Alliance for
ECONOMIC
FAIRNESS

July 26, 2023

City Council, City of Los Angeles
200 N. Spring Street
Los Angeles, CA 90012

RE: Council File: 14-1371-S13: Los Angeles Living Wage Ordinance (LWO) / Los Angeles Hotel Worker Minimum Wage Ordinance (LA HWMO) / Wage Increase / Health Care Credit / Public Housekeeping Training / Amendments

Dear Council Members,

On behalf of the diverse coalition comprising the Alliance for Economic Fairness, we are submitting the attached study from Professor Seth J. Hill from UC San Diego examining the relationship between cities' minimum wage policies and homelessness.

The Alliance for Economic Fairness is a broad coalition of businesses and community leaders who support an equitable approach that helps workers, protects jobs, and maintains critical tax revenue. The conclusions from this study are vital to consider as the City Council examines the impact of its proposed $30 Wage Ordinance, and we respectfully request that this study be included in the City Council's file. Additionally, our coalition continues to strongly support the City Council's action to request a report from the Chief Legislative Analyst (CLA) on the economic impact of the ordinance.

Professor Hill's study provides significant insights into the potential consequences of city-led minimum wage increases, particularly as they relate to the vulnerable segments of our population.

- The research concludes that municipal minimum wage increases are correlated with higher point-in-time homeless population counts, likely due to workers losing their jobs and rising rental housing prices.
- The report highlights that large minimum wage increases are likely to hurt the lowest-skilled workers who already face housing insecurity and whose marginal revenue product falls below the wage floor. These workers tend to face hardships such as low skills, low education, disabilities, criminal records, drug addictions, and mental illness, making them particularly susceptible to losing their jobs when the minimum wage is dramatically increased. This, in turn, puts them at risk of losing their housing.

- As a result, minimum wage-induced disruptions for a small number of individuals can cause meaningful increases in homelessness.
- Professor Hill's study reveals a compelling correlation between municipalities that increased minimum wages by more than $2.50 per hour from 2013 to 2018 and a subsequent average increase of 23 percent in homeless counts between 2014 and 2019, relative to municipalities with no change.

Another report from UC San Francisco examining the comprehensive causes of homelessness found that loss of income stemming from events such as losing a job is one of the leading reasons a person loses their housing.

These findings underscore the need for a comprehensive assessment of the potential impacts that raising the minimum wage by such a substantial amount could have on the most vulnerable workers within the hospitality industry. It is crucial to ensure that the City Council's actions do not inadvertently exacerbate the challenges faced by those already struggling at the economic margins.

We hope that Professor Hill's research will assist the City Council in striking an equitable balance for the long-term economic stability of Los Angeles.

Thank you for your attention to this matter.

Sincerely,

Alliance for Economic Fairness

Coalition Members:

- Apartment Association of Greater Los Angeles
- Asian Business Association of Los Angeles
- Asian Industry B2B
- Beverly Hills Chamber of Commerce
- Building Owners and Managers Association of Greater Los Angeles
- Community RePower Movement
- Compton Chamber of Commerce
- Crenshaw Chamber of Commerce
- Glendale Chamber of Commerce
- Greater San Fernando Valley Chamber of Commerce
- Hallmark Aviation Services
- Hotel Association of Los Angeles
- La Cañada Flintridge Chamber of Commerce
- Latino Restaurant Association
- Long Beach Area Chamber of Commerce
- Los Angeles Area Chamber of Commerce
- Los Angeles County Taxpayers Association
- National Association of Minority Contractors, Southern California
- Northeast Los Angeles Hotel Owners Association
- South Gate Chamber of Commerce

- Southern California Black Chamber of Commerce
- The Greater Los Angeles Hospitality Association
- Valley Industry & Commerce Association
- Warner Center Association
- West Hollywood Chamber of Commerce
- Westside Council of Chambers of Commerce
- California Asian Pacific Chamber of Commerce
- California Black Chamber of Commerce
- California Building Industry Association
- California Hispanic Chambers of Commerce
- California Hotel & Lodging Association
- California Restaurant Association
- California Retailers Association
- California Travel Association
- Airlines for America
- American Hotel and Lodging Association

CC: Mayor Karen Bass
Rachel Freeman, Deputy Mayor for Business and Economic Development
Mercedes Márquez, Chief of Housing & Homelessness Solutions
Holly Wolcott, City Clerk
Petty Santos, Executive Officer, Office of the City Clerk
Sharon Tso, Chief Legislative Analyst
Matthew Szabo, Chief Administrative Officer
Dr. Va Lecia Adams Kellum, CEO, Los Angeles Homeless Services Authority

Minimum Wages and Homelessness

Seth J. Hill*

June 16, 2023

Abstract: America's cities continue to struggle with homelessness. Here I offer a factor, the minimum wage, that adds to existing individual and structural explanations. If there are negative distributional consequences of minimum wages, they most likely harm the lowest-skill workers many of whom already face housing insecurity. To evaluate this argument, I study minimum wage changes in American cities and states 2006 to 2019. Using difference-in-differences methods for staggered treatments I find that minimum wage increases lead to increased point-in-time homeless population counts. Further analysis suggests disemployment and rental housing prices, but not migration, as mechanisms. Scholars and policymakers who aim to understand and combat homelessness should consider labor market opportunities. Distributional consequences of minimum wage laws also merit further inquiry.

Keywords: Minimum wage; employment; housing security; economic insecurity.
JEL No: J08, J38, J68, R0

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Minimum wages are a common policy tool aimed at increasing the living standards of low-wage workers. Mayor Ed Murray of Seattle, Washington wrote that a 2014 city ordinance that initiated a graduated increase in the minimum wage from $9.32/hour to $15/hour would "lift tens of thousands of families out of poverty and make Seattle more equitable and affordable for all who live here (Murray 2014)."

Economists, however, are less certain than Mayor Murray that higher minimum wages reduce poverty or increase equity and affordability. Some, in fact, would suggest effects in the opposite direction. A multi-decade debate has produced mixed evidence with some studies suggesting that minimum wages harm employment and others that they do not.

While employment might be the consequence of minimum wage laws most often evaluated (e.g., Cengiz et al. 2019; Clemens and Strain 2021; Clemens and Wither 2019; Dube 2019; Jardim et al. 2022; Meer and West 2016; Manning 2021; Neumark and Shirley 2022), economists have also looked at alternative effects (e.g., Beauchamp and Chan 2014; Braun 2019; Brown 1988; Clemens 2021; Coviello, Deserranno, and Persico 2022; Derenoncourt and Montialoux 2021; Fone, Sabia, and Cesur 2020; MaCurdy 2015; Renkin, Montialoux, and Siegenthaler 2022). An increased minimum wage might drive declines in nonwage compensation such as fringe benefits, job flexibility, or incidental experience at the workplace. An increased minimum wage might influence prices or customer service, might cause substitution of higher-skill for lower-skill labor, and might lead to increased loitering or petty crime from young males losing hours or employment. This work highlights that looking only at the aggregate employment effects of minimum wages could miss important distributional consequences.

If there are negative distributional consequences of minimum wages, they most likely fall on the lowest-skilled workers whose marginal revenue product falls below the wage floor. The causes of low-marginal revenue product – low skills, drug addictions, mental illness – likely also cause other hardships meaning that negative consequences of minimum wages might more often fall on those already struggling at the economic margins.

There could be different and compounding negative consequences of minimum wage increases for Americans at the margins. Low-skill workers could lose employment; could retain employment but see hours or benefits reduced; could have others in their support network experience employment disruptions; or, could face higher prices in the goods and services – for example housing – consumed by low-wage workers. Employment or support network disruptions might be particularly challenging for those with mental illness or drug addiction, two factors that harm marginal revenue product and relate to housing insecurity.

Aggregate labor market effects of the minimum wage need not be large to have economically important influence on homelessness. Even in the cities with the largest homeless populations, the count of the homeless is small relative to the total municipal population with housing and economic insecurity. Minimum wage-induced disruptions for only a small number of individuals can cause meaningful increases in homelessness even if many workers benefit from increases in the minimum wage.

Of course, it is also possible that labor markets hew towards monopsony where firms with market power pay wages below marginal revenue product. In this case an increase in the minimum wage would not negatively harm labor markets and could, in fact, increase employment and wages. Without disemployment, other labor market disruptions, or changes in prices, we might not expect increases in minimum wages to increase homeless populations. Increased employment might even decrease homelessness if low-skill workers are drawn into the labor force.

To estimate the effect of minimum wages on American homeless populations, I use variation across municipalities in minimum wage increases during the 2010s. Some American cities and states raised minimums during the decade, with increases of large magnitudes, while others kept their minimums pegged to the federal minimum, which stayed at the same nominal level after 2009. Figure 1 plots time-series of minimum wage annual averages from 2006 to 2019 for the 42 localities (first seven rows) with the highest

2019 minimum (data from Vaghul and Zipperer 2021). The plot shows that many localities increased their wages during this period, but that the timing and magnitude of increase varies considerably. The final row presents six examples of localities in states that followed the federal minimum wage, which rose from $5.15 per hour to $6.55 on July 24, 2008 and then to $7.25 on July 24, 2009, without subsequent increase. The inflation-adjusted minimum, of course, declines subsequent to 2009 in these localities.

Using Department of Housing and Urban Development (HUD) data on point-in-time homeless populations, I examine the relationship between changes in local minimum wages and local homelessness across 100 American municipalities from 2006 to 2019. Because of the staggered nature of rollout in increases to minimum wages across localities, traditional two-way fixed effects estimators could yield biased estimates. In a first analysis, I implement an event-study design by defining policy cohorts by changes in minimum wages during calendar time windows. The design compares homeless counts pre-window to homeless counts after the increase relative to no-change localities, removing staggered comparisons from the design.

Municipalities that increased minimum wages by up to $2.50 per hour from 2013 to 2018 saw an average increase of 14 percent in homeless counts in the years 2014 to 2019 relative to municipalities with no nominal change in the minimum wage (real decline) or with changes pegged to inflation (real no change). Municipalities that increased minimum wages by more than $2.50 per hour from 2013 to 2018 saw an average increase of 23 percent in homeless counts in the years 2014 to 2019 relative to municipalities with no change. A dynamic version of this analysis suggests the increase in homeless counts increases as time passes.

In a second analysis, I use what Baker, Larcker, and Wang (2022) call a "stacked regression estimator." I define treatment as a year-over-year increase in the minimum wage of at least $0.75 per hour (90th percentile). The stacked estimator creates event-specific data sets to eliminate the staggered treatment problem. Increases of $0.75 or more in local min-

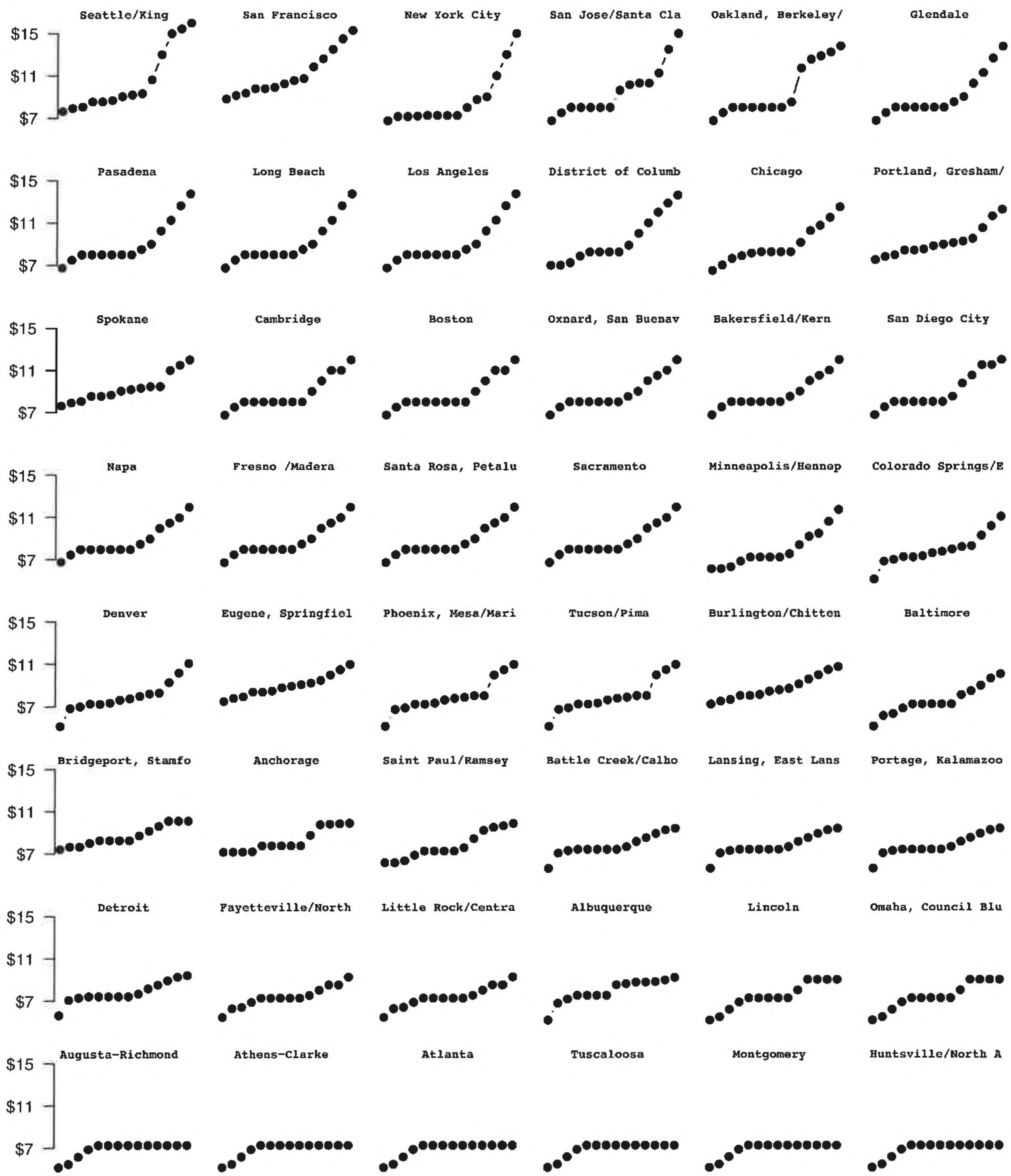

Figure 1: Annual average nominal minimum wage (Vaghul and Zipperer 2021) for 42 HUD geographies with highest and 6 HUD geographies with lowest minimum in 2019, 2006 to 2019.

imums increased relative homeless counts by about 25 percent in the years following the increase. All results hold with controls for changes in local income and local population.

In a third analysis I apply a local projection difference-in-differences model (Dube et al. 2022), which allows me to estimate the effect of continuous changes in the minimum wage rather than breaking changes into categorical treatments as required by the event-study and stacked regression approaches. This estimator suggests that when cities raise their minimum wage by 10%, relative homeless counts increase by three to four percent.

Minimum wages could cause homelessness through different mechanisms. Theory and evidence suggest multiple factors, often in interaction, cause individuals to lose access to housing. Meta-analysis and literature reviews (e.g., Fazel, Geddes, and Kushel 2014) classify these factors as either individual (poverty, substance abuse, mental illness, physical disability, domestic violence, sexual abuse, family conflict, and adverse experiences in early childhood) or structural (housing costs, the extent of income support and welfare programs, and employment opportunities).

Most obviously, to the extent minimum wages cause disemployment of low skill-workers, the lost job can exacerbate existing economic insecurity and lessen ability to pay for housing. Even if employers do not cut total employment, however, minimum wages might induce churn in the labor market. Relatively high-skill workers might enter the labor force at the higher minimum and displace those with lower skills. Current residents previously out of the labor force might enter to capture the higher minimum or workers from other geographies might migrate for the higher wage. Disemployment could occur on the extensive margin (lost job, e.g., Clemens and Strain 2021), the intensive margin (lost hours, e.g., Jardim et al. 2022), or due to jobs not created (Meer and West 2016). Lost wages might be compounded by lost Earned Income Tax Credits or other wage-related income support.

Using the event-study estimator to evaluate mechanisms, I find that increases in the minimum wage decreased employment among low-skill workers and increased costs of local rental housing in my sample. I do not find, however, evidence of in-migration by low-skill workers. If anything, the higher minimum appears to cause out-migration among low-skilled residents, which seems likely to mitigate rather than exacerbate housing insecurity.

Overall, these findings imply that minimum wages have negative distributional consequences not limited to disemployment. If a higher minimum wage causes economic harm more often for individuals with characteristics that cause hardship – low skills, lower education, mental or physical disabilities, criminal records, drug addiction – minimum wages could push some at the bottom of the economic ladder into housing, health, or physical insecurity.

My results offer a new factor in efforts to understand causes of homelessness and may help resolve a puzzling pattern. While homelessness declined in most American cities from the end of the Great Recession up to the Covid-19 pandemic, in a small number of cities such as New York, Seattle, Los Angeles, and San Francisco, homelessness surged. O'Flaherty (2019, section 6) calls this one of the "mysteries" of scholarly understanding of homelessness. Minimum wages increased in these cities by 110, 98, 71, and 63 percent from 2006 to 2019. My evidence suggests these increases could have been an important factor driving increases in homelessness.

It is important to be explicit about the setting of these results. My findings come from analysis of American municipalities, which have specific structures of economic activity, social welfare policy, housing infrastructure, and labor protections. The downstream effects of minimum wage hikes could vary significantly across contexts. In other settings, minimum wages might or might not have notable employment consequences, for example in a setting of monopsony, and might or might not have notable distributional consequences.

1 Data and research design

While homeless populations are difficult to measure, in the mid-2000s the federal Department of Housing and Urban Development (HUD) refined their efforts to count homeless populations across the United States. Each year HUD's "Annual Homeless Assessment Report" (AHAR) compiles point-in-time estimates of January homeless populations for hundreds of local geographies around the nation. Meyer, Wyse, and Corinth (2023) use restricted-access Census micro-data to show the HUD estimates have reasonable accuracy.

The 2022 AHAR reported a nightly homeless population of almost 600,000 in the United States, around 60% sheltered (emergency shelters, safe havens, transitional housing) and 40% unsheltered (on the street, in abandoned buildings, parks, cars, or in other unsuitable living environments) (U.S. Department of Housing and Urban Development 2022, p. 2). Around 70% live alone with the remainder part of family units sometimes including children.

The AHAR numbers come from annual counts implemented by local planning bodies responsible for coordinating homeless services in a specific geographic area ("Continuums of Care"). One-night counts are conducted by the local planning bodies following HUD guidelines in the last week of January in each year. I use these annual "point-in-time" counts as the measure of homeless populations for analysis.[1]

To evaluate the effect of minimum wages on these counts, I match annual average state and locality minimum wages from Vaghul and Zipperer (2021) to HUD geographies for years 2006 to 2019. For HUD geographies without a match to a city-level minimum wage, I use the Varghul Zipperer measures of state minimum wage, which in many cases is equal to the federal minimum.

I use as controls local income (a common predictor of local-geography homelessness

1. I match each January count to minimum wages from the previous year, which allows that year's minimum wage to have full calendar year effects.

per O'Flaherty 2019) and working age population. I calculate annual median household income and working age population (16-64) from American Community Survey (ACS) micro-data (Ruggles et al. 2022) at the level of Metropolitan Statistical Area (MSA). I also use the ACS to measure mechanisms of the effects: local rental housing prices, employment-population ratios, and migration.

1.1 Research design

Econometric work identifies bias in traditional two-way fixed effects estimators with staggered implementation of treatments when treatment effects are heterogeneous (e.g., Goodman-Bacon 2021). My analysis has multiple treatments unfolding over time and treatment effects might well be heterogeneous across municipalities. I address concerns about staggered treatments with three designs. My first specification is an event study approach where instead of regressing homeless counts on annual changes in the local minimum wage, I define policy cohorts who were treated with similar changes in the minimum wage during the same time window (following Clemens and Strain 2021). Single treatment definitions removes concerns about negative weights associated with staggered treatment assignments.

The event-study regressions are specified

$$y_{i,t} = \sum_{g \neq 0} \beta_g \text{Post}_t \times \text{Policy cohort}_{g(i)} + \alpha_t + \delta_i + \gamma Z_{i,t} + \varepsilon_{i,t} \tag{1}$$

where y is the natural logarithm of the HUD homeless point-in-time count in year t (measured in January of year t + 1) in locality i, g indexes policy cohorts that implement a similar change in the minimum wage during the same window, g(i) returns the policy cohort of locality i, α and δ are locality and year fixed effects (the former nesting policy cohort fixed effects), β estimates the effect of policy g on homeless counts in years after treatment, time-varying controls Z, and ε is the year-locality error.

The event-study estimator also allows a dynamic specification

$$y_{i,t} = \sum_{g \neq 0} \sum_{t \neq 0} \beta_{g,t} \mathrm{Year}_t \times \text{Policy cohort}_{g(i)} + \alpha_t + \delta_i + \gamma Z_{i,t} + \varepsilon_{i,t} \tag{2}$$

where a separate β is estimated for each policy cohort in each time period (indicator variables Year_t) relative to treatment year zero.

My second specification is a stacked regression estimator described in Baker, Larcker, and Wang (2022) and used by, e.g., Cengiz et al. (2019) and Clemens and Strain (2021). This estimator resets calendar time to event time that aligns with the year of treatment for each treated observation. The stacked regression estimator removes concerns about staggered treatments by redefining time so that units are treated in the same event-time period. For example, a locality treated in year 2013 would recode calendar year 2013 to event time zero while a locality treated in year 2015 would recode calendar year 2013 to event time –2.

The method creates separate event data sets for each policy cohort treated in the same calendar time period that includes only the observations treated at that time period plus control units never treated. Control units, therefore, are present in each event data set, which leads to a respecification of the fixed effects. The stacked regression estimator is applied to the event data sets stacked together with

$$y_{i,s,t} = \sum_{g \neq 0} \beta_g \mathrm{Post}_{p(s,t)} \times \text{Policy cohort}_{g(i)} + \alpha_{s,t} + \delta_{i,s} + \gamma Z_{i,s,t} + \varepsilon_{i,s,t} \tag{3}$$

where s indexes each event data set, the function $p(s,t)$ returns event time for an observation from event data set s in calendar year t, and α and δ are now event-year and event-locality fixed effects. A dynamic version of the stacked estimator can be specified similar to Eq. 2.

My third specification is the Dube et al. (2022) local projection difference-in-differences estimator (LP-DiD). Like the stacked estimator, LP-DiD limits analysis to treated units and "clean controls," observations who have maintained a control regime during a fixed window of time surrounding the treatment event of treated units. The LP-DiD estimator uses differenced values for outcome, treatment, and controls:

$$\Delta y_{i,t} = \beta \Delta \log(\text{Minimum wage}_{i,t}) + \alpha_t + \gamma \Delta Z_{i,t} + \sum_{p=1}^{P} \omega_p \Delta y_{i,t-p} + \varepsilon_{i,t} \tag{4}$$

where Δ is the one-period difference operator, β estimates the effect of change in the log minimum wage on homeless counts, ω controls for P lag effects of homeless counts, with remaining variables as above.

LP-DiD estimates Eq. 4 on subsets of observations defined by treatment status over a window of time periods. Treated observations are those for which change in the treatment (in this case, log minimum wage) is larger than some threshold. Control observations are those for which change in the treatment (log minimum wage) is below some threshold for a window of time defined by a parameter H. I define treated observations as those with an increase in the minimum wage of greater than 5% so that the sample is composed of cases

Treated $:= \Delta \log(\text{Minimum wage}_{i,t}) > \log(1.05)$, and

Clean control $:= \Delta \log(\text{Minimum wage}_{i,t+h}) \leq \log(1.05),\ h \in (-H, \ldots, 0)$.

2 Results

Graphical evidence.– In Figure 2, I show the relationship between changes in the minimum wage and changes in homelessness with both continuous and policy cohort measures of the minimum wage. In the top row, I plot percentage change in homeless count on nominal dollar change in the minimum wage from 2006 to 2019 across the 100 HUD geographies in my sample. A loess smooth shows that, on average, homelessness fell in the set of geographies with increases in the minimum wage of less than \$4 per hour across the 13 years. The smoother crosses zero and begins to rise with minimum wage increases above \$5. The three geographies with minimum wage increases above \$8 all saw increases in homelessness during the time period. Two of the four geographies with increases in homeless populations above 100 percent (Sacramento City/County and Santa Rosa/Petaluma/Sonoma, both of whom followed the California statewide minimum) had minimum wage increases above \$5 per hour and a third, DeKalb County Georgia (from a much lower base homeless count), \$3.50.

The second two rows present policy cohort event studies. Because most of the large increases in minimum wages occur after 2011, I plot relative log homeless count in three groups of localities following the definitions in Clemens and Strain (2021). In the second row, I define four policy cohorts by change in minimum wage from 2013-2015: no change in minimum wage (a real decline), minimums indexed to inflation, statutory increases of less than \$1, and statutory increases of more than \$1/hour. The y-axis is log-odds homeless count of the latter three cohorts relative to the no-change cohort.

Each of the three cohorts with increases in the minimum wage start in 2011 with homeless counts greater than in the no-increase reference cohort (log-odds greater than zero). Odds remain roughly similar to no-change localities through 2014, at which point the inflation-indexed cohort and the larger increase cohort begin to see relative increases in homeless counts. By 2019, all three policy cohorts have larger log-odds relative to the no-change cohort. Localities with the largest increases in minimum wages increase log-odds from about 1.2 in 2011 to nearly 1.6 in 2019.

The third row defines policy cohorts by changes in the minimum wage from 2013-2018: no change (real decline) in minimum wage, indexed to inflation, statutory increases of less than \$2.50, and statutory increases of more than \$2.50. As with 2013-2015 policy cohorts, both cohorts with statutory increases in minimum wages have larger log-odds in 2019 than in 2011 relative to the no-change cohort. Localities with increases of \$2.50 or more see log-odds increase from less than 1.5 to around 1.75.

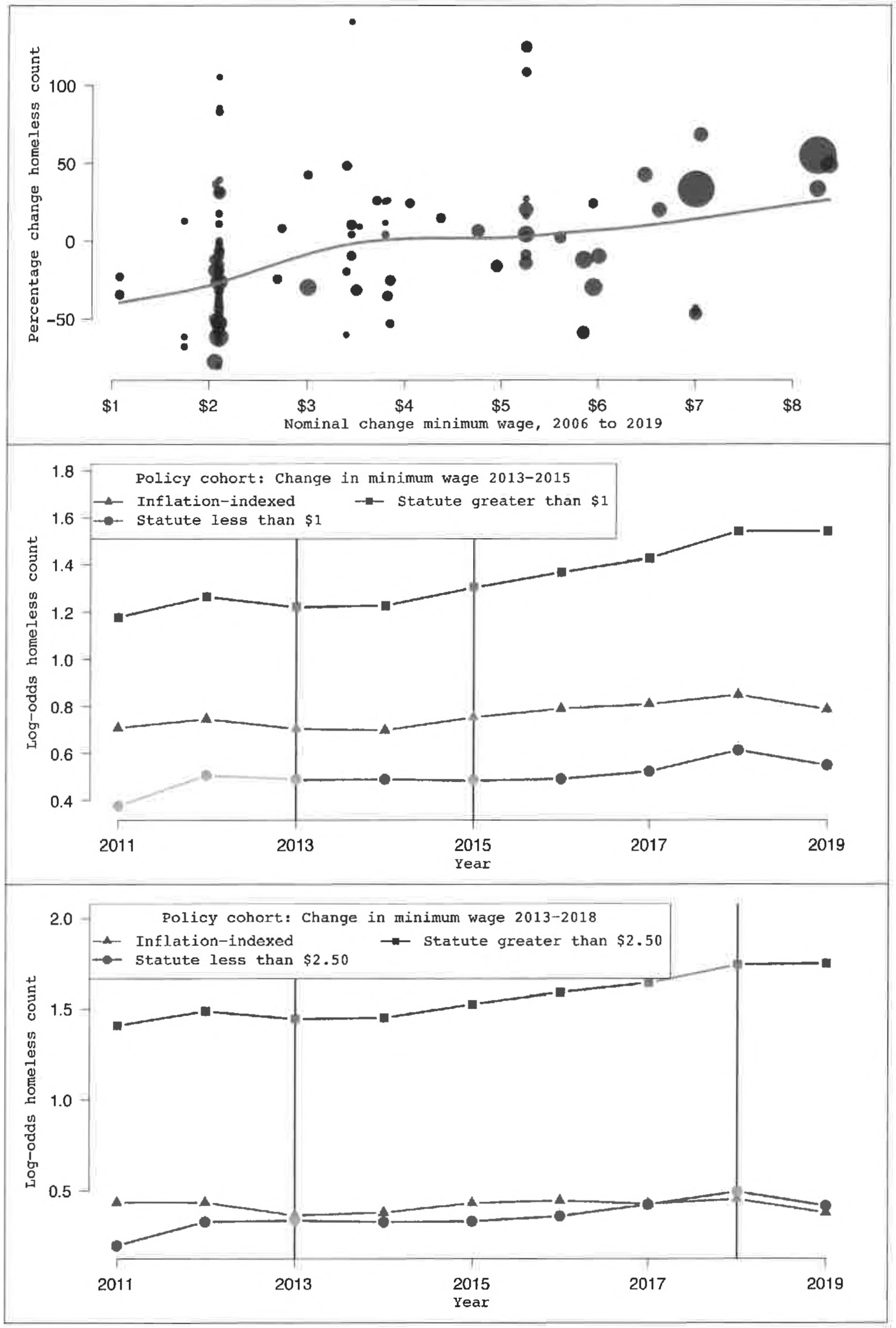

Figure 2: Top row: Percentage change in homeless count by change in nominal minimum wage, 2006 to 2019. Loess smooth with bandwidth 2/3 overlays raw data points, point size proportional to 2006 homeless count. Second and third rows: localities broken into policy cohorts defined by change in minimum wage 2013 to 2015 (second row), 2013 to 2018 (third row). Y-axis is log-odds homeless count for each of three cohorts (minimum indexed to inflation or statutory increases of two sizes) relative to fourth cohort with no nominal change in minimum (real decline).

Regression results.– Panel A of Table 1 presents coefficient estimates applying policy cohort event studies defined in Eq. 1 (and similar to Figure 2) and stacked regression estimates defined in Eq. 3. Column one and two use the 2013-2015 policy cohorts with separate effects estimated for increases of less than or more than $1; inflation indexers and no-change/real-decline municipalities are the excluded category. Coefficients indicate that municipalities with statutory increases up to $1 per hour from 2013 to 2015 saw homeless counts about 10 percent higher in years 2014 to 2019 relative to inflation-indexed and no-change municipalities. Standard errors, however, are large. For the municipalities with statutory increases of more than $1 the estimated relatie effect is on the order of 40 or 45 percent higher. Estimating dynamic effects of these treatments (Table A1) indicates the minimum wage increase induced relative increase in homeless counts beginning in 2014 and peaking in 2018.[2]

Columns three and four use the 2013-2018 policy cohorts. The policy cohort with statutory increases 2013 to 2018 of up to $2.50 saw relative increases in homeless counts of around 14 percent and the cohort with increase above $2.50 almost 25 percent. Dynamic effect estimates in Table A1 suggest these effects increase in time beginning around 2015 and peak in 2018 or 2019.

Columns five and six present results from the stacked regression estimator (Eq. 3) with treatment defined by a year-over-year change in the minimum wage of $0.75 or more. Due to federal minimum wage increases in 2007 and 2008, the sample time period for stacked regressions is calendar years 2009 through 2019. This model stacks five treated policy cohorts (years 2013, 2014, 2015, 2016, and 2017) each with never-treated control localities. Localities that increase the minimum wage in one year by $0.75 or more see subsequent increases in homeless counts by around 25 percent in the years following treatment relative to municipalities with no or small increases in the minimum.[3]

2. The dynamic specifications in Table A1 yield statistically significant effects for the less than $1 policy cohort in 2018 and 2019. Sun and Abraham (2021) suggest caution in interpreting event study dynamic effects.

3. Dynamic models using the stacked regression estimator (Table A2) yield results similar to the dynamic

Panel A. Policy cohort event study and stacked regression results

	Homeless count (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
	2013-2015 cohorts	2013-2015 cohorts	2013-2018 cohorts	2013-2018 cohorts	Stacked regression	Stacked regression
Post x Statute less than $1	0.10	0.10				
	(0.06)	(0.07)				
Post x Statute greater than $1	0.36**	0.35**				
	(0.06)	(0.06)				
Post x Statute less than $2.50			0.13*	0.13		
			(0.07)	(0.07)		
Post x Statute greater than $2.50			0.20**	0.21**		
			(0.07)	(0.07)		
Post x Minimum wage increase 75 cents or more					0.22**	0.21**
					(0.05)	(0.05)
Observations	1,482	1,334	1,482	1,334	3,608	3,073
R-squared	0.97	0.97	0.97	0.97	0.96	0.97
Locality fixed effects	✓	✓	✓	✓		
Year fixed effects	✓	✓	✓	✓		
Time-varying controls		✓		✓		✓
Locality-event fixed effects					✓	✓
Year-event fixed effects					✓	✓

Panel B. Local projection-DiD results

	Change in log homeless count					
	(1)	(2)	(3)	(4)	(5)	(6)
	H=1	H=2	H=3	H=1	H=2	H=3
Change log minimum wage, t-1 to t	0.26*	0.29*	0.28*	0.31*	0.28*	0.28*
	(0.13)	(0.13)	(0.13)	(0.14)	(0.13)	(0.14)
Observations	1,200	1,060	943	917	809	708
R-squared	0.03	0.03	0.03	0.06	0.06	0.06
Locality fixed effects	-	-	-	-	-	-
Year fixed effects	✓	✓	✓	✓	✓	✓
Lag change time-varying controls				✓	✓	✓

Robust standard errors in parentheses
** $p<0.01$, * $p<0.05$

Table 1: Minimum wages and homelessness, American municipalities, 2006 to 2019. Panel A policy cohort and stacked regression estimators, panel B LP-DiD. Dependent variable is log point-in-time estimate of homeless population from the HUD Annual Homeless Assessment Reports for 48 Major City and 52 Other Largely Urban geographies. Minimum wage policies from Vaghul and Zipperer (2021) with state minimum substituted for locality-years without local minimum. Controls = median household income and working-age population (ACS). Robust standard errors clustered on locality (locality-event for stacked regressions). Variable 'H' in panel B indicates width of window used to define clean control cases.

Panel B of Table 1 presents results of Eq. 4 with clean controls defined by window sizes H of 1, 2, and 3, corresponding to the first three columns. Columns four through six add lagged differenced outcome variables and lagged differenced control variables. All six specifications indicate that a ten percent increase in the previous year's minimum wage leads to around three percent increase in the homeless population, regardless of window size H or inclusion of controls or lagged dependent variables.

Mechanisms.– In Table 2, I consider mechanisms that might connect minimum wage increases to homelessness. Using the same sample and event study estimator as in Table 1, I estimate the effect of minimum wages on employment, migration, and rental housing prices. If minimum wage increases decrease employment, some of those with lost income might lose the ability to pay for housing. If minimum wage increases cause in-migration of low-skill workers, new workers could displace previous residents leading to lost income or increased competition for housing. If minimum wages increase demand for low-priced housing through income effects from higher-paid low-wage workers, some low-wage individuals or families might be priced out of the housing market and forced into homeless shelters, transitional housing, or the streets.

The first column in both panels of Table 2 considers employment effects on low-skill populations. Employment-to-population ratios decline by one to two percentage points for those aged 16 to 25 with less than high school education in treated 2013-2015 policy cohorts relative to policy cohorts with minimums indexed to inflation or with no change in the minimum wage (panel A). The decline is 2 percentage point for 2013-2018 localities with increases in the minimum of $2.50 or greater (panel B). Pooling treatments into one variable (see Appendix Table A3) yields estimated effects 2 (2013-2015 cohort) and 1 (2013-2018 cohort) percentage points.[4]

Columns two through four estimate minimum wage effects on low-skill migration.

effects with the policy cohort estimator.

4. These results roughly follow the design and data of Clemens and Strain (2021). Applying an LP-DiD estimator (Appendix Table A4) suggests that a 10 percent increase in the minimum wage decreases the employment ratio for 16 to 25 less than high school by about 0.6 percentage points.

Panel A. 2013-2015 policy cohort event study results

	(1)	(2)	(3)	(4)	(5)
	Employment Ratio 16 to 25 Less than H.S.	Population 16 to 25 Less than H.S. (log)	Moved from different MSA 16 to 25 Less than H.S. (log)	Moved from different state 16 to 25 Less than H.S. (log)	10th percentile gross rent (log)
Post x Statute less than $1	-0.02*	-0.03	-0.03	0.03	0.01
	(0.01)	(0.01)	(0.06)	(0.14)	(0.01)
Post x Statute greater than $1	-0.01	-0.04*	-0.01	0.10	0.04**
	(0.01)	(0.02)	(0.05)	(0.10)	(0.01)
Observations	1,336	1,336	1,336	1,336	1,336
R-squared	0.73	1.00	0.86	0.75	0.94
Locality fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓	✓

Panel B. 2013-2018 policy cohort event study results

	(1)	(2)	(3)	(4)	(5)
	Employment Ratio 16 to 25 Less than H.S.	Population 16 to 25 Less than H.S. (log)	Moved from different MSA 16 to 25 Less than H.S. (log)	Moved from different state 16 to 25 Less than H.S. (log)	10th percentile gross rent (log)
Post x Statute less than $2.50	0.00	0.02	-0.05	-0.09	0.02
	(0.01)	(0.01)	(0.08)	(0.14)	(0.01)
Post x Statute greater than $2.50	-0.02**	-0.05**	-0.10	-0.05	0.02
	(0.01)	(0.01)	(0.05)	(0.14)	(0.01)
Observations	1,336	1,336	1,336	1,336	1,336
R-squared	0.73	1.00	0.86	0.75	0.94
Locality fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓	✓

Robust standard errors in parentheses
** $p<0.01$, * $p<0.05$

Table 2: Mechanisms connecting minimum wages to homelessness, American municipalities, 2006 to 2019, event study estimator. Outcomes and controls measured from ACS. Time-varying controls depend on outcome: employment ratio working age population, working-age population log count, log count working-age population moved from different MSA or state, 90th percentile log gross rent. Robust standard errors clustered on locality. See Appendix for extended results.

Column two panel A suggests that treated localities with increases in the minimum wage 2013 to 2015 saw a decline in the count of low-skill population relative to total working-age population by 3 or 4 percentage points. The 2013 to 2018 localities with increases of $2.50 or more see a relative decline by 5 percentage points. The pooled estimates (Appendix Table A5) are 3 and 2 percentage point declines, respectively.

Column two measures net migration. Columns three and four measure in-migration using the ACS question that asks respondents where they lived one year prior to the interview. I tabulate the count of individuals 16 to 25 with less than high school who report having lived in a different MSA (column three) or different state (column four) one year prior to measure the level of in-migration to the locality. Point estimates are all negative but for the 2013-2015 policy cohorts in-migrating from another state. None of the pooled estimates are statistically distinct from zero (Table A5).

The migration results work in the direction opposite of a mechanism where new low-skill residents displace existing low-skill residents from employment or housing. There are, if anything, relatively fewer low-skill workers following increases in the minimum wage and more evidence of net out- than in-migration.

Column five estimates minimum wage effects on low-price rental housing measured using 10th percentile gross rent in each MSA-year. To control for local housing market dynamics I include as a control 90th percentile gross rent, which is plausibly exogenous to local minimum wages. Increases in the minimum wages increase 10th percentile local rental prices by one to two percentage points (pooled estimates in Table A6) relative to indexed or no-change localities and to the high-end rental market. At-risk populations appear to face higher housing costs with increases in minimum wages.

In summary, analysis of mechanisms potentially connecting minimum wages to homelessness suggests disemployment and rental housing prices, but not migration. I estimate disemployment effects on the extensive margin; intensive margin effects could also be present (Jardim et al. 2022) and relevant stressors to at-risk populations. Note also that if minimum wages lead to out-migration of low-skill residents, this would mitigate consequences for locality homelessness as these individuals depart for other localities and would not be present for point-in-time homeless counts.

Robustness.– The most likely threat to inference is that the localities with large minimum wage increases are mostly coastal cities that experienced other common factors during the 2010s, in addition to minimum wage increases, that separate them from the mostly non-coastal cities that serve as controls. Many of these cities saw large increases in housing prices as well as robust job growth and migration. These factors, of course, do not perfectly distinguish treated from control localities as many non-coastal cities (e.g., Austin, Charlotte) also experienced 2010s booms. The regressions above aim to control for these factors through inclusion of annual changes in population (migration) and income (economy), but skeptical readers might worry about additional unmeasured factors.

In Figure A2, I again plot percentage change in homeless count on nominal dollar change in the minimum wage from 2006 to 2019 but limit to coastal city geographies in my sample defined by Pacific coast or Acela Corridor. One might think of this as a dose-response analysis holding fixed coastal status. The pattern of increasing homeless count with increase in nominal minimum wage is roughly identical to that for all localities in Figure 2, suggesting the result is not driven simply by comparing coastal to non-coastal cities.

In Table A7, I reproduce the regression estimates from Table 1 adding a control for median gross rent for each locality. Note that this measure is potentially post-treatment to a minimum wage increase because, as I document in Table 2 and others have found in the context of redistributive policies (Susin 2002), redistribution can increase costs of housing through demand effects. However, despite the uncertainty about causal ordering, it is nonetheless worth considering as a control. It could be that local governments that face rising housing costs choose to increase minimum wages in hopes of helping their low-wage constituents. Table A7 shows that controlling for local rents attenuates the estimated effects of minimum wages to a small degree for the cohort analysis, to a modest degree for the stacked estimator, and by about half for LP-DiD. The control does not, however, drive estimated effects of the minimum wage to zero, suggesting that minimum wages do influence homelessness above and beyond local housing prices.

3 Discussion and conclusion

This paper documents a new consequence of minimum wages and a new factor of homelessness. Merging administrative data from HUD to state and local minimum wage laws suggests that minimum wages induce increases in homeless counts. Further analysis suggests disemployment and local rental housing prices as mechanisms. These findings build on recent work that finds differential consequences of increases to the minimum wage for individuals with different backgrounds and suggest the need for more research to evaluate the distributional effects of wage floors.

The evidence presented here implies that focus on net employment could mislead evaluation of minimum wage policy. Aggregate employment might move very little in response to an increase in the minimum wage yet individual consequences for some of the lowest-skilled workers could be large, particularly if such workers are already on the economic margin or if loss of wages leads to loss of income-based subsidies.

States and localities make policy choices over both minimum wage laws and the stubborn challenge of homelessness. By estimating effects at the intersection of the two, this paper opens up several avenues for future research along with implications for policy to address homelessness. While much research looks at the correspondence between local housing supply and housing insecurity, housing supply is only one factor in the difficult life choices faced by those at risk of homelessness. Individuals at the economic margins depend on the low-wage work of themselves or others. Any disruption to those economic circumstances can push them into housing (and economic) insecurity. Future work might consider reforms that can increase opportunities for economic self-sufficiency and skill development for those suffering from housing security as one part of a multifaceted toolkit to address the challenge of homelessness.

Appendix

A Additional tables and figures

I plot in Figure A1 time-series of homeless population counts for the 48 HUD geographies with the largest 2019 homeless counts, 2006 to 2019. Regression analysis includes an additional 52 geographies not plotted. The localities are sorted by largest 2019 homeless populations. The locality-by-locality time-series show wide variation in time trends.

Tables A1 and A2 present estimates of dynamic treatment effects for the policy cohort and stacked regression estimators. Results show that homeless counts increase as time passes away from increases in minimum wages. While there are some pre-treatment dynamic effects estimated, Sun and Abraham (2021) show that such pre-treatment effects can be biased and so suggest interpretation with caution. A previous version of Table A2 had stronger pre-treatment but had been limited to a shorter time period (2011 to 2019). The revised version includes 2009 and 2010, attenuating the suggested pre-treatment effect. Using different definitions of treatment (e.g., increases of \$0.50 instead of \$0.75) also attenuates the suggested effect, suggesting to me it is caused by sampling variability. Dynamic effects in this setting are noisy because of the limited time period and limited treatment count of the observations.

Tables A3, A4, A5, and A6 present estimates for mechanisms connecting minimum wages to homelessness: Disemployment, migration, and local rental housing prices.

Figure A2 and Table A7 evaluate coastal city status and local rental housing prices as potential confounds to minimum wage increases.

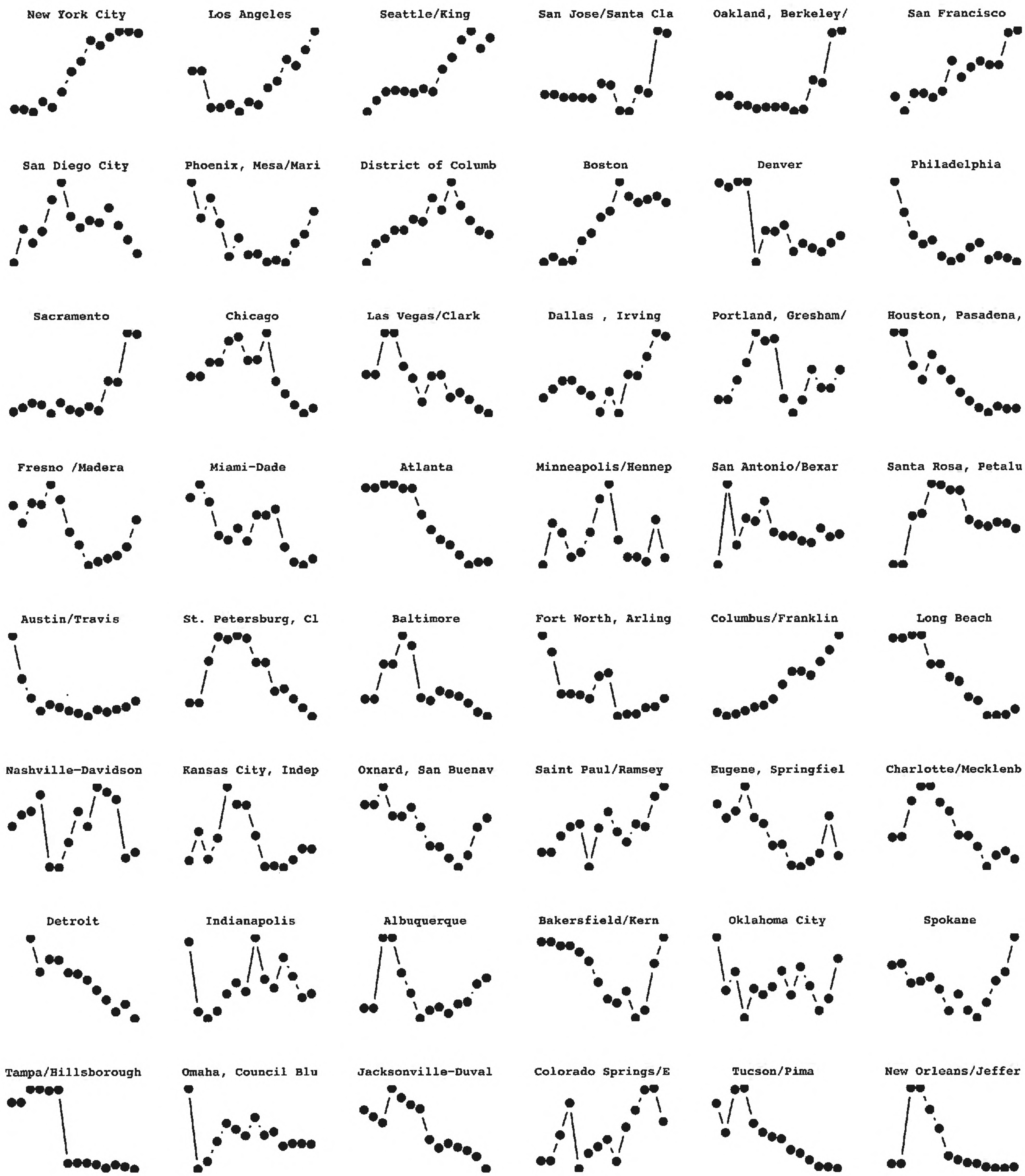

Figure A1: Annual point-in-time homeless count time-series from the HUD Annual Homeless Assessment Reports for 48 geographies with largest homeless counts in 2019, 2006 to 2019.

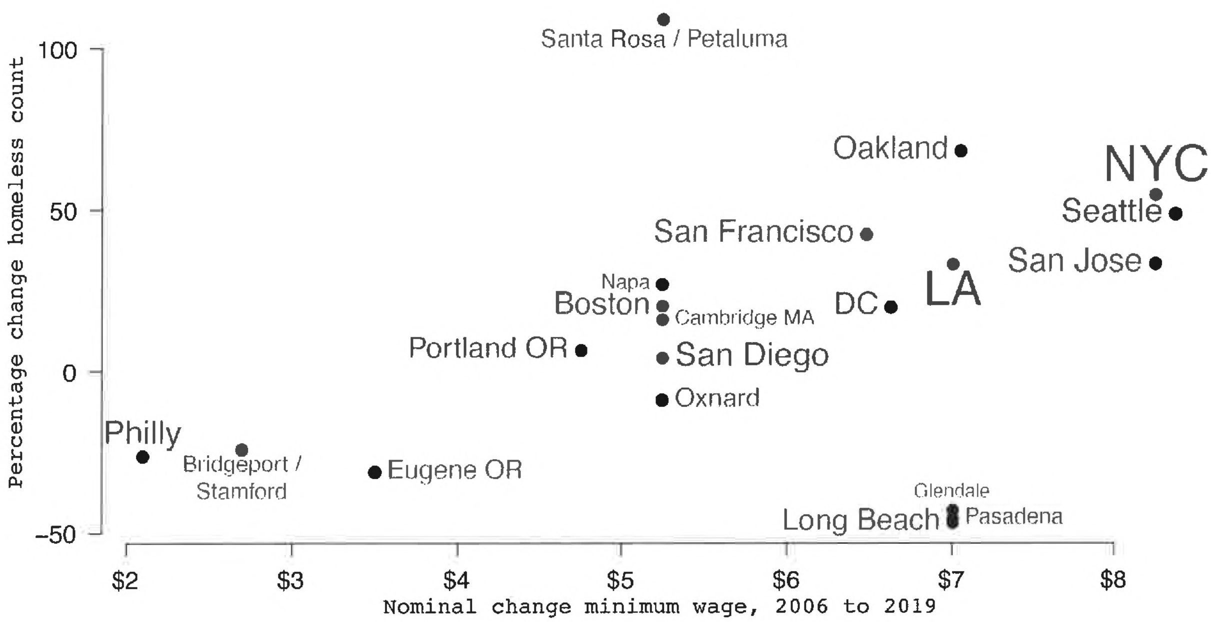

Figure A2: Percentage change in homeless count by change in nominal minimum wage, 2006 to 2019 for localities in coastal states (Pacific Coast plus Acela Corridor) excluding interior California. Text size proportional to 2006 homeless count.

	(1) Homeless count (log) 2013-2015 cohorts	(2) se
2010 x Statute less than $1	0.02	(0.05)
2011 x Statute less than $1	-0.01	(0.05)
2012 x Statute less than $1	0.08	(0.06)
2014 x Statute less than $1	0.05	(0.07)
2015 x Statute less than $1	0.05	(0.07)
2016 x Statute less than $1	0.08	(0.07)
2017 x Statute less than $1	0.10	(0.08)
2018 x Statute less than $1	0.21*	(0.09)
2019 x Statute less than $1	0.19*	(0.09)
2010 x Statute greater than $1	-0.01	(0.04)
2011 x Statute greater than $1	0.02	(0.05)
2012 x Statute greater than $1	0.18**	(0.04)
2014 x Statute greater than $1	0.25**	(0.07)
2015 x Statute greater than $1	0.30**	(0.08)
2016 x Statute greater than $1	0.40**	(0.07)
2017 x Statute greater than $1	0.37**	(0.07)
2018 x Statute greater than $1	0.49**	(0.09)
2019 x Statute greater than $1	0.45**	(0.10)
Observations	1,334	
R-squared	0.97	
Locality fixed effects	✓	
Year fixed effects	✓	
Time-varying controls	✓	

Robust standard errors in parentheses
** p<0.01, * p<0.05

	(1) Homeless count (log) 2013-2018 cohorts	(2) se
2010 x Statute less than $2.50	-0.05	(0.05)
2011 x Statute less than $2.50	-0.07	(0.05)
2012 x Statute less than $2.50	0.07	(0.07)
2014 x Statute less than $2.50	0.07	(0.08)
2015 x Statute less than $2.50	0.07	(0.09)
2016 x Statute less than $2.50	0.11	(0.07)
2017 x Statute less than $2.50	0.15	(0.08)
2018 x Statute less than $2.50	0.23*	(0.09)
2019 x Statute less than $2.50	0.15	(0.09)
2010 x Statute greater than $2.50	-0.00	(0.05)
2011 x Statute greater than $2.50	0.00	(0.04)
2012 x Statute greater than $2.50	0.10*	(0.05)
2014 x Statute greater than $2.50	0.08	(0.07)
2015 x Statute greater than $2.50	0.13	(0.07)
2016 x Statute greater than $2.50	0.20*	(0.09)
2017 x Statute greater than $2.50	0.24**	(0.08)
2018 x Statute greater than $2.50	0.34**	(0.09)
2019 x Statute greater than $2.50	0.34**	(0.10)
Observations	1,334	
R-squared	0.97	
Locality fixed effects	✓	
Year fixed effects	✓	
Time-varying controls	✓	

Robust standard errors in parentheses
** p<0.01, * p<0.05

Table A1: Minimum wages and homelessness, American municipalities, 2006 to 2019, dynamic policy cohort analysis. Dependent variable is log of the point-in-time estimate of homeless population from the HUD Annual Homeless Assessment Reports for 48 Major City and 52 Other Largely Urban geographies. Minimum wage policies from Vaghul and Zipperer (2021) with state minimum substituted for locality-years without local minimum. Controls = median household income and population (ACS). Robust standard errors clustered on locality.

	(1) Homeless count (log)	(2) Homeless count (log)
Treated x Event year = -5	-0.06	-0.04
	(0.03)	(0.03)
Treated x Event year = -4	-0.03	-0.03
	(0.03)	(0.03)
Treated x Event year = -3	-0.02	-0.01
	(0.03)	(0.03)
Treated x Event year = -2	-0.02	0.00
	(0.04)	(0.04)
Treated x Event year = 0	0.09	0.11*
	(0.05)	(0.05)
Treated x Event year = 1	0.17**	0.16**
	(0.05)	(0.05)
Treated x Event year = 2	0.21**	0.20**
	(0.06)	(0.06)
Treated x Event year = 3	0.28**	0.27**
	(0.08)	(0.08)
Treated x Event year = 4	0.25**	0.22**
	(0.07)	(0.07)
Treated x Event year = 5	0.45**	0.44**
	(0.06)	(0.05)
MSA median household income (log)		0.56**
		(0.10)
MSA population 16-64 (log)		-0.24
		(0.13)
Observations	3,608	3,073
R-squared	0.96	0.97
Locality-event fixed effects	✓	✓
Year-event fixed effects	✓	✓
Time-varying controls		✓

Robust standard errors in parentheses
** p<0.01, * p<0.05

Table A2: Minimum wages and homelessness, American municipalities, 2009 to 2019, dynamic stacked regression analysis. Excludes 2008 and prior due to increases in federal minimum wage. Robust standard errors clustered on locality-event.

	(1) Employment Ratio 16 to 25 Less than H.S. 2013-2015 cohorts	(2) Employment Ratio 16 to 25 Less than H.S. 2013-2015 cohorts	(3) Employment Ratio 16 to 25 Less than H.S. 2013-2018 cohorts	(4) Employment Ratio 16 to 25 Less than H.S. 2013-2018 cohorts
Post x Statute less than $1	-0.02*			
	(0.01)			
Post x Statute greater than $1	-0.01			
	(0.01)			
Post x Statute less than $2.50			0.00	
			(0.01)	
Post x Statute greater than $2.50			-0.02**	
			(0.01)	
Post x Either statutory increase		-0.02*		-0.01
		(0.01)		(0.01)
MSA population 16-64 (log)	-0.02	-0.02	-0.02	-0.02
	(0.03)	(0.03)	(0.03)	(0.03)
Employment-population ratio 16-64	1.14**	1.14**	1.16**	1.13**
	(0.10)	(0.10)	(0.10)	(0.11)
Observations	1,336	1,336	1,336	1,336
R-squared	0.73	0.73	0.73	0.73
Locality fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓

Robust standard errors in parentheses
** $p<0.01$, * $p<0.05$

Table A3: Minimum wages and disemployment, American municipalities, 2006 to 2019, event-study estimator. Dependent variables are MSA employment ratios calculated from the ACS at same geographies as for homeless count analysis above. Robust standard errors clustered on locality.

	(1) Employment Ratio 16 to 25 Less than H.S. H=1	(2) Employment Ratio 16 to 25 Less than H.S. H=2	(3) Employment Ratio 16 to 25 Less than H.S. H=3
Change log minimum wage, t-1 to t	-0.07*	-0.06	-0.06
	(0.03)	(0.03)	(0.03)
Change employ ratio 16 to 25 lt HS, t-2 to t-1	-0.57**	-0.58**	-0.56**
	(0.05)	(0.05)	(0.05)
Change employ ratio 16 to 25 lt HS, t-3 to t-2	-0.26**	-0.26**	-0.26**
	(0.04)	(0.05)	(0.05)
Observations	915	806	704
R-squared	0.41	0.40	0.43
Locality fixed effects	-	-	-
Year fixed effects	✓	✓	✓
Lag change time-varying controls	✓	✓	✓

Robust standard errors in parentheses
** $p<0.01$, * $p<0.05$

Table A4: Minimum wages and disemployment, American municipalities, 2006 to 2019, LP-DiD estimator. Dependent variables are MSA employment ratios calculated from the ACS at same geographies as for homeless count analysis above. Controls = changes in working-age population and employment ratio (ACS). Robust standard errors clustered on locality. Variable 'H' indicates width of window used to define clean control cases.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Population 16 to 25 Less than H.S. (log) 2013-2015 cohorts	Population 16 to 25 Less than H.S. (log) 2013-2015 cohorts	Population 16 to 25 Less than H.S. (log) 2013-2018 cohorts	Population 16 to 25 Less than H.S. (log) 2013-2018 cohorts	Moved from different MSA 16 to 25 Less than H.S. (log) 2013-2015 cohorts	Moved from different MSA 16 to 25 Less than H.S. (log) 2013-2015 cohorts	Moved from different MSA 16 to 25 Less than H.S. (log) 2013-2018 cohorts	Moved from different MSA 16 to 25 Less than H.S. (log) 2013-2018 cohorts	Moved from different state 16 to 25 Less than H.S. (log) 2013-2015 cohorts	Moved from different state 16 to 25 Less than H.S. (log) 2013-2015 cohorts	Moved from different state 16 to 25 Less than H.S. (log) 2013-2018 cohorts	Moved from different state 16 to 25 Less than H.S. (log) 2013-2018 cohorts
Post x Statute less than $1	-0.03				-0.03				0.03			
	(0.01)				(0.06)				(0.14)			
Post x Statute greater than $1	-0.04*				-0.01				0.10			
	(0.02)				(0.05)				(0.10)			
Post x Statute less than $2.50			0.02				-0.05				-0.09	
			(0.01)				(0.08)				(0.14)	
Post x Statute greater than $2.50			-0.05**				-0.10				-0.05	
			(0.01)				(0.05)				(0.14)	
Post x Either statutory increase		-0.03*		-0.02		-0.03		-0.08		0.04		-0.06
		(0.01)		(0.01)		(0.05)		(0.05)		(0.12)		(0.10)
MSA population 16-64 (log)	1.16**	1.16**	1.15**	1.16**								
	(0.05)	(0.05)	(0.05)	(0.05)								
Count moved from different MSA, 16 to 64 (log)					1.19**	1.19**	1.20**	1.20**				
					(0.07)	(0.07)	(0.07)	(0.07)				
Count moved from different state, 16 to 64 (log)									1.33**	1.33**	1.34**	1.34**
									(0.17)	(0.17)	(0.17)	(0.17)
Observations	1,336	1,336	1,336	1,336	1,336	1,336	1,336	1,336	1,336	1,336	1,336	1,336
R-squared	1.00	1.00	1.00	1.00	0.86	0.86	0.86	0.86	0.75	0.75	0.75	0.75
Locality fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time-varying controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Robust standard errors in parentheses
** p<0.01, * p<0.05

Table A5: Minimum wages and migration, American municipalities, 2006 to 2019, event-study estimator. Dependent variables are log of MSA population aged 16 to 25 with less than high school education, log count of MSA population aged 16 to 25 with less than high school moved from different MSA, county, or state since previous year, all tabulated from ACS at same geographies as for homeless count analysis above. Robust standard errors clustered on locality.

	(1)	(2)	(3)	(4)
	10th percentile gross rent (log) 2013-2015 cohorts	10th percentile gross rent (log) 2013-2015 cohorts	10th percentile gross rent (log) 2013-2018 cohorts	10th percentile gross rent (log) 2013-2018 cohorts
Post x Statute less than $1	0.01			
	(0.01)			
Post x Statute greater than $1	0.04**			
	(0.01)			
Post x Statute less than $2.50			0.02	
			(0.01)	
Post x Statute greater than $2.50			0.02	
			(0.01)	
Post x Either statutory increase		0.01		0.02*
		(0.01)		(0.01)
90th percentile gross rent (log)	0.30**	0.30**	0.30**	0.30**
	(0.05)	(0.05)	(0.05)	(0.05)
Observations	1,336	1,336	1,336	1,336
R-squared	0.94	0.94	0.94	0.94
Locality fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

Robust standard errors in parentheses
** $p<0.01$, * $p<0.05$

Table A6: Minimum wages and rental housing prices, American municipalities, 2006 to 2019, event-study estimator. Dependent variable is 10th percentile of gross rent in the MSA from ACS at same geographies as for homeless count analysis above. Robust standard errors clustered on locality.

	(1) Homeless count (log) 2013-2015 cohorts	(2) Homeless count (log) 2013-2018 cohorts	(3) Homeless count (log) stacked regression	(4) Change in log homeless count H=1	(5) Change in log homeless count H=2	(6) Change in log homeless count H=3
Post x Statute less than $1	0.09					
	(0.07)					
Post x Statute greater than $1	0.32**					
	(0.06)					
Post x Statute less than $2.50		0.13				
		(0.07)				
Post x Statute greater than $2.50		0.17*				
		(0.07)				
Post x Minimum wage increase 75 cents or more			0.13**			
			(0.05)			
Change log minimum wage, t-1 to t				0.12	0.14	0.12
				(0.14)	(0.14)	(0.14)
MSA median gross rent (log)	0.88**	0.84**	1.54**			
	(0.27)	(0.26)	(0.19)			
Change median gross rent (log), t-2 to t-1				0.81**	0.81**	0.91**
				(0.30)	(0.28)	(0.30)
Change median gross rent (log), t-3 to t-2				-0.41*	-0.38*	-0.42*
				(0.18)	(0.17)	(0.18)
Observations	1,334	1,334	3,073	989	864	762
R-squared	0.97	0.97	0.97	0.06	0.05	0.06
Locality fixed effects	✓	✓		-	-	-
Year fixed effects	✓	✓		✓	✓	✓
Time-varying controls	✓	✓	✓			
Lag change time-varying controls				✓	✓	✓
Locality-event fixed effects			✓			
Year-event fixed effects			✓			

Robust standard errors in parentheses
** $p<0.01$, * $p<0.05$

Table A7: Minimum wages and homelessness, American municipalities, 2006 to 2019, with control for local rental prices. Columns one and two policy cohort estimator, column three stacked, and columns four through six LP-DiD. Controls = median household income and working-age population (ACS). Robust standard errors clustered on locality (locality-event for stacked regressions). Variable 'H' indicates width of window used to define clean control cases for LP-DiD.

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