
<thead>
<tr>
<th></th>
<th>Treated</th>
<th>No-poach never-remove</th>
<th>Never no-poach</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of chains</td>
<td>185</td>
<td>165</td>
<td>205</td>
</tr>
<tr>
<td>Number of job ads (total)</td>
<td>671,791</td>
<td>219,715</td>
<td>578,075</td>
</tr>
<tr>
<td>Avg. number of job ads per chain</td>
<td>3631</td>
<td>1332</td>
<td>2820</td>
</tr>
<tr>
<td>Median salary (2015 USD)</td>
<td>$26,133</td>
<td>$24,928</td>
<td>$24,715</td>
</tr>
<tr>
<td>Mean salary (2015 USD)</td>
<td>$31,567</td>
<td>$30,293</td>
<td>$31,402</td>
</tr>
<tr>
<td>Top 3 chains by number of job ads</td>
<td>Domino’s Pizza Hut Home Instead</td>
<td>Chili’s BrightStar Care Marco’s Pizza</td>
<td>Hilton Hotels Chick-Fil-A Holiday Inn</td>
</tr>
</tbody>
</table>
Figure 1. **Total number of job ads in the sample, by month.** This plots the total number of job ads included in the sample (treatment group plus both clean control groups) by month during the evaluation period. The large increase starting early in 2018 coincides with a rising share of BGT job ads that include a posted salary.
Table 2. **Borusyak, Jaravel and Spiess (2022) overall treatment effect estimates.** This table reports a single treatment effect estimate ($\tau$) from entering into an AOD for three specifications: the pooled control group and each control group individually. Regression restricted to BGT postings that included salary information and starting 36 months before the settlement date associated with each cohort.

<table>
<thead>
<tr>
<th></th>
<th>(1) Both clean controls</th>
<th>(2) No-poach non-remove</th>
<th>(3) Never no-poach</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Treatment Effect</strong></td>
<td>0.033* (0.017)</td>
<td>0.027* (0.017)</td>
<td>0.037* (0.019)</td>
</tr>
<tr>
<td><strong>pre1</strong></td>
<td>-0.007 (0.039)</td>
<td>-0.043 (0.045)</td>
<td>-0.007 (0.042)</td>
</tr>
<tr>
<td><strong>pre2</strong></td>
<td>0.018 (0.037)</td>
<td>-0.017 (0.040)</td>
<td>0.018 (0.039)</td>
</tr>
<tr>
<td><strong>pre3</strong></td>
<td>0.019 (0.034)</td>
<td>-0.009 (0.036)</td>
<td>0.016 (0.037)</td>
</tr>
<tr>
<td><strong>pre4</strong></td>
<td>0.009 (0.031)</td>
<td>-0.012 (0.034)</td>
<td>0.003 (0.033)</td>
</tr>
<tr>
<td><strong>pre5</strong></td>
<td>0.007 (0.031)</td>
<td>-0.012 (0.033)</td>
<td>-0.001 (0.032)</td>
</tr>
<tr>
<td><strong>pre6</strong></td>
<td>0.033 (0.026)</td>
<td>0.020 (0.026)</td>
<td>0.025 (0.026)</td>
</tr>
<tr>
<td><strong>Ln(Minimum wage)</strong></td>
<td>0.204*** (0.017)</td>
<td>0.181*** (0.028)</td>
<td>0.233*** (0.019)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>1,434,907</td>
<td>872,874</td>
<td>1,220,004</td>
</tr>
<tr>
<td><strong>Year-quarter FEs</strong></td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td><strong>CZ FEs</strong></td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td><strong>SOC-6d FEs</strong></td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td><strong>Franchise FEs</strong></td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

*** p<0.01; ** p<0.05; * p<0.1
Note: Robust standard errors clustered at the franchise level.
Figure 2. **Sun and Abraham (2021)** time-varying coefficient estimates, combined control groups. This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.
Figure 3. Sun and Abraham (2021) time-varying coefficient estimates where the control group is no-poach chains that did not enter into an AOD. This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.
Figure 4. Sun and Abraham (2021) time-varying coefficient estimates where the control group is chains that never had a no-poach provision. This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.
A Alternative Treatment Dates

About 15% the AODs stipulate that the chains entering into them had already removed their no-poach provisions prior to the date of the agreement. This naturally raises the possibility that the date of the AOD is not the date on which those chains were treated by the enforced removal of their no-poach provision. We thus report two other sets of specifications in which the treatment date is instead, first, the date on which the AG’s investigation began for all chains, and second, the date on which the AG’s investigation began for just those chains whose AODs stipulate an earlier, voluntary removal. The rest of the chains are treated on the date of the AOD, as in the main text.

For each of these two specifications, we report both Borusyak, Jaravel and Spiess (2022) and Sun and Abraham (2021) estimates, as in the main specification that codes the date of the AOD as the treatment date. These coefficient estimates for the treatment date as investigation start date specification are for the most part insignificant, though positive when the control group is the chains that never had a no-poach. The coefficient estimates for the mixed specification in which the internal-removal chains are treated on the investigation start date and the remaining chains are treated on the date of the AOD are also statistically insignificant, but generally positive in magnitude, which is not surprising since they are, in effect, a mixture of the AOD-date specification and the investigation-start-date specification.
Table A.1. Borusyak, Jaravel and Spiess (2022) overall treatment effect estimates, treatment date is investigation start date. This table reports a single treatment effect estimate ($\tau$) from entering into an AOD for three specifications: the pooled control group and each control group individually. Regression restricted to BGT postings that included salary information and starting 36 months before the settlement date associated with each cohort.

<table>
<thead>
<tr>
<th></th>
<th>(1) Both clean controls</th>
<th>(2) No-poach non-remove</th>
<th>(3) Never no-poach</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Effect</td>
<td>0.012 (0.028)</td>
<td>-0.006 (0.028)</td>
<td>0.015 (0.030)</td>
</tr>
<tr>
<td>pre1</td>
<td>0.037 (0.028)</td>
<td>0.004 (0.039)</td>
<td>0.036 (0.030)</td>
</tr>
<tr>
<td>pre2</td>
<td>0.026 (0.028)</td>
<td>-0.006 (0.036)</td>
<td>0.025 (0.030)</td>
</tr>
<tr>
<td>pre3</td>
<td>0.023 (0.026)</td>
<td>-0.002 (0.035)</td>
<td>0.016 (0.027)</td>
</tr>
<tr>
<td>pre4</td>
<td>0.031 (0.024)</td>
<td>0.021 (0.034)</td>
<td>0.023 (0.025)</td>
</tr>
<tr>
<td>pre5</td>
<td>0.047* (0.025)</td>
<td>0.028 (0.032)</td>
<td>0.039 (0.026)</td>
</tr>
<tr>
<td>pre6</td>
<td>0.016 (0.027)</td>
<td>0.012 (0.027)</td>
<td>0.019 (0.026)</td>
</tr>
<tr>
<td>Ln(Minimum wage)</td>
<td>0.214*** (0.019)</td>
<td>0.190*** (0.029)</td>
<td>0.246*** (0.021)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Observations</th>
<th>Year-quarter FEs</th>
<th>CZ FE</th>
<th>SOC-6d FE</th>
<th>Franchise FE</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1,435,839</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Ln(Annual Salary)</td>
<td>864,195</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td></td>
<td>1,219,674</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

*** p<0.01; ** p<0.05; * p<0.1
Note: Robust standard errors clustered at the franchise level.
Figure A.1. Sun and Abraham (2021) time-varying coefficient estimates, combined control groups, treatment date on investigation start date This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.
Figure A.2. Sun and Abraham (2021) time-varying coefficient estimates where the control group is no-poach chains that did not enter into an AOD, treatment date on investigation start date. This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.
Figure A.3. Sun and Abraham (2021) time-varying coefficient estimates where the control group is chains that never had a no-poach provision, treatment date on investigation start date. This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.
Table A.2. Borusyak, Jaravel and Spiess (2022) overall treatment effect estimates, treatment date is investigation start date for internal-removal chains and AOD date for all others. This table reports a single treatment effect estimate ($\tau$) from entering into an AOD for three specifications: the pooled control group and each control group individually. Regression restricted to BGT postings that included salary information and starting 36 months before the settlement date associated with each cohort.

<table>
<thead>
<tr>
<th>Dependent Variable: Ln(Annual Salary)</th>
<th>(1) Both clean controls</th>
<th>(2) No-poach non-remove</th>
<th>(3) Never no-poach</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Effect</td>
<td>0.035* (0.020)</td>
<td>0.026 (0.020)</td>
<td>0.040* (0.022)</td>
</tr>
<tr>
<td>pre1</td>
<td>-0.007 (0.034)</td>
<td>-0.047 (0.041)</td>
<td>-0.010 (0.036)</td>
</tr>
<tr>
<td>pre2</td>
<td>0.023 (0.032)</td>
<td>-0.018 (0.038)</td>
<td>0.022 (0.034)</td>
</tr>
<tr>
<td>pre3</td>
<td>0.015 (0.029)</td>
<td>-0.016 (0.034)</td>
<td>0.010 (0.031)</td>
</tr>
<tr>
<td>pre4</td>
<td>0.008 (0.027)</td>
<td>-0.015 (0.033)</td>
<td>-0.000 (0.028)</td>
</tr>
<tr>
<td>pre5</td>
<td>0.041 (0.026)</td>
<td>0.017 (0.030)</td>
<td>0.032 (0.027)</td>
</tr>
<tr>
<td>pre6</td>
<td>0.017 (0.023)</td>
<td>0.006 (0.025)</td>
<td>0.008 (0.023)</td>
</tr>
<tr>
<td>Ln(Minimum wage)</td>
<td>0.213*** (0.018)</td>
<td>0.188*** (0.028)</td>
<td>0.244*** (0.020)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,444,343</td>
<td>873,472</td>
<td>1,228,180</td>
</tr>
<tr>
<td>Year-quarter FEs</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>CZ FEs</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>SOC-6d FEs</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Franchise FEs</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

*** p<0.01; ** p<0.05; * p<0.1

Note: Robust standard errors clustered at the franchise level.
Figure A.4. *Sun and Abraham (2021)* time-varying coefficient estimates, combined control groups, treatment date on investigation start date for internal removal chains and AOD date for all others. This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.
Figure A.5. Sun and Abraham (2021) time-varying coefficient estimates where the control group is no-poach chains that did not enter into an AOD, treatment date on investigation start date for internal removal chains and AOD date for all others. This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.
Figure A.6. Sun and Abraham (2021) time-varying coefficient estimates where the control group is chains that never had a no-poach provision, treatment date on investigation start date for internal removal chains and AOD date for all others. This figure plots the time-varying treatment effects by quarter, with a three-year window around the treatment date.