The Effect of Franchise No-poaching Restrictions on Worker Earnings*

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Abstract

We evaluate the impact of the Washington State Attorney General's enforcement campaign against employee no-poaching clauses in franchising contracts, which unfolded from 2018 until early 2020. We employ a staggered Difference-in-Differences research design and microdata from Burning Glass Technologies job vacancies and salary reports from Glassdoor to document the effect of the enforcement campaign on pay at franchising chains. Our preferred specification estimates a 3.6% increase in chain-specific annual pay according to the job vacancy data and a 1.3% increase in chain-specific reported salaries.

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1 Introduction

Franchise no-poach clauses are provisions of the standard 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, which prohibit 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 239 chains entered into an "Assurance of Discontinuance" ("AOD") with the attorney general 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. 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-/postcomparison, 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 use employer-identified job ads from Burning Glass Technologies (BGT) and salary reports from Glassdoor (GD) (referred to collectively as the microdata in what follows) to estimate the change in pay for workers at franchising chains that entered into an AOD, relative to two different control groups: a "full sample" that consists of microdata from all industries in which the treated chains hire, and a "matched sample" that includes only microdata for franchising chains whose standard contracts and Franchise Disclosure Documents we have digitized and analyzed in other work (Callaci et al., 2022). In addition to employer names, the microdata include job characteristics like occupation, variables related to geographic location, wages (for a subset of observations, depending on whether the wage is reported in the underlying source data), and whether that wage is paid on an annual, hourly, or any other basis.

We employ a staggered Difference-in-Differences design to estimate the effect of removing franchise no-poach provisions on pay. The setting lends itself to this approach in several respects: each chain entered into an AOD with the AG at different times during the enforcement effort, and not all franchising chains (and certainly not all employers) either entered into a settlement or had a no-poach provision to begin with. However, 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.

In our baseline specification using the full sample and the BGT job ad microdata, our overall treatment effect estimate is that entering into an AOD caused pay to increase by 3.6%, relative to the control group consisting of all the microdata from industries in which the treated franchising chains were active. The equivalent finding from the GD salary reports is 1.6%. Using the matched sample, we estimate a smaller overall treatment effect of 2.6% in the BGT microdata, and a null effect using the GD microdata. We interpret these findings below.

The only direct precedent for this paper arises from outside the franchising context: Gibson (2022) uses data from Glassdoor and the Department of Justice's enforcement campaign against no-poaching agreements among Silicon Valley employers to estimate the effect of employer no-poach provisions on worker earnings. In that case, the existence and duration of employer no-poaches was revealed in the legal proceedings, permitting an empirical strategy that relies on variation in no-poach "exposure" across different employers, and therefore across jobs, before and after the enforcement action, amounting to a range of treatment intensity over time for treated workers. Our empirical strategy is essentially the discrete analog of that, with the addition of staggered treatment (whereas in the Silicon Valley case, there was only one settlement removing the no-poaches agreed to by all the defendants). That paper's findings are notably similar to ours: a 2.5% reduction in earnings for workers subject to a no-poach provision for a single year.

One step removed from the evaluation of employer no-poaches (agreements between employers not to hire one another's workers) are evaluations of noncompetes (agreements between employers and workers restricting the worker from working for the employer's competitors after the current employment terminates). Lipsitz and Starr (2022) evaluate the 2008 ban on noncompetes for hourly workers in Oregon, concluding that the ban increases wages by 2-3%. That works out to a much larger effect on the subset of workers bound by noncompetes absent the ban, although one effect of the ban is likely to increase labor market competition throughout low-wage labor markets, meaning the effect spills over to workers who are not bound by noncompetes.

Beyond evaluations of employer no-poaches, noncompetes, and their removal, this paper contributes to a growing literature documenting and quantifying employer power in labor markets arising from market structure (Azar, Marinescu and Steinbaum, 2022; Benmelech, Bergman and Kim, 2022; Rinz, 2022; Qiu and Sojourner, 2022; Thoresson, 2021), mergers (Prager and Schmitt, 2021; Arnold, 2021; Guanziroli, 2022), employer conduct (Starr, Prescott and Bishara, 2021; Rothstein and Starr, 2021; Balasubramanian et al., 2022), increased prevalence of firms with low-wage business models (Bloom et al., 2018; Wiltshire, 2022), frictions to worker mobility (Schubert, Stansbury and Taska, 2022), gendered assignment of roles in the labor market and the household (Le Barbanchon, Rathelot and Roulet, 2021), and likely many other causes. This gives rise to wage-setting discretion on the part of employers (Manning, 2003, 2011) and thence to wage markdowns below the Marginal Product of Labor (Yeh, Macaluso and Hershbein, 2022; Azar, Berry and Marinescu, 2022).[1]

This paper proceeds as follows: Section 2 reports 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 introduces the data and explains our methodology for estimating the effect of the enforcement campaign. Section 4 reports our results. Section 5 discusses their implications for labor market competition, law and policy related to franchising, and labor economics more broadly. Section 6 concludes.

2 Background

The essence of the franchising business model is that national chains with brands and trademarks 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.[2] 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*a*). One regulation that remains 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 chain-level franchising contracts used in this paper: 530

[1] Sokolova and Sorensen (2021) conduct a meta-analysis of this literature.

[2] Franchisees can be natural persons, but the point is that they are legally separate from franchisors.

digitized FDDs filed in Wisconsin pertaining to the year 2015, i.e. prior to the Washington AG's enforcement campaign.[3]

Substantive regulation of the franchising relationship (as opposed to the disclosure-only regime currently in place at the federal level under the Franchise Rule) historically 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 may have an interest in maximizing sales and customer loyalty in a national market and so may impose maximum resale price maintenance, to the benefit of both itself and consumers (Spengler, 1950).

That franchising 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 contained no-poach clauses restraining franchisees from hiring workers currently or recently employed by franchisees (or the franchisor) in the same chain prior to the Washington AG's enforcement. 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).

Krueger and Ashenfelter (2022) propose two different (but related) mechanisms whereby franchise no-poach provisions would diminish labor market competition, shifting market power to employers. First, given a static monopsony model of labor market competition, they increase effective employer concentration, since other franchisees in the same franchise chain are removed as a source of outside job offers for incumbent workers at any one chain that has a franchise no-poach in place. Fewer outside employers corresponds to greater leeway to reduce wages below the marginal product of labor, or alternatively, in a wage bargaining model, they

[3] The 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 (2021*b*) and Callaci et al. (2022) for further details about the FDD data.

reduce the worker's bargaining power by reducing the threat point. Second, no-poaches depress the wage-turnover tradeoff faced by a given employer vis a vis its workforce in a dynamic monopsony model: the no-poach provides a way for employers to reduce labor turnover, holding constant the wage they choose to pay (alternatively: if employers pay low wages and face high turnover, they can reduce the latter by instigating a no-poach provision). That in turn permits employers to enlarge the wedge between what workers produce and what they earn. This paper does not formally test either mechanism to the exclusion of the other. But its findings, that entering into a legally-binding commitment not to make use of franchise no-poaches leads to an increase in chain-specific pay, should be taken as confirming the anti-competitive effect of franchise no-poaches, as a plausibly-exogenous increase in labor market competition caused wages to increase.

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 power in a relevant antitrust market, and second, 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.[4]

The timing details of the AG's enforcement campaign are as follows: the 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 investigations presumably consisted of an examination of the FDDs filed with the state of Washington to see whether they contained no-poaching language. 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.[5]

The AODs impose an enforceable commitment on each chain not to enforce existing franchise no-poach provisions going forward, to remove those provisions from future franchising contracts as they are renewed (or originated), and to notify affiliated 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. The fact that workers were not informed of the enforcement campaign or the AOD directly colors our interpretation of our findings, as further addressed in section 4.

[4] Delrahim et al. (2019). It's important to draw a distinction between three different possible provisions of a franchising contract.

1. Franchise no-poaches: franchisees are obligated not to hire workers from one another.
2. Franchisee-worker noncompetes: workers for a franchisee are restrained from working for a different franchisee once their employment ends. Some franchisor-franchisee contracts mandate that franchisees impose such noncompetes on their workers.
3. Franchisor-franchisee noncompetes: franchisees are restrained from affiliating with a competing franchisor after the conclusion of the franchise relationship.

Regardless of whether a franchise no-poach is interpreted as horizontal or vertical, it is not a noncompete, which binds workers (or franchisees) from working for someone else. No-poaches bind would-be employers.

[5] This narrative, and the paper as a whole, relies on Rao (2020) for details of the AG's enforcement campaign.

Contemporaneous with the AG's enforcement campaign, private litigation seeking retrospective damages proceeded 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 v. McDonalds*, the judge ruled for the defendant on the grounds that it did not possess market power and therefore the franchise no-poach provision could not have been anti-competitive.[6] Hence, to date, any wage-suppressing effects of franchise no-poaches have not been compensated. Moreover, franchise chains that were not investigated and/or did not enter into an AOD retain the discretion to make use of such provisions.

3 Empirical Approach

The timing of the enforcement campaign and the conclusion of each chain-specific investigation with an AOD provide the context for our staggered Difference-in-Differences methodology. The AODs were reached starting in July 2018 and continuing through February 2020. We estimate the change in pay that occurred for a given franchise chain after it entered into an AOD, relative to employers that did not enter into an AOD, net of controls for occupation, geography, employer, and calendar time. Table 1 lists all the treated franchise chains and their corresponding AOD dates.

We employ two different control groups. In the full sample, the control group consists of all employers who hired or employed at least one worker in an industry in which the treated chains were active. In the matched sample, the control group is all the franchising chains that did not enter into an AOD. The matched sample is so called because it links the microdata to franchise-chain-level digitized FDD data (which is how we identify franchising chain employers and hence franchising jobs in the microdata). The FDD data includes a variable that

[6] Alonso (2022).

indicates whether a chain had a no-poach provision in its franchise contract prior to the enforcement campaign. Hence, we can distinguish chains that did not enter into an AOD based on whether they had a no-poach provision in place. There are two categories of chains that comprise the control group for the matched sample, because they were never treated by entering into an AOD. First, chains that had a no-poach prior to the enforcement campaign and did not enter into an AOD, likely because they had little or no presence in the state of Washington and thus were not subjects of the AG's investigation. Second, chains that did not have a no-poach in place and never entered into an AOD, either because the AG's investigation found that out or because those chain were also never investigated due to not having a presence in Washington.

3.1 Summary Statistics

Table 2 reports summary statistics for the full sample and the matched sample in the Burning Glass job ads microdata. The BGT dataset is a near-universe of online job ads that are scraped from thousands of websites and online job boards. The 15-30 percent of job ads that include posted wages are used in this analysis. The treatment group is only approximately 4% of the observations of the full BGT sample, whereas it is 48% of the observations in the matched sample. The matched sample consists solely of franchising chains, either those that entered into AODs, those which are present in the digitized FDD data, or both. It is further restricted by minimum cell sizes for estimating saturated fixed effects specifications, as described further in subsection 3.4. This cell size restriction is the reason for the smaller number of treatment group observations in the matched sample. Otherwise, the treatment group in the two samples is conceptually identical: all observations in the microdata associated with any of the treated (AOD) franchise chains.

In both samples, we treat observed wages identically regardless of whether the wage is actually paid hourly, annually, or with any other periodicity. The BGT microdata reports all wages as annual salaries (sometimes as a range, in which case we use the midpoint as the wage). For job ads that post an hourly wage, the annual salary is computed assuming full-time work, regardless of the actual hours worked in the job. However, the BGT microdata also reports whether the job ad source posts an hourly wage or an annual salary (or, in rare cases, the wage at some other frequency). Thus, below, we conduct our regressions separately for jobs reporting hourly wages versus annual salaries.

Because of the innately greater similarity between the treatment and control groups in the matched sample relative to the full sample, the wage distribution is similar for the two in the matched sample, whereas in the full sample, the treatment group consists of disproportionately low-wage workers relative to the control group. Likewise, the share of hourly wage workers in the treatment and control groups is very similar in the matched sample but, in the full sample, the share of hourly workers is significantly lower in the control group.

The evaluation period extends from January 2017 through December 2021, though we restrict the number of pre-treatment quarters at 6 for every treatment cohort in the BGT microdata. There is a large increase in the number of observations starting in early 2018 for both the full sample and the matched sample in the BGT microdata. That is due to the introduction at that time of new job boards into the source material for BGT with a higher prevalence of including posted wages, relative to the pre-existing sources for the job ads data. Hence, a lengthier pre-treatment period would not add very many actual observations relative to the large number of additional fixed effects to estimate.

3.2 Staggered Difference-in-Difference Specification

We estimate the following staggered difference-in-difference specification:

$$\log w_{ijoct} = \beta \text{AOD}_j \cdot \text{Post}_{jt} + \gamma_{oj} + \delta_{ot} + \lambda_{ct} + \epsilon_{ijoct} \tag{3.1}$$

where $\log w_{ijoct}$ is the log wage for job i in occupation o at chain or employer j in local area c in calendar quarter t. AOD_j indicates whether chain j entered into an AOD, and Post_{jt}

indicates whether calendar quarter t post-dates chain j's AOD. γ_{oj}, δ_{ot}, and λ_{ct} are fixed effects for chain (or employer)-by-occupation, occupation-by-calendar-quarter, and local area-by-calendar quarter respectively. β is the coefficient of interest, interpreted as the percent change in pay for jobs at chain j after the chain entered into an AOD. The identifying assumption for attaching a causal interpretation to $\hat{\beta}$ is that if treated chains had not entered into an AOD, the pay they offer (controlling for employer wage effects, occupational and geographic mix, and calendar time) would have followed the same trend as those of untreated chains or employers.

We use Stata's `did_imputation` command implementing Borusyak, Jaravel and Spiess (2022)'s methodology for treatment effect heterogeneity-robust estimation of staggered difference-in-difference specifications. That procedure employs three steps:

1. Estimate the fixed effects and coefficients on covariates from a regression using only untreated observations.

2. For each treated observation, compute the difference between its actual outcome and the imputed outcome, given the coefficient estimates from step 1.

3. Construct a weighted average across treated observations of treatment effects estimated in step 2.

3.3 Event Study Specification

In addition to the staggered difference-in-difference specification that estimates a single treatment effect, we implement an event-time methodology that estimates a different treatment effect for each quarter-relative-to-treatment-date. That specification is

$$\log w_{ijoct} = \sum_{a=-h}^{b} \beta_a K_{jt} + \gamma_{oj} + \delta_{ot} + \lambda_{ct} + \epsilon_{ijoct} \tag{3.2}$$

where K_{jt} is an indicator variable for j being a treated chain and t being a quarters pre- or post-chain j's AOD.

We generate the event study estimates from 3.2 using the same Stata command `did_imputation` and the corresponding plots using `event_plot`, a command created by the same authors.

3.4 Implementation

As outlined in equations 3.1 and 3.2, our specifications include interacted fixed effects for employer or franchise chain by occupation, occupation by calendar quarter, and local area by calendar quarter. This means our estimates of $\hat{\beta}$ are net of time-varying average pay by occupation and local area, and employer-specific pay policies by occupation. The microdata is pooled by calendar quarter in order to ensure a sufficient number of observations in each time unit to estimate saturated specifications. Hence, the chains whose AODs are dated within the same calendar quarter are grouped together into treatment cohorts. There are seven treatment cohorts in total, starting with 2018Q3 and ending with 2020Q1. In the BGT microdata, the study period begins in 2017Q1 and h in equation 3.2 is 5 (corresponding to a different calendar quarter for each treatment cohort). In the GD microdata, the study period begins in 2008Q1 and h in equation 3.2 is 12.

In the full sample using the BGT microdata, the two interacted fixed effects where occupation is present - γ_{oj} and δ_{ot} - use the 6-digit SOC. The matched sample, however, uses the 4-digit SOC.[7] In the full sample using the GD microdata, the two interacted fixed effects where occupation is present - γ_{oj} and δ_{ot} - use Glassdoor's specific occupation field. The matched sample, however, uses the general occupation field.[8] The use of this higher level of aggregation in the matched sample aims to reduce the share of the sample that has to be dropped in order to ensure that, when using the estimator defined by Borusyak, Jaravel and Spiess (2022),

[7]The matched BGT sample includes 110 occupations defined at the 4-digit SOC level, while the full BGT sample includes 831 occupations defined at the 6-digit level.

[8]The matched GD sample includes 82 general occupations, while the full GD sample includes 1,206 specific occupations.

the full set of fixed effects can be identified for both the treatment and the control groups.

All specifications using the BGT microdata employ commuting zones as the empirical analog to the geographic local labor market. (The raw data include county identifiers, which can be aggregated to the commuting zone level.) The GD specifications use U.S. states as the analog to local labor markets, as this is the finest location available for all respondents.[9]

For our baseline specifications, the data ends in 2021Q4, which means that b in equation 3.2 varies by treatment cohort: 13 quarters for the earliest-treated chains whose AODs in the 2018Q3 cohort, 7 quarters for the latest-treated chains whose AODs are dated 2020Q1. We also conduct an analysis that ends the post-period in February 2020, prior to the onset of the COVID-19 pandemic. In that specification, b is reduced by seven quarters for every cohort (and hence the final treatment cohort is, in effect, dropped when the post-period is thus shortened).

When using the matched sample, in the baseline estimates that include the entire post-period through 2021Q4, the imputation-based estimator from Borusyak, Jaravel and Spiess (2022) imposes a minimum cell size of 17 observations in the BGT microdata, 58 observations in the GD microdata, for each type of cell defined by the three sets of interacted fixed effects defined in equations 3.1 and 3.2, for both the treated and the control groups. That minimum cell size is why there are fewer treated units and treatment group observations in the matched sample relative to the full sample reported in tables 2 and 3. When using the full sample and the entire time period until 2021Q4, such cell size restrictions are not necessary in the BGT microdata, but the GD data require a minimum cell size of 6.

4 Results

4.1 Full Sample Results

Table 4 reports estimates of $\hat{\beta}$ from equation 3.1 in the full sample for the BGT microdata. The first column uses the entire sample period through 2021Q4. In column (2) we curtail the

[9] Location is available at the MSA level for a subset of respondents.

post-period to February 2020. We do so to isolate the effect of removing the no-poaches before the onset of the COVID-19 pandemic shutdowns in the United States, which upended low-wage labor markets that are the subject of our inquiry.

Focusing first on the results using the entire sample period - i.e., column (1) - we find that entering into an AOD causes a 3.6 percent increase in chain-specific wages in the full BGT sample. We present the estimation results for equation 3.2 graphically , using the entire sample period until 2021Q4, in figure 1. For the GD microdata, the equivalent estimate for the full post-period is a 1.3 percent increase in pay, reported in table 5.

The difference in results between the two datasets is not surprising given the underlying structure of their source material. Because BGT captures the flow of new job ads, posted wages respond immediately. The smaller magnitude in GD relative to BGT plausibly arises because GD measures the stock of wages and salaries, not the flow. For example, a user might submit a report in 2019Q2 of a wage determined in 2018Q1. Because of this data structure, we expect GD wages and salaries to respond more slowly to an AOD. Figure 2 illustrates just such a dynamic. While there is no detectable effect in the first year and a half following an AOD, estimates increase thereafter, rising to nearly 3% by the thirteenth quarter.

4.2 Matched Sample Results

Tables 6 and 7 report the estimates of equation 3.1 in the matched sample for the BGT and GD microdata, respectively. In the matched sample, the overall treatment effect in the BGT data is 2.5 percent. The pattern of relative-time coefficients follows the same temporal pattern as the full sample:[10] an initial increase in wages for the treatment group, followed by a gradual decline starting around five quarters post-treatment, and then another increase toward the end of the post-period. The matched sample has larger confidence intervals, reflecting the smaller sample size. The estimated ATT in the matched GD data is indistinguishable from zero, probably due to the overall smaller sample size, and the consequent need to drop many

[10] We do not include the event time figures for the matched sample, to economize on space.

treatment observations given minimum cell size restrictions.

4.3 Results by Pay Frequency

Finally, we estimate the same regressions separately for hourly wage jobs versus annual salary jobs. Those results are shown in figures 3 and 4 for the full BGT sample, and in figures 5 and 6 for the full GD sample.[11] These results reveal an interesting temporal pattern: pay at jobs offering an annual salary increases approximately 10% at treatment, eventually declining to a zero effect about ten quarters thereafter, for an overall treatment effect of 5% through the full post-period. Hourly wages, on the other hand, show little change for seven quarters following treatment, then increase by approximately 5%, for an overall treatment effect of 2.3%.

One potential explanation for these differences in the timing of the treatment effect is the fact that workers were not notified about the franchise no-poach enforcement or the binding commitment not to enforce no-poaches contained in AODs (nor were they likely to be aware of the franchise no-poaches in the first place, since they are contained in franchising contracts to which workers are not a party). Franchisees notified about the non-enforcement of no-poaches may well have responded to the AODs by actively recruiting managers (who are more likely to be salaried) from other franchisees in the same chains, generating the immediate wage gains for salaried workers we observe in the job ads microdata. Hourly workers, on the other hand, would likely have learned about the option to work for a different franchisee in the same chain by observing co-workers move from one franchisee to another, making any wage effect of increased competition take longer to materialize.

The GD event-study coefficients do not show the same dichotomous timing of treatment effects between hourly-wage and annual-salary jobs, probably for the aforementioned reason that the flow of salary reports are observations on the stock of active jobs, and thus less immediately responsive to treatment. For both hourly-wage and annual-salary jobs, treatment effects start to manifest about six quarters post-treatment. However, estimates from both datasets are

[11] We do not include equivalent results for the matched sample, again for space reasons.

consistent in that the ATT for annual-salary workers is about twice as large in magnitude as for hourly-wage workers.

5 Discussion

The Washington AG's franchise no-poach enforcement campaign can be understood as a source of quasi-experimental variation in labor market competition. The difference in treatment effects and timing between annual-salary and hourly-wage jobs suggests a parallel with findings from other studies of variation in labor market competition such as Prager and Schmitt (2021): wages for higher-wage workers are more sensitive to variation in labor market competition than wages for lower-wage workers.[12] This finding is consistent with the theory proposed by Berger, Herkenhoff and Mongey (2022) that the wedge between marginal product and the wage is larger for higher-paid workers in monopsonized labor markets with worker heterogeneity, and inconsistent with the wage-posting model of Burdett and Mortensen (1998) and its derivatives, because the latter models imply a distribution of firm-specific wages with the largest monopsonistic markdowns for the lowest-paying firms. Interpreting our findings in light of the two dominant traditions for modeling wage-setting under imperfect labor market competition set forth by Manning (2011), ex-ante wage-posting versus ex-post bargaining, our findings suggest the availability of external options affects wages more for higher-status workers (within the overall franchising labor market, which is relatively low-wage compared to the rest of the labor market), in line with the findings about subjective experience of workers reported by Hall and Krueger (2012). Higher-paid workers are more likely to bargain, and formal labor market competition matters more for bargaining than it does for wage posting. The attenuated and delayed treatment effect for hourly-wage jobs in the BGT microdata may reflect

[12] Azar, Marinescu and Steinbaum (2022) and Azar, Berry and Marinescu (2022) find that there is no systematic difference between the level of labor market competition in high-wage versus low-wage occupations, but *changes* in the level of labor market competition may nonetheless affect higher-wage workers more. However, Guanziroli (2022) finds that one retail pharmacy merger in Brazil reduced wages more for lower-status salespeople than for higher-status pharmacists. He concludes that is due to higher levels of unionization in the latter occupation.

that wage-posting is a better model for the labor markets in which hourly workers in service industries work, where the channel by which the removal of franchise no-poaches would operate is by increasing the arrival rate of outside job offers—something that may take time to materialize.

Furthermore, our finding that an exogenous increase in labor market competition appears to benefit higher-wage workers more contrasts with studies of labor standards that tend to find the lowest-wage workers benefit most from raising the floor (e.g. Dube (2019)). If that interpretation is correct, it would mean that two different types of labor market policy intervention (enacting labor standards versus competition enforcement in labor markets) are distinguished by their distributional impact. This is an area ripe for further investigation, given greater attention to policy-driven variation in labor market competition[13] (not to mention in labor standards).

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. Our findings are at odds with that ruling. Franchise no-poach provisions are costly for workers, because they diminish competition in the labor markets where franchise employers hire. The antitrust enforcement against such restraints that has happened to date has therefore benefited them, which would not have been the case if the labor markets where franchise employers hire were, in fact, competitive. Moreover, only chains that had a presence in the state of Washington were investigated, and only those found to have been using franchise no-poaches subsequently entered into an AOD. This means that franchise no-poach provisions remain legal for franchising chains that did not enter into an AOD with the Washington State AG as of this writing, and no chain has faced penalties for using them in the past. Norlander (2022) provides evidence that most or all chains that entered into AODs subsequently removed no-poaching language from their fran-

[13] e.g. Harris (2022).

chising contracts, but that such language continues to be used by approximately 10% of chains (presumably consisting of chains that did not enter AODs).

Scholarship about the franchising sector generally presumes that its labor markets are competitive, given the large number of service-sector employers who operate within it. Nonetheless, research that finds that minimum wage 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 further evidence against perfect competition in service sector labor markets, given that no-poach provisions reduce the number of outside options available to workers. They also limit the internal job ladders that would operate in large national chains if they were unitary rather than franchised, since in a franchised chain that uses a no-poach, workers have to switch chains in order to switch jobs.

Since the 1970s, vertical restraints in franchising contracts have been legalized on pro-competitive grounds (Callaci, 2021*a*; Blair and Lafontaine, 2005), and some enforcers have considered franchise no-poach provisions to be vertical agreements and thus shielded from legal liability (Delrahim et al., 2019). But vertical restraints may have a competition-reducing effect when they can be used to affect the terms of third-party transactions, e.g. limiting the ability of would-be discounting entrants to compete in the product market. Our work points to a different set of third-party transactions and markets where competition may be reduced: the labor market in which franchisees hire workers. Prohibitions on hiring workers away from other franchisees in the same chain (i.e., workers who are already trained on the franchise chain's operating manual) may operate to bestow monopsonistic wage-setting power on franchisees in the chain, similar to the economic logic of minimum resale price maintenance proposed by Asker and Bar-Isaac (2014)—a reward for franchisees to conform to the wishes of franchisors in excluding competition.

Finally, 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).

6 Conclusion

Following the suggestion made in the postscript 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 employer-identified job-level microdata from Burning Glass Technologies and Glassdoor, we estimate the effect of entering into an AOD on worker pay. Our preferred specification indicates that the enforcement campaign increased annual salary by 3.6%. For a worker with median earnings of $26,228 in the treatment group, that corresponds to an increase of $944. We find differences in treatment effect magnitude and timing between jobs that pay an annual salary versus an hourly wage. The former appear to experience an immediate increase in wages. Wage effects for hourly workers take longer to materialize, and when they do, the effect sizes are smaller.

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Table 1. List of franchise chains and their corresponding AOD dates.

Franchise name	Settlement date	Franchise name	Settlement date	Franchise name	Settlement date
Arby's	7/12/2018	Abbey Carpet	9/23/2019	Concrete Craft	11/1/2019
Auntie Anne's	7/12/2018	Floors To Go	9/23/2019	Great Harvest Bread	11/1/2019
Buffalo Wild Wings	7/12/2018	Frugals	9/23/2019	NPM Franchising	11/1/2019
Carl's Jr.	7/12/2018	Mattress Depot	9/23/2019	Paul Davis Restoration	11/1/2019
Cinnabon	7/12/2018	Tan Republic	9/23/2019	Taco John's	11/1/2019
Jimmy John's	7/12/2018	Any Lab Test Now	9/30/2019	Tailored Living	11/1/2019
McDonald's	7/12/2018	Chuck E. Cheese	9/30/2019	Ezell's Famous Chicken	11/8/2019
Applebee's	8/20/2018	Expedia CruiseShipCenters	9/30/2019	Dollar Rent A Car	11/8/2019
Church's Texas Chicken	8/20/2018	Engel & Völkers	9/30/2019	Hertz	11/8/2019
Five Guys	8/20/2018	Krispy Kreme	9/30/2019	Real Deals	11/8/2019
IHOP	8/20/2018	Mora Iced Creamery Shop	9/30/2019	Thrifty Rent A Cat	11/8/2019
Jamba Juice	8/20/2018	Sizzler	9/30/2019	Advanced Fresh Concepts	11/15/2019
Little Caesars	8/20/2018	Starcycle	9/30/2019	Body and Brain Center	11/15/2019
Panera	8/20/2018	Aire Serv	10/7/2019	School of Rock	11/15/2019
Sonic	8/20/2018	PostalAnnex	10/7/2019	Servpro	11/15/2019
A&W Restaurants	9/13/2018	Pak Mail	10/7/2019	Spring-Green Lawn Care	11/15/2019
Burger King	9/13/2018	Drama Kids	10/7/2019	Supporting Strategies	11/15/2019
Denny's	9/13/2018	Five Star Painting	10/7/2019	The Barbers Source	11/15/2019
Pap John's	9/13/2018	Hand and Stone	10/7/2019	The Bar Method	11/22/2019
Pizza Hut	9/13/2018	InXpress	10/7/2019	Phenix Salon	11/22/2019
Popeye's	9/13/2018	MaidPro	10/7/2019	Senior Helpers	11/22/2019
Tim Hortons	9/13/2018	My Place Hotels	10/7/2019	Singers Company	11/22/2019
Wingstop	9/13/2018	Pump It Up	10/7/2019	Critter Control	12/9/2019
Anytime Fitness	10/16/2018	AlphaGraphics	10/11/2019	Good Feet	12/9/2019
Baskin-Robbins	10/16/2018	Ben & Jerry's	10/11/2019	Hobby Town	12/9/2019
Circle K	10/16/2018	Elmer's	10/11/2019	JDog	12/9/2019
Domino's Pizza	10/16/2018	F45 Training	10/11/2019	NextHome	12/9/2019
Firehouse Subs	10/16/2018	Fit Body Boot Camp	10/11/2019	Signarama	12/9/2019
Planet Fitness	10/16/2018	Global Recruiters Network	10/11/2019	Thrive Community Fitness	12/9/2019
Valvoline	10/16/2018	HomeTeam	10/11/2019	Transworld Business advisors	12/9/2019
Quiznos	11/27/2018	Huntington Learning Centers	10/11/2019	UBuildlt	12/9/2019
Massage Envy	11/27/2018	Johnny Rockets	10/11/2019	Abra Automotive Systems	12/13/2019
Frontier Adjusters	11/26/2018	Kona Ice	10/11/2019	AR Workshop	12/13/2019
Sport Clips	11/27/2018	Novus Franchising	10/11/2019	CarePatrol	12/13/2019
Batteries Plus	12/5/2018	Pillar To Post	10/11/2019	Fibrenew	12/13/2019
CK Franchising	12/5/2018	Pirtek	10/11/2019	Freshii	12/13/2019
Edible Arrangements	12/5/2018	Best In Class	10/18/2019	NMC Franchising	12/13/2019
Lq Quinta	12/5/2018	C.T. Franchising Systems	10/18/2019	Cost Cutters	12/13/2019
Merry Maids	12/5/2018	Costa Vida	10/18/2019	Smartstyle	12/13/2019
Budget Blinds	12/20/2018	Dickey's	10/18/2019	Fix Auto	12/20/2019
GNC	12/20/2018	Fujisan	10/18/2019	John L. Scott Real Estate Affiliates	12/20/2019
Jack in the Box	12/20/2018	HealthSource Chiropractic	10/18/2019	Pro Image	12/20/2019
Jackson Hewitt	12/20/2018	Molly Maid	10/18/2019	Red Lion Hotels	12/20/2019
Jiffy Lube	12/20/2018	Mr. Appliance	10/18/2019	Velofix	12/20/2019
Menchie's Frozen Yogurt	12/20/2018	Mr. Electric	10/18/2019	Weichert Real Estate Affiliates	12/20/2019
The Original Pancake House	12/20/2018	Mr. Handyman	10/18/2019	Orangetheory Fitness	12/27/2019
Bonefish Grill	1/14/2019	Mr. Rooter	10/18/2019	OsteoStrong	12/27/2019
Carrabba's Italian Grill	1/14/2019	Palm Beach Tan	10/18/2019	Padgett Business Services	12/27/2019
Management Recruiters International	1/14/2019	Rainbow International	10/18/2019	SYNERGY	12/27/2019
Outback Steakhouse	1/14/2019	Real Property Management	10/18/2019	Board and Brush	12/31/2019
Einstein Bros. Bagels	2/15/2019	Restoration 1	10/18/2019	Poke Bar Dice and Mix	12/31/2019
Express Employment Professionals	2/15/2019	Window Genie	10/18/2019	Two Men and a Truck	12/31/2019
Fastsigns International	2/15/2019	World Inspection Network	10/18/2019	Baja Fresh	1/10/2020
L&L Franchise	2/15/2019	1-800 Radiator	10/28/2019	Sharetea	1/10/2020
The Maids International	2/15/2019	Allegra Network	10/28/2019	Manchu Wok	1/10/2020
Westside Pizza	2/15/2019	BAM Franchising	10/28/2019	Pizza Factory	1/10/2020
Zeek's Restaurants	2/15/2019	CARSTAR	10/28/2019	Realty One Group Affiliates	1/10/2020
AAMCO	5/14/2019	Club Z!	10/28/2019	The Little Gym	1/10/2020
Famous Dave's	5/14/2019	Dutch Bros	10/28/2019	Tutor Doctor Systems	1/10/2020
Meineke	5/14/2019	Emerald City Smoothie	10/28/2019	Club Pilates	1/24/2020
Qdoba	5/14/2019	FYZICAL	10/28/2019	Elements Massage	1/24/2020
Villa Pizza	5/14/2019	Glass Doctor	10/28/2019	Fitness Together	1/24/2020
Aaron's	8/8/2019	Image360	10/28/2019	HomeSmart	1/24/2020
H&R Block	8/8/2019	Kiddie Academy	10/28/2019	I love kickboxing	1/24/2020
Mio Sushi	8/8/2019	MAACO	10/28/2019	ServiceMaster	1/24/2020
UPS	8/8/2019	Mac Tools	10/28/2019	Toro Tax Franchising	1/24/2020
Jersey Mike's	9/10/2019	Pelindaba Franchising	10/28/2019	Panda Express	1/31/2020
Curves	9/9/2019	Property Damage Appraisers	10/28/2019	Grease Monkey	1/31/2020
European Wax Center	9/9/2019	PuroClean	10/28/2019	Nothing Bundt Cakes	1/31/2020
Figaro's Pizza	9/9/2019	Remedy Intelligent Staffing	10/28/2019	CMIT Solutions	2/7/2020
The Habit Burger Grill	9/9/2019	Signs Now	10/28/2019	Golden Corral	2/14/2020
Home Instead	9/9/2019	Soccer Shots	10/28/2019	Tropical Smoothie Cafe	2/14/2020
ITEX Corporation	9/9/2019	The Joint Corp.	10/28/2019	Canteen	2/18/2020
The Melting Pot	9/9/2019	Urban Float Opportunities	10/28/2019	Right at Home	2/18/2020
Wetzel's Pretzels	9/9/2019	Waxing the City	10/28/2019	Fit4Mom	2/18/2020
Charleys Philly Steaks	9/20/2019	AdvantaClean	11/1/2019	InchinsBambooGarden	2/21/2020
Gold's Gym	9/20/2019	Arthur Murray	11/1/2019	PLAYlive Nation	2/21/2020
Mrs. Fields	9/20/2019	Bambu	11/1/2019	Port of Subs	2/21/2020
Kung Fu Tea	9/20/2019	CHHJ Franchising	11/1/2019	uBreakiFix	2/21/2020

Table 2. Summary statistics for the no-poach impact evaluation, BGT microdata. This table reports summary statistics for the full sample and the matched sample described in section 3.

	Treatment group (full BGT sample)	Control group (full BGT sample)	Treatment group (matched BGT sample)	Control group (matched BGT sample)
Number of chains/employers	220	1,219,742	199	297
Number of job ads (total)	734,713	17,761,674	685,001	687,196
Number of job ads (avg per chain/emp)	3,340	15	3,442	2,314
Salary (2015 USD): average	31,571	48,021	31,649	29,785
Salary (2015 USD): P10	18,367	21,520	18,423	18,020
Salary (2015 USD): P25	21,370	26,567	21,416	20,444
Salary (2015 USD): P50	26,228	36,358	26,256	24,403
Salary (2015 USD): P75	35,269	57,636	35,204	31,375
Salary (2015 USD): P90	49,628	87,996	50,178	46,355
Share of hourly wage job ads (%)	63	45	62	64

Table 3. Summary statistics for the no-poach impact evaluation, GD microdata. This table reports summary statistics for the full sample and the matched sample described in section 3.

	Treatment group (full GD sample)	Control group (full GD sample)	Treatment group (matched GD sample)	Control group (matched GD sample)
Number of chains/employers	141	33850	77	209
Number of reports (total)	92789	5516147	79810	202004
Number of reports (avg per chain/emp)	658.1	39121.6	566.0	1432.7
Pay (2012 USD): average	7681.5	46585.0	6114.3	15194.8
Pay (2012 USD): P10	8.35	11.0	8.27	8.77
Pay (2012 USD): P25	9.78	14.6	9.42	9.78
Pay (2012 USD): P50	12.0	39940.4	11.6	11.7
Pay (2012 USD): P75	15.4	79353.2	14.7	22.0
Pay (2012 USD): P90	38339.2	122366.0	33063.8	58253.7
Share hourly reports	0.83	0.45	0.83	0.76
Share monthly reports	0.0053	0.0073	0.0053	0.0014

Table 4. Borusyak, Jaravel and Spiess (2022) treatment effect estimates for DiD specification (full BGT data, interacted FEs). This table reports a single treatment effect estimate ($\hat{\beta}$ from equation 3.1). It uses (year-quarter)x(commuting zone), (year-quarter)x(SOC-6d), and (SOC-6d)x(employer) FEs; in column (2) it drops any corresponding cells with fewer than 2 observations. Further sample restrictions are as highlighted in section 3.

	(1) Ln(real pay) Full post-treatment period until 2021q4	(2) Ln(real pay) Post-treatment period until Feb-2020
ATT	0.036***	0.076***
	(0.010)	(0.027)
$\tau = -1$	-0.003	-0.001
	(0.013)	(0.013)
$\tau = -2$	0.006	0.007
	(0.011)	(0.010)
$\tau = -3$	0.004	0.004
	(0.010)	(0.009)
$\tau = -4$	-0.001	-0.002
	(0.009)	(0.009)
$\tau = -5$	0.000	-0.001
	(0.008)	(0.008)
Observations	18,496,387	7,178,615
Year-quarter x CZ FEs	Y	Y
Year-quarter x SOC-6d FEs	Y	Y
SOC-6d x Employer FEs	Y	Y

*** $p<0.01$; ** $p<0.05$; * $p<0.10$
Note: Robust standard errors clustered at the employer level in parentheses.

Figure 1. Estimate of relative-time treatment coefficients in the event study specification for the full sample. This plots the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for the full sample in the BGT micro-data.

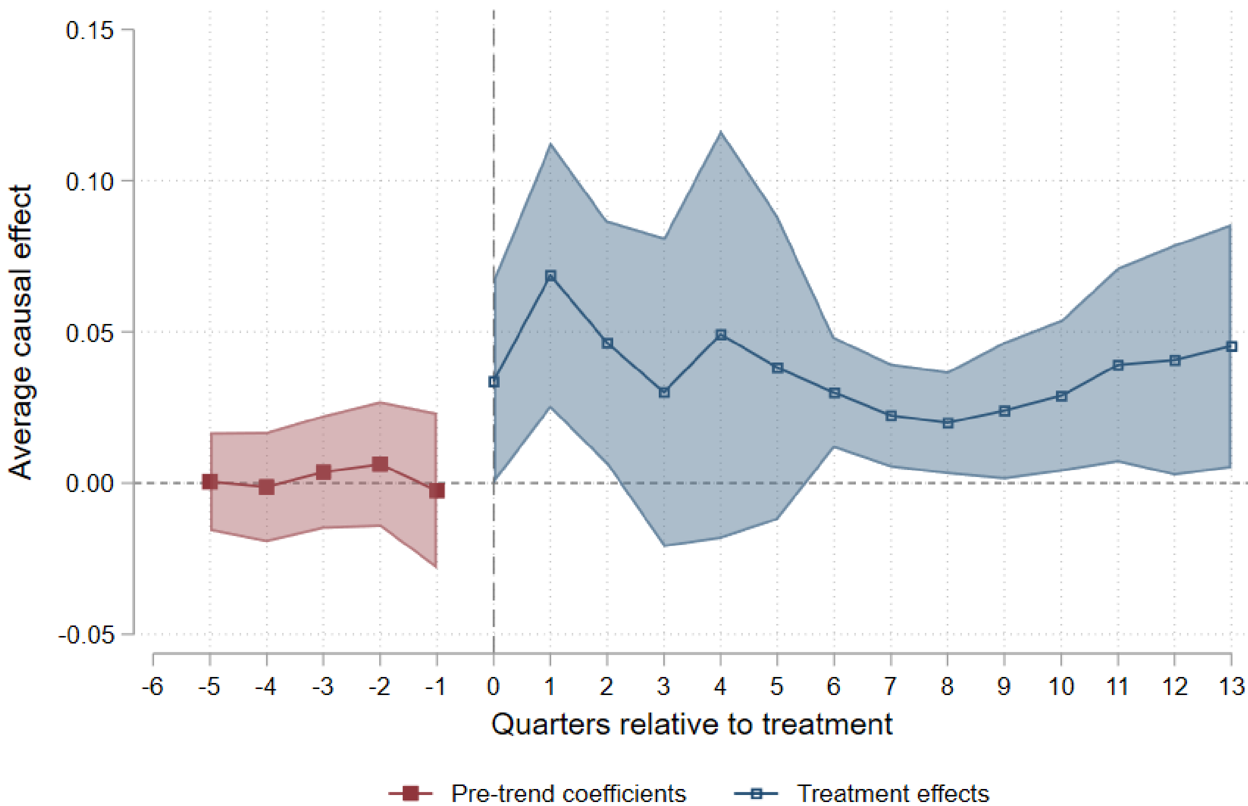

Table 5. Borusyak, Jaravel and Spiess (2022) treatment effect estimates for DiD specification (full GD data, interacted FEs). This table reports a single treatment effect estimate ($\hat{\beta}$ from equation 3.1). It uses (year-quarter)x(state), (year-quarter)x(specific occupation), and (specific occupation)x(employer) FEs; it drops any employer-occupation with fewer than 7 observations. Further sample restrictions are as highlighted in section 3.

	(1) Ln(real pay) Full post-treatment period until 2021q4	(2) Ln(real pay) Post-treatment period until Feb-2020
ATT	0.013**	-0.0046
	(0.0051)	(0.0036)
$\tau = -1$	-0.0043	-0.0037
	(0.0055)	(0.0052)
$\tau = -2$	0.00076	0.00099
	(0.0060)	(0.0059)
$\tau = -3$	0.0052	0.0055
	(0.0060)	(0.0059)
$\tau = -4$	0.00021	0.00065
	(0.0050)	(0.0048)
$\tau = -5$	0.00096	0.0011
	(0.0058)	(0.0057)
$\tau = -6$	0.000052	0.00077
	(0.0053)	(0.0052)
$\tau = -7$	-0.00092	-0.00082
	(0.0056)	(0.0055)
$\tau = -8$	0.0027	0.0030
	(0.0055)	(0.0054)
$\tau = -9$	0.00048	0.00042
	(0.0047)	(0.0046)
$\tau = -10$	-0.0025	-0.0025
	(0.0041)	(0.0040)
$\tau = -11$	0.0023	0.0023
	(0.0044)	(0.0045)
$\tau = -12$	0.0017	0.0020
	(0.0049)	(0.0048)
Observations	5608046	3358197
Year-quarter x State FEs	Y	Y
Year-quarter x Spec. occ. FEs	Y	Y
Spec. occ. x Employer FEs	Y	Y

*** $p<0.01$; ** $p<0.05$; * $p<0.10$
Note: Robust standard errors clustered at the employer level in parentheses.

Figure 2. Estimate of relative-time treatment coefficients in the event study specification for the full sample. This plots the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for the full sample in the GD micro-data.

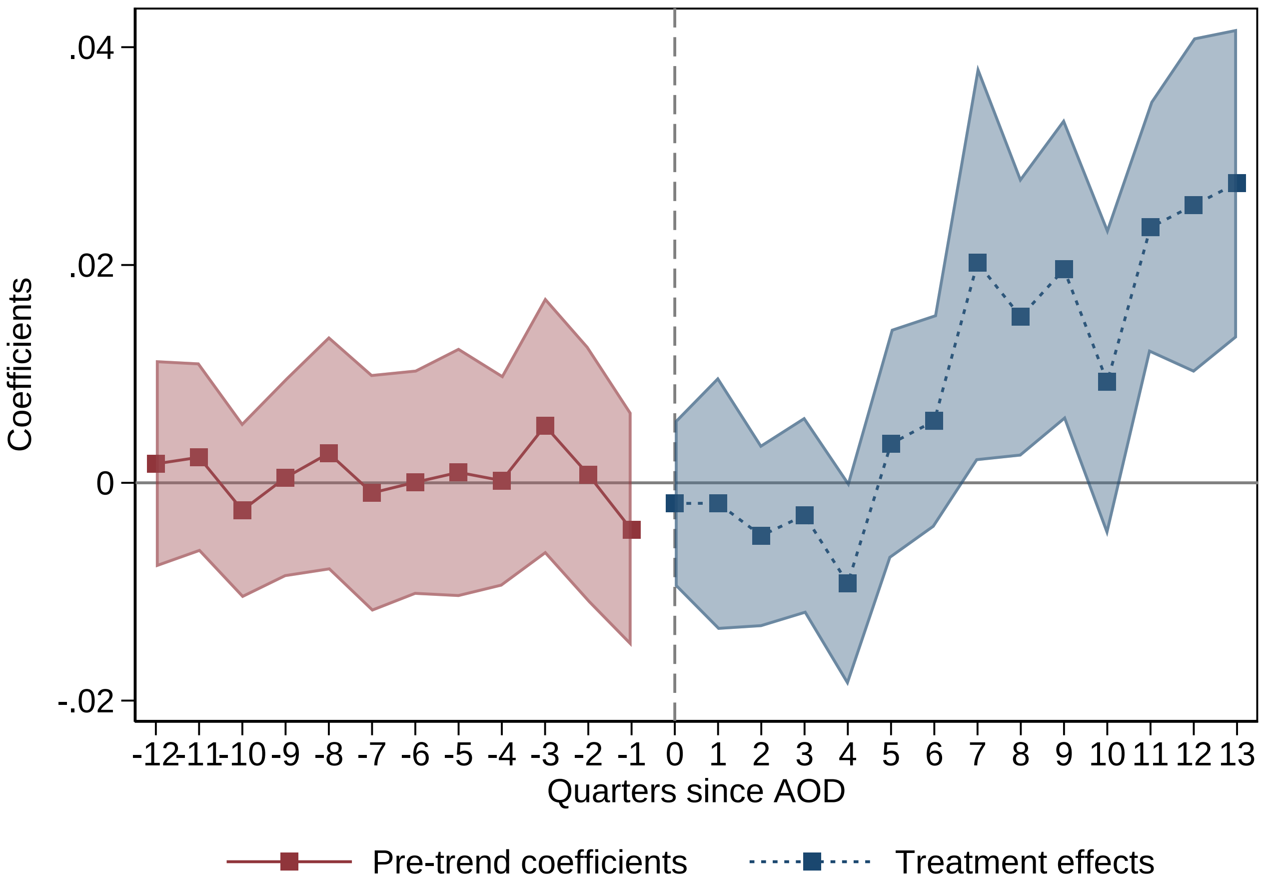

Table 6. Borusyak, Jaravel and Spiess (2022) treatment effect estimates for DiD specification (matched BGT data, interacted FEs). This table reports a single treatment effect estimate ($\hat{\beta}$ from equation 3.1). It uses (year-quarter)x(commuting zone), (year-quarter)x(SOC-4d), and SOC-4d)x(franchise) FEs; in column (1) it drops any corresponding cells with fewer than 17 observations, while in column (2) it does so for cells with fewer than 11 observations. Further sample restrictions are as highlighted in section 3.

	(1) Ln(real pay) Full post-treatment period until 2021q4	(2) Ln(real pay) Post-treatment period until Feb-2020
ATT	0.025*	0.068**
	(0.013)	(0.029)
$\tau = -1$	0.006	-0.000
	(0.016)	(0.015)
$\tau = -2$	0.021	0.013
	(0.013)	(0.013)
$\tau = -3$	0.011	0.004
	(0.013)	(0.012)
$\tau = -4$	0.009	0.003
	(0.012)	(0.011)
$\tau = -5$	0.010	0.005
	(0.011)	(0.011)
Observations	1,372,197	504,000
Year-quarter x CZ FEs	Y	Y
Year-quarter x SOC-4d FEs	Y	Y
SOC-4d x Franchise FEs	Y	Y

*** p<0.01; ** p<0.05; * p<0.10
Note: Robust standard errors clustered at the franchise level in parentheses.

Table 7. Borusyak, Jaravel and Spiess (2022) treatment effect estimates for DiD specification (matched BGT data, interacted FEs). This table reports a single treatment effect estimate ($\hat{\beta}$ from equation 3.1). It uses (year-quarter)x(state), (year-quarter)x(general occupation), and (general occupation)x(franchise) FEs; in both columns it drops any corresponding cells with fewer than 58 observations. Further sample restrictions are as highlighted in section 3.

	(1) Ln(real pay) Full post-treatment period until 2021q4	(2) Ln(real pay) Post-treatment period until Feb-2020
ATT	0.0034	-0.0049
	(0.0051)	(0.0037)
$\tau = -1$	-0.0033	-0.0033
	(0.0068)	(0.0068)
$\tau = -2$	-0.0017	-0.0020
	(0.0069)	(0.0070)
$\tau = -3$	0.0038	0.0036
	(0.0055)	(0.0056)
$\tau = -4$	-0.0014	-0.0018
	(0.0066)	(0.0067)
$\tau = -5$	-0.00083	-0.0011
	(0.0056)	(0.0056)
$\tau = -6$	0.0027	0.0025
	(0.0061)	(0.0062)
$\tau = -7$	-0.0032	-0.0032
	(0.0062)	(0.0062)
$\tau = -8$	0.0084	0.0084
	(0.0061)	(0.0061)
$\tau = -9$	0.0035	0.0034
	(0.0062)	(0.0062)
$\tau = -10$	0.0029	0.0028
	(0.0050)	(0.0050)
$\tau = -11$	-0.0017	-0.0017
	(0.0050)	(0.0050)
$\tau = -12$	0.0084*	0.0084*
	(0.0050)	(0.0051)
Observations	279774	159505
Year-quarter x State FEs	Y	Y
Year-quarter x Gen. occ. FEs	Y	Y
Gen. occ. x Franchise FEs	Y	Y

*** $p<0.01$; ** $p<0.05$; * $p<0.10$
Note: Robust standard errors clustered at the franchise level in parentheses.

Figure 3. Estimate of relative-time treatment coefficient in the event study specification for the full sample, hourly wage jobs only. This plots the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for the full sample in the BGT microdata, including only jobs with hourly wage pay frequency.

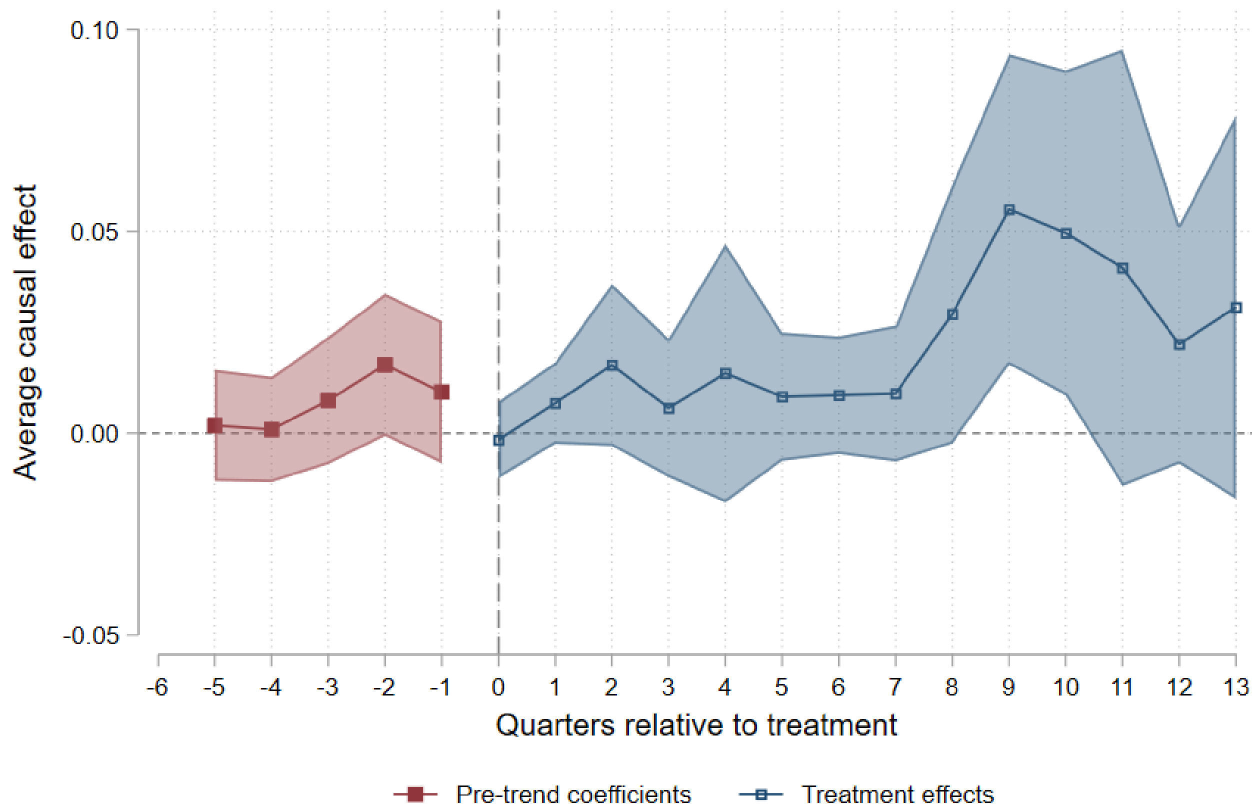

Figure 4. Estimate of relative-time treatment coefficient in the event study specification for the full sample, annual salary jobs only. This plots the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for the full sample in the BGT microdata, including only jobs with annual salary pay frequency.

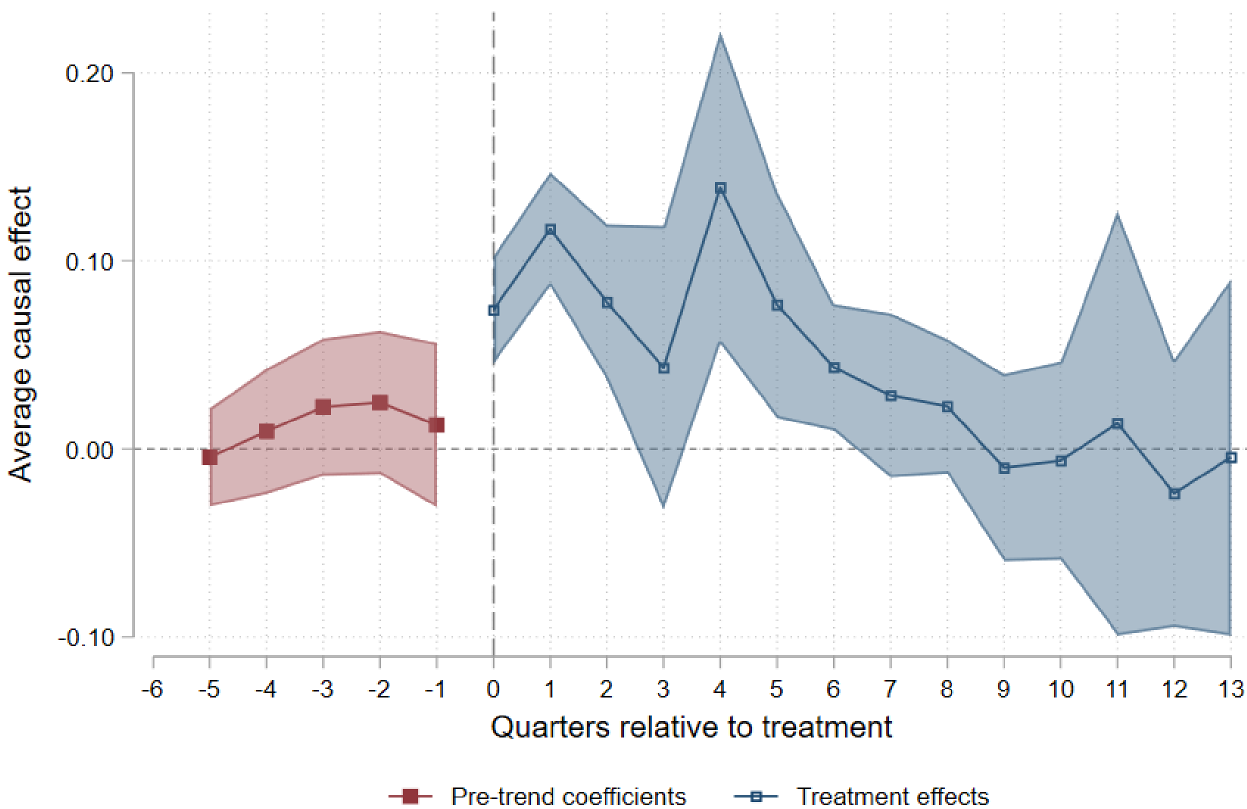

Figure 5. Estimate of relative-time treatment coefficient in the event study specification for the full sample, hourly wage jobs only. This plots the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for the full sample in the GD microdata, including only jobs with hourly wage pay frequency.

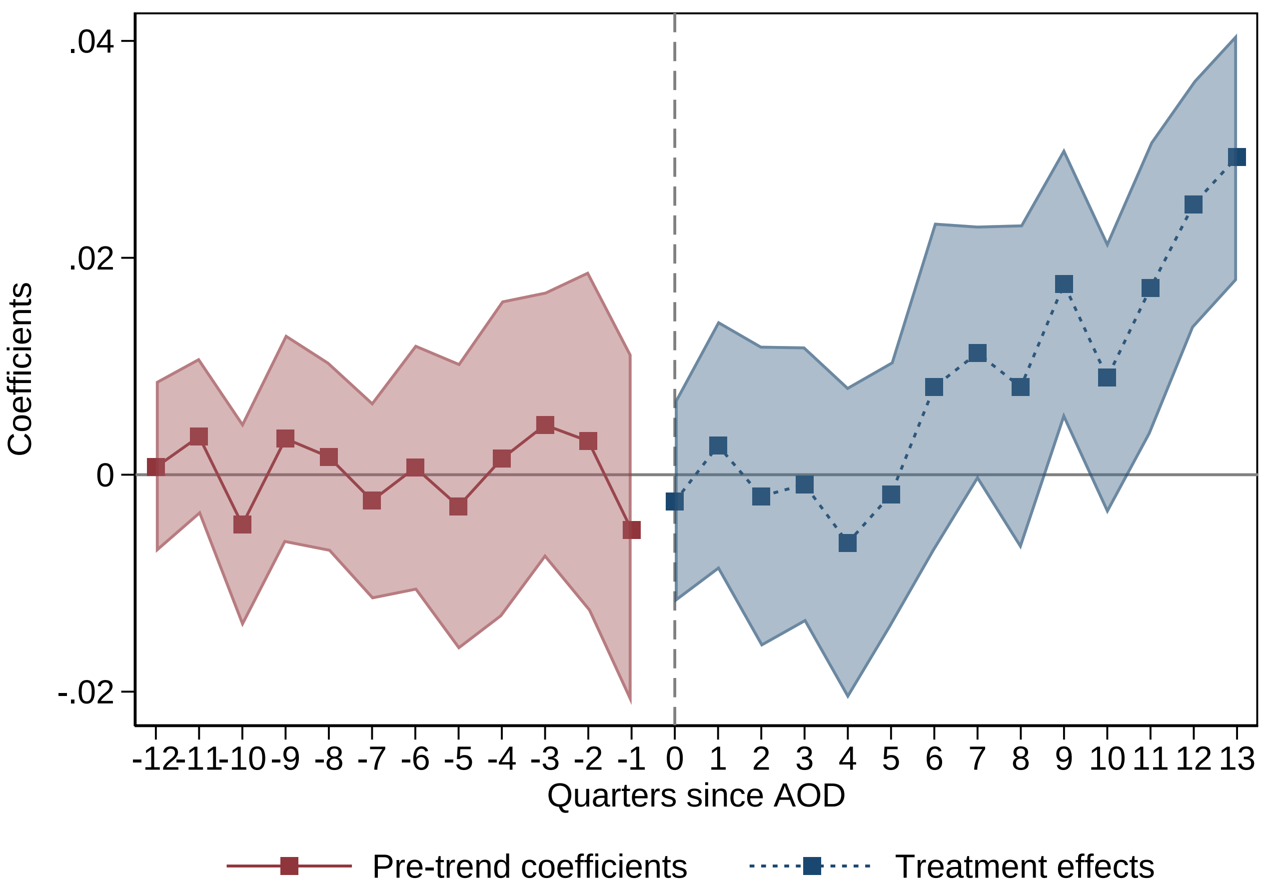

Figure 6. Estimate of relative-time treatment coefficient in the event study specification for the full sample, annual salary jobs only. This plots the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for the full sample in the GD microdata, including only jobs with annual salary pay frequency.