**Energy Institute WP 324**

# The Role of People vs. Places in Individual Carbon Emissions

Eva Lyubich

December 2021

# The Role of People vs. Places in Individual Carbon Emissions

Eva Lyubich *

*Job Market Paper*

December 5, 2021

Click here for the most recent version.

**Abstract**

There is substantial spatial heterogeneity in household carbon emissions across the US, and a strong association between emissions and local amenities such as density, transportation infrastructure, and housing characteristics. I estimate what share of heterogeneity in carbon emissions is attributable to places themselves, and what share reflects individual preferences and taste-based sorting. To do this, I construct a longitudinal panel of residential energy use and commute characteristics for over a million individuals from two decades of administrative Decennial Census and American Community Survey data. I use movers in my data to estimate place effects – the amount by which carbon emissions change for the same individual living in different places – for almost 1,000 cities and roughly 65,000 neighborhoods across the US. I find that place effects explain more than half of differences between places, and about 15-25% of overall variation in carbon emissions. My estimates suggest that decreasing neighborhood-level place effects from one standard deviation above the mean to one standard deviation below the mean would decrease household carbon emissions from residential energy use and commuting by about 40%.

*Department of Economics and Energy Institute at Haas, UC Berkeley. elyubich@berkeley.edu. Any views expressed are those of the author and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2410. (Release authorizations: CBDRB-FY21-P2410-R9136, CBDRB-FY21-P2410-R9181, CBDRB-FY21-P2410-R9308). I am grateful to Reed Walker, Emmanuel Saez, and Joe Shapiro for their guidance and support. This project has also benefited tremendously from comments and suggestions from Alan Auerbach, Matilde Bombardini, Stephanie Bonds, Severin Borenstein, David Card, Maria Dieci, Karl Dunkle Werner, Ben Faber, Meredith Fowlie, Cecile Gaubert, Hilary Hoynes, Pat Kline, Jon Kolstad, Enrico Moretti, Andrés Rodríguez-Clare, Nina Roussille, Raffaele Saggio, Jim Sallee, Elif Tasar, Danny Yagan, Katherine Wagner, Randall Walsh, Chris Walters, and seminar participants at the University of California, Berkeley. I am grateful to NSF GRFP (DGE 1752814), the Berkeley Opportunity Lab and Smith Richardson Foundation, and Washington Center for Equitable Growth for financial support. I thank Angela Andrus for her patience and help in navigating disclosure review.

# 1 Introduction

Climate change, caused by carbon emissions and other greenhouse gases, is an urgent threat to society. Global average temperatures have increased by over 1C (1.8F) relative to pre-industrial levels (NASA 2020), and the International Panel on Climate Change has cautioned that even warming of 1.5C could lead to catastrophic increases in extreme weather events, population displacement, and degradation of ecosystems. There is substantial spatial heterogeneity in household carbon emissions across regions and neighborhoods in the US, and researchers and policy makers have highlighted this variation as an opportunity for decarbonization, pointing to features of low carbon places, such as density and differences in transportation infrastructure, that other places could adopt in order to lower household carbon emissions.

However, differences in mean carbon emissions across cities and neighborhoods reflect a combination of place-based amenities, individual characteristics, and taste-based sorting. The relative contributions of these pieces is a central determinant of whether "place-based" interventions that change urban form would lead to meaningful and rapid reductions in carbon emissions. For instance, if places with large single-family homes and car-oriented transportation infrastructure are high emissions because the people who live there dislike multi-family homes and public transit, then deregulating zoning or building new subway lines would have little impact on household emissions and could end up being an expensive decarbonization strategy. Conversely, if the lack of denser housing and transit options is a constraint on household choices, rather than a reflection of their preferences, then such interventions could have the potential to decrease emissions for many households at once.

In this paper, I decompose variation in household carbon emissions into a component driven by household characteristics and a component driven by place effects – i.e., the amount by which the same household's carbon emissions would differ from place to place due to differences in the underlying features of those places. To do so, I construct a longitudinal panel of residential and transportation energy use for over a million individuals from 20 years of restricted-access Decennial Census and American Community Survey (ACS) micro-data. The longitudinal nature of my data makes it possible to link individual survey respondents over time and across places and use two related "mover design" decompositions – an event study and a two-way fixed effects model. I use changes to household carbon emissions for over 250,000 movers across roughly 1,000 cities and 65,000 neighborhoods to estimate place effects and their contribution to heterogeneity in carbon emissions.

I begin my analysis by documenting observational patterns of city and neighborhood level variation in household carbon emissions in my sample. Detailed geographic identifiers in the administrative Census data make it possible to directly estimate these values. I estimate that households in cities with high average emissions emit 50% more than households in low emissions cities, and households in neighborhoods with high average emissions emit over two times more than households in low emissions neighborhoods. Accounting for variation driven by observed household characteristics such as household size and income decreases the dispersion across place estimates, but by less than 10%.

The heterogeneity that remains after accounting for observable household characteristics reflects some combination of unobserved household characteristics or preferences and place effects. Place effects could arise from a variety of local amenities that act as complements or substitutes to energy consumption. For instance, place effects could reflect climate, public transportation, bike or pedestrian infrastructure, highway networks, density, and/or zoning regulations. They could also reflect supply-side factors that determine fuel shares and electricity emissions factors. In the conceptual framework of this paper, I show how place effects relate to the parameters of a consumer energy demand model in which the intercept and slope of the demand curve vary across place.

I estimate the contributions of place effects and unobserved household characteristics to heterogeneity in carbon emissions using two versions of a mover design. The first decomposition is based on an event study, as in Finkelstein, Gentzkow, and Williams (2016), Chetty and Hendren (2018), and Finkelstein, Gentzkow, and Williams (2020). I estimate changes in household carbon emissions as a share of the change in mean emissions between a mover's origin and destination. I estimate that the majority of differences between places – about 90% across cities and 60% across neighborhoods – is attributable to place effects. These estimates don't account for variation in carbon emissions across households within the same place, and they also impose a restriction that there is no systematic selection of certain types of households to certain types of neighborhoods.

To allow for unrestricted patterns of sorting, the second decomposition is based on estimates from a two-way fixed effects model. This approach was first introduced by Abowd, Kramarz, and Margolis (1999), and has been used extensively in the labor literature to study the role of firms in wage inequality (e.g. Card, Heining, and Kline 2013; Card, Cardoso, and Kline 2016; Lachowska et al. 2020). It yields a variance decomposition of *overall* heterogeneity, including household variation within places as well as variation not explained by the model. I account for "limited mobility bias" – the upward bias in naive estimates of variance components that arises because some place effects are estimated from only a few movers (Andrews et al. 2008) – using the heteroskedasticity-robust "leave-out" estimator proposed by Kline, Saggio, and Sølvsten (2020). I find that place effects explain 16-19% of overall heterogeneity at the city level, and 24-26% of overall heterogeneity at the neighborhood level. Controlling for variation driven by climate and energy prices decreases the place shares to 10% at the city level and 15% at the neighborhood level. I find low correlations between unobserved household and neighborhood characteristics, even at the neighborhood level. This suggests that household sorting contributes to differences between places through "segregation" of households, but not in a way that is systematically correlated with unobserved neighborhood attributes. It also implies that differences between the event study and the two-way fixed effect decompositions result primarily from household variation within places and variation unexplained by the model.

While a large share of variation in overall household carbon emissions cannot be explained by places, my estimates nevertheless highlight that interventions that decrease place effects could result in considerable reductions to household carbon emissions. I estimate that if a household goes from living in a neighborhood with place effects one standard deviation above the national mean to a neighborhood with place effects one standard deviation below the national mean, their emissions from residential energy use and commuting would decrease by about 40%. Correlates of low emissions places for the most part mirror relationships in the observational data: low emissions places have higher density, more transportation alternatives to cars, and lower shares of single family homes.

I do not identify the causal effect of each of these amenities individually. Instead, I consider the impact on carbon emissions of urbanization, generally defined. To minimize mean squared prediction error, I first adjust my estimates of place effects using a linear empirical Bayes (shrinkage) estimator to down-weight parameters that are noisily estimated. I examine three scenarios. The first two scenarios are variants on the question: what would happen to household carbon emissions if places were more like New York City? The third scenario evaluates how carbon emissions would change if every place across the US were more like the largest city in its metropolitan or micropolitan area. This exercise doesn't take into account general equilibrium effects, but I see it as a useful first order approximation of the effect on household carbon emissions of policies that "expand" cities – e.g. either through policies that make it possible for more people to live in the city (without changing its fundamentals), or through investment and regional development that limits suburban sprawl and increases the number of neighborhoods that have amenities similar to those of the largest nearby cities. I estimate that this type of "place-based climate policy" would result in 13% reductions in average household emissions from residential energy use and commuting.

Taken together, the results in this paper provide new evidence on the role of places in household carbon emissions. I provide direct estimates of nation-wide, neighborhood level variation in household carbon emissions, building on evidence that this variation is substantial (Jones and Kammen 2014; Ummel 2014; Green and Knittel 2020), but finding less heterogeneity than predicted from national data projected onto local place and household characteristics.[1] While related work has highlighted the consequences of spatial heterogeneity in household carbon emissions for allocative efficiency (Glaeser and Kahn 2010; Colas and Morehouse 2021) and distributional impacts and political economy of hypothetical climate policies (Cronin, Fullerton, and Sexton 2019; Sallee 2019; Green and Knittel 2020), the focus of my paper is on examining its causes. I show that place effects can be interpreted as summarizing the parameters of a model of heterogeneous energy demand, where average demand and price elasticities of demand vary across places as a result of different amenities and supply-side factors. Several papers have generated estimates of heterogeneous energy demand parameters, but have necessarily done so in spatially limited contexts (Auffhammer and Rubin 2018; Gillingham 2014; Nowak and Savage 2013; Spiller et al. 2014). Finally, my work builds a bridge to a set of papers that has used observational data paired with modeling techniques to estimate strong relationships between urban form and carbon emissions (e.g. Shammin et al. 2010; Timmons, Zirogiannis, and Lutz 2016; Ribeiro, Rybski, and Kropp 2019; Pomponi et al. 2021; Ko 2013).

My empirical approach builds on a recent body of work using mover designs to estimate place effects in other settings, e.g. intergenerational mobility (Chetty and Hendren 2018), health care utilization and mortality (Finkelstein, Gentzkow, and Williams 2016; 2020), home prices (Schönholzer 2021), and wages (Card, Rothstein, and Yi 2021). I believe this is the first paper to

1. Differences in estimates could also be driven in part by the fact that these papers estimate household carbon footprints from all consumption, including indirect emissions from food and durable goods.

apply these methods to estimate the role of place in household carbon emissions. Even though a large share of variation in carbon emissions is driven by factors other than places, my results highlight considerable potential reductions in household carbon emissions from changes in the distribution of place effects, adding evidence to a broader literature on the key role that places play in individual outcomes. For example, many papers in the urban and spatial literature have examined the role of density (e.g. see Duranton and Puga 2020, for a review), transportation infrastructure (Tsivanidis 2019; Allen and Arkolakis 2021), and other amenities (e.g. Diamond 2016) in determining productivity, wages, and wage inequality. A more recent set of papers has explored the costs and benefits of using place-based policy to improve aggregate welfare (e.g. Kline and Moretti 2014; Gaubert, Kline, and Yagan 2019). And several papers have studied the effect of transportation infrastructure on urban form and energy use specifically (e.g. Baum-Snow 2007; Duranton and Turner 2018).

The remainder of this paper proceeds as follows. In Section 2, I discuss my empirical setting and data, and show several stylized facts about carbon emissions in the US. In Section 3, I present my model, and discuss the interpretation of place effects. In Section 4, I describe my empirical strategy and identifying assumptions. I present my main findings on the role of unobserved place vs. person heterogeneity in carbon emissions in Section 5. I then describe correlates of unobserved heterogeneity in Section 6, and predict how aggregate carbon emissions would change under counterfactual distributions of place effects in Section 7. Section 8 concludes.

## 2 Data and Stylized Facts about Carbon Emissions in the US

I build a 20-year panel of individual and household-level data using the 2000 restricted access Decennial Census long form and the 2001-2019 American Community Survey (ACS). The Census long form is a stratified random sample covering one in six households in the US, and the ACS is a stratified random sample covering 1% of households in the US each year except for 2001-2005 when it covered roughly 0.4% of households (U.S. Department of Commerce 2014). I link individuals across surveys using Protected Identification Keys, which are unique person identifiers assigned by the Census Bureau based on names, addresses, dates of birth, other household members, and social security numbers (when available).[2]

For every individual in my panel, I observe measures of residential and transportation energy use and a rich set of demographic, household, workplace, and home characteristics, including detailed geographic identifiers. I supplement the Census and ACS with several external data sets in order to convert energy expenditures to energy services and emissions, and to characterize places. In the remainder of this section, I define my geographic units of analysis and outcome variables, provide a high-level overview of the key control and explanatory variables I use, and discuss the construction of my analysis sample. Additional details can be found in Appendix B.

2. Neither the Decennial Census nor the ACS ask respondents for their social security number. Layne, Wagner, and Rothhaas (2014) use data with social security numbers to show that the error rate in assigning Protected Identification Keys without social security numbers is below 1%. See Wagner and Layne (2014) for detailed discussion of the assignment algorithm used by the Census, and Bond et al. (2014) for discussion of the variation in assignment rates across population subgroups.

## 2.1 Geographic units of analysis

Throughout the study, I analyze spatial heterogeneity at two levels of geographic granularity which are meant to represent roughly a labor market and a neighborhood.

My primary measure of labor markets are Core Based Statistical Areas (CBSAs). CBSAs are areas designated by the Office of Management and Budget and cover the population of metropolitan and "micropolitan" areas in the US: Each CBSA is a set of contiguous counties that contain at least one core area, of at least 10,00 people, and are highly economically integrated, as measured by commuting ties. In addition to formally designated CBSAs, I define residual CBSAs by state from unassigned rural areas. My primary measure of neighborhoods are census tracts. Census tracts are county subdivisions that typically cover contiguous areas, have populations of 1,200-8,000 people (4,000 on average), and are delineated with boundaries that follow identifiable physical features. They are designed to be relatively stable, but are split or merged every ten years if populations exceed or fall below the 1,200-8,000 window.[3]

## 2.2 Carbon Emissions

My primary outcome is metric tons of carbon emissions from residential energy and passenger vehicle use, which account for roughly one third of US greenhouse gas emissions.[4]

I estimate carbon emissions from residential energy use using household-reported expenditures on electricity, natural gas, and other home heating fuels in the last year, combined with external data on local annual retail prices and fuel emissions factors. For electricity, I calculate county-level average prices using data on utility from the Energy Information Administration (2020a) Annual Electric Power Industry Report, which reports, for every major utility in the US, sales, revenues, and total customers, by sector and state, as well as counties contained in the utility's service territory. I calculate county-level retail electricity prices using customer-weighted average price (revenue divided by sales) across utilities with service territories in the county, and I compute household electricity consumption by dividing expenditures by my price estimates. I then assign households to one of 12 National Electric Reliability Council subregions using a tract-level crosswalk from the Homeland Security (2021) Infrastructure Foundation-Level Database, and use the average annual emissions rates assigned to each subregion by the U.S. Environmental Protection Agency (2021) Emissions & Generation Resource Integrated Database. For natural gas and other home heating fuels, I obtain average retail prices at the state level from the Energy Information Administration (2020b) State Energy Data System. If a household reports non-zero expenditures on "other home heating fuels", I impute the fuel used from their answer to the question "What was the primary fuel used for home heating?" Finally, I obtain fuel emissions factors from the Environmental Protection Agency (2018) Emission Factors for Greenhouse Gas Inventories.

3. Census geographic definitions vary over time to account for changes in administrative boundaries and populations. To ensure that I don't erroneously identify people who live in places where the designation changed as movers, I use the 2000-2010 census block concordance to assign 2010 geographic definitions to all years in the data.

4. 75% of US greenhouse gas emissions are from burning fossil fuels. Of these, 20% are from residential energy use (including electricity), and another 20% are from light duty (i.e. passenger) vehicles (Administration 2020).

I estimate carbon emissions from transportation energy from individually-reported commute characteristics. My outcome captures variation in carbon emissions driven by commute lengths, number of commutes, and mode of transit.[5] I estimate commute distance using the geodesic distance between home and place of work census blocks, and I estimated commute speed from estimated mileage and reported time-length of commute. I estimate gasoline usage using annual national average fuel economy from the U.S. Environmental Protection Agency and Energy (2020), accounting for the fact that in general fuel economy is roughly 30% higher on highways than in cities. Finally, I estimate the number of annual commutes using reported weeks worked last year and hours worked last week, and convert annual gallons of gasoline to carbon emissions using the motor gasoline emissions factor from Energy Information Administration (2020b) State Energy Data System. Individuals who commute by rail, subway, streetcar, bus, bike, or walk, and individuals who work from home are assigned zero $CO_2$.[6] I examine the sensitivity of my results to using the Federal Highway Administration (2019) National Household Travel Survey (NHTS) to predict fuel economy and non-commute miles from household and geographic characteristics available in both the Census and NHTS. This is not my baseline approach, as it infers how much of variation in vehicle fleets and fuel economy observed in the NHTS is driven by individual preferences vs. place-based factors from cross-sectional variation.[7]

## 2.3 Individual and Household Characteristics

Throughout the analysis, I use demographic and household characteristics to control for variation driven by observable characteristics. My primary demographic and household controls are age, education (completion of a bachelor's degree), sex, race and ethnicity, household income (from salaries and wages, interest, social security, supplemental security, public assistance, retirement, and self employment), household size, and number of kids. I control for age using bins: 18-24, 25-30, 30-34, 35-39, 40-49, 50-64, and 65+. I control flexibly for number of kids using categorical variables for 0, 1, 2, or 3+ kids. Household level characteristics are taken as averages over person characteristics.

As highlighted in Card, Cardoso, and Kline (2016), the normalization choice for categorical variables does not affect the estimated size of the place variance component or the variance component of the sum of fixed and observable household effects, but it does affect the relative sizes of the place and unobserved household effects, as well as the estimated covariances. Throughout my analysis, I choose the age bin 40-49, no college degree, male, white & non-Hispanic as the omitted categories. Other than "white", these are the categories with the highest within-group variance in outcomes, thus this normalization will err towards finding a larger unobservable preference component relative to place component.

5. Commuting accounts for about 28% of all vehicle-miles travelled, and 39% of person-miles travelled on transit systems (US Department of Transportation 2015), which means I underestimate $CO_2$ emissions from personal vehicle use for most people in my sample.

6. This is a generous assumption that favors public transit. It is roughly correct on the intensive margin, but not on the extensive margin unless new investment is required to be zero-emissions.

7. Place-based factors that contribute to variation in vehicle fleets could include social norms, perceptions of safety (e.g. if everyone around you is driving a big car it is safer for you to drive a big car; certain types of cars may be able to handle adverse weather better), road widths, ease of parking, etc.

I also observe home-owner status, whether a household lives in a detached single family home, building age, and the number of vehicles in a household. Because these characteristics are intermediate outcomes, which affect $CO_2$ and likely reflect some combination of household preferences and place characteristics, I do not use these variables as controls throughout my analysis. I do, however, use them in the second half of the paper to explore correlates of unobserved place and household heterogeneity.

### 2.4 Place characteristics and amenities

In addition to individual-level data on home characteristics from the full sample in my microdata, I use several external sources of data to characterize amenities at the block, tract, city and regional level. My focus is on amenities that are directly relevant to energy consumption and carbon emissions in the residential and transportation sectors.

To account for variation in climate, I use data on annual heating degree days (HDDs) and cooling degree days (CDDs) at the state-climate division level from National Oceanic and Atmospheric Administration (2020). Degree days are computed as the annual sum of the daily difference between that day's temperature and 65F, and are meant to be a measure of the heating and cooling requirements of a place.

My main measures of local neighborhood amenities are downloaded from Walk Score, a private company that generates estimates of the walk-ability, transit-ability, and bike-ability of every address in the US.[8] Walk Score® rankings capture proximity to different commercial amenities such as grocery stores as well as street characteristics such as block lengths and intersection widths. Bike Score™ indices capture characteristics that make biking more or less accessible, such as the existence of bike lanes, road connectivity, and hilliness. Transit Score® ratings capture proximity to different types of transit, and the frequency and connectivity of nearby options. For transit, I also observe the number of bus routes and rail routes within a half mile, as well as a set of amenity scores that measure proximity to parks and leisure and commercial amenities (e.g. grocery stores, restaurants, retail). Other than route counts, each score is an index from 0-100. I assign over 6 million unique Walk Score points reflecting data from early 2020, one to every populated census block in the US, by matching census block centroids to the nearest Walk Score latitude-longitude coordinate.

#### 2.4.1 Analysis Samples

I restrict my analysis to individuals who are at least 18 years of age, who are not identified as the householder's child or grandchild, and who are not missing any of the outcome variables or key explanatory or control variables described above. I also impose several additional restrictions related to energy variables. I exclude from the sample individuals belonging to households whose residential energy costs are included in rent, or whose gas costs are included in their electricity bill, because I don't observe expenditures in those cases. I also exclude individuals in households where residential energy use is top coded or whose commute time is top coded, as the top-coding

8. Data provided by Redfin Real Estate.

will obfuscate changes in individual consumption for the highest demand individuals. Lastly, I exclude individuals if the sum of their household residential energy expenditures is zero, if they are in the bottom 1% of non-zero residential energy cost observations, or if they are in the top 1% of commute distance observations as these outliers more likely reflect survey misreporting. My full sample consists of all individuals who meet these restrictions across the 48 continental states and the District of Columbia. This is over 16 million people across 12 million households (Table 1, column (1)). I use the full sample to estimate observational geographic and household heterogeneity.

I construct a panel sample by restricting the full sample to individuals for whom I have at least two observations, and who did not indicate in the ACS that they had moved within the last year.[9] This restriction ensures that I am assigning residential energy expenditures to the correct location. The panel sample consists of 1,062,000 people across 889,000 households (Table 1, column (2)).

Finally, I impose two additional geographic restrictions which are necessary for the implementation of my empirical strategy. First, because residential energy is determined at the household level, and place effects are identified from the variation in outcomes of movers between places, I restrict the sample to only individuals who live with the same set of other full sample individuals across observations.[10] Second, I restrict CBSAs and tracts to the "leave-out connected set" – the network of CBSAs or tracts that remain connected to each other by at least one mover when I drop all the observations in any given household. I do this after dropping tracts with fewer than 10 full sample household observations. The networks are constructed separately at the CBSA and tract level. This means it is possible for a household to be in the CBSA panel but not the tract panel if the tracts they live in are not in the leave-out connected set of tracts. The leave-out restriction drops a negligible share of (residual) CBSAs and roughly 13% of (disproportionately rural) tracts, yielding approximately a 5% sample size reduction (Table 1, Columns (4) and (6)). CBSA movers are households in the CBSA panel that live in different CBSAs across observations (99,500 people in 87,500 households, Column (5)), and similarly, tract movers are households in the tract panel that live in different tracts (within or across CBSAs) across observations (275,000 people in 236,000 households, Column(6)). The CBSA panel, tract panel, CBSA movers, and tract movers make up my four primary analysis samples. All estimates are weighted using DEC/ACS sample weights.

9. In DEC, the question asked whether respondents had moved within the last five years. Since this is significantly more restrictive, I don't drop these individuals.

10. This restriction is weaker than requiring individuals live in a consistent household across observations. In particular, if someone lives with different roommates across observations, but their roommates aren't in the full sample because of e.g. missing variables, I do not drop them from the data. Moreover, because people under the age of 18 are dropped from the full sample, this does not drop households that have new children or households in which children move out as they become adults.

### 2.5 Sample Statistics

Table 1 shows sample statistics for the full sample, unrestricted panel sample, the two geographically restricted panel samples, and the two mover samples.

A comparison across the samples yields three main take-aways. First, individuals in the panel are on average more likely to be white and have higher income than the full sample (columns (1) and (2)). This reflects known heterogeneity in Protected Identification Key assignment rates within the Census Bureau (Bond et al. 2014). The panel sample is 6 percentage points likely to live in a tract designated as urban by the Census, 8 percentage points more likely to live in a detached home, and 1 percentage point more likely to commute by car. Second, further restricting the baseline panel to the CBSA and tract panels (columns (3) and (4)) does not meaningfully change the distribution of demographics, (intermediate) outcomes, or place characteristics. Finally, movers (columns (5) and (6)) tend to be younger, more credentialed, and have higher income (conditional on age) than both stayers and the full sample. Movers also are more likely than stayers to live in urban tracts, less likely than stayers to live in detached homes, and they have higher rates of using electric heating and have lower emissions from residential energy, making them more comparable to the full sample on all of these dimensions.

Overall, about 80% of household carbon emissions in my sample are from residential energy, and about 20% are from commuting. Close to three quarters of the sample live in a detached, single family home, a vast majority of the sample commutes by car, and on average households live within half a mile of only one bus route and only 0.1 rail routes.

Table 2 shows additional statistics for the panel sample. I observe the vast majority of my sample exactly twice, with on average 8-10 years in between observations. Movers tend to be younger than stayers the first time I observe them, and are much more likely to have had a child or greater than 50% increase in household income. Households tend to move to places with higher shares of detached single family homes and worse non-car transportation amenities. The majority of moves in my household are from urban to urban tracts, urban to suburban tracts, or suburban to suburban tracts. Finally, consistent with secular trends of mobility in the US, households are generally moving to places that are warmer (16-21% reductions in cooling degree days, and 6-11% reductions in heating degree days). Additional statistics on mover selection and patterns of mobility can be found in Appendix Table A.1, Table A.2, and Table A.3a.

Table 1: **Sample Statistics**

| | | Panel Sample | | | Mover Sample | |
|---|---|---|---|---|---|---|
| | (1) Full | (2) All | (3) CBSA | (4) Tract | (5) CBSA | (6) Tract |
| **A: Demographics** | | | | | | |
| College | 0.25 | 0.25 | 0.25 | 0.25 | 0.35 | 0.31 |
| Age | 44 | 46 | 46 | 46 | 43 | 43 |
| White | 0.82 | 0.89 | 0.89 | 0.90 | 0.89 | 0.88 |
| Female | 0.48 | 0.47 | 0.47 | 0.47 | 0.45 | 0.46 |
| Household income | 103,700 | 114,700 | 114,800 | 115,500 | 116,700 | 116,200 |
| Household kids | 1.0 | 1.0 | 1.0 | 1.0 | 1.0 | 1.0 |
| Household size | 2.8 | 2.9 | 2.9 | 2.9 | 2.8 | 2.9 |
| **B: Outcomes** | | | | | | |
| Tons $CO_2$ | 18.7 | 19.9 | 19.8 | 19.9 | 18.8 | 18.7 |
| Tons $CO_2$ – *Residential* | 15.2 | 16.3 | 16.3 | 16.4 | 15.2 | 15.4 |
| Tons $CO_2$ – *Commute* | 3.5 | 3.5 | 3.5 | 3.5 | 3.5 | 3.4 |
| **C: Intermediate Outcomes** | | | | | | |
| % Detached home | 72.4 | 80.9 | 80.9 | 81.5 | 73.3 | 72.9 |
| % Use electricity only | 28.8 | 23.5 | 23.6 | 23.5 | 29.8 | 27.5 |
| % Commute by car | 94.9 | 96.3 | 96.3 | 96.6 | 95.8 | 96.2 |
| Commute minutes | 25.3 | 24.9 | 24.9 | 24.8 | 26.2 | 25.7 |
| **D: Place Characteristics** | | | | | | |
| % Urban | 32.2 | 26.3 | 26.4 | 25.6 | 30.6 | 32.0 |
| % Suburban | 46.3 | 43.9 | 44.0 | 44.4 | 44.1 | 47.7 |
| % Rural | 21.5 | 29.8 | 29.6 | 30.0 | 25.3 | 20.4 |
| Walk Score | 26.25 | 22.64 | 22.69 | 21.98 | 22.08 | 24.39 |
| Bike Score | 35.38 | 33.10 | 33.13 | 32.77 | 33.58 | 34.92 |
| Transit Score | 9.07 | 6.92 | 6.95 | 6.51 | 6.83 | 7.96 |
| N Bus routes | 1.58 | 1.16 | 1.16 | 1.07 | 1.24 | 1.35 |
| N Rail routes | 0.16 | 0.09 | 0.09 | 0.08 | 0.10 | 0.10 |
| Annual CDD | 1,364 | 1,224 | 1,226 | 1,214 | 1,361 | 1,339 |
| Annual HDD | 4,369 | 4,796 | 4,786 | 4,828 | 4,483 | 4,510 |
| N People | 16,200,000 | 1,062,000 | 1,040,000 | 1,006,000 | 99,500 | 275,000 |
| N Households | 12,190,000 | 889,000 | 836,000 | 807,000 | 87,500 | 236,000 |
| CBSAs | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 |
| Tracts | 71,500 | 69,500 | 69,500 | 60,500 | 53,500 | 60,500 |

**Note:** Column (1) shows statistics for the full sample. Column (2) shows statistics for the panel sample, with no restrictions that individuals be in the same household or live in a connected geography. Columns (3) and (4) show the panel samples restricted to individuals in a consistent household overtime and the CBSA and tract leave-one-out connected sets, respectively. Columns (5) and (6) show statistics for the CBSA and tract mover samples. All means are weighted using census sample weights. Counts and shares are unweighted and rounded according to Census Bureau disclosure rules.

Table 2: **Panel Statistics**

| | Panel Sample | | Mover Sample | |
|---|---|---|---|---|
| | CBSA | Tract | CBSA | Tract |
| **A: Sample Characteristics** | | | | |
| % first observed in 2000 | 9.8 | 9.9 | 15.3 | 13.7 |
| Years between obs | 7.8 | 7.8 | 10.2 | 9.8 |
| **B: Demographic Characteristics** | | | | |
| Age first observed | 42.1 | 42.0 | 37.2 | 37.2 |
| % $\lvert\Delta$ HH income $\rvert > 50\%$ | 28.0 | 27.9 | 44.6 | 40.3 |
| $\Delta$ num. kids | -0.12 | -0.12 | 0.07 | 0.08 |
| % $\Delta$ num. kids $> 0$ | 18.6 | 18.7 | 29.8 | 29.8 |
| **C: Mover Place Changes** | | | | |
| $\Delta$ Walk Score | | | -6.4 | -6.5 |
| $\Delta$ Bike Score | | | -3.9 | -3.9 |
| $\Delta$ Transit Score | | | -2.3 | -2.7 |
| $\Delta$ N Bus Routes | | | -0.5 | -0.5 |
| $\Delta$ N Rail Routes | | | -0.04 | -0.04 |
| $\Delta$ Tract % detached home | | | 0.05 | 0.05 |
| % Moves Urban-to-Urban | | | 12.4 | 17.9 |
| % Moves Urban-to-Suburban | | | 15.3 | 13.5 |
| % Moves Suburban-to-Suburban | | | 20.6 | 28.4 |
| %$\Delta$ CDD | | | 21.4 | 16.4 |
| %$\Delta$ HDD | | | -10.7 | -6.1 |
| N People | 1,040,000 | 1,006,000 | 99,500 | 275,000 |
| N Households | 836,000 | 807,000 | 87,500 | 236,000 |
| CBSAs | 1,000 | 1,000 | 1,000 | 1,000 |
| Tracts | 69,500 | 60,500 | 53,500 | 60,500 |

**Note:** Columns (1) and (2) shows panel statistics for the CBSA and tract panel samples. Columns (3) and (4) show statistics panel statistics as well as summary measures of mobility patterns for the CBSA and tract mover samples. All means are weighted using census sample weights. Counts and shares are unweighted and rounded according to Census Bureau disclosure rules.

### 2.6 Observational Heterogeneity

Carbon emissions from residential energy and passenger vehicle use vary immensely across individuals in the full sample. Individuals one standard deviation above the national mean emit 3.4 times as much as individuals one standard deviation below the national mean. Patterns of energy use are strongly correlated with observable characteristics such as income, household size, race & ethnicity, and education. Appendix Figure A.1 shows relationships between carbon emissions and these characteristics. Accounting for observable characteristics decreases heterogeneity across individuals, but significant variation remains: carbon emissions of individuals one standard deviation above the mean are still three times higher than those of individuals one standard deviation below the mean, holding differences in individual observables fixed. A nonparametric regression of household carbon emissions on a set of fixed effects for age, college education, race and ethnicity, household income, household size, and number of children indicates that these characteristics can explain 15% of overall variation in carbon emissions.

There is also substantial spatial variation in $CO_2$ across the United States. I estimate unconditional and conditional place means, $\mu_j$, using an ordinary least squares regression of log of individual $CO_2$ onto place fixed effects, year fixed effects $\tau_t$, and in the conditional regression, individual and household observable characteristics $X_{it}$:

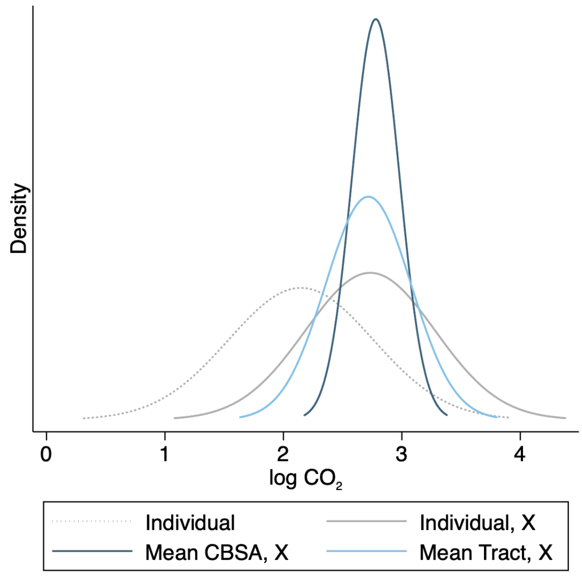
$$\ln CO_{2it} = \mu_{j(i,t)} + X_{it} + \tau_t + \varepsilon_{it} \tag{1}$$

Per capita carbon emissions in CBSAs one standard deviation above the mean are about 54% higher than per capita carbon emissions in CBSAs one standard deviation below the mean, with that difference decreasing only slightly to 50% when accounting for differences in population compositions across areas. At the neighborhood level, individuals in high emissions neighborhoods emit on average 2.2 times what individuals in low emissions neighborhoods do, or 2.1 times more after accounting for differences in observables. Figure 1 shows normal distributions reflecting the mean and standard deviation of per capita carbon emissions over individuals, CBSAs, and tracts. The dotted gray line shows the raw distribution, and the solid lines show conditional means. Even after accounting for observational characteristics, significant spatial heterogeneity remains, particularly at the neighborhood level. For the remainder of this analysis, I refer to CBSA and neighborhood means conditional on observable characteristics as "obervational means", following the terminology used by Abaluck et al. (2021).

Figure 2 shows how carbon emissions differ across urban, suburban, and rural areas. Suburban and especially rural places have higher emissions than urban places. Controlling for heterogeneity driven by individual observable characteristics decreases the gap between urban and suburban households by almost half, from 2.5 tons to 1.5 tons, and also decreases the gap between urban and rural household by 1 ton, from 6.5 to 5.5.[11]

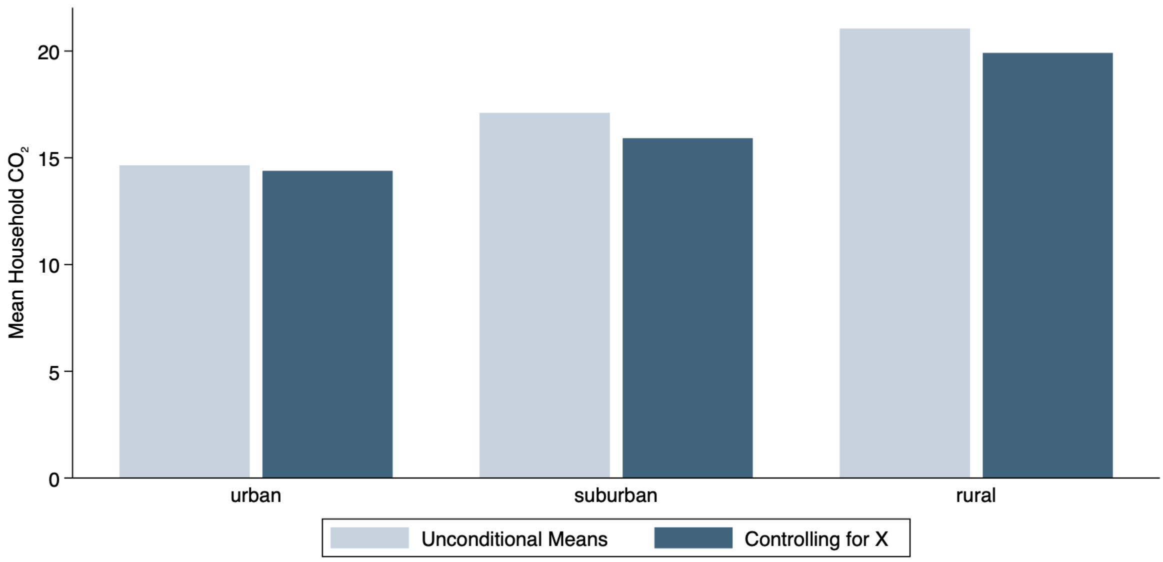
11. Relatively small differences between urban and suburban means potentially reflect a pretty broad definition of urban by the Census.

Figure 1: **Heterogeneity in Individual Carbon Emissions**




**Note:** This figure shows gaussian curves with means and normals reflecting the true distributions of per capita emissions, across individuals, CBSAs, and tracts. Raw distributions are not shown in order to facilitate Census disclosure review processes, but have higher kurtosis and are negatively skewed. The dotted gray line (labeled "Individual") corresponds to the distribution of individual $CO_2$, conditioning only on year FEs. The solid gray line (labelled "Individual, X") corresponds to the distribution of $CO_2$ over individuals conditional on year FEs and observable characteristics. The dark blue solid line ("Mean CBSA, X") and light blue solid line ("Mean Tract, X") correspond to the distributions of CBSA and tract (respectively) mean per capita $CO_2$ conditional on observable year FEs and characteristics. Observable characteristics include age, gender, race, education, household size, and number of children.

Figure 2: **Household Carbon Emissions in Urban, Suburban, and Rural Places**




**Note:** This figure shows mean household $CO_2$ for urban, suburban, and rural areas. Observable characteristics include fixed effects for age, gender, race, education, household size, and number of children. Places are defined as urban if they are designated as an urban tract by the census. Places are defined as suburban if they are not designated as an urban tract by the census, but are contained within a CBSA. Rural areas are tracts outside of CBSAs. The unconditional regression has an $R^2$ of 0.08,and the conditional regression has an $R^2$ of 0.21.

Figure 1 and Figure 2 highlight substantial spatial heterogeneity in carbon emissions. They also show that while observational heterogeneity in household carbon emissions is partially driven by sorting of households with different characteristics to different types of places, the majority remains unexplained. The fundamental goal of this paper is to understand how much of is remaining unexplained heterogeneity is driven by unobservable individual preferences, and how much is driven by causal place effects, i.e. the amount by which the same household's carbon emissions would differ from place to place, due to the underlying features of that place, holding household characteristics (including unobserved preferences or endowments) fixed.

# 3 Model

Individual $i$ living in place $j$ consumes energy $E$ in the form of four categories of fuels ($f$). In the residential sector, they can consume electricity ($e$), natural gas ($n$), and other heating fuels ($o$). In the transportation sector they can consume motor gasoline ($m$).[12] Average demand $a_j$, price elasticities of demand $\rho_j^f$, and prices $P_j^f$ are allowed to vary by place. Demand also depends on observable fixed and time varying characteristics (such as age, household size, and income) $X_{it}$, individual fixed unobserved determinants of demand, $\alpha_i$, individual time-varying unobserved determinants of demand $\varepsilon_{it}$, and national annual trends $\tau_t$. Thus, individual demand for residential and transportation energy is given by:

$$\ln E_{it} = a_j + \sum_{f \in \mathcal{F}} \rho_j^f \cdot \ln P_j^f + X_{it}\beta + \tau_t + \alpha_i + \varepsilon_{it} \tag{2}$$

Place-based differences in average energy demand and in price elasticities of demand could arise from a range of fixed and malleable characteristics of places. These include climate, built environment or urban form – e.g. public transit, pedestrian and bike infrastructure, proximity to highways and parking, density, and proximity to leisure and commercial amenities – and regulatory characteristics – e.g. zoning restrictions or clean electricity standards. For example, average demand for heating fuels is higher in cold places, and average demand for motor gasoline is higher in places where households live farther from employers or commercial amenities. Price elasticity of demand for gasoline may be higher in places with better alternative transportation options, and price elasticity of demand for electricity may be higher in places with a larger variety of home sizes and styles.

For each energy type $E^f$, carbon emissions, $CO_2$, are a product of fuel consumed and fuel-(and-place) specific carbon emissions factors $\phi^f$, which reflect the physical carbon content of fuel. Emissions factors are fixed across both time and place for natural gas, oil, and motor gasoline. Electricity emissions factors vary both over time and place, reflecting differences in fuels used for electricity generation. Thus, household consumption of residential and transportation

12. Electric vehicles are a negligible share of driving in my sample time frame. If someone has an electric vehicle, I over-estimate their emissions, both because of the comparison between gasoline and electricity, and because the electricity they use to charge their vehicle is included in residential energy (if they charge at home).

energy results in carbon emissions:

$$CO_{2it} = \phi^e_{jt} \cdot E^e_{it} + \phi^{ng} \cdot E^{ng}_{it} + \phi^o \cdot E^o_{it} + \phi^{mg} \cdot E^{mg}_{it}$$

Define $\bar{\phi}_j$ as the average emissions factor for energy consumption at place $j$. This will depend on the emissions factor of electricity in place $j$, as well as average fuel shares. Define $\phi_{ij}$ as person $i$'s average emissions factor from aggregate energy consumption when they are living in place $j$. For each person carbon emissions are given by the product of their average emissions factor and their total energy consumption

$$\begin{aligned} CO_{2it} &= \phi_{ij} \cdot E_{it} \\ &= \frac{\phi_{ij}}{\bar{\phi}_j} \cdot \bar{\phi}_j \cdot E_{it} \end{aligned}$$

Combining this expression with Equation 2 yields

$$\begin{aligned} \ln CO_{2it} &= \underbrace{\ln \bar{\phi}_j + a_{j(i,t)} + \sum_{f \in \mathcal{F}} \rho^f_{j(i,t)} \cdot \ln P^f_{j(i,t)}}_{\psi_j} + X_{it}\beta + \tau_t + \underbrace{(\ln \phi_{ij} - \ln \bar{\phi}_j) + \alpha_i}_{\tilde{\alpha}_i} + \varepsilon_{it} \\ &= \psi_j + X_{it}\beta + \tau_t + \tilde{\alpha}_i + \varepsilon_{it} \qquad (3) \end{aligned}$$

This derivation makes evident that place effects $\psi_j$ capture place-based variation in average energy demand, variation in price elasticities of energy demand, variation in prices, and average fuel emissions factors (which reflect a combination of average electricity emissions factors and average fuel shares). Individual effects $\alpha_i$ capture relative energy demand, and relative fuel emissions factors (i.e. relative fuel-specific demand, or individual deviations from place-based average fuel shares). Equation 3 is the baseline estimating equation I take to the data. A comparison with Equation 1 highlights the potential bias from inferring place-based heterogeneity from observational means: $\mu_j = E[\alpha_i | i \in j] + \psi_j$. In words – even after accounting for variation driven by sorting on observable household characteristics, observational means $\mu_j$ reflect the combination of place effects and the average unobserved characteristics of the people living in those places.

### 3.1 Variance Decomposition

Using the two-way fixed effects model derived in Equation 3, heterogeneity in household carbon emissions can be decomposed as below (lumping $\tau_t$ with $X_{it}$ for brevity):

$$\begin{aligned} Var(y_{ij}) = &Var(\psi_j) + 2 \cdot Cov(\alpha_i, \psi_j) + Var(\alpha_i) \\ &+ Var(X_{it}\beta) + 2 \cdot Cov(\alpha_i, X_{it}\beta) + 2 \cdot Cov(\psi_i, X_{it}\beta) + Var(\varepsilon_{it}) \end{aligned}$$

The focus of my analysis is on the first three terms: the variance component of place effects, the variance component of unobserved person effects, and their covariance, which captures the spatial heterogeneity that results from systematic sorting on unobserved preferences. Abusing notation, I re-define $y_{it}$ as the residualized outcome, after having regressed household carbon emissions on time effects and observed household characteristics. For the remainder of this section, I discuss the variance decomposition for this residualized outcome.

$$Var(y_{it}) = Var(\psi_j) + 2 \cdot Cov(\alpha_i, \psi_j) + Var(\alpha_i) + Var(\varepsilon_{it}) \tag{4}$$

I follow Song et al. (2019) and further decompose unobserved heterogeneity into a between-place component $Var_j(\bar{y}_j)$, which captures the variation in mean household carbon emissions across places, and a within-place component $Var_i(y_{it} - \bar{y}_j | i \in j)$, which captures the heterogeneity in carbon emissions of households living in the same place:

$$\begin{aligned} Var(y_{it}) &= Var_j(\bar{y}_j) + Var_i(y_{it} - \bar{y}_j | i \in j) \\ &= \underbrace{Var(\psi_j) + 2 \cdot Cov(\bar{\alpha}_j, \psi_j) + Var(\bar{\alpha}_j)}_{\text{Between}} + \underbrace{Var(\alpha_i - \bar{\alpha}_j) + Var(\varepsilon_{ij})}_{\text{Within}} \end{aligned} \tag{5}$$

Equation 5 highlights that heterogeneity between places reflects variation in place effects, sorting of certain types of households to certain types of places (the covariance term), and what Song et al. (2019) refer to as segregation of households, i.e. the extent to which households of different types segregate across places, whether or not this pattern reflects systematic sorting on place types.[13] In addition to the between-place heterogeneity, overall heterogeneity reflects heterogeneity in household carbon emissions within places, as well as heterogeneity that cannot be explained by the two-way fixed effects model.

## 4 Empirical Strategy

My empirical strategy uses moves across places to estimate place effects and their contribution to spatial heterogeneity in carbon emissions. The intuition behind the mover design is the following: Suppose high-emissions places are high emissions because of a causal place effect, for example because there are no alternatives to commuting other than by car, or because zoning regulations constrain the types of homes households can live in. Then when a household moves from an on average high emissions place to an on average low emissions place, their carbon emissions should decrease because of lower-emissions alternatives now available to them. Conversely, if spatial heterogeneity is driven by strong preferences, then, households that currently live in detached single family homes and commute by car would continue to do so even given alternate options, and moving from on average high to low emissions places should have little effect on household carbon emissions.

I decompose carbon emissions heterogeneity using two versions of the mover design. The first is an event study that characterizes movers' changes in emissions as a share of origin-destination differences in mean carbon emissions. This approach gives a decomposition of heterogeneity between places, although it is not a decomposition of variance terms (and place shares are not constrained to fall between zero and one). It is also unbiased only if there is no systematic

13. $\bar{\alpha}_j \equiv E[\alpha_i | i \in j]$

sorting of household types to place types. While this assumption is somewhat restrictive, the event study approach is much more efficient than estimating the full two-way fixed effects model. It yields causal estimates under these stronger assumptions on sorting, but is useful for prediction and as descriptive evidence under weaker assumptions. The second approach estimates a non-parametric distribution of household and place effects using the two-way fixed effect model derived in Section 3. This approach gives a decomposition of overall heterogeneity, and yields unbiased estimates under weaker assumptions on selection. In the remainder of this section I discuss the mover design identifying assumptions and then each of these variance decompositions in turn.

### 4.1 Identifying Assumptions

Estimates from both versions of the mover design are unbiased under three assumptions, as highlighted in Hull (2018): (1) additive separability of place effects, or constant effects (2) non-persistence of outcomes, and (3) exogenous mobility, or conditional orthogonality.

*Assumption 1:* Additive Separability of place effects, or constant effects.

A core modeling assumption of the two-way fixed effect design is that the outcome – log carbon emissions – is additively separable in person and place effects. This specification implies that place effects increase and decrease $CO_2$ proportionally by the same amount for everyone. This is realistic for several potential mechanisms through which place effects could arise. Most obviously, fuel emissions factors affect carbon emissions levels multiplicatively, and similarly, it is natural to model climate as scaling residential heating or cooling needs up or down by the same factor for everyone. To take a few other examples: if place effects are driven by density, it may be reasonable to expect places with higher density to decrease the size of homes (and therefore residential energy requirements) or the length of commutes (and therefore transportation energy requirements) by the same factor for low and high baseline users. Similarly, an increase in transportation alternatives to cars might decrease the share of trips taken by car for all households proportionally.

While it is easy to imagine place-based factors that operate in these ways, the model imposes a substantial restriction. It does not allow for heterogeneous treatment effects or match effects. Heterogeneous place effects could arise if, for example, place effects are due to a public transit option that only low-income households use but doesn't change high-income household behavior, or if all households use the public transit option but low-income households get rid of their car and eliminate all car trips, while high-income households eliminate only a share. Alternatively, heterogeneous place effects might arise if there is not a lot of variation across places in e.g. number of car trips taken or home sizes for low baseline users, but high users respond strongly to places with particularly good or bad amenities. If such heterogeneity exists *and* there's selection of certain types of households to certain types of places, then my estimates will be biased towards the local average treatment effect for the subgroup I tend to observe moving to a given place.

To rule out selection on heterogeneous effects and validate the log additive model, I follow Card, Heining, and Kline (2013) and test whether moving from a low $CO_2$ place to a high $CO_2$ place and moving from high $CO_2$ place to a low $CO_2$ place are associated with equal and opposite changes in household $CO_2$. Unlike in their setting, in which higher wages are unambiguously good, it is not ex-ante obvious whether we would expect selection to be assortative or disassortative. Nevertheless, testing for symmetry of moves provides evidence on the existence of either type of selection.

To see this, consider differences in potential outcomes across an origin $o$ and destination $d$, allowing now for there to be an interaction $\eta(\alpha_i \cdot \psi_j)$ between person and place types:

$$E[CO_{2it}(d)] - E[CO_{2it}(o)] = (\psi_d - \psi_o) + \eta(\alpha_i \cdot \psi_d) - \eta(\alpha_i \cdot \psi_o)$$

Because of the multiplicative nature of the interaction term, for a high-type household $h$ and a low type household $l$ moving between the same origin and destination:

$$|\eta(\alpha_h \cdot \psi_d) - \eta(\alpha_h \cdot \psi_o)| > |\eta(\alpha_l \cdot \psi_d) - \eta(\alpha_l \cdot \psi_o)|$$

Thus, regardless of whether the interactive term is positive or negative, and regardless of whether sorting is assortative or disassortative, this type of interaction, paired with selection, would lead to asymmetries between changes in household carbon emissions from moves to higher on-average places vs. lower on-average places.

I group places into four quartiles based on observational averages of $CO_2$, and I estimate individual $CO_2$ for each origin-destination quartile pair, adjusting for annual trends and controlling for demographic and household characteristics. Results are shown in Figure 3. For parismony, the figure shows only moves from the lowest quartile $CO_2$ places to all 4 quartiles and vice versa, as well as moves within 1st quartile places and moves within fourth quartile places as bounds in gray. Moves across quartiles lead to equal and opposite changes in household carbon emissions, suggesting that the log-linear model of place effects is a good approximation. The figures also provide evidence of selection, with households that move from the lowest quartile to a different place in the lowest quartile having lower emissions on average than households that move from the lowest quartile to higher quartiles (and vice versa).

*Assumption 2:* Non-persistent Outcomes.

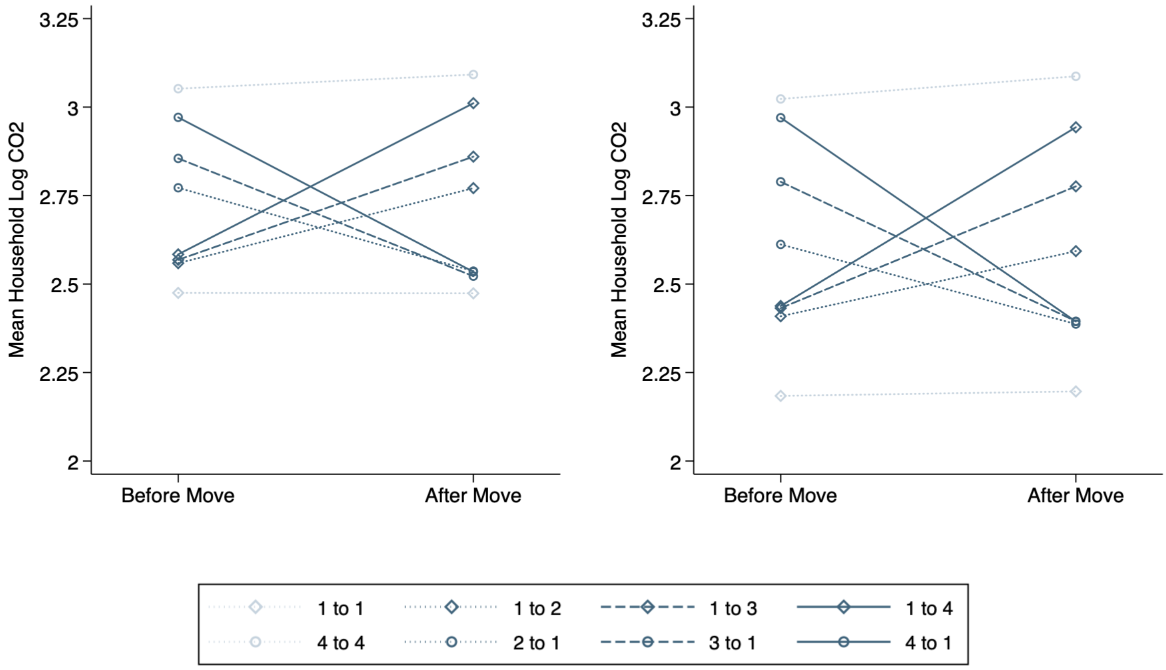
As highlighted above, relative place effects are identified from pairwise comparisons of household $CO_2$ between their origin and destination,

$$E[CO_{2it}(d)|\alpha_i, X_{it}, \tau_t] - E[CO_{2it}(o)|\alpha_i, X_{it}, \tau_t] = \psi_d - \psi_o$$

This expression holds for any two households moving between $o$ and $d$, regardless of the history of places $\{j\}$ they lived in prior. Note, however, that this doesn't rule out that the place somebody was born may have a persistent effect on their preferences and carbon emissions. Because I include household effects in the model, and only include individuals over the age of 18 in the sample, any persistent effect of place of birth and upbringing on carbon emissions will be captured by the household fixed effect.

Figure 3: **Changes in household $CO_2$ when moving across quartiles of Mean $CO_2$**

(a) CBSA (b) Tract




**Note:** This figure shows average household carbon emissions for movers across places classified into quartiles based on their mean carbon emissions in the full sample. Only the subset of moves to and from the lowest emissions places (quartile 1), as well as moves within the highest emissions places (quartile 4) are shown. Estimates are conditional on year fixed effects and the standard set of household characteristics used throughout this analysis.

*Assumption 3:* Exogenous Mobility, or conditional orthogonality.

Finally, estimates of place effects from the mover design are unbiased only if moves are strictly exogenous; in other words if shocks to unobserved determinants of $CO_2$ are conditionally uncorrelated with destination choices.

$$E[\epsilon_{it}|\alpha_i, \psi_{j(i,t)}, X_{it}, \tau_t] = 0 \tag{6}$$

Note that the two-way fixed effects model allows for unrestricted selection on fixed or time-varying observable characteristics and on fixed unobservable characteristics. For instance, entering middle age and having children is associated with an increase in energy consumption generally (Figure A.1), and also significantly increases the probability of moving to a suburb (Table A.2); however this endogeneity does not bias my estimates, because I observe age, household size, and number of children. Similarly, estimates of place effects are robust to household changes in energy demand and simultaneous moves to a new neighborhood that might arise from an increase (or decrease) in income, because I observe household income. Finally, if people have heterogeneous, but fixed, preferences for neighborhood amenities – e.g. if people have a particular distaste for public transit, a strong preference for large homes, or a particular love for walking or biking – and their choice of what neighborhood to live in reflects those preferences, estimates of place effects are unbiased because these unobserved but fixed determinants of $CO_2$ are captured by individual fixed effects. The ability to account for these time invariant unobserved preferences is a crucial benefit of the mover estimation strategy.

Thus, the main threat to identification is the possibility that moves correspond to *changes* in unobserved preferences – either single idiosyncratic shocks or evolving. A standard approach for ruling out this source of endogeneity is to test for parallel trends between movers and stayers prior to the move. A limitation of my data is that I observe the majority of my sample only twice, which makes it impossible to test for parallel trends. In Section 5.1, I show that the effect of moving appears to be stable across duration between observations, meaning estimates from households observed less than 5 years apart are similar to estimates from households observed more than 15 years apart. If moves were endogenous to preferences evolving, or "drifting" over time, you would expect that my heterogeneous parameter estimates would evolve in a parallel way. While somewhat comforting, this does not rule out the possibility of moves corresponding to a single idiosyncratic shock to preferences. To rule this out, I use data from the Panel Study of Income Dynamics (PSID), over the same sample period, and assess whether movers in the PSID exhibit any changes to energy expenditures prior to their move. While I do not know where households move from or to, I find that energy expenditures are flat leading up to a move and increase afterward, consistent with life-cycle trends presented in Table 2 of people moving to places with larger homes and fewer non-car transportation amenities, and with the secular trend over my sample frame of people moving to places with higher cooling needs. This result is shown Figure A.4.

### 4.2 Event Study Decomposition

The first decomposition I estimate is based on an event study, as in e.g. Finkelstein, Gentzkow, and Williams (2016). Consider a household $i$ that moves from origin $o$ to destination $d$. Household $i$'s expected change in carbon emissions is given by:

$$E[\ln CO_{2it}(d) - \ln CO_{2it}(o) | \alpha_i, X_{it}, \tau_t] = \psi_d - \psi_o$$

I re-express the change in place effects in terms of the share of differences between observational means, $\bar{y}_d - \bar{y}_o$, attributable to differences between place effects:

$$\begin{aligned} \psi_d - \psi_o &= \frac{\psi_d - \psi_o}{\bar{y}_d - \bar{y}_o} \cdot (\bar{y}_d - \bar{y}_o) \\ &\equiv \theta_{o,d} \cdot (\bar{y}_d - \bar{y}_o) \end{aligned}$$

Plugging this expression into the two-way fixed effect model yields an event study, which I use to estimate the share of differences *between* places attributable to place effects, $\theta$:

$$\begin{aligned}\ln CO_{2it} &= \alpha_i + \psi_{j(i,t)} + \tau_t + X_{it}\beta + \varepsilon_{it} \\ &= \alpha_i + \psi_o + \mathbb{1}[moved] \cdot (\psi_d - \psi_o) + \tau_t + X_{it}\beta + \varepsilon_{it} \\ &= \tilde{\alpha}_i + \mathbb{1}[moved] \cdot \theta \cdot (\bar{y}_d - \bar{y}_o) + \tau_t + X_{it}\beta + \varepsilon_{it} \end{aligned} \tag{7}$$

Relative to the unrestricted two-way fixed effects model, the event study approach vastly reduces the dimensionality of the estimation problem, as now the place share of heterogeneity is characterized by a single parameter $\theta$ as opposed to the full distribution of J place effects. However, this efficiency comes at the cost of an additional assumption, that heterogeneity in $\theta$ cannot be correlated with other parameters in the model. In other words, because place types are inferred from observational means, the event study limits selection of households to places so that there is no systematic sorting of e.g. high type households to high type places. In Equation 5, this amounts to requiring the covariance term to be equal to zero.[14]

### 4.3 Two-way Fixed Effects Decomposition

The second decomposition I estimate is the one described in Equation 4 (and shown again below), which is based on estimation of the full two-way fixed effects model, allowing for unrestricted correlations between place effects and household characteristics.

$$Var(y_{it}) = Var(\psi_j) + 2 \cdot Cov(\alpha_i, \psi_j) + Var(\alpha_i) + Var(\varepsilon_{it})$$

In contrast to the event study decomposition, the two-way fixed effects decomposition allows unrestricted sorting of households across places, and the variance share attributable to household heterogeneity reflects not only the between component (i.e. how households differ across places on average) but also the within component (i.e. how much variation in carbon emissions there is across observably similar households within the same place).

A well-documented challenge to estimating variance components in two-way fixed effect models is limited mobility bias (Andrews et al. 2008): estimates of place effects are noisy because they are estimated from a small sample of movers to and from each place. This creates an upward bias in the plug-in variance estimate relative to the true variance of place effects, even if estimates of place effects themselves are unbiased. To address this, I estimate variance components using the heteroskedasticity-unbiased leave-out estimator proposed by Kline, Saggio, and Sølvsten (2020), henceforth KSS. The KSS estimator uses a leave-out estimate of standard errors to correct estimates of the variance components for sampling variability.

I implement the leave-out estimator at the household level, leaving out all observations corresponding to a household match, not just an individual match. In the mover sample, the KSS estimator is robust to unrestricted heteroskedasticity and serial correlation within each match.

14. The household share term, which captures segregation of household types that is uncorrelated with unobserved place-based heterogeneity, is given by $\frac{E_d[\alpha_i + X_{it}\beta|\tau] - E_o[\alpha_i + X_{it}\beta|\tau]}{\bar{y}_d - \bar{y}_o}$.

Because it is not possible to leave out matches for stayers without dropping all their observations, if there is serial correlation in the error term, KSS estimates of the person variance component in the panel sample are an upper bound on the true value. To reduce the computational burden of the estimator, I use the Johnson-Lindenstrauss approximation (JLA) algorithm introduced by KSS to estimate the statistical leverages of each match, i.e. the amount by which estimates change when leaving out the match. KSS show that using JLA introduces an approximation error of roughly $10^{-4}$ relative to estimating statistical leverages directly. See Appendix C for some additional detail on the implementation of the empirical approach, and KSS for a complete discussion of the leave-out estimator and JLA algorithm.

# 5 Results

This section presents the core results of my paper: estimates of the share of spatial heterogeneity attributable to place effects. I begin this section by showing results from the event study specification, which – even if the stronger assumptions on selection are violated – serve as additional useful descriptive evidence. I then present results from the unrestricted two-way fixed effect model. I conclude the section with a discussion on interpreting the two versions of the analysis, as well as several sensitivity analyses.

## 5.1 Event Study

This section presents estimates from the event study derived in Section 4.2

$$\ln CO_{2it} = \tilde{\alpha}_i + \mathbb{1}[moved] \cdot \theta \cdot (\bar{y}_{d_{-i}} - \bar{y}_{o_{-i}}) + \tau_t + X_{it}\beta + \varepsilon_{it} \tag{8}$$

$\bar{y}_{j_{-i}}$ are sample means calculated from the full sample, leaving out the household observation. To the extent that there is sampling variability in the distribution of observational means, my estimate of the relationship between origin-destination mean changes and individual changes in $\log CO_2$ may be biased. In practice, using a linear empirical Bayes estimator to adjust observational means for sampling variability as in, e.g. Abaluck et al. (2021) or Finkelstein, Gentzkow, and Williams (2020) does not materially change the results.

Table 3 presents estimates of the place share, $\hat{\theta}$, from the event study. Column (1) shows estimates with no controls other than year fixed effects. Adding controls (column (2)) does not change the CBSA estimate, but decreases the share of heterogeneity attributable by tract effects by 12 percentage points. This is consistent with evidence that taste-based sorting across neighborhoods plays an important role in neighborhood-level variation in $CO_2$, while moves across CBSAs are more likely to be driven by other factors such as new job opportunities or proximity to friends or family.

One potential concern with the panel estimates presented in column (2) is that the effect of changes to observable characteristics – e.g. having kids – on carbon emissions is estimated from both stayers and movers. However, it may be the case that households who move after having children do so in part because having children changed their preferences more than having children changed the preferences of households who ended up staying where they were. If the decision of whether to move or not is driven (at least in part) by such heterogeneous preference shocks, then any differential effect of the preference shock to movers would be incorrectly attributed to place effects, biasing my estimates. To address this, I re-estimate the event study with movers only (column (4)), which allows movers to differ systematically from stayers. Once again, this does not change the estimates in the CBSA specification, but further decreases the the share of spatial heterogeneity attributable to tract effects by 3 percentage points.

Table 3: **Share of Spatial Variation in Mean $CO_2$ Attributable to Place Effects**

| | Panel | | | Movers | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **CBSA** | | | | | |
| $\bar{y}_d - \bar{y}_o$ | 0.90*** | 0.90*** | 0.90*** | 0.90*** | 0.90*** |
| | (0.007) | (0.007) | (0.013) | (0.008) | (0.017) |
| N | 1,715,000 | 1,715,000 | 664,000 | 179,000 | 44,000 |
| $R^2$ (adj.) | 0.72 | 0.75 | 0.77 | 0.69 | 0.68 |
| **Tract** | | | | | |
| $\bar{y}_d - \bar{y}_o$ | 0.77*** | 0.65*** | 0.61*** | 0.62*** | 0.60*** |
| | (0.003) | (0.003) | (0.006) | (0.004) | (0.008) |
| N | 1,656,000 | 1,656,000 | 640,000 | 483,000 | 127,000 |
| $R^2$ (adj.) | 0.73 | 0.75 | 0.77 | 0.72 | 0.72 |
| Controls | | X | X | X | X |
| No big life events | | | X | | X |

**Note:** This table reports event study estimates of place shares of spatial heterogeneity in household $CO_2$. Columns (1) and (6) report estimates from the panel sample with no controls apart from year fixed effects. Columns (2) and (7) add controls for the standard set of household characteristics. Columns (3) and (8) restrict the estimation sample to movers only, to allow movers to differ systematically from stayers. Columns (4)-(5) and (9)-(10) use the subset of the panel and mover samples that did not have a change to the number of kids in their household or a larger than 50% increase or decrease to income. All estimates are weighted using Census sample weights.

Even after accounting for the way in which movers differ from stayers on average, heterogeneity in preference shocks within movers could still lead to heterogeneity in mover destinations. For example, the decision of one household to move from a city to a suburb after having children could reflect a different shock to preferences than that of a household that moves from one neighborhood within a city to another after having children. While I cannot rule this out entirely, I explore the extent to which such selection patterns might bias my results by re-estimating the event study on a sample restricted to only households who did not experience a large shock to observable characteristics. Namely, I restrict the sample to only households who never had a change in the number of kids living in their home, and never had more than a 50% increase or decrease in household income between observations. The idea behind this is: if heterogeneity in unobserved time-varying preferences leads households to choose different types of neighborhoods, then estimating the event study using different sets of households with *observably* different preference shocks should lead to different results. Reassuringly, estimates from this approach (in columns (3) and (5)) are similar to estimates using the mover-only sample.

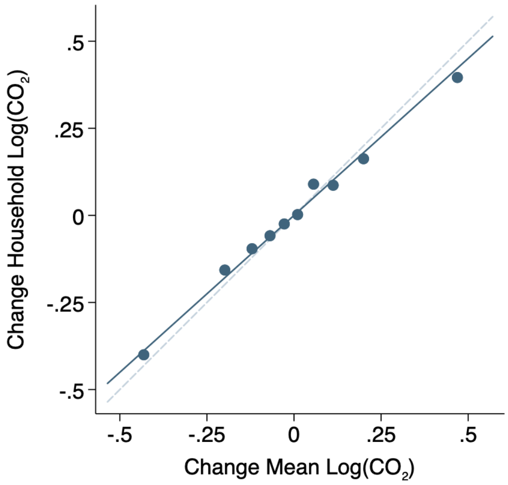
I further explore the potential role of unobserved, time-varying preference heterogeneity by estimating the share of spatial heterogeneity attributable to place effects along two additional dimensions: the type of move, measured as the size and magnitude of changes in mean household carbon emissions across origin and destination locations, and duration between observations. Figure 4 shows changes in mover households' carbon emissions by decile of origin-destination differences in observational means, with all demographic and household controls. The gray line is 45-degrees, and would correspond to place effects accounting for 100% of variation in mean differences across places. The slope of the solid line corresponds to the estimate of $\theta$ from the full mover samples (column (4) of Table 3). I find that both for moves across CBSAs and moves across neighborhoods, estimates of the share of heterogeneity attributable to place effects are symmetric and linear across deciles of origin-destination mean changes. The fact that the place share estimate is stable across deciles of move types is suggestive evidence that my results aren't being driven by only a subset of movers or mover destinations. It also provides additional validation for the log-linear model specification, serving as kind of an extension of the symmetry check presented in Figure 3.

Figure 5 shows tract-level estimates by duration between observations, with all demographic and household controls. This exercise allows me to evaluate two possible sources of bias in the model. First, it provides evidence on the extent to which my place effect estimates may be biased by life-cycle patterns of energy and $CO_2$ demand. If my estimates are unintentionally capturing changes to preferences over different stages of life (ages) of household members, I would expect estimates to be larger for households I observe 15 years apart than those I observe 5 years apart. Second, if households select where to move based on preferences that drift over time in a way that *isn't* captured by age or other life-cycle effects, my estimates of place effects would capture a combination of true causal effects and selection, and the longer the gap between observations, the larger I would expect the selection component to be. This would result in estimates of place effects that are increasing or decreasing with the duration between moves, depending on the direction of selection.

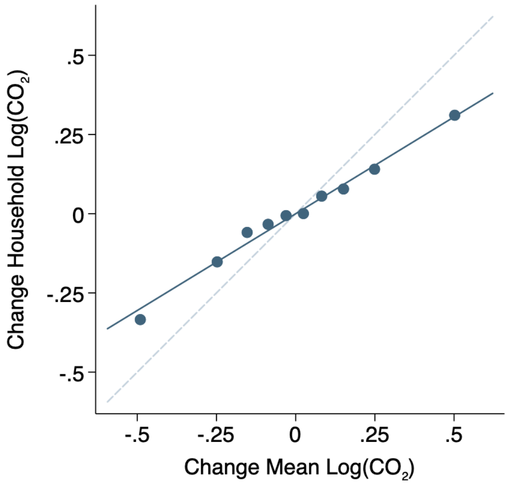
I show estimates for the full panel sample (light gray), and the restricted panel of only households with no significant changes to income or household composition (dark blue). The pooled estimate is contained in the 95% confidence interval of all but two-duration specific estimates, and coefficients appear to be mostly stable – the estimate from households observed one year apart is higher than the pooled estimate, and there is a slight but not statistically significant upward trend for estimates from households observed 16 years apart or longer. Given that these are also the duration bins with the fewest observations, I do not interpret this as strong evidence of place effect estimates being biased by drifting preferences. Analogous CBSA estimates are shown in Figure A.5, and exhibit a similar pattern.

Figure 4: **Place Share of Spatial Variation in Mean $CO_2$ , by Move Type**

(a) CBSA

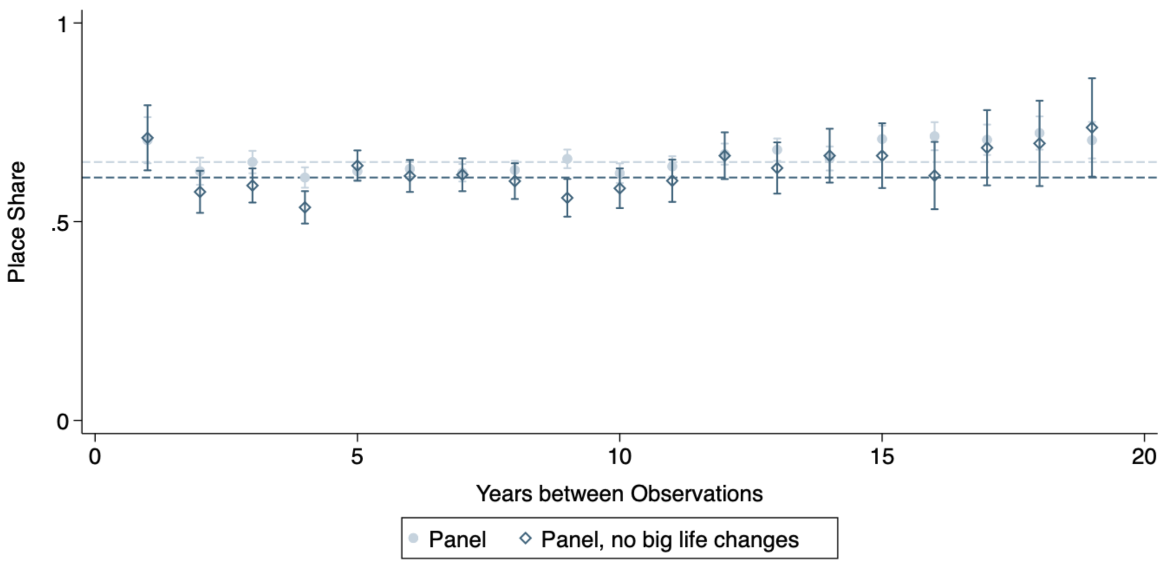



(b) Tract




**Note:** This figure shows event study estimates of the share of spatial variation in mean carbon emissions that can be explained by place effects, by size of origin-destination differences in mean household carbon emissions for movers. Movers are split into ten deciles, according to the size of the gap in mean carbon emissions across their origin and destination. All estimates are from models that control for observable household characteristics and year fixed effects. The solid lines show the regression estimates from the pooled model,and the dotted gray line denotes 45, i.e. the scenario in which moving to on average higher or lower emissions places leads to a 1-for-1 increase in own carbon emissions. All estimates are weighted using Census sample weights.

Figure 5: **Place Share of Spatial Variation in Mean $CO_2$, over Time**




**Note:** This figure shows event study estimates of the share of spatial variation in mean carbon emissions that can be explained by place effects, by duration between mover observations. In other words, each coefficient is the estimate for place effects generated from the sub-sample of households that I observe X years apart. Coefficients plotted in light gray are estimated from the model using the full panel of stayers and movers. Coefficients plotted in the dark blue are estimated from the model using the sub-sample of stayers and movers with no changes in the number of children and less than 50% change in household income between observations. All estimates are weighted using Census sample weights.

One additional result that comes out of the analysis of the duration-specific event study is that household carbon emissions appear to change instantaneously. This suggests that place effects are driven by attributes that directly impact carbon emissions demand, rather than characteristics such as peer effects or habit formation, which I would expect to lead to gradual changes in behavior over time.[15]

### 5.2 Two-Way Fixed Effect Decomposition

Estimates from the KSS bias-corrected variance decomposition are shown in Table 4. The table presents overall variance for the sample, the share of variance attributable to each of the unobserved heterogeneity components, and the KSS-adjusted coefficient of determination ($R^2$). Panel A presents estimates from the entire panel of movers and stayers, while Panel B presents estimates from the mover only sample. Estimates from a variance decomposition with no bias correction can be found in Table A.5.

In the baseline analysis with year fixed effects and the standard vector of household controls, I estimate that CBSA effects account for 16-19% of overall heterogeneity, and tract effects account for 24-26% of overall heterogeneity. To estimate the share of $CO_2$ heterogeneity attributable to place characteristics more likely reflecting local built environment and public amenities, as opposed to exogenous amenities and supply side factors, I re-estimate variance components, partialing out measures of climate and electric grid intensity, and then additionally, prices. Specifically, in columns (2) and (6) I control for heating degree days, cooling degree days, and electricity emissions factors (all in logs), and in columns (3) and (7) I also add controls for lagged fuel shares interacted with national retail prices.

I find that controlling for climate and electric grid intensity decreases the place share of spatial heterogeneity by roughly 10 percentage points, to 10-16% of overall heterogeneity. This decrease is consistent with a well-understood, robust relationship between climate and energy use (e.g. Goldstein, Gounaridis, and Newell 2020) and the mechanical relationship between electricity emissions factors and $CO_2$. However, remaining neighborhood attributes explain a larger share of variation than climate and grid intensity, underscoring the importance of residual place characteristics such as urban form. Accounting for cross-sectional fuel price variation does not further change the results, suggesting, as with climate and grid intensity, that neighborhood attributes other than price drive a meaningful share of the heterogeneity in carbon emissions attributable to places.[16]

Finally, it is possible that place effects evolve over time in ways that differ from national average trends in carbon emissions. For instance, the governments in certain states or cities may be particularly concerned about climate change and enact regulations or make place-based investments aimed at reducing emissions for their residents. In addition to transit and zoning examples I've highlighted throughout the paper, such policies could include regulatory efforts

15. I do not observe how long ago households moved, but the expected value of how long ago someone moved is increasing in the duration between observations.

16. One explanation for this could if price variation in my data is roughly correlated with variation in climate and electric grid factors. In Appendix D, I present an overview of recent estimates of energy price elasticities of demand and discuss additional methods for bounding how much of the variation I find could be driven by prices.

more directly targeting energy sources, such as renewable portfolio standards, state or regional cap and trade programs, or laws banning gas stoves in new homes. More generally, changes to place effects could arise from local or regional planning initiatives motivated by factors completely unrelated to decision-makers' climate objectives. For instance, the Phoenix metropolitan area – one of the fastest growing metropolitan areas in the US – has grown by nearly 1.6 million residents since 2000. This period of growth has been accompanied by a mix of suburban expansion, urban development, the opening of a new light rail system, and several high way expansions.[17]

To account for time variation in place effects that differs from national trends, I also estimate time-varying place effects $\psi_{jt}$ at the CBSA level.[18] I follow Lachowska et al. (2020) and estimate time-varying fixed effects using stayers to identify variation across time within place. To maintain connectivity in my set of places, and because for the most part places evolve slowly, I allow these to vary at 5-year intervals. Thus, there's a different time-varying place effect for each period 2000-2004, 2005-2009, 2010-2014, and 2015-2019. Results are shown in column (9) – allowing CBSA effects to evolve slightly increases their variance share to 21%.

The interpretation of my results is somewhat complicated by the fact that the contribution of unobserved household characteristics to overall heterogeneity is highly sensitive to whether the model is estimated on movers and stayers or movers only. In the panel sample, unobserved household heterogeneity accounts for 51% of overall heterogeneity when measuring place at the CBSA level, and 38% when measuring place at the neighborhood level. This share is stable to partialing out exogenous amenities and prices, but the CBSA estimate decreases to 31% when allowing CBSA effects to change over time. Using the mover-only sample substantially decreases the unobserved household contribution across specifications, to 11% in the CBSA specification and 10% in the mover specification. Similarly, estimates of the covariance between unobserved place and household characteristics are also sensitive to the sample choice. In both panel specifications, the covariance is slightly negative but effectively zero – the larger of the two correlation coefficients is -0.02. In contrast, in the mover sample I find a positive (though still small) correlation coefficient of .07 at the CBSA level and .06 at the tract level.

There are two reasons we might expect estimates from the panel and mover samples to differ. The first is from fundamental differences across stayers and movers, and the second is that KSS cannot correct bias induced by serial correlation in the error term among stayers. To try to shed light on the relative importance of these pieces, it is useful to compare estimates from the KSS decomposition to estimates from the naive, uncorrected (AKM) decomposition in Table A.5. If results are driven by differences between the panel and mover sample, such differences should also be evident in the AKM estimates, even though we expect estimates of both variance components in AKM to be higher than in KSS because of limited mobility biased. In contrast, if results are driven by serial correlation in stayers' error term, then we would expect the relative contributions of the unobserved heterogeneity components in the AKM estimation to be fairly similar, with differences being introduced only in the KSS correction. The AKM estimates suggest that the relative place and person shares are almost identical across the panel

17. See e.g. *The Phoenix Metro Area* (2020).

18. For parsimony in the census disclosure review process, I am not disclosing time-varying tract effects as these involve a different leave-out sample, whereas the CBSA leave-out-connected set is the same in the time-varying case as it was in the baseline.

and mover samples in the CBSA analysis. In the tract analysis, the relative size of the household variance component does drop several percentage points, but not nearly as dramatically as it does in the KSS analysis, suggesting that the estimated household variance components in the panel sample of KSS are an upper bound on the true value, with the upward bias driven by serial correlation in the stayer error term.

Table 4: **Unobserved Heterogeneity in CO2 – Variance Decomposition**

| | CBSA | | | | Tract | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **A: Panel Sample** | | | | | | | |
| $Var(\log CO_{2ij})$ | 0.29 | 0.29 | 0.29 | 0.29 | 0.28 | 0.28 | 0.28 |
| Share $Var(\psi_j)$ | 0.188 | 0.095 | 0.099 | 0.205 | 0.257 | 0.152 | 0.154 |
| Share $Var(\alpha_i)$ | 0.505 | 0.502 | 0.503 | 0.306 | 0.377 | 0.374 | 0.373 |
| Share $2 \cdot Cov(\alpha_i, \psi_j)$ | -0.001 | 0.004 | 0.004 | -0.001 | -0.006 | 0.009 | 0.010 |
| $R^2$ | 0.69 | 0.61 | 0.61 | 0.51 | 0.62 | 0.54 | 0.55 |
| **B: Mover Sample** | | | | | | | |
| $Var(\log CO_{2ij})$ | 0.32 | 0.32 | 0.32 | | 0.31 | 0.31 | 0.31 |
| Share $Var(\psi_j)$ | 0.163 | 0.098 | 0.102 | | 0.239 | 0.156 | 0.163 |
| Share $Var(\alpha_i)$ | 0.112 | 0.091 | 0.0966 | | 0.102 | 0.103 | 0.104 |
| Share $2 \cdot Cov(\alpha_i, \psi_j)$ | 0.010 | 0.013 | 0.013 | | 0.010 | 0.016 | 0.016 |
| $R^2$ | 0.30 | 0.22 | 0.22 | | 0.36 | 0.29 | 0.30 |
| Amenities | | X | X | | | X | X |
| Prices | | | X | | | | X |
| TV-FE | | | | X | | | |

**Note:** This table reports results from the heteroskedasticity-robust KSS estimation of variance components. All specifications include demographic and household controls as well as time fixed effects. Columns (1) and (5) report the baseline variance decompositions at the CBSA and tract levels. Columns (2) and (5) add controls for local mean heating degree days, cooling degree days, and electricity emissions factors (all in logs). Columns (3) and (6) additional control for a price index, constructed from lagged fuel shares interacted with national retail prices. Finally, column (4) computes time-varying CBSA place effects using 5-year windows (2000-2004, 2005-2009, 2010-2014, and 2015-2019), using stayer observations across time windows to identify time variation in place effects, while movers, as before, identify cross-sectional variation.

Serial correlation in the error term could arise as a result of several sources of measurement error in my outcome variable. While an advantage of using the Census for this analysis is that it allows me to observe many household characteristics that are unobservable in standard administrative datasets on energy use, thereby making it possible to control for changes to household characteristics that are correlated with both changes to energy demand and move propensity and destinations and decrease potential bias from unobserved preference shocks, a disadvantage is that the survey nature of the data means that my outcomes are constructed from a combination of survey responses and local external data. In particular, I use local average prices and local average emissions factors to convert reported energy expenditures and commute times into carbon emissions. Both of these could introduce serial correlation into my estimates of stayer outcomes. Additional detailed discussion of measurement error within the residential and transportation sectors, as well as implications for interpreting results, can be found in Appendix B.1 and Appendix B.2, respectively.

How do these results inform the interpretation of the event study decomposition? Here, there are also two things to note. First, recall that if there is either assortative or disassortative matching of households to places, event study estimates are biased because they assume zero covariance. My estimates from the KSS decomposition suggest that the covariance terms are very close to zero – the largest correlation coefficient across the four baseline estimates (CBSA vs. tract & panel vs. mover) is 0.06, so the bias from this assumption on selection should be minimal.

Even with no bias from selection, the event study yields estimates of shares of mean differences between places attributable to place effects, while the KSS estimates yield a variance decomposition of *overall* variation, and this can lead to meaningful discrepancies in magnitudes. To see this, imagine two places, one $\psi_{low}$ and one with $\psi_{high}$, and identical populations across the two places. If there is high variation in carbon emissions across populations and a small difference between $\psi_{low}$ and $\psi_{high}$, the event study would yield a share coefficient of 1 (since populations are identical across places, all between differences are driven by place effects), but the KSS decomposition would yield a place variance component of close to zero (because of a very large within component to the variance). In practice, this is very close to what happens at the CBSA level – the vast majority (90%) of differences between CBSAs can be attributable to variation in place effects and not household attributes, but there is much more variation in household carbon emissions within CBSAs than there is across, leading to a variance component of 16-19%, about half of which is attributable to climate and electric grid intensity, in the KSS estimation. At the neighborhood level, household sorting contributes more to variation between places, dropping event study estimates of the place share to 62%; accounting for variation within places (using the panel sample with KSS) further decreases the place variance share to 26% of overall heterogeneity (or 42% of heterogeneity explained by the model, calculated from re-scaling by the $R^2$).

### 5.3 Specification Tests and Robustness

As an additional specification test, in Appendix Figure A.6, I show binned scatter plots similar to the one presented for event study results (Figure 4), but now with deciles of changes in estimated place effects, rather than observational means, on the x axis. I plot these against two sets of changes in household mean outcomes: changes for the full mover sample, and changes in the sample restricted to only houseolds with no big life changes. In a correctly specified model, changes in place effects should lead 1-to-1 to changes in household carbon emissions, though attenuation bias from noisily estimated place effects should decrease the slope. Crucially, I find no difference across the two samples, suggesting (as in the event study analysis) that selection on heterogeneous preference shocks isn't a first order threat to identification in my analysis.

To evaluate the sensitivity of my results to my outcome definitions, Appendix Table A.4 shows estimates from a KSS decomposition using residential energy only, and using total energy but imputing carbon emissions from transportation energy using the National Highway Transportation Survey (NHTS). One version of the NHTS imputation uses a LASSO regression to predict heterogeneous fuel economy from household, geographic, and commute characteristics that are common to both the Census and NHTS surveys, and then uses predicted relationships to estimate carbon emissions from commuting accounting for variation in fuel economy. The second version additionally predicts total miles travelled, and uses both heterogeneous fuel economy and heterogeneous relationships between commuting and total miles travelled to estimate carbon emissions from car travel generally. I find that restricting the analysis to residential energy increases the overall variance by 6 points ($\sim 25\%$), and increases the share of heterogeneity attributable to place effects by 6 percentage points at the CBSA level, and by one percentage point at the tract level. Estimated place shares do not appear to be highly sensitive to re-defining the transportation outcome variable. Additional discussion of these results can be found accompanying Table A.4.

## 6 The Characteristics of Low and High Emissions Places

With estimates of place effects in hand, I move on to characterizing the local amenities that are associated with high and low emissions places. As highlighted in the conceptual model, place effects reflect a mix of differences in demand for energy, energy prices, energy demand elasticities, fuel mixes, and emissions factors.

The urban and planning literature has identified many place-based characteristics that could contribute to differences in energy demand and energy demand elasticities. For example, on the residential energy side house size is positively correlated with energy use, there's a strong relationship with density but potentially not a monotonic one because of the effect of density on micro-climate (heat island effects), and planting and surface coverage are negatively correlated with energy use (See Ko 2013, for a review). In transportation, car use is lower in places with more alternative transportation options, fewer parking minimums, and more directly connected roads (e.g. Transportation Research Board 2009; Barrington-Leigh and Millard-Ball 2017).

In Figure 6, I show the result from projecting place and person effects estimated in the KSS mover sample onto a set of some of energy-relevant amenities, as well as onto a set of demographic characteristics. I categorize place amenities into three groups. Amenities in the urban form category are ones that households effectively take as given. I include in these indicators for whether a tract is classified as urban, suburban or rural by the Census, geodesic distances between tract centroids and the centroid of the closest city and the largest city, walk scores, bike scores, transit scores, and the average number of bus routes and rail routes within half a mile of the census block centroids contained within the tract. Amenities in the capital stock category are ones that reflect a combination of the options available in a neighborhood and household preferences – these include the share of homeowners, the share of detached single family homes, the average number of rooms per house, and average number of cars per household. Climate is (for the purposes of this paper) exogenous, and captured using annual heating degree days and cooling degree days in NOAA subdivisions. Bars show coefficient estimates from a multivariate regression of fixed effects onto all three groups of amenities, as well as electric grid intensity and density which I don't report in the figure because I estimate effects larger (in absolute value) than one, so their inclusion makes the other values more difficult to see.[19] For the tract effect estimation, capital stock variables reflect tract-level means; for the household effect they reflect the household's own choice. Finally, all amenities except for suburban and rural indicators (and homeowner and detached home indicators for the household regression only) are measured in logs, so correlates should be interpreted as the percent increase or decrease in place effects associated with a one percent increase or decrease in the amenity.

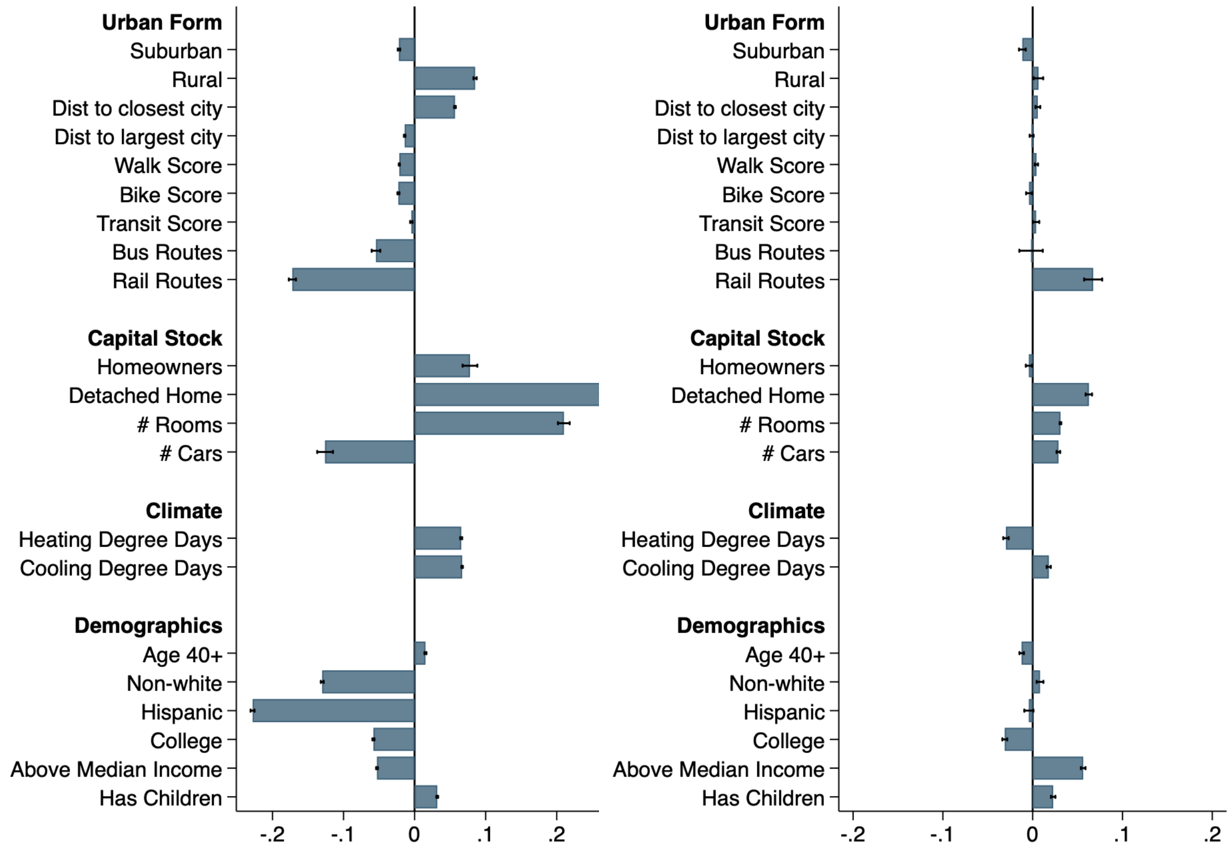
The results show that tracts with a large share of detached single family homes, bigger homes, and a larger share of homeowners tend to have significantly higher carbon emissions place effects. Unintuitively, tracts with more cars per household have lower place effects. Tracts with more bus and rail routes within a half mile, and those with better walk and bike scores have lower place effects, as do tracts that are closer to the closest city within the CBSA. Tracts closest to the largest city are higher emissions, and conditional on all these regressors, suburbs are no longer higher emissions than urban areas. One possible explanation for these last two correlations is longer commutes due to congestion. The regression of person effects onto place amenities shows much weaker (or zero) correlations for many of the amenities, consistent with minimal sorting on unobserved characteristics that I estimate in KSS, but intuitively high type households are more likely to live in larger, detached single family homes, and have more cars. They also live in places near more rail routes, potentially reflecting some combination of suburban commuter rail networks, and the high cost of living in central cities.

In the regression of unobserved characteristics on demographics, I find that non-white and Hispanic households are more likely to live in low carbon emissions places, as are college educated and above median households. Households with children live in higher carbon emissions places, as do older households (though the effect here is small). On the household side, college educated households tend to have lower unobserved preferences for carbon emissions, as do younger households (but again with a small effect), and above median income households and households with kids have higher unobserved preferences. Additional results on the correlates between observable person and observable place characteristics are presented in Appendix Table A.6.

19. I estimate a coefficient of 4 on log electric grid intensity. This suggests that places that have clean energy grids are also making other investments or decisions that make them lower carbon emissions. I estimate a coefficient of close to -10 on density, suggesting a very strong association between density and place effects.

Figure 6: **Correlates of Unobserved Heterogeneity**

(a) Tract Effects (b) Person Effects




**Note:** This figure presents estimates from the multivariate ordinary least squares regressions of place effects onto a set of local amenities (urban form, capital stock, climate, electric grid intensity, and density), place effects onto a set of local average demographics, person effects onto a set of local amenities, and local effects onto a set of local average demographics. All amenity variables are measured in logs (+1 for variables where zero is possible), except rural and suburban indicators, and – for the person effect regression only – the homeowner and detached single family home indicators.

# 7 Implications for Aggregate Carbon Emissions

I use estimates of place effects to consider some back of the envelope calculations of how aggregate carbon emissions would change under counterfactual scenarios where the distribution of place effects differs from its current realization. Because place effects are noisily estimated, I use linear Empirical Bayes, i.e. a shrinkage estimator, to forecast place effects that reflect the best (minimum mean squared error) linear prediction of the true values, given my estimates from the KSS analysis. Many papers in the public and labor literatures have used this approach to predict e.g. teacher value add or neighborhood effects in other contexts (Chetty, Friedman, and Rockoff 2014a; 2014b; Angrist et al. 2017; Chetty and Hendren 2018; Finkelstein, Gentzkow, and Williams 2020; Abaluck et al. 2021). Although the linear approximation only corresponds to the true Empirical Bayes posterior when errors are normal and homoskedastic, Kline, Rose, and Walters (2021) show that even when errors are heteroskedastic, the linear shrinkage estimator doesn't do much worse than non-parametric Empirical Bayes. The shrinkage estimates are given by:

$$\hat{\psi}_j^{EB} = \lambda_j \hat{\psi}_j + (1 - \lambda_j) \frac{1}{J} \sum_j \hat{\psi}_j \tag{9}$$

where the weights $\lambda_j = \frac{\hat{\sigma}_j^2}{s_j^2 + \hat{\sigma}_j^2}$ capture the signal-to-noise ratio of each estimate and down-weight noisy estimates to the grand mean.

I use this approach to estimate counterfactual carbon emissions under three different scenarios: What if the top 10 most populous CBSAs in the US all had the place effects of the New York City CBSA? What if the principal cities of the top 10 most populous CBSas all had the place effects of Manhattan? And what if cities and towns all had the place effects of the principal cities in their (nearest) CBSA? The goal of this exercise is to get a sense for how carbon emissions would evolve under interventions to the built environment of places, without attributing a causal effect to any single amenity, since my correlational analysis doesn't parse those causal effects out.

A naive comparison of household carbon emissions in the New York metropolitan area and the nine other largest metropolitan areas in the US (see Table A.8 for the full list) suggests that household emissions from residential energy and commuting are about 8% lower in the New York metropolitan (17.9 tons, annually, per household as compared to 19.2). However, I find that assigning the average New York Metropolitan area place effect to each of these other metropolitan areas actually increases average household emissions slightly, to 19.3 tons per household, highlighting the high place effects of the suburbs around NY. In contrast, if each principal city of the other top nine metropolitan areas had the place effect of Manhattan, household emissions from residential energy and commuting for current residents of those cities would decrease by over 50%, from 15.6 tons per household to 7.0. This is not as large as the naive decrease to 3.9 tons per household – some of Manhattan's low emissions can be explained by household sorting – but still the Manhattan place effect is significantly lower than the effect of the other 9 largest cities in the US, on average.

Manhattan is unique in its density and transit infrastructure within the US, so the last scenario I consider is intended to capture more closely the spirit of what might happen under some of the regional zoning and transit-oriented development proposals that are emerging across the US.[20] If each place had the place effect of the principal city in its CBSA, annual household carbon emissions would go down from residential energy use and commuting would go down by on average 13%, from 20.6 to 17.9 tons. Again, a naive comparison (20.6 vs. 15.04) overstates the difference between central city and surrounding areas, but my estimates suggest that changing places could yield meaningful reductions in household carbon emissions. For comparison, the Waxman-Markey bill, which failed to pass in 2009 but was, until 2021, the largest federal legislative effort

20. For example, in 2018, Minneapolis was the first city in the US to ban exclusionary zoning (which restricts land to be used for single-family homes only) city-wide. In 2021, California passed State Assembly Bills 9 and 10, which reduce administrative hurdles to "up-zoning" residential land zoned for single family homes only to allow up to four units, as well as land near transit corridors. There have also been attempts to create incentives for up-zoning at the federal level. For example, President Biden's original infrastructure bill proposal in March 2021 included grants to cities who got rid of exclusionary zoning.

to decrease carbon emissions in the US, was projected to decrease economy-wide emissions 17% in 2020 relative to 2005 (Center for Climate and Energy Solutions 2009).

This exercise lends insight into how development that shifts population shares across place types by "expanding" places with lower place effects (either by making their neighbors look more like them, or by allowing more people to live in the place without changing its fundamentals), could affect emissions in the future. My estimates yield only a first-order, partial equilibrium approximation to the effect of such interventions, as in practice there would be some re-sorting of populations, changing the distribution of household types living in each place and therefore changing aggregate carbon emissions.

## 8 Discussion

Overall, my results suggest that roughly 15-25% of heterogeneity in household carbon emissions from residential and transportation energy use across the US can be explained by place effects, or about 10-20% can be explained by place effects after partialing out variation driven by climate and electric grid intensity. While this leaves the majority of variation either to unobserved household characteristics or unexplained factors in my model, I find that over half of mean differences between places can be explained by place effects, and my estimates suggest the potential for meaningful reductions in carbon emissions from "place-based" interventions that make the distribution of place effects across the US more urban.

Whether such place-based interventions would be welfare maximizing would depend on the costs of implementing them relative to the cost of business as usual or other climate mitigating policies (e.g. a carbon tax).[21] Infrastructure in the US is notoriously expensive to build, making it unlikely that big expansions of new rail (e.g. building a NYC style subway system in Houston) would pass the cost-benefit test in current circumstances. However, correlates of place effects include many amenities that could re-purpose existing built environment without expensive new additional investments – bus lines, bike lanes, pedestrian infrastructure, and dense housing (which, through compactness could decrease related infrastructure and service costs) are all more likely potential contenders. Incorporating cost estimates for a marginal value of public funds analysis (Hendren and Sprung-Keyser 2020) is an important avenue for future research.

The welfare benefits of such interventions would also of course depend on the causal relationships between local amenities and place effects, and household preferences for local amenities. The correlations I presented between amenities and place effects don't identify causal relationships, but they highlight a strong association between many local public goods and carbon emissions, suggesting an important potential role played by local public goods. While Tiebout (1956) posits that residential sorting allows for efficient provision of local public goods, his framework only applies to amenities without scale economies. Moreover, there is reason to believe that residential sorting is not efficient due to frictions or exclusionary policies (e.g. Rothstein 2017;

21. The welfare impacts would also depend on other externalities or agglomeration benefits of such interventions, which have been studied extensively in the environmental and urban economics literatures. For example, the types of interventions considered in my paper could also impact local air pollution, congestion, traffic fatalities, and labor market productivity.

Hausman and Stolper 2020; Christensen and Timmins 2021; Avenancio-León and Howard 2020). Estimating causal relationships between local public amenities and household carbon emissions and quantifying whether emissions-relevant local public amenities are at an efficient level are additional important directions for future work.

Finally, there are several limitations of my empirical analysis that should be taken into consideration while interpreting my results. The first is that due to the survey nature of my data, carbon emissions are noisily measured. This leads to lower explanatory power of the model than is standard in papers in the labor literature using these methods to estimate firm wage premia. The relatively low explanatory power of the model could also reflect model mis-specification, but with only two observations per household for the majority of estimates, the number of specification tests I can do is limited. Second, there is relatively little variation in urban form across the US – 95% of commuters in my sample commute by car, and 75% of residential land in the US is zoned for single family homes only (Badger and Bui 2019). Moreover, place effects are identified from movers, who differ from the general US population in meaningful ways. The external validity of my results is contingent on estimates being stable to widening the distributions of place and person types that they are estimated on.

## References


Abaluck, Jason, Mauricio Caceres Bravo, Peter Hull, and Amanda Starc. 2021. "Mortality Effects and Choice Across Private Health Insurance Plans." *Quarterly Journal of Economics.*

Abowd, John M., Francis Kramarz, and David N. Margolis. 1999. "High Wage Workers and High Wage Firms." *Econometrica* 67 (2): 251–333.

Administration, U.S. Energy Information. 2020. *U.S. energy-related carbon dioxide emissions fell in 2019, mainly in electric generation.* Technical report.

Allen, Treb, and Costas Arkolakis. 2021. *The Welfare Effects of Transportation Infrastructure Improvements.* Working Paper.

Andrews, M. J., L. Gill, T. Schank, and R. Upward. 2008. "High wage workers and low wage firms: negative assortative matching or limited mobility bias?" *Journal of the Royal Statistical Society: Series A (Statistics in Society)* 171 (3): 673–697.

Angrist, Joshua D., Peter D. Hull, Parag A. Pathak, and Christopher R. Walters. 2017. "Leveraging Lotteries for School Value-Added: Testing and Estimation." *The Quarterly Journal of Economics* 132, no. 2 (February): 871–919.

Auffhammer, Maximilian, and Edward Rubin. 2018. *Natural Gas Price Elasticities and Optimal Cost Recovery Under Consumer Heterogeneity: Evidence from 300 million natural gas bills.* Working Paper, Working Paper Series 24295. National Bureau of Economic Research.

Avenancio-León, Carlos, and Troup Howard. 2020. *The Assesment Gap: Racial Inequalities in Property Taxation.* Working Paper.

Badger, Emily, and Quoctrung Bui. 2019. "Cities Start to Question an American Ideal: A House With a Yard on Every Lot." *New York Times.*

Barrington-Leigh, Chris, and Adam Millard-Ball. 2017. "More Connected Urban Roads Reduce US GHG Emissions." *Environmental Research Letters* 12 (4).

Baum-Snow, Nathaniel. 2007. "Did Highways Cause Suburbanization?" *Quarterly Journal of Economics* 122 (2): 775–805.

Bond, Brittany, J. David Brown, Adela Luque, and Amy O'Hara. 2014. *The Nature of the Bias When Studying Only Linkable Person Records: Evidence from the American Community Survey.* CARRA Working Papers. Center for Economic Studies, U.S. Census Bureau.

Borenstein, Severin, and Jim Bushnell. 2019. "Do Two Electricity Pricing Wrongs Make a Right? Cost Recovery, Externalities, and Efficiency." *Energy Institute Working Paper.*

Card, David, Ana Rute Cardoso, and Patrick Kline. 2016. "Bargaining, Sorting, and the Gender Wage Gap: Quantifying the Impact of Firms on the Relative Pay of Women." *Quarterly Journal of Economics* 131 (2): 633–686.

Card, David, Joerg Heining, and Patrick Kline. 2013. "Workplace Heterogeneity and the Rise of West German Wage Inequality." *Quarterly Journal of Economics* 128:967–1015.

Card, David, Jesse Rothstein, and Moses Yi. 2021. *Location, Location, Location.* Working Paper.

Center for Climate and Energy Solutions. 2009. "American Clean Energy and Security Act of 2009."

Chetty, Raj, John N. Friedman, and Jonah E. Rockoff. 2014a. "Measuring the Impacts of Teachers I: Evaluating Bias in Teacher Value-Added Estimates." *American Economic Review* 104 (9): 2593–2632.

———. 2014b. "Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood." *American Economic Review* 104 (9): 2633–79.

Chetty, Raj, and Nathaniel Hendren. 2018. "The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects." *The Quarterly Journal of Economics* 133 (3): 1107–1162.

Christensen, Peter, and Christopher Timmins. 2021. "Sorting or Steering: The Effects of Housing Discrimination on Neighborhood Choice." *NBER,* no. 24826.

Colas, Mark, and John Morehouse. 2021. *The Environmental Cost of Land Use Restrictions.* Working Paper.

Cronin, Julie Ann, Don Fullerton, and Steven E. Sexton. 2019. "Vertical and Horizontal Redistribution from a Carbon Tax and Rebate." *Journal of the Association of Environmental and Resource Economists* 6 (S1).

Diamond, Rebecca. 2016. *American Economic Review* 106 (3): 479–524.

Duranton, Gilles, and Diego Puga. 2020. "The Economics of Urban Density." *Journal of Economic Perspectives* 34 (3): 3–26.

Duranton, Gilles, and Matthew A. Turner. 2018. "Urban form and driving: Evidence from US cities." *Journal of Urban Economics* 108 (C): 170–191.

Energy Information Administration. 2020a. *Form EIA-861: Annual Electric Power Industry Report.*

———. 2020b. *State Energy Data System.*

Environmental Protection Agency. 2018. *Emission Factors for Greenhouse Gas Inventories.* Technical report.

Federal Highway Administration. 2019. *National Household Travel Survey.*

Finkelstein, Amy, Matthew Gentzkow, and Heidi Williams. 2020. "Place-Based Drivers of Mortality: Evidence from Migration." *American Economic Review* forthcoming.

———. 2016. "Sources of Geographic Variation in Health Care: Evidence from Patient Migration." *Quarterly Journal of Economics* 131 (4): 1681–1726.

Gaubert, Cecile, Patrick Kline, and Danny Yagan. 2019. "Place-Based Redistribution."

Gillingham, Kenneth. 2014. "Identifying the Elasticity of Driving: Evidence from a Gasoline Price Shock in California." *Regional Science and Urban Economics* 47 (4): 13–24.

Glaeser, Edward L., and Matthew E. Kahn. 2010. "The Greenness of Cities: Carbon Dioxide Emissions and Urban Development." *Journal of Urban Economics* 67 (3): 404–418.

Goldstein, Benjamin, Dimitrios Gounaridis, and Joshua P. Newell. 2020. "The Carbon Footprint of Household Energy Use in the United State." *Proceedings of the National Academy of Sciences* 117 (32): 19122–19130.

Green, Tomas W., and Christopher R. Knittel. 2020. "Distributed Effects of Climate Policy: A Machine Learning Approach." *The Roosevelt Project Special Working Paper Series.*

Hausman, Catherine, and Samuel Stolper. 2020. *Inequality, Information Failures, and Air Pollution.* NBER working paper 26682.

Hendren, Nathaniel, and Ben Sprung-Keyser. 2020. "A Unified Welfare Analysis of Government Policies." *Quarterly Journal of Economics* 135 (3): 1209–1318.

Homeland Security, U.S. Department of. 2021. *Homeland Infrastructure Foundation-Level Data.*

Hull, Peter. 2018. "Estimating Treatment Effects in Mover Designs."

Jones, Christopher, and Daniel M. Kammen. 2014. "Spatial Distribution of U.S. Household Carbon Footprints Reveals Suburbanization Undermines Greenhouse Gas Benefits of Urban Population Density." *Environmental Science and Technology* 48:895–902.

Kline, Patrick, and Enrico Moretti. 2014. "People, Places and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs." *Annual Review of Economics* 6:629–662.

Kline, Patrick, Evan Rose, and Chris Walters. 2021. "Systemic Discrimination Among Large U.S. Employers." *Quarterly Journal of Economics.*

Kline, Patrick, Raffaele Saggio, and Mikkel Sølvsten. 2020. "Leave Out Estimation of Variance Components." *Econometrica* 88:1859–1898.

Ko, Yekang. 2013. "Urban Form and Residential Energy Use: A Review of Design Principles and Research Findings." *Journal of Planning Literature* 28 (4).

Lachowska, Marta, Alexandre Mas, Raffaele D. Saggio, and Stephen A. Woodbury. 2020. *Do Firm Effects Drift? Evidence from Washington Administrative Data.* NBER Working Paper 26653.

Layne, Mary, Deborah Wagner, and Cynthia Rothhaas. 2014. *Estimating Record Linkage False Match Rate for the Person Identification Validation System.* CARRA Working Papers. Center for Economic Studies, U.S. Census Bureau.

NASA. 2020.

National Oceanic and Atmospheric Administration. 2020. *US Climate Division.*

Nowak, William, and Ian Savage. 2013. "The Cross Elasticity between Gasoline Prices and Transit Use: Evidence from Chicago." *Transport Policy* 55 (10): 885–910.

Pomponi, Franceso, Ruth Saint, Jay H. Arehart, Niaz Gharavi, and Bernardino D'Amico. 2021. "Decoupling Density from Tallness in Analysing the Life Cycle Greenhouse Gas Emissions of Cities." *npj Urban Sustainability* 1 (33).

Ribeiro, Haroldo V., Diego Rybski, and Jurgen P. Kropp. 2019. "Effects of changing population or density on urban carbon dioxide emissions." *Nature Communications* 10 (3204).

Rothstein, Richard. 2017. *The Color of Law.*

Sallee, James M. 2019. *Pigou Creates Losers: On the Implausibility of Achieving Pareto Improvements from Efficiency-Enhancing Policies.* Working Paper, Working Paper Series 25831. National Bureau of Economic Research.

Schönholzer, David. 2021. *Measuring Preferences for Local Public Goods.* Working Paper.

Shammin, Md, Robert Herendeen, Michelle Hanson, and Eric Wilson. 2010. "A multivariate analysis of the energy intensity of sprawl versus compact living in the U.S. for 2003." *Ecological Economics* 69 (October): 2363–2373.

Song, Jae, David J. Price, Faith Guvenen, Nicholas Bloom, and Till Von Wachter. 2019. "Firming up Inequality." *The Quarterly Journal of Economics* 134 (1).

Spiller, Elisheba, Heather Stephens, Christopher Timmins, and Allison Smith. 2014. "The Effect of Gasoline Taxes and Public Transit Investments on Driving Patterns." *Environmental and Resource Economics* 59 (4): 633–657.

*The Phoenix Metro Area.* 2020. Technical report. Maricopa Association of Governments.

Tiebout, Charles M. 1956. "A Pure Theory of Local Expenditures." *Journal o Political Economy* 65 (5): 416–424.

Timmons, David, Nikolaos Zirogiannis, and Manuel Lutz. 2016. "Location matters: Population density and carbon emissions from residential building energy use in the United States." *Energy Research  Social Science* 22 (December): 137–146.

Transportation Research Board. 2009. *Driving and the Built Environment: The Effects of Compat Development on Motorized Travel, Energy Use, and $CO_2$ Emissions.* Technical report. National Research Council of the National Academies.

Tsivanidis, Nick. 2019. *Evaluating the Impact of Urban Transit Infrastructure:Evidence from Bogota's TransMilenio.* Working Paper.

Ummel, Kevin. 2014. *Who Pollutes? A Household-Level Database of America's Greenhouse Gas Footprint.* Working Paper 381. Center for Global Development.

U.S. Department of Commerce, Bureau of the Census. 2014. *American Community Survey Design and Methodology.* Technical report.

US Department of Transportation, Federal Highway Administration. 2015. *2015 Status of the Nation's Highways, Bridges, and Transit: Conditions  Performance.* Technical report.

U.S. Environmental Protection Agency. 2021. *Emissions & Generation Resource Integrated Database.*

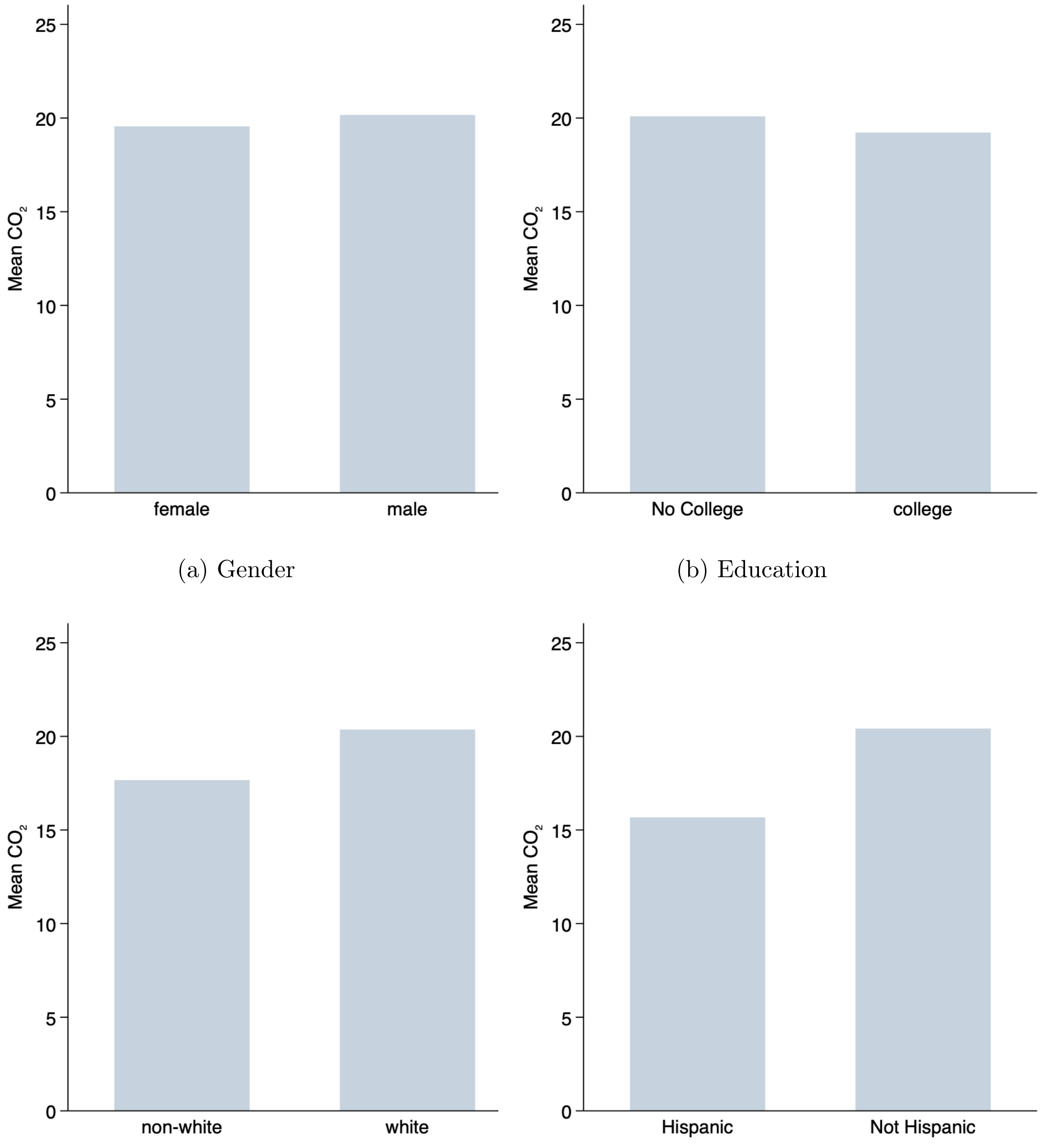
U.S. Environmental Protection Agency, Office of Energy Efficiency, and Renewable Energy. 2020.

Wagner, Deborah, and Mary Layne. 2014. *The Person Identification Validation System (PVS):Applying the Center for Administrative Records.* CARRA Working Papers. Center for Economic Studies, U.S. Census Bureau.

# A Additional Figures and Tables

## A.1 Additional Figures

Figure A.1: $CO_2$ Profiles by Demographic Characteristics (1/3)

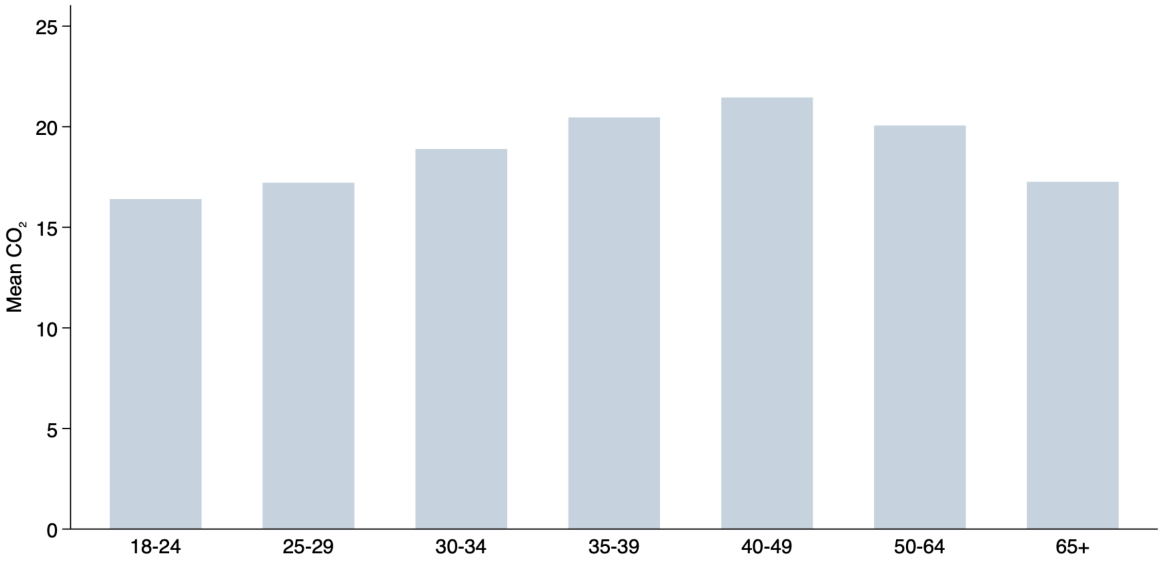



(a) Gender (b) Education

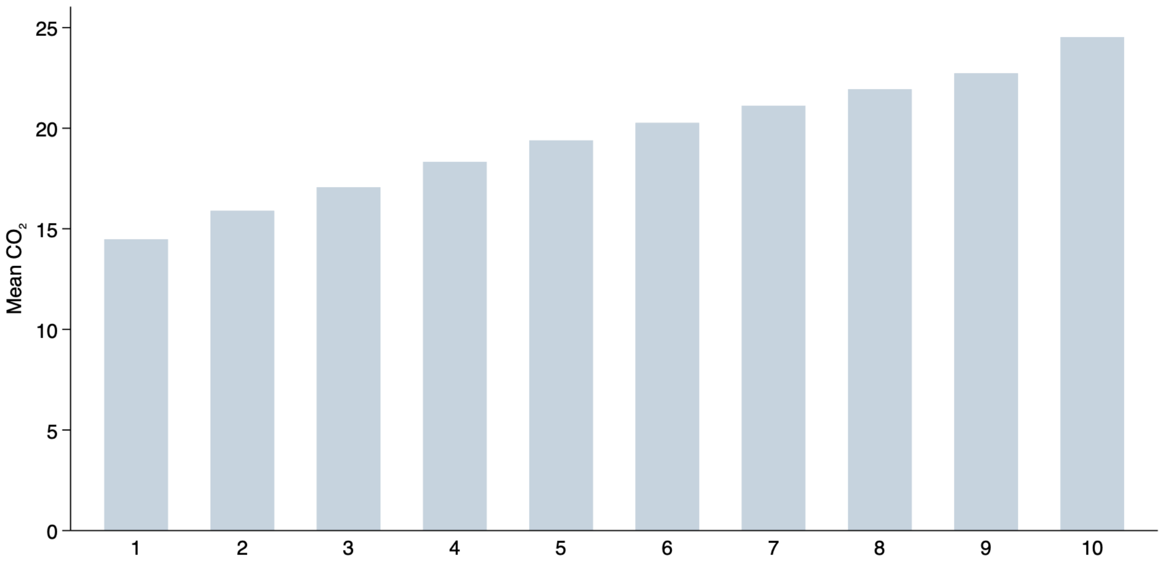
(c) Race (d) Ethnicity

**Note:** This figure shows variation in household carbon emissions by household member demographics. Panel (a) shows that households with more women (age 18+) have slightly lower emissions (consistent with women having fewer and shorter commutes). Panel (b) shows that college educated households have slightly lower emissions. Panel (c) and (d) show large differences by race and ethnicity – white households and non-Hispanic households have higher emissions on average than non-white and Hispanic households. All estimates reflect the full sample, pooled 2000-2019, weighted by Census sample weights.

Figure A.2: $CO_2$ Profiles by Demographic Characteristics (2/3)



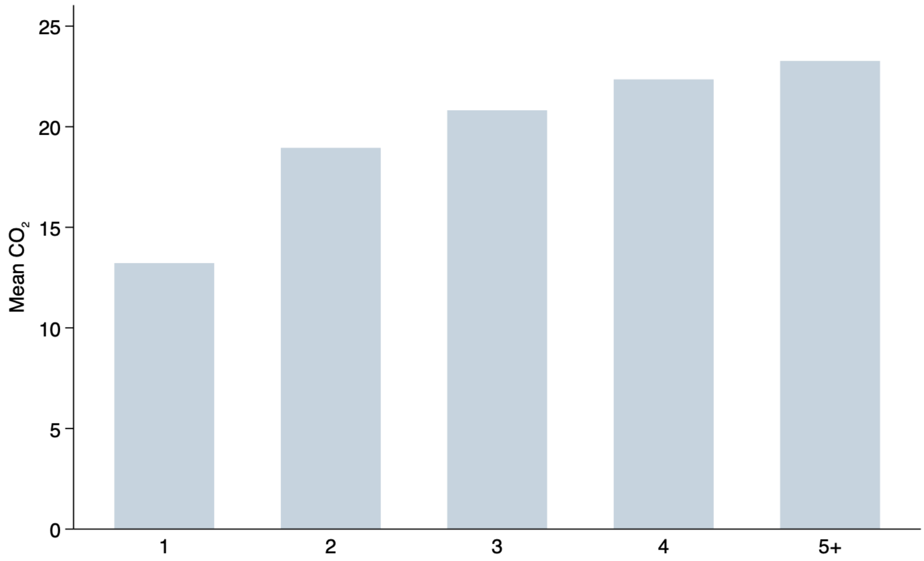

(a) Age



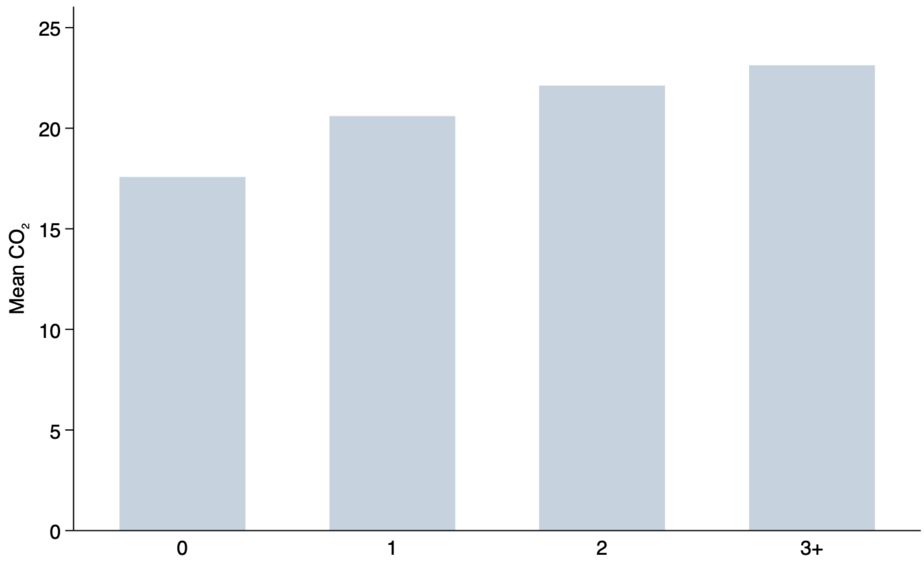

(b) Income Decile

**Note:** This figure shows variation in household carbon emissions by household member age and household income deciles. Panel (a) shows a non-linear relationship between the adult age of household members and mean carbon emissions which increases through people's 40s and then decreases again (likely reflecting a combination of higher incomes and children still being in the home). Panel (b) shows an increasing relationship between household income decile and carbon emissions. All estimates reflect the full sample, pooled 200-2019, weighted by Census sample weights. Household income is CPI-adjusted.

Figure A.3: $CO_2$ Profiles by Demographic Characteristics (3/3)




(a) Household Size

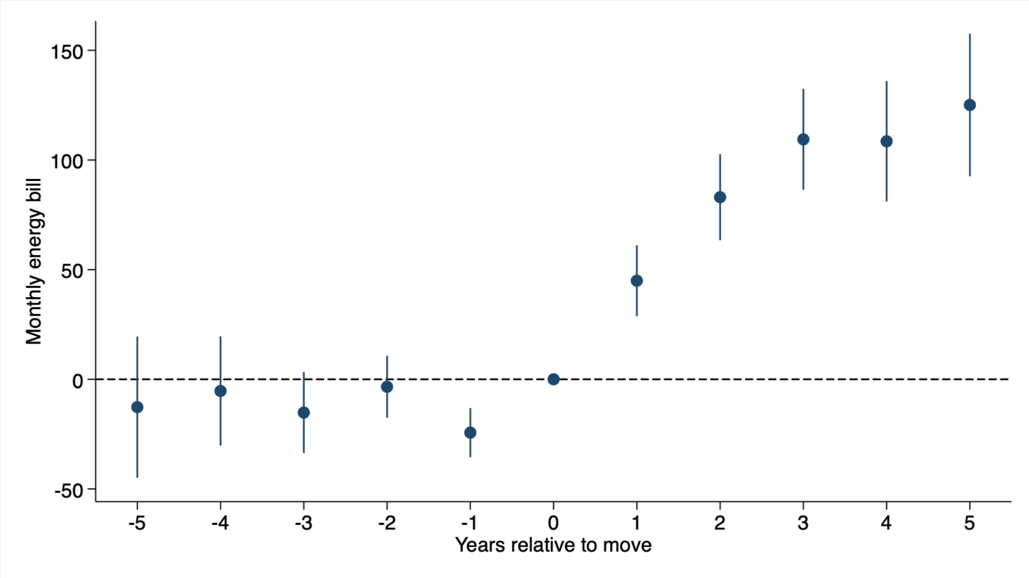



(b) Number of Kids

**Note:** This figure shows variation in household carbon emissions by household size (a) and number of children (b). Carbon emissions increase with household size and with the number of children, but less than proportionally, and the increase is fairly small going from 4 to 5+ people, or 2 to 3+ kids. All estimates reflect the full sample, pooled 200-2019, weighted by Census sample weights.

*Go back to Section 2.6.*

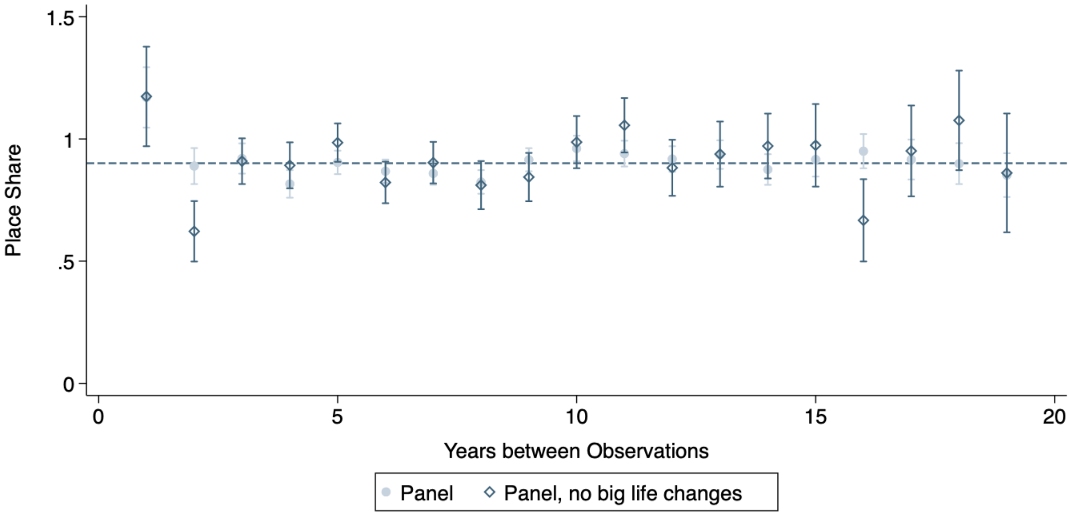
Figure A.4: Energy Expenditures in Mover Households in the PSID




**Note:** I examine whether there are pre-trends in energy consumption for movers using data from the PSID, given data limitations in my baseline data. In particular, I test whether there are significant changes to monthly energy bills in the years prior to a move, after controlling for household characteristics such as income and household size. If anything, I find a slightly countervailing pre-trend for movers, with energy bills decreasing in the year before a move, and then increasing in the several years after (consistent with a secular trend of households moving to higher emissions places).

*Go back to Section 4.1*

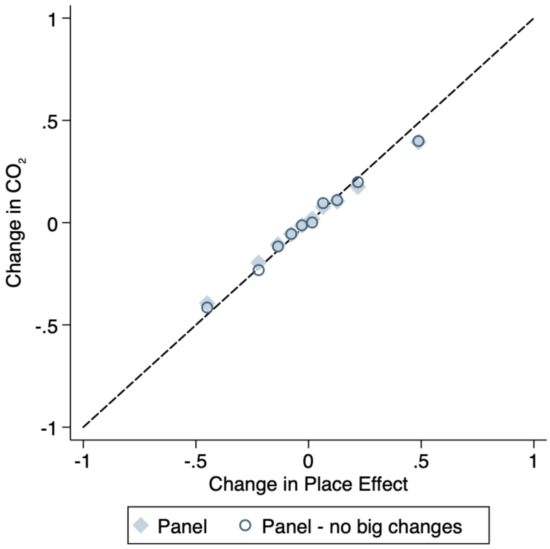
Figure A.5: **Event study by duration – CBSA**



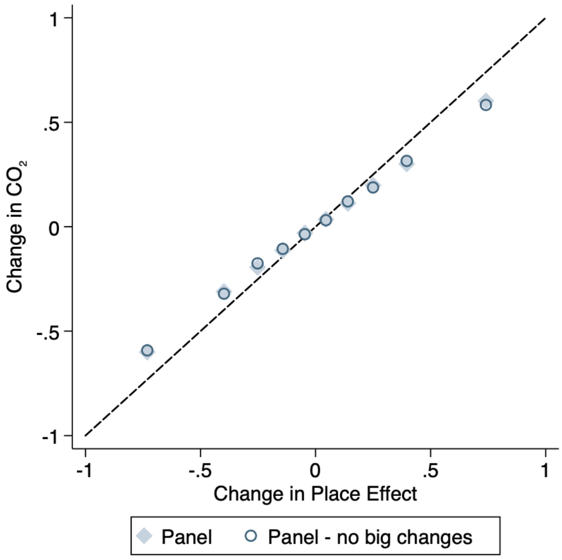

**Note:** This figure shows event study estimates of the share of spatial variation in mean carbon emissions that can be explained by place effects, by duration between mover observations. In other words, each coefficient is the estimate for place effects generated from the sub-sample of households that I observe X years apart. Coefficients plotted in light gray are estimated from the model using the full panel of stayers and movers. Coefficients plotted in the dark blue are estimated from the model using the sub-sample of stayers and movers with no changes in the number of children and less than 50% change in household income between observations. All estimates are weighted using Census sample weights.

Figure A.6: **Place Effects vs. Household Carbon Emissions**

(a) **CBSA**




(b) **Tract**




**Note:** This figure shows event study estimates of the share of spatial variation in mean carbon emissions that can be explained by place effects, by size of origin-destination differences in mean household carbon emissions for movers. To two sets of points compare the full sample of movers (solid diamond) to the sample of movers with no significant changes to income or number of children (empty circle). The dotted black line shows the 45line. All estimates are weighted using Census sample weights.

## A.2 Additional Tables

Table A.1: **Mean $CO_2$ – Movers vs. Stayers**

| | CBSA Panel | | | | Tract Panel | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Moved | 0.05*** | 0.00 | | | 0.08*** | 0.02*** | | |
| | (0.002) | (0.002) | | | (0.001) | (0.001) | | |
| From | | | -0.11*** | -0.04*** | | | -0.07*** | -0.03*** |
| | | | (0.002) | (0.002) | | | (0.001) | (0.001) |
| To | | | -0.03*** | -0.04*** | | | -0.02*** | -0.03*** |
| | | | (0.001) | (0.001) | | | (0.001) | (0.001) |
| Cons. | 2.85*** | 2.82*** | 2.86*** | 2.81*** | 2.85*** | 2.82*** | 2.88*** | 2.83*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.001) | (0.000) | (0.000) |
| $R^2$ (adj.) | 0.719 | 0.741 | 0.191 | 0.345 | 0.717 | 0.738 | 0.342 | 0.449 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |

**Note:** This table compares household carbon emissions for movers and stayers. Columns (1)-(2) and (5)-(6) compare movers overall to stayers overall, with and without controls. Movers have higher carbon emissions than stayers, but the differences is smaller after controlling for differences in income and other demographic characteristics. Columns (3)-(4) and (7)-(8) present within-comparisons of stayers in a given place to movers from that place and movers to that place. The results here highlight that generally, movers have lower emissions than stayers, both at their origin and destination locations.

Table A.2: **Probability of Moving**

| | CBSA | Tract |
|---|---|---|
| $-$ N kids | 0.007*** | 0.017*** |
| | (0.0004) | (0.0007) |
| + N kids | 0.050*** | 0.150*** |
| | (0.0005) | (0.0008) |
| $\Delta$ HH inc $<$ -50% | 0.035*** | 0.078*** |
| | (0.0006) | (0.0010) |
| $\Delta$ HH inc $>$ 50% | 0.070*** | 0.154*** |
| | (0.0005) | (0.0008) |
| Constant | 0.044*** | 0.142*** |
| | (0.0003) | (0.0004) |
| $R^2$ (adj.) | 0.018 | 0.046 |
| N | 1,715,000 | 1,656,000 |

**Note:** This table shows that households with a change in the number of children or a larger than 50% (in absolute value) change in income are much more likely to move than stay. This is especially true of positive increases in both of these outcomes, and particularly for moves across neighborhoods.

Table A.3: **Mover Origin and Destination Types**

(a) CBSA Movers

| | To Rural | To Suburban | To Urban | Total Share |
|---|---|---|---|---|
| From Rural | 0.11 | 0.09 | 0.05 | 0.25 |
| From Suburban | 0.10 | 0.21 | 0.11 | 0.42 |
| From Urban | 0.06 | 0.15 | 0.12 | 0.33 |
| Total Share | 0.27 | 0.45 | 0.28 | 1.00 |

(b) Tract Movers

| | To Rural | To Suburban | To Urban | Total Share |
|---|---|---|---|---|
| From Rural | 0.09 | 0.07 | 0.03 | 0.19 |
| From Suburban | 0.08 | 0.28 | 0.08 | 0.44 |
| From Urban | 0.04 | 0.14 | 0.18 | 0.36 |
| Total Share | 0.21 | 0.49 | 0.29 | 1.00 |

**Note:** This table shows shares of origin-destination tract types for CBSA movers (panel (a)) and tract movers (panel (b)). Close to half of households move to suburban tracts. The most common type of move (among both CBSA and tract movers) is from a suburban tract to a suburban tract. Tract movers are less likely to move either from or to a rural neighborhood, in part because rural tracts are less likely to be in the leave-out connected tract set.

Table A.4: **Place-Based Heterogeneity in CO2 – Sensitivity to Outcome Definition**

| | CBSA | | | | Tract | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Share $Var(\psi_j)$ | 0.188 | 0.242 | 0.195 | 0.197 | 0.257 | 0.270 | 0.262 | 0.238 |
| Share $Var(\alpha_i)$ | 0.505 | 0.462 | 0.498 | 0.473 | 0.377 | 0.355 | 0.371 | 0.378 |
| Share $2 \cdot Cov(\alpha_i, \psi_j)$ | -0.001 | -0.001 | -0.001 | 0.006 | -0.006 | -0.000 | -0.005 | -0.002 |
| $R^2$ | 0.69 | 0.70 | 0.69 | 0.68 | 0.62 | 0.62 | 0.62 | 0.61 |
| $Var(\log CO_{2ij})$ | 0.29 | 0.35 | 0.29 | 0.20 | 0.28 | 0.35 | 0.29 | 0.19 |
| Baseline | X | | | | X | | | |
| Residential Only | | X | | | | X | | |
| NHTS Commute | | | X | | | | X | |
| NHTS Total mi. | | | | X | | | | X |

**Note:** This table presents KSS decomposition estimates testing the sensitivity of my results to different outcome definitions. Columns (1) and (5) present baseline estimates again, to ease comparisons. Columns (2) and (6) present estimates using residential energy use only as the outcome. Results highlight that there is more heterogeneity overall in residential energy use than in commuting, and a larger share is attributable to place effects – 24% at the CBSA level and 27% at the tract level. Evidently, residential energy use drives more of the spatial heterogeneity across CBSAs than commuting, while the two sectors contribute in approximately equal parts at the tract level. In columns (3)-(4) and (7)-(8) I test the sensitivity of my results to changing my estimate of emissions from the transportation sector, and using the combined residential+transportation energy outcome. The "NHTS commute" approach uses a penalized Lasso regression to predict vehicle fuel economy from individual and household demographic characteristics (age, race, household size, household income, gender, number of vehicles, commute mode of transit, commute length) and geographic characteristics (CBSA, state, urbanity) and adjust carbon emissions from commuting for estimated fuel economy. The "NHTS Total miles" approach uses the same variables to predict total annual vehicle miles travelled. Taking these approaches decreases the overall variance in my outcome, perhaps evidence that households with longer commutes drive more fuel efficient vehicles and/or drive less for other purposes, but doesn't substantially change the place share of heterogeneity – the largest change is from using the total miles measure, which decreases the tract share of variance from 26% to 24%.

*Go back to Section 5.2 (Results).*

Table A.5: **Place-Based Heterogeneity in CO2 – No Bias Correction**

| | CBSA | | | | Tract | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **A: Panel Sample** | | | | | | | |
| $Var(\log CO_{2ij})$ | 0.29 | 0.29 | 0.29 | 0.29 | 0.28 | 0.28 | 0.28 |
| Share $Var(\psi_j)$ | 0.201 | 0.107 | 0.111 | 0.220 | 0.558 | 0.452 | 0.454 |
| Share $Var(\alpha_i)$ | 0.567 | 0.564 | 0.564 | 0.562 | 0.765 | 0.759 | 0.759 |
| Share $2 \cdot Cov(\alpha_i, \psi_j)$ | -0.014 | -0.008 | -0.008 | -0.015 | -0.277 | -0.262 | -0.261 |
| $R^2$ | 0.74 | 0.66 | 0.66 | 0.75 | 0.77 | 0.69 | 0.69 |
| **B: Mover Sample** | | | | | | | |
| $Var(\log CO_{2ij})$ | 0.32 | 0.32 | 0.32 | | 0.31 | 0.31 | 0.31 |
| Share $Var(\psi_j)$ | 0.177 | 0.112 | 0.115 | | 0.491 | 0.406 | 0.413 |
| Share $Var(\alpha_i)$ | 0.505 | 0.503 | 0.503 | | 0.588 | 0.584 | 0.584 |
| Share $2 \cdot Cov(\alpha_i, \psi_j)$ | 0.001 | 0.004 | 0.004 | | -0.159 | -0.152 | -0.152 |
| $R^2$ | 0.69 | 0.62 | 0.63 | | 0.76 | 0.69 | 0.69 |
| Amenities | | X | X | | | X | X |
| Prices | | | X | | | | X |
| TV-FE | | | | X | | | |

**Note:** This table reports results from the biased AKM estimation of variance components. All specifications include demographic and household controls as well as time fixed effects. Columns (1) and (5) report the baseline variance decompositions at the CBSA and tract levels. Columns (2) and (5) add controls for local mean heating degree days, cooling degree days, and electricity emissions factors (all in logs). Columns (3) and (6) additional control for a price index, constructed from lagged fuel shares interacted with national retail prices. Finally, column (4) computes time-varying CBSA place effects using 5-year windows (2000-2004, 2005-2009, 2010-2014, and 2015-2019), using stayer observations across time windows to identify time variation in place effects, while movers, as before, identify cross-sectional variation.

*Go back to Section 5.2 (Results).*

*Go back to Appendix B.1 (Data Appendix).*

Table A.6: **Place Correlates w/ Observable Characteristics 1/2**

| | Above Median Inc. | White | College | Has Kids |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Suburban | 0.02*** | 0.01*** | -0.01*** | 0.02*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Rural | -0.06*** | 0.10*** | -0.06*** | -0.02*** |
| | (0.002) | (0.001) | (0.002) | (0.002) |
| Dist. to Closest City | -0.05*** | 0.02*** | -0.02*** | -0.01*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Dist. to Largest City | 0.07*** | -0.04*** | 0.02*** | 0.02*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Walk Score | -0.01*** | 0.01*** | -0.01*** | -0.00 |
| | (0.001) | (0.000) | (0.000) | (0.001) |
| Bike Score | 0.01*** | -0.00*** | 0.00*** | 0.01*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Transit Score | 0.00*** | -0.03*** | 0.00 | 0.01*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Bus Routes | 0.05*** | 0.03*** | 0.04*** | -0.05*** |
| | (0.005) | (0.003) | (0.004) | (0.005) |
| Rail Routes | 0.20*** | 0.05*** | 0.10*** | 0.04*** |
| | (0.004) | (0.003) | (0.003) | (0.004) |
| Tract Share Detached Homes | -0.38*** | -0.17*** | -0.26*** | 0.13*** |
| | (0.005) | (0.004) | (0.005) | (0.005) |
| Tract Share Homeowners | 0.16*** | 0.42*** | -0.19*** | -0.07*** |
| | (0.008) | (0.006) | (0.007) | (0.008) |
| Tract Mean Cars/HH | 0.38*** | -0.13*** | -0.34*** | 0.50*** |
| | (0.008) | (0.006) | (0.008) | (0.009) |
| Tract Mean Rooms/House | 1.15*** | 0.36*** | 1.00*** | 0.21*** |
| | (0.006) | (0.004) | (0.005) | (0.006) |
| Block Density | 6.69*** | -6.47*** | 1.01*** | 4.17*** |
| | (0.149) | (0.111) | (0.136) | (0.158) |
| Constant | -2.09*** | 0.18*** | -1.09*** | -0.51*** |
| | (0.011) | (0.008) | (0.010) | (0.012) |
| $R^2$ (adj.) | 0.09 | 0.06 | 0.04 | 0.02 |

**Note:** This table reports correlation coefficients between several demographic categories and a detailed vector of place characteristics.

Table A.7: **Place Correlates w/ Observable Characteristics 2/2**

| | Above Median Inc. | White | College | Has Kids |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Cooling Degree Days | -0.00*** | -0.00*** | 0.04*** | 0.00 |
| | (0.001) | (0.000) | (0.001) | (0.001) |
| Heating Degree Days | 0.03*** | 0.10*** | 0.08*** | -0.01*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Electric Grid Intensity | -1.29*** | 0.82*** | -2.55*** | 0.28*** |
| | (0.011) | (0.008) | (0.010) | (0.012) |
| Constant | 0.61*** | -0.10*** | -0.21*** | 0.53*** |
| | (0.011) | (0.008) | (0.010) | (0.012) |
| $R^2$ (adj.) | 0.01 | 0.06 | 0.04 | 0.001 |

**Note:** This table reports correlation coefficients between several demographic categories and a vector of exogenous place characteristics.

Table A.8: **10 most populous CBSAs (2020)**

| Rank | CBSA |
|---|---|
| 1 | New York-Newark, NY-NJ-CT-PA |
| 2 | Los Angeles-Long Beach, CA |
| 3 | Chicago-Naperville, IL-IN-WI |
| 4 | Dallas-Fort Worth, TX-OK |
| 5 | Houston-The Woodlands, TX |
| 6 | Washington-Baltimore-Arlington, DC-MD-VA-WV-PA |
| 7 | Philadelphia-Reading-Camden, PA-NJ-DE-MD |
| 8 | Miami-Port St. Lucie-Fort Lauderdale, FL |
| 9 | Atlanta-Athens Clarke County-Sandy Springs, GA-AL |
| 10 | Boston-Worcester-Providence, MA-RI-NH-CT |

# B Data Appendix

## B.1 Construction of Residential $CO_2$

$CO_2$ from residential energy use is constructed from household reported expenditures and primary heating fuels, paired with local average prices and local average emissions factors. Each of these elements introduces noise into my outcome measure, which I discuss in turn below.

**Household reported values:**

Households may not accurately remember or report their energy expenditures. Inaccurate reporting could arise for example due to inattention to bills, or due to bias driven by the seasonality of energy expenditures – e.g. if household use their last monthly bill to proxy for annual expenditures.

As long as household inattention is fixed, variation driven by this source of error will be absorbed by individual effects. This will result in overestimates of the individual share of spatial heterogeneity if high users tend to overstate their energy use and low users tend to understate their energy use, and it will result in underestimates of the individual share of spatial heterogeneity if the opposite is true. In either case however, with fixed inattention, estimates of place effects themselves will be unbiased. Relative place effects will be biased if moves are associated with changes to knowledge of energy bills, for example due to changes in income constraints.

Given random sampling of the surveys, I do not expect variation in the season households were surveyed to be correlated with other components of the model. Thus, this source of error might reduce the explanatory power of my model, but likely not introduce error into either the estimates of variance components or the estimates of place effects themselves.

**Assignment of heating fuel:**

Households report the primary fuel they used for heating their home. I use this variable to assign expenditures on other fuels. There are two possible sources of error here.

The first is that if households use more than one home heating fuel, I assign all of their expenditure to the one they listed as primary. The second is that if household list expenditures on home heating fuels, but don't specify which fuel they used, I assign their home heating fuel based on the most commonly used heating fuel among other survey respondents in their state and year (out of residual oil, propane, wood) In both cases, I will overstate fuel quantity used if prices for the reported or imputed fuel are cheaper than the unobserved fuel, and understate fuel quantity used if prices for the reported or imputed fuel are more expensive than the unobserved fuel. Similarly, this will overstate $CO_2$ per estimated quantity if the reported or imputed fuel is more carbon intense than the unobserved fuel, and vice versa.

As in the previous section, if there is a positive correlation between household type and unobserved $CO_2$ from heating fuels, my estimates will understate the component of variation driven by households, and vice versa, but not impact the variance component driven by place effects. If moves coincide with shocks to unobserved fuel components (for example, if moving to places that are on average lower emissions because most people use cleaner heating fuels results in my underestimating the use of dirtier fuels by some movers), this will bias my estimates of place effects (in this case, making individual $CO_2$ appear to have decreased from moving more than it actually did, overstating the role of place effects).

In practice, the share of households reporting non-zero energy expenditures on a heating fuel other than electricity or natural gas is small, and my estimates are not meaningfully affected by looking at the entire residential sectors vs. the electricity sector only.

**Measurement of electricity prices:**

There are three sources of measurement error for electricity prices.

First, in counties served by more than one utility, I cannot match customers to the actual utility they are served by. This is mainly a concern in territories where customers can select their residential energy provider; if higher use customers are selecting into lower average price utilities, I will under-estimate person-level variation in energy use. The bias in my estimates of person-level variation of $CO_2$ will depend on the correlation between price and $CO_2$ content of the utilities in their choice set. On the other hand, if utilities are segregated across neighborhoods within a county, and people use less energy at higher-price utilities, I will underestimate the variation in use across neighborhoods. If higher price utilities have cleaner energy, this effect is attenuated with the $CO_2$ outcome.

Second, residential customers generally face a two part tariff consisting of a fixed charge and a marginal volumetric charge, where the marginal price can either be increasing or decreasing in consumption depending on the utility. Because I am using average prices, calculated from utility residential revenues and quantities sold, I overestimate the average volumetric price and in turn underestimate consumption for everyone. Moreover, for some utilities, marginal prices are either increasing or decreasing in consumption. When prices are increasing in consumption, I under-estimate prices faced by high-demand customers and over-estimate prices faced by low-use customers. This means I over-estimate quantities consumed by high-demand customers and under-estimate quantities consumed by low-demand customers, leading to an upward bias in my estimates of the quantity spread between between high and low expenditure customers. Conversely, if prices are decreasing in consumption, I underestimate the spread in demand. Borenstein and Bushnell (2019) estimate that in the US, roughly 37% of customers face increasing block pricing, and roughly 21% face decreasing block pricing, though in all cases the rate structure is fairly narrow. They also estimate that across territories, utilities that utilize increasing-block pricing generally serve lower demand customers on average. Thus, my estimates likely somewhat over-estimate variation across households within utility territories, and underestimate variation across territories. Overall, unobserved rate structures should lead me to estimate a lower bound on place-based heterogeneity and estimate an upper bound on preference-based heterogeneity.

Finally, residential rates can vary within utilities, and I don't observe which rate a household has selected. This leads to the same biases as not being able to observe which utility a customer chooses, discussed above. Relatedly, I do not observe if a household has solar. In many states, solar customers face different price schedules with significant subsidies for selling generated power back to the grid. This lowers their average price per kwh, causing me to under-estimate mmbtu and in turn $CO_2$ from electricity purchased from the grid by these customers. I further under-estimate mmbtu for customers with rooftop solar because I don't observe mmbtu consumed from their solar panels, but this consumption is zero emissions so does not affect $CO_2$ estimates.

**Measurement of electricity carbon emissions factors:**

In addition to price measurement error, there is measurement in $CO_2$ intensity of electricity. My measure of average emissions intensity does not capture the fact that electricity is generated from different fuels throughout the course of the day (e.g. solar peaks in the afternoon) and across seasons (e.g. there is less solar in the winter). If consumption profiles are correlated with these patterns, my estimates will be biased.

**Measurement of natural gas and other residential heating fuel prices and $CO_2$:**

Many of the same price measurement errors arise with natural gas as with electricity, but generally individuals have less choice over their utility, fixed charges are larger, and there is less prevalence of block pricing.

In addition to the price measurement error, in the case of natural gas a significant source of emissions is upstream methane from leaks.

*Go back to Section 2.2 (Outcome Variable Construction).*
*Go back to Section 5 (Results).*

## B.2 Construction of Transportation $CO_2$

There are several sources of error in the construction of $CO_2$ from transportation. First, I estimate commute distance from geodesic distances between work and home, and reported commute travel time. Second, I impute annual number of commutes using weeks worked last year and hours worked last week. Third, I impute energy used for commuting using national average fuel economy for car commuters and assigning zero emissions to households who commute by public transit. Finally, I only observe commuting and not total transportation. I discuss each of these in turn.

I estimate commute mileage using the GPS distance between reported home and place of work census blocks. To account for the fact that geodesic distances don't capture the indirect nature of roads, I rescale my mileage estimates to match the national average commuting distance reported in the NHTS (12 miles). For individuals who only report their county of work but not their census block of work, I impute miles travelled using reported commute time and average commute speeds for people with similar residence-job geographic pairs. I use a similar imputation for individuals for whom the travel speeds implied by dividing estimated miles by commute time are infeasible – over 80 mph on average in a car or motorcycle, and over 150 mph in a train.[22]

Because I estimate commute miles from geodesic distances between coordinates, I will underestimate speed and miles travelled for individuals who have less direct commutes than average. This will understate overall heterogeneity. Additionally, I impute miles for the people for whom I don't observe census block of work using average mph for home-place of work county pairs. This bias acts in the same direction, causing me to understate overall heterogeneity. In both cases, I'm unable to say in which direction this biases my estimates of the relative shares of place and person components of heterogeneity. I find similar results when using a simpler esimate of commute distance, based on dividing reported commute time by the average national commute speed, 32 mph (NHTS).

I convert hours worked last week to commuting days per week by assuming people work 8 hours a day up to 5 days a week, assuming people worked 5 days if they worked 40-50 hours a week, 6 days if they worked 50-60 hours in a week, and 7 days if they worked more than that. I convert commuting days per week into commutes per week by assuming everyone commutes twice a day on the days they commute. I convert commutes per week to annual commutes using weeks worked last year. This assumes hours worked are stable, that people work at the same place all year, and that information about commutes reported for last week is representative of commutes generally. Any deviations along these dimensions introduces measurement error into my outcome, which becomes a bigger problem if error is correlated to household types (for example if higher income households are more likely to have stable commutes).

I convert miles to gallons of gasoline assuming that everyone drives a vehicle with the annual national average fuel economy, using data from the NHTS.[23] This is a significant oversimplification, as it ignores patterns of heterogeneity in fuel economy both across commute lengths and across regions. I do account for the fact that in general fuel economy is roughly 30% higher when driving on highways than in cities by adjusting mpg up by 19% relative to the national average for drivers whose average commuting speed is greater than 55 mph, and down by 9% relative to the national average for drivers whose average commuting speed is lower than 40 mph (EPA, 2021).

If people with longer commmutes drive more fuel efficient vehicles, I will overstate heterogeneity. On the other hand, if people who want to conserve on gas both buy more fuel efficient vehicles and choose to have shorter commutes, I will understate heterogeneity. The bias in my estimates of relative shares is more ambiguous. If these patterns are driven soley by individual preferences, I will over/understate the relative importance of the person component in spatial variation. On the other hand, if they are driven by local norms or place characteristics such as e.g. the availability of parking, I will over/understate the relative importance of the place effect.

Another source of error arises in the assignment of emissions to other modes of transit. In practice, most public transit in the US is not zero-emissions right now. Assigning 0 emissions to public transit exacerbates heterogeneity across places with and without transit options. In

22. This is the fastest speed a train ever goes in the US, along a small segment of the Northeast Corridor (CITE).

23. For motorcycles, I scale mpg by 2 (FHWA, 2019). This is a minor point as motorcycles account for only roughly 0.6% of vehicle miles driven (EPA).

practice, only 5% of individuals commute using public transit. Also, if household alternate their mode of commute, I don't capture this variation.

Lastly, I don't observe transportation other than commuting. If commutes are a rank-preserving share of transportation emissions, I will underestimate carbon emissions magnitudes (and likely the magnitude of overall variation), but my variance component estimates will not be biased. However, if in certain places the majority of travel is driven by commutes while in other places the majority of travel is for leisure, then my estimates will not properly reflect the place effects of travel overall.

*Go back to Section 2.2.*

### B.3 Construction of Other Variables

- **Missing and imputed variables:** I follow Chetty & Hendren (2018) and Bailey, Hoynes, Rossin-Slater and Walker (2019) and treat all imputed variables as missing, unless otherwise described. Dollar values are inflated to 2019 values using the CPI. Throughout the analysis I use demographic and household characteristics to control for selection on time-varying observables, I use work characteristics to construct commuting variables, and I use home characteristics in the second half of the paper to characterize places and study associations between built environment and place effects.

- **Flags**: In 2014 the ACS flags a lot of variables as "allocated" (to 0) if they checked a box indicating that they did not use natural gas or fuel use and then left the expenditure question blank. Because of this, I make an exception to the allocation flag and allow for residential energy to be allocated to 0 based on the checkbox question.

- **Identifying kids:** I designate a household member a child and drop them from the analysis sample if they are under the age of 18, or if they are identified as a child via the Census' relationship to householder code.

- **Work characteristics:** For each individual I have employment status, industry and occupation, place of work, weeks worked last year, and hours worked last week. I allow place of work tracts or more detailed geographies to be missing, but I drop observations if county of work is missing (unless the individual works from home, in which case I impute their place of work from their home, or if they are unemployed). I also allow current employment status to be missing if weeks worked last year and hours worked last week are not missing and not imputed. In 2008-2018, the weeks worked variable is binned; I follow Chetty and Hendren (2018) and assign the midpoint to all individuals in the bin. Since these variables are an input into my measure of commuting energy use, I use the midpoint from the bin for all years to keep the variable definition consistent.

- **Carpooling**: I divide $CO_2$ by the number of car-poolers for individuals who report car-pooling.

- **Household characteristics**: I allow building age to be unknown in my analysis sample

# C Computational Appendix

For parsimony, I proceed in two steps, regressing log$CO_2$ on observable characteristics and year fixed effects, and residualizing so that I am left with

$$\tilde{y}_{ij} = \alpha_i + \psi_j + \varepsilon_{it}$$

The share of overall variance attributable to place effects can then be captured by the variance component of place effects,

$$Var(\psi_j) \equiv \sigma^2_\psi = \frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T}(\psi_{j(i,t)} - \bar{\psi})^2$$

and the covariance component between place effects and person effects

$$Cov(\alpha_i, \psi_j) \equiv \sigma^2_{\alpha,\psi} = \frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T}(\psi_{j(i,t)} - \bar{\psi}) \cdot \alpha_i$$

KSS provides an estimate for the standard error $\psi_i^2 = Var(\varepsilon_i)$ based on a leave out estimate of $\sigma_i^2$:

$$\hat{\sigma}_i^2 = y_i(y_i - x_i'\hat{\beta}_{-i}) = y_i\frac{(y_i - x_i'\hat{\beta})}{1 - P_{ii}}$$

where $P_{ii} = x_i'(x_i x_i')^{-1}x_i$ is the observation leverage.

# D Price elasticity estimates in the literature

Table D.9: Electricity Price Elasticities

| Estimate | Paper | Notes |
|---|---|---|
| [-0.054, -0.088] | Ito (2015) | California, 1999-2017<br>elasticities wrt average price<br>larger for lagged prices than contemporaneous and more so wrt the average of several lags<br>medium run |
| -0.064 | Labandeira et. al. (2017) | meta-analysis<br>average residential short run |
| -0.042 | Labandeira et. al. (2017) | meta-analysis<br>average residential medium run |

Table D.10: Residential Fuel Price Elasticities

| Estimate | Paper | Notes |
|---|---|---|
| [-0.23, -0.17] | Auffhammer & Rubin (2018) | California, 2010-2014<br><br>-0.23 is average p, -0.17 is marginal p<br>CARE customers more elastic than non-CARE<br>Elasticity significantly higher in winter (-.38) and 0 in summer<br>medium run |
| -0.065 | Labandeira et. al. (2017) | meta-analysis<br>average residential nat gas short run |
| -0.116 | Labandeira et. al. (2017) | meta-analysis<br>average residential nat gas medium run |
| -0.110 | Labandeira et. al. (2017) | meta-analysis<br>average residential heating oil short run |
| -0.481 | Labandeira et. al. (2017) | meta-analysis<br>average residential heating oil medium run |

Table D.11: VMT Price Elasticities

| Estimate | Paper | Notes |
|---|---|---|
| -0.09 | Wenzel & Fujita (2018) | Texas, 2005-2010 |
| [-0.14, -0.40] | Knittel & Sandler (2012) | CA inspection and registration data<br>long run |
| -0.15 | Knittel & Sandler (2013) | CA inspection and registration data<br>medium run |
| [-0.33, -0.17]<br>-0.22 | Gillingham (2014) | CA inspection and registration<br>range is lowest and highest quantile<br>-.22 is mean<br>medium run |
| -0.10 | Gillingham et. al. (2015) | PA inspection<br>short run |