Non-Monotonic Employment Effects by Market Structure and Minimum Wage Level*

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Abstract

Minimum wages decrease employment in competitive markets, but increase it in monopsonistic markets until reaching the marginal product of labour. We find non-monotonicity both by market structure and minimum wage level. Minimum wage hikes initially increase hours worked for minimum wage workers (MWWs) in high-concentration local labour markets (LLMs), while increasing job loss likelihood for MWWs in low-concentration LLMs. Repeated hikes reverse initial hours gains. Observing minimum wage status facilitates both within- and across-market difference-in-difference designs, whose findings provide mutual support. We combine these into a triple-difference. Our results resolve the lack of consensus around the minimum wage's employment effects.

JEL codes: J22, J23, J38, J42

Keywords: Minimum wage, Monopsony, Oligopsony, Local labour Markets.

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1 Introduction

Thirty years since the advent of the new minimum wage literature, there is little consensus on whether the employment effect of minimum wage hikes is positive, negative, or zero. While due in part to disagreement over the appropriate control group, a number of studies report estimates ranging from positive to negative using the same specification for different hikes, for the same hike in different areas, or for subsequent hikes in a given area (Neumark and Wascher 2002, Dube et al. 2010, Cengiz et al. 2019, Wang et al. 2019, McGuinness et al. 2019, Redmond and McGuinness 2022, Jardim et al. 2022).

Theory predicts that the sign of the employment effect depends on a) whether the minimum wage exceeds the marginal product of labour, and b) market structure. Hikes up to the marginal product of labour have zero effects in competitive markets, where they do not bind, but positive effects in monopsonistic markets, where employers have suppressed wages and employment. Minimum wages exceeding the marginal product of labour reduce employment in any market.

In this light the lack of consensus on the employment effect of minimum wages is unsurprising. Different markets may indeed show opposite effects, and a given market can show opposite effects at different minimum wage levels. Pooled estimates over different hikes in different markets, a common hike applied across markets (such as a national minimum wage), or even subsequent hikes in a given market, may combine effects with opposite signs, producing null results.

We test a) whether employer-concentrated local labour markets (LLMs) show more positive employment effects, and b) whether higher minimum wage levels cause more negative employment effects in these markets.[1] For each of three successive hikes in the Irish National Minimum Wage (NMW) from 2016-2019, we compare changes in hours and probability of job loss for minimum wage workers (MWWs) in high-concentration LLMs to i) MWWs in low-concentration LLMs, and ii) non-MWWs within-LLM. These alternative *across*- and *within*-market

[1]We measure LLM concentration using the Herfindahl-Hirschman Index (HHI) of employment in the intersection of a two-digit NACE industry and NUTS3 region.

difference-in-difference designs yields quantitatively similar treatment effects, mutually supporting each other's identification assumptions. We combine them into a triple-difference design to produce our preferred estimates.

The 2017 and 2018 NMW hikes increased usual weekly hours worked for MWWs in high-concentration LLMs with no effect on job loss likelihood. MWWs in low-concentration LLMs show no hours response, but increased job loss (defined as employment to non-employment transitions). Non-MWWs show economically small responses along both margins in either market type. By 2019, hours gains for MWWs in high-concentration markets reversed; point estimates of hours effects are uniformly negative, though statistically insignificant for most specifications. Job loss also shows a statistically insignificant increase for these workers, while remaining high for MWWs in low-concentration markets.

Figures 1 and 2 plot these results. They are consistent with monopsonistic competition in high-concentration LLMs, with a minimum wage that reached (or neared) the marginal product of low-wage labour by 2019. In appendix A we show the results robust to measuring actual hours worked rather than usual, to restricting the sample to workers observed both before and after each hike, or to LLMs with at least 20 MWWs observed, and to omitting from the control group workers from either the top or bottom two income deciles. Sub-group analysis shows that prime-age workers (25-54) drive hours gains through 2018, as well as losses in 2019. Gains went disproportionately to MWWs with a tertiary education, while those with only a primary education suffered subsequent losses.

A key advantage of our data is the ability to observe minimum wage status directly. The Irish Labour Force Survey (LFS) informs respondents of the current hourly minimum wage, and asks them whether they earn that amount (see section 2 for details). This gives us a cleaner indicator of minimum wage status than studies that impute it by dividing earnings by hours worked[2] – both of which are subject to measurement error in survey data (Bound and Krueger 1991, Borjas and Hamermesh 2024), which can bias treatment effects downwards (Bossler and

[2]See Stewart (2004b), Stewart (2004a), König and Möller (2009), Connolly and Gregory (2002).

Figure 1: Hours Worked

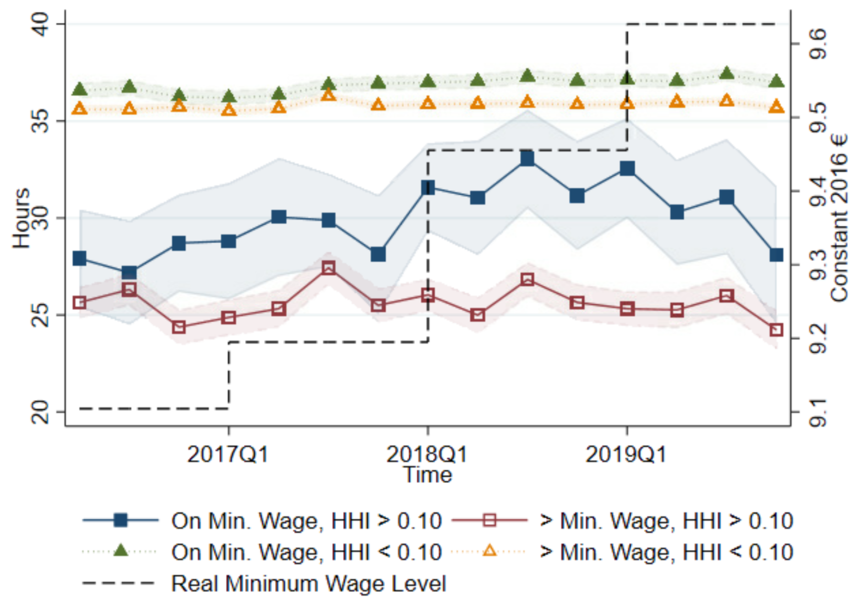


Figure plots mean usual hours worked in the reference week for MWWs and non-MWWs, in LLMs with employment HHI above and below 0.10 (equivalent to ten equally-sized employers) as of 2016. Minimum wage status based on current quarter. Dashed line indicates minimum wage hikes. 95% confidence intervals.

Figure 2: Likelihood of Job Loss

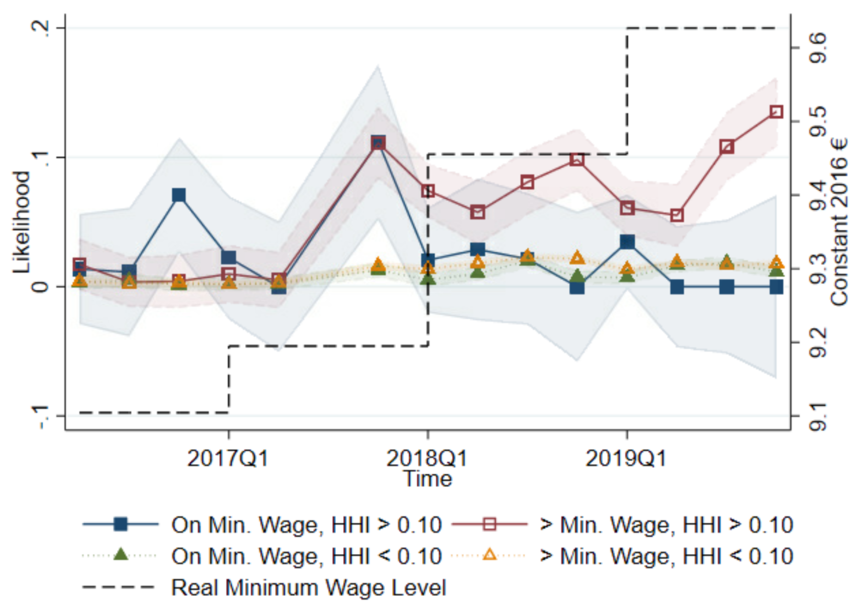


Figure plots the average rate of transition from employment to non-employment for MWWs and non-MWWs, in LLMs with employment HHI above and below 0.10 (equivalent to ten equally-sized employers) as of 2016. Minimum wage status based on previous quarter. Dashed line indicates minimum wage hikes. 95% confidence intervals.

Westermeier 2020). However, we do not observe hourly wages for non-MWWs. We therefore cleanly identify the treatment group, but the control group may contain a small number of workers directly affected by hikes – those earning between the old and new minimum wage – and others subject to spillovers, earning slightly above the new minimum wage. Robustness checks excluding workers from the bottom earnings deciles from the control group likely remove most of both groups, and confirm the main results.

Observing minimum wage status allows us to identify the causal effect of NMW hikes by comparing MWWs to non-MWWs within-market rather than comparing across jurisdictions, avoiding local policy endogeneity and geographic spillovers.[3] Our identifying assumption requires that local employment shocks unrelated to the NMW affect high- and low-wage workers similarly within a location. McGuinness et al. (2019) use the same approach to identify the employment effect of NMW hikes on MWWs using the Irish LFS. Cengiz et al. (2019) and Dustmann et al. (2022) find that minimum wages have no effect on the upper tail of the wage distribution, supporting our use of non-MWWs as a control group.

Cengiz et al. (2022) emphasize the advantages of identifying MWWs rather than resorting to proxies, using machine learning to predict minimum wage status in the CPS based on a subsample of workers reporting hourly wages. First, it avoids the problem of extrapolating to the broader population estimates from subgroups such as teens,[4] the low-educated, or workers in low-wage occupations or industries. These groups exclude large numbers of MWWs and include non-MWWs, and are usually not of specific policy interest. Second, it allows formation of a placebo group of non-MWWs. Our study shares these strengths.

Ireland having no regional variation in minimum wage level, cross-sectional

[3]Jardim et al. (2024) find geographic spillovers on wages and hours following Seattle's minimum wage hike. McKinnish (2017) and Perez (2022) find that low-wage workers increase commuting out of state after minimum wage hikes, while Kuehn (2016) and Shirley (2018) find the opposite. These channels would amplify or attenuate treatment effects respectively if spillovers were ignored. Recent studies using geographic controls avoid such confounding by excluding the nearest portions of control regions (Jardim et al. 2022, Jha et al. 2022). **cite Monras (2019)**

[4]Manning (2021) observes that teen workers are a small and decreasing share of the labour force, and of the minimum wage labour force.

variation in our data is in market concentration. Our across-market comparisons therefore do not identify the effect of hikes on low-wage employment, but only their differential effect on low-wage employment in concentrated LLMs.[5] This removes a key source of variation used in many US studies, but rules out across-market wage spillovers that may confound treatment effects. Bailey et al. (2021) and Giupponi et al. (2024) study national minimum wages in the US and UK respectively, both finding large wage increases and small disemployment effects.

A recent literature studies differential employment effects by market concentration. Azar et al. (2023) find that minimum wage hikes decrease total employment in low-concentration LLMs, but increase it in high-concentration markets, using both occupational and industrial definitions of US LLMs. Munguía Corella (2020) and Popp (2023) find similar results on teen employment and per-establishment full-time employment in the US and Germany respectively. Okudaira et al. (2019) find that Japanese manufacturing plants with greater wage markdowns do not reduce employment growth following minimum wage hikes. Wiltshire (2021) finds employment gains in US counties with a Walmart Supercenter relative to a set of synthetic controls. These studies combine quasi-experimental variation from minimum wage hikes with an empirical proxy for monopsony power to confirm a key prediction of the monopsonistic competition model.

We contribute by showing the first evidence of non-monotonic employment effects by minimum wage level in monopsonistic labour markets. In contract to Azar et al. (2023), who find the most negative employment effects in low-concentration markets, we find negative employment effects both in low-concentration markets, and in high-concentration markets for the highest levels of the minimum wage. Like them, we find positive employment elasticities of minimum wage hikes in high-concentration markets, but only when the minimum wage is below its 2019

[5]Alone, differentially positive employment effects in concentrated LLMs could result from unobserved productivity differences if low-concentration LLMs have higher marginal productivities of labour, either because of agglomeration effects or geographic advantages (Ellison and Glaeser 1997). A common minimum wage applied across all markets may then bind in high-concentration (low productivity) markets, but not in low-concentration (high productivity) markets, producing a positive differential effect in the latter. However, this would not explain the positive employment effects we find from the within-market difference-in-difference.

level. We find no evidence of non-monotonicity in low-concentration markets. By confirming another prediction of the monopsony model, our results bolster previous findings that concentrated labour markets behave monopsonistically. Our findings also agree with Berger et al. (2022), who find a relatively low threshold for the full-employment wage in a model calibrated to US data, illustrating the limits of the minimum wage as a policy tool.

Despite the theoretical basis for non-monotonicity, few past studies look for it. An exception is Jardim et al. (2022), who analyze separately the employment effects of two successive minimum wage hikes in Seattle. They find no discernible effect of the first hike, but statistically significant disemployment following the second. Clemens and Strain (2021) find that large minimum wage hikes reduce employment rates for low skilled workers, while the effect of small hikes varies around zero. As these are average effects over many markets, they are consistent with small hikes causing gains in monopsonistic and losses in competitive markets, with large hikes exceeding the full-employment wage and causing losses in both. Wang et al. (2019) use machine learning to classify US states into four groups with different treatment effects, finding a positive employment elasticity for three groups, and a negative for one. They do not allow for different effects of subsequent hikes within-group. This heterogeneity may reflect differences both market structure and the level of the full-employment wage.

None of these studies measure market concentration. Because the non-monotonic employment effects of minimum wages depend on market structure, pooling estimates across market types masks non-monotonicity by level in the same way that is masks non-monotonicity by market. We show that pooled estimates over all markets show the same qualitative pattern of non-monotonicity, but with economically negligible magnitudes and no statistical significance. Concentrated (monopsonistic) markets show non-monotonic effects, while non-concentrated (competitive) markets show monotonic effects, as predicted by the monopsony model.

The next section reviews the institutional background of minimum wages in Ireland and our data. Section 3 presents our empirical specification, section 4 the hours results, and section 5 the results on job loss likelihood. Section 6 concludes.

2 Institutional Background and Data

The Low Pay Commission (LPC) of Ireland was established in 2015 to make recommendations to the Irish government on minimum wage policy. It consists of industry and labour representatives, as well as academics, and submits a public report every July giving the recommended National Minimum Wage (NMW) for the following year. Governments have implemented the LPC recommendation every year since its inception. Their primary aim is as follows.

> "To have a minimum wage that provides an incentive to work, is set at a rate that is both fair and sustainable, and helps as many people as possible, without a significant adverse effect on competitiveness or a significant negative effect on employment."

Given their mandate, we expect LPC recommendations for the NMW to be endogenous to the state of the economy. Indeed, the LPC annual reports from 2016 to 2018, each recommending a NMW hike, all mention predictions of strong growth in the Irish economy.[6]

We begin our study in 2016, when the Irish Labour Force Survey (LFS) started asking respondents their minimum wage status. The LPC recommended hikes of €0.10, €0.30, and €0.25 for 2017, 2018, and 2019, corresponding to 0.7%, 2.7%, and 1.7% real increases respectively. The government implemented each of these in January of the corresponding year. In the first quarter of 2020 the Irish government introduced pandemic restrictions, which persisted in various forms until 2022. As we doubt that the NMW is a primary determinant of employment and hours worked during this period, we limit our study to the 2016-2019 period.

2.1 Labour Force Survey

The LFS contains quarterly data on employment status, hours, industry and region of employment, income decile, demographic characteristics including age, sex, and education level – and crucially, from 2016 on: minimum wage status.

[6] Low Pay Commission 2016, Low Pay Commission 2017, Low Pay Commission 2018.

Table 1: Summary Statistics: Hours Sample

	Mean	Median	Stdev	Min	Max	N
Actual Hours Worked	34.9	39	11.27	1	95	159870
Usual Hours Worked	35.2	39	10.41	1	95	175573
Minimum Wage Worker	.08	0	.27	0	1	175573
Male	.49	0	.5	0	1	175573
Age	40	39	12.13	15	87	175573
Secondary School	.45	0	.5	0	1	175573
University	.45	0	.5	0	1	175573
Has Children	.42	0	.49	0	1	175573
HHI	.08	.03	.13	0	1	175573
HHI $\geq$ 0.25	.09	0	.29	0	1	175573
HHI $\geq$ 0.10	.19	0	.4	0	1	175573
HHI $\geq$ Median	.54	1	.5	0	1	175573
$\geq$ 20 MWWs in LLM	.75	1	.44	0	1	175573
Year	2018	2018	1.08	2016	2019	175573

Unit of observation: a worker-quarter

Table 2: Summary Statistics: Job Loss Sample

	Mean	Median	Stdev	Min	Max	N
Employed	0.99	1	0.12	0	1	95271
Actual Hours > Zero	0.91	1	0.28	0	1	93740
Employed to Non-employed	0.01	0	0.12	0	1	95271
Employed to Unemployed	0	0	0.04	0	1	94102
Positive Hours to Zero	0.07	0	0.26	0	1	85704
Minimum Wage Worker (Lagged)	0.06	0	0.24	0	1	95271
Male	0.49	0	0.5	0	1	95271
Age	41	41	11.61	15	88	95271
Secondary School	0.44	0	0.5	0	1	95271
University	0.46	0	0.5	0	1	95271
Has Children	0.44	0	0.5	0	1	95271
HHI	0.09	0.03	0.14	0	1	95271
HHI $\geq$ 0.25	0.1	0	0.3	0	1	95271
HHI $\geq$ 0.10	0.2	0	0.4	0	1	95271
HHI $\geq$ Median	0.57	1	0.49	0	1	95271
$\geq$ 20 MWWs in LLM	0.74	1	0.44	0	1	95271
Year	2018	2018	1.05	2016	2019	95271

Unit of observation: a worker-quarter

The survey asks the following.

> "The National Minimum Wage is €X per hour. Are your gross hourly earnings excluding bonuses, overtime and allowances:
>
> - Less than €X per hour
> - Exactly €X per hour
> - More than €X per hour?"

If paid less than the NMW, the surveyor solicits an explanation. The vast majority of responses indicate that the respondent is being paid a youth or training wage; minimums for such workers are proportional to the NMW, and rise in proportion. We classify MWWs as all those being paid exactly or less than the NMW.[7]

Observing minimum wage status allows us to precisely identify the workers directly affected by minimum wage changes, providing an advantage over studies using low-wage industries or occupations or teen employment to approximate the treatment group of affected workers. It is also a more direct indicator of minimum wage status than the typical approach of dividing reported income by hours worked – both of which are potentially reported with error.[8] However, we do not observe hourly wages for non-MWWs. Because minimum wage hikes affect workers earning slightly more than the minimum wage (Cengiz et al. 2019, Dustmann et al. 2022), there likely exist spillovers onto a small number of non-MWWs.[9]

We consider hours worked as the main employment outcome. The LFS reports

[7] We do not observe non-compliance directly, though this may fall into the 'other' category. McGuinness et al. (2020) estimate that 5.6% of MWWs and sub-MWWs are non-compliant using the same dataset. Yaniv (2001) shows that partially-complying employers can respond to minimum wage hikes by employing the full-compliance quantity of labour.

[8] Borjas and Hamermesh (2024) find discrepancies in full-time employment status for over 20% of workers appearing in both the Current Population Survey (CPS) and its Annual Social and Economic Supplement (ASEC). Comparing reported earnings from the CPS to social security records, Bound and Krueger (1991) find that 20% of variation in male earnings is due to measurement error. Bossler and Westermeier (2020) conduct simulations showing that employment effects of minimum wage hikes are biased downwards by 30% due to measurement error.

[9] In appendix A we perform a robustness check that excludes from the control group workers from the bottom two income deciles.

both usual weekly hours worked and actual hours worked in the reference week. We use the former in the main text, and replicate the main results using the latter in appendix A. Usual hours are always positive if reported, while actual hours are sometimes zero, which we code to missing. The sample population is every respondent reporting usual hours, which we call the *hours sample*.

As the LFS is a short panel, following workers for up to five quarters, we can impute job loss by tracking employment status. We consider employment to non-employment transitions as the primary job loss outcome. Because the non-employed do not report MWW status, we use the one-quarter lagged value for job loss analysis. We consider as alternative job loss outcomes employment to unemployment transitions, and positive hours to zero hours transitions in appendix A. The sample population consists of every respondent who reports being employed in the previous quarter, including some non-employed and reporting zero hours, but necessarily drops the first quarter in which each respondent appears as lagged data is unavailable. We call this the *jobs sample*.

2.2 Business Register

We match worker outcomes from the LFS to employer concentration measurements from the Business Register (BR) at the local labour market (LLM) level. The BR contains an entry for every formal sector business in Ireland each year. Each business reports the county (or sub-county jurisdiction) in which it is registered, the industry in which it operates, and the number of employees.

We define a local labour market (LLM) as an industry-region, using the two-digit NACE industry and NUTS 3* region of employment.[10] Our definition of NUTS 3* regions follows the NUTS 3 regional definitions used to allocate EU

[10]Past work on wage suppression shows that defining LLMs according to occupation and industry produces similar results (Azar et al. 2020, Rinz 2022, Benmelech et al. 2020), as does allowing for wage spillovers across industry (Arnold 2021) or occupation (Schubert et al. 2021). Dodini et al. (2023) construct a novel task-based index of LLM concentration, finding that local industry HHIs compare favourably with it in predicting hours (as measured by full-time versus part-time status) and labour force participation – the same outcomes we study. Constructing a comprehensive index of outside options, Caldwell and Danieli (2024) find that distance to the workplace is the key predictor of a workers' next job.

structural funds, except that we combine Dublin and the Mideast into a single region, which we term 'Greater Dublin'. This is because of extraordinarily high rates of commuting between these regions (Devereux and Studnicka 2024). The other six regions, which coincide exactly with NUTS 3 regions, are: the Border, West, Midwest, Midlands, Southeast, and Southwest. Regional boundaries are stable throughout our sample period.

NUTS 3* regions nest Functional Urban Areas, which the OECD designs explicitly to cover commuting zones (OECD 2022). Greater Dublin corresponds exactly to the Dublin FUA. The West, Midwest, Southwest, and Southeast contain respectively the Galway, Limerick, Cork, and Waterford FUAs – each along with contiguous county neighbours that do not fall into other FUAs. The Midlands and Border regions lack cities large enough to constitute the core of a FUA. Like US commuting zones, NUTS 3* regions nest counties, and are similar in population and geographic area. Moreover, cross-region commuting patterns are similar. Monte et al. (2018) find that for the average US commuting zone, 8% of residents commute outside the commuting zone, and 7% of workers commute in; we calculate the corresponding figures for Ireland at 9.1% and 6.8%.

We measure LLM concentration using the Herfindahl-Hirschman Index (HHI). Consider a market m which contains some number of firms, indexed by f. Firm f employs n_f employees. The HHI of market m is given by

$$HHI_m = \sum_{f \in m} \left(\frac{n_f}{\sum_{g \in m} n_g} \right)^2$$

which is the sum of squared employment shares of each firm. We use HHI as of 2016, as the NMW hikes themselves may affect subsequent changes in concentration.[11]

[11] All results are robust to using contemporaneous HHI, or mean HHI over the sample period. HHI changes little within market from year to year over our sample period, and using contemporaneous HHI or average HHI over the sample period produces nearly identical estimates.

2.3 Hours Sample

Table 1 reports summary statistics for the hours sample. The average worker works around 35 hours per week, with actual hours slightly below usual hours. Only 8% of workers earn the minimum wage, and 9% of all workers work in LLMs which had a HHI above 0.25 in 2016 – the (contemporaneous) threshold for the US Federal Trade Commission's definition of a highly-concentrated market, equivalent to four equally-sized employers. When estimating heterogeneous effects of minimum wage hikes by LLM, we use this threshold as a baseline to divide concentrated from non-concentrated LLMs. We also consider a HHI threshold of 0.1, equivalent to ten equally-sized employers, above which 19% of workers fall. The median LLM has a HHI of 0.03, equivalent to 33 equally-sized employers, and we also consider this threshold. Minimum wage work is broadly distributed; nearly three-quarters of workers work in LLMs with at least 20 MWWs observed.

2.4 Job Loss Sample

Table 2 shows summary statistics for the job loss sample, consisting of worker-quarters for whom we observe the worker to be employed during the previous quarter. Unlike the hours sample, this includes some unemployed workers – but very few, as most unemployed remain so throughout the sample period. Only one percent of observations report non-employment. Nine percent report working zero hours in the reference week, with the majority of these having reported positive hours in the previous quarter.

As observing job loss requires lagged employment status, the sample size is considerably smaller (although we include some non-employed worker-quarters in this sample that we do not include in the hours sample). In addition, because non-employed workers do not report minimum wage status, nor industry and region of work, we use lagged values for these variables. A smaller proportion of this sample reports being on the minimum wage (in the previous quarter) compared to the hours sample, at only six percent. However, the concentration distribution of markets is similar, with 10% of markets with HHI above 0.25 and 20% above

0.10, with nearly three quarters of markets having at least 20 MWWs observed.

3 Empirical Model

We identify the effect of National Minimum Wage (NMW) hikes on hours worked and likelihood of job loss for minimum wage workers (MWWs) in concentrated local labour markets (LLMs) using two alternative difference-in-difference designs. First, we compare MWWs in high-concentration LLMs to MWWs in low-concentration LLMs – the *across*-market comparison. Second, we compare MWWs in high-concentration LLMs to non-MWWs in those same markets – the *within*-market comparison. We then combine both dimensions of difference into our preferred triple-difference estimator.

3.1 Across-Market Difference-in-Difference

First consider the differential effect of NMW hikes on MWWs in high-concentration markets compared those in low-concentration markets. We estimate the following equation for a sample of MWWs across all LLMs.[12]

$$\begin{aligned} E_{imt} = {} & \alpha_0' + \alpha_1' HHI_m \\ & + \alpha_2'(\mathbb{1}[y \geq 2017]) + \gamma_1'(HHI_m \times \mathbb{1}[y \geq 2017]) \\ & + \alpha_3'(\mathbb{1}[y \geq 2018]) + \gamma_2'(HHI_m \times \mathbb{1}[y \geq 2018]) \\ & + \alpha_4'(\mathbb{1}[y \geq 2019]) + \gamma_3'(HHI_m \times \mathbb{1}[y \geq 2019]) \\ & + X_{imt}\delta' + \mu_m' + \kappa_t' + \varepsilon_{imt}' \end{aligned} \tag{1}$$

The outcome E_{imt} gives hours worked (or an indicator of job loss) for worker i in market m at time t. We include fixed effects for market and calendar quarter given by μ_m and κ_t respectively, with one category omitted for each (and one additional category dropped for μ_m to avoid colinearity with HHI_m, which we

[12] When considering job loss, we use lagged minimum wage status rather than contemporary, as currently non-employed respondents do not report minimum wage status.

include for exposition). Worker-level demographic controls X_{imt} include a set of dummy variables for age, and indicators for sex, educational attainment, and the presence of children.

We consider several specifications of the market concentration variable HHI_m: HHI in levels – ranging continuously from zero to one – and indicators for HHI above the thresholds of 0.25, 0.10, and the median.[13] The binary variables $\mathbb{1}[year \geq y]$ indicate that the present year is greater than or equal to y. As NMW hikes occur on January 1 each year during the sample period, these capture the effects of each hike compared to the previous year.[14] They may also pick up contemporaneous shocks unrelated to the NMW hikes.

The coefficients on the interaction term γ_n', $n = \{1, 2, 3\}$ give the differential effect on employment of MWWs in concentrated markets compared to MWWs in non-concentrated markets. These are textbook two-group, two-period difference-in-difference estimates.[15] The identifying assumption is that trends in hours worked or job loss likelihood between these two groups would parallel over the sample period in the absence of NMW hikes. Any common shocks coinciding with the timing of the NMW hikes are not a threat to identification, so long as they affect MWWs in concentrated and non-concentrated markets equally.

NMW hikes apply the same nominal minimum wage to all LLMs. We hypothesize the following.

1. If high-concentration LLMs are monopsonistic, NMW hikes should produce positive employment effects in these markets.
2. If low-concentration LLMs are competitive, hikes should yield negative effects.

[13] A HHI of 0.25 corresponds to a market with four equally-sized competitors, and was the US FTC's threshold for a highly concentrated market during our sample period.

[14] Redmond and McGuinness (2022) estimate cumulative effects, using a similar dataset, for an overlapping time period.

[15] We do not combine the difference-in-difference estimates into a average treatment effect – a subject of much recent literature (see de Chaisemartin and d'Haultfoeuille 2022 for a review). As monopsony theory predicts a nonmonotonic relationship between the minimum wage level and employment, taking a weighted average of the three treatment effects may combine effects of opposite sign, concealing the true effects.

Although contemporaneous shocks unrelated to NMW hikes may confound these effects, positive relative effects in high-concentration market groups should hold so long as these are relatively more monopsonistic than low-concentration groups (Azar et al. 2023, Bhaskar et al. 2002).

As a placebo test, we also estimate equation (1) on a sample of high-wage workers, who are not directly affected by the NMW hikes. Differential effects in concentrated markets for this sample would mean non-parallel employment trends across market concentration group for non-MWWs, which could suggest that differentials for MWWs may be due to contemporaneous shocks other than NMW hikes that are correlated with market type.

3.2 Within-Market Difference-in-Difference

Now consider the effect of NMW hikes on MWWs compared to non-MWWs. We estimate the following equation separately for samples of high- and low-concentration LLMs.

$$\begin{aligned} E_{imt} = \alpha_0 &+ \alpha_1 MWW_{imt} \\ &+ \alpha_2(\mathbb{1}[y \geq 2017]) + \beta_1(MWW_{imt} \times \mathbb{1}[y \geq 2017]) \\ &+ \alpha_3(\mathbb{1}[y \geq 2018]) + \beta_2(MWW_{imt} \times \mathbb{1}[y \geq 2018]) \\ &+ \alpha_4(\mathbb{1}[y \geq 2019]) + \beta_3(MWW_{imt} \times \mathbb{1}[y \geq 2019]) \\ &+ X_{imt}\delta + \mu_m + \kappa_t + \varepsilon_{imt} \end{aligned} \tag{2}$$

The binary variable MWW_{imt} indicates whether the worker earns the minimum wage.[16] Other variables are as described in the previous subsection.

The parameters of interest are β_n, $n = \{1, 2, 3\}$, that fall on the interactions between minimum wage status and year indicators. These give textbook two-by-two difference-in-difference estimates. The identifying assumption is that trends in hours worked (or likelihood of job loss) would be parallel for MWWs and

[16]When estimating hours effects we use contemporaneous minimum wage status. We use lagged minimum wage status when estimating job loss effects, as workers who have lost their job no longer report minimum wage status.

non-MWWs (within market concentration group) over the sample period in the absence of NMW hikes.

In addition to estimating equation (2) within market concentration group, we also estimate with for a sample of all LLMs. Although we expect heterogeneous (and perhaps non-monotonic) treatment effects by market concentration, this provides a point of comparison with part literature that does not consider market concentration. We consider HHI thresholds of 0.25, 0.10, and the median. If the high-concentration samples correspond to monopsonistic labour markets, estimates on this sample should yield positive employment effects (positive hours effects and negative job loss effects) on MWWs. If the low-concentration samples correspond to competitive markets, they should yield negative employment effects on MWWs. The all-markets sample provides estimates comparable to McGuinness et al. (2019), who estimate hours and job loss effects over the 2017-2018 period using the LFS (but without making use of the BR to divide markets into high- and low-conentration groups).

3.3 Triple-Difference

Our preferred estimates come from a triple-difference specification that compares the differential effect on MWWs in concentrated markets over MWWs in non-concentrated markets to non-MWWs in concentrated markets over non-MWWs in non-concentrated markets. The identifying assumption is that whatever the difference in trends between MWWs in concentrated and non-concentrated markets, this difference is parallel to the difference in trends between non-MWWs in concentrated and non-concentrated markets. Equivalently, the difference in trends between MWWs and non-MWWs in concentrated markets is parallel to that same difference in non-concentrated markets.

$$\begin{aligned}E_{imt} = \alpha_0^* &+ \alpha_1^* MWW_{imt} \\ &+ \alpha_2^* \mathbb{1}[y \geq 2017] + \beta_1^*(MWW_{imt} \times \mathbb{1}[y \geq 2017]) \\ &+ \alpha_3^* \mathbb{1}[y \geq 2018] + \beta_2^*(MWW_{imt} \times \mathbb{1}[y \geq 2018]) \\ &+ \alpha_4^* \mathbb{1}[y \geq 2019] + \beta_3^*(MWW_{imt} \times \mathbb{1}[y \geq 2019]) \\ &+ \alpha_5^* HHI_m + \alpha_6^*(MWW_{imt} \times HHI_m) \\ &+ \alpha_7^*(HHI_m \times \mathbb{1}[y \geq 2017]) + \gamma_1^*(MWW_{imt} \times HHI_m \times \mathbb{1}[y \geq 2017]) \\ &+ \alpha_8^*(HHI_m \times \mathbb{1}[y \geq 2018]) + \gamma_2^*(MWW_{imt} \times HHI_m \times \mathbb{1}[y \geq 2018]) \\ &+ \alpha_9^*(HHI_m \times \mathbb{1}[y \geq 2019]) + \gamma_3^*(MWW_{imt} \times HHI_m \times \mathbb{1}[y \geq 2019]) \\ &+ X_{imt}\delta^* + \mu_m^* + \tau_t^* + \varepsilon_{imt}^*\end{aligned} \tag{3}$$

The coefficients β_n^*, γ_n^*, $n = \{1, 2, 3\}$ estimate the effect of NMW hikes on MWWs, and the effect of NMW hikes on MWWs in concentrated markets, respectively. These correspond to the treatment effects estimated using the same notation in equations (2) and (1). Control variables are as before, with the exception that we now include a richer set of year-by-quarter fixed effects τ_t, with one time period in each year from 2017 to 2019 omitted so as to avoid colinearity with the respective year indicators, which we include for the sake of exposition.

4 Hours Results

This section presents estimates of the effects on hours worked of the 2017, 2018, and 2019 National Minimum Wage (NMW) hikes for minimum wage workers (MWWs) in concentrated local labour markets (LLMs). We measure employer concentration using the Herfindahl-Hirschman Index (HHI), defined in section 3. We identify the effects of the hikes using two alternative difference-in-difference designs, which we combine into a triple difference to produce our preferred estimates.

First, we compare hours changes for MWWs in concentrated LLMs to those for MWWs in non-concentrated LLMs: the *across-market* difference-in-difference. We find that MWWs in concentrated LLMs increase hours relative to MWWs in non-concentrated LLMs for the first two hikes, and may decrease hours following the third hike (the effect being statistically significant in only one specification). We also perform a placebo test, comparing non-MWWs across market concentration groups, and find null results.

Second, we compare hours for MWWs to those for non-MWWs *within-market* before and after each hike, for samples of concentrated and non-concentrated LLMs. MWWs in concentrated LLMs show hours gains following the first two hikes relative to non-MWWs in the same markets, while non-concentrated LLMs produce no such result. We find negative point estimates of the third hike, but this effect is statistically insignificant in all specifications.

We calculate triple-difference estimates equivalently by taking the across-MWW status *within-market* difference of the *across-market* estimates, or equivalently by taking the *across-market* difference of the *within-market* estimates. This weakens the parallel trends assumption, and produces similar estimates to both difference-in-difference designs, with hours gains for MWWs in concentrated LLMs statistically significant across all specifications for the larger 2018 hike, one for the smaller 2017 hike, and hours losses statistically insignificant in all specifications for the 2019 hike.

Subgroup analysis shows that hours results are driven by prime age workers (age 25-54). Under-25s show no response, and while the 55+ age group responds similarly to those of prime age, the estimates are statistically insignificant. There is also some evidence of labour-labour substitution by education level; hours gains in 2018 were driven by MWWs with a tertiary degree, while subsequent losses were strongest for those with only a primary education.

We also study the external margin of employment using the same alternative within- and across-market difference-in-difference designs, as well as the triple-difference. MWWs in concentrated LLMs saw statistically significantly lower rates of job loss than MWWs in non-concentrated LLMs in 2017 and 2018, and higher (albeit statistically insignificantly so) rates in 2019. This is due to increased likelihood of job loss for the MWWs in low-concentration markets, who also see higher rates of job loss in 2017 and 2018 than non-MWWs in the same markets.

4.1 Hours Across-Market Difference-in-Difference

Table 3 shows the effect of NMW hikes on MWWs in concentrated and non-concentrated markets (equation 1). The sample consists only of MWWs. The first three rows give estimates of single-difference effects on MWWs in non-concentrated markets, implicitly using MWWs in those same markets in the year before each NMW hike as the control. As NMW hikes are implemented annually during this period, these effects may be confounded by other factors, such as the business cycle. Nonetheless, estimates are economically small, with mostly positive effects of magnitudes smaller than one hour for the 2017 hike, and around zero for 2018 and 2019. Hours worked did not change much for MWWs in non-concentrated markets during the sample period.

The next three rows give estimates of the difference-in-difference effect of the NMW hikes on MWWs in concentrated markets, using MWWs in non-concentrated markets as a control group. The differential effect of hikes should be positive if employers had been suppressing wages further below the marginal product of labour in more concentrated markets, as predicted by the monopsony model. The difference-in-difference estimates show a statistically significant increase in hours for MWWs in concentrated markets following the 2017 hike for two of four specifications, following the 2018 hike for three of four specifications, and a decrease in hours following the 2019 hike that is smaller in magnitude and statistically significant for only one of four specifications. Signs are consistent for each year across all specifications.

We interpret magnitudes as follows. The coefficients from the specification interacting level HHI with hike years (column 1) give the joint effect of each hike interacted with the model's prediction of how hours would increase with increasing the HHI by one unit – the equivalent of comparing perfect competition to monopsony. Since most markets fall well below the monopsony HHI level of

Table 3: Hours Effects Across-Market Difference-in-Difference

	(1)	(2)	(3)	(4)
	HHI Measurement			
	Level	≥ 0.25	≥ 0.10	$\geq$ Med.
≥ 2017	0.59*	0.77**	0.73**	0.31
	(0.29)	(0.28)	(0.29)	(0.25)
≥ 2018	-0.15	0.1	0.026	0.019
	(0.43)	(0.43)	(0.42)	(0.48)
≥ 2019	-0.1	-0.16	-0.062	-0.0099
	(0.56)	(0.56)	(0.58)	(0.65)
HHI $\times \geq 2017$	6.57**	2.09	1.14	2.06**
	(2.16)	(1.45)	(0.87)	(0.75)
HHI $\times \geq 2018$	10.0***	3.93**	2.38***	0.80
	(2.42)	(1.34)	(0.62)	(0.53)
HHI $\times \geq 2019$	-2.14	-1.22	-1.71*	-0.75
	(3.78)	(1.82)	(0.74)	(0.71)
Constant	25.1***	19.8***	19.9***	20.1***
	(1.03)	(0.95)	(0.95)	(0.91)
Quarter FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
HHI Measure	Yes	Yes	Yes	Yes
N	13781	13781	13781	13781
R^2	0.32	0.32	0.32	0.32

Table gives the difference-in-difference effects of National Minimum Wage hikes on hours worked for MWWs in concentrated markets compared to MWWs in non-concentrated markets (equation 1). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 4: Hours Effects Across-Market Difference-in-Difference (Placebo)

	(1)	(2)	(3)	(4)
	HHI Measurement			
	Level	≥ 0.25	≥ 0.10	$\geq$ Med.
≥ 2017	0.23**	0.22**	0.25***	0.19
	(0.070)	(0.079)	(0.062)	(0.10)
≥ 2018	0.20**	0.22***	0.13*	0.19*
	(0.055)	(0.047)	(0.060)	(0.083)
≥ 2019	-0.00033	0.023	0.049	-0.079
	(0.081)	(0.074)	(0.071)	(0.13)
HHI $\times \geq 2017$	-0.12	0.05	-0.12	0.051
	(0.34)	(0.24)	(0.12)	(0.10)
HHI $\times \geq 2018$	0.12	-0.17	0.38***	0.023
	(0.28)	(0.17)	(0.067)	(0.11)
HHI $\times \geq 2019$	0.58	0.27	0.0056	0.23
	(0.44)	(0.23)	(0.15)	(0.18)
Constant	17.7***	12.1***	12.2***	12.2***
	(0.71)	(0.79)	(0.80)	(0.89)
Quarter FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
HHI Measure	Yes	Yes	Yes	Yes
N	161792	161792	161792	161792
R^2	0.25	0.25	0.25	0.25

Table gives the difference-in-difference effects of National Minimum Wage hikes on hours worked for *non-MWWs* in concentrated markets compared to *non-MWWs* in non-concentrated markets (equation 1). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 5: Hours Effects Within-Market Difference-in-Difference

	(1)	(2)	(3)	(4)	(5)	(6)
	Sample HHI Threshold					
	$\geq$ 0.25	$<$ 0.25	$\geq$ 0.10	$<$ 0.10	$\geq$ Med.	$<$ Med.
$\geq$ 2017	0.39	0.22**	0.17	0.26***	0.26**	0.19
	(0.28)	(0.080)	(0.20)	(0.060)	(0.11)	(0.100)
$\geq$ 2018	0.093	0.22***	0.51***	0.13*	0.21**	0.19*
	(0.13)	(0.048)	(0.084)	(0.061)	(0.059)	(0.088)
$\geq$ 2019	0.31	0.016	0.06	0.041	0.15*	-0.086
	(0.20)	(0.079)	(0.12)	(0.075)	(0.079)	(0.13)
Min. Wage $\times \geq$ 2017	1.09	0.45	1.4	0.35	1.82**	0.051
	(1.57)	(0.28)	(1.15)	(0.28)	(0.63)	(0.21)
Min. Wage $\times \geq$ 2018	3.83*	-0.14	1.86**	-0.11	0.84*	-0.27
	(1.73)	(0.42)	(0.73)	(0.44)	(0.36)	(0.52)
Min. Wage $\times \geq$ 2019	-1.23	-0.025	-1.11	0.02	-0.56	0.28
	(1.78)	(0.51)	(0.86)	(0.52)	(0.66)	(0.61)
Constant	10.7	20.2	18.6***	20.2***	15.5***	19.7***
	(6.11)	(0.76)	(3.17)	(0.77)	(2.24)	(1.06)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes	Yes
N	17579	157994	35657	139916	97349	78224
R^2	0.22	0.3	0.29	0.3	0.26	0.33

Table gives the difference-in-difference effects of National Minimum Wage hikes on hours worked for MWWs in concentrated (and non-concentrated) markets compared to non-MWWs *in the same markets* (equation 2). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 6: Hours Effects Triple-Difference

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	$\geq$ 0.25	$\geq$ 0.10	$\geq$ Med.
Min. Wage $\times \geq$ 2017	0.45	0.28	0.43	0.32	0.0051
	(0.24)	(0.29)	(0.28)	(0.28)	(0.21)
Min. Wage $\times \geq$ 2018	0.038	-0.35	-0.13	-0.11	-0.27
	(0.42)	(0.47)	(0.42)	(0.44)	(0.52)
Min. Wage $\times \geq$ 2019	-0.13	0.037	-0.037	0.00042	0.26
	(0.54)	(0.53)	(0.51)	(0.53)	(0.61)
HHI $\times$ Min. Wage $\times \geq$ 2017		4.41	0.72	1.30	1.84**
		(3.16)	(2.06)	(1.32)	(0.66)
HHI $\times$ Min. Wage $\times \geq$ 2018		8.94**	4.36**	1.93**	1.15**
		(2.83)	(1.75)	(0.68)	(0.37)
HHI $\times$ Min. Wage $\times \geq$ 2019		-2.01	-1.53	-1.20	-0.85
		(3.38)	(1.72)	(0.85)	(0.72)
Constant	20.4***	27.3***	20.5***	20.5***	20.8***
	(0.77)	(0.67)	(0.78)	(0.82)	(0.89)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	175573	175573	175573	175573	175573
R^2	0.30	0.30	0.30	0.30	0.30

Table gives the triple-difference effect on hours worked for MWWs in concentrated and non-concentrated markets, compared to non-MWWs in the respective markets (equation 3). First column shows the average response for MWWs across all markets (equation 2) as a point of comparison. Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

unity this effectively extrapolates a linear estimate beyond the range over which most of its statistical power is estimated, resulting in large (and statistically significant) estimates of hours increases of nearly seven and ten hours in 2017 and 2018 respectively, and a (statistically insignificant) decrease of 2.1 hours in 2019.

Columns 2-4 bin markets by HHI thresholds of 0.25, 0.10, and the median (of around 0.03), which respectively place the highest 10%, 20%, and 50% of markets into the high-concentration bin. Treatment effects then give the change in hours of MWWs in the high-concentration bin relative to those in the low-concentration bin, which are straightforward to interpret, but exclude any within-bin variation. MWWs in the 10% most concentrated markets experience relative gains of over two hours and nearly four hours in 2017 and 2018 respectively, with only the latter being statistically significant. Using the top 20% bin (HHI $\geq$ 0.10) the corresponding estimates are over one hour and nearly two-and-a-half, again with only the 2018 hike showing a statistically significant response. Using the median cutoff, the 2017 hike yields a statistically significant gain of two hours, and the 2018 and 2019 a statistically insignificant gain and loss respectively, each of less than an hour.

Table 4 repeats the above analysis for the sample of non-MWWs. Although minimum wage hikes can spill over onto workers earning more than the minimum wage (see for example Cengiz et al. 2019), since most non-MWWs earn much more than the minimum wage, this constitutes a placebo test of the NMW's effect on hours.[17] Rather, any estimated effects should be due to secular trends in hours. Single-difference effect show economically small hours gains (of less than a quarter of an hour) for non-MWWs in 2017 and 2018. Only one of the difference-in-difference estimates is statistically significant at usual levels, and small in magnitude at less than half an hour.

[17] In appendix A we conduct a robustness check excluding from the control group non-MWWs in the bottom two income deciles, so as to rule out spillovers, and find nearly identical results. We also conduct a robustness check excluding from the control group non-MWWs from the top two income deciles, and confirm the main results. Cengiz et al. (2019) find that upper-tail earners are unaffected by minimum wage hikes.

4.2 Hours Within-Market Difference-in-Difference

We now consider an alternative identification approach, using a within- rather than across-market control group. Table 5 shows effect of NMW hikes on MWWs, using non-MWWs *in the same markets* as the control group. We split the sample into high- and low-concentration bins according to the 0.25, 0.10, and median thresholds, and estimate the differential effect of hikes on MWWs compared to non-MWWs for each of the six samples. For each of the three high-concentration samples (columns 1, 3, and 5), the 2017 and 2018 hikes increase hours for MWWs, with similar magnitudes to the across-market estimates using the corresponding thresholds. All effects are statistically significant for the larger 2018 hike, with only the estimate from the above-median sample showing statistical significance for the 2017 hike. All responses to the 2019 hike are negative and statistically insignificant for the high-concentration samples, again with comparable magnitudes to the corresponding across-market estimates.

Differential effects on MWWs in low-concentration market samples (columns 2, 4, and 6) constitute a placebo test of the within-market specification. All estimates are statistically insignificant, and less than half an hour in magnitude. Effects on non-MWWs in any sample, likely picking up secular trends, are all positive, mostly statistically significant, and economically small, topping out at around half an hour.

McGuinness et al. (2019) use the same difference-in-difference design, comparing hours changes for MWWs to those for non-MWWs after the 2018 NMW hike, also using the Irish LFS. They do not match the LFS to the BR, and therefore do not compute market concentration; however, in addition to their aggregate estimates, they estimate hours effects on geographic and industrial subsamples. Some of these are positive and statistically significant, ranging from around one to one-and-a-half hours. This range overlaps with our estimates of 0.84 to 3.83 hours for high-concentration markets.[18]

[18] We present the equivalent of their aggregate estimate in table 6 and confirm that it is small, positive, and statistically insignificant.

4.3 Hours Triple-Difference

Table 6 presents the triple-difference estimates of the effect of the NMW hikes on MWWs in concentrated markets versus non-concentrated over that of non-MWWs in concentrated versus non-concentrated markets (equation 3). This is equivalent to the difference of the treatment effects in table 3 and the placebo effects of table 4, or equivalently to the difference between the treatment effects in the high-concentration columns of table 5 to the placebo effects shown in low-concentration columns of the same table.

Most studies of minimum wage employment effects do not consider market concentration, estimating the effect on the treatment group averaged over all markets. This corresponds to equation 2 whose results we present in column 1, estimated on the entire sample rather than a subsample of high- or low-concentration markets (as in table 5). We show the results of this difference-in-difference specification in the first column of table 6 as a point of comparison. Averaged over all markets, MWWs show small, statistically insignificant hours increases following the 2017 and 2018 hikes, and a small and statistically insignificant hours decrease following the 2019 hike. This is consistent with previous literature finding small effects across all markets. In particular, McGuinness et al. (2019) produce a similar estimate using the same identification approach and dataset.

Columns 2-5 of table 6 give the triple-difference estimates from equation 3 for the four definitions of market concentration. The triple-difference estimates show similar patterns to the difference-in-difference estimates of table 3, with hours increases for MWWs in concentrated markets following the 2017 and 2018 hikes, and a decrease following that of 2019. The effect of the smaller 2017 hike is statistically significant only for the median bin specification, while the effect of the larger 2018 hike is statistically significant for all specifications. Though uniformly negative, the effects of the 2019 hike are statistically insignificant for all specifications. Magnitudes are similar across the board. These results validate the difference-in-difference estimates, providing similar results under weaker assumptions.

4.4 Hours Effects on MWWs in Competitive LLMs

By netting out secular changes in hours that are common to all workers, the triple-difference specification also provides more credible estimates of hours effects for MWW in non-concentrated markets as well. Although monopsony theory predicts a negative causal effect of minimum wage hikes for these workers, we find economically small and uniformly statistically insignificant hours effects for MWWs in non-concentrated LLMs for all years (seen in the first three rows of columns 2-5 of table 6). This may be because low-concentration bins do not correspond closely enough to perfectly competitive markets; indeed, no market has a HHI of zero, as this would imply infinite employers. Weighted by worker, mean HHI in the lower bins for the 0.25, 0.10, and median stands at 0.042, 0.031, and 0.012 respectively.[19] These figures correspond to the equivalent of around 24, 32, and 83 equally-sized employers respectively. Alternatively, employers may exploit forms of market power not measured by the HHI, such as search frictions.

Another possibility is that positive hours effects for MWWs in non-concentrated LLMs are second-order effects of job loss among this group in response to the hikes. In section 5 we find increased likelihood of job loss for MWWs in non-concentrated markets, for difference-in-difference designs as well as for the triple-difference design using non-MWWs in these markets as the control group.

Finally, we note that the Low Pay Commission of Ireland recommends hikes endogenously to the state of the economy; strong economic performance could be offsetting negative causal effects in non-concentrated markets over our sample period. However, as the triple-difference specification uses non-MWWs in non-concentrated markets as a control group for MWWs in non-concentrated markets, this would mean that underlying economic performance disproportionately benefits MWWs over the course of our sample. Regardless, the null effects among MWWs in non-concentrated markets does not call into question our triple-difference estimates on MWWs in concentrated markets.

[19]The corresponding figures for the high concentration bins as 0.45, 0.30, and 0.14 respectively.

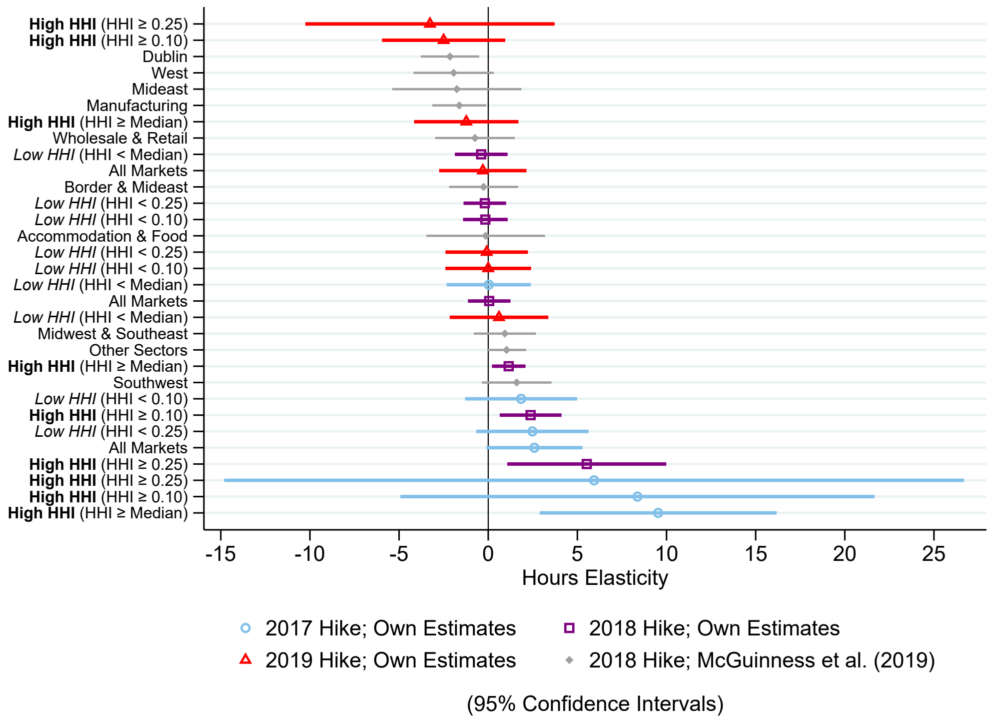
4.5 Implied Hours Elasticity

We use the hours estimates from table 6 to calculate elasticities with respect to minimum wage changes for each year and market concentration bin, as well as for a sample of all LLMs. We compare our results to the sectoral and regional estimates of McGuinness et al. (2019), who estimate the effect of the 2018 NMW hike using the same LFS data (but without market concentration information from the BR). While they do not isolate high- or low-concentration LLMs, as we define a LLM as the intersection of a sector and region of work, these estimates illustrate some of the same underlying variation.

Figure 3 plots elasticity estimates with 95% confidence intervals. The 2017 hike shows positive and implausibly large point estimates in concentrated markets, with wide confidence intervals mostly including zero. While hours gains in 2017 were modest, the hike was small in real terms, as less than 1%. Unlike other years, the hours elasticity for low-concentration LLMs in 2017 is also positive. We calculate this elasticity from the difference-in-difference estimate of MWWs in low-concentration LLMs to non-MWWs in those same LLMs, which requires stronger assumptions than the triple-difference estimates we use to estimate the differential effect on MWWs in high-concentration LLMs (over MWWs in low-). As we note above, this estimate is more likely to reflect secular trends. Correspondingly, the triple-difference estimates would be inflated by the same trends; deflating them by subtracting the double-difference estimates would yield elasticity estimates in line with 2018.

All high-concentration market groups show positive elasticity estimates in response to the 2018 hike, all statistically distinguishable from zero. The higher the HHI threshold, the higher the point estimate, as expected; the most concentrated 10% of markets, with HHI in excess of 0.25, shows a point estimate above 5. While larger than estimates from previous literature that do not condition on market concentration, this is similar in magnitude to Azar et al. (2023)'s estimates of the own-wage employment elasticity of minimum wage hikes for firms in the

Figure 3: Elasticity Comparison



top tercile of market concentration (which is just above 4).[20] Low-concentration markets show hours elasticities around zero. These markets showed employment losses along the external margin rather than the internal (see section 5).

The elasticity estimates in response to the 2019 hike are negative for high concentration markets, though statistically insignificant, and slightly larger in magnitude than the most negative of regional elasiticities from McGuinness et al. (2019). At around -3, they are comparable to the negative own-employment elasticities of Azar et al. (2023) for the least concentrated LLMs, of around -2. Large negative elasticities are consistent with Berger et al. (2022)'s finding if a steep marginal product of labour curve in a model calibrated to US data. Again, low-concentration markets show elasticities of around zero.

[20]The own-wage employment elasticity of wages is the ratio of the market-level employment elasticity of minimum wages to the market-level wage elasticity of minimum wages. Most studies of the employment elasticity of minimum wages estimate the former, using the latter as a 'first stage' to demonstrate the efficacy of the policy (for a discussion of the own-wage employment elasticity see Harasztosi and Lindner 2019 and Azar et al. 2023). As our treatment group reports being bound by the minimum wage, testing the 'first stage' is moot in our setting.

While Azar et al. (2023) find large positive elasticities of total employment in high concentration markets and negative elasticities in low-concentration markets, we find the most extreme positive *and* negative elasticities in high-concentration markets. This is consistent with the NMW crossing the full-employment threshold. Hours elasticities are close to zero for low-concentration markets in all years, as negative employment effects in low-concentration markets happen at the external margin of employment, as we show in section 5.

4.6 Hours Effects Summary and Discussion

Altogether we find consistent evidence of hours increases for MWWs in concentrated markets following the 2017 and 2018 NMW hikes, and suggestive evidence of a decrease in hours following the 2019 hike. We make use of both across- and within-market variation. Taken independently, both sources of variation yield similar results in their respective difference-in-difference specifications, showing hours increases for MWWs in concentrated markets using either MWWs in non-concentrated markets, or non-MWWs in concentrated markets as control groups. These alternative difference-in-difference specifications make use of different parallel trends assumptions, lending credence to each other. The triple-difference specification incorporates both across- and within-market variation, relying on a weaker differences-in-trends assumption, again producing similar results.

Differential hours gains for MWWs in high-concentration LLMs are consistent with monopsonistic competition in those markets. The subsequent hours losses in 2019 are consistent with monopsonistic labour markets in which the minimum wage has reached the marginal product of low wage labour. Importantly, both the gains through 2018 and subsequent losses in 2019 result from comparison to either MWWs in low-concentration labour markets or non-MWWs in high concentration markets as the control group. As these entail different identification assumptions, finding the same result using either approach reinforces the causal interpretation of the treatment effects.

In appendix A we show that all main results are robust to measuring outcomes by actual hours worked during the reference week rather than usual hours worked, and to a variety of sampling variations. As actual hours tend to be more elastic than usual hours, this alternative measure strengthens the statistical significance of our findings, particularly for the 2017 and 2019 hikes. In the following subsection we show that the main results are driven by prime-age workers.

4.7 Subgroup Analysis

In this subsection we show that both hours gains and subsequent losses were driven by prime-age workers. We also find suggestive evidence that hours gains favoured high-education MWWs, while low-education MWWs bore hours losses disproportionately.

Table 7 splits the sample into young (younger than 25), prime age, and old (55 and older) workers. We find that hours increases in concentrated markets (HHI$\geq$0.25) are larger and statistically significant for prime age workers (column 2), with the cumulative NMW hikes by 2018 and 2019 increasing hours by 6.5 and 4.6 hours per week respectively.

Because of the lack of availability of MWW status in typical data sources, many studies use teen employment as a proxy for minimum wage employment. However, teen workers are not typically primary household earners, and disemployment among teens is seen as less of a policy concern. We show that teens do not drive the hours gains and subsequent losses in our main results – prime wage workers do. Older workers also show patterns consistent with the main results, though these are statistically insignificant due to small sample size. Teens show economically small and statistically insignificant responses.

Our results stand in contrast to those of Cengiz et al. (2022), who find that teens show a larger employment gain in response to minimum wage hikes than other workers. However, they study the external margin of employment whereas our result is for the internal. Munguía Corella (2020) also finds differential employment gains for teen workers in concentrated LLMs following minimum wage hikes.

Table 7: Hours Effects Triple-Difference by Age Group

	(1)	(2)	(3)
	Age Group		
	Under 25	25 - 54	55+
Min. Wage $\times \geq 2017$	-0.27	0.95	2.50**
	(0.46)	(0.78)	(0.83)
Min. Wage $\times \geq 2018$	0.69	-1.03**	-0.81
	(0.58)	(0.36)	(1.41)
Min. Wage $\times \geq 2019$	-0.084	0.26	0.51
	(0.69)	(0.61)	(1.22)
HHI $\geq 0.25 \times$ Min. Wage $\times \geq 2017$	0.95	0.21	-1.74
	(3.04)	(2.18)	(4.23)
HHI $\geq 0.25 \times$ Min. Wage $\times \geq 2018$	0.51	6.11**	5.00
	(2.21)	(2.00)	(4.38)
HHI $\geq 0.25 \times$ Min. Wage $\times \geq 2019$	-0.17	-2.73**	-3.21
	(3.55)	(0.89)	(4.68)
Constant	22.7***	38.6***	31.7***
	(0.85)	(0.052)	(0.34)
Year $\times$ Quarter FE	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes
HHI Measure	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	Yes	Yes	Yes
HHI Measure $\times$ Year FE	Yes	Yes	Yes
N	18910	127423	29240
R^2	0.42	0.25	0.30

We calculate the median threshold separately for each group. The unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 8: Hours Effects Triple-Difference by Education Group

	(1)	(2)	(3)
	Education Group		
	Primary	Secondary	Tertiary
Min. Wage $\times \geq 2017$	0.50	0.18	0.99
	(0.55)	(0.18)	(0.77)
Min. Wage $\times \geq 2018$	0.24	0.043	-0.76
	(0.42)	(0.39)	(1.05)
Min. Wage $\times \geq 2019$	-0.67	0.15	0.24
	(0.56)	(0.52)	(0.57)
HHI $\geq 0.25 \times$ Min. Wage $\times \geq 2017$	1.78	2.91	-8.32
	(1.91)	(2.46)	(4.83)
HHI $\geq 0.25 \times$ Min. Wage $\times \geq 2018$	1.12	3.50	12.2**
	(3.29)	(1.96)	(4.58)
HHI $\geq 0.25 \times$ Min. Wage $\times \geq 2019$	-5.48*	-2.26	0.73
	(2.67)	(1.98)	(5.96)
Constant	16.8***	47.6***	37.4***
	(1.37)	(0.29)	(2.66)
Year $\times$ Quarter FE	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes
HHI Measure	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	Yes	Yes	Yes
HHI Measure $\times$ Year FE	Yes	Yes	Yes
N	20093	82394	73086
R^2	0.46	0.31	0.21

We calculate the median threshold separately for each group. The unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 8 shows triple-difference results with the sample split by maximum educational attainment. The 2017 NMW hike shows no statistically significant effect on hours for MWWs in any subsample. The 2018 hike shows large and statistically significant hours gains for MWWs who have achieved a tertiary education (over non-MWWs with the same education level). Point estimates are positive for all education groups, though not statistically significant for the others. MWWs with a primary education show large and statistically significant hours losses compared to their non-MMW within-education group counterparts following the 2019 hike. MWWs with a secondary education show a qualitatively similar effect, but it is smaller in magnitude and statistically insignificant.

In the following section we study the effect of minimum wage hikes on job loss, showing that the external margin of employment responds similarly to the internal margin studied in this section. This supports the interpretation of the hours increases in 2017 and 2018 as the result of monopsonistic competition in concentrated LLMs.

5 Job Loss Results

In section 4 we showed that the 2017 and 2018 minimum wage hikes increased hours for minimum wage workers (MWWs) in concentrated local labour markets (LLMs) – compared to MWWs in non-concentrated markets, as well as to non-MWWs within market – while the subsequent 2019 hike may have decreased hours. This adjustment at the internal margin of labour supply could mask external margin effects of hikes. Negative external-margin effects could attenuate or even reverse the interpretation of the hours results. For example, if employers lay off some MWWs in response to hikes, the remaining MWWs may work more hours. Additionally, if workers have a target income, higher wages beyond a certain point may induce hours reductions due to backward-bending labour supply, providing an alternative explanation for the negative effect of the 2019 hike; however, the external margin of labour supply should not respond negatively, since non-employed workers do not benefit from higher wages. In this section we show that the external margin effects support our interpretation of hours results as the result of monopsonistic competition in concentrated LLMs. MWWs in concentrated markets showing a lower likelihood of job loss compared to control groups, ruling out hours gains as second-order a response to job loss. We find weakly higher rates of job loss for MWWs in concentrated markets in 2019, but these effects are statistically insignificant; therefore we cannot conclusively rule out backward-bending labour supply as the cause of 2019 hours losses.

As with the hours analysis, we first show alternative across- and within-market difference-in-difference designs to estimate the effect of National Minimum Wage (NMW), and then combine these designs into a triple-difference. We estimate linear probability models of job loss conditional on lagged minimum wage status following each successive hike. Aside from considering a different outcome – job loss rather than hours worked – and conditioning on lagged minimum wage status rather than present status, the analysis is identical to the hours analysis, and the same identification assumptions hold. We define job loss as an employment to non-employment transition. The job loss indicator is equal to one for a worker who was employed in the previous quarter and non-employed (including the unemployed and those not in the labour force) in the current quarter.[21]

Because we infer job loss based on previous employment status, and condition on previous minimum wage status, we must restrict the sample to worker-quarters for which the worker was observed in the previous quarter. As the LFS is a short panel, following workers for up to five quarters, this cuts the sample size considerably. Because the non-employed lack a location and industry of work, we match them to LLMs using the last-reported values. We also classify workers by lagged minimum wage status instead of its current value. See section 2 for details.

We find that MWWs in concentrated LLMs moved from employment to non-

[21] In appendix B we consider two alternative definitions of job loss: employment to unemployment transitions only, and positive to zero actual hours worked (the latter group reporting being employed, but working zero hours in the reference week). The results are qualitatively robust but smaller in magnitude, and only weakly statistically significant. This is because very few workers transition to unemployment during the short sample panel – with most unemployed workers staying unemployed for the duration – and few report zero hours while supposedly working.

employment at lower rates than MWWs in non-concentrated markets following the 2017 and 2018 hikes. Within-market, while MWWs in non-concentrated markets were laid off at higher rates than non-MWWs, MWWs in concentrated markets were not.

5.1 Job Loss Across-Market Difference-in-Difference

Table 9 shows across-market results for a sample of MWWs (using lagged minimum wage status). The first three rows give single-difference treatment effects of the 2017-2019 NMW hikes on MWWs in non-concentrated LLMs. Job loss likelihood increases for this group over the previous year increases by four to five percentage points per year in 2017 and 2018 and is statistically significant in every specification. There is no additional increase in 2019.

The differential effects on MWWs in concentrated LLMs are uniformly negative across all specifications through 2017 and 2018, but only statistically significant for the level HHI specification (in both years) and the median bin specification in 2018. In terms of magnitude they nearly cancel out the increases across all MWWs in the first three rows for the three bin specifications, and more than cancel out those effects according to the level HHI specification – predicting a net decrease in job loss likelihood for MWWs in monopsony LLMs of six to nine percentage points. Further effects in 2019 are positive for three of four specifications, and large for the level specification, at ten percentage points – but none are statistically significant.

Table 10 shows the placebo test on non-MWWs in concentrated and non-concentrated markets. Non-MWWs in low-concentration markets show a statistically significant increase in job loss likelihood of one percentage point in each of 2017 and 2018 across all specifications. There is no further effect in 2019. As non-MWWs are not directly treated, this may represent the effects of unrelated contemporaneous shocks (which the triple-difference specification shown below would remove).[22]

[22] Alternatively, there may be complementarity between minimum wage and above-minimum wage

Table 9: Job Loss Across-Market Difference-in-Difference

	(1)	(2)	(3)	(4)
	HHI Measurement			
	Level	≥ 0.25	≥ 0.10	$\geq$ Med.
≥ 2017	0.04***	0.04***	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
≥ 2018	0.04***	0.04***	0.04***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
≥ 2019	0	0	0	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
HHI $\times \geq 2017$	-0.10**	-0.04	-0.01	-0.02
	(0.04)	(0.02)	(0.01)	(0.01)
HHI $\times \geq 2018$	-0.13**	-0.02	-0.03	-0.03*
	(0.04)	(0.01)	(0.03)	(0.01)
HHI $\times \geq 2019$	0.1	0.03	-0.01	0.02
	(0.07)	(0.02)	(0.03)	(0.01)
Constant	0.91	0.90***	0.90***	0.89***
	(0.01)	(0.02)	(0.02)	(0.02)
Quarter FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
HHI Measure	Yes	Yes	Yes	Yes
N	5732	5732	5732	5732
R^2	0.18	0.18	0.18	0.18

Table gives the difference-in-difference effects of National Minimum Wage hikes on the likelihood of an *employment to non-employment* transition for MWWs in concentrated markets compared to MWWs in non-concentrated markets (equation 1). Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level. *** $p < .01$, ** $p < .05$, * $p < .1$

Table 10: Job Loss Across-Market Difference-in-Difference (Placebo)

	(1)	(2)	(3)	(4)
	HHI Measurement			
	Level	$\geq$ 0.25	$\geq$ 0.10	$\geq$ Med.
$\geq$ 2017	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
$\geq$ 2018	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
$\geq$ 2019	-0.00*	-0.00*	0	0
	(0.00)	(0.00)	(0.00)	(0.00)
HHI $\times \geq$ 2017	-0.01	0	0	0
	(0.00)	(0.00)	(0.00)	(0.00)
HHI $\times \geq$ 2018	-0.02***	-0.01***	-0.01**	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
HHI $\times \geq$ 2019	0.01	0	0	0
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.97***	0.97***	0.97***	0.97***
	(0.01)	(0.00)	(0.00)	(0.00)
Quarter FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
HHI Measure	Yes	Yes	Yes	Yes
N	89537	89537	89537	89537
R^2	0.037	0.037	0.037	0.037

Table gives the difference-in-difference effects of National Minimum Wage hikes on the likelihood of an *employment to non-employment* transition for *non-MWWs* in concentrated markets compared to *non-MWWs* in non-concentrated markets (equation 1). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 11: Job Loss Within-Market Difference-in-Difference

	(1)	(2)	(3)	(4)	(5)	(6)
	HHI Sample					
	$\geq$ 0.25	$<$ 0.25	$\geq$ 0.10	$<$ 0.10	$\geq$ Med.	$<$ Med.
$\geq$ 2017	0	0.01***	0.01**	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\geq$ 2018	0	0.01***	0.00**	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\geq$ 2019	0	-0.00*	0	0	0	0
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Lag Min. Wage $\times \geq$ 2017	0	0.01*	0.01	0.01*	0	0.01*
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Min. Wage $\times \geq$ 2018	0	0.03***	0	0.03**	0.01	0.04**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Min. Wage $\times \geq$ 2019	0.02	0	0	0	0.01	0
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.01	0.94***	0	0.94***	0.6	0.94***
	(0.01)	(0.01)	(0.00)	(0.01)	(0.35)	(0.01)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes	Yes
N	10035	85234	20202	75067	55526	39743
R^2	0.025	0.061	0.025	0.066	0.026	0.085

Table gives the difference-in-difference effects of National Minimum Wage hikes on the likelihood of an *employment to non-employment* transition for MWWs in concentrated (and non-concentrated) markets compared to non-MWWs *in the same markets* (equation 2). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 12: Job Loss Triple-Difference

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	≥ 0.25	≥ 0.10	$\geq$ Med.
Lag Min. Wage $\times \geq 2017$	0.01*	0.01*	0.01*	0.01*	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Min. Wage $\times \geq 2018$	0.03**	0.03***	0.03***	0.03**	0.04**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Min. Wage $\times \geq 2019$	0	0	0	0	0
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
HHI $\times$ Lag Min. Wage $\times \geq 2017$		-0.08***	-0.02**	-0.01	-0.02**
		(0.02)	(0.01)	(0.01)	(0.01)
HHI $\times$ Lag Min. Wage $\times \geq 2018$		-0.10***	-0.03***	-0.03	-0.02
		(0.03)	(0.01)	(0.01)	(0.02)
HHI $\times$ Lag Min. Wage $\times \geq 2019$		0.09	0.02	-0.01	0.01
		(0.05)	(0.02)	(0.02)	(0.01)
Constant	0.95***	0.95***	0.95***	0.95***	0.94***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	95469	95269	95269	95269	95269
R^2	0.059	0.059	0.059	0.059	0.059

Table gives the triple-difference effect of National Minimum Wage hikes on the likelihood of an *employment to non-employment* transition for MWWs in concentrated and non-concentrated markets, compared to non-MWWs in the respective markets (equation 3). First column shows the average response for MWWs across all markets (equation 2) as a point of comparison. Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

The differential effects on non-MWWs in high-concentration markets is small and statistically insignificant for 2017 and 2019, but lower by one to two percentage points and highly statistically significant across all specifications in 2018, canceling out the effects across all LLMs. These results are consistent with secular trends among different types of markets, which we remove in the triple-difference specification below.

5.2 Job Loss Within-Market Difference-in-Difference

Table 11 shows the within-market difference-in-difference design comparing likelihood of job loss for MWWs with that of non-MWWs in the same market concentration group. The positive and statistically significant single-difference effects on non-MWWs seen in table 10 carries over into nearly all market concentration groups, with the exception of the highest concentration bin: LLMs with HHI greater than or equal to 0.25. There is no differential effect for MWWs in high-concentration markets (columns 1, 2, and 3) for any of the three thresholds. However, in low-concentration market groups (columns 2, 4, and 6), MWWs were statistically significantly more likely to experience job loss for the 2017 and 2018 NMW hikes according to all thresholds, by one percentage point in 2017 and three to four in 2018. There is no differential effect in 2019.[23]

5.3 Job Loss Triple-Difference

Table 12 shows results for the triple-difference. MWWs in low-concentration LLMs see a statistically significantly higher rate of job loss over non-MWWs of

work in production, with second-order effects of hikes spilling over to the latter group. Under this interpretation, the difference-in-difference specification is preferable to the triple-difference for estimating effects on MWWs. However, previous studies find high-wage labour to be imperfectly substitutable to low-wage labour (Katz and Murphy 1992), and small effects of minimum wage hikes on the upper tale of the wage distribution (Cengiz et al. 2019).

[23] Stewart (2004b) and Stewart (2007) observe that MWWs have higher job loss likelihoods than non-MWWs in general, regardless of policy interventions. Therefore a single-difference estimate during the treatment period would yield a biased effect of a hike. Our difference-in-difference estimates avoid this problem by comparing the increase in job loss likelihood for MWWs following the hike to the same increase for non-MWWs.

one to two percentage points in 2017 and three to four in 2018 (carrying over from the differential effects in low-concentration market groups in the previous table). This differential cancels out in high-concentration LLMs, with negative differential effects that are statistically significant for most specifications. The level HHI specification predicts differentials that more than cancel out in monopsony LLMs, implying MWWs in these markets see net decreases in job loss likelihood compared to non-MWWs in these markets. In 2019 MWWs in high-concentration markets see a relative increase in job loss likelihood over non-MWWs according to three of four specifications, but no effect is statistically significant. Again, the point estimate for the level HHI specification is large, at nine percentage points.

5.4 Job Loss Summary and Discussion

Taken alone, hours gains for MWWs in concentrated LLMs through 2018 could result from either monopsonistic competition or second-order effects of layoffs – with remaining workers covering for their displaced former coworkers. In this section we have shown that these workers saw a relative decrease in the likelihood of job loss during these years, supporting the former interpretation. While MWWs in high-concentration LLMs saw an increased risk of job loss through 2018 – compared to non-MWWs in these same markets – MWWs in low-concentration LLMs saw no such effect.

Subsequent hours losses for MWWs in high-concentration LLMs in 2019 would be consistent with either minimum wage levels having exceeded the marginal product of labour, or backward-bending labour supply (with MWWs voluntarily reducing hours in response to minimum wage hikes as they require fewer hours to reach a target income). However, the external margin of employment should not bend backward, as the non-employed do not receive the higher wage. In this section we find statistically null effects of the 2019 hike on job loss for this group, albeit with positive point estimates for most specifications. While negative effects on job loss likelihood would support the target income interpretation, MWWs enjoying higher wages being inclined to leave their job (but inclined to decrease hours), positive effects would support the interpretation of reduced demand. The results are not definitive, but weakly support the latter interpretation.

6 Conclusion

We find evidence of non-monotonic employment effects of minimum wage hikes in employer-concentrated markets, as predicted by the monopsony model. Initial hours gains for minimum wage workers (MWWs) in these markets reverse after subsequent hikes. MWWs in low-concentration markets show negative employment effects along the external margin of employment.

These results help to resolve the lack of consensus over the sign of the minimum wage's employment effect. As the effect varies non-monotonically both by market and minimum wage level, pooling together events along either dimension combines local treatment effects with potentially opposite signs. While we analyse each hike event separately, future work could cautiously pool together certain events based on local conditions.

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Appendix

A Hours Effects Robustness

In this appendix we show robustness of the hours results. First we replicate all difference-in-difference and triple-difference designs using actual hours worked in the reference week rather than usual hours worked. We then replicate the triple-difference analysis using a variety of alternative sampling decisions, including: restricting the sample to workers observed both before and after a given National Minimum Wage (NMW) hike; restricting the sample to local labour markets (LLMs) with at least 20 minimum wage workers (MWWs) observed in the Labour Force Survey (LFS); restricting the control group to non-MWWs in the top eight income deciles; and restricting the control group to non-MWWs in the bottom eight income deciles. The main result of hours gains for MWWs in concentrated LLMs following the 2017 and 2018 NMW hikes is generally robust, while the weak evidence of hours losses for this group following the 2019 hike is robust to measuring the outcome by actual hours, but not to some of the sample changes.

A.1 Actual Hours Worked

Although missing for some observations, we hypothesize that actual hours may respond more elastically than usual hours. In particular, workers experiencing hours losses shortly after the implementation of NMW hikes may not be able to differentiate between a temporary and permanent reduction in hours. In practice, we find quantitatively similar results across the board, with both across- and within-market variation showing gains for the first two hikes. The across-market difference-in-difference design and the triple-difference design show slightly stronger evidence of a negative hours effect following the 2019 hike, but the uniformity of statistical significance is similar across specifications.

Table 13 shows effects of NMW hikes on MWWs in concentrated LLMs, compared to MWWs in non-concentrated LLMs. The 2017 and hike shows statisti-

Table 13: Actual Hours Effects Across-Market Difference-in-Difference

	(1)	(2)	(3)	(4)
	HHI Measurement			
	Level	$\geq$ 0.25	$\geq$ 0.10	$\geq$ Med.
$\geq$ 2017	0.88**	1.13***	1.06***	0.66**
	(0.25)	(0.24)	(0.25)	(0.25)
$\geq$ 2018	-0.3	-0.082	-0.14	-0.15
	(0.43)	(0.39)	(0.38)	(0.45)
$\geq$ 2019	-0.29	-0.37	-0.28	-0.14
	(0.56)	(0.57)	(0.57)	(0.59)
HHI $\times \geq$ 2017	9.19***	3.08**	1.75*	2.23**
	(1.33)	(1.21)	(0.83)	(0.64)
HHI $\times \geq$ 2018	9.29**	4.18**	2.23**	0.75
	(3.04)	(1.57)	(0.78)	(0.79)
HHI $\times \geq$ 2019	-2.46	-1.03	-1.56*	-1.07
	(1.86)	(1.07)	(0.67)	(0.56)
Constant	19.9***	20.8***	20.8***	21.0***
	(1.23)	(1.42)	(1.42)	(1.41)
Quarter FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
HHI Measure	Yes	Yes	Yes	Yes
N	13748	13748	13748	13748
R^2	0.3	0.3	0.3	0.30

Table gives the difference-in-difference effects of National Minimum Wage hikes on hours worked for MWWs in concentrated markets compared to MWWs in non-concentrated markets (equation 1). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 14: Actual Hours Effects Across-Market Difference-in-Difference (Placebo)

	(1)	(2)	(3)	(4)
	HHI Measurement			
	Level	$\geq$ 0.25	$\geq$ 0.10	$\geq$ Med.
$\geq$ 2017	0.31***	0.29**	0.31***	0.29*
	(0.085)	(0.085)	(0.082)	(0.15)
$\geq$ 2018	0.54***	0.58***	0.52***	0.55***
	(0.060)	(0.055)	(0.060)	(0.070)
$\geq$ 2019	-0.059	-0.039	-0.048	-0.19
	(0.084)	(0.072)	(0.078)	(0.12)
HHI $\times \geq$ 2017	-0.27	0.059	-0.097	0.0023
	(0.26)	(0.16)	(0.12)	(0.12)
HHI $\times \geq$ 2018	0.42	-0.029	0.30**	0.052
	(0.30)	(0.15)	(0.10)	(0.093)
HHI $\times \geq$ 2019	0.42	0.17	0.13	0.30
	(0.61)	(0.29)	(0.20)	(0.17)
Constant	17.3***	11.8***	11.9***	11.9***
	(0.37)	(0.40)	(0.42)	(0.51)
Quarter FE	Yes	Yes	Yes	Yes
UCM FE	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes
HHI Measure	Yes	Yes	Yes	Yes
N	150713	150713	150713	150713
R^2	0.21	0.21	0.21	0.21

Table gives the difference-in-difference effects of National Minimum Wage hikes on hours worked for *non-MWWs* in concentrated markets compared to *non-MWWs* in non-concentrated markets (equation 1). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 15: Actual Hours Effects Within-Market Difference-in-Difference

	(1)	(2)	(3)	(4)	(5)	(6)
	Sample HHI Threshold					
	≥ 0.25	< 0.25	≥ 0.10	< 0.10	$\geq$ Med.	$<$ Med.
≥ 2017	0.47*	0.30**	0.26	0.34***	0.31**	0.31*
	(0.24)	(0.085)	(0.19)	(0.079)	(0.091)	(0.14)
≥ 2018	0.60***	0.58***	0.83***	0.52***	0.60***	0.56***
	(0.11)	(0.056)	(0.12)	(0.062)	(0.065)	(0.076)
≥ 2019	0.14	-0.046	0.085	-0.057	0.12	-0.20
	(0.28)	(0.075)	(0.17)	(0.082)	(0.086)	(0.12)
Min. Wage $\times \geq 2017$	2.52*	0.63**	2.50*	0.49*	2.29***	0.17
	(1.27)	(0.23)	(1.06)	(0.24)	(0.52)	(0.21)
Min. Wage $\times \geq 2018$	2.80*	-0.61	0.9	-0.6	0.31	-0.76
	(1.18)	(0.42)	(0.63)	(0.43)	(0.61)	(0.46)
Min. Wage $\times \geq 2019$	-0.42	-0.23	-1.41	-0.094	-1.18	0.30
	(1.75)	(0.50)	(1.28)	(0.48)	(0.83)	(0.52)
Constant	3.43	20.5***	11.8 **	20.5***	12.1***	20.1***
	(6.04)	(1.12)	(4.50)	(1.10)	(1.68)	(1.48)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes	Yes
N	16367	148094	32849	131612	89315	75146
R^2	0.17	0.26	0.23	0.26	0.21	0.29

Table gives the difference-in-difference effects of National Minimum Wage hikes on hours worked for MWWs in concentrated (and non-concentrated) markets compared to non-MWWs *in the same markets* (equation 2). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 16: Actual Hours Effects Triple-Difference

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	$\geq$ 0.25	$\geq$ 0.10	$\geq$ Med.
Min. Wage $\times \geq$ 2017	0.64**	0.34	0.60**	0.46*	0.13
	(0.20)	(0.23)	(0.23)	(0.23)	(0.20)
Min. Wage $\times \geq$ 2018	-0.44	-0.71	-0.57	-0.56	-0.72
	(0.42)	(0.48)	(0.43)	(0.43)	(0.48)
Min. Wage $\times \geq$ 2019	-0.29	-0.16	-0.26	-0.13	0.25
	(0.54)	(0.50)	(0.50)	(0.48)	(0.52)
HHI $\times$ Min. Wage $\times \geq$ 2017		8.23**	2.07	2.14	2.11***
		(2.43)	(1.65)	(1.17)	(0.56)
HHI $\times$ Min. Wage $\times \geq$ 2018		6.40**	3.53**	1.59**	1.11*
		(2.09)	(1.41)	(0.57)	(0.54)
HHI $\times$ Min. Wage $\times \geq$ 2019		-1.48	-0.28	-1.42	-1.48*
		(3.02)	(1.52)	(1.14)	(0.64)
Constant	21.0***	25.9***	21.1***	21.2***	21.3***
	(1.08)	(1.01)	(1.11)	(1.11)	(1.16)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	164461	164461	164461	164461	164461
R^2	0.26	0.26	0.26	0.26	0.26

Table gives the triple-difference effect of National Minimum Wage hikes on hours worked for MWWs in concentrated and non-concentrated markets, compared to non-MWWs in the respective markets (equation 3). First column shows the average response for MWWs across all markets (equation 2) as a point of comparison. Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

cally significant hours gains for all specifications, and the 2018 hike for all but the median bin specification. The 2019 hike shows hours losses for this group for all specifications, but only the HHI $\geq$ 0.10 and median bin specifications yield statistically significant estimates. Overall these results are more consistently statistically-significant across specifications than the main results using usual hours worked, and confirm the main results.

Table 14 repeats the analysis for a placebo group of non-MWWs. We find one statistically significant estimate of a small magnitude, corresponding to a less-than 20 minute hours gain for non-MWWs in markets with a HHI $\geq$ 0.10. Although slightly different in magnitude, this is the same finding as in the main results.

Table 15 within-LLM effects of hikes on MWWs compared to non-MWWs in the same markets. The 2017 hike shows statistically significant gains of around two-and-a-half hours for each high-concentration market sample. It also yields statistically significant, though economically small, hours gains in the placebo group of non-concentrated LLMs of less than one hour for the HHI $<$ 0.25 and HHI $<$ 0.10 samples. The 2018 hike shows statistically significant gains of nearly three hours using the HHI $\geq$ 0.25 sample, but statistically insignificant and economically small effects using the other samples. The 2019 hike yields negative hours effects for nearly all samples, but none are statistically significant.

Table 16 shows actual hours effects on MWWs in concentrated markets for the triple-difference design. The 2017 hike shows statistically significant gains for two of four specifications, the 2018 for all, and the 2019 hike shows statistically significant losses for the median bin specification only. All magnitudes are similar to the main results using usual hours.

Overall, measuring outcomes by actual hours worked in the reference week yields similar results to the main analysis using reported usual hours worked. The positive hours effects of the first two hikes on MWWs in concentrated markets are in fact more consistently statistically significant across specifications using across-market control groups, but less consistently so using within-market control groups. The same is true for the negative effect of the third hike. Combining both sources of variation into the triple-difference design yields more consistently statistically significant effects than when using usual hours.

A.2 Alternative Samples

We replicate the triple-difference analysis using four alternative subsamples. First, we restrict the sample to workers observed before and after minimum wage hikes. Since hikes happen only on January 1st throughout our sample period, any worker observed in quarter 4 of any year and quarter 1 of the next is retained in the sample, and others are excluded. Second, we restrict the sample to workers in LLMs with at least 20 MWWs observed in the LFS over the sample period. As indicated in section 2, this retains three-quarters of worker-quarter observations. Third, we use a sample than retains all MWWs, but excludes non-MWWs from the bottom two income deciles – who may be indirectly affected by spillovers from NMW hikes. Finally, we consider a sample retaining all MWWs but excluding non-MWWs from the top two income deciles – who may not be comparable to MWWs, and more likely to experience different hours trends even in the counterfactual. The hours gains following the 2018 hike are robust to all sample variations, while the gains from the 2017 hike and the losses from the 2019 disappear for some.

Table 17 shows results for the cross-year sample, which excludes any workers observed in only a single year. The main analysis includes these observations, and so the main results reflect a combination of hours changes for workers observed before and after the hike, and changes in average hours due to compositional changes of workers after the hikes. In principle, compositional effects could be driving the main results. For example, all incumbent MWWs may work the same hours before and after the hike, with newly-hired MWWs working more (or fewer) hours after. While we do not necessarily want to exclude compositional effects from our main results, it is useful to distinguish these from hours changes for incumbents. Estimates from the 2018 hike are similar to those from the total sample in terms of magnitude and statistical significance. However, those following the 2017 hike become economically small, at less than an hour across the board, and statistically insignificant. Hours losses following the 2019 hike retain their sign and magnitude for the level and HHI ≥ 0.25 specifications, but become small and positive for the others; no estimate is statistically significant.

Table 18 shows triple-difference results for a sample excluding all workers in LLMs with fewer than 20 MWWs observed over the duration of the sample. This rules out the possibility of effects being driven by LLMs with a small number of MWWs. The 2017 and 2018 hours gains are robust to this sampling variation across most specifications, but the 2019 losses disappear.

Table 19 shows results yielded from a sample retaining all MWWs, but excluding all non-MWWs in the bottom two income deciles. We do not observe the hourly wage for these workers; presumably these two deciles should include most low-wage workers who experience spillover effects of minimum wage hikes (see for example Cengiz et al. 2019). We exclude observations for which the income decile is missing. The 2018 hike shows statistically significant hours gains across all specifications, the 217 in two of four, and the 2019 effects retain their negative sign and remain statistically insignificant. Magnitudes are similar to the main results.

Finally, table 20 replicates the triple-difference analysis for a sample excluding non-MMWs from the top two income deciles. Signs and magnitudes of hours change estimates are similar to the main results, but the 2017 estimates remain statistically significant only for the median bin specification. The 2018 hours gains are statistically significant for all specifications, and the 2019 hours losses remain statistically insignificant.

Altogether the variations in sample composition show that the 2018 hours gains are robust, while the 2017 gains are slightly less so, and the 2019 hours losses disappear for some subsamples.

Table 17: Hours Effects Triple-Difference (Cross-Year Sample)

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	≥ 0.25	≥ 0.10	$\geq$ Med.
Min. Wage $\times \geq 2017$	0.18	0.21	0.18	0.18	0.0091
	(0.44)	(0.54)	(0.49)	(0.47)	(0.42)
Min. Wage $\times \geq 2018$	-0.37	-0.89*	-0.57	-0.49	-0.99**
	(0.43)	(0.46)	(0.44)	(0.45)	(0.37)
Min. Wage $\times \geq 2019$	0.62	0.76	0.72	0.64	0.84
	(0.54)	(0.57)	(0.55)	(0.58)	(0.58)
HHI $\times$ Min. Wage $\times \geq 2017$		-0.17	0.17	0.23	0.86
		(4.86)	(2.42)	(1.52)	(0.71)
HHI $\times$ Min. Wage $\times \geq 2018$		12.5**	5.84**	1.71	2.14***
		(4.20)	(2.32)	(1.71)	(0.30)
HHI $\times$ Min. Wage $\times \geq 2019$		-1.12	-1.75	0.15	0.085
		(4.99)	(2.48)	(1.16)	(0.76)
Constant	21.0***	29.2***	21.1***	21.2***	21.6***
	(0.73)	(0.78)	(0.75)	(0.72)	(0.75)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	110399	110399	110399	110399	110399
R^2	0.29	0.29	0.29	0.29	0.29

Table gives the response of hours worked for MWWs in concentrated and non-concentrated markets, compared to non-MWWs in the respective markets (equation 3). First column shows the average response for MWWs across all markets (equation 2) as a point of comparison. Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 18: Hours Effects Triple-Difference (Excluding LLMs With < 20 MWWs)

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	$\geq$ 0.25	$\geq$ 0.10	$\geq$ Med.
Min. Wage $\times \geq$ 2017	0.47	0.18	0.45	0.33	0.051
	(0.29)	(0.25)	(0.30)	(0.27)	(0.20)
Min. Wage $\times \geq$ 2018	-0.092	-0.37	-0.17	-0.15	-0.24
	(0.51)	(0.46)	(0.47)	(0.47)	(0.56)
Min. Wage $\times \geq$ 2019	-0.066	-0.029	-0.051	-0.050	0.24
	(0.51)	(0.55)	(0.50)	(0.51)	(0.64)
HHI $\times$ Min. Wage $\times \geq$ 2017		10.1**	0.50	2.35***	2.25**
		(3.10)	(2.56)	(0.61)	(0.76)
HHI $\times$ Min. Wage $\times \geq$ 2018		11.0**	7.80***	1.83	0.87
		(4.35)	(2.07)	(1.57)	(0.53)
HHI $\times$ Min. Wage $\times \geq$ 2019		1.71	0.18	0.11	-0.80
		(4.63)	(1.51)	(1.50)	(1.09)
Constant	20.3***	17.3***	20.3***	20.3***	20.5***
	(1.05)	(1.15)	(1.07)	(1.11)	(1.21)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	130276	130276	130276	130276	130276
R^2	0.30	0.30	0.30	0.30	0.30

Table gives the response of hours worked for MWWs in concentrated and non-concentrated markets, compared to non-MWWs in the respective markets (equation 3). First column shows the average response for MWWs across all markets (equation 2) as a point of comparison. Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 19: Hours Effects Triple-Difference (Excluding Bottom Two Income Deciles)

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	≥ 0.25	≥ 0.25	≥ 0.25
Min. Wage $\times \geq 2017$	0.21	-0.00	0.18	0.04	-0.19
	(0.21)	(0.21)	(0.23)	(0.21)	(0.20)
Min. Wage $\times \geq 2018$	-0.06	-0.42	-0.23	-0.17	-0.43
	(0.39)	(0.45)	(0.40)	(0.41)	(0.52)
Min. Wage $\times \geq 2019$	-0.42	-0.24	-0.32	-0.25	0.00
	(0.50)	(0.45)	(0.46)	(0.44)	(0.55)
HHI $\times$ Min. Wage $\times \geq 2017$		5.33*	0.76	1.69	1.80*
		(2.63)	(1.58)	(1.46)	(0.77)
HHI $\times$ Min. Wage $\times \geq 2018$		8.19**	4.24**	1.74**	1.26**
		(2.85)	(1.58)	(0.68)	(0.44)
HHI $\times$ Min. Wage $\times \geq 2019$		-1.96	-1.43	-1.45	-0.99
		(3.54)	(1.62)	(0.84)	(0.66)
Constant	26.6**	31.4***	26.8***	26.9***	27.6***
	(0.71)	(0.79)	(0.75)	(0.8)	(0.81)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	71769	71769	71769	71769	71769
R^2	0.37	0.37	0.37	0.37	0.37

Table gives the triple-difference effect (equation 3), of the 2017, 2018, and 2019 National Minimum Wage hikes on hours worked for MWWs in concentrated markets. First column shows difference-in-difference effect on MWWs (equation 2). Unit of observation is a worker-quarter. Standard errors clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

Table 20: Hours Effects Triple-Difference (Excluding Top Two Income Deciles)

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	≥ 0.25	≥ 0.25	≥ 0.25
Min. Wage $\times \geq 2017$	0.32	0.1	0.31	0.15	-0.12
	(0.25)	(0.28)	(0.26)	(0.29)	(0.26)
Min. Wage $\times \geq 2018$	-0.13	-0.44	-0.26	-0.18	-0.30
	(0.41)	(0.46)	(0.43)	(0.41)	(0.48)
Min. Wage $\times \geq 2019$	-0.15	0.019	-0.045	-0.021	0.23
	(0.52)	(0.54)	(0.51)	(0.50)	(0.73)
HHI $\times$ Min. Wage $\times \geq 2017$		5.19	0.46	1.58	1.90**
		(3.01)	(1.84)	(1.34)	(0.55)
HHI $\times$ Min. Wage $\times \geq 2018$		8.30**	4.22**	1.43*	0.84**
		(2.69)	(1.72)	(0.69)	(0.34)
HHI $\times$ Min. Wage $\times \geq 2019$		-2.56	-1.85	-1.29	-0.92
		(3.60)	(1.85)	(0.99)	(0.84)
Constant	23.2**	33.4***	23.3***	23.3***	23.8***
	(0.63)	(0.57)	(0.65)	(0.63)	(0.57)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	67130	67130	67130	67130	67130
R^2	0.33	0.33	0.33	0.33	0.33

Table gives the triple-difference effect on hours worked for MWWs in concentrated and non-concentrated markets versus non-MWWs in respective markets (equation 3). First column shows the average response for MWWs across all markets (equation 2) as a point of comparison. Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level.
*** $p < .01$, ** $p < .05$, * $p < .1$

B Alternative Job Loss Definitions

Here we present results for two alternative definitions of job loss. In the main text we considered all employment to non-employment transitions, including transitions to unemployment and labour market exits. Here we consider employment to unemployment transitions only, as well as transitions from positive actual hours worked to zero. The latter includes workers who report being employed, even while working zero hours in the reference week. The results are statistically much weaker, and economically smaller, for both alternative job loss definitions. However, they retain their sign, and some specifications yield statistically significant results.

Table 21 replicates the triple-difference analysis using job loss to unemployment only. Transitions out of the labour force are not counted. MWWs in concentrated markets experience lower rates of job loss to unemployment for the level and median bin specifications in 2017, but these are very small at two and one percent respectively. All estimates for the 2018 hike are null. The level specification predict that MWWs in concentrated markets were five percent more likely to transition to unemployment than control groups following the 2019 hike. Other specifications yield null results for 2019.

Finally, table 22 shows triple-difference results using positive to zero actual hours transitions as the definition of job loss. This excludes workers who transition to unemployment or exit the labour force and stop reporting hours. In the main text these workers would not have counted as losing their jobs, as they still report being employed despite working zero hours in the reference week. These workers were excluded from the sample used in the actual hours analysis in appendix A, which retains only those workers working positive actual hours, but are included in the hours analysis in the main text so long as they report positive usual hours worked. The zero-hours outcome yields statistically insignificant results across the board, except for a nine percent lower likelihood of job loss following the 2017 hike for the HHI ≥ 0.25 specification. The level specification predicts that MWWs in monopsony markets would experience a fourteen percent lower likelihood of job loss following the 2017 hike compared to those in a perfectly competitive market, but this estimate is statistically insignificant. Other effects are null.

Altogether, the alternative definitions of job loss yield weaker results than those in the main text, using the employment to non-employment definition. In the case of job loss to unemployment, this is due to the extremely small number of workers who transition from employment to unemployment, regardless of minimum wage status or presence in a concentrated LLM; the vast majority of unemployed workers remain so for the duration of the short panel. In the case of job loss to zero hours, the generally null results are not due to small sample size; section 5 shows that seven percent of observations report zero hours in the reference week during the current quarter, but positive in the previous. It may be that most zero-hours reporters are not in fact non-employed.

Both alternative definitions exclude labour market exiters, who are a population of interest. We consider these not to be robustness checks for the main results per se, but indicative of alternative channels of job loss, and generally supportive of the main results.

Table 21: Job Loss to Unemployment Triple-Difference

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	≥ 0.25	≥ 0.10	$\geq$ Med.
Lag Min. Wage $\times \geq 2017$	0	0	0	0	0
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Lag Min. Wage $\times \geq 2018$	0	0	0	0	0
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Lag Min. Wage $\times \geq 2019$	-0.00*	-0.01**	-0.00**	-0.00*	0
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
HHI $\times$ Lag Min. Wage $\times \geq 2017$		-0.02*	0	0	-0.01**
		(0.01)	(0.00)	(0.00)	(0.00)
HHI $\times$ Lag Min. Wage $\times \geq 2018$		0	0	0.01	0
		(0.02)	(0.00)	(0.01)	(0.01)
HHI $\times$ Lag Min. Wage $\times \geq 2019$		0.05*	0.02	0	0
		(0.03)	(0.01)	(0.01)	(0.00)
Constant	0	0.01**	0	0	0
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	94298	94100	94100	94100	94100
R^2	0.011	0.011	0.011	0.011	0.011

Table gives the triple-difference effect of National Minimum Wage hikes on the likelihood of an *employment to unemployment* transition for MWWs in concentrated and non-concentrated markets, compared to non-MWWs in the respective markets (equation **??**). First column shows the average response for MWWs across all markets (equation **??**) as a point of comparison. Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level. *** $p < .01$, ** $p < .05$, * $p < .1$

Table 22: Job Loss to Zero Hours Triple-Difference

	(1)	(2)	(3)	(4)	(5)
		HHI Measurement			
	n/a	Level	≥ 0.25	≥ 0.10	$\geq$ Med.
Lag Min. Wage $\times \geq 2017$	0.03**	0.04***	0.04***	0.03***	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Min. Wage $\times \geq 2018$	-0.01	-0.01	-0.01	0	0
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lag Min. Wage $\times \geq 2019$	0	0	0	0	0
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
HHI $\times$ Lag Min. Wage $\times \geq 2017$		-0.14	-0.09*	0.01	0.01
		(0.11)	(0.04)	(0.05)	(0.01)
HHI $\times$ Lag Min. Wage $\times \geq 2018$		0	0.03	-0.02	-0.01
		(0.12)	(0.03)	(0.05)	(0.02)
HHI $\times$ Lag Min. Wage $\times \geq 2019$		-0.02	-0.04	0	0.03
		(0.11)	(0.05)	(0.04)	(0.03)
Constant	0.02	0.08***	0.02	0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Year $\times$ Quarter FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Min. Wage	Yes	Yes	Yes	Yes	Yes
HHI Measure	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Min. Wage	No	Yes	Yes	Yes	Yes
HHI Measure $\times$ Year FE	No	Yes	Yes	Yes	Yes
N	88973	88790	88790	88790	88790
R^2	0.048	0.048	0.048	0.048	0.048

Table gives the triple-difference effect of National Minimum Wage hikes on the likelihood of an *employment to unemployment* transition for MWWs in concentrated and non-concentrated markets, compared to non-MWWs in the respective markets (equation **??**). First column shows the average response for MWWs across all markets (equation **??**) as a point of comparison. Unit of observation is a worker-quarter. Standard errors are clustered at the LLM (industry-region) level. *** $p < .01$, ** $p < .05$, * $p < .1$