April 3, 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 comments 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.

    - 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, relying on the referee reports, 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 more clear and emphasize that these patterns are to be expected given the nature of our data.
    - When using a yearly panel and our baseline sample of ZIP codes we find no significant results (Online Appendix Table 3, Panel C). The AER editor and R1 found this troublesome. Specifically, we find that the coefficients in these models have the "correct signs," but the standard errors are much larger. The reason is that these estimates are underpowered. First, as we 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 oftentimes taken over some treated and some untreated months, smoothing the key identifying variation. Second, while the relatively small number of ZIP codes is not a problem in our main estimates with a large number of months, it is when we collapse at the yearly level as each ZIP code has only a few number of time-period observations. We will revise our discussion of these results to emphasize these points further in the paper.
    - A better solution to the comments above would be to use an alternative data source for a rent measure. Unfortunately, reliable measures of average rents among existing contracts in each ZIP code are not publicly available.[1] We do have access to a monthly rental housing index constructed by Zillow that uses posted

[1]The main publicly available dataset with rents data at the ZIP code and year levels is Small Area Fair Market Rents from the US Department

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 will include estimates using this variable as another robustness check in the paper, making clear the caveats associated with it.

2. The AER editor raised a valid concern with a particular robustness check in Panel A of Table 3. In row (c), when we control for state by year-month fixed effects, the coefficients turn statistically insignificant and their signs flip. This model is identified by comparing the evolution of rents between ZIP codes exposed to a MW change vs. ZIP codes not exposed *within a state*. The problem is that ZIP codes exposed to the MW are likely to be located in cities (where the within-state variation in the MW originates), and those not exposed are likely to be rural. In other words, these ZIP codes are located in different housing markets. Consequently, the estimates obtained may reflect differential local trends rather than the effect of the MW. We will revise the discussion of these results in Section 5.1 and provide evidence in favor of this interpretation.

3. Finally, the AER editor suggested that we "oversell [our] results regarding the negative impact of the residence MW on rents." While that negative coefficient is consistent with our model where raising local prices decreases housing demand, we do not provide any direct evidence in favor of this channel. One reason for that is data availability: we are not aware of price indexes dissagregated at the ZIP code level. (R1 suggests using survey data such as PSID or CEX. R2 suggests using price data from Diamond and Moretti 2023. These datasets vary at the much broader geography: the commuting zone.) We will re-write the introduction and the results section to qualify these results.

With regards to the main comments made by the referees:

1. All three referees expressed concerns with respect to the plausibility of the results. On top of the already mentioned issue with timing, there are two types of concerns:

   - *Selection*: R1 expressed valid concerns that selection of listings into Zillow may be driving the results. We will address this comment in three ways. First, we will revise the text to discuss the problem and argue that, since we observe consistent effects in many housing categories (Online Appendix Table 6) we think this story is not likely. Second, the analysis using the Zillow rental index that we will add to the paper directly attempts to control for selection, so similar results using this variable should alleviate this concern. Third, 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 effect of the MW measures. If useful, we can incorporate these results in the paper as a new robustness check.
   - *Plausibility of effect size*: R1 suggests that estimates are too large. R2 asks for an "accounting exercise" using the structure of our model in Section 2 to make sure that the estimates are plausible. R3 correctly points out that we misinterpreted Figure 4 of Agarwal et al. (2022), whose estimates actually imply an elasticity much larger than ours. We discuss the plausibility of our estimated effects in Section 5.5. We will revise that section to incorporate an accounting exercise using estimates of the effect of the MW on prices from the literature (Leung 2021). We will also correct our interpretation of Agarwal et al. (2022).

2. We also received comments regarding our estimates of the effect of the minimum wage 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 wages in a ZIP code. We detail the comments and our proposed changes below:

of Housing and Urban Development. However, these data are not measures of current rents but imputations based on ACS demograhpics from previous years, so they are not useful for studying the effect of the MW on rents. (See footnote 20 in the paper.)

- R1 suggested that our estimated effects on income 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). 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. R1 also suggests to implement an event-study approach and study pre-trends in those estimates.
- R2 argues that we should show "the effect of an increase in the minimum wage on wages *relative to contiguous jurisdiction*s" (page 3). We include metropolitan area by time fixed effects in those estimates, so we show increases in wages relative to jurisidictions in the same metropolitan area. To show an increase relative to "contiguous jurisdictions" we could add a specification that controls for *place* by time or *county* by time fixed effects, which would show an increase relative to locations in the given geography.
- We note that the we estimate the elasticity of wages to the minimum wage in order to use it in the counterfactual section. The main goal of the paper *is not* to estimate the elasticity of income to the MW. Furthermore, our estimates are roughly in line with the literature, meaning that changing our estimated elasticity by the one found in Cengiz et al. (2019) will not affect the counterfactual exercises. As a result, we would prefer not to expand this appendix further. Of course, we can do so if you think it would add value to the paper.

3. Comments related to the model in Section 2.

   - The first comment by R3 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. We will modify the text to make two additions. First, we will point out that a model of bargaining between landlords and renters would yield similar predictions (namely, a positive effect of the workplace MW and a negative effect of the residence MW). Second, we will mention that we don't actually need fixed commuting shares. Instead, we need their changes (if any) to be uncorrelated with changes in the MW measures. Thus, workers don't necessarily need to stay in their original residential location.
   - The second comment of R3 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 mostly a tool to shed light on the empirics.
   - 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 minimum wage 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 problems 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 the editor thinks are appropriate, 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 standard deviation 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 minimum wage 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 househols are much more likely to be renters (Figure 3), and that rents per square foot are suprisingly constant across income deciles (Online Appendix Figure 2). We think that these figures suggest a non-negligible effect of the MW on rents per square foot is to be expected. To further strenghten 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 argues that the effects of the MW are found in housing units "not occupied by poor workers." We argue in the paper, and show in Online Appendix Figure 2, that the rental categories are inhabited by low-income households as well. However, we acknowledge that the share of low-wage households in some categories is relatively low. We are also aware that these estimates use a much smaller sample. As a result, we qualify the interpretation of these results in Section 5.4. We can revise the text to qualify these results even further.

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

Sincerely,
Gabriele Borg
Diego Gentile Passaro
Santiago Hermo

References

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