The Role of Food Assistance in Rural Areas*

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Abstract

We study how SNAP participants' redemption behavior responds to the local retail food environment – specifically proximity to a superstore – across the rural-urban continuum. We find that superstore entry into counties without a superstore increases redemptions in that county by a factor of approximately 15% in rural areas and approximately 20% in urban areas. In rural and urban counties without a superstore, adding an additional mile to the nearest superstore increases in-county redemptions by 2% and 7%, respectively. These effects are reversed for both rural and urban counties with an existing superstore. Understanding how SNAP participants respond to changes in their local environment can help policymakers efficiently guide resources from SNAP into rural economies.

1 Introduction

How does the entry of a superstore affect the flow of Supplemental Nutrition Assistance Program (SNAP) benefits across geographies? In this paper, we examine how changes in the food retail environment – particularly in rural areas – affect redemptions of SNAP benefits. Authorized food retailers are essential to participants' access to SNAP. In this case, the role of retailers in shaping benefit redemptions represents a black box that mediates interactions between state and federal SNAP policymakers and SNAP participants. Existing evidence describes the relationship between the food environment and food purchasing nationally, with a body of literature focusing on low-income and SNAP participating households (Allcott et al., 2019; Ver Ploeg et al., 2015; Grindal et al., 2016; Cuffey and Beatty, 2022). Importantly, access to food retailers is particularly constrained in rural areas (Cleary et al., 2018; Stevens et al., 2021)), and access to SNAP authorized retailers is at least as constrained. The effect of access to SNAP-authorized food retailers on SNAP benefit use has been studied primarily in urban areas (Cuffey and Beatty, 2022; Block, 2006). However, supply and demand factors affecting the food environment will be different in rural areas (Bitler and Haider, 2011). A notable recent trend is that entry of non-traditional food retailers like dollar stores and superstores is increasing (Stevens et al., 2021).

Rural populations use SNAP relatively more than urban populations; 13% of people in rural areas participate in the SNAP program , compared to 11% in urban areas. SNAP participants in rural areas are also more likely to be in poverty compared to participants in urban areas, with 51% of rural SNAP participants in poverty compared to 46% in urban counties.[1] Taken together, this implies that there are simultaneously proportionally more SNAP participating households in rural areas that are economically worse-off on average, and fewer local food retailers at which to redeem benefits. Considering rural areas' unique food retail environment and need for food assistance, it is important to understand how accessible SNAP redemptions are in rural areas (Johnson et al., 2014). We focus on access to superstores, which constitute a large share

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[1]Author's own calculations using the 2021 5-year ACS estimates (U.S. Census Bureau, 2024) and the 2013 Rural-Urban Continuum Codes.

of redemptions (Andrews et al., 2012; Castner and Henke, 2011). We consider the total effect of a supply side change in retailer authorization – the SNAP authorization of a superstore – on redemptions relative to issuance in a county, to show how transitions in the food environment affect the flow of SNAP benefits in rural versus urban areas. This total effect will capture changes in benefit use at the newly authorized store as well as any spillover effects within the county.

SNAP redemptions reflect participants' preferences over where to redeem their benefits, including available food selection, price point, convenience, and the shopping experience. Prices remain highly relevant to SNAP participants since benefits are structured as a fixed dollar value per month. Participants' revealed preference through observed redemptions by store type indicates that SNAP participants prefer to redeem benefits at superstores, where available. Although they represent only 7.5% of authorized retailers, superstores accounted for 51.5% of redemptions in FY 2022. Thus, a supply side change in the set of SNAP authorized superstores can plausibly shift redemption behavior of SNAP participants.

As noted in Allcott et al. (2019) and Cuffey and Beatty (2022), households' store choice depends on both supply side and demand side factors. In general, SNAP participating households face a more constrained choice of retailers to redeem benefits relative to non-benefit food shopping. On the supply side, ability to redeem SNAP benefits depends first on access to an authorized retailer. Changes in the set of authorized retailers can cause changes in the distribution of SNAP redemptions across stores. On the demand side, a household will choose a store, within the set of authorized retailers with a selection of foods they like, with prices that fit their budget, that provide a reasonable shopping experience for redeeming benefits. We examine a supply-side-only change in the set of authorized retailers that changes the food retail landscape and shifts benefits across space, holding demand for SNAP benefits constant. This supply-side variation is useful for informing policymakers about participants' responses to the food retail environment.

This paper investigates the extent to which SNAP benefits issued in rural areas are redeemed in rural areas versus nearby metro areas, and how these patterns change with entry and exit of superstores.[2] We answer the question: how do redemptions, relative to issuance, of SNAP benefits change with the presence of a superstore in the county, or with changes in the distance to a superstore? Over our study period (1998-2020), we find that superstore entry into a county increases the share of SNAP redemptions relative to issuance that are redeemed in that county. The SNAP-authorization of a superstore in a county increases redemptions by 23% holding issuance constant. For counties that lack a superstore, each additional mile of distance to a superstore from a population-weighted centroid decreases redemptions in the county by 0.3%.

We make three contributions. First, we extend work on the rural food retail environment to explicitly consider the implications of a changing *rural* food environment on low-income populations, represented by SNAP participants. Given that SNAP is at least as important in rural areas as urban areas and that the rural food environment is changing, we fill a gap in our understanding of how changes in the food environment affect the benefit use patterns of rural SNAP participants. Second, we consider how heterogeneity in the food environment across the rural-urban continuum can moderate the effect of marginal store entry on SNAP benefit flows. The continuum of rural place designations covers a wide range of geography, from larger counties adjacent to metro areas to smaller counties not adjacent to metro areas. Within these types of rural geographies, distance between stores and availability of store types vary, with rural non-metro coun-

[2] A superstore is defined as a very large store selling a wide variety of commodities. Examples include Walmart, Target, and Costco.

ties containing fewer stores and being more likely to have no grocery retailers than metro counties (Stevens et al., 2021). We thus split our analysis to examine how our effect sizes vary across rural counties of different types - whether they are adjacent to a metropolitan statistical area (MSA) or not, and whether they have an existing superstore or not. This is directly relevant to policymakers who are interested in promoting the economic health of rural communities. Finally, we use administrative records on stores, issuance, and redemptions to ensure that our results are not subject to non-response bias from survey data (Meyer et al., 2015). Our sample covers the continental United States so our results are representative of the populations and policy environment for much of the country.

This paper proceeds as follows: in the next section we provide background on the SNAP program, including SNAP issuance, SNAP redemptions, and defining SNAP multipliers. In Section 3 we discuss our data sources, describe the data, and define our issuance-redemption ratio (IRR). Section 4 outlines the methods we use to analyze the data, including our empirical specifications and identification assumptions. Next we show the results and discuss their interpretation and implications in Section 5, then Section 6 concludes.

2 Background

Beginning as the Food Stamp Program in the 1960s, the program was renamed to SNAP in the 2008 Farm Bill. SNAP is managed federally by the U.S. Department of Agriculture Food and Nutrition Service (FNS), but each state has its own department responsible for managing state participants, providing technical and sometimes financial assistance to retailers, and maintaining program integrity. FNS offers grants annually to state departments or non-profits to improve program implementation through new technology or trainings,[3] and each state is responsible for issuing SNAP benefits to enrolled participants. Benefits do not expire at the end of the month – although most participants nearly exhaust their benefits by the end of the month – and benefits may be redeemed at any SNAP authorized retailer. When we refer to issuance in this paper, we mean the total dollar value of SNAP benefits issued to participants residing in a particular county in a month. Redemptions refer to the total dollar amount of SNAP benefits redeemed monthly by participants at authorized retailers within that county. The number of persons receiving SNAP benefits varies month to month, but for our purposes we consider all individuals receiving SNAP benefits, including those receiving other public assistance or welfare support, as participants in our analyses.

Also in 2008, FNS codified the Electronic Benefit Transfer (EBT) system as the primary method of issuing SNAP benefits. EBT enables SNAP participants to receive their benefits on a debit card that is linked to a federal account. The first EBT pilot program was introduced in 1998, and by 2004, all state departments (including the District of Columbia) had implemented this system to some extent. Prior to EBT, participants in food assistance programs were issued vouchers they could use in lieu of cash to purchase approved foods at select retailers, who then submitted these vouchers for reimbursement. The use of vouchers was officially discontinued federally in 2009, but full EBT adoption by states was staggered over time. Accordingly, we will have a mix of states and counties that are using EBT and paper vouchers in the first ten years of our sample.

The mission of SNAP is to combat poverty and food insecurity among low-income households, as well as to stimulate economic growth. In theory the latter objective can result from relaxing the budget constraint

[3]Total grant rewards often total somewhere between \$5 and \$10 million, with individual awards ranging between \$300,000 and \$1 million. State departments receiving awards are then responsible for optimal distribution of funds.

for low-income households, thus increasing consumption of food and non-food items in resource-poor areas. In its evaluation of how well SNAP has met this objective, the literature has studied what it terms as the "SNAP multiplier effect". Canning and Stacy (2019) estimate that for every dollar spent on SNAP benefits, \$1.5 is added to GDP, a 50% multiplier effect. The authors conclude that this multiplier effect results from participants' increased consumption, including issued food benefits being redeemed at retailers, as well as increased expenditures on non-food categories,[4] which generates employment, increases incomes, and promotes growth. In order for this multiplier to be achieved, participants need access to authorized retailers at which to redeem benefits. Our paper highlights that redemption of SNAP benefits is constrained by participants' local food environment, which differs across the rural/urban continuum. We should note that we do not estimate a multiplier effect in this paper; rather we examine one of the mechanisms through which a multiplier effect would propagate.

3 Data

3.1 SNAP Data

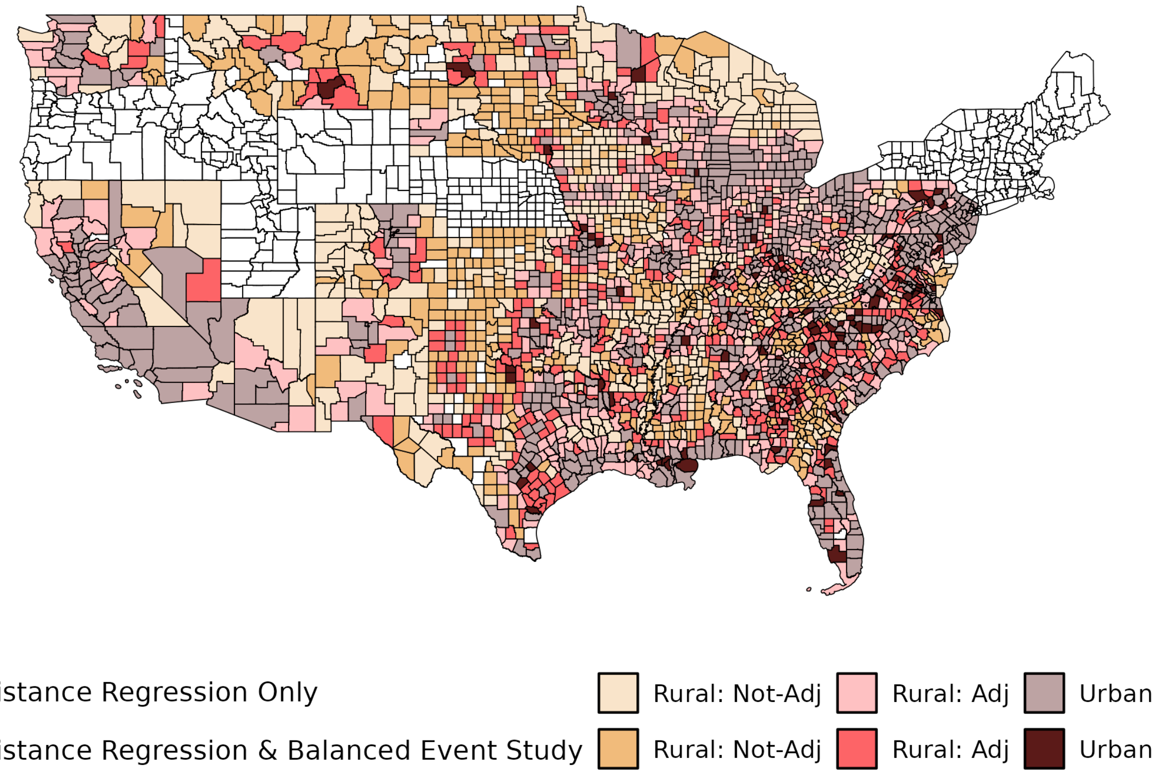
Much of the data we used was requested from FNS via a Freedom of Information Act request. We received geographic data for retailers authorized to accept SNAP benefits from the FNS Store Tracking and Redemption System (STARS). These data are continually updated and contain authorization information for SNAP retailers dating back to the beginning of the SNAP program. The last year for which we obtained data was 2020, so we observe authorizations of SNAP retailers from the beginning of the program until 2020. We also obtained county-month SNAP redemptions from STARS for the fiscal years 1998-2020.[5]. The availability of the redemptions data restricts us to looking at superstore expansions from 1998 to 2020. A small subset of county-month redemption observations (5.6% of total observations) were redacted whenever three or fewer retailers were authorized to redeem SNAP benefits for that period to preserve confidentiality. FNS publicly reports SNAP issuance and participation data at the county level in January and July of each year dating back to 1989. This means that redemptions in a county in a month may be either less than or greater than issuance in that county in that month. These data are provided by each state in December of the previous year or May of that year respectively at either the state, county, or office level.[6]

[4]Present estimates of the marginal propensity to consume food out of SNAP dollars are estimated between 0.2 and 0.59, with many estimates in the range of 0.44 to 0.48 *inter alia* (Beatty and Tuttle, 2015; Hastings and Shapiro, 2018; Hoynes and Schanzenbach, 2009; Leung and Seo, 2023)

[5]More details on how authorized locations were identified in Appendix B

[6]Since our analyses are at the county-month level, states that did not report county-level data over our entire study period were excluded. We dropped outlier observations where inconsistent reported issuance values across time periods raised concerns about data quality. See Figure 1 for a comprehensive visualization of available data.

Figure 1: Distribution of Available County-Level SNAP Data



Note: Counties were classified as rural or urban using 1993 UICs from ERS. States in white (along with Alaska and Hawaii) were not included in our analysis as they did not report county level SNAP issuance over our study period.

3.2 County and Retailer Spatial Data

Except in cases where coordinate information was missing or inconsistent with address information, we retained the retailer's latitude and longitude as given in STARS.[7] For the approximately 87,000 retailer observations with missing coordinates, we utilized the IPUMS Geomarker tool to geolocate those observations using the provided address, city, state, and zip code data.[8] Once retailers were consolidated into unique locations using address, city, state, and coordinate information, we then spatially matched authorized locations to counties using the 2019 TigerLine County Shapefiles.

Counties were flagged as either urban, rural but adjacent to a small or large metropolitan statistical area (MSA), or rural and not adjacent to a small or large MSA using the USDA's Economic Research Service

[7]For more detail on the geocoding process and assigning locations, see Appendix B.

[8]IPUMS categorizes the quality of geolocations as either 'AddressPoint', 'Parcel', 'StreetSegmentInterpolation', 'StreetCentroid', 'AddressZIPCentroid', 'POBoxZipCentroid', 'City Centroid', 'State Centroid', or 'Unmatchable'. For our purposes we dropped 20,300 observations for which IPUMS could only return coordinates of any of the latter five types, 4,200 of which were authorized over our sample period. However, only 46 of these observations were superstores in states with county-level issuance data. Of the 66,700 of observations that we retained, 43,700 were point-identified, 27,700 were street-segment interpolated, and 3,600 were property-parcel identified.

(ERS) 1993 urban influence codes (UICs).[9] Each county was assigned one of 12 codes according to the population of its largest town or city and its proximity to micro or metropolitan areas. Henceforth, we will use the term "rural adjacent" counties to refer to rural counties adjacent to a MSA, and "rural not-adjacent" counties to refer to those not adjacent to a MSA.

4 Methods

Our outcome of interest is the ratio of SNAP dollars issued to SNAP dollars redeemed (henceforth the IRR) in a county. This metric captures the degree to which SNAP dollars are redeemed in local economies, and importantly, it is scaled by level of issuance to allow comparisons across counties. An approach that takes the level of redemptions as the outcome cannot distinguish between an additional dollar of redemptions in a county with many participants – and thus higher issuance – versus a small one. An IRR closer to 0 indicates that more SNAP benefits were redeemed than were issued in the county in the month. A ratio over 1 would imply the converse - that issuance was larger than redemptions. In general, a higher IRR suggests that the food retail channel for the SNAP multiplier effect is dampened in that county at that time (although we note that there are pathways besides the food retail channel through which the multiplier effect of SNAP benefits can manifest).

4.1 OLS

Given the relative importance of superstores for SNAP participants, we expect that a county's proximity to a SNAP authorized superstore will be correlated with its IRR. This hypothesis motivates our reduced-form specification, estimated separately on the subsample of counties with a superstore and the set of counties without a superstore:

$$\begin{aligned} IRR_{ct} = \beta_0 + \beta_1 dist_{ct} + \beta_2 dist_{ct}^2 + \gamma_1 dist_{ct} \times rural_c^{NA} + \gamma_2 dist_{ct} \times rural_c^{A} + \\ \gamma_3 dist_{ct}^2 \times rural_c^{NA} + \gamma_4 dist_{ct}^2 \times rural_c^{A} + \mu_{y(t)} + \phi_c + \varepsilon_{ct} \end{aligned} \tag{1}$$

where IRR_{ct} is the IRR in county c in time period t and $dist_{ct}$ denotes the as-the-crow-flies distance (in miles) from the population-weighted centroid of county c to its nearest superstore. $rural_c^{NA}$ and $rural_c^{A}$ are indicators for rural not-adjacent and rural adjacent counties respectively. Finally, $\mu_{y(t)}$ and ϕ_c are year and county fixed effects. Standard errors are clustered at the county level to allow for correlation of errors within county over time. See Table 1 for relevant summary statistics for the first and last years in our distance regression sample.

[9] We assigned counties to buckets using the 1993 UIC data to mitigate endogeneity concerns over our sample period.

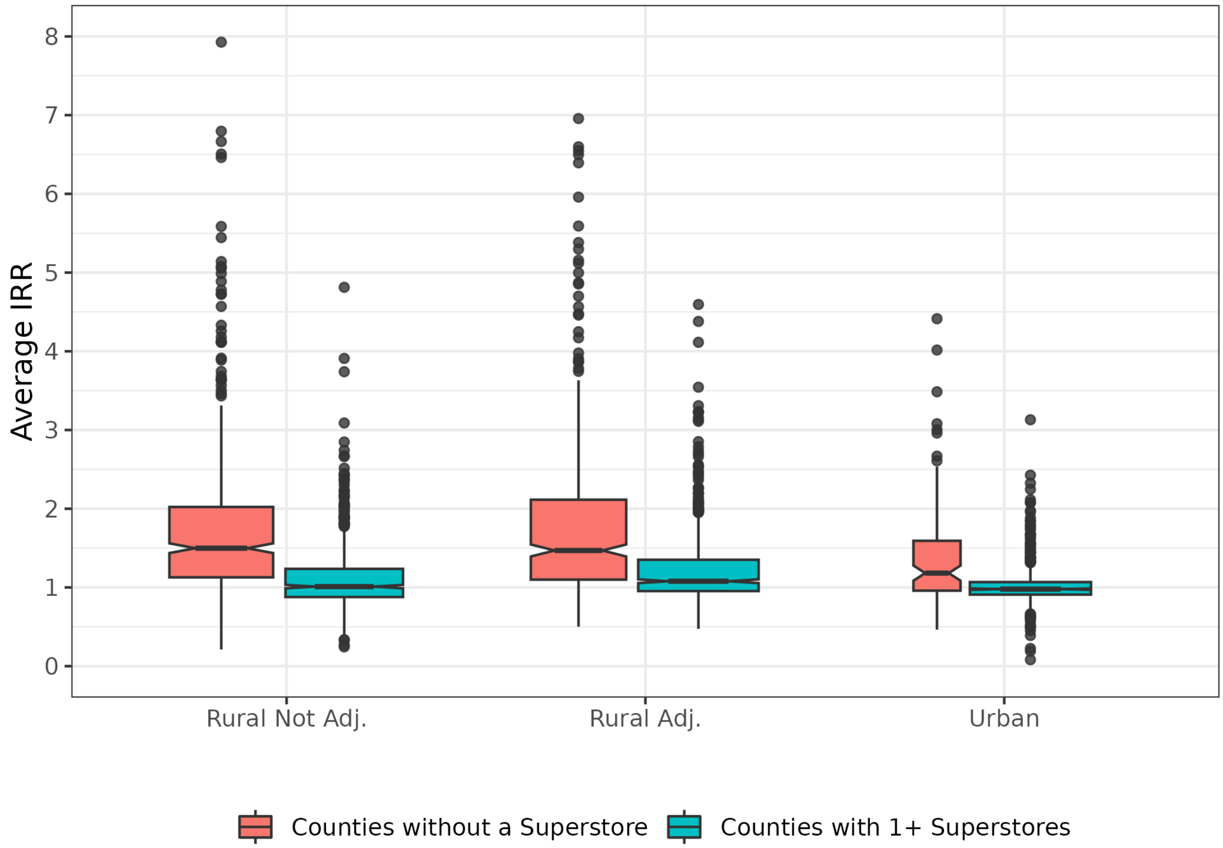
Table 1: Distance Regression Dataset: Summary Statistics

			Rural Not Adj.	Rural Adj.	Urban
1998					
Counties Without a Superstore	# of Counties		468	408	106
	Distance to Superstore	Min.	4.3	1.02	1.68
		Med.	23.65	20.27	13.5
		Max.	109.93	92.67	60.22
	I/R Ratio	Min.	0.06	0.52	0.47
		Med.	1.31	1.31	1.12
		Max.	8.42	25.05	10.61
Counties With At Least 1 Superstore	# of Counties		455	465	613
	# of Superstores	Min.	1	1	1
		Med.	1	1	4
		Max.	8	8	314
	Distance to Superstore	Min.	0.07	0.05	0.13
		Med.	2.84	3.23	2.47
		Max.	28.62	37.83	20.69
	I/R Ratio	Min.	0.27	0.01	0.26
		Med.	0.96	1.02	0.94
		Max.	10.36	7.54	2.92
2020					
Counties Without a Superstore	# of Counties		243	192	24
	Distance to Superstore	Min.	4.62	0.97	4.45
		Med.	20.19	17.48	10.41
		Max.	81.48	68.94	16.04
	I/R Ratio	Min.	0.01	0.36	0.52
		Med.	1.39	1.68	1.63
		Max.	6.23	6.6	19.83
Counties With At Least 1 Superstore	# of Counties		562	583	636
	# of Superstores	Min.	1	1	1
		Med.	2	2	11
		Max.	18	16	607
	Distance to Superstore	Min.	0.06	0.04	0.01
		Med.	2.46	2.81	1.7
		Max.	27.74	31.64	19.19
	I/R Ratio	Min.	0.12	0.21	0.06
		Med.	0.85	0.88	0.78
		Max.	6.44	6.16	2.47

Note: Distance is as-the-crow-flies distance from a county's population-weighted centroid to its nearest superstore.

We distinguish between counties with and without a superstore for this analysis and Figure 2 illustrates why this matters. The IRR in counties with a superstore is centered at 1 regardless of rural/urban status, but there is more dispersion for rural counties. For all county types without a superstore, the IRR is centered to the right and with more dispersion than for that type with a supestore. The IRR for rural counties without a superstore is centered at 1.5, and roughly a quarter of these counties have ratios between 2 and 3.5. This variation in the distribution of average IRR by county type suggests that participant responses to distance to the nearest superstore from their county's centroid will vary by whether the closest superstore is inside the county or not. We expect that the marginal effect of increasing distance to a superstore on the IRR is negative for counties without a superstore.

Figure 2: Distribution of Average IRR by County Type



Note: We average the IRR for each county by an in-county superstore indicator over our sample period. In other words, a county will be included in the distribution of both groups if it experienced periods both with and without a superstore. Each box denotes the interquartile range width of each box is a function of the number of observations in that group, where wider boxes indicate a larger sample. The notch in each box is at the median value. Finally, we limit the y-axis to allow a clearer picture of the distribution. This eliminates 4 rural adjacent counties and 2 urban counties all in the D.C. metropolitan area.

For counties that have a superstore, moving the superstore away from the own-county population-weighted centroid moves it towards another county. Conditional on the superstore remaining in the county, this increases the catchment area of the superstore from neighboring counties. We assume that this marginal increase in the distance from the population-centroid on net increases the total potential set of SNAP redeeming participants for that store. That is, the set of participants in the own or neighboring counties captured by the move is at least as large as the set of participants in the county that are deterred from redeeming benefits at the superstore by the move.[10] We thus expect positive effects of distance on the IRR for counties with a superstore.

[10]This logic is similar to that used in a Hotelling model.

The γ coefficients should be interpreted as the either the linear or quadratic marginal effect of distance to superstore in rural adjacent and rural not-adjacent counties *relative to urban counties.* We do not have an *ex ante* hypothesis about signs for these coefficients as there are multiple mechanisms that could affect both the direction and magnitude of this effect. For example, distance could be a more important factor for participants without a car in rural versus urban areas. Alternatively, distance could be less of a concern for rural participants who have a mode of transportation and are used to making out-of-county trips for food and other necessities.

4.2 Event Study: First Superstore Entry

To further evaluate the effect of a superstore on the redemption behavior of SNAP participants, we implemented an event study framework, where an event is the first authorization of a superstore in a county in our study period. We restrict our sample to counties without a SNAP authorized superstore in 1998, which is the first full year of our redemption data, and estimate the following:

$$IRR_{ct} = \sum_{p \in [-6,6] \setminus \{-1\}} \alpha_p \mathbb{1}\{t = t^* + p\}_{ctp} + \mu_{y(t)} + \phi_c + \varepsilon_{ct}, \tag{2}$$

where t^* is the event period (when the first superstore entered) in county c and $\mathbb{1}\{t = t^* + p\}_{ctp}$ is a binary indicator equal to 1 if time t is p periods before or after t^*. The county issuance/redemption data are available at the biannual level so our primary specification considers a 6 year event window with twelve observations of issuance/redemption, or 3 years before and after superstore entry. We assume treatment is irreversible in the sense that the indicator is set equal to 1 for all periods after the event window.

Recall from Section 3.1 that some issuance and redemption data are missing at the county level over our study period. Since these missing data can be correlated with superstore entry, we restrict our main sample to counties with a balanced panel, or where the IRR is not missing over the event window. We prefer the balanced panel specification to avoid potential bias from compositional changes in the groups included in the analysis due to missingness (Callaway and Sant'Anna, 2021; Schmidheiny and Siegloch, 2023). In the balanced panel, we are implicitly trading off efficiency to reduce bias. To demonstrate how our results change were we to make the opposite tradeoff, Table 7 shows results when an unbalanced panel is used. Post-period coefficients are larger for all county types and become insignificant for rural adjacent counties.[11]

In Figures 5 and 6 we show the trends in the average of the IRR in event time for ever-treated units and in calendar time for never-treated (control) units. These figures show roughly constant trends in the pre-period for counties that start the sample with no superstore and experience the entry of a superstore over our sample. These constant trends allay some potential concerns about anticipation effects. We also show relatively constant patterns (measured in calendar time, as event time is not well-defined for control units) for counties that never have a superstore in our sample. The main shock in the never-treated sample is the December 2018-January 2019 government shutdown, during which USDA instructed state offices to issue February benefits early – before January 20th – in order to ensure that participants would not

[11] For an illustrative example to see why a balanced panel is necessary, consider Banks County in Georgia, a rural adjacent county which received its first SNAP authorized superstore in the first half of 2000. At the start of our sample period, the county's IRR averaged around 35.55. In the period after superstore authorization, the number of SNAP retailers fell below the threshold so county redemptions were redacted consistently until the end of 2004. Starting in 2005, the IRR in Banks averaged around 0.81. The entire decrease was attributed to the 6th post-period in this case, which masks any effect in periods closer to entry.

run out of benefits in the face of the partial government closure. This produced extremely high levels of issuance relative to redemption as issuance approximately doubled within the month of January. Issuance and redemption levels returned to normal in the middle of 2019 before becoming somewhat more erratic as SNAP participation, issuance, and redemption patterns begin to respond to the COVID-19 shock to employment and shopping behavior. All of these shocks are obscured by averaging in the treated event time figure. If observed pre-treatment changes in the control units match observed pre-treatment changes in the ever-treated units, this supports our main identification assumption. We do not condition our results on any pre-test to avoid introducing bias that would distort our estimated treatment effects or lead to incorrect inference on the treatment effects (Roth, 2022).

4.2.1 Additional Analyses

A complementary way to capture how the presence of a superstore affects a county's IRR is to focus on newly authorized stores in neighboring counties. We expect that the IRR of counties without a SNAP-authorized superstore, but whose neighboring county receives such a store, would increase. For results to be comparable to our primary event study, we focus on the set of counties bordering the subset of counties in our main specification (i.e. those without a superstore in 1998). We require that treated counties have no superstore in the entry period but we allow for own-county entry over the sample period to keep our coefficients conservative.[12] We then restrict our set of treated counties to those with a balanced panel over the event window as defined above, and restrict our set of control counties to those that never received a superstore over our sample period and nor did any of their neighboring counties. Given the higher probability that additional authorized superstores enter one of multiple neighboring counties, we chose a four-year event window to minimize interference from additional changes to the choice set.[13]

We see this analysis as complementary, but secondary, to the own-county entry event study. The subsample that meets the inclusion criteria as the own-county sample but extended to all neighbors (in order to satisfy the Stable Unit Treatment Value Assumption) is very small. These criteria maximize comparability of coefficients across specifications, but the resulting analysis is underpowered.

5 Results

5.1 OLS

Table 2 shows results for equation 1, which estimates the relationship between distance to a county's nearest superstore and its IRR. The first two models from the left are estimated on the subset of observations where a superstore is not present in county c in time period t, while the two models furthest to the right are estimated on the subset of counties *with* a superstore in time period t.

Our results indicate a significant relationship between distance from a county's population-weighted centroid to its nearest superstore and a county's IRR. The direction and magnitude of this relationship is dependent on whether that county has a superstore and which rural-urban designation the county falls into. For urban

[12]For example, suppose we observe County A, which borders County B, and neither has a superstore at the start of our sample period. If B receives a superstore in March 1999, we require that A not have a superstore

[13]There are a few instances where a county could be in the event study twice. For example, again if we observe County A and no county has an authorized superstore at the start of our sample. If the first superstore becomes authorized in County B in 1999 and closes in 2006, then another superstore enters County C in 2014, and County A never receives a superstore, then County A would experience two events.

Table 2: IRR and Distance to Nearest SS

Dependent Variable:	Issuance/Redemption			
In-County Superstore:	No		Yes	
	(1)	(2)	(1)	(2)
Distance	-0.0160**	-0.0640*	0.0115**	0.0245**
	(0.0072)	(0.0328)	(0.0045)	(0.0097)
Distance^2	0.0001**	0.0012***	-0.0003**	-0.0015***
	(5.99×10^{-5})	(0.0005)	(0.0001)	(0.0005)
Distance × Rural Not-Adj.		0.0461		-0.0115
		(0.0336)		(0.0142)
Distance × Rural Adj.		0.0433		-0.0145
		(0.0356)		(0.0104)
Distance^2 × Rural Not-Adj.		-0.0011**		0.0012*
		(0.0005)		(0.0007)
Distance^2 × Rural Adj.		-0.0010*		0.0013**
		(0.0005)		(0.0005)
Fixed-effects				
County	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	23,943	23,943	80,092	80,092
R^2	0.76337	0.76355	0.59309	0.59314

Note: Standard errors clustered at the county level reported in parentheses.
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

counties without a superstore, each additional mile of distance to the nearest superstore is associated with an approximately 0.06 point decline in that county's IRR,[14] offset by 0.0024 points for each additional mile from the centroid. This means that the net effect of distance on IRR is negative for urban counties without a superstore, but for which the nearest superstore is within about 27 miles. When the nearest superstore moves a mile further away, redemptions within the county increase by nearly 7%. This indicates that increasing the travel costs to get to a superstore increases own-county redemptions. For urban counties without a superstore for which the nearest superstore is more than 27 miles away, theoretically shifting the nearest superstore an additional mile further away would decrease redemptions within the county (increasing the IRR holding issuance constant). While counterintuitive, we find that less than one percent of urban counties without a superstore in our sample are more than 27 miles from the nearest superstore. This indicates that this predicted positive effect is outside the support of our data/only applies to a small share of counties.

The marginal effect of increasing the distance from a rural adjacent county to its nearest superstore is not as large, with a base IRR decrease of 0.021 for an additional mile. This means that in rural adjacent counties and without their own superstore, moving the nearest superstore an additional mile away is predicted to increase redemptions in the county by 2%. Accounting for the quadratic term, this marginal effect approaches zero as the store is farther away from the population weighted centroid. The model predicts that the net effect of distance on the IRR is zero at 52 miles from the centroid, with predicted *positive* marginal effects of

[14] We note that this baseline effect would never be supported by our sample by definition, since in this sample the distance to a superstore from the centroid is always positive because there is no superstore in the county.

distance beyond this point. We find similar effects for rural not-adjacent counties, with a baseline marginal effect of -0.018 which tapers to zero at around 90 miles from the centroid. Less negative net marginal effects with increasing rurality indicate that the marginal cost of travel to redeem benefits is lower in more rural places relative to urban places. The smaller quadratic effects suggest a similar effect, that the change in travel costs with additional distance is smaller in rural areas, and especially rural areas not adjacent to a MSA, relative to urban areas.

Table 3: Marginal Effects of Distance

		Avg. Distance to Superstore	Model 1	Model 2
Counties Without a Superstore	Rural: Not-Adj.	26	-0.0086	-0.0106
			(0.0071)	(0.0087)
	Rural: Adj.	20	-0.0103	-0.0099
			(0.0071)	(0.0131)
	Urban	12	-0.0126	-0.0344
			(0.0071)	(0.0046)
Counties With At Least One Superstore	Rural: Not-Adj.	4	0.0091	0.0107
			(0.0042)	(0.0106)
	Rural: Adj.	4	0.0091	0.0085
			(0.0042)	(0.0046)
	Urban	3	0.0097	0.0155
			(0.0042)	(0.0046)

Note: The marginal effects are calculated at the average distance. Delta method standard errors presented in parentheses.

We find effects moving in the opposite direction for counties with a superstore. For an urban county with a superstore located exactly at its population weighted centroid, moving that superstore one mile away from the centroid would increase the IRR by 0.0245. Holding issuance constant, this translates to decreasing redemptions by 2.4%. The quadratic effect here also dampens the magnitude of the effect, with the net effect predicted to reach zero at about eight miles away from the centroid. Again, the effects of distance on changes in the IRR are smaller in magnitude for rural areas although the direction of the effect is the same as in urban areas. For rural adjacent counties with a superstore at the centroid, moving that superstore one mile away would decrease own county redemptions by 1%. The same effect for a rural not-adjacent county is 0.013, or a decrease in own county redemptions of 1.3%. For these two types of counties, the threshold at which the positive quadratic effects outweigh the negative linear effects are 25 and 22 miles, respectively. Beyond this point, moving the superstore further from the centroid is predicted to increase own counties redemptions, perhaps by getting sufficiently close to the county border that the superstore begins to capture redemptions from neighboring counties.

5.2 Event Study

Figure 3 plots coefficients for our balanced panel event study analysis subset by county type. Full regression results are reported in column (1) of Table 5. We omit the period prior to superstore entry as a reference period, which is shown on the graph with a black dot at zero. Pre-period coefficients in rural counties are small and not statistically different from the reference period. Once a superstore enters, however, the

IRR persistently declines over the sample period. The first post-period coefficient is approximately -0.03 for rural not-adjacent counties, and this effect increases in absolute value to approximately -0.13 by the fifth post-period. On average, the effect of superstore entry on the IRR in rural not-adjacent counties is -0.10. Holding issuance constant at the pre-treatment average across all treated counties, this effect corresponds to an average $14,086 dollar increase in SNAP redemptions in rural not-adjacent counties per post-period. This represents a 10% increase over pre-period average redemptions in this type of county.[15]

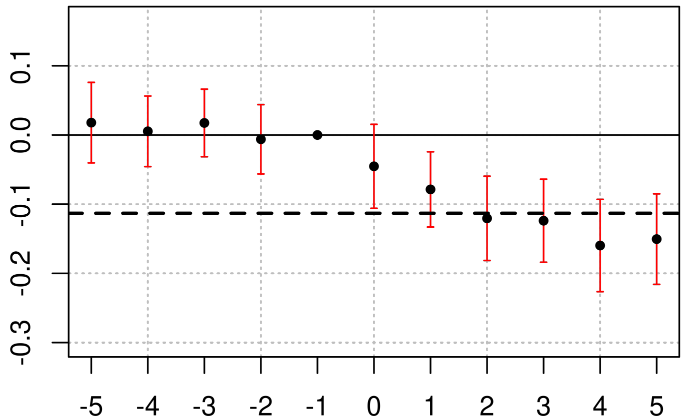
In rural adjacent counties, the magnitude of the estimates is slightly larger, as shown in panel (b) of Figure 3 and column (2) of Table 5. On average, the per six-month change in the IRR from the entry of a superstore is -0.13 in rural adjacent counties. These effects grow over the three years after superstore entry, from -0.06 to -0.18. Holding issuance constant at the pre-treatment average across counties that have a superstore enter, the average post-period effect corresponds to an average $14,725 dollar increase in SNAP redemptions in rural adjacent counties per post-period. This represents an 11% increase over pre-period average redemptions in this type of county. As in rural not-adjacent counties, the pre-period estimates are not statistically significant.

The change in the IRR for urban counties – shown in panel (c) of Figure 3 and column (3) of Table 5 – responds more quickly to the entry of the a superstore, with larger point estimates in the first year and a half after a superstore enters. In later post-periods, the effect size falls in between estimates for rural adjacent and rural not-adjacent counties. In general, these estimates are less precise although still statistically significant in the post-period (and not significant in the pre-period), which is consistent with the smaller sample size. The change in the IRR in the six months after entry is -0.07, increasing to -0.16 over the three years after entry. The average effect of superstore entry on the IRR for urban counties is -0.13. Holding issuance constant at the pre-treatment average across counties that have a superstore enter, the average post-period effect corresponds to an average $24,415 dollar increase in SNAP redemptions in urban counties per post-period. This represents a 13% increase over pre-period average redemptions in this type of county.

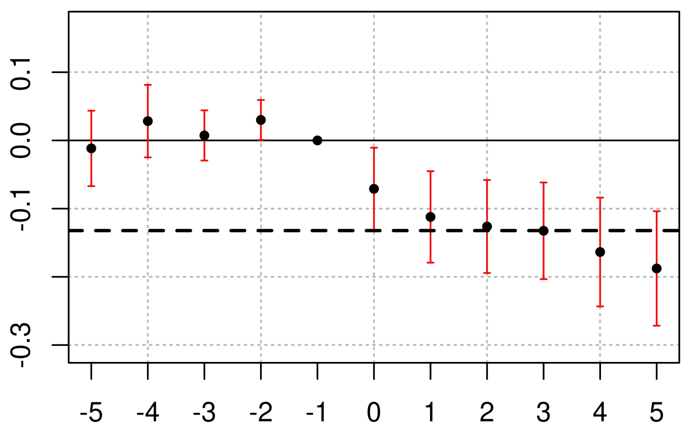
[15] Each post-period coefficient $\alpha_p = \frac{issuance_{t+p}}{redemption_t+p} - \frac{issuance_{t^*-1}}{redemption_{t^*-1}}$. So, for rural not-adjacent counties for example, $\alpha_1 = -0.07$, which, under the assumption that issuance changed a negligible amount from the reference period, implies that $\frac{(redemption_{t^*+1} - redemption_{t+p})}{redemption_{t^*-1}} \approx 0.07(redemption_{t+p})$. Thus, each coefficient, when scaled by redemption in period $t+p$, is equal to the rate of change for redemptions over this period.

Figure 3: Balanced Panel Event Study Coefficients

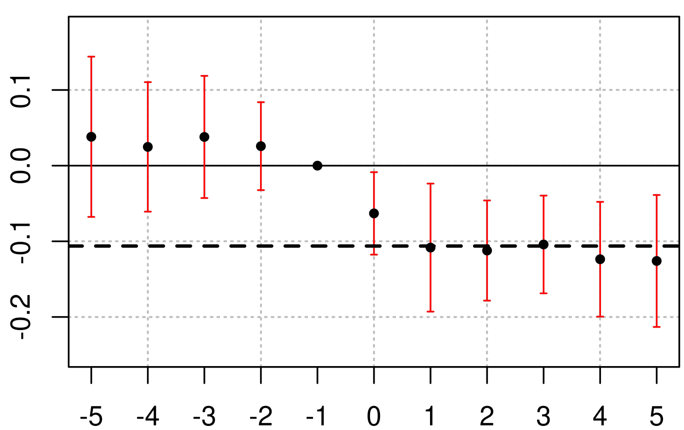
(a) Rural Counties Not Adjacent to MSA



(b) Rural Counties Adjacent to MSA



(c) Urban Counties



Note: The horizontal dashed line in each plot indicates the average post-period coefficient for that sub-sample.

5.3 Additional Analyses and Robustness Checks

Our main event study specification looks at how the first in-county superstore authorization after 1998 affects that county's IRR, and our sample includes counties with additional changes to the menu of authorized superstores in the post-period. Figure 4 shows event study coefficients when our balanced sample is restricted to counties with a clean event window, or without any additional changes to the number of superstores over its event window.[16] This specification drops 24 rural not-adjacent, 33 rural adjacent, and 21 urban counties. These results look very similar to those in Figure 3. The magnitude of the average effect sizes over the post-period are similar for rural not-adjacent, rural adjacent, and urban counties. The largest differences across the two specifications appear in urban counties. When excluding counties that have additional entries in the post-period, we find on average slightly more negative treatment effects (-0.19 versus -0.18). The magnitude of the pre-period estimates is larger, although again all pre-period coefficients across all three rural-urban designations are not statistically significant. These results suggest that our base specification reasonably captures the effect of the entry of the first superstore on the net flow of SNAP benefits in a county of a particular type. The tradeoff with efficiency, to get more precise estimates from the full balanced panel – rather than reducing the sample size to only 264 counties – seems worthwhile, as the cost in terms of bias appears small. We find evidence that additional entries in the post-period, when they occur, are not driving our results.

As discussed in Section 4.2.1, we also performed a complementary analysis to test whether redemption behavior in a county responds to superstore entry in a neighboring county. To maximize internal validity and comparability of coefficients across specifications, we restricted our sample size to those who met conditions specified in Section 4.2.1. This left a balanced panel of 120 rural not-adjacent, 121 rural adjacent, and 23 urban counties that experienced an event.[17]

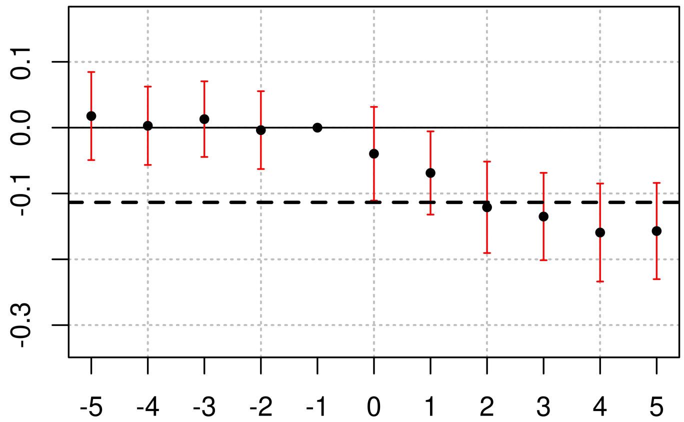
Results for this specification are markedly less conclusive than for own-county entry. Coefficients for rural not-adjacent counties are roughly zero for the entire study period, while post-period coefficients for rural adjacent and urban counties are insignificant but trend upwards. These results are largely consistent with those from our clean entry window specification shown in Table 9, with the exception that rural not-adjacent county coefficients are negative and significant in the post-period. Given the precise comparison we aimed to make with this analysis, it is likely that our counter-intuitive results are at least in part driven by selection bias.

[16] Full regression results presented in Table 6.

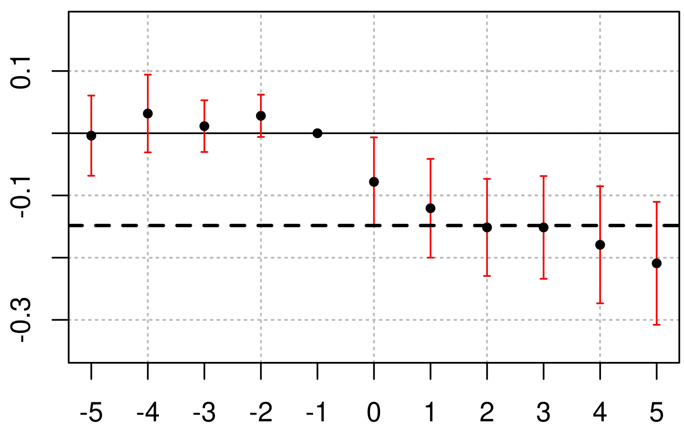
[17] Results for the full balanced panel regression presented in Table 8 and for the balanced panel regression with a clean event window in Table 9.

Figure 4: Balanced Panel with Clean Event Window Coefficients

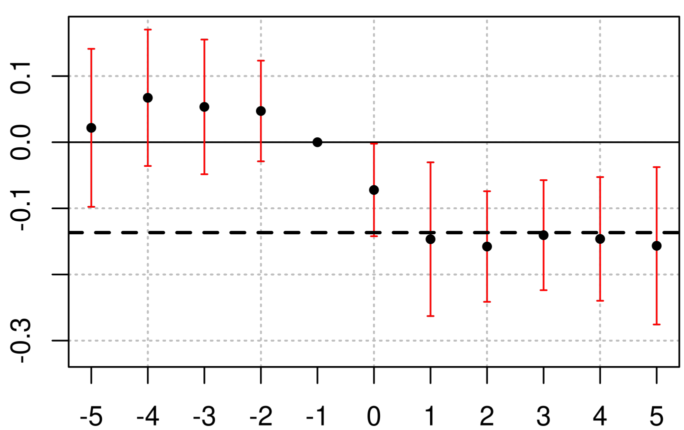
(a) Rural Counties Not Adjacent to MSA



(b) Rural Counties Adjacent to MSA



(c) Urban Counties



Note: The horizontal dashed line in each plot indicates the average post-period coefficient for that sub-sample.

6 Conclusion

We find that the food retail landscape affects the flow of SNAP benefit redemptions across space. We expand on work that examines the relationship between the food retail environment and SNAP redemptions to focus on the interaction of superstores across the rural-urban divide and the flow of SNAP benefits. This builds on the idea that superstore entrance affects the set of SNAP authorized stores (Ollinger et al., 2021) and that the food environment, particularly in rural areas, is undergoing changes that shift consumers' purchasing behavior (Stevens et al., 2021).

We focus on superstores, where more than half of all SNAP benefits are redeemed (Ollinger et al., 2021). We examine how the ratio of SNAP issuance to SNAP redemption changes along both the intensive margin – the change in the IRR for a one mile change in the distance to a store from the population-weighted county centroid – and the extensive margin – the change in the IRR when a superstore enters a county. Along the intensive margin, we find that moving a superstore further away from the centroid of a county without a superstore will increase redemptions in that county, to a point, and that moving a superstore further away from the population center of a county *with* a superstore will reduce redemption in that county, holding issuance constant. This suggests two effects: one, that where it is costly to redeem benefits at a superstore (due to travel time and expense), making superstores less accessible increases use of existing non-superstore SNAP authorized food retailers in the county; two, that the entrance of a superstore into a county shifts benefits into the county.

We find intuitive heterogeneity along the rural-urban divide, with more rural places showing smaller marginal effect sizes along the intensive margin, consistent with lower transportation costs (time per mile driven is lower because of higher average speed limits and less congestion; vehicle ownership is higher; fuel prices may be lower). Holding issuance constant, this would result from increasing redemptions in the county after the entry of the superstore. We find suggestive evidence that for metropolitan counties, when a superstore enters in a neighboring county, the IRR increases within the county, indicating that redemptions flow across counties from places without superstores to places with superstores.

We face three main limitations. The first is that we only observe SNAP issuance aggregated to the county-level in two months of each year, which has the potential to induces bias in to our estimated effects (Çakır et al., 2018). In an ideal setting, we would use individual-level issuance and retailer-level redemption data available year-round to trace the flow of benefits across space given the food environment. Similarly, while we are able to observe geographic information for retailers from STARS historical authorized store dataset, the geocoding and county affiliations of addresses can be imprecise, potentially causing misallocation of stores across counties. We note that there are months in which redemptions are redacted, indicating that FNS believes that there are fewer than four authorized stores in that county-month, but we count more than four authorized stores in the county in our data. This mismeasurement will not lead to attenuation bias as long as deviations from the true locations to store locations produced by geocoding are not systematically related to the IRR. The last limitation is that, while superstore entry is plausibly quasi-exogenously determined by factors such as distance from superstore chain headquarters (Courtemanche and Carden, 2011), distance of stores from population-weighted centroids is likely endogenously determined with local economic conditions that could determine patterns of SNAP issuance and redemption. However, the logical consistency between the extensive and intensive margin results allays some concerns about this potential endogeneity.

Our work speaks to heterogeneity in the relationship between local food retail environments and the flow of SNAP benefits across space, which has relevance for policy at multiple levels of government. Policymakers may be interested in understanding how to leverage how benefits flow in response to SNAP-authorization of a superstore to target areas for economic growth. One lever available to influence the distribution of SNAP authorized retailers are grants to firms to provide funding to assist with SNAP authorization, including purchasing necessary equipment to satisfy stocking requirements or point-of-sale systems. These grants are provided from the federal government to states who then distribute funds to retailers. The maximum allotment from the federal government to a state is $1 million. Using our estimates, we can consider a back-of-the-envelope cost-benefit calculation for these grants. Policymakers may use this sort of cost-benefit calculation to justify providing subsidies to retailers to participate in SNAP. As an upper bound, suppose each state receives the maximum allotment.[18] This implies a $50 million program cost, or $15,878 per county. If county types are distributed as described in Table 1 and an average county experiences a change in redemptions equal to our estimated average dollar changes calculated out of Figure 3, then the change in redemptions on average across counties would be $17,350, more than breaking even relative to per county cost.

We note this is not, from an economist's perspective, a generalizable strategy. To the degree that the shift in redemptions occurs from spillovers of redemptions from neighboring counties rather than additional redemptions among own-county SNAP participants, opening a superstore is in part a beggar-thy-neighbor strategy. These spillovers would mitigate the effect of opening a superstore if neighbors were simultaneously taking this strategy. In addition, our approach does not consider general equilibrium effects of additional stores. Furthermore, we are not calculating a multiplier model as in Blinder and Zandi (2015); Canning and Stacy (2019) or Weerasooriya and Reimer (2020). SNAP's positive economic spillovers could result in different average effects for store entry in a general equilibrium framework. Finally, if states distribute subsidies *en masse* with many stores opening, we would be predicting substantially out of sample relative to the rate of store openings observed in our sample. Given these limitations, we consider that this back-of-the-envelope calculation describes states' approximate decision making process. At the margins, increasing access to SNAP-authorized superstores increases net redemptions.

We conclude that policy that shifts the food environment can affect the flow of SNAP benefits, with positive economic spillovers to local communities. Particularly in rural areas, SNAP matters both because there are many participants in need and because the places to redeem benefits are limited. We show that expanding the set of available SNAP retailers in a county shifts SNAP redemptions into those places.

[18]This is a true upper bound. For instance, in FY 2023 Minnesota awarded $320,750 across all retailers.

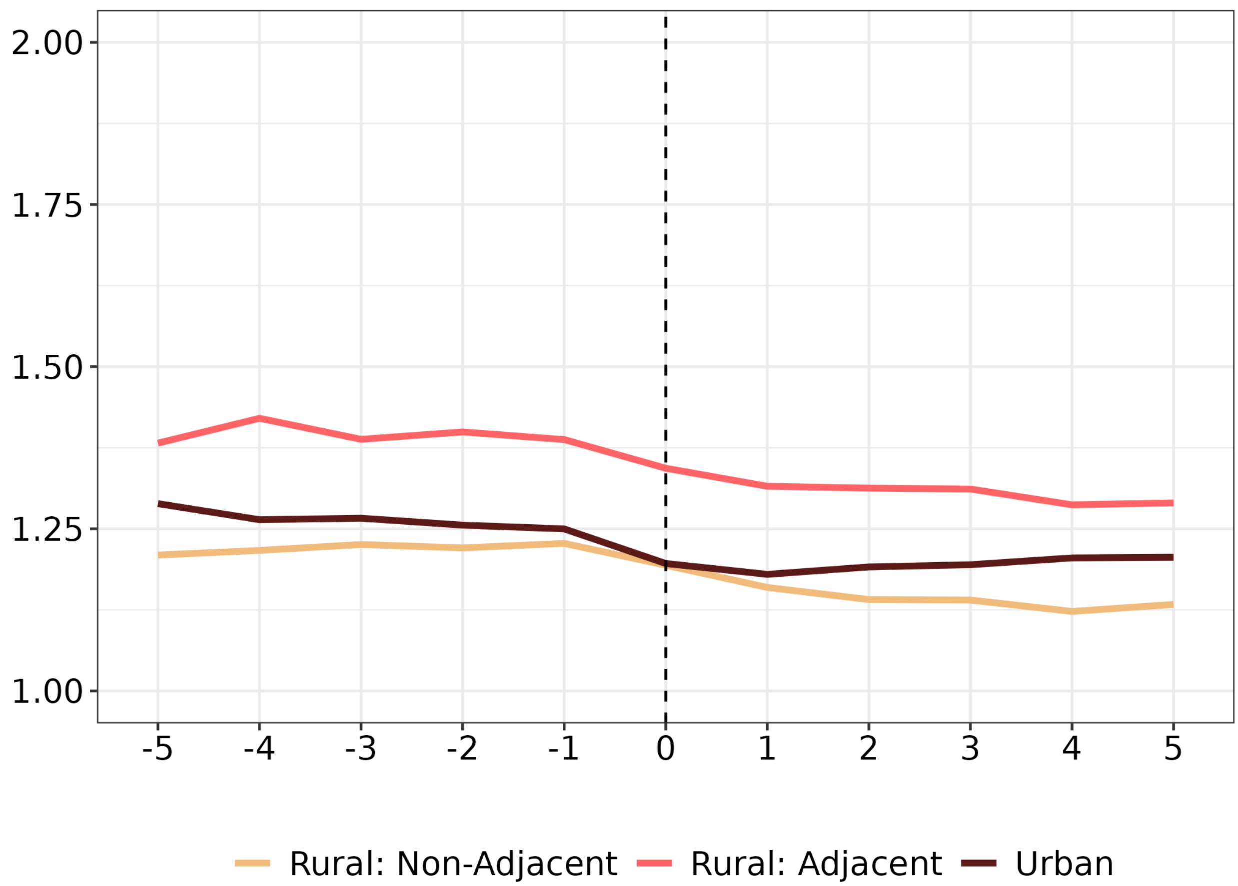
A Additional Tables and Figures

Table 4: In-County Superstore, Distance, and IRR

Dependent Variable:	IRR			
In-County SS	No	Yes	No	Yes
Model:	(1)	(2)	(3)	(4)
Variables				
Distance	-0.0050	0.0056**	0.0097	0.0057
	(0.0035)	(0.0024)	(0.0096)	(0.0049)
Distance × Rural Not-adj.			-0.0158	0.0024
			(0.0103)	(0.0064)
Distance × Rural adj.			-0.0149	-0.0009
			(0.0113)	(0.0052)
Fixed-effects				
County	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	23,943	80,092	23,943	80,092
R^2	0.76327	0.59306	0.76333	0.59306

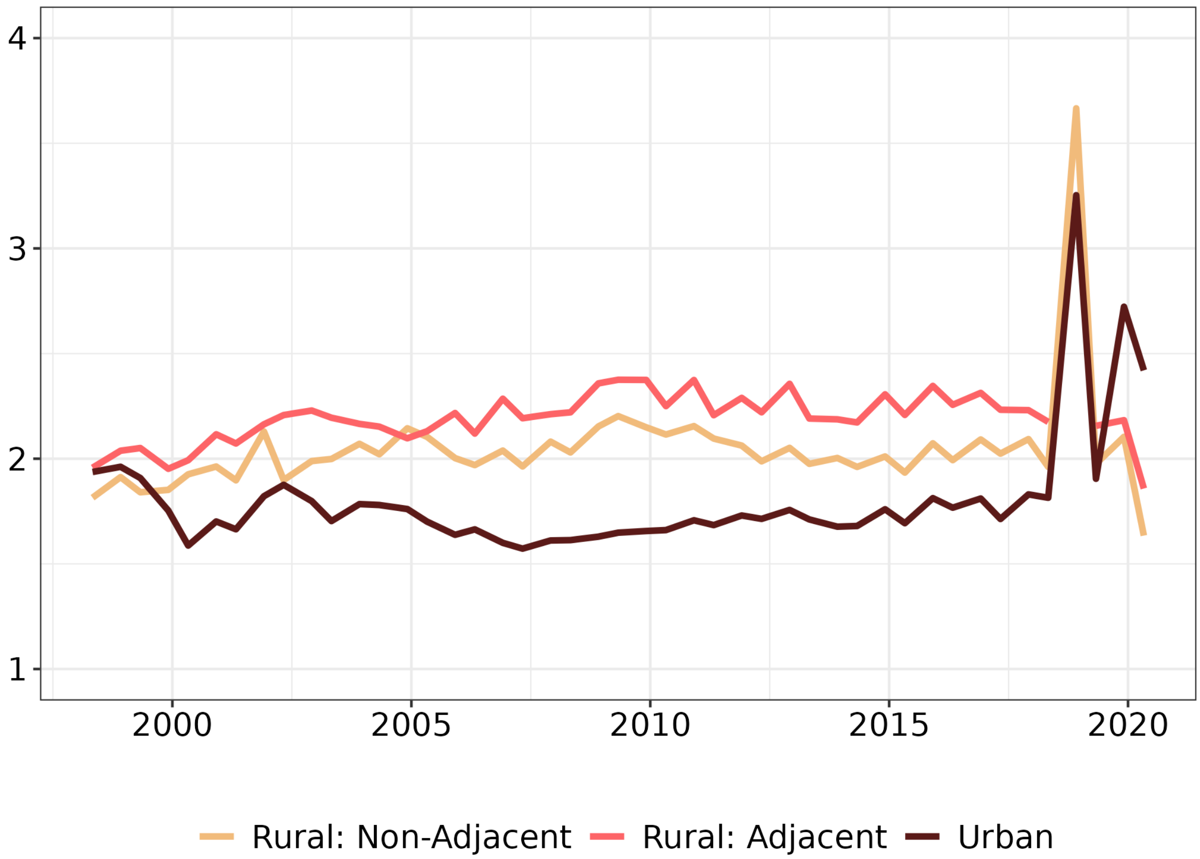
Note: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Figure 5: Average IRR over Event Window of Treated Counties



Note: The IRR for all counties spiked in January 2019 in response to the government shut-down, and again in 2020 as benefits increased to mitigate the economic effects of the coronavirus pandemic..

Figure 6: Average IRR over Sample Period for Event Study Control Counties



Note: Control counties are those that either did not receive an authorized superstore over our sample period or received more than 1 in a single period. The IRR for all counties spiked in January 2019 in response to the government shut-down, and again in 2020 as benefits increased to mitigate the economic effects of the coronavirus pandemic.

Table 5: Balanced Panel Event Study Results

Dependent Variable: Issuance/Redemption			
	Rural	Rural	Urban
	Not-adjacent	Adjacent	
Time to Treat	(1)	(2)	(3)
-6	0.0266	-0.0751	0.0916
	(0.0441)	(0.0469)	(0.0692)
-5	0.0127	-0.0166	0.0613
	(0.0281)	(0.0267)	(0.0579)
-4	0.0077	0.0245	0.0372
	(0.0249)	(0.0261)	(0.0451)
-3	0.0212	0.0068	0.0463
	(0.0233)	(0.0177)	(0.0424)
-2	0.0014	0.0269*	0.0286
	(0.0245)	(0.0145)	(0.0308)
0	-0.0346	-0.0639**	-0.0730**
	(0.0283)	(0.0299)	(0.0282)
1	-0.0723***	-0.1083***	-0.1196***
	(0.0255)	(0.0336)	(0.0416)
2	-0.1034***	-0.1250***	-0.1274***
	(0.0289)	(0.0333)	(0.0330)
3	-0.1059***	-0.1317***	-0.1300***
	(0.0283)	(0.0340)	(0.0347)
4	-0.1497***	-0.1634***	-0.1504***
	(0.0317)	(0.0380)	(0.0391)
5	-0.1299***	-0.1821***	-0.1582***
	(0.0307)	(0.0395)	(0.0459)
6	-0.1522***	-0.2355***	-0.2900***
	(0.0408)	(0.0473)	(0.0810)
Fixed-effects			
County	Yes	Yes	Yes
Year	Yes	Yes	Yes
# Counties	498	396	97
Share Treated	0.281	0.449	0.619
Observations	15,884	15,579	3,997
R^2	0.76164	0.75923	0.74184

Note: Standard errors clustered at the county level reported in parentheses. Signif. codes: ***: 0.01, **: 0.05, *: 0.1

Table 6: Robustness:Balanced Panel Event Study Results (Clean Event Window)

Dependent Variable: Issuance/Redemption			
	Rural Not-adjacent	Rural Adjacent	Urban
Time to Treat	(1)	(2)	(3)
-6	0.0341	-0.0872	0.1140
	(0.0487)	(0.0539)	(0.0786)
-5	0.0153	-0.0139	0.0414
	(0.0317)	(0.0305)	(0.0597)
-4	0.0058	0.0205	0.0739
	(0.0283)	(0.0302)	(0.0519)
-3	0.0180	0.0062	0.0538
	(0.0268)	(0.0201)	(0.0522)
-2	0.0050	0.0246	0.0413
	(0.0281)	(0.0167)	(0.0397)
0	-0.0343	-0.0711**	-0.0796**
	(0.0329)	(0.0351)	(0.0363)
1	-0.0668**	-0.1175***	-0.1477***
	(0.0295)	(0.0398)	(0.0543)
2	-0.1029***	-0.1516***	-0.1585***
	(0.0327)	(0.0377)	(0.0392)
3	-0.1141***	-0.1501***	-0.1566***
	(0.0318)	(0.0394)	(0.0407)
4	-0.1397***	-0.1769***	-0.1606***
	(0.0352)	(0.0444)	(0.0436)
5	-0.1355***	-0.2005***	-0.1818***
	(0.0347)	(0.0460)	(0.0550)
6	-0.1608***	-0.2616***	-0.3588***
	(0.0445)	(0.0516)	(0.0948)
Fixed-effects			
County	Yes	Yes	Yes
Year	Yes	Yes	Yes
# Counties	474	360	74
Share Treated	0.253	0.408	0.595
Observations	14,839	14,022	3,026
R^2	0.75584	0.75283	0.87703

Note: Standard errors clustered at the county level reported in parentheses. Signif. codes: ***: 0.01, **: 0.05, *: 0.1

Table 7: Robustness: Unbalanced Panel Event Study Results

Dependent Variable: Issuance/Redemption			
	Rural Not-adjacent	Rural Adjacent	Urban
Time to Treat	(1)	(2)	(3)
-6	-0.0396	-0.4062	0.1233*
	(0.0572)	(0.3524)	(0.0681)
-5	-0.0470	-0.3680	0.0942
	(0.0479)	(0.3532)	(0.0582)
-4	-0.0534	-0.2234	0.0741
	(0.0478)	(0.2556)	(0.0497)
-3	-0.0388	-0.2231	0.0797*
	(0.0445)	(0.2320)	(0.0464)
-2	-0.0240	-0.1517	0.0689*
	(0.0268)	(0.1909)	(0.0391)
0	-0.0534	-0.4066	-0.0417
	(0.0361)	(0.3522)	(0.0388)
1	-0.1125**	-0.4331	-0.0859*
	(0.0458)	(0.3328)	(0.0487)
2	-0.1546***	-0.4264	-0.0986**
	(0.0456)	(0.3227)	(0.0424)
3	-0.1545***	-0.4666	-0.1082**
	(0.0469)	(0.3423)	(0.0440)
4	-0.1562**	-0.4743	-0.1169**
	(0.0616)	(0.3269)	(0.0476)
5	-0.2163***	-0.5048	-0.1337***
	(0.0514)	(0.3290)	(0.0516)
6	-0.2478***	-0.5894	-0.2507***
	(0.0589)	(0.3618)	(0.0843)
Fixed-effects			
County	Yes	Yes	Yes
Year	Yes	Yes	Yes
# Counties	529	420	102
Share Treated	0.323	0.481	0.631
Observations	17,062	16,544	4,215
R^2	0.72536	0.61300	0.74119

Note: Standard errors clustered at the county level reported in parentheses. Signif. codes: ***: 0.01, **: 0.05, *: 0.1

Table 8: Robustness: Neighboring County Event Study Results

Dependent Variable:	IRR		
	Rural Not-adjacent	Rural Adjacent	Urban
	(1)	(2)	(3)
-4	0.0253	-0.1112**	-0.0574
	(0.0457)	(0.0514)	(0.0939)
-3	0.0141	-0.0355	-0.0243
	(0.0259)	(0.0321)	(0.0557)
-2	0.0170	-0.0680**	0.0391
	(0.0273)	(0.0310)	(0.0509)
0	0.0150	0.0349	0.0205
	(0.0308)	(0.0361)	(0.0484)
1	-0.0407	0.0489	0.0960*
	(0.0282)	(0.0371)	(0.0548)
2	-0.0248	0.0772*	0.0790
	(0.0316)	(0.0455)	(0.0616)
3	-0.0073	0.0790	0.1173
	(0.0329)	(0.0682)	(0.0830)
4	-0.0699	0.1164	-0.0785
	(0.0568)	(0.1137)	(0.1460)
Fixed-effects			
County	Yes	Yes	Yes
Year	Yes	Yes	Yes
# Counties	233	174	33
Share Treated	0.515	0.695	0.697
Observations	8,939	7,326	1,455
R^2	0.75094	0.72743	0.76842

Note: Standard errors clustered at the county level reported in parentheses. Signif. codes: ***: 0.01, **: 0.05, *: 0.1

Table 9: Robustness: Neighboring County Event Study Results (Clean Entry Window)

Dependent Variable:	IRR		
	Rural Not-adjacent	Rural Adjacent	Urban
	(1)	(2)	(3)
-4	-0.0570	-0.1377***	-0.0433
	(0.0530)	(0.0470)	(0.1236)
-3	-0.0147	-0.0660	-0.1249
	(0.0402)	(0.0416)	(0.0876)
-2	-0.0308	-0.0586	0.0736
	(0.0331)	(0.0464)	(0.0689)
0	-0.0513	0.0708	-0.1251
	(0.0315)	(0.0635)	(0.0725)
1	-0.0883**	0.0670	0.0340
	(0.0343)	(0.0510)	(0.1116)
2	-0.0666*	0.1064	0.0182
	(0.0398)	(0.0657)	(0.0795)
3	-0.0552	0.1126	0.1959
	(0.0364)	(0.1079)	(0.1813)
4	-0.1882***	0.1669	-0.1163
	(0.0670)	(0.1582)	(0.1627)
Fixed-effects			
County	Yes	Yes	Yes
Year	Yes	Yes	Yes
# Counties	177	120	18
Share Treated	0.362	0.558	0.444
Observations	6,629	5,020	785
R^2	0.74418	0.70720	0.71645

Note: Standard errors clustered at the county level reported in parentheses. Signif. codes: ***: 0.01, **: 0.05, *: 0.1

B Identification of Authorized Locations

The STARS SNAP historical retailer data had variation in geographic information for individual stores over time. Retailer IDs for each authorized store were established in 2017, but we sometimes observed differences in retailer IDs for the same store name and address information over time. For our purposes, we were interested in access to stores at a specific geographic location, since that would mediate our outcomes rather than examining changes in store ownership, SNAP authorization contract, or other within-location changes that could account for variation in retailer ID. For this purpose, we constructed a set of SNAP-authorized *locations*, which may correspond to multiple authorized retailers at once or across time. As described in more detail below, we assigned location IDs to retailers in part by address and coordinates rounded to 3 decimal places as provided by STARS or as geolocated by IPUMS. In the case of strip malls or other multi-business properties we would expect at least one authorized retailer per location, so we included flags for store type to more fully understand the authorized retailer choice set.

Table 10 lists the variables we use to assign location IDs to each authorized retailer. In addition to address and coordinates, retailer locations are uniquely identified using state and zip code information from STARS. Each location's coordinates were then spatially mapped to county polygons. This geographic information, along with the authorization date and end date variables from STARS, let us merge the set of authorized locations (with indicators for store type) to our county-month panel for SNAP issuance and redemption.

We now describe in more detail the process used to construct a set of SNAP-authorized *locations* from the universe of historical SNAP retailer data. To allow for small differences in coordinate information across retailers, we matched retailers to locations using coordinates rounded to the thousandths decimal place.[19] Once location IDs were assigned, we performed a series of manual checks in attempt to correct false matches and unify geographic information within each location:

1. *Identify unique address, state, city, and zip code combinations associated with multiple location IDs*: Cleaning the *city* variable allowed us to unify observations with incorrect or inconsistent geographic information. Accurate and consistent geographic information is important for our strategy of analyzing the effects of distance between locations and the nearest retailer as well as ensuring that retailers are accurately placed within counties for county level analysis.

2. *Identify unique address, state, latitude, and longitude combinations associated with multiple location*

[19] For example, coordinates for a location could differ across retailers if there was variation or errors in how addresses were reported.

Table 10: Identifying variables for SNAP authorized locations

Variable	**Description**
address	Concatenated *street_ number*, *street_ name*,*additional_ address* from STARS
state	Abbreviated state name (e.g., CA) from STARS
latitude	Latitude (from either STARS or IPUMS) rounded to 3 significant digits
longitude	Longitude (from either STARS or IPUMS) rounded to 3 significant digits
zipcode	Zipcode (from either STARS or IPUMS)

IDs: This caught instances where the zip code was inconsistent across observations. If multiple location IDs were equivalent across these variables, as well as either store name or city, we merged those locations.

3. *Populate missing coordinates with mode then take weighted average of latitude and longitude*: If a retailer associated with a location ID had missing coordinates, we assigned to it the modal latitude and longitude associated with that location. Then, we assigned to the location the weighted average of unique latitudes and longitudes across retailers.

After some preliminary cleaning of the geographic data retained from either STARS or IPUMS, we began with over 770,000 unique combinations of store name, address, latitude, and longitude over our study period. This corresponded to approximately 517,000 location IDs as defined above. We further unified locations via the first two checks listed above, which narrowed our final dataset to roughly 497,900 unique retailer locations. By identifying authorized *locations* with store type flags, we accommodate inconsistencies in store name and geographic information over time while retaining all necessary information to construct the choice set of authorized retailers for participants. Without this adaptation, we would have overestimated the number of retailer options available in a given period.

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