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Analyses of Topical Policy Issues

To be (worried) or not to be? The impact of minimum wage increases on aggregate prices

Fikret Bilenkisi [a], Filippos Maraziotis [b,*], M. Akif Yardimci [c]

[a] *University of Huddersfield, United Kingdom*
[b] *IQS School of Management, Universitat Ramon Llull, Spain*
[c] *University of York, United Kingdom*

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ABSTRACT

This paper estimates the effect of statutory minimum wage increases on aggregate consumer prices across 29 OECD countries during the synchronised inflationary cycle of 2021–2024. Exploiting staggered minimum wage reviews under a rare quasi-experimental environment of common global shocks, we implement a novel difference-in-differences estimator that accommodates the cumulative nature of wage floors and evolving treatment intensity. A 10 percent increase in the minimum wage raises aggregate prices by 0.3 percent over five months, with effects concentrated in food prices. Our estimates fall within the range of prior micro- and sectoral studies, but extend the literature by recovering the full temporal pass-through path. Our design-based approach demonstrates that credible inference is attainable in macro panels without micro-level data. The findings clarify the inflationary footprint of wage policies and offer a replicable framework for policy evaluation in macro-labour contexts.

1. Introduction

What is the effect of minimum wage hikes on aggregate prices? Until now, evidence has come exclusively from single-country or sector-specific studies, leaving open the question of how statutory wage floors shape price dynamics at a macro scale. We answer this by exploiting variation in minimum wage *doses* across 29 OECD economies during the synchronised inflation cycle of 2021–24. Using a newly developed, dynamic, non-absorbing, and *dose–response* difference-in-differences estimator developed by De Chaisemartin and d'Haultfoeuille (2024), we recover the dynamic pass-through from statutory wage increases to aggregate Consumer Price Index (CPI), quantify its concentration in food prices, and document heterogeneity by real minimum wage levels. Our cross-country DID setting delivers the first credible, generalisable dynamic pass-through effect of minimum wage on prices at the macro level.

From a policy perspective, this macro-level design allows us to assess the inflationary footprint of minimum wage adjustments directly in the national accounts—information that micro or sectoral studies cannot provide. Policymakers set statutory wage floors at the aggregate level, so understanding their system-wide price effects requires evidence that integrates cross-country and macro dynamics rather than local experiments alone.

The surge in global inflation was triggered by supply chain disruptions, energy and geopolitical shocks, offering unusual and rare quasi-experimental conditions: all countries experienced common global shocks, inflation surged and then receded in a strikingly synchronised pattern, yet institutional minimum wage reviews continued on staggered, routine schedules. This coincidence creates a quasi-experimental panel which allows the identification of aggregate pass-through under shared global shocks. We treat each

* Correspondence to: Department of Economics and Finance, IQS School of Management, Universitat Ramon Llull, Via Augusta, 390, 08017 Barcelona, Spain.
E-mail addresses: f.bilenkisi@hud.ac.uk (F. Bilenkisi), filippos.maraziotis@iqs.url.edu (F. Maraziotis), makifyardimci@gmail.com (M.A. Yardimci).

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statutory wage adjustment as an accumulating "dose" rather than a one-off policy shock, capturing how successive increases compound in their effects over time. Once implemented, each increase establishes a new wage floor that persists, and future adjustments build upon it. This motivates a dynamic treatment framework that reflects the cumulative policy reality faced by central banks and fiscal authorities. We focus on the CPI, capturing aggregate price movements, and construct a continuous treatment variable scaled to percentage changes in the statutory minimum wage.

Building on this cross-country quasi-experimental setting, we first document the conventional two-way fixed-effects (TWFE) estimators, discussing how these estimates likely conflate treated units, mis-weight comparisons in the presence of heterogeneous treatment effects, and fail to accommodate evolving treatment intensity. Moreover, recent advances in multivalued and continuous-treatment DID (e.g. Callaway et al. (2024)) remain restricted to one-shot or absorbing treatments, limiting their suitability to our setting, where wage policies adjust frequently. We therefore implement the continuous-dose DID framework of De Chaisemartin and d'Haultfoeuille (2024), which is uniquely tailored to accommodate the non-absorbing, dynamic treatment structure in our data. Their estimator allows the outcome to depend on both current and lagged treatment doses, accounts for time-varying heterogeneity in baseline trends through smooth parametric adjustments, and supports staggered adoption across units.

Our paper bridges and extends several strands of the minimum wage and inflation literature. First and foremost, to our knowledge, we are the first to estimate the effect of statutory minimum wage hikes on aggregate price levels in a cross-country framework. Prior work has largely been confined to single sectors or regions—examining, for example, restaurant prices (Ashenfelter and Jurajda, 2022; Allegretto and Reich, 2018; Card and Krueger, 1994), retail markups (Renkin et al., 2022; Leung, 2021; Ganapati and Weaver, 2017), or local CPI series in the United States (Aaronson and French, 2007; Aaronson et al., 2008; Cooper et al., 2020; MacDonald and Nilsson, 2016), in Hungary (Harasztosi and Lindner, 2019), in Brazil (Lemos, 2006), in Germany (Link, 2024), and in Mexico (Campos-Vazquez and Esquivel, 2020). A smaller body of earlier research considers the national transmission of minimum wage increases, using aggregate price or Phillips-curve specifications in single-country settings (Glover, 2019; Gramlich et al., 1976; Frye and Gordon, 1981; Falconer, 1979), but remains vulnerable to confounding and limited generalisability.

Substantively, our results show that a 10 percent minimum wage hike raises aggregate CPI by 0.027 percent in the first month, with the full pass-through unfolding over an average of 4.36 months and peaking at 0.3 percent in month five. This overall effect is driven mainly by food-price responses, as net-of-food CPI remains less responsive to wage adjustments. Placebo tests confirm that our identifying assumptions, *no anticipation* and *parallel trends*, hold firmly across specifications. The findings survive multiple robustness checks, including alternative functional forms, sub-sample restrictions, and bootstrapped inference procedures.

Our estimated cross-country elasticities fall squarely within the range of previous sectoral and country-specific findings (0.15–0.6 percent) (Ashenfelter and Jurajda, 2022; Lemos, 2006; Cooper et al., 2020; Card and Krueger, 1994), and the price responses documented in earlier studies now manifest at the macro level in various institutional settings, underscoring both the breadth and the limits of inflationary effects of minimum wages. Unlike earlier estimates, which typically rely on static comparisons and recover a single average pass-through, our dynamic framework traces the full time path of price responses, showing that inflationary effects accumulate gradually over several months. This temporal structure, absent in most prior work, sharpens the economic interpretation of minimum wage elasticity and reveals its modest, delayed transmission. The convergence with micro-level evidence lends external credibility to our identification strategy, while our macro-level, design-based approach demonstrates that credible inference is attainable even without granular data, provided the estimator matches the structure of the policy variation.

The remainder of the paper is organised as follows. Section 2 describes the institutional background and our quasi-experimental research design. Section 3 presents a benchmark TWFE analysis and highlights its limitations. Section 4 outlines our dynamic, non-absorbing, continuous-dose DID methodology. Section 5 presents our results and explores heterogeneity in price responses by real minimum wage and food CPI responses and Section 6 concludes.

2. Institutional background and research design

2.1. Institutional background

To assess the impact of minimum wage increases on aggregate prices, we use monthly data from the OECD, spanning from January 2021 to May 2024, encompassing 29 out of the 38 OECD countries. The countries included in this study are those with institutionally established national statutory minimum wages: Australia, Belgium, Canada, Chile, Colombia, Costa Rica, Czechia, Estonia, France, Germany, Greece, Hungary, Ireland, Israel, Japan, Korea, Latvia, Lithuania, Luxembourg, Mexico, Netherlands, New Zealand, Poland, Portugal, Slovak Republic, Slovenia, Spain, Turkey, United Kingdom, and the United States.

We draw on OECD data for both the Consumer Price Index (CPI) and nominal minimum wage rates. For the analysis, we use the weighted data for Canada and the United States constructed by OECD (2024), to reflect the aggregate evolution of minimum wage rates based on sub-national levels. These weighted estimates, however, do not account for special exemptions and specific rates in different provinces and states. For Canada, the weighted minimum wage is based on data from the Survey of Employment, Payrolls and Hours (SEPH) in 2019, while for the United States, it is based on the State and Metro Area Employment, Hours, and Earnings published by the BLS in 2019. For the five US states without a required minimum wage (Alabama, Louisiana, Mississippi, South Carolina, and Tennessee), the federal minimum wage was included in the estimation.

We exclude Turkey as an outlier and omit eight OECD countries without a national statutory minimum wage (Austria, Denmark, Finland, Iceland, Italy, Norway, Sweden, and Switzerland).[1]

[1] In these countries, collective agreements set de facto wage floors, and in Switzerland five cantons have introduced local minimum wages; canton-level monthly data are unavailable.

Between January 2021 and May 2024, all sampled countries enacted one or more minimum wage increases—often on annual or biannual timetables, and in some cases via automatic indexation linked to wages or prices (OECD, 2023).

2.2. Research design

At the beginning of 2021, OECD economies experienced an unusually synchronised surge in inflation, driven by global supply-chain disruptions after COVID-19, an unprecedented energy shock, and the geopolitical fallout from the Ukraine conflict. While these forces generated a common upward pressure on prices, each country's inflation trajectory retained distinct characteristics. Statutory minimum-wage adjustments are typically implemented through routine, institutionalised review processes, often on fixed annual or biannual timetables. Table A.1 documents that, over 2019–2024, positive changes in the OECD monthly minimum-wage series concentrate in a small set of calendar months (predominantly January, April, July, and October), consistent with scheduled implementation rather than high-frequency retiming.[2] Conditional on absorbing common global shocks via month fixed effects, this institutional structure provides plausibly exogenous variation in the timing of countries' wage *doses*.

A separate concern is that, even if implementation timing is largely scheduled following a formal process rather than an ad-hoc decision, the *magnitude* of increases could be responsive to inflation in a persistent inflation environment. To address this concern, we first regress the minimum wage on contemporaneous and lagged CPI. As shown in Table A.2, all coefficients are highly statistically insignificant except at lag four, which likely reflects institutional implementation lags rather than systematic endogeneity. This suggests that minimum wage adjustments are not mechanically or predictably tied to recent inflation.[3,4] We additionally supplement the predictive regressions in Table A.2 with a direct within-country diagnostic focusing on event months. Specifically, we residualise minimum-wage increments and inflation (measured using alternative short-horizon averages) with respect to country and month fixed effects, and examine whether the residual magnitude of minimum-wage changes co-varies with recent inflation deviations. Fig. A.1 shows no systematic relationship: the fitted associations are close to flat around the support where most observations lie.[5]

Together, the institutional evidence on typically scheduled implementation and the absence of a clear within-country link between recent inflation deviations and the size of increases strengthen the quasi-experimental interpretation underlying our dynamic DID design, while we continue to acknowledge that no aggregate design can fully rule out all forms of contemporaneous policy endogeneity.

Our analysis focuses on January 2021 through May 2024, a period flanked by phases of relative price stability. January 2021 marks the end of the pre-shock lull, and by May 2024 headline inflation had largely normalised. Within this window, we treat each percentage change in the statutory minimum wage as a non-absorbing cumulative dose rather than a binary treatment, allowing us to estimate the dose–response of wage increase on aggregate prices.

To address legacy effects from prior wage adjustments, a dimension often overlooked in standard policy evaluations, we introduce a six-month pre-analysis period, which we name "pre-History" window, spanning July to December 2020. Unlike conventional designs that treat the policy onset as a clean break, our pre-History window is long enough to capture adjustments related to change in aggregate prices yet short enough to minimise contamination from early-pandemic fiscal and monetary interventions. Crucially, statutory minimum wage adjustments are routine in most countries; this is not a one-off policy shock but a familiar pulse of wage reviews. Firms and households are therefore conditioned to absorb these changes.[6]

Fig. 1 plots the CPI trajectories across the OECD countries, confirming that inflation was effectively flat during our pre-History window. By explicitly anchoring both pre-History and the subsequent main "History" period, we ensure that every post-January 2021 wage hike is compared to a clean counterfactual path, avoiding left-censoring bias and ensuring that any residual impact from prior wage changes is correctly attributed.

We re-index both series to a unified July 2020 origin, setting the minimum wage index to zero and the CPI to 100. This harmonisation means that a one percent statutory wage increase translates directly into a comparable movement on the CPI scale. Our minimum wage index is built in such a way that a one-unit change corresponds to a one-percent change in the minimum wage. Therefore, using the logarithm of CPI, we are able to retrieve the elasticity: the percent change in CPI from a 1 percent change in the statutory minimum wage. Anchoring both series at the same date also eliminates the reference-period mismatches that afflict standard year-over-year or month-over-month CPI inflation, and ensures that any non-zero wage legacy entering January 2021 is correctly carried into our continuous-dose comparisons.

[2] Belgium and France exhibit multiple within-year changes, reflecting automatic indexation mechanisms that can trigger more than one adjustment per year beyond the routine annual revision. Chile also shows multiple changes without a stable calendar pattern in the OECD series, despite a formal review process. We assess the robustness of our results by re-estimating our baseline specifications excluding these countries.

[3] In addition, the Results section presents placebo estimates across a range of specifications. These consistently fail to detect any pre-implementation relationship between minimum wage and prices.

[4] The significant correlation between the minimum wage index and the fourth lag of CPI reflects institutional adjustment lags and does not raise endogeneity concerns. In general, correlation between a treatment and lagged values of the outcome does not pose a problem for causal interpretation, unless those lagged outcomes also directly affect the current outcome outside the treatment channel. In our context, the absence of correlation with more recent CPI lags further supports the view that minimum wage increases are not systematically driven by inflation trends.

[5] This diagnostic is intentionally conditional on minimum-wage increase months; it addresses whether larger increases systematically follow higher recent inflation deviations. As in the main text, we also report placebo estimates within the dynamic DID framework, which consistently fail to detect pre-implementation price movements.

[6] See Appendix A for Fig. A.2 which presents a nine-month distributed-lag model of minimum wage changes (June 2019–Jan 2021), confirming that nearly all pass-through effects materialise within the first six months and thus justifying our pre-History period choice, consistent with both low, stable inflation and standard price-setting models.

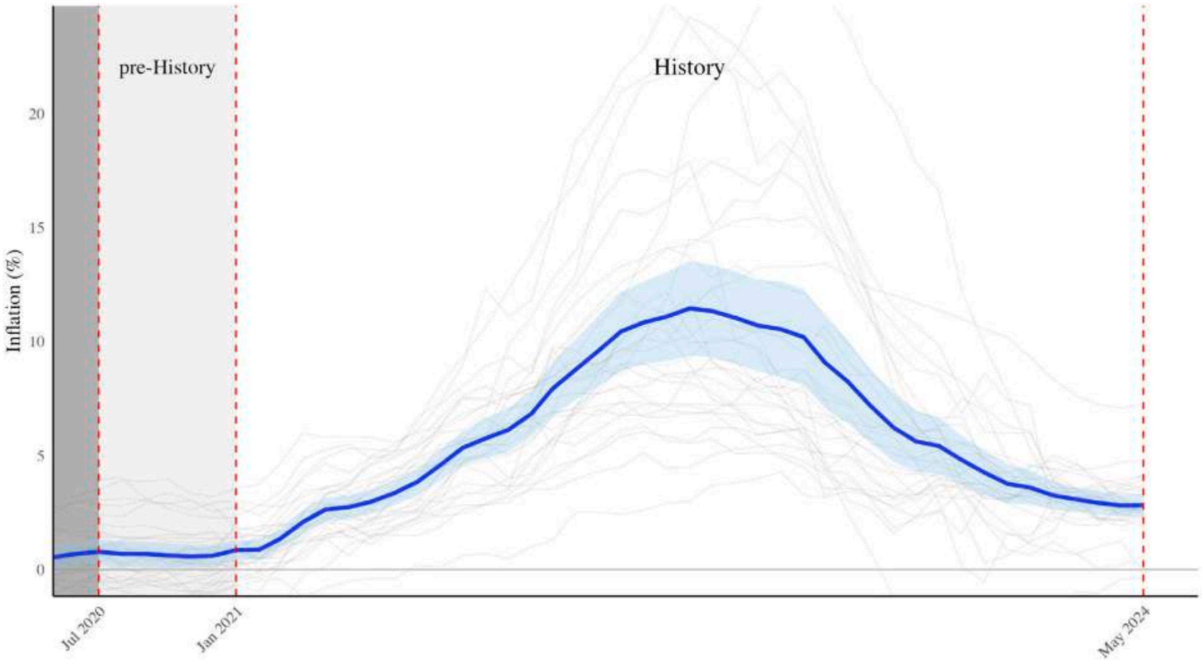

Fig. 1. The evolution of inflation.
Notes: The solid blue line traces the average monthly inflation rate across all 29 OECD countries. The light-blue shaded band represents its 95 percent confidence interval. The faint grey lines depict each country's individual inflation trajectory.

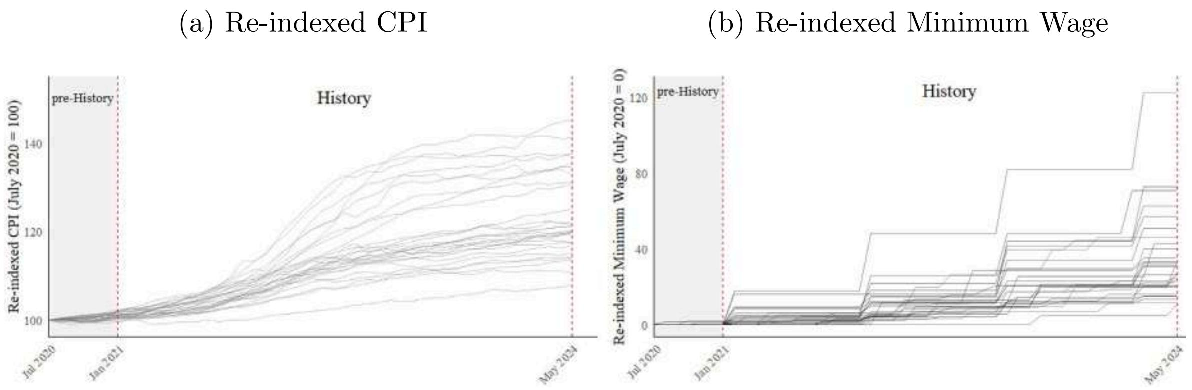

Fig. 2. Re-indexation.

Fig. 2(a) plots the re-indexed CPI trajectories from July 2020 to May 2024. The nearly flat lines during the six-month pre-History window (July–Dec 2020) confirm our choice of a stable baseline, before the synchronised, steeper inflation climb in the History phase. By anchoring all series at 100 in July 2020, we get a clear, apples-to-apples comparison of price movement across countries once global shocks hit in early 2021.

Fig. 2(b) shows each country's re-indexed minimum wage path over the same period. A small number of countries adjusted wages during the pre-History window and therefore have non-zero values at the start of the History window (Australia, Canada, Chile, Japan, Netherlands, and the US). The scheduled nature of wage reviews shows up in the graph as step-wise increases at regular intervals, and we exploit the resulting variation in the timing and magnitude of these doses to identify their impact on the CPI.

Fig. 3 orders the 29 OECD countries by their cumulative CPI growth over January 2021–May 2024 and plots each country's cumulative minimum wage increase alongside. The resulting chart highlights two key facts. First, there is substantial cross-country heterogeneity in both inflation and wage adjustments: some countries registered only modest price rises despite sizable wage hikes, while others saw large inflation with smaller statutory increases. Second, the ranking by inflation does not coincide with the ranking by wage dosage—there is no perfect monotonic relationship. This decoupling confirms that variation in minimum wage increases is

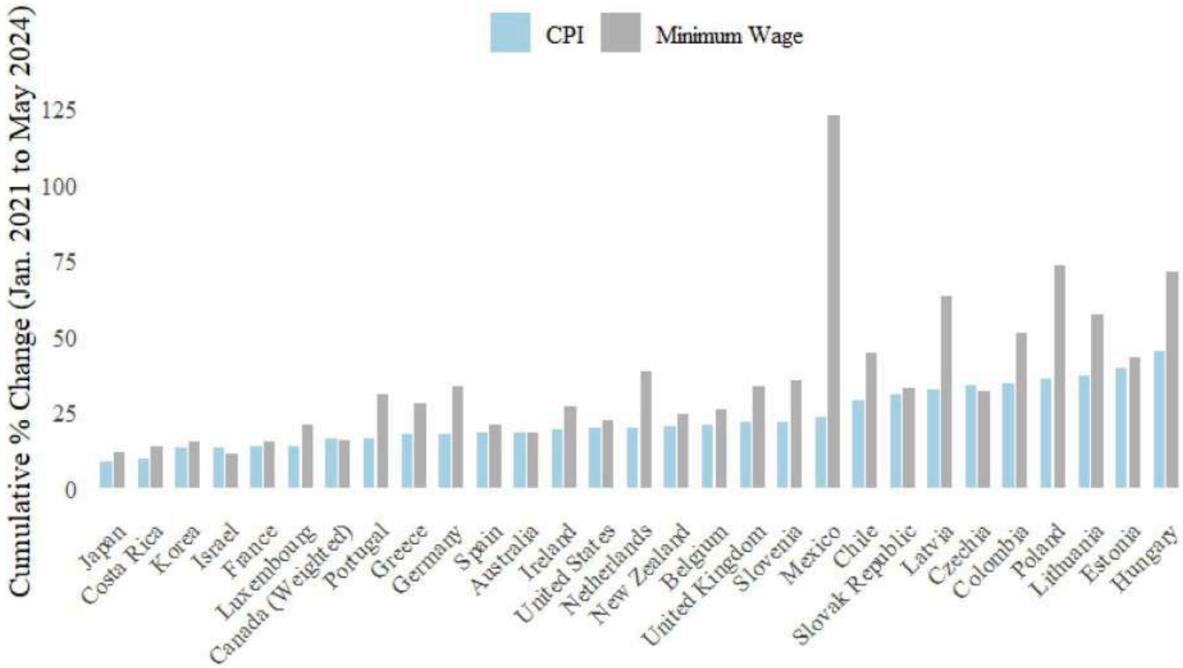

Fig. 3. CPI & minimum wage cumulative changes by country, ascending CPI.

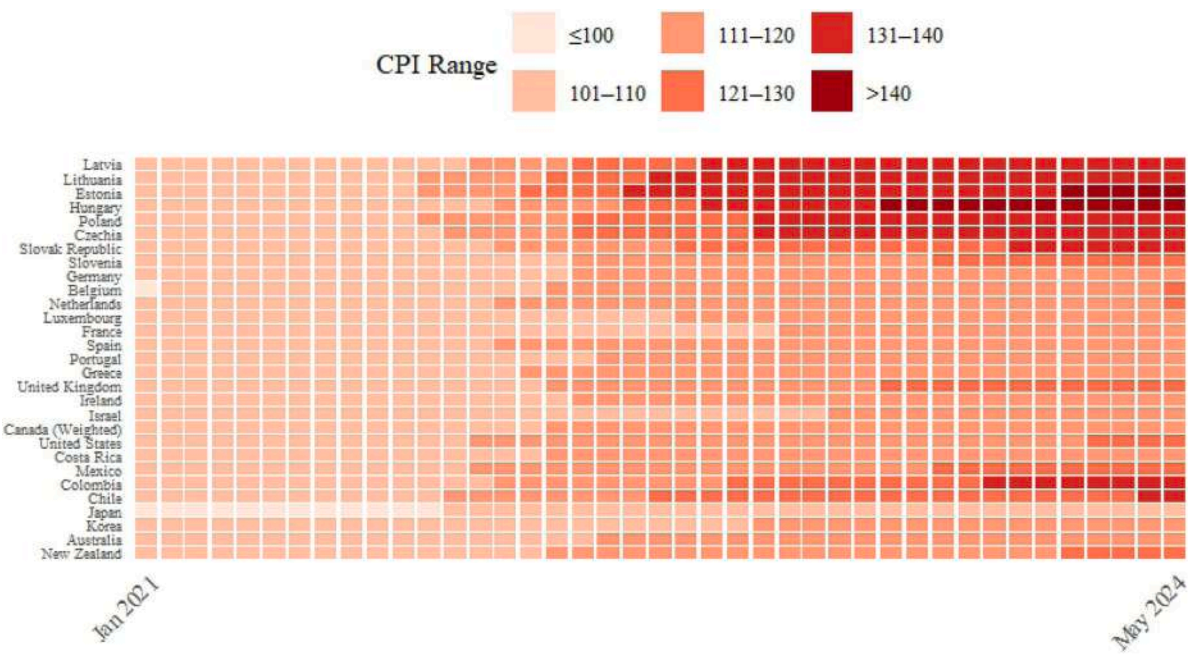

Fig. 4. Monthly CPI heatmap by country.

not mechanically tied to cumulative CPI growth and underscores our ability to exploit richly dispersed policy doses as an independent source of identifying variation.

Fig. 4's heat-map makes two things immediately clear. First, despite a common global shock in early 2021, inflation dynamics show some regional clustering: the Baltic countries (Latvia, Lithuania, Estonia) followed by Central and Eastern Europe (Hungary, Poland, Czechia, Slovakia, Slovenia) move in lock-step, especially from 2022 onwards, possibly due to the consequences of the Ukraine war. Meanwhile, Western Europe (Germany, Belgium, Netherlands, Luxembourg, France), Southern Europe (Spain, Portugal,

Table 1
TWFE estimations.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Min.Wage	0.0019 (0.0009)	0.0019 (0.0008)	0.0017 (0.0009)	0.0018 (0.0008)	0.0019 (0.0009)	0.0019 (0.0008)	0.0018 (0.0007)
Unemp.	–	–	0.0078 (0.0034)	–	–	–	0.0039 (0.0034)
Int.Rate	–	–	–	0.013 (0.0044)	–	–	0.0081 (0.0041)
B/meter	–	–	–	–	0.0024 (0.0018)	–	0.0016 (0.0015)
Prox.War	–	–	–	–	–	0.069 (0.027)	0.0521 (0.0227)
SEs adj.	Clustered	Conley	Clustered	Clustered	Clustered	Clustered	Clustered
g, t FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.20	0.20	0.24	0.28	0.20	0.30	0.35
N	1189	1189	1189	1189	1189	1189	1189

Notes: In parentheses are shown standard errors clustered at the country level in Columns (1), (3)–(7) and Conley-(900 km) corrected in Column (2). Columns (1)–(2) present the baseline specification. Columns (3)–(6) each add, in turn, controls for the unemployment rate (3), the interest rate (4), the consumer barometer (5), and a proximity-to-war indicator (6). Column (7) includes all controls simultaneously.

Greece), the British Isles (UK, Ireland), Israel, North America (Canada, United States), Latin America (Costa Rica, Mexico, Colombia, Chile), East Asia (Japan, Korea), and Oceania (Australia, New Zealand) display co-movements as well, though generally less pronounced.

Second, the apparent regional clustering raises the possibility that, beyond global supply-chain and energy shocks, local economic linkages and shared monetary–fiscal environments may produce similar inflation patterns among neighbouring countries. If these spatial spillovers are present, they could complicate our quasi-experimental comparisons by inducing cross-country dependence in price movements. We therefore flag this as a hypothesis to be tested in the following section, where we introduce a standard TWFE specification, before proceeding with our dose–response DID estimation.

3. Two-way fixed effects estimation

We begin with a standard TWFE model as a transparent benchmark. Regressing monthly log-CPI on the minimum wage index, with country and time fixed effects. We use a balanced panel of $G = 29$ OECD countries observed monthly over $T = 41$ periods, from January 2021 ($t = 1$) through May 2024 ($t = T$). Our outcome of interest is the Consumer Price Index (CPI) for each country-month, re-indexed so that July 2020 = 100 in all series. Our treatment variable is the minimum wage index, re-indexed so that July 2020 = 0 in all series, and built in such a way that a one-unit change corresponds to a one-percent change in the minimum wage.

We estimate the following specification:

$$\mathrm{Y}_{g,t} = \alpha + \delta^{TWFE} \cdot D_{g,t} + \gamma_g + \tau_t + \epsilon_{gt} \tag{1}$$

where $Y_{g,t}$ and $D_{g,t}$ are the CPI and minimum wage indexes of country g in month t; γ_g is country fixed effects capturing institutional heterogeneity and different configuration of the economies; and τ_t is the month fixed effects for the common global shocks, global economic cycles, and common disruptive effects.[7]

Table 1 presents our benchmark TWFE estimates of the elasticity of CPI with respect to the statutory minimum-wage index. All seven specifications regress the monthly log-CPI on the minimum wage index, controlling for country and month fixed effects.

Column (1) shows the baseline estimate of Eq. (1), which indicates that a one percent increase in the minimum wage raises CPI by 0.0019 percent with standard errors clustered at the country level. Column (2) repeats the baseline but uses Conley-corrected (900 km) standard errors. The negligible difference in standard errors between specifications (1) and (2) suggests that spatial correlation is not a serious concern.[8]

To assess the sensitivity of our results to additional covariates, Columns (3)–(7) sequentially introduce controls[9] and verify the robustness of the wage-price relationship. Column (3) adds the unemployment rate, (4) adds the interest rate, (5) adds the consumer's barometer index, (6) adds our proximity-to-war indicator, and (7) includes all controls together.

Across every specification, the minimum wage coefficient remains positive, of roughly the same magnitude (0.0017–0.0019), and statistically significant. The within-R^2 rises from 0.20 in the simplest model to 0.35 when all controls are included, confirming that these covariates capture additional country-month variation but do not alter the core wage-price link. With $N = 1189$ observations

[7] The proximity-to-war indicator explicitly accounts for cross-country variation in the severity of the Ukraine-related shock.

[8] See Appendix A for Table A.3 which reports Moran's I tests across several model specifications; none detect significant spatial correlation.

[9] See Appendix B for Figs. B.1, B.2, B.3, and Table B.1 which present descriptives of all control variables.

and consistent fixed-effects across panels, this table establishes a TWFE benchmark which suggests significant but negligible price pass-through.

Recent work has documented serious limitations of TWFE regressions in the presence of treatment-effect heterogeneity or staggered adoption. The TWFE estimands can place negative weights on certain group–time cells, may admit different causal interpretations, or can fail to deliver a clear summary parameter under treatment heterogeneity (Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021; Borusyak et al., 2024; Baker et al., 2025). In our non-absorbing minimum wage dose–response framework — drawing on De Chaisemartin and d'Haultfoeuille (2024) — standard TWFE estimators run into several pitfalls. Because doses accumulate rather than reset, TWFE can assign negative weights to some comparisons, placing undue emphasis on these "bad" comparisons and even reversing the sign of the overall estimate. If the pass-through of a one-point wage increase varies across countries or over time — as we expect here — TWFE no longer recovers any single, well-defined average of those heterogeneous effects. And even when all weights happen to be non-negative, the TWFE coefficient can still conflate early versus late dose responses into a misleading summary. Finally, under dynamic treatment effects, TWFE mis-attributes spillovers across time lags: by pooling all post-treatment periods into one regressor and ignoring each country's timing and dose history, its estimate of the "current" wage change actually mixes together immediate impacts with effects from earlier and later increases.

A rapidly expanding literature has therefore developed alternatives to classical TWFE.[10] Our empirical context requires an estimator that can accommodate dynamic, non-binary, and non-absorbing treatment. Statutory minimum wage increases occur at multiple points in time, vary in intensity across countries, and accumulate rather than reset—making treatment both staggered and continuously evolving. While Callaway et al. (2024) introduce a flexible framework for multi-valued treatments, their design defines treatment groups by the timing of the first departure from baseline and permits the treatment level to evolve thereafter. However, it does not recover separate causal parameters for each incremental change in treatment intensity. In contrast, the estimator developed by De Chaisemartin and d'Haultfoeuille (2024) explicitly identifies the effect of each successive dose, accommodating repeated, dose-varying, and potentially non-monotonic treatment paths, and allowing the outcome to depend on both current and lagged doses. Their framework supports fully dynamic treatment regimes with staggered adoption, non-absorbing exposure, and intertemporal treatment effect heterogeneity—precisely the structure that characterises our minimum wage–price setting. Accordingly, we implement their continuous-dose DID estimator to recover the dynamic pass-through of minimum wage changes on aggregate prices.[11]

4. Methodology

In this section, we outline the DID framework of De Chaisemartin and d'Haultfoeuille (2024), adapted to our context. To ensure clarity, facilitate direct comparison, or replicability in similar macro-policy settings, we adopt the notation and terminology of De Chaisemartin and d'Haultfoeuille (2024) throughout.

The treatment dose of interest is the statutory minimum wage index, anchored at a July 2020 baseline. Each subsequent minimum wage increase raises the index cumulatively by its size in percentage points. Formally, let

$$D_{g,t} \;=\; \text{minimum wage index of country } g \text{ in month } t,$$

with January 2021 as $t = 1$ and $D_{g,1} \in [0, 1.9]$, so that the majority of countries enter the sample at $D_{g,1} = 0$, while six countries enter that period with a non-zero treatment dose. We write each country's full wage-history vector as $D_g = (D_{g,1}, \dots, D_{g,T})$ and collect all countries' wage-histories in $D = (D_1, \dots, D_G)$. By construction $D_{g,t} \geq 0$ for all g, t and increases only in discrete jumps when a new minimum wage adjustment occurs.

Next, we introduce the potential outcomes framework. Let $Y_{g,t}(d_1, \dots, d_T)$ be the CPI index of country g at month t under hypothetical wage path $(d_1, \dots, d_T)$. We observe $Y_{g,t} = Y_{g,t}(D_{g,1}, \dots, D_{g,T})$. This dynamic-potential-outcome notation permits today's price level to depend on both past and future wage adjustments.

Some countries enacted statutory wage hikes before January 2021, which could have lingering effects beyond our sample start. To render our estimands identifiable, we impose a six-month finite-lag assumption.

Assumption 1 (*No Effects Beyond Six Lags*)**.** For any country g and month t, the potential outcome $Y_{g,t}(d_1, \dots, d_t)$ depends only on the most recent six treatments:

$$Y_{g,t}(d_1, \dots, d_t) = Y_{g,t}(d_{t-5}, \dots, d_t).$$

[10] See Roth et al. (2023), De Chaisemartin and d'Haultfoeuille (2023), Callaway (2023) and Baker et al. (2025) for surveys of modern difference-in-differences methods.

[11] "Dynamic" means the policy's impact can evolve over time rather than occur in one step. "Non-binary" indicates the treatment is not a simple on/off change but varies in size (for example, a 2% versus 10% increase). "Non-absorbing" means units can receive new treatments repeatedly, with each increase adding to previous ones instead of replacing them. "Dose–response" captures how different magnitudes of treatment translate into proportional outcome changes. "Intertemporal treatment effects" refer to how current outcomes depend not only on the latest policy change but also on the accumulated sequence of past changes. "Non-monotonic treatment paths" allow the intensity of treatment to rise or fall over time rather than move in one direction only. These technical terms are retained for consistency with the modern DID literature but are clarified here for readers less familiar with that framework.

Under this condition, wage changes older than six months exert no additional influence on the CPI at t. Therefore, all identification can proceed on the *History* window $t \geq 7$ (January 2021 onward), effectively purging unobserved pre-2021 shocks. The rationale of Assumption 1 has been discussed in detail in Section 2.2, and Fig. A.2.

This panel configuration — an aggregate country–month structure, a non-negative continuous treatment measured in percentage points, a full dynamic potential-outcome framework, and a six-month finite-lag restriction — provides the foundation for our continuous-treatment DID analysis.

4.1. Identification

We now state the conditions required for our DID estimator to recover the dynamic, per-pp pass-through effects on CPI. For each country g, let

$$F_g = \min\{ t \geq 2 : D_{g,t} \neq D_{g,t-1} \}$$

be the first month in which the minimum wage index $D_{g,t}$ departs from its January 2021 level $D_{g,1}$. F_g marks the *event* date for each country's first statutory wage hike, which anchors both our group-specific DID contrasts and the subsequent event-study structure.[12]

According to De Chaisemartin and d'Haultfoeuille (2024), there must exist at least two countries $g \neq g'$ such that $D_{g,1} = D_{g',1}$, and $F_g \neq F_{g'}$. In our data, among the countries starting January 2021 at $D_{g,1} = 0$, some raise their minimum wage in early 2021 while others delay. Thus, we observe sufficient variation in countries' initial minimum wage increases, essential for unbiased DID comparisons.

The first assumption under which the causal dose-responses of minimum wage hikes on CPI are identified is *No Anticipation*. For every country g, month t, and any hypothetical wage path $(d_1, \dots, d_T)$,

$$Y_{g,t}(d_1, \dots, d_T) = Y_{g,t}(d_1, \dots, d_t).$$

Intuitively, *No Anticipation* implies that firms and price-setters do not react in advance to minimum wage changes scheduled for future months. This rules out any spurious pre-treatment effects in the CPI and mirrors the standard non-anticipation assumption in event-study analyses.

Because statutory minimum wage reviews in OECD countries follow predictable, institutionalised timetables — most commonly on an annual or biannual cycle (OECD, 2023) — firms and households treat them as routine "check-ins" rather than surprise shocks, with exact rates announced only shortly before implementation. Any isolated sectoral front-loading vanishes once wages feed into the hundreds of goods and services in the CPI basket. Thus, aggregate CPI is effectively immune to anticipatory movements ahead of scheduled minimum wage increases. We formally present the test for *No Anticipation* assumption in Appendix C, and the results are shown in Section 5.1. Note that in our context, the *No Anticipation* assumption applies specifically to aggregate prices. Firms may well adjust employment, sourcing, or pricing plans in anticipation of known wage revisions, but our identification only requires that such micro-level expectations do not translate into measurable changes in the aggregate CPI before implementation. Any anticipatory decisions that remain within sectors but do not move the overall price index do not violate this assumption.

The second identifying assumption is *Functional Parallel Trends*. Since January 2021's initial treatment statuses $D_{g,1}$ vary, exact matching on initial dose is infeasible. Instead, there exists, for each month $t \geq 2$, a smooth function $\phi_t : \mathcal{D}_1 \to \mathbb{R}$ such that, *absent any wage change*, the expected one-month CPI growth depends on the country's baseline only through ϕ_t:

$$\mathbb{E}\big[Y_{g,t}(D_{g,1}, \dots, D_{g,1}) - Y_{g,t-1}(D_{g,1}, \dots, D_{g,1}) \mid D\big] = \phi_t\big(D_{g,1}\big).$$

To render ϕ_t estimable, we posit a parametric basis of known functions $\{f_k\}_{k=0}^{K}$ (e.g. powers of $D_{g,1}$) and write

$$\phi_t(d) = \sum_{k=0}^{K} \gamma_{t,k}\, f_k(d).$$

Intuitively, countries that never change their wage would exhibit CPI growth trends that vary smoothly with their January 2021 wage index. Modelling ϕ_t absorbs that continuous heterogeneity, allowing us to isolate the effect of changes in $D_{g,t}$.[13] We formally present the test for *Parallel Trends* assumption in Appendix C, and the results are shown in Section 5.1.

While the institutional timing of minimum wage reviews and the tests reported below lend credibility to our identifying assumptions, the analysis cannot rule out all forms of endogeneity. In particular, wage adjustments that partially reflect governments' responses to inflation expectations could bias estimates towards overstating pass-through. Our country-level data also abstract from compositional shifts or price-setting heterogeneity within sectors. The causal interpretation should therefore be viewed as applying to the aggregate, quasi-exogenous component of statutory wage increases rather than to every policy episode.

[12] If a country never increases its minimum wage over the sample period, F_g is set $F_g = T + 1$. However, this is not the case in our setting, since all countries raise their minimum wage at least once.

[13] We approximate $\phi_t(d)$ by both a linear basis, $\{1, d\}$, and a *quadratic* basis, $\{1, d, d^2\}$. A cubic term would introduce an extra parameter that our limited support of $D_{g,1}$ cannot estimate precisely, lacks clear economic motivation for additional inflection points, and in practice led to overfitting without improving out-of-sample fit. Given the smooth and monotonic evolution of CPI levels across countries (see Fig. 2(a)), we adopt a linear or quadratic specification for $\phi_t(d)$, which suffices to capture counterfactual heterogeneity in baseline price trends.

4.2. Estimation

Under *No Anticipation* and *Functional Parallel Trends*, the *actual-versus-status-quo (AVSQ)* gap for country g at horizon ℓ is defined as:

$$\delta_{g,\ell} = \mathbb{E}\left[\underbrace{Y_{g,F_g-1+\ell} - Y_{g,F_g-1}}_{\substack{\text{Observed } \ell \text{ -month} \\ \text{CPI change}}} - \sum_{t=F_g}^{F_g-1+\ell} \phi_t(D_{g,1}) \,\middle|\, D\right], \tag{2}$$

where its *status-quo trend* over ℓ months is $\sum_{t=F_g}^{F_g-1+\ell} \phi_t(D_{g,1})$, and $\phi_t(\cdot)$ is the baseline-trend function from *Functional Parallel Trends* assumption. Because the treatment variable $D_{g,t}$ is defined as the cumulative percentage change in the statutory minimum wage relative to its July 2020 baseline, a one–unit change in $D_{g,t}$ corresponds to a one–percentage–point increase in the minimum wage. Accordingly, the estimand $\delta_{g,\ell}$ measures the expected change in the CPI associated with a one–percentage–point rise in the minimum wage, and the aggregate coefficient δ_ℓ averages these per–unit effects across countries. No further scaling or normalisation is required.

Then we estimate $\delta_{g,\ell}$ by replacing ϕ_t with its fitted polynomial $\hat{\phi}_t$. Specifically,

$$\widehat{\mathrm{DID}}_{g,\ell} = \left(Y_{g,F_g-1+\ell} - Y_{g,F_g-1}\right) - \sum_{t=F_g}^{F_g-1+\ell} \hat{\phi}_t(D_{g,1}), \tag{3}$$

where $\hat{\phi}_t(D_{g,1})$ denotes the estimated functional–polynomial specification of ϕ_t. Hence, by construction, $\widehat{\mathrm{DID}}_{g,\ell}$ is conditionally unbiased for $\delta_{g,\ell}$.

Finally, to obtain the aggregate DID estimator, first we define the maximum estimable horizon as

$$L = \max_g \left(T_g - F_g + 1\right), \quad N_\ell = \left|\left\{g : F_g - 1 + \ell \le T_g\right\}\right|.$$

Because $D_{g,t} \ge D_{g,1}$ for all t by design, set $S_g = +1$ for every g. The aggregate coefficient is

$$\delta_\ell = \frac{1}{N_\ell} \sum_{g: F_g-1+\ell \le T_g} S_g\, \delta_{g,\ell} = \frac{1}{N_\ell} \sum_{g: F_g-1+\ell \le T_g} \delta_{g,\ell},$$

and its estimator is

$$\widehat{\mathrm{DID}}_\ell = \frac{1}{N_\ell} \sum_{g: F_g-1+\ell \le T_g} S_g\, \widehat{\mathrm{DID}}_{g,\ell} = \frac{1}{N_\ell} \sum_{g: F_g-1+\ell \le T_g} \widehat{\mathrm{DID}}_{g,\ell}. \tag{4}$$

De Chaisemartin and d'Haultfoeuille (2024) show that $\mathbb{E}\left[\widehat{\mathrm{DID}}_\ell \mid D\right] = \delta_\ell$.

This non-absorbing, intertemporal, and dose–response DID framework delivers a fully nonparametric event study that nets out each country's own status-quo CPI trend $\sum_t \phi_t(D_{g,1})$, differences each treated country against contemporaneous non-hiking peers, and aggregates the per-percentage-point effects across the panel. Under our maintained assumptions — *No Anticipation and Functional Parallel Trends* — Eqs. (2)–(4) cleanly identify the dynamic pass-through of minimum wage increments on aggregate prices. In the next section, we implement these estimators and report placebo and pre-trend tests to validate our identifying conditions before presenting the main dose–response estimates.

5. Results

5.1. Minimum wage pass-through on CPI

Fig. 5 plots our core dynamic pass-through estimates from the non-absorbing, intertemporal, continuous-dose DID described in Section 4. In both panels, the horizontal axis measures months relative to each country's post-January 2021 wage hikes (with negative values denoting pre-event horizons and positive values denoting post-event horizons), and the vertical axis reports the estimated percentage change in CPI per one percent increase in minimum wage. Panel (a) implements a linear basis for the status-quo trend $\hat{\phi}_t(D_{g,1})$; Panel (b) instead uses a quadratic basis.[14] In both specifications, the point estimates for all pre-event horizons ($\ell = -3$ to $\ell = -1$) lie close to zero, with their 95 percent confidence intervals spanning zero, confirming the absence of anticipatory CPI movements. These placebo tests[15] provide direct evidence in support of *No Anticipation*. At the same time, the polynomial specifications of $\hat{\phi}_t(D_{g,1})$ satisfy *Functional Parallel Trends* by capturing any smooth, dose-dependent baseline inflation patterns across countries. Together, these validation exercises confirm that our DID estimates cleanly isolate the pass-through of minimum wage increments, free from anticipatory bias or non-parallel trends.

[14] We approximate the status-quo trend $\phi_t(d)$ via low-order polynomials in the baseline dose. A linear or quadratic basis suffices to flexibly absorb smooth heterogeneity across the six nonzero starting values (0.11–1.9 pp) without overfitting. Higher-order terms introduce additional inflection points that lack strong economic justification — minimum wage regimes are not expected to exhibit three or more kinks in their counterfactual CPI trajectories — and risk poor extrapolation. In practice, cubic and quartic specifications generate unstable $\hat{\phi}_t$ estimates on our limited nonzero subsample without improving performance.

[15] Specified in Appendix C.

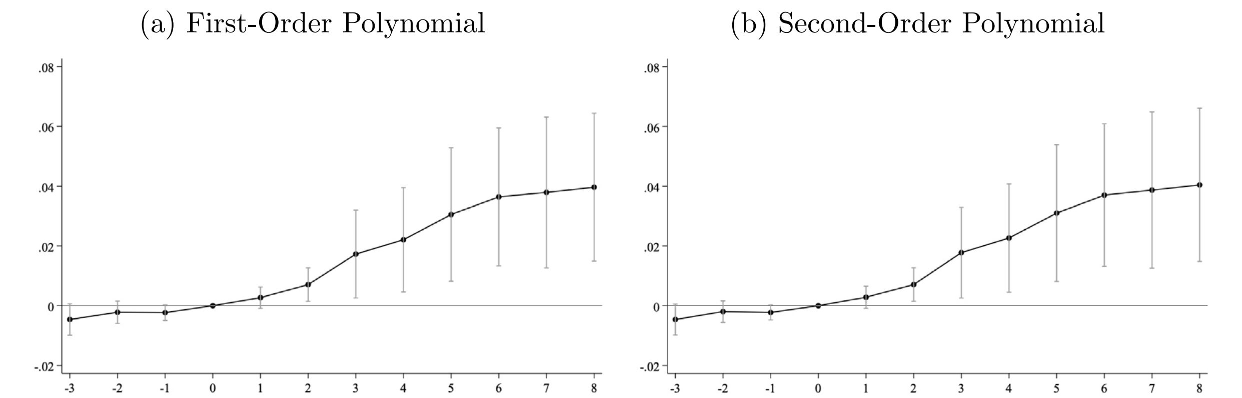

Fig. 5. DID estimates.
The figure plots the estimated dynamic pass-through of a one–percentage-point minimum wage increase on log CPI across three pre-event months ($\ell = -3, -2, -1$) and eight post-event months ($\ell = 1, \ldots, 8$), using (a) a linear and (b) a quadratic polynomial to model the baseline CPI trend, corresponding to specifications (1) and (2) in Fig. 5, respectively.

After implementation ($\ell \geq 1$), we observe a steadily rising pass-through: the one percent wage increase has limited immediate effect at $\ell = 1$, and by five months post-hike, the CPI response reaches roughly 0.03 percent. Confidence intervals remain strictly above zero from $\ell = 3$ onward, indicating a statistically significant, cumulative inflationary impact. The dynamic profile is strictly increasing over time, consistent with a moderate pass-through process that takes several months to materialise across the consumer price basket.

The close alignment between Panels (a) and (b) demonstrates that our results are robust to the choice of polynomial order in modelling the baseline CPI trend. Together, these estimates deliver our first key finding: statutory minimum wage increases produce statistically significant and economically negligible-to-low increases in aggregate prices, with pass-through materialising gradually, further highlighting the necessity of studying this relationship in a dynamic and non-absorbing setting. Note that the TWFE benchmark in Section 3 and the continuous-dose DID estimates reported here capture different parameters. The TWFE coefficient measures a single static elasticity of log CPI with respect to the level of the cumulative minimum-wage index. By contrast, the estimator of De Chaisemartin and d'Haultfoeuille (2024) identifies dynamic per-dose effects: the horizon-specific response of prices to each incremental increase in the minimum wage. These effects are then cumulated across horizons to obtain the overall pass-through profile. When policy changes occur repeatedly and responses unfold with lags, a static TWFE regression does not estimate the same object. Mechanically, because the dynamic response is near zero at short horizons and rises only after several months, a static regression that relates CPI to the cumulative index averages over periods with little contemporaneous response, thereby attenuating the estimated coefficient relative to the peak and cumulative dynamic effects. It combines contemporaneous and lagged effects and pools horizons where the marginal response differs, and it may also reflect non-transparent weighting across comparisons in staggered-timing settings (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). For this reason, the TWFE coefficient of Section 3 is interpreted as a reduced-form association, while the dynamic estimator directly traces the intertemporal response to each policy increment.

5.1.1. Sensitivity and robustness

Table 2 evaluates the robustness of our dynamic pass-through estimates across eight specifications. Columns (1)–(2) reproduce the baseline event-study using a first- and second-order polynomial, respectively, for the status-quo trend as shown in Fig. 5. Columns (3)–(4) report the same two specifications with bootstrapped clustered standard errors (399 replications), and Columns (5)–(6) add as controls the monthly interest rate and the proximity-to-Ukraine-war indicator. Column (7) restricts the sample to countries with zero initial treatment at $t = 1$, and Column (8) returns to the first-order polynomial, using the monthly log-difference as the outcome variable.

Across all eight panels, the estimated effects (DID ℓ) remain closely aligned: small and indistinguishable from zero in the three placebo pre-implementation estimates, (DID−3 through DID−1), rising steadily after implementation, and reaching 0.039–0.046 percent by month eight. DID estimates in columns (3) and (4) are identical to columns (1) and (2), respectively, but the standard errors are obtained via a country-cluster bootstrap, which transparently propagates the uncertainty from the estimation of the baseline-trend polynomial $\hat{\phi}_t$. These bootstrapped standard errors, both in (3) and (4), are expected to be larger than the asymptotic clustered ones because they capture both sampling variation in the initial $\hat{\phi}_t$ fit and the finite-sample distribution of the event-study estimates, leading to somewhat more conservative inferences. Despite the larger bootstrap-based standard errors, we retain some degrees of statistical significance, underscoring that even with expected increases in uncertainty, our core inference remains robust. Crucially, the main takeaway of this exercise is that the placebo estimates' confidence intervals remain very narrow, showing that even under maximal uncertainty, there is no pre-treatment drift in CPI. This sharp null confirms that our identifying assumptions hold firmly before any wage hike, bolstering the credibility of our estimates.

Table 2
DID estimates of minimum wage pass-through on CPI.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DID 1	0.0027	0.0028	0.0027	0.0028	0.0024	0.0027	0.0006	0.0002
	(0.0018)	(0.0019)	(0.0025)	(0.0025)	(0.0017)	(0.0018)	(0.0013)	(0.0015)
DID 2	0.0071	0.0071	0.0071	0.0071	0.0070	0.0071	0.0046	0.0014
	(0.0029)	(0.0029)	(0.0050)	(0.0047)	(0.0028)	(0.0029)	(0.0012)	(0.0011)
DID 3	0.0173	0.0178	0.0173	0.0178	0.0175	0.0181	0.0056	0.0051
	(0.0075)	(0.0078)	(0.0120)	(0.0106)	(0.0076)	(0.0079)	(0.0018)	(0.0026)
DID 4	0.0221	0.0226	0.0221	0.0226	0.0243	0.0250	0.0057	0.0042
	(0.0089)	(0.0093)	(0.0138)	(0.0112)	(0.0099)	(0.0104)	(0.0023)	(0.0020)
DID 5	0.0305	0.0310	0.0305	0.0310	0.0337	0.0345	0.0071	0.0067
	(0.0114)	(0.0117)	(0.0205)	(0.0162)	(0.0126)	(0.0130)	(0.0039)	(0.0036)
DID 6	0.0364	0.0370	0.0364	0.0370	0.0393	0.0402	0.0112	0.0093
	(0.0118)	(0.0122)	(0.0227)	(0.0190)	(0.0128)	(0.0133)	(0.0034)	(0.0043)
DID 7	0.0379	0.0387	0.0379	0.0387	0.0434	0.0445	0.0112	0.0084
	(0.0129)	(0.0133)	(0.0225)	(0.0197)	(0.0145)	(0.0151)	(0.0037)	(0.0040)
DID 8	0.0397	0.0404	0.0397	0.0404	0.0453	0.0465	0.0100	0.0070
	(0.0126)	(0.0131)	(0.0209)	(0.0205)	(0.0140)	(0.0145)	(0.0060)	(0.0038)
Duration	4.36	4.36	4.36	4.36	4.36	4.36	4.36	4.36
DID −1	−0.0023	−0.0023	−0.0023	−0.0023	−0.0017	−0.0016	−0.0025	−0.0030
	(0.0013)	(0.0013)	(0.0017)	(0.0018)	(0.0012)	(0.0011)	(0.0008)	(0.0014)
DID −2	−0.0022	−0.0020	−0.0022	−0.0020	−0.0018	−0.0016	−0.0023	0.0009
	(0.0019)	(0.0018)	(0.0021)	(0.0027)	(0.0018)	(0.0018)	(0.0012)	(0.0014)
DID −3	−0.0046	−0.0046	−0.0046	−0.0046	−0.0045	−0.0046	−0.0066	0.0019
	(0.0027)	(0.0026)	(0.0047)	(0.0049)	(0.0027)	(0.0026)	(0.0022)	(0.0015)
Polynomial	1st	2nd	1st	2nd	1st	2nd	–	1st
Controls	No	No	No	No	Yes	Yes	No	No
Bootstrap	No	No	Yes	Yes	No	No	No	No
N	1189	1189	1189	1189	1189	1189	943	1189

Notes: Each row reports the DID effects which measures the percentage change in the CPI attributable to a one-percent increase in the statutory minimum wage, (ℓ) months after implementation (DID ℓ). The outcome variable in Columns (1)–(7) is the logarithm of the CPI index (July 2020 = 100); Column (8) uses the monthly log-difference. All regressions include country and month fixed effects, and the standard errors are clustered at the country level. Columns (1) and (2) use first- and second-order polynomial specifications of $\hat{\phi}_t(D_{g,1})$, respectively. Columns (3) and (4) replicate (1) and (2) with bootstrap standard errors (399 replications). Columns (5) and (6) include controls for short-term interest rates and proximity to the Ukraine war. Column (7) restricts the sample to countries with zero treatment at baseline. *Duration* reports the average number of months over which pass-through effects are statistically significant. DID–1 to DID–3 report placebo estimates to test for the Anticipation and Parallel Trends assumptions.

In Columns (5) and (6) of Table 2, we add our most salient time-varying controls — interest rate and a proximity-to-Ukraine-war indicator[16] — to ensure that our dynamic DID estimates are not driven by concurrent monetary policy shifts or the degree of exposure to geopolitical shocks. The estimated wage-to-price pass-through remains essentially unchanged, confirming the robustness of our results to these potential confounders. In Column (7), we restrict the sample to countries with a zero baseline dose — thereby requiring no polynomial adjustment — and drop six countries, including Canada and the United States, which account for many of the minimum wage hikes. While this reduces the sample by over twenty percent and minimum wage variation by about twenty-five percent, the cumulative pass-through remains monotonic, statistically significant, and peaks at the same horizons, offering a stringent check on our functional-form assumption. Finally, Column (8) substitutes log CPI with monthly inflation. Although inherently more volatile, this outcome yields a qualitatively similar pass-through profile,[17] providing an additional layer of robustness to our core finding.[18]

The average duration of the effect is 4.36 months, which shows no sensitivity to functional form, controls, or sample composition. Finally, the bottom rows confirm that all placebo estimates for pre-treatment months remain small and, although some of them are statistically significant, hover around zero without indicating any pre-trend or pre-implementation responses, providing further evidence that our identifying assumptions hold under every tested scenario.

In sum, Table 2 shows that a 10 percent statutory minimum wage increase generates, on average, an immediate increase in the overall price level of roughly 0.027 percent, and under a monotonic and gradually pass-through, exceeds 0.3 percent by month

[16] As in specification (7) of Table 1.

[17] Column (8) looks at the change in inflation rates rather than the change in overall prices. In the earlier columns, the estimates show how much prices in total rise after a minimum-wage increase. By contrast, Column (8) tracks whether the speed of price growth goes up or down right after the policy change. It therefore captures a short-term movement in inflation, not the full build-up of prices over time. The purpose of this check is to see whether the direction and timing of the response are consistent with the main results, not to compare the numbers directly.

[18] Because our treatment effects unfold gradually over several months, focusing on month-to-month inflation — an inherently high-frequency, volatile series dominated by transitory shocks — attenuates the slowly accumulating pass-through signal and yields less precise estimates than the log-CPI level.

5. The findings survive a series of robustness checks,[19] while the placebo tests across all specifications do not detect meaningful or warning pre-implementation effects. In assessing pre-trends, our small coefficients with narrow confidence intervals are more credible than, for instance, imprecise, insignificant ones, as they suggest true stability around zero. Our pre-trends suggest a clean pass of no anticipation and parallel trends. These placebo results give us high confidence that — absent the wage increases — the CPI paths of "soon-to-treat" countries and their not-yet-treated peers would indeed have moved in parallel. Taken together, Table 2 underscores that familiar minimum wage reforms transmit to aggregate consumer prices gradually and to a quantitatively modest degree.

We further probe the identifying assumptions with three sets of sensitivity tests aimed at strengthening internal validity in a macro cross-country setting. First, in addition to the baseline functional-form checks already reported — showing that the estimated dynamic pass-through is essentially unchanged under first- versus second-order polynomials in the baseline dose, and that it remains similar when restricting to countries with a zero baseline dose (thereby requiring no functional adjustment) — we further relax the counterfactual-trend specification to address residual concerns about cross-country structural heterogeneity. Specifically, we allow for country-specific linear trends (implemented by first-differencing and cumulating back), which relaxes the assumption that untreated inflation trends are stable within baseline-dose strata up to a common time effect.[20] The resulting estimates, reported in columns (1) and (2) of Table D.1, remain close to the baseline in both magnitude and timing.

Second, identification relies on a finite-lag restriction to purge residual effects of pre-sample minimum-wage changes. We use a six-month finite lag for two reasons. Economically, the pre-period distributed-lag evidence in Fig. A.2 indicates that the pass-through of minimum-wage changes to prices is largely realised within roughly six months. Methodologically, longer lag windows require carrying a longer dose history into the estimation period, which increases exposure to earlier policy changes and reduces the number of clean comparisons at each horizon, thereby lowering precision. To assess sensitivity, we re-estimate the full dynamic design under a nine-month finite-lag restriction; Table D.1, columns (3) and (4), shows that the estimated response path remains robust.

Finally, we assess sensitivity to institutional settings in which minimum-wage adjustments may be implemented at irregular or multiple non-scheduled intervals. Belgium and France exhibit multiple within-year changes, while Chile displays multiple changes without a stable calendar pattern in the OECD series. Table D.1, columns (5) and (6), re-estimate the baseline specifications excluding Belgium, France, and Chile; the estimated dynamic pass-through profile remains similar in magnitude and timing. This robustness reduces the likelihood that our results are driven by a small subset of countries where adjustment rules differ most sharply from the scheduled-review environment motivating our quasi-experimental interpretation.

5.2. Heterogeneity analysis

We begin our heterogeneity analysis by dividing countries into two groups — those whose real minimum wage changes exceeded the sample median and those whose changes fell below it — to assess whether the magnitude of the wage dose alters the dynamic pass-through to aggregate CPI. We use the data constructed by OECD (2024). We use the data constructed by OECD(2024). In the initial dataset, the nominal minimum wage is provided as a cumulative-change index (base month May 2019). The real minimum wage is obtained by deflating this nominal index using the OECD Consumer Price Index for all items. For consistency with the pre-history setup in Fig. 2(b), we rebase the real minimum wage indices to July 2020. For the heterogeneity split in this section, we then compute — country by country — the cumulative change in the real minimum wage from January 2021 to May 2024, and classify countries as above-median or below-median depending on whether this cumulative real change exceeds the cross-country median. Further, recognising that food prices are both highly volatile and a key driver of overall inflation, we then re-estimate our event-study specifically for the food CPI sub-index. Finally, we focus on the residual-of-food CPI to quantify net-of-food wage-to-price pass-through.

5.2.1. Subsampling by real minimum wage intensity

Fig. 6 illustrates the dynamic pass-through of statutory minimum wage increases for two country groups defined by whether their real minimum wage change exceeds the sample median (3.28 pp). The specification mirrors that of Column (1) in Fig. 5, but the figure focuses exclusively on post-implementation outcomes. Panel (a) shows that countries with larger real-wage hikes experience a pronounced and statistically significant CPI response, rising steadily from month 3 and peaking between 0.02–0.03 percent by months 4 and 5, and then slightly decreasing. By contrast, Panel (b) reveals that in countries with below-median real minimum wage changes, the CPI effect is markedly attenuated, remaining close to zero throughout the post-treatment horizon. Table D.2 in the Appendix quantifies these differences. Overall, DID coefficients are larger and more precisely estimated in the high-dose subsample.

This pattern is intuitive since countries in Panel (a), which experienced, on average, larger real minimum wage increases, also exhibit a stronger CPI response, as our dose–response framework predicts. However, even though Panel (a) countries show somewhat larger estimated pass-through, this is far below levels that would suggest a distortionary or runaway wage-price spiral.

[19] Including the group of OECD countries without a national statutory minimum wage — Austria, Denmark, Finland, Iceland, Italy, Norway, and Sweden — as a never-treated comparison group leaves our estimated dynamic pass-through coefficients and their interpretation unchanged. Switzerland is excluded because five cantons have introduced local minimum wages. he corresponding results are not tabulated or graphed here, as they are quantitatively and visually indistinguishable from the baseline estimates.

[20] Conceptually, this addresses the concern that some countries may have systematically steeper (or flatter) inflation trajectories during the period for reasons unrelated to minimum-wage policy; allowing country-specific linear trends absorbs such differential drift, so identification relies on deviations from these country-specific trend components.

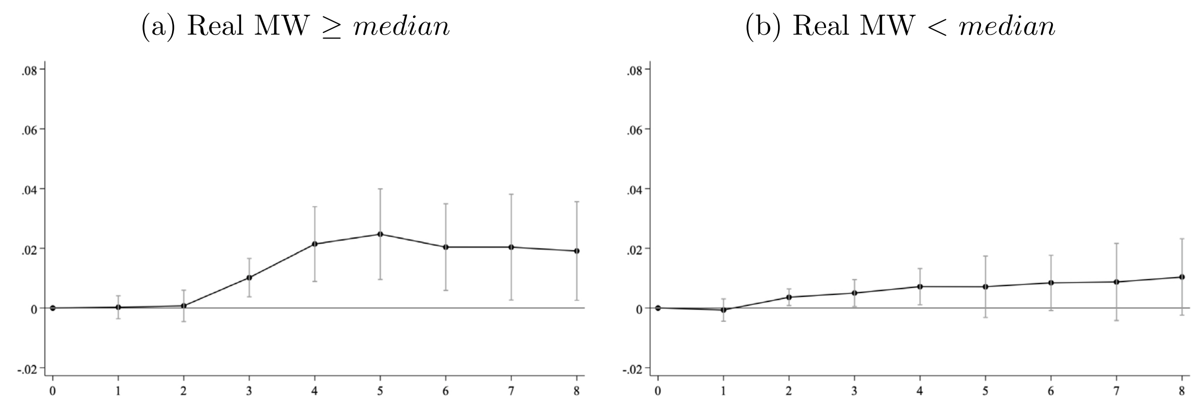

Fig. 6. DID effects by real minimum wage.
The figure plots the estimated dynamic pass-through of one percent minimum wage increase on log CPI across eight post-event months ($\ell = 1, \ldots, 8$), separately for the subsample where the real minimum-wage increase is equal to or above the median (a) and the subsample where it is below the median (b). See Table D.2 in Appendix D for full estimates.

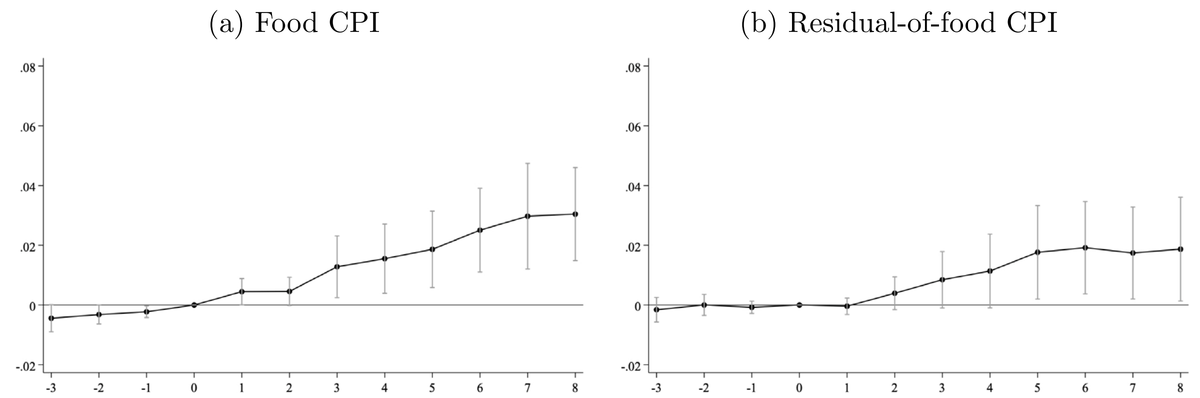

Fig. 7. Minimum wage DID effects on food and residual-of-food CPI.
The figure plots the estimated dynamic pass-through of one percent minimum wage increase on (a) the log food CPI and (b) the net-of-food price index, across three pre-event months ($\ell = -3, -2, -1$) and eight post-event months ($\ell = 1, \ldots, 8$). See Table D.2 in Appendix D for full estimates.

5.2.2. The elephant in the room? pass-through on food CPI

Food prices are not only a substantial component of the Consumer Price Index (CPI), but also a critical determinant of living costs, particularly for lower-income households, for whom food expenditures constitute a disproportionately large share of the consumption basket (Cooper et al., 2020). On the supply side, food-related sectors tend to employ a high concentration of low-wage workers, making them especially sensitive to statutory wage increases (Aaronson et al., 2008; Katz and Krueger, 1992). Together, these factors suggest that food prices are a likely and significant channel through which minimum wage hikes are transmitted into aggregate prices, which requires separate investigation.

We employ our dynamic, non-absorbing, treatment-intensity DID framework to isolate the pass-through of minimum wage hikes on the food CPI. Panel (a) of Fig. 7 shows that the food CPI responds rapidly to wage increases, turning positive by one month post-implementation and rising steadily to roughly 0.03 percent by the eighth month. This suggests that food prices absorb the wage shock more immediately and persistently than aggregate prices. On average a 10 percent minimum wage increase generates an immediate 0.045 percent rise in food prices, which increases steeper reaching 0.19 percent by month five of implementation.

To untangle non-food effects, we first regress log aggregate CPI on log food CPI[21] and then apply our DID framework to the resulting residuals. Panel (b) of Fig. 7 shows the net-of-food CPI trajectory, which remains flatter than the food CPI and peaks at just over 0.02 percent. Moreover, the pre-event DID estimates are effectively zero throughout the pre-treatment window (Table D.2),

[21] This regression omits additional controls and fixed effects to avoid over-demeaning the outcome since our DID models already incorporate country and month fixed effects.

suggesting that any anticipatory pricing response, if any, is confined to food prices. This finding aligns with Renkin et al. (2022), who document inflationary effects in grocery and takeaway prices up to three months before implementation.

Together, these results indicate that food prices constitute the key channel through which statutory minimum wage hikes propagate into aggregate prices. This finding can be attributed to two mechanisms. First, the food sector is relatively more sensitive to labour cost shocks, as it is both labour-intensive and disproportionately reliant on minimum wage workers (Godoey and Reich, 2021). Second, low-wage earners allocate a larger share of their consumption to essentials such as food and groceries, rather than discretionary or luxury items (Leung, 2021), potentially amplifying demand-side effects. Accordingly, minimum wage increases may simultaneously raise production costs in the food sector and stimulate consumer demand for food products, generating a dual supply–demand inflationary impulse.

These two mechanisms map naturally into the macro distinction between a cost (marginal-cost) channel and an income (demand) channel of transmission. On the cost side, minimum wage hikes act as a sector-specific increase in labour costs, implying stronger price pass-through in low-wage–intensive sectors such as food retail and food services (Aaronson et al., 2008; Renkin et al., 2022). On the income side, minimum wage gains raise disposable income among lower-wage households with relatively high marginal propensities to consume, and because food is a necessity with a large budget share, any demand pressure is likely to be concentrated in food categories (Cooper et al., 2020; Leung, 2021). Our finding that most of the aggregate CPI response is accounted for by food is therefore consistent with transmission operating primarily through these sectoral cost and necessity-demand margins, rather than through broad demand spillovers across the consumption basket; nevertheless, with aggregate CPI data we cannot formally decompose the relative contributions of cost versus demand channels. Additionally, other disaggregate CPI components, beyond food, may also provide further insights on heterogeneous pass-through effects.

6. Conclusion

This paper is the first to estimate the dynamic pass-through effect of statutory minimum wage increases on aggregate consumer prices across 29 OECD economies during the synchronised inflationary cycle of 2021–2024. Exploiting routine, staggered policy reviews under a common global shock, we apply a recently developed continuous-dose difference-in-differences estimator that accommodates the cumulative, non-absorbing nature of statutory wage floors and the dynamic evolution of treatment intensity, developed by De Chaisemartin and d'Haultfoeuille (2024).

We highlight three main findings. First, a 10 percent hike in the statutory minimum wage raises headline CPI by 0.027 percent in the first month, with the full effect unfolding just over 4 months and reaching at 0.3 percent by month five. Second, this effect is driven mainly by food-price responses, net-of-food CPI remains essentially flat, underlying that the food sector is both highly exposed to wage-cost shocks and central to low-income consumption baskets. This sectoral concentration is consistent with both cost-channel transmission (marginal-cost pass-through in low-wage–intensive food sectors) and income-channel transmission (higher nominal spending by minimum-wage households concentrated in necessities), indicating food prices as the primary conduit from wage floors to headline CPI. Third, while countries with larger real-wage increases exhibit somewhat stronger pass-through, even the high-dose subsample does not approach inflationary spirals. Across all specifications, placebo estimates reveal no anticipatory movements. Our findings confirm sectoral or unique-country elasticity estimates, and extend prior literature by recovering the temporal structure of price adjustment following wage increases, in contrast to static or single-period elasticity estimates.

Methodologically, our study demonstrates that dynamic, dose-varying DID designs can recover credible estimates of policy effects, even in macro panels, without granular firm- or household-level data. This approach does not challenge structural macroeconomic frameworks but instead complements them by offering a transparent, design-based strategy to identify policy effects in reduced form. Our analysis contributes to the very recent literature that leverages quasi-experimental designs to study macro-labour questions (e.g. Jordà and Nechio, 2023).

Our findings establish that statutory minimum wage adjustments function as precise macroeconomic policy levers—with quantifiable, gradual effects on aggregate inflation rather than opaque "black-box" or negligible interventions. Because the bulk of the pass-through concentrates in food prices, fiscal authorities can preserve real income gains without triggering broad-based inflation by deploying targeted measures—such as temporary food-subsidy credits or reduced VAT on essential groceries. The evidence implies that moderate, routine minimum wage adjustments contribute only marginally to headline inflation and do so mainly through food prices. These effects unfold slowly and remain contained, suggesting that wage-floor policies, if coordinated with fiscal tools, need not compromise price stability. Finally, our cross-country quasi-experimental framework, which relies solely on publicly available macro data, demonstrates a rigorous and replicable tool for policymakers to evaluate the inflationary consequences of wage-floor reforms and other institutional interventions.

Three limitations merit further attention. First, we cannot isolate supply- from demand-side transmission channels, particularly in the food sector. Our sectoral decomposition is therefore best interpreted as identifying which CPI components account for the aggregate response, rather than providing a structural attribution to marginal-cost versus demand mechanisms. Disentangling these channels would require micro-level evidence on quantities, markups, and household expenditure responses, which is beyond the scope of the aggregate CPI data used here.

Second, because both the outcome (CPI) and treatment (statutory minimum wage) are measured at the national level, our estimates should be interpreted as national average price responses. This aggregation may conceal meaningful within-country heterogeneity. Price pass-through could be larger in local labour markets where the minimum wage is more binding — e.g., where the wage floor is closer to the local wage distribution or where low-wage employment is more concentrated — than in less exposed regions, so localised price adjustments may be attenuated in national CPI aggregates. In addition, household consumption baskets vary systematically across the income distribution (notably in the food share), implying that the incidence of sectoral price changes across households may differ from the aggregate CPI response. Accordingly, our results speak to aggregate inflation dynamics and do not directly identify regional or distributional inflation effects. A natural extension of this work is to integrate the same dynamic DID framework with subnational price indices and micro-level consumption to quantify heterogeneous pass-through across regions and household types, and to translate sectoral price responses into welfare-relevant distributional inflation measures.

Third, while the institutional timing of minimum-wage reviews and the diagnostic evidence mitigate concerns about policy endogeneity, we cannot fully rule out that statutory wage-setting covaried with contemporaneous stabilisation policies during the 2021–2024 inflation surge. In particular, heterogeneous fiscal measures (e.g., subsidies, tax changes) and monetary-policy responses could have jointly influenced inflation dynamics and the political economy of wage adjustments, beyond what is absorbed by our controls for short-term interest rates and proximity to the war in Ukraine. Accordingly, our quasi-experimental interpretation should be understood as applying to the component of minimum-wage variation plausibly orthogonal to these concurrent policy responses, rather than to every policy episode in this period.

CRediT authorship contribution statement

Fikret Bilenkisi: Writing – review & editing, Writing – original draft, Software, Methodology, Formal analysis, Data curation, Conceptualization. **Filippos Maraziotis:** Writing – review & editing, Writing – original draft, Software, Methodology, Formal analysis, Data curation, Conceptualization. **M. Akif Yardimci:** Writing – review & editing, Writing – original draft, Software, Methodology, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A

See Figs. A.1 and A.2 and Tables A.1–A.3.

Appendix B

See Figs. B.1–B.3.

B.1. Construction of the "Proximity to War" variable

Let $(\text{lat}_g, \text{lon}_g)$ denote the centroid latitude and longitude of country g (in decimal degrees), and let

$$(\varphi_K, \lambda_K) = \left(50.4500^\circ \times \tfrac{\pi}{180}, 30.5167^\circ \times \tfrac{\pi}{180}\right)$$

be Kyiv's coordinates in radians. We compute a normalised angular distance in four steps:

1. **Convert to radians:**
$$\varphi_g = \text{lat}_g \times \frac{\pi}{180}, \quad \lambda_g = \text{lon}_g \times \frac{\pi}{180}.$$
2. **Haversine half-chord:**
$$a_g = \sin^2\!\left(\tfrac{\varphi_g - \varphi_K}{2}\right) + \cos(\varphi_g)\cos(\varphi_K)\sin^2\!\left(\tfrac{\lambda_g - \lambda_K}{2}\right).$$
3. **Central angle:**
$$\alpha_g = 2\arcsin\!\left(\sqrt{a_g}\right), \quad 0 \le \alpha_g \le \pi.$$
4. **Normalisation to** $[0, 1]$:
$$\text{proximity to war}_{g,t} = \begin{cases} 0, & t < \text{February 2022}, \\ 1 - \dfrac{\alpha_g}{\pi}, & t \ge \text{February 2022}. \end{cases}$$

Table A.1
Minimum wage increases, 2019–2024.

Country	2019	2020	2021	2022	2023	2024	Institutional setting
Australia	Jul	Jul	Jul	Jul	Jul	Jul	Routine/scheduled; annual review by the Fair Work Commission.
Belgium	Jan	Jan	Oct	multiple	multiple	Apr	Automatic indexation with collective bargaining; may generate multiple within-year adjustments.
Chile	Jan	Jan/Sep	May/Dec	May/Aug	Jan/May/Sep	Jul	Formal review process, but implementation months are not fixed; changes may occur at varying intervals.
Colombia	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; annual review by the Permanent Commission on Wage.
Costa Rica	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; annual review by the National Wage Council.
Czechia	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; formula-based review linked to projected average gross monthly wages (Ministry of Labour and Social Affairs).
Estonia	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; annual review through collective bargaining.
France	Jan	Jan	Jan/Oct	Jan/May/Aug	Jan	Jan	Scheduled annual update with automatic indexation triggers; government sets the final decision.
Germany	Jan	Jan	Jan/Jul	Jan/Oct	No change	Jan	Routine/scheduled; set following recommendations of the Minimum Wage Commission, guided by collective bargaining wage developments.
Greece	Feb	No change	No change	Jan	Apr	Apr	Routine/scheduled; annual review by the Ministry of Labour.
Hungary	Jan	Jan	Feb	Feb	Jan	Jan	Routine/scheduled; negotiated within the Permanent Consultative Forum of the Competitiveness Sector and the Government.
Ireland	Jan	Feb	Jan	Jan	Jan	Jan	Routine/scheduled; government decision following recommendations of the Low Pay Commission.
Israel	Apr	Apr	Apr	Apr	Apr	Apr	Routine/scheduled; annual review within a collective bargaining framework; rare ad hoc changes.
Japan	Oct	Oct	Oct	Oct	Oct	Oct	Routine/scheduled; annual review by the Central Minimum Wages Council.
Korea	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; annual review by the Minimum Wage Commission.
Latvia	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; Cabinet decision following a proposal from the Minister of Welfare.
Lithuania	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; government decision following the Tripartite Council proposal.
Luxembourg	Jan	Jan	Jan	Jan/Apr	Jan	Jan	Routine/scheduled; hybrid process (government and Chamber of Deputies) combined with automatic indexation.
Mexico	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; annual review by the National Minimum Wage Commission.
Netherlands	Jan/Jul	Jan/Jul	Jan/Jul	Jan/Jul	Jan/Jul	Jan/Jul	Routine/scheduled; biannual automatic adjustment linked to expected growth in collectively agreed wages.
New Zealand	Apr	Apr	Apr	Apr	Apr	Apr	Routine/scheduled; annual review by Cabinet following recommendations from the Minister for Workplace Relations and Safety.
Poland	Jan	Jan	Jan	Jan	Jan/Jul	Jan/Jul	Routine/scheduled; annual review within the Social Dialogue Council; occasional mid-year update.
Portugal	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; government decision after consultation with the Permanent Commission of Social Concertation.
Slovak Republic	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; annual review by the Minimum Wage Commission.
Slovenia	Jan	Jan	Jan	Jan	Jan	Jan	Routine/scheduled; multi-step process with negotiation and an automatic trigger tied to average wages; final confirmation by the Ministry of Labour.

(*continued on next page*)

Table A.1 (*continued*).

Spain	Jan	Jan	Sep	Jan	Jan	Jan	Routine/scheduled; government decision after negotiations with social partners.
United Kingdom	Apr	Apr	Apr	Apr	Apr	Apr	Routine/scheduled; annual review by the Low Pay Commission.

Notes: Entries report the month(s) in which the OECD monthly statutory minimum-wage series records a change in the corresponding calendar year. The concentration of changes in a small set of months (predominantly January, April, July, and October) and the stable calendar patterns across the years are consistent with institutionalised review schedules rather than high-frequency retiming. Two qualifications are important. First, Belgium and France exhibit multiple within-year changes, reflecting automatic indexation mechanisms that can trigger many adjustments per year beyond the routine revisions. Second, Chile shows changes without a stable calendar pattern in the OECD series; although the adjustment process is formalised, implementation months are not anchored to a fixed annual date.

Table A.2
Minimum wage and current and lagged CPI.

	Minimum wage
CPI	0.0696
	(0.3713)
CPI −1	−0.0294
	(0.2098)
CPI −2	0.0716
	(0.1685)
CPI −3	−0.1384
	(0.1762)
CPI −4	0.9536
	(0.3151)
Months FE	Yes
Country FE	Yes
Within R^2	0.24
N	1189

Notes: Robust standard errors are shown in parentheses, clustered at the country level. Country-specific regressions, controlling for month effects, suggest that out of the 29 countries, only six show a statistically significant correlation between minimum wage and the fourth lag of CPI: Australia, Canada, Hungary, Ireland, Israel, and Lithuania.

Table A.3
Spatial correlation test (Moran's I).

Specification	Cut-off	Moran's I	*p*-value
(1)	900 km.	0.38	0.35
(7)	900 km.	0.38	0.35
(1)	1500 km.	0.30	0.38
(7)	1500 km.	0.30	0.38

Notes: Specifications (1) and (7) correspond to the relevant columns of Table 1. Null hypothesis is: Spatial Randomisation.

Appendix C

C.1. Placebo tests for no-anticipation and parallel trends

To empirically assess the credibility of Assumptions 1 (No Anticipation) and 2 (Functional Parallel Trends), De Chaisemartin and d'Haultfoeuille (2024) construct placebo–DID estimators that mirror the main $\text{DID}_{g,\ell}$ contrasts but operate entirely within the pre-hike period of each country. Failure of these placebos to average to zero provides direct evidence against our identifying assumptions.

C.1.1. Placebo–DID at the country level

Fix a country g whose first wage hike occurs at month $F_g \geq 3$, and choose any horizon

$$\ell \ \leq \ \min\{\, T_g - F_g + 1,\ F_g - 2\},$$

so that g is observed at least ℓ months *before* and *after* its event date. Define the "backwards" placebo contrast

$$\widehat{\text{DID}}^{\text{pl}}_{g,\ell} \ = \ \underbrace{\left(Y_{g,F_g-1} - Y_{g,F_g-1-\ell}\right) \ - \sum_{t=F_g-\ell}^{F_g-1} \widehat{\phi}_t(D_{g,1})}_{\text{country } g \text{ pre-event jump net of its fitted status-quo trend}} .$$

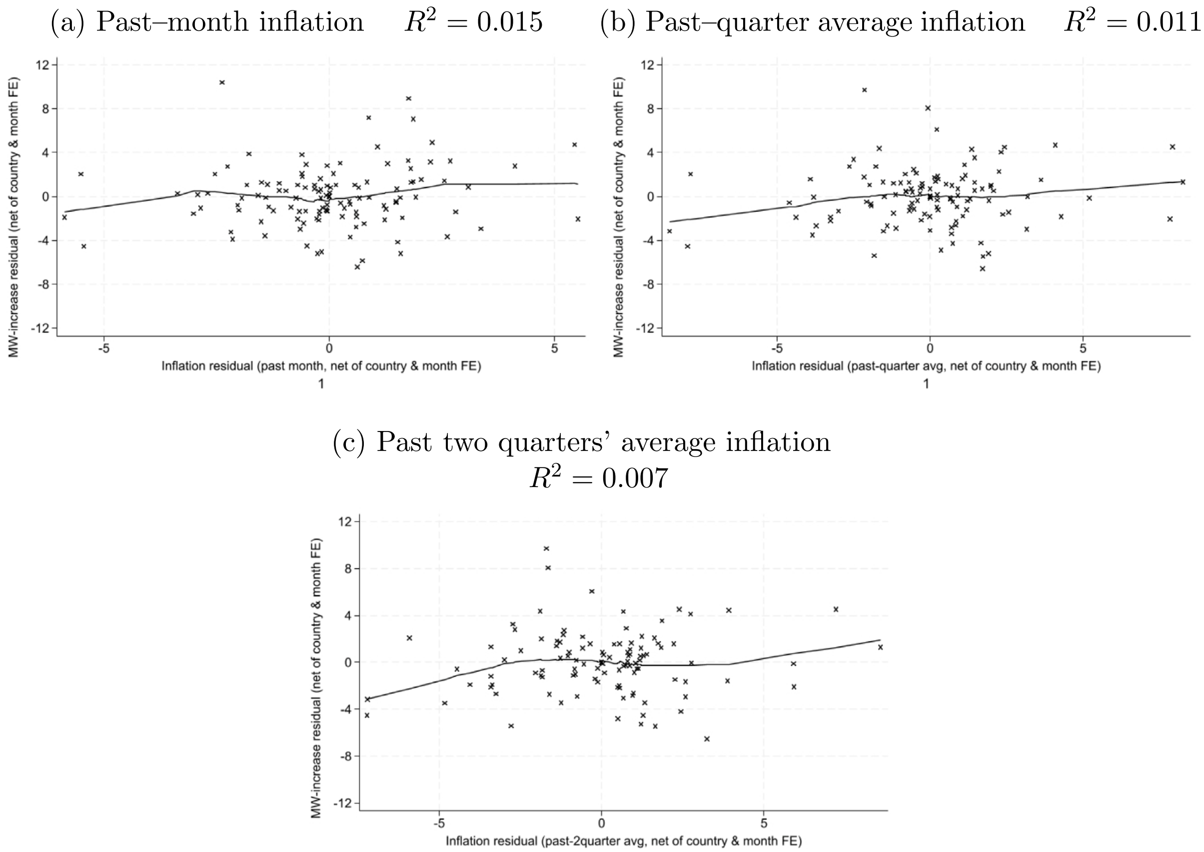

Fig. A.1. Minimum-wage increase magnitude and recent inflation deviations.
Each marker is a country–month in which the monthly statutory minimum wage increases over the window Jan 2021–May 2024. The vertical axis is the residual from regressing the monthly minimum-wage increment on country and month fixed effects. The horizontal axis is the residual from regressing inflation on the same country and month fixed effects, where inflation is measured as the year-on-year log change in CPI and then averaged over the indicated lag window. Solid lines show a LOWESS smoother. Reported R^2 values are from a bivariate OLS regression of the minimum-wage-increment residual on the inflation residual (computed separately for each panel). Because the sample is restricted to event months, the figure is descriptive of whether larger increases tend to follow higher recent inflation deviations, not of the decision to adjust the minimum wage.

Under *No Anticipation* and *Functional Parallel Trends*: $\mathbb{E}\big[\widehat{\mathrm{DID}}^{\mathrm{pl}}_{g,\ell} \mid D\big] = 0$ for all admissible ℓ.

C.1.2. Aggregated placebo event-study

Let

$$L_{\mathrm{pl}} = \max_{g} \min\{T_g - F_g + 1,\ F_g - 2\}, \qquad N^{\mathrm{pl}}_{\ell} = \Big|\{g : 1 \le F_g - 1 - \ell,\ F_g - 1 + \ell \le T_g\}\Big|.$$

Then the horizon–ℓ average is

$$\widehat{\mathrm{DID}}^{\mathrm{pl}}_{\ell} = \frac{1}{N^{\mathrm{pl}}_{\ell}} \sum_{g:1\le F_g-1-\ell,\ F_g-1+\ell\le T_g} \widehat{\mathrm{DID}}^{\mathrm{pl}}_{g,\ell},$$

and by the same logic $\mathbb{E}[\widehat{\mathrm{DID}}^{\mathrm{pl}}_{\ell} \mid D] = 0$. Rejecting this null at any ℓ provides direct evidence of either anticipatory effects or residual baseline–heterogeneous trends not captured by $\{\phi_t\}$.

If $\widehat{\mathrm{DID}}^{\mathrm{pl}}_{\ell} \neq 0$, its sign reflects the direction of systematic trend differences between future switchers and non-switchers. Under mild uniformity, the sign of $-\widehat{\mathrm{DID}}^{\mathrm{pl}}_{\ell}$ indicates the likely bias in the post-event $\widehat{\mathrm{DID}}_{\ell}$.

Because these placebos exactly mimic the main estimator's structure (including the fitted ϕ_t adjustments), they integrate seamlessly into the Rambachan and Roth (2023) framework for quantifying robustness to deviations from parallel trends.

In combination, the placebo–DID statistics deliver a direct, data-driven check on our core identifying assumptions, substantially reinforcing the credibility of the continuous-treatment DID estimates of minimum wage pass-through.

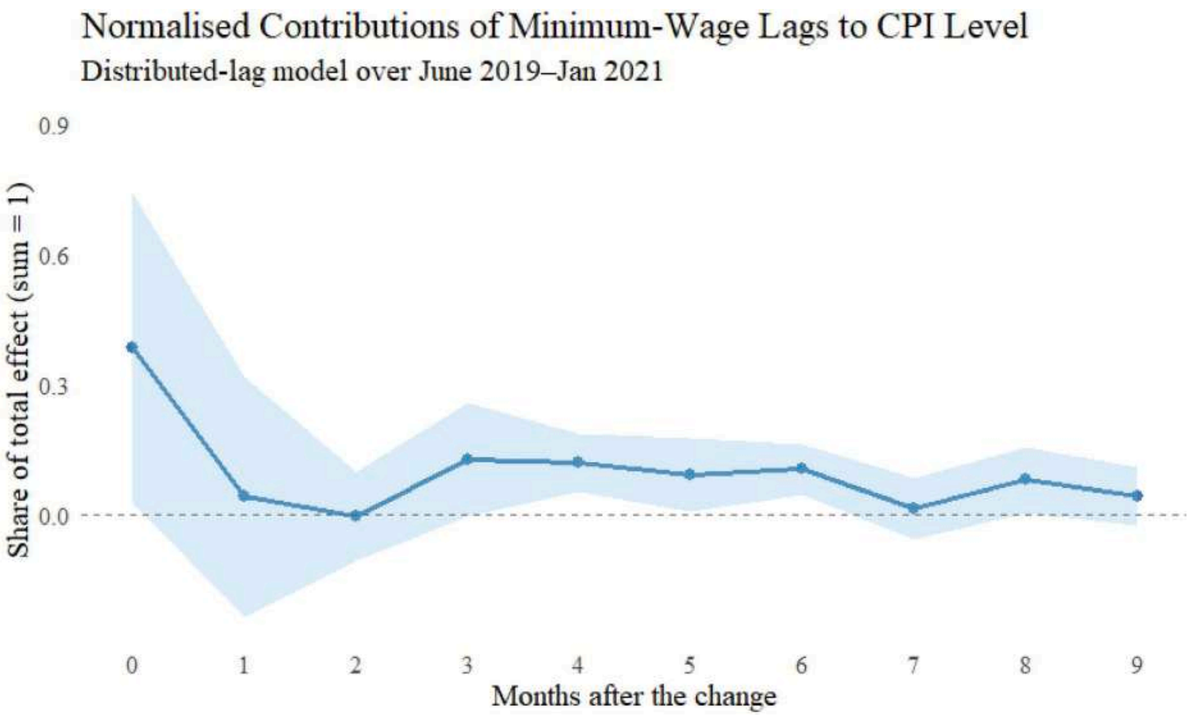

Fig. A.2. Distributed lag model for the period June 2019–January 2021.

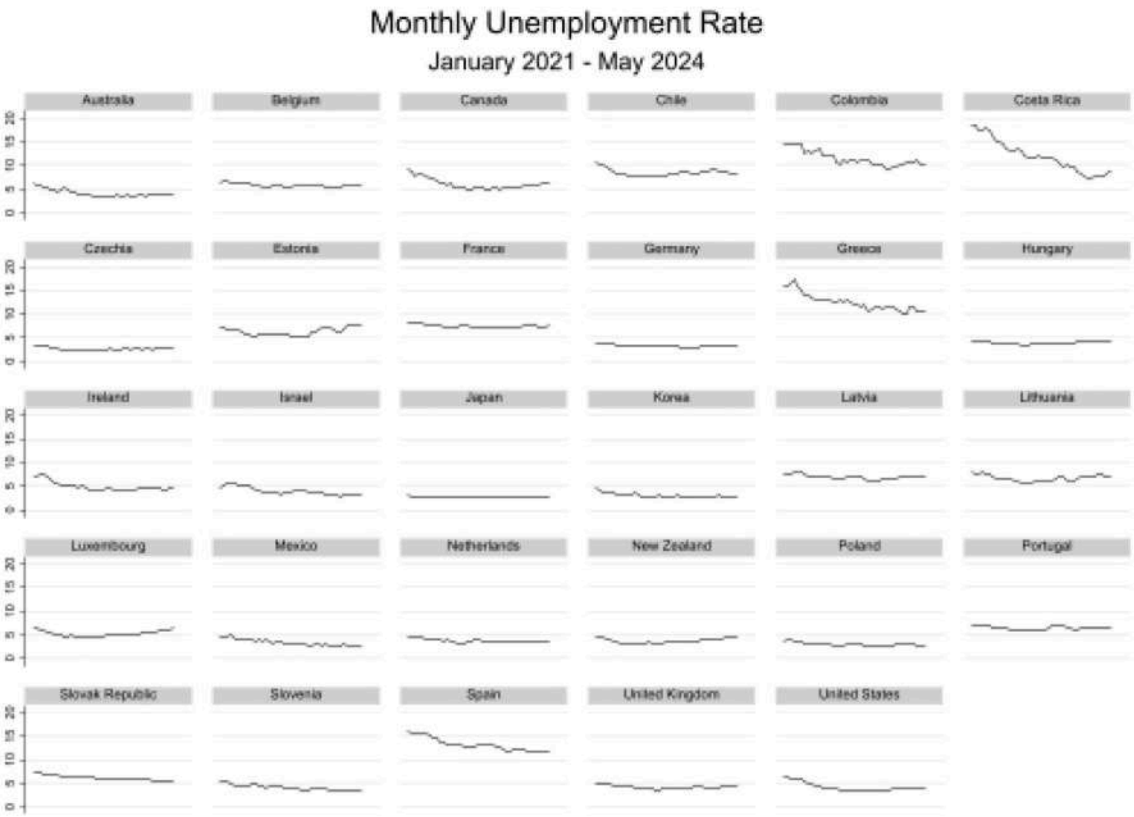

Fig. B.1. Source the OECD.

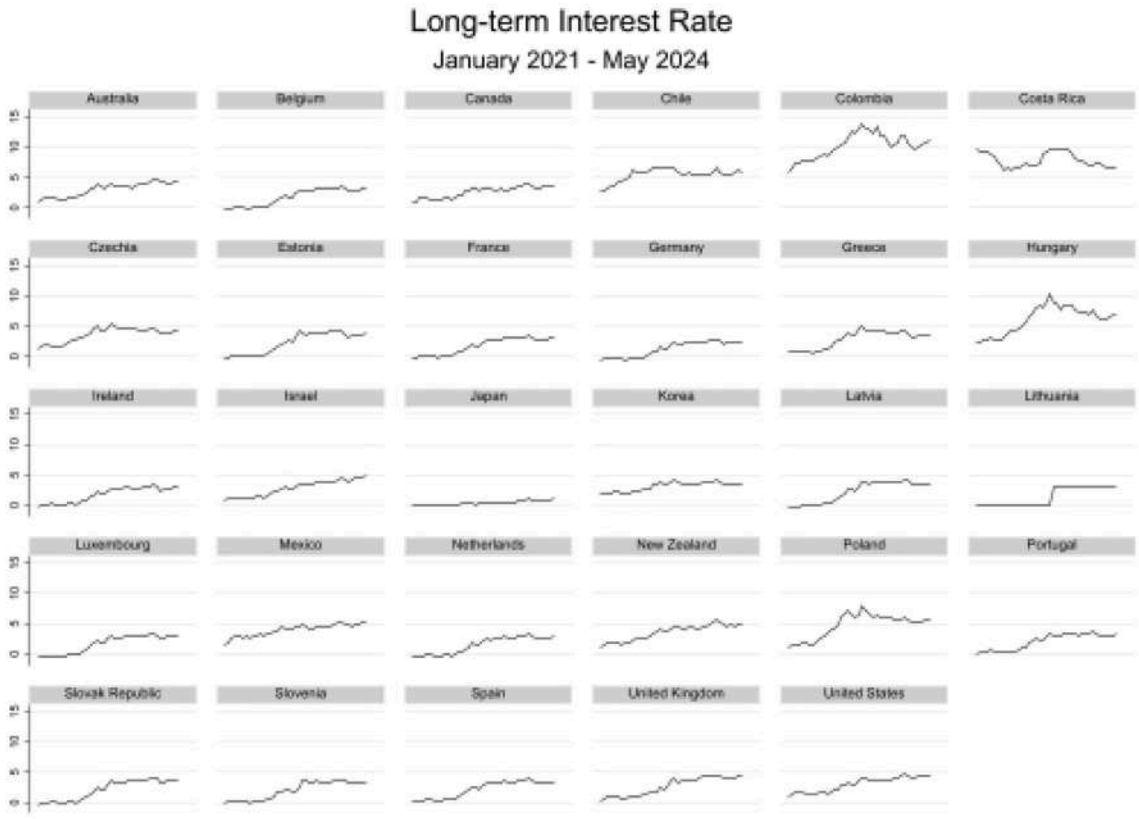

Fig. B.2. Source the OECD.

Table B.1
Proximity to War, by country.

Country	Normalised proximity
Australia	0.245331
Belgium	0.930118
Canada	0.583590
Chile	0.201657
Colombia	0.392320
Costa Rica	0.381939
Czechia	0.976529
Estonia	0.982687
France	0.912035
Germany	0.956094
Greece	0.954335
Hungary	0.988017
Ireland	0.880660
Israel	0.908579
Japan	0.549584
Korea	0.586093
Latvia	0.992355
Lithuania	0.998989
Luxembourg	0.936326
Mexico	0.396859
Netherlands	0.934912
New Zealand	0.146431
Poland	0.992655
Portugal	0.844143
Slovak Republic	0.992908
Slovenia	0.966289
Spain	0.867378
United Kingdom	0.900668
United States	0.495918

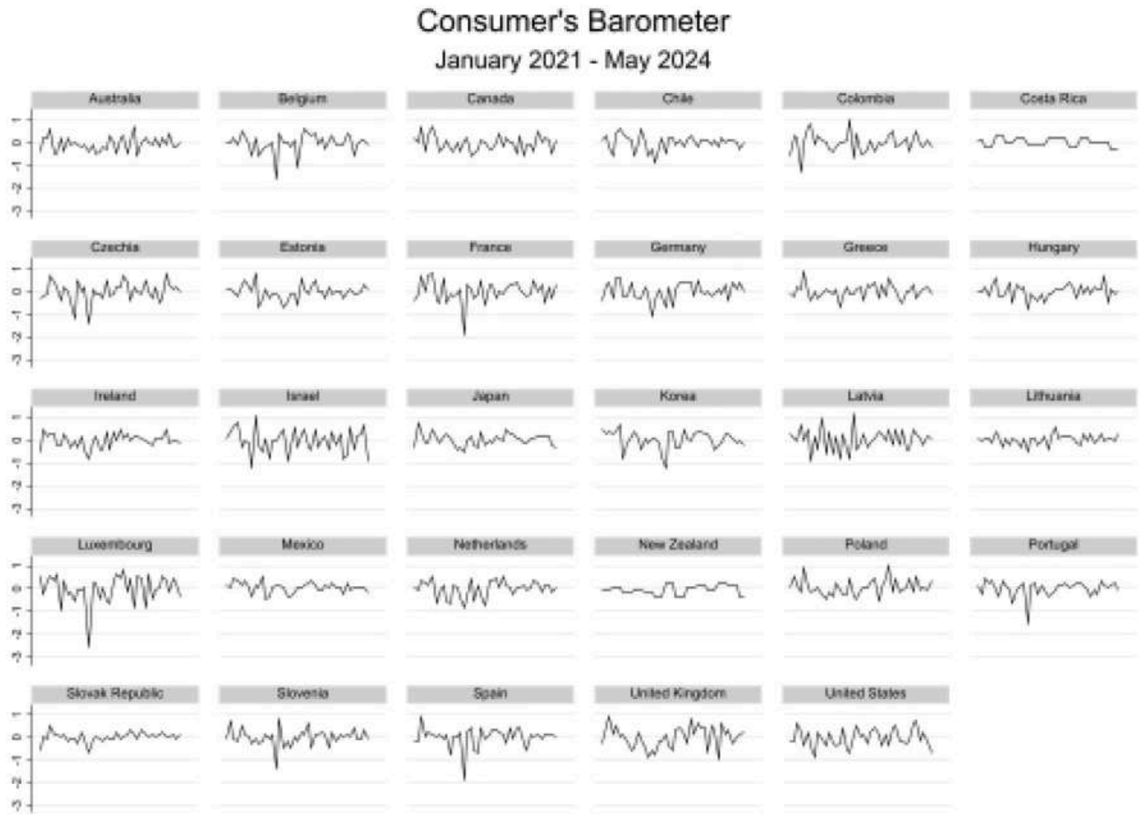

Fig. B.3. Source the OECD. The OECD consumer barometer corresponds to the monthly growth rate of the normalised consumer confidence indicator (CCI). The CCI is computed as the arithmetic average of the seasonally adjusted net balances (share of positive and negative replies) of the following four questions: 1. Financial situation over the past 12 months; 2. Expected financial situation for the next 12 months; 3. Expected generic economic situation for the next 12 months; and 4. Expected major purchases for the next 12 months.

Appendix D

See Fig. D.1 and Tables D.1–D.2.

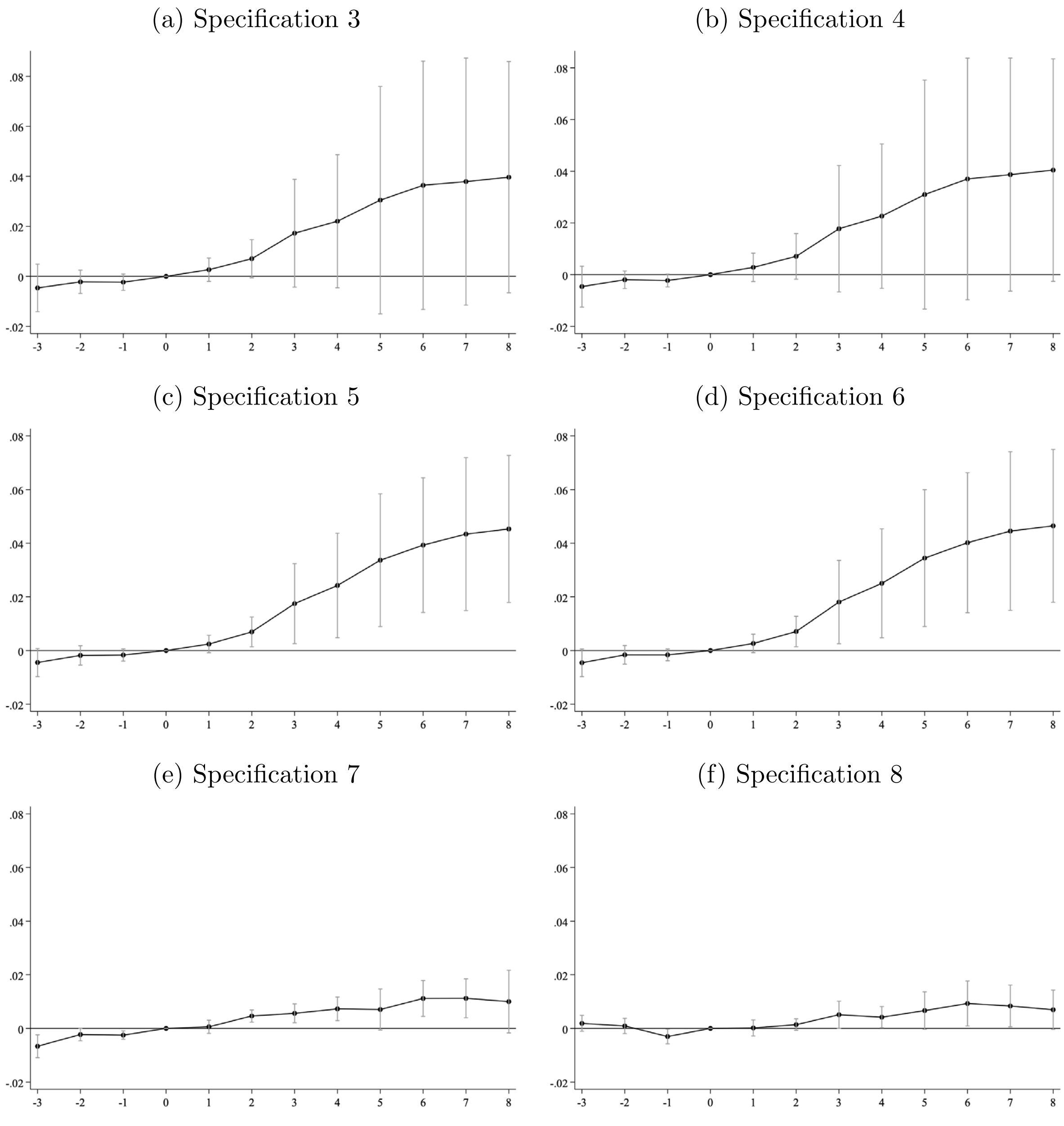

Fig. D.1. DID estimates for Table 2 Columns (3)–(8).
The figure plots the estimated dynamic pass-through of a one percent minimum wage increase on log CPI across eight post-event months ($\ell = 1, \ldots, 8$), and three pre-event months $\ell = -1, \ldots, -3$. Each panel plots the respective specifications of Table 2. The outcome variable in Specifications 3–7 is the logarithm of the CPI index; Specification 8 uses the monthly log-difference. All specifications include country and month fixed effects, and the standard errors are clustered at the country level. Specifications 3 and 4 use bootstrapped standard errors, clustered at the country level. Specifications 5 and 6 include controls for short-term interest rates and proximity to the Ukraine war. Specification 7 restricts the sample to countries with zero treatment at baseline.

Table D.1
Further sensitivity analyses for dynamic DID pass-through estimates.

	(1)	(2)	(3)	(4)	(5)	(6)
DID 1	0.0019	0.0003	0.0030	−0.0113	0.0032	0.0034
	(0.0012)	(0.0026)	(0.0020)	(0.0100)	(0.0019)	(0.0020)
DID 2	0.0025	−0.0012	0.0080	−0.0102	0.0087	0.0090
	(0.0028)	(0.0057)	(0.0034)	(0.0111)	(0.0031)	(0.0033)
DID 3	0.0125	0.0079	0.0187	−0.0403	0.0183	0.0191
	(0.0052)	(0.0065)	(0.0083)	(0.0356)	(0.0064)	(0.0067)
DID 4	0.0232	0.0176	0.0416	−0.0174	0.0213	0.0220
	(0.0085)	(0.0089)	(0.0192)	(0.0243)	(0.0071)	(0.0075)
DID 5	0.0412	0.0346	0.0648	0.0056	0.0233	0.0240
	(0.0150)	(0.0143)	(0.0273)	(0.0180)	(0.0075)	(0.0080)
DID 6	0.0642	0.0567	0.1204	0.0613	0.0294	0.0301
	(0.0211)	(0.0183)	(0.0468)	(0.0209)	(0.0094)	(0.0099)
DID 7	0.0832	0.0745	0.1221	0.0631	0.0314	0.0323
	(0.0284)	(0.0243)	(0.0459)	(0.0215)	(0.0108)	(0.0113)
DID 8	0.1032	0.0934	0.1239	0.0648	0.0332	0.0341
	(0.0346)	(0.0295)	(0.0414)	(0.0197)	(0.0117)	(0.0122)
Duration	4.36	4.36	4.36	4.36	4.36	4.36
DID −1	−0.0029	−0.0034	−0.0017	−0.0016	−0.0036	−0.0037
	(0.0014)	(0.0015)	(0.0012)	(0.0012)	(0.0011)	(0.0011)
DID −2	−0.0017	−0.0025	−0.0015	−0.0011	−0.0038	−0.0038
	(0.0034)	(0.0035)	(0.0019)	(0.0018)	(0.0016)	(0.0016)
DID −3	0.0024	0.0030	−0.0042	−0.0038	−0.0073	−0.0071
	(0.0056)	(0.0056)	(0.0027)	(0.0026)	(0.0025)	(0.0024)
Polynomial order	1st	2nd	1st	2nd	1st	2nd
Country-specific linear trends	Yes	Yes	No	No	No	No
Finite-lag horizon	6 months	6 months	9 months	9 months	6 months	6 months
Exclude BEL/FRA/CHL	No	No	No	No	Yes	Yes
Controls	No	No	No	No	No	No
N	1189	1189	1189	1189	1066	1066

Notes: Each row reports dynamic DID effects (DID ℓ) measuring the percentage change in the CPI attributable to a one-percent increase in the statutory minimum wage, ℓ months after implementation. All specifications include country and month fixed effects and cluster standard errors at the country level. Columns (1)–(2) allow for country-specific linear trends (implemented via first-differencing and cumulating back). Columns (3)–(4) re-estimate the design under a nine-month finite-lag restriction. Columns (5)–(6) exclude Belgium, France, and Chile. Placebo coefficients (DID–1 to DID–3) test the no-anticipation and parallel-trends assumptions.

Table D.2
Heterogeneity by real minimum wage and food CPI.

	(1) Real MW ≥ 3.28%	(2) Real MW < 3.28%	(3) Food CPI	(4) Residual CPI
DID 1	0.0002	−0.0007	0.0045	−0.0004
	(0.0020)	(0.0019)	(0.0022)	(0.0014)
DID 2	0.0007	0.0036	0.0045	0.0039
	(0.0027)	(0.0014)	(0.0024)	(0.0028)
DID 3	0.0102	0.0050	0.0128	0.0085
	(0.0032)	(0.0023)	(0.0053)	(0.0048)
DID 4	0.0214	0.0072	0.0155	0.0114
	(0.0064)	(0.0031)	(0.0059)	(0.0063)
DID 5	0.0247	0.0071	0.0186	0.0177
	(0.0077)	(0.0052)	(0.0065)	(0.0080)
DID 6	0.0204	0.0084	0.0251	0.0192
	(0.0074)	(0.0047)	(0.0072)	(0.0079)
DID 7	0.0204	0.0087	0.0300	0.0174
	(0.0090)	(0.0066)	(0.0090)	(0.0079)
DID 8	0.0191	0.0104	0.0304	0.0187
	(0.0084)	(0.0065)	(0.0080)	(0.0089)
Duration	4.38	4.13	4.36	4.36
DID −1			−0.0023	−0.0008
			(0.0010)	(0.0011)
DID −2			−0.0032	−0.0000
			(0.0016)	(0.0018)
DID −3			−0.0046	−0.0016
			(0.0023)	(0.0021)
Polynomial	1st	1st	1st	1st
Controls	No	No	No	No
N	615	574	1189	1189

Notes: Each row reports the DID effects which measures the percentage change in the CPI attributable to a one-percent increase in the statutory minimum wage, (ℓ) months after implementation (DID ℓ). The outcome in Columns (1) and (2) is the log CPI, assessing post-implementation pass-through effects across two subsamples: Column (1) includes countries with real minimum wage growth at or above the median (3.28%) from January 2021 to May 2024; Column (2) includes countries below the median. Column (3) uses the log of food CPI as the outcome. Column (4) uses the residual from a log–log regression of log aggregate CPI on log food CPI, so that the residual CPI captures price movements net of food-price effects (coefficient = 0.6884, SE = 0.0374, $R^2 = 0.89$). All regressions include country and month fixed effects. Standard errors are clustered at the country level.

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