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Reply to Jason Sorens on Housing Supply Deregulation

Greg Howard[1] and Jack Liebersohn[2]

LINK TO ABSTRACT

Jason Sorens (2025) argues for decreasing regulation to increase the supply of housing, a typical view among economists. In particular, he claims that three recent papers do not support this view (Howard and Liebersohn, 2021; Gyourko and McCulloch, 2024; Harris and Lin, 2024), and presents arguments against each of their conclusions.

In this note, we argue that our paper (Howard and Liebersohn 2021) should not be taken as against housing supply deregulation. We also briefly comment on the specific criticisms mentioned by Sorens (2025). We conclude with an argument agreeing with Sorens (2025) that understanding the population elasticity to local rents is a critical elasticity for the literature and an encouragement to researchers to continue to develop methods to estimate that elasticity.

Howard and Liebersohn (2021) is not an argument against more housing

Sorens (2025) begins with an overview of why local increases in housing supply might lower the local cost of housing. He correctly reasons that when demand is very elastic (e.g. many people will move in because of lower rents), then the effect of housing supply is small on local house prices and rents. He correctly points out that Howard and Liebersohn (2021) estimates demand to be very elastic—the preferred estimate in that paper is infinity. He concludes that Howard and Liebersohn (2021) could be used to argue against increasing housing supply via local deregulation.

[1] University of Illinois. glhoward@illinois.edu
[2] University of California-Irvine. cjlieber@uci.edu

We do not agree with this conclusion. We do agree that under the parameters of the model, a local increase in housing supply in one city will have negligible effects on local rents. However, that is because the rent decrease, instead of being localized to the city that implemented it, is spread across many places. When the initial city increases in population, that lowers demand for the places that people moved from. So instead of a large decrease in one city, we have a small decrease in many cities. If the goal of a policymaker is to make housing more affordable in general, then building more housing is effective regardless of the population elasticity.

This can be seen explicitly in Section 2.2 of Howard and Liebersohn (2021). Define σ_i to be the local housing supply elasticity in city i, and λ to be the (common) demand elasticity for the quantity of housing.[3] In the extreme cases of perfectly elastic or inelastic populations, the national change in rents in response to an increase in housing supply is given by:

$$\text{Average Change in Log Rents} = \frac{\mathbb{E}_i \text{Log Increase in Housing Supply}_i}{\mathbb{E}_i \sigma_i + \lambda}$$

when population is perfectly elastic, or

$$\text{Average Change in Log Rents} = \mathbb{E}_i \frac{\text{Log Increase in Housing Supply}_i}{\sigma_i + \lambda}$$

when population is perfectly inelastic to rent. $\mathbb{E}_i$ indicates an average over all cities in the U.S. For intermediate population elasticities, the average change will be somewhere in between, but the math is less clean. In any case, more housing supply brings down the average rents by similar amounts. The main difference is whether those declines are local to the housing construction or not.

In our model, the first-order welfare effects on renters (not inclusive of landlords) are directly proportional to the average change in log rents. The welfare benefits of more housing to renters are present—and of similar magnitudes—regardless of whether populations are highly elastic or highly inelastic. While the population parameter does change the welfare consequences if we focus on only local renters, we would take the stance that housing deregulation advocates should care about the welfare of others too. Under an inclusive welfare criterion, the population elasticity has very little effect on the benefits of increased housing supply.

To summarize, a careful reading of Howard and Liebersohn (2021) would not conclude that increasing housing supply is ineffective at raising affordability or welfare. The gains are simply spread nationally instead of locally.

[3] In other words, if the rent goes up by 1%, people will consume λ% less housing quantity.

Addressing specific criticisms from Sorens (2025)

Additionally, Sorens makes two criticisms of the methodology in Howard and Liebersohn (2021). The first is that our main regression used to estimate the population elasticity to local rents is not sufficiently well-identified. We are sympathetic to this criticism and would welcome better identification, a point that we will revisit later. As we wrote in the paper, there is an "ideal" way to estimate this parameter which would require exogenous shocks to housing supply, and our methodology is not that. Sorens particularly mentions that our estimate using Bartik (1991) shocks is almost statistically significant ($t = 1.59$). However, the sign of this estimate suggests that wages have *larger* effects on rents in more elastic cities, not the *smaller* effects that would imply a smaller population elasticity. If anything, this specification provides stronger evidence of a high population elasticity.

The other criticism that Sorens makes is that some share of the population is living in shrinking cities, and that in these cities we should expect housing supply to be particularly inelastic. He suggests that the right thing to do would be to drop these cities from the analysis. We are sympathetic to this concern. However, only 7 percent of the 2000 U.S. population lived in cities that would shrink over the next 18 years (the time frame of Howard and Liebersohn (2021)).[4] Given this low share, we doubt it would have much of a quantitative impact on the main results. We would, however, be very interested in the results of a reanalysis along the lines that Sorens proposes.

The importance of the population elasticity

We are glad to see more discussion of what the population elasticity is to rents. One of the main points of Howard and Liebersohn (2021) was that this is an incredibly important parameter for understanding rent increases and that there is not consensus in the literature on what the right value should be. As Sorens points out, this is a critical parameter to understanding whether the rent decreases due to construction will be concentrated locally or spread nationally. And for Howard and Liebersohn (2021), it was important to understand how much of past rent increases were due to increased demand to live in housing-supply-inelastic regions. Other important economic questions, such as the incidence of local tax changes and the population effects of localized shocks, are also dependent on this parameter.

While our estimate is close to infinity, we do not think that our empirical

[4] 20.5 percent lived in New Orleans, which should arguably not count since much of its housing supply was destroyed in Hurricane Katrina.

strategy is foolproof. It comes with big confidence intervals, and we would welcome new strategies and perspectives on ways to estimate this parameter. In fact, our future research clearly demonstrates that we do not truly believe this number is infinity; some of our more recent papers use other parameterizations or focus on reasons why people do not move to better opportunities (e.g., Howard, Liebersohn, and Rodrigues, 2025; Howard and Weinstein, 2025; Howard, Liebersohn, and Ozimek, 2023; Howard and Shao, 2025; Liebersohn and Rothstein, 2025). While the estimation strategy we used in Howard and Liebersohn (2021) was the best we could think of, we do not think that it should be taken as the last word on the topic.

We greatly appreciate Sorens's analysis regarding this elasticity, and we would encourage him to formalize it to estimate a specific number. One substantial difference between his case studies and our analysis is that ours takes place over 18 years, whereas all of his case studies have a shorter time frame. We share his prior that a longer time frame would lead to a higher elasticity, so it is conceivable that the analyses are not contradictory. Separately, his regression analysis of population growth rates on cost of living are suggestive of a *dynamic* spatial model that we would highly encourage him to estimate. Our *static* model would suggest a regression of changes in population on changes in cost of living (not levels) so it is not clear how to interpret his results as a measure of the elasticity of interest.

We hope that Sorens and other researchers will continue to develop new estimation strategies to give us a better sense of what this population elasticity truly is.

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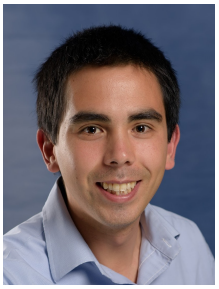
About the Authors



Greg Howard is an Associate Professor of economics at the University of Illinois, with a Ph.D. from MIT in 2018. He is an urban and macroeconomist. His work has estimated causal effects of internal migration on U.S. cities, used spatial general equilibrium models to better understand aggregate implications of local shocks to housing markets, proposed new models of internal migration for use in quantitative modeling, estimated causal effects of regional universities on the local economy, and tested empirical predictions of rational inattention in data-rich real-world settings. His email is glhoward@illinois.edu.

Jack Liebersohn is an Assistant Professor of economics at the University of California, Irvine, with a Ph.D. from MIT in 2018. He is an urban and financial economist. His work has examined the effects of mortgage rate lock-in on geographic mobility, evaluated the role of financial technology in shaping access to credit, measured the impact of banking mergers on deposit rates, and studied mortgage repayment behavior. He combines structural and reduced-form methods to study how financial frictions and regional mobility affect urban and regional outcomes. His email is liebersohn@gmail.com.

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