Minimum Wages and Vertical Restraints in Franchising*

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Abstract

We explore the effects of state-level minimum wage increases on pay and turnover in the franchised service sector in the United States from 2010 to 2020. Our analysis focuses on heterogeneity in minimum wage effects with respect to the presence and type of vertical restraints used in different franchise chains. We first show that franchisee-autonomy-reducing restraints, i.e. *Resale Price Maintenance* ("RPM"), is associated with lower pay and higher chain-level job turnover, while franchisee-autonomy-increasing restraints, i.e. *Exclusive Territory*, have the opposite effect, suggesting that where franchisees (who are the employer of record) have greater autonomy from franchisor control, workers enjoy higher labor standards. We then show that minimum wage increases lead to higher real hourly wages and reduced turnover in all franchise chains, but the real-wage elasticity and job retention elasticity of minimum wage increases is greater for franchise chains operating under RPM, and lower for franchise chains with Exclusive Territories. This heterogeneity in responses to exogenous (from the firm level) statutory improvements in labor standards highlights one source of inequality in firm-level pay and other labor standards: the degree to which employers-of-record are constrained by dominant "Lead Firms."

Keywords: minimum wage; vertical restraints; monopsony; job vacancy data
JEL Codes: J23; J38; J88; L6

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1 Introduction

Franchising represents a hybrid business model on a spectrum between vertical integration, on the one hand, and total independence of supply chain segments on the other. The franchisor owns intellectual property, which it licenses to separately-incorporated franchisees, who carry out the business functions associated with the brand or trademark. As such, the franchisees are the employers of record for most of the franchise chain's personnel.[1] The franchisor not only licenses its intellectual property; it controls various aspects of the franchisee's business by means of vertical restraints, including resale price maintenance (setting the retail prices at which franchisees sell the licensed product or service to consumers), exclusive dealing (franchisee must distribute exclusively for the franchisor), and exclusive territories (granting the franchisee an exclusive local distributorship with respect to the brand and its products). Because the franchisor controls aspects of the franchisee's business, franchising is a paradigmatic example of what David Weil terms the Fissured Workplace: a "lead firm" (the franchisor) exercises de facto control over the labor standards of workers employed by a separate employer-of-record (the franchisee) (Weil, 2014).

This paper investigates the impact of minimum wage increases in franchising, marrying a longstanding policy question in labor economics with the question of where to draw firm boundaries and opening the "black box" of firm-specific pay-setting. The hypothesis we test is that employer-entrepreneurs with less autonomy over other aspects of their business offer poorer labor standards, one potential explanation for inter-firm pay differentials (Card, 2022; Song et al., 2019). Our findings are consistent with that hypothesis.

Specifically, we investigate the differential effect of state-level minimum wage increases on pay and job turnover across different franchise chains that either do or do not bind franchisees with the vertical restraints listed above: resale price maintenance ("RPM"), and exclusive territories. We consider RPM to be a *franchisee-autonomy-reducing restraint* because it

[1] In this paper, the term *franchise chain* refers to a single *franchisor* and multiple *franchisees* operating under a common brand or trademark.

restricts the scope of franchisee business decision-making, while exclusive territories are *franchisee-autonomy-increasing restraints*, because the enjoyment of a local monopoly with respect to the franchisor's brand potentially grants the franchisee greater bilateral power to resist franchisor control as well as in the output market, since local consumers cannot go elsewhere.[2]

Our panel dataset reporting which chains employ which vertical restraints in each year comes from Atz et al. (2025), who find that franchisee-autonomy-reducing restraints have increased in prevalence over 2009-2024, while franchisee-autonomy-increasing restraints (namely, exclusive territories) have become much less prevalent. We link that panel dataset to the universe of online job ads from Lightcast, which report posted pay in about 20% of cases. We use the count of job ads by chain (by state) as our measure of chain-specific labor market turnover.

Overall, we find that legislated minimum wage increases improve pay and reduce turnover in what is generally a low-pay sector, in line with the literature on minimum wages (Bassier et al., 2022; Cengiz et al., 2019; Dube et al., 2016; Kudlyak et al., 2022; McPherson et al., 2024; Otterby et al., 2024). More tellingly for the research question at hand, we find that the baseline effect (higher pay and reduced turnover) is accentuated in chains that employ franchisee-autonomy-reducing restraints, because the baseline labor standard is lower in those chains and so, we conjecture, minimum wage increases are more binding. By contrast, the baseline effect is muted in chains that employ franchisee-autonomy-increasing restraints, namely granting an exclusive territory, because the baseline labor standard is concomitantly higher.

Our findings speak to several strands of the literature in labor, Industrial Organization, and the Theory of the Firm. The Industrial Organization literature evaluating vertical restraints tends to focus on their competitive effect in output markets (Asker and Bar-Isaac,

[2]If labor markets are segmented by franchise brand, as some recent evidence (Callaci et al., 2024) and litigation (*Deslandes v. McDonalds*) indicates, exclusive territories would also confer a local labor market monopsony, although we report on evidence inconsistent with that labor market power being used to harm workers.

2014; Bernheim and Whinston, 1998; Blair and Lafontaine, 2005), i.e., do they serve to restrict or expand output, and raise or lower retail prices? Their effect on the labor market is a more novel area of inquiry and has thus far focused on expressly labor-oriented restraints like within-chain no-poaches, noncompetes prohibiting franchise workers from leaving the chain post-employment, and other provisions that putatively restrict employees' ability to disseminate confidential chain-specific information (Callaci et al., 2024, 2025; Lafontaine et al., 2025, 2023; Norlander, 2025; Posner, 2024).

Labor researchers, by contrast, tend to treat firm-level heterogeneity in pay and other worker outcomes as a "black box" captured by a fixed effect whose variation is not otherwise explained. Some work investigates the importance of concentrated buyer power in supply chains for its wage impact on upstream firms (Romero, 2025; Wilmers, 2018), a conceptually similar approach to what we undertake at the other end of the supply chain in this paper, which arrives at similar results. More broadly, Azar et al. (2023) also considers heterogeneous effects of minimum wage increases by horizontal concentration of employers in a labor market (using the same job-posting data from Lightcast that we rely on). This paper could be understood as a vertical analog to that one in that our dimension of heterogeneity between employers is the degree to which upstream firms control downstream ones.

Our baseline results are comparable to the previous literature on the impact of minimum wage increases on real hourly wages and job vacancies. However, our dataset, and especially our focus on vertical restraints in the franchise sector, represent a novel departure. We report results from four previous studies on minimum wage effects on labor market outcomes in Table 3.

The remainder of the paper is organized as follows. Section 2 provides background on the franchising business model and the vertical restraints we focus on. Section 3 introduces the dataset used in the study. Section 4 outlines our preferred identification strategy and empirical approach. Section 5 reports results, and section 6 offers a detailed discussion. Section 7 concludes.

2 Background

Vertical restraints included in standard contracts issued by franchisors to their franchisees are legal agreements that govern decisions about the retail price of a product, the sourcing of inputs, and which products get sold, among many other aspects of the franchised business. Collectively, vertical restraints preempt business decisions by either the franchisor or the franchisee (or both) with respect to third parties, be they consumers, employees, suppliers, or rival franchise chains (Paul, 2023). In this paper, we confine our analysis to two vertical restraints:

- **Resale Price Maintenance**: Franchisors set retail prices for products sold by franchisees, for either all retail transactions or a sub-set of them. Franchisees are obligated to adhere to these pricing guidelines in order to maintain brand consistency and integrity. This includes the requirement that the franchisee "honor all discounts," i.e. must abide by chain-level price promotions and/or accept gift cards purchased from another franchisee, or the franchisor.

- **Exclusive Territory**: This provision grants an exclusive distributorship to the franchisee, i.e. a local retail monopoly vis-a-vis the franchisor's brand. It is usually defined geographically, although in some cases it pertains additionally to particular classes of customer, e.g. residential versus commercial for construction services. May be termed "designated territory" or "protected territory."

Resale Price Maintenance restricts the franchisee's ability to make decisions about the prices of their products, which in turn restricts their ability to pass through costs they incur to retail prices, and may also prevent them from choosing which products or services to offer customers. Therefore, we consider *Resale Price Maintenance* to be a 'franchisee-autonomy-reducing' restraint.

By contrast, an exclusive territory provides the franchise chain with the sole right to operate and market a given brand within a geographic area. Franchisees with exclusive territories face less competition from franchisors (or their other designees, including other franchisees), potentially granting them greater bargaining leverage in bilateral negotiations with the franchisor and a greater degree of autonomy in the retail market since they don't face competition from other franchisees offering the same branded product or service.

Figure 1 shows that the use of exclusive territories has diminished since 2012, while the use of resale price maintenance has grown. This larger pattern (encompassing many more provisions than these two)—that power has shifted in franchising away from franchisees and toward franchisors—is the central finding of Atz et al. (2025).

3 Data

This study relies on a merged dataset of digitized Franchise Disclosure Documents (FDDs) and appended contracts taken from Atz et al. (2025), employer-identified job advertisements from Lightcast, which is the near-universe of online job ads, state level quarterly minimum wage data from Vaghul and Zipperer (2016), and state-level quarterly unemployment data from the Local Area Unemployment Statistics series (LAUS) obtained from the U.S. Bureau of Labor Statistics.[3] Further description of the Lightcast data can be found in Callaci et al. (2024, 2025). Very briefly, our job ads data contains identifying information about each online job ad posted by a franchise employer in a given state in a given month and the associated details about the job (such as the posted hourly or annual salary, occupational characteristics, minimum educational requirements, and the NAICS 6-digit industrial classification of the franchise chain). Franchise chains are identified in the job ads data by exact text-matching of the `employer` field to a master list of franchise chains compiled from numerous sources, most especially FRANdata.

Information on vertical restraints is constructed from the FDDs and matched to the job ads data. FDDs are standard-form disclosures mandated by the Federal Trade Commission's

[3]Lightcast was created through a merger of Emsi and Burning Glass Technologies and became the official name of the combined organization in 2024. The job ads data used in this paper is primarily sourced from Burning Glass Technologies.

Franchise Rule. Franchisors offering franchises to the public are obligated to provide prospective franchisees with their current FDD prior to signing any franchise agreement. The FDD discloses contractual provisions like the restraints we study in this paper, in addition to past performance of the franchisor and, optionally, franchisees, as well as the capital required to launch a new franchise. The vertical restraints are dummy-coded variables in our panel data, reflecting whether the chain uses a given restraint in a given year. For example, RPM is 1 for a franchise chain if there is language in the FDD for that year that restricts franchisees from choosing retail prices. Atz et al. (2025) explains how the franchise-chain-level panel dataset is constructed, including sample rules that encode vertical restraints.

To examine the effect of minimum wage increases, we combine the franchise chain-level FDD data and the job ads data from Lightcast with state-level quarterly minimum wage data from Vaghul and Zipperer (2016) (which has been updated periodically to reflect changes in state minimum wages). Using franchise chain level variation in posted pay and the count of job advertisements and quarterly state-level minimum wage changes between 2010q1 and 2020q4, we show how the effects of minimum wage increases depend on the vertical restraints in place for a franchise chain.

There are significant advantages in using the Lightcast data for our study. Posted pay represents a leading indicator of employer- and market-level competitive conditions in the labor market (Callaci et al., 2024). In combination with the employer identification, it is therefore highly informative about how chains respond to minimum wage increases in their hiring. Using posted wages and job vacancy data also helps avoid concerns about worker misreporting of wage information and the selection bias of reported earnings.

There are important limitations to using online job ads. We do not observe post-bargaining wages, and there might be questions about how much reliable information these posted job ads contain.[4] Batra et al. (2023) show that on online portals, and as of 2017,

[4]Historically, the concern has been that posted pay represents a wage floor above which prospective hires may negotiate. However, there is evidence in Mancini and Steinbaum (2025) that posted pay may exceed realized pay.

few job ads contain information about wages and that wages are posted primarily as a range rather than a fixed number. Since then, the share of job ads that are both employer identified and include posted pay has increased considerably. Previous studies have examined employer-identified job ads data from Lightcast to show that the increases in the wages of new hires in the online data are consistent with the increases in wages for new hires in the Current Population Survey (CPS) data and with average earnings for new hires from the Quarterly Workforce Indicators (QWI) series (Hazell and Taska, 2020). The Burning Glass Technologies (now Lightcast) data has been used to document concentration in US labor markets (Azar et al., 2020), evidence of upskilling in response to economic recessions (Hershbein and Kahn, 2018), the growth in Artificial Intelligence-related vacancies (Acemoglu et al., 2022), and the effect of mergers on workers (Arnold, 2025; Farag et al., 2025). In the absence of matched employer-employee administrative datasets for franchise chains, Lightcast data provides a valuable employer-identified alternative for examining firm-level responses to minimum wage policies.

Our full sample consists of 740 unique franchise chains with 6,712,977 posted job vacancies between 2010q1 and 2020q4 across the 50 states plus the District of Columbia, restricted to franchises with non-missing vertical restraints information. In this dataset, within a given franchise identifier, there is time variation in the application of each vertical restraint (but not variation across states within a chain-year cell). There is both state-level and time variation in the statutory minimum wage, which is common across chains (within a state-quarter or state-year cell). For our wage regressions, the analysis subsample consists of 850,214 job postings and 704 unique franchise chains (across 45 states plus the District of Columbia) with salary information and identified franchise chain (and hence restraint) information.[5] This is the analysis subsample we utilize for our pay estimates throughout this paper.

For the analysis in which the count of job ads is the dependent variable, we collapse the full sample containing 6,712,977 job ads to the annual level to compute a chain-specific

[5] In subsection 4.2 we explain why five states are dropped from the minimum wage analysis.

job ad count outcome variable (so each observation is a count of job ads within a franchise chain-state-year cell). If a franchise chain posts no job ads for a given state and year, then the count of job ads is recorded as zero. This provides us with a balanced panel of 80,052 franchise-chain-by-state-by-year observations across 739 unique franchise chains and 45 states, plus the District of Columbia (our sample of control and treated states).[6] For the job ad count regressions, each quarterly minimum wage change is recorded to an annual indicator by mapping each quarter's minimum wage change to its corresponding calendar year.

Our analysis subsample for the wage regressions covers the period 2010Q1 to 2020Q4 (correspondingly 2010 to 2020 for the count-of-job-ads analysis). The choice to restrict our analysis subsample to 2020q4 is guided by the need to avoid treatment contamination that could bias pay and hiring differentials in the labor market while also including a full four-year post-period for the final cohort of state minimum wage increases analyzed (2016Q4).[7] As discussed earlier, the share of franchise chains using resale price maintenance has increased over time, while the share of franchise chains with exclusive territories has decreased. We show this trend in Figure 1 by plotting the share of franchise chains and posted job ads by franchise chains with the two vertical restraints in question.

In our stacked difference-in-differences specifications and event study for estimating the minimum wage effect on log real wages, we use 8-quarter leads pre-event and 16-quarter lags post-event. This corresponds to using 2-year leads pre-event and 4-year lags post-event for our estimations for the effect of minimum wage on the log count of job ads (which we analyze at an annual frequency). For this purpose we only consider those minimum wage increases for which a full set of pre- and post treatment observations are available. This corresponds to the set of state-level quarterly minimum wage changes between 2012q1 and 2016q4, which are plotted in Figure 2. This is the set of treatments we consider in our empirical analysis

[6] The count of franchise chains included in the pay and job ad count analyses differ because some chains do not post ads that include pay.

[7] Because the first minimum wage change during the pandemic occurred in 2021Q1, we restrict our sample to 2020Q4 to exclude pandemic-era treatments.

of the effects of minimum wage increases on hourly pay and the number of job ads.

Our control group consists of states that followed the federal minimum wage (which has not increased since July 2009) between 2012q1 and 2016q4. The treated group consists of those states that observed at least one minimum wage increase between 2012q1 and 2016q4. Several states increased their minimum wage either just before or just after our study period of 2012q1-2016q4. These states are not considered in either the control or the treated group in our estimations. For instance, Virginia had its first minimum wage increase in 2021q2 and is therefore not considered in the treated group in our estimation period. New Mexico, Illinois, and Nevada are also not included in our treatment group for the same reason. [8] This means that, in our specification, five states— Maine, Illinois, Nevada, New Mexico, and Virginia—are excluded. Excluding these five states allows us to create a clean control group, as we compare only states that are never treated to those that experience at least one treatment during our event window (2012q1-2016q4). For further robustness checks, we report the results by including these states in our control group. These results are provided in Appendix 1.2. Results including these five states from the control group do not materially affect our findings.

Between 2012q1 and 2016q4, our analysis subsample for pay estimates and the count of job ads estimates, therefore, consists of 26 treated states (including DC) and 20 control states. As shown in figure 2, increases in the minimum wage in our treated states are staggered over time, and these hikes vary in their intensity. For instance, in Arizona, the minimum wage increased in the first quarter of each year from 2012 to 2015 (a total of four increases). In many cases, minimum wage increases occur in multiple states in the same period. For instance, the minimum wage increased in 13 states in 2016q1. The highest quarterly state minimum wage within this period was in the District of Columbia ($11.50 in

[8] For Illinois, the first minimum wage increase during our sample occurred in 2010Q3, which leaves only two quarters of pre-treatment data within our 2010Q1 start date, falling short of the eight-quarter pre-periods required for inclusion in the event study for wage regressions. As a result, Illinois is excluded from our treated and control group in both the static and dynamic specifications. A similar logic applies to Nevada, where the proximity of the minimum wage increases to the sample start date prevents the construction of the full lead–lag structure necessary for our analysis.

2016q3 and currently \$17.95 per hour effective July 2025), where three minimum wage hikes occurred between 2012q1 and 2016q4.

We construct the hourly wage series from posted annual salary data for each job ad taken from Lightcast. Hourly wages are constructed under the assumption that each job ad represents a 40-hour workweek with 52 weeks in a year. Figure 3 shows the distribution of the difference between our constructed log real hourly wage and log minimum wage. The available sample has a higher proportion of job ads with posted real wages being greater than the state-level minimum wage, which suggests a near-perfect compliance with minimum wage policy. However, the majority of constructed hourly wages in our sample lie within 0–1 log points above the binding minimum wage, indicating that while compliance is high, at least for posted pay, this is still a low-wage sector. Figure 4 shows the number of posted job ads per quarter in the dataset.

4 Empirical Strategy

4.1 Effect of vertical restraints on log hourly wages and count of job ads - TWFE

We begin by estimating the effect of the two vertical restraints (RPM and exclusive territory) on hourly wages and posted job ads at the franchise chain level. As shown in equation 1, we use a simple two-way fixed effects model with franchise chain and year-quarter fixed effects to estimate the impact of these two vertical restraints on log hourly wages. Note that for the job ad count outcome, we construct the count of posted job ads by a franchise chain in a given state in a given year. Accordingly, as shown in equation 2, we estimate the impact of the two vertical restraints on the log count of job ads, controlling for franchise-chain, calendar year, and state-level fixed effects.

$$\log(w_{ift}) = \alpha + \beta \cdot \text{Restraint}_{ft} + \gamma_f + \delta_t + \varepsilon_{ift} \tag{1}$$

where $\log(w_{ift})$ is the log wage posted in job ad i by franchise chain f in quarter t. γ_f are franchise chain-level fixed effects, and δ_t are year-quarter fixed effects. Restraint_{ft} indicates whether chain f employs one of our two restraints in year t. Hence, in this specification, the restraint effect on pay is identified by time variation in whether f uses or does not use a given restraint (an improvement on Callaci et al. (2025), which uses only cross-sectional variation in similar regressions).

Our job-ad-count estimating equation is

$$\log(1+j)_{sft} = \alpha + \beta \cdot \text{Restraint}_{ft} + \gamma_f + \zeta_s + \delta_t + \varepsilon_{sft} \tag{2}$$

where $\log(1+j)_{sft}$ is the natural logarithm of one plus the count of posted job ads posted by chain f in state s in year t, γ_f are chain-level fixed effects, ζ_s are state fixed effects, and δ_t are calendar year fixed effects. We add one before taking the logarithm to accommodate state-by-franchise-chain-by-year observations with zero posted job ads.

The results are summarized in figures 5-6. We find that posted wages are lower for franchise chains with the autonomy-reducing restraint - *RPM*. The count of posted job ads is higher for franchise chains with *RPM*. On the other hand, franchise chains with *exclusive territory* pay higher wages and post fewer job ads. In terms of the size of the effect, implementing RPM is associated with 2.19% lower hourly pay, and having an Exclusive Territory Granted restraint is associated with 3.12% higher pay compared to franchise chains without such restraints. For the count of job ads, similarly, RPM-restrained franchise chains post 3.21% more job ads, while Exclusive Territory Granted-restrained chains post 5.27% fewer job ads compared to franchise chains without such restraints; however, for the job ad count outcome, we cannot reject a null effect for either restraint. This pattern motivates our interpretation of job ad counts as reflecting chain-specific turnover: chains paying less post job ads more frequently, indicating they have a greater need to fill vacancies. Note that Kudlyak et al. (2022) reach a similar conclusion regarding the interpretation of job ad counts in response to minimum wage increases, which we follow in the next section.

These results are indicative of lower pay and higher turnover in franchise chains with autonomy-reducing restraints, at least those pertaining to resale price maintenance. Based on these results, we now turn our focus to studying what happens to real wages and job postings in vertically restrained franchises vs non-restrained franchises in the event of a minimum wage increase.

4.2 Stacked Difference-in-Differences and Event Study specifications

In order to identify the causal impact of an increase in the statutory minimum wage, we rely on a stacked Difference-in-Differences approach followed by a dynamic stacked Event Study. As minimum wage increases are multiple staggered events, a classic two-way fixed effects model may produce biased estimates due to heterogeneous treatment timing (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2023; Goodman-Bacon, 2021; Sun and Abraham, 2021). This bias arises due to contamination from already-treated units serving as controls for later-treated units. In contrast, a stacked difference-in-differences approach addresses this issue by constructing separate cohorts based on treatment timing and comparing each treated group only to appropriate never-treated units within a defined event window. By stacking these comparisons and estimating effects relative to treatment timing, this method isolates cleaner counterfactuals, avoids negative weighting problems, and provides more reliable and interpretable estimates of dynamic treatment effects in settings with staggered adoption.

For estimation purposes, we first define an event window—eight quarters before and sixteen quarters after treatment—spanning 2010q1 to 2020q1. This constrains the set of events with a full set of pre- and post periods to 2012q1 to 2016q4. Specifically, we consider each minimum wage increase as a unique exogenous event, with a group of treated states (states that experienced an increase in a given quarter) and a group of control states (this includes all states that follow the federal minimum wage). We use the same event window for the count of job-ad regressions. States treated outside this window are automatically excluded from our analysis. Based on the time period of the event, states are classified into different *cohorts* or *stacks*, with each *cohort* or *stack* consisting of states where the minimum wage increased in the same year-quarter (for the pay regressions)/in the same year (for the job ad count regressions) and a group of control states where no minimum wage increase occurred at any time. An example of such a stack, corresponding to the 2014q1 minimum wage increase, is shown in Figure 7.

Trimming the analysis sample to only cover those events where the complete set of treatments can be estimated ensures compositional balance and that event study results are not driven by late and early treatment-adopting groups (Wing et al., 2024). Once the stacked dataset is constructed this way, we implement a Difference-in-Differences (DiD) and an event study estimation strategy, incorporating corrective sample weights that recover the aggregate target effect of interest.[9]

4.3 Implementation - Stacked DID

Our empirical specification for wage estimations is given by Equation 3.

$$log(w)_{i,s,f,t} = \beta_1(T_{s,f,d} * P_{t,d}) + \gamma_{f,d} + \zeta_{s,d} + \delta_t + X'_{s,t,f}\theta + \epsilon_{i,s,f,t} \tag{3}$$

where $\log(w_{isft})$ is the natural logarithm of the hourly wage posted for job ad i by franchise chain f in state s quarter t. $T_{s,f,d}$ is an indicator that denotes that franchise chain (f) in state (s) is treated in the stack or sub-experiment (d). $P_{t,d}$ denotes that the time-period (t) is in the post-period in the sub-experiment (d). $\zeta_{s,d}$ are state-by-stack fixed

[9] Corrective sample weight construction follows the steps laid out in Wing et al. (2024). Briefly, our sample weights are constructed by taking the treated group's share of observations within each stack and dividing it by the control group's share of observations in that stack. Further information about the weighting method is provided in Appendix A.1.

effects, $\gamma_{f,d}$ are franchise chain-by-stack fixed effects, δ_t denotes the year-quarter fixed effects. We include the state-level unemployment rate as an additional control. This specification estimates the average treatment effect of a minimum wage increase (regardless of size) on real wages by comparing only clean controls (states with no minimum wage increases within our treatment window) to treated states within a given sub-experiment. We examine the effect of the two vertical restraints in question by simply estimating equation 3 separately for franchises practicing a given vertical restraint and for franchise chains without that vertical restraint. This is equivalent to estimating equation 3 for various subsets of franchises where vertical restraints are present, and these results are compared with the baseline specification for the entire sample. Consequently, the treatment effects we report are average treatment effects specific to the subsample of franchises that operate under each vertical restraint.

To estimate the impact of minimum wage increases on the count of posted job ads, we follow a strategy similar to that used for wage regressions. In order to address the sparseness in franchise chain-by-state cells, we construct the count of posted job ads per franchise in a state for a given year and include all posted job ads, with or without salary information. Following this, we construct a stacked dataset including cohorts with minimum wage increases in a particular year as the treatment group and all states with no minimum wage increase in the treatment window as the control group. For the count of job ads estimation, we consider treatments spanning 2012-2016, corresponding to wage regressions where our treatment window was 2012q1-2016q4. This specification is described in equation 4.

$$log(1+j)_{s,f,t} = \beta_1(T_{s,f,d} * P_{t,d}) + \gamma_{f,d} + \zeta_{s,d} + \delta_t + X'_{s,t,f}\theta + \epsilon_{s,f,t} \quad (4)$$

where $log(1+j)_{s,f,t}$ represents the natural logarithm of one plus the count of posted job ads by franchise chain f in state s in year t. We add one before taking the logarithm to accommodate state-by-franchise-chain-by-year observations with zero posted job ads. $T_{s,f,d}$ is an indicator that denotes if franchise (f) in state (s) is treated in the stack or sub-experiment (d). $P_{t,d}$ denotes that the time-period (t) is in the post-period of the sub-experiment (d). We control for state-level variation and franchise variation over time using state-by-stack fixed effects ($\zeta_{s,d}$), franchise-chain-by-stack fixed effects ($\gamma_{f,d}$), and year fixed effects (δ_t). Similar to wage regressions, we also control for the state-level annual unemployment rate. Our primary coefficient of interest is β_1, which reports the post-treatment effect of minimum wage increases on the log of one plus the count of job ads by franchise chain.

Given that both our posted pay and job ad count regression equations include state and franchise chain fixed effects (as well as calendar time fixed effects), the variation that identifies the effect of minimum wage increases on each outcome is within chain, across states that are and are not exposed to minimum wage increases, within a treatment cohort. The estimated cohort-level effects are then aggregated into our overall treatment effect estimates.

4.4 Implementation - Stacked Event Study

In the dynamic stacked event study specification, we allow for treatment effects up to eight quarters before and sixteen quarters after a minimum wage event. Corresponding to the stacked DID specification, the real wage estimations are carried out at the job ad level and use quarterly minimum wage increases as treatments, while the job ad count regressions are at the annual level. The specifications are laid out in equation 5 and equation 6, respectively, for real wage and job ads regressions.

$$log(w)_{i,s,f,t} = \sum_{j=-8}^{-1} \beta_j (Lead_j)_{s,f,t,d} + \sum_{k=0}^{16} \beta_k (Lag_k)_{s,f,t,d} + \gamma_{f,d} + \zeta_{s,d} + \delta_t + X'_{s,t,f}\theta + \epsilon_{i,s,f,t} \tag{5}$$

$$log(1+j)_{s,f,t} = \sum_{j=-2}^{-1} \beta_j (Lead_j)_{s,f,t,d} + \sum_{k=0}^{4} \beta_k (Lag_k)_{s,f,t,d} + \gamma_{f,d} + \zeta_{s,d} + \delta_t + X'_{s,t,f}\theta + \epsilon_{s,f,t} \tag{6}$$

Where the dependent variables, $\log(1 + j)_{s,f,t}$ is the log of one plus the count of job ads posted by chain f in state s in year t, and $\log(w)_{i,s,f,t,}$ is the log real hourly wage in job ad i posted by chain f in state s in year-quarter t. $Lead_j$ and Lag_k are both dummy variables that indicate the time to treatment based on whether a year-quarter (or a year in the job ads estimations) relative to a minimum wage hike is leading up to the minimum wage hike ($Lead_j$) or whether it is post a minimum wage hike (Lag_j). $Lead_0$ indicates the time period (year-quarter for real wage estimations and year for job ad regressions) in which the minimum wage increase occurred. In our event studies, $Lead_1$ is omitted to capture the baseline difference between the treated and the control groups. The final estimated coefficients will be an average of these leads and lags across the multiple stacks. Similar to the Difference-in-Difference specifications in equations 3 and 4, we control for a set of franchise chains by stack fixed effects, state by stack fixed effects, time fixed effects, and state-level unemployment rate.

To ascertain the impact of vertical restraints in mediating the effects of minimum wage changes on pay and turnover, we perform stacked event-study estimations (equations 5 and 6) for chains that use and do not use vertical restraints. For example, we estimate equations 5 and 6 just for chains that use RPM versus only chains that do not use RPM, where the definitions of the treatment and control groups for each stack are the same as in the baseline estimates. Similarly, for franchise chains with and without exclusive territory granted restraints. Consequently, the treatment effects we report are average treatment effects, specific to the subsample of franchises that operate under each vertical restraint.

5 Results

5.1 Impact of minimum wage increases on log hourly wages - Stacked DID results

Figure 8 and Table 1 show the results from estimating equation 3, in which the outcome of interest is log real hourly wages. We compare the baseline results reported in column 1 of Table 1 (estimated on all franchise chains) with the results from the restricted samples consisting of chains with and without a given restraint (for example, column 2 of this table reports the average treatment effect for the sample consisting of franchise chains without an RPM restraint). We find positive and significant effects of minimum wage increases on log real hourly wages in the baseline specification. Specifically, an increase in the minimum wage raises real hourly wages by 3.86% on average in the treated states relative to the control group, comprising states where no minimum wage increase occurs within our treatment window.

More interestingly, the treatment effect of increasing the minimum wage on log hourly wages is larger when franchise chains operate under the autonomy-reducing vertical restraint, RPM, and smaller when franchise chains operate with an exclusive territory. An increase in the minimum wage causes a 7.55% increase in real hourly wages for franchises in treated states operating under RPM versus 1.33% for chains that do not use RPM. The difference in coefficients across the two subsamples (RPM vs non RPM franchise chains) is statistically significant at the 5% level (t-test for equality of coefficients, $p = 0.04$). For franchise chains using exclusive territories, the causal effect of a minimum wage increase on real hourly wages is 1.82%, versus 4.17 % for chains that do not use exclusive territories. However, the difference in coefficients across the two subsamples (Exclusive Territory vs No Exclusive Territory) is insignificant at the 5% level ($p = 0.382$).

5.2 Impact of minimum wage on log count of posted jobs - Stacked DID results

Figure 9 and Table 2 show the results of our preferred specification (equation 4) to estimate the impact of minimum wage increases on the log count of job ads, where the count of job ads is measured annually within a franchise chain in a state. We find a negative effect of minimum wage increases on the log count of job ads in the baseline and the vertical restraint specific regressions. More importantly, the magnitude of the minimum wage effect is larger for chains using autonomy-reducing vertical restraints.

In the baseline specification, the average treatment effect is that an increase in minimum wage causes posted job ads to decline by 1.36% for franchise chains in the treated states relative to the control group. We find that an increase in minimum wages causes a 6.61% decrease in the count of posted job ads in franchise chains that are operating under RPM, whereas the estimate is -2.89% for chains that do not use RPM. For franchise chains using exclusive territories, the causal effect of a minimum wage increase on the count of posted job ads is -0.84%, verus -2.89% for chains that do not use exclusive territories.

Due to the aggregated nature of the job ads data, however, our estimated coefficients for the count of job ads regressions are insignificant. The difference in coefficients across the subsamples for both RPM and Exclusive Territory restraints is also statistically insignificant at the 5% level. These job ad regression results, although less conclusive in comparison to the real-wage regression estimates, suggest that autonomy-reducing vertical restraints (such as RPM) raise the responsiveness of the number of job vacancies to minimum wages. We interpret variation in the count of posted job ads within a chain over time as variation in turnover—more job ads posted means more recruiting is necessary to sustain a given level of employment. Hence, our interpretation of these results is that increasing minimum wages leads to higher job retention. Franchise chains operating under autonomy-reducing vertical restraints respond to minimum wage hikes by reducing turnover to a greater extent (from a higher baseline, as shown in figure 6) than franchises without such restraints.

We find that, controlling for variation across states, franchise chains, and time variation, franchise chains with an autonomy-reducing vertical restraint (RPM) see a larger positive minimum wage effect on real wages and a larger effect (in absolute terms) on the count of posted job ads. On the other hand, if franchisees have more autonomy thanks to exclusive territories, the minimum wage effect on real wages and the count of posted job ads are lower.

5.3 Stacked Event Study estimation results

Figures 10 and 11 plot the event study estimates for the log hourly wages and log count of job ads regressions, respectively, controlling for fixed effects as laid out in equations 5 and 6. The event study plots show that there is a significant positive effect of minimum wages on log hourly wages and a large but generally statistically insignificant negative effect of minimum wage increases on the log count of posted job ads. The event study charts on log hourly wages and job ads for the baseline results do not show any pre-trends, which suggests that our regression specification satisfies the parallel trends assumption.

When we consider chains with and without a given restraint, we find evidence of heterogeneous treatment effects in line with the difference-in-difference estimates. For franchisee autonomy-reducing vertical restraint (RPM), we find that the positive impact of minimum wage increases on real hourly wages and the corresponding negative effect of minimum wage increases on the count of posted job ads is larger in magnitude. In figure 12, RPM1 denotes treatment effect coefficients on the leads and lags for franchises in treated states operating with resale price maintenance relative to franchise chains in the control group. The point estimates for wage regressions are higher for franchise chains with an RPM restraint. Similarly, comparing the estimates from the job ads regressions shows that RPM-restrained franchise chains post fewer job ads than non-RPM-restrained chains following a minimum wage increase. The confidence intervals for event study estimates are wide, and for the RPM1 chains, there are pre-trends in the wage regressions. Still, the downward divergence between RPM 1 and RPM 0 is visually evident, suggesting that franchises with resale price maintenance (RPM) reduce turnover more than chains that don't use RPM in response to a minimum wage hike. Finally, the wage effect of a minimum wage increase is smaller for chains using exclusive territories, as is the effect on vacancy-posting.

6 Discussion

Our results document two key patterns in labor market outcomes across franchise chains: first, that vertical restraints significantly shape baseline wage and hiring behavior, and second, that such restraints mediate how firms respond to minimum wage increases. Autonomy-reducing restraints—such as resale price maintenance (RPM)—are associated with lower baseline wages and higher job vacancy postings, while autonomy-increasing restraints —such as exclusive territories —are associated with higher posted pay and fewer job postings. These baseline differences, established through two-way fixed effects estimates, provide a good theoretical and empirical reason for understanding heterogeneous responses to policy shocks such as a minimum wage increase.

Following minimum wage increases, franchise chains with vertical restraints show heterogeneous responses in both wages and hiring behavior. Stacked Difference-in-Differences estimates indicate larger positive wage effects and sharper reductions in job postings for chains in which franchisees operate with less autonomy (RPM), compared to chains without such restraints. To be precise, we find that an increase in the state-level minimum wage has a significant positive causal impact of 3.86% on real hourly wages and a negative causal impact of -1.36% on posted job ads. The causal impacts of minimum wage hikes for franchise chains with the autonomy-reducing restraint RPM are larger in absolute terms i.e. an increase in hourly pay by 7.55% and a decrease in job postings by 6.61%, larger estimates (in magnitude) than we find for chains that do not use RPM. For our franchisee autonomy-increasing restraint, exclusive territories, we find the opposite pattern: more pronounced effects of minimum wage increases for chains that do not grant franchisees an exclusive territory.

Dynamic stacked event study results broadly support these patterns. As a response to minimum wage increase real wages rise more strongly, and job postings fall more steeply, in chains where franchisees are less autonomous (RPM-restrained chains). Again, we find the opposite pattern when franchisees enjoy more autonomy thanks to an exclusive territory. This suggests that the combination of greater bargaining power vis-à-vis franchisors and enhanced output market power thanks to local monopolies means franchisees operate on larger margins, which may be partly shared with workers in the form of better labor standards.

Altogether, the evidence in this paper is consistent with the hypothesis that vertical restraints influence the elasticity of labor standards to variations in the policy floor. Resale Price Maintenance, which is our autonomy-reducing vertical restraint, appears to amplify the wage and hiring effects of minimum wage increases, possibly because franchisees in the chains that use them are more likely to be operating at or near the wage floor, because reduced franchisee autonomy impels franchisees to operate by worsening terms for their own employees. In contrast, autonomy-increasing arrangements like exclusive territories may allow franchisees more flexibility in absorbing policy shocks without needing to sharply adjust wages or hiring behavior. The less business autonomy employers enjoy, the worse employers they are from their workers' perspective, which is exactly the mechanism for the broad erosion of labor standards identified by Weil (2014).

7 Conclusion

In this paper, we examine the heterogeneous impact of minimum wage increases across franchise chains, incorporating variation in the application of vertical restraints. Using Stacked Difference-in-Differences and Stacked Event Study techniques, we find that increases in the statutory minimum wage have a positive effect on real hourly wages and a negative but statistically insignificant effect on the count of job vacancies. More importantly, for chains utilizing Resale Price Maintenance, i.e. the franchisor sets retail prices, we find that the real wage elasticity with respect to minimum wage increases is higher, and the job vacancy elasticity with respect to minimum wages is larger in magnitude. The opposite result holds for franchise chains with exclusive territories, which we characterize as an autonomy-increasing vertical restraint: franchisees who have exclusive territories pay better, and pay and turnover are not as elastic to changes in the statutory minimum wage.

Our results provide meaningful insights into the labor market effects of both minimum wage increases and vertical restraints in franchise chains: low-wage employers are the ones with less autonomy over their day-to-day business, and for whom labor standards set by policy are more binding.

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8 Tables and figures

Table 1: **Stacked DID wage regressions.** Each column reports the ATTs obtained from the regression specification in equation 3 for the entire sample (column 1) and for specific subsamples of franchise chains with and without a restraint (columns 2 -5). The last row reports p-values from t-tests comparing the post treatment coefficients between RPM0 and RPM1 (columns 2-3) and between Excl Terr 0 and Excl Terr 1 (columns 4-5). Sample sizes reflect the stacked panel structure and include multiple copies of the same observation across different treatment cohorts.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Base	RPM 0	RPM 1	Excl Terr 0	Excl Terr 1
post_treat	0.0386**	0.0133	0.0755***	0.0417**	0.0182
	(0.0144)	(0.0174)	(0.0252)	(0.0176)	(0.0199)
unemployment	0.0111	0.00451	0.0475*	0.0168**	0.0171**
	(0.00686)	(0.00615)	(0.0238)	(0.00767)	(0.00831)
Observations	3,429,959	2,082,894	773,232	1,991,310	864,640
R-squared	0.361	0.343	0.384	0.357	0.381
State by Stack FEs	Y	Y	Y	Y	Y
Franchise_ID by Stack FEs	Y	Y	Y	Y	Y
Year-Quarter FEs	Y	Y	Y	Y	Y
T-test: Excl0 vs Excl1 (p-val)					0.382
T-test: RPM0 vs RPM1 (p-val)			0.0483		

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

Note: Clustered at Statefips level.

Table 2: **Stacked DID count of job-ads regressions.** Each column reports the ATTs obtained from the regression specification in equation 4 for the entire sample (column 1) and for specific subsamples of franchise chains with and without a restraint (columns 2 -5). The last row reports p-values from t-tests comparing the post treatment coefficients between RPM0 and RPM1 (columns 2-3) and between Excl Terr 0 and Excl Terr 1 (columns 4-5). Sample sizes reflect the stacked panel structure and include multiple copies of the same observation across different treatment cohorts.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Base	RPM 0	RPM 1	Excl Terr 0	Excl Terr 1
post_treat	-0.0136	-0.0163	-0.0661	-0.0289	-0.00844
	(0.0294)	(0.0366)	(0.0470)	(0.0429)	(0.0357)
unemployment	-0.0272*	-0.00699	-0.00158	-0.0131	0.0168
	(0.0154)	(0.0208)	(0.0242)	(0.0208)	(0.0218)
Observations	600,278	122,311	76,281	123,472	75,046
R-squared	0.638	0.647	0.600	0.639	0.612
State by Stack FEs	Y	Y	Y	Y	Y
Franchise_ID by Stack FEs	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y
T-test: Excl0 vs Excl1 (p-val)					0.716
T-test: RPM0 vs RPM1 (p-val)			0.408		

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

Note: Clustered at Statefips level.

Table 3: Estimations of minimum wage increases on real wages, job vacancies, turnover rate and employment(selected papers)

Literature	**Average treatment effect on wages**	**Average treatment effect on overall employment, layoffs, quit rates**	**Average treatment effect on job vacancies/job turnover**	**Time period**
Dube et al. (2016)	10% increase in min wage leading to 2.2% increase in average hourly wage	No significant disemployment effects	10% minimum wage increase leads to a fall in turnover rates by 2%.	2000-2011
Cengiz et al. (2020)	The effect of the minimum wage on average wages is 6.8%.	Employment elasticity with respect to Minimum wage is 2.8% (std. err. 2.9%).		1976-2016
Kudlyak et al. (2022)	-	-	10% increase in minimum wage leads to a 2.4% decrease in job vacancies for at-risk occupations.	2005-2018
Wursten and Reich (2023)	10% minimum wage change increases average wages by 1.6 percent	No significant disemployment effects	-	1990-2019

Figure 1: **Share of franchise chains and job ads posted by franchise chains with a vertical restraint in place.** Our full sample contains information for 740 unique franchise chains with 6,712,977 posted job vacancies between 2010q1 and 2020q4 across the 50 states plus the District of Columbia. Based on this sample we calculate the share of posted job ads, and the share of unique franchise chains with RPM and Exclusive Territory Restraints for every year-quarter. The share of job ads is computed as the ratio of job ads posted by franchise chains with a given vertical restraint to the total number of job ads posted in that year-quarter. The share of franchise chains is computed as the ratio of franchise chains with a given vertical restraint to the total number of franchise chains active in that year-quarter. Solid lines show the share of job ads and dotted lines show the share of franchise chains.

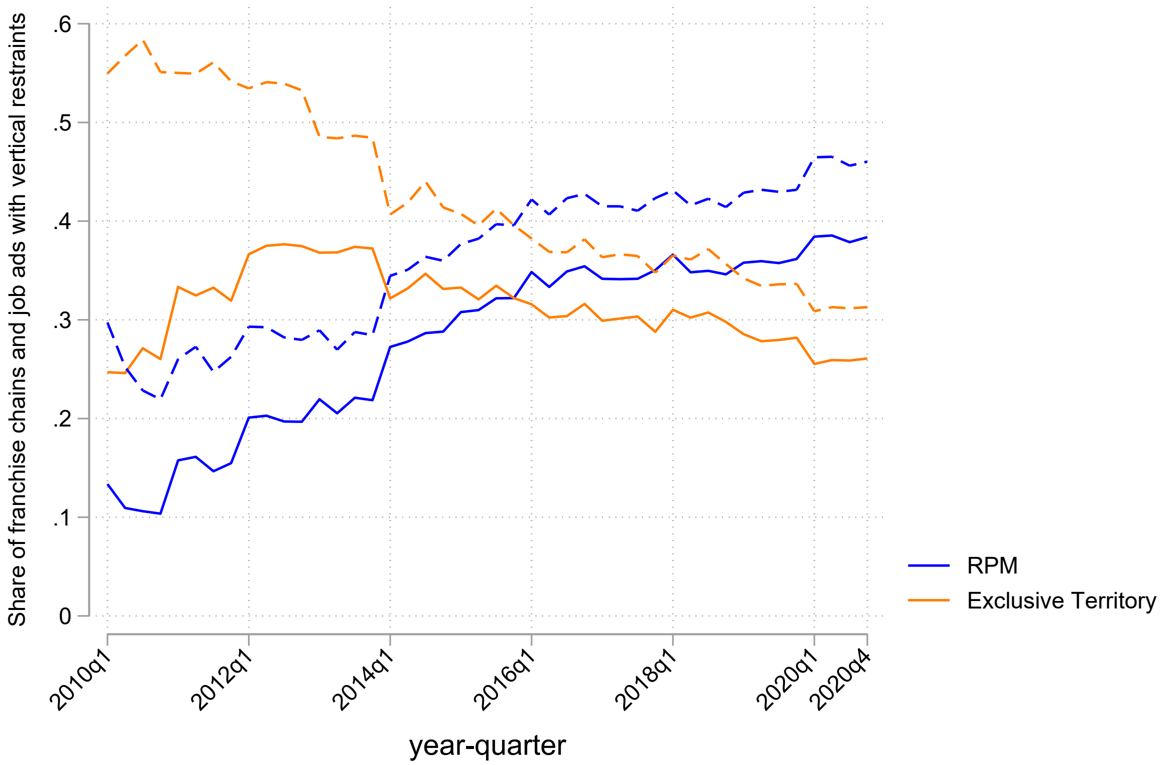

Figure 2: **Minimum wage across states 2012q1-2016q4 (quarterly).** This figure plots state-level quarterly minimum wage increases based on Vaghul and Zipperer (2016) for all the states in our sample. Only events between 2012q1 and 2016q4, which is our preferred set of treatments with a full set of pre- and post-periods estimated, are considered. Labels for only selected states are shown.

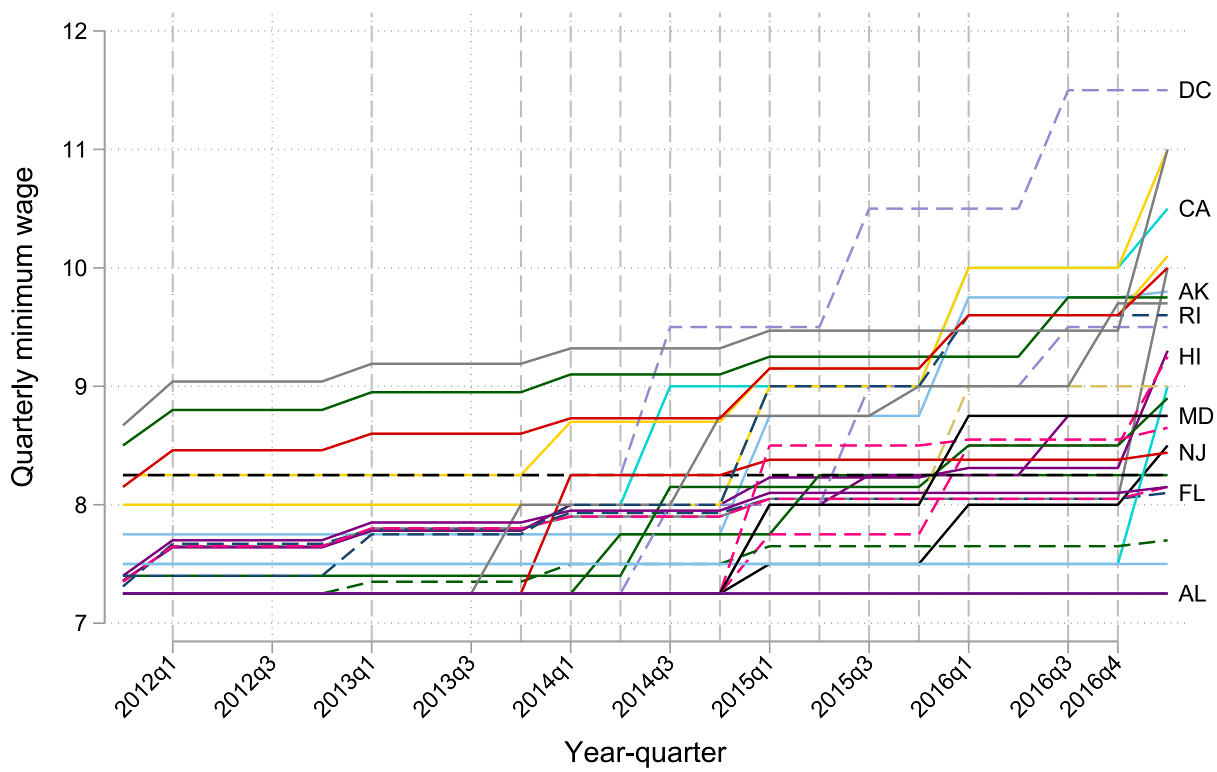

Figure 3: **Distribution of the difference between log real hourly wage and log minimum wage for all workers.** This figure plots the distribution of the difference between log hourly wage for a given franchise chain x state x year-quarter cell from the state level quarterly minimum wage for the entire sample (consisting of 932,789 job ads from Lightcast). The sample consists of a higher share of observations where posted wages at the job ad level are greater than the imposed state-level minimum wage.

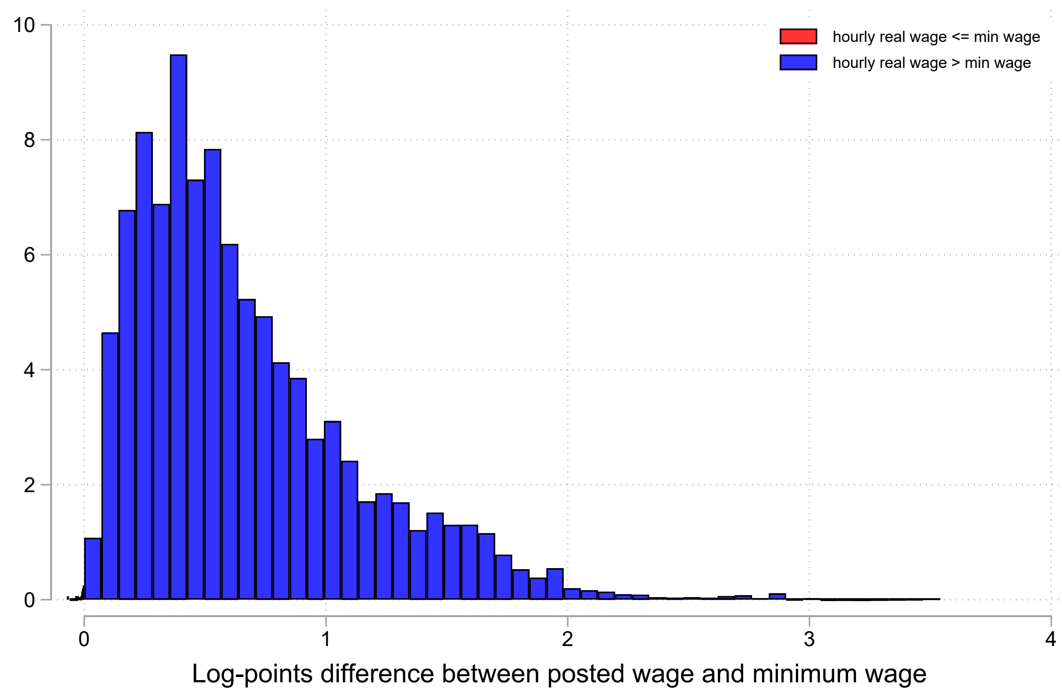

Figure 4: **Posted job ads 2010q1-2020q4**. This figure plots the total count of job ads per year-quarter, summing across all franchise chains in a year-quarter.

Figure 5: **Summary of coefficient estimates of log hourly wage on vertical restraints.** Coefficients for each restraint are obtained from 1 for log hourly wages, controlling for year-quarter and franchise chain level fixed effects.

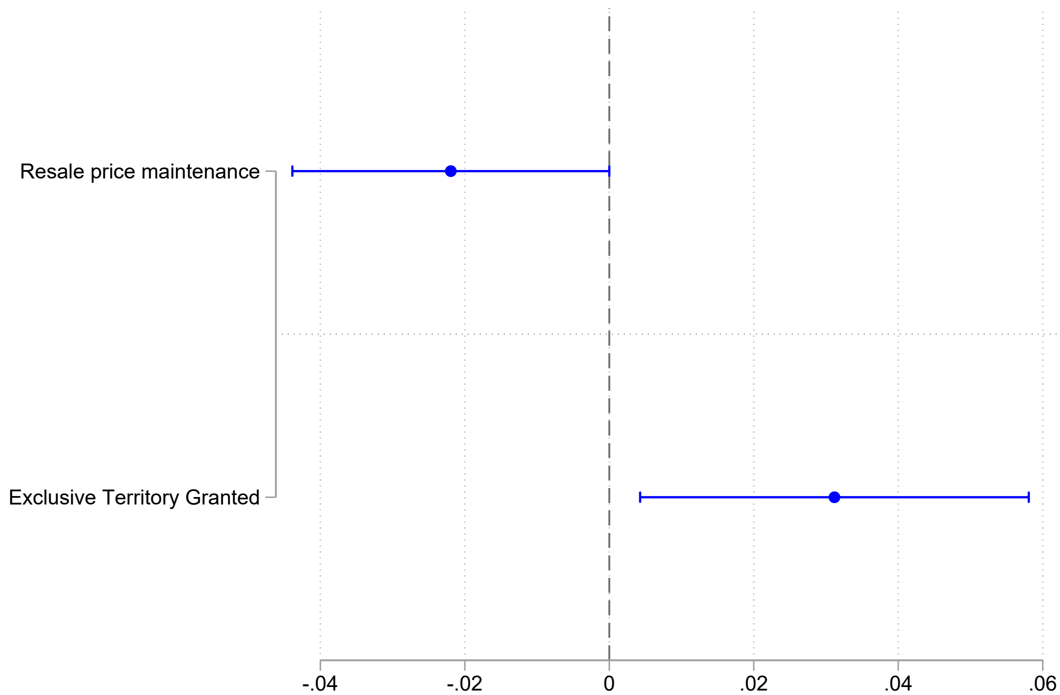

Figure 6: **Summary of coefficient estimates of log count of job ads on vertical restraints.** Coefficients for each restraint are obtained from 2 for log count of job ads, controlling for year-quarter and franchise chain level fixed effects.

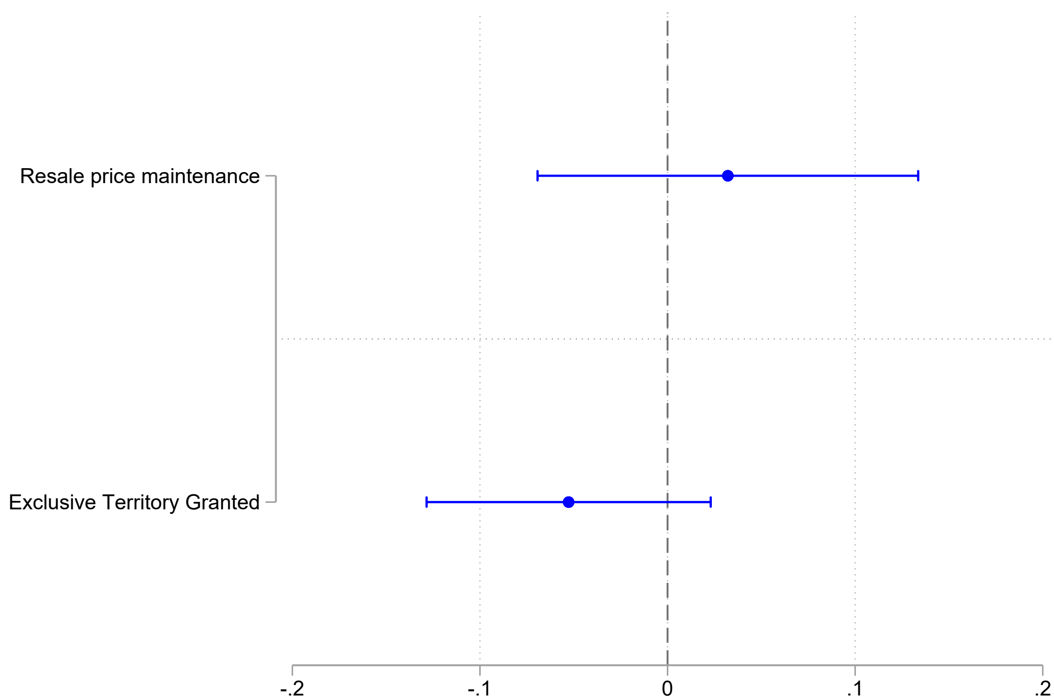

Figure 7: **Control and treatment groups corresponding to the state-level minimum wage increases in 2014q1.** Treated states (in blue) correspond to the group of states for which a minimum wage hike occurred in 2014q1. Control states (in yellow) are those with no minimum wage increase in the entire treatment period (2012q1-2016q4). The states designated in white are neither in the treatment nor the control group for the 2014q1 stack because they are treated at other times within our treatment window.

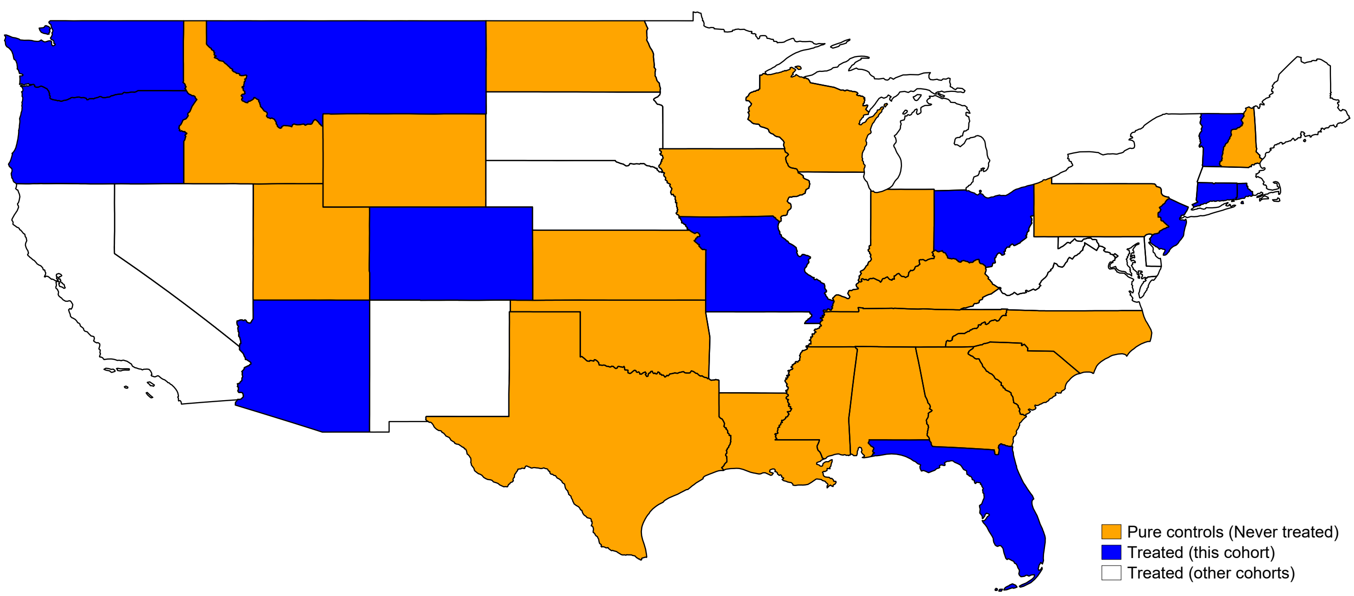

Figure 8: **Average Treatment Effects - Stacked DID estimates for wage regressions.** ATTs corresponding to log hourly wage regressions from the estimating equation 3 for the effect of increasing minimum wage on log hourly wage.

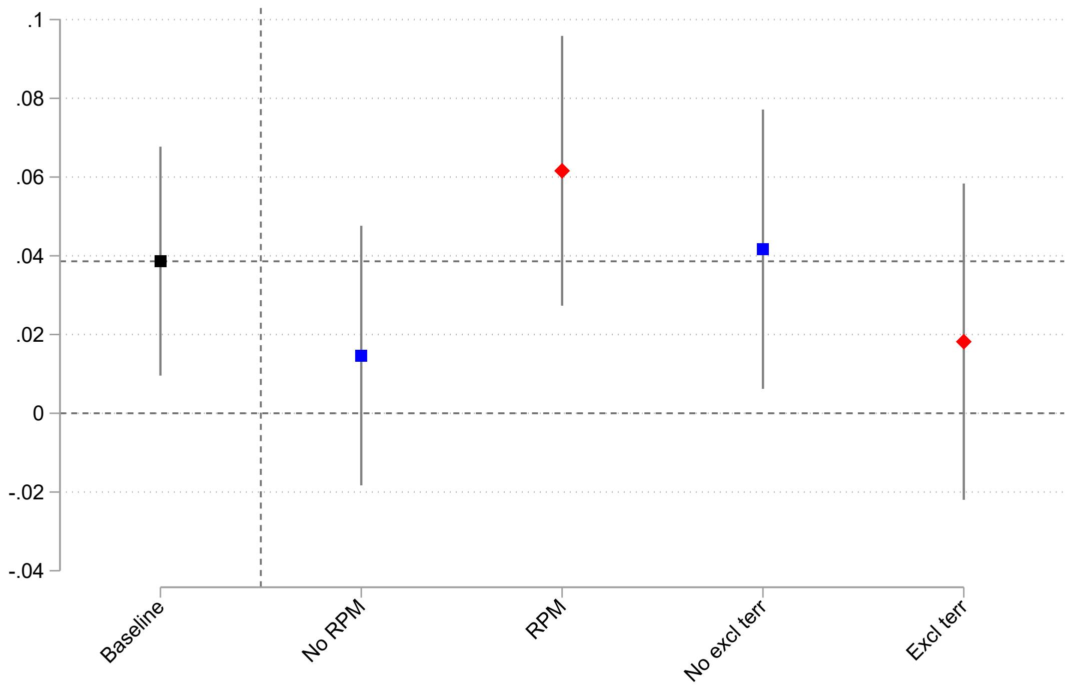

Figure 9: **Average Treatment Effects - Stacked DID estimates for job ads regressions.** ATTs correspond to log count of job ads regressions from the estimating equation 4 for the effect of increasing minimum wage on log count of job ads.

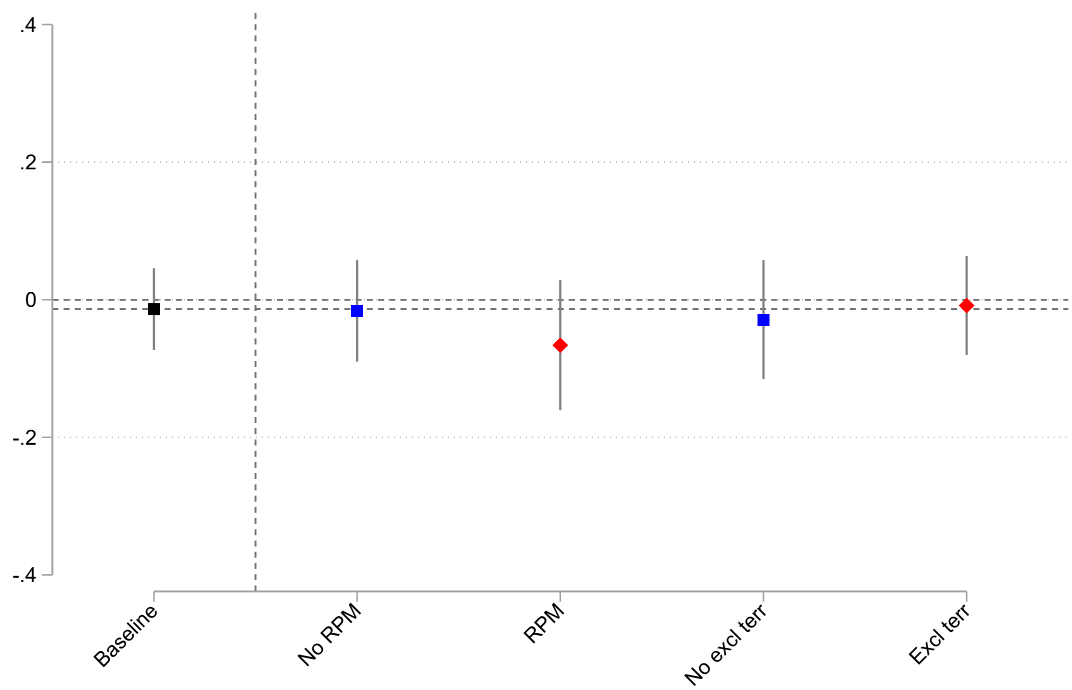

Figure 10: **Average Treatment Effects - Stacked Event Study estimates for log hourly wage regressions**. Event-study ATTS correspond to log hourly wage regressions from the estimating equation 5 for the effect of increasing minimum wage on log hourly wage controlling for upto eight quarters leading upto the minimum wage and sixteen quarters post the minimum wage increase.

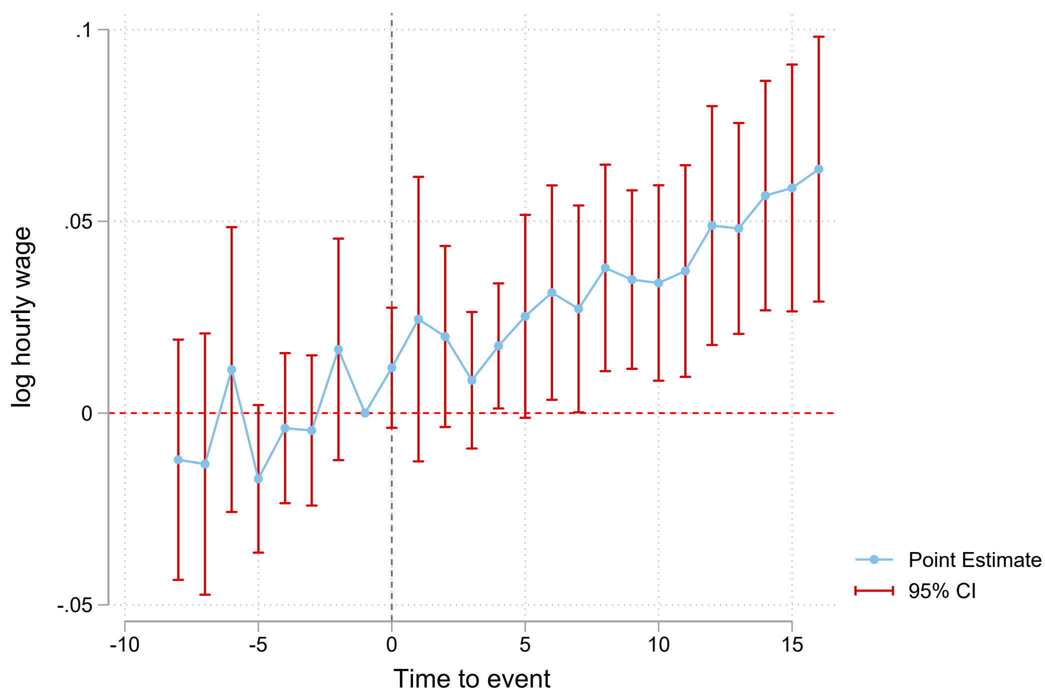

Figure 11: **Average Treatment Effects - Stacked Event Study estimates for log count of job ads regressions.** Event-study ATTS correspond to log count of job ads regressions from the estimating equation 6 for the effect of increasing minimum wage on log count of job ads controlling for upto two years leading upto the minimum wage and four years post the minimum wage increase.

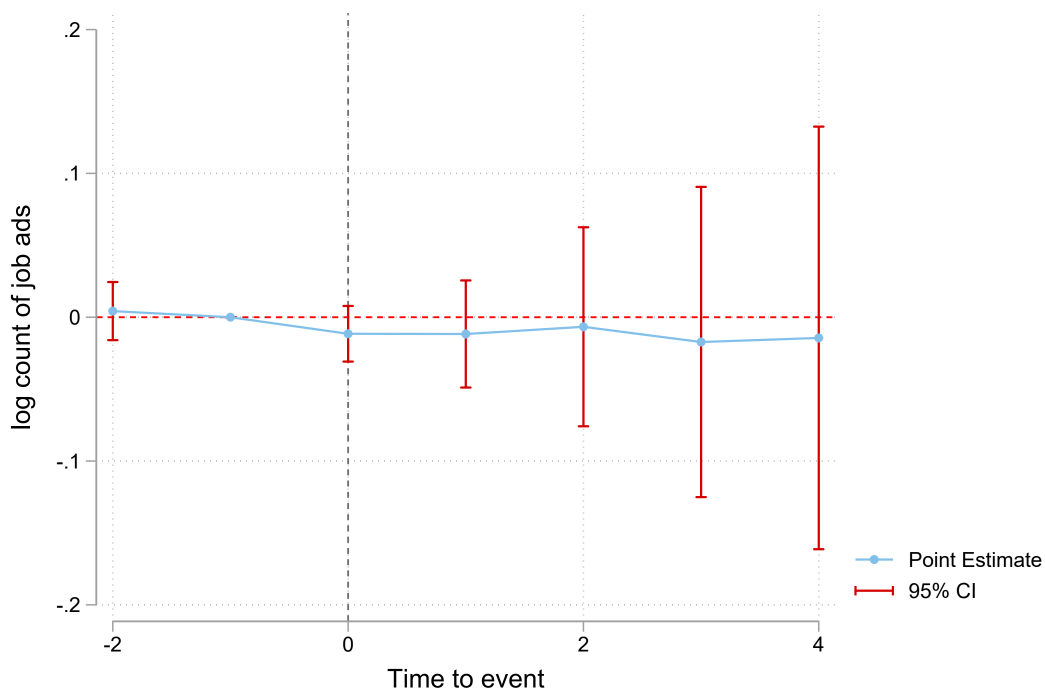

Figure 12: **Average Treatment Effects - Stacked Event study estimates - RPM.** The figures below plot the effect of minimum wage increases on log hourly wages and log count of job ads in the specified stacked event study approach outlined in equation 5 and equation 6 for franchise chains that use Retail Price Maintenance (RPM 1) and franchise chains that do not use Resale Price Maintenance (RPM 0).

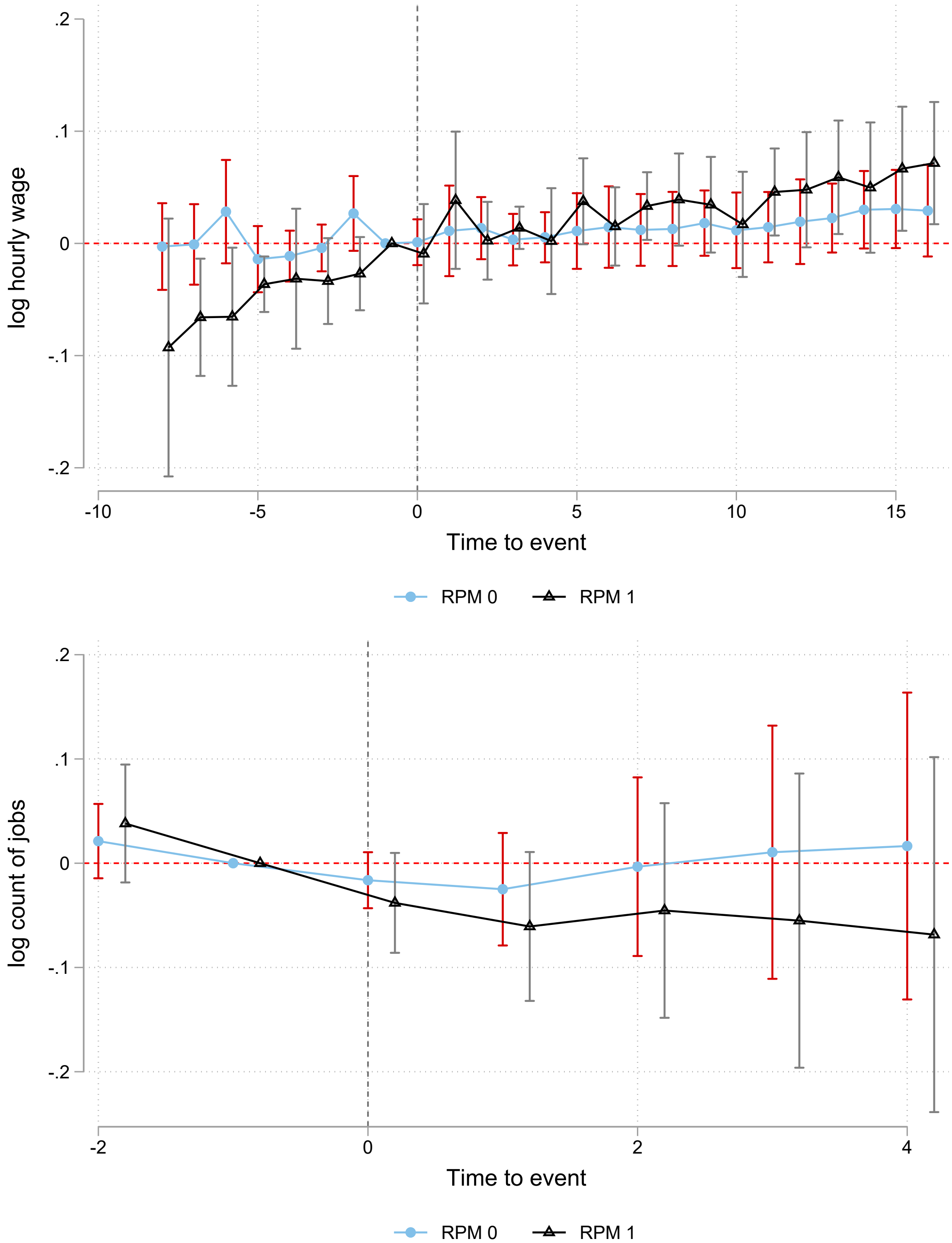

Figure 13: **Average Treatment Effects - Stacked Event study estimates - Exclusive Territory.** The figures below plot the effect of minimum wage increases on log hourly wage and log count of job ads in the specified stacked event study approach outlined in equation 5 and equation 6 for franchise chains that grant an exclusive territory (Excl terr 1) and franchise chains that do not (Excl terr 0).