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Minimum Wages in the Philippines: Stylized Facts, Identification and Estimation Challenges, and a Path Forward

by

Adolfo Jose Montesa[1], Vincent Ramos[2,1], Judy Padillon[1], Carlos Harry De Taza[1], Edgar Suguitan[1], and Rexian Irlandez[1]

[1]University of the Philippines School of Economics
[2]Heidelberg University

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Adolfo Jose Montesa*[1], Vincent Ramos*[2,1], Judy Padillon[1],
Carlos Harry De Taza[1], Edgar Suguitan[1], and Rexian Irlandez[1]

[1]University of the Philippines - Diliman School of Economics
[2]Heidelberg University

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Abstract

Since 1989, minimum wages in the Philippines have been set by regional tripartite wage boards, producing a patchwork of statutory floors whose levels, timing, and bite vary across the country. This article presents novel descriptive patterns of minimum wages, price levels, and employment using pooled Labor Force Survey microdata and the Occupational Wages Survey. We foreground three key findings. First, the real value of the minimum wage has been eroded by inflation since 1989 across all regions except NCR. Second, the minimum wage has a high bite of up to 1.2 times the median wage of the covered sector. This arguably reflects both a highly compressed wage distribution and the presence of exemption channels for smaller enterprises. Finally, in quarters succeeding wage orders, we find a reallocation pattern to wage bins just above the new wage floor, with no conclusive evidence of any immediate large-scale "displacement" of employment. Reflecting on these patterns, we lay out why unbiased estimates of employment effects remain challenging in the Philippine context and how a more holistic analysis of wage setting is complicated by the lack of transparency in and predictability of the existing wage-setting regime. We close with proposals for the path forward.

Keywords: minimum wages, stacked event-study design, reallocation, Philippines
JEL Codes: J31, J08

*Corresponding Authors. Please direct feedback and queries to both Vince (vrramos@up.edu.ph) and AJ (aamontesa@up.edu.ph). We also acknowledge and thank the Philippine Statistics Authority for granting access to the Occupational Wages Survey microdata.

1 Introduction

There is currently no consensus in the academic literature on the effects of minimum wages on employment, wages, investment, and inequality. In empirical studies before and during the early 1990s, the most influential work on this topic relied on panel data wherein (youth) employment rates are regressed on minimum wage levels. Neumark and Wascher (1992) demonstrate that a 10% higher minimum wage rate is associated with a 1 to 2% lower employment rate, and these negative relationships are disproportionately larger among more vulnerable groups (e.g., younger workers, minorities). It is unsurprising that around this period, an overwhelming majority of economists had an unfavorable view of minimum wage regulations. 87% of labor economists affiliated with the American Economic Association agreed that minimum wages increase unemployment among young and unskilled workers, with a median estimate that a 10-percent increase in the minimum wage would reduce employment by about 2 percent, the same estimate reported in the NW paper (Whaples 1996).

The 1990s were nonetheless a period of rapid developments in the minimum wage literature. A seminal paper by Card and Krueger (1994) utilized a novel neighboring state design wherein the effects of New Jersey's 19% increase in the minimum wage were analyzed by comparing its labor market outcomes to those of the contiguous state of Pennsylvania. Their results challenged both the theoretical predictions and prevailing empirical evidence of that time by demonstrating that raising the minimum wage does not necessarily increase unemployment. This paper arguably altered the minimum wage literature in the decades that followed. Recent evaluations of minimum wage nationalization tell a similar story. In the United Kingdom and Germany, national minimum wage rates were adopted only in 1999 and 2015, respectively, and ex-post analyses generally suggest that these policies primarily raised worker incomes with little to no negative impact on overall employment (Caliendo et al. 2025; Dustmann et al. 2022; Metcalf 2008).

There are multiple reasons for the muted or statistically insignificant employment effects

reported in recent literature. First, minimum wage effects are of course context-dependent, having less to do with whether or not minimum wages are increased but rather with how large and drastic these changes are. Second, these insignificant, and at times positive, employment effects align well with monopsony models in the labor market (Manning 2021), which suggest that in labor markets where firms hold significant wage-setting power, a carefully calibrated minimum wage rate can effectively raise both wages and employment. Finally, on the more empirical side, firms have multiple adjustment margins to respond to changes in input labor costs, including changes in working hours (intensive margin), consumer prices, job vacancies, non-wage amenities, profit margins, executive compensation, intra-firm wage compression, statutory non-compliance, and outsourcing, among others.

The monopsony explanation has been gaining traction. Evidence from other countries highlights the welfare gains from minimum wage reforms in contexts where firms hold considerable market power in input labor markets. This is arguably relevant for the Philippines, which has one of the highest wage markdowns in the world, with a wage share of 28% for the median manufacturing establishment (Amodio et al. 2024). This implies that for every one peso of additional output generated per worker, only 28 cents are passed through as additional wages by a firm with median-level productivity, while 72 cents accrue to the firm as a labor market rent (monopsony wedge). This excessive wage-setting power in the Philippines might also be due to high local labor market concentration, with one in four broadly aggregated markets having a high level of employment concentration (Ramos et al. 2026). This implies that policies aimed solely at increasing worker productivity are likely to be insufficient in addressing low wages if the underlying distribution of these productivity gains remains uneven.

The monopsony framework has been focal in explaining the results from recent evaluations of minimum wage effects. On its distributional implications, minimum wage adjustments reduce inequality among workers, helping close both gender and ethnic wage gaps (Lopresti and Mumford 2016; Blau et al. 2023). Influential work by Cengiz

et al. (2019) also shows how minimum wage reforms in the US have not led to massive unemployment effects, similar to findings across OECD countries more broadly (Sturn 2018). Among developing countries, similar evidence on inequality reduction and insignificant disemployment has been found in Brazil (Lemos 2009; Engbom and Moser 2022), a country that experienced a 128% increase in the real minimum wage rate between 1996 and 2018. In India, a recent study emphasized the moderating role of enforcement mechanisms, such that weak compliance can lead to null employment effects and stricter enforcement can lead to employment gains (Soundararajan 2019).

The purpose of reviewing a snippet of this large literature is not to say that policymakers can increase minimum wages uncontrollably without any labor market implications. However, there needs to be a more holistic assessment of what the data and evidence can and cannot tell us. Policymakers may take comfort in predictive analysis — what would happen to Y if we do X? This leads them to depend on either speculative assessments based on textbook models (e.g., market-clearing supply and demand) or simulation-based projections wherein assumptions arguably do the heavy lifting. In a recent article, Eloriaga et al. (2025) show that as a result of a PHP 750 national minimum wage in the Philippines, formal sector employment is projected to fall by a large magnitude ranging from 37 to 64% across regions. This projection is unsurprising, given that the model precludes alternative adjustment margins by which firms can respond to minimum wage increases such as profit margins, job vacancies, non-wage amenities, and non-compliance. By design, this ensures that any increase in the minimum wage, ceteris paribus, must reduce the quantity of labor demanded for the affected group (assuming a positive elasticity parameter) and all responses are channeled only through this margin. This is the load-bearing assumption of the model. Consequently, it would be unsurprising that for any minimum wage policy change simulated using these types of models, be it a national minimum wage or even just a one peso increase across current regional wage rates, the projection will always yield an unambiguous reduction in labor demand and an increase in unemployment.

One lesson from the literature is the need to read ex-ante projections of minimum wage effects with extreme caution. Take the case of Germany, a high-income economy with marked regional disparities in price, wage, and productivity levels (i.e., persistent "East" vs. "West" differences post-reunification) which implemented a federal minimum wage only in 2015. In the parliamentary debates leading to this policy change, opponents of the measure were citing dynamic model-based projections derived using various rich data sources, from household and firm longitudinal surveys to linked firm-employee data. In model-based projections, some authors predicted the federal minimum wage policy to reduce labor demand by 900,000 (see results from the microsimulation model of Müller and Steiner 2013). The policy was nonetheless approved by parliament, and in ex-post evaluations of the German minimum wage reform, employment effects were statistically insignificant or muted at best (see Table VII of Dustmann et al. 2022). Despite the rich data sources, why and how did these model-based projections miss the mark? Storm and Isaacs (2016) proffer a highly critical view of the National Treasury and Development Policy Research Unit (DPRU) of South Africa, stating that their negative employment projections are a "direct result of the architecture and assumptions of these models" and that "by design, these models preclude alternative outcomes and are rather unsuitable as guides to policymaking".

In lieu of ex-ante projections, we contend that the biggest gap in the minimum wage literature in the Philippines is a comprehensive and descriptive assessment of its historical patterns, which we discuss in Section 2 and present in Section 3. While we interpret historical wages, prices, and employment particularly around wage orders, we refrain from interpreting them as causal effects, due to challenges in both identification and estimation, as discussed in Section 4. These challenges form part of the reason why credible evaluations of minimum wage reforms in the Philippines have been scarce to date. Beyond these empirical difficulties, we note in Section 5 that certain institutional features of the country's minimum wage setting regime add complexity to any analysis of the topic. Concluding in Section 7, we chart a promising way forward that policymakers and researchers can build on.

2 Data and Methods

2.1 Data Sources

To establish stylized facts on historical patterns of minimum wages, prices, and employment, we draw on four data sources — regional wage orders from the National Wages and Productivity Commission (NWPC), pooled waves of the Philippine Labor Force Surveys (LFS), price data from the Philippine Statistics Authority's (PSA) chained Consumer Price Index (CPI), and the Occupational Wages Survey microdata. First, we construct a monthly panel of regional minimum wages from July 1989 to the latest available month, compiled from NWPC records of the wage orders issued by the 17 regional wage boards (National Wages and Productivity Commission nd). Since wage orders take effect on irregular dates and remain in place until superseded, each order was expanded into a monthly panel using an as-of rule (i.e., the rate in force is the most recent order whose effectivity date precedes the month of a superseding wage order). Regions created after 1989 are classified into their institutional predecessors (e.g., IV-A and IV-B inherit undivided Region IV before their own first orders; BARMM inherits ARMM; Caraga inherits Region X) so that all 17 current regions carry a continuous series back to the start of the regional wage-board era.

Second, prices are drawn from the chained Consumer Price Index (2018 = 100), with national, NCR, and "Areas Outside NCR" coverage from 1957 and full 17-region coverage from 1981. The continuity at every base-year splice (1981, 1994, 2006, 2018) was checked against expected month-on-month inflation to rule out rebasing artifacts before the series were used to deflate wages. These series are then used to construct region-specific real minimum-wage indices, rebasing each region's cumulative nominal wage growth and cumulative CPI growth to 100 at July 1989, which marked the start of the regional wage-setting regime.

Third, to study employment patterns around minimum wage orders, we draw on a national analysis of harmonized LFS microdata from Q1 2004 to Q2 2025. This results

in an analytical sample with 86 quarterly rounds and roughly 18.8 million person-quarter records, with raw region codes recoded into 17 continuous "wage-board lineages" to handle subsequent boundary changes. We use the default raising factors as weights in our analyses for representativeness. Our analytical sample is restricted to private-establishment, non-agricultural, and salaried workers aged 15–64 with positive standardized daily basic pay, since they are the ones covered by non-agricultural wage orders, by definition. This represents around 62 percent of all employed wage-worker person-quarters. Since a given regional wage board may have more than one listed non-agricultural wage rate at a time (which is true in roughly two-thirds of the quarters), and because a nominal peso does not have the same purchasing power across regions, wages and floors are converted onto a common scale before between-region comparisons. In doing so, each wage is deflated by its own region's CPI to 2018 pesos, then further divided by a spatial price index, θ_r, built from the 2018 provincial poverty-threshold ratios to NCR, yielding "NCR-2018-equivalent" pesos in which a given peso distance from the minimum wage represents the same purchasing power in every region. Finally, we supplement our analyses of wage distributions in the covered sector using microdata from the 2014 and 2022 waves of the Occupational Wages Survey, containing detailed information on establishment-level characteristics.

2.2 Stacked Event-Study Design

Using these data sources, we implement a stacked wage-bin event study modeled after Cengiz et al. (2019)'s novel distributional bunching design, which is suitable for repeated cross-sectional data that do not follow individual workers over time. The unit of observation is redefined from the worker to a PHP25-wide wage bin measured as the distance between a worker's real wage and the prevailing real minimum wage, expressed in the common scale described above. Note that since each bin's per-capita employment mass (bin employment divided by the region's working-age population) can be tracked quarter to quarter even though no respondent can, a worker-level panel is transformed to a bin-level one and, unlike

a share, allows total covered employment in a bin to rise or fall.

For this analysis, an event $e = (r, t_0)$ is defined as the first LFS round in which wage-board lineage r's highest listed non-agriculture rate increases by at least 3 percent, where t_0 denotes the calendar quarter the order takes effect. Every observation within the event window (event time $t = -4$ to $+7$ relative to the order, with a fixed pre-period average over $t = -4$ to -1) is assigned to a PHP25 bin b of its distance from that specific event's new real floor, with explicit underflow (below PHP-300) and overflow (at or above PHP625) bins so the accounting is not truncated at both ends. Formally, letting $\mathrm{WAP}_{r,q}$ denote lineage r's working-age population in quarter q, per-capita employment in event e's bin b at event time t is

$$E_{e,\tau,b} \;=\; \frac{\sum_{i \in C_{r,\,t_0+\tau},\,b}\,\omega_i}{\mathrm{WAP}_{r,\,t_0+\tau}}, \tag{1}$$

where the sum runs over all sampled workers i in lineage r's analytical sample in quarter $t_0 + t$ whose distance from the event's real floor falls in bin b. For each bin, the outcome of interest is the change in per-capita employment from its own pre-event average to its post-event average. These changes are then averaged with equal weights across the full set $\mathcal{E}$ of qualifying events ("stacking"), taking each event's own mean post-period change before averaging across events:

$$\widehat{\Delta E}_b \;=\; \frac{1}{|\mathcal{E}|}\sum_{e\,\in\,\mathcal{E}}\left(\frac{1}{4}\sum_{t=0}^{3}\Delta E_{e,t,b}\right). \tag{2}$$

Bars in Figures 5–7 plot $\widehat{\Delta E}_b$ for each displayed bin, colored by whether the bin's wage range sits at or above the new floor (blue) or below it (red), and the overlaid line is the running cumulative sum of $\widehat{\Delta E}_b$ across bins, so its value on the right edge of the displayed range is the net change in employment mass across the whole span of the plotted wage distribution (i.e., excluding explicit underflow and overflow wage bins further away from the minimum wage). Timing-based control lineages, such as regions with no change to any of their own

listed rates within ±4 quarters of the event, are used in a difference-in-differences version of the design to net out shocks common to all regions (national inflation, seasonality, aggregate demand), with each event requiring at least three such controls.

As a supplemental test for heterogeneity, we re-estimate the same event-bin design separately for sex- and education-based subsamples of the same pool of headline events, and report employment changes relative to each group's own working-age population, expressing the outcome as employed per 1,000 people to remove the influence of group size. This "rate version" of the outcome is the appropriate scale to test whether wage orders change the employment distribution of either men or women and either highly-educated or below-highly-educated adults more significantly. We report these estimates in Figures 6 and 7.

We cautiously note, as do Cengiz et al. (2019), that this method provides a better-adjusted descriptive contrast rather than a causal estimate. Across many jurisdictions, there is non-random selection into treatment timing since the issuance of wage orders usually take into account broader macroeconomic factors. Further, no lineage remains permanently untreated, and pre-trend and far-above-floor placebo checks do not pass cleanly in every specification. Hence, the net low-wage employment changes associated with these figures should be read as suggestive and imprecisely estimated rather than a true causal estimate of wage order effects. We discuss further identification and estimation issues in Section 4.

3 Stylized Facts

Stylized Fact 1: Real Wage Erosion

Across most regions, inflation-adjusted minimum wages have fallen below their baseline levels at the beginning of the regional wage-setting regime in 1989.

In Figure 1, we present a single real-wage index whereby we divide the wage growth by price growth, effectively rebasing the nominal wage growth net of inflation to the 1989 baseline. Except for NCR, wherein the real non-agricultural minimum wage growth has

been hovering around zero, minimum wages across all sixteen other regions have eroded in value relative to the 1989 baseline. This erosion is visually deepest in BARMM and in Central Visayas. However, the unusually steep decline in Central Visayas is concentrated in a short period of time between 1989 and 1995, which may partly reflect a CPI-chaining statistical artifact. Notwithstanding, minimum wages across most regions have suffered from purchasing power erosion relative to when the regional wage-setting mechanism began. This figure contradicts any assertion that regional wage-setting has historically raised the real wage floor.

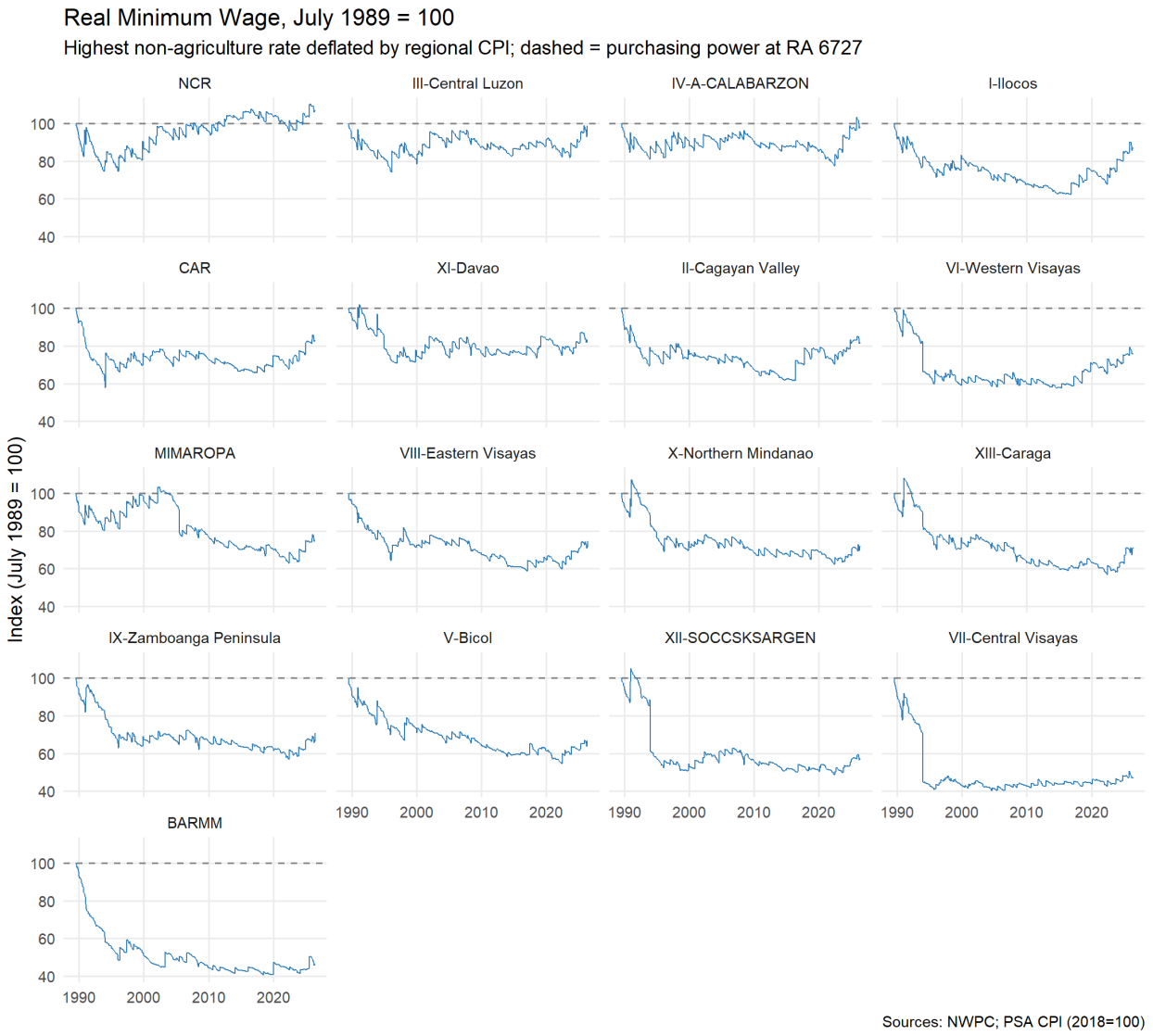

Figure 1: Real minimum wages by region, 1989–2025

In Figure 2, we present both historical CPI and nominal wage series across regions to see the pace and magnitude of divergence between price and wage growth. Visually, the gap between the price line and the wage line directly represents the percentage shortfall in wage growth relative to inflation. In Figure 2, we see a similar erosion story to that presented in Figure 1. In the vast majority of regions, the price line has far outpaced the wage line since July 1989, and only in the best-performing regions (chiefly NCR) do the two lines run close together, consistent with the broader finding that regional wage boards have, at best, defended rather than improved upon the purchasing power established at the start of the regional wage-setting era.

Stylized Fact 2: High "Bite" of the Minimum Wage

Minimum wage levels lie close to the median wages of the covered sector, but this is NOT necessarily a wage-setting issue.

Who are paid below, at, or above the minimum wage? Figure 3 classifies the covered private-sector, non-agriculture wage employment nationally into workers paid more than 5 percent below, within 5 percent of, or more than 5 percent above the prevailing regional standard rate, shown separately against the highest and the lowest rate a region may list. In the latest quarter, roughly 27 percent of covered employment sits more than 5 percent below the highest listed rate, 25 percent within the band, and 48 percent above it, while the below-band share falls to about 20 percent when measured against the lowest listed rate instead. The difference between both panels reflects the sensitivity to which of a region's possibly multiple listed rates is used as the reference. Supplementing this distribution plot, we note that our calculation of the Kaitz index (minimum wage divided by the median wage) ranges from 0.88 to 1.20 — far above the international thresholds conventionally used to mark a "meaningfully binding" floor internationally (around 0.6). In fact, in 10 of the 17 regions, the real minimum wage sits at or above the covered-sector's own real median wage.

This stylized fact has been used in policy discussions to argue that the minimum wage

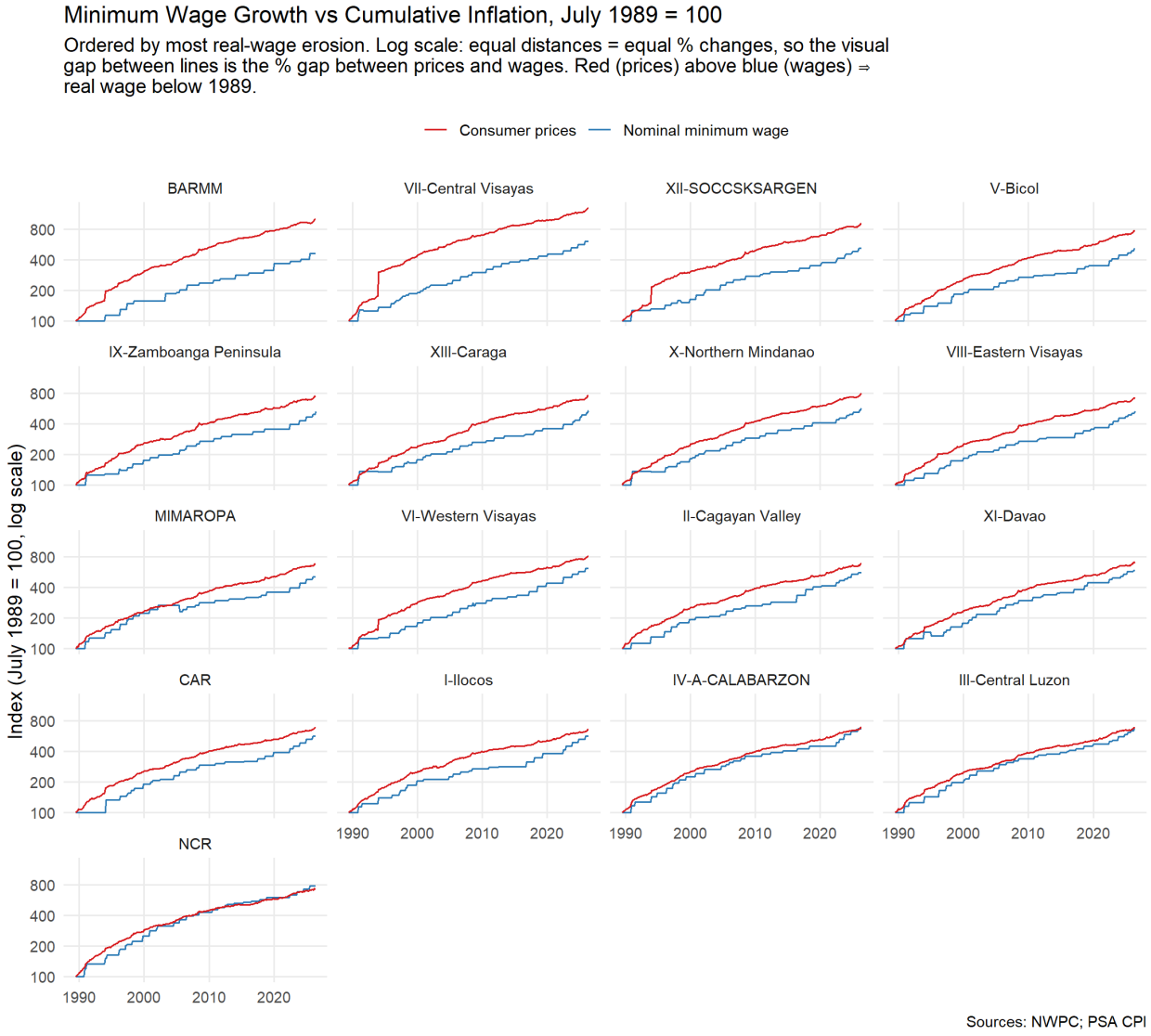

Figure 2: Minimum wage growth and cumulative inflation by region, 1989–2025

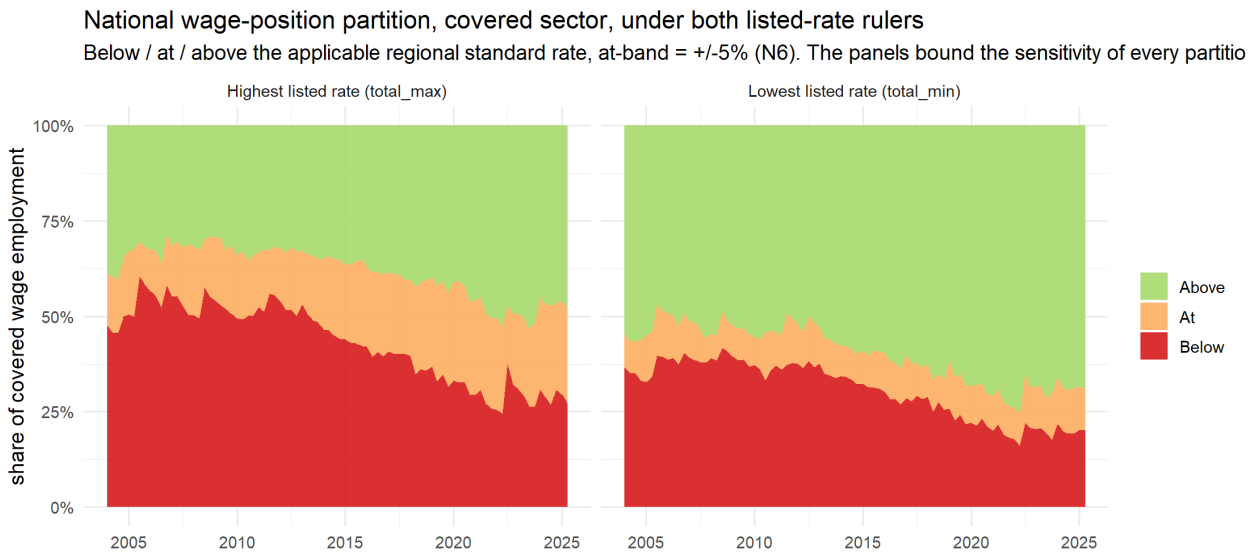

Figure 3: National distribution of wages relative to the minimum wage, covered sector

in the Philippines is "set too high". We have a different view. The high bite merely reflects other wage and employment characteristics in the Philippine labor market that affect either component of the Kaitz index; namely, legal exemptions to minimum wage coverage, noncompliance, and wage compression. First, there are legal mechanisms that provide an exemption from the prevailing regional wage rate (i.e., for microenterprises). An application for exemption from a new wage order may be filed by small businesses with 10 or fewer regular employees, or businesses located in an area affected by and declared under a state of calamity. Since we are using the LFS, we are thus unable to disentangle those working in exempt firms from those in non-exempt ones.

Second, there have also been reported issues of firms that are non-compliant with the prevailing minimum wage. DOLE's own random-inspection data have put compliance historically at around 82% (Paqueo et al. 2016). To the extent that non-compliance might be more concentrated among smaller or less visible establishments not well captured by DOLE's inspection regime, this reliance on worker-reported wages in survey data should, if anything, capture more of this non-compliance than administrative inspection statistics do. This implies that our "below" shares in Figure 3 likely reflect a mix of legal exemption and

outright non-compliance.

Beyond (legal) exemptions and (illegal) non-compliance, we also note that a high Kaitz index might also imply that even full, universal adherence to prevailing wage rates still places a large share of the workforce clustered at or above the minimum wage if there is little room in the distribution for it not to. In other words, a highly compressed wage distribution might also partly explain the high bite. Drawing on firm-reported pay statistics using the Occupational Wages Survey microdata, we show kernel density estimates of the (log) daily wage distribution that support this compression argument in Figure 4. Wages in the covered (e.g., formal) sector are tightly compressed into a narrow band rather than spreading out into a long right tail one would expect if the minimum wage sat well below a broadly dispersed wage distribution.

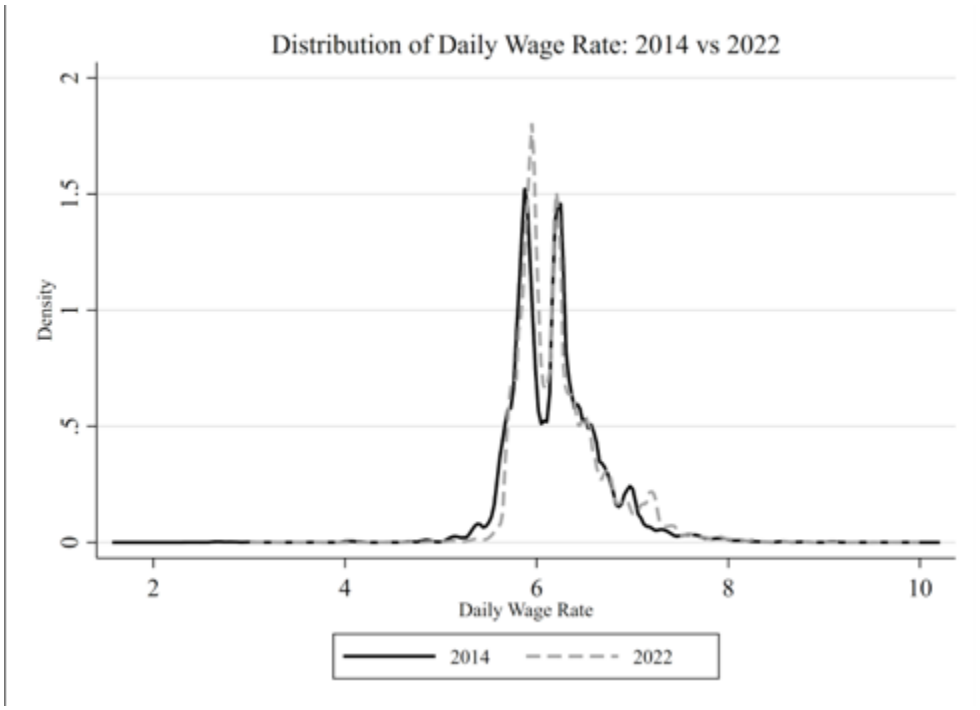

Figure 4: Distribution of daily wages in the formal sector, 2014 and 2022.
Note: Trends in daily average real basic pay of wage and salary workers from establishments with at least 20 workers, deflated by the 2018 CPI. The monthly wage rate was converted to a daily wage rate using the daily rate specified by PSA in its technical note.
Source: Occupational Wages Survey.

Comparing 2014 and 2022 suggests that the wage compression has even partly

accentuated over time, with the lower of the two peaks being taller and narrower in 2022 than in 2014. Meanwhile, the upper peak appears slightly receded, indicating that a larger share of covered workers clustered around the lower wage point by 2022. Taking this into account along with Figures 1 and 2, we note that the growing concentration of wages near the statutory floor reflects a wage structure in which more of the workforce is being pulled toward (or held at) eroding compressed wages.

Stylized Fact 3: Reallocation without Immediate Displacement

Historical wage orders reallocate workers along the wage distribution, without evidence of a large-scale immediate displacement. However, the precise net effect remains inconclusive.

Analyzing 158 historical wage orders, our stacked wage-bin event study results in Figure 5 suggest that employment mass declines in real-wage bins just below the new floor and rises in bins just above it — a reallocation pattern that holds in both the aggregate population and across sex (Figure 6) and education (Figure 7) subgroups. The scale of this reallocation is modest. In Figure 5, the raw stacked estimate implies a 0.28 percentage-point decline in below-floor employment as a share of the working-age population, which is offset by a 0.43 percentage-point increase in the mass of workers paid within PHP 100 just above the new floor. The implication is that most of the mass leaving sub-floor bins reappears just above the new floor rather than disappearing outright, as what some model-based estimators would predict. Indeed, the net change of 0.15 is statistically insignificant at conventional levels, suggesting an inconclusive, if not null, net effect of wage orders.

We note, however, that these findings should not be read as evidence that historical wage orders had no net employment effect. Indeed, one may also argue that these net reallocation estimates may simply be due to an upward-trending employment rate over time that is independent of population growth. Hence, we also present supplementary difference-in-differences specifications that net out common trends in Appendix Table 1. The credibility

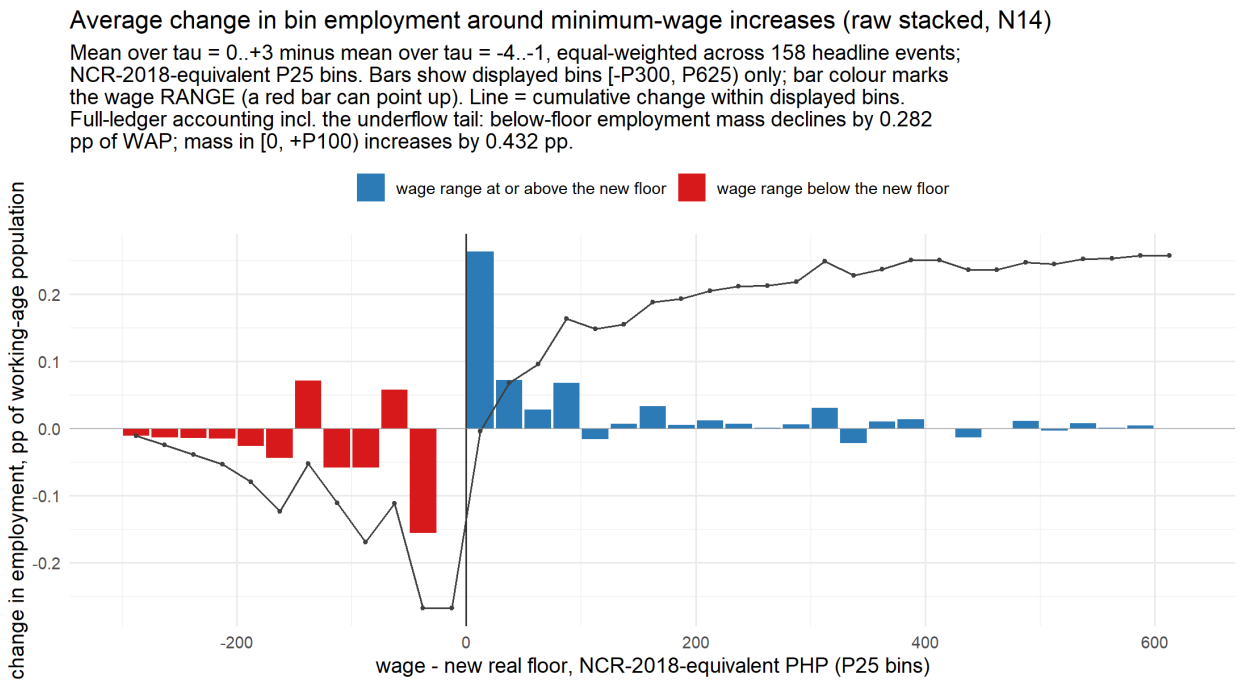

Figure 5: Average changes in bin employment after minimum wage increases

of this estimate is limited by non-random control selection and imperfect (i.e., non-parallel) pre-trend patterns. Nevertheless, these estimates also suggest a statistically insignificant net reallocation pattern, with confidence intervals being wide enough to admit both modest losses and gains. We therefore do not argue that the raw stacked estimates in Figure 5 are not biased by secular trends, only that the localization of changes near the floor and their small absolute magnitude make an aggregate overall uptrend in employment rates an unlikely sole explanation for what we observe.

Heterogeneities by Sex and Education. While previous literature makes the argument that certain demographic groups (i.e., women) are "more affected" by minimum wage reforms (Paqueo et al. 2016; Lanzona 2014), we find no evidence that either women or non-college-educated workers disproportionately lose around wage orders. Figure 6 re-estimates the event-bin design separately for men and women, expressing the outcome as the change in employment per 1,000 members of each group's own working-age population. We find that for both men and women, employment losses in bins below the new floor are offset by gains

in bins above it, and the net cumulative change is statistically insignificant for men and positive and statistically significant for women at the 5% level. This means that especially among women, we do not find evidence of a large negative displacement that prior studies have demonstrated.

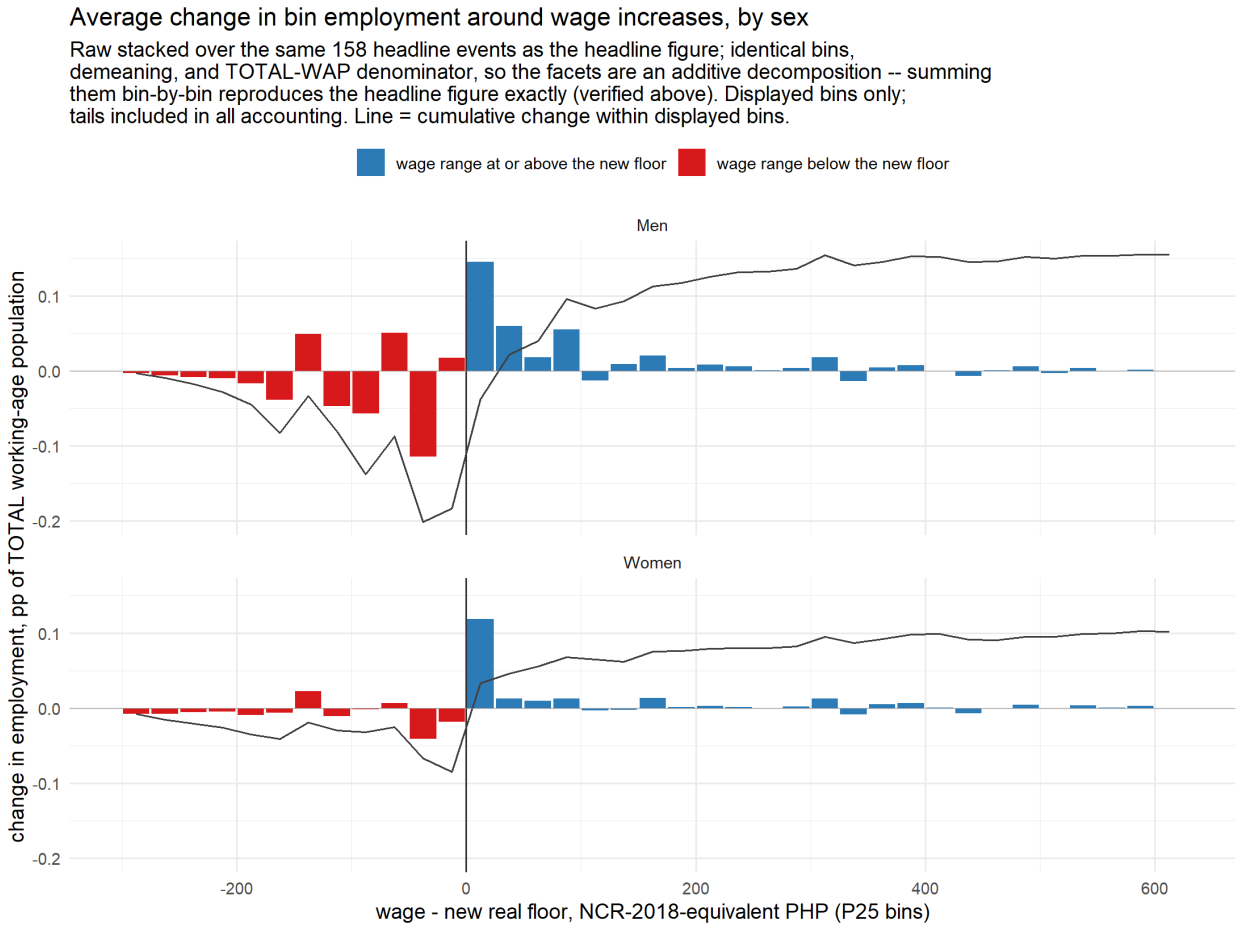

Figure 6: Average changes in bin employment after minimum wage increases, by sex

Similarly for education, the net reallocation estimates for college-or-higher-educated (0.9 per 1,000 capita, statistically insignificant) and non-college-educated workers (1.82 per 1,000 capita, statistically significant at the 5% level) suggest that the reallocation patterns of minimum wage reforms look broadly similar between the two groups. This speaks in contrast to arguments suggesting that "low-educated workers" bear the brunt of minimum wage orders in the Philippines.

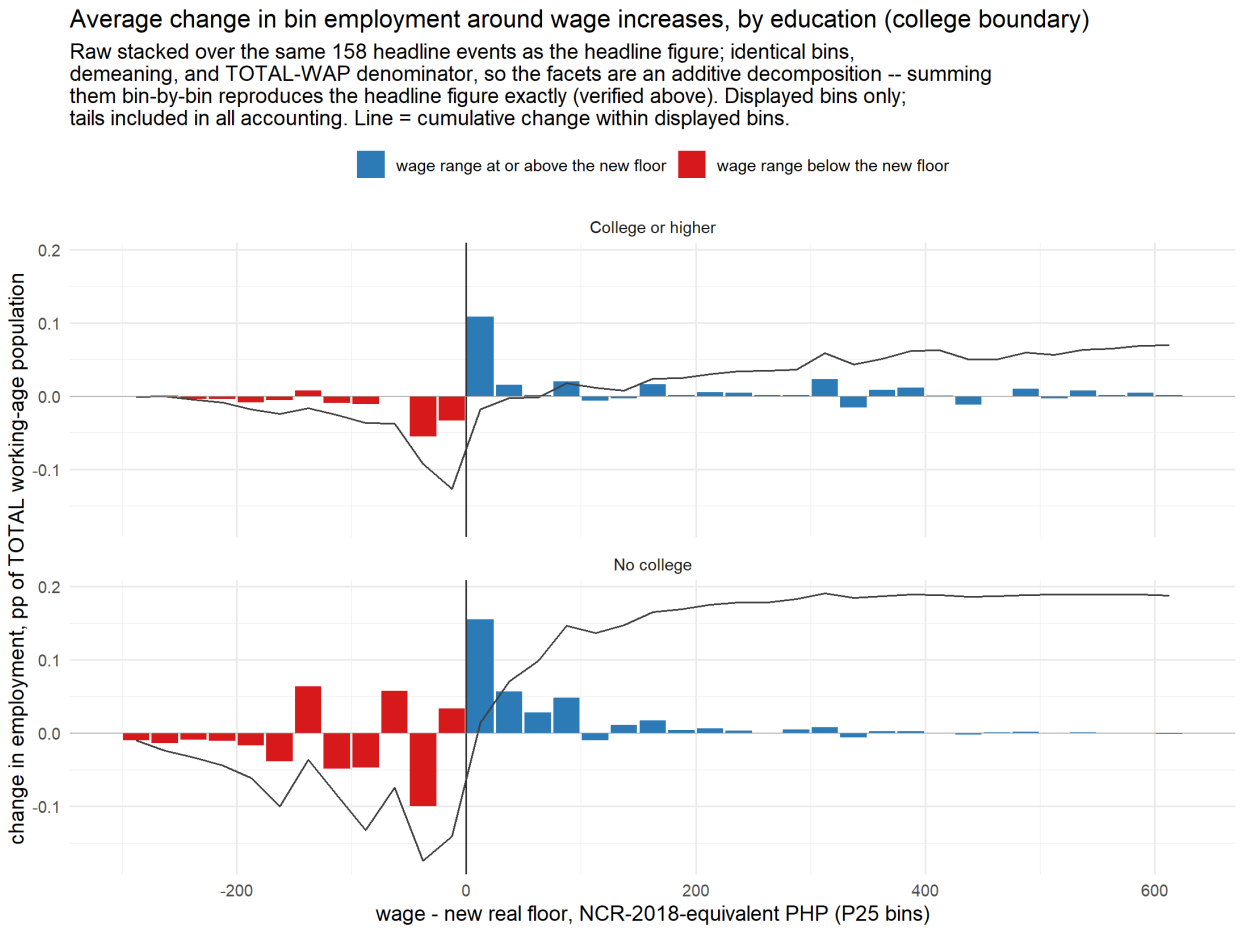

Figure 7: Average changes in bin employment after minimum wage increases, by college education

4 Causal Identification and Estimation Challenges

4.1 Identification Challenges

Despite the novel stacked wage bin approach used in the preceding section, we reiterate that we refrain from making any causal claims regarding the effects of minimum wage changes in the Philippines. Credible causal identification, in a way that is done in the state-of-the-art literature, is arguably difficult in the Philippine context. First, minimum wage changes in the Philippines are determined by tripartite regional wage boards across different periods with varying effectivity dates, scope and coverage, and absolute levels. Take Wage Order number NCR-17 issued on 17 May 2012 as an example, which provided a PHP 30 increase in non-agricultural wages in two tranches — PHP 20 effective on 03 June and the remaining PHP 10 effective on 01 November of the same year. This "splitting" might yield anticipation effects, such that outcome changes may precede the imposition of a treatment, muddying the clear causal effect of the wage order.

Contamination. Second, we do not have many "comparison regions" that can serve as "clean controls" for estimating employment effects. In Cengiz et al. (2019), stacked event study designs relied on counterfactuals that are unaffected by any contemporaneous minimum wage change, or "clean control" states that did not raise their own minimum wage within the same event window, to trace out the wage and employment trends that treated states would have followed absent the policy. Pooling across their 138 minimum wage events, this design implicitly requires that treatment and control units evolve independently of one another, such that a state's outcomes should not be influenced by whether nearby or economically linked states are undergoing their own wage changes at the same time, nor should employers and workers pre-adjust to an increase before its effectivity date. Figure 8, however, shows that there is strong contamination among treatment groups in the Philippines due to both anticipation of wage orders and spillovers from geographical proximity (e.g., NCR

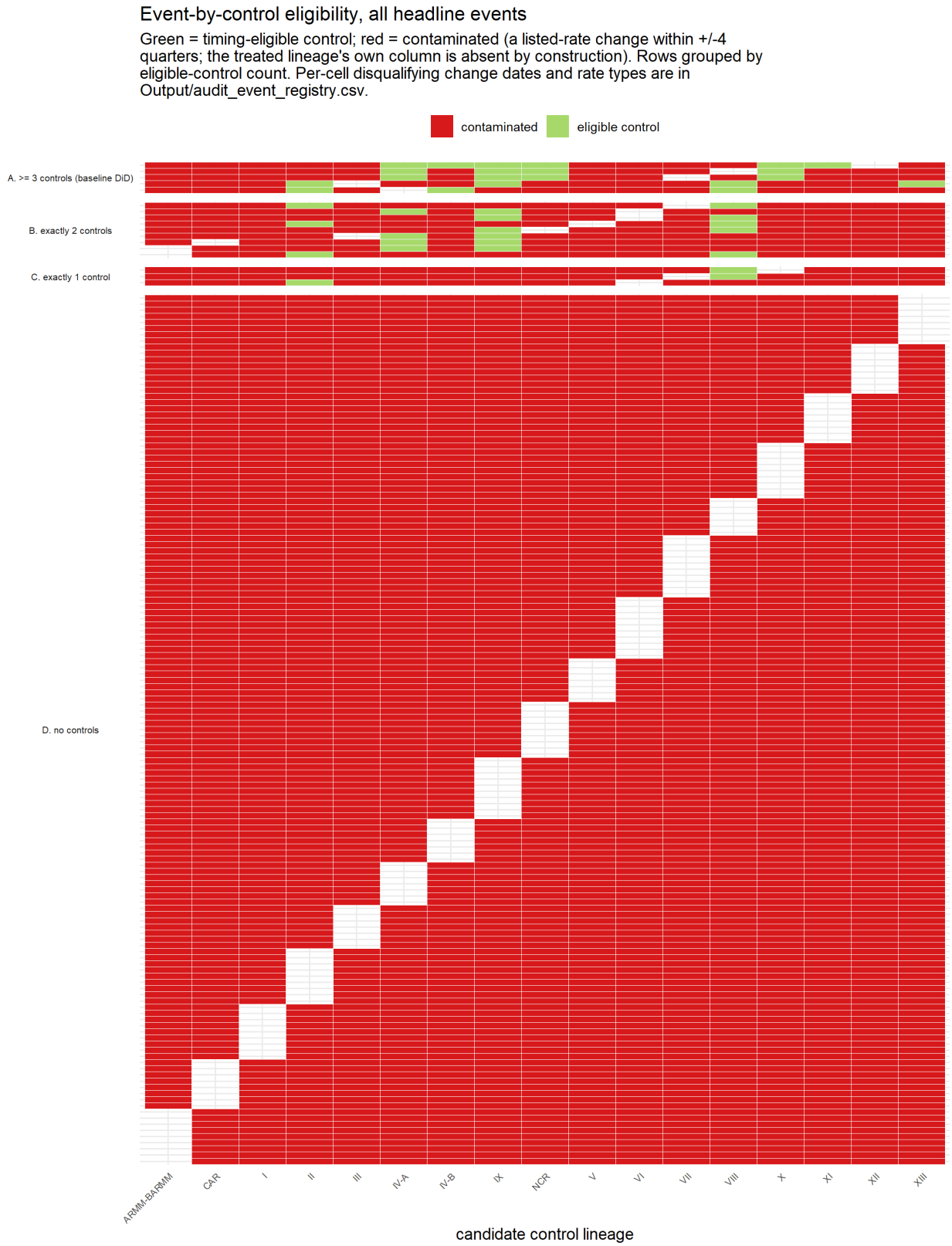

Figure 8: Historical wage orders with clean and contaminated potential comparison groups

and CALABARZON), thereby violating the assumption that units are not affected by other confounding factors.

Staggered Treatment Timing. Third, the minimum wage setting regime of the Philippines highlights another roadblock to causal identification — regions are continuously treated and remain so until subsequent wage orders. To the extent that wage orders and their treatments are recognized as staggered treatments, there exist different methods for reliable estimation (Roth et al. 2023; Sun and Abraham 2021). However, there are only very few time windows since treatment in the Philippine setting that allow for clear-cut effects in event study designs, as Figure 8 shows. Indeed, this challenge of causal estimation with 'switchers' has been addressed by newer developments in the difference-in-differences literature (de Chaisemartin et al. 2024; Liu et al. 2024; Imai et al. 2023). These augmentations to the usual event study/DiD frameworks come with a plethora of new assumptions and drawbacks given that they also target other parameters of interest (Baker et al. 2026). Nevertheless, these may still not be reliably estimated from the Philippine case due to the strength of contamination of control groups from treatment groups. The lack of reliable comparison groups or reliable event groups in the data underpowers the causal inference design from the onset, and additional assumptions may further weaken the identification strategy.

Exemptions. Fourth, what confounds causal identification severely are the legal exemptions granted to small businesses, most notably through the Barangay Micro Business Enterprises (BMBE) Act of 2002. This act allows registered micro-enterprises to legally pay below the minimum wage, driving a wedge between statutory coverage and actual compliance, further biasing estimates of employment effects. Since firms must actively apply for BMBE status through the Department of Labor and Employment, it is impossible to observe in standard LFS exactly which micro-enterprises are legally exempt. Furthermore, the BMBE asset threshold introduces a perverse incentive for firms to endogenously sort

through misreporting their assets or artificially restricting their scale to maintain their exempt status. These exemptions contaminate the quasi-experimental set-up and make it impossible to accurately define the covered sector from the data.

4.2 Estimation Challenges

Measurement Error. Apart from the above-mentioned identification challenges, we note estimation-specific ones arising from data limitations. The first issue relates to measurement error, given that wages in household survey data are self-reported. Respondents may thus rely on rounding to ease cognitive load rather than reporting their exact wages. This creates wage heaping in the data, which becomes an estimation threat. If a statutory minimum wage is set at a round number, heaping artificially inflates the count of workers appearing at that exact wage level. Consequently, it becomes impossible to separate true policy compliance from survey rounding, leading to biased estimates of the minimum wage's true impact on employment. We show in Appendix Table 2 suggestive evidence of wage heaping in the LFS data—the majority of reported wages across most regions are perfectly divisible by 50. Beyond heaping, the pay data reported in the LFS is standardized into daily rates, which means that conversion and standardization issues may arise for workers with irregular work and compensation arrangements.

Informality. The second challenge deals with the identification of informal employment. In developing economies characterized by dualized labor markets, such as the Philippines, minimum wage hikes may induce reallocation from formal to informal work. Evidence from Brazil shows that the reduction in formal employment as a result of minimum wage increases is mainly reallocated to informal employment, with no significant effect on wages (Derenoncourt et al. 2025). This creates truncation and compositional bias in traditional survey data, since informal workers are more likely not to appear in formal establishment records, and more likely to misrepresent their wages in survey data. The identification of

informality using LFS data is also imperfect, needing to rely heavily on assumptions based on other employment characteristics (Ramos 2024). Relatedly, minimum wages exert a powerful "lighthouse effect", operating as an anchor for wage bargaining even in uncovered sectors. Since informal wages are systematically pulled upward by formal wage mandates, the informal sector is inherently affected by the policy (as cited in Souza and Baltar 1979; Boeri et al. 2011). Consequently, any attempt to utilize exempt micro-enterprises or informal workers as a clean counterfactual control group will yield biased estimates, as this control group is affected indirectly by the intervention itself.

5 Regional Tripartite Wages and Productivity Boards

Beyond these empirical issues, we briefly describe other institutional features that further complicate a more holistic understanding of minimum wage effects. The regional variation in minimum wage growth vis-à-vis the CPI in Figure 2 emphasizes the localized character of minimum wage setting in the Philippines. All wage orders are discussed and decided by regional tripartite wages and productivity boards (RTWPBs) composed of representatives from the employee, employer, and government sectors. The updating process is initiated either automatically at 60 days prior to the anniversary of the last wage order, or through a petition. Although the annual review of the minimum wage level is automatic, an adjustment is not guaranteed, nor is a wage order, even a legislated one, immune from postponement. The legal standards for determining an appropriate minimum wage level are shared across all regions and account for broad and often competing factors: workers' needs and the cost of living, firms' capacity to pay, and the requirements of development. On paper, these are weighed through official statistics, consultations, and public hearings, but the actual process of weighing these factors is considerably less mechanical than it appears.

Beneath its technical framing, wage-setting under the RTWPBs is ultimately a political bargaining process. The legal guidelines of incorporating the cost of living, capacity to

pay, and development needs function more as selectively invoked anchors to justify positions in negotiation. Public hearings and consultations create the air of an evidentiary process, but the numbers ending up in a wage order are settled through closed-door caucusing among board members, where relative bargaining power and political considerations weigh as heavily as the statistics. Outside wage boards, there exist other avenues in which minimum wages are negotiated and contested, such as courts and Congress. These issues are compounded by the empirical limitations of inputs available in the first place, including limited survey coverage, infrequent data collection, and coarse regional aggregation. This creates a two-fold problem. On the one hand, decisions are structurally negotiated rather than computed, and on the other, even the resources that could discipline negotiations are too sparse and asymmetrically held (employers typically have far better firm-level cost data than labor representatives). This dynamic is most acute where information and power asymmetries within RTWPBs are largest, and wage orders consequently diverge furthest from what the cost-of-living and productivity data alone would imply. We note that as of this writing, we find no publicly available technical appendix to any wage orders nor minutes to any meetings that would better inform empirical analyses of minimum wage effects.

These limitations motivate a look at wage-setting schemes that constrain or bypass discretionary bargaining. Cross-country evidence suggests many minimum-wage-setting economies rely on expert commissions or explicit formulas rather than tripartite negotiation, with some setting rates by sector instead of geography (Dickens 2023). Indonesia offers an instructive case, since it combines an automatic formula with a bounded negotiation stage rather than eliminating bargaining outright. Under Government Regulation No. 51/2023, the annual wage adjustment is calculated as inflation plus economic growth multiplied by a labor-contribution coefficient, alpha, an index of labor's contribution to regional economic growth. Rather than being fixed centrally, alpha is chosen by each Provincial Wage Council within a government-set range, which in turn is calculated by Regional Wage Councils and submitted as a recommendation to the governor. This design preserves a role for the tripartite

council closest to local labor-market conditions, but confines its discretion to a single bounded parameter, narrowing the scope for closed-door negotiation dynamics. The Philippines could draw on this template and explore a formula that anchors wage growth to observable inflation and productivity data, paired with a rule-bound role for the RTWPBs. This would preserve local responsiveness while disciplining the negotiation process.

6 Path Forward

Given these challenges and institutional context, we lay out what may be done to elevate the discourse on minimum wages, and continue pursuing unbiased estimates of minimum wage effects in the Philippine context. To inch closer to this ideal, it makes sense for government agencies – many sitting on heaps of rich administrative and regulatory data – to invest in establishing a data infrastructure that would allow for open and transparent analyses similar to the frontiers in minimum wage research. Indeed, neither household nor firm survey data can trace how a specific worker's wage and employment outcomes evolve over time and how they are structured relative to firms. There are a number of starting points that the government ought to consider.

First, integrate the Annual Survey of Philippine Business and Industry (ASPBI) with tax and social security records using individual worker identifiers. Currently, ASPBI captures establishment-level data but does not include individual worker wage and non-wage data and persistent worker identifiers that would allow the same individual to be tracked across firms and over time. Linking these sources would create a matched employer-employee dataset that would allow for the tracking of wage trajectories and job mobility before and after minimum wage adjustments. Such a dataset would open several lines of inquiry on topics beyond minimum wage. For example, researchers would be able to conduct analyses similar to Engbom and Moser (2022) in Brazil and Bassier (2023) in South Africa, which estimate the equilibrium effects of the minimum wage increases and identify how sensitive job separations

are to wage changes in settings with high unemployment and inequality, respectively. We could also examine how much of the variation in labor outcomes (e.g., earnings and job quality) is attributable to firms versus employee characteristics (Abowd et al. 1999; Bryson et al. 2026).

Investing in this data infrastructure also means that the estimation of informality premia would be within reach. For instance, Alloush et al. (2013) find that the formality premium is about 22 percent and 12 percent in Lebanon and Syria, respectively, even after controlling for firms' and workers' characteristics and industry and region dummies. A comparable exercise could be done to establish Philippine-specific estimates, specifically when the informal sector accounts for about a third of total employment (Ramos 2024).

Second, we recommend the resumption of a rotating panel component in the LFS. While there are costs to administering a rotating panel, we believe that the potential benefits outweigh the costs. A rotating panel provides more information than cross-sectional data, allowing researchers to observe participants' wage outcomes and behaviors over time. For example, Canales (2014) used rotating LFS waves to examine the effect of minimum wage on hours worked and the probability of gaining or retaining employment using a regression discontinuity design. This approach was made possible because of a panel structure allowing the same individual to be observed before and after a treatment.

Finally, we suggest augmenting the Integrated Survey on Labor and Employment (ISLE)'s headcount question by attaching individual worker identifiers. ISLE already asks for the headcount of workers paid exactly the minimum wage, but pairing it with individual identifiers permits the tracking of real earnings histories from firms' prior responses across waves. Such an addition will convert repeated cross-sectional data into a linkable panel and allow us to assess whether the same minimum wage earner got a raise, stayed the same, or left the firm.

7 Conclusion

Public and media discourse is replete with causal assertions of how minimum wage reforms and pending proposals will affect a range of labor market outcomes. For instance, a social media infographic from the Foundation for Economic Freedom stoked fears regarding the pending 12% minimum wage hike in the National Capital Region, noting that "It will kill jobs, especially for young and unskilled Filipinos." Our reading of the existing literature and our analysis of available data suggest that such a headline is unsubstantiated. In fact, our analysis in Figure 7 suggests that college-educated (or higher) and non-college-educated workers had relatively similar and statistically insignificant changes in the employment distribution across wage bins following previous wage orders. We contend that our analyses in this article are a more up-to-date and holistic assessment of the distributional implications of historical minimum wage adjustments, rather than a narrow analysis of select regional wage orders.

We highlight the following stylized facts. First, there is a clear erosion in the real value of the minimum wage since 1989, specifically outside the Greater Manila Areas. Second, the minimum wage level has an outsized bite ranging from 0.88 to 1.20 times the median wage. This arguably reflects other wage and employment characteristics in the Philippine labor market, namely, legal exemptions to minimum wage coverage, noncompliance, and wage compression. Finally, we see no immediate evidence supporting large-scale disemployment post-hike, in the aggregate and in sex and education subgroups.

In a similar vein, we note that model-based projections that preclude alternative adjustment margins available to firms, ceteris paribus, will always yield negative macroeconomic implications, regardless of the type of reform or the magnitude of the adjustment. Whether done by government agencies or academics, simulations that do not take into account firm heterogeneities, statutory non-compliance, profit margins, and job vacancies are also highly likely to be upward biased. More often than not, these analyses lead to overstated claims about the effects of minimum wage hikes, specifically

citing disemployment, large-scale inflation, and increasing poverty. While these projections may give the impression that minimum wage hikes are detrimental, a review of the evidence indicates that this causal link is far from established.

While we contribute to the literature by providing a comprehensive overview of historical minimum wage and employment patterns, we refrain from making causal claims. By design, minimum wage setting is not an exogenous event easily isolated, and counterfactual scenarios are not easily established. This is compounded by limitations in the data infrastructure of labor reporting in the Philippines. Hence, we propose some paths forward: integrating the ASPBI with social security records, the resumption of a rotating panel in the LFS, and finally, augmenting the ISLE with individual worker identifiers. Extensions to existing surveys, such as asking respondents whether they know the effective minimum wage for their area, their classification, and exception status (informally employed or in a MW-exempted firm), would also help; in addition, perhaps granular data on hourly pay instead of just the daily pay to account for those in non-regular work hour arrangements. These go hand-in-hand with a broader call for greater data granularity, transparency and accessibility.

Looking into the long term, we believe the current minimum wage-setting institutions require revisiting. Tripartite wage boards, while well-intentioned, determine the magnitude and timing of wage orders in a seemingly discretionary and opaque manner. These negotiations reflect pre-existing power and information asymmetries beyond measurable economic conditions. Including some formula-based safeguards in minimum-wage setting, as is the case in Indonesia, could foster a more responsive and transparent, yet still flexible, minimum wage setting regime. In the spirit of recognizing the right of workers to their just share in the fruits of production, these proposals are a starting point to catalyze rigorous minimum wage and labor policy research and discourse and to reverse the decades-long erosion of the welfare of the Filipino working class.

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A Appendix

Table 1: Stacked Wage Bin Design: Main Result and Alternative Specifications

Estimator	n_{events}	Decline below floor (pp)	Increase [0, +PHP100] (pp)	Local low-wage net (pp)	Net 95% CI	Net as % of affected
Raw stacked, all headline events	158	0.282	0.432	0.150	[−0.005, 0.300]	1.526
Raw stacked, same events as DiD	5	2.435	0.777	−1.658	[−4.018, 0.144]	−12.574
DiD, equal-weight controls	5	1.935	1.379	−0.556	[−1.937, 0.341]	−4.215
DiD, pre-treatment-weighted controls	5	1.743	1.196	−0.547	[−2.020, 0.496]	−4.147

Table 2: Heaping by Lineage, Pooled (Weighted %)

Region	**Divisible by 50**	**Divisible by 100**
NCR	46.8	33.6
CAR	65.1	42.3
I	64.7	38.8
II	64.9	39.2
III	65.1	44.8
IV-A	49.7	34.0
IV-B	63.3	38.4
V	61.6	36.9
VI	55.9	32.7
VII	48.5	30.7
VIII	61.7	36.6
IX	57.3	33.9
X	53.3	31.7
XI	54.1	33.4
XII	50.0	30.0
XIII	57.6	35.2
ARMM-BARMM	70.8	40.9