DRAFT Dissertation Prospectus

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Contents

Dissertation Overview

Chapter 1 is an evaluation of the effectiveness of the Double Up Food Bucks program. "Effectiveness" will be defined by the change in total sales and volume of produce sold within 17 grocery stores that implement Double Up (treatment group). The control group comprises 15 stores where Double Up was not implemented. A difference-in-differences and regression discontinuity design will be used to measure the size of the effect.

Improving health and food equity of SNAP participants is the broader policy concern. The mechanism is a financial incentive—Double Up Food Bucks—designed to increase fruit and vegetables consumption. A comparison will be made with another financial incentive program called the *Healthy Incentives Pilot* (HIP). I will argue how and why an evaluation of the Double Up program is an important addition to the current literature.

Chapter 2 exploits the random assignment of the Durham Connects program to measure its long term impact on social service applications. Durham Connects (DC) provides free in-home nursing visits to residents of Durham County. It was implemented as an RCT between July 2009 to December 2010. The design treats DC as an information treatment. The DC information treatment (e.g. a nurse contact to ask questions and provide assistance) is expected to lower the learning and barrier costs of low income families with children when applying for social services.

Chapter 3 extends the work of Schenck-Fontaine et al. (2016). Through surveys, Schenck-Fontaine et al. (2016) examine how families coping with poverty and economic instability supplement their SNAP benefits with additional assistance from informal and formal resources. In my paper, I dig deeper into how and when families use additional formal resources beyond SNAP—such as food banks and emergency cash assistance. My aim is to provide a more detailed understanding of these role formal networks play in supporting Durham families coping with poverty.

Chapter 1

An Evaluation of the Double Up Food Bucks program

Motivation

Research Question

> How effective is the DUFB financial incentive at increasing the fresh fruits and vegetables purchased by SNAP shoppers within a grocery store environment?

Hypothesis 1: I expect a slight increase (1 - 2%) in SNAP EBT expenditures on fruits and vegetables (FVs) during the months the DUFB incentive is active in the experimental stores (Aug - Dec). I expect the effect will fade over time. I expect no significant differences in fruit and vegetable expenditures during the months the incentive is not in effect (Jan - July).

Note that the DUFB incentive is applied automatically to all SNAP shoppers earning or redeeming DUFB points (covered in more detail in the Data Section). Therefore, by default, all SNAP shoppers at a DUFB store are technically "participating". However, SNAP shoppers should be considered "passive" or "active" participants. "Active participation" would characterize shoppers responding to the DUFB incentive; "passive participation" would characterize shoppers who continue to shop as usual, despite automatically redeeming points.

From prior implementations of the program, participation rates in DUFB-like incentive programs can be low. In this case, I expect SNAP shoppers to consist of mostly "passive" participants. Combined with the fact that SNAP dollars account for less than 5% of all spending in these stores, I don't think there is a very large pool of SNAP participants to draw from. As a result, the "active" participants, who respond to the incentive and purchase more fruits and vegetables, will consist of a small fraction of total FV spending. Therefore, to capture this effect, it is necessary to focus purchases made by the targeted group (SNAP households).

I expect awareness of the DUFB program for this particular grocery chain appears to be low. This is includes both staff and the shoppers themselves. I don't know if the corporate office, which selected and implemented the DUFB incentive, clearly communicated or advertised the DUFB program to store management/staff and customers. As a result, I expect "active" participation rates to be low. I must be clear that this is merely a hunch. I don't think there is a large profit motive behind implementing this program for the retailer. I think it is an opportunity to pretend to be doing some good and to have something which reflects a positive corporate image. A survey of store staff and of some customers to test this hunch would be great.

Hypothesis 2: If measurable, I expect spending increases to occur within the first 3 weeks of each month, when most SNAP benefits are distributed and consumed. I also expect week 4 for all stores, regardless of DUFB participation, to be relatively similar.

Prior research finds that SNAP shoppers spend their benefits soon after receiving them, generally in one large shopping trip (Wiig and Smith, 2009; Damon et al., 2013). The grocery stores are located in a state where benefits are disbursed every odd day of the month between the 3rd and 21st, spanning the first 3 weeks of each month. Therefore, by the 4th week, few unspent dollars will exists. Since the DUFB incentive is only triggered by the use of SNAP benefits, SNAP shoppers will be unable to make use of DUFB.

I expect that, were we to remove the 4th week of each month from all stores, the DUFB effect size will increase. I do not anticipate to see shoppers behave in strategic saving behavior of DUFB points (which is quite difficult given the automatic accrual and redemption system) nor do I think FV spending to increase much without the presence of the incentive. More importantly, since I cannot track link purchases to individuals (more on this later), it will be impossible to identify which transactions are made by SNAP shoppers when they are not using their SNAP EBT cards.

Concept in a Plot

What I am hoping to find is displayed in Figure 1.1. Note that the data is fake and the time interval is at the monthly level (I plan to use daily data). The point of the graph is to emphasis that I expect to find a jump in dollars spent on fruits and vegetables in the experimental (treated) stores once the DUFB program begins in August. While I intend to use a difference-in-difference-in-differences (DDD) model as part of my analysis, I display a difference-in-differences (DD) to more easily highlight the effect of interest.

Assume that the DD displayed is isolating on the subpopulation of SNAP transactions observed. Then the dotted green line beginning at month 8 would represent the gap under the "parallel trends" assumption. The difference between the solid green line and the dotted green line represents the increase in dollars spent that I hope to identify.

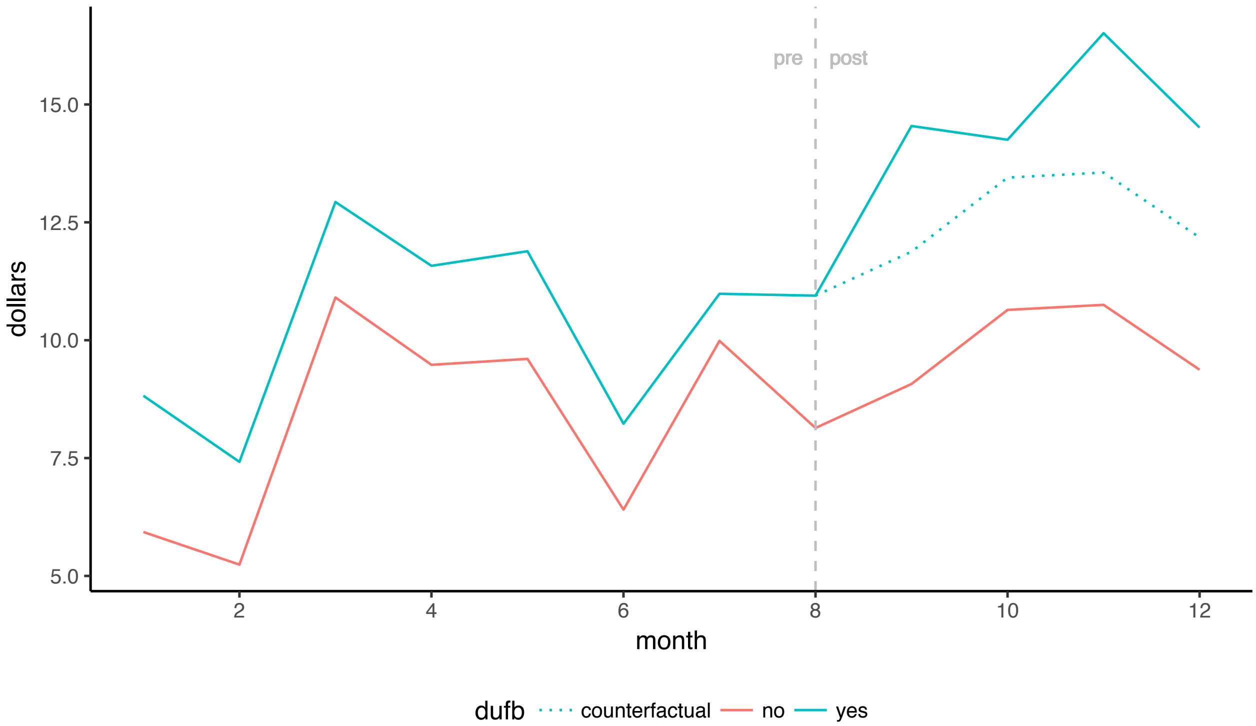


Figure 1.1: Example of Hypothetical Result (Fake Data

Introduction

Chronic conditions like obesity, heart disease, and other metabolic risk factors (stroke, type II diabetes, etc.) are estimated to cost the US health care system between 200 to 400 billion dollars annually (Cawley and Meyerhoefer, 2012; Chatterjee et al., 2014). More importantly, these diseases account for hundreds of thousands of deaths each year. Heart disease alone is the leading cause of death for all persons in the US, with stroke fifth and diabetes seventh (National Center for Health Statistics, 2015). Diet is closely linked to these conditions, particularly obesity and cardiovascular disease. There is strong evidence that a diet high in (1) vegetables, fruits, nuts, unsaturated oils, fish, and poultry, but low in (2) red and processed meat and sugar-sweetened foods and drinks, helps lower body weight, blood pressure, and the risk of cardiovascular disease (Mente et al., 2009; Nutrition Evidence Library, 2014). Improving the diet of Americans has therefore become an increasing priority for the United States, especially for struggling families that participate in the Supplemental Nutrition Assistance (SNAP) program.

SNAP is a federal aid program administered by the Food and Nutrition Service (FNS), an agency of the U.S. Department of Agriculture (USDA). At 74 billion dollars in FY2015 with roughly 45.8 million participants, it is the largest food assistance program in the US (USDA FNS, 2016b). To be eligible for SNAP, a household must be sufficiently budget constrained that hunger is considered likely without assistance. Eligibility is a function of countable resources, vehicle ownership and value, household size, gross or net monthly income, household composition, and meeting certain work requirements.[1] Some eligibility requirements vary by state, but in general, a family with less than $2000 in countable

[1] For more details, visit `http://www.fns.usda.gov/snap/eligibility`

resources, where the adults work at least part-time earning a gross (net) monthly income at or below 130% (100%) of the federal poverty line, is eligible to receive SNAP benefits. Aside from a few restrictions—no alcohol, tobacco, non-food items, read-to-eat meals, or hot foods—households can use SNAP benefits to purchase any foods that will be prepared and consumed at home. Unfortunately, the purchasing patterns of the average SNAP household are not conducive to a healthy diet.

Research on the dietary patterns of households receiving SNAP benefits has found that they are significantly *less* likely to meet USDA dietary guidelines than the average US household and much *more* likely to consume unhealthy foods (Andreyeva et al., 2015; Nguyen and Powell, 2015; Wolfson and Bleich, 2015). A smaller set of research has found that SNAP households, at best, consume same amount of unhealthy foods (e.g. sugar-sweetened beverages, baked goods, snacks, candy, etc) compared to SNAP-ineligible households (Todd and Ploeg, 2014; Hoynes et al., 2015). In other words, SNAP households consume foods that are less healthy or about the same as SNAP-ineligible households. This is a concerning result given that most US households, regardless of income, already purchase and consume far too much meat and foods rich in sugars and fats, and far too few fruits, vegetables and whole grains (USDA, 2015; Frazão, 1999). However, the purpose of SNAP is to keep struggling families from going hungry, not to ensure they consume the best possible diet. SNAP is designed to act like cash, helping families access more food than they could otherwise do so without assistance (Hoynes et al., 2015). It is therefore not a failing of the SNAP program if benefits are used to purchase unhealthy foods.

The SNAP program could be change such that it could continue satisfying its role as an anti-hunger program while simultaneously encouraging healthier purchases. Blumenthal et al. (2014) and Leung et al. (2013) both surveyed a field of stakeholders and policy experts in the SNAP program about what they would do to improve the dietary quality of purchases. In both studies, restricting the purchase of unhealthy foods (e.g. sugar-sweetened beverages) and promoting healthy purchases through monetary incentives were the two most popular improvements (i.e. ranked the highest or most often suggested).[2].

One common suggestion is to restrict the SNAP program to the same set of eligible foods as the Woman, Infants, and Children (WIC) program (Dinour et al., 2007). The WIC program provides food vouchers which limit households to a select group of food products. These food products are specifically selected to ensure women and their children receive nutritious, healthy foods. In other words, the WIC program, by design, places restrictions on food choices by defining a list of *eligible* items, as opposed to the SNAP program, which defines a list of *ineligible* items. Another common, and simpler, suggestion is to expand the existing list of ineligible items (e.g. alcohol) with products that are unambiguously lacking in nutrition and easy to identify, like soda or candy. New York City, for example, attempted to ban the purchase of sugar-sweetened beverages, and the state of Maine attempted to restrict the purchase of sodas, candy, and any other taxable food items (Gundersen, 2015). Both restrictions were overturned by the USDA.

There are problems with "improving" the SNAP program by implementing even greater purchasing restrictions. First, there is no reason to believe that such a restriction would work. The restriction assumes that, under WIC-like requirements, households will substitute healthy foods for unhealthy

[2] It should be mentioned that there were other, less popular recommendations, such as modifying how SNAP benefits are distributed and improving nutrition education. For more details, see Blumenthal et al. (2014) and Leung et al. (2013)

foods when using SNAP benefits. What would most likely happen is that households would shift to purchasing unhealthy foods with cash. Second, such restrictions would likely lead to a drop in SNAP participation (Gundersen, 2015). Restricting choice is a paternalistic policy that would further stigmatize SNAP participation. It would give the impression that SNAP beneficiaries are assumed to have worse diets and that they cannot be trusted to make healthy food purchases. Participation would also drop due to increased transactions costs of purchasing items with SNAP. Not all stores would clearly mark which items were SNAP eligible nor should participants be expected to remember. The result would be longer, more frustrating shopping trips. Lastly, it is important to remember that for many SNAP recipients, freedom of choice is what makes the SNAP program popular and easy to use (Edin et al., 2013).

The most popular "improvement" was providing a monetary incentive to SNAP participants for purchasing healthy foods (Blumenthal et al., 2014; Leung et al., 2013). Monetary (or financial) incentives, in this context, tend to be a rebate or voucher awarded to SNAP households for using their benefits to buy certain healthful foods, generally mineral-rich and nutrient-dense fruits and vegetables (i.e. leafy greens but not white potatoes). These monetary incentives for buying "targeted" fruits and vegetables (aka TFVs) are exclusive to SNAP participants. Much like a grocery stores loyalty card or a student ID card, retailers can "discriminate on price" (aka "target the incentive") using SNAP Electronic Benefit Transfer (EBT) cards to identifying eligible participants. Monetary incentives in the food retail environment are popular for two main reason. First, the framing of the "improvement" is positive. Instead of "punishing" SNAP participants through paternal restriction or disincentives (not covered), monetary incentives reward participants for healthy shopping behavior (Gundersen, 2015). Retailers also prefer the positive framing of monetary incentives. For the moment, monetary incentives programs for SNAP participants are not wide spread. Therefore, taking up an incentive program, assuming the cost of implementation isn't too expensive, creates an opportunity for retailers to differentiate themselves from their competitors (Hartmann, 2011). The second reason is a strong theoretical framework established by neoclassical economics supporting incentives as an effective mechanism for changing human behavior. In practice, however, incentives have had mixed results, but there is building evidence that incentives may work in the food retail space.

How, why, and to what effect incentives may encourage SNAP participants to purchase more targeted fruits and vegetables will be discussed in detail below, and is the motivating question behind this paper.

Financial Incentives to Encourage Healthy Food Purchases

Encouraging healthy behavior through financial incentives has a long history. Results are mixed. For example, financial incentives have been shown to help individuals commit to regular exercise, improve dieting, increase weight loss, and to quit smoking, but the intended effect of the financial incentives were often only short-term (see Gneezy et al. (2011) and Cawley (2015) for an overview). Gneezy et al. (2011) also explain, through a review of the literature, that depending on the context and design, incentives can backfire, producing an effect known as "crowd out". Crowd out occurs when an incentive displaces the intrinsic reward of a behavior (originally defined for "prosocial" behavior, like volunteering or giving blood; see Bénabou and Tirole (2006)). The behavior then becomes dependent on the extrinsic reward. As a result, having shifted from being intrinsically rewarding to extrinsically rewarding, the positive behavior continues only as long as the monetary incentive is provided. More significantly, the intrinsic reward of the behavior does not return once it has been "crowded out". Therefore, the long-term effect of a monetary incentive can be negative, despite showing positive effects in the short-term. That said, incentives can produce successful long-term results if they are instead used as a mechanism to build good habits. This requires that the incentives be salient and produce immediate feedback without neglecting behavioral findings such as loss aversion and mental accounting (John et al., 2011).

Given the research on incentives, it is reasonable to assume that a monetary incentive for SNAP participants to purchase healthy foods, like fruits and vegetables, may fail or even backfire. Should the act of purchasing healthy foods be intrinsically rewarding to SNAP shoppers, introducing an incentive may produce a "crowd out" effect. However, recent field experiments find that incentives can establish healthy food choice as a habit, possibly overriding any crowd out. Daily incentives encourage children to make healthier food choices in school lunchrooms, who in turn develop positive, long-term food habits (Loewenstein et al., 2016; List and Samek, 2015; Belot et al., 2014). Outside of the school lunchroom environment, List et al. (2015) found that similar habit formation is possible with incentives in a more traditional food retail environment. In their experiment, List et al. (2015) provided an incentive to 222 shoppers ($1) to use their rewards cards, but then randomly assigned each participant to the control group to one of three interventions: *information*, *incentive*, or *combination*. The *information* treatment was a flyer with tips on how to prepare fruit and vegetable dishes as well as the health benefits of eating more fruits and veggies. The *incentive* was an additional dollar for every 5 cups of targeted fruits and vegetables (TFVs) purchased. The *combination* treatment group included both. The intervention lasted for 5 months but each group continued to be observed for roughly 6 weeks months after. The *information* intervention had no effect, but the *incentive* and *combination* interventions on average doubled their purchase of fruits and vegetables in comparison to the control group. Most promisingly, the gap persisted with minimal shrinkage for 6 weeks following the end of the intervention. However, there was no follow up after the 6-week post-intervention period. It is therefore possible the gap closed over multiple months (as opposed to multiple weeks).

The design, food environment, and target of the incentive in each of these experiments is important. First, the incentive in these experiments is designed to be salient and immediate. In the school lunchroom experiments, the children are aware of the incentive and receive the reward (e.g. a small token) immediately after selecting the healthy food item. Likewise, the shoppers received their additional $1 reward for every 5 cups of TFVs at checkout. One distinction between the designs is frequency. The children are exposed to the incentive every school day in the lunchroom experiments. The shoppers, on the other hand, were exposed as frequently or as infrequently as they chose. This distinction is important, as the latter better reflects the experience of shoppers using the SNAP benefits. Second, the food environment is important because it determines what choices are available. The children have a finite set of options in the school lunchroom and they also have no outside option (besides not eating lunch). The children optimize on a relatively small set of choices and, for the duration of the intervention, the incentive always existed. Food retail environments are drastically different. There are numerous competing food choices and generally other outside options.[3] It is substantially more

[3]It should be noted that this is not always the case. In List et al. (2015), for example, the store that was selected for the experiment was one of the few places local shoppers could find fresh produce. The store itself was located in one of Chicago's poorer neighborhoods.

difficult for the shopper to optimize over such a large set of choices. Last, and most obviously, one set of studies targets children, the other adults. A priori, we would expect a monetary incentive to affect children differently than adults. The fact that habit formation through incentives appears possible for both target groups is promising.

Research where SNAP participants are the target group is nascent. The USDA's Food and Nutrition Services (FNS) ran the first large scale randomized control trial investigating the impact of a financial incentive for targeted fruits and vegetables in 2011. The experiment was called the Healthy Incentives Pilot (HIP). HIP is the precursor to every incentive program currently being funding by the USDA. It also provides the data for the few papers recently published on incentives for SNAP participants.

The Healthy Incentives Pilot

A brief overview of the Healthy Incentives Pilot is necessary to provide context to, and contrast with, more recent financial incentive programs.

The UDSA's Food and Nutrition Services designed the Healthy Incentives Pilot. The pilot was funded by the Food, Conservation, and Energy Act of 2008 to test whether financial incentives would increase consumption of targeted fruits and vegetables (TFVs). SNAP participants were the target group.

HIP was designed as a large scale randomized control trial (RCT). FNS partnered with the Massachusetts Department of Transitional Assistance to implement HIP. The pilot lasted from early 2011 to the end of 2012. The population included all 55,095 SNAP participants in Hampden County, MA. Hampden County is the poorest county in Massachusetts and has the highest rates of obesity and other diet-related chronic illness (e.g. type 2 diabetes).

Of the 55,095 SNAP participants, 7,500 were randomly assigned to the treatment group. The remainder fell into the control group. The treatment was a 30 cent (or 30%) rebate on every dollar spent on TFVs. The rebate was capped at $60 per month. To receive the rebate, selected SNAP participants had to use their EBT cards at participating retailers. The rebate, which was returned to their EBT account, could then be used on any food item. That is, the rebate could only be earned buying TFVs, but could be redeemed buying any SNAP eligible food item. Most HIP participants spent about $12 a month on TFVs, earning an average of $3.65 per month in rebates—drastically lower than the $60 per month rebate cap.

The evaluation was conducted using 24-hour dietary recall surveys. A total of 5,000 participants were selected to be surveyed, even split between treatment and control (2,500 HIP, 2,500 non-HIP). The first survey was conducted prior to the start of the pilot. This established a baseline. The second survey occurred 4 to 6 months in to the pilot and the third survey occurred 9 to 11 months in. (The variation, e.g. 4 to 6 months, was due to the treatment being implemented in 3 waves of 2500.)

The evaluation found that the 30% rebate lead to about a 26% increase in consumption of TFVs. This was equivalent to about 0.24 cups of TFVs. Roughly 60% of the increase was due to increased vegetable consumption and 40% due to increased fruit consumption. The effect, in absolute terms (0.24 cups), seems small. But a 0.87 price elasticity, relative to other results in the literature, is quite high—0.7 and 0.48, on average, for fruits and vegetables, respectively (Andreyeva et al., 2010).

Despite some limitations and technical problems—underreporting on the 24-hour recall survey and system glitches early in the pilot (see pages 60 and 208-210 of Bartlett et al. (2014))—HIP was considered to be an overall success (Klerman et al., 2014; Olsho et al., 2016). It implemented on of the largest, most complex RCTs to isolate how incentives can increase household consumption of TFVs. It also provided a feasible model for nationwide expansion (assuming cost reductions due to economies of scale; see An (2015)).

HIP also provides a framework for understanding how a financial incentive, expanded dramatically in one geographic area, could improve TFV consumption. But, as noted in the final HIP report, one of the most prominent retailers in Hampden County chose not to participate (page 61, Bartlett et al. (2014)). Its third-party processor decided it was too difficult and too costly to implement the financial incentive on its point-of-sale technology. This strategic behavior by the retailer, which had a significant presence in Hampden County, impacted where participants could use the incentive.

Most financial incentive programs work at the local level, expanding non-randomly. We should anticipate certain retailers (firms) to behave strategically when participating in any of these incentive programs. Likewise, we should anticipate voluntary (non-random) self-selection by SNAP beneficiaries into these financial incentives programs. To this end, more research is needed to understand the impact of incentive programs under *real-world* conditions. HIP provided evidence that incentive programs can work, but barring state-wide or nation-wide adoption of point-of-sale financial incentives, we should expect growth to occur organically under non-experimental conditions.

An example of such a financial incentive program for SNAP participants is the Double Up Food Bucks program (DUFB or Double Up). The non-random expansion and impact of this financial incentives program will remain the focus of this paper

The Double Up Food Bucks Program

The success of HIP paved the way for the Food Insecurity Nutrition Initiative (FINI), established by section 4208(b) of the Agricultural Act of 2014 (aka 2014 Farm Bill). FINI—a 100-million-dollar initiative—in turn piloted numerous non-profit financial incentive programs aimed at improving the diets of SNAP participants.

Of specific interest is Double Up Food Bucks (DUFB or Double Up), an incentives-based program funded by FINI. In 2009, the non-profit organization Fair Food Network (FFN) launched the Double Up Food Bucks program in Detroit, Michigan. The intention of the program was to get more low-income families visiting and participating in local Detroit farmer's markets. The mechanism for increasing participation was a dollar-for-dollar match of locally grown fruit and vegetable purchases. This subsidy was accessible only to low-income families receiving SNAP benefits, who could exchange up to $20 of their benefits for a wooden token that could be used on up to $40 worth of locally grown produce.

The DUFB program was considered successful given it had expanded to more than 150 farmer's markets in 2014 from just 5 farmer's markets in 2009. SNAP benefits have been used more than 200,000 times to purchase fresh produce, with more than 10,000 first time SNAP customers visiting farmer's markets in 2013 alone (Network, 2014). The program is considered by Fair Food Network to be a "three-fold" win given that the program helps local low-income families buy more fresh produce, provides new customers for local farmer's, and stimulates the local food economy. Relative to farmer's markets in other states, DUFB did seem to be bringing in substantially more SNAP dollars ($1.7 million in Michigan versus $307,000 in Illinois, the second largest).

A 5.17 million dollar FINI grant was awarded to Fair Food Network to help it pilot three adjustments to the Double Up Food Buck program (USDA NIFA, 2015). First, FFN needs to test DUFB as a year-round program in select locations instead of the current seasonal format. Second, shift away from the token system to providing DUFB electronically at point-of-sale. Third, the DUFB needs to expand from farmer's markets into other retail environments, like supermarkets and grocery stores.

Successful expansion into supermarkets and grocery stores is critical. Approximately 80% of all SNAP benefits in 2015 were used in supermarkets or super stores (USDA FNS, 2016a). Less than 1% percent of SNAP benefits were used at local farmer's markets. The amount of SNAP benefits used in local farmer's markets has increased since 2009, but no where near the growth necessary to reach the type of stores most frequented by low-income families. If localized financial incentive programs like DUFB are going to be considered one of the USDA's many tools to increase food access and combat obesity, then they must be successfully implemented and scaled across supermarkets and grocery stores. Most importantly, incentive programs like DUFB must prove they are effective in changing purchasing habits within supermarket/grocery store food environments.

Double Up Food Bucks vs the Healthy Incentives Pilot

There are notable differences between DUFB and HIP that make the evaluation of DUFB more difficult. In short, HIP was implemented as an RCT. DUFB implementation is not. Let's explore in greater detail.

HIP had substantially more participating stores, all within the same county (Hampden County, MA). DUFB has fewer participating stores, spread across many different counties, and across many different grocery store chains. Therefore, the probability of a SNAP shopper in Hampden County having walked into a HIP participating store was much higher than a SNAP shopper walking into any DUFB participating retailer.

The incentive delivery mechanisms also differ. First, all SNAP beneficiaries who shop at a DUFB participating store receive the benefit automatically. In other words, SNAP households that patron a store with DUFB receive the incentive regardless of their intentions or awareness of the DUFB incentive. Therefore, evaluating DUFB has the added difficulty of identifying which shoppers are optimizing in response to DUFB, as opposed to shopping normally. In contrast, SNAP households assigned to the HIP treatment group were made aware of incentive and were eligible to use it (even if they didn't quite understand how the incentive program worked — see Bartlett et al. (2014)). Households in the control group were not aware of the incentive and were not eligible to use it. And because participants were assigned, HIP evaluators could identify treated participants from control participants.

Second, the DUFB financial incentive is substantially larger but more restrictive. The DUFB incentive is a dollar-for-dollar match of locally grown produce purchases capped at $20 per day. The matched dollars are accrued as points on a store loyalty card. Existing points are then automatically redeemed as dollars on *any* fresh produce purchases, not just locally grown produce. In comparison, the HIP financial incentive was a return of 30 cents per dollar spent on TFVs which could be spent on *any* food item. That is, the DUFB incentive doubled the purchasing power of every dollars spent on TFVs *only for more TFVs*; the HIP incentive increased the purchasing power of every dollars spent on TFVs by 30% *for any SNAP eligible food item*.

Finally, the experimental designed of HIP allowed researchers to form a causal interpretation of their results; the average treatment effect is the same as the average treatment effect on the treated. Any difference in the purchase and consumption of TFV between the treatment and control groups could therefore be attributed to the incentive. This is not the case for DUFB. However, HIP implementation is the exception. How DUFB, and similar financial incentive programs are implemented, is the norm. The contribution of this paper will be evaluating and understanding the impact of DUFB, given that DUFB and similar programs are implemented in the "real-world" (non-experimental conditions)

Evaluating Double Up Food Bucks in Non-experimental Conditions

DUFB's expansion and implementation into supermarkets and grocery stores did not follow an experimental design. Fair Food Network searched for local partners in the Detroit area willing to participate in DUFB. Not all grocery stores, especially the smaller independent stores, had the capacity to implement the point-of-sale technology necessary for the incentive—even if FFN offered to help cover the upgrade costs. The result is a self-selected group of stores participating in DUFB. This, in some ways, parallels what occurred in HIP, where one of the largest retailers decided integrating their point-of-sale systems to include the incentive was too expensive. This type of strategic firm behavior is important to consider, even if complicates the evaluation of an incentive program like DUFB.

In the real world, stores seek to maximize profits and will opt to participate only if they expect to profit. Similarly, individuals will self-select into participating; participation is optional and more likely to occur with well-informed and motivated SNAP shoppers. Selection, in this case, is a feature, not a flaw, of such incentive programs when implemented by non-profits or policy makers. The evidence, thanks to HIP, exists that incentives can lead to an increase in consumption. The goal of this paper is therefore to accurately measure the effect of the DUFB on TFV purchases while taking the selection into account. That effect can then be extrapolated forward, albeit weakly, using the results of HIP, to measure changes in consumption.

Fair Food Network started testing and gathering data from grocery stores implementing DUFB in 2014. One of FFN's largest partners, a Michigan grocery retail and distribution company, piloted the program in 2 of its stores in 2014. The company has since expanded to 5 stores in 2015 and then to 17 of 62 stores in 2016. Rapid scaling was possible due to the point-of-sale technology used by the company to implement DUFB across its stores. It provides, to date, the best case study of a firm strategically scaling DUFB across numerous grocery stores that span different geographic areas and populations.

All transaction data from 2014 - 2016 will be provided for every store that has, at any point, participated in DUFB. These data are complete (i.e. no records have been removed) and at the item level. A complete set of data will also be provided from another 15 stores where DUFB was not implemented.

Currently, no research exists evaluating DUFB, or similar incentive programs, using a complete set of store transaction data. HIP, for example, only had transactions records for SNAP EBT cards. Transactions, should a different tender be used by the same individual, could not be observed. Therefore, these data provide an unprecedented opportunity to analyze how the DUFB financial incentive performs under real-world conditions. This paper will be, to the best of my knowledge, the first to perform an evaluation of a financial incentive, targeted at SNAP participants, using a complete set of data, from multiple stores, across multiple years, and collected under non-experimental conditions.

Data Description

These data come from a large grocery distributor and retailer serving multiple grocery chains. Three years of data will be made available, 2014 through 2016. To my understanding, this includes months where the DUFB incentive is active (Aug 1 to Dec 31) and inactive (Jan 1 to July 31) across all stores. These data are transaction level data and will include (at least) store number, register, transaction ID, date and time of purchase, payment type, item, dollars, and quantity.

Double Up implementation was considered for a single grocery chain. The chain has more than 60 stores, 17 of which were selected as "treatment" stores (with Double Up). Of the remaining stores, data is being made available from an addition 15 to serve as "controls". The quotes here signify that these terms will be used as shorthand, but the terminology is somewhat misleading. The use of "treatment" and "control" could lead one to think store assignment was random. It was not.

[TK MISSING real specific details about the data e.g. total transactions observed etc.]

How the DUFB Incentive is Implemented

The DUFB incentive can differ in implementation. Three different implementations have been observed: earn/redeem DUFB points via loyalty card, single-use paper coupon, and immediate discount. The earn/redeem DUFB point system is unique to the retailer that provided these data. For information on the other two DUFB implementations, see (Margaret Schnuck, 2016).

The DUFB implementation for this particular grocery store chain is a point system. SNAP shoppers can *earn* points by buying locally grown produce using their SNAP EBT card and their loyalty card. The loyalty card is required because it is used to store and track earned DUFB points. Each dollar spent buying locally grown produce earns a DUFB point. SNAP shoppers are eligible to receive up to $20 dollars worth of DUFB points (20 points) per day. Earned points are not reflected immediately on loyalty cards. They are redeemable the day after.

Points can be redeemed on *any* eligible produce (excludes frozen and canned fruits and vegetables). Any form of payment can be used when redeeming points; it is not necessary to use a SNAP EBT card to redeem points.

There are four important details about this implementation of the DUFB incentive. I already mentioned the first but it is worth reiterating: earned points take a day to be processed on a loyalty card. This forces SNAP shoppers to delay the reward of the DUFB incentive earned by at least a day. While perhaps a technological necessity (it takes time to process and reflect earned points on loyalty card accounts), this delays the transactional utility that could potentially be earned by the SNAP shopper (Thaler, 1985). I expect that delaying the transactional utility reduces the "pleasure" and effectiveness window of the DUFB incentive. SNAP shoppers tend to spend benefits in one large shopping trip soon after receiving monthly benefits. These shopping trips therefore correspond to the single largest opportunity to earn DUFB points. Unless the shoppers returns to the store frequently to buy fresh produce, the "reward" of redeeming points.

Second, the incentive alternates between earning and redeeming states. A loyalty card with a DUFB point balance of zero is in an "earning" state. However, once DUFB points are earned by buying *locally* grown fresh produce, the card switches to a "redeeming" state; loyalty cards with a point balance greater than zero will redeem until the point balance is once again zero. This removes the possibility of strategically "banking" earned points to be redeemed all at once (say for a holiday shopping trip). Should I purchase $10 dollars of local produce and earn 10 points, the next time I purchase *any* produce (even local produce), my purchases will be redeemed from the point balance until the 10 points are gone.

Third, the points *earned* are not communicated to the shopper at the moment of sale (Family Fare, 2016). In other words, the fact that shoppers are earning points is *not salient*. There is no feedback connecting the purchase of fresh healthy produce to the fact that the shopper is earning points. Currently available points (those processed in prior shopping trips) are printed at the bottom of each receipt, and can even be checked on-line or on in-store kiosks. But no feedback or information is shared to the shopper during the current sale. Earning points, at least, ends up being like any other shopping trip. I would argue this is more a con than a pro. Certainly a bell shouldn't ring when SNAP shoppers earn points. One of the great consequences of moving to an EBT card is that the potential stigma of using food stamps has greatly diminished. But shoppers could benefit from some sort of feedback that is informative without producing a spotlighting effect. For example, shoppers could be told, "You saved $4.50 today and you also earned 5 DUFB points".

[**I need to confirm that this does not happen**. Considering the points earned don't appear on receipt, I do not see how the clerk know to inform the customer. Does it appear on the machine?]

The fourth, and most important point, is the automatic earning and redeeming of DUFB points. This implies the incentive works only if individuals choose to actively participate in the program. This is distinct to standard experimental procedure where individuals are assigned to the treatment or control group and then choose to participate (or not).

What is a "participant" for this DUFB implementation?

How a participant responds to assignment is generally referred to as *compliance* (Angrist and Pischke, 2008).[4] But in this case, it is the stores, not the individual shoppers, that have been "assigned" to a treatment or control group. Stores, if assigned to the treatment group by the retail chain, are "compliers" by default; the DUFB incentive is implemented on store's point-of-sale (POS) system. Some stores (4) behaved somewhat like "always-takers", having asked to participate in DUFB, but most store (13) are "compliers".[5]

How, then, does one think about SNAP shopper participation in the DUFB program if it is stores that are ultimately assigned to the DUFB program? SNAP shoppers have the option to benefit from the program without ever being "assigned" to any treatment group. A shopper's active participation in DUFB is therefore driven by another type of self-selection. I imagine use of the DUFB incentive depends on a series latent variables corresponding to individual shoppers, stores, and the retail chain.

[4] Participants can be further categorized into "compliers", "never-takers", "always-takers", and "defiers". These categorizations provide useful terminology but are not relevant in the context of the DUFB incentive.

[5] I must note that, while this creates some worries of "self-selection" by stores, I think this bias can be handled by a model that includes a store-level fixed-effect.

For example, demographics, price sensitivity, food preferences, health consciousness are all latent variables that could affect shopper DUFB activity. Other latent variables include how effectively the retail chain markets the DUFB program to management of participating stores and how effectively this information is relayed by stores to individual shoppers. Management's enthusiasm for the program is likewise a latent retail chain and store-level variable.

Automatically redemption of the DUFB points also complicates identifying individual participation. Automatic redemption of DUFB "points" means I cannot identify which SNAP transactions are responding to the incentive versus "shopping as usual". That is, I will observe many SNAP transactions earning or redeeming DUFB points for fruits and vegetables that are oblivious to the existence of the incentive. I will also observe individuals who have chosen to actively participate in the program. In aggregate, however, I assume that any increase in the total amount of fruit and vegetables purchases in DUFB stores can be attributed to the incentive. This is where having purchasing data from the non-DUFB stores is important. The non-DUFB stores will help improve estimation by controlling for any changes in fruit and purchases that may occur for reasons other than the DUFB incentive e.g. seasonal or macroeconomic conditions.

Purchases Cannot Be Linked to Individuals (No Loyalty Card Data)

One important variable that will not be made available is a variable for loyalty card numbers. The company's use of loyalty cards across its many chains was an exciting prospect. Previous transaction data from smaller independent grocery chains had no way linking purchases to a single unique identifier over time because these smaller chains did not have advanced point-of-sale systems.

In earlier conversations with the company, it was understood that loyalty cards would be made available. However, months into working with the company, I was informed that this was no longer possible. Per the company's legal department, the company cannot share any personal information about their customers. Unfortunately for us, in the loyalty card contract signed by customers, the loyalty card number itself is considered personal information, meaning loyalty card numbers fall under the same legal category as phone numbers and home addresses.

DUFB Incentive Inconsistency Across Years

The retail company informed us that the way the DUFB incentive worked in 2016 is distinct from 2014 and 2015. The DUFB incentive in 2016 worked by earning points for each dollar spent on *locally grown* fresh produce. (Recall that each point is equal to one dollar.) Points are then redeemed automatically on *any* fresh produce. However, in 2014 and 2015, the incentive was the *opposite*. In those two years, the DUFB incentive worked by earning points on *any* fresh produce, automatically redeeming points on *locally grown* fresh produce.

This is important because *locally grown* fresh produce is a much smaller subset of the *any* fresh produce. Therefore, in years 2014 and 2015, shoppers could easily earn points but had a constrained set of produce on which to redeem points. In any case, estimates of the incentive for the year 2015 cannot be compared to estimates in the year 2016.

Limited Dependent Variable

It is very likely that for any recorded visit to the cashier—what I call a "transaction"—a customer does not purchase fresh fruits or vegetables (FV). If I split a customer's items purchased during a transaction into a few general categories (e.g. dairy, candy, meat, etc.) and aggregated expenditure over these categories, I will observe a non-trivial amount of zeros. More importantly, these are "true" zeros aka *corner solutions*. That is, these zeros are not substitutes for missing data or representing negative values but the result of a utility-maximizing choice.

These concerns are covered in greater detail in the Methods Section.

Other Information

Past Experience with Similar Data

This is not my first experience working with transaction data. At this point, I have more than 3 years working with transaction data. Furthermore, this is not my first experience with transaction data where (1) DUFB was implemented and (2) transactions were not linked to individuals.

I performed an analysis in April of 2016 for FFN using 5 months of transaction data from 3 small Detroit-area grocery stores. Figure 1.2 was produced with those data. It was easy to distinguish when SNAP benefits were being used in those data. Likewise, it was easy to tell when transaction made use of the DUFB incentive (either an issuing of DUFB or a redemption). A simple aggregation could determine the total amount of dollars spent per some unit time (*day* was the smallest possible unit of time). I expected data for my prospectus will be very similar. The empirical models in the next section were developed under these expectations of the data.

SNAP Spending is Cyclical

In some prior work, I've observed that SNAP spending is cyclical, peeking in the 2nd week. This is due to the state's monthly SNAP benefits transfer schedule. Benefits are distributed every odd day of the month between the 3rd and 21st. Each day maps to the digits `0` through `9`. SNAP participants receive their benefits once a month on the day corresponding to the last digit of their SNAP ID number. For example, ID numbers that end in `0` receive their benefits on the 3rd of each month. SNAP EBT benefits are spent quickly. As a result, there are always fewer SNAP purchases during the 4th week of the month. And fewer SNAP benefits means fewer transaction capable of receiving the DUFB incentive. I'm not yet sure what impact this will have on my analysis this time around, but I thought it important and interesting to point out and consider.

The week-to-week cyclical pattern of SNAP EBT spending can be observed in Figure 1.2. (Note that these are from a different data source and different store chain, but from the same US state.) At the start of the each month, SNAP EBT transactions (red line) increase until peeking at the second week. The count then declines steadily through the 4th week before once again spiking during the 1st week of the following month. (Ignore the green line; these are DUFB counts from a different data set.)

Supply Chain Concerns

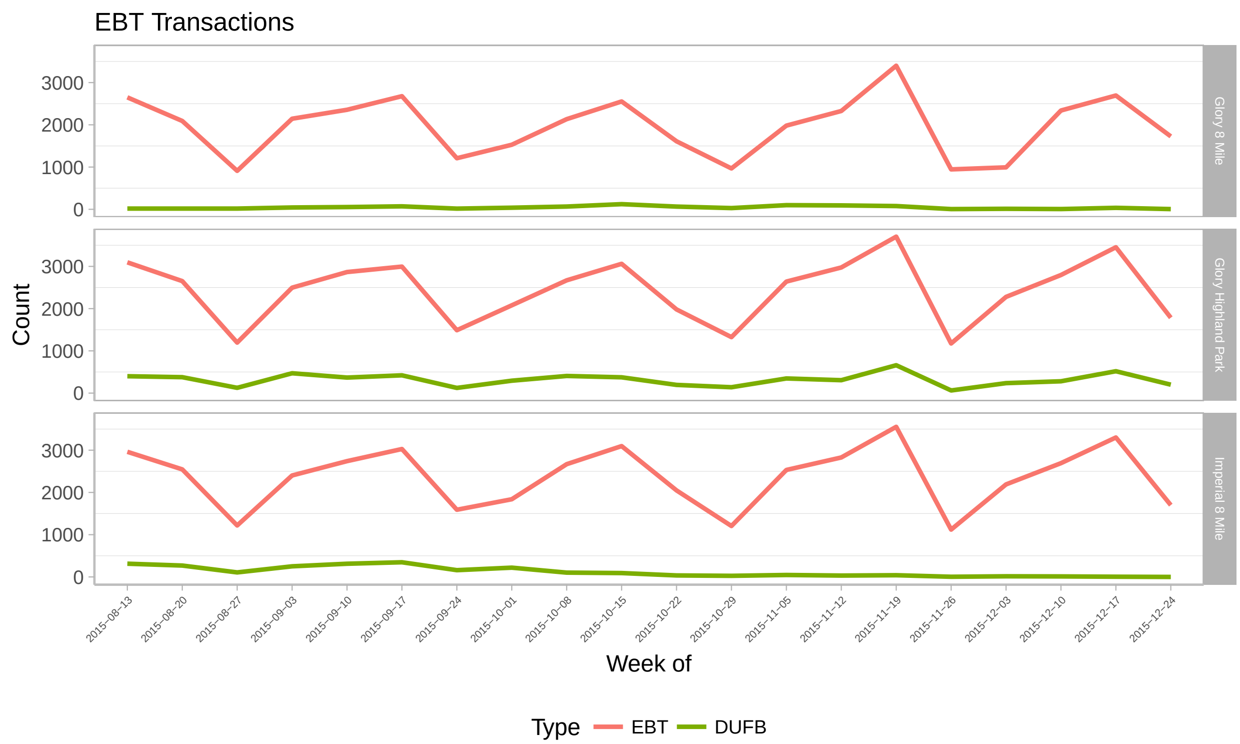


Figure 1.2: Example of how SNAP EBT benefits are spent in a predicable, week-to-week, cycle. It is the result of how benefits are distributed (uniformly across the first 3 weeks) and of how most SNAP participants spend their benefits (quickly and soon after being received). The red line is the count of transactions where SNAP EBT benefits were used as tender. Ignore the green line.

One concern I had was if local supply of produce differed geographically across the state where the stores are located. The company representative told me that should not be a factor because all stores are supplied from the same warehouse. Therefore, in theory, each store should have the same local produce. I plan to visit the stores on a later date to confirm that this is actually the case.

Overview of Store Selection and Expansion

How the 17 "treatment" stores and 15 "control" stores were selected in 2016 is important. First and foremost, selection was *not* random. Stores were either selected by the company (13 of 17) or self-selected into Double Up (4 of 17). Second, the 15 control stores were selected *after* the selection of the 17 treatment stores. Data from all remaining stores was requested but the request was denied; only 15 stores had been approved by the company's management. Finally, and most importantly, the selection criteria for the 17 treatment stores is *observable*. The implications of this will be covered in more detail in the Methods section.

Selection and Expansion of Double Up Stores

The first 2 stores were piloted with Double Up in 2014. Both were in geographically distinct areas (these will be referred to as "`Node 0`" and "`Node 1`"). There was a small expansion adding 3 more stores in 2015. The 3 stores were selected because they were geographically close to the 2 original pilot stores (2 close to `Node 0`, 1 close to `Node 1`). The 5 stores are referred to as the "core". The location of these 5 stores, separated in two clusters, established the geographic constraints that were then used to determine most of the additional stores in 2016.

Double Up was expanded to 12 more stores in 2016, totaling 17. Of those 12, 6 were selected due to their proximity to the 5 core stores, their SNAP EBT[6] sales figures, and similarity in surrounding demographics (high population density, more African-American). In other words, 9 of the 17 stores—excluding the initial 2 pilot stores—–were selected on a set of *observable* characteristics. The remaining 6 stores were not.

Of the remaining 6 stores, 4 asked if they could be included in the program. These stores *self-selected* into Double Up, making these stores fundamentally distinct. They were considered, and then included, only because they fell within the "Top 50". The final 2 stores were selected by the company for "strategic business decision". The best interpretation of this is that the company thought that Double Up would provide a competitive edge to the 2 included stores given some internal calculus. How the company came to this decision is *unknown* and therefore *unobserved*.

Table 1.1 helps understand the year by year expansion of Double Up. Stores are classified as either `assigned`, `self-selected`, or `unobserved`. To be `assigned` means a stores participation in Double Up was determined (assigned) by the company; `self-selected` means the store asked the company to participate; `unobserved` means that the company selected the store to participate in Double Up but for unknown and unobserved reasons. Numbers were assigned to each store for easy reference but otherwise have no meaningful interpretation.

Expansion on Observables

An example expansion on *observables* (using fake data) can be seen in Figure 1.3. In the top frame, one can see two blue dots. These blue dots simulate the first two pilot stores in 2014. The left blue dot is `Node 0` and the right blue dot is `Node 1`. The gray zones represent areas of higher population density. Dark gray is considered *urban*, defined as having a population density of 1500 persons or more per square mile. The light gray are small towns and cities, more densely populated than very rural areas, but could not be considered *urban*. The expansion in 2015 (middle frame) proceeds to the stores closest to the original pilot stores. The expansion continues to 6 more stores in 2016 (bottom frame) away from the nodes but also along areas of higher population density.

Not conveyed in Figure 1.3 is that the 2015 and 2016 expansions also move through stores that happen to be "highly ranked"—that is, have relatively higher SNAP EBT sales.[7] Also not conveyed is the fact that there is a strong correlation between geography, population density, racial composition, and

[6] Electronic Benefit Transfer.
[7] All stores within the chain were ranked by SNAP EBT sales as a percentage of total sales.

Table 1.1: Year by Year Store Selection. Stores 1 and 2 represent the initial 2014 pilot stores.

Store	2014	2015	2016
1	pilot	pilot	pilot
2	pilot	pilot	pilot
3		assigned	assigned
4		assigned	assigned
5		assigned	assigned
6			assigned
7			assigned
8			assigned
9			assigned
10			assigned
11			assigned
12			self-selected
13			self-selected
14			self-selected
15			self-selected
16			unobserved
17			unobserved

SNAP EBT sales. The 2015 expansion to the most nearby stores also meant that it was an expansion to stores with high SNAP EBT sales in densely populated, African-American neighborhoods. The 2016 Double Up expansion was more explicit given that set of feasible stores substantially increases as one moves away from each node. Double Up stores were thus specifically selected not just by geographic proximity, but also by SNAP EBT sales ranking and demographic compositions similar to the initial 2014 stores.

Expansion Data

Data for about each store was built by merging 4 different sources. The core data came from the grocery retailer directly, which provided a list of stores participating in DUFB from 2014 - 2016. The grocery retailer also provided a list of stores ranked by EBT sales as a fraction of total store sales and the size (square footage) of each store. Demographic and socioeconomic data came from the Data Science Toolkit API[8] (DSTK) and the American Communities Survey API[9] (ACS). The DSTK API provides access to US Census data from 2000 at the *census block* level and the ACS API provides data spanning 2010 - 2014 at the *zip code* level. Lastly, data was extract by mining the Family Fare[10] website.

Matching was done with the ACS data. The ACS zip code data was preferred because it provided income and housing data. Zip code level demographics are sufficiently descriptive; stores are evenly

[8] `http://www.datasciencetoolkit.org/`

[9] `http://www.census.gov/data/developers/data-sets/acs-survey-5-year-data.html`

[10] `https://www.shopfamilyfare.com/store-locator`

distributed across zip codes. Specifically, 58 stores are spread across 58 zip codes and 4 stores split between 2 zip codes (60 zip codes and 62 stores).[11]

Ideally, prior to matching, demographic data from the neighborhoods surrounding the store, who shopped at the store, and how the store was performing, its size, and goods made available would be known. Unfortunately, most of the publicly available data was not store-specific. The only store-specific data came either from the retail parent company directly or from mining the website.

Selection of Control Stores

Ideally, all remaining stores would have been available to use as a control group but the company only approved that data be released for 15 stores. This left the added—and incredibly important—step of selecting the control stores since the company approved, but did not explicitly select, the 15 stores.

Selecting the control stores proceeded in two steps. First, stores that either self-selected or were selected using some unobservable criteria were matched using *Coarsened Exact Matching* (CEM) (Iacus et al., 2011). Second, stores assigned Double Up were pooled with nearby control stores and then scored using a linear probability model. Each step is explained in detail.

Step 1: Coarsened Exact Matching

The 6 stores classified as `self-selected` or `unobserved` (stores `12` through `17`; see Table 1.1) were compared against all possible control stores for matches. Matching was done across 5 dimensions: race, income, population density, store attributes, store EBT sales. One variable per dimension was selected: percentage of population that is African-American (zip code level); people per square mile (zip code level); median income for people who have received SNAP or similar assistance (zip code level); the number of associates employed in each store; and the percentage of total stores sales attributed to EBT/SNAP.

Of the 6 stores (stores `12 - 17`), only 3 produced viable matches. However, each of the 3 matched stores had matched to more than one control stores. The closest stores, by driving distance, were selected as the tie-breaker for each matched store. Stores were sufficiently far apart, with very sparsely populated areas between, that "spill-over" was considered unlikely. That is, it is considered unlikely that a shopper near a store without Double Up would opt to drive 30 or more minutes to shop at the store *with* Double Up.

This left 12 stores to be allotted to the control group and 3 treatment stores to be effectively discarded.

Step 2: Scoring via Linear Probability Model

Assignment to treatment and control can be perfectly determined since we know and observe the criteria used for assignment: geographic distance from an initial store (node), SNAP EBT sales rank,

[11] I must also admit that my spatial and geocoding skills improved drastically in the months following the matching process. At the time, I did not know how determine census blocks from lat/long coordinates, relying on the DSTK API that did the conversion. The downside is that it returned 2010 data. I'm confident I could do it now, but I still think it is not worth the effort given the US Census Data excludes income data.

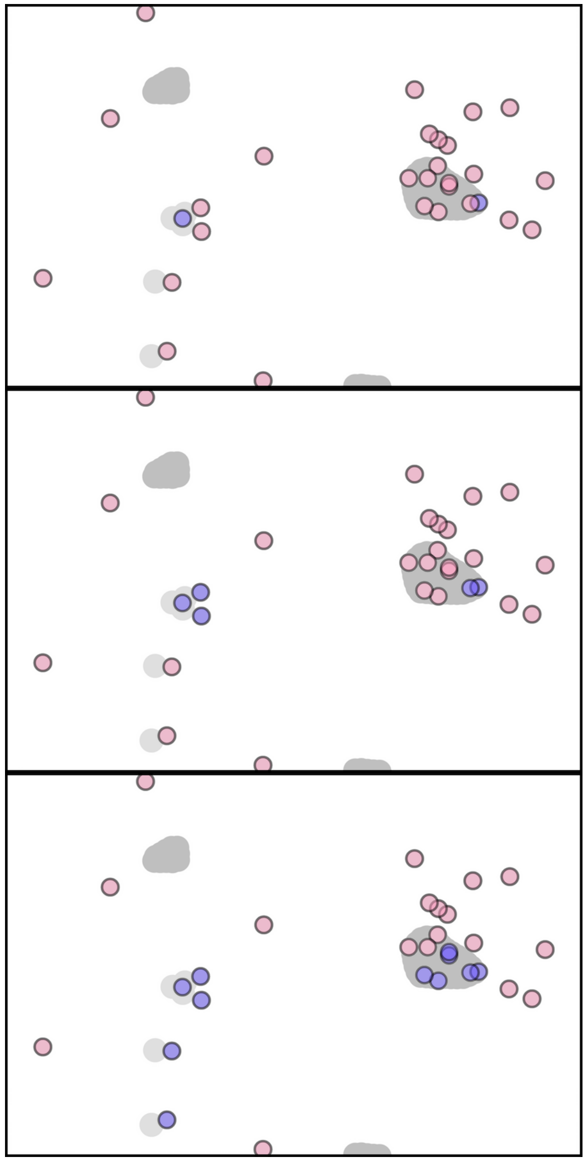


Figure 1.3: Example expansion over time from 2014 to 2016 (top to bottom) using fake data. Blue dots denote stores with Double Up, pink dots denote without. Gray sectors denote higher population density. The initial nodes can be seen in the top (2014) frame.

and demographics—specifically population density and percentage African-American.[12] A scoring function was created by fitting a linear probability model to all stores within 140 kilometers of the two initial pilot stores.

$$\begin{aligned} \boldsymbol{s} &= P(\widehat{\mathbf{D} = 1 | \boldsymbol{X}}, \boldsymbol{N}) \\ &= \mathbf{X}\hat{\boldsymbol{\beta}} + \hat{\alpha}\mathbf{N} + (\mathbf{X} \odot \mathbf{N})\,\hat{\boldsymbol{\gamma}} \end{aligned}$$

[12]It should be noted that the company did not explicitly say population and race were part of the selection criteria. Instead, they said something along the lines of "stores serving a similar population as the original stores."

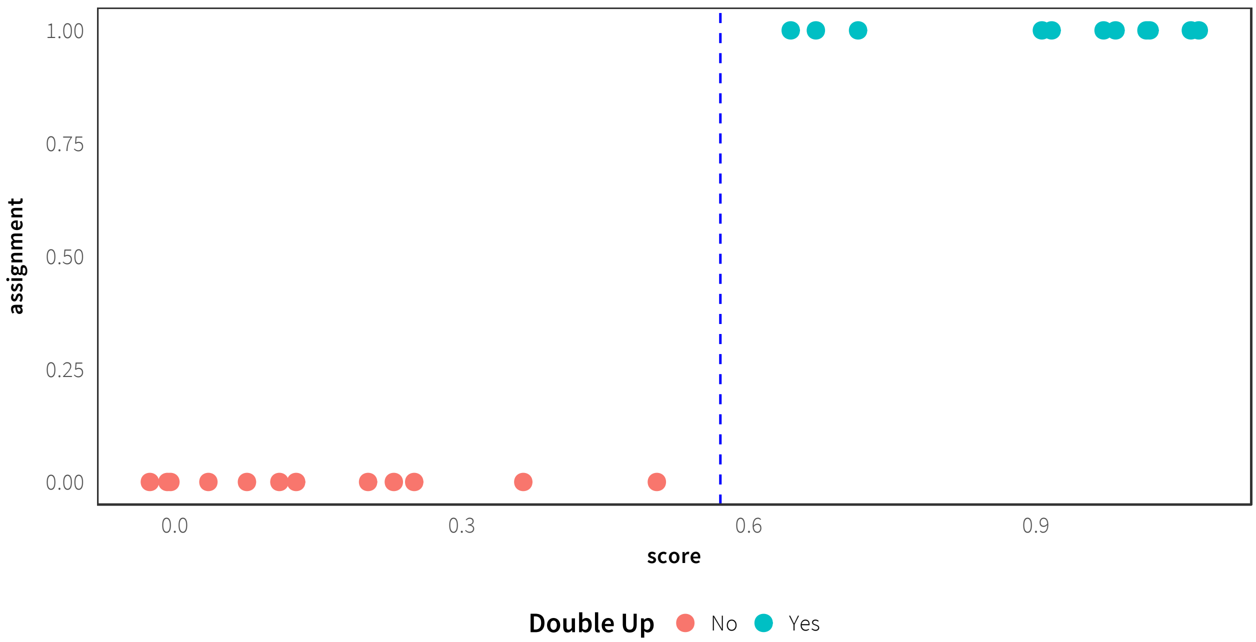


Figure 1.4: Store Score vs Double Up Assignment

$\boldsymbol{s}$ are the fitted values of the estimated linear probability model; $\mathbf{D} \in \{0, 1\}$ is a $n \times 1$ vector of store assignments to Double Up; $\mathbf{X}$ is an $n \times k$ matrix of normalized observable covariates that determine assignment; $\mathbf{N} \in \{0, 1\}$ is an $n \times 1$ dummy vector denoting the closest pilot store aka "Node", where 0 is `Node 0` and 1 is `Node 1`. $\odot$ represents element-wise multiplication aka "Hadamard product".

Stores were sorted by the fitted values of the model, $\boldsymbol{s}$. There is perfect separation between Double Up stores and those without (see Figure 1.4). Therefore, the top 11 stores by score value are all Double Up stores. The next 12 stores by score value are then allotted to the control group.

Motivation for Matching

Not all treated stores will be matched to a control. As mentioned, this is due to the nature of how the 17 treated stores were selected. The parent company intentionally selected stores with some of the highest EBT (aka SNAP Electronic Benefit Transfer (EBT) Card) sales that were also within relatively similar geographic locations. This reduced the burden of advertising and implementing DUFB for The parent company. The unfortunate downside of this implementation is that it effectively removed any likely matches for treated stores located in the most Urban areas (e.g. Grand Rapids and Battle Creek).

Here is an example to illustrate why it is infeasible to matching all treated stores and instead expand selection algorithmically on observables. If we calculate the percentage of the population by *zip code* that is African American then split the data into treatment and control groups, we get the following:

```
#> Difference in Means (Treated - Control) = 7.848600

#>
```

```
#> Population, % Black (Treated, Top 10):

#>  [1] 26.060929 21.945444 19.790582 18.880000 18.688795 14.693513 11.857163
#>  [8]  8.531952  5.644237  5.644237

#>
#> Population, % Black (Control, Top 10):

#>  [1] 8.575720 8.141256 7.271242 5.057010 4.613969 4.374678 3.256329
#>  [8] 2.955071 2.676733 2.593660
```

What these results tell us is how potentially distinct the populations are within the zip codes containing the treated stores. Sorting population percentages in descending order, no good match exists within the control stores for the top 7 treated stores. One variable is the simplest case; matching only gets more difficult as one brings in more variables to match.

Considering the separation between some of the treated stores and all of the control stores, it was prudent to rethink the store selection and matching strategy.

It must be noted that matching is not a necessary step during every design phase. It is, in large part, a way to hedge against the possibility that merely selecting the next top 15 stores by EBT sales could sour the estimates. Matching a smaller set of treatment stores against a larger pool of controls can often produce estimates less sensitive to even the smallest changes in some model specifications (Imbens and Rubin, 2015). However, other models and tools (like regression) are in relatively unperturbed by a lack of design-phase matching, but still benefit from having a larger sample size (Angrist and Pischke, 2008).

Matching Details

Like most data-dependent endeavors, the most tedious part of matching the stores was obtaining enough variables. Once enough data were obtained, variables were selected on how best they captured data from the following dimensions:

- Demographics (e.g. race)
- Income/wealth
- Population density (e.g. urban vs rural)
- Store attributes
- Store EBT sales

One may assume that more variables makes matching easier. This is only true insofar as it provides one with a large pool of options. It is still necessary to carefully select how many variables one is using because matching becomes more and more difficult with each added variable used. This is especially true with a small sample size.

The matching covariates that were finally selected are:

- `pct_black` : Percentage of population that is black (zip code level)
- `dens_pop` : The population density (people per square mile, zip code level)

Table 1.2: CEM Match Matrix

	G0	G1
All	44	17
Matched	14	3
Unmatched	30	14

- `income_p50_snap_yes` : Median income for people who have received SNAP or similar assistance (zip code level)
- `store_n_associates` : The number of associates employed in each store.
- `ebt_sales_pct` : Percentage of total stores sales attributed to EBT/SNAP.

Results of Match

`G0` represents the "control" group and `G1` represents the "treated" group. One can observe that 3 "treated" stores were matched to 14 "control" stores. Each of the 3 treated stores was matched to its closest control store by driving distance.

Covariate Cut-points

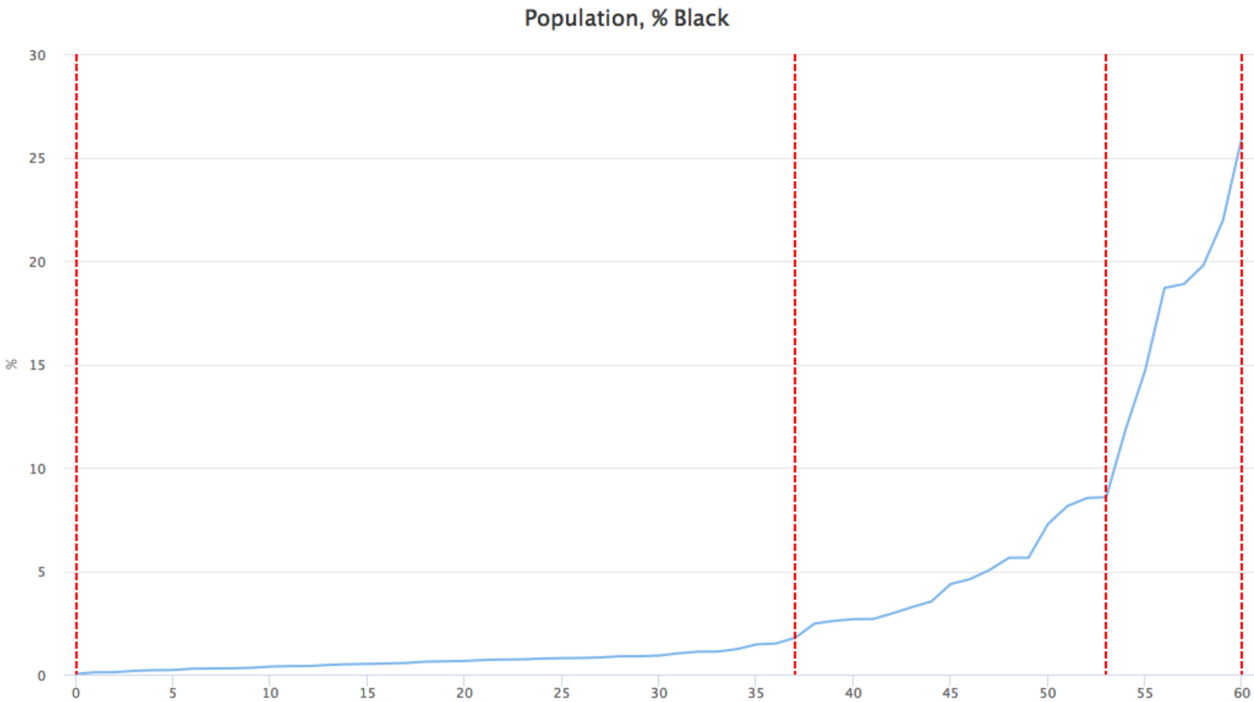
The CEM procedure depends heavily on the "cut-points" selected for each variable. This is akin to setting the cut-off points when turning a continuous variable into a categorical variable. For example, when converting income values from dollars into `low-` `middle-` and `high-income` groups, at least 4 cut-points are required (2 of which are the maximum and minimum). What the other 2 cut-points are will greatly affect the match. This leads to the question, for example, should the cut-points be `25000` and `100000` or perhaps the median and the top 10%?

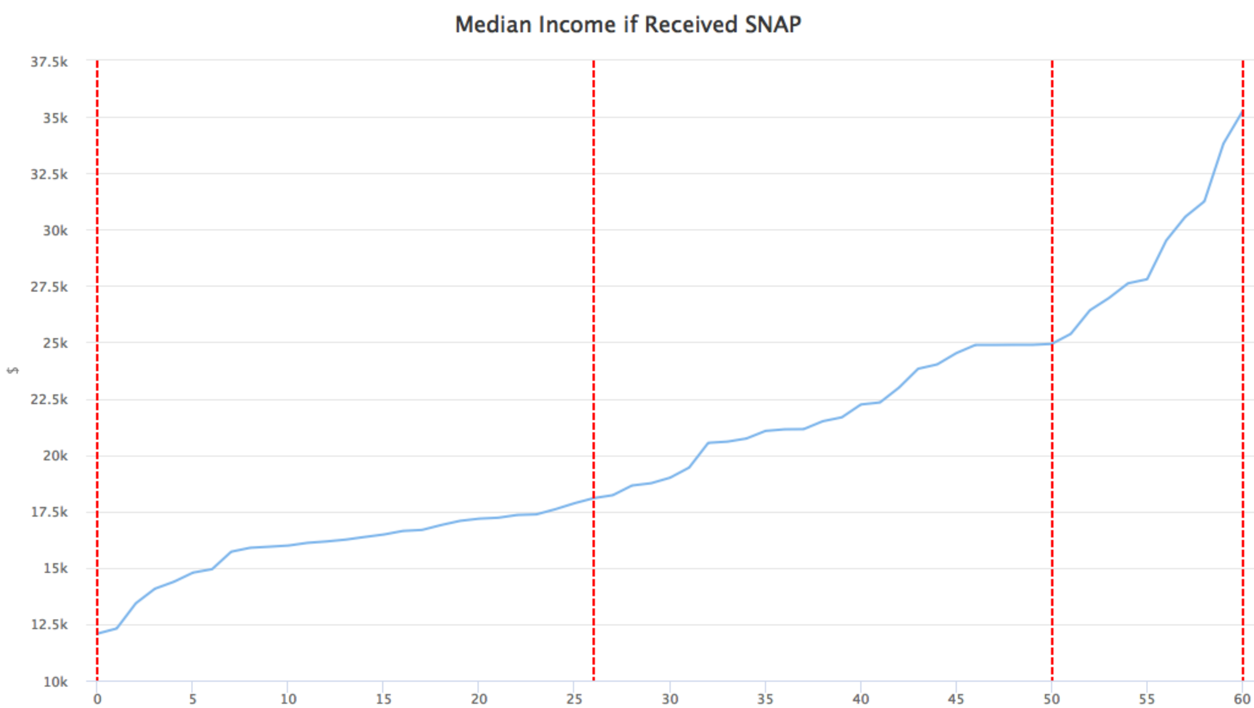
For the matches produced, the following cut-points were created.

```
#> $pct_black
#> [1]  0  2 10 40
#> 
#> $dens_pop
#> [1]    0  200 1000 5000
#> 
#> $ebt_sales_pct
#> [1] 0.00 1.65 3.00 5.00
#> 
#> $store_n_associates
#> [1]  20  40  60  80 130
#> 
#> $income_p50_snap_yes
#> [1] 12000 18000 25000 40000
```

Understanding why is best explained using a visualization. Below are graphs of the variables `pct_black` and `income_p50_snap_yes` with their corresponding cut-points. The aim of each cut-point is to balance the creation of reasonably sized partitions while still marking obvious shifts in the underlying distribution.

For example, in the first plot (`pct_black`), there are clearly points where the slope dramatically increases — and then spikes — in the percentage of African Americans. But in the second plot, the slope is more gradual, so the partitioning is aimed more at getting relatively balanced groups.





Methods

Set up

Recall the research question of interest—whether the DUFB incentive increases spending on fresh fruits and vegetables (FV) within stores participating in the program. The outcome variable, in this case, is total spending on FV.

I've considered other possible values for the outcome variable. For example, the proportion of dollars spent per transaction or the total ounces of FV purchased. Each added an extra layer of complication. Using proportion of expenditure per given transaction would vary wildly, particularly with small transactions, and would creating two mass points at zero and one (no FV purchased and only FV purchased). Total ounces depends on the variable and quality of the data. I cannot be sure the data received will contain counts or ounces for fresh fruits. Fresh fruits generally do not have UPC values. Dollars spent (expenditures) on FV gets to the heart of the question and is the data guaranteed to be in the data.

Unfortunately, using the total expenditure of FV is complicated by 3 problems.

1. Purchases not linked to customers.
2. Outcome Variable with non-trivial amount of zeros (corner solutions).
3. Consumers maximize across multiple product types.

The first problem is not specific to the outcome variable. The inability to link purchases to individuals means I cannot use Panel Data Methods at the customer level; I will be unable to look at how customers behavior changes over-time and cannot control for unobserved customer heterogeneity. I'm instead limited to methods for repeated cross-sections over-time.

The second and third problems are directly related to the preferred outcome variable. I anticipate a non-trivial amount of zeros because I expect to observed a large fraction of transaction where no FVs are never purchased. Likewise, FV expenditures are often a part of a basket of goods. Just as I anticipate a non-trivial amount of zeros, I also anticipate that FVs are not purchased independently of other goods. When spending their money, consumers optimize expenditure across a large set of options. This optimization is also complicated by the first problem (cannot link) but I will go into more details later.

Methods Overview

Difference-in-Difference-in-Differences

I want to measure the difference in SNAP EBT dollars being spent or redeemed on fresh produce. If the incentive is working, then I should see in increase in SNAP EBT dollars spending on fresh produce within stores implementing the DUFB incentive. I do not think it is enough to assume the DUFB incentive, if effective, will be measurable without considering heterogeneity. That is, if a store's implementation of DUFB affects individual behavior, the effect could be hard to measure if measured across the entire distribution of FV purchases, instead of the subset of SNAP EBT dollars spent.

The Difference-in-Difference-in-Differences (DDD) regression is a fitting framework if I expect the SNAP population that patrons DUFB (experimental) stores to be systematically different from the SNAP population that patrons the non-DUFB (non-experimental) stores.[13] Differencing along time, store experimental group, and the type of transaction (SNAP or not) will reasonably capture any systematic differences.

This is unfortunately complicated given that I cannot link transaction to individuals. But I can tell if a purchase was made with SNAP EBT dollars or that it redeemed DUFB points. This is enough to split transactions into SNAP/DUFB-redeemed versus other standard transactions. This provides a way of grouping to perform a DDD.

Any model like DDD that is estimate via OLS, however, ignores the second and third problems. I think it is prudent to consider consumer choice behavior and the mechanisms that generate zero-expenditures. Numerous demand models exists for cross-sectional data that, with a few assumptions (like each transaction represents a distinct individual), may better estimate the impact of the DUFB incentive than straight OLS.

There is a vast literature on consumer purchasing behavior aka choice models (see Train (2009) for an introductory overview). Multiple-discrete choice models, in particular, have become popular given the increased availability of transaction-level (scanner) data (Dubé, 2004; Hendel, 1999). Multiple-discrete choice models, however, are too product-specific. It is not important for me to model which brand and quality of bananas or carrots were purchased. What is important to me is how much was spent on bananas, carrots, and other fruits and vegetable types versus other non-FV types. Remember, my outcome variable is expenditure on *total* FV spending. I'm far more concerned about whether or not consumers are observed buying *any* FV than I am with the exact types. That said, expenditure on non-FV is also important. *Therefore, what is needed are model framework flexible enough to handle a continuous outcome variable with a non-trivial amount of zeros (corner solutions) and multiple types of goods.*

Continuous Outcome Variables and Corner Solutions

Corner solutions for expenditures can occur for various reasons. Pudney (1989) covers three of the most likely mechanisms producing "true" zeros in cross-sectional data. The first is that the data was gathered in too short a period of time for the purchase to occur. This problem is far more common in cross-sectional data of infrequently purchased goods (i.e. durable goods like cars or refrigerators). The second is due to a supply side factor the customer has no control over. For example, there could be a shortage of FVs or none are available. Imagine searching for FV purchases in collection of convenience store data. Many zeros would exists because some convenience stores do not sell FVs. The third zero results from a customer's decision as a function of prices, preferences, and income constraints. A zero is perhaps observed because FVs are too expensive and the customer can get larger quantities of other, equally or more preferred, foods for the same price.

I expect first mechanism could apply to food purchases under specific circumstances. If data are left disaggregated or the period of observation is shrunk substantially, zeros for FVs will exists due to infrequency of purchase. For example, customers that make frequent trips to the store may not always buy FVs. A cross-section of data from one day may have a non-zero FV value for the customer but

[13] See Wooldridge (2010) for more details. I'm assuming familiarity with the DDD model.

zero the next. The third mechanism applies very naturally to the grocery store environment (classical utility theory). The second will require further verification. I do not expect to find out that there were shortages of FVs in the stores observed, but I could be wrong.

Humphreys (2013) and Carlevaro et al. (2016) discuss cross sectional models that accommodate corner solutions—Tobit, *Two-Part*, and *Hurdle* models, specifically.[14] The classic "corner solution" regression model is the Tobit model. Corner solutions are utility maximizing and no assumptions are made about the decision not to purchase/consume. In contrast to the Tobit model, the decision to participate in consumption is explicitly formulated in the Two-Part and Hurdle models. Participation is the "first hurdle", the amount purchased/consumed is the "second hurdle".

In the "Naive" Two-Part model, the decision to buy is estimate separately and sequentially from how much (amount) to buy; Probit is used to estimate the decision-to-buy process and OLS is used to estimate the amount purchased. The Double Hurdle model estimates both these decision simultaneously via maximum likelihood. The full Double Hurdle model allows for correlation between the error terms in the decision and amount/consumption equations (Jones, 1992). Imposing the assumption that unobservable factors between the decision and amount decisions are uncorrelated reduces the full "Jones" Double Hurdle to the "Cragg" model (Cragg, 1971).

It is unclear to me if Two-Part or the Double Hurdle models that formalize "participation" are necessary when considering FV purchases. This makes more intuitive sense for something like cigarette or alcohol consumption, where zero-expenditure can be the result of abstention or price/income constraints.[15] There are, after all, consumers who will never consume cigarettes or alcohol, even if free (abstention). But I'm not sure if an abstention mechanism is reasonable when considering FV purchases. Do consumers really opt out (or abstain) form buying FVs altogether?

An infrequency of purchase mechanism for FVs, however, seems more reasonable. Deaton and Irish (1984) introduced this mechanism as an expansion to the Tobit model. This Double Hurdle has been used to model infrequent purchases of butter, pork, and prepared meals (Yen and Su, 1995; Su and Yen, 1996; Newman et al., 2003). I find it reasonable to expect that, for any given daily cross-section of data, some of the zeros observed will be due to infrequent purchase of FVs.

It's worth noting the general increased popularity of Hurdle models as alternatives to Tobit, and other related models, like Adjusted Tobit and Heckit models. There is a long existing debate over which models perform better, with most of the criticism falling on Tobit. Monte Carlo comparison under different error distributions and exclusion restrictions find Hurdle models consistently out performing Tobit and Heckit models, both in model fit and coefficient bias (Hay et al., 1987; Manning et al., 1987). Monte Carlo studies have also been used to defend both approaches, encouraging a more flexible, case-by-case approach on choosing the appropriate model (Leung and Yu, 1996; Dow and Norton, 2003; Madden, 2008). But applied work in the applied social science, like health care expenditure and consumer purchases, generally tend to favor use of Hurdle models over Tobit or Heckit models (Yen, 1993; Smith and Brame, 2003; Stewart, 2013).

Multiple Goods

[14]Wooldridge (2010) does not appear to differentiate between "Two-Part" models and "Hurdle" models. I will use it following Humphreys (2013) language, which does.

[15]See García and Labeaga (1996) and Aristei et al. (2008) for abstention-style hurdles.

The limitation of Tobit and other hurdle models is the estimated demand of a single good. Ideally, expenditure on different types of goods better captures the decisions and purchasing behavior of customers within a grocery store. That is, instead of collapsing all expenditure on FVs into one outcome variable, the model would allow for customers to optimize across *multiple* goods of different types, while also allowing corner solutions. Multiple discrete choice models allow the purchase of multiple types of goods, but do not capture the intensive margin (the amount) of the good purchase/consumed.

Dubin and McFadden (1984) construct a *discrete-continuous* model were consumers can select from multiple goods/options but that they are mutually exclusive, perfect substitutes. The *discrete* component of the model captures the decision to buy a non-zero amount; the *continuous* part captures the amount purchased/consumed/utility acquired. The mutually exclusive, perfect substitute condition, however, means that one cannot be observed buying/consuming more than one good/option.

For any observed trip to the store, zero-expenditure in FV implies non-zero expenditure for some non-FV. Ignoring this seems unwise and I plan, at a minimum, to implement a model optimize along two dimensions—FV and non-FV. The best model I've found is the *multiple* discrete-continuous model extreme value (MDCEV) model introduced by Bhat (2005). MDCEV is an extension of the Kuhn-Tucker based model developed by Wales and Woodland (1983). MDCEV allows non-zero consumption across multiple goods. Kim et al. (2002) solved the intractability of the Wales and Woodland (1983) model. Bhat (2005) simplified both to be more realistically applicable.

In the next subsection, I will formally introduce the different modeling structures I intend to use in my paper. I will first introduce the DDD framework were estimation via OLS is sufficient. I then expand from OLS to Tobit and other Hurdle models before expanding into the more complex MDCEV model. The share goal, across all models, is the best possible measurement of the impact the DUFB incentive has on fruit and vegetable expenditures between participating and non-participating stores.

Difference-in-Difference-in-Differences (DDD)

I observe transaction $i = 1, ..., L$ in store $j = 1, ..., N$ across $t = 1, ..., T$ days. Let y_{ijt} be total FV expenditures for transaction i in store j on day t. The DDD regression is

$$\begin{aligned} y_{ijt} = \alpha_0 &+ \alpha_1 dE_j + \alpha_2 dS_i + \alpha_3 dE_j \cdot dS_i \\ &+ \theta_1 dP_t + \theta_2 dP_t \cdot dE_j + \theta_3 dP_t \cdot dS_i \\ &+ \delta dE_j \cdot dS_i \cdot dP_t + \boldsymbol{x}'_{ijt}\boldsymbol{\beta} + \lambda_t + \epsilon_{ijt} \end{aligned}$$

where dE_j represents store assignment to experimental group, dS_i represents a SNAP or SNAP related transaction (target group), and dP_t represent the treatment period, August - December. λ_t captures daily (time) effects and $\boldsymbol{x}'_{ijt}$ is a vector of observable characteristics about transaction i in store j on day t. The coefficient of interest is δ. ϵ_{ijt} are idiosyncratic errors at the transaction level.

Again, I do not observed individuals, only transactions. Yet I think it reasonable to assume that the structure of daily transaction data more closely resembles that of repeated cross-sections than of panel data. Assuming it was possible to link individuals to purchases and build panel data. The same individual would likely observed *sporadically* within in a given month. That is, were this to be panel of the same N individual shoppers across T total days, many—if not most—of those days would have missing data. There would certainly be shoppers observed multiple times per week, but I expect such shoppers to be rare. I certainly would not expect to have a balanced panel and no shopper would be observed all 365 days.

I therefore find it reasonable to treat each day of observed data as a single independent cross-section of L transactions generated by an unknown $N \leq L$ individuals across J many stores. Aligned sequentially, these form a repeated cross-sections over-time. Each transaction also falls naturally into a cluster—the store where it occurred—that is time-invariant and determined prior to the data being collected.

The model DDD model above can be improved by introducing store effects. These effectively capture experimental assignment and all other time-invariant store-level characteristics. The dE dummy, for example, drops out. The notation can also be cleaned up by condensing the others dummies, emphasizing only variation. The spruced up model is

$$y_{ijt} = \gamma_j + \lambda_t + \phi_0 D_i + \phi_1 D_{ij} + \phi_2 D_{it} + \phi_3 D_{jt} + \delta_t D_{ijt} + \boldsymbol{x}'_{ijt}\boldsymbol{\beta} + \epsilon_{ijt} \tag{1.1}$$

where γ_j now represents store effects, $dS_i \equiv D_i$, and the remaining dummy variables $D_{ij},\ D_{jt},\ D_{it},\ D_{ijt}$ represent the dimensions along which they vary—i transaction type (SNAP or not), j store experiment group, and t day during DUFB treatment period (August - December). To capture more detail than just the average, δ_t is allowed to vary by day.

Unobserved Effects

I still do not know what the transaction characteristic variables will be and hence do not know what variables go into $\boldsymbol{x}'_{ijt}$. I do know, however, that without panel data, I have no methods for dealing with unobserved individual effects. That is, some unobserved individual effect c_i likely exists such that $e_{ijt} = c_i + u_{ijt}$ where $\exists t \ni E[\boldsymbol{x}'_{ijt} c_i] \neq 0$. In short, my estimates will be biased due to an omitted variables problem.

I'm am still thinking about how I can capture part of the unobserved individual effect c_i. I am open to suggestions.

Tobit and other Hurdle Models

Calculating fruit and vegetable expenditures, y_{ijt}, for each transaction will result with a non-trivial amount of zeros. These zeros are not "censored" values in the Heckman selection problem sense. They are genuine zeros aka "corner solution". But the mechanisms behind the zeros is unknown.

Tobit Model

The Tobit model is agnostic to the economic mechanism generating corner solutions. It is the basic approach when little else is known other than the decision not to purchase fruits and vegetables is resulting in zero-expenditures. The basic structure is

$$
\begin{aligned}
y^* &= x'\beta + u \\
y &= max(0, y^*)
\end{aligned}
$$

where y^* is a latent variable and y is observed. Tobit can be generalized beyond requiring homoskedastic error terms but requires normality. This is important because error terms in repeated cross sectional data are assumed independent *not* identically distributed. In other words, heteroskedastic. Other than error term specifications, any additive and linear-in-parameters regression can estimated using Tobit. In other words, I can set y^* equal to equation (1.1).

For details on the likelihood functions, see Amemiya (1984).

Hurdle and (Naive) Two-Part Models

Hurdle models generally have two parts (i.e. "Double" Hurdle)—a decision-to-buy (participate) and the amount to purchase (consumption) (Jones, 1989). Let $I^* \in 0, 1$ indicate the decision to purchase fruits and vegetables and y^* be the amount of dollars spent. Both are latent variables. Let y be observed expenditures. Formally,

$$
\begin{aligned}
I^* &= z'\gamma + v \\
y^* &= x'\beta + u \\
y &= I^* \times max(0, y^*)
\end{aligned}
$$

$$
\begin{aligned}
(u, v) &\sim N(0, \Omega) \\
\Omega &= \begin{pmatrix} \sigma_u^2 & \sigma_{uv} \\ \sigma_{uv} & \sigma_v^2 \end{pmatrix}
\end{aligned}
$$

The Jones (1989) "Full" Double Hurdle model and the Cragg (1971) have the same framework. The distinction is that in the Cragg model assumes no correlation between u and v (i.e. $\sigma_{uv} = 0$). The likelihood function for the Cragg model is, in other words, a simplification of the Jones model. Formally, the Jones model is

$$
\begin{aligned}
L = \prod_0 & \left[1 - \Phi\left(\frac{z'\gamma}{\sigma_v}, \frac{x'\beta}{\sigma_u}, \rho\right)\right] \times \\
& \prod_+ \Phi\left[\frac{\left(\frac{z'\gamma}{\sigma_v} + \rho(y^* - x'\beta)\right)}{\left(\sqrt{1-\rho^2}\right)}\right] \frac{1}{\sigma_u}\phi\left(\frac{y^* - x'\beta}{\sigma_u}\right)
\end{aligned}
$$

Once again, setting the DDD to be y^* is not a problem. The main problem is determining the participation equation I^*. What factors variables participation i.e. the decision-to-buy fruits and vegetables? Variables about individual characteristics would certainly be helpful here but that isn't possible. There is clearly some mechanism driving consumers to buy or not buy fruits and vegetables. The likeliest candidate is data on *other* products purchased within a given transaction. The existence of complimentary goods (e.g. olive oil, meats, etc) may increase the likelihood of observing FV purchases within the same trip. The size of the shopping basket likely increases overall chances of FV purchases.

Likely day of the week or week of the month, since government benefits and pay schedules may increase the chances of shopping trips in aggregate.

I anticipate estimate both the Jones and Cragg model as a robustness check. But I do not expect $Cov(\sigma_u, \sigma_v) = 0$ given I cannot capture individual effects. Unobserved individual effects likely affect both participation and consumption. In both equations, individual effects are absorbed by the error terms, making them correlated by construction.

I will also estimate a "Naive" Two-Part model, where the participation and consumptions equations are estimate independently from the other (Probit + OLS). Again, just another comparison point.

Multiple Discrete-Continuous Extreme Value Models

The Bhat (2005) Multiple Discrete-Continuous Extreme Value (MDCEV) framework allows for choice along a vector of non-mutually exclusive goods. Non-negative consumption is allowed in across all goods.

I will attempt to use a later iteration of the MDCEV model from Bhat (2008). The distinction that interest me is that the Bhat (2008) model allows for price variation across goods and explicitly formulates the Kuhn-Tucker constraints using expenditures. The econometric model is as follows.

Utility from purchasing vector of goods $(x) = (x_1, x_2, ..., x_k)$ is defined as

$$\begin{aligned}
\tilde{U} &= \sum_k \frac{\gamma_k}{\alpha_k^*} \psi(z_k, \epsilon_k) \left[\left(\frac{y_k}{\gamma_k p_k} + 1 \right)^{\alpha_k} - 1 \right] \\
\psi(z_k, \epsilon_k) &= exp(z_k' \beta + \epsilon_k) \\
\sum_k p_k &= Y
\end{aligned}$$

where z_k is a vector of attribute variables about product k and of the consumer, y_k is expenditure on product/good k, p_k is the price, and Y is total expenditure on basket of goods (x). ϵ_k are idiosyncratic shocks with an extreme-value distribution. To understand the role of ψ_k, α_k and γ_k, see Bhat (2008) for details.

Using the first good as a reference group, the KT conditions that solve the optimal expenditure problem (the Lagrangian above) are

$$\begin{gathered}
V_k^* + \sigma\epsilon_k = V_1^* + \sigma\epsilon_1 \text{ if } y_k^* > 0 (k = 2, 3, ..., K) \\
V_k^* + \sigma\epsilon_k < V_1^* + \sigma\epsilon_1 \text{ if } y_k^* = 0 (k = 2, 3, ..., K), \text{ where} \\
V_k^* = \sigma z_k' \beta + \sigma(\alpha_k - 1) \text{ln} \left(\frac{y_k}{\gamma_k p_k} + 1 \right) - \text{ln} p_k
\end{gathered}$$

where V_k is identified only when α_k or γ_k is fixed (both terms estimate related "satiation" behaviors). See Bhat (2008) (equations 18 and 19) for the Jacobian and closed form expression for the probability of spending y_k^*. Example likelihood function to solve the equation for $i = 1, ..., L$ transaction (or N individuals) in a given cross-section can be found in Bhat (2005) (equation 18) and Bhat and Sen (2006) (equation 8).

Expected Challenges with this Model

Despite the theoretical advantages of the MDCEV framework (i.e. optimization over multiple goods allowing corner solutions), there are a few challenges I anticipate with using the MDCEV framework.

The first is a concern about prices. The most effective way to incorporate price variation is to make the basket of available goods equal to the full universe of observed products. This would likely be huge. The MDCEV framework is flexible enough to do it, but my fear is that it will lead to some difficulties in interpretation. In reality, however, I don't care as much about expenditure at the product level as much as I do about expenditure on particular types. That is, I care more about spending on FVs versus non-FVs. Therefore, at the simplest level, my vector of possible goods would be just 2. However, how would I price FV versus non-FV? I could construct a price index for just those two groups but it would combine far too many distinct food types to be reasonable. Moving towards something like having between 20 to 40 general food categories seems like better approach. For example, Harding and Lovenheim (2014) estimate the prices for 33 different product groups by using the Stone price index, which depends only on observable price values.

The second concern is programming related. There are no available packages that implement the MDCEV package. I would have to adapt the GAUSS code provided by Bhat on his website in order to get the model running. This isn't a concern about being able to do it as much as the amount of time it would require to learn/understand GAUSS in order to implement it in R or Python.[16]

[16] It looks like some folks at the company Mobility Analytics[17] have started, which is very promising.

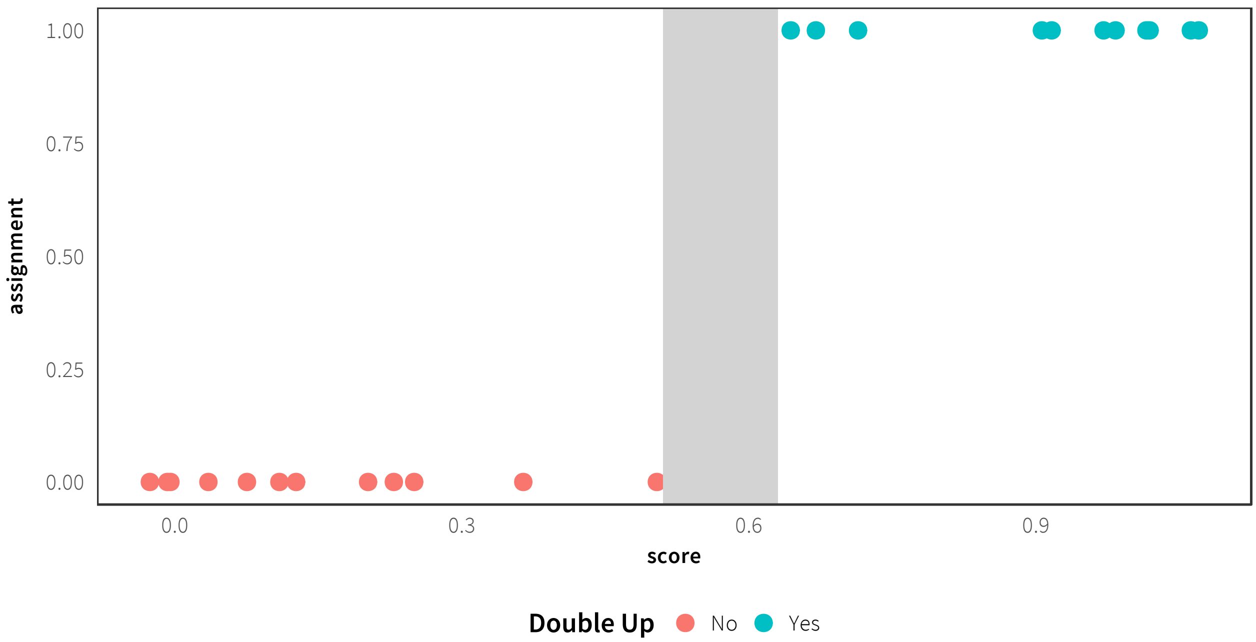


Figure 1.5: Store Score vs Double Up Assignment with Uncertainty Band (light gray)

!!BELOW HERE BE DRAGONS (SUPER WORK IN PROGRESS)!!

Regression Discontinuity (RD)

I will also perform a secondary Regression Discontinuity (RD) Design analysis.

In the Store Selection section, I discussed the construction of the score function $\boldsymbol{s} = \widehat{P(\mathbf{D} = 1|\boldsymbol{X}, \boldsymbol{N})}$. The score of each store can be determined via observable data, $s_j = \widehat{P(D_j = 1|\boldsymbol{x}_j, n_j)}$. These scores, when ordered, produced perfect separation between experimental stores and control stores (see Figure 1.5).

An RD design requires a *running variable* where, above some value c, the probability of being assigned to the experimental group is 1. Assume I make the score function $\boldsymbol{s}$ my running variable such that $D_j = \mathbf{1}[s_j \geq c]$.

In my case, assignment D_j is determined by s_j by construction. Recall that s_j is a function estimated on observable covariates. These are the same observable covariates the company used to determine assignment for a subset of stores. I used a linear probability model to estimate the score function and the estimated model perfectly predicted assignment. I then ordered stores by their score value and selected the next 12 unassigned stores.

This problem is that I do not actually know c. I only know that $c \in (0.50, 0.64)$. The light gray band in Figure 1.5 displays the possible values of c. The problem, in essence, is that I do not have—and never will have—enough stores, so I'm lacking density around where the separation occurs.

I propose to estimate the RD design using various values of c. The perpetual gap means any model estimate to the left or right of some $c_0 \in (0.50, 0.64)$ will have to be extrapolated up to c_0.

Set-up

The outcome of variable, as before, is *the total daily amount of dollars spent on fruits and vegetables per store transaction*. I decided on using days as the unit of observation to increase the sample amount of data for estimating. I expect there to be enough transactions per day for this to be possible. The time frame will be August - December (months 8 - 12) of 2016, when the DUFB incentive is place. Only SNAP transactions will be considered and transactions will be pooled. Given that stores may vary in sales volume sold, I may divide by total SNAP dollars spent so that the outcome variable is instead a proportion. In total, there will be 11 treated (experimental) stores and 12 control stores.

Let y_{ijt} represent the outcome variable where $i = 1, ..., K$, $j = 1, ..., N$ denotes stores across $t = 1, ..., T$. Let c denote the cutoff; s_j the score computed for store j; and D_j the assignment variable. Each draw (or row) of data for store j is a vector (y_{jt}, s_j, D_j)corresponding to a single day. λ_t are time effects and u_{jt} is an idiosyncratic store level error term.

The RD model I propose follows the setup of Lee and Lemieux (2010) (section 4.3):

$$y_{jt} = \alpha + \lambda_t + \rho D_j + \gamma(s_j - c) + \delta D_j(s_j - c) + u_{jt}$$

Expected Results

Plotting RD data and observing a visual gap is standard. The first thing I would do is plot the outcome variable of interest—total daily (fraction of) SNAP dollars spent on fruits and vegetables–against the running variable s_j. A total of $N \times T$ (23 × 153) data points exists; each value of $s_j,\ j = 1, ..., 23$ will contain $T = 153$ points.

The graph produced will have more gaps compared to conventional RD graphs. In more conventional RD graphs, each point represents a single value for a single person (e.g. score on an exam), producing more points along the running variable axis. I do not have enough stores to produce enough s_j values for this to be possible. Instead, this RD design plots multiple points per stores, creating a distribution such that the mean (or median) value (with some confidence interval) becomes the fitted values of interested. Estimation wise, this distinction changes very little; $E[y_{jt}|s_j, D_j]$ effectively fits a mean value to each different store. But graphically, what matters in my RD is the overlap between distributions before and after the cut-off point.

Chapter 2

The Durham Connects RCT and Requests for Social Services

Motivation

Research Question

> For families that participated in the Durham Connects (DC) RCT program, did the intervention (education, resources, and nurse contact) impact the future probability of applying for social services?

Hypothesis: I expect to observed a larger proportion of families that participated in Durham Connects requesting social services. My hypothesis is that the information and nurse contact provided to DC families decreased the complexity of applying and navigating the social safety net. As a result, DC families are observed more frequently not because they are, on average, in greater need of assistance, but because they have been informed and encouraged to seek out available resources.

Concept in a Plot

Figure 2.1 represents what I would ideally observe in the data should my hypothesis prove correct. Unit of analysis is aggregated counts of observed applications (requests) per person between time `0` and `T` (`T` is some fixed unit of time, perhaps 1 or 2 years). Time `0` is the date of birth for the baby that is (or is not) assigned to the Durham Connects RCT.

The histogram of green bars represents counts for parents that were randomly assigned to DC. Red bars are those assigned to the control. Should my hypothesis prove correct, then I would see, in aggregate per person, more requests for services families that were assigned to DC.

Note that these distributional comparisons are not the best way to test any hypothesis with what is event data. For more details on methods, please see the Methods section.

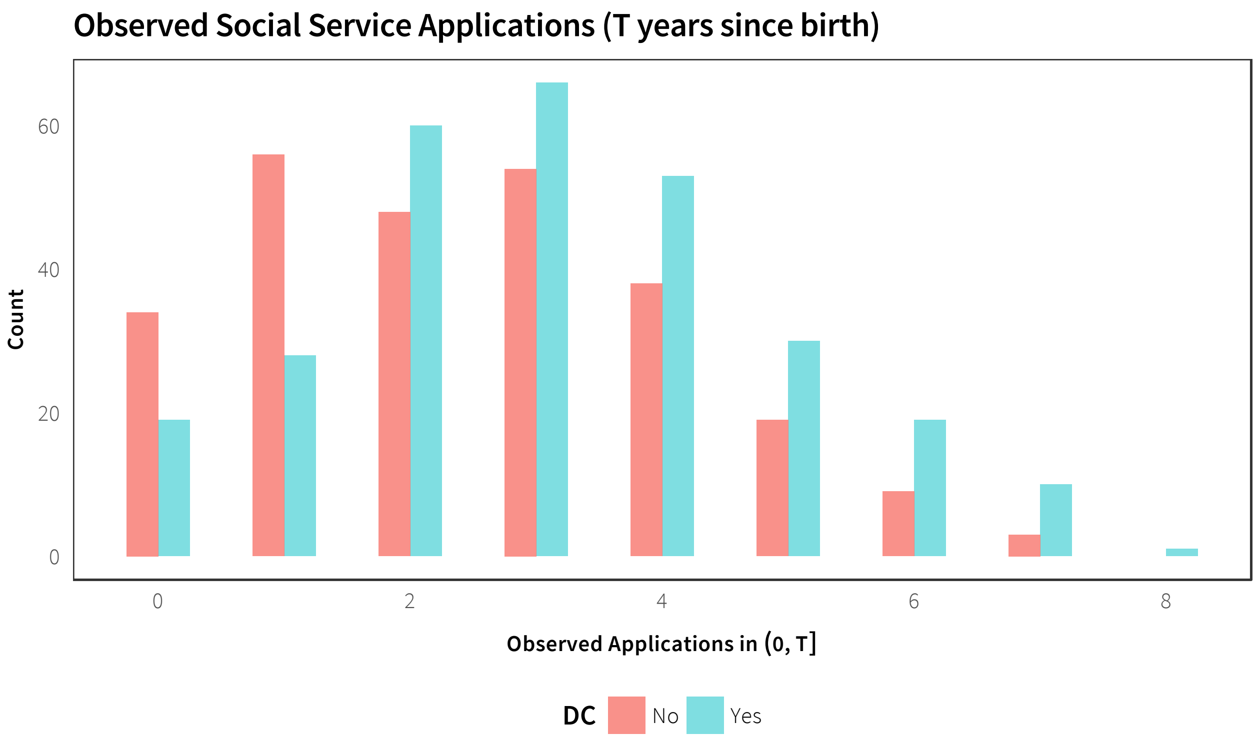


Figure 2.1: Example of Hypothetical Result (Fake Data)

Introduction

Not all individuals that are eligible for social services apply to receive them. The proportion of those eligible for a social service that are actually receiving the service is known as the "take-up" rate. Ideally, given complete information about eligibility and the existence of social service programs—and zero costs in apply for, or receiving, government assistance—the take-up rate would be 100%. To do otherwise would be to "leave money on the table"—a violation of neoclassical economic theory. In the United States and across the globe, however, most social service programs never reach a full take-up rate.

Explanations for low take-up rates are numerous. Economists, for example, developed models of social stigma to explain an individual's decision for not applying for government assistance (Moffitt, 1983). Stigma is a costs that reduces the total utility acquired from receiving government assistance. Stigma and available benefits can vary across individuals. Those that acquire insufficient net benefits once the costs of stigma are included would therefore, reasonably, choose not to apply for government assistance. That is, the take-up rates can fall below 100% within any set of eligible individuals when the costs of stigma outweigh those of the benefit. Stigma can also be extended to include other standard costs, like transaction costs or search costs.

Low take-up rates can also be explained by relaxing standard neoclassical assumptions, like complete information. If one instead assumes that most individuals have *incomplete* information about existing social services programs and corresponding eligibility requirements, then lower take-up rates are unsurprising. Eligibility requirements, for example, are dynamic. Families and individuals can fall in or out of eligibility given a change in policy, leaving families with an outdated understanding of requirements. For example, following welfare reform in 1996, federal requirements shifted for programs like Medicaid, Supplemental Security Income (SSI), and cash assistance (renamed to Temporary Assistance for Needy Families or TANF). Welfare reform also gave more power to states to determine eligibility standards. Dissemination and implementation of these new eligibility requirements was not instantaneous and the confusion lead to a reduction in take-up rates for some programs (Stuber and Kronebusch, 2004). Shifting requirements aside, many families that are eligible for social service programs can be misinformed or confused. It can be difficult to navigate the application process when requesting government assistance. Information about programs and their eligibility requirements is diffuse and complicated. We do not expect everyone to fully grasp how to complete their taxes without assistance or making errors. Likewise, we should not expect families coping with poverty to be fully informed about benefit levels or the application process of every existing social service program.

Behavioral economics offers other compelling reasons for low take-up rates. Status quo bias, for example, is a common task-completion deterrent (Kahneman et al., 1991). Simple inertia means that most people will procrastinate. This can keep any person from completing things like applications, making doctors appointments, or even folding laundry. Bounded rationality and the human tendency to use heuristics for decision making also reduce take-up rates. Often, individuals assume, based on some rule-of-thumb, that they must not be eligible. If this assumption is never challenged, either by seeking out information or having it provided, then they will never discover otherwise. Time-inconsistent preferences also present a challenge for take-up rates. Hyperbolic discounting implies that humans will favor putting off any decision where gains are felt far in the future but losses are felt almost immediately. In applying for government assistance, future benefits are discounted heavily while the short-term cost of applying are not (Currie, 2006).

Perhaps the most simple, albeit depressing, explanation for low take-up rates is intentional bureaucracy. Governments purposefully make the application process burdensome and confusing to deter applications. The take-up rate of TANF, for example, has plummeted since the federal government overhauled welfare in 1996 (Ribar, 2014). TANF gave enormous leeway to states on how to distribute TANF dollars with little oversight. As a result, over the last 20 years states have shifted away from using TANF dollars for its intended purpose of providing cash assistance to needy families. On average, 26 percent of TANF dollars go towards provided basic cash assistance (Schott et al., 2015). This is because state and local governments, particularly in the deep south, where there is a long history of racial animus towards minorities and the "undeserving" poor, have made TANF eligibility requirements unreasonably strict (Keiser et al., 2004). North Carolina, for example, added drug testing a requirement to its TANF program known as "Work First" (Lynn Bonner, 2016). Adding insult to injury, minority families receiving TANF benefits are also more likely to be sanctioned by local caseworkers than white families, losing access to an already difficult-to-acquire benefit (Monnat, 2010). All in all, take-up rates for some social service programs can be low because state and local governments are actively increasing the costs for applying and maintaining benefits.[1]

Low take-up rates should concern policy makers. The positive impact many government programs

[1]It is important to note that there is also research showing application complexity always punishes the poor. When applying for federal student aid, which is open to all families, increased complexity burdens poor families the most (Dynarski and Scott-Clayton, 2006). This further implies that the poor are also those most likely to benefit from targeting marketing and educational assistance (Bertrand et al., 2006).

have on the quality of life of participating individuals is well researched. Following passage of the Affordable Care Act save lives, state Medicaid expansion increased self-reported health status, lowered rates of depression, and reduced financial strain and poverty (Baicker et al., 2013; Sommers et al., 2012; Sommers and Oellerich, 2013). Families that participate in the Supplemental Nutrition Assistance Program (SNAP) increase the nutritional value of their diet and free up cash for use on other priorities, like health care (Bartfeld et al., 2015; Sonik, 2016; Miller and Morrissey, 2017). Increased use of the Earned Income Tax Credit (EITC) increases cash on hand, improves educational outcomes for children, and improves labor market out comes for adults (Grogger, 2003; Bhargava and Manoli, 2012; Dahl and Lochner, 2012). Lastly, eligible participants in the Women, Infants, and Children (WIC) programs improves child health outcomes, especially for the most disadvantage population, compared to eligible non-participants (Bitler and Currie, 2005). Therefore, considering the positive impact of most government assistance programs, it should be a priority to champion any low-cost policy changes that increase take-up rates.

In the next section, I will introduce the Durham Connects (DC) program and how I believe it may help increase requests for social service programs. Note that increasing requests for social services is not equivalent to increasing take-up. A request for service, however, is a necessary first step for any eligible participant to receive benefits. Increased requests, therefore, serve as a proxy for increased take-up rates.

The Durham Connects Randomized Control Trial

The Durham Connects[2] (DC) program is a postnatal nurse home-visiting program designed to be universal, short-term, and less intensive than other nurse home-visiting programs like Healthy Families America and Nurse-Family Partnership. The DC program, in brief, aims to celebrate with new mothers, support and connect then with community resources, and then follow-up if necessary.

Before expanding, a randomized control trial (RCT) of the DC program was implemented from July 1, 2009 to December 31, 2010 to determine its efficacy. The RCT included `4777` births from two Durham hospitals. These two hospitals account for a vast majority of birth in Durham County. The randomization procedure assigned all babies born on even days to the DC program. Babies born on odd days were assigned to the control—the hospital status quo.

Soon after birth, a DC program liaison at the hospital schedules a home-visit by a registered nurse with the infant's family. The initial home-visit is generally schedule about 3 weeks following the birth but can be sooner. During the initial home-visit, the nurse assess and rates 12 factor along 4 domains of family support: health of the mother and child, the safety of the home environment, planning care for the infant, and general wellbeing of the parent(s). Each of the 12 factors received a rating between `1` and `4` from the home-visiting Nurse. Each number represented varying levels of need and risk:

- `1` - No risk/needs. General supportive guidance provided.
- `2` - Low risk/needs. Concerns can be addressed by education and in-home demonstrations during visit.

[2] `https://www.durhamconnects.org/about/`

- 3 - Moderate or high risk/needs. Family are linked with community resources best suited to support risk areas.
- 4 - Urgent needs. Immediate intervention required.

Follow-up visits are scheduled as necessary. Generally, the more support a family needs, the more follow-up visits will occur.

Prior research has exploited the RCT structure of DC. Notably, Dodge et al. (2013) found that the DC nurse home-visiting program reduced emergency medical care costs and increase health outcomes for both mother and child. Furthermore, the DC program was found to be cost-effective, saving an estimated 3 dollars in medical costs for every dollar spent on implementation. In short, DC was impressively successful at improving the physical health and mental health of mother and child.

I'm interested in exploiting the randomized assignment to measure what I would consider a secondary effect of the nurse home-visiting program. An "essential" part of the program was establishing a link between DC families and Durham Social Services (DSS) to "facilitate the … ease of access to and knowledge about eligible services" (O'Donnell and Wright, 2015). I hypothesize that by providing a trusted contact (the nurse), educational resources, and a link to local social services, a secondary effect of the DC program was that participants had a higher take-up rate than non-participants.

The nurse–patient relationship is key to the success of the DC program. The days and weeks following a new birth are periods of high need for both mother and child[3]. High need for postnatal/postpartum support means new mothers and families are more willing to accept help when offered—especially from nurses and nurse-centered programs. Patients, generally, view nurses favorably, but interactions tend to be viewed more positively when they occur outside the hospital/clinic environment (Jansson et al., 2002). Likewise, perceptions about community health programs tends to be more positive when they are known to be organized and conducted by nurses (Kneipp et al., 2009). Nurses that assist in home-visiting program for new mothers, in particular, tend to be viewed very positively by their patients—as both a professional expert and friend (Landy et al., 2012).

While initial DC home-visit may be brief in some cases (e.g. rating is 1), new mothers requiring the most support and resources are visited more than once[4]. Nurses are most involved with families that receive factors rated 3 or 4. In these instances, families are *referred* to Durham Social Services for help. That is, DSS is made aware that the family is in need of a specific type of support and, with the help of the nurse, links up with the family directly. This is distinct to families with factors of only 1 and/or 2. For these families, nurses only *recommend* community services and resources. The option to utilize the resources is then left to the family.

In both cases, however, notice that the nurse is, in one way or another, reducing the costs associated with applying for social services. At a minimum, the nurse reduces the *search* costs for families by provided information for resources. At a maximum, the nurse works connect families with DSS directly to determine eligible services. In the latter case, the nurse, in partnership with DSS, also lowers the *transaction cost* of applying.

I should pause to specify what it meant for a family to be "linked with DSS". It is my understanding that this means a family was put in contact with, or place "on the radar" of, a specific DSS worker

[3]TK seems obvious but need source; can't find good one

[4]TK Need reference showing that new mothers, in particular, are open to help and support

assigned to assist DC families (name omitted). The degree, and success, to which this worker was involved in assisting families apply for services would have had a significant impact take-up rates. I have yet to interview this person, but I plan on doing so.

Data

This paper will depend on two sources of data. The first data source is Durham Social Services (DSS). DSS has generously provided administrative records to determine what services where applied for and by whom. The second data source is the Center for Child and Family Health at Duke University (CCFH). CCFH has already collected and corrected the short-form birth records of all children born during the RCT. While these data are public record, CCFH's data drastically reduces the burden of

Administrative Records

Durham Social Services provided the Durham Children's Data Center (DCDC) with access to their administrative records. The pertinent data within the collection provided is known as the *Scheduler* data. This is a database that logs all request for social services assistance from the DSS.

I do not yet have explicit permission to write about what is in these data. That said, I do not think I'm revealing anything but the obvious by noting that the *Scheduler* data contains name, age, service requested, and date of request. These 4 variable are sufficient to the explain my methodological approach. I have requests multiple years but need to verify if I have all of what is available.

Short-form Birth Certificate Data

Short-form birth certificates are public record. It is possible, for example, to request birth records directly from the North Carolina Department of Health and Human services[5]. Collecting all recorded births in Durham County from the 2 largest hospitals would take a considerable amount of time. Fortunately, the CCFH already collected and corrected the short-form birth certificate data.

Short-form birth certificate includes sufficient information to identify which families were randomly assigned to Durham Connects. It also provides enough information to identify parents should they appear other administrative records that contain name and date-of-birth. Specifically, the child's date-of-birth, the child's name, the mother's maiden name, and the father's name (if present on long-form birth certificate).

These four pieces of information are enough to search and tag when the parent's of a child born during the DC RCT applied for services. The child's date of birth is all that is needed to is required to determine assignment status.

[5] http://vitalrecords.nc.gov/order.htm

Matching Strategy

An important step will be to link the two datasets. This will likely be done by matching on the names of the parents and dates of birth. Fuzzy matching procedures for character strings exists in `R` and `Stata`. I do not think this will too difficult or computationally intensive as the set of names that need to be searched for is quite small (about `5000`).

IRB and Permissions

Currently, I am in the process of submitting an IRB request to access the CCFH's database contain the digitized and curated public short-form birth records.

I am also waiting to hear if I have permission from the director of Durham Social Services to link these two data sets. I have no reason to believe the director will not approve. It is just a matter of having an opportunity to sit down and explain the idea and what it entails.

Methods

Methods for this paper are greatly simplified by the fact that assignment to Durham Connects was *random*. This immediately provides an instrument where, by design, the exclusion restriction is true (always exogenous) to virtually any outcome variable of interest. For this study, I am interested Durham Social Services *applications*, which will serve as a proxy for take-up rate. Requesting (applying for) government assistance does not guarantee receipt of benefits. But it is a necessary step for any eligible individual/family to apply.

A Simple Model of Social Service Demand

A simple economic model helps illustrate how I believe the Durham Connects program may help increase take-up.

Assume all individuals equally value government benefits and incur the same costs when applying for benefits. Individuals, however, have different characteristics. These characteristics determine both eligibility and size of benefits. Let's also assume complete information, rationality, and that errors when applying are non-existent.

Individual i with characteristics vector $\boldsymbol{x_i}$ will apply for government assistance only if the *net benefit* amount is greater than zero. That is,

$$r_i = \begin{cases} 1 \text{ if } NB(\boldsymbol{x_i}, c) > 0 \\ 0 \text{ if } NB(\boldsymbol{x_i}, c) \leq 0 \end{cases}$$

$$NB(\boldsymbol{x_i}) = B(\boldsymbol{x_i}) - c$$

where r_i is the decision to apply for benefits; $NB(\boldsymbol{x_i}, c)$ is the net benefit; $B(\boldsymbol{x_i})$ is the benefit amount as a function of individual characteristics, $\boldsymbol{x_i}$; and c is the cost of applying. Eligible individuals have a value $B_i > 0$. Note that for convenience, $B_i \equiv B(\boldsymbol{x_i})$ and $NB(\boldsymbol{x_i}, c) \equiv NB_i$

My hypothesis is that the Durham Connects program reduces the cost of applying. In this simple model, assume that the DC program drops the cost of applying down to some $\tilde{c} < c$. Let $d_i = 1$ represent assignment to DC. Before assignment, each individual is eligible for net benefits

$$NB_i|_{d_i=1} = NB_{i1} = B_i - \tilde{c}$$
$$NB_i|_{d_i=0} = NB_{i0} = B_i - c$$

such that $NB_{i1} > NB_{i0}$ for all individuals. This implies,

$$P(r_i = 1|d_i = 1) \geq P(r_i = 1|d_i = 0)$$

In words, if assignment to DC reduces the costs of applying, then the probability of applying for all individuals must either increase or stay the same. Under this framework, this would imply that there must exists a population of eligible individuals ($B_i > 0$) such that $NB_{i1} > 0$ but $NB_{i0} < 0$. All other individuals are either ineligible ($B_i \leq 0$) or their application decision remain unchanged after assignment ($B_i > 0$ but $NB_{i1} > NB_{i0} > 0$ or $NB_{i0} < NB_{i1} \leq 0$).

In reality, the cost of applying is also a function of (both observed and unobserved) individual characteristics. But the model, even with simple costs, still helps to illustrate why I believe the DC program may lead to higher observed counts within the treated group.

Analytical Framework

I plan to use the local average treatment effects (LATE) framework to measure changes in the timing and volume of applications to Durham Social Services (Angrist and Pischke, 2008). I can be quite—but not totally—confident that, whatever the measured difference between the two subpopulations, it was driven solely by the *compliers*. I say "quite" because there are still some assumption that need to be made that would be costly to verify. I'll expand on this later in the section.

In this context, *compliers* are assigned to the treatment (Durham Connects) and, due to a *recommendation* or *referral* from a nurse home-visitor, apply for some eligible social service[6]. *Never-takers* would be families that, despite any referral or recommendation, refuse to apply for social services and *always-takers* are families that would have applied for social services with or without assistance from Durham Connects. *Non-compliers* will be ignored; I find it highly unlikely that a family will, given assignment to Durham Connects, choose not to apply for social services, even when informed of eligibility.

I also want to distinguish between the *timing* and *volume* of applications. That an eligible family chooses to apply for a service would affect that family's application *volume*. But also important is the *timing* of the application. That a family applies earlier than they would have otherwise is important.

[6]TK a percentage of who those who opted out would be great.

As mentioned, hyperbolic discounting and status quo bias push humans to procrastinate. I could count the number of applications submitted within a fixed window (say, 2 years) between the two groups and find no differences. Lost in this comparison, however, would be the *timing* of the applications. That is, if the treated group happened to submit the same amount of applications as the control group, but did so *sooner* than the control, that difference would be missed.

To this end, I propose using two analytical approaches. The first is a simple Probit model. For the outcome variable, I determine which families with children born during the RCT were observed during some fixed period of time. I will then pool all families and regress whether or not they were observed on vector of observed characteristics, eligibility, assignment group, and other variables, like distance from the DSS office.

This first approach is mostly to determine if there is really a "there" there. It is inelegant and ignores some very important aspects of the application process. For example, this totally ignores that eligibility for social services is dynamic. Most families, for example, only receive SNAP benefits for a few months at a time. However, this model is useful if I make the assumption that these dynamic shifts between the two groups are, on average, the same. That is, I'm assuming the two groups are balance in both observable and unobservable character, such as eligibility, even over time.

The second model will be a Cox proportional hazard (Cox PH) models for recurrent events. These event models explicitly account for time and counts. I must admit, though, that these recurrent event models are relatively new to me but I understand the basics enough to get how to structure the data. (I can certainly see that there is a relationship between Poisson regression and any hazard model.) I'm still deciding what type of Cox PH model suits this study (see Chapters 7 and 8 of Aalen et al. (2008)).

I've also thought about creating a dynamic choice model where, from week to week (or month to month), an individuals is either eligible for a particular social program. If so, then the individual decides whether or not to apply for a specific assistance program. Unfortunately, I do not know enough about these dynamic decision models and will need to read up on them more before suggesting anything.

Poisson Regression Model with Pooled Data

Assume I've collected data for some fixed interval of time T and know the complete population of RCT individuals, N. Any individual observed applying for assistance is tagged $y_i = 1$. Those never observed are tagged $y_i = 0$. I'm assuming that the treated and control groups are balanced along both observed and unobserved characteristics. The probit model is as follows:

$$w^* = \boldsymbol{x'\beta} + \delta D + u, \quad u \sim N(0, \sigma^2)$$

$$P(y = 1|\boldsymbol{x}, D) = \Phi\left(\frac{\boldsymbol{x'\beta} + \delta D}{\sigma}\right), \quad y = \begin{cases} 1 & \text{if } w^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

$$\star\, E[u|D] = 0 \,\star$$

The emphasis on $E[u|D] = 0$ is to reiterate that assignment, D, was random, and is therefore *exogenous*. What I care to find out is the difference in probability of observing *any* social service application given D. Furthermore, my hypothesis is that it leads to an *increase*, such that

$$E[y|\boldsymbol{x}, D = 1] - [y|\boldsymbol{x}, D = 0] > 0$$

$$\Rightarrow E\left[\Phi\left(\frac{\boldsymbol{x}'\boldsymbol{\beta} + \delta}{\sigma}\right) - \Phi\left(\frac{\boldsymbol{x}'\boldsymbol{\beta}}{\sigma}\right)\right] > 0$$

Of course, since $E[u|D] = 0$, the average treatment effect (ATE) is sufficient (and without bias)

Stratified Cox Proportional Hazard Model

TDB.

2.1 Next Steps

- Data
- [ ] Get IRB approval for short-form certificate data
- [ ] Submit proposal to Michael Becketts for approval to link
  - DC data to DSS
  - DSS data to other DSS data
- Methods
- [ ] Research Stratified Cox Proportional Hazard Model

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