

April 6, 2023

Editorial Office,
American Economic Journal: Economic Policy

RE: Article Submission

Dear Editor,

We are pleased to submit our article titled “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 timing of our rent measures and providing complementary evidence using alternatives measure of rents.
 - Our main results are obtained using a monthly panel dataset. We find a discrete jump in rents right on the month of the change in the MW variables, and no effect of leads or 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 we find these time patterns because our measure of rents reflects posted prices in newly-available rental units, rather than rental rates of existing contracts. We agree. We will revise the paper to make this point clearer and emphasize that these 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 no significant results (Online Appendix Table 3, Panel C), which the *AER* editor and R1 found troublesome. This model is simply an averaged version of the monthly model, so it tries to get at the same coefficients.¹ In fact, we find that our baseline point estimates are included in the confidence interval of the yearly model, however the standard errors of the yearly model are much larger. The reason is lack of power. First, the number of observations is much lower: if N is the original sample size, these estimates use $N/12$ observations. Taking into account that the standard error is inversely proportional to the square root of the number of observations, we expect standard errors to be $\sqrt{12} \approx 3.46$ times larger. Comparing columns 2–4 for rows (iii) in Panels A and C of Online Appendix Table 3, we find that standard errors are between 3.8 and 4.6 times larger. Second, as we

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

mention in the paper, sometimes the change in the MW happens in the middle of the year (most commonly, July). As a result, the yearly averages in these models are often taken over treated and untreated months, smoothing the key identifying variation. The intention of including these estimates was to illustrate the importance of using monthly data to identify the elasticities we are interested in. This model is not well suited to estimate the elasticity of average yearly rents to the MW, which seems to be the interpretation of the AER editor and R1. It was our error to generate a false expectation around these estimates. We will revise the discussion and clarify the interpretation of these results.

- Another approach to the comments above could be to use alternative data sources to obtain more suitable rent measures. Unfortunately, reliable measures of average per-square-foot rents among existing contracts in each ZIP code are not publicly available, neither at the month nor year levels.² However, we can use imperfect alternative measures to construct robustness exercises for each of these issues. For the first issue we will use a monthly rental housing index constructed by Zillow that uses posted rental prices and the structure of the housing stock in a ZIP code to proxy for average rents of existing contracts. The downside of this variable is that it is smoothed over time, so we would expect the effect to occur before the MW change. In fact, this is what we find. We also find that the effects last for a few months. For the second issue we may use the Fair Market Rents county-level data. This dataset uses ACS surveys and other data sources and attempts to capture the 40th percentile of rents in a given year and county.³ While, per our arguments in favor of granular spatial units, we prefer not to use counties, we could explore this data to try to capture the effect of the MW on a more consistent statistic of the distribution of yearly rents. We will pursue this additional analysis only if you see it as a valuable addition to the paper.
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 off of within-geography comparisons. Rows (c) and (d) use county and CBSA (i.e., metropolitan area), and we find noisier but consistent results. In row (e) we use state by year-month fixed effects, and find that the coefficients turn statistically insignificant and their signs flip. We note that the baseline point estimates cannot be rejected in this model. In the paper we argue that this estimate may be biased due to undesirable comparisons, without giving much evidence in favor of this interpretation. We will revise the discussion and frame it in terms of our identifying assumptions.⁴ We will also provide evidence in favor of it.
 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 consumption prices.⁵ In the paper we cite related literature that explores this channel (e.g., Leung 2021). The point of including this variable is to show

²The main publicly available dataset with rents data at the ZIP code and year levels is Small Area Fair Market Rents from the US Department of Housing and Urban Development. Unfortunately, these data are not measures of current rents but imputations based on ACS demographics from previous years, so they are not useful for studying the effect of the MW on rents at the ZIP code level. (See footnote 20 in the paper.)

³This dataset is used by the working paper version of Yamagishi (2021). In the published version the author dropped the analysis using the FMR data and focused on his data from Japan.

⁴Some details on how we plan to frame this argument in terms of the identification assumptions of the model. For our main estimates we assume strict exogeneity: we require that, on average across ZIP codes in the data, the error term in the regression is uncorrelated with each MW measure, conditional on the other. Estimates controlling for geography by year-month fixed effects make the stronger assumption that this holds within each geography. In the case of states most of the variation in MW measures comes from local MW ordinances. As a result, ZIP codes exposed to the residence MW are located in jurisdictions with high statutory MW changes, whereas those not exposed are located in different jurisdictions. If positive trends in rental prices are present in the jurisdictions that pass the MW ordinance (violating the required strict exogeneity assumption), they would bias upwards the effect of the residence MW, explaining the results in row (e) in Panel A of Table 3.

⁵In fact, to our knowledge data on prices of consumption at the ZIP code level is not available.

spatial heterogeneity which the empirical results suggest is important, especially in the heterogeneity analyses in Table 5. As a result, we will re-write the introduction and the results section to note that more work is needed to 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 trickier than suggested by R1 since one needs to take into account expenditure shares.⁶ Second, we will incorporate the accounting exercise suggested by R2 to discuss the plausibility of γ .⁷
- *Selection:* R1 expressed concerns that selection of listings into the Zillow data may be driving the results. We can address this comment in two ways. First, we will incorporate the analysis using a Zillow rental index discussed earlier. This index directly attempts to control for selection of listings into the data, so similar results using this variable should alleviate this concern. Second, while we do not observe the number of listings for rent, we do observe the number of listings *for sale*. Using this variable as an outcome we find statistically insignificant effects of the MW measures. Under the assumption that selection for rents follows a similar pattern than selection for sales, this analysis suggests that the selection story is less likely. If useful, we can add to the paper the analysis using listings for sale as a new robustness check.

2. We also received comments regarding our estimates of the effect of the MW on income. These estimates are discussed in Appendix D, and show that a 10% increase in the workplace MW leads to a roughly 1% increase in wage income in a ZIP code. Before going over the comments, we note that the goal of the paper is *not* to estimate the effect of the MW on wage income, but rather to pick a sensible value for the parameter ε in Section 6. However, we understand that an inaccurate value for this parameter would lead to incorrect conclusions in the counterfactual exercises. As a result, we will expand Section 6 by adding a figure that shows how the conclusions of the counterfactuals change with different values of ε . As for Appendix D, we can justify our estimates of the elasticity of income to the MW from the literature, so we are willing to drop it entirely. If you believe that providing valid estimates of this parameter is an important contribution, we could either maintain Appendix D with minor additions or deep dive in the analysis and move it into the main paper.

⁶Consider 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 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 $s_H \eta_{RW} = 0.0685/3 = 0.023$. 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.

⁷R2 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.

- *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).⁸ R1 thinks we should use the share of MW workers in the wage bill, around 6%. 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.⁹ 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 for the estimates in Online Appendix Table 7. On top of our baseline estimate, that table shows that the effect of the workplace MW is increasing in our estimate of the share of MW workers in a ZIP code, and shows null effects of the MW on aggregate dividends, as one might expect. R2 argues that we should show “the effect of an increase in the minimum wage on wages *relative to contiguous jurisdictions*” (page 3). We include metropolitan area by time fixed effects in those estimates, so we show increases in wages relative to jurisdictions in the same metropolitan area. To show an increase relative to “contiguous jurisdictions” we could add a specification that interacts the time fixed effects with smaller geographies, such as *place* or *county*. We think that showing sensitivity of the counterfactuals to this elasticity would suffice for the scope of the paper. However, we are willing to pursue these analyses if you think that estimating the wage income elasticity robustly would be valuable.

3. Comments related to the model in Section 2.

- R3’s first comment is 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. We see the point. Our first reaction is that, in our 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.¹⁰ We also note that we could allow for households moving to nearby locations outside their own ZIP code as a response to the MW. In that case we would end up with an extra term for the changes in commuting shares in Proposition 2. This term would go to the residual in our main models. We 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 similar results. We will revise the text in Section 2 to incorporate this discussion.
- R3’s second comment is that we over-emphasize the theoretical novelty of our model. While we find Section 2 helpful in interpreting our empirical strategy, we agree that it’s far from a modern spatial model. We will qualify our claims of novelty, and stress further that it’s a useful tool to shed light on the empirical analysis.
- R3 also points to an imprecision in the proof of Proposition 1. We will of course fix this.

⁸Using a state-level analysis, Cengiz et al. (2019) finds 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%.

⁹Cengiz et al. (2019) find spillovers up to \$3 above the new MW. Aaronson and French (2007, Table 1) estimates 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.

¹⁰We emphasize that, as long as workers can in principle move, prices will respond even if they decide to stay in the same location.

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.) This would introduce measurement error in the estimates, as using the residence MW instead of the true “shopping MW” would lead to bias. Unfortunately, data on consumption by origin and destination is not publicly available. We will address this comment by discussing the issues introduced by this measurement problem. If you think it would add value to the paper we can also explore heterogeneity of our estimates by a measure of retail concentration in each ZIP code.

We could also make the following changes if you think are needed, although we do not feel they are essential to the revision.

1. 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).
2. 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 could 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).
3. 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 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 can provide some evidence for this constructing a new appendix figure that shows the share of low-wage workers in each household by household income.

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

Sincerely,
Gabriele Borg
Diego Gentile Passaro
Santiago Hermo

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