The Economics of Food Access

A DISSERTATION
SUBMITTED TO THE FACULTY OF THE GRADUATE SCHOOL
OF THE UNIVERSITY OF MINNESOTA
BY

Joel Cuffey

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF
Doctor of Philosophy

Elton Mykerezi

July, 2018

Acknowledgements

I would like to thank Tim Beatty for initially agreeing to work with me, and continuing to do so despite the distance between Minnesota and California. I would also like to thank Elton Mykerezi for all of his guidance on the dissertation and job search process. Additonal thanks goes to the rest of my committee for invaluable input: Marc Bellemare and Lisa Harnack. I would also like to acknowledge the essential help of Len Kne and the University of Minnesota U-Spatial center for access to ArcGIS, which made the first and second chapters possible.

I would like to especially thank Paul McNamara of the University of Illinois, who originally introduced me to the field of agricultural economics, and who remains a source of inspiration and role model.

Finally, I thank my parents for their support of me, Katie and the kids through difficulties of graduate school. Most importantly, I thank my wife Katie, whose love and patience made writing a dissertation possible.

Dedication

To Katie, Josiah, and Samara.

Abstract

Many U.S. food policies aim to improve access to food for low-income households by either increasing household resources or providing more places to spend resources on healthy foods. In my dissertation I investigate how low-income households respond to policies designed to improve food access. My first chapter explores how policy incentives influence consumer choice of food retail store format. In my second chapter, I pose and test an new explanation for the speed at which U.S. food assistance benefits are spent throughout the month. Finally, my last chapter measures the impact of a food assistance work requirement on labor market outcomes. Each chapter provides novel insights into how low-income households interact with policies to improve access to food.

Contents

List of Tables

List of Figures

Chapter 1

Introduction

Many U.S. food policies aim to improve access to food for low-income households. These policies generally either increase household resources - for example, the Supplemental Nutrition Assistance Program (SNAP) - or provide more places to spend existing resources - for example, the Healthy Food Financing Initiative. Since poor access to food can lead to poor diets and health problems such as obesity, diabetes, and high blood pressure, these policies play an important role in the U.S. social safey net. There is much, however, that remains unknown about how households respond to these policies. My dissertation therefore seeks to add to current understanding of how households are influenced by and interact with food access policies.

In my first essay, I measure the influence of two policy options on consumer choice of food retail store format. The two policy options I investigate are: (1) opening more grocery stores, and (2) giving households more resources. These are the two most commonly-suggested policies for encouraging households to shop more at grocery stores and less at convenience stores, which do not stock many healthy food options. Using the estimates of policy impacts, I am able to discuss under what conditions one policy option is preferred over another, and quantify the tradeoffs between policies.

My second essay asks to what extent transaction costs influence how quickly SNAP participants spend their benefits. SNAP participants on average spend most of their benefits within the first few days of the month, leaving little to use later in the month. Previous research has sought to understand why this pattern exists, and no satisfactory explanation has been found except for observing that SNAP spending behavior is consistent with the behavior we would expect from individuals with inconsistent time preferences. I ask whether difficulty in traveling to the grocery store can explain (some of) this phenomenon.

In my third essay, I examine how SNAP participants respond to a SNAP work requirement. Safety net programs such as SNAP provide clear benefits to participants and potential participants. However, a common worry is that generous safety net programs will discourage participants from working. Work requirements have therefore been implemented for SNAP as well as other programs, and have been suggested for other programs. Able-bodied adults without dependents (ABAWDs) are required to work in order to participate in SNAP for more than a few months. This work requirement can be waived in areas and during times of high unemployment. To quantify the impact of the work requirement, I take advantage of spatial and temporal variation in waivers to measure the impact of waiving the work requirement. I measure the impact of work requirement waivers on the labor force outcomes of ABAWDs.

Chapter 2

Consumer Choice of Store Format: Response to Policy Incentives

2.1 Introduction

Policies to address obesity in the U.S. often focus on encouraging households to shop for food at grocery stores instead of alternatives with less-healthy options, such as convenience stores. Previous research has shown that individuals in low-income neighborhoods are more likely to suffer from obesity and diet-related disease, and also often live further away from grocery stores, than those in higher-income neighborhoods (Black and Macinko, 2008). These assocations have driven the concern that low-income households rely on convenience stores because grocery stores are too far away. Since convenience stores stock less-healthy food than grocery stores, and purchases from convenience stores are on average less healthy than purchases from grocery stores,[1] this over-reliance on convenience stores relative to grocery stores is often cited as a driver of diet and health disparities.[2]

[1] For evidence on stocking behavior by store format, see for example Handbury *et al.* (2016), Glanz *et al.* (2007), Cannuscio *et al.* (2013), and Leone *et al.* (2011). Stern *et al.* (2016) and Volpe *et al.* (2017) report differences in purchase healthfulness by store format. We note that the evidence for a causal relationship between shopping at a store format and the healthfulness of purchases is limited.

[2] For overviews of the reseach on the relationship between diet or obesity and the food retail environment, see for example Gordon-Larsen (2014), Larson *et al.* (2009), Walker *et al.* (2010), and Caspi *et al.*

Most policies to encourage shopping at grocery stores in low-income neighborhoods have sought to increase the *supply* of grocery stores by providing targeted incentives for stores to locate in underserved areas.[3] Alternatively, scholars have suggested that policies to increase the *demand* for grocery store food might encourage grocery store shopping (Bitler and Haider, 2011; Andrews *et al.*, 2016). The extent to which supply-focused and demand-focused policies increase shopping at grocery stores relative to other store formats is an open question.

In this paper, we measure the short-term impact of a supply-side policy - increasing the supply of grocery stores - and a demand-side policy - increasing household food resources - on a household's choice of store format. We use novel Supplemental Nutrition Assistance Program (SNAP) administrative data that provide exact household locations and exact locations of available stores, as well as a record of a household's SNAP transactions over time. We leverage quasi-experimental variation in household proximity to a store format - via store openings and closings - and income - via nationwide SNAP benefit increases - to measure household response to supply *vs* demand policy proposals. Our results provide estimates of the sizes of the household response which allow us to contrast the impacts of supply and demand policies.

This paper is closest in spirit to two recent working papers that also measure the effect of grocery store openings on household shopping behavior. Handbury *et al.* (2017) and Allcott *et al.* (2017) estimate the effect of multiple grocery store openings using Nielsen scanner data and information on store locations over time.[4] Handbury *et al.* (2017) find that a greater concentration of stores near a household's census tract centroid increases the nutritional quality of food purchases for wealthier households but not for lower-income households, calling into question the impact of supply-side policies on the diets of low-income households. Allcott *et al.* (2017) find that most of the expenditures at supermarkets that open close to a household's census tract centroid are diverted from other grocers, and that store entry has no impact on purchase healthfulness. Given the small impact of access on outcomes, Handbury *et al.* (2017) and Allcott *et al.* (2017)

(2012).

[3]At the federal level, the primary legislation to improve the supply of grocery stores is the Healthy Food Financing Initiate. Examples of state and local policies are the Pennsylvania Fresh Food Financing Initiative (2004-2010) and the New York City Food Retail Expansion to Support Health program.

[4]Handbury *et al.* (2017) use store location data from the Nielsen TDLinx dataset, and Allcott *et al.* (2017) collect opening dates and locations of new supermarkets from a number of specific chains.

attribute differences in shopping behavior primarily to preferences rather than time or income constraints that limit access to grocery stores.

We extend Handbury *et al.*'s (2017) and Allcott *et al.*'s (2017) analysis by using a comprehensive dataset on a policy-relevant low-income population as well as looking at an alternative policy to potentially increase grocery store purchases. The Nielsen data used by Handbury *et al.* and Allcott *et al.* are known to under-represent low-income households,[5] which is the population that we would expect this policy to have the largest influence on as well as the population for which these policies are designed. It is therefore unclear whether the small impact found by Handbury *et al.* and Allcott *et al.* reflects the response of low-income populations more generally, or is an artefact of the Nielsen dataset. Our data allow us to test this by looking specifically at SNAP households, which are by definition low-income. In addition to providing information on a low-income population, our datset has other benefits relative to the Nielsen data that make it particularly well-suited for examing household response to the food retail environment. With exact household and store locations, our data allow us to measure household proximity to store formats with unprecedented detail - the Nielsen data that Handbury *et al.* (2017) and Allcott *et al.* (2017) use only allow identification of households at the census tract level. Measurement error introduced by locating households at the census tract centroid could be expected to attenuate any potential impacts, and could be another reason for the small impacts found by Handbury *et al.* (2017) and Allcott *et al.* (2017). Furthermore, our data provides information on each SNAP transaction made by each household, whereas the Neilsen data have documented underreporting of purchases and shopping trips (Einav *et al.*, 2010) which could be worse for certain demographics (Zhen *et al.*, 2009). Finally, we have information on SNAP transactions made at - and store availability of - non-traditional store formats such as corner stores, gas-marts, pharmacies, and small ethnic grocers, which are not as well captured in national store databases. In addition to a dataset that has multiple benefits over the Nielsen data, we leverage our purchase information over time for SNAP households to compare the effect

[5]Handbury *et al.* (2016), a previous version of their 2017 working paper, acknowledge this. They show in both versions that their results are robust to the *exclusion* of food assistance-eligible households. Allcott *et al.* (2017) show that the impact of grocery store openings is larger for low-income households in the Nielsen dataset, though not large enough to change their conclusions.

of opening a grocery store with the impact from increasing SNAP benefits.[6]

A few other papers in the public health literature have sought to use quasi-experimental methods to examine the impact of opening grocery stores on household shopping behavior. Cummins *et al.* (2014) and Elbel *et al.* (2015) measure the effect of two separate grocery store openings, and in results echoing Handbury *et al.* (2017) and Allcott *et al.* (2017) find no impact on diets even among households that use the new store. We build on these studies by using all store openings within a city over a two-year period, allowing store openings to have an impact beyond an arbitrarily-defined neighborhood, and allowing the impacts *within* neighborhoods to vary.

Little is known about the impact of increasing resources on household store format choice. Conceptually, households trade off convenience, variety, and prices when deciding where to shop, and the net effect of more resources on the choice of store format is unclear. Andrews *et al.* (2013) use a similar identification strategy as ours along with county-level SNAP redemption data. Their results are ambiguous, which they attribute to using aggregated data. We are able to directly observe household response to increasing SNAP benefits rather than inferring it from county-level data.

We find that opening a grocery store near a household increases the share of expenditures at grocery stores, and that the impact varies by distance to the grocery store and household car ownership. The effect strongly depends on car ownership: households with cars are responsive to grocery store openings at greater distances, while the impact on households without cars is concentrated on those closer to the new store. On average, a grocery store opening within 0.5 miles of a household increases the share of expenditures at grocery stores by 4 percentage points for households with cars and 8 percentage points for households without cars. Previous research has highlighted the relationship between car ownership and employment (Baum, 2009; Gurley and Bruce, 2005), but to our knowledge we are the first to illustrate the importance of car ownership on the use of public assistance benefits and food shopping behavior.[7] When a new grocery store opens, all households reduce shopping at small ethnic stores, and households with cars also

[6]Cleary *et al.* (2018) use results from a structural model of grocery store entry to simulate the impact of a demand-side and supply-side policy to improve access to grocery stores, which they then compare. They are, however, unable to observe actual grocery store openings or resource increases.

[7]Inagami *et al.* (2009) let car ownership moderate the association between restaurant concentration and body mass index, but do not look at household shopping *per se*.

reduce shopping at discount stores. Furthermore, in response to a grocery store opening households without cars shop somewhat less at convenience stores, while households with cars may actually increase shoppping at convenience stores.

When households receive increased resources, they increase the share of expenditures at grocery stores and correspondingly decrease the share at convenience stores. This impact is largest for households without cars. Most of the extra resources are spent at grocery stores, which is consistent with the fact that households spend most of their SNAP benefits at grocrey stores overall. When we compare the percent of extra resources spent at each store format with the average budget shares for each format, we find that while households without cars spend the extra resources as we would expect, car owners spend much less at grocery stores and much more at small ethnic grocers than we would expect.

Given estimates of household responses to both demand- and supply-side policies, we are able to directly compare these policy directions. Our results suggest that demand-side policies unambiguously shift relative spending away from convenience stores and towards grocery stores, but total spending would increase at convenience stores for households without cars and car owners spend more than expected at ethnic stores. Finally, we are also able to use our results to quantify the amount of extra resources that a household would need to receive in order to have the same impact on grocery store shopping as opening a new store. To have the same impact on the share of expenditures at grocery stores as opening a grocery store within 0.5 miles of a household, we would have to give households without cars an extra $381 and households with cars $1,300 per month. To have the same impact on total grocery store expenditures, however, we would have to give households without cars $14.3 and car owners $19.5.

The rest of our paper is organized as follows. We start by developing a theoretical model that provides intuition regarding the choices faced by households which also motivates our empirical analysis. We then describe the unique features of our administrative SNAP dataset and how we use it to examine the impact of both changing access and resources on household shopping behavior. This empirical framework as well as the results of improving access and resources follow. We end with a discussion of the implications for policy as well as limitations and directions for future work.

2.2 Theoretical model

2.2.1 Setup

We now propose a simple theoretical model to illustrate the stylized choice faced by a household choosing between a grocery store far away that stocks both unhealthy and healthy foods, and a nearby convenience store that stocks only unhealthy food. We assume the household solves the problem in two stages: In the second stage the household decides how much to buy conditional on store choice, and in the first stage the household decides where to shop. This model extends Hausman and Leibtag (2007) and Anas (2006) by explicitly examining factors influencing the store choice.

The household receives utility from a healthy food, "kale" (k), unhealthy food, "candy bars" (b), leisure time t_l, and an additive stochastic term, and has a one-period food budget Y to spend on either kale or candy bars. The household chooses to visit one of two stores: a convenience store located t_c minutes away, or a grocery store located t_g minutes away. Following the usual stylized facts of food deserts, the grocery store is further away than the convenience store ($t_g > t_c$). The convenience store sells only candy bars at price p_{cb}, while the grocery store sells candy bars at p_{gb} and kale at p_{gk}. Finally, the household has time allocation T to split between leisure time and grocery shopping, such that $t_l = T - t_c$ or $t_l = T - t_g$.[8] We assume there is only enough time in this period to shop at one of the stores ($t_c < t_g < T < 2t_c$), so the model reduces to a discrete choice between visiting the convenience store or the grocery store.

In the second stage, once the household has chosen a store to visit, the household maximizes utility[9] subject to the store-specific budget constraint. The household chooses in the first stage the store that will give it the greatest (indirect) utility. With this setup, we show in Appendix A.1 the following implications regarding the effects of demand- and supply-focused policies to encourage households to visit grocery stores:

Implication 1: When a grocery store opens closer to the household ($t'_g < t_g$), the household is more likely to visit the grocery store than before the new store opened.

Implication 2: When the household's food budget increases ($Y + \Delta Y > Y$), the impact on the probability of visiting the grocery store *vs.* the convenience store depends on

[8] We assume that labor supply decisions, and therefore the allocations T and Y, are fixed in this single period.

[9] For notational convenience, in the lower stage we suppress the additive stochastic term.

what the household does with the extra money ΔY, and the net impact is ambiguous. To be more concrete, we can define the opportunity cost of going to the grocery store given ΔY as the utility from the extra candy bars that the household would have bought at the convenience store relative to the grocery store, and the opportunity cost of going to the convenience store as the utility from the extra kale that could be bought using ΔY. The household will be more likely to go to the grocery store if the opportunity cost of going to the grocery store is less than the opportunity cost of going to the convenience store. However, if the household would use ΔY to buy more candy bars at the grocery store than they would at the convenience store, receiving the extra resources will unambiguously increase the probability of visiting the grocery store. This could be the case, for example, if convenience store food is significantly more expensive than the equivalent grocery store food. Previous studies have found that convenience store food is at least 10 percent (Broda *et al.*, 2009) and up to 50 percent (Caspi *et al.*, 2017) more expensive than comparable grocery store food, suggesting that this is a real possibility.

2.3 Data

2.3.1 Dataset

We use administrative data containing the universe of SNAP transactions for all SNAP participants in the Minneapolis-St. Paul metropolitan area for the period October 2007 - September 2010. Our data consist of 133,548,882 unique transactions from 71,256 households. For each transaction we observe the amount, the date, and the store name and address. We also observe monthly information on each household, including household age and racial composition, car ownership, and the household's street address. In order to describe each household's food retail environment, we geocode each household and store address.[10,11] For tractability we collapse these transaction-level data to a monthly[12] summary of transactions for each household. These data allow us to describe

[10]97 percent of household addresses are geocodable. The ungeocodable addresses are homeless households with no address, and addresses with mistakes. Due to the number of addresses, we do not correct all household address mistakes. A few households report as their address their county SNAP office. We compare addresses to a list of county offices and treat such households as ungeocodable.

[11]100 percent of the store addresses within a two-county buffer of our metropolitan area are geocodable.

[12]What we refer to as "month" throughout this paper is the household's benefit month, defined as the period of time between the first benefit receipt in a calendar month and the first benefit receipt in the

both the household's store format choices as well as the set of stores to which a household had access. The average household is in our data for 12 months and makes seven transactions per benefit month.

For our regression analyses our initial household-month sample size is 1,910,437. We drop from the sample households who ever had an ungeocodable address or multiple addresses in one month (806,202 household-months).[13] To restrict our analysis to urban settings, we further drop from our sample households who lived in rural census tracts at any point (61,283 household-months). A few households had conflicting data on the number of household members,[14] and are excluded from our final sample as well (1,453 household-months). Given uncertainty as to the validity of the given address, we also take out of our sample households that have a homeless household member at some point during the time that they are observed in our data (60,120 household-months). Key to part of our identification strategy,[15] we also exclude household-months with missing data for our constructed census-tract level variables (65,120 household-months) and households that moved residences during the period that we observe them (228,062 household-months). Finally, to mitigate endogenous selection into samples defined over car ownership, we also exclude households whose car ownership status changed at some point (67,575 household-months).[16] Our final total household-month sample size is 620,622, to which we merge summary statistics of each household-month's transactions.

An important first step is to use the store name and location to define store formats. We choose a scheme that enables us to distinguish between formats with different expected assortment of healthy foods, based on the results of previous studies, without requiring detailed data on store size and stocking. *Convenience stores*, the format least likely to stock healthy food options such as fresh fruits and vegetables, include corner

next calendar month. This definition allows for a standard "month" definition for months in which a household receives benefits more than once, as well as months in which a household's benefit receipt schedule is different from the previous or next month. Both of these situations happen for administrative reasons. In our analysis we control for the length of the benefit month as well as the number of benefit issuances in that benefit month.

[13]Multiple address listings can occur when a household moves and each address is valid at some point during the month in which the move occurs.

[14]Specifically, the number of eligible household members did not match the number of members of the household in the demographic data.

[15]See Section 2.4.2.

[16]A change in car ownership status is defined as switching from owning any car to not owning any car, or *vice versa*.

stores, gas-marts, pharmacies, and dollar stores. This definition matches that of Stern *et al.* (2016), Caspi *et al.* (2016), and Cannuscio *et al.* (2013), all of whom find that stocking and purchases at these stores are significantly less healthy than at other stores. *Grocery stores* are more likely to stock healthier food options, and include all grocery stores, superstores, supercenters, mass merchandisers, and club stores.[17] The grocery store format is most likely to stock healthier food options (Glanz *et al.*, 2007; Cannuscio *et al.*, 2013; Leone *et al.*, 2011; Handbury *et al.*, 2016) and shopping at grocery stores is associated with healthier purchases (Stern *et al.*, 2016; Volpe *et al.*, 2017). *Discount stores* consist of limited assortment stores, primarily Aldi. We separate these stores from grocery stores because they offer a narrower assortment of all foods, including healthy foods, than grocery stores, and there is some indication that they offer less healthy foods on average than more traditional grocery store formats (Black *et al.*, 2014). We classify any Hispanic, Asian, or East African store or market as *Ethnic stores*. Ethnic stores often have a mixture of grocery store and convenience store properties: they are often smaller than grocery stores, and have more limited assortment, but may also sell fresh fruits and vegetables. All other stores such as cooperatives, natural food stores, butchers, bakeries, and farmers markets are classified as *Other*. Food purchased from Ethnic and Other store formats may be as healthy as purchases from grocery stores (Stern *et al.*, 2016).

We also use the transaction data to define the availability of each store to SNAP participants over time.[18] One source of concern is that the universe of SNAP authorized stores may not adequately characterize the food retail environment. While SNAP store availability is important for our outcomes describing how households spend SNAP benefits, there may be other stores not authorized to accept SNAP that should be considered part of the household retail food environment. We do not include non-SNAP stores in our analysis, but we can characterize the share of total stores that we observe as SNAP stores. In Appendix A.3 we compare the counts of SNAP-authorized stores to counts from the County Business Patterns for the 11 Minnesota counties covered by

[17]While mass merchandisers may stock less healthy foods than grocery stores, it was often difficult in our data to distinguish between mass merchandisers and supercenters since most mass merchandise stores have been converted into supercenters. We do not have the necessary information on the date of conversion for each store.

[18]See Appendix A.3 for more information on how we define store availability.

our data. SNAP stores account for between 80 to 100 percent of the total establishment counts in the most urban counties, where most of our SNAP participants live. Caspi *et al.* (2015) survey small, non-traditional stores including corner stores, gas-marts, pharmacies and dollar stores in the Minneapolis-St Paul metropolitan area, and report that between 70 percent (corner stores) and 100 percent (dollar stores) accept SNAP benefits. If these non-traditional stores are less likely to accept SNAP benefits than more traditional store formats, we would expect at least 70 percent of total stores would be authorized to accept SNAP.

2.3.2 Grocery store openings

In order to quantify the impact of store openings on households, we require a time-varying measure of each household's food retail environment. We choose two complementary methods to define household proximity to stores of a particular format. First, we leverage knowledge of the precise location of each household and store to specify a series of overlapping indicators for whether a household has a store within 0.5 ($D05$), 1 ($D10$), and 1.5 ($D15$) miles. The 0.5 and 1 mile indicators follow the access definitions used by the USDA Food Access Research Atlas data for urban areas. Figure **??** illustrates the indicators for households relative to a grocery store opening. The indicators split households into four types, which we label households A-D. Household A is within 0.5 miles of the grocery store opening, and each indicator is equal to one. Household B is within 1 mile, but not within 0.5 miles, of the opening, and so $D05 = 0$ while $D10 = 1$ and $D15 = 1$. Similarly, household C is within 1.5 miles but not within 1 mile (or 0.5 miles), so $D05 = 0$, $D10 = 0$ and $D15 = 1$. Finally, household D is further than 1.5 miles, so all of the indicators are equal to zero.

While the indicators allow us to observe the impact of access to stores within 1.5 miles of a store opening, households often do not shop at the closest grocery store (Ver Ploeg *et al.*, 2015) and so the food retail environment within 1.5 miles may be a poor measure of household access to stores. We therefore examine the robustness of our results using a continuous measure of access. For the continuous measure, we follow common measures in the accessibility literature (Bhat *et al.*, 2000) and define a distance-weighted index of the 20 closest stores of a particular format:

$$I_{htf} = \sum_{s=1}^{20} \frac{1}{d_{hs}^{\theta}}$$

where d_{hs} is the Euclidean distance[19] between household h and store s. Our baseline value for θ is 1/2.[20]

For most households in our data, the grocery store format index varies between 6-16, with higher values indicating greater store concentrations. The mean grocery store concentration index is 12, and the mean convenience store index is 17, showing that on average households have a greater concentration of convenience stores close to their house. The average grocery store index in food desert tracts[21] is 11.7, and the average grocery store index in non-food desert tracts is 12.2, so consistent with our expectation households in food deserts have lower grocery store concentrations. The average convenience store index in food desert tracts is 17.3, while the average convenience store index in non-food desert tracts is 17.7. Thus households in food deserts also on average experience lower concentrations of convenience stores in the vicinity of the house.

To illustrate the relationships between the two measures, Figure 2.1 shows a nonparametric regression of the grocery store index on the distance to the nearest grocery store. Vertical lines on the figure show distances of 0.5, 1, and 1.5 miles to the closest grocery store, which correspond to the distances we use to define our overlapping indicators. The difference between having a grocery store located immediately adjacent and having the closest grocery store located 0.5 miles away is 4: the highest value of the concentration index is 16, and is 12 for households 0.5 miles away from the closest grocery store. Between 0.5 and 1, and 1 and 1.5, the concentration index experiences smaller decreases that persist through 4 miles.

[19]While Euclidean distance is not necessarily the same as travel time, in our data they are practically identical: the correlation coefficient between Euclidean distance and travel time as calculated by ArcGIS is 0.98.

[20]We also check robustness to values of θ and other definitions of the kernel in Appendix **??**. Handbury *et al.*, for example, use a Gaussian kernel to define an index of accessibility to grocery stores. Our index definition is more robust to the choice of parameters than the Gaussian index, as seen in Appendix A.4. Khuang (2017) provides an example of our preferred index definition from the economics literature.

[21]Low-income, low-access tracts as defined by the USDA ERS.

2.3.3 Increasing resources

We require a change in household SNAP resources to measure the impact of increasing household resources. We leverage two national SNAP policy changes which increased benefits for all households. The first policy change during the period of our data was a cost-of-living (COL) adjustment, which occured in October 2008. The second and change largest occured on April 1, 2009, when the maximum SNAP benefit available increased by 13.6 percent due to a provision of the American Recovery and Reinvestment Act (ARRA). The ARRA was passed by Congress in response to the recession, and the goals of the SNAP portion of the ARRA were to improve the food security of low-income households and stimulate the economy (USDA, 2010).[22] Figure 2.2 shows average benefit levels between October 2007 and September 2010 for our dataset, along with the 5$^{\text{th}}$ and 95$^{\text{th}}$ percentiles. The dotted black lines delineate October 2008 and April 2009. Average benefit levels jumped by about \$40 per household due to the ARRA, and about half as much for the COL adjustment.

Using information on each household's current benefit level as well as their household size and income, we define the variable $Benefit\,Increase$ as the actual benefits received in a month less the benefits that the household would have received under pre-COL SNAP policy. *Benefit Increase* therefore measures the increase in benefits attributable solely to policy changes and not to changes in the household's income or size.

2.3.4 Shopping at formats

To describe a household's choice of store format we use two outcome measures. First, we construct the percent of monthly SNAP expenditures that a household redeems at each store format.[23] Since total spending at each format can increase with a boost in resources, our secondary outcome measure is the total amount of expenditures at each store format.

[22] Prior to the ARRA, cost-of-living adjustments occured annually. After the ARRA, cost-of-living adjustments were suspended for the remainder of the period of our data.

[23] While we could use either the share of trips or share of expenditures at a store format as our dependent variable, we focus on share of expenditures to facilitate comparisons with previous studies. Our results are robust to using trips shares.

2.3.5 Summary statistics

Table 2.1 reports summary statistics on household demographics, store concentration, and the household's census tract of residence for household-months greater than 1.5 miles, between 1 and 1.5 miles, between 0.5 and 1 mile, and less than 0.5 miles from the nearest grocery store.[24] Households closer to grocery stores are on average slightly smaller and have lower incomes,[25] though the relationship with income is non-monotonic - households within 0.5 miles of a grocery store have higher incomes than those just over 0.5 miles.

Table 2.2 displays average outcomes for the same samples of households by distance to the nearest grocery store. Households generally redeem between 60 and 70 percent of monthly SNAP expenditures at grocery stores. Within 1.5 miles of a grocery store, households closest to the grocery store redeem relatively more at grocery stores. Outside of a 1.5 mile radius of a grocery store, households redeem almost as high a percent at grocery stores as those within 0.5 miles. We observe the opposite pattern for convenience, discount, and ethnic store format expenditure shares: Households over 1.5 miles from a grocery store redeem relatively smaller amounts at each format, while within 1.5 miles of a grocery store, the format SNAP expenditure share is larger and decreases over distance to the grocery store. Although within 1.5 miles of a grocery store the percent of SNAP expenditures at grocery stores substantially increases, the total amount of SNAP expenditures does not experience much increase. Households within 1.5 miles of a grocery store on average redeem over $20 less at grocery stores than households further from grocery stores. Much of this discrepancy is made up by households within 1.5 miles of a grocery store redeeming more at ethnic stores.

[24] To facilitate comparison we make the distances mutually exclusive in Table 2.1, even though our indicator measures of store proximity are overlapping and not mutually exclusive.

[25] While our data do not include household income directly, we are able to infer income from its authorized benefit amount. This amount is a function of the maximum benefit level and the household's monthly income net of SNAP deductions. Using the standard SNAP benefit formula and information on the maximum benefit over time from the Food and Nutrition Service, we calculate the household's net income. About 29% of our sample has no net income; households with some income receive on average $538 in monthly net income. Note that this amount is net of allowable SNAP deductions. We do not have information on specific deductions, so we are unable to calculate the household's gross income which would be substantially higher than the net income.

2.4 Empirical strategy

2.4.1 General empirical strategy

Our primary specification models household h's shopping outcome for format f in month t as a function of access to each store format $\sum_{f=1}^{5} A_{htf}$, the increase in benefits due to policy changes $Benefit Increase_{ht}$, time-varying household demographics Z_{ht}, month-year fixed effects τ_t, and household fixed effects ϵ_h:

$$s_{htf} = \sum_{f=1}^{5} A_{htf} + \delta \text{Benefit Increase}_{ht} + \theta Z_{ht} + \tau_t + \epsilon_h + \varepsilon_{ht}. \tag{2.1}$$

As explained in Section 2.3.2, access A_{htf} is defined in two ways:

$$A_{htf} = \eta_f I_{htf} \tag{2.2}$$

where I_{htf} is the continuous concentration index as defined in Section **??**, or the series of discrete indicators

$$A_{htf} = \alpha_f D05_{htf} + \beta_f D10_{htf} + \gamma_f D15_{htf} \tag{2.3}$$

where $D05$, $D10$, and $D15$ are the indicators as defined in Section **??**. Since the indicators are not mutually exclusive, the total impact of having a store within 0.5 miles (relative to having a store greater than 1.5 miles away) is $\alpha + \beta + \gamma$.

Z_{ht} includes controls for household size, household racial composition (black, white, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, household vehicle ownership (whether the household owns one car/motorcycle and whether the household owns more than one car/motorcycle), income, the household's monthly benefit amount, the length of the benefit month, the number of times in the benefit month the household was issued benefits, and the cumulative number of consecutive benefit months that we observe the household receiving SNAP benefits.[26]

[26]The number of consecutive months is therefore specific to each spell that the household is on SNAP. For households with one spell on SNAP, the number of consecutive months is just the total number of

Given the potential importance of car ownership in decreasing the time needed to travel to a store, we present results split by household car ownership.

2.4.2 Identification of access coefficients

To measure the causal impact of increasing access to grocery stores and increasing resources, our coefficients of interest are the grocery store access coefficients η or α, β, and γ, and the benefit increase coefficient δ. The ideal experiment would have grocery stores open in random months and neighborhoods throughout our city and sample period. This is clearly infeasible, so we rely on actual store openings and closings to estimate the impact of grocery store access. Household-level variation in grocery store supply can theoretically come from two sources: changes in household location, and grocery store openings and closings. As noted in Section dataset, we exclude households that move to remove the possibility of endogenous residential sorting, so the only variation in access comes from changes in the locations of grocery stores. Identification of store access parameters η (or α, β, and γ) requires the assumption that grocery store location decisions are unrelated to changes household decisions on where to purchase food, conditional on household fixed effects and the other controls. This assumption could be violated in three major ways.

First, grocery stores could explicitly locate near households that are *already* increasing their shopping at grocery stores. Conditioning on household fixed effects, grocery store location decisions would have to be made in reference to *specific* households. Since this is unlikely to be the case, we argue that grocery store location decisions are plausibly not determined by changes in shopping behavior, conditional on household fixed effects. A related problem that is separate from the identification of our access parameters arises if stores locate near households that are more likely to change their shopping *in the future*. While not affecting the identification of access changes, this would limit the external validity of our results. We do not address this issue directly, but note that this is primarily a concern if households are more likely to change their behavior due to unobserved factors, which we discuss next.

months the household has been receiving SNAP up until that month. Households with more than one spell on SNAP during our period will have more than one "first" consecutive months. This variable, then, captures breaks in SNAP benefit receipt.

The second way that our identifying assumption could be violated is if there are unobserved factors influencing both changes in household shopping as well as grocery store openings or closings. These unobserved factors could cause either household location changes relative to grocery stores, or grocery store location changes relative to household. Restricting models to households that do not move removes any variation in household location that could be due to unobserved factors that also influence changes in shopping behavior. More problematic is the possibility that stores could choose to locate or close in areas where household food preferences are changing in specific ways. Grocery stores may observe these changing shopping preferences and decide to locate in that neighborhood. We address this potential threat to identification in a few ways. Including household fixed effects as well as time-varying household characteristics plausibly controls for many demographic characteristics that influence store location decisions. We also include proxies for unobserved preference changes in the vicinity of the household: one- and three-month lagged average store format expenditure shares for the household's census tract,[27] the average income of SNAP participants in the tract, and the percentage of SNAP participants in the tract.[28]

The final strategy to address unobserved factors utilizes the small variation in distance to the nearest grocery store given by the indicators $D05$, $D10$, and $D15$. This strategy of taking advantage of small variation in distances is similar to the one employed by Currie *et al.* (2010) to identify the impact of fast food restaurants on obesity. Since the indicators $D05$, $D10$, and $D15$ are overlapping, in Equation 2.1 each coefficient measures the *additional* impact relative to the next-smallest distance. Identification of these variables rests on the assumption that the exact location of grocery stores *within 1.5 miles* of a household is determined by factors unrelated to changing household shopping behavior. That is, within 1.5 miles of a household, the location of a grocery store is determined by available land or infrastructure, the location of which is unrelated to that household's shopping decisions. This assumption allows stores to strategically locate to neighorhoods but requires that the location of the stores *within neighborhoods* is (conditionally) as good

[27] The average tract shares for household i are calculated for all households $j \neq i$ in the household's census tract so that the dependent variable is mechanically independent from the average tract shares. We exclude from our sample households that are the only SNAP participants in their tract.

[28] The percentage of SNAP participants in a tract is the total number of participants from our demographic data that live in the tract, divided by the 2010 Census tract population.

as random. One can think of two households in a particular neighborhood: Household A is within 0.5 miles of the new store opening and Household B is 1 mile away. Our assumption allows the new grocery store to locate to this neighborhood (approximated by the figure's frame) based on changing neighborhood characteristics, but the reason that the store opens closer to household A than household B has nothing to do with the differences between household A's and B's unobserved characteristics. Instead, the store locates closer to household A because for example that land was for sale at the right time. Our assumption is that the timing and location of land coming up for sale in a particular neighborhood is unrelated to differences between household A and household B. Thus when we compare households A and B, the only difference between them is their distances to the grocery store, not unobserved differences which would contaminate any comparison. Note that our use of this identification assumption relies on our detailed geographic data, and is perhaps less plausible for studies that measure access at a census tract level or higher. While this assumption is intuitively plausible, and we report tests of this assumption below, we also acknowledge that this strategy utilizes variation in small distances around a grocery store and so might be a poor description of a household's food retail environment. Analyzing results from both the index and the indicators will give a robust and comprehensive view of the impact of geographic access on household shopping behavior, though we acknowledge that our case for the causal identification of the indicators is stronger.

One final threat to identifying the access coefficients is systematic measurement error. We believe that this is not a concern in our data due to our detailed location information and our data processing procedure. The distance measures are based on detailed coordinates derived from household and store addresses, so the only significant measurement error would be any misreporting of addresses. Households addresses are used for communications with the SNAP program, so there is a strong disincentive to misreport addresses. Further, as described in Section 2.3.1, we exclude households from our sample for whom there is ambiguity as to the validity of their address, i.e. households who have had more than two addresses listed in a month and households that ever had a homeless member are dropped. Stores within the state are required by law to report their addresses to the program, creating a disincentive for stores to misreport addresses. In addition, we performed online searches for most of the stores within a two-county buffer of our metropolitan area of interest and were able to confirm their locations. Finally, as mentioned above, we were able to successfully geocode all of the store addresses within the two-county buffer.

2.4.3 Identification of benefit increase coefficients

Identification of δ requires assuming that the timing and amount of the benefit changes is not correlated with changes in trip shares, conditional on the observables. The first potential threat to our identification of δ would result from changing household shopping behavior directly causing benefit increases. Since the cost-of-living and ARRA benefit increases were implemented as a national SNAP policy, changing preferences or shopping patterns of any individual household had no impact on the timing of the increase. Further, the benefit increases were not given in order to influence household store format choice. The ARRA benefit increase was intended to counteract the effects of the recession on low-income households by improving food security and stimulating the economy (USDA, 2010). The cost-of-living adjustment in October, 2008 was part of pre-ARRA annual SNAP policy to adjust for increases in the cost of a set basket of foods. Reverse causality seems unlikely to be a concern in estimating the impact of benefit increases on store format choice.

The second threat is that there may be unobserved factors that influenced the implementation of a benefit increase as well as influencing household shopping patterns. One set of unobserved factors is other policy changes. There were two state SNAP policy changes around the same time as the ARRA. In March 2009, the state introduced six-month reporting, requiring households with earned income to report their earnings only every six months instead of monthly. In January 2009, able-bodied adults without dependents were waived from the work requirement, leading to an increase in the SNAP caseload. Both policy changes made it plausibly easier for a household to obtain or stay on SNAP. Within-household, though, it seems unlikely that they changed the incentives for visiting a grocery store relative to other store formats.

We finally note that our data come from the period of the recession. To control for differential impacts over time for various (observable) groups, in addition to household demographic controls that account for any changing household composition due to the recession, we include interactions between our control variables and a linear time trend.

While this arguably mitigates some confounding influence of the recession, the time period may limit the applicability of our conclusions to non-recessionary periods.

2.5 Results

2.5.1 Opening grocery stores

Descriptive

We observe 22 instances of a grocery store opening, and 6 instances of a grocery store closing. To begin, we examine the impact of changing the supply of grocery stores by looking at a simple before-after comparison. To make this comparison, we restrict our sample to households that gained a grocery store within one mile or within one half mile of their location (and that had no grocery stores within the relevant distance beforehand). We look at the average trip shares for the three months before and six months after the store opening.[29] 253 households met these requirements for a grocery store opening within one mile, and 66 households met these requirements for a grocery store opening within 0.5 miles. Figure 2.3 shows the average expenditure shares for households with a grocery store opening within one mile and Figure 2.4 shows the average expenditure shares for households that experienced a grocery store opening within 0.5 miles. After the grocery store opening, expenditure shares at grocery stores increased by 5-10 percentage points, and shares at ethnic stores most notably decreased.

Impact of grocery store access on store format expenditure shares

Table 2.3 shows the results from using Equation 2.1 to measure the impact of changing grocery store access on expenditure shares to all store formats. Panel A uses the concentration indices to define household proximity to store formats. In Panel B, we repeat the regressions from Panel A using a census tract centroid-based concentration

[29] We also require that households have transaction data for each of the three months before and 6 months after store opening, and that the household is not within three months of another store opening or closing within the relevant distance.

index instead of a household address-based index.[30] The difference between the results in Panel A and Panel B therefore illustrate the attenuation bias that arises from spatial mismeasurement. Panel C of Table 2.3 presents the corresponding results using the indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles. Each of the coefficients represents the impact on grocery store trip share relative to the next largest distance. The cumulative effect of having a grocery store locate within 0.5 miles of a household is shown below the indicators, along with the F-statistic of the test for no cumulative impact.

An increase in proximity to grocery stores increases the share of expenditures at grocery stores in all specifications. There is evidence of significant attenuation bias for the impact on households without cars when using tract-level instead of household-level concentration indices: The coefficient on the grocery store and convenience store indices decrease in absolute value by almost half when the location of households without cars is approximated by the census tract centroid. The overall impacts of grocery store access using the concentration indices, however, are modest: Moving a household from the average food desert tract to the average non-food desert tract increases the share of trips to grocery stores by only 0.4 percentage points for households with cars and 0.75 percentage points for households without cars. The indicators show that households without cars are increasingly more responsive to a store opening within 1.5 miles as the household gets closer to the store. Households within 0.5 miles of a store opening increase their share of expenditures at grocery stores by almost 6 percentage points relative to all households within 1 mile of the grocery store opening. Similarly, households within 1 mile of a grocery store opening increase their share of expenditures at grocery stores by 2.4 percentage points relative to households just within 1.5 miles from the grocery store opening. This implies a cumulative impact of a grocery store opening for households without cars of 8 percentage points. Households with cars are not similarly impacted by getting closer to a store opening within 1.5 miles, although overall households within 1.5 miles of a store opening do increase their share of expenditures at grocery stores by 2.1

[30] The centroid-based index is defined as

$$I_{ctf} = \sum_{s=1}^{20} \frac{1}{d_{cs}^{\theta}}$$

where d_{cs} is the Euclidean distance between the household's tract centroid c and store s.

percentage points relative to households over 1.5 miles away from the store.

In response to a grocery store opening, convenience store shopping decreases. The coefficient on the household grocery store index suggests that when considering the broader food retail environment most of the increase in grocery store expenditures comes at the expense of convenience store expenditures. The indicators tell a more complicated story. Within 1.5 miles of a grocery store opening, households without cars substitute away from convenience stores increasingly more. For example, households without cars within 0.5 miles of a store opening thus shop less at convenience stores than households just within 1 mile by 2.7 percentage points. However, households within 1.5 miles overall actually *increase* shopping at convenience stores by relative to households greater than 1.5 miles away. Cumulatively, there is little impact of a grocery store opening on convenience store shopping at 0.5 miles from the grocery store. Instead, the largest decrease in format shares comes from ethnic stores. All households within 1.5 miles of the grocery store shop less at ethnic stores by 2-3 percentage points relative to households over 1.5 miles away, and the impact gets larger as the household gets closer to the grocrey store. The indicators also provide some evidence that a grocery store opening takes away from discount store expenditures for households with cars.

We can compare these results to those of Allcott *et al.* (2017), who find that households with an income under $25,000 increase grocery store expenditure share by approximately 1 percentage point when a store opens within 10 minutes of the household. Our grocery store concentration index results broadly support Allcott *et al.*'s finding, and our indicator results for households with cars could be interpreted as close to Allcott *et al.*'s. Households without cars, however, cumulatively increase their grocery store expenditure share by 8 percentage points when a grocery store opens within 0.5 miles, which is substantially larger than Allcott *et al.*'s result. Since two-thirds of our urban population of SNAP participants do not own cars,[31] their behavior could substantially influence the effectiveness of food policy designed for urban areas. In addition, households without cars are twice as likely to be elderly[32] and almost twice as likely to be the only

[31] 63 percent of the overall sample do not own cars. See Table 2.1 for this over distance to grocery store.

[32] 15 percent of households without cars have someone in the households over 65, compared to 8 percent of car owners.

person in the household,[33] suggesting they may face additional constraints to accessing healthy food.

2.5.2 Impact of grocery store access on store format SNAP redemptions

In this section we present the results from using total SNAP expenditures at each store format as the outcome in Equation 2.1 (Table 2.4). Households with cars increase redemptions at grocery stores due to a greater supply of grocery stores - the concentration index coefficient implies that moving from the average food desert to average non-food desert tract would increase household SNAP expenditures at grocery stores by $1.6 per month on average. Households with cars that live within 1.5 miles spend over $6 more at grocery stores due to a grocery store opening. As seen in the previous section, discount store and ethnic store SNAP expenditures drop as a result of a grocery store opening. There is weak evidence that households with cars actually increase the amount spent at convenience stores due to a grocery store opening. Households without cars also spend more at grocery stores - within 0.5 miles, they spend cumulatively over $8 more at grocery stores. For households without cars within 1.5 miles of the grocery store opening, this comes at the expense primarily of ethnic stores.

Placebo tests

We test our identifying assumptions underpinning the grocery store access analysis in three ways displayed in Table 2.5. First, we ask whether our grocery store access measures predict household characteristics, conditional on household fixed effects and our time-varying variables. If our measures are not conditionally correlated with unobservables, we would expect that they would not predict household demographics in these regressions. In our summary statistics in Section 2.3.5, we observed that income varied most clearly with distance to the nearest grocery store. In addition, it is likely that stores would locate in order to be close to households that can afford their products. We therefore run models that relate our access measures to household-month net income $Income_{ht}$,

[33] 74 percent of households without cars have only one person in the household, compared to 40 percent of car owners.

conditional on household fixed effects and the other controls (except for income)[34]:

$$Income_{ht} = \sum_{f=1}^{5} A_{htf} + \theta Z_{ht} + \tau_t + \epsilon_h + \varepsilon_{ht}, \tag{2.4}$$

Column 1 uses the grocery store indicators $\alpha_f D05_{htf} + \beta_f D10_{htf} + \gamma_f D15_{htf}$ to predict household income, and column 2 uses the grocery store concentration index. Neither grocery store access measure is significantly related to income changes.

Our second test asks whether our primary outcome of interest - expenditure share to grocery stores - predicts our grocery store access measures, conditional on observables and household fixed effects. If grocery store supply is assocated with unobservables, we would expect the grocery store expenditure share to significantly predict a household's supply of grocery stores. For this test, we regress grocery store access A_{htg} on grocery store expenditure shares s_{htg} and household fixed effects and other demographics:

$$A_{htg} = \alpha s_{htg} + \theta Z_{ht} + \tau_t + \epsilon_h + \varepsilon_{ht}, \tag{2.5}$$

Column 3 shows the results of this regression using the grocery store concentration index as A_{htg}, column 4 uses $D05_{htf}$ as A_{htg} and conditions on the other distance indicators, and column 5 uses $D10_{htf}$ as A_{htg} and conditions on the other distance indicators. Grocery store expenditure share has a precisely estimated but very small relationship with grocery store supply.

Our final test asks whether future grocery store supply predicts grocery store trip share. We define future grocery store supply at month t as the concentration index or distance indicators at $t+3$. Conditional on observables, fixed effects, and current grocery store supply, if our identifying assumption is correct we expect future store supply to be uncorrelated with current grocery store trip share. Our final placebo regressions are therefore of the form:

$$s_{h,t,g} = \sum_{f=1}^{5} A_{h,t,f} + \sum_{f=1}^{5} A_{h,t+3,f} + \theta Z_{h,t} + \tau_t + \epsilon_h + \varepsilon_{h,t}. \tag{2.6}$$

Column 6 uses the access indices and column 7 uses the access indicators. We only

[34] The benefit increase is included in Z_{ht}.

display the relevant coefficients for grocery store access, though access measures for all store formats are included. While current supply displays a clear relationship (at least in the case of the indicators), the relationship with future grocery store supply is an order of magnitude smaller and in all cases statistically insignificant.[35] Taken as a whole or individually, our robustness checks support our identifying assumptions.

2.5.3 Increasing resources

Descriptive

Figure 2.5 shows the average total monthly expenditures for each format over time, along with the share of expenditures at grocery stores. Vertical lines on the figure indicate the COL and ARRA benefit increase months. Expenditures increased for all formats as a result of the ARRA, with smaller increases due to the COL change. There is little indication, however, that the grocery store share changed as a result of the benefit increases.

Impact of increasing resources

Table 2.6 shows the coefficient of $\textit{Benefit Increase}_{ht}$ for regressions following Equation 2.1. Panel A uses expenditure shares as the outcome, and Panel B uses total format expenditures. In Panel A we see that SNAP benefit increases lead to larger shares of expenditures at grocery stores. An extra \$100 in benefits would lead to 2.1 percentage points higher grocery store expenditure shares for households without cars. This is primarily offset by drops in the share at convenience stores, which decrease by 1.4 percentage points for households without cars. Giving households with cars leads to smaller effects but in the same directions. As suggested by the average amounts of expenditures at each store format (Table 2.1), Panel B shows that most of the extra benefits were spent in grocery stores, followed by ethnic stores. Households without cars do, however, also increase spending at convenience stores by a statistically significant \$5.2.

We see in Table 2.6 that households increase spending at all store formats when given extra benefits, as would be expected. The larger question is whether this increase

[35] We obtain qualitatively similar results if we measure future store supply at $t+1$ and $t+2$.

is smaller or larger than we would expect. To examine this question, we compare the percentage of extra spending[36] at each store format to the corresponding budget share in Tables 2.7 and 2.8. Car owners spend less of the extra benefits than we would expect at grocery stores and substantially more than we would expect at ethnic stores. Though the dollar amount is small, these households also spend twice as much as expected at "Other" stores. In contrast, households without a car spend *more* than we would expect at grocery stores, and less at convenience stores. Households without a car display far smaller differences between the extra spending induced by the benefit increase and what we would expect from average budget shares, though they also spend twice as much at "Other" stores than expected.

2.6 Comparison of supply-side and demand-side policies

In Section 2.5.1 we observed that a grocery store opening increased total and relative spending at grocery stores and decreased shopping at ethnic stores, but had an ambiguous impact on convenience store shopping. There is some indication from the concentration index that households without cars shop less at convenience stores, but this is tempered by the observation that within 1.5 miles of a grocery store opening, households actually shop *more* at convenience stores (relative to outside of 1.5 miles of a grocery store). This counterintuitive result disappears for households within 0.5 miles of a grocery store opening. A grocery store opening within 1.5 miles leads households to shop less mainly at ethnic stores.

When households receive more resources, we saw in Section 2.5.3 that the share of expenditures at grocery stores increases and the share at convenience stores decreases. This impact is largest for households without cars, who also use the extra resources to spend \$5.2 more at convenience stores. Car owners spend the extra resources in unexpected ways - far more is spent at ethnic stores and substantially less is spent at grocery stores than we would predict from average budget shares.

The desirability of either policy will depend on the intended change in shopping behavior. If the goal of policy is simply to shift shares of household spending away

[36]The amount of extra spending is the coefficient from Panel B in Table 2.6. The percentage of extra spending is this coefficient divided by the sum of all coefficients in Panel B.

from convenience stores and towards grocery stores, giving households extra benefits unambiguously accomplishes this goal. Howevever, total spending at convenience stores does increase for households without cars, and the impact of this policy on car owners depends on how they use the extra resources at ethnic stores. We can also quantify the tradeoffs between the policies. A grocery store opening within 0.5 miles increased the share of expenditures at grocery stores for households without cars by 8 percentage points and households with cars by 3.9 percentage points (relative to no store within 1.5 miles). The same policy would increase the amount spent at grocery stores by $8.4 per month for households without cars and $6.3 per month for households with cars. Giving households more resources lead to 2.1 percentage point increase in grocery store expenditure share for households without cars and a 0.3 percentage point increase for households with cars. Therefore, in order to have the same impact on grocery store shares as opening a grocery store within 0.5 miles of a household, we would have to give households without cars $381 and households with cars $1,300 more per month. On the other hand, to have the same impact on total grocery store expenditures, we would have to give households without cars $14.3 and households with cars $19.5.

2.7 Discussion and conclusion

This paper measures the impact on store format choice of changing household access to grocery stores and increasing household resources. We also estimate the impact of changing household convenience store access, which we compare with the impact of the supply and demand policies. We find that opening a grocery store near a household leads to an increase in shopping at grocery stores *versus* other store formats but little change in shopping at convenience stores. Instead, a grocery store opening reduces shopping at discount and ethnic stores. The impact is concentrated among households within 0.5 miles of the grocery store that do not own a car.

In response to receiving more resources, we find that households increase their share of expenditures at grocery stores and decrease their share of expenditures at convenience stores. Total expenditures at all store formats increase, and in particular households without cars spent more at convenience stores. While households without cars spend the extra resources broadly as we would expect given average budget shares, car owners show greater flexibility and spend far more than expected at ethnic stores and less than expected at grocery stores.

Using our results, we are able to compare policies and quantify some of the tradeoffs between polices. Giving households more resources unambiguously shifts spending shares away from convenience stores and towards grocery stores - unlike opening grocery stores - but also results in more total spending at convenience stores and much more spending at ethnic stores than we would expect for households with cars. The amount of extra resources needed to have the same impact as a grocery store opening within 0.5 miles varies substantially by car ownership, and is far smaller if the goal of policy is simply to increase total spending at grocery stores instead of the share of expenditures at grocery stores.

We note here a few limitations of our analysis. Since our data only cover three years, and most households are observed for shorter periods, our results can be thought of as measuring the potential short-term impact on household behavior of policies to improve access to grocery stores. Households would plausibly switch purchases from other grocery stores to the closer store. If this keeps the closer store economically viable, it is possible that this could lead to long-term changes in preferences as households are exposed more frequently to a grocery store format. The extent to which this happens, and the time frame, could be a fruitful avenue for further research with different data. In addition, our data consist only of SNAP transactions. Since SNAP transactions consitute on average 67% of a household's total monthly food expenditures,[37] understanding how households spend their SNAP benefits gives us insight into a large portion of SNAP household food spending, but admittedly not all of the household's food spending. Given the nature of SNAP administrative data, we are unable to observe what specific items that households purchase. Finally, our data are from the Minneapolis-St. Paul metropolitan area for a two-year period. This might limit the external validity of our results.

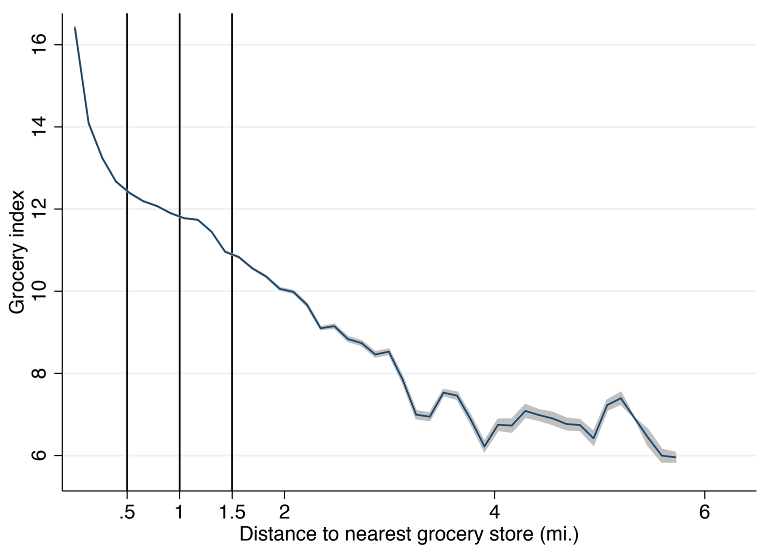
Our results bring up a number of directions for further investigation. We find that a grocery store opening leads to more shopping at convenience stores. Understanding why this would be the case, and for whom, could provide important information on the tradeoffs households make when deciding between retail food store formats. Ethnic stores appear to play an important role in the food shopping behavior of SNAP participants

[37] Authors' calculations based on data from the Current Population Survey for years 2004-2013.

in our city, and households with cars prefer to use extra resources at ethnic stores more than any other store format. Further understanding the role of ethnic stores in the food purchasing behavior of low-income households would be important in understanding how households choose to acquire food.[38] We also have documented substantial differences in shopping behavior between car owners and households that do not own cars. Given the substantial number of urban SNAP participants who do not own cars, a greater understanding of these differences has potentially significant implications for the effectiveness of many policies that aim to change the shopping behavior of low-income households.

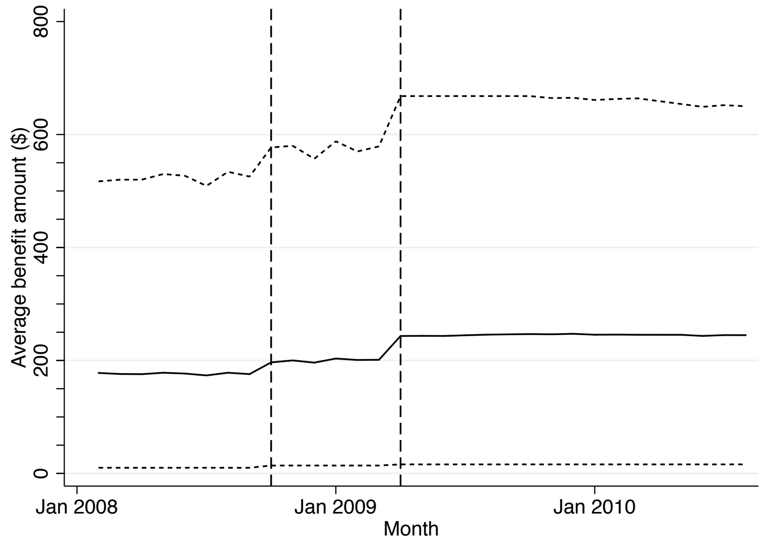
2.8 Figures and Tables

Figure 2.1: Nonparametric regression of grocery index on distance to nearest grocery store



[38]While spending at ethnic stores in Minneapolis-St. Paul is high, previous research has suggested significant roles for ethnic stores also in Chicago (Block, 2006) and San Diego (Joassart-Marcell *et al.*, 2017).

Figure 2.2: Benefit levels over time



The central line depicts the average benefit level over time. The top dotted line displays the 95^{th} percentile of benefit levels, and the bottom dotted line displays the 5^{th} percentile of benefit levels over time.

Figure 2.3: Average expenditure shares before and after grocery store opening within 1 mile

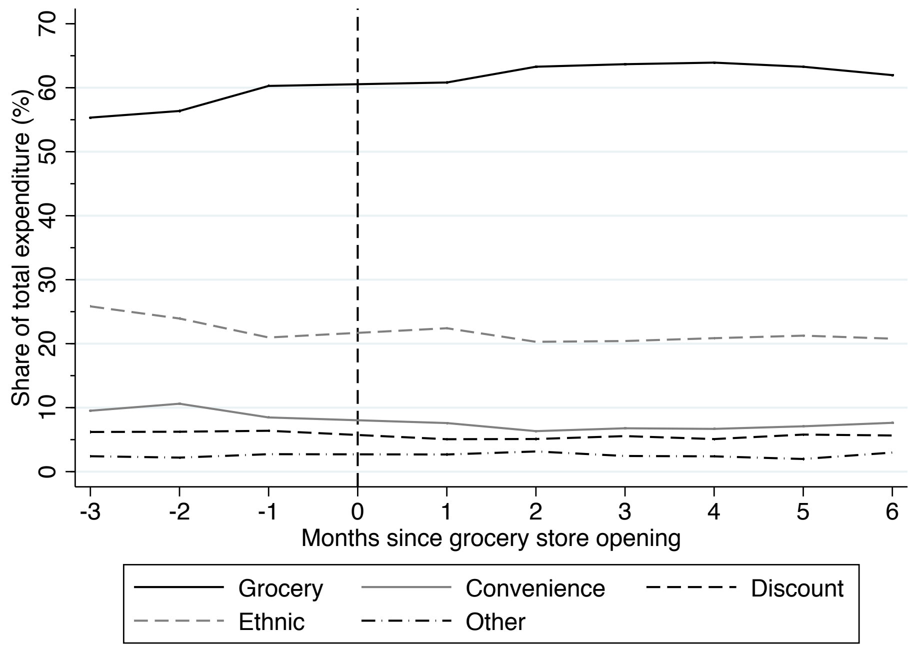


Figure 2.4: Average expenditure shares before and after grocery store opening within 0.5 mile

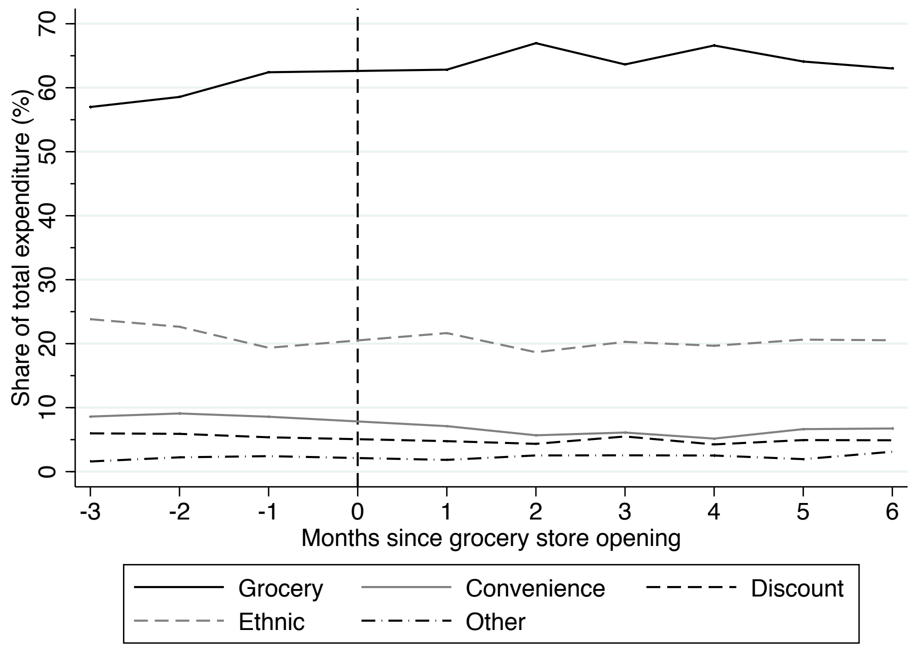


Figure 2.5: Average total expenditures by format and % expenditures at grocery stores, over time

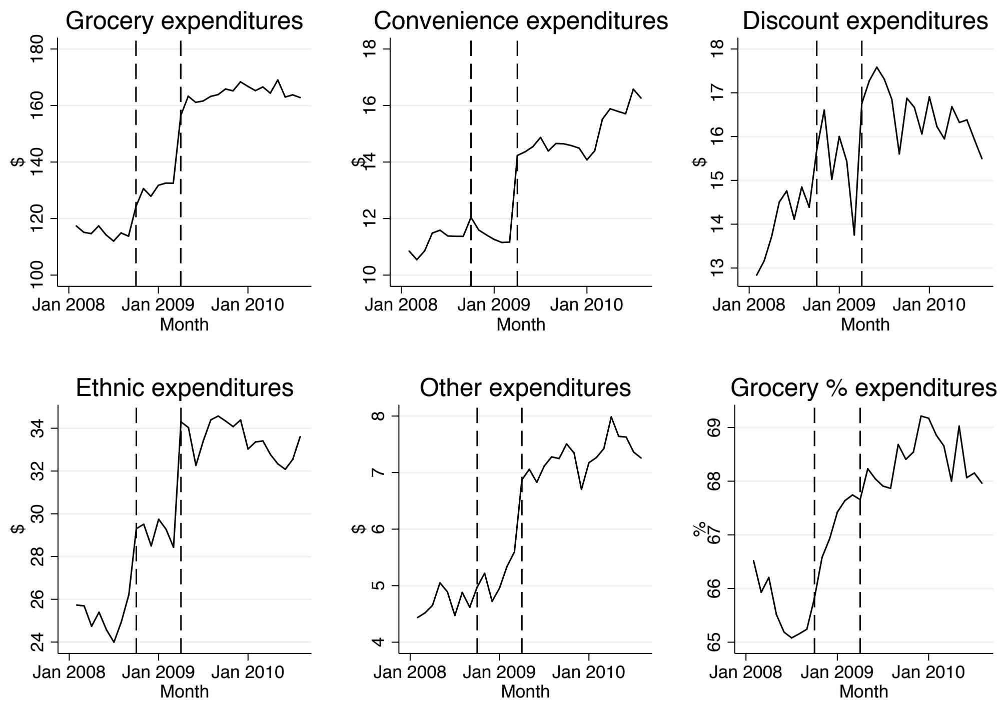


Table 2.1: Summary statistics of control variables by distance to nearest grocery store

	>1.5 mi	1 - 1.5 mi	0.5 - 1 mi	<0.5 mi
Household size	2.12	2.02	1.98	1.94
White	0.55	0.42	0.44	0.49
Black	0.22	0.35	0.30	0.30
Hispanic	0.04	0.04	0.05	0.04
Other race	0.19	0.19	0.21	0.17
% under 18 years old	24.44	22.97	22.61	22.35
% between 18 and 30 years old	15.85	18.09	17.82	16.95
% between 31 and 45 years old	18.49	18.33	18.54	17.95
% between 46 and 65 years old	31.79	32.05	33.34	32.01
% between 66 and 80 years old	7.59	7.11	6.15	8.24
% over 80 years old	1.84	1.41	1.52	2.48
Whether single parent household	0.22	0.20	0.19	0.20
Household has no car	0.55	0.64	0.65	0.66
Household has 1 car	0.33	0.28	0.27	0.27
Household has more than 1 car	0.11	0.08	0.08	0.07
SNAP net income ($)	391.46	375.88	357.88	379.77
Length of benefit month (days)	30.08	30.10	30.09	30.10
Number of benefit issuances	1.02	1.02	1.02	1.02
Consecutive number of months on SNAP	8.92	9.40	9.47	9.67
Convenience index	14.13	18.18	18.78	18.13
Grocery index	10.20	11.55	12.10	13.14
Discount index	6.36	7.15	7.12	7.00
Ethnic index	10.20	15.00	15.30	14.68
Other index	9.54	11.64	12.17	11.96
Census tract mean income	384.22	367.61	351.97	360.49
Census tract % SNAP participants	2.90	4.61	4.21	3.90

Continued on next page

Table 2.1: Summary statistics of control variables by distance to nearest grocery store

	>1.5 mi	1 - 1.5 mi	0.5 - 1 mi	<0.5 mi
N	66465	167568	241970	144619

Table 2.2: Summary statistics of store format expenditure shares by distance to nearest grocery store

	>1.5 mi	1 - 1.5 mi	0.5 - 1 mi	<0.5 mi
A. Expenditure shares (%)				
Convenience	6.40	9.23	7.75	7.32
Grocery	71.27	63.60	67.08	71.74
Discount	7.86	8.85	7.72	7.08
Ethnic	11.33	14.31	12.93	10.23
Other	1.92	2.69	3.06	2.33
N	66465	167568	241970	144619
B. Total expenditures ($)				
Convenience	11.41	15.05	14.14	12.92
Grocery	170.24	145.15	148.68	148.60
Discount	15.85	17.45	15.96	13.82
Ethnic	24.90	35.55	33.03	26.13
Other	4.86	6.26	7.40	5.99
N	66465	167568	241970	144619

Table 2.3: Impact on store format expenditure shares of changing access to grocery stores

	Grocery		Conv.		Discount		Ethnic		Other	
	C	NC	C	NC	C	NC	C	NC	C	NC
A. Grocery store index - household										
Index	0.8**	1.5***	0.3	-1.1***	-0.3	0.02	-0.5**	-0.7***	-0.3*	0.03
	(0.4)	(0.5)	(0.2)	(0.4)	(0.2)	(0.2)	(0.3)	(0.2)	(0.2)	(0.1)
B. Grocery store index - tract										
Index	0.8**	0.8*	0.4*	-0.7**	-0.2	0.2	-0.6**	-0.5**	-0.3*	0.02
	(0.4)	(0.4)	(0.2)	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)	(0.2)	(0.1)
C. Grocery store indicators										
I(0.5 miles)	0.9	5.8***	-0.1	-2.7**	0.1	-1.6**	-1.7	-1.6**	0.6	-0.02
	(1.3)	(1.3)	(1.0)	(1.2)	(0.7)	(0.7)	(1.1)	(0.8)	(0.5)	(0.4)
I(1 mile)	0.9	2.4**	-0.5	-2.1**	0.2	0.7	-0.2	-0.6	-0.3	0.1
	(1.0)	(1.1)	(0.7)	(1.0)	(0.6)	(0.6)	(0.9)	(0.6)	(0.5)	(0.3)
I(1.5 miles)	2.1*	-0.2	2.0*	3.3***	-1.9**	0.004	-2.0**	-3.3***	-0.5	-0.2
	(1.3)	(1.1)	(1.1)	(1.0)	(0.8)	(0.5)	(1.0)	(0.8)	(0.5)	(0.4)
Cumulative effect of opening within 0.5 miles										
Effect	3.9**	8.0***	1.4	-1.5	-1.7*	-0.9	-3.8***	-5.5***	-0.2	-0.1
F-stat	5.6	27.6	1.5	1.0	3.0	1.0	6.9	27.5	0.08	0.02
N	224314	396308	224314	396308	224314	396308	224314	396308	224314	396308

Notes: C = Car; NC = No car. Each column of Panels A, B, and C is a different household fixed effect regression of access and controls on the percent of expenditures at each store format as defined in the text. $I(0.5)$, $I(1)$, and $I(1.5)$ are indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles, respectively. The cumulative effect at 0.5 miles is the percent increase on store format share of having a store within 0.5 miles relative to 1.5 or more miles. The F-statistic is the test statistic for the test that the cumulative effect at 0.5 miles equals zero. Standard errors are clustered at the household level. In addition to the corresponding access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, whether the household owns 0, 1, or more cars/motorcycles, income, amount of benefits, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares, mean tract income, and tract % SNAP participants. Regressions also incude a interactions between a linear time trend and all control variables. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 2.4: Impact on store format total expenditures of changing access to grocery stores

	Grocery		Conv.		Discount		Ethnic		Other	
	C	NC	C	NC	C	NC	C	NC	C	NC
A. Grocery store index - household										
Index	3.2**	1.4	0.6	-0.6	-1.2*	-0.4	-1.7	-0.2	-0.9	-0.4*
	(1.5)	(0.9)	(0.8)	(0.4)	(0.6)	(0.4)	(1.2)	(0.5)	(0.7)	(0.2)
B. Grocery store index - tract										
Index	3.5**	0.9	1.2	-0.5	-1.3**	-0.4	-2.5*	-0.08	-1.1	-0.4
	(1.7)	(1.0)	(1.1)	(0.4)	(0.7)	(0.5)	(1.4)	(0.6)	(0.9)	(0.3)
C. Grocery store indicators										
I(0.5 miles)	-10.2**	6.0**	-0.6	-1.8	0.9	-1.9	-1.0	-2.2*	2.1	-1.6
	(5.1)	(2.4)	(2.3)	(1.1)	(1.6)	(1.3)	(4.3)	(1.2)	(1.4)	(1.2)
I(1 mile)	10.2**	4.2**	1.3	-0.4	0.3	1.0	-2.1	-0.3	-2.1*	-0.9
	(4.9)	(2.1)	(2.3)	(1.2)	(1.6)	(1.1)	(3.8)	(1.2)	(1.1)	(0.9)
I(1.5 miles)	6.3	-1.9	2.9	2.8*	-3.8**	-0.10	-2.0	-1.7	-1.3	0.9
	(4.3)	(2.3)	(2.3)	(1.4)	(1.5)	(1.0)	(3.3)	(1.5)	(1.2)	(0.8)
Cumulative effect of opening within 0.5 miles										
Effect	6.3	8.4***	3.6	0.6	-2.6	-1.0	-5.1	-4.1***	-1.4	-1.6
F-stat	1.6	13.6	1.6	0.2	1.6	0.8	1.5	8.0	0.7	1.7
N	224314	396308	224314	396308	224314	396308	224314	396308	224314	396308

Notes: C = Car; NC = No car. Each column of Panels A, B, and C is a different household fixed effect regression of access and controls on the total amount spent at each store format as defined in the text. $I(0.5)$, $I(1)$, and $I(1.5)$ are indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles, respectively. The cumulative effect at 0.5 miles is the percent increase on store format share of having a store within 0.5 miles relative to 1.5 or more miles. The F-statistic is the test statistic for the test that the cumulative effect at 0.5 miles equals zero. Standard errors are clustered at the household level. In addition to the corresponding access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, whether the household owns 0, 1, or more cars/motorcycles, income, amount of benefits, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares, mean tract income, and tract % SNAP participants. Regressions also incude a interactions between a linear time trend and all control variables. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 2.5: Impact of grocery store access: Placebos

	Dependent variables						
			Grocery store access				
	Inc ($10)		Index	I(0.5 miles)	I(1 mile)	Groc share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Groc share			-0.00005	0.000004	0.00002*		
			(0.00004)	(0.000008)	(0.00001)		
Groc index		-0.03				1.2***	
		(0.1)				(0.4)	
Groc index in 3 mos.						0.3	
						(0.3)	
I(0.5 miles)	-0.8**				0.5***		4.4***
	(0.4)				(0.03)		(1.1)
I(1 mile)	0.06			0.3***			1.3
	(0.4)			(0.02)			(0.9)
I(1.5 miles)	-0.09			0.06***	0.4***		0.3
	(0.4)			(0.02)	(0.02)		(0.9)
I(0.5 mi) in 3 mos.							-0.1
							(1.0)
I(1 mi) in 3 mos.							1.6*
							(0.8)
I(1.5 mi) in 3 mos.							-0.4
							(0.9)
N	620622	620622	620622	620622	620622	505706	505706

Notes: Each column is a separate household fixed effect regression, with the sample restricted to non-movers. $I(0.5)$, $I(1)$, and $I(1.5)$ are indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles, respectively. Regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, whether the household owns 0, 1, or more cars/motorcycles, amount of benefits, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares, mean tract income, and tract % SNAP participants. Regressions also incude a interactions between a linear time trend and all control variables. Models 3-7 additionally control for income. Models 1-2 and 6-7 control for access to other store formats. Standard errors are clustered at the household level. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 2.6: Impact of benefit increase of $100 on all store format outcomes

	Groc		Conv		Discount		Ethnic		Other	
	C	NC	C	NC	C	NC	C	NC	C	NC
A. Expenditure share										
Ben. incr.	0.3	2.1***	-0.5*	-1.4***	-0.4	-0.6	0.2	0.09	0.3*	-0.06
	(0.5)	(0.7)	(0.3)	(0.5)	(0.3)	(0.5)	(0.3)	(0.5)	(0.2)	(0.3)
B. Expenditure ($)										
Ben. incr.	44.1***	43.1***	2.1	5.2***	3.8***	3.8***	24.9***	10.9***	3.9***	3.7***
	(2.8)	(2.9)	(1.5)	(1.5)	(1.0)	(1.4)	(2.5)	(2.1)	(1.4)	(1.3)

Notes: Each column is a different household fixed effect regression of access and controls on store format-specific outcomes. Panel A shows the results for the expenditure share dependent variable, Panel B shows the results for expenditure amount, Panel C shows the results for trip share, Panel D shows the results for total number of trips, Panel E shows the results for average expenditure per trip, and Panel F shows the results for average trip distance. Standard errors are clustered at the household level. In addition to access indices for all store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, whether the household owns 0, 1, or more cars/motorcycles, income, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares, mean tract income, and tract % SNAP participants. Regressions also incude a interactions between a linear time trend and all control variables. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 2.7: Comparison of extra spending with average budget share: Car owners

	$ increase	% of extra spending	Budget share (%)
Grocery	44.1	56.0	74.3
Convenience	2.1	2.7	4.4
Discount	3.8	4.8	6.6
Ethnic	24.9	31.6	11.2
Other	3.9	4.9	2.6

Notes: This table compares the increase in spending at each store format due to SNAP benefit increases to the average budget shares, for car owners. The column "$ increase" shows the relevant coefficient from Table **??**. The next column shows this as a percent of the total increase in spending, defined as the sum of the coefficients in the first column. The last column shows the relevant average budget share from the data over all years.

Table 2.8: Comparison of extra spending with average budget share: Without car

	$ increase	% of extra spending	Budget share (%)
Grocery	43.1	64.6	63.9
Convenience	5.2	7.8	9.9
Discount	3.8	5.7	8.6
Ethnic	10.9	16.3	13.2
Other	3.7	5.5	2.6

Notes: This table compares the increase in spending at each store format due to SNAP benefit increases to the average budget shares, for households without cars. The column "$ increase" shows the relevant coefficient from Table **??**. The next column shows this as a percent of the total increase in spending, defined as the sum of the coefficients in the first column. The last column shows the relevant average budget share from the data over all years.

Chapter 3

The impact of access to food retail stores on food assistance spending over the month

3.1 Introduction

The different consumption patterns of poor households are often thought to contribute to worse health, lower well-being, and even a lack of resources, compared to non-poor households. Common explanations for divergent consumption behavior broadly suggest that either poor individuals are rational and make the best decisions given their situations, or that poor individuals make mistakes in decision-making that should be corrected for by outside organizations such as government, or a little of both (Bertrand *et al.*, 2004).[1] One particular behavior that has attracted is the sensitivity of consumption - and food consumption in particular - to the timing of income receipt. Previous research has documented steep declines in food and other non-durable consumption between income payments (Huffman and Barenstein, 2005; Stephens, 2006), Social Security payments (Mastrobuoni and Weinberg, 2009; Stephens, 2003), and food assistance benefit payments (Wilde and Ranney, 2000; Shapiro, 2005). Such lumpy consumption patterns - large expenditures and consumption close to the day of payment which decline until the next

[1] Mullainathan and Shafir (2013) also suggest that resource scarcity itself can impede cognitive functioning and so contribute to mistakes in decision-making.

payment - has been linked to worse diet quality (Kuhn, 2017; Todd, 2015), poor health such as acute hypoglycemia (Seligman *et al.*, 2014), poor student test scores (Cottie *et al.*, 2016), financially-motivated crime (Foley, 2011), and can influence measurements of food security (Gregory and Smith, 2018). Previous research has ruled out many explanations that attribute this behavior to rational decisions on the part of the poor,[2] and generally concludes that sensitivity to payment timing is due to time-inconsistent preferences.[3] One alternative hypothesis that could explain temporally lumpy consumption is that transaction costs make multiple store trips infeasible.

In this paper, we test whether transaction costs contribute to the monthly food expenditure cycle for a sample of households on the Supplemental Nutrition Assistance Program (SNAP). Households spend time reaching grocery stores and carrying out food shopping trips, and also spend money either on public transportation or gas for cars. If households only have a specific amount of time in which to shop each month, or are constrained in non-food expenditures such that there is only enough transportation money for a set amount of shopping trips, households may make a rational decision to lump all food expenditures into a few shopping trips. By the end of the month, the household may have depleted its stock of food but due to a constrained time or money budget be unable to make another grocery store shopping trip, causing the household to either skip meals (Kuhn, 2017) or to increase purchases from convenience stores. Damon *et al.* (2013) find evidence for this general pattern of substituting convenience store purchases for grocery store purchases later in the month, which might help explain the worsening diet quality. We specifically test the effect of two mechanisms which would be expected to decrease transaction costs for food shopping trips: Proximity to a grocery store, and access to a car.

To the best of our knowledge, this is the first paper to test whether the monthly expenditure cycle for SNAP households is due to costs associated with traveling to grocery stores, although this has clear and important implications for policy. One commonly-suggested policy correction to lumpy food expenditures of SNAP households

[2]Such explanations include food spoilage, theft, and intra- and inter-household competition (Shapiro, 2005), and changes in food quality as well as intertemporal price differences (Hastings and Washington, 2010).

[3]Smith *et al.* (2016) examine the role of another behavioral mechanism - income fungibility - in contributing to the food expenditure cycle for households on food assistance.

is to provide smaller benefit amounts multiple times in a month. If the cycle is due to short-run impatience - as suggested by Shapiro (2005) among others - multiple benefit issuances in a month would help households by enforcing consumption smoothing. If the cycle is due to difficulty in accessing grocery stores, providing benefits multiple times in a month could impose extra travel or time costs on households already facing a shortage of income and free time.

Only a few studies have examined a relationship between store access and consumption smoothing over the month for households on SNAP. Wilde and Ranney (2000) use survey and food intake data from the Continuing Survey of Food Intake by Individuals (CSFII) and find that households that shop more frequently than once per month ("frequent" shoppers) do not experience significant end-of-month drops in consumption compared to households that only shop once in a month. They find that greater distance to the primary grocery store decreases the probability of being a frequent shopper, though the effect is small. On the other hand, Shapiro (2005) uses the same dataset and finds that households with greater shopping frequency actually have greater declines in consumption. Kuhn (2016) provides an analysis that is closest in spirit to our paper; he uses survey and expenditure data from the United States Department of Agriculture's FoodAPS dataset and finds no relationship between the amount that expenditures decrease over the month and travel time to the household's primary grocery store. Of necessity, each of these previous studies have relied on survey data with self-reported distance or travel time to a primary store.

This paper makes five contributions. First, instead of relying on information about a single primary store, we measure proximity to all food stores that accept SNAP benefits in our area of interest. Second, while access and store choice are related, a household's choice of store includes considerations such as product variety and prices. Thus measuring access to potential stores - as we are able to do - is more informative of the impact of store proximity than the distance to a chosen store. Third, we leverage information on households and stores over time to observe the impact of store openings and closings. This lets us both address issues of the endogeneity of store proximity as well as directly estimate the impact of changes to store proximity. Finally, we are uniquely able to explore the impact of car ownership itself on the benefit expenditure cycle, as well as how car ownership interacts with store access.

To address this question we use a novel transaction-level and household administrative state SNAP data. We have the universe of SNAP transactions - including benefit disbursements - for the population of SNAP participants in the Minneapolis-St. Paul metropolitan area over a period of two fiscal years. These transaction data importantly include store's precise locations. Additionally, we combine information on transactions to household data, including precise household locations. The household and store locations provided in these data let us describe a household's proximity to grocery stores as well as to any other store where households can spend SNAP benefits. These data then allow us to relate the speed at which households spend SNAP benefits over a month to store proximity.

Households that wish to shop more often at grocery stores may choose to locate near these stores, which would confound our results. We account for this by focusing on within-household variation in store access and restricting our sample to households that do not move locations. This narrows any time series variation in store proximity to store openings and closings. Stores, however, similarly choose where to locate, and may locate near households with smoother (or less smooth) monthly expenditure patterns. They may do so because they value certain expenditure patterns in themselves, or because these patterns are related with other (unobservable) household characteristics. Looking at within-household variation in store proximity, we account for store location decisions by comparing households that differ by only small distances to the store opening (or closing). Within the small distances we are able to observe, changing household unobservables are plausibly exogenous to a store's decision to locate near one household relative to another household. This strategy allows us to measure the causal impact of store proximity on a household's spending over the month. While we do not argue that we are able to identify the causal impact of car ownership, we provide evidence that household spending over the month does not determine whether a household gains or loses a car and that future car ownership does not correlate with contemporaneous shopping behavior.

We find that proximity to grocery stores allows households to smooth expenditure over the benefit month. This impact is noticeable for households that do not own cars, and is smaller for household that own cars. Since whether a household owns a car influences the impact of proximity, we also measure directly the effect of car ownership. We find that households which lose a car and live far from a grocery store are more likely to concentrate expenditures at the beginning of the month, and so transaction costs likely constrain their ability to visit grocery stores. However, we also find that households which gain a car have more pronounced monthly expenditure cycles, suggesting that there is demand for larger shopping trips among at least some households. We also find that households which lose a car and live close to a grocery store actually have smoother expenditures over the month than before they lose a car, and that some of this smoothing could be due to substitution towads convenience store expenditures later in the month. Our results suggest that common policy solutions to the monthly SNAP cycle may have unintended detrimental impacts on households that do not have access to a car. If benefits are disbursed more often, these households will have to incur more travel and time costs to make additional trips to grocery stores, and may substitute expenditures away from grocery stores and towards convenience stores, which could have additional implications for diet quality.

We will first discuss the data and how we use it to measure store proximity. Then we turn to our empirical strategy for estimating the impact of store proximity as well as car ownership. Next we should decriptive and analytical results, and conclude with a summary and policy implications.

3.2 Data

3.2.1 Dataset

To measure the effect of store access on SNAP expenditure smoothing, we use administrative data provided by a state SNAP program. Our data cover 133,548,882 unique SNAP transactions from 71,256 households in the Minneapolis-St. Paul metropolitan area from October 2007 – September 2010. For each transaction we observe the amount, the date, and the store name and location. The transaction data also include the time and date on which benefits were given to each household. We combine these transaction-level data with monthly administrative data on each household, including information on household demographics, amount of authorized SNAP benefits, car ownership, and the household's address. These data allow us to describe a household's benefit month[4] and

[4]See Appendix B.1 for details on how benefit months were defined and days since benefit issuance was calculated.

access to stores of a range of formats. Tables 3.1 and 3.2 displays summary statistics for households at different distances to the nearest grocery store by car ownership.

We use two samples from these administrative data to measure the impact of store proximity and car ownership. To measure the effect of store proximity, we include households for which we can always identify the household address, who live in urban census tracts, who never move addresses during the time they are observed,[5] or who received benefits more than once in a benefit month.[6] In addition, to better control for changing household preferences, we exclude households that have no car for part of the time they are observed but do have a car for other months. This yields a total household-benefit month sample size of 483,092. To measure the impact of car ownership, we use a similarly-defined sample except that we include *only* households that experience a change in car ownership status, giving a household-month sample size of 66,337. This household-month sample includes 1,267 households that gain a car and 1,019 households that lose a car.[7]

3.2.2 Store formats

Information on the store name and address allow us to categorize stores in the Minneapolis-St. Paul metropolitan area into five different groups. In this paper we focus on grocery stores, but our comprehensive store data make it possible to control for proximity to other store formats. Given the possibile relationship between store format shopping and diet quality, our store format definitions focus on the likely assortment of healthy foods such as fresh fruits and vegetables. *Grocery stores* generally have the greatest assortment of healthy foods, and are traditional grocery stores and supermarkets as well as supercenters, mass merchandisers, and warehouse club stores. *Convenience stores* have the smallest assortment of healthy foods (Caspi *et al.*, 2016; Cannuscio *et al.*, 2013) and consist of corner stores, gas marts, dollar stores, and pharmacies. *Limited assortment* stores include stores such as Aldi which stock a limited variety healthy foods. We classify as *Ethnic stores* any store that we can identify from the name as catering to a specific

[5]Variation in proximity to stores can also come from households moving location. We restrict our sample to households that do not move so that temporal variation in proximity is due solely to store location changes.

[6]See Appendix B.1.

[7]Gaining a car is defined as transitioning from no car to having at least one car, and losing a car is defined as transitioning from having at least one car to having no cars.

ethnic customer base.[8] Ethnic stores are smaller and often more conveniently-located than grocery stores, but are also more likely than convenience stores to stock healthy foods such as fresh vegetables. Finally, we categorize as *Other stores* all cooperatives, natural food stores, farmers markets, butchers, and bakeries.

3.2.3 Store proximity

For each store format, we define geographic proximity as the concentration of stores near a household. Greater store concentration means lower the travel time and greater ease of accessing a particular store format. We measure store format concentration in two ways. First, we measure the concentration of stores within a small area around a household using a series of overlapping indicators for whether a household has a store within 0.5 miles, 1 mile, and 1.5 miles of its address. These indicators follow the access definitions similar to those used by the USDA Food Access Research Atlas data for urban areas, and provide a similar strategy to the one used by Currie *et al.* (2010) to measure the impact of fast food restaurants on obesity. Each indicator measures the *additional* impact of being within a specific distance, relative to being in the next-largest radius.

Previous research has shown, however, that households often do not shop at the closest grocery store (Ver Ploeg *et al.*, 2015). In this case, the food retail environment within 1.5 miles may not reflect the geographic access to stores as experienced by households. We therefore use a second measure of geographic access that summarizes the concentration of a wider range of store distances into one number. Following commonly-used definitions for concentration indices,[9] we define a distance-weighted index of the closest 20 stores of a particular store format:

$$I_{htf} = \sum_{s=1}^{20} \frac{1}{d_{hs}^{\theta}}$$

where d_{hs} is the Euclidean distance between household h and store s. Our baseline value for θ is 1/2.[10]

[8] Ethnic stores in the Minneapolis-St. Paul metropolitan area commonly identify as Hispanic, Asian, or East African.

[9] The various methods for defining indices that summarize access are summarized in Bhat *et al.* (2000). Kuang (2017) provides an example from the economics literature.

[10] We also check robustness to values of θ and other definitions of the kernel in Appendix B.2.

3.2.4 Describing shopping over the month

Much of the research documenting monthly expenditure and consumption cycles reports regress weekly expenditures (or log expenditures) on indicators for the number of weeks since benefit issuance. The coefficients on the indicators are generally increasingly negative, indicating increasingly lower expenditure/consumption over the course of the month. We use two outcomes that provide similar information on the speed at which household spend down their SNAP benefits, but also allow greater flexibility in terms of the empirical strategy. First, we use the percent of total monthly SNAP expenditures that is used in the first week of a specific benefit month.[11] Second, we define an indicator for whether all of the SNAP expenditures in a particular benefit month occured within the first week of the benefit month (and so the value of the first outcome is 100). These outcomes are similar in spirit to the approach used by Dobkin and Puller (2007), who use the ratio of early-month to late-month hospital admissions to describe the monthly cycle of admissions, and Dorfman *et al.* (2018), who use the ratio of expenditures in the first four days of the month to describe the monthly SNAP expenditure cycle.

3.3 Empirical strategy

3.3.1 General empirical strategy

Our primary specification describes the relationship between our outcomes of interest to store proximity, car ownership, and other household characteristics. Specifically, we model a monthly outcome O_{ht} for household h in benefit month t as:

$$O_{ht} = \sum_{f=1}^{5} A_{htf} + \theta Z_{ht} + \tau_t + \epsilon_h + \varepsilon_{ht}, \tag{3.1}$$

where A_{htf} is a household's proximity to a particular store format, Z_{ht} are household-month controls, τ_t denote month fixed effects, and ϵ_h are household fixed effects. Since to our knowledge all previous work has reported cross-sectional relationships (i.e. without

[11] The denominator in this outcome is the total SNAP expenditures in a month. Alternately we could use the total SNAP benefits issued to the household that month. We choose the former because benefits can roll over from month to month, and in any given month a household could spend more or less than their given benefit amount.

household fixed effects), we report cross-sectional results as well as those with household fixed effects. As explained above, we define access for household h in month t to store format f A_{htf} in one of two ways:

$$A_{htf} = \eta I_{htf} \tag{3.2}$$

where I_{htf} is the concentration index as defined in Section 3.2.3, or

$$A_{htf} = \alpha D05_{htf} + \beta D10_{htf} + \gamma D15_{htf} \tag{3.3}$$

where $D05$ is an indicator for whether household has a store of format f within 0.5 miles in month t, and $D10$ and $D15$ are similar indicators for 1 mile and 1.5 miles, respectively. Note that the indicators are not mutually exclusive; the total impact of having a store within 0.5 miles is $\alpha + \beta + \gamma$.

Our first outcome is the percent of expenditures that occur in the first week of the benefit month. A positive coefficient on a variable implies an increase the percent of expenditures in the first week and so lumpier expenditure patterns over time. The second outcome is the indicator for whether a household uses SNAP benefits only in the first week of the month. A positive coefficient implies a greater probability that the households spends all of their SNAP expenditures early in the month, and so similarly indicates a more pronounced SNAP cycle.

Z_{ht} are household-month controls: household size, household racial composition (black, white, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, whether the benefit month starts on a Friday,[12] the length of the benefit "month", the extra benefits due only to SNAP policy changes,[13] and the cumulative number of consecutive benefit "months" that we observe the household receiving SNAP benefits.[14]

[12]Castellari *et al.* (2016) find that spending patterns are different when households receive benefits on weekends than on weekdays.

[13]This is defined to be the household's authorized benefit level in a month less the amount of benefits the household would have been authorized to receive before the October 2008 cost of living adjustment.

[14]The number of consecutive "months" is therefore specific to each spell that the household is on SNAP. For households with one spell on SNAP, the number of consecutive months is just the total number of months the household has been receiving SNAP up until that month. Households with more

To answer whether proximity to stores influences how quickly households spend their SNAP benefits over a month, we are interested in the relationships given by coefficients η, α, β, and γ. Since we would expect that the degree to which households are influenced geographic proximity to stores to differ by car ownership, we estimate Equation **??** separately for households with cars and households without cars. Furthermore, Todd (2015) shows that the monthly consumption cycle is less pronounced after a large benefit increase in 2009 due to the American Recovery and Reinvestment Act (ARRA). We therefore estimate separate models using pre-ARRA months.[15]

We use a similar specification with the sample of households that change car ownership to measure the effect of having a car:

$$O_{ht} = \sum_{f=1}^{5} A_{htf} + \delta I(car) + \theta Z_{ht} + \tau_t + \epsilon_h + \varepsilon_{ht}, \tag{3.4}$$

where the only difference from above is the addition of $I(car)$, an indicator for car ownership. The primary coefficient of interest from this model is δ. We estimate Equation 3.4 separately households that gain a car and households that lose a car.[16] In order to make the coefficient interpretable as the impact of a car gain *versus* loss, in car gain regressions $I(car)$ is an indicator for the presence of a car, and in car loss regressions $I(car)$ is an indicator for the absence of a car. To investigate how grocery store proximity changes the relationship between car ownership and the SNAP cycle, we interact $I(car)$ with either the household-month grocery store concentration index or indicators for whether the household has a grocery store within X miles, where X is either 0.5 or 1.5 miles.

$$O_{ht} = \sum_{f=2}^{5} A_{htf} + \delta_1 I(car) + \delta_2 I(car) \times I(\text{X miles}) + \delta_3 I(\text{X miles}) + \theta Z_{ht} + \tau_t + \epsilon_h + \varepsilon_{ht}, \tag{3.5}$$

than one spell on SNAP during our period will have more than one "first" consecutive months. This variable, then, captures breaks in SNAP benefit receipt.

[15]Restricting models to pre-ARRA months, however, also restricts the variation in store proximity to a smaller number of store openings and closings. The pre-ARRA analysis is therefore possibly lower-power test of the impact of proximity than including all months.

[16]We exclude households that both gained and lost a car.

3.3.2 Causal identification

For identification of the impact of store proximity on benefit usage over the month, we require that stores do not locate near households due to the speed at which they spend benefits. We address these requirements using three strategies. First, household fixed effects control for any time-invariant differences in unobservables such as preferences that would influence store location decisions. When we report cross-sectional results from models that do not include household fixed effects, we will speak of associative relationships. Second, we control for past neighborhood-level shopping behavior.[17] These controls account for the attractiveness of any neighborhood to a store that is related to the neighborhood's (changing) shopping patterns.

The second method we use is conceptually similar to Currie *et al.*'s (2010) strategy for identifying the impact of fast food restaurants on obesity and relies on observing small variation in proximity between households. The series of 0.5-, 1-, and 1.5-mile indicators allow us to compare households that are 0.5 miles away from a store to households that are 1 mile away from a store. If store and household location *within 1.5 miles* of a specific household is based on factors (conditionally) unrelated to store access, then a comparison of households within that range gives a valid causal estimate of the impact of store access. This strategy allows stores to locate to neighborhoods based on changing neighborhood characteristics, but that the location within neighborhoods is constrained to which sites are available for purchase at the time that the store is looking to buy. Note that we exclude households which change car ownership status from the sample measuring the impact of store proximity, so our comparison of the impacts of proximity for car owners and households without cars is plausibly not influenced by selection of household-months into the car owner or not-car owner sample.

In order to measure the causal impact of car ownership on households that change car ownership status, we would require that households obtain and lose cars at random over time.[18] As shown in Figures 3.3 and 3.4, this is not likely the case - average household

[17] We use census tracts to define neighborhoods. To avoid neighborhood averages mechanically reflecting an individual household's behavior, a household's tract-level average variables are defined to exclude that household. Tract-level shopping variables that we include are 1- and 3-month lagged tract average percent of spending within the first 2 weeks, and the 1- and 3-month average tract lagged share of expenditures at each store format.

[18] Conditional of household fixed effects.

size suddenly changes at the same time as a household changes car ownership status, and income displays trends both before and after car gain/loss. We do observe, however, that households that lose a car and live within 0.5 miles of a grocery store display on average far smaller trends in household size and income at the same time as losing the car, suggesting that this sample could provide more valid information on the effect of car ownership apart from other factors.

3.4 Results: Store proximity

3.4.1 Descriptive

Differences in spending over the month

We first document differences in the pace at which households draw down benefits over proximity to grocery stores. Figure 3.1 shows the average spending, for households that differ by distance to the nearest grocery store and car ownership, for each day of the benefit month as a proportion of total SNAP expenditures. Each population experiences a sharp drop in expenditures after the first day of the benefit month. For both households within 0.5 miles and greater than 1.5 miles from a grocery store, those without cars display somewhat lumpier expenditure than those with cars. Households without cars noticeably spend approximately 5 percent more on the first day than households with car. Comparing across Figure 3.1a and 3.1b, however, there is little difference between households that live within 0.5 and further than 1.5 miles from the nearest grocery store.

Table 3.3 displays summaries of household spending over the benefit month, over distance to the nearest grocery store and car ownership. Similar to Figure 3.1, we see that there are very small differences between households at different distances to the nearest grocery store. On average, however, households without cars display evidence of less expenditure smoothing: Higher average percent of expenditures early in the benefit month, and lower expenditures at the end of the month.

Opening a grocery store

We next document the average share of total expenditures that is spent in the first two weeks of the month when a grocery store opens nearby. Figure 3.2 shows the distribution of the percent of expenditures in the first week of the month before and after a grocery store opens near a household. After the grocery store opens, there is a modest decrease in the proportion of households that spend all of their benefits in the first month, and an increase in households that spend between 20 and 70 percent in the first week. These distributional changes suggest that a grocery store opening may induce a small improvement in expenditure smoothing over the month (or at least beyond the first week).

3.4.2 Empirical model

Tables 3.4 and 3.5 display the grocery store proximity coefficients from our primary empirical specification for our outcomes. As mentioned above, we estimate separate models for households with and without cars and for pre-ARRA months. Models 1-2 and 5-6 are cross-sectional and exclude household fixed effects, while Models 3-4 and 7-8 include household fixed effects. At the bottom of each table, we include the implied cumulative impact of having a store within 0.5 miles of the household.

In Table 3.4 we see little impact of grocery store access on the average percent of expenditures in the first week. The cross-sectional results show a consistently negative relationship between proximity and early-month expenditures, suggesting that proximity could contribute to proportionately less spent early in the month and therefore greater expenditure smoothing. The results using household fixed effects, however, display considerable uncertainty and no consistent relationship.

Table 3.5 presents the results from the model using as the dependent variable an indicator for whether all expenditures occured in the first week. The coefficients then show the impact of grocery store proximity on the probability of having spent everything in the first week. While the concentration index coefficients are only statistically significant in models without fixed effects, they consistently show a negative relationship between proximity and spending everything. When we compare households within 1.5 miles of a grocery store opening, however, we do observe an impact of proximity. Households within 0.5 miles of an opening have substantial and statistically significant decreases in the probability of spending everything compared with even households just within 1 mile. Notably, the impact of proximity is concentrated in households within 0.5 miles of a grocery store opening who do not own a car. Prior to the ARRA, being within 0.5

miles of a grocery store opening resulted in a 6 percent lower probability of spending everything in the first week.

3.4.3 Placebos

To test our identifying assumptions, we run three types of placebo analyses. First, we regress grocery store proximity along with all of our controls (including household fixed effects) on household observables to test whether store location decisions are influenced by observables even after controlling for everything. Primary observables that change over time in our data are income and household size. Table 3.6 shows the results from this exercise. We find that proximity to grocery stores does not significantly predict household income or size, suggesting that store locations are not being determined by anything more than we include in our models.

Our second placebo tests whether our outcomes predict store location relative to households. If our identifying assumption is correct, outcomes should not predict store location, conditional on all of our controls. This test is shown in columns 1-3 of Tables 3.7 and 3.8. The coefficients on our outcomes are tiny and statistically insignificant.

The final test regresses current and future store proximity measures on our outcomes, again including all of our usual controls. This test is displayed in columns 4-5 of Tables 3.7 and 3.8. Proximity of grocery stores three months later does not predict our outcomes with statistical significance at conventional levels, and the effects of future proximity are generally smaller or wrong-signed. We take these placebo tests as support for identifying assumption that store location decisions are exogenous to our model after including our control variables.

3.5 Results: Car ownership and expenditure smoothing

3.5.1 Descriptive

The previous section showed that proximity to grocery stores allows greater expenditure smoothing primarily for households without cars. A related question, then, is the extent to which car ownership itself matters. In this section we show how car ownership is related to expenditure smoothing for a sample of households that change car ownership status.

We first document changes in the percent of expenditures within the first week before and after the gain/loss of a car. Figure 3.5 describes the percent of expenditures in the first week before and after a household gains a car, and Figure 3.6 does the same for a car loss. Panel (a) displays the average early-month expenditure percent for the three months before and six months after the car gain/loss, and Panel (b) shows a kernel density plot of the distribution of first-week expenditures before and after the car gain/loss. We see substantial variation in the averages over time, but Figure 3.5a suggests that households over 1.5 miles from a grocery store actually had less smooth expenditures after buying a car - the percent of expenditures in the first week increases significantly though for only a short time. On the other hand, Figure 3.6a does show that losing a car may contribute to lumpier consumption - after losing a car, households over 1.5 miles from a grocery store spend consistently more in the first week than they did before. Turning to the distributional changes due to car ownership, Figure 3.5b suggests that households which gain a car are less likely to spend everything in the first week and more likely to spend between 20 and 80 percent of expenditures during that time. The distributional impacts of losing a car are less clear.

3.5.2 Empirical model

We present the results of gaining a car in Tables 3.9 –3.11. First, Tables 3.9 shows the coefficient δ from Equation 3.4. Table 3.9 shows very little impact of gaining a car on early-month expenditures. The effects are measured with considerable noise and do not tell a consistent story. Table 3.10 displays how the impact of car ownership varies over distance to the grocery store for all months. Columns 1 and 4 interact car ownership with the concentration index, columns 2 and 5 interact car ownership with an indicator for whether there is a grocery store within 0.5 miles, and columns 3 and 6 interact car ownership with whether there is a grocery store within 1.5 miles. Table 3.11 displays the same models using only pre-ARRA months. The relationships using all months are measured with considerable uncertainty and the effect sizes are generally much smaller than the models focusing on the pre-ARRA time period. We can say with some degree of confidence, however, that the null effect from Table 3.9 masks heterogeneity over proximity to grocery stores. In pre-ARRA months (Table 3.11 column 6), for households which are greater than 1.5 miles away from a grocery store, gaining a car *increases* the probability of spending everything in the first week. This effect disappears as households get within 1.5 miles of a grocery store. We note that, while statistically insignificant, this general pattern is evident in the different measures of proximity shown in columns 4 and 5. Thus we find evidence that gaining a car decreases expenditure smoothing for households far from a grocery store.

Tables 3.12 - 3.14 show the corresponding models using the sample of households that lost a car. Although in all months not having a car is associated with a greater probability of spending everything in the first week,[19], in the fixed effects specification losing a car is associated with a *smaller* probability of spending everything early in the month. Thus losing a car is associated with more expenditure smooothing. When we examine how this relationship differs over grocery store proximity, we see in Table 3.14[20] that households which live far from a grocery store and lose a car generally have greater probability of spending everything in the first week. Column 6 shows that households greater than 1.5 miles from a grocery store which lose a car increase the probability of spending everything early in the month.[21] Households which live within 1.5 miles of a grocery store and lose a car, however, are 5 percent less likely[22] to spend everything in the first week. Thus we find heterogeneity in the overall result from Table 3.12: Households which lose a car and live far from a grocery store tend to have less smooth expenditure, while households which lose a car and live close to a grocery store actually have more smooth consumption.

3.5.3 Car ownership and transaction costs

We have so far shown that gaining a car is associated with lumpier expenditure patterns for households far from a grocery store, and losing a car is similarly associated with lumpier expenditure patterns for households far from a grocery store. Households

[19]Table 3.12 column 1.

[20]We will emphasize the pre-ARRA results since they display the same general relationship but have larger effect sizes. The pre-ARRA results are likely most relevant to time periods without the ARRA level of benefits - the benefit increase ended in 2013. However, we report the results for all months in Table 3.13 for completeness.

[21]Though this relationship is not statistically significant, it is qualitatively similar to the relationship in column 4.

[22]0.04 - 0.09

which lose a car and are close to a grocery store have less lumpy expenditures. More concentrated expenditure at the beginning of the month for households which lose a car and live far from a grocery store is consistent with a story in which transaction costs constrain the ability of these households to visit the store more often throughout the month. However, the other two main results are not consistent with a transaction costs story. If transaction costs were the dominant reason for an expenditure cycle, we would expect that gaining a car would allow less lumpy expenditure patterns for all households, and that losing a car would similarly force lumpier expenditures even at close distances to the grocery store.[23]

Our results suggest the following two explanations. First, households choose to acquire a car (partially) in order to make larger trips to the grocery store. Thus households demand even lumpier consumption than they are able to accomplish without a car. Second, we note that the alternative to lumpier expenditures at a grocery store is not necessarily smoother grocery store expenditures. Instead, one alternative is smoother expenditures from convenience stores (or stores of other store formats). That is, households which do not spend as much in grocery stores at the beginning of the month may simply spend more at convenience stores later in the month. With this story, we have to testable hypotheses:

1. Households which gain a car and live far from a grocery store have a more pronounced cycle because they substitute away from convenience store expenditures to grocery store expenditures at the beginning of the month.

2. Households which lose a car and live close to a grocery store have a less pronounced cycle because they substitute convenience store expenditures throughout the month for concentrated grocery store expenditures at the beginning of the month.

To test these hypotheses, we run Equation 3.5 using as an outcome the percent of monthly expenditures at convenience stores. Tables 3.15 and 3.16 show the results of this test. Households which gain a car and live far from a grocery store do not, in fact, spend a smaller percent at convenience stores. In the hypothetical case where a

[23]We would expect that the impact of losing a car would be attenuated at close distances to a grocery store compared to further distances, but we would not expect the relationship to switch sign, as it does in our results.

household is proximate to no grocery stores (the concentration index equals 0), gaining a car is associated with a 6 percentage point increase in convenience store expenditures (column 4 of Table 3.15). At the mean grocery store concentration index for households with a grocery store over 1.5 miles away (10.33), the impact of gaining a car is no change in the percent of expenditures at convenience stores. Turning to households which lose a car and live close to a grocery store, Table 3.16 shows that households close to a grocery store do increase convenience store expenditures when they lose a car. Column 6 implies that the percent of expenditures at convenience stores increases by 1 percentage point[24] We therefore find some support for our conjecture that consumption smoothing by households which lose a car is due to substitution towards convenience store expenditures, but we do not find evidence that this is the case for households which gain a car.

3.6 Discussion and conclusion

Lumpy monthly spending patterns of poor households have presented researchers with an empirical puzzle. One particular manifestation of this is with food assistance programs: SNAP households experience significant declines in SNAP expenditure over the month until they receive their next benefit payment. Previous research suggests that this decline creates cycles in total food consumption, is related to decreasing diet quality over the month, and may entail adverse health effects. The reasons for this monthly expenditure decline are not well understood, with much previous work attributing it to irrational time-inconsistent preferences. We test an alternative explanation: Whether the decline could be due to transaction costs associated with grocery shopping. Using unique administrative SNAP data from a large metropolitan area and a robust identification strategy, we show that proximity to grocery stores does allow households to smooth expenditure more over the month. This effect is most noticeable in households without cars. Given the heterogeneous impacts of proximity over car ownership, we also show assocations between car ownership and SNAP expenditure smoothing. Contrary to our transactions cost hypothesis, we find that gaining a car is associated with more concentrated expenditures at the beginning of the month. We also find that losing a car

[24] $2.7 - 1.7$, though the value 1.7 is not statistically different from zero.

is associated with smoother expenditure for some households, and show that part of this smoothing could be due to substitution towards convenience store expenditures later in the month.

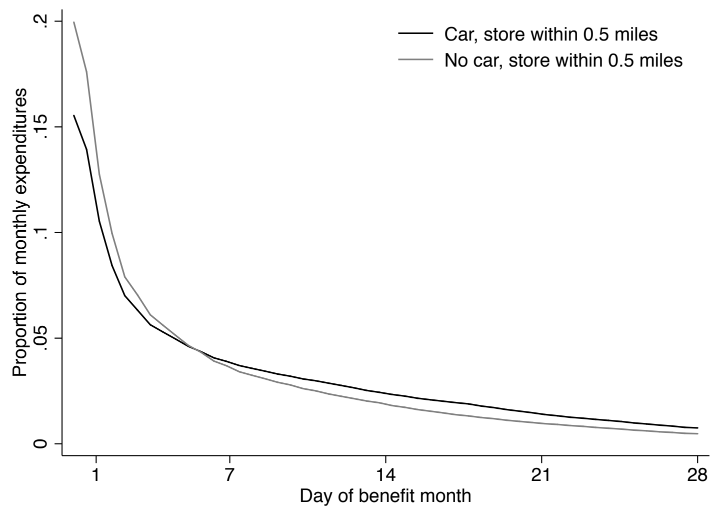
We note that our analysis has a few limitations. Our data come from the period of the recent recession in the Twin Cities metropolitan area, which could potentially restrict its external validity to other times and locations. We also do not see non-SNAP expenditures, which could be used more (or less) smoothly over the SNAP benefit month. Finally, what we gain with our data in comprehensiveness as to the population of interest - SNAP participants - we give up in the ability to observe each household in more depth. We therefore cannot tell, for example, why a household chose to obtain a car, which could add to the causal implications of car ownership.

Our results suggest that access to stores - both proximity to stores as well as access to a car - contributes to the SNAP cycle. Furthermore, we show the importance of considering proximity and car access together. Proximity plays a smaller role for households that own cars, but is a potentially significant cause of lumpier expenditures over the month for households that do not own cars. This finding implies that the common policy suggestion of paying benefits to households more than once per month could negatively impact those who do not have easy access to a car by imposing additional time and travel costs to make another trip to the grocery store. If the car-less household cannot add more grocery store trips into their monthly schedule, spending at convenience stores may increase. This shift towards convenience store spending could have further implications for diet quality and health. Expenditure smoothing in households with access to cars, however, is not noticeably impacted by store proximity. Policies such as more frequent disbursements to enforce smoother spending may help these households while causing fewer negative unintended consequences.

3.7 Figures and Tables

Figure 3.1: Nonparametric regressions of expenditure over benefit month

(a) Households within 0.5 miles of a grocery store



(b) Households greater than 1.5 miles from a grocery store

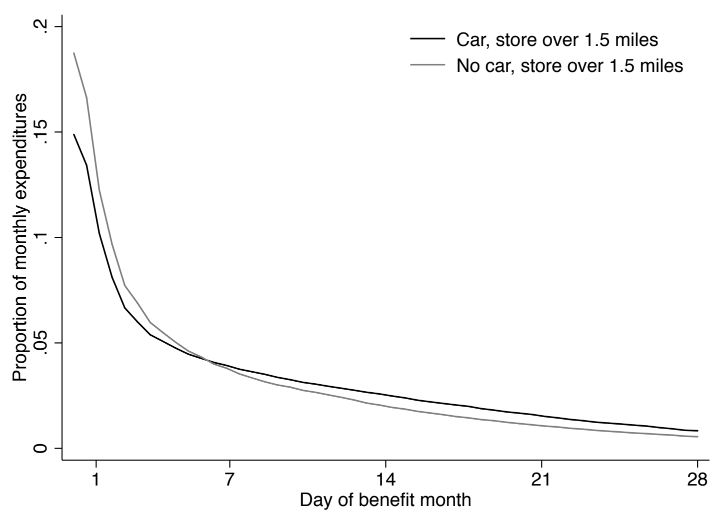
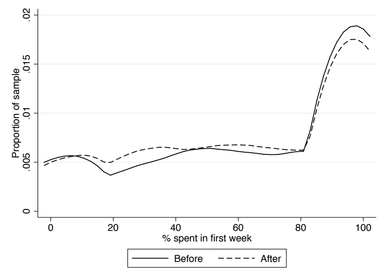
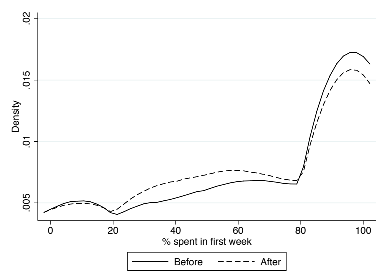


Figure 3.2: Distribution of the percent of expenditures spent in first week, before and after grocery store opening

(a) Households with a store opening within 1 mile



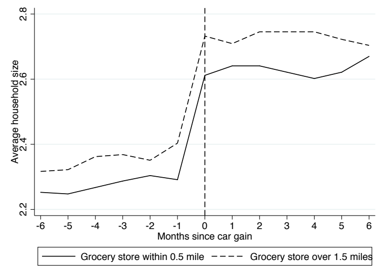
(b) Households with a store opening within 0.5 mile



Notes: The figure shows kernel density graphs of the distribution of the percent of expenditures in the first week of the benefit month for the 3 months before ("Before") and the 3 months after ("After") a grocery store opening.

Figure 3.3: Description of household characteristics before and after gaining a car

(a) Household size before and after car gain



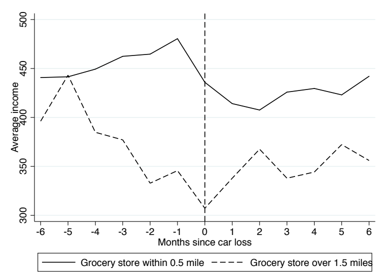
(b) Income before and after car gain

Notes: The figures show the average monthly household size and income of households whose car ownership status changed from having no car to having at least one car ("car purchase") for 6 months before to 6 months after the addition of the car.

Figure 3.4: Description of household characteristics before and after losing a car

(a) Household size before and after car loss

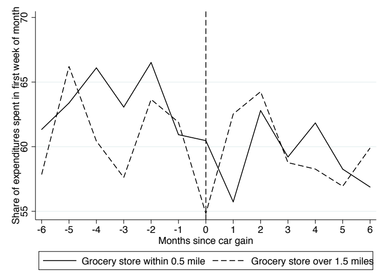
(b) Income before and after car loss



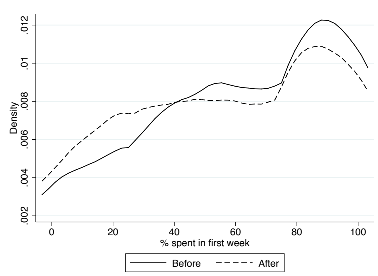
Notes: The figures show the average monthly household size and income of households whose car ownership status changed from having at least one car to having no car for 6 months before to 6 months after the loss of the car.

Figure 3.5: Description of outcomes before and after gain of a car

(a) Average percent of expenditures in the first week, before and after car gain



(b) Distribution of the percent of expenditures in the first week, before and after car gain

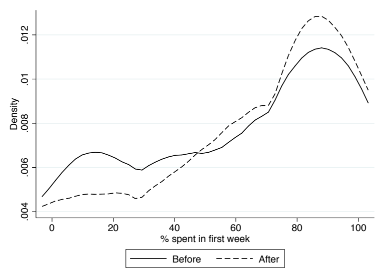


Notes: The figures show the average and distribution of the percent of expenditures spent in the first week, for households whose car ownership status changed from having no car to having at least one car for 6 months before to 6 months after the addition of the car.

Figure 3.6: Description of outcomes before and after loss of a car

(a) Average percent of expenditures in the first week, before and after car loss

(b) Distribution of the percent of expenditures in the first week, before and after car loss



Notes: The figures show the average and the distribution of the percent of expenditures spent in the first week, for households whose car ownership status changed from having at least one car to having no car for 6 months before to 6 months after the addition of the car.

Table 3.1: Summary statistics of control variables by distance to nearest grocery store (in miles): Households with cars

	>1.5	1 - 1.5	0.5 - 1	<0.5
Household size	2.80	2.84	2.77	2.75
White	0.66	0.53	0.55	0.58
Black	0.14	0.23	0.19	0.20
Hispanic	0.01	0.02	0.02	0.01
Other race	0.19	0.22	0.24	0.20
% under 18	33.10	33.59	31.37	32.83
% 18-30	13.36	13.65	13.26	13.66
% 31-45	19.44	19.21	19.03	18.62
% 46-65	28.25	27.84	30.52	27.52
%66-80	5.46	5.07	5.10	6.14
% 81+	0.38	0.59	0.71	1.21
Whether single parent	0.28	0.30	0.27	0.30
Income	519.66	535.01	521.26	539.86
Length of benefit month	29.98	30.01	30.00	30.01
Consecutive months	8.29	8.92	9.01	9.06
Grocery store index	9.89	11.22	11.71	12.51
Convenience store index	12.68	16.15	16.81	15.94
Discount store index	6.10	6.84	6.80	6.60
Ethnic store index	9.15	12.87	13.15	12.28
Other store index	9.01	10.76	11.15	10.75
Tract mean income	400.34	383.36	376.65	388.95
Tract % SNAP	2.23	3.66	3.57	3.18
N	29008	58915	83319	48642

Table 3.2: Summary statistics of control variables by distance to nearest grocery store (in miles): Households without cars

	>1.5	1 - 1.5	0.5 - 1	<0.5
Household size	1.56	1.56	1.55	1.51
White	0.47	0.36	0.38	0.44
Black	0.29	0.41	0.36	0.36
Hispanic	0.06	0.05	0.06	0.06
Other race	0.19	0.18	0.19	0.14
% under 18	17.19	16.80	17.68	16.67
% 18-30	17.64	20.40	20.08	18.46
% 31-45	17.69	17.80	18.26	17.55
% 46-65	34.97	34.71	35.15	34.64
%66-80	9.47	8.37	6.82	9.48
% 81+	3.04	1.89	1.98	3.18
Whether single parent	0.16	0.15	0.15	0.14
Income	290.25	288.56	271.05	297.95
Length of benefit month	30.20	30.19	30.19	30.19
Consecutive months	9.56	9.79	9.84	10.12
Grocery store index	10.45	11.74	12.31	13.47
Convenience store index	15.31	19.33	19.84	19.27
Discount store index	6.58	7.33	7.30	7.21
Ethnic store index	11.04	16.20	16.46	15.92
Other store index	9.97	12.13	12.72	12.59
Tract mean income	371.54	359.07	338.85	345.83
Tract % SNAP	3.45	5.16	4.56	4.27
N	36311	105826	154453	93537

Table 3.3: Summary statistics of spending over the month by distance to nearest grocery store (in miles)

	>1.5	1 - 1.5	0.5 - 1	<0.5
Car owners				
% expenditures in first day	16.07	17.36	17.29	17.41
% expenditures in first week	53.65	55.57	55.72	55.72
% expenditures in last week	9.41	8.81	8.66	8.78
N	29008	58915	83319	48642
Households without cars				
% expenditures in first day	20.28	21.38	21.43	20.84
% expenditures in first week	62.39	64.15	63.47	62.77
% expenditures in last week	6.56	6.31	6.71	6.67
N	36311	105826	154453	93537

Table 3.4: Impact of grocery store access on the proportion of SNAP expenditures used in the first week of the benefit month

	All months				Pre-ARRA			
	Car	No car	Car	No car	Car	No car	Car	No car
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Grocery store index								
Grocery	-0.3**	-0.1	-0.4	0.7	-0.3	-0.05	-0.4	0.6
	(0.1)	(0.1)	(0.5)	(0.4)	(0.2)	(0.2)	(1.0)	(0.8)
B. Grocery store indicators								
I(0.5 miles)	-0.04	-0.5	1.8	1.0	-0.09	-0.9*	0.7	0.6
	(0.5)	(0.4)	(1.8)	(1.4)	(0.7)	(0.5)	(2.6)	(1.9)
I(1 mile)	0.3	0.2	-0.008	-0.2	0.6	0.5	-0.5	1.5
	(0.4)	(0.4)	(1.3)	(1.2)	(0.6)	(0.5)	(2.0)	(1.6)
I(1.5 miles)	0.2	-0.5	-3.3**	0.3	0.2	-0.4	-3.2	-2.1
	(0.6)	(0.6)	(1.6)	(1.1)	(0.8)	(0.7)	(2.5)	(1.5)
Cumulative effect within 0.5 miles								
Effect	0.415	-0.757	-1.507	1.120	0.749	-0.763	-3.010	0.030
F-stat	0.4	1.7	0.7	0.7	0.8	1.0	1.8	0.0003
N	219884	390127	219884	390127	69782	134863	69782	134863
Household fixed effects			X	X			X	X

Notes: Each column of Panel A and each column of Panel B is a different regression of access and controls on the percent of the household's total SNAP expenditures that occur within the first two weeks of the household's benefit month. The sample in columns (5)-(8) is restricted to months prior to the American Recovery and Reinvestment Act (ARRA) in April 2009. $I(0.5)$, $I(1)$, and $I(1.5)$ are indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles, respectively. The cumulative effect at 0.5 miles is the percent increase on the outcome of having a store within 0.5 miles relative to 1.5 or more miles. The F-statistic is the test statistic for the test that the cumulative effect at 0.5 miles equals zero. Standard errors for all regressions are clustered at the household level. In addition to the corresponding access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.5: Impact of grocery store access on whether the 100% of expenditures occur in first week of the benefit month

	All months				Pre-ARRA			
	Car	No car	Car	No car	Car	No car	Car	No car
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Grocery store index								
Grocery	-0.004***	-0.002*	-0.002	-0.001	-0.007***	-0.003	-0.01	-0.02
	(0.001)	(0.001)	(0.006)	(0.006)	(0.002)	(0.002)	(0.01)	(0.01)
B. Grocery store indicators								
I(0.5 miles)	-0.004	-0.01***	-0.007	-0.01	-0.006	-0.02***	-0.06*	-0.06**
	(0.005)	(0.004)	(0.02)	(0.02)	(0.007)	(0.006)	(0.03)	(0.03)
I(1 mile)	-0.00006	-0.0005	0.02	-0.008	0.002	0.0004	0.01	0.02
	(0.005)	(0.004)	(0.02)	(0.01)	(0.007)	(0.006)	(0.03)	(0.02)
I(1.5 miles)	0.00002	-0.009	-0.01	-0.01	-0.002	-0.004	0.004	-0.03
	(0.006)	(0.007)	(0.02)	(0.01)	(0.009)	(0.009)	(0.03)	(0.02)
Cumulative effect within 0.5 miles								
Effect	-0.004	-0.022***	0.001	-0.030*	-0.006	-0.020**	-0.043	-0.059**
F-stat	0.4	10.3	0.0009	2.8	0.4	4.8	2.6	6.6
N	219884	390127	219884	390127	69782	134863	69782	134863
Fixed effects			X	X			X	X

Notes: Each column of Panel A and each column of Panel B is a different regression of access and controls on an indicator for whether all of the household's SNAP expenditures occur within the first week of the household's benefit month. The sample in columns (5)-(8) is restricted to months prior to the American Recovery and Reinvestment Act (ARRA) in April 2009. $I(0.5)$, $I(1)$, and $I(1.5)$ are indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles, respectively. The cumulative effect at 0.5 miles is the percent increase on the outcome of having a store within 0.5 miles relative to 1.5 or more miles. The F-statistic is the test statistic for the test that the cumulative effect at 0.5 miles equals zero. Standard errors for all regressions are clustered at the household level. In addition to the corresponding access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.6: Relationship between store location and household demographics

	Dependent variables			
	Income ($10)		Household size	
	(1)	(2)	(3)	(4)
Grocery store index	3.3		-0.0008	
	(3.9)		(0.006)	
I(0.5 miles)		-1.0		-0.01
		(11.1)		(0.02)
I(1 mile)		-0.8		0.03
		(11.3)		(0.02)
I(1.5 miles)		-8.0		-0.02
		(10.1)		(0.02)
N	610011	610011	610011	610011
N	610011	610011	610011	610011

Notes: Each column is a different regression on the dependent variable specified. Models 1-2 regress income on grocery store proximity and controls, and Models 3-4 regress household size on grocery store proximity and controls. $I(0.5)$, $I(1)$, and $I(1.5)$ are indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles, respectively. Standard errors for all regressions are clustered at the household level. In addition to the corresponding access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.7: Impact of grocery store access on percent of expenditures in first week: Placeobs

	Dependent variables				
	Grocery store access				
	Index	I(0.5 miles)	I(1 mile)	% in first week	
	(1)	(2)	(3)	(4)	(5)
% in first week	0.000005	0.000003	-0.0000002		
	(0.00001)	(0.000003)	(0.000003)		
Grocery store index				0.6	
				(0.4)	
Grocery index 3 months later				-0.5	
				(0.4)	
I(0.5 miles)			0.5***		1.9
			(0.03)		(1.3)
I(1 mile)		0.3***			-1.2
		(0.02)			(1.0)
I(1.5 miles)		0.07***	0.4***		-0.6
		(0.02)	(0.02)		(1.1)
I(0.5 miles) 3 months later					-0.5
					(1.4)
I(1 mile) 3 months later					1.7
					(1.1)
I(1.5 miles) 3 months later					-1.2
					(1.3)
N	610011	610011	610011	496890	496890

Notes: Each column is a different regression on the dependent variable specified. Models 1-3 regress grocery store proximity on shopping behavior and controls, and Models 4-5 regress shopping behavior on contemporaneous and future store proximity. $I(0.5)$, $I(1)$, and $I(1.5)$ are indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles, respectively. Standard errors for all regressions are clustered at the household level. In addition to the corresponding access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.8: Impact of grocery store access on whether 100% of expenditures were spent in first week: Placeobs

	Dependent variables				
	Grocery store access				
	Index	I(0.5 miles)	I(1 mile)	% in first week	
	(1)	(2)	(3)	(4)	(5)
% in first week	0.000005	0.000003	-0.0000002		
	(0.00001)	(0.000003)	(0.000003)		
Grocery store index				0.6	
				(0.4)	
Grocery index 3 months later				-0.5	
				(0.4)	
I(0.5 miles)			0.5***		1.9
			(0.03)		(1.3)
I(1 mile)		0.3***			-1.2
		(0.02)			(1.0)
I(1.5 miles)		0.07***	0.4***		-0.6
		(0.02)	(0.02)		(1.1)
I(0.5 miles) 3 months later					-0.5
					(1.4)
I(1 mile) 3 months later					1.7
					(1.1)
I(1.5 miles) 3 months later					-1.2
					(1.3)
N	610011	610011	610011	496890	496890

Notes: Each column is a different regression on the dependent variable specified. Models 1-3 regress grocery store proximity on shopping behavior and controls, and Models 4-5 regress shopping behavior on contemporaneous and future store proximity. $I(0.5)$, $I(1)$, and $I(1.5)$ are indicators for the presence of a grocery store within 0.5, 1, and 1.5 miles, respectively. Standard errors for all regressions are clustered at the household level. In addition to the corresponding access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.9: Impact of car gain: Overall

	All months		Pre-ARRA	
	(1)	(2)	(3)	(4)
A. % spent in first week				
Whether car	0.2	0.2	-2.7	-0.3
	(1.1)	(0.7)	(1.8)	(1.5)
N	18245	18245	5901	5901
B. Whether 100% spent in first week				
Whether car	-0.006	-0.01	-0.005	0.006
	(0.01)	(0.009)	(0.02)	(0.02)
N	18245	18245	5901	5901
Household fixed effects		X		X

Notes: Each column is a different regression of an indicator for whether a household owns a car and controls on the outcome specified in italics. The outcome for Panel A is the percent of expenditures spent in the first two weeks of the benefit month, and for Panel B is an indicator for whether 100% of expenditures occured in the first week. Models 2,4,6, and 8 interact car ownership with the grocery store concentration index. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the month's benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.10: Impact of car gain: Over distance, all months

	(1)	(2)	(3)	(4)	(5)	(6)
A. % spent in first week						
Whether car	-0.5	-0.4	-1.2	-1.7	0.8	0.3
	(6.0)	(1.1)	(2.4)	(4.2)	(0.8)	(1.5)
Index × whether have car	0.06			0.2		
	(0.5)			(0.4)		
I(0.5) × whether have car		-2.0			-1.6	
		(2.1)			(1.4)	
I(1.5) × whether have car			0.4			0.2
			(2.5)			(1.6)
N	18245	18245	18245	18245	18245	18245
B. Whether 100% spent in first week						
Whether car	0.06	-0.02	0.01	0.003	-0.007	0.02
	(0.07)	(0.01)	(0.03)	(0.05)	(0.010)	(0.02)
Index × whether have car	-0.006			-0.001		
	(0.006)			(0.004)		
I(0.5) × whether have car		-0.01			0.002	
		(0.02)			(0.02)	
I(1.5) × whether have car			-0.03			-0.03
			(0.03)			(0.02)
N	18245	18245	18245	18245	18245	18245
Household fixed effects				X	X	X

Notes: Each column is a different regression of an indicator for whether a household owns a car and controls on the outcome specified in italics. The outcome for Panel A is the percent of expenditures spent in the first two weeks of the benefit month, and for Panel B is an indicator for whether 100% of expenditures occured in the first week. Models 2,4,6, and 8 interact car ownership with the grocery store concentration index. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the month's benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.11: Impact of car gain: Over distance, Pre-ARRA

	(1)	(2)	(3)	(4)	(5)	(6)
A. % spent in first week						
Whether car	-10.1	-1.6	-0.06	-6.1	0.5	0.5
	(10.3)	(2.0)	(4.4)	(9.7)	(1.6)	(2.8)
Index × whether have car	0.6			0.5		
	(0.9)			(0.8)		
I(0.5) × whether have car		-3.6			-3.6	
		(3.9)			(3.2)	
I(1.5) × whether have car			-2.8			-0.9
			(4.6)			(3.1)
N	5901	5901	5901	5901	5901	5901
B. Whether 100% spent in first week						
Whether car	0.02	-0.001	0.07	0.02	0.02	0.06**
	(0.1)	(0.03)	(0.06)	(0.1)	(0.02)	(0.03)
Index × whether have car	-0.002			-0.001		
	(0.01)			(0.01)		
I(0.5) × whether have car		-0.01			-0.06	
		(0.05)			(0.05)	
I(1.5) × whether have car			-0.09			-0.07**
			(0.06)			(0.03)
N	5901	5901	5901	5901	5901	5901
Household fixed effects				X	X	X

Notes: Each column is a different regression of an indicator for whether a household owns a car and controls on the outcome specified in italics. The outcome for Panel A is the percent of expenditures spent in the first two weeks of the benefit month, and for Panel B is an indicator for whether 100% of expenditures occured in the first week. Models 2,4,6, and 8 interact car ownership with the grocery store concentration index. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the month's benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.12: Impact of car loss: Overall

	All months		Pre-ARRA	
	(1)	(2)	(3)	(4)
A. % spent in first week				
No car	1.0	0.4	-0.2	-1.0
	(1.3)	(0.8)	(2.0)	(1.8)
N	15662	15662	5485	5485
B. Whether 100% spent in first week				
No car	0.03*	-0.0006	0.01	-0.04*
	(0.02)	(0.01)	(0.02)	(0.03)
N	15662	15662	5485	5485
Household fixed effects		X		X

Notes: Each column is a different regression of an indicator for whether a household owns a car and controls on the outcome specified in italics. The outcome for Panel A is the percent of expenditures spent in the first two weeks of the benefit month, and for Panel B is an indicator for whether 100% of expenditures occured in the first week. Models 2,4,6, and 8 interact car ownership with the grocery store concentration index. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the month's benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.13: Impact of car loss: Over distance, all months

	(1)	(2)	(3)	(4)	(5)	(6)
A. % spent in first week						
No car	0.9	0.6	5.2*	0.9	0.2	-0.3
	(6.7)	(1.3)	(2.9)	(5.1)	(0.9)	(2.1)
Index × whether no car	0.01			-0.05		
	(0.6)			(0.4)		
I(0.5) × whether no car		0.1			-0.3	
		(2.4)			(1.6)	
I(1.5) × whether no car			-5.3*			0.5
			(2.9)			(2.2)
N	15662	15662	15662	15662	15662	15662
B. Whether 100% spent in first week						
No car	0.003	0.02	0.05	0.04	0.004	0.01
	(0.07)	(0.02)	(0.04)	(0.06)	(0.01)	(0.02)
Index × whether no car	0.002			-0.003		
	(0.006)			(0.005)		
I(0.5) × whether no car		-0.003			-0.03	
		(0.03)			(0.02)	
I(1.5) × whether no car			-0.03			-0.02
			(0.04)			(0.03)
N	15662	15662	15662	15662	15662	15662
Household fixed effects				X	X	X

Notes: Each column is a different regression of an indicator for whether a household owns a car and controls on the outcome specified in italics. The outcome for Panel A is the percent of expenditures spent in the first two weeks of the benefit month, and for Panel B is an indicator for whether 100% of expenditures occured in the first week. Models 2,4,6, and 8 interact car ownership with the grocery store concentration index. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the month's benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.14: Impact of car loss: Over distance, Pre-ARRA

	(1)	(2)	(3)	(4)	(5)	(6)
A. % spent in first week						
No car	-0.4	-0.7	9.1*	3.5	-1.4	-0.3
	(12.8)	(2.2)	(5.1)	(11.5)	(2.0)	(4.3)
Index × whether no car	0.02			-0.4		
	(1.1)			(1.0)		
I(0.5) × whether no car		2.8			1.9	
		(4.0)			(3.2)	
I(1.5) × whether no car			-10.6**			-0.8
			(5.3)			(4.5)
N	5485	5485	5485	5485	5485	5485
B. Whether 100% spent in first week						
No car	0.06	-0.0006	0.1*	0.2*	-0.04	0.04
	(0.2)	(0.03)	(0.07)	(0.1)	(0.03)	(0.05)
Index × whether no car	-0.004			-0.02**		
	(0.01)			(0.01)		
I(0.5) × whether no car		0.04			0.01	
		(0.05)			(0.05)	
I(1.5) × whether no car			-0.1*			-0.09*
			(0.07)			(0.05)
N	5485	5485	5485	5485	5485	5485
Household fixed effects				X	X	X

Notes: Each column is a different regression of an indicator for whether a household owns a car and controls on the outcome specified in italics. The outcome for Panel A is the percent of expenditures spent in the first two weeks of the benefit month, and for Panel B is an indicator for whether 100% of expenditures occured in the first week. Models 2,4,6, and 8 interact car ownership with the grocery store concentration index. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the month's benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.15: Impact of car gain on monthly percent of expenditures at convenience stores

	(1)	(2)	(3)	(4)	(5)	(6)
Whether car	12.7	0.2	3.2	6.4**	-0.4	1.1
	(7.8)	(1.2)	(3.5)	(2.7)	(0.8)	(2.1)
Index × whether have car	-1.1*			-0.6**		
	(0.7)			(0.3)		
I(0.5) × whether have car		-3.2*			-0.8	
		(1.8)			(1.0)	
I(1.5) × whether have car			-4.3			-1.9
			(3.6)			(2.2)
N	5901	5901	5901	5901	5901	5901
Household fixed effects				X	X	X

Notes: Each column is a different regression of an indicator for whether a household owns a car and controls on the percent of monthly expenditures spent at convenience stores or ethnic stores. Models 1 and 4 interact car ownership with the grocery store concentration index, models 2 and 5 with an indicator for whether there is a grocery store within 0.5 miles, and 3 and 6 with an indicator for whether there is a grocery store within 1.5 miles. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the month's benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.16: Impact of car loss on monthly percent of expenditures at convenience stores

	(1)	(2)	(3)	(4)	(5)	(6)
No car	-10.0**	1.1	-0.07	0.6	1.3*	2.7*
	(5.0)	(0.8)	(1.6)	(2.4)	(0.7)	(1.5)
Index × whether no car	0.9**			0.07		
	(0.5)			(0.2)		
I(0.5) × whether no car		1.0			-0.6	
		(2.7)			(1.1)	
I(1.5) × whether no car			1.5			-1.7
			(1.9)			(1.6)
N	5485	5485	5485	5485	5485	5485
Household fixed effects				X	X	X

Notes: Each column is a different regression of an indicator for whether a household owns a car and controls on the percent of expenditures spent at convenience or ethnic stores. Models 1 and 4 interact car ownership with the grocery store concentration index, models 2 and 5 with an indicator for whether there is a grocery store within 0.5 miles, and 3 and 6 with an indicator for whether there is a grocery store within 1.5 miles. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the month's benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, the number of times in the benefit month the household was issued benefits, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 3.17: Impact of car ownership: Placebos

	Outcome	
	%. first week	100% first week
	(1)	(2)
A. Dependent variable: Whether household owns car		
Outcome	-0.0001	-0.008
	(0.00009)	(0.007)
B. Dependent variables: Outcomes		
Household has car	0.2	0.0004
	(0.6)	(0.008)
Has car, +1 month	-0.7	-0.01
	(0.7)	(0.009)
Has car, +2 months	-0.04	0.002
	(0.7)	(0.009)
Has car, +3 months	0.1	0.004
	(0.6)	(0.007)
N	46219	46219

Notes: Each column of Panel A and each column of Panel B is a different regression. The regressions in Panel A use the outcome variables to predict whether a household owns a car; only the coefficient on the outcome variable is shown. Panel B show the car ownership coefficients from a regression of current car ownership and car ownership in the subsequent three months on the respective outcome. Standard errors for all regressions are clustered at the household level. In addition to access measures to all other four store formats, regressions control for month-year fixed effects, household size, household racial composition (black, Hispanic, and other), household age composition (percent under 18, percent 18-30, percent 31-45, percent 46-65, percent 66-81, percent over 81), whether the household is headed by a single parent, income, amount of benefits, whether the benefit issuance occured on a Friday, the extra benefits due only to SNAP policy changes, the length of the benefit month, number of consecutive months on SNAP, one- and three-month lagged tract average store format shares and tract average share of expenditures spent in first two weeks of the month, mean tract income, and tract % SNAP participants. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Chapter 4

Labor Market Outcomes and Food Assistance for Able-Bodied Adults in the U.S.

4.1 Introduction

A strong social safety net provides clear benefits for potential recipients, but a major concern is that it may decrease incentives to work. This has generated much public debate on the possibility of tying safety net participation with employment through the use of work requirements. Previous research on work requirements have focused on cash welfare requirements imposed as a result of the welfare reform efforts in the mid-1990's. This research has suggested that work requirements increase employment but not necessarily income or other outcomes (Karoly, 2001). The Supplemental Nutrition Assistance Program (SNAP) also imposes a work requirement on certain populations, which creates significant monetary and other costs on states to administer (Czajka *et al.*, 2001). Hoynes and Schanzenbach (2012) find significant work disincentives due to SNAP especially among low-income single mothers, suggesting a potential role for a work requirement. The SNAP work requirement, however, is imposed on a very different population: able-bodied adults without dependents. The effect of the SNAP work requirement on this population is not currently known.

We estimate the labor supply impact of SNAP availability for able-bodied adults without dependents (ABAWDs), a population that is required to work to receive benefits for more than 3 months out of 36. Each state is able to apply for waivers to the work requirement for certain areas (which may vary from a few counties to the entire state), if they have relatively high unemployment. Many areas received waivers over the years, while almost all restirctions were lifted in 2009, as part of the American Recovery and Reinvestment Act (ARRA). We use this variation to examine the impact of relaxing the work requirement on ABAWD employment and work intensity. One of the intended effects of a social safety net is to allow displaced workers the space to retool; without a work requirement, it is plausible that ABAWDs find other productive uses of time that enable better employment in the future. We therefore also estimate the impact of relaxing the work requirement on ABAWD educational time use - part-time enrollment in formal schooling as well as vocational training.

We combine two primary data sets to measure the impact of SNAP work policy on ABAWDs. The Current Population Survey (CPS) provides data on employment, hours worked, educational enrollment, and location. To identify the waiver status of ABAWDs in the CPS, we also collect a rich database of SNAP ABAWD policies between 2005 and 2009 from a number of different sources and match this dataset to the CPS. This policy database allows us to identify each ABAWD's waiver status, and also includes several other facets of SNAP ABAWD policies that are expected to influence the impact of the work requirement waivers. We take advantage of the fact that the CPS uses the same dwelling multiple times in its sampling, and use within-person variation in waiver status over time to estimate changes in labor supply and educational outcomes. We also examine heterogeneity in within-individual impacts over time and over local characteristics such as the minimum wage.

Our results show that waiver implementation decreases the probability of being employed among low-income ABAWDs by 3-5 percent, with larger impacts among poor ABAWDs most at risk to be eligible for SNAP, such as having less than a high school education. We find similar decreases in the number of weekly hours worked and the probability of working over 20 hours per week conditional on not working full-time. Furthermore, we find that these impacts disappear by three months after waiver implementation for individual ABAWDs, even conditional on still being in poverty.

Finally, we observe that individuals in areas with high minimum wages exhibit no evidence of waivers being a work disincentive, and employment may even increase due to waivers in areas with high minimum wages. We find no impact on educational time use.

In addition to estimating contemporary effects of work requirements for the SNAP program, our paper contributes to current knowledge on the labor supply impacts of SNAP.[1,2] Perhaps the best evidence on SNAP work disincentives comes from the quasi-experimental results of Hoynes and Schanzenbach (2012), who estimate the labor supply response during the original rollout of the Food Stamp Program throughout the 1960's and 1970's. They find no impact on the population in general, but a significant decrease in work effort among households headed by a single female.

We estimate the labor supply response to the SNAP work requirement, which adds to Hoynes and Schanzenbach's (2012) results in three ways. First, we look at response to a current policy option (imposing or waiving a work requirement), rather than introducing the Food Stamp program entirely. Second, since we look at a current policy option, we use current SNAP program policy and contemporary economic conditions, which could be significantly different from when the Food Stamp program was first rolled out. Third, due to the nature of the SNAP work requirement we estimate labor supply response for a very policy-relevant population. ABAWDs, in contrast to single mothers, have arguably fewer reasons to decrease work effort even in the absence of a work requirement. General welfare impacts of single mother work effort are less clear than those of ABAWDs: greater single mother employment has been shown to lead to less breastfeeding and less time reading to children (Herbst, 2017), lower child test scores and greater behavioral problems (Herbst and Tekin, 2010), greater likelihood of childhood obesity (Herbst and Tekin, 2012), and worse parent-child interactions (Herbst and Tekin, 2014).

The rest of the paper is structured as follows. First, we review current knowledge on ABAWDs and their SNAP participation. We then turn to a description of our individual and policy datasets and sources. After that, we describe our estimation strategy and discuss causal identification of the impact of the work requirements. We next present our results, and end with a discussion of limitations and implications.

[1]The original name of SNAP was the Food Stamp Program. Since most of the literature refers to this name, we will here use "SNAP" and the Food Stamp Program interchangably.

[2]See Moffitt (2002) for a review of the literature.

4.2 Literature Review: ABAWDs and SNAP

Due to the unique circumstances of ABAWDs as well as the unique policies targeting them, ABAWDs tend to have a different relationship to SNAP than the rest of the caseload. Those who are eligible are less likely to participate in SNAP than other households (Farrell and Gibbs, 2003; MacKernan and Ratcliffe, 2003). There are mixed results regarding how responsive ABAWD participation is to economic factors: Gleason *et al.* (1998) and Kornfeld (2002) find that economic conditions are influential in ABAWD SNAP participation, while Currie and Grogger (2001) find that SNAP participation of single-person households - a large proportion of which are ABAWDs - is less responsive to economic conditions than other households.

The work requirement may change how ABAWDs interact with SNAP. Wilde *et al.* (2000) and Ziliak, Gunderson, and Figlio (2003) use state-level caseload data and find that the share of ABAWDs covered by a waiver is associated with a higher caseload, though this result is not robust to different specifications (Wilde *et al.*, 2000). In a similar vein, Mulligan (2012) uses SNAP Quality Control data to estimate that ARRA waivers increase total SNAP participation by 2.3 percent. Danielson and Klerman (2006), however, used state-level caseload data and a broader index measure representing the severity of ABAWD time-limit policies, and found no effect of ABAWD policies on caseloads. Ribar *et al.* (2010) use administrative data from South Carolina to analyze the length of time and frequency that adult-only households received SNAP benefits. They find that households who were subject to ABAWD time limits received benefits for a shorter amount of time. These results are mirrored by state caseload data, which show significant ABAWD caseload increases soon after waivers take effect (Minnesota Department of Human Services, 2010) and large caseload decreases when work requirements are re-implemented (Rosenbaum, D., and B. Keith-Jennings, 2016). No study that we are aware of directly measures the impact of work requirements on employment or other outcomes in the ABAWD population.[3]

[3]Kramer-LeBlanc *et al.* (1997) use pre-PRWORA information on non-working ABAWDs to estimate the impact of losing Food Stamp benefits, assuming that there is no work response to the requirement.

4.3 Data

4.3.1 CPS

To measure the impact of work requirement waivers on ABAWD outcomes, we require a dataset that will identify potential ABAWDs and provide sufficient geographic and time detail to identify ABAWDs in waived areas/months. The CPS[4] surveys housing units throughout the US, and allows us to observe basic demographic, work, and educational characteristics for monthly samples of individuals. Key to our estimation strategy, the CPS follows housing units over time in a rotating panel. A household is interviewed for four consecutive months in one year, and then in the same four consecutive months in the next year before rotating out of the sample. Each monthly sample contains households that have been respondents for anywhere from one to eight months, over a span of up to 18 months. Since housing units are followed across time, individuals are generally identifiable across survey months (Drew *et al.*, 2014). Since families can move and be replaced in the survey by new families in the same housing unit, we restrict our sample to ABAWDs whose race and sex do not change, and whose age difference from first survey to last survey is under two years. In addition to labor supply variables and part-time schooling status for each month, we obtain vocational training enrollment information from the October education supplement and SNAP participation from the December food security supplement for each year.

We define an ABAWD as any person between the ages of 18 and 49 who has no one under 18 and no one over 65 in the household, who is not out of the labor force due to a disability, and who is a citizen and not in school full-time. We include the last two criteria in the definition to exclude people who are generally ineligible for SNAP (non-citizens), or who are not subject to the work requirement (full-time students). We restrict the sample to households with no elderly to exclude households where the ABAWD may be taking care of an older household member. Our definition is very similar to that of others who have used non-administrative data to identify ABAWDs.[5] To target the population of ABAWDs most likely to be eligible for SNAP, we further restrict our analysis to ABAWDs who are under 150 percent of poverty for the entire

[4]We obtain CPS data from every month January 2005 to March 2010 through IPUMS-CPS (King *et al.*, 2010).

[5]See Appendix C.1 for a summary of how other studies using survey data have defined ABAWDs.

time they are observed.

The definition of an ABAWD that is feasible with CPS data may differ from the SNAP definition of ABAWDs in two ways: The CPS household definition does not necessarily correspond to the SNAP household unit, and an individual whom the state SNAP program classifies as disabled may not necessarily be out of the labor force due to a disability. To investigate the impact of these definitional differences on the size of the ABAWD population, Figure 4.1 compares the percentage of SNAP recipients who are ABAWDs in the CPS data with the percentage in the SNAP Quality Control (QC) data over our period of interest.[6] The proportions track quite closely throughout our period, with ABAWDs making up generally between 3-6 percent of the total CPS SNAP caseload.

In the CPS, the geographic location of households is given at a number of different levels. For all observations, the state is identified. Within a state, large counties and most metropolitan statistical areas (MSA) are identified. For many MSAs, the CPS also identifies individuals as living either in a principal city of the MSA or a non-principal city area (balance). Finally, some specific cities are identified in large MSAs with multiple principal cities. Using all levels of identification, we classify as closely as possible each individual in the CPS as living in a waived area or unwaived area in that month. Comparing the CPS geographies with the geographies that each state uses to waive areas, we create stable geographic definitions across time to which we can assign a waiver status.[7] Since we restrict our sample to individuals that did not move in or out of a housing unit during the time that the individual is observed in the CPS, each individual's waiver status across time is therefore identified using the same geographic definition.[8]

The CPS allows us to examine both labor supply and educational outcomes. We measure whether an individual is employed as an indicator that takes 1 if the individual reported being at work last week or has a job but was not at work last week. To measure work intensity, we use the total hours worked in all jobs over the past week.[9]

[6] We restrict the QC ABAWD sample to ABAWDs in households without any elderly. The CPS ABAWD sample in this figure is not restricted to those under 150 percent of poverty.

[7] See Appendix C.2 for more information on the area definitions.

[8] We are unable to identify the waiver status of individuals living in areas for which we have missing waiver data, and for individuals living in areas with very heterogeneous waiver status. We are missing waiver information for Georgia in 2008, Louisiana in 2006, Maine in 2008, and West Virginia in 2006. Of our total CPS sample, 88 percent is assigned to an area definition and therefore a waiver status.

[9] We do not include in our analyses the 2 percent of individuals for whom the CPS hours worked data

Since working 20 hours or more per week is considered "employed" according to work requirement policy,[10] we might suspect that ABAWDs subject to the work requirement would work just over 20 hours. If they would prefer to work under 20 hours, relaxing the work requirement would allow ABAWDs to decrease their work intensity to below the 20 hour per week requirement. Our final labor supply outcome is then an indicator for whether an individual worked under 20 hours in the past week, conditional on working between 0 and 39 hours in each month they are in our sample.

Our two educational outcomes are whether an individual is enrolled in high school or university part-time, and whether the individual is enrolled in a vocational training program. Individuals enrolled in school at least half time are not required to meet the work requirement, but ABAWDs enrolled under half time are subject to the work requirement. A waiver might allow ABAWDs more freedom to enroll in school part-time or take part in a vocational training program. On the other hand, a waiver might induce ABAWDs to participate less in SNAP E&T programs, decreasing participation in classes or training programs. The basic monthly CPS asks whether an individual is enrolled in school full-time or part-time. While this is an imperfect measure of half-time enrollment, we estimate the impact of a waiver on part-time enrollment in a high school or college/university. Each October the CPS fields the Education Supplement, which includes a question on whether the individual is enrolled in a vocational training program. This allows us to measure - with a smaller sample size since it comes only from the October supplement - the impact of waivers on vocational training enrollment. We expect that this is mostly likely to pick up any decrease in SNAP E&T activity due to waiver implementation.[11]

It should be noted that we estimate reduced form waiver impacts without estimating the "first stage" impact on ABAWD SNAP participation. We choose not to estimate the first stage because the CPS entails notoriously under-reported SNAP participation (Meyer and Mittag, 2015). This under-reporting may be greater or less among ABAWDs, and it is difficult to assess any results on SNAP participation without understanding more about ABAWD SNAP under-reporting. On the other hand, the CPS is the premier

are imputed.

[10]See Section 4.3.2.

[11]Note that this decrease may be due either to lack of incentives on the part of the ABAWD or to states discontinuing SNAP E&T programs in the waived areas.

U.S. labor force survey, and we expect any labor force results to have minimal problems with misreporting.

4.3.2 ABAWD policy

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) restricted ABAWDs to three months of SNAP benefits in a 36-month period unless they are employed 20 or more hours per week (80 monthly hours total), participating in qualifying SNAP Employment and Training (E&T) components, or participating in workfare.[12] States have significant leeway in deciding how extensive the state SNAP E&T program is, whether participation is mandatory for ABAWDs, and how many workfare positions to offer. A state can further choose to pledge to provide an E&T or workfare position for all ABAWDs at risk of losing SNAP eligibility. States which make this pledge ("pledge states") share $20 million in federal funds to fund the necessary E&T program costs.[13]

There are two policies by which an ABAWD may be exempted or waived from the work requirement. First, states can request a waiver from the Food and Nutrition Service (FNS) for jurisdictions with an insufficient number of jobs. Up until 2009, states initiated waiver requests for sub-state areas with high unemployment; these areas could be individual cities, counties, multi-county regions, or even the entire state. The FNS could - and sometimes did - deny all or parts of waiver requests. In April, 2009, the American Recovery and Reinvestment Act (ARRA) waived restrictions for the entire country due to high levels of unemployment. Only a few areas (all in pledge states) opted to continue enforcing the work requirement.

The second policy allows states to exempt from the work requirement - without FNS approval - up to 15 percent of the ABAWDs who would otherwise be subject to the work requirement. States have complete flexibility in deciding how to use their 15 percent exemptions: some states use the 15 percent policy to exempt ABAWDs in certain areas where FNS waiver requests were denied, other states exempt ABAWDs based on age or other demographic criteria, and some allocate 15 percent exemptions to individual

[12] Appendix C.3 provides details on the definition of an ABAWD as well as waiver and exemption details.

[13] 14 states make this pledge at some point between 2005-2010.

counties to use them as they see fit.

This study therefore collected data on FNS approved waivers as well as state and local policies that would impact how binding the work restrictions are for ABAWDs. Information on ABAWD policies between 2005 and 2009 come from a number of different sources, summarized in Appendix C.4. The FNS provided documentation of the full history of waiver requests and outcomes (approval/denial) for each year between 2005 and 2009. However, this information does not cover exemptions that states awarded by using their 15 percent allocation. To obtain information on state 15 percent usage and other local policies, we first surveyed state SNAP programs. This survey yielded at least partial data on how the state combines federal waivers with local options for enforcing the work restriction for 18 states. For states from which we did not receive sufficient information, we searched for further information on the state's SNAP policy and legislative websites. We were able to add at least partial 15 percent exemption information for 13 states.

As noted, some states deliberately used their 15 percent exemptions to cover areas that were marginally ineligible for federal waivers, or certain populations in such areas. Where this was the case, the exempt areas or populations were coded directly into our exemptions data. For 24 states we were unable to determine 15 percent exemption usage. This is either because we do not have the policy information available (if our data source is only the FNS), or because the state does not use geographic or demographic criteria for allocating exemptions. We therefore supplement our policy data with state-month level 15 percent exemption usage, obtained from the FNS.

Between 2009 and 2010, most areas are waived due to the ARRA, although some areas refused to implement FNS waivers. Since many of these decisions were implemented at the state/county/city level with little input from FNS, this post-ARRA waiver history was obtained through an internet search of the relevant states' SNAP policy and legislative websites. Combined with the previous sources, we have waiver and (to some extent) exemption status for all months 2005-2010.

In addition to waivers and exemptions, the SNAP work requirement might be less binding for those states which have pledged to provide a SNAP E&T spot for all ABAWDs, or which provide greater numbers of E&T spots without a pledge. We therefore obtained from FNS the list of pledge states, federal pledge allocations, and state E&T participation numbers for the fiscal years which overlap our years of interest

4.3.3 Other data

We supplement our detailed SNAP ABAWD policy data with state minimum wage information (Urban Institute and Brookings Institution, Tax Policy Center, `http://www.taxpolicycenter.org/statistics/state-minimum-wage-rates-1983-2014`). In addition, we obtain monthly unemployment rates for the geographic areas we are able to identify in the CPS from the Bureau of Labor Statistics, Local Area Unemployment Statistics. Finally, we include state-month SNAP policy data from the SNAP policy database (United States Department of Agriculture, Economic Research Service, `https://www.ers.usda.gov/data-products/snap-policy-database`), which is available through December 2011.

4.4 Estimation and identification strategy

4.4.1 Benchmark model

Our benchmark model estimates each of the five outcomes as a function of the waiver status of the area in which the individual lives W_{at},[14] year fixed effects τ_t, individual fixed effects η_i, time-varying individual $\mathbf{X}_{it}$, area $\mathbf{X}_{at}$, and state characteristics $\mathbf{X}_{st}$:

$$Y_{it} = \alpha W_{at} + \beta_1 \mathbf{X}_{it} + \beta_2 \mathbf{X}_{at} + \beta_3 \mathbf{X}_{st} + \eta_i + \tau_t + \varepsilon_{it}, \tag{4.1}$$

In this framework, α is an unbiased estimator if the area's waiver status W_{at} is uncorrelated with time-varying unobserved drivers of labor market outcomes ε_{it}, conditional on the other covariates.

One threat to this assumption comes from the possibility of unobserved time-varying factors influencing both the timming of waivers and individual outcomes. We are worried primarily about three sources of these factors. First, local labor market conditions

[14] W_{at} is defined as a binary variable for whether over 50 percent of the area's 2010 Census population is covered by a waiver in that month. We use a binary variable instead of the the percent of the population covered by a waiver because most of the area-months have percents of either 0 or 100: 83 percent of area-months in our sample have fully homogenous waiver status, and 94 percent of area-months have homogenous waiver status for over 80 percent of the area's population. The binary waiver definition simplifies coefficient interpretation while losing very little policy information.

are a basis for an area's waiver status, and worsening labor markets mean ABAWDs would be less likely to work. To account for this, we control for area contemporaneous unemployment rate.[15] The second source of unoberved heterogeneity stems from the fact that a state's waiver and exemption strategy fits in with the state's broader SNAP strategy, which could influence ABAWD labor supply outside of waiver status. We therefore also control for characteristics of the state SNAP program[16] and whether the state is a pledge state. Finally, the ARRA increased SNAP benefits in addition to waiving the work requirement, and was implemented in 2009 as a result of the recession when many other factors might have been changing for ABAWDs. We therefore estimate additional models restricted to pre-ARRA years.

One final concern is that waiver status in our data may be measured with systematic error. This could be the case for two reasons. First, our waiver status measure captures 15 percent exemptions for only some states. If conditions in these states are changing in different ways from states for which we have imperfect exemption information, our results might be biased. Second, our waiver status measure is a binary measure for whether most of the population in that area was covered by a waiver. Economic conditions in areas for which we have full confidence that an individual is waived might change in different ways from areas that we know only an individual's waiver status with a certain probability. We explore these concerns more fully in Appendix C.5, and find that our results are larger in absolute value for individuals in areas for which we have full 15 percent exemption information but that the results are not different for individuals in areas with a non-homogenous waiver status.

While we restrict our analysis to ABAWDs under 150 percent of poverty, further subpopulations of ABAWDs might be more impacted by SNAP policy. We therefore estimate separate models for minority (non-white) ABAWDs under 150 percent of poverty and ABAWDs under 150 percent of poverty with less than a high school education

[15]To further account for local economic conditions, in analyses not shown here we included the state's housing price index, obtained from Zillow. Our results are robust to the inclusion of the housing price index. We do not include this index in our final analysis because it is missing for a number of states.

[16]SNAP program characteristics that we include are: whether the state uses broad-based categorical eligibility, whether the state uses a SNAP application combined with the Supplementary Security Income application, whether SNAP participants are disqualified for not meeting other non-SNAP welfare requirements, whether a telephone interview is sufficient, whether the state allows an online application, the total amount of outreach expenditures, and whether the state uses simplified reporting. Note that a state's SNAP characteristics can and do change over time.

(“low-educated ABAWDs”).

4.4.2 Testing impact heterogeneity

Characteristics of the state policy environment are likely to impact how binding the SNAP work requirement is for ABAWDs. Since pledge states choose to provide all ABAWDs with qualifying work opportunites, we would expect that changes in an area's waiver status would have less of an impact on ABAWD employment than in non-pledge states. We therefore test whether the impact is different for pledge state-months[17] by interacting the waiver status with an indicator variable for whether the individual lives in a pledge state-month (P_{st}):[18]

$$Y_{it} = \alpha_1 W_{at} + \alpha_2 W_{at} \times P_{st} + \beta_1 \mathbf{X}_{it} + \beta_2 \mathbf{U}_{at} + \beta_3 \mathbf{X}_{st} + \eta_i + \tau_t + \varepsilon_{it}, \tag{4.2}$$

State SNAP E&T programs vary widely even among non-pledge states. Some E&T components satisfy the work requirement, so ABAWDs in states with more widely-available or comprehensive E&T programs could plausibly have an easier time fulfilling the requirement without formal employment. Thus waiving the work requirement would have a smaller impact, since fewer ABAWDs were being required to work when the requirement was binding. To test this, we interact the waiver impact with the state-month SNAP E&T slots filled, following the form of Equation 4.2.

In addition to state SNAP policy, minimum wages differ by state and over time. Since ABAWDs are characterized by low skill and low education, the minimum wage provides a plausible opportunity cost of unemployment. A higher minimum wage would increase the cost of unemployment, so we would expect that waiving the work requirement in states-months with higher minimum wages would decrease the potential negative impact on employment. In a third specification, we therefore interact the waiver impact with whether the state-month has a minimum wage between 101 percent and 140 percent of the prevailing federal minimum wage (“medium minimum wage”) and whether the state-month minimum wage is over 140 percent of the federal minimum wage (“high

[17] States choose to pledge each fiscal year. A few states decided to pledge all years; more states pledged in only a few of the years in our sample.

[18] We include the level of the test variable P_{st} in the vector of state-month controls $\mathbf{X}_{st}$.

minimum wage").[19] We estimate separate models testing impact heterogeneity for all years and for just pre-ARRA years.

4.4.3 Event study model

We also allow the waiver impact to vary over time in an event study framework by adding a series of variables for lagged waiver status and future waiver status. Controlling for current waiver status, if waivers have an increasing impact over time we would expect lagged waiver status to be negatively associated with current outcomes. On the other hand, if there are no unobservables determining both waiver status in a particular month and an individual's outcome, future waiver status should be uncorrelated with the contemporaneous outcome.

To implement the event study framework, we first group months into quarters and define quarters relative to the current month ("Quarter 0"). Quarter -1 includes months -1 to -3. Quarter -2 includes months -4 to -6, quarter -3 includes months -7 to -9, quarter -4 includes months -10 to -12, and quarter -5 includes months -13 through -15. The lead quarters are defined similarly, where for example quarter 1 includes months 1 through 3.[20] A quarter is waived if any of the (lagged or lead) months are waived. We therefore estimate:

$$Y_{iq} = \sum_{j=-5}^{5} \alpha_j W_{i,t+j} + \beta_1 \mathbf{X}_{it} + \beta_2 \mathbf{U}_{at} + \beta_3 \mathbf{X}_{st} + \eta_i + \tau_t + \varepsilon_{it}, \tag{4.3}$$

where $W_{i,t+j}$ is the waiver status of the lagged-j quarter, and the coefficients of interest are the α_j. We restrict the sample to low-income ABAWDs with less than high school education in pre-ARRA years who live in areas that only gained a waiver once in pre-ARRA years, and we exclude pledge states.[21]

[19] Over 140% identifies the top 75th percentile of the minimum wage distribution. The interactions are relative to the waiver impact in low minimum wage states, or those with a minimum wage equal to the prevailing federal minimum wage. We also include the levels of whether the individual lives in a medium or high minimum wage state.

[20] Results are robust to whether the current month is included as a separate pseudo-quarter 0, in quarter -1, or in quarter 1.

[21] Due to the small sample size, the vocational training event study regression uses the full sample of low-income ABAWDs over all years.

4.4.4 Placebo models

The event study specification provides an initial placebo test by testing whether future waiver status predicts current outcomes. We provide further placebo tests by restricting the sample to subsamples who are less likely to have been subject to the SNAP work requirement: ABAWDs over 250 percent of poverty, ABAWDs with at least some college education, and individuals who live in households with children under 18. High-income and highly-educated ABAWDs are less likely to be eligible for SNAP and therefore have work decisions constrained by the work requirement, and individuals in households with children are by definition not ABAWDs and are not subject to the work requirement.

4.5 Results

4.5.1 Descriptive

Table 4.1 shows the means of the control variables for non-ABAWD adults, all ABAWDs, and ABAWDs under 150 percent of poverty. ABAWDs in general are wealthier, younger, better-educated, and are more likely to be white and less likely to be Hispanic than non-ABAWDs. However, ABAWDs under 150 percent of poverty are more likely to be non-white and have less education than the non-ABAWD population. Almost 60 percent of ABAWDs under 150 percent of poverty have a high school education or less, compared with just over 30 percent for ABAWDs in general and just under 50 percent for non-ABAWDs.

In Table 4.2, we show the means of the outcome variables for ABAWD-months in waived and unwaived areas, restricting the sample to low-income ABAWDs with waiver variation over time. Waived ABAWD-months exhibit lower employment and fewer hours of work than unwaived ABAWD-months. This is consistent with waivers being used in areas and at times of higher unemployment. There is no difference in part-time school enrollment and vocational training enrollment between waived and unwaived ABAWD-months.

4.5.2 Benchmark model

Table 4.3 shows the results for the benchmark model for all ABAWDs under 150 percent of poverty, ABAWDs under 150 percent of poverty with less than a high school education, and minority ABAWDs under 150 percent of poverty. Models 4-6 restrict the sample further to only pre-ARRA years. Each cell reports the coefficient α on the variable indicating whether the individual was covered by a waiver in that month.

Panel A shows consistent evidence that a waiver decreases the probability that a low-income ABAWD is employed. Overall, waivers decrease the probability of employment for low-income ABAWDs by 3 to 5 percent. Low-educated and minority ABAWDs respond more strongly to waiving the SNAP work requirement than low-income ABAWDs in general. This likely reflects the fact that low-educated and minority ABAWDs are more likely to participate in SNAP and therefore be effected by a change in SNAP policy. Waivers in pre-ARRA years have a larger effect than in all years, and cause a decrease in the probability of employment by up to 10 percent in populations most likely to participate in SNAP.

The impact of waivers on weekly hours worked, displayed in Panel B, tells a similar story. Waivers decrease hours worked in the overall low-income ABAWD population between 0.5 and 1.5 hours, though these impacts are not statistically significant. Hours worked decreases primarily in minority and low-educated low-income ABAWD populations, which are most likely to participate in SNAP, with larger effects before the ARRA. Waivers decrease weekly hours worked by approximately 2-5 hours in minority and low-educated ABAWDs. Panel C shows the impact of waivers on the indicator for whether the ABAWD worked over 20 hours, conditional on working less than full-time. Waiver implementation leads to a reduction of up to 12 percent in the probability that an ABAWD works over 20 hours per week, with larger impact prior to the ARRA and in minority and low-educated populations.

Panels D and E show the impact of waivers on whether the ABAWD is enrolled part-time in school or a vocational training program. Waivers have no impact on whether an individual enrolls part-time in school - the estimated coefficient is both very small and statistically insignificant. While statistically insignificant - possibly due to the very small sample size of ABAWDs that we observe from October to October with variation in waiver status and vocational training - waivers have a heterogeneous impact on vocational training. Waivers decrease the probability of vocational training in the overall low-income ABAWD population, but low-educated poor ABAWDs increase participation in vocational training due to waivers. Prior to the ARRA, waivers increased the probability of vocational training participation among low-educated ABAWDs by almost 50 percent, though we hesitate to emphasize this given the substantial uncertainty surrounding the estimate.

4.5.3 Impact heterogeneity

We investigate heterogeneity in our results in Table 4.4, which displays the coefficients on the waiver status and the interaction between waiver and relevant test variable as defined in Section 4.4.2. Since the results discussed in the previous section indicated that the waiver impacts were concentrated in populations most likely to participate in SNAP, we restrict the sample for this analysis to minority ABAWDs under 150 percent of poverty.

Looking across Panels A-E in Table 4.4, we see first that being in a pledge state does not appreciably change the waiver impact. The number of state SNAP E&T slots has a similarly small and statistically insignificant impact on how the waiver influences ABAWD outcomes.[22] When considering how the minimum wage interacts with waiver status, however, a robust story emerges: A higher minimum wage can completely swamp any negative impacts of waivers on labor market outcomes. In areas with a low minimum wage, waivers decrease the probability of employment by up to 14 percent, the number of hours worked by 3, and the probability of working over 20 hours by 17 percent. A high minimum wage, in contrast, eliminates the negative impact of a waiver on employment and work intensity: The cumulative waiver impact on the probability of employment in areas with a high minimum wage is -0.01 for all months and 0.024 in pre-ARRA months, and is statistically insignificant.[23] Though not statistically significant, a similar story is evident for hours worked[24] and whether the ABAWD worked over 20 hours per

[22] The mean level of total E&T participants divided by 100,000 is 0.18. Thus for example the waiver impact over all months at the mean SNAP E&T levels on the probability of employment is -0.07, on the number of weekly hours worked is -0.98, and on the probability of working over 20 hours is -0.01.

[23] The standard error on the cumulative waiver impact in high minimum wage areas for all months is 0.04 and has a p-value of 0.785 on the two-tailed test of the null that the cumulative impact equals zero. Similar results hold for the pre-ARRA cumulative impact.

[24] The waiver impact for all months in high minimum wage states on weekly hours worked is a

week.[25]

4.5.4 Event Study

Figure 4.2 displays the event study coefficients α_j from Equation 4.3.[26] We observe very small and statistically insignificant impacts of future waiver status on employment and hours worked, which provides confidence in our identification strategy. While multicollinearity between the lagged and future waiver status means that the coefficients are often not statistically significantly different from zero, having a waiver in the current month decreases the probability of being employment by almost 20 percent and decreases hours worked by approximately 3. A current waiver slightly decreases the probability of working over 20 hours, but we do not emphasize this result due to a potential upward trend in the future waiver status. Part-time schooling and vocational training display instability in future waiver status and little discernable effect around waiver implementation. Interestingly, vocational training dips substantially two quarters prior to waiver implementation. One potential explanation for this could be states ramping down E&T programs, which can include vocational training components, in anticipation of a waiver implementation. We note, however, that we did not find policy or anecdotale documentation of such activity.

The waiver appears to mainly have a short-term impact: conditional on current waiver status, having a waiver in the previous quarter actually increases the probability of being employed in the current quarter. This lagged increase in probability swamps much of the previous decrease in employment. A similar trend is evident for hours worked and whether the individual worked over 20 hours per week.

4.5.5 Placebo samples

Table 4.5 shows the results of running the benchmark model on our placebos. Column 1 uses a sample of ABAWDs with incomes over 250% of poverty, column 2 uses a sample

statistically-insignificant -0.11 hours, with a standard error of 1.68.

[25] The waiver impact for all months in high minimum wage states on the probability of working over 20 hours per week is a statistically-insignificant 0.03, with a standard error of 0.04.

[26] We estimate Equation 4.3 using the minority ABAWD sample, which provides us qualitatively similar results to the low-educated sample while increasing the sample size. A larger sample size helps in teasing apart often very strongly-correlated relationships between lagged and future waiver status.

of ABAWDs who have at least some college education, and column 3 uses a sample of adults in households with children. The waiver has tiny and mostly statistically insignificant impacts on populations most unlikely to participate in SNAP. While the waiver coefficient is statistically significant in predicting part-time school enrollment among ABAWDs with some college and adults with children, the effect sizes are miniscule. These placebos indicate that the waiver did not have an impact on households who were (mostly) ineligible to participate in SNAP and so should not have been directly impacted by the waiver.

4.6 Conclusion

In this paper we test the extent to which ABAWDs respond to the SNAP work requirement, which has generated significant policy discussion. We find that waiving the work requirement decreases ABAWD employment by up to 10 percent, reduces weekly hours worked by up to 4 hours among individuals most likely to participate in SNAP, and some evidence of a reduction in the probability of working over 20 hours per week. The impact is short-term, however, and is substantially smaller in the first quarter after a waiver is implemented. Higher minimum wages substantially decrease the impact of waivers on employment and work intensity. We find no effect on part-time schooling and vocational training.

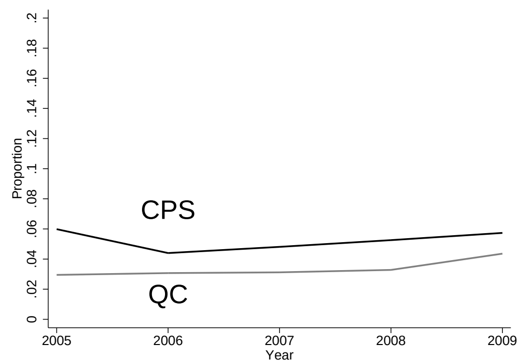
Our results contribute to the discussion surrounding work disincentives of SNAP and other social safety net policies, as well as the impact of work requirements designed to mitigate any work disincentives. We note a few limitations of our approach. First, the CPS only imperfectly allows for identification of ABAWDs. If our ABAWD definition includes many non-ABAWDs who would not be subject to the work requirement, our results may provide a lower bound for the impact on "true" ABAWDs. Second, ABAWDs who are eligible to participate in SNAP may not be fully captured by surveys such as the CPS. ABAWDs are often transient, creating difficulties for programs that seek to remain in contact with them (Ohio Association of Foodbanks, 2014). ABAWDs may therefore be less likely to participate in the CPS and may be more likely to move during the CPS survey period, thus limiting our ability to observe ABAWDs in the CPS. Third, the CPS only allows researchers to follow individuals for up to one year. We therefore do not observe a specific ABAWD for any more than a year after gaining a waiver, which restricts our ability to infer long-term impacts.[27] Finally, our waiver data include imperfect information on 15 percent exemptions and related local policies. We find evidence that our identification strategy is valid, but this lack of comprehensive local information likely introduces error into our waiver status classification.

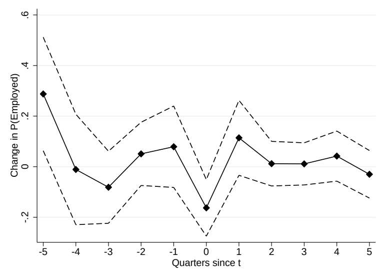
With these limitations in mind, we find that when ABAWDs are not required to work in order to participate in SNAP, in some cases they choose not to. We do not evaluate whether the drop in the probability of employment or the drop in weekly hours worked is in any way "large" or "small". However, the fact that ABAWDs respond differently to waivers based on the prevailing minimum wage - and that higher minimum wages imply smaller work disincentives - suggests that a work disincentive is not simply the case of an individual choosing to be "lazy." Instead, much of what we observe to be work disincentives due to the social safety net may be a result of a lack of proper incentives outside of the structure of the social safety net.

[27]The CPS does, however, provide a large sample of ABAWDs who are at varying lengths past waiver implementation, which we exploit in our event study.

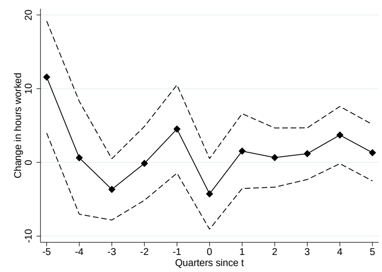
4.7 Figures and Tables

Figure 4.1: Comparison of % SNAP recipients who are ABAWDs: CPS and SNAP Quality Control (QC)

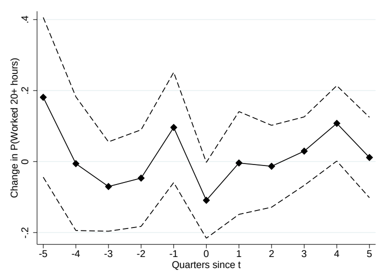




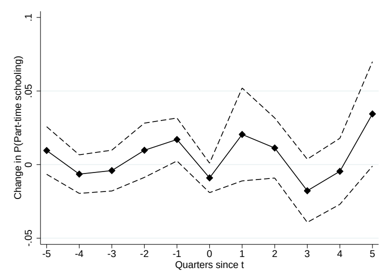
(a) Employed



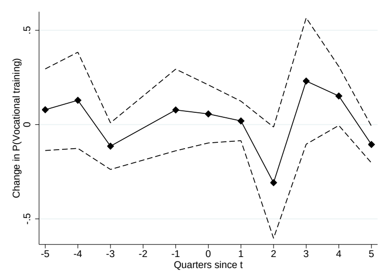
(b) Hours worked



(c) Whether worked above 20 hours



(d) Part-time school



(e) Vocational training

Figure 4.2: Estimated impact of current, future, and past waiver exposure

Table 4.1: Means and proportions describing the CPS sample

		ABAWDs	
	Non-ABAWDs	All	< 150% poverty
Age	33.82	33.83	31.23
	(9.24)	(9.45)	(9.94)
White	0.56	0.72	0.55
	(0.50)	(0.45)	(0.50)
Black	0.13	0.13	0.26
	(0.34)	(0.34)	(0.44)
Hispanic	0.22	0.09	0.13
	(0.42)	(0.29)	(0.34)
Income: under $20,000	0.10	0.07	0.85
	(0.30)	(0.26)	(0.36)
Income: $20,000-29,999	0.13	0.11	0.15
	(0.33)	(0.31)	(0.36)
Income: $30,000-49,999	0.17	0.18	0.00
	(0.37)	(0.39)	(0.06)
Income: $50,000-74,999	0.17	0.18	0.00
	(0.37)	(0.39)	(0.00)
Income: $75,000 or more	0.44	0.45	0.00
	(0.50)	(0.50)	(0.00)
Education: Less than high school	0.16	0.06	0.20
	(0.37)	(0.25)	(0.40)
Education: High school	0.29	0.29	0.39
	(0.45)	(0.46)	(0.49)
Education: Some college	0.30	0.30	0.27
	(0.46)	(0.46)	(0.44)
Education: College or higher	0.25	0.34	0.14
	(0.43)	(0.48)	(0.35)
Household size	4.01	2.14	2.01
	(1.47)	(0.97)	(1.04)
Observations	2169487	1073641	72876

Notes: This table compares means of the control variables in non-waived *vs* waived months for adult non-ABAWDs between 18 and 49 years old, all ABAWDs, and ABAWDs under 150% of poverty. Means are weighted by the CPS sampling weight; differences in the number of observations are due to sampling weights and missing values. Standard deviations are displayed in parentheses under the mean.

Table 4.2: ABAWD outcome means, by waiver status

	Not waived	Waived
A. Whether employed	0.66	0.61
	(0.47)	(0.49)
Number of ABAWD-months	2335	2310
Number of unique ABAWDs		939
B. Weekly hours worked	20.47	18.90
	(19.15)	(19.79)
Number of ABAWD-months	2138	2120
Number of unique ABAWDs		854
C. Whether worked 20+ hours	0.52	0.48
	(0.50)	(0.50)
Number of ABAWD-months	2335	2310
Number of unique ABAWDs		939
D. Whether enrolled in part-time school	0.01	0.01
	(0.10)	(0.08)
Number of ABAWD-months	2335	2310
Number of unique ABAWDs		939
E. Whether enrolled in vocational training	0.03	0.03
	(0.17)	(0.18)
Number of ABAWD-months	153	134
Number of unique ABAWDs		170

Notes: This table compares mean outcomes in non-waived *vs* waived months for ABAWDs under 150% of poverty with variation in work requirement waiver status over time. Means are weighted by the CPS sampling weight; differences in the number of observations are due to sampling weights and missing values. Standard deviations are displayed in parentheses under the mean. Also displayed are the number of individual ABAWDs that have variation in waiver status and no missing values for that variable.

Table 4.3: Impact of waivers: benchmark model

	All months			Pre-ARRA		
	All ABAWDs (1)	Low-educated (2)	Minority (3)	All ABAWDs (4)	Low-educated (5)	Minority (6)
A. Whether employed						
Waiver	-0.025	-0.092**	-0.069**	-0.048*	-0.10	-0.096*
	(0.018)	(0.044)	(0.032)	(0.027)	(0.065)	(0.050)
N	52,751	9,204	19,395	38,091	6,748	13,772
B. Weekly hours worked						
Waiver	-0.55	-3.65**	-1.29	-1.47	-5.73**	-2.03
	(0.73)	(1.56)	(1.26)	(1.01)	(2.36)	(1.87)
N	51,535	9,014	18,898	37,162	6,587	13,389
C. Whether worked 20+ hours						
Waiver	-0.013	-0.073*	-0.019	-0.048*	-0.12*	-0.047
	(0.019)	(0.041)	(0.029)	(0.026)	(0.061)	(0.041)
N	52,751	9,204	19,395	38,091	6,748	13,772
D. Whether enrolled in part-time school						
Waiver	-0.0019	-0.000071	-0.0023	-0.0058	-0.0028	-0.0085
	(0.0036)	(0.0031)	(0.0049)	(0.0055)	(0.0044)	(0.0078)
N	52,751	9,204	19,395	38,091	6,748	13,772
E. Whether enrolled in vocational training						
Waiver	-0.028	0.040	0.044	-0.036	0.47	-0.0098
	(0.042)	(0.045)	(0.035)	(0.071)	(0.32)	(0.055)
N	4,257	711	1,536	3,138	531	1,094

Notes: Each parameter is from a separate OLS regression of the outcome on whether the individual lived in an waived area in a particular month, using as samples all ABAWDs under 150% of poverty, ABAWDs under 150% of poverty with less than a high school education, and non-white ABAWDs under 150% of poverty. Models 4-6 are restricted to pre-ARRA years 2005-2008. Standard errors in parentheses are clustered at individual level and observations are weighted using average CPS survey weights. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 4.4: Impact heterogeneity

	All months			Pre-ARRA		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Whether employed						
Waiver	-0.069**	-0.060	-0.12**	-0.096*	-0.098	-0.14**
	(0.032)	(0.043)	(0.060)	(0.049)	(0.069)	(0.067)
Waiver × pledge state	0.014			-0.019		
	(0.083)			(0.11)		
Waiver × state E&T slots		-0.030			0.0057	
		(0.089)			(0.15)	
Waiver × high minimum wage			0.11*			0.17**
			(0.063)			(0.085)
Waiver × medium minimum wage			0.050			0.030
			(0.052)			(0.053)
N	19,395	19,395	19,395	13,772	13,772	13,772
B. Weekly hours worked						
Waiver	-1.28	-0.41	-2.58	-2.04	-0.97	-3.28
	(1.26)	(1.75)	(2.28)	(1.87)	(2.72)	(2.59)
Waiver × pledge state	-0.72			0.17		
	(2.46)			(3.24)		
Waiver × state E&T slots		-3.12			-3.76	
		(3.89)			(5.72)	
Waiver × high minimum wage			2.48			4.52
			(2.45)			(3.16)
Waiver × medium minimum wage			1.17			0.54
			(2.00)			(2.04)
N	18,898	18,898	18,898	13,389	13,389	13,389
C. Whether worked more than 20 hours						
Waiver	-0.018	0.00070	-0.069	-0.047	-0.016	-0.091
	(0.029)	(0.040)	(0.055)	(0.041)	(0.060)	(0.060)
Waiver × pledge state	-0.021			0.015		
	(0.076)			(0.099)		
Waiver × state E&T slots		-0.068			-0.11	
		(0.096)			(0.14)	

Continued on next page

Table 4.4 – continued from previous page

	All months			Pre-ARRA		
	(1)	(2)	(3)	(4)	(5)	(6)
Waiver × high minimum wage			0.099			0.14
			(0.063)			(0.083)
Waiver × medium minimum wage			0.046			0.031
			(0.053)			(0.055)
N	19,395	19,395	19,395	13,772	13,772	13,772
D. Whether enrolled in part-time school						
Waiver	-0.0024	-0.0032	-0.014	-0.0088	-0.012	-0.018
	(0.0049)	(0.0087)	(0.013)	(0.0078)	(0.016)	(0.014)
Waiver × pledge state	-0.00088			0.0078		
	(0.0092)			(0.012)		
Waiver × state E&T slots		0.0031			0.014	
		(0.022)			(0.032)	
Waiver × high minimum wage			0.025			0.028
			(0.018)			(0.018)
Waiver × medium minimum wage			0.010			0.0081
			(0.012)			(0.011)
N	19,395	19,395	19,395	13,772	13,772	13,772
E. Whether enrolled in vocational training						
Waiver	0.050	0.073	0.044	-0.025	0.034	0.0060
	(0.038)	(0.050)	(0.040)	(0.051)	(0.055)	(0.052)
Waiver × pledge state	-0.26			-0.32		
	(0.16)			(0.22)		
Waiver × state E&T slots		-0.16			-0.20	
		(0.16)			(0.20)	
Waiver × high minimum wage			-0.035			-0.046
			(0.055)			(0.067)
Waiver × medium minimum wage			0.016			0.0087
			(0.027)			(0.023)
N	1,536	1,536	1,536	1,094	1,094	1,094

Continued on next page

Table 4.4 – continued from previous page

	All months			Pre-ARRA		
	(1)	(2)	(3)	(4)	(5)	(6)

Notes: Each parameter is from a separate OLS regression of the outcome on whether the individual lived in an waived area in a particular month and an interaction between waiver status and the relevant variable, using the sample of non-white ABAWDs under 150% of poverty. Relevant variables are: whether the individual lives in a high-minimum wage state (>140% of the federal minimum wage), whether the individual lives in a mid-minimum wage state (between 100 and 140% of the federal minimum wage, exclusive), and the number of SNAP E&T participants in that state-month divided by 100,000. The minimum wage indicators are all relative to states with the minimum wage equal to the federal minimum wage. Models 4-6 are restricted to pre-ARRA years 2005-2008. Standard errors in parentheses are clustered at individual level and observations are weighted using average CPS survey weights. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Table 4.5: Impact of waivers: placebos

	High income (1)	High education (2)	With children (3)
A. Whether employed			
Waiver	-0.0024	-0.0033	0.00082
	(0.0026)	(0.0029)	(0.0017)
N	721,028	548,729	2,275,392
B. Weekly hours worked			
Waiver	-0.030	-0.11	0.059
	(0.14)	(0.16)	(0.068)
N	705,462	538,277	2,239,844
C. Whether worked 20+ hours			
Waiver	0.00044	0.00090	0.00093
	(0.0031)	(0.0036)	(0.0017)
N	721,028	548,729	2,275,392
D. Whether enrolled in part-time school			
Waiver	-0.00072	-0.0018*	-0.00072*
	(0.00082)	(0.0011)	(0.00043)
N	721,028	548,729	2,275,392
E. Whether enrolled in vocational training			
Waiver	-0.0057	-0.0046	0.0015
	(0.0053)	(0.0069)	(0.0027)
N	57,589	44,218	179,441

Notes: Each parameter is from a separate OLS regression of the outcome on whether the individual lived in an waived area in a particular month. The high income sample includes ABAWDs with incomes over 250% of poverty, the high education sample includes individuals with at least some college education, and the sample with children includes all adults in households with at least one child under 18. Standard errors in parentheses are clustered at individual level and observations are weighted using average CPS survey weights. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Chapter 5

Conclusions

This dissertation investigates how households respond to policies designed to improve access to food for low-income households.

In my first chapter, I ask whether a supply- or demand-focused policy is more effective at encouraging households to shop for food more at grocery stores *vs* convenience stores. I find that a demand-focused policy unambiguously increases shopping at grocery stores relative to convenience stores, but that neither policy reduces absolute shopping at convenience stores. My estimates allow me to quantify the tradeoff between the two policy levers. The results from this chapter highlight the role of small ethnic stores in the food environment for low-income urban SNAP participants. I also find substantially heterogenous impacts by car ownership. These results point to future research directions into how urban residents use small ethnic stores, and how car ownership and access influence a household's store choice decision.

My second chapter tests whether transaction costs contribute to the rate at which SNAP participants spend their benefits. I find that proximity to grocery stores can matter, but that the impact is likely nonlinear over distance. I also find that car ownership matters, but in somewhat unexpected ways.

Finally, my third chapter measures SNAP participant labor market responses to the SNAP work requirement. I find that waiving the work requirement has the largest impact on low-educated participants, but that the impact is short-lived. Importantly, I also find that a high minimum wage decreases the waiver impact substantially. This suggests that the work requirement may not be an important driver of employment if low-income workers are already paid enough. It also highlights the role of the broader labor market in determining work outcomes for SNAP participants.

References

[1] Allcott, H., R. Diamond, and J.P. Dube. 2015. The geography of poverty and nutrition: Food deserts and food choices across the United States. Working paper, July 20, 2015.

[2] Anas, A. 2007. A unified theory of consumption, travel and trip chaining. *Journal of Urban Economics* 62: 162-186.

[3] Anderson, P. and K.F. Butcher. 2016. *The relationships among SNAP benefits, grocery spending, diet quality, and the adequacy of low-income families' resources.* Center on Budget and Policy Priorities, Policy Futures report, 14 June 2016.

[4] Andrews, M., R. Bhatta, and M. Ver Ploeg. 2013. An alternative to developing stores in food deserts: Can changes in SNAP benefits make a difference? *Applied Economic Perspectives and Policy.* 35(1): 150-170.

[5] Baum, C. 2009. The effects of vehicle ownership on employment. *Journal of Urban Economics* 66: 151-163.

[6] Bernstein, J., and B. Spielberg. 2017. Why Medicaid work requirements won't work. *The New York Times* 22 March, 2017. Retrieved from https://www.nytimes.com.

[7] Bertrand, M., S. Mullainathan, and E. Shafir. 2004. A behavioral economics view of poverty. *The American Economic Review* 94(2): 419-423.

[8] Bhat, C., S. Handy, K. Kockelman, H. Mahmassani, Q. Chen, and L. Weston. 2000. *Development of an urban accessibility index: Literature review.* Center for Transportation Research, Bureau of Engineering Research, The University of Texas at Austin.

[9] Bitler, M., S.J. Haider. 2011. An economic view of food deserts in the United States. *Journal of Policy Analysis and Management* 30(1): 153-176.

[10] Black, C., G. Ntani, H. Inskip, C. Cooper, S. Cummins, G. Moon, and J. Baird. Measuring the healthfulness of food retail stores: variations by store type and neighborhood deprivation. *International Journal of Behavioral Nutrition and Physical Activity* 11(69)

[11] Black, J., and J. Macinko. 2008. Neighborhoods and Obesity. *Nutrition Reviews* 66(1): 2-20.

[12] Block, D. 2006. What fills the gaps in food deserts? Mapping independent groceries, food stamp card utilization and chain fast-food restaurants in the Chicago area. *Appetite* 47(3): 386.

[13] Broda, C., E. Leibtag, and D. Weinstein. 2009. The role of prices in measuring the poor's living standards. *Journal of Economic Perspectives* 23(2): 77-97.

[14] Cannuscio, C., K. Tappe, A. Hillier, A. Buttenheim, A. Karpyn, K. Glanz. 2013. Urban food environments and residents' shopping behaviors. *American Journal of Preventative Medicine* 45(5): 606-614.

[15] Caspi, C., K. Lenk, J. Pelletier, T. Barnes, L. Harnack, D. Erickson, M. Laska. 2017. Association between store environment and customer purchases in small grocery stores, gas-marts, pharmacies and dollar stores. *International Journal of Behavior Nutrition and Physical Activity* 14(76).

[16] Caspi, C., K. Lenk, J.E. Pelletier, T.L. Barnes, L. Harnack, D.J. Erickson, and M.N. Laska. 2017. Association between store environment and customer purchases in small grocery stores, gas-marts, pharmacies and dollar stores. *International Journal of Behavior Nutrition and Physical Activity* 14(76).

[17] Caspi, C., J. Pelletier, L. Harnack, D. Erickson, K. Lenk, and M. Laska. 2017. Pricing of staple foods at supermarkets versus small food stores. *International Journal of Environmental Research and Public Health* 14(8): 915

[18] Caspi, C., G. Sorensen, S.V. Subramanian, I. Kawachi. 2012. The local food environment and diet: A systematic review. *Health & Place* 18(5): 1172-1187.

[19] Castellari, E., C. Cotti, J. Gordanier, and O. Ozturk. 2016. Does the timing of food stamp distribution matter? A panel-data analysis of monthly purchasing patterns of US households. *Health Economics* 26(11): 1380-1393.

[20] Cleary, R., A. Bonanno, L. Chenarides, and S. Goetz. 2018. Store profitability and public policies to improve food access in non-metro U.S. counties. *Food Policy* 75: 158-170.

[21] Cottie, C., J. Gordanier, and O. Ozturk. 2016. When does it count? The timing of food stamp receipt and educational performance. Working Paper.

[22] Cummins, S., E. Flint, and S.A. Matthews. 2014. New neighborhood grocery store increased awareness of food access but did not alter dietary habits or obesity. *Health Affairs* 33(2): 283-291.

[23] Currie, J., J. Grogger, G. Burtless, and R.F. Schoeni. 2001. Explaining recent declines in Food Stamp Program participation [with comments]. In: Gale, W. and J. Rothenberg-Pack (eds.) *Brookings-Wharton Papers on Urban Affairs*, 203244.

[24] Currie, J., S. DellaVigna, E. Moretti, and V. Pathania. 2010. The effect of fast food restaurants on obesity and weight gain. *American Economic Journal: Economic Policy* 2: 32-63.

[25] Czajka, J.L., S. McConnell, S. Cody, and N. Rodriguez. 2001. Imposing a time limit on food stamp receipt: Implementation of the provisions and effects on Food Stamp Program participation, Volume 1. Mathematica Final Report, September 4, 2001.

[26] Damon, A., R. King, and E. Leibtag. 2013. First of the month effect: Does it apply across food retail channels? *Food Policy* 41: 18-27.

[27] Danielson, C., and J.A. Klerman. 2006. Why did the Food Stamp caseload decline (and rise?): Effects of policies and the economy. Institute for Research on Poverty Discussion Paper No. 1316-06, University of WisconsinMadison.

[28] Delaney, A. 2017. Food Stamp work requirements are kind of a sham: A closer look at the supposed success of work requirements in Kansas and Maine. *Huffington Post*, 12 July, 2017.

[29] Dorfman, J., C. Gregory, Z. Liu, and R. Huo. 2018. Re-examining the SNAP benefit cycle allowing for heterogeneity. *Applied Economics Perspectives and Policy*

ppy013.

[30] Drew, J.A., S. Flood, and J.R. Warren. 2014. Making full use of the longitudinal design of the Current Population Survey: Methods for linking records across 16 months. *Journal of Economic and Social Measurement* 39: 121-144.

[31] Einav, L., E. Leibtag, and A. Nevo. 2010. Recording discrepancies in Nielsen Homescan data: Are they present and do they matter? *Quantitative Marketing and Economics* 8(2): 207-239.

[32] Elbel, B., A. Moran, L.B. Dixon, K. Kiszko, J. Cantor, C. Abrams, and T. Mijanovich. Assessment of a government-subsidized supermarket in a high-need area on household food availability and children's dietary intakes. *Public Health Nutrition* 18(15): 2881-2890.

[33] Farrell, Mary, and Robert Gibbs. 2003. The relationship of earnings and income to Food Stamp participation: A longitudinal analysis. Washington, D.C.: Economic Research Service, U.S. Department of Agriculture.

[34] Foley, C. 2011. Welfare payments and crime. *The Review of Economics and Statistics* 93(1): 97-112.

[35] Glanz, K., J.F. Sallis, B.E. Saelens, L.D. Frank. 2007. Nutrition environment measures survey in stores (NEMS-S): Development and evaluation. *American Journal of Preventative Medicine* 32(4): 282-289.

[36] Gleason, Philip, Peter Schochet, and Robert Moffitt. 1998. *The dynamics of Food Stamp Program participation in the early 1990s.* Washington, D.C.: Food and

Nutrition Service, U.S. Department of Agriculture.

[37] Gordon-Larsen, P. 2014. Food availability/convenience and obesity. *Advances in Nutrition* 5: 809-817.

[38] Gregory, C., and T. Smith. forthcoming. Salience, food security, and SNAP receipt. *Food Policy.*

[39] Gurley, T., and D. Bruce. 2005. The effects of car access on employment outcomes for welfare recipients. *Journal of Urban Economics* 58: 250-272.

[40] Handbury, J., I. Rahkovsky, and M. Schnell. Is the focus on food deserts fruitless? Retail access and food purchases across the socioeconomic spectrum. Working paper, April 1, 2016.

[41] Hastings, J., and E. Washington. 2010. The first of the month effect: Consumer behavior and store responses. *American Economic Journal: Economic Policy* 2: 142-162.

[42] Hausman, J. and E. Leibtag. Consumer benefits from increased competition in shopping outlets: Measuring the effect of Wal-Mart. *Journal of Applied Econometrics* 22: 1157-1177.

[43] Herbst, C. 2017. Are parental welfare work requirements good for disadvantaged children? Evidence from age-of-youngest-child exemptions. *Journal of Policy Analysis and Management* 36(2): 327-357.

[44] Herbst, C., and E. Tekin. 2010. Child care subsidies and child development. *Economics of Education Review* 29: 618-638.

[45] Herbst, C., and E. Tekin. 2012. The geographic accessibility of child care subsidies and evidence on the impact of subsidy receipt on childhood obesity. *Journal of Urban Economics* 71: 37-52.

[46] Herbst, C., and E. Tekin. 2014. Child care subsidies, maternal health, and child-parent interactions: Evidence from three nationally representative datasets. *Health Economics* 23: 894-916.

[47] Hoynes, H.W., and D.W. Schanzenbach. 2012. Work incentives and the Food Stamp Program. *Journal of Public Economics* 96, 151-162.

[48] Huffman, D., and M. Barenstein. 2005. A monthly struggle for self-control? Hyperbolic discounting, mental accounting, and the fall in consumption between paydays. IZA Discussion Paper 1430.

[49] Inagami, S., D.A. Cohen, A.F. Brown, and S.M. Asch. 2009. Body mass index, neighborhood fast food and restaurant concentration, and car ownership. *Journal of Urban Health* 86(5): 683-695.

[50] Joassart-Marcelli, P., J. Rossiter, and F. Bosco. 2017. Ethnic markets and community food security in an urban "food desert". *Environment and Planning A* 49(7): 1642-1663.

[51] Karoly, L.A. 2001. Estimating the effect of work requirements on welfare recipients: A synthesis of the national literature. 1 October, 2001. Testimony before the Subcommittee on 21st Century Competitiveness, Committee on Education and the

Workforce, US House of Representatives.

[52] Kornfeld, Robert. 2002. *Explaining recent trends in Food Stamp Program caseloads: Final report.* Washington, D.C.: Economic Research Service, U.S. Department of Agriculture.

[53] Kramer-LeBlanc, C.S., P.P. Basiotis, and E.T. Kennedy. 1997. Maintaining food and nutrition security in the United States with welfare reform. *American Journal of Agricultural Economics* 79 (5): 1600-1607.

[54] Kuang, C. 2017. Does quality matter in local consumption amenities? An empirical investigation with Yelp. *Journal of Urban Economics* 100: 1-18.

[55] Kuhn, M. 2017. 2017. Who feels the calorie crunch and why?: The impact of school on the incidence of cyclical food insecurity. Working Paper March 10, 2017.

[56] Laska, M.N., C.E. Caspi, J.E. Pelletier, R. Friebur, L.J. Harnack. 2015. Lack of healthy food in small-size to mid-size retailers participating in the Supplemental Nutrition Assistance Program, Minneapolis-St. Paul, Minnesota, 2014. *Preventing Chronic Disease* 12: E135.

[57] Larson, N.I, M.T. Story, and M.C. Nelson. 2009. Neighborhood environments: Disparities in access to healthy foods in the U.S. *American Journal of Preventative Medicine* 36(1): 74-81.

[58] Leone, A.F., S. Rigby, C. Betterley, S. Park, H. Kurtz, M.A. Johnson, J.S. Lee. 2011. Store type and demographic influence on the availability and price of healthful foods, Leon County, Florida, 2008. *Preventing Chronic Disease* 8(6): A140.

[59] Martin, K.S., E. Havens, K.E. Boyle, G. Matthews, E.A. Schilling, O. Harel, and A.M. Ferris. If you stock it, will they buy it? Healthy food availability and customer purchasing behavior within corner stores in Hartford, CT, USA. *Public Health Nutrition* 15(10): 1973-1978.

[60] Mastrobuoni, G. and M. Weinberg. 2009. Heterogeneity in intra-monthly consumption patterns, self-control, and savings at retirement. *American Economic Journal: Economic Policy* 1(2): 163-189.

[61] Meyer, B., and N. Mittag. 2015. Using linked survey and administrative data to better measure income: Implications for poverty, program effectiveness and holes in the safety net. NBER working paper No. 21676, October 2015.

[62] Mullainathan, S., and E. Shafir. 2013. *Scarcity: Why having too little means so much*. New York: Times Books.

[63] Miriam King, Steven Ruggles, J. Trent Alexander, Sarah Flood, Katie Genadek, Matthew B. Schroeder, Brandon Trampe, and Rebecca Vick. 2010. Integrated Public Use Microdata Series, Current Population Survey: Version 3.0. [Machine-readable database]. Minneapolis, MN: Minnesota Population Center [producer and distributor].

[64] McKernan, Signe-Mary, and Caroline Ratcliffe. 2003. Employment factors influencing Food Stamp Program participation: Final report. Washington, D.C.: Urban Institute.

[65] Minnesota Department of Human Services. 2010. The growing food support caseload. *Evaluation Reports*: Issue 21.

[66] Moffit, R. 2002. Welfare programs and labor supply. In: Auerbach, A.J. and Feldstein, M. (eds.), *Handbook of Public Economics*. Amsterdam: North Holland.

[67] Ohio Association of Foodbanks. 2014. *Franklin County Work Experience Program: Comprehensive report*. Columbus, Ohio. Retrieved from http://admin.ohiofoodbanks.org/uploads/news/ABAWD_Report_2014-2015-v3.pdf

[68] Ratcliffe, C., S.M. McKernan, and K. Finegold. 2008. Effects of Food Stamp and TANF policies on Food Stamp receipt. *Social Service Review* 82(2): 291-334.

[69] Ribar, D.C., M. Edelhoch, and Q. Liu. 2010. Food Stamp participation among adult-only households. *Southern Economic Journal* 77(2): 244270.

[70] Rosenbaum, D., and B. Keith-Jennings. 2016. SNAP caseload and spending declines accelerated in 2016: Return of three-month time limit a factor in many states. Center on Budget and Policy Priorities. December 29, 2016.

[71] Ruff, R.R., A. Akhund, T. Adjoian. 2016. Small convenience stores and the local food environment: An analysis of resident shopping behavior using multilevel modeling. *American Journal of Health Promotion* 30(3): 172-180.

[72] Seligman, H., A. Bolger, D. Guzman, A. Lopez, and K. Bibbins-Domingo. 2014. Exhaustion of food budgets at month's end and hospital admissions for hypoglycemia. *Health Affairs* 33(1): 116-123.

[73] Shapiro, J. 2005. Is there a daily discount rate? Evidence from the food stamp nutrition cycle. *Journal of Public Economics* 89: 303-325.

[74] Smith, T., J. Berning, X. Yang, G. Colson, and J. Dorfman. 2016. The effects of benefit timing and income fungibility on food purchasing decisions among Supplemental Nutrition Assistance Program households. *American Journal of Agricultural Economics* 98(2): 564-580.

[75] Stephens, M. 2006. Paycheque receipt and the timing of consumption. *The Economic Journal* 116: 680-701.

[76] Stern, D., S.W. Ng, and B. Popkin. 2016. The nutrient content of U.S. household food purchases by store type. *American Journal of Preventative Medicine* 50(2): 180-190.

[77] Todd, J. 2015. Revisiting the Supplemental Nutrition Assistance Program cycle of food intake: Investigating heterogeneity, diet quality, and a large boost in benefit amounts. *Applied Economic Perspectives and Policy* 37(3): 437-458.

[78] United States Department of Agriculture (USDA). 2010. Supplemental Nutrition Assistance Program (SNAP) American Recovery and Reinvestment Act Plan Update, 6/23/2010. Accessed July 10, 2017, at `www.fns.usda.gov/sites/default/files/SNAP_ARRA-Plan.pdf`.

[79] ver Ploeg, M., L. Mancino, J. Todd, D. Clay, B. Scharadin. 2015. Where do Americans usually shop for food and how do they travel to get there? Initial findings from the National Household Food Acquisition and Purchase Survey. United States Department of Agriculture, Economic Research Service, Economic Information Bulletin Number 138.

[80] Volpe, R., E. Jaenicke, and L. Chenarides. 2017. Store formats, market structure, and consumers' food shopping decisions. *Applied Economic Perspectives and Policy*

doi:10.1093/aepp/ppx033

[81] Walker, R.E., C.R. Keane, J.G. Burke. 2010. Disparities and access to healthy foods in the United States: A review of food deserts literature. *Health & Place* 16(50): 876-884.

[82] Wilde, P.E., P. Cook, C. Gundersen, M. Nord, and L. Tiehen. 2000. *The decline in Food Stamp Program participation in the 1990's.* Food Assistance and Nutrition Research Report No. 33793. Washington, D.C.: Economic Research Service, U.S. Department of Agriculture.

[83] Wilde, P., and C. Ranney. 2000. The monthly food stamp cycle: Shopping frequency and food intake decisions in an endogenous switching regression framework. *American Journal of Agricultural Economics* 82(1): 200-213.

[84] Zhen, C., J. Taylor, M. Muth, and E. Leibtag. 2009. Understanding differences in self-reported expenditures between household scanner data and diary survey data: A comparison of Homescan and Consumer Expenditure Survey. *Review of Agricultural Economics* 31(3): 470-492.

[85] Ziliak, J.P., C. Gundersen, and D. N. Figlio. 2003. Food Stamp caseloads over the business cycle. *Southern Economic Journal* 69(4): 903919.

Appendix A

Consumer Choice of Store Format: Response to Policy Incentives

A.1 Theoretical framework: Details

In the *second stage*, the household maximizes utility[1] subject to the store-specific budget constraint:

In the convenience store, the household solves

$$\underset{h}{\text{maximize}} \quad U(b, t_l) \quad \text{subject to} \quad p_{cb}h \leq Y$$

which yields Marshallian demand for candy bars $B_c(p_{cb}, Y)$.[2]

In the grocery store, the household solves

$$\underset{h,k}{\text{maximize}} \quad U(b, k, t_l) \quad \text{subject to} \quad p_{gb}h + p_{gk}k \leq Y$$

yielding Marshallian demands $B_g(p_{gb}, p_{gk}, Y)$ and $K_g(p_{gb}, p_{gk}, Y)$.

[1]For notational convenience, in the lower stage we suppress the additive stochastic term.

[2]Since there is only one good, $B_c(p_{cb}, Y) = \frac{Y}{p_{cb}}$. Since we do not assume any functional form, specifying this does not add anything to the model. We thus continue to use $B_c(p_{cb}, Y)$ instead.

Thus a trip to the convenience store provides the household with indirect utility

$$V^c = V(B_c(p_{cb}, Y), T - t_c) + \varepsilon_c,$$

and a trip to the grocery store provides the household indirect utility

$$V^g = V(B_g(p_{gb}, p_{gk}, Y), K_g(p_{gb}, p_{gk}, Y), T - t_g) + \varepsilon_g$$

In the *first stage*, the household compares indirect utilities, and will choose to visit the grocery store if $V^g > V^c$.

A.1.1 Comparative statics

Setup

Let the probability that a household chooses the grocery store over the convenience store to be Pr_g. Following the standard discrete choice model setup, this probability is the cumulative distribution function F of $\varepsilon_c - \varepsilon_g$:

$$\begin{aligned} Pr_g =& Pr(V(B_g(p_{gb}, p_{gk}, Y), K_g(p_{gb}, p_{gk}, Y), T - t_g) + \varepsilon_g - V(B_c(p_{cb}, Y), T - t_c) - \varepsilon_c > 0) \\ =& Pr(V(B_g(p_{gb}, p_{gk}, Y), K_g(p_{gb}, p_{gk}, Y), T - t_g) - V(B_c(p_{cb}, Y), T - t_c) > \varepsilon_c - \varepsilon_g) \\ =& F_{(\varepsilon_c - \varepsilon_g)}(V(B_g(p_{gb}, p_{gk}, Y), K_g(p_{gb}, p_{gk}, Y), T - t_g) - V(B_c(p_{cb}, Y), T - t_c)) \end{aligned} \tag{A.1}$$

Letting $D_g \equiv V(B_g(p_{gb}, p_{gk}, Y), K_g(p_{gb}, p_{gk}, Y), T - t_g) - V(B_c(p_{cb}, Y), T - t_c)$ and $\varepsilon = \varepsilon_c - \varepsilon_g$, we summarize (A.1) as

$$Pr_g = F_\varepsilon(D_g). \tag{A.2}$$

We now look at the effect of opening a grocery store - which would decrease the time necessary to get to the grocery store t_g - and increasing household resources - which would increase the food budget Y - on the probability of visiting a grocery store given in Equation A.2.

Impact of changing access

A grocery store opening close to the household decreases the time that it takes to get to a grocery store.[3] As long as the household continues to derive utility from having extra leisure time, it is more likely to visit the grocery store than before. Formally, let $\tilde{t_g} = -t_g$ denote the decreasing distance to the grocery store. The probability of visiting the grocery store is increasing with decreasing distance if:

$$\begin{aligned} \frac{\partial Pr_g}{\partial \tilde{t_g}} &= \frac{\partial F_\varepsilon}{\partial D_g} \times \frac{\partial D_g}{\partial \tilde{t_g}} \\ &= \frac{\partial F_\varepsilon}{\partial D_g} \times V^g_{t_l} \end{aligned}$$

where $V^g_{t_l}$ is the marginal utility of leisure time, conditional on going to the grocery store. Note that cumulative distribution functions are non-decreasing and thus $\frac{\partial F_\varepsilon}{\partial D_g} \geq 0$. The effect of a grocery store opening closer to a household therefore crucially depends on $V^g_{t_l}$. As long as the marginal utility of leisure time is positive, both terms on the right hand side are non-negative and $\frac{\partial Pr_g}{\partial \tilde{t_g}} \geq 0$. Thus the probability of going to the grocery store weakly increases when a closer grocery store opens.

While t_g changes when stores open and close, we would also expect t_g also to be smaller for households with a car. In the empirical work, we therefore estimate the relationship between Pr_g and distance to the grocery store separately for households with and without cars.

Impact of increasing resources

Next, consider a policy of increasing the household's food budget Y through, for example, raising SNAP benefit levels. While the ultimate decision of whether to visit the grocery store or the convenience store will still depend on the indirect utilities that include leisure time as well as purchases, the impact of giving a household extra money depends on what it does with that extra money. Thus the household compares the utility from the kale and candy bars that the extra money enables it to purchase at the grocery store

[3] We assume here that all grocery stores offer the same products for the same prices, so the household will only consider the closest grocery store. Relaxing this assumption creates a multinomial choice model between three stores which complicates the comparison but does not provide further intuition into the expected policy effects.

with the utility from the candy bars that can be purchased from the convenience store with that extra money.

Formally, we can investigate the impact of increasing the food budget on the probability of going to the grocery store by differentiating Equation A.2 with respect to Y:

$$\begin{aligned} \frac{\partial Pr_g}{\partial Y} &= \frac{\partial F_\varepsilon}{\partial D_g} \times \frac{\partial D_g}{\partial Y} \\ &= \frac{\partial F_\varepsilon}{\partial D_g} \times (V_b^g \frac{\partial B_g}{\partial Y} + V_k^g \frac{\partial K_g}{\partial Y} - V_b^c \frac{\partial B_c}{\partial Y}), \end{aligned} \tag{A.3}$$

where V_b^g and V_k^g are the marginal utilities of candy bars and kale, respectively, conditional on going to the grocery store, V_b^c is the marginal utility of candy bars conditional on going to the convenience store, and we suppress the arguments of the Marshallian demands B_g, K_g, and B_c. Since $\frac{\partial F_\varepsilon}{\partial D_g} \geq 0$, the impact of increasing the food budget depends on how the extra resources influence the difference between the (indirect) utilities derived from a grocery store visit and a convenience store visit $\frac{\partial D_g}{\partial Y}$, which in turn depends on the marginal propensity to consume candy bars at the convenience store, candy bars at the grocery store, and kale at the grocery store, as well as the relative impact that more candy bars and kale have on utility. There is no guarantee that $\frac{\partial D_g}{\partial Y} \geq 0$, so the impact of increasing the food budget on grocery store shopping is ambiguous. To illustrate the choice that the household faces, assume that the utility from candy bars and kale does not depend on the choice of store type,[4] i.e. $V_b^g = V_b^c = V_b$ and $V_k^g = V_k$. We can then rewrite Equation A.3 as:

$$\underbrace{V_k \frac{\partial K_g}{\partial Y}}_{\substack{\text{Opportunity cost} \\ \text{of convenience store}}} \geq \underbrace{V_b(\frac{\partial B_c}{\partial Y} - \frac{\partial B_g}{\partial Y})}_{\substack{\text{Opportunity cost} \\ \text{of grocery store}}} \tag{A.4}$$

Note that the opportunity cost of going to the grocery store after the increase in Y is the utility from the additional candy bars that the household would have bought at the convenience store relative to the grocery store, and the opportunity cost of going to the convenience store is the utility from the additional kale that would have been bought at the grocery store. Thus this condition states that the probability of going to the

[4]Thus there are no utility complementarities between kale, candy bars, and leisure time - more of one does not change the marginal utility of another.

grocery store after receiving extra resources is increasing if the opportunity cost of going to the grocery store is less than the opportunity cost of going to the convenience store. If $\frac{\partial B_c}{\partial Y} - \frac{\partial B_g}{\partial Y} < 0$, that is, the household would choose to buy more candy bars at the grocery store than the convenience store with the extra money, then as long as kale is a normal good ($\frac{\partial K_g}{\partial Y} > 0$) Equation A.4 will always be true, and an increase in the food budget will unambiguously increase the probability of going to the grocery store. This could be the case, for example, if convenience store food is significantly more expensive than the equivalent grocery store food.

A.2 Data undergirding store access definitions

We use two sources of data to characterize a household's food store access. Our primary source is the transaction data, from which we create a dataset of all the stores visited by SNAP households each month.[5] There are two reasons a store might be observed in one month but not the next in our transaction data: either the store does not exist, or no household visited that store in that month. Stores with a large number of transactions can be identified with confidence as existing in any particular month. There is more uncertainty for stores that are visited by a handful of people. If the few people who visited the store in the previous month do not to visit the store in the next month, for example, we do not observe the store, although it is still open. To avoid misclassifying these stores as not existing for those months, we supplement our store dataset with a list of stores and authorization dates for all stores in our city that were authorized to accept SNAP benefits. We use the transaction data to assign store-month availability for all stores with over 50 average monthly transactions, as whether we observe these stores is less likely to be impacted by the actions of just a few households. We use the comprehensive list of stores and authorization dates to assign monthly availability for all stores with fewer than 50 average monthly transactions.[6] In our data a few large stores are authorized to accept SNAP before households start visiting or after households stop visiting (in the case of a store closing). The transaction data are therefore our preferred

[5]We define a "store" as a store format-coordinate combination. This enables us to avoid mis-identifying store name or ownership changes as store supply changes.

[6]This includes the four percent of stores in the comprehensive list that were open but never visited by any household in our dataset.

source for information for stores with enough transactions per month to provide a stable time series of store availability.

A.3 Comparison of stores in transaction data to all food retail stores

To compare the number of stores that appear in our data to the total possible food stores available, we use establishment counts at the county level. We obtain total numbers of establishments using County Business Patterns (CBP) for each county in our area of interest. Table B1 displays the number of SNAP establishments in each county as a percent of the number of CBP establishments in 2009 (results are the same using other years). Results are shown for all establishments overall, and then for different establishment categories. Retail food stores are defined here as all establishments that are either grocery stores (NAICS 2007 code 4451), specialty (NAICS 4452), drug (NAICS 44611), gasoline stations with convenience stores (NAICS 44711), and warehouse clubs and supercenters (NAICS 45291). While these are the categories of stores found in our SNAP data, two categories may be less likely to accept SNAP benefits and also less important to the shopping needs of low income households: Specialty stores include meat, fish, and fruit and vegetable markets, and only make up a very small share of the total SNAP expenditures, while many drug stores and pharmacies may not carry food. Thus, we provide alternate percentages, where these categories are taken out of both the CBP and SNAP store count data. Counties 5 and 7 are the most urban counties, and have the highest percent of stores represented in the SNAP data. For these populous counties, around 80%-100% of the possible stores are represented in our SNAP data. This coverage decreases significantly for less-populous counties.

Table C1: Number of SNAP transaction establishments as % of CBP establishment numbers, 2009 by county

	Overall	Non-specialty	Non-specialty, non-drug
County			
1	58.9	58.5	69.7
2	51.9	44.9	61.1
3	20	14.3	17.6
4	57.7	54.3	65.9
5	66.8	66.5	78.4
6	50	40.9	50
7	80.7	82.2	102.7
8	50.7	55.7	67.3
9	36	35.7	44.1
10	57.8	54.3	68.5
11	38.1	37.3	41.4

A.4 Comparison of concentration index definitions

	Grocery		Convenience		Discount		Ethnic		Other	
	Car	No car	Car	No car	Car	No car	Car	No car	Car	No car
A. Weight: Inverse of $distance^{\frac{1}{2}}$										
Index	0.8**	1.5***	0.3	-1.1***	-0.3	0.02	-0.5**	-0.7***	-0.3*	0.03
	(0.4)	(0.5)	(0.2)	(0.4)	(0.2)	(0.2)	(0.3)	(0.2)	(0.2)	(0.1)
B. Weight: Inverse of $distance^{\frac{1}{3}}$										
Index	1.0**	1.6***	0.4	-1.3***	-0.3	0.10	-0.7**	-0.8**	-0.4*	0.03
	(0.5)	(0.6)	(0.3)	(0.5)	(0.3)	(0.3)	(0.3)	(0.3)	(0.2)	(0.1)
C. Weight: Inverse of $distance^{\frac{1}{4}}$										
Index	1.2*	1.8**	0.5	-1.5***	-0.4	0.2	-0.8*	-1.0**	-0.5*	0.03
	(0.6)	(0.8)	(0.3)	(0.5)	(0.4)	(0.4)	(0.4)	(0.4)	(0.3)	(0.2)
D. Weight: Gaussian kernel, bandwidth 1 mile										
Index	3.3***	3.1**	0.6	-2.3***	-1.4*	-0.4	-1.8**	-1.8**	-0.8	0.2
	(1.1)	(1.3)	(0.7)	(0.9)	(0.8)	(0.8)	(0.8)	(0.8)	(0.5)	(0.4)
E. Weight: Gaussian kernel, bandwidth 3 miles										
Index	0.8	0.6	0.6*	-0.4	-0.2	0.08	-0.6	-0.6	-0.6*	-0.1
	(0.6)	(0.7)	(0.3)	(0.4)	(0.5)	(0.4)	(0.4)	(0.4)	(0.3)	(0.2)
F. Weight: Gaussian kernel, bandwidth 10 miles										
Index	-0.2	1.4	2.2***	-0.6	-0.3	0.2	-0.8	-1.8	-1.1	-0.3
	(1.6)	(2.1)	(0.8)	(1.2)	(1.2)	(1.3)	(0.9)	(1.3)	(0.8)	(0.6)

Notes: Each column is a different household fixed effect regression of access and controls on store format expenditure shares, using different methods of defining the store concentration index. Panel A weights each store by the inverse of $distance^{\frac{1}{2}}$, Panel B weights stores by the inverse of $distance^{\frac{1}{3}}$, Panel C weights stores by the inverse of $distance^{\frac{1}{4}}$, Panel D uses a Gaussian kernel with bandwidth of 1 mile, Panel E uses a Gaussian kernel with bandwidth 3 miles, and Panel F uses a Gaussian kernel with bandwidth of 10 miles. Standard errors are clustered at the household level. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Appendix B

The Impact of Access to Food Retail Stores on Food Assistance Spending Over the Month

B.1 Definition of benefit months

In Minnesota, regular benefit distribution is staggered over the 4th-13th of every calendar month.[1] The exact day that a household receives benefits is determined by the last digit of the household's administrative case number. We define the household's benefit month to be the time in between the first benefit issuance in a particular calendar month and the first benefit issuance in the next calendar month. For a household that regularly receives benefits on the 4th of the month, therefore, the household's May benefit month (for example) will consist of the days between the 4th of May and the 4th of June.[2] Using the date of benefit disbursement, we calculate the number of days between the disbursement and each EBT transaction. We define the first 7 days of the benefit month to be the first week of the month.

[1] Households that are newly approved to receive SNAP benefits, or whose benefits are reinstated, receive benefits as soon as they are approved. Thus the initial benefit issuance will not necessarily be on the regular schedule, but all subsequent disbursements will follow the regular staggered schedule.

[2] If income or household size changes are reported too late in the month to be reflected in a current month's regular benefit issuance, additional benefits may be authorized during that month. A household can therefore receive benefits multiple times in the same benefit month. We restrict our analysis to months in which households received benefits only once.

B.2 Comparison of grocery store index definitions

In this section we compare the impact of grocery store proximity using different continuous measures of access, first varying the weight θ and then using a Gaussian kernel with different bandwidths. Panel A is the measure used in the paper. In general, indices that are more sensitive to smaller distances show a larger impact for households without cars. The index chosen for this paper is the most conservative of the indices.

Table B1: Impact of the grocery store concentration index on whether the household spends SNAP benefits in the last half of the month

	Dependent variables			
	% in first week		100% in first week	
	Car	No car	Car	No car
A. Weight: Inverse of distance$^{\frac{1}{2}}$				
Index	-0.4	0.7	-0.002	-0.001
	(0.5)	(0.4)	(0.006)	(0.006)
B. Weight: Inverse of distance$^{\frac{1}{3}}$				
Index	-0.5	0.9	-0.003	-0.002
	(0.7)	(0.6)	(0.008)	(0.007)
C. Weight: Inverse of distance$^{\frac{1}{4}}$				
Index	-0.6	1.2	-0.004	-0.002
	(0.8)	(0.8)	(0.010)	(0.009)
D. Weight: Gaussian kernel, bandwidth 1 mile				
Index	-2.2	1.3	-0.01	-0.003
	(1.6)	(1.4)	(0.02)	(0.02)
E. Weight: Gaussian kernel, bandwidth 3 miles				
Index	-0.4	1.2	-0.004	0.002
	(0.9)	(0.8)	(0.01)	(0.009)
F. Weight: Gaussian kernel, bandwidth 10 miles				
Index	0.5	3.4	-0.009	0.003
	(2.5)	(2.9)	(0.03)	(0.03)

Notes: Each column is a different household fixed effect regression of access and controls on the outcome, using different methods of defining the store concentration index. Panel A weights each store by the inverse of $distance^{\frac{1}{2}}$, Panel B weights stores by the inverse of $distance^{\frac{1}{3}}$, Panel C weights stores by the inverse of $distance^{\frac{1}{4}}$, Panel D uses a Gaussian kernel with bandwidth of 1 mile, Panel E uses a Gaussian kernel with bandwidth 3 miles, and Panel F uses a Gaussian kernel with bandwidth of 10 miles. Standard errors are clustered at the household level. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$

Appendix C

Labor Market Outcomes and Food Assistance for Able-Bodied Adults in the U.S.

C.1 ABAWD definitions

Kramer-LeBlanc *et al.* (1997) use the Continuing Survey of Food Intakes by Individuals (CSFII) 1989-1991 to estimate the impact of losing Food Stamps on non-working ABAWDs. They define an ABAWD household as one with no children under 18, no pregnant female, and no disabled household head, but including a household member between 18 and 50 years old who is not working or working under 20 hours per week.

Farrell *et al.* measure income and SNAP participation among demographic groups using the Survey of Income and Program Participation (SIPP). For their study, an ABAWD is an individual who is not disabled, has no children, and is between 18 and 50. Disability is determined as either having a physical or mental limitation that made them unable to work, or receiving Supplemental Security Income or disability payments.

McKernan and Ratcliffe (2003) measure employment and SNAP participation also using the SIPP. An ABAWD is an individual 18-50 in a household that does not contain any children, elderly, or disabled members.

Ratcliffe *et al.* (2008) use the SIPP to estimate the impact of Food Stamp policies on participation in the program. They define an ABAWD as an individual between 18 and 50 with no physical or mental work limitation, who live in a household with no children, no person over 60, and no disabled individuals.

Currie *et al.* (2001) estimate determinants of the (then-) declining Food Stamp participation using the March supplements of the Current Population Survey. Instead of directly defining ABAWDs, they recognize that most ABAWDs live alone and use their results for adults living alone to infer the impact on ABAWDs.

C.2 Area definitions

To the extent possible, area definitions are the smallest level identifiable in the CPS with a homogenous waiver status. Areas are defined to never cross state lines. The CPS geography identifiers allow for eight possible levels of areas: state, county, MSA,[1] MSA principal city, MSA balance, city,[2] state balance,[3] county-principal city,[4] and exceptions.[5] When an area exhibited a mixture of waiver levels, we used the 2010 Census populations of the constituent sub-areas to calculate the proportion of that area that is waived. As is apparent from the area definitions, the sizes of these areas vary greatly.

C.3 ABAWD policy details

An Able-bodied adult without dependent (ABAWD) is defined to be an individual between the ages of 18 and 49 who is not disabled and does not have any dependents. A dependant can be a child under 18 in the household, or an older incapacitated household member. An individual must receive medical certification that they are unable to work

[1]Properly, the part of the MSA in a specific state. This level is used when the entire MSA has homogenous waiver status across time, or if the principal city/balance is not identified for that MSA.

[2]The CPS occasionally identifies specific principal cities in large MSAs

[3]The state balance definition is the residual area of a state, after taking out the CPS-identified areas. This level is used (for the rare occasions) when the entire state is waived or unwaived across time, with the exception of some CPS-identifiable areas

[4]The county-principal city level is used for one city where the principal cities are split between counties, and have different waiver status.

[5]The exception level is used for parts of MSAs that, with the exception of another CPS-identified area, has the same waiver status. For example, the Indiana part of the Chicago MSA balance minus Lake county is never waived until the ARRA, whereas Lake County, Indiana is waived before the ARRA. So this region is split into a county area (Lake County) and an exception area.

due to a physical or mental condition to be exempt from the work requirement.

The FNS allows several definitions of an insufficient number of jobs that states can use to make waiver requests. Each year, the Department of Labors Employment and Training Administration (DOLETA) publishes a list of labor surplus areas, which can include counties, cities, and other equivalent sub-state jurisdictions. A jurisdiction is classified as a labor surplus area when its average unemployment rate is at least 20 percent above the average unemployment rate for the nation during the previous two calendar years, with a ceiling of 10 percent and a floor of 6 percent for periods of very high or low national unemployment. In addition, states may apply the same rule (20 percent over the national average over 24 months), but to loosely defined economic areas (which could include counties, cities, or parts of counties, as well as multiple-county economic regions). Waivers can also be requested for areas with unemployment of at least 10 percent, on average, over the last 12 or 3 months, if an area had a historically high seasonal unemployment, or had experienced rapid economic changes. Finally, if a state is eligible for extended unemployment benefits at any point, it is eligible for a statewide waiver for the ensuing fiscal year.

15 percent exemption allocations are calculated by the FNS based on the number of ABAWDs who would be subject to the work requirement, taking into account any waivers the state uses. Exemption allowances carry over from the previous year, allowing states to accumulate exemptions or to overuse exemptions and "pay back" those exemptions over time. Some states, such as New Jersey, exempt substate areas that were ineligible for FNS-approved waivers. Other states use the exemptions to give all ABAWDs additional months of benefits in the 36-month period. For example, Massachussetts in 2005 decided to use the 15 percent allocation to give all ABAWDs an additional three months of benefits. Exemptions can also be used for "older" ABAWDs, or ABAWDs who live in rural areas and are far from potential jobs or far from SNAP E&T or workfare placements. Pennsylvania, for example, allows caseworkers to exempt individuals who are exempt from participating in the E&T program due to distance or other hardship, and until 2004 exempted ABAWDs aged 47-49. Pledge states may strategically combine 15 percent exemptions and waivers to focus E&T effort on certain counties while exempting ABAWDs in others. Texas, for instance, uses 15 percent exemptions and waivers together to waive ABAWDs in "minimum service" counties where E&T services are limited. Some

states decide not to make a decision - California allocates 15 percent allocations to counties to use as they wish. Still other states decide not to use the 15 percent exemptions at all.

C.4 Sources for waiver and exemption policy information

Table D1: Sources of waiver and 15% exemption data for 2005-2009, by state

	Waiver	Exemption	
	FNS	Survey	Internet
Alabama	x		
Alaska	x		
Arizona	x		
Arkansas	x	x	x
California	x		
Colorado	x	x	
Connecticut	x		
Delaware	x		
District of Columbia	x	x	
Florida	x		x
Georgia	x		
Hawaii	x		
Idaho	x	x	x
Illinois	x		x
Indiana	x		
Iowa	x		
Kansas	x	x	
Kentucky	x	x	
Louisiana	x		
Maine	x		
Maryland	x	x	
Massachusetts	x		
Michigan	x		
Minnesota	x		
Mississippi	x		
Missouri	x	x	
Montana	x		x
Nebraska	x	x	
Nevada	x		
New Hampshire	x		
New Jersey	x	x	
New Mexico	x	x	
New York	x	x	

Continued on next page

	Waiver	Exemption	
	FNS	Survey	Internet
North Carolina	x		x
North Dakota	x	x	
Ohio	x	x	x
Oklahoma	x	x	
Oregon	x		
Pennsylvania	x		x
Rhode Island	x		
South Carolina	x		
South Dakota	x	x	x
Tennessee	x	x	
Texas	x		x
Utah	x		x
Vermont	x		
Virginia	x	x	
Washington	x		
West Virginia	x		
Wisconsin	x		x
Wyoming	x		x

C.5 Systematic measurement error of waiver status

To investigate whether imperfect policy information leads to systematic non-classical measurement error in waiver status, we run regressions following Equation **??**, using as P_{st} (1) an indicator for whether the individual lives in an area-month for which we were unable to incorporate full 15 percent exemption information, and (2) an indicator for whether the individual lives in an area-month with has non-binary waiver status (i.e. the percent of the population waived is not 0 and not 100). The results are shown in Table E2. The negative impact of waivers on employment and work intensity are driven by areas for which we have full 15 percent exemption information. The waiver impact is not different in areas with binary *vs* no-binary waiver status. Thus we conclude that our main results are not subject to bias due to error in the measurement of waiver status.

Table E2: Imperfect policy information

	All months		Pre-ARRA	
	(1)	(2)	(3)	(4)
A. Whether employed				
Waiver	0.022	-0.067	0.044	-0.065
	(0.068)	(0.049)	(0.10)	(0.074)
Waiver × full 15% info	-0.14*		-0.18	
	(0.077)		(0.12)	
Waiver × non-binary		-0.11		-0.14
		(0.097)		(0.12)
N	9,204	9,204	6,748	6,748
B. Weekly hours worked				
Waiver	1.96	-3.44**	6.30	-4.50*
	(3.78)	(1.71)	(6.21)	(2.53)
Waiver × full 15% info	-6.55		-14.4**	
	(4.12)		(6.68)	
Waiver × non-binary		-2.70		-5.32
		(4.12)		(5.25)
N	9,014	9,014	6,587	6,587
C. Whether worked more than 20 hours				

Continued on next page

Table E2 – continued from previous page

	All months		Pre-ARRA	
	(1)	(2)	(3)	(4)
Waiver	0.042	-0.064	0.13	-0.078
	(0.095)	(0.045)	(0.13)	(0.069)
Waiver × full 15% info	-0.14		-0.31**	
	(0.099)		(0.14)	
Waiver × non-binary		-0.093		-0.16
		(0.11)		(0.13)
N	9,204	9,204	6,748	6,748
D. Whether enrolled in part-time school				
Waiver	-0.0052	-0.000060	-0.0091	-0.0057
	(0.0056)	(0.0043)	(0.0080)	(0.0065)
Waiver × full 15% info	0.0061		0.0079	
	(0.0066)		(0.0079)	
Waiver × non-binary		0.014		0.023
		(0.022)		(0.027)
N	9,204	9,204	6,748	6,748
E. Whether enrolled in vocational training				
Waiver	0.019	0.034	0.47	0.47
	(0.046)	(0.053)	(0.32)	(0.31)
Waiver × full 15% info	0.023		0	
	(0.055)		(.)	
Waiver × non-binary		0.025		-0.019
		(0.067)		(0.081)
N	711	711	531	531

Notes: Each parameter is from a separate OLS regression of the outcome on whether the individual lived in an waived area in a particular month and an interaction between waiver status and the relevant variable, using the sample of low-educated ABAWDs under 150% of poverty. Relevant variables are: whether the individual lives in an area-month for which full 15% exemption information is unavailable, and whether the percent of an area's population waived is not 0% or 100% ("non-binary"). Models 3-4 restrict the sample to pre-ARRA years 2005-2008. Standard errors in parentheses are clustered at individual level and observations are weighted using average CPS survey weights. $^{***}p < 0.01^{**}p < 0.05^{*}p < 0.1$