The Role of People vs. Places in Individual Carbon Emissions

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Abstract

There is substantial spatial heterogeneity in household carbon emissions. I leverage movers in two decades of administrative Decennial Census and American Community Survey data to estimate place effects – the amount by which carbon emissions change for the same household living in different places – for almost 1,000 cities and roughly 61,500 neighborhoods across the US. The results indicate that place effects explain 14-23% of overall heterogeneity. Decreasing neighborhood-level place effects from one standard deviation above the mean to one standard deviation below would decrease household carbon emissions from residential energy use and commuting by about 40%. *JEL Codes:* H41, Q40, R20.

*U.S. Census Bureau. eva.lyubich@census.gov. Any opinions and conclusions expressed herein are those of the author(s) and do not represent the views of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this data product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release (Release authorization numbers: CBDRB-FY24-CES014-016 and CBDRB-FY24-CES014-017). I am grateful to Reed Walker, Pat Kline, Emmanuel Saez, and Joe Shapiro for their guidance and support throughout this work. This project has also benefited tremendously from comments and suggestions from Alan Auerbach, Matilde Bombardini, Stephanie Bonds, Severin Borenstein, David Card, Lucas Davis, Karl Dunkle Werner, Ben Faber, Meredith Fowlie, Cecile Gaubert, Kendall Houghton, Hilary Hoynes, Jon Kolstad, Enrico Moretti, Andrés Rodríguez-Clare, Raffaele Saggio, Jim Sallee, Elif Tasar, Danny Yagan, Katherine Wagner, Randall Walsh, Chris Walters, and seminar participants at UC Berkeley. I am grateful to NSF GRFP (DGE 1752814), the Berkeley Opportunity Lab and Smith Richardson Foundation, and Washington Center for Equitable Growth for financial support. I thank Angela Andrus for her patience and help in navigating disclosure review.

1 Introduction

Increased carbon and other greenhouse gas emissions since the onset of the industrial revolution have caused global average temperatures to rise by over 1C (1.8F) relative to preindustrial levels (NASA 2020). In 2015, the US signed the Paris Accord, a global agreement aimed at mitigating the potential damages from climate change by limiting overall warming to below 2C. In search of opportunities for decarbonization, researchers and policymakers have pointed to substantial spatial heterogeneity in household carbon emissions, suggesting that perhaps higher-emissions places could adopt features of lower-emissions places, such as density and high-quality public transportation infrastructure, in order to lower household carbon emissions (e.g. Jones and Kammen 2014a; International Energy Agency 2021; Pomponi et al. 2021; Wagner 2021).

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 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 61,500 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 2014b; 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 just a little under 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 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. Unobserved household characteristics could include things like preferences for spending time in a large back yard versus a public park, intrinsic risk tolerances for biking versus driving, a strong distaste for being cold, or simply being the type of household that makes an effort to reduce the emissions that result from its choices. Place effects could result from a variety of local amenities and supply-side factors that determine patterns of household energy use. They could reflect aspects of urban form such as public transportation, bike and pedestrian infrastructure, highway networks, density, 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. I show how place effects can be interpreted through the lens 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 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. For intuition, consider the following thought experiment. Imagine two households that are identical in every observable way - same size, same income, same education levels, etc. One lives in a dense, urban neighborhood well-served by public transit, while the other lives in a car-dependent suburb with large homes. The urban household likely has lower carbon emissions. But is that because the place itself determines emissions, or because the urban household has different unobservable characteristics, like stronger environmental preferences, that led them to select into the lower emissions neighborhood? If I observe the same household move between neighborhoods, any resulting change in that household's emissions can no longer be attributed to fixed unobserved household preferences or characteristics, and I can use those changes to infer differences in place effects. In order for estimates from the mover design to be unbiased, the central assumption is that mover destinations are uncorrelated with changes to unobserved determinants of household carbon emissions. 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 and confound estimates of place effects. In other words, the identifying strategy does not allow for households to move in response to a sudden shift in environmental preferences, but it does allow for households to move in response to changes in income, the the number of children, and lifecycle changes in preferences that are captured by age, because I observe these characteristics.

For the first set of results, 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. Intuitively, if spatial heterogeneity is driven in part by underlying differences between places, when a household moves, I should see their emissions shift towards the mean of their new location. The larger the shift, the more important the role of place is. I find that, on average, when households move to a new city, their carbon emissions change by about 85% of the mean difference between their origin and destination cities. As one would expect, sorting plays a larger role in neighborhood-level variation than in variation across cities, but the role of place remains meaningful; I estimate that when households move to a new neighborhood, their carbon emissions change by about 55-60% of the mean difference between their origin and destination neighborhoods.

I explore heterogeneity in the estimated share parameter along several dimensions. My estimates remain stable when restricting the sample to households without significant changes in observable characteristics and when grouping movers based on the magnitude of differences in mean emissions between their origin and destination places. Estimated shares are also symmetric for moves to lower versus higher-emissions places. Finally, I find minimal evidence of drift in the share parameter when splitting the sample by duration between observations. To interpret the event study shares as unbiased causal estimates of the effect of any household moving between any pair of places, it is necessary to impose an additional assumption – that there is no systematic sorting of households. The above heterogeneity analyses suggest that perhaps bias from this restriction is limited, but it is nevertheless a strong assumption, particularly at the neighborhood level. However, even under the weaker baseline assumptions, the event study estimates are informative: they yield unbiased predictions about how household carbon emissions change for any set of observed moves. This makes it possible to calculate the carbon emissions externality of policies or regulatory restrictions that drive existing patterns of household migration, for instance, policies that restrict housing supply in on-average lower emissions cities.

In my second set of results, I first estimate the full non-parametric distribution of household and place effects using a two-way fixed effects model, and then do a variance decomposition to estimate the share of overall heterogeneity explained by each of these components. This approach allows for unrestricted patterns of sorting, but the weaker identifying assumptions it affords come at the cost of limited mobility bias – naive estimates of variance components are upward biased because some place effects are estimated from a small number of 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 sorting on unobserved household characteristics 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 23% of overall heterogeneity. Over half of the city share is accounted for by climate, electricity emissions factors, or energy prices, but at the neighborhood level, controlling for these elements in order to try to isolate the dimensions of place that more likely reflect urban form decreases the place share by less than half, to about 15%. While this leaves the majority of 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 went 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 41%.

I characterize low and high-emissions neighborhoods by presenting correlations between estimated tract effects and observable tract-level characteristics. I use observable characteristics from within the Census Bureau microdata, 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] These data lend unique insight into highly granular variation in neighborhood characteristics. I find that the correlations between amenities and neighborhood place effects for the most part mirror the relationships in the observational data. Low-emissions places have clean electricity and mild climates, and they have amenities that are characteristic of urban areas. Among the local amenities, density, proximity to principal cities, and the quality of local bike infrastructure appear to have the most explanatory power.

I conclude my analysis by examining how household carbon emissions would differ if some households were exposed to place effects of more urban neighborhoods than they currently live in. If suburban and rural households lived in a place with the average place effect of the principal city closest to them – a scenario that captures in spirit how households' exposure to place effects might shift in response to regulations that limit urban sprawl and encourage up-zoning and infill development – I estimate that their emissions from residential energy use and commuting would decrease by about 15%. 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 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] Under deeper urbanization, if households in the nine other largest cities in the US lived instead in a city with the average place effect of Manhattan, I estimate their carbon emissions would decline by on average 60%.

In many ways, the basic physical design and urban fabric of cities, suburbs, and towns creates the foundational patterns of transportation and residential energy use. These features are shaped by decades of housing, transportation, and land use policies. The wide distribution of place effects estimated in this paper implies that there may be potential for "place-based climate policies" – i.e. policies that aim to reduce household carbon emissions from residential and transportation energy by changing the underlying characteristics of the places people live in – to lead to meaningful reductions in carbon emissions. While this paper does not estimate the causal drivers of place effects, the correlational analysis presented here, and many observational and model based studies (e.g. Shammin et al. 2010; Timmons, Zirogiannis, and Lutz 2016; Ribeiro, Rybski, and Kropp 2019; Pomponi et al. 2021; Ko 2013) provide some hypotheses. Estimating causal relationships between specific amenities and place effects using credible exogenous variation is an important direction for future research. One might have worried about whether the settings in which such studies can be done are too selected; what if the households who live

1. www.walkscore.com

2. Data provided by Redfin Real Estate.

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).

in places that make changes to local infrastructure or regulatory restrictions are different from households in places that do not make those changes? The core results of this paper suggest that a meaningful share of variation in emissions between places is driven by variation in place effects, mitigating some of these concerns about external validity.

This paper makes several contributions. A large body of work in labor and urban economics finds significant wage, employment, and productivity benefits from density and integrated land use and transit policies (Tsivanidis 2019; Allen and Arkolakis 2022; Duranton and Puga 2020). These studies suggest that spatial equilibria are inefficient due to agglomeration economies and other externalities, but have largely not considered the carbon emissions externality into their analysis. The theoretical justification for using place-based policies in cases where agglomeration economies and other local externalities exist is well-established. Federal intervention can correct inefficient market equilibria and improve welfare by supplementing local government provision of under-provided amenities, fostering agglomeration externalities, offering a "big push" towards an optimal equilibrium when several exist, intervening to restrict the growth of productive areas, or insuring residents against place-based shocks (Glaeser and Gottlieb 2008; Kline 2010; Kline and Moretti 2014; Glaeser 2013; Austin, Glaeser, and Summers 2018). However, the empirical evidence on the efficacy of place based policies is mixed. While some studies find that tax incentives targeting areas with lower employment can improve welfare (Busso, Gregory, and Kline 2013; Austin, Glaeser, and Summers 2018; Bilal 2021), others suggest that spatial policies promoting growth in less developed areas may have negligible or even negative aggregate effects on productivity and welfare (Kline and Moretti 2013; Gaubert 2018). Duranton and Venebles (2018) highlight the challenges in evaluating place-based policies, including in the context of urban transport, housing, and infrastructure. The welfare implications of place-based climate policies would crucially depend on their design, implementation details, costs, and household preferences for local amenities; a welfare analysis is beyond the scope of this paper. Carbon emissions are a canonical example of a global externality, but many of the theoretical justifications for place-based policy outlined above could be relevant, given the observed relationship between carbon emissions and factors of urban form such as density and urban transport. This, together with evidence from this paper that places play an important role in driving household carbon emissions, suggests that that further research is warranted.

Methodologically, I build on a growing literature in labor examining wage inequality across firms, and a growing literature 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 2017; Card, Rothstein, and Yi 2023). My paper is the first to use a mover design to study household energy use and carbon emissions, yielding new insights into spatial heterogeneity in these outcomes. Previous work has highlighted the consequences of spatial heterogeneity in carbon emissions for allocative efficiency (Glaeser and Kahn 2010a; Colas and Morehouse 2022) as well as for distributional impacts and political economy of hypothetical climate policies (Cronin, Fullerton, and Sexton 2019; Sallee 2019; Green and Knittel 2020), but these papers did not examine the causal role of places in their findings. Several papers in the literature have generated estimates of heterogeneous energy demand parameters, but they 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). The estimates generated in this paper on the relative roles of place effects versus household sorting could assist in resolving some of the challenges identified in the environmental economics literature around distributional impacts and political economy. For instance, if lock-in of urban form limits the share of household carbon emissions that could be targeted by pricing instruments in the short-to-medium term, estimates of the share of spatial heterogeneity driven by place effects could inform decisions around how much to redistribute carbon dividends when using geography as a tag. And as highlighted previously, they raise an important question as to whether place-based climate policies could be a welfare-improving complement to traditional instruments for addressing this global externality.

2 Data and Stylized Facts about Carbon Emissions in the US

My primary sources of data for estimating household emissions from these sectors are the administrative Decennial Census and American Community Survey. In the remainder of this section, I discuss the construction of relevant analysis variables, and then provide some descriptive evidence on heterogeneity in carbon emissions in my sample.

2.1 Data and Key Variables

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 2000 Decennial Census long form is a stratified random sample covering one in six households in the US. After 2000, the ACS replaced the Decennial Census long form in order to gather detailed information on individuals and households more regularly. The ACS is a stratified random sample covering roughly 0.4% of households in 2001-2005, and roughly 1% of households in each year after 2005 (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 the 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.

4. Neither the Decennial Census nor the ACS ask respondents for their social security number. Wagner and Layne (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 Bond et al. (2014) for detailed discussion of the assignment algorithm used by the Census. There is some variation in assignment success 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.

2.1.1 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]

2.1.2 Carbon Emissions

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

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 (2021a) Emissions & Generation Resource Integrated Database. For natural gas and other home heating fuels, I obtain average retail prices at the state level from the Energy Information Administration (2020b) State Energy Data System. If a household reports non-zero expenditures on "other home heating fuels", I impute the fuel used from their answer to the question "What was the primary fuel used for home heating?" Finally, I obtain fuel emissions factors from the U.S. 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,

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, combining blocks in cases where they correspond to a single 2000 block.

number of commutes, and mode of transit.[6] 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 an emissions factor based on their mode of transit and data from the National Transit Database (see Appendix A.1 for more details). I examine the sensitivity of my results to using the Federal Highway Administration (2019) National Household Travel Survey (NHTS) to predict fuel economy and non-commute miles from household and geographic characteristics available in both the Census and NHTS. This is not my baseline approach, as it infers how much of variation in vehicle fleets and fuel economy observed in the NHTS is driven by individual preferences vs. place-based factors from cross-sectional variation.[7]

2.1.3 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, number of children, and homeowner status.

I also observe whether a household lives in a detached single family home, the number of rooms in a home, and the number of vehicles in the household. These characteristics are intermediate outcomes, which directly affect carbon emissions. They also very likely reflect a combination of household preferences and place characteristics – many places impose restrictions on multi-unit homes and/or minimum lot sizes, and lack transportation options for households without a car, so household choices are likely to differ from place to place depending on these constraints. Therefore, I do not treat these variables as observable household characteristics when estimating place and household effects, but use them later to explore correlates of unobserved place and household heterogeneity.

It is not obvious whether home ownership should be considered an observable household characteristics or part of a place effect. Homeownership rates vary dramatically across CBSAs (Raetz 2021; Mateyka and Mazur 2021), and housing regulations can price people out of

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 (U.S. Energy Information Administration 2020). Commuting accounts for about 28% of all vehicle-miles travelled, and 39% of person-miles travelled on transit systems (U.S. Department of Transportation 2015), which means I underestimate CO_2 emissions from overall personal vehicle use for most people in my sample.

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

homeownership. In these cases, treating homeownership as an intermediate outcome seems appropriate. On the other hand, the choice to become a home owner simultaneously impacts where people live and factors related to their carbon emissions. For instance, homeowners may want extra space for potential family expansion or space-intensive leisure activities, or may choose homeownership in order to be able to install solar panels and have a place to charge their electric vehicle. To be conservative and err on the side of finding a smaller role of place effects, I treat homeownership as an observable characteristic in my baseline analysis.

2.1.4 Place Characteristics and Amenities

I supplement Census micro-data with 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 US Climate Division level (National Oceanic and Atmospheric Administration 2020). The National Oceanic and Atmospheric Administration (NOAA) divides the contiguous states into a total of 344 Climate Divisions based on regional differences in climate within state. Degree days are computed as the annual sum of the daily difference between that day's temperature and 65°F, 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.[8] Walk Score® rankings capture proximity to different commercial amenities such as grocery stores, as well as street characteristics such as block lengths and intersection widths. Bike Score™ indices capture characteristics that make biking more or less accessible, such as the existence of bike lanes, road connectivity, and hilliness. Transit Score® ratings capture proximity to different types of transit, and the frequency and connectivity of nearby options. For transit, I also observe the number of bus routes and rail routes within a half mile. 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.

Finally, I estimate density at the tract-level using 2010 Census block-level information on area and population. I define urban tracts as those that are characterized as urban by the Census *and* surpass the density threshold set for urban centers by the EU-OECD definition of a functional urban area (Dijkstra, Poelman, and Veneri 2019).[9] I define suburban tracts as those contained within a CBSA but not designated as urban. Tracts outside of CBSAs are classified rural.

8. Data provided by Redfin Real Estate.
9. This threshold is 1,500 people per square kilometer.

2.1.5 Sample Restrictions

I restrict the analysis to individuals who are at least 18 years of age, who are not identified as the householder's child or grandchild, and who are not missing any of the outcome variables or key explanatory or control variables described above. I also impose several additional restrictions related to energy variables. I exclude from the sample individuals belonging to households whose residential energy costs are included in rent, or whose gas costs are included in their electricity bill, because I don't observe expenditures in those cases. I discuss this subsample of individuals and the potential impact of its exclusion in detail in Appendix A. 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 almost 17 million people across over 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.[10] This restriction ensures that I am assigning residential energy expenditures to the correct location. The panel sample consists of 1,097,000 people across 916,000 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.[11] 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 B 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 12% of (disproportionately rural) tracts, yielding approximately a 5% reduction in the number of households in the sample (Table 1, Columns (3) and (4)).

10. 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.

11. 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.

CBSA movers are households in the CBSA panel that live in different CBSAs across observations (93,000 households, Column (5)), and similarly, tract movers are households in the tract panel that live in different tracts (within or across CBSAs) across observations (248,000 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, and other household level characteristics are taken as averages over person characteristics. All estimates are weighted using Census sample weights.

2.2 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, households in the panel are on average more likely to be white, have higher income, and are more likely to be homeowners than households in 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 7 percentage points less likely to live in an urban tract, 9 percentage points more likely to live in a detached home, and 2 percentage points 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 college educated, and have higher income 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, a little under 80% of household carbon emissions in my sample are from residential energy, and a little over 20% are from commuting.[12] 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.

12. Household carbon emissions from residential and transportation energy are roughly evenly split (U.S. Energy Information Administration 2020). Given that commuting makes up about 30% of transportation energy emissions, we would expect a slightly higher than 3:1 ratio of residential to commuting energy. My estimates appear to overstate residential energy use relative to commuting by a few percentage points at most.

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	0.48	0.48	0.48	0.45	0.47
Household Income	103,400	114,100	114,200	114,900	116,100	115,400
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
Homeowner	0.75	0.85	0.85	0.85	0.72	0.73
B: Outcomes						
Tons CO_2	15.1	16.1	16.0	16.1	15.0	15.0
Tons CO_2 - Residential	11.9	12.8	12.8	12.8	11.8	11.9
Tons CO_2 - Commute	3.3	3.3	3.3	3.3	3.3	3.1
C: Intermediate Outcomes						
Detached Home	0.72	0.81	0.81	0.81	0.73	0.73
Use Electricity Only	0.30	0.24	0.24	0.24	0.30	0.28
Commute by Car	0.94	0.96	0.96	0.96	0.95	0.96
Commute Minutes	25.1	24.9	24.9	24.8	26.0	25.6
D: Place Characteristics						
Urban	0.29	0.22	0.23	0.22	0.20	0.25
Suburban	0.62	0.62	0.63	0.63	0.68	0.67
Rural	0.09	0.15	0.15	0.15	0.11	0.08
Walk Score	27.2	23.0	23.1	22.4	23.0	25.2
Bike Score	35.9	33.4	33.4	33.1	34.1	35.4
Transit Score	9.0	6.7	6.7	6.3	6.7	7.9
N Bus Routes	1.6	1.1	1.1	1.0	1.2	1.3
N Rail Routes	0.15	0.09	0.09	0.07	0.10	0.10
Cooling Degree Days	1,359	1,213	1,215	1,205	1,356	1,335
Heating Degree Days	4,376	4,824	4,815	4,851	4,494	4,518
N People	16,900,000	1,097,000	1,073,000	1,042,000	107,000	290,000
N Households	12,600,000	916,000	860,000	833,000	93,000	248,000
CBSAs	1,000	1,000	1,000	1,000	1,000	1,000
Tracts	71,500	70,000	70,000	61,500	54,500	61,500

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. Sample 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	0.10	0.10	0.16	0.14
Years Between Observations	7.9	7.9	10.3	9.8
B: Demographic Characteristics				
Age First Observed	42.0	42.0	37.1	37.2
Share with Large Change in Income	0.28	0.28	0.45	0.41
Share with Change in N Kids	0.45	0.45	0.55	0.55
Change in N Kids	-0.12	-0.12	0.08	0.08
Share Rent to Own	0.11	0.11	0.26	0.26
C: Mover Place Changes				
Δ Walk Score			-7.0	-7.1
Δ Bike Score			-4.2	-4.0
Δ Transit Score			-2.3	-2.7
Δ N Bus Routes			-0.57	-0.56
Δ N Rail Routes			-0.04	-0.04
Δ Tract Share Detached Home			0.05	0.05
% Moves Urban-to-Urban			0.08	0.14
% Moves Urban-to-Suburban			0.17	0.14
% Moves Suburban-to-Suburban			0.44	0.46
%Δ Cooling Degree Days			214	136
%Δ Heating Degree Days			-331	-188
N People	1,073,000	1,042,000	107,000	290,000
N Households	860,000	833,000	93,000	248,000
CBSAs	1,000	1,000	1,000	1,000
Tracts	70,000	61,500	54,500	61,500

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. Sample counts are unweighted and rounded according to Census Bureau disclosure rules.

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, experience a large change in real household income[13], or go from

13. I define a large change in income as a greater than 0.5 (in absolute value) change in log income. This approximates a 50% increase/decrease in income, and corresponds to about the top quarter in absolute income change in my panel sample.

renting to owning their home. 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. 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 F.3, Table F.4, and Table F.5 respectively.

2.3 Observational Heterogeneity

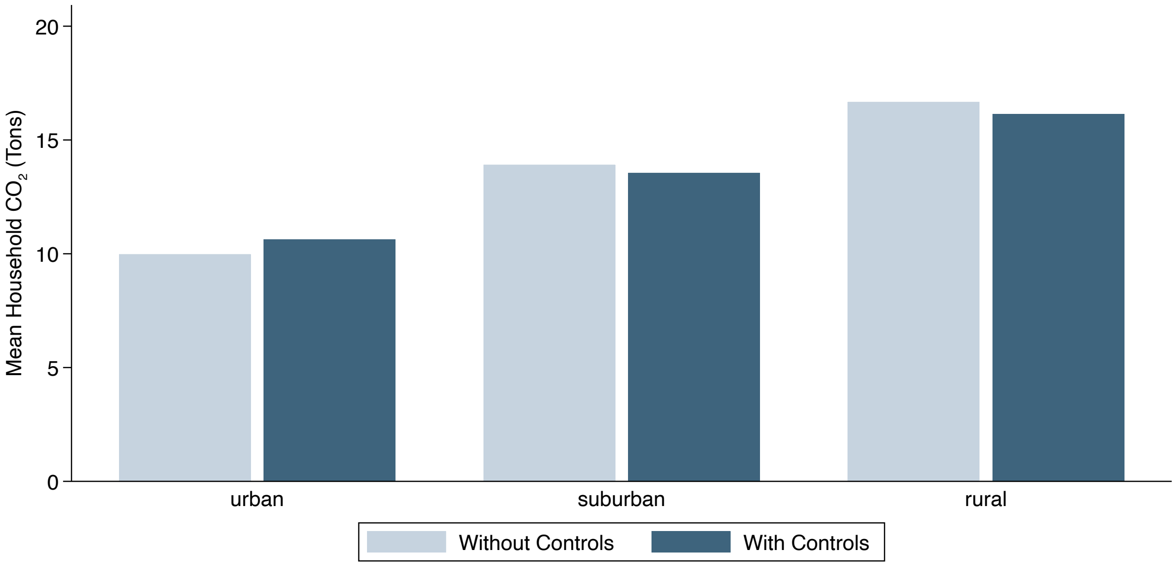
Carbon emissions from residential energy and commuting vary immensely across individuals in the full sample (Figure F.1). This variation is strongly correlated with both geographic and household attributes. For example, it is well understood that at the regional level, carbon emissions vary with climate and with the emissions intensity of local fuel sources. At the more local level, many have observed a relationship between emissions and local amenities that characterize urban form, such as local public transit, bike infrastructure, green space, density, etc.(Ou et al. 2013; Philips, Anable, and Chatterton 2022).

Figure 1, presents variation in household carbon emissions across urban, suburban, and rural neighborhoods. Households residing in suburban and rural areas have substantially higher emissions compared to those living in urban areas. These differences could in theory be attributed to sorting based on observable household characteristics such as college education, race and ethnicity, household income, or number of children, among others (Figure F.2). However, controlling for observable household characteristics only marginally reduces the differences between rural, suburban, and urban areas. While there is some sorting of higher-emissions households to suburban and rural areas and lower-emissions households to urban areas, after accounting for these differences, households in suburban tracts still emit over 20% more per year than observationally similar households in urban tracts, while households in rural tracts emit about 50% more.

To examine spatial heterogeneity across CBSAs and tracts in more detail, for each geography, I estimate unconditional and conditional place means, μ_j, using an ordinary least squares regression of log of household CO_2 onto place fixed effects, year fixed effects τ_t, and in the conditional regression, individual and household observable characteristics X_{it}.

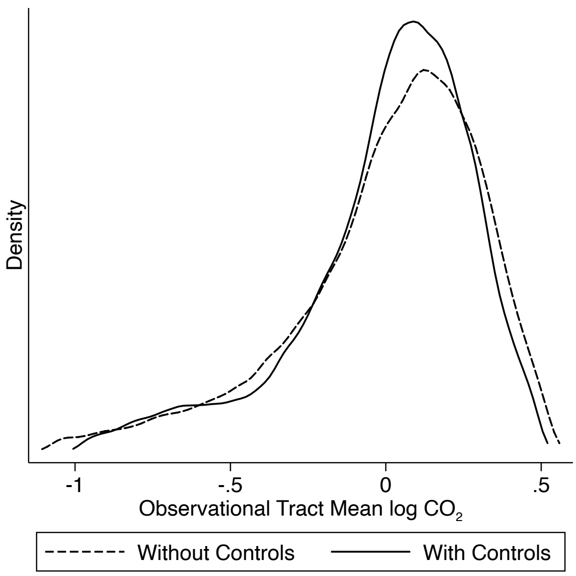
$$\ln CO_{2it} = \mu_{j(i,t)} + X_{it}\beta + \tau_t + \varepsilon_{it} \tag{1}$$

Figure 1: **Household Carbon Emissions in Urban, Suburban, and Rural Places**



Note: This figure shows estimates of household carbon emissions by urbanity, derived from the regression of log carbon emissions (in metric tons) onto indicators for whether a tract is urban, suburban, or rural and year fixed effects in the case of the specification with no controls, and additionally onto age, gender, race, education, household size, number of children, and homeowner status, in the case of the regression with controls. Places are defined as urban according to the definition in Dijkstra, Poelman, and Veneri 2019. They are defined as suburban if they are not designated as urban but are contained within a CBSA. Rural areas are tracts outside of CBSAs. The unconditional regression has an R^2 of 11% and the conditional regression has an R^2 of 29%.

Figure 2: **Heterogeneity in Tract-Level Carbon Emissions**



Note: This figure shows Kernel Density estimates, using a Gaussian kernel function, of tract-level average household carbon emissions. The dotted gray line labeled “Without Controls” corresponds to the distribution of log CO_2 conditional on year FEs only, and has a standard deviation of 0.33, while the solid line labeled "With Controls" conditions on observable household characteristics, and has a standard deviation of 0.30. Both distributions are de-meaned to match the model with controls and censored at the top and bottom 1% of observations in order to abide by Census Disclosure Avoidance rules. Observable characteristics include age, gender, race, ethnicity, education, home owner status, household income, household size, and number of children.

I use an Empirical Bayes "shrinkage estimator" to adjust the estimates for statistical noise (see Appendix C for details). In practice, the distributions and relevant moments are almost identical for the adjusted and unadjusted estimates. Figure 2 presents the adjusted conditional and unconditional distributions of $\hat{\mu_j}$. Defining a high-emissions neighborhood as one standard deviation above the mean and a low-emissions neighborhood as one standard deviation below the mean, I estimate that households in high-emissions neighborhoods emit approximately 1.9 times more than those in low-emissions neighborhoods, or 1.8 times more after accounting for differences in observable characteristics. For the remainder of this paper, I refer to means conditional on observable characteristics as "observational means", following the terminology used by Abaluck et al. (2021).

The remaining heterogeneity in observational means reflects some combination of place-based characteristics and unobserved household characteristics. To illustrate this, I rewrite Equation 1 as a two-way fixed effects model

$$\ln CO_{2it} = \alpha_i + \psi_j + X_{it}\beta + \tau_t + \varepsilon_{it} \tag{2}$$

where ψ_j represents the place-based characteristics and α_i represents the unobserved, fixed, household characteristics. Comparing Equation 1 and Equation 2 highlights the bias that can arise when inferring place effects from observational means: $\mu_j = \psi_j + E[\alpha_i | i \in j]$. In other words, observational means reflect a combination of place effects and an average over the unobserved characteristics of the residents of a place.

The primary objective of this paper is to disentangle these components and quantify the extent to which heterogeneity in household carbon emissions is driven by unobserved household characteristics and 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 fixed. This motivates the mover design, which leverages this exact variation.

3 Model

In the previous section, I demonstrated how a two-way fixed effects model can be used to mitigate biases that arise when making inferences based on observational means. To provide further insight into the model, I now illustrate how it relates to a standard way of modeling consumer energy demand, and discuss the interpretation of place and household effects.

Consider a household, i, living in place, j, that consumes quantity Q of energy 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]

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. As electric vehicles (EVs) become a

Average demand a_j, price elasticities of demand ρ_j^f, and prices P_j^f are permitted to vary by place. Place-based differences in average energy demand and in price elasticities of demand could stem from a range of fixed and malleable characteristics of places. These characteristics include climate, local public goods and urban form (e.g. density, public transit, pedestrian and bike infrastructure, proximity to highways and availability of parking, and proximity to leisure and commercial amenities), and regulatory characteristics (e.g. zoning restrictions that change the size and density of homes, and building codes that change energy efficiency requirements or eliminate natural gas hook ups). All of these features could potentially shift energy demand, both on average and in slope.

In addition to place-based characteristics, demand also depends on observable fixed and time varying household characteristics (such as age, household size, and income) X_{it}, individual fixed unobserved determinants of demand (such as a person's intrinsic risk tolerance for biking on shared roads, aversion to public transit vs. traffic, or relative enjoyment of spending time in their own back yard vs. a public park) α_i, individual time-varying unobserved determinants of demand (perhaps, an increased willingness to bike after reading the latest Intergovernmental Panel on Climate Change report) ε_{it}, and national annual trends τ_t. Taken together, household demand for residential and transportation energy is given by:

$$\ln Q_{it} = a_j + \sum_{f \in \mathcal{F}} \rho_j^f \cdot \ln P_j^f + X_{it}\beta + \tau_t + \alpha_i + \varepsilon_{it} \tag{3}$$

If all households within a place used the same proportion of fuel types, it would be simple to express the above equation, with log carbon emissions as the outcome, in terms of a place-based average emissions factor $\bar{\phi}_j$, and to in turn rewrite that expression as the two-way fixed effects model defined in the previous section:

$$\begin{aligned} \ln CO_{2_{it}} &= \ln(\bar{\phi}_j Q_{it}) \\ &= \underbrace{\ln \bar{\phi}_j + a_j + \sum_{f \in \mathcal{F}} \rho_j^f \cdot \ln P_j^f}_{\psi_j} + X_{it}\beta + \tau_t + \alpha_i + \varepsilon_{it} \end{aligned}$$

Thus, in this simplified setting, place effects ψ_j capture the combination of place-based variation in five key factors: average energy demand, fuel price elasticities, fuel prices, electricity emissions factors, and fuel shares. In reality, fuel shares vary not only across places but also across households. For instance, some states and municipalities are attempting to ban natural gas connections for new construction (O'Brien 2023; Cornfield 2023), but even if natural gas is available as an option, some households may still opt to fully electrify their home, while others may have a strong preference for cooking on gas stoves.

In Appendix D, I examine how the interpretation of the two-way fixed effects model is affected when fuel shares are allowed to vary across individuals. The more complex model includes a term that captures the interaction between a household's fuel shares relative to the place-based average and fuel emissions factors. Because electricity emissions factors vary across places,

larger share of the market, observing household vehicle choices, and perhaps the place-based role of EV charging networks on these choices, will be critical for future research.

a household that disproportionately uses electricity will experience a larger drop in emissions when moving from a place with relatively high emissions electricity to a place with relatively clean electricity, compared to the average household. This lack of separability implies that there is some inherent mis-specification in the two-way fixed effects model in this setting.

The model makes two additional simplifications. First, place effects are assumed to be fixed, implying that any time variation, including changes in prices, is absorbed in the place effects, which reflect average differences between places over the sample time frame. Second, while the model allows price elasticities of demand to vary across places, it does not allow for them to systematically vary across households. For example, the model does not allow for different elasticities between high and low-income households. Allowing for heterogeneity in demand elasticities across household characteristics would introduce additional interaction terms in the error, as elasticities would be interacted with place-specific prices. Together, this interaction along with the interaction between household fuel shares and place-specific electricity emissions factors, motivate treating the errors as heteroskedastic.

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 impose constraints on minimum home sizes and density. Then there would be households who live in high-emissions places for work, or to be near family or near other amenities they enjoy, who would like to make lower-emissions residential and transportation choices but are unable to. If those households move from an on average high-emissions place to an on average low-emissions place where lower-emissions alternatives are now available to them, their carbon emissions should decrease. 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 use the mover design to estimate two versions of a heterogeneity decomposition of household carbon emissions. I begin with an event study, which, under strong assumptions characterizes the share of differences *between* places attributable to place effects, inferred from movers' changes in emissions relative to origin-destination mean differences. Under weaker assumptions, the event study serves as additional descriptive evidence and model validation for the second decomposition. I then estimate the full, non-parametric distribution of household and place effects and decompose *overall* heterogeneity into variance components. In the following subsections, I first discuss modeling and identifying assumptions, and then I describe each of these decompositions in more detail.

4.1 Main Assumptions

My empirical strategy at its foundation pairs a two way fixed effect model with a mover design. In order for estimates from this approach to be unbiased, three assumptions need to hold: (1) additive separability of place effects, or constant effects, (2) non-persistence of outcomes, and (3) exogenous mobility, or conditional orthogonality. I discuss each below.

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 household and place effects. Expressing the outcome in logs is both statistically and conceptually appealing. Statistically, it reflects the approximate log-normality of the household carbon emissions distribution (Figure F.1). Conceptually, this specification implies that place effects increase and decrease carbon emissions proportionally by the same amount for everyone. In other words, if a relatively low emissions household and a relatively high emissions household move from a high emissions place to a low emissions place, their emissions will decrease by the same percentage. This is an intuitive way to conceptualize many 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 all households, regardless of their baseline energy consumption. If place effects are driven by density, it is reasonable to expect denser places 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 all households. Similarly, an increase in transportation alternatives to cars might decrease the share of trips taken by car for all households proportionally. The model assumes that a relatively high emissions household will continue to have relatively high emissions even if they move to low emissions place – as long as place effects don't explain 100% of spatial variation – but that their emissions will decrease proportionally to the difference in their origin and destination place effects as a result of some of the mechanisms highlighted above.

Nevertheless, the two-way fixed effects model imposes a substantial restriction: it does not allow for heterogeneous treatment effects or match effects. Heterogeneous place effects could arise if, for example, place effects are due to a public transit option that only low emissions households use but doesn't change the behavior of high emissions households, or if all households use the public transit option but low emissions households get rid of their car and eliminate all car trips while high emissions households eliminate only a share of car trips. Alternatively, heterogeneous place effects might arise if low emissions households make the same consumption choices wherever they live, but high emissions households respond strongly to places with particularly low- or high-emissions amenities.

Moreover, I already showed in Section 3 that there is some mis-specification built into the model, as there is an interaction between individual fuel shares and place-specific emissions factors – a person who prefers electricity to natural gas for 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. 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.

To see why testing for symmetry provides evidence on the existence of selection on heterogeneous effects, consider differences in potential outcomes across an origin o and destination d, allowing now for there to be an interaction $\eta(\alpha_i, \psi_j)$ between person and place types:

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

Therefore, as long as:

$$\eta(\alpha_h, \psi_d) - \eta(\alpha_h, \psi_o) \neq \eta(\alpha_l, \psi_d) - \eta(\alpha_l, \psi_o) \tag{4}$$

this type of interaction, paired with selection, would lead to asymmetries between changes in household carbon emissions for moves to higher on-average places vs. to lower on-average places. Note that this condition holds for a broad class of functions, including the simplest interaction, $\eta = \alpha_i \cdot \psi_j$, since $\alpha_i \cdot (\psi_d - \psi_o) \neq \alpha_j \cdot (\psi_d - \psi_o)\ \forall i \neq j, o \neq d$.[15] Intuitively, returning to one of the examples above, suppose place effects are due to a public transit option that only low emissions households use but that doesn't change the behavior of high emissions households. Suppose also, that the households who want to use public transit disproportionately move to places where it is available, whereas the households who don't want to use public transit disproportionately move to places where it is not available, then the decline in emissions for households moving from a high emissions place to a low emissions place will be larger than the increase in emissions for households moving in the opposite direction.

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 four quartiles and vice versa, as well as moves within first quartile places and moves within fourth quartile places as bounds in gray.

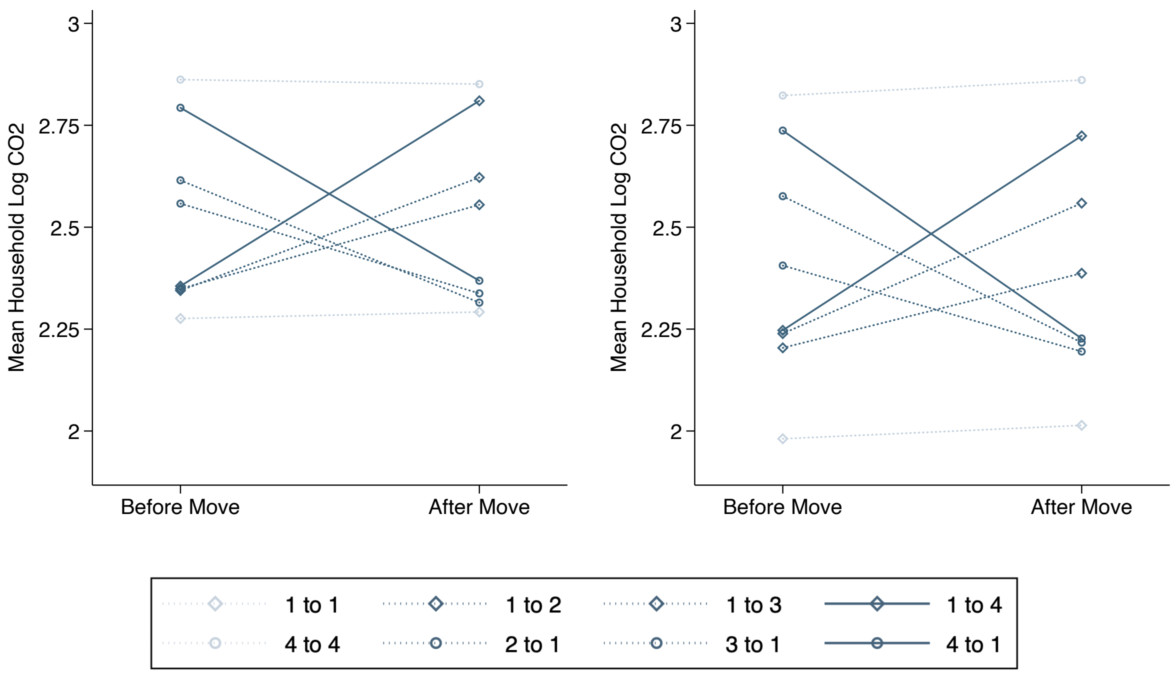
First and foremost, this figure shows that moves across quartiles lead to equal and opposite changes in household carbon emissions. This suggests that the log-linear two-way fixed effect model of household carbon emissions is a good approximation for the role of place effects and alleviates concern about selection on heterogeneous treatment effects. Second, this figure provide evidence of selection at the tails, particularly for tract-level moves: households that move from the lowest quartile to a different place in the lowest quartile have lower emissions on average

15. More generally, Equation 4 implies that $\frac{\partial \eta}{\partial \alpha}[\psi_d - \psi_o] \neq 0$, and in turn, $\frac{\partial^2 \eta}{\partial \alpha \partial \psi} \neq 0$. In words, the symmetry check rules out any interaction whose change between places grows or shrinks with household type.

than households that move between the lowest quartile and any of the three higher quartiles. Similarly, households that move between places in the fourth quartile have higher emissions than those that move between the fourth and the first, though the difference at the top is less pronounced. Finally, it is worth noting that households that move between tracts within the same quartile experience a small increase in emissions, which is consistent with the general trend of households moving to on average higher emissions places.

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, which means that it cannot differ from household to household as a result of differences in the history of places $\{j\}$ they lived in prior. In other words, the expected change for two households moving between the same origin and destination should be the same even if one of the households previously lived in Houston while the other previously lived in New York. Note, however, that the non-persistent outcomes assumption does *not* rule out that the place someone was born and raised 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.

If the first two assumptions hold, it means that the model is a reasonable approximation to the real world, and then random variation in exposure to place would identify place effects. Thus the final, identifying, assumption that is necessary for this empirical approach to be unbiased is that moves are conditionally exogenous; in other words, household destination choices are not related to changes in unobserved determinants of carbon emissions.

$$E[\epsilon_{it}|\alpha_i, \psi_{j(i,t)}, X_{it}, \tau_t] = 0 \tag{5}$$

It is important to emphasize here that that the two-way fixed effects model allows for a broad set of sorting behaviors. First, it allows for unrestricted sorting of households on fixed or time-varying observable characteristics. A key advantage of the Census microdata is the ability to observe numerous time-varying household characteristics that could potentially bias the estimates if unobserved. Factors such as entering middle age, having children, experiencing a change in income, or becoming a home owner are all associated with an increase in energy consumption generally (Figure F.2), and the last three of these also significantly increase the probability of moving (Table F.4). However, this endogeneity does not bias the estimates, as I observe age, household size, number of children, household income, and home-owner status, and am therefore able to separate the effect of these characteristics on household emissions from the influence of a household's choice of new city or neighborhood.

Second, and crucially, the two-way fixed effect model allows for unrestricted sorting on fixed unobservable characteristics. In other words, if households have heterogeneous, but fixed, preferences for neighborhood amenities – for instance, if someone has 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 by this selection because these unobserved but fixed determinants of carbon emissions are captured by household fixed effects. The ability to account for, and allow for unrestricted sorting on, these time invariant unobserved preferences is a critical benefit of the pairing of the two way fixed effect model and mover estimation strategy.

Note also that the fact that I might never observe a very high emissions household move to a very low emissions place is not a problem if you believe Assumption 1, that places do not have heterogeneous effects on different types of households. It is sufficient to have a connected network of pairwise moves between places in order to identify relative place effects, and household effects are identified relative to other households within the same place. Consequently, even if I observe only lower emissions households moving from Pittsburgh to New York, and only higher emissions households moving from Pittsburgh to Houston, a comparison of their emissions within

Pittsburgh combined with the observed changes to their emissions at their destinations identify the relevant household and place effects. I have already provided some evidence that place effects do not appear to be heterogeneous in the symmetry check shown in Figure 3. I will provide additional evidence on this when I examine heterogeneity in the event study results, in Section 5.1. If this model assumption is violated and there is selection on heterogeneous treatment effects, then the place effects I estimate should be thought of as local average treatment effects for the population moving to those places.

Thus, the main threat to identification stems from the possibility that moves correspond to *changes* in unobserved preferences – either through an idiosyncratic shock or via preference "drift", i.e. a gradual evolution in preferences that is not captured by aging. Revisiting the example in the previous paragraph, this would reflect a scenario where the two households in Pittsburgh were initially similar, but then one household becomes increasingly concerned about climate change and makes lifestyle choices to reduce its carbon emissions, subsequently relocating to New York to facilitate these choices, while the other household's preferences remain unchanged. In this example, the first household's carbon emissions would have changed to some extent even if it had stayed in Pittsburgh, and that portion of the change would be incorrectly attributed to the New York place effect.

A standard approach for ruling out endogenous moves 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 this impossible. Instead, 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 increase following a move, 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. Importantly, there is no pre-trend in energy expenditures leading up to a move. This result is shown Figure F.6.

In Section 5.1, I will also show evidence on preference "drift." If moves were endogenous to preferences evolving, or drifting, over time, you would expect the selection component, and in turn, the parameter estimate, to grow over time. There is some evidence of drift in the baseline analysis, but the magnitude of drift appears to be quite small, and becomes insignificant when looking at a restricted sample with no observable major life events.

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). The event study begins with the two-way fixed effect model defined in Equation 2, but summarizes heterogeneity with a single parameter, rather than the full distribution of J place effects. Consider a household i that moves from origin o to destination d. Based on the two-way fixed effects moel, 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:

$$\begin{aligned}\ln CO_{2it} &= \alpha_i + \psi_j + \tau_t + X_{it}\beta + \varepsilon_{it} \\ &= \alpha_i + \psi_o + \mathbb{1}[moved] \cdot (\psi_d - \psi_o) + \tau_t + X_{it}\beta + \varepsilon_{it} \\ &= \tilde{\alpha}_i + \mathbb{1}[moved] \cdot \theta \cdot (\bar{y}_d - \bar{y}_o) + \tau_t + X_{it}\beta + \varepsilon_{it} \qquad (6)\end{aligned}$$

By characterizing the place share of heterogeneity with a single parameter, θ, the event study approach vastly reduces the dimensionality of the estimation problem. However, this efficiency comes at the cost of an additional assumption: In order for $\hat{\theta}$ to reflect an unbiased, causal parameter, it cannot be correlated with other parameters in the model. In other words, unlike estimation of the full two-way fixed effect model, this approach does not permit systematic sorting of certain households, either based on observable characteristics or unobservable types, to certain type places. This is because it infers place types from observational means; if there were sorting of, for example, high type households to high type places, $\bar{y}_d - \bar{y}_o$ would grow faster than $\psi_d - \psi_o$ as places grow more different, leading to biased estimates.

The stronger restriction on sorting is more plausible at the CBSA level, where people are more likely to move for job opportunities or to be close to family, than at the neighborhood level, where choice is more likely to be driven by local amenities. I will provide evidence in Section 5.1 that estimates of the share parameter are not heterogeneous along several dimensions, suggesting that perhaps bias from this stronger assumption is minimal. However, even under weaker baseline assumptions, where event study results can't be interpreted as causal, the results are informative for two reasons. First, they serve as useful descriptive evidence and additional model intuition and validation for the KSS analysis to come. Second, they yield unbiased predictions about how household carbon emissions will change for any set of *observed* moves. This is particularly useful, as researchers have highlighted that restrictive zoning and high cost of living in productive areas drives people to higher emissions locations (Glaeser and Kahn 2010b). The labor and urban literatures have identified that such moves lead to decreased welfare due to loss of agglomeration externalities. The event study specification makes it possible to additionally estimate the carbon emissions externality of these regulatory restrictions.

4.3 Variance Decomposition

The second decomposition is based on the full set of nonparametric fixed effect estimates from the model. Specifically, heterogeneity in household carbon emissions can be decomposed as below, lumping τ_t with X_{it} for brevity:

$$\begin{aligned} Var(y_{ij}) = & Var(\psi_j) + 2 \cdot Cov(\alpha_i, \psi_j) + Var(\alpha_i) \\ & + Var(X_{it}\beta) + 2 \cdot Cov(\alpha_i, X_{it}\beta) + 2 \cdot Cov(\psi_i, X_{it}\beta) + Var(\varepsilon_{it}) \end{aligned} \tag{7}$$

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. Each variance component describe the share of *overall* heterogeneity attributable to the relevant component.

In contrast to the event study decomposition, the two-way fixed effects decomposition allows unrestricted sorting of households across places; it imposes no limitations on the magnitude or sign of the covariance terms. However, allowing unrestricted sorting comes at an econometric cost. 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. See Appendix E for additional computational details, and KSS for a complete discussion of the leave-out estimator.

5 Results

This section presents the core results of my paper: estimates of the share of spatial heterogeneity attributable to place effects. I begin 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 variance decomposition of 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

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

$\bar{y}_{j_{-i}}$ are sample means calculated from the full sample, leaving out the household observation.[16]

Table 3 presents event study estimates of the place share, $\hat{\theta}$ of spatial heterogeneity in household CO_2. Columns (1)-(3) show results from regressions examining CBSAs while columns (4)-(6) show results from regressions examining tracts. The top panel of the table shows results estimated from the panel sample consisting of movers and stayers; the bottom panel shows results using just the mover sample. While both the panel sample and mover sample leverage variation from household moves to identify place effects, the estimated place share of emissions may differ across the two samples if the relationship between observable household characteristics and emissions systematically varies between movers and stayers. For instance, if households who move after having children make that choice because having children increases their desire for a bigger house with a yard, while households who do not move after having children make that choice because having children strengthened their preference for being within walking distance of many leisure amenities, the inclusion of stayers in the panel regression would result in a downward bias in the direct effect of children on movers' emissions. This in turn would lead to an upward bias in the estimate of the place effect. Across specifications, the implied place share from using the mover sample is at most four percentage points lower than the panel equivalent, suggesting that movers appear to have marginally different systematic responses to changes in observables than stayers do, which leads to a slight upward bias in the estimate of the place effect when using the full panel.

Columns (1) and (4) present estimates with no controls other than year fixed effects. In columns (2) and (5), I control for observable household characteristics – age, household income, household size, number of children, and homeowner status, all of which are likely to simultaneously influence household carbon emissions and neighborhood choice. This purges the place share estimates from confounding variation directly attributable to changes in these attributes. I estimate that this decreases the CBSA share estimates by 5 percentage points or less, and decreases the tract share estimates by almost 20 percentage points. As you would likely expect, household sorting across neighborhoods plays a fairly important role in neighborhood-level variation in carbon emissions, while moves across CBSAs are more likely to be driven by other factors such as job opportunities or family.

16. To the extent that there is sampling variability in the distribution of observational means, my estimate of the relationship between origin-destination mean changes and individual changes in $\log CO_2$ may be biased. In practice, using a linear empirical Bayes estimator to adjust means for sampling variability as does not materially change the results.

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

	CBSA			Tract		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Panel Sample						
Place share of mean difs.	0.91***	0.86***	0.86***	0.78***	0.60***	0.55***
	(0.008)	(0.007)	(0.016)	(0.003)	(0.003)	(0.007)
N	1,764,000	1,764,000	633,000	1,710,000	1,710,000	613,000
R^2 (adj.)	0.73	0.75	0.77	0.74	0.76	0.77
B: Mover Sample						
Place share of mean difs.	0.89***	0.85***	0.84***	0.76***	0.57***	0.53***
	(0.009)	(0.009)	(0.020)	(0.004)	(0.004)	(0.009)
N	191,000	191,000	36,000	508,000	508,000	102,000
R^2 (adj.)	0.64	0.70	0.70	0.69	0.73	0.73
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. The place share of mean difs. corresponds to the coefficient estimate $\hat{\theta}$ on the difference in average carbon emissions ($\bar{y}$) between a mover's origin and destination locations. Panel A reports estimates from the panel sample, while panel B restricts the estimation sample to movers only in order to allow movers to differ systematically from stayers. Columns (1) and 4 report estimates from CBSA and tract (respectively) moves with no controls apart from year fixed effects. Column (2) and (5) add controls for the standard set of household characteristics. Columns (3) and (6) use the subset of the sample that did not have a change to the number of children in their household, or a larger than 50% increase or decrease in income. All estimates are weighted using Census sample weights.

Even after accounting for this rich set of observable characteristics, household destination choices may still be influenced by heterogeneous preference shocks. While such selection cannot be entirely ruled out, I explore the extent to it may bias my results by restricting the sample to households with minimal observable shocks, motivated by the premise that these households are less likely to have experienced large concurrent unobservable changes. The results reported in Columns (3) and (6) are estimated on a subsample that excludes households who experienced "big life events" between time periods – those who had a change in the number of children residing in the household, those who experienced a greater than 0.5 log point increase or decrease in income, and those who transitioned from renting to homeownership. The "no big life events" subsample retains approximately 20% of the original household observations. Estimates of the place share for this subsample are at most 5 percentage points lower than the baseline estimates, and remain above 50%. The fact that households least likely to have experienced unobservable shocks yield similar place share estimates to the baseline sample is reassuring and suggests limited bias from shocks to unobserved preference heterogeneity.

Another potential source of bias arises from "preference drift," or a gradual evolution in preferences that is not fully captured by changes in age or other observable characteristics. To investigate this, Figure 4 presents tract-level place share estimates by duration between observations, controlling for demographic and household characteristics. If households exhibit drifting preferences and relocate in response, the estimated place shares would conflate true causal effects with selection, and the selection component would likely increase with the duration between moves. Consequently, preference drift would manifest in place effect estimates that trend upward with the duration between observations. The light gray points depict estimates for the baseline mover sample, while the dark blue bars correspond to the restricted subsample of movers with no big life changes between observations.

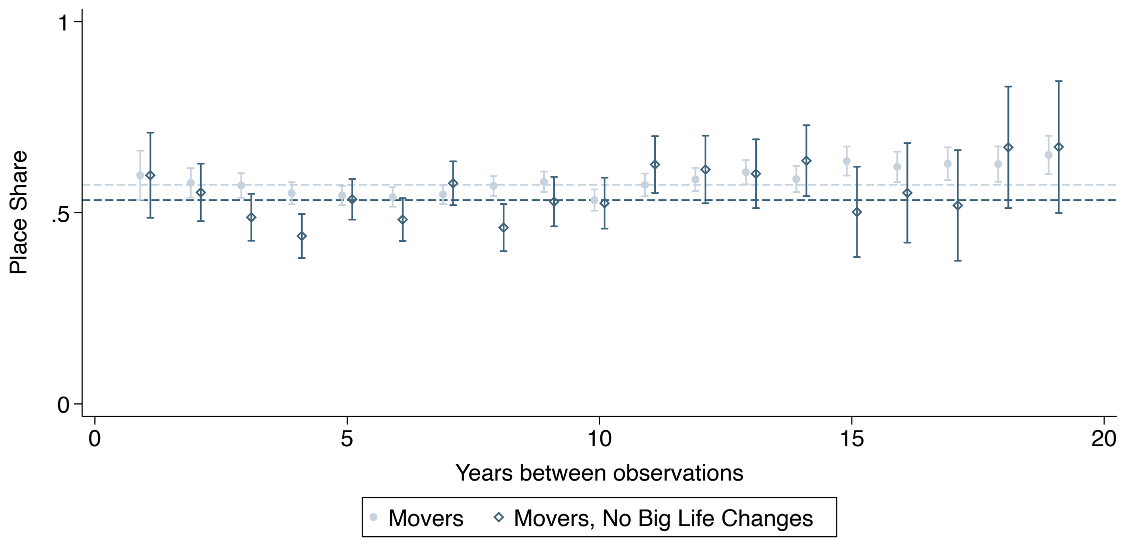
For the full mover sample, a slight upward trend emerges, with place share estimates for durations in the 15-19 year range exceeding the pooled estimate. This pattern dissipates when considering the restricted subsample of movers without observable big life changes; the pooled estimate for this group is not contained within the 95% confidence interval for four out of the 19 duration-specific estimates, but these are spread out over the time frame.Analogous CBSA estimates are shown in Figure F.7, and exhibit a similar pattern, except the upward trend observed in the longer-duration coefficients becomes insignificant in the full mover sample as well. This figure suggests that there is potentially some amount of preference drift, which could lead to upward biased estimates of the place share. However, the magnitude of the drift appears to be quite small relative to observed changes in emissions between places, and this bias does not appear to be a concern in the sample restricted to households with no big life events. 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, rather than characteristics such as peer effects or habit formation, which I would expect to lead to gradual changes in behavior over time.[17]

The final dimension of heterogeneity I explore is heterogeneity across different types of moves. A potential concern with the results presented thus far is that households choosing to relocate from low-emissions places to high-emissions places (or vice versa) may systeamtically differ from those moving between areas with similar emissions levels. If the choice of destination itself provides newly observable information about the preference shock experienced by the mover, the event study (and KSS) estimates would be biased, attributing the effect of that shock to their destination instead.

To explore this possibility, Figure 5 depicts changes in mover households' carbon emissions across deciles of origin-destination differences in observable means, controlling for demographic and household characteristics. In other words, I separate households moving from high to low-emissions places (leftmost points), those moving from low to high-emissions places (rightmost points), and those moving between places with similar average emissions (central points). The x-axis represents changes in mean emissions between origin and destination, while the y-axis corresponds to changes in household emissions. The gray 45-degree line represents a scenario where place effects account for 100% of the variation in carbon emissions across locations. The slope of the solid line corresponds to the pooled estimate of the relationship between tract-level mean changes and household-level changes.

17. 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.

Figure 4: **Place Share of Spatial Variation in Mean CO_2, over Time**

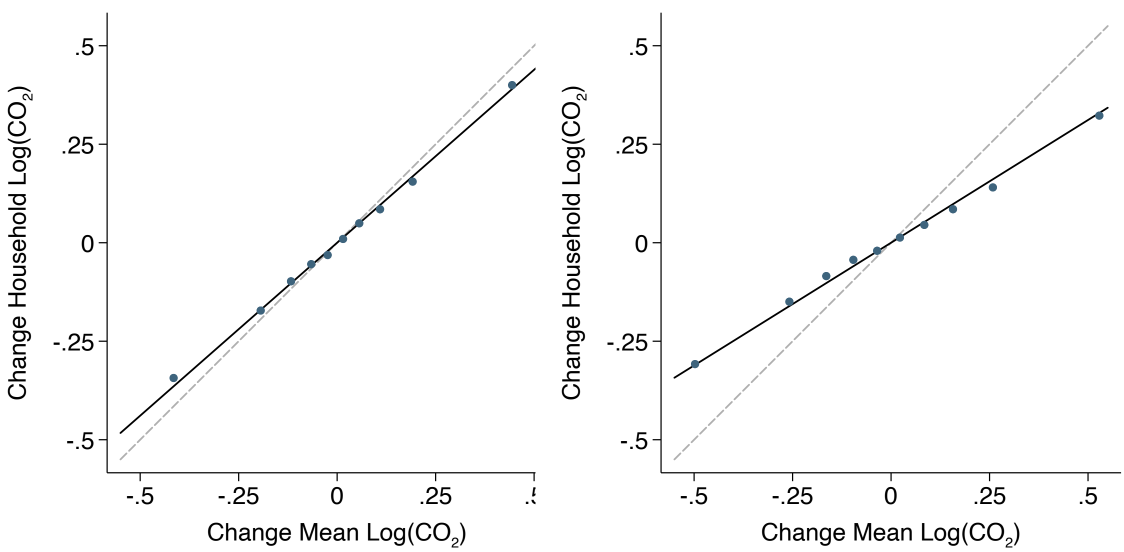


Note: This figure shows event study estimates of the share of spatial variation in mean carbon emissions that can be explained by place effects, by duration between mover observations. In other words, each coefficient is the estimate for place effects generated from the sub-sample of households that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

Figure 5: **Place Share of Spatial Variation in Mean CO_2 , by Move Type**

(a) CBSA

(b) Tract



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

I find that, for both moves across CBSAs and moves across neighborhoods, place share estimates are symmetric and linear across origin-destination mean emissions changes. In other words, the share of spatial heterogeneity attributable to place-based differences is consistent for moves to higher emissions places, moves to lower emissions, moves between very similar places, and moves between very different places. The symmetry and stability of share estimates have several implications. First, this result provides additional validation for the log-linear model specification, serving as an extension of the symmetry check presenting in Figure 3 and suggesting that the two-way fixed effect model is a reasonable approximation of household carbon emissions. Recall that this implies place effect estimates will be unbiased even if there are certain household types who are never observed moving to certain place types. Second, this result indicates that the event study estimates presented in Table 3 are not being driven by a subset of movers or mover destinations, alleviating concerns that households that move to a vastly different destination do so due to large unobserved preference shocks and that estimates primarily reflect changes in these households' emissions. Finally, this result provides another dimension along which heterogeneity in the estimated share parameter appears to be limited.

In summary, the event study estimates indicate that movers' carbon emissions change by over half of origin-destination differences in neighborhood means (and about 85% of origin-destination differences in CBSA means) when they move. Under strong assumptions on sorting, this can be interpreted as a causal share parameter. I estimate that heterogeneity in the share parameter is limited along several dimensions explored throughout this section – estimates are within a 5 percentage point range of each other across subsamples with vastly different changes to observed characteristics, across different observation time horizons, and across varying move types (between similar versus dissimilar places) – suggesting that perhaps bias from violations of this assumption may also be minimal.

However, even under weaker assumptions, these estimates serve as a useful specification check for the two-way fixed effect model, and perhaps more interestingly, they provide unbiased prediction of how household carbon emissions will change for any observed move. Recent research and media coverage have highlighted migration patterns from more expensive, but on average lower emissions places to less expensive but on average higher emissions (Kolko 2021; Eisen 2019). For instance, in 2019, there was a net migration of about 45,00 people from California to Texas. While not all these moves were from San Francisco to Houston, using these two cities as an illustrative example: carbon emission in Houston are about 21% higher than they are in San Francisco (Jones and Kammen 2014b). Combined with the event study findings, this implies that regulatory constraints that restrict housing, increase the cost of living, and drive this pattern of migration increase household carbon emissions by 12% or more, imposing a sizeable carbon externality.[18]

5.2 Variance Decomposition

In this section, I weaken the restriction on sorting imposed by the event study, and present estimates from the variance decomposition shown in Equation 7

18. The 12% figure would correspond to treating San Francisco and Houston as neighborhoods. If we were to treat them more like CBSAs, the predicted increase in emissions would be closer to 19%.

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

For each sample and specification, Table 4 presents the overall variance of the outcome, $\log(CO_2)$, the share of variance attributable to each of the unobserved heterogeneity components (place effects, ψ_j and household effects, ψ_i), the correlation between the unobserved heterogeneity components, and the bias-corrected standard deviation of place effects. The top panel presents estimates from the entire panel of movers and stayers, while the bottom panel presents estimates from the mover only sample.

In the baseline analysis, with year fixed effects and the standard vector of household controls, I estimate that CBSA effects account for roughly 15% of overall heterogeneity (column 1), and tract effects account for roughly 22% of overall heterogeneity (column 5). In this baseline specification, variation in place effects could be driven in part by variation in climate or electricity emissions factors. However, interventions targeting urban form cannot appreciably change a place's climate, and emissions factors are generally determined at geographic scales larger than CBSAs and tracts. To get closer to estimating the share of heterogeneity attributable to just local regulations, built environment, and public amenities, I re-estimate variance components adding heating degree days, cooling degree days, and log electricity emissions factors to the set of controls in columns (2) and (6). In columns (3) and (7) I additionally partial out variation driven by prices, using a price index constructed from interacting local lagged fuel shares with national retail prices.

I find that controlling for climate and electric grid intensity decreases the CBSA share of spatial heterogeneity by roughly 10 percentage points, or by more than half, to 4-7% of overall heterogeneity. In Appendix Table F.6, I control for only one of these two factors at a time, and I find that the majority of that decrease comes from controlling for electricity emissions factors, which is consistent with the mechanical relationship between electricity emissions factors and CO_2. There is also a well-understood and robust relationship between climate and energy use (e.g. Goldstein, Gounaridis, and Newell 2020), but climate alone appears to not be driving a large share of variation across CBSA place effects. At the neighborhood level, controlling for climate and electric grid intensity decreases the place share of heterogeneity by roughly 6-7% (Column (6)), and remaining neighborhood attributes explain a larger share of variation ($\sim$ 15%) than climate grid and intensity, underscoring the importance of residual place characteristics such as urban form or local regulations. Accounting for variation in the price index does not further change the results at either the CBSA or tract level.

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

	CBSA				Tract		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel Sample							
Variance of log(CO_2)	0.31	0.31	0.31	0.31	0.31	0.31	0.31
Share attributable to places	0.16	0.07	0.08	0.17	0.23	0.15	0.15
Share attributable to hhs	0.50	0.50	0.50	0.30	0.36	0.36	0.36
Corr. of place and hh effects	0.01	0.03	0.03	0.02	0.02	0.08	0.11
SD of place effects	0.23	0.15	0.16	0.23	0.26	0.22	0.22
Mover Sample							
Variance of log(CO_2)	0.35	0.35	0.35		0.33	0.33	0.33
Share attributable to places	0.14	0.04	0.04		0.22	0.16	0.16
Share attributable to hhs	0.14	0.16	0.17		0.10	0.10	0.10
Corr. of place and hh effects	0.07	0.08	0.08		0.08	0.16	0.18
SD of place effects	0.22	0.12	0.12		0.27	0.23	0.23
Climate + Electricity CO_2		X	X			X	X
Price Index			X				X
Time-Varying FEs				X			

Note: This table reports results from the heteroskedasticity-robust KSS estimation of variance components. For each sample and specification, the table reports the overall outcome variance, the share of variance attributable to place effects (ψ_j), the share of variance attributable to household effects (α_i), the correlation between place effects and household effects, which captures how much households sort on unobserved characteristics, and the bias-corrected standard deviation of place effects. All specifications include year fixed effects and the standard set of household controls used throughout the paper. Columns (1) and (5) report the baseline variance decompositions at the CBSA and tract levels. Columns (2) and (6) add controls for local mean heating degree days, cooling degree days, and log electricity emissions factors. Columns (3) and (7) 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 periods (2000-2004, 2005-2009, 2010-2014, and 2015-2019), using stayer observations across periods to identify time variation in place effects, while movers, as before, identify cross-sectional variation.

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 have 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. Changes to place effects could also 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.[19]

To allow for these place-specific changes, I follow Lachowska et al. (2023) and estimate time-varying fixed effects ψ_{jt} at the CBSA level, using stayers to identify variation across time within place.[20] 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 (4) – allowing CBSA effects to evolve increases their variance share by only one percentage point relative to the baseline specification. This implies that changes in places over my sample period are either mostly captured by national secular trends, or are unrelated to household carbon emissions. Appendix Figure F.8, lends additional evidence to this. Pooling all time-varying fixed effect estimates together and grouping pooled values into four quartiles, the vast majority of CBSAs either do not change rank or become lower emissions from the first period (2000-2004) to the last period (2015-2019). In contrast, defining quartiles within year, the distribution of whether CBSAs become relatively lower or higher emissions than their counterparts between those two periods is roughly symmetric, but with over half of CBSAs not changing relative rank.

There was likely a discontinuous change in CBSA effects following the COVID-19 pandemic and the shift to remote work, which was accompanied by a steep decline in commuting and a shift towards larger homes to accommodate home offices (Van Nieuwerburgh 2023; D'Lima, Lopez, and Pradhan 2022). Cicala (2023) finds that during the acute parts of the pandemic (Q2-Q4 of 2020), residential energy consumption increased by about 8%, while the use of transportation fuel consumption declined by about 16%. The resulting increase in residential energy is likely to increase the gap in place effects between suburban and urban tracts, though of course the net impact on emissions is modulated by the decrease in commercial energy. In contrast, the decrease in commuting is likely to decrease the gap between suburban and urban tracts. It is a limitation of my data that I only observe commuting miles, but in my sample time fram, using NHTS to predict overall transportation from commuting does not substantively change the results. In the COVID-era this data limitation becomes prohibitive as commuting and overall transportation miles become completely disentangled. Finally, it is worth noting that while initially it seemed like there might be a permanent structural shift to remote work and a decline of cities (e.g. Gupta et al. 2021), as of 2024 it appears that many employers are requiring workers to return to the office[21], calling into question whether the pandemic will have had a long term impact on cities. The net effect of all these countervailing forces, and the extent to which they result in a permanent, structural shift in place effects, is an empirical question which this pa-

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

20. Because tracts by definition consist of many fewer observations than CBSAs, including a time varying component introduces either substantial noise, or a substantial geographic restriction to only the most populated tracts, so I do not estimate time-varying tract effects. Being able to observe and study neighborhood-level changes is an important direction for future study.

21. See e.g. this Resume Builder (2023) report.

per does not have enough data to address at this time, but is an important avenue for future research.[22]

Returning to the main results, a comparison between the panel sample results and the mover sample results reveals that the contribution of unobserved household characteristics to overall heterogeneity is highly sensitive to which sample the model is estimated on. In the panel sample, unobserved household heterogeneity accounts for 50% of overall heterogeneity when defining place at the CBSA level, and 36% when measuring place at the neighborhood level. This share is stable to partialling out exogenous amenities (climate and electricity emissions factors) and prices, but the CBSA estimate decreases from 50% to about 30% when allowing CBSA effects to change over time. Using the mover-only sample substantially decreases the unobserved household contribution across specifications, to 14-17% in the CBSA specification and 10% in the tract specification. The correlation estimates between unobserved place and household characteristics are also sensitive to sample choice, increasing across all specifications by up to 8 percentage points when switching from the panel to the mover sample. This implies that there is some assortive matching of household types to place types (especially neighborhoods), but in the specification where the correlation coefficient is largest, it remains relatively low, at under 20%. The share of overall heterogeneity attributable to matching implied by the covariance term (which is not shown in Table 4 but can be computed from the correlation coefficient and variance components) is less than 5% across all specifications.

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. In the event study decomposition in Section 5.1, estimates of the place share from the mover population appeared to be only a few percentage points lower than those from the full panel, which would point to serial correlation among stayers, rather than movers being different from stayers, being the primary culprit. To shed additional light on the relative importance of these pieces, I compare estimates from the KSS decomposition to estimates from the naive, uncorrected AKM decomposition (Abowd, Kramarz, and Margolis 1999) , which are shown in Table F.7. 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 due to 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 large differences being introduced only in the KSS correction. Indeed, the AKM estimates suggest that the relative place and person shares are fairly stable across the panel and mover samples. The size of the household component relative to the place component does drop a few points in several of the specifications (consistent with the small change in the share component seen in

22. It is also worth highlighting that the relative importance of place in driving household carbon emissions will decline as the residential and transportation sectors electrify and as the electric sector decarbonizes. However, many have highlighted that a current challenge to clean electrification is transmission capacity and aging transmission infrastructure (e.g. U.S. Department of Energy 2015). To the extent that any given intervention could weaken energy demand and alleviate pressure on the grid, I expect it to be complementary to these more traditional instruments.

the event study), but not nearly as dramatically as it does in the KSS analysis. This suggests that the KSS estimates of household variance components are likely more reliable in the mover sample; estimates in the panel sample are an upper bound on the true value, with the upward bias driven primarily by serial correlation in the stayer error term.

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 residential energy expenditures into carbon emissions, and I use reported commute times and a mix of local and national fuel economy factors to estimate carbon emissions from commuting. Each 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 A.2.

The above referenced sources of measurement error within the residential and transportation sectors also have the potential to introduce bias into my estimates separate from the inflation of variance components due to serial correlation. To evaluate the sensitivity of the KSS results to outcome definitions, Appendix Table F.8 shows estimates from a KSS decomposition, on both the panel and the mover sample, using several alternative outcome definitions. Each outcome definition is a row in the table, with columns showing overall variance, the share attributable to place effects, the share attributable to person effects, and the correlation between unobserved components of heterogeneity, for both the CBSA and tract notion of place. I begin by reproducing baseline estimates. I then address potential issues within both the residential and transportation sector with a variety of modifications to the construction of carbon emissions, including e.g. accounting for tiered electricity pricing or allowing for heterogeneous vehicle fuel economy. I find that my estimates of place and household variance components are for the most part qualitatively stable to these variants, except in the case of using marginal instead of average electricity emissions factors, which increases my estimate of the place share of spatial heterogeneity by about 7 percentage points relative to the baseline estimates.

As a final specification test, in Appendix Figure F.9, I show binned scatter plots similar to the one presented for event study results (Figure 5), 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 events. In a correctly specified model, changes in place effects should lead 1-to-1 to changes in household carbon emissions, though we expect attenuation bias from noisily estimated place effects to decrease the slope by some. This is roughly what I observe, and as with the event study estimates, I find no discernible difference between the primary sample and the sample restricted to households with no big life events, which provides additional reassurance that selection on heterogeneous preference shocks isn't a first order threat to identification in my analysis.

How do the KSS 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 have shown evidence that the event study estimates are fairly stable across several observable dimensions of heterogeneity in the data. I also showed in the KSS decomposition that the covariance between unobserved components of heterogeneity is relatively small – the largest correlation coefficient across the four baseline estimates corresponding to the specifications examined in the event study is .08. Together, these pieces of evidence suggest that bias from this assumption on selection should be minimal.

Even when unbiased, 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 help illustrate this, consider a simplified version of the two-way fixed effects model, where $y_{it} = \alpha_i + \psi_j + \varepsilon_{it}$. Song et al. (2019) show that unobserved heterogeneity can be decomposed further 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:

$$\begin{aligned} Var(y_{it}) &= Var_j(\bar{y}_j) + Var_i(y_{it} - \bar{y}_j | i \in j) \\ &= \underbrace{Var(\psi_j) + 2 \cdot Cov(\bar{\alpha}_j, \psi_j) + Var(\bar{\alpha}_j)}_{\text{Between}} + \underbrace{Var(\alpha_i - \bar{\alpha}_j) + Var(\varepsilon_{ij})}_{\text{Within}} \end{aligned} \tag{9}$$

Equation 9 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 "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.[23] 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.

Intuitively, 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 one (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 (85%) 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 $\sim 15\%$ in the KSS estimation, (more than half of which is attributable

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

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 roughly 55-60%; including variation within neighborhoods and variation not explained by the model in the denominator decreases the neighborhood share to 22-23% of overall heterogeneity, or about 15% when excluding climate and electric grid intensity.

6 The Characteristics of Low and High-Emissions Places

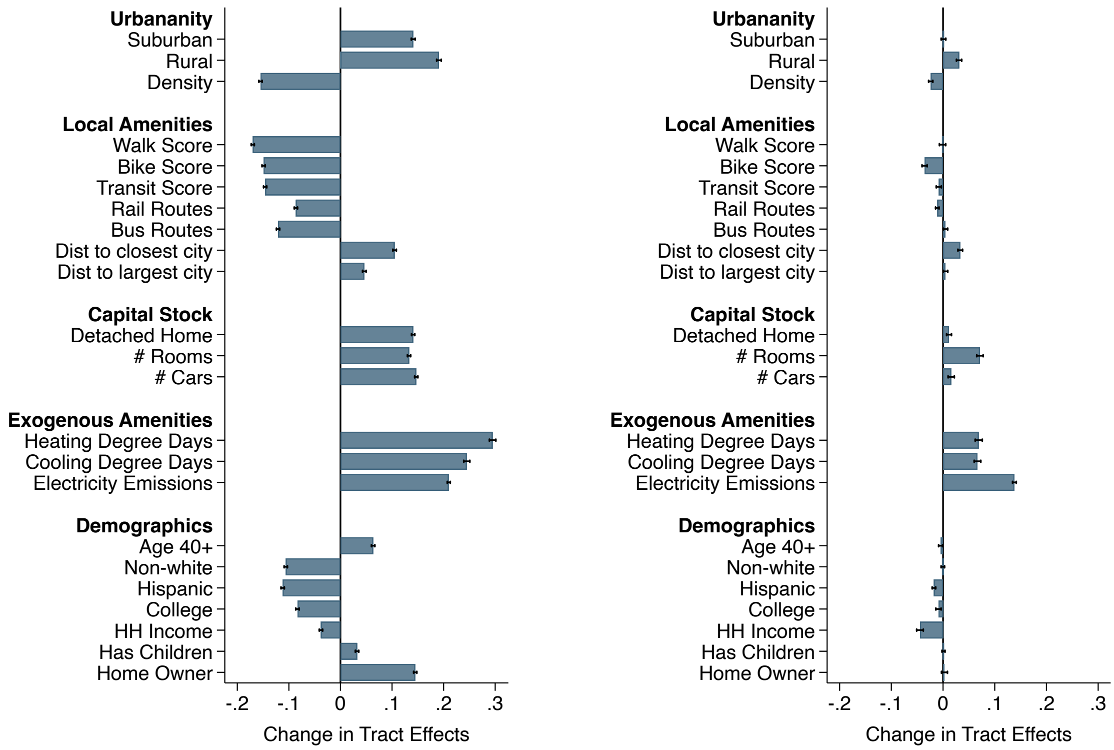
With estimates of place effects in hand, I proceed to characterizing high and low-emissions places. As highlighted in the conceptual model, place effects reflect a mix of differences in demand for energy quantities, energy prices, energy demand elasticities, fuel mixes, and emissions factors. The urban 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. Many of these amenities are intertwined and simultaneously relate to residential and transportation emissions, yielding additional motivation for studying these sectors together; density means smaller homes and the potential for more effective public transportation networks, as it decreases the distances people need to cover by other means between transit stops and their origin and final destination.

In Figure 6, I show the results from regressions of estimated tract effects onto a set of observational tract level characteristics. In the left panel, I show coefficients from bivariate regressions of tract effects onto each of the shown tract-level characteristics independently (except for suburban and rural, which are estimated in a single regression), while in the right panel, I show coefficients from a single regression onto all of the characteristics.[24]

24. I used a LASSO regression with 10x crossfold validation to select characteristics, and it retained all of the variables.

Figure 6: **Correlates of Tract Effects**

(a) Bivariate Regressions (b) Single Regression



Note: This figure presents estimates from OLS regressions of estimated tract effects on a set of observable place-based and household characteristics. Panel (a) shows results from separate bivariate regressions, while panel (b) shows results from a single regression on all covariates. All amenity variables are tract level means, normalized to have mean zero and standard deviation one, except the rural and suburban indicators, which are retained as indicators. Regressions are weighted using ACS sample weights.

Observable characteristics are all measured at the tract level, and are normalized across tracts to have mean zero and a standard deviation of one (except for suburban and rural, which are retained as indicators). Thus, the interpretation of the shown coefficients is the average change in place effect associated with a one standard deviation change in observable characteristics. I visually group observable characteristics into five categories: urbanity and local amenities cover aspects of urban form that households effectively taken as given, but that vary neighborhood to neighborhood. These include whether a tract is urban, suburban, or rural, its density, measures of local public transportation amenities (walk scores, bike scores, transit scores, number of nearby rail routes and bus routes), and measures of sprawl (captured by geodesic distance between tract centroids and the centroid of the closest city and the largest city within the CBSA). 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 (whether a home is detached, its size, and the number of cars in the household). Exogenous amenities are those that are unrelated to local urban form, i.e. annual heating degree days, annual cooling degree days, and electric grid intensity.[25] Finally, the demographics

25. As mentioned earlier, urban form can quite meaningfully impact local micro climates through urban heat island effects or conversely tree cover and shade (Hoffman, Shandas, and Pendleton 2020), but in this paper my measure of climate is at the NOAA climate division level which is a geographically much coarser definition.

category captures tract level variation in household observable characteristics, namely age, race, ethnicity, college education, income, whether the household has children, and home ownership status.

The results of the bivariate regressions (panel a) are consistent with observational data and conclusions in the urban planning literature. Higher emissions neighborhoods tend to be less urban and less dense, have worse walkability, bikability, and public transit, and be farther away from cities within their CBSA. High-emissions neighborhoods also have higher shares of detached homes, have larger homes, and households living in them have on average more cars per household. They have more weather extremes, and have higher electricity emissions factors. Finally, looking at the projection of tract effects onto demographics, I find that non-white, Hispanic, college-educated, and higher income households are more likely to live in low-emissions tracts, while older households, households with children, and homeowners are more likely to live in high-emissions tracts.

When including all of these characteristics in a single regression (panel b), directionally the relationships remain the same, but many of the coefficient magnitudes decrease substantially. The largest coefficient in this regression is electricity emissions factors, followed by home sizes, and then the two climate variables. The remainder of tract effects appear to load primarily onto measures of sprawl, the quality of local bike infrastructure, rurality, density, and household income. It is notable that measures of walkability, transit quality, share of detached homes, and number of cars appear to become less important after conditioning on density, sprawl, and home size. It is possible that this is because these characteristics are very co-linear in the observational data; as highlighted earlier, many of these relationships are intertwined.

It is also important to emphasize that these regressions do not elucidate causal relationships. The two-way fixed effect model allows for unrestricted correlations between place effect estimates and other parameters in the model. To illustrate this, consider that when I re-estimate these regressions using tract effect estimates from the specification that separately controls for climate and electricity emissions – meaning, the role of these parameters in driving carbon emissions should be accounted for outside of the place effects – the coefficient estimates look almost identical across the board except that the coefficient on heating degree days switches to being negative. The fact that non-zero coefficients remain on characteristics that are excluded from place effect estimates highlights their correlational nature. Many of the lowest emissions neighborhoods are in older cities located in the Northeast (Tomer et al. 2021), and many places that are otherwise low-emissions are in regions that have made efforts to decarbonize their electricity (Murray and Maniloff 2015; Petek 2020).

As is the case with inference about place effects as a bundle, inference about the role of specific amenities from observational data alone is biased. Nevertheless, the observational data suggests features of urban form whose effect on household carbon emissions should be studied further with credible exogenous variation in the amenity itself. Previously, one might worry about whether a setting for such a case study is too selected. Can we learn about the role of a subway extension generally from a subway extension in New York if New Yorkers are so different from the rest of the population? Are Californians more inclined than the average individual to take advantage of improved bike infrastructure? The core results of my paper suggest that a meaningful share of variation between places is driven by variation in place effects, mitigating some of these concerns about external validity.

Appendix Figure F.10 shows analogous projections of household fixed effects onto observable neighborhood-level characteristics. I find that correlations between household effects and observable characteristics are about an order of magnitude weaker than those between tract effects and observable characteristics, in both the bivariate regressions and the full regression. This is consistent with minimal sorting on unobserved household characteristics that I estimate in KSS. The largest coefficients imply that households with high unobservable potential for carbon emissions sort to tracts where people are on average higher income and have more children, where it is warmer, where electricity is higher emissions, and where houses are bigger. Appendix Table F.9 presents additional results on the correlates between observable household characteristics and observable place characteristics.

7 Implications for Aggregate Carbon Emissions

The wide distribution of place effects implies 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 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 gained some traction in the US in recent years. In 2018, Minneapolis became 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, the California State Assembly passed Bills 9 and 10, which streamlining the process of "up-zoning" residential land, allowing for the development of up to four units on land previously zoned for single-family homes only and facilitating higher-density construction near transit corridors. This was followed by AB 2097 in 2022, which eliminated most parking minimum requirements for new developments. 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. The Federal Government has a history of investing into transportation infrastructure, with the Federal Highway System being its most notable example (U.S. Department of Transportation 1977). More recently, the Bipartisan Infrastructure Law provided a significant increases to transit funds across the country, with the Federal Transit Administration announcing in early 2024 that it would be investing 9.9 billion dollars to support local transit systems in urban areas across the US U.S. Department of Transportation 2024. While adding and expanding public transit networks is perhaps the most obvious example of an intervention that might decrease place effects, 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 (Ville de Paris 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" – programs which closed certain streets to car traffic in order to give pedestrians and cyclists more space to socially distance – in many US cities . In some places, there are ongoing efforts to make these initially temporary programs permanent (New York City Department of Transportation 2023; Combs 2020).

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, less sprawling, neighborhoods. Using this as motivation, I begin by examining how carbon emissions would change if the national distribution of place effects were more urban than it currently is. Specifically, to approximate a scenario in which the US limits suburban sprawl and increases the share of households within a region that live in an urban neighborhood, I estimate the effect on emissions if households currently living in a suburb or rural area lived instead in a place with the average place effect of the nearest principal city. A naive comparison of household emissions shows that, on average, households living in principal cities emit 20% less than households living in surrounding areas. After accounting for sorting of households between suburban and urban neighborhoods, my estimates suggest that if every tract had the place effect of the nearest principal city, the emissions of suburban and rural households would decrease by approximately 15%.

The majority of principal cities in the US would not be considered particularly urban on a global scale. To consider the potential effect of deeper urbanization, 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 examine how the emissions of household in the principal cities of the other nine largest CBSAs in the US would change if those cities developed into a place with the average place effect of Manhattan. The observational gap is enormous, even using other large cities as a comparison group; households in Manhattan emit about 73% less than observably comparable households in the other nine large cities. Accounting for unobserved fixed differences between households reduces the gap to 60%; people who choose to live in Manhattan are a selected sub-sample, but their emissions are nevertheless much lower as a result of living in Manhattan than they would be if they lived in another large U.S. city.

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. More importantly, my estimates don't lend insight into the specific interventions that would result in the largest changes to place effects – this is a critical direction for future research. My results highlight that the potential for urbanization-induced reductions to carbon emissions is overstated when inferred from observational means across places, because people do 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 15-23% of this variation – depending on whether you allow climate and electricity emissions to impact place effects – they also imply that even relatively small shifts in the distribution of place effects that households are exposed to 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 up to 23% of overall heterogeneity in household carbon emissions from residential energy use and commuting across the US can be explained by neighborhood effects, or roughly 15% can be explained by neighborhood effects after accounting for variation driven by climate and electric grid intensity. Paired with high overall heterogeneity, these estimates imply that interventions that change the population weighted distribution of place effects across the US, either through direct changes to places, or through regulatory changes that make it possible for more people to live in low emissions places, could result in meaningful reductions in household carbon emissions.

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 critical 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. 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 2021; Christensen and Timmins 2023; Avenancio-León and Howard 2022). 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 Data Appendix

A.1 Additional Details on Variable Construction

- **Missing and imputed variables:** I follow Chetty and Hendren (2018) and Bailey et al. (2020) in treating 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 retain information on employment status, 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.

- **Demographic characteristics** I control for age using bins: 18-24, 25-29, 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 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.

- **Observations dropped due to missing utility data:** I exclude from the sample households whose residential energy costs are included in rent, or whose gas costs are included in their electricity bill, because I do not observe expenditures in those cases. As shown in Table F.1 this sample is significantly lower income, less likely to own their home, less likely to live in a detached single family home, less likely to commute by car, and more likely to live in an urban neighborhood. For the subsample who would have been in the panel if not for this restriction, I estimate these households are more likely to have an increase in income, an increase in the number of children in the household, and go from renting to owning than the baseline sample. They are also twice as likely to move from an urban neighborhood to another urban neighborhood when moving. To the extent that you might worry that 1) households who live in the most urban neighborhood are most selected and would respond differently to changes in place than a more average household or 2) large changes in observable characteristics signal that estimates may be biased by accompanying large changes in unobservable characteristics, dropping these households from the analysis is likely to decrease any upward bias in place share estimates that could arise from violations of either the constant effects assumption or the exogenous mobility assumption.

- **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 (U.S. 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 (U.S. Environmental Protection Agency 2021b). As a robustness check, I also use data from the National Household Travel Survey (NHTS) data to estimate heterogeneous fuel economy values. I discuss the construction of these estimates in Appendix A.2.

- **Carpooling**: I divide carbon emissions by the number of car-poolers for individuals who report carpooling.

- **Emissions from public transportation:** I assign emissions factors to commutes by public transportation using estimates derived from the National Transit Database (Federal Transit Administration). Specifically, for each transit agency and year, I use reported data on fuel consumption and passenger miles travelled (PMT), by mode, in order to estimate carbon emissions per PMT for six possible modes of commute: subway or elevated rail, commuter rail, streetcar, bus, ferry, or taxi.[26] As with residential emissions, I assign fuel emissions factors using data from the U.S. Environmental Protection Agency 2018 and I assign electricity emissions factors using average values at the NERC region level. After estimating mode-specific emissions for all reporting transit agencies, I estimate passenger mile weighted mode emissions factors in each urban area, and assign individuals an emissions factor based on the urban area they live in. For individuals who live outside of an

26. NTD modes of commute are more granular than ACS modes of commute. I group heavy rail, monorail/automated guideway, light rail, and aerial tramway into the subway & elevated rail group. I treat commuter rail and hybrid railroad as railroad. I group streetcar, cable car, inclined plane, and trolleybus into the streetcar mode. Bus, bus rapid transit, and commuter bus all get categorized as bus. And lastly, demand response, demand response taxis, and vanpools all get categorized as taxi.

urban area with a reporting agency, I use their mode's national annual average emissions per passenger miles travelled to estimate emissions.

- **Emissions from Walking or Biking**: I assign zero emissions to commutes by walking or biking. This will be an underestimate of emissions from biking as electric bikes, or e-bikes, grow in popularity. Unfortunately, I cannot distinguish in the ACS what kind of bikes commuters are using, and in the majority of my sample traditional bicycles dominated the market. An important question for future research is how e-bike subsidies and local bike-share programs change commute mode choice and emissions.[27]

- **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 re-scale my mileage estimates to match the national average commuting distance, by mode, reported in the NHTS. 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 150 mph in a train[28], or over 80 mph on average in other modes.

- **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 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.

- **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.

A.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 both 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 the various possible sources of measurement error, as well

27. Xu (2020) finds that bike commuting is more common in cities with bike share programs.
28. This is the fastest speed a train ever goes in the US, along a small segment of the Northeast Corridor.

as potential biases that arise in my estimates as a result. In cases of greater concern, I discuss the construction of alternate variables used for robustness checks in the paper.

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

In my baseline specifications, I estimate electricity prices from total utility revenues divided by total utility customers, by county (using data from EIA Form 861). 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 (2022) 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.

Alternate Electricity Price Estimates for Robustness Checks

To test the sensitivity of my results to the issues described above, I construct several alternate estimates of residential electricity prices.

First, I account for fixed charges, closely following Borenstein and Bushnell (2022) in my approach. I supplement the EIA 861 data with annual data from the Utility Rate Database (URDB), which contains utility-level data on rate schedules. I collect fixed charges from the set of utilities in the URDB that report detailed retail pricing information, using the median fixed charge in the standard tariff for each utility-state pair in cases where utilities reported multiple rates.

The URDB is not perfectly populated, and is much sparser in the earlier years[29]. In cases where I observe a fixed charge for some but not all years of a utility-state pair, I impute values for missing years using values from the closest available year. If I observe two different

29. Coverage of EIA 861 utilities goes from 16% in 2000 to 79% in 2019.

fixed charges with missing years in between, I impute the value for those missing years using the mean of the observed values.

I then estimate the variable price component for each utility-state pair by combining my fixed charge estimates with annual total revenue, generation, and customer data from the EIA 861. I subtract estimated total fixed revenue (fixed charge times number of customers) from total revenue reported in EIA 861, and then divide variable revenue by total sales to get a variable price per kwh of electricity. Consistent with the fact that fixed charges are generally a low share of the two-part tariff (I estimate that across my sample fixed charges make up roughly 9% of total revenues), the distribution of average and variable prices look pretty similar. I proceed as in the baseline estimation, constructing a county-level average variable price as the customer-weighted mean variable price of all utilities serving a given county. In the microdata, for counties without a variable price estimate, I continue to use my average price estimate.

Second, I account for the fact that sometimes utility tariffs follow a tiered pricing schedule, where marginal prices either increase or decrease with the quantity of electricity consumed. URDB also contains some information on price schedules with tiered pricing, but these data are even more complex and sparse than the fixed charge data. I have no way of knowing which customers choose a rate with tiered pricing, or even what share of customers are on each schedule. To bound the issues that could arise from tiered pricing, I gather information on the mean price difference between the top and bottom price for each utility-state-year. I do this separately for tariffs with increasing block rates vs. decreasing block rates. As with the baseline and variable price estimation, I estimate a county-level average price difference for increasing and decreasing block prices. I then estimate top and bottom county-level prices as the variable price in that county plus/minus half the price difference. I estimate the price step as being at the median county level quantity consumed, as estimated using average variable price.

I then explore three bounding scenarios. In the first, I assume that every customer who lives in a county where an increasing block price schedule is available chooses the increasing block price schedule. In counties without any increasing block price schedules, customers are assigned the average variable price. In the second, I make an analogous assumption but with decreasing block prices. Finally, I consider a selected scenario, in which customers with below median electricity costs for their county select into an increasing block pricing schedule, while customers with above median electricity costs for their county select into a decreasing block pricing schedule. Note, this selected scenario also yields some insight into the bias that would arise from customers selecting across utilities based on price. While none of these perfectly capture the actual price schedules faced by all customers in the data, they should provide some bounds on the bias incurred by not accurately observing marginal prices.

Using estimated variable prices or assigning tiered pricing schedules to households does not meaningfully impact variance component estimates (Table F.8).

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.

Alternate Electricity Emissions Estimates for Robustness Checks

In my baseline outcomes, I estimate household electricity emissions using average emissions factors computed from aggregate production and fuel use at the NERC level. Conceptually, I believe that this is the right emissions factor to use because a change in the place effect simultaneously affects all residents of the place. However, I also construct a measure of household electricity emissions using marginal emissions factors. Note that this doesn't address the issue of emissions varying across hours and seasons and that variation possibly being correlated with usage patterns, because I cannot distinguish differences in marginal emissions across households within a place.

I follow Borenstein and Bushnell (2022) and estimate marginal emissions for each of 9 regions – the eight reliability regions of the North American Electric Reliability Coorporation (NERC), with the Western Interconnection region (WECC) split into California and non-California sub-parts – by regressing hourly carbon emissions on hourly load using the following specification

$$CO_{2it} = \beta Load_{it} + \alpha_{mn} + \gamma_i LoadInterconnect_{-it} + \epsilon_t$$

where α_{mn} represents month of sample by hour of day fixed effects and γ_i represents the marginal effect of load from other parts of the interconnect onto carbon emissions in a given region. Marginal emissions from electricity load in a region are then given by $\beta_i + \sum_{j \neq i} \gamma_j$. In practice, allowing for the impact of other regions' load on marginal emissions does not make a big difference.

I construct hourly carbon emissions from power plants in each region using data from the Environmental Protection Agency (EPA) Continuous Emissions Monitoring System (CEMS). I extend estimates of hourly load from Cicala (2022) through 2019 using data from the Federal Energy Regulatory Commission's Form-714 Survey. In a few subregion-year pairs where supplementary data were required but unavailable, I interpolate subregion marginal emissions from the nearest available year.

Using marginal emissions estimates to construct carbon emissions from electricity increases the share of covariance attributable to places by about 7 percentage points relative to baseline estimates (Table F.8).

Natural gas and other residential heating fuel prices and Emissions

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.

The "Commute from hrs" rows in Table F.8 show that 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.

Indeed, the results in the"Commute from hrs, fixed n." row in **??** are qualitatively similar to the baseline estimates.

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 – and my inability to observe fuel economy is a significant limitation of my data – 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. As with my broader analysis, there is a question of whether regional patterns are driven by individual preferences or place-based differences. If regional variation in fuel economy is driven by individual preferences, I will 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 understate the relative importance of the place effect.

Additionally, 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. On the other hand, if they walk or bike part of the distance of their commute, I will over-estimate their carbon emissions and understate overall heterogeneity.

Allowing for Heterogeneous Vehicle Fuel Economy

In the baseline specification, I assign a national average fuel economy to all households. To explore the sensitivity of my results to this assumption, I construct three estimates of fuel economy using data from the NHTS, allowing for heterogeneity across geographic characteristics only (CBSA, state, urbanity), individual and household characteristics only (age, race, household size, household income, gender, number of vehicles, and commute mode of transit interacted with commute length), and both sets of characteristics. For each specification, I use a penalized Lasso regression to predict individual-level vehicle fuel economy based on the included set of characteristics, and then I use these estimates of mpg to estimate emissions from commuting.

Results are presented in the three "MPG" rows of Table F.8, and are not qualitatively different from baseline estimates.

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.

Estimating Total Vehicle Miles Travelled

In the baseline specification, miles commuted serve as a proxy for total vehicle miles. To explore the sensitivity of my results to this assumption, I also construct an estimate of total miles travelled. This estimate is constructed using a penalized Lasso regression to predict total miles from the set of both household and geographic variables described above in the NHTS data. Table F.8 shows that this does not meaningfully affect the results.

B The Leave-One-Out Connected Set

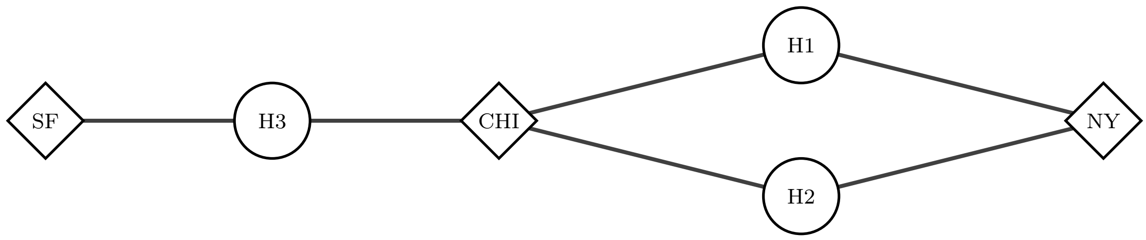
Consider the following data:

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.

C Empirical Bayes Adjustment

When discussing distributions of either observational means or place effects, I account for the fact that these parameters are noisily estimated by using linear Empirical Bayes, i.e. a shrinkage estimator. 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:

$$\hat{y}_j^{EB} = \lambda_j \hat{y}_j + (1 - \lambda_j)\frac{1}{J}\sum_j \hat{y}_j \tag{10}$$

where y represents the neighborhood-level parameter of interest, and 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.

D Model

Household i, living in place j, consumes quantity Q of energy in the form of four types of fuels: electricity (e), natural gas (n), other heating fuels (o), and motor gasoline (m). Each of these fuels has an emissions factor $\phi_{(jt)}$; these factors vary over time and place for electricity but are fixed along both of these dimensions for the other three fuel types. Household carbon emissions are therefore given by the following expression, with it subscripts temporarily supressed for easier legibility:

$$CO_2 = \phi^e_{jt} \cdot Q^e + \phi^n \cdot Q^n + \phi^o \cdot Q^o + \phi^m \cdot Q^m$$

Note that it is possible to re-express the above in terms of fuel shares, where for each fuel

$$s^f = \frac{Q^f}{\sum_f Q^f}$$

And therefore

$$CO_2 = \left(\sum_f s^f \cdot \phi^f_{(jt)} \right) \cdot Q$$

where, as before, Q represents total energy consumption across the four fuels.

Returning to Equation 3

$$\ln Q_{it} = a_j + \sum_{f \in \mathcal{F}} \rho^f_j \cdot \ln P^f_j + X_{it}\beta + \tau_t + \alpha_i + \varepsilon_{it}$$

it follows that

$$\ln CO_{2_{it}} = \ln \left(\sum_f s^f_{it} \cdot \phi^f_{(jt)} \right) + a_j + \sum_{f \in \mathcal{F}} \rho^f_j \cdot \ln P^f_j + X_{it}\beta + \tau_t + \alpha_i + \varepsilon_{it}$$

I add and subtract log of the average emissions factor, $\bar{\phi}_j$, which I used in the simplified exposition of the model in **??**, and rearrange terms to get the following expression:

$$\ln CO_{2_{it}} = \ln \bar{\phi}_j + a_j + \sum_{f \in \mathcal{F}} \rho^f_j \cdot \ln P^f_j + X_{it}\beta + \tau_t + \alpha_i + \varepsilon_{it} + \ln \left(\frac{\sum_f s^f_{it} \cdot \phi^f_{(jt)}}{\bar{\phi}_j} \right)$$

Observe that if not for the last term, this expression would be equivalent to **??**, but when household fuel shares vary, there is an interaction between household fuel shares relative to the average in the place where it lives, and place specific electricity emissions intensities. A household that disproportionately uses electricity wherever it lives will have a larger drop in emissions when moving from a place with relatively dirty electricity to a place with relatively clean electricity than the average household will. This variability gets absorbed by the error term in my regressions, and motivates the use of heteroskedastic errors.

E 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:

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

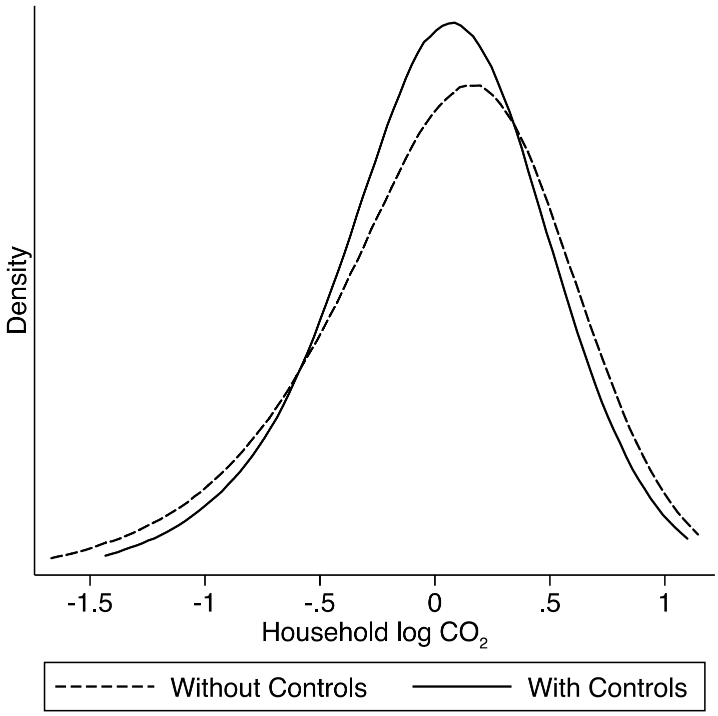
where $P_{ii} = x_i'(x_i x_i')^{-1} x_i$ is the observation leverage.

To reduce the computational burden of the KSS 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 KSS for a complete discussion of the leave-out estimator and JLA algorithm.

F Additional Figures and Tables

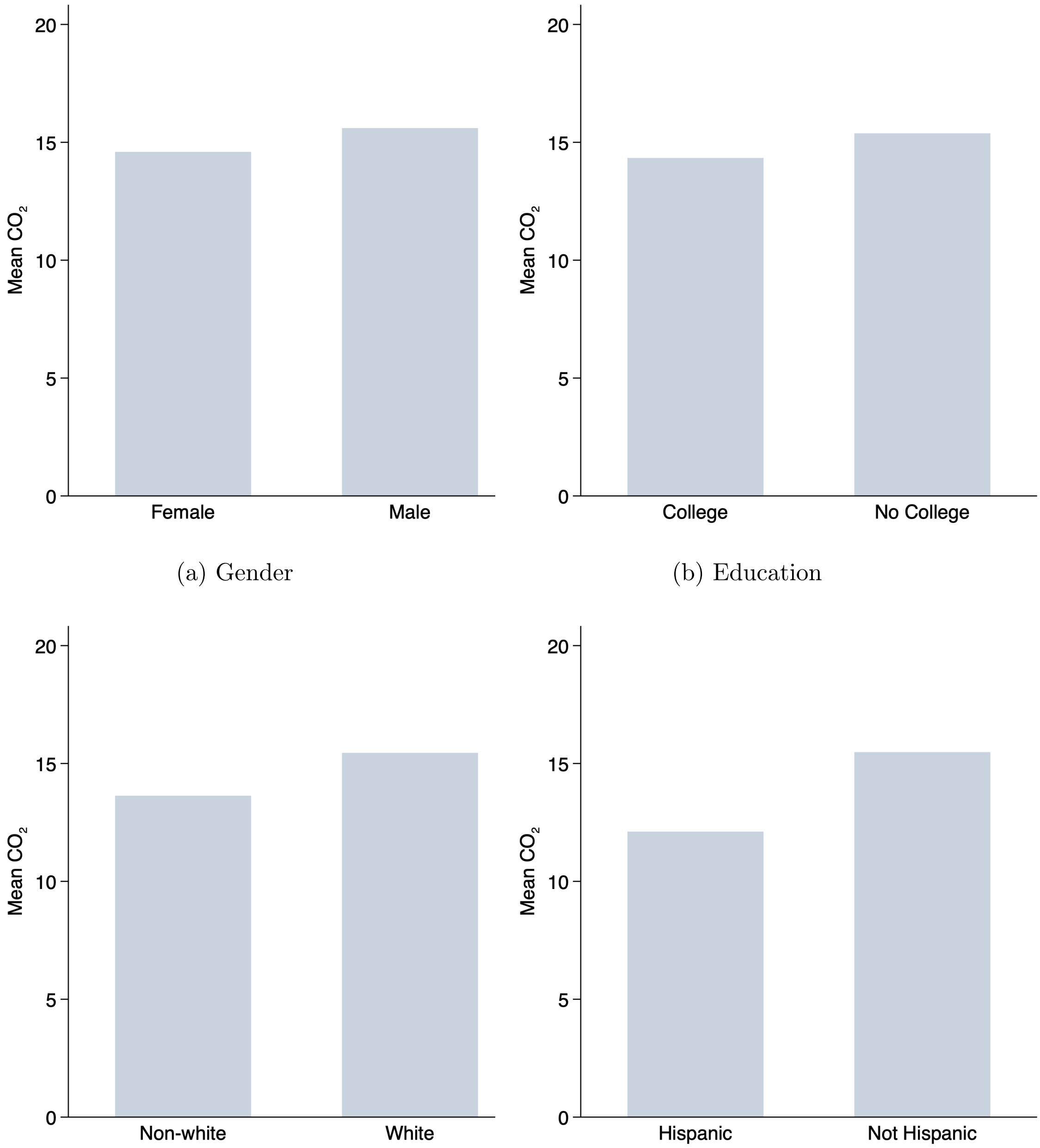
F.1 Additional Figures

Figure F.1: **Heterogeneity in Household Carbon Emissions**



Note: This figure shows Kernel Density estimates, using a Gaussian kernel function, of de-meaned household carbon emissions. The distribution is censored at the top and bottom 1% of observations in order to abide by Census Disclosure Avoidance rules. The dotted gray line labeled “Without Controls” corresponds to the distribution of log CO_2 conditional on year FEs only, and has a standard deviation of 0.59, while the solid line labeled "With Controls" conditions on observable household characteristics, and has a standard deviation of 0.52. Observable characteristics include age, gender, race, ethnicity, education, home owner status, household income, household size, and number of children.

Figure F.2: CO_2 Profiles by Demographic Characteristics (1/4)



(a) Gender

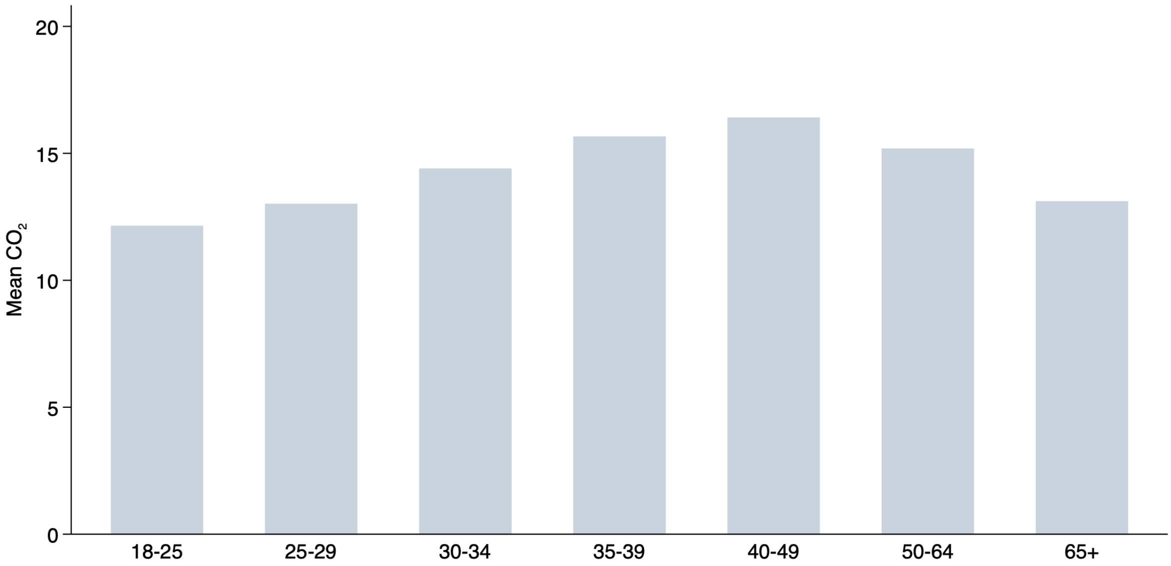
(b) Education

(c) Race

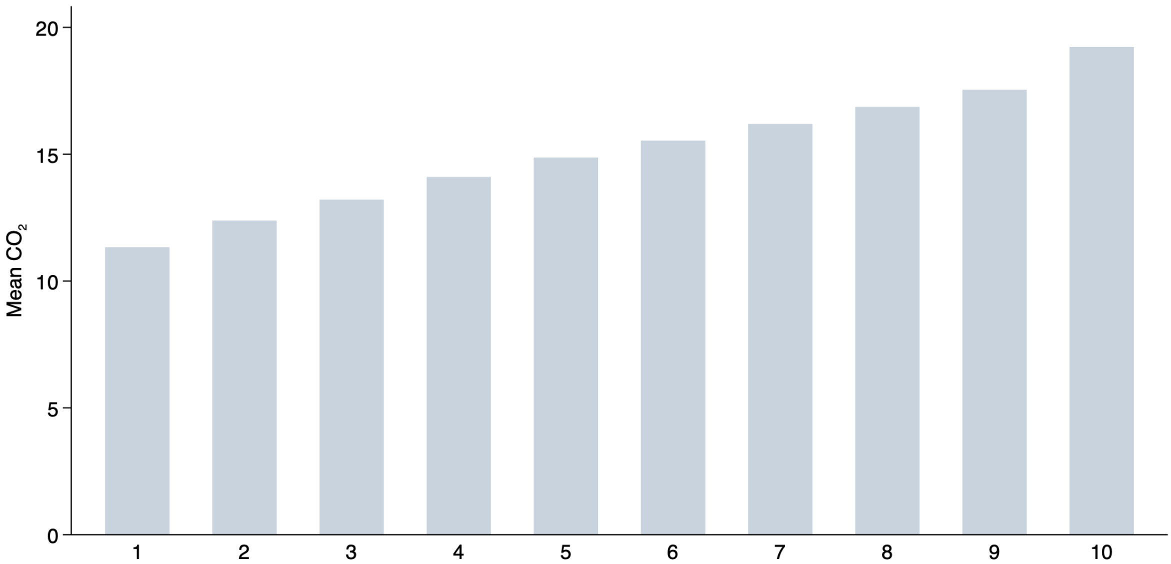
(d) Ethnicity

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 F.3: CO_2 Profiles by Demographic Characteristics (2/4)



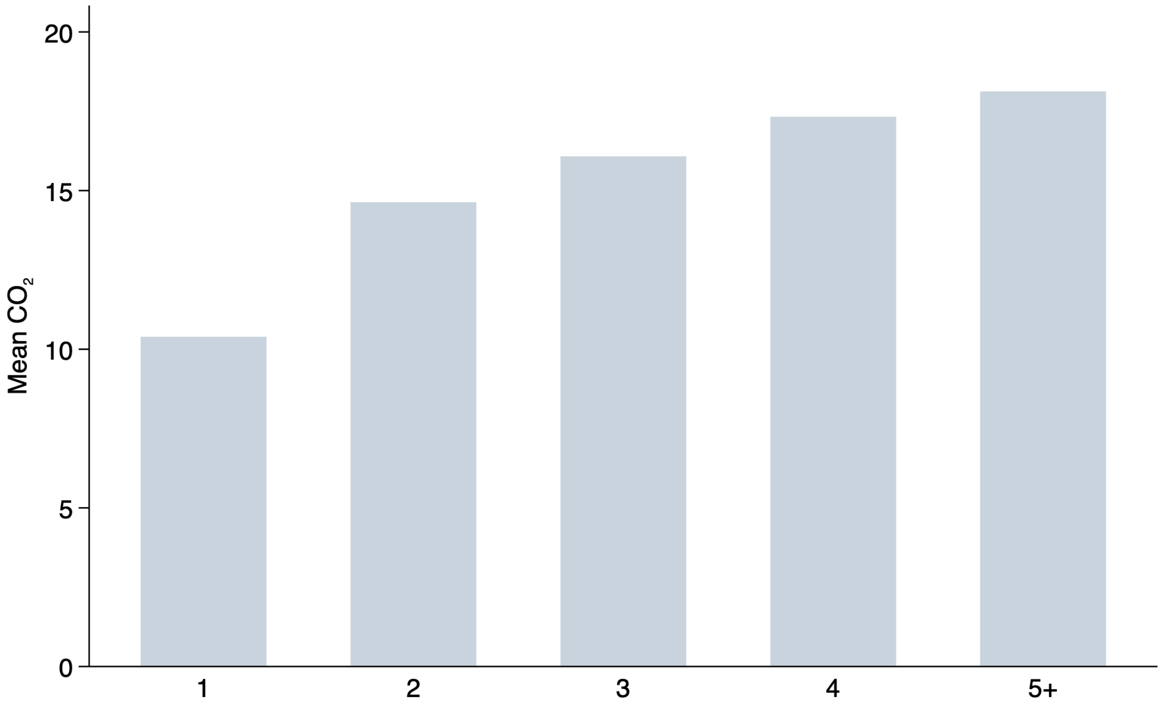
(a) Age



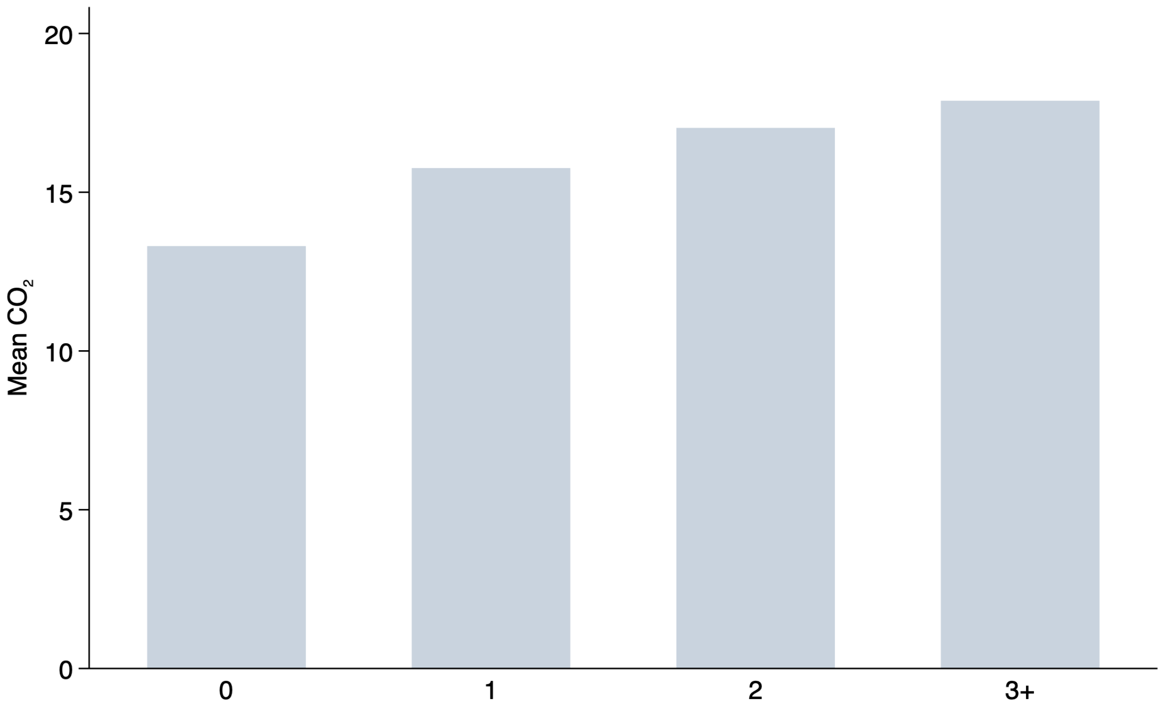
(b) Income Decile

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 F.4: CO_2 Profiles by Demographic Characteristics (3/4)



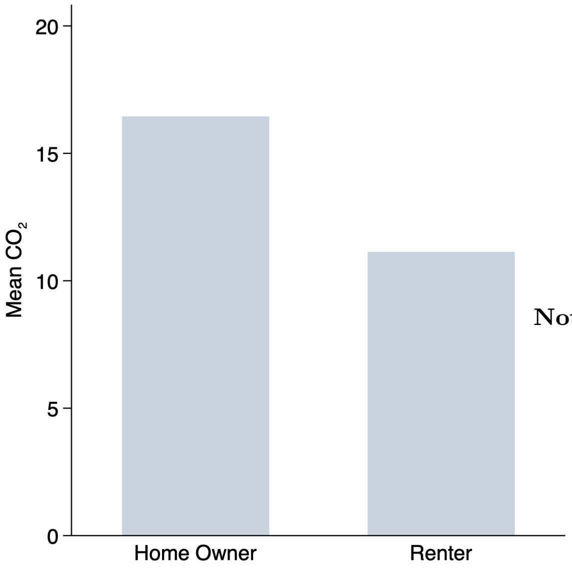
(a) Household Size



(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.

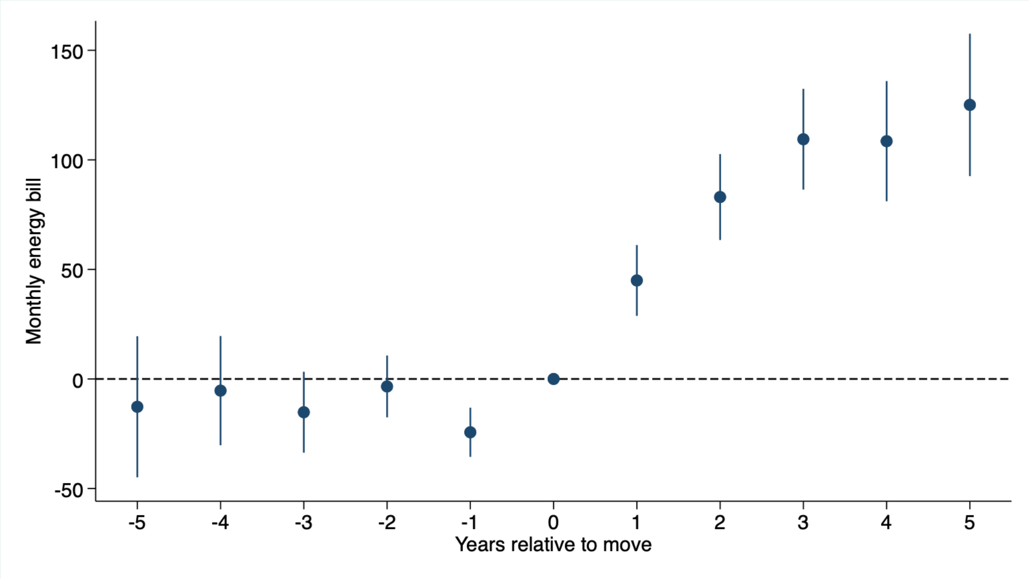
Figure F.5: CO_2 Profiles by Demographic Characteristics (4/4)



(a) Home Ownership

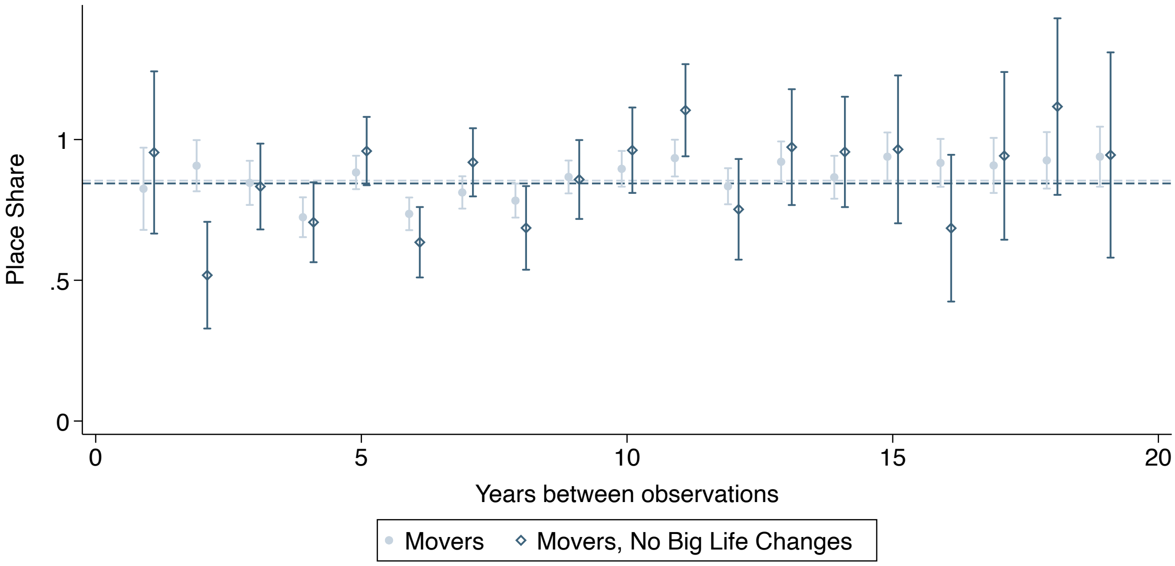
Note: This figure shows variation in household carbon emissions by homeowner status, highlighting that renters have lower emissions on average than homeowners. All estimates reflect the full sample, pooled 200-2019, weighted by Census sample weights.

Figure F.6: Energy Expenditures in Mover Households in the PSID



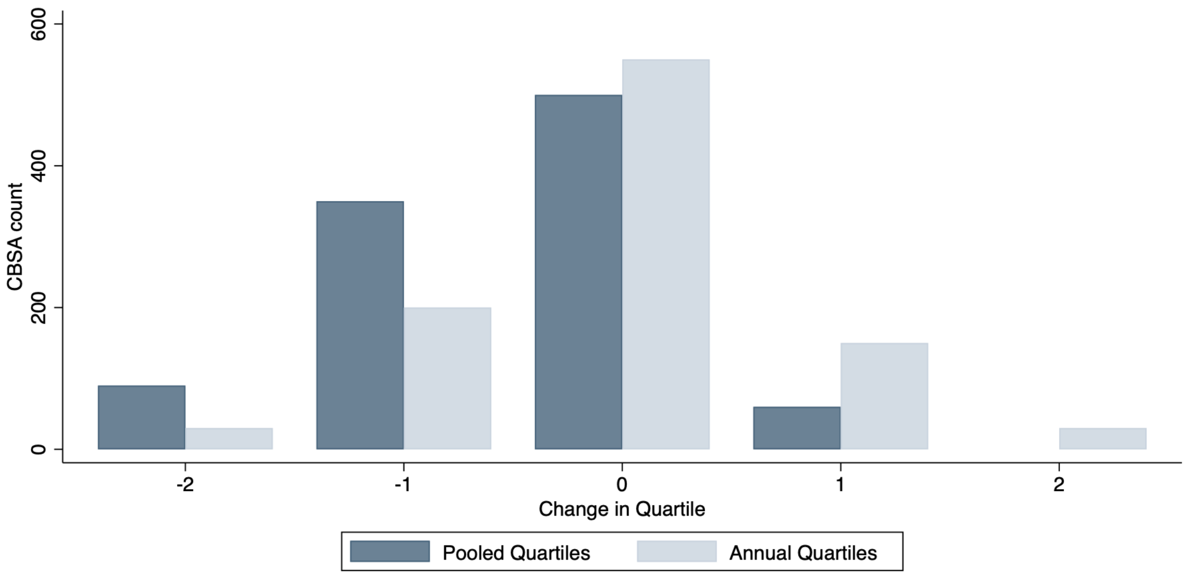
Note: I examine whether there are pre-trends in energy consumption for movers using data from the PSID, given data limitations in my baseline data. In particular, I test whether there are significant changes to monthly energy bills in the years prior to a move, after controlling for household characteristics 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).

Figure F.7: **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 circles are estimated from the model using the entire sample of movers. Coefficients plotted in the dark blue diamonds are estimated from the model using the sub-sample of movers with no change in the number of children, a less than 0.5 log point change in household income, and no change in home-ownership status between observations. All estimates are weighted using Census sample weights.

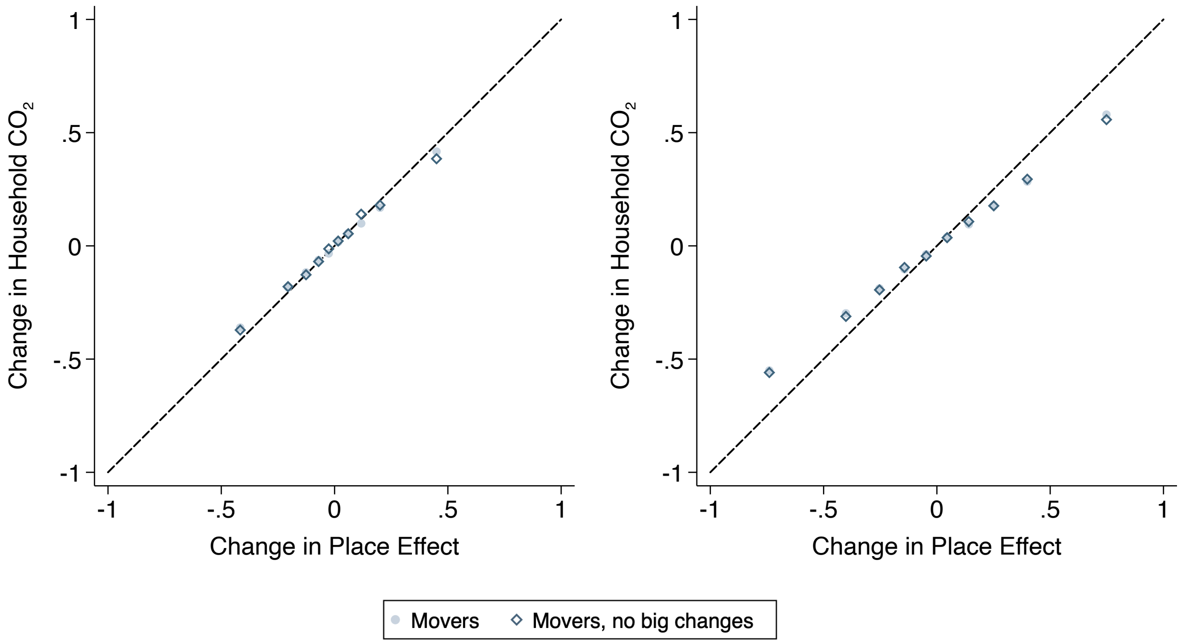
Figure F.8: **Changes in Time-Varying CBSA Effect Ranks**



Note: This figure shows the distribution of rank changes in time varying CBSA effects from the 2000-2004 period to the 2015-2019 period. The dark blue bars show changes in pooled quartiles, while the light blue bars show changes in within-year quartiles.

Figure F.9: **Place Effects vs. Household Carbon Emissions**

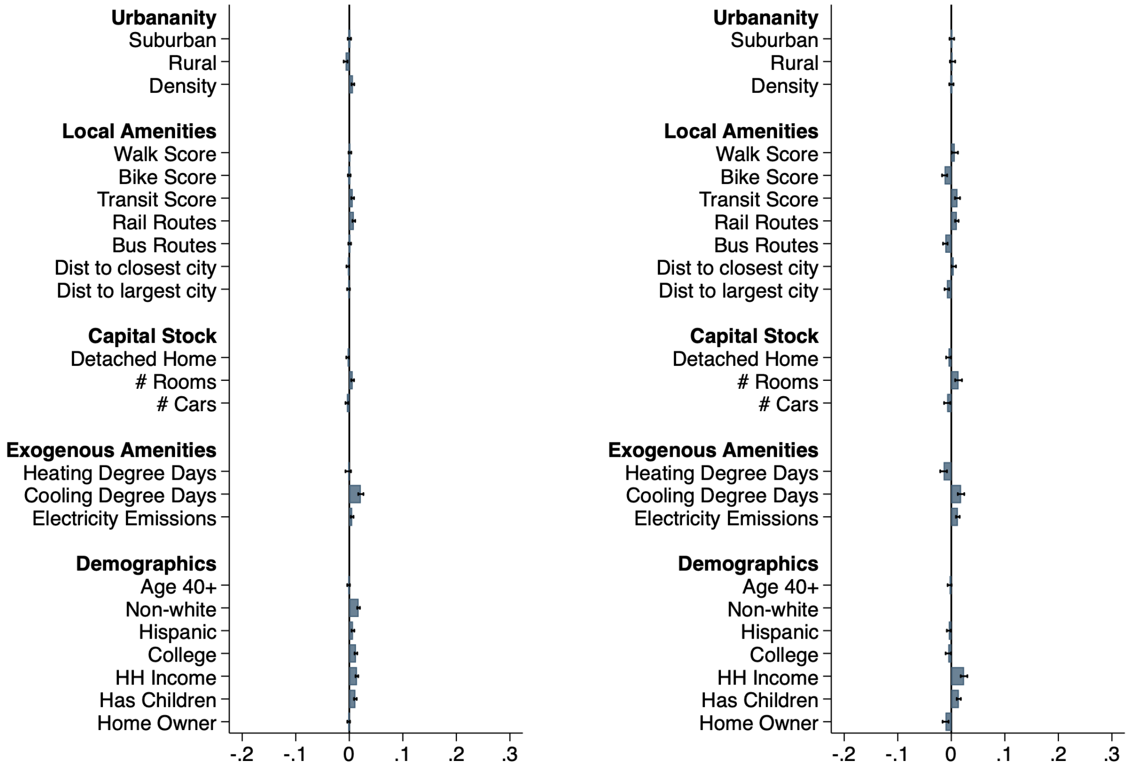
(a) **CBSA** (b) **Tract**



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 the KSS estimates of place effects. The two sets of points compare the full sample of movers (solid light grey circle) to the sub-sample of movers with no change in the number of children, a less than 0.5 log point change in household income, and no change in home-ownership status between observations (empty dark blue diamond). The dotted black line shows the 45°line. All estimates are weighted using Census sample weights.

Figure F.10: **Correlates of Unobserved Household Heterogeneity**

(a) Bivariate Regressions (b) Single Regression



Note: This figure presents estimates from OLS regressions of estimated tract effects on a set of observable place-based and household characteristics. Panel (a) shows results from separate bivariate regressions, while panel (b) shows results from a single regression on all covariates. All amenity variables are tract level means, normalized to have mean zero and standard deviation one, except the rural and suburban indicators, which are retained as indicators. Regressions are weighted using ACS sample weights.

F.2 Additional Tables

Table F.1: **Summary Statistics for Sample Dropped Due to Missing Energy Info**

	Full				Panel	Mover	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Elec in Rent	Gas in Rent	Gas in Elec.	All	CBSA	Tract
A: Demographics							
College	0.31	0.21	0.28	0.33	0.33	0.41	0.36
Age	42	40	39	43	44	40	41
White	0.72	0.67	0.68	0.74	0.83	0.83	0.81
Female	0.48	0.46	0.48	0.48	0.48	0.45	0.48
Household Income	85,010	51,800	65,310	98,860	100,800	102,700	100,100
Household Kids	0.9	0.7	0.6	1.0	0.9	0.9	0.9
Household Size	2.6	2.2	2.1	2.8	2.7	2.7	2.6
Homeowner	0.47	0.10	0.16	0.67	0.65	0.53	0.55
B: Outcomes							
Tons CO_2 - Commute	2.5	1.7	1.8	3.0	2.6	2.7	2.5
C: Intermediate Outcomes							
Detached Home	0.44	0.15	0.08	0.65	0.60	0.54	0.52
Use Electricity Only	0.04	0.24	0	0	0.09	0.14	0.12
Commute by Car	0.83	0.70	0.70	0.89	0.88	0.88	0.88
Commute Minutes	25.4	23.9	25.4	25.7	25.0	25.0	25.5
D: Place Characteristics							
Urban	0.34	0.44	0.47	0.28	0.26	0.25	0.28
Suburban	0.18	0.15	0.19	0.18	0.15	0.12	0.15
Rural	0.48	0.41	0.34	0.54	0.59	0.64	0.57
Walk Score	46.1	53.8	59.3	40.2	38.3	36.2	39.7
Bike Score	47.5	52.3	55.6	43.8	43.2	43.5	44.5
Transit Score	18.8	23.7	26.6	15.2	14.4	14.0	15.9
N Bus Routes	4.7	6.9	7.9	3.3	3.5	3.5	3.8
N Rail Routes	0.93	1.24	1.84	0.54	0.63	0.55	0.65
Cooling Degree Days	947	1,110	924	919	932	1,062	983
Heating Degree Days	5,028	4,754	5,236	4,993	5,272	5,021	5,151
N People	1,810,000	389,000	722,000	980,000	165,000	24,500	68,000
N Households	1,410,000	322,000	593,000	721,000	272,000	44,500	121,000
CBSAs	1,000	1,000	1,000	950	950	950	950
Tracts	68,500	53,000	56,500	55,000	49,000	26,000	43,000

Note: This table shows summary statistics for households dropped from the analysis as a result of having their electricity bills included in rent, their natural gas bills included in rent, or their natural gas bills included in their electricity bills. Column (1) shows statistics for the entire set of households who would've been in the full sample but got dropped for any one of those three reasons. Columns (2)-(4) show summary statistics broken out by group. Column (5) shows summary statistics for the sub-sample of column (1) who would've been in the panel sample if not for these unobserved bills, and columns (6) and (7) show households who would have been in the mover sample. All statistics are weighted by ACS household weights.

Table F.2: **Panel Statistics for Sample Dropped Due to Missing Energy Info**

		Movers	
	(1)	(2)	(3)
	Panel	CBSA	Tract
A: Sample Characteristics			
First Observed in 2000	0.08	0.13	0.12
Years Between Observations	8.6	10.4	9.9
B: Demographic Characteristics			
Age First Observed	40.6	34.8	35.9
Share with Large Change in Income	0.43	0.62	0.56
Share with Change in N Kids	0.44	0.51	0.51
Change in N Kids	0.03	0.30	0.23
Share Rent to Own	0.22	0.39	0.38
C: Mover Place Changes			
Δ Walk Score		-7.8	-7.7
Δ Bike Score		-5.1	-4.9
Δ Transit Score		-2.7	-3.6
Δ N Bus Routes		-0.96	-1.22
Δ N Rail Routes		-0.06	-0.15
Δ Tract Share Detached Home		0.07	0.08
% Moves Urban-to-Urban		0.19	0.28
% Moves Urban-to-Suburban		0.22	0.21
% Moves Suburban-to-Suburban		0.34	0.32
%Δ Cooling Degree Days		146	122
%Δ Heating Degree Days		-265	-159
N People	165,000	24,500	68,000
N Households	142,000	22,000	60,000
CBSAs	950	950	950
Tracts	49,000	26,000	43,000

Note: This table shows panel statistics for households dropped from the main analysis as a result of having their electricity bills included in rent, their natural gas bills included in rent, or their natural gas bills included in their electricity bills. The Column (1) shows statistics for households who would have been in the panel if not for unobserved billing information, while Columns (2)-(3) show statistics for households who would have been in the mover sample. All statistics are weighted by ACS household weights.

Table F.3: **Mean CO_2 – Movers vs. Stayers**

	CBSA Panel				Tract Panel			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mover	-0.07***	-0.05***			-0.11***	-0.08***		
	(0.001)	(0.001)			(0.001)	(0.001)		
Mover x Orig.			-0.12***	-0.05***			-0.08***	-0.04***
			(0.002)	(0.002)			(0.001)	(0.001)
Mover x Dest.			-0.04***	-0.04***			-0.03***	-0.03***
			(0.001)	(0.001)			(0.001)	(0.001)
Cons.	2.64***	2.58***	2.64***	2.59***	2.67***	2.61***	2.65***	2.61***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
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 lower carbon emissions than stayers, with a slightly less pronounced difference after controlling for differences in income and other demographic characteristics. Columns (3)-(4) and (7)-(8) present within-comparisons of stayers and movers within a given place. The "Mover x Orig." coefficient compares movers with stayers at their origin, while the "Mover x Dest." coefficient compares movers with stayers at their destination. Movers have lower emissions than stayers at both their origin and their destination. The origin difference looks more pronounced in the specifications without controls, but is effectively the same as the destination difference after controlling for observable household characteristics. All estimates are weighted by ACS household sample weights.

Table F.4: **Probability of Moving**

	(1) Moved CBSA	(2) Moved Tract
Decrease in Kids	0.02***	0.05***
	(0.001)	(0.001)
Increase in Kids	0.06***	0.18***
	(0.001)	(0.001)
Large Decrease in Income	0.06***	0.13***
	(0.001)	(0.001)
Large Increase in Income	0.09***	0.16***
	(0.001)	(0.001)
Rent $\rightarrow$ Own	0.16***	0.48***
	(0.001)	(0.002)
Constant	0.06***	0.19***
	(0.000)	(0.001)
R^2 (adj.)	0.05	0.17

Note: This table shows that households with a change in the number of children at home, a larger than 0.5 log point change in income, or who go from renting to owning are much more likely to move than stay. This is most pronounced for households who go from renting to owning their home, and is also more pronounced for positive changes in children or income than negative changes. All estimates are weighted by ACS household sample weights.

Table F.5: **Mover Origin and Destination Types**

(a) CBSA Movers

	To Rural	To Suburban	To Urban	Total Share
From Rural	0.03	0.09	0.01	0.13
From Suburban	0.07	0.44	0.09	0.60
From Urban	0.01	0.17	0.08	0.26
Total Share	0.11	0.50	0.18	1.00

(b) Tract Movers

	To Rural	To Suburban	To Urban	Total Share
From Rural	0.03	0.06	0.01	0.10
From Suburban	0.05	0.46	0.09	0.60
From Urban	0.01	0.17	0.14	0.32
Total Share	0.09	0.69	0.24	1.00

Note: This table shows shares of origin-destination tract types for CBSA movers (panel (a)) and tract movers (panel (b)). The most common type of move, for both CBSA and tract movers, is from a suburban tract to another suburban tract. Moves between urban and rural tracts are exceedingly uncommon. All estimates are weighted by ACS household sample weights.

Table F.6: **Place-Based Heterogeneity in CO_2 – Climate vs Electricity Emissions**

	CBSA				Tract			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel Sample								
Variance of log(CO_2)	0.31	0.31	0.31	0.31	0.31	0.31	0.31	0.31
Share attributable to places	0.163	0.146	0.080	0.074	0.228	0.217	0.151	0.152
Share attributable to hhs	0.496	0.496	0.494	0.494	0.363	0.362	0.362	0.361
Corr. of place and hh effects	0.013	0.036	-0.003	0.027	0.016	0.070	0.024	0.081
SD of place effects	0.23	0.21	0.16	0.15	0.26	0.26	0.22	0.22
Mover Sample								
Variance of log(CO_2)	0.35	0.35	0.35	0.35	0.33	0.33	0.33	0.33
Share attributable to places	0.140	0.125	0.046	0.039	0.218	0.220	0.145	0.155
Share attributable to hhs	0.136	0.129	0.160	0.156	0.099	0.098	0.102	0.101
Corr. of place and hh effects	0.073	0.102	0.048	0.084	0.084	0.160	0.084	0.161
SD of place effects	0.22	0.21	0.13	0.12	0.27	0.27	0.22	0.23
Climate		X		X		X		X
Electricity CO_2			X	X			X	X

Note: This table reports KSS estimates of variance components, deliniating between the contribution of local climate conditions vs the contribution of local electricity emissions factors. All specifications include demographic and household controls as well as time fixed effects. To ease comparison, Columns (1) and (5) replicate baseline estimates shown in columns (1) and (5) of Table 4, while columns (4) and (8) of this table replicate estimates accounting for the contribution of both climate and electricity emissions factors simultaneously (i.e. columns (2) and (6) of Table 4). Columns (2) and (6) of this table show estimates accounting for just the role of climate in the variance components, while Columns (3) and (7) show estimates accounting for just the role of electricity carbon emissions. All estimates are weighted by ACS household sampling weights.

Table F.7: **Place-Based Heterogeneity in CO2 – No Bias Correction**

	CBSA				Tract		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel Sample							
Variance of log(CO_2)	0.31	0.31	0.31	0.31	0.31	0.31	0.31
Share attributable to places	0.175	0.086	0.090	0.188	0.523	0.449	0.453
Share attributable to hhs	0.558	0.554	0.554	0.557	0.748	0.747	0.746
Corr. of place and hh effects	-0.026	-0.028	-0.027	-0.026	-0.417	-0.429	-0.427
SD of place effects	0.23	0.16	0.17	0.24	0.40	0.37	0.37
Mover Sample							
Variance of log(CO_2)	0.35	0.35	0.35		0.33	0.33	0.33
Share attributable to places	0.153	0.051	0.053		0.461	0.399	0.403
Share attributable to hhs	0.505	0.502	0.502		0.582	0.581	0.581
Corr. of place and hh effects	0.008	-0.008	-0.005		-0.291	-0.298	-0.294
SD of place effects	0.23	0.13	0.14		0.39	0.36	0.37
Climate + Electricity CO_2		X	X			X	X
Price Index			X				X
Time-Varying FEs				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. All estimates are weighted by ACS household sampling weights.

Table F.8: **Place-Based Heterogeneity in CO_2 – Alternate Outcome Definitions**

	CBSA				Tract			
	V(y)	S(ψ_j)	S(α_i)	corr.	V(y)	S(ψ_j)	S(α_i)	corr.
Panel Sample								
Baseline	0.31	0.163	0.496	0.013	0.31	0.228	0.363	0.016
Electricity Emissions Estimates								
Marginal Emissions	0.34	0.236	0.470	-0.016	0.337	0.295	0.343	-0.011
Variable Prices	0.33	0.155	0.498	0.018	0.321	0.218	0.363	0.026
Decreasing Block Prices	0.35	0.150	0.503	0.016	0.341	0.216	0.373	0.011
Increasing Block Prices	0.31	0.186	0.480	0.013	0.303	0.246	0.345	0.033
Selected Block Prices	0.32	0.159	0.500	0.014	0.311	0.220	0.366	0.024
Transportation Emissions Estimates								
Commute from hrs	0.29	0.160	0.487	0.015	0.287	0.206	0.367	0.024
Commute from hrs, fixed num.	0.29	0.168	0.487	0.009	0.286	0.213	0.366	0.020
MPG from NHTS (dem. only)	0.32	0.169	0.495	0.008	0.313	0.235	0.365	0.006
MPG from NHTS (geo. only)	0.32	0.173	0.493	0.011	0.313	0.240	0.363	0.007
MPG from NHTS	0.32	0.174	0.490	0.011	0.312	0.238	0.360	0.010
Total Transportation from NHTS	0.20	0.177	0.468	0.037	0.200	0.218	0.376	0.015
Mover Sample								
Baseline	0.35	0.140	0.136	0.073	0.333	0.218	0.099	0.084
Electricity Emissions Estimates								
Marginal Emissions	0.39	0.219	0.136	0.004	0.378	0.281	0.094	0.025
Variable Prices	0.36	0.133	0.135	0.077	0.349	0.209	0.100	0.088
Decreasing Block Prices	0.39	0.131	0.136	0.069	0.372	0.207	0.105	0.062
Increasing Block Prices	0.34	0.161	0.137	0.067	0.329	0.240	0.097	0.091
Block Prices; selected	0.35	0.138	0.134	0.073	0.336	0.211	0.097	0.086
Transportation Emissions Estimates								
Commute from Hrs	0.32	0.142	0.132	0.070	0.310	0.202	0.101	0.089
Commute from hrs, fixed num.	0.32	0.149	0.132	0.063	0.309	0.210	0.099	0.075
MPG from NHTS (dem. only)	0.36	0.145	0.138	0.070	0.341	0.221	0.102	0.077
MPG from NHTS (geo. only)	0.36	0.150	0.138	0.076	0.341	0.227	0.100	0.083
MPG from NHTS	0.35	0.150	0.138	0.075	0.341	0.225	0.097	0.089
Total Transportation from NHTS	0.21	0.169	0.171	0.0772	0.207	0.214	0.118	0.0862

Note: This table reports KSS estimates of variance components, using a variety of different outcome definitions to test robustness of the baseline estimates. Overall variance of the outcome, the variance component of place, the variance componanent of households, and the correlation between place and household effects are reported at the CBSA level in columns (1)-(4), respectively, and at the tract level in columns (5)-(9). Estimates are reported for both the full panel sample (top half of table) and the mover only sample (bottom half of table). Baseline estimates are replicated in the first row of each sample's section to ease comparability. Outcome variants are grouped into two categories: one which impacts residential carbon emission estimates, and one which estimates transportation emissions estimates. All estimates are weighted by ACS household sampling weights.

Table F.9: **Place Correlates w/ Observable Characteristics 1/2**

	College	Age > 40	Non-white	HH Income	Has Kids	Homeowner
Density	-0.002*	0.004***	0.006***	0.033***	0.017***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Suburban	0.014***	0.005***	-0.025***	0.015***	-0.010***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Rural	0.001	0.009***	-0.025***	-0.028***	-0.019***	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Walk Score	0.002*	0.006***	-0.022***	0.025***	0.007***	-0.021***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Bike Score	0.025***	-0.011***	-0.004***	0.027***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Transit Score	0.006***	0.004***	0.027***	0.030***	-0.007***	0.006***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
CDD	-0.015***	0.000	-0.063***	-0.044***	0.003	0.025***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
HDD	-0.030***	-0.003***	-0.103***	-0.070***	-0.012***	0.033***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Elec. CO_2	-0.018***	-0.020***	0.008***	-0.062***	-0.010***	-0.004***
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
N Rail Routes	0.007***	0.002***	-0.012***	0.036***	0.002	0.010***
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
N Bus Routes	0.016***	-0.003***	-0.012***	0.016***	-0.013***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Detached Home	-0.043***	0.009***	-0.000	-0.085***	0.013***	0.047***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N Rooms	0.121***	0.055***	-0.004***	0.309***	0.032***	0.071***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N Vehicles	-0.036***	0.017***	-0.055***	0.076***	0.043***	0.041***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Dist. Closest City	-0.013***	-0.005***	-0.011***	-0.041***	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Dist. Largest City	0.005***	0.012***	0.010***	0.064***	0.007***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.25***	0.63***	0.14***	11.29***	0.53***	0.78***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
Adj. R^2	0.451	0.465	0.326	0.643	0.155	0.767

Note: This table reports correlation coefficients between tract-level mean observable household characteristics and a detailed vector of observable place characteristics. All estimates are weighted by ACS household sampling weights.

Table F.10: **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

This table presents the ten most populous CBSAs as of 2020, which are used in the analysis evaluating how overall emissions would change under different distributions of place effects. Source: https://en.wikipedia.org/wiki/Combined_statistical_area