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ABSTRACT

We evaluate the nationwide impact of the Washington State attorney general's 2018-2020 enforcement campaign against no-poach clauses in franchising contracts, which prohibited worker movement across locations within a chain. Implementing a staggered difference-in-differences research design using Burning Glass Technologies job vacancies and Glassdoor salary reports from numerous industries, we estimate a 6 percent increase in posted annual earnings from the job vacancy data and a 4 percent increase in worker-reported earnings.

JEL Classification Codes: J42, K21, L40, J31

Key Words: Employer market power, oligopsony, monopsony, franchising chains, antitrust, wages, salaries

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1 Introduction

Franchise no-poach clauses ("no-poaches") are provisions of the standard contracts between franchisors, national chains with recognizable consumer brands, and franchisees, local retailers or operators that conduct the business associated with the national brand. Such clauses prohibit the franchisee from hiring workers employed by other franchisees in the national network, or by the franchisor. In July 2017, Alan Krueger and Orley Ashenfelter released a working paper (Krueger and Ashenfelter, 2017) reporting that 58 percent of 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, the attorney general (AG) of Washington State began an investigation into the prevalence of franchise no-poaches among chains with significant presence in the state, including their legality under state and federal antitrust law. The investigation yielded results: starting in July 2018, a total of 239 chains entered into legally binding "Assurances of Discontinuance" (AODs), committing to remove no-poach provisions from future franchising contracts and not to enforce those contained in existing contracts. The AODs bind chains throughout the United States, not only in the state of Washington. The last of these AODs was signed in February 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* (Krueger and Ashenfelter, 2022), including a postscript recounting the Washington AG's enforcement campaign. That postscript notes, "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 use employer-identified job ads from Burning Glass Technologies (BGT) and salary reports from Glassdoor (GD) (together, "the microdata") to execute a staggered difference-in-differences design, which recovers the effect of removing franchise no-poach provisions on pay. The setting lends itself to this approach in several respects: chains entered into AODs at different times during the enforcement effort, but 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 heterogeneous treatment effects (Goodman-Bacon, 2021; Baker, Larcker and Wang, 2021). To avoid this problem, we use the estimator of Borusyak, Jaravel and Spiess (2024). Our preferred specification estimates that by the end of the study period, the AODs caused pay to increase by roughly 6 percent in the BGT microdata and 4 percent in the GD microdata.[1]

We are also able to separately estimate treatment effects for jobs that pay an annual salary versus an hourly wage. In the franchising context on which we focus, the former are likely to be unit-manager or supervisor positions, while the latter are more likely line-worker positions. Estimated treatment effects are larger for annual salary workers than for hourly wage workers and exhibit very different post-treatment dynamics. These differences may shed light on the application of alternative models of imperfect labor market competition to different segments of what is generally a low-earning workforce.

Lafontaine, Saattvic and Slade (2023) also estimate the effect of removing franchise no-poach provisions, focusing on the restaurant industry during the period 2014–2019.[2] That paper argues that broad antitrust enforcement, including but not limited to the Washington AG's campaign, brought the effectiveness of franchise no-poaches to an end. As a consequence, the paper's methodology differs from ours. It compares all franchise fast-food chains that had a no-poach provision in place as of 2016 to other fast-food employers, which were either non-franchised or did not have a no-poach in place. Those authors

[1]See Figure 1(C)-1(D). Averaging over all post-AOD time periods, estimated effects are 6.0 percent in BGT and 2.3 percent in GD.

[2]Additional discussion of Lafontaine, Saattvic and Slade (2023) appears in Appendix B.

report sizeable positive effects after 2016 on fast-food worker earnings at franchise employers that formerly used no-poaches.

The most direct precedent for this paper arises from outside the franchising context: Gibson (2024) uses Glassdoor data to study the Department of Justice's enforcement campaign against secret no-poaching agreements among Silicon Valley employers. The paper finds that the no-poaching agreements reduced worker pay by an average of 5.6 percent. Compared to Gibson (2024), the current study differs in several important respects. First, it covers a broad set of industries—such as health care, clothing retail, tax preparation, and real estate—employing many low-earning workers, rather than a single industry employing largely high-earning workers. Second, the geographic scope of our study is nationwide, while technology firms cluster in high-income metropolitan areas. Third, this study examines explicit contractual clauses. While they were not widely publicized prior to Krueger and Ashenfelter (2017), the franchise no-poach clauses we study were not deliberately unwritten secrets, as the Silicon Valley agreements were. Secrecy is relevant because worker behavior (e.g., bargaining, job search) may depend on available information.

More broadly, this paper joins 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, 2024), mergers (Prager and Schmitt, 2021; Arnold, 2021; Guanziroli, 2022; Compton, Farag and Steinbaum, 2024), employer conduct (Johnson, Lavetti and Lipsitz, 2023; Lipsitz and Starr, 2022; 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, 2023), frictions affecting worker mobility (Schubert, Stansbury and Taska, 2024), 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; Roussille and Scuderi, 2023).[3] Our paper contributes to this literature by combining U.S.-wide, multi-industry scope with quasi-experimental variation in labor market competition.

The remainder of the paper proceeds as follows: Section 2 provides background on the franchising business model and its use of no-poach restraints. Section 3 introduces the data and explains our methodology for estimating the effect of the Washington AG's enforcement campaign. Section 4 reports empirical results. Section 5 discusses their implications. Section 6 concludes.

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. The relationship between franchisors and franchisees has historically been subject to regulation, albeit of decreasing onerousness since the 1970s (Callaci, 2021). The Federal Trade Commission's Franchise Rule obliges franchisors to disclose the provisions of the franchising contract to potential franchisees in advance of their agreeing to it, in the form of a franchise disclosure document. Substantive regulation of the franchising relationship (as opposed to the current federal disclosure-only regime) historically focused on the output market. That franchising terms could affect the balance of power in the labor market is a relatively novel focus of academic and policy interest.

The legality of franchise no-poaches has been contested since they came to light. The Washington AG took the position that multiple employers agreeing not to hire workers employed by one another constituted naked market division and was hence *per se* illegal.

[3] Sokolova and Sorensen (2021) conduct a meta-analysis of this literature, and Card (2022) reflects on the paradigm shift in labor economics this literature represents.

That is, the mere fact of the agreement was sufficient to adjudicate its illegality. The Department of Justice took the view that a franchise no-poach is a vertical restraint, like all the others in the franchising relationship, and hence subject to antitrust's Rule of Reason. This means that liability requires the parties to the agreement to possess market power in a relevant antitrust market and that anti-competitive harm may be traded off against pro-competitive efficiencies (e.g., a better-trained workforce), or alternatively, the anti-competitive effect of the restraint may be ancillary to a legitimate business purpose.

The AG's enforcement campaign began shortly after the release of Krueger and Ashenfelter (2017). All chains with a presence in Washington for which the AG found no-poaching language in their franchise disclosure document ultimately signed AODs. Only one chain, Jersey Mike's, filed a motion to dismiss the AG's lawsuit. In rejecting the motion, the court credited 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 afterward (Rao, 2020).

The AODs imposed a legally binding commitment on each chain not to enforce no-poach provisions going forward, to remove those provisions from future franchising contracts, and to notify affiliated franchisees that the no-poach no longer binds them. Norlander (2023) shows that franchise disclosure documents became much less likely to include no-poaches following the Washington AG's enforcement campaign. No notification of workers was required, and the signatories did not admit liability or pay retrospective damages. The fact that workers were not directly informed of the enforcement campaign or the AODs colors the interpretation of our empirical findings, as discussed in Section 4.

Starting in 2017, private litigation seeking damages proceeded on the basis that franchise no-poaches are vertical. In *Deslandes v. McDonalds*, a U.S. district court 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: "It is undisputed that, within three miles of Deslandes's home were two McDonald's restaurants and between

42 and 50 other quick-serve restaurants. Within ten miles of Deslandes's home were 517 quick-serve restaurants. Accordingly, Deslandes cannot plausibly allege that defendants had market power in the relevant market within which she sold her labor" (*Deslandes v. McDonald's Corp.* , 2022). That ruling was overturned by the 7th Circuit Court of Appeals in 2023 (*Deslandes v. McDonald's Corp.* , 2023), sending the case back to district court, likely to trial. In 2023, the state of Minnesota banned the use of franchise no-poaches. Hence this paper's findings remain relevant to ongoing policy and litigation. To date, any pay-suppressing effects of franchise no-poaches have not been compensated. Franchise chains that were not investigated and/or did not enter into an AOD (e.g., those without a presence in the state of Washington) retain the ability to use such clauses outside Minnesota.

3 Empirical Approach

The timing of the enforcement campaign and the conclusion of each chain-specific investigation with an AOD motivate our staggered difference-in-differences research design. 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 A.1 lists all treated franchise chains and corresponding AOD dates, illustrating the scope of the enforcement campaign. Examples include McDonald's (fast food), Jackson Hewitt (tax preparation), Expedia CruiseShipCenters (travel), European Wax Center (personal care), Hertz (car rental), and Weichert Real Estate Affiliates.

We employ three different control groups. In the **same-industry sample**, the control group consists of all employers who *advertised at least one job (BGT) or employed at least one worker (GD)* in an industry in which the treated chains were active. The **inverse sample** consists of all employers who *did not advertise any jobs (BGT) or employ any workers (GD)* in the industries where the treated chains were active.[4] Table A.2 lists industries in these two

[4]In BGT, employers and industries can be mapped on a one-to-many basis. An employer is considered

samples for GD, while Table A.3 does the same for BGT.[5] Finally, the **unconnected sample** consists of the union of same-industry and inverse sample employers, minus employers who are ever observed to employ the same worker as a treated chain. The unconnected sample can only be constructed in GD since we do not observe worker flows in BGT.

3.1 Summary Statistics

Tables A.4, A.5, and A.6 report summary statistics for the same-industry, inverse, and unconnected samples, respectively. Of the 239 chains that concluded AODs with the Washington State AG, 223 (93 percent) are represented in the same-industry BGT sample and 186 (78 percent) in the same-industry GD sample. In both same-industry and inverse samples, we treat observed pay identically regardless of whether the pay period is an hour, a year, or another period. The BGT microdata report all pay as annual salaries.[6] For job ads that post an hourly wage, BGT computes the annual salary assuming full-time work, regardless of the actual hours worked in the job. The BGT microdata also report whether the underlying job ad posts an hourly wage or an annual salary (or, in rare cases, the pay at some other frequency). In Section 4.3 we estimate separate regressions for jobs reporting hourly wages versus annual salaries. The GD microdata report pay at hourly, monthly, or annual frequency. To facilitate comparison with BGT results, we annualize sub-annual GD reports assuming full-time work.

The evaluation period extends from January 2008 through December 2021 using GD data, and from January 2017 through December 2021 using BGT data. There is a large increase in the number of observations starting in early 2018 in the BGT microdata. That is due to the increased prevalence of wage posting in job vacancies from state policies and other causes (Stahle, 2023), as well as the incorporation of new job boards with a higher

active in a given industry if at least 1 percent of its job ads are assigned the corresponding industry code.

[5] Industry names are not comparable across the two datasets, as GD uses its own industry classification and BGT uses NAICS4.

[6] Sometimes BGT reports a range, in which case we use the midpoint as the corresponding annual salary.

prevalence of posted wages. A lengthier BGT pre-treatment period would not add many observations relative to the large number of additional fixed effects required. Appendix B provides evidence that this increase in the number of posted-salary observations in the BGT microdata does not bias our results.

3.2 Data Quality

The BGT and GD microdata complement each other, as their strengths and weaknesses differ. BGT pay is as posted in a job advertisement. BGT data are administrative in the sense that they are posted by firms rather than recalled by workers, avoiding concerns around worker misreporting and selection of workers into reporting. The principal weakness of BGT data is that they do not record the pay actually received by workers due to bargaining, strategic manipulation by employers to induce applications, or other causes.

The most comprehensive evaluation of the BGT data is Hazell and Taska (2020). The paper shows that some occupations are over-represented in BGT, relative to the CPS, but this over-representation is time-invariant, and nearly all 6-digit SOC codes are covered.[7] Additionally Hazell and Taska (2020) regress CPS state-quarter log means on the corresponding BGT log means using the split-sample IV method of Angrist and Krueger (1995). The coefficient on the BGT mean is estimated with high precision, and the paper fails to reject a null hypothesis that it is one. Hazell and Taska (2020) also compare BGT wages to average new-hire earnings from the Quarterly Workforce Indicators (administrative data) and find a strong correspondence.[8] Batra, Michaud and Mongey (2023) raise concerns about the infrequency of wage posting in online job vacancies, especially when forming inferences on firm-specific pay. However, their findings relate to 2017 and earlier, before the aforementioned increased frequency. Furthermore, their example of econometric bias that infrequent wage posting can introduce does not apply in our set-

[7]This corroborates an earlier finding in Hershbein and Kahn (2018).

[8]More evidence on the representativeness of BGT data appears in Deming and Kahn (2018) and Azar et al. (2020).

ting since we do not assign treatment based on firm-level pay. Peer-reviewed studies including Hershbein and Kahn (2018), Forsythe et al. (2020), Clemens, Kahn and Meer (2021), and Acemoglu et al. (2022) have relied on BGT data.

The GD microdata are reported by workers. Their strength is that they record received (actual) worker pay. Their principal weakness is that they are potentially vulnerable to bias from misreporting and selection. However, several papers have evaluated GD data and found they correspond well to other high-quality data sets. Karabarbounis and Pinto (2018) compare GD data to the Quarterly Census of Income and Wages and the Panel Study of Income Dynamics. The paper finds industry-level mean salaries are highly correlated (0.87 and 0.9, respectively) with GD. Martellini, Schoellman and Sockin (2023) compare GD to the U.S. Department of Education's College Scorecard, which derives from tax data. The authors find the distribution of differences between the two data sources "is symmetric, centered near zero, and has small tails" (Martellini, Schoellman and Sockin, 2023). Similarly, Sockin (2022) compares industry-occupation means across GD and the CPS Annual Social and Economic Supplement, finding a correlation of 0.92. Peer-reviewed studies including Green et al. (2019), Marinescu, Skandalis and Zhao (2021), and Sockin and Sojourner (2023) have relied on GD data.

3.3 Staggered Difference-in-Differences Estimation

Following Borusyak, Jaravel and Spiess (2024), we model outcomes using the following equation

$$\log w_{ijoct} = \text{AOD}_{jt}\Gamma'_{it}\theta + \alpha_{oj} + \beta_{ot} + \delta_{ct} + \epsilon_{ijoct} \tag{3.1}$$

where $\log w_{ijoct}$ is log annual earnings for job i in occupation o at chain or employer j in local area c in calendar quarter t. AOD_{jt} indicates whether chain j was subject to an AOD in calendar quarter t. $\Gamma = \mathbb{I}_{N_1}$ is the identity matrix of dimension N_1, the number of treated observations, and Γ_{it} is the vector from that matrix corresponding to observation it. Using the identity matrix implies that treatment effects are not restricted. θ is a vector of observation-specific treatment effects. The parameters α_{oj}, β_{ot}, and δ_{ct} are fixed effects for chain (or employer)-by-occupation, occupation-by-calendar-quarter, and geographic location-by-calendar quarter, respectively.[9]

The elements of θ are obtained from the "imputation" estimator of Borusyak, Jaravel and Spiess (2024). Intuitively, for each treated observation, the untreated potential outcome is estimated using a variant of Equation 3.1 with the first (AOD) term omitted. The values of remaining parameters are estimated using only untreated observations (never-treated and not-yet-treated units), and these form the basis of the counterfactual. The treatment effect corresponding to a given observation is simply the difference between the observed, treated potential outcome and this estimated counterfactual. This approach avoids bias from the interaction of heterogeneous treatment effects and staggered treatment timing. Identifying assumptions are discussed in Section 3.5 below.

Our estimand of interest is the "target" $\tau_w = w_1^{'}\Gamma\theta$, where all elements $w_{it} = 1/N_1$, so τ_w is the average effect of treatment on the treated (ATT).[10] This may be interpreted as the average percentage change in pay after a chain enters into an AOD. We also consider event studies where individual treatment effects are averaged to form a separate ATT τ_{wh} within each horizon (event-time period) h, with $w_{it} = 1[K_{it}=h]/N_{1h}$ and K_{it} equal to the number of periods since the AOD.

Standard errors are clustered at the chain/employer level throughout the paper. For estimation we use the `did_imputation` package, which implements the method of Borusyak, Jaravel and Spiess (2024) in Stata. Corresponding event study plots are generated using `event_plot`, a graphical package by the same authors.

[9] That is, all locations within a chain are grouped together. Non-chain businesses are not grouped.

[10] This notation largely follows Equation (4) of Borusyak, Jaravel and Spiess (2024).

3.4 Implementation

As outlined in Equation (3.1), our specifications include fixed effects for employer or franchise chain by occupation, occupation by calendar quarter, and location by calendar quarter. The microdata are pooled by calendar quarter in order to ensure a sufficient number of observations in each period to estimate saturated specifications. Hence, chains whose AODs are dated within the same calendar quarter are grouped together into a treatment cohort. There are seven treatment cohorts in total, starting with 2018Q3 and ending with 2020Q1. In the BGT microdata, the study period begins in 2017Q1; in GD it begins in 2008Q1.

The Borusyak, Jaravel and Spiess (2024) estimator requires that the same set of fixed effects is identified by both control observations and the complete set of observations. Intuitively, if the counterfactual for a treated observation involves a parameter that cannot be estimated using only control observations, then that counterfactual cannot be imputed. In our setting this can occur when a given employer-occupation occurs in the treatment group but not in the control group. To avoid the problem, we restrict all of our samples based on a minimum employer-occupation cell size. The needed restrictions are quite modest. In the BGT microdata the minimum employer-occupation cell size is one observation (no restriction) for the same-industry sample and three observations for the inverse sample. In the GD microdata the minimum employer-occupation cell size is two observations for both the same-industry and inverse samples. The Borusyak, Jaravel and Spiess (2024) requirement also motivates our use of 4-digit SOC occupations (BGT) and general occupation (GD)[11] in the fixed effects γ_{oj} and δ_{ot}. Finer occupations lead to larger minimum employer-occupation cell sizes. Nonetheless in Appendix A we show our results are robust to the use of 6-digit SOC occupations (BGT) and specific occupation (GD).

All specifications using the BGT microdata define geographic locations based on com-

[11]Glassdoor calls this the "major Glassdoor occupational classification."

muting zones.[12] The GD specifications define locations based on U.S. states, as this is the finest resolution available for all respondents.

3.5 Identifying Assumptions

Attaching a causal interpretation to our difference-in-differences estimates requires familiar assumptions: 1) no anticipation; 2) common trends in untreated potential outcomes, conditional on covariates; and 3) the stable unit treatment value assumption (SUTVA).[13] More concretely, in our setting the common trends assumption requires that pay at treated chains (which signed AODs) would have evolved in parallel with pay at control employers had the Washington AG's office never launched its enforcement campaign. Equation (3.1) allows us to evaluate assumptions 1 and 2 indirectly in the usual way: by estimating pre-treatment differences between treated and control employers. The resulting estimates are discussed in Section 4; they are consistent with common trends and no anticipation.

The SUTVA requires the absence of spillovers. In our setting, control-group pay is assumed not to respond to the AODs. This assumption could be violated, for example, if control-group employers considered treated-chain pay in their own pay-setting processes. We evaluate SUTVA empirically in two ways. First, in both BGT and GD samples we estimate effects of the first wave of AODs on chains that did not sign AODs, relative to other untreated employers. Non-zero estimates potentially reflect spillovers. Second, we use the unconnected sample. If the AODs produced positive spillovers to connected control-group employers (who employ the same workers as treated chains), we expect the unconnected sample estimates to be larger in magnitude than those from the same-industry sample. As discussed in Section 4.2 below, we find some evidence of spillovers, and this motivates our use of the inverse sample (in which control employers come from

[12] The raw data include county identifiers, which allow aggregation to the commuting zone level.

[13] In Borusyak, Jaravel and Spiess (2024), both common trends and SUTVA are implied by Assumption 1', which states that the expected value of the untreated potential outcome $Y(0)$ is given by the model with treatment excluded. In our setting this is $E[Y(0)_{ijoct}] = \alpha_{oj} + \beta_{ot} + \delta_{ct}$.

industries without any treated employers).

4 Results

4.1 Baseline Results and Initial Robustness

Figure 1 presents event-study estimates based on Equation (3.1). Shaded bands represent 95 percent confidence intervals. Panels (A) and (C) use BGT data, while panels (B), (D), and (F) use GD data. Within a dataset, the treatment group is identical or nearly so, but the control group differs across the same-industry, inverse, and unconnected samples.[14] Consistent with the no-anticipation and common-trends assumptions, pre-treatment estimates are never statistically significant against a zero null hypothesis, nor do they exhibit obvious trends. Exact numerical pre-treatment estimates and associated standard errors appear in Table 1.

In Figure 1(A), same-industry-sample BGT estimates show an immediate pay increase of roughly 5 percent in the first quarter following an AOD. Estimates in subsequent quarters range from 3 percent to nearly 10 percent, but there is no clear trend. Inverse-sample BGT estimates in panel (C) are broadly similar but larger, peaking near 15 percent rather than 10 percent. Pooled ATT estimates are reported in panels (A) and (C): 5.1 percent using the same-industry sample and 6.0 percent using the inverse sample. Both estimates are statistically significant at the 1 percent level.

In panel (B) of Figure 1, same-industry-sample GD estimates begin trending upward two quarters after an AOD. They rise to roughly 3 percent by the seventh quarter in event time. The inverse-sample GD estimates in panel (D) are similar, but the upward trend begins one quarter after an AOD, and estimates stabilize at a higher level, near 4 percent. The unconnected-sample GD estimates in panel (F) are strongly similar to those

[14] In GD data the treatment group is identical across all three samples. In BGT data 4 of 223 treated chains from the same-industry sample are absent from the inverse sample because of the employer-occupation cell size restriction described in Section 3.4.

in (D). The larger ultimate magnitudes from the inverse and unconnected samples are potentially consistent with positive spillovers in the same-industry-sample control group (Section 4.2 discusses spillovers in greater depth). Pooled ATT estimates are 1.8 percent using the same-industry sample, 2.3 percent using the inverse sample, and 1.9 percent using the unconnected sample. All three estimates are statistically significant at the 1 percent level.

The different post-treatment dynamics of BGT and GD estimates in Figure 1 are unsurprising given the construction of these datasets. Because BGT captures the flow of new job ads, posted salaries can respond immediately to market changes. The smaller magnitudes in GD relative to BGT plausibly arise 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. If not all pay reported to GD had adjusted to the AODs by the end of our study period, then our GD estimates likely represent lower bounds on long-term causal effects.

Taken together, BGT and GD estimates are consistent with substantial employer market power. By the end of our analysis, the AODs agreed between chains, and the Washington AG increased posted pay (BGT) by roughly 5–6 percent and reported pay (GD) by 3–4 percent. Broadly similar results are obtained using same-industry employers, other-industry employers (inverse sample), and unconnected employers. The question of which control group should be preferred is addressed in Section 4.2, which follows directly.[15]

4.2 Spillovers to Untreated Employers

As discussed in Section 3.5, it is natural to ask whether the AODs affected pay set by employers who were not treated. The econometric concern is a violation of SUTVA, in the form of spillovers from treated to untreated units. In principle, spillovers could have

[15] See Appendix C for estimates restricting the treatment period to the pre-pandemic.

a positive or negative sign: if franchisees in a given chain started competing with one another in response to an AOD, that might have increased demand for workers at other chains and increased the pay those employers had to offer. If that were the case, the results reported in Section 4.1 would be biased downward. On the other hand, if removing the no-poach caused franchisees to shift their hiring from workers at other employers to those employed by rival franchisees in the same chain, that could have reduced demand for "outside" workers and reduced the pay their employers needed to offer. If that were true, the results in Section 4.1 would be biased upward.

In order to test empirically for spillovers, we construct a placebo treatment: franchise chains that did not enter into AODs are coded as treated in 2018Q3, i.e., alongside the first cohort of treated chains.[16] This placebo group is motivated by the intuition that franchise employers may be closer competitors than non-franchise employers. The control group is either the remainder of the same-industry sample, the entirety of the inverse sample, or the remainder of the unconnected sample. If we estimate a non-zero treatment effect from this placebo procedure, that is consistent with spillovers from the AODs onto pay at untreated rival employers.[17] This is not a sharp test, as non-zero estimates could also arise from shocks specific to franchises, as opposed to independent employers or unitary chains.

Figure 2 reports the results of estimating this placebo specification graphically. GD placebo estimates from all samples are positive and sometimes statistically significant, though substantially smaller in magnitude than their counterparts in Figure 1. BGT placebo estimates are positive, but small and not statistically significant. Together these placebo results suggest that franchise chains that did not enter into AODs might have had to increase their pay in response to increased labor-market competition. This implies that the Washington AG's enforcement campaign affected pay and welfare for workers

[16] Franchise chains that did not sign AODs are identified using the dataset in Callaci et al. (2023).

[17] If spillovers are substantial, they could affect both groups in our placebo exercise, but chain employers are plausibly more exposed to spillovers.

not only at chains that entered into AODs, but more broadly in labor markets where franchise employers participate.

The pattern of results in Figure 2 suggests that the control groups for which the SUTVA assumption is better satisfied are probably the inverse and unconnected samples. This is plausible given that both worker flows and output markets are proxies for closeness in the labor market and therefore exposure to spillovers. Because the inverse and unconnected samples yield similar results in GD data, and because the inverse sample is available in both BGT and GD data, we prefer the inverse sample. The same-industry sample results underestimate the change in pay because some of the control units are affected by the treatment. That inference is consistent with the larger treatment effects estimated using the inverse sample (Figure 1).

4.3 Results by Pay Frequency

Last among our empirical analyses, we estimate Equation (3.1) separately for hourly wage jobs and annual salary jobs. Inverse-sample results are shown in Figure 3.[18] An interesting temporal pattern emerges. In the BGT microdata, pay at jobs offering an annual salary increases approximately 10 percent in the year after treatment. The effects diminish thereafter for the remainder of the post-treatment period, so the ATT is 5.3 percent. Hourly wages, on the other hand, increase steadily over the post-treatment period, for an ATT of 4.5 percent. In the GD microdata we do not see the same difference in hourly and annual treatment dynamics, but that is to be expected given the lagging nature of GD means (previously discussed).[19]

One potential explanation for the different treatment dynamics for annual salary versus hourly wage workers in BGT data is that workers were not notified about the AODs or the binding commitment that no-poaches would not be enforced. Additionally it is un-

[18] Same-industry-sample results appear in Figure A.1. These figures use annualized pay as the dependent variable, irrespective of the whether the underlying pay is hourly or annual.

[19] Note that the vertical scale in Figure 3(D) differs from those of other figures using GD data.

likely workers were aware of the franchise no-poaches in the first place, since they were contained in franchising contracts to which workers are not parties. Franchisees notified about the non-enforcement of no-poaches might well have responded to the AODs by actively recruiting managers, who are likely to be salaried, from other franchisees in the same chains, generating the immediate pay 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. This kind of trial-and-error information diffusion would have resulted in slower realization of treatment effects on hourly workers. The Figure 3 estimates from both datasets are consistent in that the ATT for annual salary workers is larger in magnitude than 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 the magnitude of the treatment effect estimates 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-status workers with greater occupational specificity in their skill profile are more sensitive to variation in labor market competition than wages for lower-status workers. This finding is consistent with the theory proposed by Berger, Herkenhoff and Mongey (2022) that the wedge between wage and marginal product is larger for higher-paid workers in monopsonized labor markets with worker heterogeneity. By contrast, the wage posting model of Burdett and Mortensen (1998) and its derivatives predicts that the lowest-paid workers suffer the largest monopsonistic markdowns. 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 with greater scope to bargain. This is 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 labor market competition matters more for bargaining than it does for wage posting. The attenuated and delayed treatment effect for hourly wage jobs may reflect that wage posting is a better model of the labor markets for hourly workers in service industries, where the channel by which the removal of franchise no-poaches would operate is to increase the arrival rate of outside job offers—something that may take time to materialize.

Furthermore, our finding that a quasi-exogenous increase in labor market competition appears to benefit higher-earning workers more contrasts with studies of labor standards that tend to find the lowest-earning workers benefit most from raising the floor (e.g., Dube (2019)). This contrast suggests that two different types of labor market policy interventions—labor standards and labor market competition enforcement—are distinguished by their distributional impact.[20] This is an area ripe for further investigation, given current attention to both labor standards and policy-driven variation in labor market competition.[21]

In June 2022, *Deslandes v. McDonalds* was dismissed on the grounds that McDonald's does not possess labor market power and hence its no-poaching provision could not have been anti-competitive. Our findings—that entering into a legally binding commitment not to make use of franchise no-poaches leads to an increase in chain-specific pay—may be interpreted as confirming both the labor market power of franchise employers and the anti-competitive effect of franchise no-poaches, and hence lend support to the 7th Circuit decision that overturned the lower court's dismissal of the case.

[20]This is not to say that the minimum wage is irrelevant to labor market competition—there is strong evidence that increasing the minimum wage reduces the scope for employers to exercise monopsony power, e.g., McPherson et al. (2024).

[21]See, for example, U.S. Department of the Treasury (2022).

6 Conclusion

We evaluate the impact of the Washington State attorney general's franchise no-poach enforcement campaign. The campaign secured nationwide, legally enforceable agreements (Assurances of Discontinuance [AODs]) from most franchise chains that had previously made use of no-poach provisions not to make use of them 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 earnings by 6 percent in the Burning Glass Technologies microdata and approximately 4 percent in the Glassdoor microdata.[22] We find differences in treatment-effect magnitude and timing between jobs that pay an annual salary and those that pay an hourly wage. The former experience an immediate increase in wages. Wage effects for hourly workers take longer to materialize, and when they do, the increases are smaller.

[22]The latter refers to the GD point estimates at the right of Figure 1(D).

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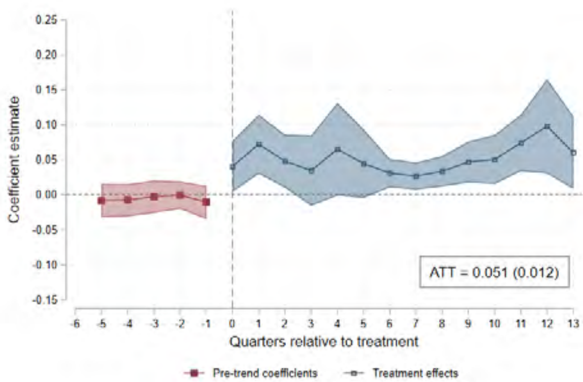
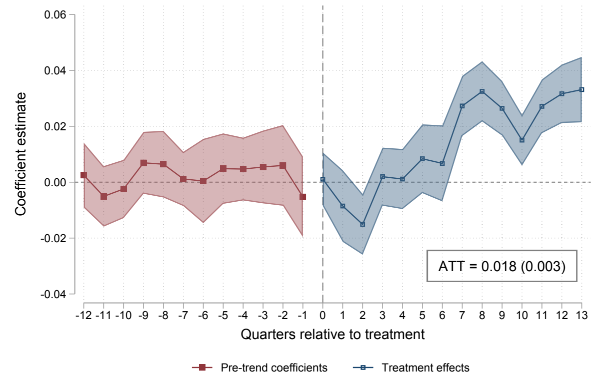
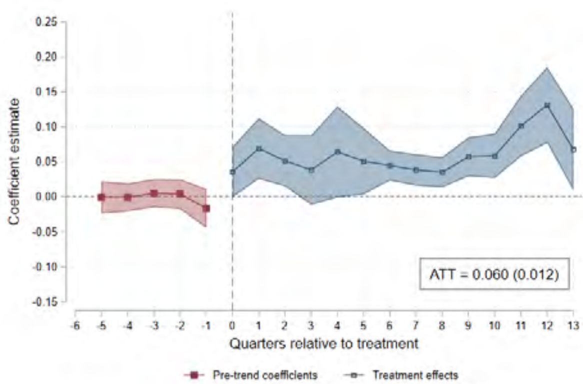
Figure 1. Event study estimates, same-industry and inverse samples. Dots are estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific elements of θ in Equation (3.1). The left column employs both same-industry and inverse samples in the BGT microdata. The right does the same in the GD microdata, with the addition of the unconnected sample. The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level.



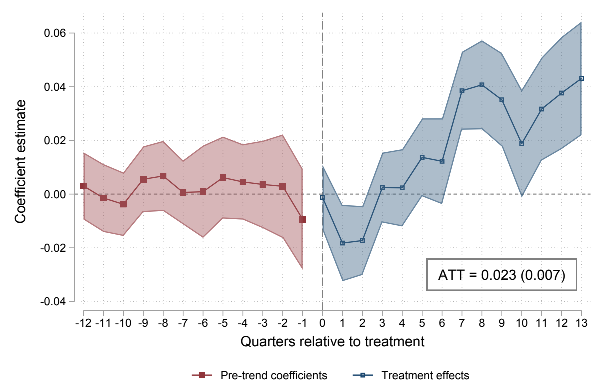
(A) Same-Industry BGT Sample



(B) Same-industry GD Sample



(C) Inverse BGT Sample



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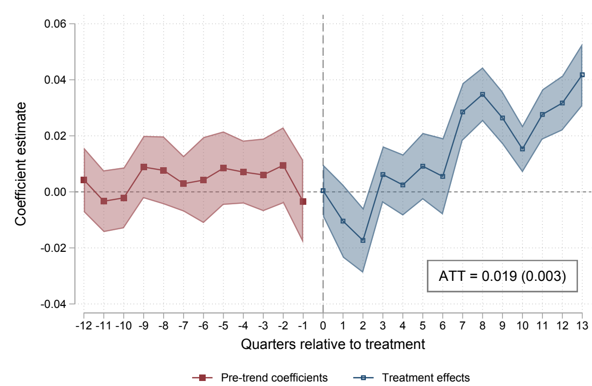
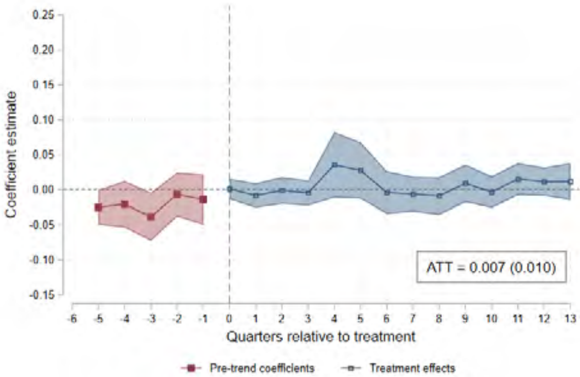


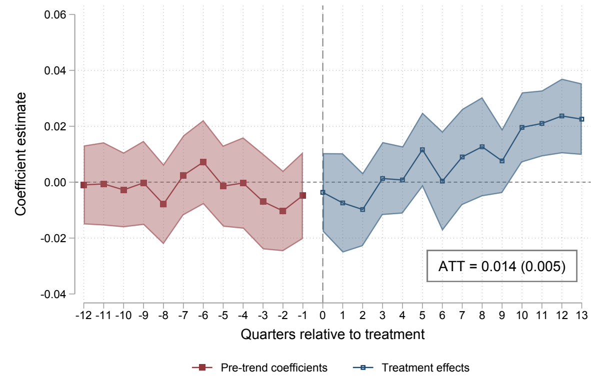
(E) Unconnected BGT Sample (N/A)

(F) Unconnected GD Sample

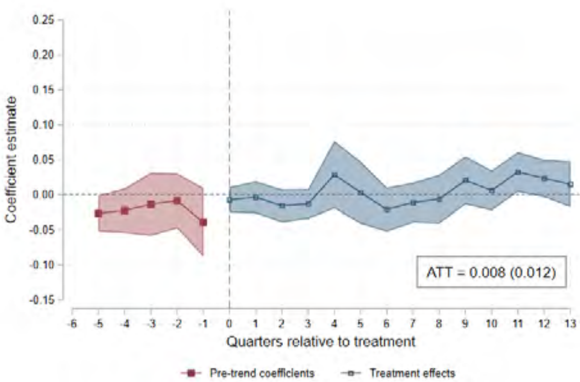
Figure 2. Placebo treatment of non-AOD chains in 2018Q3. Dots are estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific placebo effects for chains that did not enter into AODs, coded as though they were treated in 2018Q3. The control group is either the remainder of the same-industry sample (top row), the inverse sample (middle row), or the remainder of the unconnected sample (bottom row). The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level.



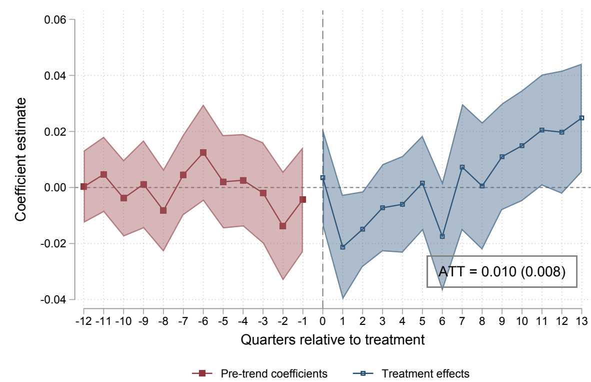
(A) Same-Industry BGT Sample



(B) Same-Industry GD Sample

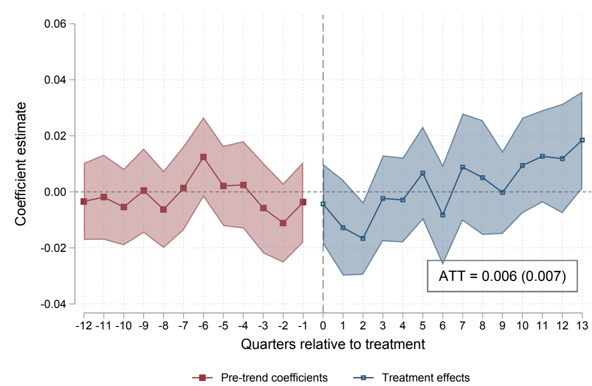


(C) Inverse BGT Sample



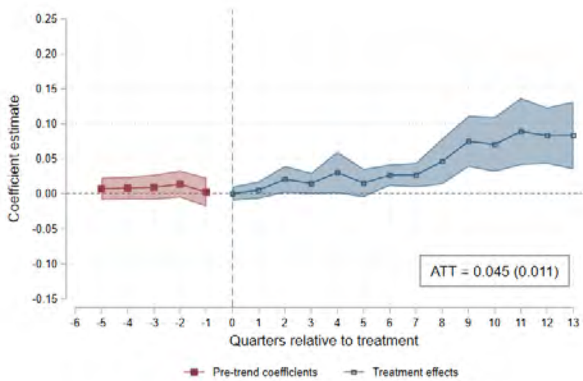
(D) Inverse GD Sample

(E) Unconnected BGT Sample (N/A)

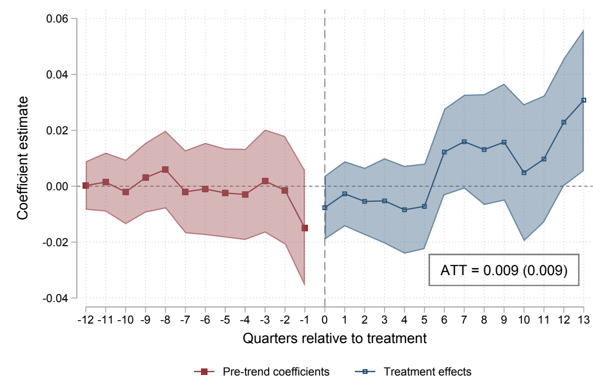


(F) Unconnected GD Sample

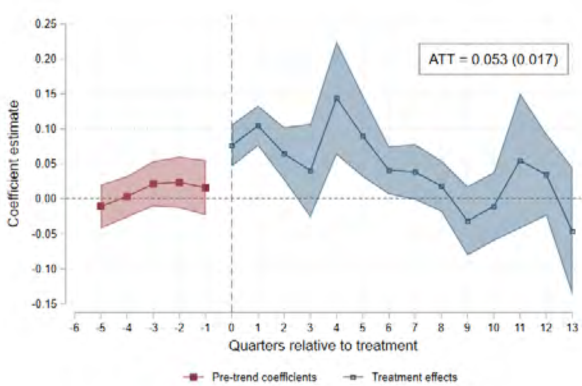
Figure 3. Event study estimates, annual salary versus hourly wage workers, inverse sample. Dots are estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific elements of θ in Equation (3.1), by pay frequency, for the inverse sample in 1) the BGT microdata (left column) and in 2) the GD microdata (right column). The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level. Note that the vertical scale in panel (D) differs from that of other panels based on Glassdoor data.



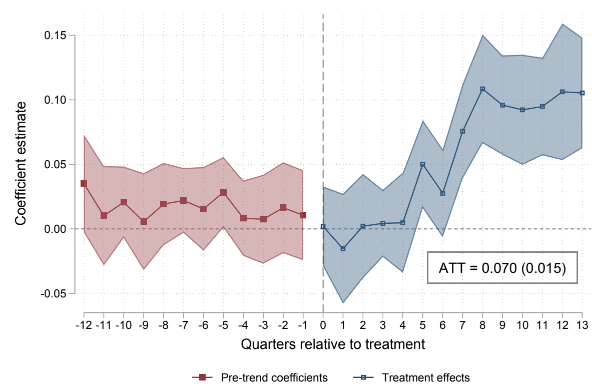
(A) Hourly Wage Workers, BGT



(B) Hourly Wage Workers, GD



(C) Annual Salary Workers, BGT



(D) Annual Salary Workers, GD

Table 1. Event study estimates, full, inverse, and unconnected samples, BGT and GD microdata. Quarter-relative-to-treatment coefficients τ_{wh} are unweighted averages over observation-specific elements of θ in Equation (3.1), for each sample and dataset. Coefficients on dummies for negative event time ($\tau < 0$) allow tests for pre-treatment trend differences. Columns (1) and (2) present the results for the BGT full and inverse samples, respectively. Columns (3), (4), and (5) present the results for the GD full, inverse, and unconnected samples, respectively. The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. SEs are clustered at the chain/employer level.

	(1)	(2)	(3)	(4)	(5)
	Ln(real pay)	Ln(real pay)	Ln(real pay)	Ln(real pay)	Ln(real pay)
	BGT	BGT	GD	GD	GD
	Same-industry	Inverse	Same-industry	Inverse	Unconnected
$\tau = -12$			0.003	0.003	0.004
			(0.006)	(0.006)	(0.006)
$\tau = -11$			-0.005	-0.001	-0.003
			(0.006)	(0.006)	(0.006)
$\tau = -10$			-0.002	-0.004	-0.002
			(0.005)	(0.006)	(0.006)
$\tau = -9$			0.007	0.006	0.009
			(0.006)	(0.006)	(0.006)
$\tau = -8$			0.006	0.007	0.008
			(0.006)	(0.007)	(0.006)
$\tau = -7$			0.001	0.001	0.003
			(0.005)	(0.006)	(0.005)
$\tau = -6$			0.000	0.001	0.004
			(0.008)	(0.009)	(0.008)
$\tau = -5$	-0.009	-0.001	0.005	0.006	0.008
	(0.012)	(0.012)	(0.006)	(0.008)	(0.007)
$\tau = -4$	-0.008	-0.001	0.005	0.005	0.007
	(0.012)	(0.010)	(0.006)	(0.007)	(0.006)
$\tau = -3$	-0.003	0.005	0.005	0.004	0.006
	(0.012)	(0.010)	(0.007)	(0.008)	(0.007)
$\tau = -2$	-0.001	0.004	0.006	0.003	0.009
	(0.010)	(0.011)	(0.007)	(0.010)	(0.007)
$\tau = -1$	-0.011	-0.017	-0.005	-0.009	-0.003
	(0.012)	(0.014)	(0.007)	(0.010)	(0.008)
$\tau = 0$	0.040	0.035	0.001	-0.001	0.000
	(0.019)	(0.019)	(0.005)	(0.006)	(0.005)
$\tau = 1$	0.072	0.069	-0.009	-0.018	-0.010

	(0.022)	(0.022)	(0.007)	(0.007)	(0.007)
$\tau = 2$	0.048	0.051	-0.015	-0.017	-0.017
	(0.019)	(0.019)	(0.006)	(0.007)	(0.006)
$\tau = 3$	0.034	0.038	0.002	0.002	0.006
	(0.026)	(0.025)	(0.005)	(0.007)	(0.005)
$\tau = 4$	0.065	0.064	0.001	0.002	0.002
	(0.034)	(0.033)	(0.005)	(0.007)	(0.006)
$\tau = 5$	0.044	0.051	0.008	0.014	0.009
	(0.025)	(0.024)	(0.006)	(0.007)	(0.006)
$\tau = 6$	0.031	0.045	0.007	0.012	0.006
	(0.010)	(0.011)	(0.007)	(0.008)	(0.007)
$\tau = 7$	0.026	0.038	0.027	0.038	0.029
	(0.010)	(0.011)	(0.005)	(0.007)	(0.005)
$\tau = 8$	0.033	0.035	0.032	0.041	0.035
	(0.011)	(0.011)	(0.006)	(0.008)	(0.005)
$\tau = 9$	0.047	0.057	0.026	0.035	0.026
	(0.015)	(0.014)	(0.005)	(0.009)	(0.005)
$\tau = 10$	0.050	0.058	0.015	0.019	0.015
	(0.018)	(0.016)	(0.005)	(0.010)	(0.004)
$\tau = 11$	0.074	0.101	0.027	0.032	0.028
	(0.021)	(0.022)	(0.005)	(0.010)	(0.005)
$\tau = 12$	0.098	0.131	0.032	0.038	0.032
	(0.035)	(0.028)	(0.005)	(0.011)	(0.005)
$\tau = 13$	0.060	0.067	0.033	0.043	0.042
	(0.027)	(0.030)	(0.006)	(0.011)	(0.006)
Observations	16,886,816	4,306,504	5,760,895	3,060,317	3,265,322
Pre-treatment F	0.525	1.000	0.901	0.746	0.676
Pre-treatment p	0.758	0.416	0.545	0.707	0.776

Appendices

A Additional Figures and Tables

This appendix includes exhibits that supplement the main text of the paper. In order, those exhibits are:

1. Table A.1 gives the names and settlement dates for all the franchise chains that entered into AODs with the Washington AG.

2. Tables A.2 and A.3 report the industries that comprise the same-industry and inverse samples for GD and BGT, respectively.

3. Tables A.4, A.5, and A.6 report summary statistics for the same-industry, inverse, and unconnected samples, respectively.

4. Figure A.1 reports event study estimates by pay frequency in which the same-industry sample is the control group (as opposed to Figure 3, which does that for the inverse sample).

5. Figure A.2 reports event study results when using finer occupational categories than in Figure 1.

Table A.1. List of AODs. Data are from the Office of the Attorney General, Washington State.

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 Car	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
Papa 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	UBuildIt	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
La 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 A.2. Industries in GD microdata. Column (1) reports industries from the GD same-industry sample, in which treatment and control employers participate in the same industries, in order of frequency. Column (2) reports control-group industries from the GD inverse sample in order of frequency. GD uses its own industry classification rather than a standard one like the NAICS. The inverse-sample list is not exhaustive, as GD data contain a very large number of industries.

GD same-industry-sample industries	GD inverse-sample control industries
Health Care Services & Hospitals	Computer Hardware Development
Restaurants & Cafes	Banking & Lending
Department, Clothing & Shoe Stores	Internet & Web Services
Information Technology Support Services	Enterprise Software & Network Solutions
Business Consulting	General Merchandise & Superstores
Advertising & Public Relations	Grocery Stores
Investment & Asset Management	Transportation Equipment Manufacturing
Consumer Product Manufacturing	Architectural & Engineering Services
HR Consulting	Wholesale
Home Furniture & Housewares Stores	Health Care Products Manufacturing
Machinery Manufacturing	Broadcast Media
Taxi & Car Services	Publishing
Accounting & Tax	Research & Development
Real Estate	Beauty & Personal Accessories Stores
Hotels & Resorts	Financial Transaction Processing
Food & Beverage Manufacturing	Film Production
Electronics Manufacturing	Security & Protective
Construction	Chemical Manufacturing
Other Retail Stores	Airlines, Airports & Air Transportation
Beauty & Wellness	Sporting Goods Stores
Shipping & Trucking	Preschools & Child Care Services
Consumer Electronics & Appliances Stores	Pet & Pet Supplies Stores
Sports & Recreation	Colleges & Universities
Building & Personnel Services	Metal & Mineral Manufacturing
Drug & Health Stores	Video Game Publishing
Vehicle Dealers	Gambling
Food & Beverage Stores	Membership Organizations
Education & Training Services	Travel Agencies
Culture & Entertainment	Pet Care & Veterinary
Car & Truck Rental	Media & Entertainment Stores
Office Supply & Copy Stores	Software Development
Primary & Secondary Schools	Gift, Novelty & Souvenir Stores
Catering & Food Service Contractors	Beauty & Wellness
Convenience Stores	Rail Transportation
Automotive Parts & Accessories Stores	Wood & Paper Manufacturing
Toy & Hobby Stores	Photography
Vehicle Repair & Maintenance	Farm Support
Crop Production	Staffing & Subcontracting
Commercial Equipment Services	Parking & Valet
Consumer Product Rental	Auctions & Galleries
General Repair & Maintenance	Stock Exchanges
Commercial Printing	Audiovisual

Table A.3. Industries in BGT microdata. Column (1) reports industries from the BGT same-industry sample, in order of frequency. Column (2) reports control-group industries from the BGT inverse sample in order of frequency. Industry names correspond to NAICS4 categories. Both columns are restricted to the top 40 industries.

BGT full-sample industries	**BGT inverse-sample industries**
Restaurants & Other Eating Places	Electronic Shopping & Mail-Order Houses
General Medical & Surgical Hospitals	Investigation & Security Services
Colleges, Universities, & Professional Schools	Administration of Human Resource Programs
Executive, Legislative, & Other Gen'l Gov't Support	Couriers & Express Delivery Services
General Freight Trucking	Department Stores
Insurance Carriers	Justice, Public Order, & Safety Activities
Traveler Accommodation	Automobile Dealers
Elementary & Secondary Schools	Wireless Telecommunications Carriers
Business Support Services	Postal Service
National Security & International Affairs	Computer Systems Design & Related Services
Services to Buildings & Dwellings	Administration of Economic Programs
Depository Credit Intermediation	Used Merchandise Stores
Grocery Stores	Motor Vehicle Manufacturing
Management, Scientific, & Technical Consult. Serv.	Grocery & Related Product Merchant Wholesalers
Home Health Care Services	Automotive Parts, Accessories, & Tire Stores
Offices of Physicians	Pharmaceutical & Medicine Manufacturing
Other Amusement & Recreation Industries	Lessors of Real Estate
Child Day Care Services	Activities Related to Credit Intermediation
Activities Related to Real Estate	Psychiatric & Substance Abuse Hospitals
Offices of Real Estate Agents & Brokers	Aerospace Product & Parts Manufacturing
Other Professional, Scientific, & Technical Services	Scheduled Air Transportation
Individual & Family Services	Administration of Environmental Quality Programs
Building Equipment Contractors	Specialty (exc. Psychiatric/Substance Abuse) Hospitals
Offices of Other Health Practitioners	Oil & Gas Extraction
Clothing Stores	Waste Treatment & Disposal
Offices of Dentists	Social Advocacy Organizations
Legal Services	Water, Sewage & Other Systems
Scientific Research & Development Services	Waste Collection
Other General Merchandise Stores	Other Ambulatory Health Care Services
Automotive Repair & Maintenance	Shoe Stores
Health & Personal Care Stores	Medical Equipment & Supplies Manufacturing
Junior Colleges	Semiconductor & Other Component Manufacturing
Continuing Care Retirement & Assisted Living	Community Food & Housing, & Emergency / Other Relief
Automotive Equipment Rental & Leasing	Securities & Commodity Contracts, Intermediation & Brokerage
Personal Care Services	Household Appliances Merchant Wholesalers
Architectural, Engineering, & Related Services	Other General Purpose Machinery Manufacturing
Cable & Other Subscription Programming	Disability, Mental Health, & Substance Abuse Facilities
Software Publishers	Drycleaning & Laundry Services
Religious Organizations	Fruit/Vegetable Preserving & Specialty Food Manufacture
Building Material & Supplies Dealers	School & Employee Bus Transportation

Table A.4. Same-industry sample summary statistics, BGT and GD microdata. This table reports summary statistics for the same-industry sample described in Section 3 for both BGT and GD microdata.

	Treatment group (same-ind. GD sample)	Control group (same-ind. GD sample)	Treatment group (same-ind. BGT sample)	Control group (same-ind. BGT sample)
Number of chains/employers	186	175,796	223	1,169,579
Number of observations (total)	113,220	5,647,675	745,733	16,141,083
Number of observations (avg per chain/emp)	609	30,364	3,344	14
Salary (2015 USD): average	31,577	60,860	33,359	50,628
Salary (2015 USD): P10	18,412	24,157	19,337	22,525
Salary (2015 USD): P25	21,622	31,605	22,553	27,984
Salary (2015 USD): P50	26,535	49,382	27,738	38,433
Salary (2015 USD): P75	33,691	78,423	37,311	60,710
Salary (2015 USD): P90	50,131	115,198	52,606	92,862
Share of hourly wage observations (%)	0.77	0.45	0.63	0.44

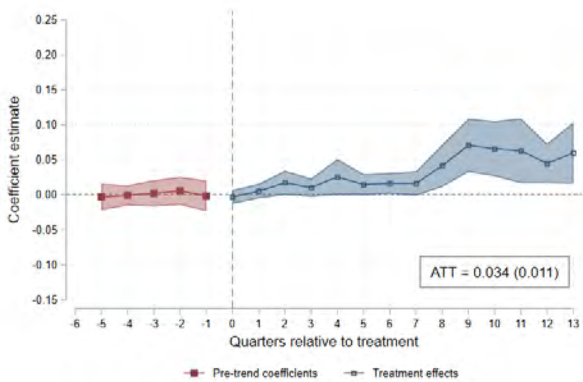
Table A.5. Inverse sample summary statistics, BGT and GD microdata. This table reports summary statistics for the inverse sample described in Section 3, for both BGT and GD microdata.

	Treatment group (inverse GD sample)	Control group (inverse GD sample)	Treatment group (inverse BGT sample)	Control group (inverse BGT sample)
Number of chains/employers	186	39,789	219	28,299
Number of observations (total)	113,220	2,947,097	739,712	3,566,792
Number of observations (avg per chain/emp)	609	15,845	3,378	126
Salary (2015 USD): average	31,577	71,875	33,369	48,728
Salary (2015 USD): P10	18,412	26,141	19,338	24,988
Salary (2015 USD): P25	21,622	35,454	22,561	29,751
Salary (2015 USD): P50	26,535	58,175	27,741	35,159
Salary (2015 USD): P75	33,691	96,848	37,311	55,536
Salary (2015 USD): P90	50,131	140,902	52,617	90,588
Share of hourly wage observations (%)	0.77	0.37	0.63	0.57

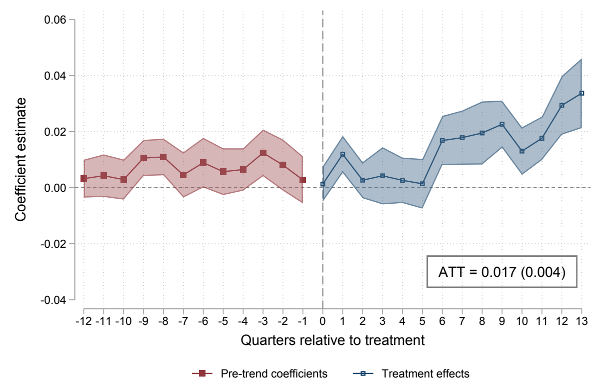
Table A.6. Unconnected sample summary statistics, GD microdata. This table reports summary statistics for the unconnected sample described in Section 3, for the GD microdata.

	Treatment group (unconnected GD sample)	Control group (unconnected GD sample)
Number of chains/employers	186	205,834
Number of observations (total)	113,220	3,152,102
Number of observations (avg per chain/emp)	609	16,947
Salary (2015 USD): average	31,577	64,896
Salary (2015 USD): P10	18,412	26,888
Salary (2015 USD): P25	21,622	36,378
Salary (2015 USD): P50	26,535	54,304
Salary (2015 USD): P75	33,691	82,175
Salary (2015 USD): P90	50,131	119,072
Share of hourly wage observations (%)	0.77	0.39

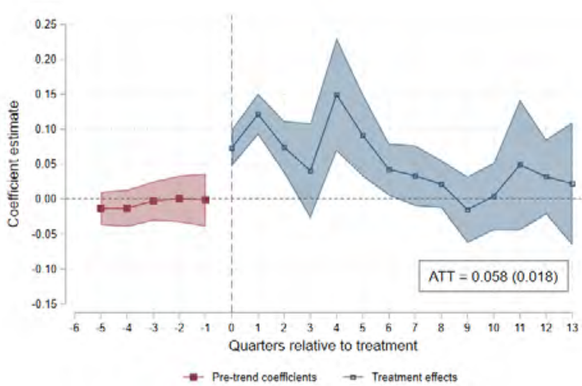
Figure A.1. Event study estimates, annual salary versus hourly wage workers, same-industry sample. Dots are estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific elements of θ in Equation (3.1), by pay frequency, for the same-industry sample in the BGT microdata (left column) and the GD microdata (right column). The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level.



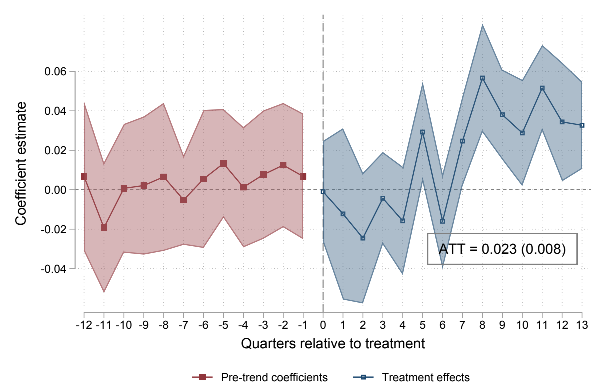
(A) Hourly Wage Workers, BGT



(B) Hourly Wage Workers, GD



(C) Annual Salary Workers, BGT



(D) Annual Salary Workers, GD

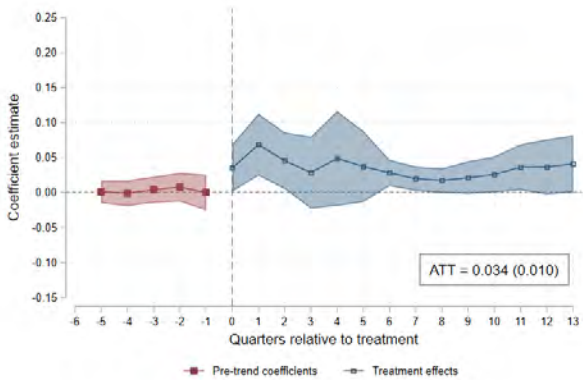
Results with Finer Occupational Categories

Section 4.1 shows the robustness of our results to the choice of sample: both same-industry and inverse samples yield positive, practically meaningful estimates. It remains to evaluate robustness to the choice of specification. Equation (3.1) already employs high-dimensional fixed effects. However it is possible to go further by using more detailed occupational categories: six-digit SOC codes (BGT) and specific occupation (GD).[23] Figure A.2 shows that in the same-industry sample, both BGT (panel A) and GD (panel B) results are similar to our primary results when using controls based on more detailed occupations. Using more detailed occupations with the inverse sample requires limitation of the sample to large employer-occupation cells.[24] Estimation is possible in BGT only with a minimum cell size of 152 observations, shown in panel (C). Estimation is possible in GD with a minimum employer-occupation cell size of 47. Figure A.2 panel (D) presents these GD inverse-sample estimates. The change in sample means that comparisons with our other results are not straightforward. Having emphasized that caveat, the inverse-sample GD point estimates in panel (D) are large and positive, peaking above 5 percent in the last two quarters in event time.

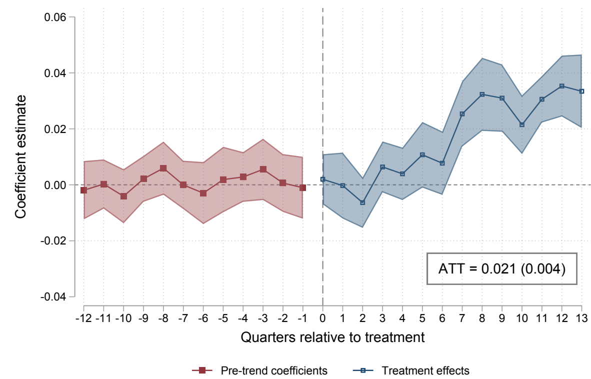
[23] Glassdoor's term is "Glassdoor occupational category."

[24] The estimator of Borusyak, Jaravel and Spiess (2024) requires that the same fixed effects are identified in the control sample and the same-industry sample. In our setting, allowing small employer-occupation cells frequently violates this condition.

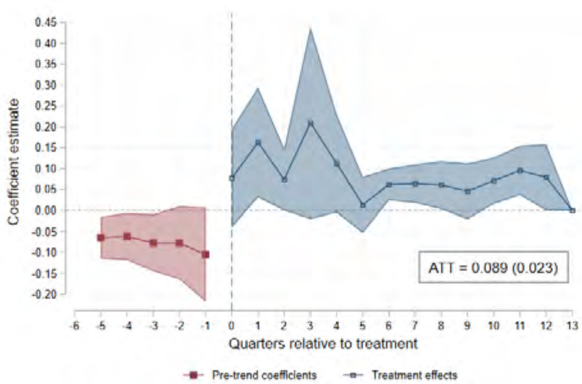
Figure A.2. Event study estimates, same-industry and inverse samples, detailed occupations. Dots are estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific elements of θ in Equation (3.1), for the BGT microdata (left column) and the GD microdata (right column). The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Specifications differ from Figure 1 in employing occupation controls based on SOC-6d (BGT) and specific occupation (GD). Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level. Inverse-sample BGT results with SOC-6d codes (panel C) require a very large minimum cell size (152), which requires dropping over half of the sample; as a result, note that the vertical scale in panel (C) differs from that of other panels based on BGT data.



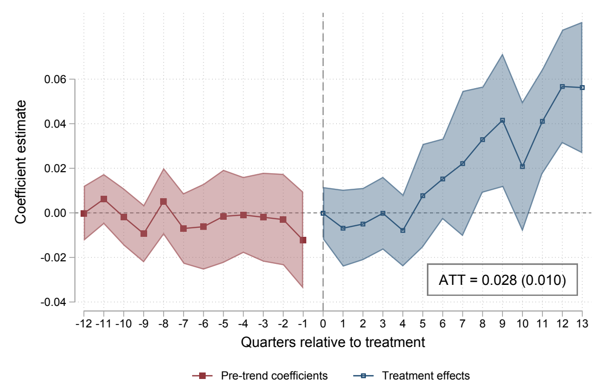
(A) Same-Industry BGT Sample



(B) Same-Industry GD Sample



(C) Inverse BGT Sample



(D) Inverse GD Sample

B Imputed Salaries in BGT Microdata

The BGT microdata consist of digitized online job vacancies, some of which report posted salaries. We use those posted salaries as an outcome variable of interest. The number of job ads that include posted salaries in the BGT microdata increased significantly starting in 2018, as we describe in Section 3.1. Stahle (2023) reports on the wider underlying trend: in part thanks to state regulations mandating posting pay in job advertisements, posting pay has become much more widespread since 2018, especially in managerial occupations.

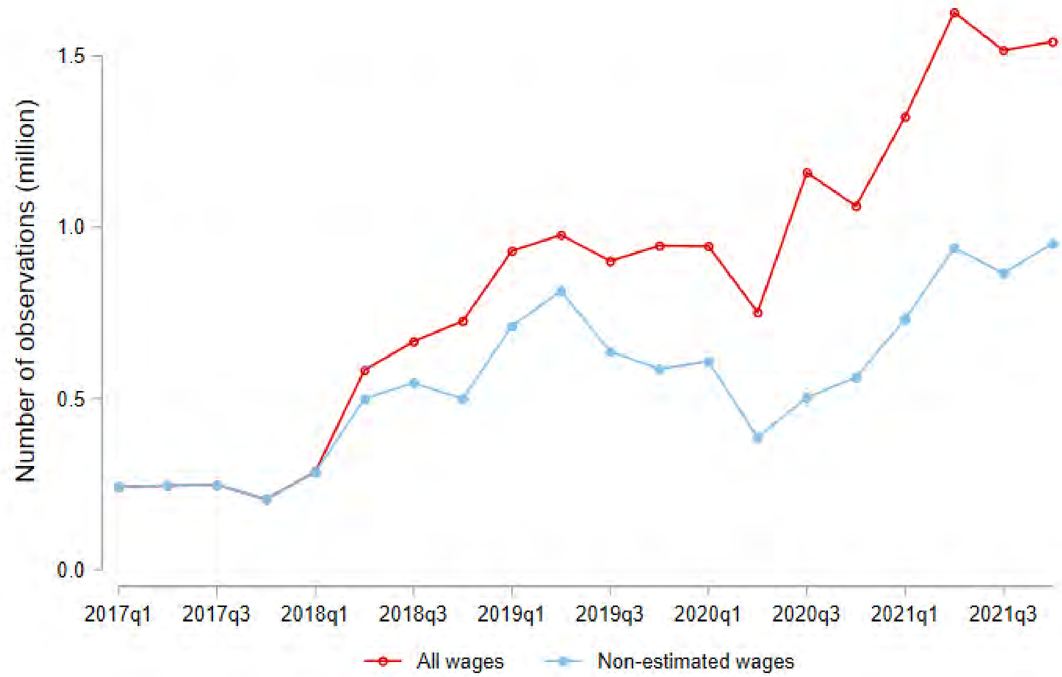
Lafontaine, Saattvic and Slade (2023) point out that some of the salaries reported at the job ad level in the BGT microdata may not actually be stated by the employer posting the job ad, but rather imputed from similar employers and/or similar jobs.[25] They argue that biases our estimates of the effect of the Washington AG's enforcement campaign in the following way: if the salary imputation includes job ads posted by similar employers/franchise chains that either did not have a no-poach provision or did not enter into an AOD (or both), and those non-AOD chains paid more on average (as we show in Callaci et al. 2023), then we may erroneously interpret converging post-treatment pay observations between AOD and non-AOD chains as reflecting a treatment effect, as opposed to a mechanical effect of imputing salaries and therefore chain-specific pay that is not in fact chain-specific. We address those concerns in this appendix.

Specifically, following an analysis of which salaries reported in BGT are likely to be imputed (e.g., because the text of the job ad does not mention pay, but the salary variable is populated), we concluded that the most reliable indicator of imputed salary is whether the job ad was sourced from the online job boards LinkedIn or Indeed. We therefore drop *all* the job ads sourced from LinkedIn or Indeed from the reestimates of Equation 3.1. This procedure is over-inclusive, since not all job ads sourced from those boards have imputed salaries.

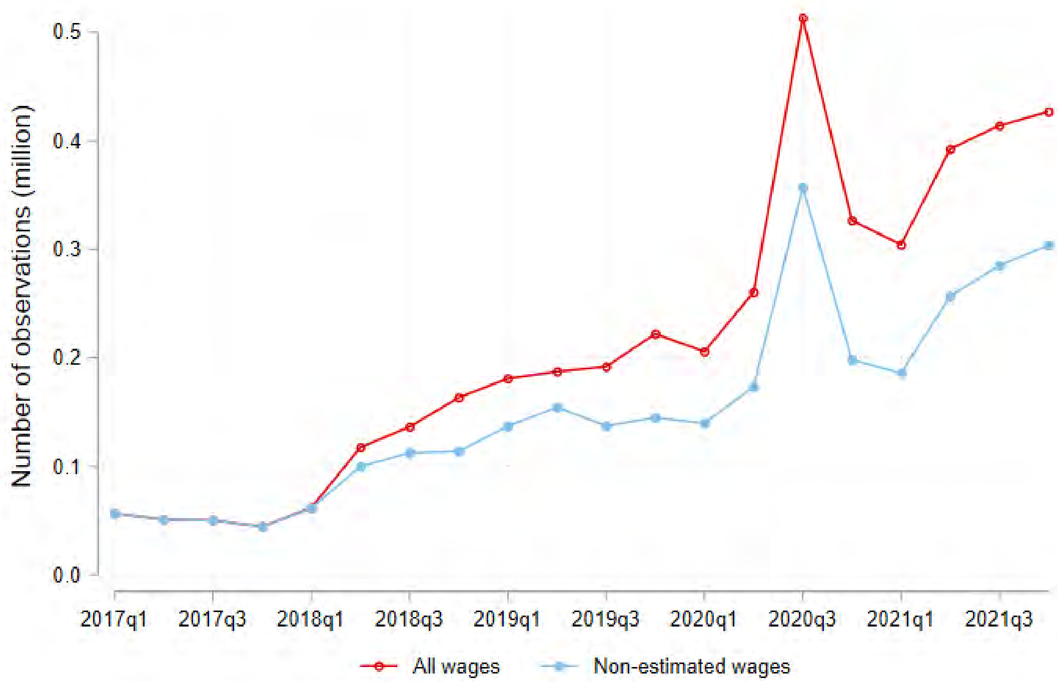
[25] Two iterations of our study were posted to SSRN prior to Lafontaine, Saattvic and Slade (2023). Relevant dates appear on the SSRN pages for the respective papers.

Figure B.1 plots the time series of the observation count before and after dropping the job ads sourced to LinkedIn and Indeed, for both the same-industry and inverse samples. Figure B.2 reports the event-study figures from that procedure and compares them to the baseline estimates from Figure 1, and Table B.1 reports the event study results for the both samples, comparing the baseline estimates to the equivalent specification dropping the imputed-salary job ads. Dropping imputed-salary job ads does not meaningfully alter our results—if anything, the treatment effect we estimate is larger in magnitude, and the difference between these results and the baseline is similar across the same-industry and inverse samples. Note also that our analyses based on Glassdoor data are not subject to any critique based on imputation.

Figure B.1. Count of BGT observations by quarter, with and without imputed salaries. We plot the observation count for the analysis period before and after implementing our rule for dropping imputed-salary observations, which is whether the job ad is sourced to LinkedIn or Indeed.

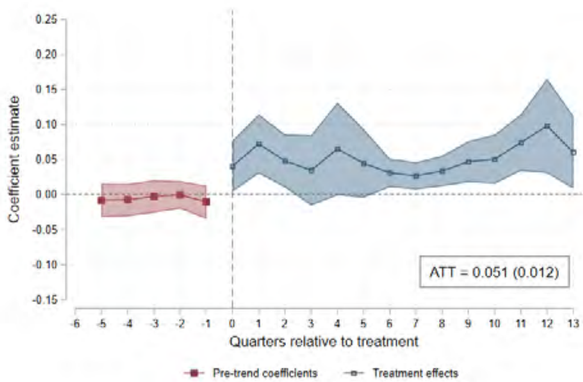


(A) Same-Industry BGT: Count of Observations

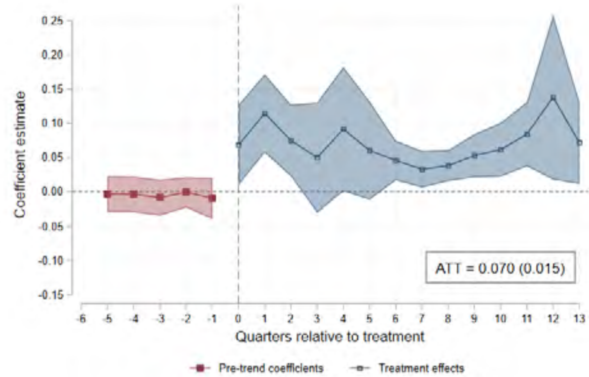


(B) Inverse BGT: Count of Observations

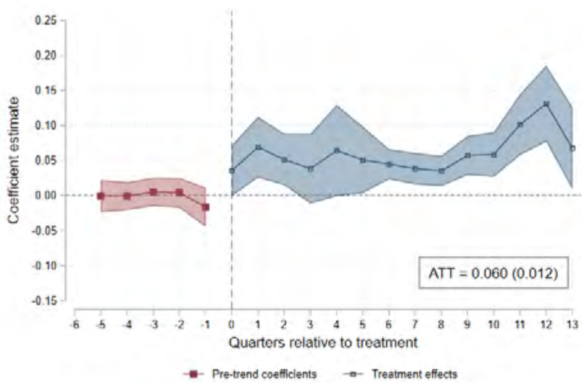
Figure B.2. BGT event study estimates, without imputed salaries. Dots are estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific elements of θ in Equation (3.1), for the BGT microdata, dropping potentially imputed salaries in the right column. The left column repeats Figures 1(A) and 1(C) for comparison. The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level.



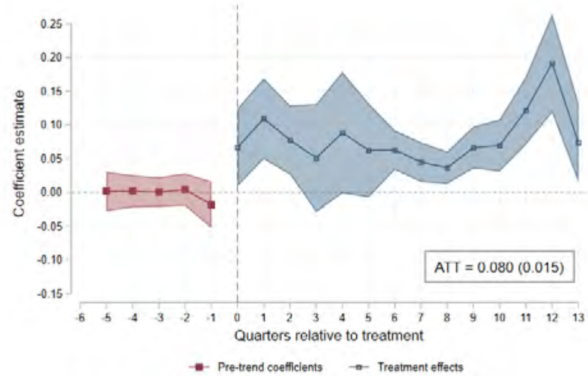
(A) Same-Industry BGT: baseline



(B) Same-Industry BGT: without imputed observations



(C) Inverse BGT: baseline



(D) Inverse BGT: without imputed observations

Table B.1. Event study estimates, omitting observations with imputed salaries (BGT data, same-industry and inverse samples). This table reports the estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific elements of θ in Equation (3.1), for each BGT sample, omitting potentially imputed salary observations in column (2) and (4). Columns (1) and (3) reproduce our primary full and inverse sample estimates, respectively, to facilitate comparison.

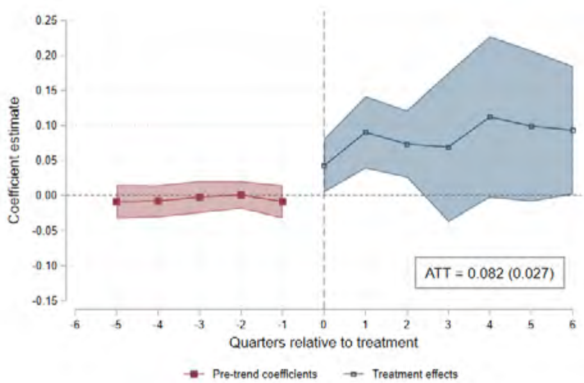
VARIABLES	(1) Ln(real pay) Same-ind. sample Baseline All observations	(2) Ln(real pay) Same-ind. sample Excluding all Indeed-Linkedin	(3) Ln(real pay) Inverse sample Baseline All observations	(4) Ln(real pay) Inverse sample Excluding all Indeed-Linkedin
$\tau = -5$	-0.009	-0.004	-0.001	0.001
	(0.012)	(0.013)	(0.012)	(0.015)
$\tau = -4$	-0.008	-0.004	-0.001	0.002
	(0.012)	(0.013)	(0.010)	(0.012)
$\tau = -3$	-0.003	-0.009	0.005	0.000
	(0.012)	(0.013)	(0.010)	(0.011)
$\tau = -2$	-0.001	-0.001	0.004	0.004
	(0.010)	(0.011)	(0.011)	(0.012)
$\tau = -1$	-0.011	-0.010	-0.017	-0.018
	(0.012)	(0.015)	(0.014)	(0.017)
$\tau = 0$	0.040	0.068	0.035	0.066
	(0.019)	(0.030)	(0.019)	(0.029)
$\tau = 1$	0.072	0.114	0.069	0.109
	(0.022)	(0.029)	(0.022)	(0.031)
$\tau = 2$	0.048	0.075	0.051	0.078
	(0.019)	(0.027)	(0.019)	(0.026)
$\tau = 3$	0.034	0.050	0.038	0.051
	(0.026)	(0.041)	(0.025)	(0.041)
$\tau = 4$	0.065	0.091	0.064	0.088
	(0.034)	(0.046)	(0.033)	(0.046)
$\tau = 5$	0.044	0.060	0.051	0.062
	(0.025)	(0.037)	(0.024)	(0.036)
$\tau = 6$	0.031	0.046	0.045	0.063
	(0.010)	(0.015)	(0.011)	(0.015)
$\tau = 7$	0.026	0.033	0.038	0.045
	(0.010)	(0.014)	(0.011)	(0.015)
$\tau = 8$	0.033	0.038	0.035	0.036
	(0.011)	(0.012)	(0.011)	(0.012)
$\tau = 9$	0.047	0.053	0.057	0.066
	(0.015)	(0.016)	(0.014)	(0.016)
$\tau = 10$	0.050	0.061	0.058	0.070
	(0.018)	(0.020)	(0.016)	(0.020)
$\tau = 11$	0.074	0.084	0.101	0.121
	(0.021)	(0.024)	(0.022)	(0.026)
$\tau = 12$	0.098	0.138	0.131	0.191
	(0.035)	(0.061)	(0.028)	(0.037)
$\tau = 13$	0.060	0.072	0.067	0.074
	(0.027)	(0.031)	(0.030)	(0.030)
Observations	16,886,816	11,069,218	4,306,504	3,062,844
Year-quarter x CZ FEs	Y	Y	Y	Y
Year-quarter x SOC-4d FEs	Y	Y	Y	Y
SOC-4d x Employer FEs	Y	Y	Y	Y
Pre-treatment F-stat	0.525	0.568	1.000	0.803
Pre-treatment p-value	0.758	0.725	0.416	0.547

C Pre-Pandemic Results

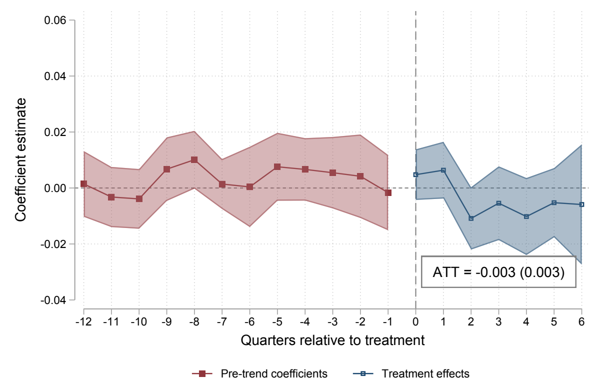
Because the AODs occurred from mid 2018 through early 2020, it is important to evaluate the influence of the COVID-19 pandemic on our estimates. In BGT data, limiting the sample to the pre-COVID period (February 2020 and earlier) yields estimated ATTs of 8.2 percent using the same-industry sample and 7.6 percent using the inverse sample. In GD data, limiting the sample to the pre-COVID period (February 2020 and earlier), estimated ATTs are -0.3 percent using the same-industry sample and -1 percent using the inverse sample. The cause of these negative pre-COVID GD ATTs is apparent from panels (B) and (D) of Figure 1. While point estimates are positive starting three quarters after an AOD, the largest estimates occur starting seven quarters after treatment. Even for the earliest wave of AODs (July 2018), quarters 7 through 13, where the largest effects are seen, occurred during the pandemic. Limiting the GD samples to pre-COVID observations discards these large positive estimates. To put the point intuitively, because average GD pay responds slowly to an AOD (as discussed previously), there is not enough time for the full magnitude of a treatment effect to appear in pre-pandemic GD data.

The pandemic also informs the interpretation of our estimates because many of the treated pay observations occurred under unusual labor market conditions, potentially highly slack in some markets and highly tight in others. It is reasonable to surmise that the effects of the AODs would have been different had the pandemic not occurred. Note that this is a question of heterogeneous treatment effects and external validity, not bias, as our control groups experienced the same unusual pandemic labor markets. Our BGT estimates show no evidence of such heterogeneous treatment effects, however; estimated ATTs are similar with and without pandemic-influenced observations. It remains possible that had the pandemic never occurred, different treatment effects would have emerged in BGT data for larger values of event time.

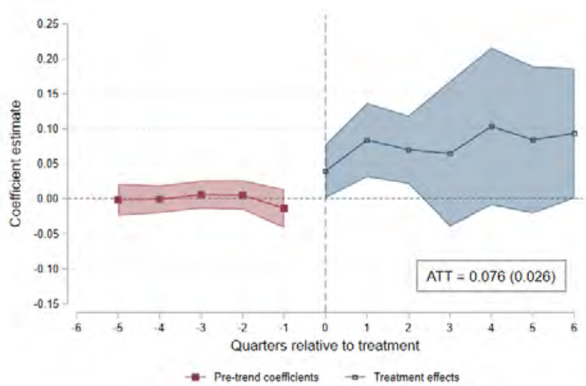
Figure C.1. Event study estimates, pre-pandemic period, same-industry and inverse samples. Dots are estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific elements of θ in Equation (3.1), for the BGT microdata (left column) and the GD microdata (right column), with the treatment period capped at February 2020. The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level.



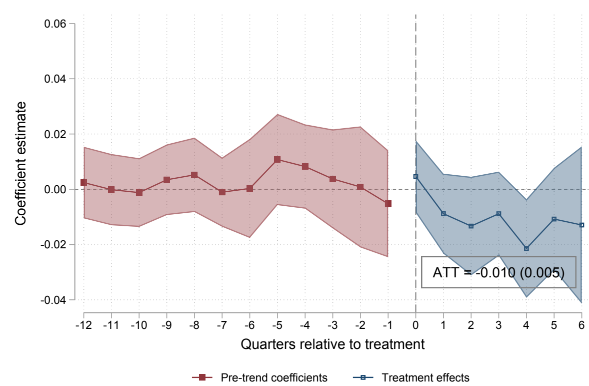
(A) Same-Industry BGT Sample



(B) Same-Industry GD Sample



(C) Inverse BGT Sample



(D) Inverse GD Sample

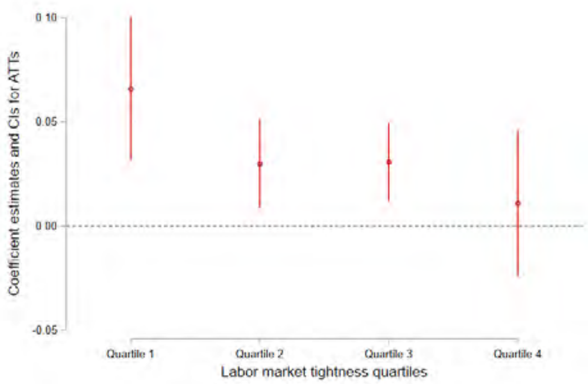
D Heterogeneity by Labor Market Tightness

One natural question that arises from the analysis contained in the main body of this paper is how the removal of franchise no-poaches interacts with larger labor market dynamics. For example, if jobs are abundant and unemployment is low in a local labor market, the ability to switch employers to a different franchisee in the same chain may matter less to wage growth than if jobs are scarce and within-chain opportunities are among the few available.

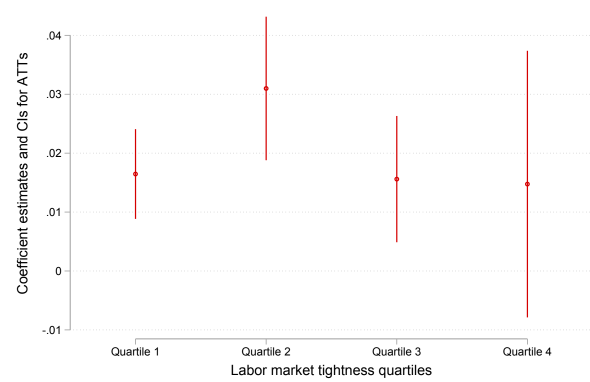
We test whether treatment effects of the AODs vary by labor market tightness, as measured by the ratio of job vacancies to unemployment. We construct tightness following Appendix E of Azar et al. (2020) by using the monthly count of job ads from 2017m1 to 2021m12 in the BGT microdata itself as our proxy for vacancies, and the count of unemployed workers by county (aggregated to commuting zones) and month in BLS Local Area Unemployment Statistics. We compute the ratio at the commuting zone-year-month level. We then assign a labor market tightness to each treated chain by merging our computed tightness estimates to the main analysis sample and taking an unweighted average tightness across each treated chain's observations (spanning both pre- and post-treatment). We rank the treated chains into four quartiles according to the average tightness of the labor markets where they hire. We then estimate Equation 3.1 separately for each of the four tightness quartiles. The ATTs are plotted in Figure D.1 for the treated-industry and inverse samples, in both the BGT and GD data.

The BGT results indicate the largest treatment effects for chains that hire in slack labor markets, indicating that when "outside" job opportunities are scarce, the ability to move to a different employer in the same chain matters more for wages and hence the effect of entering into an AOD on pay is larger. We do not observe the same robust pattern in the GD data: using both control groups, the treatment effect is largest for the 2nd-tightness-quartile chains.

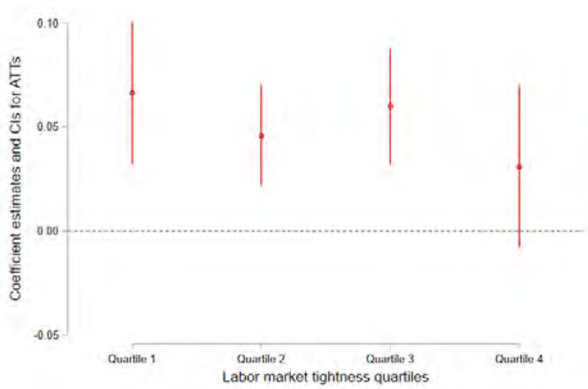
Figure D.1. Event study estimates, by labor market tightness quartile, same-industry and inverse samples. Dots are the estimated ATT, by labor market tightness quartile, for the BGT microdata (left column) and the GD microdata (right column). The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level.



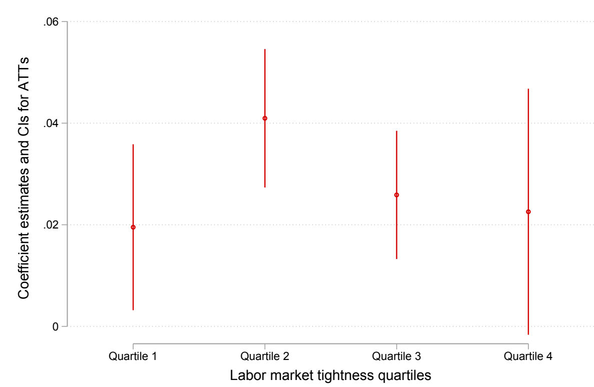
(A) Same-Industry BGT Sample



(B) Same-Industry GD Sample



(C) Inverse BGT Sample



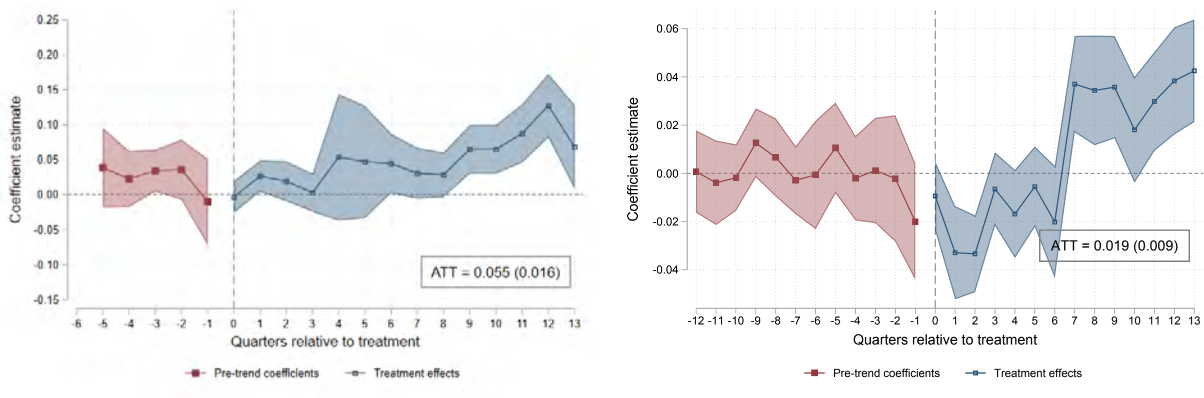
(D) Inverse GD Sample

E Results for the Restaurant Industry

Since both Krueger and Ashenfelter (2022) and Lafontaine, Saattvic and Slade (2023) focus on restaurant franchise chains in particular, in this appendix we report results for the subset of treated chains that are in the restaurant industry. We do not alter the composition of the control group relative to the main body of the paper. We estimate the effect of entering into an AOD using only treated chains in the restaurant industry.

Figure E.1 plots event study results. The overall estimated ATTs are similar to the estimates from the full treatment group. The dynamics of post-treatment coefficients estimates are somewhat different in Figure E.1(A) relative to Figure 1(C): for the restaurants-only treatment group, post-treatment coefficients start off small and increase in size in event time. This is probably due to the relatively large number of hourly wage workers in this industry, hence overall treatment effects look more like Figure 3(A) than Figure 3(C). As discussed in Section 4.3, we conjecture that treatment effects for hourly wage workers take longer to materialize because opportunities to move to a different employer in the same chain would have arrived more slowly than for annual salary workers, who are more likely to have been directly recruited in response to an AOD.

Figure E.1. Event study estimates, inverse samples, restaurant industry. Dots are estimated quarter-relative-to-treatment coefficients τ_{wh}, which are unweighted averages over observation-specific elements of θ in Equation (3.1), for the inverse sample in the BGT microdata (left column) and the GD microdata (right column). In both data sets the treatment group is limited to chains in the restaurant industry. The dependent variable is log real annual pay. Controls are chain/employer-occupation, occupation-calendar-quarter, and location-calendar quarter fixed effects. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level.



(A) Inverse BGT Sample

(B) Inverse GD Sample