The Role of People vs. Places in Individual Carbon Emissions

Eva Lyubich *

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Abstract

There is substantial spatial heterogeneity in household carbon emissions across the US, and a strong relationship 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 household characteristics and sorting. To do this, I construct a longitudinal panel of residential energy use and commuting behavior 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 60,500 neighborhoods across the US. I find that place effects explain more than half of mean differences between places, and about 15-25% of overall variation in carbon emissions. 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%, suggesting that place-based climate policies to could yield sizable reductions in household carbon emissions.

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1 Introduction

Climate change, caused by carbon emissions and other greenhouse gases, is an urgent threat. 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 consequences, including increased frequency and severity of extreme weather events, degradation of ecosystems, and population displacement. There is substantial spatial heterogeneity in household carbon emissions across cities 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 high-quality public transportation infrastructure, that higher-emissions places could adopt in order to lower household carbon emissions.

However, differences in mean carbon emissions across places reflect a combination of local amenities, household 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 rail lines would have little impact on household emissions. Conversely, if the lack of denser housing and transit options is a constraint on household choices, rather than a reflection of their preferences, then interventions that change these local public amenities could have the potential to decrease carbon 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 one 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. I use a mover design, examining changes to household carbon emissions for over 250,000 movers across roughly 1,000 cities and 60,500 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. While previous work has shown evidence of substantial variation in household carbon emissions (e.g. Jones and Kammen 2014; Ummel 2014; Green and Knittel 2020), the level of geographic granularity in publicly available data has limited researchers to predicting neighborhood-level carbon emissions from national data projected onto local place and household characteristics. In contrast, detailed geographic identifiers in the administrative Census Bureau microdata make it possible to directly estimate neighborhood-level means. 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 and causal place effects. Place effects could result from a variety of local amenities and supply-side factors that determine patterns of household energy use. For example, place effects could reflect aspects of urban form such as public transportation, bike and pedestrian infrastructure, highway networks, density, and/or zoning regulations. They could also be driven by natural amenities such as climate. Lastly, they could arise from supply-side factors that determine fuel shares and electricity emissions factors, both of which shift the carbon emitted for a given level of energy use. In the conceptual framework of this paper, I show how place effects relate to the parameters of a consumer energy demand model in which average energy demand, energy demand elasticities, energy prices, and average emissions factors vary across places.

My empirical strategy uses movers in my data to estimate the contributions of place effects and household characteristics to heterogeneity in household carbon emissions. The mover design accounts for unobserved differences between households by comparing carbon emissions for the same household living in different places. The central assumption in the mover design is that changes to unobserved determinants of household carbon emissions are uncorrelated with mover destinations. A crucial advantage of undertaking this analysis with Census Bureau microdata is that I observe, and can control for, many time-varying household characteristics that could correlate with both potential emissions and destination choices, including income and children. I proceed in two steps.

First, I use an event study to estimate how much carbon emissions change after households move, as a share of the mean difference between their origin and destination. On average, when households move to a new city, their carbon emissions change by about 90% of the mean difference between their origin and destination cities. There is more sorting of households across neighborhoods, but even at the neighborhood level more than half of mean differences between neighborhoods is explained by place effects: when households move to a new neighborhood, their carbon emissions change by about 60% of the mean differences between their origin and destination neighborhoods. These shares are symmetric for moves to lower or higher emissions places, and they are stable across moves between places with large mean differences or small mean differences in carbon emissions. Under the baseline assumptions of the mover design, the estimates from the event study can be used to calculate the emissions externality of policies that drive existing patterns of household migration, for example policies that restrict housing supply in on average low-emissions cities. To interpret the event study shares as unbiased causal estimates of the effect of moving between any pair of places, it is necessary to impose an additional assumption that there is no systematic selection of certain types of households to certain types of places.

To allow for unrestricted patterns of sorting, I estimate a non-parametric distribution of household and place effects from a two-way fixed effects model. I use these estimates to do a variance decomposition of overall heterogeneity, including heterogeneity across households living in the same place as well as heterogeneity not explained by the data. The weaker identifying

assumptions afforded by the two-way fixed effects decomposition come at the cost of limited mobility bias: naive estimates of variance components are upward biased because some place effects are estimated from only a few movers (Andrews et al. 2008). I account for this upward bias using the heteroskedasticity-robust "leave-out" estimator proposed by Kline, Saggio, and Sølvsten (2020). I find low correlations between unobserved household and place effects, even at the neighborhood level. This suggests that unobserved sorting contributes to differences between places through "segregation" of households, but not in a way that is systematically correlated with unobserved neighborhood attributes. City effects explain roughly 15% of overall heterogeneity, while neighborhood effects explain roughly 25% of overall heterogeneity. Controlling for variation driven by climate, electricity emissions factors, and energy prices in order to isolate the dimensions of place that more likely reflect urban form decreases the place shares to 10% at the city level and 15% at the neighborhood level. While this leaves the majority over overall heterogeneity to other factors, my estimates nevertheless imply the potential for considerable reductions to household carbon emissions from interventions that decrease place effects: I estimate that if a neighborhood goes from having a place effect one standard deviation above the national mean to having a place effect one standard deviation below the national mean, household emissions for residents of that neighborhood would decrease by about 40%.

I characterize low and high emissions neighborhoods by examining observable characteristics from within the Census Bureau data as well as from Walk Score[1], a private company that generates estimates of the walk-ability, transit-ability, and bike-ability of every address in the US.[2] I find that the correlations between amenities and neighborhood place effects for the most part mirror the relationships in the observational data; many of the amenities observed in low emissions places are characteristic of urban areas. For example, low emissions neighborhoods are substantially higher density, they have better walking, biking, and public transportation amenities, and they have smaller homes and a lower share of single-family homes.

Together, the wide distribution of place effects and the strong relationship between place effects and observable place amenities imply that "place-based climate policies" – i.e. policies that aim to reduce household carbon emissions from residential and transportation energy by changing the amenities of the places that people live in – do have the potential to lead to substantial carbon emissions reductions. To explore this further, I consider the impact on carbon emissions of several urbanization scenarios, which range from limiting suburban sprawl and shifting the joint distribution of people and place effects so that more people live in neighborhoods with the place effect of the city closest to them, to deep urbanization in which more cities across the US grow to look like New York City. These exercises don't take into account general equilibrium effects, but serve as a useful first order approximation of the effect on household carbon emissions of policies that lead more people to live in more urban neighborhoods. In my preferred scenario – where more households live in neighborhoods with the place effect of the city nearest to them – I estimate that, on average, carbon emissions from residential energy use and commuting would fall by 13% for exposed households. To put this estimate into context, the Inflation Reduction Act, which was signed into law in August 2022 and is the largest Federal effort to address climate

1. www.walkscore.com
2. Data provided by Redfin Real Estate.

change to date, is projected to decrease economy-wide emissions in 2030 by an additional 15% relative to projected reductions from 2005 levels under business as usual.[3]

This paper provides new evidence on the role of places in household carbon emissions, making several contributions to the literature on spatial heterogeneity in this setting. First, 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. Second, my results help with the interpretation of a set of related papers that have 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). For example, knowing the relative roles of place effects vs household sorting in spatial heterogeneity in carbon emissions can help inform decisions about how much to re-distribute carbon dividends using geography as a tag. Third, I focus on the causes, rather than the consequences, of spatial heterogeneity in household carbon emissions, and 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 and sector-specific contexts (Auffhammer and Rubin 2018; Gillingham 2014; Nowak and Savage 2013; Spiller et al. 2014). Fourth, my paper builds a bridge to a set of papers that uses 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). While I don't identify causal effects of any specific aspect of urban form, my paper lends new causal evidence on the extent to which the collective bundle of amenities that define a place matters as a whole.

This paper also contributes to a broader literature spanning public finance, labor, and urban economics on the role that places play in determining other individual outcomes of interest. My paper is the first to use a mover design to examine household heterogeneity in energy use and carbon emissions. In using this empirical strategy, I am building on and contributing to a growing body of work that uses mover designs to estimate place effects on other individual outcomes, e.g. nutritional choices (Allcott et al. 2019), health outcomes and health care utilization (Eid et al. 2008; Finkelstein, Gentzkow, and Williams 2016; 2020), intergenerational mobility (Chetty and Hendren 2018), and wages (de la Roca and Puga 2018; Card, Rothstein, and Yi 2021). My paper also relates to a large set of papers that examine the role of density (e.g. see Duranton and Puga 2020, for a review) and transportation infrastructure (Tsivanidis 2019; Allen and Arkolakis 2021) – two amenities closely related to energy use and carbon emis-

3. Three separate efforts to model IRA reductions have been commonly cited by advocates and lawmakers. The Rhodium Group estimates that under IRA emissions will fall 31-44% from 2005 levels by 2030, with 24-34% reductions under business as usual (Larsen et al. 2022). For the same time frame, Energy Innovation estimates 37-41% reductions under the IRA and 24% under business as usual (Mahajan et al. 2022), and The REPEAT project estimates 42% reductions under IRA and 17% under business as usual (Jenkins et al. 2022).

sions – in determining productivity, wages, and wage inequality, and relates even more closely to several papers that have studied the effect of transportation infrastructure on urban form and energy use specifically (e.g. Baum-Snow 2007; Duranton and Turner 2018). Finally, in examining potential benefits of "place-based climate policies", this paper relates to a literature estimating the valuation of local public goods (Bayer, Ferreira, and McMillan 2007; Schönholzer 2021) and the costs and benefits of using place-based policies to improve aggregate welfare (e.g. Kline and Moretti 2014; Gaubert, Kline, and Yagan 2019; Fagjelbaum and Gaubert 2020). Overall, the evidence on the relative importance of places vs. sorting, and the potential benefits of place-based policies, is mixed, and in my paper a large share of variation in carbon emissions is driven by factors other than places. Nevertheless, my results highlight considerable potential reductions in household carbon emissions from changes in the distribution of place effects, adding evidence on one channel through which places do play a key role in individual outcomes.

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).[4]

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

4. 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. There is some variation in assignment succes rate across demographic groups – in particular white and higher income individuals are more likely to be successfully assigned a Protected Identification Key – but for all demographic subgroups the success rate is greater than 85%. See Bond et al. (2014) for additional discussion of the variation in assignment rates across population subgroups.

I use, and discuss the construction of my analysis sample. Additional details can be found in Appendix B.

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 city or labor market and a neighborhood.

My first geographic unit of analysis is a Core Based Statistical Area (CBSA). CBSAs are 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 with strong commuting ties and at least one urban core area of at least 10,000 people. In addition to formally designated CBSAs, I define residual CBSAs by state from unassigned rural areas. My second geographic unit of analysis is a census tract. 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.[5]

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.[6]

I estimate carbon emissions from residential energy use from 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 from the Energy Information Administration (2020a) Annual Electric Power Industry Report. This report contains sales, revenues, and total customers for every major utility in the US, by sector and state. It also delineates counties contained in each utility's service territory. I calculate county-level retail electricity prices using customer-weighted average prices (revenue divided by sales) across all utilities with service territories containing the county, and I compute household electricity consumption by dividing reported expenditures by my price estimates. I then assign households to one of 12 National Electric Reliability Council sub-regions using a tract-level crosswalk from the Homeland Security (2021) Infrastructure Foundation-Level Database, and compute emissions using the average annual emissions rates assigned to each sub-region 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

5. 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.

6. 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).

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.

I estimate carbon emissions from transportation energy from individually-reported commuting behavior. My outcome captures variation in carbon emissions driven by commute lengths, number of commutes, and mode of transit.[7] 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 carbon emissions from commuting.[8] 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.[9]

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.

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

7. 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.

8. 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.

9. 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.

omitted categories. Other than "white", these are the categories with the highest within-group variance in outcomes. Thus this normalization should err towards finding a larger unobservable person component relative to place component.

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. Since 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 capture 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.

To account for neighborhood-level variation in transportation and leisure amenities, I use data from Walk Score, a private company that generates estimates of the walk-ability, transit-ability, and bike-ability of every address in the US.[10] 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. Finally, I observe 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

10. Data provided by Redfin Real Estate.

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 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.[11] 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 sample 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.[12] 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 (see Appendix C for an illustration). 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. The main analysis is implemented at the household level: carbon emissions are given by household residential emissions plus the sum of individual commuting emissions over all individuals in the household,

11. In the 2000 Decennial Census, the question asked whether respondents had moved within the last five years. Since this is significantly more restrictive, I don't drop these individuals.

12. 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.

and other household level characteristics are taken as averages over person characteristics. All estimates are weighted using Census sample weights.

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 also 6 percentage points less 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 samples. I observe the vast majority of households in my panel 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 more than 50% change 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). For additional comparisons of movers vs. stayers, estimates of the likelihood of moving given shocks to household income or number of children, and the full set of transition probabilities across urban, suburban, and rural places, see Appendix Table A.1, Table A.2, and Table A.3 respectively.

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 sample statistics are weighted using Census sample weights. Counts 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
Δ num. kids	-0.12	-0.12	0.07	0.08
% Δ num. kids $\neq 0$	18.6	18.7	29.8	29.8
C: Mover Place Changes				
Δ Walk Score			-6.4	-6.5
Δ Bike Score			-3.9	-3.9
Δ Transit Score			-2.3	-2.7
Δ N Bus Routes			-0.5	-0.5
Δ N Rail Routes			-0.04	-0.04
Δ 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
%Δ CDD			21.4	16.4
%Δ 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 sample statistics are weighted using census sample weights. Counts 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 and 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 carbon emissions across the United States. I estimate unconditional and conditional place means, μ_j, using an ordinary least squares regression of log of individual CO_2 onto place fixed effects, year fixed effects τ_t, and in the conditional regression, individual and household observable characteristics X_{it}:

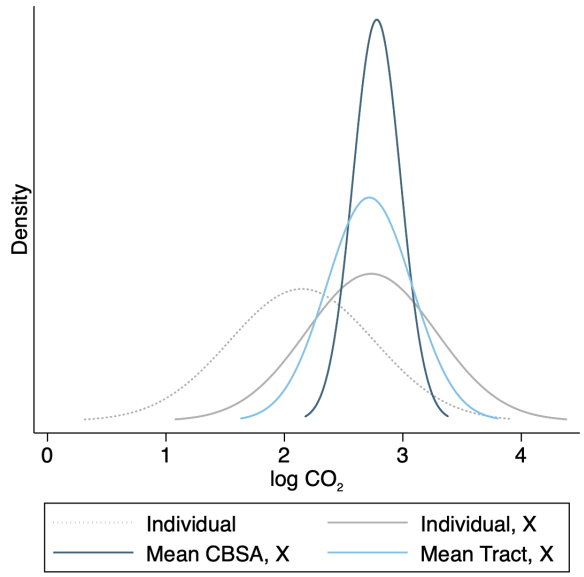
$$\ln CO_{2it} = \mu_{j(i,t)} + X_{it} + \tau_t + \varepsilon_{it} \tag{1}$$

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.[13]

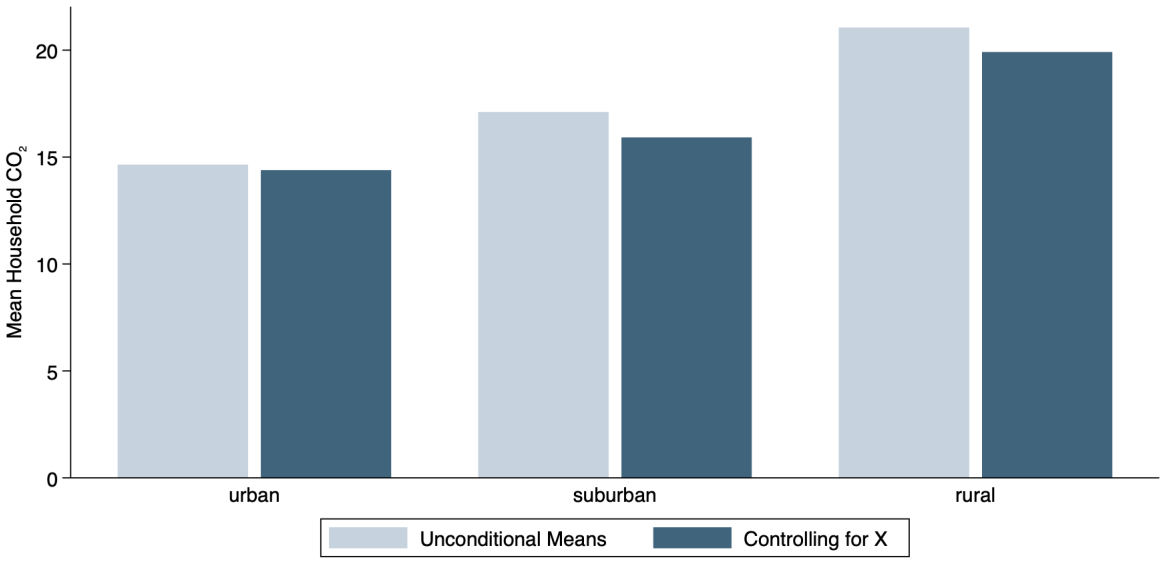
13. Relatively small differences between urban and suburban means potentially reflect a pretty broad definition of urban by the Census.

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).[14] Average demand a_j, price elasticities of demand ρ_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, α_i, individual time-varying unobserved determinants of demand ε_{it}, and national annual trends τ_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, building codes, 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 the quantity of fuel consumed and the fuel's carbon emissions factor ϕ^f. Emissions factors reflect the physical carbon content of fuel. They are constant over time and place for natural gas, oil, and motor gasoline, but vary for electricity as a result of differences in fuels used for electricity generation. Thus, household consumption of residential and transportation energy results in carbon emissions:

$$CO_{2it} = \phi_{jt}^e \cdot E_{it}^e + \phi^{ng} \cdot E_{it}^{ng} + \phi^o \cdot E_{it}^o + \phi^{mg} \cdot E_{it}^{mg}$$

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

For each fuel f, define s_i^f as household i's fuel share of fuel f. Also define s_j^f as the average fuel share of fuel f in place j, and $\tilde{s}_i^f$ as household i's relative fuel share, $\frac{s_i^f}{s_j^f}$. This allows me to rewrite household carbon emissions as a function of their total energy consumption, fuel emissions factors, and (relative) fuel shares

$$CO_{2it} = \sum_{f \in \mathcal{F}} \left(\tilde{s}_i^f s_j^f \cdot \phi_{jt}^f \right) \cdot E_{it}$$

Combining this expression with Equation 2 yields

$$\begin{aligned} \ln CO_{2it} &= \underbrace{\ln \left(\sum_{f \in \mathcal{F}} s_j^f \cdot \phi_{jt}^f \right) + a_j + \sum_{f \in \mathcal{F}} \rho_j^f \cdot \ln P_j^f}_{\psi_j} + X_{it}\beta + \tau_t + \alpha_i + \ln \left(\sum_{f \in \mathcal{F}} \tilde{s}_i^f \cdot \phi_{jt}^f \right) + \varepsilon_{it} \\ &= \psi_j + X_{it}\beta + \tau_t + \tilde{\alpha}_i + \tilde{\varepsilon_{it}} \end{aligned} \tag{3}$$

Equation 3 is the baseline estimating equation that I take to the data. The above derivation shows that place effects ψ_j capture place-based variation in average energy demand, variation in price elasticities of energy demand, variation in prices, average electricity emissions factors, and average fuel shares. Household effects α_i capture relative energy demand. In a simplified world in which household's relative fuel shares and electricity emissions factors are constant across places, household effects also capture household relative fuel shares (scaled by fuel emissions factors). However, in reality the model contains an interaction between household and place specific factors – households with the same fuel shares across places will have lower emissions in places with cleaner electricity, and similarly, moving to a place with cleaner electricity will lead to larger reductions among households with higher electricity shares. This means that there is some mis-specification built into the two-way fixed effects model.

It is worth noting two additional simplifications that the model makes. First, I allow price elasticities of demand to vary across places (as a result of amenities that serve as complements or substitutes to energy consumption) but not to vary across households. Allowing for heterogeneity in demand elasticities across households introduces a second interaction term in the error, as household elasticities are interacted with place-specific prices. Both this interaction and the interaction between household fuel shares and place-specific electricity emissions factors motivate treating the errors as heteroskedastic. Second, in the baseline model place effects are fixed, meaning that any time variation (including e.g. in prices) is absorbed in the place effects, which reflect average differences between places over my sample time frame.

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 τ_t with X_{it} for brevity):

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

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.[15] 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-

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

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 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: (1) additive separability of place effects, or constant effects (2) non-persistence of outcomes, and (3) exogenous mobility, or conditional orthogonality.[16]

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. For example, 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 examples centered on urban form: 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.

Nevertheless, the two-way fixed effects model imposes a substantial restriction: it does not allow for heterogeneous treatment effects or match effects. I already showed in Section 3 that the model is mis-specified, as there is an interaction between individual fuel shares and place-specific emissions factors – a person who prefers electricity to natural gas for cooling and heating will experience a larger decrease in emissions when moving to a place with a clean electricity grid than a person who prefers natural gas to electricity, all else equal. In addition to this, 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

16. These are discussed in much more depth in Hull (2018)

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.

Given this, the two-way fixed effects model should be treated as an approximation, and the question becomes whether there is selection of certain types of households to certain types of places. If there is an interaction term in the error, as long as there is no selection on this interaction, the mover design will yield unbiased estimates of the average place effects. To rule out selection on heterogeneous effects, I follow Card, Heining, and Kline (2013) and test whether moving from a low emissions place to a high emissions place and moving from high emissions place to a low emissions place are associated with equal and opposite changes in household carbon emissions. 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:

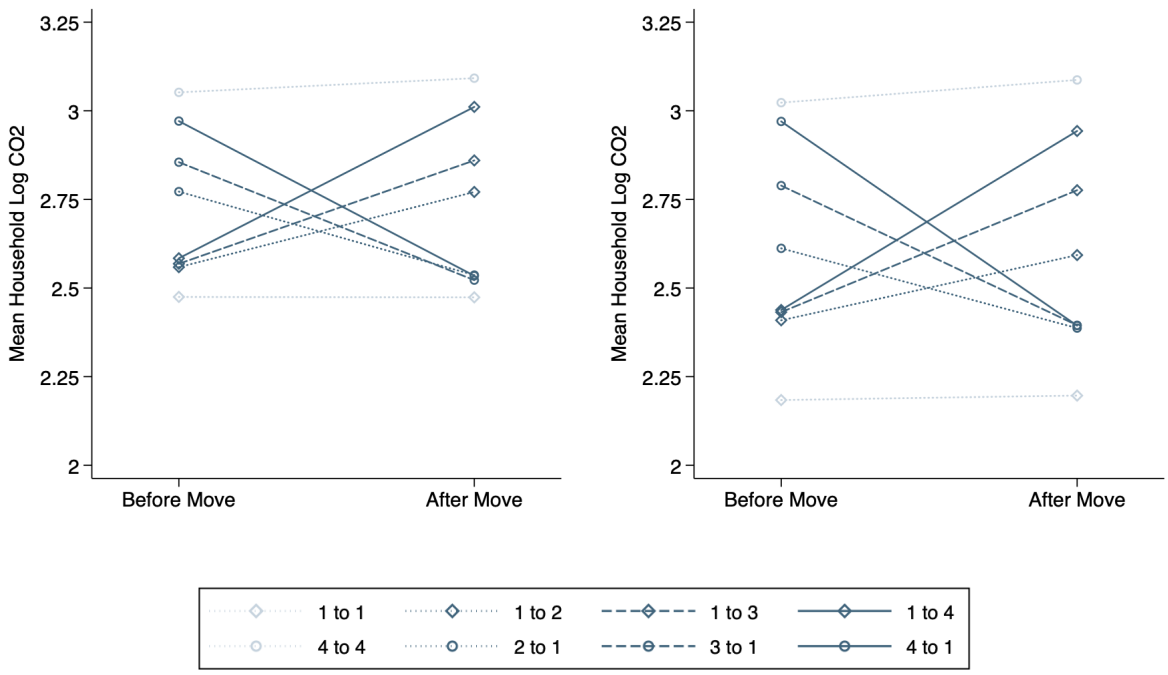
$$|\eta(\alpha_h \cdot \psi_d) - \eta(\alpha_h \cdot \psi_o)| > |\eta(\alpha_l \cdot \psi_d) - \eta(\alpha_l \cdot \psi_o)|$$

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 carbon emissions, and I estimate household carbon emissions 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 emissions 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).

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 2: Non-persistent Outcomes.

As highlighted above, relative place effects are identified from pairwise comparisons of household carbon emissions 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.

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 sorting of households 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 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, θ:

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

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 θ as opposed to the full distribution of J place effects. However, this efficiency comes at the cost of an additional assumption, that heterogeneity in θ 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.[17]

4.3 Variance 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

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

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. 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 D 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 descriptive evidence and can be used to predict how household carbon emissions will change for movers under existing patterns of mobility. 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 Decomposition

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.[18]

18. 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

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.

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 household sorting across neighborhoods plays an important role in neighborhood-level variation in carbon emissions, while moves across CBSAs are more likely to be driven by other factors such as new job opportunities or proximity to friends or family.

In column (2), 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

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.

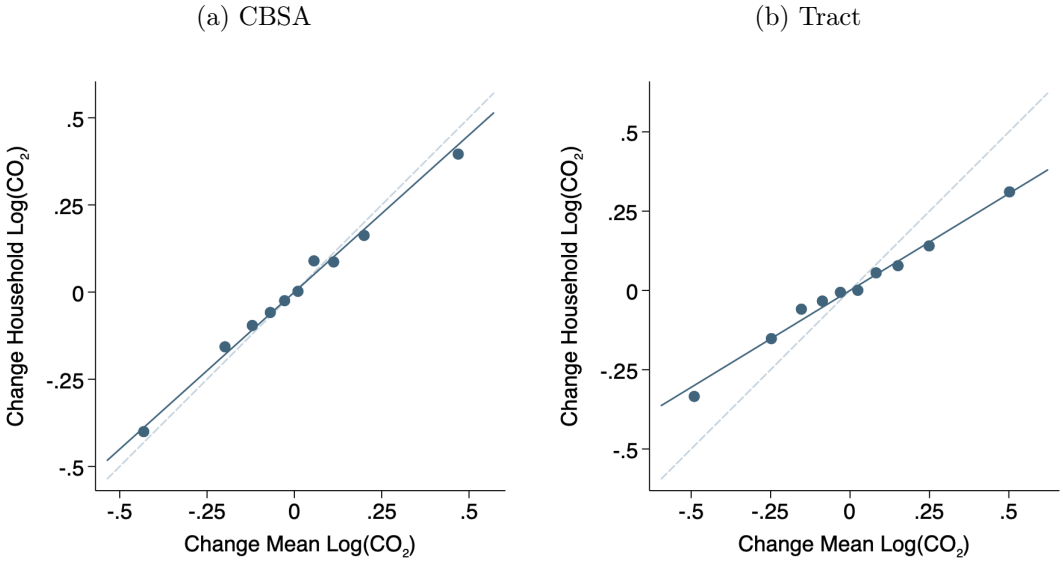
to differ systematically from stayers. Once again, this does not change the estimates in the CBSA specification, but further decreases the the share of spatial heterogeneity attributable to tract effects by 3 percentage points.

Even after accounting for differences between movers and stayers, mover destinations might still reflect heterogeneous preference shocks. 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. 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 explore two additional dimensions of heterogeneity: the magnitude and sign of changes in mean carbon emissions between movers' origin and destination, and the duration between mover 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 θ 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 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.

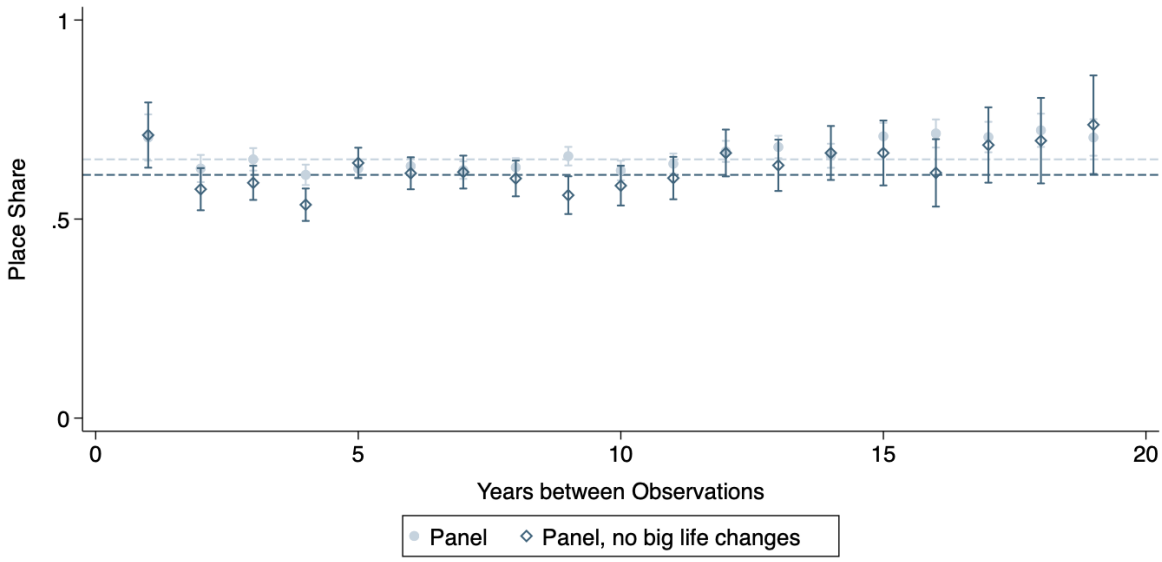
Figure 5 shows tract-level estimates by duration between observations, with all demographic and household controls. This exercise makes it possible to evaluate two possible sources of bias in the model. First, it provides evidence on the extent to which estimates of place effects may be biased by life-cycle patterns of energy and CO_2 demand. If the 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 observed 15 years apart than those observed 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.

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



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.

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.

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.[19]

5.2 Variance 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 more likely attributable to just local built environment and public amenities, 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.

19. 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.

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
SD(ψ_j)	0.23	0.16	0.17	0.24	0.27	0.21	0.21
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	0.097		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
SD(ψ_j)	0.23	0.18	0.18		0.27	0.22	0.22
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.

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 the transit and zoning examples I've highlighted throughout the paper, such policies could include regulatory efforts 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 highway expansions.[20]

To account for time variation in place effects that differs from national trends, I also estimate time-varying place effects ψ_{jt} at the CBSA level.[21] 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 the KSS 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 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 bias. 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 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.

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

21. 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.

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 ACS data 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 the outcome variables are constructed from a combination of survey responses and local external data. Specifically, 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.2.

How do these results inform the interpretation of the event study decomposition? There are two things to note. First, recall that the event study estimates are unbiased only if heterogeneity in the share parameter is uncorrelated with observed and unobserved household characteristics. I've shown evidence that my event study estimates are stable several observable dimensions of heterogeneity in the data. I also showed in the KSS decomposition that the covariance between unobserved components of heterogeneity are very close to zero – the largest correlation coefficient across the four baseline estimates (CBSA vs. tract & panel vs. mover) is 0.06. Together, this suggests that 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 ψ_{low} and one with ψ_{high}, and identical populations across the two places. If there is high variation in carbon emissions across populations and a small difference between ψ_{low} and ψ_{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% in the KSS estimation, about half of which is attributable to climate and electric grid intensity. 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

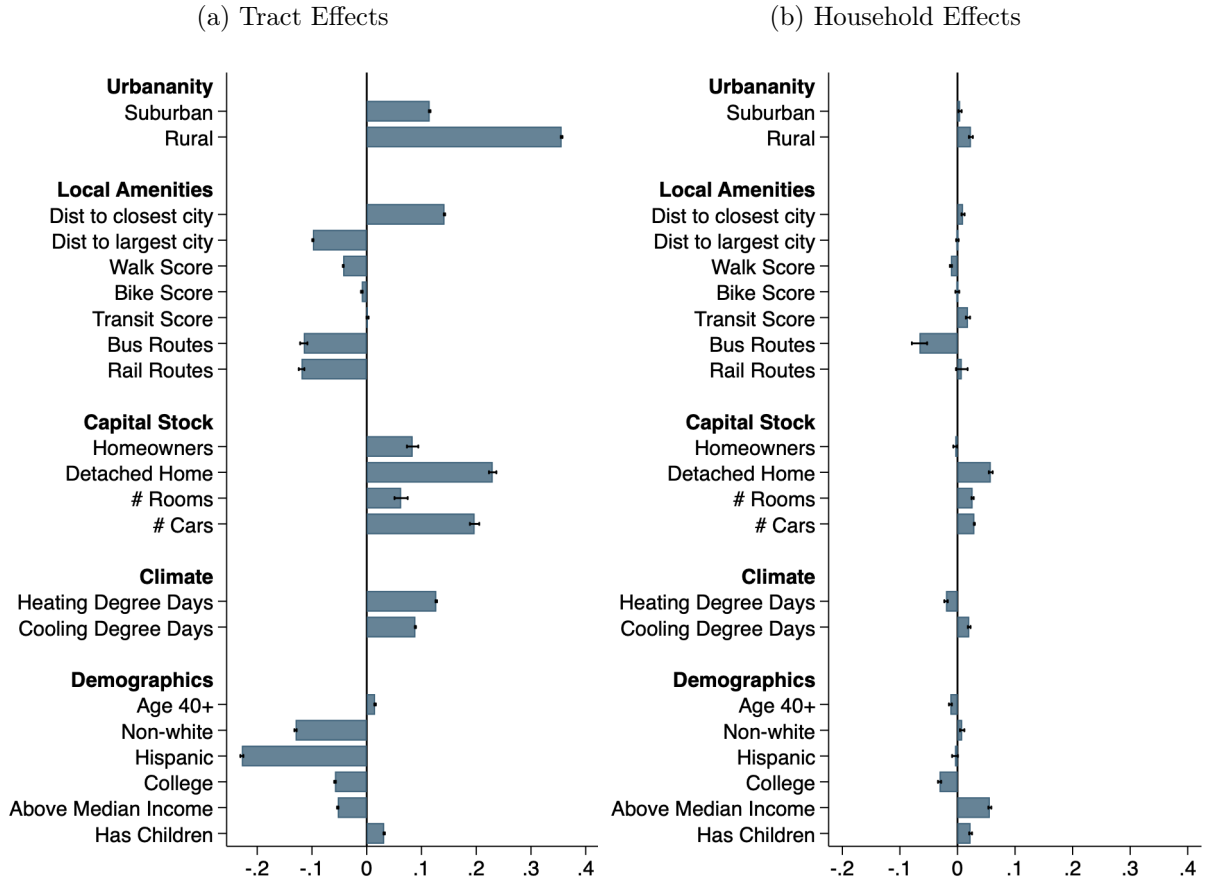
As an additional specification test, in Appendix Figure A.6, I show binned scatter plots similar to the one presented for event study results (Figure 4), but now with deciles of changes in estimated place effects, rather than observational means, on the x axis. I plot these against two sets of changes in household mean outcomes: changes for the full mover sample, and changes in the sample restricted to only households 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 proceed to characterizing 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 local amenities that could contribute to differences in average household energy demand and energy demand elasticities. For example, in the residential energy sector, larger homes tend to use more energy, as do single-family homes; there is a strong relationship between carbon emissions and density, though it is potentially not monotonic because of the effect of density on micro-climates (e.g. through heat island effects); and parks, plants, and green surface coverage are all negatively correlated with energy use (see e.g. Ko 2013, for a review). In the transportation sector, car use is lower in places with better public transportation, less parking, and more directly connected roads (e.g. Transportation Research Board 2009; Barrington-Leigh and Millard-Ball 2017), and mechanically, people drive fewer miles when they live closer to where they work, shop, and spend their leisure time.

Figure 6: **Correlates of Unobserved Heterogeneity**



Note: This figure presents estimates from separate multivariate ordinary least squares regressions of neighborhood effects and household effects on: 1) urbanity indicators, 2) local amenities and density, 3) capital stock characteristics, 4) climate and electric grid intensity, and 5) 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. Regressions are weighted using ACS sample weights. See text for additional details.

In Figure 6, I show the results from ten regressions projecting 1) place and 2) household effects estimated in the KSS mover sample onto four sets of energy-relevant place characteristics and a set of demographic characteristics. Urbanity and local amenities cover aspects of urban form that households effectively take as given. These include 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. Characteristics in the capital stock category are ones that reflect household choices among the set of options available in a place, which are determined at least in part by local zoning regulations or other policies. 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. For the tract effect estimation, capital stock variables reflect tract-level means; for the household effect they reflect the household's own choice. Climate is measured using annual heating degree days and cooling degree days in NOAA subdivisions. As with the capital stock regression, demographic variables reflect tract means in the projection

of tract effects, and household values in the projection of household effects. I have excluded the coefficient estimates for density (which is included in the "local amenities" regression) and electric grid intensity (which is included in the regression with climate variables) from the figure, as I estimate correlation coefficients between these variables and tract effects that are larger (in absolute value) than one, so their inclusion makes the other values more difficult to see.[22] All non-dummy amenities 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 are largely consist with observational data and conclusions in the urban planning literature. Suburban and rural tracts are higher emissions than urban tracts. Tracts that are closer to a city, those with more rail and bus routes within a half mile of people's homes, and those with better bike and walk scores have lower emissions. Somewhat unintuitively, I estimate that tracts that are closer to the largest city within their CBSA have higher emissions. One possible explanation for this correlation is that households from suburbs just outside the largest city may have longer commutes due to congestion. Tracts with a larger share of homeowners and detached single family homes, a larger average home size, and more cars per household are much higher emissions. Consistent with what is well-documented in the energy literature, tracts with more weather extremes are higher emissions. Finally, looking at the projection of place effects onto demographics, I find that non-white, Hispanic, college-educated, and below-median income households are more likely to live in low emissions neighborhoods, while households with children and older households are more likely to live in high emissions places.

I find much weaker relationships between person effects and observable place amenities, which is consistent with minimal sorting on unobserved household characteristics that I estimate in KSS. To highlight just a few patterns: high emissions households are a little more likely to live in rural areas, and they're less likely to live in places near a bus route. They're also more likely to live in a detached single family home, have a larger house, and have more cars. Finally, as expected households with higher income and children have higher potential carbon emissions, while college educated and younger households tend to have lower potential carbon emissions. Additional results on the correlates between observable person and observable place characteristics are presented in Appendix Table A.6.

7 Implications for Aggregate Carbon Emissions

Together, the wide distribution of place effects and the strong relationship between place effects and observable place characteristics imply that there may be an opportunity to substantially reduce household carbon emissions from residential and transportation energy use through what I refer to as place-based climate policies – i.e. policies that aim to reduce household carbon emissions by changing the characteristics of the places that people live in. These include a broad set of interventions to the built environment of places, which it is conceptually useful to split

22. I estimate a coefficient of roughly 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 -11 on log density, suggesting a very strong association between density and place effects.

into two categories: 1) changes in zoning and land-use regulations, and 2) direct investments into infrastructure and local public goods.

In the first category, zoning deregulation and transit-oriented development have especially gained traction in the US in recent years. 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, and in 2022, California state legislature passed AB 2097, which eliminates most parking minimums. At the Federal level, President Biden's original infrastructure bill proposal in March 2021 included grants to cities that got rid of exclusionary zoning. While this portion of the bill did not get passed, and many states and municipalities are still debating but not implementing zoning reforms, these examples illustrate the relevance of such approaches in the current policy debate.

The second category is dominated by investments into transportation infrastructure. While adding and expanding public transit networks is perhaps the most obvious case, many cities have been undertaking efforts that are less costly, and that involve converting existing space rather than making new capital investments. For example, in Paris, Plan Velo has converted over 200 miles of road into (often protected) bike lanes so far, with plans to continue to extend the network through 2026 ("Un nouveau plan ve'lo pour une ville 100% cyclable" 2021). Barcelona has a plan to repurpose more than half of streets currently devoted to cars to mixed-use public space called super blocks, or Superillas (Roberts 2019). In the US, the COVID-19 pandemic gave rise to "slow streets" or "open streets" in many US cities – programs which closed certain streets to car traffic in order to give pedestrians and cyclists more space to socially distance. In some places, there are ongoing efforts to make these initially temporary programs permanent.

To understand the potential effects of place-based climate policies, I examine how household carbon emissions would change in response to changes in the national distribution of place effects. This paper does not identify a causal relationship between any specific amenity and place effects, or any specific intervention and place effects, but it has shown evidence that low emissions places are low emissions not just due to sorting of low emissions people to those places, and that low emissions places tend to have amenities that are characteristic of more urban neighborhoods. Using this as motivation, I look at several counterfactual distributions of place effects that are meant to reflect different patterns of urbanization.

I account for the fact that place effects are noisily estimated by using linear Empirical Bayes, i.e. a shrinkage estimator. When estimating counterfactual emissions, I use forecasted place effects, which 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. This approach yields a lower bound on the potential aggregate reductions from different distribution shifts, as it attenuates place effect estimates.

I begin by examining how carbon emissions would change in a counterfactual scenario in which the US limits suburban sprawl and increases the share of households within a region that live in an urban neighborhood. To approximate this, I estimate how emissions would change if every household lived in a neighborhood with the place effect of the principal city of the CBSA they are in (or are closest to). A naive comparison of household emissions shows that, on average, households living in principal cities emit 27% less than households living in surrounding areas (20.6 tons vs. 15.0 tons). Accounting for the difference in potential carbon emissions of households who have chosen to live in different types of neighborhoods, I estimate that if every tract had the place effect of the principal city in its CBSA, annual household emissions would go down by on average 13%, from 20.6 to 17.9 tons a year.

The majority of cities in the US, even the majority of principal cities, would not be considered particularly urban on the global scale. To consider the potential effect of deeper urbanization, in the second and third scenarios I examine how household carbon emissions would change if more people lived in places like New York, which is the most populous CBSA in the country, and whose principal city, Manhattan, is uniquely dense, walkable, and transit oriented within the US context. Specifically, I estimate how emissions would change if households living in the nine other largest CBSAs in the US (see Table A.8 for the full list) lived somewhere with the average place effect of 1) the New York CBSA or 2) Manhattan.

A naive comparison shows that household emissions from residential energy and commuting are about 8% lower in New York (17.9 tons, annually, per household as compared to 19.2) than in the nine other largest CBSAs in the US. However, I find that assigning the average New York CBSA place effect to each of these other areas actually increases average household emissions slightly, to 19.3 tons per household. This result is consistent with New Yorkers having lower person effects than the average resident of these other CBSAs, and the New York suburbs having higher place effects than other suburbs. In contrast, if each principal city of the other top nine CBSAs 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 (the average household emissions in Manhattan) – some of Manhattan's low emissions can be explained by household sorting – but it still implies that Manhattan's place effect is significantly lower than the effect of the other 9 largest cities in the US, on average.

The results presented in this section lend 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. My results imply that the potential for urbanization-induced reductions to carbon emissions is overstated when comparing observational means across places because people choose where to live based in part on their potential carbon emissions. Nevertheless, because there is substantial variation in household carbon emissions and neighborhood effects explain 15% or more of this variation, they also imply that even relatively small shifts in the distribution of place effects, e.g. through mild urbanization policy, could meaningfully decrease carbon emissions.

8 Discussion

This paper is the first to estimate the causal effects of places on household carbon emissions and decompose spatial heterogeneity in carbon emissions into a component driven by these place effects and a component driven by household characteristics and sorting. I find that roughly 15-25% of overall 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 accounting for 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 there is potential for meaningful reductions in carbon emissions from place-based climate policies that make the distribution of place effects across the US more urban.

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 and transportation options across the US – 75% of residential land in the US is zoned for single family homes only (Badger and Bui 2019), 95% of commuters in my sample commute by car, and there is only one high speed rail line in the entire country, which operates at high speed over only roughly 50 miles of track. This is in stark contrast with other parts of the world, in which cities tend to be denser and substantially less car oriented. Moreover, place effects are identified from movers, who differ from the general US population in meaningful ways. Thus, 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.

Establishing that place matters – and how much it matters – for household carbon emissions is a crucial first step to investigating the welfare impacts of specific place-based climate

policies. The welfare effects of a given intervention depend on several parameters whose estimation is outside of the scope of this paper. First, they depend on the causal relationships between local amenities and place effects, and on household preferences for local amenities. The correlations I presented between amenities and place effects do not 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; 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 important directions for future work. Second, the welfare impacts of place-based interventions would depend on the costs of implementing them relative to the cost of business as usual or other contender climate policies. Costs can vary dramatically for the same intervention in different settings (Goldwyn et al. 2022), making this estimation difficult, but incorporating cost estimates for a marginal value of public funds analysis (Hendren and Sprung-Keyser 2020) is another important avenue for future research. It is worth noting that because built environment is sticky, investments made in the present day have the potential to increase or decrease the costs of carbon mitigation efforts in the future. Finally, welfare impacts would depend on other externalities or agglomeration benefits of the intervention. For example, the types of interventions suggested in my paper could also impact local air pollution, congestion, traffic fatalities, and labor market productivity. These co-benefits and harms have been extensively studied in the environmental and urban economics literatures, and estimates could be incorporated into an aggregate welfare effect.

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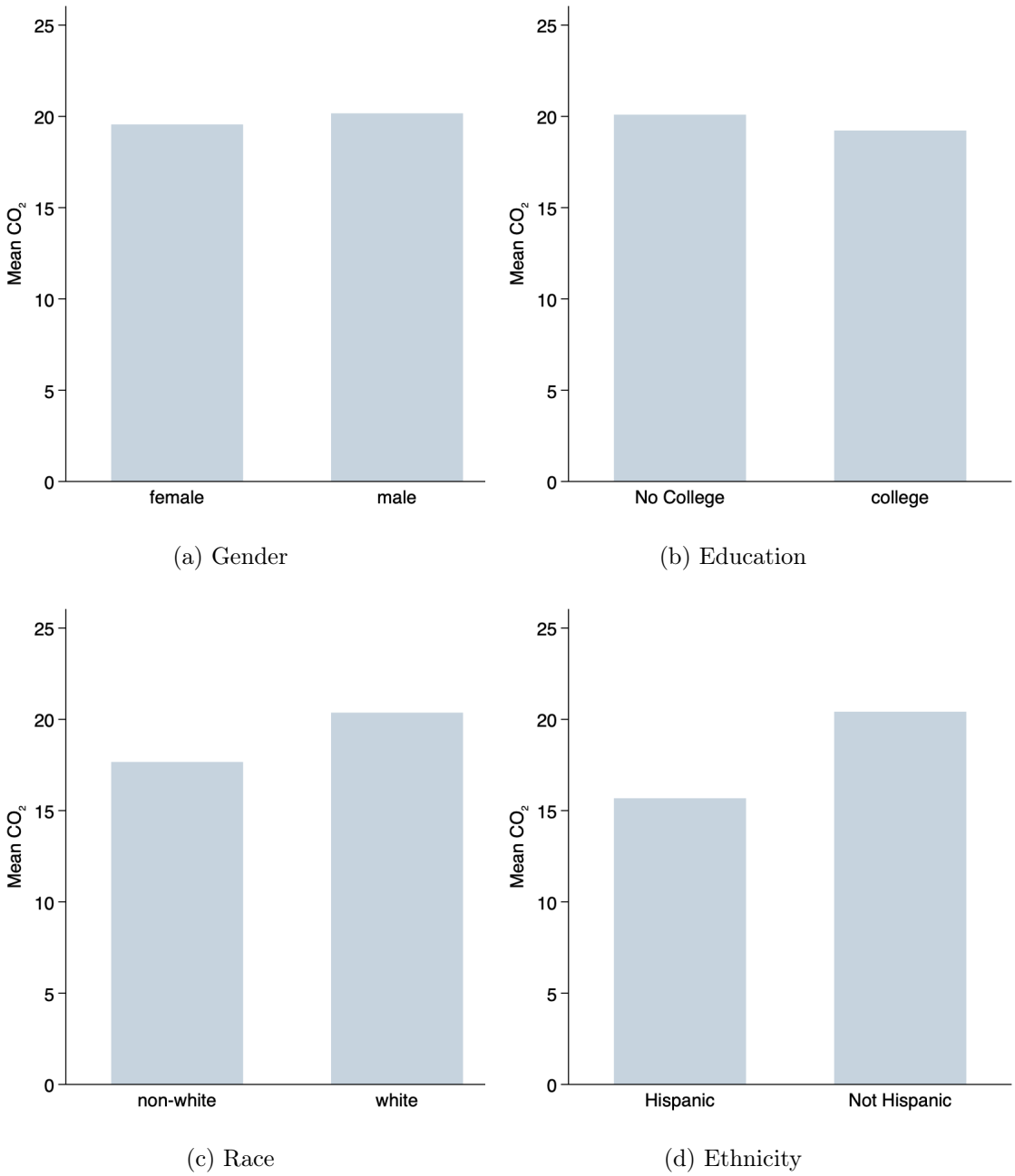
A Additional Figures and Tables

A.1 Additional Figures

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



(a) Gender

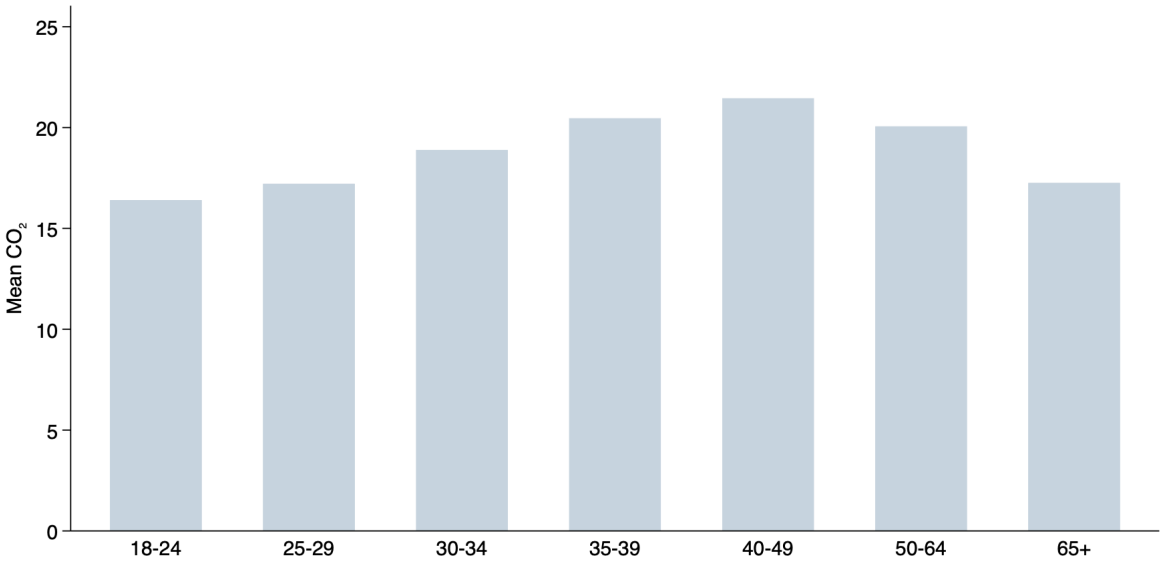
(b) Education

(c) Race

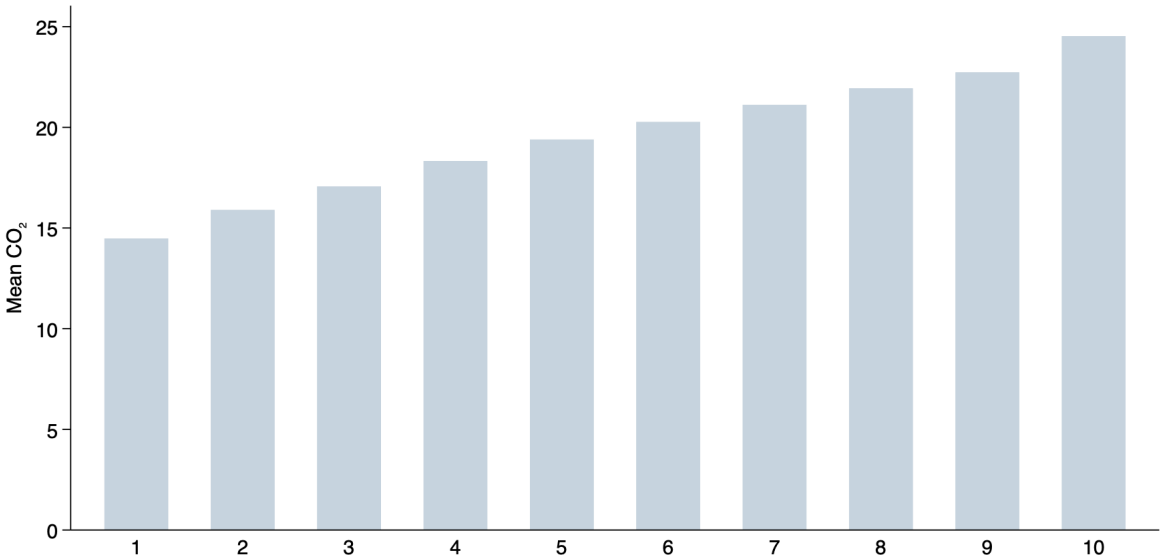
(d) Ethnicity

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)



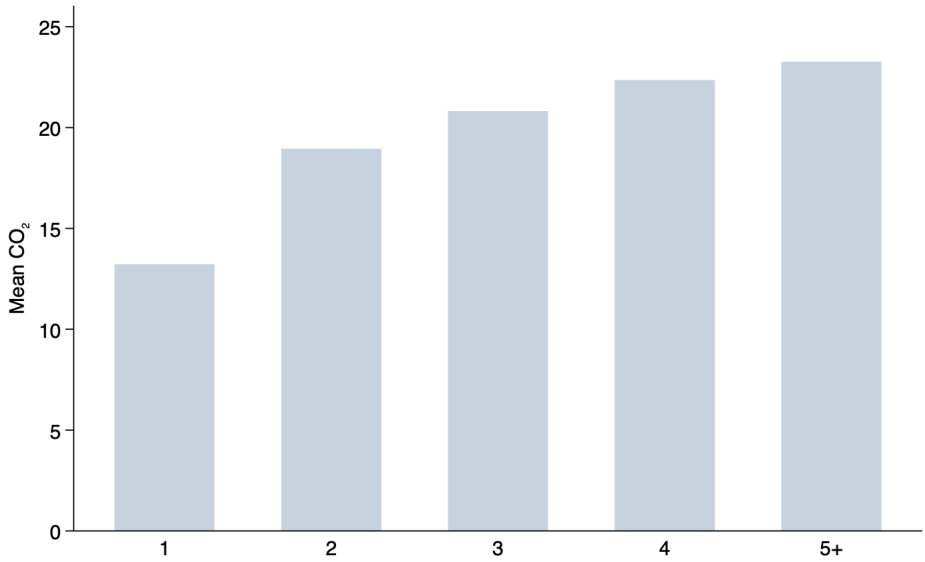
(a) Age



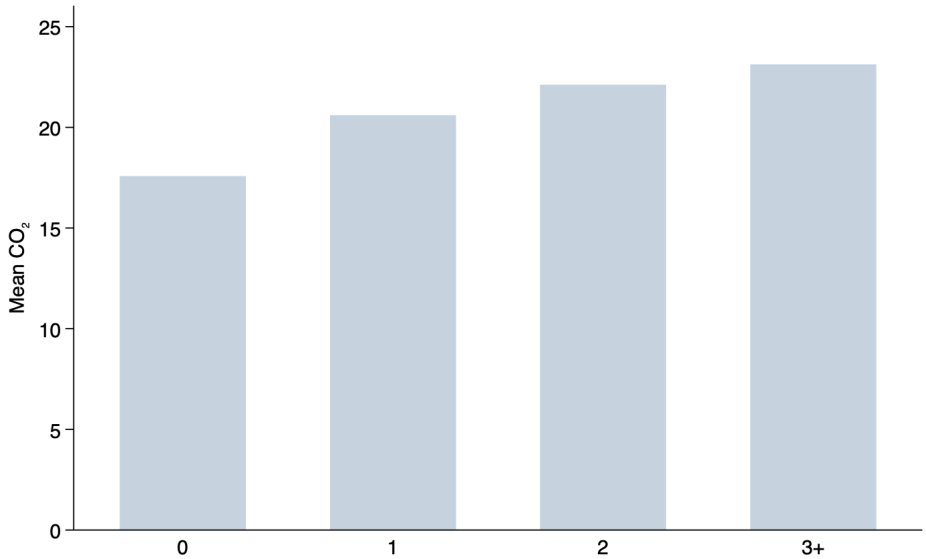
(b) Income Decile

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

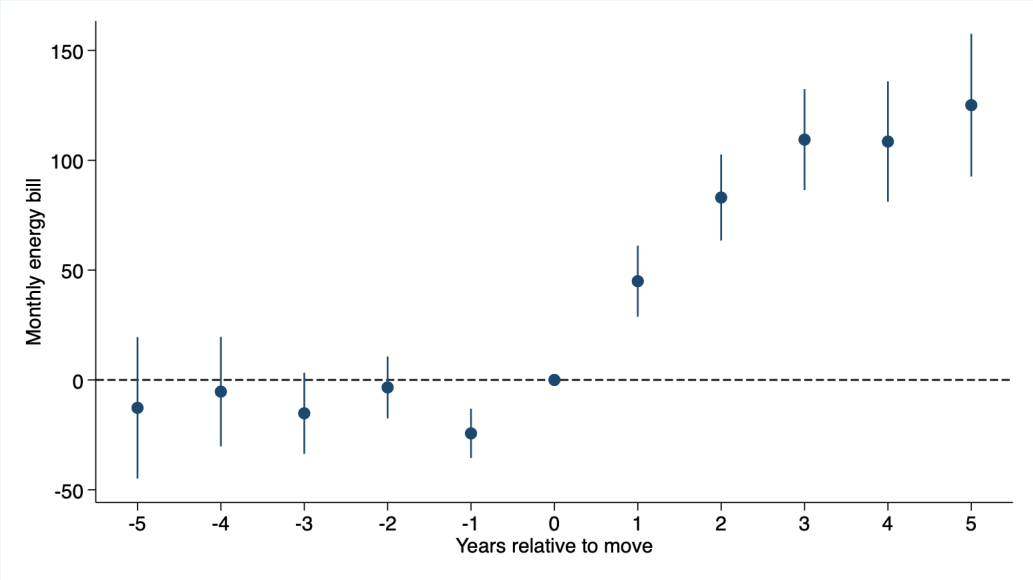


(b) Number of Kids

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.

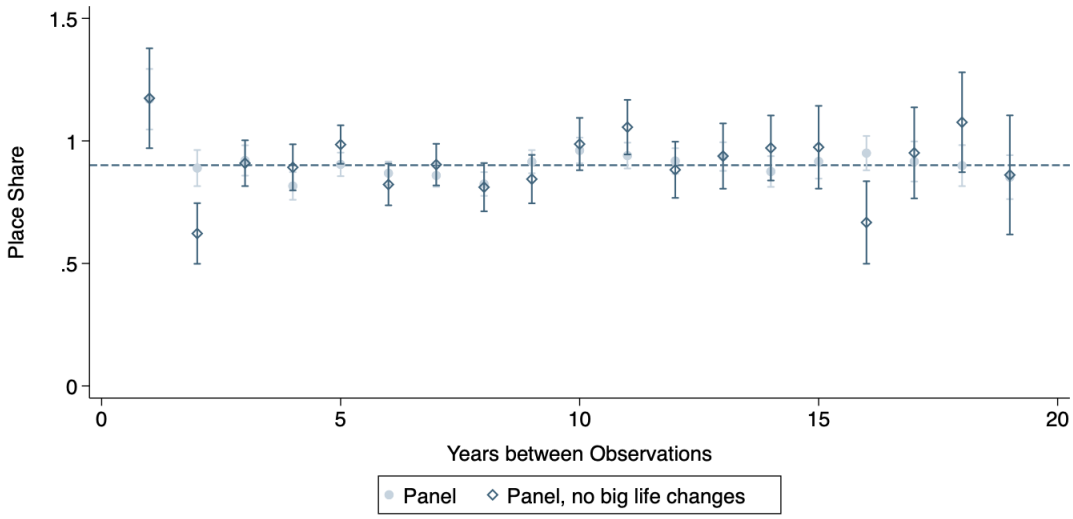
Figure A.4: Energy Expenditures in Mover Households in the PSID



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

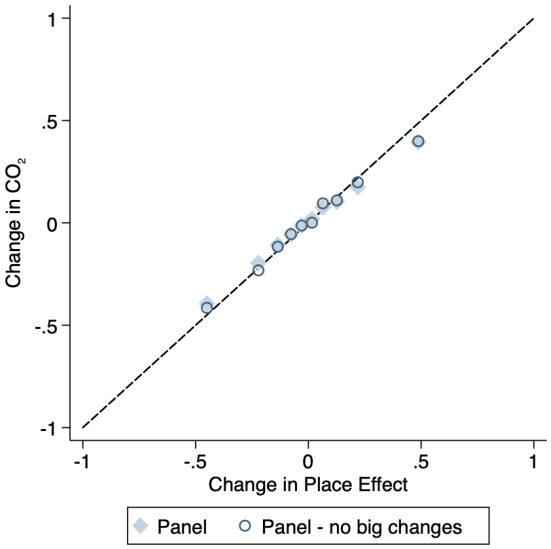
Figure A.5: **Event study by duration – CBSA**



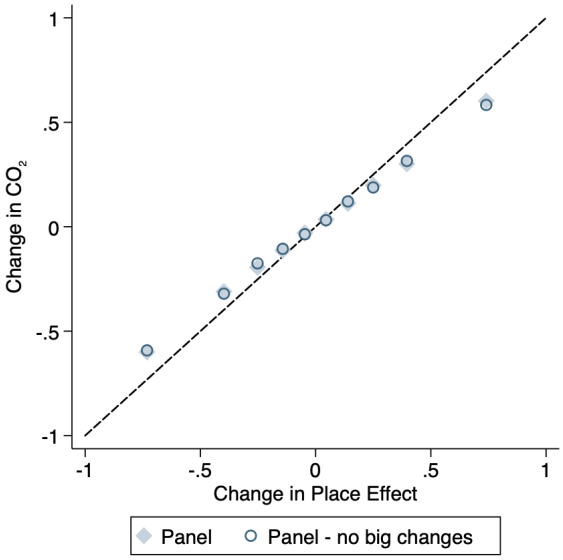
Note: This figure shows event study estimates of the share of spatial variation in mean carbon emissions that can be explained by place effects, by duration between mover observations. In other words, each coefficient is the estimate for place effects generated from the sub-sample of households that 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.

Go back to Section 2.5.

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)
Δ HH inc < -50%	0.035***	0.078***
	(0.0006)	(0.0010)
Δ 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.

Go back to Section 2.5.

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.

Go back to Section 2.5.

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

	CBSA				Tract			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Var(\log CO_{2ij})$	0.29	0.35	0.29	0.20	0.28	0.35	0.29	0.19
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
SD(ψ_j)	0.232	0.292	0.239	0.196	0.270	0.305	0.274	0.215
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
SD(ψ_j)	0.24	0.18	0.18	0.25	0.40	0.36	0.36
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
SD(ψ_j)	0.24	0.19	0.19		0.39	0.35	0.36
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.2 (Data Appendix).

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

	Above Median Inc. (1)	White (2)	College (3)	Has Kids (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. (1)	White (2)	College (3)	Has Kids (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 Additional Details on Variable Construction

- **Missing and imputed variables:** I follow Chetty and Hendren (2018) and Bailey et al. (2020) 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.

- **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 carpooling.

- **Commuting Distance**: 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.[23]

- **Number of annual commutes**: I estimate commuting days per week using reported hours worked last week and 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

23. This is the fastest speed a train ever goes in the US, along a small segment of the Northeast Corridor.

50-60 hours in a week, and 7 days if they worked more than that. I assume everyone commutes twice a day, and that commuting behavior is the same for all the weeks worked last year.

- **Vehicle fuel economy:** I assume individuals that commute by car or taxi do so in a vehicle with annual national average fuel economy, using data from the Federal Highway Administration (2019). For motorcycles, I scale mpg by 2 (US Department of Transportation 2015). This is a minor point as motorcycles account for only roughly 0.6% of vehicle miles driven (U.S. Environmental Protection Agency and Energy 2020). I also 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 (Agency 2021).
- **Emissions from public transportation:** I assign zero emissions to commutes by public transportation, walking, or biking.
- **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.
- **Building age**: I allow building age to be unknown in my analysis sample

Go back to Section 2 (Data).

B.2 Measurement Error in Household Carbon Emissions

There are several sources of measurement error in household carbon emissions from residential and transportation energy use. This could introduce bias in either estimates of household and place effects, estimates of the variance components, or both.

Note that if errors are random but serially correlated within a household, both a naive variance decomposition and a KSS variance decomposition on a sample consisting of stayers and movers will overstate the share of heterogeneity attributable to households; however, when I restrict to the mover only sample, the KSS correction accounts for serial correlation in the error term and gives unbiased estimates of variance components.

Below, I discuss sources of measurement error, as well as potential biases that arise in my estimates as a result.

Household reporting of residential energy expenditures:

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.

If household inattention is fixed it will be absorbed by the household effect. If inattention leads high types to overstate their expenditures, and low types to understate their expenditures, this would lead to an upward bias in the household component of heterogeneity and vice versa. It is also reasonable to think that inattention may be random but serially correlated within household.

With fixed or random inattention, estimates of place effects themselves are unbiased. However, if moves are correlated with changes in attention, this could lead to bias in estimates of place effects. For example, if households move after positive income shocks, and higher income households pay less attention to their energy bills, *and* this inattention leads to systematic under- or over-estimation of expenditures, estimates of place effects with more inattentive residents would be biased.

Seasonality is unlikely to bias my estimates because surveys are sent out randomly, and therefore the season households were surveyed shouldn't be correlated with other components of the model.

Electricity prices:

I estimate electricity prices from total utility revenues divided by total utility customers, by county. This introduces three sources of measurement error in electricity prices.

First, in counties served by more than one utility, I cannot match customers to the actual utility they are served by. If customers in an area can select their residential energy provider, this could lead to bias in the household component of heterogeneity. For example, if higher type customers are selecting into lower average price utilities, I will underestimate the household component of heterogeneity. Similarly, if there are several utilities serving different neighborhoods within the same county, this could lead to bias in the place component of heterogeneity. In particular, I will over-estimate consumption in neighborhoods served by more expensive utilities, and under-estimate consumption in neighborhoods served by cheaper utilities. If more expensive utilities generally serve lower consumption neighborhoods, this will lead me to underestimate the place component of heterogeneity.

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 (more so for households in high fixed charge service territories). 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 household variance component. Conversely, if prices are decreasing in consumption, I underestimate the household variance component. 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. Additionally, I do not observe if a household has solar, and 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 quantity consumed and in turn CO_2 from electricity purchased from the grid by these customers.

Electricity carbon emissions factors:

I estimate carbon emissions intensity of electricity using average emissions factors at the NERC subregion. This 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). The error in household carbon emissions that results from this is likely serially correlated within household, and can be accounted for in the mover-only KSS specification. However, if consumption profiles are also correlated with these patterns, my estimates of household carbon emissions will be biased. For example, if low type users consume more electricity when marginal emissions are higher then I would tend to under-estimate their carbon emissions and over-estimate the household component of heterogeneity.

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. Unlike electricity, fuel emissions factors for other fuels are the same regardless of where a household lives. However, in the case of natural gas a significant source of emissions is upstream methane leaks, which I don't consider in this analysis.

Assignment of heating fuel:

I estimate carbon emissions from fuel use by assigning all expenditures on "other home heating fuels" to the fuel reported as the primary fuel. If a household has non-zero other fuel expenditures, but they don't list a primary fuel, I impute their primary fuel based on the most commonly used primary fuel among other survey respondents in their state and year (out of residual oil, propane, and wood). If households use more than one heating fuel, or use a heating fuel other than the one I imputed for them, there will be error in my measurement of carbon

emissions, both as a result of dividing expenditures by the wrong fuel price, and as a result of assigning the wrong carbon emissions factor. I will overestimate household carbon emissions if reported or imputed fuel prices are lower than actual average fuel prices faced by the household, or if reported or imputed fuel types have higher emissions factors than the fuels actually used.

If I tend to overestimate carbon emissions from heating fuels for otherwise high-type households and underestimate carbon emissions from heating fuels for otherwise low-type households, then my household variance component will be biased upward, and vice versa. Moreover, if moves are correlated with shocks to unobserved fuel components, this could lead to bias in my estimates of place effects. For example, if a household uses the same heating fuel everywhere they live but doesn't report this fuel, if they move to a place where their neighbors use an on average higher emissions heating fuel, I would overestimate the place effect. In practice, the share of households reporting non-zero energy expenditures on heating other than electricity or natural gas is small, and my estimates are not meaningfully affected when I exclude other heating from the calculation.

Commuting Distances:

Because I estimate commute miles from geodesic distances between coordinates, I will underestimate speed and miles travelled for individuals who have less direct commutes. If the directness of a commute is the result of place-based constraints (e.g. the result of living in a gated community or a neighborhood with many winding roads and cul-de-sacs), and if these types of neighborhoods tend to be farther from employment centers and have longer commutes to begin with, then I will underestimate the place component of spatial heterogeneity.

Additionally, I impute miles for the people for whom I don't observe census block of work using average mph for home and place of work county pairs. This will lead me to overstate commute distances for people with slower than average commutes, and understate commute distance for people with faster than average commutes. If faster than average commutes are also longer than average, then I will underestimate the person component of spatial heterogeneity. My estimates are not sensitive to using a simpler measure of commute distance, calculated from simply dividing reported commute time by the average national commute speed, 32 mph (Federal Highway Administration 2019), suggesting that errors in commute speeds are unlikely to bias my estimates.

Total Commuting Miles:

I use weeks worked last year to estimate total commuting from typical commuting behavior last week. This assumes that 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. It is likely that such errors are more likely to arise for lower income households with less job stability, but it is unlikely that it results in a systematic over- or under-estimate of

commute miles on average.

Commuting Emissions:

I assume everyone drives a vehicle with the annual national average fuel economy, using data from the NHTS. This is a significant oversimplification, as it ignores patterns of heterogeneity in fuel economy both across commute lengths and across regions. If people with longer commutes 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 solely 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 zero emissions to public transit over-states heterogeneity across transit vs. car commuters.

Finally, if households change their mode of transit over the year, or if they use multiple modes of transit in a single commute, I do not capture this variation. For example, if households report taking public transit as their primary mode, but in reality they drive part of the distance of their commute, I will under-estimate their carbon emissions and overstate overall heterogeneity.

Non-Commuting Transportation Emissions:

I don't observe transportation other than commuting. In particular, I I don't observe local travel for errands or leisure, nor do I observe airplane travel. Thus, I (weakly) underestimate carbon emissions magnitudes. If commuting is a rank-preserving share of total transportation emissions, my results will be qualitatively correct but off in magnitudes. However, if for example places with long commutes have lower other transportation emissions (because everybody spends leisure time in their back yard) whereas places with short commutes have higher other transportation emissions (because people go away for the weekend), then my estimates cannot be used to infer anything about transportation emissions overall.

Go back to Section 2 (Data).
Go back to Section 5 (Results).

C The Leave-One-Out Connected Set

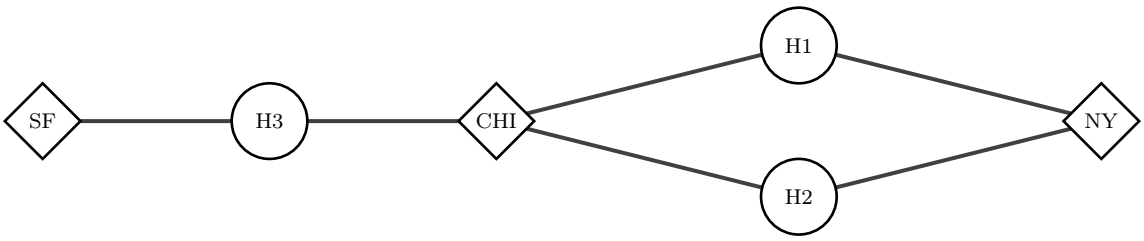
Consider the following data:

Individual & Household Geographic Locations

Year	Household	Place
1	1	NY
2	1	CHI
1	2	CHI
2	2	NY
1	3	SF
2	3	CHI

Household 1 moves from NY to Chicago, household 2 moves from Chicago to NY, and household 3 moves from San Francisco to Chicago. This data can be visualized as a network, where each place is a node, each household is a node, and edges connect households to each place they've lived in.

Household + Place Network



In this figure, San Francisco, Chicago, and New York are all connected by movers – this is a connected set. The leave-out connected set is the set of places that remains connected after dropping any household from the data. In this example, San Francisco is *not* in the leave-out connected set, because it is only connected to the rest of the network through H3.

Go back to subsubsection 2.4.1.

D 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 σ_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.