

Editorial Office,
American Economic Journal: Economic Policy

RE: Article Submission

Dear Editor,

We are pleased to submit our article “Minimum Wage as a Place-Based Policy: Evidence from US Housing Rental Markets” for possible publication in the *American Economic Journal: Economic Policy*. The article has previously been submitted to the *American Economic Review* and received three referee reports. As per the *AEJ*’s submission guidelines, we are requesting the *AER* submission materials to be shared with you in the online submission form.

Also in accordance with submission guidelines, we have decided not to make any changes to the article at this stage. However, we have taken note of the comments made by the *AER* editor and the referees. We list below the main revisions we would make if we were given the opportunity to revise the article for the journal.

With regards to the review made by the *AER* editor:

1. The *AER* editor found the main results “quite striking” and seemed persuaded by our argument in favor of using granular spatial data. However, she was not convinced by the time dimension of our results. We can address these concerns by clarifying the nature of our rent measures and improving our discussion of these results.
 - Our main results are obtained using a monthly panel dataset. We find a discrete jump in rents on the month of the change in the MW variables, and no effect of leads and lags of these variables. The editor questioned the plausibility of these dynamics. The argument is that the effect should be sluggish as existing rental contracts are re-negotiated over time. Referee 3 (R3) suggests that these patterns arise because our measure of rents reflects posted prices in newly-available rental units, rather than rates of existing contracts. We agree. Furthermore, we see this as a feature of our rents measure, since rents of new contracts are more reflective of market conditions (Ambrose et al. 2015). We will revise the paper to discuss this point and emphasize that these time patterns are to be expected given the nature of our data.
 - When using a yearly model and our baseline sample of ZIP codes we find similar patterns in point estimates but no statistical significance (Online Appendix Table 3, Panel C), which the *AER* editor and R1 found troublesome. The yearly model is simply an averaged version of the monthly model, so it tries to get at the same coefficients.¹ However, statistical power is much different across these models, and in particular the yearly model uses much less identifying variation in minimum wages. In fact, comparing rows (iii) across Panels A and C of Online Appendix Table 3 for our rents outcome, we find that state-clustered standard errors are between 3.8 and 4.6 times larger in the yearly model. As a result, the yearly model does not reject our baseline estimates. We will revise the discussion and clarify the interpretation of these results.
2. The *AER* editor raised a valid concern with a particular robustness check in Panel A of Table 3. In that panel we interact the year-month fixed effects with indicators for geographies, identifying the effects of the MW measures

¹Omitting the controls for simplicity, the monthly model can be written as $\Delta r_{it} = \delta_t + \gamma \Delta w_{it}^{\text{res}} + \beta \Delta w_{it}^{\text{wkp}} + \varepsilon_{it}$, where t represents monthly dates. The yearly model can be obtained by taking early averages of the previous equation, i.e., $\overline{\Delta r}_{iy} = \overline{\delta}_y + \gamma \overline{\Delta w}_{iy}^{\text{res}} + \beta \overline{\Delta w}_{iy}^{\text{wkp}} + \overline{\varepsilon}_{iy}$ where y represents years and \overline{x}_y is the average of x over months in year y . In principle, these models should estimate the same parameters.

off of within-geography comparisons. Rows (c) and (d) use county and CBSA (i.e., metropolitan area), and we find noisier but largely consistent results. In row (e) we use state and find that the standard errors are larger and the signs of point estimates flip. In the paper (p. 22), we argue that this may happen because “within-state comparisons are not appropriate,” involuntarily casting doubt on our identifying assumption. While it is true that the models in Panel A of Table 3 rely on slightly different assumptions, we see how this imprecise discussion generated confusion. In reality, as we formally test in Table 1 (included in this letter), this model cannot reject our baseline estimates. We think that the absence of pre-trends in Figure 4, the “stacked” model discussed in Section 5.2, the non-parametric estimates in Online Appendix Figure 9, and the fact that the demanding models in Panel A of Table 3 are generally consistent with our baseline, are strong indications in favor of our identification assumptions. We will revise the discussion of these results to, first, drop the imprecise claims on inappropriate comparisons in the state-by-year-month model and, second, make it clear that these models are statistically indistinguishable from our baseline.

3. Finally, the AER editor suggested that we “oversell [our] results regarding the negative impact of the residence MW on rents,” and indicated that more evidence on the price channel that according to our model underlies said negative impact is needed. R2 made comments in the same direction, which we discuss below. We note here that the goal of the paper is *not* to estimate the effect of the MW on the prices of local, non-housing consumption. In fact, to our knowledge aggregate data on prices of local consumption at the ZIP code level is not publicly available. In the paper we cite related literature that explores this channel (e.g., Leung 2021). We included this variable to account for spatial heterogeneity which both our model and the empirical results suggest is important, especially so in some variations of our model (e.g., the heterogeneity analyses in Table 5 and the “stacked” model in Online Appendix Table 4). We will revise the paper to note that due to data constraints we cannot test this channel directly and that more work is needed to conclusively establish that the negative coefficient arises from changes in local prices.

With regards to the main comments made by the referees:

1. All three referees expressed concerns with respect to some aspect of the results. On top of the already mentioned issue with timing, there are two types of concerns:
 - *Plausibility of effect size:* R1 suggests that our estimated elasticities of rents to the MW are too large in light of the effects on income. On the other hand, R3 correctly points out that we misinterpreted Figure 4 of Agarwal et al. (2022), whose estimates actually imply an elasticity an order of magnitude larger than ours. R2 asks for an “accounting exercise” using the structure of our model in Section 2 to make sure that the estimates of the effect of the residence MW (γ in the paper) are plausible. We discuss the plausibility of our estimated effects in Section 5.5. We will revise that section, correct our interpretation of Agarwal et al. (2022), and expand our discussion. First, we will compare the magnitude of our rent-to-minimum-wage elasticities with literature estimates of the effect of the MW on income. Comparing elasticities across these outcomes is tricky since one needs to take into account expenditure shares.² Second, we will incorporate

²To illustrate, consider a budget constraint for of the form $PC + RH = Y$, where P is price of consumption, C is a consumption aggregate, R is the rental price of housing, H is a housing aggregate, and Y is nominal income. Differentiating this equation with respect to the minimum wage \underline{W} , and for simplicity assuming no changes in C and H , we get

$$s_C \eta_{PW} + s_H \eta_{RW} = \eta_{YW}$$

where $s_C = PC/Y$, $s_H = RH/Y$, and the η 's are elasticities. Our estimate of η_{RW} in Table 2 is 0.0685 (we pick the largest elasticity to the workplace MW), and of η_{YW} in Appendix D is 0.01. If the housing expenditure share is 1/3, then $s_H \eta_{RW} = 0.0685/3 = 0.023$ and $s_H \eta_{RW} / \eta_{YW} = 0.23$. Hence, this simple calculation implies that 23% of the new income generated by the MW goes to housing. However, as we point out in Section 6, the housing expenditure shares and rent elasticities will vary spatially.

the accounting exercise suggested by R2 to discuss the plausibility of γ .³

- *Selection*: R1 expressed concerns that selection of listings into the Zillow data may be driving the results. We will include a new robustness analysis that uses a newly available variable in Zillow that directly attempts to control for selection of listings. We provide more details on this new variable below, where we discuss alternative sources of data more generally.
2. We also received comments regarding our estimates of the effect of the MW on income. These estimates, discussed in Appendix D and displayed in Online Appendix Table 7, show that a 10% increase in the workplace MW leads to a roughly 1% increase in wage income in a ZIP code. This estimate is an input of the counterfactual analyses in Section 6. Thus, we will expand Section 6 by adding a figure that shows how the conclusions of the counterfactuals change with different values of the elasticity of income to the minimum wage that can be justified based on different assumptions and estimates in the literature.
- *Magnitude of elasticity of aggregate wage income to the MW*: R1 suggested that our estimates are too large relative to Cengiz et al. (2019). Our illustrative comparison in Appendix D assumes a share of MW workers of 15% and finds that our estimates are of similar magnitude to Cengiz et al. (2019).⁴ R1 thinks we should use the share of MW workers in the wage bill, “around 6%” (p. 2). However, if there are spillovers above the MW (as suggested by Cengiz et al. 2019), the share of the wage bill affected by a MW increase will be larger.⁵ We will revise this comparison and discuss this point. We will expand our literature review to get a better sense of the available estimates of the effect of the MW on wage income. We will base our sensitivity analysis for the counterfactual exercises on the range of estimates collected from the literature.
 - *Estimates in Appendix D*: Both R1 and R2 had comments on our analysis. R1 suggests to implement an event-study analysis to increase the credibility of these results. R2 argues that we should show “the effect of an increase in the minimum wage on wages *relative to contiguous jurisdictions*” (page 3). By including CBSA by time fixed effects, we show increases in wages relative to jurisdictions in the same metropolitan area. We are willing to pursue additional analyses if you think that estimating the wage income elasticity in a different way would be valuable.
3. Comments related to the model in Section 2.
- R3’s first comment argues that assuming perfectly flexible adjustments in the intensive margin of housing demand (as we do) seems contradictory with the “short-run” nature of the analysis where commuting shares are fixed. R3 also points out that the counterfactual policy evaluation would be invalid unless this issue is addressed.
 - Our initial reaction is that, in our static model, people are allowed to move within a ZIP code as long as commuting shares don’t change. Appendix A presents a simple extension with a time dimension and people signing new contracts every 12 months where this dynamic is modelled explicitly.⁶ Proposition 2 would also hold in a model where workers are allowed to move to different locations but the

³R2 suggests to construct an estimate of the rise in cost of living following a 10% increase in the MW, and compare that to our estimate of γ (which implies that rents would decline by 0.22%). We will collect aggregate expenditure data and combine it with estimated elasticities of prices to the MW from the literature to make this calculation.

⁴Using a state-level analysis, Cengiz et al. (2019) find that “8.6% of workers were below the new minimum wage” in the 138 MW events they study (Section II.C). Our rough estimate using ACS data (for 2014, taking into account local MW policies as well) implies that the average share of MW workers across ZIP codes in the main estimation sample is 15%.

⁵Cengiz et al. (2019) find spillovers up to \$3 above the new MW, and estimate that they account for 40% of the wage effect of the MW. Aaronson and French (2007, Table 1) estimate that the share of the wage bill affected by the MW in the restaurant industry is 0.17 (the share of MW workers in the industry is 0.33). Dube (2019, Table 4) finds positive effects of the MW on up to the 30th quantile of the distribution of family incomes 3 years after the increase.

⁶We emphasize that, as long as workers can in principle move, prices will respond even if they decide to stay in the same housing unit.

commuting shares do not change as a result. We will revise the text in Section 2 to make these points more salient.

- We do observe changes in commuting shares annually, and use time-varying commuting shares to construct our workplace MW measure in Panel B of Table 3, finding very similar results. The difference between the time-varying shares workplace MW and the fixed-shares one is very small, and is centered around zero (the 95% confidence interval goes from -0.0052 to 0.0052). As a result, we think that changes in commuting shares are not playing a large role. We will address this comment more directly by adding an appendix where commuting shares are explicitly allowed to change as response to the MW, which will result in a proposition analogous to Proposition 2 that contains a new term summarizing the effect of the MW on commuting. We will argue that the extra term will be of second order and can thus be safely ignored for our counterfactual analysis.
- R3’s second comment is that we over-emphasize the theoretical novelty of our model. We will revise the text to frame the model as a tool for interpreting the empirical analysis and will moderate our claims of novelty.
- R3 also points to an imprecision in the proof of Proposition 1. We will of course fix this.

4. Other comments related to the empirical results.

- R2 notes that the residence MW does not take into account where residents of a ZIP code shop, it simply assumes that they shop in their own ZIP code. (In footnote 11 we mention a possible extension to the model where this would be allowed.) We are not aware of origin-destination data on consumption intensity between ZIP codes, but we do have access to data on the amount of retail activity in each ZIP code, which we can use to check for heterogeneity in our findings.

We now discuss alternative sources of data. In addition to the sensitivity analysis we discuss above, we plan to explore two data sources that have become available since the first version of our paper.

1. We will add estimates using Zillow’s new monthly rental housing index (ZORI). This index, based on methodology by Ambrose et al. (2015), directly attempts to control for changes in the composition of posted rents on the platform by estimating change in rents for the same rental unit over time. We hope that analyses based on this variable provide more confidence on our conclusions, in particular with respect to the concern that selection of listings may be driving our results (as suggested by R1).
2. We will pull ZIP code-level monthly data on rents from a newly available API by Realty Mole since 2020. These data provide the average rent and also incorporates the number of units with a different number of bedrooms that make up that average, potentially allowing us to study changes in the composition of rentals. The downside of these data is its short time span. We will do our best to perform additional sensitivity analysis with these data despite its limitations.

Finally, we could also make the following changes if you think they would be an improvement.

1. R1 argues that most “workers in the USA economy are not MW workers and so the aggregate effects on rental prices will be quite limited” (p. 2) and asks to discuss the household structure more through the lens of the model. We acknowledge that the model abstracts away from household structure, the reason being that our commuting data can’t be mapped to households as it counts number of *jobs* between origin-destination pairs. We show in the paper that low-income households are much more likely to be renters (Figure 3), and that rents

per square foot are surprisingly constant across income deciles (Online Appendix Figure 2). In our view these figures suggest that a non-negligible effect of the MW on rents per square foot is to be expected. To further strengthen this case we will add a new online appendix figure showing that, for low-income households, the household head is usually low-income as well (and thus likely affected by the MW).

2. R3 points out that the effects of the MW are found in housing units “not occupied by poor workers,” and thinks this may be worrisome. This claim comes from Online Appendix Table 6, where we estimate our model in different housing categories in the Zillow data. We discuss those results in Section 5.4, where we warn against strong conclusions as these estimates are quite noisy. We note that these rental categories are inhabited by low-income households as well (Online Appendix Figure 3), though we acknowledge that the share of low-wage households in some categories is relatively lower. R3 suggests that a possible reason these effects are there is that many MW workers may not reside in poor households. We will construct a new appendix figure showing the share of low-wage workers in each household by household income to discuss this point.
3. R2 (point 3) asks to show that effects are increasing in the share of MW workers. In Section 5.3 we show that the estimates are increasing in the *standardized* share of MW workers. We will provide in the text the value of the standard deviation of this variable to better interpret these numbers. We could also include heterogeneity estimates without standardizing the share. However, we prefer the standardized versions as they allow comparing heterogeneity analyses using different variables (see Table 5).

We thank you for considering our submission and look forward to hearing back from you soon.

Sincerely,
Gabriele Borg
Diego Gentile Passaro
Santiago Hermo

Table 1: Comparing the state by year-month model with the baseline model

	State-time	Baseline	Diff.	SE	<i>t</i> -value	<i>p</i> -value
Workplace MW	−0.0162	0.0687	−0.0849	0.0687	−1.24	0.2168
Residence MW	0.0176	−0.0199	0.0375	0.0338	1.11	0.2668

Notes: Data are from the baseline estimation sample. The measure of rents per square foot corresponds to the Single Family, Condominium and Cooperative houses from Zillow. Let “st” denote the model with state by year-month fixed effects, and “base” the baseline model. Then, the first row tests the null $\beta^{\text{st}} - \beta^{\text{base}} = 0$, whereas the second row tests the null $\gamma^{\text{st}} - \gamma^{\text{base}} = 0$. The first column shows point estimates of the model with state by year-month fixed effects. The second column shows point estimates of the baseline model. The third column shows the value of the difference of the relevant coefficients, and the fourth column shows the standard error of the difference. The fifth and sixth columns show the implied *t*- and *p*-values. We constructed these tests by estimating both regression equations jointly, including economic controls from the QCEW as in the paper. Standard errors in parentheses are clustered at the state level.

References

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