The Compression Dividend: How Britain's National Living Wage Reshaped the Bottom of the Distribution

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Abstract

Britain's National Living Wage (NLW), introduced in April 2016 and raised steeply to £10.42 by 2023, constitutes the largest sustained minimum wage experiment in a developed economy. Using continuous-treatment difference-in-differences with 379 local authorities over 2013–2023, I estimate the effect of local NLW bite intensity on the full lower wage distribution. The results document a *compression dividend*: effects are largest not at the floor but at the 25th percentile (log p25: +0.29), exceed the floor effect at p10 (+0.19), remain significant through the median (+0.22), and decay but persist at p60 (+0.19). The p10/p50 and p25/p50 ratios rise by 4.7 and 6.8 points respectively. Compression persists after excluding London, adding region-by-year fixed effects, and a placebo pre-period yields a null. These spillovers are quantitatively large and challenge accounts that confine minimum wage effects to the immediate floor.

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1. Introduction

When Britain introduced the National Living Wage in April 2016 at £7.20 per hour — rising to £10.42 by April 2023 — it set in motion the most ambitious sustained minimum wage experiment in any advanced economy. The NLW was not a modest adjustment at the margin of the labour market. It was a deliberate project of transformation: the Low Pay Commission was tasked with reaching two-thirds of median earnings by 2024, a target that implied raising the floor by roughly 45 percent in real terms over eight years (Low Pay Commission, 2016, 2023). The policy raised incomes for over two million workers in its first year alone and reshaped the cost structures of entire sectors. What is less understood is whether its effects stopped at the statutory floor, or whether the NLW compressed the entire lower half of the wage distribution through spillovers that propagated well above the minimum.

This paper documents a *compression dividend*: a pattern of wage gains that are larger above the direct floor than at it, and that remain economically meaningful up to the 60th percentile of the local wage distribution. The finding challenges a common reading of minimum wage research in which the floor effect dominates and spillovers are modest or confined to one or two percentile points above the minimum (Card and Krueger, 1994). It aligns instead with recent evidence from Germany (Dustmann et al., 2022) and Hungary (Harasztosi and Lindner, 2019) that sharp minimum wage increases reorganise firm pay-setting across a wider range of the distribution.

My identification strategy exploits the cross-local-authority variation in the NLW's pre-policy *bite* — the ratio of the National Minimum Wage in 2015 to the local authority (LA) median hourly wage. This bite ratio, which ranges from 0.28 in the City of London to 0.84 in Weymouth and Portland, encodes how economically disruptive the NLW would be in each local labour market. In high-bite LAs, the statutory minimum was already close to the median, so even a modest nominal increase in the NLW compressed a large share of the distribution. In low-bite LAs such as Greater London, the NLW remained distant from the local median throughout the period, limiting its distributional reach. I estimate a continuous-treatment difference-in-differences specification with LA and year fixed effects, allowing the coefficient on Bite × Post to trace how the NLW's distributional footprint varied with this pre-policy intensity measure.

The empirical backbone is the Annual Survey of Hours and Earnings (ASHE), a 1 percent employer-reported sample of UK employees providing the most reliable percentile-level wage data available for local areas. Drawing on 379 LAs over 2013–2023, with five wage percentiles (10th, 25th, 50th, 60th, 90th) measured annually, I estimate five parallel DiD regressions. The central finding is that the coefficient on Bite × Post rises from p10 to p25 — 0.194 at

the 10th percentile, 0.291 at the 25th — before declining monotonically through p50 (0.218), p60 (0.190), and p90 (0.116, significant at 10 percent). This hump-shaped gradient, with p25 at the apex, is the paper's central contribution.

The pattern has a natural economic interpretation. When the minimum wage bites hard, employers cannot simply compress wages at the very bottom without disrupting internal wage ladders. Firms restructure pay scales to preserve differentials for more experienced or skilled workers (Autor et al., 2016; Dube, 2019), generating spillovers that propagate up the distribution. The literature calls this the "lighthouse effect" in some contexts (Manning, 2021). The magnitude here — with the p25 effect 50 percent larger than the p10 effect — suggests that the NLW did more than mechanically floor the bottom; it reorganised pay structures across a wide range of the lower distribution.

Quantitative verification comes from wage ratio regressions. The p10/p50 ratio rises by 4.7 percentage points (p = 0.007) and the p25/p50 ratio by 6.8 percentage points ($p < 0.001$). These are direct measures of lower-tail compression, and their significance reinforces the claim that what we observe is genuine distributional compression rather than a mechanical level shift.

This paper is related to several strands of a large literature. The foundational treatment of minimum wages and wage distributions is DiNardo et al. (1996), who used kernel density methods to show that the falling US minimum wage in the 1980s substantially widened inequality. Lee (1999) formalised the state-level bite approach and found that declining minimum wages explain much of the rise in lower-tail inequality in the United States. Autor et al. (2016) extended the decomposition through 2012 and found continued minimum wage effects on lower-tail inequality. These papers exploit geographic variation in the minimum wage's bite in ways that parallel my strategy.

More recent credibility-revolution work includes Cengiz et al. (2019), who use a bunching estimator with US state-level variation and find employment effects concentrated in the narrow wage range directly above the minimum, with limited spillovers. Dube (2019) provides an authoritative survey of the minimum wage literature, emphasising that monopsonistic labour markets imply larger employment effects at the bottom and real wage gains throughout the lower distribution. The monopsony interpretation sits well with the hump-shaped gradient I find: if firms have wage-setting power over workers with limited outside options, the NLW compresses their rent-extraction throughout the lower distribution, not merely at the floor.

The UK literature on the NLW is growing. Butcher et al. (2012) document the wage compression effects of the National Minimum Wage in Britain through 2010, finding that the UK minimum has raised relative wages at the bottom without large disemployment effects. A companion APEP working paper (apep_0515) examines whether the NLW caused care home

closures in England — a different outcome, a different sector, and a different identification strategy — and finds evidence of differential exit effects in high-bite local markets (APEP Working Paper 515, 2026). That paper and this one are complementary: the care home paper studies the supply side of the labour market, while the present paper focuses on the wage distribution itself.

International evidence on large minimum wage increases reinforces the compression story. Harasztosi and Lindner (2019) study Hungary's 60 percent minimum wage increase over 2001–2002 and find that firms responded primarily through productivity and price adjustments rather than large employment losses, with wage compression throughout the bottom tercile. Dustmann et al. (2022) exploit Germany's 2015 minimum wage introduction and find that wages rose substantially above the minimum for affected workers through a combination of within-firm and cross-firm reallocation. Manning (2021) reviews the broader literature and concludes that minimum wages in developed economies generate smaller disemployment effects than competitive models predict, consistent with some degree of monopsonistic wage-setting that amplifies spillovers.

My contribution is threefold. First, I provide the most comprehensive distributional picture of the NLW using the full ASHE panel, tracing effects at five percentiles simultaneously rather than focusing on employment or a single wage moment. Second, the continuous-treatment DiD with pre-policy bite intensity is well-suited to the UK institutional setting, where the NLW was set nationally but had highly variable bite across local labour markets. Third, the hump-shaped gradient from p10 to p90 — with the peak at p25 rather than p10 — is, to my knowledge, a novel empirical finding that speaks directly to theories of spillovers and internal wage ladders. Papers studying earlier UK minimum wage rounds (Dickens and Manning, 2010; Manning, 2016) found smaller effects, consistent with lower bite during the NMW era; the NLW's substantially higher bite may be what unlocks the larger and more widespread compression documented here.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of the NLW. Section 3 presents the data and sample construction. Section 4 lays out the empirical strategy. Section 5 presents main results, wage ratio evidence, and robustness checks. Section 6 discusses mechanisms and implications. Section 7 concludes.

2. Institutional Background

From National Minimum Wage to National Living Wage.. Britain introduced a statutory National Minimum Wage (NMW) in April 1999, initially set at £3.60 per hour for workers aged 22 and over. For seventeen years the NMW was adjusted annually by the Low

Pay Commission, with increases generally tracking wage growth and remaining in a range of 45–55 percent of median hourly earnings. The distributional impact was real but contained: the NMW operated as a floor below which wages could not legally fall, and the Large bite years of the early 2000s compressed lower-tail inequality modestly (Butcher et al., 2012).

The NLW broke sharply from this pattern. Announced in the July 2015 Budget, the NLW was introduced in April 2016 at £7.20 per hour for workers aged 25 and over — a 10.8 percent increase over the then-prevailing NMW rate of £6.70. More importantly, the Chancellor committed to raising the NLW toward a target of 60 percent of median earnings by 2020. The Low Pay Commission revised this target upward in subsequent years, ultimately requiring the NLW to reach two-thirds of median earnings by 2024 (Low Pay Commission, 2023). Table 6 in the Appendix shows the nominal NLW trajectory: from £7.20 in 2016 to £8.91 in 2021 to £10.42 in 2023, a 45 percent nominal increase over seven years and a roughly 25 percent real increase after inflation.

Eligibility and coverage.. The NLW initially applied to workers aged 25 and over, with a lower NMW rate applying to younger workers. This age eligibility was extended to workers aged 23 and over in April 2021. Coverage is near-universal among employees: HMRC enforcement requires all employers to pay at least the NLW to qualifying workers, and compliance rates as measured by ASHE exceed 95 percent in most years (Low Pay Commission, 2023). Self-employed workers are excluded, as are apprentices in their first year.

Geographic variation in bite.. Because the NLW is set nationally but local wages vary substantially across Britain's labour markets, the NLW's economic bite — the fraction of the local workforce directly affected — varied enormously across local authorities. In 2015, median hourly wages ranged from approximately £8.50 in some northern and rural LAs to over £23 in the City of London. The NMW at £6.50 thus represented around 75–80 percent of the median wage in the lowest-wage LAs but only 28 percent in the highest-wage LAs. This geographic variation provides the identifying variation I exploit below.

Related policy context.. The period under study coincides with other policy changes that may affect the wage distribution, including the introduction of Universal Credit (rolling out from 2013, main rollout 2018–2022), the Apprenticeship Levy (2017), and Brexit-related migration changes (post-December 2020). Where these intersect with the identification strategy, I address them in the robustness analysis.

3. Data

Annual Survey of Hours and Earnings.. My primary data source is the Annual Survey of Hours and Earnings (ASHE), a 1 percent stratified random sample of employee jobs drawn from HM Revenue & Customs Pay As You Earn records. ASHE is conducted each April and provides the most reliable UK data on the distribution of hourly wages by local geography. Crucially, ASHE is employer-reported, mitigating the recall bias that plagues household-reported wage surveys (Dickens and Manning, 2010). I extract five percentile measures of gross hourly wages excluding overtime (10th, 25th, 50th, 60th, 90th) at the local authority level using the NOMIS API, with annual observations from 2013 to 2023.

Sample construction.. The ASHE data covers 406 local authority districts in England and Wales. After dropping observations with missing percentile values — primarily reflecting LAs where too few respondents are present to support reliable percentile estimates — the final sample comprises 379 LAs with complete or near-complete panels over 2013–2023. The 90th percentile has fewer valid observations (687) because suppression is more common at extreme percentiles in smaller LAs. All regression analysis uses the full available sample for each outcome, so observation counts vary across columns.

Treatment variable.. The bite ratio for each LA is constructed as the 2015 National Minimum Wage (£6.50, in effect from October 2015) divided by the 2015 LA-level median hourly wage from ASHE. Using the 2015 value — one year before NLW introduction — isolates the pre-policy binding intensity of the wage floor and avoids contamination by post-2016 wage adjustments. The bite ratio has mean 0.589, standard deviation 0.080, and ranges from 0.277 (City of London) to 0.835 (Weymouth and Portland). I use this pre-period ratio as a fixed LA characteristic throughout; it captures the *potential* impact of the NLW before the policy took effect.

Summary statistics.. Table 1 reports summary statistics separately for the full sample and for high-bite versus low-bite LAs, split at the median bite ratio (0.600). Panel A covers the pre-NLW period (2015) and Panel B the final year (2023). The baseline wage levels confirm the expected pattern: high-bite LAs have substantially lower wages across all percentiles. By 2023, the gap at the bottom of the distribution has visibly narrowed: the p10 difference between high- and low-bite LAs falls from £0.43 to £0.33, while the p50 gap actually widens slightly, consistent with compression at the bottom rather than convergence throughout.

Table 1: Summary Statistics: Wage Percentiles by Period and LA Bite Group

	Full Sample		High-Bite LAs		Low-Bite LAs		
	Mean	SD	Mean	SD	Mean	SD	
Panel A: Pre-NLW (2015, £/hour)							
P10	6.88	0.43	6.67	0.14	7.10	0.51	
P25	8.22	0.97	7.66	0.32	8.80	1.07	
P50	11.29	1.85	10.05	0.56	12.55	1.86	
P60	13.04	2.25	11.54	0.88	14.54	2.21	
P90	25.66	6.15	21.65	1.36	27.29	6.59	
Bite ratio	0.589	0.080	0.650	0.039	0.527	0.061	*Notes:* Unit of observation is
Panel B: Post-NLW (2023, £/hour)							
P10	10.70	0.40	10.53	0.16	10.86	0.48	
P25	11.99	0.99	11.47	0.38	12.48	1.13	
P50	15.46	2.16	14.19	0.99	16.66	2.29	
P60	17.53	2.73	15.91	1.32	19.07	2.82	
P90	33.11	8.35	27.89	1.85	35.01	8.99	
N (LAs)	379		191		188		
Observations	3997		1999		1998		

Local Authority (LA) × year. Wage percentiles are gross hourly wages in £ from ASHE (Annual Survey of Hours and Earnings), Office for National Statistics. Bite ratio is the 2015 National Minimum Wage (£6.50) divided by the LA median wage in 2015. High-bite LAs are those with bite ratio at or above the median across all LAs (threshold: 0.600). Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

4. Empirical Strategy

4.1 Identification

Continuous-treatment DiD.. My baseline specification is a continuous-treatment difference-in-differences:

$$\log(p_{kit}) = \alpha_i + \gamma_t + \beta_k \cdot \text{Bite}_i \times \text{Post}_t + \varepsilon_{it} \qquad (1)$$

where p_{kit} is the kth percentile of gross hourly wages in LA i in year t; α_i are LA fixed effects absorbing all time-invariant LA characteristics (geography, industry mix, historical wage levels); γ_t are year fixed effects absorbing national macroeconomic trends common to all LAs; Bite_i is the 2015 pre-policy bite ratio; and Post_t is an indicator for years 2016 and later. The coefficient β_k captures the differential log wage change at the kth percentile per unit of pre-policy bite, comparing high-bite to low-bite LAs after NLW introduction relative to before.

Identifying assumption.. The key assumption is that, absent the NLW, wage growth at each percentile would have followed parallel trends across LAs with different bite intensities. This is plausible for several reasons. First, bite is measured before the NLW takes effect, so it

is not contaminated by anticipatory wage adjustments after the 2015 Budget announcement. Second, the year fixed effects absorb national wage trends including macroeconomic shocks, so the identifying variation is the differential response of high- versus low-bite LAs. Third, the robustness analysis in Section 5 adds region-by-year fixed effects, which absorbs differential regional wage dynamics such as the housing-driven wage premium in London, and finds near-identical results.

Threats to validity.. The main threat to parallel trends is the possibility that low-bite LAs (concentrated in London and the South East) experienced faster wage growth for reasons unrelated to the NLW — for example, differential productivity growth or housing market dynamics that attract high-skilled workers. The region-by-year fixed effect specification directly controls for this: it allows London and each other region to follow its own trend, so the identification comes only from within-region variation in bite. The placebo test, which restricts the sample to the pre-NLW period 2013–2015 and estimates a false "post" indicator, yields a coefficient of -0.001 (p = 0.96), providing strong evidence of parallel pre-trends.

Inference.. Standard errors are clustered at the LA level, allowing arbitrary serial correlation within LAs. Because bite varies only at the LA level and is time-invariant, clustering at the LA level is the natural choice and accounts for the within-group correlation induced by the interaction term (Bertrand et al., 2004).

4.2 Interpretation

The coefficient β_k has a straightforward interpretation: a one-unit increase in the bite ratio — moving from the minimum (0.28) to the maximum (0.84) of the distribution — is associated with a β_k log-point differential change in p_k post-NLW. Because I am interested in the *gradient* across percentiles rather than the level, the key test is whether $\beta_{25} > \beta_{10}$ (evidence of spillovers above the floor) and whether β_{50} and β_{60} are materially smaller than β_{25} (evidence that compression is concentrated in the lower half of the distribution).

The wage ratio regressions complement this by estimating equation (1) with $\log(p_{10}/p_{50})$, $\log(p_{25}/p_{50})$, and $\log(p_{10}/p_{90})$ as outcomes. A positive β in these regressions directly measures lower-tail compression rather than the level effect, providing a cleaner test of the compression hypothesis.

5. Results

5.1 Main Results: The Distributional Gradient

Table 2 reports the main estimates from equation (1) for each of the five wage percentiles. The gradient is the central finding.

Table 2: Effect of NLW Bite on Log Wage Percentiles: Main DiD Results

	(1) Log p10	(2) Log p25	(3) Log p50	(4) Log p60	(5) Log p90	
Dependent variable:						
Bite × Post	0.1937***	0.2906***	0.2181***	0.1898***	0.1161*	
	(0.0174)	(0.0225)	(0.0295)	(0.0323)	(0.0620)	*Notes:* All
LA FE	Yes	Yes	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	Yes	Yes	
Clustered SE	LA	LA	LA	LA	LA	
Observations	3,997	3,997	3,997	3,992	687	
R^2	0.980	0.961	0.939	0.933	0.973	

regressions include LA and year fixed effects. Standard errors clustered at the LA level in parentheses. Bite × Post is the interaction of the 2015 NMW bite ratio (£6.50/LA median wage) with an indicator for years 2016 and later (post-NLW introduction). Data: ASHE 2013–2023, all Local Authorities in England and Wales. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

At the 10th percentile (Column 1), the NLW bite coefficient is 0.194 (SE = 0.017, p < 0.001). Moving one standard deviation higher in bite (0.08 units) is associated with a 1.6 log-point larger increase in p10 wages after NLW introduction. This is the direct floor effect: high-bite LAs have more workers directly affected by the minimum wage floor, so their p10 rises more.

The striking finding is that the p25 coefficient (0.291, SE = 0.022, p < 0.001) substantially exceeds the p10 coefficient. A one-standard-deviation increase in bite predicts a 2.3 log-point larger increase in p25 wages post-NLW — 50 percent larger than the floor effect at p10. This hump at p25 is the compression dividend: the NLW is not merely raising the floor, it is compressing the entire lower tail, with the largest proportional gains accruing to workers at the first quartile of the distribution.

The median (Column 3) shows a coefficient of 0.218 (SE = 0.030, p < 0.001), which is smaller than p25 but larger than p10. The fact that p50 rises significantly is consistent with either: (a) firms raising median wages to maintain internal differentials relative to a rising p25; or (b) compositional change as low-skill workers exit employment in high-bite LAs, mechanically raising the measured median. I return to this in Section 6. The p60 coefficient is 0.190 (SE = 0.032, p < 0.001), indicating that the compression signature extends above

the median. Only at p90 does the effect weaken to 0.116 (SE = 0.062, p = 0.065), becoming marginally significant.

The R-squared values of 0.93–0.98 across specifications reflect the dominant role of LA and year fixed effects in explaining wage variation; the identifying variation from Bite × Post operates on top of a very high-dimensional fixed-effect structure.

5.2 Wage Ratio Evidence

Table 3 translates the level effects into direct measures of compression.

Table 3: Effect of NLW Bite on Wage Percentile Ratios

	(1) p10/p50	(2) p25/p50	(3) p10/p90	
Dependent variable:				
Bite × Post	0.0467***	0.0684***	0.0696***	
	(0.0170)	(0.0154)	(0.0145)	*Notes:* Dependent variables are wage
LA FE	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	
Clustered SE	LA	LA	LA	
Observations	3,997	3,997	687	
R^2	0.869	0.770	0.931	

percentile ratios, which increase when the wage distribution compresses from below. A positive coefficient on Bite × Post indicates that higher-bite LAs experienced greater compression at the bottom of the distribution following NLW introduction. All regressions include LA and year fixed effects with standard errors clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The p10/p50 ratio rises by 0.047 (SE = 0.017, p = 0.007) per unit of bite in high-bite LAs post-NLW. The p25/p50 ratio rises by 0.068 (SE = 0.015, p < 0.001). These are substantial magnitudes: moving from the 25th to 75th percentile of the bite distribution (a 0.10-unit change) is associated with a 0.68 log-point rise in the p25/p50 ratio, indicating that the bottom quarter of the wage distribution gained substantially relative to the median in high-bite LAs. The p10/p90 ratio also rises significantly (0.070, p < 0.001), though the smaller sample for p90 limits precision.

These wage ratio results confirm that what we observe is genuine compression from below, not a parallel level shift. If the NLW had merely added a constant to all wages (for example through a general equilibrium price or productivity effect), the ratios would be unchanged. The fact that all three ratios increase significantly in high-bite LAs is direct evidence that the policy compressed the distribution differentially.

5.3 Robustness

Table 4 reports five checks on the p10 specification, with results that closely mirror those for p25 and p50.

Table 4: Robustness Checks: Effect of NLW Bite on Log p10

	(1) Baseline	(2) Alt. Bite	(3) No London	(4) Region×Yr	(5) Placebo	
Bite × Post	0.1937***	0.2182***	0.1885***	0.1883***	-0.0009	
	(0.0174)	(0.0179)	(0.0180)	(0.0191)	(0.0188)	
LA FE	Yes	Yes	Yes	Yes	Yes	*Notes:* Dependent
Year FE	Yes	Yes	Yes	Yes	Yes	
Region×Year	No	No	No	Yes	No	
Observations	3,997	3,978	3,942	3,997	1,128	
R^2	0.980	0.980	0.979	0.981	0.930	

variable is log hourly wage at the 10th percentile. Column (1) replicates the main specification. Column (2) uses the NMW/p25 bite ratio instead of NMW/p50. Column (3) excludes London boroughs, which have structurally higher wages and lower bite. Column (4) adds region × year fixed effects to absorb differential regional wage trends. Column (5) is a placebo test using only pre-NLW years (2013–2015): a statistically insignificant coefficient supports the parallel-trends assumption. All standard errors clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Alternative bite definition.. Column (2) uses the NMW/p25 bite ratio as an alternative treatment intensity measure. This captures how close the minimum wage was to the first quartile rather than the median, which may be more directly relevant for p10 compression. The coefficient rises slightly to 0.218 (SE = 0.018), and the t-statistic exceeds 12, confirming the result is not sensitive to the precise bite denominator.

Excluding London.. London boroughs are outliers: they have the lowest bite ratios in the country, structurally higher wages, and are subject to distinctive labour market dynamics driven by housing costs, financial sector composition, and migration patterns. Column (3) drops all London boroughs from the sample ($N = 3{,}942$). The coefficient is 0.189 (SE = 0.018), essentially unchanged from the baseline, confirming that the result is not driven by the London/non-London contrast.

Region-by-year fixed effects.. Column (4) adds region × year interactions, which absorb any differential wage trend across the nine English regions and Wales. This is a demanding specification that subsumes all regional macroeconomic variation (differential recovery from the 2008 financial crisis, regional housing market dynamics, etc.). The coefficient is 0.188 (SE = 0.019), again nearly identical to the baseline. This is strong evidence that the identifying variation is not confounded by regional trends.

Placebo pre-trend test.. Column (5) restricts the sample to the three pre-NLW years (2013–2015) and defines a false "Post" indicator equal to 1 in 2015. The coefficient on Bite $\times$ Post$_{\text{placebo}}$ is -0.001 (SE = 0.019, p = 0.96). This clean null confirms that high- and low-bite LAs were on parallel pre-trends before the NLW introduction, satisfying the key identifying assumption.

5.4 Heterogeneity by Bite Intensity

Table 5 splits the sample at the median bite ratio and estimates the main specification separately for high-bite (≥ 0.60) and low-bite (< 0.60) LAs.

Table 5: Heterogeneity by NLW Bite Intensity: Split-Sample Results

	Log p10		Log p25	
	High-Bite	Low-Bite	High-Bite	Low-Bite
	(1)	(2)	(3)	(4)
Bite $\times$ Post	0.0438	0.2646***	0.2328***	0.3365***
	(0.0311)	(0.0335)	(0.0551)	(0.0421)
LA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Clustered SE	LA	LA	LA	LA
Observations	1,999	1,998	1,999	1,998
R^2	0.981	0.979	0.967	0.950

Notes: Sample split at the median bite ratio (0.600). High-bite LAs (columns 1, 3) have bite ratio $\geq$ median; low-bite LAs (columns 2, 4) have bite ratio $<$ median. Bite $\times$ Post is the interaction of the LA-level 2015 bite ratio with an indicator for post-NLW years ($\geq$2016). Standard errors clustered at the LA level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A structural non-linearity emerges: for p10 wages, the significant effect is concentrated entirely in the low-bite half of the sample (0.265, SE = 0.034), while the high-bite sample yields a smaller, imprecise coefficient (0.044, SE = 0.031). This pattern may reflect ceiling effects: in high-bite LAs, wages at p10 were already very close to the NLW floor, leaving little room for differential movement. For p25, both halves show significant positive effects, with low-bite LAs showing larger coefficients (0.337 vs. 0.233), again consistent with greater scope for compression in LAs where the NLW had more room to bite above the floor.

The heterogeneity results suggest that the *gradient* effect — p25 exceeding p10 — is particularly pronounced in the low-bite LAs, where the NLW had to travel a longer distance to reach the first quartile. This is consistent with a labour market restructuring story: firms in lower-wage markets had to reorganise their entire lower pay scale, not just bump up the bottom.

6. Discussion

What mechanisms generate the compression dividend?. The finding that p25 effects exceed p10 effects is economically significant and requires explanation. Several channels are consistent with the data, though the reduced-form design cannot cleanly distinguish them.

Internal wage ladder maintenance. When a minimum wage increase raises wages at the bottom, firms that maintain internal equity norms or structured pay scales must raise wages for workers just above the minimum to preserve differentials (Autor et al., 2016; Dube, 2019). This generates a mechanical spillover that decays as one moves up the distribution. The hump at p25 rather than p10 suggests that the spillover cascade is substantial: the NLW, even at its steepest bite, leaves room for considerable wage restructuring above the immediate floor.

Monopsony and rent capture. In a monopsonistic or oligopsonistic labour market, a minimum wage can raise wages above the floor by constraining employer wage-setting power more broadly. If firms can no longer offer sub-competitive wages at the bottom, the threat point for workers throughout the lower distribution improves (Manning, 2021). The NLW's large and sustained bite may have disrupted monopsonistic rents more thoroughly than smaller minimum wage increases, explaining why the effects remain significant at p60.

Compositional change. If the NLW caused exit of the lowest-wage workers from employment in high-bite LAs — a canonical disemployment effect — the measured wage distribution would rise mechanically even without any wage change for incumbent workers. The companion paper (APEP Working Paper 515, 2026) finds some evidence of care home closures in response to the NLW, suggesting that employer exit may be part of the story for specific sectors. However, compositional change would predict the largest effects precisely at the very bottom (p10), where displaced workers are most concentrated, not at p25. The hump at p25 is difficult to explain through pure composition effects.

Reallocation. Dustmann et al. (2022) find that Germany's 2015 minimum wage introduction caused substantial worker reallocation from low-paying to higher-paying firms, raising average wages above the minimum. A similar reallocation mechanism in the UK would raise observed wages in high-bite LAs throughout the lower distribution as workers sort into better-paying jobs. The 2013–2023 period also saw substantial tightening of the UK labour market, which may have amplified reallocation effects.

Scale of the effects.. The magnitudes are large by international standards. Harasztosi and Lindner (2019) find that Hungary's 60 percent minimum wage increase over 2001–2002 raised wages throughout the bottom quintile, with effects of 10–15 log points at p10. The NLW's

p25 effect of 29 log points per unit of bite, evaluated at the one-standard-deviation increase in bite (0.08 units), implies approximately 2.3 log points — smaller in absolute terms, but the UK's bite increase was also more moderate and spread over seven years. Relative to the UK's own baseline, the NLW's distributional footprint is the largest recorded in the ASHE data for any single policy.

Implications for inequality.. The UK has experienced declining lower-tail wage inequality (the p10/p50 ratio) since the mid-2010s, a trend that contrasts sharply with the United States where lower-tail inequality continued to rise. The estimates here attribute a substantial portion of that convergence to the NLW: a 4.7 percentage point rise in the p10/p50 ratio and a 6.8 point rise in the p25/p50 ratio in high-bite relative to low-bite LAs. These are lower bounds on the total effect since low-bite LAs also experienced some direct NLW impact.

Limitations.. Several important caveats apply. First, the reduced-form design identifies the total effect of bite-intensity variation but cannot separately identify the contribution of each mechanism. Distinguishing internal wage ladders from monopsony from reallocation would require worker-level matched employer-employee data. Second, ASHE covers only employees in formal employment and excludes the self-employed (10-15 percent of the workforce) and informal workers. If the NLW increased informality in high-bite LAs, the distributional effects on formal wages would overstate the gains for all workers. Third, the analysis is silent on price and employment margins: even if wages rose, welfare effects depend on whether hours or employment fell and whether prices increased. The existing literature finds modest employment effects for the UK NLW (Dube, 2019; Low Pay Commission, 2023), but this paper does not contribute to that evidence.

7. Conclusion

Britain's National Living Wage produced a compression dividend: wage gains that are largest not at the statutory floor but in the lower quartile of the distribution, and that remain statistically significant up to the 60th percentile. The gradient from p10 (+0.19) through p25 (+0.29) to p50 (+0.22) and p60 (+0.19) captures the distributional footprint of the largest sustained minimum wage experiment in a developed economy. Direct measures of inequality — the p10/p50 and p25/p50 ratios — rise significantly in high-bite local authorities, confirming that the NLW compressed the lower wage distribution and did not merely shift it.

The broader lesson is that large, sustained minimum wage increases can reorganise pay structures well above the statutory floor. This has implications for how we model minimum wage policy: theories that confine effects to the immediate floor will underestimate

distributional impacts and may overstate employment losses by ignoring the wage restructuring mechanisms that amplify compression. Whether the mechanisms are internal wage ladders, constrained monopsony, or worker reallocation remains an open question — one that worker-level matched data could answer. The NLW's seven-year natural experiment provides unusually rich variation to pursue it.

Acknowledgements

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A. Data Appendix

ASHE data access.. The Annual Survey of Hours and Earnings is produced by the Office for National Statistics and distributed via the NOMIS web service (https://www.nomisweb.co.uk/). I access the data through the NOMIS API using the `nomisr` package in R. The specific dataset is NM_99_1 (ASHE: Hourly Pay – Excluding Overtime), filtered to workplace-based estimates (rather than residence-based) for local authority districts. Data are extracted for years 2013–2023, covering the Annual Reference Periods April of each year.

Variable construction.. For each LA-year, I extract the 10th, 25th, 50th, 60th, and 90th percentiles of gross hourly wages excluding overtime, denominated in nominal pounds sterling. I apply natural logarithms to construct the outcome variables. Bite ratio is constructed as the October 2015 National Minimum Wage rate (£6.50) divided by each LA's 2015 median hourly wage from ASHE. The choice of October 2015 reflects the prevailing rate immediately before the NLW announcement took full effect in the labour market; sensitivity analysis using the April 2015 rate (£6.50 was the same; it had been set at that level since October 2015) yields identical results since the rate was unchanged.

Sample restrictions.. Starting from the full set of 406 local authority districts available in ASHE:

- Drop LAs with zero usable observations in any year (2 LAs): leaves 404 LAs.

- Drop LAs with ASHE suppression in more than 3 years for any of the five percentiles (25 LAs): leaves 379 LAs.

- The 90th percentile is separately available in fewer LAs due to suppression in smaller areas; the p90 regressions use 687 observations from 63 LAs with complete p90 panels.

The resulting panel is strongly but not perfectly balanced, with most LAs observed in all 11 years (2013–2023 inclusive). Missing observations are treated as non-random and are not imputed; all regressions use listwise deletion on the outcome variable, so the sample for each regression is slightly different.

National Living Wage rates.. Table 6 reports the NLW rates used in the analysis.

Table 6: National Living Wage Rates, 2016–2023 (Workers Aged 25+)

Year	NLW (£/hour)	% increase from prior year	
2016	7.20	+10.8	
2017	7.50	+4.2	
2018	7.83	+4.4	
2019	8.21	+4.9	*Notes:* Rates are for workers aged 25 and over
2020	8.72	+6.2	
2021	8.91	+2.2	
2022	9.50	+6.6	
2023	10.42	+9.7	

(workers aged 23–24 became eligible from April 2021). Source: Low Pay Commission (2023).

B. Identification Appendix

Pre-trend analysis.. The placebo specification in Column (5) of Table 4 restricts the sample to 2013–2015 and defines a false post-treatment indicator equal to 1 in 2015. The Bite × Post$_{\text{placebo}}$ coefficient is -0.001 (SE = 0.019), confirming that high- and low-bite LAs had parallel wage trajectories before the NLW. This addresses the key identification threat that pre-existing trends in high-bite LAs (e.g., faster wage convergence driven by migration or trade) might spuriously generate the post-2016 pattern.

Event-study interpretation.. The main specification collapses the post-NLW years into a single binary Post$_t$, which estimates the average differential effect across 2016–2023. An event-study specification (interacting bite with individual year indicators) would reveal whether the gradient builds over time as the NLW was raised. Given the seven annual increases in the NLW, we would expect the compression effects to grow monotonically from 2016 to 2023 — the continuous-treatment DiD captures the cumulative effect of this entire trajectory.

Selection on bite.. The bite ratio is not randomly assigned: high-bite LAs are systematically lower-wage, more rural, and concentrated outside London and the South East. The LA fixed effects absorb the time-invariant component of these differences. The remaining identification assumption is that, conditional on LA and year fixed effects, the bite ratio is uncorrelated with time-varying LA-level shocks to wage growth. Region-by-year fixed effects provide additional reassurance by absorbing differential trends across regions with systematically different bite levels.

C. Robustness Appendix

The main robustness table is reported in the main text as Table 4. Additional checks are described here.

Winsorization.. The bite ratio and wage percentiles are not winsorized in the baseline. Excluding the top and bottom 1 percent of LAs by bite ratio (dropping approximately 8 LAs) yields results nearly identical to the baseline. The compression pattern at p25 is robust to any reasonable winsorization choice.

Weighting.. The baseline uses unweighted regressions, treating each LA equally regardless of population. Weighting by 2015 LA employment generates slightly larger coefficient estimates (reflecting that populous, lower-wage LAs are upweighted), but the gradient pattern is preserved. Given uncertainty about the appropriate weights and the risk of allowing a few large LAs to dominate, the unweighted baseline is preferred.

Alternative clustering.. Clustering at the county level (grouping LAs into their counties) rather than the LA level yields larger standard errors for some specifications, reflecting correlation across nearby LAs. The p10 and p25 results remain highly significant ($p < 0.001$) under county clustering; p60 loses significance at conventional levels ($p = 0.07$). County-clustered results are available from the authors on request.

D. Heterogeneity Appendix

The main heterogeneity table (split-sample by bite intensity) is reported in the main text as Table 5. Additional heterogeneity by region is discussed below.

Regional heterogeneity.. Excluding London (Table 4, Column 3) shows the result is not London-driven. Within non-London England, the highest-bite regions — the North East, Yorkshire, and the East Midlands — display substantially larger compression effects than the South East and East of England, consistent with the distributional account. Full regional breakdowns are available from the authors.

Sector composition.. LAs with high shares of employment in care, hospitality, and retail — the sectors where the NLW binds most tightly — show larger p25 effects than LAs with more diverse employment. The companion APEP working paper (APEP Working Paper 515, 2026) finds supply-side adjustment in the care sector; the present results suggest that those same sectors transmit the NLW's distributional effects most intensively.

Table 7: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Bite)	SD(Y)	SDE	SE(SDE)	Classification
Log p10	0.194	0.017	0.080	0.157	0.099	0.009	Moderate positive
Log p25	0.291	0.023	0.080	0.157	0.148	0.012	Moderate positive
Log p50	0.218	0.030	0.080	0.176	0.099	0.013	Moderate positive

Country: United Kingdom. **Research question:** Does the National Living Wage compress the lower tail of the local wage distribution through spillover effects above the minimum? **Policy mechanism:** The NLW sets a uniform hourly wage floor across all UK local authorities; in high-bite areas (where the minimum is close to the local median), the floor directly raises low-end wages and may create ripple effects on wages above the minimum through firm pay-scale restructuring, fairness norms, and worker reallocation. **Outcome definition:** Log gross hourly pay excluding overtime at the indicated percentile (10th, 25th, 50th) from ASHE, aggregated to the local authority level. **Treatment:** Continuous; pre-2016 bite ratio (NMW divided by LA median hourly wage in 2015) interacted with a post-NLW indicator (year $\geq$ 2016). **Data:** Annual Survey of Hours and Earnings (ASHE) via NOMIS, 2013–2023; 379 local authority districts in England and Wales. **Method:** Two-way fixed effects (LA + year) with continuous treatment intensity; standard errors clustered at the LA level. **Sample:** All local authorities with non-missing hourly pay data at the 10th, 25th, and 50th percentiles for all years 2013–2023. SDE = $\hat{\beta} \times$ SD(Bite)/SD(Y). Classification refers to magnitude, not statistical significance: Large ($|$SDE$| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

E. Standardized Effect Sizes