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 through early 2020. Implementing a staggered difference-in-differences research design using Burning Glass Technologies job vacancies and Glassdoor salary reports, we document the nationwide effect of the enforcement campaign on pay at franchising chains across numerous industries. Our preferred specification estimates a 6.6% increase in posted annual earnings from the job vacancy data and an approximate 4% increase in worker-reported earnings. (JEL: J42, K21, L40, J31)

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1 Introduction

Franchise no-poach clauses ("no-poaches") 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. Such clauses prohibit the franchisee from hiring workers currently or recently employed by other franchisees in the national network.[1] In July 2017, Alan Krueger and Orley Ashenfelter released a working paper (Krueger and Ashenfelter, 2017) reporting 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 (AG) 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. The investigation quickly 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. That is, the chains did not have unilateral discretion to resume using no-poaches. The 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 postscript recounting the Washington AG's enforcement campaign. That postscript notes that, "In principle, be-

[1] Instead or in addition, some franchise no-poach clauses cover current and recent employees of the franchisor.

cause 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)—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. Estimates are relative to two different control groups: a "full sample" consisting of all employers in industries containing treated chains, and an "inverse sample" consisting of all employers in industries not containing treated chains. In addition to employer names, the microdata include job characteristics like occupation, variables related to geographic location, base pay, and pay period (e.g. annual or hourly).

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: 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 (2022).

In our baseline specification using the full sample and the BGT job ad microdata, we estimate that entering into an AOD caused pay to increase by 5.1% on average, relative to the control group consisting of all microdata from industries in which the treated franchising chains were active (the full sample). The equivalent finding from the GD salary reports is 1.9%. Using the inverse sample, we estimate a larger average treatment effect of 6.1% in the BGT microdata and a 2.4% effect in the GD microdata.[2] Below we discuss which sample is more likely to produce unbiased estimates.

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 probably line-worker positions. Estimated treatment effects are approximately twice as large 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 from 2014-2019.[3] 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. Lafontaine, Saattvic and Slade (2023) reports sizeable positive effects after 2016 on fast-food worker earnings at treated franchise employers (those that formerly used no-poaches).

The most direct precedent for this paper arises from outside the franchising context: Gibson (2022) 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 4.8 percent. Compared to Gibson (2022), the current study differs in several important respects. First, it covers a broad set of industries—for example health care, clothing retail, tax prepara-

[2] These estimates average over all post-treatment periods. The approximate 4% estimate from GD data mentioned in the abstract comes from the event study in panel (D) of Figure 1, which is discussed in Section 4.1.

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

tion, 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.

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 that prohibit the worker from taking employment with 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%. The franchise no-poaches we study are narrower than noncompetes in that they do not extend beyond the boundaries of the chain. Given that relative narrowness, our large estimated effects of franchise no-poaches on worker pay are striking.

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, 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 affecting 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; Roussille and Scuderi, 2023).[4] Our paper contributes to this literature by combining US-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, its use of no-poach restraints, and the history of antitrust enforcement against them. 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 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.[5] 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*). The Federal Trade Commission's Franchise Rule obliges franchisors 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). Some states further require FDDs to be filed and recorded by a state regulatory agency. Such filings form the source of the data on chain-level franchising contracts used in Section 4.2 of this paper: 530 digitized FDDs filed in Wisconsin from the year 2015, i.e. prior to the

[4]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.

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

Washington AG's enforcement campaign.[6]

Substantive regulation of the franchising relationship (as opposed to the current federal disclosure-only regime) historically focused on the allocation of decision-making power between franchisors and franchisees. In particular, regulation was concerned with 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 focus of academic and policy interest.[7] As mentioned in Section 1, 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., 2023).

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 oligopsony model of labor market competition, they increase effective employer concentration, since other franchisees in the same franchise chain are removed as a source of alternative employment for incumbent workers at any one chain that has a franchise no-poach in place. Fewer alternative employers create

[6]The criteria for inclusion are 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. (2023) for further details on the FDD data.

[7]This could be regarded as an outgrowth of the overall paradigm shift in labor economics described by Card (2022), since previously the maintained assumption was that the vast majority of labor markets are highly or even perfectly competitive, particularly for low-wage workers.

greater leeway to reduce wages below the marginal product of labor, or alternatively, in a wage bargaining model, they reduce the worker's bargaining power by reducing the threat point. Second, no-poaches reduce 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. Our paper does not attempt to evaluate these theoretical mechanisms. But 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.

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, they argued 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 a franchise no-poach clause is a vertical restraint, like all the others in a franchising relationship, and hence subject to the antitrust Rule of Reason. This means that first, antitrust liability requires the parties to the agreement to possess market power in a relevant antitrust market. Second, 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. In this setting, that legitimate purpose could be providing consumers with the standard commercial experience associated with the franchisor's brand.[8]

[8]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

The timing details of the AG's enforcement campaign are as follows. The investigation began shortly after the release of Krueger and Ashenfelter (2017) and its coverage in the New York Times in the autumn of 2017. The investigations examined chain-level FDDs to see whether they contained no-poaching language. The first settlements of 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, 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 afterward.[9]

The AODs imposed a legally binding 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 notification of 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 the interpretation of our empirical findings, as discussed in Section 4.

Starting in 2017, i.e. the year prior to 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 labor market power was

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.

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

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 (Alonso, 2022). To date, any wage-suppressing effects of franchise no-poaching clauses have not been compensated. Moreover, 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.

As mentioned in Section 1, Lafontaine, Saattvic and Slade (2023) interprets the chain of events recounted here as signifying that franchise no-poaching clauses became void across the board during the period 2016-2019. That paper therefore conducts an analysis comparing franchise restaurant employers that used no-poaches prior to 2016 to a control group of restaurant employers that did not. It does not evaluate chain-specific effects.[10] By contrast, our interpretation of these events is that the legal status of franchise no-poaches became stronger, rather than more questionable. Rulings for the defendants in the class actions and the *Deslandes* individual action make it more likely that franchise employers will escape liability for franchise no-poaching clauses in the future.[11] The AODs secured by the Washington AG are the sole reason the prevalence of franchise no-poaches declined during our study period.[12] Moreover, franchise chains that had no-poaches in place and did not enter into AODs cannot be said to have removed them, as Lafontaine, Saattvic and Slade (2023) assumes.[13] For all of these reasons, our research design employs

[10] That is, Lafontaine, Saattvic and Slade (2023) ignores the distinctions between AOD and non-AOD chains, and between investigated and non-investigated chains.

[11] The ruling by a Washington state court in favor of the AG on Jersey Mike's motion to dismiss is the sole example of a court seeming to credit the view that franchise no-poaches are illegal as of this writing. If that motion were brought in federal court now in a jurisdiction where the *Deslandes* precedent binds, the defendants would probably win.

[12] Norlander (2023) shows that FDDs became much less likely to include no-poaches following the Washington AG's enforcement campaign, in line with the requirements of the AODs.

[13] To the best of our knowledge, there is no evidence that "even those franchisors that did not drop the clauses stopped enforcing them when they realized the enforcement could trigger crippling and potentially expensive legal action" (Lafontaine, Saattvic and Slade, 2023). In light of jurisprudence to date, this conjecture is unlikely. No compensation for prior use of franchise no-poaches has yet been awarded.

a staggered chain-level treatment based on AODs, as opposed to a general treatment of all chains that used no-poaches.[14]

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. 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. 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. Any brief list of examples fails to provide an accurate sense of the scope of the AODs; we recommend browsing Table 1.

We employ two different control groups. In the **full 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 the employers who *did not advertise any jobs (BGT) or employ any workers (GD)* in the industries where the treated chains were active. Table B.1 lists industries in these two samples for GD, while Table B.2 does the same for BGT.[15]

[14]From an econometric perspective, defining all chains that had no-poaches as treated introduces measurement error (which need not be classical) into the treatment variable.

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

3.1 Summary Statistics

Table 2 reports summary statistics for the full sample, and Table 3 does the same for the inverse sample. Of the 239 chains that concluded AODs with the Washington State AG, 220 (92%) are represented in the full BGT sample and 186 (78%) in the full GD sample. In both full 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.[16] 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 incorporation of new job boards with a higher 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 A 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

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

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, e.g. after bargaining.[17] 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.[18] Additionally Hazell and Taska (2020) regresses 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.[19] 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) compares 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 (.87 and .9, respectively) with GD. Martellini, Schoellman and Sockin (2023) compare GD to the US 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,

[17] Additionally, there is the possibility that firms strategically manipulate posted wages such that they do not accurately reflect changes in received pay.

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

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

2023). Similarly Sockin (2022) compares industry-occupation means across GD and the CPS Annual Social and Economic Supplement, finding a correlation of .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-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 log annual earnings 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. The parameters 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.[20] The coefficient of interest is β, interpreted as the percentage change in pay for jobs at chain j after the chain entered into an AOD. Identifying assumptions—common trends and the stable unit treatment value assumption—are discussed in Section 3.6 below. Standard errors are clustered at the chain/employer level throughout the paper.

We use the `did_imputation` package, which implements the estimator of Borusyak, Jaravel and Spiess (2022) in Stata. This method is robust to heterogeneous treatment effects when treatment timing is staggered. In general terms, the estimator involves 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

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

and the imputed outcome (fitted value) given by the coefficient estimates from step 1.

3. Construct a weighted average of treatment effects estimated in step 2. This yields the average effect of treatment on the treated (ATT).

3.4 Event Study Specification

In addition to the staggered difference-in-difference specification that estimates a single pooled treatment effect (ATT), we implement an event-time methodology that estimates a different treatment effect for each quarter in event time. 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 before or after chain j's AOD. Estimates again come from the `did_imputation` package and the corresponding plots are generated using `event_plot`, a graphical package also from Borusyak, Jaravel and Spiess (2022).

3.5 Implementation

As outlined in equations (3.1) and (3.2), our specifications include 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 are pooled by calendar quarter in order to ensure a sufficient number of observations in each time unit 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 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.

The Borusyak, Jaravel and Spiess (2022) estimator requires that the same set of fixed effects is identified by both control observations and the complete set of observations. Meeting this condition requires us to 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 full sample and four observations for the inverse sample. In the GD microdata the minimum employer-occupation cell size is two observations for both the full and inverse samples. The Borusyak, Jaravel and Spiess (2022) requirement also motivates our use of 4-digit SOC occupations (BGT) and general occupation (GD)[21] in the fixed effects γ_{oj} and δ_{ot}. Finer occupations lead to larger minimum employer-occupation cell sizes. Nonetheless in Section 4.4 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 employ commuting zones as the empirical analog to the geographic local labor market.[22] The GD specifications use U.S. states as the analog to local labor markets, as this is the finest location available for all respondents.[23]

For our baseline specifications, the data end in 2021Q4, which means that b in equation 3.2 varies by treatment cohort: 13 quarters for the earliest-treated chains whose AODs are in 2018Q3, 7 quarters for the latest-treated chains whose AODs are in 2020Q1. We also conduct an analysis that truncates the post-treatment period in February 2020, prior to the onset of the COVID-19 pandemic in the continental United States. In that specification, b is reduced by seven quarters for every cohort.

[21]Glassdoor calls this the "major Glassdoor occupational classification."
[22]The raw data include county identifiers, which allow aggregation to the commuting zone level.
[23]Location is available at the MSA level for a subset of respondents.

3.6 Identifying assumptions

Attaching a causal interpretation to our difference-in-differences estimates requires two familiar assumptions: 1) common trends in untreated potential outcomes, conditional on covariates; and 2) the stable unit treatment value assumption (SUTVA). 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 (3.2) allows us to evaluate this assumption 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 the common trends assumption.

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 chains 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 placebo "effects" of the first wave of AODs on chains that did not sign AODs, relative to other untreated employers. Non-zero placebo estimates potentially reflect spillovers. Second, we conduct a variant of our GD analysis that excludes control employers connected to treated employers by worker flows. If the AODs produced positive spillovers to connected control-group employers, we expect the resulting estimates to be larger in magnitude than those from the full 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 industries without any treated employers).[24]

[24]The "full" and "inverse" samples were defined at the beginning of Section 3.

4 Results

4.1 Baseline Results & Initial Robustness

Figure 1 presents estimates from the event-study specification of equation (3.2). Shaded bands represent 95 percent confidence intervals. Panels (A) and (C) are based on BGT data, while panels (B) and (D) are based on GD data. Within a data set, the treatment group is identical or nearly so, but the control group differs across the full and inverse samples.[25] Consistent with the common trends assumption, 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 4 (BGT) and Table 5 (GD).

In panel (A) of Figure 1, full-sample BGT estimates show an immediate pay increase of roughly 5% in the first quarter following an AOD. Estimates in subsequent quarters range from 3% to nearly 10%, but there is no clear trend. Inverse-sample BGT estimates in panel (C) are broadly similar but larger, peaking near 15% rather than 10%. Pooled ATT estimates corresponding to panels (A) and (C) of Figure 1 appear in Table 4: 5.1% using the full sample and 6.6% using the inverse sample. Both estimates are statistically significant at the one percent level.

In panel (B) of Figure 1, full-sample GD estimates begin trending upward two quarters after an AOD. They rise to roughly 3% 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%. Pooled ATT estimates corresponding to panels (B) and (D) appear in Table 5: 1.9% using the full sample and 2.4% using the inverse sample. Both estimates are statistically significant at the one

[25] In GD data the treatment group is identical across full and inverse samples. In BGT data 6 of 220 treated chains from the full sample are absent from the inverse sample because of the employer-occupation cell size restriction described in Section 3.5.

percent level.

The different post-treatment dynamics of BGT and GD estimates in Figure 1 are unsurprising given the construction of these data sets. Because BGT captures the flow of new job ads, posted salaries can respond immediately to market changes. 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. 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.

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 8.1% using the full sample and 7.9% using the inverse sample. In GD data, limiting the sample to the pre-COVID period (February 2020 and earlier), estimated ATTs are -1.1% using the full sample and -1.7% 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.

Taken together, BGT and GD estimates are consistent with substantial employer market power. By the end of our event-time analysis, the AODs agreed between chains and the Washington AG increased posted pay (BGT) by roughly 5-6% and reported pay (GD) by 3-4%. Similar results are obtained using two non-overlapping control groups: same-industry employers (full sample) and other-industry employers (inverse sample). The question of which control group should be preferred is addressed in Section 4.2, which follows directly.

4.2 Spillovers to Untreated Employers

As discussed in Section 3.6, 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 these spillovers could have a positive or negative sign: if franchisees in a given chain started competing with one another in response to their franchisor entering into an AOD, that might have increased demand for workers at other chains and hence increased the pay those employers had to offer. If that were the case, we would expect the results reported in subsection 4.1 to 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 would have reduced demand for "outside" workers and hence reduced the pay those employers needed to offer. If that were true, we would expect the results in subsection 4.1 to be biased upward, essentially selecting the units where pay increased without taking into account that the same treatment reduced pay at other units.

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.[26] This placebo group is motivated by the intuition that chain employers may be closer to each other in the labor market than they are to non-chain employers. The control group is either the remainder of the full sample or the entirety of the inverse 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. This is not a sharp test, as non-zero estimates could also arise from shocks specific to chain (as opposed to independent) employers. If non-zero estimates arise from spillovers, we expect larger placebo magnitudes when using the inverse sample, where control firms are in other industries.[27]

Figure 2 reports the results of estimating this placebo specification graphically. GD placebo estimates from both full and inverse samples are positive and sometimes statistically significant, though smaller in magnitude than their counterparts in Figure 1. BGT placebo estimates from the full sample are very close to zero, but there is evidence of positive effects relative to the inverse-sample control group. Together these placebo results suggest that franchise chains that did not enter into AODs nonetheless had to increase their pay in response to increased competition in the labor markets where they hire. Hence, the control group for which the SUTVA assumption is better satisfied is probably the inverse sample. The full sample results underestimate the change in pay because some of the control units are in fact affected by the treatment. That inference is consistent with the fact that altogether, treatment effects estimated in the inverse sample

[26]Placebo "treated" franchise chains are those represented in our Wisconsin FDD data (described in Section 2) that did not enter into AODs.

[27]If spillovers are substantial, they may affect both groups in our placebo exercise. But spillovers to the control group may be smaller when that group is comprised of employers who do not participate in the same output markets as treated firms. If so, effects on placebo-treated chains will be larger when the counterfactual is based on the inverse sample.

have slightly larger magnitude, as shown in Figure 1.

As an additional check of spillover concerns, we take advantage of the fact that GD data provide short panels for users who submit multiple reports. This allows us to observe employer-to-employer worker flows. Employers connected to each other by worker flows are arguably labor market competitors, and control-group employers that are connected to treatment-group employers may be susceptible to spillovers. In Figure 3 we estimate AOD effects relative to a control group comprised only of employers unconnected to all treatment-group employers. That is, the control group is the union of the full- and inverse-sample control groups, but employers connected to any treatment-group firm are excluded. As elsewhere in the paper, different locations belonging to the same chain are treated as a single employer, so this definition of potentially spillover-affected employers is quite conservative. For example, if GD data include reports from the same worker at both a Roto Rooter (control) location and an I Love Kickboxing (treatment) location (however improbable this might be in life), all locations within those chains are connected under our definition, and all Roto Rooter locations are removed from the unconnected-sample control group.[28] The resulting estimates in Figure 3 are larger than their full-sample analogs (panel (B) of Figure 1), peaking near 4% rather than 3%. Like our placebo analysis, that difference is consistent with positive spillovers. The estimates of Figure 3 are strongly similar to their inverse-sample analogs (panel (D) of Figure 1). Conditional on the assumption that worker flows are a good spillover proxy, this strong similarity suggests that the inverse-sample analysis eliminates any quantitatively meaningful spillovers.

Broadly our analyses of spillovers have two important implications. First, the SUTVA is more likely to hold in our inverse-sample analyses and for that reason we prefer them to our full-sample analyses. Second, the Washington AG's enforcement campaign affected

[28]Roto Rooter is a franchise chain in the plumbing industry that did not have a no-poach provision and hence never entered into an AOD. I Love Kickboxing is a franchise chain in the fitness industry that did enter into an AOD.

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

4.3 Results by Pay Frequency

Last among our empirical analyses, we estimate the same regressions separately for hourly-wage jobs and annual-salary jobs.[29] Results are shown in Figure 4 for the full sample and Figure 5 for the inverse sample.[30] As discussed in the previous section, we prefer Figure 5 because there is stronger evidence that the SUTVA holds in the inverse sample. An interesting temporal pattern emerges. In the BGT microdata, pay at jobs offering an annual salary increases approximately 12% in the year after treatment. The effects diminish thereafter for the remainder of the post-treatment period so the ATT is 7.6%. Hourly wages, on the other hand, increase steadily over the post-treatment period, for an ATT of 5.5%. 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).

One potential explanation for the different hourly and annual treatment dynamics 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 unlikely 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 dif-

[29] As indicated in Section 3.1, BGT reports whether underlying pay is hourly, annual, or another frequency, even though the pay variable is always annual. GD reports pay at hourly, monthly, or annual frequency.

[30] These figures use annualized pay as the dependent variable, irrespective of the whether the underlying pay is hourly or annual.

ferent 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 5 estimates from both datasets are consistent in that the ATT for annual-salary workers is larger in magnitude than for hourly-wage workers. We discuss the implications of that finding in Section 5.

4.4 Additional Robustness

Section 4.1 has shown the robustness of our results to the choice of sample: both full and inverse samples yield positive, practically meaningful estimates. It remains to evaluate robustness to the choice of specification. Equation (3.2) 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).[31] Figure B.1 shows that in the full 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.[32] Estimation becomes infeasible in BGT. Estimation is possible in GD with a minimum employer-occupation cell size of 47. Figure B.1 panel (D) presents these GD inverse-sample estimates for completeness, but 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% in the last two quarters in event time.

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

[32]The estimator of Borusyak, Jaravel and Spiess (2022) requires that the same fixed effects are identified in the control sample and the full sample. In our setting, allowing small employer-occupation cells frequently violates this condition.

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-wage workers are more sensitive to variation in labor market competition than wages for lower-wage workers.[33] 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. 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). 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 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 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-

[33] 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.

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-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)). If that interpretation were correct, it would mean that two different types of labor market policy interventions—labor standards and labor market competition enforcement—are distinguished by their distributional impacts. This is an area ripe for further investigation, given greater attention to policy-driven variation in labor market competition (not to mention in labor standards).[34]

The settlements the Washington 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 McDonald's 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 such workers, 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 (2023) provides evidence that most or all chains that entered into AODs subsequently removed no-poaching language from their

[34] See, for example, Harris (2022).

franchising contracts, but that such language continues to be used by approximately 10% of chains (presumably consisting of chains that did not enter into AODs). As discussed in Section 2, if anything the legal status of franchise no-poaches has been solidified since these enforcement efforts were undertaken.

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 findings that minimum wage increases do not negatively impact employment point in the direction of oligopsony, in which employers face upward-sloping residual labor supply curves and trade off wages against labor turnover (Card, 2022). The results in our 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 conforming 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 be among the reasons: 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 evaluate the impact of the Washington State Attorney General's franchise no-poach enforcement campaign. The campaign secured nationwide, legally-enforceable agreements (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 6.6% in the BGT microdata, and approximately 4% in the GD microdata.[35] 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.

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

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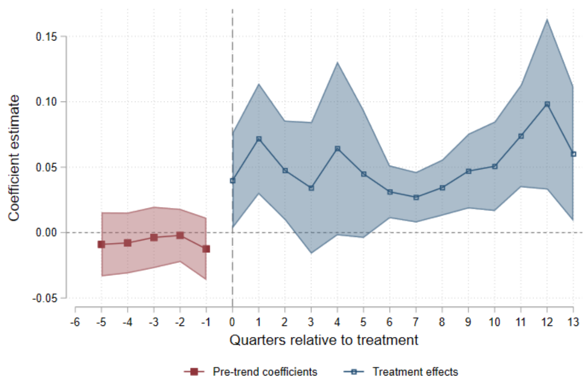
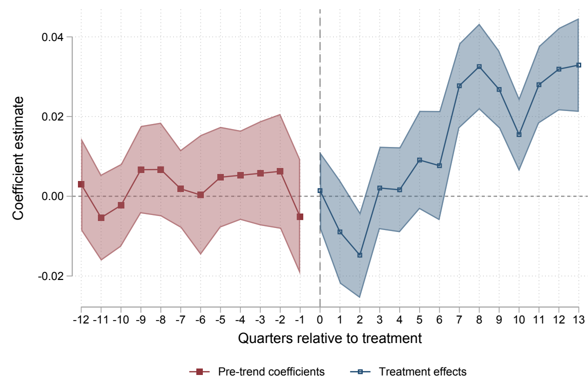
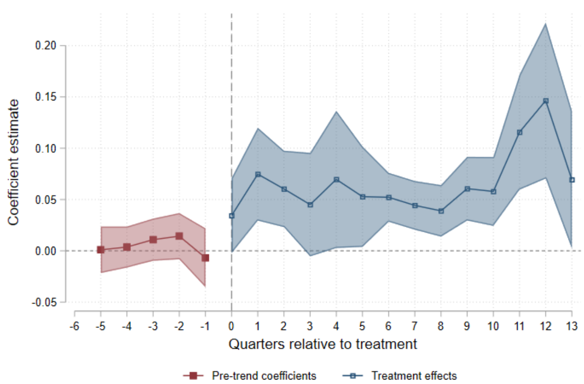
Figure 1. Event study estimates, full and inverse samples. Points are the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for both full and inverse samples in 1) the BGT microdata (left column) 2) the GD microdata (right column). The dependent variable is log real annual 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.



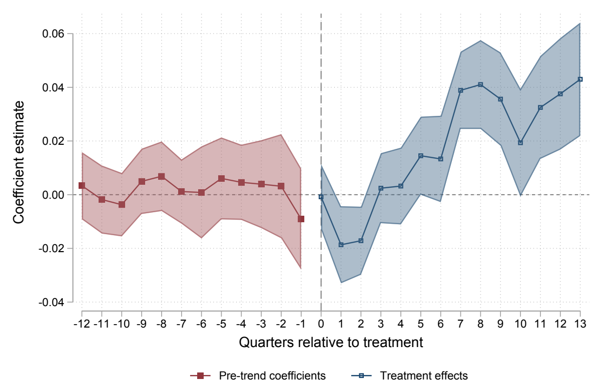
(A) Full BGT Sample



(B) Full GD Sample

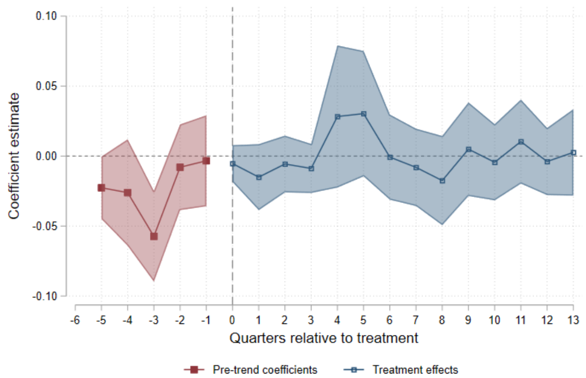


(C) Inverse BGT Sample

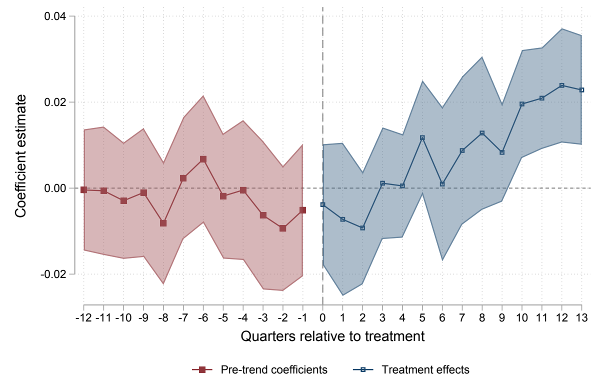


(D) Inverse GD Sample

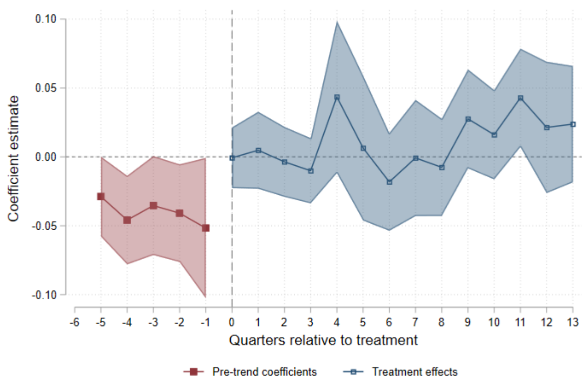
Figure 2. Placebo "treatment" of non-AOD chains in 2018Q3. Points are the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for chains that did not enter into AODs, as though they were treated in 2018Q3. The control group is either the remainder of the full sample (top row) or the inverse 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.



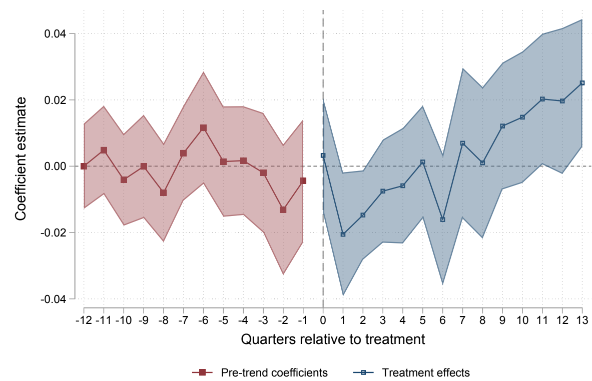
(A) BGT, full sample



(B) GD, full sample



(C) BGT, inverse sample



(D) GD, inverse sample

Figure 3. Event study estimates, GD unconnected employers. Points are the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for the GD unconnected-employer 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.

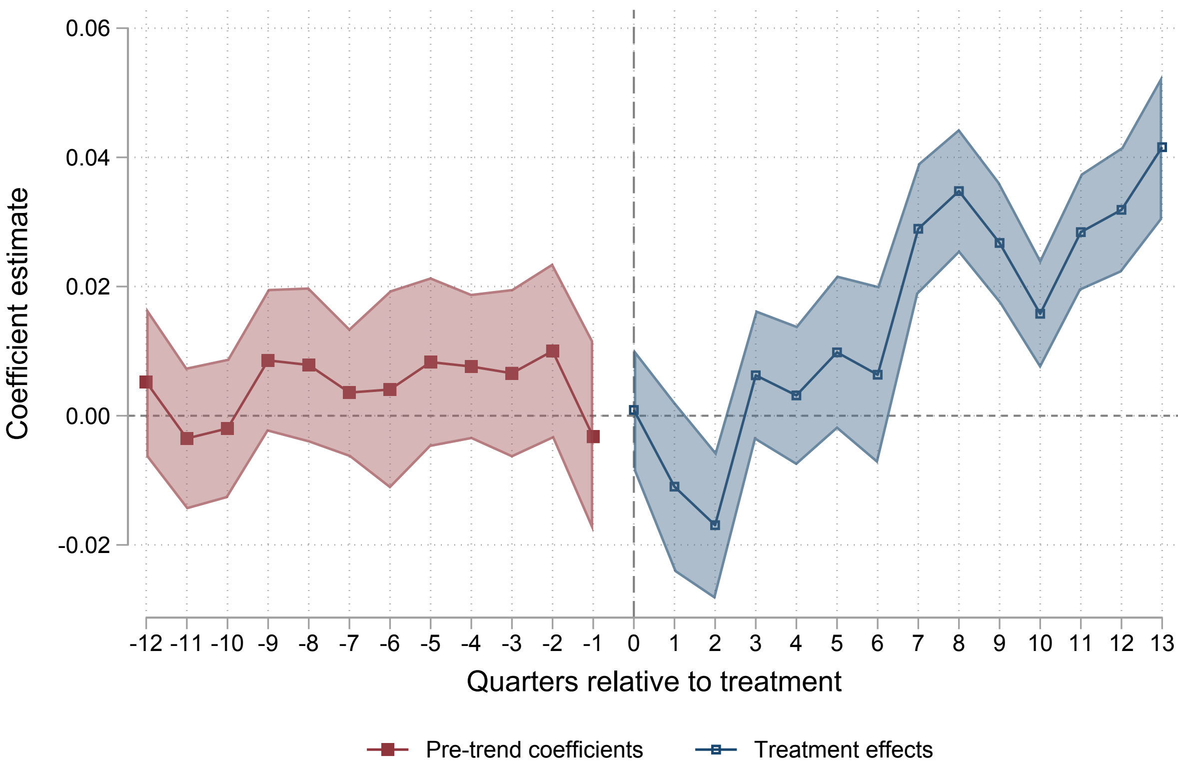
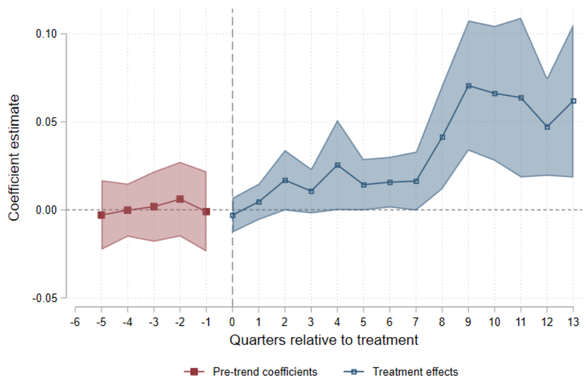
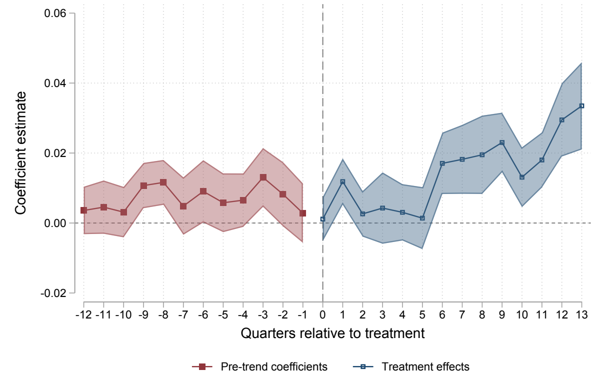


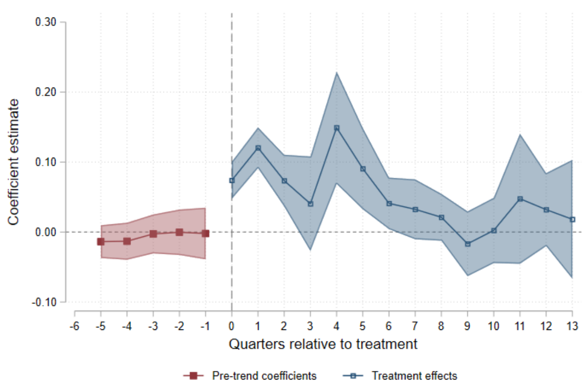
Figure 4. Event study estimates, annual salary versus hourly wage workers, full sample. Points are the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2, by pay frequency, for the full sample in 1) the BGT microdata (left column) 2) the GD microdata (right column). The dependent 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.



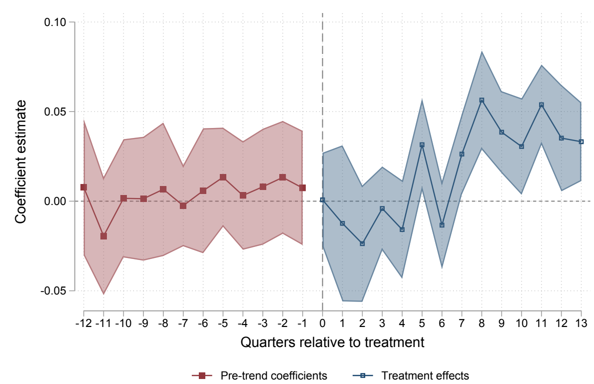
(A) Hourly Wage Workers, BGT



(B) Hourly Wage Workers, GD

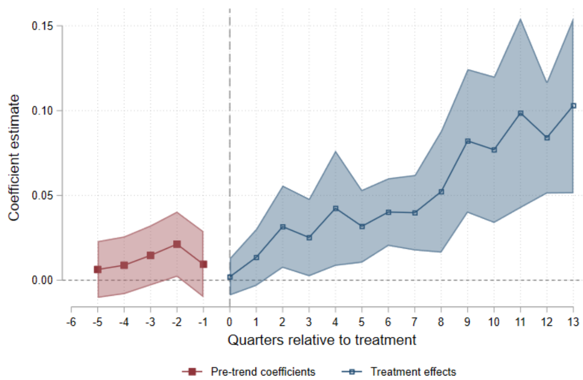


(C) Annual Salary Workers, BGT

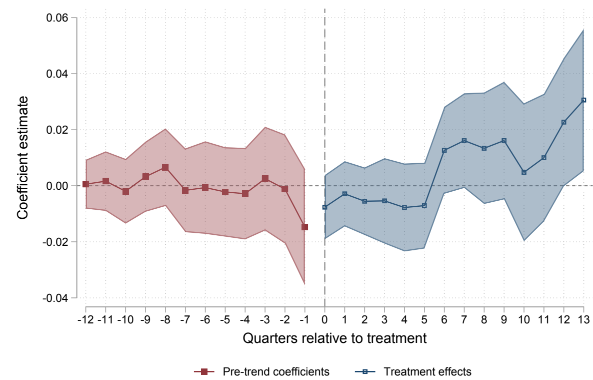


(D) Annual Salary Workers, GD

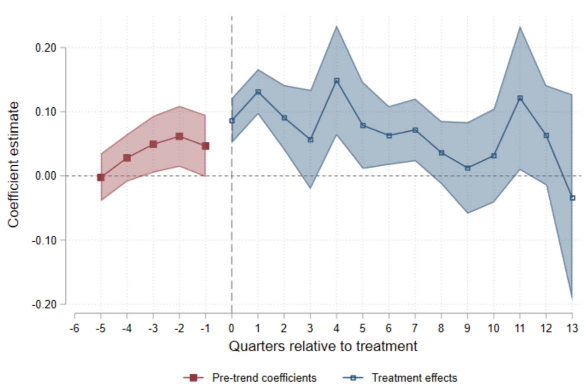
Figure 5. Event study estimates, annual salary versus hourly wage workers, inverse sample. Points are the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2, by pay frequency, for the inverse sample in 1) the BGT microdata (left column) 2) the GD microdata (right column). The 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.



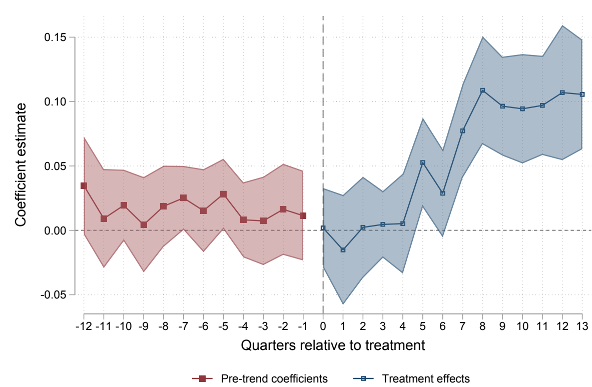
(A) Hourly Wage Workers, BGT



(B) Hourly Wage Workers, GD



(C) Annual Salary Workers, BGT



(D) Annual Salary Workers, GD

Table 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 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
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 2. Full sample summary statistics, BGT and GD microdata. This table reports summary statistics for the full sample described in Section 3 for both BGT and GD microdata.

	Treatment group (full GD sample)	Control group (full GD sample)	Treatment group (full BGT sample)	Control group (full BGT sample)
Number of chains/employers	186	176,014	220	1,218,293
Number of observations (total)	113,271	5,663,712	734,713	17,595,745
Number of observations (avg per chain/emp)	609	30,450	3,340	14
Pay (2015 USD): average	30,761	59,577	31,571	47,937
Pay (2015 USD): P10	17,913	23,529	18,367	21,510
Pay (2015 USD): P25	21,036	30,767	21,370	26,544
Pay (2015 USD): P50	25,741	48,140	26,228	36,330
Pay (2015 USD): P75	32,715	76,493	35,269	57,446
Pay (2015 USD): P90	48,846	112,866	49,628	87,705
Share of observations reporting wage paid hourly	0.77	0.45	0.63	0.45

Table 3. 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	39,835	214	4,764
Number of observations (total)	113,271	2,958,785	733,508	2,238,623
Number of observations (avg per chain/emp)	609	15,907	3,428	470
Pay (2015 USD): average	30,761	70,459	31,622	45,871
Pay (2015 USD): P10	17,913	25,438	18,388	24,803
Pay (2015 USD): P25	21,036	34,591	21,388	28,328
Pay (2015 USD): P50	25,741	56,733	26,256	32,536
Pay (2015 USD): P75	32,715	94,782	35,307	50,631
Pay (2015 USD): P90	48,846	138,413	49,963	87,343
Share of observations reporting wage paid hourly	0.77	0.37	0.63	0.61

Table 4. ATT estimates, full and inverse samples, BGT microdata. This table reports the estimated average effect of treatment on the treated (ATT; $\hat{\beta}$ from equation 3.1). Coefficients on dummies for negative event time ($\tau < 0$) allow tests for pre-treatment trend differences. Column (1) presents results for the full sample and column (2) those for the inverse sample. 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) Ln(real pay) Full sample	(2) Ln(real pay) Inverse sample
ATT	0.051***	0.066***
	(0.012)	(0.013)
$\tau = -1$	-0.013	-0.007
	(0.012)	(0.015)
$\tau = -2$	-0.002	0.014
	(0.010)	(0.011)
$\tau = -3$	-0.004	0.011
	(0.012)	(0.010)
$\tau = -4$	-0.008	0.004
	(0.012)	(0.010)
$\tau = -5$	-0.009	0.001
	(0.012)	(0.012)
Observations	18,340,066	2,972,131
Year-quarter x CZ FEs	Y	Y
Year-quarter x SOC-4d FEs	Y	Y
SOC-4d x Employer FEs	Y	Y

*** p<0.01; ** p<0.05; * p<0.10

Table 5. ATT estimates, full and inverse samples, GD microdata. This table reports the estimated average effect of treatment on the treated (ATT; $\hat{\beta}$ from equation 3.1). Coefficients on dummies for negative event time ($\tau < 0$) allow tests for pre-treatment trend differences. Column (1) presents results for the full sample and column (2) those for the inverse sample. 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) Ln(real pay) Full sample	(2) Ln(real pay) Inverse sample
ATT	0.019***	0.024***
	(0.0035)	(0.0072)
$\tau = -1$	-0.0052	-0.0091
	(0.0074)	(0.0096)
$\tau = -2$	0.0063	0.0031
	(0.0074)	(0.0099)
$\tau = -3$	0.0058	0.0039
	(0.0067)	(0.0084)
$\tau = -4$	0.0053	0.0046
	(0.0057)	(0.0071)
$\tau = -5$	0.0048	0.0061
	(0.0064)	(0.0078)
$\tau = -6$	0.00033	0.00083
	(0.0077)	(0.0088)
$\tau = -7$	0.0019	0.0012
	(0.0050)	(0.0061)
$\tau = -8$	0.0067	0.0069
	(0.0060)	(0.0066)
$\tau = -9$	0.0067	0.0050
	(0.0056)	(0.0062)
$\tau = -10$	-0.0023	-0.0037
	(0.0053)	(0.0060)
$\tau = -11$	-0.0054	-0.0018
	(0.0055)	(0.0065)
$\tau = -12$	0.0030	0.0033
	(0.0059)	(0.0064)
Observations	5,776,983	3,072,056
Year-quarter x State FEs	Y	Y
Year-quarter x Gen. occ. FEs	Y	Y
Gen. occ. x Employer FEs	Y	Y

*** p<0.01; ** p<0.05; * p<0.10

Appendices

A 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 throughout this analysis. 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.

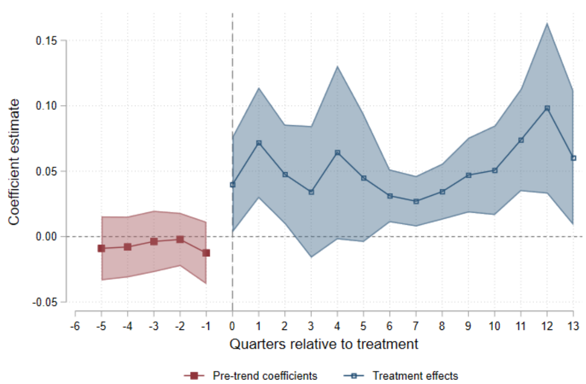
Recently, Lafontaine, Saattvic and Slade (2023) have pointed out that some of the salaries reported at the job ad level in the BGT microdata may not actually be stated by the employer posting the job ad, but rather imputed from similar employers and/or similar jobs.[36] They argue that biases our estimates 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 do not have a no-poach provision or did not enter into an AOD (or both), and those non-AOD chains pay more on average (as we show is the case 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, we follow Lafontaine, Saattvic and Slade (2023)'s Appendix C.4 to identify job ads that report a salary that is likely to be imputed. If the job ad text includes a sentence with the word "estimated" and the "$" symbol, or it includes the phrase "similar jobs pay," we tag that job ad as having an imputed salary and drop it from the re-estimates of equations 3.1 and 3.2. Figure A.1 reports the event-study figures from that procedure and compares them to the baseline estimates from Figure 1, and Table A.1 reports the

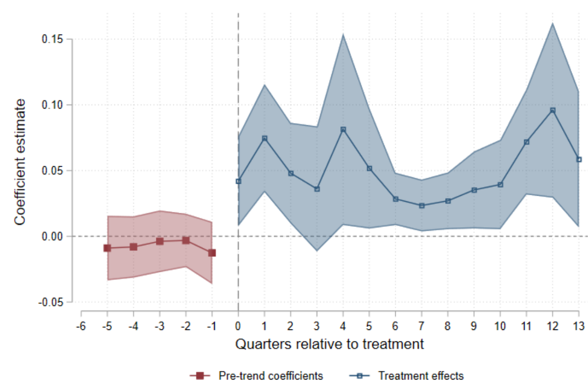
[36] Two iterations of our study were posted to SSRN prior to Lafontaine, Saattvic and Slade (2023). Relevant dates appear on the SSRN pages for the respective papers.

staggered difference-in-differences results for the inverse sample, 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. Note also that our analyses based on Glassdoor data are not subject to any critique based on imputation.

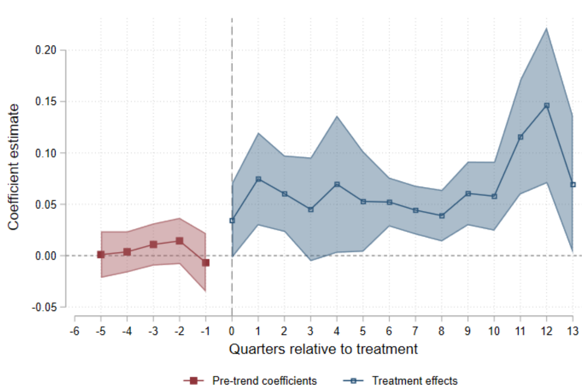
Figure A.1. BGT event study estimates, without imputed salaries. Points are the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for the BGT microdata, dropping potentially imputed salaries in the right column. The left column repeats figures 1(A) and 1(C) for comparison. 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.



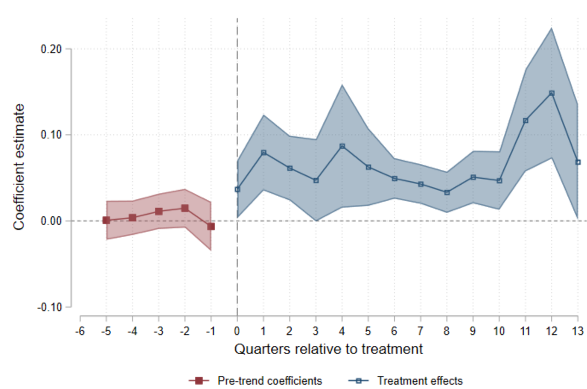
(A) Full BGT Sample



(B) Without Imputed Observations



(C) Inverse BGT Sample



(D) Without Imputed Observations

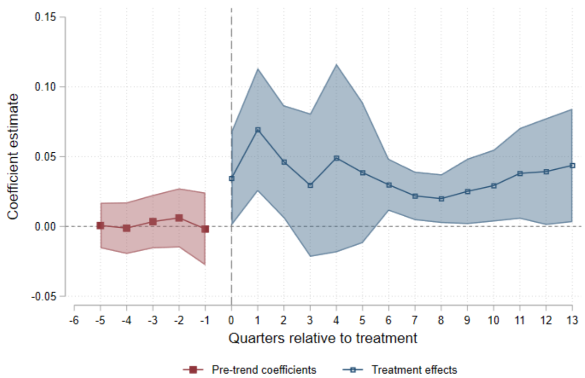
Table A.1. ATT estimates, omitting observations with imputed salaries (BGT data, inverse sample). This table reports a single treatment effect estimate ($\hat{\beta}$ from equation 3.1) for the inverse sample, omitting potentially imputed salary observations in column (2). Column (1) reproduces our primary inverse-sample estimate to facilitate comparison. Coefficients on dummies for negative event time ($\tau < 0$) allow tests for pre-treatment trend differences. 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) Ln(real pay) Baseline All salary observations	(2) Ln(real pay) Omitting imputed salary observations
ATT	0.066***	0.066***
	(0.013)	(0.013)
$\tau = -1$	-0.007	-0.006
	(0.015)	(0.015)
$\tau = -2$	0.014	0.015
	(0.011)	(0.012)
$\tau = -3$	0.011	0.011
	(0.010)	(0.010)
$\tau = -4$	0.004	0.004
	(0.010)	(0.010)
$\tau = -5$	0.001	0.001
	(0.012)	(0.012)
Observations	2,972,131	2,899,368
Year-quarter x CZ FEs	Y	Y
Year-quarter x SOC-4d FEs	Y	Y
SOC-4d x Employer FEs	Y	Y

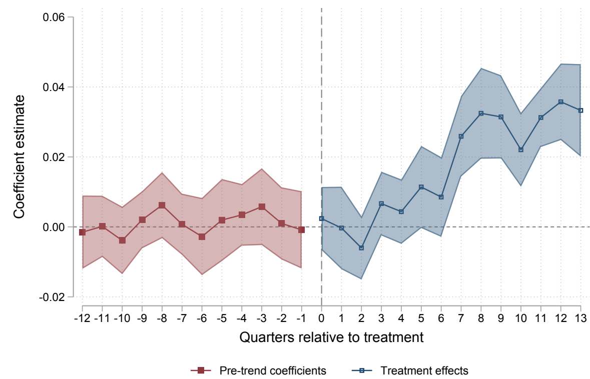
*** p<0.01; ** p<0.05; * p<0.10

B Additional Figures and Tables

Figure B.1. Event study estimates, full and inverse samples, detailed occupations. Points are the quarter-relative-to-treatment coefficients $\hat{\beta}_a$ from equation 3.2 for in 1) the BGT microdata (left column) 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. Specifications differ from Fig. 1 in employing occupation controls based on SOC-6d (BGT) and specific occupation (GD). Inverse-sample BGT results with SOC-6d codes (panel C) are not estimable, as explained in Section 4.4. Shaded bands represent 95 percent confidence intervals based on SEs clustered at the chain/employer level.

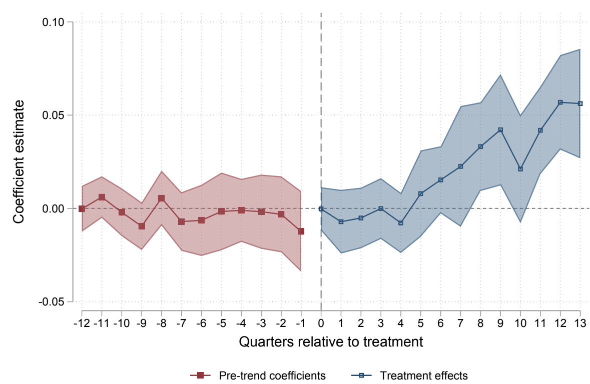


(A) Full BGT Sample



(B) Full GD Sample

(C) Inverse BGT Sample–Not Estimable



(D) Inverse GD Sample

Table B.1. Industries in GD microdata. Column (1) reports industries from the GD full 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 full-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 B.2. Industries in BGT microdata. Column (1) reports industries from the BGT full sample, in which treatment and control employers participate in the same industries, 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 control industries**
Restaurants & Other Eating Places	Electronic Shopping & Mail-Order Houses
General Medical & Surgical Hospitals	Administration of Human Resource Programs
Colleges, Universities, & Professional Schools	Couriers and Express Delivery Services
Executive, Legislative, & Other Gen'l Gov't Support	Building Material & Supplies Dealers
General Freight Trucking	Computer Systems Design & Related Services
Insurance Carriers	Administration of Economic Programs
Traveler Accommodation	Civic and Social Organizations
Elementary and Secondary Schools	Used Merchandise Stores
Business Support Services	Automotive Parts, Accessories, & Tire Stores
National Security and International Affairs	Aerospace Product & Parts Manufacturing
Services to Buildings and Dwellings	Administration of Environm. Quality Programs
Depository Credit Intermediation	Social Advocacy Organizations
Investigation and Security Services	Waste Collection
Grocery Stores	Medical Equipment & Supplies Manufacturing
Management, Scientific, & Technical Consult. Serv.	Semiconductor & Other Component Manufacturing
Home Health Care Services	Household Appliances Merchant Wholesalers
Offices of Physicians	Other General Purpose Machinery Manufacturing
Other Amusement and Recreation Industries	Fruit Preserving & Specialty Food Manufacturing
Child Day Care Services	Support Activities for Forestry
Activities Related to Real Estate	Utility System Construction
Offices of Real Estate Agents & Brokers	Computer & Peripheral Equipment Manufacturing
Other Professional, Scientific, & Technical Services	Grantmaking & Giving Services
Individual & Family Services	Motor Vehicle Parts Manufacturing
Building Equipment Contractors	Other Motor Vehicle Dealers
Offices of Other Health Practitioners	Commercial & Service Machinery Manufacturing
Clothing Stores	Ventilation, Heating, & Commercial Refrigeration
Offices of Dentists	Sugar & Confectionery Product Manufacturing
Legal Services	Other Support Services
Scientific Research & Development Services	Soap & Cleaning Compound Manufacturing
Other General Merchandise Stores	Drugs & Druggists' Sundries Merchant Wholesalers
Automotive Repair & Maintenance	Freight Transportation Arrangement
Health & Personal Care Stores	Agriculture, Construction, & Mining Machinery
Junior Colleges	Cement & Concrete Product Manufacturing
Continuing Care Retirement & Assisted Living	Support Activities for Road Transportation
Automotive Equipment Rental & Leasing	Urban Transit Systems
Personal Care Services	Lawn and Garden Equipment and Supplies Stores
Architectural, Engineering, & Related Services	Pulp, Paper, & Paperboard Mills
Cable & Other Subscription Programming	Travel Arrangement and Reservation Services
Justice, Public Order, & Safety Activities	Miscellaneous Durable Goods Merchant Wholesalers
Software Publishers	Other Fabricated Metal Product Manufacturing