The Geography of Consumption and Local Economic Shocks: The Case of the Great Recession

Authors Abe Dunn, Mahsa Gholizadeh, U.S. Bureau of Economic Analysis[1]

Date January 2023

Abstract We estimate across-county spending flows between firms and consumers for every county in the United States, providing a new consumption link that has not been studied previously. We highlight the importance of this link by estimating the effect of changes in local housing wealth on consumption and employment from 2001 to 2019. We generally find that the effect from changes in housing wealth crosses borders to affect consumption and employment in a pattern consistent with our spending flows. However, we find potential consumers who reside outside the local commuting zone disproportionately affect local spending and employment during the Great Recession.

Keywords Spending Flows, Housing Wealth, the Great Recession

JEL Code R1, R2, E2, E3

1. We would like to thank Christian Awuku-Budu, Mary Bohman, Ben Bridgman, Eva De Francisco, Lasanthi Fernando, Dennis Fixler, Aaron Flaaen, Kyle Hood, Matt Knepper, Justine Mallatt, and Scott Wentland for comments. We would also like to thank seminar participants at the Meeting of the Urban Economics Association (October 2020), Bank of Italy and Federal Reserve Board joint conference on Nontraditional data (November 2020), and the Winter Meetings of the Econometric Society (December 2020). We would also like to thank Fiserv for the use of their data and the substantial work of the employees at Palantir who helped manage and work with the enormous Fiserv database, especially Albert Altarovici, Brady Fowler, and Daniel Williams. We would also like to thank Ledia Guci for some preliminary analysis of Fiserv data for purposes of measuring regional consumption.

1. Introduction

Consumers regularly travel to consume outside their home county, affecting both revenues and employment across different locations. In this paper we argue that this consumption link across geographic markets has important implications for economic measurement. Although there are rich data sources that provide detailed and nearly complete coverage of both consumption (e.g., the Economic Census(EC)) and employment (e.g., the Quarterly Census of Employment and Wages (QCEW)) for every county in the United States, they are centered around the location of the firm and not the location of the consumer. Data centered around the firm do not identify the primary cause of the change in revenues, which is rooted in the consumption patterns of consumers both local and afar. To study the consumption link between counties, and to fully utilize these rich data sources, we introduce a complementary new data source on spending flows between consumers and firms for all counties in the United States. We combine traditional and novel data sources to study how changes in housing wealth over the 2001 to 2019 period are differentially affected by local and more distant economic shocks to consumption. This is the first paper to construct a comprehensive data set of consumption flows across counties for the entire United States, and also the first to measure the importance of shocks to consumption transmitted across geographic markets.

We construct the spending flow estimates using card transaction data from Fiserv, one of the largest card transaction intermediaries in the United States, with well over $2 trillion in card volume going through their system worldwide annually. Typically, when a firm uses Fiserv services, all associated debit and credit card transactions go through their systems. At a micro level, these data include information about both the location of consumers' residence as well as the physical location of firms, allowing the measurement of cross-county spending flows. The data are aggregated and anonymized across firms and consumers by county and by three-digit North American Industry Classification System (NAICS) industry codes. While there are around 4.5 million establishments underlying the data, they still represent a sample of the total establishments in the United States. In this paper, these card data are combined with EC data and other sources to build representative estimates of spending flows across all counties in the United States for 15 three-digit NAICS categories for the year 2015. The focus of the analysis is on brick-and-mortar stores and excludes the nonstore retail category such as e-commerce firms (e.g., Amazon and eBay).[2]

[2]Our paper is related to Dolfen et al. (2019) that uses detailed Visa data on consumer location and spending habits

The 15 NAICS categories we study account for a total of about 79 percent of consumer spending nationally after excluding housing, health care, and financial services.[3] On average, we find that around 68 percent of expenditures take place in the same county in which consumers reside and 81 percent of spending occurs within the Commuting Zone (CZ) of the consumer.[4] While these statistics show that spending typically occurs near where consumers reside, spending outside the home county still makes up a substantial share of total spending and may vary greatly depending on the local geography and industry. This turns out to be extremely important for some industries, such as accommodations, where only 14 percent of spending occurs in a person's home county, but less important for other industries, such as food and beverage stores, where 83 percent of spending takes place in the home county.

To better understand across-county spending patterns, we estimate descriptive regressions that highlight factors that influence spending across counties. Distance is an important explanatory variable affecting where consumers spend, and the distance traveled to consume greatly depends on the industry, as highlighted in Agarwal et al. (2017). We also find important heterogeneous factors that influence across-county spending. For example, we find that counties with higher per capita incomes are less sensitive to distance and more willing to travel outside of CZs. We also find CZs are economically meaningful borders that have an influence on consumption patterns, even after flexibly accounting for distance. Numerous factors influence across-county spending patterns, suggesting simple proxies, such as distance, will not accurately capture the across-market consumption link that is reflected in the spending flow data.

Next, we apply the spending flow data to analyze how economic shocks are transmitted across geographic markets. Specifically, we examine the effects of changes in housing wealth on spending and employment over the 2001–2019 period, including the period of the Great Recession (GR). We follow the well-known work of Mian et al. (2013), Mian and Sufi (2014), and Guren et al. (2020) to study how local changes in housing wealth affect local spending and employment, although the focus of our paper is distinctly

across locations to assess the gains in e-commerce. They find large gains from the introduction and expansion of e-commerce. In contrast, our paper focuses more explicitly on brick-and-mortar stores for two reasons. First, the coverage of our data set is more complete and accurate for brick-and-mortar stores. With additional data, the basic approach laid out in our paper could be adapted to e-commerce sales. Second, during the earlier part of our sample, e-commerce was a much smaller share of spending. According to Dolfen et al. (2019) online sales were around 5 percent in the 2007–09 period of focus in our study and the share was less than 7 percent in 2015. We also exclude the nonstore retail and airline categories, which are the two industries with the highest share of e-commerce sales according to Dolfen et al. (2019).

[3]These percentages were computed for the years 2001 to 2019 and remained fairly stable throughout this period.

[4]A commuting zone is a geographic area comprised of counties intended to capture local labor markets where counties are grouped together based on cross-county commuting patterns (Tolbert and Sizer (1996)).

centered around the consumption link across markets. Our paper starts with spending estimates from official sources that are centered around the location of a firm and considers the housing wealth of all consumers, including both local consumers and those traveling from other counties, in determining the effect of housing wealth changes on firm revenue and employment. The across-county flow estimates provide detailed information regarding the location of potential consumers across counties. Similar to Guren et al. (2020), we apply both instrumental variable (IV) and panel methods to account for the potential endogeneity from changes in the local economy on housing wealth. In general, we find that firms are affected by changes in housing wealth of their potential consumers, as proxied for using spending flows, even if their potential consumers reside in another county. While this connection is clear in theory, this is the first paper to empirically measure these across-market effects.

Spending flows are important to obtain a measure of the housing wealth change relevant to firms. Firms in high consumption export counties – those counties with higher levels of consumption driven by residents outside the county (e.g., Clark County, Nevada) – are relatively unaffected by housing wealth changes within their own county, but are instead affected by housing wealth changes from the counties of their potential consumers outside the local market. Alternatively, those counties with low consumption exports are less affected by housing wealth changes in other counties, and are primarily affected by housing wealth changes in their own county. As a result, ignoring spending flows across counties tends to reduce the elasticity of housing wealth changes on spending and employment.

We use this unique data source to investigate the heterogeneous effects of changes in housing wealth. In particular, we allow for heterogeneous effects from potential consumers that reside outside of the local geographic market, as defined by CZs, and we also allow for heterogeneous effects during the GR. We find that the effects of the housing wealth change on spending is particularly large during the GR, more than two times the effect, potentially due to a greater multiplier effect during the downturn. We generally find that changes in housing wealth affects spending and employment in a pattern consistent with our spending flows. However, during the GR we find a disproportionately large effect on spending and employment for housing wealth changes from potential consumers that reside outside of the local CZ.

One possible explanation for the larger effect outside of the CZ during the GR is that trips outside the CZ may be viewed as more luxury goods, which individuals disproportionately cut back on during the GR.

To investigate this idea, we divide the data into industry categories, including local industries typically purchased near the home county (e.g., grocery stores) and export industries more often consumed away from home (e.g., accommodations and restaurants). We find that the housing wealth changes by potential consumers away from home primarily affects the export industries, arguably more of a luxury good, and not the local industries, arguably necessity goods, and this effect is especially large during the GR. We also find that spending on export industries tends to be more elastic to housing wealth changes, relative to spending on local industries. One alternative we consider is that spending by consumers outside the CZ could generate a different multiplier effect (e.g., the change in spending in accommodations affects local incomes, and subsequently affects local grocery consumption). If the multiplier effect is strong, we should expect it to affect all industries, including both local and export industries, but we find no evidence of this spillover effect on local industries. Regardless of the cause, our estimates imply that the across-market consumption effect is economically important.

Our study is the first to show that an astonishing 19 percent or more of the spending and employment effects are generated by housing wealth changes outside of the local CZ. Additionally, during the GR, spending from consumers outside the local CZ had a disproportionate impact, accounting for 43 percent of the decline in spending and employment. Therefore, not accounting for this across-market effect will systematically understate the effects of local economic shocks, and this may be especially important during large changes in the economy. Moreover, ignoring these flows leads to inaccurate estimates regarding the location of firms affected.

Our key results are robust to a number of alternative specifications. For instance, as a robustness check for how we define local markets, rather than using CZs we look at the distance between firms and consumers and find disproportionately larger effects from potential consumers that reside more than 100 miles away. In addition to our main instruments, we apply IVs directly from Guren et al. (2020) and Saiz (2010). We also consider alternative estimates of spending flows, which take into account potential biases from card transaction data, as well as shifts in spending patterns over time. While estimates change slightly across specifications, the main findings are robust to these alternatives.

This paper relates to a large literature that studies geographic markets for specific industries, which often focus on particular cities or regions.[5] Our paper contrasts with this literature as we study several

[5] Allcott et al. (2019) - groceries and food, Houde (2012) - gasoline stations, Davis et al. (2019) - restaurants, Davis (2006), and many papers in the health care sector, where the geography of markets is featured prominently in the literature

industries across all counties in the United States. Our paper also relates to Agarwal et al. (2017) who use card transaction data to investigate spending patterns across geographic markets and for multiple industries. Similar to our work, they find consumer mobility varies substantially across industries. However, the focus of Agarwal et al. (2017) is in understanding factors that influence the spatial structure of geographic markets (e.g., how storability of goods affects firm location by industry). In contrast, our paper focuses on how observed spatial linkages in consumption have implications for the transmission and measurement of local economic shocks across areas.

Our paper also relates to a growing literature using more granular and real-time data sources on both consumption, employment, and economic activity (Aladangady et al. (2021), Baker et al. (2020), Chetty et al. (2020), and Cox et al. (2020), and Bureau of Economic Analysis County GDP (Aysheshim et al. (2020))). Understanding the consumption link across local geographic markets will only increase as additional rich data sources become available.

2. Data

A primary data source used in this paper is from Fiserv, a card transaction intermediary, which processes transactions for establishments around the world, including credit and debit that includes all types of card transactions (e.g., Visa, MasterCard, Discover, and others).[6] The unit of observation on the Fiserv system is a single transaction at a firm. Once a firm signs up for Fiserv services, typically all card transactions go through the Fiserv system. However, we do not see the data at this micro-level of detail. In collaboration with Palantir, a software company that specializes in the management and analysis of big data, Fiserv has aggregated and anonymized transaction data to the county level in a way that provides detailed and meaningful economic information while still protecting the identity of both firms and individuals. The data contain 4.5 million firms and billions of transactions that span all states in the United States and the District of Columbia. Although the database includes transactions from e-commerce (primarily captured in NAICS category 454 for nonstore retailers), we exclude e-commerce firms due to relatively poor coverage.

(see Gaynor et al. (2015) for a review)).

[6]Other electronic card transactions are also included, such as prepaid gift cards and Electronic Benefit Transfer, a system used by recipients of government aid, such as Supplemental Nutrition Assistance Program (SNAP). The coverage of all card types makes the Fiserv data source more representative than data sources only focused on credit cards, as credit cards are more predominately used by high-income populations (Matheny et al. (2016)).

For counties within the United States, the home location of each card holder is estimated based on the transaction history of the card using information on all transactions across all industries.[7] A home location algorithm is applied to predict the home county of the consumer and is optimized based on a subset of cards within the Fiserv database, where the home location of the card holder is known.[8]

The Fiserv data we use are from 2015 and include aggregate county-level information by three-digit NAICS industry. For every county-industry combination, the database contains an estimate of the share of revenues for establishments in that county coming from consumers residing in one of the more than 3,000 counties in the United States. For instance, these data include information on the share of accommodation revenues (NAICS 721) in Clark County, Nevada (i.e., Las Vegas) coming from cards in Orange County, California. The total shares across all areas add up to one. We study 15 select three-digit industries that are important contributors to consumer spending and have good coverage in the Fiserv data.[9] These select 15 industries account for 64 percent of personal consumption spending after excluding housing and financial services. They account for 79 percent of consumer spending if health care is also excluded.[10] Notably, the spending data exclude the purchase of cars, which are typically not paid for by debit or credit cards.

To protect the anonymity of firms and consumers in the Fiserv data, information on the transaction flows across geographies are suppressed in some cases. This is especially common in areas where revenues for an industry in a particular county are small.[11] Using information from the EC, we find that flows are suppressed for 14 percent of spending for these select industries.

For those county-industry pairs with suppressed flow information, we apply flexible models based on observable transactions in the database across counties to generate estimates of transaction flows across all counties of the United States. To impute spending flows, we use information for those industries in which transactions are observed in a county (e.g., the category restaurants and bars (NAICS 722), where

[7] See data appendix section A.2 for more details.

[8] As an additional check on the home-location algorithm, we also have a version of the data based solely on those consumers for whom the home location is known. These data are also similarly aggregated and anonymized to the county level. We find the two estimates of spending flows to be quite similar.

[9] We consider flows with good coverage to be the flows with lowest number of suppressed observations which happen to be mostly retail and some services (e.g., restaurants and hotels).

[10] The 15 select industries account for 41 percent of total consumption, including all consumption categories based on data from 2001 to 2019.

[11] The specific rule is that there needs to be 10 or more firms in that three-digit NAICS, with no firm having more than a 20 percent market share. In addition to these criteria, some firms have agreements with Fiserv to "opt out" of their data being used, and their data are not included.

98 percent of spending is unsuppressed), combined with information on distances traveled, revenues estimated based on the EC, and other covariates to impute the remaining spending flows. For instance, if we are missing accommodations flows in an area, but we observe flows of restaurant services, we can use information on the restaurant service flows between areas, combined with information on how far individuals typically travel to purchase accommodation services, as well as other information such as population and revenues, to impute the flows for accommodation services. We have explored a variety of flexible models to impute this missing information and selected our current specification using a hold-out sample and cross-validation. We chose the method with the lowest mean squared error in our hold-out sample. Additional details regarding coverage and the imputation method are described in the appendix sections A.2.1 and A.3.

In addition to card transaction data from Fiserv, we also construct estimates of county-level spending and employment for the 15 select industries. For the employment data we use the QCEW, which is an official U.S. Bureau of Labor Statistics (BLS) data source that includes quarterly employment and wage estimates for 95 percent of jobs at the county level and by detailed NAICS industry category. The source of the QCEW is administrative data from state unemployment insurance programs. While nearly all employment is included, it excludes select areas such as proprietors and the self-employed. QCEW is the same data source used by Guren et al. (2020). In contrast to Guren et al. (2020) who focus exclusively on retail employment, we study employment for our 15 select industries. Our version of the QCEW data includes complete coverage of all counties at the three-digit industry level from 2001 to 2019.[12]

For the spending estimates we use the Geographic Area Series of the 2002, 2007, 2012, and 2017 ECs that contain information on revenues and establishment counts by NAICS industry and county-level geographies.[13] Next, to estimate spending for all of the intercensal years, we use the QCEW growth rates in wages to interpolate county-level growth rates by NAICS. Specifically, the annual QCEW growth rates are rescaled by the ratio of the annualized 5-year EC growth rate to the annualized 5-year QCEW growth rate. This method essentially anchors the annual growth rates in QCEW wages to match the

[12] Guren et al. (2020) must drop some counties due to data disclosure issues. (Mian and Sufi, 2014) use County Business Patterns (CBP) data from Census, which also provides information on employment and earnings. The CBP data are annual, and QCEW data are quarterly, and there are also slight differences in coverage. Overall, the two sources are similar for the industry categories studied here.

[13] A subset of counties in the EC contain suppressions at the three-digit industry level, representing about 1 to 2 percent of spending. The estimates for suppressed counties are imputed using state-level EC data and QCEW data to create estimates for all counties in the United States for these benchmark years. Additional details are discussed in section A.1.2.

average growth rate in the EC. A similar method is applied in the Bureau of Economic Analysis (BEA) regional economic accounts and private sector organizations such as Moody's and the Survey of Buying Power, as historically there is a high correlation between the growth rate in the EC and wages from the QCEW. In the appendix section A.1, we discuss the imputation of EC data and show that wage data perform quite well in predicting growth rates in revenues based on the EC years.

The growth rates for employment are distinct from our spending estimates because the spending estimates are matched to the EC. Therefore, we view the local spending estimates as an additional contribution, which contrasts with Guren et al. (2020) and Chodorow-Reich et al. (2021) that use employment as a proxy for spending. As we explain later, this distinction matters as we generally find a higher elasticity of housing wealth with respect to spending than for employment.[14]

Finally, we use a county-level housing price index from the Federal Housing Finance Agency (FHFA) (Bogin et al. (2019)). The index is a repeated purchase index available every year from 2001 to 2019 for around 2,700 counties. We supplement this data with housing price data from Zillow to fill in missing price information.[15] The remaining counties are small rural counties with relatively little economic activity. For missing counties, we assume the price change is equal to the median price change across counties in the same CZ.[16]

3. Descriptive Statistics

Table 1 shows total estimated spending in 2015 by NAICS industries, in which the total is decomposed into the percent of spending where spending flows are observed, imputed, and unknown (i.e., spending flows could not be imputed). Across-county spending flows are observed for about 86 percent of spending; therefore no additional imputation is required. About 14 percent of the spending flows are

[14]Employment is a good proxy, but it is imperfect. Even at an aggregate level, consumption and wages are distinct. Aggregate correlations of personal consumption growth and wage growth from BEA data show a correlation of 0.82 (Table 2.3.5. Personal Consumption Expenditures and Table 2.2B. Wages and Salaries from 1998 to 2021). The national data also reveal some important points of divergence, such as during the initial year of the COVID-19 pandemic when wages rose in 2020, even as consumption fell.

[15]The Zillow data was downloaded from: https://www.zillow.com/research/data/. In general the FHFA data is more complete than the Zillow data, but there are select observations where Zillow price information is available and FHFA data are not. In cases where pricing data is available from both Zillow and FHFA sources the correlation in the price change is 0.95. See the appendix section A.8 for additional details on the housing price data.

[16]This imputation has very little effect on the estimates and allows us to examine effects of housing wealth changes across all counties. We prefer not to exclude these more rural counties that have missing data because consumers in these counties also travel to consume, so we chose to impute the price change rather than drop observations.

imputed using the method described in the appendix section A.3. For less than 0.1 percent of spending, it was not possible to impute the flows. The amount of imputation needed varies greatly by industry. For food services and drinking places (NAICS 722), we observe 98 percent of spending flows, but we observe just 63 percent for performing arts, spectator sports, and related industries.

Table 1. Spending by Industry

	Total $Millions	% Observed	% Imputed	% Unknown
Accommodation (NAICS 721)	225,765.3	85.59	14.34	0.07
Ambulatory Health Care Services (NAICS 621)	960,110.3	95.74	4.21	0.04
Amusement, Gambling, and Recreation Industries (NAICS 713)	119,829.8	86.72	13.21	0.07
Building Material and Garden Equipment and Supplies Dealers (NAICS 444)	341,689.4	70.10	29.76	0.14
Clothing and Clothing Accessories Stores (NAICS 448)	232,950.2	95.87	4.11	0.02
Food Services and Drinking Places (NAICS 722)	660,300.4	98.31	1.68	0.01
Food and Beverage Stores (NAICS 445)	720,160.9	87.92	12.04	0.04
Furniture and Home Furnishings Stores (NAICS 442)	126,712.8	82.53	17.34	0.13
Gasoline Stations (NAICS 447)	523,039.2	84.00	15.94	0.06
General Merchandise Stores (NAICS 452)	749,349.3	66.71	33.16	0.13
Miscellaneous Store Retailers (NAICS 453)	138,279.0	95.36	4.61	0.03
Performing Arts, Spectator Sports, and Related Industries (NAICS 711)	104,468.5	63.40	36.53	0.07
Personal and Laundry Services (NAICS 812)	110,372.0	94.68	5.20	0.12
Repair and Maintenance (NAICS 811)	181,223.3	89.66	10.23	0.11
Sporting Goods, Hobby, Book, and Music Stores (NAICS 451)	103,789.1	82.05	17.86	0.09
Total	5,298,039.5	85.99	13.95	0.07

Notes: The total spending for 2015 estimates are based on our estimate of total spending by firms in each county. Additional detail regarding the 2015 spending estimates by industry are provided in the appendix section A.3. The percent imputed for each NAICS category is computed as the total revenues where spending flows are not observed across all counties, divided by the total revenues across all counties.

Representativeness of Spending Flows — The spending flow data could potentially over-represent high-income populations as higher income groups tend to pay more often using card transactions, relative to lower income groups (Matheny et al. (2016)). This bias is likely to be larger in databases with only credit cards, which are used more by high-income populations, while debit cards are used more widely. Relative to credit card data, Fiserv data are more representative, as they include credit cards, debit cards and also Electronic Benefit Transfer (EBT), which is similar to a debit card system used by recipients of government aid, such as Supplemental Nutrition Assistance Program (SNAP) participants. While we find some evidence that the Fiserv data may be skewed toward higher income populations, we find that the bias is small. Using the raw Fiserv data and controlling factors related to Fiserv presence in the market, we estimate that counties in the 90th percentile of income per capita have about 0.6 percent more coverage than counties in the 10th percentile of income per capita. This amount should be compared to the mean coverage across counties and industries where spending is observed, which is 10.5 percent.[17] See appendix section A.4 for additional details.

[17] Counties where coverage is observed are important because those are the counties used for imputation. If coverage is

The main approach to address the potential bias is to rescale the data. Expenditures must be scaled to be representative of spending in the area because Fiserv data is based on a sample of firms. We scale all of the data to the level of EC, so that firms in both low-income and high-income areas match the EC. The implicit assumption for rescaling is that the establishments included in the Fiserv data in each county are a representative of firms in the county, so that the resulting rescaled spending flows provide accurate estimates. We investigate the rescaled data by using the implied consumption to income ratios after this rescaling.[18] If the bias toward higher income areas is large, we should expect a low consumption to income ratio in the low-income counties and a high consumption to income ratio in high-income counties. In fact, we find the share of consumption to income to be larger in the lowest income quartile counties (mean of 0.36) relative to the highest income quartile counties (mean 0.33). We validate these levels using external data on income to consumption ratios from the consumer expenditure survey where we find income to consumption ratios are fairly constant across geographic areas of different income levels (see appendix section A.4 and Table A4 for more details).

Adjustment for Potential Card Transaction Bias in Spending Flows — The descriptive statistics provide evidence that rescaled spending flow estimates are not biased, but it is possible that even after rescaling, high-income individuals could account for a greater share of the flows. As an additional robustness check, we produce an alternative set of spending flows to adjust for this potential issue. Specifically, we impose an assumption that the ratio of income to consumption is the same in all counties, which we believe is a reasonable approximation based on external evidence.[19] After imposing this assumption and adjusting consumer spending (so the ratio of consumption to income is constant) we then use an accounting relationship between consumption and firm revenue to adjust spending flows. With this adjustment, areas that show a very low consumption to income ratio (perhaps due to low card use) will be scaled up, while areas that show a very high consumption to income ratio (perhaps due to more card use) will be scaled down. To be clear, suppose there is a county where we observe the consumption to income ratio is particularly low. A potential reason for the low consumption to income ratio could be that consumers do not use card transactions as frequently in that county, so we rescale this spending by consumers in that

0, this could be due to suppression rules or limited Fiserv presence. Considering areas where coverage is 0 or positive, the median coverage in spending across states is 8.5 percent.

[18] We use the spending flows to send spending to the location of the consumer to calculate the consumption to income ratio for every county.

[19] While consumption to income ratios vary across individuals, averaging across individuals in a county likely reduces this variation considerably, which we find from external data. See appendix section A.4 for details. In addition, in the appendix section A.5, using a simple accounting relationship we show that assuming consumption is a fixed share of income is highly correlated with independent consumption estimates based on the spending flow information.

county so the consumption to income ratio matches the average. Using the newly estimated spending amounts, we recalculate flows for each county, where the county that is scaled up will now account for a greater share of the flows. The methodology for adjustment is similar to a biproportional RAS adjustment used in estimating input-output tables, Stone (1961). This rescaling and the accounting relationship are described in sections A.5 and A.6 of the appendix. Despite the distinct methodology, the correlation of the main flow estimates and the adjusted flows is 0.90. As we discuss in more detail later, we find that our main results are unaffected by this adjustment.

3.1. Geography of Consumption

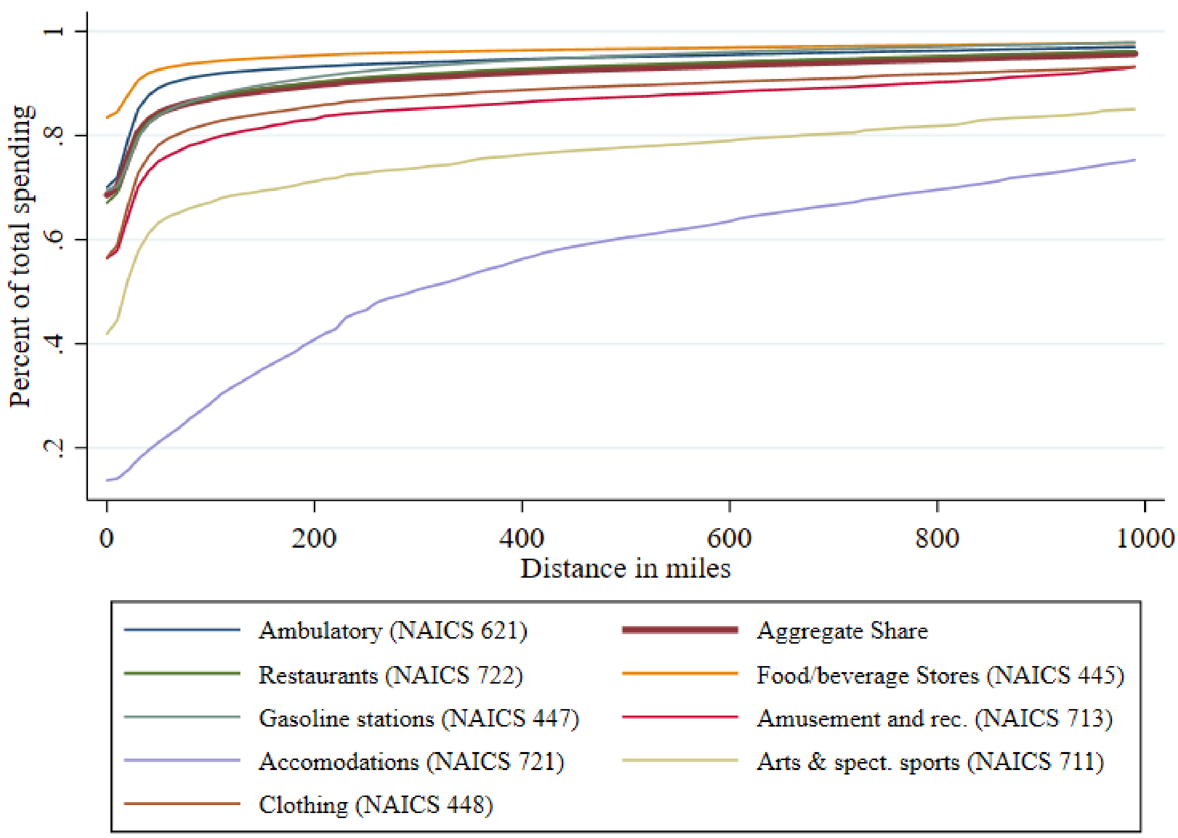
The amount individuals travel to consume varies greatly by industry. Figure 1 shows the cumulative distribution of spending by NAICS for the first 1,000 miles away from a firm's home location, where the location within each county is based on the population centroid.[20] Categories such as food and beverage stores and general merchandise stores are among those in which most consumption occurs locally. The finding that preferences for food and beverage stores (i.e., grocery stores) is highly localized relates to the literature on food deserts (Allcott et al. (2019)). In contrast, consumers tend to travel farther for arts and spectator sports, and accommodations. Additional descriptive statistics of spending by distance are included in section A.7 of the appendix.

While there is considerable variation across industries, both the geography of different locations as well as the concentration of different industries and populations across the United States leads to large variation in how much consumers spend outside of their home county. Figure 2 shows the share of consumption that is consumed in the home county of the consumer, with darker shades indicating that more consumption is occurring in the home county. Figure 2 shows that for most counties more than 50 percent of consumption occurs in the home county, and this is particularly true in large cities.[21] In contrast, in rural areas consumers tend to travel farther to consume.

[20] We truncated the distribution at 1,000 miles to better highlight the differences across industries.

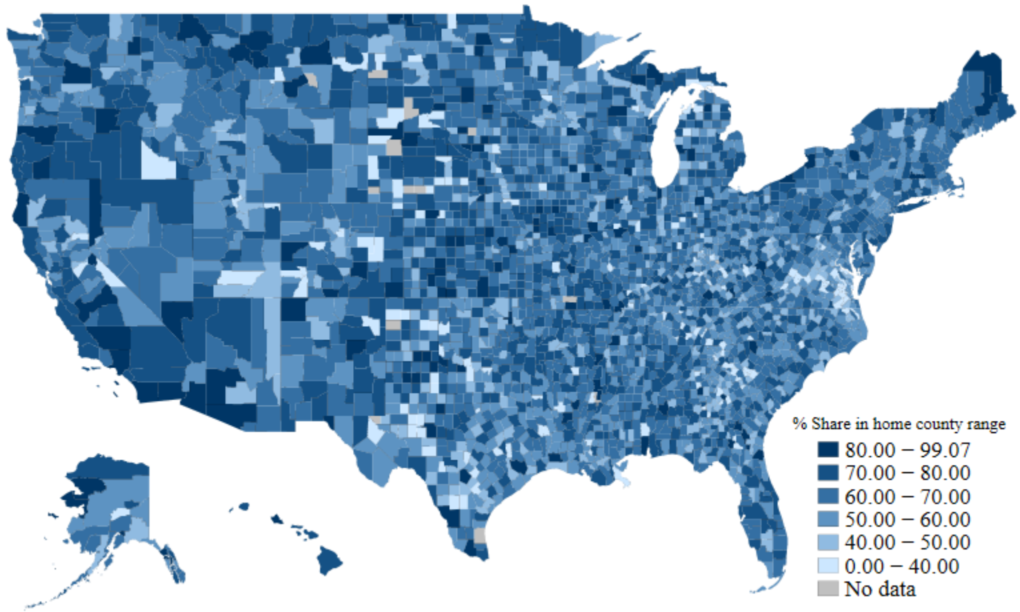
[21] We construct the same figure based on firm revenue share occurring in the home county, but it essentially shows the same pattern, in which most consumption occurs locally in more populated areas of the United States.

Figure 1. Cumulative Distribution of Spending by Distance, Truncated at 1,000 Miles



Notes: The cumulative spending is calculated for each NAICS category based on the total share of spending occurring within a distance radius of the merchants' location where the location in each county is determined by the population centroid of the county. The figure shows 9 of the 15 NAICS categories in our data. Additional details by industry are in Table A5 of the appendix. Spending tends to be more local for food/beverage stores and ambulatory services, whereas for accommodations and arts and spectator sports it tends to have a greater share of revenue coming from more distant locations.

Figure 2. Share of Consumer Consumption in Home County

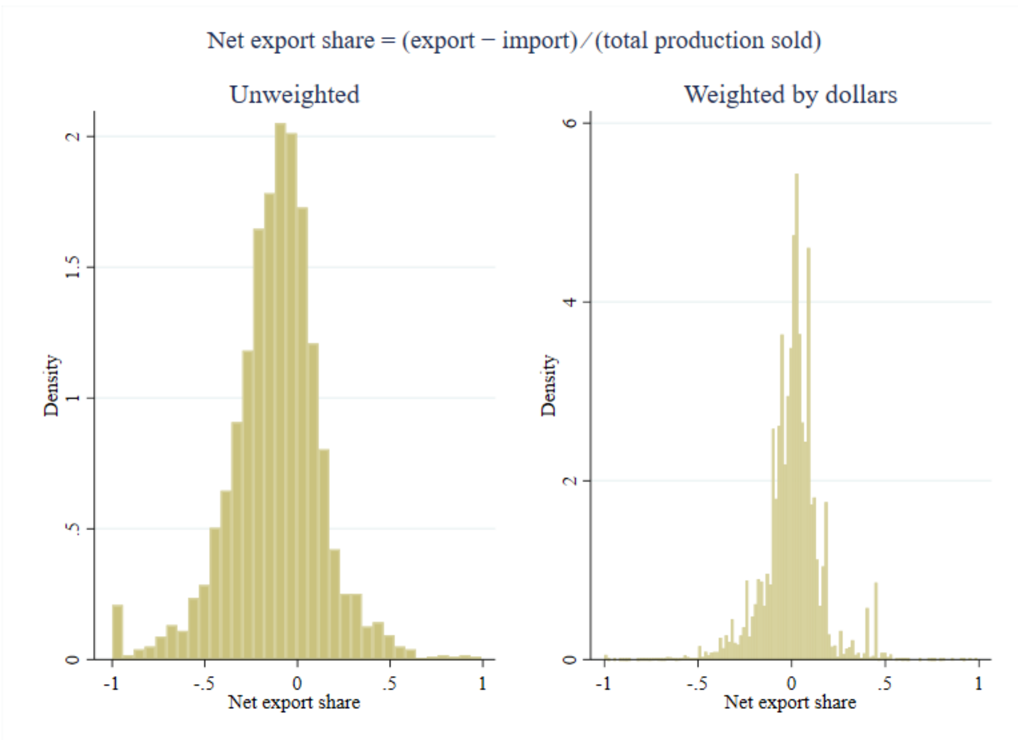


Notes: We use spending flow estimates for all counties and all 15 NAICS categories to calculate the total spending by consumers in their home county, relative to total spending across all counties. We then take the ratio of home county consumption to total consumption. Darker shaded areas indicate more consumption is occurring in a consumer's home county. In general, more rural areas tend to have lighter shading as consumers tend to travel to consume, while more urban areas have darker shading.

Counties may differ greatly in how much spending flows in and out, and the net difference may not be symmetric. We summarize the share of net flows by calculating the total exports (i.e., firm revenues from consumers outside of the county), minus imports (i.e., the total amount of revenue from consumers leaving the county), divided by the total amount of consumption sold in the county. Figure 3 shows the distribution of net exports across the United States, both unweighted and weighted by the consumption sold in the county. Figure 3 shows a large variation across the United States, especially for more rural counties, which are more represented in the unweighted distribution.

Next, we show this distribution in the form of a map, with Figure 4 showing the distribution of net export shares across the United States with darker shades of red indicating a high net export share, while darker shades of blue indicate a higher import share. Here we see many expected patterns, including high export shares from places like Nevada and Hawaii, which are top tourist destinations. Overall, these patterns in Figures 3 and 4 highlight the idea that counties are interconnected through consumption, indicating the potential importance of across-county consumption patterns.

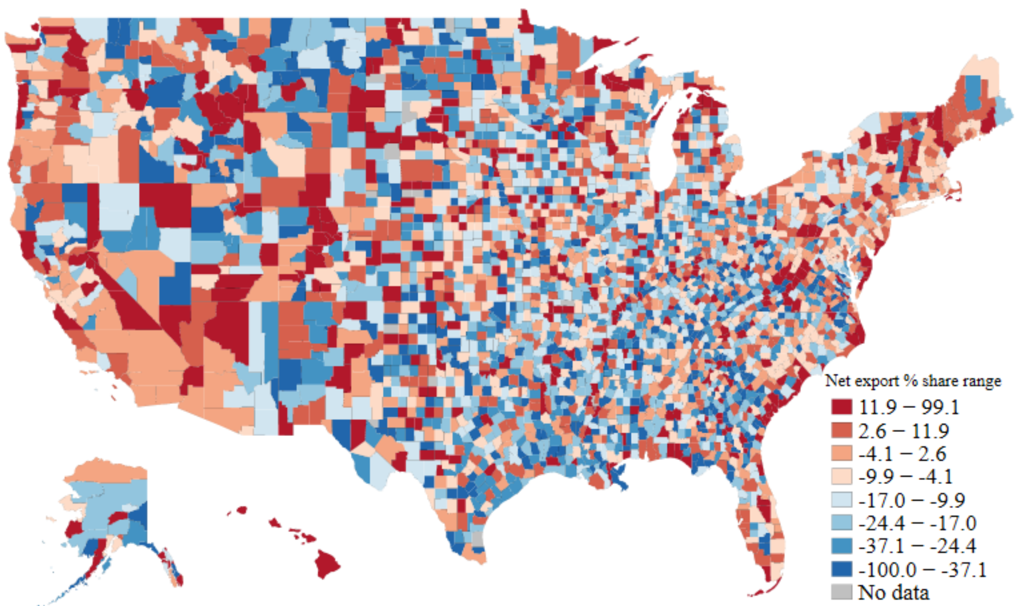
Figure 3. Distribution of Net Export Share



Notes: The net export share of each county is calculated as total exports (i.e., firm revenues from consumers outside of the county), minus imports (i.e., the total amount of revenue from consumers leaving the county), divided by the total amount of final consumption sold in the county. The distribution has been winsorized at -1 to avoid the long tail of rural counties that import most of their consumption.

Figure 4. Distribution of Net Export Share in the United States

Red – high export share Blue – high import share



Notes: The net export share of each county is calculated as total exports (i.e., firm revenues from consumers outside of the county), minus imports (i.e., the total amount of revenue from consumers leaving the county), divided by the total amount of final consumption sold in the county. Positive values indicate higher net export share and are shown in red and negative values indicate lower net export share and are shown in blue. Top tourist destinations (e.g. Nevada and Hawaii) with high export shares are shown in dark red, as expected. and more remote counties with high import shares are shown in dark blue.

3.2. Descriptive Across-County Regression

We estimate a descriptive gravity regression to highlight some of the key factors driving spending patterns across counties, where the key dependent variable is the log of firm revenue, $log(R_{i,j,n})$, for industry n in county j being sold to consumers in a county i. The regression takes the following functional form:

$$log(R_{i,j,n}) = \beta^1_{i,j,n} \cdot X_{i,n} \cdot X_{j,n} + \beta^2 \cdot W_{i,j,n} \cdot d_{i,j} + \gamma_{j,n} + \sigma_{i,n} + \epsilon_{i,j,n} \tag{1}$$

The regression equation (1) includes county-industry fixed effects for the firms, $\gamma_{j,n}$, and county-industry fixed effects for the consumers, $\sigma_{i,n}$. These fixed effects account for the firm-specific and consumer-specific factors that explain the total amount of revenues produced and consumed for each industry, respectively. The term $X_{i,n}$ captures variables specific to the consumers in county i, and $X_{j,n}$ captures variables specific to the firms in county j. The variable $W_{i,j,n}$ captures factors that might affect how consumers respond to distance $d_{i,j}$ between areas. The term $\epsilon_{i,j,n}$ is the error term.

Table 2 shows the results of four regression estimates. We run a series of regressions from the simplest to more flexible models to demonstrate some key patterns. Before estimating the model with county-industry fixed effects, ($\gamma_{j,n}$ and $\sigma_{i,n}$), we estimate a very simple model to confirm basic patterns in the data. The first model includes four variables: the log of firm revenue in county j for industry n (i.e., total spending by all consumers at firms in county j and industry n), log of personal income in the consumer county i, an indicator if the firm and consumer are in the same county (i.e., "home county"), and an indicator if the firm and consumer are in the same CZ (i.e., "home CZ"). The estimates are shown in column (1) and, as expected, there is a highly significant relationship with personal income as well as with firm revenue. The regression also demonstrates that location matters, as residing in the home CZ has a significant effect on spending, and residing in the home county has an additional effect, implying that the consumers location within a CZ also matters.

Table 2. Regression of Log Spending Across Counties by Industry

	(1) (log (Spend))	(2) (log (Spend))	(3) (log (Spend))	(4) (log (Spend))
(log (Personal Inc. Cnty))	0.936***			
	(0.0286)			
(log (Tot. Revenue Cnty))	0.636***			
	(0.0206)			
Home County	3.342***			
	(0.0807)			
Home CZ	5.530***		0.794***	3.598***
	(0.0744)		(0.0786)	(0.691)
(Restaurants (NAICS 722) . (Home County))		0.639***		
		(0.168)		
(Accommodations (NAICS 721). (Home County))		-2.906***		
		(0.176)		
(NAICS 722 . (Home County))		-1.518***		
		(0.129)		
(NAICS 445 . log(Distance+1))		-1.440***		
		(0.0315)		
(NAICS 721 .log(Distance+1))		-1.301***		
		(0.0253)		
(NAICS 722 . log(Distance+1))		-1.610***		
		(0.0297)		
(log (Avg Inc Cons Loc) . Home CZ)				-0.824***
				(0.193)
(log (Avg Inc Cons Loc) . log(Distance+1))				0.309***
				(0.0293)
(log (Avg Inc Cons Loc) . log(Avg Inc Firm Loc))				0.970***
				(0.121)
(log (Pop. Cons Loc) . log(Pop. Firm Loc))				0.161***
				(0.00582)
Urban to Urban				0.215***
				(0.0329)
N	25612841	25613313	25613313	25612680
R squared	0.490	0.768	0.779	0.799
Log(Distance+1) Controls	No	Yes	Yes	Yes
County FE	No	Yes	Yes	Yes
Flex Distance Controls	No	No	Yes	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The estimates show the regression of log spending between consumer and firm locations by industry on a number of covariates. All estimates are clustered based on the firm county. The first column includes just the four variables listed. The three columns (2)-(4) follow the specification in equation (1) include county fixed effects for firm and consumer locations by industry. For column (2), only coefficients for select industries are shown, although all industries are included in the regression. In columns (3) and (4), all of the flexible interactions of distance and industry are not shown. All estimates are clustered based on the county of the firm and weighted by the population in the firm county, but estimates are similar when alternative weighting strategies are applied.

Column (2) of Table 2 estimates equation (1) that includes county-industry fixed effects for consumers ($\sigma_{i,n}$) and firms ($\gamma_{j,n}$). The regression also includes the home county variable interacted with industry, and industry fixed-effects interacted with the $log(distance + 1)$, to capture how distance is related to consumption across different industries. While interactions are included for all industries, Table 2 shows the relationship for just three categories (food/beverage stores (NAICS 445), accommodations (NAICS 721), and restaurants (NAICS 722)). The regression matches the patterns in Figure 1, showing grocery stores (food/beverage, NAICS 445) are highly local, with a positive and significant relationship on the home county dummy, while accommodations (NAICS 721) are typically consumed away from home, as indicated by the negative coefficient on the home county dummy. After controlling for the likelihood of consuming in the home county, consumption declines with distance for all industries. These additional controls explain a much greater share of the variation in the data, with an R-squared of 0.77.

Column (2) assumes a log relationship on distance, but there may be nonlinear effects of distance. In column (3) of Table 2 we include flexible distance dummy variables interacted with industry fixed effects to account for non-linearities in distance traveled for each industry.[22] We also include the dummy variable for the home CZ. The more flexible regression generates a slight improvement in the R-squared to 0.78. Even after numerous nonlinear controls are included, the CZ is significant in explaining spending patterns, demonstrating the economic relevance of the CZ beyond just capturing geographic proximity. Demonstrating the relevance of the CZ is important as the next section of the paper differentially considers the effect of economic shocks generated inside and outside the CZ.

In column (4), we investigate additional heterogeneous factors that influence spending flows across areas. The specification in column (4) includes the flexible distance and industry controls included in column (3), but also considers a number of additional factors: an indicator for the home CZ interacted with the log of per capita income, an interaction of distance with log of per capita income, an interaction of log per capita income of the consumer county and the log per capita income of the county of the firm, an interaction of log population of the consumer county and log population of the firm county, and finally an indicator if it is an urban-to-urban transaction (i.e., the firms and the consumer both reside in urban counties).[23] We find evidence that areas with higher per capita income are less sensitive

[22] We include miles traveled fixed effects for each 50-mile increments up to 500 miles and then 100-mile increments up to 2000 miles, then an over-2000-mile dummy. Each distance dummy is interacted with industry dummies to absorb all distance-related factors.

[23] The definition of urban we apply is 500 or more people per square mile. This definition essentially captures all non-rural areas, as the U.S. Department of Agriculture (USDA) defines rural as less than 500 individuals per square mile.

to travel cost, as indicated by the negative coefficient on the interaction with the home CZ and the positive coefficient for the interaction with distance. This has implications for how spending patterns might change during economic fluctuations, as these estimates imply the cost of travel falls as per capita income rises. We also find a positive association between high-income areas, as indicated by the positive coefficient on the interaction of log per capita income at the consumer and firm locations, indicating that higher income consumers tend to consume more where other higher income consumers reside. Finally, population and population density also matter, as indicated by the positive and significant coefficient on the population interaction and urban-to-urban dummy variables. While these heterogeneous patterns are statistically significant and interesting, they do not improve the R-squared by a substantial amount, with the R-squared increasing from 0.779 to 0.799. This suggests the spending patterns are complex and not easily captured, either by simple controls, such as CZ or distance, or even nonlinear functions of multiple variables. In the next section, we show how these complex spending flows are important for understanding how economic shocks are transmitted across counties and affect spending and employment.

4. Effects of Changes in Housing Wealth on Spending and Employment

In this section we analyze the effects of housing wealth changes on spending and employment. We specifically focus on the differential effects of housing wealth changes from consumers in the home CZ compared to more distant locations, as well as differential effects during the GR. The analysis centers on firms, rather than consumers, as the spending flow information from Fiserv is based on firm-level data, and the focus on firms allows us to analyze how more distant changes in housing wealth affect firms, relative to changes in the home market.

The dependent variable is a growth rate computed as percent changes between years t and $t-2$ following Mian et al. (2013) and Mian and Sufi (2014):

$$\Delta Y_{j,t} = \frac{Y_{j,t} - Y_{j,t-2}}{Y_{j,t-2}}$$

The variable $\Delta Y_{j,t}$ is either the growth rate in spending or employment. For our main specification, the 15 NAICS categories included in both the spending and employment estimates, $\Delta Y_{j,t}$, corresponds to the same NAICS categories used in the flow estimates.[24]

The key explanatory variable is the measure of housing net wealth change for county i in period t that is calculated as:

$$\Delta HNW_i^t = \frac{P_{h,i}^t - P_{h,i}^{t-2}}{P_{h,i}^{t-2}}$$

where ΔHNW_i^t is the change in housing wealth computed by the change in housing price from $t-2$ to t, where $P_{h,i}^t$ is the housing price for county i in year t.

4.1. Weighting Housing Wealth Change by Spending Flows

Our base measure of housing wealth change for consumers residing in county i is ΔHNW_i. Assuming that consumption does not cross county borders, then the wealth change relevant for firms in county j is then ΔHNW_i where $i = j$.

The hypothesis in this paper is that the effect of the change in housing wealth is not constrained to county borders. To distribute housing wealth shocks to firms more accurately, we use an aggregate measure of spending flow across all industries in our data based on where consumers reside. The aggregate expenditure flows are measured as the share of revenues coming from each industry, weighted by the industry spending in the county:

$$S_{i,j}^{AGG} = \frac{\sum_{\forall n \in I} R_{j,n}.S_{i,j,n}}{\sum_{\forall n \in I} R_{j,n}}$$

This share, $S_{i,j}^{AGG}$, better captures the potential consumers from county i for firms located in county j. For examples, if 60 percent of a firm's revenue in county A comes from the home county A, $S_{i=A,A}^{AGG} = 60\%$, then we should expect changes in the wealth of those potential consumers in A to account for around 60 percent of the total effect. The remaining 40 percent would be from exports (i.e., consumption from consumers that reside outside county A).

[24] As a robustness check and for comparison, we have also estimated spending and employment using retail categories. We obtain estimates very similar to the main results shown in the text.

Taking these shares as fixed over time, the housing wealth change that is more relevant for firms in county j is then:

$$\Delta HNW_j^{Flow} = \sum_{\forall i \in C} (\Delta HNW_i).S_{i,j}^{AGG} \tag{2}$$

Continuing with the example, suppose the local housing decline was 20 percent in the home county, A, that has 60 percent of the potential consumers, but just a 2 percent decline for counties outside of the home county, then the associated change for firms located in county A would be $\Delta HNW_A^{Flow} = -20\% \cdot 60\% + -2\% \cdot 40\% = -12.8\%$.

This can be decomposed into two components of the housing wealth change: one measure from consumers that reside in the same county as the firm, and another measure from consumers outside of the county, $\Delta HNW_j^{Flow} = \Delta HNW_j^{Home} + \Delta HNW_j^{Export}$. More specifically these can be measured as:

$$\Delta HNW_j^{Home} = (\Delta HNW_{i=j}) \cdot S_{i=j,j}^{AGG}$$

and also a separate measure from consumers that reside outside the county:

$$\Delta HNW_j^{Export} = \sum_{\forall i \neq j \in C} (\Delta HNW_i) \cdot S_{i,j}^{AGG}$$

For clarity, we calculate each of these components for the hypothetical example in Table 3.

Table 3. Hypothetical Example County A

Home Share	60%
Export Share	40%
Home Housing Price Change	-20%
Export Housing Price Change	-2%

Calculations of ΔHNW

$$\Delta HNW_A^{FLOW} = (-20\% \cdot 60\%) + (-2\% \cdot 40\%) = -12.8\%$$
$$\Delta HNW_A^{Home} = -20\% \cdot 60\% = -12\%$$
$$\Delta HNW_A^{Export} = -2\% \cdot 40\% = -0.8\%$$

Note: The table contains hypothetical numbers to demonstrate how the housing net wealth change variable is calculated.

Similar steps can be taken to construct measures of housing wealth change for potential consumers that reside outside of the home CZ j:

$$\Delta HNW_j^{CZ-Export} = \sum_{\forall i \notin CZ_j} (\Delta HNW_i) \cdot S_{i,j}^{AGG}$$

Focusing on the CZ is justified by the descriptive regression model from Table 2, which confirms the importance of the CZ in affecting spending patterns. To test whether changes in spending from potential consumers outside the home CZ have a differential impact on the local market, our main estimates include both ΔHNW_j^{Flow} and $\Delta HNW_j^{CZ-Export}$.

4.2. Empirical Specification

The regression we analyze takes the form:

$$\Delta Y_{j,t} = \beta_1 f(\Delta HNW_{j,t}, S_{i,j}^{AGG}) + \beta_{2,t} X_{j,t} + \omega_j + \tau_{r,t} + \epsilon_{j,t} \tag{3}$$

where $f(\Delta HNW_j, S_{i,j}^{AGG})$ is a function of housing wealth changes and across-market spending flows.[25]

[25] Similar to Guren et al. (2020), conceptually we think of changes in housing wealth as being caused by a one-time, unexpected, and permanent aggregate shock, which changes the demand for housing. In this empirical model we want to capture changes in housing price differences across areas caused by differences in supply elasticity, so we can identify the causal effect of housing price changes on consumer spending and associated employment.

The analysis measures the partial equilibrium effect of housing price changes on consumption, where there may also be local general equilibrium effect as argued in Guren et al. (2020). That is, we consider $\beta_1 f(\Delta HNW_{j,t}, S_{i,j}^{AGG})$ to be the partial effect of housing price changes on consumption multiplied by a local general equilibrium effect. We examine different housing wealth measures, including those that ignore across-county spending flows ΔHNW_j^{NoFlow}[26], and those that incorporate across-county spending flows, such as ΔHNW_j^{Flow}. We also explore a variety of other specifications, such as those that include both ΔHNW_j^{Flow} and $\Delta HNW_j^{CZ-Export}$ to allow for the effect of housing wealth changes outside of the CZ to be different.

Local spending and employment may be affected by other factors that need to be accounted for in the analysis. To account for these factors, we include additional covariates that may affect growth in spending or employment. We exploit the panel dimension of the data and include county fixed effects, ω_j, in all the regressions. The county fixed effects account for all county-specific factors influencing the growth rate of individual counties over the period of study. We also include year-region fixed effects, $\tau_{r,t}$, to account for the differential growth rates across different areas of the United States in each year.

One of the main controls included in, $X_{j,t}$, are two-digit industry shares in each county that account for the general growth rate of different sectors over this time period. The industry share variable is interacted with the year dummies to allow for flexibility in the growth rates of different industries in different years. The inclusion of industry shares mitigates the potential endogeneity concern that industry structure could be associated with changes in housing wealth.

The controls account for many of the key factors that might influence growth over time. However, there is still the possibility that the estimates are affected by endogenous factors. For instance, employment declines could cause a reduction in housing prices. For this reason, we apply instrumental variables in our regressions.

4.2.1. Instrumental Variables

The main instrument that we use is a sensitivity instruments following the approach of Guren et al. (2020). The methodology exploits the panel aspect of the data to measure the sensitivity of different

[26] The variable $\Delta HNW_j^{NoFlow} = \Delta HNW_i$ where $i = j$.

areas to regional housing price changes. The intuition is that certain areas will be more susceptible to regional price fluctuations as the elasticity of housing supply may be higher in some areas than others. Using panel data, an econometric model can be used to identify the sensitivity of each county to regional price fluctuations, which can be used to form an instrumental variable. The basic econometric specification is:

$$\Delta HNW_{j,t} = \gamma_j \cdot \Delta HNW_{R,t} + \alpha_1 \Delta Y_{j,t} + \alpha_2 \Delta Y_{R,t} + \beta_t W_{j,t} + \epsilon_{j,t} \quad (4)$$

The left-hand side variable, $\Delta HNW_{j,t}$, is the housing wealth change in the county. The right-hand side includes an interaction of the regional change in housing prices, $\Delta HNW_{R,t}$, interacted with a county-specific fixed effect, γ_j. The goal of the regression is to recover estimates of γ_j, which capture the sensitivity of the county with respect to regional housing price changes. Importantly, the regression includes a number of additional controls, such as local and regional spending growth, $\Delta Y_{j,t}$, $\Delta Y_{R,t}$, as well as two-digit industry shares interacted with year dummies, which could potentially be confounding factors that would correlate with both regional and local housing wealth growth. After estimating equation (4), the variable γ_j interacted with the regional price change is the sensitivity instrument applied in the analysis.[27]

There are several advantages to this methodology. The sensitivity instruments can be customized to the county-level and covers all geographic areas. Moreover, Guren et al. (2020) found the sensitivity instruments produced more precise estimates relative to other approaches in the literature (e.g., Saiz (2010)). Guren et al. (2020) argue that an advantage of their approach is that this sensitivity instrument can account for a range of factors that might influence the supply elasticity. This contrasts with the approach in Saiz (2010), which is applied in Mian et al. (2013) and Mian and Sufi (2014) and is based on the geographic characteristics of areas that might influence the cost of expanding housing supply in different locations, which is primarily related to land availability.[28]

We compare our instruments directly to those of Guren et al. (2020) and Saiz (2010). Although the instruments of Guren et al. (2020) and Saiz (2010) are only available at higher levels of aggregation (i.e., CBSA and MSA), and are not available for all geographic areas, we are able to compare the

[27] The spending flows applied to the housing wealth changes are applied identically to the constructed instruments.

[28] More specifically, the Saiz (2010) instrument relies on a measure of the share of land within a 50 km radius of a city center that is amenable to construction due to water or steep slopes.

instruments where they are available.[29] We find that the three instruments are distinct, but correlated. The correlation between our main instrument and the sensitivity instrument directly from Guren et al. (2020) is 0.94 and the correlation between the Saiz (2010) instrument and our instrument is 0.32, which is similar in magnitude to the correlation between Saiz (2010) and Guren et al. (2020) of 0.47. We provide additional details and discussion of the IV methodology in the appendix section A.9.[30]

5. Results

We first report baseline regressions to benchmark estimates to the prior literature and demonstrate the importance of spending flows. Next, we examine heterogeneous effects of housing wealth changes where we allow for different effects for potential consumers that reside outside of the CZ and for different effects during the GR.

We report the effects on both spending and employment for our main estimates, as these variables are distinct. However, as the results are qualitatively similar, our robustness analysis focuses solely on the spending effects, as changes in housing wealth arguably have a more direct effect on spending. Additional employment results are reported in the appendix section A.12.

5.1. Baseline

The top panel of Table 4 provides the baseline results for spending. Column (1) includes the housing wealth change based on the county where the firm is located, which ignores spending flows, $\Delta HNW(NoFlow)$, and is an OLS regression. Column (2) differs from (1) by using the spending flow information. In columns (3) and (4) we repeat those of (1) and (2), but apply IV methods. The effect of housing wealth on spending is positive and significant across all specifications, consistent with previous literature. The IV method reduces the magnitude of both coefficients, similar to the effect of instrumenting in Guren et al. (2020). We find that the magnitude of the effect using the spending flows

[29] The instrument in Guren et al. (2020) is distinct from the sensitivity instrument that we construct because it is available at the CBSA level, which is more aggregate and doesn't cover all counties. In addition, Guren et al. (2020) is constructed using 40 years of data.

[30] We also conduct additional robustness checks that apply different instrument methodologies. Specifically, we directly use the instrument constructed in Guren et al. (2020) and Saiz (2010). We find all three methodologies produce qualitatively similar results, although we lose both observations and precision when we apply the alternatives, as they do not cover all counties and are more geographically aggregate.

(columns (2) and (4)) to be about 20 percent larger than the estimates that exclude spending flows (columns (1) and (3)). Column (4) shows the preferred specification and indicates an elasticity of 0.12. If housing wealth increases by 10 percent, there is a 1.2 percent increase in spending.

To compare these estimates to other work in the literature, we convert the elasticity of spending from changes in housing wealth to a marginal propensity to consume out of housing wealth by dividing these elasticities by the ratio of housing wealth to consumption, which we estimate to be 2.47.[31] The Marginal Propensity to Consume (MPC) from housing wealth based on the estimates without the flows, but applying IV (column 3) is 3.8 cents on the dollar. Our preferred specification (column 4) indicates a value of 4.9 cents on the dollar. Both of these estimates are similar in magnitude to related papers in the literature.[32]

The estimates in the bottom panel of Table 4 are identical to the top panel, but the dependent variable is employment rather than spending. The same pattern is observed in both the top and bottom panel, although the magnitude of the coefficient is smaller for employment, relative to spending. The difference in the magnitude of the effect is an important point as researchers apply employment as a proxy for spending. We find that the effect on spending is more elastic than employment, and there may be fundamental economic reasons for this difference (e.g., cost of selling a different amount of goods and services may be different that the costs of contracting, hiring, or firing employees). Understanding why there are differential effects on spending and employment may be an important area for future research.[33]

[31] The elasticity in the regression captures the percent change in spending from a percent change in housing prices. To arrive at a dollar change in spending from a dollar change in housing wealth, we divide the elasticity by housing wealth and multiply it by the level of consumption following Guren et al. (2020). We estimate the value of the housing spending based on the market value of owner-occupied real estate from the Flow of Funds and we estimate the value of consumption based on total personal consumption expenditures net of housing and utilities from the BEA. We calculate the average of the consumption and housing value components over the period 2000–2019 and then form the ratio.

[32] Aladangady (2017) estimates a MPC out of housing wealth based on microdata of 4.7 cents for homeowners and finds no effects for renters. Based on a homeownership rate of 65 percent, this corresponds to an MPC of 3.1 cents overall. Di Maggio et al. (2020) examines the MPC based on stock returns and find estimates of 5 cents on the dollar or more. Guren et al. (2020) find a MPC of around 2.4 cents on the dollar looking over a longer time horizon and different geographic market. The Guren et al. (2020) estimates are also based on employment, rather than spending, which we find produces a slightly lower implied elasticity. Our estimate is lower than Mian et al. (2013) who find an MPCH out of housing of around 7.2 cents on the dollar, although we are examining a different time period. In the next section of the paper, we find that the magnitude of the effect approximately doubles around the GR, producing estimates similar in magnitude to Mian et al. (2013) when we look at the same time period. Our estimates are of similar magnitude to others in the literature, but it is important to keep in mind that the methodologies differ along a number of dimensions. For instance, our estimates are based on over 3,000 counties, while other estimates in the literature use a subset of counties or different geographies (e.g., CBSA).

[33] In intercensal years the spending estimate is an approximation, which is a reason to also examine employment effects directly. Additional research is necessary to further validate these findings.

Columns (5) and (6) present a test of the importance of the spending flows by including two measures of housing wealth changes, one with spending flows ($\Delta HNW(Flow)$) and one without spending flows ($\Delta HNW(NoFlow)$). Column (5) is an OLS regression while column (6) applies an IV. In all cases, the housing wealth variable that incorporates the flows is positive and statistically significant, but the variable that excludes the flows is insignificant, suggesting that incorporating the flows produces more accurate and relevant estimates.

Table 4. Housing Wealth Change on Spending and Employment Growth

	(1)	(2)	(3)	(4)	(5)	(6)
	% Chg. Spend	% Chg. Spend	% Chg. Spend	% Chg. Spend	% Chg. Spend	% Chg. Spend
Δ HNW (No Flow)	0.139***		0.0956***		0.0624	-0.241
	(0.00815)		(0.0128)		(0.0417)	(0.182)
Δ HNW (Flow)		0.166***		0.119***	0.0928*	0.397*
		(0.00999)		(0.0146)	(0.0514)	(0.207)
N	52875	52875	52875	52875	52875	52875
R squared	0.345	0.345	0.343	0.343	0.345	0.340
IV Estimate	No	No	Yes	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.
Δ HNW (No Flow)	0.0929***		0.0649***		0.0269	-0.112
	(0.00625)		(0.0110)		(0.0347)	(0.0875)
Δ HNW (Flow)		0.111***		0.0793***	0.0796*	0.209**
		(0.00736)		(0.0127)	(0.0415)	(0.100)
N	52874	52874	52874	52874	52874	52874
R squared	0.407	0.407	0.406	0.406	0.407	0.405
IV Estimate	No	No	Yes	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01
Note: The table presents results from a panel IV regression estimate of the change in spending and employment for 15 select industries in the county from 2003 to 2019 on the change in housing wealth variable(s). All estimates include county fixed effects. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county variables interacted with year fixed-effects, and also region-year fixed-effects.

As an additional check on the importance of spending flows, we divide counties into export quartiles, based on the share of spending exported (i.e., the share of spending coming from consumers that reside outside the county). For each county we calculate the housing wealth change in the home market (i.e., housing wealth change in the county where the firm is located) and the average housing wealth change in the export markets.[34] We then interact the export quartile with the housing wealth change in the home market of the firm, $\Delta HNW(NoFlow)$, and the average housing wealth change in the export market $Avg.\Delta HNW(Export)$. Table 5 reports the results, where column (1) reports OLS estimates, and column (2) applies instruments. Across both columns (1) and (2) we find the expected pattern. In the high export counties (Q1 Export and Q2 Export), we find effects of housing wealth changes in the home market, Δ $HNW(NoFlow)$, to be substantially lower than in the low export counties (Q3 Export and Q4 Export). In contrast, we find that the high export counties (Q1 Export and Q2 Export) are positively and significantly impacted by average changes in housing wealth away from home, $Avg.\Delta$ $HNW(Export)$, while low export counties (Q3 Export and Q4 Export) are not affected. Similar results are found when analyzing employment as the dependent variable (see Table A27). These estimates demonstrate that local changes in housing wealth may not be as important as those in the export markets when a large share of potential consumers reside outside the home county.

Overall, this section shows comparable results to the related literature, although we find the magnitude of the effect of housing wealth changes on spending and employment to be higher when spending flows are incorporated. We also show how spending flows are important when analyzing local economic shocks in two ways: showing that the statistical relationship between housing wealth and spending are stronger when spending flows are incorporated (Table 4 columns (5 and 6)); and showing that the housing wealth changes that are most relevant greatly depend on where potential consumers reside (Table 5).

[34] For each county, excluding the firm's home county, we calculate the average housing wealth change using a weighted average of the share of consumer spending coming from each export county.

Table 5. Average Housing Wealth Changes on Spending Growth: Home and Away by Export Quartile

	(1) % Chg. Spend	(2) % Chg. Spend
Δ HNW (Home) · Q1 Export	0.102***	-0.0236
	(0.0193)	(0.0895)
Δ HNW (Home) · Q2 Export	0.0825***	-0.125**
	(0.0261)	(0.0604)
Δ HNW (Home) · Q3 Export	0.186***	0.0625
	(0.0219)	(0.0484)
Δ HNW (Home) · Q4 Export	0.160***	0.142**
	(0.0217)	(0.0649)
Avg. Δ HNW (Away) · Q1 Export	0.0631**	0.204**
	(0.0309)	(0.104)
Avg. Δ HNW (Away) · Q2 Export	0.0618*	0.300***
	(0.0336)	(0.0785)
Avg. Δ HNW (Away) · Q3 Export	-0.0601*	0.0922
	(0.0332)	(0.0726)
Avg. Δ HNW (Away) · Q4 Export	-0.0212	0.00753
	(0.0311)	(0.0829)
N	52875	52875
R squared	0.346	0.339
IV Estimate	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: Q1 is high export quartile, so more potential consumers are away from the home county; and Q4 is low export quartile, so more consumers are in the home county. The table presents results from panel regression estimates of the change in spending for 15 select industries in the county from 2003 to 2019 on the change in housing wealth variables. Column (1) shows the OLS estimates and column (2) applies instrumental variables. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county variables interacted with year dummies, and also region-year fixed-effects.

5.2. Heterogeneous Effects During the Great Recession both Home and Away

The prior section shows that changes in housing wealth from potential consumers in both home and more distant markets affect spending and employment. However, it is not clear if these effects are symmetric, as potential consumers in more distant markets could have differential effects. For instance, consumers may view spending away from home as more of a luxury good and adjust their spending differently in local and more distant markets in response to economic shocks. Alternatively, spending by potential consumers in distant markets could have a differential multiplier effect through a variety of possible mechanisms.[35]

To measure the effects of housing wealth changes of potential consumers in the home market compared to potential consumers outside of the CZ, we run the following specification:

$$\Delta Y_{j,t} = \alpha_1 \cdot \Delta HNW_j^{Flow} + \alpha_2 \cdot \Delta HNW_j^{CZ-Export} + \beta_t \cdot X_{j,t} + \omega_j + \tau_{r,t} + \epsilon_{j,t}. \tag{5}$$

Recall that the variable, $\Delta HNW_j^{CZ-Export}$, is the housing wealth variable using only spending flows outside the home CZ, where a significant effect on this variable would indicate the housing wealth change in areas outside the CZ have a greater effect on $\Delta Y_{j,t}$, relative to changes in housing wealth within the CZ.

The effects of housing wealth changes may also depend on how much slack there is in the economy, which might affect the associated multiplier, so we also allow for differential effects during the GR (i.e., 2008–2009) compared to the rest of the sample period.[36] The effect of housing wealth changes outside the CZ may also be distinct during the GR, so we also test for this differential effect. Specifically, we examine the effects of how spending and employment are affected differently by local changes to housing wealth compared to more distant changes (i.e., outside of the CZ), as well as analyze these effects during the depths of the GR compared to the rest of the sample period, where dummy values for the GR are interacted with housing wealth change variables to test for the differential effect.

[35]The changes in spending by consumers in more distant markets might spur employment for lower income jobs, where a greater share of income is consumed. Alternatively, the multiplier effect could also be different if the labor supply elasticity is higher for jobs catering to more distant consumption, or if there is greater complementarity between consumption and labor for spending in more distant markets (see Hall (2009)).

[36]Multiplier effects may be larger when there is greater slack in economic activity, as it reduces the likelihood of crowding out the private sector, (Nakamura and Steinsson (2014) and Hall (2009)).

We apply the IV method discussed previously. However, the potential endogeneity of $\Delta HNW_j^{CZ-Export}$ is reduced, as it measures changes in housing wealth outside the CZ, so it is less likely to be endogenous through reverse causality (i.e., changes in the labor market affecting local housing prices).[37] Therefore, for some specifications we do not instrument for $\Delta HNW_j^{CZ-Export}$.

The results of this analysis on spending are shown in Table 6. Column (1) estimates whether the effects of the housing wealth change are significantly different during the GR. We find that the difference is large and statistically significant, with the change in housing wealth during the GR having almost three times the effect as the change in housing wealth during the rest of the sample period.[38] The magnitude of the difference during the GR is comparable to the differential multiplier effect of fiscal stimulus in Nakamura and Steinsson (2014), where they found the fiscal multiplier during slack periods in the economy to be three to four times larger than periods with less slack in the economy.[39]

Columns (2) and (3) of Table 6 test whether housing wealth changes outside the home CZ have a larger effect on spending. Both estimates apply the IV method to ΔHNW_j^{Flow}, but column (2) does not instrument for $\Delta HNW_j^{CZ-Export}$, while column (3) does. Both estimates show a positive coefficient on $\Delta HNW_j^{CZ-Export}$, indicating changes in housing wealth outside the CZ have a larger effect than changes in the CZ, but only column (2) is statistically significant, while column (3) is not. This provides some mixed evidence regarding the relative importance of spending outside the CZ. The next two columns (4) and (5) interact both variables, ΔHNW_j^{FlowW} and $\Delta HNW_j^{CZ-Export}$ with the GR dummy, where column (4) does not instrument for $\Delta HNW_j^{CZ-Export}$, while column (5) does. Both estimates in column (4) and (5) indicate that the multiplier effect during recessionary periods is larger, with a significant coefficient on $\Delta HNW_j^{FLOW} \cdot GR$. Both estimates also indicate that changes in housing wealth outside of the CZ has a disproportionately larger effect on spending compared to changes in housing wealth within the CZ. The main difference is that column (4) indicates that the effect is larger throughout the sample period, while column (5) indicates that the differential effect occurs only during the GR. We prefer the specification in (5) as $\Delta HNW_j^{CZ-Export}$ is potentially endogenous.

[37] In other words, the housing wealth change that affects a firm are not necessarily specific to the location of the firm.

[38] The effect during the GR is 0.22(=0.08+0.14), while the effect during the rest of the period is 0.08.

[39] Although one distinction is that Nakamura and Steinsson (2014) found estimates to be moderately significant, while we find the differential estimates for the GR to be highly significant. Our analysis is difficult to compare directly because Nakamura and Steinsson (2014) are measure the effects of local government spending, while we are examining changes in housing wealth.

Moreover, evidence that the differential effect primarily occurs during the GR is further supported by the employment estimates.

Table 7 repeats this analysis but uses employment as the dependent variable rather than spending. The results are similar to Table 6, but show no differential effect of changes in housing wealth outside the CZ on employment, except during the GR, when the effect is particularly large across specifications in columns (4) and (5).

The bulk of the evidence in Tables 6 and 7 suggests that excluding the GR period, the changes in housing wealth affect spending and employment in ways that we expect. Namely, changes in housing wealth of potential consumers within a CZ have a similar effect to changes in housing wealth outside of the CZ. During the GR, the results are clearly different, and changes in housing wealth outside of the CZ, have a disproportionately large effect on both spending and employment. These results suggests that the across-market consumption link may grow in importance during substantial changes in the economy, acting to amplify local effects across geographic markets.

Table 6. Housing Wealth Change on Spending Growth: Differential Effects During the GR, Both Home and Away

	(1) % Chg. Spend	(2) % Chg. Spend	(3) % Chg. Spend	(4) % Chg. Spend	(5) % Chg. Spend
Δ HNW (Flow)	0.0800***	0.0921***	0.107***	0.0486**	0.0734***
	(0.0179)	(0.0195)	(0.0189)	(0.0236)	(0.0226)
Δ HNW (CZ-Export)		0.297***	0.127	0.338***	0.0611
		(0.100)	(0.112)	(0.111)	(0.125)
Δ HNW (Flow) · GR	0.142***			0.158***	0.110***
	(0.0228)			(0.0325)	(0.0296)
Δ HNW (CZ-Export) · GR				-0.133	0.422**
				(0.200)	(0.188)
N	52875	52875	52875	52875	52875
R squared	0.342	0.342	0.343	0.340	0.342
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from panel regression and IV regression estimates of the change in spending for 15 select industries in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Table 7. Housing Wealth Change on Employment Growth: Differential Effects During the GR, Both Home and Away

	(1)	(2)	(3)	(4)	(5)
	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.
Δ HNW (Flow)	0.0412**	0.0691***	0.0814***	0.0317	0.0498***
	(0.0164)	(0.0165)	(0.0152)	(0.0207)	(0.0192)
Δ HNW (CZ-Export)		0.114*	-0.0241	0.0962	-0.103
		(0.0649)	(0.0594)	(0.0748)	(0.0683)
Δ HNW (Flow) · GR	0.140***			0.123***	0.100***
	(0.0190)			(0.0249)	(0.0220)
Δ HNW (CZ-Export) · GR				0.239*	0.493***
				(0.124)	(0.104)
N	52874	52874	52874	52874	52874
R squared	0.406	0.405	0.406	0.405	0.406
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from panel regression and IV regression estimates of the change in employment for 15 select industries in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

An important question is: why is the effect from potential consumers outside the home CZ larger during the GR? As mentioned previously, one possibility is that consumers disproportionately cut spending away from home during the GR, as spending outside the local area may be viewed as a luxury good. Indeed, the changes caused by the GR may have been substantial enough to affect relative expenditures by a large amount, as wealth, income and expectations of consumers changed substantially. Consumer sentiment reached a low point in the 2008 and 2009 period.[40] Moreover, distant trips away from home may be quite costly discrete choices that are also discretionary. Alternatively, it may be that the multiplier effect is different, so that a $1 decline in spending by a consumer in a market away from home, may have a larger effect than a $1 decline in spending by a consumer in the local CZ market. There are a range of possible factors that could generate different multipliers (Hall (2009)).

Suggestive evidence from the descriptive regression in Table 2 supports the idea that consumers may disproportionately cut back on spending in more distant markets. Recall in column (4) of Table 2 we

[40]Consumer sentiment data is from the University of Michigan Surveys of Consumers: http://www.sca.isr.umich.edu/.

find that, all else equal, higher per capita income indicates that consumers are less sensitive to distance and spending outside the CZ, and thus, more likely to spend away from home. Through this mechanism, the GR, which was accompanied by reduced wealth, incomes, and consumer sentiment, could lead to less spending outside of the local CZ.

5.3. Differential Effects by Industry

To better understand the relatively large effects on spending and employment from housing wealth changes outside the CZ during the GR we look at differential effects by industry. If the larger effect outside of CZ is caused by a larger multiplier effect, then spending away from home should have a significant and large effect on all sectors, including those primarily consumed locally. For example, the additional dollars that are brought into the market from potential consumers that reside outside the CZ, generate additional income locally, so grocery shopping increases. Alternatively, if the effect is primarily consumers cutting back on distant trips, a luxury good effect, then we might expect to only observe the larger effect for industry categories that export to consumers outside the CZ.

We divide the data into three broad industry groups based on the share of spending outside the county, so that roughly one third of spending is in each group.[41] We denote the first category the "local industry" group that includes the industries with the largest share of spending from consumers that reside in the same county as the firm, which includes food and beverage stores (NAICS 445), general merchandise stores (NAICS 452), and building material and garden stores (NAICS 444). These are spending categories that are both consumed locally, but also tend to sell basic necessities. The second category is a "export industry" group. This group includes industries with a relatively large share of spending from consumers that reside away from home, which includes a variety of recreational industries, such as accommodations (NAICS 721), food services (NAICS 722), and amusement and performing arts among other categories (NAICS categories: 442, 453, 451, 713, 448, and 711 respectively). These industries may be viewed as more luxury goods. The third group is an "intermediate group" that falls between the other two, which includes industries like repair and maintenance and gas stations (NAICS categories: 812, 811, 621, and

[41] We focus on three broad industry groups and more aggregate estimates for a few reasons. First, there is potential for some noise in all the data sources, and we can reduce this noise through aggregation. Second, different industries may offer similar goods, such as a meal in a hotel or meal in a restaurant or purchases of food in a general merchandise store that could also be found in a grocery store. Therefore, grouping industries together may create more comparable groups. Third, individuals across different areas could spend their money on different items, so by starting with more aggregate spending categories we are more likely to capture the full effect on spending.

447). For this analysis we primarily focus on the local and export industry groups, as they are more distinct, and report the results for the intermediate category in the appendix.

The first three columns of Table 8 show the results of this analysis that repeats the estimates from columns (1), (3) and (5) of Table 6, but uses spending from the local industry category as the dependent variable. Note, we are still using a spending flow variable that covers all of the industries as a means to check whether the local industry category is affected by the housing wealth change of potential consumers across all industries. Similar to the previous analysis, we find that the effect from the change in housing wealth is significantly larger during the GR. However, we do not find any evidence that changes in housing wealth outside of the CZ have any differential effect on spending for local industries, including during the GR. We think the reason for no differential effect outside the CZ is that most of the spending from outside the CZ is not directed at the local industries and there is not a large spillover effect (as we might expect if the differential effect was caused by a larger multiplier effect). Columns (4), (5) and (6) of Table 8 repeats this analysis, but use spending from the export industries as the dependent variable. We find that the elasticity of spending on export industries is larger relative to the local industries, as shown by the coefficients on ΔHNW (Flow) and ΔHNW (Flow) $\cdot\, GR$ in columns (1) and (4). The key finding from this table comes from comparing columns (3) and (6). We find that there is a large and disproportionate response to changes in housing wealth outside the CZ for the export industry, but no differential effect for the local industry. We conduct the same regression using employment, rather than spending and find similar results that we report in the appendix (see Table A12).[42] This differential response for local and export industries is consistent with consumers cutting back substantially more on export industries consumed away from home, which may be considered more of a luxury good.

Evidence Using Industry-Specific Spending Flows — The estimates in Table 8 use aggregate spending flows across all industries, allowing for across-industry effects. For instance, the potential consumers for all industries, including the export and intermediate industries, is allowed to effect the local industry. This could be important if, for instance, multiplier effects are large, so there are spillover effects. Table 8 shows no evidence of a spillover effect, as the changes in housing wealth outside the CZ primarily effect the export industry.

[42]When we study the intermediate industries (Table A11), we get results that are less statistically significant outside of the GR. One possible reason for this is that all of these industries are quite distinct and more difficult to characterize, including categories such as gas stations and ambulatory care.

Table 8. Housing Wealth Change on Spending for Local and Export Industry Category: Differential Effects During the GR, Both Home and Away

	(1) Local Industries	(2) Local Industries	(3) Local Industries	(4) Export Industries	(5) Export Industries	(6) Export Industries
Δ HNW (Flow)	0.128***	0.154***	0.139***	0.147***	0.173***	0.153***
	(0.0254)	(0.0241)	(0.0300)	(0.0240)	(0.0236)	(0.0286)
Δ HNW (CZ-Export)		-0.0520	-0.117		0.0139	-0.0799
		(0.128)	(0.144)		(0.121)	(0.131)
Δ HNW (Flow) · GR	0.0776**		0.0449	0.102***		0.0562*
	(0.0374)		(0.0488)	(0.0286)		(0.0329)
Δ HNW (CZ-Export) · GR			0.396			0.568***
			(0.278)			(0.217)
N	52722	52722	52722	52772	52772	52772
R squared	0.212	0.212	0.212	0.301	0.301	0.301
IV Estimate	Yes	Yes	Yes	Yes	Yes	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from panel regression and IV regression estimates of the change in spending for local and export industries in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and industries. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

As an additional check, we run estimates using industry-specific flows. For example, for the local industry regression we use spending flows specific only to the local industry to calculate the effect of housing wealth changes.[43] Similar to prior analysis we can look at differential effects during the GR and outside of the home CZ, but using industry-specific flows.

We run parallel specifications for local industries in columns (1)–(3) and for export industries in columns (4)–(6). Columns (3) and (6) are unique relative to prior specifications, as they allow for cross-industry effects from potential consumers outside of the CZ (e.g., for the local industry we include the housing wealth change outside the CZ for the export industry).[44]

[43]We calculate spending flows for local industries in a parallel way to aggregate spending flows. Specifically, $S_{i,j}^{\text{Local Ind.}} = \frac{\sum_{\forall n \in \text{Local Ind.}} R_{j,n} \cdot S_{i,j,n}}{\sum_{\forall n \in \text{Local Ind.}} R_{j,n}}$. Industry specific shares are also be calculated for the export industry.

[44]An alternative specification would be to include the housing wealth change from the local industry and the export industry in the same specification, but since most potential consumers are relatively local, these two variables are highly collinear. In contrast, the housing net wealth change from potential consumers in the export industries that reside outside the CZ is much less correlated with local industry housing wealth variable.

Comparing columns (1) and (4) of Table 9 we again see that the export industries appear to be more elastic with respect to spending, compared to the local industries. Comparing columns (2) and (5) that allow for differential effects outside the CZ, we find that during the GR there is a disproportionate effect for potential consumers that reside outside the CZ for both industry groups. However, by definition, the share of potential consumers outside the home CZ is much smaller for local industries, so the economic importance of this effect is also smaller. For columns (3) and (6) we allow for cross-industry effects outside the local CZ, where a differential effect could potentially occur through a higher multiplier effect. We find no evidence of cross-industry effects.[45] Again, these results are consistent with the idea that consumers are disproportionately cutting back on spending away from home, especially for the export industries.

For completeness, we run the same specification using employment (see Table A10), where we find qualitatively similar results.

5.4. Robustness Checks

5.4.1. Effects by Distance

The CZ is just one definition of a local geographic market. Another reasonable alternative would be to use distance more directly. To examine variation by distance we start by including the baseline $\Delta HNW(TotalFlow)$ variable, we then interact the housing net wealth variable with spending shares at different distance intervals (i.e., outside the county but less than 100 miles; and more than 100 miles). The results reported in Table 10 provide support for using CZ as we generally find no differential effect of spending flows within a 100-mile radius of the county. That is, it is only for the more distant potential consumers where we observe a differential effect of housing wealth on spending. Notably, this specification shows differential spending effects for potential consumers more than 100 miles away more generally, not only during the GR. However, the identical estimate using employment, rather than spending, only shows a differential effect for more than 100 miles away during the GR (see Table A13).

[45] One may be concerned that the reason for no cross industry effects could be the relative sizes of the local and export industries. In an alternative specification, we adjust for industry size using the average relative spending in the two industries in a county for the entire sample period and interacting this with the wealth effect, but still find no significant cross-industry effect.

Table 9. Housing Wealth Change on Spending for Local and Export Industries Using Industry-Specific Flows: Differential Effects During the GR, Both Home and Away

	(1) Local Industries	(2) Local Industries	(3) Local Industries	(4) Export Industries	(5) Export Industries	(6) Export Industries
Δ HNW (Local Ind. Flow)	0.120***	0.125***	0.125***			
	(0.0240)	(0.0260)	(0.0283)			
Δ HNW (Local Ind. Flow) · GR	0.0649*	0.0377	0.0356			
	(0.0358)	(0.0428)	(0.0469)			
Δ HNW (Export Ind. Flow)				0.163***	0.159***	0.163***
				(0.0258)	(0.0302)	(0.0306)
Δ HNW (Export Ind. Flow) · GR				0.110***	0.0483	0.0428
				(0.0322)	(0.0378)	(0.0371)
Δ HNW (Local Ind. CZ-Export)		-0.104	-0.102			-0.277
		(0.151)	(0.166)			(0.194)
Δ HNW (Local Ind. CZ-Export)· GR		0.603*	0.557			0.387
		(0.333)	(0.358)			(0.382)
Δ HNW (Export Ind. CZ-Export)			-0.00193		0.0156	0.114
			(0.105)		(0.0898)	(0.114)
Δ HNW (Export Ind. CZ-Export)· GR			0.0407		0.534***	0.394*
			(0.218)		(0.153)	(0.209)
N	52722	52722	52722	52772	52772	52772
R squared	0.212	0.212	0.212	0.301	0.301	0.301
IV Estimate	Yes	Yes	Yes	Yes	Yes	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from panel regression and IV regression estimates of the change in spending for local and export industries in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and industries. The housing wealth changes in this table are based on industry-specific flows. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Finally, to check if this could potentially represent a higher multiplier effect, we examine how the local industry category is affected by the change in housing wealth from potential consumers at different distances (appendix Table A14). Similar to the previous results, we find no significantly differential effect from potential consumers more than 100 miles away, again supporting the idea that there is a shift away from export industries, rather than a differential multiplier effect.

Table 10. Spending by Distance

	(1) % Chg. Spend	(2) % Chg. Spend	(3) % Chg. Spend	(4) % Chg. Spend
Δ HNW (Flow)	0.0931***	0.0930***	0.0599**	0.0579*
	(0.0234)	(0.0240)	(0.0290)	(0.0299)
Δ HNW (Export: ≤ 100 Miles)	0.0593	0.0594	0.0261	0.0311
	(0.0911)	(0.0924)	(0.111)	(0.113)
Δ HNW (Export: > 100 Miles)	0.413***	0.414***	0.366**	0.400**
	(0.134)	(0.142)	(0.153)	(0.163)
Δ HNW (Flow) · GR			0.0962**	0.104**
			(0.0414)	(0.0419)
Δ HNW (Export: ≤ 100 Miles) · GR)			0.280	0.261
			(0.236)	(0.239)
Δ HNW (Export: > 100 Miles) · GR)			0.298	0.166
			(0.209)	(0.214)
N	52875	52875	52875	52875
R squared	0.343	0.343	0.341	0.341
IV Estimate	Yes	Yes	Yes	Yes
IV Over 100 miles	No	Yes	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from IV panel regression estimate of the change in spending from 2003 to 2019 on the change in housing wealth variables. The specification allows for housing wealth effects to differ by distance. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of 100 miles. The housing wealth change is calculated for consumers at different distances. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

5.4.2. Other Robustness Checks

The robustness checks below are all compared to the main estimates reported in Tables 6 and 7.

Adjusting for Potential Bias in Spending Flows — We estimate alternative spending flows that adjust spending so that consumption is a fixed share of income to account for potential bias in the use of card transactions in the population. This approach was described briefly in the data section and in additional detail in appendix section A.5 and A.12.1. The results are reported in Tables A15 and A16. These estimates are quite close to those reported in our main specification.

CZ-wide Effects — The main analysis focuses on county-specific effects, with flows and housing wealth changes specific to each county. The advantage of this approach is that the changes in housing wealth are directly tied to the associated county. The disadvantage is that the effects on spending and employment in one county in a CZ could affect the entire CZ.[46] As an alternative specification, we aggregate over the housing wealth effect for the entire CZ (i.e., all counties in a CZ share the same effect from changes in housing wealth).[47] The estimates are reported in Tables A17 and A18. Using this alternative specification, we essentially get results identical to the analysis in the main results.

Sensitivity Instrument from Guren et al. (2020) — The main instruments used in the paper are derived using the methodology laid out in Guren et al. (2020). However, the instruments constructed in Guren et al. (2020) are different along a couple of dimensions discussed previously. As an alternative to our instrumental variable strategy, we directly use the instruments from Guren et al. (2020). Specifically, we multiply their sensitivity parameters for each county by the average regional price change to derive an alternative IV. We link the instruments to the county-level data (where all counties within the same CBSA receive the same value for the IV). The estimate using this alternative IV strategy are shown in Tables A19 and A20. The results are qualitatively identical to the main estimates, although the statistical significance declines in some of the estimates.

Housing Supply Elasticity Instrument from Saiz (2010) — We also apply the instrument from Saiz (2010) that is constructed using satellite-generated data on various terrain characteristics (e.g., elevation, presence of water) to estimate a measure of land-unavailability. The instrument that is applied is constructed by interacting the land-unavailability measure with the national price change. The results are in the appendix Tables A21 and A22. Again, all of the results are qualitatively similar to the main estimates, although the sample size drops substantially and estimates are noisier.

Predicted Spending Flows for Each Year — Our estimates apply fixed spending flows based on information for 2015. In the appendix section A.5, we use the accounting relationship between consumers and firms to investigate the validity of the fixed share assumption, and find evidence that constant shares are a reasonable approximation. While this confirms that the assumption is reasonable, the location of firms and consumers may shift over time and affect spending flows and associated estimates. As an alternative to using only 2015 spending shares to capture flows and potential consumers across areas, we form a prediction of flows in the year 2015 using historical information on the location of firms and

[46] In addition, the measurement at the CZ level could contain less noise, as it is an average over multiple counties.

[47] The averaging across counties within a consumption zone is based on the total spending flows aggregating over all counties within a CZ for our 15 industries.

consumers. We then use the predicted flows for the relevant time period, rather than the fixed flows from 2015. For example, for analyzing the effect of the change in housing wealth from 2003 to 2005, we use predicted spending flows for 2003 to capture the location of potential consumers and firms at that time. Additional details regarding how the potential flows are formed is reported in the appendix section A.7.1. The results are reported in Tables A25 and A26, which show similar results to our main estimates.

Alternative Industry Category — For our main estimates we choose consumption categories that align well with card transaction data from Fiserv. Using the broadest measure of consumption allowed us to better track how spending is affected by changes in housing wealth. As a robustness check, we also examine the categories used in Mian and Sufi (2014), which are primarily retail categories also used in Guren et al. (2020).[48] The results are reported in Tables A23 and A24. Again, the results are qualitatively similar to our main results.

Excluding Tourism Areas — It is possible that results are entirely driven by particular states, such as Nevada, that receives a lot of tourism and was also an area where housing prices fell considerably during the GR. However, we remove Nevada and find our results remain the same. The results are robust to removing other tourism states, such as Florida, California, New York and Hawaii. This suggests that our findings are relevant more generally, and not driven solely by high-tourism locations. The estimates are also robust to removing the largest population counties, such as those with more than 3 million individuals in 2007. Results can be made available upon request.

Great Recession Period — We use the period 2008–2009 to define the GR as this best covers the official period of the GR based on the business cycle dating from the National Bureau of Economic Research. It also coincides with a particular low point in the University of Michigan consumer sentiment index. However, we also obtain similar results if we adjust the GR period to 2009 only, 2009–2010 or 2008–2010, instead of 2008–2009. Again, results can be made available upon request.

[48] Specifically, the NAICS categories include 442,445,447,448,451,452,453, and 722. We only include those categories that also match with the 15 Fiserv categories.

6. Implications

To quantify the importance of the different housing wealth effects, we conduct counterfactual analysis using estimates from our main specification of Table 6 column (5) during the GR.

The results of the counterfactual analysis are shown in Table 11. The top of the Table reports the observed level of spending in 2007 for the 15 industries, which is around \$4 trillion.[49] For our baseline estimates, we apply all of the estimated coefficients in Table (5) and find around \$120-billion decline in spending due to the decline in housing wealth between 2007 and 2009, which is around a 2.4 percent decline relative to the 2007 levels. This effect is decomposed into the effect from changes inside and outside the CZ. Around 43 percent of the effect from the GR is from changes in housing wealth outside of the home CZ (i.e., 1.3/3.0), while 57 percent is within the CZ.

Table 11. Decomposition the Local Geographic Effects of the GR on Spending

Total Revenue (in Millions) of Dollars 2007	3,999,555		
	Chg. Spend (in Millions)	Percent Decline	Share Misallocation
Baseline	-120,623	-3.0	-
Baseline (Within CZ Effect)	-67,820	-1.7	-
Baseline (Outside CZ Effect)	-53,166	-1.3	-
Scenario 1. No Differential CZ Effect	-82,848	-2.1	0.184
Scenario 2. No Differential CZ, No GR Effect	-30,187	-0.8	0.184
Scenario 3. Only Within CZ Effect	-67,820	-1.7	0.311
Scenario 4. Only Within CZ Effect, No GR Effect	-24,711	-0.6	0.311

Note: This table reports the effects of the housing wealth change during the 2007–2009 period based on the regression estimates in Table 6 and column (5). For instance, the baseline estimate shows the total effect of the housing wealth change on spending was around \$121 billion, with \$68 billion coming from changes in housing wealth within the CZ and \$53 billion coming from changes in housing wealth outside of the CZ. Scenario 1 assumes no differential effects outside the CZ; scenario 2 assumes no differential effects outside the CZ, and no differential effect from the GR; scenario 3 assumes effects are only within CZ; and scenario 4 assumes effects are only within CZ and there is no differential effect from the GR. These results highlight the importance of both the larger effects during the GR. The last column of the table also reports the level of misallocation computed as the absolute value of the share of the total effect on spending occurring in each county compared relative to the counterfactual share of spending occurring in each county. In scenario 3 and 4, 31 percent of the total effect would be misallocated.

To explore the importance of the differential effects, the bottom portion of Table 11 examines effects of different scenarios. In scenario 1 we assume no differential effect by geography, that is we assume the coefficients on $\Delta HNW(CZ - export)$ and $\Delta HNW(CZ - export) \cdot Recession$ are zero.

[49] Estimates are in current dollars and not adjusted for inflation.

This counterfactual analysis allows for effects from across-market flows, but it assumes that the effects are symmetric inside and outside of CZ. The effect on spending is about one-third smaller with around a 2.1 percent decline relative to 2007 spending levels. This indicates that the differential effects inside and outside the CZ are economically important.

In addition to calculating the magnitude of the effect, we calculate the geographic allocation relative to the baseline, which we refer to as misallocation. The misallocation captures the difference in the location of the actual effect on spending, as implied by column (5) estimates, compared to the location implied by the scenario (e.g., scenario 1 no differential effects outside the CZ). Scenario 1 misallocation is calculated by first determining the share of the total effect occurring in each county in the baseline calculation, and then recalculating that share assuming the coefficients on $\Delta HNW(CZ - Export)$ and $\Delta HNW(CZ - Export) \cdot GR$ are zero. We then calculate the sum of the absolute value of that difference.[50] If the total difference is 0, then there is no misallocation. At the other extreme, if they are entirely different, the value is 2, indicating that both distributions do not match and are each 100 percent off.[51] When we ignore the differential effect outside the CZ, we calculate a misallocation of 0.18.

Scenario 2 assumes no differential effect outside the CZ and no differential effect during the GR. Eliminating these two effects greatly diminishes the effect of the housing wealth change on spending, with a total decline of just 0.8 percent, which is more than two-thirds smaller than the baseline estimate. Overall, this highlights the importance of both the multiplier effect during the GR and the differential across-market effect. The allocation of the effect is unchanged relative to scenario 1, as this scenario only changes the magnitude of the effect during the GR, not the distribution of the effect across counties.

Scenario 3 is the same as scenario 1, but only allows for changes in housing wealth within a CZ to affect the associated firms in that CZ, and imposes the assumption that flows outside the local CZ have no effect. This analysis demonstrates the importance of these across-market effects and is identical to the baseline (within CZ Effect) in line 2 of the Table. The misallocation in this scenario is 0.31, even larger than scenarios 1 and 2. It is also interesting to note that scenario 1 is about 20 percent larger than

[50] Let S_i^b be the share of the total effect in county i for the baseline case and let $S_i^{\text{No Diff. CZ}}$ be the share of the total effect in county i for the case of not allowing for a differential effect outside the CZ. The misallocation is calculated as $\sum_{i \forall Counties} |S_i^b - S_i^{\text{No Diff. CZ}}|$.

[51] As an example, if there are two counties, A and B, and one assumes the entire effect happened in A, but the entire effect actually happens in B, this indicates that the effect in A will be 100 percent off as will the effect in B.

scenario 3, providing an estimate of the relative importance of effects outside of the CZ when there are no differential effects from the GR.

Scenario 4 assumes only within CZ effects and no differential effects during the GR, and the magnitude of the decline is only one fifth the size of the baseline, demonstrating how both across-market flows and potential multiplier effects from the GR are key determinants of the total effect of the GR. Similar analysis is conducted for employment and shown in the appendix Table A28.

These results demonstrate that excluding the across market effects can greatly understate the aggregate effects from housing wealth changes because a large part of the effect is not local. In addition, the misallocation results show that the location of the effects could be substantially off. The understatement and misallocation of the economic effects could lead to policy responses that are not the optimal size and not directed at the correct locations.

7. Conclusion

We introduce a new data source based on card transaction data that provides estimates of across-county spending flows for the United States, providing a new consumption link across counties that has not previously been studied. We show net exports of consumption vary greatly across counties, and this has implications for how each county is affected by local economic shocks. We find that consumption in one market may have effects on firm revenues and employment across different geographic markets. We find that estimates that ignore spending flows tend to understate the magnitude of the effects of housing wealth changes on spending and employment. The effects are especially understated during the GR, where it appears individuals reduced spending by a greater amount in more distant markets, particularly affecting high export industries (e.g., accommodations, entertainment, and restaurants). Looking at the housing price changes during the GR from 2007 to 2009, we find that 43 percent of the associated decline in spending comes from consumers outside of the CZ.

The estimates in this paper establish the importance of across-county link in consumption for local economic measurement, which has implications for policy design. The effect of local targeted policies on either firms or consumers may have broader effects outside of local markets, depending on the spending patterns of consumers.

More generally, the across-county consumption link is an important aspect of spatial economics that has received relatively little attention, likely due to data limitations, as explained by Redding and Rossi-Hansberg (2017). There are many potential applications to the data constructed in this study. These data may be used to help understand the effects of local tax policies, income shocks to consumers, or policies that affect the population heterogeneously, such as the Affordable Care Act (ACA) of 2010. This data may also help clarify the economic effects of the COVID-19 pandemic, where the typically stable spending flows studied here, were likely disrupted, potentially leading to large changes in spending and employment, especially for high export areas. In addition to these applications, these data may also be used to help define local consumption markets, akin to how labor markets are defined using commuting data to construct CZs. Across-county links in goods and factor markets have been shown to be empirically important, such as in the work by Monte et al. (2018), in which they examine labor demand shocks on employment elasticities using a general equilibrium framework. The across-county consumption link may be an important addition to this literature.

There are a number of potential areas for additional related research. One area is the topic of e-commerce. This was not a limitation for the 2007 to 2009 period, when e-commerce was a relatively small share of consumption, but this is an area of growing importance. Researchers may want to turn to alternative data sources to capture this aspect of spending. Also, we excluded foreign spending to simplify the analysis, but it may be of particular interest in future work to better understand how foreign consumption spending can impact local markets. Finally, for our across-market spending analysis we focus on a single cross-section in 2015. It may be of interest in future work to look at changes in spending flows and the determinants of across-county spending flows over time.

References

Agarwal, S., Jensen, J. B., and Monte, F. (2017). Consumer mobility and the local structure of consumption industries. Working Paper 23616, National Bureau of Economic Research.

Aladangady, A. (2017). Housing wealth and consumption: Evidence from geographically-linked microdata. *American Economic Review*, 107(11):3415–46.

Aladangady, A., Aron-Dine, S., Dunn, W., Feiveson, L., Lengermann, P., and Sahm, C. (2021). *"From Transactions Data to Economic Statistics: Constructing Real-Time, High-Frequency, Geographic Measures of Consumer Spending"*. In Aladangady et al. (2021).

Allcott, H., Diamond, R., Dubé, J.-P., Handbury, J., Rahkovsky, I., and Schnell, M. (2019). Food deserts and the causes of nutritional inequality. *The Quarterly Journal of Economics*, 134(4):1793–1844.

Aysheshim, K., Hinson, J. R., and Panek, S. D. (2020). A primer on local area gross domestic product methodology. *Survey of Current Business*, 100(3):1–13.

Baker, S. R., Farrokhnia, R. A., Meyer, S., Pagel, M., and Yannelis, C. (2020). Income, liquidity, and the consumption response to the 2020 economic stimulus payments. Working Paper 27097, National Bureau of Economic Research.

Bogin, A., Doerner, W., and Larson, W. (2019). Local house price dynamics: New indices and stylized facts. *Real Estate Economics*, 47(2):365–398.

Chetty, R., Friedman, J. N., Hendren, N., Stepner, M., and Team, T. O. I. (2020). The economic impacts of covid-19: Evidence from a new public database built using private sector data. Working Paper 27431, National Bureau of Economic Research.

Chodorow-Reich, G., Nenov, P. T., and Simsek, A. (2021). Stock market wealth and the real economy: A local labor market approach. *American Economic Review*, 111(5):1613–57.

Cox, N., Ganong, P., Noel, P., Vavra, J., Wong, A., Farrell, D., Greig, F., and Deadman, E. (2020). Initial impacts of the pandemic on consumer behavior: Evidence from linked income, spending, and savings data. *Brookings Papers on Economic Activity*, 2020(2):35–82.

Davis, D. R., Dingel, J. I., Monras, J., and Morales, E. (2019). How segregated is urban consumption? *Journal of Political Economy*, 127(4):1684–1738.

Davis, P. (2006). Spatial competition in retail markets: movie theaters. *The RAND Journal of Economics*, 37(4):964–982.

Di Maggio, M., Kermani, A., and Majlesi, K. (2020). Stock market returns and consumption. *Journal of Finance*, forthcoming.

Dolfen, P., Einav, L., Klenow, P. J., Klopack, B., Levin, J. D., Levin, L., and Best, W. (2019). Assessing the gains from e-commerce. Working Paper 25610, National Bureau of Economic Research.

Dubé, J.-P., Hortaçsu, A., and Joo, J. (2021). Random-coefficients logit demand estimation with zero-valued market shares. *Marketing Science*.

Dunn, A., Hood, K., Batch, A., and Driessen, A. (2021). Measuring consumer spending using card transaction data: Lessons from the covid-19 pandemic. *American Economic Review, Papers and Proceedings*, 111.

Gaynor, M., Ho, K., and Town, R. J. (2015). The industrial organization of health-care markets. *Journal of Economic Literature*, 53(2):235–84.

Guren, A. M., McKay, A., Nakamura, E., and Steinsson, J. (2020). Housing wealth effects: The long view. *Review of Economic Studies*, April, 2020.

Hall, R. E. (2009). By how much does gdp rise if the government buys more output? Technical report, National Bureau of Economic Research.

Houde, J.-F. (2012). Spatial differentiation and vertical mergers in retail markets for gasoline. *American Economic Review*, 102(5):2147–82.

Matheny, W., O'Brien, S., and Wang, C. (2016). The state of cash: preliminary findings from the 2015 diary of consumer payment choice. In *Federal Reserve Bank of San Francisco*, pages 11–03.

Mian, A., Rao, K., and Sufi, A. (2013). Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics*, 128(4):1687–1726.

Mian, A. and Sufi, A. (2014). What explains the 2007–2009 drop in employment? *Econometrica*, 82(6):2197–2223.

Monte, F., Redding, S. J., and Rossi-Hansberg, E. (2018). Commuting, migration, and local employment elasticities. *American Economic Review*, 108(12):3855–90.

Nakamura, E. and Steinsson, J. (2014). Fiscal stimulus in a monetary union: Evidence from us regions. *American Economic Review*, 104(3):753–92.

Redding, S. J. and Rossi-Hansberg, E. (2017). Quantitative spatial economics. *Annual Review of Economics*, 9:21–58.

Saiz, A. (2010). The geographic determinants of housing supply. *The Quarterly Journal of Economics*, 125(3):1253–1296.

Stone, R. (1961). *Input-output and national accounts*. Organisation for European Economic Co-operation.

Tolbert, C. M. and Sizer, M. (1996). Us commuting zones and labor market areas: A 1990 update. Technical report, United States Department of Agriculture, Economic Research Service.

A. Appendix

A.1. Economic Census Revenue and Imputation

The Geographic Area Series of the Economic Census (EC) is collected every 5 years at detailed geographic and NAICS industry levels. The EC contains information on industry-level revenues which we use to create measures of consumer spending. Our study focuses on county-level estimates for 15 industries that are important contributors to personal consumption expenditures, and which also have good coverage in the Fiserv database. While EC provides detailed information for many industries at the county level, there are some geography and NAICS combinations that are suppressed. We have used county-level three-digit NAICS industries for 2002, 2007, 2012 and 2017 as our benchmark years.

Table A1 lists the industries included in our analysis with their associated share of suppressed revenues to total revenues for each census year.[52] The levels of suppression vary across industry, but in general are extremely low. Industries such as gasoline stations have high coverage and only suppress 0.5 percent of all receipts. Meanwhile, industries including performing arts, and amusement and recreation had relatively higher suppression rates in early years (10 percent in 2002 and 2007) before filling out more in later years (6.5 and 3 percent, respectively in 2012 and 2017.)

A.1.1. Imputing Revenue for Suppressed Values in Economic Census Benchmark Years

Overall suppression in EC years is quite low, but to obtain complete coverage across counties, we perform some imputations. To address the issue of suppression in the benchmark years, the annual series of the Quarterly Census of Employment and Wages (QCEW) is used to create full set of revenues for all county-NAICS combinations. Annual QCEW data for privately owned establishments provide information on payroll, employment, and wages, and do not contain any suppression across counties. The method used for these imputations is relatively simple and uses wage data to allocate missing revenues across counties.[53]

To impute the revenues in benchmark years, we take three steps. First, we use wages in QCEW to

[52] The rate of suppression is determined by comparing to unsuppressed national estimates.

[53] The method used here is consistent with the method used by the BEA to create consumption estimates using EC revenues.

Table A1. Share of Suppressed Receipts to Total in Selected NAICS Industries (Percentages)

NAICS	NAICS Description	2017	2012	2007	2002
442	Furniture and Home Furnishings Stores	1.9	3.5	1.2	2.0
443	Electronics and Appliance Stores	2.7	2.5	1.4	3.0
444	Building Material and Garden Equipment	1.4	2.1	0.5	0.7
445	Food and Beverage Stores	1.6	2.2	0.4	0.5
446	Health and Personal Care Stores	1.6	2.6	1.0	1.5
447	Gasoline Stations	0.9	1.1	0.3	0.5
448	Clothing and Clothing Accessories Stores	1.2	1.6	1.6	0.8
451	Sporting goods, hobby, book and music stores	2.6	4.6	2.0	2.0
452	General Merchandise Stores	5.2	8.3	11.8	10.0
453	Miscellaneous Store Retailers	7.4	8.8	9.3	10.3
541	Professional, Scientific Services	2.5	4.0	6.0	5.0
621	Ambulatory Health Care Services	1.9	3.4	4.0	4.0
711	Performing Arts, Spectator Sports	3.5	6.5	10	10
713	Amusement, Gambling, and Recreation	5.2	7.5	10	15
721	Accommodation	1.5	2.2	1.4	1.3
722	Food Services and Drinking Places	1.3	2.06	1.0	1.4
811	Repair and Maintenance	0.7	1.3	1.8	1.5
812	Personal and Laundry Services	0.6	1.1	2.8	2.3

Source: Authors' calculation

Notes: The table reports the percentage of spending that is suppressed in the Economic Census data at the county level for the years 2002, 2007, 2012 and 2017. The suppressed share is computed by comparing the national total spending by industry (which is unsuppressed) with the total of all of the unsuppressed county-level revenues by industry. For example, the table shows that 1 percent of the accommodation revenues are suppressed in 2017. North American Industry Classification (NAICS).

impute missing payroll data on EC. Second, we calculate the ratio of payroll to revenue for the non-suppressed receipts by industry. Third, we multiply the payroll data from the QCEW to the ratio of revenue to payroll by industry to impute the missing revenue for NAICS-county combinations.[54]

A.1.2. Imputing Revenues for Intercensal Years

For the two benchmark years t to $t+5$, the revenues are observed $Revenue_t$ and $Revenue_{t+5}$. For the years between ECs, we interpolate revenues using annual QCEW wage data.

[54]The assumption is that if there are wages being paid in that NAICS industry there should be revenue associated with the wages being paid. Only if both QCEW and census receipt are missing or are zero in a location for a specific industry, it is assumed that the revenue is zero.

The interpolation adjusts revenues based on the growth rate in wages, but there is an annual adjustment to account for the divergence in growth rates between revenues and wages over the 5 years of the EC. Let t represent a benchmark year, and let $t+n$ be an intercensal year where n is between 1 and 4. The revenue in year $t+n$ is calculated as:

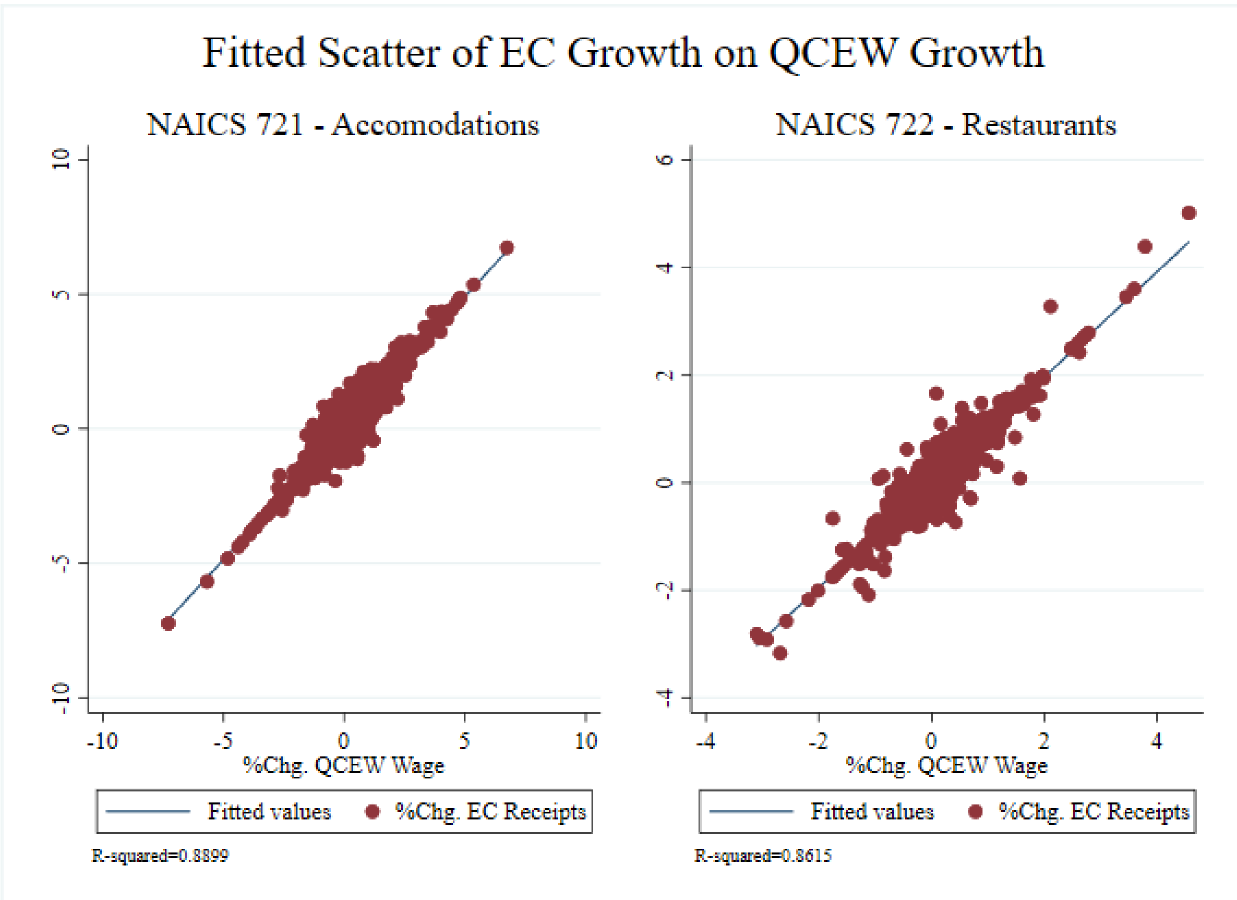
$$Revenue_{t+n} = \left(\frac{Wage_{t+n}}{Wage_t} \cdot Revenue_t\right) \cdot \left(\frac{Revenue_{t+n}/Wage_{t+n}}{Revenue_t/Wage_t}\right)^{(n/5)}$$

The first term, $\left(\frac{Wage_{t+n}}{Wage_t} \cdot Revenue_t\right)$, is the estimated annual revenue based solely on the growth rate in wages. The second term, $\left(\frac{Revenue_{t+n}/Wage_{t+n}}{Revenue_t/Wage_t}\right)^{(n/5)}$, is the annual adjustment to better align changes in wages to predicted revenues. This first term suggests that our estimated changes in revenues may deviate from changes in wages.

While revenue growth is constrained to the growth rate in benchmark revenues, the year-to-year allocation of the 5-year revenue growth is determined by wages. To determine if applying wage data in this way is reasonable, we examine how well wages predict revenues in benchmark years. Figure A1 is the graphical representation of regressing growth rates of EC revenues in the benchmark years on QCEW wage growth rates over the same periods for accommodations (NAICS 721) and restaurants (NAICS 722). The QCEW growth rates are closely correlated with EC growth rates. The R^2 for both accommodations and restaurants is around 89 percent.

This method does quite well more generally. Table A2 shows the R^2 performs well not only for our select industries (in red) but broadly for other NAICS industries too. The R^2 for our select industries are all above 0.70, except for NAICS categories 447 (gasoline stations) and 451 (sporting goods) which have R^2 of around 0.5. The low R^2 for 447 is likely due to gas price fluctuation. Overall, the interpolation of revenue growth using wage data appears to do quite well at approximating revenues for many industries.

Figure A1. Growth in Spending from the EC and Wage Growth from the QCEW



Notes: This figure shows a scatter plot and fitted line of the change in county spending from the Economic Census (EC) on the change in wages from the Quarterly Census of Employment and Wages (QCEW) spanning economic census years. The plot is reported for two three-digit NAICS categories, 721 and 722. The R-squared from additional fitted values is shown in Table A2.

Table A2. Regression Economic Census Growth Rates on the QCEW Growth Rate for Selected Industries for Census Years 2002, 2007, 2012 and 2017

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
NAICS	**441**	**442**	**443**	**444**	**445**	**446**	**447**	**448**	**451**	**452**
R^2	0.691	0.899	0.785	0.872	0.748	0.689	0.530	0.934	0.552	0.955
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
NAICS	**453**	**454**	**481**	**483**	**484**	**485**	**486**	**487**	**488**	**492**
R^2	0.835	0.490	0.674	0.667	0.775	0.915	0.855	0.976	0.879	0.867
	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)
NAICS	**493**	**511**	**512**	**515**	**517**	**518**	**519**	**521**	**522**	**523**
R^2	0.661	0.656	0.930	0.674	0.902	0.485	0.850	0.589	0.891	0.918
	(31)	(32)	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)
NAICS	**524**	**531**	**532**	**533**	**541**	**551**	**561**	**562**	**611**	**621**
R^2	0.955	0.688	0.856	0.556	0.584	0.937	0.178	0.874	0.608	0.800
	(41)	(42)	(43)	(44)	(45)	(46)	(47)	(48)	(49)	(50)
NAICS	**622**	**623**	**624**	**711**	**712**	**713**	**721**	**722**	**811**	**812**
R^2	0.923	0.902	0.613	0.707	0.659	0.810	0.868	0.905	0.786	0.711

Notes: This table demonstrates the relationship between the growth in spending estimates from the EC and the growth in wages from the QCEW. For every three-digit NAICS category we run a regression of the growth in spending from the economic census on the growth in wages over the same period. The table reports the R-squared from each regression, which are typically above 0.7 and above 0.9 for many categories. The three-digit NAICS colored in **red** are the NAICS categories used in our analysis. The wage data from the QCEW is used to interpolate spending estimates between economic census years. The high R-squared values across most categories, suggest that interpolation using wages should perform well.

A.2. Fiserv Data, Spending Flows and the Home Location Algorithm

The micro data from Fiserv contains transaction level information for each firm.[55] Fiserv data contains well over one-third of all United States credit card transaction spending which includes more than 4.5 million United States firm locations and dollar amounts equal to 10 percent of the total GDP of the United States. To maintain the anonymity of card holders and firms, there are a number of suppression rules. The following suppression rules are applied: (1) no series has observation within a given NAICS and geography containing fewer than ten firms, and (2) across the series, no firm makes up more than 20 percent of the transaction volume. The card transactions flows include information on hashed card number, firm ID, transaction date, and transaction amount. For each firm, the firm ID is mapped to the address and firm category code (MCC), which indicates the type of firm, which is mapped to its corresponding NAICS category.

The level of observations is a single transaction, although we do not see the data at this level of detail. The data engineers have access to detailed information on each transaction and they use this information

[55] Throughout this paper we use the term firm to refer to a particular establishment in a county and not the associated parent company.

to form a prediction of the home location (HL) for each card holder in the data, in order to construct the spending flow estimates used in our analysis. The HL algorithm uses transaction patterns to determine the most likely HL of a particular card based on all of that card's transactions across all firms. The raw data for modeling the location of the consumer consists of aggregated transaction counts for each card by three-digit NAICS categories and information on the firm zip codes. The estimated HL is formed based a subset of cards for whom the HL of the cardholder is known. HL is based on a discrete loss function and covariates that help predict the likelihood that consumers reside in different areas. Covariates include information on spending across industries in each potential location. To assess the performance of the prediction, we use a hold out sample of 30 percent to evaluate the accuracy of the algorithm. The algorithm predicts the correct county for each card around 75 percent of the time. This 75 percent estimate may be lower than the actual accuracy for two reasons: (1) the cards that have more spending are likely to have more information on the spending patterns of that cardholder, generating more accurate estimates for those cards that are economically more important; (2) the zip code reported for the known home-location may be imperfect in some instances, such as, college students living away from home. In any case, the overall spending flow patterns from the known-card holder data matches well with the patterns based on the full sample in which the HL algorithm is applied.

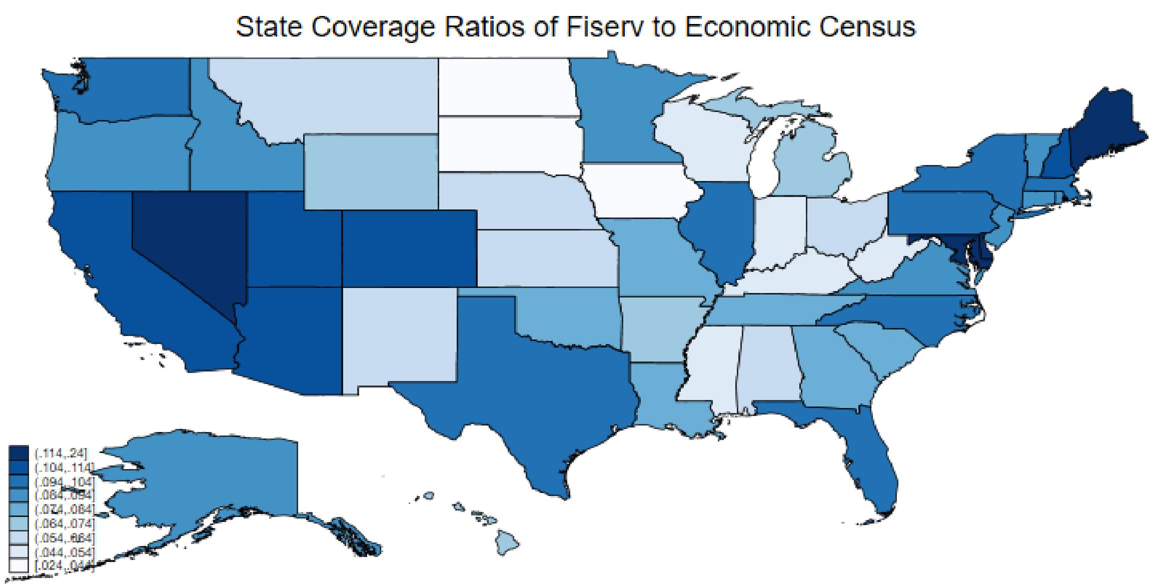
For our analysis we could have chosen either the known HL sample or the full predicted HL sample, as the two are quite similar. However, we chose the full predicted HL sample because it is based on more observations and can also help correct for the cases in which the zip code indicated by the card does not match where the individual actually resides.

An alternative cut of the Fiserv data has been used in research to produce timely regional estimates Aladangady et al. (2021) and timely national estimates around the pandemic Dunn et al. (2021). While the underlying source data is the same, the cleaning of the data used in Aladangady et al. (2021) and Dunn et al. (2021) is focused on providing spending estimates over time. To accomplish this goal, the methodology discussed in detail in Aladangady et al. (2021) systematically excludes firms that might interfere in accurately measuring changes in spending over time (e.g., a firm entering or leaving Fiserv's system during the sample period). In contrast, the focus of our paper is to derive accurate cross-sectional estimates of spending between consumers and firms across areas, so we include the full set of firms available.

A.2.1. Fiserv Coverage

Figure A2 shows coverage of spending across states in the U.S. for the select categories. There is variation in the level of coverage across states, but we see coverage in all 50 states. The median state has a coverage rate of 8.5 percent.[56] While the geographic coverage is a potential limitation, all of the estimates are scaled to the estimated EC across all regions to capture 100 percent of spending. Scaling the Fiserv spending to the estimated EC helps to address both the differential coverage across areas, and to address the issue that some populations use card transactions more than others (e.g., high-income vs low-income populations).

Figure A2. Coverage Map of Fiserv Relative to the 2012 Economic Census



Notes: The map shows coverage of spending across states in the U.S. for the select categories. There is variation in the level of coverage across states, but we see coverage in all 50 states. The coverage ratios are calculated relative to the 2012 Economic Census.

The main assumption is that the observed spending flows are representative of spending flow patterns for that area, which allows us to scale estimates to the EC to produce meaningful spending flow data. An analysis of our spending flow data, through both descriptive statistics and regression analysis, suggest that the data are reasonable and match expected patterns. For instance, spending declining with distance away from the firm's location, spending varying by industry in expected ways, and more spending coming from counties with higher incomes.

[56]The coverage rate is computed as the ratio of aggregate Fiserv spending over all 15 categories for each state divided by the aggregate spending total for those 15 categories estimated for 2015.

A.3. Estimating Final Expenditure Flows

To obtain a complete system of consumption flows for the United States, we need to estimate the consumption flows in locations where the Fiserv estimates are suppressed. Overall, imputation is needed for 14 percent of spending for our select categories. The goal of our imputation is to provide the best possible estimate for these missing expenditures. We examined a variety of flexible linear models to impute the missing spending flows, then we chose the method that performed the best based on cross-validation, a model validation technique, from a holdout sample.[57]

One factor that helps with imputation is that even when spending flows are suppressed, our data provide information regarding the set of counties where consumers are coming from, so we do not need to impute the set of potential counties. For instance, if NAICS category 448 (clothing) is suppressed in Montgomery County, Maryland, we still observe the set of counties that customers came from to purchase in 448, but we do not observe the actual spending shares across locations. To impute the share of revenues for firms in industry n and county j going to location i, we estimate a flexible linear regression model with the log share of spending on the left-hand side $log(S_{i,j,n})$. Importantly, the right-hand side of the equation includes a county-pair fixed effect, $\tau_{i,j}$, to capture economic activity occurring between two counties, using shares observed in other industries to help impute the industry share. For instance, suppose the share of a firm's revenues from a particular county for general merchandise stores is missing, but restaurants are observed. The county-pair fixed effect will capture the observed economic activity between locations in food services to help infer the amount of activity between areas for general merchandise stores. The right-hand side also includes a number of additional covariates, including revenues ($R_{j,n}$), distance ($distance_{i,j}$), population (pop_i), and industry fixed-effects ($industry_n$). The function $f()$ is specified as a flexible model that includes interactions of these variables and polynomials of distance. For instance, it includes polynomial of distance interacted with industry fixed effects and distance interacted with revenues and population. The model is specified as:

$$\log\left(S_{i,j,n}\right) = f(R_{j,n}, distance_{i,j}, pop_j, industry_n) + \tau_{i,j} + \epsilon_{i,j,n}. \tag{6}$$

[57]The holdout method randomly divides the data into training and testing sets. To find the best model, each model is estimated using the training set only. Then we use the model to predict the output values for the data in testing set.

The term $\epsilon_{i,j,n}$ is the error term. The imputed share is then calculated using the exponential of the expected value: $ImputedShare_{i,j,n} = \frac{exp(\log\widehat{(S_{i,j,n})})}{\sum_i exp(\log\widehat{(S_{i,j,n})})}$. For the relatively small number of areas where the county-pair fixed effects cannot be included, we use flexible linear regression models without fixed effects to impute these values.

We test a variety of alternative models and examine the fit in the holdout sample based on mean squared error. We selected the methodology with the smallest mean squared error based on a 5 percent hold-out sample.

A.4. Representativeness of Payment Flows

This section investigates the representativeness of the spending flow data. One particular concern with the spending flow data is that card transactions are not equally likely to be used for all demographic groups, with card transactions being used more heavily by higher income groups or groups with higher education levels (see Atlanta Fed Survey of Consumer Payment Choice, Matheny et al. (2016)). This bias is particularly large for credit cards, as low-income households use this payment much less frequently. However, the Fiserv data captures all card types, including credit cards, debit cards and Electronic Benefit Transfer (i.e., cards used by Supplemental Nutrition Assistance Program (SNAP) participants), which are used by lower income populations. In other words, the Fiserv data are more representative than data sources relying only on credit cards. While we think the bias is smaller than credit card data sources, there is still a notable difference in total card transactions, as households with incomes below \$25,000 use credit or debit cards for around 33 percent of payments while households with incomes of \$25,000 or more use credit or debit between 40-50 percent of the time.

We find that this difference shows up in our data, although it is not statistically significant. To see this, we run a regression of the level of coverage as the dependent variable (i.e., ratio of Fiserv spending to EC estimates) in a regression that includes NAICS-level fixed effects and covariates for log per capita income and log population density. Also, do to suppression rules affecting coverage, we have to account for the size of the market and the probability that Fiserv will be present in the market. We include the log of total spending in the market and industry fixed effects. The results are reported in Table A3. In column (1) we find a 10 percent increase in the log per capita income leads to a 0.1 percent increase in coverage, although the effect is not statistically significant. Population density in column (2) is not correlated with coverage and column (3) includes both coverage and population density and shows no significant relationship.

Table A3. Regression of Coverage on Per Capita Income and Density

	(1) Coverage	(2) Coverage	(3) Coverage	(4) Coverage
Log(Per Capita Income)	0.0230***		0.0212***	0.0141*
	(0.00761)		(0.00694)	(0.00778)
Log(Population Density)		0.00269*	0.00158	
		(0.00153)	(0.00133)	
N	37213	37205	37205	37213
R squared	0.105	0.104	0.105	0.118
State FE	No	No	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The left-hand side variable of this regression is a measure of coverage in the Fiserv data, relative to the EC. The covariates include log of per capita income and log of population density at the county level. Control variables include industry fixed effects and the log of total spending in the area based on our estimate of the EC in 2015. In column (4) we also control for state fixed effects. The controls are used to account for the presence of Fiserv as well as suppression rules that might vary with the total amount of spending in an area. Estimates are clustered at the county level.

To gauge the magnitude of the possible bias, we look at the difference between the 10th and 90th percentile per capita income levels and multiply by the coefficient 0.01 to measure the effect on coverage, which is equal to 0.06 percent relative difference in coverage. Much of the variation is potentially explained by broad regional differences in coverage (e.g., stronger Fiserv presence in certain areas), rather than income-specific effects. To see if this is the case, we run the same regression, but include state fixed effects, column (4). We still find a positive relationship between income and coverage, but the coefficient falls to 0.009, which implies that the difference in coverage is 0.5 percent between the 10th and 90th percentiles in per capita income levels. This is relative to an average coverage of 10.5 percent for the average county that reports positive coverage.

This potential bias may be greatly alleviated by scaling all of the data to the level of the EC, so that firms in both low-income and high-income areas match the EC. After this rescaling, we use the consumption flows to estimate the consumption to income ratio across areas. That is, we send the consumption to the location of the consumer and calculate the ratio of consumption to income in each county, we then report this ratio by income quartile in Table A4. If the bias toward higher income areas is high, we should expect a low consumption to income ratio in the low-income counties. In contrast, we find the share of consumption to income to be slightly larger (mean of 0.36) relative to the highest income quartile (mean 0.32).

We think these estimates of consumption to income are reasonable. Using external data from the Consumer Expenditure Survey (CEX) for 2015 and 2016, we also observe a relatively constant consumption to income ratio across geographies with different income levels. More precisely, we use spending categories comparable to our 15 select industries in the CEX data and look at the most disaggregate geographic detail available in the CEX data, the primary sampling unit (PSU), and we find a fairly constant consumption to income ratio across PSU income quartiles.

Table A4. County Consumption to Income Share by Per Capita Income Quartile

	Mean	Median	SD	N
1st Quartile	0.359	0.360	0.115	779
2nd Quartile	0.365	0.366	0.077	784
3rd Quartile	0.364	0.370	0.076	779
4th Quartile	0.328	0.321	0.137	779
Total	0.345	0.340	0.116	3,121

Note: Using data scaled to the EC for 2015, we calculate the share of consumption to income in all counties. Consumption is calculated for the select 15 industries in our data, and we "send" the consumption to the location of the consumer to form the consumption to income ratio. This table reports the share of consumption to income by per capita income quartile of the county.

Overall, the evidence in this section suggests that the representativeness of the payment flows data appear reasonable, especially after the adjustment to the EC spending levels. However, there is still the potential for bias because, conditional on a certain level of coverage, the spending flows across areas could still be affected based on consumer tendencies to use cards. For instance, coverage could be equal in all counties, but a disproportionate share of the flows could come from areas that more heavily use card transactions. For this reason, we also propose an alternative adjustment to the flows in section A.6.

A.5. Consumption Flow Accounting

The level of spending by consumers in a county must be equal to the amount of final consumption sold, minus the export of consumption to other areas by firms in the county, plus the imports of consumption by consumers traveling to other counties to consume, as shown in equation (7):

$$\begin{aligned}\text{Household Consumption} =& \text{Final Product Sold} - \text{Export of Consumption}\\ &+ \text{Imports of Consumption}\end{aligned} \tag{7}$$

We use this basic accounting relationship for two purposes. First, we use it as part of an exercise to test this accounting relationship empirically to validate the data. Second, we use the accounting formula to correct for potential biases that may exist in card transaction data, by forcing a reconciliation between the flows based on the card transaction data and independent estimates of consumption and sales across regions. The adjustment method we apply is related to a biproportional RAS method pioneered by Stone (1961) to apply to input-output matrices.

A.5.1. A Simple Test of Correlation

We use the accounting relationship to both test the validity of the data, which also highlights the importance of these cross-market spending flows in understanding the consumption link across counties. To test this relationship, we first need empirical counterparts for each element. The empirical components on the right-hand side are constructed using spending flow and revenue measures, while we use an independent source for the empirical measure of consumption on the left-hand side. Therefore, empirically estimating this relationship provides an external validity check on the data and this accounting relationship.

Moving from left to right of equation (7), the first estimate that is needed is an independent measure of household consumption. Household consumption at the county level is not an official statistic that currently exists. Indeed, one motivation for working with spending flow measures is to obtain a county-level measure of consumption, potentially from the right-hand side of the accounting relationship. However, we can empirically approximate an independent value assuming that consumer preferences are homothetic at the county level. This allows us to assume a constant share of income is devoted to the goods and services in our 15 select NAICS categories. We further assume that this budget share is constant across the entire United States for a given year. With this assumption, we then look at the national budget share of consumption going to our NAICS categories, which averages to be 38 percent of income. Next, we multiply the national budget share in each year by the income in each county from the BEA to obtain an estimate of consumption in county j, $\text{Household}\widehat{\text{Consumption}}_{j,t}$.

The next necessary element for equation (7) is an estimate of $\text{Final}\widehat{\text{Product}}\text{Sold}_j$ in county j. This estimate is taken directly from our spending estimates based on the EC data where the total spending over industries n is aggregated:

$$\text{Final }\widehat{\text{Product}}\text{ Sold}_j = R_j = \sum_{\forall n \in I} R_{j,n},$$

where $R_{j,n}$ is the total sold by firms in the county j for industry n and set of industries I.

The estimate of the exports of consumption is the total amount sold by firms in the county to consumers that reside outside of the county. This is calculated as:

$$\text{Exports of }\widehat{\text{Consumption}}_j = \sum_{\forall n \in I} \sum_{\forall i \in C s.t. i \neq j} R_{j,n} S_{i,j,n}$$

where $S_{i,j,n}$ is the total share of revenues for firms in industry n located in county j selling to consumers that reside in county i. The estimated share, $S_{i,j,n}$, is based on 2015 estimates, so the implicit assumption is that these shares are constant across years in the sample.

We conduct a similar exercise to estimate dollar amount of imports coming from a county. The estimate of consumption import is the total amount consumed outside of a county by consumers that reside in county j. This amount may be estimated as:

$$\text{Imports of }\widehat{\text{Consumption}}_j = \sum_{\forall n \in I} \sum_{\forall k \in C, s.t. i = j, k \neq j} R_{k,n} S_{i,k,n}$$

After obtaining the empirical counterpart for each element of (7), we can estimate a simple regression model to test the accounting relationship:

$$\begin{aligned}\text{Household }\widehat{\text{Consumption}}_{j,t} =&\beta_1(\text{Final }\widehat{\text{Product}}\text{ Sold}_{j,t}) - \beta_2(\text{Exports of }\widehat{\text{Consumption}}_{j,t})\\ &+ \beta_3(\text{Imports of }\widehat{\text{Consumption}}_{j,t}) + \epsilon_{j,t}\end{aligned} \tag{8}$$

If consumption flows are important, we should reject the hypothesis that they are equal to zero $\beta_2 = \beta_3 = 0$. In addition, if the accounting relationship holds, then we should not be able to reject the hypothesis $\beta_2 = \beta_3 = 1$.

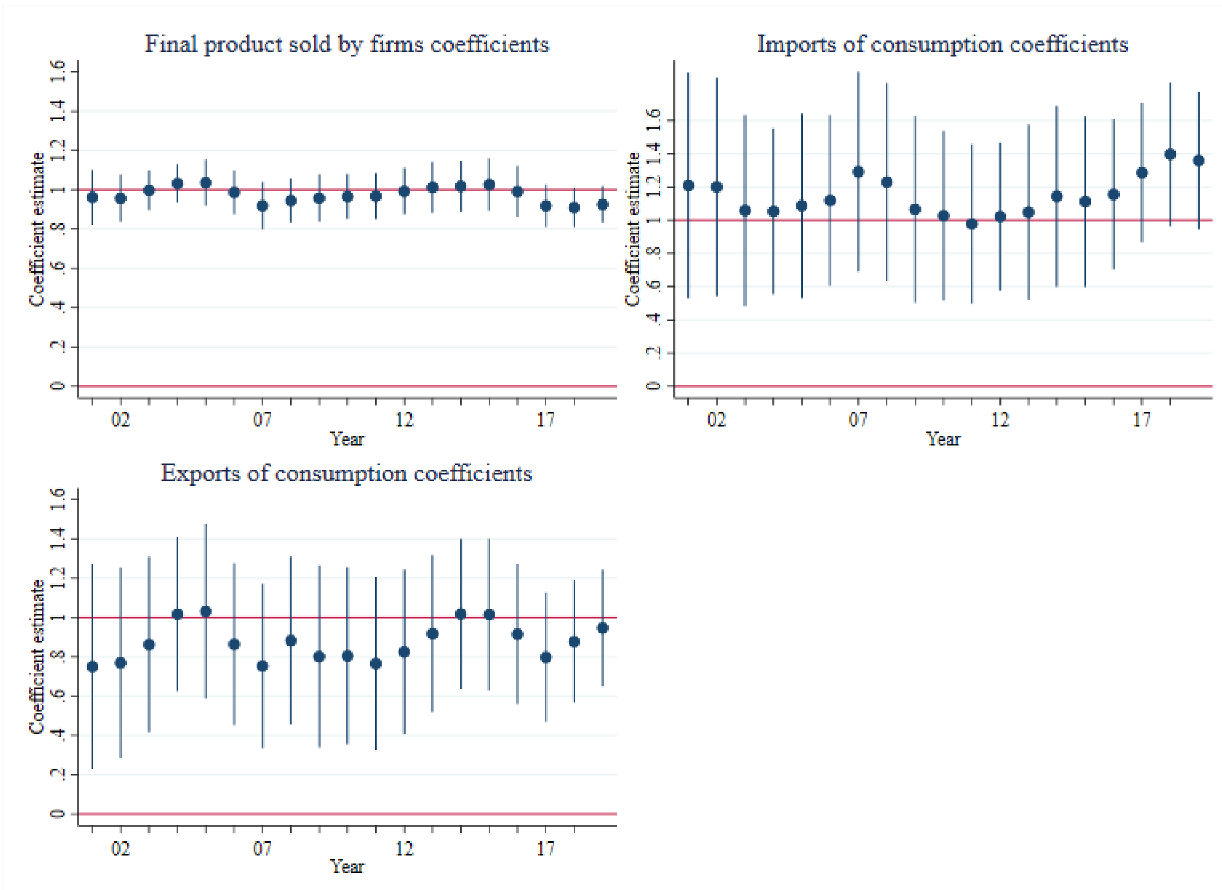
The empirical test is run in a joint regression for every year and county in our data from 2001 to 2019, but with different coefficients for each year. The coefficient for each year is shown in Figure A3. Across all years we see that we can strongly reject the hypothesis that our consumption import and export measures are insignificant $\beta_2 = \beta_3 = 0$, as the estimates are significantly different from zero in each year. The import and export coefficients center around 1 across all years, and we cannot reject the hypothesis that estimates are equal to 1 in any year with a 95 percent confidence interval. In other words, we cannot reject the hypothesis that this accounting relationship holds in the data.

We find this strong relationship despite the possibility of measurement error entering the equation from multiple sources. In particular, there may be measurement error from assuming shares $S_{i,k,n}$ are constant across years, from the Fiserv data measurement error, or from assuming a fixed share of income goes to consumption across counties (i.e., the right hand side). If these measurement errors are large, this increases the likelihood of attenuating these estimates and reducing the statistical significance of the import and export variables. As we find a strong statistical relationship across years, it suggests that the assumptions (e.g., stable shares) are reasonable and the measurement error is low.

These estimates suggest that the right-hand side of the accounting relationship provides meaningful information about the components of consumption at the county level, which will be the focus of the analysis of the GR. It also suggests that the 2015 spending flows are relatively stable across years, including from 2007 to 2009.

The assumption of relatively stable shares is applied when analyzing the effects of the GR, where we apply 2015 spending flows to our estimates. Although we relax this assumption in some robustness checks where we estimate the predicted spending flows in all years, rather than use observed spending flows in 2015.

Figure A3. Regression Coefficients from Accounting Tests Across Years



Notes: This figure shows the coefficient estimates from the regression equation (8). The regression is run on the full sample of counties and years with interactions of both counties and years using the income in the county in 2007 as a weight and clustering the standard errors at the state level. The upper left box shows the coefficient based on total sales by firms in the county. The upper right box shows the coefficient on imports of consumption. The lower left box shows the coefficient on exports of consumption. The blue dots represent the point estimates for the coefficient and the vertical lines represent the 95 percent confidence interval of the coefficients. Based on our regression results shown in these graphs the hypothesis that our consumption import and export measures are insignificant is strongly rejected. The import and export coefficients center around 1 across all years, therefore we cannot reject the hypothesis that the accounting relationship holds in the data.

A.6. Adjustment for Potential Bias

After scaling the data to be representative of the EC totals to capture all of the spending for the select industries, we argue that the card transaction data provide accurate measures of spending flows. We do an external validity check using the accounting relationship in equation 8 and find that the accounting relationship generally holds. We also check whether there are obvious systematic biases after rescaling to the EC, such as much larger levels of consumption, relative to income, in high-income areas, where consumers likely use card transactions more. We find no evidence of large difference by income level.

Despite all of this evidence, it is still possible for biases to enter through, for example, differences in card usage across areas. As an additional robustness check we produce an alternative set of flows to correct for any systematic bias from differences in card transaction use across areas. The basic intuition is that we can view the accounting relationship, and specifically the RHS of equation (7), as a matrix, where the rows add up to total household consumption, and the columns add up to total production. With this accounting relationship, Stone (1961) shows that knowing information on the total for the rows and columns, we can come up with a new estimate for the matrix, using the RAS biproportional smoothing methodology.

Let M be a I by J matrix where J represents all of the counties that firms sell to consumers and let I be the set of all counties where consumers reside. The element of the matrix $M_{i,j}$ represents the total amount of spending from a consumer located in county i at firms in county j. The total amount sold by firms in county, j, can be calculated by adding the rows of column j to obtain a column total. The total amount purchased by consumers located in county, i, can be calculated by adding the columns to obtain a total for each row. In our data, the elements of the matrix M are calculated by multiplying the observed spending flows across areas with our estimate of the firm spending in that location.

The main issue with this estimate of the matrix M is that the amount consumers use cards in transactions may vary across areas, and this could potentially lead to consumption levels that are either too high or two low. Using a RAS intuition, we can apply an adjustment factor to each row so that the level of consumption is closer to an independent estimate of consumption. We think that a reasonable independent estimate of consumption can formed as a share of total income in the county, which is partly validated by the accounting test of equation (8).

For this adjustment, we treat the estimated matrix as an initial estimate, M^0, and make adjustments based on independent data on the level of consumption for each consumer i, c_i^*, where our independent measure of consumption is based on consumer income. Suppose the level of consumption for consumer i based on the matrix M^0 is c_i^0 (where c_i^0 is calculated by summing row i of M^0) then the adjustment term for that row is $\frac{c_i^*}{c_i^0}$. Each row is multiplied by this adjustment term to get a level of consumption that is consistent with our external estimate. Each component of the matrix is derived as: $M_{i,j}^1 = M_{i,j}^0 \cdot \frac{c_i^*}{c_i^0}$. This gives us a revised matrix M^1. Using this revised matrix, we can calculate revised flows, where the amount of revenue for firms located in county j will be calculated by summing the rows of M^1 for column j, so that we get $R_j^1 = \sum_{\forall i \in I} M_{i,j}^1$. The share of revenue that firm j receives from consumers residing in county i using the matrix M^1 is then $\frac{M_{i,j}^1}{R_j^1}$. We then calculate our main estimates using this adjusted matrix.

To apply this method to our data, we use the national budget share for our selected categories of 38 percent, discussed in the previous section. We multiply the national budget share of 0.38 by the county income to obtain the adjusted consumption level, c_i^*. We then apply the adjustment described in the previous paragraph to arrive at our adjusted flows. The correlation of the main flow estimates and the adjusted flows is 0.90. That is, we find the correlation in shares to be quite high, despite strong assumptions imposed by the proposed adjustment in this section.

A.7. Spending By Industry and Distance

This section provides additional information regarding spending by industry and distance away from the home county of the firm. Table A5 shows share of spending based on the distance between the firm and the home location of the consumer weighted by spending. The first column indicates the share of spending coming from consumers that reside in the same location as the firm. The information provides similar information to that in Figure 1, but presents it in numerical form for all industries.

We use Table A5 to categorize industries into three broad industry groups based on the share of spending coming from the home location. We divide the broad industry groups so that roughly one third of spending is in each group. The first group is a "home industry" group were a large share of spending is from consumers that reside in the same county as the firm, which includes NAICS categories 445, 452, and 444. The second group is an "export industry" group were a relatively large share of spending is from consumers that reside away from the firm's home location, which includes NAICS categories 722, 442, 453, 451, 713, 448, 711, and 721. The third group is an intermediate group that falls between the other two, which includes NAICS categories 812, 811, 621, and 447.

Table A5. Spending Share By Distance Weighted By Spending

	Share Home	Share Under 100 Miles	Share 100 to 500 Miles	Share 500+ Miles
Accommodation (NAICS 721)	0.136	0.145	0.314	0.404
Ambulatory Health Care Services (NAICS 621)	0.700	0.216	0.034	0.049
Amusement, Gambling, and Recreation Industries (NAICS 713)	0.565	0.227	0.080	0.128
Building Material and Garden Equipment and Supplies Dealers (NAICS 444)	0.757	0.184	0.027	0.032
Clothing and Clothing Accessories Stores (NAICS 448)	0.565	0.257	0.072	0.106
Food Services and Drinking Places (NAICS 722)	0.670	0.203	0.062	0.066
Food and Beverage Stores (NAICS 445)	0.834	0.107	0.025	0.034
Furniture and Home Furnishings Stores (NAICS 442)	0.606	0.258	0.053	0.084
Gasoline Stations (NAICS 447)	0.694	0.182	0.077	0.047
General Merchandise Stores (NAICS 452)	0.767	0.162	0.033	0.038
Miscellaneous Store Retailers (NAICS 453)	0.616	0.196	0.071	0.117
Performing Arts, Spectator Sports, and Related Industries (NAICS 711)	0.424	0.248	0.104	0.224
Personal and Laundry Services (NAICS 812)	0.739	0.172	0.035	0.054
Repair and Maintenance (NAICS 811)	0.724	0.192	0.039	0.045
Sporting Goods, Hobby, Book, and Music Stores (NAICS 451)	0.609	0.230	0.065	0.096

Notes: The table reports spending shares by industry and distance between the firm and consumer counties. The four distance categories include: (1) the share home (indicating spending share of consumers that reside in the same county as the firm); (2) share under 100 miles (indicating spending share of consumers that reside outside of firms county, but whose county's population centroid is less than or equal to 100 miles); (3) share 100 to 500 miles (indicating spending share of consumers that reside outside of firms county but whose county is more than 100 miles away, but less than or equal to 500); and (4) share 500 (indicating spending share of consumers that reside more than 500 miles from the home location of the firm).

While Table A5 shows differences in spending by industry, it is important to note that this information is weighted by spending, and this weighting will disproportionately weight more populated areas of the United States. To show the variation across counties in the data, Table A6 shows the distribution of the share of spending in the consumers home location across counties in the U.S. Table A6 shows substantial variation in the amount that different counties and industries rely on exports of consumption outside of the firm's county. For example, for food and drinking establishments (NAICS 722) the 10th percentile county shows just 10 percent of the revenue coming from consumers that reside in the county, while the 90th percentile shows that around 91 percent of revenues come from consumers that reside in the county.

Table A6. Distribution of Spending Share From Consumers that Reside in the Same County as the Firm

	Median	10th	25th	75th	90th
Accommodation (NAICS 721)	0.152	0.058	0.104	0.215	0.312
Ambulatory Health Care Services (NAICS 621)	0.760	0.563	0.664	0.873	0.939
Amusement, Gambling, and Recreation Industries (NAICS 713)	0.509	0.233	0.353	0.665	0.788
Building Material and Garden Equipment and Supplies Dealers (NAICS 444)	0.824	0.639	0.738	0.893	0.941
Clothing and Clothing Accessories Stores (NAICS 448)	0.590	0.359	0.482	0.703	0.852
Food Services and Drinking Places (NAICS 722)	0.633	0.409	0.527	0.712	0.769
Food and Beverage Stores (NAICS 445)	0.829	0.657	0.760	0.880	0.909
Furniture and Home Furnishings Stores (NAICS 442)	0.591	0.353	0.470	0.726	0.897
Gasoline Stations (NAICS 447)	0.651	0.442	0.545	0.736	0.795
General Merchandise Stores (NAICS 452)	0.811	0.646	0.736	0.867	0.918
Miscellaneous Store Retailers (NAICS 453)	0.617	0.353	0.492	0.723	0.820
Performing Arts, Spectator Sports, and Related Industries (NAICS 711)	0.315	0.098	0.186	0.437	0.578
Personal and Laundry Services (NAICS 812)	0.762	0.556	0.671	0.840	0.916
Repair and Maintenance (NAICS 811)	0.735	0.507	0.629	0.833	0.909
Sporting Goods, Hobby, Book, and Music Stores (NAICS 451)	0.665	0.444	0.557	0.798	0.937

Notes: For each county and each industry in the data we compute the share of spending coming from consumers that reside in the same county as the firm. The table reports the distribution of that share across all counties in the data. For example, for food services and drinking places (722) the median county receives 56 percent of their spending from consumers that reside in the same county as the firm.

A.7.1. Predicted Shares: Relaxing Assumption of Constant Shares from 2015

Our main estimates assume that the location of potential demand is reflected in spending flow shares observed in 2015. The accounting test that we include from the estimates of equation (7) provides evidence that these shares are relatively stable over time. However, shifts in spending flows over time could potentially reduce the precision of the estimates. Ideally, we would use spending flows observed in each year to more accurately capture potential consumption at each period.

To relax the assumption of constant spending flows, we predict the share of revenues that a firm receives from consumers residing across all counties in the United States for the year 2015 across all 15 of our industries. The prediction model relies on spending information at firms that is observed in both 2015 and in the prediction year, for example 2007. We first estimate the model using the 2015 income, population, and revenue information. Next, we substitute in the prediction year data on income, revenues, and population (e.g., 2007 data) and apply the estimated covariates from the regression model for 2015. Finally, using the model parameters based on 2015 estimates, we predict the spending flows for the prediction year (e.g., 2007). As the goal of this model is prediction, we specify a flexible functional form, which includes the log of the income of consumers in the county, the log of receipts plus one for firms in the county for that industry, polynomials of distance, industry-specific fixed effects, and numerous interactions of these variables (e.g., distance and industry)

To form our prediction, we apply a conditional logit model that is related to the Constant Elasticity of Substitution (CES) functional form (see Dubé et al. (2021)). For all of the markets we assume the outside good is the home county of the firm for a particular industry (e.g., for restaurants in Montgomery County, Maryland, the outside good is the share of spending going to consumers that reside in Montgomery County, Maryland.). The market shares of each industry sum to one, but the regression models for each industry are run jointly across industries to include common covariates across industries that might affect the market share. Recall that the share, $S_{i,j,n}$, is the share of spending at firms in industry n, located in county j, and sold to consumers residing in county i and the outside good share is $S_{i=j,j,n}$. The conditional logit model may be estimated using the following linear functional form based on 2015 data:

$$\begin{aligned} log(S^{2015}_{i,j,n}) - log(S^{2015}_{i=j,j,n}) = \\ g(\delta_j, distance_{i,j}, income^{2015}_i, income^{2015}_j, spend^{2015}_{i,n}, spend^{2015}_{j,n}, NAICs_n) + \gamma^{2015}_{i,j,n} \end{aligned} \tag{9}$$

The term $g()$ indicates a flexible functional form where log functional forms and interactions are applied among these different variables, where δ_j is a vector of parameters to be estimated. To simplify notation, denote the function g as $g(*)^{2015}$. The term $\gamma^{2015}_{i,j,n}$ is the error term. Based on this functional form, the spending share for consumers coming from county i may be calculated as:[58]

$$S^{2015}_{i,j,n} = \frac{exp(g(*)^{2015} + \gamma^{2015}_{i,j,n})}{1 + \Sigma_{\forall i \in C} exp(g(*)^{2015} + \gamma^{2015}_{i,j,n})} \tag{10}$$

Equation (9) is estimated using a linear regression model using population weights based on the firm's home market in 2007. For the potential set of counties, we only use those counties for which we observe some consumers purchasing in 2015.[59] After running the predictions of the model for 2015 using 2015 covariates, we use the variables from the prediction year (e.g., 2007) to predict shares in that year. We

[58] The home market share for the case where $i = j$ is: $S^{2015}_{i=j,j,n} = \frac{1}{1+\Sigma_{\forall i \in C} exp(g(*)^{2015} + \gamma^{2015}_{i,j,n})}$

[59] For example, for restaurants in Montgomery County, Maryland if we see consumers from 1,000 counties, then those 1,000 counties will enter our prediction model and others will be excluded. This will likely exclude very rural counties in the set of possible locations for many markets.

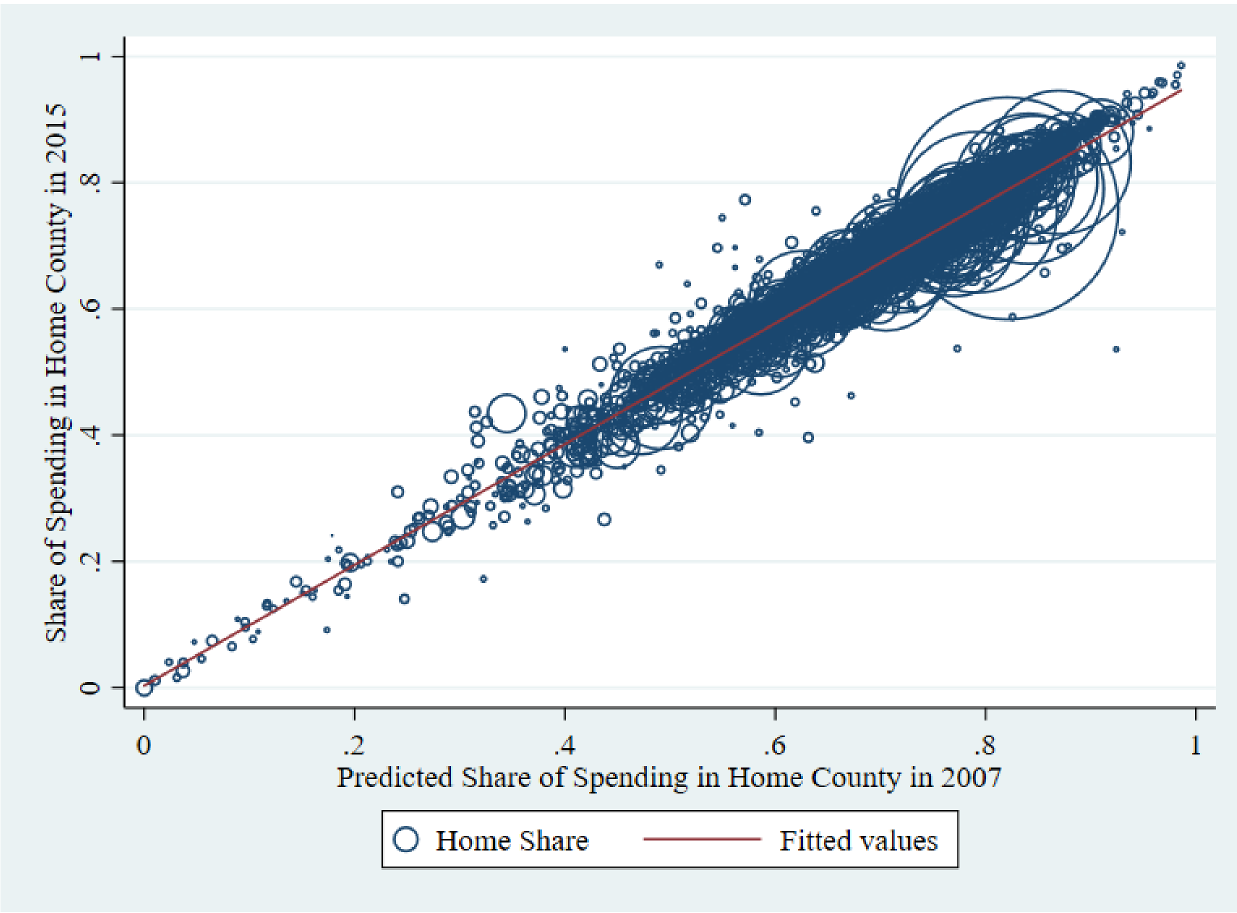
assume that the mean error term, $\gamma_{i,j,n}^{2015}$ does not change across years, so the error term from the 2015 prediction model is applied in the prediction year. For example, suppose we are predicting for the year 2007. If we let $\widehat{g(*)^{2007}}$ be the fitted values from the linear regression model using 2007 data, then the predicted shares for 2007 are calculated as:

$$\widehat{S_{i,j,n}^{2007}} = \frac{exp(\widehat{g(*)^{2007}} + \widehat{\gamma_{i,j,n}^{2015}})}{1 + \Sigma_{\forall i \in C} exp(\widehat{g(*)^{2007}} + \widehat{\gamma_{i,j,n}^{2015}})} \tag{11}$$

To compare the predicted 2007 shares with the 2015 shares we calculate the aggregate share of spending across all 15 industries for both the predicted 2007 shares and the 2015 shares. We aggregate over the 2007 shares using 2007 spending estimates in each county and we aggregate over the 2015 shares using the 2015 spending estimates. To compare these spending flow estimates we focus on the aggregate spending share from the home county (i.e., what share of spending is from consumers that reside in the same county as the firm). Figure A4 shows a scatter plot and fitted line of this predicted home share in 2007 on the home share observed for 2015. We find the two measures to be highly correlated and the associated regression has a regression coefficient of 0.95. This high degree of correlation is somewhat expected as many aspects of the geography are unlikely to change dramatically over this period (e.g., population, county borders, geographic features, infrastructure, etc.)

Next, to investigate the robustness of our results to the fixed-share assumption, we calculate the housing net wealth variable applying the exact formula applied in equation (2), but using predicted shares specific to the base year, rather than fixed shares for 2015. We then repeat the analysis from our main tables, but using the predicted flows. The results are reported in Tables A25 and A26.

Figure A4. Regression of the Predicted Home Share of Spending in 2007 on the Observed Home Share of Spending in 2015.



Note: The scatter plot is based on the aggregate home shares across all 15 industries in 2015 and the corresponding predicted home share across all 15 industries in 2007. The red line is the fitted value, which indicates a strong positive relationship between the predicted and observed shares.

A.8. House Price Index Data

The main housing price data used in this project is from the Federal Housing Finance Agency (FHFA). Specifically, we use the county annual housing price index, discussed in detail in Bogin et al. (2019) and called the Annual House Price Indexes, Counties (Developmental Index; Not Seasonally Adjusted).[60] The county price information is based on a repeat purchase index and covers around 2700 counties over our sample period.

For cases where the FHFA county index is unavailable, we use the Zillow home value index (ZHVI). ZHVI is seasonally adjusted measure of typical home value and market changes across a given region and housing type. Zillow publishes ZHVI for all single-family residences, for condo or coops, for all homes with 1, 2, 3, 4 and 5 and more bedrooms, and the ZHVI per square foot. We focus on change in home prices using county-level data which cover approximately 2000 counties within the United States. The data is available at: https://www.zillow.com/research/data/. For areas where Zillow and FHFA data overlap, we find the price changes to have a correlation of 0.95. For the small number of rural counties missing price change information in FHFA and Zillow, we use the price change from other counties within the same CZ.

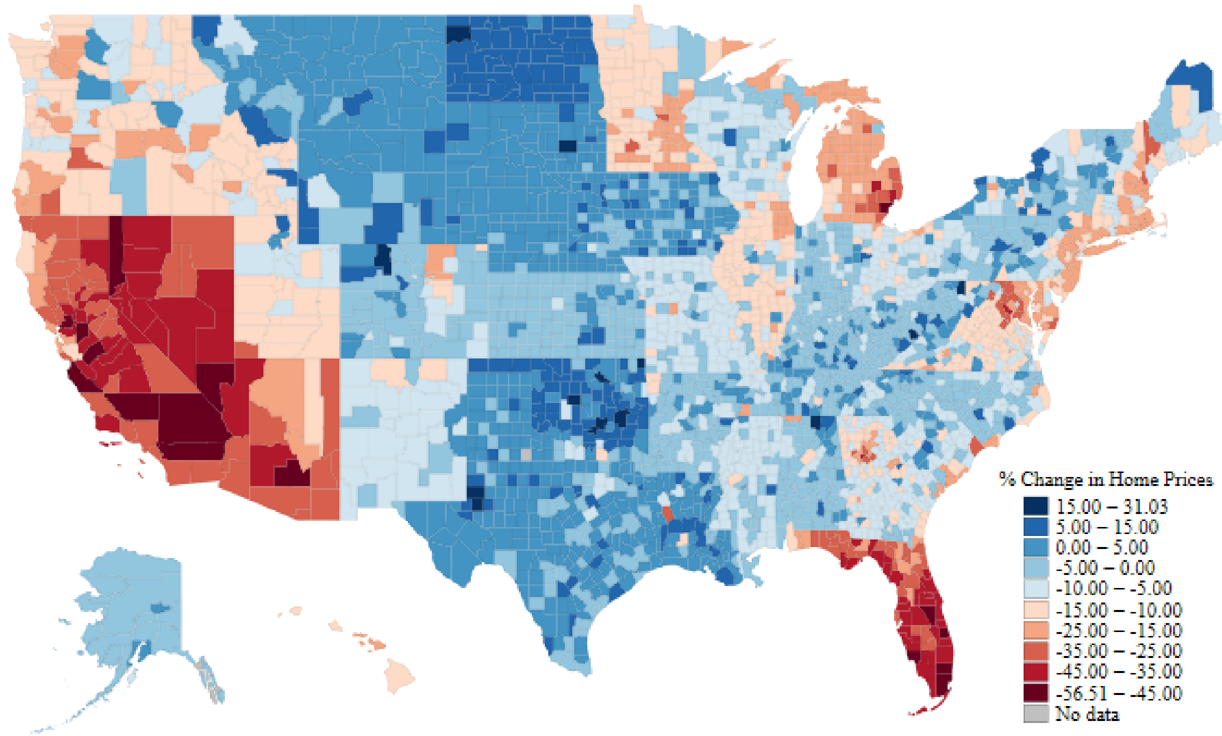
Using the final data set, Figure A5 shows percent change in home prices across counties in the United States between 2006 and 2009 with darker shades of red indicating larger declines in home prices, while the darker shades of blue indicate a handful of counties that experienced larger increases in home prices.

A.9. Instrumental Variable

In the main text we outline the steps used to form the instrumental variables applied in the paper, following the work of (Guren et al., 2020). The idea is to use the history of housing wealth changes over a period of time to identify the sensitivity of different areas to national or regional shocks. As the text highlights, the constructed instrument is correlated with previous instruments used in the literature. To further highlight the strength of the instrument and to demonstrate how it compares to alternative instruments used in the literature, we estimate a first-stage regression including our main IV variable, and then include the IV variables from previous work, including Guren et al. (2020) and Saiz (2010).

[60] The data is available at: https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx.

Figure A5. Percent Change in Home Prices between 2007 and 2009



Notes: The estimates are based on the FHFA housing price data, which includes some imputations using Zillow home value index reported on the Zillow website.

The estimates are reported based on the full sample from 2003 to 2019 and an additional sample that excludes the GR. We obtain similar results if we only look at the pre-GR period or post-GR period separately. The estimates are reported in Table A7 and show the main IV strategy applied in our main analysis is highly significant and correlated with housing wealth changes (column 1). The coefficient is very similar when we apply the Guren et al. (2020) instrument (column 2), which is not surprising given that the methodology for constructing the instruments is very similar. However, the estimates are different as the Guren et al. (2020) is based on a longer sample period, and uses the CBSA, rather than the county. The sample size from applying the Guren et al. (2020) IV is considerably smaller, as many counties are not included in their data. The Saiz (2010) instrument also shows strong correlation with the price change, although the coefficient is negative, since the higher the elasticity indicates lower sensitivity to regional or national shocks to housing prices. All three instruments are highly significant, even when the GR period is excluded, indicating that the instruments are not solely related to changes in price during the GR, but are more generally picking up sensitivity to regional or national shocks.

Table A7. First-Stage Regression Estimates of Housing Wealth Change on Instruments

	(1) Δ HNW (No Flow)	(2) Δ HNW (No Flow)	(3) Δ HNW (No Flow)	(4) Δ HNW (No Flow)	(5) Δ HNW (No Flow)	(6) Δ HNW (No Flow)
County-level Sensitivity Inst.	0.900***			0.799***		
	(0.0263)			(0.0261)		
Sensitivity Inst. from Guren et al.		0.936***			0.782***	
		(0.0303)			(0.0361)	
Saiz Instrument			-0.247***			-0.228***
			(0.0265)			(0.0242)
N	52876	19632	14743	46656	17322	13008
R squared	0.854	0.854	0.809	0.828	0.825	0.798
Sample	Full Sample	Full Sample	Full Sample	Excl. GR	Excl. GR	Excl. GR

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from a first-stage panel regression estimate of the change in housing wealth in a county on the change in the instrument. The table includes three IV variables: the first is the county-level sensitivity variable (the main IV applied in this paper), the second is the sensitivity instrument taken directly from Guren et al. (2020) and multiplied by our regional price variable, and the third is the Saiz instrument based on land unavailability. We also show these estimates for two time periods, including the full sample period (2003–2019) and excluding the GR years (i.e., excluding 2008–2009). The county-level sensitivity instrument performs similar to the Guren et al. (2020), while the Saiz instrument shows strong correlation, but is less statistically significant. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Recall that the main instrument is a sensitivity parameter that is constant and specific to a county, which is then interacted with regional price changes. To better understand the instrument, we regress the county-specific sensitivity parameters with numerous covariates to help understand if there are systematic factors that are correlated with this sensitivity measure. We include a number of variables, including population, per capita income, race, ethnicity and education and focus on estimates for the year 2007, given the sensitivity parameters do not change. In some specifications we also include two-digit industry employment share for the county. We also include the housing supply elasticity measure from Saiz (2010), which primarily captures physical land unavailability in an area.

The results are shown in Table A8. The first column (1) shows some correlation with the demographic variables, but these variables have little explanatory power with a reported R-squared of just 0.15. The R-squared is the same when we limit the sample to those counties where we observe the Saiz instrument (column 2). The third column adds two-digit industry shares which increases the R-squared to 0.46. Columns (4) and (5) are the same as columns (2) and (3), respectively, but the Saiz housing supply elasticity is added. The housing supply elasticity variable is highly significant and raises the R-squared substantially in both specifications. This suggests that one of the factors the sensitivity instrument seems to capture is physical land unavailability in the area.

Table A8. Regression of Sensitivity Parameter with Demographics and Saiz Elasticity Measure

	(1) Sensitivity Inst.	(2) Sensitivity Inst.	(3) Sensitivity Inst.	(4) Sensitivity Inst.	(5) Sensitivity Inst.
Saiz Housing supply elasticity				-0.335***	-0.265***
				(0.0926)	(0.0573)
Pop. in Millions	0.0610*	0.0501*	0.0637***	0.00903	0.0257**
	(0.0323)	(0.0283)	(0.0190)	(0.0139)	(0.0125)
Per Cap. Inc. (Thousands)	5.759	7.859	5.153	1.700	0.972
	(5.905)	(7.647)	(3.403)	(4.112)	(2.577)
Share Black	0.874**	1.248***	0.601	0.727**	0.447
	(0.336)	(0.413)	(0.389)	(0.291)	(0.330)
Share Other	0.498	1.192*	1.837***	0.619	1.384***
	(0.422)	(0.704)	(0.455)	(0.597)	(0.347)
Share Hispanic	1.596	2.112*	1.186	1.431*	0.886
	(1.130)	(1.241)	(0.817)	(0.846)	(0.627)
Share High School	4.970	6.944*	5.069*	4.584*	3.685*
	(3.024)	(3.946)	(2.979)	(2.508)	(2.089)
Share College	3.432*	4.377*	2.262	2.576	1.310
	(1.966)	(2.559)	(1.787)	(1.726)	(1.247)
N	2909	868	868	868	868
R squared	0.147	0.169	0.464	0.386	0.579
2-digit Ind. Shares Included	No	No	Yes	No	Yes
Sample	Full	Saiz Sample	Saiz Sample	Saiz Sample	Saiz Sample

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: This table shows a cross-sectional regression of the county sensitivity parameters on a number of variables. The variables include population, income per capita, race, Hispanic, and education. Census-region fixed effects are also included. Additional variables include the Saiz instrument and two-digit industry share variable. As this is a cross-sectional regression, all estimates are clustered at the state level. The sensitivity parameters do not vary by year, so we only run this regression for 2007. Several of the demographic variables are from the Area Resource File: https://data.hrsa.gov/topics/health-workforce/ahrf.

Table A9. Housing Wealth Change on Employment for Local and Export Industry Category: Differential Effects During the GR, Both Home and Away

	(1) Local Industries	(2) Local Industries	(3) Local Industries	(4) Export Industries	(5) Export Industries	(6) Export Industries
Δ HNW (Flow)	0.0496**	0.0978***	0.0597**	0.0593***	0.0964***	0.0725***
	(0.0199)	(0.0197)	(0.0241)	(0.0190)	(0.0171)	(0.0221)
Δ HNW (CZ-Export)		-0.0837	-0.113		-0.0533	-0.153**
		(0.0983)	(0.112)		(0.0655)	(0.0764)
Δ HNW (Flow) · GR	0.150***		0.132***	0.119***		0.0692**
	(0.0270)		(0.0324)	(0.0232)		(0.0277)
Δ HNW (CZ-Export) · GR			0.217			0.608***
			(0.197)			(0.148)
N	52719	52719	52719	52758	52758	52758
R squared	0.227	0.227	0.228	0.333	0.333	0.333
IV Estimate	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from panel regression and IV regression estimates of the change in employment for select local and export industries in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and the industry category of either local or export industries. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Table A10. Effects on Employment for Local and Export Industries Using Industry-Specific Flows: Differential Effects During the GR, Both Home and Away

	(1) Local Industries	(2) Local Industries	(3) Local Industries	(4) Export Industries	(5) Export Industries	(6) Export Industries
Δ HNW (Local Ind. Flow)	0.0460**	0.0510**	0.0494**			
	(0.0185)	(0.0204)	(0.0224)			
Δ HNW (Local Ind. Flow) · GR	0.136***	0.115***	0.122***			
	(0.0255)	(0.0289)	(0.0303)			
Δ HNW (Export Ind. Flow)				0.0663***	0.0805***	0.0785***
				(0.0199)	(0.0221)	(0.0224)
Δ HNW (Export Ind. Flow) · GR				0.126***	0.0627**	0.0636**
				(0.0255)	(0.0294)	(0.0296)
Δ HNW (Local Ind. CZ-Export)		-0.100	-0.120			0.143
		(0.126)	(0.151)			(0.118)
Δ HNW (Local Ind. CZ-Export)· GR		0.444*	0.571**			-0.0486
		(0.231)	(0.280)			(0.246)
Δ HNW (Export Ind. CZ-Export)			0.0194		-0.109**	-0.161**
			(0.0883)		(0.0514)	(0.0657)
Δ HNW (Export Ind. CZ-Export)· GR			-0.115		0.510***	0.529***
			(0.166)		(0.0996)	(0.132)
N	52719	52719	52719	52758	52758	52758
R squared	0.227	0.227	0.227	0.333	0.333	0.333
IV Estimate	Yes	Yes	Yes	Yes	Yes	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from panel regression and IV regression estimates of the change in employment for local and export industries in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and industries. The housing wealth changes in this table are based on industry-specific flows. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Table A11. Housing Wealth Change on Spending for Intermediate Industry Category: Differential Effects During the GR, Both Home and Away

	(1) Intermediate Industries	(2) Intermediate Industries	(3) Intermediate Industries
Δ HNW (Flow)	0.0334	0.0440	0.0126
	(0.0292)	(0.0274)	(0.0346)
Δ HNW (CZ-Export)		0.268	0.214
		(0.171)	(0.202)
Δ HNW (Flow) · GR	0.127***		0.103*
	(0.0436)		(0.0544)
Δ HNW (CZ-Export) · GR			0.354
			(0.326)
N	52776	52776	52776
R squared	0.272	0.273	0.272
IV Estimate	Yes	Yes	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from panel regression and IV regression estimates of the change in spending for select intermediate industries in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

A.10. Additional Industry Category Effects

Table A12 repeats the analysis in Table 8, but using employment, rather than spending. The results are qualitatively similar to those in the main text that focus on spending.

Table A12. Effects on Employment for Local Industry Category: Differential Effects During the GR, Both Home and Away

	(1) Local Industries	(2) Local Industries	(3) Local Industries	(4) Export Industries	(5) Export Industries	(6) Export Industries
Δ HNW (Flow)	0.0496**	0.0978***	0.0597**	0.0593***	0.0964***	0.0725***
	(0.0199)	(0.0197)	(0.0241)	(0.0190)	(0.0171)	(0.0221)
Δ HNW (CZ-Export)		-0.0837	-0.113		-0.0533	-0.153**
		(0.0983)	(0.112)		(0.0655)	(0.0764)
Δ HNW (Flow) · GR	0.150***		0.132***	0.119***		0.0692**
	(0.0270)		(0.0324)	(0.0232)		(0.0277)
Δ HNW (CZ-Export) · GR			0.217			0.608***
			(0.197)			(0.148)
N	52719	52719	52719	52758	52758	52758
R squared	0.227	0.227	0.228	0.333	0.333	0.333
IV Estimate	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from panel regression and IV regression estimates of the change in employment for select "local" industries in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

A.11. Additional Distance Specifications

Effects By Distance — Table A13 repeats the analysis by distance in Table A13, but uses employment rather than spending. Table A14 repeats the spending analysis by distance, but focusing only on local industries. This figure shows that local industries are not differentially affected by spending changes outside of the local area.

Table A13. Employment by Distance

	(1) % Chg. Emp.	(2) % Chg. Emp.	(3) % Chg. Emp.	(4) % Chg. Emp.
Δ HNW (Flow)	0.0655***	0.0702***	0.0311	0.0342
	(0.0182)	(0.0181)	(0.0233)	(0.0236)
Δ HNW (Export: ≤ 100 Miles)	0.0686	0.0578	0.0705	0.0644
	(0.0555)	(0.0553)	(0.0664)	(0.0672)
Δ HNW (Export: > 100 Miles)	0.0872	0.0123	-0.0400	-0.0884
	(0.0763)	(0.0757)	(0.0903)	(0.0906)
Δ HNW (Flow) · GR			0.108***	0.115***
			(0.0280)	(0.0282)
Δ HNW (Export: ≤ 100 Miles) · GR)			0.0481	0.0263
			(0.1000)	(0.103)
Δ HNW (Export: > 100 Miles) · GR)			0.771***	0.658***
			(0.156)	(0.150)
N	52874	52874	52874	52874
R squared	0.406	0.406	0.406	0.407
IV Estimate	Yes	Yes	Yes	Yes
IV Over 100 miles	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from a linear regression estimate of the change in spending for 15 select industries in the county from 2003 to 2019 on the change in housing wealth variable(s). The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes. We exclude outliers where the absolute value of the change in spending exceeds 50 percent, although the estimates are unaffected by the exclusion of outliers. Standard errors are in parentheses and are clustered by state. Estimates are weighted by 2007 population levels. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Table A14. Spending by Distance for Local Industries

	(1) % Chg. Spend	(2) % Chg. Spend	(3) % Chg. Spend	(4) % Chg. Spend
Δ HNW (Flow)	0.138***	0.142***	0.125***	0.128***
	(0.0311)	(0.0311)	(0.0385)	(0.0387)
Δ HNW (Export: $\leq$ 100 Miles)	0.0459	0.0344	0.00102	-0.00481
	(0.116)	(0.117)	(0.125)	(0.126)
Δ HNW (Export: $>$ 100 Miles)	0.125	0.0457	0.0643	0.0172
	(0.151)	(0.160)	(0.159)	(0.176)
Δ HNW (Flow) $\cdot$ GR			0.0227	0.0319
			(0.0669)	(0.0655)
Δ HNW (Export: $\leq$ 100 Miles) $\cdot$ GR)			0.320	0.291
			(0.329)	(0.330)
Δ HNW (Export: $>$ 100 Miles) $\cdot$ GR)			0.357	0.199
			(0.273)	(0.298)
N	52722	52722	52722	52722
R squared	0.211	0.212	0.211	0.211
IV Estimate	Yes	Yes	Yes	Yes
IV Over 100 miles	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from a linear regression estimate of the change in spending for 15 select industries in the county from 2003 to 2019 on the change in housing wealth variable(s). The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes. We exclude outliers where the absolute value of the change in spending exceeds 50 percent, although the estimates are unaffected by the exclusion of outliers. Standard errors are in parentheses and are clustered by state. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

A.12. Robustness Specifications

The tables in this section perform various robustness checks on the main results presented in the paper. Most of the estimates are variations of the main results presented in Tables 6 and 7.

A.12.1. Consumer Share Adjustment

The estimates in Tables A15 and A16 are the same as the main estimates, but apply an adjustment so that flows are calculated assuming that spending is a constant share of income in all counties. This corrects for potential biases in the flows, possibly caused by lower income populations using card transactions less often than high-income populations. The results are very similar to the main results.

Table A15. Housing Wealth Change on Spending Growth - Consumer Share Adjustment: Differential Effects During the GR, Both Home and Away

	(1) % Chg. Spend	(2) % Chg. Spend	(3) % Chg. Spend	(4) % Chg. Spend	(5) % Chg. Spend
Δ HNW (Total Flow)	0.0799***	0.0910***	0.106***	0.0475**	0.0728***
	(0.0179)	(0.0193)	(0.0184)	(0.0233)	(0.0220)
Δ HNW (CZ-Export)		0.290***	0.138	0.320***	0.0581
		(0.0912)	(0.0983)	(0.101)	(0.108)
Δ HNW (Total Flow) · GR	0.143***			0.156***	0.102***
	(0.0230)			(0.0334)	(0.0300)
Δ HNW (CZ-Export) · GR				-0.0765	0.525***
				(0.203)	(0.185)
N	52875	52875	52875	52875	52875
R squared	0.342	0.342	0.343	0.340	0.342
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from IV panel regression estimate of the change in employment for 15 industries from 2003 to 2019 on the change in housing wealth variables. The spending flow estimates are adjusted to account for potential biases in spending flows. Specifically, spending flows are adjusted so that all counties have a constant consumption to income ratio, based on the 2015 flow data. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Table A16. Housing Wealth Change on Employment Growth - Consumer Share Adjustment: Differential Effects During the GR, Both Home and Away

	(1)	(2)	(3)	(4)	(5)
	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.
Δ HNW (Total Flow)	0.0415***	0.0699***	0.0833***	0.0325*	0.0518***
	(0.0159)	(0.0153)	(0.0137)	(0.0190)	(0.0168)
Δ HNW (CZ-Export)		0.102*	-0.0376	0.0834	-0.112*
		(0.0581)	(0.0570)	(0.0652)	(0.0641)
Δ HNW (Total Flow) · GR	0.140***			0.122***	0.0977***
	(0.0188)			(0.0235)	(0.0193)
Δ HNW (CZ-Export) · GR				0.236**	0.492***
				(0.118)	(0.101)
N	52874	52874	52874	52874	52874
R squared	0.406	0.405	0.406	0.405	0.407
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from IV panel regression estimate of the change in employment for 15 industries from 2003 to 2019 on the change in housing wealth variables. The spending flow estimates are adjusted to account for potential biases in spending flows. Specifically, spending flows are adjusted so that all counties have a constant consumption to income ratio, based on the 2015 flow data. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

A.12.2. CZ-Wide changes in Housing Wealth

The results in Table A17 aggregate over the housing wealth effect for the entire CZ (i.e., all counties in a CZ share the same effect from changes in housing wealth).

Table A17. Housing Wealth Change on Spending Growth - CZ-Wide Change in Housing Wealth: Differential Effects During the GR, Both Home and Away

	(1) % Chg. Spend	(2) % Chg. Spend	(3) % Chg. Spend	(4) % Chg. Spend	(5) % Chg. Spend
Δ CZ HNW (Flow)	0.113***	0.107***	0.132***	0.0670**	0.109***
	(0.0176)	(0.0240)	(0.0225)	(0.0293)	(0.0265)
Δ CZ HNW (CZ-Export)		0.328**	0.113	0.381**	0.0259
		(0.132)	(0.140)	(0.151)	(0.158)
Δ CZ HNW (Flow) · GR	0.116***			0.144***	0.0672
	(0.0267)			(0.0500)	(0.0424)
Δ CZ HNW (CZ-Export) · GR				-0.182	0.536**
				(0.296)	(0.263)
Observations	52875	52875	52875	52875	52875

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from IV panel regression estimate of the change in spending for local industries from 2003 to 2019 on the change in housing wealth variables. The housing wealth variable and associated instruments in this specification is aggregated to the level of the CZ, so that all counties within the same CZ have the same housing wealth change variable. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Table A18. Housing Wealth Change on Employment Growth - CZ-Wide Change in Housing Wealth: Differential Effects During the GR, Both Home and Away

	(1)	(2)	(3)	(4)	(5)
	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.
Δ CZ HNW (Flow)	0.0507***	0.0664***	0.0894***	0.0259	0.0590***
	(0.0174)	(0.0194)	(0.0166)	(0.0248)	(0.0208)
Δ CZ HNW (CZ-Export)		0.190**	-0.0131	0.204**	-0.0740
		(0.0761)	(0.0705)	(0.0872)	(0.0802)
Δ CZ HNW (Flow) · GR	0.138***			0.138***	0.0988***
	(0.0210)			(0.0331)	(0.0264)
Δ CZ HNW (CZ-Export) · GR				0.0654	0.397***
				(0.166)	(0.139)
Observations	52874	52874	52874	52874	52874

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from a linear regression estimate of the change in employment for 15 select industries in the county from 2003 to 2019 on the change in housing wealth variable(s). The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes. Housing wealth changes and associated instruments are computed by averaging over the entire CZ, so that there is one housing wealth change per CZ and per time period. We exclude outliers where the absolute value of the change in spending exceeds 50 percents. Standard errors are in parentheses and are clustered by state. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

A.12.3. Alternative Instruments - Sensitivity - Guren et al. (2020)

Table A19 and A20 repeat the results, but substitute the main instrument using the instrument directly from the the paper Guren et al. (2020). For these estimates we drop those counties where the instrument from Guren et al. (2020) is not available, but their instrument covers around 90 percent of the population.[61]

The instruments in Guren et al. (2020) are constructed for the retail sector, so we limit the industry categories to the retail sector. The results are qualitatively the same to those in the main analysis, although the standard errors increase on some of the estimates.

Table A19. Housing Wealth Change on Spending Growth - Sensitivity Instrument (Guren et al. (2020)): Differential Effects During the GR, Both Home and Away

	(1) % Chg. Spend	(2) % Chg. Spend	(3) % Chg. Spend	(4) % Chg. Spend	(5) % Chg. Spend
Δ HNW (Total Flow)	0.0944***	0.101***	0.132***	0.0451	0.0980***
	(0.0248)	(0.0274)	(0.0229)	(0.0343)	(0.0273)
Δ HNW (CZ-Export)		0.404***	0.0772	0.483***	-0.0512
		(0.132)	(0.124)	(0.151)	(0.131)
Δ HNW (Total Flow) · GR	0.145***			0.172***	0.0740*
	(0.0319)			(0.0475)	(0.0399)
Δ HNW (CZ-Export) · GR				-0.180	0.870***
				(0.289)	(0.255)
N	19635	19635	19635	19635	19635
R squared	0.467	0.467	0.470	0.463	0.467
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from IV panel regression estimate of the change in spending for retail industries and food service from 2003 to 2019 on the change in housing wealth variables. The IV variable in this table is taken directly from Guren et al. (2020) and multiplied by our regional price variable. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. The housing wealth change is calculated for consumers at different distances. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

[61] The instrument from Guren et al. (2020) is at the CBSA-level, so we apply that instrument value to all counties within the CBSA. Since we use spending flows for the entire U.S., for those markets where the Guren et al. (2020) estimate is not available, estimate the value using our main IV approach. This only affects housing wealth changes outside of the home county.

Table A20. Housing Wealth Change on Employment Growth - Sensitivity Instrument (Guren et al. (2020)): Differential Effects During the GR, Both Home and Away

	(1)	(2)	(3)	(4)	(5)
	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.
Δ HNW (Total Flow)	0.0721***	0.0791***	0.0886***	0.0759**	0.0885***
	(0.0232)	(0.0219)	(0.0188)	(0.0306)	(0.0258)
Δ HNW (CZ-Export)		0.0667	-0.0322	-0.0522	-0.176*
		(0.0856)	(0.0775)	(0.108)	(0.0916)
Δ HNW (Total Flow) · GR	0.0437			-0.0234	-0.0330
	(0.0304)			(0.0441)	(0.0366)
Δ HNW (CZ-Export) · GR				0.817***	0.899***
				(0.220)	(0.195)
N	19622	19622	19622	19622	19622
R squared	0.530	0.530	0.530	0.530	0.531
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from IV panel regression estimate of the change in employment for retail industries and food service from 2003 to 2019 on the change in housing wealth variables. The IV variable in this table is taken directly from Guren et al. (2020) and multiplied by our regional price variable. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. The housing wealth change is calculated for consumers at different distances. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

A.12.4. Alternative Instruments - Land Unavailability - Saiz (2010)

Table A21 and A22 repeat the results, but substitute the main instrument using the instrument directly from Saiz (2010). For these estimates we drop those counties where the instrument from Saiz (2010) is not available, but the instrument covers around 70 percent of the population.[62] These estimates are similar to those using the main sensitivity instruments applied in the paper, although the standard errors increase on some of the estimates.

Table A21. Housing Wealth Change on Spending Growth - Saiz Instrument: Differential Effects During the GR, Both Home and Away

	(1) % Chg. Spend	(2) % Chg. Spend	(3) % Chg. Spend	(4) % Chg. Spend	(5) % Chg. Spend
Δ HNW (Total Flow)	0.107***	0.156***	0.133***	0.0927*	0.0862**
	(0.0406)	(0.0443)	(0.0364)	(0.0511)	(0.0412)
Δ HNW (CZ-Export)		0.0546	0.365*	0.214	0.293
		(0.183)	(0.205)	(0.197)	(0.215)
Δ HNW (Total Flow) · GR	0.207***			0.280**	0.163*
	(0.0726)			(0.111)	(0.0870)
Δ HNW (CZ-Export) · GR				-0.998*	0.505
				(0.585)	(0.510)
N	14742	14742	14742	14742	14742
R squared	0.434	0.438	0.437	0.433	0.433
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from IV panel regression estimate of the change in spending for 15 industries from 2003 to 2019 on the change in housing wealth variables. The IV variable in this table is taken directly from Saiz (2010) and interacted with the average national price change. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. The housing wealth change is calculated for consumers at different distances. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and region-year fixed-effects.

[62] The instrument from Saiz (2010) is at the MSA-level, so we apply that instrument value to all counties within the MSA. Since we use spending flows for the entire U.S., for those markets where the Saiz (2010) estimate is not available, we estimate the value using our main IV approach. This only affects housing wealth changes outside of the home market.

Table A22. Housing Wealth Change on Employment Growth - Saiz Instrument: Differential Effects During the GR, Both Home and Away

	(1)	(2)	(3)	(4)	(5)
	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.
Δ HNW (Total Flow)	-0.0132	0.0299	0.0386	-0.0320	-0.0169
	(0.0320)	(0.0342)	(0.0268)	(0.0417)	(0.0328)
Δ HNW (CZ-Export)		0.216	0.0987	0.273*	0.0504
		(0.132)	(0.156)	(0.152)	(0.177)
Δ HNW (Total Flow) · GR	0.230***			0.250***	0.207***
	(0.0407)			(0.0619)	(0.0492)
Δ HNW (CZ-Export) · GR				-0.315	0.287
				(0.337)	(0.344)
N	14729	14729	14729	14729	14729
R squared	0.519	0.522	0.523	0.516	0.519
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from IV panel regression estimate of the change in employment for 15 industries from 2003 to 2019 on the change in housing wealth variables. The IV variable in this table is taken directly from Saiz (2010) and interacted with the average national price change. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. The housing wealth change is calculated for consumers at different distances. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Effects using Industry Categories from Mian and Sufi (2014) — Tables A23 and A24 are the same as the main estimates in the text, but use the industry categories applied in Mian and Sufi (2014), which are also similar to those in Guren et al. (2020). These categories include all the retail categories and restaurants (NAICS 722).

Table A23. Effects on Spending using Industry Categories from Mian and Sufi (2014): Differential Effects During the GR, Both Home and Away

	(1)	(2)	(3)	(4)	(5)
	% Chg. Spend	% Chg. Spend	% Chg. Spend	% Chg. Spend	% Chg. Spend
Δ HNW (Flow)	0.120***	0.132***	0.146***	0.0865***	0.107***
	(0.0171)	(0.0184)	(0.0175)	(0.0210)	(0.0197)
Δ HNW (CZ-Export)		0.311***	0.157*	0.360***	0.129
		(0.0929)	(0.0928)	(0.101)	(0.0973)
Δ HNW (Flow) · GR	0.148***			0.167***	0.134***
	(0.0229)			(0.0312)	(0.0274)
Δ HNW (CZ-Export) · GR				-0.179	0.206
				(0.202)	(0.175)
N	52871	52871	52871	52871	52871
R squared	0.382	0.382	0.383	0.381	0.382
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from panel regression and IV regression estimates of the change in spending for retail and restaurant industries from Mian and Sufi (2014) in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Table A24. Effects on Employment using Industry Categories from Mian and Sufi (2014): Differential Effects During the GR, Both Home and Away

	(1)	(2)	(3)	(4)	(5)
	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.
Δ HNW (Flow)	0.0882***	0.103***	0.108***	0.0942***	0.102***
	(0.0183)	(0.0162)	(0.0149)	(0.0225)	(0.0208)
Δ HNW (CZ-Export)		0.00796	-0.0457	-0.0763	-0.165**
		(0.0594)	(0.0532)	(0.0762)	(0.0680)
Δ HNW (Flow) $\cdot$ GR	0.0570**			0.0103	0.000292
	(0.0265)			(0.0358)	(0.0331)
Δ HNW (CZ-Export) $\cdot$ GR				0.586***	0.695***
				(0.174)	(0.165)
N	52872	52872	52872	52872	52872
R squared	0.427	0.427	0.428	0.428	0.428
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results from panel regression and IV regression estimates of the change in spending for retail and restaurant industries from Mian and Sufi (2014) in the county from 2003 to 2019 on the change in housing wealth variables. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

A.12.5. Predicted Share Adjustment

The estimates in Tables A25 and A26 are the same as the main estimates, but flows are predicted specific to each year of the data. This relaxes the fixed share assumption in the main analysis. The results are qualitatively the same.

Table A25. Housing Wealth Change on Spending Growth - Predicted Share Adjustment: Differential Effects During the GR, Both Home and Away

	(1) % Chg. Spend	(2) % Chg. Spend	(3) % Chg. Spend	(4) % Chg. Spend	(5) % Chg. Spend
Δ HNW (Flow Pred)	0.0779***	0.0875***	0.0997***	0.0491**	0.0698***
	(0.0172)	(0.0178)	(0.0173)	(0.0215)	(0.0205)
Δ HNW (CZ-Export Pred)		0.354***	0.192*	0.367***	0.0919
		(0.101)	(0.114)	(0.111)	(0.125)
Δ HNW (Flow Pred) $\cdot$ GR	0.136***			0.138***	0.0977***
	(0.0226)			(0.0317)	(0.0290)
Δ HNW (CZ-Export Pred) $\cdot$ GR				0.0176	0.560***
				(0.221)	(0.203)
N	52875	52875	52875	52875	52875
R squared	0.342	0.342	0.343	0.341	0.342
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from IV panel regression estimate of the change in spending for 15 industries from 2003 to 2019 on the change in housing wealth variables. The spending flow estimates are based on predicted spending flows in the base year (e.g., for the 2004–2006 change, the base year is 2003), where the methodology for predicting flows is described in appendix section A.7.1. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

Table A26. Housing Wealth Change on Employment Growth - Predicted Share Adjustment: Differential Effects During the GR, Both Home and Away

	(1)	(2)	(3)	(4)	(5)
	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.	% Chg. Emp.
Δ HNW (Flow Pred)	0.0399**	0.0624***	0.0724***	0.0262	0.0412**
	(0.0159)	(0.0157)	(0.0148)	(0.0196)	(0.0185)
Δ HNW (CZ-Export Pred)		0.181***	0.0495	0.171**	-0.0257
		(0.0678)	(0.0630)	(0.0776)	(0.0726)
Δ HNW (Flow Pred) · GR	0.135***			0.126***	0.106***
	(0.0184)			(0.0237)	(0.0213)
Δ HNW (CZ-Export Pred) · GR				0.160	0.419***
				(0.131)	(0.110)
N	52874	52874	52874	52874	52874
R squared	0.406	0.405	0.406	0.405	0.406
IV Estimate	Yes	Yes	Yes	Yes	Yes
IV Outside CZ	-	No	Yes	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from IV panel regression estimate of the change in spending for 15 industries from 2003 to 2019 on the change in housing wealth variables. The spending flow estimates are based on predicted spending flows in the base year (e.g., for the 2004–2006 change, the base year is 2004), where the methodology for predicting flows is described in appendix section A.7.1. The specifications across the columns differ by the inclusion of distinct measure of housing wealth changes and whether IV is applied to housing wealth changes outside of the CZ. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county interacted with year, and also region-year fixed-effects.

A.12.6. Effects of Housing Wealth Change on Employment Growth

Alternative Estimates Based on Export Quartile — Table A27 repeats the analysis by export quartile in Table 5, but using employment rather than spending.

Table A27. Average Housing Wealth Changes on Employment Growth: Home Market and Export Market by Export Quartile

	(1) % Chg. Spend	(2) % Chg. Spend
Δ HNW (Home) · Q1 Export	0.102***	-0.0152
	(0.0191)	(0.0863)
Δ HNW (Home) · Q2 Export	0.0824***	-0.122**
	(0.0260)	(0.0606)
Δ HNW (Home) · Q3 Export	0.189***	0.0593
	(0.0219)	(0.0487)
Δ HNW (Home) · Q4 Export	0.161***	0.143**
	(0.0217)	(0.0644)
Avg. Δ HNW (Away) · Q1 Export	0.0625**	0.194*
	(0.0308)	(0.101)
Avg. Δ HNW (Away) · Q2 Export	0.0612*	0.297***
	(0.0335)	(0.0785)
Avg. Δ HNW (Away) · Q3 Export	-0.0634*	0.0962
	(0.0333)	(0.0728)
Avg. Δ HNW (Away) · Q4 Export	-0.0235	0.00596
	(0.0312)	(0.0824)
N	52874	52874
R squared	0.336	0.330
IV Estimate	No	Yes

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: The table presents results from panel regression estimates of the change in employment for 15 select industries in the county from 2003 to 2019 on the change in housing wealth variables. Column (1) shows the OLS estimates and column (2) applies instrumental variables. We exclude the top and bottom 50 observations with the largest changes, which is roughly 0.2 percent of the sample. Standard errors are in parentheses and are clustered by counties. Estimates are weighted by 2007 population levels. All estimates include county fixed effects, two-digit industry employment share by county variables interacted with year dummies, and also region-year fixed-effects.

B. Implications for Employment

Table A28. Decomposition the Local Geographic Effects of the GR on Employment

Total Employment in 2007 (in Thousands)	25,850		

	Chg. in Employment (in Thousands)	Percent Decline	Share Misallocation
Baseline	-630	-2.4	-
Baseline (Within CZ Effect)	-344	-1.3	-
Baseline (Outside CZ Effect)	-290	-1.1	-
Scenario 1. No Differential CZ Effect	-423	-1.6	0.193
Scenario 2. No Differential CZ, No GR Effect	-132	-0.5	0.193
Scenario 3. Only Within CZ Effect	-344	-1.3	0.327
Scenario 4. Only Within CZ Effect, No GR Effect	-108	-0.4	0.327

Note: This table reports the effects of the housing wealth change during the 2007–2009 period based on the regression estimates in Table 7 and column (5). For instance, the baseline estimate shows the total effect of the housing wealth change on employment was around 630 billion, with 344 thousand coming from changes in housing wealth within the CZ and 290 thousand coming from changes in housing wealth outside of the CZ. Scenario 1 assumes no differential effects outside the CZ; scenario 2 assumes no differential effects outside the CZ, and no differential effect from the GR; scenario 3 assumes effects are only within CZ; and scenario 4 assumes effects are only within CZ and there is no differential effect from the GR. These results highlight the importance of both the larger effects during the GR. The last column of the table also reports the level of misallocation computed as the absolute value of the share of the total effect on spending occurring in each county compared relative to the counterfactual share of spending occurring in each county. In scenario 3 and 4 about 30 percent of the total effect would be misallocated.